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**The Impact of School Choice on Educational Performance, Segregation and Costs:
Swedish Evidence**

Anders Böhlmark
Stockholm University

Mikael Lindahl
Uppsala University

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The Impact of School Choice on Educational Performance, Segregation and Costs: Swedish Evidence^{*}

Anders Böhlmark
Swedish Institute for Social Research
Stockholm University
anders.bohlmark@sofi.su.se

Mikael Lindahl
Department of Economics
Uppsala University, IFAU and IZA
mikael.lindahl@nek.uu.se

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Abstract

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 short-, medium- and long-term educational outcomes 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 induces improved short- and medium-term educational outcomes such as average GPA and the fraction of students who chooses an academic high-school track. However, we do not find any impact on long-term educational outcomes such as university attainment and years of schooling. We investigate whether this difference is due to inflated grades in school markets where the reform has had a larger impact, but find no evidence of this being the case. We do not either find that private schooling at the high-school level confound with our estimates in a way that masks actual long-term effects. We further find that more competition from private schools increases overall school costs. There is also evidence of an increased sorting of pupils along socioeconomic and ethnic lines.

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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 of 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. Private schools might also be more innovative with regard to organization and pedagogical profile, with potential spillover effects to public schools. 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 high achieving classmates, involved parents and good teachers to the private sector. More generally, 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 (forthcoming) and Rothstein (forthcoming). For overviews of the school choice literature, see McEwan (2000) and Gill et al. (2007).

² Examples of such work are: Angrist et al. (2002, 2006) evaluating a private secondary school voucher experiment in Columbia; Rouse (1998) evaluating 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). Other studies have used quasi-experimental design to estimate individual effects of attending a preferred public schools, see Cullen, Jacob and Levitt (2006) who looks at choice among public high schools in Chicago.

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.

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.³ 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 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.⁴

This differential variation that developed after the reform is used to answer the causal questions whether a higher incidence of private school enrolment at the compulsory-school level impact: (i) overall pupil performance at the compulsory-school level? (ii) overall student performance at the high-school level? (iii) overall long-term educational performance? (iv) school costs? (v) segregation of pupils?

Even though the issue of choice and competition in the school sector is a highly active area of research, there is still much uncertainty about what the effects are, not the least since it is

³ 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.

⁴ Swedish private schools share some similarities with U.S. charter schools in that they are privately-run but publicly funded. However, a difference is that U.S. charter schools are not allowed to be run as for-profit businesses. 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, lottery-winners and lottery-losers can be compared. Note that they are not able to say anything about the effects of competition between charter and public schools.

empirically very challenging to estimate such effects. Researchers have estimated choice/competition effects on overall performance (and more scarcely on segregation) either by evaluating large-scale choice reforms or by using instrumental variable estimation of choice among school districts (markets) where choice already has been in effect.⁵

We adopt the first strategy, which implies that one needs aggregated data, where average outcomes are measured for both private and public school students, and also a varying degree of private schooling between regions. Preferably, the existence of private schooling should also be predated by a situation with little choice availability. The use of outcomes for both private and public school pupils was emphasized by Hsieh and Urquiola (2005) who argued that there is virtually impossible to separate the existence of performance effects from peer-effects (through sorting of pupils) when evaluating competition. A second challenge is that there are biases that tend to work in different directions in this setting. On the one hand, there are likely factors that are correlated with both the amount of private schooling and student outcomes that one needs to control for. One therefore needs very detailed background characteristics as well as panel data on school markets to take this into account. Omission of these factors probably leads to overstating the impact of private schooling on student outcomes. On the other hand, private schools might more likely open up in areas where public schools are of poor quality. This will instead tends to understate the impact of competition on student outcomes.

In this paper we estimate models where we control for a full set of year and municipality indicators, as well as a detailed set of family and demographic characteristics. Such

⁵ Examples of studies of school choice effects are: Hoxby (2000) and Urquiola (2005) estimating the effects of choice between school districts in the US (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. A similar school reform as introduced Sweden was introduced in New Zealand around the same time and are perhaps most relevant to the Swedish setting. Fiske and Ladd (2000) present many consequences of this reform. 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 reform as perceived by teachers and principals.

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 utilizing several years of data prior to the implementation of the reform. We make several tests of the existence of differential pre-program trends across municipalities, and also explicitly investigate the potential endogeneity of private schooling, by associating pre-program variables with the growth rates in private schooling following the reform.

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 in compulsory school and in high school, as well as information on educational attainment at universities. We have also data on parental education and income as well as school and residential information. The richness of the data allows us to make several significant contributions to the literature.⁶ First, we have data on compulsory-school pupils for a time period of 16 years, which makes it possible to study effects by exploiting variation over a long time span after the reform as well as to test for differences in pre-reform trends between municipalities. Second, we are able to track these individuals over time (up to 2006), which makes it possible to also examine the impact on medium- and long-term educational outcomes. Third, the detailed demographic information and school data allows us to not only examine productivity gains from school-choice, but also to study segregation and costs within the same empirical framework.

The paper is organized as follows: Section 2 briefly describes the features of the Swedish compulsory schooling system, and the school reform in 1992 as well as its consequences for the evolution of private schooling in Sweden. Section 3 describes how the data set is constructed and considers what the determinants are of private school share changes across

⁶ For instance, the data allows us to significantly improve on the previous Swedish studies of private school effects: Ahlin (2003); Björklund et al. (2005); and Sandström and Bergström (2005).

municipalities. Section 4 discuss the identification strategy and reports the main results from estimations of the private school share on our measures of average educational achievement. Section 5 report results from a number of sensitivity analyses. In section 6, we study effects on school costs and segregation. Section 7 concludes.

2. Public and private schooling in Sweden

2.1. Sweden's school organization and the 1992 reform⁷

Before 1991, public schools were operated by local governments (municipalities), but school funding and control was largely centralized. Municipalities received earmarked funds from central government to cover the schools' operational costs. These funds covered the largest part of the school funding and determined teacher-pupil ratios. Municipalities had the opportunity to complement these funds by local income tax revenues to be used, for instance, on non-pedagogical staff. Teachers were state employees and directly paid by central government and schools had to follow a national curriculum.

In this old system pupils were assigned to, and had to attend, the public school in their local catchment area. Although private schools existed, these accounted for less than one percent of total enrollment.⁸ Most of these private schools were privately funded and some received state funding. However, importantly, the funding of public schools was independent of the number of pupils enrolled in private schools. Hence, these schools did not compete for resources with public schools. These private schools were not required to follow central guidelines.

⁷ For a more detailed description of the old school system and the reforms, see Ahlin and Mörk (forthcoming); Björklund et al. (2005).

⁸ 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).

There were three key elements in the school reforms implemented in Sweden in the early 1990s, and they are still in practice today. First, the financial responsibility for public schools was transferred from the state to municipalities in two steps, 1991 and 1993. In 1991, teachers became municipality employees and were no longer paid directly by central government. The system of central government grants to municipalities was replaced by a system providing municipalities with a grant to cover all school costs. In 1993, the system of school funding was again changed so that the central government was to provide municipalities with a general lump-sum grant that could be used also for other municipal services (such as social services and child care). This lump-sum grant constituted, on average, about 20 percent of the municipalities' total revenues during the first year of the reform (and municipalities naturally supplement these funds with own revenues). Hence, municipalities were given more flexibility in their priority over expenditures on education versus other municipal expenditures.

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 municipality. This choice is, however, conditional on slots being available after those residing closest to the school had made their choices. For this reason, choice between public schools has remained quite restrictive in practice even after this reform.⁹

Third, the most radical component of the school reforms was perhaps that in 1992 municipalities 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 residing in the municipality who choose to enroll in a private school.¹⁰ Municipalities could also choose to

⁹ Unfortunately, this is something we cannot check with our data. According to interviews in 2000/2001, municipal administrators stated that about 20 percent of municipalities have more than 5 percent of students, and only a few municipalities above 10 percent of students, in other than the original assigned public school (see page 73 in publication by the National Agency for Education, 2003)

¹⁰ The minimum required funding percentage has changed over the years. Starting with the school year 1992/1993, it was 85 percent, and in 1995, 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. In Juli 1st 1997, the system

provide additional resources. With this reform, ordinary pupils are 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 municipality can express opposition to an approved application to open a private school, but the number of applications rejected has been quite small.¹¹ Private schools are not allowed to charge any fees.¹² 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.¹³

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

Figure 1 shows the evolution of the number of public and private schools in Sweden since the reform. Note that the figures refer to the number of schools with students attending ninth grade, which are much smaller than the total number of compulsory schools in Sweden (currently slightly above 5000). The number of private schools were 42 the first year after the reform, but has increased sixth-fold ten years after the reform. Interestingly, the number of new private schools has been about equally matched with the number of new public schools

again changed: private school funding depended on school housing costs and was not strictly limited to a certain percent of costs for municipality schools. Since 1993, the goal has always been that public and private schools should be subject to about the same funding conditions.

¹¹ For instance, in 2000, there were 153 applications to start a private school at (any) compulsory school level the school year 2001/2002. Of these, 13 were rejected. Reasons were that the application was incomplete, that the school would not provide sufficient educational standard, or that the owner was financially instable. Of these 13 applications, 2 were denied because it was expected that starting this private school would lead to very negative effects for the public schools in the municipality (National Agency of Education, 2001).

¹² Private schools that received public funding were initially allowed to charge some fees, but these were heavily circumscribed. After 1997, private schools at the compulsory level are no longer allowed to charge any fees. However, private high schools in Sweden are still allowed to do so.

¹³ The ownership structure has changed over time. At the start of the reform period, the most common new private school owner was a non-profit organization (*Ideell förening*). This has changed over time, and for-profit corporations (*Aktiebolag*) are now most common.

during this period. This is surprising, especially since the number of ninth grade pupils has increased by only about 10 percent during this period. Apparently, there has been a trend of decreasing numbers of 9th-grade pupils per school.¹⁴

Figure 2 shows the evolution of private schooling at the compulsory level in Sweden from 1988 to 2003, using information from the National Agency of Education. The line shows the fraction of pupils who attended a private school in the ninth grade in a given year. We see that there was a small fraction of pupils, around 0.5 percent, who attended private schools before the reform in 1992. After the reform, there has been a sharp increase in the private school share. It has increased from less than 1 percent in 1993 to more than 5 percent in 2003. A comparison of the numbers in figures 1 and 2 further shows that the fraction of private schools always has been much higher than the fraction of private school pupils in Sweden. In general, private schools have smaller student populations than public schools.

In Figure 3, we divide the ninth-grade private school pupils into groups defined by the type of school attended. We show the development as the percentage of pupils in all private schools divided on special pedagogy schools (e.g., Montessori and Waldorf), general-profile schools and a category in which we group all other school types into one (denoted other). Hence, the lines sum vertically to 100 in all years. The category “other” includes pupils from language, international, religious, special subject and boarding schools as well as pupils from schools for students with special needs. Together with the special pedagogy schools, these were also the school types that existed on a small scale before the reform. The figure makes it clear that what happened after the reform was that private schools with a general profile entered the scene and that this school type, similar to the public schools in terms of educational profile, has increasingly gained market shares at the expense of, first of all, the school types in the “other” category. This implies that the largest increase in private schooling

¹⁴ A likely explanation for this is that many schools that previously only provided teaching at grades 1-6 (which is common) have extended their business with grades 7-9.

is driven by a new market for private schools that compete by other means than offering something that is distinctly different from what is generally available in public schools.¹⁵

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 93 out of 284), the average private school share was 9 percent. There were, further, 23 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 4, 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 in 2003. Hence, the vertical axis shows the fraction of pupils in 2003 who are living in a municipality with a certain change. We see that the private school share has not changed at all in many municipalities where a large fraction of pupils is living. They constitute about 2/3 of all municipalities but host clearly less than half of the total pupil population. This reveals that the reform has had a small impact in more rural areas of Sweden. On the other hand, within the third of the municipalities with private schooling in 2003 (and that host clearly more than half of the pupil population) there are municipalities with both small and large changes over time. Hence, the penetration of private schooling has differed greatly across municipalities.

3. Data and correlates of private schooling

3.1. Construction of the data set used in the estimations

¹⁵ The increase in the overall private-school share is driven by an increased enrollment in all school types, except in boarding and international schools and in schools directed to students with special needs for which the number of pupils enrolled in relation to the total population of pupils (private and public) have remained fairly constant over time. We have kept pupils in these three distinctly different school types in our sample, and reassuringly our main estimates are not sensitive for including them or not.

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. Information on school grades and educational attainment are available for all pupils from nationwide school and educational registers. We also 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 of compulsory school and in high school and the region of residence of each pupil as well as the regional location of the schools. No information of which school a pupil attended in grades one through eight in compulsory school is available. The school registers contain information about all schools in Sweden, which allow us to identify whether or not a school is private. Throughout this paper we use the term school cohort to denote the cohort of pupils who leave the ninth grade, i.e., the last year of compulsory school, in a certain year (which implies that the pupils' school information is observed in the spring each year). We use a range of educational outcomes which we shortly describe in this section. For a more thorough description we refer to the appendix. The sorting and cost outcomes are described in section 6.

