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# AN EVALUATION OF A NATIONAL PROGRAM TO REDUCE STUDENT ABSENTEEISM IN HIGH SCHOOL

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## ABSTRACT

Starting in the 2016/17 academic year, high school students in Norway who missed more than 10 percent of the hours in a given course without a medical excuse could not receive a final grade. We examine the impacts of this policy on student absenteeism, the incidence of the no grade penalty and two measures of student achievement. The policy had the intended impact on absenteeism, reducing total absence by 20-28 percent, and chronic absence by 29-39 percent in the high school grades. This behavioral response was largely sufficient to avoid the academic penalty for absence over the 10 percent threshold under the new law. Finally, we find a mixed impact on student achievement: little impact on externally graded, end of year exams, and modest evidence of a positive impact of 6 percent of a standard deviation on teacher awarded GPA.

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## Introduction

Writing in 1993, Romer (1993) noted that while most teachers were aware that class attendance was not always 100 percent, "There is surprisingly little systematic evidence…, about attendance and its effects" (p. 167). Almost three decades later there are now a number of studies of student absenteeism, but our knowledge remains fragmentary. Representative data sources that systematically track students' attendance and performance are not common. Moreover, it is not straightforward to estimate the impact of student absence, because we suspect that students with better grades are more likely to attend classes, while students with lower grades are more likely to skip.

For the US, estimates of the average percentage of students "in class" by year, is available in the *Digest of Education Statistics*. For example, in 2007/08 and 2011/12 it averaged around 93 percent nationally, and in the latter year was higher in primary than in secondary schools (Snyder and Dillow 2018). The US Department of Education's 2015-16 Civil Rights Data Collection sheds more light. Focusing on what is called chronic absenteeism, defined as missing at least 15 days of school in a year, these data reveal 16 percent of the student population was chronically absent (U.S. Department of Education 2016). This proportion was generally higher for minority groups than whites,[1] and higher in high school where it was in excess of 20 percent of students.[2]

Whether this level of absenteeism constitutes a crisis, as some have claimed (U.S. Department of Education 2016), depends in part on the consequences of absenteeism for student

[1] The exception is students of Asian descent.

[2] Whitney and Liu (2016) note that absenteeism statistics often miss part of the story because they focus on full day absences while many absence spells are, in fact, part day. Drawing on administrative data for middle and high school students in a large urban school district in California, they report that part day absence is more prevalent than full day absence by a factor of roughly three. Furthermore, while slightly over half of full day absences are unexcused, in excess of 90 percent of part day absences are unexcused.

success. One could argue that students cannot learn if they are not in class, and absenteeism is associated with poorer academic and socioemotional outcomes (e.g., Jacob and Lovett 2017). However, it is less clear that absenteeism has a causal effect on student performance.

The reasons for student absence range from illness through avoiding a threatening school environment to a general lack of interest in school. Because the types of students who are absent for these different reasons are not necessarily the same, we might expect the impact of absence to vary across them. For example, those who are not interested in school may find little benefit from either being encouraged or forced to attend. Those who would otherwise attend if they were not sick, or threatened at school, might find greater benefit from attendance.

Policies to address truancy include measures to address the structural causes of absence and simpler penalty/reward schemes to encourage attendance. An advantage of the latter is that they are likely to have lower direct costs and are relatively easy to scale. In contrast, programs that attempt to address root causes may be delivered in one-on-one settings by trained practitioners.

In this paper, we contribute to the literature on student absenteeism by providing, to our knowledge, the first evaluation of a national, system wide program to directly address high school truancy. The application is to the Norwegian high school system where (pre policy) chronic absenteeism in the high school grades was in excess of 25 percent. Starting in the 2016/2017 school year, high school students absent more than 10 percent of classes in a given course without a documented medical excuse did not receive a final grade.

Focusing on this policy innovation in Norway offers a number of advantages. First, our data capture students' absences, academic outcomes, progress through grades, they record both full day and part day absence and they provide important demographic characteristics and

background variables. Second, the policy affected all students in Norway, and our data for the entire population of high school students allows us to look for heterogeneous impacts within the population. Third, student absence in Norway is comparable to that in other developed countries, enhancing the external validity of the results. Finally we have access to comparable data for students in the grade preceding high school—grade 10—who provide a plausible control group for the analysis.

Our results indicate that the policy had its first order intended impact. Once the policy was in place total student absence fell by 20-28 percent, and chronic absence fell by 29-39 percent. The initial impact of the policy is larger than the impact in subsequent years, especially in the higher high school grades, suggesting that students may have adjusted their absence to the new rules over time, perhaps by spreading it over different courses. We also find that this adjustment in behavior was largely sufficient to avoid the penalties under the new law. We find no impact on the incidence of receiving a no grade in a course, which is also true for demographic groups with higher pre-policy levels of absence. Finally, we fail to find consistent evidence that the substantial increase in student attendance led to higher levels of student achievement. On one hand, we find little impact on externally graded end of year exams. On the other, we document modest evidence of small positive effects, of 6 percent of a standard deviation, on teacher awarded course grades.

## 2 Literature

A common summation of research on truancy is that while an association between absenteeism and a host of negative student outcomes has been well documented, establishing a causal link between the two is more elusive (e.g., Jacob and Lovett 2017). As noted above part of the challenge is that high quality data on absenteeism and student achievement covering broad populations is not typically available.

One branch of inquiry is non-experimential and has based inference on a variety of fixed effects or instrumental variables identification strategies. For example studies using student or family fixed effects (e.g., Martins and Walker 2005, Stanca 2006, Gottfried 2011) suggest a positive link between attendance and student performance, although Arulampalam et al. (2012) argue that this is only true for high performing students. Two recent examples, Cattan et al. (2021) who adopt a family fixed effects specification and study elementary school truancy in Sweden, and Liu et al. (2021) whose identification strategy exploits within student, between subject variation in absence in secondary schools in California, come to a fairly similar conclusion that 10 days of absence reduces academic performance by 3 to 4.5 percent of a standard deviation.[3] Studies using quasi-experimental approaches, for example using the distance between a student's home and school to instrument for absence, also support a causal negative relationship between absence and achievement (e.g., Gottfried 2010).

Another branch of inquiry is experimental which typically speaks to absence in more select populations. For example, some studies are based in experimental interventions in university classes, randomly assigning students to sections at different times of the day, or enforcing policies of mandatory attendance (e.g., Marburger 2006, Chen and Lin 2008, Dobkin

[3] Cattan et al. (2021) also present direct evidence of the impact of this absence on adult income.

et al. 2009, Arulampalam et al. 2012). These studies typically find that increased attendance leads to better outcomes on the course final exam. Another strand of this research is on specialized programs that attempt to address the structural causes of truancy in groups with low levels of attendance. For example, some interventions connect students with supports and mentors that can help match students' problems with the appropriate social services. They can also provide personalized monitoring and encouragement. A recent example is Guryan et al.'s (2020) evaluation of the Check and Connect mentoring program within the Chicago school system. Within a sample of disadvantaged children, their RCT evidence indicates that this program did reduce absence but had little effect on achievement in grades 1-4.[4]

There are also evaluations of policies that either directly or indirectly attempt to encourage attendance or penalize absence, similar to the approach here. For example, Barua and Vidal-Fernandez (2014) investigate the impact of "no pass, no drive" laws passed in some US states. By these laws, teenagers must attain certain school grades and attendance standards to maintain their driver's license. The authors report that these laws led to a consequent reduction in absenteeism.

Still another line of investigation is whether better school inputs can increase attendance. In this vein, recent research indicates that "better" teachers can have substantive effects on the absence of older students (Jackson 2018, Liu and Loeb 2021).

Finally, also relevant to the question of whether presence in classroom matters is research on events such as snow days (e.g., Goodman 2014), flu outbreaks (e.g., Aucejo and Romano 2016) or teacher strikes (e.g., Baker 2013), which shock student attendance. They generally find a positive relationship between attendance and achievement.

[4] Instituted monitoring and support systems have also been found to reduce absenteeism (e.g., Faria et al. 2017) in more targeted RCTs.

Against this background, our contribution is the evaluation of a public policy that targets student absenteeism at a system wide level. This episode not only provides an attractive basis for identification of the effects of absenteeism, but also direct evidence on the efficacy of a relatively simple policy to limit truancy. The Norwegian policy imposes a penalty for absence, with no accompanying set of supports to encourage compliant behavior. Also, the policy is universal in application addressing truancy among a heterogeneous group of students. This type of approach clearly should not reduce absence due to, for example, illness, and medically documented absence is exempt from the penalty. It may lead students facing threatening environments at school to attend anyway, to avoid the penalty. Finally, the most certain potential impact is on those students who skip classes due to lack of interest and /or the availability of more attractive alternatives.

## 3 Institutional Setting

### 3.1 The Norwegian school system

The Norwegian school system consists of primary school (grades 1-7), lower secondary school (grades 8-10) and high school (upper secondary, grades 11-13). Whereas primary and lower secondary school are compulsory, high school is voluntary. While most students enroll in the 11$^{th}$ grade, the dropout rate in high school is approximately 25 percent.[5] The vast majority of high schools in Norway are public, with private schools enrolling about 8 percent of students (Statistics Norway 2021).[6] The public high schools are administered at the county level.

[5] Defined as not having completed high school five years after end of compulsory education, based on years 2013 to 2018 (data source described below).
[6] The share of private school students in compulsory schooling is even lower at about 4 percent.

There is a two-track system in Norwegian high schools. The standard academic track is three years, and upon completion, the student is eligible for tertiary education. The regular duration of the vocational track is two years of school and two years as an apprentice with professional guidance. Following a reform in 2006 (Kunnskapsløftet), the 9 fields of specialization available to students on the vocational track are technical and industrial production, electricity and electronics, building and construction, restaurant and food processing, health and social sciences, design and crafts, media and communication, service and transport and agriculture, fishing and forestry. Upon completion of the vocational track, the student earns a craft certificate.

There is some mobility between the vocational and academic tracks. Students on the vocational track may become eligible for university if they complete a supplementary year of general education courses.[7]

### 3.2 The policy change

Starting in the 2016/2017 school year, the Norwegian Education Authority introduced a new regulation aimed at reducing student absenteeism in high schools. It stated that students could not miss more than 10 percent of the hours in a given course without presenting a medical certificate. Those who exceeded the 10 percent threshold without a medical excuse would not receive a final grade in the course.

