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Does grade configuration matter for school performance?

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WORKING PAPER 2017:6

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ISSN 1651-1166

Does grade configuration matter for school performance? Short- and long-run effects of school reorganisation[1]

by

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May 18, 2017

Abstract

This paper studies the effects of school organisation on short- and long-run pupil outcomes, exploiting a policy change that reorganised Swedish middle school education. The reorganisation induced pupils to remain in small local schools throughout grades 1–9, as opposed to making a transition to large middle schools between grades 6 and 7. We find that the reorganisation had large consequences for pupils, who came to attend smaller schools closer to home, whose teachers had lower qualifications. Despite that the previous literature has found that school transitions and school size are important inputs in the education production function, we find no evidence that remaining in a small local school and avoiding a transition to a large middle school had effects on educational outcomes. We reconcile our evidence with the previous literature using a survey which shows that Swedish pupils do not perceive large differences in the school environment between schools of different grade configurations.

Keywords: Grade configuration; educational production; school transitions; school size
JEL-codes: H52; I21; I28

[1] The authors wish to thank Hans Grönqvist, Monique de Haan and seminar and conference participants at IFN, University of Sussex, the IFAU education conference 2016, the ESPE conference 2016 and the EALE conference 2016 for valuable comments. Financial support from the Swedish Research Council for Health, Working Life and Welfare (FORTE) [grant nr 2013-0645], and the Swedish Research Council (Vetenskapsrådet) [grant nr 2013-01992] is gratefully acknowledged.
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1 Introduction

The question of how pupils are grouped and schools are organised has received a considerable amount of attention both in policy making and in education research. Examples include the shift from selective to comprehensive school systems in many countries, the use of ability tracking, the nature of peer effects, and class size. More recently, the economics of education literature has started to address whether school size and grade configuration are important in the education production function. If these latter aspects of how a school is organised are important for student outcomes, school reorganisation might constitute an effective means to improve pupil performance at a potentially low cost. Needless to say, many of the determinants of child development, such as family background, are pre-determined or costly to alter. When it comes to other inputs, such as teacher quality, policies that aim to increase the skills and qualifications of teachers will affect the stock of teachers only in the long run. As such, schools' organisation might in the short run serve as a more efficient policy.

This paper evaluates a reorganisation of Swedish middle school education, which took place in the mid-1990s after the passing of a new curriculum. As a result of the new curriculum, many feeder schools that had previously covered grades 1–6, came to expand and include also middle school grades 7–9. This reorganisation of schools' grade configuration implied that pupils who would otherwise have had to transition to a new school between grades 6 and 7, remained in the same school throughout compulsory education. This allows us to study the effects on pupils' educational attainment from attending a grade 1–9 school, as opposed to transitioning to a grade 7–9 middle school. This paper thus studies the effects of grade configuration in school in a context outside of the U.S., where Rockoff and Lockwood (2010) and Schwerdt and West (2012) have concluded that middle schools are detrimental for pupils' learning and that moving from primary to a middle school in grade 6 or 7 implies 0.10–0.15 standard deviation lower test scores both in the transition year and beyond, compared to remaining in a K–8 school. The current study therefore contributes to the education literature by shedding light on whether these results, based on evaluations of public schools in New York City and Florida, can be generalized to a wider context. Our study also speaks to the literature on school size and its effects on educational attainment. Importantly, the use of Swedish data allows us to follow pupils who were exposed to a change in grade configuration into adulthood, and we can therefore address the question as to whether short-run effects are carried over into long-term outcomes.

Why, and how, does grade configuration affect the inputs in the education production function? Two main mechanisms are discussed in the previous literature. First, middle schools typically have several parallel classes in each grade and hence larger cohort sizes compared to local schools that cover all grades (e.g. K–8 in the US, 1–9 in Sweden). It has been proposed that adolescents may be difficult to educate in a school with many peers of the same age (Rockoff and Lockwood 2010). For example, anti-social behaviour might become more widespread and more difficult to prevent in schools with large numbers of teenagers. Second, it is possible that any move to a new school has detrimental effects on school performance (Hanushek et al. 2004), especially for young adolescents whose behaviour and self-image may be negatively affected by moving to a middle or junior high school (Steinberg 2011).

Although most of this discussion points to negative effects of organizing schools in a manner that implies a transition to large middle schools, there might also be effects going in the opposite direction. For example, large middle schools that serve many neighbourhoods typically have a more mixed pupil composition than local schools. Depending on the nature of peer effects, this may or may not have consequences for pupil performance. Furthermore, cohort size in itself might have implications for the efficiency of resource allocation. If economies of scale are important in education production, larger cohort sizes and/or larger schools should be more efficient. Large schools (or larger cohorts) might for example allow for a more efficient allocation of teachers to classes, and might also provide a more stimulating work environment for teachers.

Earlier research has found that remaining in a K–8 school (Rockoff and Lockwood 2010; Schwerdt and West 2012), and attending small high schools (Abdulkadiroglu 2013; Schwartz et al. 2013; Bloom and Unterman 2014; Barrow et al. 2015), have positive effects on pupils' outcomes. Rockoff and Lockwood (2010) and Schwerdt and West (2012) find that this positive effect is particularly pronounced for disadvantaged children, and the evidence from small high schools also comes from primarily disadvantaged and minority pupils. Trying to understand the underlying mechanisms behind these results, the papers suggest that early transitions and school environment are key explanations.

To study the effects of grade configuration in the Swedish context, we identify students that were exposed to an organisational change: treated students lived in neighbourhoods where the default school was a primary feeder school (grade 1–6) that expanded to cover all grades 1–9, and as such pupils were induced to stay in a 1–9 school rather than transitioning to a middle school. We adopt a difference-in-differences strategy, where pupils belonging to treated neighbourhoods are compared to pupils living in untreated neighbourhoods or in neighbourhoods where treatment occurred at a different point in time. Importantly, within the same municipality (i.e. local authority) there are both treated and untreated neighbourhoods, which allows us to include cohort*municipality fixed effects. Letting the cohort effects vary by municipality is a way of controlling for local education policy initiatives, which is important in Sweden's decentralised school system.

The identification is based on the assumption that untreated neighbourhoods can accurately represent the trend in treated neighbourhoods in the absence of any intervention. This assumption is not testable, but we are able to test for parallel trends in the period before the intervention was introduced. We do this by including pre-treatment "placebo" dummies in all our regressions. We show that the reorganisation had large consequences for both the social and learning environment for treated pupils, but these do not carry over into effects on pupils' educational achievement. Exposure to treatment reduced the probability of pupils transitioning to a grade 7–9 middle school by 76 percent. Remaining through grade 7–9 in an expanding school implied a reduction in the peer cohort size in the school of more than two full classes (60 pupils); it increased the probability of attending a school close to home (in the same neighbourhood) by 38 percentage points; and it resulted in more homogenous peer groups. Moreover, we find that pupils attending an expanding school had less qualified teachers, although the quantitative teacher inputs in terms of teacher-pupil ratios were unaffected. Moving on to study educational outcomes, we find no evidence that attending a grade 1–9 school had any effects on average, on pupils' progression through the education system. The effects on GPA, high school graduation and university attendance are close to zero and in most cases precisely estimated. The lack of effects should not be attributed to the initial start-up phase of a new school, as we can show that the effects are stable around zero up to four years after the school reorganised. To further understand this finding, we turn to pupil surveys to learn more about the school environment in schools of different grade configurations and of different size. We find neither systematic nor statistically significant differences in pupils' perception of the school environment between schools of different grade configuration or size. The absence of any differences in the survey data thus corroborates our findings, and also squares well with the previous literature where effects on school performance have been accompanied by differences in survey-reports on the school environment across school types.

The remainder of the paper is organised as follows: Section 2 summarises earlier related work on school organisation and school size, Section 3 presents the relevant institutional details of the Swedish education system and the change in schools' organisation, Section 4 presents the data and descriptive statistics, Section 5 focuses on the empirical model, the main results and discusses potential threats to identification. Finally, Section 6 offers a discussion of our results in relation to the previous literature and concludes the paper.

2 Previous literature related to school organisation

Earlier work on school organisation, including effects of school size and grade configuration, has primarily been based on empirical correlations, and has not adopted methods that allow for a causal interpretation. Recently, however, a number of studies have emerged which address the causal impact of schools' organisation on pupil outcomes.

In the literature on alternative grade configurations, Rockoff and Lockwood (2010) and Schwerdt and West (2013) compare the progression of pupils who transition to middle schools in grade 6 or 7, with pupils who remain in elementary schools throughout K–8. Identification comes from adopting student-fixed effects, and given that type of school attended is likely to be an endogenous choice, the transition to a middle school is instrumented using the terminal grade of the school attended in grade 3. These two studies use similar empirical designs on data from New York and Florida, respectively, and their findings are consistent: attending a middle school has a negative effect on pupils' test scores. Pupils experience a sharp drop in achievement in the transition year (0.10–0.15 standard deviations); the negative effects persist beyond this year, and are stronger for pupils with low initial achievement. A number of hypotheses related to both school resources and school environment are put forward as potential explanations to this result. While resources do not seem to differ between schools, middle schools have larger cohort sizes and survey evidence report that middle schools are considered less safe and have more problems with anti-social behaviour than elementary schools (Rockoff and Lockwood 2010). It is also possible that the transition in itself has detrimental effects: switching schools is generally found to have a negative effect on school performance (Hanushek et al. 2004).

The interplay between schools' organisation and pupils' motivation and educational performance has also been studied in the psychology literature. Steinberg (2011) summarises some of the recent findings, and concludes that school transitions among young adolescents generally imply a drop in academic motivation and that behaviour and self-image may be affected by moving to a middle or junior high school. School transitions are found to be more detrimental for pupils who had initial low achievement or behavioural or psychological problems. Taken together, the evidence both from economics and psychology point in the same direction: transitions to middle schools have negative impacts on students' academic achievement. These are likely explained by the disruption due to the school transition experienced by the pupil, and by a worse school environment in terms of safety and behaviour in larger middle schools compared to smaller elementary schools.

A different but related strand of the literature on how schools are organised focuses on school size. In the U.S., the so called “small schools movement” has gained attention and philanthropic support, and in a number of cities (Chicago and New York among others) initiatives to open small high schools have emerged. The small schools movement is motivated by the idea that small schools provide a better learning environment for adolescents: small schools are believed to be safer, to provide better relationships between teachers and students, to allow for better monitoring of disruptive behaviour and to increase parents’ and students’ engagement with the school (see e.g. Barrow et al. 2015 for a summary).

The effects of small high schools have been studied with two different empirical approaches. Abdulkadiroglu et al. (2013) and Bloom and Unterman (2014) exploit the random assignment mechanism to New York City’s high schools in an IV framework to assess the causal effects of attending small high schools. They find large benefits of small school attendance on a range of outcomes that capture test scores, high school graduation rates and college attendance. Abdulkadiroglu et al. (2013) present evidence from student and teacher surveys which indicate that the small school environment is favourable in many respects – it offers a higher level of engagement and better relationships between teachers and students, and a higher level of safety. Schwartz et al. (2013) and Barrow et al. (2015) instead use distance to small schools as an instrument for small school attendance in New York City and Chicago, respectively. Their findings indicate that small school attendance increases high school graduation rates, while there is less evidence of any effects on test scores.

