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ABSTRACT

Starting in the 2016/17 academic year, high school students in Norway who missed more than 10 percent of hours in a given course without a medical excuse could not receive a final grade. Across all students, the new policy reduced total absence by 21-28 percent, and chronic absence by 29-39 percent. This behavioral response was largely sufficient to avoid the academic penalty for absence over the 10 percent threshold. We also find the policy had a positive impact on teacher awarded GPA for groups with larger baseline absence and greater absolute changes in attendance.

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1 Introduction

School absenteeism has long been a concern, but the COVID-19 pandemic nudged it to the top of the educational agenda in many countries.[1] At the height of the pandemic, the large learning loss due to homeschooling was arguably the greatest concern, but as public health in many countries has improved, school absence persists. Across institutional settings, students struggle to get back into the routine of regularly attending school. In the UK, the share of secondary students persistently absent, i.e., missing 10 percent or more of school sessions, jumped from 13.7 percent to 26.5 percent between 2018/19 and 2022/23, while those "severely" absent (missing 50 percent or more of school sessions) rose from 1.3 percent to 3.4 percent.[2] Dee (2024) reports that "chronic" absenteeism (missing 10% or more of school days) in U.S. public schools rose 91 percent between 2018/19 and 2021/22, growing from 14.8 percent to 28.3 percent. Across the OECD increasing rates of student absenteeism post COVID-19 have been identified as a development of significant concern.[3]

The persistent high levels of student absence post pandemic are poorly understood. More traditionally, the reasons for students being absent range from illness through avoiding a threatening school environment to a general lack of interest in school. Policies to address truancy include measures to address the structural causes of absence and 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 attempting to address root causes may be delivered in one-on-one settings by trained professionals.

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[1] See for example, https://www.economist.com/united-states/2023/08/24/post-covid-american-children-are-still-missing-far-too-much-school

[2] See https://researchbriefings.files.parliament.uk/documents/CBP-9710/CBP-9710.pdf.

[3] See https://www.oecd-ilibrary.org/docserver/a38f74b2-en.pdf?expires=1717795595&id=id&accname=guest&checksum=3F22740FF63FF19058A2A8A79CF8FE50.

In this paper, we seek to expand our understanding of one approach to student absenteeism: a penalty scheme intended to encourage attendance in high school. We contribute to the literature by providing, to our knowledge, the first evaluation of a national, system wide program to directly address high school truancy. While this policy was introduced prior to the pandemic, we argue that insights about its effectiveness and consequences can inform the debate of how to address school absence in the post-pandemic era.

The application is to the Norwegian high school system where chronic absenteeism in the high school grades pre policy was almost 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. Evaluating this policy innovation offers a number of advantages. First, we have access to data that captures students' absences, academic outcomes, grade progression, record both full day and part day absence and 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 comparison group for the analysis. We employ an event study difference-in-differences strategy to estimate effects on student absence, chronic absenteeism, penalties and student achievement following the reform.

Our study is related to two strands of research on student absenteeism: a) mechanisms to increase school attendance, and b) whether school attendance matters.

Regarding mechanisms to increase school attendance, Singer (2024) identifies three common approaches. The first is communication strategies to inform parents of their children's attendance and/or the importance of minimizing absences, which overall lead to reductions in absenteeism ranging from substantial to small (e.g., Bergman & Chan, 2021, Robinson et al., 2018; Swanson, 2022). More intensive communication leads to larger increases in attendance, but at greater cost (e.g., Stemler et al. 2022). The second attempts to improve school culture, but there is mixed empirical evidence whether these approaches work (e.g., Hamlin 2020, Jackson 2018, Liu and Loeb 2021). The third approach is to provide students resources to overcome barriers to attendance[4], which has shown some success, but requires significant funding (Kearney et al. 2023).

The policy we evaluate is a form of incentive that might be placed within the school culture approach. Research on the use of in school awards, cash payments etc. for good attendance appear to face mixed success (e.g., Balu, and Ehrlich, 2018, Robinson et al. 2021). Incentives further afield include "no pass, no drive" laws passed in some US states, which have led to consequent reductions in absenteeism (Barua and Vidal-Fernandez, 2014).

The other relevant strand of literature attempts to establish a causal link between school attendance and student success (see e.g., Jacob and Lovett 2017). Studies using student or family fixed effects (e.g., Martins and Walker 2005, Stanca 2006, Gottfried 2011, Cattan et al. 2023, Liu et al 2021) suggest a positive link between attendance and student performance, although Arulampalam et al. (2012) argue that this only holds for high performing students. Gottfried (2010), using distance to school to instrument absence, supports a causal negative relationship between absence and achievement. Two recent examples, Cattan et al. (2023) who adopt a

[4] e.g., transportation, connections to social services

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.[5] Also relevant here are studies of events such as snow days (e.g., Goodman 2014), flu outbreaks (e.g., Aucejo and Romano 2016) or teacher strikes (e.g., Baker 2013), interpreted as a shock to student attendance, that generally find a positive relationship between attendance and achievement. Finally, experimental studies (e.g., Marburger 2006, Chen and Lin 2008, Dobkin et al. 2009, Arulampalam et al. 2012) typically find that increased attendance leads to better outcomes on the course final exam. Attempting to address the more structural causes of low attendance is Guryan et al.'s (2020) evaluation of the Check and Connect mentoring program, indicating reduced absence but little effect on achievement in grades 1-4 for disadvantaged children.[6]

Against this background the Norwegian policy stands out for its simplicity and use of a significant penalty to encourage compliance. Penalties do not appear to be a preferred option of policy makers and researchers in this area. However, in a recent summary of the literature Robinson et al. (2018) conclude "…there are only a handful of successful experimentally-proven programs that reduce absenteeism and are scalable…" (e.g., Rogers and Feller, 2018; Robinson, Lee, Dearing, & Rogers, 2018; Guryan, et al., 2021), and so the evaluation of this big-scale, low-cost program can make an important contribution to the literature.

Our strongest evidence is that the policy had the intended first order impact. Once the policy was in place, total student absence fell by 21-28 percent, and chronic absence fell by 29-

[5] Cattan et al. (2023) also present direct evidence of the impact of this absence on adult income.
[6] Instituted monitoring and support systems have also been found to reduce absenteeism (e.g., Faria et al. 2017) in more targeted RCTs.

39 percent. In the upper high school grades of the academic track, the initial impact of the policy is larger than the impact in subsequent years, suggesting that students may have adjusted their absence to the new rules over time, perhaps by strategically spreading it over different courses. In the vocational track, where baseline absence rates are higher, the impact is generally larger and persistent over time. For example, chronic absenteeism of students from immigrant households in this track fell by almost 50 percent. We also provide clear evidence that these adjustments in behavior were largely sufficient to avoid the penalties under the new law; we find little evidence of an 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 present evidence suggesting the new policy positively affected teacher awarded grades for students who exhibited the largest adjustments in attendance.

Our study provides at least two policy-relevant findings. First, a simple penalty system can significantly reduce high school absenteeism. Importantly, the effect is large and pronounced for demographic groups with high levels of baseline truancy. This finding adds to the limited stock of evidence for effective, at scale, low cost educational interventions. Second, the reduction in absenteeism was achieved without excessive imposition of academic penalties—the new penalties under the law were largely effective in their threat rather than their application. We argue that this finding enhances the appeal of the new rule, as it implies that the increase in attendance was achieved without unduly punishing student groups who were already struggling in school. This is, perhaps, an unexpected finding for a "one size fits all" policy.

These results are based on a policy that was put in place prior to the pandemic, and hence we cautiously interpret its relevance to the post pandemic rise in student absenteeism. In all likelihood, the post-pandemic truancy will require a variety of policies and approaches.

However, to the extent that current absenteeism transcends traditional student markers of this behavior, universal approaches, such as the Norwegian initiative we study, may be relevant. Importantly, remediation of the learning loss caused by the pandemic is likely to take place in schools. Therefore, efforts to address the post pandemic increase in student absenteeism should be part and parcel with the supplementary learning resources needed to address this deficit.

2 Institutional Setting

2.1 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 11th grade, the dropout rate in high school is approximately 25 percent.[7] The vast majority of high schools in Norway are public, with private schools enrolling about 8 percent of students (Statistics Norway 2021).[8] The public high schools are administered at the county level.

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

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

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.[9] We omit these, small in number, grade 13 vocational track students from the analysis to facilitate analyses by track.

2.2 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 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.

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

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.[10] There were certain exceptions, such as longer medical absences, 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.).

3 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 (partial day) and full 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.

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. In this window, across the three grades we observe cohorts entering high school in the years 2011/12 through 2018/19. 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 lower secondary education for all high school students in our sample. Note that not all cohorts are observed for all their high school years. In our data window, we only observe the

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

cohort entering high school in 2011/12 in grade 13, while we only observe the cohort entering in 2018/19 in grade 10.

Schools register full days and hours of absence separately. We construct a variable that sums up total hours the student has been absent.[11] We also construct a measure of chronic absenteeism, defined as being absent the equivalent of 15 full days or more.[12]

The data record both documented and undocumented absence and thus mirrors actual presence at school. We cannot distinguish between documented absence (due to doctor-certified medical/health reasons, funerals, religious holidays etc.) and undocumented absence (absence without a documented reason). Since the new policy introduced sanctions based on undocumented absence, it provided incentives to get absence certified by a doctor and we might expect documented absence to increase. If our data only included undocumented absence, we would wrongly interpret this relabelling of absences as an increase in student attendance. Importantly, our absence variable captures both types of absence.

This said, our record of absence is potentially subject to one particular type of measurement error. Students can request that up to ten full days of absence per year be erased from their record (from the variable we observe as absence) due to documented health reasons, if those conditions last for more than three consecutive days, or if the absence is due to certain student activities.[13] For absence due to health reasons, days of a spell in excess of 3 could be erased. This regulation was in place both before and after the reform.

[11] 1 day=6 hours

[12] 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.

[13] This is specified in §3-47 Føring av fråvær i vidaregåande opplæring (Registration of absence in high school), which was sent to all the schools from the Directorate of Education in 2010 (Udir-1-2010): "A student can demand that up to 10 school days in an academic year is not registered on the Diploma if it can be documented that the

We do not observe whether days of absence have been erased from a student's record. Erasure for student activities should only be relevant for a small number of students. However, deletion for medical reasons could pose a threat to the accuracy of our absence records if it were more likely post reform. Note this is a concern for long spells, as the first three full days of an absence for health reasons will be captured in the data both before and after the reform.

There were strong incentives to request allowable deductions of absence for health reasons before and after the policy was introduced. High absence registered on the diploma could be costly for a student for reasons unrelated to the reform. First, it could affect the likelihood of getting a grade, as this was up to the teacher to decide prior to the new policy. Second, it reflected poorly on job applicants to have high recorded absence on their diploma. Thus, for longer sickness spells (again, the first three days could not be deducted from the diploma), we would expect that students would see their doctor regardless of the absence regulation (i.e. also before the reform).

Ultimately, we cannot rule out that the amount of absence erased for health reasons reacted to the new policy. Only full days of absence could be erased for sickness however, so the measure of hours of (partial day) absence should be unaffected. We therefore compare the impact of the new policy on hours and days of absence separately to provide some perspective on this issue.

We also examine the incidence of the penalties under the new law. To study the probability of not receiving a final mark, we a 0/1 indicator whether a student did not receive a

absence is due to: a) Health and welfare reasons b) Work as an elected representative (i.e. for student council or similar) c) Political work d) Volunteering e) Legally obligatory meetings and f) Representation on a national or international level. To deduct absence due to reason a) it needs to last more than three days, and only absence from day 4 can be deducted." The entire translated § is available from the authors upon request.

final mark in at least one course, by year. As further evidence, in the appendix we investigate the intensive margin using a measure counting the number of no grade events in a year including the zeros.

Finally, we use students' annual average of teacher awarded grades (henceforth "GPA") to study the impact of the new law on student achievement. Some argue externally graded evaluations are preferred to grades awarded by a student's teacher as a measure of student achievement.[14] The fear is that the teacher could be affected by the treatment, 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. This said, logically the same effect might also be at work for the externally graded tests. 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.

In their favor, teacher awarded grades can be viewed as capturing a wider span of student attributes and achievement and therefore provide a more comprehensive picture of the impact of the new attendance law. We also note that the teacher awarded grades in the Norwegian

[14] Norwegian grade 10 and high school students write centrally administered, externally graded end of year exams, but their structure complicates a straightforward analysis. First, some exams are oral and therefore not standardized in delivery or evaluation across students. Second, the subject of many exams is by random draw at the end of the school year, and so exam schedules may have an important impact on student performance (see Bensnes 2020). Third, the incidence of many of the exams is also by random draw, with as little as 20 percent of students in a given grade writing, which can lead to small sample sizes. Finally, the subjects of exams vary by grade, limiting comparability between the treatment (high school students) and control (grade 10 students) groups, and further limiting sample sizes.

educational system are "high stakes" for students who plan to proceed to post-secondary education. If there is excess demand for a post-secondary course of study, places are filled based on an application score derived from high school GPA (Kirkebøen et al. 2016).

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 17. We exclude observations with negative absence[15] 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[16] families is quite constant over the period, as

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

[16] 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, the sample size is quite stable over the sample years.[17]

The new policy potentially affects the composition of our sample over time if the new rules led some students to not 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 through "graduation" from high school (4 years later) by the year they enter high school. We henceforth adopt the convention of identifying school years by their end—rather than their start—year, so the first cohort of grade 10s we observe entering high school enters in 2014 (i.e., 2013/14). The fourth point of a line for given cohort (i.e. the "fourth" year) reveals the proportion who have not graduated four years after entering high school. The proportions of grade 10 students who are observed in grade 11 are relatively stable 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. After 4 years, 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.

4 Empirical Strategy

Our primary empirical strategy is an event study, difference-in-differences framework, in which we use students in grade 10 as a control group. Using the sample of grade 10 and high school students, our base difference-in-differences specification is:

[17] 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),

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

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. The new law comes into effect in 2017 (recall that we identify school years by their end year), 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. The estimates for 2014 and 2015 provide evidence of any pre-event differences between the treatment and control groups. The estimates for the years 2017, 2018 and 2019 provide evidence of the impact of the new policy.

Grade 10 students serve as the control group in this empirical framework. For the analysis of student absence, grade 10s are presumably exposed to similar viral infections and weather 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 comparison, 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 specific confounders that might undermine this identification approach. Also, we overview each of results graphically and our specification will highlight any violation of common trends in the pre policy period, so to make clear the

contribution of the grade 10 controls in each step of the analysis and better elucidate any possible biases.

Identification in this specification includes a common trends assumption and a no anticipation condition. Namely, that there are no shocks correlated with the advent of the new absence policy that differentially affect the outcomes in the high school grades and in grade 10. While we do find some evidence of differential pre trends, we visually demonstrate that the policy effects underlying our conclusions are a magnitude larger than any pre trends in our data, and we formally evaluate their importance using the methods of Rambachan and Roth (2023).

To shed more light of on the impact of the control group on our inference we also present estimates for absence from a fixed effects estimator. In this case we estimate the equation

$$o_{it} = \mu(t) \cdot yr_t + \sigma X + \phi_i + \varepsilon_{it}, \quad (2)$$

where yr_t is a set of single year dummy variables for 2015-2019, omitting 2018, *X* includes full sets of grade and field of study fixed effect and ϕ_i is an individual fixed effect.

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.

5 Results

5.1 Absence

We begin our analysis with the first order target of the new law—the amount of time students miss class. An overview is provided in figure 2 where we graph total hours of 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. We observe both of these effects in figure 2. In the years prior to the reform (pre 2017) the difference in total absence 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. 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.

