Educational Incentives and School Choice*

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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 register data and a dose-response difference-in-differences design, we show that students respond by dropping science and specialization courses in favor of easier subjects. This shift leads to slightly higher high school grades but lowers overall admission scores, reducing access to selective college programs. Affected students are subsequently less likely to complete STEM college degrees and pursue masters degrees. We also find moderate but economically meaningful reductions in expected earnings at age 35. To understand behavioral responses, we conducted a novel survey of high school students, which reveals that decisions about courses are shaped by perceived difficulty and information gaps, with the strongest responses among high-achieving students. Our findings demonstrate how seemingly small changes in education policy can alter the supply of skills, shape long-run labor market outcomes, and affect inequality in access to opportunity.

JEL Codes: I21; I24; I26; I28; J24
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1 Introduction

Students often base their educational choices on perceived returns (e.g., Arcidiacono et al., 2012), but informational frictions, institutional constraints, and behavioral biases can distort these decisions. As a result, choices may diverge from labor market demand, contributing to skill mismatches, worker shortages in critical occupations, and lost productivity. Addressing these inefficiencies is critical for aligning talent with economic needs and sustaining long-term growth.

To better align human capital supply with labor market demand in the presence of frictions, two classes of policies are commonly considered: increasing the returns to specific fields or lowering the relative costs of enrollment. While raising returns is often difficult, reducing costs—whether financial, institutional, or psychological—is more feasible but has uncertain effects. Lowering costs may encourage more students to enter high-demand fields, alleviating skill shortages and improving economic efficiency. However, it can also result in students enrolling in programs that do not align with their interests or abilities, reduce their motivation to work hard, and ultimately hinder the development of valuable skills. The challenge lies in designing interventions that correct distortions without introducing new inefficiencies - an issue central to education policy and labor market design.

This paper provides a comprehensive analysis of how targeted changes in the cost of enrolling in specific subjects shape students educational and career trajectories. We study a Norwegian reform that eliminated college admission bonus points for science and advanced specialization courses, effectively reducing the incentive to enroll in these courses in high school. Using unique individual-level register data, we analyze how this policy shift affected course selection, college enrollment, and long-term labor market outcomes. By evaluating both efficiency and equity implications, we capture not only immediate behavioral responses but also broader consequences for human capital allocation and skill supply. Our findings provide key insights into how targeted changes in educational incentives can reshape the future workforce and labor market dynamics.

We leverage unique features of the Norwegian education system and a policy reform that directly increased the cost of enrolling in specific high school courses, providing a natural setting to examine how students respond to targeted reductions in course incentives. Admis-

sion to higher education in Norway is centralized and primarily determined by high school GPA, with additional bonus points awarded for completing certain courses, particularly in hard sciences and advanced specialization fields. Introduced in 1998 to encourage STEM enrollment, this bonus point system effectively reduced the relative cost of taking these select courses by boosting admission scores.[1] In 2006, the government sharply reduced the bonus points for these courses, increasing the relative cost of enrolling in them.

The 2006 reform provides a unique opportunity to study students' adjustments to their course selection, educational attainment, and career trajectories when the cost of enrolling in specific subjects rises. Using a dose-response difference-in-differences framework, we track the effects on high school course choices, college enrollment, and long-term labor market outcomes. By connecting these findings to broader discussions on skill mismatches and labor market frictions, we provide new insights into how targeted cost changes in education influence human capital formation, economic efficiency, and the supply of skilled labor.

Beyond our main analysis, which leverages rich, population-wide register data, we draw on survey evidence to further explore how students respond to changes in educational incentives. By directly asking students how they make their course selections, we provide clear evidence on how they perceive and react to shifts in incentives. Together, these analyses deepen our understanding of the broader implications of targeted incentive policies and the behavioral channels through which they operate.

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 important implications for understanding how incentive structures shape skill supply and the allocation of talent. We organize the core results around three central findings: immediate shifts in course selection, long-term educational consequences, and effects on labor market outcomes.

First, students respond immediately to the removal of incentives by opting out of science and advanced specialization courses, substituting them with easier alternatives. While these courses yield slightly higher high school grades due to a lower difficulty level, the performance gains are too small to fully compensate for the lost bonus points, leading to lower admission

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

scores and restricted access to higher education opportunities.

Second, these shifts in course selection translate into long-term differences in educational attainment. Students more exposed to the reform enroll in lower-quality college programs, are less likely to pursue STEM degrees, and experience a decline in master's degree completion. These effects highlight how seemingly small adjustments in high school incentives can fundamentally alter the composition of students' human capital entering the labor market.

Third, these educational shifts have significant labor market consequences. Affected students are less likely to enter high-return fields, and they experience a substantial drop in their expected occupational earnings premium and the likelihood of holding management roles by age 35. In other words, the removal of bonus point incentives steers students toward lower-return majors, limiting career mobility and weakening the long-term supply of high-skilled workers.

These patterns reflect more than just adjustments in course-taking, they underscore the broader role of incentive design in shaping educational and labor market trajectories. Important for the interpretation of these results is the fact that the bonus point system we study operates on the extensive margin: students earn additional admission points simply for completing advanced courses, regardless of performance. This design encourages students to enroll in more demanding coursework without mechanically undermining their effort. Our findings show that removing these incentives alters individual course choices and, more broadly, distorts the allocation of talent.

To better understand how students respond to incentive structures, we field a survey of Norwegian high school students. The survey yields three main findings. First, there is substantial heterogeneity in awareness of the bonus point system. Students with higher GPAs and more educated fathers are significantly more likely to understand how the system works while lower-performing students and those from less-educated families are less informed and may miss opportunities to improve their admissions chances. 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.

These findings underscore that bonus point incentives operate through both informational and perceived academic cost channels. Because awareness is uneven and students weigh incentives against expected grades, such policies may primarily influence high-achieving or well-informed students. This dynamic helps explain why the removal of bonus points disproportionately affected advantaged groups — and why such incentives may unintentionally reinforce inequalities in access to selective educational tracks.