The literature on school organisation has also evaluated the effects of school consolidations and school closings. In a recent paper Brummet (2014) finds that the closing of low-performing schools in Michigan generated some achievement gains for displaced students, but also some negative spillover effects on students in receiving schools. Recent evidence from European countries include Beuchert et al. (2016) who find that school consolidation in Denmark generally had adverse effects on student achievement but that the effects seem to be short-lived and due at least in part to a temporary disruption, and De Haan et al. (forthcoming) who find that an increase in the minimum required school size in the Netherlands had a small positive effect on student achievement.

Taken together, the U.S. literature suggests that remaining in K–8 schools and attending small high schools is beneficial for pupil performance and progression through the education system. There is less evidence from European countries on these topics and it is not clear whether results based on mostly disadvantaged populations in the U.S. can be generalised to other contexts. We will return to this discussion when we interpret our results in the concluding Section 6.

3 The Swedish education system and the change in schools' organisation

Compulsory education in Sweden spans grades 1–9, with school starting in the fall the child turns seven. Compulsory education is comprehensive, and it is uncommon to use ability tracking within schools.[4] After 9 years, pupils move on to vocational or academic upper-secondary education ("high school") for three years.[5] Selection into high school is based on 9th grade GPA. The school organisation has traditionally been divided into three stages, comprising grades 1–3, 4–6, and 7–9, and schools were often organised around these stages into grade 1–6 schools, grade 7–9 schools, or grade 1–9 schools spanning all years of compulsory education.

In the early 1990s, compulsory and high school education was decentralised and has since been run by Sweden's about 290 local authorities ("municipalities"), which are responsible for the implementation of the national curriculum and the organisation of schools.[6] In order to further enforce the decentralised model, a new curriculum was introduced in 1994, in which goal-steered instruction became a key element. The new curriculum abandoned the previous three-stage division of compulsory education, and introduced extensive discretion for municipalities in terms of how to organise educational content and among other things implied complete freedom to allocate subject teaching hours across all nine compulsory years. As a response to this curriculum change, many municipalities considered the transition of pupils from primary feeder schools to grade 7–9 middle schools to be outdated and in conflict with

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[4] Two-level tracking in English and mathematics was in practice in grades 7–9 for cohorts graduating from 9th grade until 1997.

[5] From 1994 and onwards, all high school tracks were 3 years long. In the preceding system vocational tracks were 2 years, but these programmes were gradually prolonged and from 1994 and onwards all pupils entering high school faced 3 year programmes.

[6] The decentralisation reform took place in the early 1990s and comprised of several elements. First, the ear-marked state grant for education spending was gradually abolished and education funding was included in the state block grant to municipalities. As such, municipalities were given full discretion over allocation of resources across different municipal tasks. Second, a choice and voucher reform implied that municipalities had to provide a per-pupil school voucher to approved independent schools and that pupils could choose to opt out of the local public school in their catchment area, either to another public school or to an independent school. Finally, a new curriculum was introduced and one of its elements was to allow for a local allocation of the mandated teaching hours across the nine years of compulsory schooling.

the intentions of the curriculum, which endorsed a local and flexible allocation of teaching hours. School transitions would require a more centralised plan of how to allocate teaching hours to ensure that all pupils would be guaranteed the minimum amount of hours in each subject. To accommodate the intentions of the 1994 curriculum, many primary schools were therefore reorganised and became 1–9 schools, spanning the full length of compulsory education. The decision to expand a primary school was made by the municipality, i.e. the governing body of the school. Figure 1 depicts the pre- and post-intervention systems for the affected students. In the pre-intervention period, students in the affected schools made a transition from a feeder school to a middle school between grades 6 and 7. The middle school received students from more than one feeder school. The middle school was either a separate middle school (only grades 7–9) or a middle school section within a 1–9 school. The school expansion was gradually phased in, implying that the first year, the school added a 7^{th} grade, the second year an 8^{th} grade and the third year the school was fully expanded and covered grades 1–9. As a consequence, students were not forced to transition to a middle school.

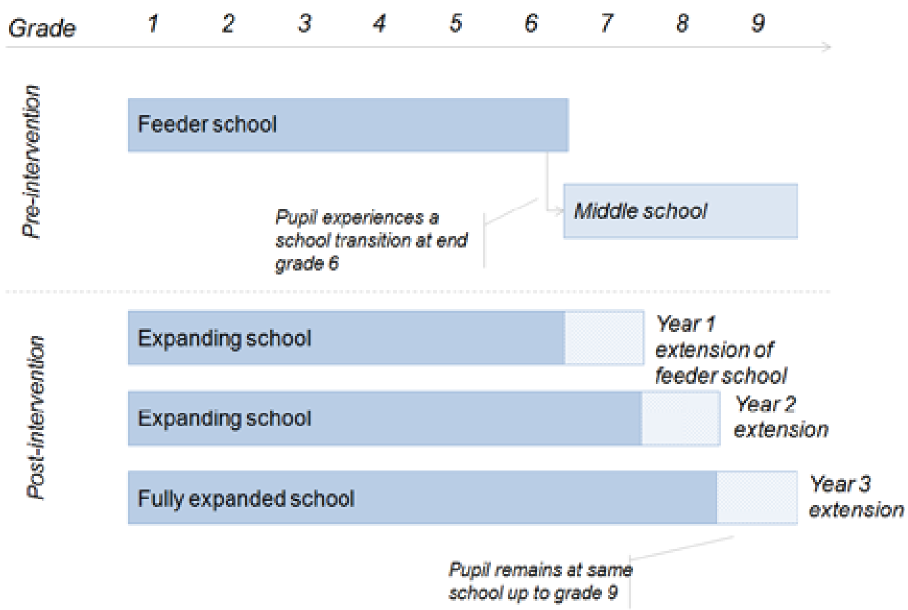

Figure 1. Pre- and post intervention systems for affected pupils

As a result of this organisational change, the total number of schools with grades 7–9 increased: instead of grouping many middle school-aged students in large schools, students were allocated over a larger number of school units. In Figure 2 this development is notable: the number of municipality-run grade 1–9 schools grew from 523 in 1994 to 819 in 2002. The reorganisation was however concentrated to small school units, and the fraction of pupils graduating from a grade 1–9 school increased only by 10 percentage points, from 43 to 53 percent. Naturally, at the same time the number of grade 7–9 middle schools decreased, which is due both to the shift toward more 1–9 schools but also due to some of these schools incorporating grade 6.

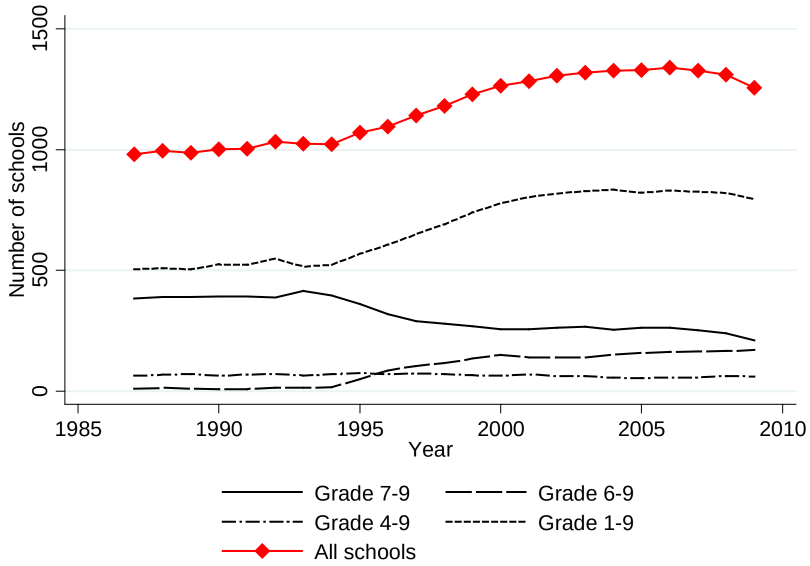

Figure 2. Number of public schools with different grade configurations in Sweden

Note: Including only schools with 9th grade

A majority of children attend the local public school in their catchment area. Formally, there is school choice and families can choose a school (public or publicly funded but "independent") outside of their neighbourhood.[7] However, since the admissions rules to public schools are based on proximity, de facto school choice between public schools is limited and as such the neighbourhoods in close proximity to a school constitute its catchment area. In the period that we study, we approximate that 79 percent of all 9th grade pupils attended the public school assigned to them based on their place of residence in grade 1 (see Table A2).[8] After the time period of our analysis, it became more common to opt out of the assigned school, but opting out could also have been affected by the intervention and it will be investigated further in Section 5.

[7] Fully private education is very rare in Sweden. Less than 1 percent of compulsory level pupils attended tuition-charging private schools in the time period that we study in this paper.

[8] If we instead assign the default public school based on pupils' residence in 9th grade, the fraction attending the assigned school is 85 percent. As an alternative, we can assign pupils to default schools based on where pupils in the cohort one year older went to school; then the corresponding fraction is 74.

4 Data and descriptive statistics

Our data are based on administrative registers from Statistics Sweden. The population of interest is all pupils observed in the *grade 9 register,* graduating from 9th grade (the year they turn 16) in the period 1994–2002. Using the *multi-generation register,* we merge pupils to their parents. We then use several different registers to retain information on pupil outcomes and parental background, and to construct catchment areas and define the treatment and control group.

Pupil outcomes. We measure pupils' outcomes in the short run using their GPA at age 16. Our primary measure of GPA is the percentile rank of the average grades across all subjects, but we also specifically look at percentile ranks in English and mathematics.[9] Sweden, unlike many other countries, does not have centralised and externally marked tests. Although pupils take centralised tests in some subjects, these are often graded by the pupils' own teachers using centrally provided guidelines. Results from centralised tests are not available in the register data for the cohorts in this study, and we are therefore left with the teacher assessed GPA as our only measure of short-run school performance. This is a limiting factor since we cannot rule out that teachers at different types of schools are more or less lenient in setting grades. By studying English and mathematics, subjects in which pupils take centralised tests, we may to some extent alleviate this concern since teachers can use the test results to benchmark grades. We also try to gain some insight into whether the use of the grade scale differs across schools by constructing a measure of grade inflation. This outcome measures the difference between grades in "soft" subjects (music, arts, handicraft, home economics and physical education) and grades in English and mathematics. The rationale for this measure is that there are centralised tests in English and mathematics, which help teachers to benchmark the grades, while teachers have more discretion in other subjects and soft subjects have been shown to be particularly prone to inflation (Vlachos 2010). As such, the difference in percentile ranked grades between soft subjects and exam subjects is an indicator of whether the school is on average more or less lenient in the adaptation of the grade scale.

There is still reason to be concerned, however, that the GPA is not an accurate measure of school performance. Therefore we turn to alternative outcomes, which capture effects on pupils' progression in the education system in the medium- and long-run. We focus on graduation from upper-secondary (high) school and type of high school degree (academic or

[9] The grade scale in place for cohorts graduating until 1997 ranged from 1–5 and the GPA is calculated as the average across all subjects. For later cohorts, a different grade scale was introduced: fail, pass, pass with distinction and pass with special distinction. In order to make the grades from the two different scales comparable, we first converted the non-numerical scale into numbers and then converted all scores into per-cohort percentile ranks.

vocational), and a measure of college attendance by 2013, when the youngest cohort is 27 years old.[10]

Additional outcomes at the pupil level that are explored in the analysis include mandatory school transitions (described below) and type of school attended (assigned school, independent school, school located in pupil's residential neighbourhood), and peer exposure. Peer exposure is a measure of the fraction of peers (within school and cohort) that belong to a certain group, e.g. with high or low educated parents.