As expected, the time pattern in total absence for grade 10 tells a different story. 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 a modest downward trend consistent with the pattern in the pre reform years. Note also, however, that grade 10 absence displays a larger decline between 2014 and 2015 compared to the other grades. This difference between grade 10 and grades 11-13 will show up in the regression estimates and is evidence against a strict requirement of parallel trends. As noted above, we have scanned for a grade 10 event in 2015/14 that might account for this difference but have found none. In interpreting our results, we attempt to implicitly account for this anomaly by looking for the estimate of the treatment effects to be "well" in excess of the estimate of the interaction of *HS* and $\lambda(2014)$. We also more formally evaluate the impact of this deviation from parallel trends on our inference using the methods of Rambachan and Roth (2023).

In figure 3 we report estimates of $\lambda(t)$ from (1), and their 95% confidence intervals, for total absence broken down into its components full days and hours—partial days—of absence, and for chronic absence. The underlying parameter estimates are reported in the top panel of

table 2. Note that the policy comes into effect in 2017 and we use the normalization $\lambda(2016) = 0$. Average pre reform (2016) hours of absence are 20.6 (first column of table 2), the equivalent of almost 3.5 full days. The estimates of $\lambda(t)$ for hours post reform—2017-2019—are all negative, statistically significant. Relative to the 2016 mean they indicate reductions in part day absence ranging from 18 to almost 30 percent. Average total full days of absence in pre-reform year is just over 8 days (second column of table 2). Again, the post policy estimates of $\lambda(t)$ are all negative, statistically significant and range from just over, to just under, 2 days. Relative to the 2016 mean they represent reductions of 22 to 29 percent.

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 vary by the academic and vocational tracks and by specialization within tracks. For students pursuing general studies within the academic track for science, math, languages, social science and economics, annual instruction hours are 840 hours per year.[18] Against this standard, in 2016 average total absence (8.2 x 6 hours + 20.6 hours = 69.8 hours) was in excess of 8 percent of total instructional hours.

There are both similarities and differences in the results for full and partial day absence. First, the proportionate reductions in the two measures of absence are very similar, as is the tapering of the policy effect over time. With regards to the latter, in table 2 we report the result of a test of the hypothesis that the estimates for 2017 and 2019 are equal. It is rejected in both cases. Recall that the measure of full day absence is potentially biased if students were more likely to remove health related absence from their records after the reform. In this case we

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

would expect our estimate for full days of absence to be an upper bound. It is interesting, therefore, that the proportional reductions of both full and partial absence are very similar and that both exhibit a decreasing policy impact. 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.[19] As documented below this evidence of "student learning" is primarily a characteristic of students in the academic track.

Second, consistent with the evidence in figure 2, for full days we obtain a negative and statistically significant estimate for 2014 which is roughly one-half the initial policy impact in 2017. In contrast, the estimate for 2014 for partial days of absence, while statistically significant, is much smaller in proportional terms relative to the initial policy (2017) impact (roughly 11 percent). Therefore, the deviation in pre trends observable in figure 2 is primarily a result of the full day component, as is apparent in figure 3.

In the final panel of Figure 3 and the last column of table 2 are the estimates for chronic absence. In the year prior to the reform year, the chronic absence rate is just under 25 percent. 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 track. 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 has a substantial impact on excessive absence by this measure. The estimates indicate a reduction in chronic absence ranging from 7 to 10 percentage points or 29 to 39 percent. The impact of the law again diminishes monotonically over time and we can reject the hypothesis that the estimates for 2017 and 2019 are equal

[19] Unfortunately, we do not have a record of absence by courses to examine how the distribution of absence across courses changes with the new policy.

In the second panel of table 2 we present fixed effects estimates of the impact of the policy (equation (2)). Note that 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 estimates imply that while the control group leads to marginally larger estimates in most cases, the message of the analysis remains the same.

We next conduct the analysis at a more granular level presenting estimates by grade, by track and by demographic groupings. Knowledge of high school rules and systems clearly vary by grade, as do the stakes of the new policy. By splitting the analysis by track, we potentially compromise the validity of our control group (grade 10) which combines students who will eventually be in the different tracks. However, as documented below there are interesting differences in the impacts of the law across the two tracks, as well as in baseline rates of absence. Finally, a potential concern with a Norway's uniform policy is that it could disproportionately impact groups with high pre existing levels of absence, which we identify in our demographic splits.

Going forward we focus on our measure of chronic absence, which is often used as a barometer of the severity of truancy, as a policy relevant summary.[20] In addition, we present the estimates graphically to more effectively demonstrate heterogeneity in the estimates and to conserve space, but we report the underlying parameter estimates in the appendix.

As reported in table 3, the baseline rates of chronic absence vary significantly by grade and track. In the academic track, in grade 13 the rate is almost double that in grade 11. There is less variation by grade in the vocational track, but the levels are substantially higher than in the

[20] Full results for hours and days of absence separately for our demographic splits are reported in appendix tables A3 and A4.

academic track. In figure 4 we present estimates of grade specific policy effects by track. We modify equation (1) by replacing $\lambda(t) \cdot HS_{it} \cdot yr_t$ with a full set of interactions between the grade fixed effects and the year fixed effects. The underlying parameter estimates are reported in table A1. In the academic track, the estimated impacts of the policy vary in absolute value by grade, by 2019 ranging from 3.3 percentage points in grade 11 to 7.6 percentage points in grade 13. Note that the estimated policy effects for grade 11 are not much larger than the estimate of the corresponding deviation in pre-policy trends in 2014/15. The attenuation of the impact over time varies substantially by grade: the estimates for grade 11 do not exhibit any trend and we cannot reject the hypothesis that the 2017 and 2019 estimates are the same, while the estimates for grades 12 and 13 decline monotonically over time and we easily reject the across years restriction. An implication is that the initial (2017) impact of the law is much larger in proportionate terms in the higher grades, but by 2019 is close to similar across grades. If the attenuation of the law's impact over time reflects student learning, then this pattern indicates that high school experience is an important facilitator of this effect.

In the vocational track the estimated impacts of the law are large in both absolute and proportionate terms for each grade, and persistent over time. They are also consistently much larger than the estimated deviations in pre trends for 2014. As was the case for the academic track, we cannot reject the hypothesis that the estimates for 2017 and 2019 are the same for grade 11 students. New here, is that while the estimated impact of the law for grade 12 diminishes with time, the decline at less than 2 percentage points is quite modest. By 2019, the estimated impact of the new law is almost 40 percent of the baseline rate to be compared to an impact of 21 percent for grade 12s in the academic track. This difference across tracks suggests that the

diminution of the law's impact over time in the pooled sample is driven by students in the academic track.

The results thus far indicate that the new law led to widespread reductions in chronic absence. The estimates imply that by 2019 average chronic absence was 17 percent or less in all grades save 13 in the academic track. However, a criticism of uniform, system wide policies to address absence is that they may have disparate impacts by demographic group, which negatively affect those with higher rates of absence. We next split the students by various characteristics, by track, to investigate this issue.

In table 3, we document stark differences in baseline chronic absence rates. Higher rates of chronic absence are observed for students from low income and immigrant households, and the differences are especially pronounced in the vocational track where rates are in excess of 40 percent. Also note that the gender difference reverses with track: the baseline chronic absence rate is lower for girls in the academic track but higher in the vocational track.

The estimated policy impacts for these groups are reported in figure 5 (underlying parameter estimates are reported in table A2). In the academic track there is a clear impact of the policy on absence rates for each group. In general, the absolute effects are larger for the students with higher baseline rates—boys, and those from immigrant or low income households. There is also attenuation of the estimated policy impact for all groups over time. In the split by gender it is reasonably similar over time. However, in the split by immigrant/native born households the attenuation of the policy effect over time is more pronounced among the native born which leads to a narrowing of the absence gap between the two groups: it is 40 percent in the baseline rates but 17 percent by 2019. A similar pattern is observed in the household income split, with greater

attenuation of the policy impact for the higher income group leading to a narrowing of the difference in the estimated rates of chronic absence between the two groups by 2019.

The results for the vocational track in the second panel tell a somewhat different story. Again, the groups with the higher baseline rates exhibit the larger absolute impacts of the law: girls and those from immigrant or low income households. What is new here is much less evidence of an attenuation in the policy effect. At the 5 percent level, we can only reject the hypothesis that the 2017 and 2019 estimates are equal for students from native born households.

The results for students from immigrant and low income households are a noteworthy impact of the new policy because these groups have high baseline rates of chronic absence. For example, we estimate that chronic absence is reduced by almost 50 percent for students from immigrant households in the vocational track, and this impact persists very strongly over time. As perspective on whether this reflects more erasure of health related absence post policy by these groups, in figures A1 and A2 of the appendix (the underlying estimates are in tables A3 and A4) we present the results separately for hours (partial days) and full days of absence by our demographic splits. Keeping in mind that a potential bias from erasing health related absence would not affect the estimates for hours, we take the substantial, persistent reductions in hours absent in these figures, for students from immigrant and low income households, as clear evidence that the reform increased school presence for these students. We also note the similarity of the time pattern of the estimates for partial and full days of absence for these groups.

These results document that the new policy had its first order intended effect. There are large reductions in absence across grades, tracks and demographic groups with the advent of the

new policy. Therefore, this relatively simple penalty scheme appears to have been an effective antidote, at scale, for high school absence.

5.2 Penalties

We next investigate whether the penalty under the new law disproportionately affected certain groups of students. While we document a significant behavioral response to the new absence policy, it may have been accompanied by an uptick in the students not receiving a grade in a course. This is possible if the new penalties under the law catch up students who could not, or would not, change their absence behavior. However, given the magnitude of our estimates of the impact of the law on absence, the behavioral response may have been sufficient to actually lower the rates of no grade in a course. Recall that prior to the new law 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 evaluation.

Receiving a no grade is a relatively rare event affecting roughly 1 to 6 percent of students across grades and demographic groups and exhibiting considerable variability across years. In Figure 6, we report the estimates of the impact of the new policy on the incidence of a no grade, by grade, by track (see also table A5). In neither track do we observe any systematic impact of the policy. We might have expected to see a positive trend break for the high school students starting in 2017, but instead the estimates post policy are either zero or negative.

Perhaps of greater interest are the estimates by our demographic groups, by track. Here we can investigate whether the penalties under the new policy disproportionately affected groups with higher rates of pre-policy absenteeism. The estimates are reported in Figure 7 (and table A6). For most groups in both tracks the inference is very similar to that from figure 6. There is little evidence of an uptick in the incidence of no grades with the advent of the new policy.

Perhaps harder to read are the graphs for students from the immigrant households. Here, for the first time, we observe post policy estimates that are systematically positive and in the academic track statistically significant. That said, in neither track are the post policy estimates distinct in magnitude from the deviation in the pre trend in 2014. Furthermore, they appear to be as much from variation in the incidence of no grades among grade 10 students from immigrant households than from an effect of the policy. In figure A3 of the appendix we graph the time series of the no grade rate, by track, for grade 10 and for high school students, separately in immigrant and native born households. For the academic track, in immigrant households the grade 10 and high school grades no grade rates trend in different directions in the pre policy period. Both change direction in 2017 and then level off. Therefore, the positive estimates for the post policy period, evident in Figure 7, are as much due to the downturn in the grade 10 no grade rate than to the uptick in high school no grade rate. In the vocational track we observe a similar pattern. The grade 10 and high school grade rates exhibit divergent trends pre policy but converge in 2017. Overall, we do not interpret this as compelling evidence that the no absence policy led to a significant increase in no grades among students from immigrant households.

We have also examined the impact of the new policy on the number of no grade events, which include both the extensive and intensive margins. The results by grade, reported in appendix table A7, also do not reveal any systematic impact of the policy.

In summary, there is little evidence of a large systematic effect of the policy on the incidence of no grades, as we see for 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. Of important note is that we do not observe an uptick in the incidence of the penalty in groups that had high baseline rates of absenteeism.

5.3 Student Achievement

The new absence policy increased the amount of time students attended class. However, concerns about high levels of truancy are often connected to its suspected impact on academic achievement. In figure 8 we present our estimates of the impact of the new policy on teacher awarded GPA (see also table A8). We standardize GPA so the estimates can be read in standard deviations. In the academic track the estimates for grade 11 and 12 do not provide strong evidence of an impact of the new policy. The post policy estimates are never much larger than the deviation in pre trends in 2014, and by 2019 are small and statistically insignificant for both grades. In contrast the estimates for grade 13 reveal a policy effect that grows over time to over 10 precent of a standard deviation. This conclusion is tempered by the deviation in pre-trends in 2014 but a discrete impact of the new law in 2017 is also evident.

The results for the vocational track indicate impacts of the law for both grade 11 and 12 students. The inference is clearer for grade 11, as there is less evidence of pre trends. The results for grade 12 indicate a larger impact of again almost 10 precent of a standard deviation, but this evidence is tempered by the estimate for 2014.

Note that the imposition of penalties (i.e., no grade) under the new policy might mechanically raise GPA if their incidence was higher in the lower part of the mark distribution. While we find little evidence of an increase of no grades under the new policy, in Table A9 of the appendix we report estimates from a sample in which we include students receiving no grade in a course, awarding them a grade of zero. The inference from this table is consistent with our conclusions from figure 8.

The estimates by demographic splits of the data are reported in figure 9 (and table A10). In the academic track, there is evidence of a positive impact of the new law on students' GPA,

which is tempered by the evidence of pre trends for most groups in 2014. The one anomalous result is for students from immigrant households for whom there is no persistent evidence of an effect. The story is different for the vocational track. First, the estimated policy impacts are generally larger than the estimated breaks in parallel trends in 2014. Second, perhaps the clearest evidence of a positive impact of the new law is in the immigrant/native born split of the data. The estimated impact for students from immigrant households, who had some of the highest rates of absenteeism pre policy, is 10+ per cent of a standard deviation, and persistent over time. It is also larger than the estimated impact for students from native-born households suggesting the new law helped close the GPA gap between these two groups of students, which at baseline was over 0.4 grade points, or more than 40 per cent of a standard deviation.

5.4 Pre Trends

Throughout the analysis we have noted a deviation from common pre trends for many outcomes between 2014 and 2015. The fact that it is observed in the same year for the different outcomes suggests some unobserved shock to grade 10 results which we have been unable to identify. As is evident in, for example, figure 2, the time series for grade 10 outcomes otherwise exhibit relatively minimal year to year variation, conditional on a trend. In attributing causality to the different estimated impacts, we have looked for the treatment estimates to be "well" in excess of the estimate for we observe for 2014 in the pre treatment period.

To more formally evaluate this intuition we follow the method suggested by Rambachan and Roth (2023). This involves constructing confidence intervals for specific estimated treatment effects which account for post treatment deviations in common trends as multiples of the maximum deviation from common trend observed in the pre treatment period.

Figure 10 reports the confidence intervals for the estimated 2017 treatment effects reported in figure 4. $\bar{M}$ denotes the multiple of the maximal pre trend. The original confidence interval is reported in blue corresponding to $\bar{M} = 0$. For example, we observe a "breakdown value", the value of $\bar{M}$ for which the estimated confidence interval spans 0, of about one for the estimated treatment effect for grade 11 chronic absence in the academic stream: it is robust to pre trends roughly less than one times the maximal pre trend observed in that outcome. This grade 11 result is the "worse case" scenario. The estimates for the other 2017 treatment effects are robust to pre trends up to 1.5 times the maximal pre trend, and most are robust to 2 times the maximal pre trends.

The confidence intervals for later, 2018 and 2019, treatment effects (not shown) are wider and the breakdown values smaller. For 2018, the estimated treatment effects for grade 13 in the academic track and all grades in the vocational track are robust to pre trends at least 1.5 times the maximal observed. For 2018 all estimates are robust to pre trends at least one half the maximal. For the academic track this is an expected result. As noted above the estimated impact of the new policy on absence attenuates over time, perhaps due to student learning, and the estimated treatment effects are closer in magnitude to the estimates for 2014. We do not observe a similar attenuation in the treatment effects for the vocational track, but the longer time span allows more time for the grades' trends to diverge.

