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ABSTRACT

The Impact of School Choice on Pupil Achievement, Segregation and Costs: Swedish Evidence*

This paper evaluates school choice at the compulsory-school level by assessing a reform implemented in Sweden in 1992, which opened up for publicly funded but privately operated schools. In many local school markets, this reform led to a significant increase in the quantity of such schools as well as in the share of pupils attending them. We estimate the impact of this increase in private enrolment on the average achievement of all pupils using within-municipality variation over time, and controlling for differential pre-reform municipality trends. We find that an increase in the private-school share by 10 percentage points increases average pupil achievement by almost 1 percentile rank point. We show that this total effect can be interpreted as the sum of a private-school attendance effect and a competition effect. The former effect, which is identified using variation in school choice among siblings, is found to be only a small part of the total effect. This suggests that the main part of the achievement effect is due to more competition in the school sector, forcing schools to improve their quality. We use grade point average as outcome variable. A comparison with test data suggests that our results are not driven by differential grade-setting standards in private and public schools. We further find that more competition from private schools increases school costs. There is also some evidence of sorting of pupils along socioeconomic and ethnic lines.

JEL Classification: I22, I28, H40

Keywords: school-choice reform, private-school competition, pupil achievement, segregation

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1. Introduction

The question of whether school choice improves the quality of schooling is hotly debated in many countries throughout the world.[1] A central issue in this debate concerns the effects expected from encouraging competition in the school sector and letting families choose freely between public and private schools. There are two arguments to support the view that choice would improve the quality of schooling. First, there is the view that private schools simply are better than public schools. There is an extensive literature on this question, and a number of recent papers have turned to quasi-experimental evidence to assess the extent to which pupils benefit from attending private schools.[2] Clearly then, if private schools are better than public schools, choice should improve average school performance by the mere process of reallocating pupils and resources from the inefficient public sector to the private sector. The second argument is that choice induces competition among schools (for pupils and resources), which would provide them with an incentive to improve their quality.[3] Thus, an increased availability of choice should improve the quality of education for both private and public school pupils.

While acknowledging the potential productivity effects of school choice, critics worry about its effects on inequality. In particular, they worry about the implications for pupils who remain in public schools. While these pupils might benefit from the effect of competition on the public sector's productivity, they may be hurt by the departure of classmates and good teachers to the

[1] On this issue for Sweden, see the exchange between Bergström and Sandström (2001, 2002) and Wibe (2002). For the United States, see Hoxby (2005) and Rothstein (2005).

[2] Examples of such work are: Angrist et al. (2002, 2006) evaluating a private secondary school voucher experiment in Columbia; Rouse's (1998) work on the Milwaukee school voucher initiative; the work by Peterson et al (2002) on voucher initiatives in several US cities; see also Krueger and Zhu (2004).

[3] Examples of studies of school choice effects are: Cullen, Jacob and Levitt (2005, 2006) looking at the choice among public high schools in Chicago; Hoxby (2000) and Urquiola (2005) estimating the effects of choice between school districts in the US (the so-called Tiebout choice); Gibbons, Machin and Silva (2006) finding no effects of choice (or competition) for the U.K.; Lavy (2006) finding positive effects of choice for Israel; Hsieh and Urquiola (2006) estimating choice effects from the large-scale reforms that dramatically increased school choice in Chile during the 1980s and finding no effect on aggregate achievement but finding effects on segregation.

private sector (or through resources per student becoming diminished). More generally, the concern is that school choice would result in greater segregation of pupils by ability, income, ethnic background or religion, and that such segregation would have negative effects. Another debated issue is the role of school choice for the overall costs in the school sector.

This paper evaluates these arguments by assessing a school reform implemented in Sweden in 1992 that significantly increased the possibility for Swedish families to choose between different types of school. The reform required every municipality to cover the cost for each pupil residing in the municipality and attending a private school, a grant equivalent to almost all of the average per-pupil expenditure in the municipal public school system.[4] Sweden is a most interesting country for evaluating the effects of an increased private school share. The country went from a situation where pupils were assigned to their closest public school (*närhetsprincipen*), where the possibility of choosing another school was very limited, to a system that allowed pupils to freely choose among both public and private schools. Yet, the possibility for pupils to choose a private school (without moving) differs widely among municipalities and over time, since in some municipalities it took much longer to open new private schools than in others, and in a large number of municipalities they still do not exist.[5]

The differential variation in private schooling that developed after the reform is used to answer the following causal question: Does a higher incidence of private school enrolment impact overall

[4] Henceforth we use the term private school for all non-public schools, even though a more accurate term might be independent or free schools. Except for three boarding schools, all private schools in Sweden are publicly funded. School fees are not allowed.

[5] Swedish private schools are quite similar to the U.S. charter schools. Hoxby and Rockoff (2004) find in Chicago positive effects of charter school attendance for lower elementary grades but no effects for upper elementary grades. Since oversubscribed charter schools use a lottery to determine further admittance, lotteried-in and lotteried-out individuals can be compared. Note that they are not able to say anything about the effects of competition between charter and public schools. Similar school reforms introduced in New Zealand around the same time as in Sweden may be most relevant to the Swedish setting. Fiske and Ladd (2000) present many consequences of the reforms. However, due to the absence of test scores or data on grades, it was not possible to estimate the effects on any objective measure of achievement. The best that could be done was to rely on the impact of the reforms as perceived by teachers and principals.

pupil performance, sorting of pupils and school costs? We estimate models where we control for a full set of year and municipality indicators, as well as family and demographic characteristics. Such difference-in-differences models generate unbiased estimates if unobservable municipality characteristics are fixed over time. In order to assess whether this is the case, we also control for municipality-specific linear trends, and utilize data prior to the implementation of the reform to control for differential pre-program trends across municipalities. Further, we show that the total effect of the private school share on average pupil performance can be separated into a private school attendance effect (*private-attendance effect*) and a spillover effect of private school enrollment on pupils in both private and public schools (*competition effect*). We estimate the private-attendance effect by using variation in school type attendance between siblings. We also study the effects on segregation and on municipalities' costs for school funding. The bottom line is whether the productivity gains from choice and competition outweigh the costs.

We use a large administrative data set on individuals graduating from compulsory school (grade 9) in Sweden from 1988-2003. We have information on the children's (and their siblings') grades in individual subjects, as well as education and income of their parents. We have further merged school and residential information to this data set. The richness of the data allows us to improve on earlier studies in several respects.[6] First, we have data for a time period of 16 years, making it possible to study long-term effects as well as to test for differences in pre-reform trends between municipalities (since we have data prior to the reform). Second, by using variation in school choice between siblings we make a serious attempt to separate the total effect of private schooling into a private-attendance effect and a competition effect.

[6] This includes the previous Swedish studies of private school effects: Ahlin (2003); Björklund et al. (2005); and Sandström and Bergström (2005).

The paper is organized as follows: Section 2 describes the features of the Swedish school reform, the construction of the data set used and the evolution of private schooling in Sweden. Section 3 considers the impact of the reform on private school enrollment and how pupil achievement has evolved in municipalities with differential private school shares. Using these descriptive numbers, we present simple difference-in-differences estimates of the impact of the private school share on average grades (for comparison to the multivariate analyses that follow). Covariates to private school enrollment are further examined and some estimation issues are discussed. Section 4 reports results from our main difference-in-differences estimations of the private school share on average achievement and from sensitivity analyses. In Section 5, we report results from estimations of the private-attendance effect. In section 6, we study effects on school costs and segregation. Section 7 concludes.

2. Institutions, data and the evolution of private schooling

2.1. Sweden's school organization and the 1992 reform

Before 1991, public schools were operated by local governments/municipalities, but school funding and control was largely centralized. Local governments received earmarked funds from central government to cover the schools' operational costs. Teachers were state employees and directly paid by central government and schools had to follow a national curriculum. Pupils were assigned to, and had to attend, the public school in their local catchment area. Although private schools existed, some of which received some state funding, these accounted for less than one

percent of total enrollment.[7] These private schools were not required to follow central guidelines, and the funding of public schools was independent of the number of pupils enrolled in them. Hence, these schools did not compete for resources with public schools.

There were three key elements in the school reforms implemented in Sweden in the early 1990s, and they are still in practice today.[8] First, the financial responsibility for public schools was transferred from state to local government (in 1991-1993). Teachers became local government employees and are no longer paid directly by central government. Instead, central government provides local governments with block grants to cover their expenses (including teachers' salaries and other costs of running the schools). Local governments can also supplement these funds from their own revenues. Second, while every pupil was required to attend the public school in their neighborhood prior to the reforms, pupils are now allowed to choose between all public schools within the entire school region (municipality). This choice is, however, conditional on slots being available after those residing closest to the school had made their choices (this is the so-called *närhetsprincipen*). For this reason, choice between public schools has remained quite restrictive in practice even after this reform.

Third, the most radical component of the school reforms was perhaps that in 1992 local governments were required to provide private schools with a grant, equivalent to (most of) the average per-pupil expenditure in the public school system, for each pupil enrolled in private

[7] These were three boarding schools (very old schools with high fees: Gränna, Lundsberg and Sigtuna) and two Stockholm-based schools with a similar profile (Enskilda gymnasiet and Carlssons skola); two schools directed to pupils with special needs; five international schools (mainly directed towards children whose families were on temporary stays in Sweden); seven schools operated by Christian communities; and sixteen schools with a special pedagogical profile (e.g., Waldorf and Montessori).

[8] For a detailed description of the reforms, see Björklund et al. (2005); Ahlin and Mörk (forthcoming).

schools,[9] and could even choose to provide additional resources. With this reform, ordinary pupils were now given the option of attending non-public schools free of charge.

To be eligible for public funding, private schools have to be approved by the National Agency for Education. These schools then have to follow the national curriculum and are not allowed to choose their pupil bodies. If a school is oversubscribed, the decision as to whom to enroll next is based on a waiting list (where each child's' place is determined by the date of the parents' application). The local government can express opposition to an approved application to open a private school, but the number of applications rejected has been quite small. Private schools are not allowed to charge any fees.[10] Nor are there any restrictions on the ownership structure of the private schools eligible for public funding – whether religious, non-profit parent cooperatives, or for-profit corporations.

2.2. Construction of the data set

Our data set consists of approximately 20 percent of all individuals graduating from the ninth grade each year from 1988-2003, as well as their siblings and parents.[11] Information on grades at the end of compulsory school is available for all pupils from nationwide school registers. We also

[9] The minimum required funding percentage has changed over the years. In 1992 it was 85 percent, in 1999, 75 percent. It is less than 100 percent because it is calculated that there is some extra cost involved for public schools regarding special education. The goal is that public and private schools should be subject to about the same funding conditions.

[10] Private schools that received public funding were initially allowed to charge some fees, but these were heavily circumscribed. After 1997, private schools were no longer allowed to charge any fees. The boarding schools are exceptions. These are anyway not included in our analysis.

[11] The original sample criteria are a 20 percent random sample of the entire population of individuals born in Sweden in 1962-1987, as well as of the population of foreign-born individuals arriving in Sweden prior to their 18th birthday. To this random sample we then asked Statistics Sweden (SCB) to match the siblings and the parents, via the multi-generation registry and the censuses (obtained bi-decennially 1960-90). Unless otherwise indicated, all our data were provided by SCB, and the matching is based on the individuals' national identification numbers. Note that we have information for all siblings born 1987 or earlier for the individuals belonging to the random sample 1962-1984, but that we lack information for the siblings born prior to 1985 for the random sample drawn for cohorts 1985-1987.

have access to detailed individual demographic information as well as to data on the educational and economic outcomes of parents. This data set provides information on the school attended (in the ninth grade) and the region of residence of each pupil as well as the regional location of the school. The school registers contain information about all schools in Sweden, which allow us to identify whether a school is private and of what type (e.g., religious, special pedagogy, general).[12]

Information on pupils' school-leaving grades is available for each subject included in the curriculum. We use final grades from classes in the following mandatory subjects: natural sciences (physics, chemistry, technology and mechanics, biology), social sciences (history, religion, social studies, geography), English and mathematics.[13] The teachers' grading of each subject is on a 1-4 scale (1-5 in earlier years). To make sure that these scores are comparable across pupils (in any given year), the National Education Association issues guidelines to teachers that spell out the specific criteria a pupil must meet in order to qualify for a certain score.[14] The ninth grade scores are the main measures of a pupil's performance in the last three grades of compulsory school and secondary school track admittance is entirely based on these grades. We convert the scores to percentile ranks (based on grades for the 20 percent sample of pupils) and use the average percentile rank of each pupil as our main measure of academic achievement, which we label GPA. The mean (standard deviation) of this variable for the years 1988-2003 is 49.89 (23.17).[15]

[12] The school registers are matched with individual identifiers to determine what type of school an individual went to (as indicated by the school from which the grades were obtained at the end of compulsory school). Census registers are used to obtain individuals' location of residence for each year. Indicators for municipality of residence were generated from information on residence in the year the student started the ninth grade.