School transitions. Feeder schools send their pupils to attend grades 7–9 either in separate middle schools (only grades 7–9) or in a middle school section of a large grade 1–9 school. In the data we observe the grade configuration of all schools, but at the pupil level we only observe the school attended (and its grade configuration) in 9th grade. This means that we can identify school moves for pupils who transition between grades 6 and 7 into a separate 7–9 middle school, but not for pupils who transition into a middle school section of a 1–9 school. In the former case it is obvious that a transition has taken place, while in the latter case we cannot distinguish between pupils who entered the school in grade 7 from those who attended the 1–9 school also in their primary years.

We define a school transition as attendance of a grade 7–9 middle school, but using this definition the fraction of pupils making a transition is underestimated, as will be the "first stage" effect of the intervention on the school transition probability. In Section 5.1 we will address this issue by discussing how we can back out the correct first stage. Importantly, however, we choose to base the empirical analysis on *all* expanding feeder schools (that would send pupils either to 7–9 middle schools or to grade 7 in a 1–9 school). The reason for this is that grade 1–9 schools that receive pupils in grade 7 will have a middle school section that resembles a separate middle school, and avoiding a transition from a feeder school to a receiving school should in this case be comparable regardless of the exact grade configuration of the receiving school. Put differently, remaining in a small local 1–9 school is something very different from making a mandatory transition between grades 6 and 7 and ending up in a large 1–9 school.

Parental background. A pupil's background is characterized by the number of siblings, mother's and father's years of schooling, an indicator for whether mother and/or father holds an elite university degree (law, medicine, economics or engineering) and by mother's and

[10] Pupils who are registered in high school (either at an academic or vocational track) in the third and final year, at any point in time, are defined as high school graduates. Unfortunately, the records that define a high school diploma have changed over time, which implies that we cannot use the actual completion of a diploma to measure high school graduation. College attendance is measured as having college credits corresponding to at least one semester of full time studies.

father's earnings, measured as an average of parents' earnings when the child is aged 7–16. Furthermore, migration history is captured by indicators for whether the pupil is foreign-born, or born in Sweden to two foreign-born parents. We also adopt a measure of predicted GPA, which is the GPA predicted by pupils' parental background and migration history, using the variables described above. Whenever pupils are categorised as having parents with high or low education, high education refers to at least one parent with post-secondary education, while remaining pupils are classified as having low educated parents.

School and teacher characteristics. The school register provides information on the number of pupils in different grades, which we use to categorise schools into different types of grade configurations. We also use pupil-level data to construct school-by-cohort-level measures of the variation in pupils' socio-economic background. In detail, we calculate the coefficient of variation in parental background characteristics (mother's and father's years of schooling).

Short of data on education spending, we turn to the teacher register to understand how resources are allocated across schools, and to shed light on the qualifications held by teachers at different types of schools. Teachers arguably constitute the most expensive resource in schools and provide an important insight into how different grade configurations affect resource efficiency.[11] As this is the only resource measure at our disposal, we cannot shed light on other aspects of economies of scale, such as costs related to running of school premises and the like.

The teacher register links teachers to schools and provides information about which subjects teachers teach, and what qualifications they hold. The register does not link teachers to specific classes and pupils at the school. We base our teacher variables exclusively on teachers that can be identified from the register to primarily teach in grades 4–9 (henceforth "middle school teachers").[12] We focus on teacher inputs only in grades 4–9, as this is the level were pupils in expanding schools might face different teacher inputs compared to a counter-factual situation of switching school between grades 6 and 7. In order to measure quantitative teacher inputs, we define the teacher-pupil ratio as the number of (full-time equivalent) middle school teachers per grade 4–9 pupil in a given school and year. We also construct three proxies for teacher quality: the fraction of qualified teachers[13]; years of experience; and

[11] According to national statistics, about 50 percent of school resources go to teacher salaries (National Agency for Education 2016).

[12] The teacher categories included are "subject teachers" that teach grades 7–9 and teachers with grade 4–9 competence.

[13] Teachers are considered non-qualified if they lack a teacher degree of if they lack the adequate teacher degree for the specific subjects they teach.

age. Besides that skills typically improve with experience, there are reasons to expect that experience and age are positively related to teacher quality in Sweden due to cohort differences in the quality of teacher education and in the selection into teacher education. It is, for example, well documented that there has been a negative trend over time in the abilities of new teachers who graduate from teacher education.[14]

One drawback with the teacher register is that it is not possible to link teachers to schools before the school year 1995/1996, implying that we have teacher variables only for pupils who finish the 9th grade in 1996 and onwards (we lack teacher data for two cohorts of pupils compared with all other analyses in the paper).

Identifying treatment and treatment assignment. In order to identify treated neighbourhoods, we use the school register, and retain information on grade 1–6 schools that expand to successively include grades 7, 8 and 9. All neighbourhoods where the default school is an expanding school are considered treated. The default school is not directly observed in the data, but is approximated by using the most common school attended by students in a given neighbourhood. The treatment (i.e. school expansion) is assigned to the pupil based on his/her residential neighbourhood at the age of 7, 6 years before treatment, which should ensure that treatment had not been announced and that there is no sorting to schools based on the future school grade expansion.[15] The procedure to identify treated areas and to assign treatment status to students is described in more detail in Appendix A where we also provide descriptive statistics of the approximation of default schools.

We are able to identify 226 expanding feeder schools in the school register in the period 1994–2002. 191 of these can be mapped to neighbourhoods.[16] We further require that each treated area should have at least one untreated neighbourhood within the same municipality, and that the default schools had at least 10 assigned pupils in 1994. We restrict the treatment period to 1994–2002 because we would like to exploit school grade expansions that are related to the 1994 curriculum, that is, we use only expansions from 1994 and onwards. The last treatment year, 2002, is a somewhat arbitrary restriction, but follows from Figure 2 which shows that the increase in the number of 1–9 schools levels off around 2002.

[14] Grönqvist and Vlachos (2016) document the evolution of cognitive ability, non-cognitive social interactive ability, and teacher GPA among new subject teachers in the middle school system 1980–2006. They show that there has been a marked decline in all ability measures, most pronounced in cognitive ability which has declined by close to 20 percentile ranks between 1990 and 2006.

[15] We cannot observe the pupil's residential neighbourhood before age 16, whereby we use the mother's residential neighbourhood at the time the pupil is aged 7 as an indicator of pupils' place of residence.

[16] The remaining 35 schools cannot be mapped to neighbourhoods because they are too small to be the default school of any neighbourhood.

The estimation sample includes the treated neighbourhoods, as well as untreated neighbourhoods within the same municipalities. As an additional restriction, we exclude untreated pupils assigned to receiving schools where treated pupils would have gone, had they not been exposed to treatment. These pupils are to some extent also affected (with a different treatment), since their schools do no longer accommodate pupils from the expanding feeder schools. We also exclude the first treated cohort from the analysis, which will be discussed in greater detail in Section 5.1. After all the restrictions described above, the final sample includes 108 school expansions mapped to 484 neighbourhoods (that is, the treated neighbourhoods), and 2,793 neighbourhoods (treated and untreated) in total across 65 out of Sweden's about 290 municipalities. Appendix Figure A1 shows a map of Sweden where each dot represents an expanding school. As is evident from the figure, the expanding schools are distributed across the country and concentrated to areas with high population density.

Table 1 displays the number of pupils and neighbourhoods by treatment year in our sample. These numbers vary a little year-to-year, but it is clear from the table that school expansions were affecting a substantial number of neighbourhoods and pupils throughout the whole period.

Table 1. Number of pupils and neighbourhoods by treatment year

	Pupils			Neighbourhoods		
Treatment year	**Freq.**	**Percent**	**Cum.**	**Freq.**	**Percent**	**Cum.**
1994	2,507	0.98	0.98	33	1.18	1.18
1995	4,747	1.85	2.83	28	1.00	2.18
1996	4,602	1.79	4.62	63	2.26	4.44
1997	2,400	0.94	5.56	37	1.32	5.76
1998	5,557	2.17	7.73	68	2.43	8.20
1999	5,239	2.04	9.77	47	1.68	9.88
2000	7,150	2.79	12.56	73	2.61	12.50
2001	5,776	2.25	14.81	61	2.18	14.68
2002	7,675	2.99	17.80	74	2.65	17.33
Not treated	210,798	82.20	100	2,309	82.67	100
Total	256,451	100		2,793	100	

Appendix Table A2 presents summary statistics of our treatment and control groups, as well as for the population as a whole. In terms of variables related to school organisation, Panel A of the table shows that pupils in the treatment group on average have fewer peers (lower cohort size) and are less likely to make school transitions. Moreover, the treatment group has a slightly higher teacher-per-pupil ratio, but it has on average a lower fraction of qualified teachers who also tend to be somewhat younger and less experienced. Panel B of the table shows very small average differences between treatment and control groups in terms of parental education and income.

5 Empirical analysis

Treatment is defined as exposure to a school grade expansion in the neighbourhood, that is, the default school expanded from grades 1–6 to grades 1–9. Our empirical analysis adopts a difference-in-differences estimation, exploiting cohort variation in treatment status among pupils in treated neighbourhoods. Treated neighbourhoods are compared to other neighbourhoods that are treated at a different point in time, or never treated. We include cohort*municipality effects to allow for cohort effects that vary by municipality, which has the virtue of controlling for locally implemented school policies that might vary across different municipalities. This is particularly important since the Swedish school system was decentralized during the period that we study, which means that local policies came to play a larger role in the education system. Cohort*municipality effects also account for year-to-year variation in the composition of the student body in the municipality.

Before turning to the regression analysis, Figure 3 illustrates the variation in the data by showing means in the treatment group, compared to means in untreated neighbourhoods within municipality, before and after the school grade expansion (which is depicted as year 0). Figure 3a shows the first stage: in treated areas, the probability to attend a school that requires a transition is reduced from about 0.4 to 0.1, while there is no drop in the comparison group. The comparison group here consists of a mix of neighbourhoods where students either transition to a 7–9 school or remain in a 1–9 school, but where there is no change in grade configuration at any point in time.

As explained in Section 4, the fraction of pre-reform pupils that makes a transition is underestimated, and as such the first stage is also likely to be underestimated. We will return to this issue further on in Section 5.1. The key message from Figure 3a is nevertheless that there is a large drop in the probability to transition in the treatment year, and the probabilities do not seem to trend for either treatment or control group.

Next, we turn to Figure 3b, which shows that the probability of attending the assigned school is about 0.8 and decreasing over time in the control group, and slightly below in the treatment group. The negative trend is not surprising, since the time period we study coincides with increased school choice opportunities. The figure also shows a small negative jump among treated students in the treatment year, indicating that pupils exposed to treatment are less likely to attend their assigned schools. We will estimate this effect in the difference-in-differences framework in Section 5.1 and discuss its implications. Figure 3c presents the expected drop in cohort size in treated neighbourhoods and shows that it is substantial (from 117 down to 77 pupils per cohort), and that in control areas cohort size remains constant. Finally, Figure 3d depicts predicted GPA percentiles in the treatment and control areas. Predicted GPA is calculated using the fitted values from a regression model of GPA on parental background characteristics (see Section 4) and is used as an index of family background. It is evident that treatment and control areas are reasonably close in terms of levels of predicted GPA, but that the treatment group (which is smaller) fluctuates more on a year-to-year basis. It is thus possible that compositional differences in the treatment and control group may confound our results. We will pay attention to this concern by conducting a test for whether treatment is associated with pre-determined background characteristics, and we include a detailed set of family background covariates in our regression model when studying outcomes of educational performance.

