Does Performance-Based School Choice Affect Student Achievement?

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This paper exploits an upper secondary school choice reform for one county in Norway to investigate whether students respond to enhanced importance of lower secondary achievement by exerting extra effort. The difference in difference approach provides evidence which supports the hypothesis of a rise in student effort as a response to performance based school choice. Evaluated for all students, lower secondary exam score rise on average of about five percent of a standard deviation when school choice is implemented, while targeted on the subgroup of students that face the largest change in choice set, the reform effect is almost doubled. As this paper focus on individual *student incentives* induced by school choice, it differs from the standard school choice literature. Hence, this paper contributes with new perspectives on school choice.

1. Introduction

Access to education has expanded in the majority of developed countries, and among OECD countries most people have now received education beyond basic, compulsory schooling (OECD, 2011). This situation is also the case for Norway, where approximately 95% of each lower secondary graduate cohort enrolls directly in upper secondary school. One factor explaining the expansion of the Norwegian education system is the legal right to education, which guarantees graduating students admission to an in-county

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upper secondary school. This type of mass expansion would normally lower enrollment requirements and may thus weaken student incentives to exert effort during lower secondary school. Therefore, the implementation of policies that potentially induce student effort may be a useful tool for increasing student achievement.

In the present study, I hypothesize that the availability of performance-based upper secondary school choice can create an incentive for students to exert extra effort and hence increase their achievement levels in lower secondary school, as this policy potentially enhances the importance of lower secondary achievement through entry restrictions for the most popular schools. As the Norwegian counties are free to decide how students are allocated between upper secondary schools, there exist differences in enrollment regimes across counties. A residence-based enrollment scheme restricts the students' choice set to the neighborhood school, whereas performance-based school choice allows students to prioritize among all in-county schools. Hence, the latter enrollment policy could influence entry requirements for attractive upper secondary schools, as admission to oversubscribed schools is determined by students' grades in lower secondary school. Performance-based school choice thus enhances the importance of lower secondary achievement and may induce students to exert more effort. A change in enrollment policy for one county (Hordaland) in 2005 enables me to use a difference-in-differences approach to investigate the hypothesis under study.

The idea is motivated by the work performed by Simon and Woo (1995), who exploit variations in enrollment among countries to examine how institutional factors influence human capital accumulation. Restricted admission, defined as competition for a limited number of available places or entry requirements, is one such institutional factor that is analyzed. Their results indicate that restricted admission to higher education can affect student outcomes in different ways, both through a reduction in the number of students attending higher education and through increased human capital accumulation by the end of basic education. In my case, however, every student is guaranteed a study place in upper secondary through the legal right to education, but only a few students enroll in high-quality schools. Thus, the students aiming for high-quality schools will experience the need to meet entry requirements.

According to the mechanisms outlined in the literature on educational standards (cf.: Costrell, 1994; Betts, 1998), the student's decision to exert effort or not in response to rising achievement requirements depends on the amount of effort necessary to meet the increased requirement. In the models by Costrell and Betts, an increase in the standard leads to increased effort/performance for the students in the top group, whereas

low achievers may decide to exert zero effort and hence perform poorly. As performance-based school choice does not increase entry requirements in every upper secondary school, there exists not only one standard but several standards in this case. Although theories on educational standards are not directly transferable to the case discussed here, it is possible to use this framework to portray potential responses to a change in entry requirements, especially for high achievers. Within this framework an increase in achievement requirements for enrollment in the most popular schools is expected to lead to increased effort/performance for the students in the top group. If the same achievement level was required for entrance to every other upper secondary school, this theory predicts reduced achievement for low achievers; however, as there are several standards in use, it is not obvious how this policy would affect low achievers.

This study also relates to the school choice literature that addresses potential achievement effects. Proponents of school choice view school choice policies as a potential instrument to incentivize schools to improve productivity and school quality, and, hence improve student achievement.[1] Given the importance of student effort (cf.: Stinebrickner and Stinebrickner 2008) and student incentives (cf.: Bishop, 1996; 2006; Angrist and Lavy, 2009)[2] in education production, incentivizing students to exert extra effort may therefore be a powerful tool for policy makers. The use of performance-based school choice in Norwegian upper secondary school is a type of school choice that may induce student effort, as lower secondary performance get enhanced importance in a system with school choice. As the present analysis addresses individual *student incentives* induced by school choice, it differs from the standard school choice literature and thus contributes new perspectives on school choice.

The difference-in-differences approach provides evidence that supports the hypothesis of

[1] The economics of school choice emerge from work by Milton Friedman (1955), who argues against a neighborhood rule that requires parents to send their child to the nearest public school. The fundamental argument is that this organization of schooling is economically inefficient, as it prohibits competition among schools. There is a large literature analyzing various aspects of school choice. Hoxby (2003) provides a collection of work done within this field. More recently, Figlio and Hart (2010) examine how student test scores are influenced by school competition. The authors also provide a good overview of research done in this field, in both international and American studies. Potential segregation effects are another main question related to school choice (e.g., Bifulco and Ladd, 2006; Söderström and Uusitalo, 2010). Most relevant for the present study is the analysis of Söderström and Uusitalo (2010) who evaluate potential segregation effects (in Sweeden) from a reform that share similarities with the reform under study herein.

[2] John Bishop provides a thorough illustration of the importance of student incentives, where, among other things, he argues that student effort responds to extrinsic rewards: when there is a payoff attached to effort, an incentive is created for the student to devote more time to studying. This finding is also supported by Angrist and Lavy, who document a positive effect of pecuniary award on student achievement.

an increase in student effort in response to the implementation of performance-based school choice. Evaluated for all students, lower secondary exam scores rise on average by approximately five percent of a standard deviation when school choice is implemented; focusing on the subgroup of students who face the largest change in choice set, the effect of this reform is almost doubled. A Herfindahl analysis on how lower secondary school representation within an upper secondary school is altered when school choice is implemented brings us closer to revealing the underlying mechanism. The fact that I find a significant increase in school fragmentation as a response to this reform supports the interpretation of a positive reform effect on student achievement in response to increased competition or changes in enrollment requirements for the most popular schools.

The effect is not negligible and illustrates that student effort may become lower as the educational system expands. Performance-based school choice as a policy tool, however, is not problem-free: a major concern is the possible occurrence of sorting effects in upper secondary schools (e.g., Söderström and Uusitalo, 2010). As the school choice analyzed here is performance-based, sorting is expected to arise along the performance dimension, perhaps leading to negative sorting effects through the composition of peers. How performance-based school choice affects sorting is an empirical question and lies beyond the scope of this study.

The rest of the chapter is organized as follows. Section 2 describes upper secondary enrollment in Norway and the changes in enrollment regimes. Section 3 describes the data, whereas section 4 discusses the estimation strategy and reports the main results. Section 5 concludes the chapter.

2. Upper secondary enrollment

Through a national reform of upper secondary education in 1994, everyone in Norway has a legal right to three years of upper secondary schooling. This reform ensures that students graduating from lower secondary school are guaranteed acceptance into one of three education programs at the upper secondary school level, but the legal right must be utilized within five years after upper secondary enrollment and before age 24. Although there are differences in enrollment regimes among counties, the main rule is that the pupils can apply to schools within the county they reside in, where the ranking of students when there are more applicants than openings is based on the students' final grade point averages (GPAs). The GPA consists of both assessment grades and grades from final exams that occur at the end of lower secondary school, with both assessment

grades and central exam results given equal weight.