[13] We do not utilize the grades in Swedish as a measure of school performance since separate classes and grading scales are given to natives and some in the immigrant population. This has been the case for all years analyzed, and the fraction of immigrants taking special classes has changed a great deal over the years.

[14] Prior to 1996, these guidelines were based on a standardized test given in all the subjects. After 1996, the standardized tests were limited to Swedish, English, and math.

[15] We use the grades in math and English along with the average grades in natural and social science subjects to compute a student's average percentile rank. These four subject grades are weighted equally in the calculation of GPA. Where pupils had missing grades in some subject(s), we used their non-missing grades to compute an average

Given the possibility of some subjectivity in grade setting by teachers, we would have preferred to complement our grade analysis with test scores. These are only available, however, for a few years and for a selective sample of schools. The exception is 2003, for which year test scores are available for most pupils in mathematics and English. If we correlate the pupils' test scores (scaled in percentile ranks) and grades for 2003, we get estimated correlation coefficients of 0.81 (math) and 0.86 (English). The most important issue for us, however, is whether grades are inflated or deflated in private schools or in regions with more private schooling. We return to this later in Section 4.2.1 where we show that grade inflation does not seriously bias our results.

The main measure of private school choice used in this paper is the share of pupils attending private school in the school region (hereafter, municipality). More precisely, this is calculated as the number of ninth grade pupils residing in a given municipality that attend a private school (located inside or outside municipal borders) divided by the total number of ninth grade pupils residing in the municipality. It is important to use the share of private school pupils, based on municipality of residence (as opposed to municipality of enrollment), since the amount of resources potentially devoted to public schools in a school region/municipality is determined by the number of school age individuals residing in the municipality (and not of those enrolled in its schools). We calculate this measure for each year and municipality.

Note that the few private schools that existed prior to the reform were not required to report grades before 1993. When we calculate average grades before 1993, therefore, we simply ignore the pupils in these schools and use averages for the pupils in public schools (for 1988-92).

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grade. For each subject, a percentile point rank is attached to each grading. The average of these subject grades are then used as a measure of average performance. Finally, we deleted from our sample the small number of pupils who had scores from two years. The inclusion of math and English is not straightforward, however, since these subjects were taught at two levels prior to 1998 so that grades are not comparable across pupils. We assume that the grade at the lower (1 to 5) level equals the grade at the higher (1 to 5) level minus one. This appears to be a reasonable approximation if one compares the math and English grades to grades in natural and social sciences, which where taught at only one level. Using alternative mappings do not alter the results.

2.3. The impact of the reform on the evolution of private schooling

Figure 1 shows the evolution of private schooling at the compulsory level in Sweden from 1988 to 2003 for three different measures of the fraction of private school pupils. The top line shows the fraction of pupils in any of grades 1-9 attending private schools in a given year. The other two lines represent only those pupils who attended a private school in the ninth grade in a given year. The middle line includes all these pupils, whereas the solid (i.e., the bottom) line is based only on those who received a grade.[16] Our main measure used in the analysis is the one represented by the solid line, and we will refer to this as our baseline measure of the private school share. The reason for using this measure is that average grade is our main outcome variable. However, the results are very similar if we also include those without grades in the measure of the share of private school pupils (i.e., by using the measure graphically displayed by the dashed line). If we instead use the fraction based on pupils in all grades (the measure used in previous Swedish studies and represented by the dash-dotted line in Figure 1), we get somewhat larger estimates. This is an issue we return to in section 4.2.1. We will refer to these two latter measures as alternative measures of the private school share.

It is clear from Figure 1 that the 1992 reform resulted in a sharp increase in the number of pupils in private schools, where the share of pupils attending a private school in the ninth grade increased from less than 1 percent in 1989 to more than 5 percent in 2003. We also see that this increase naturally was faster for the fraction of private school pupils in all grades (1-9) in the years right after the reform, since newly established private schools often did not have any ninth

[16] The difference is not mainly due to a higher number of dropouts in private schools, but instead that some special pedagogy schools use a different grading system. These observations are therefore not included in the main estimations.

grade pupils. However, in 2003 there appears to be about the same amount of private school pupils in the ninth as in the other grades. This indicates that there will be a slowdown in the growth of private schooling in the next decade. From Figure 1, we further see that there were indeed some pupils attending private schools prior to the 1992 reform, but that this fraction was roughly constant up to 1993.

In Figure 2, we divide the ninth-grade private school pupils (with grades) into seven groups defined by the type of school attended. These groups are special pedagogy (e.g., Montessori and Waldorf), language, international, general, religious, special subject, and boarding schools. The increase in the private school share is mostly due to more pupils attending schools with a general profile.[17] We also note that the share for the boarding schools has been roughly constant throughout the period. These schools are quite special and we exclude them from the main analysis in this paper.

The increase in private schooling varies much between municipalities. Among the municipalities where any private school (with ninth grade pupils) existed in 2003 (which was the case in 92 of 284), the average private school share was 8.9 percent. There were, further, 21 municipalities where at least 10 percent of the ninth grade pupils attended private schools in this year, where the municipality with the largest share had 39.4 percent of its pupils in private schools. In Figure 3, we use Kernel density estimation to show the distribution of the municipality-specific changes in the private school share between 1993 and 2003. Each municipality is weighted by the number of pupils. The vertical axis shows the fraction of municipalities with a certain change. We see that the private school share has not changed at all in a large fraction of municipalities (i.e., those where private schools were not established), but

[17] If one includes pupils without grades it becomes clear that also the fraction of pupils in special-pedagogy private schools has increased markedly.

that there also are municipalities with small and large changes over time. Hence, the penetration of private schooling has differed greatly across municipalities.

3. Descriptive estimations and analytical framework

3.1. The basic identification strategy

An ideal identification strategy for estimating the effects of private schooling would be to randomly assign private schools to some municipalities but not to others, and then compare the outcomes between these two sets of municipalities. This kind of setting is rarely the case in most social programs and was not either so in the Swedish school choice reform, which was also introduced at the same time in the whole country. However, as described in the previous section, the private school share has evolved very differently across municipalities in Sweden since the reform was implemented.

Our empirical strategy is to look across municipalities in Sweden and compare the changes in outcomes (for all pupils, i.e., both public- and private-school pupils) within municipalities where the reform had a larger impact on the private school share to the changes within municipalities where it had a smaller impact. This difference-in-differences strategy is valid as long as the factors that explain the evolvement of the private school share are fixed over time within municipalities. This should not be taken for granted, but we can test this identifying assumption in several ways. We do this by including a large set of time-varying covariates, by controlling for municipality-specific linear trends as well as for potential pre-reform trends in academic achievement within municipalities. It is very important to control for pre-reform trends since the

key assumption in difference-in-differences models is that the treatment and control regions are on the same trajectory (Angrist and Krueger 1999). In our case, this means that any trend in the outcome variable would have been the same across municipalities in the absence of the reform. Since we have data for several years before the reform, we can investigate the occurrence of differential pre-reform trends and also control for such trends.

3.2. Descriptive estimations

We start by showing how grades have evolved over time for pupils in municipalities with more or less private schooling. We create four groups of municipalities based on the private school share (in the ninth grade) in 2002/2003. Municipalities with no private schooling make up one group, while the other three groups are those with high, medium and low private school shares respectively – these groups defined to contain a roughly equal number of pupils. Table 1 shows average grades for pupils in each group for the last two (2002/2003) post-reform years, and for the first two (1988/1989) and last two (1991/1992) pre-reform years that are available in our data set.

In the right-hand panel we report the private school share for these different groups. In the high private-share group, 11.8 percent of the pupils attended a private school in 2003. Hence, the difference between the high-share and the zero-share groups is 0.118, and between the high-share and the low-share groups 0.105. In Section 2 we pointed out that private schooling already existed on a smaller scale before the 1992 reform. However, these schools were operating in a completely different institutional setting where they did not compete for resources with public schools. The private school share is therefore set to zero for the pre-reform years. Consequently,

the difference-in-the-differences between the post and the pre-reform years (for Hi-Lo and Hi-Ze respectively) are equal to the differences in 2002/2003.[18]

Focusing on the left panel of Table 1, we see that pupils in the high-share group had, on average, somewhat higher grades than other pupils already before the reform. However, we also see a marked increase in average grades between 1991/1992 and 2002/2003 for this group, while no such increase can be observed for the other groups. GPA for the high-share group increased from 50.67 to 52.26 between 1991/1992 and 2002/2003. For the other groups, GPA decreased or remained about the same. In columns 5 and 6 of the last two rows, we report estimates of the difference in the changes in average GPA over time between municipalities with many and those with few/no private school pupils. These differences in the changes between 1991/1992 and 2002/2003 for the high-low and the high-zero group comparisons are 2.01 and 1.49 percentile rank grade scores respectively.[19]

Looking at the pre-reform years, we see that grades are roughly unchanged between 1988/1989 and 1991/1992 for both the high-share and the low/zero-share groups. There is no evidence that the high-share group of pupils were attending school in municipalities that experienced a positive trend in achievement prior to the reform. For the pre-reform years the differences in the changes between high and low, and high and zero, are virtually zero. Hence,

[18] In Appendix Table 1, we show comparable figures for the private school share using all ninth grade pupils in the municipality. This includes pupils who went to private schools prior to 1993, for which we lack information on grades. The calculated estimates using these figures are very similar to those discussed in the text.

[19] We can also compare the figures for 1993/94, when the reform just had been put in practice, with the ones for 2002/03. This comparison is also interesting since data on grades for private school pupils attending the ninth grade are not available before 1993. We then find the estimates (standard errors) to be somewhat lower: 1.49 (0.60) and 1.13 (0.63), respectively. Note that we expect (and observe) only a small estimated effect from 1991/92 to 1993/94, since it takes time for the reform to generate new private schools and for pupils to have time to graduate from these schools. Comparing the figures for 1993/94 across groups, it is evident that already at the beginning of the post-reform period, the high-share group had a somewhat higher private school share (0.024 versus 0.002 and 0.001 for the low and zero group respectively. The estimates (standard errors), comparing the private-school share figures for 1993/94 with the ones for 2002/03 are: 0.084 (0.010) and 0.095 (0.010) respectively.

there is no evidence at all about any difference in pre-reform achievement trends between municipalities with a high or low/no growth in private schooling.

By dividing the 2002/2003 difference estimates for GPA with the corresponding ones for the private school share, we get a cross-sectional estimate of the impact of the private school share on average GPA for 2002/2003. These estimates are 33.14 for the high-low comparison (by dividing 3.48 with 0.105), and 26.53 for the high-zero comparison (3.13/0.118). By dividing the difference-in-differences estimates for GPA with the corresponding one for the private school share, we obtain estimates of the effect of a one-unit increase in the private school share on average GPA. These estimates are found to be 19.1 for the high-low comparison (2.01/0.105) and 12.6 for the high-zero comparison (1.49/0.118),[20] and can be interpreted thus; moving all pupils from public to private schools would generate between 12 and 19 higher percentile rank grades for the average pupil. More realistically, our interpretation of these estimates is that a 10 percent increase in the private school share would generate an increase in average GPA by 1-2 percentile ranks. These are our descriptive estimates. In our main analysis in section 4, we control for changes in observable characteristics over time, as well as incorporating changes across all years and municipalities.

3.3. Explaining private school enrolment

In order to interpret the reported difference estimates in Table 1 as causal effects, we need to assume that municipality characteristics either are unchanged over time or are uncorrelated with

[20] If we use data for 1993/94 and 2002/03, we arrive at only slightly lower figures; 17.7 and 11.9. Note that we expect (and observe) only slightly smaller difference-in-differences estimates using 1991/92 and 2002/03 data compared to using 1993/94 and 2002/03 data, the reason being that it takes time for the reform to generate new private schools and for pupils to have time to graduate from these schools.

the evolution of private schooling. To see whether this is the case and what explains private school attendance in general, we investigate the association between a set of potentially important control variables and the private school share.