The consequences for students who did not meet the new attendance rules and thus ended up without a final mark in a subject, depended on whether the student was in grade 11, 12 or 13. For students in grade 13, the student would not be able to get their diploma, and hence could not apply for college/university that year. Students in grades 11 and 12 had the opportunity to take a

[7] Bertrand et al. (2021) provide more details on the vocational track in Norway.

private exam, at a cost of 1100 NOK per subject. If they passed the exam, they could progress with their agemates. However, preparing for the private exam added to the normal course load, and this could be a challenge for weaker students. In some school districts, students without marks in more than three subjects were required to repeat the entire year. Finally, principals had the discretion to expand the threshold to 15 percent of the hours in a course under extraordinary circumstances.

Prior to this new policy, teachers would register days and hours of absence throughout the school year. Subsequently, the total amount of absence by grade (11, 12 and 13) would appear on the student's diploma at graduation.[8] There were certain exceptions, such as longer medical absences,[9] political activities such as participation in student organizations, religious holidays and sports activities on the national/international level. Teachers had the discretion to award a final mark in a course to students with a high level of absence based on whether they believed they had sufficient basis for student assessment (e.g., if the student had participated on tests throughout the year, been active during class etc.).

## 4 Data

To study the cap on absence and its effects on student absence and performance, we use rich register data from Statistics Norway. We have detailed information on hours and days of absence for each year for every student in high school and the last year of lower secondary education (grade 10), as well as their marks and time of high school completion. We thus can track students from age 15 to when they leave high school.

[8] The recording of days and hours of absence continued under the new policy.

[9] Absence for more than 3 consecutive days due to medical absences would be erased from the diploma upon presenting a medical certificate. This did not change after the new policy.

Data on absence from high school is available for the school years 2013/2014-2018/2019—three years prior to and three years post the policy reform. Information on absence and student attainment from grade 10 is available from the school year 2008/2009 onwards, which means that we have information on school achievement and absence from secondary education for all high school students in our sample. Schools register full days and hours of absence separately. In addition, our data contains a variable that sums up total hours the student has been absent.[10] We also construct a measure of chronic absenteeism, defined as being absent the equivalent of 15 full days or more.

We also examine measures of the penalties under, and possible consequences of, the new law. To study the probability of not receiving a final mark, we construct two measures. First, is a measure of incidence—a 0/1 indicator whether a student did not receive a final mark in at least one course, by year. The second adds the intensive margin by counting the number of no grade events in a year including the zeros.

To explore whether the new policy affected student performance, we study average grades on end of year exams[11] and average teacher awarded grades. End of year exams are administered centrally, externally graded, and students are randomly drawn to exams in different subjects depending on their grade.[12] Grade 10 students have two exams, one oral and one written. The written exam subject is either Norwegian, English or Mathematics. The subject for each student is decided through a random draw. For the academic track, in grade 11 about 20 percent of students will be drawn to write an exam or have a practical or oral exam. In grade 12,

[10] 1 day=6 hours

[11] The score used is an individual score for each subject.

[12] This is based on information from the Directorate of Education, accessed January 3, 2022 at https://www.udir.no/regelverkstolkninger/opplaring/eksamen/trekkordning-ved-eksamen-for-grunnskole-og-videregaende-opplaring-udir-2-2018/

all students complete a randomly drawn written or oral exam. All students in grade 13 sit one written exam in Norwegian, and, depending on their field of study, will also sit 1-2 other exams, as well as having one oral or practical exam. For the vocational track, about 20% are randomly drawn to an exam in grade 11. In grade 12, all students have an exam in their field of study, plus about 20 percent are again randomly drawn to an additional exam. Students that start in the vocational track and decide to complete an additional year to qualify for academic studies, face similar rules as those in grade 13 in the academic track and sit one written exam in Norwegian as well as are randomly drawn to one written and one oral or practical exam.

We restrict our sample to the written exams across grades. An advantage of the written external exams is that most are centrally administered and graded by a randomly assigned external grader. However, only a subsample of students write these exams in certain grades and the samples for select subjects can be small. We also exclude the results in math exams for all grades. This is due to a revision of the grade 10 math exam in 2015 which led to an increase in average performance starting in 2016 (see figure A1 in the appendix).

The second measure of students' academic outcomes is the annual average of teacher awarded grades to the student, henceforth "GPA". In most applications externally graded evaluations are preferred to grades awarded by a student's teacher. The fear is that the teacher could be affected by the treatment under consideration, and so teacher awarded grades do not provide an unbiased measure of the treatment's effect. Teachers' incentives to award better or worse grades once attendance changes are not straightforward. One possibility is that teachers adhere to historical averages in courses. These averages will reflect a certain level of (pre reform) absenteeism, so that if, for example, absenteeism declines, and attendance is positively related to academic outcomes, any improvement in average student performance will be

suppressed to adhere to the historical average. Another possibility is that teachers respond empathetically to better attendance as a signal of student effort, and reward this behavior with better grades which do not reflect an underlying improvement in achievement.

We obtain information on the background characteristics of the students and their families from administrative registers. Demographic data includes continent of origin, gender and age. To control for student background, we construct separate dummy variables capturing whether the mother and/or father has a college education. We also use a measure of family income at age 14 before the students enter high school. To capture immigrant background, we construct a dummy variable equal to one if the student has two foreign-born parents. As immigrants may face different circumstances depending on where they are from, we also include a dummy variable for the mother's continent of origin.

To be included in the sample, an individual must be registered as a high school student and be between the ages of 16 and 21, or be a grade 10 student between ages 15 and 18. We exclude observations with negative absence[13] and observations where absence is above the total number of school hours during the year.

In table 1 we present some summary statistics for our analysis sample. The educational attainment of parents and the share of immigrants increases over the sample years. There are also small changes in the composition of immigrants as the proportion with mothers born in other parts of Europe declines, while the proportion with mothers born in Asia or Africa increases. The proportion of students living in low income[14] families is quite constant over the period, as

[13] This must be due to errors in registration.

[14] As noted above family income is measured at age 14 before the students enter high school. We define low income in accordance with the EU60 measure of the poverty line, which implies that the family earns less than 60 percent of the median income weighted after family type. We simplify this measure slightly by relying on data we have access to (i.e. the family income of parents of teen-agers), and end up with including about 17 percent in this definition. To compare, this share is 14 percent when it is calculated for the entire population, see Sandvik (2020).

are the proportions of the sample in grade 10, the academic track and the various vocational fields of study. Finally, while the sample size is quite stable over the sample years, there is a slight decrease starting in 2017. This is likely related to smaller birth cohorts from 2001 and onwards.[15]

The new policy could potentially affect the composition of our sample over time if the new rules led some students to choose not to attend high school, or severely hindered their progress through high school, perhaps leading them to drop out. We provide an overview of this issue in figure 1. By cohort, we graph the proportion of grade 10 students who we observe in grade 11 (in year $y$+1) through graduation (in year $y$+4) by their year of graduation, $y$, from lower secondary. The proportions of grade 10 students who are observed in grade 11 are relatively stable over the sample period at roughly 92 percent, save for a small decrease in 2016 of just over 3 percentage points. The proportions observed in later grades are also quite stable over the period. Also notable, the proportion of the 2016 cohort observed in the later grades is comparable to the other cohorts. During year four (measured in October) we consistently observe just over 40 percent of each cohort that has still not graduated high school. This figure provides some preliminary evidence that the penalties under the new policy did not affect students' progress through high school.

## 5 Empirical strategy

Our primary empirical strategy is a difference-in-differences framework, in which we use observations on outcomes in grade 10 as a control. For example, for the analysis of student absence, grade 10 students will presumably be exposed to similar viral infections and weather

[15] From 2000 to 2001 the number of births in Norway declined from 58 393 to 55 882, and continued on that level until it increased again in 2006 (see https://www.ssb.no/befolkning/fodte-og-dode/statistikk/fodte),

conditions as their slightly older counterparts in high school, but are not subject to the new cap on absence, which only applied to grades 11 through 13. The suitability of grade 10 outcomes as a control, however, does vary with the outcome considered. For example, for academic performance, on one hand grade 10 students are of an adjacent birth cohort to their high school counterparts, and so will have been exposed to similar environments in childhood and previous school grades. On the other, grade 10 is part of lower secondary school and so administratively distinct from grades 11-13 which are high school. We have scanned for possible grade 10 confounders that might undermine this identification approach, and as noted above omit grades on math exams due to the revision of the grade 10 exam in 2015. That said, we make clear the contribution of the grade 10 controls in each step of the analysis to better elucidate any possible bias from these considerations.

The base difference-in-differences specification is:

$$o_{it} = \theta(t) \cdot yr_t + \lambda(t) \cdot HS_{it} \cdot yr_t + \psi X + \omega_{it}, \quad (1)$$

where $o_{it}$ is an outcome for individual *i* in year *t*, $HS_{it}$ is a dummy variable which equals 1 for students in grades 11-13 who are subject to the new policy, $yr_t$ is a full set of single year dummy variables, and *X* includes full sets of grade, age (single year), field of study, and school fixed effects and controls for immigrant background, gender, family income, parents' education and county of origin. We adopt the convention of identifying school years by their end—rather than their start—year, so the new law comes into effect in 2017, and we omit the year effect for 2016, the pre-reform year. Our primary focus is on the estimates of the $\lambda(t)$ on the interactions between the *HS* dummy variable and the year effects, particularly in the years 2017, 2018 and 2019—the post policy period.

We present standard errors clustered on school, which is the unit at which the new policy was administered. We have also estimated standard errors clustering on individual identifiers and on grade*year. These standard errors are generally smaller than the ones reported, and in any event have no substantive effect on our inference.

# 6 Results

## 6.1 Absence

We begin our analysis focusing on the first order target of the new law—the amount of time students miss class. In figure 2, we graph the average total hours of student absence by grade over the period the law came into effect. We might expect that average total hours of absence would differ by grade, because, for example, students' health habits or proclivity to skip class without an excuse, might vary by age. However, we would also expect these averages to display common temporal influences such as seasonal flus and viruses.