By leveraging detailed individual-level panel data, this paper provides novel causal evidence on how shifting costs for individual classes shape students educational choices and long-term labor market trajectories. Combining these policy variations with survey evidence on student decision-making, we offer new insights into the role of targeted incentives in shaping skill supply, career progression, and labor market efficiency. Our findings contribute to several strands of literature.

First, a long-standing body of literature examines college major choice, investigating how students select their fields of study based on anticipated returns and personal preferences (e.g., Wiswall and Zafar, 2015; Bordon and Fu, 2015; Kirkeboen et al., 2016; Zafar, 2013). This research has established that factors such as expected earnings, individual interests, and economic primitives like risk aversion strongly influence students decisions. We extend this literature by showing that relatively small, policy-driven changes in the incentive structure-operating independently of students perceived returnscan meaningfully shift course choices. By exploiting a reform to the Norwegian bonus point system, we provide new causal evidence on how such incentives shape educational trajectories and, in turn, long-term labor market outcomes. Our findings suggest that institutional rules and admissions mechanisms can play a central role in directing the allocation of human capital, sometimes creating misalignment between student preferences and labor market needs. This perspective broadens the existing framework for understanding major choice by highlighting the importance of intermediate policy levers in shaping students educational paths.

Second, this paper provides new insights into STEM shortages and barriers, a topic that has received substantial attention in recent years (e.g., Arcidiacono et al., 2016; Stinebrickner and Stinebrickner, 2014; Black et al., 2021). Existing studies emphasize academic and financial barriers as key drivers of the persistent underrepresentation of students in STEM fields. We show that external incentives that alter the cost of STEM participation can contribute to

this problem, reducing the number of STEM graduates and exacerbating labor shortages in high-demand fields. Our findings demonstrate that even intermediate incentives—unrelated to academic quality—can significantly influence student demand for STEM courses, highlighting the broader role of policy design in shaping the supply of skilled labor.

Third, a growing body of research examines the effects of academic leniency and grading standards on 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). These studies highlight how lenient grading can inflate grades without improving actual skills, widen performance gaps between high- and low-performing students, and influence major selection. Bowden et al. (2023) shows that grading leniency can distort student performance measures, while Hvidman and Sievertsen (2021) finds that it alters academic pathways. Ahn et al. (2024) demonstrates that grading standards in STEM fields can affect female students participation, and Dee et al. (2019) provides evidence that score manipulation in high-stakes exams can artificially boost graduation rates without strengthening preparedness. Our study takes a different approach by examining a policy that removed incentives for taking more challenging courses without directly impacting student effort. We show that students respond to such effort-neutral incentives by shifting away from advanced courses, leading to weaker qualifications for higher education and poorer labor market outcomes. These findings suggest that even policies that leave grading standards untouched can significantly affect students academic choices and long-term prospects.

Finally, this research contributes to the literature on talent allocation and educational match (e.g., Hvide, 2003; Nechyba, 2006; Dillon and Smith, 2020; Black et al., 2023). We provide evidence that weakening academic incentives for challenging courses reduces the supply of specialized human capital and disrupts the distribution of talent across fields and specializations. This reallocation has broad labor market implications, affecting wages, employment, and STEM participation, with potentially important long-term consequences for productivity and innovation.

While the reform we study is specific to Norway, the underlying mechanism — students responses to changes in the relative cost of courses — has broader relevance. Many countries face similar challenges in aligning educational choices with labor market needs, especially in STEM fields. Our results point to a more general insight: incentive structures embedded in

admissions systems and course design can shape human capital in ways comparable to more visible policies like tuition subsidies or targeted scholarships. Recognizing the role of these often-overlooked levers is critical for designing education systems that allocate talent more effectively.

2 Background

To examine how external incentives that raise the cost of enrolling in certain courses influence students educational choices and labor market outcomes, we leverage a unique 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 2006 reform that reduced incentives for taking these more demanding courses.

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. There are two tracks available: an academic track, which prepares students for higher education (3 years), and a vocational track, which leads to a trade or journeyman's certificate but does not provide direct access to higher education (3-4 years). Approximately 50% of students choose each track. Admission to high schools is competitive, based solely on students' GPA from compulsory schooling.

In the academic high school track — the focus of this study — students choose one of five programs: specialization in general studies, sports and physical education, art, design and architecture, media and communication, and music, dance and drama.[2] The specialization in general studies program is by far the most common, enrolling approximately 70% of students in the academic track.

All students follow a common core curriculum but also take program-specific subjects.

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

Within their chosen program, students must complete a minimum number of instructional hours, although they have substantial flexibility to take electives from other programs. Most electives are chosen in the final year of high school.

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.[3] Some majors require specific high school courses (e.g., biology, chemistry, and physics for medical school). 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 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.

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 pre-requisites, admission is exclusively based on a students 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 strategy proof.

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) plus the total number of earned bonus points.

The central government introduced bonus points in 1998 to encourage specific demographic groups to pursue higher education and to steer students toward certain major choices.

[3]Individuals can also qualify through the vocational track by completing additional courses, though this pathway is less straightforward.

Most of these points are tied to characteristics that students cannot easily influence, such as age, military service, and gender.[4]

However, one key category of bonus points directly rewards academic choices: students receive additional admission points for taking specific high school courses in science and advanced specialization subjects.[5] These academically targeted incentives play a central role in our analysis, as they directly affect students course selection and higher education opportunities.

Students earned science points by completing advanced courses in mathematics, biology, physics, and chemistry. For example, taking an advanced physics course granted 1 science point. 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. A student's base admission score was primarily calculated as their average GPA multiplied by 10. Earning 1 bonus point was more beneficial than improving three full letter grades in a core subject (e.g., from D to A) with respect to raising the application score, making these incentives a powerful lever for boosting admission chances.

