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An empirical study of the 2021 Akershus/Viken admission reform on middle school and upper secondary school average grades.

Graduate thesis in Economics
Supervisor: Colin Peter Green
June 2024

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## Abstract

The subject of different admission systems for upper secondary school has been a topic of debate in Norway for an extended period. There is disagreement among politicians about what system is most beneficial for students, with part of the discussion concerning student attainment. In the school year 2021/22, students in Akershus going from middle school to upper secondary school faced a change in the admission system. The region had gone from grade-based admission to residence-based admission. This admission reform affected students' school choice by reducing the number of schools each student is eligible to apply to, based on geographical proximity in addition to grades. The reform provided a unique opportunity to analyse the short-term effect of such a change on student attainment.

In this master thesis we have utilised the admission reform to look at its effect on average grades from middle schools and upper secondary schools in the region of Akershus. Our data is gathered from both before and after the reform, and for both treated and untreated schools. We perform a Difference-in-differences regression analysis to investigate the effect of the policy change on grades. One of the motivations for the reform was to minimise the difference in grades between higher and lower performing schools. We therefore also perform a Difference-in-difference-in-differences regression to investigate a potential redistributive effect of the reform.

Our results show a negative effect on grades in middle school, which is in line with previous Norwegian research on admission reforms to upper secondary schools. We also find indications of a negative effect on grades in upper secondary schools, but no indications of redistributive effects for the achievement gap between higher and lower performing schools. This contradicts earlier findings on how admission systems affect between-school segregation.

## Sammendrag

De forskjellige inntakssystemene til videregående skoler er et tema som har blitt diskutert i Norge over lengre tid. Det er uenighet blant politikere om hvilket inntakssystem som er mest fordelaktig for elever, der del av debatten har fokusert på elevenes prestasjoner. Skoleåret 2021/22 opplevde elever i Akershus som skulle fra ungdomsskole til videregående skole en endring i inntektssystemet. Fylket gikk fra karakterbasert opptak til et nærskoleprinsipp. Denne inntaksreformen påvirket elevenes skolevalg ved å redusere antallet skoler hver elev var kvalifisert til å søke på, basert på geografisk nærhet i tillegg til karakterer. Reformen ga en unik mulighet til å analysere den kortsiktige effekten av en slik endring på elevprestasjoner.

I denne masteroppgaven har vi utnyttet reformen for å se på effekten på gjennomsnittlige karakterer fra ungdomsskoler og videregående skoler i Akershus fylke. Dataen vår er hentet fra både før og etter reformen, for både behandlede og ikke behandlede skoler. Vi gjennomfører en Difference-in-differences-regresjonsanalyse for å undersøke effekten av reformen på karakterer. En av årsakene bak reformen var å minimere forskjellene i karakterer mellom høyt- og lavt-presterende skoler. Vi gjennomfører derfor også en Difference-in-difference-in-differences-regresjon for å undersøke en mulig omfordelende effekt av reformen.

Resultatene viser en negativ effekt på karakterer på ungdomsskolen, som samsvarer med tidligere norsk forskning på inntaksreformer til videregående skoler. Vi finner også indikasjoner på en negativ effekt på karakterer i videregående skoler, men ingen indikasjoner på en omfordelende effekt som utjevner karakterforskjeller mellom høyt- og lavt-presterende skoler. Dette motsier tidligere funn på hvordan inntakssystem påvirker forskjeller mellom skoler.

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# 1. Introduction

### *1.1 Motivation*

Education is a topic that continues to be pertinent in political discussions and research. The subject is greatly discussed as it is of significance for both individuals and societies as a whole. Education provides individuals with the knowledge needed to make informed decisions, while also leading to better job prospects and economic stability. Schools also make a very important social arena for children, in which they make friends and gain confidence. For societies, higher education leads to higher productivity, technological advancement, more equality and better healthcare and wellbeing. Educational systems are therefore continually evaluated and reformed.

One key component in the discussion of education that often leads to great debate is regarding school choice, a topic that has been discussed for decades. The topic is also the focus of this thesis, as we analyse the importance of school choice on student attainment.

The first notable work on school choice was written by Milton Friedman (1955) in *The Role of Government in Education.* Here, he argued that instead of the government funding schools, parents should receive *vouchers* to spend on any approved institution of their choosing (Tooley, 2014). Friedman believed that giving parents the opportunity to choose between schools would improve the quality of all schools, both public and private, because of increased competition (Friedman, 1962; Rangazas, 1997). Not only low-income families, but particularly the rich and the middle class should be involved in the voucher system to stimulate innovation in education (Tooley, 2014). He viewed education as an externality affecting other people's welfare, and the government's role would be limited to ensuring that schools met certain standards and for compulsory school to be required.

*The Coleman Report* was published in 1966 and has been quoted as "the best-known and most controversial piece of educational research" (Christopher Jencks, 1969, cited in Kantor & Lowe, 2017, p. 571). It shifted the focus on equal opportunity in education from concerning equal availability of resources to concerning equal outcomes. The report followed the Civil Rights Act of 1964 and concluded that school segregation was not the primary cause for students of different ethnic, racial, and economic backgrounds to perform worse at school, but rather that family background was the "primary determinant of educational success" (Kantor

& Lowe, 2017, p. 573). The report found that children from poorer backgrounds benefit from attending schools with children of economic advantage, and could thus be interpreted as implying that integration served as the solution to giving equal opportunities for children with different backgrounds.

The works of Friedman and that in the Coleman report laid the foundation for the debate around school choice and school outcomes. Despite the long standing interest in school choice, it remains a highly controversial topic with large variations in policy. This includes in Norway where there are frequent changes in school choice policy, particularly with respect to upper secondary school admission. This dissertation returns to this issue focusing on a recent admission reform for upper secondary schools in a region of Norway.

Norway has two main admission systems that vary across admission areas. One is the grade-based admission system, commonly referred to as "free school choice", in which students freely choose between schools and compete for seats with their grade point average (GPA). The alternative is the residence-based admission system, in which students are prioritised at schools in proximity to where they live. For the application process to primary- and middle schools, admission is entirely residence-based. However, for upper secondary schools, this system may vary depending on the region.

In the school year 2021/22, students in the region *Akershus* faced a change in the upper secondary admission system, going from grade-based admission to residence-based admission. In the process of learning more about this reform, we spoke to Balder Alvær Olafsen, a politician who helped form the new admission system (personal communication, January 18 2024). He himself is from Bærum, and was a Viken board representative from the left-wing party SV.[1] Olafsen points out four main reasons for why this reform was implemented. (1) The travelling distance for each student would be reduced. (2) The redistributive effect from the new admission system would minimise socioeconomic differences. (3) More students would have the freedom to choose, and not only the students with the highest GPA. And lastly, (4) there would be higher chances to gain admission to a school together with middle school friends.

[1] "SV" is an acronym for *Sosialistisk Venstreparti*, translated to the Socialist Left-party. SV together with AP, the Norwegian Labour-party, were the main advocates for the residence-based admission system.

Immediately, there was great debate concerning the change. Some students were for the change, with Ashish Kumar Bhargava claiming it made students feel safer in the process of applying for upper secondary schools, while also pointing out the importance of avoiding elite schools (Bhargava, 2021).

However, not all students agreed. During the three years of residence-based admission, many youths continuously conveyed their dismay in newspapers' opinion pieces,[2] with one opinion piece expressing the writers' concern that students are unable to choose. In the newspaper *Aftenposten*, they claim that "The residence-based admission system is an assault on students' freedom to choose in Viken" and "The regional council disregarded the voice of the youth" (Aftenposten, 2022. Authors' translation).

Another such opinion piece was by a student who, at the time of writing, was finishing middle school and applying to upper secondary schools. In the newspaper *Nettavisen*, the student claimed that "50 000 youths are being deprived of their first choice" of schools. She also claimed in her title that "AP made the lives of several thousands of youths difficult"[3] (Nettavisen, 2023. Authors' translation). While we haven't checked the accuracy of these numbers, we still believe her statements convey an important message about students' experience of the admission-reform. Her opinion is clear; the implementation of a residence-based admission system was, and is, against students' wishes.

In our thesis we want to investigate whether the reform has had an effect on student learning. We do this by analysing how grades were affected in Asker and Bærum compared to Oslo. Our research question is:
*How does the admission-reform affect average grades for students in the last year of middle school and first year of upper secondary school?*

[2] See opinion pieces under Additional Resources: Utdanningsnytt, 2020; Budstikka, 2021; Aftenposten, 2020.
[3] "AP" is in this context referring to *Arbeiderpartiet*, the Norwegian Labour Party. The regional labour party in Viken is often given the sole responsibility for the admission-reform.

### *1.2 Approach*

To answer this research question, we investigate the effect of the reform using a *Difference-in-differences* (DiD) regression analysis. Our main data consists of Asker and Bærum as treatment groups, Oslo as a control group, and data on grades both in the years before and after the reform. Thus, we can analyse the short-term effect to determine what significance, if any, this change has had on student performance in the region. The DiD regression isolates the effect of the change from grade-based to residence-based admission on students' grades. Specifically, this paper looks at how the average grade in different subjects has been affected by the admission-reform.

The politicians implementing the residence-based admission system stated that one of the purposes is minimising the differences between higher and lower performing schools. Our theory is that there is a redistributive effect, meaning higher performing schools decrease in average grades more compared to other schools. To investigate this, we therefore performed a *Difference-in-difference-in-differences* (DiDiD) regression analysis on upper secondary schools.

We find the policy to have different effects across subjects and between middle school and upper secondary school. We find statistically significant results for middle school at a 1% level, showing effects of the reform on average grades by a quarter of a grade decrease. We attribute this to less competition for students when applying to upper secondary schools. For upper secondary school we find varying results across subjects, with some estimates showing statistically significant effects by half a grade decrease in average grades as a result of the reform. All estimates indicate a negative treatment effect on average grades in Asker and Bærum. We discuss whether this could be a direct result of lower actual learning in middle school, or as a result of a change in student composition. We also find no indications of redistributive effects when looking at the DiDiD estimates in upper secondary schools.

### *1.3 Thesis Structure*

In section 2 we discuss previous literature on school choice and admission systems. Firstly, we look at between-school differences, before we look at competitive and redistributive effects of different admission systems. In section 3 we  explain the relevant aspects of the Norwegian educational system, the different admission systems and the Viken reform that led to the admission-reform, before presenting our data. In section 4 we present our methodological approach and explore different threats to identification while presenting our control variables. In section 5 we present our results, and in section 6 we conclude.

## 2. Literature Review

A substantial amount of research has been done that focuses on educational systems and student performance. For example, Card & Krueger (1992) researched the effects of school quality on the rates of return on education. They measured school quality in terms of the student/teacher ratio and teacher salaries, and found significant positive correlation between school quality, average years of schooling and mean earnings. Evidence like these raise normative questions regarding school admission systems. Given that between-school differences affect student learning, what factors should determine which students gain admission to which schools?

### *2.1 Between-school Differences*

A Danish report from 2010 looked at "the relationship between factors in primary and lower secondary schools (input and process) and pupils' learning (output and outcome)" (Nordenbo et al., 2010, p. 5). In the report they summarised "good schools" as being characterised by an orderly atmosphere, where students feel safe and have high engagement without peer pressure. These schools are also characterised by support and respect among students and staff, positive relationships between students, and various means of communication with the parents and parental involvement in terms of, for example, being active on the school boards or the like. In this section we look at what makes up between-school differences in Norway.

While The Education Act, a law regarding primary and secondary education in Norway (The Education Act, 1998), states that most of the responsibility for compulsory and upper secondary education lies with the municipalities and regions, it also provides common regulations to ensure equal quality in areas such as "the physical and psychosocial school environment", "pupil and parent participation (including council bodies)", "school leadership and teacher competence", etc. (Ministry of Education and Research, 2023).

Previous research on the Norwegian educational system has primarily been value-added analyses, following the same students and measuring their performance over time. The focus has been on areas such as what causes some students to have higher grades than others, or the schools' effect on students' performance. Anders Bakken provided one such study in 2009, looking at how schools can help close gaps in students' learning potential and performance, caused by factors such as parents' educational level, minority status, and gender (Bakken, 2009).

In a similar study, Wiborg et al. (2011) presented three types of measures for school characteristics that affect student learning; *school material and human resources*, *school learning environment*, and *student composition in school*. These factors can account for differences in student learning between schools. Although they found these differences to be small, they concluded with the statement "Good schools are good for everyone, and bad schools are bad for everyone" (Bakken og Danielsen, 2011, p. 197, cited in Wiborg et al., 2011, p. 152. Authors' translation).

These papers focused on student performances in primary school, as this allowed them to follow students for several years. In upper secondary schools, there are only three years of schooling, and value-added analyses are therefore not as easy to perform. Still, there are papers attempting to find the correlation between school contribution and performances in upper secondary schools as well.

Hægeland et al. (2010) looked at the first year of upper secondary schools in Oslo. They found no clear relation between school contribution and performances, and state that "Student composition and random variation contribute substantially to the schools' average scores" (Hægeland et al., 2010, p. 5). When comparing the effect on performance with and without controlling for student composition, they state that "the differences are much smaller when controlling for student composition" (Hægeland et al., 2010, p. 29. Authors' translation).

Falch & Strøm (2013) extended this research to all schools in Norway for all years of upper secondary school. They found substantial differences between schools, even when controlling for individual characteristics. They also looked at regional differences, and found that there are substantial differences between regions in school quality. They mentioned several possible reasons for this, one being that regions have different boards. They also questioned whether different admission systems could be another reason.

Reviewing existing literature demonstrates that there are between-school differences that affect learning, making the choice of which schools students attend crucial in terms of the quality of their education. However, other school differences might also need to be discussed. Another decisive area for students choosing schools is class environment and student wellbeing. Students will presumably have this as a high priority when choosing upper secondary schools. Student wellbeing is also an essential foundation for learning (Lindsay et al., 2023).

While many papers have been written on the subject, not many have tried to measure the correlation between wellbeing and student achievement. One study, however, did look at mental health among upper secondary school students in Oslo and Bergen, two major Norwegian cities (Bütikofer et al., 2023). Features of these high achieving schools were higher teacher/student ratios and being surrounded by higher achieving peers. Bütikofer, Løken and Landaud (2023) found that entering such a school can increase pressure to perform. They also found that higher achieving schools had less bullying and a more peaceful classroom environment relative to other schools. Hence, being surrounded by higher achieving peers can have both positive and negative effects on a students' stress and anxiety. The paper found that the effect of being eligible for a high achieving school was zero on the use of health care services during upper secondary school, but reduced the likelihood of receiving a mental health diagnosis in the three years after school. In other words, the added effects of high achieving schools on mental health were overall positive in this study.

In summary, between-school differences in achievement can arise from different factors, the most prominent factor seems to be through student composition. However, factors not addressed in this research is how competition between schools can affect student achievement. Admission systems affect this competition by liberating or restricting students' school choice. An interesting discussion to address is therefore what competitive effects that come with the different admission systems and the change thereof.

### *2.2 Admission Systems and Competitive Effects*

A common view is that competition between schools has a positive effect on productivity, and benefits all schools and students (Hoxby, 2003). Schools have incentives to be productive, as they otherwise risk losing more productive students to other schools. In this way competition would be a "rising tide that lifted all boats" (Hoxby, 2003, p. 288). Hoxby focused on the tuition fees as a great incentive for schools to compete for students. These incentives can also occur if schools are financed by taxes and face a high degree of choice, meaning that parents consider school districts when choosing a residence. Hoxby further emphasised that this degree of choice depends on housing patterns and parents' jobs.

In Norway, education is free. School incentives might therefore be different. In our analysis, we take advantage of a recent school reform, making research on previous school reforms particularly relevant. There are a few Norwegian studies that look specifically at the different admission systems.

Machin & Salvanes (2016) studied the effect of housing prices after the Oslo reform in 1997 changing the admission system from residence-based to grade-based. Before the change, higher performing schools were correlated with higher housing prices. This linkage was found to be weakened after the reform. From this, they conclude that parents value higher performing schools, and are willing to pay a higher price for housing so that their children are eligible to apply to such a school. This literature indicates how admission reforms lead to altered behaviour. They do not, however, analyse the effect of the reform on students' performance.

Fidjeland (2023) compared the different admission systems in Norway in terms of the performance on the national exams at the end of compulsory school. These grades are the last opportunity for the students to improve their GPA before upper secondary school. Fidjeland found that students exposed to grade-based admission, and having at least three schools within travelling distance, have higher performing grades in these exams. He then presents two explanations for this mechanism, the first being that students increase their effort on the exam day, and the second being that students undertake sustained learning over time in order to be ready for the exam. It is from the latter that policy design can affect students' incentives to invest in schooling, which again will give positive long-term effects in both education and the labour-market.

Haraldsvik (2014) exploited a reform from residence-based admission to grade-based admission in Hordaland, Norway, that took place in 2005. She found that the introduction of school choice led to increased fragmentation in upper secondary school in terms of what middle schools the students came from, and stated that “performance-based school choice may lead to increased competition” (Haraldsvik, 2014, p. 16). She found that grades from middle school increased after the reform, and she concluded that pupils respond to more competition by increasing effort, because of the increased importance of performance. Haraldsvik also discussed heterogeneous effects between municipalities, and found the effect on grades to be stronger in the municipalities with the most competition (higher number of schools). She presented the concern that grade-based admission is expected to lead to "negative sorting effects through the composition of peers" (Haraldsvik, 2014, p. 5).

In summary, previous literature found grade-based admission to increase middle school grades, through an increase in competition between upper secondary schools. In accordance with these findings, a change from grade-based to residence-based admission should theoretically have the opposite effect. Additionally, restricting choice by implementing residence-based admission would affect movement between school districts. Previous literature does not take into account how admission systems can affect schools differently. One of the main motives for the Viken admission reform was to decrease achievement gaps between schools. It is therefore also necessary to discuss how admission systems can result in redistributive effects between schools.

### *2.3 Admission Systems and Redistributive Effects*

Fidjeland (2023) mentions that when accounting for productivity, any measures after the reform could be a result of a different composition in the peer group and not from the reform itself working to incentivize students. In this thesis, the between-school differences are especially interesting when looking at the redistributive effects from the reform.

With perspectives from a school reform in Seoul, researchers Oh & Sohn (2021) aim to explain why free school choice leads to more and less between-school segregation. Free school choice can lead to more segregation because students self-sort into schools where they are surrounded by like-minded people, but it can also lead to less segregation by "breaking the link between neighbourhood segregation and school segregation" (Oh & Sohn, 2021, p. 417). In their research, Oh & Sohn found prominent between-school segregation based on achievements.

Östh et al. (2013) compared Stockholm students in their actual upper-secondary schools from grade-based admission to their hypothetical upper secondary school in terms of residence-based admission. They argued that if residential segregation is the reason for performance gaps, then between-school variation should be observed in both admission systems. On the other hand, if school choice is the reason for performance gaps, the between-school variation should only be observed in the grade-based admission system. The results from the analysis were that grade-based admission, rather than neighbourhood segregation, increased the between-school variation in performance.

The effect on address changes were analysed in a study of the Danish residence-based admission system reform in 2012. Bjerre-Nielsen et al. (2023) analysed the probability that students change their address to a false address to be eligible for admission to another school and "play the system" (Bjerre-Nielsen et al. 2023, p. 1). They found that address manipulation happened before the admission deadline mainly in the areas of the most popular upper secondary schools and by pupils of higher educated parents. They concluded that this result implies regressive redistribution, as already privileged students had a higher probability of playing the system to enter a more popular school.

Edmark et al. (2014) examined the redistributive effects of the Swedish school reform in 1992. They found that the reform, changing to free school choice, had no or a slight positive effect on grades for disadvantaged groups compared to other groups, meaning that students from a low-income or immigrant background were not harmed by the free school choice reform.

In summary, previous research highlights between-schools differences that indicate the possibility of a redistributive effect when changing student composition. However, there is little Norwegian literature on redistributive effects from admission systems or recent admission reforms in Norway. We have found no other research on the effects from the Viken admission reform of 2021 in particular. This thesis therefore contributes to the existing literature with recent and distinctive findings on the competitive and redistributive effects from admission reforms.

# 3. Institutional Background and Data

## *3.1 The Norwegian School Admission Systems*

In Norway, there are 4 different stages of school; primary school (grade 1-7), middle school (grade 8-10), upper secondary school (grade 11-13, or 11-14 for the vocational upper secondary) and at last college and university. Primary and middle school are compulsory in Norway, and are therefore collectively called "compulsory school". By national law, there are no grades in primary school and the middle school admission system is fully residence-based

(The Education Act, 1998, §8-1). While compulsory school is administered by the different municipalities, upper secondary school is the responsibility of the different regions. This includes designing how the admission system works and defining the different admission areas within the region (Regulation related to the Education Act, Chapter 6, 2013, §6-2). Consequently, the school admission system differs between regions in Norway, and is a political decision from the region's elected representatives.

*Admission Areas*

The admission area determines which schools students are eligible to apply to, and can include municipalities or parts thereof, or a whole region. Akershus is a region consisting of three admission areas. Our primary focus is on Asker and Bærum, which are two neighbouring municipalities that constitute one admission area. This means that habitants of Asker and Bærum are eligible to apply to schools in both municipalities, but are not eligible to apply to schools in other admission areas, such as Oslo. For students in Asker and Bærum to go to a school in Oslo, they either need to change their address, or apply to a private school, of which there are few.