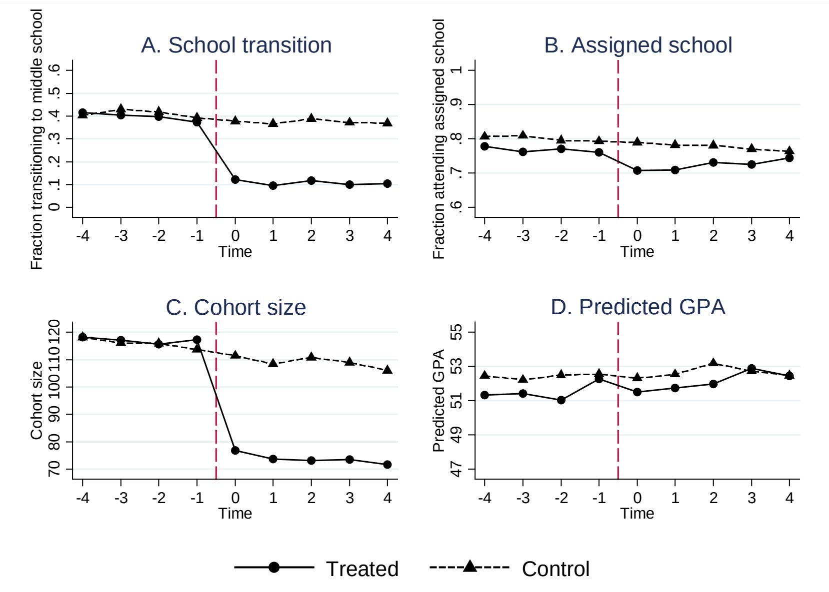

Figure 3. Means of treatment and control groups before and after school grade expansion

Note: The figure is constructed by grouping all treated neighbourhoods with the same treatment year within a municipality and comparing them with untreated neighbourhoods within the same municipality. We calculate means over treated and untreated groups within the same municipality, and then take the average over all treated and untreated groups over all municipalities and treatment years (not considering pupil weights).

5.1 Empirical model

As mentioned above, we have identified 108 treated schools, that are mapped to 484 treated neighbourhoods. Our estimation sample also includes non-treated neighbourhoods in the same municipalities. All in all, the sample covers 65 of Sweden’s approximately 290 municipalities. We estimate the following model:

$$y_{inmt} = \alpha_n + \beta Treated_{nmt} + X'_{inmt}\delta + \mu_{mt} + \varepsilon_{inmt} \quad (1)$$

where y_{inmt} is the outcome of pupil *i*, living in neighbourhood *n* in municipality *m*, belonging to cohort *t*. $Treated_{nmt}$ is a dummy variable taking the value 1 if cohort *t* in neighbourhood area *n* is treated (i.e., exposed to a school grade expansion), zero otherwise. The model includes neighbourhood-fixed effects (α_n), cohort*municipality effects (μ_{mt}) and individual background covariates X. The parameter of interest, β, is the difference-in-differences (DID) estimate of exposure to treatment, and should be interpreted as an intent-to-treat (ITT) estimate of the school grade expansion. Standard errors are clustered at the municipality level, allowing for the error terms within municipalities to be correlated.[17]

The empirical strategy relies on the parallel trends assumption, that is, the untreated neighbourhoods should adequately represent the trend in the treated neighbourhoods in the absence of treatment. In all our specifications, we add pre-treatment placebo dummies to test for parallel trends in the period before the intervention was introduced.

We believe that it is the effect of attending a grade 1–9 school, and thus avoiding a transition to a middle school, that is the key question of policy interest. Can the ITT estimates shed light on this issue, i.e., can we be sure that they have external validity and actually speak to the question we are interested in? There are three main concerns that the ITT estimates do not capture the effect of attending a grade 1–9 school. First, we exploit schools that expand and for the first time teach pupils in grade 7, 8 and 9. The first treated 7th grade cohort will thus be exposed to a new organisation that may still be in its establishment phase, and will not have older peers in school which would be the case in an already existing school. Can these schools really tell us something about attending a grade 1–9 school in general? This is a concern that we cannot fully rule out, but we can reduce the likelihood that this is a problem by *excluding the first treated cohort* from the analysis. We thus let the second and later treated cohorts identify the effect, which means that we give the school at least one year to establish its new grade configuration and all treated pupils will have gone through grades 7 and 8 with older peers in the school. It is reasonable to believe that the expanding schools can be compared to already existing grade 1–9 schools of similar size after one year. The estimates thus speak to the question of the effects of attending a small local school throughout grades 1–9, as compared to transitioning to a larger middle school.[18] In a sensitivity analysis in Section

[17] An alternative to clustering at the municipality level is to cluster at the neighbourhood level. The former is the most conservative approach, although in most cases there are very small differences between the two.

[18] All estimates presented in the paper come from a sample where the first treated cohort is excluded. The first treated cohort is however included in the sample used to construct Figure 2. Estimates where the first treated cohort is included are very similar to those presented in the paper and can be obtained from the authors upon request.

5.5 we will also address whether effects differ by time since the start of the expansion, which sheds light on whether effects are to be considered transitory or permanent. It is also worth emphasising that the previous literature suggests that the mechanisms by which grade configuration matters may relate to cohort size and early transitions; in our case these two elements of treatment are immediate and if they have an effect on pupils' outcomes we should expect to detect them directly after a school expansion.

The second concern relates to mobility as a response to the intervention. We match treatment to pupils based on their residential neighbourhood 6 years before they start 7th grade, which means that treatment status should not capture mobility or sorting that come as a result of the school expansion. We see families' decisions to opt out of the assigned school or move to a different neighbourhood as a potential effect of the intervention, which might have implications for the interpretation of the results on pupil performance. If pupils opt out as a result of the new school organisation, we might capture the effect of opting out rather than the effect of remaining in the expanding school. We will return to this issue when we discuss the internal and external validity of our findings in Section 5.5.

The third and final concern that the ITT dos not capture the policy parameter of interest is that grade 1–9 schools might have been more successful at implementing the new curriculum compared to schools with other grade configurations. If this is the case we might capture an effect of the use of the curriculum rather than the effect of grade configuration itself. We believe that this is likely to be a minor issue and that it would not influence pupils' attainment, but cannot rule out such a mechanism. For this reason, we focus on the ITT estimates rather than adopting an IV strategy: we cannot rule out that the school expansions affect outcomes through other channels than grade configuration. However, when discussing our results we find it informative to scale our ITT estimates by the first stage (i.e. the effect on the probability of avoiding a school transition) in order to get a better understanding of the magnitudes of the estimated effects in terms of the treatment on the treated (TOT).

In the following sections we turn to the results. We begin by studying the effects on various organisational aspects of schools in Section 5.2, before moving to the effects on pupils' outcomes in Section 5.3.

5.2 Effects on schools' organisation and pupil sorting

We start out by investigating whether there is a first stage of exposure to treatment, that is, are pupils induced to stay on in their local school and to avoid a transition to a middle school? In column 1, Table 2, we present the effect on the probability of making a mandatory school transition as a result of the school grade expansion, estimated using the DID specification described above. We find that the probability of a mandatory transition is reduced by 32 percentage points. Evaluated at the pre-intervention mean in the treatment group (0.42), this corresponds to a drop by 76 percent. Unfortunately, our definition of school transitions (attending a 7–9 school) implies that we underestimate the fraction of students that avoid a transition. The “first stage” coefficient in column 1 will result in very large TOT:s if used to scale the ITT parameters. In order to get a more realistic first stage we use the estimates in Table 2 and make a couple of assumptions. We are able to approximate the first stage assuming that the first stage is equally strong for the students that make a transition into a middle school section of a 1–9 school, as it is for those who transition to a 7–9 school. In the treated neighbourhoods, 75 percent of students attended their assigned school in the pre-treatment period (see column 4, Table 2). This fraction is reduced somewhat as a result of the school grade expansion, to 72 percent. If we assume that the first stage is a 76 percent drop in the probability of making a transition and that 72 percent of all students comply, we can back out the first stage: 0.76*0.72=0.55. This implies that the school expansion reduced the probability of making a mandatory transition by 55 percentage points.[19]

We use this approximated first stage to scale the estimates in the paper, whenever applicable. But the reader may also choose to scale the estimates differently – one possible approach is to bound the estimates by assuming a low first stage (using 0.32 in column 1) and a high first stage (all pupils in the treatment group who attend their assigned schools in the pre-intervention period are induced to avoid a transition, 0.75).

Table 2 also presents pre-reform placebo estimates dated one and two years before the first treatment year. There is no indication of any pre-reform trends in the probabilities to make mandatory school transitions, and it is evident that the intervention has a substantial impact on the type of schools pupils attend. We therefore continue to investigate whether this has implications for the average cohort size in the school attended and whether pupils, as a result of the school reorganisation, are more likely to attend a school closer to home or opt out of their assigned school. Column 2 shows the effect on cohort size, i.e. the size of the age-specific peer group in school. The point estimate of 33 pupils implies that pupils who are induced to remain in an expanding school were faced with a reduction of their age-specific peer group by 60 pupils (33/0.55), which approximately corresponds to two classes. Moving on to column 3, we find that exposure to treatment leads to a large increase (of 21 percentage

[19] The first stage from a regression on a restricted sample including only neighbourhoods where the pre-reform default school was a 7–9 school is 0.59. As such, our approximation of 0.55 is close.

points) in the probability of attending a school close to home, defined as attending a school in the same residential neighbourhood. In columns 4 and 5 we focus on the probability of opting out, and we find that exposure to a change in a school's grade configuration reduced the probability of attending the assigned school by 3.2 percentage points (over a mean of 0.75), but did not affect the likelihood of attending a private school.[20]

So far, we have established that the reorganisation of Swedish middle school education had some clear implications for pupils: treated pupils were induced to avoid a school transition between grades 6 and 7, and they attend schools with significantly smaller cohort sizes that are located closer to their homes. In the remaining part of this section, we will address whether these changes also were accompanied by changes in the learning environment, defined as teacher resources and peer group composition.

Table 2. Effects of school grade expansion on type of school

	(1) School transition	(2) Cohort size	(3) School in home nbhd	(4) Attends assigned school	(5) Attends indep. school
Grade expansion	-0.319**	-33.027**	0.211**	-0.032*	-0.004
	(0.048)	(4.693)	(0.034)	(0.015)	(0.003)
Grade expansion t-1	-0.030	1.774	-0.005	-0.006	-0.002
	(0.043)	(3.092)	(0.012)	(0.014)	(0.005)
Grade expansion t-2	-0.012	-0.268	-0.004	-0.007	-0.002
	(0.038)	(2.191)	(0.012)	(0.011)	(0.002)
Observations	256,451	256,451	256,451	256,451	256,451
R-squared	0.550	0.556	0.665	0.140	0.117
Neighbourhood f.e.	X	X	X	X	X
Municipality*cohort f.e.	X	X	X	X	X
Outcome mean	0.377	118.0	0.177	0.750	0.0220
Pre-treat outcome mean	0.421	122.6	0.156	0.749	0.0119

Notes: Outcome variables are defined as follows. School transition: pupil attends a grade 7-9 school, restricted sample is a sample where pupils are expected to make such a transition; cohort size: nr of pupils per cohort and school; school in home neighbourhood: pupil attends school located in his/her residential neighbourhood; attends assigned school: pupil attends expected school based on residential location; pupil attends independent school: pupil attends non-public school. Standard errors in parentheses are clustered at the municipality level. ** p<0.01, * p<0.05, + p<0.1

We begin by studying whether there are any differences in the teacher resources that pupils face at the middle school level. The estimate in column 1, Table 3, shows that the teacher-pupil ratio is unaffected. Thus, pupils who avoid a transition to middle school seem to face similar quantitative teacher recourses during the last three years in school as pupils who transition to a middle school. However, we find evidence that exposure to treatment has negative effects in the teacher quality dimension. The estimates in columns 2–4 reveal that pupils who are induced to remain in an expanding school were faced with a reduction in the

[20] We cannot rule out that the drop in the probability of attending the assigned school is the result of measurement error. The opening of a new school might increase the imprecision in the mapping between schools and neighbourhoods described in Section 4.

fraction of qualified middle school teachers by 12 percentage points (0.065/0.55), and that their middle school teachers have on average 1.9 years less experience (1.07/0.55) and are 5.4 (3.0/0.55) years younger. These results might be explained by the facts that expanding schools need to hire new teachers and that newly educated young teachers presumably are overrepresented among those searching for jobs at any point in time. Similarly, if there is a shortage of qualified teachers on the job market, expanding schools are more likely to hire unqualified teachers. The effects on teacher quality could therefore be a result of the initial expansion phase, and effects might vanish in the long run. But expanding schools are also smaller and might not have enough pupils to offer full-time teaching positions in some subjects, whereby teachers will have to teach in subjects where they lack qualifications. In Table A4 in the appendix, we present yearly DID estimates which show that after 4 years, pupils in treated neighbourhoods are still exposed to less experienced teachers, but the negative effect on the fraction qualified fades out over time. We discuss this result in further detail in Section 5.5.