In Table 2, we show means and standard deviations for some of the variables. Summary statistics for the individuals are shown in columns 1 and 2 for data from 1993-2003. Private school pupils do not only have higher GPA, but are also more likely to be second-generation immigrants and to have parents who are university educated.[21] The immigrants who are most likely to be found in private schools tend to have arrived at an early age.

Summary statistics for weighted aggregated data are shown in column 3.[22] The figures for the pupil characteristics are now for the data from 1988-2003, which are the years used in most aggregate level estimations. These figures are based on the individual characteristics aggregated up to municipality-year level, where the weights are based on the frequency of ninth grade individuals in each municipality-year cell in the 20 percent random sample. About 2 percent have exams from private schools. The majority of the parents have either high school or university education, and about 12 percent are either immigrants or second-generation immigrants.

In the lower part of Table 2, we show summary statistics for some aggregate variables for 1993-2003 that are supposed to capture the degree of competition between public schools and possible consequences of the decentralization of school financing, the two other features of the Swedish school reform of the early 1990s. These variables differ between municipalities and

[21] We use indicators of whether at least one of the parents has obtained a university degree (at least 3 years) or at least high school education (at least 3 years). We use income from work (including self-employment and sickness benefits) as our measure of income, where the income data are based on compulsory employer reporting. We set income to missing if it is less than 20,000 SEK (about 2,500 USD in 2000 prices) and calculate family income as the average of the sum of the parents' income when the child is about 5 and about 10 years old. Since we have data on immigration status for both children and parents we can look at separate effects for first- and second-generation immigrants. We also know when they arrived and from which country. We use immigration age and region of origin of the child to further control for the background of immigrant pupils.

[22] Throughout, we weight the aggregate estimates with the number of pupils in each municipality-year cell. In our main estimations, unweighted estimations of the effect of the private school share on average grades give very similar results as the weighted estimations.

years. Public school competition is proxied for by the number of public schools per 100 pupils and the mean logarithm of distance to nearest public school for the pupils residing in the municipality. The means of these variables are 1.47 and 7.28 (1,450 meters) respectively. School financing variables are captured by the average pupil-teacher ratios in schools and by the logarithm of total school costs per pupil. The school financing variables are averages for all compulsory school pupils in the municipality.[23] The means for these variables (all measured for grades 1-9) are 12.63 pupils per teacher, 14.44 pupils per certified teacher and 54,720 SEK (10.91 log points or about 7,000 EUR/9,000 USD) yearly costs per pupil.

Next, we perform some aggregate weighted descriptive regressions, where the dependent variable is the private school share in the ninth grade. All variables are municipality-year averages for 1993-2003. Results are reported in Table 3. The cross-section estimates in column 1 show that higher private school shares are more likely to be found in municipalities with more parents with university education and a higher fraction of second-generation immigrants. Larger municipalities (with larger pupil populations) are also strongly related to higher private school shares. In column 2 we report estimates from a regression with municipal fixed effects. The general picture is unchanged. Private school shares are positively associated with parental education, the fraction of second-generation immigrants, and size of pupil populations also when we compare changes in municipalities over time. Hence, there are reasons to expect that multivariate estimation might yield different results compared to the descriptive ones reported in Table 1.

[23] The distance variable is based on data from Statistics Sweden, asked to combine school and residential location to derive measures of the distance from the student's home to the nearest public school and to the nearest private school respectively in the municipality of residence, and to the nearest private school not in the municipality of residence. Here we only use the information about distance to nearest public school. Pupil-teacher ratios are calculated as the number of pupils in the municipalities' compulsory schools divided by the number of full-time certified (equivalent) teachers. The school cost variables include all costs, i.e., costs for teachers as well as semi-fixed costs such as rents.

In columns 3 and 4, we add variables capturing differential public school competition and school financing between municipalities. The variables are jointly statistically significant in the cross-section regression (p-value=0.09) and in the fixed-effect regression (p-value=0.03). In the fixed-effect regression, the distance to public school variable is barely statistically significant (p-value=0.12), whereas the pupil-teacher ratio variables are jointly and individually significant. If the pupil-teacher ratio variables are entered separately, both are estimated positive, but only the one calculated for certified teachers is significant.[24] We conclude that there is only weak evidence of the private school share is associated with variables indicating the degree of competition between public schools. We will return to this issue in Section 4.2.2., where we investigate how sensitive the estimates of the private-school share effect on achievement are to the inclusion of these variables.

3.4. Analytical framework

Before we proceed to the estimations, we need to discuss some issues related to the interpretation and estimation of such models. We discuss the estimation of individual-level as well as aggregate models.

Let us formulate a very simple private schooling model, which contains a private-attendance effect and a competition effect, and some other individual factors that we cannot observe.

(1) $y_i^1 = a^1 + b^1 \overline{P} + \varepsilon_i^1$ if $P_i = 1$, and

(2) $y_i^0 = a^0 + b^0 \overline{P} + \varepsilon_i^0$ if $P_i = 0$,

[24] If we also add the log cost per pupil as a control variable, its impact is statistically insignificant.

where y_i^1 and y_i^0 are counterfactual achievement outcomes for pupil *i* attending either a private ($P_i = 1$) or a public ($P_i = 0$) school; b^0 (b^1) is the marginal gain for a public (private) school pupil from an increase in the private school share in the municipality, $\overline{P}$. The coefficients b^0 and b^1 capture any spill-over from other pupils' private school choice on own achievement. We consider b^0 and b^1 as competition effects, since competition probably is the main mechanism with which this spill-over work. The difference between b^1 and b^0 measures how much larger the competition effect is for private school pupils vis-à-vis public school pupils. The difference between a^1 and a^0 (Δa) is the average gain from attending a private school (net of competition effects). The terms ε_i^0 and ε_i^1 are individual-specific random terms, assumed to have zero means. The difference between ε_i^1 and ε_i^0 ($\Delta\varepsilon$) is the individual-specific-gain (deviating from the average gain Δa) from attending a private school. ε_i^0 and ε_i^1 are allowed to be correlated with P_i.

Combining (1) and (2) gives us

$$(3) \qquad y_i = a^0 + [\Delta a + \Delta\varepsilon_i]P_i + [b^0 + \Delta b P_i]\overline{P} + \varepsilon_i^0,$$

where $\Delta a = a^1 - a^0$ and $\Delta b = b^1 - b^0$. Model (3) specifies the relationship between a pupil's achievement and private schooling, where the marginal impact of P_i varies among individuals (as in a random coefficient model), and the marginal impact of $\overline{P}$ is allowed to differ for private and public school pupils.[25]

[25] Note that estimation of a model of average achievement for public school pupils on the private school share (as in an empirical version of equation (2)) is estimated in many papers including Hoxby (2000) and Sandström and Bergström (2005). However, Hsieh and Urquiola (2005) point out that even if school choice (i.e., the private school share) would be randomly assigned across municipalities, such a model do not yield consistent estimates of the causal effects of choice, if choice impact both school productivity and the sorting of students. Since we look at outcomes for both public and private-school students, we are able to control for sorting in our aggregate estimations under the assumption that students with varying characteristics benefit equally much from interacting with better peers, i.e., that peer effects are linear. For Sweden, there exists no study of peer effects at the compulsory level. However, Sund (2007) find some evidence of non-linear peer effects (low achievers benefit the most) at the high school level in Sweden.

In order to understand the problems to estimate (3), we also need to assume how a pupil's private-attendance decision is made. It is reasonable to assume that a private school is chosen if the expected return from attending such a school is larger than the expected return from attending a public school. If the choice is made with knowledge about $\Delta\varepsilon_i$, the individual-specific gain, we have that $\text{cov}(\Delta\varepsilon_i, P_i) > 0$.

Let us start by discussing how to estimate the parameters in model (3) using individual data. Assuming, for simplicity, that $\Delta b = 0$ (so that competition effects are the same for private and public school students), adding subscript for municipality, *m*, and adding a term, δ_m, (which captures unobservable municipality-level variables, including those at the individual level averaged up to the municipality level), we can rewrite (3) as

$$(4) \qquad y_{im} = a^0 + \Delta a P_{im} + b^0 \overline{P}_m + \delta_m + v_{im}$$

Since $v_{im} = \varepsilon_{im}^0 + \Delta\varepsilon_{im} \cdot P_{im}$, v_{im} will be correlated with private school attendance because $\text{cov}(\Delta\varepsilon_{im}, P_{im}) \neq 0$ and $\text{cov}(\varepsilon_{im}^0, P_{im}) \neq 0$. If this correlation is positive we will overestimate Δa, which is the private-attendance effect.[26]

There are some special problems involved in estimating a model that includes both an individual-level variable and the same variable grouped at some aggregate level. Even if $\text{cov}(\overline{P}_m, \delta_m) = 0$, but $\text{cov}(P_{im}, v_{im}) \neq 0$, estimates of both Δa and b^0 will be biased. Thus, omitting relevant variables that differ within municipalities and that also are correlated with private school attendance would lead to biased estimates of both Δa and b^0.[27] For instance, if

[26] If $\Delta b \neq 0$, we have that $y_{im} = a^0 + [\Delta a + \Delta b \overline{P}_m] P_{im} + b^0 \overline{P}_m + \delta_m + v_{im}$, so that the marginal impact of P_i (the private-attendance effect) is a function of two terms, Δa and $\Delta b \overline{P}_m$, where the later term is the part that is due to the competition effect being larger for private school pupils than for public school pupils.

[27] Acemoglu and Angrist (2000) discuss this issue in another application (where they attempt to separately identify the individual and external returns to education).

v_{im} is unobserved pupil ability varying within, but not between, municipalities (so that $V(\bar{v}_m) = 0$ and $V_m(v_i) \neq 0$), and is correlated with P_{im}, then the bias in the estimate of the impact of P_{im} carries over to the estimate for the impact of $\bar{P}_m$. In fact, it follows that we get an unbiased estimate of $\Delta a + b^0$ and that $bias(\hat{\Delta a}) = -bias(\hat{b}^0)$.[28] Estimating both Δa and b^0 consistently in (4) requires that the researcher either can include all those variables that sufficiently control for the omitted terms or finds two exogenous instruments, one for P_{im} and one for $\bar{P}_m$.

In this paper, we attempt to control for unobservable factors by including family fixed effects, so that we compare achievement differences between siblings attending different school types (i.e., a private or public school). This means that we assume that $\text{cov}(\Delta\varepsilon_i, P_i)$ and $\text{cov}(\varepsilon_i^0, P_i)$ are the same among siblings from the same family. See section 5.2.

Let us now turn to a discussion of the estimation of aggregate level models. Aggregating (3) up to the municipality level generates

$$\bar{y} = a^0 + (\Delta a + b^0)\bar{P} + \Delta b\bar{P}^2 + \overline{\Delta\varepsilon \cdot P}, \quad (5)$$

where average achievement, $\bar{y}$, is a quadratic function of $\bar{P}$. This is because a higher $\bar{P}$ means more private school pupils in the municipality and the benefit from a higher private school share for those pupils might be different than for public school pupils. The specification is linear only if $\Delta b = 0$. The last term, $\overline{\Delta\varepsilon \cdot P}$, is the covariance between the individual-specific net gain of

[28] Let us specify the model $y_i = c_0 + c_1 x_i + c_2 \bar{x} + e_i$, where $\text{cov}(x_i, e_i) \neq 0$. Rewriting this model as $y_i = c_0 + c_1 z_i + h\bar{x} + e_i$, where $z_i = x_i - \bar{x}$ and $h = c_1 + c_2$, it follows that $\text{cov}(z_i, \bar{x}) = 0$. Then one can show that $p\lim \hat{c}_1 = c_1 + bias$ and $p\lim \hat{c}_1 + c_2 = c_1 + c_2$, where $bias = \text{cov}(x_i, e_i) / \text{var}(x_i - \bar{x})$. This gives that $p\lim \hat{c}_2 = c_2 - bias$. Hence, the sum of the impacts of the individual and aggregate variables can be estimated consistently, whereas it is not possible to consistently estimate both the individual and aggregate impacts separately. The upward bias in the estimate of the individual impact will be proportional to the downward bias in an estimate of the aggregate impact.

attending a private school and private school attendance. This term is positive if those who are more likely to choose a private school do so because their expected return from attending such a school is higher.