The legal right to upper secondary education relates to an educational program and not to a specific school; thus, although students may prioritize among schools, they are not entitled admission to a specific school. When subject to a neighborhood enrollment system of upper secondary schooling, the students have limited possibilities for influencing school enrollment apart from choosing an educational program. This type of enrollment regime is quite restrictive, in contrast to a performance-based enrollment system, wherein students can prioritize among all in-county schools. Hence, opening schools to performance-based enrollment increases the individual student's choice set and allows for a district-wide school choice. As admission to over-subscribed schools is determined by lower secondary school grades, this enrollment policy can possibly influence the entry requirements for attractive upper secondary schools. This situation implies that the *possible* choice set of the individual students should increase due to the school choice policy, but the *real* choice set will depend on lower secondary performance and factors that potentially limit the perceived choices, e.g., the distance to and density of schools, due to the potential cost of making a choice that deviates from the neighborhood alternative.

Another factor that affects the real change in choice set by this type of policy reform depends on the school structure and organization within various educational programs. General academic education programs are singled out as the one curriculum that enrolls approximately 40% of each cohort, and due to the relatively low costs of offering this program, it is represented at approximately 74% of the schools in the county; in urban areas this curriculum may even be taught at several schools within short distances of one another.[3] The vocational track consists of nine different educational programs, among which no single educational program dominates over the others. In fact, the majority of the vocational education programs are only offered at a few schools within each county; thus, there may be limited possibilities for prioritizing among schools within the vocational track, even in case of school choice. Hence, it is the pupils applying for the academic track that potentially face a real increase in choice set with the school choice policy.

In this analysis, I exploit the change in enrollment regime from neighborhood enrollment to performance-based enrollment to investigate the hypothesis that competition for enrollment in attractive upper secondary schools affects student effort (and performance)

[3]The value of 74% of the schools represents a mean over the 19 counties surveyed from 2002-2007. The lowest share is 50%, and the highest share is 100%, which was observed for the northernmost county, which is characterized by large distances. Thus, every upper secondary school there provides at least a general academic education program.

in lower secondary schools.

Changes in upper secondary enrollment regimes

In the fall of 2004, Hordaland County announced a change in enrollment policy effective from the 2005-06 school year onward: the existing neighborhood enrollment regime was to be replaced with performance-based school choice.[4] If students graduating from lower secondary schools received this information when it first was announced, they had barely one school year to respond to the change in enrollment policy before graduation in the spring of 2005. As knowledge acquisition is a cumulative process, it is likely that it requires some time for the reform effect to appear in student performance. In addition, the first cohort facing school choice had no observations on how the new school choice regime actually influenced admission requirements. Thus, it is likely that the effect of the reform accelerated throughout the post-policy years, given that the new policy led to restricted entry in the most popular schools.

From 2002-2007, only four of the 19 counties in Norway changed their upper secondary enrollment policies, whereas the remaining 15 counties practiced either school choice or neighborhood rule for the entire period.[5] Although Hordaland implemented school choice, the three other counties imposed restrictions on existing school choice. This was the case for Oslo, Trondheim and its suburban area, and several peripheral municipalities in Finnmark, the northernmost county in Norway.[6] The Hordaland reform implementation has an equal implication for every student in the county; hence, this is a very clean regime change, which provides a rare opportunity to analyze the effect on student performance in lower secondary school. The other enrollment reforms are more fuzzy and are much more difficult to evaluate.[7]

[4] The enrollment regime prior to 2005 was a strict neighborhood regime in which the students only had the opportunity to apply for education programs.

[5] The counties with unchanged enrollment policies are quite evenly divided between the two enrollment regimes; 8 counties practice neighborhood enrollment and 7 counties practice school choice.

[6] Prior to 2004, the students in Finnmark that had to move (Finnmark is a scarcely populated but large county) to attend upper secondary school had the opportunity to freely prioritize schools within the county. The remaining students in Finnmark were subject to neighborhood enrollment. As of 2004, all students in Finnmark were subject to a neighborhood rule.

[7] In Oslo, half of the slots at each school were reserved for students residing in the neighborhood of the school. Similarly, for Trondheim and its suburbs, address of residence got enhanced significance, largely restricting the choice set for students living in the bordering areas. As the reform had different implications depending on residence address, the existing data are limited to enable a comprehensive analysis of the effect of the reform in Trondheim. Evaluating the effect of policy reform in Oslo is not straightforward either, as Oslo also introduced reforms at the lower secondary level during the same period.

3. Data description

The data

The data are provided by Statistics Norway, and cover six adjacent cohorts (2002-2007) in transition between lower and upper secondary school. The data are administrative data measured at the individual student level and include rich information on individual and family characteristics. In addition, the data set provides information on the schools attended in the tenth grade and in upper secondary school, and the regional locations of the schools. Information about upper secondary enrollment regimes in Norway was gathered through interviews with representatives of the counties during the summer of 2003 and then updated in 2011 by exploiting public documentation available on the internet.[8]

In the Norwegian education system, it is not common to repeat a class. Thus, pupils who start school on time and complete compulsory school without breaks graduate at age 16, but there are exceptions though. Students who are born close to the end of the year may start one year later; similarly, some pupils may start one year earlier because they are viewed as being ready for school. This situation is reflected in the fact that 99.7% of the total sample was between 15 and 17 years old when they completed compulsory school, with the majority (97.4%) being 16 years old. To measure the effect on the typical lower secondary graduate, I restricted the sample to include students between 15 and 17 years old at graduation. Adult students do not have the same right to upper secondary education, as the legal right is limited to persons under the age of 24. Thus, a reform in upper secondary enrollment such as I explore here is not expected to affect this group.

Furthermore, the sample only includes students who graduated from lower secondary school *for the first time* during the years 2002-2007. This restriction excludes observations of students who attempted to improve their grades after graduation. The important identification is whether the students graduated from lower secondary school pre- or post-policy change; therefore, I put no restrictions on upper secondary enrollment. Note that only approximately two percent of each cohort does not enroll in upper secondary school directly after graduating from lower secondary school.

[8] I conducted the interviews myself, either by phone or in person.

Student performance measures

Two measures of student performance at the end of tenth grade are available: teacher assessment scores and test scores from the final exam. The tenth grade assessment scores are the main measures of a student's performance during the three years of lower secondary school. Upper secondary track admittance is based on these grades together with test scores from final exams; both assessment grades and central exam results are equally weighted. For the final exam, each student is tested in at least one of the following subjects: mathematics, English, or Norwegian.[9] Which of the subject exams the students take is decided by drawing. Wheres the assessment scores capture the effort students put into schooling for the last three years, exam results measures student performance on a single test. Centralized exams have an obvious advantage over teacher grading in that they do not capture the potential teacher response to the policy change. Subject grade setting by teachers potentially constitutes a problem in the analysis if differential grading standards were developed among municipalities with different upper secondary enrollment regimes. As this policy reform enhances the importance of lower secondary grades, it may influence teachers when assessing marginal students. Thus, for this analysis, I focus on exam results; however, as a robustness check I will use teacher assessment grades.