Table 3. Effects of school grade expansion on average teacher resources and characteristics

	(1) **Teachers per pupil**	(2) **Fraction qualified**	(3) **Teacher experience**	(4) **Teacher age**
Grade expansion	0.001	-0.065**	-1.071**	-2.952**
	(0.003)	(0.020)	(0.162)	(0.650)
Grade expansion t-1	0.004	-0.004	-0.049	0.544
	(0.003)	(0.012)	(0.097)	(0.390)
Grade expansion t-2	0.004*	-0.006	-0.108	0.098
	(0.002)	(0.010)	(0.090)	(0.371)
Observations	185,120	185,120	185,120	185,120
R-squared	0.302	0.591	0.654	0.529
Neighbourhood f.e.	X	X	X	X
Municipality*time f.e.	X	X	X	X
Outcome mean	0.429	0.674	4.892	45.59

Notes: Outcome variables are defined as follows. Teachers per pupil: the number of teachers who primarily teach in grade 4–9 divided by the number of pupils in grade 4–9 in school; qualified: the fraction of teachers who primarily teach in grade 4–9 that hold formal qualifications to teach at this level; experience: mean years of experience in the teaching profession among the teachers in school who primarily teach in grade 4–9; age: mean age among the teachers in school who primarily teach in grade 4–9. Robust standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1

Next, attending a small school catering to a limited geographic area is likely to imply a more homogenous peer group, as opposed to attending a large middle school which serves a several residential neighbourhoods. To shed light on this possible mechanism, we assess the effects on peer group composition in several different ways. Columns 1 and 2 in Table 4 show the coefficient of variation in mother's and father's years of schooling in the school and cohort that pupils attend. Avoiding the transition to a middle school has a negative effect on the variation in peers' educational background: the peer group becomes more homogenous in terms of parental education. The estimate on the variation in father's years of schooling corresponds to 0.18 of a standard deviation.

To gain further insight into the effects on peer group composition, we study differences by student background in exposure to peers from low educated families. In columns 3 and 4 in Table 4, we split the sample by parental education and estimate the effect on exposure to peers with low educated parents.[21] Exposure measures the fraction of the peer group (excluding the pupil him/herself) with low educated parents. We hypothesise that pupils with low educated parents should become more exposed to peers with low educated parents, and pupils with high educated parents should become less exposed to peers with low educated parents when induced to stay in a local school. We find a far from significant estimate which indicates that for pupils with low educated parents, exposure to peers with similar backgrounds increases with 0.6 percentage points (over a mean of 0.52). For pupils with highly educated parents there is no indication of a change in the peer group. Our conclusion is therefore that the expansion of local schools potentially implied a more homogenous peer group for the average pupil, but that the evidence in favour of this hypothesis is relatively weak.

Table 4. Effects of school grade expansion on pupil sorting and peer group exposure

	(1) **Coef. of variation Mother's years of schooling.** All pupils	**(2)** **Coef. of variation Father's years of schooling** All pupils	**(3)** **Exposure to low educated peers** Pupils with low educated parents	**(4)** **Exposure to low educated peers** Pupils with high educated parents
Grade expansion	-0.003+	-0.005**	0.006	0.000
	(0.002)	(0.002)	(0.007)	(0.007)
Grade expansion t-1	-0.000	0.001	-0.001	-0.001
	(0.001)	(0.002)	(0.006)	(0.005)
Grade expansion t-2	-0.002	-0.001	-0.002	0.001
	(0.002)	(0.002)	(0.005)	(0.007)
Observations	256,451	256,450	134,442	122,009
R-squared	0.532	0.556	0.602	0.663
Neighbourhood f.e.	X	X	X	X
Municipality*cohort f.e.	X	X	X	X
Outcome mean	0.186	0.210	0.517	0.440

Notes: Outcome variables are defined as follows. Coefficient of variation of mother's years of schooling and father's earnings: coefficients of variation calculated at the school cohort level. Exposure to low educated peers: the fraction of school-cohort peers whose parents have no tertiary education; pupil rank: pupil's within school-cohort percentile rank in predicted GPA. Standard errors in parentheses are clustered at the municipality level. ** p<0.01, * p<0.05, + p<0.1

[21] High (low) educated neighbourhoods are defined as the neighbourhood being below (above) the pupil-weighted median in the fraction of pupils with low educated parents.

To sum up the findings of this section, we conclude that the expansion of feeder schools that induced pupils to attend small local schools throughout nine years of compulsory education has affected pupils in several important ways. Pupils do not have to make a transition between grade 6 and 7; pupils attend schools with a smaller and more homogenous peer group closer to home; pupils attend schools with teachers who tend to be younger, less experienced and with lower qualifications on average. From the previous literature (mainly from the U.S.) we have learned that attending small schools and avoiding transitions to middle schools have beneficial effects on pupil's school results. For this reason, we should expect positive effects on educational outcomes. In contrast, our results for teacher quality and experience would have an expected negative impact on pupil performance in expanding feeder schools. In a nutshell, the above summary emphasises one of the difficulties with studying the effects of how schools are organised: there are many different mechanisms at stake, which may predict effects that work in different directions and offset each other. On the one hand, it might be difficult to generalise from studies on school organisation in one school system to another, because the content of the "black box" of organisational change can be very different and we cannot always pin down the exact mechanisms. On the other, comparing studies from different contexts and countries may provide useful in pinning down some general mechanisms, if there are common elements in the school systems that are compared. Next, in Section 5.3, we turn to the effects on educational outcomes.

5.3 Effects on educational outcomes

Before discussing the results on educational outcomes, we test whether our empirical approach is robust to compositional differences between treated and control areas. If demographic trends in some areas correlate with the intervention, we would be concerned that any effects could be driven by such compositional effects. Table 5 displays the DID estimates of grade expansion on pupils' pre-determined background characteristics, in terms of their predicted GPA, mother's years of schooling, and whether the pupil has immigrant background or not. The estimates are small and not statistically different from zero, which suggests that the empirical design is able to control for differences in compositional trends between treated and control areas. In column 4, we also test whether treatment is correlated with population growth in the neighbourhood, and we do not find different trends neither pre- nor post school reorganisation across treated and control areas.

Table 5. Estimates of school grade expansion on pre-determined characteristics and neighbourhood population growth

	(1) Predicted GPA	(2) Mother's years of schooling	(3) Immigrant background	(4) Nbhd population
Grade expansion	-0.342	0.018	-0.002	1.211
	(0.338)	(0.046)	(0.010)	(1.362)
Grade expansion t-1	-0.045	-0.003	-0.011+	1.558
	(0.227)	(0.045)	(0.006)	(2.110)
Grade expansion t-2	-0.370	-0.012	-0.003	-0.094
	(0.242)	(0.036)	(0.005)	(0.829)
Observations	256,451	256,451	256,451	256,451
R-squared	0.213	0.159	0.268	0.967
Neighbourhood f.e.	X	X	X	X
Municipality*cohort f.e.	X	X	X	X
Outcome mean	53.39	11.95	0.0897	33.97

Notes: Predicted GPA is the prediction of pupil's GPA using the following variables: gender, mother's and father's years of schooling; mother and father holding "elite degree" (law, medicine, engineering, economics), mother's and father's log earnings (averaged across child's age 7–16); foreign born or both parents born abroad. Standard errors in parentheses are clustered at the municipality level. ** p<0.01, * p<0.05, + p<0.1

We now turn our focus to the effects on educational outcomes. Table 6 presents our baseline results. The table shows effects for five different outcomes: percentile ranked GPA, grade inflation, any high school degree, academic high school degree, and college attendance. For each outcome, we present two specifications, excluding and including parental background controls. We begin with column 1, which shows an insignificant estimate of grade 1–9 expansion of 0.14 percentile points. When adding background controls in column 2, the estimate increases but is still close to zero and statistically insignificant. The placebo test for parallel trends in the pre-reform period likewise shows small and non-significant estimates, which thus adds support to the underlying identifying assumption in the DID model.[22] Our preferred estimates are those including parental background covariates, and taking the estimate in column 2 at face value, we can bound the ITT effect in the range (-0.8 – 1.7) percentile points. We can rule out a non-zero ITT larger than 6 percent of a standard deviation.[23] In Table A3 we also present the estimates for English and maths, subjects in which pupils sit centralised tests, and the results for these subjects are very similar to those for the overall GPA.

As discussed in the data section, a limitation when studying short-run effects is that we only have data on teacher assessed outcomes (i.e. the GPA). What if some school types (e.g. small schools, or new schools, or schools with a certain type of teachers) are more or less

[22] When studying educational outcomes, we exclude the last untreated cohort (as well as the first treated cohort) from the treated neighbourhoods. The reason is that the last untreated cohort is exposed to a different kind of treatment, attending a school where there are no younger cohorts from the same neighbourhood making the transition to the school. If relative age is important, the last untreated cohort is also affected by the reorganisation, and is therefore dropped from the analysis sample.

[23] The standard deviation in percentile ranked GPA is 28, see Appendix Table A2.

lenient in their adaptation of the grade scale? We cannot fully rule this out and it is therefore possible that the GPA outcome captures “grading cultures” rather than actual school performance. We can however provide a suggestive test for grading leniency using a measure of grade inflation, which is the difference in percentile ranked grades between soft subjects and centrally tested subjects (see Section 4 for more details). Our assumption is that the adaptation of the grade scale will be more uniform across schools in subjects that have central tests, and less so in the case of non-academic subjects. In columns 3 and 4 we test whether school expansion is related to this measure of grade inflation. Again, we find small and statistically insignificant estimates which suggest that pupils exposed to treatment are neither more nor less likely to attend schools with a different degree of leniency, compared to the comparison group. This is indicative that the effects on GPA are likely to capture effects on pupils’ school performance, but not enough to draw conclusions. We therefore turn to medium- and long-term outcomes to assess the impact on pupils’ progression through the education system. Any large effects on performance would likely have an impact on the degree of high school completion or on the choice between academic or vocational high school programmes.

Columns 5–6 in Table 6 present the results for high school completion. We find small and precisely estimated effects close to zero: we can rule out ITT effects outside the range (-0.006 – 0.018), over a mean of 0.92. The insignificant effects on academic high school in columns 7–8 are somewhat less precise, and can be bounded to (-0.023 – 0.025), over a mean of 0.51. Finally, effects on college attendance follow the same pattern: we find no effect of exposure to treatment on this outcome.

