DID SCHOOL CHOICE IN SWEDEN IMPROVE ACADEMIC ACHIEVEMENT?*

Anders Böhlmark
SOFI, Stockholm University

Chang-Tai Hsieh
University of California, Berkeley and NBER

Mikael Lindahl
SOFI, Stockholm University

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I. INTRODUCTION

The question of whether school choice might improve the quality of schooling is hotly debated in many countries throughout the world.[1] There are basically two arguments that underlie the view that choice would improve the quality of schooling. First, there is the view that private schools are simply better than public schools. There is an enormous literature on this question, and a number of recent papers have turned to quasi-experimental evidence to assess the extent to which students benefit from attending private schools.[2] Clearly, if private schools are better than public schools, then choice could improve schooling quality by the mere process of reallocating students, and resources, from the inefficient public sector to the private sector. The second, and perhaps the most powerful argument for choice, comes mainly from our instinct that inefficient institutions are ones that face weak incentives. There is of course abundant anecdotal evidence of inefficient public school bureaucracies, of custodian unions that drain resources from schools, of teacher unions that prevent bad teachers from being fired and good teachers from getting hired. So there is every reason to believe that if one were to put in the right set of incentives to public school administrators, then these administrators will finally start to do what is necessary to improve the school's efficiency.

While acknowledging the potential productivity effects of school choice, detractors worry about its effects on inequality. In particular, they worry about the effect of choice on students who remain in public schools. While these students might benefit from the effect of competition on the public sector's productivity, they might be hurt from the departure of their classmates to the private sector. More generally, the concern is that school choice would result in greater segregation of students by ability, income, ethnic background, or religion.

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

This paper evaluates these arguments by assessing the impact of a school reform implemented in Sweden in the early 1990s that significantly increased the choice of schools available to Swedish families. There were three key elements of the 1991-92 reforms. First, financial responsibility for public schools was transferred from a National Board of Education to local municipalities.[3] Second, while every student were required to attend the public school in their neighborhood prior to the reforms, many municipalities allowed students to choose between any public school in the municipality after 1991/92. Third, every municipality were required to provide non-public (i.e. independent)[4] schools with a grant equivalent to almost all of the average expenditure in the municipal public school system for each student enrolled in the private schools.[5]

There are at least two possible effects of these reforms, not mutually exclusive, which tend to support opposite sides of the school choice debate. First, easier choice among schools may generate greater sorting of students along income or ethnic lines. The fear is that this sorting may adversely affect children from disadvantaged backgrounds by increasing the inequality of school spending and by segregating children from disadvantaged backgrounds from their more advantaged peers. On the other hand, easier choice may encourage competition among schools, which may provide them with an incentive to improve their quality and thus improve the quality of education for all students.

In this paper we focus on the consequences of establishment of non-public schools. We look at effect for the individual of attending an independent school, and from the frequency of independent schooling in the region. We also look at effects on segregation, along ability, ethnic and socioeconomic lines. The issue is whether increased freedom of

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[2] See, for example, Rouse's (1998) work on the Milwaukee school voucher initiative, Bettinger et. al.'s (2002) work on vouchers in Colombia, and the work by Peterson and his coauthors on voucher initiatives in several US cities (Peterson's results have been challenged by Krueger and Zhu (2002)).

[3] There are currently 289 municipalities in Sweden.

[4] Note that we use the terms non-public and independent schools interchangeably. These are schools that are publicly funded, but not publicly run. We do therefore not use the term private schools.

[5] The minimum required percentage has changed over the years. In 1992 it was 85 percent, in 1999, 75 percent.

choosing non-public schools harms public schools by draining resources and the best pupils from them or whether the productivity of public schools would improve due to increased competition.

Sweden is an ideal country for evaluating effects from increased choice in the school area since the country went from a situation were students were assigned to their closest school (“närhetsprincipen”) and there were almost no opportunities to deviate from this principle, to a system were it was not only possible to choose among other public schools, but students were also given the option to choose among privately run (but publicly funded) schools. Further, these opportunities differ widely among municipalities and over time, since in some municipalities it took much longer to open up private schools and they still do not exist in a large number of municipalities.

Existing Swedish studies of the effects of independent schools have in general found very large effects on academic outcomes (Sandström & Bergström, 2005, Ahlin, 2003, and Björklund et al, 2005). Although, there is a very extensive international literature about school choice and reforms,[6] what is most relevant to the Swedish reforms are the similar school reforms introduced in New Zealand around the same time as in Sweden. Fiske & Ladd (2000) present many consequences of the reforms. However, due to the absence of test scores or data on grades, it was not possible to estimate the effects on any objective measure of achievement. The best that could be done was to rely on the impact of the reforms as perceived by teachers and principals.

This paper uses a large administrative data set on a large sample of individuals graduating from Swedish compulsory school (grade 9) in Sweden 1988-2003. We have information on

[6] Acknowledging that this is a very incomplete list; we may mention Angrist et al. (2002, 2004) evaluating a private secondary school voucher experiment in Columbia; Cullen, Jacob & Levitt (2003, 2005) looking at the choice among public high schools in Chicago; Hoxby (2000) and Urquiola (2005) estimating the effects of choice between school districts in the US (so-called Tiebout choice); Hsieh & Urquiola (2005) and McEwan (2002) estimating effects of the large-scale reforms that dramatically increased school choice in Chile during the 1980s.

the children's (and their siblings) grades in individual subjects, and the educational and economic outcomes of their parents. We further have merged school information to this data.

The basic finding is that the individual gain about 1-4 percentile points form attending an independent school. The gain from the independent school share in the region (municipality) of schooling is positive and statistically significant in most specifications. However, the effect ranges from -2 to 7 percentile points. These estimates are much smaller than what has found in earlier studies on Swedish data. Further, we find some sorting effects from a higher fraction of independent schooling in the municipality. It appears that an increase in the independent school share make public schools loose students that are native born and those with parents that have high income and education and are immigrants. There are no statistically significant sorting effects for academic outcomes.

The paper is organized as follows: Section II describe the features of the school reform and the data used in the analysis. Section III discusses some estimation issues and the empirical framework. Section IV reports our productivity and sorting results. Sections V present some sensitivity analysis. Section VI discuss our results in relation to previous Swedish evidence, and section VII concludes.

II. INSTITUTIONS AND DATA

II.A. THE 1992 SCHOOL REFORM

Prior to 1992, public schools were operated by local governments (municipalities), but school funding and control was largely centralized. Schools had to follow a national curriculum. Teachers were employees of and directly paid by the central government. Municipalities received earmarked funds from the central government to cover the schools' operational costs. Students were assigned to the public school in their local catchment area. Although there were non-public schools, and some of these schools received some state funding, these schools accounted for less than one percent of total enrollment. These schools were elite schools (very old schools with high school fees) and international schools (for children whose families were on temporary stays in Sweden), and some religious (catholic) schools.

Sweden implemented a number of school reforms in 1991-92, which significantly increased the choice of schools available to Swedish families. There were three key elements of the 1991-92 reforms. First, financial responsibility for public schools was transferred from the state to local municipalities. Teachers became municipal employees and were no longer paid directly by the central government. Instead, the central government provided block grants to municipal governments to cover teachers' salaries and other costs of running the schools. Municipal government supplemented these funds with their own revenues. Second, while every student was required to attend the public school in their neighborhood prior to the reforms, municipalities now allowed students to choose between any public schools. This choice was conditional on slots being available after those residing closest to the school had made their choice.