The grade score outcomes are observed at three points during the individuals' schooling: at the end of 9th grade; after the first year in high school; at the end of high school. At all times we use grades from classes in the following core subjects: mathematics, English, sciences and social sciences. Information on pupils' compulsory school-leaving grades in 9th grade is available for school cohorts 1988-2003. The ninth grade scores are the main measures of a pupil's performance in the last three years of compulsory school and secondary school track admittance is entirely based on these grades. To make sure that grade 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. Still, there is a possibility of some subjectivity in grade setting by teachers. We would

therefore have preferred to complement our grade analysis with test scores. However, these are only available for a few years and for a selective sample of schools.¹⁶ The most important issue for us, however, is whether there are differential grading standards in regions with more private schooling. We return to this in Section 4.5.2 where we show that grade inflation does not seriously bias our results for grades at the end of compulsory school.

Information about pupils' high school grades is available for school cohorts 1994-2003. We calculate a measure of achievement at the end of the first year of high school, when courses in the four core subjects are compulsory for all high-school students. We also use a measure of grades at the end of high school which is calculated as the average of the grades in courses at higher levels in the same four core subjects. We include the levels that are required in most theoretical study programs (academic tracks).

When we use grade scores as outcome variable, we first convert the score to a percentile rank based on the distribution of scores in each subject for each school cohort. We then use the average percentile rank of each pupil as a main measure of academic achievement for the individual. This gives us three GPA measures: one at the end of compulsory school, which we label GPA0, one at the end of the first year in high school, which we label GPA1, and one at the end of high school, which we label GPA3. We also use mathematics and English as separate outcomes (and label them correspondingly to the GPA outcomes). It is enough for a student to have grade in one of the subjects to be included in the measure at each time. GPA0 is missing for the few pupils who attended private schools before the reform, since these schools then were not required to report grades, and for students in some special pedagogy schools, since grades are not available from these schools. However, these individuals are included when we look at outcomes beyond compulsory school. However, GPA1 is missing for individuals who do not continue to high-school as well as for high-school dropouts, and

¹⁶ The exception is 2003, for which year test scores are available for most compulsory school 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).

GPA2 is only defined for the selective sample of students who have chosen an academic high-school track. We define three dummy variables indicating if grades are available at these occasions: *observed with any grade* (at end of compulsory school), *observed with any grade in high school* (1st year high school), and *academic track in high school* (at end of high school), the last measure being a strong predictor of continuation to university. These outcomes are used for examining the potential issue of sample selection bias, but are also interesting per se.

We use three measures of long-term educational achievement: The fraction of a school cohort that have completed at least one semester of university education (attained 20 credit points or more in an academic field of study) within 6 years after they left compulsory school (UNIV6); The fraction of a school cohort that have completed at least 4 semesters of university education (attained 80 credit points or more in an academic field of study) within 7 years after they left compulsory school (UNIV7); The average educational attainment (years of schooling) of a school cohort within 8 years after they left compulsory school (EDU8). For UNIV6 we study the school cohorts 1994-2000, for UNIV7 the 1993-1999 cohorts and for EDU8 the 1993-1998 cohorts. The reason for studying different school cohorts for different measures is a combination of the two facts that we want to look at attainment after different durations of time since the 9th grade and that time-consistent information about educational attainment is restricted to the years 2000-2006.¹⁷

The main measure of private school choice used in this paper is the share of pupils attending private school in the 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. We use the share of private school pupils based on municipality of

¹⁷ The classification for levels and types of education, used by Statistics Sweden, changed between 1999 and 2000.

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. We also use the private school share for the high school level in a few estimations. This is calculated as the number of private high-school students (independent of where they attend high school) in a school cohort from a given municipality divided by all high school students from the same school cohort and municipality (where the municipality of origin is observed at the end of 9th grade).

Note that all variables are aggregated up to municipality and year by school cohort. That is, all variables are based on individuals residing in a municipality at the time they leave compulsory school no matter where they later live (as long as they have not left Sweden permanently). Hence, we can really look at the impact of the private school share at the compulsory level for the very same individuals later in life.

3.2. Descriptive statistics

Table 1 shows means and standard deviations for most of the variables that we use.¹⁸ We present these summary statistics for the post-reform school cohorts 1993-2003, except for the high-school outcomes (defined for school cohorts 1994-2003), and the educational attainment outcomes (1993-2000). Column 1 shows statistics for weighted aggregated data for all municipalities. These figures are based on the individual characteristics aggregated up to municipality-year (school cohort) level, where the weights are the numbers of individuals in each municipality-school cohort cell in the 20 percent random sample. These are the aggregated levels later used in the estimations. In columns 2 and 3, we instead show summary statistics based on the individual-level data (which explains why the standard deviations here

¹⁸ Exceptions are parental education in terms of years of schooling, immigrants' origin, and also the segregation measures which are discussed separately in section 5.

are much larger) for private and public school pupils respectively. Columns 4 and 5, displays again summary statistics for the municipal-year aggregated level, but here they are presented separately for municipalities with and without private schools in a given year. A municipality is counted as having a private school if it has at least one private school with pupils attending 9th grade in the relevant year.

We see that the average private school share over these years is 0.023, being 0.05 in municipalities with private schools, and that a small fraction of pupils who are living in municipalities without private schools attend a private school in another municipality. About 1 percent of the pupils have not attained a valid grade in any of the subjects that we study. It is interesting to note that the fraction of pupils with a valid grade mark in at least one subject is lower among private school pupils than among public school pupils.¹⁹ On the other hand, private school pupils with grades have on average much higher grades from the compulsory level than public school pupils (about 9 percentile rank units higher). This might seem as a contradiction, but a likely explanation is that the heterogeneity in the pool of pupils that attend private schools is relatively larger. Private schools seem to attract high performing pupils as well as pupils from the lower end of the ability distribution (in which a considerable fraction fail to obtain grades). It can also be observed that grade scores are somewhat higher in municipalities with private schools.

Next we consider the high school outcomes. The fraction of students in a school cohort with grades in high school is 75 percent. Again, this fraction is lower for students who attended a private school in the ninth grade, which is not surprising since valid grade marks from the end of the compulsory level is a minimum requirement for being qualified for high school. Surprisingly, however, this fraction is also clearly lower in municipalities with private schools in relation to municipalities with only public schools. The comparison of the average

¹⁹ This is not explained by the fact that about 760 special-pedagogy private school pupils have received grades from another grade scale, since these individuals are not counted in this measure.

grade scores between columns 2 and 3 and between columns 4 and 5 are in line with what we observe for the ninth grade: scores are much higher for private school pupils and somewhat higher in private school municipalities. An interesting difference is that the fraction who chooses an academic high school track is 61 percent in private school municipalities compared to 53 percent in municipalities where only public schools are available.

The summary statistics for the educational attainment outcomes, next, show that on average 27 percent of the individuals in a school cohort have at least one semester of university studies, and 17 percent have at least four semesters. Mean years of schooling is 12.41. Former private school pupils have higher levels of attainment, while the differences between municipalities with and without private schools are small.

Turning to the family background and demographic variables, we see that private school pupils' parents are relatively older, they clearly have more education and they are more often immigrants. Overall, about 13 percent of the pupils are either immigrants or second-generation immigrants. Noticeable differences between private school municipalities and the ones without private schools are that the parental educational level as well as the fraction of immigrants is higher in the former category. Interestingly, mean family income is very similar across columns, in contrast to mean parental education which differs rather much.

Finally, we consider the descriptives for our school level variables. These variables are supposed to capture the degree of competition between public schools and possible consequences of the decentralization of school financing, the two other main school reforms in the early 1990s. These variables differ between municipalities and years. We use 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 as proxies for public school competition. School financing variables are captured by the average pupil-teacher ratios in schools and by the logarithm of total school costs per pupil, all measured for grades 1-9. The school financing

variables are averages for all compulsory school pupils in the municipality. The numbers in columns 2-5 reveal that the size of the pupil population clearly is larger in municipalities with private schools, indicating that private schooling mainly is an urban phenomenon (as also indicated by the distance to school figures). Furthermore, the pupils per teacher ratios are relatively lower for public school pupils as well as in municipalities that only has publicly operated schools. Average municipal school costs are similar in all five columns.²⁰

3.3. Explaining changes in private school enrolment across municipalities

Next, we perform some aggregate weighted descriptive regressions, where the dependent variable is the private school share in the municipality.²¹ Results are shown in table 2. In columns 1-4 we regress the private school share in 2003 on a number of variables determined in 1992, the year before the reform was implemented. The purpose of these estimations is to investigate whether the differential development of private schooling (as seen in Figure 4) can be explained by observable characteristics determined prior to the implementation of the reform. In columns 1-2 we use all municipalities available. In columns 3-4 we restrict ourselves to the municipalities with any private schooling in 2003.

From the estimates in column 1 it is clear that some variables in 1992 are very strong predictors of growth in private schooling until 2003. The increase in private schooling is positively related to the average age of the parents, the average students' family income and the fraction of immigrants in the municipality, and negatively related to the fraction of students with parents having no income (a proxy for the unemployment rate). There is only

²⁰ The mean logarithm of distance to nearest public school for the pupils residing in the municipality is 7.27, which amounts to about 1.5 kilometers. The mean for the log school costs per pupil is 10.96 log points, which amounts to 54,720 SEK (or about 7,000 EUR/9,000 USD).

²¹ Throughout this paper, we weight the aggregate estimates with the number of individuals in each municipality-year cell for whom we have non-missing observations on the respective outcome. In all of our main regressions, non-weighted estimations yield results that are very similar to our weighted estimations.

weak evidence that the private school share has increased more in municipalities with lower certified teacher densities (marginally significant when school cost per pupil is excluded).

In column 2 we add two variables capturing the level and change of average pupil achievement in public schools in the municipality prior to the reform. These are the mean of GPA0 in 1992 and the change in mean GPA0 between 1988 and 1992. We find no evidence that the private school share has increased more in municipalities with high achieving public school pupils or public school pupils who's performance have changed a lot during the last five pre-reform years. These results are very important since one might make the argument that private schools mainly were established in municipalities with poor public schools. We have also added a dummy variable indicating whether a private school existed in the municipality prior to the reform. This variable is statistically insignificant.²² When we run the same specifications on the smaller sample of municipalities with private schooling in 2003, we obtain qualitatively similar results.

In columns 5-6 we regress the private school share in 1993-2003 on variables measured in the same years. That is, all variables are municipality-year averages for 1993-2003. Higher private school shares are more likely to be found in municipalities with more parents with university education and with a higher fraction of second-generation immigrants. A large student population is strongly positively related to higher private school shares. In column 5 we report estimates from a regression with municipal fixed effects. The general picture is unchanged. The increase in the private school share is positively associated with an increasing average parental education, a larger fraction of immigrants, and growing municipalities as measured by size of pupil populations. Hence, these variables are important to control for when we examine the impact of the private school share on educational achievement in our fixed effects models. The variables capturing differential public school competition and

²² All estimates are very similar if we omit the variables capturing school financing and public school competition.

school financing between municipalities are jointly statistically significant in the both the cross-sectional and fixed-effect regressions. This is entirely driven by the pupil-teacher ratio variables which are jointly and individually significant. If they are entered separately, both are estimated positive, but only the one calculated for certified teachers is significant. The school costs (in grades 1-9), number of public schools and distance to nearest public school variables are always statistically insignificant. We conclude that there is no evidence of the private school share is associated with variables indicating the degree of competition between public schools. Hence, this issue will not bias estimates of the private school share on educational outcomes. Still, since we see an impact of the school financing variables, we will return to this issue in Section 5.5., where we investigate how sensitive the estimates of the private-school share effect on achievement are to the inclusion of these variables.

4. Effects of the private school share on educational outcomes: identification strategy and main results

4.1. 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, make enough students attain these schools, 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 earlier, 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. First, we include a large set of time-varying covariates in order to control for changes in the composition in the demographic and family background of pupils across municipalities over time. Second, we control for municipality-specific linear trends with the purpose of controlling for unobservable variables that varies linearly within municipalities over time.

We estimate versions of the following empirical model of the impact of the private school share on average educational performance:

$$(1) \quad \bar{y}_{mt} = c + \beta(\bar{P}_{mt} \cdot T) + \lambda \bar{X}_{mt} + \gamma_m + \alpha_t + \delta_m \cdot \text{trend} + \varepsilon_{mt},$$

Where \bar{y}_{mt} denotes the average educational outcome variable for pupils residing in the municipality (or school market) m at time t ; \bar{P}_{mt} denotes the share of pupils residing in municipality m that attend a private compulsory 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 ($t \leq 1992$); \bar{X}_{mt} are observable factors measured at the municipality level (averages of individual characteristics, and municipality characteristics such as size of pupil population); γ_m and α_t represent municipality- and year-specific effects respectively; $\delta_m \cdot \text{trend}$ represents municipality specific effects interacted with a linear time trend; and ε_{mt} is a random error term. Equation (1) takes into account that the data are available both pre-reform ($T=0$) and post-reform ($T=1$), and we have assumed that the observable characteristics have the same impact for all pre- and post-reform years. We further allow the regression error to be correlated within municipalities over time. Note that the specification in (1) is quite general, and for several outcomes we only use years from the post-

reform period (where $T=1$) and more restrictive versions of the specification. Recall that year (t) denotes school cohort, i.e., the year the pupils leave the ninth grade, irrespective of if the outcome is a ninth-grade variable or a more long-term achievement measure. This means that the private school share and most control variables are observed at year t whereas the outcome variables are observed at year t (the ninth grade outcomes) or at years in the range $t+1$ to $t+8$ (high school and educational attainment outcomes).