Treatment and control groups

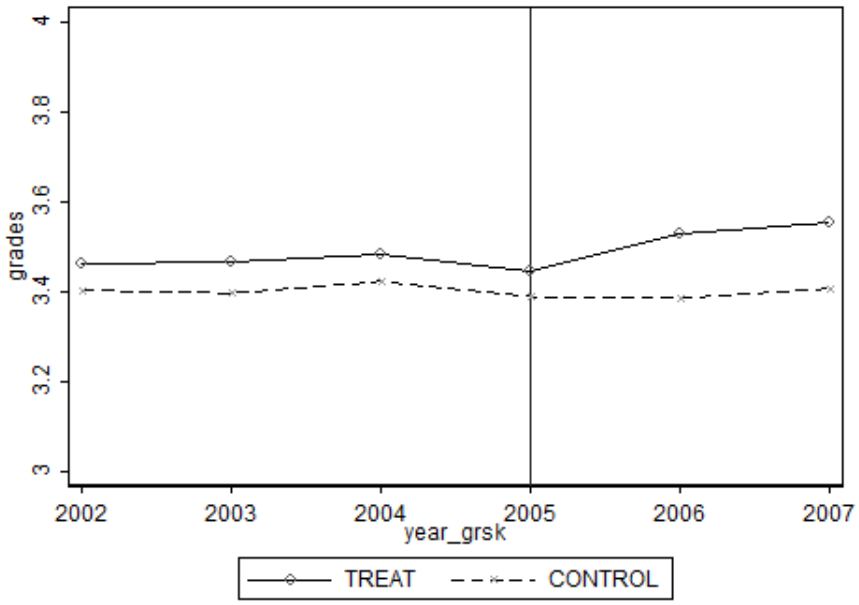
The Hordaland experiment provide grounds for a difference-in-differences approach, where Hordaland is the treatment group. To serve as the control group, I use all other counties with a neighborhood enrollment regime in the period 2002-2007.[10] The total number of students in my sample is 150,107, of which the control group consists of 116,843 students, and 33,264 observations constitute the sample for Hordaland. Appendix table A.1 reports the summary statistics for the total sample and separate measures for the treatment and the control groups.

Hordaland is a large county, both in population size and area, and is characterized by urban areas and rural areas with large distances between settlements. This demographic

[9] Alhough mathematics and English are one-day exams, the Norwegian exam lasts two days and covers the two official written languages ("bokmål" and "nynorsk"). All students learn both languages but differ regarding whether "nynorsk" or "bokmål" is the main language.

[10] This sample includes all municipalities in 9 counties, viz., Østfold, Hedmark, Buskerud, Telemark, Vest-Agder, Sogn og Fjordane, Nord-Trøndelag, Nordland, and Troms, as well as two counties where municipalities with changed enrollment regimes are excluded: Sør-Trøndelag (except for Trondheim and its suburban areas) and Finnmark (except for the peripheral municipalities).

Figure 1: Average exam results in the treatment and control groups



is also represented in the comparison group; however, because Bergen, the main city of Hordaland, is Norway's second largest city, the urban dimension may not be perfectly matched in the control group. The policy rules prior to the reform, however, are similar in both the treatment and control groups.

Figure 1 shows the separate development of exam scores for the treatment and control groups from 2002 to 2007. Although the two groups follow a common trend pre-policy, this is not the case in the post-policy years. The increasing trend in the gap between the treatment and non-treatment groups during the post-policy years corresponds to the fact that knowledge acquisition is a cumulative process. The last year of my sample includes students who had been "in treatment" during all three years of lower secondary school. Thus, this is the first cohort that is fully treated during lower secondary school.

Figure 1 supports the assumption that the treatment and control groups followed similar trends in the outcome variable in the pre-treatment period. In addition to this parallel trend assumption, the identification of the true policy effect using the difference-in-differences approach relies on exogeneity and the sharpness of the event. The fact that the authority that decides on the upper secondary policy (the county) differs from the authority that is responsible for lower secondary education (the municipality) speaks in favor of the fact that the event is exogenous to the development of student performance

in lower secondary school.

Does a change in enrollment regime affect enrollment patterns?

The hypothesis to be tested is that students change their behavior in lower secondary school when school choice is introduced in upper secondary school. A necessary condition for a change in the enrollment system to affect achievement in lower secondary is that the new system induces a change in the allocation of students between schools in the upper secondary system. Subject to a neighborhood enrollment system, each upper secondary school recruits students from a number of lower secondary schools, but when this system is replaced by school choice, the number of potentially recruiting schools increases. Therefore, I expect the composition of lower secondary schools to be more fragmented within each upper secondary in the case of school choice. To examine this, I generate a traditional Herfindahl index which is a widely used concentration index.[11] The index is calculated as

$$HERF_{jct} = \sum_{s=1}^{S} (SH_{sjct})^2 \tag{1}$$

where SH_{sjct} is the share of representatives from lower secondary school s in upper secondary school j in county c at time t. The index takes the maximum value of 1 when a single lower secondary school holds all students in one upper secondary school, whereas the minimum value of 1/S is attained when the students are equally divided among the S lower secondary schools. The index can be interpreted as the probability that two randomly selected students within one upper secondary represent the same lower secondary school. Alternatively, we can say that this index captures the number of lower secondary schools represented in an upper secondary school and the distribution of students among them. The value of the index is reduced (fragmentation increases) when the number of lower secondary schools increases and when the students are more equally divided among a given number of lower secondary schools.

To evaluate how the introduction of school choice affects the allocation of students among upper secondary schools, the Herfindahl index enters a difference-in-differences (DD) regression as the dependent variable. The following relationship is estimated:

[11] The Herfindahl index, also known as the Herfindahl-Hirschman index, was originally developed to measure to degree of concentration in an industry; see Hirschman (1945, 1964) and Herfindahl (1950).

$$HERF_{jct} = \alpha + \beta dSC_c + \delta_1 d2 + \delta_2 d2 * dSC_c + \tau_t + \rho_j + \gamma_c + \epsilon_{jct} \quad (2)$$

where $HERF_{jct}$ measures the concentration/fragmentation of lower secondary schools in upper secondary school j in county c at time t. dSC is a dummy variable that captures possible differences between the treatment group (Hordaland) and the control group (all counties with neighborhood enrollment and no changes in the enrollment system) prior to the policy change. The time period dummy, *d2*, captures aggregate factors that would cause changes in y even in the absence of a policy change. The coefficient of interest, δ_2, multiplies the interaction term, *d2*dSC,* which is the same as a dummy equal to one for those observations in the treatment group in the second period. If fragmentation increases in response to school choice, I expect the interaction term to have a negative coefficient. ρ_j captures upper secondary school fixed effects, γ_c is a county fixed effect, and τ_t is a year fixed effect. ϵ_{jct} is a random error term.

As I expect the competition induced by school choice to vary among upper secondary schools, I split the sample by upper secondary tracks and perform a separate regression for private schools. Private schools were not bounded by the neighborhood rule prior to the reform; therefore, pupils enrolled at these schools are not expected to have been affected by the change in enrollment regime.[12] For the vocational track, the diversity in the educational programs makes these schools less bounded by a neighborhood rule. Several educational programs are offered at a small number of upper secondary schools; hence, these schools recruit from a larger area of the county regardless of the neighborhood system for upper secondary schooling. The (public) upper secondary schools offering academic track are thus expected to experience the largest change in enrollment patterns.