Third, the most radical component of the school reforms might have been that every municipality was required to provide independent schools with a grant equivalent to (most of) the average expenditure in the municipal public school system for each student enrolled in the independent schools. Municipalities choosing to provide additional resources (in addition to the minimum required amount) to students enrolled in independent schools were allowed to do so. Prior to this reform, no state funding was available for non-public schools. For the first time, ordinary students were now given the option to attend non-public schools free-of-charge.

To be eligible for public funding, independent schools had to be approved by the National Agency for Education. These schools had to follow the national curriculum and were not allowed to choose their student body. If a school was oversubscribed, it had to follow the same rules as a public school in deciding whom to enroll. The municipal government could express its opposition to the independent school's application, but the number of applications rejected has been quite small. Independent schools receiving public funding were initially allowed to charge some fees, but these were heavily circumscribed. After 1997, private schools were no longer allowed to charge any fees. There were no restrictions on the ownership structure of private schools eligible for public funds -- they could be religious schools, non-profit parent cooperatives, or run by profit-seeking corporations.[7]

In Figure 1 we show the evolution of independent schooling 1989-2004 in Sweden. It is obvious that the 1992-reform has resulted in a sharp increase in the number of students in independent schools. The share of students attending 9th grade of a non-public school has increased from less than 1 percent in 1989 to more than 6 percent in 2005. This increase was especially prevalent in some areas. Among the municipalities where independent schools have existed (about 1/3 of all municipalities), the share of independent school students increased to almost 10 percent a decade after the reform, with the municipality with the largest share having almost 40 percent of independent school students. In Figure 1, we also show the fraction of independent school students in all compulsory grades. As expected, the increase was faster, but in 2004 there appears to be about the same amount of independent school students in 9th grade as in other grades. This can perhaps be interpreted as expecting slow-down in the growth of independent schooling in the future. From figure 1, we also see that there were indeed some students attending non-public schools prior to the 1992 reform, but that this fraction appears to have been roughly constant up to 1993.

[7] Also show the increase in the number of independent schools and in the "market-share" of independent schools. Also show the effect between different types of "markets" in the country, so that there is differential impact of the reform across municipalities.

In Figure 2, we divide the independent school pupils in 9th grade into the type of school attended. The increase in the share of independent schooling is mostly due to more pupils attending general and special pedagogy (Montessori, Waldorf) schools. We also note that the international and elite schools have been roughly constant throughout the period. Since these schools are quite special, we exclude them from our analysis in this paper.[8]

The measure of independent school share we use in this paper is the share of students in a municipality attending 9th grade in independent schools.[9] Since non-public schools were not required to report grades before 1993, we focus in most of our analysis on the years 1993-2003.

II.B. DATA AND SUMMARY STATISTICS

Our data is the random 20 percent sample of all primary school graduates (9th grade) from 1988 to 2003, which we obtained from the grade 9 registers.[10] This data-set also provides information on the school attended by each student and the location of the school. We merged this sample with the data from other registers to get the students' grades in ninth grade (based on classes taken during grades 7 through 9). The classes include natural sciences (physics, chemistry, technology and mechanics, and biology), social sciences (history, religion, social studies, and geography), English and Math.[11] The scores for each class are given by the student's teacher on a 1-4 scale (1-5 in earlier years). To make sure that these scores are comparable across students (in a given year), the National Education Association issues annual guidelines to teachers that spell out the specific criteria a student

[8] Including these schools do not affect any of our estimates.
[9] In the sensitivity analysis section we discuss other measures used in previous Swedish studies.
[10] Unless otherwise indicated, all our data was provided by Statistics Sweden, and the matching is based on the individuals national identification number. The original sample criterions are a 20 percent random sample of 1) the entire population of individuals born in Sweden 1962-1987, and 2) the population of foreign-born individuals arriving in Sweden prior to 18th birthday.
[11] 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 have changed a lot over the years.

has to meet to qualify for a certain score.[12] The ninth grade scores are the main measure of a student's performance in lower secondary school and high school placements are entirely based on these grades. We convert the scores to percentile ranks and use the average percentile rank of each student in main subjects as our central measure of academic achievement.[13]

We would have preferred to use actual test scores. Unfortunately, these are only available for a few years and for a selective sample of schools. The exception is for 2003, when tests cores are available for most pupils. If we correlate the test scores (scaled in percentile ranks) and grades, for those students where those are available, we get estimated correlation coefficients of 0.81 (for math), 0.86 (English) and 0.78 (Swedish). Hence, the grades clearly capture most of the variation in academic achievement. The most important issue for us is whether grades are inflated in independent schools or in areas with a lot of independent schooling. We return to this issue in section V.

To obtain demographic information for our sample of ninth grade students, we merged our sample of students with the data from the multi-generation registry and the Swedish Censuses (bi-decennial 1960-90). This allows us to obtain information on the sex, year of birth, immigration status, and identity of the parents for each student. With information on the parents' identity, we can identify the siblings in our sample of ninth grade students. In addition, with the identity of the parent, we can combine our data with educational and

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[12] 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.

[13] We use the grades in all the subjects, including Math and English, to compute a student's average percentile rank. The inclusion of Math and English is a bit complicated because these subjects were taught at two levels prior to 1998, and thus the grades are not comparable across students. We assume that the grade in the lower level (1 to 5) equals the grade at the higher (1 to 5) level minus one. If one compares the Math and English grades to grades in natural and social sciences, who where taught only at one level, this appears to be a reasonable approximation. Using alternative mappings to do not alters the results. In addition, roughly 10-15 percent of students receive one combined grade for natural sciences (rather than separate grades for the four science classes). Similarly 15-25 percent of students have one combined grade for the social sciences. For students having a common grade for the natural sciences or social sciences, we assume that the common grade is the grade for the individual subjects. Some students had missing grades in some subjects, and we used their non-missing grades to compute their average grade (check if the results are identical of we delete these students

administrative tax records to obtain information on the parents' educational background and income. The educational background of the parents is provided in 7 categories, and we converted these categories to years of schooling and take the average of the two parents as our measure of parental education. We also use indicators of whether at least one of the parents have obtained university education (at least 3 years) and high school education (at least 3 years). We use labor income (including benefits) as our measure of income and calculate family income as the average of the sum of the parent's income when the child is about 5 and 10 years old.[14] Since we have data of immigration status for both children and parents we can look at separate effects for first- and second generation immigrants. We also know when they immigrated and from which country.

Finally, we combined our data with school registers, which contain information about all schools in Sweden. This allows us to identify whether a school is independent, and what type of independent school (i.e religious, special pedagogy, general among others), the number of students in the school in each grade, as well as to compute the student-teacher ratio in each school. These registers are matched with individual identifiers to determine what type of school an individual went to (by obtained grades at the end of compulsory school).[15]

Table 1 report the means and standard deviations of the variables used in the analysis. We report these statistics for individual (panel 1) and aggregate (panel 2) characteristics, where the latter (except for the independent school share who is based on the total population of students in 9th grade) are based on the individual characteristics aggregated up to municipality-year level. The weights we use are based on the frequency of individuals in each cell based on our 20 percent random sample. Hence, the means look very similar for the

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from the sample). Finally, a small number of students (X percent of our sample) had scores from two years. We deleted these students from our sample.

[14] We set income to missing if it is less than $20,000 SEK (in 2000 prices).