Note that when we use aggregate data, an estimate from a linear regression of average achievement on the private school share in a municipality can be directly interpreted as the sum of the private-attendance effect, Δa, and the competition effect (from private schooling on public school pupil performance), b_0, only if (1) and (2) are correctly specified, $\Delta b = 0$, and if $\overline{\Delta\varepsilon \cdot P}$ is constant among municipalities. In the estimations, we will test for any evidence of quadratic effects of the private school share on average achievement. When we use aggregate data, the aggregate unit is a municipality-year cell. We will then assume that $\overline{\Delta\varepsilon \cdot P}$ are identical for all units. On the other hand, we allow for correlation between $\Delta\varepsilon_i$ and P_i within each unit. Hence, pupils (or their parents) are allowed to make rational choices of whether or not to attend a private school, as long as this rationality is the same between units or, if we control for municipality and year indicators, as long as the change in this term is the same within municipalities over time.

Hence, it is much less demanding to obtain an unbiased estimate of the total effect of private schooling $\Delta a + b^0$ than to obtain unbiased estimates for the respective components, i.e the private-attendance and competition effects. The reason being that the former only requires controlling for unobservable municipality characteristics (but not for unobservable characteristics at the individual level). Since we have panel data, we can control for unobservable municipality characteristics that are fixed over time. Hence, our focus is to estimate aggregate versions of model (4), such as

(6) $\quad \bar{y}_{mt} = c + \beta \bar{P}_{mt} + \gamma_m + \alpha_t + \bar{\eta}_{mt}$,

where $c = a^0$, $\beta = \Delta a + b^0$, $\gamma_m = \bar{v}_m + \delta_m$, and α_t is an added term capturing time effects, and where we have assumed that $\Delta b = 0$. This is the basic empirical approach of the paper.

Still, an empirical challenge is to control for unobservable variables that differ across municipalities and years (such as varying pupil composition with respect to family and demographic background, and differential development of school characteristics). We attempt to do this in several ways: First, we include a number of time-varying municipality-specific variables as controls. Second, since we have data for several years before the reform we are able to control for differential pre-reform trends in achievement. We do this by including a variable in the regressions that differ across municipalities as predicted by the pre-reform (1988-1992) trend in achievement in the municipality. Third, we also include municipality-specific trends for the whole period as controls. Any unobservable characteristics of municipalities that determine the differential impact of school choice are then allowed to change over time as long as these changes are similar for a municipality throughout the analyzed period.

4. Effects of the private school share on average performance

4.1. Basic results

Using data for the years 1988-2003, we estimate the following empirical model of the impact of the private school share on average school performance:

$$(7) \qquad \bar{y}_{mt} = c + \beta(\bar{P}_{mt}T) + \lambda\overline{X_{1,mt}} + \varphi X_{2,mt} + \gamma_m + \alpha_t + \bar{\varepsilon}_{mt},$$

where $\bar{y}_{mt}$ denotes the average GPA for pupils residing in the municipality (or schooling market) *m* at time *t*; $\bar{P}_{mt}$ denotes the share of pupils residing in municipality *m* that attend private school in any municipality at time *t*; *T* is a reform dummy, taking the value one for the years following the introduction of the reform (for t>1992) and zero prior to the reform (t<=1992); $\overline{X_{1tm}}$ and $X_{2,mt}$ are observable factors measured at the municipality level; γ_m and α_t represent municipality- and year-specific effects respectively; and $\bar{\varepsilon}_{mt}$ is a random error term. Thus, equation 7 takes into account that the data are available both pre-reform (T=0) and post-reform (T=1). In equation 7, we have assumed that the unobservable characteristics have the same impact for all pre- and post-reform years. We further allow the regression error to be correlated between individuals in the same municipality.

In Table 4 we report estimates of model 7. Columns 1 and 2 show estimates from cross-section models and columns 3-7 show estimates from models where we have included the full set of municipal indicators. In the specifications underlying the estimates reported in column 2 and in columns 4-7, we also control for a number of time-varying covariates (listed in Table 3). In the first column, we see that the private school share is strongly associated with average GPA. A one-unit higher private school share is associated with 28.4 percentile rank higher GPA. In the second column, we can see that this estimated effect is greatly decreased and becomes statistically insignificant when adding the covariates. Hence, selection is very important in our estimations.

Next, we consider our baseline difference-in-differences regressions. In columns 3-4 we show these estimates, which are identified using the association between changes in GPA and changes in the private school shares within municipalities over time. With municipal fixed effects, but no covariates, we find that a one-unit increase in the private school share is associated with 10.6

percentile rank points higher GPA. This estimate is very much in line with the simple estimate from Table 1, where we compared changes from 1991/1992 (prior to the reform) to 2002/2003 for the group with the highest private school share and the group with no private schooling. This estimate was found to be 12.6. Hence, adding all the other years and separate measures for all municipalities has only a minor impact on the estimated effect. When we add covariates to the fixed effects estimations in Table 4, we see that this has only a minor impact on the private school share estimate. Hence, when we control for municipal fixed effects, any remaining selection seems to be very small. With municipal fixed effects and controls, the effect of a one-unit increase in the private school share is an increase in average GPA by about 9 percentile rank points. An interpretation of the magnitude of this effect is that if the average private school share is 5 percent of the pupils, a doubling of this would increase average GPA by almost 0.5 percentile rank points. In column 5, we see that this effect is roughly constant over time.

When, in column 6, we add controls for municipality-specific trends for all years, so that these estimates use the association between changes in GPA and changes in private school shares that deviate from a linear time trend within municipalities, the private school share estimate is not affected. In column 7, we add controls for municipality-specific pre-reform trends, so that these estimates use the association between changes in GPA and changes in the private school shares within municipalities, controlling for differential trends in average GPA in the municipalities before the introduction of the reform.[29] Again, the private school share estimate is unaffected.

We have also carried out the test of adding a quadratic term of the private school share to specification 7. These estimates are shown in Table 5. We find no evidence of a non-linear effect of the private school share on average grades (but the standard errors are very large), and the

[29] This is done by controlling for a variable that is predicted for all years (1988-2003) from a regression of GPA on municipality-specific effects interacted with linear trends, using only the pre-reform years (1988-1992). The predicted variable captures pre-reform trends in GPA (conditional on other variables included in the regressions).

estimates of the main effects are close to the corresponding baseline estimates in Table 4. As was shown in Section 3.4, the total effect can be interpreted as a private-attendance effect plus a competition effect for public school pupils, where the quadratic effect is the difference in the competition effect for private and public school pupils respectively. Since there is no evidence of a non-linear effect of private schooling, we can interpret the estimated total effect in a linear specification as a private-attendance effect plus a competition effect which is the same for pupils in both private and public schools.

We have also investigated whether the impact of the private school share on average GPA differs across sub-populations. Our results have so far shown evidence of a positive overall effect of private schooling. Does this mean that all sub-groups are winners, or are there losers as well? To investigate this issue we look at effects for immigrants, for two groups by parental education, and for groups by family income. Results are shown in Table 6. Our baseline overall estimate is displayed for reference in column 1. In the second column, we look at effects for pupils who are first or second generation immigrants. This estimated effect is very similar to the baseline one; an increase in the private school share by 10 percentage points increases the average GPA among immigrants with 0.8 percentile rank points. In columns 3 and 4 we show results for subgroups divided by parental education; regression 3 only includes children whose parents have less than high school education; and regression 4 only includes children who have at least one parent with university education. Interestingly, the estimated effect is clearly larger for the group with high parental education, whereas the estimate is statistically insignificant for the group of pupils whose parents have low education. In column 5 we report the corresponding estimate for pupils whose parents' income is in the lowest quartile (measured in relation to pupils in the same cohort). Column 6 displays the estimate for those belonging to the highest quartile. The estimated effects for these two groups are both somewhat larger than our baseline estimate for all pupils.

From this analysis we learn that even though there is some evidence of heterogeneous effects, none of these sub-groups are losing from a higher private school share.

4.2. Sensitivity analysis

4.2.1. Mismeasurement of variables

Alternative grade outcomes

So far, we have used GPA as the outcome measure. However, this might hide different effects for different subjects. In Table 7, we show the estimates resulting from model 7 using separate subject grades as outcomes. The results are very similar for math, English and the science subjects, but somewhat lower for social science subjects. Anyway, the results are statistically identical across subjects.

Differential grading standards in private and public schools

When we look at the effects of private schooling on pupil achievement, we have so far used average subject grades at the end of the ninth grade as proxy for pupil achievement. It would not necessarily be problematic if these grades were imperfect measures of achievement. Random measurement error in the dependent variable only affects the standard errors and not the unbiasedness of the estimated effects. However, for us it would be a serious issue if teachers in public and private schools have different grading standards. We are, in fact, able to investigate this for 2003. For this year, grades and scores on achievement tests from the ninth grade are available for most pupils in mathematics and English. We proceed by estimating models at the individual and municipality level, respectively:

$$(8a) \qquad y_i^s = c + \pi_1 P_i + \pi_2 T_i^s + \lambda X_i + \varepsilon_i ,$$

(8*b*) $\quad \overline{y^s}_m = c + \pi_1^{/} \overline{P}_m + \pi_2^{/} \overline{T^s}_m + \lambda^{/} \overline{X}_m + \overline{\varepsilon}_m$,

where y_i^s and $\overline{y^s}_m$ are individual and municipality-average grades in subject *s* and T_i^s and $\overline{T^s}_m$ are individual and average test scores in subject *s*. The private school attendance parameter π_1 captures whether individuals in private schools have higher or lower grades than pupils in private schools, given test scores and other observable characteristics. Hence, π_1 is a direct measure of how inflated the grades are in private schools. Similarly, $\pi_1^{/}$ is a measure of how inflated grades are in municipalities with different private school shares.

Tables 8a and 8b show results from the estimation of regressions (8a) and (8b) using mathematics and English as outcomes. In the regression results for columns 1 and 3, no covariates have been included, whereas results in columns 2 and 4 include the set of covariates. The individual-level regressions reveal that the estimate is negative and statistically significant for English, whereas there is no detected effect for mathematics. The negative estimate for English means that private school teachers give too low grades given achievement as measured by tests. Hence, perhaps surprisingly, we find some evidence not of grade inflation, but of grade deflation in private schools.[30]

From the aggregate level regressions we find that the estimate is positive for English (with covariates), but negative for mathematics. However, none of the estimates are statistically significant. Thus, this exercise gives no reason to believe that the use of test scores instead of

[30] We find for the other covariates that, given test scores and the private school dummy, both higher math and English grades are associated with pupils being female, having older parents, having parents with higher education, having parents with higher income or parents with non-reported incomes. Indicators for being an immigrant or second-generation immigrant are statistically insignificant.

grades would generate different results in the main regressions of the private school share on average achievement.[31]

The private school share: alternative measures

So far, we have estimated effects of the private school share at time *t* for the pupils in their last year of compulsory school on average grades for the very same pupils used to calculate the private school share. This is reasonable, even though we ideally would like to know if and when pupils have switched school types. However, since we lack information about the individuals' attended school type in all but the last grade, we have to assume that endogenous school type mobility before the ninth grade is a minor issue. Also, schools might redistribute resources from higher to lower grades, as a response to more private schooling in lower grades. If this issue is important, this would lead to a downward bias in our estimates using the private school share in grade 9. We therefore estimate model 7 using alternative private school share measures. This is also necessary since we want to compare our results to the ones from previous Swedish studies, all of which have used different measures than our baseline one.

As alternative measures, we use the private school share based on available aggregate information about private school attendance in grades 9, 7-9 and 1-9. We also calculate the average of the private school share for year *t*, *t*-1 and *t*-2 for these variables and use these as additional alternative measures. The 7-9 and 1-9 measures might deal with a potential reallocation of resources from earlier grades. However, they also have some obvious drawbacks. First, increases in the private school share in grades 1-9 will to a large extent reflect that private

[31] Wikström & Wikström (2005) compare scores given by high school teachers with scores from the standardized university entrance examination (SweSAT) and find evidence that private schools inflate grades, along with weaker evidence that public schools in municipalities with many public schools do the same thing. Their results suggest that average grades will tend to be inflated in municipalities with more public high schools (more densely populated) and with more private high schools (relative to the total number of high schools). Since we are looking at compulsory school levels, results for grade inflation might very well differ. However, it is also the case that the SweSAT test-takers only are those individuals planning to attend university, that the pupils can take the test several times, and that the test sometimes is taken a long time after completed high school.

schooling has increased faster in lower grades (as seen in figure 1). Since we lack any measure of achievement for pupils attending earlier grades, however, we actually estimate the private school share effect on achievement where achievement is calculated for only a small sub-sample of the pupils used in calculating the private school share measure. Second, it is common to organize compulsory school so that the different levels (grades 1-3, 4-6 and 7-9) take place in separate schools, between which any resource redistribution is unlikely to happen.

