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Causal effects of mathematics*

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Abstract

This paper exploits that students at age 16 in Norway are randomly selected into one compulsory exit exam in either mathematics or languages. A few days before the actual exam day, the students are notified about exam subject. The students have an intensive preparation period, and preparation in mathematics relative to languages is found to decrease dropout from high school, increase enrollment in higher education, and increase enrollment in natural science and technology education programs. The causal effects are strongest for males, and depend on prior skills in mathematics. We explore several mechanisms that might contribute to these findings.

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Keywords: Mathematical training; Intervention; External exit examination; High school graduation; Higher education

JEL: I21; J24

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1. Introduction

It is in general a concern that insufficient student skills in mathematics leads to shortages of key competencies in a time with rapid technological change. A number of empirical studies find that test scores in mathematics are important predictors of future earnings and other individual outcomes, see Murnane et al. (1995) and the literature review in Hanushek (2002). Moreover, recent cross-country studies suggest that aggregate measures of test scores in mathematics and science are important determinants of economic growth (Hanushek and Woessmann, 2008 and 2012). While this evidence suggests an important role for mathematical skills, causal evidence on the impact of mathematics relative to other subjects in school is still scarce.

A small, but growing literature initiated by Altonji (1995), investigates the impact of high school curriculum on further school and labor market outcomes. The typical finding is that more mathematics courses in high school increases educational attainment and earnings. The identification issue in this literature is not trivial, however, because choice of coursework is clearly endogenous. Various instruments for coursework choice are used in the literature, but the identification strategies can be criticized (Altonji et al., 2012).

We explore a random intervention in mathematical training. At the end of compulsory education in Norway, at the age of 16, about 40 percent of the students are randomly selected to sit for a high stake external exit examination in mathematics, while the rest of the students have an examination in Norwegian or English language. The students are informed of their exam subject a few days in advance, such that there is a period of intensive preparation with extensive support from teachers. The preparation period varies from 2 to 5 working days in our empirical period 2002-2004.

The experimental setting that we exploit provides evidence of whether the observed relationships between skills in mathematics and educational outcomes represent causal effects or merely student sorting. The observed relationships clearly indicate that even a short, but intensive, training period in mathematics can have non-negligible treatment effects. We use the population of Norwegian students from administrative registers in the analysis, and find that treatment in mathematics as opposed to languages significantly decreases dropout from high school and increases enrollment in natural science and technology studies in higher education.

The paper is organized as follows: Section 2 reviews related literature and Section 3 presents relevant institutional settings, data and empirical strategy. The empirical results are presented in section 4, which includes several robustness and heterogeneity analysis, while Section 5 contains concluding remarks.

2. Related literature

A number of papers have investigated the impact of test scores in mathematics and science on earnings and other individual outcomes. For example Bishop (1989), Murnane et al. (1995), and Altonji and Pierret (2001) find that measures of achievement is important determinants of individual earnings for given educational attainment and observed individual and family characteristics. In a recent paper, Koedel and Tyhurst (2012) use a resume-based field experiment and find that stronger mathematical skills improve labor market outcomes.

Another strand of the literature has studied the impact of school curriculum on individual earnings, following the seminal paper by Altonji (1995). These studies typically ask to what extent earnings depend on the number and levels of mathematics and science courses taken in high school. For the US, Altonji (1995), Levine and Zimmerman (1995), and Rose and Betts (2004) generally find a positive impact on earnings of taking more mathematics and science courses. It is a question, however, whether these estimates can be interpreted causally or whether they represent selection effects or omitted variables (Altonji et al., 2012). Given the problem to find credible instruments for students' coursework, or other credible identification strategies, it is not surprising that the results vary somewhat across studies.

Three recent studies apply more credible strategies to identify the impact of curriculum on earnings. Goodman (2009) uses US state-level changes in high school mathematics requirements as instruments for students' actual coursework and find that additional mathematics coursework increases earnings, especially for low-skilled students. Joensen and Nielsen (2009) explore a pilot scheme implemented in some Danish high schools, in which students were allowed to select different combinations of high school courses than students enrolled in other schools. Using this variation as instrument for the students' actual choices, they find that taking more advanced mathematics courses have a significant and sizable positive impact on earnings. Their estimates imply that taking one extra course in mathematics increases earnings by 20-25 percent. The main mechanism seems to be increased likelihood of taking higher education.

Cortes et al. (2012) study an algebra policy implemented in Chicago in 2003. Students with achievement below the national median result in an eighth grade exam in mathematics are assigned to algebra courses with double instructional time in ninth grade. Using a regression discontinuity design, they find sizable effects of the double-dosing in algebra on high school graduation rates, college entrance exam scores, and college enrollment rates. The intervention seems to have been most successful for students with relatively low reading skills.

These three studies have different identification approaches, but all find sizable effects of increased coursework in mathematics during the school year. However, in all cases, the increased coursework in mathematics is at the expense of coursework in other subjects. Thus, the estimated effects of mathematical coursework are to some extent relative to other coursework. Our study shares this feature, although we do not study coursework per see. The intervention we study differs from the above studies in at least three important ways. First, we study the effect of intensive preparation in a few days without any other school work for the students. Second, the preparation is directly related to a high-stake test very close in time, and third, we are able to estimate average treatment effects because the whole cohort is included in the random assignment of examination subject.

Our paper is also related to the growing literature on the impact of instruction time. For example Marcotte and Hemelt (2008) and Hansen (2011) find that reduced instruction time due to more snow-related school day cancelations reduces student performance. In addition, Hansen (2011) finds that variation in the number of instruction days across cohorts implied by state-mandated shifts in test-date is related to student performance. A similar identification strategy is pursued by Carlsson et al. (2012). They exploit the conditionally random variation in the actual date for the test taken by 18 years-old males in Sweden in preparation for military service. They find that 10 days of schooling increases the score on crystallized intelligence by one percent of a standard deviation. Lavy (2010) uses international comparable student tests and exploits variation in instruction time across subjects in a within-student framework. He also finds a positive effect of instruction time on test scores.

3. Institutional setting, data and empirical strategy

3.1. Institutions

The Norwegian school system consists of ten compulsory years, where the first seven years are attained in primary schools and the last three years in lower secondary schools. Students are normally enrolled the year they turn six years old. There is no possibility to fail a class, which implies that everybody finish compulsory education 10 years after enrollment. However, the weakest students do not get a grade in every subject.

At graduation the students receive a diploma containing 13 different grades set by the teachers. These grades are determined before the results on the external exit examinations are set. The grading scale is from one to six, where six is the highest grade. In addition, the diploma includes results from external exit examination.

After the end of compulsory education, students can choose to leave school or to enroll in high school education. In high school students can choose between 15 different study tracks in the empirical period. Three of the study tracks qualify for higher education (academic tracks) and 12 tracks give a certificate for work in a broad amount of occupations (vocational tracks). The academic tracks consist of three years, while the vocational study tracks normally consist of two years in school plus two years as apprentice.

About 95 percent enroll high school the year they finish compulsory education. Students have to rank three different study tracks when applying for enrollment. All students have a legal right to be enrolled in one of these three tracks, but which track and school they actually enroll into depends on achievement in compulsory education measured by their teacher grades and the results on the exit examination. The application deadline is in the winter/early spring, many weeks before the external exit examination. At this stage the students are well informed about their achievements in the different subjects. The diploma with the final grades is forwarded after the end of the school year. There are some possibilities to reverse the priority ranking of study tracks after the diploma is ready, and there are some possibilities to change study track early in the fall after the enrollment in high school. To our knowledge such changes are rare, but application data has not been available for this project.

Public compulsory schools have a common curriculum and the same number of teaching hours in each subject.[1] The 430 municipalities are responsible for compulsory education, while the 19 counties are responsible for high school education. The municipalities use about one-fifth of their budget on education, while the counties spend over 50 percent on education. Enrollment into compulsory schools is based on catchment areas, while the counties have major leeway on enrollment in high schools. They determine the capacity of the individual schools and study tracks according to local needs and student demand. Some counties use catchment areas for the individual study tracks, other counties have free school choice within certain regions, while some do not have any restrictions on school choice.