We present a similar analysis of the estimated treatment effects for teacher awarded grades in figure 11. Based on our previous discussion of the estimates in figure 8, we focus on the results for the vocational track and grade 13 of the academic track. For the estimated impact for grade 13 students in the academic track in 2017 the breakdown value is greater than 1 but less than 1.5. For the estimates in later years, the breakdown values (not shown) are consistently less

than 1. In the vocational track, the stronger inference is for the grade 12 students. The breakdown value for the 2017 estimate is again between 1 and 1.5. For the grade 11 students, the breakdown estimate is less than 1.

As noted above, a common criticism of "one size fits all" approaches to student truancy are that they potentially punish groups with high pre exiting levels of absence. We have noted that the Norwegian policy appears to have 1) significantly lowered absence, 2) *not* excessively penalized and 3) raised GPA for, students from immigrant and low income households. In figure A4 of the appendix we report robust confidence intervals for the 2017 absence and GPA estimates, by track, for students from immigrant households. In both tracks, the breakdown value for absence is greater than 2. In later years (not shown) it is in excess of 1.5 for 2018 and around 1 for 2019. For GPA the breakdown value in the academic track is less than 0.5, consistent with our discussion of this result in figure 9. For the vocational track the breakdown value is about 1.5, falling (not shown) to around 1 for 2018 and 0.5 for 2019.

6 Discussion

Our analysis indicates that Norway's new absence policy had large persistent effects on the attendance of most students within the high school system. The absolute impact was generally larger for groups with higher baseline levels, and particularly large for students in the vocational track where baseline rates of chronic absenteeism are very high. Given this substantive behavioural response to the policy it might be expected that the new penalties for absenteeism under the law were effective more in their threat than their application. Correspondingly, we find no evidence of changes in the incidence of no grades in a course with the introduction of the new policy to match the estimated impacts on absence. Therefore, this

simple penalty regime appears to have been an effective tonic for high rates of chronic absenteeism in Norway's high schools.

Our evidence of the impact of the new policy on students' academic performance is less definitive, although the evidence suggests positive impacts for the students with the highest baseline rates of absence and the largest behavioral response: students in the highest grade of the academic track and those in the vocational track.

The potential impact of the new policy is complicated by evidence consistent with student learning under the new regime. Spreading out a target level of absence over many courses is a strategy to avoid the penalties under the new policy. However, 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. By reallocating absence in these courses, students may undermine their interest in school and/or lower their overall achievement. Unfortunately our data do not allow us to formally investigate this potential response to the new policy.

Taking the estimated impacts on teacher awarded grades at face value, they range from over 8 percent of a standard deviation for grade 13s in the academic track to a range of 4 to over 10 percent of a standard deviation in the vocational track. 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 track students would be expected to have a much smaller impact than 8 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 in Cattan et al. (2023) 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 is 2-3 days at the aggregate level, ranging from 1 to 1 ½ days for grade 11s in the academic track to 3-6 days in the vocational track. By these metrics the estimated impact on GPA at 4 to 10 percent of a standard deviation is larger than in this previous research. However, treated students clearly differ in this comparison.

7 Conclusions

In the autumn of 2016, Norwegian educational authorities implemented a new policy for students in high school. 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 21 to 28 percent and chronic absence was reduced by up to 44 percent. Larger impacts are observed for grades and groups with higher baseline absence. 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, our evidence suggests the policy had a positive effect on teacher awarded grades for groups that exhibit the largest adjustments in attendance.

The Norwegian policy is penalty based. 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. However, 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. Therefore, there may be lessons from our analysis for the widespread, high rates of truancy which have persisted following the COVID-19 pandemic. We also note that there may be other benefits to increased school attendance not captured by academic performance, including important social development milestones and better work habits.

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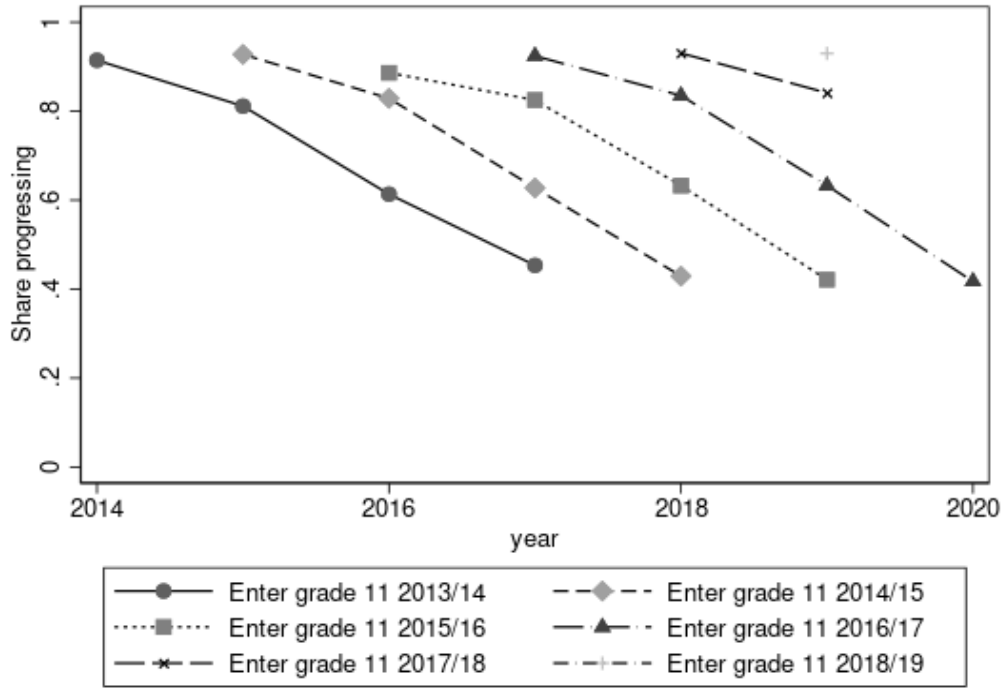
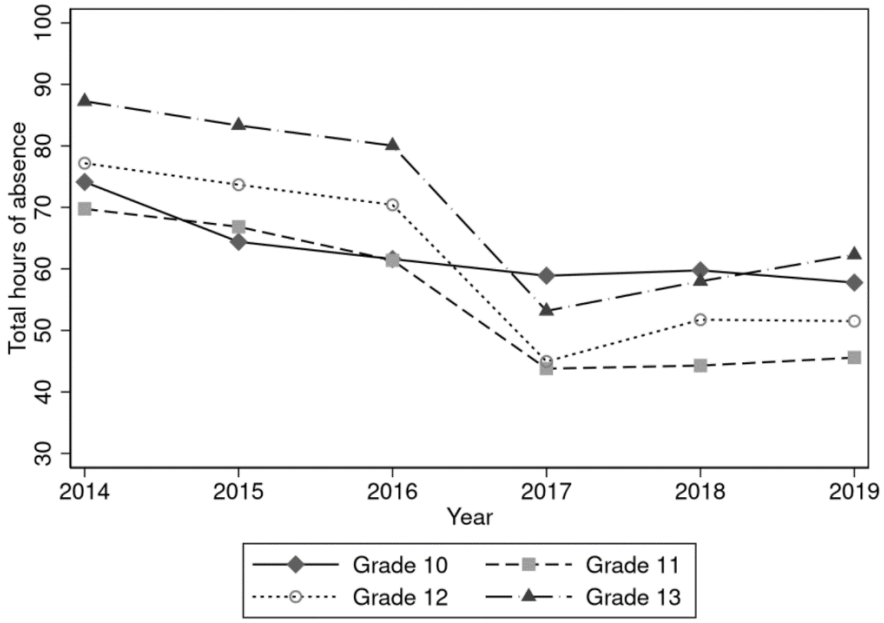
Figure 1: The Proportion of Grade 10 cohorts Progressing through High School to Graduation (2013-2018 Cohorts)



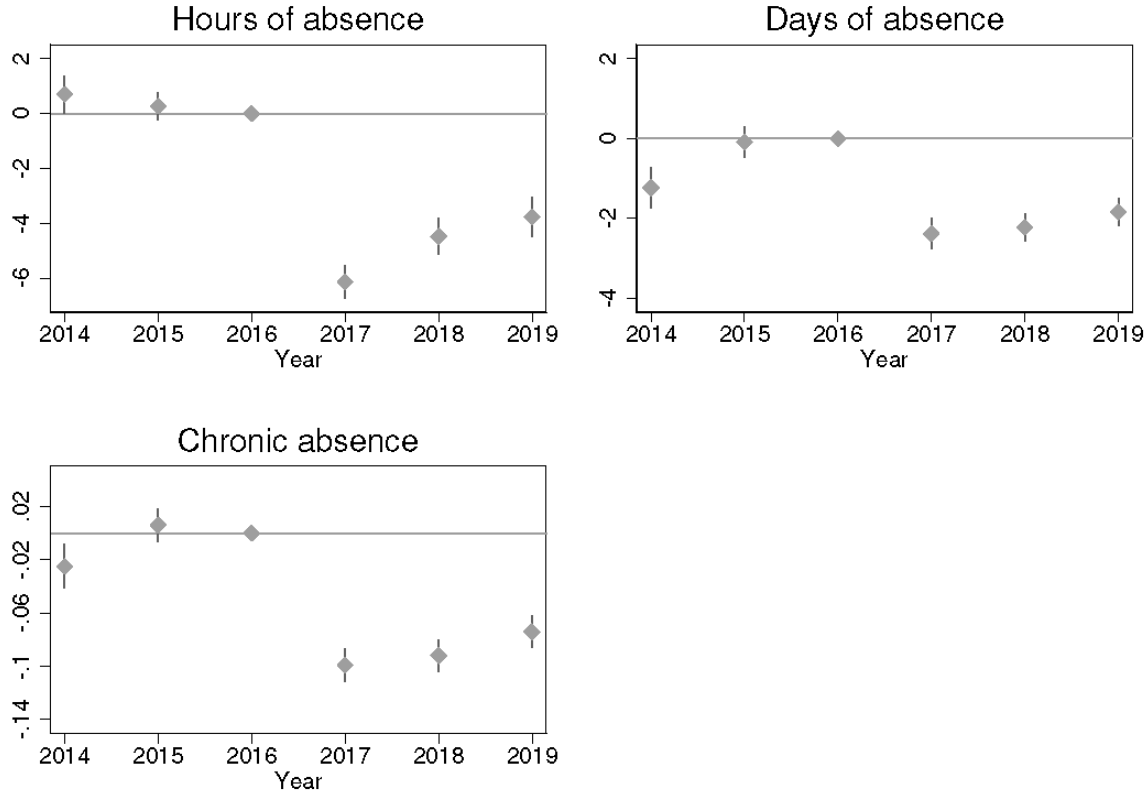
Notes: Authors' calculations from Norwegian Registry data.

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



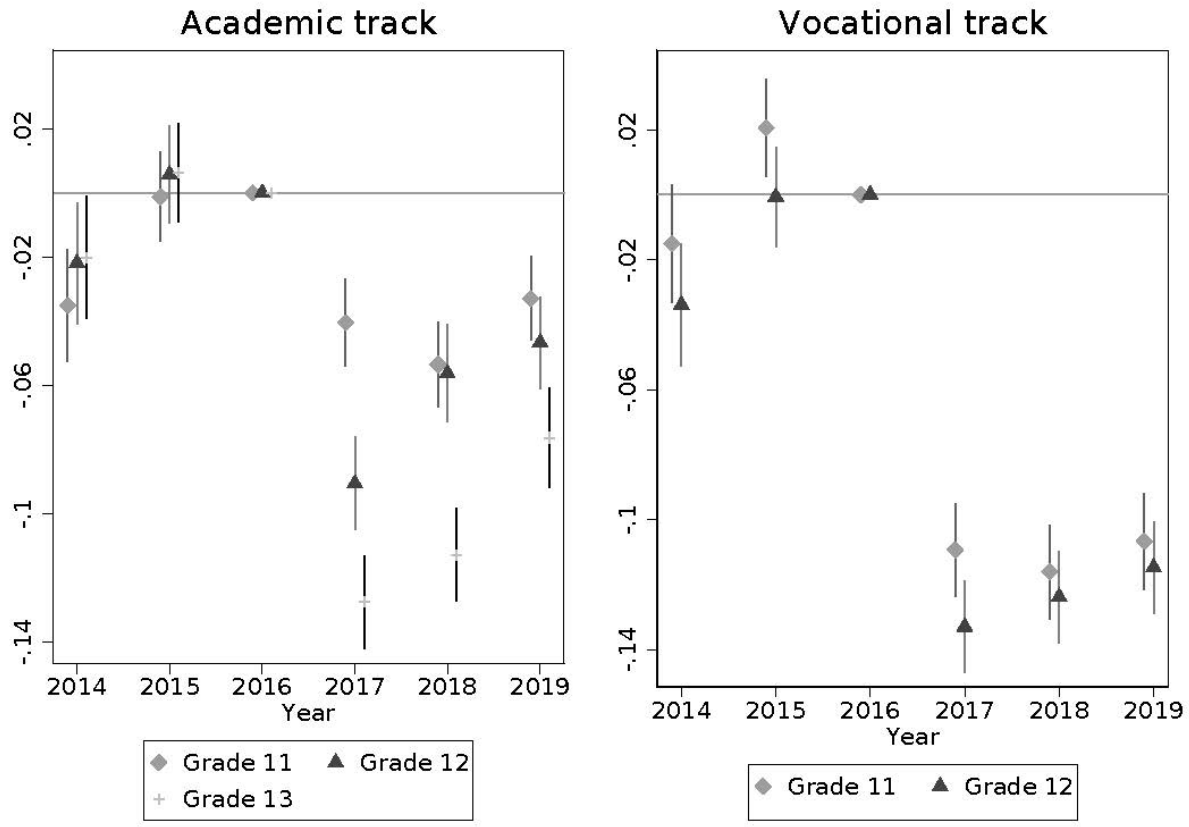
Notes: Authors' calculations from Norwegian Registry data.

Figure 3: The Effect of the New Absence Policy on Hours, Days and Chronic Absence, 2014-2019



Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The reported estimates (and their 95% confidence intervals) are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Average total hours of absence in 2016 for grades 11-13 is 69.4. Average hours of absence in 2016 is 20.6. Average days of absence in 2016 is 8.2. Average chronic absence in 2016 is 0.249. Standard errors clustered by school.

