Course-Taking Incentives, College Admissions, and the Human Capital Allocations of Students*

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Abstract

We study how targeted educational incentives shape human capital formation, educational trajectories, and labor market outcomes. Exploiting a nationwide reform in Norway that removed bonus credits from university admission scores for taking science and advanced specialization courses in high school, we provide novel evidence of how marginal changes in course-level incentives affect students' academic decisions and long-run career paths. Using population-wide administrative data and a dose-response difference-in-differences design, we show that students more exposed to the reform shift away from advanced science and specialization courses and toward easier alternatives. This substitution leads to modest GPA gains but lower admission scores and reduces completion of prerequisite courses required for selective programs. As a result, affected students experience a contraction in their higher-education opportunity sets, sort into less selective programs, and become less likely to complete master's degrees, especially in STEM fields. These educational adjustments translate into economically meaningful reductions in realized earnings at age 30. We further show that instead of improving outcomes the policy worsens them for the whole distribution, with larger losses among the most exposed. Survey evidence suggests that these responses operate through both perceived academic costs and unequal awareness of the admissions system. Our findings show that seemingly modest changes in admissions incentives can reshape skill investment, educational sorting, and long-run labor market outcomes.

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1 Introduction

Students often make educational decisions based on the perceived returns to different courses and subjects (e.g., Arcidiacono et al., 2012; Kirkeboen et al., 2016; Wiswall and Zafar, 2015). However, these perceived returns are shaped in part by institutional features of the education system, and may not fully reflect underlying labor market demand. When these incentives are misaligned, early human capital investments may lead to inefficient allocation of skills, affecting both the composition of the workforce and the distribution of talent across sectors.

To better align human capital supply with labor market demand, policymakers typically consider two broad approaches: increasing the returns to specific fields or lowering the relative costs of enrollment. While raising returns is difficult, reducing costs — whether financial, institutional, or psychological — is more feasible but may have ambiguous effects. On the one hand, lower costs can encourage entry into high-demand fields, potentially alleviating shortages and improving efficiency (e.g., Joensen and Nielsen, 2009). On the other hand, lower costs may also distort sorting and effort, inducing students to enroll in programs misaligned with their interests or abilities and potentially weakening skill development. Designing policies that correct distortions without creating new ones remains a central challenge in education policy.

This paper studies how targeted changes in the incentives to enroll in specific courses shape students' educational and career trajectories. We exploit a Norwegian reform that eliminated college admission bonus points awarded for specific science and advanced specialization courses in high school. By reducing the admission advantage associated with these subjects, the reform effectively increased the relative cost of enrolling in them and weakened incentives to take advanced coursework. Using population-wide administrative data, we examine how this policy shift affected course selection and performance in high school, access to higher education, and long-run labor market outcomes. Our analysis traces both the immediate behavioral responses to the policy and the downstream consequences for skill composition and labor market attainment.

The institutional setting provides a natural environment to study these mechanisms. Admission to higher education in Norway is centralized and primarily determined by high school GPA. In addition to grades, students historically received bonus points for completing certain courses, particularly in mathematics, natural sciences, and advanced specialization. These bonus points increased students' college admission scores and thereby lowered the effective cost of enrolling in such courses.[1] In 2009, the government sharply reduced these bonus points, substantially weakening the incentive to enroll in advanced science coursework.

We exploit variation in how strongly students were exposed to this reform to identify how students adjust their course choices, educational attainment, and subsequent labor market outcomes when the cost of enrolling in specific subjects rises. Using pre-reform cohorts, we estimate how observable student characteristics predict enrollment in courses that generated admission bonus points. We then use these estimates to construct a predicted exposure measure that captures how likely each student would have been to take such courses under the pre-reform system. When the reform reduced bonus points, students with higher predicted exposure were more strongly affected by the policy. We leverage this variation using a dose-response difference-in-differences framework that compares outcomes before and after the reform across students with different levels of predicted exposure.

We supplement our administrative analysis with a survey designed to examine how current high school students perceive and respond to changes in educational incentives. By directly eliciting how students make course selection decisions, the survey provides evidence on the mechanisms underlying our main findings.

Our central finding is that small, targeted changes in high school course incentives can meaningfully shift students' educational trajectories and long-term labor market outcomes. This has implications for understanding how incentive structures shape skill supply and the allocation of talent. We organize the core results around three findings.

First, students adjust their course portfolios in ways that reduce both admission score and prerequisite completion. They substitute away from the most advanced science and specialization courses toward easier alternatives. Although this substitution yields modest grade improvements, the gains are too small to offset the loss of bonus points, leading to lower admission scores. In addition, because many selective programs require specific advanced specialization and science courses, these shifts also reduce prerequisite completion and the

[1] Earning one additional bonus point had a much larger effect on the admission score than improving a single course grade from a D to an A.

set of high-quality programs for which students are eligible, constraining access even as admission thresholds fall mechanically due to the system-wide decline in bonus points.

Second, these changes primarily reallocate students across programs and fields rather than reducing college enrollment. Specifically, we find little evidence of a meaningful change in overall BA enrollment. Instead, greater exposure to the reform shifts students toward less selective and lower-quality programs and reduces master's degree completion, particularly in STEM fields.

Third, the educational reallocation translates into meaningful labor market consequences. Using earnings records through 2023, we document reductions in realized earnings at age 30 for students more exposed to the reform, consistent with shifts away from higher-return programs and fields with steeper career ladders.

A key question in this setting is how the reform affects outcomes once the admissions system adjusts in general equilibrium. Because university admissions operate through a centralized, capacity-constrained clearing system, a system-wide reduction in bonus points mechanically lowers admission scores and leads admission thresholds to adjust. We document that program-level admission cutoffs decline following the reform, including in selective STEM programs. However, this adjustment does not translate into broader access to high-return programs, in part because prerequisite requirements remain binding in these programs and students' admissions scores drop by more on average than the thresholds. Instead, outcomes decline across the distribution, with larger losses among the most exposed. While this implies an improvement in income equality, it is driven by reductions at the top of the distribution rather than gains at the bottom.

Our survey complements these results with three main findings that speak to information about bonus points, decision weights in course selection, and perceived academic costs. First, awareness of the bonus point system varies substantially across current high school students. Those with higher GPAs and more educated fathers are significantly more likely to understand how the system works, while lower-performing and low SES students are less informed and may miss opportunities to improve their admission prospects. Second, students currently enrolled in school—who face fewer opportunities to earn bonus points—place less weight on the incentives when selecting courses, instead emphasizing personal interest, expected grades, and job prospects. Third, in a hypothetical conjoint experiment, students are willing to switch into bonus-point courses when doing so does not entail a drop in expected grades. However, responsiveness declines sharply when switching is expected to lower performance.

Taken together, the survey evidence shows that bonus point incentives operate through both informational and perceived-cost channels. Because awareness is unequal and incentives are evaluated against expected grades, the policy is most salient for high-achieving and well-informed students. This helps explain why the removal of bonus points disproportionately affected advantaged groups and suggests that incentive-based admissions policies can unintentionally reinforce inequalities in access to selective educational tracks.

This paper studies how targeted educational incentives shape students' course-taking decisions and long-term outcomes. By examining how changes in the relative payoff of individual high school courses affect behavior, we show that policy-engineered incentives can meaningfully redirect educational investments well before college entry. These shifts have persistent consequences for postsecondary sorting, STEM attainment, and earnings, even when curricula, grading standards, and degree-level returns remain unchanged. The paper advances several strands of literature.

First, we contribute to a large literature on educational choice and human capital investment that emphasizes expected returns, preferences, beliefs, and constraints as key drivers of field selection and degree attainment (e.g., Wiswall and Zafar, 2015; Bordon and Fu, 2015; Kirkeboen et al., 2016; Zafar, 2013). This work shows that anticipated earnings, perceived ability, and risk considerations shape educational decisions. We extend this literature by highlighting admissions incentives as an additional component of the choice environment. Our results demonstrate that institutional rules embedded in admissions systems can influence educational investments independently of students' preferences and beliefs about returns, operating through course-level incentives rather than changes in curricula or grading.

Second, the paper contributes to research that treats course-taking as a key margin of educational investment, particularly earlier in the educational pipeline. In economics, this literature emphasizes learning about ability, expectation formation, and tradeoffs between perceived difficulty and returns in course selection (e.g., Zafar, 2013; Wiswall and Zafar, 2015; Kirkeboen et al., 2016; Stinebrickner and Stinebrickner, 2012; Altonji et al., 2016). Parallel work in education and psychology similarly highlights the roles of perceived costs, expected success and fit, peer environments, and information frictions in shaping academic choices (e.g., expectancy-value theory; Eccles and Wigfield, 2002; social cognitive career theory; Lent et al., 1994). Related work on STEM persistence shows an important role for peer environments, early exposure, and advising and guidance (Carrell et al., 2013; Patacchini et al., 2017; Mouganie and Wang, 2020; De Philippis, 2023; Canaan and Mouganie, 2023) We extend this literature by providing causal evidence that high school course portfolios respond to policy-induced incentives and that these responses have lasting consequences for prerequisite completion, program eligibility, and subsequent educational trajectories. This highlights course-taking as a central, policy-sensitive margin of human capital investment.

Third, our analysis relates to work studying how curricular policies affect educational and labor market outcomes. Most closely, Joensen and Nielsen (2009) examines a Danish reform that expanded access to advanced mathematics by relaxing a binding curricular constraint in the course bundle, lowering the cost of taking advanced math for students who were previously deterred by required course combinations. In contrast, the reform we study raises the cost of advanced and specialized coursework by removing admissions-point subsidies attached to these courses within a centralized, capacity-constrained admissions system. As a result, the students in our setting are those on the margin of taking demanding coursework, who adjust their choices when the admissions incentives attached to these courses are reduced. This distinction matters for both interpretation and generalization. Whereas Joensen and Nielsen (2009) studies the effects of expanding feasible pathways into advanced coursework, we isolate how changes in incentive strength within an otherwise unchanged curriculum reshape preparation decisions, program eligibility, and subsequent sorting in higher education. A key feature of our setting is that many of the affected courses also serve as prerequisites for selective programs, so weakening these incentives affects not only admission scores but also whether students qualify for particular programs. Moreover, because admissions operate through a centralized and capacity-constrained system, the reform induces equilibrium adjustments in admission thresholds, which we document. We trace how these incentive changes propagate through multiple margins, from course-level choices in high school and track selection to program eligibility, college enrollment, master's degree completion, and earnings at age 30.

Fourth, a separate literature studies how grading standards and academic leniency shape student behavior and educational outcomes (e.g., Bowden et al., 2023; Hvidman and Sievertsen, 2021; Ahn et al., 2024; Dee et al., 2019; Figlio and Lucas, 2004; Denning et al., 2025). This work shows that easier grading can raise measured achievement without improving underlying skills, widen achievement gaps, and negatively affect later labor market performance. It also finds that information about grades can steer students toward courses with inflated grading, particularly among lower-ability students (Bar et al., 2009). Our paper highlights a distinct channel through which academic investment responds to incentives. Rather than changing grading standards or evaluation practices, the policy we study alters incentives to enroll in more demanding courses while leaving performance incentives within courses unchanged. We show that removing these performance-neutral incentives shifts students away from advanced coursework, with consequences for preparation, program eligibility, and labor market outcomes.

Finally, the paper contributes to the literature on STEM participation, talent allocation, and educational match, which emphasizes how preparation and early sorting shape long-run outcomes (e.g., Arcidiacono et al., 2016; Stinebrickner and Stinebrickner, 2014; Black et al., 2021; Hvide, 2003; Nechyba, 2006; Dillon and Smith, 2020; Black et al., 2023; Dalla-Zuanna et al., 2025; De Philippis, 2023; Canaan and Mouganie, 2023). We show that course-level incentives materially affect STEM course-taking and prerequisite completion, leading to persistent differences in access to selective programs, degree attainment, and labor market outcomes. These findings highlight how educational policies that appear narrow or intermediate in scope can nonetheless alter the supply and allocation of skills, with lasting consequences for inequality in access to opportunity and for labor market performance.

2 Background

To examine how changes in incentives to enroll in certain courses influence students' educational choices and labor market outcomes, we leverage a natural experiment in Norway. Specifically, we study a policy change that increased the relative cost of enrolling in science and advanced specialization courses in high school. This section provides an overview of the Norwegian education system, with a focus on the university application process and the 2009 reform that reduced incentives for taking science and specialization courses in high school.

The Norwegian Education System. The Norwegian education system requires 10 years of compulsory schooling, starting at age six. Children must attend the school closest to their residence. Funding for schools is provided by the municipality of residence on a per-student basis, ensuring equal resources for all schools within each municipality. After completing compulsory education, students can enroll in upper secondary school for 3 to 4 years on a voluntary basis. Most students enroll in upper secondary school, and admission is highly competitive (Landaud et al., 2024).

Upper secondary education in Norway has two main pathways: an academic track that provides university matriculation and a vocational track that leads to a trade certificate (with the option to obtain university matriculation rights through supplementary coursework). Approximately 50% of students choose each track. Admission to high schools is competitive, based solely on students' GPA from compulsory schooling.

Within the academic track, students choose among five study programs: specialization in general studies, sports and physical education, art, design and architecture, media and communication, and music, dance and drama.[2] Specialization in general studies is the most common, enrolling roughly 70% of academic-track students. Within this program, students further specialize through their elective choices, either in science (realfag) or in language, social sciences, and economics (språk, samfunnsfag og økonomi).

All students complete a common core curriculum, and study programs differ primarily in the composition of required subjects and the menu of electives. Students must satisfy minimum instructional-hour requirements within their chosen program, but they retain flexibility to take electives across programs. Elective choices are typically concentrated in the final year of high school, when many of the advanced science and specialization courses relevant for university eligibility and bonus points are taken.

Higher education in Norway is primarily offered by public, tuition-free universities and colleges. Admission generally requires completing the academic track of upper secondary school and meeting minimum grade requirements.

Many selective university programs impose explicit high school course prerequisites, most commonly in mathematics and the natural sciences. For example, admission to programs in

[2]Media and communication was classified as a vocational program until 2016, after which it was reclassified to grant university matriculation rights.

medicine, engineering, and several STEM and quantitatively intensive fields requires completion of specific combinations of advanced mathematics, physics, chemistry, and biology courses. Appendix Table A1 documents the prerequisite requirements across university programs and shows that such requirements are widespread among the most selective majors.

Norwegian universities follow the Bologna Process, offering three-year bachelor's degrees and five-year integrated bachelor's-master's programs. When the number of applicants exceeds the number of available spots, selection is based on admission scores (detailed below). Education is free at all levels, and students receive financial support from the Norwegian State Educational Loan Fund, which provides a combination of generous loans and grants. Support is not means-tested based on parental income, but the amount is reduced if students themselves earn income during their studies above a certain amount.

The college application process is centralized through the Norwegian Universities and Colleges Admission Service, which handles admissions for all universities and most university colleges. Students apply for specific fields of study at particular institutions (e.g., engineering at the University of Oslo) and can list up to 16 different combinations. Conditional on meeting any subject-specific prerequisites, admission is exclusively based on a student's admission score (GPA plus bonus points), with students ranked by score and placed in the highest-ranked program on their list for which they qualify. The admission rule is a strategy-proofed procedure.

Admission thresholds for specific programs tend to be persistent over time, reflecting stable capacity constraints and applicant demand. Past cutoffs are therefore informative about future access and may shape students' expectations. Importantly, many of the course choices that affect admission scores and bonus points are made well before applications are submitted and require advance planning. In addition, applicants typically apply to multiple programs across fields and institutions rather than targeting a single cutoff (Kirkeboen et al., 2016). Students are therefore unlikely to optimize against a single known cutoff for a specific program. Instead, bonus points expand the range of programs for which a student is eligible rather than determining admission to any one program. This creates incentives to accumulate bonus points even when a student is not close to a particular threshold, given uncertainty across multiple applications. Additionally, there is uncertainty in high school GPA because it is influenced by end of school exams in randomly selected subjects (Landaud et al., 2024).

Course Incentive Reform. Individual course grades in high school range from 1 to 6 (only integer values), with the GPA calculated as the average of all course grades that the students have received in high school. The university admission score — the sole metric on which universities and colleges rank students — is $10 * AverageGPA$ (rounded to two decimal places) which ranged from 10 to 60 plus the total number of earned bonus points.

The government introduced bonus points in 1998 to encourage specific demographic groups to pursue higher education and to steer students toward certain major choices. Most of these points are tied to characteristics that students cannot easily influence, such as age, military service, and gender.[3]

However, one category of bonus points directly rewards academic choices: students receive additional admission points for completing specific high school courses in science and advanced specialization subjects.[4] These academically targeted incentives are central to our analysis because they directly shape course selection and, through prerequisite requirements and admission score, access to higher education programs. Importantly, these are precisely the incentives that were reduced in the reform passed in 2006 and implemented for the 2009 graduating cohort.

More specifically, prior to the reform, students earned science points by completing advanced courses in mathematics, biology, physics, and chemistry. Advanced specialization points were awarded for taking in-depth elective courses beyond the basic level, with students receiving 2 points for each subject that had at least two levels. Some courses provided both types of points. For instance, an advanced physics course awarded both 1 science point and 2 specialization points, whereas an advanced history course provided 2 specialization points but no science points. These bonus points carried substantial weight in the admission process.

Students could earn up to ten bonus points through their selection of science and advanced specialization courses—six points from science courses and four points from specialization courses. Because increasing the average letter grade across all courses by one step raises the

[3]Gender points are awarded to either gender for specific programs where that gender is underrepresented. Additional information is available from the Norwegian Universities and Colleges Admission Service is available at the link here.

[4]Rules for science points are set out in the 2005 regulation for higher education admission (Chapter 7) for cohort 2006/07; rules for specialization points are set out in Chapter 9 of the same regulation. See ğ7-14 and ğ9-2.

admission score by ten points (e.g., moving from all B's to all A's raises the score from 50 to 60), earning the full ten bonus points could offset an entire grade step across all subjects. This made bonus points a powerful lever for improving admission chances, as they could substantially raise rankings without higher grades in core subjects.

In 2006, the Norwegian government changed the bonus point system by introducing two major adjustments to the scheme. First, they completely removed bonus points for advanced specialization courses. Second, they reduced the number of science bonus points an individual could get from 6 to 4.[5] As a result, the potential bonus points available to students through strategic course selection dropped from 10 to 4. These adjustments effectively increased the cost of enrolling in advanced specialization and science courses without directly changing the incentives to exert effort or earn higher grades. The law was passed in 2006, but did not affect students already enrolled in high school. Therefore, the first cohort affected by the reform consists of students who entered high school in 2006 and graduated in 2009.

Figure 1 illustrates the reform's impact on bonus point accumulation. Panel 1a shows a sharp drop in the average number of bonus points earned, from about five in the pre-reform period to just one post-reform. Panel 1b highlights the resulting shift in the bonus point distribution: whereas pre-reform students earned between 0 and 10 points — with many clustered at the upper end — post-reform students earned no more than four points, and many received none at all. Panel 1c shows that this change is specific to course-based incentives: bonus points from other sources, such as age-based or demographic criteria, remain stable over time and are quantitatively small. The decline in total bonus points reflects both the elimination of specialization points and a reduction in science bonus points following the reform. Although science courses remain the only way to accumulate multiple bonus points after the reform, doing so requires a more science-intensive course portfolio than before, which helps explain why science-based bonus points also decline, albeit by less than specialization points, following the reform.

While the reform substantially reduced incentives to pursue science and advanced specialization courses by lowering the associated bonus points, most STEM-oriented higher-

[5]See 2008 "Forskrift om endring i forskrift om opptak til høyere utdanning", chapter 7. Prior to the reform, twelve science courses awarded bonus points: six courses granted 1 additional point and six granted 0.5 points. After the reform, only two courses–advanced Mathematics and advanced Physics–continued to grant 1 additional point, while all remaining science courses granted 0.5 points.

education programs continued to require completion of the most advanced mathematics and science courses as prerequisites. As a result, students still needed these courses to qualify for selective programs, but now faced weaker rewards for taking them — creating a potential mismatch between admissions incentives and program prerequisites. This creates an ex ante ambiguity: binding prerequisite requirements could limit the impact of reduced bonus-point incentives, while weaker incentives could nonetheless induce students to forgo advanced courses despite their role in program eligibility.