[1] Few students enroll in private schools. About two and five percent of a cohort enroll in private compulsory schools and high schools, respectively.

3.2. The treatment

All students have to sit for a written external exit examination at the end of compulsory education either in Norwegian language, English language, or mathematics. The exam is a test that lasts five hours. The Norwegian Directorate for Education and Training prepares the exams, decides external examiners (teachers from other schools than the students they evaluate), and give clear instructions about randomization of students. Local authorities, including the county governor who is appointed by the central government, are responsible for the assignment of examination subjects to schools and students. For this purpose they mainly randomize schools, but at some schools different students are examined in different subjects. The randomization process is not clearly spelled out, probably in order to keep the assignment hard to predict. Casual evidence clearly suggests that students have no clue about their examination subject, and there is a lot of anxiety among students before the announcement of the subject assignment.

The examination subject is unknown up to a few days before the examination day. The exam is carried out on the same day in all subjects in the empirical period of this paper, and the students are well informed in advance about the procedure.[2] The length of the period from when the students are informed about their exam subject to the examination day varies across the years. In 2002, 2003 and 2004 the students were informed 2, 7 and 5 days prior to the exam, respectively. In the latter two years the preparation period included a weekend

[2] There are two formal written Norwegian languages. Students that are drawn to have their exam in Norwegian have two exam days, one in each language. The first exam day is the same as for the students drawn for exam in the other subjects.

and the national day, which implies that the preparation period ranges from 2 to 5 working-days.[3] During this period, the students have no other obligations at school than the preparation for the exam. How the preparation is done depends to a large extent of the individual student. Their teachers offer extensive support during school hours, but the students are expected to work on the examination subject also in the evenings.

About 40 percent of the students are randomly selected to sit an examination in mathematics, while the other students sit an examination in either Norwegian language or English language. However, the share of students with the examination in mathematics varies across the counties. For example, this share varied from 0.41 to 0.30 in 2002-2004 in the smallest of the 19 counties in the country. In six percent of the schools no student had the examination in mathematics during the period 2002-2004, while two percent of the schools had students with examination in mathematics each year.

This written external exit examination is the final written test in compulsory education. Most students also have an oral external examination after the written external examination. The oral examination is organized by the individual schools, and initial analysis suggests, that the allocation of students is not random[4].

[3] In 2002 the students were informed about their exam subject on May 22 and sat the exam on May 24. The relevant dates were May 15 and May 22 in 2003 and May 14 and May 19 in 2004.

[4] About two thirds of the students have an oral examination in one of the subjects on the curriculum. The oral examination is organized by the school district in cooperation with the individual schools, without any influence by the Directorate for Education and Training. Inspection of the data indicates that in particular students without any oral examination tend to have low teacher set grades.

3.3. Data

We use register data from Statistics Norway covering all students that finished compulsory education in the years 2002-2004. To make the sample more homogeneous we only include students that turn 16 years of age the year they finish compulsory education in the empirical analysis.[5] In addition, we only include students with teacher grading information on the three examination subjects and information on which compulsory school they graduated from. We also exclude a few students registered with exam grades in both mathematics and one of the languages, and students with exemption from the external examination. Details of the data reduction are presented in Table 1. The analytical sample consists of 89.4 percent of the population, amounting to 155,702 observations.

Table 2 contains descriptive statistics of the main variables used in the analysis. Panel A describes the intervention variable. Close to 40 percent of students were examined in mathematics, which is the treatment group in our analysis. Another 38 percent were examined in English language, while 21 percent were examined in Norwegian language. About two percent of the students did not appear on the examination day, where illness might be one explanation. These shares are the same for each of the cohorts.

Panel B in Table 2 presents descriptive statistics for our main outcome variables. About 46 percent of the cohort enrolls high school in an academic study track the year they finish

[5] Since no students fail any grade in Norwegian compulsory education, one could expect that all students turn 16 years of age the year they finish compulsory education. However, there are some exceptions. If a child is not considered to be mature enough, the parents together with the school and psychologists can postpone enrollment one year. It is also possible to start one year ahead the birth cohort. In addition, some older students return to improve their grades, and immigrants are often over-aged.

compulsory education. The majority of the sample enrolls in a vocational study track (about 51 percent), while some do not enroll this year (about 3 percent). The dropout from high school is, however, relatively large. Only about 71 percent of the sample graduates high school education within 5 years after the end of compulsory education. About 44 percent of the sample enrolls higher education, defined as enrollment within six years after the end of compulsory education.[6] Higher education programs in science and technology are the most demanding in terms of mathematical skills. About 6 percent of the sample enrolls in higher education programs in science or technology within six years after the end of compulsory education.[7] All the outcomes are relatively stable in the empirical period.

In the empirical analysis below we perform separate analyses for females and males. We also distinguish between students with low prior skills in mathematics, defined as a teacher set grade in mathematics of 3 or lower (51.4 percent of the sample), and students with high prior skills in mathematics, defined as a teacher set grade of 4 or higher (48.6 percent of the

[6] The fact that about the same number of students enrolls higher education as the number of students enrolling an academic study track in high schools arise from some students changing study track in high school. 12 percent of the students not enrolling the academic study track right after compulsory education graduate with an academic certificate within five years, while 3 percent of the students enrolling the academic study track graduate with a vocational certificate within five years

[7] We use the Norwegian classification of education. At the level "first stage tertiary education, undergraduate level", the educational programs are divided into 10 different areas. The area that requires the most in terms of mathematical and cognitive skills is denoted "Natural science, vocational and technical education", which we denote higher education programs in science and technology. This area is sub-divided into 9 different fields. 50 % of the students enroll in "Information and computer technology" and "Electrical, electronic, mechanical and machine subjects". For some study programs there are explicit requirements of advanced mathematics in the high school diploma, but that is not the case for most of the study programs. While 40 percent of the students who achieve the academic certificate have second year mathematics in high school, that is the case for 75 percent of the students that enroll a higher education program in science and technology.

sample). This grade is set by the teachers prior to the examination. The descriptive statistics presented in Table 2 shows that the probability of examination subject is equal across these subsamples, but that the mean values of the outcome variables vary as expected.

We include in some of the models a range of socioeconomic characteristics, including immigration, birth quartile, parental education, parental income, parental employment, and parental marital status. Descriptive statistics for the variables are presented in Appendix Table A1. Parental education is classified into four levels (only compulsory education; graduated from high school; bachelor degree; master or PhD degree) and is based on the education category of the parent with the highest education. Parental income is measured by taxable income and is included as quartile indicators. For marital status we use indicators of whether the parents are married when the student finishes compulsory education and whether the parents are divorced at that time. 61.5 percent of the parents were registered as married, 12.5 percent were registered as divorced and 26 percent had never been married (including cohabitants).

3.4. Educational outcomes and subject specific achievement in compulsory education

The empirical literature generally finds a positive relationship between educational attainment and test scores in mathematics. In this section we show that this is the case also in Norway. While the evidence is descriptive, it suggests some hypotheses regarding the effects of the intensive training in mathematics that we investigate below.

We run regression models with grades set by the teachers in the potential exit examination subjects (mathematics, English, and Norwegian language) as explanatory variables. In addition, the model includes the grade point average (GPA) covering the 13 teacher set grades on the diploma from compulsory school. Since GPA is included in the models, the coefficients for the specific subjects should be interpreted as to what extent the subject contributes more or less than the other subjects. The models include the rich set of socioeconomic characteristics described above. To account for possible differences in grading practices between schools, the models also include cohort times compulsory school fixed effects.