Table 1 reports the results of the policy implementation on student allocation in upper secondary schools. The first three columns include public upper secondary schools with an academic track, where column (1) reports the average effect of the three post-policy years, and columns (2) and (3) report year-specific effects. The difference between columns (2) and (3) is that the latter imposes a fake treatment in the pre-policy period. Furthermore, enrollment changes in vocational tracks in public upper secondary schools are examined in columns (4) and (5), and estimates for private schools are reported in columns (6) and (7). Columns (4) and (6) report year-specific treatment effects, whereas columns (5) and (7) report placebo effects.

[12] It is obvious that a change in a county's enrollment regime may also affect enrollment in private schools, but the counts of lower secondary schools represented is probably not the best parameter for measuring how the reform influences private schools.

Table 1: The effect of upper secondary school choice on student allocation among upper secondary schools. The dependent variable is a Herfindahl-index of lower secondary school concentration.

	Academic track			Vocational track		Private schools	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post policy period	-0.0257*	-0.0242*	-0.0303*	-0.0187**	-0.0164*	-0.0322**	-0.0356**
	(0.0135)	(0.0145)	(0.0161)	(0.00924)	(0.00976)	(0.0139)	(0.0150)
School choice*post policy period	-0.0920***						
	(0.0196)						
School choice*y2003			0.0379		-0.0108		0.0207
			(0.0306)		(0.0156)		(0.0200)
School choice*y2004			0.0410		-0.0240		0.0320
			(0.0339)		(0.0164)		(0.0252)
School choice*y2005		-0.0988***	-0.0721**	-0.0107	-0.0232	-0.0112	0.00768
		(0.0261)	(0.0324)	(0.0177)	(0.0211)	(0.0203)	(0.0229)
School choice*y2006		-0.0759***	-0.0491*	0.0107	-0.00175	-0.0162	0.00283
		(0.0209)	(0.0294)	(0.0144)	(0.0170)	(0.0182)	(0.0213)
School choice*y2007		-0.101***	-0.0738*	0.00888	-0.00357	-0.0400*	-0.0213
		(0.0310)	(0.0387)	(0.0128)	(0.0170)	(0.0203)	(0.0223)
R-squared	0.116	0.117	0.118	0.066	0.067	0.078	0.079
N	1,469	1,469	1,469	2,036	2,036	820	820
N(low sec schools)	272	272	272	332	332	94	94

Note: All specifications include year fixed effect, county fixed effect, and upper secondary school fixed effect. The table presents weighted regression with Herfindahl index of lower secondary concentration in upper secondary. Number of students in upper secondary is used as weights. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Columns (1) and (2) report a negative coefficient for the interaction term(s) for upper secondary schools within the academic track, which indicates that the introduction of school choice led to increased fragmentation of lower secondary schools at a given upper secondary. This effect is statistically significant at the one-percent level. The fact that the effect varies by year in the post-policy period is not surprising, as the competition for openings may vary across cohorts. The results in column (3), where I impose a fake treatment in the pre-policy period supports a picture of change in student allocation after the implementation of school choice. Compared with the reference year (2002), the "effect" of school choice is not statistically significant in the pre-policy period, whereas the effect is negative and statistically significant in each of the post-policy years. The "effect" also shifts sign from positive to negative when the school choice policy was implemented, indicating that the reform induced a substantial change in enrollment behavior.

Both the estimates for public upper secondary schools that offer vocational tracks (columns (4) and (5)) and estimates for private schools ((columns (6) and (7)) indicates an absence of significant effects on school fragmentation after school choice implementation. For private schools, however, there appears to be a negative and statistically significant effect in 2007, but this estimate is only marginally significant. Given the different structure of the educational programs in the academic and vocational tracks and the fact that private schools were present in each student's possible choice set prior to the reform, this finding indicates that performance-based school choice result in enrollment changes in the case where the reform increases the choice set of schools within one educational program.

These findings support the assumption that introducing school choice affects student allocation among upper secondary schools as long as the reform leads to changes in the choice set of schools. I find indications of increased lower secondary school fragmentation in the academic upper secondary schools after school choice was implemented, whereas I find no clear alteration of the fragmentation pattern in the vocational upper secondary schools. Thus, the findings of this section suggests that performance-based school choice may lead to increased competition, at least for students aiming at enrollment in academic tracks.

4. Estimating the effects of school choice enrollment reform on educational achievement

Estimation strategy

I estimate different versions of the following empirical model of the impact of performance-based school choice on educational achievement:

$$y_{ict} = \alpha + \beta dSC_c + \delta_1 d2 + \delta_2 d2 * dSC_c + X_{it}\alpha + dSUBJ + \tau_t + \gamma_s + \epsilon_{ict} \quad (3)$$

The main differences between Eq. 3 and Eq. 2 are the dependent variable, the level of aggregation and several of the control variables. Here, the dependent variable, y_{ict}, measures the lower secondary school performance of an individual student i living in county c at time t. Educational outcome is measured by achievement on central exams at the end of lower secondary school, and dummies for the test subject on the final exam are included among the controls ($dSUBJ$). X_{it} is a vector of student covariates that includes gender, nuclear family, number of siblings, birth order, age, immigrant status, and mother's and father's education levels, γ_s captures lower secondary school fixed effects, and τ_t is a year fixed effect. ϵ_{ict} is a random error term.

The variables describing the reform effect are similar to those in Eq. 2. I estimate both models with an average reform effect of the three post-policy years and models with separate indicators for each post-policy year, which makes it possible to delineate the effect of the policy change over time.

Main results

Table 2 reports the reform effect for two different achievement measures. The results with the central exam score as the dependent variable are reported in columns 1-3, whereas effects on teacher assessment grades are reported in columns 4-5. All columns report DD estimates regressed with lower secondary school fixed effect.[13] Columns 1, 2 and 4 report the average reform effects for all three treatment years, whereas columns 3 and 5 report the year-specific reform effects.

Column 1 represents the simplest specification, where only year dummies and subject dummies are included among the covariates. This simple specification indicates that the

[13] Using OLS reports a slightly higher reform effect and slightly higher significance level. Here, I only report the fixed effects regressions.

Table 2: Fixed school effects estimates of the reform effect of upper secondary school choice on lower secondary achievement.

	Exam results			Grade score	
	(1)	(2)	(3)	(4)	(5)
School choice*post-policy	0.0453**	0.0464***		0.0543***	
	(0.0184)	(0.0164)		(0.0140)	
School choice*y2005			0.0273		0.0317
			(0.0280)		(0.0196)
School choice*y2006			0.0439**		0.0806***
			(0.0189)		(0.0185)
School choice*y2007			0.0684***		0.0509***
			(0.0265)		(0.0195)
Year dummies	Yes	Yes	Yes	Yes	Yes
Subject dummies	Yes	Yes	Yes	Yes	Yes
Family controls	No	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes	Yes
R-squared	0.008	0.161	0.161	0.263	0.263
N	150,107	150,107	150,107	150,107	150,107
N(low sec schools)	720	720	720	720	720

Note: Each column represents a separate regression. All specifications include year dummies and subject dummies. The table presents lower secondary school fixed effects of a change in upper secondary enrollment policy from neighborhood enrollment to school choice on standardized exam score (columns 1-3) and grade point average (columns 4-5). Standardized exam score is measured as an aggregate of four tests - Norwegian (main and second), math and English. Each student is examined in at least one of the subjects, but this varies. Subject dummies are included to control for subject examined. Standardized grade score is teacher assessment in a minimum of 10 subjects. Individual and family characteristics include: gender, nuclear family, number of siblings, birth order, age, immigrant status, mother's and father's education level (vector of dummies). Appendix table A.2 reports estimates of all covariates. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

estimated effect on lower secondary exam results amounts to approximately 4.5 percent of a standard deviation, with significance at the five percent level. Adding controls for family background and individual characteristics (column 2) has basically no influence on the estimated average reform effect, but the effect is more precisely estimated and significant at the one-percent level.

