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## Does school privatization improve educational achievement? Evidence from Sweden's voucher reform

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Educational Achievement?  
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September 2008

# **Does School Privatization Improve Educational Achievement? Evidence from Sweden's Voucher Reform**

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## ABSTRACT

### **Does School Privatization Improve Educational Achievement? Evidence from Sweden's Voucher Reform<sup>\*</sup>**

This paper evaluates general achievement effects of choice and competition between private and public schools at the nine-year school level by assessing a radical voucher reform that was implemented in Sweden in 1992. Starting from a situation where the public schools essentially were monopolists on all local school markets, the degree of privatization has developed very differently across municipalities over time as a result of this reform. We estimate the impact of an increase in private enrolment on short, medium and long-term educational outcomes of all pupils using within-municipality variation over time, and control for differential pre-reform and concurrent municipality trends. We find that an increase in the private school share moderately improves short-term educational outcomes such as 9<sup>th</sup>-grade GPA and the fraction of students who choose an academic high school track. However, we do not find any impact on medium or long-term educational outcomes such as high school GPA, university attainment or years of schooling. We conclude that the first-order short-term effect is too small to yield lasting positive effects.

JEL Classification: I22, I28, H40

Keywords: private schooling, choice, competition, educational achievement

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The question of whether school choice improves the quality of schooling is hotly debated in many countries throughout the world. A central issue in this debate concerns the effects of letting families choose freely between public and private schools in relation to a setting where the public schools are local monopolists. There are two main reasons for why free choice could improve the overall quality of schooling. First, private schools may generally be better than public schools. There is an extensive literature on this question, and a number of papers have turned to quasi-experiments (e.g., voucher lotteries) in order to estimate private-school attendance effects.<sup>1</sup> Clearly, if private schools are better, average school performance should improve by reallocating pupils from the public to the private sector. Second, local monopolists may lack the right set of incentives for providing the best possible education, but are forced to improve when they are faced with competition. A competitive school market is also likely to be more diversified with schools that reflect the preferences and needs of different pupils. Competition may further stimulate innovation (with regard to organization and pedagogy) and create spillover effects within the school sector. Thus, school choice should improve the quality of education for both private and public school pupils and lead to improved overall educational outcomes. However, despite these arguments in favor of choice, the evidence from the literature evaluating general effects of school choice has been mixed.<sup>2</sup>

We re-assess the validity of these arguments by examining a nation-wide school reform implemented in Sweden in 1992. With its introduction of vouchers and free choice between public and private schools, and by fairly little of regulations restricting new schools to entry

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<sup>1</sup> The space is here too limited for a review of this very large number of studies. See Gill et al. (2007) and Howell and Peterson (2006) for two overviews of this literature.

<sup>2</sup> Hsieh and Urquiola (2006) estimate private school-choice effects from a large-scale reform that dramatically increased school choice in Chile during the 1980s. They find no effect on aggregate achievement, but they do find effects on segregation. The impacts of a large-scale school voucher reform in New Zealand in the early 1990s are evaluated by Fiske and Ladd (2000). Due to the absence of data on educational achievement, however, the best that could be done was to rely on the impact of the reform as perceived by teachers and principals. Clark (2007) evaluates a U.K. reform where high schools were allowed to become autonomous (but still publicly funded) schools. He finds large positive effects for these schools, but little spillover effects for neighborhood schools. Other studies have estimated achievement effects of choice and competition among public schools. Positive effects are found for the U.S. (Hoxby, 2000) and for Israel (Lavy, 2008), whereas Gibbons, Machin and Silva (2008) find no effect for the U.K.

the market, this reform was really designed to enhance competition between schools.<sup>3</sup> This, and the fact that it created a completely new sector of private schools, makes Sweden a very interesting country to study. The possibility to choose was very limited before the reform and post-reform increases in private schooling differs widely across local school markets.

We use this differential within-municipality variation that developed after the reform to answer the causal questions whether a higher incidence of private enrolment at the compulsory-school level impact: (1) overall pupil performance at the compulsory-school level? (2) overall medium-term and long-term educational achievement? Our estimates are thus aimed to capture the combined effect of any private-school attendance effects and any competition or spillover effects on individuals who attend any type of school. Note that only the former effect can be estimated using voucher lotteries. To estimate the overall effect, researchers need to utilize large-scale school-choice reforms or find credible instruments.

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 difference-in-difference (D-I-D) models generate unbiased estimates if unobserved 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 further provide evidence which suggest that differential pre-program trends in school performance do not explain the differential private school penetration across municipalities.

We use a large administrative data set on individuals graduating from the 9<sup>th</sup> grade in Sweden between 1988 and 2003. The richness of this data allows us to make several contributions to the literature. For example, we observe compulsory-school pupils for as many as 16 years, which makes it possible to exploit variation over a long time span after the reform as well as to test for differences in pre-reform trends between municipalities. We are

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<sup>3</sup> 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.

furthermore able to track these individuals over time (up to 2007). This makes it possible to examine the impact on medium and long-term educational outcomes.

The paper is organized as follows. Section 1 describes the 1992 voucher reform and the evolution of private schooling. Section 2 describes the data and examines determinants of private schooling. Section 3 discusses the estimation strategy and reports the main results. Section 4 reports results from robustness checks and additional analyses. Section 5 concludes.

## **1. The voucher reform and the development of private schooling**

There were three sweeping 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 the municipalities. Second, whereas every pupil was required to attend the public school in their neighborhood before 1992, pupils are now allowed to choose between public schools within the municipality. However, pupils who live closest to the school have priority, and choice is restricted to empty slots. For this reason, choice between public schools has remained quite restrictive in practice even after this reform.<sup>4</sup>

Third, the most radical reform was that starting in 1992 municipalities had 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.<sup>5</sup> Thus, the resources devoted to public schools are directly affected by the choices of pupils. To be eligible for public funding, private schools have to be approved by the National Agency for Education (NAE). These schools then have to follow the national curriculum and are not allowed to select pupils by ability, socio-economic characteristics or

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<sup>4</sup> 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 (p. 73, Swedish National Agency for Education, 2003)

<sup>5</sup> The minimum required funding percentage has changed over the years. The school year 1992/1993, it was 85 percent, and in 1995, 75 percent. It was less than 100 percent because of the extra costs involved for public schools regarding special education. In 1997, the system changed yet again: the size of the voucher should basically be equally large as the average cost per pupil in the respective municipality. Since 1992, however, the guiding principle has essentially always been that public and private schools should compete on equal terms.

ethnicity. If a school is oversubscribed, three selection criteria for admittance are allowed: proximity to the school; waiting list (where each child's place in line is determined by the date of the parents' application); priority to children who have siblings already enrolled in the school. Private schools are not allowed to charge any fees.<sup>6</sup> Hence, top-up funding over and above the voucher is not allowed. Local authorities can appeal against applications approved by the NAE, but the number of rejected applications has been small.<sup>7</sup> Nor are there any restrictions on the ownership structure of private schools eligible for public funding – whether religious, non-profit cooperatives, or for-profit corporations. Hence, the regulation does not constitute a great obstacle for new schools to enter the market and receive public funding.