Students could earn up to 10 bonus points through their selection of science and advanced specialization courses—6 points from hard science courses and 4 points from specialization courses. The GPA component of the admission score ranged from 10 to 60, calculated as the students average GPA (on a 16 scale) multiplied by 10. Bonus points were then added to this base score to determine the final admission score. Since each letter grade increase across all courses raises the admission score by 10 points (e.g., going from all Bs to all As moves a student from 50 to 60), earning the full 10 bonus points could compensate for an entire grade step across all subjects. This made bonus points a powerful tool for improving admission

[4]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.

[5]Rules for science points can be found in the 2005 regulation for higher education admission for cohort 2006/7 are in Chapter 7 of the laws on higher education and can be viewed at the link here. Rules for specialization points can be found in the 2005 regulation for higher education admission for cohort 2006/7 Chapter 9 of the laws on higher education and can be viewed at the link here.

chances, as they could significantly boost rankings without requiring higher grades in core subjects.

In 2006, the Norwegian government changed the bonus point system by introducing two major adjustments to the scheme related to bonus points for science and advanced specialization courses. 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.[6] As a result, the potential bonus points available dropped from 10 to 4, effectively increasing the cost of enrolling in advanced specialization and science courses without diminishing the benefits associated with student effort. The law was passed in 2006 but did not affect students already enrolled in high school. Therefore, the first cohort impacted by the reform was the one that started high school in 2006 and graduated in 2009.

Figure 1 illustrates the reforms impact on bonus point accumulation. Panel 1a shows a sharp drop in the average number of bonus points earned, from about 5 in the pre-reform period to just 1 post-reform. Panel 1b highlights how the distribution compressed after the reform: while pre-reform students earned between 0 and 10 points — with many clustered at the upper endpost-reform students earned no more than 4 points, and many received none at all.

While the reform significantly reduced the incentive of pursuing science and advanced specialization courses by limiting the bonus compensation for these subjects, it is important to note that most higher education institutions specializing in STEM still require students to take the most advanced courses in mathematics and hard sciences during high school as a program prerequisites. That means students still needed those courses to qualify for selective programs, but now faced lower rewards for taking thempotentially creating a mismatch between admissions incentives and program prerequisites.

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 restrict the analysis to

[6] See 2008 “Forskrift om endring i forskrift om opptak til høyere utdanning”, chapter 7.

students graduating between 2005 and 2011—capturing four pre-reform cohorts and three post-reform cohorts.[7]

We draw on data 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 focus on Grade 13 course selection and grades, as this is the year when students typically take specialization and advanced science courses that determine bonus point eligibility and program prerequisites. 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. Together, these data provide detailed information on age, gender, immigration status, parental characteristics, socioeconomic conditions, and labor market outcomes ten years after high school completion. The result is a comprehensive longitudinal panel linking the full population of Norwegian students to rich demographic, educational, and labor market data.

The richness of our data allows us to examine a wide range of outcomes, capturing various margins of student adjustment in response to the reform. We begin by analyzing immediate behavioral responses, focusing on enrollment in science and advanced specialization courses. These outcomes shed light on whether students respond to changes in incentives when selecting courses, potentially indicating shifts in academic focus.

Next, we assess whether students adjust the difficulty of the courses they choose. To do this, we construct a measure of course difficulty based on *relative grades*. For each course in the pre-reform period, we calculate the average ratio of a student's absolute grade to their overall GPA in their final year:

$$\text{Relative Grade}_{ij} = \frac{\text{Absolute Grade}_{ij}}{\text{GPA}_i}$$

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

where GPA_i is student i's average last year of high school grade. This provides a within-student benchmark, capturing how challenging each course is relative to their general academic performance. 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 ability. While relative and absolute grades are usually closely aligned — suggesting stable course difficulty over time —some subjects, particularly in science and math, show lower relative grades. This reflects positive selection: stronger students are more likely to choose these harder subjects, which raises their average absolute grades but lowers their relative grades when adjusted for each student's overall GPA.

Third, we analyze performance effects by examining changes in high school GPA and college admission scorestwo key indicators of academic success in Norway. The expected impact on these outcomes is theoretically ambiguous. If the reform led students to select easier courses, GPA may rise, partially offsetting the drop in admission scores due to the removal of bonus points. However, shifts in course selection could also affect the alignment between students motivation, abilities and their coursework: a poorer match may reduce GPA, while a better match could enhance it.

Fourth, we examine the reforms impact on higher education outcomes, focusing on how it influenced both the set of programs students could access and the quality of those they ultimately attended. We focus on three key dimensions: program availability, program quality, and educational trajectory.

In terms of program availability, we define each students opportunity set as the set of college-program combinations for which their final admission score meets the admissions cutoff. 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. This shift may have led some students to enroll in less selective institutions or programs that were not their first choice.

In terms of program quality, we use the academic credentials of a students peers in their chosen program as a proxy for quality. Specifically, we measure quality by the average high

school GPA of students in the same cohort and program. If the reform systematically lowered the admission scores of certain students, it may have caused sorting effects, pushing them into programs with peers who had lower average 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 the lower admission scores.

In terms of educational trajectory, we examine two detailed higher education outcomes: STEM college degree attainment and any master degree.

Fifth, we assess labor market implications by focusing on the expected wage premiums associated with students educational choices. Because the reform occurred in 2009 and our data end in 2019, we cannot observe earnings at prime working age (35), which are highly predictive of lifetime income.[8] Instead, we use national education and labor market registers to estimate age-35 earnings for each program-university combination in Norway. We implement a Mincer-style wage regression that includes cohort and municipal fixed effects, and use the predicted earnings as a measure of the reforms anticipated labor market effects. We apply the same approach to estimate the probability of holding a management position at age 35. Earnings are defined as the sum of cash wages and salaries, including taxable sickness and parental leave benefits. Together, these predicted outcomes capture both the immediate academic adjustments and the longer-term economic consequences of the reform.