Column 3 is similar to column 2 except that here a year-specific reform effect replaces the average effect for the three reform years. By including separate indicators for each post-policy year, it is possible to trace the effect of the policy over time. As observed in column 3, the reform effect increases during the three years in treatment. As expected from the graphical representation of the development of exam results over the years 2002 through 2007 (figure 1), the first cohort does not respond to the reform. The

first positive and statistically significant reform effect is measured for the cohort that graduated from lower secondary school in 2006. This cohort has been treated for two years and has observed how the new enrollment regime influences admission requirements. The estimated reform effect for the 2006 cohort is very similar to the average treatment effect and significant at the five-percent level. The largest effect is observed for the cohort that is fully treated: evaluated for 2007, the estimated effect of upper secondary school choice on lower secondary exam results is 6.8 percent of a standard deviation. The fact that the treatment effect increases during the reform years is supported by the cumulative characteristic of knowledge acquisition. It is also plausible that later cohorts have better information about the actual competition for enrollment and the performance levels expected for admission to specific upper secondary schools.

The last two columns (4 and 5) are similar to columns 2 and 3 except for the dependent variable.[14] Estimating the average policy effect on teacher assessment grades reveals the same pattern as for the exam scores, but the magnitude of the reform is larger, with an estimated reform effect of 5.4 percent of a standard deviation. The year-specific effect on grade scores, reported in column 5, corresponds with the year-specific effect on exam scores (column 3) in that both cases report no reform effect for the first cohort facing the changed enrollment regime but significant and positive effects for the last two years. However, although the effect on exam score increased over the years, the post-policy effect on grade scores is the largest for the second cohort. Comparing the estimates in the second reform year, the estimated effect of school choice on grade scores is almost twice the estimated effect on the exam score, that is, 8 percent of a standard deviation for the grade scores versus 4.4 percent of a standard deviation for exam scores.

Grade score reflects the teachers' grading practices, and is thus more sensitive to potential teacher responses to a policy change. As the transition from a neighborhood-based enrollment to school choice increases the importance of grades, some teachers may change their grading practice to "help" students achieve their goals of enrollment in specific schools. Additionally, parents and students may put more pressure on teachers for easier grading. The large improvement in grade score in 2006 for the treated students is consistent with such hypotheses. In other words, the observed effects of the reform on exam results are more likely to reflect real improvements in student achievement than are the observed effects on grade scores. For the subsequent analysis, I therefore use exam score as the dependent variable.

[14] When I use grade score instead of exam score, I do not include subject dummies, as they are only relevant for the exam score.

Table 3: Placebo testing.

	(1)	(2)
School choice*y2003	0.0120	0.0137
	(0.0348)	(0.0330)
School choice*y2004	0.0104	0.0132
	(0.0320)	(0.0314)
School choice*y2005	0.0271	0.0364
	(0.0363)	(0.0338)
School choice*y2006	0.0517*	0.0530**
	(0.0278)	(0.0257)
School choice*y2007	0.0801**	0.0776**
	(0.0338)	(0.0309)
Year dummies	Yes	Yes
Subject dummies	Yes	Yes
Family controls	No	Yes
Individual controls	No	Yes
Observations	150,107	150,107
R-squared	0.008	0.161
N(lower secondary schools)	720	720

Note: Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

A remaining concern is that the estimated reform effect reflects time trends in the output measure. To address this concern, I investigate lower secondary performance trends both before and after the reform by estimating models where the treatment is introduced prior to the reform, known as placebo difference-in-differences. The reform effect is estimated relative to 2002.

As observed in table 3, there is little evidence of a positive trend in student performance in the years preceding the policy reform. Although the estimates are positive, they are far from being significant. The first significant effect of the introduction of school choice appears for the second and third cohorts in treatment, consistent with the results reported in column 3 of table 2. Placebo testing is not sufficient to rule out that the effect I capture is not driven by an increasing long-term trend in lower secondary performance, but the graphical representation in figure 1, indicating a common trend in the pre-policy years, but not in the post-policy years, supports the hypothesis that the reform affected student performance. At a minimum, it appears that introducing school choice accelerated a trend of increasing exam scores. However, the Herfindahl analysis presented

in table 1 supports an interpretation of the policy effect on student achievement as being a consequence of changes in student incentives/behavior when the importance of performance is enhanced, rather than being the result of a long-term trend.

Heterogeneous effects

The potential change in choice opportunities as a result of the policy reform should vary within Hordaland as several municipalities have many nearby schools, whereas others have only a small number or none. If I knew the numbers of upper secondary schools within a given distance for the entire data set, it would be possible to conduct a difference-in-difference-in-difference (DDD) analysis that utilizes the varying degrees of potential choice opportunities within Hordaland compared with similar areas in the control group. A crude measure would be to count the number of upper secondary schools in each municipality, but this count cannot capture the varying distances within and between municipalities, which in turn has a great impact on the real change in choice set. The second-best approach for exploring the degree of heterogeneity in choice possibilities within Hordaland is to perform jack-knifing where I eliminate each municipality in the treatment group one at a time. This procedure constitutes a test of whether some municipalities contributed to the reform effect more than others, and the results indicate that the estimated reform effect depends on Bergen. When Bergen is left out of the regression, the estimated coefficient of the reform effect remains positive but drops in magnitude and is no longer significant. The exclusion of the other municipalities does not alter the estimated coefficient as long as Bergen is included in the sample (see Appendix table A.3). The interpretation of this result is that Bergen is the one municipality that in fact faces a real increase in choice options after the reform. Knowing that Bergen has three times as many schools as the municipality with the second-largest number of upper secondary schools in Hordaland, this result is not surprising. Other municipalities of the county are characterized by large distances between communities/towns and fewer schools within a reachable area, which induces higher costs of choosing a school other than the neighborhood school. Therefore, the jack-knife analysis supports the hypothesis that it is the students facing a real change in their choice options that are actually incentivized by the reform.

Another way to analyze how the actual change in choice opportunities affects student incentives to increase their efforts is to separate the analysis by upper secondary track. As noted above, the effects of increased competition for upper secondary enrollment may differ across students depending on what study track each individual aims for. The

findings in table 1 indicate that students enrolled in the academic track faced increased competition when school choice was implemented. Students enrolled in the vocational track, or in privately owned upper secondary schools appear to be less influenced by the policy reform. This finding is in line with the expectations, as the reform induces the largest change in choice set for the academic track.

To investigate whether the change in enrollment pattern supports the hypothesis that school choice induces competition, I run separate regressions conditional upon different study tracks and upper secondary school ownership.[15] If the change in enrollment pattern reflects increased competition, I expect to find the largest effect on lower secondary achievement for the subgroup of students who face the largest change in enrollment pattern.