Before 1992, 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 (42 schools with 9<sup>th</sup> grade pupils in 1992). Most of these schools were privately funded and some received state funding. However, the funding of public schools was independent of the number of pupils enrolled in private schools. Hence, the few alternatives that existed did not make up any competitive pressure on the public schools. Moreover, these schools attracted a rather special selection of pupils: they were boarding schools (attracting a small selection of upper class children); schools for pupils with special needs; international schools (mainly for foreign pupils); Christian-community schools; special pedagogy schools (e.g., Waldorf and Montessori).

The first wave of new private schools after 1992 was mainly made up of special pedagogy schools and also some religious schools and parent cooperatives. We may denote this initial wave of new schools as being founded by idealists, and a non-profit organization was the typical owner at the time. After the early reform years, the clearly strongest growth has been

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<sup>6</sup> A small category of schools at the compulsory level is under a different regulation. These are 3 old boarding schools (Gränna, Lundsberg and Sigtuna) that exist outside of the voucher system and charge high fees.

<sup>7</sup> For instance, in 2000, there were 153 applications to start a private school at the compulsory level the following school year. Of these, 13 were rejected for reasons such as: the application was incomplete, the school was expected to not provide sufficient educational standard, or the owner was financially instable. Of these 13 applications, only 2 were denied because of an expectation that this private school would lead to negative effects for the public schools in the municipality (NAE 2001).



in private schools of a general profile. This new type of private schools, similar to the public schools in terms of educational profile, have increasingly gained market shares and are now the most common type of private school. Contrary to the first wave, they compete by other means than offering something that is distinctly different from what is generally available in public schools. These schools were typically opened up by principals or teachers from the public school sector (for whom the main incentive was not profits, but instead a desire to realize their ideas of how a school should be run) or by for-profit school corporations, which started to establish themselves on the market in the mid 1990s (today the typical owner is a joint-stock company). The number of private schools with 9<sup>th</sup> grade pupils increased six-fold during the ten years between 1993 and 2003, from 42 to 246 schools.

Figure 1 shows the evolution of private schooling at the compulsory level in Sweden between 1988 and 2003. The line shows the fraction of pupils who attended a private school in the 9<sup>th</sup> grade in a given year. Only a small fraction of pupils, around 0.5 percent, attended private schools before the reform in 1992. After the reform, there was a sharp increase in the private school share, and by 2003 it had increased to above 5 percent.

The penetration of private schooling has differed greatly across municipalities. 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. Yet, other municipalities have faced substantial increases in private-school enrollment. Among the municipalities where any private school (with 9<sup>th</sup> grade pupils) existed in 2003, which was the case in 93 out of 284, the average private school share was 9 percent, where the municipality with the largest share had 39.4 percent of its pupils in private schools.

In Figure 2, we use weighted Kernel density estimation to show the distribution of the municipality-specific changes in the private school share between 1993 and 2003. 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.

This differential increase in private schooling across municipalities constitutes a useful source of variation which we can exploit to net out time-invariant factors that are related to both the key independent variable and the outcome. Thus, if all determinants of private-school penetration are fixed-municipal effects, we are able to identify the effects of interest just by using the panel-dimension of the data to associate changes in the incidence of private schooling to changes in educational achievement. However, there may also be relevant factors that change over time, and failing to control for these may lead to both positive and negative biases. For example, if the demand for private schooling is increasing in some municipalities due to a trend of high educated parents moving in, this would likely lead to a positive bias. On the other hand, private schools might tend to open up in areas where public-school quality is trending downwards. This would instead understate the impact on student outcomes. It is therefore very important to identify such confounding factors and try to control for them.

They who probably know most about what factors are important are the school companies. We therefore performed interviews with leading representatives for the 4 largest Swedish school corporations at the compulsory level (*Kunskapsskolan; Vittra; Pysslingen; Ultra*). We asked what municipal characteristics are important when they consider to opening up a new school. The answers we got clearly point at two main factors. One, the attitudes to privatization among local politicians' and voters' are considered as most important. This is not surprising since these attitudes reflect the demand for private schooling. However, even if

a substantial share of parents in a municipality demand private schooling for their children, the attitudes of the majority may matter much for the availability of private schools. First, the regulation implies some arbitrariness in how the size of the voucher is determined by municipal authorities (see, e.g., SOU, 2008). Pro-/anti- privatization authorities may thus offer slightly better/worse economic terms for the private schools (in relation to the level of cost in the municipality). Second, local authorities may facilitate/obstruct the entry of private schools by supplying/not supplying school buildings on good terms. Third, entry may be delayed by local authorities appealing against approved applications (even though their power to finally prevent entry is limited). Non-socialist governments are generally friendlier toward private schools than socialist governments. This is a fixed effect in municipalities with stable majorities, but possible changes in opinions can be proxied for by controlling for changes in political majority. The second main factor is the potential market share in the municipality. The potential market share is determined by the demand for private schooling among families and the competition from other already existing private schools, as well as the size of the pupil population. Socio-economic characteristics and immigrant density are potential correlates to the demand for private schooling (and also to political majority), and changes in these might thus be important to control for (together with changes in market size).

Surprisingly, the performance in public schools was considered as a criterion for entry by only one out of four companies, and it was not ranked as one of the most important ones. Still, we cannot neglect the possibility that the demand and supply of private schooling is greater in municipalities with downward-sloping trends in public-school quality. Hence, we try to examine this potential source of bias carefully.

## **2. Data, descriptive estimations and determinants of private schooling**

### *2.1. Data and variable construction*

Our data set consists of approximately 20 percent of all individuals finishing the 9<sup>th</sup> grade of compulsory school (normally at age 16) each year from 1988-2003. Information on school grades and educational attainment are available for all pupils from nationwide registers. We also have access to detailed 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 9<sup>th</sup> grade and in high school, and the region of residence of each pupil (at the time of 9<sup>th</sup> grade attendance) as well as the regional location of the school. Information about which school a pupil attended in grade 1-8 is, however, not available. The school registers contain information about all schools in Sweden, which allow us to identify whether a school is privately or publicly operated. Henceforth, we use the term school cohort to denote the cohort of pupils who leave the 9<sup>th</sup> grade in a certain year.

We observe grade scores at three points in time during the individuals' schooling: at the end of 9<sup>th</sup> 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: math, English, sciences and social sciences.<sup>8</sup> Information on pupils' grade scores in 9<sup>th</sup> grade is available for school cohorts 1988-2003.<sup>9</sup> The 9<sup>th</sup> grade scores are the main measures of a pupil's performance in the last 3 years of compulsory school and high school track admittance is entirely based on these grades. Information about pupils' high school grades is available for school cohorts 1994-2003. We use grades at the end of the first year of high school, when courses in core subjects are mandatory for all high school students, as well as grades at the end of high school in higher-level courses which are required in most theoretical study programs (academic tracks).