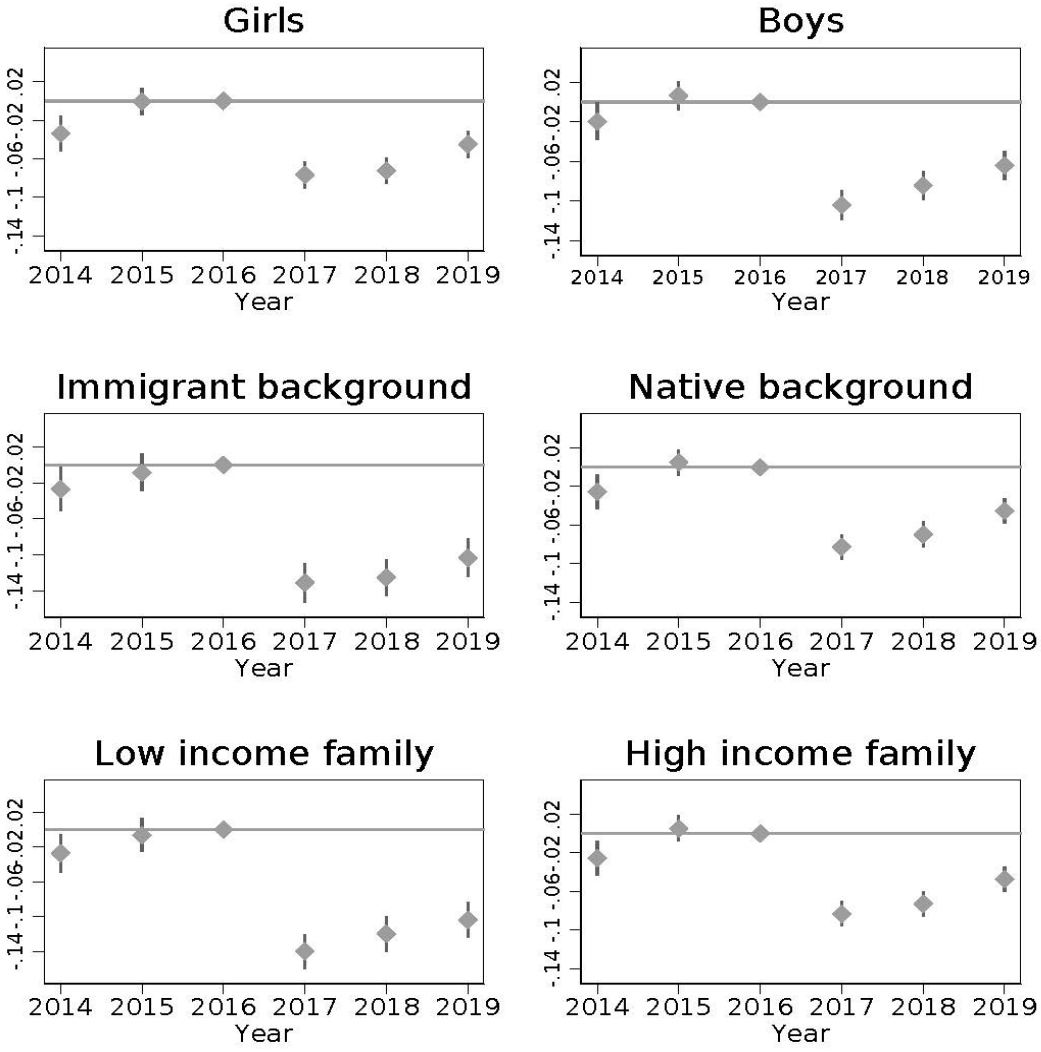
Figure 4: The Effect of the New Absence Policy on Chronic Absence by Grade and Track, 2014-2019



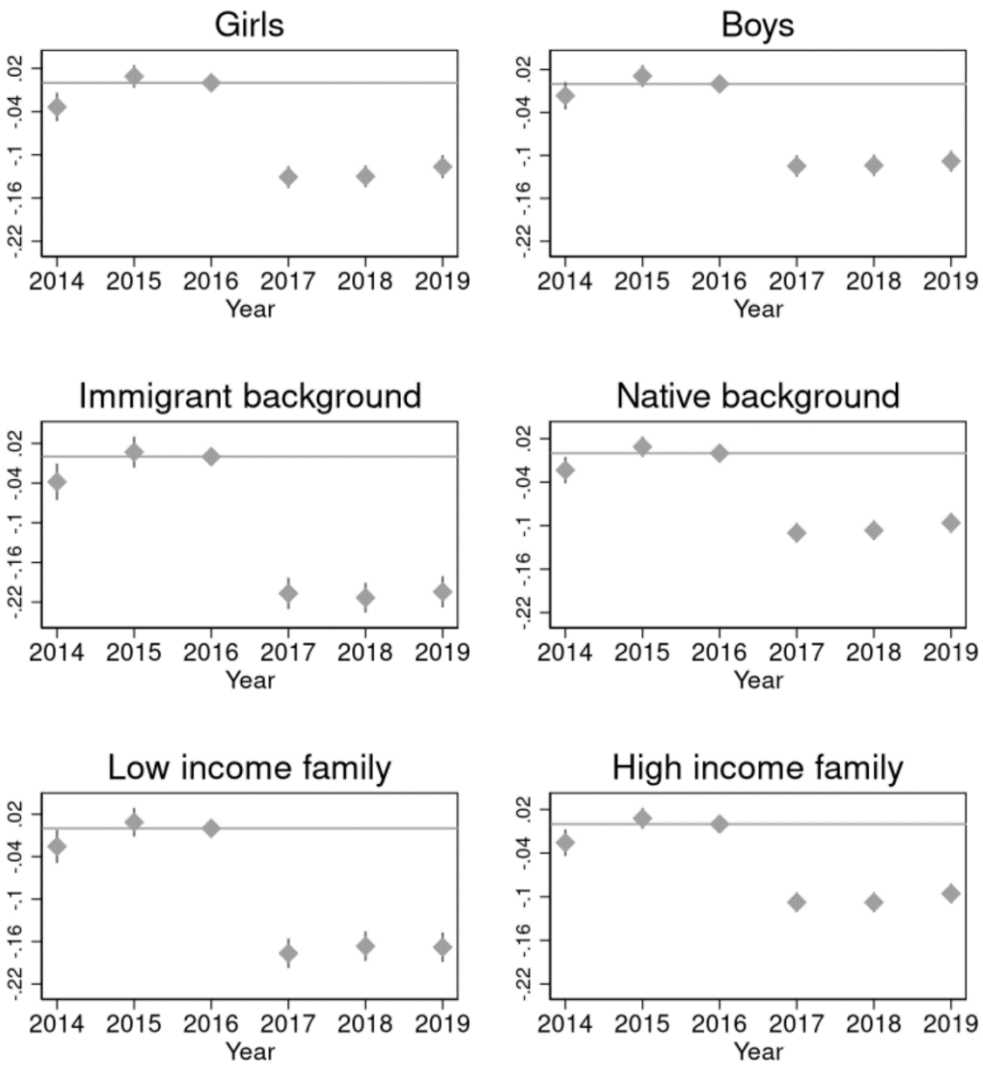
Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The reported estimates (and their 95% confidence intervals) following (1) are for the interactions of grade fixed effects with year fixed effects with the effects for 2016 normalized to 0, and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Average chronic absence in 2016 for each grade by track is reported in table 3. Standard errors clustered by school.

Figure 5: The Effect of the New Absence Policy on Chronic Absence by Demographic Groups and Track, 2014-2019

Academic Track

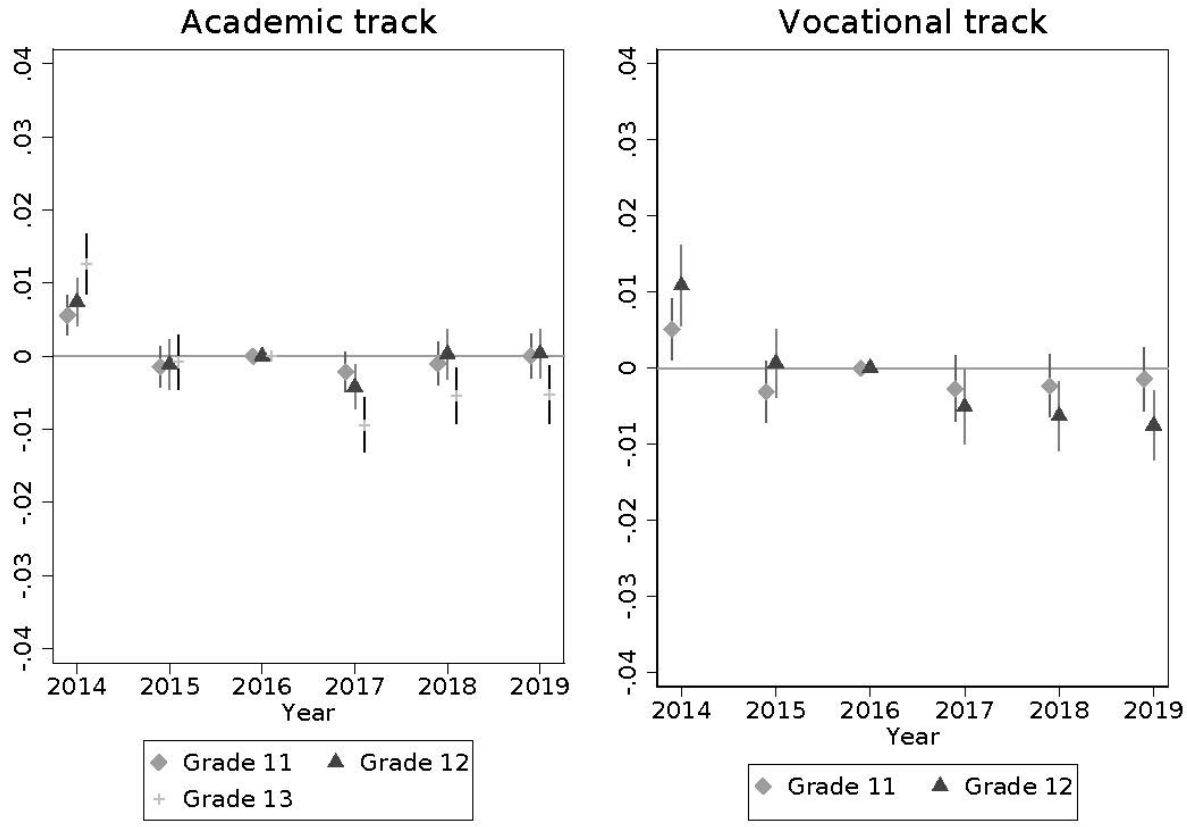


Vocational Track



Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The reported estimates (and their 95% confidence intervals) are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Average chronic absence in 2016 for each demographic group by track is reported in table 3. Standard errors clustered by school.

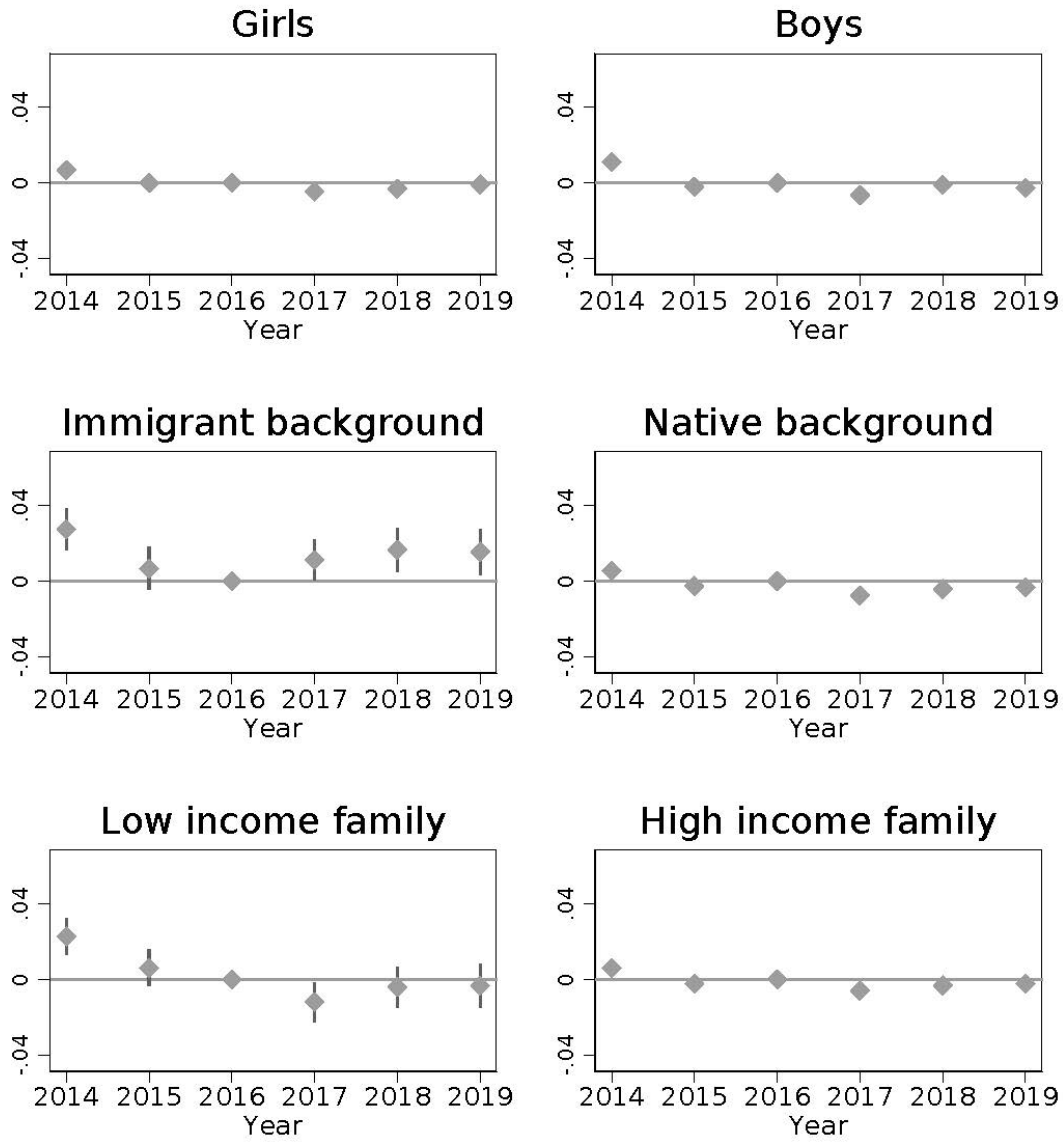
Figure 6: The Effect of the New Absence Policy on the Incidence of a No Grade in a Course, by Grade, by Track 2014-2019



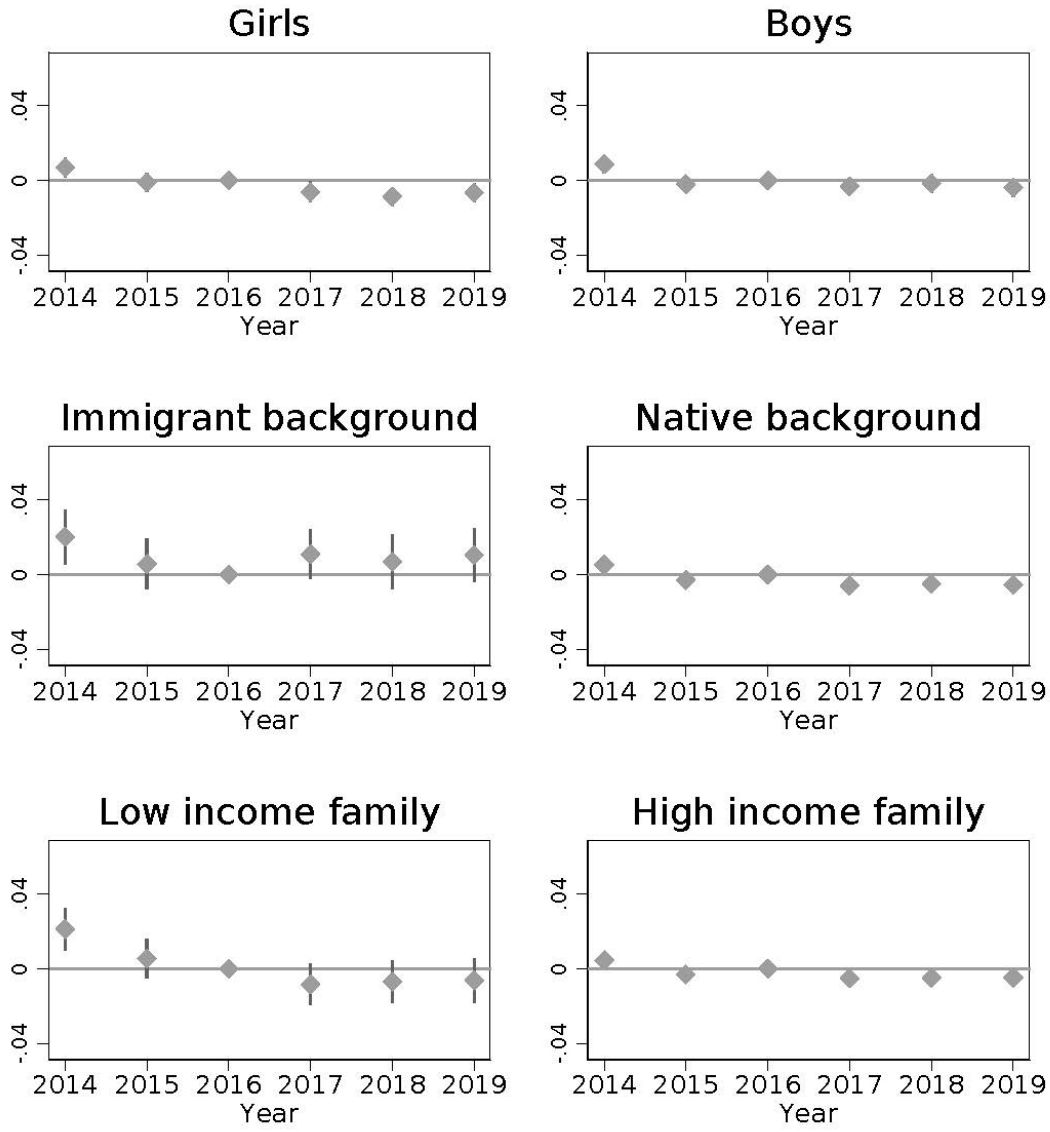
Notes: Authors' calculations from Norwegian Registry data. The reported estimates (and their 95% confidence intervals) following (1) are for the interactions of grade fixed effects with year fixed effects with the effects for 2016 normalized to 0, and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Average incidence of no grade in 2016 for each grade by track is reported in table A5. Standard errors clustered by school.