Note also that we use outcome variables averaged over both private and public school students for $T=1$. 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 aggregate 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.²³ We return to the issue of sorting in section 6.2.

In this paper, we only focus on estimating the effects of the private school share on average outcomes. However, this effect consists of both the impact for individuals of attending a private school instead of a public school (a private attendance effect) and any spillover effects of an increased private school share on students in both private and public schools (a competition effect). This means that only focusing on the private attendance effect can give very misleading estimates of the overall impact of private schooling. For instance, if increased competition between private and public schools improve outcomes equally much for private

²³ Both Hanushek et al. (2003) and Hoxby (2000) find evidence of peer-effects of achievement, but neither study finds any support that such effects are non-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.

and public school students, one will find no beneficial effect of attending a private school, even though all students clearly have benefited.

The issue of whether or not trends in achievement differs between municipalities who had a large, small or no growth in private schooling is very important for the credibility of our estimates using difference-in-differences estimation. 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 with different growth in private schooling in the absence of the reform. For instance, if it were to be the case that there is a lower demand for private schooling after the reform in municipalities whose students show the fastest achievement gains prior to the reform, our estimates of (1) using municipality and year fixed effects would risk being biased. It is therefore very comforting that we in section 3.3. did not find any statistically significant association between pre-reform achievement level and change and the private school share in 2003 across municipalities. We will later in this section make more use of that we have data for several years before the reform and further investigate the occurrence of differential pre-reform trends across municipalities.

An issue we need to consider, however, is the potential simultaneous impacts of other school reforms introduced in the early 1990s. For instance, the introduction of choice among public schools might have enhanced competitive pressure more in more densely populated municipalities, compared to rural municipalities where there is less potential for choice and competition (e.g., due to long commuting distances). If public-school competition has led to an improvement of public schools in urban areas, this might have reduced the demand for private schools in such areas. Hence, an empirical challenge is to control for differential development of school characteristics that might not be caused by the private school share, but that are correlated with both the private school share and average achievement.

4.2. Educational achievement at the end of the compulsory school level

Table 3 reports the results from estimating different versions of model (1). Columns 1 and 2 show estimates from cross-sectional models and columns 3-6 show estimates from models where we have included the full set of municipal indicators. The models underlying the estimates of columns 5 and 6, in addition, include municipality indicators interacted with a linear time trend. In the specifications underlying the estimates reported in column 2, 4 and 6 we control for a set of time-varying family and demographic variables (which are specified in Table 1). All regressions include year fixed effects.

We first consider if an increase in the private school share has an impact on the fraction of pupils with grade marks. We find a negative association (first row, first column) that disappears once we introduce the set of controls in column 2. By further adding the fixed effects and municipality specific trends in columns 3-6 we find no evidence of such an impact. Hence, there is no need to control for this fact that we do not observe grades for all pupils in our grade analysis that follows.

Next, we study the impact on average GPA, math grades and English grades at the end of compulsory school. In the first column, we see that the private school share is strongly positively associated with GPA, as well as with Math and English grades. A one-unit higher private school share is associated with 30.1 percentile rank higher GPA. The estimate is even higher for English. When we include our set of controls, the estimated effects are greatly decreased, and for GPA and Math they become statistically insignificant. Hence, selection is very important in our estimations.

The result from our baseline difference-in-differences regression is shown in column 3. This estimate, which is identified using the association between changes in GPA and changes in the private school shares within municipalities over time, show that a one-unit increase in

the private school share is associated with 9.3 percentile rank points higher GPA. When we also add covariates to this model (column 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 8.3 percentile rank points. This estimate can be interpreted thus; moving all pupils from public to private schools would generate an increase in average GPA by nearly 10 percentile ranks. A more realistic interpretation is that an increase in the private school share by 10 percentage points is expected to increase average GPA by nearly 1 percentile rank point. Note that the corresponding estimates for math and English are very similar.

Next, we add controls for municipality-specific trends for all years (columns 5-6) 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. We see that the point estimates become slightly smaller (and the ones for math become statistically insignificant), but the overall conclusion is that private school share estimate barely is affected. These results suggest that even though municipalities with more or less private schooling are different on a number of characteristics, controlling for municipality fixed effects are sufficient to adjust for such differences. This means that municipalities with differential changes in the private school share after the reform are similar in terms of unobservable within-municipality trends in factors affecting academic achievement. Thus, it does not seem to be the case that municipalities that have faced the largest increases in private schooling did so in response to differential trends in unobservable variables prior to the reform.²⁴

We have also performed additional tests of the assumption that the evolution of grades before the reform was implemented in 1993 is similar for municipalities with differential

²⁴ We have also carried out tests of the existence of non-linear effects of the private school share on average GPA by adding a quadratic term of the private school share in the regressions. We can never reject linearity, but the standard errors for the quadratic terms are very large.

development of the private school share after the reform. First, we have regressed municipal changes in GPA between 1988 and 1992 (the pre-reform years) on the municipal changes in the private school share between 1993 and 2003 (the post-reform years). This gave statistically insignificant estimates of 0.74 (4.78) without the set of covariates, and 1.94 (4.75) with covariates. Second, we regressed averaged grades in 1988-1992 on municipality and year indicators, as well as municipality indicators interacted with a linear time trend. We then predicted average GPA for municipalities and years in 1993-2003. This predicted variable was then regressed on municipality and year indicators, and the private school share for years 1993-2003. We found positive but statistically insignificant estimates of 4.57 (6.09) without the set of covariates, and 4.41 (6.72) with covariates. Third, in Böhlmark and Lindahl (2007) we showed that differences in average grades between 1988/89 and 1991/92 were very similar for the 1/3 of the municipalities with the highest private school share in 2002/2003 and the 1/3 of the municipalities with the lowest (but positive) private school share in these years, as well as for municipalities with no private schooling.²⁵ The conclusion from these exercises is that we find no evidence of a relationship between the change in academic achievement during the pre-reform period and the change in the private school share in the post-reform period. Hence, our findings of no association between the private school share in 2003 and average grades in 1992, and the change in average grades between 1988 and 1992, conditional on a number of background variables, are here strongly supported by additional analysis.

A related issue is if the municipalities that had private schools already before the reform was implemented are special (in ways that are not captured by the controls and fixed effects) to an extent that they drives the results. Even though we found no association between having had a private school before the reform in 1993 and the private school share in 2003,

²⁵ The estimate (standard error) of the differences in average grades between 1988/89 and 1991/92 was 0.03 (0.38) for the 1/3 of the municipalities with the highest private school share in 2002/2003, 0.03 (0.32) for the 1/3 of the municipalities with the lowest (but positive) private school share in 2002/2003, and 0.22 (0.29) for the municipalities with no private schooling.

conditional on all the background variables, we have performed an additional test by simply omitting these 22 municipalities for all years. This was done using, again, the specification underlying column 4 in Table 3. The result is that the inclusion of these municipalities does not seem to be important for the estimate, which is 7.04 (3.40) using this restricted sample. This is very reassuring also for the generality of our results since we in this exercise have omitted a few large urban municipalities.

There are a few previous Swedish studies that have estimated the private school share on 9th grade achievement. In Böhlmark and Lindahl (2007) we extensively discuss our GPA results in relation to these studies. We concluded that our estimates were significantly lower than in previous studies (Sandström and Bergström, 2005, and Björklund et al., 2005).²⁶ The main reasons for this are that we use a different (and we argue superior) measure of the private school share and that we analyze a much longer time-period.²⁷ We return to the issue of alternative private school share measures in section 5.2.

As an example of the magnitude of our basic estimate, imagine a municipality with 1,000 ninth grade pupils, of whom 100 pupils (10 percent) initially attended private schools. If an additional 100 pupils would change over from a public to a private school, the average pupil

²⁶ Sandström and Bergström (2005) 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) use test scores (about 50,000 pupils, non-representative sample) 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. Björklund et al. (2005) find positive and statistically significant effects for all subjects; about 40 percentile rank effects for grades, and even larger for test scores. Ahlin (2003) also use Swedish data but focus on estimating the effect of private school attendance on individual achievement.

²⁷ Both Björklund et al. (2005) and Sandström and Bergström (2005) use a private school share measure in which pupils in grades 1-9 are observed at year t . This measure has some drawbacks: First, increases in the private school share in grades 1-9 will to a large extent reflect that private schooling has increased faster in lower grades. 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. In Böhlmark and Lindahl (2007) we show that the larger estimate obtained with the grade 1-9 measure is due to the private school share in grades 1-6. We argue that it is difficult to believe that there would be such a 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. Second, even though this measure might deal with a potential reallocation of resources from earlier grades, 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. Resource redistribution is unlikely to happen between such separate schools.

(in any school) would gain almost one percentile point in GPA. This seems like a fairly small effect. Next we proceed by studying if this effect pertains in terms of medium- and long-run educational performance.

4.3. Educational achievement in high school

In Table 4 and Table 5 we report estimates of different versions of model 1 for first-year high school outcomes and second-/third-year high school outcomes, respectively. The structure of these two tables, in terms of specifications used, is the same as in Table 3. Note that we only use data for school cohorts 1994-2003 when we study high-school outcomes. Note also that we present results from estimating regressions for the 9th-grade GPA, using data for the very same cohorts (1994-2003) and individuals (Table 4: those who subsequently are observed with high-school grades; Table 5: those who in addition are academic-track students), as a benchmark.

In the first row of Table 4, we present estimates for the impact of the private school share on the fraction of individuals who have grade marks in high school (from A-courses). The fraction without grades includes individuals in a school cohort who never continued to high school as well as high school dropouts. We see that there is a negative association when considering the first naive cross-sectional regression, but that there is no evidence that this would represent a causal impact when looking at our regressions with fixed effects and controls. This finding of a zero effect is perhaps somewhat surprising given that we find a positive effect on 9th-grade GPA. However, it is also reassuring for the high-school grade analysis that follows, since it shows that the private school share does not determine the probability of belonging to the sample being analysed.²⁸ Hence, there is no need to worry about sample selection bias in our analysis of effects on grades in first year of high school.

²⁸ As a sensitivity analysis, we included the fraction of individuals in a municipality-year with grades as a control variable in the grade regressions (using the specification underlying the estimates in column 4), in order to

Before we consider first-year GPA, math and English note that the main 9th-grade GPA estimates are unaffected when we switch to study the 1994-2003 school cohorts (and subsequent high-school students). Now, looking at the high school estimates for GPA, math and English, this positive effect seems to persist to the first year of high school. That is, we estimate significant coefficients of the same magnitude in columns 3 and 4, although they get smaller and statistically insignificant when we control for municipal specific linear trends in columns 5 and 6. Since we here only use post-reform years, the results from these last two specifications should probably be interpreted with some care.²⁹

Recall that a nice thing with the first-year grades is that they are from courses that are mandatory for students in all regular study programs (A-courses), theoretical as well as non-theoretical. When we now turn to study the impact on grades from courses taken at the second and third year, we are consequently considering a more selective sample of students, namely those who have chosen an academic track. It is therefore important to first examine the impact of private schooling on the fraction of students who choose academic tracks (among high-school students). The first row in Table 5 presents these results. We find a positive effect. An increase in the private-school share with 10 percentage points gives an increase in the share of students choosing an academic track with about 2.5 percentage points, and this result is stable across all fixed effects regressions 3-6.

We do, however, not find any effects on second-/third-year GPA, math or English grades. This finding is also basically robust for adding the fraction of students choosing an academic track as a control variable in these regressions (in the specification underlying the results in column 4).³⁰ However, it is important to compare with the 9th-grade GPA estimates for this

control for selection into high school (and hence into the sample). The main estimates that are reported in column 4 (rows 3-5) were affected very little when this variable was included (and this is just in line with the findings in row 1, using the fraction with grades as the dependent variable).

²⁹ An argument against controlling for region-specific trends when we use only post-reform years is if there is a large fraction of individuals, previously restricted in their choice, who suddenly act and thereby creates a spike in the outcomes variable, this can lead to biased effect estimates, see Wolfers (2006).

³⁰ Estimates (standard errors) are for GPA: 3.79 (3.82); math: 1.37 (5.31) and; English: 8.90 (5.40).

selective group. These estimates are presented in the second row, and we see that they are all statistically insignificant. Thus, we cannot draw the conclusion that the positive effects that we have found for the 9th grade and for the first year in high school vanish over time. Rather, we do not expect to find any high-school GPA effect for the group of academic-track students. On the other hand, we find that an increased private school share increases the fraction of students who attends an academic track (which is expected since we find a positive overall effect on 9th-grade GPA). Since academic-track attendance is a predictor of university attendance and final educational attainment it is interesting to study if there exist long-term achievement effects. This is the next issue to be examined.

4.4. Long-term educational achievement

In Table 6 we report estimates of different versions of model (1) for university attainment as well as for number of years of schooling. Recall that these are municipality-year aggregated variables based on all individuals in a school cohort, no matter if they ever continued to or completed high-school after leaving compulsory school. We look at school cohorts a) 1994-2000 and b) 1993-1999 for our two measures of completed university semesters, and at school cohorts c) 1993-1998 for years of schooling. The structure of the table is otherwise the same as in tables 4 and 5. First, note that we estimate 9th-grade GPA regressions for each of these school cohorts (a, b and c). These estimates for GPA turn out to be of the same magnitude as before, but the standard errors are about twice as large (especially when comparing the main estimates in column 4). Hence, precision is an issue now when we are restricted to use smaller samples, and since the GPA estimates are insignificant we find it difficult to expect any long-run effects.