The evidence in figure 2 exhibits both of these effects. In the years prior to the reform (pre 2017) total absence does vary by grade—the difference between students in grade 11 and 13 is close to 20 hours. There are also common time effects, as the average in each of the high school grades falls over this sub-period in almost parallel fashion. In 2017, there is a sharp decline in total absence in each of grades 11 through 13 suggesting that the new policy had the intended effect. The decline appears largest for grade 12, as in 2017 average total absence is almost the same in grades 11 and 12. By 2019, there is a modest "recovery" in absence in grades 12 and 13, although it is still lower than in the pre-reform years.

We also report total absence for grade 10 students in figure 2. Absence for these students provides a counterfactual for any shocks or trends that are common to students in all these grades. Note that pre-reform total absence is very similar in grades 10 and 11, suggesting that

higher absence in grades 12 and 13 is acquired behavior rather than an impact of high school entry per se. Note also that Grade 10 absence exhibits a steeper decline between 2014 and 2015 than the other grades, a difference that will show up in the regression estimates. Finally, starting in 2017 there is no evidence of sharp reduction in absence, which we expect given that grade 10 students were not subject to the new policy. It instead continues on a modest downward trend consistent with the pattern in the reform years.

The difference-in-differences estimates of $\lambda(t)$ from the estimation of (1) for different measures of absence are reported in the top panel of Table 2. Recall each estimate here is relative to 2016. In the first column, the pre-reform year (2016) mean for total hours of absence is just under 71 hours, or almost 12 days of instruction. The difference-in-differences estimates for total hours absent post reform—2017-2019—are all negative, statistically significant and range between 14 and 20 hours. Relative to the 2016 mean this represents reductions of roughly 20 to 28 percent, or 2-3 days. Note that the (pre policy) estimate for 2014 is almost 7 hours and statistically significant. As per the discussion of figure 2, this is expected as the 2014/15 decline in absence for grade 10 exceeded that in the other grades.

High school courses in Norway range from 56 to 168 hours (75 to 224 school hours ) of instructional time per year (Sjaastad et al 2016). Total annual hours of instruction varies by the academic and vocational streams and by specialization within streams. For students pursuing general studies within the academic stream for science, math, languages, social science and economics, annual instruction hours are 840 hours per year.[16] Against this standard, in 2016 average total absence was in excess of 8 percent of total instructional hours, falling almost 2.4 percentage points in the first year of the reform.

[16] See https://www.vilbli.no/nb/en/agder/subjects-and-choice-of-subjects/a/030669 (accessed January 15, 2021). In many vocational stream the annual hours are in excess of 900.

In the next two columns are estimates for full day absences and hours of part day absences, separately. In each case there is a clear impact of the reform starting in 2017. These results also indicate that the deviation in the pre-trends in 2014, between grade 10 and grades 11-13 is more to do with differences in the estimates for full day absences.

Finally, in the last column are the estimates for chronic absence, which is often used as a summary statistic to document the severity of the truancy issue.[17] In the preform reform year the chronic absence rate was over 25 percent in our sample. The threshold for chronic absence represents more than 90 hours of annual absence or almost 11 percent of the previously cited 840 hours of annual instruction in the academic stream. Therefore, if this absence is in a single course, it would exceed the 10% threshold to be awarded a no grade under the policy reform. The new policy appears to have had a substantial impact on excessive absence by this measure, reducing it between 7 and 10 percentage points or 29 to 39 percent.

Note that for each of the outcomes the point estimates indicate an initial (2017) policy effect which is larger than in subsequent years. In the last row of the panel we report the result of a test of the hypothesis that the estimates for 2017 and 2019 are equal. It is rejected for each outcome. This pattern might be expected if students learn to spread their desired level of absence across courses so as not to approach or exceed the 10 percent threshold in any one course. Note also, that proportionately the impact is fairly similar for hours of partial day absence and full days of absence. This might be surprising if full day absence is more likely due

[17] We cumulate all hours of absence and using an average of 6 hours of instruction per day and construct a 0/1 indicator of being absent more than 15 days, or 3 weeks. Note constructing a measure of chronic absence based solely on full day absence leads to much lower incidence underlining the observation of Whitney and Lui (2018) of the importance of partial day absence.

to less discretionary causes such as sickness, while partial day hours of absence is due to skipping, and thus more easily adapted to the changed circumstances.

In the second panel of table 2 we present fixed effects estimates of the impact of the policy. Here we add individual level fixed effects to our empirical specification. Note we lose some observations, primarily for grade 13 students in the first year of our sample and grade 10 students in the last year. This change of specification provides some perspective on the role of the control group in our differences-in differences approach. The results are largely consistent with the estimates in the first panel—clear impacts of the policy and substantive effects on both total absence and chronic absence.

In table 3 we present the difference-in-differences estimates by grade. Average total absence (top panel) varies pre reform (in 2016) from over 10 days in grade 10 and 11 to over 13 days in grade 13. The regression estimates for total absence in the top panel are consistent with the inference from figure 2. The attenuation of the effect of the policy on total absence in grades 12 and 13 in 2018 and 2019 is clear in the estimates. In contrast, the impact of the policy on grade 11 is relatively constant, and the F-test result indicates that we cannot reject the hypothesis that the estimates for 2017 and 2019 are equal. In the bottom panel are estimates for chronic absence. Here we see that chronic absence grows dramatically with grade. The 2016 average in grade 13 is more than 50 percent larger than the grade 11 average, and in excess of 30 percent. Again, there is a relatively constant impact of the reform for grade 11s but a diminishing impact for grades 12 and 13. However, in proportionate terms the recovery in chronic absence is less pronounced than the recovery in total absence for these higher grades.

One criticism of uniform, system wide policies to address truancy is that they may have disparate impacts on different demographic groups. In table 4 we investigate the impact of the

policy on absence by gender, immigrant status and by our measure of low income. The results by gender are perhaps surprising, revealing that girls had marginally higher pre-reform levels of total and chronic absence, and that the policy had larger absolute and proportional effects on boys' behavior. Part of this difference is a substantial attenuation of the impact of the reform on girls' absence in 2019.

The results by immigrant status reveal very large differences in preform absence ranging from 25 to 33 percent. Total hours of absence averages more than 15 days for students from immigrant families in 2016, which is the threshold for chronic absence. Correspondingly the reform has larger absolute and proportionate impacts on the absence of these students. Because the threshold for receiving a no grade penalty does not vary by pre reform absence, we might expect those groups with higher absence would make a larger adaptation to the new policy. Note also that the attenuation of the policy effect post reform is greater for the native born in proportionate terms, particularly with regards to chronic absence.

The results by low income are similar to those by immigrant status, except here the differences in pre-reform behavior are even larger. We see a larger absolute and proportionate adjustment to the policy among low income students in light of the uniform threshold for receiving a no grade outcome.

These results provide clear evidence that the new policy had its first order intended effect. There were substantial impacts on both total and chronic absence. At the aggregate level, chronic absence is estimated to be 29 percent lower than its pre-reform mean by 2019 and is almost 28 percent lower for students from immigrant families.

The results are also consistent with some form of student learning about the new policy as time passed. The initial impact of the policy is substantively larger than the impact three years

after the reform for many groups. Notably this attenuation effect is largely absent for grade 11 students, and is less so for students from immigrant and low income families. Certainly spreading out a target level of absence over many courses is a strategy to avoid the penalties under the new policy. Pre policy it may have been optimal to be absent primarily in courses that either held little interest or in which the consequences of absence were less. In this way it may be that any direct effects of the new policy on student achievement through higher attendance would be attenuated by students decisions post policy to now be absent in "higher stakes" courses.

Overall, these results provide a strong "first stage" to investigate whether the new policy had any impact on students' progress through high school through its penalties, and on students' academic performance through its impact on students' attendance.

### 6.2 Penalties

We next investigate whether students' adjustment of their absence behavior to the new policy was sufficient to avoid its penalties for excessive absence. In Figure 3, we plot the incidence of a no grade event, by grade, over our sample period. First note that a no grade is a relatively rare event affecting roughly 2 to 4 percent of students across years. The time series for the high school grades move roughly in parallel in the pre-policy period. In the first year of the reform the incidence of no grades continues to decline in grades 11 and 12, but exhibits a modest uptick in grade 11. There is then a modest uptick in each of these grades in 2018, which resolves in the higher grades in 2019, and persists in grade 11. In contrast to the high school grades, the incidence of no grades in grade 10 exhibits a sharp increase in 2015 before moving largely in tandem with the grade 11 time series in subsequent years.

The difference-in-differences estimates are presented in the first column of table 5. Consistent with figure 3, there is a statistically significant and positive estimate for 2014 (relative to 2016) for grades 11-13, and very small and mostly statistically insignificant estimates for the post reform years. In the second column are the estimates for our measure of the number of no grade events. This measure includes zeros, and so encompasses both the extensive and intensive margins. Here the estimates post policy are again fairly small but some are statistically significant. However, they are all smaller than the estimate for 2014, which is not due to the policy, and so are more likely due to some difference in trends between the treatment and control groups.

Further detail is provided in Table 6 where we present the difference-in-differences estimates for these measures by grade. As is evident in figure 3, in the top panel there is a difference in pre trends between grade 10 and the high school grades between 2014 and 2015. Subsequently, any differences in the time series across the four grades are relatively small, save for a reduction in the incidence of no grades for grade 13 in 2017. The inference for the number of no grades is largely similar for the pre-reform years. Starting in 2017 there is some evidence of a relative increase in the number of no grades in the high school grades, but the inference here is limited by the imprecision of the estimates.

Finally, in table 7 we present the estimates by our demographic groups. The split by gender reveals the higher incidence and number of no grades among boys—the incidence is almost 30 percent higher in 2016. However, there is little evidence of a systematic policy impact on the incidence of a no grade for either gender. For the number of no grades there is perhaps a modest policy effect on the number of no grades for boys, although all the post policy estimates are within the range of the estimate for 2014. The splits by immigration and low income

highlight the much higher incidence of no grades within the minority groups—2 to 3 times the incidence for the students with native born parents or from higher income families. Also, while the inference from the estimates by income are consistent with that from the split by gender and the aggregate results, the estimates by immigrant status appear to indicate some policy effect for immigrants. Some perspective on this result for the incidence of no grade is provided in figure 4. Here we plot the time series for the no grade event, separately for grade 10 and high school students, by immigrant background. For immigrants, the incidence of no grade increases in the high school grades and decreases in grade 10 over the post policy years, leading to the positive and statistically significant estimates in the table. However, prior to the reform the time series for grade 10 and high school exhibit separate trends and the overall variation pre and post policy are not discernibly different.