The two other admission areas are Follo and Romerike. These are two sub-regions of Akershus consisting of several, relatively smaller, municipalities each. In section 5.4 we extend the research to include these areas.

*Law of Upper Secondary Admission*

In Norway, upper secondary education is not compulsory, but a right for students up to 24 years old who have completed compulsory school (The Education Act, 1998, §3-1). When applying for upper secondary studies, students choose between different educational programs, one of them being Specialisation in General Studies which qualifies them for higher education. When applying, students fill out an application form (Vigo, n.d b) where they choose their top three programs and which schools they prefer to attend that offer these programs.

Before the first round of admissions, all schools set aside 5% of their spots to students who, during admission, don't receive an offer from any of their preferred programs or schools (Forskrift om inntak til videregående opplæring, Akershus, 2018, §3-1). During the first round, most students will receive an offer from either their first, second or third choice of program. Which school they receive an offer from, depends on their grade point average (GPA). All students have a right to an upper secondary education, even when they don't receive an offer during the first round of admissions. These students are therefore placed at a school close to them, filling up the 5% of spots that were set aside at each school. The second round is where students who did receive an offer, but chose to be put on a waiting list, can receive spots on their preferred school if there are any openings. Unless they're accepted into their preferred school, the student keeps their spot in the school they did receive an offer from. This way, all students who apply to upper secondary schools are guaranteed a spot, and an upper secondary education.

School admission systems may vary in terms of what program the student chooses. For clarity, we will focus on admission to the Specialisation in General Studies program, which we also call the "general studies" program. Most schools offer this program. Our sample consists of 9 schools in Asker and Bærum, 21 schools in Oslo, 6 in Follo, 12 in Romerike and 8 in Stavanger and surrounding areas.

*Grade-based Admission*

The grade-based admission system is also commonly referred to as "free school choice". Here, students compete with their grade point average (GPA) from the last year of compulsory school, year 10, for admission to an upper secondary school. Norwegian grades go from 1 to 6, the latter being the highest grade. For admission, their average grades are multiplied by 10, meaning that a student will have a maximum GPA of 60. In our dataset, Oslo and Stavanger have had grade-based admission the whole period, while Akershus had this practice up to the school year 2020/21. After this, they changed to residence-based admission.

*Residence-based Admission*

An alternative to the grade-based system is the residence-based system, in which students are prioritised when competing for the schools geographically closer to where they live. The region board has the responsibility of deciding how this system works in practice, and which postcode is assigned to which neighbouring schools. In this section we focus on the change to residence-based admission in Akershus as it became a part of Viken.

The residence-based admission system in Viken granted 100 extra points to the GPAs of students choosing a "neighbouring school", determined by the proximity of the residence to the school (Forskrift om inntak til videregående opplæring og formidling til læreplass, Viken, 2020, §3-1). These extra points made it impossible in practice for a student that refrained from applying to their neighbouring school to compete with a student that did. This is because students otherwise only compete with up to 60 points.

Students' neighbourhood schools were determined by their postcodes. Table 3.1.1 shows all neighbouring schools with the associated postcodes in Asker and Bærum under the residence-based admission system. In Bærum each postcode had four neighbouring schools, while in Asker each postcode had two. It is interesting to note that the upper secondary school *Valler* was included as a neighbouring school in all postcodes in Bærum. Competition to attend this school may have persisted, even though students living in Asker were not eligible to apply to this school in the residence-based admission system.

*Table 3.1.1 Postcodes and associated neighbouring schools in Asker and Bærum.*

| Admission area | Municipality | Postcode | Neighboring Schools |
|---|---|---|---|
| Asker+Bærum | Bærum | 1336 1337 1338 Sandvika, 1341 Slependen, 1365 Blommenholm, 1339 Vøyenenga, 1340 Skui, 1346 Gjettum, 1348 1349 Rykkinn, 1350 Lommedalen, 1351 Rud, 1352 Kolsås, 1353 1354 Bærums Verk, 1311 Høvikodden | Valler, Rosenvilde, Dønski & Sandvika |
| | | 1344 Haslum, 1356 1357 Bekkestua, 1363 Høvik, 1358 Jar, 1359 Eiksmarka, 1361 Østerås, 1362 Hosle, 1360 1364 Fornebu, 1366 Lysaker, 1367 Snarøya, 1368 1369 Stabekk, | Eikeli, Stabekk, Nadderud & Valler |
| | Asker | 1383 1384 1385 1386 1387 Asker, 1388 Borgen, 1390 Vollen, 1392 Vettre, 1395 Hvalstad | Asker & Nesbru |
| | | 1394 Nesbru, 1396 Billingstad, 1397 Nesøya | Nesbru & Sandvika |

*Source: Viken Nærskoleoversikt, vilbli, 2024*

Picture 3.1.1 illustrates all municipalities in Akershus. Table 3.1.2 shows the number of associated postcodes for the three admission areas *Asker and Bærum*, *Follo* and *Romerike*, similarly to table 3.1.1. In Follo, each postcode had only 1 neighbouring school, eliminating all competition, while in Romerike, each postcode mostly had one neighbouring school, while some had two or three. We see from the number of postcodes in the table that the schools Lillestrøm and Skedsmo, as well as Lørenskog and Mailand, still had some competition between them.


*Picture 3.1.1 Municipalities in Akershus*

*Consisting of the admission areas Asker+Bærum (southwest of Oslo), Follo (south of Oslo) and Romerike (northeast of Oslo). Source: Store Norske Leksikon [Great Norwegian Encyclopedia], 2024.*

*Table 3.1.2 Number of postcodes associated with each neighbouring school in Asker, Bærum, Follo and Romerike.*

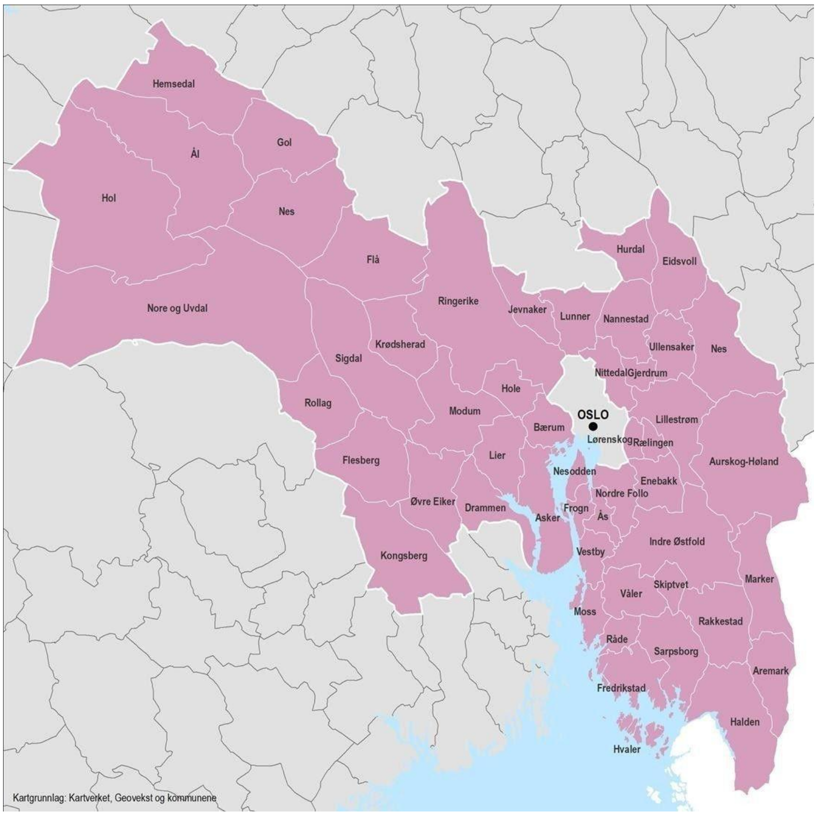
| **Admission area** | **Municipality** | **N/o postcodes** | **Neighboring Schools** |
|---|---|---|---|
| Asker+Bærum | Bærum | 16 | Valler, Rosenvilde, Dønski & Sandvika |
| | | 14 | Eikeli, Stabekk, Nadderud & Valler |
| | Asker | 9 | Asker & Nesbru |
| | | 3 | Nesbru & Sandvika |
| Follo | Vestby | 7 | Vestby |
| | Nordre Follo | 5 | Roald Amundsen |
| | | 9 | Ski |
| | Ås | 1 | |
| | | 4 | Ås |
| | Frogn | 9 | Frogn |
| | Nesodden | 7 | Nesodden |
| | Enebakk | 1 | Ski |
| Romerike | | 2 | Rælingen |
| | Aurskog-Høland | 7 | Bjørkelangen |
| | Rælingen | 1 | Skedsmo, Lillestrøm & Rælingen |
| | | 2 | Rælingen |
| | | 2 | Lørenskog & Mailand |
| | Lørenskog | 11 | |
| | Lillestrøm | 1 | |
| | | 12 | Skedsmo & Lillestrøm |
| | | 3 | Sørumsand & Bjørkelangen |
| | | 3 | Sørumsand |
| | Nittedal | 6 | Bjertnes |
| | Ullensaker | 14 | Jessheim |
| | Gjerdrum | 1 | Nannestad & Jessheim |
| | Nannestad | 1 | |
| | | 3 | Nannestad |
| | Eidsvoll | 7 | Eidsvoll |
| | Hurdal | 1 | |
| | Nes | 7 | Nes |
| | | 2 | Nes, Sørumsand |

*Source: vilbli.no (2024)*

### *3.2 The Viken Reform and the Following School Admission Reform*

The three regions Akershus, Østfold and Buskerud were merged into the new Viken region in January 2020 as decided by the parliament as part of the national "region merger" policy from 2017. The three original regions were opposed to the policy on different grounds (Kommunal rapport, 2016; NRK, 2016; Buskerud Fylkeskommune, 2017), but were nonetheless eventually merged into the new region of Viken. The new region laws therefore had to be implemented by January 2021 (Forskrift om sammenslåing av Akershus, Østfold og Buskerud fylkeskommuner, 2018, §2).

The Viken board then had to create a new school admission system applicable for the whole 24 592 square kilometre region spanning from the mountain range in the west, to the coast in the east, and the densely populated areas surrounding Oslo. The board implemented a residence-based admission system for upper secondary schools in Viken, keeping the same admission areas. This meant that the urban areas in Viken, familiar with grade-based admission, now had to adapt to restrictions in terms of which schools they could apply to.




*Picture 3.2.1 Municipalities in Viken*

*Previous regions are "Buskerud" (northwest), "Østfold" (south) and "Akershus" (surrounding Oslo). The picture is retrieved from an article in Nettavisen written by Heidi Schei Lilleås (2019). Previously retrieved from the website of Viken region (defunct) and created by Kartverket.*

Siv Henriette Jacobsen is a spokesperson from the former regional council that started the process of implementing the new admission system. She explained in an entry in the newspaper *Dagsavisen Demokraten* that one of their main reasons for the change concerned evening out achievement gaps between schools (Dagsavisen Demokraten, 2020). She claimed that a decentralised school system would help close this gap. Jacobsen also explained that the main goal with a residence-based admission system was to ensure that as many students as possible would be able to finish upper secondary education in Viken, as students with poorer grades and possibly the least motivation were those who would end up commuting as a result of grade-based admission.

Viken was dissolved by their own political board after stating that "Viken is an inexpedient construction, the Parliament has merged Akershus, Østfold and Buskerud without their consent" (as reported in Nettavisen, 2019. Authors' translation). In 2021, the new elected government allowed Viken to dissolve, and the old regions, Akershud, Østfold and Buskerud, re-appeared in January 2024. The new board in Akershus implemented the grade-based admission system effective from the school year 2024/25.

The Viken reform can be viewed as exogenous because the implementation was imposed by the government and was not the political agenda of the regions themselves. Therefore, the new admission system can be viewed as a *natural experiment* for two notable reasons. Firstly, the Viken board was elected in the fall of 2019 and had to implement an admission system in January 2021. In this period the elected council had to both assemble Viken and implement numerous different laws for the region. This included creating an admission system that would take into consideration not only the demographic of Akershus, but also the rest of the new region.

Secondly, Akershus has typically been administered by politically right-wing elected boards. However, when merged to Viken, the elected board was in majority politically left-winged, showing a political conflict of interest between Akershus and the new areas of Viken. When deciding on the admission system for Viken, the left-winged board was able to implement a system the right-winged parties, and Akershus, disagreed with. Consequently, once Viken was dissolved, the new right-winged elected board for Akershus abolished the residence-based admission system effective immediately. Table 3.2.1 includes the elective representatives in

Akershus and Viken, illustrating that Akershus is traditionally right-winged, while the Viken region had a left-wing majority.

*Table 3.2.1 Number of representatives in regional elections*

| Political position | Representatives | | | | | |
|---|---|---|---|---|---|---|
| | Akershus 2023 | Viken 2019 | Akershus 2015 | Akershus 2011 | Akershus 2007 | Akershus 2003 |
| Right (Høyre, FrP) | 25 | 30 | 19 | 22 | 21 | 19 |
| Left (AP, SV, SP, Rødt) | 15 | 41 | 17 | 17 | 17 | 18 |
| Centre (MDG, Venstre, Krf) | 7 | 12 | 7 | 4 | 7 | 4 |
| Other | 2 | 4 | 0 | 0 | 0 | 0 |

*Source: NRK (2023), NRK (2019), Tvedt & Tjernshaugen (2024).*

### *3.3 Data*

*Grades and School-specific Data*

The data used in this paper is publicly available and consists of average grades for each school by year, provided by the Norwegian Directorate for Education and Training (Utdanningsdirektoratet, 2023c). We also use average results from the Pupil Survey, a survey created by the directorate, as a control variable. We have panel data from before and after the reform for both treated and untreated schools. The data is gathered from the school years 2015/16 through 2022/23 for upper secondary schools, and 2014/15 through 2021/22 for middle schools.

Our school specific data is available on the website of the directorate which is the executive agency for the Norwegian Ministry of Education and Research, (Utdanningsdirektoratet, 2023a). They, along with the county governor and the municipalities, are responsible for making sure that all schools and kindergartens meet the requirements in the laws set by the government (Utdanningsdirektoratet n.d. a). On their website, udir.no, they also provide comprehensive material on a number of areas regarding education in Norway, including numbers and statistics (Utdanningsdirektoratet, 2023b). Before publishing, Udir conducts a quality check on the data, ensuring for example no duplication in answers or grades.

Udir gathers their data on grades from the website of VIGO, which is a tool used for application to and administration of upper secondary education in Norway (Vigo, n.d. a). Individual data on grades are gathered from this administration website and then used to create aggregated data to publish (Utdanningsdirektoratet, n.d. b).

Grades in middle school and upper secondary education are integers set on a scale from 1 to 6, in which grades from 2 and up are passing grades and 6 is the highest possible grade. There are three different types of grades that a student can earn in each subject. First, the main grade is the student's semester grade. This is set by the teacher based on the work and tests the student has taken throughout the year. These are the grades that make up the school's average grade in the subject. The two other types of grades are those set by oral and written national exams. In the first year of upper secondary, only some students are chosen to take the exam.

As there are only a select number of the schools that have each of the exam grades, we have chosen to focus on average grades for each school. The subjects in question are English, Geography, Mathematics, Science and Social Science for upper secondary schools. These subjects are chosen as they are compulsory for all students taking the general studies program, and because these grades are set in the first year of upper secondary school.[4] Mathematics in upper secondary school is divided into two subjects; Maths 1P for a more practical variant, and Maths 1T for a more theoretical variant. Each student chooses which variant they would like before starting their first year. For middle school we have data for the subjects English, Mathematics, Science, Norwegian (first choice form)[5] and Social Science. Grades in middle school are gathered from the year before those of upper secondary schools, i.e. from 2014/15. This is so that the year 10 grade corresponds to the same students as the first year of upper secondary. Students who started upper secondary in 2015/16 finished middle school in 2014/15. These middle school grades can therefore be used as controls for the upper secondary school grades.

Before publishing any data, Udir is strict in shielding any personal data, ensuring no students could be directly or indirectly linked to any answers or grades. This includes shielding results in which too few students answered one particular answer in the survey, or in which too few students received a certain grade. In doing this, they may also shield surrounding answers as an added measure to ensure privacy.

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[4] Certain subjects, such as Norwegian and foreign languages (excluding English), are taught for more than one year. Final grades are therefore not set until students have completed their final year with the subject

[5] First choice form refers to the students' primary version of Norwegian, as there are two official written versions of the Norwegian language.

*The Pupil Survey*

One important control variable for our analysis is the Pupil Survey. This is obligatory for all schools to conduct each autumn for students in years 7, 10 and the first year of upper secondary (Utdanningsdirektoratet, 2023d). However, participation in the survey is voluntary. Students that choose to partake are held anonymous and all questions are also voluntary for them to answer.

Each question/statement has five different answers for the students to choose from. These are numbered from 1 to 5, with a higher score indicating more positive answer (Utdanningsdirektoratet, n.d. c). In order to have the most comparable results possible, we selected a number of questions and statements that have been asked each year we gathered data from, presented in table 3.3.1.

*Table 3.3.1 Survey questions (English translation)*

| | |
|---|---|
| 1 | There is a good working atmosphere in the classes. |
| 2 | In my class, we believe it's important to work well on our school assignments. |
| 3 | Do you enjoy school? |
| 4 | Are you interested in learning at school? |
| 5 | How much do you like schoolwork? |
| 6 | I look forward to going to school. |
| 7 | Do you get enough challenges at school? |

*Corona Restrictions*

One of the main challenges with our analysis is the fact that the coronavirus pandemic occurred around the same time as the reform. To control for the pandemic affecting grades, we gathered data on corona restrictions affecting schools in each admission area. We found that the number of infected was not a representative proxy variable for school restrictions during the pandemic. Instead, we used the "Traffic Light Model" imposed by the government, which directly represented the level of restrictions on schools each municipality had at different times throughout the pandemic.

In Norway, all schools closed on March 12th 2020. In the following months, The Institute of Public Health, Folkehelseinstituttet, created a "Traffic Light Model" for schools and kindergartens (Nilssen et al., 2020). This meant that schools were regulated at a local level, depending on whether the municipalities were green, yellow or red. Red level would mean small cohorts at schools and either partially or fully homeschooling, while yellow and green level had open schools and less restrictions. National advice always surpassed local advice, but in periods with lack of national guidelines we observe variation in restrictions between the regions. The local restrictions were taking into account the number of people infected in the region, and the development of the infection rate.

We have focused on gathering data about red level restrictions for the different admission areas. Oslo conducted a report on the traffic light model in the region, which includes the timeline of restrictions in Oslo (Oslo Municipality, 2022). For the other admission areas we searched local papers for their reports regarding red level restrictions.

*Other Control Variables*

Other than the school-specific variables from the directorate's website and that of the pandemic, our control variables are gathered from Statistics Norway (SSB). Statistics Norway is "the national statistical institute of Norway" (Statistics Norway, n.d. a) and are the official provider of statistics in Norway. They have a Quality Assurance System for assuring that the quality of the statistics follows the Statistics Act in Norwegian law (Statistics Norway, n.d. b). They provide statistics on most areas, a lot of which lies publicly available in their StatBank. We utilise statistics on areas such as Income, Education, Population and Movements which is publicly available at municipal level.

### *3.4 Summary Statistics*

Here, we present the summary statistics for the outcome variables in the treatment group and control group.[6] This data represents school average grades, as the data is at school level.

In table 3.4.1 we see that students in Asker and Bærum have slightly higher mean grades in Social science and English, and the lowest mean grades in practical mathematics (Maths P). In Oslo we see slightly lower mean grades than for students in Asker and Bærum. We also observe more variation in average grades between schools in Oslo compared to Asker and Bærum, with both minimum observations being lower, and maximum observations being higher in Oslo.[7] This is also illustrated by higher standard deviations in Oslo.

[6] See summary statistics for school variables in table 8.1.3 and 8.1.4 in the Appendix, and summary statistics for regional variables in table 8.1.5 and 8.1.6 in the Appendix.

[7] To ensure that these variations in average grades are caused by between-school differences, and not differences over time, we hold time constant and include only the observations from 2018/19 in Table 8.1.1 and 8.1.2 in the Appendix. Here we see the same tendencies as in tables 3.4.1 and 3.4.2.

*Table 3.4.1 Summary statistics for all average grades in upper secondary schools in Asker+Bærum and Oslo*

| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| English | 70 | 4.62 | .379 | 3.7 | 5.3 |
| Geography | 71 | 4.558 | .462 | 3.6 | 5.4 |
| Maths P | 62 | 3.705 | .487 | 2.6 | 4.5 |
| Maths T | 57 | 4.072 | .448 | 3 | 4.9 |
| Science | 68 | 4.557 | .469 | 3.1 | 5.3 |
| Social science | 62 | 4.768 | .42 | 3.8 | 5.5 |
| **Oslo** | **Obs** | **Mean** | **Std. Dev.** | **Min** | **Max** |
| English | 139 | 4.36 | .585 | 2.7 | 5.5 |
| Geography | 149 | 4.438 | .716 | 3 | 5.9 |
| Maths P | 135 | 3.515 | .723 | 2.1 | 5.3 |
| Maths T | 128 | 3.773 | .645 | 2.1 | 5 |
| Science | 139 | 4.246 | .689 | 2.7 | 5.6 |
| Social science | 143 | 4.552 | .614 | 2.8 | 5.6 |

When looking at middle school grades in table 3.4.2, we see that Asker and Bærum and Oslo are very similar in terms of mean grades. However, we still observe slightly lower mean grades and more variation between schools in Oslo compared to Asker and Bærum. These variations could be a result of neighbourhood segregation because middle school admission is residence-based. For middle schools, mean grades are lower in Mathematics and higher in English and Social science, as we also observed for upper secondary schools.