[15] Moreover, we know the distance from the home to the nearest public and independent school, and we have information (for Stockholm) that makes it possible to determine whether students choose another school than the one to which they were assigned (that is located closest to home). Therefore, we can analyze both the choice between public and independent schools, and between public schools. This work is currently in progress.

individual and aggregate variables. All data are from 1993-2003. Most of the parents have either high school or university education, and about 13 percent are either immigrants or second generation immigrants, and almost 4 percent of the immigrants leaving compulsory schooling, arrived in Sweden at age 10 years or later.[16]

II.C. INDEPENDENT SCHOOL ENROLLMENT

As can be seen in Table 1 there are slightly over 2 percent of the individuals in our sample that are attending an independent school. However, there is a large variation between municipalities and between years. To see what explains independent school attendance we perform some descriptive regressions.

We first investigate what explains independent school attendance for a student. Results are presented in Table 2. The dependent variables is an indicator for whether the school that the individual attends is independent (P=1) or public (P=0). The reported estimates are marginal effects from estimation of a Probit model. All regressions include year indicators. In the model underlying the estimates in column (2) we control for municipality indicators. In column (1) we find that independent school attendance is positively related to student being a female, parents' age, parents' education and immigrant status for the student and parent. In column 2, where we look at within municipality variation, estimates are very similar. A difference is that immigrant status of the child no longer is related to independent school attendance.

We then look at aggregate (weighted) estimations in Table 3. The dependent variables in columns 1-2 are the share of independent schooling in grade 9, and in columns 3-4 an indicator for whether or not independent schooling exists in the municipality. In columns 1-2 we only utilize municipalities with any independent schooling in any of the years, whereas in

[16] In Böhlmark (2005), there is shown that this is the critical age for immigration to Sweden, regarding their grades in 9th grade.

columns 3-4 we use all observations available. All regressions include year indicators. In the model underlying the estimates in columns 2 and 4, we also control for municipality fixed effects. All variables are municipality-year averages. We find that a municipality with more educated parents and a higher fraction of second generation immigrants are associated with a higher fraction of students in independent schooling. When we control for municipality fixed effects, many estimates are smaller, although the general picture is unchanged. We also see, in columns 3-4, that having an independent school in a municipality is more likely if parents are older, are university educated or if there are a higher fraction of (first and second) generation immigrants. When we control for municipality fixed effects most estimates become insignificant.

III. EMPIRICAL FRAMEWORK

III.A. ESTIMATING THE EFFECTS OF SCHOOL CHOICE

There are three issues one has to deal with when measuring the effects of increased school choice. The first issue is that one needs to have a control group or a counterfactual. The second issue is that if choice leads to segregation, then it will be difficult to separately identify the two sources of productivity gains from choice. The third issue is how to measure the degree of competitive forces unleashed by school choice. We address these in turn.

To begin, the ideal would be if one could randomly assign choice to some communities, and not to others, and then compare the changes in outcomes between these two sets of communities. This is rarely the case in most social programs, and clearly not possible in the case of Sweden since the school choice program was introduced across the entire country at the same time. However, there were large differences in the impact of the program on the increased availability of independent schools across municipalities in Sweden.[17] The program lead to a larger increase in the independent school enrollment in municipalities that were wealthier, more populated, and governed by conservative municipal governments (see Sandström and Bergström, 2005). This differential impact is clearly endogenous to the characteristics of a given community, so it does not make any sense to only look at cross-sectional differences across communities at any given point in time (and it's virtually impossible to think of instruments that would pass standard exclusion restrictions).

In this paper we instead use a difference-in-differences approach, i.e. we compare the change in outcomes in a given community on the change in the availability of schools. As long as the characteristics of a municipality that determine the differential impact of the choice program do not change over time, this model simply difference away these fixed characteristics. This assumption should not be taken for granted. However, we can test this identification assumption in several ways. First, we can include municipality specific trends as controls. The characteristics of communities that determine the differential impact of school choice can then change over time as long as the change is approximately linear. Second, we examine whether the change in our outcome measures in other periods of time (such as the 1980s) is correlated with the differential impact of the school choice program after 1992. This will be done by estimating the difference-in-difference model for years preceding the reform (we use 1989-1992), predicting average GPA for the municipality for post-reform years, and then include this variable as a control variable in the difference-in-differences regression for 1993-2003. This will be the empirical approach we take in our paper.[18]

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[17] Show a figure with the distribution of independent school enrollment change 1993-2003.

[18] This discussion has so far assumed that the marginal effect of choice is the same across school markets, but an additional complication is when there are heterogeneous treatment effects. For example, it could be the case that the productivity advantage of independent schools differs across municipalities. In this case, it is likely that there will be a greater penetration of independent schools in communities where those schools are more productive. If this is the case, then if we're comparing the change in achievement in communities with a greater penetration of independent schools (where those schools are more productive) with communities where independent schools had a smaller effect (where presumably independent schools are less productive), this would yield estimates that exceed the effect of choice in an *average* community. Put differently, what we would be doing is to estimate the average *marginal* impact of choice, which would larger than the average effect of choice. This is the standard issue that one runs into whenever there are heterogeneous treatment effects. See, for

The second issue that one faces is that there are two possible consequences of increased school choice -- segregation and productivity -- that are clearly not mutually exclusive. Suppose that choice does lead to productivity benefits (either because independent schools are better or because competition prompts public schools to improve). Even so, as long as choice also leads to segregation (and if peer effects are important), it will be difficult to disentangle the effects of segregation from that of productivity. If we look at outcomes for both public and independent school students we partly deal with this even in the presence of sorting effects. However, if we are uncertain about whether the gains to better students from interacting with good peers equals the loss of students interacting with bad peers, we would not, even if choice would be randomly assigned, be able to estimate causal effects of choice.[19]

The third issue is how to measure the degree of choice in a municipality. There is no measure that is completely appropriate. Our measure in this paper is the change in the enrollment share in independent schools. However, this misses out the effect of competition between public schools, another feature of the 1992 school reform. It is therefore important to point out that if the variation in the share of independent schooling correlates with the variation in the degree of choice between public schools (or with some consequences of the decentralization of school funding in 1992), within municipalities, then it is difficult to isolate the effect of independent schooling.[20]

To summarize, our empirical strategy will be to look across municipalities in Sweden and compare the changes in communities where choice had a larger effect relative to municipalities where it had a smaller effect. As mentioned, this difference-in-differences strategy is valid as long as the factors that explain the differential impact of choice in a municipality is fixed over time (and we will check for this later in the paper). Using this

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example, the recent work on IV estimates of returns to schooling (see Card 2000 for a summary). See Angrist and Imbens (1995) for the conditions under which this interpretation is appropriate.

[19] See Hsieh and Urquiola, 2005, where this argument is laid out in more detail.

empirical strategy to deal with the endogenous impact of choice across municipalities, we will look for two effects of choice. We will measure the extent to which choice leads to greater segregation, and the extent to which it lead to improved educational outcomes. To get around the problem of selection caused by segregation when measuring educational outcomes, we will include both public and independent school students in the analysis.

III.B. EMPIRICAL FRAMEWORK

A basic empirical model of the impact of attending an independent school and of independent school attendance in the region on school achievement can be expressed as

(1) $$y_{ismt} = \beta_1 P_{ismt} + \beta_2 \overline{P}_{mt} + \lambda_m + \lambda_t + \varepsilon_{ismt},$$

where y_{ismt} denotes performance at the end of the schooling (9th grade) for student i in school s in municipality (or schooling market) m at time t; P_{ismt} is an indicator for whether the school that the individual attend is independent (P=1) or public (P=0); $\overline{P}_{mt}$ denotes the share of students attending independent school at the end of 9th grade in municipality m at time t; λ_m and λ_t represents municipality and year specific effects, respectively; and ε_{ismt} is a random error term. The independent school coefficient β_1 measures the effect of attending an independent school (as opposed to a public school), given the amount of independent schooling in the municipality, for the average student. The independent-school share coefficient β_2 measures the individual's effect of the frequency of private schooling in the municipality, given own school-type attendance.