Figure 7: The Effect of the New Absence Policy on the Incidence of a No Grade in a Course, by Demographic Groups and Track 2014-2019

Academic Track

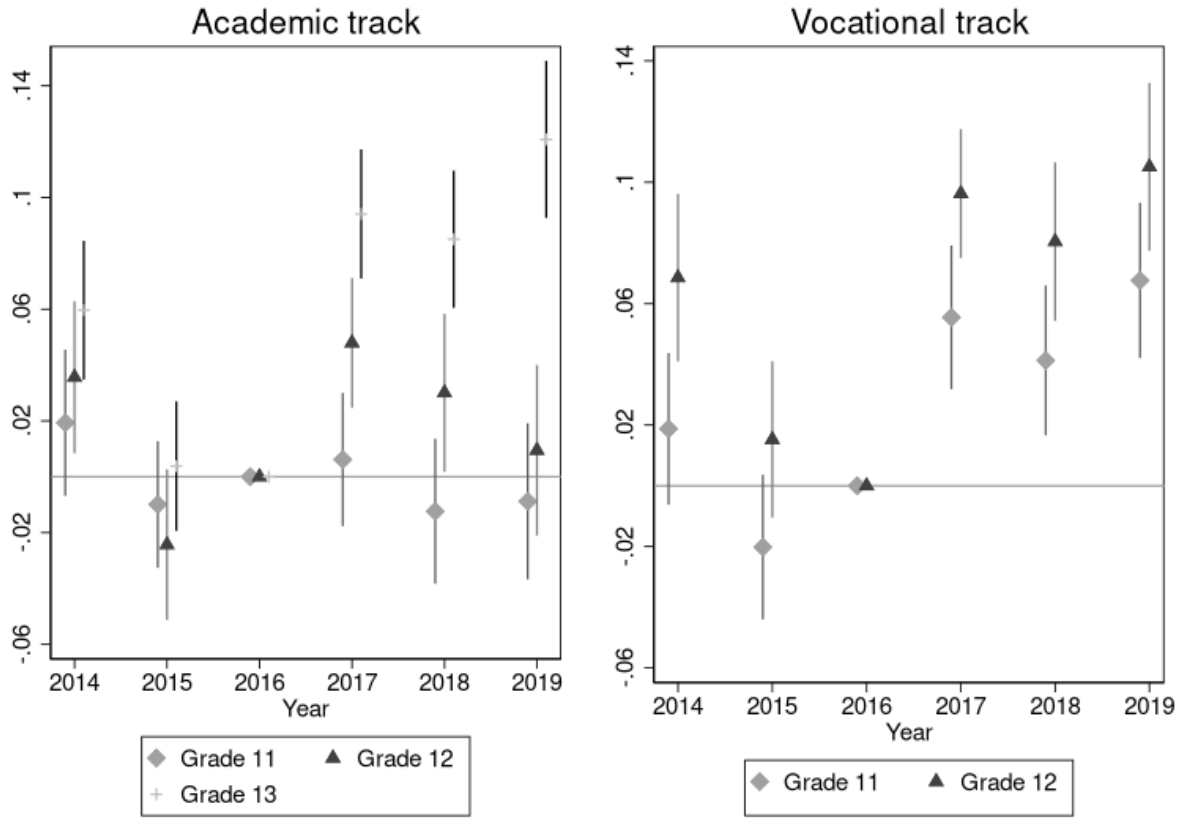


Vocational Track



Notes: Authors' calculations from Norwegian Registry data. The reported estimates (and their 95% confidence intervals) are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Average incidence of no grade in 2016 for each demographic group by track is reported in table A6. Standard errors clustered by school.

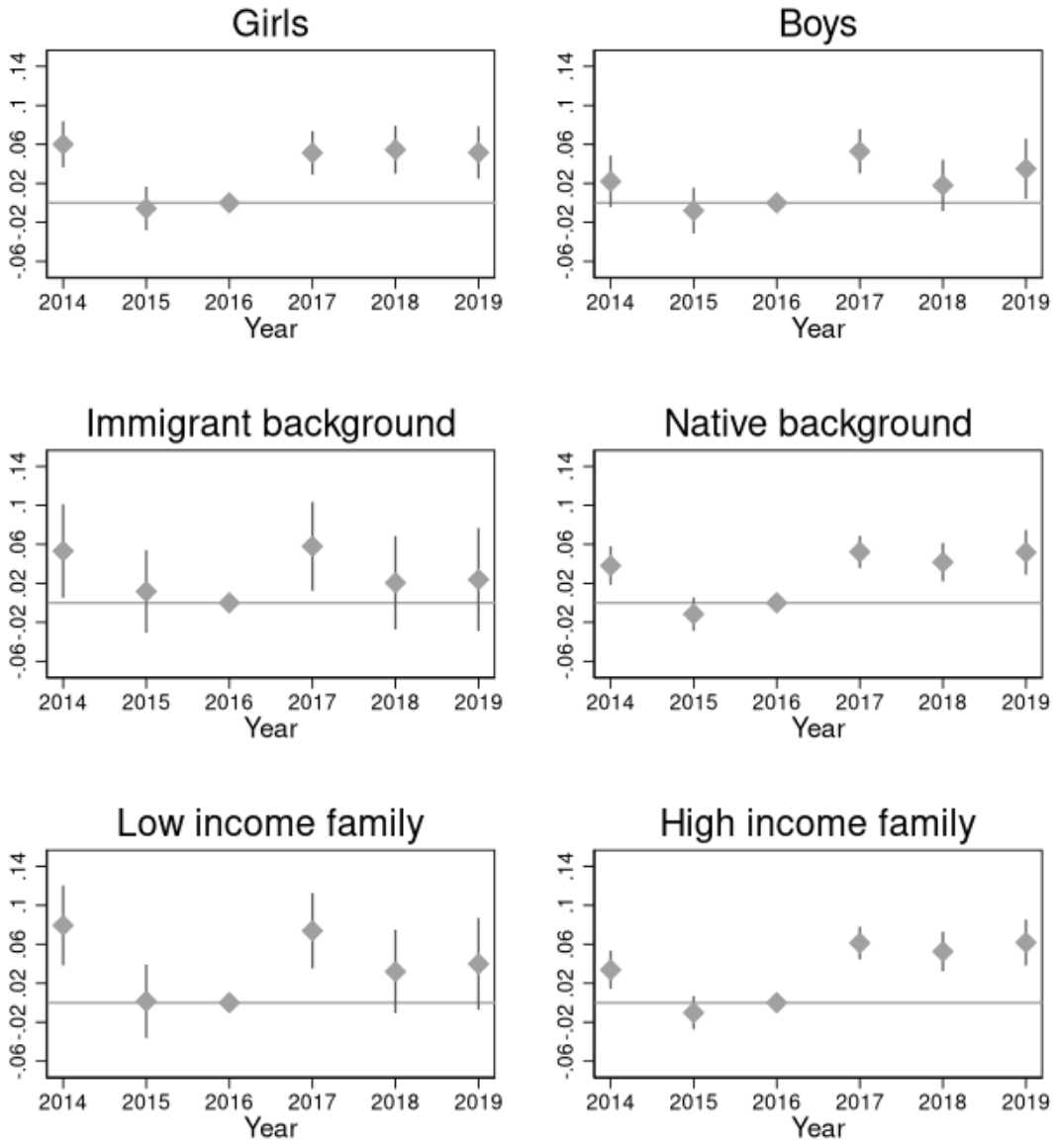
Figure 8: The Effect of the New Absence Policy on Average Teacher Awarded GPA, by Grade, by Track, 2014-2019



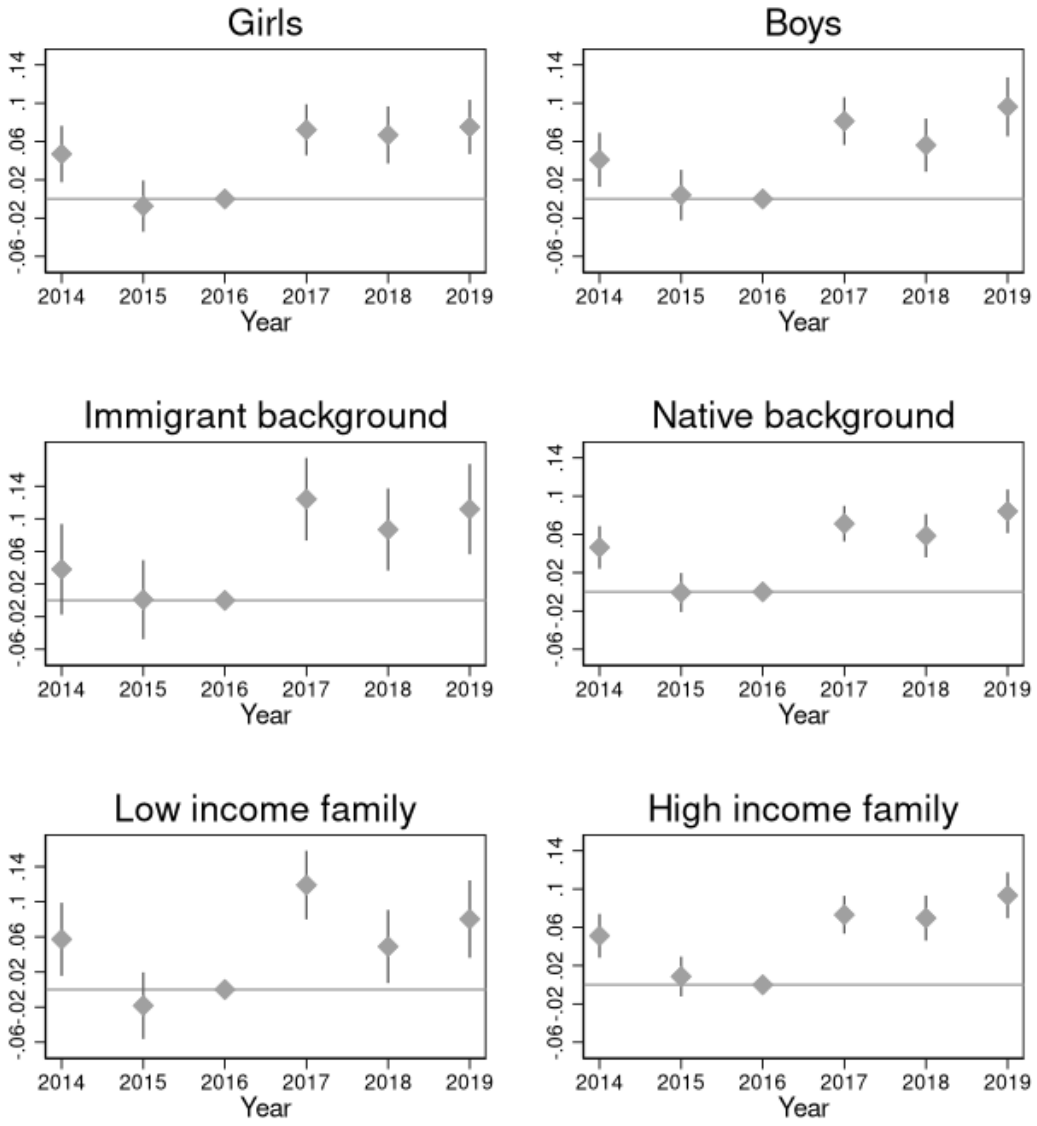
Notes: Authors' calculations from Norwegian Registry data. The reported estimates (and their 95% confidence intervals) following (1) are for the interactions of grade fixed effects with year fixed effects with the effects for 2016 normalized to 0, and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only).). GPA is standardized is standardized the estimates can be read in standard deviations. Standard errors clustered by school.

Figure 9: The Effect of the New Absence Policy on Average Teacher Awarded GPA, by Demographic Groups and Track, 2014-2019

Academic Track



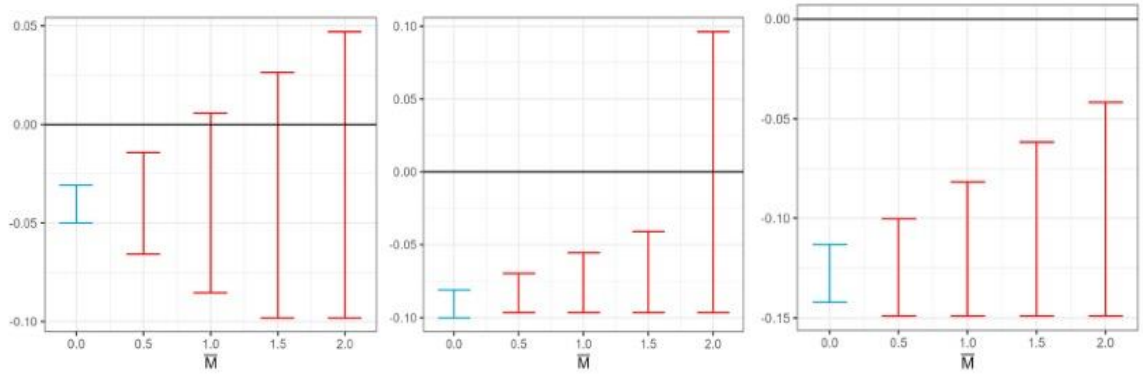
Vocational Track



Notes: Authors' calculations from Norwegian Registry data. The reported estimates (and their 95% confidence intervals) following (1) are for $\lambda(t)$ with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only).). GPA is standardized is standardized the estimates can be read in standard deviations. Standard errors clustered by school.