3 Data

We use comprehensive administrative registers covering the full population of Norwegian residents. A unique personal identifier allows us to follow individuals over time and across data sources. The first cohort exposed to the points reform graduated from high school in 2009. To ensure adequate coverage before and after the reform, we focus on students graduating between 2005 and 2011, which covers four pre-reform cohorts and three post-reform cohorts.[6]

Our key administrative data come from the primary school, high school, and university registers. The high school register contains detailed information on school attended, courses taken, and grades received, allowing us to observe whether students enrolled in bonus-point-eligible courses and whether they met prerequisites for specific college programs at the time of application. We link this to the primary school register, which provides students' GPA at the time of high school admission — serving as a measure of baseline ability in our dose-response framework. Finally, we incorporate university register data on college enrollment, major choice, and institution attended.

We merge the education data with information from the demographic, income, and population registers. Annual earnings are measured as pre-tax labor income (wages plus self-employment income). Together, these data provide detailed information on age, gender, parental characteristics, socioeconomic conditions, and labor market outcomes at age 30, more than ten years after high school completion. Descriptive statistics can be found in Appendix Table A2.

The richness of our data allows us to examine a wide range of outcomes, capturing

[6]Students typically start high school at age 16 and graduate at 19.

various margins of student adjustment in response to the reform. We begin by documenting changes in high school course-taking patterns following the reform, focusing on enrollment in science and advanced specialization courses. Because course enrollment is not mechanically affected by the change in bonus-point rules, these outcomes capture adjustments in students' course-selection decisions in response to the altered incentives.

Next, we assess whether students respond to weaker incentives by adjusting the difficulty of the courses they choose. If students substitute away from science and advanced specialization courses, such substitution should occur primarily toward courses where it is easier to earn high grades. To examine this margin, we construct a measure of course difficulty based on relative grades.

We measure how "lenient" a student's elective bundle, denoted E_i, is relative to mandatory courses, denoted M, using only pre-reform course means. Formally,

$$Y_i^{\text{pre}} = \frac{1}{N_i} \sum_{c \in E_i} \left(\frac{\bar{G}_c^{\text{pre}}}{\bar{G}_{\mathcal{M}}^{\text{pre}}} \right). \tag{1}$$

Values above one indicate that the electives selected by i are, on average, historically graded more generously than mandatory subjects; values below one indicate the opposite. Because both numerator and denominator are anchored in pre-period grades, Y_i^{pre} isolates the compositional tilt of course choice toward historically easier or harder electives.

This provides a benchmark capturing how challenging each selected course is relative to general academic performance in the pre-period. To compute the difficulty of the courses students choose after the policy change, we assign these pre-reform average relative grades to all students based on the courses they select and then compute the average difficulty of their course portfolio at the student level. The relative grade measure allows us to isolate the difficulty of a particular class, accounting for possible selection into different classes by fixed ability. Importantly, because the relative-grade measure is constructed using pre-reform course-level benchmarks, it is not mechanically affected by the reform and reflects changes in the difficulty of courses students choose rather than changes in grading standards (see Appendix Figure A1 for the relative grade of each course).

In addition to course-level adjustments, we examine whether students respond along a broader margin by sorting into different high-school tracks, in particular between science-oriented and non-science programs. Track choice shapes the menu of advanced courses students typically take–and thus bonus point accumulation and prerequisite completion–making it a natural margin for adjustment when incentives change.

Finally, we analyze performance effects by examining changes in high school GPA and college admission scores—two key indicators of academic success.

To help disentangle mechanical effects of the policy from behavioral responses, we supplement these performance analyses with counterfactual exercises that hold the pre-reform bonus point rules fixed while applying them to observed post-reform course portfolios. This allows us to isolate how much of the change in admission scores reflects altered incentives mechanically versus changes in students' course-taking decisions.

Fourth, we examine the reform's impact on higher education outcomes, focusing on how it influenced the set of programs students could access, the quality of the programs they ultimately attended, and whether they enrolled in a bachelor's program at all.

In terms of program availability, we define each student's opportunity set as the set of college-program combinations for which their final admission score meets the admissions cutoff and for which they satisfy the prerequisites. The removal of bonus points directly reduced admission scores for affected students, potentially restricting access to certain programs, particularly those with highly competitive entry requirements or stringent prerequisites. This shift may have led some students to enroll in less selective institutions or programs.

We use two main measures of quality. First, we create a rank of programs based on the pre-reform thresholds required for admission to the program. We then impute this rank to students' post-reform program choices. Importantly, this measure is unaffected by any supply side changes in admissions thresholds because the ranks are imputed from the pre-reform period. Second, we use the academic credentials of a student's peers as an alternative proxy for program quality. Specifically, we measure quality using the jackknifed minimum high school GPA among students in the same cohort and program, excluding the individual's own GPA to avoid mechanical correlation. Because admission is determined by a cutoff rule, the lowest admitted GPA provides a direct measure of program selectivity. If the reform systematically lowered admission scores or altered eligibility sets for certain students, it may have caused sorting effects, pushing them into programs with peers who had lower GPAs. Alternatively, if students adjusted their course selections strategically after the reform, they may have been able to maintain access to high-quality programs despite lower admission scores. This measure is also not affected by changes in the thresholds because it uses students' peers academic performance in high school, not their admissions points.

Because admissions are centralized and capacity constrained, changes in applicants' admission scores can also generate general-equilibrium adjustments in program cutoffs. We therefore examine changes in admission thresholds directly and document how cutoffs shift following the reform. To ensure that our measures of program quality are not mechanically driven by these equilibrium changes, we anchor quality to pre-reform admission criteria and peer characteristics, rather than contemporaneous cutoffs.

In terms of educational attainment, we examine three higher education outcomes: BA enrollment, STEM Master's degree attainment, and MA degree attainment by age 25.

Fifth, we assess labor market implications using realized earnings from administrative tax records. We examine pre-tax earnings at age 30 among attached workers.[7] Prior work shows that earnings observed around age 30 exhibit substantial persistence and are informative about longer-run income trajectories, while avoiding many of the biases associated with very early-career earnings.[8] Earnings are defined as the sum of cash wages and salaries, including taxable sickness and parental leave benefits.

We complement our administrative analysis with evidence from a survey of 491 Norwegian high school students. This survey provides a rare opportunity to examine the mechanisms behind students' course selection decisions; insights not visible in administrative data. We assess students' awareness of the bonus point system, the extent to which it influenced their academic choices, and their perceptions of the difficulty and value of science and advanced specialization courses. To probe responses to external incentives further, we include hypothetical scenarios in which students choose between course options under varying bonus point structures. By combining population-wide register data with direct student responses, this analysis offers a more comprehensive view of how incentive structures shape educational decision-making.

The survey was fielded by Norstat in 2025 using the Norstat Panel, which is designed to

[7]Attached workers are defined as individuals with annual labor income above 2G, a commonly used threshold to identify individuals with a minimum degree of labor market attachment. The *Grunnbeløp* (G) is the base amount used to determine benefit levels and eligibility thresholds in Norway's social insurance system; in 2009, 1G was NOK 72,881.

[8]See Solon and Haider (2006) and Bihlmayer and Lindquist (2020).

be approximately nationally representative along certain key dimensions including gender, age, and geography. For this project, Norstat sampled respondents in the 15-19 age range and implemented soft quotas on gender and region, meaning that invitation volumes were targeted to match population shares while allowing flexibility during fieldwork. According to Norstat, the panel achieves near-perfect geographic representativeness, and the gender composition is very close to population benchmarks (54% boys and 46% girls in the panel, compared to 52% and 48% nationally). Our sample is 59% female, which is consistent with higher female representation in the academic track in high school (Almås et al., 2016). Respondents were screened to ensure that all participants were currently attending upper secondary school.

4 Empirical Specification

To examine how changes in course-level incentives affect students' course selection and subsequent educational and labor market outcomes, we rely on a dose-response difference-in-differences specification.

Our approach exploits the fact that students were differentially exposed to the reform, depending on how many science and advanced specialization courses they would have taken in the absence of the policy change. Because this counterfactual is not directly observed for post-reform cohorts, we construct a predicted measure of exposure using pre-reform data.

Specifically, we estimate a prediction model on pre-reform cohorts that relates bonus point accumulation to predetermined characteristics such as middle school GPA, parental education, and school background. We then apply this model to post-reform cohorts to generate a counterfactual prediction of how many bonus points each student would have earned under the pre-reform rules. The resulting predicted bonus points that students would have earned serves as our measure of treatment intensity. Variation in predicted exposure allows us to compare outcomes before and after the policy change across students with different baseline propensities to take bonus-point-eligible courses, thereby identifying how targeted incentives shape high school course choices, subsequent educational opportunities, and longer-run labor market outcomes.[9]

[9]Students earned bonus points toward higher education simply by completing these courses. This design preserved effort incentives as admission scores continued to depend on GPA while encouraging enrollment in more demanding coursework.

Formally, we estimate the prediction model on pre-reform cohorts (graduating between 2005 and 2008) as follows:

$$BonusPoints_{ic} = \beta + \gamma X'_{ic} + \delta t + \theta_m + \alpha_{ms} + \Gamma_{hs} + \epsilon_{ic}, \tag{2}$$

where X'_{ic} is a vector of prediction variables that includes sex, primary school GPA, parental education (level and program), parental employment status, household income (per capita), birth month, and cohort while δt is the year trend, θ_m is municipality of birth fixed effect, α_{ms} is primary school fixed effect, and Γ_{hs} is high school fixed effects.

The results from this exercise are shown in Appendix Table A3, where many of the individual coefficients are strongly associated with students' eventual bonus points in the pre-reform cohorts. The overall strength of the prediction is high, with an F-statistic of 25.

To assess the quality of the treatment intensity measure, we examine its relationship with realized bonus points among students who graduated before the reform. As shown in Appendix Figure A2, predicted and actual bonus points in the pre-period are relatively strongly correlated, with a correlation coefficient of approximately 0.42. This indicates that the prediction model tracks bonus point accumulation in the pre-reform cohorts relatively well. The correlation is also consistent across years with predictions of the model in 2005 being similarly correlated with those in subsequent pre-reform cohorts (see Appendix Table A4). Additionally, we assess whether the predictive relationship underlying our treatment intensity measure is stable over time. A potential concern is that the mapping from predetermined characteristics to bonus point accumulation may change across cohorts, which would weaken the interpretation of predicted bonus points as a valid counterfactual for post-reform students. To examine this, Appendix Figure A3 applies the 2005 prediction model to each pre-reform cohort and compares predicted to realized bonus points. The model predicts observed bonus points equally well across all pre-period cohorts, indicating that the relationship between predictors and bonus point accumulation is stable over time.

We estimate the dynamic effects of the reform using the following event-study specification:

$$
\begin{aligned}
Y_{ict} = \alpha + \sum_{q=2005}^{2011} \pi_q \left(1[c = q] \cdot \widehat{\text{BonusPoints}}_{ic}\right) \\
+ \gamma X_{ic}^{'} + \widehat{\phi}_{pre}(2008 - c)\,\widehat{\text{BonusPoints}}_{ic} + \epsilon_{ict},
\end{aligned} \tag{3}
$$

where Y_{ict} is an outcome for individual i in cohort c at time t. The vector $X_{ic}^{'}$ is defined as above. Standard errors are clustered at the year x middle-school GPA bracket level, as the variation identifying our instrument primarily arises from middle-school GPA (Abadie et al., 2023).

Since our analysis spans multiple cohorts, we include $\widehat{\phi}_{pre}(2008 - c)\widehat{BonusPoints}_{ic}$ as a linear trend to account for potential systematic differences in outcomes across cohorts by predicted exposure (e.g., Jakobsen et al., 2019). This term allows outcomes to follow smooth, differential pre-reform trends across levels of predicted exposure, absorbing any such systematic differences present prior to the policy change (e.g., Wolfers, 2006; Roth, 2022). This term is estimated from the four pre-reform cohorts and addresses the concern that students with higher predicted exposure may have followed different trajectories even absent the reform.

With this control included, identification comes from deviations from these smooth pre-existing linear trends at the time of the reform. This specification therefore assumes that pre-reform trends evolve approximately linearly. To examine how sensitive our results are to this assumption, we also report estimates from specifications that omit this control. For most outcomes, the two approaches yield nearly identical results and the event studies show no evidence of differential pre-reform trends across exposure levels. A small number of secondary outcomes exhibit modest trends in the raw specification. For two of these outcomes, the event studies show clear breaks in the outcome trajectory at the time of the reform, consistent with a causal response to the policy change. For the remaining outcome, the differential trend continues smoothly through the reform period with no visible break, indicating no discernible policy effect. We report estimates from both specifications and discuss these cases explicitly when presenting the results.

To provide a more easily interpretable summary of the results, we also estimate a simpler difference-in-differences (DiD) specification that captures the average treatment effect in the post-reform period. This is implemented using the following model:

$$
\begin{aligned}
Y_{ict} = \alpha + \pi_1 \text{Post}_t + \pi_2 \widehat{\text{BonusPoints}}_{ic} + \pi_3(\text{Post}_t \times \widehat{\text{BonusPoints}}_{ic}) \\
+ \gamma X'_{ic} + \widehat{\phi}_{pre}(2008 - c)\, \widehat{\text{BonusPoints}}_{ic} + \epsilon_{ict},
\end{aligned} \tag{4}
$$

where Post_t is an indicator function that equals 1 if the cohort graduation year c is after 2008 (i.e., in the post-reform period), and 0 otherwise. The coefficient π_3 captures the average effect of the reform on the outcome variable Y_{ict} associated with the dosage measure $\widehat{\text{BonusPoints}}_{ic}$ in the post-reform period. Because the treatment varies continuously, our difference-in-differences estimates compare students who are more versus less exposed to the reform. Unlike a standard discrete DiD design, all students in our sample receive some degree of treatment, so identification comes from differences in treatment intensity rather than treated versus untreated groups.

Our empirical approach is a dose-response difference-in-differences specification. Because identification relies on comparisons across adjacent cohorts, we include cohort-specific linear trends to account for smooth underlying changes in course-taking and educational demand over time. In our preferred specification, identification therefore comes from differential deviations from these trends at the time of the policy change across students with different levels of predicted exposure, measured by the number of bonus points students would have received in the post-period had the policy not been implemented. To interpret these deviations as the causal effect of changing incentives in high school course selection, three key assumptions must hold: relevance, common trends, and the absence of confounding shocks.

Relevance requires that the predicted bonus points accurately capture the bonus points students would have received in the absence of the reform. We assess this using pre-reform cohorts, where both predicted and actual bonus points are observed. The first-stage results show a relatively strong relationship, with an F-statistic of 25 (Appendix Table A3) and a correlation coefficient of 0.42 between predicted and actual bonus points (Appendix Figure A2). This suggests that our prediction model, based on demographic and educational characteristics, captures meaningful variation in counterfactual bonus point exposure and supports the relevance of the treatment intensity for causal inference.

Appendix Table A3 further shows that treatment intensity is strongly increasing in prior academic achievement, with GPA emerging as a particularly strong predictor of predicted bonus point losses. As a result, the students most affected by the reform are disproportionately high-achieving and drawn from more advantaged socioeconomic backgrounds. This pattern highlights an important equity dimension: the original bonus point system conferred the greatest benefits on students who were already well-positioned in terms of ability, information, and family resources. In this sense, the reform not only altered academic incentives but also reduced a source of structural advantage, potentially contributing to a more equitable distribution of access to high-return educational opportunities.

Common trends require that, in the absence of the reform, students with different levels of predicted exposure would have followed similar outcome trajectories over time. While this assumption cannot be tested directly, we assess its plausibility using both descriptive analyses by cohort and event study estimates from our preferred specification. The descriptive analyses show little evidence for differential trends across bonus point terciles (see Figures 2a and 2b). Our event study estimates show no evidence of differential pre-trends prior to the policy change (Figure 3).

Because our design relies on a continuous difference-in-differences framework, in which treatment intensity is based on predicted bonus point exposure, the parallel trends assumption is slightly more demanding than in the standard binary DiD case (Callaway et al., 2024). In particular, it requires that, absent the reform, outcome trajectories would have evolved similarly across students facing different levels of predicted exposure, rather than simply between treated and untreated groups. To assess this, we examine whether pre-reform trends differ systematically across the distribution of treatment intensity. We partition students into terciles based on predicted bonus points and estimate event study coefficients separately for each group, measuring all effects relative to the middle tercile. As shown in Appendix Figure A4, students in both the top and bottom terciles closely track the pre-reform outcomes of the middle group, providing no evidence of differential pre-trends across treatment intensities. Additionally, it is important to note that the interpretation of our parameter of interest is a comparison to the lowest predicted bonus points. As a result, our difference-in-differences framework identifies the relative effect of the policy.

The absence of confounding shocks requires that no other policy changes or external events occurred at the same time as the reform that would differentially affect students by their predicted bonus points. We confirm that our predictions are based only on predetermined demographic and educational background variables and investigate whether any other major reforms or shocks coincided with the policy. We find no evidence of such confounding factors. The lack of differential pre-trends further supports the idea that unobserved shocks are unlikely to bias our estimates.

Because identification in our preferred specification comes from deviations from group-specific linear trends, this approach imposes a functional form assumption on the evolution of outcomes in the absence of the reform. In particular, it assumes that any systematic differences in outcome trajectories across exposure levels would have evolved smoothly over time. To ensure that this assumption is not driving our results, we also estimate all specifications without group-specific trend controls. Across outcomes, the estimated effects are very similar, and we present results from both specifications throughout the paper.

5 Results

In this section, we present our core results on how the removal of bonus point incentives affect students' educational choices and subsequent outcomes. We begin by examining changes in course-taking behavior, focusing on enrollment in science and advanced specialization courses that previously generated bonus points. We then study additional margins of adjustment, including shifts in the difficulty of the courses students choose and sorting across high-school tracks. Next, we analyze performance effects, examining changes in high school GPA and overall admission scores, and distinguishing mechanical effects of the policy from behavioral responses in course selection. We then assess downstream educational outcomes, including students' access to higher-education programs, the quality of the programs they attend, and completion of bachelor's and master's degrees. Finally, we evaluate longer-run labor market outcomes, including earnings at age 30.

Following the presentation of the main results, we conduct robustness and sensitivity analyses. We conclude the section by drawing on survey evidence to shed light on the mechanisms underlying students' course-selection responses.

Behavioral Response: Course Enrollment. To assess the descriptive validity of our study design, we plot cohort-level trends in the share of course hours allocated to specialization and science courses by tercile of predicted bonus points. Figure 2a shows specialization courses, and Figure 2b shows science courses. Both figures show a decline in course hours following the reform across all terciles. However, the decline is steepest for students in the top tercile of predicted bonus points, consistent with greater exposure to the reform.

Figure 3 presents our event study results based on Equation 3, using the number of courses that previously qualified for bonus points as the outcome variable. The figure highlights two key findings. First, there is no evidence of differential pre-trends across levels of predicted exposure, supporting the common trends assumption required for causal interpretation. Second, there is a sharp decline in the number of bonus point-granting science and advanced specialization courses taken directly following the reform, indicating an immediate response to the change in course incentives.[10] Event study estimates without linear trend controls are presented in Figure 4.

To facilitate interpretation of these results, Table 1 reports coefficients from a difference-in-differences specification estimating the effect of the reform on (1) the share of instructional hours that previously qualified for bonus points, (2) the share of instructional hours in hard science courses, and (3) the share of instructional hours in advanced specialization courses. The results indicate that students shifted away from both science and specialization courses in response to the reform, but that the drop was larger with respect to specialization courses. This is consistent with the reform, which removed all specialization points but kept the possibility of earning some science points.

Taken together, these results show a relatively strong behavioral response to the reform. There is no evidence of differential pre-trends across exposure groups, and course-taking declines sharply in the first cohort affected by the policy. The response is strongest among students with greater predicted exposure and is concentrated in specialization courses, where bonus points were fully removed. These patterns are consistent with students adjusting course portfolios in direct response to the change in admission incentives.

