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Student Outcomes:
Evidence from an Admission Reform in Stockholm

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Abstract

This paper analyzes what happened to compulsory school grades after they became more important for admissions to public high schools in the City of Stockholm. The City of Stockholm changed the selection mechanisms to its public high schools for the fall 2000 intake, abolishing proximity-based priority. Instead students could apply to any school with compulsory school grades as the only admission criteria. The change meant that students could be forced away from their neighborhood school, but on the other hand they could be admitted to any of the municipality's public high schools if they had good enough grades. One motivation behind the reform was to increase students' effort in compulsory school. To determine the effect of the reform on academic achievement in compulsory school, a difference-in-differences analysis is conducted using school-cohort level data 1998-2005, where the control group consists of compulsory schools in other municipalities in Stockholm County. The results indicate that the reform on aggregate had a small positive effect on compulsory school grades. However, the effect is not immediate. The results should therefore be interpreted cautiously. An analysis of heterogeneous effects reveals that schools where parents have relatively high education drive the results, suggesting that the gains of the reform on the compulsory school level were concentrated among students with relatively highly educated parents.

Keywords: Economics of education, Compulsory school, Grades, Incentives

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1 Introduction

Since the 1980s, school markets in several countries, such as the U.S, Chile, Denmark, the Netherlands, New Zealand and Sweden, have been deregulated and decentralized with the hope that market forces will lead to better matching between students and schools, more innovation and more efficient use of resources as a result of more competition and school choice (Epple, Romano & Urquiola, 2017). However, because the market for education has unusually large problems with information asymmetries, it is theoretically ambiguous whether more market mechanisms will lead to better academic outcomes (MacLeod & Urquiola, 2012). Yet the many empirical observations from a large number of local contexts seem to suggest that more choice and competition could be a viable way to improve student achievement, but the issue is far from settled, warranting further research (Epple, Romano & Urquiola, 2017).

Sweden in particular has been keen on adopting market forces into its school system. Starting in the the early 1990s, a series of reforms changed Sweden's school system from being highly centralized to highly decentralized (Björklund et al., 2005). Following the radical 1992 national school choice reform, students in Sweden could freely choose to apply to any independent high school in the country and be admitted on equal terms regardless of residence. However, for admission to public high schools, the residence principle still applied. This meant that in practice, students who wanted to attend a public high school applied to a high school program in their municipality. If they were admitted to the program, they were allocated to the school closest to their home. Since many of these reforms have been implemented simultaneously on a national level, evaluating their effects have been difficult (Holmlund et al., 2014).

Stockholm City's change in admission system for the fall 2000 intake to its public high schools is a rare example of a local reform. The municipality completely abolished the system of proximity-based priority. Instead, residents of Stockholm City could apply to any public school in the municipality, and the only selection mechanism was grades from compulsory school (USK, 2002). The admission process thus became more competitive. Students could be forced away from their neighborhood school, but on the other hand they could be admitted to any of the municipality's public high schools as long as they had good enough grades.

Two earlier papers study the reform's short-run effects on segregation and student performance in high school. As expected, the reform had a drastic effect on student mobility and ability sorting, leading to more segregation along various dimensions (Söderström & Uusitalo, 2010). However, despite the opportunity to choose a school that better matched one's preferences, students in Stockholm City performed no better in high school after the reform (Söderström, 2006).

The current paper does not seek to evaluate the reform as such, but instead analyzes how information about the new selection mechanism to high school impacted students' grades in compulsory school. It therefore distinguishes itself from earlier work that has focused on effects of the reform in high school. Given what we know of the reform, it is interesting for several reasons to also know whether the reform had an effect on student achievement

already in compulsory school. First, one motivation behind the reform was to promote hard work and incentivize students in compulsory school (City of Stockholm, 1999). This clearly shows the policy relevance of exploring how the reform affected students' grades in compulsory school. Second, since theoretical and empirical research show that students value school choice (Epple, Romano & Urquiola, 2017), it would be interesting to know whether school choice can translate into higher achievement when choice is made conditional on achievement. Third, the results of field experiments show that extrinsic motivation in the form of financial and non-financial rewards can lift student achievement (Koch, Nafziger & Skyt Nielsen, 2015; Lavecchia, Liu & Oreopoulos, 2016). Evidence from this policy reform in the City of Stockholm, where school choice can be seen as a reward for high performance, offers complementary evidence of what the magnitude of aggregate effects can be when performance incentives are implemented on a large scale in education systems.

The admission reform in Stockholm City thus offers a rare opportunity to study how education performance changes when grades become more important for school admissions. This can generate insights into how students and schools respond when school performance becomes more important for students. Moreover, a key difference between this reform and most other school choice reforms is that the Stockholm City admission reform increased school choice conditional on achievement. In fact, a feature of most school choice reforms is that schools are not allowed to admit students based on previous achievement. Examples of such reforms include charter schools in the U.S. (Hoxby, 2003), school choice to middle schools in Israel (Lavy, 2010) and school choice to compulsory schools in Sweden (Wondratschek et al., 2013a, 2013b). If schools cannot select students based on previous achievement, there is little reason to believe that students would try to improve their performance in the education level below the one where students can exercise choice. It has therefore been natural in these studies to only study the effect on achievement after choice has been exercised.

If students value the opportunity to attend selective schools (or the threat of being left out in an unpopular school) we would expect them to exert more effort to get better grades. If good grades become more important for students, then also teachers and schools may become more focused on lifting student achievement. Studying the effects of such incentives is relevant because many education systems have elements of admission based on previous educational performance. For example, in many countries high school grades are important for admission to higher education. Having a better understanding of the effects of these admission rules would be beneficial for policy makers who consider implementing changes to the school system.

The research question of the study is thus: Did the reform have an effect on compulsory school grades in Stockholm City? The broader purpose of the paper is to contribute to the understanding of the effects of school choice conditional on previous education performance. From a policy perspective it is important to evaluate to what extent the goal to promote hard work and incentivize students in compulsory school was achieved. This is also important for policy makers who consider implementing a similar admission reform. If performance increases, students would be better prepared for the next level in the school system.

To determine the effect of the reform on compulsory school achievement, measured by grades in the final year of compulsory school, I use publicly available school-cohort level data from the Swedish National Agency for Education (NAE). Like Söderström (2006) and Söderström and Uusitalo (2010) I use a difference-in-differences (DiD) strategy to estimate the aggregate effect of the reform. I use schools in the municipality Stockholm City as the treatment group, whereas schools in the other 25 municipalities in Stockholm County serve as a control group.[1]

The results provide some evidence that the reform on aggregate had a modest, positive effect on grades in Stockholm City. Estimates of heterogeneous effects show that the positive aggregate effects are concentrated in compulsory schools with relatively good socio-economic conditions, as measured by parental education levels. One cause of concern, however, is that the positive effect of the reform only appears some years after the policy change. Further research is therefore needed to verify that there are no other underlying trends affecting treatment group and control group differently.

The paper is organized as follows. Section 2 describes the Swedish education system including the Stockholm City admission reform. Section 3 reviews the relevant theoretical and empirical literature. Section 4 presents the data and descriptive statistics. Section 5 describes the empirical strategy. Section 6 presents the results. Finally, section 7 discusses the results and concludes.

2 The Swedish Education System and the Stockholm Admission Reform

2.1 School reforms in the 1990s

The Swedish education system used to be centralized with little autonomy for municipalities and little choice for students. Policy started to change during the late 1980s, when initiatives were taken to make the system more decentralized with a quasi-market for education (Holmlund et al., 2014). In the early 1990s the central government transferred financial and administrative responsibilities for schools to municipalities. This allowed municipalities to decide how much resources they wanted to allocate to education. Formally, municipalities became the operator of public schools instead of central government. Reforms also made it easier for independent schools to operate, as municipalities started to finance them on a per-student basis—on almost equal terms as public schools (Björklund et al., 2005). Schools can be organized by either municipalities or independent education providers (independent schools). An independent education provider may be a for profit or non profit company, foundation or association. The education independent schools provide should be the same as that provided in public schools (NAE, 2017a).

[1] I use the terms control group and comparison group interchangeably.

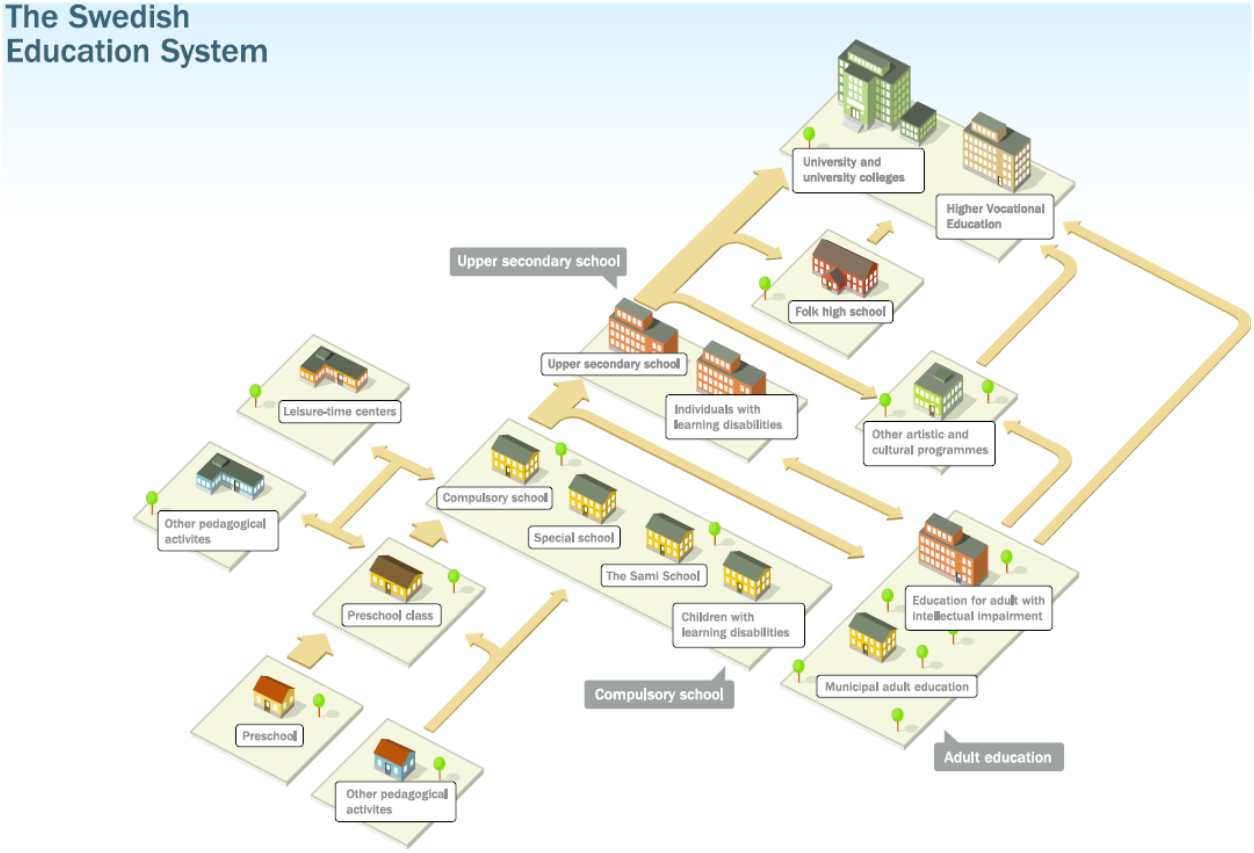
Although no compulsory school is allowed to admit students based on academic achievement, there are differences in admission criteria between public and independent compulsory schools (Holmlund et al., 2014). Students are allowed to freely choose among public schools and independent schools, both within and outside their municipality, but public schools must prioritize children within the school's catchment area while independent schools can use other criteria, for instance queue time, which can be several years. In general, students who can be expected to perform well in school, based on family background, are over represented in independent schools. This is not the case for students who choose to attend a different public school than they were first assigned to according to the residence principle (Holmlund et al., 2014).