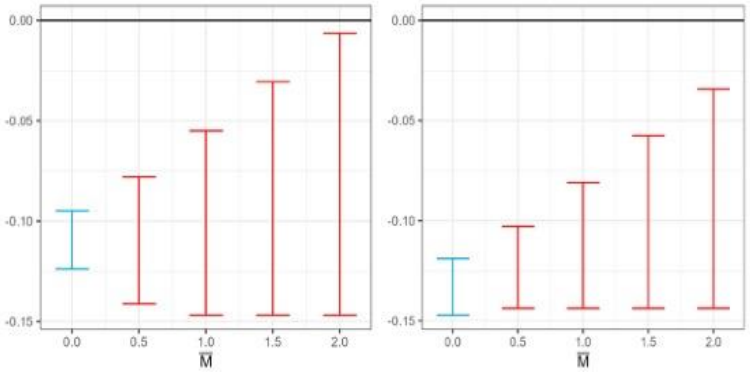
Figure 10: Robust Confidence Intervals for Difference-in-Differences Estimates of the Impact of the New Absence Policy on Students' Chronic Absence by Grade, by Track, in 2017

Academic Track



Grade 11 Grade 12 Grade 13

Vocational Track

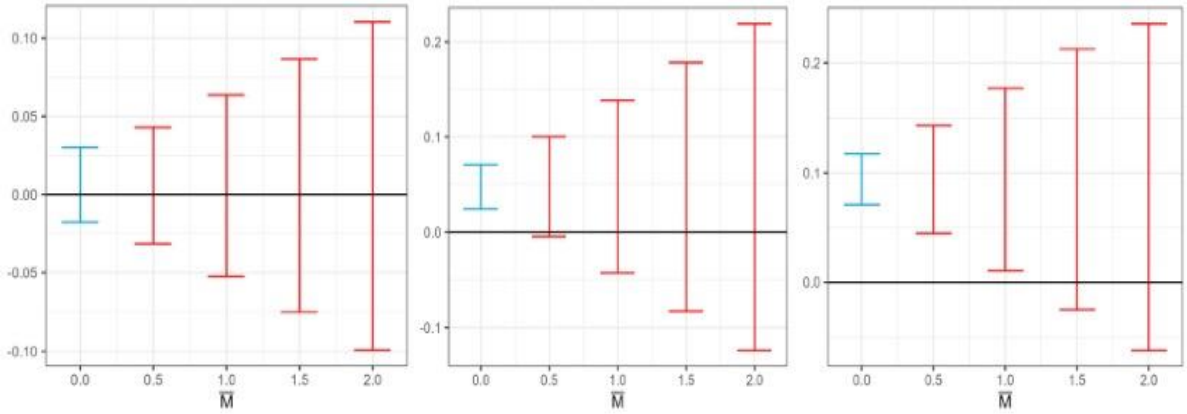


Grade 11 Grade 12

Notes: All confidence intervals are for the 2017 estimates in table 3. Blue lines are the original 95% confidence intervals. The red lines are the robust 95% confidence intervals estimated following Rambachan and Roth (2023).

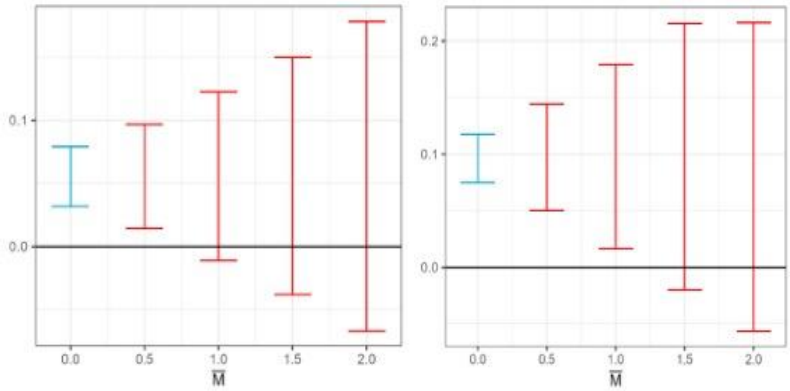
Figure 11: Robust Confidence Intervals for Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher Awarded GPA by Grade, by Track in 2017

Academic Track



Grade 11 Grade 12 Grade 13

Vocational Track



Grade 11 Grade 12

Notes: All confidence intervals are for the 2017 estimates in table 7. Blue lines are the original 95% confidence intervals. The red lines are the robust 95% confidence intervals estimated following Rambachan and Roth (2023).

Table 1: Summary Statistics, by Year, for the Analysis Sample

	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	0.89	0.88
Mother finished HS	0.71	0.73	0.74	0.75	0.76	0.76
Father finished HS	0.70	0.71	0.71	0.72	0.73	0.73
Family income (NOK)	888476	927681	963654	1005761	1041494	1072642
Low income	0.17	0.17	0.18	0.17	0.17	0.17
Area of study						
Lower secondary	0.28	0.28	0.28	0.27	0.27	0.28
Academic track	0.46	0.47	0.47	0.47	0.47	0.46
Vocational Track						
Building and construction	0.03	0.03	0.03	0.03	0.03	0.03
Design and crafts	0.02	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	0.05	0.06	0.06	0.06	0.07
Media and communication	0.03	0.03	0.02	0.02	0.03	0.02
Agriculture, fishing and forestry	0.01	0.01	0.01	0.01	0.02	0.01
Restaurant and food processing	0.01	0.01	0.01	0.01	0.01	0.01
Service and transport	0.03	0.03	0.03	0.02	0.02	0.03
Technical and industrial production	0.05	0.05	0.04	0.04	0.04	0.04
N	212797	213359	214218	211171	209482	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 Measures of Student Absence

Difference in Differences N=1268601			
	Hours Absent	Days absent	Chronic absence
2014	-0.702	-1.231	-0.025
	(0.352)**	(0.266)	(0.008)***
2015	0.277	-0.085	0.006
	(0.256)	(0.197)	(0.006)
2017	-6.139	-2.385	-0.099
	(0.299)***	(0.194)	(0.006)***
2018	-4.472	-2.223	-0.092
	(0.331)***	(0.172)	(0.006)***
2019	-3.758	-1.838	-0.074
	(0.365)***	(0.173)	(0.006)***
2016 Mean	20.6	8.2	0.249
F-test	58.23	9.17	17.28
	[0.00]	[0.00]	[0.00]
Fixed Effects N=1124798			
	Hours Absent	Days absent	Chronic absence
2015	-0.028	0.045	0.002
	(0.108)	(0.055)	(0.002)
2017	-4.498	-1.900	-0.081
	(0.228)***	(0.091)***	(0.003)***
2018	-3.474	-1.715	-0.070
	(0.294)***	(0.132)***	(0.005)***
2019	-3.491	-1.928	-0.077
	(0.387)***	(0.183)***	(0.007)***
2016 Mean	20.6	8.12	0.249
F-test	15.88	0.05	0.69
	[0.00]	[0.82]	[0.408]

Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). The fixed effects estimates follow (2) with additional controls for 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. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table 3: Baseline (2016) Chronic Absence by Track, Grade and Demographic Groupings

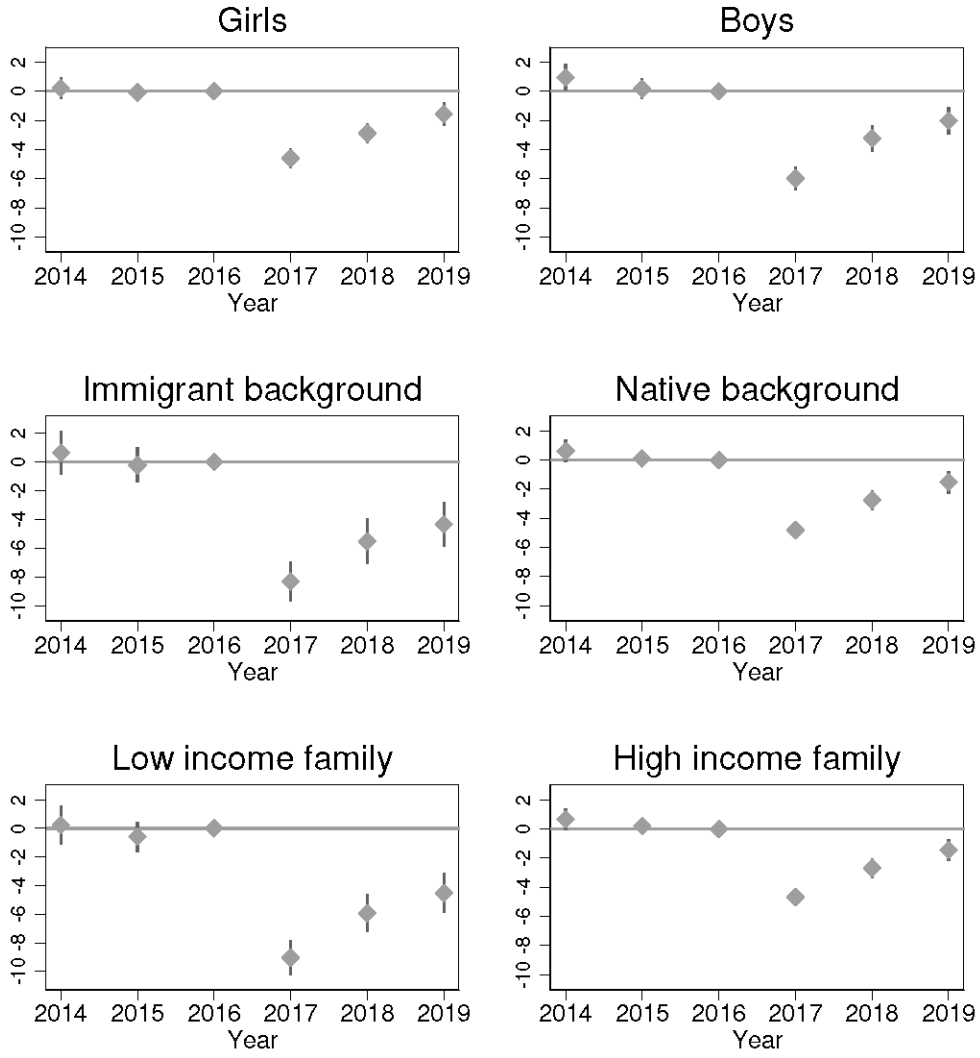
	Average in 2016
Full Sample	0.249
Grade 10	0.204
Academic Track	
Grade 11	0.155
Grade 12	0.222
Grade 13	0.308
Girls	0.218
Boys	0.252
Immigrant Households	0.310
Native Born Households	0.222
Low Income Households	0.343
High Income Households	0.213
Vocational Track	
Grade 11	0.268
Grade 12	0.288
Girls	0.330
Boys	0.244
Immigrant Households	0.430
Native Born Households	0.258
Low Income Households	0.400
High Income Households	0.240

Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14.

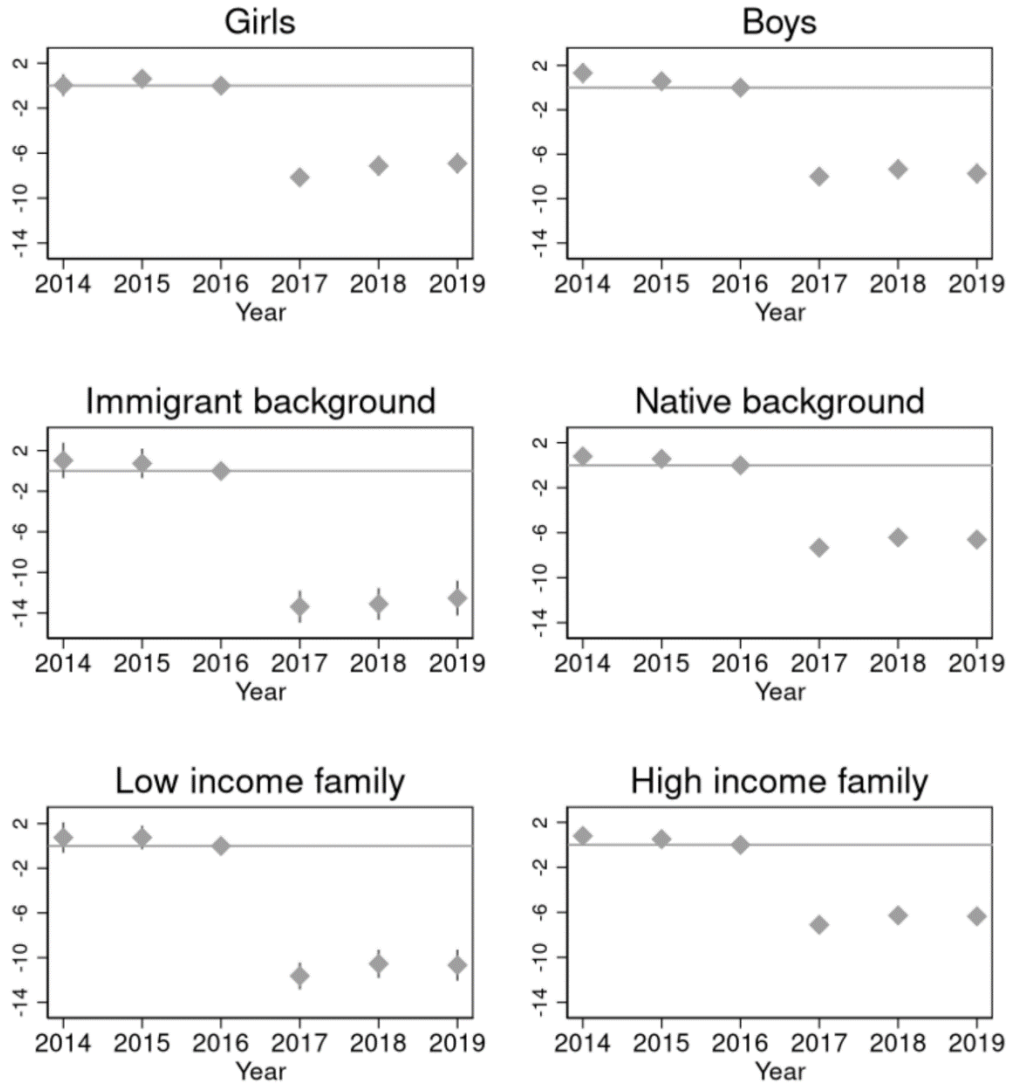
Appendix

Figure A1: The Effect of the New Absence Policy on Hours of Absence by Demographic Groups and Track, 2014-2019