We have noted that earlier Swedish studies that estimate effects of private school choice have used different measures and analyzed different periods, compared with our study. Sandström and Bergström (2005) use the private school share measure in which pupils in grades 1-9 are observed at year *t*. They use individual-level data and study the effects of private school share on average grades and mathematics test scores for public school pupils in 1998. They also perform some analysis using aggregated data and examine the effects of the private school share on average grades for the years 1992 and 1994-1997. Björklund et al. (2005) also use the private school share measure in which pupils in grades 1-9 are observed at year *t*, but they have averaged it over *t*, *t*-1 and *t*-2. They use test scores (about 50,000 pupils) and grades (total population of pupils) in mathematics, English and Swedish for 1998-2001. Both these studies find much larger effects of the private school share on achievement than we do. Sandström and Bergström find enormous effects of the private school share on pupil achievement for public school pupils using individual data for 1998 and instrumental variable techniques.[32] Björklund et al. (2005) find

[32] The estimates in Sandström & Bergström (2005) are here transformed to percentile point ranks and re-scaled in order to be comparable to our estimates. Using individual data for 1998, they find very large effects of the private school share on average grades and math test scores for public school pupils using OLS. The estimated effect amounts to about 28 percentile rank point higher test scores if we go from a situation from no to only private schooling. Since they only look at outcomes for public school pupils, they attempt to correct for this by using a standard sample selection model, where the identification comes from assumptions about the distribution of the error term in the main equation. The private school share estimate then almost triples. They also use instrumental variable techniques. To correct for endogeneity of non-public schooling, they predict the private school share using estimates from a (sample selection corrected) model using all 284 municipalities. They further assume that average grades in

positive and statistically significant effects for all subjects; about 40 percentile rank effects for grades, and even larger for test scores.

What then is a large effect? If, for example, we interpret the estimated effect for mathematics grades in Björklund et al. (2005), this effect amounts to a 4 percentile point gain for the average pupil (in both public and private schools) of an additional 10 percentage point pupils in private schools. This would mean that for a municipality with 1,000 ninth grade pupils, of whom 25 initially attended private schools, if an additional 25 pupils should change over from a public to a private school, the average pupil (in any school) would gain 1 percentile point in mathematics. This is an enormous effect that clearly speaks in favor of a policy of establishing more private schools. The estimated effects in our study are only about one-fourth of these.[33] In order to try to understand these differences, we have analyzed alternative measures of the private school shares, and also restricted the time-periods to the ones analyzed in previous studies.

The main results from this analysis are shown in Table 9. Column 1 displays our baseline estimate based on our preferred measure (aggregate units based on ninth grade pupils resident in a municipality and receiving school-leaving grades in a given year). We do not include the pre-reform years since these alternative private-school share measures are only available for 1993-2003. In column 2, we show the estimate when using pupils enrolled in ninth grade in any school

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1992, the share of municipality responsibilities contracted out to private enterprise, a non-socialist government dummy, the voting share, and population density do not affect the share of pupils in private schools, given other control variables. We note that it is difficult to believe that some of these variables are valid instruments since there is every reason to believe that children of parents that vote for conservative local governments or demand private services are likely to have better educational outcomes, regardless of the private school share. If so, the produced estimates would be biased. Their estimate is then very similar to the sample-selection adjusted estimate. When they use both instrumental variables and sample selection adjustment, their estimate increases even more. Their aggregate panel-data estimates are positive and sometimes statistically significant. However, it is difficult to interpret the size of the estimates since the standard deviation of the dependent variable is not reported.

[33] The motivation in Björklund et al. (2005) for using private school shares in grades 1-9 is that the school as a whole might change its manner of teaching in response to a change in competition. Although this is a possibility, we note that it is common that there are separate schools for higher and lower compulsory-school grade levels.

located within the municipality.[34] The private school share estimate decreases somewhat, but is still highly statistically significant.

In columns 3-7, we use measures similar to the one in column 2, but where we either use averages calculated over three years and/or measures for shares in several grades combined (7-9 or 1-9). Previous Swedish studies have used the measure for grades 1-9. Sandström and Bergström (2005) use the measure that we use in the specification underlying the estimate in column 6 (grade 1-9 at year *t*), and Björklund et al. (2005), use the column 7 measure (grade 1-9 averaged over *t*, *t*-1 and *t*-2.). Table 9 shows that the private school share estimates from using the 3-year averages or the grade 7-9 measure are close to the other alternative grade-nine estimate in column 2. However, the estimate almost doubles if all grades are used (i.e., grades 1-9). The conclusion is that the very high estimated effects reported in previous Swedish studies appear to be partly driven by the use of private school share in grades 1-9. The picture seen from Table 9 is that much of this is driven by the private school share in grades 1-6 (compare columns 5 and 7). We find it difficult to believe that there would be such as large causal effect from the amount of private schooling in early grades in year *t* on the grades received by those leaving grade 9 in the same year.

Another reason why the results differ compared to previous Swedish studies may be the time-periods analyzed. This also turns out to be the case when we compare our study with Björklund et al (2005). If we use their private school share measure and data for the same period as they do (1998-2001), we get very similar estimates as in their study (around 40 percentile rank effect for GPA). However, if we use the private school share measure calculated for the pupils in the ninth grade alone, we get small effects. Sandström and Bergström (2005) estimate aggregate models

[34] The private school share measures in columns 2-7 are calculated for all pupils, i.e. also pupils for whom we do not observe grades. The measures are also based on all pupils attending school in the municipality, even if they lived in another municipality. This is also the definition used in Björklund et al. (2005).

for 1992 and for 1994-1997, a period when private schooling had just started to increase. If we use these years and their private school share measure, we get an estimate of around 11, very similar to the main estimate in the present paper for all years. However, if we estimate an unweighted regression (i.e., we weight big-city municipalities like Stockholm the same as small rural school regions), which is what they do, we get an estimate of about 26 for GPA and an only slightly higher one for mathematics.

To conclude, it seems that the estimates are sensitive to both what private school share measure one uses and the time-period analyzed, and that this explains why much larger estimates have been found in previous studies.

4.2.2. Potential influence from other components of the reform

As we mentioned in section 2.1., there were three key elements in the school reforms implemented in Sweden in the early 1990s. An implicit assumption in the estimations so far has been that the private school share estimates only are due to changes following the private-school reform in 1992 (or that the other changes are sufficiently controlled for in our estimations by the inclusion of observable variables, municipal fixed effects or municipality-specific or pre-reform trends). However, if this is not the case, our estimates might reflect the influence from choice among public schools and the decentralization of school funding (i.e., the other features of the school reforms in the early 1990s).[35]

We therefore estimate model 7 with added variables capturing public school competition and school finance decentralization (see section 3.3.), even though they are probably simultaneously

[35] Söderström (2006) study the impact on achievement of an admission selection reform in Stockholm high schools in 2000. This reform increased school choice opportunities among Stockholm public high schools by making admission to be solely based on grades, whereas prior to the reform students had priority to the school located closest to where they live. This reform encouraged competition among public high schools. The finding is that this reform did not have a positive impact on achievement.

determined with the private school share.[36] These estimates are shown in Table 10. The baseline private school share estimate is shown in column 1, but here we have only included municipalities for which we have information on the additional control variables. In column 2, we have added the two public school choice measures (number of public schools per 100 pupils and average log distance to nearest public school). None of them are statistically significant, so increased availability of public school choice does not seem to be associated with better pupil achievement. The private school share estimate is also unaffected. In column 3, we have added the two school financing measures (pupil-teacher and pupil-certified teacher ratios). The pupil-teacher ratio is statistically significant, but the private school share estimate is again unaffected. Hence, we find no evidence that our estimates of the private school share effects are influenced by other components of reforms in the early 1990s.

5. Separating private-attendance effects from competition effects

5.1. Basic results

We now turn to the challenging issue of separating the effect for the individual of attending a private school from the effect of other individuals' private school attendance. We have shown that the sum of these two effects equals the total effect of private schooling. This total effect is estimated to be about 9.5 percentile rank points in our baseline specification 7 for 1993-2003,

[36] If increased private school enrollment causes the degree of public school competition to change, this would not be problematic, since public school competition would be one of the mechanisms linking private school choice and achievement. However, suppose that the reform generated more public school competition, and that this led to increased (since it signals willingness to actively choose schools in general) or decreased (since there already are many schools to choose from) incentives to start private schools nearby. There is then a risk that our private school estimate captures public school competition effects as well.

which are the years we focus on here (i.e, T=1 in specification 7). For reference, this estimate is shown in column 1 of Table 11.

We formulate the following empirical model of the impact of private school attendance on individual pupil performance:

$$(9) \qquad y_{imt} = c + \beta_1 P_{imt} + \beta_2 \overline{P}_{mt} + \lambda_1 X_{1,imt} + \lambda_2 \overline{X_{1,mt}} + \varphi X_{2,mt} + \gamma_m + \alpha_t + \varepsilon_{imt},$$

where y_{imt} denotes GPA for pupil *i* residing in municipality (or schooling market) *m* at time *t*; P_{imt} is an indicator for whether the school that the individual attends in the ninth grade is private (P=1) or public (P=0); $X_{1,imt}$ represents observable factors measured at the individual level; $\overline{X_{1tm}}$ and $X_{2,mt}$ denotes observable factors measured at the municipality level; and ε_{imt} is a random error term. The private-school variables and fixed effects are the same as in the aggregate specification 7. We allow the regression error to correlate between individuals in the same municipality. Given that we sufficiently control for all confounding factors and that private and public school pupils are affected equally from an increase in the private school share, β_1 represents the private-attendance effect and β_2 the competition effect.

In column 2 of Table 11, we report estimates from the regression of specification 9. We can see that the estimates of the private-attendance effect and the competition effect are 3.7 and 5.8 percentile rank points respectively. Hence, about 60 percent of the total benefit of private schooling is found to be due to competition effects in these naïve estimations.[37] In column 3, we

[37] The reason that the sum of the estimates in column 2 is slightly higher then the estimate in column 1 is that a few (about 200) individuals with missing values on some variables, are anyway included in the individual estimations. Indicator variables for missing values are therefore included in the estimations. However, when we aggregate these variables at the municipality level, we take averages for only positive values, and then include the fraction of individuals with missing values as controls. If we run model (1) and (2) with identical variables, we get identical results, i.e., we get $\hat{\beta} = \widehat{\beta_1} + \widehat{\beta_2}$.

show the estimate from specification 9 where we only include individual characteristics (as well as year indicators). This gives a very similar private-attendance estimate.[38] [39]

As indicated above, these specifications assume that we have been able to control perfectly for all relevant individual factors. This seems like a strong assumption given factors such as ability and motivation, which probably are correlated with both private school attendance and GPA.

5.2. Siblings estimations

To get an unbiased estimate of β_1, we turn to additional information and estimations. We therefore estimate a variant of equation 9 using siblings. We believe this approach to more accurately control for unobserved ability. We also believe that sibling estimations in the setting of this paper potentially are superior to most other settings since the within-family variation is combined with an exogenous increase in the availability of private schooling. An example might illustrate how the ideal setting would work: Suppose there are two families with two children each. In one family, the older sibling started at public school in a municipality without private schooling, whereas the reform made it possible for a private school to start in time for the younger sibling's school start. Hence, the main reason why one sibling attends a public school and the other sibling a private one is that private schooling suddenly became accessible. The children of the other family instead grow up in a municipality where (for some reason) no private school was ever started in time for either of them to attend it. As an estimate of the effect of

[38] The estimate is very similar if we also include a full set of interactions between municipality and year indicators.
[39] In one of the three previous Swedish studies, Ahlin (2005) analyze the private school attendance effect on ninth grade test scores in math and English, using more than 6,000 individuals leaving compulsory school in 1998. Controlling for individual and family background characteristics and test scores from the sixth grade, she finds that private school attendance increases the math score by 5.0 percentile ranks and the English score by 2.2 percentile ranks. However, only the math score estimate is statistically significant.

private school attendance, a difference-in-differences estimator would then compare the difference in GPA between the siblings in the first family with the difference in GPA between the siblings in the second family. In reality, we run sibling estimations, where the variation in school choice between siblings probably is a combination of endogenous choice and exogenous forces.