Figure 1 shows an overview of the Swedish education system. All education in Sweden is financed through taxes; tuition fees are not allowed. The parliament decides the national syllabuses, which all schools must follow. However, municipalities, the lowest tier of government, formulate educational plans for themselves and make sure they are carried out. Most children start 1st grade of compulsory school in the fall the year they turn seven and graduate after 9th grade. Students who complete compulsory school can attend high school, and more than 90 percent do. Like compulsory schools, high schools can be either public or independent. There are 18 national programs in high school, including both university preparing programs and vocational programs. Students who complete high school either continue to higher education or start working (NAE, 2017a; Wikström, 2006).

In terms of assessment, the system has also undergone changes. Until 1996, there was a relative grading system in compulsory school. Students were graded 1 to 5 on a curve, relative to the national distribution of scores on standardized tests. Therefore, there was no fixed limit on the number of students in a particular school that could receive a particular grade (Holmlund et al., 2014).

In 1994 it was decided that a goal-oriented, criterion-referenced grading system would replace the relative system from 1996 onward (Holmlund et al., 2014). The new scale, which was used until 2011, had four grade levels per subject: IG (fail), G (pass), VG (pass with distinction) and MVG (pass with special distinction). Teachers were responsible for both teaching and grading, for some subjects with guidance from standardized national test scores (Wikström, 2006).[2] All students with at least a passing grade in Swedish, English and mathematics are eligible for high school. The cohort of students that started 8th grade fall 1996 was the first cohort being graded with the new system. These students and their teachers therefore had experience with the new grading system three semesters before the students received their final compulsory school grades in spring 1998. Note that while grades are also given in 8th grade, only the grades from the final year in compulsory school are used for high school applications (Holmlund et al., 2014).

[2] Predominantly Swedish, English and mathematics.



Source: National Agency for Education

Figure 1: The Swedish education system

2.2 The Stockholm City admission reform

The municipality Stockholm City reformed its admission system for the fall 2000 intake to its 27 public high schools. It completely abolished the system of proximity-based priority. Instead, residents of Stockholm City could apply to any public school in the municipality, and the only selection mechanism was grades from compulsory school (USK, 2002). With 56 votes for and 39 votes against the proposal, the city council took the official decision on October 18, 1999 (Wennerholm, Karlberg & Olsson, 1999).

The change came amid dissatisfaction with the residence principle, whereby students were supposed to attend the high school closest to their home (City of Stockholm, 1999). The residence principle was criticized because it exacerbated the spillover effects of residential segregation on high school admissions. In general, schools in the city center were considered better than schools in the municipality's suburbs. Since the municipality was residentially segregated, with well-of people predominantly living in the municipality's city center, the system was considered unfair (City of Stockholm, 1999). Moreover, students and families abused the system. It was common during the application period to temporarily register

with a relative living close to a school a student wanted to attend. Students could circumvent the residence principle by doing so. Therefore, another rationale for the reform was to curb cheating, instead promoting hard work and incentivize students to obtain good grades in compulsory school (City of Stockholm, 1999).

Until the reform in the year 2000, school choice was highly limited for those who applied to public high schools. Students in Stockholm County's 26 municipalities applied to specific high school programs in their local municipality and ranked their preference of program. If the number of applicants to a specific program exceeded the number of slots, students were ranked according to their compulsory school grades and those with the highest grades were admitted. Once admitted to a program, students were given a place in the school closest to their home. Students had the possibility to indicate which school they preferred to attend, but if the slots at the school were oversubscribed, those who lived within the school's catchment area were prioritized (USK, 2002). As the widespread cheating shows, in practice choice was limited.

Similarly, it was possible to apply to a program in another municipality than the one in which the student resided. However, if the home municipality offered the same program, a student could only be admitted if there were openings left after all local residents had been admitted and the home municipality was willing to pay. If the home municipality did not offer the program, the student could be admitted on the same terms as local residents (USK, 2002). As will be described in section 5.1.2, the share of students in the county's other municipalities that was admitted to programs in Stockholm City on the same terms as locals was marginal. It was not until 2008 the municipalities in Stockholm County introduced a common high school market, where all students competed on equal terms, regardless of residence, for spots in public high schools (City of Stockholm, 2015).[3]

USK (2002) describes how the reform changed the admission procedure. Students in Stockholm City graduating from compulsory school in the year 2000 applied to both program and high school. They could rank their preferred combinations of programs and schools and could freely submit how many combinations they wanted. They were admitted solely based on grades from compulsory school. If they were not admitted to their first choice, the second was considered and so on. Even if they were admitted to a lower ranked alternative, the students could still be on the reserve list for their higher ranked choices. Ultimately I would like to access data on students' choices, but unfortunately such data are not available.

The reform thus dramatically expanded school choice to public schools and made the admission process more competitive: students not only competed for spots at a given program, but also for which school to attend. Given the design of the admission process, there was no reason for students to be strategic when ranking their combinations of schools and programs. As students could submit how many applications they wanted without any penalty and be on the reserve list for schools they were not admitted to, the admission system ensured that students could reveal their true preferences regarding which school-program combinations

[3] Social sciences and natural sciences programs in Stockholm City's public schools were exempt from this decision until 2011.

they wished to attend. Under the previous system, some students may have faced a trade-off between which school and which program they wanted to attend. Since the grade requirements for different programs may have differed, some students may have applied for a less competitive program to increase the chances of being admitted to the nearby school.

Under the new system, students could choose programs from 27 public high schools, 16 of which offered the popular university-preparing programs natural sciences and social sciences, which about 40 percent of first-year students attend (USK, 2002). As expected, compulsory school grades of admitted students varied more across high schools following the reform. For example, between 1999 and 2001, the school with the largest increase in the median student's compulsory school grades had an increase in this measure with 22 percent. In contrast, one school saw a decrease of 20 percent. In general, high schools in the municipality's city center experienced an increase in the compulsory school grades of their admitted students, whereas schools in the suburbs experienced a decrease.

The reform did not affect the regulations for how students from other municipalities could apply to high schools in Stockholm City (USK, 2002). It is therefore unsurprising that the share of students from other municipalities in Stockholm City's public schools was unchanged after the reform. In both 1999, before the reform, and 2001, the second year with the reform, 16 percent of new first-year students in Stockholm City's high schools resided in another municipality. In the university-preparing programs the number was stable at 8 percent, and in other programs equally stable at 21 percent. However, the socio-economic composition of students may have changed, even if USK (2002) provides no such information.

The reason these numbers differ is the regulations regarding under what conditions residents of other municipalities could be admitted to high schools in the City of Stockholm (USK, 2002). Students from other municipalities could only apply to a program in Stockholm City on the same terms as locals if the program was not given in the home municipality and the municipality did not have an agreement with another municipality that offered the program. Non-local students could still apply to schools in Stockholm City even if their home municipality offered the program, but could then only be admitted if there were spots left after all locals had been admitted. According to law, a municipality must offer its own high school programs if there is sufficient demand from its students (Swedish Code of Statutes, 1985). Therefore, most municipalities offer the university-preparing programs and some other popular vocational programs. As I will discuss in section 5.1.2, few students residing in Stockholm County's other municipalities were eligible to apply for these programs in Stockholm City and be admitted on equal terms as locals.

Figure 2 shows the different municipalities in Stockholm County. Stockholm City is located in the county's center and is the county's most populous municipality.

Source: Statistics Sweden

Figure 2: Municipalities in Stockholm County

Figure 3 shows a more detailed view of Stockholm City. The municipality has both rich and poor neighborhoods (City of Stockholm, 2013). Several of the most popular public high schools are located in the city center. It takes about one hour to commute with public transportation from the South to the Northwest.

Source: City of Stockholm

Figure 3: Map of Stockholm City

3 Literature

3.1 Theoretical motivation for school choice

To understand what effects school choice reforms may have, it is important to have a solid theoretical understanding of how school markets work, including the different incentives schools, parents and students have.

There exist two common ways of organizing schooling: the proximity model and the school choice model, and most school systems use a combination of both. In the proximity model, students attend the school closest to their home and in their catchment area. In the school choice model, students choose schools, which receive government funding based on the number of students enrolled, creating a quasi-market for schools (Holmlund et al., 2014).

Gibbons et al. (2008) describe the arguments made in favor of each of these models. Economic theory provides two efficiency arguments in favor of the school choice model. The first is a competition effect. Under the residence principle, schools are local monopolies. Therefore, the incentives to improve teaching and innovate could be weak. Improvements and innovation require costly effort that will not have financial benefits for the individual schools if students cannot choose a different school if they want to, perhaps because they perceive

their school's quality to be low. Ways to create such incentives in the absence of a market mechanism, as in the proximity model, include having strong institutions, governance and performance-related pay.

In the school choice model, where funding is linked to the number of students, the market for students generates the incentives for schools to improve. Funding is linked to popularity, so schools have to be popular and actively chosen by students. Students will leave poor schools, forcing failing schools to shut down. Facing tougher competition, schools will have stronger incentives to innovate to stay in the market. This competition effect can lead to overall improvement of schools. Second, allowing students and parents to choose schools according to their preferences can lead to better matching between students and schools. This can have positive effects on learning (Gibbons et al., 2008).

There are reasons, however, to question whether school choice can have the positive effects mentioned above (MacLeod & Urquiola, 2012). For a market to function well, it is important that buyers know what they get, i.e. that there are no strong information asymmetries between education providers and students. There are several information asymmetries in the school market, making it hard for students to know which school to choose. For example, students do not know which classmates and teachers they will have or what textbooks they will use. Moreover, as parents and schools cannot contract on education outcomes, and students and teacher efforts jointly determine grades, it is difficult to hold schools accountable for how students perform. Additionally, output is not only grades, but also satisfaction or pleasure from attending a particular school, for example from making friends. The academic output, grades, are also observed a long time after students start attending a school, normally at least three years in Swedish compulsory schools.[4] Yet should students be disappointed with their school's quality, it is costly and difficult to switch to another school, for example because students may have to retake courses.