Behavioral Response: Course Difficulty and Track Choice. We next examine whether students adjusted along a substitution margin by changing the difficulty of the courses they selected. If students substituted away from science and advanced specialization courses following the reform, such adjustments should primarily occur toward courses that are easier

[10]Appendix Figure A5 shows no differential effects between men and women, despite potential differences in risk preferences, confidence, and behavior (e.g., Croson and Gneezy, 2009; Niederle and Vesterlund, 2007; Exley and Kessler, 2022; Hirshman and Willén, 2024).

to earn high grades in. To study this margin, we assign each high school course a relative difficulty score. The score is based on average grades in the pre-reform period normalized to the grades in the mandatory courses. We then compute the average difficulty of each student's course portfolio based on the courses they choose to take. Courses which have average pre-reform grades above the mandatory courses are greater than one and courses with pre-reform grades lower than the mandatory courses on average have scores less than one. Figure 5 presents event-study estimates from our preferred specification with a linear control, while Table 2 reports the corresponding difference-in-differences estimates; Figure 6 shows the event-study estimates without the control.

The results from this analysis provide clear evidence of substitution toward less demanding courses. Students shifted away from science and advanced specialization subjects and toward elective courses associated with higher average grades, by approximately 0.002 grade points relative to a mean of 1.01. While the absolute effect appears small, the measure has a standard deviation of .047 so the marginal effect per predicted bonus point represents a 4.3 percent of a SD increase in the relative grade portfolio. Because course difficulty is benchmarked using pre-reform grades, these patterns reflect changes in course choice rather than changes in grading standards.

To unpack these changes further, we examine course-level responses and find clear evidence of substitution following the reform. Enrollment in advanced science courses—Advanced Math 2, Chemistry 2, Physics 2, Biology 2, and Geo-science 2—declines by more than one percentage point per predicted bonus point reduction (see Table 2 Column 3). Although these courses continue to yield science bonus points, the reform eliminates their additional specialization premium, reducing their marginal return relative to other subjects. In response, students reallocate toward lower-level science courses, which continue to yield bonus points (see Table 2 Column 4) , as well as toward non-science subjects such as English and other language courses, which do not yield bonus points but are typically less demanding and carry lower performance risk (see Table 2 Column 2). This pattern indicates substitution away from advanced courses once their marginal admission advantage is reduced. Because advanced science courses serve as prerequisites for many selective higher-education programs, these substitutions have the potential to affect subsequent program eligibility.

In addition to course-level substitution, we examine whether students adjusted along a broader margin by sorting into different high school tracks (see Table 2 Column 5). As discussed in Section 2, within the main academic program students specialize either in science or in language, social sciences, and economics. We find evidence of movement away from science-oriented specializations toward non-science alternatives, indicating that the reform affects not only course-level choices but also broader program-level decisions.

Taken together, these results show that the removal of bonus point incentives leads students to move away from science and advanced specialization courses toward easier courses and less science-oriented specializations, implying that students' course-taking choices are highly responsive to admission incentives.

Performance Effect. To examine how the course adjustments documented in the previous subsection translate into academic performance, we estimate our event study specification using high school GPA and college admission scores as outcomes. Figure 7 shows our preferred event-study estimates with the linear time trend; Figure 8 shows the corresponding estimates without the control

Two main findings emerge. First, GPA increases slightly following the reform, consistent with students shifting toward easier courses in response to weaker incentives. However, this effect is economically small.[11] Second, admission scores decline sharply at the time of the reform, indicating that the modest GPA increase associated with course substitution is insufficient to offset the mechanical loss of bonus points.

To aid interpretation, Table 3 reports estimates from the simpler difference-in-differences specification based on Equation 3. The reform increases GPA by 0.33 per predicted bonus point, but despite this, admission scores fall by 0.24 per predicted bonus point. The relatively muted GPA response likely reflects a ceiling effect. Following the reform, students shift into relatively less demanding courses, which could in principle lead to higher grades. However, the students most exposed to the policy already have relatively high baseline GPAs, leaving limited scope for further improvement. As a result, any gains from moving into easier courses are likely to be modest in the aggregate.

To determine whether the decline in admission scores reflects purely mechanical rule changes or active behavioral responses, we decompose the total change in bonus points into

[11] In the event study without the linear time trend (Figure 8), GPA exhibits a declining pre-trend, with the reform introducing a break in this trajectory. Our baseline specification accounts for this by including a linear pre-reform trend (e.g., Wolfers, 2006; Roth, 2022); we report both specifications for transparency.

its mechanical and behavioral components (see Table 3 columns 3 and 4). We first compute the number of bonus points students would have earned under the pre-reform system given the courses they actually chose, and compare this counterfactual to the observed decline. We estimate a total reduction of 0.562 bonus points per predicted bonus point. Of this, 0.156 bonus points arise from changes in course selection rather than from the rule change itself. This implies that approximately 28 percent of the total decline is attributable to students' behavioral reallocation of coursework under the new incentive regime.

Education Quality and Attainment. The results so far show that reduced incentives for studying science and advanced specialization courses led students to substitute toward easier courses and, for some students, to sort away from science-oriented high-school tracks. These adjustments did not fully offset the decline in admission scores caused by the loss of bonus points, and the switching in itself may have had broader implications in terms of prerequisites and program eligibility to selective programs and institutions. We now examine how these changes translated into students' access to higher education, the quality of the programs they attended, and subsequent educational attainment.

Figure 9 Panel (a) presents event study estimates using the share of college-program combinations for which a student qualifies based on their admission score. This outcome captures changes in students' higher-education opportunity sets following the reform. Panel (b) shows the event study estimates for the measure of program quality constructed based on the pre-reform ranking of programs by admissions thresholds. This measure captures the quality of programs unaffected by shifts in the program thresholds induced by the policy. Panel (c) presents corresponding estimates using the jackknifed minimum high-school GPA of peers in the college-program that the student ultimately attends. We use this measure as an alternative proxy for program quality. Results from the simplified difference-in-differences specification are reported in Table 4. Additionally, event study estimates without linear trend controls are shown in Figure 10.

All panels show stable pre-reform trends across exposure levels, providing support for the common trends assumption. In the post-reform period, we observe a marked decline in both the size of students' opportunity sets and the quality of the programs they attend, with effects emerging within two years of the reform.[12]

The decline in opportunity sets and program quality may reflect changes in eligibility driven by prerequisite requirements. Because many selective programs require specific advanced mathematics and science courses, the earlier substitution toward less demanding coursework could mechanically restrict access along this margin.

To examine the role of prerequisite requirements directly, we collect detailed information on program-specific prerequisites across all higher-education programs and restrict attention to programs with explicit course requirements. Using these rules, we map each student's observed high-school course portfolio into eligibility indicators for each program-institution combination, and construct the share of programs for which the student satisfies all prerequisites. This allows us to trace how the reform affected students' eligibility across programs with different prerequisite structures.

We find that students exposed to the reform become less likely to satisfy prerequisites for programs with more demanding mathematics and science requirements (Table 5). While the estimates are somewhat noisy, they show a clear and statistically significant decline for specialization-intensive science programs at the 10 percent level, with no comparable change for non-specialization science programs or when considering all programs combined. This pattern is suggestive of a substitution toward lower-level coursework following the removal of bonus points.

As a result, even when admission thresholds fall mechanically due to lower bonus point accumulation, students' eligibility changes along a different margin. They remain eligible for higher education, but for a different set of programs–typically those with less demanding prerequisites, lower pre-reform selectivity, and a different field composition.

We next examine whether these changes affected students' educational attainment. In addition to program quality, we study three outcomes: (a) enrollment in a bachelor's degree program, (b) completion of a master's degree, and (c) completion of a STEM master's degree. Event study estimates for these outcomes are shown in Figure 11, with corresponding difference-in-differences estimates reported in Table 6.

[12]For the jackknifed measure, the event study without the linear adjustment (Figure 10, Panel (c)) exhibits an upward-sloping pre-trend across exposure levels. The reform generates a clear break from this trajectory. Our baseline specification therefore includes a linear exposure trend estimated from the pre-reform cohorts to account for this pre-existing pattern (Wolfers, 2006; Roth, 2022).

The results indicate no statistically meaningful change in bachelor's enrollment.[13] However, there is a substantial reduction in the likelihood of completing a master's degree among students most affected by the reform, especially in STEM.

Admission Cutoffs. The results above show a decline in students' opportunity sets and in the quality of the programs they attend following the reform. Given the centralized and capacity-constrained nature of the Norwegian admissions system, a natural question is whether these patterns reflect general equilibrium adjustment: if admission scores fall system-wide, should access not become mechanically easier for at least some students? We address this question directly.

We begin by documenting how the admissions system adjusts following the reform. We compare the pre- and post-reform distributions of program admission thresholds – the minimum score required for admissionand find a clear downward shift in required scores (Figure 13). This shift is driven primarily by the top of the distribution, where admission thresholds fall most sharply. As a result, for any given points total, applicants qualify for a broader set of programs than before. Holding prerequisite eligibility fixed, admission therefore becomes easier on the points margin.[14]

Next, and to further unpack any potential general equilibrium shifts in the admissions thresholds, we plot year-on-year changes in admission thresholds at the program level for 2006-2010 (Appendix Figure A6). Each point compares a program's threshold in year t to its threshold in $t + 1$; in the absence of any change, all observations would lie on the 45-degree line. Prior to the reform (2006-2007 and 2007-2008), thresholds are tightly clustered around this line. In contrast, in 2008-2009 – the first post-reform year– we observe a clear downward shift, with thresholds systematically lower across programs. This pattern holds across all fields, including STEM, indicating a broad reduction in admission score requirements following the reform. In 2009-2010, thresholds again cluster around the identity line, indicating that the initial decline persists. Taken together, the evidence points to a discrete and permanent reduction in admission thresholds following the reform.

However, these mechanical threshold shifts do not mechanically translate into broader

[13]The event study without the linear trend adjustment shows a downward pre-trend in BA enrollment that continues after the reform, with no clear break at the time of the policy (Figure 12, Panel A), indicating no discernible effect on BA enrollment.

[14]Comparing the average changes, admissions thresholds drop by 1.13 points, while average admissions scores drop by 3.55 points.

access to high-return programs because prerequisite requirements remain fixed and continue to bind. As shown above (Table 5), students exposed to the reform become less likely to satisfy prerequisites for programs with demanding mathematics and science requirements. As a result, many students remain ineligible for these programs despite the decline in admission thresholds.

Taken together, these results clarify the general equilibrium adjustment induced by the reform. While admission cutoffs fall mechanically, fixed prerequisite requirements combined with behavioral responses in course-taking lead to a contraction in eligibility for selective programs. The resulting reallocation weakens sorting into high-return fields without generating offsetting gains for lower-exposure students.

Labor Market Effects. We next examine whether the educational adjustments documented above translate into differences in labor market outcomes in adulthood. To obtain an aggregate measure of the labor-market implications of the educational reallocation induced by the reform, we examine realized earnings at ages where annual income becomes substantially more informative about longer-run earnings trajectories (Haider and Solon, 2006; Böhlmark and Lindquist, 2006). Specifically, we estimate effects on pre-tax earnings at age 30. The corresponding results are shown in Figure 14, Figure 15, and Table 7.

There are three main takeaways from these analyses. First, the event studies show no differential pre-trends in the outcomes across treatment intensity, supporting the common trends assumption. Second, there is a significant decrease in earnings for these individuals at age 30, which occurs relatively quickly and persists over the next several cohorts. In terms of magnitude, the effect suggests a reduction in annual earnings of 6,414 NOK per predicted bonus point, equivalent to approximately 1 percent of the pre-reform mean shown in the table. Third, distributional analyses show negative effects across the entire earnings distribution, with larger losses in the upper tail (see Figure 16 and Appendix Figure A7). As a result, the reform compresses the earnings distribution and improves income equality, but this compression is driven by disproportionately larger losses at the top rather than gains at the bottom. We do not find evidence of meaningful improvements for lower-exposure students that would offset these losses.

6 Robustness

To examine the robustness of our findings and address potential concerns regarding our identification strategy, we conduct several additional analyses.

First, we investigate the monotonicity assumption more thoroughly by estimating the first-stage relationship between the predicted bonus points and actual course selections across different subgroups (Bhuller et al., 2020). This approach enables us to verify that the dosage measure consistently influences students' behavior in the expected direction across various demographic and educational segments. For this exercise, we split the sample by gender, middle school GPA (high and low), and parental income (high and low). The results are shown in Appendix Table A5 and demonstrate that the coefficients on predicted bonus points are positive and highly statistically significant across all subgroups examined.

Second, we conduct a diagnostic exercise to assess whether behavioral responses are concentrated among students for whom bonus point incentives are plausibly irrelevant ex ante. Specifically, we examine students in the bottom tail of the predicted bonus point distribution. For these students, an additional bonus point is unlikely to have meaningful expected value at the time course choices are made, as they are sufficiently far from realistic admission thresholds that marginal changes in bonus points do not expand their feasible choice set.

We use this group as a low-exposure benchmark and compare their course-taking responses to those of students with higher predicted exposure. Consistent with the incentive-based interpretation of the reform, we find little evidence of course adjustment among students in the bottom tail, while responses increase sharply with predicted exposure. We emphasize that this exercise is not intended to define a mechanically untreated control group, but rather to illustrate how behavioral responses scale with ex ante incentive exposure. The results are reported in Appendix Figures A8 and A9 and reinforce the interpretation of predicted bonus points as a meaningful measure of treatment intensity.

Third, we examine whether aggregating science and specialization incentives into total bonus points is an appropriate summary of exposure. Because the reform affected these two components differently, a concern is that our results may be driven by one margin rather than the other. We first study their joint distribution and find that the two measures are highly positively correlated: students who accumulate more specialization points also tend to accumulate more science bonus points (Appendix Figures A10 and Appendix Table A6).[15] We then re-estimate our main specifications predicting science and specialization bonus points separately and treating them as distinct margins. The resulting estimates are very similar to those obtained using total bonus points (Appendix Table A7), indicating that our main results are not driven by aggregating these components.

Fourth, a different concern is that the decline in science and specialization course-taking reflects supply-side adjustments by high schools rather than changes in student demand. For example, if fewer students enroll in these courses after the reform, schools might respond by offering fewer sections, which could mechanically constrain students' choices. This would not invalidate the results we find, but it would change the interpretation of those results substantially. To assess this possibility, we examine whether the number of science and specialization course sections offered at the school level changes following the reform. The results from this exercise are shown in Appendix Figure A11.

We find no evidence of such supply-side adjustment: the number of science and specialization sections offered remains stable over time. This indicates that the observed decline in enrollment reflects changes in students' course-selection behavior rather than rationing or reduced availability of courses.

Fifth, we assess the sensitivity of our results to potential outliers in our prediction model. We exclude observations where the predicted bonus points fall below 3.6 or above 7.4, which correspond to the 1st and 99th percentiles, respectively. By focusing on the central portion of the distribution, where our predictions are most accurate, we can determine whether our main effects are influenced by cases where the model's predictive power is weaker. Appendix Table A8 presents the results after excluding outliers based on predicted bonus points. Our findings are robust to this adjustment, demonstrating that our core results are not driven by extreme values in the predicted bonus points distribution.

Sixth, we examine the impact of adding or removing control variables in our specifications. By testing various specifications, we evaluate the robustness of our estimates and ensure that our results are not sensitive to the inclusion or exclusion of specific controls.

[15]The underlying distribution of bonus points per source (science vs. specialization) are shown in Appendix Figure A12.

Appendix Table A9 shows that excluding key control variables such as parental employment status or household income does not significantly alter our baseline findings. This provides support for the interpretation that the observed negative effects on students' educational choices, attainment, and expected earnings are attributable to the reform itself, rather than confounding factors associated with our specification.

7 Survey Evidence

Our administrative data show that removing bonus points for science and advanced specialization courses reduced student enrollment in these subjects, prompting them to choose less rigorous alternatives. While this shift led to marginally higher grades, it lowered overall admission scores, restricting access to competitive undergraduate programs and weakening long-term educational and career prospects.

To better understand how students select their high school courses, we conduct a survey in 2025 of 491 Norwegian high school students (290 female, 166 male, 8 non-binary, 5 prefer not to answer, with an average age of 17.4 years; for additional descriptive statistics see Appendix Table A10). The survey measures self-reported knowledge of the university points system, ratings of 11 factors influencing course choices on a 1-5 scale, and a four-item conjoint experiment assessing willingness to enroll in a course that grants a bonus admission point at a GPA cost of 0, 1, or 2 grades below their average. It also collects demographic information. These measures provide direct evidence of how bonus points influence course selection and how students weigh trade-offs between grades and admission advantages. The full survey is available in the appendix. These measures give us direct evidence of the role of bonus points in course selection and the way students might trade off grades and bonus points.

Students report an average familiarity with the points-based university admissions system of 2.91, slightly below the midpoint of the scale ($\beta = -0.09$, s.e.=0.06, p=0.093). Familiarity is higher among students with stronger academic records, those in their final year of high school, and those whose fathers have post-secondary education (see Table 8). Students in the first year in our sample report an average of 2.53, in the second year an average of 2.71, and 3.35 in the third year. This pattern suggests that awareness of the system develops gradually, often solidifying late in high school— potentially after many critical course decisions have already been made. As a result, differences in information and timing may contribute to disparities in course selection, with students from lower-GPA or less-educated family backgrounds less equipped to make strategic choices that maximize their university admission prospects.

In Table 9, we present regression results comparing the importance of bonus points in course selection to the other ten factors. Participants rate bonus points at the midpoint of our scale, with only social considerations—captured by "subjects my friends take" — ranking lower on average. The highest-rated factors emphasize personal match value, expected grades, future job considerations, and meeting admissions criteria, which students consider substantially more important than bonus points. These results suggest that students tend to prioritize subjects they excel in rather than challenging themselves with more demanding coursework. Moreover, by not placing strong weight on bonus points, they may be making course choices that do not fully optimize their university admission prospects.

We also examine the relationship between student demographics, family background, knowledge of the admissions system, and the perceived importance of bonus points in Table 10. Without controlling for knowledge, GPA and father's education are significant predictors of how much weight students place on bonus points, suggesting that those already performing well are most attuned to the system as it currently operates. When we account for self-reported knowledge of the points-based admissions system, the effect of GPA diminishes to near zero, indicating that awareness of bonus points largely mediates this relationship. The positive effect of having a highly educated father remains significant, though somewhat reduced. These findings suggest that bonus points may function as a form of hidden curriculum, more salient to high-achieving students from more educated backgrounds.

In our analysis of administrative data, the removal of bonus points led students to shift toward courses that had historically awarded higher grades. To assess students' willingness to trade bonus points for lower grades, we ask participants whether they would be interested in switching to a randomly selected course that offers a bonus point.[16] Each student was randomly assigned a grade cost of 0, 1, or 2 points below their self-reported GPA (rounded to the nearest whole number).

We measured willingness to switch on a 10-point scale (1 = not at all interested, 10 =

[16]Students first reported their current enrollment from a set of courses that previously awarded bonus points. They were then asked about courses they were not already taking.

very interested). Column 1 of Table 11 estimates the impact of grade costs without course fixed effects. When no GPA penalty is applied, students report a willingness to switch above the midpoint of the scale, suggesting that, if incentivized, they would be open to taking different courses in the absence of a grade tradeoff. However, interest declines sharply as grade costs increase.

Column 2 groups courses by type, revealing that language courses are substantially less preferred-potentially due to students' lack of experience with a given language. Excluding language courses does not meaningfully alter the estimated impact of grade tradeoffs (see Appendix Table A11). Column 3 introduces course fixed effects, with results remaining stable. Column 4 examines the role of GPA in shaping students' willingness to switch. High-GPA students are significantly more likely to switch when grade costs are low or absent, but this relationship disappears when the costs are high. This finding aligns with our administrative data: academically stronger students are more inclined to take advantage of bonus points when they are available. As such, the bonus point system tended to amplify existing advantages, and its removal promoted a form of equality–not by expanding opportunities for disadvantaged students, but by eliminating a targeted benefit that disproportionately favored the already advantaged.