By conditioning the regression upon upper secondary enrollment, I consider an event that occurs after the exam is completed. Hence, enrollment reflects the type of educational programs available for the individual student after completing lower secondary school. The application for upper secondary, however, is completed prior to the exam. If the final sorting of students between academic and vocational study tracks differs considerably from the track applied for prior to the exam, my findings will be biased. However, as academic track typically attracts high achievers, it is more likely that the allocation differs among education programs within each study track. This assumption is supported by the fairly similar developments of the treatment and control groups in the allocation between academic and vocational tracks over the investigated time span (see appendix table A.4 for an overview of the shares of students in the academic track).

Table 4 reports the treatment effect when I condition upon enrollment in academic study track. All regressions include full specification of the control variables (cf.: columns 3 and 5 of table 2). Although the first two columns report regression results including all pupils enrolled in academic tracks, columns 3-4 restrict the sample further to include only enrollment in public upper secondary school. Column 1 reports the year-by-year effect of school choice conditional upon enrollment in the academic track. Compared with the similar regression for the full sample (column 2 in table 2), the estimated reform effect is larger (positive) and statistically significant, even for the first cohort subject to school choice. The estimated effect of school choice on exam score is approximately 7 percent of a standard deviation for the first two cohorts, whereas the effect for the fully treated cohort is estimated to be just below 10 percent.

[15] As this condition implicitly restricts the sample to include only the students who enroll in upper secondary school directly after lower secondary graduation, the sum of observations for these two subsamples are smaller than the total sample in the previous regressions.

Table 4: Heterogeneous effects of school choice conditional upon enrollment in the academic study track.

	Pupils enrolled in academic track			
	All schools		Public schools	
	Year specific	Placebo	Year specific	Placebo
School choice*y2003		0.0277		0.0372
		(0.0447)		(0.0462)
School choice*y2004		0.0223		0.0347
		(0.0392)		(0.0404)
School choice*y2005	0.0747**	0.0914**	0.0944**	0.119***
	(0.0339)	(0.0423)	(0.0374)	(0.0451)
School choice*y2006	0.0702***	0.0869**	0.103***	0.127***
	(0.0232)	(0.0342)	(0.0262)	(0.0365)
School choice*y2007	0.0970***	0.114***	0.122***	0.146***
	(0.0306)	(0.0402)	(0.0336)	(0.0442)
Year dummies	Yes	Yes	Yes	Yes
Subject dummies	Yes	Yes	Yes	Yes
Family controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
R-squared	0.086	0.086	0.086	0.086
N	68,063	68,063	63,986	63,986
N(lower secondary schools)	702	702	690	690

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See notes in table 2 for details.

A comparison of columns 1 and 3 reveals that the average treatment effect is even stronger when private schools are excluded from the sample. Evaluated for the 2005 cohort, the estimated effect of introducing school choice is approximately 9 percent of a standard deviation, and the effects for the second and third post-policy years shows that the estimated effect of school choice enrollment policy grows stronger over time. The fact that the placebo testing (columns 2 and 4) reveals no significant positive effects for the years prior to the policy change indicates that long-term trends are not responsible for my results.

Table 5 reports the results from a similar exercise where the sample is restricted to pupils enrolled in the vocational track. The coefficients for the interactions between school choice and post-policy years are generally not significantly different from zero. As I find no effect of the policy change either on the composition of lower secondary students in upper secondary schools (table 1) or on lower secondary achievement (table 5), there

Table 5: Heterogeneous effects of school choice conditional upon enrollment in the vocational study track.

	Pupils enrolled in vocational track			
	All schools		Public schools	
	Year specific	Placebo	Year specific	Placebo
School choice*y2003		0.0337		0.0255
		(0.0350)		(0.0361)
School choice*y2004		0.0428		0.0401
		(0.0357)		(0.0377)
School choice*y2005	-0.00214	0.0243	-0.00446	0.0183
	(0.0334)	(0.0390)	(0.0335)	(0.0404)
School choice*y2006	0.0138	0.0404	0.00162	0.0244
	(0.0275)	(0.0326)	(0.0283)	(0.0361)
School choice*y2007	0.0427	0.0692**	0.0344	0.0572
	(0.0285)	(0.0330)	(0.0295)	(0.0349)
Year dummies	Yes	Yes	Yes	Yes
Subject dummies	Yes	Yes	Yes	Yes
Family controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
R-squared	0.095	0.095	0.090	0.090
Observations	82,044	82,044	76,644	76,644
N(lower secondary schools)	718	718	714	714

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

are strong indications that performance-based school choice induces little competition for this group of students. This finding is in line with the assumption that the reform has a small influence on the available choice set of upper secondary schools within the vocational track; thus, this group of students does not face the same treatment as does the group of students aiming for the academic track.

The findings presented in tables 4 and 5 support the hypothesis that the type of of school choice studied here influences various subgroups of pupils differently. The strong positive and significant effect for pupils enrolled in the academic track and the absence of an effect for pupils enrolled in the vocational track corresponds well with the Herfindahl analysis (see table 1), which indicated that the students attending academic track changed their enrollment pattern when school choice was implemented. Furthermore, the exclusion of private schools from the sample leads to an increased reform effect, which is also supported by the absence of a reform effect on the Herfindahl analysis for private upper secondary schools. These findings substantiate the hypothesis that performance-based

school choice creates incentives for (some) lower secondary students to exert additional efforts to improve their performance.

The case of restricted school choice

The case I have addressed so far is that of increased competition through upper secondary school choice. The opposite case, where the enrollment regime moves from performance-based enrollment to a more restricted scheme also exists in the data. In Trondheim and its suburbs, residence address gained enhanced significance when the 2004 policy reform restricted students to choosing schools within a distance of 6 km from home. As the schools are located more densely closer to the city center, the policy reform for Trondheim and its suburb, Klæbu, had varying implications depending on residence, primarily restricting the choice set for students living in the bordering area. To perform a comprehensive analysis of the reform effect in Trondheim, I require additional information about residence and the supply of upper secondary schools within a distance of 6 km from home, but this was not available in the existing data set. The regression presented here thus only serves as an indication of the opposite incentive effect of reducing school choice, hence reducing the importance or reward attached to performance in lower secondary school. The estimation strategy for this case corresponds to the Hordaland case, except that the implications of the policy are the opposite.

To serve as a control group for Trondheim, Norway's third-largest city, and Klæbu I use Stavanger and its suburb, Sandnes. Stavanger is Norway's fourth-largest city; hence, it well matches the Trondheim case. Prior to the policy change, Trondheim was subject to performance based school choice. Stavanger and Sandnes were subject to performance-based enrollment for the entire period.

Figure 2 provides a graphical representation of the development of exam scores in the treatment and control groups from 2002-2007. Both treatment and control follow a common trend over the entire period, but something appears to occur in the post-policy years, and the curves even cross.

If we assume that upper secondary school choice induces competition, which in turn affects lower secondary performance (effort) positively, the move from school choice to restricted choice then implies reduced competition that may lead to a reduction in student effort and hence a negative effect on lower secondary performance. Table 6 reports the DD results for the move from open enrollment to restricted school choice. In line with the theoretical expectations, the table reports a negative effect of restricted school choice.

Figure 2: Average exam result in Trondheim and suburbs (treatment group) and Stavanger and suburbs (control group).

