The Short- and Long-term Effects of School Choice on Student Outcomes –

Evidence from a School Choice Reform in Sweden

by

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This version: April 2013

Abstract

This paper evaluates the effects of a major Swedish school choice reform. The reform in 1992 increased school choice and competition among public schools and lead to a large-scale introduction of publicly funded private schools. We estimate the effects of school choice and competition, using precise geographical information on the locations of school buildings and children's homes for the entire Swedish population for several cohorts affected at different stages in their educational career. We can measure the long-term effects up to age 25. We find that increased school choice had very small but positive effects on marks at the end of compulsory schooling, but virtually zero effects on longer term outcomes such as university education, employment, criminal activity and health.

Keywords: school choice, school competition, treatment evaluation, cognitive and noncognitive skills
JEL-codes: I20, C21

Acknowledgements:
We thank Louise Johannesson, Aline Schmidt, Lennart Ziegler and Nina Öhrn for excellent research assistance. We also thank Mikael Lindahl, Anders Böhlmark, Jonas Vlachos, Peter Fredriksson, and participants at seminars at IFAU, IFN, ZEW and University of Mannheim and the HECER Economics of Education Summer Meeting, for valuable comments. We gratefully acknowledge project funding from The Swedish Research Council. The first author would furthermore like to thank IFAU and IFN for kindly hosting her as guest researcher on several occasions. The first author gratefully acknowledges support from the Leibniz Association, Bonn, in the research network "Non-Cognitive Skills: Acquisition and Economic Consequences". The second author is grateful for financial support from the Jan Wallander and Tom Hedelius foundation. The third author acknowledges support from the Research Center (SFB) 884 "Political Economy of Reforms" Project B5, funded by the German Research Foundation (DFG).

1 Introduction

Whether or not students should be allowed to choose their school of attendance is a highly controversial topic in many countries. Whereas some see school choice as a means to improve students' results, others fear that choice and competition will have adverse effects on the school system. Economic theory has no clear predictions on this matter: the aggregate expected effects of school choice and competition on students' outcomes are ambiguous. Empirical evaluations of existing school choice reforms are therefore important as they provide information on the actual effects of school choice policies.

In this paper we evaluate the effects on short-term and long-term student outcomes of a large-scale school choice reform in Sweden. The reform was implemented in 1992 and has significantly increased the amount of school choice in compulsory education. It affected the entire country and profoundly changed the workings of the Swedish school sector. Before the reform, students were assigned to the school in their catchment area. Now, 20 years after the reform, choosing school is a normal phenomenon, especially in more urban communities, and many municipalities encourage active school choice and provide information about the schools available. The reform essentially contained two elements: first, it allowed publicly funded but privately run schools[1] to set up and compete on basically equal terms with the publicly run schools; second, it encouraged choice among the already existing public schools. We believe that this reform, together with the detailed data that we have access to, provides a good opportunity for obtaining empirical evidence of the effects of school choice. Moreover, since the reform was introduced 20 years ago, we can now assess not only its short- but also the long-term effects.

The first part of the reform, the introduction of privately run, but publicly funded, schools, has been extensively studied (see Ahlin (2003), Sandström and Bergström (2005), Björklund, Edin, Fredriksson and Krueger (2004), Böhlmark and Lindahl

[1] The Swedish term is friskolor, i .e. "independent schools", but we will refer to them as private schools throughout the paper.

(2007) and (2012), and Hensvik (2012)). The overall evidence of the previous studies suggests that competition and choice, in terms of a higher share of students in the municipality attending private schools, has had fairly modest effects on short term school results and basically no effect on long-term results.

Our study differs from those in several ways. First, while previous studies focussed on the effects of *private* schools, we study the overall effects of the choice reform, including in particular the choice among *public* schools. We examine the effects of school choice via permitting more private schools as well as via choice between the existing public schools. The latter could be particularly important since choice between public schools could be exerted immediately after the reform, whereas choice among private schools naturally requires that such new schools be founded, something that may take time and may not happen in all parts of the country. In fact, even in school year 2004/05, private schools existed only in 166 out of the 290 municipalities (National Board of Education, 2005, p.29). Also, a survey conducted by the National Board of Education revealed that, in school year 2000/01, choosing another public school than the nearest one was more common than choosing a private school (National Board of Education, 2003, pp. 48f.). Hence, private schools represent only one facet of the choice options, and the establishment of new private schools might in fact be an endogenous outcome of what is offered by the existing public schools.

Second, whereas the previous studies evaluate the effects of private schools measured by their share of students within the municipality, we use detailed geographical information on the locations of schools and student residences to construct measures of choice and competition that are specific for each student and each school. In particular, we calculate student-specific measures of the number of schools available, and school-specific measures for the competition they face from other schools. Our evaluation method then consists of comparing the outcomes of students with different degrees of school choice and competition, *before* and *after* the reform. The idea is that students with few schools nearby will in practice be unaffected by the introduction of the choice reforms (i.e. they will only have one school to choose from anyway), while for students with many schools nearby, the choice reforms will have a large impact on the actual choice opportunities.

Using identifying variation at the student-level, instead of at the level of the municipality, is potentially important since municipalities vary a lot in size, both in terms of population and area,[2] which means that variation only across municipalities may be too crude to capture the essential variation in choice and competition. Our approach also has the advantage of estimating the effects of choice *opportunities*, whereas the share of students in private schools only measures the degree to which students *exercised* choice to private schools. This is a possibly important distinction, as (potential) choice and competition could affect school quality, even if we only observe few people to actually change their school of attendance. A further advantage of having access to detailed geographical data is that we can construct different measures for the degree of competition facing each school, and the degree of choice facing each student. In supplementary analyses, we will thus distinguish between general effects caused by an increased competition among schools, and individual effects caused by students' possibility of choosing a school that best matches their preferences.

An important methodological issue that we need to deal with is the fact that the location of schools after the reform, in particular of the private schools, is likely to be endogenous with respect to student and community factors (such as student ability and background, or population density), or with respect to the performance of existing schools in the area (demand for private schools could for example be higher where public schools are bad). Moreover, if school choice and competition leads to improved school quality, it might also be that parents who are very concerned about education may move to regions with many schools.[3] If we knew which factors were important, we could control for them; yet, several factors may be unobserved.

Our empirical strategy is to use the *pre-reform* locations of schools and students' homes to measure choice and competition. That is, for students choosing a school in or after autumn 1992, we will measure choice as present right before the reform, in 1991. As we argue later, the school choice reform came largely as a surprise because of an

[2] The largest municipality in terms of population, Stockholm, had 864,324 inhabitants in 2011, while the smallest, Bjurholm, had 2,431. The largest municipality in terms of area, Kiruna, is 20715 km^2 and the smallest, Sundbyberg, is 9 km^2. (Source: www.scb.se)

[3] Before the reform, we would see parents move as close as possible to a good school, which does not imply that there are many schools in the area.

unexpected federal election outcome. Hence, the location of schools and families was pre-determined to the reform. For students choosing a school under the old system, i.e. before 1992, we will therefore measure choice in the year they make their decision without risking endogeneity with respect to the reform. Using this strategy, we also permit that the establishment of new (private) schools or the closure of schools may be an endogenous outcome of the school choice reform. In our main specification we will thereby estimate the effects of school choice and competition as introduced by the 1992 reform. Our estimates will thus include all effects resulting from the dynamic processes that were triggered by choice and competition as it was present at the outset of the reform, like the opening or closing of schools and parents moving in response to the new options. In additional analyses we will also examine these processes.

Obviously, the location of schools even before 1992 was not random and also school choice was possible to some extent before by moving residence (i.e. Tiebout choice[4]). To deal with this, we control for many observable background characteristics at the individual and regional level and include municipality fixed effects. Moreover, as mentioned before, we also observe unaffected cohorts in our dataset which allows us to control for all time-constant relationships between having many schools nearby and student outcomes. Further, we make use of these unaffected cohorts to test whether pseudo treatment effects are indeed zero and control for pre-reform trends. The intuition for our identification strategy can thus be summarised as follows: The reform of 1992 came as a surprise to the population. Until then, parents had to move homes to exercise choice; afterwards, school choice was much easier. The number of schools available in 1992 is pre-determined for those cohorts entering school then. While differences in contextual factors between many-schools and few-schools areas have already existed before, we can control for many observed covariates and make use of the many pre-reform cohorts to additionally control for time-constant unobserved factors. Additionally, we can use unaffected cohorts to test whether pseudo-treatment effects are indeed zero.

[4] The term stems from the work of Tiebout (1956).

We draw on very informative register data on the entire population of students and schools in Sweden, including a broad range of short term and long term student outcomes, ranging from educational results to labour market outcomes and socio-economic indicators. We can hence study the effects on a wide array of outcomes. The data cover a long period and hence enable us to evaluate the effects both immediately after the reform and many years later. This is important since effects on non-cognitive skills may not be fully reflected in school test scores but may become visible only later in labour market outcomes or criminal activity.

Our empirical results reveal that the effects of school choice as well as competition were very small during the period considered. This finding applies to the short-term effects on test scores and grades as well as to the longer-term effects on employment, higher education, criminal activity and health, where there is often no effect. The effects become larger for younger cohorts, i.e. those affected by the reform earlier in life, yet still remain very small. While the effects of choice and competition are hard to disentangle because of a high correlation, choice tends to have a *positive* effect, while competition tends to have a *negative* effect on marks, but almost only for students that were already in school as the reform was enacted. The latter could be due to the reform causing a disruption to the previously stable school system to which the schools eventually adjusted.

The magnitudes of all effects are very small, though. A potential explanation for this is that the previously existing Tiebout choice (i.e. moving homes) may already have delivered sufficient choice options for those families who wanted to choose. Moreover, according to economic theory, the school choice reform is expected to affect students' outcomes in various ways, and it is possible that the very small estimated effects reflect that negative and positive effects in practice cancel each other out.

2 The Swedish School System

2.1 General information on the Swedish school system

Sweden has nine years of compulsory schooling, starting the year the student turns seven. Throughout these grades, all students follow the same basic curriculum. After the compulsory schooling, the great majority of students continue with voluntary secondary school.[5]

Compulsory education is organised in three stages: grades 1 to 3, grades 4 to 6 and, finally, grades 7 to 9. Grades 1 to 6 are referred to as primary school, whereas secondary school starts with grade 7. Schools usually offer either only grades 1 to 6, or grades 7 to 9, while some offer all grades 1 to 9. Therefore, school choice is particularly relevant for entering school (i.e. grade 1) but also for grade 7, where many students graduate from elementary schools offering only grades 1 to 6.

Compulsory education is organised and provided by the municipalities, and the main source of finance of compulsory education is municipal tax revenues, followed by central government grants. Both the tax base and the grants are adjusted by equalisation formulas that are designed to give municipalities with different population structures roughly equal economic conditions.

2.2 The school system before the reform in 1992

The school choice reforms that are studied in this paper took place in the early 1990s. Before that, school choice in Sweden was very limited as students were placed in the school of their catchment area. Privately run schools existed, but they were few, and public funds were restricted to schools with alternative pedagogic profiles.[6] There also existed a few public schools with special profiles, such as music, which accepted students based on their skills in the relevant subject. In general, however, school choice was limited to Tiebout choice, i.e. to moving near the desired school.[7]

Politically, there were however heated debates on choice and competition in the public sector, including the education system. The right-wing opposition, especially the party Moderaterna, argued in favour of increased school choice and competition

[5] In 2011, 98 per cent of students entered secondary school. The share of students graduating from secondary school in at most 4 years was approximately 75 per cent in years 1999–2011 (see The National Board of Education: www.skolverket.se).

[6] In fact, until 1987, in order to receive public funding, schools were in addition required to prove that the use of these alternative pedagogical methods also benefited the development of the public schools, see The National Board of Education (2003).

[7] The allocation of students to schools was regulated in the compulsory school decree (Grundskoleförordningen 1988:655 Chapter 2 § 23), where it was stated that the allocation shall be based on what is appropriate in terms of transportation, efficient usage of facilities and other educational resources, and on parents' and students' wishes.

throughout the 1980s, but the Social democrats, who were in power for most of the decade, had a much more restrictive attitude. This reluctance started to soften during the late years of the 1980s, but, even then, the idea of the Social Democrats was first and foremost to increase choice and flexibility by increasing the local influence within schools, for example in terms of allowing schools to profile in terms of pedagogical style or special subjects.[8] It can be noted that school choice was tentatively discussed also by the Social Democrats in the late 80s and early 90s, but then mainly in terms of making it easier to choose schools with special profiles, should these become more common.[9] Apart from the few existing private schools and schools with special profiles, school choice only existed at the idea stage. This was however soon to change.

2.3 The 1990s school choice reforms

The regime shift in terms of school choice began in the fall of 1991, after a very tight parliamentary election brought a right wing coalition to power.[10] The newly elected government took a series of steps to increase choice and competition in the education sector:

In March 1992, the government proclaimed, in proposition 1991/92:95, that the aim was to "achieve the largest possible freedom for children and parents to choose school". It furthermore stated that "This freedom should apply both to choice between the existing public municipal schools, and to private schools."[11]

In June 1992, the parliament voted in favour of the proposition, and thus opened up for more choice between the existing public schools as well as for publicly funded but privately run compulsory schools to operate under basically the same conditions as the public schools. This new type of privately run school was to receive funding, through a

While the regulation hence specified parents' and students' wishes as one (out of many) factor(s) to be considered, the general rule was that students were allocated to the nearest school.

[8] See for example proposition 1988/89:4.

[9] See pp. 56–57 proposition 1988/89:4.

[10] The right wing coalition (Moderaterna; Folkpartiet; Centerpartiet; and Kristdemokraterna) obtained 46.6% of votes, the socialist block (The Social Democrats and the Left Party (Vänsterpartiet)) 42.2%, and a populist party, New Democracy, which has since then disappeared from politics, obtained 6.7% and hence acquired a power balancing position. The greens, Miljöpartiet, received 3.7% of the votes and were hence only 0.3% away from parliamentary representation. In 1994 the Social Democrats came back to power, but by then the school choice reform was largely accepted, and no attempts were made to reverse it.

[11] See proposition 1991/92:95: "Målet är att åstadkomma största möjliga frihet för barn och föräldrar

voucher system, from the enrolled students' home municipalities, at a minimum of 85% of the average cost per student in the home municipality. [12] The schools were to be open to all students, and could only charge very limited additional student fees.

In 1994, another change in the school law, following proposition 1992/93:230, opened up for choosing a public school in another municipality than that of residence, something that was previously only allowed for independent schools, or in special cases such as bullying.[13]

In summary, propositions 1991/92:95 and 1992/93:230 established private schools as a publicly funded alternative, and made a strong statement that the central government viewed school choice as important. While the main law changes implemented in these reforms treated the opening up for independent schools, it is clear from the propositions that the aim was to increase overall school choice, both by facilitating for privately run schools to enter, and by encouraging choice between existing publicly run schools. Evidence by the National Board of Education suggests that school choice, both to private and public schools, has increased a lot during the 20 years that the reform has been in place, in particular in more urban areas.[14]

2.4 Other education-related reforms

The school choice reforms in the early 1990s were not the only education-related changes taking place in the 1990s, but they were part of a broad decentralisation and choice-enhancing trend in the organisation of the educational sector, as well as in the public sector in general. The main changes consisted of making the municipalities, instead of the central government, responsible for the provision and organisation of compulsory education, and of replacing the system of ear-marked central government

att välja skola. Denna frihet bör innebära möjlighet att välja mellan det offentliga skolväsendet och fristående skolor men också att välja skola inom det kommunala skolväsendet och att välja också en skola i annan kommun."

[12] The reason for setting the minimum compensation level to less than 100% of the public schools' average cost reflected that the public schools were still ultimately responsible for granting all students in the municipality compulsory education. This, it was argued, could give rise to higher costs for example for administrative costs for ensuring that all students in the municipality attend school and costs from having to offer schooling to children from private schools that stop operating. In addition, public schools have to cater to all students, and cannot select students by, for example, offering only certain profiles (see prop 1991/92:95.) In1994, following the return of the Social Democrats to power, the minimum voucher level was lowered to 75% of the average cost.

[13] Following this proposition, the independent school reform was also expanded to secondary school level (grades 10-12).

[14] See Section 8.1.1 in the appendix.

grants with a system of general central government grants.[15] Since these other reforms increased the municipalities' influence over compulsory education, it is reassuring that our analysis is, in contrast to most other studies of the Swedish choice reform, not conducted at the level of the municipality, which would risk to pick up effects of these other reforms.

3 Mechanisms of School Choice and Competition

In pre-reform Sweden, students were in general allocated to schools according to the proximity principle (i.e. to the nearest school), and the only way to change school was by moving. With the reform, choice could be exercised without moving. These enhanced choice options could affect student outcomes through various channels.

First, school choice can improve the matching of students and schools, e.g. regarding the desired pedagogical tools or any other aspect of the student-school match that improves the productivity of education. This should have an unambiguously positive effect on student and school results. In addition, students may increase their effort if they are allowed to attend the school of their liking.

Second, school choice may affect the allocation of students, which in turn gives rise to different peer effects.[16] Theoretically, it is not clear how school choice should affect the composition of students between schools: On the one hand, loosening the link between residential address and school of attendance could in principle decrease segregation[17] with respect to parental background (income, immigrant background etc.) as students are no longer required to attend the school nearest to their home. That is, students from poorer areas can gain access to schools in rich areas, even though they cannot afford to live there. On the other hand, however, school choice can also lead to

[15] For a more detailed overview of these reforms, see Section 8.1.2 in the appendix.

[16] See for example Epple and Romano (1998) for a theoretical model on school choice where students sort according to ability and where peer effects are modelled. For empirical evidence on peer effects, see for example Zimmerman (2003), Sacerdote (2001), Lefgren (2004), Hanushek, Kain, Markman and Rivkin (2002), Angrist and Lang (2004), Ammermueller and Pischke (2009), Lavy and Schlosser (2007) and Hoxby (2000).

[17] Segregation may refer to different aspects of student and parental characteristics. Here we deliberately use the term loosely, in the sense of "less mixing" with respect to any characteristic that may be of importance for peer effects and so to the productivity of education.

more segregation, if parents/students increasingly choose to attend schools with similar peers[18].

It is also a priori unclear how being surrounded by more or less similar peers (with respect to academic ability, parental background etc.) may affect students' educational outcomes. On the one hand, more homogenous classes are easier to teach. On the other hand, weaker students may benefit disproportionally from stronger students, which are only available in more heterogeneous classes. The overall effects are ambiguous.