*Table 3.4.2 Summary statistics for all average grades in middle schools in Asker+Bærum and Oslo*

| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| English | 189 | 4.456 | .249 | 3.8 | 5.3 |
| Mathematics | 187 | 3.962 | .288 | 3 | 4.7 |
| Norwegian | 189 | 4.133 | .293 | 3.3 | 5.1 |
| Science | 190 | 4.363 | .279 | 3.7 | 5.1 |
| Social science | 191 | 4.554 | .256 | 3.8 | 5.4 |
| **Oslo** | **Obs** | **Mean** | **Std. Dev.** | **Min** | **Max** |
| English | 476 | 4.354 | .349 | 3.5 | 5.3 |
| Mathematics | 476 | 3.758 | .434 | 2.5 | 5 |
| Norwegian | 474 | 3.958 | .4 | 2.7 | 5.3 |
| Science | 478 | 4.267 | .385 | 3.1 | 5.2 |
| Social science | 476 | 4.43 | .359 | 3.4 | 5.3 |

We also observe that the standard deviations in upper secondary schools are around twice as big as the standard deviations in middle schools. This could reflect self-sorting into upper secondary schools making the between-school differences higher. We also observe that the difference between the standard deviations from middle school to upper secondary school is bigger in Oslo than in Asker and Bærum. The degree of self-sorting can therefore be higher in Oslo, causing bigger between-school differences in upper secondary schools.

The descriptive statistics indicate between-school differences in average grades in both the treatment group and the control group. From the statistics, these differences seem to be caused by both neighbourhood segregation and self-sorting.

## 4. Empirical Methodology

Our focus is mainly on estimating the effect of the reform on average grades. Our data is gathered for both before and after the reform, and for both treated and untreated schools. We also argue that the admission reform is a natural experiment from a policy change, the Viken reform. This makes our analysis suitable for a *Difference-in-differences* approach.

### *4.1 Difference-in-Differences Regression*

Difference-in-differences (DiD) is an econometric method that exploits a natural experiment generating a treatment group and a control group that arise from a policy change or "treatment" (Wooldridge 2019, p. 434), in this case the admission reform. The treatment group (T) is the schools affected by the reform and the control group (C) is schools unaffected by the reform, i.e. the comparison group.

A difference-in-difference regression gives causal estimates of the policy change under certain assumptions. The *Parallel trend assumption* is crucial to identifying the effect of the reform in a DiD strategy (Wooldridge, 2019, p. 436). It assumes that in the absence of treatment, the treatment group would see the same results as the control group. We further address this in section 5.2. Another assumption is that of no manipulation of the treatment, which can occur if students move between admission areas and thus affect their treatment status. Further manipulation occurs if students self-sort by moving closer to their preferred school in the treated group.[8] These cross-over effects are in violation of the SUTVA assumption, which we explain in section 4.4. We address these issues by introducing the variables *movement from* and

[8] Moving between and within admission areas can occur even before the admission for schools begin, as the reform was announced in December 2020, a few months before admission to the school year 2021/22.

*house sales* in section 5.6. Finally, an underlying assumption is that there is no endogeneity in the reform itself. We address this in section 4.4 and conclude that this is not an issue since the reform was exogenous, as argued in section 3.2.

The difference in grades between the groups before the reform, also known at the *first difference*, is:

$$grade_{T,before} - grade_{C,before}$$

The difference in grades between the groups after the reform, and known as the *second difference,* is:

$$grade_{T,after} - grade_{C,after}$$

The DiD-estimator measures the effect of the policy by controlling for the differences between the treatment and control group before and after the reform. This can also be explained as "taking the second difference minus the first difference" (Babu et al., 2017, p. 214).

$$\beta_{DiD} = \left(grade_{T,after} - grade_{C,after}\right) - \left(grade_{T,before} - grade_{C,before}\right)$$

This can be rearranged as such.

$$\beta_{DiD} = \left(grade_{T,after} - grade_{T,before}\right) - \left(grade_{C,after} - grade_{C,before}\right)$$

The first term is the policy effect on the treated group. The second term is the same trend for the control group, and by subtracting this from the trend of the treatment group the purpose is to retrieve a causal estimate of the policy reform (Wooldridge, 2019, p. 434). We see that if the difference in groups before the reform is zero, the effect of the reform becomes the differences in groups after the reform. This illustrates the importance of parallel trends.

In middle school (m) we expect the average grades to be reduced for each school in each subject, as less competition decreases student motivation to improve their grades. Thus, the *null hypothesis* $H_0$ is there being no or a positive effect on grades, while the *alternative hypothesis* $H_A$ is a decrease in grades because of the reform.

$$H_0^m: \beta_{DiD} \geq 0$$
$$H_A^m: \beta_{DiD} < 0$$

In upper secondary school, the effect of the reform is more difficult to predict. We expect a decrease in average grades as an indirect effect from lower middle school grades, if this is caused by lower actual learning. However, a direct effect from the admission reform is altered student composition. With grade-based admission, it is reasonable to assume that students self-sort in such a manner that some schools have more motivated, higher performing students on average. When school choice is more restricted, it results in less segregation between the schools in terms of motivation (unless there is underlying neighbourhood segregation concerning this).

This causes an effect on average grades that can be both positive and negative for upper secondary schools. First, motivated students will motivate previously unmotivated students, and lift the average grades. Second, unmotivated students will demotivate previously motivated students, and lower average grades. The alternative hypothesis is therefore an effect other than zero on upper secondary grades, as the predicted effect from the admission reform is ambiguous.

$$H_0^u: \beta_{DiD} = 0$$
$$H_A^u: \beta_{DiD} \lessgtr 0$$

### *4.2 Difference-in-Difference-in-Differences Regression*

In this section, we address heterogeneous school effects to examine whether the reform affected schools differently.

In this thesis we refer to schools with higher admission GPAs as A-schools or elite schools, while the rest of the schools are referred to as B-schools. We focus on the within-regional differences for the types of schools. The admission GPA for an A-school is therefore different in Asker and Bærum compared to Oslo. We also gathered data from 2015 in Asker and Bærum, to make sure the data on A-schools is from before the reform was implemented. The title of "elite schools" has not rotated throughout the years in our dataset, as the status is a result of a self-reinforcing pattern of high admission GPA over several years. For this reason the A-schools in Asker and Bærum, based on average grades from 2015, and the A-schools in Oslo, based on average grades from 2023, are comparable and can be grouped together as elite schools.

We assume that A-schools before the reform had more motivated students, and B-schools had less motivated students.

*Table 4.2.1 List of A-schools*

| **Admission area** | **A-schools** | **Admission GPA** | **Year gathered** |
|---|---|---|---|
| Asker + Bærum | Asker, Nadderud, Sandvika, Valler | 45.9 | 2015 |
| Oslo | Edvard Munch, Elvebakken, Foss, Fyrstikkalleen, Lambertseter, Nydalen, Oslo Katedralskole, Ullern | 47.1 | 2023 |

The difference-in-differences for A-schools and B-schools in the treatment group before and after the reform is:

$$\beta_{DiD}^{T} = \left(grade_{A,T,after} - grade_{A,T,before}\right) - \left(grade_{B,T,after} - grade_{B,T,before}\right)$$

The difference-in-differences for A-schools and B-schools in the control group before and after the reform is:

$$\beta_{DiD}^{C} = \left(grade_{A,C,after} - grade_{A,C,before}\right) - \left(grade_{B,C,after} - grade_{B,C,before}\right)$$

The DiDiD-estimator gives the estimated effect of the policy in A-schools compared to B-schools in the treatment group, controlling for the same trend in A- and B-schools in the control group, before and after the reform. We can explain the DiDiD-estimator, or the *triple difference estimator,* as "the difference between two difference-in-differences" (Olden & Møen, 2022, p. 536), as arranged below:[9]

$$\begin{aligned}\beta_{DiDiD} = &\left(\left(grade_{A,T,after} - grade_{A,T,before}\right) - \left(grade_{B,T,after} - grade_{B,T,before}\right)\right) \\ &- \left(\left(grade_{A,C,after} - grade_{A,C,before}\right) - \left(grade_{B,C,after} - grade_{B,C,before}\right)\right)\end{aligned}$$

The null hypothesis is that there is no or a positive effect from the reform on average grades in A-schools compared to B-schools in the treatment group. The alternative hypothesis is that average grades in A-schools compared to B-schools decrease. This is the redistributive effect.

$$H_0^u: \beta_{DiDiD} \geq 0$$
$$H_A^u: \beta_{DiDiD} < 0$$

[9] There are in total three ways to arrange this DiDiD-estimator, the two other ways are included in Equation 8.1.1 and 8.1.2 in the Appendix. All interpretations estimate the effect on A-schools compared to B-schools from the reform, and the estimator does not depend on how we are differencing (Wooldridge, 2019, p. 435).

### *4.3 The Regression Equations*

Our paper analyses the effect of an admission reform (treatment) in Akershus. Our main approach is to compare Asker and Bærum to Oslo. The two regions are geographically close and share a large border between Oslo and Bærum. Oslo is a city and a municipality with 1561 inhabitants per square kilometre, while Asker and Bærum are municipalities with 648 inhabitants per square kilometre. The two regions have many schools in their areas that students can choose from, with Asker and Bærum having 9 upper secondary schools offering general studies, and Oslo having 21. There are also 23 middle schools in Asker and Bærum and 54 middle schools in Oslo. Although Oslo is a city and Asker and Bærum are Oslo's suburban areas, both regions have more wealthy and poorer areas, while there's also some higher performing schools often considered elite schools. Overall, we view it as randomly assigned whether the habitant lives in either region because of the geographical closeness.

As an extension to the research, we also included the admission areas Follo and Romerike because these areas were also affected by the reform. We address this in section 5.4. The control group is Oslo. We also added Stavanger and neighbouring municipalities as an alternative control group for the purpose of a robustness test. We further address this in section 4.4. These control groups have had no reform and kept a grade-based admission system both before and after the Viken admission-reform.

*Table 4.3.1: Admission areas*

| Admission area | Region | Admission system | Group |
|---|---|---|---|
| Asker + Bærum | Akershus | Residence-based (from 2021) | Treatment |
| Oslo | Oslo | Grade-based | Control |
| Follo | Akershus | Residence-based (from 2021) | Treatment |
| Romerike | Akershus | Residence-based (from 2021) | Treatment |
| Stavanger + Sandnes + Sola + Randaberg | Rogaland | Grade-based | Control |

Equation (1) is the upper secondary school *naïve model*, for this analysis the naïve model only includes the DiD- and DiDiD-estimates. Since admission areas can include more than one municipality, we do not have municipality-specific numbers, only variables that vary between schools (s), admission area (a) and time (t). The variable *Treated* is equal to 1 if the school is in the treatment group, and equal to 0 if the school is in the control group. The variable *Post* is equal 1 if the year is after the reform, and equal to 0 if the year is before the reform. $\beta_1$ is the

DiD-estimator where $(Treated_{sa} * Post_t) = 1$ if the school is in the treatment group, accounting for observations after the reform is implemented. The variable *Aschool* is equal to 1 if the school is categorised as an A-school, and equal to 0 if the school is a B-school. $(Aschool_s * Post_t) = 1$ if the school is an A-school, both in the treatment and control group, accounting for the observations after the reform is implemented, and $\beta_2$ is the coefficient of this interaction term. $\beta_3$ is the DiDiD-estimator where $(Treated_{sa} * Aschool_s * Post_t) = 1$ if the school is an A-school in the treatment group, accounting for observations after the reform is implemented. At last, $\alpha_0$ is the constant and $u_{sat}$ is the error term.

$$(1)\ grade_{sat} = \alpha_0 + \beta_1(Treated_{sa} * Post_t) + \beta_2(Aschool_s * Post_t) + \beta_3(Treated_{sa} * Aschool_s * Post_t) + u_{sat}$$

Equation (2) is the middle school naïve model, only including the DiD-estimate in the regression equation in addition to the constant $\alpha_0$ and the error term $u_{smat}$. Middle schools are the responsibility of municipalities. We have therefore included municipality specific data in addition to the upper secondary school admission areas. $\beta_1$ is the DiD-estimator where $(Treated_{sma} * Post_t) = 1$ if the middle school is in the treatment group after the reform is implemented.

$$(2)\ grade_{smat} = \alpha_0 + \beta_1(Treated_{sma} * Post_t) + u_{smat}$$

### *4.4 Threats to Identification*

*Spurious Effects*

The coronavirus spread to Norway in the beginning of 2020, the same year as the Viken reform was implemented. This had a notable effect on student performance the school year 2020/21 for some regions. When looking at the parallel trend models for these regions, we see a peak in student grades in year 6, i.e. 2020/21.[10] A report from Rambøll (2021) states that the reason for an increase in the grades during the pandemic was not increased learning, but rather more generous teachers who gave the students the benefit of the doubt and multiple chances of taking tests.

However, this causes there to be uncertainty about the true causal effect of the reform. While we see that the peak is reduced again in the year 2021/22 when the residence-based admission

[10] See table 8.4.1 in Appendix and 5.2.1 in Results for the parallel trends in subjects in upper secondary school.

system was introduced, the effects of the pandemic make it difficult to conclude in a regular DiD analysis that any effect on grade is the causal effect of the admission-reform. This is because the pandemic impacted the regions differently in terms of share of infections and the strictness of the restriction.

In order to account for our results on grades not being a spurious effect of the reform, we include corona restrictions as a control variable, which will be further explained in section 4.5. We also explore corona infections and the teacher strike of 2022 as alternative explanations for the results of the reform. This is presented in section 5.6.

*Self-sorting Manipulation Through Movement*

Another threat to identification is if students within admission areas move to get closer to their preferred schools. This is an issue for the analyses as it could directly affect the results. If students move to get into schools they would have otherwise attended with grade-based admission, there would be little or no difference in student composition before and after the reform. This would mean that the analyses would find no changes in average grades as a result from students manipulating admission by moving, rather than a direct result from the reform. Thus, we have measured the effect on *movements within* admission areas in table 8.6.3 in the appendix. If this manipulation occurs, movement within admission areas should also increase. However, we find no evidence of this in our analysis, indicating little or no manipulation by self-sorting through movement.

*Cross-over Effects*

Because of the geographical proximity of Akershus and Oslo, there is a risk of students moving to Oslo specifically to attain the grade-based admission system and attend the schools they prefer. This is a violation of the SUTVA assumption.

The Stable Unit Treatment Value Assumption, or SUTVA for short, is an essential assumption of unbiased causal effects in regression analyses (Schwartz et al. 2012; Gerber & Green, 2010). Angrist, Imbens and Rubin (1996) defined this assumption and explained that "SUTVA implies that potential outcomes for each person *i* are unrelated to the treatment status of other individuals" (Angrist, lmbens, & Rubin, 1996, cited in Gerber & Green, 2010, p. 4). This includes there not being any spillover effects between treatment and control groups. With the likelihood of migration between areas as a result of the reform, we could not claim that this assumption was not violated.

Consequently, to control for this, we added Stavanger as an alternative control group to our dataset. Stavanger is a city and municipality in the west part of Norway that has had a grade-based admission system to upper secondary schools since before and throughout the timeframe of this analysis.[11] We also included the surrounding areas of the municipality that are in the same admission area as Stavanger. Stavanger and the surrounding areas make up a total of 8 upper secondary schools offering general studies, and 34 middle schools. Due to the distance between Stavanger and Akershus, it is unlikely that any movement between the regions is associated with simply wishing to gain access to a grade-based admission system. Any migration to and from these two regions would be rare and caused by other unrelated factors.

To further control for migration, we also added the variable *movements from* in our dataset to see how many people move from each municipality each year that are in the age group 6-17 years old. We should see an increase of movements from Asker and Bærum for the age group if this migration is a significant issue. We investigate this as an alternative outcome of the reform in section 5.5.

*Biased Estimates*

There are differences between regions both in variation in time and differences between schools that are controlled for by using a fixed effects model. These differences, for example in population and economic states, can cause biased estimates unless controlled for. If schools in the treated group have better teacher quality or school grounds, construction and location, the effect on grades will be biased by these unobserved differences between schools. Unobserved differences between years, such as economic and political factors that affect all schools and students alike, can also affect student motivation and achievements. If these year differences are not accounted for, we could observe an effect that is not explained in our model. These are factors that are not controlled for with our regional and school level control variables. We therefore used school fixed effects to account for time invariant differences, and time fixed effects to account for common shocks over time, in order to avoid biased estimates.

[11] Stavanger is the fourth largest city after Oslo, Bergen and Trondheim. Bergen and Trondheim were not suitable as control groups because Bergen had an admission reform in 2020, and Trondheim had residence-based admission up to 2024. Due to the lack of other regions with enough between-school competition and grade-based admission, Stavanger was the only remaining control group after Oslo.

*Measurement Error*

Caroline Hoxby (2003) presents the problem of measuring *productivity*. She emphasises that one should not use grades that have different meanings in different schools and times, as this can cause biased estimates. A Norwegian study looking at systematic differences between schools in terms of teacher-set grading relative to grading in national exams found an indication that teachers, especially at schools with lower performing students, tended to exaggerate students' abilities when grading them (Galloway et al., 2011). This means that there is a certain level of subjectiveness that causes the estimates to not necessarily reflect actual learning. This could make interpretation of the results somewhat difficult.

Grades on national exams might then seem like a better measurement for actual learning rather than grades set by teachers, as the exams are graded anonymously by external examiners. However, there are two challenges with this; firstly, as mentioned above, exams are randomly distributed in terms of which school will be examined in which subject. Additionally, only some students are selected each year. The number of participants therefore varies between schools for each subject. Some subjects are only carried out in certain schools, and in year 1 of upper secondary schools some pupils do not have any exams at all. Secondly, due to the pandemic there are years of exam results missing, as the national exams were cancelled due to infection prevention measures. The lack of continuous observations for exam grades over time can give biased results because of variations in the sample between schools and over time. Since the number of observations would have been reduced by looking at national exams, and in particular the lack of exams in the period immediately before and after the reform, we concluded that teacher set grades were the most appropriate observations.

Additionally, grades do not only represent actual learning, but they also represent future prospects both in terms of access to higher education and job/wage prospects. In our DiDiD-analysis we also want to look at the redistributive effect of the reform, as the between-school differences in grades can be viewed as a measurement of opportunity. Students with higher grades will have more freedom to choose a study program at their preferred university after upper secondary school. The distribution of grades before and after the reform will give an insight into the redistributive effects in terms of opportunity to attend university.

*Selection Bias*

Selection bias occurs when individuals or observations are selected in such a way that it skews the estimates. This is in particular a risk when looking at the Pupil Survey. Grade setting in Norway is relatively problem free in terms of selection bias, as all students will receive a grade regardless of background, performance, etc. However, the Pupil Survey is an online survey that is voluntary for students to partake in. Students can also choose to skip questions they do not wish to answer (Utdanningsdirektoratet, n.d. c). Strøm (2006) informs that selection bias can occur if absence of participation occurs systematically. An example of this could be if students who are bullied consistently chose not to partake in the survey or chose not to answer questions on topics that are difficult for them to address.

While the participation rate has been between 85% and 90% the past three years for compulsory school (Utdanningsdirektoratet, 2024a) and between 80% and 85% for upper secondary school (Utdanningsdirektoratet, 2024b), this still leaves thousands of students choosing not to participate. Particularly in upper secondary school, there are between 10 000 and 15 000 students at national level for each of the past three years that have not participated in the survey.

Since the survey is anonymous, Udir has no way of knowing or controlling for which students don't participate. We therefore must proceed with our research under the assumption that the results from the Pupil Survey aren't skewed by systematic absence of participation.

For grades, we observe that Udir has shielded some information due to privacy. While this is systematic in the sense that it applies to small classes with few individuals, we have no reason to believe that the reform itself has had different effects on students in small classes relative to bigger classes.

*Endogeneity*

Another possible issue with looking at the effects of the school admission system is that it is a political, and therefore endogenous, decision in every region. A researcher may therefore end up looking at the wrong causal explanations, because the correlation is biased. Hoxby (2003) gives the example that a school district could want to increase productivity by implementing grade-based admission. Productivity may then seem lower in districts with this admission system, which could make it appear as if the system itself causes low productivity. This would cause two-way causality bias as the change in admission was based on grades, rather than grades being a result of the reform.

Following this argument, one could think that Akershus wanted to reduce grades in school by limiting competition.[12] It could then look like the residence-based admission system is correlated with higher grades. This would have been an issue had the reform occurred as a result of the region wanting to reduce pressure on grades or for other reasons reduce grades. However, this is not the case. The reform was implemented because of a region-merger that Akershus had no control over. The admission reform was thus implemented exogenously, as argued in section 3.2.

Another potential source of endogeneity is anticipation of treatment, which can cause students to alter behaviour that affects grades prior to the reform. In our example, students could lower grades in middle school before the reform has happened, as they already knew that they wouldn't need as high grades to attend upper secondary schools. Assuming the control groups do not have this reaction,[13] the trends between the treatment group and control group would not be parallel in anticipation of treatment. This would be a violation of the parallel trend assumption. However, due to the nature of the reform, students had relatively little knowledge of it in the time before its implementation. Endogeneity should therefore not be an issue in this regard.