To make sure that grade scores are comparable across pupils (in any given year), the National Agency for Education distribute national tests in main subjects as well as issue

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<sup>8</sup> 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.

<sup>9</sup> The few private schools that existed prior to the reform were then not required to report grades. When we calculate mean GPA(9) before 1993, we therefore simply ignore the pupils in these schools. Also, for some special pedagogy private schools (Waldorf, Montessori), GPA(9) are missing throughout the whole period.

guidelines to teachers that spell out the specific criteria a pupil must meet in order to qualify for a certain score. Still, subjective grade setting by teachers could potentially constitute a problem for us if differential grading standards have developed between municipalities with differential private school penetration. We examine this issue in section 4.2.<sup>10</sup>

For the respective school level, we first convert the score to a percentile rank based on the distribution of scores in each subject for each school cohort in the whole country. We then use the average percentile rank of each pupil/student as the main measure of individual academic achievement. It is enough for a pupil to have grade in at least one of the core subjects to be included in the calculations. This gives us three GPA measures which we denote GPA(9), GPA(HS1) and GPA(HS3). Using raw scores gives very similar results.<sup>11</sup> We also define three dummy variables indicating if grades are available at these occasions.

We use two measures of long-term educational achievement: a dummy for having completed at least one semester of university education within 6 years after leaving compulsory school (about at the age of 22); Years of schooling within 8 years after leaving compulsory school (about at the age of 24). For these outcomes we study the school cohorts 1988-2001 and 1988-1999, respectively. The availability of data up to 2007 naturally limits the use of the youngest cohorts when studying these variables. Note that these variables are based on all individuals in a school cohort, no matter if they ever continued to or completed high school after leaving compulsory school.

The key independent variable is the share of 9<sup>th</sup>-grade pupils living in a municipality who attend private school inside or outside the municipality's borders. Few pupils choose to attend school in another municipality, but they who do bring their voucher from the municipality of

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<sup>10</sup> We would have preferred to complement our grade analysis with test scores. But these are only available for a few years and for a selective sample of schools. The exception is 2003, for which year test scores are available for most compulsory school pupils in math and English. Correlating 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).

<sup>11</sup> The reason to use percentile rank instead of raw scores is that we are forced to use grades from two different grading systems for the 9<sup>th</sup> grade (from a relative to an absolute system starting with the 1998 school cohort), where transformation of scores across systems not is straightforward. By using percentile ranks conversion, we use the distribution of scores for each school cohort to combine the two. For high school grades, we only use data from the same system. We can therefore compare standardized estimates from using raw scores and percentile ranks. We find very similar effect sizes.

residence. We calculate this measure for each year and municipality. All variables are aggregated up to municipality-year level by school cohort, and hence based on individuals residing in a municipality at the time they leave compulsory school no matter where they later live. Thus, we can really look at the overall impact of the private school share at the compulsory level for the very same individuals later in life. The key independent and dependent variables are all listed in Table 1 along with sample characteristics for school cohorts 1988-2003. The set of controls used in the main estimations is also listed in this table.

## *2.2. Descriptive estimations*

Table 2 displays estimates of mean changes over time in key variables. Results for the post-reform period (1993-2003) are shown in columns 1 and 2, and results for the pre-reform period (1988-1992) are displayed in columns 3 and 4. For each period, we show separate statistics for two groups of municipalities defined on the basis on whether they had any private schools (with 9<sup>th</sup> grade students) in 2003. We label the group of municipalities who eventually established any private school, the treated group, and those who never opened up a private school, the control group. Numbers are in bold typeface if they are statistically significantly different across treated and controls in a period. We have two main purposes with these estimations. First, we use them to illustrate parts of the source of variation that are later used in our D-I-D estimations.<sup>12</sup> Second, we investigate threats to this technique by investigating if pupils attending school in treated municipalities experienced a different trend in educational achievement prior to the reform. If so, there is a risk that the simple D-I-D technique will lead to biased estimates of private school share effects. In addition, we study if various school variables have changed differently between treated and control municipalities.

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<sup>12</sup> If we instead compare levels of variables between treated and control municipalities, it is clear that individuals in treated municipalities on average have higher GPA, and are more likely to choose academic tracks and to attend university. However, there is no difference in years of schooling. We also note that the treated group consists of individuals that are more likely to be immigrants and having higher educated parents. These municipalities are also much larger, indicating that private schooling mainly is an urban phenomenon, and are less likely to have a socialist municipality government.

We first consider the educational achievement variables.<sup>13</sup> The post-reform changes are statistically similar across treated and control municipalities, with two exceptions; the fraction observed with any grade mark from the 9<sup>th</sup> grade has decreased less and mean GPA(HS1) has increased more for students in treated municipalities.<sup>14</sup> The numbers in the last two columns are for school cohorts not exposed to any private school competition, since they left compulsory school before the reform was introduced. Changes in educational achievement between these school cohorts should therefore not differ systematically between these two categories of municipalities. This is also what we find. Changes in the educational outcomes (that are observable this period) are always statistically identical between categories.

Finally, we consider changes in the following school characteristics: number of public schools per 100 pupils; mean logarithm of distance to nearest public school (for the pupils residing in the municipality); average pupil-teacher ratio; logarithm of total school costs per pupil. The first two variables are supposed to capture the degree of competition between public schools in the municipality and the latter two possible consequences of the decentralization of school financing, the two other main school reforms in the early 1990s. If these variables have evolved similarly after the reform in municipalities with and without private schools, it is less likely that estimates of the effects of private schooling would be influenced by consequences of the “other” school reforms. This is also supported by the data, since these variables all are statistically similar across categories. Interestingly, the increase in the number of public schools (with 9<sup>th</sup> grade) since 1993 and the mean proximity to the nearest public school have been distributed equally across categories. This indicates that the establishment of new private schools not has been followed by closures of public schools.

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<sup>13</sup> From a comparison of the numbers in columns 1 and 2 in row 1, we also see that a small fraction of pupils who are living in municipalities without private schools attend a private school in another municipality.

<sup>14</sup> Note that simple D-I-D estimates of this crude comparison between treated and untreated municipalities can be obtained by dividing the differences in the changes of the respective outcome with the differences in the changes of the private school share. We do not show these estimates here for the sake of space.

This is at odds with one of the main ideas of increased school competition, which is that the worst schools will be forced to close down and be replaced by new more efficient schools.<sup>15</sup>

In this section we have compared changes in characteristics of treated and untreated municipalities over time. In later sections we develop this into a multivariate regression framework where we also allow treatment intensity to vary within and between the municipalities which we here crudely have categorized as treated.