For the reasons discussed in the previous paragraphs, the relationship between student and school is more complicated than that between buyer and seller of standard commodity goods. Holmlund et al. (2014) describe the relationship as more like joint ventures between companies and employment contracts on the labor market, where schools can be seen as employers and students as employees. Just like some employees may prefer non-demanding employers, so students could choose to attend schools that require little of students. In the case of Stockholm City, if students place small weight on school quality, introducing school choice may not necessarily lead to more effort in compulsory school to be admitted to high quality schools. On the other hand, if more choice given grades increases students' willingness to study, compulsory schools will be held more accountable for their students' grades as their importance increases for students. The incentives for students and schools alike would then become more focused on working towards better grades. This effect may still be present even if families cannot observe high school quality or value factors other than academic quality (MacLeod & Urquiola, 2012). For example, the authors argue that parents who choose

[4] Some schools do not offer grades 7-9, so students attending those schools must switch to another school for their final three years of compulsory school. In other cases, students attend the same school for their entire compulsory school education.

schools take average grade levels as a signal they hope is correlated with value added and future outcomes in education and the labor market for the school's students.

Given that the school choice model in theory offers no guarantee to improve academic achievement, it is not surprising that the proximity model has some support. Proponents of this school model argue that teaching is better in a stable environment without competitive forces. Additionally, since students and parents do not need to research schools to attend, the proximity model has lower search costs and lower travel costs because students will attend the school closest to their home. School choice will lead to greater travel distances, which may lead to more stress, lateness and absence. Moreover, it is not necessarily the case that schools will respond to tougher competition by increasing quality, as proponents of school choice would argue. Since providing education is a costly service, some schools may find it financially advantageous to reduce spending and only serve students with weak preferences for quality. Furthermore, some schools may want to take measures to serve only the brightest students, so called "creme skimming" (Gibbons et al., 2008).

Increased school choice can affect educational outcomes in at least two ways, a direct effect on students and an indirect effect through school competition (Wondratschek et al., 2013a; Bayer & McMillan, 2005; Gibbons et al., 2008). These effects represent the possible channels through which the reform may affect educational achievement. In the Stockholm City context, the first effect refers to how students are incentivized in compulsory school to be admitted into their preferred high school. The second effect, the competition effect, refers to how compulsory schools may react to the increasing importance of compulsory school grades. Gibbons et al. (2008) note that for schools, these two effects are the same: where parents have more choice, schools face tougher competition. The threat of changing to another school also matters, not only whether this option is actually exercised. In Stockholm City, this could lead to schools being more responsive to student and parent demands to satisfy an increased importance of grades in compulsory schools.

If grades become more important for students, schools may respond by changing teaching methods or increasing effort to raise grades. If they do not, students can exercise choice earlier in the education system to choose other schools, for instance using Tiebout choice (Tiebout, 1956), i.e. moving to a different neighborhood or another municipality with better schools, or attending an independent school. Therefore, schools will be under more competitive pressure to increase academic achievement. Note that Swedish schools cannot gain market share by changing location or price. Instead they must either increase overall quality or try to differentiate their services. For example, schools can offer different subject specializations, such as math or sports, or use different teaching methods, such as more use of computers.

Yet the extent to which competitive pressure will affect school conduct depends on what schools maximize (Bayer & McMillan, 2005). They exemplify using two polar cases. Suppose public schools maximize educational quality. Then there is no room for quality improvement due to competition, as quality would already be at maximum level. The other extreme is that public schools maximize rents. In this case, schools face a trade-off between attracting students and minimizing effort. If parents start valuing quality, then schools would need

to increase effort to attract students, thereby raising academic achievement. In a formal principal-agent model of rent-seeking in schools, Borland and Howsen (1992) reach this conclusion too. Hoxby (2003) shows that both for-profit independent schools and non-profit independent schools will maximize quality on a competitive school market with choice, given that quality is what students and parents value when choosing schools.

To summarize, demand for a particular school is made up of two parts. On the one hand the future expected outcomes that the school offers, correlated with its average grades, and on the other hand other school characteristics. If students value attending selective high schools, and compulsory school grades become more important for attending such schools, it would be expected that compulsory school grades rise. The mechanisms through which this may happen are both increased student effort and more focus from schools on raising academic achievement. In contrast, if students value other factors more than attending selective high schools, student and school effort in compulsory school may not increase. It is ultimately an empirical question to determine the academic effects of school choice.

3.2 Empirical evidence on the effects of choice and competition

As schools vary drastically in their characteristics, Hanushek (1995) argues that it is unlikely that centrally set policies are successful in raising school achievement. He argues that performance incentives could be a more viable alternative for lifting achievement. For instance, increased school choice would reward good performance, given that students and parents value such characteristics. Market forces combined with decentralized decision-making could then lead to individual schools figuring out the best way for them to meet performance targets. Several school choice reforms have taken place in recent decades, providing an opportunity to evaluate whether market-based reforms can lift school achievement.

Hoxby (2000) provides early evidence that school choice can incentivize schools and students to improve achievement. Using instrumental variable regressions she finds that more inter-district choice among schools in U.S. metropolitan areas increases average reading and math scores in public schools while lowering per-pupil spending, thus improving school productivity. Hoxby (2003) continues the analysis and reviews three school choice reforms in the 1990s that increased the competitive pressure on public schools—school vouchers in Milwaukee, charter schools in Michigan and charter schools in Arizona. These reforms provided vouchers that students could use to attend an alternative charter school instead of their neighborhood public school. She uses a DiD method with school level data and shows that the public schools most at risk to lose students to charter schools raised students' test scores, suggesting that public schools intentionally improved their performance because of fiercer competition. By using more pre-reform and post-reform years, Chakrabarti (2008) extends the analysis in the Milwaukee case, confirming the main results of Hoxby (2003).

That schools seem to perform better when competition increases is not a U.S. phenomena; there is also support from other school markets. Using school-level data, Bradley and Taylor (2002) show that the introduction of a quasi-market for education in the U.K. increased

competition between schools, leading to better GCSE[5] exam scores, particularly for the schools under most intense competition. Studying the same country, Levačić (2004) finds that headteachers' perceived competition have a positive effect on GCSE results. Filer and Münich (2013) document the development of performance in secondary schools and high schools in the Czech Republic after the fall of communism. Several market reforms took place during this time period and competition between schools increased significantly. The authors find that the state schools facing more competition after the reforms tried to improve their quality by reallocating their resources to classroom instruction and reduction in class sizes.

A potential concern with many studies on school choice is that they only look at short-term effects, such as completing compulsory education in time or results on test scores. After all, long-term effects are most important, and the ultimate purpose of schooling is to generate positive long-term effects. Hence, it is perhaps even more important to evaluate whether short-term effects carry over to the long term. To analyze the short- medium- and long-term effects of a school choice reform in Tel-Aviv, Lavy (2010, 2017) follows students from school choice at the end of 6th grade to outcomes on the labor market after university.

In 1994 students in one of Tel-Aviv's school districts were granted the right to freely choose which one of five middle schools to attend. Previously they were bussed to schools without having any choice. The reform was implemented two years later in other school districts. Lavy (2010) exploits this timing difference to estimate the short- and medium-term effects of the reform. He uses not only a DiD approach, but also a regression discontinuity design, which compares students living on different sides of the district's border. These two approaches yield highly similar results: high-school drop-out rates decreased roughly 35 percent and high-school grades improved by about 10 percent. The mechanisms behind these large effects were better matching between students and schools, more competition among high schools because they needed to attract students, and higher schooling quality. The mechanisms through which choice increased performance are thus consistent with the theoretical justifications for implementing school choice as described previously.

Lavy (2017) investigates the long-term effects of the reform. He studies several long-term outcomes such as employment and earnings, observed almost twenty years after the reform. He finds that school choice increased the likelihood to enroll in post-secondary education by 11 percent. Furthermore, annual earnings after high school increased 6-7 percent. The evidence from the Tel-Aviv reform thus suggests that there can be substanstive long-term gains from introducing school choice.

Several studies try to evaluate the Swedish school reforms that have taken place on the compulsory school level since the 1990s. The following paragraphs summarize their findings. Sandström and Bergström (2005) find that school competition between independent and public schools increased test scores and grades among 9th-graders in public compulsory schools, without an increase in costs. In other words, competition had a positive effect on school productivity. Robustness checks using individual-level cross-section data and munic-

[5] General Certificate of Secondary Education.

ipality panel data allow the authors to rule out other plausible explanations for this effect, grade inflation, for instance. Björklund et al. (2005) point out that a limitation with the study is that the authors only use data from 24 of 288 municipalities, so we do not know whether the results are representative for a larger set of school markets. Another potential limitation is that the authors only use data from 1998, when enrollment in independent schools was still small. It is possible that the effect would change over time.

Since these early studies, the number of independent schools has grown over time, and the most recent research suggest that the school choice reforms in the early 1990s have had positive long-term effects on achievement. Böhlmark and Lindahl (2015) find that it took approximately a decade until independent schools had a positive effect on performance. The mechanisms are external effects from competition rather than independent schools performing better than public schools. This study is perhaps the most convincing to date because it tracks individuals over time, allowing the authors to also study medium- and long-term effects. They also have a much longer time period than other studies—5 pre-reform cohorts and 17 post-reform cohorts. The authors' rich dataset also allows them to test whether more school competition leads to grade inflation. They find no such evidence. If anything, municipalities with more competition from independent schools have lower grade inflation.

Wondratschek, Edmark and Frölich (2013b) find that better opportunities to choose among public schools as a result of the market reform in 1992 have had positive effects on average student performance in 9th grade, albeit with smaller effect sizes than previously thought. Their method is to compare cohorts starting different grades in compulsory school just before 1992 to cohorts starting the same grades just after choice was introduced. As they cannot observe whether students actually made a school choice, they measure choice as the number of schools within commuting distance from a student's home. It is therefore difficult to know whether the effect is due to having the opportunity to choose or actually exercising choice. For example, it may be beneficial to just live in areas with many schools because the threat of switching school can force schools to improve.

Despite documenting positive effects on grades at the end of compulsory school, Wondratschek, Edmark and Frölich (2013b) find only minimal long-term effects. For instance, there is no effect on employment at age 25. Since the effect fades over time, the authors test in the working paper version of their study, Wondratschek, Edmark and Frölich (2013a), whether the increase in grades was due to grade inflation. These robustness checks show that grade inflation only have a small effect on their their main results. The main results thus contrast those of Lavy (2017), who finds positive effects of school choice to middle school on long-term outcomes. A potentially important difference between these studies is that Lavy (2010, 2017) knows which students exercised choice, whereas Wondratschek et al. (2013a, 2013b) only know which students had better opportunities to choose. Edmark, Frölich and Wondratschek (2014) extend the analysis by also studying whether the reform had heterogeneous effects. They find that the effects are the same for students in different socio-economic groups.