*Omitted Variables*

When including school fixed effects in our model, the treatment-variable, A-school-variable, and the interaction term between these variables are omitted because of collinearity. We therefore ran a regression where we excluded school fixed effects as a robustness check and found that the results still hold.[14]

A concern for this approach is that some school inputs are unobservable, such as those determining school quality, as well as the students' motivation and innate ability (Hoxby, 2003). For determining school productivity, this can be biased simply from student inputs

[12] This can be the case if the politicians want to minimise pressure on students because of tendencies of mental health issues in the treatment group.
[13] The students in the control group and treatment group may face regional differences.
[14] See table 8.2.1 in Appendix, here we see that the DiD- and DiDiD-estimates in the preferred model do not change much in coefficients and significance when excluding fixed effects.

leading to higher grades at this school. To solve the problem, one can compare an environment with little or no choice to an environment with free choice (Hoxby, 2003). This is what we aim to do when looking at Akershus after the reform constraining school choice and compare the results to Oslo and Stavanger. This way we can conclude if student inputs in a specific area or school quality determines average grades.

### *4.5 Control Variables*

In 2003, Todd and Wolpin addressed the *specification and estimation of the production function for cognitive achievement*. They explained how children's learning is affected by a number of family and school related factors, most of which previously hadn't been common practice to include when measuring cognitive achievement. Thus, we have included several control variables in our model that are likely to influence educational attainment, based on research on education in Norway.

*Demographic Variables*

There are a number of international studies showing a correlation between parents' socioeconomic background and student achievement. Norwegian studies show that specifically parents' education level and income has a significant effect on student performance.

Parents' education and income

The research of Wiborg et al. (2011) and Bakken (2009) are examples of previous research on the effect of parents' education level on student attainment. They found a positive correlation between the two. Their research also indicates that classes with high parental education creates a class environment that is positive for student attainment. This environment was particularly beneficial for disadvantaged students. Wiborg et al. (2011) also mention parents' income having an impact on student achievement.

The research of Marks and Pokropek (2019) looked at the effect of parental income on several countries and found that "income is significantly related to student achievement" (Marks & Pokropek, 2019, p. 770). They attribute this to better resources, schools and living conditions, among other reasons. A few factors might make these effects less prominent in Norway, such as relatively low income inequality,[15] few private schools, and The Education Act asserting

[15] Norway had a Gini coefficient of 0.285 in 2021. By comparison, the United States of America had a Gini coefficient of 0.395 in 2021 (OECD, 2024).

that education is free.[16] However, there are still likely prominent variations between schools that correlate with income.

As an indicator for parental background, we have therefore included both data on the percentage of people with higher education and median household income at municipality level for middle school and at admission area level for upper secondary school.

*Middle School Controls for Upper Secondary Regression*

Previous achievement and satisfaction

When examining differences in grades between upper secondary schools, Hægeland et al. (2010) found that the grades in middle school provide a good measure of students' knowledge level when entering upper secondary school. They found that this was of greater importance in determining upper secondary grades than family background and gender. We have therefore included middle school grades and satisfaction as control variables in the upper secondary regression. Since we don't use individual data, middle school grades are coded to be regional, accounting for differences in previous achievements between the admission areas. As the data is not linked to individuals, middle school grades might not capture as much of the effect of parental background. These variables are therefore all included.

*School Variables*

School satisfaction

Research also shows that school satisfaction is of great importance for student learning. Wiborg et al. (2011) used answers from the Pupil Survey to measure the average student's perception of satisfaction in school. In the survey, students answer questions regarding wellbeing, bullying, school environment, motivation and support from teachers. As part of our measure of school satisfaction, we also look at answers from the survey. As the data from this survey is based on students' subjective experience, the variable gives an indication of the average student's satisfaction for each year.

Number of students

Both Scandinavian and international research points to there being a small positive relationship between school size and student learning up to a point, and then the relationship becomes

[16] As education is free, schools are also required to provide the necessary printed and digital teaching materials and digital equipment (The Education Act, 1998, §3-1)

negative, all else equal (Centre for Practice-Oriented Educational Research, 2022). To account for this correlation, we included the number of students for year ten of compulsory school (middle school) and the first year of upper secondary school in the general studies program.

School resources and class size

Wiborg et al. (2011) looked at school material and human resources to investigate the relationship between school characteristics and student grades. They found a negative correlation between school resources and student achievements in the 8th grade. They stated that the reason for this was reverse causality, meaning worse student performance led to schools investing in more material or human capital.

Research on the significance of class size has previously yielded ambiguous results. Card and Krueger (1992) found a negative correlation between the number of students per teacher in a class and returns to schooling. Their result was substantiated by further research.[17] Angrist & Lavy (1999) also found a negative relationship between larger classes and student achievement. However, in a redux paper twenty years later they no longer found this relationship (Angrist et al., 2019). This is in line with Norwegian findings, for example that of Falch et al. (2017), showing little or no effect of class size.

While the effect of class size evidently is disputed, we have included *teacher density* in our dataset to see what effect, if any, this has on our results. However, this data is only added for middle school grades, as there is no publicly available data on this for upper secondary schools.

*Corona Restrictions*

Since the corona restrictions were different between the regions, we include this as a control in our analysis. Scandinavian studies show how homeschooling during the pandemic not only had implications for student learning and grades in general but was detrimental for vulnerable students and had further implications for the achievement gap between students with minority status and low socioeconomic status (Nøkleby et al., 2022). The pandemic is therefore a challenge in the analysis of the admission-reform, as it occurred around the same time as the pandemic. In section 3.3 we described the traffic light model, which reflects the number of restrictions for each municipality. From this, we gathered information on *weeks with red level*

[17] See Krueger (1999) about positive correlation between small classes and student attainment in the "Tennessee STAR experiment" and Krueger & Whitmore (2001) for positive long-run effect from this study.

for each school year and each admission area, which we used to control for the pandemic. As a robustness test, we also controlled for the *percentage of corona infected of the population*, which varied between municipalities in middle school data, and admission areas in upper secondary school data.

### *4.6 The Preferred Models*

In section 4.3 we introduced the naïve models. In this section we present the *preferred models* of our analysis. To estimate the causal effect of admission-reform on average grades, the preferred model for upper secondary school includes year dummies $\delta_t$, school fixed effects $\gamma_s$, school controls $S_{sat}$, demographic controls $D_{at}$, middle school controls $M_{at}$ and corona restrictions $CR_{at}$.

$$(1') \; grade_{sat} = \alpha_0 + \beta_1(Treated_{sa} * Post_t) + \beta_2(Aschool_s * Post_t) + \beta_3(Treated_{sa} * Aschool_s * Post_t) + \delta_t + \gamma_s + \beta_4 S_{sat} + \beta_5 D_{at} + \beta_6 M_{at} + \beta_7 CR_{at} + u_{sat}$$

$\beta_1$ is the DiD-estimator, which estimates the change in average grades between Asker and Bærum and Oslo in the time after the reform. This indicates the effect of the reform on grades in Asker and Bærum. $\beta_3$ is the DiDiD-estimator, which estimates the change in average grades between A-schools and B-schools in Asker and Bærum after the reform, compared to the same difference in Oslo. This indicates the heterogeneous effect of the reform in Asker and Bærum. The estimates provide the causal effect of the reform under the assumptions mentioned in section 4.1.

We also want to examine how the reform affected middle school grades, as altered competition in admission for upper secondary schools can affect effort in middle school. The preferred model for middle school average grades includes year dummies, school fixed effects, school controls, demographic controls and corona restrictions. In equation (2'), $\beta_1$ is the DiD-estimator, estimating how the reform affected grades in middle school after the reform in Asker and Bærum compared to Oslo. This regression also includes year dummies $\delta_t$, school fixed effects $\gamma_s$, school controls $S_{smat}$, demographic controls $D_{mat}$, and corona restrictions $CR_{at}$.

$$(2') \; grade_{smat} = \alpha_0 + \beta_1(Treated_{sma} * Post_t) + \delta_t + \gamma_s + \beta_2 S_{smat} + \beta_3 D_{mat} + \beta_4 CR_{at} + u_{smat}$$

In section 5, we present the main results on a few subjects in middle and upper secondary school. For causal interpretations of the findings, we then present the parallel trend and event study graphs. As an extension to the study, we present results from estimates on additional subjects. We then introduce Stavanger as an alternative control group for a robustness test of the results, and alternative treatment groups to investigate alternative treatment effects. As a robustness test, we examine alternative outcomes of the reform to investigate if other factors than grades were affected from the reform and if these alternative outcomes could affect grades. At last, we present some alternative explanations for our main results, specifically the teacher strike in 2022 and the percentage of corona infected.

# 5. Results

Before presenting the results, we clarify that the treatment group is Asker and Bærum and the control group is Oslo, unless other groups are specified. We first present the main results before introducing model specifications to address threats to identification. Finally, we explore alternative explanations for our results in section 5.6.

## *5.1 Main Results*

In the main results for middle school and upper secondary school, we build up to our preferred model by including model specifications with different control variables. We start by presenting the results from middle school and then the results from upper secondary school, before further addressing the DiD- and DiDiD-estimates of the latter.

*Middle School DiD-estimates*

Middle school grades may be affected from the admission reform because of reduced competition in admission for upper secondary schools.

In table 5.1.1 we present the main results with different model specifications for Mathematics in middle school, where *Treated_post* is the DiD-estimate. We see that all results are significant at a 1% level. Model 1 shows that the difference between the treatment and non-treatment group in the time after the reform when including year dummies is at -0.151. This indicates a negative treatment effect causing average grades in Asker and Bærum to be lower by 0.151. When introducing school fixed effects, this effect becomes -0.150, remaining relatively similar. After introducing school controls, the effect is at -0.146, still indicating a relatively similar treatment effect. However, after introducing demographic controls, the effect becomes notably stronger at -0.217. Model 5 gives us the preferred model where we also control for corona restrictions. We see that the treatment effect in this model specification is at -0.249, indicating that the reform decreased middle school average grades in Asker and Bærum by almost 0.25.

*Table 5.1.1 Estimated effect of the reform on average Mathematics grades in middle school when adding control variables*

| Middle school Mathematics | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Treated_post | -0.151*** | -0.150*** | -0.146*** | -0.217*** | -0.249*** |
| | (0.0484) | (0.0486) | (0.0467) | (0.0522) | (0.0530) |
| Year dummies | YES | YES | YES | YES | YES |
| School fixed effects | | YES | YES | YES | YES |
| School controls | | | YES | YES | YES |
| Demographic controls | | | | YES | YES |
| Corona restrictions | | | | | YES |
| Constant | 3.582*** | 3.650*** | 2.483*** | -8.900* | -6.821 |
| | (0.0517) | (0.0281) | (0.296) | (4.588) | (4.615) |
| Observations | 663 | 663 | 648 | 648 | 648 |
| R-squared | | 0.217 | 0.274 | 0.289 | 0.299 |
| Number of S_id | 76 | 76 | 76 | 76 | 76 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

While previous research states that increased competition from grade-based admission systems will increase student effort, and therefore achievements (Haraldvik, 2014; Fidjeland; 2023), we found that decreased competition from a residence-based admission reform in upper secondary school admission decreased student grades in middle school. Our results therefore show the same indications as previous studies and are likely explained by the same mechanism; competition and student achievements (effort) are positively correlated. We can explain the decrease in middle school average grades by less incentives to improve grades, because of the lessened importance of grades in upper secondary school admission after the reform.

*Upper Secondary School Estimates*

As we saw above, reduced competition decreases average grades in middle schools. One of our main points of interest is to see whether the same is true for upper secondary schools. If the decrease in grades in middle school is caused by lower actual learning, we expect to see a negative effect of this on grades in upper secondary school. We might also expect to see a change in average grades as a result of different class compositions after the reform. This is especially the case for A-schools, where we expect to find a negative estimated effect. We present here the results for three different subjects in upper secondary school: English, Practical Mathematics (Maths P), and Science. Each table has six model specifications, where *Treated_post* is the DiD-estimate and *Treated_Post_A* is the DiDiD-estimate.

*Table 5.1.2 Estimated effect of the reform on English average grades in upper secondary school when adding control variables*

| Upper secondary school English | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Treated_post | -0.154* | -0.152* | -0.166* | -0.134 | -0.109 | -0.163 |
| | (0.0801) | (0.0812) | (0.0978) | (0.124) | (0.169) | (0.180) |
| Treated_Post_A | 0.0496 | 0.0479 | 0.096 | 0.100 | 0.100 | 0.102 |
| | (0.122) | (0.124) | (0.134) | (0.134) | (0.135) | (0.135) |
| Year dummies | YES | YES | YES | YES | YES | YES |
| School fixed effects | | YES | YES | YES | YES | YES |
| School controls | | | YES | YES | YES | YES |
| Demographic controls | | | | YES | YES | YES |
| Middle school controls | | | | | YES | YES |
| Corona restrictions | | | | | | YES |
| Constant | 3.893*** | 4.368*** | 3.448*** | 4.324 | 9.602 | 13.01 |
| | (0.109) | (0.0367) | (0.595) | (12.52) | (23.15) | (23.48) |
| Observations | 209 | 209 | 202 | 202 | 202 | 202 |
| R-squared | | 0.218 | 0.256 | 0.267 | 0.267 | 0.271 |
| Number of S_id | 30 | 30 | 30 | 30 | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In table 5.1.2, we present the main results for the DiD- and DiDiD-estimates on English in upper secondary school. We see that the DiD-effects vary with the model specifications in terms of coefficients and significance. In model 1 we see from the DiD-estimate that, with a 10% level of significance, English grades reduce with 0.154 as an effect from the reform. This model includes year dummies, accounting for common shocks affecting grades for all schools in our dataset. In model 2 we add school fixed effects, accounting for unobservable time invariant differences between schools, and see that the estimate is marginally reduced and still statistically significant. When adding school controls in model 3, the DiD-estimate increases to -0.166, and is still significant. School controls also account for differences between schools, but they are observable and vary over time. When adding demographic controls, the estimate is no longer significant and reduced to -0.134, we find this to be due to the control variable *higher education* rather than *median household income*. In model 5 we add middle school controls, including middle school *English grades* and *school satisfaction* the year prior. Here the estimates are further reduced to -0.109. At last, when controlling for corona restrictions in model 6 the effect from the reform increased to -0.163, though still insignificant.

For the DiDiD-estimate in table 5.1.2 for all model specifications, we observe a statistically insignificant positive effect from the reform on English grades in A-schools compared to B-schools. We also see that the heterogeneous effect from the reform increases from 0.0479 to 0.096 when adding school controls in model 3. In model 4, 5 and 6 we interpret the difference in English grades between A-schools and B-schools to increase with almost 0.1 grade after the reform.

*Table 5.1.3 Estimated effect of the reform on average Practical Mathematics grades in upper secondary school when adding control variables*

| Upper secondary school Mathematics P | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Treated_post | -0.297** | -0.286** | -0.379** | -0.464** | -0.257 | -0.518 |
| | (0.141) | (0.140) | (0.162) | (0.217) | (0.252) | (0.508) |
| Treated_Post_A | 0.283 | 0.289 | 0.409* | 0.409* | 0.413* | 0.410* |
| | (0.214) | (0.214) | (0.233) | (0.234) | (0.233) | (0.233) |
| Year dummies | YES | YES | YES | YES | YES | YES |
| School fixed effects | | YES | YES | YES | YES | YES |
| School controls | | | YES | YES | YES | YES |
| Demographic controls | | | | YES | YES | YES |
| Middle school controls | | | | | YES | YES |
| Corona restrictions | | | | | | YES |
| Constant | 3.161*** | 3.615*** | 3.346*** | -16.05 | 7.075 | -3.151 |
| | (0.118) | (0.0649) | (0.922) | (23.58) | (26.97) | (32.08) |
| Observations | 197 | 197 | 191 | 191 | 191 | 191 |
| R-squared | | 0.120 | 0.139 | 0.148 | 0.167 | 0.169 |
| Number of S_id | 30 | 30 | 30 | 30 | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.1.3 illustrates the effects from the reform on Practical Mathematics. In models 1 to 4 the DiD-estimates are statistically significant at 5% level, and when adding demographic controls, the reform reduced grades in Maths P by 0.464, almost half a grade. However, when adding middle school controls, the DiD-estimate reduces to -0.257, and becomes statistically insignificant. When adding corona restrictions as a control in model 6, the negative effect from the reform increases to -0.518, still insignificant.

The DiDiD-estimates for Practical Mathematics are just under 0.3 in model 1 and 2, and increase to 0.409 when adding school controls, significant at a 10% level. In model 6, the estimated effect from the reform is a 0.410 increase in the difference in grades between A- and B-schools.

*Table 5.1.4 Estimated effect of the reform on average Science grades in upper secondary schools when adding control variables*

| Upper secondary school Science | | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| Treated_post | | -0.119 | -0.120 | -0.222* | -0.279* | -0.330* | -0.562** |
| | | (0.0961) | (0.0956) | (0.119) | (0.149) | (0.199) | (0.281) |
| Treated_Post_A | | 0.200 | 0.199 | 0.293* | 0.290* | 0.287* | 0.283* |
| | | (0.143) | (0.142) | (0.160) | (0.160) | (0.161) | (0.161) |
| Year dummies | | YES | YES | YES | YES | YES | YES |
| School fixed effects | | | YES | YES | YES | YES | YES |
| School controls | | | | YES | YES | YES | YES |
| Demographic controls | | | | | YES | YES | YES |
| Middle school controls | | | | | | YES | YES |
| Corona restrictions | | | | | | | YES |
| Constant | 3.827*** | 3.779*** | 4.347*** | 4.309*** | -0.998 | -5.990 | -10.24 |
| | (0.0511) | (0.106) | (0.0412) | (0.700) | (15.08) | (17.31) | (17.67) |
| Observations | 207 | 207 | 207 | 200 | 200 | 200 | 200 |
| R-squared | 0.619 | | 0.090 | 0.109 | 0.118 | 0.121 | 0.129 |
| Number of S_id | | 30 | 30 | 30 | 30 | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.1.4 shows the estimated effect of the admission reform on Science grades in upper secondary schools. We see in model 1 a negative effect of 0.119 when controlling for year dummies, and a negative effect of 0.120 when adding school fixed effects in model 2. However, these results are insignificant. The results become significant at a 10% level when we introduce school controls in model 3, where we also see a notably increased negative effect of 0.222.

This is further increased in models 4 and 5, when introducing respectively demographic controls and middle school controls. Our preferred model, model 6, which also includes corona controls, is significant at a 5% level, and shows a drastic negative effect of 0.562.

The DiDiD-estimates are significant at a 10% level when adding school controls. In model 6 the estimates indicate that the reform caused an increase in the difference in grades between A- and B-schools by 0.283 grades, above a quarter of a grade.

*DiD-estimates*

There can be several reasons for the decline in grades in upper secondary school. One reason is actual decreased learning following decreased motivation in middle school, as mentioned above. In table 8.3.1 in the Appendix, we find that the grades in Mathematics in upper secondary school and middle school are positively correlated.[18] A decrease in Mathematics grades in middle school therefore also decreases Mathematics grades in upper secondary school. In model 5 in tables 5.1.2-5.1.4 above, we controlled for middle school grades in the admission area the year before. The impact of middle school grades on upper secondary school grades should therefore be captured in our model, and control for the indirect effect a decrease in middle school grades have on upper secondary school grades.

Another explanation can be decreased motivation in upper secondary school as a result of altered student composition, where previously motivated students are affected by a student composition consisting of a higher percentage of unmotivated students. We also controlled for school satisfaction in models 3 to 6 in the tables above, which should capture the decrease in grades if there is lower school satisfaction after the reform. However, we see that the effect on grades is stronger after controlling for school variables such as school satisfaction. Therefore, loss of motivation is likely not the reason for the decrease in grades. To emphasise this further we control for every question from the satisfaction survey separately.[19] All survey questions are correlated with the DiD-estimates, while not affecting the estimates too much and therefore likely not explaining the decrease in grades.

[18] Table 8.3.1 in the Appendix indicates that some of the effect on upper secondary school grades is a result of a change in middle school grades, rather than the reform alone.

[19] See table 8.3.2 in the Appendix with satisfaction survey questions as control variables.

A third explanation follows Hoxby's argument about competition being beneficial for every student due to schools improving quality to attract motivated students, thereby raising school quality for all students. In this case, schools have less incentives to be productive, as they no longer compete for students to the same degree. This potential decrease in quality among upper secondary schools leads to all students being affected negatively, resulting in a decrease in average grades.

Fidjeland (2023) argues that any result from the reform can be because of the altered student composition, rather than the reform working to incentivize students or schools. A fourth explanation for the decrease in grades in upper secondary school can be that students with lower grades in middle school have higher chances of gaining access to a close or preferred school, as a result of the reform. This could result in a higher number of lower-achieving students applying and going to upper secondary schools than before. We have controlled for the number of pupils in the first year of upper secondary school and found that a higher enrollment number likely does not explain the decrease in average grades. However, a higher percentage of lower achieving peers should lead to a decrease in average grades across the whole admission area. A decline in grades because of altered student composition aligns with Hægeland et al. (2011) finding student composition to be the most important factor for changes in student achievements.