Overall, our survey results suggest that students place less weight on bonus points than other factors when selecting courses, prioritizing personal fit, expected grades, and future job prospects instead. However, this does not mean bonus points are inconsequential. When students were presented with a direct tradeoff between bonus points and lower bonus, they were generally willing to switch to courses that granted extra points, so long as there was little or no GPA cost. As the grade penalty increased, willingness to switch declined sharply. This pattern suggests that while bonus points do influence decision-making, their effect is constrained by students' reluctance to accept lower grades.

Moreover, our results highlight disparities in information and strategic awareness. Our survey results suggest that while students prioritize course fit and expected grades over bonus points in their decision-making, they are still responsive to point incentives,particularly when there is little or no GPA tradeoff. Students with higher GPAs and more educated fathers are significantly more attuned to the admissions system. In contrast, students with lower academic performance or less-educated family backgrounds appear less aware of the system's structure, potentially missing opportunities to maximize their admissions prospects. These findings suggest that knowledge of bonus points—and their potential benefits—is unequally distributed and some students may unknowingly forgo opportunities to maximize their chances of entering competitive university programs. Students' preferences for familiar and higher-graded subjects may be utility-maximizing in the short term, by maximizing enjoyment of classes. Additionally, to the extent that students use their high school transcripts for jobs and grants, the only relevant information would be their grade, and bonus points would not be shown. However, the previous bonus point system appears to have successfully nudged them toward more challenging coursework, potentially better preparing them for university and long-term career outcomes.

8 Discussion

Students often base their educational choices on perceived returns. These choices do not always align with labor market demand, potentially generating mismatches between the supply of and demand for skilled labor. Such mismatches can lead to shortages in certain fields and inefficient allocation of talent across occupations.

Two broad approaches can be pursued to address such mismatches and better align the supply of human capital with labor market demand: increasing the returns associated with specific fields of study or reducing the costs of enrollment in those programs. While increasing returns is often difficult to implement, reducing costs is generally more feasible. However, lowering costs can have theoretically ambiguous effects on students' human capital investments and subsequent labor market outcomes. This is especially true if reducing costs weakens the match between students' interests and talents and the courses they choose.

This paper provides new evidence on how incentive structures embedded in education systems shape the skills students acquire and the careers they pursue. We exploit a nationwide reform in Norway that eliminated college admission bonus points for science and advanced specialization courses, using rich individual-level register data to trace its long-term consequences. By analyzing how students adjust their course selections and how these choices affect higher education and labor market outcomes, we shed light on how even modest changes in policy design can shift human capital development and the allocation of talent across the economy.

Our analysis shows that a reduction in the incentive to pursue science and advanced specialization courses in high school generates significant behavioral changes in students' course selections. In particular, we see that these students substitute the more difficult science and advanced specialization courses with easier courses that tend to generate higher grades. However, their performance in these courses are only slightly better than their performance in the advanced courses they switch out of.

Thus, even though we find evidence of course-switching behavior consistent with these students trying to mitigate the policy's impact on their college admission scores, this course switch proves insufficient to offset the decline in admission scores caused by the reform. Consequently, the reform results in lower admission scores for these students, which subsequently reduces their access to high-quality college programs. This has significant implications for their likelihood of pursuing STEM degrees and postgraduate education. Ultimately, we observe meaningful reductions in wages at age 30.

By examining how changes in educational incentives shape course selection, our analysis demonstrates that external factorssuch as policy reforms and institutional practicescan significantly influence students' academic trajectories, with long-run consequences for labor market outcomes. These shifts can generate mismatches between individual choices and labor market demand, affecting both students and society. Moreover, we show that the students most affected by the removal of bonus points were high-achieving individuals from more advantaged backgrounds, suggesting that the original system amplified existing inequalities. The reform, by eliminating a targeted reward that disproportionately benefited already well-positioned students, contributed to a more equitable distribution of educational opportunities. However, this reflects a regressive form of equality—achieved by compressing the top rather than advancing the bottom.

These results provide valuable insights into how strategic changes in educational incentives can shape the future workforce and affect both individuals and communities. The policy change we study altered the relative weights of grades and bonus points in the admissions formula. However, our findings point to a broader insight: intermediate admissions incentives can encourage students to take more challenging courses with downstream benefits. At the same time, policymakers must carefully balance the strength of these incentives. If the advantage is too small, students may not undertake the more demanding coursework that improves preparation and long-term outcomes. If the advantage is too large, the resulting distortions in course choice and studentprogram match may outweigh the benefits of increased academic challenge. They also advance the existing literature by highlighting the need for a comprehensive approach to understanding human capital choices—one that considers not only long-term returns and individual preferences, but also the design of intermediate incentives that structure educational environments and access to opportunity.

At a broader level, many countries face persistent shortages of workers in STEM fields, and projected demand suggests these constraints are likely to intensify. This has raised long-standing questions about why relatively few students pursue STEM education and which margins of educational choice are most responsive to policy. From an economic perspective, students' field-of-study decisions reflect a tradeoff between expected benefits and perceived costs, shaped by academic difficulty, preparation requirements, preferences over fields, and anticipated labor-market returns. A growing economics literature therefore studies how educational choices related to STEM respond to the incentives embedded in education systems.

Existing work highlights several distinct policy-relevant margins. One strand emphasizes expected labor-market returns and comparative advantage across fields, showing that differences in earnings prospects and learning about ability play a central role in educational sorting (Kirkeboen et al., 2016; Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2015). A second line of research studies financial incentives, such as merit-based aid and subsidies, documenting how changes in the monetary returns to performance or field choice affect major selection and STEM degree completion (Dynarski, 2003; Sjoquist and Winters, 2015). A related literature focuses on information, mentoring, and social-environment interventions, demonstrating that beliefs, aspirations, and perceived fitshaped by advisers, role models, and information about performancealso influence STEM participation (Bettinger and Long, 2005; Porter and Serra, 2020; Canaan et al., 2022). Finally, curriculum-based reforms that relax constraints or expand feasible pathways into advanced coursework show that reducing preparation costs can substantially increase STEM take-up (Joensen and Nielsen, 2009; 2016).

The policy studied in this paper operates along a different margin than most interventions examined in the STEM policy literature. Rather than altering labor-market returns, providing financial aid, expanding curricular opportunities, or directly targeting beliefs, the reform reweighted how demanding courses are rewarded within an existing secondary-school curriculum and centralized admissions system. In this sense, it changed incentives without expanding course offerings, altering prerequisites, or introducing new financial resources. This highlights the potential importance of intermediate incentives embedded within education systems. Admissions rules that reward rigorous coursework can shape preparation decisions well before students apply to college, influencing eligibility and sorting into STEM programs. Because such incentives operate within existing institutional structures, they may provide a comparatively low-cost policy lever for encouraging STEM preparation and aligning students' course-taking choices with broader labor market needs.

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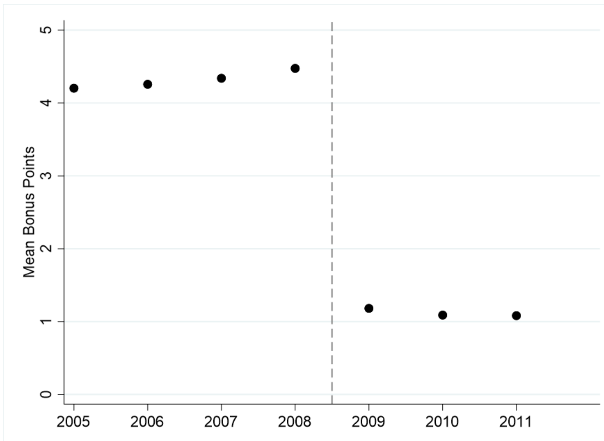
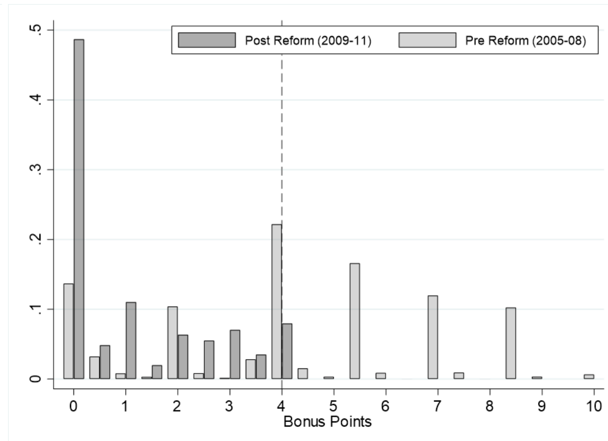
Tables and Figures

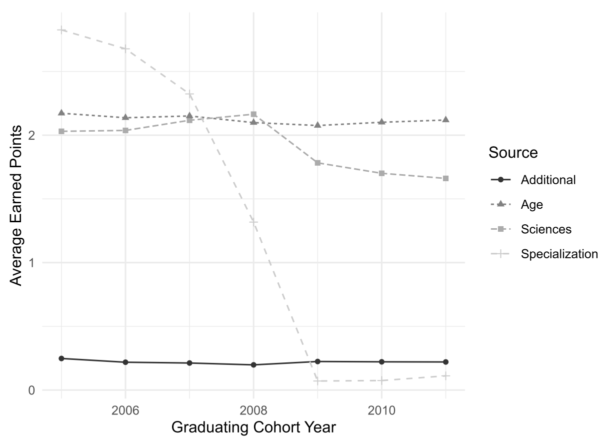
Figure 1: Pre- and post-reform bonus point distribution



(a) Mean Bonus Points by Year



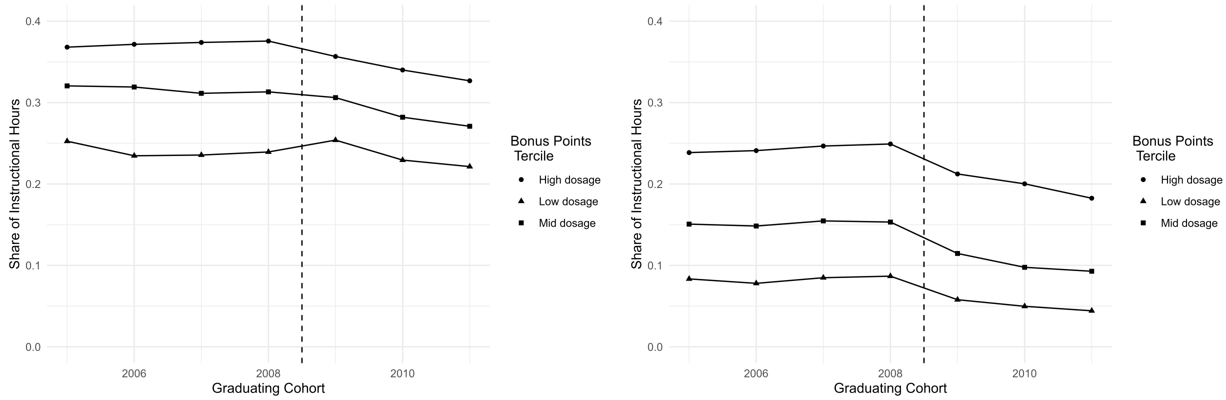
(b) Distribution of Bonus Points



(c) Bonus Points Earned by Type

Notes: Panel a shows the mean bonus points by graduating cohort, spanning from 2006 to 2011. Panel b shows a histogram of the bonus points for cohorts that graduated before the reform (2007-2008) and those that graduated after the reform (2009-2010). Panel c shows the bonus points earned by type by year. The types include our main focus, Specialization and Sciences points, but also Age and Additional categories.

Figure 2: Advanced Courses by Predicted Bonus Points Tercile (Share of Total Instructional Hours)



(a) Specialization Courses

(b) Science Courses

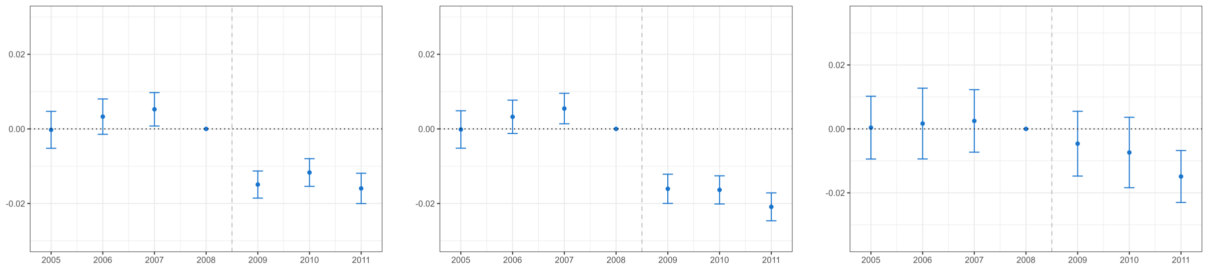
Notes: This figure shows the descriptive analysis of the share of course hours in specialization courses in Panel A and science courses in Panel B split by predicted bonus point tercile. The share of instructional hours in these courses declined following the reform across all terciles, but most strongly for the high dosage group. The sample is restricted to students who graduated in high school from 2005 to 2011. Students are divided into terciles based on predicted bonus points exposure..

Table 1: Effects on Course Allocation

	(1) Bonus Points Courses	(2) Specialization Courses	(3) Science Courses
One Predicted Bonus Point x Post-Reform	-0.016***	-0.020***	-0.010***
	(0.001)	(0.001)	(0.003)
Observations	120,263	120,263	120,263
Baseline Mean	0.328	0.313	0.164

Notes: This table shows the dose-response difference-in-differences specification for the main high school course selection dependent variables. In column one, we show the effect on the share of instructional hours in bonus point granting courses (before the reform). In column two, we show the effect on the share of instructional hours in specialization point granting courses. In column three, we show the effect on the share of instructional hours in science point granting courses. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are from equation 4. Standard errors are clustered at the middle school GPA Group-Cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

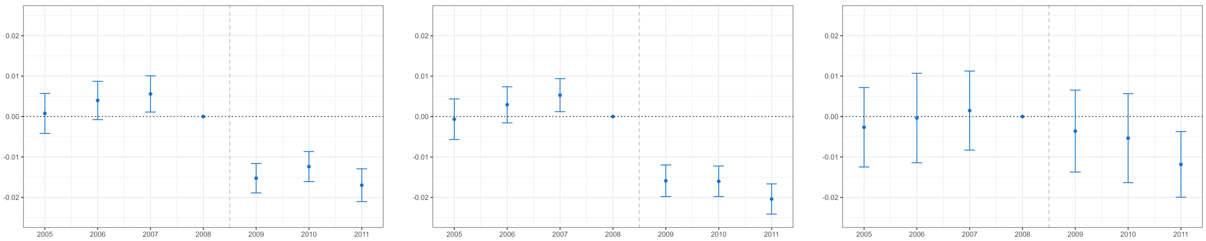
Figure 3: Effects on Course Allocation (Share of Total Instructional Hours)



(a) Bonus Points Courses (b) Specialization Courses (c) Science Courses

Notes: This figure shows event study estimates based on the linear trend adjusted specification over time for the main high school course selection dependent variables. Panel a shows the effect on the share of instructional hours in bonus points courses, Panel b shows the effect on the share of instructional hours in specialization courses, and Panel c shows the effect on the share of instructional hours in science courses. The sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculated from equation 3. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA Group-Cohort level.

Figure 4: Effects on Course Allocation (Share of Total Instructional Hours) - No Linear Trend Correction



(a) Bonus Points Courses (b) Specialization Courses (c) Science Courses

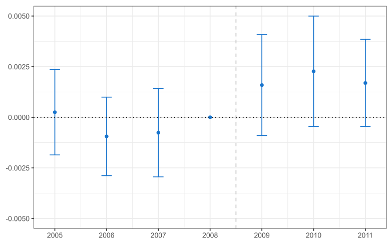
Notes: This figure shows event-study estimates based on the baseline (nonlinear-trend-adjusted) specification for the main high school course selection outcomes. Panel a shows the effect on the share of instructional hours in bonus points courses, Panel b shows the effect on the share of instructional hours in specialization courses, and Panel c shows the effect on the share of instructional hours in science courses. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

Table 2: Effects on Course Types and Program Choice

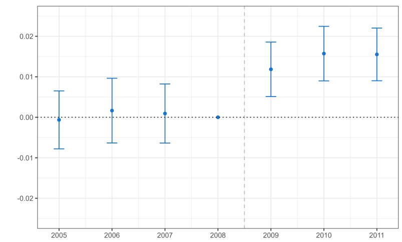
	(1) Pre-reform Relative Grades in Electives	(2) Language Courses	(3) Specialization Science Courses	(4) Non-Specialization Science Courses	(5) Science Program
One Predicted Bonus Point	0.002***	0.014***	-0.014***	0.013***	-0.014*
x Post-Reform	(0.001)	(0.001)	(0.003)	(0.001)	(0.008)
Observations	116,915	120,165	120,263	120,263	120,263
Baseline Mean	1.013	0.123	0.149	0.097	0.305

Notes: This table shows the dose-response difference-in-differences specification for additional course choices in high school. Column one shows the effect on the relative grade measure, a proxy for course difficulty. Column two shows the effect on the share of instructional hours in language courses. Column three shows the effect on the share of instructional hours in courses that granted both specialization points and science points prior to the reform. Column four shows the effect on the share of instructional hours in courses that granted science points but not specialization points prior to the reform. Column five shows the likelihood of selecting the science program track in academic high school. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are from equation 4. Standard errors are clustered at the middle school GPA Group-Cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

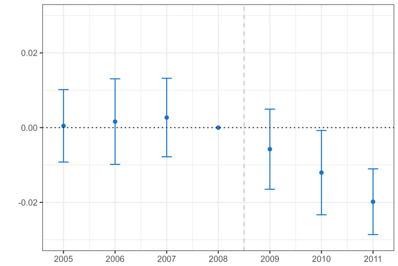
Figure 5: Effects on Course Types and Program Choice



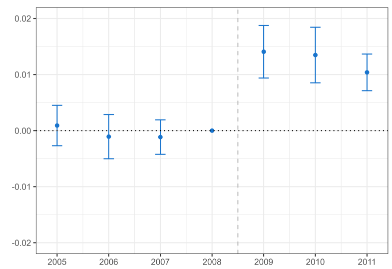
(a) Pre-Reform Relative Grades in Electives



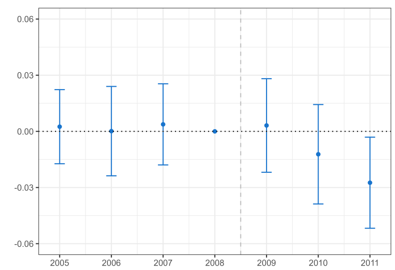
(b) Share of Language Courses



(c) Specialization Science Courses



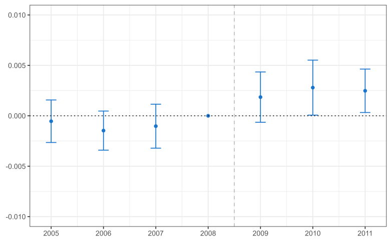
(d) Non-Specialization Science (Grades 12 and 13)



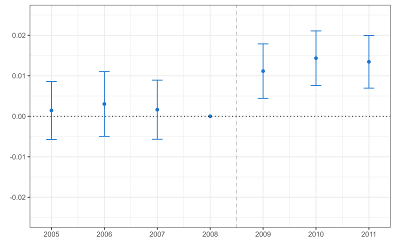
(e) Science Program

Notes: This figure shows event study estimates based on the linear-trend adjusted specification over time for additional course choices in high school. Panel a shows the effect on relative grades, a proxy for course difficulty. Panel b shows the share of instructional hours in language courses. Panel c shows the share of instructional hours in courses that granted both science and specialization points prior to the reform. Panel d shows the share of instructional hours in courses that granted science but not specialization points prior to the reform. Panel e shows the likelihood of selecting the science program track in academic high school. The sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculated from equation 3. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA Group-Cohort level.