Column (1) in Table 3 presents the results for the full sample. The grade in mathematics appears to have a much larger impact than grades in Norwegian and English languages on three of the four outcomes. Given GPA, one standard deviation increase in mathematics grade is associated with 1.6 percentage point higher probability to graduate high school, 5.0 percentage points higher probability to enroll in higher education, and 5.0 percentage points higher probability to enroll in science or technology higher education programs. In all these cases the associations with the grades in Norwegian and English are very small or negative. Regarding the probability to enroll in an academic study track in high school, the association with the grade in mathematics and the grade in English is about the same. Based on these results, the natural hypothesis is that the training intervention in mathematics has a positive effect on all outcomes except the choice of academic study track in high school.

The associations between the outcomes and the grades differ by gender as shown in columns (2) and (3) in Table 3. The association with mathematics is strongest for males both with regard to high school graduation, enrollment in higher education, and the choice of a science or technology study program in higher education. As to the choice of academic study track in high school, the results differ strongly between males and females. For this outcome, languages seem to be more important than mathematics for males, while the opposite is the case for females.

Columns (4) and (5) in Table 3 divide the sample according to prior skills in mathematics. For high school graduation, mathematics seems much more important for students with low skills than for students with high skills, holding GPA constant. The opposite is true for enrolling a higher education program in science or technology, while the difference is smaller for enrollment in higher education in general.

Overall, the results suggest that the association between educational outcomes and compulsory school grades is stronger for mathematics than for languages. Based on these findings and the results in the literature, a natural hypothesis to test is that students randomly selected to the mathematics examination are exposed to a treatment more important for educational attainment than students selected to language examination. In addition, the treatment effects are expected to be most pronounced for males, while heterogeneity related to prior mathematical skills is expected to depend on the specific outcome variable. As to the choice of study track in high school, which is made prior to the external examination in compulsory education, we expect no treatment effect.

3.5. Empirical strategy

We investigate the causal effect of treatment in terms of intensive training in mathematics relative to languages by exploring that each student is randomly selected to external exit examination in only one of the subjects. In the empirical analysis we take treatment intensity into account as the number of treatment days varies from 2 to 5 working-days during the empirical period. Obviously, since the treatment consists of training in a short period of time, effect sizes should be much lower than the impact of one standard deviation in the teacher set grades in mathematics.

We estimate variants of the following model

$$Y_{ic} = \alpha + \beta NTD_i + X_i{}'\delta + \gamma_c + \varepsilon_{ic}, \tag{1}$$

where Y_{ic} represents the outcome for individual i in cohort c, NTD is the number of treatment days, X is a vector of socioeconomic characteristics, γ_c is cohort specific effects, and ε_{ic} is the random error term. β can be interpreted as the average treatment effect. If treatment is random, the estimated treatment effect would be independent of whether the model condition on X and γ_c. To gauge the plausibility of the randomness assumption, we present results both for the model in (1) and for models without any controls included.

In his study of the Tennessee STAR experiment, Krueger (1999) includes school fixed effects to take account of the fact that randomization was done within schools. In our case the central government gives clear instruction about randomization, while actual implementation is done at the local level under inspection of the county governor. Thus, as specification checks we also present model versions with fixed effects for the counties, the municipalities, and the schools, respectively.

To further investigate the issue of randomness, Table 4 presents descriptive evidence on the relationship between treatment and student characteristics. Columns (1) – (3) show that the mean values of the teacher set grades, the socioeconomic characteristics, and the cohort dummy variables are similar in the treatment and the control group. In particular, the mean values of the teacher set grade in mathematics are identical in the two groups. Out of the 26 variables in the table, three of the differences are significant at 10 percent level and none at 5 percent level.

Column (4) in Table 4 replaces the dummy variable for treatment with the number of treatment days and presents partial regressions. Since the number of treatment days only varies across cohorts, they are clearly related to the cohort dummy variables. For the other variables, the relationship is significant at 10 percent level in only two cases.

Column (5) in Table 4 presents results from a multivariate regression with the number of treatment days as the dependent variable. This model includes the socioeconomic characteristics that are used as control variables in the analyses below, in addition to the teacher set grades. The indicator for whether the parents' highest educational level is high school education is the only variable that turns out as significant at 10 percent level. Using an F-test we clearly cannot reject the hypothesis that all explanatory variables have jointly zero effects (p-value of 0.38).

The last column in Table 4 presents a regression only including the variables that are used as control variables in analysis below, i.e., we only condition on socioeconomic characteristics and not on prior grades. As expected, excluding the grades from the equation does not alter the results, since each of them are unrelated to the treatment. As a further check on treatment randomness, Appendix Table A2 presents results from this regression for the subsamples we use in the analysis below. The socioeconomic characteristics are jointly unrelated to the treatment in each year, for both genders, and both for students with low and high prior skills in mathematics.[8]

Overall, the empirical evidence clearly indicates that the treatment is random, as it should be according to the institutions.

4. Empirical results

We first estimate causal effects of the treatment in mathematical training on the educational outcomes presented above. Thereafter we investigate in more detail the possible channels behind these effects. In addition to presenting average effects for the population, we split the sample with regard to gender and prior skills in mathematics. Finally, we provide some analyses on the robustness of our specification of the treatment.

4.1. Educational attainment

Table 5 presents the results. The models in column (1) include only the treatment variable. As discussed above, the treatment effect on the probability to enroll in an academic study track in high school is expected to be zero because this choice is basically made several weeks before treatment takes place. The results in Panel A confirm this hypothesis. The effect of the treatment is very close to zero and highly insignificant.

[8] The treatment is also unrelated to the teacher set grades in mathematics and English. For the full sample, the correlation with the teacher set grade in Norwegian language is significant at 10 percent level as shown in Table 4. This is driven by males and students with low mathematical skills.

Panel B presents results for the probability to graduate high school within 5 years after the end of compulsory education. One day of intensive mathematical training increases the probability to graduate high school with 0.2 percentage points. The effect is significant at 5 percent level, and it is a non-trivial average treatment effect. For the cohort in 2003, which had a treatment of five working days, the estimate indicates an effect of 1 percentage point.

The result in Panel C in Table 5 implies that treatment of one day increases the probability to enroll higher education by 0.15, while Panel D shows that the effect on enrollment in study programs in science and technology in higher education about 0.11 percentage points. This is the educational fields that are the most demanding in terms of mathematical skills. The effect of treatment of one day is 1,7 percent of the mean value, and is significant at one percent level.

The models in column (2) in Table 5 include socioeconomic characteristics and cohort fixed effects, similar to Equation (1) above. This does not change the estimated treatment effects, but increases the precision somewhat. In particular, the effect enrollment in higher education is significant at 5 percent level in this model. Since the county governors control the randomization process, the models in column (3) include county fixed effects for the 19 counties. This does neither affect the estimated effects. The municipalities are responsible for compulsory education and are involved in the assignment of exam subjects, and the models in column (4) include fixed effects for the 440 municipalities. In this case the effect on enrollment in higher education increases to 0.19 percentage points. Notice that in particular in small municipalities, there will typically be some clustering of exam subjects for a given cohort, although that is less likely across the 3 cohorts in our sample. Finally, column (5) includes school fixed effects. Also in this case the estimated treatment effects mainly remain unchanged.

Taking the point estimates in Table 5 at face value, most of the gain in high school graduation maturates in enrollment in higher education, and about 2/3 of the latter turns up in the fields of science and technology. This does not necessarily imply that the marginal students induced to enroll higher education by the treatment enroll in these fields. Rather, it is plausible that some students enrolling in less demanding fields in terms of mathematical skills in the absence of treatment switch to science and technology because of the treatment.

A possible interpretation is that intensive training in a short period of time is more productive for mathematics than for languages as competency in languages requires longer time to mature.. While our natural experiment does not allow for a rigorous test, this interpretation does not easily fit with the descriptive evidence suggesting a stronger positive relationship between longer-term outcomes and grades in mathematics than of grades in languages.