We complement our administrative analysis with evidence from a nationally representative 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 further probe responses to external incentives, 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 full survey instrument is included in the appendix.

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

4 Empirical Specification

To examine how external incentives influence students course selection and, in turn, shape educational attainment and labor market outcomes, we exploit the Norwegian points reform using a dose-response difference-in-differences specification.

Our approach builds on the idea 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. On a conceptual level, to construct a measure of exposure to the reform, we first estimate a model using pre-reform cohorts that predicts how many bonus point-eligible courses a student would take based on observable characteristics such as 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 old system. This predicted number of lost bonus points serves as our treatment intensity measure. Variation in predicted exposure allows us to estimate the effect of losing bonus points by comparing students with different baseline propensities to take qualifying courses before and after the policy shift.

To formalize our approach, we implement a dose-response difference-in-differences design, leveraging within-cohort variation in exposure to the policy over time. Rather than comparing two distinct groups, as in a conventional difference-in-differences approach, we exploit the continuous variation in how much each student was affected by the reform. This enables us to measure the impact of explicit, targeted, incentives on students high school course choices, their subsequent educational opportunities, and their eventual labor market performance.[9]

As discussed above, the key challenge we face is that we do not directly observe how many bonus points students would have earned post-reform had the policy not been implemented. To estimate this counterfactual, we take advantage of the rich Norwegian register data and predict students likely bonus points based on a detailed set of demographic and educational background characteristics determined before the announcement of the reform. Specifically, we use data from pre-reform cohorts (2005 – 2008) to estimate a model that predicts bonus

[9]Students received bonus points toward higher education simply by passing these courses. This structure preserved effort incentivesstudents still earned admission points based on their GPAwhile encouraging them to take on more challenging coursework.

point accumulation as a function of individual characteristics. We then apply this model to post-reform cohorts, generating a counterfactual measure of how many bonus points students would have received in the absence of the reform.

This predicted number of lost bonus points serves as our dosage variable, capturing how much each student was affected by the policy change. We use this measure to estimate the impact of the reform on course selection, admission outcomes, and longer-term educational and labor market trajectories. Specifically, we estimate the following regression to construct the dosage variable:

$$BonusPoints_{ic} = \beta + \gamma X'_{ic} + \epsilon_{ic}, \tag{1}$$

where X'_{ic} is a vector of prediction variables and includes; sex, primary school GPA, parental education (level and program), parental employment status, household income (per capita), birth month, municipality of birth, primary school identifier.

The results from this exercise are shown in Table 2, 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 68.

To validate our treatment intensity variable, we examine its relationship with actual bonus points among students who graduated before the reform. As shown in Figure 3, the correlation between predicted and actual bonus points exceeds 0.9, confirming that our modelbased on demographic and educational background characteristicsclosely approximates the number of bonus points students would have received in the pre-reform period. This strong correlation supports the validity of our dosage measure, which we use in the following event study specification to estimate the reforms impact:

$$\begin{aligned} Y_{ict} = \alpha + \sum_{q=2005}^{2011} \pi_q \left(1[c = q] \cdot \widehat{\text{BonusPoints}}_{ic} \right) \\ + \gamma X'_{ic} + \widehat{\phi}_{pre}(2008 - c)\, \widehat{\text{BonusPoints}}_{ic} + \epsilon_{ict}, \end{aligned} \tag{2}$$

where Y_{ict} is an outcome for individual i in cohort c at time t. The vector X'_{ic} is defined as above. We cluster our standard error at the school level. Since our analysis spans multiple cohorts, we include $\widehat{\phi} pre(2008 - c)\widehat{BonusPointsic}$ 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 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. We return to this specification in the results section, showing that it absorbs a mild pre-trend in GPA, while all other outcomes are unaffected by its inclusion. Since we focus on course selection in 13th grade, we also add as a control the share of both science and specialization courses instructional hours in 12th grade.

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

$$\begin{aligned} Y_{ict} = \alpha + \pi_1 \text{Post}_t + \pi_2 \widehat{\text{BonusPoints}}_{ic} + \pi_3 (\text{Post}_t \times \widehat{\text{BonusPoints}}_{ic}) \\ + \gamma X'_{ic} + \widehat{\phi}_{pre} (2008 - c) \, \widehat{\text{BonusPoints}}_{ic} + \epsilon_{ict}, \end{aligned} \quad (3)$$

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.

Our empirical approach is a dose-response difference-in-differences specification. Identification comes from variation in exposure to the reform, measured by the number of bonus points students were predicted to receive in the post-period had the policy not been implemented. To interpret our estimates 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 strong relationship, with an F-statistic of 68 (Table 2) and a correlation exceeding 0.9 between predicted and actual bonus points (Figure 4). This confirms that our prediction model, based on demographic and educational characteristics, captures meaningful variation in counterfactual bonus point exposure and satisfies the relevance assumption for causal inference.

Interestingly, we find that the students most affected by the reform — those with the highest predicted bonus point losses—are disproportionately high-achieving students 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 assume that, in the absence of the reform, students would have followed similar trends in outcomes over time. While this assumption cannot be tested directly, we assess its plausibility using event study estimates, which show no evidence of differential trends in the full sample prior to the policy change (Figure 4).

Because our design relies on a continuous difference-in-differences frameworkwhere treatment intensity is based on predicted bonus point exposure – we also examine whether pre-trends differ across levels of predicted exposure. To do so, we divide students into terciles based on predicted bonus points and estimate event study coefficients separately for each group. In this analysis, all effects are measured relative to the middle tercile. As shown in Figure 5, students in the top and bottom terciles follow similar trends to those in the middle group before the reform, supporting the plausibility of parallel trends across treatment intensities.

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.