Third, school choice can put competitive pressure on schools to improve quality in order to attract students.[19] That more competition leads to higher quality, however, hinges on a couple of assumptions: i) that school quality is a determining factor for the choice of school; ii) that parents can observe school quality; iii) that schools have an incentive to attract students. The fact that funding for Swedish schools is, at least partly, based on the number of students,[20] suggests that there is an incentive for schools to at least attract enough students to fill the classes in order to cover the fixed costs for facilities and teachers. Having many applicants may in addition be desirable as it signals high reputation and status, and teachers and headmasters have a clear incentive to avoid a situation where the number of students is so low that the school is forced to shut down.

The first assumption – that school choice is based on the quality of the school – is complicated by the fact that school quality can be difficult to observe. This means that,

[18] In additional analyses, we do not find evidence for a change in overall segregation at schools in terms of the socio-economic background of the parents, characterised by income, educational level and being born outside of Sweden, after the reform. Our measure of segregation, which is the yearly average of the standard deviation in the share of students with different characteristics across schools in Sweden, does however not take into account changes in residential sorting, i.e. it does not imply that school choice did not change sorting into schools on the local level.

[19] See Hoxby (2003) on school choice and school quality. See also Hanushek (1986) for an early overview of education production functions.

[20] There exists little information on the different resource allocation models used by the municipalities: the first country-wide survey, covering all municipalities, refers to the situation in 2007 (The National Board of Education (2009)). The survey suggests that the vast majority of municipalities base the resource allocation on the number of students (although part of the budget is not per-student-based, but based on, for example, special needs). Only 9 per cent of the municipalities responded that none of the budget was directly volume based, and that the allocation was instead made through an application-procedure (the Swedish term is: äskanden), and through dialogue with the school units. According to the authors of the report, it is however likely that volume was indirectly considered also in these municipalities, although not necessarily through an exact amount per student (p. 39). The survey furthermore suggests that the budget allocation procedures have often been in place for a long time: 52 per cent of the municipalities respond that they have used the same model for the last six budget years or more. 22 per cent respond that the current model has been used for 4–5 budget years, and the remaining 26 per cent respond that the current model has been used for less than four budget years.

even though parents, all else equal, may want to choose the better school, they may in practise not be able to observe this. In the Swedish case, this is a relevant aspect since the only school level results that are publicly available are the final average grades, i.e. grades when students exit compulsory school in grade 9. In addition, if school choice is determined by student grades, schools have an incentive to inflate grades, which naturally devaluates their value as quality indicators.[21]

In addition, there are a number of factors – apart from school results – that potentially influence the choice of school, such as proximity, facilities, peers, extra-curricular activities etc.[22] These factors may or may not be correlated with students' learning. The competitive pressure on schools to attract students can hence in principle even give rise to negative effects on student outcomes by shifting focus from factors that improve teaching and learning to factors that are unrelated to students' learning, but potentially more easily observable, such as peer quality.

In sum, school choice can in theory give rise to various mechanisms, and it is hence a priori unclear which effects we should expect on students' outcomes. This makes an empirical evaluation of school choice reforms all the more important.

It is also worth mentioning that the Swedish school choice reforms are likely to give rise to a *process* of changing incentives. For example, even if competition between schools eventually gives rise to over-all higher quality, this is a process that is likely to take time, and that may in the meantime cause disruptions, as bad schools downsize and better performing schools expand. The effects of the school choice reform may hence take time, and may also look different over time. This is important to take into account in the empirical analysis.

[21] Vlachos (2010) suggests that the competition stemming from the introduction of independent schools has given rise to some, but very modest, grade inflation. His estimations suggest that a ten percentage point increase in the private school share would give rise to a 1–2 unit increase in the average student credit values (which is a measure of students GPA). This is a small effect, considering that student credit values are given at a 0–320 scale, with mean value at 206. We examine grade inflation in Section 8.1.5 in the appendix.

[22] For example, Burgess, Greaves, Vignoles and Wilson (2009) suggest that British families choosing school care both about the academic performance and the student composition.

4 Data

The study uses Swedish register data for the full population born in the years 1972 to 1990 and contains data from Statistics Sweden, the Swedish National Council for Crime Prevention, the Military Archives and the Swedish Defence Recruitment Agency.

First, as previously mentioned, we have access to detailed information on the geographical location of schools (for years 1988–2006) and students' residences (for 1985–2006), which enable us to construct student- and school-level indicators of choice and competition.[23] How these are constructed will be explained in the following section.

Second, our data contain information on a broad set of short-term and long-term student outcomes: First, we can observe the educational attainment at age 16 in the form of average final grades from compulsory school, i.e. by the end of grade 9 and, for the last 4 cohorts in our sample, the 9th grade test scores in English, Swedish and Maths. Since the latter are only available for a subsample of students, we will note make use of the information on the 9th grade test scores in the main analysis, but only in order to test for grade inflation in Section 8.1.5 in the appendix. In addition, for the male students, we have access to cognitive ability test scores from the military draft. These test scores, which are also used in for example Grönqvist, Öckert and Vlachos (2010) and Lindqvist and Vestman (2011), contain the overall scores from four subtests that measure the draftees' verbal, logical, spatial and technical ability, and are used to sort draftees to different assignments in the military service. The draft test scores are available for all cohorts, although for the later cohorts, the share of draftees drops significantly.[24] In terms of longer-term outcomes, we observe whether the individual was employed at the age of 25, as well as the highest educational degree the individual had completed at that age. We choose this age since it allows us to include many cohorts in the analysis – choosing a later age would have the benefits of capturing also older graduates, but

[23] Specifically, we have access to the midpoint coordinate of 100*100 m squares for student residence and school location, i.e. the coordinates measure the residential location with a maximum error of approximately 70 meters.
[24] Until the late 90s, virtually all 18–year-old males were required to take the test. After that, although the universal draft remains on paper, in practice only a minority of each cohort goes through the military service, see Figure 5 in the appendix. According to anecdotal evidence, the drafting decision can now in practice be influenced by the draftees, which leads to potential selection problems in this variable. We have analysed whether the selection is related to the choice reform (see Section 8.1.6 in appendix) but find only a very small association, which we do not believe to have important effects for our results.

would on the other hand decrease the number of cohorts for which we observe the outcome. We also observe whether individuals had health problems, indicated by receiving sickness benefits[25], at age 22, and whether the individual had ever been convicted for crime (including all crimes, from pilfering and petty traffic- and drug related crimes, to more serious types of crime, but excluding civil penalty) [26] at the same age.

An important task will be to control for all covariates that could potentially influence the outcomes, while also being correlated with the choice/competition variables. We therefore use a broad set of background covariates at the level of the student (including parental background information) as well as at the level of the local area (parish and/or municipality)[27]. The list of control variables is given in the note to Table 4 and further descriptive statistics are given in Table 3 [28].

Table 1 shows descriptive statistics of students' outcomes for affected and non-affected cohorts. Non-affected cohorts are those that have left 9th grade before autumn 1992, i.e. before the reform was implemented. These are all students born in the years 1972 to 1976. Summary statistics of variables measuring choice and competition will be given in Section 5.2.

Comparing the development of outcomes for the two different cohort groups, we see an increase in the share of individuals with a university degree at age 25 from 35% to 41% and a decrease in share of those employed at the same from 71% to 69%. It has to be taken into account, of course, that there are also still students who have not yet finished their studies at this age, which might thus reduce the share of employed individuals. The percentile rank in the grade point average at grade 9, which ranges from 0 to 100, has a mean of 48.21 for the non-treated and 49.40 for the treated cohorts

[25] This variable is based on the sum of the yearly benefits received as sickness benefits and as benefits for early retirement due to bad health. We define an individual as having health problems if she/he received an amount exceeding the price base amount, which is an amount used in the social welfare legislation, and which varies with the aggregate price level. During the data period of our study, this amount was approximately €4,000.

[26] The Swedish term is "ordningsböter".

[27] The municipality level covariates were downloaded from the webpage of Statistics Sweden (www.scb.se), except for the indicator for urban municipality, which was constructed based on the 2005 year municipality classification by the Swedish Association of Local Authorities and regions (SKL). The parish level covariates were generated from individual level data generously made available from the Institute for Labour Market Policy Evaluation (IFAU). These data, as well as our individual-level covariates, come from the national registers held by Statistics Sweden.

and a standard deviation of 28.6 for both.[29] The cognitive score is a standardised measure that ranges between one and nine, with median value 5, and has a mean of around 5 and a standard deviation of about 1.9 in both cohort groups. The share of those having committed any criminal offense up until age 22 is 16 per cent for the untreated and 14 per cent for the treated cohorts. Note that this also includes small offences, like speeding or petty crimes, which explains why the share is not smaller. Since school choice may affect a student's peer group and the degree of segregation, which in turn could affect the social adjustment of students, we believe that it is important to also include these less serious types of offences.

5 Empirical Strategy

5.1 Identification

In order to estimate the effect of school choice as introduced by the 1992 reform, we need to address two main empirical challenges. The first is to separate the effect of having more school choice due to the reform from effects of other factors that are related to our choice measure, i.e. the number of schools close-by, also in the absence of a free school choice regime. The second is the potential endogeneity of schools' choice of location and parents' choice of residence after the reform. To deal with the first, i.e. to separate the effect of school choice from background factors that are correlated with living in an area with many schools, we include many regional- and individual-level covariates and municipality fixed effects in our estimation. Moreover, we control for the effect of our school choice measure on student outcomes in a situation without free school choice by including the unaffected cohorts in our dataset. Thus, we estimate the *additional* effect of having more schools nearby for cohorts that chose a school after the reform was implemented, compared to cohorts that chose a school before reform. We thereby control for time-invariant influences of unobserved factors that are correlated with both the choice measure and the outcome variable, conditional on many control

[28] All monetary variables have been deflated to year 2006 monetary value, using the consumer price index (source: Statistics Sweden, www.scb.se).
[29] The reason for the mean rank value not being exactly 50 is that ties in the data were given the same rank.

variables. Our identifying assumption is that the effect of having many schools nearby in the counterfactual situation, i.e. if the 1992-reform had never been enacted, can be estimated by the effect of having many schools nearby for cohorts that left education before the reform was enacted. This is similar to the assumption of common trends in a difference-in-differences design as we assume that the effect of having more schools nearby would have been the same as it was before the reform if there had been no reform.

This assumption is not directly testable, but we can make it more plausible by including a large set of control variables on the individual, municipal and parish level. Whatever is not controlled for is thus assumed to be constant over time. Importantly, we can assess the credibility of the assumption by performing *placebo tests* on the five pre-reform student cohorts. That is, we pretend the reform had happened two years earlier and estimate the effect of this “placebo”-reform. If our control variables successfully capture all correlation between our choice-index and other factors that affect student outcomes, and there is no additional time-varying influence of other factors, we expect the resulting placebo-effects to be zero. Furthermore, we can test for time trends in the effect of having more schools nearby in the pre-reform cohorts. Not finding any such trends can be seen as an indication that the results of our analysis are not due to time trends that are unrelated to the choice reform. Finally, even in cases where we do find evidence of time trends before the reform, we can use the five non-affected cohorts of students to estimate and control for such pre-reform time-trends when we estimate the choice-effect of the reform.

The second empirical challenge stems from the location of new schools, and the residential choice of parents, after the reform. Many new private schools opened up and their chosen locations are certainly not random. Some of them operate as for-profit schools and would base their location decision on expected profits. The many new private not-for-profit schools follow a social mission and would also not choose geographical location randomly. Ignoring such deliberate location choices in a regression analysis would lead to biased results, where the direction of the bias is uncertain. It could be positive if schools locate in areas where students perform well, e.g. in order to cream skim the best students, or to meet a demand for good schools in areas where parents and students are eager to learn and willing to invest time in actively choosing a school. On the other hand, the bias could be negative if schools locate in areas where the educational quality was previously low.

To deal with these problems, our main empirical strategy is to base our measures of choice and competition on the *pre-reform* location of schools and students. Since the school reform should have come as a surprise to the population, due to the tight race in the 1991 national election, we can consider them as pre-determined and thus not endogenously affected by the reform itself. Hence, for cohorts that chose a school after 1991, we will approximate the amount of choice they faced by measuring the amount of schools they had nearby in 1991, just before the reform. For cohorts that chose school before the reform, we use students' actual location of residence in the year they enter 7th grade, or, depending on data restrictions, the information that is closest to that year.[30]

By measuring choice and competition via the pre-reform location of schools and students, we will measure the overall effect of the reform that goes through having more schools nearby at the beginning of the process. This effect will comprise all dynamic processes happening after the reform, such as new schools opening up or schools closing down. In later sections we will also examine how the school choice reform affected the number of public and private schools, that is how our *pre-reform* measures of school choice are related to school choice measured after the reform. We believe that our approach captures the policy-relevant parameter, particularly for a school reform that encourages and supports *non-public* schools, such that the exact placement of these schools is more market-driven and less centrally determined. (In many countries, Tiebout choice with only few private schools is still the status quo.)

Our estimates of the school choice effects are to be interpreted relative to the Tiebout choice that already existed before the reform: Families had always been able to choose schools via changing their place of residence and moving into the catchment area of their preferred school. We imagine that Tiebout-type migration was more frequent in

[30] We have information on individuals' residential coordinates starting from year 1985. However, when for example constructing a measure for choice on the grade level 1-3 for cohort 1972, we would need to know their coordinates in the year 1979. In cases like this, we use their coordinates in 1985 instead. For schools, we only have information on coordinates starting from 1988. Therefore, when merging the competition measure to individuals who started a certain grade level before 1988, we merge the school competition measure from 1988 instead.

areas with many schools, where merely a short move was sufficient in order to switch catchment area. In addition to Tiebout choice, also another potential mechanism existed before the reform through which the number of schools might have affected student outcomes: Having had many schools nearby may have given parents the possibility to compare different schools and thus increase their ability to judge the quality of the school their children go to. This would have enabled them to complain and put pressure on the local education authorities to increase quality. Our estimates will thus reflect the *additional* effect due to being able to choose without moving homes.

One can imagine that the new choice possibilities after the reform may reduce Tiebout-type choice as the reform weakened the link between location of residence and school of attendance. While we cannot thoroughly test that hypothesis, a descriptive analysis in Section 8.1.3 in the appendix, however, shows no evidence for it.[31]

5.2 Measuring the degree of choice among schools

The degree to which students can exercise school choice crucially depends on the availability of alternative schools in the vicinity of students' homes. Thus, we measure the degree of school *choice* by exploring the distance between a student's home and the schools a student could potentially choose from.[32] Specifically, we count the number of schools within a given radius around a student's home in order to measure her choice possibilities.[33] As Sweden is a geographically diverse country with very rural but also urban areas, our preferred radius is the median commuting distance within each municipality in 1992.[34] This radius takes different local settings into account in a very flexible way and, in our opinion, can be used to approximate the area within which parents might consider different schools for their children. The average median commuting distance across all municipalities is about 5km. In addition to this flexible

[31] It can be added, however, that our graphs only show descriptive statistics starting from 1991. They do hence not rule out that Tiebout choice existed before then. If the degree of Tiebout choice, for some reason, was changing during the years before that, i.e. in the pre-reform period, then this could give rise to pre-reform trends in the outcomes and cause our placebo-test to fail.

[32] See Section 2.3 for details on which schools a child could in principle attend.

[33] See also Gibbons, Machin and Silva. (2008), Himmler (2009) and Noailly, Vujic and Aouragh (2009) for other studies using the distance between a student's home and schools.

radius, we will also estimate the effects using a 2km radius as a test of the robustness of the results.[35]

Another issue refers to the point in time in a child's schooling career when one should measure the degree of available school choice. In the Swedish compulsory schooling system, it is common not only to choose a school when starting first grade, but to also potentially change school at the beginning of 7th, and sometimes also 4th, grade. For this reason, there are three points in time in the schooling career at which the degree of school choice might potentially be important. We found however, that these measures are very highly correlated, i.e. having more schools in the neighbourhood that offer grades 1-3 is highly correlated with also having more schools that offer grades 7-9. Because of the high correlations we were unable to include these different measures in the same regression. Hence, we will only include choice measured at one grade level at a time in the estimations, and following the previous Swedish studies, which all analyse choice and competition in grade 9, we focus on choice opportunities when choosing a school that offers grades 7-9. Note also that this is a point in a child's educational career at which parents might pay special attention to choosing a school, as the marks at the end of 9th grade are important for admission into high school. In our main specification, we thus measure among how many schools offering grades 7-9 a child may choose from at the age of 13, which is when children enter 7th grade, or, as explained in the last section, in 1991, if the child started seventh grade after the reform.[36]

Table 2 shows descriptive statistics for our choice measures separately for affected and non-affected cohorts. The average number and the standard deviation of the distribution of schools offering grades 7-9 within median commuting distance around a student's home are 3.45 and 4.66 for the non-affected cohorts[37]. With a mean of 5.91, students born after 1976 have on average more schools within their median commuting distance, measured at their place of residence in 1991 and taking into account schools

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[34] We are grateful to John Östh for providing information on municipality commuting distances. The distances are measured "as the crow flies", and do not take into account the directions of roads etcetera.
[35] We also explored several different other radii and obtained similar empirical results.
[36] We only have geographical information on schools starting from year 1988. Students born in the years 1972-1974, who should be matched to schools' location in the years 1985-87, will be matched to schools' location in the year 1988 instead.

existing in 1991. The reason for this increase is that our choice-measures are computed taking into account the 1994 law change (see Section 2.3) that enabled students to attend public schools also in other municipalities, something which was previously restricted to special cases or private schools. For the smaller radius of 2 km, this change has less impact, and the average number of schools within 2km around a student's home only increases from 1.24 to 1.35 schools for affected versus non-affected cohorts.

Another fact to note is that the median number of schools within 2 km from students' homes is only one, meaning that for at least 50% of the sample, this measure implies no choice close to home. When using the radius that is endogenous to local circumstances, namely the median commuting distance, the median number of schools is two, thus already capturing some choice also for those in the lower part of the distribution. The measures will thus compare different groups of people and will have a different power in measuring choice in different regions.

5.3 Estimation

In a first step, we estimate the effect of choice on student outcomes separately for each cohort and graphically inspect whether we can see a pattern in how the effect evolves over time. This approach has the advantage of being very flexible in identifying how the effect changes over time but comes at the price of not using between-cohort variation to control for time-constant effects, which might help with the identification. In all estimations we use least squares for continuous outcome variables and probit estimation for binary outcome variables and report marginal effects in all tables. We allow for clustering of the error term on the school level[38] as it is likely that error terms of students at the same school will not be independent.