4.2. Heterogeneous treatment effects

The models in Table 6 split the sample according to gender and mathematical skills prior to the exam. We present results for models without any control variables and for models including socioeconomic characteristics and cohort fixed effects. Panels A and B present gender specific models. Again, it turns out that choice of study track in high school education is unrelated to treatment. For the other three outcomes, the treatment only seems to affect males. Interestingly, a larger effect of the treatment on males than on females is in accordance with the association between these outcomes and compulsory school grades in mathematics reported in Table 3. In addition, the relative size of the causal effects on the different outcomes is similar for males as for the population in Table 5.

The last part of Table 6 splits the sample by compulsory school grades in mathematics set by teachers before treatment takes place. Again, both for students with low and high prior skills in mathematics, there is no treatment effect on choice of study track in high school. However, for the other outcomes, some interesting patterns emerge.

First, the treatment effect on high school graduation only appears for students with mathematical skills below the mean. This result makes sense since 70 percent of the students graduate, and the students on the margin of graduation is likely to be in the group with low prior mathematical skills. For this group, the treatment effect is equal to 0.6 percent of the average graduation rate. We find the same pattern for the probability to enroll higher education, where the treatment effect is equal to 1.0 percent of the average value for students with prior mathematical skills below the mean.

Second, for the enrolment in higher education studies in science or technology, the treatment effect is present only for students with prior mathematical skills above the mean. Again, this makes sense when we take into account that these are the students for which such studies are the most likely alternative. 10.8 percent of the students with above average prior skills in mathematics enroll such study programs, while that is the case for only 1.5 percent of the students with prior skills below average (see Table 2). For the students with prior mathematical skills above mean, the estimated treatment effect is 1.6 percent of the mean value.

A student's perceived probability to obtain a low or high grade on the exam grade depends on examination subject, and since the result on the external exit examination matters for high school enrollment this may potentially affect student effort. Suppose students' perception is that a good exam grade is less likely in mathematics than in languages. A student with weak prior performance in mathematics relatively to languages could then have an incentive to exert more effort if he/she is randomly assigned to an exam in mathematics as opposed to languages.[9] To investigate whether such mechanisms can be driving our results, we have restricted the sample to the students with at least the same teacher set grade in mathematics as in both language subjects. However, when using this restricted sample, we get qualitatively the same results as reported in Table 5.[10]

Taken together, the results indicate that the treatment affects students across the whole ability distribution, but at different margins. The effects seem to be mediated through different channels for students located at different points in the ability distribution. In the next sections we investigate treatment effects on outcomes during high school education in order to shed some more light on the potential channels through which the causal effects on high school graduation and higher education enrollment might spell out.

[9] In our sample, the average grade on the exam is 3.30, 3.67, and 3.58 in Mathematics, Norwegian, and English, respectively. The respective averages for the teacher set grades are 3.48, 3.85, and 3.73.

[10] 48 percent of the students have at least the same grade in mathematics as in both Norwegian and English. Restricting the sample to these students, the estimated effects are -0.0001, 0.0016, 0.0009, and 0.0018 for enrolling academic study track, graduating from high school, enrolling higher education, and enrolling higher education program in science or technology, respectively, in the models without controls.

4.3. Student progression in high school

Table 7 investigates treatment effects on the progression in high school education. The mean values of the dependent variables are presented in the table. Column (1) shows that one day of treatment implies a statistically significant 0.15 percentage points increase in the probability to enroll on-time in the third year in high school. In the academic study track this is the final year, while in vocational study tracks it depends on whether an apprentice part is included in the program. The normal progression in the apprentice system is to start as an apprentice in the beginning of the third school year and graduate with a craft certificate two years later. The treatment effect on progression is entirely driven by males and students with prior mathematical skills below the mean, and the coefficient sizes are close to the results for graduation within five years in Table 5.

Columns (2) and (3) in Table 7 investigate whether the treatment effect on progression differs between students in the academic and vocational study tracks. We split the sample according to students' study track in the first year in high school since it is relatively common to change study track during high school education,[11] in particular from a vocational to the academic study track. While division of the sample by outcome variables might in general introduce selection problems, this is likely not a problem here since we found no treatment effect on the initial choice of study track. The results in column (2) and

[11] Three percent of the sample are not registered in high school education the fall in the year they finish compulsory education, but a few of those students nevertheless graduate high school within five years. They are included in the sample of students not enrolling an academic track in Table 5 in order to keep the population of students in the regressions. The qualitative results are not altered by excluding these students.

(3) show that the positive treatment effect on progression is entirely driven by the students enrolling a vocational study track. Further, the effect is strongest and highly significant for males (panel C) and for students with low prior mathematical skills (panel D), similar to the findings above.

Since several vocational study tracks are stipulated to four years study, we also estimate the effect on enrolling the final semester within five years for the relevant students. Notice that in this case we allow the students to be delayed compared to the normal progression. In the sample of students enrolling a vocational study track immediately after compulsory education, 75.4 percent enroll the final semester within five years.[12] The treatment effect on this outcome (column (4)) is very close to the effect on on-time progression. For completeness, we also in column (5) present results for the probability of becoming an apprentice. No significant treatment effects are found for this outcome. Overall, the results in Table 7 clearly indicate that the positive treatment effect on high school graduation is largely a result of decreased dropout of students initially enrolled in vocational study tracks. However, these results cannot explain the positive treatment effect on the probability to enroll higher education studies in science and technology. First, decreased dropout rate is found for students initially enrolled in vocational study tracks and most of them achieve a vocational certificate that does not qualify for higher education. Second, decreased dropout rate occur for students with prior mathematical skills below mean, while the treatment effect on enrollment in higher education in science and technology was found for students with prior skills above mean.

[12] This number can be decomposed into 58.3 percent who graduate, 10.9 percent who fail the final examination, and 6.2 percent who drop out before the final examination.

4.4. Student achievement in high school

In order to identify possible channels for the treatment effect on enrollment in science and technology higher education programs, we exploit information from high school diplomas and estimate treatment effects on high school grades in mathematics for students initially enrolling academic study tracks. The results are presented in Table 8.[13]

In the first year of the academic study track, all students take a two-semester course in mathematics. While the students in our sample had the same curriculum in the first semester (fall), in the second semester (spring) the students could choose between an advanced course and a "practical" course. Column (1) in Table 8 investigates this choice. The results indicate that the treatment slightly increases the probability to choose the advanced course, but the effect is clearly insignificant. The effect is largest, however, for students with prior mathematics skills above mean.

Column (2) in Table 8 uses the value added in grade in mathematics from compulsory education to the end of the first year in high school as the outcome.[14] The value added is calculated using standardized values, and the mean value is negative because there is a

[13] This sub-sample is neither significantly related to the treatment. Relating the sample to the number of treatment days, the p-value is equal to 0.46 and 0.16 with and without control variables, respectively. The average grade in mathematics from compulsory education is equal to 4.22 and 4.23 for the students with and without treatment, respectively.

[14] Both compulsory and high school grades are set by the students' teachers. Notice that the high school grades are from two different courses and that some students are enrolled in two minor academic tracks. The models that condition on socioeconomic characteristics include a dummy variable for the advanced mathematics course and dummy variables for which academic track that are enrolled. Excluding these dummy variables from the models do not affect the qualitative results.

selection of students with relatively high grades into the academic study track. The treatment effect is positive for the full sample of students, and significant at 5 percent level in the model including socioeconomic characteristics. The estimated effect implies that one day of intensive training increases value added in mathematics by 0.8 percent of a standard deviation. This effect is positive for both females and males, but not for student with low prior skills. However, the latter sample is very small due to the selection into study tracks.

Mathematics was not a compulsory subject in the second year in high school in the empirical period. Students studying further mathematics could choose between an advanced course and a course in "business" mathematics. Column (3) in Table 8 shows estimated treatment effects on the probability to study further mathematics (either of the two courses). Column (4) estimates treatment effects on the value added in the mathematics grade from compulsory education to the end of the second year in high school, given that mathematics courses are chosen.[15] For both these outcomes, the estimated treatment effects are small and insignificant. Even though the treatment seems to increase mathematics performance in the first year, that does not seem to carry on to the second year for those students who choose mathematics the second year.