5 Results

In this section, we present our core results on how policies that lower the cost of taking specific courses influence students' course selection, educational attainment, and labor

market outcomes. We begin by examining the immediate behavioral response, focusing on changes in the probability of pursuing science and specialization courses that previously yielded bonus points. Next, we analyze performance effects, including high school GPA and overall admission scores. We then assess broader educational outcomes, such as college quality, the likelihood of enrolling in STEM programs, and pursuit of master's degrees. Finally, we evaluate the long-run implications for labor market outcomes, including predicted earnings at age 35 and the probability of holding a managerial position.

Following the main findings, we conduct robustness and sensitivity analyses to rule out alternative explanations and strengthen our causal interpretation. Lastly, we present our survey data to disentangle mechanisms and validate our results.

Behavioral Response. Figure 4 presents our event study results based on Equation 2, 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 based on our dosage variable, supporting the common trends assumption necessary for causal inference. Second, there is an immediate decline in the number of science and specialization courses taken following the reform, indicating a prompt response to the change in course incentives.[10]

To facilitate the interpretation of this result, Table 3 reports the coefficients from our simple difference-in-differences specification, estimating the effect of the reform on (1) the number of courses that previously qualified for bonus points, (2) the number of hard science courses in this category, and (3) the number of advanced specialization courses.

The results indicate that students shifted away from both science and specialization courses in response to the reform. Scaling the point estimate by the pre-policy mean, the effect size is approximately 5% per predicted bonus point. Given an average predicted bonus of 5.2 points, this translates to a total reduction of about 25% relative to the pre-policy mean.

By assigning a relative "difficulty" score to each high school course-based on the average grade received by students in the pre-reform period-we find clear evidence of course difficulty substitution. Students shifted away from science and advanced specialization subjects and

[10]Notably, Appendix Table A6 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).

toward courses associated with higher average grades (by 0.3 points relative to a mean of 100), as shown in Figure 6 and Table 4. This pattern suggests a strategic response to the reform: in the absence of bonus points, students appear to have compensated by selecting courses that are relatively easier to perform well in.

Examining these changes more closely, we find that students became more likely to take an English course, while enrollment in advanced science courses[11] declined by over one percentage point per predicted bonus point reduction. This pattern reinforces the interpretation that students affected by the reform substituted away from more demanding subjects toward courses perceived as easier.

Taken together, these results show that students responded quickly and strategically to the removal of bonus point incentives. Rather than just absorbing the decline in admission scores caused by the elimination of bonus points, they substituted away from academically demanding subjectsparticularly advanced science and specialization coursestoward courses that were easier to score well in. This behavioral adjustment highlights the power of seemingly modest policy incentives to shape curricular choices at a critical stage of skill formation.

Performance Effect. To examine the impact of the course switching behavior documented in the previous subsection on student performance, we estimate our event study specification using high school GPA, as well as college admission score, as outcomes. The results from these exercises are shown in Figure 7. Note that GPA is standardized such that the interpretation is in percentage of standard deviations.

There are three important take-aways from the results provided in Figure 7. First, there is no evidence of differential pre-trends as a function of our dosage variable either in terms of GPA or admission score, providing support for the common trends assumption required for causal inference in our setting. Second, the GPA improves slightly in response to the immediate course adjustments that the students make in response to the incentive reform, consistent with the notion that the students are switching to easier classes in order to offset the drop in bonus points. However, this effect is economically very small, and it does not occur immediately. Third, there is a sharp drop in admission score following the reform, directly illustrating that the modest increase in GPA caused by students switching to slightly easier courses is not sufficient to offset the impact of the bonus point reduction on admission

[11] Advanced Math 2, Chemistry 2, Physics 2, Biology 2, and Geo-science 2

score.

To aid interpretation, Table 5 provides the coefficient from our simple difference-in-differences specification based on Equation 1, illustrating that the policy reform translates into an increase in GPA of 0.23 per predicted bonus point and a drop in admission score of 0.29.

The relatively weak effect on GPA may arise for several reasons. First, the match between students' abilities and courses may shift, potentially offsetting the decreased difficulty of the courses. Alternatively, because the courses are perceived as easier, students may reduce their effort in ways that are ultimately harmful to their grades. Finally, students who are predicted to earn bonus points may be getting high grades already, so there is a limit to how much the easier courses can help their grades. These findings highlight a key tension in incentive design: while students responded strategically, their adjustments did not necessarily improve their overall admissions outcomes (a result we explore in much more detail below). Our analysis does not allow us to disentangle the relative importance of these three mechanisms, we view this as an interesting avenue for future research.

Education Quality and Attainment. The results thus far indicate that the reduced incentives for studying science and advanced specialization courses led students to substitute these subjects for alternatives that appeared easier on paper. However, this course switching did not fully compensate for the decline in admission scores caused by the loss of potential bonus points. This shortfall may have broader implications for students college opportunities and choices, which we now examine.

First, Panel 8a of Figure 8 shows results from our event study using the number of program-college combinations that the students qualify for given their admissions score. This provides a rough proxy of the impact that the incentive reform had on education opportunities at the college level. Panel 8b of the same figure presents results from our event study using the minimum admissions score that the peers of the student had in the college major that the student eventually chose. As discussed in Section 3, we use this measure as a proxy for the quality of the program-college combination that the student eventually ends up in. Results from our simplified difference-in-differences specification are provided in Table 6.

Both panels in Figure 8 show stable pre-trends in outcomes across treatment groups,

providing strong support for the common trends assumption. In the post-reform period, both panels reveal a sharp decline in quality measures by the second year, suggesting a significant reduction in both field quality and college availability as a consequence of the reform.

To further explore these effects, we estimate our event study specification using two additional outcomes: (a) the probability of obtaining a master's degree and (b) the probability of earning a STEM degree.