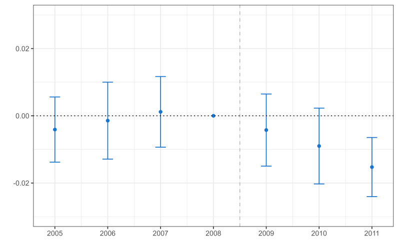
Figure 6: Effects on Course Types and Program Choice - No Linear Trend Correction



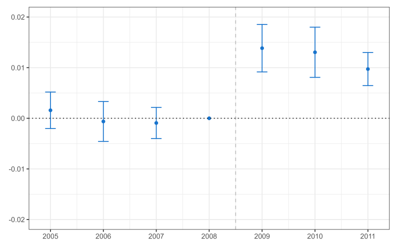
(a) Pre-Reform Relative Grades in Electives



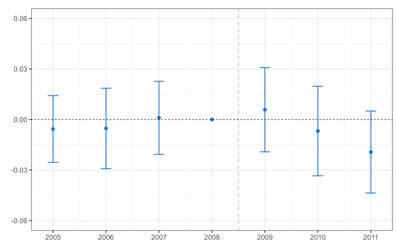
(b) Language Courses



(c) Specialization Science Courses



(d) Non-Specialization Science Courses (Grades 12 and 13)



(e) Science Program

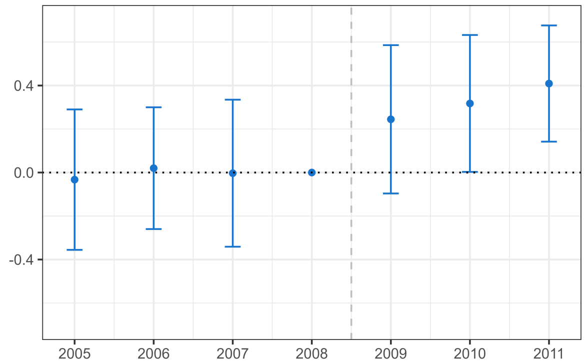
Notes: This figure shows event-study estimates based on the baseline (nonlinear-trend-adjusted) specification for additional high school course choices and program selection outcomes. Panel a shows the effect on pre-reform relative grades in electives, a proxy for course difficulty. Panel b shows the effect on the share of instructional hours in language courses. Panel c shows the effect on the share of instructional hours in specialization science courses. Panel d shows the effect on the share of instructional hours in non-specialization science courses (Grades 12 and 13). Panel e shows the effect on the probability of selecting the science program track in academic high school. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

Table 3: Effects on High School GPA and Bonus Points

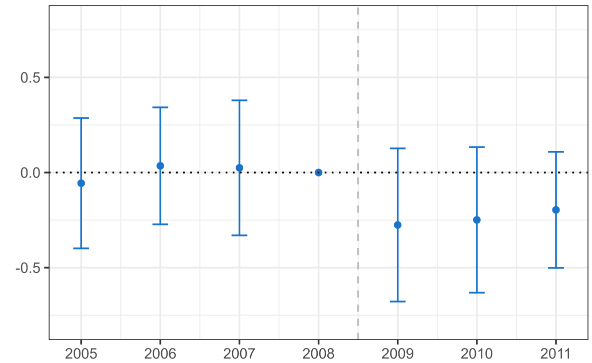
	(1) High School GPA	(2) HS GPA + Obs Bonus Points	(3) Counterfactual Bonus Points	(4) Observed Bonus Points
One Predicted Bonus Point	0.325***	-0.239**	-0.156***	-0.562***
x Post-Reform	(0.082)	(0.10)	(0.013)	(0.026)
Observations	120256	120256	120301	120301
Baseline Mean	40.84	45.17	4.33	4.33

Notes: This table shows the dose-response difference-in-differences specification for the high school GPA and bonus point measures. Column one shows the effect on high school GPAs, column two shows the total effect on admissions scores from high school GPAs and bonus points earned, column three shows the number of bonus points students would have earned from their post-reform course portfolio under pre-reform bonus point rules, and column four shows the observed number of bonus points students earned. The sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculated from equation 4. Standard errors are clustered at the middle school GPA Group-Cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

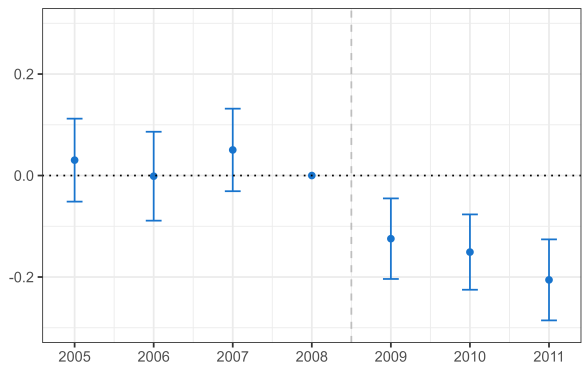
Figure 7: Effects on High School GPA and Bonus Points



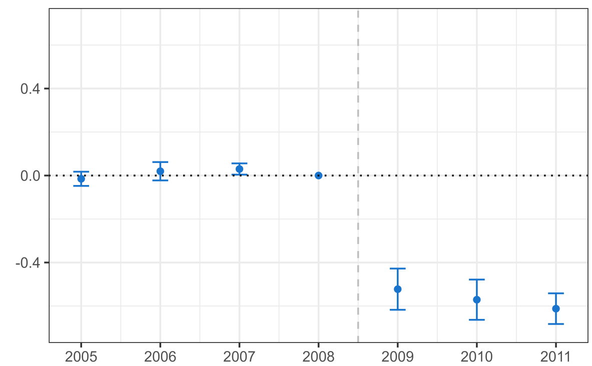
(a) School GPA



(b) School GPA + Observed Bonus Points



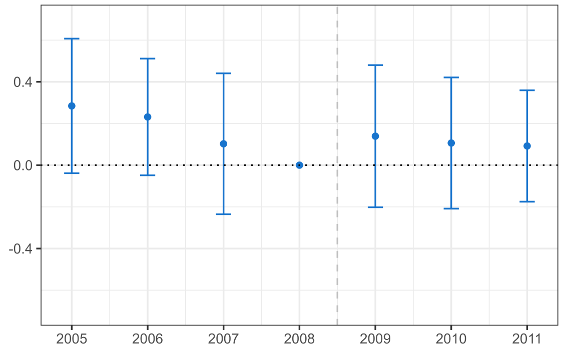
(c) Counterfactual Bonus Points



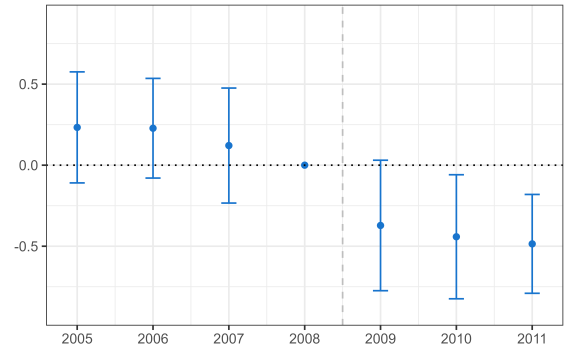
(d) Observed Bonus Points

Notes: This figure shows event study estimates based on the linear-trend adjusted specification over time for the high school GPA and bonus point measures. Panel a shows the effect on high school GPA over time, Panel b shows the total effect on admissions scores from high school GPAs and bonus points earned, Panel c shows the number of bonus points students would have earned from their post-reform course portfolio under pre-reform bonus point rules, and Panel d shows the observed number of bonus points students earned. The sample is restricted to students who graduated in high school from 2005 to 2011. School GPA refers to the average of high school grades, which is the primary basis for the admission score, in addition to bonus points. All estimates are calculated from equation 3. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA Group-Cohort level.

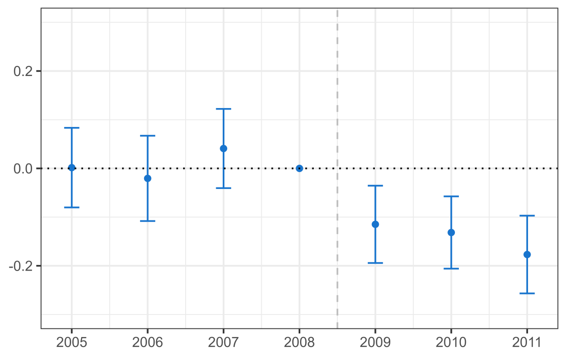
Figure 8: Effects on High School GPA and Bonus Points - No Linear Trend Correction



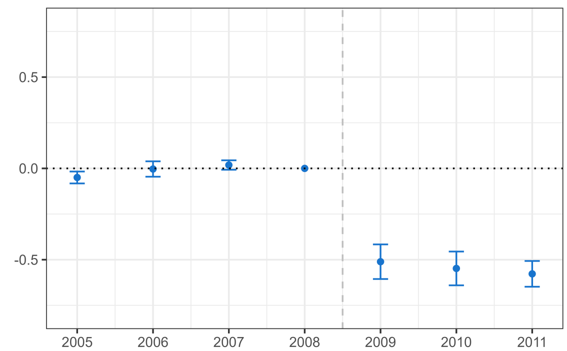
(a) School GPA



(b) School GPA + Bonus Points



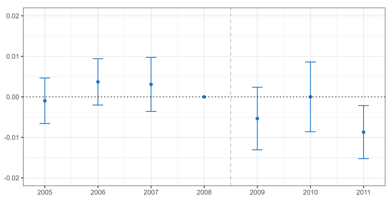
(c) Counterfactual Bonus Points



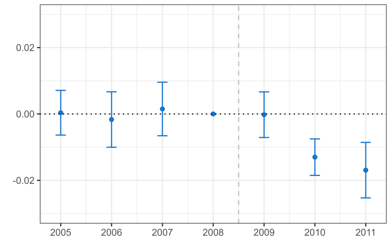
(d) Observed Bonus Points

Notes: This figure shows event-study estimates based on the baseline (nonlinear-trend-adjusted) specification for high school GPA and bonus point outcomes. Panel a shows the effect on high school GPA over time. Panel b shows the total effect on admission scores combining high school GPA and observed bonus points. Panel c shows the counterfactual number of bonus points students would have earned from their post-reform course portfolio under the pre-reform bonus point rules. Panel d shows the observed number of bonus points students earned. The sample is restricted to students who graduated from high school between 2005 and 2011. School GPA refers to the average of high school grades, which constitutes the primary component of the admission score alongside bonus points. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

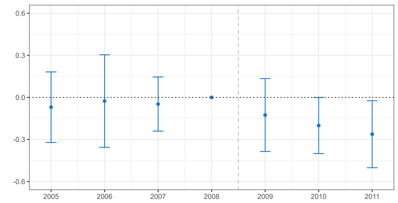
Figure 9: Effects on College-Program Quality



(a) Eligibility Analysis



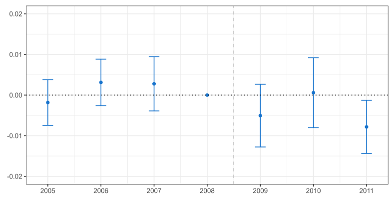
(b) Program Rank



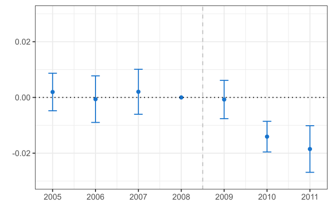
(c) Jackknifed Analysis

Notes: This figure shows event-study estimates from the linear trend-adjusted specification for college-program quality. Panel a reports effects based on an eligibility analysis, where the outcome is the share of college programs students would be eligible for based on both admission scores and prerequisites. Panel b reports effects on the percentile rank of the enrolled college program in the admission cutoff distribution in the pre-reform period; lower values correspond to lower-ranked (i.e., less selective) programs. Panel c reports effects using a jackknife-based (leave-one-out) construction of the minimum high school GPA of peers in each program-year based on the program in which the student is enrolled until the age of 20. The sample is restricted to students who graduated from high school between 2005 and 2011 and enrolled in a higher education program within three years of graduation. All estimates are calculated from equation 3. Dots represent the π_q estimates and bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

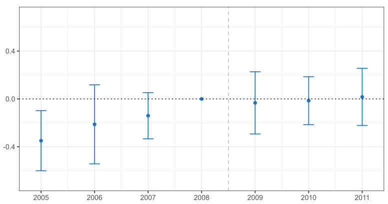
Figure 10: Effects on College-Program Quality - No Linear Trend Correction



(a) Eligibility Analysis



(b) Program Rank



(c) Jackknifed Analysis

Notes: This figure shows event-study estimates from the baseline (nonlinear-trend-adjusted) specification for college-program quality. Panel a reports effects based on an eligibility analysis, where the outcome is the share of college programs students would be eligible for based on both admission scores and prerequisites. Panel b reports effects on the percentile rank of the enrolled college program in the admission cutoff distribution in the pre-reform period; lower values correspond to lower-ranked (i.e., less selective) programs. Panel c reports effects using a jackknife-based (leave-one-out) construction of the minimum high school GPA of peers in each program-year based on the program in which the student is enrolled until the age of 20. The sample is restricted to students who graduated from high school between 2005 and 2011 and enrolled in a higher education program within three years of graduation. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates and bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

Table 4: Effects on College-Program Quality

	(1) College Quality (eligibility analysis)	(2) Program Rank (percentile)	(3) College Quality (jackknifed analysis)
One Predicted Bonus Point	-0.006***	-0.010**	-0.125***
× Post-Reform	(0.002)	(0.002)	(0.040)
Observations	120301	80034	77212
Pre-policy mean	0.742	0.678	27.0

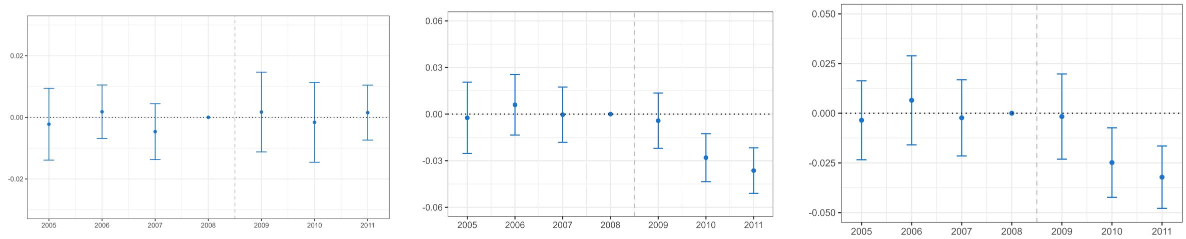
Notes: This table reports estimates from the dose-response difference-in-differences specification for college-program quality. Column (1) reports effects based on an eligibility analysis, where the outcome is the share of college programs students would be eligible for based on both admission scores and prerequisites. Column (2) reports effects on the percentile rank of the enrolled college program in the admission cutoff distribution in the pre-reform period; lower values correspond to lower-ranked (i.e., less selective) programs. Column (3) reports effects using a jackknife-based (leave-one-out) construction of the minimum high school GPA of peers in each program-year based on the program in which the student is enrolled until the age of 20. The sample is restricted to students who graduated from high school between 2005 and 2011 and enrolled in a higher education program within three years of graduation. All estimates are calculated from equation 4. Standard errors are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effect of Bonus Point Reform on Programs for which prerequisites are met

	Specialization Science	Non-Specialization Science	All Programs
Predicted Bonus × Post	-0.019*	-0.002	-0.010
	(0.010)	(0.007)	(0.008)
Observations	120,297	120,297	120,297
Baseline Mean	0.249	0.341	0.294

Notes: This table reports estimates of prerequisite completion from the dose-response difference-in-differences specification. The sample is restricted to students who graduated from high school between 2005 and 2011. The outcome is the share of college programs for which students completed the required pre-requisite subjects in high school. Column (1) focuses on prerequisites that require specialization science courses, Column (2) on prerequisites that require only non-specialization science courses, and Column (3) on all programs. Estimates correspond to the coefficient on predicted bonus points interacted with the post-reform indicator. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 11: Effects on College Enrollment and Master Completion



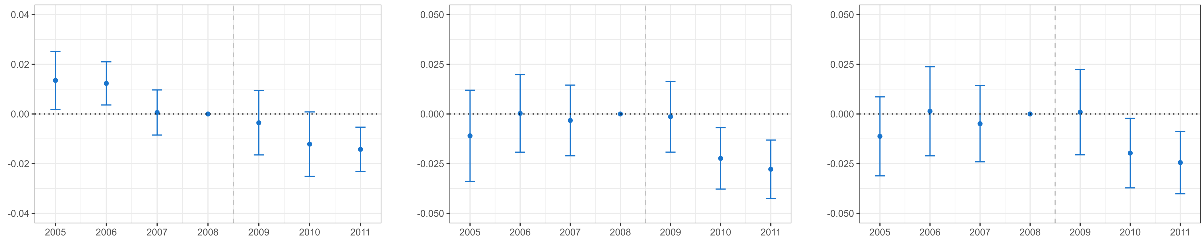
(a) BA Enrollment

(b) MA Completion

(c) MA Completion, STEM

Notes: This figure shows event study estimates based on the linear-trend adjusted specification over time for educational attainment measures. Panel a shows the effect on college enrollment at the age of 21, Panel b shows the effect on master completion at the age of 25, Panel c shows the effect on master completion in STEM at the age of 25. The sample is restricted to students who graduated in high school from 2005 to 2011. School GPA refers to the average of high school grades, which is the primary basis for the admission score, in addition to bonus points. All estimates are calculated from equation 3. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA Group-Cohort level.

Figure 12: Effects on College Enrollment and Master Completion - No Linear Trend Correction



(a) BA Enrollment (b) MA Completion (c) MA Completion, STEM

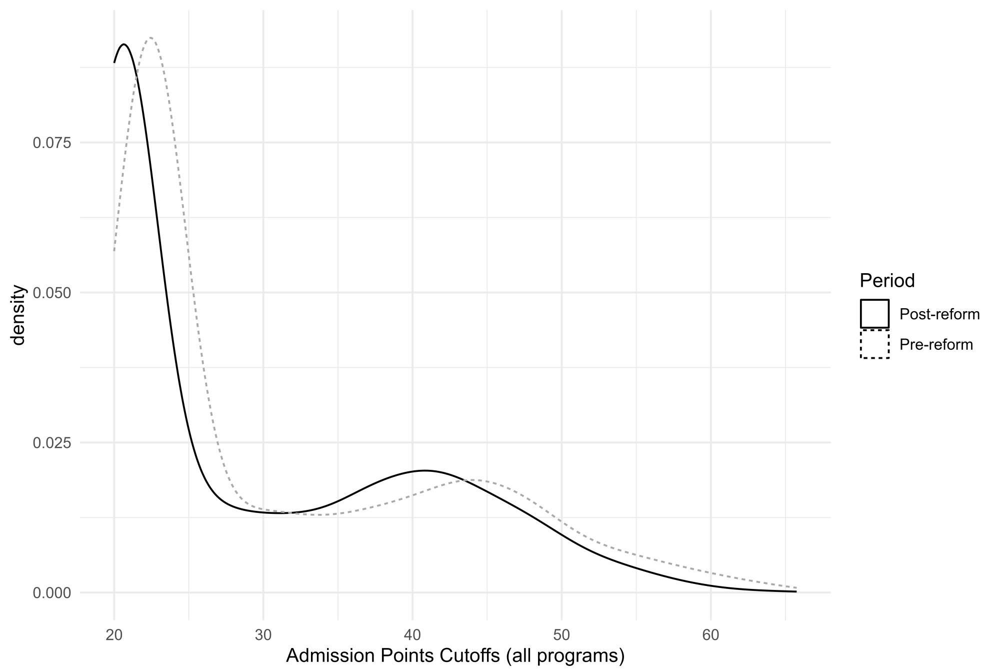
Notes: This figure shows event-study estimates based on the baseline (nonlinear-trend-adjusted) specification for educational attainment outcomes. Panel a shows the effect on college enrollment at age 21. Panel b shows the effect on master's degree completion at age 25. Panel c shows the effect on master's degree completion in STEM at age 25. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

Table 6: College Enrollment and Master 's Degree Completion

	College at 21	Master at 25	Master in STEM at 25
Estimate	0.002	-0.024***	-0.019***
	(0.003)	(0.005)	(0.005)
Observations	120,301	120,301	120,301
Baseline mean	0.779	0.135	0.056

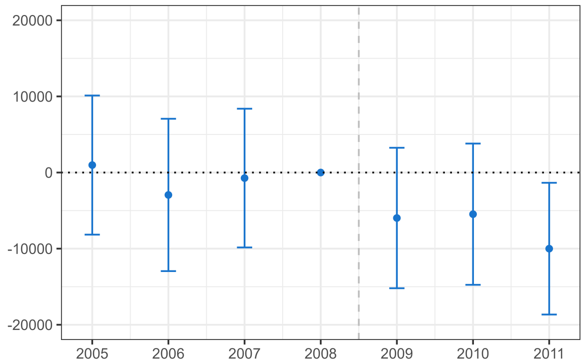
Notes: This table reports estimates from the doseresponse difference-in-differences specification. The sample is restricted to students who graduated from high school between 2005 and 2011. The outcomes are indicators for college enrollment by age 21, completion of a master's degree by age 25, and completion of a master's degree in a STEM field by age 25. Estimates correspond to the coefficient on predicted bonus points interacted with the post-reform indicator, as specified in equation 4. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 13: Admission Score Cutoffs Distribution

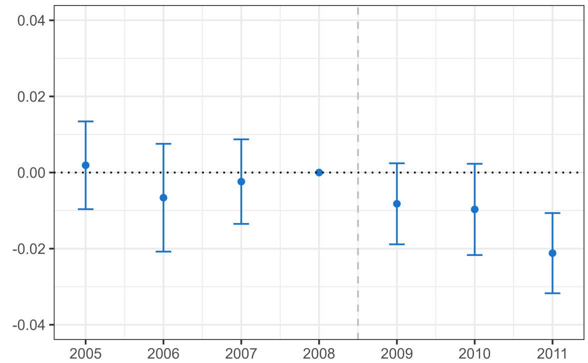


Notes: This figure displays the distribution of admission point cutoffs across all college programs before and after the reform. Calculations are based on administrative register data from the Ministry of Education. Because most programs faced excess demand, admission was determined by ranking applicants according to total admission score (high school GPA plus bonus points). The cutoff represents the average minimum admission score of the last admitted applicant in the pre- and post-reform. For programs without excess demand (i.e., all applicants were admitted), the minimum program-year GPA is used as the cutoff.