The key insights from our findings so far is that despite some quite clear organisational changes that affected pupils’ schooling in several ways, there is no evidence that this has affected neither their short-run school performance nor their achievement at higher stages in the education system. The results are consistent across outcomes and specifications, in most cases precisely estimated around zero, and the tests for parallel pre-reform trends are robust. We thus conclude that on average, there was no difference in terms of education production of attending a small local grade 1–9 school as opposed to making a mandatory transition to a large middle school in Sweden in the 1990s.

Table 6. Effects of school grade expansion on pupil outcomes

	(1) GPA	(2) GPA	(3) Grade inflation	(4) Grade inflation	(5) High school graduation	(6) High school graduation	(7) Academic high school	(8) Academic high school	(9) College attendance	(10) College attendance
Grade expansion	0.142	0.446	0.414	0.468	0.004	0.006	-0.002	0.001	-0.005	-0.003
	(0.718)	(0.631)	(0.520)	(0.555)	(0.006)	(0.006)	(0.012)	(0.012)	(0.010)	(0.009)
Grade expansion t-2	0.323	0.644	0.174	0.153	0.009	0.009	0.003	0.006	-0.003	-0.000
	(0.504)	(0.524)	(0.536)	(0.552)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.009)
Grade expansion t-3	0.596	0.435	0.030	0.074	0.006	0.005	-0.001	-0.003	0.007	0.006
	(0.471)	(0.460)	(0.506)	(0.484)	(0.005)	(0.005)	(0.012)	(0.010)	(0.009)	(0.008)
Observations	251,085	251,085	251,085	251,085	251,085	251,085	251,085	251,085	251,085	251,085
R-squared	0.089	0.260	0.037	0.081	0.035	0.056	0.085	0.176	0.037	0.077
Neighbourhood f.e.	X	X	X	X	X	X	X	X	X	X
Municipality*cohort f.e.	X	X	X	X	X	X	X	X	X	X
Family background		X		X		X		X		X
Outcome mean	53.39	53.39	-0.517	-0.517	0.921	0.921	0.505	0.505	0.274	0.274

Notes: Outcome variables are defined as follows. GPA: percentile rank of pupil's GPA over all subjects in 9th grade; grade inflation: the difference between percentile ranked grades in music, arts, handicraft, home economics and physical education, and grades in mathematics and English; high school graduation: pupil is registered in high school at any point in time in the third and final year; academic high school: pupil is registered in the third and final year at an academic high school track at any point in time; college attendance: pupil has completed college coursework corresponding to at least one full semester of college. Family background and other additional control variables include: gender, number of siblings, mother's and father's years of schooling; mother and father holding "elite degree" (law, medicine, engineering, economics), mother's and father's log earnings (averaged across child's age 7-16); foreign born or both parents born abroad. Standard errors in parentheses are clustered at the municipality level.

** p<0.01, * p<0.05, + p<0.1

5.4 Heterogeneous effects

Some of the studies in the previous literature have found that the effects of attending small schools or middle schools are more pronounced for low-achieving and disadvantaged pupils. It is possible that our on average zero effects mask heterogeneity in response to treatment, and we therefore move on to separate analyses by subgroups of pupils. In Table 7 we present results by gender, by high/low parental education, for pupils with immigrant/Swedish background, and by urban or rural areas. We find no evidence of heterogeneous effects: the effects are generally close to zero and estimated with precision similar to what we found for the main results in Table 6. There are some exceptions to this pattern, however: we find that pupils with highly educated parents have a higher probability of completing high school. But we also find a few significant pre-reform placebo estimates in the table and therefore remain reluctant to pushing this result without further evidence. Given the number of estimates presented in Table 7, it is not unlikely that we in a few cases falsely reject the null. All in all, the margins where we expect to find effects, namely GPA and choice of high school track, are unaffected for all subgroups.

5.5 Threats to internal and external validity

Our results indicate that in terms of educational outcomes, pupils are unaffected by exposure to an expanding grade 1–9 school. In this section we discuss two potential threats to identification that are critical for the interpretation of this finding.

First, Table 2 showed that the expansion of feeder schools induced some pupils to opt out of their assigned schools, that is, to attend a different school than the expanding feeder school. If these pupils opted out in order to attend particularly good or bad schools, this might counterbalance the effect for the compliers attending the expanding feeder school. We cannot rule out this possibility, but conclude that the fraction of pupils attending the assigned school decreased by a modest 3.2 percentage, and the effects of opting out must be very large in order to net out effects resulting from avoiding a school transition.

Last, we ask ourselves whether we can generalise our findings to grade 1–9 schools in general. Even if we drop the first treated cohort from the analysis, it is possible that the adjustment to a new school organisation is a process that takes several years. In Appendix Table A4 and Table A5 we therefore present DID estimates separately for the second, third and fourth treated cohorts. Table A4 shows that the effects on outcomes that capture the organisation of the school (i.e., probability of school transition, cohort size, proximity to school and the probability of opting out) are stable over time. However, we might expect that adjustments to the allocation of teacher resources after a reorganisation take several years.

Column 7 shows that the negative effects on the fraction of qualified teachers fade out over time. One interpretation of this finding is that with time, schools adjust to the new organisation and in the long run we should not expect any differences in resources between school types. Columns 8 and 9 show that the effects on teacher age and experience are persistent over time, although reduced by about 20–30 percent. An alternative interpretation is therefore that even after four years, the small 1–9 schools are not able to staff all classes with teachers equally experienced to those in comparison schools. We lack conclusive evidence here, but conclude that it is not unlikely that smaller schools have difficulties in optimising the allocation of teacher resources and that adjustments to shocks might take longer for small schools.

In Table A5, we turn to the evidence on pupil outcomes. We find that the zero effects persist up to four years after the school reorganisation. This suggests that our findings should not merely be attributed to the initial reorganisation phase, and that the effects of exposure to a grade expansion are informative of the effects of attending small grade 1–9 schools in Sweden.

Table 7. Heterogeneous effects of school grade expansion on pupil outcomes

	(1) Boys	(2) Girls	(3) High parental edu	(4) Low parental edu	(5) Native background	(6) Immigrant background	(7) Rural	(8) Urban
A. GPA								
School grade expansion	-0.012	0.868	0.481	0.417	0.343	2.222	-0.045	1.324
	(0.877)	(0.736)	(0.873)	(0.969)	(0.561)	(1.830)	(0.762)	(0.980)
Grade expansion t-2	0.102	1.086	-0.172	1.468*	0.468	2.173	0.319	1.297+
	(0.808)	(0.792)	(0.925)	(0.699)	(0.533)	(2.227)	(0.698)	(0.666)
Grade expansion t-3	-0.268	0.855	0.147	0.869	0.418	0.799	0.197	0.860
	(0.634)	(0.674)	(0.782)	(0.779)	(0.488)	(2.371)	(0.583)	(0.732)
Outcome mean	47.92	59.11	63.81	43.93	53.92	48.02	51.86	56.26
B. Grade inflation								
School grade expansion	0.442	0.529	0.220	0.697	0.516	0.419	0.855	-0.251
	(0.772)	(0.703)	(0.768)	(0.715)	(0.563)	(1.235)	(0.598)	(1.038)
Grade expansion t-2	0.487	-0.145	0.455	-0.005	0.083	0.387	0.155	0.081
	(0.552)	(0.761)	(0.784)	(0.574)	(0.562)	(1.165)	(0.777)	(0.680)
Grade expansion t-3	-0.671	0.844	0.594	-0.329	0.281	-1.308	0.022	0.072
	(0.652)	(0.598)	(0.654)	(0.633)	(0.494)	(1.910)	(0.511)	(0.977)
Outcome mean	-2.478	1.529	-3.264	1.979	-0.628	0.608	-0.674	-0.224
C. High school graduation								
School grade expansion	0.010	0.002	0.015*	-0.003	0.005	0.005	0.004	0.008
	(0.008)	(0.008)	(0.007)	(0.008)	(0.006)	(0.025)	(0.007)	(0.009)
Grade expansion t-2	0.020+	-0.001	0.004	0.011	0.009	0.019	0.018+	-0.004
	(0.010)	(0.011)	(0.009)	(0.010)	(0.010)	(0.029)	(0.010)	(0.013)
Grade expansion t-3	0.003	0.007	-0.001	0.008	0.005	0.006	0.007	0.001
	(0.009)	(0.008)	(0.006)	(0.009)	(0.006)	(0.027)	(0.007)	(0.007)
Outcome mean	0.919	0.923	0.954	0.891	0.925	0.885	0.927	0.911

	(1) Boys	(2) Girls	(3) High parental edu	(4) Low parental edu	(5) Native background	(6) Immigrant background	(7) Rural	(8) Urban
				D. Academic high school				
School grade expansion	-0.003	0.002	-0.011	0.004	-0.004	0.021	-0.007	0.014
	(0.016)	(0.014)	(0.015)	(0.016)	(0.010)	(0.040)	(0.013)	(0.022)
Grade expansion t-2	-0.006	0.017	-0.002	0.009	0.004	0.031	-0.000	0.017
	(0.014)	(0.012)	(0.015)	(0.012)	(0.010)	(0.047)	(0.012)	(0.019)
Grade expansion t-3	-0.007	0.002	-0.006	-0.001	-0.003	-0.008	-0.000	-0.007
	(0.013)	(0.015)	(0.014)	(0.013)	(0.011)	(0.039)	(0.015)	(0.011)
Outcome mean	0.487	0.525	0.666	0.360	0.505	0.510	0.473	0.566
				E. College attendance				
School grade expansion	-0.018	0.013	0.005	-0.009	-0.003	-0.028	-0.011	0.014
	(0.012)	(0.015)	(0.012)	(0.014)	(0.009)	(0.031)	(0.010)	(0.015)
Grade expansion t-2	-0.020+	0.018	-0.006	0.002	-0.006	0.027	-0.017	0.030
	(0.011)	(0.013)	(0.014)	(0.012)	(0.010)	(0.034)	(0.012)	(0.018)
Grade expansion t-3	-0.005	0.015	-0.003	0.013	0.005	-0.018	0.001	0.016
	(0.012)	(0.010)	(0.018)	(0.011)	(0.009)	(0.030)	(0.012)	(0.012)
Outcome mean	0.254	0.295	0.369	0.188	0.276	0.258	0.262	0.298
First stage	-0.629	-0.588	-0.613	-0.608	-0.622	-0.506	-0.618	-0.581
Observations	128,221	122,864	119,547	131,538	228,543	22,542	161,175	89,910
Neighbourhood f.e.	X	X	X	X	X	X	X	X
Cohort*municipality f.e.	X	X	X	X	X	X	X	X
Parental background controls	X	X	X	X	X	X	X	X

Notes: See Table 6. Standard errors in parentheses are clustered at the municipality level.
** p<0.01, * p<0.05, + p<0.1

5.6 Understanding the mechanisms: Evidence from pupil surveys

In this section, we turn to survey evidence in order to shed light on the school environment in different types of schools. The previous literature has found substantial differences in school environment between small and large schools, and between 8th graders attending K–8 schools compared to middle schools (Abuduldadiroglu et al. 2013; Rockoff and Lockwood 2010). This survey evidence has been key in interpreting the results on pupil outcomes.