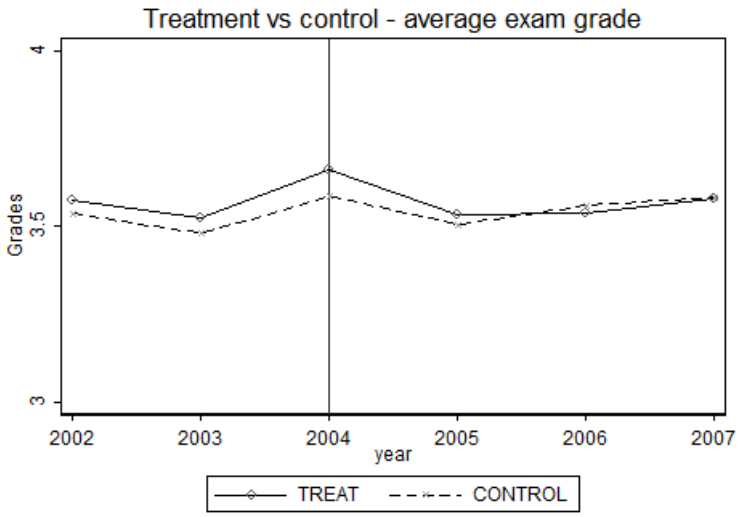


Table 6: Fixed effects estimates of restricted school choice on lower secondary achievement. Policy implementation in Trondheim in 2004. Stavanger and Sandnes together serve as the control group.

	(1)	(2)	(3)
Restricted choice*post-policy	-0.0418	-0.0297	
	(0.0501)	(0.0446)	
Restricted choice*y2004			0.0623
			(0.0497)
Restricted choice*y2005			-0.0890
			(0.0630)
Restricted choice*y2006			-0.0488
			(0.0571)
Restricted choice*y2007			-0.0408
			(0.0565)
Year dummies	Yes	Yes	Yes
Subject dummies	Yes	Yes	Yes
Family controls	No	Yes	Yes
Individual controls	No	Yes	Yes
R-squared	0.010	0.190	0.191
N	22,868	22,868	22,868
N(lower secondary schools)	65	65	65

This negative effect is not statistically significant; however, considering that this policy reform has different implications depending on residence address, where some students in fact still face a large choice set, the average negative effect, although insignificant, may indicate that the students with the largest reduction in choice are negatively influenced.

As evident in column 3, the year-specific reform effect produces no significant effects either, but the first post-policy year shows a positive effect. This cohort has anticipated school choice for at least two of the three years of lower secondary; hence, they had incentives to exert more effort in lower secondary school, as reflected in the positive, although insignificant, estimate.

5. Concluding remarks

The analysis presented herein provides robust and statistically significant estimates of the effect of implementing performance-based school-choice reform. The overall average effect of upper secondary school choice on lower secondary achievement is highly significant and estimated at approximately 5 percent of one standard deviation. By generating more homogeneous groups of pupils compared with the competition, the reform effect is almost doubled, which supports the hypothesis that the reform actually creates an incentive, at least for some of the students. The negative, although statistically insignificant, estimates of the effect of reducing school choice in Trondheim contribute to our understanding that performance-based school choice may create an incentive for student effort.

Furthermore, through the Herfindahl analysis, I come closer to revealing the underlying mechanism for the effect of this policy on student achievement. The significant increase in upper secondary school fragmentation in response to the reform indicates that (some) students change their behavior and enroll in a school other than the neighborhood school. This change in turn substantiates the assertion that the increased achievement reflects a student response to increased competition (or changes in the enrollment requirements of the most popular schools), i.e. additional effort by the students.

These findings demonstrate that pupils respond to the enhanced importance of grades by exerting extra effort. The effect is not negligible, and the findings are consistent with the predictions of the Simon and Woo model (1995): student effort may be lower at lower levels of the educational system as the admission requirements decrease when the system expands. The findings reported here thus highlight a paradox: as the education system expands, the demand for adequate student incentives may be more important, but the expansion itself tends to weaken the incentives associated with admission requirements.

Achievement-based admission to the most attractive upper secondary schools may increase the incentives but will probably generate unwanted student sorting.

As the reform effect reported here assumes that the intensity of the reform is equal within each county, it ignores the fact that the perceived choice set may depend on factors such as distance to upper secondary schools and school density. An extension of the analysis to include these elements would allow for a differentiation of the reform effect. Enriching the model in this way is unlikely to alter the general conclusions of the present analysis but it may provide a better tool for identifying the underlying mechanisms.

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A. Appendix tables

Table A.1: Summary statistics.

	Treatment+Control N=150,107		Control N=116,843		Treatment N=33,264	
	mean	sd	mean	sd	mean	sd
Standardized achievement measures						
Central exam	0	(1.000)	-0.0233	(1.002)	0.0818	(0.990)
GPA						
Subject at exam (dummy)						
English	0.382	(0.486)	0.384	(0.486)	0.375	(0.484)
Math	0.393	(0.488)	0.395	(0.489)	0.385	(0.487)
Norwegian (main)	0.227	(0.419)	0.223	(0.416)	0.238	(0.426)
Norwegian (second)	0.210	(0.407)	0.204	(0.403)	0.230	(0.421)
Individual characteristics						
Share boys	0.510	(0.500)	0.511	(0.500)	0.508	(0.500)
Share 15 years old	0.005	(0.071)	0.004	(0.064)	0.008	(0.090)
Share 16 years old	0.985	(0.122)	0.985	(0.123)	0.986	(0.117)
Share 17 years old	0.010	(0.010)	0.011	(0.106)	0.006	(0.075)
Birth order	1.889	(0.999)	1.879	(0.999)	1.923	(0.997)
First generation immigrant (share)	0.031	(0.173)	0.033	(0.179)	0.022	(0.148)
Second generation immigrant (share)	0.013	(0.112)	0.013	(0.114)	0.011	(0.106)
Family characteristics						
Number of siblings	2.021	(1.248)	2.001	(1.267)	2.09	(1.178)
Nuclear family (share)	0.664	(0.472)	0.654	(0.476)	0.698	(0.459)
Dissolved family (share)	0.260	(0.438)	0.267	(0.442)	0.234	(0.423)
One parent household (share)	0.076	(0.266)	0.079	(0.269)	0.068	(0.251)
Father's level of education (shares)						
Low sec (0-10 years)	0.225	(0.418)	0.235	(0.424)	0.189	(0.391)
Up sec basic (11-12 years)	0.143	(0.350)	0.151	(0.358)	0.117	(0.322)
Up sec, final or post-sec, non-tert (13-14 years)	0.367	(0.482)	0.361	(0.480)	0.389	(0.487)
Tertiary ed, undergrad lev (14-17 years)	0.167	(0.373)	0.162	(0.368)	0.187	(0.390)
Tertiary ed, grad lev (18-20+)	0.067	(0.251)	0.060	(0.238)	0.092	(0.290)
Missing info	0.030	(0.170)	0.031	(0.173)	0.026	(0.160)
Mother's level of education (shares)						
Low sec (0-10 years)	0.279	(0.448)	0.281	(0.449)	0.271	(0.445)
Up sec basic (11-12 years)	0.157	(0.364)	0.162	(0.368)	0.140	(0.347)
Up sec, final or post-sec, non-tert (13-14 years)	0.266	(0.442)	0.267	(0.442)	0.262	(0.440)
Tertiary ed, undergrad lev (14-17 years)	0.258	(0.437)	0.253	(0.434)	0.275	(0.447)
Tertiary ed, grad lev (18-20+)	0.0265	(0.161)	0.022	(0.147)	0.042	(0.200)
Missing info	0.0149	(0.121)	0.016	(0.126)	0.011	(0.103)

Table A.2: Full results of table 2.