Our first analysis is used mainly to obtain a graphical representation of the correlation between choice and outcomes over time, shown in Figure 2. The following regression (1) is estimated *separately* for each cohort born in {1972,...,1990}:

(1) $$Y_i = \beta \cdot c_i + \delta \cdot X_i + \lambda_{municipality} + u_i \quad \text{estimated separately for each cohort}$$

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[37] The average median commuting distance over all municipalities is 5.8km, with a standard deviation of 4.2km, minimum of almost 1km and a maximum of 26km.

[38] Since we cannot link schools over time in our dataset, the clustering will not be on the school level over time but just within cohorts.

where c_i is the choice measure, X_i is a vector of control variables, $\lambda_{municipality}$ are municipality fixed effects, and u_i is an error term. The list of control variables X is given below Table 4. Descriptive statistics on these variables are given in Table 3.

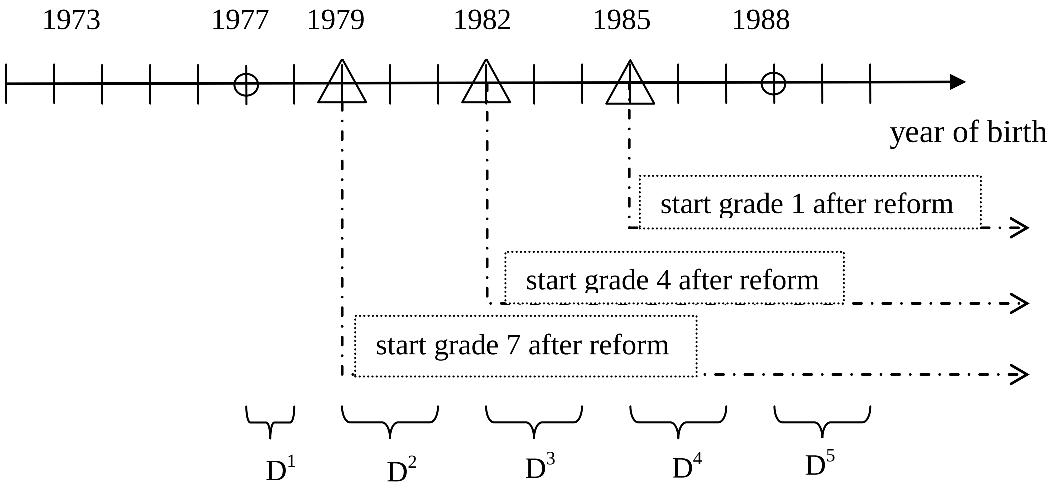
In our main analysis we instead pool the observations from *all cohorts* and estimate the differential effect of choice before and after the reform. In principle, we could permit the effect of choice to vary from year to year, i.e. one cohort happened to be in grade five when the reform was enacted, the next cohort was in grade six etc. For statistical precision and also because choice is usually exercised only at grades 1, 4 or 7 and only very rarely at grades 2, 3, 5, 6, 8 or 9, in our main specification we will however define treatment windows of three years length instead. Therefore, we define the five dummy variables:

$$(2) \qquad \begin{pmatrix} D_i^1 = 1 \; if \;\; born \; in \, 1977 \; or \; 1978; \; zero \; otherwise \\ D_i^2 = 1 \; if \;\; born \; in \, 1979 \; or \; 1980 \; or \; 1981; \; zero \; otherwise \\ D_i^3 = 1 \; if \;\; born \; in \, 1982 \; or \; 1983 \; or \; 1984; \; zero \; otherwise \\ D_i^4 = 1 \; if \;\; born \; in \, 1985 \; or \; 1986 \; or \; 1987; \; zero \; otherwise \\ D_i^5 = 1 \; if \;\; born \; in \, 1988 \; or \; 1989 \; or \; 1990; \; zero \; otherwise \end{pmatrix}$$

and note that all these treatment dummies are zero for the pre-reform cohorts.

The choice of these windows is motivated by considering which cohorts are affected by school choice and competition at which stage in their educational career. Figure 1 displays this, together with the different treatment groups D^1 to D^5 that we define. One can see that the first cohort to be possibly affected by competition at grade level 7–9 is the cohort born in 1977. They went to grade nine in the school year 1992/1993 and could therefore potentially have been affected from an increased competitive pressure. However, they are unlikely to change school one year before graduation and are therefore unlikely to benefit from choice. The first cohort of students to be really affected by choice is born in 1979, as they started grade 7 in fall 1992. Starting with this birth cohort, we could imagine measurable effects of choice on academic outcomes. We nevertheless place cohorts 1977 and 1978 into treatment group D^1 since they are not a clean control group: Even though these two cohorts could not choose the school at which they started grade 7-9, they were still in grades 8-9 as the reform was enacted and could thus potentially have been affected by increased competitive pressure. Only students born *before 1977* were not affected at all by the reform.

Figure 1: Treated cohorts



Using these treatment windows, we estimate

(3) $Y_i = \beta_1 D_i^1 c_i + \beta_2 D_i^2 c_i + \beta_3 D_i^3 c_i + \beta_4 D_i^4 c_i + \beta_5 D_i^5 c_i + \alpha \cdot c_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i$

where γ_{cohort} and $\lambda_{municipality}$ are cohort and municipality fixed effects. We use OLS for continuous outcomes and Probit for binary outcomes, and cluster standard errors at the school level as before.

The coefficient α measures the relationship between (Tiebout) school choice and outcomes for the pre-reform cohorts (we do not assume a causal interpretation for α), whereas the β coefficients measure the differential effects of free choice after the reform, i.e. without the need to move residence. Including c_i in the regression nets out all effects our choice-measure might have had also on non-affected cohorts.

In addition to allowing the effect of choice to differ for groups of cohorts after the reform, compared to a constant effect before the reform, as we do in Equation (3), we also run a specification that includes a parametric time trend in the effect of choice on student outcomes. The time trend is defined as t_i = year of birth – 1972. As shown in Equation (4), where the coefficients α^t and β^t refer to the time trends, we allow the effect of choice to exhibit a linear time trend both before the reform (captured by the term $\alpha^t \cdot c_i \cdot t_i$) and, with a different slope in each treatment window group, after the reform. With this specification, we can test whether the effect of having many schools nearby already changed over time before the reform and can control for such a pre-reform trend. In this case, α^t would be significantly different from zero.

$$\begin{aligned} Y_i &= \beta_1 D_i^1 c_i + \beta_2 D_i^2 c_i + \beta_3 D_i^3 c_i + \beta_4 D_i^4 c_i + \beta_5 D_i^5 c_i \\ &+ \beta_1^t D_i^1 c_i t_i + \beta_2^t D_i^2 c_i t_i + \beta_3^t D_i^3 c_i t_i + \beta_4^t D_i^4 c_i t_i + \beta_5^t D_i^5 c_i t_i \\ &+ \alpha \cdot c_i + \alpha^t \cdot c_i t_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i \end{aligned} \tag{4}$$

where t_i = year of birth – 1972 and coefficients α^t and β^t refer to the time trends.

As discussed in Section 5.1, we will use students' residential location and schools' location from right before the reform, that is from year 1991, if the student started grade 7 after the reform. This will exclude endogenous relocation with respect to the choice-reform from the estimation. For the same reason, we will also measure all municipal- and parish-level covariates in 1991 if the choice for grade 7 was taken after 1991. For students who started grade 7 before the reform, we measure all variables at the time when they started grade 7 or, if we do not have data from that year, the most current information.

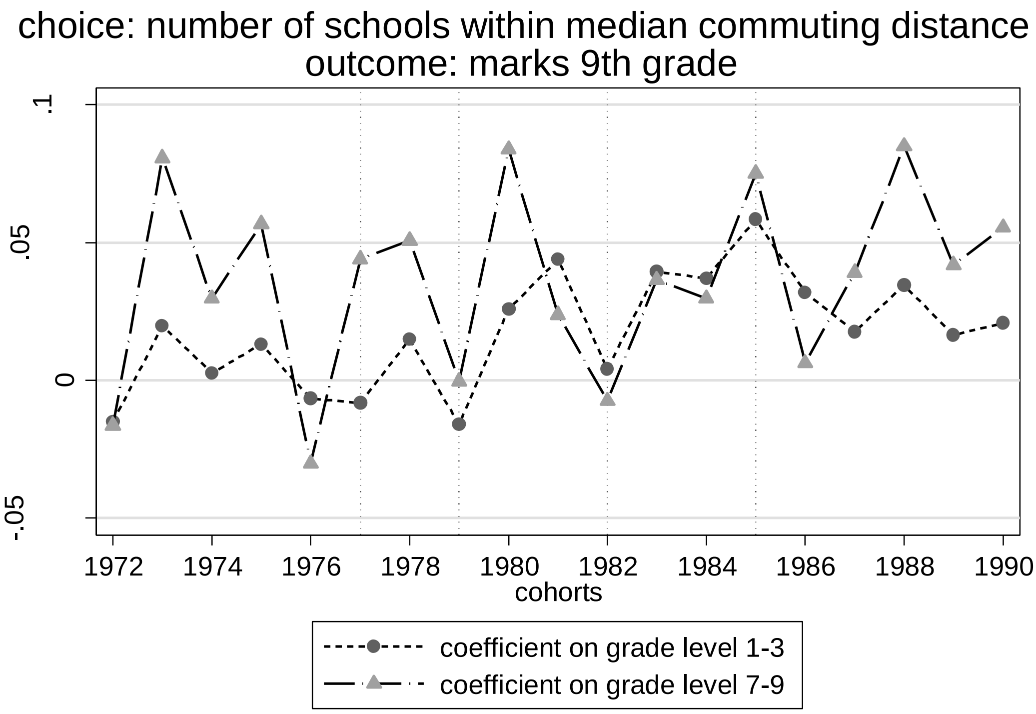
6 Results

6.1 Main specifications

We start by analysing the effect of having more schools to choose from close to home by regressing different student outcomes on the number of schools within the median commuting distance of the home municipality for each cohort separately, in accordance with Equation (1).

Figure 2 displays the estimation coefficient for the outcome percentile rank in GPA 9, using the choice measure based on the number of schools offering grade 1–3 and 7–9, respectively. For the cohorts that were completely unaffected by the reform, that is those who left primary education before autumn 1992, having more schools close to their home has no significant effect on the percentile rank of their grade point average in grade 9 (cohorts 1972-1976). This supports the hypothesis that having more schools nearby without being able to choose from them should not have any effect on student outcomes. For cohorts born after 1977, one can see a slight positive trend in the effect of choice, but this effect is statistically significant only for the youngest cohorts. Economically, the effect is very small, with an increase of 0.05-0.08 (0.02 to 0.05) percentile ranks in the GPA 9 for every additional school that offers classes on the level 7-9 (1-3) within the median commuting distance of the municipality.

Figure 2: Estimation coefficients of percentile rank in marks in grade 9 on choice



In order to quantify the difference between the importance of having more schools nearby for the GPA in grade 9 before and after the reform, and to test whether there is a statistically significant difference, Table 4 shows regression results from estimating Equation (3) for grade level 7-9. These estimates denote the overall effects of the choice reform that work through having more schools nearby at the place of residence right before the reform.

Column 1 shows results from a specification where a constant treatment effect of the reform is assumed, that is we compare the average effect of having more schools nearby for cohorts that left primary education before and after 1992. To that end, we interact the number of schools within the median commuting distance of the municipality, denoted “Choice” in the table, with a treatment window indicator that captures all cohorts that were potentially affected by the reform, i.e. all individuals born in or after 1977. The resulting point estimate shows that having one more school within the median commuting distance increases the percentile rank in GPA of cohorts that are affected by the reform by 0.06. Taking into account the observed variation in the sample[39], a one standard deviation increase in the choice measure, that is 9.35 more schools, leads to an increase in the percentile rank by about 0.56. The effect is thus very small.

However, as the reform was enacted only gradually over time, allowing for a time-varying effect is potentially important. The second column of Table 4 therefore shows results from estimating Equation (3), with treatment windows as specified in Equation system (2). The estimated effect of choice is not significantly different from zero for students born between and in the years 1977 and 1984, but is positive and significant for cohorts 1985–1990, which started first grade after the choice reform was enacted. Moreover, with an increase in the percentile rank of 0.13 for each additional school within the commuting distance, the effect is largest for the youngest cohorts. It is, however, not increasing in a linear way, which is why we prefer modelling the effect in the piecewise constant fashion rather than with a time trend. In terms of a standard deviation increase in the number of schools within the median commuting distance, the percentile rank in GPA increases on average by 1.2 for cohorts born between 1988 and 1990, and by 0.7 for cohorts born between 1985 and 1987. Thus, the effect of having more schools nearby on the marks in 9th grade is modest also for these later cohorts.

We now turn to analysing the effects of school choice on later outcomes in order to see whether the small effects on marks at the end of 9th grade fade out over time or

[39] We use the standard deviation for the post-reform cohorts here.

transform into long-lasting effects on students' adult outcomes. Table 5[40] shows the corresponding results, again using the treatment window specification displayed in Equation (3). The table shows coefficients and clustered standard errors for the cognitive score, and marginal effects at the mean and corresponding standard errors for all other outcomes[41].

The point estimates for the effect of having more choice among schools on the cognitive score are very small for all cohorts and none of them is significantly different from zero. As mentioned before, although the draft is still mandatory, in practice it has become more voluntary over time, and among the younger cohorts, there is an increasing share of men who did not take the test, which raises issues of selection problems for this variable. Section 8.1.6 in the appendix shows that the selection into taking the test is slightly related to our choice measure, but the correlation is very small.

Column three shows marginal effects at the mean for the probability of having a university degree at age 25. For the youngest cohorts, which is the only one for which we find a marginal effect that is significantly different from zero, we estimate an increase of 0.14 percentage points in the probability of having a university degree for each extra school within the commuting distance. This is again a very small effect. However, it shall be noted that the placebo-test for this outcome fails; students born in years 1975-1976 had a disadvantage from having more schools nearby, compared to those born between 1972-1974[42]. This cannot be attributed to the reform as the reform had not been enacted yet while cohorts 1975-1976 where in compulsory education. Since this violates our identifying assumption, the marginal effects for this outcome cannot easily be interpreted, even though the negative placebo effect suggests that the found effect may be a lower bound. Also for the outcome "being employed at age 25", the estimates indicate a placebo effect, although again very small. With a marginal effect of 0.12, it is positive and in the range of the marginal effects we find for the

[40] Column one repeats the results for the percentile rank in order to ease comparability. The military test score in column two is a continuous outcome that varies between one and nine. All other outcomes are binary and denote the probability of a certain outcome being true.

[41] See Table 18-Table 20 for coefficient estimates of the Probit models, and coefficients and marginal effects for the placebo-specifications and specifications allowing for a pre-reform trend.

[42] The estimated marginal effect for cohorts 1975-1976 is -0.12 percentage points for each additional school, so again, a very small effect. See Table 19 for detailed results.

youngest cohorts; that is an increase in the probability of being employed by 0.07 percentage points. Even though the placebo test fails, we see that both estimates are very small, which indicates that there is no notable effect on employment at age 25.

We find nearly zero effects for the probability of ever having been convicted for a criminal offence (largest estimate is -0.07 percentage points per extra school, see column five) or having serious health problems at age 22 (only significant above the 95 per cent level for the youngest cohort, with a marginal effect of 0.026 percentage points per extra school, see column six).

Summing up, our main results, using the number of schools within the median commuting distance of the home municipality in 1991, show that more choice leads to marginally higher grades at the end of 9th grade but does not seem to have affected our long-term outcomes in an economically significant way (keeping in mind the identification problems for some of the long-term outcomes).[43] Although we cannot exclude possible grade inflation, additional empirical analysis suggests that it cannot be the main explanation.[44] Hence, choice seems to have (very) small effects on grades, but these fade out as the children grow older.

6.2 Alternative measure of choice opportunities

Which radius to take into account, when assessing choice opportunities for students is, as previously discussed, not a priori obvious. This section presents how the magnitude of the results differs with respect to the chosen radius.

Using the median commuting distance of a municipality as the relevant choice area has the advantage of automatically adjusting the radius to the local situation, but it is less transparent and harder to understand than a fixed radius. As an alternative measure, we use a radius of 2 km which is easier to relate to and always within close reach of students' home. The disadvantage of this constant radius is that it does not even comprise the nearest school for many children who live in rather rural areas and may

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[43] Additional empirical analysis suggests that the positive and small effects of the choice index on the short term outcomes are limited to more urban regions, and to municipalities that have been more active in promoting school choice. See Section 8.1.4 in the appendix for details.

[44] Available data on 9th grade standardised test scores in English, Swedish and Maths, taken in years 2005–2008, have enabled us to partially test for different degrees of grade inflation between high and low choice areas. The results

therefore give a too crude picture of the degree of choice in these areas.[45] At the same time, the 2 km radius might still be too large to distinguish between choice and no-choice areas in larger cities. Nevertheless, it illustrates how the size of the estimated effect varies with the chosen measure[46].

Table 6 presents the results from this exercise, showing only the preferred specification that models the treatment effect as a piecewise constant function of cohorts of Equation (3)[47]. Starting with the effect on the percentile rank in GPA 9, the point estimate on having one more school to choose from within 2 km is now negative but insignificant, or marginally insignificant, for all cohorts that were already in compulsory school when the reform was enacted. For the youngest cohorts, i.e. those born between 1988 and 1990, having one more school within a 2 km radius of their place of residence in 1991 significantly increases the percentile rank by 0.298. This effect is larger than the increase caused by an additional school in the median commuting distance. However, taking the observed variation into account, an increase in this choice measure by one standard deviation, that is 1.69 schools, amounts to a 0.5 percentile rank improvement in the GPA, i.e. rather close to what we find using the median commuting distance as radius.

Turning to the effect on cognitive skills as measured by the military draft test score, we can see that one more school within 2 km distance raises the cognitive score by about 0.01 to 0.02 points for the younger cohorts. With a standard deviation in the outcome of 1.9, this amounts to a very small effect of 0.5-1% of a standard deviation. So again, even though we now find a significant effect, economically, the effect is small. The pattern is similar for the other outcomes: the point estimates are larger when using this measure, but economically, they are still small.

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reported in Section 8.1.5 in the appendix suggest that there is some grade inflation related to having more schools to choose from, but that its effect on our estimates is probably small.