The evidence on school choice presented above is mixed. Whereas several studies find positive

short-term effects, the long-term effects are not clear. For example, Lavy (2017) finds positive long-term effects in Israel while Wondratschek et al. (2013a) do not in Sweden. These contrasting results can be due to many factors, including differing data, methods and local contexts. In an overview of the school choice literature, Epple, Romano and Urquiola (2017) conclude that school choice often have substantial positive effects on some subgroups of students, although the aggregate effects are more ambiguous. This means that school choice should be valuable for some students, something worth exerting effort for. In the Stockholm City context, if there are expected benefits with school choice, especially with attending selective schools, students balancing effort costs today with expected future benefits may increase effort as a result of the reform.

Regarding the Stockholm City admission reform, Söderström and Uusitalo (2010) study the effect of the reform on segregation in Stockholm City's high schools. They use four years of data on individual students—two years before the reform and two years after. The segregation measures they use are ability, family background and immigrant status. To estimate the causal effect of the reform on these measures they use a DiD strategy with students attending schools located in Stockholm County's other municipalities as a comparison group.

Söderström and Uusitalo (2010) find that student mobility increased sharply within Stockholm City following the reform. In 1998 55 percent of first-year students attended a high school in the parish they lived in or in any of the adjoining parishes. In 2001, this number had decreased to 37 percent. These changes were driven by high-ability students from poor areas who commuted longer distances after the reform. The reform also increased ability sorting based on compulsory school grades, but a possible caveat is that ability sorting also increased one year prior to the reform, violating the common trends assumption. Nevertheless, ability sorting explains why also segregation on family background increased. Interestingly, segregation between immigrants and natives increased even conditional on previous grades. As expected, the changes in segregation were primarily driven by changes in public schools. The authors point out that the results are interesting because one goal with the reform was to limit the effect of residential segregation on school segregation. While this aim was achieved, segregation on all other dimensions increased.

Söderström (2006) evaluates whether the reform had any effect on high school performance. A key reason why this is likely is because the reform increased competition among high schools. Since there are economies of scale in the provision of schooling, and schools are remunerated on a per-student basis, high schools have incentives to expand or at least to not lose students. To not lose students they would need to keep quality high. He employs a similar DiD method as Söderström and Uusitalo (2010), where the comparison group is students in high schools in other municipalities in Stockholm County. However, he only has data on two cohorts—those who entered high school in Stockholm County 1999 and 2000. Unfortunately, this means that the analysis only includes one post-reform cohort.

The results indicate that students in Stockholm City performed, as measured by high-school leaving grades, no better after the reform. In fact, high-performing students performed slightly worse. These results should be interpreted with caution as he only has data on one

post-reform cohort. The author suggests that the mechanism behind these results is that students cannot choose high schools optimally as school quality is hard to observe. Since the value added by schools and their productivity are unobserved, students often resort to observable measures of input and output, such as student characteristics or grade averages, when deciding which school to attend. It may also be the case that students choose schools based on non-academic criteria.

3.3 Effect of incentives on student performance

Given that the educational process is highly complex, it is difficult for policy makers to know how to improve academic performance and decision-making. Moreover, why children do not invest more in education is puzzling since the return to education is high (Levitt et al., 2016). Koch, Nafziger and Skyt Nielsen (2015) review the emerging fields of behavioral and experimental economics of education, which combine insights from psychology and sociology to better explain student behavior. Among the topics these fields cover are the roles of extrinsic and intrinsic motivation. The authors argue that providing extrinsic motivation in the form of incentives to obtain good grades can be a way to increase childrens' investment in education. Gneezy, Meier and Rey-Biel (2011) discuss the applications when extrinsic incentives are likely and less likely to modify behavior. They note that extrinsic incentives can increase students' motivation to study as they provide immediate returns to effort. These incentives can help mitigate too little effort arising from the fact that the returns to schooling normally occur far ahead in the future. However the authors note that providing extrinsic motivation, for example in the form of monetary payments, may crowd out the intrinsic motivation to study.

Lavecchia, Liu and Oreopoulos (2016) provide an extensive review of the existing empirical evidence of school intervention studies, many of which have focused on providing extrinsic motivation to study. The authors' general conclusion is that school interventions in many cases are a cost-effective way to improve school results. For example, using a randomized experiment on poor performing high schools in Israel, Angrist and Lavy (2009) study the effect of cash incentives on high school performance. Students in Israel take a series of high-stakes subject-specific tests that are used for admission to further education. The results show that cash payments of about USD 1,429 had a positive effect on test scores, but that the effect was concentrated among girls, in particular girls who could have been expected to do relatively well on the tests compared to other girls in the sample. Survey evidence show that increased study preparation was the effect's main channel. The results thus suggest that students can increase effort in response to performance incentives.

Kremer, Miguel and Thornton (2009) evaluate the impact of a merit scholarship program provided to girls in Kenya who performed well on exams in primary school. A scholarship program was randomly assigned to some schools in a set of candidate schools, allowing an estimation of its effect by comparing test scores of program schools and comparison schools. The structure of the program was such that the top 15 percent of students in a school district

received a scholarship of a substantial amount. They find that the program not only had a substantial positive effect on performance among girls, there was also a smaller positive effect on boys' performance. Higher teacher attendance and learning externalities are two possible mechanisms for the effect, supported by the fact that test scores among the lowest performing girls also increased, suggesting that the program also was beneficial for those who were unlikely to gain from it directly. Since boys and low-performing girls attended the same classes as high-performing girls, it is likely that they all benefited from increased teacher attendance.

Bettinger (2012) notes that short-run incentives may have a stronger impact on young children, who tend to be impatient. Therefore, he analyses how cash incentives of up to USD 100 impact test scores in poor schools in Ohio in grades 3 to 6. He finds that cash incentives has a positive effect on math performance, but not on performance in reading, social sciences or science. The effect on math scores was strongest for students who were either among the top performers or among the worst performers. According to surveys of teachers, one mechanism behind the effect was increased student motivation. In contrast to Bettinger (2012), Fryer (2011) finds that short-term financial incentives has no effect on performance in Chicago, New York City and Dallas. For instance, 9th-graders in Chicago were rewarded with up to USD 2,000 per year for their grades in core subjects, but despite these generous payments the students' GPA did not increase.

Fryer (2016) considers instead whether non-financial incentives in the form of information can have an effect on student performance. He conducts a field experiment where students in sixth and seventh grade receive information about the returns to schooling in the form of daily text messages for one year. He finds that this information changes students' beliefs about the return to education, but does not have any effect on short-run test scores. However, effort and long-run achievement improved. The author brings two explanations for this finding. It is possible that students have high discount rates, so they do not think it is worth investing in studies today to reap future benefits. The other explanation is that students do not know how to transform effort into better grades.

In the Swedish context, Jalava, Schroeter Joensen and Pellas (2015) study the impact of non-financial incentives on test performance of 6th-graders in the City of Stockholm. Students are given a math test and are assigned to different treatments, where student performance leads to different rewards, including criterion referenced grades as well as prizes and relative rankings. They find that all rank-based rewards have a positive effect on test-performance, whereas just receiving a letter grade does not affect performance. The effect is stronger for high-performing students and those at the margin, whose increased effort is likely to lead to a reward.

4 Data

4.1 Variables used

I gather the data I use from SIRIS, a publicly available online database on school results and quality from the NAE. Individual schools are responsible for reporting their grade statistics to NAE every year. NAE then combines this data with register data from Statistics Sweden. NAE itself considers the data to be of high quality (NAE, 2017b). Jalava, Schroeter Joensen and Pellas (2015) also use data from this database to construct control variables.

The database contains several different datasets on the national, county, municipality, school and cohort level (e.g. all students in 9th grade at the school). It contains no information on individual students. Not all schools that offer 9th-grade education are included. To be included, schools need to have at least 15 students in 9th grade and information on parents' education for at least 75 percent of these students. Both public schools and independent schools are included in the database. The first year in the database is 1998, which is the first year students graduated under the new grading system. As such, there is one observation per graduating cohort of 9th-graders per year. The variables I use are described below.

GPA is the average final grades of the 9th-graders in a given school. These grades determine admission when applying to high schools in Stockholm City after the reform. The GPA is calculated in the following way. The letter grades IG (fail), G (pass), VG (pass with distinction) and MVG (pass with special distinction) give 0, 10, 15 and 20 points, respectively, per subject. There are 17 subjects, but only a student's 16 best grades are used to calculate the final GPA, which is a sum of the points for the individual courses. Hence, the maximum GPA is 320 points ($20 * 16$). Only the grades given in the final year of compulsory school, 9th grade, are used for calculating the GPA.

Pass is the percentage of the 9th-graders that received at least a passing grade in all 17 subjects. While GPA captures a school's overall performance, the pass rate is more a measure of how low-achieving students perform as it is only affected by students who fail at least one course.

Education is a measure of the average education level of the 9th-graders' parents. The values are as follows: zero points are given if the parent never completed compulsory education. One point is given for completed compulsory education. Two points are given for completed high school. Three points are given for passing at least one semester of university courses within the same subject. Therefore, the value for a given cohort in a given year can vary between zero and three. If, for a given student, data is available on both parents' education level, the average is reported. Otherwise only data on one parent is reported.

Foreign born is the percentage of 9th-graders that are born in a country other than Sweden.

Foreign background is the percentage of 9th-graders who are born in Sweden but whose

parents are both born in a country other than Sweden.

Boys is the percentage of 9th-graders who are boys.

Independent is a dummy variable that takes the value one if the school is run by an independent education provider. It takes the value zero for public schools.

Stockholm is a dummy variable that takes the value one if the school is located in the municipality Stockholm City.

4.2 Descriptive statistics

Table 1 presents descriptive statistics of the variables described above. Means and standard deviations are presented by year (1998-2005) and group: schools in the City of Stockholm and schools in the other municipalities in Stockholm County. The statistics show that the composition of students in schools in Stockholm City is highly similar to that of schools in the surrounding municipalities, suggesting that schools in other municipalities in Stockholm County could potentially be an adequate comparison group.

Student achievement in Stockholm City is higher than in the control group. This may be because students in Stockholm City have more educated parents on average. Apart from grades and education levels, there are minor cross-sectional differences between the two groups. Students in Stockholm City have a slightly higher pass rate. Schools in Stockholm City also have a slightly higher proportion of foreign born students and students with a foreign background. Unsurprisingly, both groups' schools have about 50 percent boys. Standard deviations of all variables are also similar in the two groups. Note that only schools with data for all years are included in the table to enhance year-by-year comparability.

Turning to the time dimension of the table, a few remarks can be made. Average grades increase for both groups, whereas the pass rate decreases. The variability of performance within the groups seems to increase consistently throughout the sample period, as measured by the standard deviations of GPA and Pass. Education levels stay flat in Stockholm City but increase marginally in the comparison group. Both groups experience a decrease in the share of foreign born students and an increase in the share of students with a foreign background. Overall, the two groups have similar trends in all variables.