Academic Track



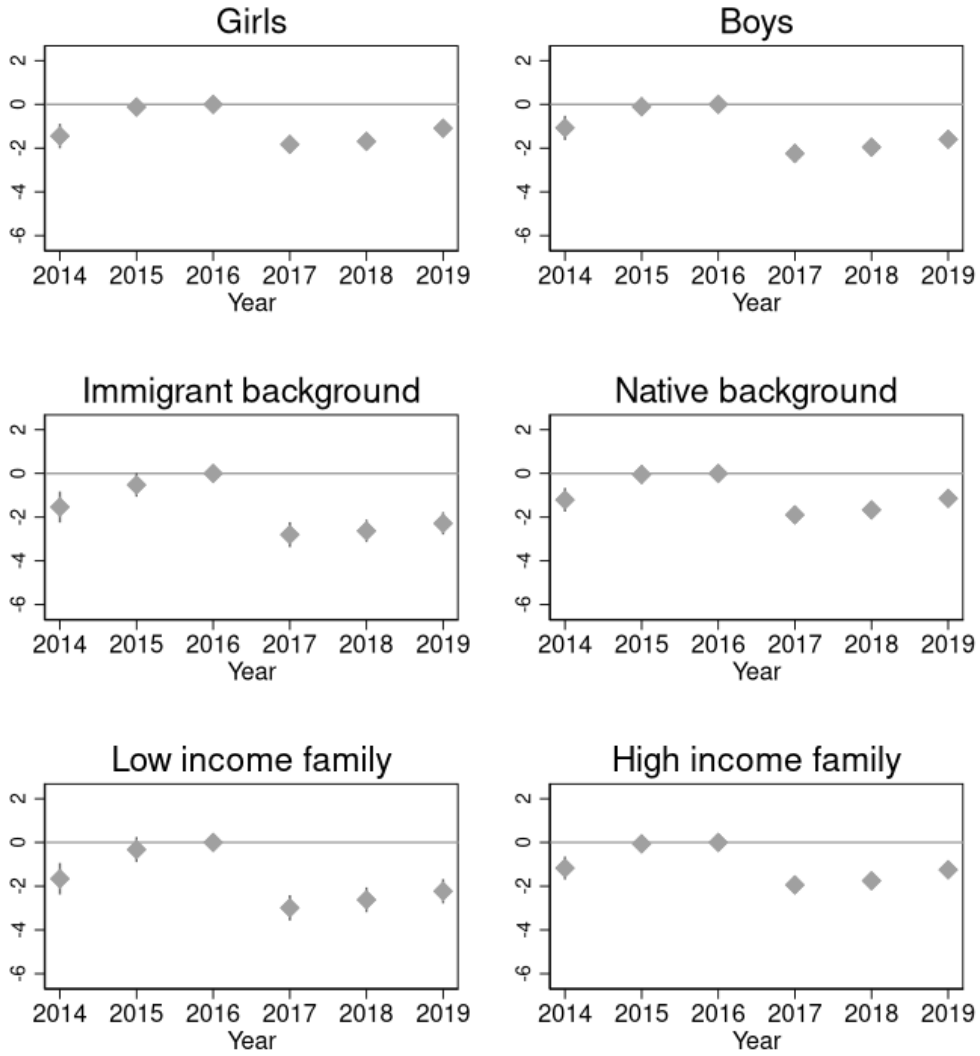
Vocational Track



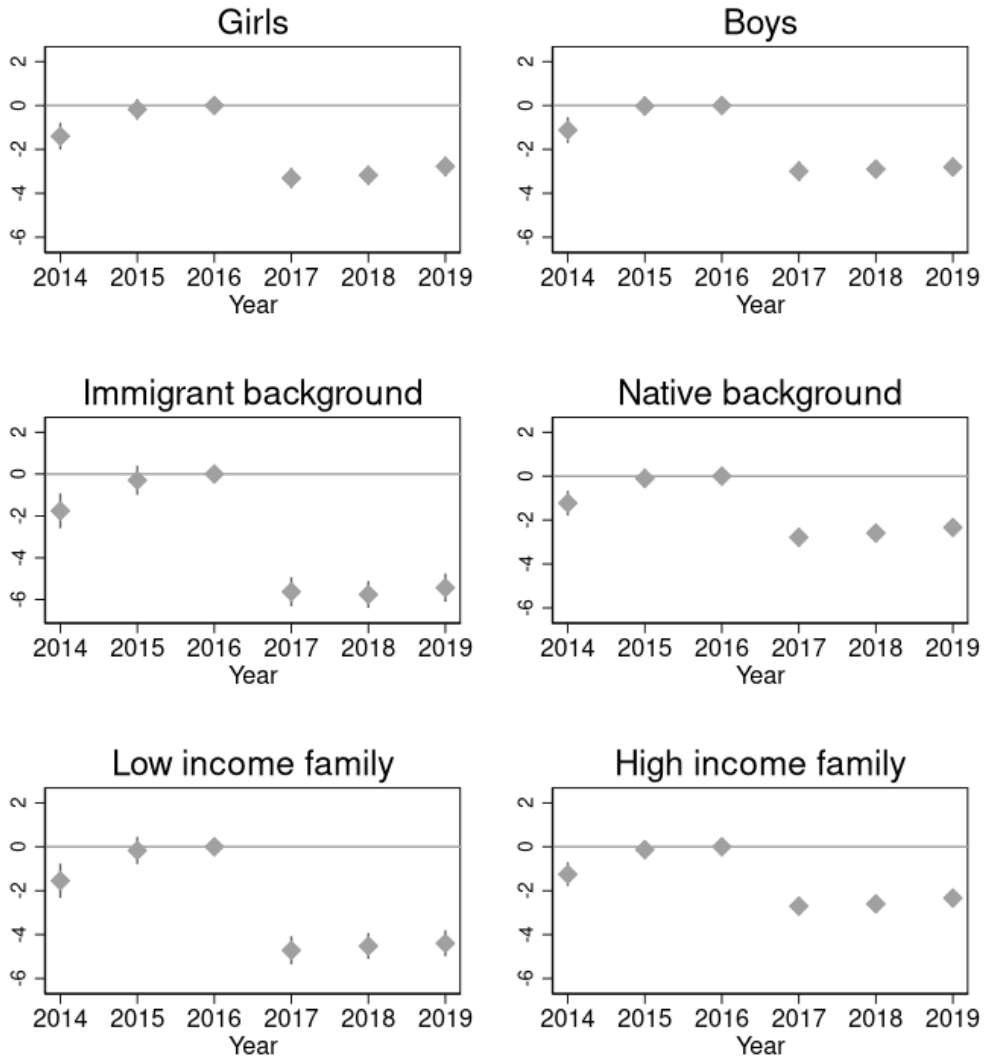
Notes: Authors' calculations from Norwegian Registry data. The reported estimates are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Standard errors clustered by school.

Figure A2: The Effect of the New Absence Policy on Full Days of Absence by Demographic Groups and Track, 2014-2019

Academic Track



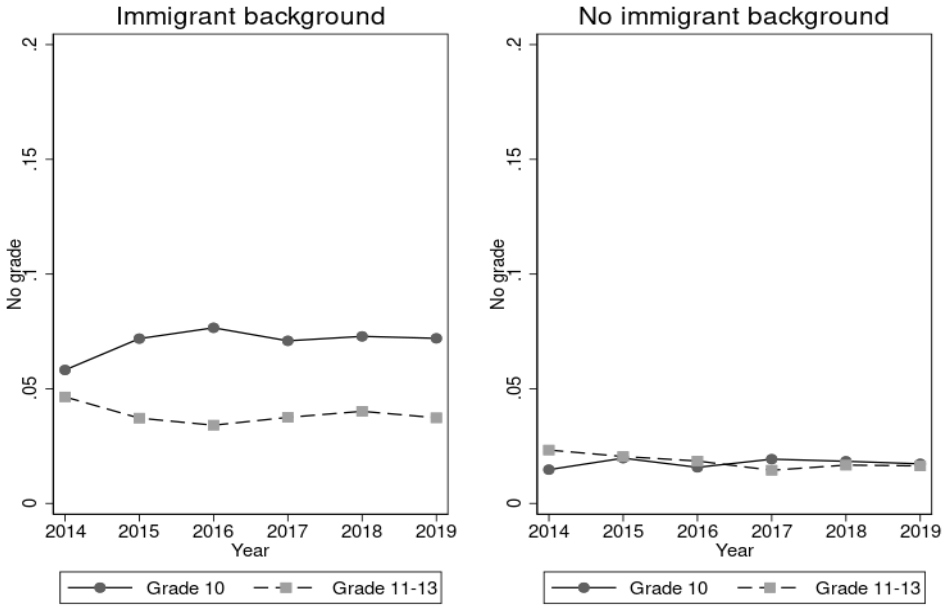
Vocational track



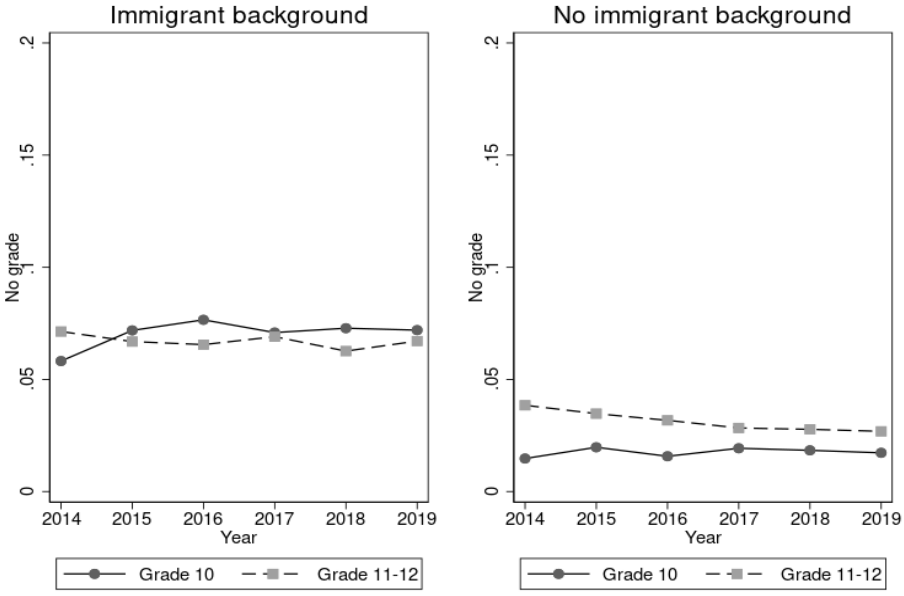
Notes: Authors' calculations from Norwegian Registry data. The reported estimates are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Standard errors clustered by school.

Figure A3: The Incidence of a No Grade in a Course, by Immigrant/Native Born Household, by Track 2014-2019

Academic Track



Vocational Track

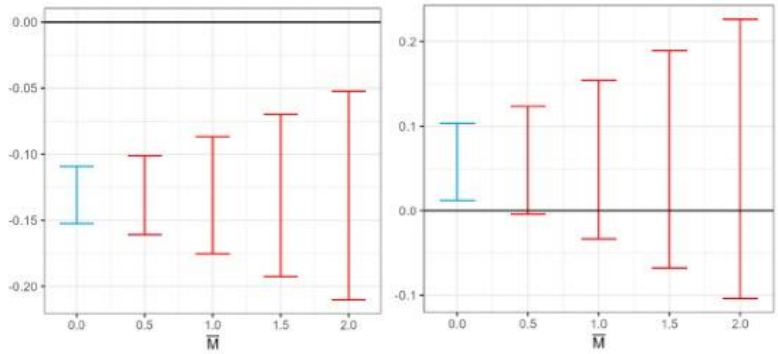


Notes:

Notes: Authors' calculations from Norwegian Registry data. The reported estimates are for $\lambda(t)$ following (1) with $\lambda(2016) = 0$ and additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). Standard errors clustered by school.

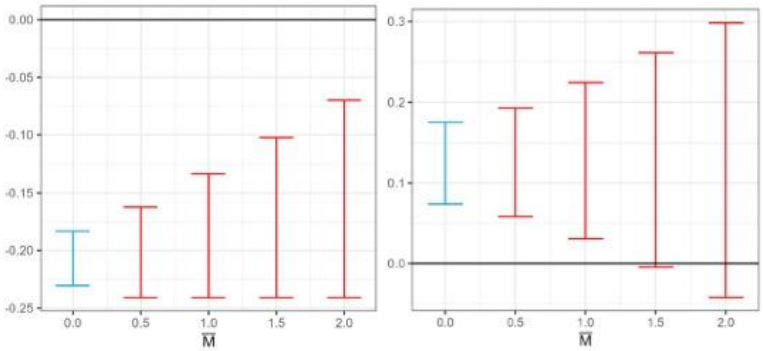
Figure A4: Robust Confidence Intervals for Difference-in-Differences Estimates of the Impact of the New Absence Policy on Absence and Teacher Awarded GPA, by Track for Students from Immigrant Households in 2017

Academic Track



Absence GPA

Vocational Track



Absence GPA

Notes: All confidence intervals are for the 2017 estimates in table 4 (absence) and 8 (GPA). Blue lines are the original 95% confidence intervals. The red lines are the robust 95% confidence intervals estimated following Rambachan and Roth (2023).

Table A1: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Students' Chronic Absence by Grade, by Track

	Grade 10	Grade 11	Grade 12	Grade 13
Academic Track N=940644				
2014	0.054	-0.035	-0.022	-0.020
	(0.008)***	(0.009)***	(0.010)**	(0.010)**
2015	0.011	-0.001	0.006	0.006
	(0.006)*	(0.007)	(0.008)	(0.008)
2017	-0.014	-0.040	-0.091	-0.128
	(0.006)**	(0.007)***	(0.007)***	(0.007)***
2018	-0.002	-0.053	-0.056	-0.113
	(0.005)	(0.007)***	(0.008)***	(0.007)***
2019	-0.012	-0.033	-0.047	-0.076
	(0.005)**	(0.007)***	(0.007)***	(0.008)***
2016 Mean	0.204	0.155	0.222	0.308
F-test		1.24	39.57	49.94
		[0.266]	[0.00]	[0.00]
Vocational Track N=678520				
2014	0.053	-0.015	-0.034	
	(0.008)***	(0.009)	(0.010)***	
2015	0.010	0.021	-0.001	
	(0.006)*	(0.008)***	(0.008)	
2017	-0.013	-0.109	-0.133	
	(0.006)**	(0.007)***	(0.007)***	
2018	0.000	-0.116	-0.124	
	(0.005)	(0.007)***	(0.007)***	
2019	-0.010	-0.107	-0.115	
	(0.005)*	(0.008)***	(0.007)***	
2016 Mean	0.204	0.268	0.288	
F-test		0.15	6.96	
		[0.70]	[0.01]	

Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). 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. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A2: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Chronic Absence by Selected Demographic Characteristics, by Track

	Girls	Boys	Immigrants	Native Born	Low income	Non-low income
Academic Track						
2014	-0.034	-0.019	-0.027	-0.025	-0.027	-0.026
	(0.009)***	(0.010)**	(0.012)**	(0.009)***	(0.011)**	(0.009)***
2015	-0.000	0.006	-0.009	0.005	-0.006	0.005
	(0.007)	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)
2017	-0.077	-0.104	-0.131	-0.083	-0.140	-0.083
	(0.007)***	(0.007)***	(0.011)***	(0.007)***	(0.010)***	(0.007)***
2018	-0.072	-0.084	-0.125	-0.070	-0.120	-0.073
	(0.007)***	(0.008)***	(0.010)***	(0.007)***	(0.010)***	(0.007)***
2019	-0.045	-0.064	-0.103	-0.045	-0.104	-0.048
	(0.007)***	(0.007)***	(0.011)***	(0.007)***	(0.010)***	(0.007)***
2016 Mean	0.218	0.252	0.310	0.222	0.343	0.213
F-test	22.20	31.60	6.56	34.14	11.43	31.71
	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]
N	499231	441430	122295	818366	142002	798659
Vocational Track						
2014	-0.034	-0.016	-0.038	-0.023	-0.026	-0.026
	(0.010)***	(0.010)*	(0.014)***	(0.009)**	(0.012)**	(0.009)***
2015	0.009	0.011	0.007	0.009	0.009	0.008
	(0.008)	(0.008)	(0.012)	(0.007)	(0.010)	(0.007)
2017	-0.131	-0.115	-0.207	-0.110	-0.177	-0.108
	(0.008)***	(0.008)***	(0.012)***	(0.007)***	(0.011)***	(0.007)***
2018	-0.130	-0.114	-0.213	-0.106	-0.166	-0.108
	(0.008)***	(0.008)***	(0.012)***	(0.007)***	(0.011)***	(0.007)***
2019	-0.116	-0.108	-0.204	-0.096	-0.168	-0.096
	(0.008)***	(0.007)***	(0.012)***	(0.007)***	(0.011)***	(0.007)***
2016 Mean	0.330	0.244	0.430	0.258	0.400	0.240
F-test	3.59	0.91	0.04	4.41	0.65	3.46
	[0.06]	[0.34]	[0.84]	[0.04]	[0.042]	[0.06]
N	305578	372959	88118	590419	131505	547032

Notes: Authors' calculations from Norwegian Registry data. Chronic absence is the proportion of the sample with (total hours of absence)/6) greater than 14. The difference-in-differences estimates are conditional on controls for year, grade, age, gender, field of study, The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). 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. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A3: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Hours Absent (Part Day) by Selected Demographic Characteristics, by Track

	Girls	Boys	Immigrants	Native Born	Low income	Non-low income
Academic Track						
2014	0.223	0.958	0.628	0.627	0.234	0.664
	(0.384)	(0.446)**	(0.772)	(0.376)*	(0.68)	(0.369)*
2015	-0.074	0.174	-0.236	0.11	-0.588	0.19
	(0.299)	(0.345)	(0.613)	(0.278)	(0.531)	(0.277)
2017	-4.594	-5.983	-8.302	-4.802	-9.043	-4.677
	(0.326)***	(0.394)***	(0.689)***	(0.311)***	(0.604)***	(0.307)***
2018	-2.884	-3.224	-5.529	-2.747	-5.94	-2.674
	(0.349)***	(0.457)***	(0.806)***	(0.344)***	(0.673)***	(0.339)***
2019	-1.572	-2.037	-4.343	-1.512	-4.522	-1.457
	(0.395)***	(0.480)***	(0.790)***	(0.389)***	(0.695)***	(0.379)***
2016 Mean	18.5	22.8	28.5	19.3	28.8	19
F-test	71.46	107.75	33.27	102.98	49.31	100.53
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
N	499231	441430	122295	818366	142002	798659
Vocational Track						
2014	0.045	1.318	1.038	0.792	0.748	0.796
	(0.491)	(0.452)***	(0.888)	(0.407)*	(0.695)	(0.401)**
2015	0.614	0.579	0.743	0.57	0.755	0.513
	(0.378)	(0.364)	(0.74)	(0.307)*	(0.532)	(0.311)*
2017	-8.151	-8.000	-13.378	-7.326	-11.639	-7.107
	(0.397)***	(0.404)***	(0.807)***	(0.324)***	(0.613)***	(0.320)***
2018	-7.137	-7.349	-13.114	-6.415	-10.547	-6.274
	(0.447)***	(0.438)***	(0.797)***	(0.366)***	(0.645)***	(0.357)***
2019	-6.912	-7.735	-12.532	-6.606	-10.671	-6.353
	(0.454)***	(0.449)***	(0.875)***	(0.375)***	(0.708)***	(0.361)***
2016 Mean	22.3	5.771	19.231	11.033	17.291	9.781
F-test	8.56	0.50	0.88	5.18	1.94	6.07
	[0.00]	[0.48]	[0.35]	[0.02]	[0.639]	[0.01]
N	305578	372959	88118	590419	131505	547032

Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates are conditional on controls for year, grade, age, gender, field of study, The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). 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. *, **, ** denote statistical significance at the 10, 5 and 1 percent levels respectively.