[45] In terms of the exogenous pre-reform measures, 68% of the individuals in the sample have no or only one school offering grades 7-9 within 2 kilometres around their home. This share reduces to 45% when using the median commuting distance of the municipality instead.

[46] Results using a radius of 3km, 4km or 5km lie within the region spanned by results using 2 kilometres and the commuting distance (which is about 5km on average).

[47] See Table 21-Table 23 for coefficient estimates of the Probit models, and coefficients and marginal effects for the placebo-specifications and specifications allowing for a pre-reform trend.

As in Table 5, we find that for the outcomes “university degree at 25” and “employed at 25” there are some unresolved identification problems, as indicated by the significant placebo tests[48], which is why the results for these variables shall be interpreted with caution. Nevertheless, estimated marginal effects are below 0.6 percentage points for each additional school within 2km for both outcomes, and thus, again, economically very small. We do not find any significant effects on the probability of having been convicted for a criminal offence until age 22 or having health problems at that age.

Summing up, we find that, qualitatively, the results are robust to using a different radius to approximate the area in which parents may consider choosing schools.

6.3 Disentangling the effects of choice and competition

As discussed in Section 3, the school choice reform might have affected student outcomes through various channels. On the one hand, students have more choice, and on the other hand, schools may compete to attract students. In this section, we try to distinguish these two mechanisms – competition and choice – by adding a competition regressor to the estimations.

In order to measure possible effects of *competition*, we calculate, for each school, the competitive pressure it experiences by taking into account the number of nearby schools as well as the size of their student bodies. The exact formula for the competition measure $comp$ of school j is:

$$comp_j = \sum_{k=1}^{K} \frac{1}{dist_{k,j}} \cdot size_k \quad , \tag{5}$$

where $k = 1,...,K$ indexes potential competitors within a radius of 100 km, $dist_{k,j}$ is the distance between school j and school k and $size_k$ is the number of students visiting school k. We will also use alternative measures of competition where we simply count the number of schools within a certain distance around the school. As our analysis is on

[48] When estimating the probability of having received a university degree until the age of 25, we left out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not. The only difference is that the placebo test for the outcome “university degree at age 25” is not

the individual and not on the school level, we assign each student the competition measure of the school she attends in 9th grade, or, for post-reform cohorts, of the closest school offering grades 7–9 measured in year 1991[49]. We note that the appropriate geographical range for the definition of competition is clearly ambiguous since competitive pressure might be felt at different levels. At first sight, it appears that the headmaster should mostly be concerned about being compared to her immediate neighbours. In addition, though, there is a competition for good teachers, particularly when larger numbers of new private schools opened up who need to hire teachers. Yet, teacher labour markets certainly pass beyond the immediate vicinity. We therefore also examined various other definitions of competition in our robustness analyses.

The measures for school competition and school choice are related but still have a number of distinct features. First, a student's choice options are not directly influenced by the relative size of the schools she might attend, while the competitive pressure a school (director) faces from a neighbouring school is strongly influenced by the capacity of the competing school to take up a significant share of its own students. Second, only schools that offer the grade level a student plans to attend in the following school year are relevant for her choice, while schools will also feel competitive pressure from schools that offer other common grade levels. Nevertheless, the measures for school competition and school choice capture similar phenomena and are therefore highly correlated.[50]Since it is not obvious which is the appropriate radius for measuring choice and competition, we will present results for different definitions of choice and competition in order to gauge the sensitivity of the results.[51] Table 8 shows the effect of choice and competition at grade level 7-9 on the percentile rank in GPA in grade 9 for different combinations of our choice and competition measures, when including both measures in the regression. The first two rows of the table display which measure was

significant when excluding household income and its squared term. We are therefore careful in our interpretation of these results.

[49] The mean and standard deviation for this measure for cohorts that were not affected by the reform are 7.13 and 11.24 respectively. For affected cohorts, i.e. those born between 1977 and 1990, the mean is 14.6 and the standard deviation 17.44, and thus a lot higher than for pre-reform cohorts. This difference is however mostly explained by the 1994 law changed that allowed to choose also among public schools in municipalities other than the one of residence,

[50]See Table 7.

[51] It should also be kept in mind that assigning a competition measure to a student is less accurate than assigning a choice measure as we cannot use the actual school the student went to for affected cohorts.

used for choice and which one for competition. In column one, choice is defined as the number of schools within the median commuting distance and competition as the number of schools within a 100km radius, weighted by their distance and student body size (see Equation (5) for the mathematical formula). Columns two to five show the results for all combinations of choice and competition measures that count the number of schools within a 2km and the median commuting distance radius[52].

The overall pattern that emerges from this exercise is that the positive but small effect of choice that was found in the baseline analysis is in most specifications robust to adding our measures of competition. The size of the choice effects is fairly similar to the baseline result: whereas they vary between the specifications in the table, they are always small, and are often positive and statistically significant.

On the contrary, the effect of the nearest school facing a lot of competitive pressure is negative and significant for the older cohorts. However, it gets more positive for the youngest cohorts and, depending on the choice measure used, even reaches positive significance for the youngest cohorts. Again, all effects are economically small. One interpretation of these results is that the competitive pressure might at first have shaken up the system and caused disruptions[53], resulting in a lower school quality, but that this effect faded out as schools learned to adjust to the new situation. However, as the results on the competition effect depend on the combination of choice and competition measures included in the estimation, and as both measures are highly correlated, it seems difficult to reliably separate between the two effects.

6.4 Effects of time-varying post-reform measures of choice

In our empirical analysis so far, we examined the impacts of choice and competition based on the pre-determined location of schools and individuals since these were plausibly not affected by the choice reform.

It is likely that the reform itself started an endogenous process of school development, where some schools, particularly non-public schools, started up, and others were closed. This process may also have affected families' choices of where to

[52] We do not show the combination: choice 2km with competition 100km since the geographical reach of these measures is too different.

live. In order to shed light on these processes, we will study the relation between the pre-determined and the actual availability of schools in this section. Moreover, we will re-run the baseline regression equation using the actual locations of residence and schools to measure choice and competition instead of the pre-reform measures. This allows us to specifically take into account choice opportunities among public and private schools separately. Even though these results will be only suggestive due to the endogenous location choices, they are still interesting since we can already control for many individual- and region-level covariates.

We will label definitions of choice based on the location of schools and individuals before the reform as *pre-reform* measures, whereas the *post-reform* measures (also called *actual* measures in the below sections) will refer to the location at the time when a student potentially chooses school, i.e. when starting grade 7, in our analysis. For example, for students entering first grade in 1995, the post-reform measure will be calculated based on the locations in 1995, for those entering in 1996, the locations in 1996 will be used, etcetera.

6.4.1 Number of schools before and after the reform

We start by exploring how the pre-reform and post-reform choice measures differ. The aim is to achieve a better understanding of how the pre-reform situation is related to the choice situation that evolves after the reform. Specifically, we will test if the change over time in the number of available schools as measured by our choice-index is correlated with the choice-index at the time the reform was implemented.

In order to do so, we take the difference between the post-reform and the pre-reform choice measures, and regress this difference on the pre-reform choice measure and on a linear trend that is interacted with the pre-reform choice measure.[54] This specification will show how the change in the number of available schools over time is correlated with the initial choice situation that a student faced in 1991. In a second specification, we add all control variables used in the main estimations and, additionally, the parish-average 9th grade percentile rank of cohort 1972, i.e. of the first cohort for which we

[53] See Waslander, Pater and van der Weide (2010) for a related case study on Stockholm.

have information on educational outcomes and who finished 9th grade in 1988, four years before the choice reform.[55] The idea of controlling for the latter variable is to explore whether the difference in the number of schools is correlated with previous educational outcomes. Keeping the notation used in Section 5, the estimation equation for the second specification, including all covariates, reads as follows:

$$(6)\quad c_i^{post-reform} - c_i^{pre-reform} = \alpha \cdot c_i^{pre-reform} + \beta \cdot c_i^{pre-reform} \cdot t_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i$$

where t_i = year of birth – 1972.

Table 9 shows the results of the estimations. We focus on the choice measure "number of schools within median commuting distance" and run separate regressions looking at all schools, only public schools, and only private schools, respectively.[56] Note that the difference between the pre- and post-reform measures is due both to schools opening up and closing down, and to students moving. For the private schools, the difference between the pre-reform and the post-reform measure reflects the growth of the private school sector after the reform as there were almost no private schools in Sweden before the reform.

The upper panel of Table 9 shows the coefficients on the pre-reform choice measure and its interaction with a trend. The resulting marginal effects for each treatment window cohort group are displayed in the lower panel. Note that these are averages of the cohort-specific marginal effects of all cohorts in the respective treatment window. A cohort-specific marginal effect is computed by adding the base coefficient to the product of the interaction coefficient and the value of the trend variable for the specific cohort. Columns two, four and six include additional covariates.

Focusing first on the marginal effects when no additional covariates are included, that is columns one, three and five in the lower panel, we see that having more schools around before the reform is associated with a more positive difference between the actual and the pre-reform number of overall, public and private schools for all cohorts

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[54] The estimations include only data on individuals that started grade seven in or after the year 1992, i.e. birth cohorts 1979-1990, as these are the ones for which we use the pre-reform measures in the main estimations.
[55] As in the previous estimations, all control variables are again based on the pre-reform location of residence.
[56] Note that we always use the median commuting distance *before* the reform, i.e. the range of the geographical area considered is not permitted to be endogenously changed by the reform.

except for those in the first treatment window, cohorts 1979–1981. This holds both when we pool the public and private schools, and when we run separate regressions. However, controlling for all covariates and municipality and cohort fixed effects that are also included in the main estimation, we estimate negative marginal effects for all but the youngest three cohorts when we use only the public schools. The estimates on the increase in private schools are mostly unchanged, resulting in somewhat lower correlations between the pre-reform and actual measure when we pool both types of schools.

Since these effects are a combination of schools opening and closing, and families moving homes, it is hard to interpret this in terms of school openings only, especially for the public schools[57]. However, an important fact to note for the interpretation of our results is that the pre-reform measures of choice and competition are positively correlated with their post-reform counterparts in levels, and that having a higher pre-reform choice measure is positively related to the increase in the number of available private schools. This means that our pre-reform measure also captures the opening up of private schools after the reform, and that this dynamic process is thus included in our estimated effects.

Table 9 also reports the effect of the parish level average of the percentile rank in 9th grade GPA of students born in 1972 (that is the class finishing 9th grade in 1988) on the difference between pre- and post-reform choice measures (see the upper panel). It is always negative, though very small and mostly statistically insignificant, suggesting that, conditional on parental and parish level characteristics such as income and

[57] To illustrate this, imagine for example a student who will start 7th grade in the year 2000. In 1991, when we measure the pre-reform choice value, her parents might not have thought much about schooling yet, and may thus live in a region that has relatively few schools. By 2000, they may have moved to a region that allows their child to attend a school nearby. So we see an increase between the post- and the pre-reform measure. Another family in the same situation may be living in an area with many schools since long, so once their child starts school they do not need to move, and the difference between their pre-reform measure and the actual measure is zero if no school has opened up or closed. So without any new schools opening up, we see that the difference between the actual measure and the pre-reform measure is negatively related to having many schools nearby before the reform. The second mechanism is the opening up of new schools. Ignoring any moves by families, having more schools nearby before the reform may mean that fewer schools open up in the same area after the reform, and there may even be more potential for schools closing down. Again, this results in a negative correlation between the difference in the actual and the pre-reform choice measure and the number of schools nearby before the reform.

education, on average private schools do not seem to sort into areas based on student grades.[58]

6.4.2 Effects of choice when using the actual choice opportunities at 7th grade

The previous section indicated that, as expected, the choice-index changes somewhat over time. In this section we therefore show the results of our main regression specification when we, instead of using the pre-reform measures, use the actual measures of choice (that is, measured at the time a student starts grade 7). There are a few issues that are worth discussing before we turn to the results. Using the actual measures instead of the pre-reform counterparts means making a different set of assumptions and also gives estimates that have a different interpretation.

First, under the assumption that schools opened up randomly and parents did not move in reaction to the reform, using the actual choice-measure would estimate the pure effect of having more schools to choose from when entering grade 7. This is a different effect from the one we estimate when we use the pre-reform measures, which includes all dynamic processes (including subsequent changes in the number of schools and residential location) associated with having many schools nearby at the time of the implementation of the reform[59].

Second, however, as discussed in Section 5.1, there are reasons to believe that those assumptions, i.e. that schools opened up randomly and parents did not move in reaction to the reform, might not hold[60]. By including our extensive set of control variables on regional and individual characteristics we may be able to reduce this endogeneity problem to some extent, but we cannot be certain that it is fully eliminated.

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[58] Under the assumption that student grades are a valid indicator of ability, this can be generalised to indicating that the choice of location of private schools is not endogenous with respect to student ability.

[59] Another caveat is that, when we measure the number of schools contemporaneously, we do not know at which point we are in the equilibrium process of schools opening up and closing in response to parental/student demand. It could for example be that having more schools nearby in period t leads to the bad schools closing down or being overtaken in period $t+1$, which leads to having only few schools nearby in period $t+2$. If the market worked perfectly, the schools remaining would be the very good schools, which would lead us to observe a positive association between few schools (low measures of choice) and good outcomes. A low choice index in $t+2$ could hence actually be the result of a highly competitive process. Thus, estimating the effect of more choice and competition in a dynamically evolving environment poses identification problems of a new nature.

[60] For example, schools that work for profit will probably have chosen the location of business such as to maximise profits, and parents who are concerned about their children's outcomes are more likely to move to areas that will increase the chances of educational success.

Keeping this in mind, it is still interesting to analyse the association between the number of public and private schools nearby and student outcomes at the time when a choice is made. The first column of Table 10 presents the effect of the actual number of schools in the median commuting distance near a student's home offering grades 7-9 on the percentile rank in GPA 9 (estimated according to Equation (3)). Rather surprisingly, we see that having more schools nearby at the time of making a decision has no significant positive effect for most cohorts, but even a small negative one for birth cohorts 1982-1984. Column three shows that using the number of schools within 2km as a choice measure leads to similar conclusions.

In order to obtain a better understanding of this result, we calculated the choice measures for public and private schools separately. Column two and four show results from estimations including these two measures and their interaction terms with treatment window dummies. For the radius "median commuting distance", the coefficients on the effect of having more public schools to choose from are always small and negative but only statistically significant at the 90% level for cohorts 1982-1984. However, using a 2km radius instead, we find small but significant negative effects of having more public schools nearby on the percentile rank in marks. At the same time, the point estimates on the number of private schools are mostly positive but again very small, economically zero, and almost never statistically significant.

Table 11 shows the results for the outcome "cognitive skills at age 18" which is estimated for men only. Also in this case the specifications using the choice measure based on the median commuting distance yields very small, basically zero, marginal effects. Using instead the 2km-choice radius yields somewhat larger effects, which are negative for the private but positive for the public schools.

In sum, controlling for our broad set of individual and region level covariates, we only find small and often insignificant associations between school choice at the time the individuals make their choice, both concerning public and private schools, and student outcomes.

7 Conclusion

In this paper, we analyse the effects of choice and competition caused by the introduction of the Swedish school choice reform in 1992. We find that more school choice, measured by having more schools nearby right before the reform, has small positive effects on marks at the end of compulsory schooling, and, depending on the choice measure used, very small effects on cognitive skills at age 18. We also analyse longer term outcomes such as university education, employment, criminal activity and health, and sometimes find small, but no economically relevant positive effects in these dimensions. Additional analyses to disentangle the effects of choice and competition suggest that competition, as opposed to choice, may have had small negative effects on marks right after the introduction of the reform, though these mostly fade out over time and are no longer present for the youngest cohorts in our sample. Even though we use different methods and identify a slightly different effect than previous studies on the Swedish choice reform, we come to a similar conclusion, namely that the choice reforms did not lead to large changes in average student outcomes, especially in the long run.

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8 Appendix

8.1 Additional information and analyses

This section presents more detailed information on the Swedish school choice, other education-related reforms and additional analyses.

8.1.1 School choice in practice

For our study, it is important to know to what extent the reforms actually affected school choice as perceived by parents and students. This section aims to shed light on this issue.

The school choice reforms implemented in the early 1990s give quite some leeway for interpretation for the municipalities, which are in charge of providing compulsory schooling. Sweden’s 290 municipalities vary a lot in size, from the small rural municipalities with a few thousand inhabitants and few schools, to the large densely populated urban municipalities with several hundred thousand inhabitants and many schools. It is hence likely that the practical implementation of the reforms differed between municipalities.

While information on the share of students opting for private schools is readily available, there exists relatively little information on the amount of active school choice taking place between public schools, especially for the early years after the reforms. Two surveys from the National Board of Education however provide information on the situation during school years 1994/95 and 2000/01.[61]

The 1994/95 survey, which contains information from the local authorities in ten large and predominantly urban municipalities[62], reports that seven per cent of the students switched to another school than the one they were assigned to before the start of the school year 1994/95. Out of these seven per cent, two per cent switched to a

[61] See The National Board of Education 1996 and 2003.

[62] The surveyed municipalities are: Stockholm, Göteborg, Malmö, Uppsala, Linköping, Helsingborg, Södertälje, Botkyrka, Täby and Östersund. The study also contains case studies of 38 schools, out of which eight were private schools, in 12 municipalities.

private school and five per cent to a public school, and most of the changes took place between grade 3-4 and grade 6-7. In the country as a whole, only 1.5 per cent of students chose another public school than the nearest one, while 1.8 per cent of all students attended private schools.[63] It hence seems that by the mid-nineties, a fairly small share of students chose another school than the one assigned.