We use the 1987 birth cohort of the ETF (Evaluation through follow-up) survey.[24] The ETF questionnaire targets 9th grade pupils and we focus on questions that relate to the school learning environment and to the social climate at the school. Pupils rank their answers on a scale from 1–5 and we standardize the answers to have a mean of zero and standard deviation of one, where a higher value means that the pupil agrees more with the statement or that an event is occurring with higher frequency. Table 8 presents regression coefficients of the associations between survey responses and school types. The first row shows the unconditional mean difference between 9th graders attending grade 1–9 schools and 9th graders attending middle schools. We find no indication that schools spanning the full length of compulsory education are perceived better in any of the dimensions covered by the survey: there are no significant differences between school types when it comes to how pupils perceive their school work, the degree of stress, feeling of exclusion and bullying, and whether pupils like their school and their peers. The second row focuses on school size, and displays correlations between survey responses and the number of pupils (divided by 10) in grade 9 at the school. We see no indication that larger schools provide an environment that is perceived as worse when it comes to school work and relationships with peers and teachers. One out of seventeen coefficients turns out marginally significant: larger schools are associated with greater levels of stress. If we scale the coefficient by the reduction in cohort size found in our analysis of school grade expansions (60 pupils), we arrive at a relatively small difference of 0.054 standard deviations (0.009*6).

To sum up our findings from the pupil survey, we conclude that there are no apparent differences in terms of the learning environment between schools of different grade configuration or different size. This piece of evidence thus adds support to our finding that grade configuration does not seem to matter for middle school education in the particular context studied here.

[24] The ETF survey is based on a 10 percent stratified sample and is run by the Department of Education at the University of Gothenburg; see Härnqvist (2000) for a description of the data.

Table 8. Differences in school environment by grade configuration and school size in the ETF survey

	(1) Hard to keep up	(2) Give up easily	(3) Need more help	(4) Hard to concentrate	(5) Worried	(6) Stressed	(7) Feel excluded	(8) Feel alone	(9) Unfair treatment teachers	(10) Unfair treatment others	(11) Bullied teachers	(12) Bullied pupils	(13) Like class	(14) Like school	(15) Like pupils	(16) Like teachers	(17) Like school work
Grade 1–9 school	-0.043	0.007	0.008	-0.034	-0.005	0.009	-0.001	0.001	0.016	-0.006	0.003	0.009	-0.009	-0.061	-0.003	0.014	0.048
	(0.034)	(0.031)	(0.039)	(0.036)	(0.033)	(0.037)	(0.029)	(0.029)	(0.033)	(0.029)	(0.026)	(0.029)	(0.037)	(0.048)	(0.037)	(0.042)	(0.035)
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.001
Number of pupils in grade 9	0.005	-0.001	0.002	-0.005	0.004	0.009*	0.004	0.006	0.005	0.001	0.002	0.004	-0.003	0.000	-0.004	-0.001	-0.007
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)	(0.005)	(0.007)	(0.004)
R-squared	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001
Observations	6,316	6,318	6,319	6,326	6,298	6,325	6,319	6,314	6,322	6,319	6,319	6,321	6,331	6,321	6,321	6,311	6,316

Note: Each coefficient is from a separate regression. The coefficients in the first row show unconditional mean differences in survey responses of 9th grade pupils in grade 1–9 schools compared to other school grade configurations (i.e. middle schools). The coefficients in the second row show unconditional associations between survey responses and the [number of pupils/10] in grade 9 in the school. The outcomes are standardised to mean 0 and standard deviation 1, where a higher value indicates agreement with the statement or more frequent occurrence of an event. Standard errors in parenthesis clustered at school level.

** p<0.01, * p<0.05, + p<0.1

6 Concluding discussion

To summarize our findings, we have shown that pupils who were induced to stay in a local school covering grades 1–9, instead of making a transition to a middle school between grades 6 and 7, attended smaller and potentially less diverse schools closer to their homes. We also found that they were exposed to less qualified teachers. Turning to educational outcomes, we found no evidence that this treatment had any impact neither on GPA nor on longer-term outcomes such as high school graduation and college attendance, on average. The zero effects persist up to four years after the initial reorganisation of the school which suggests that the results should not be attributed solely to the initial start-up phase of the expanding school. Our findings are further corroborated by survey data that do not display any differences in the school environment between schools of different grade configurations.

Previous literature has shown substantial negative impacts of transitions to middle schools and large positive gains of attending small high schools, in particular for disadvantaged pupils. How can we reconcile our findings with this literature? We offer a number of potential explanations as to why our results are different.

First, while we should expect that a smaller school-cohort and avoiding a school transition are beneficial for educational outcomes, it is not clear what to make of the other changes that accompanied the organisational change. Changes in peer group composition implied more homogenous peer groups in terms of parental education, but we find no clear effect on peer quality. As such, sorting and potential consequences in terms of peer effects do not seem to offer a clear-cut explanation. Instead, we find that treated pupils are exposed to teachers who are younger, less experienced and less likely to be qualified. It is thus possible that any positive effects of attending a smaller local school throughout grade 1–9 are offset by negative effects due to lower teacher qualifications and experience. All of these features of the organisational change together highlight one of the difficulties when studying effects of school organisation – it is hard to get into the “black box” to understand which mechanisms are driving the results, and therefore it may be problematic to generalise findings to wider contexts.

A second explanation to our result is related to the specific context that we study. Contrasting evidence from the school system in a Scandinavian country to that of mainly urban populations in the U.S. means that we compare not only different institutional features and educational cultures, but also different demographic groups and societies with different levels of inequality. The populations studied in for example Rockoff and Lockwood (2010) and Abdulkadiroglu et al. (2013) are primarily Black or Hispanic with high levels of poverty, as indicated by eligibility for reduced price lunch. We instead study a population that resembles the pupil average in Sweden, distributed across both urban and rural areas. These differences across countries and samples are not unlikely to give rise to different school environments – and it is possible that school organisation plays a more important role in one system than in the other. It is also worth noting that evidence from other European countries does not indicate that large schools necessarily pose problems (Beuchert et al. 2016; De Haan et al. forthcoming), which further reinforces the argument that country differences may matter.

Finally, it is possible that the changes in school context resulting from the school grade expansion were not significantly large to affect pupils' outcomes. Abdulkadiroglu et al. (2013) estimate the effect of school cohort size on pupil performance, and conclude that small changes in the student body are unlikely to result in large effects. Taken at face value, their (upper bound) estimates imply that a reduction in cohort size of 30 pupils (similar to the reduction in our setting) leads to an increase in math and English test scores by 0.007 standard deviations, and an increased probability of high school graduation of 0.3 percentage points.[25] These are very small effects and we do not have the statistical power to detect effects of this size in our study. Differences in cohort size between middle schools and K–8 schools in the U.S. also appear larger than what we have found in this paper: Rockoff and Lockwood (2010) show that the cohort size is over 200 in middle schools and about 75 in K–8 schools in New York City. In addition, it is also clear from the surveys that pupils do not report any systematic differences by school type or school size which again lends support to the explanation that the differences between school types are not large enough to result in effects on educational outcomes.

Despite these challenges to understanding the mechanisms, which offer avenues for future research, our study contributes to the literature in several ways. In our particular case we do not find that school organisation matters for pupils' progression in the education system – but evidence from the previous literature indicates that school organisation should not be overlooked in the education production function and our study also confirms that potential inputs in the production of education were altered by the change in grade configuration.

A key insight from the paper is that the effects of school organisation may mask mechanisms which potentially have off-setting effects and therefore complicate the interpretation of the overall impact. As such, the results from one setting should not necessarily be generalised to a wider context. Finally, our results are also interesting in the light of recent

[25] We use the estimates in Abdulkadiroglu et al. (2013), Table 12, and scale them by 0.3 to get the effect of a 30-pupil increase in cohort size.

policy initiatives in Sweden which have re-introduced large middle schools as a means to limit segregation. The results of this paper suggest that such a policy will be neither successful nor harmful for educational outcomes.

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Appendix A

Default schools and treatment assignment

The empirical method of the paper is a difference-in-differences where we observe variation in treatment status over residential neighbourhoods and over cohorts. The residential neighbourhoods are so called "SAMS" areas and correspond to small geographic neighbourhoods.[26] This section describes the procedure that identifies treated schools and the mapping of treatment status to the students' neighbourhood. Furthermore, it provides descriptive statistics that assess the quality of treatment assignment. The procedure consists of three steps.

In the first step we use the school register, and retain information on grade 1–6 schools that expand to successively include grades 7, 8 and 9. We use geographic coordinates to identify schools in order to avoid that pupils who attend the same school premises might be divided into separate administrative units. The first treatment year is the year when grade 7 is observed at the school.

In the second step we map treatment status to neighbourhoods. We cannot directly observe the catchment area, i.e. the default school that pupils of a specific neighbourhood are expected to attend. However, we are able to make a good approximation of the school that pupils should attend, given their residential neighbourhood. For each neighbourhood unit, we identify the most common school attended by the pupils residing in the unit. We require that the neighbourhood has at least two students, and that one school dominates all others (that is, neighbourhoods where there are two or more schools with the same number of students are dropped). We then match treatment status to neighbourhoods using the typical school of each neighbourhood.

In the third and final step, we assign treatment status (i.e. school expansion) to the pupil based on his/her residential neighbourhood at the age of 7, 6 years before treatment, which should ensure that treatment had not been announced and that there is no sorting to schools or neighbourhoods based on the future school grade expansion.

The main disadvantage with this procedure is that SAMS areas might not perfectly coincide with catchment areas. In practice, this means that students within the same SAMS may have different default schools. This will introduce measurement error in treatment status. Below we present descriptive statistics that describe the degree of measurement error in the treatment variable. Columns 1 and 2 of Table A1 show the distribution of the fraction of

[26] A SAMS unit is a geographical neighbourhood, developed to correspond to "real" physical neighbourhoods. On average, a SAMS unit has 1000 inhabitants, and there are around 9,200 units in total.

pupils in a neighbourhood that attend the approximated default school (i.e. the most common school). This fraction can be equal to one under two conditions: i) there is only one default school in each SAMS; and ii) all students attend the default schools. Deviations from one are thus in part due to measurement error, in part due to some students opting out from the assigned school.

Columns 1 and 2 of Table A1 show that on average, a large majority of students attend the default school. At the 25th percentile of the distribution, as many as 78 and 73 percent of students attend the default school in the full population and in the estimation sample, respectively. At the median, we find that the corresponding numbers are 90 and 86 percent. These statistics suggest that treatment assignment is correctly assigned to a large majority of students.

Columns 3 and 4 display the number of different schools observed by neighbourhood. We see that typically, students residing within the same neighbourhood attend more than one school. From Columns 1 and 2 we can however draw the conclusion that although several schools are represented within the same SAMS, the majority of students attend the default school.