### *2.3. Explaining changes in private school enrolment across municipalities*

In this section we examine potential determinants of differences in private school penetration across municipalities. We start by estimating the association between whether or not a private school (with levels 7-9) is opening up in a municipality in a given post-reform year and a number of characteristics. The estimates are displayed in column 1 of Table 3. The only variables that are statistically significant predictors of entry are the fraction of parents with university education, second generation immigrants and the size of the pupil population, which all enter with positive sign. Right-wing government is positively associated with private entry, but not really significant.<sup>16</sup> Importantly, mean student achievement and the school variables are all insignificant. In another specification we tested if the lagged number of existing private schools in the municipality matter for entry. We found that it did not.

Next, we regress the private school share in 2003 on a number of variables determined in 1992, the year before the reform was implemented.<sup>17</sup> If the differential development of private schooling across municipalities can be explained by observable characteristics determined prior to the implementation of the reform we need to adjust for this in our later estimations of

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<sup>15</sup> Family and demographic variables are not included in Table 2. Similar comparisons of these reveal that private-school municipalities have had a smaller increase in the fraction of high school educated parents and family income. They further have had a larger inflow of immigrants and of more pupils in general. Hence, it is important to control for these variables in the later estimations.

<sup>16</sup> The political majority variables are lagged 3 years in all estimations throughout this paper.

<sup>17</sup> Note here that the level of private schooling in 2003 basically is the same variable as the change in private schooling between 1992 and 2003 since no voucher-funded private schools existed before the reform.



private school share effects on educational achievement (by including controls for background characteristics and municipality fixed effects). Weighted estimates are reported in column 2.<sup>18</sup> Some variables measured in 1992 are very strong predictors of growth in private schooling until 2003. The increase in private schooling is positively related to the pre-reform values of mean family income, the fraction of immigrants in the municipality and the municipality being run by a right-wing local government. Also, the higher the school cost per pupil at the time of the reform implementation the larger the increase in the private school share in the post-reform period. 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.<sup>19</sup>

Finally and most important, we have included 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 GPA(9) in 1992 and the change in mean GPA(9) 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 with public school pupils who's performance have changed a lot during the last five pre-reform years. This result is very important since one might make the argument that private schools mainly were established in municipalities with failing public schools. Reassuringly, nothing in these estimations, neither in regression 1 or 2, supports such an assertion.

### **3. Estimating the effects of the private school share on educational achievement**

#### *3.1. Estimation strategy*

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<sup>18</sup> 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 specifications, non-weighted estimations yield results that are very similar to our weighted estimations.

<sup>19</sup> If we regress the private school share in 1993-2003 on variables measured in the same years, a higher private school share is more likely to be found in municipalities with more parents with university education, a higher mean family income, a higher fraction of second-generation immigrants and with a large student population. The general picture is basically unchanged if we add municipal fixed effects, although the estimate for mean family income switches sign. 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.

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

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

Where  $\bar{y}_{mt}$  denotes the average educational outcome for pupils residing in municipality  $m$  at time  $t$ ;  $\bar{P}_{mt}$  denotes the share of pupils residing in municipality  $m$  and who attend a private compulsory school in any municipality at time  $t$ ;  $T$  is a reform dummy (=1 after 1992 and =0 before the reform);  $\bar{X}_{mt}$  are observable factors measured at the municipality level;  $\gamma_m$  and  $\alpha_t$  represent municipality- and year-specific effects respectively;  $\delta_m \cdot \text{trend}$  represents municipality specific effects interacted with a linear time trend; and  $\varepsilon_{mt}$  is a random error term. We allow the regression error to be correlated within municipalities over time. The set of time-varying covariates,  $\bar{X}_{mt}$ , is included in order to control for relevant changes within municipalities and in their composition of pupils (the most important ones: political majority, size of pupil population and various family background variables are listed in Table 1). The municipality-specific linear trends,  $\delta_m \cdot \text{trend}$ , are included in order to control for unobservable variables that varies linearly 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 medium/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).

It is important to note that we use variables averaged over both private and public school pupils. To study public school pupils separately would generate inconsistent estimates of private school competition since it is not a random selection of pupils who remain in public

schools (Hsieh and Urquiola, 2005). Hence, by including all pupils, we are able to control for sorting in our estimations under the assumption that pupils with varying characteristics benefit equally much from interacting with better peers, i.e., that peer effects are linear.<sup>20</sup>

### 3.2 Main results

Table 4 reports the results from estimating different versions of model (1). All Columns show estimates from models where we have included the full set of municipality and year (school-cohort) indicators. The models underlying the estimates in columns 3 and 4 also include municipality indicators interacted with a linear time trend. The set of time-varying controls is included in the regressions that produce the estimates reported in columns 2 and 4.<sup>21</sup>

We first consider if an increase in the private school share has an impact on the 9<sup>th</sup> grade outcomes. These estimates are shown in panel a. We find no impact on the fraction of pupils with grade marks, which ensures that sample selection bias will not be an issue at this level. However, we find evidence of a positive and statistically significant effect on GPA in the 9<sup>th</sup> grade, an effect that is very robust across the four specifications. This estimate can be interpreted thus; an increase in the private school share by 10 percentage points is expected to increase average GPA by nearly 1 percentile rank point. We consider this a fairly small effect.

Next we proceed by studying if this effect pertains for high school outcomes. These estimates are displayed in panel b. In the first row we estimate 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 find no effect. Hence, sample

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<sup>20</sup> 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 more) at the high school level in Sweden.

<sup>21</sup> For models without municipality indicators and without controls, we find that a higher private school share is positively associated with GPA at all levels and with the likelihood of choosing academic track in high school and starting university education, negatively associated with having grades in compulsory and high school. We find no association for years of schooling. The positive estimates are throughout much larger than the estimates in column 1 of Table 4. For example, the estimate (standard error) is 30.10 (6.68) for GPA in 9<sup>th</sup> grade. Adding controls, the estimates become much smaller throughout and are never statistically different from the estimates using municipality fixed effects in columns 1 and 2.

selection does not seem to be an issue at this level either, when we next consider grades. The first-year high school GPA estimates are statistically significant and of the same magnitude in columns 1 and 2. However, they get smaller and statistically insignificant when we control for municipal specific linear trends in columns 3 and 4. We conclude from this that the positive effect for the 9<sup>th</sup>-grade GPA does not seem to persist to the next study year.

Recall that an advantage with the first-year grades is that they are from courses that are mandatory for students in all regular study programs, 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 using a more selective sample of students, namely those who have chosen an academic track. Examining the impact of private schooling on the fraction of students who chooses academic tracks (among high school students), 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, a result which is stable across all columns. However, we do not find any effect on GPA at the end of the 3<sup>rd</sup> year in high school. This finding is also robust for adding the fraction of students choosing an academic track as a control variable in these regressions (to control for non-random selection into our sample of academic high school students). Thus, we conclude that the positive effects that we have found for the 9<sup>th</sup> grade vanish over time.