While drawing nuanced conclusions about the penalties under the new policy is frustrated by both the lack of precision of some of the estimates and the low incidence of no grades, there is certainly little evidence of a systematic effect of the policy on the incidence of no grades, as we see in the various measures of absence. These two findings are likely related—by lowering absence significantly, students were able to largely avoid the penalty under the new law.

### 6.3 Student performance

The new truancy policy increased the amount of time students attended class. However, concerns about high levels of truancy are typically due to its suspected impact on academic achievement, rather than due to concerns about absence per se. Given the substantial reduction in absence we observe after the policy change, we have an attractive basis to evaluate the relationship between attendance and achievement for this broad group of students.

The time series for externally graded exams and teacher awarded GPA, by grade, are reported in figures 5 and 6. The time series for grade 10 exams (figure 5) is quite flat over the period, exhibiting a small uptick in 2017 and then subsequent decline in 2019. Pre policy, there are roughly parallel trends for grades 10, 11 and 13, while the relative trend for grade 12 is modestly positive. With the new policy, the grade 11 and 13 scores initially move in tandem with the grade 10 results, but then exhibit a relative upwards trend in the later years. The time series for grade 12 continues on the pre policy, relative, positive trend until 2017 and then moves in step with the grade 10 results.

The time series for teacher awarded GPA (figure 6) reveals that grade 10 grades were on a reasonably constant upward trend over the period. The pre trends are fairly parallel for grades 10, 11 and 12, but the grade 13 time series is flatter. Starting in 2017, there is evidence of modest convergence of the high school outcomes with the grade 10 results, particularly for grade 13. Also, the convergence for the grade 12 time series appears mostly complete by 2017 and arguably reflects its pre policy trend. Finally, note that there is a general upward trend in all grades' average GPAs over the period, perhaps undermining an argument that teachers adhere to long run historical averages in awarding grades.

The regression estimates are in table 8. Consistent with figure 5 there is less evidence of a policy effect than of pre trends in the data in the externally graded exams. The estimate for 2019 is relatively large and statistically significant reflecting the downtick in grade 10 exam results in this year. In contrast, the estimates for 2017-2019 for GPA provide some modest evidence of a policy effect. The estimates for 2017-2019 are positive and span a reasonably tight interval. Note the estimate for 2014 is also positive, although of a smaller magnitude, reflecting the relatively larger increase in grades 11 and 12 grades between 2014 and 2016. If we view the

2017-2019 estimates as a policy impact, taking 0.055 as their midpoint implies an effect that is 1.4 percent of the 2016 mean or just over 6 percent of a standard deviation in GPA among high school students in 2016.

A challenge to the identification of a policy effect on teacher awarded grades could be that students are not awarded marks in subjects where they failed to meet the new attendance requirement. If the cap systematically pushed certain groups out of the mark distribution, there might be a mechanical policy effect on student performance. Of particular concern here is that lower performing students were less likely to meet the new requirement. The previous evidence that the new policy had little impact on the incidence of a no grade event helps allay this concern. As a further check, we have re-estimated our model assigning students with no grade in a subject the lowest possible mark to check for this mechanical effect. Reassuringly, the results (see Appendix Table A1) are similar to the results in table 8.

Because the academic and vocational streams have distinct curriculums and testing schedules, we have also considered the results restricting the high school samples to students in the academic stream. The estimates, reported in Appendix Table A2, lead to very similar inference to that from table 8.

In table 9 we present the estimates for our two measures of student achievement by grade. They largely match the messages of Figures 5 and 6. For end of year exams, there is clear evidence that the results for grade 11 deviated from both the grade 10 results in the post policy years, but also from the results in grades 12 and 13. Recall that only 20 percent of grade 11 students complete an external exam, fewer a written external exam, so these estimates are based on a relatively quite small sample.[18] There is also a large positive estimate for each of the high

[18] Accordingly, omitting grade 11 students from the sample leads to "aggregate" estimates very similar to those reported in table 8.

school grades in 2019, due to the downtick in the grade 10 results in this year. For teacher awarded grades, we again see some evidence of a policy effect. It is present for all grades and is largest for grade 13 and smallest for grade 11, consistent with Figure 6.

Finally, the estimates by our demographic splits are presented in tables 10 and 11. First note there are sizable differences in the 2016 means across the demographic groups, which are generally larger for the teacher awarded grades. For example, the female advantage is 26 percent of standard deviation in 2016 for the end of year exams, but 32 percent of a standard deviation in the teacher awarded grades. The difference across the two evaluation instruments is more pronounced for the other splits of the data. The immigrant/native born difference is 36 percent of a standard deviation for the end of year exams, but 48 percent for the teacher awarded grades. Likewise, the low income difference is almost 35 percent of a standard deviation for end of year exams, but 50% for GPA.

Turning to the regression estimates, the results for the end of year exams, reinforce the message of the preceding two tables that there is no systematic evidence of an increase in student achievement related to the new policy, although there is a positive and statistically significant estimate for all groups in 2019. In contrast, the results for GPA reveal positive estimates of roughly the same absolute magnitude for all groups starting in 2017.

A factor not explicitly considered so far is that if the effects of better attendance on academic achievement are cumulative then we might expect that the impact of the policy would grow over time for students in the higher grades. In all years of the sample grade 11 students are exposed to one year of the new policy. If the effect of this exposure is constant, then this is less consistent with estimated increasing policy effect on end of year exams of grade 11s in table 9, and more consistent with the relatively constant policy impact on GPA. Grade 12 students will

be exposed to one year of the new policy in 2017, but two years in 2018 and 2019. Again the estimated effect of the policy on end of year exams moves around a fair bit, while the estimated impact on GPA is again fairly constant. Finally the exposure of grade 13 students grows linearly with time ranging from one year in 2017 to three years in 2019. The estimated policy effect for this grade in end of year exams swings around a bit, while it is first relatively constant then increases for GPA.

Complicating this interpretation of the results, however, is that while students' exposure to the policy grows with grade in high school, as documented in table 3 the impact of the policy on absence diminishes somewhat over time. Thus while the grade 13s in 2019 have been exposed to the policy for 3 years, their absence is higher than grade 13s in the preceding years.

If the results for teacher awarded grades are interpreted as causal policy effects they present the puzzle that the higher attendance induced by the policy led to higher achievement on one evaluation instrument—GPA—but less so on another—end of year exams. This might be rationalized if teachers rewarded the higher attendance induced by the policy with higher marks. Alternatively, teacher set evaluations might more comprehensively examine material taught in class than the end of year exams, and therefore be more sensitive to changes in attendance.

Taking the impact on teacher awarded grades at face value, is 6 percent of a standard deviation, from table 8, a large effect? Estimates of student progress in the high school years in different subjects have been estimated to range between 0.2 to 0.25 of a standard deviation (e.g., Bloom et al. 2008). Average absence prior to the reform was roughly 8 percent of total instruction for academic track students, somewhat less for those in the vocational track. If instruction was purely additive and the estimates of average student development in the high school years were due solely to high school instruction, then the estimated decrease in absence of

two percent of instructional hours for academic stream students would be expected to have a much smaller impact than 6 percent of a standard deviation. However, instruction is more likely cumulative in many subjects so that a certain amount of absence is likely to have a greater impact than that calculated simply by its proportion of total instructional time.

Another perspective is provided by the estimates from Cattan et al. (2021) and Liu et al. (2021) that 10 days of absence reduces academic performance by 3 to 4.5 percent of a standard deviation. Here the reduction in total absence averages just under 3 days (table 2), so the estimated impact on teacher awarded grades indicates a larger effect.

## 7 Conclusions

Student absenteeism has been associated with poorer academic and socioemotional outcomes. Whether these associations represent causal relationships remains an open area of investigation. In the autumn of 2016, Norwegian educational authorities implemented a new policy for students in high school. Prior to this, teachers had discretion to award marks to their students who were regularly absent, whereas after the policy change, students could not be absent more than 10 percent of classes in a given course without a documented medical excuse. Students in violation of the new rule would not receive a mark in that course subject.

An analysis of this reform reveals it was an effective tool to reduce student absenteeism. Total absence fell by 20 to 30 percent and chronic absence was reduced by at least 30 percent. Furthermore, these reductions in absence were attained without an excessive incidence of the penalties under the new policy, which mandated that students absent more than 10 percent of hours in a class would not receive a final grade. Finally, we find that the policy had little impact on students' scores on externally graded end of year written exams, and evidence that it had a small positive effect on teacher awarded grades.

The Norwegian policy is primarily a penalty based system to address student absenteeism. Its direct costs are mostly administrative, plus (potentially) increased remedial instruction costs if the incidence of the penalty (the number of no grade events) increases substantially. That said, there may have been indirect costs of the policy, for example an increased demand for medical notes to justify student absence. On average, this structure appears to have provided an effective tool to address student truancy without unduly penalizing certain groups within the student population, as we are able to capture them in the data. Our estimates of the benefits of the policy are mixed, varying by whether the outcome is externally graded end of year exams or teacher awarded grades. However, there may be other benefits to increased school attendance not captured by academic performance, including important social development and better work habits.