Overall, the treatment seems to have increased graduation from high school by increasing the progression of male students and students with low prior mathematical skills, who choose to enroll in a vocational study track. On the other hand, the treatment might have increased the enrollment in higher education study programs in science and technology by

[15] The sub-sample of students with mathematics in the second year in high school does not seem to be a selected sample related to the treatment. Relating the sample to the number of treatment days, the p- value is equal to 0.89 and 0.72 with and without control variables, respectively.

increasing the mathematical skills of students with high prior mathematical skills. This is a group of students who are not on the margin of dropping out of high school.

4.5. Specification of the treatment variable

The treatment effects above are estimated using information on both treatment status and variation in the number of treatment days - the period between students receive notification on exam subject and the exam day – across cohorts. As a robustness check we investigate in this section whether the treatment effect varies across cohorts in accordance with this model formulation.

Firstly, we estimate models where the number of treatment days is replaced by a treatment indicator. Since the treated have on average 3.3 working days of training, we expect the effect of the dummy variable for treatment to be around 3.3 times as large as the effect of the number of treatment days as reported above. Table 9 presents the results. As before, there are no significant effects on the choice of study track. Column (2) shows the same pattern for high school graduation as in Table 5. In fact, the coefficients that are significantly different from zero are very close to expectation; they are 3.2–3.8 times larger than the effects reported in Table 5. This is a strong indication that the formulation with the number of treatment days represents the data generating process quite well. The findings for enrolling a study program in science or technology in higher education are similar (column (4)), while for enrollment in higher education (column (3)), the treatment effects tend to be less precisely estimated than in the model formulation with the number of treatment days.

In order to discriminate between the model formulations in Tables 5 and 9, we also used an encompassing approach by estimating a general model including both the treatment indicator and the number of treatment days as explanatory variables. However, limited variation in the number of treatment days makes it impossible to statistically discriminate between the formulations. However, the effect of the number of treatment days does not change much when the treatment dummy variable is included in the model, while the effect of the dummy variable gets much smaller than in the models reported in Table 9. For example, in the model for enrollment in higher education, the effect of the treatment dummy variable turns negative in all cases.[16]

Secondly, we estimate models with cohort specific treatment effects. Table 10 shows that for graduating high school within five years, the average effects are about 0.5, 1.0, and 0.5 percentage points in 2002, 2003, and 2004, respectively. These differences in effect sizes are very similar to the differences in the training period. The effect is clearly largest in 2003 when the training period was longest. Notice, however, that because the treatment effect is small, we cannot formally reject that it is equal across the cohorts.[17]

[16] In the model for high school graduation including both the number of training days and the dummy variable for the intervention, the effects are 0.0017 (0.0025) and 0.0013 (0.0092), respectively, in the model specification without controls and using all observations (standard errors in parentheses). Including controls changes the estimates to 0.0017 (0.0026) and 0.0011 (0.0095), respectively. In the model for enrollment in a study program in science or technology, the effects are 0.0007 (0.0045) and 0.0014 (0.0035), respectively, in the model specification without controls, and 0.0002 (0.0011) and 0.0034 (0.0041), respectively, in the models including controls.

[17] For example, the p-value on a test of equal treatment effects on high school graduation across cohorts is equal to 0.59 in the model without controls. Likewise, the p-value on a test on whether the effect in 2003 is larger than the other years is equal to 0.31.

The effect is largest in 2003 also for the other outcomes. While the effect on enrollment in higher education is estimated to be close to zero in the other years, the effect for enrollment in a study program in science or technology is estimated to be of about the same size in 2002 as in 2003.[18]

6. Concluding remarks

We estimate the causal effect of training in mathematics relative to languages by exploiting random selection of students into external exit examination in different subjects in Norwegian compulsory education. We find that treatment in terms of an intensive preparation period of 2–5 days in mathematics instead of languages increases the probabilities to graduate from high school, to enroll higher education, and to enroll a study program in natural sciences or technology in higher education. Five days of intensive training is estimated to increase these probabilities by about 1.0, 0.8, and 0.5 percentage points, respectively. For all outcomes, males appear to benefit the most from the treatment.

The treatment generally affects students across the whole ability distribution although at different margins. The positive effect on high school graduation is mostly related to improved progression for students initially enrolling in vocational study tracks in high school. These students have typically relative low prior skills. On the other hand, the positive effect on enrollment in science and technology programs in higher education seems

[18] The p-values of a test of equal treatment effects across cohorts are equal to 0.30 and 0.19 for enrollment in higher education and enrollment in science and technology, respectively, in the model specifications without controls.

to be restricted to students with relatively high skills in mathematics prior to the treatment. For these students, the treatment seems to have a positive short-term effect on grades in mathematics in high school.

The causal evidence is in accordance with simple descriptive associations between skills in different subjects measured by teacher set grades and the relevant outcomes. Taken together, the results suggest that mathematical skills are more important for broad measures of educational success than skills in languages.

One policy implication of these findings might be that all students should have an external exit examination in mathematics. If everybody knew that they should have their external examination in mathematics, they would, however, most likely prepare for that examination during the whole school year. The effect of such an institutional change is not possible to predict from the present study since student effort incentives for such a long period are likely to differ from that occurring in a short-term intensive preparation period just before the high-stake test. The present study indicates, however, that an intensive preparation period prior to mathematics exams is beneficial.

Taken together with the existing literature, our findings suggest that it would be beneficial for the students to be exposed to more training in mathematics in school. That can be achieved by extending the mathematics courses or by increasing the incentives by making mathematical skills more high-stake. For example, putting more weight on mathematical skills in enrolment procedures of students in high schools and higher education institutions would make mathematics a more high-stake subject for the students.

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Table 1. Data reduction

	Observations	Percent
Finish compulsory education in 2002-2004	174,067	100.0
Not turning 16 years the year finishing compulsory education	10,059	5.8
Missing information about teacher assessed grade for at least one of the subjects Mathematics, Norwegian language and English language	6.878	4.0
Missing compulsory school identifier	704	0.4
Central exit examination in Mathematics and one of the languages	578	0.3
Have exemption from central exit exam	146	0.1
Analytical sample	155,702	89.4

Table 2. Descriptive statistics for the intervention and educational attainment, percent

	All	2002	2003	2004	Females	Males	Low prior math skills	High prior math skills
Panel A: Central exit examination								
Examination in Mathematics	38.6	39.7	38.5	37.9	38.6	38.7	38.7	38.6
Examination in Norwegian	21.1	20.5	21.5	21.4	21.1	21.1	21.0	21.2
Examination in English	38.1	38.3	37.5	38.6	38.1	21.1	38.0	38.2
Students not appearing on the examination day	2.1	1.5	2.7	2.2	2.3	2.0	2.3	2.0
Panel B: Educational attainment								
Enrolling academic study track in high school	46.5	46.9	46.0	46.5	50.7	42.4	26.2	67.9
Graduating high school within 5 years	70.6	70.3	70.3	71.0	75.1	66.1	52.4	89.7
Enrollment in higher education	44.4	44.0	44.1	45.2	54.6	34.5	21.5	68.7
Enrollment in higher education, science or technology	6.0	5.9	6.1	6.0	4.2	7.8	1.5	10.8
Observations	155,702	49,534	51,185	54,983	76,770	78,932	80.038	75.664

Table 3. The relationship between teachers assessed grades in compulsory education and educational attainment