Table 1: Descriptive statistics

Variable	Group	1998	1999	2000	2001	2002	2003	2004	2005
GPA	Sto.	209.9	213.5	215.5	216.8	218.7	220.5	221.3	219.7
		(23.1)	(24.4)	(25.7)	(27.1)	(29.0)	(28.5)	(27.9)	(32.1)
	Comp.	200.1	202.1	201.7	205.1	205.6	205.4	206.3	208.0
		(17.3)	(18.7)	(21.3)	(21.3)	(21.8)	(22.1)	(23.2)	(22.2)
Pass	Sto.	78.8	78.3	76.2	75.6	75.7	75.7	77.5	75.7
		(13.9)	(13.8)	(14.7)	(15.0)	(15.6)	(14.2)	(14.1)	(16.3)
	Comp.	77.1	74.0	72.1	71.5	71.0	70.9	71.6	73.0
		(12.5)	(13.2)	(14.3)	(13.4)	(13.7)	(15.0)	(15.0)	(14.7)
Education	Sto.	2.28	2.27	2.26	2.27	2.28	2.26	2.27	2.26
		(0.27)	(0.27)	(0.28)	(0.26)	(0.27)	(0.27)	(0.27)	(0.30)
	Comp.	2.11	2.10	2.11	2.14	2.13	2.14	2.15	2.16
		(0.27)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.25)
Boys	Sto.	50.6	51.2	50.5	49.9	49.9	50.7	50.7	51.6
		(7.5)	(7.4)	(6.6)	(6.3)	(6.6)	(6.1)	(7.7)	(6.9)
	Comp.	50.9	51.4	51.5	50.7	51.1	50.6	51.2	51.3
		(5.6)	(7.1)	(5.8)	(7.1)	(7.1)	(6.2)	(7.2)	(6.8)
Foreign born	Sto.	13.1	14.0	13.3	12.7	12.5	13.7	11.5	11.6
		(11.0)	(10.9)	(10.9)	(10.4)	(11.8)	(12.0)	(11.7)	(10.9)
	Comp.	10.4	11.3	10.8	10.2	10.1	9.6	9.2	8.3
		(8.2)	(8.4)	(8.0)	(7.6)	(7.7)	(7.8)	(8.4)	(7.8)
Foreign backg.	Sto.	11.2	10.5	11.7	12.9	12.7	13.5	14.6	15.8
		(10.8)	(10.5)	(12.0)	(13.6)	(11.9)	(14)	(15.2)	(16.1)
	Comp.	11.5	10.9	10.9	12.0	11.9	12.5	12.4	13.1
		(13.0)	(13.0)	(12.5)	(13.9)	(13.1)	(13.6)	(13.6)	(14.6)

Note: The table presents descriptive statistics of the variables used in the study. Standard deviations are in parentheses below means. The table only includes schools that have data for all 8 years 1998-2005. 51 schools in Stockholm City are included, of which 7 are independent. 98 schools in the comparison group are included, of which 3 are independent. Source: Own calculations based on data from NAE.

Table 2, which presents cross-correlations for all the variables in the analysis, largely confirm the differences between the groups discussed above, but also provide additional information. The table shows how the variables co-vary within schools. It is not surprising that academic performance and parents' education levels are strongly positively correlated, whereas the indicators of foreign nationality show a negative correlation with grades. We also see that students in independent schools have higher grades, more educated parents and are more

likely to have a foreign background.

Table 2: Cross-correlations

Variables	GPA	Pass	Inde.	Edu.	F. born	F. backg.	Boys	Stockh.
GPA	1.00							
Pass	0.83	1.00						
Inde.	0.41	0.18	1.00					
Edu.	0.82	0.72	0.26	1.00				
F. born	-0.49	-0.58	-0.02	-0.48	1.00			
F. backg.	-0.37	-0.44	0.13	-0.56	0.46	1.00		
Boys	-0.14	-0.06	-0.12	-0.05	-0.03	-0.06	1.00	
Stockh.	0.25	0.13	0.20	0.24	0.14	0.03	-0.03	1.00

Note: The table presents cross-correlations for all variables used in the paper. The calculations are only based on schools that have data for all 8 years 1998-2005. Source: Own calculations based on data from NAE.

Table 3 shows how many compulsory schools are included in the database per year. The number of public schools and the number of independent schools have increased over time in Stockholm City and the comparison group. This is natural since student cohorts have become larger: the number of 9th-graders have increased significantly, in the comparison group from 11,384 students in 1998 to 14,649 students in 2005, a 29 percent increase. The corresponding figures for Stockholm City are 6,196 students in 1998 to 8,259 students in 2005, a 33 percent increase (NAE, 2017b).

Table 3: Compulsory schools in the dataset

		1998	1999	2000	2001	2002	2003	2004	2005
Stockh.	Public	46	50	52	54	57	58	59	60
	Indep.	8	11	11	11	16	22	24	26
	Total	**54**	**61**	**63**	**65**	**73**	**80**	**83**	**86**
Comp.	Public	108	114	121	126	129	134	137	140
	Indep.	6	8	10	12	18	31	34	37
	Total	**114**	**122**	**131**	**138**	**147**	**165**	**171**	**177**
Total	Public	154	164	173	180	186	192	196	200
	Indep.	14	19	21	23	34	53	58	63
	Total	**168**	**183**	**194**	**203**	**220**	**245**	**254**	**263**

Note: The table presents the number of schools included in the dataset used in the study. Source: Own calculations based on data from NAE.

5 Empirical Strategy

5.1 Difference-in-differences analysis

5.1.1 Difference-in-differences specification

The difficulty in estimating the effect of the reform on compulsory school grades in Stockholm City arises from the fact that there is no clear counterfactual outcome, especially because the grading system changed for the year 1998, so there is no long time series of grades under the new scale for the group itself. One way of overcoming this problem is to compare how grades changed in this group between before and after the reform to how they changed in similar schools in Stockholm County that were unaffected by the reform. The assumption is then that any difference in differences between the two groups can be attributed to the reform.

Therefore, I conduct a DiD analysis using schools that have reported data both before and after the reform. By using the same schools in each period and differencing the variables I control for school fixed effects (see section 13.4 in Wooldridge, 2009). Since I have access to two years of data before the reform (1998 and 1999) and six years of data after the reform (2000 to 2005), I first take the mean of each variable per school separately for the period before and after the reform.[6] Doing so constructs two periods: one before the reform and one after. To illustrate this methodology, call the constructed pre-reform period $t = 1$ and the post-reform $t = 2$, then the model for the two periods is

$$y_{it} = \beta_0 + \delta_0 D_t + \beta_1 reform_{it} + \beta_2 edu_{it} + \beta_3 boys_{it} + \beta_4 f.born_{it} + \beta_5 f.backg_{it} + a_i + \epsilon_{it} \quad (1)$$

for $t = 1, 2$. The model includes the following variables. y_{it} is either the average GPA or the pass rate of the graduating cohort of 9th-graders in school i in period t. D_t is a dummy variable for the treatment period, taking value 0 for $t = 1$ and value 1 for $t = 2$. $reform_{it}$ is a dummy variable taking value 1 if schools are affected by the reform and 0 otherwise. Therefore, this variable will take value 1 in period $t = 2$ for schools located in Stockholm City. edu_{it}, $boys_{it}$, $f.born_{it}$ and $f.backg_{it}$ are different control variables on the school-cohort level in period t described in section 4.1: parents' average level of education, percentage of boys, percentage of foreign born students and percentage of students with a foreign background in the cohort, respectively. a_i is an unobserved school fixed effect, which consists of unobserved variables that are constant over time but have an effect on the level of y_{it} that is assumed to be constant over time. This can include the school's management, facilities, teachers and the fact that some schools are run by independent education providers. ϵ_{it} is an error term for school i in period t, consisting of unobserved variables also affecting y_{it}. Differencing (1)

[6] For example, consider a school that has data on all variables for all years. Then I first calculate the mean of each variable, say GPA, for the first two years (before the reform), and then the mean for the last six years (after the reform). As a last step I construct the variable ΔGPA by taking the difference between the two means.

to remove a_i gives

$$\Delta y_i = \delta_0 + \beta_1 \Delta reform_i + \beta_2 \Delta edu_i + \beta_3 \Delta boys_i + \beta_4 \Delta f.born_i + \beta_5 \Delta f.backg_i + u_i, \quad (2)$$

where $u_i = \Delta \epsilon_i$ and consists of unobserved variables assumed to behave like random noise affecting Δy_i. The other variables are just the differenced versions of those in (1). Note that $\Delta reform_i$ will take value 1 for schools in Stockholm City and 0 otherwise. Parameters to estimate in (2) include δ_0, β_1 (the parameter of interest), β_2, β_3, β_4 and β_5. I estimate equation (2) with OLS.

To show that (2) is a DiD equation, note that the estimate of δ_0, the intercept, will be the estimated mean difference in achievement for the control group after controlling for the effect of other variables. Call this difference $\overline{\Delta y}_{control}$. Similarly, the estimated mean difference for the treatment group, schools in Stockholm City, after controlling for other variables will be $\delta_0 + \beta_1$, which can be denoted $\overline{\Delta y}_{treat}$. The difference in differences between treatment group and control group after controlling for other variables is $\overline{\Delta y}_{treat} - \overline{\Delta y}_{control} = \delta_0 + \beta_1 - \delta_0 = \beta_1$. Therefore, if the estimate of β_1 is statistically significantly different from zero, there is a difference in differences between treatment group and control group after controlling for other variables (Wooldridge, 2009). The estimate of β_1 will be a measure of the effect of the reform on Δy_i for schools in Stockholm City if we assume the two groups would have followed identical trends in the absence of reform. This is a strong assumption and it is discussed in the following sections.

5.1.2 Treatment period and treatment group

It is important to define both the treatment period and the treatment group when conducting a DiD analysis. I define the treatment period as the year 2000 onward, because this corresponds to the years with the new admission selection mechanism to public high schools in Stockholm City. As noted previously, the official decision of changing the admission rules was taken on October 19, 1999 (Wennerholm, Karlberg & Olsson, 1999). Therefore, the first cohort of 9th-graders treated in this setting were those graduating from compulsory school in Stockholm City the summer 2000. They could freely rank school-program combinations and were admitted solely based on grades. One possible caveat to this definition of treatment period would be, for example, if the plan had been to implement the change one year earlier, but the implementation was delayed. However, City of Stockholm (1999) provides background information about the reform, and there seems to be no reason to believe that the plan had been known for a long time. In the municipality's 1999 budget, the city council's department of education had been tasked with researching possible changes to the admission rules, and on September 17, 1999, it proposed the admission changes for the city council to vote on. Students in Stockholm City should therefore have been notified of the reform at around this time, so 9th-graders knew about the new rules in the beginning of the academic year. Therefore, I refer to the years 1998 and 1999 as the pre-treatment period. Students graduating these years applied for high schools using the old system, with no school choice conditional on compulsory school grades.