*DiDiD-estimates*

The DiDiD estimates indicate whether there are any redistributive effects of the reform, as class composition has changed for A-schools and B-schools post-reform. If this is the case, we expect average grades in A-schools to decrease as there is a higher number of lower-achieving students attending these schools than before the reform. However, we found that these estimates are positive, and not negative as we assumed. This suggests that there are no redistributive effects from the reform on average grades between A-schools and B-schools. The positive DiDiD-estimates indicate that residence-based admission leads to more between-school segregation in terms of performance, contradicting the findings of Östh et al. (2013) and Oh & Sohn (2021), but aligning with the results from Bjerre-Nielsen et al. (2023) about regressive redistribution.

One reason for this can be that there still exists competition in the admission system. As the choices per student in Bærum is reduced from nine to four, and in Asker the choices reduce from nine to two, there is still some remaining competition, especially in Bærum. When competition between schools is not eliminated, there is still some degree of self-sorting resulting in between-school differences.

Another explanation can be that there exists neighbourhood segregation in Asker and Bærum, leading to residence-based admission systems still generating between-school differences in performance. When presenting our control variables in section 4, we explained how neighbourhood segregation could lead to differences in student performance or create different class learning environments. One such factor was whether classes consist of students whose parents have high education and income level. While we have controlled for these two factors at municipal level, there still might be some differences at local levels causing neighbourhood segregation.

A third theory is that the school environment creates a learning culture that resides even as student composition changes. We previously mentioned studies showing how schools where classes consist of mostly students with parents that have higher education create a learning environment, or learning culture, that affect the grades of lower performing peers. If such a learning culture is one of the defining traits of elite schools, such a culture will certainly still be prominent for students already attending the school (i.e. classes in year two and three of upper secondary schools for the first year of the admission-reform). It is not unreasonable then to believe that such a learning culture could transmit to new students. Additionally, elite schools are commonly known as elite schools due to a history of having high performing students. Such a history might adjust the mindset of students that attend these schools, adding expectations of a certain level of performance.

### *5.2 Parallel trends*

*Difference-in-differences Assumption*

Our main results show that average grades in middle school decrease from the reform. We also find indications that average grades in upper secondary school decrease and that there are no negative redistributive effects. However, these results only hold under the assumption of parallel trends. We have therefore tested this assumption in this section.

The parallel trend assumption is crucial for a DiD approach. The treatment and control groups should have similar trends before the reform, as the trend for the control group post-treatment represents the trend for the treatment group in the absence of treatment. If the trends are not the same pre-treatment, we are unable to claim that we have a suitable control group for the treatment group, and the causal effect of the policy will not be measured.[20] We have therefore investigated whether these results stem from parallel trends. In figure 5.2.1 we see the observed means in each subject, where the red line is the treatment group, and the blue line is the control group.

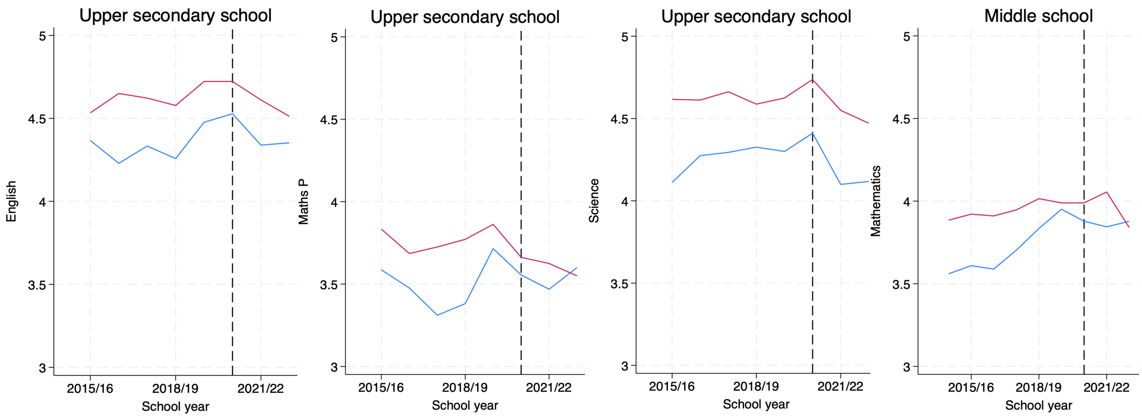


*Figure 5.2.1 Observed means for average grades in English, Practical Mathematics, and Science for upper secondary schools and Mathematics for middle schools*
*Treatment group is red and control group is blue*

We see that the lines appear broadly parallel before treatment. In middle school Mathematics, Oslo grades increased in the school year 2019/20, when the pandemic was impacting the closing of schools. We also observe that the groups are parallel the year after treatment, but then converge. This can be because corona still affected grades in the school year 2021/22, but with absence of restriction in school year 2022/23, the reform is the sole component affecting grades this year.

*Difference-in-difference-in-differences Assumption*

The parallel trend assumption for the DiDiD-approach requires the relative grade results between A-schools and B-schools in the treatment group to trend the same way as the relative grade results in the control group (Olden & Møen, 2021, p. 536).

[20] If the trends are not similar before the reform for the treatment and control group, the estimates might capture differences between the two groups rather than the true effect of the reform. This is therefore a vital assumption.

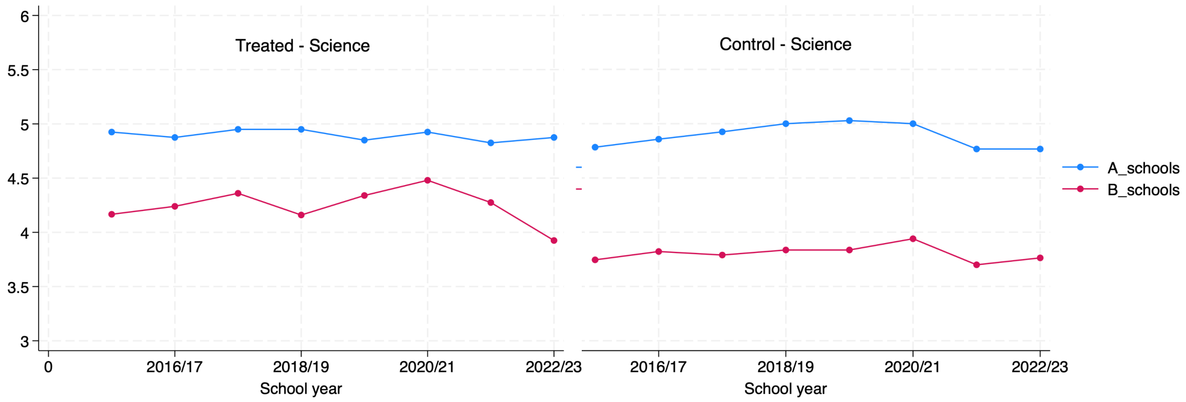


*Figure 5.2.2 Observed means over time for A-schools and B-schools*

In figure 5.2.2 we have tracked the observed mean results in Science for the A-schools and B-schools in Asker and Bærum (treated) and Oslo (control).[21] We see that the difference between A-schools and B-schools is stable over time in Oslo, while in Asker and Bærum the difference increases in 2018/19 as well as in 2022/23. We also observe in figure 5.2.2 that the increase in difference is a result of a decrease in mean grades for B-schools, while the mean grades for A-schools remains unchanged. From figure 5.2.2 we also observe that the mean grades in A-schools in the treated and control groups are similar, while the mean grades in the B-schools are higher in the treated group until 2022/23.

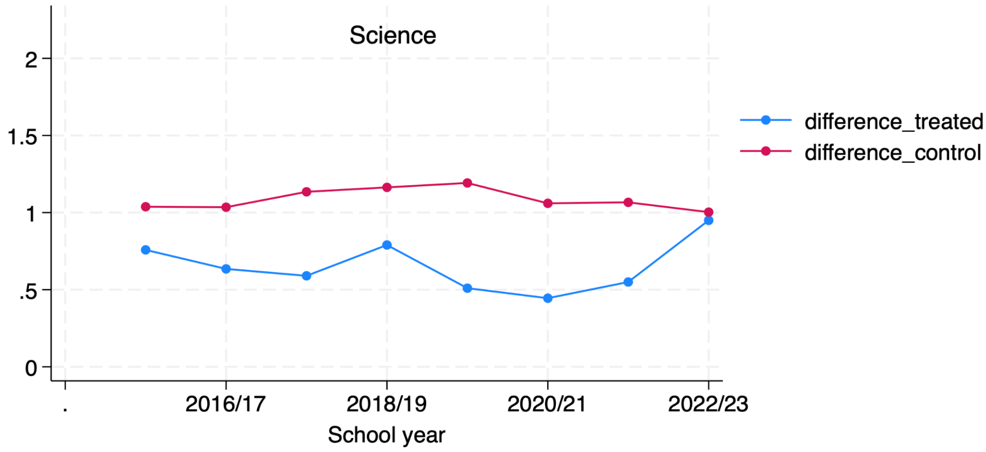


*Figure 5.2.3 Differences in observed means between A-schools and B-schools for average Science grades in upper secondary schools Asker+Bærum (treatment) and Oslo (control)*

Figure 5.2.3 displays the differential in the outcomes of A-schools and B-schools in the treated group and control group over time, which is the parallel trend assumption for the DiDiD estimator (Olden & Møen, 2021). We see that, except from 2018/19, the trend between relative

[21] The observed means for the remaining upper secondary school subjects are in table 8.4.3 in the Appendix.

outcomes is somewhat parallel in absence of treatment (before 2021/22). In the post treatment years (2021/22 and 2022/23) the difference in outcomes increases in the treated group, as indicated in the main results.

*Event Study*

An approach for further examining the parallel-trend assumption is to adopt an event study approach, which we present in figure 5.2.3. This reports event studies in the subjects with the preferred model from the main results, controlling for school and regional differences, as well as differences in corona restrictions. The event study regressions include interaction terms between treated schools and each time period, such as $(2015/16_t * Treated_s)$. This allows us to illustrate the grades in the treated group compared to the control group for every school year. Equation (3) and equation (4) are the event studies conducted for upper secondary school and middle school respectively.[22]

$$(3)\ grade_{sat} = \alpha_0 + \beta_1(2015/16_t * Treated_s) + \beta_2(2016/17_t * Treated_s) + \cdots + \beta_8(2022/23_t * Treated_s) + \theta_1 S_{sat} + \theta_2 D_{at} + \theta_3 M_{at} + \theta_4 CR_{at} + \delta_t + \gamma_s + u_{sat}$$

$$(4)\ grade_{smat} = \alpha_0 + \beta_1(2014/15_t * Treated_s) + \beta_2(2015/16_t * Treated_s) + \cdots + \beta_9(2022/23_t * Treated_s) + \theta_1 S_{smat} + \theta_2 D_{mat} + \theta_3 CR_{at} + \delta_t + \gamma_s + u_{smat}$$

In the graphs in figure 5.2.4, we see that grades are declining after the reform in 2021 in the subjects English and Science, while in Maths P the grades decline the year before.

For middle school Mathematics, grades decrease further in the year 2022/23. This can be explained by a *lagged treatment effect*. While the reform directly affects upper secondary schools by restricting school choice, the reform indirectly affects middle schools by altering student behaviour. As competition is lower than before, because of restricted school choice, middle school students have less incentives to improve their grades. When the reform was introduced in 2021, students at the end of middle school could have been unsure of how the reform would affect them. They might then still have worked hard throughout the year to gain

[22] The control variables are the same as in the preferred models. Here, $\theta$ is the coefficient of the control variables, as opposed to $\beta$ in the preferred models.

admission to their preferred school. The year after, however, middle school students would have better knowledge of the consequences of the reform and how it affected admission. As a result, this class could have worked less hard, since school admission is not solely determined by hard work in middle school, but also by residence. The lagged effect would explain the drop in grades in middle school the year after the reform.

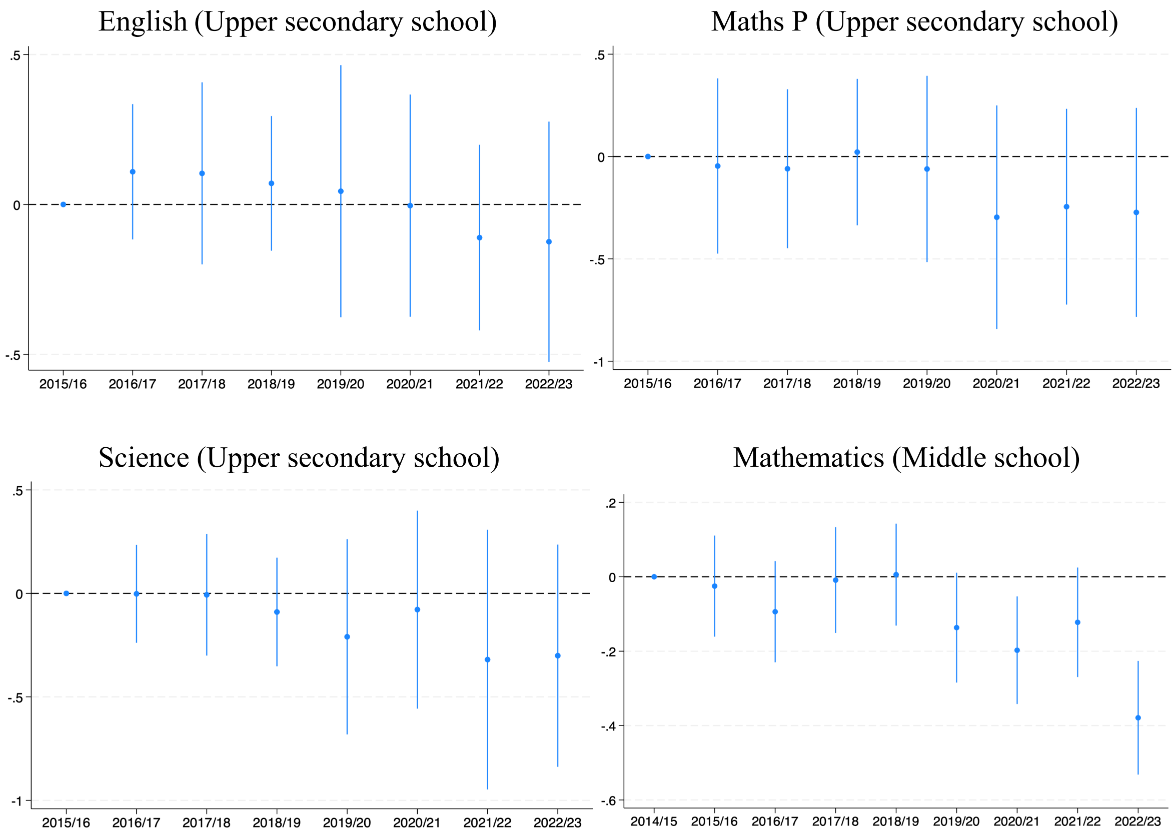


*Figure 5.2.4 Event study for average grades. Treatment occurs in the school year 2021/22*

### *5.3 Additional Subjects*

Up to now we have focused on the grades with the most statistically significant effect of the reform. However, in our dataset we have included all compulsory subjects in the first year of upper secondary school, and similar subjects in the final year of middle school. We therefore present here the results on all subjects included in the dataset.

In table 5.3.1 we see the DiD- and DiDiD-estimates from the preferred model specification in all subjects. We note that all subjects except Maths T have a negative DiD-estimator, and all subjects have a positive DiDiD-estimator. This suggests a decline in most grades due to the reform and no redistributive effects.[23]

*Table 5.3.1 Estimated effect of the reform on all average grades for upper secondary schools*

| Subject | Treated_post | P-value | Treated_Post_A | P-value |
|---|---|---|---|---|
| English | -0.163 | 0.366 | 0.102 | 0.453 |
| | (0.180) | | (0.136) | |
| Geography | -0.442 | 0.156 | 0.146 | 0.391 |
| | (0.310) | | (0.169) | |
| Maths P | -0.519 | 0.309 | 0.410* | 0.081 |
| | (0.508) | | (0.234) | |
| Maths T | 0.555 | 0.307 | 0.111 | 0.659 |
| | (0.541) | | (0.251) | |
| Science | -0.563** | 0.047 | 0.283* | 0.082 |
| | (0.281) | | (0.162) | |
| Social science | -0.182 | 0.380 | 0.249 | 0.118 |
| | (0.207) | | (0.158) | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 5.3.2 Estimated effect of the reform on all average grades for middle schools*

| Subject | Treated_post | P-value |
|---|---|---|
| English | -0.0248 | 0.575 |
| | (0.0442) | |
| Mathematics | -0.249*** | 0.000 |
| | (0.0530) | |
| Science | -0.205*** | 0.000 |
| | (0.0554) | |
| Norwegian | -0.105** | 0.033 |
| | (0.0490) | |
| Social science | -0.125*** | 0.008 |
| | (0.0473) | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In middle school, table 5.3.2, all subjects have a negative DiD-estimator, suggesting a decrease in grades in middle school due to the reform. In four out of five subjects these results are significant, indicating that middle school students respond to lower competition by lowering

[23] In the Appendix: See table 8.4.1 for DiD parallel trends, table 8.4.3 for DiDiD parallel trends and table 8.5.1 for event study graphs for the remaining subjects for upper secondary school.

effort.[24] These results are in line with previous findings, such as those by Haraldsvik (2014) and Fidjeland (2023).

### *5.4 Alternative Control- and Treatment Groups*

*Alternative Control*

In section 4.4 we presented the Stable Unit Treatment Value Assumption, SUTVA, which requires no spillover effects between groups for the DiD estimation to yield unbiased results. This assumption is violated if students in Asker and Bærum moved to Oslo in order to avoid the restrictions of the admission-reform. Therefore, this section explores the issue by looking at an alternative control group Stavanger.

With Stavanger as a control group in table 5.4.1, we found varying results. In Maths P and Science, there are still negative DiD-estimators, while in English the estimate is close to zero. For the DiDiD-estimators, the results could indicate a redistributive effect, but these results are not consistent between subjects, and are not statistically significant.

*Table 5.4.1 The estimated effect of the reform on average grades in English, Practical Mathematics and Science in upper secondary schools when the control group is Oslo (to the left) and Stavanger (to the right)*

| Control:<br>Treatment: Asker+Bærum | Oslo | | | Stavanger | | |
|---|---|---|---|---|---|---|
| Upper secondary school subject: | English | Maths1P | Science | English | Maths1P | Science |
| Treated_post | -0.163 | -0.518 | -0.562** | 0.0365 | -0.318 | -0.237 |
| | (0.180) | (0.508) | (0.281) | (0.237) | (0.417) | (0.283) |
| Treated_Post_A | 0.102 | 0.410 | 0.283 | -0.0680 | -0.128 | 0.125 |
| | (0.135) | (0.233) | (0.161) | (0.162) | (0.287) | (0.177) |
| Constant | 13.01 | -3.151 | -10.24 | 2.169 | 11.63 | 3.430 |
| | (23.48) | (32.08) | (17.67) | (9.609) | (8.734) | (6.932) |
| Observations | 202 | 191 | 200 | 129 | 123 | 126 |
| R-squared | 0.271 | 0.168 | 0.129 | 0.262 | 0.194 | 0.305 |
| Number of S_id | 30 | 30 | 30 | 17 | 17 | 17 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[24] In the Appendix: See table 8.4.2 for DiD parallel trends and table 8.5.2 for event study graphs for the remaining subjects in middle school.

One explanation of why the estimators are different with Stavanger as the control group could be that more motivated students from Asker and Bærum move to Oslo. This way, grades decrease in Asker and Bærum and increase in Oslo. This could then be why grades in Asker and Bærum are lower compared to Oslo, but not compared to Stavanger, after the reform. To test this we controlled for different demographic variables in the DiD- and DiDiD-estimation, including *movements from* for 6-17 year olds, which captures how many people between the age of 6 and 17 that move from an admission area, and *house sales*.[25] Here we found that movements and house sales do not cause changes in the results, indicating that the movement from Asker and Bærum was not increasing abnormally compared to Oslo, causing the decline in grades.

A second explanation is that students from Asker and Bærum may apply for private schools. This is a possibility as admission to private upper secondary schools is not restricted by admission areas or regional admission systems. Most private schools are located in Oslo, not Asker or Bærum. We therefore focus on the admission numbers to schools in Oslo. In table 5.4.2 we see that the admission number decreased by 5 students in 2021 and then increased by 12 students in 2022. We have no information on whether there is an increase in applicants from Asker and Bærum in this period. However, this change in admission number is small, and would likely not affect grades in Asker and Bærum.

*Table 5.4.2 Admission numbers for private schools in Oslo*

| 2015/16 | 2016/17 | 2017/18 | 2018/19 | 2019/20 | 2020/21 | 2021/22 | 2022/23 |
|---|---|---|---|---|---|---|---|
| 856 | 841 | 785 | 835 | 892 | 890 | 885 | 897 |

*Source: Norwegian Directorate for Education and Training, Udir.*

A third explanation could then be that Stavanger is not a suitable control variable for Asker and Bærum. The groups are similar in population density and median income. However, there can still be unobserved cultural differences directly affecting learning. For instance, Stavanger is the "oil capital", and students in upper secondary school could be more focused on this sector, while students in Asker and Bærum focus more on academic disciplines like medicine, law and economics. However, when analysing the parallel trends, we find that the trends are parallel

[25] See table 8.6.3 in Appendix about demographic control variables including *movements from* and *house sales*.

between the two admission areas, especially for subjects like English and Science.[26] A final explanation could therefore be that the results are simply not robust.

*Alternative Treatment*

As an extension to our analysis, we added the admission areas Follo and Romerike as alternative treatment groups.[27] We discussed previously how some of the results might be affected by the fact that competition in Asker and Bærum are not eliminated, but rather just reduced. Follo and Romerike are therefore interesting to look at since these areas have gone from some competition to none or almost none.