	(1)	(2)	(3)	(4)	(5)
Sample	All	Females	Males	Low prior math skills	High prior math skills
Panel A: Enrolling academic study track in high school					
Grade in Mathematics	0.0236***	0.0274***	0.0178***	-0.0235***	-0.0097***
	(0.0022)	(0.0032)	(0.0030)	(0.0038)	(0.0037)
Grade in Norwegian language	0.0066***	-0.0091***	0.0232***	0.0123***	-0.0098***
	(0.0024)	(0.0033)	(0.0033)	(0.0030)	(0.0035)
Grade in English language	0.0251***	0.0035	0.0420***	0.0335***	0.0104***
	(0.0021)	(0.0031)	(0.0028)	(0.0028)	(0.0031)
Grade point average (GPA)	0.2018***	0.2394***	0.1697***	0.1755***	0.2707***
	(0.0038)	(0.0054)	(0.0050)	(0.0046)	(0.0059)
Panel B: Graduating high school within five years					
Grade in Mathematics	0.0159***	-0.0016	0.0306***	0.0720***	-0.0250***
	(0.0020)	(0.0027)	(0.0028)	(0.0044)	(0.0024)
Grade in Norwegian language	-0.0281***	-0.0351***	-0.0241***	-0.0252***	-0.0133***
	(0.0021)	(0.0029)	(0.0030)	(0.0032)	(0.0025)
Grade in English language	-0.0548***	-0.0568***	-0.0535***	-0.0593***	-0.0408***
	(0.0018)	(0.0026)	(0.0026)	(0.0029)	(0.0023)
Grade point average (GPA)	0.2915***	0.3101***	0.2803***	0.3155***	0.1876***
	(0.0032)	(0.0045)	(0.0043)	(0.0045)	(0.0045)
Panel C: Enrollment in higher education					
Grade in Mathematics	0.0501***	0.0400***	0.0570***	0.0031	0.0083**
	(0.0021)	(0.0030)	(0.0027)	(0.0033)	(0.0036)
Grade in Norwegian language	0.0044**	-0.0104***	0.0207***	0.0059**	-0.0063*
	(0.0022)	(0.0032)	(0.0028)	(0.0027)	(0.0033)
Grade in English language	-0.0033*	-0.0298***	0.0185***	-0.0029	-0.0082***
	(0.0019)	(0.0028)	(0.0025)	(0.0024)	(0.0030)
Grade point average (GPA)	0.2261***	0.2916***	0.1694***	0.2057***	0.2882***
	(0.0035)	(0.0047)	(0.0046)	(0.0042)	(0.0056)
Panel D: Enrollment in higher education, science or technology					
Grade in Mathematics	0.0500***	0.0400***	0.0600***	0.0087***	0.0694***
	(0.0013)	(0.0015)	(0.0020)	(0.0011)	(0.0029)
Grade in Norwegian language	-0.0051***	-0.0043***	-0.0076***	-0.0009	-0.0143***
	(0.0012)	(0.0015)	(0.0020)	(0.0009)	(0.0024)
Grade in English language,	0.0074***	0.0018	0.0116***	0.0022***	0.0119***
	(0.0011)	(0.0013)	(0.0017)	(0.0008)	(0.0021)
Grade point average (GPA)	-0.0014	-0.0017	0.0005	0.0089***	-0.0023
	(0.0018)	(0.0021)	(0.0028)	(0.0014)	(0.0041)
Socioeconomic characteristics	Yes	Yes	Yes	Yes	Yes
Cohort times school fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	155,702	76,770	78,932	80,038	75,664

Note. The socioeconomic characteristics included in the models as described in section 3.3 and presented in Appendix Table A1 are included in all models. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 4. Intervention in mathematics, balancing tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean treatment	Mean control	Difference	Partial OLS on number of treatment days	OLS on number of treatment days	OLS on number of treatment days
Grade in Mathematics	-0.001	0.000	-0.0009 (0.0093)	0.0040 (0.0081)	-0.0195 (0.0178)	-
Grade in Norwegian language	0.006	-0.004	0.0095 (0.0094)	0.0151* (0.0080)	0.0142 (0.0195)	-
Grade in English language	0.002	-0.002	0.0041 (0.0093)	0.0065 (0.0082)	-0.0077 (0.0147)	-
Grade point average (GPA)	0.005	-0.003	0.0077 (0.0093)	0.0127 (0.00783)	0.0187 (0.0282)	-
Female	0.493	0.493	-0.0008 (0.0026)	0.0078 (0.0091)	-0.0117 (0.0129)	0.0016 (0.0088)
First generation immigrant	0.033	0.034	-0.0005 (0.0017)	-0.0134 (0.0423)	0.0062 (0.0468)	0.0073 (0.0468)
Second generation immigrant	0.021	0.020	0.0004 (0.0020)	0.0221 (0.0821)	0.0211 (0.0821)	0.022 (0.0822)
Parents' highest educational level is high school education	0.470	0.464	0.0061 (0.0043)	0.0178 (0.0149)	0.0294* (0.0157)	0.031** (0.016)
Parents' highest educational level is bachelor degree	0.288	0.290	-0.0015 (0.0028)	0.0059 (0.0118)	0.0153 (0.0196)	0.0197 (0.0194)
Parents' highest educational level is master or PhD	0.102	0.105	-0.0031 (0.0033)	-0.0287 (0.0308)	-0.0079 (0.0323)	-0.0029 (0.0329)
Benefits due to disease before the age of 18	0.019	0.019	-0.0006 (0.0007)	-0.0404 (0.0331)	-0.0535 (0.0378)	-0.0533 (0.0378)
Benefits due to disabilities before the age of 18	0.024	0.025	-0.0003 (0.0008)	0.0058 (0.0294)	0.0246 (0.0329)	0.0224 (0.0328)
One parent employed	0.239	0.243	-0.004 (0.0028)	-0.0151 (0.0130)	0.0046 (0.0242)	0.0052 (0.0242)
Both parents employed	0.710	0.704	0.0058 (0.0035)	0.0161 (0.0147)	0.0304 (0.0276)	0.0316 (0.0276)
2^{nd} quartile parental income	0.250	0.250	0.0008 (0.0031)	-0.0237* (0.0143)	-0.0224 (0.0152)	-0.022 (0.0151)
3^{rd} quartile parental income	0.253	0.248	0.0047* (0.0028)	0.0199 (0.0131)	-0.0046 (0.0200)	-0.0039 (0.0199)
4^{th} quartile parental income	0.246	0.252	-0.0060 (0.0049)	0.0090 (0.0232)	-0.0203 (0.0275)	-0.0192 (0.0274)
Married parents	0.613	0.608	0.0056* (0.0032)	0.0066 (0.0123)	0.0071 (0.0146)	0.0088 (0.0146)
Divorced parents	0.124	0.127	-0.0031* (0.0018)	-0.0172 (0.0146)	-0.0160 (0.0166)	-0.0166 (0.0165)
Mobility	0.112	0.111	0.0012 (0.0021)	0.0137 (0.0181)	0.0207 (0.0188)	0.0199 (0.0188)
Mobility unknown	0.021	0.022	-0.0011 (0.0009)	-0.0371 (0.0381)	-0.0176 (0.0377)	-0.0208 (0.0377)
Born second quartile	0.265	0.268	-0.0028 (0.0024)	-0.0096 (0.0107)	-0.0065 (0.0126)	-0.0069 (0.0125)
Born third quartile	0.258	0.259	-0.0009 (0.0024)	-0.0053 (0.0107)	-0.0029 (0.0125)	-0.0038 (0.0124)
Born fourth quartile	0.230	0.227	0.0029 (0.0022)	0.0213* (0.0115)	0.0132 (0.0127)	0.0117 (0.0126)
Cohort 2003	0.327	0.330	-0.0025 (0.0254)	0.949*** (0.113)	1.130*** (0.114)	1.130*** (0.114)
Cohort 2004	0.346	0.357	-0.0112 (0.0233)	-0.231*** (0.082)	0.342*** (0.070)	0.340*** (0.070)
Test of joint significance, excluding cohort specific effects, p-value					0.383	0.377

Note. 155,702 observations. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 5. Causal effects of mathematics on educational attainment