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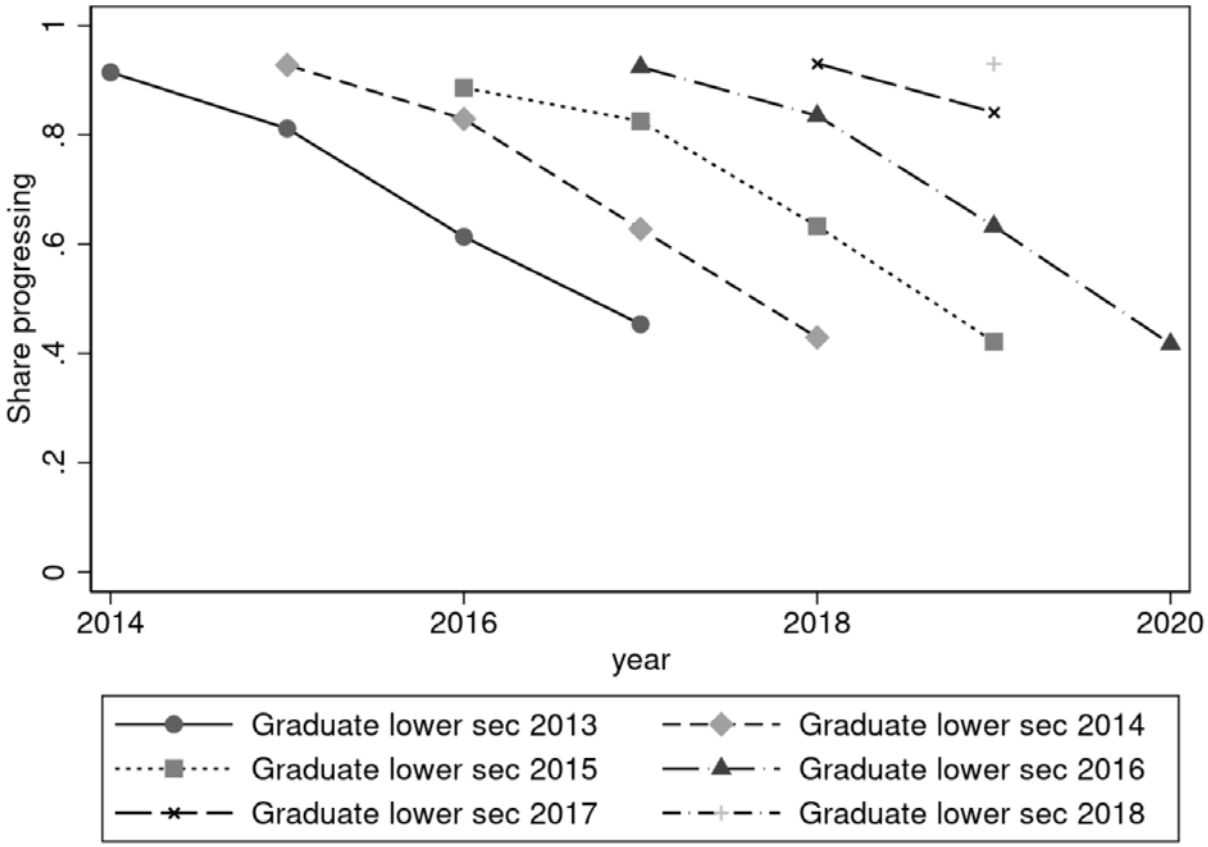
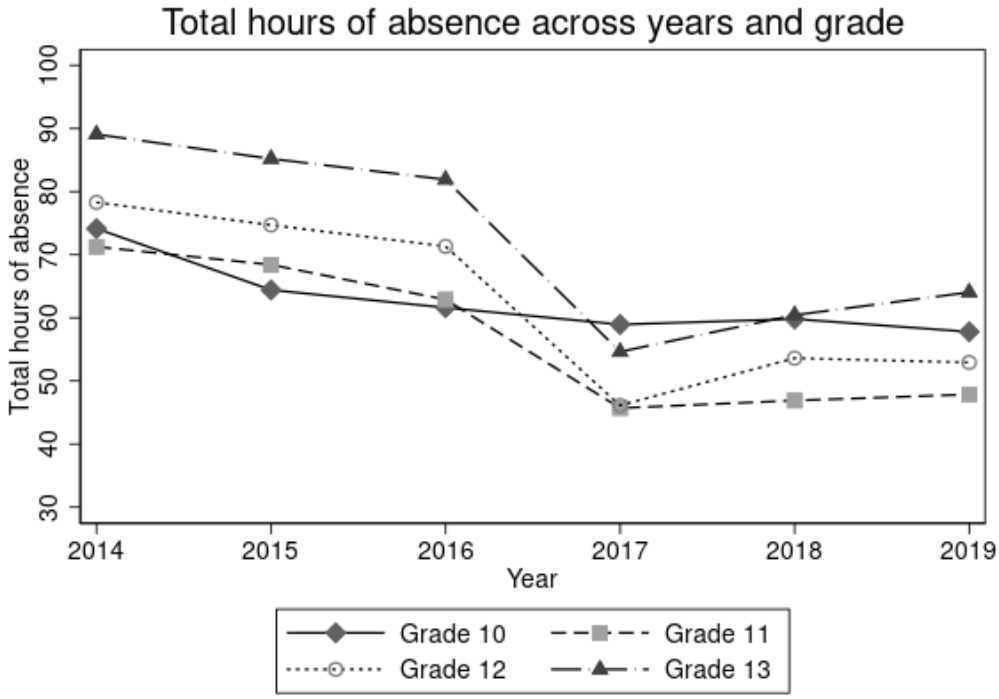
**Figure 1: The Proportion of Grade 10 cohorts Progressing through High School to Graduation (2013-2018 Cohorts)**



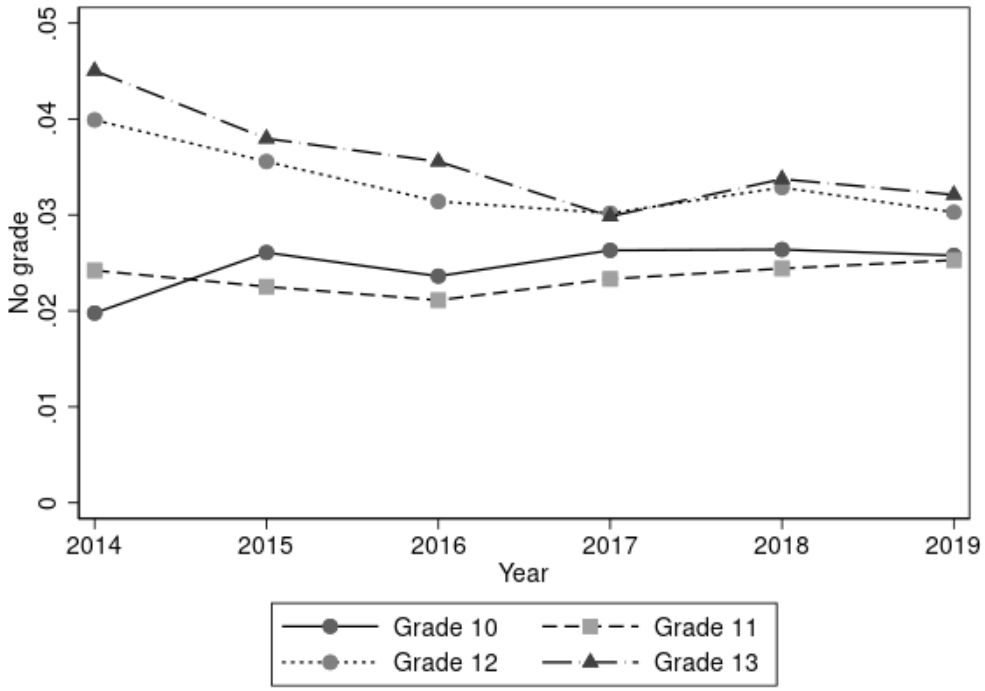

Notes: Authors' calculations from Norwegian Registry data.