Table A1. Descriptive statistics of default schools and neighbourhoods

	Fraction of pupils in approximated default school		**Number of observed schools per neighbourhood**	
	(1)	(2)	(3)	(4)
Percentile	Full population	Estimation sample	Full population	Estimation sample
1%	0.43	0.39	1	1.06
5%	0.57	0.50	1.17	1.28
10%	0.65	0.59	1.28	1.44
25%	0.78	0.73	1.61	1.89
50%	0.90	0.86	2.33	2.79
75%	0.96	0.94	3.68	4.83
90%	0.98	0.97	5.84	9.53
95%	0.99	0.98	8.37	14.05
99%	1	0.99	17.21	20.37
Observations	736,573	256,451	736,573	256,451

Appendix B

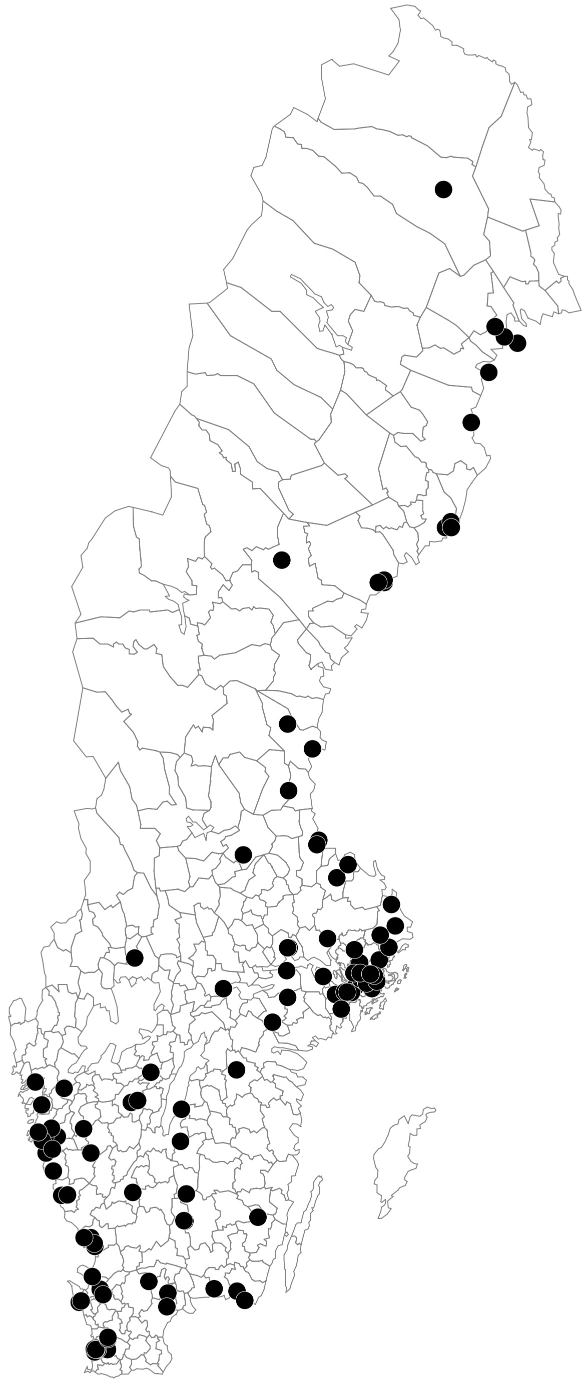

Figure A1. School grade expansions in Sweden 1994–2002

Note: Figure includes only expansions in the estimation sample.

Table A2. Summary statistics

	(1) All pupils	(2) Pupils in treated neighbourhoods	(3) Pupils in control neighbourhoods
	Mean (std.dev)	Mean (std.dev)	Mean (std.dev)
School organisation and outcomes			
School transition	0.355	0.293	0.395
	(0.479)	(0.455)	(0.489)
Cohort size	114.968	105.585	120.662
	(46.080)	(44.593)	(43.042)
School in home nbhd	0.175	0.207	0.171
	(0.380)	(0.405)	(0.377)
Attends assigned school	0.785	0.728	0.755
	(0.411)	(0.445)	(0.430)
Attends independent school	0.015	0.018	0.023
	(0.122)	(0.135)	(0.149)
Teachers per pupil	0.054	0.057	0.054
	(0.018)	(0.021)	(0.018)
Fraction qualified teachers	0.687	0.657	0.678
	(0.165)	(0.187)	(0.178)
Teacher experience	4.930	4.569	4.959
	(1.279)	(1.354)	(1.261)
Teacher age	45.999	44.834	45.742
	(4.092)	(5.090)	(4.079)
Coef. of var mother’s schooling	0.185	0.186	0.187
	(0.023)	(0.026)	(0.024)
Coef. of var father’s schooling	0.211	0.210	0.210
	(0.026)	(0.027)	(0.025)
Exposure peers with low edu parents	0.524	0.503	0.476
	(0.139)	(0.147)	(0.140)
Pupil rank within school	53.128	53.620	53.853
	(28.592)	(28.397)	(28.393)
GPA (percentile ranked)	52.001	52.760	53.514
	(28.255)	(28.172)	(28.256)
English grade (percentile ranked)	51.012	51.780	52.498
	(26.862)	(26.751)	(26.763)
Math grade (percentile ranked)	51.345	51.942	52.527
	(26.543)	(26.596)	(26.610)
Grade inflation	-0.284	-0.124	-0.583
	(18.410)	(18.452)	(18.440)
High school graduation	0.920	0.920	0.921
	(0.272)	(0.271)	(0.269)
Academic high school	0.470	0.488	0.509
	(0.499)	(0.500)	(0.500)
College attendance	0.255	0.261	0.277
	(0.436)	(0.439)	(0.448)
Background characteristics			
Predicted GPA	52.051	52.667	53.548
	(13.421)	(13.578)	(13.920)
Mother’s years of schooling	11.789	11.848	11.967
	(2.221)	(2.254)	(2.273)
Father’s years of schooling	11.504	11.589	11.801
	(2.548)	(2.564)	(2.579)
Elite degree (mother)	0.093	0.097	0.097
	(0.290)	(0.295)	(0.296)
Elite degree (father)	0.081	0.081	0.097
	(0.273)	(0.273)	(0.296)
Mother’s log earnings	11.711	11.732	11.766
	(0.834)	(0.825)	(0.822)
Father’s log earnings	12.214	12.228	12.243
	(0.763)	(0.782)	(0.793)
Foreign-born	0.021	0.026	0.024
	(0.145)	(0.160)	(0.153)
Foreign background	0.069	0.094	0.089
	(0.253)	(0.292)	(0.284)

	(1) All pupils	(2) Pupils in treated neighbourhoods	(3) Pupils in control neighbourhoods
Background characteristics, cont.			
Number of siblings	2.247	2.241	2.209
	(0.891)	(0.886)	(0.873)
Observations	736,573	45,653	210,798

Note: All descriptive statistics, except for the teacher variables, are based on data for the years 1994-2002. The descriptive statistics for the teacher variables are for the years 1996-2002. The number of observations for the 4 teacher variables is 534,118; 31,590 and 153,530 for the respective column in the table.

Table A3. Effects of school grade expansion on grades in English and mathematics

	(1) English	(2) English	(3) Maths	(4) Maths
Grade expansion	0.160	0.366	0.132	0.277
	(0.893)	(0.907)	(0.755)	(0.749)
Grade expansion t-2	-0.248	0.018	0.262	0.427
	(0.605)	(0.655)	(0.515)	(0.549)
Grade expansion t-3	-0.005	-0.129	0.134	-0.005
	(0.526)	(0.555)	(0.559)	(0.548)
Observations	251,085	251,085	251,085	251,085
R-squared	0.072	0.194	0.069	0.173
Neighbourhood f.e.	X	X	X	X
Municipality*time f.e.	X	X	X	X
Outcome mean	52.39	52.39	52.44	52.44
Family background		X		X

Notes: Outcome variables are percentile ranked grades in English and mathematics. Family background and other additional control variables include: gender, number of siblings, mother's and father's years of schooling; mother and father holding "elite degree" (law, medicine, engineering, economics), mother's and father's log earnings (averaged across child's age 7-16); foreign born or both parents born abroad. Standard errors in parentheses are clustered at the municipality level. ** p<0.01, * p<0.05, + p<0.1

Table A4. Effects of school grade expansion on type of school and teacher resources - time varying treatment effects

	(1) School transition	(2) Cohort size	(3) School in home nbhd	(4) Attends assigned school	(5) Attends indep. school	(6) Teachers per pupil	(7) Fraction qualified teachers	(8) Teacher experience	(9) Teacher age
2nd treated cohort	-0.296**	-32.824**	0.196**	-0.056**	-0.002	-0.002	-0.081**	-1.105**	-3.513**
	(0.045)	(3.717)	(0.032)	(0.018)	(0.003)	(0.003)	(0.021)	(0.168)	(0.653)
3rd treated cohort	-0.305**	-32.371**	0.223**	-0.030	-0.004	-0.001	-0.059**	-1.055**	-3.415**
	(0.054)	(4.207)	(0.035)	(0.018)	(0.005)	(0.003)	(0.020)	(0.162)	(0.566)
4th treated cohort	-0.334**	-34.522**	0.229**	-0.002	-0.005	-0.000	-0.024	-0.871**	-2.319**
	(0.061)	(3.946)	(0.036)	(0.013)	(0.004)	(0.003)	(0.024)	(0.186)	(0.681)
Observations	256,451	256,451	256,451	256,451	256,451	185,120	185,120	185,120	185,120
R-squared	0.550	0.556	0.665	0.140	0.117	0.302	0.592	0.654	0.530
Neighbourhood f.e.	X	X	X	X	X	X	X	X	X
Municipality*time f.e.	X	X	X	X	X	X	X	X	X
Outcome mean	0.714	118.0	0.177	0.750	0.0220	0.0549	0.674	4.892	45.59

Notes: Outcome variables are defined as follows. School transition: pupil attends a grade 7-9 school, restricted sample is a sample where pupils are expected to make such a transition; cohort size: nr of pupils per cohort and school; school in home neighbourhood: pupil attends school located in his/her residential neighbourhood; attends assigned school: pupil attends expected school based on residential location; pupil attends independent school: pupil attends non-public school; teachers per pupil: the number of teachers who primarily teach in grade 4–9 divided by the number of pupils in grade 4–9 in school; qualified: the fraction of teachers who primarily teach in grade 4–9 that hold formal qualifications to teach at this level; experience: mean years of experience in the teaching profession among the teachers in school who primarily teach in grade 4–9; age: mean age among the teachers in school who primarily teach in grade 4–9.
Standard errors in parentheses are clustered at the municipality level. ** p<0.01, * p<0.05, + p<0.1

Table A5. Effects of school grade expansion on pupil outcomes - time varying treatment effects

	(1) **GPA**	(2) **Grade inflation**	(3) **High school graduation**	(4) **Academic high school**	(5) **College attendance**
2nd treated cohort	0.693	0.447	0.009	0.001	-0.004
	(0.594)	(0.593)	(0.006)	(0.011)	(0.008)
3rd treated cohort	0.188	0.206	0.000	-0.001	-0.003
	(0.911)	(0.704)	(0.006)	(0.019)	(0.012)
4th treated cohort	0.281	0.816	0.006	0.003	-0.002
	(0.771)	(0.616)	(0.007)	(0.014)	(0.013)
Observations	251,085	251,085	251,085	251,085	251,085
R-squared	0.260	0.081	0.056	0.176	0.077
Neighbourhood f.e.	X	X	X	X	X
Municipality*time f.e.	X	X	X	X	X
Family background	X	X	X	X	X
Outcome mean	53.39	-0.517	0.921	0.505	0.274

Notes: Outcome variables are defined as follows. GPA: percentile rank of pupil's GPA over all subjects in 9th grade; grade inflation: the difference between percentile ranked grades in music, arts, handicraft, home economics and physical education, and grades in mathematics and English; high school graduation: pupil is registered in high school at any point in time in the third and final year; academic high school: pupil is registered in the third and final year at an academic high school track at any point in time; college attendance: pupil has completed college coursework corresponding to at least one full semester of college. Family background and other additional control variables include: gender, number of siblings, mother's and father's years of schooling; mother and father holding "elite degree" (law, medicine, engineering, economics), mother's and father's log earnings (averaged across child's age 7-16); foreign born or both parents born abroad. Standard errors in parentheses are clustered at the municipality level.
** p<0.01, * p<0.05, + p<0.1