Hence, β_2 measures the spillover (or externality) from other individuals' school choice. More independent schooling can affect individual performance (given own school choice) through several mechanisms. Increased competition can force all schools to improve,

[20] One way to capture this latter effect would be to look at effects of some measures of public school competition on academic outcomes. We are currently working on this.

since well-informed parents will choose the best schools thereby forcing poor performing schools to either improve or close down (β_2 >0). Well-functioning public schools can loose students to poorly run but well marketed independent schools. Since there are fixed costs of running schools and money follows the student, this makes it harder for the school to keep the quality of education at a high level. Also, student turnover likely increase with more schools to choose from, something that can have negative consequence for achievement (β_2 <0).

When we estimate (1) we control for individual and (time-varying) municipality variables, as well as a municipality-, and year specific effect. We do not control for school level variables since they are arguably endogenous to the amount of private schooling in the municipality. We also allow the regression error to be correlated between individuals in the same municipality-year cell.[21]

By aggregating (1) at the municipality-year level we get:

(2) $$\overline{y}_{mt} = (\beta_1 + \beta_2)\overline{P}_{mt} + \lambda_m + \lambda_t + \overline{\varepsilon}_{mt}$$

Hence, when we estimate this equation at the aggregated level we get that the independent school-share coefficient equals the sum of the individual effect, β_1, and the spillover effect, β_2, and hence measure the total, or social, effect of independent schooling on the outcome variable. When we estimate (2) we control for time-varying municipality variables, municipality-, and year specific effects, municipality specific linear trends, as well as a variable capturing the pre-reform trend in the outcome variable in the municipality.

We can also estimate (1) using siblings:

(3) $$y_{ijmt} = \beta_1 P_{ijmt} + \beta_2 \overline{P}_{ijmt} + \lambda_j + \lambda_t + \varepsilon_{ijmt}$$

[21] If we allow the regression error to be correlated between individuals in the same municipality we go from 3123 to 284 clusters), the standard errors for the estimate of individual school attendance roughly doubles, and the estimate for the independent school share increase by about 25 percent. These standard errors would likely be upper bounds of the true standard errors.

where y_{ijsmt} denotes performance at the end of the schooling (9th grade) for student i in family j in a school in municipality m at time t; P_{ijmt} is an indicator for whether the school that the individual attend is independent (P=1) or public (P=0); $\overline{P}_{jmt}$ denotes the share of students attending private school at the end of 9th grade at the same time that sibling i ends schooling; λ_j represents family fixed effects; and ε_{ijsmt} is a random error term. The independent school coefficient β_1 measures the effect of attending an independent school for an individual whose sibling went to a public school (or vice versa). The independent school-share coefficient β_2 measures the spillover effect from the amount of school choice in a municipality for an individual whose sibling attend school at the time when there is a different amount of school choice made in the municipality.

IV. RESULTS

IV.A. PRODUCTIVITY

We regress GPA of the student on an independent school dummy and the share of independent school students, using individual data. This is model (2). Table 4 reports these independent school estimates. In columns 1 and 4 we estimate an OLS model. In columns 2 and 5 we control for a full set of municipality indicators, so that these estimates use the association between changes in GPA and changes in independent schooling shares over time, within municipalities. In columns 3 and 6 we also include controls for municipality specific trends, so that these estimates use the association between changes in GPA and changes in independent schooling shares that are deviating from a linear time trend, within municipalities. All estimations include controls for the year the student leave compulsory school. In columns 1-3 no additional controls are added, whereas in columns 4-6 a number of individual and municipality-year controls are added (those are listed in Table 1). In the first row we show estimates of independent school attendance, and in row 2, estimates of the share of independent schooling in the municipality.

We first regress GPA of the student on an independent school dummy and the share of independent school students (column 1). These OLS estimates reveal that attending independent school is associated with 7 percentile ranks higher grades, on average, and one percentile point higher independent school share (about ¼ of a standard deviation) is associated with 0.2 percentile ranks higher grades, on average. In the next two columns we apply the fixed-effects approach. The effect of independent school attendance for the individual is unaffected, whereas the spillover effect decreases a lot and is now smaller than the individual's own return; 0.02-0.04 percentile ranks from a one percentage point increase in this share. In the next three columns we add individual and municipality controls. This makes the individual attendance estimate decrease to 3-4 percentile ranks from attending independent school. The effect for the independent school share is 0.07 percentile points and statistically significant when municipality indicators and individual and municipality characteristics are controlled for.

In table 5, we show estimates from estimating the model (1) using separate subject grades as outcomes. The effect estimates are very similar across subject. For independent school attendance the estimates varies between 3-4 percentile ranks, for independent school share between 5-8 percentile ranks.

In table 6, we show estimates from estimating the model (2) using aggregate data. A expected, the sum of the individual and aggregate independent school estimates from tables 4 and 5, approximately equals the estimates reported in table 6, when the same control variables are included. The social return to independent school attendance is 6-8 percentile ranks, when fixed municipality effects are includes as controls (the last three columns). In column 5, we included a pre-reform trend as control in the fixed effects model. The estimate for the pre-reform is positive and statistically significant. However, including this measure only slightly lowers the estimate.

We now turn to estimation of model (3), using siblings. Results are reported in table 7. In column (1) we estimate the basic municipality fixed effects model using the siblings sample. The estimates are very similar as before (column 2 of Table 4). In column 2-3, we estimate a model where we instead of municipality fixed effects included family fixed effects. Hence, we only use the variation in the data that is due to differences between siblings, within families. The independent school attendance effect is now estimated to about 1.5 percentile rank, about half compared to the estimate without sibling fixed effects (but with other controls; see columns 4-6 of table 4). This estimate is still statistically significant. Regarding the independent school share effects, the estimates now becomes negative but statistically insignificant. The sum of the two estimates is now roughly zero, indicating no social return to independent schooling.

One reason for these low effects is that there is very little variation in these variables between siblings. If we investigate the data we see that there are 1,376 families where at least two siblings choose different schooling types. Regarding the variation in the independent school share, we have that 46 percent of the families have siblings that experience different shares in their municipality at the time they attend 9th grade. However, of the total variance in the independent school share, only about 18 percent is due to variation between families. Hence, these estimates should be interpreted with some care.

IV.B. SORTING

When we look at the effects of choice on segregation, we estimate the following regression:

$$(4) \quad \frac{\overline{X}_{pub,mt}}{\overline{X}_{mt}} = \delta_0 + \delta_1 \overline{P}_{mt} + \lambda_m + \lambda_t + v_{mt}$$

where $\frac{\overline{X}_{pub,mt}}{\overline{\overline{X}}_{mt}}$ is the ratio of the average characteristic (or "quality") of public school students in municipality j to the average characteristics of all students in a municipality; $\overline{P}_{mt}$ is the independent school share over the same time period; λ_m and λ_t represents municipality and year specific effects, respectively; and v_{mt} is a random error term.. The coefficient of interest, δ_1, measures whether the average characteristic of students of public school students in a given community (relative to the average characteristics of all students in the community) has fallen by more in a community where the school choice program had a larger effect. If δ_1 is negative (using a characteristic indicating students performing well), this suggests that school choice caused the public schools to lose their best students. We use the following characteristics to measure the "quality" of the student pool; GPA, whether student does not examines at age (grade repeating=1), parents income, parents education, parents are immigrants and student is an immigrant (all known to be highly correlated with student performance).

Results are reported in Tables 8-10.