	(1)	(2)	(3)	(4)	(5)
Panel A: Enrolling academic study track in high school					
Number of treatment days	0.00052	0.00118	0.00119	0.00080	0.00071
	(0.00126)	(0.00103)	(0.00097)	(0.00092)	(0.00088)
Panel B: Graduating high school within 5 years					
Number of treatment days	0.00199**	0.00198***	0.00185**	0.00200***	0.00188***
	(0.00088)	(0.00073)	(0.00072)	(0.00071)	(0.00072)
Panel C: Enrollment in higher education					
Number of treatment days	0.00153	0.00171**	0.00164**	0.00190**	0.00153*
	(0.00112)	(0.00083)	(0.00082)	(0.00079)	(0.00081)
Panel D: Enrollment in higher education, science or technology					
Number of treatment days	0.00105***	0.00107***	0.00105***	0.00106***	0.00093**
	(0.00038)	(0.00039)	(0.00038)	(0.00040)	(0.00042)
Socioeconomic characteristics	No	Yes	Yes	Yes	Yes
Cohort specific effects	No	Yes	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes	Yes
School district fixed effects	No	No	No	Yes	Yes
School fixed effects	No	No	No	No	Yes
Observations	155,702	155,702	155,702	155,702	155,702

Note. The socioeconomic characteristics included in columns (2)-(5) are described in section 3.3 and presented in Appendix Table A1. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 6. Mathematics and educational attainment, heterogeneous effects

	(1)	(2)	(3)	(4)
	Enrolling academic study track in high school	Graduating high school within 5 years	Enrollment in higher education	Enrollment in higher education, science or technology
Panel A: Females				
Without control variables	0.00118 (0.00148)	0.00092 (0.00106)	0.00090 (0.00132)	0.00062 (0.00045)
With control variables	0.00179 (0.00132)	0.00126 (0.00094)	0.00109 (0.00108)	0.00063 (0.00046)
Observations	76,770	76,770	76,770	76,770
Panel B: Males				
Without control variables	-0.00023 (0.00157)	0.00293** (0.00114)	0.00191 (0.00139)	0.0015*** (0.00059)
With control variables	0.00054 (0.00131)	0.0026*** (0.00098)	0.00229** (0.00112)	0.00147** (0.00061)
Observations	78,932	78,932	78,932	78,932
Panel C: Low prior math skills				
Without control variables	0.00026 (0.00121)	0.0032*** (0.00116)	0.00214** (0.00104)	0.00042 (0.00027)
With control variables	0.00103 (0.00115)	0.0031*** (0.00112)	0.00225** (0.00096)	0.00039 (0.00027)
Observations	80,038	80,038	80,038	80,038
Panel D: High prior math skills				
Without control variables	0.00060 (0.00143)	0.00062 (0.00073)	0.00069 (0.00120)	0.00167** (0.00069)
With control variables	0.00160 (0.00129)	0.00100 (0.00070)	0.00151 (0.00107)	0.00178** (0.00071)
Observations	75,664	75,664	75,664	75,664

Note. Each cell represents an independent regression with the number of treatment days as independent variable. The control variables include the socioeconomic characteristics described in section 3.3 and presented in Appendix Table A1 and cohort fixed effects. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 7. Mathematics and progression in high school education

	(1)	(2)	(3)	(4)	(5)
	Enrolling third year on-time	Enrolling third year on-time, academic study track	Enrolling third year on-time, not academic study track	Enrolling final semester within 5 years, not academic study track	Has been apprentice, not academic study track
Panel A: All					
Without control variables	0.00188**	0.00066	0.00267**	0.00280***	0.00134
	(0.00088)	(0.00080)	(0.00122)	(0.00100)	(0.00130)
With control variables	0.00151**	0.00032	0.00204*	0.00228**	0.00075
	(0.00074)	(0.00076)	(0.00113)	(0.00092)	(0.00118)
Observations	155,702	72,352	83,350	83,350	83,350
Mean dependent variable	0.752	0.900	0.623	0.754	0.409
Panel B: Females					
Without control variables	0.00098	0.00059	0.00076	0.00222*	0.00071
	(0.00103)	(0.00096)	(0.00161)	(0.00130)	(0.00141)
With control variables	0.00073	0.00029	0.00047	0.00225*	0.00058
	(0.00093)	(0.00093)	(0.00152)	(0.00123)	(0.00142)
Observations	76,770	38,918	37,852	37,852	37,852
Mean dependent variable	0.786	0.913	0.656	0.780	0.236
Panel C: Males					
Without control variables	0.00268**	0.00068	0.0043***	0.00331**	0.00181
	(0.00115)	(0.00115)	(0.00154)	(0.00131)	(0.00170)
With control variables	0.00220**	0.00034	0.00327**	0.00225*	0.00078
	(0.00101)	(0.00113)	(0.00146)	(0.00124)	(0.00160)
Observations	78,932	33,434	45,498	45,498	45,498
Mean dependent variable	0.719	0.886	0.595	0.732	0.552
Panel D: Low prior math skills					
Without control variables	0.00253**	0.00072	0.00310**	0.0033***	0.00095
	(0.00119)	(0.00191)	(0.00136)	(0.00119)	(0.00138)
With control variables	0.00233**	0.00082	0.00257*	0.00272**	0.00071
	(0.00113)	(0.00189)	(0.00132)	(0.00115)	(0.00128)
Observations	80,038	20,983	59,055	59,055	59,055
Mean dependent variable	0.603	0.779	0.540	0.685	0.397
Panel E: High prior math skills					
Without control variables	0.00105	0.00061	0.00177	0.00182*	0.00231
	(0.00066)	(0.00059)	(0.00149)	(0.00100)	(0.00218)
With control variables	0.00084	0.00033	0.00147	0.00162	0.00036
	(0.00065)	(0.00060)	(0.00147)	(0.00102)	(0.00187)
Observations	75,664	51,369	24,295	24,295	24,295
Mean dependent variable	0.909	0.950	0.824	0.923	0.438

Note. Each cell represents an independent regression with the number of treatment days as independent variable. The control variables include the socioeconomic characteristics described in section 3.3 and presented in Appendix Table A1 and cohort fixed effects. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 8. Mathematics and high school achievement, academic study track

	(1)	(2)	(3)	(4)
	Choosing first year advanced mathematics	Value added in first year mathematics	Choosing second year mathematics	Value added in second year mathematics
Panel A: All				
Without control variables	0.00059 (0.00167)	0.0048 (0.0030)	-0.00020 (0.00158)	0.0013 (0.0041)
With control variables	0.00127 (0.00163)	0.0080** (0.0031)	0.00136 (0.00143)	0.0038 (0.0042)
Observations	58,012	58,012	58,012	29,392
Mean dependent variable	0.618	-0.665	0.505	-1.007
Panel B: Females				
Without control variables	0.00129 (0.00193)	0.0056 (0.0035)	0.00088 (0.00195)	0.0027 (0.0051)
With control variables	0.00134 (0.00197)	0.0083** (0.0035)	0.00230 (0.00185)	0.0052 (0.0053)
Observations	32,500	32,500	32,500	14,048
Mean dependent variable	0.556	-0.654	0.431	-0.912
Panel C: Males				
Without control variables	-0.00001 (0.00216)	0.0037 (0.0037)	-0.00124 (0.00211)	-0.0008 (0.0051)
With control variables	0.00118 (0.00208)	0.0076** (0.0037)	0.00016 (0.00186)	0.0027 (0.0051)
Observations	25,512	25,512	25,512	15,344
Mean dependent variable	0.698	-0.679	0.600	-1.095
Panel D: Low prior math skills				
Without control variables	-0.00258 (0.00286)	-0.0004 (0.0040)	-0.00287 (0.00227)	0.0020 (0.0101)
With control variables	-0.00050 (0.00289)	0.0028 (0.0041)	-0.00062 (0.00223)	-0.0017 (0.0105)
Observations	12,051	12,051	12,051	2,058
Mean dependent variable	0.222	-0.457	0.169	-0.527
Panel E: High prior math skills				
Without control variables	0.00117 (0.00169)	0.0062* (0.0033)	0.00028 (0.00170)	0.0019 (0.0041)
With control variables	0.00176 (0.00163)	0.0090* (0.0032)	0.00173 (0.00152)	0.0046 (0.0042)
Observations	45,961	45,961	45,961	27,334
Mean dependent variable	0.722	-0.720	0.593	-1.044