The results for this exercise, presented in Panels 9a and 9b of Figure 9, indicate a substantial decline in STEM degree attainment and a reduction in the likelihood of earning a masters degree. A key mechanism behind the STEM effect is the loss of prerequisite coursework: STEM programs often require advanced math and science courses, and students affected by the reform may no longer meet these requirements after shifting away from these subjects. Estimates from our simplified difference-in-differences specification, reported in Table 7, further support these findings.

Labor Market Effects. The results presented in the prior section indicate that a reduction in incentives for more challenging high school courses leads to significant behavioral changes in students' course selections as they attempt to mitigate the policy's impact on their college admission scores. However, the decision to change courses was insufficient to offset the decline in admission scores caused by the reduction in potential bonus points. Consequently, the reform resulted in lower admission scores for these students and a subsequent reduction in access to high-quality college programs. This has important implications for their likelihood of pursuing STEM degrees and postgraduate education, which are closely associated with beneficial wage premiums later in life.

To obtain an aggregate measure of the overall implications of the educational effects on exposed students, we estimate the impact on students' wage premiums at prime working age-age 35 (e.g., Bihlmayer and Lindquist, 2020; Solon and Haider, 2006). Since the reform occurred in 2009 and our data conclude in 2019, we cannot directly examine earnings at this age. Therefore, we utilize the population-wide labor market and education register to predict the earnings premium at age 35 for each program-university combination available in Norway. This prediction is based on a Mincer wage equation that includes cohort and municipal fixed effects. We use the imputed earnings premium as a measure of the anticipated

labor market effects of the reform. Using the same approach, we also estimate the likelihood that the individual will end up in a managerial position by age 35. The results from this exercise are shown in Panels 9c and 9d of Figure 9 (event study specification) and Table 7 (Difference-in-Differences specification).

There are three main takeaways from these analyses. First, the event studies shows a flat and stable relative trend in the outcomes as a function of treatment status in the pre-shock years, providing strong evidence in favor of the common trends assumption. Second, the results indicate a slight decline in the probability of exposed individuals holding managerial positions following the incentive reform. Third, there is a significant decrease in the earnings premium for these individuals at age 35, which occurs relatively quickly and persists over the next several cohorts. In terms of magnitude, fully exposed individuals experience an annual earnings reduction of approximately 35,000 NOK (about $3,300),or 7.7% percent relative relative to non-exposed cohorts mean. This reduction is substantial, equivalent to moving 15 percentiles from the median in the income distribution among 35 year olds.

Interestingly, although the reform induces behavioral responses across the full student distribution, our first-stage results demonstrate that the effects are concentrated among high-achieving studentsthose with the highest predicted exposure to bonus points. As a result, our findings suggest that the reform narrows gaps in course-taking and downstream outcomes across the ability and socioeconomic distributions, but does so by dampening the trajectory of students most likely to pursue advanced coursework and high-return fields. In this sense, the equalizing effect operates through a reduction in top-end participation rather than gains at the bottom. This dynamic underscores a key feature of the reform: although formally universal, its incentive structure disproportionately shaped the behavior of students with stronger academic profiles, leading to a regressive form of leveling in skill investment once it was removed.

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 Table 8 and demonstrate that the coefficients on predicted bonus points are positive and highly statistically significant across all subgroups examined.

Second, 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. Table 9 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.

Third, we examine the impact of adding or removing control variables in our specifications. By testing various specifications, we evaluate the stability of our estimates and ensure that our results are not sensitive to the inclusion or exclusion of specific controls. This exercise confirms that the observed effects are indeed attributable to the reform and are not confounded by omitted variable bias. Table 10 shows that excluding key control variables such as parental employment status or household income does not significantly alter our baseline findings. The consistency in the sign, magnitude, and statistical significance of the results indicates that our findings are robust to the inclusion and exclusion of additional controls and not driven by these specific characteristics. This reinforces the conclusion 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.

Fourth, we assess the importance of controlling for cohort-specific trends by estimating specifications that omit the linear trend in predicted exposure. Figures A1-A5 present the event study estimates from this alternative specification. While the main results remain stable, the GPA outcome exhibits a pre-trend if this control is removed. Including the linear cohort trend accounts for this and ensures that any differential trajectories by predicted

exposure are properly controlled for (Jakobsen et al., 2019).

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 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 A1). 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 an 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 11). 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 12, 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 13. 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 lead 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 offer an bonus point.[12] 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 = very interested). Column 1 of Table 14 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

[12]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.

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 A2). 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 pointsso 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 incentivesparticularly 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 systems 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 frequently base their educational choices on perceived returns. These choices do not always align with labor market demand or societal needs, creating negative social externalities and mismatches between the supply and demand for skilled labor. This misalignment can lead to critical shortages in essential occupations and industries, potentially impeding economic efficiency and growth.

Two fundamental strategies can be pursued to solve the mismatch issue and better align the supply of human capital with future labor market demands: increasing the returns associated with specific majors or reducing the costs of enrollment in those programs. While increasing returns is often challenging, reducing costs is generally more feasible. However, lowering costs can have theoretically ambiguous effects on overall human capital development.

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 ripple through 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 sightly better than their performance in the advanced courses they switch out from,

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 a drop in the predicted likelihood of exposed individuals securing management positions, along with meaningful reductions in expected wage premiums at age 35.

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, may thus have contributed to a more equitable distribution of educational opportunities. However, this reflects a regressive form of equalityachieved not by lifting up disadvantaged students, but by withdrawing a structural advantage from those already ahead.

These results provide valuable insights into how strategic changes in educational incentives can shape the future workforce and affect both individuals and communities. They also enrich the existing literature by highlighting the need for a comprehensive approach to understanding major choiceone 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.