As shown in this section, a major effect of the competitive forces unleashed by the 1992 school reforms in Sweden is to induce greater segregation of students by parental income, schooling, and immigration status. In the context of the school choice debate, this finding is important because a central concern in this debate is the impact of choice programs on inequality.

V. SENSITIVITY ANALYSIS

V.A. GRADE INFLATION

Results are reported in Tables 11-12.[22]

VI. DISCUSSION

In this section we discuss our results in relation to those in earlier Swedish studies on the effects of independent schooling, summarized in Table 13. The earliest one is by Sandström & Bergström, (2005), who study the effects of the share of students in independent schools in a municipality on grades and math test scores for a non-representative sample of 28,000 public school students in 34 (out of 284) municipalities in 1998. The find that an increase of one percentage point in the independent school share increase the GPA by 0.24 percentile points (or 0.008 units of a standard deviation) for the average public school students. If they look at math test scores the estimate amounts to 0.42 percentile points.

To correct for endogeneity of non-public schooling, they predict the independent school share using estimates from (a sample selection corrected) estimation using all 284 municipalities, and assume that average grades in 1992, the share of municipality responsibilities that are contracted out to private enterprises, non-socialist government dummy and voting share and population density, do not affect the share of students in independent schools, given other control variables. When this is done, the estimates double.

[22] Relate to Wikström and Wikström (2002), who compare scores given by high school teachers with scores from standardized university entrance examination (SweSAT) and find evidence that private school inflate grades and weaker evidence that public schools in municipalities with many public schools do the same thing. Clearly, Wikström and Wikström's results are at the high school level, and not at the primary school level, but they suggest that average grades will tend to be inflated in municipalities with more public schools (more densely populated regions) and with more private schools (relative to the total number of schools).

We note that it is difficult to believe that some of these variables are valid instruments since there is every reason to believe that children from parents that vote for conservative local governments or demand private services are likely to have better educational outcomes, regardless of the independent enrollment rate. If so, the produced estimates would be biased. Since they only look at outcomes for public school students, they attempt to correct for this by using a standard sample selection model, where the identification comes from assumptions about the distribution of the error term in the main equation.

They also use municipality level data with information on GPA for all municipalities in 1992, 1994-1997. When they run aggregate (unweighted) regressions and control for municipality and year indicators (and some additional variables), they find positive, but statistically insignificant, effects. The data used is here for both public and independent school students, so the estimates capture both the individual and spillover effects.

Ahlin (2005) is able, for a sub-sample of students in the test-score sample in Sandström & Bergström, to control for tests scores in sixth grade, when analyzing individual data on the effect of going to a independent school on grades in ninth grade in 1998. Furthermore, she includes the outcome of independent school students in her sample, and hence, analyzes the effect of independent schooling on all (public and non-public) students. She finds statistically significant positive effects on math performance (half a percentile rank of a percentage point increase in share of independent schooling), but negative and insignificant effects in English, of the share of independent school students. She further finds positive and statistically significant effects of independent school attendance for the individual in Math (about 5 percentile ranks).

Björklund et al. (2003, 2005) extend the analysis in Sandström & Bergström by looking at test scores (about 50,000 students)and grades (total population of students) in math, English and Swedish for 1998-2001. Hence, they can control for municipality fixed effects using test scores. For test scores they find that the share of independent school students to have positive effects on math, English and Swedish test scores. Using grades in these subjects, and the same specifications and sample of students, they find statistically insignificant effects on grades. These effects are always smaller than the test score effects and even negative for math. If they instead use grades for the entire population of students, they find positive and statistically significant effects for all subjects: about 0.4 percentile rank of an increase of 1 percentage point in the independent school share. These effects are much smaller then the results for tests cores. Thus one should be very careful analyzing data from a non-random sample of students, as is done in Sandström & Bergström. The estimated effects of individual independent school attendance are always positive and statistically significant, and ranges from 2-10 percentile ranks.

In general, one might note the very large effects of the independent school share. Even if we take one of the lower estimates, the one for math grade in Björklund et. al., the effect amounts to a 4 percentile point gain of an additional 10 percentage point students in independent school, for the average student (in both public and independent school). This would mean that for a municipality with 1,000 9th grade students and 20 of these independent schools, taking an additional 20 students from the public school to the independent school would lead to the average student to gain almost 1 percentile point in math. Another way of expressing the magnitude of this estimate is to say that the spillover effect is eight times as large as the individual return (40 versus 5 percentile ranks). This is an enormous effect that much certainly should lead to a policy of facilitate the establishment of more independent schools.

The estimate we produce in this paper show individual returns of about 4 percentile ranks and externalities of about 7 percentile ranks (except for the estimates using sibling fixed effects which are much smaller). For math the estimated effects are very similar. Hence, the estimated spillover effects are about 6 times as small in our study. What is then explaining these differences? All these three studies all use independent school share measured for all grades (1-9) in compulsory schooling (Björklund et al (2005), in addition, take averages between last 3 years).[23] Hence, the effects that are analyzed are for an X percentile point increase in independent schooling in grades 1-9, on the academic outcome for a student in ninth grade. In tables 14 (GPA) and 15 (Math), we show results for alternative independent school share measures. Let us concentrate on the estimates in row 2, for independent school share. For GPA, we see that the estimate increase only somewhat if 3 year averages are, or if grades 7-9 is used instead of grade 9. However, the estimate doubles if grades 1-9 are used instead of grade 9. A similar picture is seen for math in table 15. The conclusion is that most of the very high estimated effects reported in earlier Swedish studies appear to be driven by the usage of independent school share in grades 1-9. The picture seen from tables 14 and 15 is that most of this is driven by the independent school share in grades 1-6. We believe it is very difficult to argue why there should be a causal effect from the amount of independent in early grades in year t, on the grades received by those leaving grade 9 the same year.

VII. CONCLUSIONS

[23] The motivations for using independent school shares in grades 1-9 are that pupils in the last three grade levels of public schools might be affected by increased potential competition (Ahlin) and that the school as a whole might change its way of teaching in response to a change in competition (Björklund et al.).

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FIGURE 1

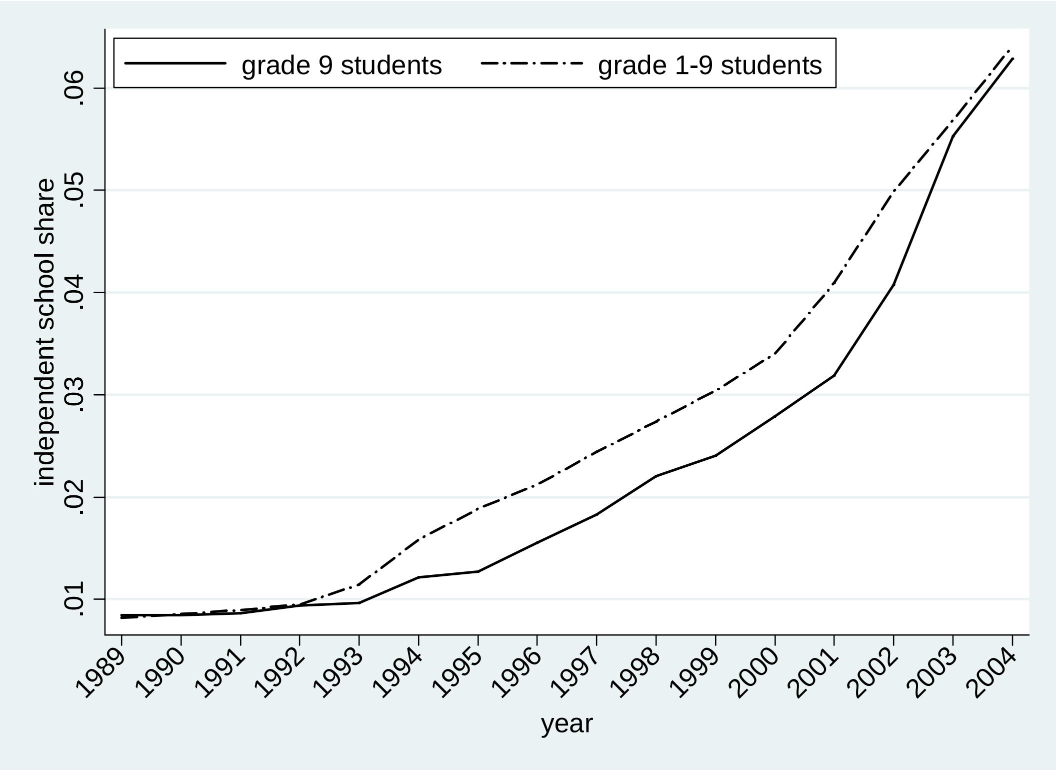
grade 9 students
grade 1-9 students
independent school share
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FIGURE 2