Next, we consider the impact on long-term educational performance in terms of university attendance and years of schooling (note that these are based on all former pupils in the respective school cohort). These estimates are shown in panel c-d. We find statistically insignificant estimates throughout for the long-term outcomes. This is pretty much in line with what one would expect, given the results for high school GPA (although the positive estimates for academic track attendance induce expectations in the other direction).

Note that the estimates are very similar across specifications for most outcomes in Table 4.<sup>22</sup> This suggests that including municipality indicators is a sufficient way of controlling for differences across municipalities. Thus, observed time-varying factors and unobserved factors that evolve linearly over time do not seem to impact our D-I-D estimates. We also note that the stability across specifications holds when estimating models with data including several pre-reform school cohorts (i.e., all estimations in panels a, c and d). Hence, our estimates are not sensitive to differential pre-reform trends in unobservable variables.<sup>23</sup> The only estimates that change a lot depending on whether municipality-specific trends are included are the ones for GPA in first-year of high school.<sup>24</sup>

Note that we use restricted samples for all medium and long-term educational outcomes (panels b-d) compared to the 9<sup>th</sup>-grade analysis in panel a. Since an increase in the private school share is likely to have a first-order effect on 9<sup>th</sup> grade school performance, we have re-estimated the GPA(9) regressions using the very same school cohorts (and individuals) as in the restricted samples for comparison. These results are very similar to the baseline estimates reported in panel a. We have further converted all coefficients to standardized effect estimates. This revealed that the 9<sup>th</sup> grade GPA estimates (using comparable samples) always are larger in magnitude than the medium and long-term estimates. Hence, it seems fair to

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<sup>22</sup> We have also performed various tests if the main estimates are robust in other dimensions: (1) In section 2.3 we showed that there is no association between having had a private school before the reform and the private school share in 2003. Still, we have re-estimated using a sample omitting these 33 municipalities for all years. The estimates are not affected. (2) We have carried out a test of constant effects, by including quadratic terms in the GPA(9) regressions. We can never reject linearity of the effects, but the standard errors for the quadratic terms are very large. (3) We have tested for heterogeneity across sub-groups (by: immigrant status; low and high educated parents; low and high family income) using all outcomes. We can never reject equality of the effects across groups. Importantly, 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 (the estimates for these groups are mostly positive and never statistically significantly negative).

<sup>23</sup> We also performed a more direct counterfactual test of the importance of pre-program trends. This was done by regressing mean GPA(9) in 1988-1992 on municipality and year indicators, as well as municipality indicators interacted with a linear time trend, then predicting mean GPA(9) 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. The argument is that the private school share should have no impact on grades, using only the part of the variation in grades that stem from the pre-reform development. If we would find positive and statistically significant effects comparable to those found in our main results in table 3, we would conclude that those estimates are entirely driven by municipality differences in pre-reform trends in mean student achievement. However, this exercise always produced statistically insignificant (but imprecise) estimates: 4.57 (6.09) without the set of covariates, and 4.41 (6.72) with covariates.

<sup>24</sup> One argument against controlling for region-specific trends when using only post-reform years, which we do in the high school analysis, is that if a large fraction of individuals who previously were restricted in their choice suddenly act, it may create a spike in the outcome variable. This may lead to biased estimates (Wolfers, 2006). However, when using the very same restricted sample with GPA(9) as the outcome, we estimate coefficients that are robust across specifications.

conclude that the positive first-order effect on 9<sup>th</sup>-grade performance vanish over time. It is probably too small to yield lasting effects on subsequent educational achievement.

#### **4. Robustness checks and additional analyses**

##### *4.1. Sensitivity analyses*

In Tables 2 and 3 we introduced variables aimed to capture the degree of competition between public schools and the decentralization (and current level) of school financing. Since these variables probably are simultaneously determined with private schooling, we did not control for them in our main estimations of private school share effects in Table 4. However, omitting controls for public school competition could lead to biased estimates if the reform generated differential changes in such competition, and that this differently affected the incentives to opening up private schools in different municipalities. There is then a risk that our private school estimates capture effects of public school competition as well. It may also be important to control for school resources. For example, municipalities with increasing school budgets may attract more private schools. We therefore added the four school variables to the fixed effects specifications of model (1). Reassuringly, they have virtually no impact on the estimates of the private school share on educational outcomes. The reason is that changes in these variables are uncorrelated with changes in the private school share.

Next we examine heterogeneous treatment effects. We have thus far assumed a constant effect of private schooling. However, there are reasons to believe that effects can vary across time in two dimensions. First, effects can vary across school cohorts. For example, the character of choice and competition may have changed over time (type of treatment) since the types of new private schools established and the characteristics of pupils attending them have changed over time. Second, the effects may differ across school cohorts within municipalities depending on how long a school cohort have been exposed to private schooling. Perhaps it

takes time for competition to start having an effect (since it takes time for poor public schools to close down), or perhaps effects are largest soon after the first private school is established (since it is the introduction of competition that has the largest impact).

We examine this issue using the outcomes 9<sup>th</sup>-grade GPA and university attainment, two variables which are available also for the pre-reform years. The results are displayed in Table 5. In panel A we show results from estimating a version of model (1) where the effects are allowed to differ for four groups of school cohorts: 1993-1996, 1997-1999, 2000-2001 and 2002-2003. The 1988-1992 cohorts, who never were exposed to the voucher program, constitute the reference category. We find that the GPA estimates increase for later school cohorts, whereas this does not seem to be the case for university attainment. In panel B we investigate the issue of heterogeneous effects based on years since entry of the first private school (with 7-9 grade levels) in the municipality. We allow for separate effects for three groups; 1-2, 3-4 and 5-11 years since entry of first private school. We find that there is a clear tendency that the GPA estimates increase with years since first entry. This is in line with the argument that it takes time for competition effects to have an impact on public schools. On the other hand, the pattern that appears for university attainment lends no support for this. One explanation for these different patterns for 9<sup>th</sup>-grade GPA and long-term achievement is that generous grading standards may have become practice in municipalities with a long period of private schooling. However, this is not supported by the analysis of grade inflation that we perform in the next section.<sup>25</sup>

#### 4.2. *Why do we find short-term effects, but not long-term effects?*

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<sup>25</sup> We have also performed estimations using additional private school share measures. First, we used 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. Second, we used 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 may also capture effects of a delayed reaction of schools to increased competition. Third, we used the fraction of private schools as an instrument for the fraction of private school pupils. Hence, we only utilize the variation that stem from schools opening up or closing down. None of these estimations did produce estimates that were significantly different from the ones in Table 4.

A possible explanation for positive effects on short-term outcomes but no effect in the long term 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 is that high average grades might work as a signal that their school is a successful one. If grades are systematically inflated in municipalities where the private school share has increased much, we should not interpret our results for GPA(9) as genuine achievement effects.