I define the treatment group as compulsory schools in the municipality Stockholm City. Conversely, the control group consists of compulsory schools located in other municipalities in Stockholm County. One underlying assumption in doing so is that students attending compulsory schools in Stockholm City also reside in this municipality and vice versa. Unfortunately there exists no data to verify that this assumption holds during the sample period. The earliest such data available is for the academic year 2008/2009. In this year, 9 percent of students in compulsory schools in Stockholm City was not registered as residents of the municipality. However, this is primarily due to independent schools. Only about 3.6 percent of students in public schools did not reside in Stockholm City. It is more rare for residents in Stockholm City to attend schools in another municipality. If all the municipality's outgoing students attended schools in the comparison group, they would make up around 1.3 percent of the total number of students in the comparison group, and 1 percent of the students in public schools. The corresponding figures for the period under study are likely lower because independent schools have become more common over time and education markets more integrated. Furthermore, these figures are likely slightly inflated because of instances where students have moved to another municipality but residence have not yet been recorded. Overall, it seems reasonable to assume students residing in Stockholm City attend compulsory schools there whereas students residing in other municipalities do not.

Another central assumption in the analysis is that the reform did not have an effect on the importance for students in the control group to obtain good grades. If so, there would be spillover effects of the reform to students in other municipalities. In other words, the incentives to get better grades should only increase in schools located in Stockholm City. Since the municipalities in the region are so connected, there is a risk that the reform affected at least some students in the comparison group. It is therefore important to evaluate whether this assumption is reasonable.

As described earlier, there are rules regarding under what conditions students from other municipalities can be admitted to public high schools in Stockholm City. If a student's home municipality offers a particular high school program, the student can only be admitted to that program in public high schools in other municipalities if there are slots left after all locals have been admitted. The SIRIS database has data on what programs are offered in the different municipalities, so it is possible to approximate the extent of spillover problems.

In 1999 and 2001, around 8 percent of first-year students in university-preparing programs in public high schools in Stockholm City were residents in other municipalities (USK, 2002).[7] Of the 25 municipalities in the comparison group, 22 offered these programs themselves. This means that students from these 22 municipalities could only be admitted to those programs in Stockholm City if there were slots over, i.e. regardless of their grades, both before and after the reform. The three other municipalities did not have any public high school with ordinary programs during the sample period (they are small municipalites).[8] It is therefore possible that students in the three municipalities could apply to schools in Stockholm City on equal terms as local students if the municipalities did not have an agreement with other

[7] Not necessarily only from the comparison group, but probably almost only from this group.
[8] Ekerö, Nykvarn and Vaxholm.

municipalities. As I do not know which agreements were in place, I perform a robustness test where I exclude from the analysis compulsory schools in these three municipalities.

Residents in other municipalities made up around 21 percent of first-year students in Stockholm City's other programs (USK, 2002). Because there are around 16-18 such programs, which are mostly vocational, the analysis becomes more complicated. Most municipalities offer around 8-10 of these programs themselves (Stockholm City around 15). Under the assumptions that all non-Stockholm City residents attending these programs were from the comparison group and that they all were admitted on the same terms as locals, that number of students would amount to 8 percent of the graduating 9th-graders in the comparison group. Since at least a few students likely are from municipalities other than the comparison group and that some probably are from municipalities offering the program themselves, any spillover effects should be marginal.

Furthermore, since two of the three municipalities mentioned above adjoin Stockholm City, a significant fraction of the non-local students in Stockholm City is probably from these two municipalities. Excluding those from the analysis reduces potential bias further. Additionally, 11 of the 14 vocational programs in Stockholm City are offered by 3 high schools or fewer (NAE, 2017b). Therefore, the reform did not make the admission process for these programs that more competitive since you still had to be admitted to the program before the reform. In general these programs are less popular and easier to be admitted to than the university-preparing programs (USK, 2002).

In sum, there is little reason to believe that the reform also made grades more important for students in the comparison group. For the group that is expected to drive the results—those who want to apply to popular university-preparing programs in Stockholm City—the reform only affected students in Stockholm City, except for the three municipalities that did not offer any high school programs themselves. I will therefore also present results that exclude these municipalities. Thus, it is not a big concern that the reform may have affected some students in the comparison group, as they make up a minimal fraction of the total number of students in the comparison group.

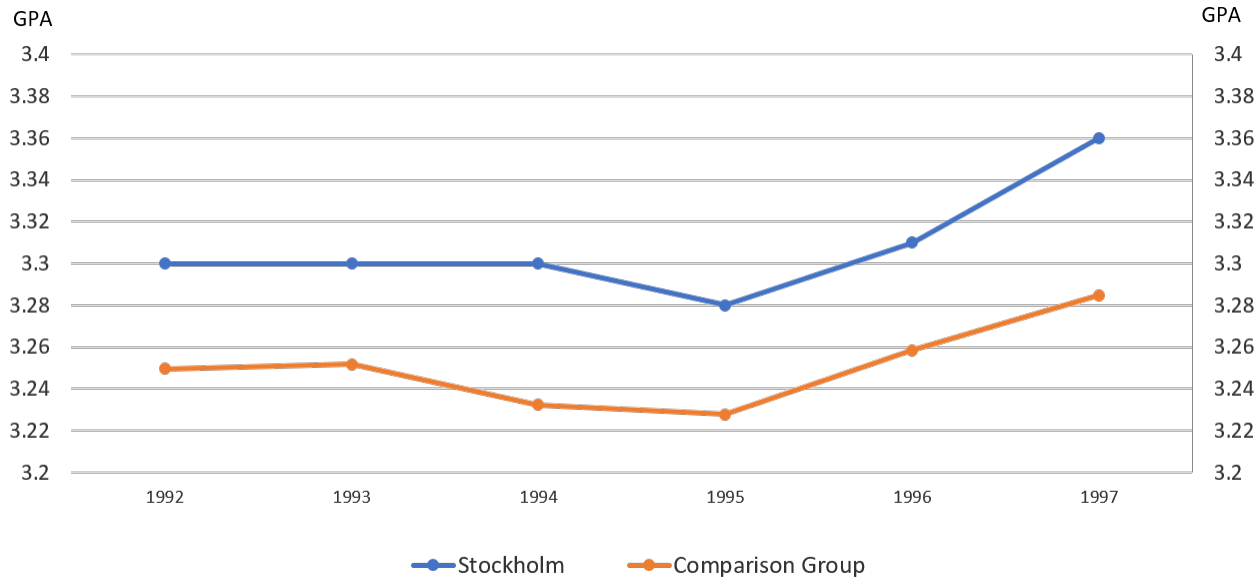
In the case that spillover effects bias the estimates, it would be a bias toward zero, since the concern is that some students in the comparison group could have been incentivized as well, while some students in Stockholm City from other municipalities may not have been incentivized. This integration of markets could reduce the effect on grades in Stockholm City schools and increase the effect on grades in schools in the comparison group, making the estimated difference-in-differences between them smaller than would have been the case had there been no overlap. This is important to keep in mind when interpreting the results.

5.1.3 Pre-reform trends

A central assumption in any DiD analysis is that treatment group and control group follow the same pre-treatment trends. If this assumption is violated, the estimated treatment effect

will be biased. Differences in changes in the outcome variable will incorrectly be attributed to the treatment, even though the variables just follow their individual pre-treatment trends. Unfortunately, grade statistics from individual schools are only available from 1998 onward. Before 1998, students were graded 1-5 using a different system, as described in section 2.[9] However, public schools' average grades at the municipality level are available together with the total number of enrolled 9th-graders. It is therefore possible to construct a weighted average of grades in the control group's 25 municipalities. This measure serves as a proxy for school-level data as municipalities with many students also have more schools. The first year of data is 1992, so I can investigate how achievement in 9th grade has developed in the control group compared to Stockholm City six years before the change of grading system.

Figure 4 illustrates how grades developed in Stockholm County 1992 to 1997 for Stockholm City and the comparison group. Students in Stockholm City consistently achieve better grades than their peers in the county's other municipalities. As shown in table 1 this was also the case under the new grading system. Most importantly, the two groups' trends are strikingly similar: grades tend to change in the same direction and with the same magnitude. Therefore, figure 4 provides evidence of parallel pre-reform trends. There are some caveats though. Data from independent schools are not included. However, enrollment in these schools was still very small (a few percent) so even if their trends differed from those of public schools, the groups' overall trends would remain relatively unchanged.



Source: Own calculations based on data from NAE

Figure 4: Pre-trends in GPA

[9] I asked NAE if grades from the old scale could be transformed to the new scale. They told me this is not possible.

5.2 Heterogeneous effects

The evidence from studies on performance incentives suggests that more able students benefit more (Gneezy, Meier & Rey-Biel, 2011; Jalava, Schroeter Joensen & Pellas, 2015; Angrist & Lavy, 2009; Kremer, Miguel & Thornton, 2009). This seems intuitive when rewards are based on students' relative performance: low-ability students have a much lower chance of winning, so may not find it worthwhile to exert more effort in an attempt to increase their grades. In the present application high-ability students also have more to gain as school choice expanded differently for students interested in different programs and who differed in ability. As mentioned earlier, in practice, the reform expanded school choice most for those interested in attending the university-preparing programs, for which admission is most competitive (USK, 2002). As a result, it is expected that the effect of the reform on these students' compulsory school grades is larger than the effect on grades of students more interested in vocational programs. In other words, the reform probably had heterogeneous effects.

Given the data available, one way to study whether the reform had heterogeneous effects is to use an exogenous variable to split the sample between those who could be assumed to be more and less affected by the reform. I do this using the variable for average parental education levels. Parents' education can be treated as exogenous with respect to the reform since it is pre-determined—most parents have finished formal education when their children are 15 to 16 years old. As shown in table 2, cohort-level GPA has the strongest association with the measure of parents' average level of education with a correlation of 0.82 between them. The reform could be expected to have a stronger effect on students whose parents have high education as these students on average probably have more interest (and possibly higher ability) to attend competitive, university-preparing high school programs.

There are several potential ways of splitting the sample based on parents' education levels. I split it separately for the control and treatment groups. First, I calculate each school's average value of the education variable using all the observations available for the school. Then I calculate the median of the schools' average value separately for the treatment group and the control group. Next I assign schools in the treatment group whose average value of the education variable is above the group's median to the high-education group. I do the same for the control group: schools in the control group whose average value of the education variable is above the group's median are assigned to the high-education group. The other schools are assigned to the low-education group in the respective treatment and control group.