**Figure 2: Total Hours Absence by Grade, 2014-2019**



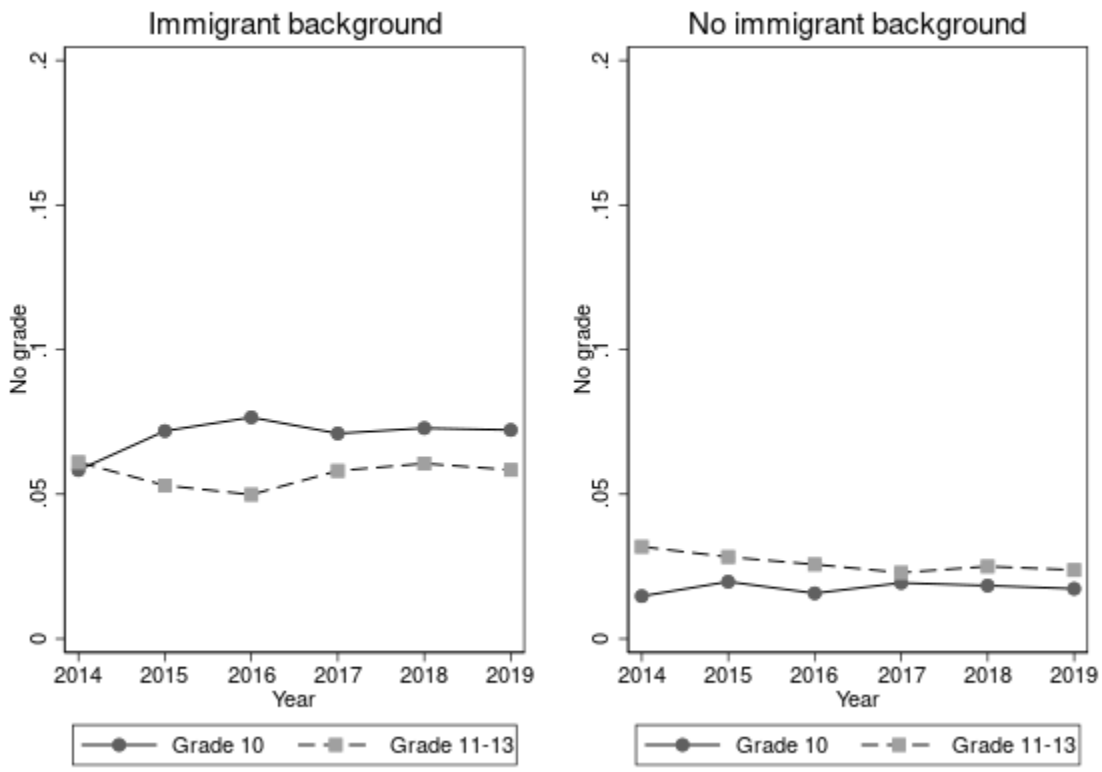

Notes: Authors' calculations from Norwegian Registry data

**Figure 3: The Incidence of a No Grade in a Course, by Grade, 2014-2019**



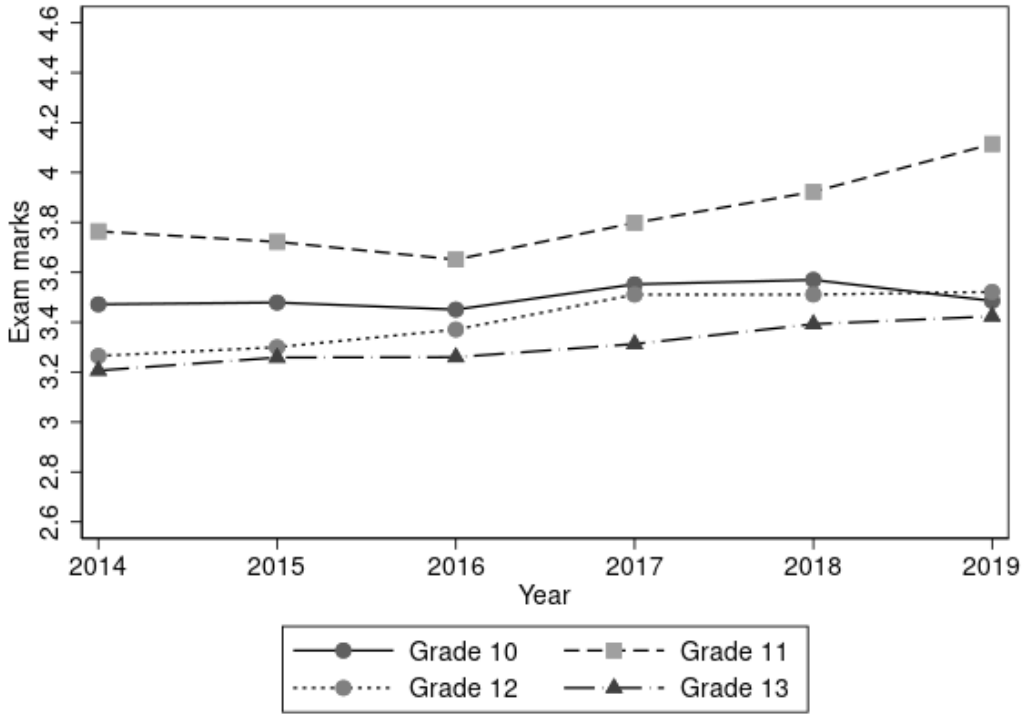

Notes: Authors' calculations from Norwegian Registry data

**Figure 4: The Incidence of a No Grade in a Course, by Immigrant Status, 2014-2019**



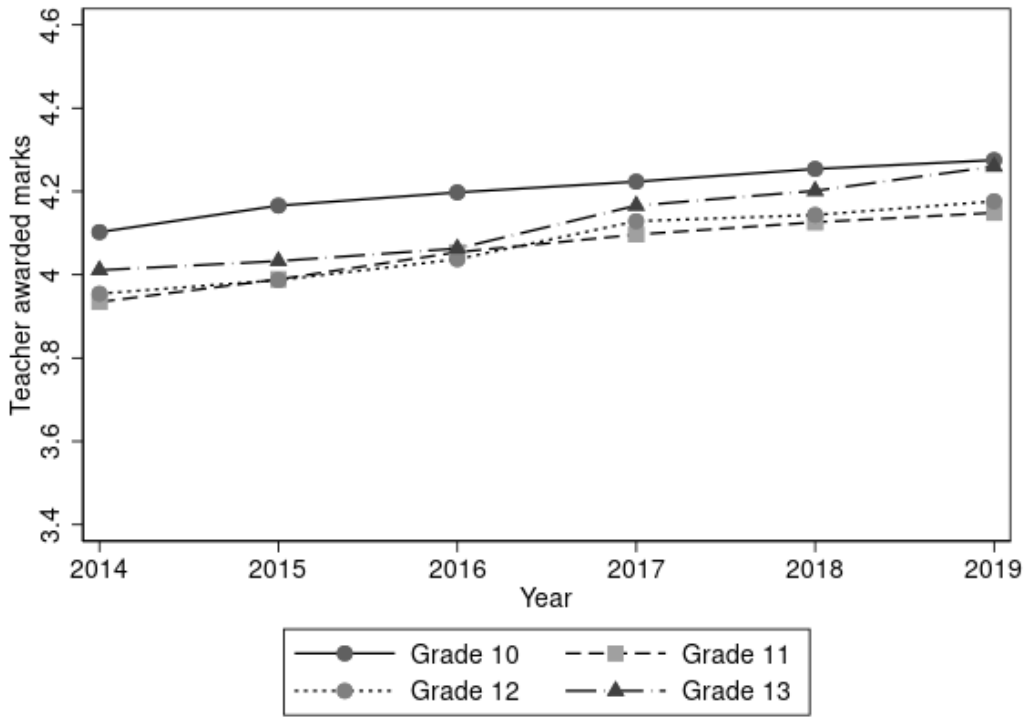

Notes: Authors' calculations from Norwegian Registry data

**Figure 5: Average Score on Externally Graded End of Year Exams, by Grade, 2014-2019**




Notes: Authors' calculations from Norwegian Registry data

**Figure 6: Average Teacher Awarded GPA, by Grade, 2014-2019**




Notes: Authors' calculations from Norwegian Registry data

**Table 1: Summary statistics**

| | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|
| Female | 0.50 | 0.50 | 0.50 | 0.50 | 0.50 | 0.50 |
| Immigrant | 0.11 | 0.12 | 0.12 | 0.13 | 0.14 | 0.15 |
| Asia | 0.06 | 0.06 | 0.07 | 0.07 | 0.07 | 0.08 |
| Africa | 0.02 | 0.02 | 0.02 | 0.03 | 0.03 | 0.03 |
| America/Oceania | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Europe | 0.91 | 0.90 | 0.90 | 0.89 | 0.88 | 0.88 |
| Mother finished HS | 0.71 | 0.72 | 0.74 | 0.75 | 0.76 | 0.76 |
| Father finished HS | 0.70 | 0.71 | 0.71 | 0.72 | 0.72 | 0.73 |
| Family income (NOK) | 885709 | 924544 | 960599 | 1001685 | 1036603 | 1067422 |
| Low income | 0.17 | 0.17 | 0.18 | 0.17 | 0.17 | 0.17 |
| **Area of study** | | | | | | |
| Lower secondary | 0.27 | 0.27 | 0.27 | 0.27 | 0.27 | 0.27 |
| Academic track | 0.45 | 0.46 | 0.46 | 0.46 | 0.46 | 0.45 |
| Building and construction | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 |
| Design and crafts | 0.01 | 0.01 | 0.01 | 0.02 | 0.02 | 0.02 |
| Electricity and electronics | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 |
| Health and social work | 0.06 | 0.06 | 0.06 | 0.06 | 0.06 | 0.07 |
| Media and communication | 0.04 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 |
| Agriculture, fishing and forestry | 0.01 | 0.01 | 0.01 | 0.01 | 0.02 | 0.02 |
| Restaurant and food processing | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Service and transport | 0.03 | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 |
| Technical and industrial production | 0.05 | 0.05 | 0.04 | 0.04 | 0.04 | 0.04 |
| N | 217792 | 218357 | 218867 | 216152 | 215001 | 214483 |

Notes: Authors' calculations from Norwegian Registry data. The 2014 column reports statistics for the students attending high school during the academic year 2013/2014.

**Table 2: Difference-in-differences and Fixed Effects Estimates of the Impact of the New Absence Policy on Various Measures of Student Absence**

| | Total Hours Absent | Days absent | Hours absent | Chronic absence |
|---|---|---|---|---|
| Difference in Differences N=1300652 | | | | |
| 2014 | -6.873 | -1.250 | 0.630 | -0.026 |
| | (1.844)*** | (0.267)*** | (0.354)* | (0.008)*** |
| 2015 | -0.363 | -0.100 | 0.236 | 0.005 |
| | (1.357) | (0.198) | (0.256) | (0.006) |
| 2017 | -20.511 | -2.409 | -6.059 | -0.099 |
| | (1.365)*** | (0.195)*** | (0.297)*** | (0.006)*** |
| 2018 | -17.156 | -2.185 | -4.044 | -0.090 |
| | (1.232)*** | (0.172)*** | (0.333)*** | (0.006)*** |
| 2019 | -14.722 | -1.870 | -3.502 | -0.074 |
| | (1.269)*** | (0.175)*** | (0.365)*** | (0.006)*** |
| 2016 Mean | 70.99 | 8.35 | 20.90 | 0.254 |
| F-test | 20.11 | 8.70 | 64.82 | 16.41 |
| | [0.00] | [0.00] | [0.00] | [0.00] |
| Fixed Effects N=1157851 | | | | |
| 2015 | 0.297 | 0.057 | -0.042 | 0.001 |
| | (0.379) | (0.055) | (0.108) | (0.002) |
| 2017 | -15.763 | -1.899 | -4.367 | -0.081 |
| | (0.701)*** | (0.092)*** | (0.228)*** | (0.003)*** |
| 2018 | -12.919 | -1.659 | -2.964 | -0.069 |
| | (0.959)*** | (0.131)*** | (0.298)*** | (0.005)*** |
| 2019 | -13.893 | -1.846 | -2.818 | -0.075 |
| | (1.324)*** | (0.184)*** | (0.390)*** | (0.006)*** |
| 2016 Mean | 70.99 | 8.35 | 20.90 | 0.254 |
| F-test | 4.36 | 0.18 | 35.08 | 1.97 |
| | [0.04] | [0.67] | [0.00] | [0.16] |

Notes: Authors' calculations from Norwegian Registry data. For the difference-in-differences estimates additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. For the fixed effects estimates additional controls include grade and field of study fixed effects. The reported means are for the year 2016 are for grades 11-13. Standard errors clustered on school reported in parentheses. F-test is for the hypothesis that the estimated effects for 2017 and 2019 are equal, p-values in square brackets. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14.