The statistically insignificant results in columns 2-6, for our three long-term measures of educational performance, are also in line with this expectation (although we find positive

associations for the first two outcomes when using the simple cross-sectional regression in column 1). The coefficients for our long-term outcomes are, however, all estimated positive if we consider the fixed effects regressions. At the same time, the 9th-grade GPA effects are insignificant for these cohorts, while we know they are not if we also incorporate younger cohorts (who have been more exposed to private schooling). This holds true also for the variable academic track, for which we did a re-estimation using only these older cohorts (not shown in table). For these reasons, a re-estimation of this long-term analysis in a few years (when this is possible) is not unlikely to show positive effects also for the long term. To conclude, we do not find any evidence for long-term effects, but since we either not find any short-run effects for these older cohorts this is just in line with our expectation. However, since we know that the precision improves much if we also add the 3-5 youngest school cohorts it is likely that this would happen also with the long-term estimations, if we were able to include these cohorts here as well.

5. Effects of the private school share on educational outcomes: additional analysis

In this section we examine the robustness of our educational-performance results. We have already reported the results from some robustness checks above. Most importantly, we examined the potential relation between municipal pre-reform trends in GPA and the private school share (in section 4.2). We found neither support for the existence of such differential trends for broad groups of municipalities defined by their subsequent development in private schooling, nor for that any such trends would seriously bias our results. In this section we study in particular five issues: 1) the existence of heterogeneous effects; 2) the sensitivity to using alternative private school share measures and potential competition, 3) the role of grade

inflation; 4) the role of private schooling at the high-school level; 5) The potential influence from other school reforms introduced in the early 1990s.

5.1. Different effects for different groups?

So far we have studied average effects on the basis of all individuals in a school cohort (except for some of the high-school analyses where we looked at selective groups). Thus, the important question of heterogeneous effects has been neglected until now when we re-estimate our main regressions for different sub-populations. These sub-populations are: immigrants (1st and 2nd generation); individuals whose parents both have less than high-school education; individuals who have at least one parent having university education; individuals whose family's income being in the first quartile; individuals whose family's income being in the fourth quartile. We consistently re-estimate the versions of model (1) that was referred to as the regression underlying the estimates in column 4 in tables 4, 5 and 6 (i.e., the one with fixed effects and controls). In order to conserve space we have restricted the number of grade outcomes to include only GPA (and not English and math) for the respective level, as well as to include only the first of our three university outcomes (the results for the other two are in line with the ones for this one).

The results are displayed in Table 7, where the average overall results are repeated in the first column. Note first that the standard errors naturally get larger when we study different sub-populations separately, so we need to be careful when we interpret these results. We see that the 9th-grade GPA estimates are positive for all five groups, but statistically significant only for the high parental education group and for the low- and high-family income groups. Thus, there might be groups that gain more from an increase in the private school share (in fact, that the coefficients for high- and low-parental education groups are significantly different from each another at the 10% level). A perhaps more important finding, however, is

that there is no evidence that any of these groups are losing, in terms of GPA, from more private schooling.

What critics of more school choice and increased competition probably would worry about the most is that disadvantaged groups of pupils might learn less in school than before. So, when we now try to summarize the remaining results in the table, we do this by first looking across the rows for significant negative effects for the three groups that we know are relatively more disadvantaged in school, i.e., the immigrant group and the low education and income groups. We do not find any statistically significant negative effects for any of these three groups for any of the outcomes. For immigrants, we instead find a positive effect on GPA in the first year in high school, and for low family income we find a positive effect on 9th-grade GPA.

For the high family education and income groups we find positive effects on 9th-grade GPA, on the probability of having high-school grades (the high parental education group) and on choosing an academic track in high school. Surprisingly we find that the typical individual in the high family income group are losing in terms of probability of being observed with a grade in the 9th grade and third year high-school GPA, as a consequence of an increase in the private school share.

From this analysis we learn that even though there is some evidence of heterogeneous effects, none of the three groups that are generally regarded as being disadvantaged in school (immigrants, low parental income or education) are losing from a higher private school share.

5.2. Alternative private-school share measures and potential competition

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 educational outcomes, and used all pupils in a school cohort to calculate the private school share. This is very reasonable given that we do not have

any information about if and when pupils have switched school types prior to 9th grade.

However, there are some alternative measures we also could use. In table 8 we show results from these alternative measures. The results from using our baseline measure are reported in the first row (A).

In the second row (B), we show estimates from using the private school share in the third level of compulsory school, i.e., based on grades 7-9. If resources are redistributed across grades in response to increased competition, this measure might be more suitable. However, we see that the results are very similar to the baseline estimates. In the third row (C), we report results from using the private school share in 9th grade calculated as the average of year t , $t-1$ and $t-2$. With this measure we attempt to take into account that the speed in the change in private schooling might have differed across municipalities, and that pupils in different municipalities consequently experienced different numbers of accumulated years in private schools and of private school competition. It might also capture effects of a delayed reaction of schools to increased competition. We see that the estimates again are similar to the baseline ones, whereas the standard errors almost double.

Finally, we use an alternative estimator that to a much higher degree allow for dynamic competition effects of the reform, and at the same time use variation from the establishment of private schools (as opposed to actual attendance). This instrumental variable estimation use the fraction of private schools (with grade 9 pupils in year t) as an instrument for the fraction of private school pupils at year t (the baseline private-school share measure). In this specification, which includes year and municipality fixed effects, we only utilize the variation in the change of the private school share that is due to variation in the change of the fraction of private schools. Hence, the only variation used is the one that stem from schools opening up or closing down. Consistent estimates are obtained as long as school openings are uncorrelated with unobserved factors affecting changes in educational achievement (i.e.,

through other ways than via changes in the control variables or a change in the private school share). Results are reported in the fourth row (D). We see that most of the estimates are larger, but none of them are statistically different than the baseline ones in row 1.

5.3. Are there differential grading standards in public and private schools?

So far, we have not discussed the mechanisms behind our findings more than stating that the overall effects that we are estimating consist of a private-attendance effect and a competition effect. Following this reasoning, a plausible explanation of the positive 9th-grade GPA effects is that average achievement has increased due to more choice since more pupils attend private schools (which perhaps are of better quality) and since the competition for pupils has given all schools (private and public) incentives to improve their quality.

A competing explanation is that schools indeed have reacted to competition, not by improving their quality but instead by becoming more generous in their grading standards. Their incentive for this would be that high average grades might work as a signal that their school is a successful one. Note that this explanation agrees with the positive estimates for academic track in high school (since acceptance to different study programs are based on grades received at the compulsory level) and with the finding of no significant effects after the first year in high school (since genuine skills might be what matters in the longer run). This explanation do, however, not agree equally well with the positive effects that we find for grades in the first year of high school. These grades are given in new courses, typically by new teachers in new schools (and genuine skills that students bring with them should be a factor that matters when acquiring new skills, poor measures of their previous skills should not). This indicates that grade inflation at the compulsory level is not the mechanism. However, it represents no evidence. For instance, grade inflation at the high-school level

might also be an issue, and the municipal private school shares at the compulsory and high-school levels are correlated.³¹

It would not necessarily be problematic if the grades that we use (as proxies for pupil performance) 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, as was discussed above, if grades are systematically inflated in municipalities where the private school share has increased much, than we cannot interpret our results as achievement effects. We are, in fact, able to investigate this issue for 2003, where grades and scores on achievement tests (from the ninth grade) are available for most pupils in mathematics and English. We estimate at the municipality level:

$$(2) \quad \bar{y}_m^s = \pi_0 + \pi_1 \bar{P}_m + \pi_2 \bar{T}_m^s + \pi_3 \bar{X}_m + \zeta_m,$$

Where \bar{y}_m^s is average grades in subject s and \bar{T}_m^s is average test scores in subject s . The private school share parameter π_1 captures whether pupils in municipalities with higher private school shares have higher grades than pupils in areas with lower private school shares, given average test scores and other observable characteristics. Hence, π_1 is a measure of how inflated grades are in municipalities with different private school shares.

Table 9 shows results from the estimation of model (2) using mathematics and English as outcomes. In columns 1 and 3, no covariates have been included. Columns 2 and 4 display the results when the set of covariates are included. These regressions reveal 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 grades would generate different results in the main regressions of the private school share on average achievement.³²

³¹ A weighted correlation coefficient for the private school share at the two levels is 0.53, for school cohorts 1994-2003.

³² 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,

Note again that we only study a single cross-section here, and it would have been even more reassuring if we had been able to analyze a longer time period and then found the same results. However, we believe that analyzing this issue for the last year in our data is superior to analyzing any earlier single year. First, the municipal level of private schooling in 2003 is strongly related to the municipal increase in private schooling 1993-2003. Second, if grade inflation was increasing with the level of choice and competition than it should be most severe in the last year.

5.4. The role of private schooling in high school

It is also important to study the role of private schooling at the high school level for our post-compulsory school results. Our high-school and university estimates might partly capture the effects of choice and competition at the high-school level, and thereby perhaps mask actual long-term effects. Therefore, we investigate whether our main estimates for the post-compulsory school outcomes are robust for the inclusion of the private school share in high school as an additional control variable. This variable is defined based on individuals in a school cohort who have continued to high school, and it measures the private school share in the year they leave high school (normally 3 years after the ninth grade). We simply include this variable in the by now familiar version of model (1) with controls and municipality fixed effects. Table 10 shows the results from these regressions.

Reassuringly, we find that the basic private school share estimates (9th-grade) are affected very little when we also include the corresponding variable for the high-school level. We see that these estimates in the first and third columns are not affected at all (compared to the baseline estimates which are not repeated in this table), and that the coefficients for the high-

along with weaker evidence that public schools in municipalities with many public schools do the same thing. 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 can be taken a long time after completed high school.

school private school share are insignificant. This is what we would expect, especially for the outcome share of students who choose an academic track, since this choice hardly is affected by the private school share in high school. For first-year GPA we see that the estimate (standard error) of main interest is very similar as before; 6.13 (3.25) compared to 7.66 (3.33) in table 4. The effect of the private-school share in high school appears to be of a similar magnitude. For third-year GPA we find a positive estimated effect, 4.69 (2.65), for the high-school private school share (and very similar if we omit the 9th-grade PSS from the regression), but an insignificant coefficient for the ninth grade measure (as before). The main estimates for the three long-term outcomes are hardly affected at all (compared with baseline estimates presented in column 4 in Table 6), and somewhat surprisingly we find that the private school share in high school does not seem to have any impact either. Finally, the estimates for the two private school share measures are jointly significant only in column 2 (p-value: 0.000) and in column 3 (p-value: 0.009).

5.5. 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 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).³³

³³ 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

We have therefore estimated model (1) with added variables capturing public school competition and school finance decentralization (these are listed in Table 1), even though they are probably simultaneously determined with the private school share.³⁴ We also already know from the results reported in Table 2 that the proxies for public school competition should not lead to bias in estimates of the private school share on average outcomes, since they did not impact the private school share. We have performed this robustness check using all outcomes. However, for the sake of space, and since the pattern turned out to be very similar for all outcomes, we only present the estimates for 9th-grade GPA in table (and refer to a few other results in the text below).

We show these estimates in Table 11. 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* and the two school financing measures *pupil-teacher ratio* and *pupil-certified teacher ratio*. These variables are jointly statistically significant in the regression (p-value: 0.059). Even though these school measures are jointly significant, we find that the private school share estimate is unaffected from the inclusion of these variables (which is also the case if we in addition add the control for *total municipal school costs per pupil in grades 1-9*). This is also true for the other outcomes. For example, the corresponding estimates (standard errors) for the outcomes 1st-year high school GPA and academic track with and without the inclusion of these school variables are 7.45 (3.34), 6.99 (3.43) and 0.24 (0.08), 0.22 (0.08) respectively.

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.

³⁴ 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.

Hence, we find no evidence that our estimates of the private school share effects are influenced by other components of the reforms in the early 1990s.

6. Effects of the private school share 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 lose 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 (which is consistent with the pattern in figure 1). It is also possible that local authorities who are in favor of school choice and competition invest additional resources 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:

$$(3) \quad \log[\text{Costs/pupil}]_{mt} = \kappa_0 + \kappa_1 \bar{P}_{mt} + \kappa_2 \bar{X}_{mt} + \gamma_m + \alpha_t + \nu_{mt}$$

where the outcome variable is total school costs per pupil residing in the municipality, 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 2.³⁵ If the private school share increases by 10 percentage points, overall school costs increase by about 2 percent. 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 (indicating 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

³⁵ 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.

private schooling on school costs.³⁶ Comparing the results for GPA at end of compulsory school with these 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 high short-term return to an additional SEK (or dollar) spent.³⁷

6.2. Segregation

We have seen in Table 1 that pupils' whose parents have a higher education and/or are immigrants are overrepresented in private schools and in areas where private schools exist. Similarly, Table 2 showed that an increased fraction of pupils with these characteristics is associated with an increase in the private school share. In this section we study the issue of segregation, or sorting, of pupils in more depth. Although we have already seen that there tend to be stratification along the lines of parental education and ethnicity, we start this section by describing the level of segregation along various characteristics for 1993 and 2003. We use the dissimilarity index, which in our setting measures how many percentage of a group (say immigrant pupils) that have to change school type, for private and public schools to each have the same percentage of group members.³⁸ When there are only two school types the dissimilarity index for a given year and municipality can be calculated as:

$$(4) \quad DI = \left| \frac{\text{Nr of natives in public schools}}{\text{Nr of natives in all schools}} - \frac{\text{Nr of immigrants in public schools}}{\text{Nr of immigrants in all schools}} \right|$$

³⁶ If we add linear trends interacted with municipal indicators we 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.