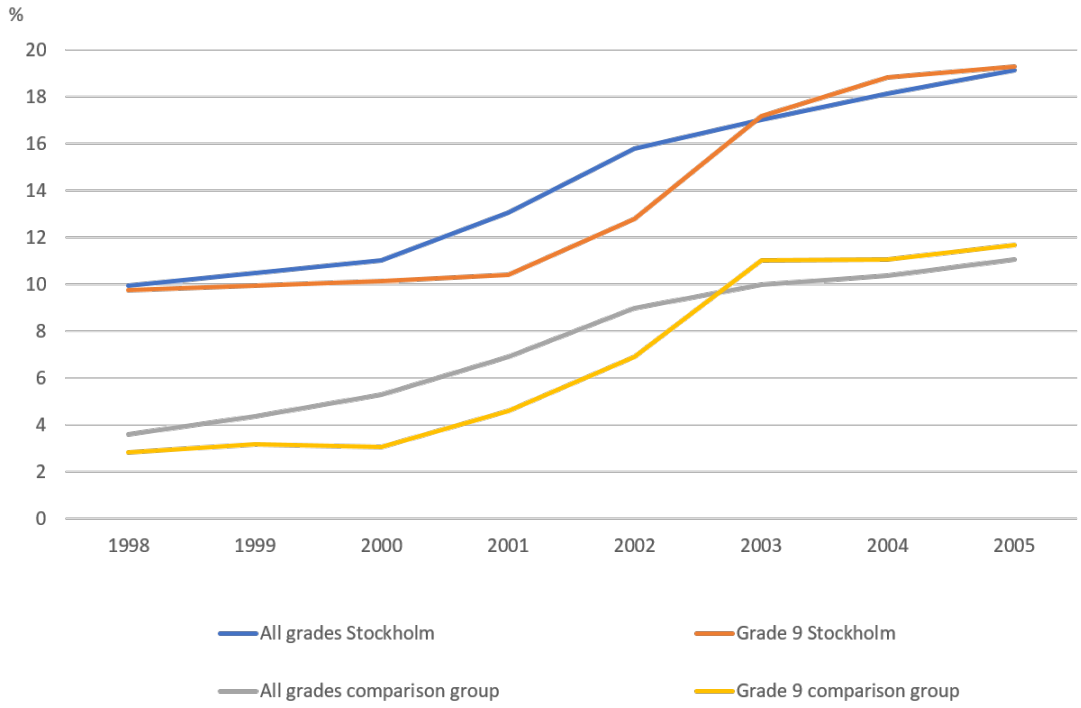
This approach has the advantage that the low education group and the high education group will of be equal size, and the proportion of treated schools will be the same in the two groups. The approach entails that schools with relatively high values for parents' education in Stockholm City will be compared to those with relatively high values in the control group. Similarly, schools in the low-education group in Stockholm City are compared to schools in the low-education group in the control group. I estimate equation (2) separately for the high-education group and low-education group.

5.3 Robustness and alternative explanations

Given the potential issue with slow-moving confounding trends, it may be unreliable to attribute to the reform changes in grades up to six years after the reform. Moreover, it is possible that some students switched compulsory school because of the reform. If so, the reform would also affect the control variables because of changes in student composition, making it unclear how to interpret the results. In an attempt to limit such potential bias, I perform a robustness check where I exclude the last three years from the sample used. This means that only the first three years after the reform are used to construct the post-reform variables.

The motivation for specifically using the first three years after the reform is that only those who had just started 9th, 8th and 7th grades at the time of the announcement of the reform will be included in the sample. Some degree of school choice exists between 6th and 7th grade as there are many compulsory schools that do not teach the last three years of compulsory school as they are too small. Students in these schools must choose a different school when starting 7th grade. When compulsory school grades became more important for high school applications, it is possible that they took compulsory schools' grade averages more into account when deciding which school to attend. Using only the post-reform years 2000, 2001 and 2002 thus restricts the sample to include only those who had already made their compulsory school choice at the time of the reform. This sample period restriction then ensures that the choice of compulsory school is exogenous with respect to the reform.

As figure 5 shows, the market shares of independent compulsory schools (measured as percentage of total enrollment in Stockholm City and the comparison group) increased during the sample period, albeit from relatively low levels. This can have an effect on competition, leading to higher or lower grades in public schools. However, note that the market shares increase by about 8 percentage points in both Stockholm City and the comparison group. If any such competition effects are linear, they would be the same for both groups and not introduce any bias. However, it could be the case that competition has a non-linear effect on grades, and only kicks in once enrollment in independent schools reaches a certain threshold. Using fewer years after the reform could limit this potential source of bias.



Source: Own calculations based on data from NAE

Figure 5: Market shares independent schools

6 Results

6.1 Difference-in-differences estimates

Table 4 presents the results of an estimation of equation (2). Column 1 shows results with ΔGPA as outcome variable without including any control variables. The estimated intercept shows that average GPA has increased for the comparison group between the pre-reform period and the post-reform period, as was also suggested by the descriptive statistics. The estimated coefficient on $\Delta Reform$ is weakly significant at the 10 percent level, and the point estimate suggests a positive effect of the reform of about 3 grade points on average GPA in Stockholm City.

The specification in column 2 also uses ΔGPA as outcome variable but includes the control variables in the regression. The coefficients on the control variables show the expected signs. For example, the coefficient on change in parents' education level is positive, whereas the coefficient on change in share of foreign born students is negative. Interestingly, the estimates suggest that the change in percentage of boys in the cohort has a stronger negative effect on ΔGPA than has the change of percentage of foreign born students in the cohort. However,

estimates of coefficients on control variables do not necessarily have a causal interpretation.

Relative to the results in column 1 we see that the estimate of the effect of the reform is more significant (p<0.01) in column 2 and the effect size is slightly larger. Furthermore, the magnitude of the intercept is smaller. Taken at face value, the estimates suggest that the reform increased average GPA in Stockholm City's schools with about 4.3 grade points. This is a small, but not negligible effect. Five grade points correspond to the average student in the average school achieving a better grade in one of the 16 subjects, by for example going from pass to pass with distinction. The magnitude of the effect can be compared to that of other variables. For instance, decreasing the proportion of foreign born students in the cohort by 10 percentage points would have a roughly similar effect on a cohort's average GPA, according to the estimates.[10] Such comparisons should of course be taken with a grain of salt, since, again, coefficients on control variables may not have a causal interpretation.

Table 4: Difference-in-differences estimates

	(1) ΔGPA	(2) ΔGPA	(3) ΔPass	(4) ΔPass
Intercept	3.713***	2.338**	-4.053***	-5.192***
	(0.920)	(0.967)	(0.675)	(0.739)
ΔReform	3.010*	4.279***	0.805	1.493
	(1.814)	(1.622)	(1.420)	(1.229)
ΔEdu.		24.03**		16.87**
		(10.73)		(6.679)
ΔBoys		-0.542***		-0.270***
		(0.146)		(0.0998)
ΔF.born		-0.427***		-0.463***
		(0.147)		(0.110)
ΔF.backg.		-0.0840		-0.0276
		(0.194)		(0.149)
R^2	0.017	0.218	0.002	0.191
N	182	182	182	182

Note: The table presents regression results based on equation (2) with data from 1998-2005. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns 3 and 4 show the results from the same specifications but with the change in the pass rate as outcome variable. The intercepts suggest that average pass rates decrease between the two periods for the comparison group, a trend that was also observed in the descriptive statistics. The point estimate of the coefficient on $\Delta Reform$ is positive in both columns,

[10] The coefficient on $\Delta F.born$ is approximately -0.43 in column 2 and the variable is measured in percent.

but is far from being statistically significant (p=0.23 in column 4). We can therefore reject that the reform had an effect on the pass rate. Thus, the estimates suggest that treatment group and control group experience the same change in the pass rate. Unsurprisingly, the estimated coefficients on control variables show the same signs as when using ΔGPA as outcome variable.

Moreover, in interpreting the results, it is important to keep in mind the potential bias towards zero arising from the slight overlap in education markets between Stockholm City and the comparison group discussed in section 5.1.2. The effect sizes reported may therefore be slightly understated. As mentioned earlier I also present results excluding the three municipalities where spillover problems are most likely. Those results are presented in table A1 in the appendix. The estimated coefficients on $\Delta Reform$ reported there are almost identical to the ones reported in this section, providing some reassurance that including the three municipalities does not cause spillover problems. I will therefore focus on estimations where these municipalities are still included.

6.2 Heterogeneous effects

Table 5 presents the results of equation (2) with the sample split between schools where parents have low and high education levels. Columns 1 and 2 show results for schools where parents have relatively high education and relatively low education, respectively, with ΔGPA as outcome variable. Columns 3 and 4 follow the same exposition but use $\Delta Pass$ as outcome variable.

With ΔGPA as outcome variable, as in columns 1 and 2, it can be inferred that grades appear to have increased in the comparison group in schools where parents have relatively high education, but not where they have relatively low education, as shown by the intercepts. The coefficient on $\Delta Reform$ is significant at the 1 percent level for the high-education group but not is not significant and approximately 0 for the low-education group. Furthermore, the coefficient on $\Delta Reform$ is larger for the high-education group than when the combined sample was used, as in column 2 in table 4. These estimates thus suggest that schools in Stockholm City where parents have relatively high education drive the aggregate effect of the reform on grades that was reported in table 4.

There is weaker evidence of an heterogeneous effect of the reform on the pass rate, as shown in columns 3 and 4, where the coefficient on $\Delta Reform$ is significant at the 10 percent level for the high-education group, but is not significant and close to 0 for the low-education group. The point estimate suggests that the reform had a positive effect on the pass rate of about 2.4 percentage points in schools where parents have relatively high education. As shown by the intercept, the average pass rate decreases for the control group during the sample period, both for schools where parents have relatively high education and where they have relatively low education, but it decreases less in the former group.

Table 5: Heterogeneous effects

	(1) ΔGPA High edu.	(2) ΔGPA Low edu.	(3) ΔPass High edu.	(4) ΔPass Low edu.
Intercept	4.200***	-0.0255	-3.558***	-7.122***
	(1.314)	(1.322)	(0.884)	(1.117)
ΔReform	7.836***	0.0278	2.377*	0.121
	(1.932)	(2.576)	(1.305)	(2.273)
ΔEdu.	25.68	25.83**	14.27	21.45***
	(18.38)	(11.37)	(9.497)	(8.135)
ΔBoys	-0.617***	-0.553***	-0.274**	-0.297*
	(0.219)	(0.185)	(0.106)	(0.172)
ΔF.born	-0.454**	-0.373*	-0.539***	-0.400**
	(0.206)	(0.207)	(0.139)	(0.181)
ΔF.backg.	-0.0540	0.191	-0.177	0.213
	(0.322)	(0.277)	(0.219)	(0.240)
R^2	0.306	0.234	0.237	0.197
N	91	91	91	91

Note: The table presents regression results based on equation (2) with data from 1998-2005. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.3 Robustness and alternative explanations

As a final robustness check I also present results that omit the last three years of data when constructing the variables for the post-reform period. As discussed in section 5.3, limiting the post-reform sample period has two potential benefits and but also drawbacks. First, the sample will include only those students who started 7th to 9th grade at the time of the reform. These students had already chosen which school to attend for their last three years in compulsory school when the decision to reform the system was taken (fall 1999). As mentioned earlier, some degree of school choice exists between grades six and seven. This sample period restriction then ensures that the choice of compulsory school is exogenous with respect to the reform. The second argument for using a restricted sample period is that the number of independent schools increases considerably in both groups between 2002 and 2003 (see table 3). Additionally, as shown in figure 5, the market shares of independent schools, measured as percentage of enrolled students, reach relatively high levels toward the end of the sample period, potentially having an effect on the competition for students. However, one potential downside of using a restricted sample period is that if it takes time for the reform to have an effect, then the sample period may be too short to capture it.