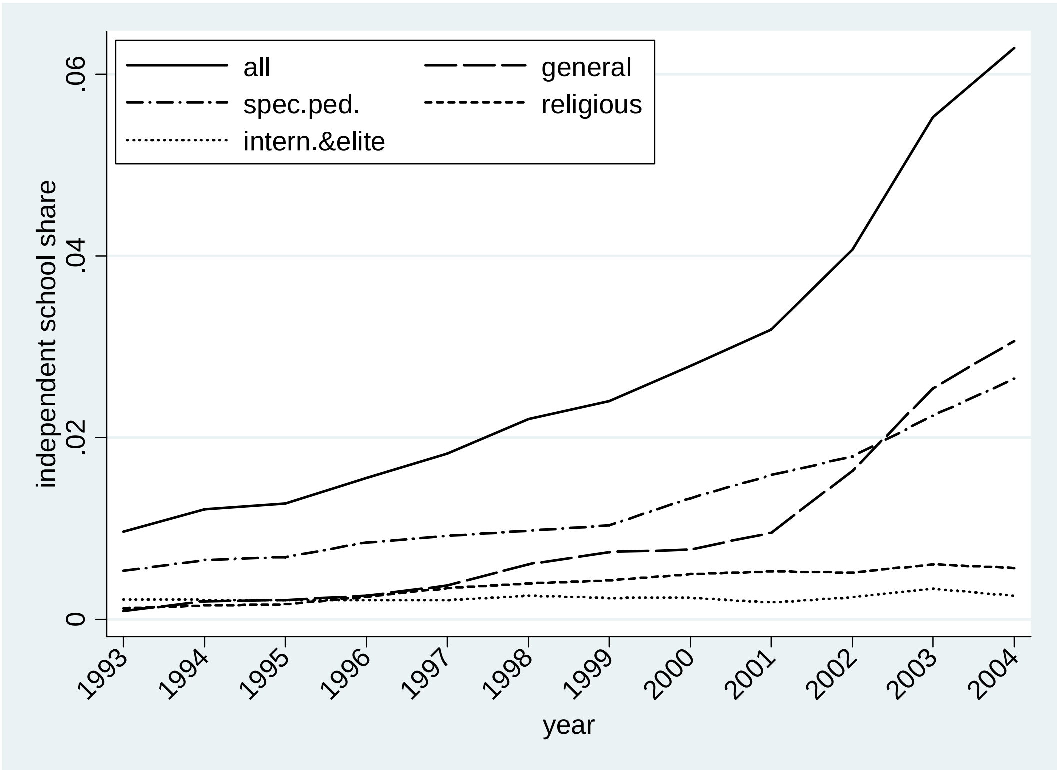
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Table 1: Descriptive statistics

Individual variables

	(1) Mean	(2) St.Dev.
GPA	49.87	23.01
Independent school=1	0.021	0.145
Individual controls		
Male=1	0.51	0.50
Age of father	46.9	5.82
Age of mother	43.98	5.12
Schooling of father	11.57	2.69
Schooling of mother	11.90	2.42
parent university educated=1	0.25	0.43
parent high school educated=1	0.43	0.49
Log family income	8.00	0.51
2nd gen immig	0.063	0.243
Immigrant	0.070	0.255
Immigrant age	0.80	2.99
Immigrant age>=10	0.038	0.192

Municipality-year variables

	(1) Mean	(2) St.Dev.
Independent school share	0.023	0.040
Municipality-year controls		
Fraction male students	0.51	0.061
mean parents' age	45.44	0.96
fraction families with at least one parent university education	0.25	0.10
fraction families with at least one parent high school education (but not long university education)	0.43	0.07
mean log family income	8.00	0.12
fraction 2nd generation immigrants	0.063	0.059
Fraction immigrants	0.070	0.053
fraction immigrated >=10 years of age	0.038	0.029

Notes: GPA is the average (percentile) grades in English, math, natural science and social science subjects. The summary statistics for municipality level variables are calculated for 11 years and all municipalities (note that only 92 out of 284 have any private schooling). Number of observations ranges between 206,311-216,501.

Table 2: Individual level independent school regression

Dependent variable: 9th grade from independent school=1 (public school=0)		
	(1)	(2)
	Probit: marginal effects	Probit with munic controls: marginal effects
Male=1	-0.002	-0.002
	(0.000)**	(0.001)**
Age of father	0.000	0.000
	(0.000)**	(0.000)**
Age of mother	0.000	0.000
	(0.000)**	(0.000)**
Schooling of father	0.002	0.001
	(0.000)**	(0.000)**
Schoolingh of mother	0.001	0.002
	(0.000)**	(0.000)**
parent university educated=1	0.005	0.005
	(0.001)**	(0.001)**
parent high school educated=1	0.002	0.002
	(0.001)**	(0.001)**
Log family income	-0.001	-0.004
	(0.001)	(0.001)**
2nd gen immig	0.028	0.013
	(0.002)**	(0.002)**
Immigrant	0.010	-0.001
	(0.002)**	(0.002)
Age of immigration	0.000	0.001
	(0.000)	(0.000)*
Immigrant age>=10	-0.002	-0.003
	(0.002)	(0.003)
Observations	216501	125248

Notes: All regressions control for year fixed effects. In column (2) we control for municipality fixed effects. We have added indicators who control for missing observations for any of the control variables. The estimates are marginal effects from estimation of Probit models. Standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3: Aggregated independent school regression

Dependent variable: In columns 1-2: fraction of 9th graders in independent schools. In columns 3-4: If any independent school in municipality (=1).