With sibling fixed effects we can formulate a model for sibling *i* in family *j* in municipality *m* leaving school in year *t* as:

$$(10) \qquad y_{ijmt} = c + \beta_1 P_{ijmt} + \lambda X_{ijmt} + \delta_j + \gamma_m + \alpha_t + \varepsilon_{ijmt}$$

where X_{ijmt} includes individual characteristics and; δ_j represents family fixed effects. Since we have included municipal fixed effects in this model we are able to control for endogenous within-family mobility between municipalities. The private school coefficient β_1 measures the effect of attending a private school for an individual whose sibling went to a public school (or vice versa).

When we estimate model 10, we only include families with at least two full biological siblings for whom we can observe grades. Results are reported in columns 4 and 5 in Table 11. In column 4, we report the estimate of the same specification underlying the estimate in column 3 (i.e., cross-section). We see that excluding only-children families has no effect on the private school attendance estimate. In column 5, we estimate model 10 where we include family fixed effects. Hence, we only use the variation in the data that is due to differences between siblings, within families. The private-attendance effect is now estimated to about 1.2 percentile point ranks and is statistically significant. This private-attendance estimate is only a very small part (about 13 percent) of the estimated total effect. Hence, we conclude that most of the achievement gain from private schooling is due to competition effects.[40]

[40] We also experimented with using log distance to nearest private school (controlling for log distance to nearest public school) as an instrument for private-school attendance (as in Cullen, Jacob and Levitt, 2005). However, the standard errors were much too large for the (negative) estimates to be informative.

6. Effects on school costs and segregation

6.1. School costs

When we look at reform effects, we are ultimately interested in the effects on school productivity, i.e., output per money unit spent. So far we have only looked at output (the numerator). Now we look at costs (the denominator in the school productivity measure). The sign of an estimate of the effect of the private school share on school costs is ambiguous. On the one hand, increased competition can force schools to operate more efficiently and thereby lower their costs. On the other hand, if schools that loose many pupils do not close down, costs will increase, since each school has some fixed costs, e.g. for buildings, that are independent of the number of pupils attending them. It is, for example, possible that local governments for various reasons (e.g., ideological) provide additional resources to threatened public schools, leading to an over-capacity in the school sector. It is also possible that local authorities who are in favor of school choice and competition invest additional recourses in all schools in order to stimulate a fair competition among them. Another reason for expecting higher costs is that school competition might induce competition for the best teachers, which in turn might push up the overall wage level among teachers.

In order to examine the impact of private schooling on costs, we estimate the following model:

$$(11) \qquad \log[Costs / pupil]_{mt} = \kappa_0 + \kappa_1 \overline{P}_{mt} + \kappa_2 \overline{X_{1,mt}} + \kappa_3 X_{2,mt} + \gamma_m + \alpha_t + \overline{\varepsilon}_{mt}$$

where the outcome measure is total school costs per pupil residing in the municipality (the information about school costs comes from the National Agency of Education), and other notations are as before. This information is based on all pupils and schools from first to ninth

grade (available for 1993-2003). Therefore, we use the comparable measure of private school share in grades 1-9 in these regressions. The school cost measure includes all types of school costs for a pupil residing in a municipality.

Results are reported in Table 12. The models in all columns include controls for year indicators and the same observable characteristics as in earlier estimations. In column 1, we estimate a cross-section model and find no statistically significant effect of the private school share on school costs. Comparing a cross-section of municipalities is, however, not a credible strategy if there are unobserved, time-invariant municipal factors related to both the private school share and school costs. Therefore, we turn to municipal fixed effects estimations. We then find a positive and statistically significant effect that is reported in column 2.[41] This is our baseline estimate in this cost analysis.

Next, we want to learn something about the mechanisms behind this positive effect on costs. In the remaining two columns of table 12, we report estimates from regression in which we have included controls for the number of schools in the municipality and teacher-pupil ratios respectively. Since the estimate reported in column 3 is unchanged compared to our baseline one, it appears that the cost effect is not due to the cost of opening more schools. On the other hand, the costs of more teachers appear to cut down the estimate to half (implying that costs for teachers make up a large part of the increased costs from more private schooling). Nevertheless, we conclude that there is evidence of a positive effect of private schooling on school costs.[42]

[41] Björklund et al. (2005) estimate the relationship between the change in total school costs per student and the change in private school share in grades 1-9 between the years 1992 and 2001, controlling for some covariates. They find a positive but statistically insignificant effect. Their estimate is 0.14, with a standard error of 0.72. The magnitude of the estimate in the present paper is therefore in line with their estimate.

[42] If we add linear trends interacted with municipal indicators we also find statistically insignificant small effects. However, the standard error more than doubles compared to the ones reported in columns 2-4 of Table 12. The reason is probably that there is very little cost-variation left when incorporating such detailed controls. Hence, we tend to prefer the estimates without such interactions.

Comparing the achievement and cost results, we find that an increase in the private school share by 10 percentage points would generate 1 percentile rank points higher achievement on average and 2 percent higher school costs. Compared with most estimates of resource effects in the literature, this seems like a very high return to an additional SEK (or dollar) spent.[43]

6.2. Segregation

When we look at the effects of the private school share on segregation, we estimate the following regression:

$$(12) \quad \frac{\overline{X}_{pub,mt}}{\overline{X}_{mt}} = \delta_0 + \delta_1 \overline{P}_{mt} + \gamma_m + \alpha_t + \overline{\varepsilon}_{mt},$$

where $\frac{\overline{X}_{pub,mt}}{\overline{X}_{mt}}$ is the ratio of the average characteristic (or 'quality') of public school pupils in municipality *m* to the average characteristics of all pupils in a municipality, and other notations are as before. The coefficient of interest, δ_1, measures whether the average characteristics of public school pupils in a given municipality (relative to the average characteristics of all pupils in the municipality) change by more in a municipality where the school choice program resulted in more private schooling. If δ_1 is negative (using a characteristic indicating pupils performing well), this suggests that school choice caused the public schools to lose their best pupils. We use the following characteristics to measure the 'quality' of the pupil pool; parents' income, parents'

[43] For example, Swedish studies of the effect of school resources (in terms of smaller classes) on pupil achievement have found that more resources improve pupil achievement (Lindahl, 2005, and Björklund et al., 2005). The magnitudes of the estimated effects in these studies amount to a 2 percent increase in costs improving pupil achievement with about 0.2 percentile rank points.

education, immigrant parents, and immigrant pupil (all known to be highly correlated with pupil performance).

Results are reported in Table 13. Considering the municipal fixed effects models as our baseline models, we find no evidence of sorting of pupils by parental income and pupil's first-generation immigrant status. We do, however, find that more private schooling makes public schools lose pupils who are second-generation immigrants and/or whose parents have high education. If we add controls for a linear trend interacted with municipality indicators we obtain very similar estimates. Hence, there is some evidence that an effect of increased school choice in Sweden, following the 1992 school reform, is to induce greater segregation of pupils by parental education and second-generation immigration status.[44] Whether such segregation is good or bad, however, is an open question.[45]

7. Conclusions

We have estimated the impact of choice between public and private schools in Sweden, using a large administrative data set on individuals graduating from compulsory school in 1988-2003. We utilized a school reform implemented in Sweden in 1992 that significantly increased the possibility for Swedish families to choose between different school types.

The basic finding is that the effect on average grades of a 10 percentage point increase in the private school share is just below 1 percentile rank point. This effect is statistically significant,

[44] Söderström and Uusitalo (2005) study the impact on segregation of the admission selection reform increasing school choice opportunities for students applying to Stockholm high schools in 2000. The reform is found to increase segregation by ability, family background and between natives and immigrants.

[45] See, for example, Cutler and Glaeser (1997) who formulate a model of racial segregation. In their model, increased segregation can have a positive as well as negative effect on the welfare of black people in the US, depending on the segregation by skill within the black community.

very stable across specifications and does not seem to be driven by differential grade-setting standards in private and public schools. We do find some evidence of differential effects across sub-groups defined by socioeconomic characteristics and immigrant status, but no group is found to loose from a higher private school share. The individual gain from attending a private school (the private-attendance effect) is estimated to be only a small part of the total effect, about 0.1 percentile rank point. Thus, the total achievement effect is mainly driven by other peoples' choice of private school in the municipality. We interpret this as evidence of competition effects where more school competition forces all schools to improve. Moreover, we find some segregation effects from a higher fraction of private schooling in the municipality. Children of parents with long education and/or immigrant background are more likely to be sorted into private schools, when the private school share increases. We also find that an increase in the private school share by 10 percentage points generates about 2 percent higher average municipal school costs.

Overall, we thus find evidence that the competitive forces unleashed by the 1992 school reform in Sweden induced higher pupil achievement, but also higher costs and greater segregation. The question of whether more or less choice is warranted is therefore not a straightforward one. However, in the light of previous findings in the literature studying resource effects, we consider the comparison of our estimated achievement and cost effects as an indication of very high returns to an additional SEK (or dollar) spent. It is more complicated, and ultimately a question of values, to contrast the results for achievement with those for segregation

The results in this paper are for the compulsory-school level only. We will also study medium- and long-term consequences of choice at this level (high-school outcomes and educational attainment). We will further study the impact of private school choice at the high-school level, where the enrolment in private schools has increased even more rapidly. Studying the high-

school level allows us to control for students' choices and achievements at the compulsory level. This work is currently in progress.

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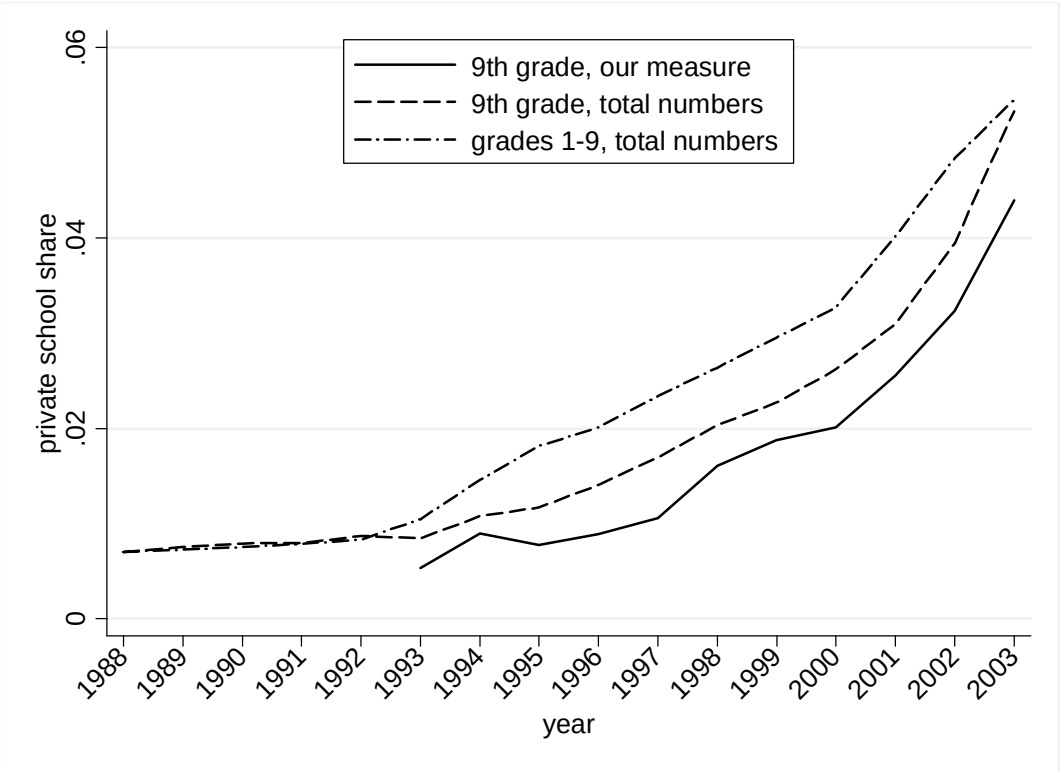
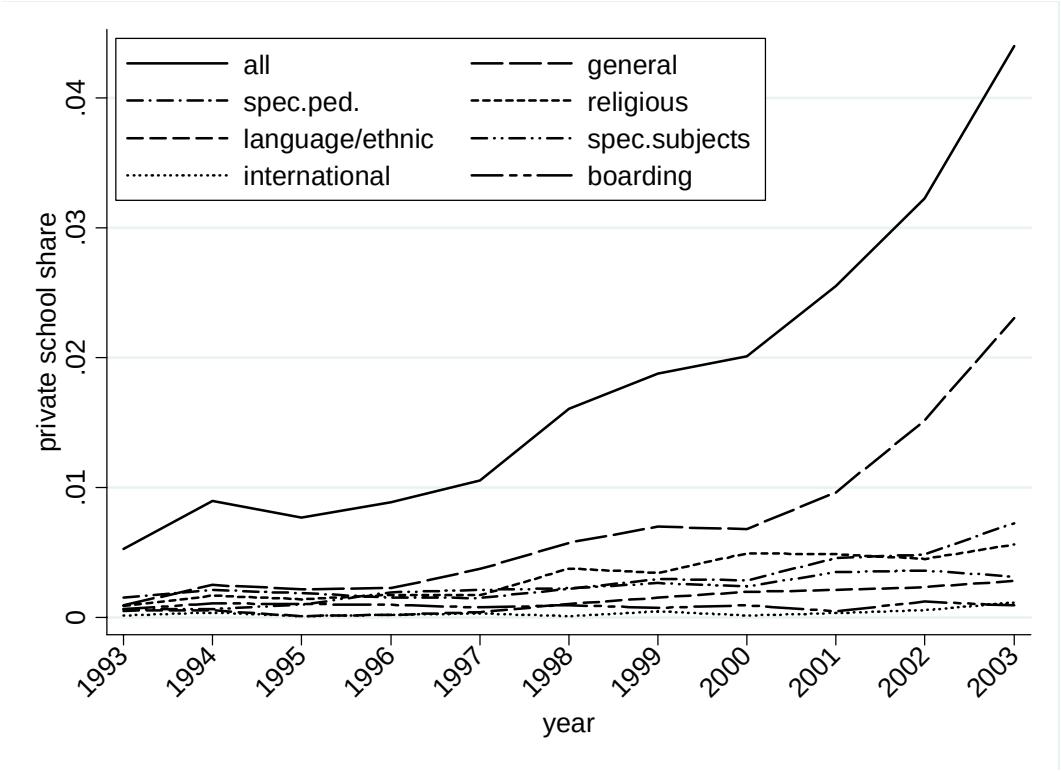
Figure 1 **The private school share 1988-2003**