Note. Each cell represents an independent regression with the number of treatment days as independent variable. The control variables include the socioeconomic characteristics described in section 3.3 and presented in Appendix Table A1 and cohort fixed effects. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 9. Causal effects of mathematics on educational attainment, dummy variable specifications

	(1)	(2)	(3)	(4)
	Enrolling academic study track in high school	Graduating high school within 5 years	Enrollment in higher education	Enrollment in higher education, science or technology
Panel A: All				
Without control variables	0.00019 (0.00482)	0.00686** (0.00333)	0.00287 (0.00413)	0.00377*** (0.00145)
With control variables	0.00198 (0.00366)	0.00671** (0.00264)	0.00436 (0.00295)	0.00397*** (0.00140)
Observations	155,702	155,702	155,702	155,702
Panel B: Females				
Without control variables	0.00053 (0.00563)	0.00249 (0.00386)	0.00045 (0.00483)	0.00275 (0.00168)
With control variables	0.00323 (0.00470)	0.00328 (0.00333)	0.00271 (0.00385)	0.00307* (0.00165)
Observations	76,770	76,770	76,770	76,770
Panel C: Males				
Without control variables	-0.00000 (0.00590)	0.01126*** (0.00433)	0.00554 (0.00518)	0.00470** (0.00216)
With control variables	0.00093 (0.00462)	0.00997*** (0.00355)	0.00631 (0.00395)	0.00473** (0.00215)
Observations	78,932	78,932	78,932	78,932
Panel D: Low prior math skills				
Without control variables	0.00019 (0.00468)	0.01144*** (0.00436)	0.00577 (0.00385)	0.00110 (0.00098)
With control variables	0.00060 (0.00404)	0.01006** (0.00395)	0.00563* (0.00337)	0.00119 (0.00096)
Observations	80,038	80,038	80,038	80,038
Panel E: High prior math skills				
Without control variables	0.00164 (0.00540)	0.00330 (0.00265)	0.00142 (0.00443)	0.00692*** (0.00262)
With control variables	0.00399 (0.00462)	0.00351 (0.00247)	0.00387 (0.00379)	0.00682*** (0.00259)
Observations	75,664	75,664	75,664	75,664

Note. Each cell represents an independent regression with the number of treatment days as independent variable. The control variables include the socioeconomic characteristics described in section 3.3 and presented in Appendix Table A1 and cohort fixed effects. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table 10. Cohort specific effects of mathematics on educational attainment, dummy variable specifications

	(1)	(2)	(3)	(4)
	Enrolling academic study track in high school	Graduating high school within 5 years	Enrollment in higher education	Enrollment in higher education, science or technology
Panel A: 2002				
Without control variables	-0.0078 (0.0102)	0.0040 (0.0071)	-0.0053 (0.0090)	0.0058** (0.0027)
With control variables	-0.0045 (0.0069)	0.0052 (0.0053)	-0.0015 (0.0056)	0.0058** (0.0025)
Observations	49,534	49,534	49,534	49,534
Panel B: 2003				
Without control variables	0.0160 (0.0108)	0.0137** (0.0069)	0.0171* (0.0095)	0.0058** (0.0026)
With control variables	0.0127* (0.0072)	0.0099** (0.0051)	0.0130** (0.0058)	0.0053** (0.0024)
Observations	51,185	51,185	51,185	51,185
Panel C: 2004				
Without control variables	-0.0074 (0.0110)	0.0033 (0.0069)	-0.0026 (0.0093)	0.0001 (0.0024)
With control variables	-0.0028 (0.0075)	0.0053 (0.0050)	0.0010 (0.0058)	0.0010 (0.0023)
Observations	54,983	54,983	54,983	54,983

Note. Each cell represents an independent regression with the number of treatment days as independent variable. The control variables include the socioeconomic characteristics described in section 3.3 and presented in Appendix Table A1 and cohort fixed effects. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Appendix

Appendix Table A1. Descriptive statistics for independent variables

	Mean value
Female	0.493
First generation immigrant	0.034
Second generation immigrant	0.020
Parents' highest educational level is high school education	0.466
Parents' highest educational level is bachelor degree	0.289
Parents' highest educational level is master or PhD	0.103
Benefits due to disease before the age of 18	0.019
Benefits due to disabilities before the age of 18	0.025
One parent employed	0.241
Both parents employed	0.706
Parental income 2^{nd} quartile	0.250
Parental income 3^{rd} quartile	0.250
Parental income 4^{th} quartile	0.250
Married parents	0.610
Divorced parents	0.126
Mobility	0.111
Mobility unknown	0.022
Born second quartile	0.267
Born third quartile	0.259
Born fourth quartile	0.228
Observations	155,702

Appendix Table A2. Number of treatment days, balancing tests for subsamples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	2002	2003	2004	Females	Males	Low prior math skills	High prior math skills
Female	-0.0168*	0.0099	0.0103	-	-	0.0153	-0.0128
First generation immigrant	-0.0275	-0.0361	0.0844	-0.0204	0.0336	0.0403	-0.0465
Second generation immigrant	-0.0280	-0.0454	0.1070	0.0470	-0.0046	0.0072	0.0424
Parents' highest educational level is high school education	-0.0164	0.1240***	-0.0142	0.0123	0.0494**	0.0333*	0.0272
Parents' highest educational level is bachelor degree	-0.0269	0.1110**	-0.0248	0.0041	0.0351	0.0333	0.0088
Parents' highest educational level is master or PhD	-0.0007	0.0691	-0.0723	-0.0253	0.0188	-0.0139	-0.00415
Benefits due to disease before the age of 18	0.0098	-0.1980**	0.0227	-0.0339	-0.0685	-0.0078	-0.1220**
Benefits due to disabilities before the age of 18	-0.0342	0.1000	-0.0020	0.0440	0.0111	-0.0187	0.0934*
One parent employed	0.0517**	-0.0218	-0.0071	0.0041	0.0066	0.0113	-0.0035
Both parents employed	0.0569**	-0.0110	0.0521	0.0310	0.0324	0.0264	0.0388
Parental income 2nd quartile	0.0009	-0.0255	-0.0410*	-0.0288	-0.0153	-0.0219	-0.0202
Parental income 3rd quartile	-0.0060	0.0179	-0.0256	0.0149	-0.0221	-0.0201	0.0168
Parental income 4th quartile	-0.0348	0.0525	-0.0735	-0.0274	-0.0112	-0.0259	-0.0082
Married parents	0.0188	0.0130	-0.0023	0.0012	0.0157	0.0227	-0.0108
Divorced parents	0.0094	-0.0234	-0.0331	-0.0044	-0.0290	0.0060	-0.0533**
Mobility	0.0270	0.0650	-0.0279	-0.0068	0.0477*	0.0229	0.0136
Mobility unknown	-0.0168	0.0073	-0.0564	0.0061	-0.0460	-0.0731	0.0481
Born second quartile	-0.0185	0.0013	-0.0036	0.0036	-0.0169	-0.0240	0.0102
Born third quartile	-0.0208	-0.0119	0.0198	0.0076	-0.0145	-0.0005	-0.0080
Born fourth quartile	-0.0088	0.0287	0.0139	0.0275	-0.0035	0.0139	0.0078
Cohort specific effects	-	-	-	Yes	Yes	Yes	Yes
Observations	49,534	51,185	54,983	76,770	78,932	80,038	75,664
Test of joint significance of the socioeconomic characteristics, p-value	0.403	0.262	0.470	0.589	0.305	0.446	0.160

Note. Standard errors in parentheses are clustered at the compulsory school level. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.