	(1) OLS	(2) FE	(3) OLS: any independent school	(4) FE: any independent school
Fraction male students	-0.022	-0.009	-0.001	-0.011
	(0.016)	(0.013)	(0.071)	(0.047)
mean parents' age	-0.000	-0.001	0.014	-0.003
	(0.001)	(0.001)	(0.006)*	(0.005)
fraction families with at least one parent university education	0.053	0.022	0.914	0.091
	(0.014)**	(0.017)	(0.078)**	(0.065)
fraction families with at least one parent high school education	0.027	0.027	0.006	-0.046
	(0.015)+	(0.013)*	(0.068)	(0.048)
mean log family income	-0.021	-0.009	-0.008	-0.092
	(0.011)+	(0.013)	(0.061)	(0.053)+
fraction 2nd generation immigrants	0.083	0.020	1.025	0.213
	(0.022)**	(0.033)	(0.128)**	(0.132)
Fraction immigrants	-0.027	-0.080	1.174	-0.048
	(0.035)	(0.036)*	(0.180)**	(0.143)
fraction immigrated >=10 years of age	0.071	0.054	-0.155	0.057
	(0.054)	(0.044)	(0.260)	(0.175)
Constant	0.237	0.163	-0.518	1.178
	(0.099)*	(0.120)	(0.548)	(0.466)*
Observations	1012	1012	3123	3123
R-squared	0.26	0.60	0.17	0.70

Notes: All regressions control for year fixed effects. In column (2) and (4) we control for municipality fixed effects. Columns 1-2 only use observations from municipalities with any independent schooling. The dependent variables are: the share of students in independent schooling (columns 1-2) and an indicator=1 is there exist independent schooling in the municipality (columns 3-4). Standard errors in parentheses.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 4: Individual level GPA regressions, against an independent school indicator and the independent school share in municipality

Dependent variable: GPA for student

	(1) OLS	(2) FE	(3) FE + munic spec trends	(4) OLS + controls	(5) FE + controls	(6) FE + controls, munic spec trends
independent school=1	7.212	7.464	7.444	3.559	3.662	3.635
	(0.685)**	(0.684)**	(0.686)**	(0.512)**	(0.508)**	(0.509)**
Fraction of 9th grade students in independent schools; 1993-2003	21.923	2.384	4.108	2.785	6.703	5.148
	(2.881)**	(3.397)	(3.784)	(2.306)	(2.887)*	(3.182)
Constant	49.566	49.778	49.972	-21.148	-35.131	-37.018
	(0.276)**	(0.184)**	(0.244)**	(6.068)**	(7.347)**	(7.248)**
Observations	213873	213873	213873	213873	213873	213873
R-squared	0.00	0.02	0.02	0.21	0.22	0.22

Notes: All regressions control for year fixed effects. Individual controls are included in columns (4)-(6) and are listed in Table 2. We also add indicator variables controlling for missing observations for any of the control variables. Municipality controls are included in columns (2)-(6) which are listed in Table 2. The standard errors in parentheses are corrected for potential serial correlation between students within municipalities-year cells.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 5: Individual level GPA regressions, against an independent school indicator and the independent school share in municipality

Dependent variable: Grades for student

	(1) GPA, FE + controls	(2) Maths, FE + controls	(3) English, FE + controls	(4) Soc.Science, FE + controls	(5) Science, FE + controls
independent school=1	3.662	3.308	4.374	4.122	2.819
	(0.508)**	(0.618)**	(0.628)**	(0.572)**	(0.517)**
fraction of 9th grade students in independent schools; all	6.703	7.442	5.637	4.624	7.741
	(2.885)*	(3.084)*	(3.876)	(3.147)	(2.771)**
Constant	-35.131	-25.889	-40.677	-41.187	-34.307
	(7.342)**	(9.484)**	(9.230)**	(8.595)**	(8.635)**
Observations	213873	213530	212875	213295	213372
R-squared	0.22	0.14	0.17	0.20	0.18

Notes: All regressions control for year fixed effects. Individual controls and municipality controls are included in all columns. We also add indicator variables controlling for missing observations for any of the control variables. The standard errors in parentheses are corrected for potential serial correlation between students within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 6: Aggregate GPA regressions

Dependent variable: Average GPA of all students in municipality

	(1) OLS	(2) OLS + controls	(3) FE + controls	(4) FE + municspec trends	(5) FE with pre-reform trend
Fraction of 9th grade students in independent schools; all	28.248	4.663	7.325	8.042	
	(2.856)**	(2.236)*	(2.831)**	(3.573)*	
Fraction of 9th grade students in independent schools; 1988-2003 agg measure -19					6.587
					(2.458)**
pre-reform trend in GPA, cond on vars					0.049
					(0.011)**
Constant	49.381	2.768	-4.968	-6.471	-8.282
	(0.306)**	(6.147)	(7.739)	(8.093)	(6.515)
Observations	3123	3123	3123	3123	4543
R-squared	0.07	0.44	0.60	0.66	0.56

Notes: All regressions control for year fixed effects. All are weighted regressions, where the weights are the number of 9th grade graduates from all schools in the municipality. Municipality controls are included in columns (2)-(6) and are listed in Table 2. The standard errors in parentheses allow for potential serial correlation within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 7: Individual level GPA regressions, using siblings

Dependent variable: GPA for student	(1) FE	(2) Sibling-differences	(3) Sibling-differences & controls	(4) Sibling-differences & controls, FE
Independent school=1	7.836	1.267	1.433	1.509
	(0.646)**	(0.486)**	(0.477)**	(0.486)**
Fraction of pupils (in grade 9) in independent schools	3.554	-0.351	-0.807	-0.591
	(3.092)	(2.281)	(2.269)	(2.475)
Constant	52.270	54.101	57.089	60.183
	(0.205)**	(0.147)**	(0.165)**	(2.292)**
Observations	195171	195171	195171	195171
R-squared	0.02	0.72	0.73	0.73

Notes: All regressions control for year fixed effects. Individual controls (that varies between siblings) are included in columns 3 and 4 and are listed table 2. The standard errors in parentheses are corrected for potential serial correlation between students within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 8: Aggregate sorting regressions

Dependent variable: Average outcome of all students in municipality schools in municipality / average outcome of all students in all schools in municipality

	(1) GPA: OLS	(2) GPA: FE	(3) GPA: FE + mun spec trend	(4) GRADEREP: OLS	(5) GRADEREP: FE	(6) GRADEREP: FE + mun spec trend
Fraction of 9th grade students in independent schools; 1993-2003	-0.119	-0.016	-0.010	-0.131	-0.422	-0.454
	(0.027)**	(0.033)	(0.029)	(0.268)	(0.652)	(0.747)
Constant	1.000 (0.001)**	0.998 (0.001)**	1.000 (0.001)**	0.986 (0.010)**	0.990 (0.014)**	0.984 (0.024)**
Observations	1012	1012	1012	860	860	860
R-squared	0.26	0.56	0.65	0.01	0.21	0.34

Notes: All regressions control for year fixed effects. All are weighted regressions, where the weights are the number of 9th grade graduates from all schools in the municipality. The standard errors in parentheses allow for potential serial correlation within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 9: Aggregate sorting regressions

Dependent variable: Average outcome of all students in municipality schools in municipality / average outcome of all students in all schools in municipality

	(1) INCOME: OLS	(2) INCOME: FE	(3) INCOME: FE + mun spec trend	(4) EDUC: OLS	(5) EDUC: FE	(6) EDUC: FE + mun spec trend
Fraction of 9th grade students in independent schools; 1993-2003	-0.011	-0.004	-0.010	-0.069	-0.022	-0.032
	(0.003)**	(0.005)	(0.005)*	(0.011)**	(0.010)*	(0.010)**
Constant	1.000	1.000	1.000	1.000	0.999	0.999
	(0.000)**	(0.000)**	(0.000)**	(0.000)**	(0.000)**	(0.001)**
Observations	1012	1012	1012	1012	1012	1012
R-squared	0.11	0.41	0.54	0.37	0.65	0.72

Notes: All regressions control for year fixed effects. All are weighted regressions, where the weights are the number of 9th grade graduates from all schools in the municipality. The standard errors in parentheses allow for potential serial correlation within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 10: Aggregate sorting regressions

Dependent variable: Average outcome of all students in municipality schools in municipality / average outcome of all students in all schools in municipality