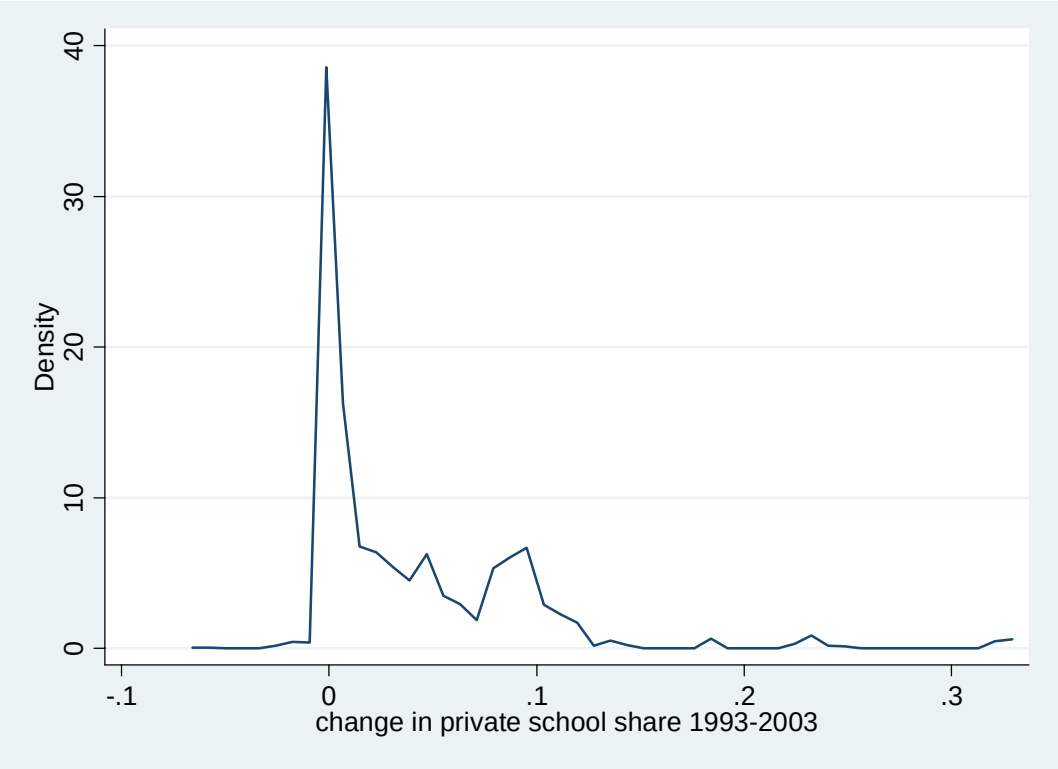
Notes: The line denoted "9th grade, our measure" is based on data from the ninth-grade register (*årskurs 9 registret*) and the residential registers. It is a sample measure of the private school share in municipalities, based only on pupils residing in the municipality (and attending a school either inside or outside municipal borders) who have received any grade from the ninth grade. The lines denoted "9th grade, total numbers" and "grades 1-9, total numbers" are based on information from the school register (*skolregistret*), i.e., on total numbers for the whole student population in grade 9 and in grades 1-9, respectively, and there is no restriction on whether the pupils have received any grade or not. The shares, in these two latter measures, are calculated for pupils attending school within the municipality.

Figure 2 **The private school share 1993-2003, by type**



Note: Based on data from the ninth grade register (*årskurs 9 registret*), the school register (*skolregistret*) and the residential registers.

Figure 3 **Kernel density estimation of the change in the private school share between 1993 and 2003 (weighted)**



Notes: The graph is from kernel density estimation of the change in the share of private schooling in municipalities (weighted) between 1993 and 2003. It is based on data from ninth-grade and residential registers, and refers only to pupils residing in the municipality (and attending a school inside or outside municipal borders) who had received any grade from the ninth grade.

Table 1 **Descriptive estimations**

	GPA (percentile ranks)						Private school share					
Years	The Level of Private Schooling in 2002/2003				Differences		The Level of Private Schooling in 2002/2003				Differences	
	High	Med.	Low	Zero	Hi-Lo	Hi-Ze	High	Med.	Low	Zero	Hi-Lo	Hi-Ze
Post-reform												
2002/2003	52.26	50.62	48.78	49.12	3.48 (1.09)	3.13 (0.96)	0.118	0.045	0.012	0.000	0.105 (0.008)	0.118 (0.007)
Pre-reform												
1991/1992	50.67	50.68	49.20	49.03	1.47 (0.82)	1.64 (0.65)	0	0	0	0	0	0
1988/1989	50.64	51.30	49.17	48.81	1.46 (0.73)	1.83 (0.57)	0	0	0	0	0	0
Differences												
Change, 91/92-02/03	1.59 (0.68)	-0.06 (0.54)	-0.42 (0.37)	0.10 (0.31)	2.01 (0.77)	1.49 (0.74)	0.118	0.045	0.012	0.000	0.105 (0.008)	0.118 (0.007)
Change, 88/89-91/92	0.03 (0.38)	-0.62 (0.42)	0.03 (0.32)	0.22 (0.29)	0.00 (0.49)	-0.18 (0.47)	0	0	0	0	0	0

Notes: All estimates are weighted, where the weights are the number of ninth grade pupils residing in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 2 **Descriptive statistics**

	Individual level (1993-2003)		Aggregate level (weighted) (1988-2003)
	(1) Private school pupils Mean (St.dev)	(2) Public school pupils Mean (St.dev)	(3) All pupils Mean (St.dev)
GPA	58.62	49.74	49.90
	(22.96)	(22.97)	(3.96)
Private school=1			0.018
			(0.034)
FAMILY AND DEMOGRAPHIC VARIABLES			
Male=1	0.48	0.51	0.51
	(0.50)	(0.50)	(0.06)
Parents' mean age	46.89	45.41	44.99
	(5.35)	(5.06)	(1.11)
At least one parent university educated=1	0.46	0.25	0.24
	(0.50)	(0.43)	(0.10)
At least one parent high school educated=1	0.49	0.43	0.41
	(0.50)	(0.49)	(0.08)
Log family income	8.10	8.00	7.99
	(0.65)	(0.51)	(0.12)
Log family income is missing	0.05	0.04	0.03
	(0.22)	(0.20)	(0.03)
2nd generation immigrant	0.15	0.06	0.06
	(0.36)	(0.24)	(0.05)
Immigrant	0.09	0.07	0.06
	(0.28)	(0.25)	(0.05)
Immigrant age (if immigrant=1)	6.56	8.04	6.81
	(4.11)	(4.00)	(3.80)
SCHOOL LEVEL VARIABLES			
Nr of 9^{th} grade pupils / 100			1.36
			(0.55)
Nr of public schools per 100 pupils (1993-2003)			1.47
			(0.62)
Mean log distance to pupils' nearest public school			7.28
(1993-2003, 3,028 municipalities)			(0.64)
Nr of Pupils per teacher in the municipality			12.63
(1993-2003)			(0.98)
Nr of Pupils per certified teacher in the municipality			14.44
(1993-2003)			(1.72)
Log school costs per pupil in the municipality			10.91
(1993-2003, 3,086 municipalities)			(0.15)

Notes: GPA is the average (percentile) grades in English, math, natural science and social science subjects. The summary statistics for municipality-level variables are calculated for 11 years and for all municipalities. The number of observations for most variables is 3,811 for private school pupils, and 209,801 for public school pupils. Exceptions are university and high school education (3,801 for private and 209,391 for public school pupils), family income (3,622 and 200,680) and immigrant age (333 and 13,964). The number of municipality-year cells is 3,124 in columns 1 and 2. The number of municipality-year cells is 4,544 in column 3.

Table 3 **Aggregate private school share regressions, 1993-2003**

Dependent Variable: Private school share in the municipality	(1)	(2)	(3)	(4)
Fraction male pupils	-0.008	0.001	-0.008	-0.000
	(0.009)	(0.008)	(0.009)	(0.008)
Mean parents' age	0.001	-0.000	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Fraction families with at least one parent university education	0.048	0.029	0.057	0.028
	(0.026)+	(0.011)**	(0.023)*	(0.011)*
Fraction families with at least one parent high school education	0.014	0.005	0.018	0.006
	(0.013)	(0.007)	(0.012)	(0.007)
Mean log family income, only those with pos income for any parent	0.021	-0.016	0.013	-0.012
	(0.016)	(0.010)	(0.014)	(0.009)
Fraction of individuals with missing info on parents income	0.049	0.034	0.052	0.026
	(0.060)	(0.038)	(0.058)	(0.036)
Fraction 2nd generation immigrants	0.097	0.048	0.070	0.047
	(0.042)*	(0.028)+	(0.042)+	(0.025)+
Fraction immigrants	-0.010	-0.049	-0.008	-0.039
	(0.030)	(0.029)+	(0.031)	(0.025)
Nr of 9^{th} grade pupils / 100	0.003	0.016	0.003	0.016
	(0.001)*	(0.003)**	(0.002)	(0.003)**
Nr of public schools per 100 pupils			0.000	-0.000
			(0.002)	(0.001)
Mean log distance to pupils' nearest public school			-0.001	-0.003
			(0.002)	(0.002)
Nr of pupils per teacher in the municipality			-0.003	-0.006
			(0.002)	(0.003)*
Nr of pupils per certified teacher in the municipality			0.005	0.007
			(0.002)*	(0.002)**
Municipal fixed effects	NO	YES	NO	YES
Observations	3,124	3,124	3,028	3,028
R-squared	0.37	0.67	0.39	0.69

Notes: All regressions control for year fixed effects. All estimates are weighted, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 4 **Aggregate GPA regressions, 1988-2003**

Dependent Variable: Average GPA in the municipality							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Private school share	28.37	3.76	10.60	9.24		9.52	9.33
	(7.23)**	(3.74)	(4.44)*	(3.31)**		(2.75)**	(3.23)**
Private school share 1993-1995					8.85		
					(5.77)		
Private school share 1996-1999					9.70		
					(4.96)+		
Private school share 2000-2003					9.16		
					(3.45)**		
Municipal controls[a]	NO	YES	NO	YES	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	NO	YES	NO
Municipal pre-reform trends[b]	NO	NO	NO	NO	NO	NO	YES
Observations	4,544	4,544	4,544	4,544	4,544	4,544	4,544
R-squared	0.04	0.44	0.48	0.57	0.57	0.63	0.58