By 2000/01, making an active school choice had become much more common. The report by the National Board of Education contains information from a survey to parents in six large urban municipalities where the scope for choice is deemed to be large[64], and in a set of smaller and more rural municipalities.[65] In the survey, 67 per cent of parents in the "high-choice municipalities" and 34 per cent of parents in the "low-choice municipalities" state that they have made an active school choice. About two thirds of these, for both sets of municipalities, were, however, choices to the nearest public school, to which the student was assigned anyway. For the remaining third of those who had made an active choice, again for both sets of municipalities, choosing another public school than the nearest one was a bit more common than choosing a private school. A small share of parents, 1–3 per cent, furthermore states that they made an active choice, but that they were not accepted due to lack of slots. It hence seems that the preferences of parents could be satisfied in the majority of cases. These figures suggest that by 2001, school choice was relatively common, but that there were large differences between municipalities.[66]

The 2003 report also collected information from the local authorities in all municipalities and town districts.[67] According to the estimates of the local authorities, reported by the survey, in school year 2000/01, almost a quarter of all students lived in municipalities and town districts where five per cent or more students attend another public school than the one in their catchment area, and five per cent of students lived in

[63] See p. 57 The National Board of Education (1996) for public schools, and, for private schools, the website of The National Board of Education: Table 1.1.A on http://www.skolverket.se/statistik_och_analys/.
[64] These municipalities are large and urban, and were also covered in the 1996 report, see The National Board of Education (1996).
[65] The high-choice municipalities" are: Botkyrka, Stockholm, Södertälje, Uppsala, Helsingborg and Västerås. The survey info for the "low-choice municipalities" was gathered for a large set of municipalities – and they are not reported by name in the report.
[66] Source: The National Board of Education (2003), pp. 48f.

municipalities and town districts where this share was 15 per cent or higher.[68] Regarding the private schools, the National Board of Education (2005) reports that the number of private compulsory schools nationwide grew from a bit over 200 in 1995 to over 500 in 2005. In 2002 the municipality-wise share of students that attended a private school was on average 5 per cent, and was considerably higher, 12 per cent, among the large cities.[69]

The 2003 survey from the National Board of Education also suggests that, by 2000/01, a bit less than 30 per cent of all municipalities and town districts[70], predominantly in urban areas, have a policy to encourage parents/students to make an active school choice, and almost 40 per cent provide parents/students with comprehensive information about the schools available in the municipality. About a quarter of the municipalities and town districts state that they provide school transport also to other schools than the nearest one.

About half of the parents in the 2003-survey also report that they had enough knowledge to make a well-informed choice.

We conclude that school choice has become increasingly common during the almost 20 years that have passed since the choice-reforms of the early 90s, and parents/students choose both to attend another public school than the nearest one and to attend private schools.

8.1.2 Other education-related reforms

The school choice reforms in the early 1990s were not the only education-related changes taking place in the 1990s, but they were part of a broad decentralisation and choice-enhancing trend in the organisation of the educational sector, as well as in the public sector in general. This section gives an overview of the other reforms that took place in the 1990s. This is useful both in order to provide a deeper understanding for the

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[67] The larger municipalities are in general divided into town districts, which are responsible for some of the operations of the public sector.

[68] These figures were calculated using the raw data from the survey to the local authorities, which we were generously given access to from the National Board of Education. See also Table 3.8, The National Board of Education (2003)

[69] See the National Board of Education website for education statistics http://www.jmftal.artisan.se.

[70] The survey was directed to officials of the municipalities, or, in the case of larger municipalities which are divided into town districts, officials of the town districts.

environment in which the school choice reforms took place, and in order to discuss other changes that took place in relation to our evaluation method.

One of the major education-related reforms of the 1990s, apart from the choice-reforms, was the 1991 decentralisation of the Swedish compulsory school system.[71] The reform changed the role of the central government from providing detailed regulation for the municipalities and schools to follow, to specifying broad goals on what the students should know at each completed level of education but by large leaving it to the schools and municipalities to decide how to achieve these goals. The evaluation of whether the schools meet the goals specified in the Law and National curriculum was, until the establishment of the Swedish Schools Inspectorate in 2008, by large left to the schools themselves.[72] [73]

After the reform, municipalities and the individual schools were thus given considerable freedom to design the education, as long as they follow the basic curriculum.[74] The reform also made teachers and headmasters, previously state-employed, employees of the municipalities.[75]

Another part of the decentralisation trend of the early 90s was the replacement of the previously earmarked central government grants[76] with a system of general grants in 1991. At first, the grants were sector-specific, but in 1993, the grants were made completely general. Through the reform, the local politicians were hence given more decision power over the use of central government grants, both in terms of how much to allocate to education per se and how much to allocate to different education-related items.

[71] See Proposition 1990/91:18, SOU 2008:8 (p. 49f), and von Greiff (2009).
[72] National standardised grade 9 tests were in addition made mandatory in 2003, and in 2009 for grades 3 and 5. Previously, standardised tests were available but were up to the schools to use or not.
[73] See proposition 2008/09:87, SOU 2007:28, SOU 2007:101, and Björklund et al (2004).
[74] The school law (1985:1100) names the overall goals for the education system, as well as overall guidelines for the overall design of the education. It specifies the minimum requirements that the schools need to fulfil, such as how much time should be devoted to each subject.
[75] At first, teacher pay negotiations remained centralised, but in 1995, the responsibility for the negotiations was transferred to the school level, and many schools adopted partly individualised wage schemes (Björklund, Clark, Edin, Fredriksson and Krueger (2005)).
[76] Until 1991, central government grants were earmarked for specific educational expenses. These grants were to cover for expenses that were directly related to actual teaching, with teacher salaries being the largest post. According to von Greiff (2009), the system for the allocation of the central government grants was very complex and non-transparent. The municipalities were responsible for financing, through income tax revenues, facilities, school food,

In addition to the reforms described above, there are a couple of changes in the economic regulations for municipalities that took place during the 90s merit mentioning. First, during 1991–1994, municipalities were prevented from raising the local income tax, which constitutes their main source of income. Second, the rules for municipal budget balance have changed over time; the requirement for local budget discipline was relaxed in 1992 in order to become stricter again in 1997.[77]

We can conclude that there is a set of other reforms that are related to the education sector during the period under study. In which sense are they relevant for our study?

First, the decentralisation reforms gave the municipalities more say in how to organise compulsory education and how to allocate resources, while the tax rate cap and the stricter budget discipline are likely to have contributed to making the local education budget more sensitive to the local economic development. One can suspect that this may have given rise to larger variation in the education policy between the municipalities, but there is little guidance available from previous studies on whether this actually happened.[78] Second, the decentralisation reforms mean that schools have more freedom to choose pedagogical style and curriculum, and potentially also over the local budget process.[79]

8.1.3 Moving patterns

Figure 3 and Figure 4 show the municipality average for the share of households that moved during the year, separately for households in which the oldest child was aged 0–3, 4–6 etcetera, and in which there were no children aged 0–17[80]. Moving is defined as either moving into the municipality or moving between parishes within the

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school transport, school medical care and teaching material. In addition, they were free to add to the central government transfers for all posts except for the teacher salaries.

[77] In 1992, the previous requirement of yearly balanced municipal budgets was changed to a more general statement that the local economy should follow “good economic housekeeping” (the Swedish term is: “god ekonomisk hushållning”). In 1997, the municipality law again was made stricter, stating that at the latest in 2000 all municipalities should follow a balanced budget over a 3-year period.

[78] In fact, the only evaluation study that we are aware of, Ahlin and Mörk (2008), find some evidence of a less disperse distribution of education resources (measured as per student costs and teacher density) between municipalities after the decentralisation reform, and find no correlation between the municipal tax base and per student school resources (excluding costs for facilities), neither before nor after the reforms.

[79] See p. 21 in The National Board of Education (2009).

[80] This analysis was conducted on data that was generously made available from the Institute for Labour Market Policy Evaluation (IFAU). These data contain indicators for number of children living in the household, in the age spans 0–3, 4–6, 7–10, 11–15 and 16–17.

municipality. If Tiebout migration (in terms of moving in order to get into a good school) was affected by the school choice reform, we would expect to see a different moving pattern after the reform for households with school aged children, and probably especially so for households in which the oldest child is about to enter school. Unfortunately, we can only observe the migration patterns for households with and without children starting from year 1991, that is only one year before the reform, which is why it is hard to draw too much on the pre-reform moving patterns from the figures. Still, if Tiebout migration changes in response to the reform, it is likely that the moving pattern changes gradually as there is evidence that the impact of the reform was also gradual (see Section 6.1). Section 8.1.4 also suggests that school choice is more common in more urban, more densely populated, areas. We therefore show the moving patterns separately for urban and small/rural municipalities. While the share that move varies a bit over time, there is no clear indication in that the households for whom Tiebout migration can be expected to be relevant, i.e. those with children who are just about to start school (i.e. children aged 4–6), or children who have just started school (children aged 7–10) have changed their moving pattern after the choice reform. The figures neither suggest that households with school-aged children in urban municipalities have become relatively less likely to move, compared to the small and rural municipalities, which would be expected if Tiebout migration decreased as a result of increased options to choose school without moving.

Figure 3: Share moving to the municipality or between parishes within the municipality, average for urban municipalities, for households with the oldest child in different age spans

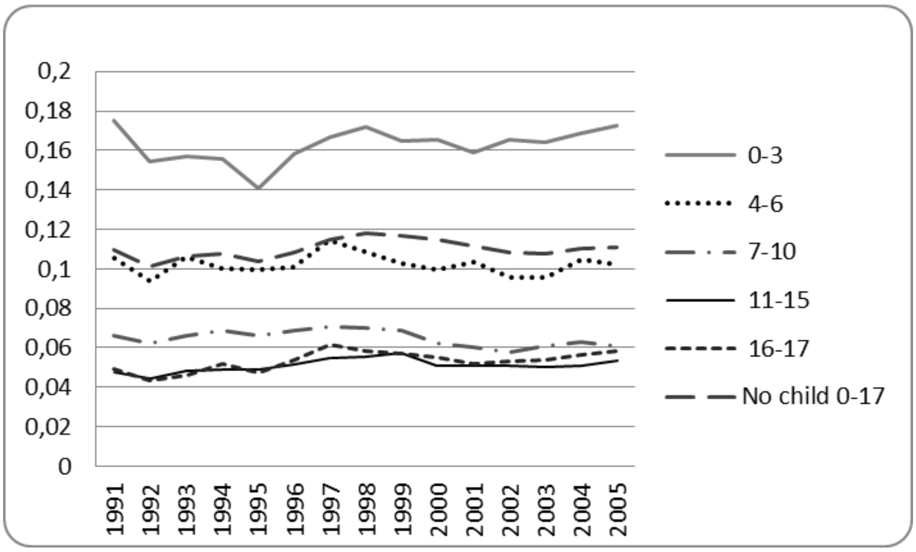
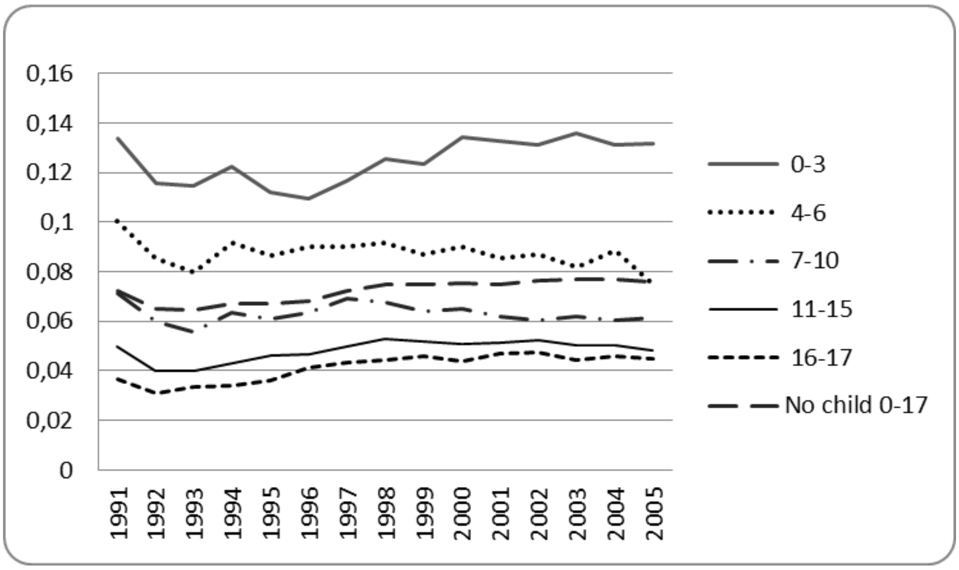


Figure 4: Share moving to the municipality or between parishes within the municipality, average for smaller/rural municipalities, for households with the oldest child in different age spans



8.1.4 Heterogeneity of effects with respect to region

The analyses in Section 6 explored the average effect of choice and competition introduced by the choice reform. However, it is possible that different types of municipalities have had different policies on school choice, which means that the effects may vary across municipalities. In order to address this issue, in this section, we re-estimate our specifications for 9th grade GPA and military draft test score, when we divide municipalities along the following two dimensions.

First, we analyse the effects separately for individuals living in urban and non-urban municipalities before the reform[81]. The reason for this division is that school choice is likely to be more of an urban phenomenon due to, for example, the higher population density and easier transportation in urban areas.

Second, even for municipalities in the more urban areas, the impact of the school choice reform is likely to differ due to differences in local policy. In order to take this into account, we focus on the municipalities in the county of Stockholm and divide them into municipalities that were early or late adopters of school choice, in terms of actively facilitating and encouraging residents to make active school choices.[82] The early adopters consist of those that actively encouraged school choice in the early or mid 1990s, for example through providing information on schools and how to make a choice in practice, or by having clear student-based systems for allocating resources between schools, whereas the late adopters are those where school choice became more of an issue later on in the 2000s.

Table 12 and Table 13 present separate estimations for the two groups of urban and non-urban municipalities, and Table 14 and Table 15 show the corresponding for the two groups of early and late school choice adopters within Stockholm county.

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[81] The classification of municipalities as urban and rural follows the classification of the Swedish Association of Local Authorities, see http://www.skl.se/kommuner_och_landsting/om_kommuner/kommungruppsindelning. This divides municipalities into nine categories (metropolitan, suburban, large cities, commuter, sparsely populated, manufacturing, and other – divided into population >25,000, 12,500-25,000 and <12,500) on the basis of structural parameters such as population, commuting patterns and economic structure as of Jan 1 2005. We classify a municipality as urban if it is thus defined as a city, suburb or “large town”. This results in 68 municipalities being labelled as urban.

[82] In order to make this division of the sample, we contacted the municipalities in the county of Stockholm and asked them if they have a policy to actively facilitate and encourage residents to make an active school choice, and if so, when this policy was implemented. It shall be noted that, even though school choice is today a normal phenomenon in these urban municipalities, it was often not easy to find out exactly when it became common practice. The information gathered is therefore often not very detailed. In addition, we failed to receive answers from 11 out of 26 municipalities, although most of the non-respondents were the smaller municipalities in the county. The respondents were the following municipalities: Danderyd, Tyresö, Sollentuna, Haninge, Nacka, Norrtälje, Vallentuna, Upplands-Bro, Stockholm, Upplands-Väsby, Huddinge, Salem, Ekerö, Botkyrka and Täby. The non-respondents were: Lidingö, Solna, Sundbyberg, Södertälje, Järfälla, Österåker, Värmdö, Nykvarn, Vaxholm, Sigtuna and Nynäshamn. The

Focusing first on the division of municipalities into urban and non-urban, the overall pattern in Table 12 and Table 13 is that school choice is related to more positive outcomes in the urban municipalities, while there is some indication of a negative relation for the non-urban municipalities. Specifically, the regressions on 9th grade GPA (Table 12, Columns 3 and 5) yield positive and statistically significant estimates for the treated cohorts in the urban municipalities, which are similar to the estimates from the pooled baseline specification (see first Columns in Table 5 and Table 6). In contrast, the estimation including only non-urban municipalities yields non-significant effects when the median commuting distance is used, see Column 2, although the empirical identification is problematic for this outcome[83], and negative effects when we use the choice-measure based on the 2-km radius (Table 12, Column 4). With around -0.4 to -0.5 percentile ranks per additional school, the effect is however small, especially when taking into account that the standard deviation of this choice measure in non-urban areas is only 0.85. We conclude that the positive, albeit small, estimated effect of school choice seems to be limited to the urban municipalities, and that there is some, although weak, evidence of negative effects in the non-urban municipalities.

The estimates for the outcome cognitive skills from the military draft test scores in Table 13 show, similarly to the main estimations, little evidence of any significant effect when using the median commuting distance as radius (Columns 1-2), while the radius 2 km yields small positive effects of more choice in both urban and non-urban municipalities, although the estimates are almost only significant for the former.

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following of these were classified as being early adopters of school choice: Tyresö, Stockholm, Vallentuna, Nacka, Danderyd, Täby and Upplands-Bro.

[83] Note that the result in Column 1 suggests statistically significant estimates for the youngest cohorts and those born between 1982 and 1984. However, we also find a significantly negative placebo effect of -0.251 percentile ranks, implying that the negative effects were not necessarily caused by the reform but already there before it was enacted. When we include a pre-reform trend and interact all our treatment windows with the trend (Equation (4)), this placebo-test is no longer significant, though the pre-reform trend also is not. In this specification, shown in the second column, the standard errors are much larger and the point estimates are now positive and no longer significantly different from zero. However, since the pre-reform trend that is estimated on cohorts 1972-1976 is being extrapolated to form a control group as far as 14 years into the future, this specification is very sensitive to the estimated trend. Since the pre-reform trend is not even statistically significant from zero, one should be very cautious in interpreting this result.

Turning to the division between early and late adopters among the municipalities in Stockholm county[84], Table 14 shows that, when we use the choice measure based on median commuting distance (Columns 1–2), students living in the early-choice-adopting municipalities seem to have benefited slightly more in terms of receiving higher grades, while students in the late choice-municipalities sometimes even have a negative effect of having more schools nearby. However, this effect disappears when looking at the measure using a 2km radius: In Columns 3–4, we see that students in both municipalities seem to have benefited from having more schools nearby, even though the standard errors are a bit larger in the smaller sample living in the "late-adopting" municipalities, thus causing the marginal effects to be less often statistically significant.

Table 15 presents results for the outcome cognitive skills. They indicate some significantly negative effects of having more schools nearby in the "late-adopting" municipalities, when we use the larger radius "commuting distance". However, the negative effects and the differences between the two groups vanish when using the 2km radius.

Summarising, we find that having more schools nearby right after the reform seems to have had more positive effects on marks and cognitive skills of students living in urban areas at that time, even though the size of the effect is still small. Looking at Stockholm county only, differences only show up when using the radius "commuting distance" to measure choice but mostly vanish when using the 2km-radius.

8.1.5 Grade Inflation

It is a major concern that grades have been inflated since competition between schools has increased. If parents care about grades, schools have an incentive to give slightly better grades in order to attract more students. The grade point average at the end of 9th grade determines admission into upper secondary schools in Sweden and will thus be an important and observable scholastic output for parents. In addition, standardised national tests, which can be used by teachers as a check for the grade

[84] Naturally, the sample for this analysis is restricted to students living in municipalities that provided information in our survey, in 1991 or the year they make the decision to start 7th grade, if that was before 1991, respectively.

levels, were not mandatory for schools to use during most of the years we study. The schools' need to be attractive for students should be larger in areas with a lot of competition, such that any potential positive effects of more competition on marks cannot easily be disentangled from grade inflation.