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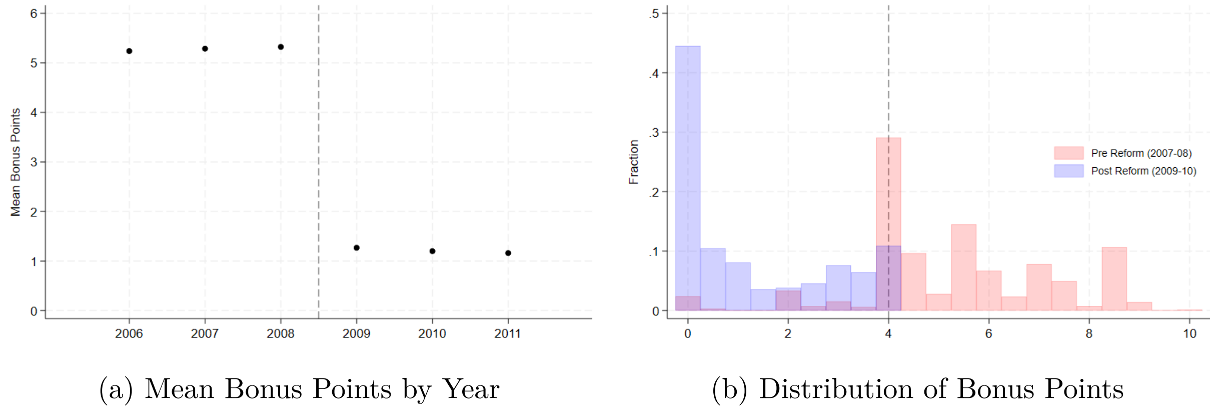
Tables and Figures

Figure 1: Pre- and post reform bonus point distribution



(a) Mean Bonus Points by Year

(b) Distribution of Bonus Points

Notes: This figure shows authors' calculations from register data generated by Statistics Norway. Panel 1a shows the mean bonus points by graduating cohort, spanning from 2006 to 2011. Panel 1b shows You are showing a hisogram of the bonus points for cohorts that graduated before the reform (2007-2008) and those that graduated after the reform (2009-2010).

Table 1: 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 (3rd grade)	0.334	0.167	0.266	0.186
College-Program Minimum Admission Grade	27.9	19.6	25.2	19.7
Share of Eligible College-Program	0.677		0.641	
Graduated in STEM Program	0.135		0.135	
Master Degree	0.133		0.133	
Education Wage Premium (1000 NOK)	48.3	11.2	48.1	11.2
Probability of Management Occupation	0.015		0.015	
School GPA	40.8	7.1	40.7	7.1
School GPA + Bonus Points	46.1	7.9	41.9	7.8

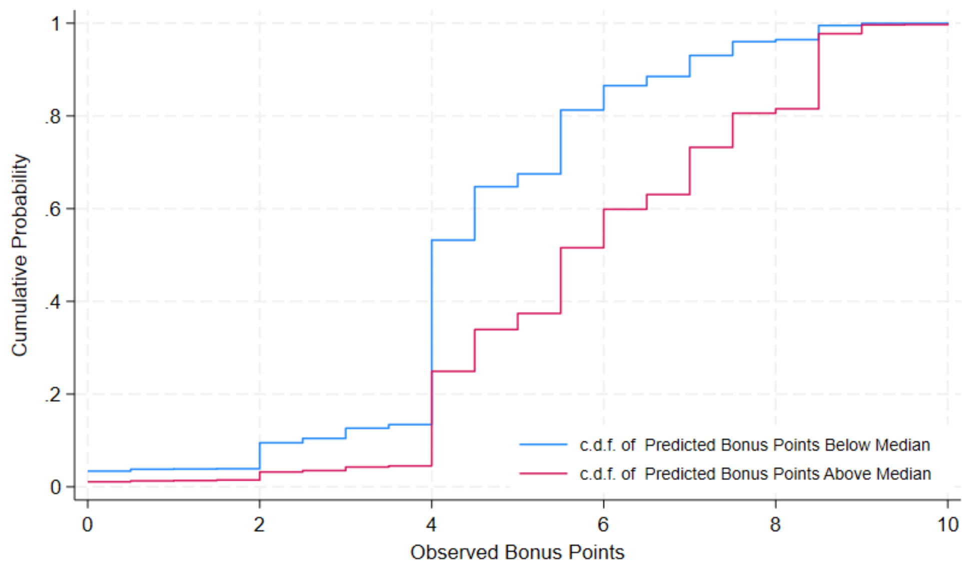
Notes: This table shows authors' calculations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008 (Pre Reform) and 2009 to 2011 (Post Reform).

Table 2: First Stage - Predicting Bonus Points

Variable	Estimate
Mother Employment Status	-0.022
	(0.028)
Father Employment Status	0.108***
	(0.030)
Middle School GPA (1-1.5]	0.773
	(0.769)
Middle School GPA (1.5-2]	0.334
	(0.316)
Middle School GPA (2-2.5]	0.556*
	(0.323)
Middle School GPA (2.5-3]	0.900**
	(0.315)
Middle School GPA (3-3.5]	1.202***
	(0.317)
Middle School GPA (3.5-4]	1.537***
	(0.315)
Middle School GPA (4-4.5]	1.812***
	(0.317)
Middle School GPA (4.5-5]	2.162***
	(0.315)
Middle School GPA (5-5.5]	2.078***
	(0.323)
Middle School GPA (5.5-6]	2.536***
	(0.317)
Man (Dummy)	0.520***
	(0.014)
Per Capita Household Income (ln)	-0.247***
	(0.009)
Observations	68585
R2	0.273
F(217,64517)	68.2
FEs: Parents Education (Level & Program); Municipality of Birth; Month of Birth; Year	