**Table 3: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Student Absence by Grade**

| | Grade 10 | Grade 11 | Grade 12 | Grade 13 |
|---|---|---|---|---|
| Total Absence N=1300652 | | | | |
| 2014 | 11.550 | -6.523 | -7.833 | -5.907 |
| | (1.766)*** | (1.873)*** | (1.944)*** | (2.052)*** |
| 2015 | 2.792 | 0.523 | 1.202 | -0.431 |
| | (1.288)** | (1.428) | (1.458) | (1.584) |
| 2017 | -2.168 | -15.824 | -22.521 | -24.577 |
| | (1.227)* | (1.427)*** | (1.451)*** | (1.552)*** |
| 2018 | -0.475 | -15.804 | -16.709 | -19.562 |
| | (1.069) | (1.265)*** | (1.359)*** | (1.485)*** |
| 2019 | -2.214 | -14.125 | -15.407 | -14.599 |
| | (1.054)** | (1.321)*** | (1.359)*** | (1.566)*** |
| 2016 Mean | 61.61 | 62.89 | 71.33 | 81.91 |
| F-test | | 1.62 | 27.52 | 44.28 |
| | | [0.20] | [0.00] | [0.00] |
| Chronic Absence N=1300652 | | | | |
| 2014 | 0.054 | -0.025 | -0.028 | -0.022 |
| | (0.008)*** | (0.009)*** | (0.009)*** | (0.010)** |
| 2015 | 0.011 | 0.009 | 0.002 | 0.005 |
| | (0.006)* | (0.007) | (0.007) | (0.008) |
| 2017 | -0.014 | -0.070 | -0.109 | -0.128 |
| | (0.006)** | (0.007)*** | (0.007)*** | (0.007)*** |
| 2018 | 0.002 | -0.079 | -0.086 | -0.112 |
| | (0.005) | (0.006)*** | (0.007)*** | (0.007)*** |
| 2019 | -0.012 | -0.065 | -0.078 | -0.082 |
| | (0.005)** | (0.006)*** | (0.007)*** | (0.008)*** |
| 2016 Mean | 0.204 | 0.208 | 0.256 | 0.314 |
| F-test | | 0.64 | 22.70 | 39.56 |
| | | [0.42] | [0.00] | [0.00] |

Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. F-test is for the hypothesis that the estimated effects for 2017 and 2019 are equal, p-values in square brackets. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14.

**Table 4: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Student Absence by Selected Demographic Characteristics**

| | Females | Males | Immigrants | Native Born | Low income | Non-low income |
|---|---|---|---|---|---|---|
| **Total Absence** | | | | | | |
| 2014 | -8.396 | -5.560 | -8.493 | -6.739 | -9.175 | -6.549 |
| | (1.949)*** | (1.940)** | (2.733)*** | (1.881)*** | (2.607)*** | (1.820)*** |
| 2015 | 0.819 | -0.065 | -2.954 | 0.166 | -1.564 | 0.245 |
| | (1.457) | (1.459) | (2.062) | (1.404) | (2.001) | (1.361) |
| 2017 | -19.179 | -21.869 | -31.557 | -18.994 | -32.304 | -18.668 |
| | (1.452)*** | (1.493)*** | (2.112)*** | (1.393)*** | (2.110)*** | (1.347)*** |
| 2018 | -16.380 | -17.903 | -27.520 | -15.570 | -26.465 | -15.649 |
| | (1.316)*** | (1.362)*** | (1.919) *** | (1.278)*** | (1.969)*** | (1.212)*** |
| 2019 | -12.725 | -16.629 | -25.841 | -12.855 | -25.664 | -12.779 |
| | (1.386)*** | (1.357)*** | (2.109)*** | (1.293)*** | (2.025)*** | (1.236)*** |
| 2016 Mean | 71.77 | 70.21 | 91.86 | 68.06 | 96.68 | 65.26 |
| F-test | 21.61 | 14.23 | 6.26 | 22.40 | 9.58 | 21.41 |
| | [0.00] | [0.00] | [0.01] | [0.00] | [0.00] | [0.00] |
| **Chronic absence** | | | | | | |
| 2014 | -0.034 | -0.019 | -0.031 | -0.025 | -0.027 | -0.026 |
| | (0.009)*** | (0.009)** | (0.012)*** | (0.009)*** | (0.011)** | (0.009)*** |
| 2015 | 0.002 | 0.008 | -0.005 | 0.006 | -0.000 | 0.006 |
| | (0.007) | (0.007) | (0.010) | (0.007) | (0.009) | (0.007) |
| 2017 | -0.093 | -0.107 | -0.152 | -0.092 | -0.153 | -0.091 |
| | (0.007)*** | (0.007)*** | (0.011)*** | (0.006)*** | (0.010)*** | (0.006)*** |
| 2018 | -0.088 | -0.093 | -0.148 | -0.081 | -0.133 | -0.083 |
| | (0.007)*** | (0.007)*** | (0.010)*** | (0.006)*** | (0.010)*** | (0.006)*** |
| 2019 | -0.067 | -0.082 | -0.135 | -0.064 | -0.130 | -0.064 |
| | (0.007)*** | (0.007)*** | (0.010)*** | (0.006)*** | (0.010)*** | (0.006)*** |
| 2016 Mean | 0.255 | 0.251 | 0.357 | 0.239 | 0.375 | 0.226 |
| F-test | 14.28 | 13.10 | 2.93 | 19.28 | 5.36 | 18.32 |
| | [0.00] | [0.00] | [0.09] | [0.00] | [0.02] | [0.00] |
| N | 649410 | 651242 | 167925 | 1132727 | 225880 | 1074772 |

Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. F-test is for the hypothesis that the estimated effects for 2017 and 2019 are equal, p-values in square brackets. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14.

**Table 5: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of No Grade Received in a Course**

| | No Grade | Number of No Grades |
|---|---|---|
| N=1300201 | | |
| 2014 | 0.008<br>(0.001)*** | 0.022<br>(0.004)*** |
| 2015 | -0.001<br>(0.001) | -0.005<br>(0.004) |
| 2017 | -0.004<br>(0.001)*** | 0.003<br>(0.005) |
| 2018 | 0.001<br>(0.001) | 0.013<br>(0.005)** |
| 2019 | 0.002<br>(0.001) | 0.008<br>(0.005)* |
| 2016 Mean | 0.029 | 0.068 |

Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. Number of no grades includes 0s.

**Table 6: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of No Grade Received in a Course, by grade**

| | Grade 10 | Grade 11 | Grade 12 | Grade 13 |
|---|---|---|---|---|
| No grade N=1300201 | | | | |
| 2014 | -0.005<br>(0.001)*** | 0.005<br>(0.001)*** | 0.009<br>(0.002)*** | 0.012<br>(0.002)*** |
| 2015 | 0.002<br>(0.001)* | -0.002<br>(0.001) | -0.001<br>(0.002) | -0.001<br>(0.002) |
| 2017 | 0.003<br>(0.001)** | -0.001<br>(0.002) | -0.004<br>(0.002)* | -0.008<br>(0.002)*** |
| 2018 | 0.003<br>(0.001)** | 0.001<br>(0.002) | -0.001<br>(0.002) | -0.003<br>(0.002) |
| 2019 | 0.003<br>(0.001)* | 0.001<br>(0.002) | -0.003<br>(0.002) | -0.004<br>(0.002)* |
| 2016 Mean | 0.024 | 0.021 | 0.031 | 0.036 |
| Number of No Grades N=1300201 | | | | |
| 2014 | -0.014<br>(0.003) | 0.015<br>(0.005)*** | 0.021<br>(0.005)*** | 0.035<br>(0.006)*** |
| 2015 | 0.009<br>(0.003) | -0.008<br>(0.004) | -0.003<br>(0.005) | -0.002<br>0.006) |
| 2017 | 0.010<br>(0.003) | 0.005<br>(0.005) | 0.004<br>(0.006) | -0.001<br>(0.007) |
| 2018 | 0.009<br>(0.003) | 0.011<br>(0.006) | 0.012<br>(0.006)* | 0.018<br>(0.007)** |
| 2019 | 0.008<br>(0.003) | 0.009<br>(0.005) | 0.005<br>(0.005) | 0.012<br>(0.007) |
| 2016 Mean | 0.055 | 0.050 | 0.069 | 0.091 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. Number of no grades includes 0s.

**Table 7: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of No Grade Received in a Course by Selected Demographic Characteristics**

| | Females | Males | Immigrants | Native Born | Low income | Non-low income |
|---|---|---|---|---|---|---|
| No Grade | | | | | | |
| 2014 | 0.007 | 0.010 | 0.0025 | 0.005 | 0.022 | 0.005 |
| | (0.002)*** | (0.002)*** | (0.006)*** | (0.001)*** | (0.005)*** | (0.001)*** |
| 2015 | -0.000 | -0.002 | 0.006 | -0.003 | 0.006 | -0.003 |
| | (0.002) | (0.002) | (0.006) | (0.001)** | (0.005) | (0.001)** |
| 2017 | -0.004 | -0.003 | 0.016 | -0.006 | -0.007 | -0.005 |
| | (0.002)** | (0.002)* | (0.006)*** | (0.001)*** | (0.005) | (0.001)*** |
| 2018 | -0.003 | 0.002 | 0.021 | -0.003 | 0.002 | -0.002 |
| | (0.002) | (0.002) | (0.006)*** | (0.001)** | (0.005) | (0.001)** |
| 2019 | -0.002 | -0.001 | 0.019 | -0.004 | -0.001 | -0.003 |
| | (0.002) | (0.002) | (0.006)*** | (0.001)** | (0.006) | (0.001)** |
| 2016 Mean | 0.025 | 0.032 | 0.050 | 0.026 | 0.058 | 0.022 |
| Number of No Grades | | | | | | |
| 2014 | 0.019 | 0.025 | 0.066 | 0.015 | 0.053 | 0.015 |
| | (0.005)*** | (0.005)*** | (0.016)*** | (0.004)*** | (0.014)*** | (0.003)*** |
| 2015 | -0.002 | -0.007 | 0.003 | -0.007 | 0.005 | -0.006 |
| | (0.005) | (0.006) | (0.015) | (0.004)* | (0.014) | (0.003)* |
| 2017 | -0.002 | 0.013 | 0.075 | -0.006 | 0.010 | -0.003 |
| | (0.005) | (0.006)*** | (0.015)*** | (0.004) | (0.015) | (0.004) |
| 2018 | -0.001 | 0.028 | 0.073 | 0.007 | 0.031 | 0.007 |
| | (0.006) | (0.007)*** | (0.021)*** | (0.004)* | (0.017)** | (0.004)* |
| 2019 | -0.001 | 0.018 | 0.073 | 0.001 | 0.027 | 0.004 |
| | (0.006) | (0.006)*** | (0.018)*** | (0.004) | (0.017)*** | (0.004) |
| 2016 Mean | 0.061 | 0.075 | 0.115 | 0.062 | 0.142 | 0.052 |
| N | 649268 | 650933 | 167861 | 1132340 | 225731 | 1074470 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. Number of no grades includes 0s.