Figure 14: Effects on Earnings (pre-tax, 2015 NOK)



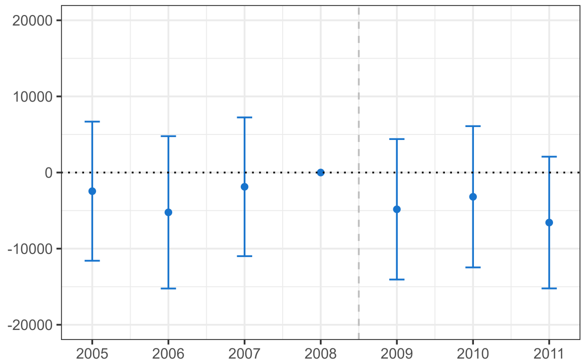
(a) Earnings at the age of 30 (NOK)



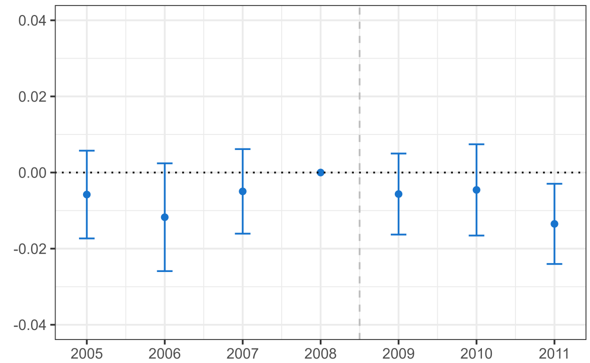
(b) Earnings at the age of 30 (log)

Notes: This figure reports linear controlled event-study estimates of the effect of the reform on labor earnings at age 30. Panel a shows effects on annual earnings measured in pre-tax 2015 NOK, while Panel b shows effects on the natural logarithm of annual earnings. The sample is restricted to students who graduated from high school between 2005 and 2011 and who are classified as attached workers at age 30, defined as individuals with annual labor income above NAV's 2G threshold. Estimates correspond to the coefficient on predicted bonus points interacted with cohort indicators, as specified in equation 3. Dots represent the π_q estimates and bars represent 95% confidence intervals. Standard errors are clustered at the middle school GPA group-by-cohort level.

Figure 15: Effects on Earnings - No Linear Trend Correction



(a) Earnings by the age of 30 (NOK)



(b) Earnings at the age of 30 (log)

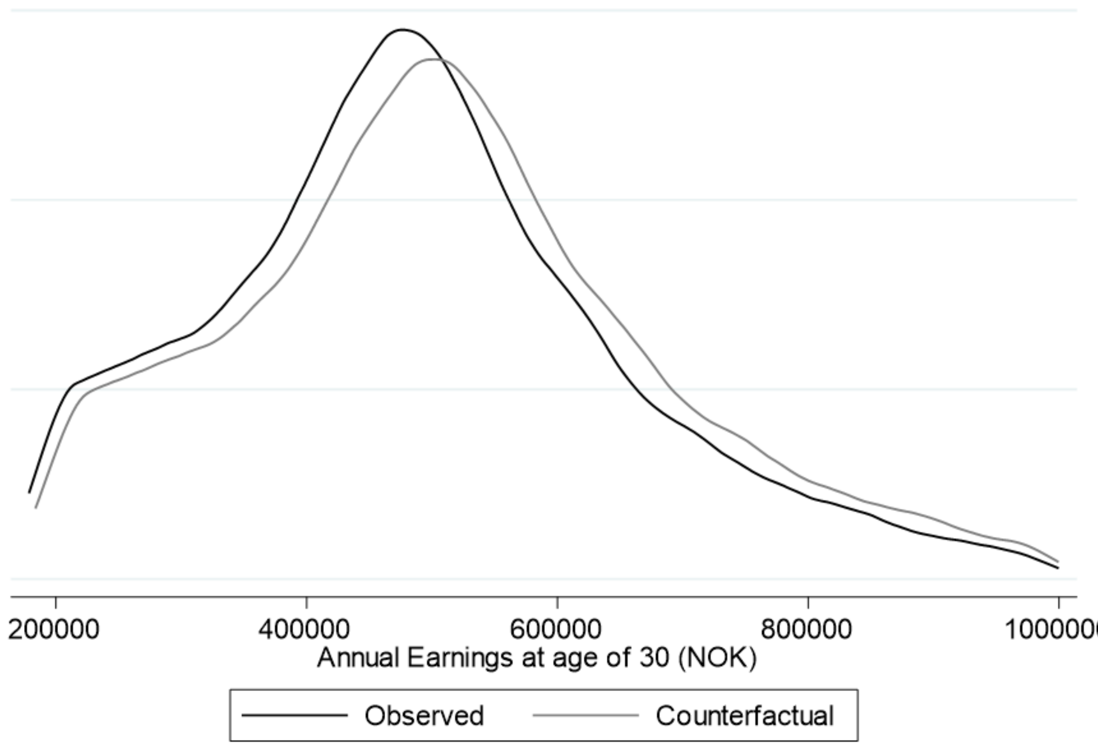
Notes: This figure reports event-study estimates based on the baseline (nonlinear-trend-adjusted) specification for labor earnings at age 30. Panel a shows effects on annual earnings measured in pre-tax 2015 NOK, while Panel b shows effects on the natural logarithm of annual earnings. The sample is restricted to students who graduated from high school between 2005 and 2011 and who are classified as attached workers at age 30, defined as individuals with annual labor income above NAV's 2G threshold. All estimates are calculated from equation 3 without imposing a linear trend correction. Dots represent the π_q estimates and bars represent 95% confidence intervals. Standard errors are clustered at the middle school GPA group-by-cohort level.

Table 7: Effects on Earnings at Age 30

	NOK	**ln**
Estimate	$-6,414^{***}$	-0.011^{***}
	(1657)	(0.003)
Obs.	98,720	98,720
Baseline mean	510,565	13.08

Notes: This table reports estimates from the dose-response difference-in-differences specification for labor earnings at age 30. Earnings are measured in pre-tax 2015 NOK and, in the log specification, in natural logarithms. The sample is restricted to students who graduated from high school between 2005 and 2011 and who are classified as attached workers, defined as individuals with annual labor income above NAV's 2G threshold. Reported coefficients are the interaction between predicted bonus points and the post-reform indicator, as specified in equation 4. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 16: Observed and Counterfactual Earnings Distribution



Notes: This figure displays the observed and counterfactual distributions of annual earnings at the age of 30. The sample is restricted to students who graduated from high school between 2009 and 2011 and are classified as attached workers, defined as individuals with annual labor income above NAV's 2G threshold. Earnings are measured in pre-tax 2015 NOK. The counterfactual distribution is constructed using the doseresponse difference-in-differences specification reported in equation 4, applying the estimated earnings effect to each individual's predicted bonus point exposure. The figure therefore illustrates how the earnings distribution would shift in the absence of the reform-induced change in bonus point incentives.

Table 8: Predictors of Knowledge of the University Admissions Point System

	Knowledge
(Intercept)	-0.363
	(0.641)
GPA	0.636***
	(0.082)
Year 2	0.103
	(0.135)
Year 3	0.654***
	(0.134)
Mother: Less than university	-0.040
	(0.135)
Mother: Master's or Greater	-0.004
	(0.150)
Father: Less than university	-0.148
	(0.140)
Father: Master's or Greater	0.278*
	(0.164)
Non-Binary	-0.168
	(0.630)
Female	0.033
	(0.498)
Male	0.112
	(0.503)
Observations	451

Notes: This table shows estimates from the survey on participants' self-assessed knowledge of the university admissions point system. The outcome variable is a 5 point Likert scale asking about self-assessed knowledge of the university admissions point system. The scale ranged from 1-Not so much to 5-A lot. Regressors include self-reported GPA, separate dummy variables for year of school, mother's and father's education, and gender (where the holdout group is 'prefer not to answer'). Standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9: Importance of Different Attributes for High School Course Selection

	Importance
(Intercept)	2.994***
	(0.054)
Fulfilling university admission requirements	0.737***
	(0.054)
Which subjects my friends are taking	-0.633***
	(0.070)
How good I am at the subject	0.923***
	(0.060)
What grade I think I will get in the subject	0.943***
	(0.060)
How interesting a job I can get after university	0.878***
	(0.062)
How easy I think it will be to get a job after university	0.530***
	(0.061)
How much I like the subject	1.348***
	(0.068)
How much money I think I will earn after higher education	0.538***
	(0.063)
How much time it will take me to study the subject	0.289***
	(0.064)
How important the work I can do after higher education is for society	0.051
	(0.063)
Observations	5401

Notes: This table shows estimates from the survey on participants' importance ratings for different attributes of high school course selection. The outcome variable is a 5 point Likert scale asking about the importance of different attributes to students' selection of courses. The scale ranged from 1-Not very important to 5-Very important. The intercept term represents the average importance of bonus points so coefficients can be interpreted as the difference between the importance of bonus points and the other outcome. Standard errors clustered at the respondent level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Predictors of the Importance of Bonus Points in High School Course Selection

	Bonus Points Importance	Bonus Points Importance
(Intercept)	1.738*	1.824**
	(0.689)	(0.673)
GPA	0.203*	0.053
	(0.088)	(0.091)
Year 2	-0.158	-0.182
	(0.145)	(0.142)
Year 3	-0.201	-0.355*
	(0.145)	(0.145)
Mother: Less than university	-0.028	-0.018
	(0.145)	(0.142)
Mother: Master's or Greater	-0.252	-0.251
	(0.161)	(0.158)
Father: Less than university	0.146	0.181
	(0.150)	(0.147)
Father: Master's or Greater	0.364*	0.299*
	(0.176)	(0.172)
Non-binary	-0.117	-0.077
	(0.678)	(0.662)
Female	0.366	0.358
	(0.536)	(0.523)
Male	0.386	0.360
	(0.540)	(0.528)
Knowledge of University Points system		0.235***
		(0.050)
Observations	451	451

Notes: This table shows estimates from the survey on participants' importance ratings of bonus points in high school course selection. The outcome variable is a 5 point Likert scale asking about the importance of bonus points to students' selection of courses. The scale ranged from 1-Not very important to 5-Very important. Regressors include self-reported GPA, separate dummy variables for year of school, mother's and father's education, and gender (where the holdout group is prefer not to answer) in column one. In column two, self-reported knowledge of the University Admission Score system on a 1-5 scale is included as a control as well. Standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Conjoint Analysis of Interest in Switching Courses to Earn a Bonus Point

	Interest in Switching	Interest in Switching	Interest in Switching	Interest in Switching
(Intercept)	5.813***	5.929***		
	(0.132)	(0.141)		
1 point grade cost	-1.438***	-1.422***	-1.403***	-1.409***
	(0.153)	(0.152)	(0.153)	(0.150)
2 point grade cost	-2.560***	-2.548***	-2.547***	-2.550***
	(0.160)	(0.160)	(0.160)	(0.156)
Language course		-0.685***		
		(0.152)		
Science course		0.077		
		(0.150)		
GPA				0.832***
				(0.192)
1 point grade cost x GPA				-0.212
				(0.216)
2 point grade cost x GPA				-0.725**
				(0.240)
Observations	1953	1953	1953	1953

Notes: This table shows estimates from the conjoint that assessed participants' willingness to switch to a course that granted one additional bonus point. The outcome variable is a 10 point Likert scale indicating respondents' interest in switching into a randomly selected course for one additional bonus point with a randomly assigned grade cost of 0, 1 or 2 grade points. The scale ranged from 1-Not very interested to 10-Very interested. Column 1 shows the interest in switching as a function of grade costs. Column 2 adds in controls for the category of the course with the holdout group representing other courses. Column 3 includes course fixed effects. Column 4 includes controls for students' self-reported GPA and the interaction between GPA and the grade costs. Standard errors clustered at the respondent level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix

Table A1: Higher Education Fields with Science-Based Admission Requirements

Field / Program	**Subject Requirements**
A. Health Sciences	
Medicine, Dentistry, Nutrition, Pharmacy	Mathematics R1 *or* Mathematics (S1+S2) + Physics 1 + Chemistry (1+2)
Biomedical Engineering	Mathematics R1 *or* Mathematics (S1+S2) + Physics 1 *or* Biology 1 *or* Chemistry 1
Orthopedic Engineering	Mathematics (R1+R2) + Physics 1
Pharmacy Technician / Dental Technician	Mathematics R1 *or* Mathematics (S1+S2) *or* Physics 1 *or* Chemistry 1
B. Veterinary Sciences	
Veterinary Medicine	Mathematics R1 *or* Mathematics (S1+S2) + Chemistry (1+2)
C. Informatics and Natural Sciences	
Informatics	Mathematics R1 *or* Mathematics (S1+S2)
Natural Sciences, Environmental Sciences	Mathematics R1 *or* Mathematics (S1+S2) + One or more of the following: Mathematics (R1+R2), Physics (1+2), Chemistry (1+2), Biology (1+2), Information Technology (1+2), Geoscience (1+2), Technology and Research (1+2)
D. Architecture and Engineering	
Architecture (NTNU)	Mathematics (R1+R2) + Physics 1
Integrated Master in Engineering	Mathematics (R1+R2) + Physics 1
3-year Engineering Degree	Mathematics (R1+R2) + Physics 1
Maritime Studies	Mathematics R1 + Physics 1
2-year Engineering Degree	2-year Technical College (relevant field)
E. Economics and Business Administration	
Integrated Master in Economics/Business	Mathematics R1 *or* Mathematics (S1+S2)

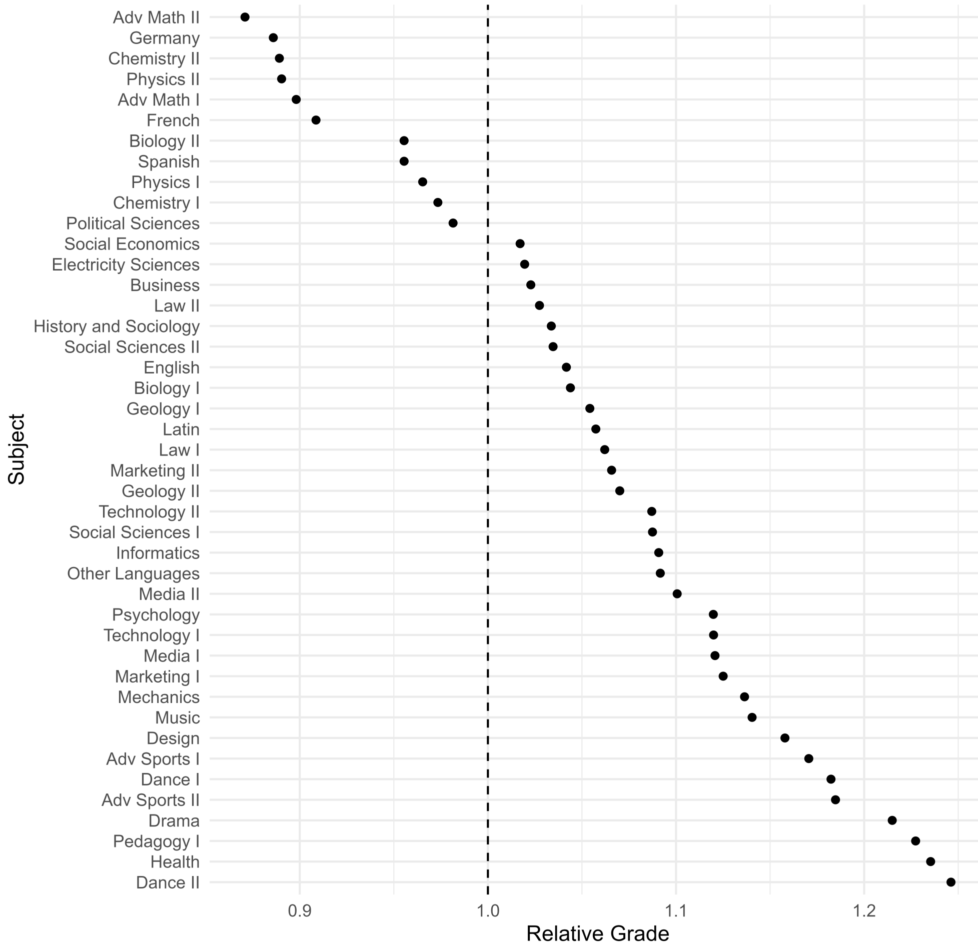
Notes: This table summarizes subject-specific admission requirements for selected higher education fields in Norway during the period studied. Mathematics R refers to *realfagsmatematikk* (scientific track mathematics), while Mathematics S refers to *samfunnsfaglig matematikk* (social sciences-oriented track mathematics). Subjects labeled 1+2 indicate completion of both levels of the course sequence. Requirements are based on national admission regulations.

Table A2: Descriptive Statistics of Outcomes

Variable	Pre-Reform Mean	Pre-Reform Standard Deviation	Post-Reform Mean	Post-Reform Standard Deviation
Share of Instructional Hours in Bonus Points Courses (13th grade)	0.328	0.179	0.319	0.168
Share of Specialization Science Courses (13th grade)	0.148	0.179	0.119	0.165
Share of Language Courses (13th grade)	0.034	0.063	0.010	0.043
School GPA	40.8	6.5	40.5	6.7
School GPA + Bonus Points	45.2	7.8	41.7	7.3
Share of Eligible College-Program	0.677		0.642	
Master's Degree	0.132		0.133	
Master's in STEM Program	0.054		0.044	
Earnings (1000 NOK)	51.0	23.0	50.6	21.5

The table shows descriptive statistics for the main measures used in the paper. Column one shows the pre-reform mean, column two shows the pre-reform standard deviation, column three shows the post-reform mean, and column four shows the post-reform standard deviation. The sample is restricted to students who graduated in high school from 2005 to 2008 (pre-reform) and 2009 to 2011 (post-reform).

Figure A1: Relative Grade by Subject



Notes: This figure shows authors' calculations on grades by subject normalized by mandatory courses' average score. Grades are first normalized at the individual level for pre-reform students, then averaged by subject. If a relative grade is lower than one, it means the expected grade at the individual level is lower than the mandatory courses. Easier subjects are expected to have higher relative grades.