In order to investigate this issue, we need both teachers' grades and scores on achievement tests for the same subjects and study level. This information is available for math and English for most ninth graders in 2003. For school cohort 2003, we are thus able to estimate:

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

where  $\bar{y}_m$  is average GPA in the municipality (based on the average of math and English grades for the individual), and  $\bar{T}_m$  is average test scores (based on the scores on standardized national tests in the same two subjects). The private school share parameter  $\pi_1$  captures whether pupils in municipalities with higher private school shares have higher grades than pupils in areas with lower private school shares, given test scores. Hence,  $\pi_1$  is a measure of how inflated grades are in municipalities with high private school shares.

Table 5 shows the results from estimating model (2), with and without including the set of covariates. Grades and test scores are strongly related, whereas the estimates for the private school share are statistically insignificant in both columns. Thus, this exercise gives us 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.<sup>26</sup> Note again that we only study a single cross-section here. However, we believe that analyzing this issue for the last year in our data is superior to analyzing any earlier single year, since if grade inflation was increasing with private schooling it should be most severe in the last year.

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<sup>26</sup> This result is also supported by a recent report by the Swedish National Agency of Education (2006), which study the years 1998-2006. Wikström & Wikström (2005) do not either find any evidence of differential grading standards in municipalities without and with private high schools using data for high school leavers in 1997.



Even though the absence of long-term effects is reasonable given the small positive effects we find for the short term, we shortly discuss some potential explanations for why they vanish. There might exist *intermediating mechanisms* that act in a compensatory manner, i.e., that some factors negatively/positively affect the subsequent educational achievement of school leavers in areas with much/less private schooling. One example is endogenous compensatory policy actions. Another potential intermediating mechanism is the role of private schooling at the high school level. The argument is that 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. We have therefore re-estimated the medium and long-term estimations including the private school share in high school as an additional control variable.<sup>27</sup> We find that the basic 9<sup>th</sup>-grade private school share estimates are not affected when we also include the corresponding variable for the high school level.

## 5. Conclusion

We have estimated general achievement effects of choice and competition between public and private schools in Sweden. For this purpose, we have used administrative data on compulsory school graduates in 1988-2003, and exploited the differential within-municipality variation in private schooling that developed as a consequence of the 1992 voucher 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 9<sup>th</sup>-grade GPA 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 different pre-reform or concurrent trends in unobservable variables across municipalities. We also find a positive impact on the fraction of students who chooses

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<sup>27</sup> 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).

an academic high school track. 2) We find very little support for that the positive 9<sup>th</sup>-grade effect also leads to better performance in high school for the same individuals (who enter HS). 3) There is no evidence that an increase in private schooling at the compulsory level has an effect on subsequent university attainment or years of schooling. Thus, we conclude that the positive first-order effect on 9<sup>th</sup> grade performance vanishes over time. It does not seem to be large enough to lead to lasting positive effects. One potential reason for why we find such a small impact is that the entry of new private schools not has been followed by the closing down of public schools. Hence, it might be that increasing shares of school budgets have been devoted to maintain operating public schools of poor quality.

Finally, our result for the short term contrast the evidence in previous Swedish studies (Ahlin, 2003; Björklund et al., 2005; Sandström and Bergström, 2005) which look at earlier and fewer reform years and find large effects for the 9<sup>th</sup> grade. On the other hand, our findings are not surprising given the results in the 8<sup>th</sup> grade TIMSS 1995 and 2003 surveys. The Swedish scores were falling considerably between these years, both in absolute terms and in relation to many other comparable countries (see, e.g., the Swedish National Agency for Education, 2004). Given the large increase in private enrollment 1995-2003, this suggests, in line with our findings, that school choice and competition is not a panacea for improving overall educational achievement. Our results are also in line with the evidence in Hsieh and Urquiola (2006) who examine Chile's voucher program. This is interesting since large-scale private school choice reforms are rare, and rarely assessed. An important difference between Chile's and Sweden's experiences, however, is that whereas an increased sorting of pupils seems to have been a major effect in Chile, the existing evidence for Sweden point at small sorting effects at the nine-year level (Böhlmark and Lindahl, 2007). Hence, it seems that the Swedish reform was successfully designed to limit sorting.

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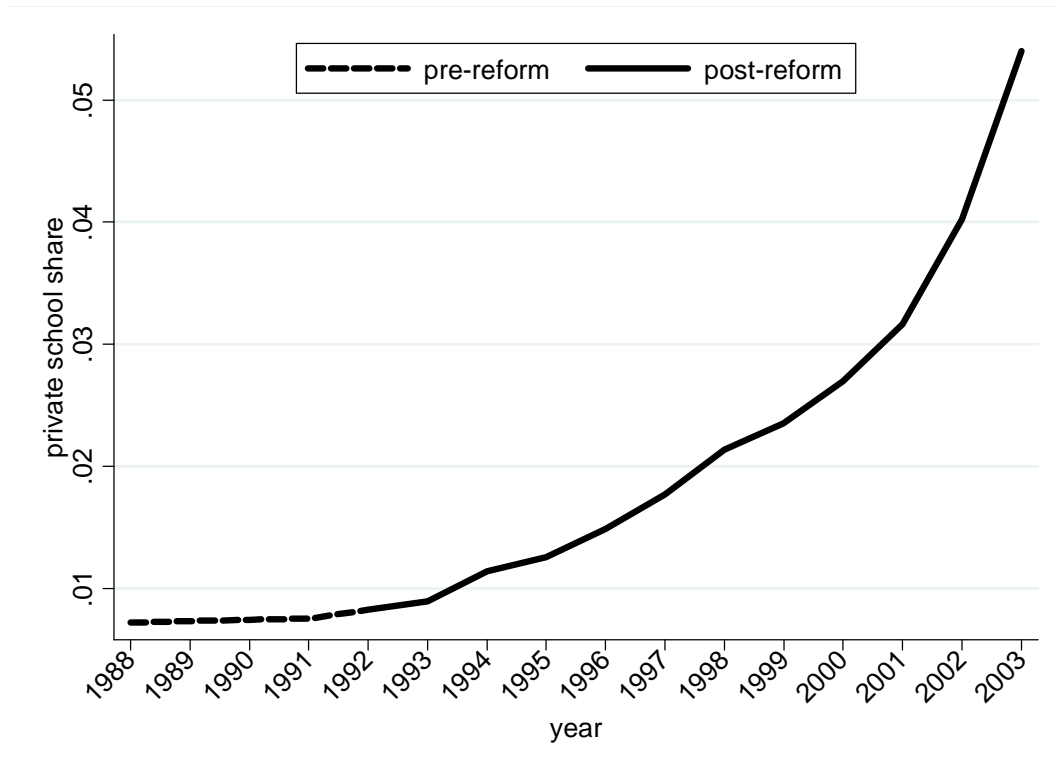
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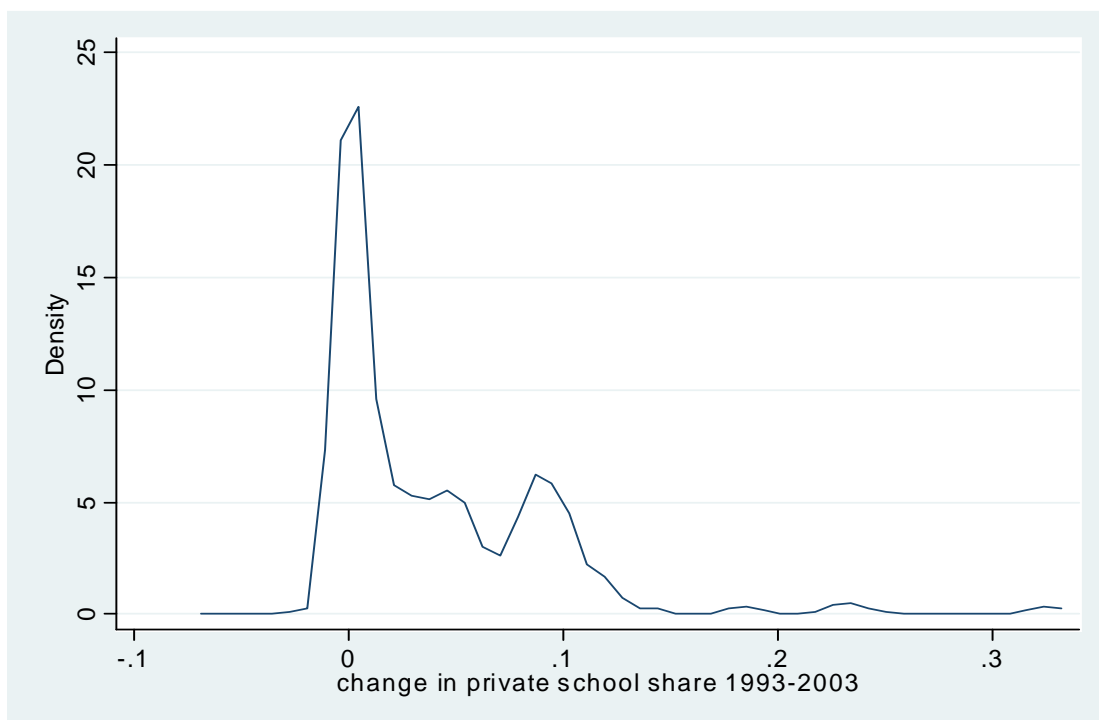
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Figure 1 **The private school share 1988-2003**