	(1) IMMIG: OLS	(2) IMMIG: FE	(3) IMMIG: FE + mun spec trend	(4) 2 GEN IMMIG: OLS	(5) 2 GEN IMMIG: FE	(6) 2 GEN IMMIG: FE + mun spec trend
Fraction of 9th grade students in independent schools; 1993-2003	0.293	0.382	0.519	-0.387	-0.334	-0.459
	(0.096)**	(0.220)+	(0.263)*	(0.119)**	(0.262)	(0.318)
Constant	0.997	0.995	1.006	0.995	0.995	0.981
	(0.004)**	(0.005)**	(0.012)**	(0.007)**	(0.009)**	(0.014)**
Observations	886	886	886	862	862	862
R-squared	0.05	0.22	0.37	0.05	0.21	0.30

Notes: All regressions control for year fixed effects. All are weighted regressions, where the weights are the number of 9th grade graduates from all schools in the municipality. The standard errors in parentheses allow for potential serial correlation within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 11: Individual “grade inflation” regressions

Dependent variable: subject grade of individual (at end of 9^{th} grade level)

	(1) ENG: OLS	(2) ENG: OLS + controls	(3) MATH: OLS	(4) MATH: OLS + controls
Independent school=1	-1.580	-1.914	0.159	-0.556
	(0.908)+	(0.904)*	(0.947)	(0.914)
english test score, combined; 9th grade, 1998-2000&2003	0.959	0.935		
	(0.004)**	(0.005)**		
math test score, combined; 9th grade, 1998-2000&2003			0.800	0.777
			(0.006)**	(0.007)**
Constant	4.718	-9.024	11.909	-5.548
	(0.453)**	(2.002)**	(0.550)**	(2.329)*
Observations	20575	20575	15210	15210
R-squared	0.75	0.75	0.65	0.66

Notes: All regressions control for year fixed effects. Individual controls are included in columns (2) and (4) and are the one listed in Table 2. We have added indicators who control for missing observations for any of the control variables. The standard errors in parentheses allow for potential serial correlation within schools.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 12: Aggregate "grade inflation" regressions

Dependent variable: aggregate subject grade (at end of 9th grade level)

	(1) ENG: OLS	(2) ENG: OLS, controls	(3) MATH: OLS	(4) MATH: OLS, controls
Fraction of 9th grade students in independent schools; 1993-2003	-6.202	-1.674	0.728	-1.485
	(3.603)+	(4.636)	(4.584)	(5.610)
(mean) pengt	0.911	0.950		
	(0.028)**	(0.036)**		
(mean) pmatht			0.754	0.722
			(0.044)**	(0.043)**
Constant	6.990	11.034	13.370	41.106
	(1.409)**	(12.443)	(2.369)**	(16.007)*
Observations	327	327	315	315
R-squared	0.75	0.76	0.63	0.67

Notes: All regressions control for year fixed effects. Aggregated municipality-year controls are included in columns (2) and (4) and the one listed in table 2. The standard errors in parentheses allow for potential serial correlation within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 13: Earlier Swedish studies of independent school effects on achievement

Effect estimates:				Individual attend independent school (β_1)	Independent school share (β_2)	$\beta_1 + \beta_2$	Notes (How effect variable is measured, and other issues)
Sandström and Bergström (2005),	Indiv data, n=28,000 (only public school students. Non-random sample)	GPA	OLS		24.1 (12.8)		*Measure of independent school share is based on grades 1-9. (mean (stdev)) =0.016 (0.031), min=0, max=14.6. * Standard errors adjusted for clustering at the school level.
			IV		66.4 (30.2)		
		Math test	OLS		42.5 (16.7)		
			IV		84.8 (40.5)		
	Aggreg. data, 92, 94-97 m=288	GPA	OLS			132 ? (45.0)	
			FE			61.0 (64.2)	
Ahlin (2003),	Indiv data, n>6,000	Math test (B)	OLS			69.0 (25.8)	*Measure of independent school share is based on grades 1-9, for those residing in the municipality. (mean (stdev)=0.017 (0.023), min=0, max=9.7.
			VA	5.01 (2.07)	51.2 (22.5)		
		English test	OLS			-5.2	

						(24.8)	
			VA	2.24 (3.08)	-24.4 (20.3)		*Standard errors adjusted for clustering at the school level
<u>Björklund, Clark, Edin, Fredriksson & Krueger (2005),</u>	Indiv data, n=50,000 for test scores (non-random sample). Total population for grades Years 1998-2001	Math grade	FE	5.09 (1.23)	40 (11)		*Measure of independent school share is based on grades 1-9, averaged over three three latest years. *Standard errors adjusted for clustering at the municipality level
		Math test (A)	FE	2.46 (1.94)	101 (56)		
		Math test (B)	FE	6.62 (2.67)	111 (48)		
		English Grade	FE	7.41 (1.48)	41 (12)		
		English test A	FE	9.36 (1.53)	206 (49)		
		English test B	FE	8.57 (1.03)	164 (61)		
		Swedish Grade	FE	5.39 (1.08)	36 (14)		
		Swedish test	FE	7.73 (1.45)	194 (76)		

Notes: All effect estimates are scaled in percentile ranks.

Table 14: Individual level GPA regressions, against an independent school indicator and the independent school share in municipality, for various measures of independent school share

Dependent variable: GPA for student

	(1) grade9, baseline	(2) grade9, 3 years average	(3) grade7-9, baseline	(4) grade7-9, 3 years average	(5) grade1-9, baseline	(6) grade1-9, 3 years average
independent school=1	3.662	3.689	3.637	3.651	3.637	3.619
	(0.508)**	(0.506)**	(0.508)**	(0.507)**	(0.511)**	(0.511)**
Fraction of 9th grade students in independent schools; 1993-2003	6.703	8.811	8.107	9.708	13.075	17.466
	(2.885)*	(4.135)*	(2.735)**	(3.771)*	(3.752)**	(4.796)**
Constant	-35.131	-34.772	-35.722	-35.159	-36.534	-36.504
	(7.342)**	(7.340)**	(7.339)**	(7.340)**	(7.370)**	(7.367)**
Observations	213873	213873	213873	213873	213873	213873
R-squared	0.22	0.22	0.22	0.22	0.22	0.22

Notes: All regressions control for year fixed effects. Individual and municipality controls are included in all columns and are listed in Table 2. The standard errors in parentheses are corrected for potential serial correlation between students within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 15: Individual level GPA regressions, against an independent school indicator and the independent school share in municipality, for various measures of independent school share

Dependent variable: Math grade for student	(1) grade9, baseline	(2) grade9, 3 years average	(3) grade7-9, baseline	(4) grade7-9, 3 years average	(5) grade1-9, baseline	(6) grade1-9, 3 years average
independent school=1	3.308	3.302	3.292	3.276	3.274	3.247
	(0.618)**	(0.614)**	(0.616)**	(0.616)**	(0.618)**	(0.617)**
Fraction of 9th grade students in independent schools; 1993-2003	7.442	12.063	8.525	11.877	14.967	20.514
	(3.084)*	(4.528)**	(3.002)**	(4.110)**	(4.188)**	(5.334)**
Constant	-25.889	-25.721	-26.438	-26.064	-27.538	-27.581
	(9.484)**	(9.479)**	(9.484)**	(9.479)**	(9.515)**	(9.510)**
Observations	213530	213530	213530	213530	213530	213530
R-squared	0.14	0.14	0.14	0.14	0.14	0.14

Notes: All regressions control for year fixed effects. Individual and municipality controls are included in all columns and are listed in Table 2. The standard errors in parentheses are corrected for potential serial correlation between students within municipalities-year cells.
+ significant at 10%; * significant at 5%; ** significant at 1%