³⁷ 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 expenditures improving pupil achievement with about 0.2 percentile rank points.

³⁸ See Echenique and Fryer, Jr (2007) for a nice overview of advantages and disadvantages with different segregation measures.

Note that, since we only have two school types, the number is the same if we instead calculate the fractions of pupils in private schools. If there are no pupils in private schools, or if the fraction of immigrants in public schools is the same as in private schools, the dissimilarity index equals zero. If we calculate DI for 1993, where very few pupils went to private schools, we get a value of around 0.01 if we divide pupils into groups of having high/medium educated parents relative to having low educated parents. Hence, 1 percent of pupils in either of this group would have to be reallocated between school types, in order for group evenness to be attained. In 2003, the DI for the same education groups is around 0.02. A similar pattern can be seen for income groups. For groups divided by immigrant status the index increase from about 0.02 in 1993 to 0.04 in 2003. Hence, the pupils who attended private schools in 1993, when very few existed, was more unevenly distributed across school types, then what was the case in 2003.

Next we attempt to answer the question whether an increase in the private school share causes an increased sorting of pupils along these characteristics. Thus, what we are interested in here is not the level of segregation but instead changes in the composition of the pupils who attend public and private schools. When we study this question we estimate the raw (i.e., non-absolute) value of DI on the private school share:

$$(5) \quad DI_{X,mt}^{RAW} = \lambda_0 + \lambda_1 \bar{P}_{mt} + \gamma_m + \alpha_t + \nu_{mt}$$

We show estimates of λ_1 in Table 13. For both the cross-sectional and fixed-effect estimations, we see that pupils with low educated and Swedish parents are sorted into public school, when the share of private school pupils increases. There is no impact on sorting with respect to parental income or pupil's immigrant status.

Another approach is taken by Hsieh and Urquiola (2006) who estimate the following regression:

$$(6) \quad \frac{\bar{X}_{pub,mt}}{\bar{X}_{mt}} = \delta_0 + \delta_1 \bar{P}_{mt} + \gamma_m + \alpha_t + \bar{\eta}_m$$

Where the dependent variable, $\frac{\bar{x}_{pub,mt}}{\bar{x}_{mt}}$, is the ratio of the average characteristic of public school pupils in municipality m to the average characteristics of all pupils in a municipality.³⁹ This measure is higher (lower) than 1 if the average characteristic, say the fraction of immigrants, is larger in public (private) schools than in all schools. The measure equals 1 for municipality-year units without private schools or where the average characteristic is the same in public and private schools. We study the following characteristics: parents' income; parents' education; second generation immigrant pupil (i.e., pupils with immigrant parents but who were themselves born in Sweden) and immigrant pupils. All these characteristics are known to be highly correlated with pupil performance in Sweden.

From the cross-section estimates in Table 14 we see that the relative income and educational attainment in public schools is lower in municipalities with higher private school enrollment. We also see that the relative fraction of immigrant (2nd generation immigrant) pupils in public schools is higher (lower) in municipalities with higher private school enrollment.

When we add municipality indicators, 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 private schooling grew more. 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. Estimates are shown in columns 2, 4, 6 and 8 of Table 14. In these fixed effects models, we find no evidence of sorting of pupils by parental income and pupil's immigrant status. We do, however, find that an increase in the private school share makes public schools to lose pupils who are second-generation immigrants and/or whose parents have high

³⁹ For a dichotomous variable of pupil being native (versus immigrant) the dependent variable can be written as:

$$= \left(\frac{\text{Nr of native students}}{\text{Nr of all students}} \right)_{\text{public schools}} / \left(\frac{\text{Nr of native students}}{\text{Nr of all students}} \right)_{\text{all schools}}$$

education.⁴⁰ 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.⁴¹ Importantly, the segregation results in Tables 13 and 14 are qualitatively similar.

How do these results compare to studies evaluating large-scale private school reforms in other countries? Our segregation result for parental education is in line with what has been found for Chile (Hsieh and Urquiola, 2006) and New Zealand (Fiske and Ladd, 2000). Both these studies found that the main consequence of the respective country's school choice program was to generate an inflow of pupils with high social background to private schools. However, we note that our results for other outcomes are at odds with such a conclusion for Sweden. For pupils with immigrant parents, who on average perform less well in school, we even found an increased inflow to private schools. It is also important to point out that whether or not segregation is good or bad, is an open question.⁴²

7. Conclusions

We have estimated achievement, cost and segregation effects of choice between public and private schools in Sweden. For this purpose, we have used a large administrative data set on individuals graduating from compulsory school in 1988-2003, and exploited the differential

⁴⁰ To interpret the magnitude of the estimates, consider the estimate of -0.36 for 2nd generation immigrants in the last column, which would be interpreted as an increase in the private school share by 10 percentage points generating a 3.6 percent lower fraction of 2nd generation immigrants in the public schools in a municipality. Hence, relative to a situation of no sorting, so that the fraction of 2nd generation immigrants being 6 percent in public schools and 6 percent in all school in a representative municipality, we now end up with a situation where there are $6 \cdot (1 - 0.036) = 5.78$ percent of 2nd generation immigrants in public schools and 6 percent immigrants in all school in a municipality.

⁴¹ 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.

⁴² 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.

within-municipal variation in private schooling that developed as a consequence of the 1992 school reform.

The basic findings for short-, medium- and long-term educational achievement are: 1) The impact of a 10 percentage point increase in the private school share on average grades at the end of compulsory school is just below 1 percentile rank point. We consider this as a fairly small effect. Nevertheless, it is very stable across specifications and it does not seem to be driven by either differential grade-setting standards or in different pre-reform or current trends in unobservable variables across municipalities. There is some evidence of heterogeneous effects across sub-groups defined by socioeconomic characteristics and immigrant status, but no group is found to loose from a higher private school share. 2) The effect on performance at the end of compulsory school pertains for the same individuals who enter high school, when their grades are examined after the end of their first year in high school. However, there is no significant impact on the probability of entering high school. 3) The share of individuals who choose an academic track in high school is estimated to increase by 2.5 percentage points due to a 10 percentage point increase in the private school share. We do, however, not find any grade effect in the second/third year in high-school for this selective group of students who have chosen an academic track. 4) There is no evidence that an increase in the private school share (at the compulsory level) has an effect on subsequent university attainment or years of schooling. However, we cannot rule out the possibility that we would find significant positive effects also in the long-term were we able to incorporate additional (now too young) school cohorts in our analysis.

The basic finding from the study of school costs is that an increase in the private school share by 10 percentage points generates about 2 percent higher average municipal school costs.

The main findings from the segregation analysis are the following: 1) An increase in the private school share makes public schools lose pupils who are second-generation immigrants and/or whose parents have high education. 2) There is no evidence of increased sorting based on pupil's parental income or immigrant status.

Overall, we thus find evidence that the competitive forces unleashed by the 1992 school reform (at the compulsory level) in Sweden induced higher student achievement at the compulsory and high-school levels, but also higher costs and greater segregation. The question of whether more school 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 unit of money spent (although we consider our estimated achievement effects as fairly small). It is more complicated, and ultimately a question of values, to contrast the results for achievement with those for segregation.

Appendix: construction of the educational achievement, demographic and family background, and school level variables

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.

The school registers contain information about all schools in Sweden, which allow us to identify whether or not a school is private. 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.

Information on pupils' compulsory school-leaving grades in 9th grade is available for each subject included in the curriculum for school cohorts 1988-2003. We use 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. 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.

If a pupil has a grade in at least one of these subjects, it is included in the calculation of average grades for a school cohort in that municipality. About 5 percent of a birth cohort, and about 1 percent of the pupils who are registered in the ninth-grade registers in each school cohort, do not have any valid grade in any of these subjects in any year. Note that the few private schools that existed prior to the reform were then not required to report grades. 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). For some special pedagogy private schools (Waldorf, Montessori), grades are missing throughout the whole period (i.e., up to 2003). About half of these schools do not give the same type of grades as in other schools. Still, the grade scores used in these schools are converted to points in applications to high schools. Hence, students from these private compulsory schools can continue to high school. Unfortunately these numbers are not available in the grade 9 registry.

Information about pupils' high school grades is available in most subjects at the end of each course for school cohorts 1994-2003. We calculate a measure of achievement at the end of the first year of high school (for so called A courses), when courses in English, math, science (*naturkunskap*) and social science are compulsory for all high-school students. About 25 percent of the individuals in a school cohort do not have 1st year high school grades in any the four subjects that we study. This includes individuals who do not continue to high-school as well as high-school dropouts.

We also use a measure of grades at the end of high school which is calculated as the average of the grades in courses at higher levels in the same four subjects. These courses are normally completed at different points in time during the students' high-school years (normally 3 years), and are given at levels B and C in all four subjects and up to level E in math. We include only B and C courses since these are required in most theoretical study programs (academic tracks), while levels D and E are only required for students in the "natural science program". On the other hand, only level A is required for most non-academic tracks. Hence, this outcome is only defined for the selective sample of students who have chosen an academic high-school track.

The teachers' grading of each subject is on a 1-4 scale. However, grades were given on a 1-5 scale before 1994, and the grading system also changed from being a relative one to a criterion based one in this year. This new system was implemented gradually so that students who had already started the senior level of compulsory school, or the first year in high school, by 1994, continued to be graded according to the old scale. This implied that the school cohort of 1998 was the first to receive grades in 9th grade according to the new system, and the school cohort of 1994 was the first to receive grades according to the new system in high school. 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. 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.

When we calculate the average percentile rank of each pupils' grade scores, 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. 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. If pupils have missing grades in single subjects, we use their non-missing grades to compute GPA. 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 (1 to 5) at the lower level equals the grade (1 to 5) at the higher 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 were taught at only one level. Using alternative mappings do not alter the results. Note also that this is not an issue for the high school variables since we only use grade data for 1998 and onwards (i.e., school cohorts 1994 and younger) in the high school analysis.

Turning to the family background and demographic variables: 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.

Finally, we consider the school level variables. We use 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 average pupil-teacher ratios in schools and by the logarithm of total school costs per pupil. 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 (for all teachers, for certified teachers) are calculated as the number of pupils in the municipalities' compulsory schools divided by the number of full-time (equivalent) teachers. The school cost variables include all costs, i.e., costs for teachers as well as semi-fixed costs such as rents.