**Table 8: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of Student Achievement**

| | End of Year Exams | Teacher Awarded GPA |
|---|---|---|
| 2014 | -0.080<br>(0.015)*** | 0.029<br>(0.008)*** |
| 2015 | -0.045<br>(0.013)*** | -0.006<br>(0.007) |
| 2017 | -0.009<br>(0.014) | 0.055<br>(0.007)*** |
| 2018 | 0.017<br>(0.015) | 0.045<br>(0.008)*** |
| 2019 | 0.133<br>(0.016)*** | 0.063<br>(0.009)*** |
| 2016 Mean | 3.313 | 4.050 |
| N | 606304 | 1293333 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

**Table 9: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of Student Achievement by Grade**

| | Grade 10 | Grade 11 | Grade 12 | Grade 13 |
|---|---|---|---|---|
| End of Year Exams N=606304 | | | | |
| 2014 | 0.035<br>(0.011)*** | 0.088<br>(0.047)* | -0.106<br>(0.022)*** | -0.078<br>(0.016)*** |
| 2015 | 0.036<br>(0.011)*** | 0.055<br>(0.046) | -0.087<br>(0.022)*** | -0.030<br>(0.014)** |
| 2017 | 0.090<br>(0.010)*** | 0.114<br>(0.044)*** | 0.042<br>(0.022)* | -0.044<br>(0.014)*** |
| 2018 | 0.104<br>(0.010)*** | 0.202<br>(0.044)*** | 0.012<br>(0.022)*** | 0.007<br>(0.015) |
| 2019 | 0.014<br>(0.011) | 0.453<br>(0.052)*** | 0.109<br>(0.023)*** | 0.123<br>(0.016)*** |
| 2016 Mean | 3.501 | 3.828 | 3.412 | 3.310 |
| Teacher Awarded GPA N=1293333 | | | | |
| 2014 | -0.080<br>(0.006)*** | 0.002<br>(0.010) | 0.041<br>(0.010)*** | 0.051<br>(0.010)*** |
| 2015 | -0.025<br>(0.005)*** | -0.019<br>(0.008)** | -0.003<br>(0.010) | 0.007<br>(0.010) |
| 2017 | 0.011<br>(0.005)** | 0.026<br>(0.009)*** | 0.064<br>(0.008)*** | 0.083<br>(0.010)*** |
| 2018 | 0.037<br>(0.006)*** | 0.017<br>(0.009)* | 0.051<br>(0.010)*** | 0.075<br>(0.011)*** |
| 2019 | 0.052<br>(0.006)*** | 0.033<br>(0.010)*** | 0.055<br>(0.010)*** | 0.115<br>(0.012)*** |
| 2016 Mean | 4.198 | 4.054 | 4.037 | 4.063 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

**Table 10: Difference-in-Differences Estimates of the Impact of the New Absence Policy on End of Year Exam Performance, by Selected Demographic Characteristics**

| | Females | Males | Immigrants | Native Born | Low income | Non-low income |
|---|---|---|---|---|---|---|
| 2014 | -0.059 | -0.097 | -0.020 | -0.087 | -0.048 | -0.085 |
| | (0.017)*** | (0.019)*** | (0.034) | (0.015)*** | (0.028)* | (0.015)*** |
| 2015 | -0.028 | -0.058 | -0.007 | -0.049 | -0.055 | -0.043 |
| | (0.016)* | (0.018)*** | (0.030) | (0.014)*** | (0.026)** | (0.014)*** |
| 2017 | 0.016 | -0.034 | 0.022 | -0.010 | -0.024 | -0.001 |
| | (0.016) | (0.018)* | (0.030) | (0.014) | (0.026) | (0.014) |
| 2018 | 0.051 | -0.016 | -0.037 | 0.029 | -0.052 | 0.038 |
| | (0.018)*** | (0.018) | (0.031) | (0.015)* | (0.028)* | (0.015)*** |
| 2019 | 0.178 | 0.084 | 0.126 | 0.137 | 0.121 | 0.145 |
| | (0.018)*** | (0.020)*** | (0.032)*** | (0.016)*** | (0.029)*** | (0.016)*** |
| 2016 Mean | 3.437 | 3.166 | 2.976 | 3.356 | 3.006 | 3.370 |
| N | 316890 | 289414 | 73416 | 532888 | 93648 | 512656 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

**Table 11: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher Awarded GPA, by Selected Demographic Characteristics**

| | Females | Males | Immigrants | Native Born | Low income | Non-low income |
|---|---|---|---|---|---|---|
| 2014 | 0.040 | 0.020 | 0.032 | 0.028 | 0.053 | 0.026 |
| | (0.010)*** | (0.010)* | (0.019) | (0.008)*** | (0.016)*** | (0.008)*** |
| 2015 | -0.008 | -0.002 | 0.006 | -0.008 | -0.006 | -0.005 |
| | (0.009) | (0.009) | (0.017) | (0.007)*** | (0.015) | (0.007) |
| 2017 | 0.054 | 0.058 | 0.075 | 0.053 | 0.084 | 0.058 |
| | (0.009)*** | (0.009)*** | (0.018)*** | (0.007)*** | (0.015)*** | (0.007)*** |
| 2018 | 0.058 | 0.033 | 0.044 | 0.046 | 0.038 | 0.054 |
| | (0.010)*** | (0.010)*** | (0.018)*** | (0.008)*** | (0.016)*** | (0.008)*** |
| 2019 | 0.065 | 0.061 | 0.058 | 0.066 | 0.058 | 0.073 |
| | (0.011)*** | (0.012)*** | (0.020)*** | (0.009)*** | (0.017)*** | (0.009)*** |
| 2016 Mean | 4.198 | 3.902 | 3.670 | 4.103 | 3.678 | 4.133 |
| N | 645790 | 647543 | 166285 | 1127048 | 223355 | 1069978 |

Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

## Appendix

**Table A1: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher awarded GPA with "Results" for No-grade Students Included**

| | Teacher Awarded GPA |
|---|---|
| 2014 | 0.024<br>(0.008)*** |
| 2015 | -0.006<br>(0.007) |
| 2017 | 0.048<br>(0.007)*** |
| 2018 | 0.032<br>(0.008)*** |
| 2019 | 0.055<br>(0.009)*** |
| 2016 Mean | 4.021 |
| N | 1297090 |

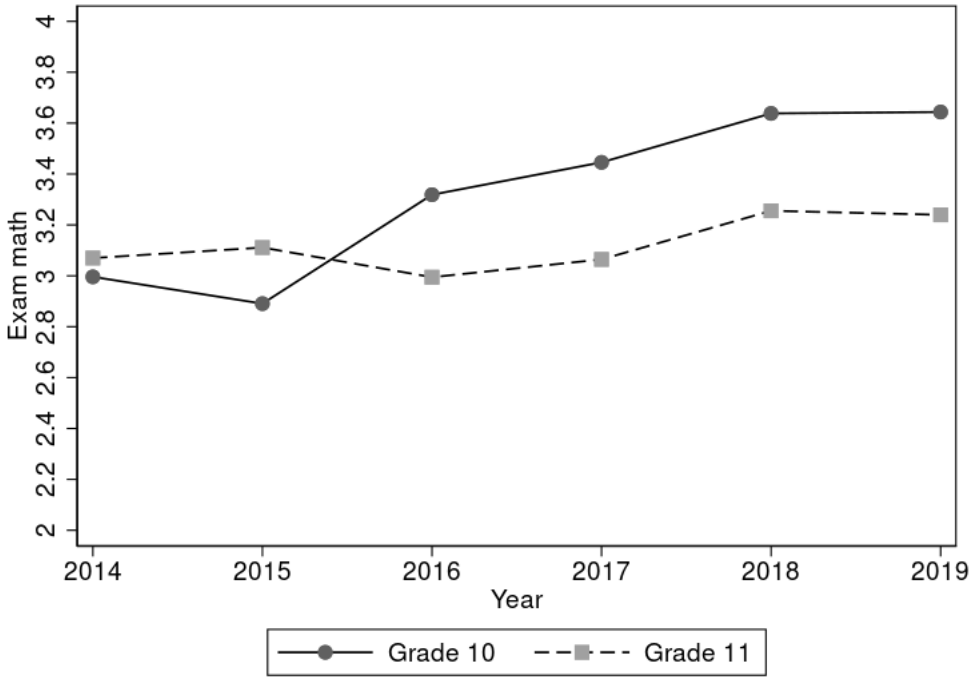
Notes: Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

**Table A2: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Various Measures of Student Achievement, Academic track**

| | End of Year Exams | Teacher Awarded GPA |
|---|---|---|
| 2014 | -0.086<br>(0.015)*** | 0.024<br>(0.008)*** |
| 2015 | -0.059<br>(0.013)*** | -0.012<br>(0.007) |
| 2017 | -0.009<br>(0.014) | 0.048<br>(0.007)*** |
| 2018 | 0.027<br>(0.015)* | 0.039<br>(0.009)*** |
| 2019 | 0.130<br>(0.016)*** | 0.048<br>(0.010)*** |
| 2016 Mean | 3.34 | 4.175 |
| N | 558 144 | 940 663 |

Notes: Notes: Notes: Authors' calculations from Norwegian Registry data. The additional controls include year, grade, age, gender, field of study, family income, mother's education, indicators for immigrants and mother's continent of origin (immigrants only) and school fixed effects. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses.

**Figure A1: Average Score on Externally Graded Exams in Math, Grades 10 and 11, 2014-2019**




Notes: Authors' calculations from Norwegian Registry data