In Follo and Romerike, elite schools are not as prominent. This is mainly because of large distances between upper secondary schools in the areas. We have therefore not performed the DiDiD regression on these regions. The larger distances could also result in less competition. Let us call a school within a 5-kilometre radius of another school a "competitive school". In Follo, there are six schools, and no such competitive schools. In Romerike, only five schools out of twelve have competitive schools. In comparison, all nine schools in Asker and Bærum have competitive schools,[28] and six of these schools have more than five competitive schools.

*Table 5.4.3 The estimated effect of the reform on average grades in upper secondary schools in Asker+Bærum, Follo and Romerike*

| Treatment: Control: Oslo | Asker+Bærum | | | Follo | | | Romerike | | |
|---|---|---|---|---|---|---|---|---|---|
| Upper secondary school subject: | English | Maths1P | Science | English | Maths1P | Science | English | Maths1P | Science |
| Treated_post | -0.101 | -0.311 | -0.422 | 0.325* | 0.732 | 0.161 | -0.630 | 0.373 | -0.410 |
| | (0.166) | (0.501) | (0.271) | (0.168) | (0.635) | (0.207) | (0.435) | (2.710) | (0.592) |
| Constant | 12.98 | -4.574 | -11.24 | -18.23 | 109.4 | 24.66 | 8.563 | -36.15 | 23.16 |
| | (23.43) | (32.32) | (17.74) | (35.17) | (118.3) | (27.38) | (16.13) | (118.9) | (56.61) |
| Observations | 202 | 191 | 200 | 180 | 171 | 180 | 224 | 224 | 226 |
| R-squared | 0.265 | 0.145 | 0.110 | 0.333 | 0.169 | 0.089 | 0.278 | 0.157 | 0.066 |
| Number of S_id | 30 | 30 | 30 | 27 | 27 | 27 | 33 | 33 | 33 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[26] In the Appendix: See table 8.6.1 for DiD parallel trends and table 8.6.2 for DiDiD parallel trends comparing Stavanger to Asker and Bærum.

[27] Follo, Romerike and Asker+Bærum constitute the region Akershus, and all admission areas were therefore affected by the Viken reform and the admission reform.

[28] See descriptive statistics over "schools in 5km radius" in table 8.6.4 in the Appendix.

From table 5.4.3 we see that grades in Follo increase, while grades in Romerike are inconsistent across subjects. This indicates that the reform did not affect grades in these areas the same as it did in Asker and Bærum, even though we expected a negative effect since competition was completely removed. The reason for this could be that the schools and students in Follo and Romerike were not characterised by as much competition before the reform as Asker and Bærum. Particularly in Follo, upper secondary schools are placed far apart from each other. Consequently, students might have already been prioritising schools geographically closer to where they live. With a small degree of competition both before and after the reform, it could have had no effect on grades.[29] Like Haraldsvik (2014), we find that the admission-reform has a stronger effect in municipalities where competition is more prominent.

The fact that grades increased in Follo after the reform could be that, due to students now gaining an advantage when applying to neighbourhood schools, students who previously would have applied to their neighbourhood school without being able to get in due to poorer grades, now end up in the schools they wanted either way. This could help them get into a school environment that might be better for them. While the reform then wouldn't change their mindset in middle school, it could potentially be motivating for them in upper secondary school to attend the school they originally wanted to go to. This follows the theory that students in Follo originally wanted to enter schools geographically close to them both before and after the reform and would therefore have a different effect than the one in Asker and Bærum.

*Table 5.4.4 Estimated effect of average Mathematics grades for middle schools in Asker+Bærum, Follo and Romerike*

| Treatment: | Asker+Bærum | | Follo | Romerike |
|---|---|---|---|---|
| Control: | Oslo | Stavanger | Oslo | Oslo |
| Middle school subject: | Mathematics | | | |
| Treated_post | -0.249*** | -0.0556 | -0.00586 | -0.139 |
| | (0.0530) | (0.0670) | (0.0740) | (0.0900) |
| Constant | -6.821 | 4.661*** | 4.173** | 0.976 |
| | (4.615) | (1.011) | (1.870) | (1.317) |
| Observations | 648 | 459 | 612 | 756 |
| R-squared | 0.299 | 0.248 | 0.318 | 0.264 |
| Number of S_id | 76 | 56 | 72 | 89 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[29] In the Appendix: See tables 8.6.5 and 8.6.6 for parallel trends for respectively Follo and Romerike compared to Oslo. We see here that for the subjects in table 5.4.3, the trends are fairly parallel.

In table 5.4.4 we observe similar results for middle schools as for upper secondary schools. The effect on grades is gradually smaller the less competition there was between upper secondary schools before the reform. In Follo, upper secondary schools were placed the farthest apart from each other and therefore had the least competition, which also resulted in the reform having the least effect on grades in Mathematics there. In Romerike, there were areas with competitive upper secondary schools, and the effect on middle school grades from the restrictive reform is larger here, although not statistically significant.

When introducing Stavanger as a control in table 5.4.4, the results still give a negative DiD-estimate, but with a notably smaller coefficient, which also turns statistically insignificant. It is unlikely that, because of the reform, middle school students moved to Oslo before completing middle school, to attend a desired upper secondary school. This explanation therefore doesn't hold much weight for why the DiD-estimate decreases drastically when introducing Stavanger as a control group. We therefore return to the possibility that Stavanger and Asker and Bærum are not comparable groups, and we see no effects from the reform.

### *5.5 Alternative Outcome Variables*

This section seeks to investigate other factors that could affect our results, which aren't included in our preferred model. The main goal for this section is to exclude the possibility that the effect on grades is spurious and caused indirectly by another effect from the reform. For this robustness test we replaced grades as the outcome variable with alternative outcomes. We have also included relevant control variables, such as corona restrictions, corona infections and median income to account for differences between the municipalities.

The effect on grades can be due to changes in movement between the treated and control group. To investigate whether the reform affects movement and house sales, we have included *movements from a municipality* and *house sales* as outcome variables in table 5.5.1. It is also reasonable to assume that the reform affected absence in school, which in turn affects grades. We therefore included absence in both middle school and upper secondary school as outcome variables.

In table 5.5.1, when looking at movements we see a small and insignificant increase in movements from the treatment group as a result of the reform. As this variable is measured in numbers, the reform led to an increase in 17 people between 6 and 17 years old moving out of Asker or Bærum. This is a very small change and suggests that the reform did not cause much movement out of the municipalities. To investigate whether the DiDiD-estimator is affected by manipulation by self-sorting, we have also added *movements within* a municipality as a control variable in table 8.6.3 in the Appendix. Here we see that movements within does not alter the DiDiD-estimator, and the lack of redistributive findings is likely not due to manipulation by moving closer to a preferred school.

*Table 5.5.1 Estimated effects on alternative outcome variables when controlling for the pandemic and median household income*

| Variables | Movements from municipality (6-17 years old) | House sales percent of population | Middle school absence | Upper secondary school absence |
|---|---|---|---|---|
| Treated_post | 17.91 | 0.298*** | 3.870*** | 8.540** |
| | (13.13) | (0.0334) | (0.984) | (3.658) |
| Weekswithredlevel | -113.8*** | 0.154*** | -4.540*** | -0.740 |
| | (9.795) | (0.0249) | (0.734) | (0.732) |
| Coronainfectedpercent | 148.3*** | -0.114*** | 2.713*** | 2.842* |
| | (8.225) | (0.0209) | (0.624) | (1.698) |
| Incomehouseholdmedian | -0.00679*** | -0.0000423*** | -9.30e-05** | -0.001 |
| | (0.000489) | (1.24e-06) | (4.21e-05) | (0.001) |
| Year dummies | YES | YES | YES | YES |
| School fixed effects | | | YES | YES |
| Constant | 4,395.6*** | 19.63*** | 52.43*** | 365.6* |
| | (259.7) | (0.525) | (19.90) | (403.1) |
| Observations | 684 | 684 | 671 | 239 |
| R-squared | 0.870 | 0.744 | 0.363 | 0.121 |
| Number of S_id | | | 76 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The table also suggests that house sales increased by about 0,3 percent as a result of the reform. These findings align with Machin & Salvanes (2016) finding that house patterns are being affected by admission reforms. However, when controlling for house sales in table 8.6.3 in the Appendix, this did not alter the DiD-estimate on grades. It is also less likely that parents would move houses for their children to gain admission to another school within only two years with the reform. This relation is therefore highly interesting, but an unlikely explanation of the effect on grades from the reform.

We also see that the reform increased absence by 3.8 hours in middle school and 8.5 hours in upper secondary school. The absence is lower with more restrictions, as home-schooling during the pandemic changed the rules for absence in school. Absence also increased with infections, which is also reasonable. After controlling for restrictions and infections from the pandemic, absence still increased after the reform. These are interesting results, which can have several explanations. Students can have less motivation because the reform decreased the need for good grades in middle school. Students can also have less motivation in school when they do not get the chance to gain admission to the school they prefer. We will not try to explain these results further, other than investigate whether absence can explain the decrease in grades from the reform. When controlling for absence in upper secondary schools, we find that absence does not alter the DiD-estimate on grades, and there seems to be little to no correlation between the two.[30]

We therefore do not find the decrease in grades to be a spurious effect of the reform due to the reform affecting movement or absence.

### *5.6 Alternative Explanations*

In this section we seek to investigate specifically whether there are any alternative explanations for our estimated results. The two explanations we address here are the teacher strike in 2022 and corona infections, as these are events that occurred around the same time as the reform and could affect grades. Particularly the pandemic is one of the main challenges with estimating the effect of the reform on average grades. We therefore investigate the teacher strike first and the corona infected last.

*Teacher Strike*

The teacher strike lasted from 20th of June to 27th of September in 2022, not long after the last corona restrictions were lifted in February 2022. In the beginning of the school year 2022/23, students were excited to finally have a school year without closed schools and restrictions. However, due to the teacher strike, classes could not be held after all, and students were left without any form of teaching. We clarify that this only affected the students in schools that were chosen by The Education Association (Utdanningsforbundet) to strike. Schools in Oslo were not affected, but some schools in Asker and Bærum were.

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[30] See table 8.7.1 in the Appendix where absence is a control variable to the DiD- and DiDiD-estimates in upper secondary school.

The Education Association publishes which schools, and the number of teachers per school, that are selected to strike in every phase of the strike (Utdanningsforbundet, 2022). We therefore have school specific information where we include the number of teachers on strike per school. This affected both middle schools and upper secondary schools. In tables 5.6.1 and 5.6.2 we present descriptive statistics of the number of teachers selected to strike per school for the whole duration of the strike.

*Table 5.6.1 Number of teachers on strike in upper secondary schools in Asker, Bærum and Oslo (vgs is short for "videregående skole" meaning "upper secondary school")*

| School in Asker+Bærum | Teachers on strike |
|---|---|
| Asker vgs | 0 |
| Dønski vgs | 26 |
| Eikeli vgs | 23 |
| Nadderud vgs | 25 |
| Nesbru vgs | 0 |
| Rosenvilde vgs | 51 |
| Sandvika vgs | 28 |
| Stabekk vgs | 33 |
| Valler vgs | 25 |
| Schools in Oslo | 0 |

*Table 5.6.2 Number of teachers on strike in middle schools in 2022*

| Middle school in Bærum | Teachers on strike |
|---|---|
| Bekkestua | 42 |
| Bjørnegård | 20 |
| Eineåsen | 19 |
| Gjettum | 24 |
| Hauger | 24 |
| Hosletoppen | 15 |
| Hundsund | 25 |
| Mølladammen | 26 |
| Ramstad | 41 |
| Ringstabekk | 27 |
| Vøyenenga | 23 |
| Østerås | 24 |
| Middle schools in Asker | 0 |
| Middle schools in Oslo | 0 |

To control for the teacher strike in the fall of 2022, we have calculated the percentage of teachers on strike per student for each school and added the "teacher strike" as a control to the preferred model.

We see in table 5.6.3 that the negative DiD-estimates and positive DiDiD-estimates are not altered much, though Science is no longer statistically significant. We also observe that the teacher strike had a varying, small and insignificant effect on the DiD-estimates. This indicates that the teacher strike had no effect on the decrease in grades in Asker and Bærum compared to Oslo.

*Table 5.6.3 Estimated effect of the reform on average grades when controlling for Teacher strike in the preferred model*

| | Upper secondary school | | | Middle school |
|---|---|---|---|---|
| Variables | English | Maths1P | Science | Mathematics |
| Treated_post | -0.185 | -0.484 | -0.521 | -0.254*** |
| | (0.183) | (0.520) | (0.328) | (0.0648) |
| Treated_Post_A | 0.110 | 0.403* | 0.280* | |
| | (0.136) | (0.235) | (0.163) | |
| Teachers on strike | 0.0230 | -0.0137 | -0.00742 | 0.0589 |
| (per student) | (0.0308) | (0.0417) | (0.0303) | (0.449) |
| Constant | 21.13 | -1.642 | -8.922 | -7.060 |
| | (25.90) | (32.50) | (18.54) | (4.966) |
| Observations | 202 | 191 | 200 | 648 |
| R-squared | 0.274 | 0.170 | 0.129 | 0.299 |
| Number of S_id | 30 | 30 | 30 | 76 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Corona Infections*

A second explanation for the decrease in grades after the reform in 2021, are the amount of corona infections. After the restrictions lifted in February 2022, the infection numbers were rising, which could affect the attendance in school and student grades. The number of infections during the pandemic are, although affecting, not consequent with the degree of restrictions. To control for infection in addition to restrictions will give a holistic picture of the impact from the corona pandemic.

From publicly available data from The Norwegian Institute of Public Health, we have gathered the number of corona infected for each admission area in upper secondary data, and for each municipality for middle school data. Hence, we included "corona infections as a percentage of the population" as a control to the preferred model.

As we see in table 5.6.4, when controlling for corona infections the negative DiD-estimates and positive DiDiD-estimates are even stronger. In upper secondary school, corona infections and grades are not highly correlated. In middle school, corona infections are likely to affect grades negatively, but controlling for how infections affect grades, the effect from the reform is still negative on grades.

*Table 5.6.4 Estimated effect of the reform on average grades when controlling for Corona infections in the preferred model*

| | Upper secondary school | | | Middle school |
|---|---|---|---|---|
| Variables | English | Maths1P | Science | Mathematics |
| Treated_post | -0.518 | -0.574 | -0.610** | -0.361*** |
| | (0.480) | (0.511) | (0.300) | (0.0643) |
| Treated_Post_A | 0.105 | 0.416* | 0.287* | |
| | (0.136) | (0.234) | (0.162) | |
| Corona infections | -0.187 | -0.113 | 0.0490 | -0.130*** |
| (percent of population) | (0.235) | (0.116) | (0.106) | (0.0432) |
| Constant | -40.56 | -2.577 | -11.31 | -3.022 |
| | (71.25) | (32.09) | (17.87) | (4.752) |
| Observations | 202 | 191 | 200 | 648 |
| R-squared | 0.274 | 0.175 | 0.130 | 0.311 |
| Number of S_id | 30 | 30 | 30 | 76 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In the school year 2022/23, corona infections were no longer regarded as a *disease dangerous to the public*, and would, as any other flu, likely not have different regional effects on attendance and grades. Therefore, we set the infection numbers equal to zero in 2022/23 and find that this modification does not alter the results.[31] It therefore seems that the reform still negatively impacts the grades, even when accounting for corona infections.

When exploring alternative explanations to the results on grades, we found that neither the teacher strike nor corona infections alter the results. We therefore conclude that the results on grades stem from the admission reform.

[31] See table 8.8.1 in Appendix accounting for no infections in 2022/23.

## 6. Conclusion

In this thesis we have analysed the effect on grades from the admission reform of 2021, restricting school choice for upper secondary admission in Asker and Bærum. We have analysed the effect on average grades in middle school, to investigate whether the reform had competitive effects on effort. In addition, we have analysed the effect on grades in upper secondary school, and discussed whether there were any sustained effects from middle school on grades. Lastly, we have performed a DiDiD regression to analyse the heterogeneous effects from the reform on A-schools compared to B-schools, to find if there are any redistributive effects from the reform.

Our main results indicate lower middle school grades, lower upper secondary school grades and no redistributive effects. We find statistically significant results for middle schools at a 1% level, showing effects of the reform on average grades by a quarter of a grade decrease. These findings are aligned with previous research and indicate that less competition for admission leads to less effort in school. With little previous research estimating the effect from an admission reform on upper secondary grades, this thesis contributes to the research and found indications of lower grades in upper secondary school as a result from the residence-based admission reform. These results vary across subjects but indicate a negative treatment effect on average grades in Asker and Bærum. This could be a direct result of lower actual learning in middle school, or as a result of a change in student composition. However, these results seem to vary between regions as well, as Follo and Romerike show no indications of a negative treatment effect for upper secondary schools. Our findings on no redistributive effects from the residence-based admission reform, is not aligned with previous research finding that restricted school choice leads to less between-school differences. A reason for this can be that the reform was incapable of eliminating competition.

In our analysis, we have also explored alternative outcomes and alternative explanations. We found the reform to have other outcomes, such as an increase in house sales and absence in school. However, these variables did not explain the effect on grades from the reform, and we conclude that the effect on grades was not a spurious effect. We also explored alternative explanations for the effect on grades, such as the teacher strike in 2022, and corona infections. We found that these explanations did not reduce the effect on grades from the reform, and we thereby conclude that the admission reform is the likely factor affecting grades in our analysis.

A limitation in the data for this thesis is that individual data was not available to us, making the analysis unable to control for individual factors affecting student attainment. We also faced restrictions to what we could find and use due to the data having to be public. Operating with school data may cause the variation in our data to be significantly lower, leading to less precise estimates and this impacting upon statistical significance. We recommend further research on the Akershus/Viken admission reform, as this is a unique opportunity to analyse the effect of restricting school choice on grades, or other outcomes.

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### *7.1 Additional Resources*

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# 8. Appendix

## *8.1 Data*

*Table 8.1.1 Summary statistics for grades in upper secondary school, only year 2018/19*

| *Upper secondary school 2018/19* | | | | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| English | 9 | 4.578 | .335 | 4.2 | 5 |
| Geography | 9 | 4.533 | .545 | 3.6 | 5.1 |
| Maths P | 7 | 3.771 | .594 | 3.1 | 4.5 |
| Maths T | 7 | 3.971 | .553 | 3.1 | 4.5 |
| Science | 9 | 4.511 | .494 | 3.7 | 5 |
| Social science | 7 | 4.843 | .412 | 4.2 | 5.3 |
| Oslo | Obs | Mean | Std. Dev. | Min | Max |
| English | 19 | 4.258 | .688 | 2.7 | 5.3 |
| Geography | 21 | 4.443 | .69 | 3.5 | 5.7 |
| Maths P | 16 | 3.381 | .735 | 2.2 | 4.6 |
| Maths T | 15 | 3.933 | .673 | 2.9 | 5 |
| Science | 19 | 4.326 | .678 | 3.4 | 5.6 |
| Social science | 20 | 4.48 | .685 | 2.9 | 5.4 |

*Table 8.1.2 Summary statistics for grades in middle school, only year 2018/19*

| *Middle school 2018/19* | | | | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| English | 22 | 4.361 | .213 | 4.05 | 5 |
| Mathematics | 22 | 4.023 | .318 | 3.4 | 4.7 |
| Norwegian | 20 | 4.095 | .224 | 3.8 | 4.5 |
| Science | 21 | 4.39 | .277 | 4 | 5.1 |
| Social science | 21 | 4.514 | .274 | 4.1 | 5.1 |
| Oslo | Obs | Mean | Std. Dev. | Min | Max |
| English | 54 | 4.324 | .307 | 3.65 | 5 |
| Mathematics | 54 | 3.826 | .349 | 3 | 4.4 |
| Norwegian | 54 | 3.922 | .344 | 3.3 | 4.6 |
| Science | 54 | 4.209 | .336 | 3.6 | 4.9 |
| Social science | 54 | 4.419 | .313 | 3.8 | 5.2 |

*Table 8.1.3 Summary statistics for school variables in upper secondary school (number 1-7 are the student satisfaction survey questions)*

| *School variables* | | | *Upper secondary school* | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| Q1: Work peace | 77 | 4.009 | .322 | 3.3 | 4.6 |
| Q2: Class motivation | 78 | 3.938 | .345 | 3.1 | 4.6 |
| Q3: Satisfaction | 77 | 4.295 | .118 | 4 | 4.5 |
| Q4: Learning interest | 76 | 3.945 | .184 | 3.4 | 4.4 |
| Q5: School work interest | 79 | 3.301 | .161 | 2.9 | 3.7 |
| Q6: School interest | 79 | 3.616 | .193 | 3.1 | 4 |
| Q7: Challenges | 78 | 4.479 | .15 | 4.1 | 4.8 |
| Schoolsin5kmradius | 81 | 4.111 | 1.803 | 1 | 6 |
| Teachers on strike | 81 | 2.605 | 8.93 | 0 | 51 |
| Pupils | 72 | 619.292 | 167.764 | 394 | 953 |
| vg1Generalstudies | 72 | 156.917 | 45.505 | 83 | 242 |
| Absence | 62 | 8.943 | 6.218 | 2 | 32.5 |
| Oslo | Obs | Mean | Std. Dev. | Min | Max |
| Q1: Work peace | 184 | 3.755 | .352 | 2.9 | 4.6 |
| Q2: Class motivation | 182 | 3.84 | .328 | 3 | 4.5 |
| Q3: Satisfaction | 183 | 4.204 | .169 | 3.4 | 4.5 |
| Q4: Learning interest | 183 | 3.923 | .175 | 3.5 | 4.4 |
| Q5: School work interest | 184 | 3.372 | .194 | 2.8 | 4 |
| Q6: School interest | 185 | 3.622 | .213 | 3 | 4.1 |
| Q7: Challenges | 182 | 4.304 | .236 | 3.4 | 4.7 |
| Schoolsin5kmradius | 189 | 9.714 | 4.496 | 0 | 15 |
| Teachers on strike | 189 | 0 | 0 | 0 | 0 |
| Pupils | 187 | 760.973 | 251.879 | 152 | 1451 |
| vg1Generalstudies | 187 | 179.492 | 59.959 | 56 | 327 |
| Absence | 117 | 12.0726 | 7.511 | 2 | 33 |