	Central exam				GPA			
	(1)		(2)		(3)		(4)	
School choice*treatyear	0.046***	(0.016)			0.054***	(0.014)		
School choice*y2005			0.027	(0.028)			0.032	(0.019)
School choice*y2006			0.044**	(0.019)			0.081***	(0.018)
School choice*y2007			0.068***	(0.026)			0.051***	(0.019)
Exam dummy: English	0.383***	(0.046)	0.383***	(0.046)				
Exam dummy: Math	0.369***	(0.046)	0.369***	(0.046)				
Exam dummy: Norwegian (main)	-0.024	(0.048)	-0.025	(0.048)				
Exam dummy: Norwegian (second)	0.403***	(0.028)	0.403***	(0.028)				
y2003	-0.007	(0.014)	-0.007	(0.014)	0.053***	(0.010)	0.053***	(0.011)
y2004	0.005	(0.013)	0.005	(0.013)	0.079***	(0.011)	0.079***	(0.011)
y2005	-0.055***	(0.013)	-0.051***	(0.014)	0.079***	(0.011)	0.084***	(0.011)
y2006	-0.044***	(0.012)	-0.043***	(0.013)	0.077***	(0.012)	0.072***	(0.013)
y2007	-0.020	(0.013)	-0.025*	(0.013)	0.070***	(0.012)	0.071***	(0.013)
Father's education level								
Up sec, basic (11-12 years)	0.184***	(0.008)	0.184***	(0.008)	0.242***	(0.008)	0.242***	(0.008)
Up sec, graduate (13-14 years)	0.220***	(0.007)	0.220***	(0.007)	0.269***	(0.006)	0.269***	(0.006)
Tert ed, undergrad lev (14-17 years)	0.496***	(0.009)	0.496***	(0.009)	0.573***	(0.009)	0.572***	(0.009)
Tert ed, grad lev (18-20+)	0.678***	(0.013)	0.678***	(0.013)	0.730***	(0.012)	0.730***	(0.012)
Missing info	0.182***	(0.019)	0.182***	(0.019)	0.181***	(0.019)	0.180***	(0.019)
Mother's education level								
Up sec, basic (11-12 years)	0.185***	(0.008)	0.184***	(0.008)	0.246***	(0.008)	0.246***	(0.008)
Up sec, graduate (13-14 years)	0.254***	(0.007)	0.254***	(0.007)	0.318***	(0.007)	0.318***	(0.007)
Tert ed, undergrad lev (14-17 years)	0.501***	(0.007)	0.501***	(0.007)	0.597***	(0.008)	0.597***	(0.008)
Tert ed, grad lev (18-20+)	0.712***	(0.016)	0.712***	(0.016)	0.759***	(0.014)	0.759***	(0.014)
Missing info	-0.051*	(0.029)	-0.051*	(0.029)	-0.072***	(0.027)	-0.072***	(0.027)
Nuclear family	0.188***	(0.006)	0.188***	(0.006)	0.318***	(0.006)	0.318***	(0.006)
Birth order	-0.035***	(0.003)	-0.035***	(0.003)	-0.043***	(0.003)	-0.043***	(0.003)
Number of siblings	-0.022***	(0.002)	-0.022***	(0.002)	-0.028***	(0.002)	-0.028***	(0.002)
First generation immigrant	-0.067***	(0.021)	-0.067***	(0.021)	-0.038*	(0.020)	-0.038*	(0.020)
Second generation immigrant	0.019	(0.027)	0.019	(0.027)	0.020	(0.030)	0.020	(0.030)
Boy	-0.287***	(0.007)	-0.287***	(0.007)	-0.501***	(0.007)	-0.501***	(0.007)
15 year old	0.314***	(0.033)	0.314***	(0.033)	0.236***	(0.025)	0.237***	(0.026)
17 year old	-0.406***	(0.026)	-0.406***	(0.026)	-0.365***	(0.024)	-0.365***	(0.024)
Constant	-0.704***	(0.048)	-0.704***	(0.048)	-0.452***	(0.013)	-0.452***	(0.013)
Observations	150,107		150,107		150,107		150,107	
R-squared	0.161		0.161		0.263		0.263	
Number of lower secondary schools	720		720		720		720	

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Jack-knife analysis.

	School choice*post-policy	N	N(low sec)	R-squared
Municipality excluded				
Bergen	0.0202 (0.0220)	134,063	686	0.160
Etne	0.0466*** (0.0164)	149,780	718	0.161
Sveio	0.0473*** (0.0165)	149,719	718	0.161
Bømlo	0.0486*** (0.0162)	149,108	716	0.161
Stord	0.0496*** (0.0167)	148,665	718	0.161
Fitjar	0.0466*** (0.0164)	149,864	719	0.161
Tysnes	0.0468*** (0.0164)	149,874	719	0.161
Kvinnherad	0.0487*** (0.0167)	149,070	715	0.161
Jondal	0.0474*** (0.0164)	150,034	719	0.161
Odda	0.0465*** (0.0166)	149,595	718	0.161
Ullensvang	0.0475*** (0.0164)	149,872	718	0.161
Eidfjord	0.0463*** (0.0164)	150,014	719	0.161
Ulvik	0.0463*** (0.0164)	150,019	719	0.161
Granvin	0.0455*** (0.0164)	150,010	719	0.161
Voss	0.0518*** (0.0162)	149,045	717	0.161
Kvam	0.0451*** (0.0164)	149,425	716	0.161
Fusa	0.0466*** (0.0165)	149,828	719	0.161
Samnanger	0.0464*** (0.0164)	149,916	719	0.161
Os	0.0459*** (0.0168)	148,833	717	0.161
Austevoll	0.0440***(0.0164)	149,709	718	0.161
Sund	0.0484*** (0.0164)	149,633	719	0.161
Fjell	0.0522*** (0.0160)	148,208	715	0.162
Askøy	0.0456*** (0.0166)	148,557	715	0.161
Vaksdal	0.0463*** (0.0164)	149,817	717	0.161
Modalen	0.0471*** (0.0164)	150,068	719	0.161
Osterøy	0.0466*** (0.0166)	149,459	719	0.161
Meland	0.0448*** (0.0165)	149,586	718	0.161
Øygarden	0.0459*** (0.0165)	149,753	719	0.161
Radøy	0.0469*** (0.0165)	149,737	719	0.161
Lindås	0.0442*** (0.0164)	149,104	714	0.161
Austrheim	0.0459*** (0.0164)	149,891	718	0.161
Fedje	0.0461*** (0.0164)	150,054	719	0.161
Masfjorden	0.0467*** (0.0164)	149,957	717	0.161

Note: Each row represents a separate regression of column 2 in table 2 where one municipality in treatment group is excluded one at a time. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Shares of students enrolled in an academic track.

	Students enrolled in academic track			
	Treatment group		Control group	
	N	Share	N	Share
2002	5060	46 %	17882	43 %
2003	5214	43 %	18585	43 %
2004	5539	43 %	19337	43 %
2005	5742	45 %	20258	45 %
2006	5806	50 %	20130	48 %
2007	5903	49 %	20651	47 %