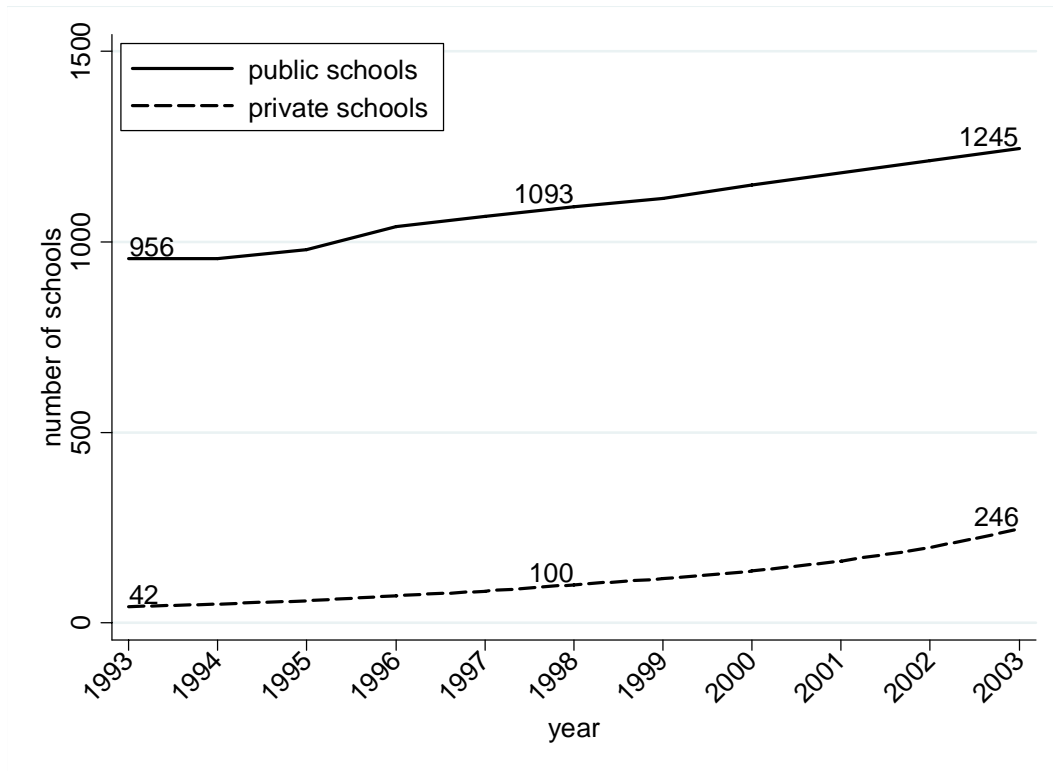
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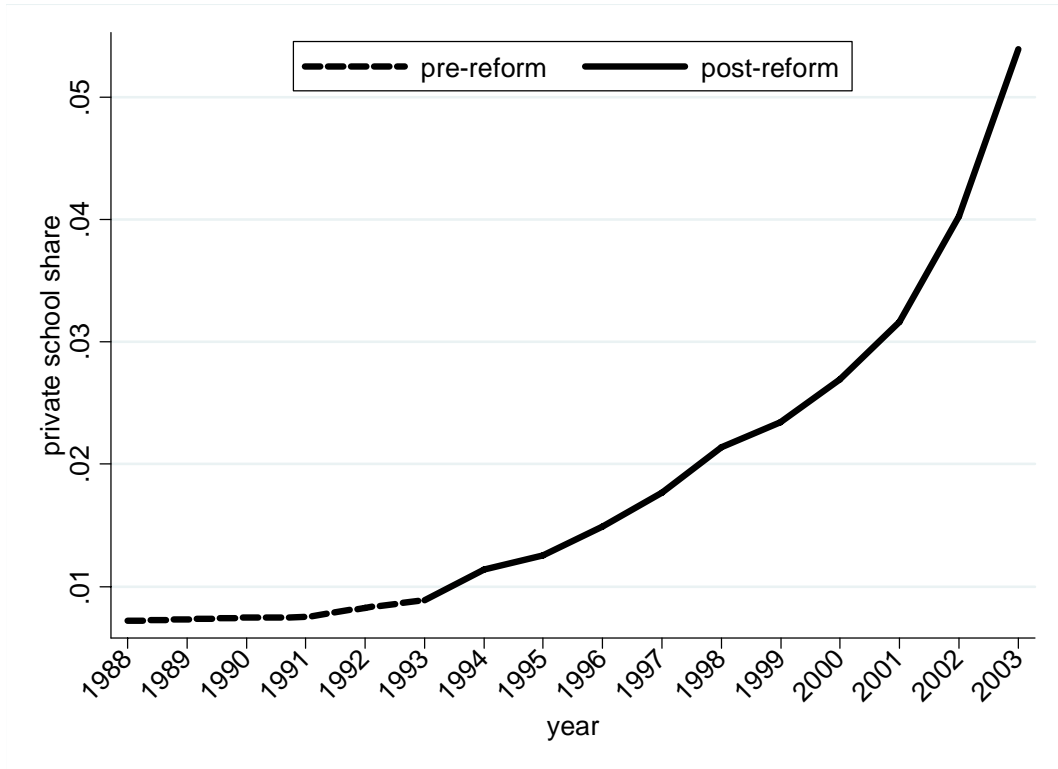
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Figure 1 The number of private and public schools with ninth grade pupils, 1993-2003



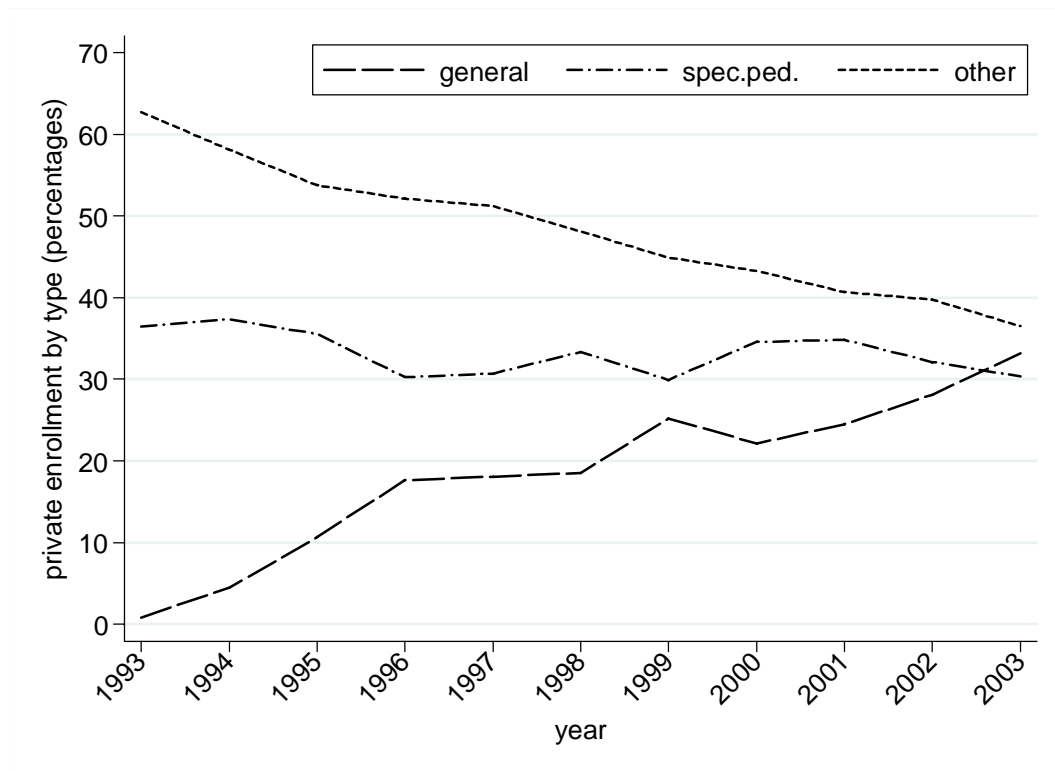
Note: Based on data from the school register (skolregistret).

Figure 2 The private school share 1988-2003



Note: The private school share displayed in the figure is based on nationwide total numbers of pupils in private and public schools. This data stem from registers held by the National Agency of Education.

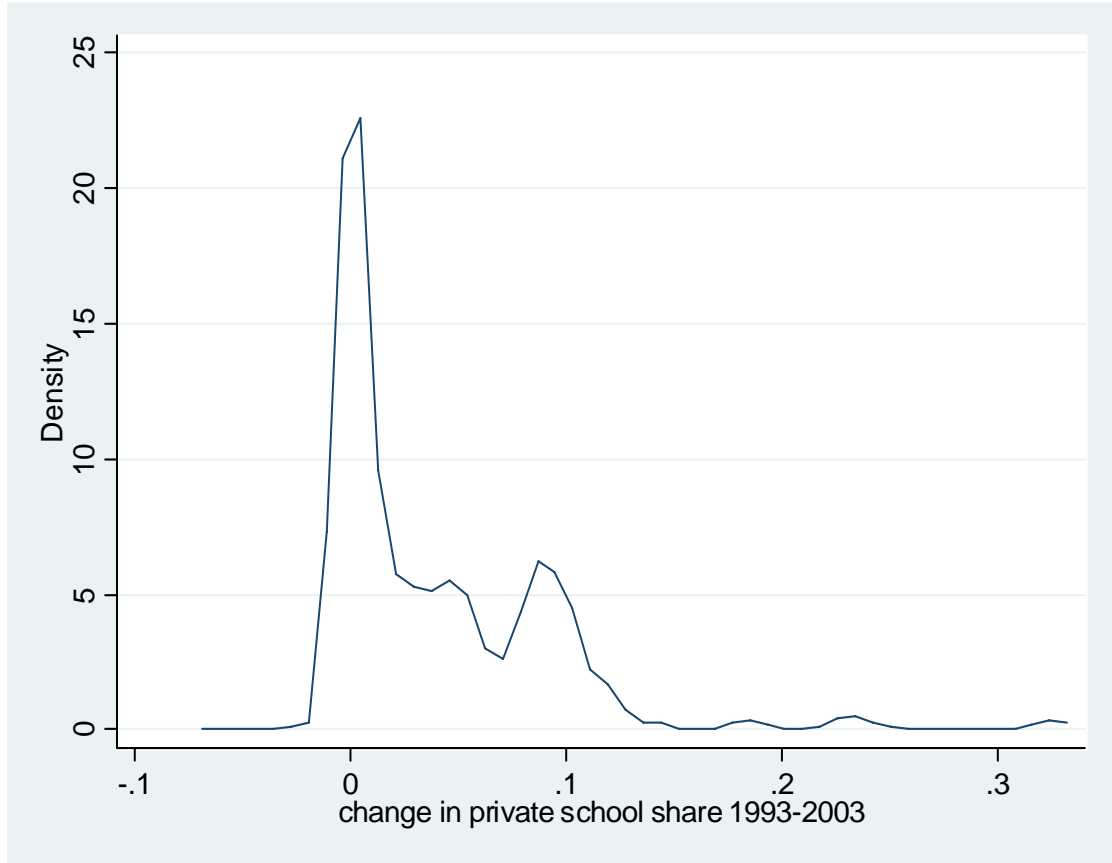
Figure 3 Private school enrollment 1993-2003, by type of private school



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

Figure 4 **Kernel density estimation of the change in the private school share between 1993 and 2003 (weighted)**

Weighted by nr pupils in 2003:



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 statistics, school cohorts graduating from the 9th grade in the post-reform years 1993-2003**

	(1) All pupils	(2) Private school pupils	(3) Public school pupils	(4) Munic. with private schools	(5) Munic. without private schools
Level of observation (weighted by N^i if aggr. level)	Municipal-year	Individual	Individual	Municipal-year	Municipal-year
N municipality-year cells (N^{mt})	3,124	-	-	570	2,554
N individuals (N^i)	216,841	5,037	211,804	90,507	126,334
[exceptions of N^{mt} and/or N^i within brackets]	Mean (St.dev)	Mean (St.dev)	Mean (St.dev)	Mean (St.dev)	Mean (St.dev)
Private school=1	0.023 (0.037)	1	0	0.050 (0.043)	0.004 (0.012)
<u>9th GRADE OUTCOMES</u>					
Observed with any grade=1 [$N^i=216,075$]	0.991 (0.012)	0.939 (0.240)	0.992 (0.090)	0.987 (0.011)	0.993 (0.012)
GPA (GPA0) [$N^i=214,074$]	49.88 (4.06)	58.58 (22.82)	49.71 (22.98)	50.78 (3.74)	49.24 (4.15)
Math grade (MA0) [$N^i=213,081$]	50.00 (4.60)	57.74 (27.23)	49.85 (26.76)	50.66 (4.00)	49.54 (4.94)
English grade (EN0) [$N^i=213,081$]	50.00 (5.04)	60.71 (27.23)	49.79 (27.19)	51.77 (4.45)	48.96 (5.17)
<u>HIGH SCHOOL OUTCOMES (defined for school cohorts 1994-2003)</u>					
Observed with any grade=1 [$N^{mt}=2,840$; $N^i=197,645$]	0.75 (0.08)	0.69 (0.46)	0.75 (0.43)	0.73 (0.06)	0.77 (0.09)
GPA at end of first year in HS (GPA1) [$N^{mt}=2,840$; $N^i=148,401$]	49.97 (4.57)	58.40 (21.57)	49.78 (21.49)	51.63 (4.09)	48.75 (4.51)
Math grade at end of 1 st year in HS (MA1) [$N^{mt}=2,840$; $N^i=147,795$]	50.09 (5.12)	57.09 (26.80)	49.93 (26.74)	51.21 (4.26)	49.27 (5.53)
English grade at end of 1 st year in HS (EN1) [$N^{mt}=2,840$; $N^i=147,063$]	50.04 (5.94)	61.18 (26.53)	49.78 (26.86)	52.22 (5.31)	48.44 (5.86)
Academic track in HS=1 [$N^{mt}=2,840$; $N^i=148,401$]	0.56 (0.12)	0.75 (0.43)	0.56 (0.50)	0.61 (0.09)	0.53 (0.12)
GPA at end of HS (GPA3) [$N^{mt}=2,833$; $N^i=83,682$]	50.03 (4.98)	55.17 (21.94)	49.87 (21.69)	50.77 (4.01)	49.40 (5.59)
Math grade at end of HS (MA3)	47.69 (5.95)	50.86 (26.33)	47.59 (25.76)	47.94 (4.52)	47.48 (6.93)

[N^m=2,833; Nⁱ=82,161]

English grade at end of HS (EN3)	48.71 (6.32)	56.83 (26.20)	48.45 (26.14)	49.99 (5.20)	47.63 (6.95)
[N ^m =2,833; N ⁱ =82,565]					

EDUCATIONAL ATTAINMENT OUTCOMES

20 credits =1 (At least 1 semester of university studies within 6 years after leaving 9 th grade in 1994-2000)	0.27 (0.07) [1,988; 134,589]	0.36 (0.48)	0.27 (0.44)	0.28 (0.06)	0.26 (0.07)
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80 credits =1 (At least 4 semesters of university studies within 7 years after leaving 9 th grade in 1993-1999)	0.17 (0.06) [1,988; 134,407]	0.22 (0.41)	0.17 (0.37)	0.17 (0.04)	0.17 (0.06)
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Years of schooling (8 years after leaving 9 th grade in 1993-1998)	12.41 (0.30) [1,704; 113,335]	12.74 (1.86)	12.41 (1.76)	12.39 (0.28)	12.42 (0.32)
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FAMILY AND DEMOGRAPHIC VARIABLES[†]