**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 Swedish National Agency for Education.

Figure 2 **Kernel density estimation of the change in the private school share between 1993 and 2003**



**Notes:** The graph is from kernel density estimation of the change in the private school share in municipalities between 1993 and 2003 (weighted by nr of pupils in 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 for school cohorts 1988-2003**

	Mean	St. Dev.
Private school share in 9 <sup>th</sup> grade	0.016	0.032
<u>9<sup>th</sup> GRADE OUTCOMES</u>		
Observed with any grade	0.99	0.09
GPA in 9 <sup>th</sup> grade	49.8	23.2
<u>HIGH SCHOOL OUTCOMES (1994-2003)</u>		
Observed with any grade	0.75	0.43
GPA end of 1 <sup>st</sup> year in HS	50.0	21.5
Academic track in HS	0.56	0.50
GPA at end of HS	50.0	21.7
<u>LONG-TERM EDUCATIONAL OUTCOMES</u>		
At least 1 semester of univ. studies at age 22 (1988-2001)	0.25	0.43
Years of schooling at age 24 (1988-1999)	12.2	1.66
<u>FAMILY, DEMOGRAPHIC AND POLITICAL VARIABLES<sup>†</sup></u>		
At least one parent university educated	0.24	0.42
At least one parent high school educated	0.41	0.49
Log family income	8.0	0.12
Log family income is missing (unemployment proxy)	0.04	0.19
2nd generation immigrant	0.06	0.24
Immigrant	0.06	0.24
Nr of 9 <sup>th</sup> grade pupils / 100	2.1	2.8
Right-wing majority	0.31	0.46
Coalition	0.29	0.46

**Notes:** The number of observations in the basic sample is 320,570. <sup>†</sup>The following control variables are not listed in the table (but belong to this set): Gender of student, Parent's average age at birth of child, Mothers years of schooling; Fathers years of schooling; indicator variables for missing parental schooling; 6 dummies for immigrants region of origin, immigrants' age at immigration. The income variables are based on information from compulsory employer reporting on income from work (including self-employment and sickness benefits). Income is coded missing if it is less than 20,000 SEK (about 2,500 USD in year 2000 prices). Family income is the average of the sum of the parents' income when the child is 5 and 10 years old. Note that st. dev. are based on individual-level data for all variables except for the private school share, for which numbers are from weighted aggregate levels.

Table 2 **Aggregate descriptive estimations**

	Post-reform: Change 1993 -2003		Pre-reform: Change 1988-1992	
	(1) Municipalities with private schools	(2) Municipalities without private schools	(3) Municipalities with private schools	(4) Municipalities without private schools
	Mean (St.err)	Mean (St.err)	Mean (St.err)	Mean (St.err)
Private school share in 9 <sup>th</sup> grade	<b>0.064</b> <b>(0.006)</b>	<b>0.005</b> <b>(0.001)</b>	--	--
<u>9<sup>th</sup> GRADE OUTCOMES</u>				
Observed with any grade	<b>-0.004</b> <b>(0.001)</b>	<b>-0.007</b> <b>(0.001)</b>	0.000 (0.001)	0.001 (0.001)
GPA in 9 <sup>th</sup> grade	0.25 (0.36)	0.09 (0.40)	0.08 (0.34)	-0.18 (0.38)
<u>HIGH SCHOOL OUTCOMES</u> (1994-2003)				
Observed with any grade	-0.09 (0.01)	-0.11 (0.01)	--	--
GPA end of 1 <sup>st</sup> year in HS	<b>1.90</b> <b>(0.38)</b>	<b>0.68</b> <b>(0.43)</b>	--	--
Academic track in HS=1	0.04 (0.01)	0.01 (0.01)	--	--
GPA at end of HS	-0.92 (0.46)	-0.31 (0.67)	--	--
<u>LONG-TERM EDUCATIONAL</u> <u>OUTCOMES</u>				
At least 1 semester of university studies at age 22 (1988-2001)	0.021 (0.006)	0.019 (0.007)	-0.007 (0.006)	-0.015 (0.007)
Years of schooling at age 24 (1988- 1999)	-0.034 (0.024)	-0.006 (0.028)	0.849 (0.023)	0.822 (0.024)
<u>SCHOOL CHARACTERISTICS</u>				
Nr of public schools/100 pupils	0.29 (0.05)	0.31 (0.06)	0.32 (0.04)	0.30 (0.04)
Mean log distance to pupils' nearest public school	-0.18 (0.04)	-0.13 (0.04)	--	--
Pupil/teacher ratio in municipality	0.48 (0.08)	0.62 (0.07)	<b>0.03</b> <b>(0.10)</b>	<b>-0.65</b> <b>(0.05)</b>
Mean log school costs per pupil	0.19 (0.01)	0.18 (0.01)	--	--

**Notes:** There are 93 municipality observations in column 1 and 3, and 191 municipality observations in column 2 and 4. The numbers for respective variable are weighted with the number of individuals in the 2003 school cohort. Boldfaced numbers indicate statistically significant differences between the two categories of municipalities, for the post- and pre-reform period respectively.