*Table 8.1.4 Summary statistics for school variables in middle school (number 1-7 are the student satisfaction survey questions)*

| *School variables* | | | *Middle school* | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| Q1: Work peace | 194 | 3.658 | .274 | 2.9 | 4.3 |
| Q2: Class motivation | 194 | 3.702 | .231 | 3 | 4.4 |
| Q3: Satisfaction | 194 | 4.213 | .153 | 3.8 | 4.6 |
| Q4: Learning interest | 194 | 3.891 | .175 | 3.3 | 4.3 |
| Q5: School work interest | 194 | 3.24 | .164 | 2.9 | 3.7 |
| Q6: School interest | 194 | 3.42 | .191 | 2.9 | 4.1 |
| Q7: Challenges | 194 | 4.313 | .133 | 3.9 | 4.6 |
| Operational costs | 198 | 109053.89 | 11363.336 | 91772 | 131971 |
| Wage expenses | 198 | 81501.434 | 7316.292 | 69743 | 96466 |
| Inventory expenses | 198 | 1015.747 | 405.483 | 465 | 1735 |
| Material expenses | 198 | 1578.581 | 761.369 | 843 | 3955 |
| Teachers on strike | 198 | 1.566 | 6.465 | 0 | 42 |
| Pupils 10$^{th}$ grade | 198 | 115.586 | 29.82 | 55 | 228 |
| Teacher density | 198 | 20.581 | 2.848 | 15.26 | 33.78 |
| Absence | 194 | 7.351 | 4.988 | 0 | 25 |

| Oslo | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Q1: Work peace | 465 | 3.568 | .31 | 2.6 | 4.7 |
| Q2: Class motivation | 465 | 3.665 | .26 | 2.7 | 4.6 |
| Q3: Satisfaction | 466 | 4.182 | .176 | 3.1 | 4.7 |
| Q4: Learning interest | 466 | 3.906 | .21 | 3.1 | 4.4 |
| Q5: School work interest | 466 | 3.327 | .209 | 2.3 | 3.9 |
| Q6: School interest | 466 | 3.423 | .226 | 2.8 | 4.1 |
| Q7: Challenges | 466 | 4.19 | .177 | 3.7 | 4.6 |
| Operational costs | 486 | 130162.22 | 17933.617 | 110424 | 165403 |
| Wage expenses | 486 | 90924.778 | 10114.084 | 79611 | 109929 |
| Inventory expenses | 486 | 1510.667 | 342.75 | 1049 | 2173 |
| Material expenses | 486 | 1554.667 | 473.305 | 1187 | 2490 |
| Teachers on strike | 486 | 0 | 0 | 0 | 0 |
| Pupils 10th grade | 478 | 106.651 | 52.77 | 6 | 267 |
| Teacher density | 481 | 20.03 | 3.377 | 8.48 | 33.76 |
| Absence | 477 | 8.937 | 4.536 | 0 | 26 |

*Table 8.1.5 Regional variables in upper secondary school dataset*

| *Regional variables* | *Upper secondary school* | | | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| Weeks with red level | 81 | 3.556 | 6.419 | 0 | 20 |
| Population 10-14 y/o | 81 | 15028.778 | 313.364 | 14397 | 15451 |
| Population 15-19 y/o | 81 | 14578.333 | 307.542 | 14266 | 15199 |
| Total population | 81 | 221791.89 | 5050.917 | 213359 | 229736 |
| Corona infected | 81 | 7374.667 | 18204.453 | 0 | 58273 |
| Corona infections percent | 81 | 3.279 | 8.088 | 0 | 25.891 |
| House sales | 81 | 3924.222 | 536.338 | 3336 | 5046 |
| House price | 81 | 27100808 | 5904886.6 | 20138352 | 38966262 |
| Median income | 72 | 658262.24 | 36375.412 | 612226 | 721870 |
| Households percent | 72 | 7.847 | .045 | 7.754 | 7.909 |
| Households number | 72 | 17323.375 | 263.692 | 16874 | 17653 |
| Higher edu percent | 72 | 14.421 | 1.329 | 12.769 | 16.523 |
| Higher edu number | 72 | 31897.25 | 3560.151 | 27244 | 37615 |
| Movements from 6-17 y/o | 81 | 635.556 | 67.146 | 525 | 745 |
| Movements within | 81 | 11906 | 1262.4 | 10564 | 14105 |
| Oslo | Obs | Mean | Std. Dev. | Min | Max |
| Weeks with red level | 189 | 4.556 | 7.896 | 0 | 24 |
| Population 10-14 y/o | 189 | 34300.889 | 1787.382 | 30922 | 36274 |
| Population 15-19 y/o | 189 | 32149.667 | 1782.688 | 30171 | 35427 |
| Total population | 189 | 688529.67 | 18790.849 | 658390 | 717710 |
| Corona infected | 189 | 26711.556 | 61541.269 | 0 | 197787 |
| Corona infections percent | 189 | 3.818 | 8.794 | 0 | 28.262 |
| House sales | 189 | 11326.444 | 811.814 | 10330 | 12731 |
| House price | 189 | 76555444 | 12811358 | 56342641 | 91354876 |
| Median income | 168 | 485750 | 36057.962 | 438000 | 548000 |
| Households percent | 168 | 5.665 | .139 | 5.453 | 5.885 |
| Households number | 168 | 38818.375 | 1878.51 | 35901 | 41725 |
| Higher edu percent | 168 | 17.378 | 1.183 | 15.645 | 19.358 |
| Higher edu number | 168 | 119210.88 | 10960.34 | 103003 | 137256 |
| Movements from 6-17 y/o | 189 | 1738.667 | 115.602 | 1597 | 2000 |
| Movements within | 189 | 88981.667 | 2840.789 | 83619 | 92345 |

*Table 8.1.6 Regional variables in middle school dataset*

| *Regional variables* | *Middle school* | | | | |
|---|---|---|---|---|---|
| Asker+Bærum | Obs | Mean | Std. Dev. | Min | Max |
| Population 10-14 y/o | 198 | 7634.197 | 1036.519 | 6163 | 8738 |
| Population 15-19 y/o | 198 | 7404.47 | 916.767 | 6223 | 8505 |
| Total population | 198 | 112778.15 | 16182.017 | 89974 | 129874 |
| Corona infected | 198 | 4332.495 | 10960.541 | 0 | 38916 |
| Corona infected percent | 198 | 3.745 | 9.371 | 0 | 30.172 |
| Weeks with red level | 198 | 3.222 | 5.955 | 0 | 17 |
| House sales | 198 | 2053.914 | 633.895 | 927 | 2814 |
| House price | 198 | 13738846 | 5384601.9 | 5369706 | 23794401 |
| Median income | 198 | 652126.26 | 39836.075 | 593000 | 724000 |
| Households percent | 198 | 7.857 | .146 | 7.715 | 8.237 |
| N/o households | 198 | 8840.641 | 1143.942 | 7403 | 10081 |
| Higher edu percent | 198 | 14.281 | 3.332 | 8.727 | 18.498 |
| Higher edu number | 198 | 16614.167 | 5797.16 | 7852 | 24024 |
| Movements from 6-17 y/o | 198 | 321.646 | 70.328 | 198 | 418 |
| Movements within | 198 | 6209.753 | 1822.929 | 3130 | 8266 |
| Oslo | Obs | Mean | Std. Dev. | Min | Max |
| Population 10-14 y/o | 486 | 33640.667 | 2091.952 | 30099 | 36274 |
| Population 15-19 y/o | 486 | 31518.556 | 1488.425 | 29747 | 34515 |
| Total population | 486 | 680748.11 | 19552.454 | 647676 | 709037 |
| Corona infected | 486 | 29526.667 | 69273.092 | 0 | 223005 |
| Corona infected percent | 486 | 4.221 | 9.899 | 0 | 31.866 |
| Weeks with red level | 486 | 3.556 | 6.608 | 0 | 19 |
| House sales | 486 | 11392.667 | 796.745 | 10330 | 12731 |
| House price | 486 | 72448018 | 13641064 | 53287496 | 91354876 |
| Median income | 486 | 478777.78 | 39254.286 | 423000 | 548000 |
| Households percent | 486 | 5.631 | .161 | 5.364 | 5.885 |
| N/o households | 486 | 38365.222 | 2184.178 | 34740 | 41725 |
| Higher edu percent | 486 | 17.09 | 1.38 | 14.785 | 19.358 |
| Higher edu number | 486 | 116604.89 | 12680.929 | 95757 | 137256 |
| Movements from 6-17 y/o | 486 | 1721.654 | 140.363 | 1465 | 2000 |
| Movements within | 486 | 89455.111 | 2641.324 | 83619 | 92345 |

*Equation 8.1.1 DiDiD-estimator rearranged to look at the effect on A-schools in the treatment compared to the control group, before and after the reform, controlling for B-schools.*

$$\beta_{DiDiD} = \left(\left(grade_{A,T,after} - grade_{A,C,after}\right) - \left(grade_{A,T,before} - grade_{A,C,before}\right)\right) - \left(\left(grade_{B,T,after} - grade_{B,C,after}\right) - \left(grade_{B,T,before} - grade_{B,C,before}\right)\right)$$

*Equation 8.1.2 DiDiD-estimator rearranged to look at the effect on A-schools compared to B-schools before and after the reform, controlling for the control group*

$$\beta_{DiDiD} = \left(\left(grade_{A,T,after} - grade_{B,T,after}\right) - \left(grade_{A,T,before} - grade_{B,T,before}\right)\right) - \left(\left(grade_{A,C,after} - grade_{B,C,after}\right) - \left(grade_{A,C,before} - grade_{B,C,before}\right)\right)$$

## *8.2 Empirical Methodology*

*Table 8.2.1 Main results (preferred model) with and without fixed effects*

| Upper secondary school<br>English | (1) | (2) |
|---|---|---|
| Treated | - | 4.550 |
| | | (10.58) |
| Post_reform | 3.724 | 3.593 |
| | (7.069) | (7.267) |
| Treated_post | -0.163 | -0.207 |
| | (0.180) | (0.185) |
| A_schools | - | 0.741*** |
| | | (0.116) |
| Treated_A | - | -0.374* |
| | | (0.199) |
| Post_A | 0.0206 | 0.0445 |
| | (0.0801) | (0.0811) |
| Treated_Post_A | 0.102 | 0.150 |
| | (0.136) | (0.139) |
| Fixed effects | YES | NO |
| Constant | 13.016 | 11.582 |
| | (23.49) | (20.84) |
| Observations | 202 | 202 |
| R-squared | 0.271 | |
| Number of S_id | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

## *8.3 Main Results*

*Table 8.3.1 Estimated effect of the reform on upper secondary grades when controlling for middle school grades*

| | *Upper secondary grades* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Middle school grades** | (1)<br>English | (2)<br>English | (3)<br>Geography | (4)<br>Geography | (5)<br>Maths1P | (6)<br>Maths1P | (7)<br>Maths1T | (8)<br>Maths1T | (9)<br>Science | (10)<br>Science | (11)<br>Social Science | (12)<br>Social Science |
| Treated_post | -0.152* | -0.147* | 0.120 | 0.0650 | -0.286** | -0.233 | 0.000114 | -0.0824 | -0.120 | -0.176 | -0.117 | -0.137 |
| | (0.0812) | (0.0820) | (0.101) | (0.124) | (0.140) | (0.141) | (0.162) | (0.161) | (0.0956) | (0.114) | (0.0942) | (0.107) |
| Treated_Post_A | 0.0479 | 0.0487 | -0.118 | -0.118 | 0.289 | 0.289 | 0.0690 | 0.0778 | 0.199 | 0.198 | 0.0951 | 0.0936 |
| | (0.124) | (0.124) | (0.155) | (0.155) | (0.214) | (0.211) | (0.231) | (0.226) | (0.142) | (0.142) | (0.142) | (0.142) |
| Science | | | | -0.0276 | | | | | | -0.422 | | |
| | | | | (0.721) | | | | | | (0.474) | | |
| Socialscience | | | | -0.450 | | | | | | | | -0.190 |
| | | | | (0.690) | | | | | | | | (0.482) |
| English | | 0.172 | | | | | | | | | | |
| | | (0.347) | | | | | | | | | | |
| Maths | | | | | | 1.029** | | -1.429*** | | | | |
| | | | | | | (0.474) | | (0.508) | | | | |
| Constant | 4.368*** | 3.639** | 4.466*** | 6.534*** | 3.615*** | -0.0344 | 3.859*** | 8.931*** | 4.347*** | 6.084*** | 4.601*** | 5.425** |
| | (0.0367) | (1.466) | (0.0453) | (2.391) | (0.0649) | (1.684) | (0.0628) | (1.803) | (0.0412) | (1.954) | (0.0434) | (2.094) |
| Observations | 209 | 209 | 220 | 220 | 197 | 197 | 185 | 185 | 207 | 207 | 205 | 205 |
| R-squared | 0.218 | 0.220 | 0.096 | 0.100 | 0.120 | 0.146 | 0.077 | 0.125 | 0.090 | 0.094 | 0.089 | 0.090 |
| Number of S_id | 30 | 30 | 30 | 30 | 30 | 30 | 29 | 29 | 30 | 30 | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Table 8.3.2 The estimated effect of the reform on average English grades in upper secondary school when controlling for school satisfaction*

| **School satisfaction** English | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Treated_post | -0.152* | -0.169* | -0.168* | -0.189** | -0.185** | -0.177** | -0.156* | -0.176** |
| | (0.0812) | (0.0880) | (0.0861) | (0.0942) | (0.0886) | (0.0885) | (0.0873) | (0.0870) |
| Treated_Post_A | 0.0479 | 0.0729 | 0.0999 | 0.119 | 0.0956 | 0.0767 | 0.0512 | 0.0890 |
| | (0.124) | (0.130) | (0.128) | (0.133) | (0.130) | (0.130) | (0.127) | (0.129) |
| Good work peace | | 0.127* | | | | | | |
| | | (0.0670) | | | | | | |
| High class motivation | | | 0.313*** | | | | | |
| | | | (0.0809) | | | | | |
| Enjoying school | | | | 0.291** | | | | |
| | | | | (0.121) | | | | |
| Interested in learning | | | | | 0.292** | | | |
| | | | | | (0.135) | | | |
| Enjoying schoolwork | | | | | | 0.211 | | |
| | | | | | | (0.133) | | |
| Looking forward to school | | | | | | | 0.248** | |
| | | | | | | | (0.104) | |
| Enough challenges | | | | | | | | 0.452*** |
| | | | | | | | | (0.134) |
| Constant | 4.368*** | 3.879*** | 3.134*** | 3.117*** | 3.179*** | 3.634*** | 3.457*** | 2.404*** |
| | (0.0367) | (0.260) | (0.320) | (0.519) | (0.549) | (0.461) | (0.383) | (0.584) |
| Observations | 209 | 204 | 202 | 202 | 203 | 204 | 205 | 202 |
| R-squared | 0.218 | 0.233 | 0.283 | 0.250 | 0.239 | 0.228 | 0.243 | 0.267 |
| Number of S_id | 30 | 30 | 30 | 30 | 30 | 30 | 30 | 30 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

### *8.4 Parallel Trends*

*Figure 8.4.1 DiD observed means for the remaining upper secondary school subjects*

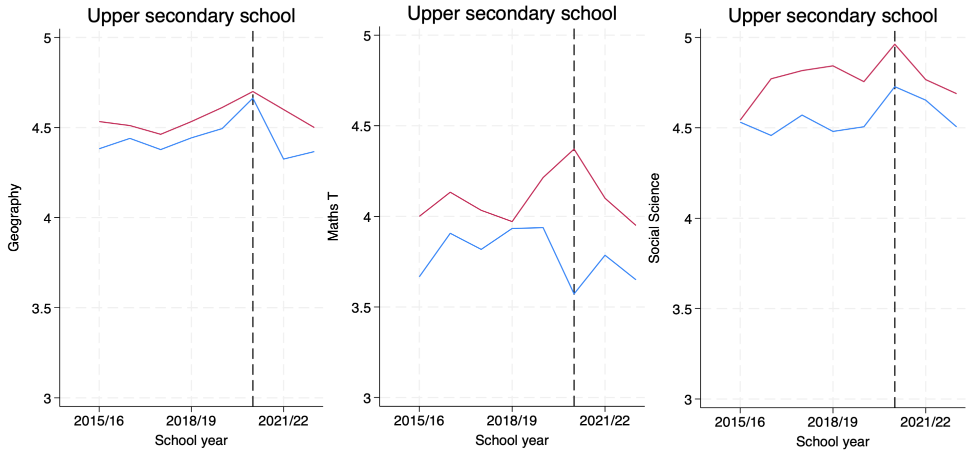


*Figure 8.4.2 DiD observed means for the remaining middle school subjects*

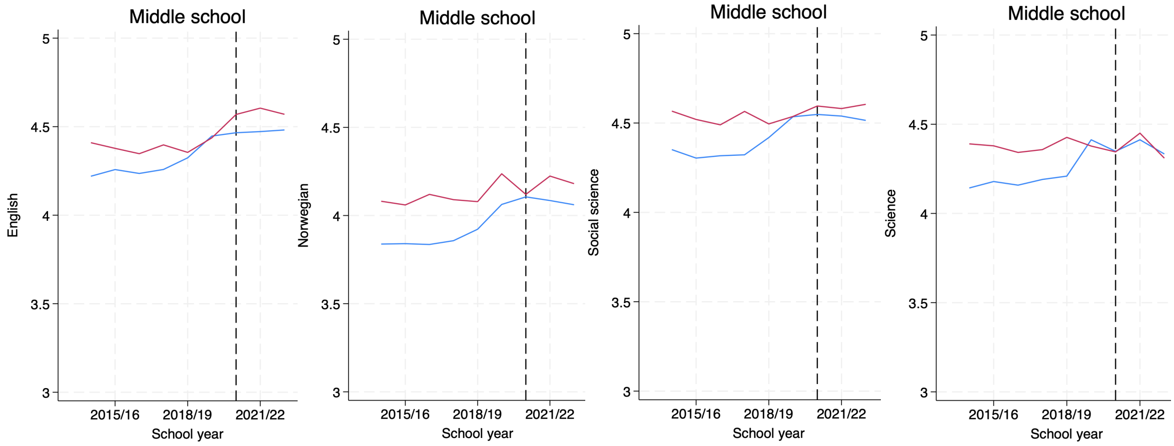
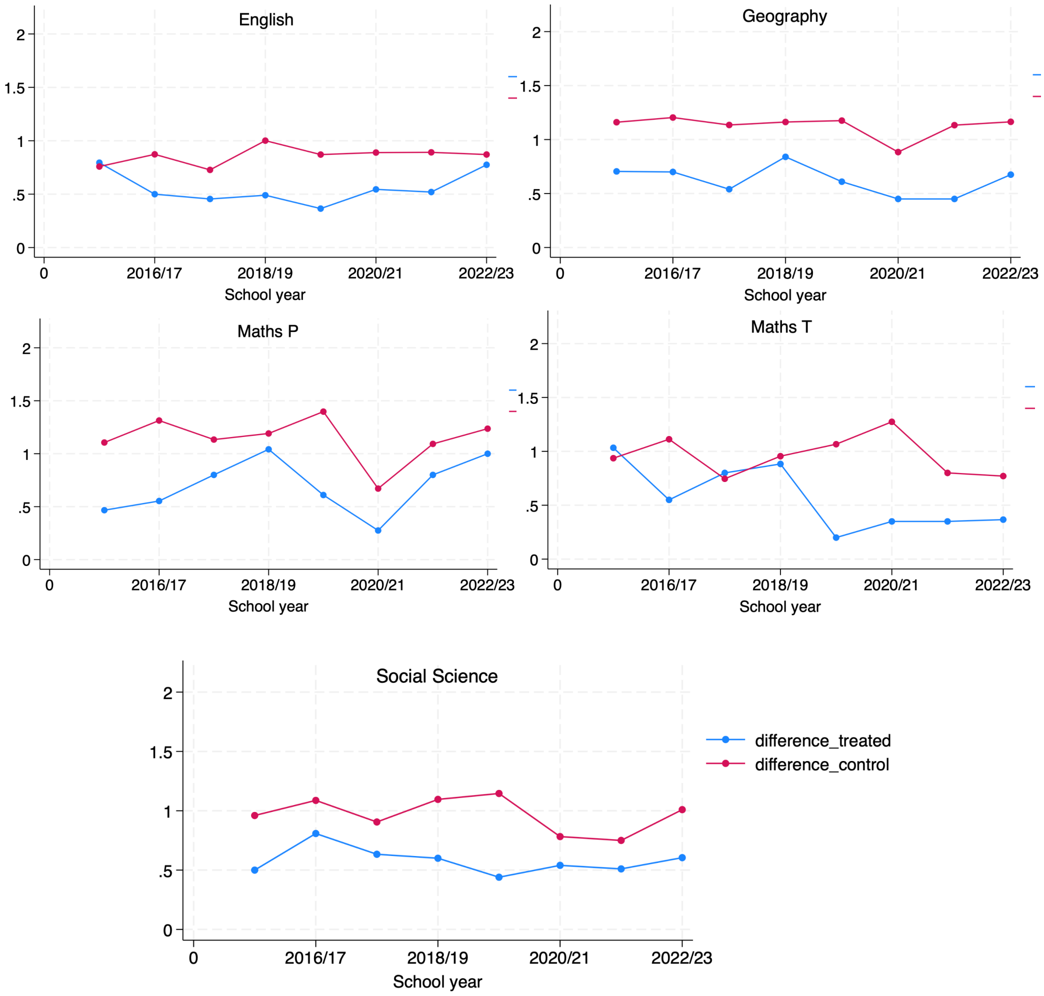