How serious is this concern in the Swedish case? Wikström and Wikström (2005), who compare the final grades from upper secondary school in 1997 to the SweSAT national test scores, find no evidence that competition from private schools, as measured on the municipality level, leads to grade inflation. However, they find that the difference between a standardised test and grades at the end of upper secondary school is much larger in independent schools. Vlachos (2010), who uses data on grades and national standardised test scores, finds no indication of different rates of grade inflation between private and public schools overall but finds some evidence of higher grade inflation in private for-profit schools.

The potential link between school competition and grade inflation should still be taken seriously, and in this study we have addressed this by considering also outcomes determined outside of the school, like the cognitive score in the military test and labour market outcomes. However, it could still be that inflated grades permit admission to better high schools which in turn also improves these "real" outcomes. For that reason, we provide another robustness check by using data on student grades from mandatory standardised national tests in English, Swedish and Maths, taken at the end of grade 9, that we have available for the years 2003-2008[85]. We use these data to test if our school choice measures can predict grades even when we control for the test results, something which would indicate that grade inflation is present. However, it is to be kept in mind that this approach has a potential pitfall: if grades measure something different than just performance in tests, then any additional explanatory power of school choice over and above tests could result from choice positively affecting other skills that are necessary to obtain a good grade.[86]

[85] Previously, these tests were voluntary for the schools, and are not centrally available, which is why we cannot use them for all cohorts.

[86] In Sweden, teachers form grades by assessing the class room performance of students. Noncognitive skills, such as pro-social behaviour, patience and the ability to control ones temperament, might be of higher importance in receiving a good grade in the class room than in performing well in a standardised test.

Since we are interested in whether the effects that we capture with our choice measures are driven by grade inflation, we use the exactly same measures as in the main estimations to test for grade inflation here. As we only observe the test scores for 9th graders in the years 2003-2008, we only use cohorts from our sample that were 16 in these years, i.e. cohorts 1987-1990.

We estimate Equation (7), using the percentile rank in GPA in grade 9 ($rank\ GPA_i$) as outcome and controlling for all three test results that we have information on (Swedish, English, Maths) at the same time. The subject-specific grades are given on an ordinal 1–4 scale (no pass, pass, pass with distinction and pass with special distinction). The test grades are included as dummies for each of the $k=4$ pass-categories. We include the same covariates as we did in the main estimation, including the municipality dummies, and our choice measure.

$$(7)\ rank\ GPA_i = \alpha \cdot c_i + \beta_{km} \cdot math9_{ik} + \beta_{ks} \cdot swedish9_{ik} + \beta_{ke} \cdot english9_{ik} + \gamma_{cohort} + \delta \cdot X_i + u_i\ ,$$

The idea is that even though these test results only account for some of the grades that make up the grade point average, finding no effects of our choice measure when we include them could be seen as an indication that grade inflation is probably not correlated with our choice-measure. Table 16 shows the results for the choice measures counting the number of schools within the median commuting distance and 2km around a student's home in columns one and two respectively. We find small statistically significant effects of choice on the grades.[87] However, the coefficients are smaller than the marginal effects from our baseline estimation in Sections 6.1 and 6.2. We can thus conclude that, even if there is some grade inflation related to having more schools to choose from, its effect on our estimates is probably small.

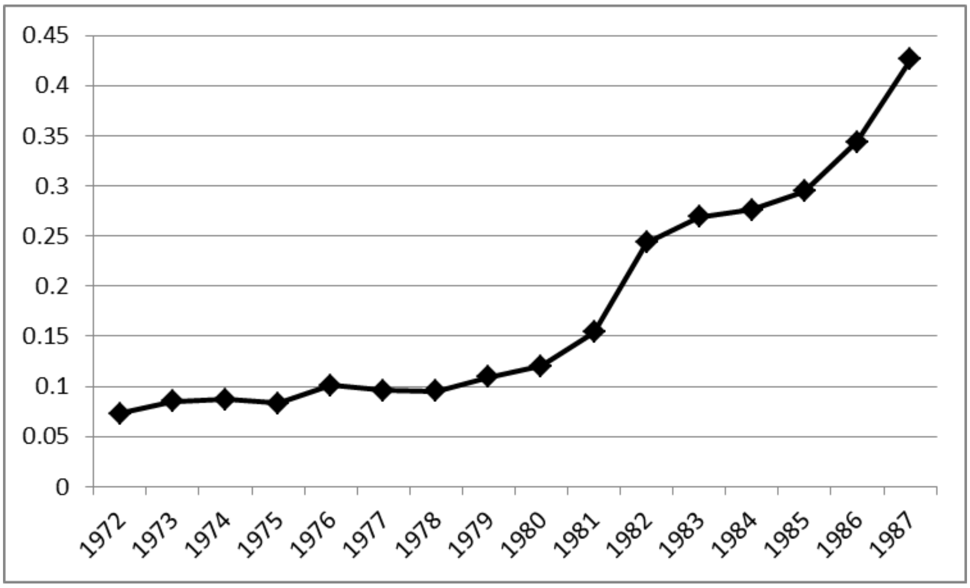
8.1.6 The military score

As mentioned in Section 4, the share of men going through the drafting process declined significantly starting from the late 1990s. This means that for the younger

[87] Note for the interpretation of the coefficients that student test score grades are measured at an ordinal 1–4 scale, while our dependent variable for 9th grade GPA is in terms of percentile rank.

cohorts in our sample, no longer all Swedish men took part in the military test. Figure 5 shows that the share of men not taking part in the draft test rose from around 7 per cent for the cohort born in 1972 to around 43 per cent for cohort 1987. The sharp increase started with men born in the years 1980 and 1981.

Figure 5: Share of men in each cohort for which we do not observe the military test



This raises concerns about potential selection into taking the test and being ready to possibly serve in the military. In order to test whether such selection is likely to bias our results, we run our treatment effect analysis with the same covariates we use for the main estimations on the outcome "not taking part in the test". Results of this analysis are presented in Table 17.

We find that having one more school offering grade 7-9 within the median commuting distance of a student's place of residence in or before 1991 decreases the probability of missing the test by 0.44 percentage points for the cohorts that are most affected by the reform. We find similar results using the number of schools within a 2km radius; however, the placebo-test fails for this specification. Even though this effect is mostly statistically significant, it is relatively small compared to the share of men not taking part in the test in these youngest cohorts, which is 30 to 43 per cent. We thus do not believe that our results on the cognitive skills test score are biased in a quantitatively relevant way.

8.2 Tables

The tables are presented in the following order: First, Section 8.2.1 presents tables on descriptive statistics and estimation results from main analyses. Second, Section 8.2.2 presents tables on additional analyses that are included in Section 8.1 in the Appendix. Lastly, Section 8.2.3 shows, for reporting purpose, more detailed estimation results relating to the analyses in Section 6.1 and 6.2.

8.2.1 Tables from main analyses

Table 1: Descriptive statistics for outcome variables

	PRE-REFORM COHORTS (*cohorts 1972-1976 are not affected*)			POST-REFORM COHORTS (*cohorts 1977-1990 are affected*)		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Percentile rank GPA 9	48.21	28.58	437 953	49.40	28.60	1 277 468
Cognitive score	5.06	1.93	213 145	5.04	1.94	403 161
University degree (at age 25)	0.35	0.48	445 295	0.41	0.49	692 729
Employed (at age 25)	0.71	0.45	446 509	0.69	0.46	698 068
Entry in criminal record (until age 22)	0.16	0.36	449 802	0.14	0.35	990 157
Health problem (at age 22)	0.07	0.26	448 043	0.08	0.27	985 478

Note: Sample contains only observations with full information on all covariates *X* given below Table 4.

Table 2: Descriptive statistics for pre-reform choice measures

	PRE-REFORM COHORTS (*cohorts 1972-1976 are not affected*)				POST-REFORM COHORTS (*cohorts 1977-1990 are affected*)			
	Mean	Std. Dev.	Median	Obs.	Mean	Std. Dev.	Median	Obs.
School choice								
Number of schools within median commuting distance	3.45	4.66	2	449 802	5.91	9.35	2	1 306 879
Number of schools within 2km	1.24	1.50	1	449 802	1.35	1.69	1	1 306 879

Note: The table displays pre-reform measures on grade level 7-9. Sample contains only observations with full information on all covariates *X* given below Table 4.

Table 3: Descriptive statistics on covariates in the estimation

	Mean	Std. Dev.	Median
Municipality level variables			
Population density	392.35	876.36	64.00
Average taxable income in year t-2 in 100 SEK, deflated to 2006	1 079.05	153.39	1 067.89
Urban municipality	0.54	0.50	
Parish level variables			
Share of 16-64 year olds born in Sweden	0.89	0.08	0.92
Average yearly earnings of 20-64 year olds in 100 SEK	1 140.46	224.25	1 150.94
Share of 20-64 year olds with university degree	0.20	0.09	0.18
Share of 20-64 year olds that are employed	0.83	0.04	0.84
Population density of 7-15-year-olds in lower quartile of distribution	0.09	0.28	
Population density of 7-15-year-olds in highest quartile of distribution	0.64	0.48	
Individual level variables			
Household income in 1000 SEK, deflated to 2006	373.77	382.38	350.00
Household received welfare	0.06	0.24	
Age of mother at birth	27.78	5.05	27.00
Single parent household	0.22	0.42	
Number of children	2.23	1.01	2.00
Only child	0.23	0.42	
Child born in Sweden	0.96	0.19	
Mother born in Sweden	0.89	0.32	
Mother born in Scandinavia, outside of Sweden	0.05	0.21	
Mother born in western Europe, North America or Australia	0.01	0.10	
Father born in Sweden	0.88	0.32	
Father born in Scandinavia, outside of Sweden	0.04	0.19	
Father born in western Europe, North America or Australia	0.02	0.13	
Mother has university degree	0.31	0.46	
Mother's highest degree is from secondary education	0.49	0.50	
Father has university degree	0.27	0.44	
Father's highest degree is from secondary education	0.46	0.50	
Number of observations: 1 756 681			

Notes: summary statistics are on individual level, thus, statistics on municipal and parish level variables are weighted with the share of inhabitants. E.g.: this says that 55% per cent of the sample lives in an urban municipality, it does not mean that 55% of municipalities are urban.

Table 4: Results from main estimation for percentile rank in marks in grade 9

Outcome: PercentilePercentile rank GPA Grade 9
Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Constant treatment effect	Piecewise constant treatment effect
Choice × Cohorts 1988-1990		0.130*** (0.0183)
Choice × Cohorts 1985-1987		0.0772*** (0.0186)
Choice × Cohorts 1982-1984		0.0100 (0.0183)
Choice × Cohorts 1979-1981		0.0231 (0.0195)
Choice × Cohorts 1977-1978		-0.0139 (0.0227)
Choice × Cohorts 1977-1990	0.0607*** (0.0168)	
Choice	-0.0195 (0.0172)	-0.0334* (0.0173)
Constant	25.33*** (5.099)	26.95*** (5.055)
Observations	1,715,421	1,715,421
R-squared	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 99, 95 and 90% level is denoted by ***, **, *. The definition of the placebo tests is explained in Section 5.1. The following control variables are included in the estimation:
On the municipality level: population density, taxable income and taxable income squared
On the parish level: share of Swedish citizens among the 16-64-year-old, mean earnings of the 20-64-year-olds, share of university graduates among the 20-64-year-olds, share of employed persons among the 20-64-year-olds, indicator variables for whether the population density of 7-15-year-olds is in the lowest or highest quartile across Sweden
On the individual level: household income and household income squared, whether the household received welfare, age of the mother at birth, whether living in a single parent household, number of children in the household, whether child was only child, whether child has Swedish citizenship, indicator variables on mothers and fathers citizenship separately (Swedish, Nordic (=Norwegian, Finnish, Danish), from other western country(=Western Europe, North America, Australia), rest of the world is base category), indicator variables on whether mother and/or father graduated from university or secondary education

Table 5: Results for later outcomes

Choice Measure: Number of schools in median commuting distance
Grade Level: 7-9

	Percentile Percentile Rank Grade 9	Cognitive Draft Score *(Men only)*	University Degree at Age 25 †	Employed Age 25	Any Crime until Age 22	Health Age 22
Cohorts 1988-1990	0.130***					
rel. to untreated	(0.0183)					
Cohorts 1985-1987	0.0772***	0.00203			-0.000560***	0.000262**
rel. to untreated	(0.0186)	(0.00154)			(0.000190)	(0.000118)
Cohorts 1982-1984	0.0100	0.000503	0.00138***	0.00072***	-0.000680***	0.000131
rel. to untreated	(0.0183)	(0.00155)	(0.000292)	(0.000258)	(0.000195)	(0.000120)
Cohorts 1979-1981	0.0231	-0.000367	0.000183	0.000621**	-0.000577***	0.000252*
rel. to untreated	(0.0195)	(0.00162)	(0.000308)	(0.000282)	(0.000209)	(0.000129)
Cohorts 1977-1978	-0.0139	0.00267	-0.000581	0.000012	-0.000022	0.000201
rel. to untreated	(0.0227)	(0.00213)	(0.000395)	(0.000328)	(0.000278)	(0.000165)
Untreated Cohorts	-0.0334*	0.000143	-0.000191	-0.00121***	0.000744***	-0.000333***
(1972-1976)	(0.0173)	(0.00155)	(0.000290)	(0.000265)	(0.000195)	(0.000117)
Placebo test:	Pass	Pass	Fail	Fail	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,715,421	610,182	1,120,459	1,120,845	1,409,092	1,402,829
R-squared ‡	0.186	0.146	0.126	0.0300	0.0382	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not.

‡Pseudo R-squared for binary outcomes.

Table 6: Estimation results using number of schools within 2 km as distance measure

Choice Measure: Number of schools within 2 km
Grade Level: 7-9

	Percentile Rank Grade 9	Cognitive Score *(Men Only)*	University Degree at Age 25 †	Employed Age 25	Any Crime until Age 22	Health Age 22
Cohorts 1988-1990 *rel. to untreated*	0.298***					
	(0.0570)					
Cohorts 1985-1987 *rel. to untreated*	0.0722	0.0193***			0.000351	0.000690*
	(0.0614)	(0.00507)			(0.000592)	(0.000413)
Cohorts 1982-1984 *rel. to untreated*	-0.115*	0.0103**	0.00565***	0.00229***	0.0000935	-0.000034
	(0.0602)	(0.00519)	(0.00103)	(0.000827)	(0.000628)	(0.000435)
Cohorts 1979-1981 *rel. to untreated*	-0.0729	0.0110**	0.00257**	0.00129	-0.000174	0.000283
	(0.0638)	(0.00534)	(0.00103)	(0.000875)	(0.000658)	(0.000441)
Cohorts 1977-1978 *rel. to untreated*	-0.106	0.0138**	0.000383	0.00141	0.00117	0.000456
	(0.0653)	(0.00615)	(0.00119)	(0.00101)	(0.000784)	(0.000475)
Untreated Cohorts (1972-1976)	0.138***	-0.0122***	0.000259	-0.00549***	0.000524	-0.000465*
	(0.0411)	(0.00371)	(0.000721)	(0.000656)	(0.000464)	(0.000282)
Placebo test	Pass	Pass	Pass †	Fail	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,715,421	610,182	1,120,459	1,120,845	1,409,092	1,402,829
R-squared ‡	0.186	0.146	0.126	0.0301	0.0382	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively almost the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not. The only difference is that the Placebo test does not pass in these cases.

‡Pseudo R-squared for binary outcomes.