Table A3: First Stage - Predicting Bonus Points

Variable	Estimate
Mother Employment Status	-0.048
	(0.052)
Father Employment Status	0.098*
	(0.056)
Middle School GPA (1–1.5]	2.763*
	(1.505)
Middle School GPA (1.5–2]	1.536**
	(0.656)
Middle School GPA (2–2.5]	1.821***
	(0.668)
Middle School GPA (2.5–3]	2.179***
	(0.653)
Middle School GPA (3–3.5]	2.536***
	(0.655)
Middle School GPA (3.5–4]	2.894***
	(0.653)
Middle School GPA (4–4.5]	3.248***
	(0.655)
Middle School GPA (4.5–5]	3.589***
	(0.653)
Middle School GPA (5–5.5]	3.423***
	(0.665)
Middle School GPA (5.5–6]	4.013***
	(0.656)
Man (Dummy)	0.566***
	(0.025)
Per Capita Household Income (ln)	-0.323***
	(0.017)
Observations	67,450
R^2	0.656
F(212, 35,009)	25.170
FEs: Parents' Education (Level & Program); Municipality of Birth; Month of Birth; Year; High School	

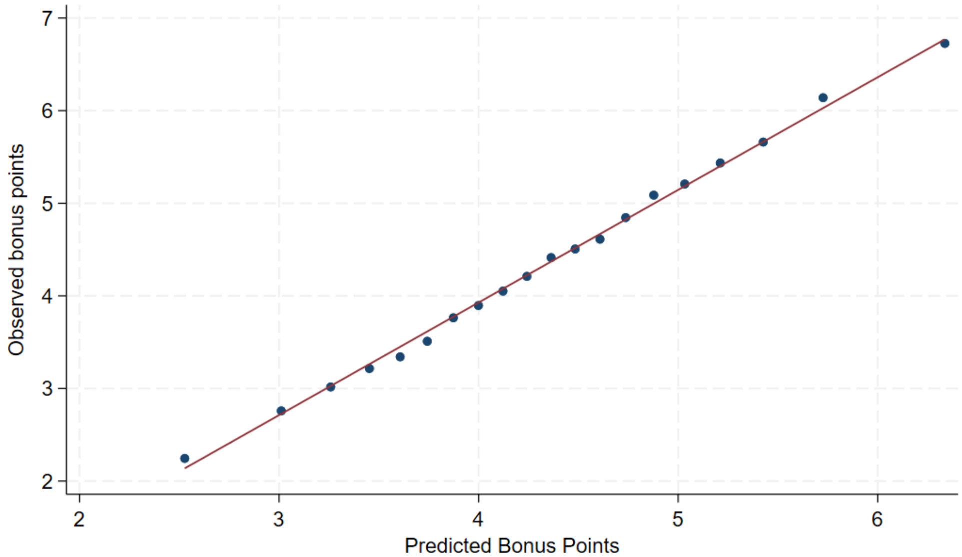
Notes: This table shows the results of the bonus point prediction exercise that is used for the dose-response difference in differences estimation. The sample is restricted to students who graduated from high school between 2005 and 2008. All estimates are from equation 2. Standard errors are clustered at the middle school GPA Group-Cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Correlation between predicted bonus points in 2005 and actual bonus points each year

Year	ρ
2005	0.3552
2006	0.3587
2007	0.3482
2008	0.3531

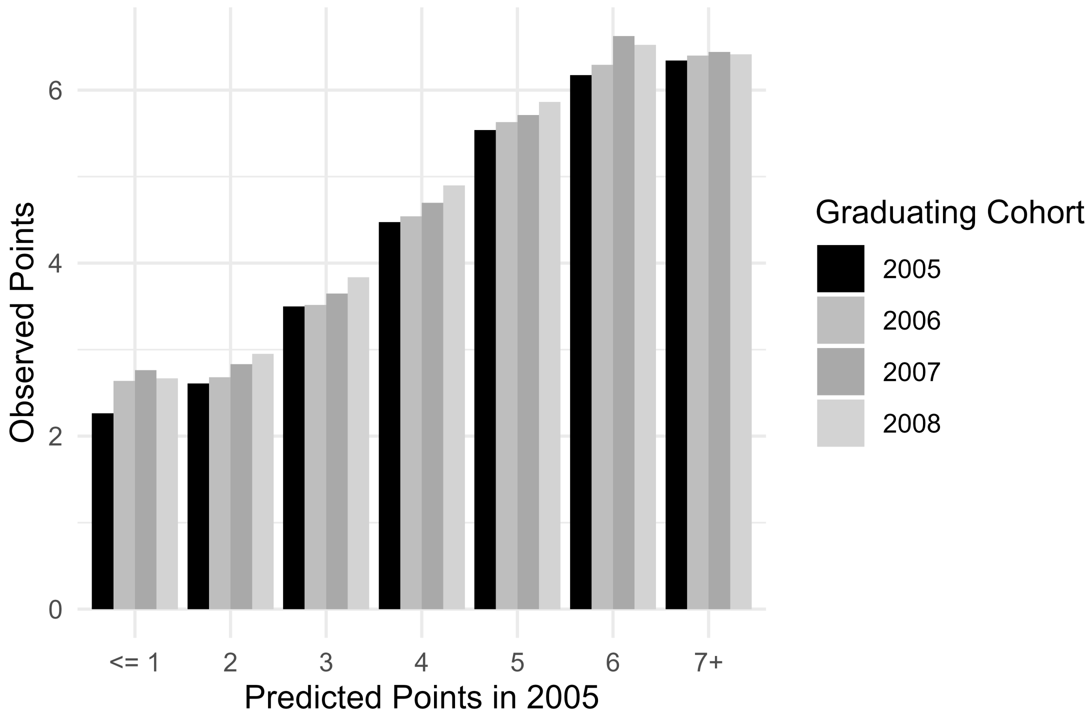
Notes: This table shows the raw correlation between predicted bonus points in 2005 and observed bonus points in each year. Predicted bonus points are estimated in Equation 2 only for the year of 2005.

Figure A2: Predicted and Observed Bonus Points



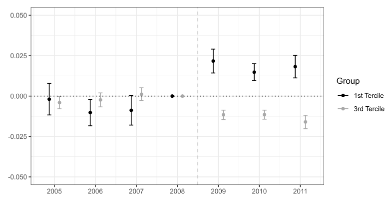
Notes: This figure shows a bin-scatter for the relationship between predicted bonus points and their expected observed bonus points. The sample is restricted to students who graduated in high school from 2005 to 2008, before the reform. Predicted points are estimated in Equation 2. Dots are pooled in bin-scatter groups of equal size, showing the mean of the observed and predicted bonus points within each bin.

Figure A3: Bonus Points Mean by Prediction in 2005

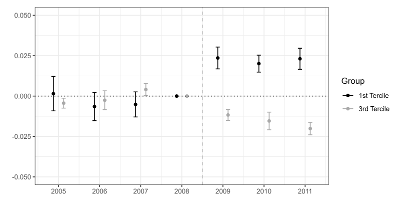


Notes: This figure shows mean bonus points earned each year by the level of predicted bonus points in 2005. The sample is restricted to students who graduated in high school from 2005 to 2008. Predicted bonus points are estimated in Equation 2, restricted to the year of 2005.

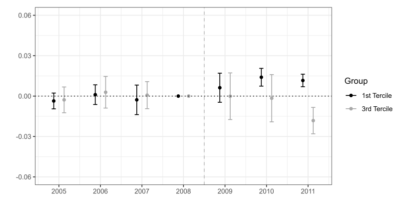
Figure A4: Effects on Main Outcomes by Predicted Bonus Points Terciles



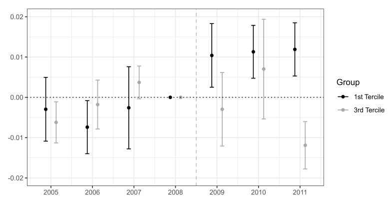
(a) Bonus Points Courses



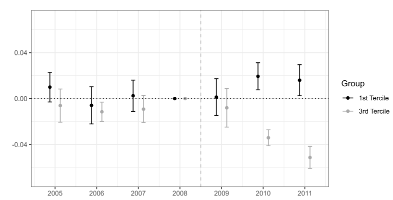
(b) Specialization Courses



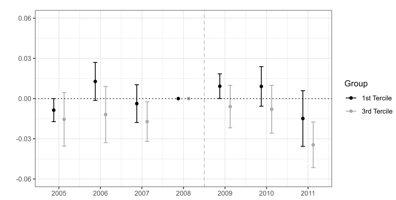
(c) Science Courses



(d) Program Eligibility Analysis



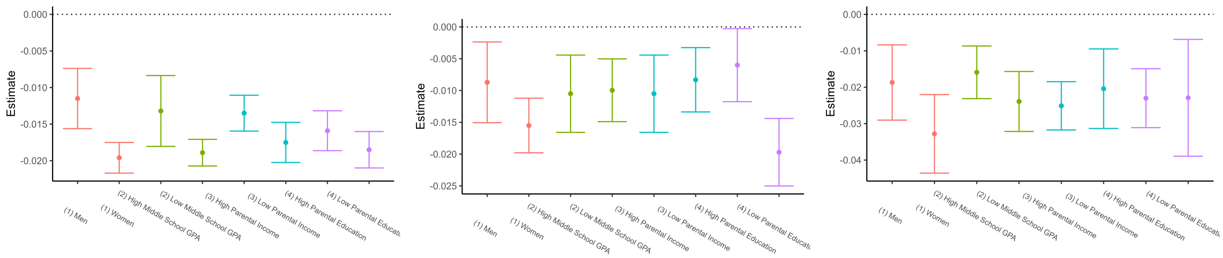
(e) Master's Degree



(f) Earnings at the age of 30 (ln)

Notes: This figure reports event-study estimates by terciles of predicted bonus point exposure. Students are divided into terciles based on predicted bonus points constructed as described in equation 2. The middle tercile serves as the reference group, while the first and third terciles represent lower and higher predicted exposure to the reform incentives, respectively. The estimates therefore trace differential cohort effects across dosage groups. Panel (a) shows effects on the share of instructional hours in bonus point courses. Panel (b) shows effects on the share of instructional hours in specialization courses. Panel (c) shows effects on the share of instructional hours in science courses. Panel (d) shows effects on the share of programs students would be eligible for based on their own program's cutoff. Panel (e) shows effects on master's degree completion by age 25. Panel (f) shows effects on log earnings at age 30. The sample is restricted to students who graduated from high school between 2005 and 2011. All estimates are calculated from equation 3 without imposing a linear pre-trend correction. Dots represent the π_q estimates and bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA group-by-cohort level.

Figure A5: Effects by Heterogeneity



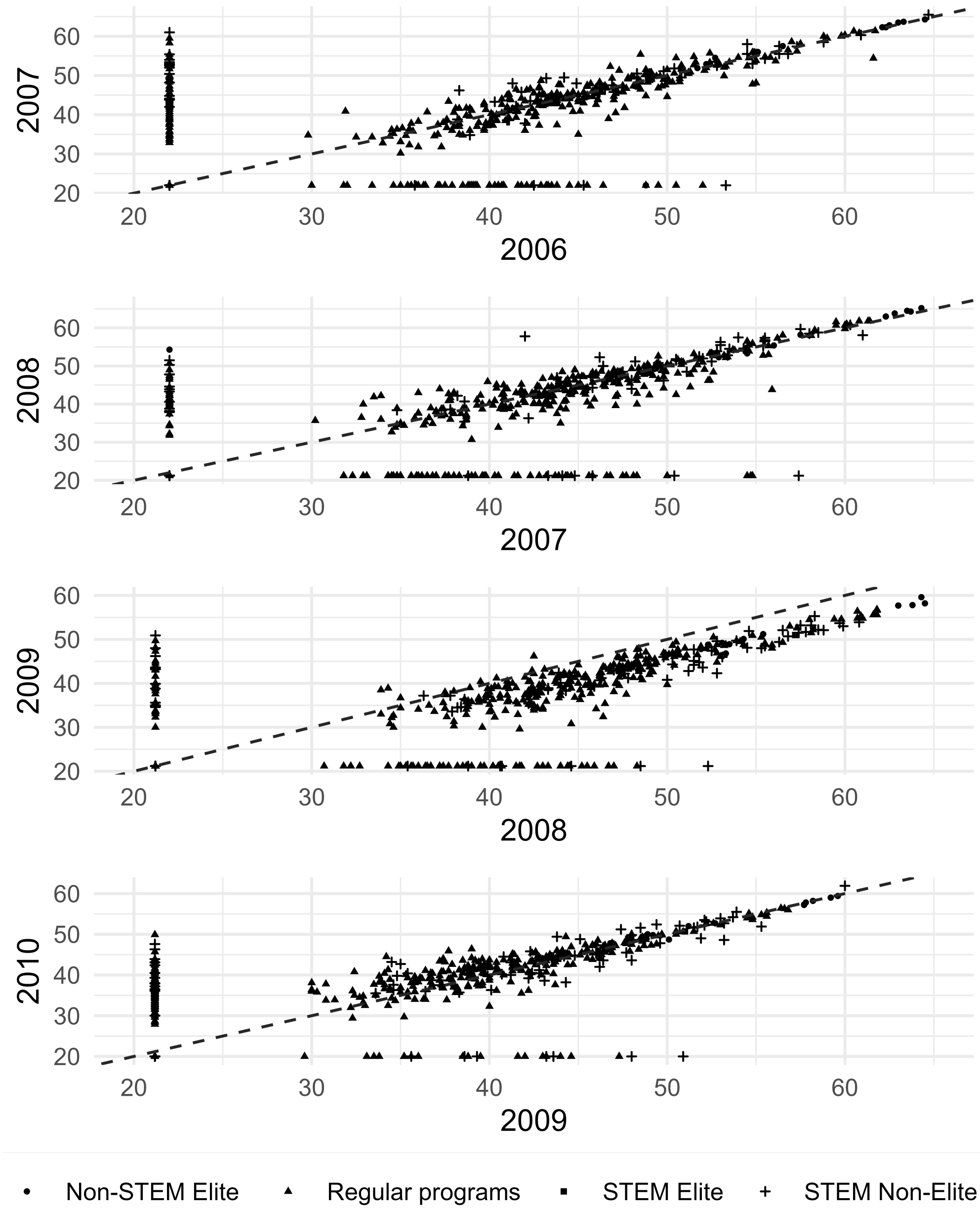
(a) Share of Bonus Courses Instructional Hours

(b) Program Rank (percentile)

(c) Master's Degree at 25 years old

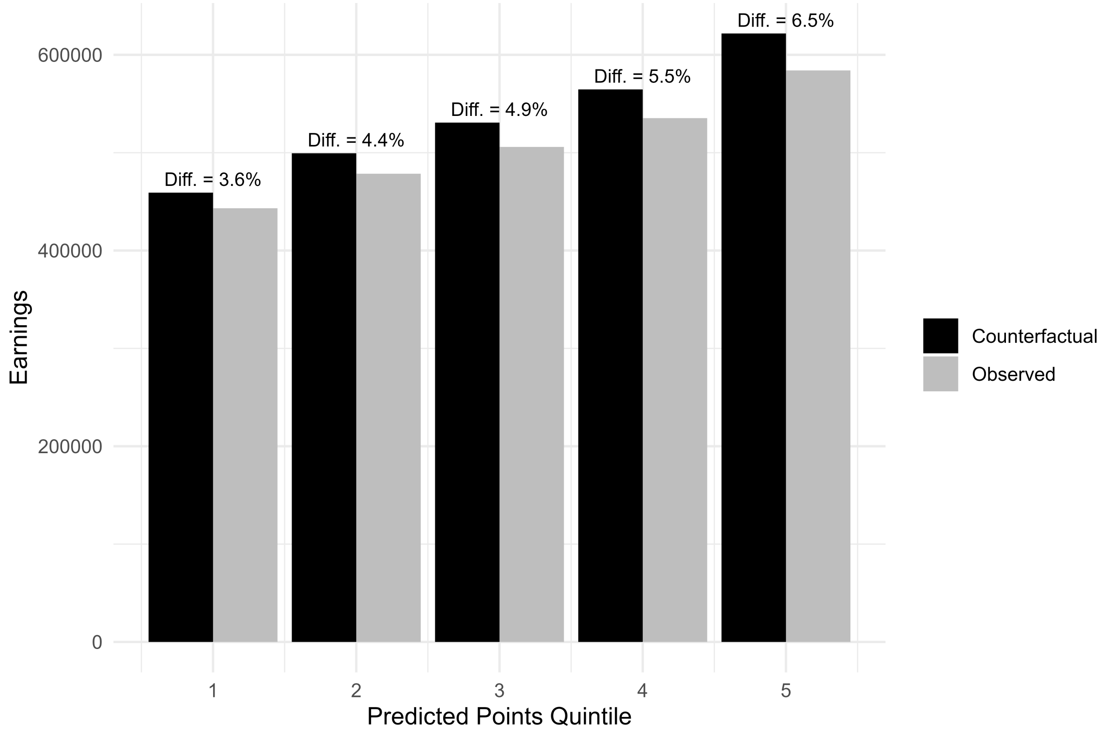
Notes: This figure shows heterogeneity estimates for several main measures splitting the sample by different demographics. Sample is restricted to students who graduated in high school from 2005 to 2011. Panel a shows the share of instructional hours in bonus points courses. In panel b, the sample is restricted further to students who were enrolled in a higher education program in the three years following graduation. The outcome is the share of college-programs the students would be eligible for. The outcome of Master's degree completion in panel c is measured at the age of 25 years old. All estimates are calculated from equation 4. Points represent the aggregate point estimates; bars represent 95% confidence intervals, with standard errors clustered at the middle school GPA Group-Cohort level.

Figure A6: Admission Score Cutoffs by Program year-by-year



Notes: This figure shows year-to-year admissions point cutoffs for the Undergraduate programs in Norway. Specifically, these cluster around the identity line except for plot three which shows the year the reform went into effect. In that plot, thresholds are almost uniformly higher in 2008 than 2009. When the number of applicants exceeds the number of available spots, selection is based on admission scores (GPA + Bonus Points). The cutoffs show the minimum admission score for admitted applicants. Each point is a program-year cutoff in the first quota, and marker shape by program group. For programs with no competition (all applicants were accepted), we input the minimum admission score that year. Elite programs are defined as in Cattan et al. (2025).

Figure A7: Observed and Counterfactual Earnings by Predicted Bonus Points Quintile



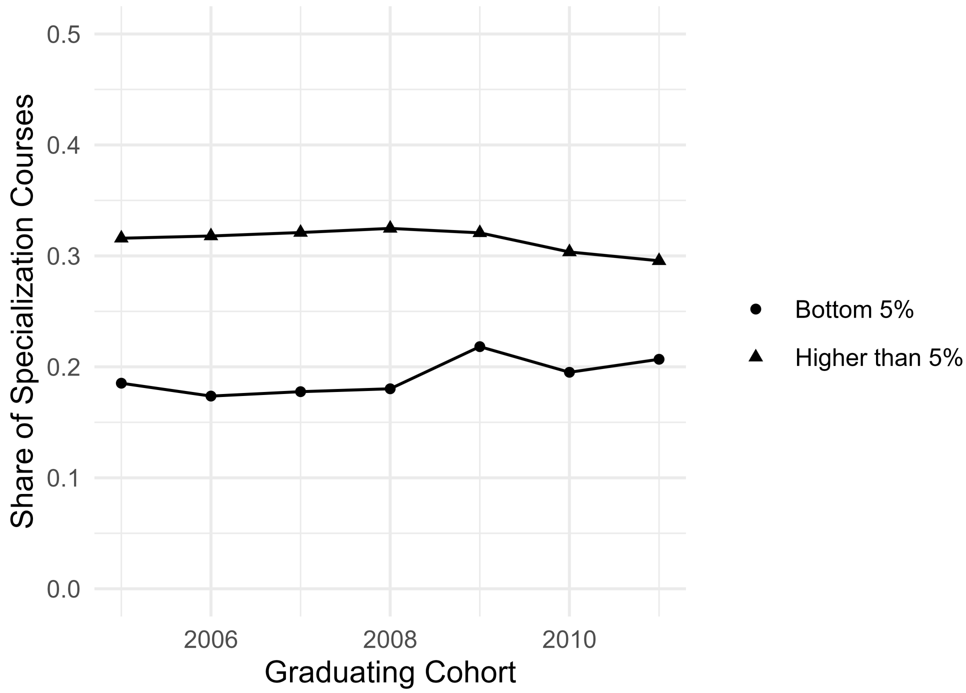
Notes: This figure shows observed and counterfactual average annual earnings at the age of 30 by quintile of predicted bonus point exposure. The sample is restricted to students who graduated from high school between 2009 and 2011 and are classified as attached workers, defined as individuals with annual labor income above NAV's 2G threshold. Earnings are measured in pre-tax 2015 NOK. The counterfactual earnings are constructed using the dose-response difference-in-differences specification in equation 4, applying the estimated marginal earnings effect to individuals' predicted bonus point exposure. Predicted bonus points are divided into five quintiles based on their pre-reform distribution. The reported percentage differences indicate the proportional increase in earnings implied by the reform across the exposure distribution.