**Table 3 Private school entry and private school share regressions**

Dependent Variable:	Private school entry 1993-	Private school share in 2003
	2003 (1)	(2)
Fr. parents university educated	0.083 (0.050)+	0.029 (0.046)
Fr. parents high school education	-0.0004 (0.033)	0.014 (0.037)
Mean log family income	-0.022 (0.040)	0.158 (0.041)**
Fr. parents income missing	0.006 (0.166)	-0.432 (0.292)
Fraction 2nd generation immigrants	0.380 (0.134)**	0.104 (0.178)
Fraction immigrants	0.031 (0.129)	0.567 (0.281)*
Number of 9th grade graduates / 100	0.097 (0.009)**	0.001 (0.002)
Right-wing political majority	0.017 (0.011)	0.026 (0.006)**
Coalition	0.007 (0.012)	0.002 (0.010)
Nr of public schools/100 pupils	0.007 (0.005)	0.004 (0.006)
Mean log distance to pupils' nearest public school	-0.007 (0.005)	-0.003 (0.005)
Pupil/teacher ratio in municipality	0.002 (0.005)	0.007 (0.004)
Mean log school costs per pupil	-0.008 (0.049)	0.064 (0.033)+
9 <sup>th</sup> -grade GPA, lagged 3 years	0.0006 (0.0007)	
9 <sup>th</sup> -grade GPA in 1992		-0.0009 (0.0008)
Change in 9 <sup>th</sup> -grade GPA 1988-1992		0.0002 (0.0007)
Private school existed in municipality prior to reform		0.007 (0.011)
Year fixed effects	YES	NO
Independent variables defined for school cohorts:	1993-2003	1992
R2	0.190	0.593
N	3124	284

**Notes:** The estimates in column 2 are weighted, where the weights are the number of ninth grade pupils living in the municipality. In column1 we have lagged the school finance and public school competition variables 3 years. The regressions also include controls for the mean students' gender and average age of parents and indicators for missing values of mean log distance and mean school costs. The standard errors (in parentheses) allow for clustering at the municipality level in column 1 and are robust in column 2. + significant at 10 percent; \* significant at 5 percent; \*\* significant at 1 percent.



Table 4 **Regressions of the municipal private school share in the 9<sup>th</sup> grade on short, medium and long-term educational achievement**

Samples & Dep. Variables	(1)	(2)	(3)	(4)
<b>a) 1988-2003; N<sup>mt</sup>=4,544</b>				
Observed with grade marks from 9 <sup>th</sup> grade	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.02)	0.00 (0.02)
R2	0.25	0.28	0.33	0.35
GPA in 9 <sup>th</sup> grade	9.37 (4.15)*	8.65 (3.26)**	8.35 (3.54)*	7.12 (2.61)**
R2	0.48	0.57	0.55	0.63
<b>b) 1994-2003; N<sup>mt</sup>=2,840</b>				
Observed with grade marks from high school (owg)	0.04 (0.07)	0.04 (0.07)	-0.06 (0.08)	-0.07 (0.08)
R2	0.47	0.49	0.55	0.58
GPA at end of 1 <sup>st</sup> year in high school, if owg=1	10.17 (4.01)*	7.93 (3.15)*	3.17 (3.01)	1.58 (3.01)
R2	0.56	0.61	0.63	0.67
Academic track in high school (AT)	0.26 (0.08)**	0.24 (0.07)**	0.28 (0.11)*	0.25 (0.12)*
R2	0.59	0.61	0.66	0.68
GPA at end of 3 <sup>rd</sup> year in high school, if AT=1 (N <sup>mt</sup> =2,834)	2.06 (3.61)	2.78 (3.68)	1.65 (4.76)	1.43 (4.52)
R2	0.34	0.36	0.43	0.45
<b>c) 1988-2001; N<sup>mt</sup>=3,976</b>				
At least 1 semester of university studies within 6 years	0.04 (0.09)	0.02 (0.08)	0.06 (0.10)	0.07 (0.09)
R2	0.53	0.58	0.58	0.63
<b>d) 1988-1999; N<sup>mt</sup>=3,408</b>				
Years of schooling within 8 years	-0.20 (0.38)	0.23 (0.39)	0.52 (0.55)	0.57 (0.50)
R2	0.78	0.81	0.81	0.83
Municipal and school-cohort fixed effects	YES	YES	YES	YES
Municipal specific trends	NO	NO	YES	YES
Municipal controls <sup>a</sup>	NO	YES	NO	YES

**Notes:** <sup>a</sup>Family, demographic and political variables listed in Table 1 (and specified in the note to 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 municipal private school share in the 9<sup>th</sup> grade on short and long-term educational achievement, allowing for heterogeneous effects**

	(1)	(2)
	9-th grade GPA	At least one semester university studies
<b>A. Private school share effects for different school cohorts</b>		
1993-1996	-3.06 (4.75)	0.18 (0.13)
1997-1999	11.73 (4.46)**	0.33 (0.12)**
2000-2001	10.60 (3.77)**	0.03 (0.11)
2001-2003	14.21 (3.55)**	--
Adj R2	0.59	0.54
<b>B. Private school share effects for different years after first private school was established</b>		
1-2 years	4.67 (4.27)	0.16 (0.17)
3-4 years	6.21 (4.84)	0.12 (0.12)
5 -11 years	17.54** (4.25)	0.21 (0.15)
Adj R2	0.59	0.54
N	4,544	3,976
Municipality Fixed effects	YES	YES
Municipality specific trends	YES	YES
Municipality controls <sup>a</sup>	YES	YES

**Notes:** <sup>a</sup>Family, demographic and political variables listed in Table 1 (and specified in the note to 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 Differential grading standards in the 9<sup>th</sup> grade, school cohort 2003

Dep. variable: GPA <sup>a</sup>	(1)	(2)
Private school share	-1.35 (2.99)	2.98 (3.64)
Mean of test scores	0.79 (0.03)**	0.75 (0.04)**
Municipal Controls <sup>b</sup>	NO	YES
N	279	279
R2	0.76	0.80

**Notes:** <sup>a</sup>GPA is here based only on grades and test scores in math and English. <sup>b</sup> Family, demographic and political variables listed in Table 1 (and specified in the note to 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.