*Figure 8.4.3 DiDiD observed means for the remaining upper secondary school subjects (Treatment year from 2021/22)*



## *8.5 Event Study*

*Table 8.5.1 Event study in the rest of the upper secondary school subjects*

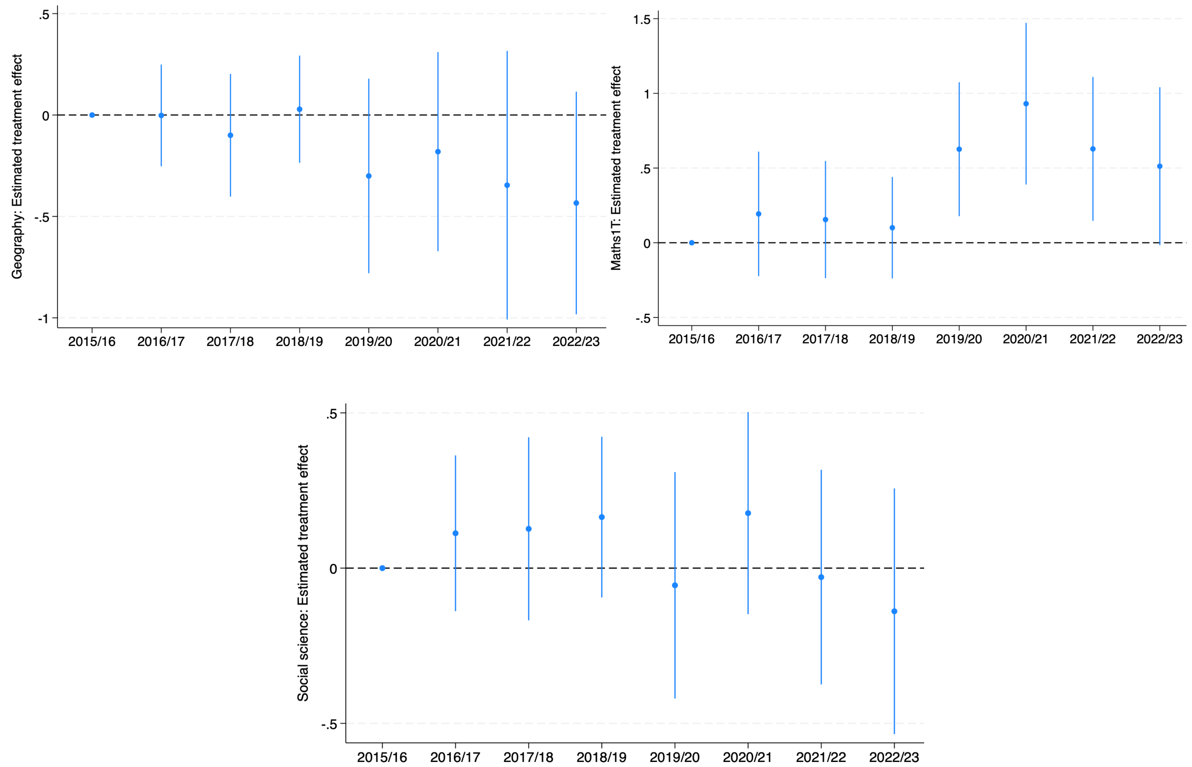
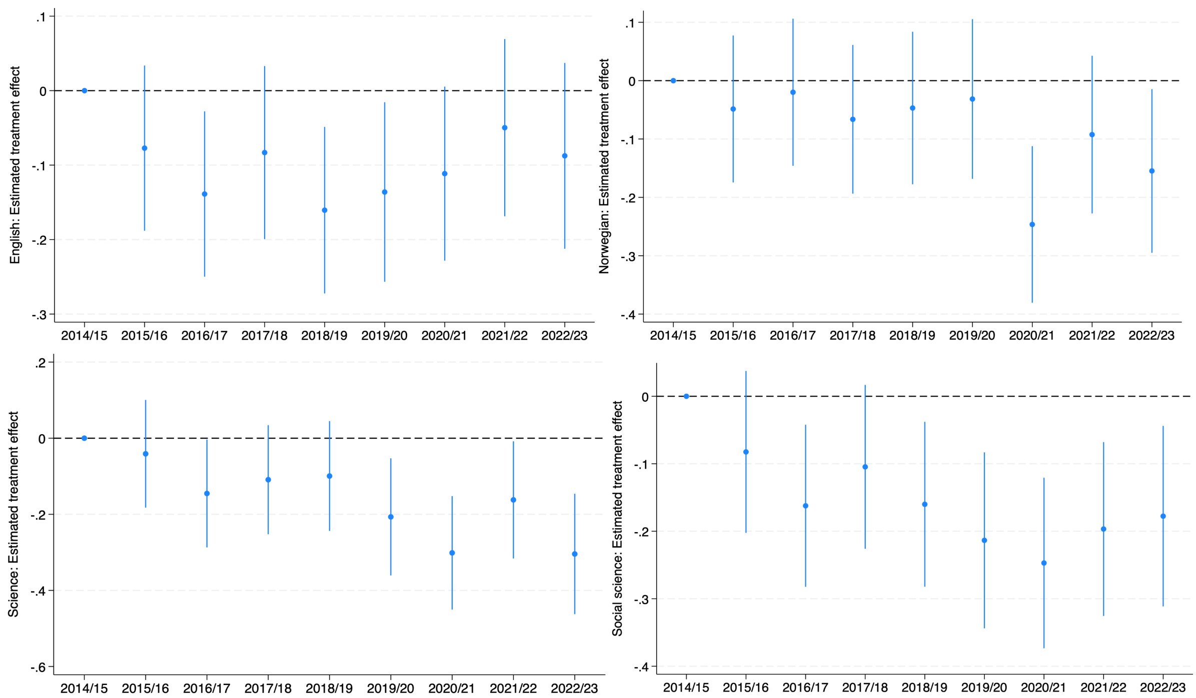



*Table 8.5.2 Event study in the rest of the middle school subjects*



### *8.6 Alternative Control- and Treatment Groups*

*Table 8.6.1 DiD observed means for Stavanger (blue) and Asker+Bærum(red)*

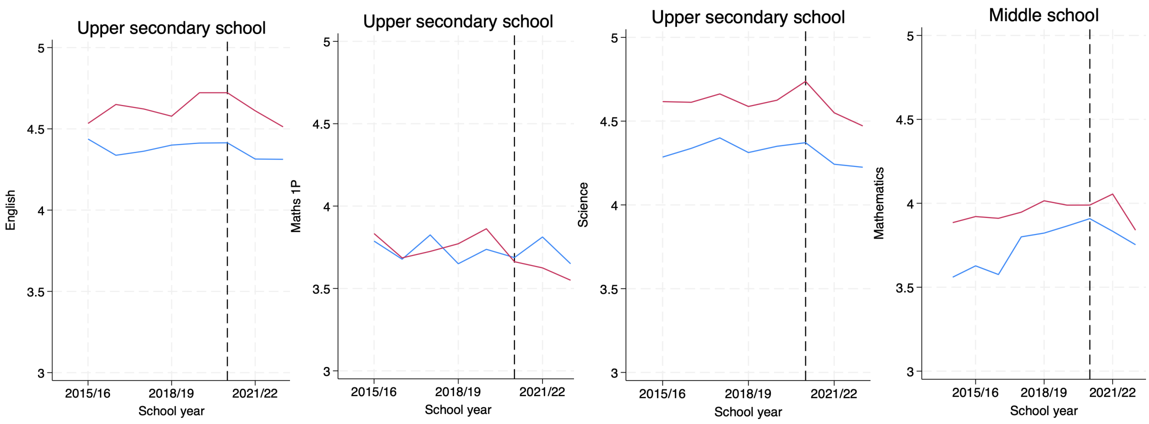



*Table 8.6.2 DiDiD observed means for Stavanger (red) and Asker+Bærum (blue)*

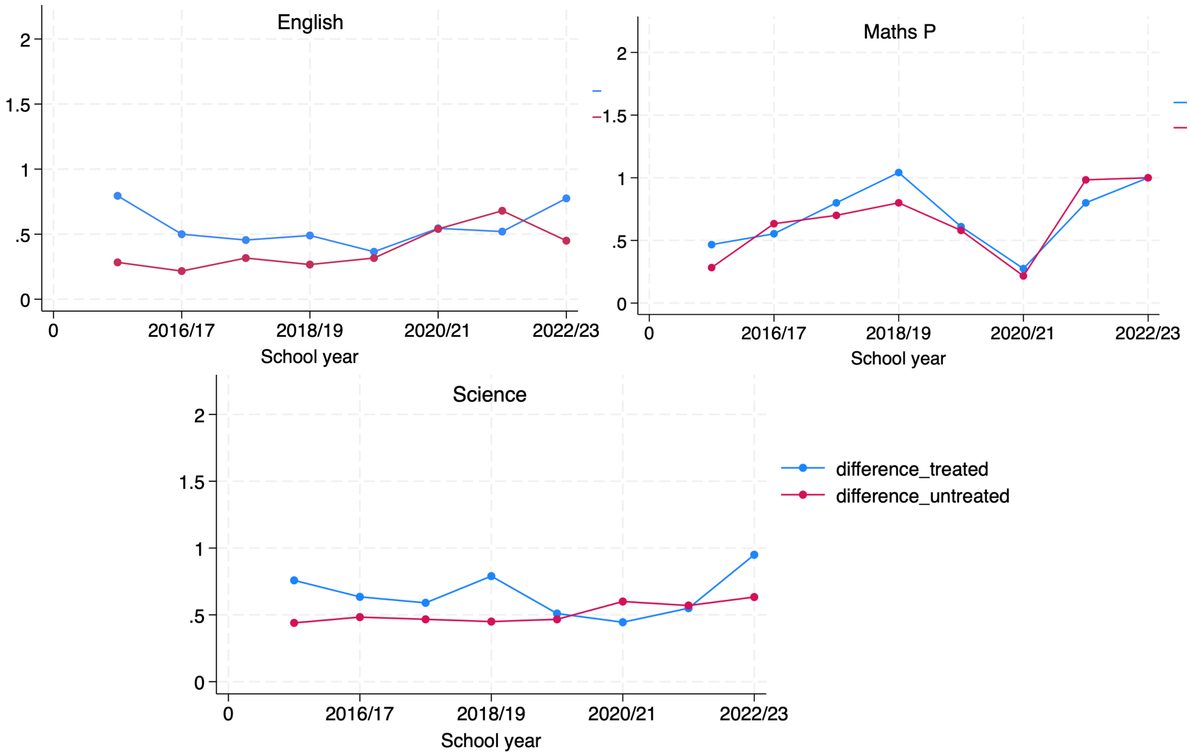


*Table 8.6.3 The estimated effect of the reform on average English grades when controlling for other demographic variables*

| **Demographic variables** English | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Treated_post | -0.143* | -0.146* | -0.196** | -0.0998 | -0.0701 | -0.174** | -0.122 | -0.151 |
| | (0.0837) | (0.0872) | (0.0869) | (0.0863) | (0.110) | (0.0825) | (0.0912) | (0.243) |
| Treated_Post_A | 0.0482 | 0.0482 | 0.0498 | 0.0480 | 0.0514 | 0.0520 | 0.0511 | 0.0571 |
| | (0.124) | (0.124) | (0.123) | (0.123) | (0.124) | (0.123) | (0.124) | (0.124) |
| Housepriceaggregated1000kr | 1.57e-09 | | | | | | | -7.55e-08 |
| | (3.46e-09) | | | | | | | (1.42e-07) |
| Housesalesaggregated | | -6.84e-06 | | | | | | 0.000532 |
| | | (3.49e-05) | | | | | | (0.000986) |
| Incomehouseholdmedian | | | 2.22e-05 | | | | | 4.38e-06 |
| | | | (1.62e-05) | | | | | (1.25e-05) |
| Higheredupercent | | | | -0.175* | | | | -0.156 |
| | | | | (0.102) | | | | (0.117) |
| Householdspercent | | | | | 0.264 | | | 2.873 |
| | | | | | (0.239) | | | (5.149) |
| Movementsfrom617yo | | | | | | -0.000375 | | -0.000524 |
| | | | | | | (0.000274) | | (0.000454) |
| Movementswithin | | | | | | | -8.73e-06 | -2.48e-06 |
| | | | | | | | (1.21e-05) | (2.75e-05) |
| Constant | 4.298*** | 4.427*** | -6.665 | 6.941*** | 2.707* | 4.847*** | 4.923*** | -13.93 |
| | (0.160) | (0.302) | (8.044) | (1.493) | (1.503) | (0.352) | (0.768) | (41.34) |
| Observations | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 |
| R-squared | 0.219 | 0.219 | 0.227 | 0.232 | 0.224 | 0.227 | 0.221 | 0.246 |
| Number of S_id | 30 | 30 | 30 | 30 | 30 | 30 | 30 | 30 |

*Asker+Bærum is the treatment group while Oslo is the control group*

*Table 8.6.4 Descriptive statistics over the "number of schools in 5km radius" (or the number of competitive schools) each school in the admission area have*

| Schools in 5km radius | Obs | Mean | Std. Dev | Min | Max |
|---|---|---|---|---|---|
| Asker+Bærum | 9 | 4.111 | 1.9 | 1 | 6 |
| Oslo | 21 | 9.714 | 4.595 | 0 | 15 |
| Follo | 6 | 0 | 0 | 0 | 0 |
| Romerike | 12 | 1.167 | 1.586 | 0 | 4 |
| Stavanger | 8 | 1.5 | 1.603 | 0 | 3 |

*Table 8.6.5 Parallel trends for Follo (red) and Oslo (blue)*

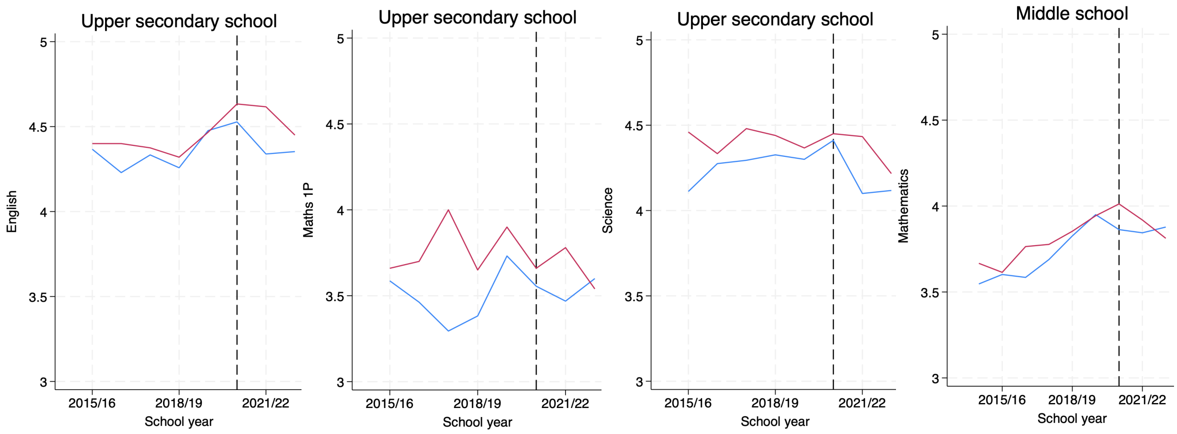
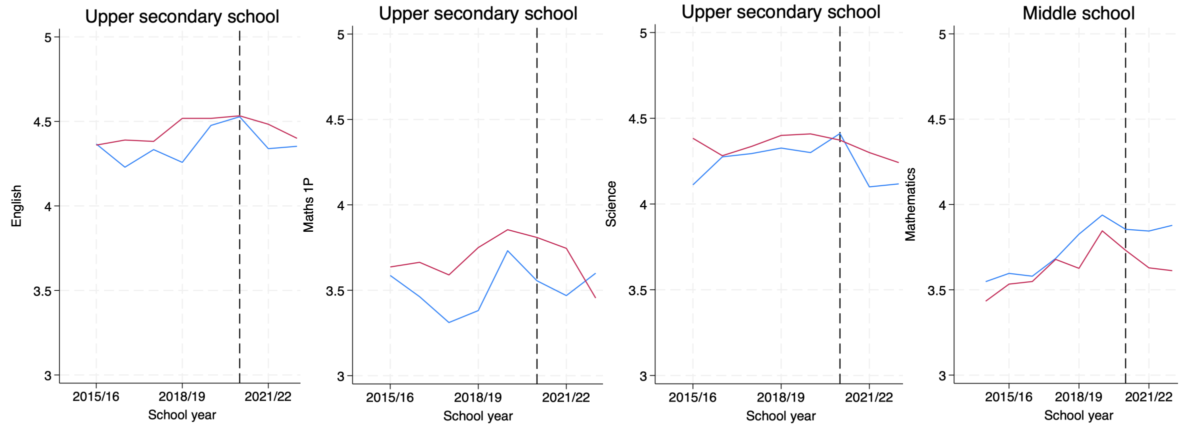


*Table 8.6.6 Parallel trends for Romerike (red) and Oslo (blue)*




## *8.7 Alternative Outcome Variables*

*Table 8.7.1: Estimated effect of the reform on average English grades when controlling for School variables*

| **School variables** English | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Treated_post | -0.152* | -0.190** | -0.174** | -0.159* | -0.143* | -0.166** | -0.189** | -0.168 |
| | (0.0812) | (0.0950) | (0.0812) | (0.0824) | (0.0833) | (0.0807) | (0.0942) | (0.107) |
| Treated_Post_A | 0.0479 | 0.0655 | 0.0699 | 0.0593 | 0.0426 | 0.0925 | 0.119 | 0.111 |
| | (0.124) | (0.144) | (0.123) | (0.126) | (0.124) | (0.124) | (0.133) | (0.151) |
| Absense | | -0.00226 | | | | | | -0.000559 |
| | | (0.00454) | | | | | | (0.00450) |
| Generalstudies | | | 0.000702* | | | | | 0.00199** |
| | | | (0.000362) | | | | | (0.000896) |
| vg1 | | | | 0.000316 | | | | 0.00182 |
| | | | | (0.000661) | | | | (0.00148) |
| vg1Generalstudies | | | | | -0.000491 | | | -0.00432** |
| | | | | | (0.000964) | | | (0.00196) |
| Pupils | | | | | | 0.000524** | | -0.000473 |
| | | | | | | (0.000254) | | (0.000678) |
| S3 | | | | | | | 0.291** | 0.308** |
| | | | | | | | (0.121) | (0.154) |
| Constant | 4.368*** | 4.446*** | 4.036*** | 4.286*** | 4.451*** | 4.000*** | 3.117*** | 2.749*** |
| | (0.0367) | (0.0656) | (0.175) | (0.177) | (0.166) | (0.182) | (0.519) | (0.701) |
| Observations | 209 | 167 | 209 | 209 | 209 | 209 | 202 | 162 |
| R-squared | 0.218 | 0.200 | 0.235 | 0.219 | 0.220 | 0.238 | 0.250 | 0.294 |
| Number of S_id | 30 | 29 | 30 | 30 | 30 | 30 | 30 | 29 |

*Absence, General studies, vg1, vg1Generalstudies and pupils are measured in number of students. S3 is the question "Do you enjoy school" from the Pupil survey and is measured on a scale from 1-5*

### *8.8 Alternative Explanations*

*Table 8.8.1: Corona infections as a control, infections in the year 2022/23 is equal to zero*

| *Estimated effect with no corona infections in 2022/23* | | | | |
|---|---|---|---|---|
| | *Upper secondary school* | | | *Middle school* |
| Variables | English | Maths1P | Science | Mathematics |
| Treated_post | -0.467 | -0.555 | -0.619** | -0.341*** |
| | (0.422) | (0.510) | (0.307) | (0.0606) |
| Treated_Post_A | 0.105 | 0.416* | 0.287* | |
| | (0.136) | (0.234) | (0.162) | |
| Corona infections | -0.164 | -0.102 | 0.0441 | -0.121*** |
| (percent of population) | (0.205) | (0.105) | (0.0951) | (0.0395) |
| Constant | -37.07 | -1.903 | -11.65 | -3.253 |
| | (67.12) | (32.11) | (17.98) | (4.727) |
| Observations | 202 | 191 | 200 | 648 |
| R-squared | 0.274 | 0.175 | 0.130 | 0.311 |
| Number of S_id | 30 | 30 | 30 | 76 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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