Table 7: Correlation between competition and choice measure on grade level 7-9

Choice Measures: Number of schools within radius…

	Radius: Median Commuting Distance	Radius: 2km
Competition Measures		
No. of schools, weighted by distance and size of student body	0.6898	0.5277
No. of schools within median commuting distance	0.9462	0.5016
No. of schools within 2km	0.4482	0.7594

Table 8: Results disentangling effects of choice and competition

Outcome: Percentile rank GPA in Grade 9
Choice Measure: Number of schools within certain radius around students' home
Competition Measure: Number of other schools within certain radius around school
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE			RADIUS: 2KM	
	Weighted by Distance and Student Body Size	Radius: Median Commuting Distance	Radius: 2km	Radius: Median Commuting Distance	Radius: 2km
Choice					
Cohorts 1988-1990 *rel. to untreated*	0.116***	0.237***	0.157***	0.114*	0.236***
	(0.0200)	(0.0330)	(0.0204)	(0.0631)	(0.0751)
Cohorts 1985-1987 *rel. to untreated*	0.0865***	0.212***	0.112***	0.0159	0.106
	(0.0204)	(0.0325)	(0.0208)	(0.0665)	(0.0749)
Cohorts 1982-1984 *rel. to untreated*	0.0315	0.167***	0.0459**	-0.0514	-0.0265
	(0.0200)	(0.0328)	(0.0205)	(0.0667)	(0.0762)
Cohorts 1979-1981 *rel. to untreated*	0.0611***	0.216***	0.0695***	0.0817	0.133*
	(0.0208)	(0.0353)	(0.0212)	(0.0697)	(0.0754)
Cohorts 1977-1978 *rel. to untreated*	0.0343	0.236***	0.0377	0.130*	0.116
	(0.0249)	(0.0416)	(0.0264)	(0.0767)	(0.0819)
Untreated Cohorts (1972-1976)	-0.0492***	-0.179***	-0.0742***	0.112**	0.0342
	(0.0182)	(0.0299)	(0.0196)	(0.0461)	(0.0523)
Competition					
Cohorts 1989-1990 *rel. to untreated*	0.00928	-0.126***	-0.0486	0.0584**	0.0531
	(0.00966)	(0.0388)	(0.0883)	(0.0268)	(0.104)
Cohorts 1986-1988 *rel. to untreated*	-0.0152*	-0.162***	-0.233***	0.0121	-0.135
	(0.00892)	(0.0372)	(0.0876)	(0.0259)	(0.0985)
Cohorts 1983-1985 *rel. to untreated*	-0.0221**	-0.178***	-0.181**	-0.0350	-0.132
	(0.00922)	(0.0375)	(0.0877)	(0.0261)	(0.0993)
Cohorts 1980-1982 *rel. to untreated*	-0.0377***	-0.217***	-0.352***	-0.0676**	-0.373***
	(0.00897)	(0.0387)	(0.0875)	(0.0263)	(0.0980)
Cohorts 1977-1979 *rel. to untreated*	-0.0413***	-0.290***	-0.336***	-0.126***	-0.407***
	(0.00897)	(0.0430)	(0.0904)	(0.0285)	(0.0981)
Untreated Cohorts (1972-1976)	0.0269***	0.179***	0.338***	0.0412	0.250***
	(0.00760)	(0.0352)	(0.0737)	(0.0253)	(0.0804)
Placebo test	Pass	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,688,234	1,688,234	1,688,234	1,688,234	1,688,234
R-squared	0.186	0.186	0.186	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. The combination (choice 2km, competition 100km) not shown because of very different geographical range. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

Table 9: Relation between pre-reform and post-reform number of schools

Outcome: Difference between number of schools before and after the reform
Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	All Schools	All Schools	Public Schools	Public Schools	Private Schools	Private Schools
Coefficients						
Pre-reform No. of schools	-0.534***	-0.638***	-0.161***	-0.217***	-0.373***	-0.420***
	(0.0423)	(0.0318)	(0.0245)	(0.0333)	(0.0261)	(0.0203)
Pre-reform No. of schools ×Linear Trend (cohort-1972)	0.0600***	0.0565***	0.0174***	0.0132***	0.0427***	0.0433***
	(0.00432)	(0.00255)	(0.00282)	(0.00214)	(0.00218)	(0.00145)
Pre-reform parish average percentile rank GPA 9		-0.00587		-0.00424*		-0.00164
		(0.00363)		(0.00234)		(0.00394)
Constant	0.893***	13.60	0.754***	-4.383	0.139**	17.99
	(0.172)	(10.32)	(0.134)	(14.03)	(0.0552)	(13.32)
Marginal Effects†						
Cohorts 1988-1990	0.486***	0.323***	0.134***	0.00757	0.353***	0.316***
	(0.0346)	(0.0410)	(0.0267)	(0.0171)	(0.0158)	(0.0096)
Cohorts 1985-1987	0.306***	0.154***	0.0815***	-0.0321**	0.225***	0.186***
	(0.0227)	(0.0153)	(0.0189)	(0.0156)	(0.0112)	(0.0078)
Cohorts 1982-1984	0.126***	-0.0160	0.0294**	-0.072***	0.0967***	0.0558***
	(0.0132)	(0.0137)	(0.0121)	(0.0167)	(0.00940)	(0.00812)
Cohorts 1979-1981	-0.054***	-0.186***	-0.0226**	-0.112***	-0.031***	-0.074***
	(0.0129)	(0.0160)	(0.00893)	(0.0198)	(0.0117)	(0.0150)
Cohort dummies	No	Yes	No	Yes	No	Yes
Municipality dummies	No	Yes	No	Yes	No	Yes
Full set of covariates	No	Yes	No	Yes	No	Yes
Observations	1,214,130	1,117,774	1,214,130	1,117,774	1,214,130	1,117,774
R-squared	0.207	0.245	0.035	0.083	0.524	0.554

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a list on the full set of covariates see Table 4.

† The marginal effects are averages of the cohort specific marginal effects of all cohorts in the respective treatment window. A cohort specific marginal effect is computed by adding the base coefficient to the product of the interaction coefficient and the value of the trend variable for the specific cohort.

Table 10: Effects of actual choice measures on percentile rank in GPA 9

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Cohorts 1988-1990 *rel. to untreated*	0.0137		-0.0431	
	(0.0185)		(0.0489)	
Cohorts 1985-1987 *rel. to untreated*	-0.00643		-0.0916*	
	(0.0187)		(0.0519)	
Cohorts 1982-1984 *rel. to untreated*	-0.0387**		-0.124**	
	(0.0185)		(0.0533)	
Cohorts 1979-1981 *rel. to untreated*	-0.0145		-0.0417	
	(0.0188)		(0.0584)	
Cohorts 1977-1978 *rel. to untreated*	-0.0184		-0.0596	
	(0.0215)		(0.0624)	
Untreated Cohorts (1972-1976)	0.0190		0.205***	
	(0.0180)		(0.0406)	
Public choice				
Cohorts 1988-1990 *rel. to untreated*		-0.0127		-0.192***
		(0.0264)		(0.0613)
Cohorts 1985-1987 *rel. to untreated*		-0.0230		-0.212***
		(0.0263)		(0.0633)
Cohorts 1982-1984 *rel. to untreated*		-0.0456*		-0.160**
		(0.0252)		(0.0657)
Cohorts 1979-1981 *rel. to untreated*		0.0388		-0.0129
		(0.0245)		(0.0636)
Cohorts 1977-1978 *rel. to untreated*		-0.0255		-0.0661
		(0.0310)		(0.0736)
Untreated Cohorts (1972-1976)		-0.0243		0.197***
		(0.0230)		(0.0442)
Private choice				
Cohorts 1988-1990 *rel. to untreated*		0.0303		0.273
		(0.0700)		(0.175)
Cohorts 1985-1987 *rel. to untreated*		0.0746		0.291
		(0.0753)		(0.189)
Cohorts 1982-1984 *rel. to untreated*		0.0634		0.0200
		(0.0784)		(0.200)
Cohorts 1979-1981 *rel. to untreated*		-0.206**		-0.151
		(0.0881)		(0.236)
Cohorts 1977-1978 *rel. to untreated*		0.0328		-0.0284
		(0.102)		(0.245)

Table 10 continued

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Untreated Cohorts (1972-1976)		0.138**		0.171
		(0.0653)		(0.162)
Constant	19.86***	20.63***	19.04***	20.47***
	(3.176)	(3.134)	(3.157)	(3.148)
Placebo test	Pass	Pass	Pass	Pass
Pre-reform trend	No	No	No	No
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,743,753	1,743,753	1,743,753	1,743,753
R-squared	0.186	0.186	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

Table 11: Effects of actual choice measures on cognitive score

Outcome: Cognitive skills
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Cohorts 1985-1987 *rel. to untreated*	-0.000212		0.00878*	
	(0.00160)		(0.00478)	
Cohorts 1982-1984 *rel. to untreated*	-0.000881		0.00563	
	(0.00159)		(0.00489)	
Cohorts 1979-1981 *rel. to untreated*	-0.00148		0.00814	
	(0.00158)		(0.00514)	
Cohorts 1977-1978 *rel. to untreated*	0.00262		0.0128**	
	(0.00205)		(0.00609)	
Untreated Cohorts (1972-1976)	0.00109		-0.0116***	
	(0.00159)		(0.00375)	
Public choice				
Cohorts 1985-1987 *rel. to untreated*		0.00325		0.0157***
		(0.00228)		(0.00573)
Cohorts 1982-1984 *rel. to untreated*		0.00449**		0.0192***
		(0.00221)		(0.00585)
Cohorts 1979-1981 *rel. to untreated*		0.00191		0.0162***
		(0.00220)		(0.00605)
Cohorts 1977-1978 *rel. to untreated*		0.00281		0.0158**
		(0.00292)		(0.00708)
Untreated Cohorts (1972-1976)		-0.00207		-0.0187***
		(0.00208)		(0.00420)
Private choice				
Cohorts 1985-1987 *rel. to untreated*		-0.0130**		-0.0497***
		(0.00590)		(0.0164)
Cohorts 1982-1984 *rel. to untreated*		-0.0224***		-0.0704***
		(0.00610)		(0.0174)
Cohorts 1979-1981 *rel. to untreated*		-0.0144**		-0.0488**
		(0.00668)		(0.0196)
Cohorts 1977-1978 *rel. to untreated*		0.00230		-0.0140
		(0.00839)		(0.0239)
Untreated Cohorts (1972-1976)		0.0133***		0.0462***
		(0.00493)		(0.0147)
Constant	2.518***	2.457***	2.449***	2.360***
	(0.267)	(0.269)	(0.267)	(0.269)
Placebo test	Pass	Pass	Pass	Pass
Pre-reform trend	No	Yes †	No	No
Observations	615,225	615,225	615,225	615,225
R-squared	0.150	0.150	0.150	0.150

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

8.2.2 Tables relating to analyses presented in the appendix

This section presents tables on additional analyses conducted in Section 8.1 in the appendix.

Table 12: Heterogeneity of effects with respect to urban vs. non-urban municipalities and outcome percentile rank GPA 9
Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE			RADIUS: 2KM	
	Non-Urban Area		Urban Area	Non-Urban Area	Urban Area
Marginal effects					
Cohorts 1988-1990 *rel. to untreated*	-0.235** (0.0914)	0.807 (0.682)	0.147*** (0.0201)	-0.494*** (0.122)	0.349*** (0.0646)
Cohorts 1985-1987 *rel. to untreated*	-0.0715 (0.0942)	0.763 (0.549)	0.102*** (0.0205)	-0.419*** (0.131)	0.165** (0.0702)
Cohorts 1982-1984 *rel. to untreated*	-0.198** (0.0942)	0.426 (0.414)	0.0409** (0.0200)	-0.369*** (0.138)	0.0325 (0.0681)
Cohorts 1979-1981 *rel. to untreated*	-0.107 (0.0929)	0.291 (0.284)	0.0497** (0.0216)	-0.153 (0.126)	0.0130 (0.0747)
Cohorts 1977-1978 *rel. to untreated*	-0.0604 (0.115)	0.182 (0.196)	0.00708 (0.0248)	-0.193 (0.144)	-0.0118 (0.0739)
Untreated Cohorts (1972-1976)	0.250*** (0.0695)		-0.0632*** (0.0189)	0.385*** (0.0843)	0.0562 (0.0470)
Coefficients					
Choice		0.392*** (0.115)			
Trend×Choice *(pre-reform trend)*		-0.0699 (0.0453)			
Constant	15.93** (6.959)	16.95** (6.996)	30.33*** (6.216)	17.31** (6.960)	24.40*** (6.471)
Placebo test	Fail	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows×Trend	Treatment Windows	Treatment Windows	Treatment Windows
Observations	784,494	784,494	930,927	784,494	930,927
R-squared	0.164	0.164	0.199	0.164	0.199

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

Table 13: Heterogeneity of effects with respect to urban vs. non-urban municipalities and outcome cognitive skills

Outcome: Cognitive skills
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Non-Urban Area	Urban Area	Non-Urban Area	Urban Area
Cohorts 1985-1987 *rel. to untreated*	0.0194**	0.00146	0.0114	0.0223***
	(0.00954)	(0.00172)	(0.0135)	(0.00589)
Cohorts 1982-1984 *rel. to untreated*	0.0121	0.000786	0.0243*	0.0145**
	(0.00885)	(0.00174)	(0.0135)	(0.00612)
Cohorts 1979-1981 *rel. to untreated*	0.00757	-0.000396	0.00816	0.0154**
	(0.00946)	(0.00183)	(0.0127)	(0.00637)
Cohorts 1977-1978 *rel. to untreated*	0.00583	0.000771	0.0125	0.0118*
	(0.0115)	(0.00239)	(0.0146)	(0.00710)
Untreated Cohorts (1972-1976)	-0.0146**	0.000842	-0.0270***	-0.0110***
	(0.00675)	(0.00170)	(0.00840)	(0.00426)
Constant	0.927	2.566***	0.968	2.229***
	(0.639)	(0.473)	(0.639)	(0.471)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	281,734	328,448	281,734	328,448
R-squared	0.124	0.161	0.124	0.161

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

Table 14: Heterogeneity of effects within Stockholm county and outcome percentile rank GPA 9

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within…
Sample: early vs. late adopters within Stockholm
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Late Adopter	Early Adopter	Late Adopter	Early Adopter
Cohorts 1988-1990 *rel. to untreated*	-0.130	0.129***	0.577*	0.557***
	(0.0948)	(0.0432)	(0.316)	(0.123)
Cohorts 1985-1987 *rel. to untreated*	-0.158*	0.0866*	0.0931	0.437***
	(0.0951)	(0.0457)	(0.332)	(0.142)
Cohorts 1982-1984 *rel. to untreated*	-0.194**	0.0382	0.458	0.178
	(0.0946)	(0.0462)	(0.345)	(0.141)
Cohorts 1979-1981 *rel. to untreated*	-0.156	0.0238	1.027***	-0.0572
	(0.0952)	(0.0463)	(0.330)	(0.155)
Cohorts 1977-1978 *rel. to untreated*	-0.0285	-0.00243	0.663	0.0281
	(0.137)	(0.0426)	(0.411)	(0.142)
Untreated Cohorts (1972-1976)	0.216**	-0.0823**	-0.242	-0.149
	(0.0979)	(0.0348)	(0.243)	(0.0944)
Constant	-37.56	29.27***	-4.459	27.36***
	(68.16)	(8.584)	(67.65)	(8.379)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	83,024	144,247	83,024	144,247
R-squared	0.176	0.205	0.176	0.205

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

Table 15: Heterogeneity of effects within Stockholm county and outcome cognitive skills

Outcome: Cognitive skills
Choice Measure: Number of schools within radius…
Sample: early vs. late adopters in Stockholm
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Late Adopter	Early Adopter	Late Adopter	Early Adopter
Cohorts 1985-1987 *rel. to untreated*	-0.0233***	-0.00101	0.0519*	-0.000437
	(0.00832)	(0.00357)	(0.0313)	(0.0116)
Cohorts 1982-1984 *rel. to untreated*	-0.0292***	-0.000454	0.0209	-0.0216*
	(0.00834)	(0.00375)	(0.0319)	(0.0120)
Cohorts 1979-1981 *rel. to untreated*	-0.0298***	-0.000499	0.0256	-0.00473
	(0.00840)	(0.00371)	(0.0327)	(0.0123)
Cohorts 1977-1978 *rel. to untreated*	-0.0178	-0.00617	0.00655	0.000848
	(0.0117)	(0.00456)	(0.0367)	(0.0148)
Untreated Cohorts (1972-1976)	0.0301***	0.00320	0.00244	-0.0138
	(0.00873)	(0.00285)	(0.0225)	(0.00878)
Constant	3.349	1.871**	5.119	2.201***
	(6.795)	(0.783)	(6.636)	(0.792)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	28,684	48,433	28,684	48,433
R-squared	0.167	0.166	0.166	0.167

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

Table 16: Grade inflation

Outcome: Percentile rank GPA 9
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	Radius: median commuting distance	radius: 2km
Maths:		
Pass	11.17***	11.17***
	(0.152)	(0.152)
Pass with distinction	27.20***	27.20***
	(0.189)	(0.189)
Pass with special distinction	35.31***	35.31***
	(0.208)	(0.208)
English:		
Pass	6.297***	6.299***
	(0.210)	(0.210)
Pass with distinction	12.18***	12.18***
	(0.236)	(0.236)
Pass with special distinction	16.34***	16.34***
	(0.263)	(0.263)
Swedish:		
Pass	8.577***	8.575***
	(0.214)	(0.214)
Pass with distinction	25.92***	25.91***
	(0.257)	(0.257)
Pass with special distinction	36.25***	36.25***
	(0.289)	(0.289)
Choice	0.0320**	0.0737*
	(0.0126)	(0.0433)
Constant	213.3**	214.0**
	(102.8)	(102.9)
Observations	173,284	173,284
R-squared	0.666	0.666

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4.

Table 17: Selection into taking the military test

Outcome: Not taking the military test
Choice measure: Number of schools within radius..
Grade Level: 7-9

	Radius: Median Commuting Distance	Radius: 2 km
Cohorts 1985-1987 *rel. to untreated*	-0.00440***	-0.00651***
	(0.000239)	(0.00103)
Cohorts 1982-1984 *rel. to untreated*	-0.00381***	-0.00619***
	(0.000226)	(0.00110)
Cohorts 1979-1981 *rel. to untreated*	-0.00178***	0.00138*
	(0.000210)	(0.000742)
Cohorts 1977-1978 *rel. to untreated*	-0.000639***	0.000904
	(0.000247)	(0.000777)
Untreated Cohorts (=1972-1976)	0.00186***	0.00286***
	(0.000175)	(0.000411)
Placebo test	Pass	Fail
Specification	Treatment Windows	Treatment Windows
Observations	723,147	723,147
Pseudo R-squared	0.134	0.134

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4.

8.2.3 Tables presenting additional specifications related to main analyses

The tables in this section show the coefficients and marginal effects from different specifications for the regression analyses in Sections 6.1 and 6.2 for reporting purpose.

Table 18: Different specifications, choice measure with radius "median commuting distance", outcomes marks and cognitive skills

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Coefficients						
Trend×Choice × Cohorts 1988-1990			0.0266**			
			(0.0132)			
Trend×Choice × Cohorts 1985-1987			0.0168			0.00178
			(0.0138)			(0.00113)
Trend×Choice × Cohorts 1982-1984			0.0273**			-0.000591
			(0.0135)			(0.00112)
Trend×Choice × Cohorts 1979-1981			0.0408**			-0.00162
			(0.0164)			(0.00146)
Trend×Choice × Cohorts 1977-1978			-0.0329			-0.00593*
			(0.0340)			(0.00340)
Trend×Choice (*Pre-reform trend*)			-0.00647			-0.000227
			(0.00910)			(0.000813)
Choice × Cohorts 1988-1990	0.130***	0.125***	-0.228			
	(0.0183)	(0.0228)	(0.165)			
Choice × Cohorts 1985-1987	0.0772***	0.0717***	-0.0831	0.00203	0.00154	-0.0205*
	(0.0186)	(0.0229)	(0.149)	(0.00154)	(0.00187)	(0.0113)
Choice × Cohorts 1982-1984	0.0100	0.00444	-0.234**	0.000503	0.000015	0.00873
	(0.0183)	(0.0227)	(0.115)	(0.00155)	(0.00187)	(0.00900)
Choice × Cohorts 1979-1981	0.0231	0.0175	-0.283**	-0.000367	-0.000859	0.0146
	(0.0195)	(0.0236)	(0.120)	(0.00162)	(0.00195)	(0.0104)
Choice × Cohorts 1977-1978	-0.0139	-0.0195	0.187	0.00267	0.00217	0.0361*
	(0.0227)	(0.0266)	(0.184)	(0.00213)	(0.00237)	(0.0187)
Choice	-0.0334*	-0.0280	-0.0204	0.000143	0.000610	0.00106
	(0.0173)	(0.0219)	(0.0276)	(0.00155)	(0.00185)	(0.00237)
Placebo: Choice×Cohorts 1975-176		-0.0113			-0.000948	
		(0.0267)			(0.00227)	

Table 18 continued

Choice Measure: Number of schools within median commuting distance **Grade Level**: 7-9						
	Percentile Rank GPA 9			Cognitive Score (Men only)		
Marginal effects						
cohorts 1988-1990 *rel. to untreated*	see coefficients	see coefficients	0.225* (0.134)	see coefficients	see coefficients	
cohorts 1985-1987 *rel. to untreated*			0.153 (0.108)			0.00446 (0.00964)
cohorts 1982-1984 *rel. to untreated*			0.0662 (0.0805)			0.00222 (0.00727)
cohorts 1979-1981 *rel. to untreated*			0.0428 (0.0552)			0.00160 (0.00492)
cohorts 1977-1978 *rel. to untreated*			0.00569 (0.0361)			0.00349 (0.00337)
Untreated cohorts † †						
Constant	26.95*** (5.055)	27.01*** (5.055)	26.68*** (5.107)	2.412*** (0.398)	2.412*** (0.398)	2.352*** (0.398)
Specification	Treatment Windows	Placebo test	Treatment Windows×Trend	Treatment Windows	Placebo test	Treatment Windows×Trend
Observations	1,715,421	1,715,421	1,715,421	610,182	610,182	610,182
R-squared‡	0.186	0.186	0.186	0.146	0.146	0.146

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

† † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974.