Table 6 presents the results of a re-estimated equation (2) that restricts the post-reform sample period to three years, 2000-2002. As before, columns 1 and 2 use ΔGPA as outcome variable whereas columns 3 and 4 present corresponding results with $\Delta Pass$ as outcome variable.

Table 6: Difference-in-differences estimates short sample period

	(1) ΔGPA	(2) ΔGPA	(3) ΔPass	(4) ΔPass
Intercept	2.675***	1.885**	-4.230***	-4.621***
	(0.913)	(0.878)	(0.683)	(0.713)
ΔReform	2.308	2.466	0.563	0.866
	(1.690)	(1.556)	(1.403)	(1.252)
ΔEdu.		23.13**		10.07
		(10.56)		(8.168)
ΔBoys		-0.429***		-0.170*
		(0.128)		(0.0879)
ΔF.born		-0.303**		-0.412***
		(0.148)		(0.121)
ΔF.backg.		0.122		-0.148
		(0.208)		(0.185)
R^2	0.011	0.167	0.001	0.128
N	182	182	182	182

Note: The table presents regression results based on equation (2) with data from 1998-2002. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The coefficients on control variables reported here are similar to those reported in table 4. However, the coefficients of interest, those on $\Delta Reform$, are smaller and not significant. For example, in column 2 with ΔGPA as outcome variable, the point estimate suggests an effect of the reform of about 2.5 grade points (p=0.12), compared to about 4.3 when the whole sample period was used. The coefficients on $\Delta Reform$ are again not significantly different from zero when using $\Delta Pass$ as outcome variable and are smaller in magnitude than when the full sample period was used.

The results of this sample period restriction therefore provide no statistically significant evidence of a reform effect. It is difficult to know why the last three years appear to drive the aggregate effects observed when using the full sample period. One explanation elaborated upon earlier could be that more independent schools enter the market in later years, having an effect on the competition for students that differs between treatment group and control group, forcing existing schools to either improve or to inflate grades to keep students from choosing a new independent school. This study cannot disentangle whether this is actually

the case, so more evidence is needed to determine whether the entry of independent schools in the market have an effect on grades in other schools. Clearly, the main results presented in table 4 should be interpreted cautiously.

7 Conclusion and discussion

This paper explores the effects on compulsory school grades after they became more important for high school admissions. This is an important question to analyze since one motivation for reforming the admission system in the City of Stockholm, thereby making compulsory school grades more important, was to incentivize students to obtain better compulsory school grades (City of Stockholm, 1999). The question is also relevant for policy makers in other countries who consider implementing a similar reform, which links school choice to previous academic achievement.

Moreover, the paper is also relevant with respect to the issues currently discussed in the literature on the economics of education, since a puzzle in this field of research is why students do not invest more effort in education, even though the returns are high (Levitt et al., 2016). The difficulty for students to take into account the long-term consequences of schooling is one hypothesis discussed (Fryer, 2016). Several authors have therefore tried to remedy the problem of students' present bias by incentivizing students through providing immediate returns to academic achievement, often in the case of rewards for good grades or test scores (Gneezy, Meier & Rey-Biel, 2011). Viewed in this context, the admission reform in Stockholm City can be seen as a way to provide immediate, tangible rewards in the form of more school choice for those performing well in compulsory school. Studying this reform thus offers an opportunity to get some insight into what the aggregate effects could be if performance incentives are implemented in a school system.

The results presented in this paper provide some evidence that the new selection mechanism to public high schools in the City of Stockholm had a positive effect on grades in compulsory school. The results indicate that it took time for the reform to have an effect, but this finding means that we should be careful with drawing too strong conclusions based on the results since there may have been other trends, for example relating to independent schools, that could have a stronger effect on schools in Stockholm City than on schools in the comparison group. Moreover, the effect seems to be stronger when using a measure of average school performance (grades) instead of one that better captures achievement among low-performing students (pass rate) and driven by schools where parents have relatively high education, providing some evidence of heterogeneous effects.

There are several possible explanations for why the aggregate effect of the reform appears to be modest, according to this paper corresponding to slightly less than on average achieving a better grade in one of 16 subjects used to calculate GPA in 9th grade. Perhaps students do not find attending prestigious or selective schools desirable or worth the effort. As discussed in section 3.1, choosing what school to attend is difficult since school quality is to a large

extent unobserved (MacLeod & Urquiola, 2012). If a large fraction of students do not consider school choice beneficial, they will not exert more effort to increase the chances of being admitted to selective schools. Whereas this explanation is plausible, another potential reason worth emphasizing is the nature of the data used in this study. Using school level data does not take into consideration that schools differ in size. If the reform did not incentivize a large fraction of students, the students affected need to raise their grades considerably to have a distinguished effect on the cohort-level average. Clearly, using individual level data would make the estimated aggregated effects more precise and also allow for a more detailed analysis of heterogeneous effects, as using school-level data may mask potentially large such effects. As a result, the results presented in this paper should be interpreted cautiously.

The results also indicate that it took time for the reform to have an effect. The robustness check with a shorter post-reform sample period suggests that there was no effect during the first three years after the reform. There are several potential explanations for this finding. One explanation could be that students graduating in later years knew about the new selection mechanism for a longer period. They could study more in earlier grades to be better prepared for 9th grade. Furthermore, schools may also have become more focused on raising student achievement over time. As several authors have pointed out, the educational process is slow and complex, making it hard to predict how schools respond to a changing environment (Epple, Romano & Urquiola, 2017). As the effect appears to to have increased over time the results presented here support this, even if the exact mechanisms cannot be pinned down. However, one cause of concern and potential alternative explanation is that the number of independent schools in the market increased considerably during the sample period, reaching relatively high levels, especially in Stockholm City, during the later part of the sample period (2003-2005). This may be a reason why the effect appears to increase over time. For example, competition for students may become more fierce in Stockholm City, forcing public schools in the municipality to improve. This has previously been observed in the U.S. (Hoxby, 2003). One assumption in the analysis is therefore that this effect is not present, or at least not differs between the comparison group and Stockholm City. Future research with access to individual-level data may be able to determine whether this assumption holds.

As in many other studies on school choice reforms (Epple, Romano & Urquiola, 2017), the results indicate heterogeneity in the response to the reform. The results suggest that achievement rose more in schools where parental education levels were relatively high. Parents' education levels may impact how students respond to the reform, for example because relatively well educated parents could be more aware of the returns to education, making their children more aware of the potential benefits of choosing a good school. For this reason children of highly educated parents could also be expected to be more interested in attending the university-preparing programs, to which school choice expanded the most and admission is most competitive (USK, 2002). Therefore, students whose parents have relatively high education could be expected to benefit most from the reform. For these reasons children to relatively well educated parents may be more willing to study more to achieve higher grades and increase their chances of being admitted to a preferred school.

The heterogeneous results reported in this study are in line with those of other studies. For example, Jalava, Schroeter Joensen and Pellas (2015) find that providing non-financial incentives has a stronger effect on effort on (Swedish) high-performing 6th-graders than their low-performing peers, particularly when the incentive includes an element of relative ranking between students. These findings are in line with Kremer, Miguel and Thornton (2009) and Angrist and Lavy (2009) who also find that extrinsic motivation in the form of prizes given based on relative performance increase student achievement among those who could be expected to benefit most, i.e. high-performing students. The evidence of heterogeneous effects presented in this paper therefore adds further evidence to the body of research suggesting that heterogeneous effects are likely when rewards are based on relative performance.

Given that one goal of the reform was to raise student achievement in compulsory school, it is important for policy purposes to determine whether this goal was achieved. The results indicate that grades rose, but mainly among students with relatively highly educated parents. For policy makers considering implementing a similar reform, it is important to stress that this paper only indicates what *can* happen if a similar reform is implemented under similar circumstances. For example, this reform did not change the system from zero choice and no importance of grades to a system with full choice conditional on grades. Recall that before the reform, students were still admitted to programs based on grades and could be admitted to a school outside their catchment area if there were slots left. Reforms where the baseline is no school choice and no importance of grades would likely have a larger effect on compulsory school grades.

Despite the local context of the reform, a potentially important lesson for policy makers who consider implementing a similar admission reform is the need to take into account dynamic effects with respect to heterogeneity. For example, a policy maker may be interested in the segregational effects before deciding whether to implement the reform, as interschool segregation on ability can be expected to increase on the high school level. When modelling these effects it will be important to recognize that students with relatively highly educated parents may increase their grades more than other students. Söderström & Uusitalo (2010) showed that segregation on ability increased across high schools following the reform. It would be interesting to know how much of this was because of mechanical sorting and how much was due to these dynamic effects. With mechanical sorting I mean the sorting that would naturally occur when school admissions are based on previous achievement. With dynamic effects I mean that some sub-groups of students, those with relatively highly educated parents in this case, intentionally increase their grades more than others in order to be admitted to their preferred schools. It would be difficult to separate these mechanisms in practice, however, as normally only the combined effect on sorting would be observed.

To conclude, an important motivation for implementing the reform was to incentivize students to study more and obtain better grades in compulsory school (City of Stockholm, 1999). The results of this study indicate that the reform on aggregate had a small, positive effect on grades in compulsory schools. Tying school choice to previous achievement may therefore be a a way to raise some students' achievement. However, these findings should

be interpreted cautiously as it seems like it took time for the reform to have an effect, raising questions about whether there are uncontrolled other trends that also have a positive effect on grades in Stockholm City. Nevertheless, the positive effect on grades seems to be stronger among students with relatively highly educated parents, suggesting that the reform had heterogeneous effects in compulsory school.

Due to data limitations this paper mainly focuses on aggregate effects and provides only suggestive evidence. This means that future research could make several improvements to this study. For example, future research with access to individual-level data could seek to verify the assumptions this paper relies on, such as the effects of independent schools and the composition of students in the two groups. On a more general level, future research may want to explore in more detail the effects of competition among students on academic achievement.

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Appendix

Table A1: Municipality robustness: Difference-in-differences estimates

	(1) ΔGPA	(2) ΔGPA	(3) ΔPass	(4) ΔPass
Intercept	3.747***	2.287**	-4.233***	-5.448***
	(0.941)	(0.991)	(0.685)	(0.747)
ΔReform	2.976	4.309***	0.984	1.716
	(1.825)	(1.637)	(1.425)	(1.232)
ΔEdu.		24.42**		16.94**
		(10.74)		(6.636)
ΔBoys		-0.532***		-0.277***
		(0.148)		(0.101)
ΔF.born		-0.423***		-0.471***
		(0.148)		(0.110)
ΔF.backg.		-0.0780		-0.0193
		(0.194)		(0.149)
R^2	0.017	0.215	0.003	0.200
N	178	178	178	178

Note: The table presents regression results based on equation (2) with data from 1998-2005. Schools located in the municipalities Ekerö, Nykvarn and Vaxholm are excluded from the regressions. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$