Male=1	0.51 (0.06)	0.49 (0.50)	0.51 (0.50)	0.51 (0.04)	0.51 (0.07)
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Parents' mean age	45.44 (0.97)	47.11 (5.34)	45.40 (5.06)	45.80 (0.86)	45.18 (0.96)
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At least one parent university educated=1	0.25 (0.10)	0.48 (0.50)	0.24 (0.43)	0.30 (0.10)	0.21 (0.09)
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At least one parent high-school educated=1	0.43 (0.07)	0.49 (0.50)	0.42 (0.49)	0.44 (0.05)	0.42 (0.08)
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Log family income	8.00 (0.12)	8.08 (0.65)	8.00 (0.51)	8.03 (0.12)	7.98 (0.12)
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Log family income is missing	0.05 (0.04)	0.06 (0.23)	0.05 (0.21)	0.07 (0.04)	0.03 (0.03)
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2nd generation immigrant	0.06 (0.06)	0.15 (0.36)	0.06 (0.24)	0.09 (0.06)	0.04 (0.05)
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Immigrant	0.07 (0.05)	0.09 (0.29)	0.07 (0.25)	0.10 (0.05)	0.05 (0.04)
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Immigrant age (if immigrant=1)	8.37 (2.42)	6.80 (4.38)	8.26 (4.11)	8.00 (1.59)	8.69 (2.91)
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SCHOOL LEVEL VARIABLES

Nr of 9 th grade pupils / 100	2.13 (2.85)	4.45 (4.15)	2.08 (2.80)	3.95 (3.66)	0.83 (0.56)
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Nr of public schools per 100 pupils	1.36 (0.55)	1.38 (0.46)	1.36 (0.55)	1.30 (0.44)	1.41 (0.62)
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Mean log distance to pupils' nearest publ. school ^a	7.27 (0.64)	6.80 (0.50)	7.29 (0.64)	6.93 (0.54)	7.53 (0.60)
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Nr of Pupils per teacher in the municipality	12.64 (0.98)	12.72 (0.86)	12.63 (0.98)	12.64 (0.83)	12.63 (1.07)
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Nr of Pupils per certified teacher in municipality	14.45 (1.72)	15.35 (1.66)	14.43 (1.72)	14.90 (1.60)	14.13 (1.74)
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Log school costs per pupil in the municipality ^b	10.96 (0.12)	11.03 (0.12)	10.96 (0.12)	11.01 (0.12)	10.93 (0.12)
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^a [N^m=3,031; Nⁱ=214,079], ^b [N^m=3,086; Nⁱ=215,213]

Notes: Our basic sample for this post-reform period consists of 216,841 individuals living in 284 different municipalities (and the number of municipality-year cells is 11*284=3124). These are all individuals in our 20% random sample that are registered in the grade 9 register (*årskurs9 registret*), which contains information about all pupils that leave compulsory school. A small fraction of these pupils leave compulsory school without any valid grade mark in any of the subjects that we include in this study. The group without grades consists basically of two groups of pupils: (1) "dropouts" (2) pupils in certain special pedagogy private schools that receive grades from another system than the ordinary one and that are reported as having missing grades (although this does not necessarily indicate failure). The former category is coded zero in the "observed with any grade dummy" and missing in the other 9th grade outcomes, while the latter group is coded missing in all 9th grade outcomes. The high-school outcomes are only defined for school cohorts 1994-2003. Individuals who are not observed with grades in high school are those who never started high school and high-school dropouts. The first-year courses in high school are mandatory for all students independent of program attended. The outcomes at the end of high school are only defined for the more selective group of students who take higher-level courses in academic fields. The educational attainment outcomes are defined for all students in the basic sample who belong to the school cohorts indicated in the table. The reason for studying restricted numbers of school cohorts is that we are only able to observe educational attainment up to 2006. Distance to school and school cost information is not available for all municipalities in all years (93 respective 38 municipal-year observations are missing). GPA denotes the average (percentile) grades in English, math, natural science and social science subjects at both the compulsory level and at the high-school level. [†]The following control variables are not listed in the table (but belong to this set): Mothers years of schooling; Fathers years of schooling; indicator variables for missing parental schooling; 6 dummies for immigrants region of origin.

Table 2 Aggregate private school share regressions

Dependent Variable:	<u>Private school share in 2003</u>				<u>Private school share</u>	
	<u>>=0 in 2003</u>		<u>>0 in 2003</u>		<u>1993-2003</u>	
Sample:	(1)	(2)	(3)	(4)	(5)	(6)
Fraction male students	0.034 (0.028)	0.024 (0.027)	0.055 (0.079)	0.033 (0.078)	-0.007 (0.009)	0.001 (0.008)
Mean parents' age	0.009 (0.004)*	0.009 (0.004)*	0.021 (0.009)*	0.022 (0.008)**	0.001 (0.001)	-0.000 (0.001)
Fraction families with at least one parent university education	0.053 (0.040)	0.052 (0.051)	0.018 (0.061)	0.028 (0.090)	0.072 (0.021)**	0.029 (0.012)*
Fraction families with at least one parent high school education	-0.006 (0.037)	0.005 (0.036)	-0.007 (0.076)	0.006 (0.078)	0.018 (0.012)	0.008 (0.008)
Mean log family income, only those with positive income for any parent	0.149 (0.043)**	0.150 (0.042)**	0.114 (0.072)	0.114 (0.073)	0.022 (0.014)	-0.015 (0.009)
Fraction of individuals with missing parents income	-0.539 (0.302)+	-0.586 (0.308)+	-1.096 (0.528)*	-1.150 (0.523)*	0.051 (0.061)	0.015 (0.039)
Fraction 2nd generation immigrants	0.079 (0.083)	0.083 (0.083)	0.120 (0.147)	0.140 (0.146)	0.096 (0.034)**	0.059 (0.028)*
Fraction immigrants	0.635 (0.269)*	0.628 (0.283)*	0.770 (0.363)*	0.750 (0.388)+	-0.007 (0.031)	-0.037 (0.025)
Number of 9th grade graduates / 100	0.003 (0.002)	0.002 (0.002)	-0.000 (0.002)	-0.000 (0.002)	0.003 (0.002)*	0.013 (0.003)**
Number of public schools/100 students	0.0004 (0.0064)	0.001 (0.007)	-0.006 (0.012)	-0.005 (0.012)	0.001 (0.002)	-0.000 (0.001)
Mean log distance to students' nearest public school	-0.002 (0.004)	-0.002 (0.004)	-0.012 (0.010)	-0.012 (0.011)	-0.001 (0.002)	-0.001 (0.002)
Pupil/teacher ratio in the municipality	-0.000 (0.009)	-0.002 (0.008)	-0.007 (0.015)	-0.009 (0.016)	-0.005 (0.002)*	-0.006 (0.002)*
Pupil/certified teacher ratio in the municipality	0.011 (0.007)	0.012 (0.007)	-0.021 (0.013)	-0.022 (0.013)	0.007 (0.002)**	0.007 (0.002)**
Log school costs per pupil in the municipality	0.038 (0.029)	0.030 (0.028)	0.030 (0.052)	0.022 (0.052)	0.004 (0.019)	-0.007 (0.021)
GPA0 in 1992		-0.0002 (0.0009)		-0.0006 (0.0021)		
The change in GPA0 between 1988 and 1992		-0.0006 (0.0008)		-0.0006 (0.0017)		
Private school exists in municipality prior to reform in 1993		0.009 (0.011)		0.005 (0.013)		
Year fixed effects	NO	NO	NO	NO	YES	YES
Municipality fixed effects	NO	NO	NO	NO	NO	YES
Independent variables defined for school cohorts	1992	1992	1992	1992	1993-2003	1993-2003
R2	0.56	0.57	0.47	0.48	0.47	0.72
N ^{mt}	278	278	107	107	2,994	2,994

Notes: All estimates are weighted, where the weights are the number of ninth grade pupils living in the municipality. In columns 1-4 the weights are from 2003, and in 5-6 for t. There are fewer observations in the regressions that since we do not have distance to public schools and cost information for all municipality-year cells. The standard errors (in parentheses) are robust in columns 1-4 and allow for clustering at the municipality level in columns 5-6. + significant at 10 percent; * significant at 5 percent; ** significant at 1 percent.

Table 3 **Regressions of the private school share on outcomes at the end of compulsory school. School cohorts: 1988-2003**

Main independent variable: Private school share in the municipality						
Dependent variables (municipality level averages):	(1)	(2)	(3)	(4)	(5)	(6)
Observed with grade marks from 9 th grade	-0.07 (0.02)**	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.02)	0.01 (0.02)
R2	0.09	0.19	0.25	0.28	0.33	0.34
GPA	30.44 (6.78)**	4.89 (3.43)	9.28 (4.15)*	8.27 (3.12)**	7.80 (3.34)*	6.32 (2.48)*
R2	0.05	0.43	0.48	0.57	0.55	0.63
Math grade	25.36 (6.48)**	5.14 (3.40)	9.33 (4.52)*	8.48 (3.58)*	4.68 (3.05)	3.30 (2.95)
R2	0.03	0.31	0.42	0.48	0.50	0.55
English grade	47.50 (8.89)**	6.72 (3.40)*	9.42 (3.95)*	9.43 (3.27)**	8.85 (4.23)*	7.49 (3.45)*
R2	0.08	0.46	0.51	0.58	0.57	0.63
Year fixed effects	YES	YES	YES	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	YES	YES
Municipal controls ^a	NO	YES	NO	YES	NO	YES
N ^{mt}	4,544	4,544	4,544	4,544	4,544	4,544

Notes: These high-school outcomes are generally observed 1 year after graduation from ninth grade for each school cohort. ^aFamily and demographic variables specified in Table 1. All regressions are weighted by the number of students with a non-missing outcome living in the municipality at the time they left compulsory school. 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 **Regressions of the private school share on first-year high school outcomes.**
School cohorts: 1994-2003

Main independent variable: Private school share in the municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables (municipality level averages):						
Observed with grade marks from high school (owg)	-0.26 (0.11)*	-0.05 (0.09)	0.04 (0.07)	0.04 (0.07)	-0.07 (0.08)	-0.08 (0.08)
R2	0.12	0.23	0.46	0.49	0.55	0.57
GPA at end of 9 th grade in compulsory school, if owg=1	39.96 (8.25)**	5.74 (3.97)	10.14 (4.81)*	7.46 (3.37)*	4.45 (4.18)	2.53 (3.18)
R2	0.13	0.41	0.53	0.57	0.69	0.70
GPA at end of 1 st year in high School, if owg=1	45.92 (8.56)**	8.14 (3.25)*	10.06 (4.15)*	7.66 (3.33)*	3.76 (3.06)	1.59 (3.04)
R2	0.14	0.45	0.56	0.61	0.63	0.67
Math grade at end of 1 st year in high school, if owg=1	30.07 (6.15)**	6.39 (4.16)	9.64 (4.27)*	7.69 (3.95)+	3.66 (4.54)	2.13 (4.51)
R2	0.05	0.25	0.44	0.47	0.53	0.55
English grade at end of 1 st year in high school, if owg=1	63.42 (12.21)**	5.95 (3.85)	13.10 (4.91)**	10.50 (4.42)*	5.74 (4.53)	3.05 (4.63)
R2	0.15	0.48	0.58	0.61	0.65	0.68
Year fixed effects	YES	YES	YES	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	YES	YES
Municipal controls ^a	NO	YES	NO	YES	NO	YES
N ^{mt}	2,840	2,840	2,840	2,840	2,840	2,840

Notes: These high-school outcomes are generally observed 1 year after graduation from ninth grade for each school cohort. ^aFamily and demographic variables specified in Table 1. All regressions are weighted by the number of students with a non-missing outcome living in the municipality at the time they left compulsory school. 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 Regressions of the private school share on second-/third- year high school outcomes. School cohorts: 1994-2003

Main independent variable: Private school share in the municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables (municipality level averages):						
Academic track in high school (AT)	1.24 (0.22)**	0.14 (0.11)	0.25 (0.08)**	0.24 (0.08)**	0.28 (0.12)*	0.23 (0.13)+
R2	0.17	0.44	0.59	0.62	0.66	0.68
N ^{my}	2,840	2,840	2,840	2,840	2,840	2,840
GPA at end of 9 th grade in compulsory school, if AT=1	8.13 (5.23)	-2.33 (3.70)	7.43 (4.70)	4.29 (3.49)	7.43 (4.70)	4.29 (3.49)
R2	0.62	0.62	0.62	0.62	0.62	0.62
GPA at end of 3 rd year in high school, if AT=1	21.94 (6.46)**	0.33 (3.64)	2.23 (3.89)	2.53 (3.97)	2.54 (4.92)	1.63 (4.67)
R2	0.03	0.18	0.34	0.36	0.43	0.45
Math grade at end of 3 rd year in high school, if AT=1	6.37 (5.43)	-3.11 (4.58)	-1.43 (5.40)	0.05 (5.38)	-0.66 (6.94)	-1.53 (7.01)
R2	0.02	0.09	0.32	0.33	0.41	0.42
English grade at end of 3 rd year in high school, if AT=1	41.95 (9.91)**	3.60 (6.99)	7.76 (5.13)	7.66 (5.48)	5.87 (5.89)	5.19 (5.70)
R2	0.06	0.24	0.39	0.40	0.49	0.51
Year fixed effects	YES	YES	YES	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	YES	YES
Municipal controls ^a	NO	YES	NO	YES	NO	YES
N ^{mt}	2,833	2,833	2,833	2,833	2,833	2,833