Table A4: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Days by Selected Demographic Characteristics, by Track

	Girls	Boys	Immigrants	Native Born	Low income	Non-low income
Academic Track						
2014	-1.445	-1.073	-1.536	-1.207	-1.661	-1.172
	(0.284)***	(0.282)***	(0.361)***	(0.275)***	(0.367)***	(0.267)***
2015	-0.12	-0.107	-0.524	-0.048	-0.324	-0.063
	(0.213)	(0.212)	(0.278)*	(0.207)	(0.291)	(0.2)
2017	-1.828	-2.243	-2.799	-1.894	-2.991	-1.942
	(0.205)***	(0.214)***	(0.291)***	(0.202)***	(0.296)***	(0.196)***
2018	-1.691	-1.956	-2.628	-1.667	-2.621	-1.753
	(0.184)***	(0.198)***	(0.260)***	(0.186)***	(0.289)***	(0.176)***
2019	-1.091	-1.596	-2.284	-1.143	-2.231	-1.247
	(0.190)***	(0.191)***	(0.260)***	(0.183)***	(0.282)***	(0.176)***
2016 Mean	7.53	7.67	8.43	7.47	9.62	7.24
F-test	14.39	10.31	3.35	15.86	6.80	14.10
	[0.00]	[0.00]	[0.07]	[0.00]	[0.01]	[0.00]
N	499231	441430	122295	818366	142002	798659
Vocational Track						
2014	-1.397	-1.119	-1.758	-1.229	-1.549	-1.252
	(0.312)***	(0.300)***	(0.424)***	(0.287)***	(0.398)***	(0.277)***
2015	-0.176	-0.024	-0.297	-0.107	-0.168	-0.13
	(0.242)	(0.226)	(0.356)	(0.216)	(0.317)	(0.213)
2017	-3.305	-2.994	-5.625	-2.786	-4.715	-2.701
	(0.233)***	(0.234)***	(0.354)***	(0.213)***	(0.326)***	(0.208)***
2018	-3.174	-2.896	-5.758	-2.592	-4.521	-2.6
	(0.221)***	(0.213)***	(0.326)***	(0.196)***	(0.300)***	(0.190)***
2019	-2.772	-2.802	-5.433	-2.337	-4.397	-2.334
	(0.228)***	(0.210)***	(0.343)***	(0.195)***	(0.300)***	(0.190)***
2016 Mean	10.6	8.09	12.7	8.6	12.2	8.12
F-test	5.85	0.86	0.34	5.32	1.10	3.73
	[0.02]	[0.35]	[0.56]	[0.02]	[0.29]	[0.05]
N	305578	372959	88118	590419	131505	547032

Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates are conditional on controls for year, grade, age, gender, field of study, The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). 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. *, **, ** denote statistical significance at the 10, 5 and 1 percent levels respectively.

Table A5: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Incidence of No Grade Received in a Course by Grade, by Track

	Grade 10	Grade 11	Grade 12	Grade 13
Academic Track N=940661				
2014	-0.004	0.006	0.007	0.013
	(0.001)***	(0.001)***	(0.002)***	(0.002)***
2015	0.003	-0.001	-0.001	-0.000
	(0.001)**	(0.001)	(0.002)	(0.002)
2017	0.003	-0.002	-0.004	-0.009
	(0.001)***	(0.001)	(0.002)***	(0.002)***
2018	0.003	-0.000	0.000	-0.005***
	(0.001)**	(0.002)	(0.002)	(0.002)
2019	0.002	0.000	0.000	-0.005
	(0.001)*	(0.002)	(0.002)	(0.002)***
2016 Mean	0.024	0.010	0.016	0.033
Vocational track N=678537				
2014	-0.005	0.005	0.011	
	(0.001)***	(0.002)**	(0.003)***	
2015	0.002	-0.003	0.000	
	(0.001)**	(0.002)	(0.002)	
2017	0.003	-0.003	-0.005**	
	(0.001)***	(0.002)	(0.003)	
2018	0.003	0.002	-0.006***	
	(0.001)***	(0.002)	(0.002)	
2019	0.003	-0.001	-0.008	
	(0.001)**	(0.002)	(0.002)***	
2016 Mean	0.024	0.028	0.043	

Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). For grade 10 we report the base estimates of $\theta(t)$. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A6: Difference-in-Differences Estimates of the Impact of the New Absence Policy on the Incidence of No Grade in a Course by Selected Demographic Characteristics, by Track

	Girls	Boys	Immigrants	Native Born	Low income	Non-low income
Academic Track						
2014	0.007	0.011	0.028	0.006	0.023	0.006
	(0.002)***	(0.002)***	(0.006)***	(0.001)***	(0.005)***	(0.001)***
2015	-0.000	-0.002	0.007	-0.002	0.006	-0.002
	(0.002)	(0.002)	(0.006)	(0.001)**	(0.005)	(0.001)**
2017	-0.004	-0.007	0.011	-0.008	-0.012	-0.006
	(0.002)***	(0.002)***	(0.005)**	(0.001)***	(0.005)**	(0.001)***
2018	-0.003	-0.001	0.017	-0.004	-0.004	-0.003
	(0.002)*	(0.002)	(0.006)***	(0.001)***	(0.006)	(0.001)***
2019	-0.001	-0.003	0.016	-0.003	-0.003	-0.002
	(0.002)	(0.002)	(0.006)**	(0.001)**	(0.006)	(0.001)**
2016 Mean	0.016	0.026	0.034	0.019	0.042	0.017
N	499231	441430	122295	818366	142002	798659
Vocational Track						
2014	0.007	0.009	0.020	0.005	0.021	0.005
	(0.003)**	(0.002)***	(0.007)***	(0.002)***	(0.006)***	(0.002)***
2015	-0.001	-0.002	0.006	-0.003	0.005	-0.003
	(0.003)	(0.002)	(0.007)	(0.002)*	(0.005)	(0.002)**
2017	-0.006	-0.003	0.011	-0.006	-0.008	-0.005
	(0.003)**	(0.002)	(0.007)	(0.002)***	(0.006)	(0.002)***
2018	-0.009	-0.002	0.007	-0.005	-0.007	-0.005
	(0.003)***	(0.003)	(0.007)	(0.002)***	(0.006)	(0.002)***
2019	-0.007	-0.004	0.010	-0.006	-0.006	-0.005
	(0.003)***	(0.002)	(0.007)	(0.002)***	(0.006)	(0.002)***
2016 Mean	0.039	0.034	0.066	0.032	0.062	0.027
N	305578	372959	88118	590419	131505	547032

Notes: Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. Number of no grades includes 0s. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A7: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Number of No grades, by grade, by track

	Grade 10	Grade 11	Grade 12	Grade 13
Academic Track N=940661				
2014	-0.014	0.016	0.018	0.035
	(0.003)***	(0.004)***	(0.004)***	(0.006)***
2015	0.010	-0.007	-0.006	-0.003
	(0.003)***	(0.004)	(0.005)	(0.006)
2017	0.010	-0.007	-0.010	-0.013**
	(0.003)***	(0.004)	(0.004)**	(0.006)
2018	0.008	-0.004	-0.002	0.002
	(0.003)**	(0.004)	(0.004)	(0.006)
2019	0.006	-0.001	0.000	-0.002
	(0.003)*	(0.004)	(0.004)	(0.006)
2016 Mean	0.055	0.016	0.026	0.076
Vocational Track N=678537				
2014	-0.015	0.015	0.024	
	(0.003)***	(0.005)***	(0.006)***	
2015	0.009	-0.008	0.000	
	(0.003)**	(0.005)	(0.006)	
2017	0.011	-0.003	0.007	
	(0.003)***	(0.006)	(0.07)	
2018	0.010	-0.001	-0.002	
	(0.003)***	(0.007)	(0.007)	
2019	0.008	0.000	-0.006	
	(0.003)**	(0.006)	(0.006)	
2016 Mean	0.055	0.086	0.076	

Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). For grade 10 we report the base estimates of $\theta(t)$. The reported means are for the year 2016. Standard errors clustered on school reported in parentheses. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A8: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher Awarded GPA by Grade, by Track

	Grade 10	Grade 11	Grade 12	Grade 13
Academic Track N=940661				
2014	-0.099	0.020	0.036	0.060
	(0.007)***	(0.013)	(0.014) **	(0.013)***
2015	-0.032	-0.010	-0.024	0.004
	(0.007)***	(0.012)	(0.014)*	(0.012)
2017	0.015	0.006	0.048	0.094
	(0.007)**	(0.012)	(0.012)***	(0.012)***
2018	0.047	-0.012	0.030	0.085
	(0.007)***	(0.013)	(0.014)**	(0.013)***
2019	0.065	-0.009	0.009	0.121
	(0.007)***	(0.014)	(0.016)	(0.014)***
2016 Mean Level	4.198	4.272	4.197	4.075
Vocational Track N=678537				
2014	-0.093	0.019	0.069	
	(0.007)***	(0.013)	(0.014) ***	
2015	-0.029	-0.020	0.015	
	(0.007)***	(0.012)*	(0.013)	
2017	0.009	0.055	0.096	
	(0.007)	(0.012)***	(0.011)***	
2018	0.040	0.041	0.080	
	(0.007)***	(0.013)***	(0.013)***	
2019	0.056	0.068	0.105	
	(0.007)***	(0.013)***	(0.014)***	
2016 Mean Level	4.198	3.776	3.867	

Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). For grade 10 we report the base estimates of $\theta(t)$. GPA is standardized is standardized the estimates can be read in standard deviations. The reported mean levels are for the year 2016. Standard errors clustered on school reported in parentheses. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A9: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher awarded GPA with "Results" for No-grade Students Included

	Grade 10	Grade 11	Grade 12	Grade 13
Academic Track N=940661				
2014	-0.094	0.015	0.031	0.050
	(0.007)***	(0.013)	(0.013)**	(0.012)***
2015	-0.033	-0.010	-0.022	0.005
	(0.007)***	(0.011)	(0.013)*	(0.012)
2017	0.012	0.006	0.046	0.090
	(0.007)*	(0.012)	(0.012)***	(0.012)***
2018	0.044	-0.013	0.024	0.076
	(0.007)***	(0.013)	(0.014)*	(0.012)***
2019	0.063	-0.011	0.004	0.108
	(0.007)***	(0.014)	(0.015)	(0.014)***
Vocational Track N=678537				
2014	-0.088	0.016	0.063	
	(0.007)***	(0.013)	(0.014) ***	
2015	-0.030	-0.017	0.016	
	(0.007)***	(0.012)	(0.013)	
2017	0.007	0.051	0.083	
	(0.007)	(0.013)***	(0.011)***	
2018	0.036	0.035	0.071	
	(0.007)***	(0.013)***	(0.013)***	
2019	0.053	0.060	0.095	
	(0.007)***	(0.013)***	(0.014***	

Notes: Notes: Notes: Authors' calculations from Norwegian Registry data The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). For grade 10 we report the base estimates of $\theta(t)$. GPA is standardized is standardized the estimates can be read in standard deviations. Standard errors clustered on school reported in parentheses. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.

Table A10: Difference-in-Differences Estimates of the Impact of the New Absence Policy on Teacher Awarded GPA, by Selected Demographic Characteristics, by Track

	Girls	Boys	Immigrants	Native Born	Low income	Non-low income
Academic Track						
2014	0.060 (0.012)***	0.022 (0.014)	0.053 (0.024)**	0.038 (0.010)***	0.079 (0.021)***	0.034 (0.010)***
2015	-0.005 (0.011)	-0.008 (0.013)	0.012 (0.022)	-0.012 (0.009)	0.001 (0.019)	-0.010 (0.009)
2017	0.051 (0.011)***	0.053 (0.012)***	0.058 (0.023)**	0.052 (0.008)***	0.074 (0.020)***	0.061 (0.009)***
2018	0.055 (0.013)***	0.018 (0.013)	0.021 (0.024)	0.042 (0.010)***	0.001 (0.019)	0.053 (0.010)***
2019	0.052 (0.014)***	0.035 (0.016)**	0.024 (0.027)	0.052 (0.012)***	0.079 (0.021)***	0.062 (0.012)***
2016 Mean Level	4.280	4.044	3.803	4.228	3.804	4.240
N	499231	441430	122295	818366	142002	798659
Vocational Track						
2014	0.047 (0.015)***	0.041 (0.014)***	0.038 (0.028)	0.046 (0.011)***	0.057 (0.021)***	0.051 (0.012)***
2015	-0.007 (0.014)	0.004 (0.013)	0.001 (0.025)	-0.001 (0.010)	-0.018 (0.019)	0.009 (0.011)
2017	0.072 (0.014)***	0.081 (0.013)***	0.124 (0.026)***	0.071 (0.010)***	0.119 (0.020)***	0.073 (0.010)***
2018	0.067 (0.015)***	0.056 (0.014)***	0.087 (0.026) ***	0.059 (0.011)***	0.049 (0.021)**	0.070 (0.012)***
2019	0.075 (0.014)***	0.096 (0.016)***	0.112 (0.028) ***	0.084 (0.012)***	0.080 (0.022)***	0.093 (0.012)***
2016 Mean Level	4.003	3.706	3.415	3.882	3.528	3.920
N	305578	372959	88118	590419	131505	547032

Notes: Notes: Authors' calculations from Norwegian Registry data. The difference-in-differences estimates follow (1) with additional controls for year, grade, age, gender, field of study, mother's education and school fixed effects, family income, and indicators for immigrant households and mother's continent of origin (immigrants only). The reported means are for the year 2016. GPA is standardized is standardized the estimates can be read in standard deviations. The reported mean levels are for the year 2016. Standard errors clustered on school reported in parentheses. *, **, ** denote statistical significance at the 10, 5 and 1 precent levels respectively.