Notes: All regressions control for year fixed effects. [a]The time varying municipality controls are the family and demographic variables listed in Table 2. In column (5), the explanatory variable of interest is divided into 3 variables for 3 different time-periods. For example, PSS 93-95 is the private school share for the years 1993-1995 and zero for all other years. [b]We use data for the years 1988-1992 to predict a trend in GPA for each municipality for 1993-2003. This pre-reform trend variable is then included as control in the estimations. All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 5 **Aggregate GPA regressions, 1988-2003, quadratic specification**

Dependent Variable: Average GPA in the municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Private school share	38.09	-1.32	11.00	7.08	8.91	8.34
	(12.36)**	(6.26)	(7.65)	(5.82)	(5.21)+	(5.78)
Private school share squared	-75.76	36.03	-2.75	14.10	3.64	6.47
	(61.80)	(28.77)	(37.18)	(25.24)	(28.72)	(25.23)
Municipal controls[a]	NO	YES	NO	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	YES	NO
Municipal pre-reform trends[b]	NO	NO	NO	NO	NO	YES
Observations	4,544	4,544	4,544	4,544	4,544	4,544
R-squared	0.04	0.44	0.48	0.57	0.63	0.58

Notes: All regressions control for year fixed effects. [a]The time varying municipality controls are the family and demographic variables listed in Table 2. [b]We use data for the years 1988-1992 to predict a trend in GPA for each municipality for 1993-2003. This pre-reform trend variable is then included as control in the estimations. All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 6 **Aggregate GPA regressions for different sub-samples, 1988-2003**

Dependent Variable: Average GPA in the municipality for pupils in the resp. sub-group	(1)	(2)	(3)	(4)	(5)	(6)
Private school share	9.24	8.27	3.47	15.15	13.21	11.25
	(3.31)**	(5.70)	(4.05)	(4.55)**	(4.60)**	(3.47)**
Municipal controls[a]	YES	YES	YES	YES	YES	YES
Municipal fixed effects	YES	YES	YES	YES	YES	YES
Sub-group	All	1st or 2nd generation immigrants	Low educated parents	High educated parents	Low family income	High family income
Observations	4,544	3,713	4,542	4,419	4,526	4,375
R-squared	0.57	0.30	0.23	0.22	0.23	0.28

Notes: All regressions control for year fixed effects. [a]The time varying municipality controls are the family and demographic variables listed in Table 2. The dependent variables are calculated as the average GPA for pupils belonging to the respective sub-sample. The sub-samples include, respectively, pupils: who are first and second generation immigrants (column 2); who's parents' have less than high school education (3); who have at least one parent having university education (4); who's family's income being in the first quartile (4); who's family's income being in the fourth quartile (5). The sample sizes differ between columns since not all sub-groups are represented in all municipality-year cells. All are weighted regressions, where the weights are the number of ninth grade pupils belonging to the specific sample living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 7 **Aggregate level grade regressions using various subject measures, 1988-2003**

Dependent variable: Average subject grade in the municipality					
	(1)	(2)	(3)	(4)	(5)
Subject grade:	GPA	Math	English	Soc. Science	Science
Private-school share	9.24	10.41	9.21	5.54	9.07
	(3.31)**	(3.79)**	(3.42)**	(4.10)	(3.47)**
Municipal controls[a]	YES	YES	YES	YES	YES
Municipal fixed effects	YES	YES	YES	YES	YES
Observations	4,544	4,544	4,544	4,544	4,544
R-squared	0.57	0.48	0.58	0.50	0.45

Notes: All regressions control for year fixed effects. [a]The time varying municipality controls are the family and demographic variables listed in Table 2. All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 8a **Individual "grade inflation" regressions, 2003**

Dependent variable: Subject grade for pupil				
	(1)	(2)	(3)	(4)
Subject grade:	English	English	Math	Math
Private school=1	-1.59	-1.90	0.48	-0.16
	(0.94)+	(0.94)*	(1.00)	(0.96)
English test score	0.95	0.93		
	(0.00)**	(0.01)**		
Math test score			0.77	0.75
			(0.01)**	(0.01)**
Individual controls[a]	NO	YES	NO	YES
Observations	16,943	16,943	11,598	11,598
R-squared	0.746	0.751	0.646	0.655

Notes: [a]Individual-level controls are the family and demographic variables listed in Table 2. The standard errors (in parentheses) allow for clustering at the school level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 8b **Aggregate "grade inflation" regressions, 2003**

Dependent variable: Aggregate subject grade				
	(1)	(2)	(3)	(4)
	English	English	Math	Math
Private school share	-1.78	1.63	-1.00	-0.94
	(2.18)	(2.17)	(2.71)	(3.38)
Mean of English test score	0.89	0.89		
	(0.02)**	(0.04)**		
Mean of Math test score			0.72	0.70
			(0.04)**	(0.04)**
Municipal Controls[a]	NO	YES	NO	YES
Observations	277	277	265	265
R-squared	0.80	0.82	0.64	0.68

Notes: [a]Time varying municipality controls are the family and demographic variables listed in Table 2. All are weighted regressions, where the weights are the number of ninth grade pupils (with observable subject grade and test score) living in the municipality. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 9 **Aggregate GPA regressions using various measures of the private school share, 1993-2003**

Dependent variable: Average GPA in the municipality							
Private school share is	(1)	(2)	(3)	(4)	(5)	(6)	(7)
- measured at :	Grade 9	Grade 9	Grade 9	Grade 7-9	Grade 7-9	Grade 1-9	Grade 1-9
- baseline or alt. :	Baseline	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.
- average of 3 years :	NO	NO	YES	NO	YES	NO	YES
Private school share	9.50	7.16	7.80	7.25	11.93	13.17	17.24
	(2.67)**	(2.63)**	(3.64)*	(2.57)**	(3.94)**	(3.72)**	(4.83)**
Municipal controls[a]	YES	YES	YES	YES	YES	YES	YES
Municipal fixed effects	YES	YES	YES	YES	YES	YES	YES
Observations	3,124	3,124	3,124	3,124	3,124	3,124	3,124
R-squared	0.61	0.61	0.61	0.61	0.61	0.61	0.61

Notes: All regressions control for year fixed effects. [a]The time varying municipality controls are the family and demographic variables listed in Table 2. The baseline private school share measure is based on the grade 9 pupils who reside in the municipality and who belong to our sample (the one used in our main estimations). The alternative private school share measures are based on total population numbers for pupils attending school in the municipality. The "average of 3 years"- measures are calculated as the average of the private school share in the last three years (year *t*, *t*-1 and *t*-2). All are weighted regressions, where the weights are the number of ninth grade pupils living in (column 1) or attending school in the municipality (columns 2-7). The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 10 **Aggregate GPA-regressions including proxies for competition between public schools and controls for school resources, 1993-2003**

Dependent variable: Average GPA in the municipality			
	(1)	(2)	(3)
Private-school share	9.32	9.32	9.15
	(2.66)**	(2.67)**	(2.76)**
Nr of public schools / 100 pupils		-0.08	-0.07
		(0.15)	(0.15)
Mean log distance to pupils' nearest public school		-0.01	-0.03
		(0.21)	(0.21)
Nr of Pupils per teacher in the municipality			0.33
			(0.16)*
Nr of Pupils per certified teacher in the municipality			-0.02
			(0.12)
Municipal fixed effects	YES	YES	YES
Municipal controls[a]	YES	YES	YES
Observations	3,028	3,028	3,028
R-squared	0.62	0.62	0.62

Notes: All regressions control for year fixed effects. [a]Time varying municipality controls are the family and demographic variables listed in Table 2. Proxies for the competition between public schools are: "Mean log distance for pupils to nearest public school in the municipality in year *t*" and "number of public schools per 100 pupils in the municipality in year *t*". Controls for school resources are: "number of pupils per full time equivalent teacher in the municipality in year *t*" and "number of pupils per full time equivalent certified teacher in the municipality in year *t*". All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 11 **Individual GPA regressions, estimating the private-attendance effect**

Dependent variable: GPA for pupil (Average GPA in the municipality in column (1))					
	(1)	(2)	(3)	(4)	(5)
Private school share	9.50	5.84			
	(2.67)**	(2.40)*			
Private school=1		3.73	3.93	4.04	1.22
		(0.69)**	(0.71)**	(0.68)**	(0.53)*
Municipal controls[a]	YES	YES	NO	NO	NO
Municipal fixed effects	YES	YES	YES	YES	YES
Individual controls[b]	-	YES	YES	YES	YES
Sibling fixed effects:	-	NO	NO	NO	YES
Sample:	Random (aggregate)	Random (individual)	Random (individual)	Sibling	Sibling
Observations	3,124	213,612	213,612	210,733	210,733
R-squared	0.61	0.22	0.22	0.23	0.73

Notes: All regressions control for year fixed effects. [a]Time varying municipality controls are the family and demographic variables listed in Table 2. [a]Individual controls are the family and demographic variables listed in table 2. The estimate reported in column 1 is from a weighted regression, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 12 **Aggregate cost per pupil regressions**

Dependent variable: The logarithm of school costs per pupil in the municipality				
	(1)	(2)	(3)	(4)
Private school share, grades 1-9 alt. measure	0.14	0.23	0.21	0.13
	(0.14)	(0.13)+	(0.13)	(0.13)
Municipal controls[a]	YES	YES	YES	YES
Municipal fixed effects	NO	YES	YES	YES
Control for number of schools	NO	NO	YES	YES
Control for pupil-teacher ratios	NO	NO	NO	YES
Observations	3,086	3,086	3,086	3,086
R-squared	0.65	0.89	0.89	0.90

Notes: All regressions control for year fixed effects. [a]Time varying municipality controls are the family and demographic variables listed in Table 2. All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 13 **Aggregate sorting regressions with respect to socioeconomic background and immigration status**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Income	Income	Education	Education	Immigrants	Immigrants	2nd generation immigrants	2nd generation immigrants
Private school share	-0.011	-0.005	-0.062	-0.029	0.182	0.111	-0.433	-0.358
	(0.005)*	(0.006)	(0.015)**	(0.011)**	(0.113)	(0.163)	(0.103)**	(0.150)*
Municipal fixed effects	NO	YES	NO	YES	NO	YES	NO	YES
Observations	3,124	3,124	3,124	3,124	2,243	2,243	2,017	2,017
R-squared	0.11	0.34	0.36	0.62	0.02	0.18	0.06	0.23

Notes: All regressions control for year fixed effects. The dependent variables are defined as the ratio of the average characteristics of the public school pupils in the municipality divided by the average characteristics of all pupils in the municipality. The characteristics are: the logarithm of family income (columns 1 and 2); parental-mean years of schooling (3 and 4); first generation immigrant status (5 and 6); second generation immigrant status (7 and 8). The mean (standard deviation) of the dependent variables are: 0.9999 (0.0011) in columns 1-2; 0.9988 (0.0034) in columns 3-4; 1.0023 (0.0496) in columns 5-6; and 0.9878 (0.0656) in columns 7-8. All are weighted regressions, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Appendix table 1 **Descriptive estimations for the private school share, using an alternative private school share measure**

	Private school share, all pupils available					
Years	The Level of Private Schooling in 2002/2003				Differences	
	High	Med.	Low	Zero	Hi-Lo	High
Post-reform						
2002/2003	0.134	0.057	0.016	0.003	0.118	0.131
					(0.011)	(0.010)
Pre-reform						
1991/1992	0.029	0.006	0.003	0.002	0.026	0.027
					(0.013)	(0.013)
1988/1989	0.014	0.002	0.001	0.001	0.013	0.013
					(0.007)	(0.007)
Differences						
Change, 91/92-02/03	0.105	0.051	0.013	0.001	0.092	0.104
	(0.012)	(0.004)	(0.002)	(0.001)	(0.012)	(0.012)
Change, 88/89-91/92	0.015	0.004	0.002	0.001	0.013	0.014
	(0.006)	(0.001)	(0.001)	(0.001)	(0.006)	(0.006)

Notes: All estimates are weighted, where the weights are the number of ninth grade pupils living in the municipality. The standard errors (in parentheses) allow for clustering at the municipality level. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.