Notes: These high-school outcomes are generally observed 2-3 years after graduation from ninth grade for each school cohort. ^aFamily and demographic variables specified in Table 1. All regressions are weighted by the number of students with a non-missing outcome living in the municipality at the time they left compulsory school. 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 Regressions of the private school share on university attainment and years of schooling. School cohorts: a) 1994-2000; b) 1993-1999; c) 1993-1998

Dependent Variable: Private school share in the municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables (municipality level averages):						
a) 1994-2000						
GPA at end of 9 th grade in compulsory school	34.67 (7.97)**	6.89 (4.37)	8.73 (6.76)	8.21 (5.60)	8.60 (6.26)	6.82 (5.91)
R2	0.06	0.44	0.54	0.62	0.64	0.70
At least 1 semester of university studies within 6 years	0.39 (0.11)**	-0.05 (0.09)	0.14 (0.14)	0.10 (0.11)	0.13 (0.13)	0.11 (0.12)
R2	0.04	0.30	0.50	0.56	0.60	0.65
N ^{mt}	1,988	1,988	1,988	1,988	1,988	1,988
b) 1993-1999						
GPA at end of 9 th grade in compulsory school	37.28 (8.71)**	7.92 (4.83)	7.39 (6.32)	7.81 (6.02)	9.32 (6.44)	9.06 (6.21)
R2	0.06	0.43	0.54	0.61	0.64	0.69
At least 4 semesters of university studies within 7 years	0.17 (0.07)*	-0.02 (0.09)	0.02 (0.12)	0.02 (0.12)	0.09 (0.14)	0.09 (0.14)
R2	0.02	0.20	0.41	0.45	0.53	0.57
N ^{mt}	1,988	1,988	1,988	1,988	1,988	1,988
c) 1993-1998						
GPA at end of 9 th grade in compulsory school	38.23 (9.78)**	7.91 (4.86)	8.21 (6.59)	6.84 (5.55)	11.37 (7.93)	9.98 (8.42)
R2	0.05	0.43	0.54	0.62	0.65	0.71
Years of schooling within 8 Years	-0.68 (1.04)	-0.02 (0.71)	0.66 (0.66)	0.58 (0.65)	0.48 (0.97)	0.51 (0.96)
R2	0.04	0.33	0.58	0.64	0.67	0.72
N ^{mt}	1,704	1,704	1,704	1,704	1,704	1,704
Year fixed effects	YES	YES	YES	YES	YES	YES
Municipal fixed effects	NO	NO	YES	YES	YES	YES
Municipal specific trends	NO	NO	NO	NO	YES	YES
Municipal controls ^a	NO	YES	NO	YES	NO	YES

Notes: ^aFamily and demographic variables specified in Table 1. All regressions are weighted by the number of individuals with a non-missing outcome living in the municipality at the time they left compulsory school. 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 GPA regressions for different sub-groups**

Main independent Variable: The private school share in the municipality						
	(1)	(2)	(3)	(4)	(5)	(6)
Sub-group:	All	1 st or 2 nd generation immigrants	Low educated parents	High educated parents	Low family income	High family income
Outcomes:						
Observed with grade marks from 9 th grade	-0.01 (0.01)	0.00 (0.04)	-0.01 (0.02)	-0.02 (0.02)	-0.00 (0.03)	-0.03 (0.01)+
N ^{mt}	4,544	3,752	4,542	4,415	4,526	4,380
R2	0.28	0.19	0.21	0.17	0.25	0.11
GPA in 9 th grade	8.27 (3.12)**	7.99 (4.89)	3.79 (3.90)	14.75 (4.17)**	14.36 (4.66)**	9.60 (3.47)**
N ^{mt}	4,544	3,738	4,542	4,415	4,525	4,379
R2	0.57	0.29	0.21	0.21	0.23	0.27
Observed with grade marks from high school	0.04 (0.07)	0.08 (0.12)	-0.02 (0.12)	0.17 (0.09)+	-0.01 (0.12)	0.09 (0.10)
N ^{mt}	2,840	2,386	2,839	2,773	2,825	2,735
R2	0.49	0.25	0.38	0.26	0.34	0.26
GPA in 1 st grade in HS	7.66 (3.33)*	12.25 (7.24)+	3.26 (4.56)	5.50 (4.23)	13.18 (8.34)	-2.83 (3.07)
N ^{mt}	2,840	2,171	2,836	2,749	2,779	2,689
R2	0.61	0.32	0.21	0.30	0.23	0.35
Academic track in HS	0.24 (0.08)**	0.03 (0.23)	0.20 (0.16)	0.34 (0.10)**	0.27 (0.19)	0.25 (0.10)*
N ^{mt}	2,840	2,171	2,836	2,749	2,779	2,689
R2	0.62	0.33	0.29	0.28	0.34	0.32
GPA in 3 rd grade in HS	2.53 (3.97)	13.00 (10.60)	7.46 (8.58)	-2.46 (5.80)	11.92 (13.51)	-12.08 (5.55)*
N ^{mt}	2,833	1,777	2,711	2,645	2,495	2,538
R2	0.36	0.26	0.17	0.20	0.16	0.24
At least 1 semester of university studies within 6 years	0.10 (0.11)	0.18 (0.20)	0.11 (0.12)	0.21 (0.22)	0.11 (0.14)	0.02 (0.23)
N ^{mt}	1,988	1,662	1,988	1,941	1,977	1,907
R2	0.56	0.27	0.26	0.27	0.23	0.35
Year fixed effects	YES	YES	YES	YES	YES	YES
Municipal fixed effects	YES	YES	YES	YES	YES	YES
Municipal controls ^a	YES	YES	YES	YES	YES	YES

Notes: ^aFamily and demographic variables specified in Table 1. The dependent variables are calculated as the average for individuals belonging to the respective sub-sample. The sub-samples include, respectively, individuals: who are first and second generation immigrants (column 2); whose parents' have less than high school education (3); who have at least one parent having university education (4); whose family's income being in the first quartile (4); whose 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 8 **Re-estimations of the main achievement regressions, using various measures of the private school share, as well as the fraction of private schools as instrument for the private school share**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Obs with Grades in 9 th grade	9-th grade GPA	Obs with Grades in HS	1 st -year GPA	Academ. Track in HS	3 rd -year GPA	1 sem. at univ.	4 sem. at univ.	Years of school.
A.									
PSS in 9 th grade	-0.01 (0.01)	8.27 (3.12)**	0.04 (0.07)	7.66 (3.33)*	0.24 (0.08)**	2.53 (3.97)	0.10 (0.11)	0.02 (0.12)	0.58 (0.65)
R2	0.28	0.57	0.49	0.61	0.62	0.36	0.56	0.45	0.64
B.									
PSS in Grades 7-9	0.003 (0.008)	8.52 (2.92)**	0.06 (0.06)	9.65 (3.07)**	0.23 (0.08)**	3.11 (3.88)	0.04 (0.13)	-0.16 (0.12)	0.91 (0.53)+
R2	0.28	0.57	0.49	0.61	0.62	0.36	0.56	0.46	0.64
C.									
Avg. of PSS In 9 th grade	0.018 (0.012)	9.97 (4.58)*	0.16 (0.13)	8.55 (5.57)	0.38 (0.12)**	-4.18 (7.18)	0.05 (0.15)	-0.05 (0.18)	0.60 (0.80)
R2	0.28	0.57	0.49	0.61	0.62	0.36	0.56	0.45	0.64
D.									
IV: Fraction of private Schools	0.002 (0.016)	11.11 (4.12)**	0.09 (0.14)	14.25 (6.89)*	0.34 (0.17)+	-2.70 (9.00)	0.26 (0.30)	0.25 (0.29)	2.12 (1.84)
R2	0.28	0.57	0.49	0.61	0.62	0.35	0.56	0.45	0.63
N ^{mt}	4,544	4,544	2840	2840	2840	2833	1988	1988	1704
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mun. FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mun. contr. ^a	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: ^aFamily and demographic variables specified in Table 1. The main independent variable is the private school share in grade 9 (panels A and D), the private school share in grades 7-9 (B), and the average of the private school share in grade 9 over the last three school cohorts (C). In panel D, the fraction of private schools is used as an instrument for the private school share in grade 9. All variables are aggregated up to the municipal level and all regressions are weighted by the number of individuals with a non-missing outcome living in the municipality at the time they left compulsory school. 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 9 Aggregate “grade inflation” regressions, 2003

Dependent variable: Aggregate subject grade				
	(1)	(2)	(3)	(4)
	English	English	Math	Math
Private school share	-1.60 (2.23)	2.14 (2.51)	-0.08 (2.79)	-0.65 (4.13)
Mean of English test score	0.88 (0.02)**	0.89 (0.04)**		
Mean of Math test score			0.71 (0.04)**	0.71 (0.04)**
Municipal Controls ^a	NO	YES	NO	YES
N	277	277	265	265
R2	0.80	0.82	0.64	0.69

Notes: ^aFamily and demographic variables specified in Table 1. 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 10 **Re-estimations of the main post-compulsory school regressions, including the private school share (PSS) at the high-school level**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Obs with grades	1 st -year GPA	Academ. Track	3 rd -year GPA	1 sem. At univ.	4 sem. at univ.	Years of school.
PSS in 9 th grade	0.04 (0.07)	6.13 (3.25)+	0.24 (0.08)**	1.74 (3.95)	0.08 (0.11)	0.01 (0.12)	0.56 (0.64)
PSS in HS	-0.02 (0.04)	7.66 (1.76)**	-0.04 (0.06)	4.69 (2.65)+	0.05 (0.04)	0.05 (0.04)	0.06 (0.20)
N ^{mt}	2840	2840	2840	2833	1988	1988	1704
R2	0.49	0.61	0.62	0.36	0.56	0.45	0.64
Year FE	YES	YES	YES	YES	YES	YES	YES
Mun. FE	YES	YES	YES	YES	YES	YES	YES
Mun. contr. ^a	YES	YES	YES	YES	YES	YES	YES

Notes: ^aFamily and demographic variables specified in Table 1. All variables are aggregated up to the municipal level and all regressions are weighted by the number of individuals with a non-missing outcome living in the municipality at the time they left compulsory school. 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 **Aggregate GPA-regressions including proxies for competition between public schools and controls for school resources. School cohorts: 1993-2003**

	(1)	(2)
Private-school share	7.35 (2.93)*	7.20 (3.03)*
Nr of public schools / 100 pupils		-0.02 (0.16)
Mean log distance to pupils' nearest public school		-0.08 (0.20)
Nr of Pupils per teacher in the municipality		0.32 (0.17)+
Nr of Pupils per certified teacher in the municipality		-0.00 (0.13)
Municipal fixed effects	YES	YES
Municipal controls ^a	YES	YES
Observations	3,031	3,031
R-squared	0.61	0.62

Notes: All regressions control for year fixed effects. ^aFamily and demographic variables specified in Table 1. 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 number of observations in these regressions is less than 3,124 since we do not have the information about distance to public schools for all municipality-year cells. 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. School cohorts: 1993-2003**

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 (0.14)	0.22 (0.13)+	0.20 (0.13)	0.12 (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	0.50	0.84	0.84	0.86

Notes: All regressions control for year fixed effects. ^aFamily and demographic variables specified in Table 1. 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. School cohorts: 1993-2003. Estimates are from estimation of equation (5).**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High/medium relative to low parental income	High/medium relative to low parental income	High/medium relative to low parental education	High/medium relative to low parental education	Swedish relative to immigrant pupils	Swedish relative to immigrant pupils	Swedish relative to pupils who are 2 nd generation immigrants	Swedish relative to pupils who are 2 nd generation immigrants
Private school share	-0.05 (0.11)	0.03 (0.12)	-0.52 (0.09)**	-0.28 (0.12)*	-0.13 (0.12)	- 0.23 (0.15)	0.46 (0.09)**	0.39 (0.16)*
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Municipal fixed effects	NO	YES	NO	YES	NO	YES	NO	YES
Observations	3,108	3,108	3,121	3,121	2,279	2,279	2,031	2,031
R-squared	0.00	0.25	0.32	0.48	0.01	0.18	0.05	0.20

Notes: The dependent variables are defined as the differences of the average characteristics. For example, in columns 5 and 6, the dependent variable is the number of Swedish pupils in public schools relative to Swedish pupils in all schools minus the number of immigrant pupils in public schools relative to immigrant pupils in all schools. The mean (standard deviation) of the dependent variables are: 0.017 (0.034) in columns 1-2; 0.025 (0.043) in columns 3-4; 0.026 (0.063) in columns 5-6; and 0.032 (0.078) 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.

Table 14 **Aggregate sorting regressions with respect to socioeconomic background and immigration status. School cohorts: 1993-2003. Estimates are from estimation of equation (6).**

	(1) Parental Income	(2) Parental Income	(3) Parental Education	(4) Parental Education	(5) Immigrant pupil	(6) Immigrant pupil	(7) 2 nd generation immigrants	(8) 2 nd generation immigrants
Private school share	-0.011 (0.005)*	-0.005 (0.006)	-0.062 (0.015)**	-0.029 (0.011)**	0.182 (0.113)	0.111 (0.163)	-0.433 (0.103)**	-0.358 (0.150)*
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
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: 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.