‡Pseudo R-squared for binary outcomes.

Table 19: Different specifications, choice measure with radius "median commuting distance", outcomes university degree at age 25 and employed at age 25

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	University Degree At Age 25 †			Employed Age 25		
Coefficients						
Trend×Choice×Cohorts 1982-1984			0.00160*** (0.000548)			-0.000511 (0.000510)
Trend×Choice×Cohorts 1979-1981			0.00161** (0.000725)			0.000203 (0.000683)
Trend×Choice×Cohorts 1977-1978			-0.00229 (0.00162)			0.000342 (0.00140)
Trend×Choice (*Pre-reform trend*)			-0.000724* (0.000403)			0.000963*** (0.000362)
Choice×Cohorts 1982-1984	0.00365*** (0.000777)	0.00191* (0.000987)	-0.00774* (0.00426)	0.00187** (0.000731)	0.00344*** (0.000912)	-0.000657 (0.00416)
Choice×Cohorts 1979-1981	0.000489 (0.000817)	-0.00127 (0.00102)	-0.00885* (0.00519)	0.00163** (0.000802)	0.00322*** (0.000965)	-0.00581 (0.00491)
Choice×Cohorts 1977-1978	-0.00151 (0.00104)	-0.00330*** (0.00121)	0.0132 (0.00870)	-0.00006 (0.000932)	0.00156 (0.00108)	-0.00493 (0.00761)
Choice	-0.000509 (0.000774)	0.00115 (0.000971)	0.00114 (0.00123)	-0.00340*** (0.000742)	-0.00490*** (0.000896)	-0.00587*** (0.00113)
Placebo: Choice×Cohorts 1975-1976		-0.00326*** (0.00116)			0.00304*** (0.00103)	
Marginal Effects						
Cohorts 1982-1984 rel. to untreated	0.00138*** (0.000292)	0.000737** (0.000364)	0.00126*** (0.000301)	0.000724*** (0.000258)	0.00134*** (0.000332)	0.000911*** (0.000265)
Cohorts 1979-1981 rel. to untreated	0.000183 (0.000308)	-0.000469 (0.000376)	-0.000125 (0.000336)	0.000621** (0.000282)	0.00124*** (0.000348)	0.000619** (0.000293)
Cohorts 1977-1978 rel. to untreated	-0.000581 (0.000395)	-0.00125*** (0.000455)	-0.000727* (0.000397)	0.000012 (0.000328)	0.000641* (0.000386)	0.000149 (0.000332)
Untreated Cohorts † †	-0.000191 (0.000290)	0.000424 (0.000357)	-0.000114 (0.000297)	-0.00121*** (0.000265)	-0.00180*** (0.000330)	-0.00141*** (0.000270)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000271* (0.000151)			0.000347*** (0.000131)
Placebo Cohorts		-0.00123*** (0.000436)			0.00117*** (0.000365)	
Constant	-1.463*** (0.235)	-1.453*** (0.233)	-1.494*** (0.235)	0.382* (0.203)	0.375* (0.203)	0.407** (0.204)
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,120,459	1,120,459	1,120,459	1,120,845	1,120,845	1,120,845
R-squared‡	0.126	0.126	0.126	0.0300	0.0300	0.0300

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that don't. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974. Pseudo R-squared for binary outcomes.

Table 20: Different specifications, choice measure with radius "median commuting distance", outcomes crime until age 22 and health at age 22

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Coefficients						
Trend×Choice × Cohorts 1985-1987			0.00105* (0.000609)			0.00238*** (0.000741)
Trend×Choice × Cohorts 1982-1984			0.00189*** (0.000620)			0.000487 (0.000742)
Trend×Choice × Cohorts 1979-1981			-0.000411 (0.000839)			0.000712 (0.000897)
Trend×Choice × Cohorts 1977-1978			0.00296 (0.00193)			0.00473** (0.00230)
Trend×Choice (*Pre-reform trend*)			-0.000603 (0.000439)			-0.00137** (0.000542)
Choice × Cohorts 1985-1987	-0.00228*** (0.000824)	-0.00272*** (0.00105)	-0.0101* (0.00599)	0.00240** (0.00104)	0.00104 (0.00125)	-0.0147** (0.00715)
Choice × Cohorts 1982-1984	-0.00287*** (0.000847)	-0.00331*** (0.00106)	-0.0185*** (0.00494)	0.00141 (0.00105)	0.000047 (0.00125)	0.00821 (0.00576)
Choice × Cohorts 1979-1981	-0.00243*** (0.000908)	-0.00286*** (0.00111)	0.00471 (0.00605)	0.00241** (0.00108)	0.00103 (0.00128)	0.00507 (0.00615)
Choice × Cohorts 1977-1978	0.00003 (0.00122)	-0.000420 (0.00138)	-0.0144 (0.0104)	0.00184 (0.00144)	0.000427 (0.00160)	-0.0198 (0.0127)
Choice	0.00318*** (0.000835)	0.00360*** (0.00104)	0.00466*** (0.00131)	-0.00297*** (0.00105)	-0.00167 (0.00123)	-7.79e-05 (0.00151)
Placebo: Choice×Cohorts 1975-176		-0.000839 (0.00121)			-0.00317* (0.00162)	
Marginal_effects						
Cohorts 1985-1987 *rel. to untreated*	-0.000560*** (0.000190)	-0.000672*** (0.000246)	-0.000626*** (0.000195)	0.000262** (0.000118)	0.000140 (0.000162)	0.000236** (0.000118)
Cohorts 1982-1984 *rel. to untreated*	-0.000680*** (0.000195)	-0.000792*** (0.000248)	-0.000742*** (0.000199)	0.000131 (0.000120)	0.000008 (0.000163)	0.000105 (0.000120)
Cohorts 1979-1981 *rel. to untreated*	-0.000577*** (0.000209)	-0.000690*** (0.000259)	-0.000529** (0.000221)	0.000252* (0.000129)	0.000126 (0.000170)	0.000272** (0.000136)
Cohorts 1977-1978 *rel. to untreated*	-0.000022 (0.000278)	-0.000137 (0.000319)	-0.000087 (0.000279)	0.000201 (0.000165)	0.000074 (0.000197)	0.000155 (0.000166)
Untreated Cohorts † †	0.000744*** (0.000195)	0.000852*** (0.000246)	0.000809*** (0.000199)	-0.000333*** (0.000117)	-0.000218 (0.000161)	-0.000316*** (0.000118)
Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000144 (0.000105)			-0.000150** (5.93e-05)
Placebo Cohorts		-0.000218 (0.000283)			-0.000206 (0.000176)	
Constant	-0.237 (0.249)	-0.232 (0.250)	-0.216 (0.250)	-0.844*** (0.306)	-0.820*** (0.304)	-0.803*** (0.304)

Table 20 continued

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,409,092	1,409,092	1,409,092	1,402,829	1,402,829	1,402,829
R-squared ‡	0.0382	0.0382	0.0382	0.0290	0.0290	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1.

† † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974

‡ Pseudo R-squared for binary outcomes

Table 21: Different specifications, choice measure with radius 2km, outcomes marks grade 9 and cognitive skills

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Coefficients						
Trend×Choice × Cohorts 1988-1990			0.135**			
			(0.0566)			
Trend×Choice × Cohorts 1985-1987			0.00413			0.00432
			(0.0632)			(0.00507)
Trend×Choice × Cohorts 1982-1984			0.185***			-0.00385
			(0.0583)			(0.00513)
Trend×Choice × Cohorts 1979-1981			0.109*			-0.00615
			(0.0628)			(0.00553)
Trend×Choice × Cohorts 1977-1978			-0.0601			-0.0156
			(0.103)			(0.0100)
Trend×Choice (*Pre-reform trend*)			-0.0344			0.00121
			(0.0245)			(0.00230)
Choice×Cohorts 1988-1990	0.298***	0.264***	-1.494*			
	(0.0570)	(0.0663)	(0.877)			
Choice×Cohorts 1985-1987	0.0722	0.0387	0.421	0.0193***	0.0212***	-0.0554
	(0.0614)	(0.0691)	(0.823)	(0.00507)	(0.00587)	(0.0639)
Choice×Cohorts 1982-1984	-0.115*	-0.148**	-1.855***	0.0103**	0.0122**	0.0418
	(0.0602)	(0.0695)	(0.596)	(0.00519)	(0.00594)	(0.0508)
Choice×Cohorts 1979-1981	-0.0729	-0.106	-0.744	0.0110**	0.0129**	0.0530
	(0.0638)	(0.0726)	(0.471)	(0.00534)	(0.00613)	(0.0404)
Choice×Cohorts 1977-1978	-0.106	-0.139*	0.339	0.0138**	0.0157**	0.0955*
	(0.0653)	(0.0736)	(0.555)	(0.00615)	(0.00684)	(0.0548)
Choice	0.138***	0.170***	0.212***	-0.0122***	-0.0140***	-0.0147**
	(0.0411)	(0.0526)	(0.0659)	(0.00371)	(0.00458)	(0.00604)
Placebo: Choice×Cohorts 1975-1976		-0.0740			0.00426	
		(0.0724)			(0.00647)	
Marginal effects						
Cohorts 1988-1990 *rel. to untreated*	see coefficients	see coefficients	0.801**	see coefficients	see coefficients	
			(0.368)			
Cohorts 1985-1987 *rel. to untreated*			0.479			0.00501
			(0.298)			(0.0277)
Cohorts 1982-1984 *rel. to untreated*			0.185			-0.000495
			(0.224)			(0.0211)
Cohorts 1979-1981 *rel. to untreated*			0.124			0.00381
			(0.156)			(0.0144)
Cohorts 1977-1978 *rel. to untreated*			0.00856			0.00977
			(0.104)			(0.00979)
Untreated Cohorts † †						
Constant	22.25***	22.38***	22.24***	2.330***	2.322***	2.310***
	(5.241)	(5.245)	(5.261)	(0.397)	(0.398)	(0.398)

Table 21 continued

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,715,421	1,715,421	1,715,421	610,182	610,182	610,182
R-squared‡	0.186	0.186	0.186	0.146	0.146	0.146

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974.
‡ Pseudo R-squared for binary outcomes.

Table 22: Different specifications, choice measure with radius 2km, outcomes university degree at age 25 and employed at age 25

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	University Degree at Age 25 †			Employed Age 25		
Coefficients						
Trend×Choice×Cohorts 1982-1984			0.00399			-0.00315
			(0.00268)			(0.00237)
Trend×Choice×Cohorts 1979-1981			0.00510*			-0.00406*
			(0.00276)			(0.00238)
Trend×Choice×Cohorts 1977-1978			-0.00489			-0.00105
			(0.00518)			(0.00469)
Trend×Choice (*Pre-reform trend*)			-0.000607			0.00386***
			(0.00121)			(0.00111)
Choice×Cohorts 1982-1984	0.0149***	0.0124***	-0.0238	0.00542**	0.0102***	0.00604
	(0.00272)	(0.00317)	(0.0265)	(0.00245)	(0.00278)	(0.0234)
Choice×Cohorts 1979-1981	0.00667**	0.00416	-0.0308	0.00287	0.00766***	0.0129
	(0.00271)	(0.00314)	(0.0201)	(0.00253)	(0.00286)	(0.0171)
Choice×Cohorts 1977-1978	0.000994	-0.00151	0.0299	0.00361	0.00840***	-0.00346
	(0.00314)	(0.00351)	(0.0275)	(0.00288)	(0.00315)	(0.0254)
Choice	0.000690	0.00306	0.00202	-0.0154***	-0.0199***	-0.0235***
	(0.00192)	(0.00243)	(0.00322)	(0.00184)	(0.00223)	(0.00301)
Placebo: Choice×Cohorts 1975-176		-0.00534			0.0106***	
		(0.00346)			(0.00299)	
Marginal effects						
Cohorts 1982-1984 rel. to untreated	0.00565***	0.00473***	0.00555***	0.00229***	0.00421***	0.00253***
	(0.00103)	(0.00118)	(0.00103)	(0.000827)	(0.000968)	(0.000829)
Cohorts 1979-1981 rel. to untreated	0.00257**	0.00165	0.00243**	0.00129	0.00320***	0.00153*
	(0.00103)	(0.00118)	(0.00103)	(0.000875)	(0.00101)	(0.000876)
Cohorts 1977-1978 rel. to untreated	0.000383	-0.000542	0.000332	0.00141	0.00333***	0.00164
	(0.00119)	(0.00132)	(0.00119)	(0.00101)	(0.00112)	(0.00101)
Untreated Cohorts † †	0.000259	0.00113	0.000302	-0.00549***	-0.00732***	-0.00562***
	(0.000721)	(0.000900)	(0.000725)	(0.000656)	(0.000819)	(0.000658)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000228			0.00139***
			(0.000455)			(0.000399)
Placebo Cohorts		-0.00201			0.00418***	
		(0.00131)			(0.00105)	
Constant	-1.697***	-1.690***	-1.702***	0.483**	0.469**	0.485**
	(0.233)	(0.232)	(0.233)	(0.201)	(0.200)	(0.201)
Specification	Treatment Windows	Placebo Test	Treatment Windows ×Trend	Treatment Windows	Placebo Test	Treatment Windows ×Trend
Observations	1,120,459	1,120,459	1,120,459	1,120,845	1,120,845	1,120,845
R-squared‡	0.126	0.126	0.126	0.0301	0.0301	0.0301

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974. ‡Pseudo R-squared for binary outcomes.

Table 23: Different specifications, choice measure with radius 2km, outcomes crime until age 22 and health at age 22

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Coefficients						
Trend×Choice×Cohorts 1985-1987			0.00475*			0.00724**
			(0.00270)			(0.00336)
Trend×Choice×Cohorts 1982-1984			0.00611**			-0.00295
			(0.00286)			(0.00340)
Trend×Choice×Cohorts 1979-1981			-0.00612**			-6.64e-05
			(0.00295)			(0.00316)
Trend×Choice×Cohorts 1977-1978			0.00637			0.0183***
			(0.00576)			(0.00681)
Trend×Choice (*Pre-reform trend*)			-0.000452			-0.00438***
			(0.00124)			(0.00153)
Choice×Cohorts 1985-1987	0.00203	0.00269	-0.0592*	0.00596*	0.00362	-0.0425
	(0.00272)	(0.00314)	(0.0335)	(0.00348)	(0.00391)	(0.0426)
Choice×Cohorts 1982-1984	0.000759	0.00142	-0.0624**	0.000286	-0.00206	0.0725**
	(0.00288)	(0.00327)	(0.0284)	(0.00355)	(0.00397)	(0.0335)
Choice×Cohorts 1979-1981	-0.000667	-1.01e-05	0.0510**	0.00287	0.000532	0.0301
	(0.00291)	(0.00335)	(0.0217)	(0.00340)	(0.00384)	(0.0227)
Choice×Cohorts 1977-1978	0.00533	0.00599	-0.0282	0.00407	0.00171	-0.0815**
	(0.00346)	(0.00382)	(0.0309)	(0.00411)	(0.00447)	(0.0369)
Choice	0.00224	0.00162	0.00316	-0.00414*	-0.00191	0.00403
	(0.00199)	(0.00252)	(0.00335)	(0.00251)	(0.00306)	(0.00392)
Placebo: Choice×Cohorts 1975-176		0.00145			-0.00623	
		(0.00341)			(0.00447)	
Marginal effects						
Cohorts 1985-1987 *rel. to untreated*	0.000351	0.000498	0.000313	0.000690*	0.000460	0.000727*
	(0.000592)	(0.000704)	(0.000595)	(0.000413)	(0.000497)	(0.000413)
Cohorts 1982-1984 *rel. to untreated*	0.000097	0.000244	0.000091	-0.000034	-0.000264	0.000003
	(0.000628)	(0.000730)	(0.000627)	(0.000435)	(0.000514)	(0.000433)
Cohorts 1979-1981 *rel. to untreated*	-0.000174	-0.000027	-0.000164	0.000283	0.000051	0.000326
	(0.000658)	(0.000770)	(0.000656)	(0.000441)	(0.000522)	(0.000441)
Cohorts 1977-1978 *rel. to untreated*	0.00117	0.00132	0.00115	0.000456	0.000226	0.000426
	(0.000784)	(0.000875)	(0.000785)	(0.000475)	(0.000547)	(0.000473)
Untreated Cohorts † †	0.000524	0.000383	0.000527	-0.000465*	-0.000249	-0.000530*
	(0.000464)	(0.000596)	(0.000466)	(0.000282)	(0.000398)	(0.000281)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000106			-0.000485***
			(0.000290)			(0.000170)
Placebo Cohorts		0.000317			-0.000474	
		(0.000792)			(0.000480)	
Constant	-0.289	-0.291	-0.282	-0.806***	-0.793***	-0.774**
	(0.253)	(0.253)	(0.253)	(0.307)	(0.306)	(0.306)

Table 23 continued

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,409,092	1,409,092	1,409,092	1,402,829	1,402,829	1,402,829
R-squared‡	0.0382	0.0382	0.0382	0.0290	0.0290	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 4. The definition of the placebo tests is explained in Section 5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974. ‡ Pseudo R-squared for binary outcomes.