Table A5: First Stage by Group

Groups	(1) Men	(2) Women	(3) High Middle School GPA	(4) Low Middle School GPA	(5) High Parental Income	(6) Low Parental Income
One Predicted Bonus Point	1.109***	0.914***	1.213***	0.924***	0.931***	1.067***
	(0.0258)	(0.0205)	(0.0480)	(0.0216)	(0.0205)	(0.0211)
Observations	28,012	39,438	20,907	46,543	33,905	33,516

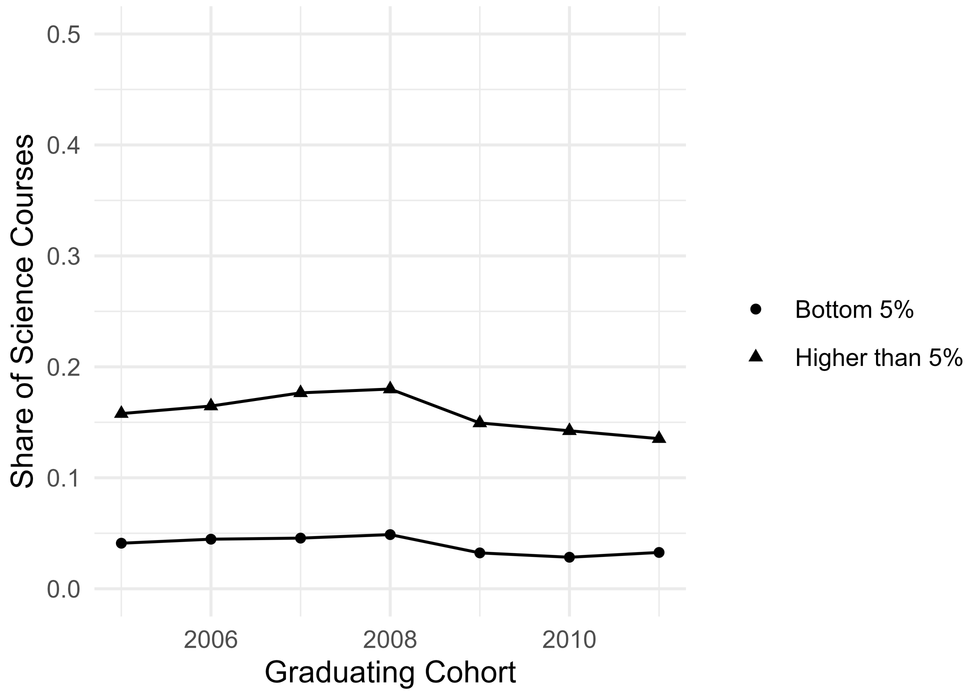
Notes: This table reports pre-reform associations between predicted and observed bonus points by demographic subgroup. The sample is restricted to students who graduated from high school between 2005 and 2008, prior to the reform. The outcome is observed bonus points earned in high school, and the regressor of interest is predicted bonus points constructed from students' middle school GPA profiles. Each column presents estimates for the indicated subgroup. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A8: Share of Specialization Subjects by Bonus Points level



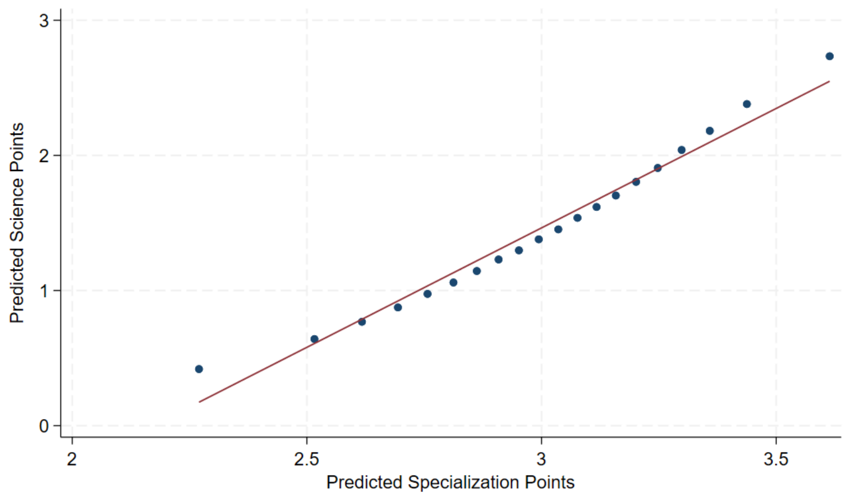
Notes: This figure shows the average share of specialization subjects taken in high school by graduating cohort, separately for students in the bottom 5% of the predicted bonus point distribution and for those above the 5th percentile. The sample is restricted to students who graduated from high school between 2005 and 2011. Predicted bonus points are constructed as described in equation 2. The figure illustrates raw cohort trends by exposure group.

Figure A9: Share of Science Subjects by Bonus Points level



Notes: This figure shows the average share of science subjects taken in high school by graduating cohort, separately for students in the bottom 5% of the predicted bonus point distribution and for those above the 5th percentile. The sample is restricted to students who graduated from high school between 2005 and 2011. Predicted bonus points are constructed as described in equation 2. The figure illustrates raw cohort trends by exposure group.

Figure A10: Binscatter by Type of Predicted Bonus Points



Notes: This figure presents a binscatter plot of predicted science bonus points against predicted specialization bonus points. Each point represents the mean value within bins of the predicted specialization bonus point distribution, and the fitted line shows the corresponding linear relationship. Predicted bonus points are constructed as described in equation 2, based on students' demographics and middle school GPA profiles. The sample is restricted to students who graduated from high school between 2005 and 2011. The figure illustrates the strong positive correlation between the two predicted exposure measures used in the dose-response difference-in-differences specification.

Table A6: Correlation Matrix on Predicted Points

	Total Bonus Points	Science	Specialization
Total Bonus Points			
Science	0.9861		
Specialization	0.9415	0.8724	

Notes: This table reports pairwise correlations between alternative measures of predicted bonus point exposure: total predicted bonus points, predicted science bonus points, and predicted specialization bonus points. Predicted bonus points are constructed as described in equation 2, based on students' demographics and middle school GPA profiles. The sample is restricted to students who graduated from high school between 2005 and 2011. The high correlations reflect the strong overlap across the alternative exposure measures used in the dose-response difference-in-differences specification.

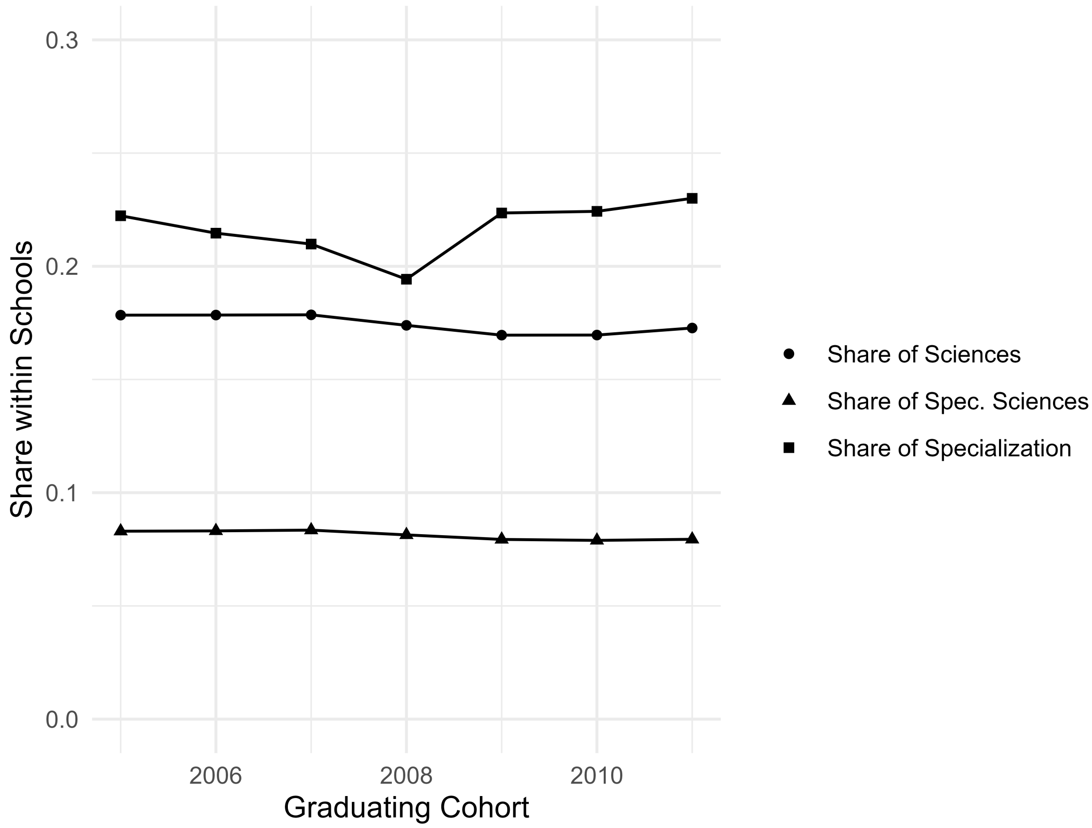
Table A7: Regression Results by type of predicted bonus points

	Panel A					
	Science Program	Specialization (all)	Specialization (Science)	Specialization (Non-Science)	Non-Specialization (Science)	Language Courses
Combined	-0.014*	-0.020***	-0.014***	-0.006*	0.013***	0.003***
	(0.008)	(0.001)	(0.003)	(0.003)	(0.001)	(0.000)
Science	-0.021***	-0.027***	-0.020***	-0.007	0.018***	0.004***
	(0.007)	(0.002)	(0.003)	(0.004)	(0.001)	(0.001)
Specialization	-0.029	-0.058***	-0.035**	-0.023**	0.038***	0.008***
	(0.038)	(0.004)	(0.015)	(0.013)	(0.009)	(0.001)
Observations	120,263	120,263	120,263	120,263	120,263	120,263
Baseline mean	0.305	0.313	0.149	0.164	0.097	0.034

	Panel B				
	High School GPA	College Quality (Threshold Analysis)	Master's Diploma	STEM Master's	Earnings (age of 30)
Bonus	0.325***	-0.010***	-0.023***	-0.020***	–6,414***
	(0.083)	(0.002)	(0.005)	(0.005)	(1,657)
Science	0.396***	-0.012***	-0.035***	-0.029***	-11,156***
	(0.067)	(0.004)	(0.005)	(0.007)	(2,132)
Specialization	1.320*	-0.033***	-0.059**	-0.048**	–8,466
	(0.745)	(0.005)	(0.021)	(0.018)	(6,837)
Observations	120,256	80,034	120,301	120,301	98,720
Baseline mean	40.840	0.677	0.133	0.055	510,565

Notes: This table reports estimates from the dose-response difference-in-differences specification using different bonus point prediction models: combined as in our main results, science only, and specialization only. The sample is restricted to students who graduated from high school between 2005 and 2011. The rows correspond to alternative constructions of predicted bonus point exposure based on bonus-point, science, and specialization course profiles. Panel A reports effects on high school course allocation and program choice outcomes. Panel B reports effects on academic performance, college-program quality, postsecondary attainment, and labor earnings at age 30 (conditional on full-time employment). Reported coefficients correspond to the interaction between predicted bonus points and the post-reform indicator, as specified in equation 4. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A11: Share of Schools with Different Courses



Notes: This figure reports school-level shares of course offerings by graduating year. The unit of observation is the high school. The data shown are the mean of the share of science courses, specialization science courses, and specialization courses offered at the school level, respectively. The sample includes schools attended by students who graduated from high school between 2005 and 2011. Calculations are based on administrative register data from Statistics Norway.

Table A8: Results Excluding Outliers

VARIABLES	(1) Share of Bonus Points Courses	(2) Share of Specialization Science Courses	(3) High School GPA	(4) HS GPA + Bonus Points
One Predicted Bonus Point	-0.017***	-0.014***	0.231***	-0.353***
x Post-Reform	(0.001)	(0.001)	(0.037)	(0.041)
Observations	117,860	117,860	117,852	117,852
Baseline Mean	0.330	0.149	40.89	45.24
VARIABLES	(5) Program Rank (percentile)	(6) Master Graduate	(7) Master in STEM	(8) Earnings (Age of 30)
One Predicted Bonus Point	-0.008***	-0.025***	-0.020***	-5,342***
x Post-Reform	(0.002)	(0.002)	(0.002)	(1,723)
Observations	78,435	117,895	117,895	96,701
Baseline Mean	0.679	0.135	0.055	511,220

Notes: This table reports estimates from the dose-response difference-in-differences specification excluding outliers. The sample includes students who graduated from high school between 2005 and 2011, excluding individuals with predicted bonus points below the 1st percentile or above the 99th percentile of the distribution. Columns (1)-(4) report effects on high school course choices and performance. Columns (5)-(7) examine postsecondary attainment outcomes, and Column (8) reports effects on annual earnings at the age of 30. In Column (5), the sample is further restricted to students who enrolled in higher education within three years of graduation. Earnings are measured in pre-tax Norwegian kroner and deflated to 2015 price levels. School GPA refers to the average of high school grades, which constitute the primary component of the admission score alongside bonus points. Reported coefficients correspond to the interaction between predicted bonus points and the post-reform indicator, as specified in equation 4. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A9: Effects Excluding Controls

Panel A: No Mother Employment Status				
VARIABLES	(1) Share of Bonus Points Courses	(2) Share of Specialization Science Courses	(3) High School GPA	(4) HS GPA + Bonus Points
One Predicted Bonus Point x Post-Reform	-0.016*** (0.001)	-0.014*** (0.001)	0.333*** (0.035)	-0.235*** (0.039)
VARIABLES	(5) Program Rank (percentile)	(6) Master Graduate	(7) Master in STEM	(8) Earnings (Age of 30)
One Predicted Bonus Point x Post-Reform	-0.010*** (0.002)	-0.023*** (0.002)	-0.019*** (0.002)	-6,471*** (1,600)
Panel B: No Household Income				
VARIABLES	(1) Share of Bonus Points Courses	(2) Share of Specialization Science Courses	(3) High School GPA	(4) HS GPA + Bonus Points
One Predicted Bonus Point x Post-Reform	-0.016*** (0.001)	-0.014*** (0.001)	0.265*** (0.035)	-0.301*** (0.039)
VARIABLES	(5) Program Rank (percentile)	(6) Master Graduate	(7) Master in STEM	(8) Earnings (Age of 30)
One Predicted Bonus Point x Post-Reform	-0.011*** (0.002)	-0.020*** (0.002)	-0.018*** (0.002)	-7,103*** (1,600)
Panel C: No Mother Employment Status nor Household Income				
VARIABLES	(1) Share of Bonus Points Courses	(2) Share of Specialization Science Courses	(3) High School GPA	(4) HS GPA + Bonus Points
One Predicted Bonus Point x Post-Reform	-0.016*** (0.001)	-0.014*** (0.001)	0.266*** (0.035)	-0.300*** (0.039)
Observations	120,193	120,193	120,186	120,186
VARIABLES	(5) Program Rank (percentile)	(6) Master Graduate	(7) Master in STEM	(8) Earnings (Age of 30)
One Predicted Bonus Point x Post-Reform	-0.011*** (0.002)	-0.020*** (0.002)	-0.018*** (0.002)	-7,044*** (1,600)
Observations	79,991	120,231	120,231	98,674

Notes: This table reports robustness estimates from the dose-response difference-in-differences specification. The sample includes students who graduated from high school between 2005 and 2011. Each panel re-estimates equation 4 while excluding selected controls from the baseline specification: Panel A excludes mother's employment status; Panel B excludes household income; Panel C excludes both controls. Columns (1)-(4) report effects on high school course choices and performance. Columns (5)-(7) examine postsecondary outcomes, and Column (8) reports effects on annual earnings at the age of 30. Reported coefficients are interactions between predicted bonus points and the post-reform indicator. Standard errors, reported in parentheses, are clustered at the middle school GPA group-by-cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A10: Descriptive Statistics for the Survey Sample

	Statistic
Participants (N)	491
Age (mean)	17.4 years
Gender	
Male (N)	166
Female (N)	290
Non-binary (N)	8
Prefer not to answer (N)	5
GPA (mean)	4.67
Father's Education	
Less than University (N)	244
Bachelor's (N)	113
Master's or Greater (N)	98
Mother's Education	
Less than University (N)	194
Bachelor's (N)	153
Master's or Greater (N)	108

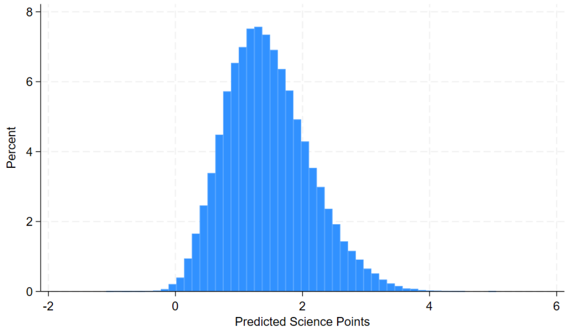
Notes: This table shows descriptive statistics from the survey sample.

Table A11: Conjoint Analysis of Interest in Switching Courses to Earn a Bonus Point Excluding Language Courses

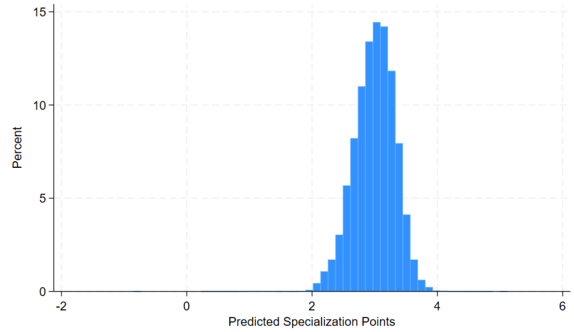
	Interest in Switching
(Intercept)	5.956***
	(0.141)
1 point grade cost	-1.394***
	(0.171)
2 point grade cost	-2.558***
	(0.176)
Observations	1523

Notes: This table shows estimates from the conjoint that assessed participants' willingness to switch to a course that granted one additional bonus point excluding language courses. The outcome variable is a 10 point Likert scale indicating respondents' interest in switching in to a randomly selected course for one additional bonus point with a randomly assigned grade cost of 0, 1 or 2 grade points. The scale ranged from 1-Not very interested to 10-Very interested. The analysis shows the analysis from column 1 in Table 11 excluding language courses. Standard errors clustered at the respondent level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure A12: Predicted Bonus Points Distribution by Type



(a) Predicted Science Points



(b) Predicted Specialization Points

Notes: This figure displays the distribution of predicted bonus point exposure by type. Panel a shows the distribution of predicted science bonus points, and Panel b shows the distribution of predicted specialization bonus points. Predicted bonus points are constructed as described in equation 2, based on students' pre-reform course-taking patterns and middle school GPA profiles. The sample is restricted to students who graduated from high school between 2005 and 2011. The histograms report the percentage distribution of predicted exposure within the sample.