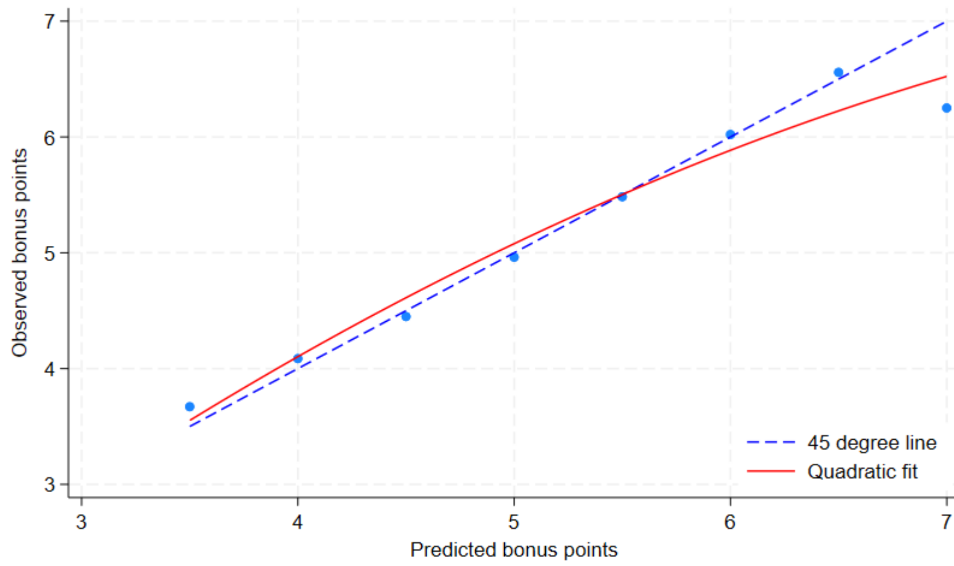
Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008. All estimates are calculations from equation 1. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Figure 2: CDF Monotonicity: Bonus Points CDF for the Cohorts Before the Reform



Notes: This figure shows authors' calculations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008. Predicted points are estimated in Equation 1, with a median of 5.2.

Figure 3: Predicted and Observed Bonus Points



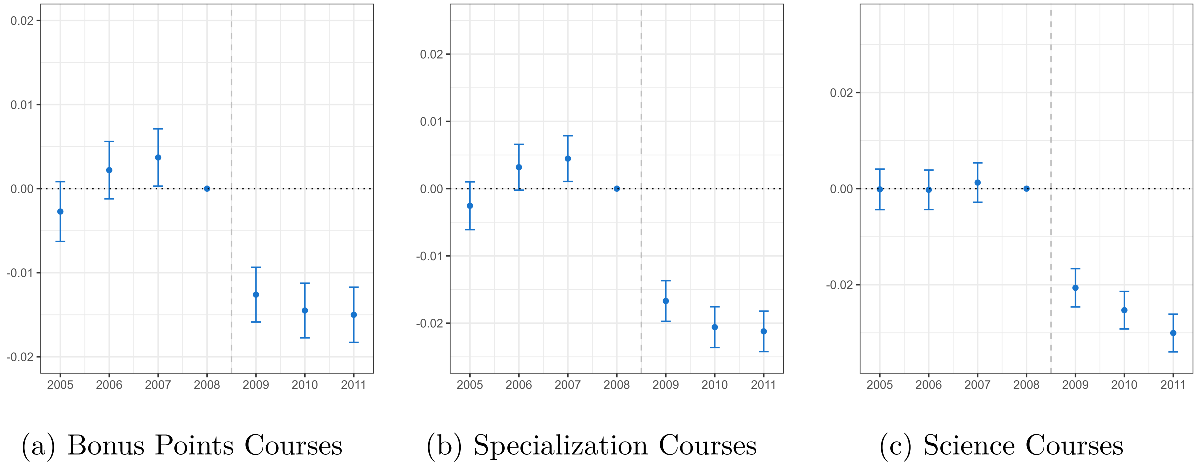
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008, before the reform. Predicted points are estimated in Equation 1. Dots are pooled in 0.5 intervals. Predicted bonus points are pooled below 3.5 and above 7.

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

	(1) Bonus Points Courses	(2) Specialization Courses	(3) Science Courses
One Predicted Bonus Point	-0.015***	-0.021***	-0.026***
x Post Reform	(0.001)	(0.001)	(0.001)
Average Effect	-0.077	-0.108	-0.133
Observations	119442	119442	119442
Pre-policy mean	0.334	0.321	0.164

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 3. Average Effect is the coefficient multiplied by the predicted bonus points mean (5.2). Standard errors are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

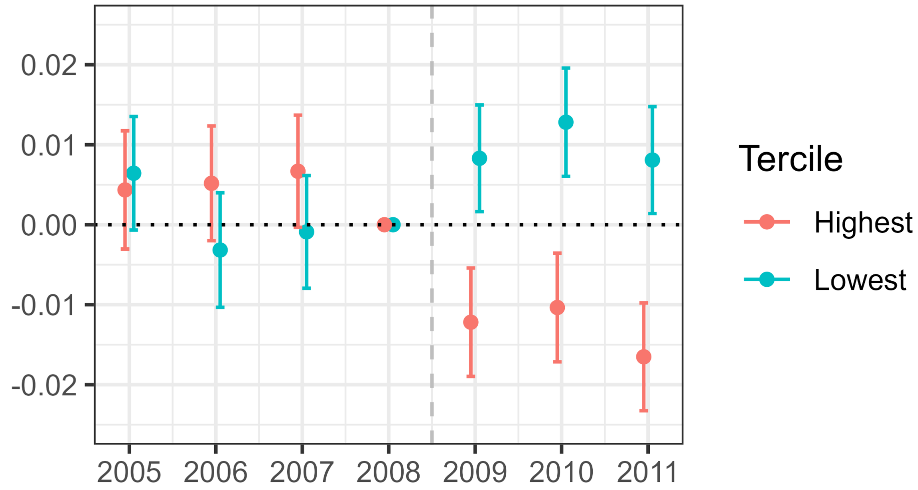
Figure 4: Effects on Course Allocation (Share of Total Instructional Hours)



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

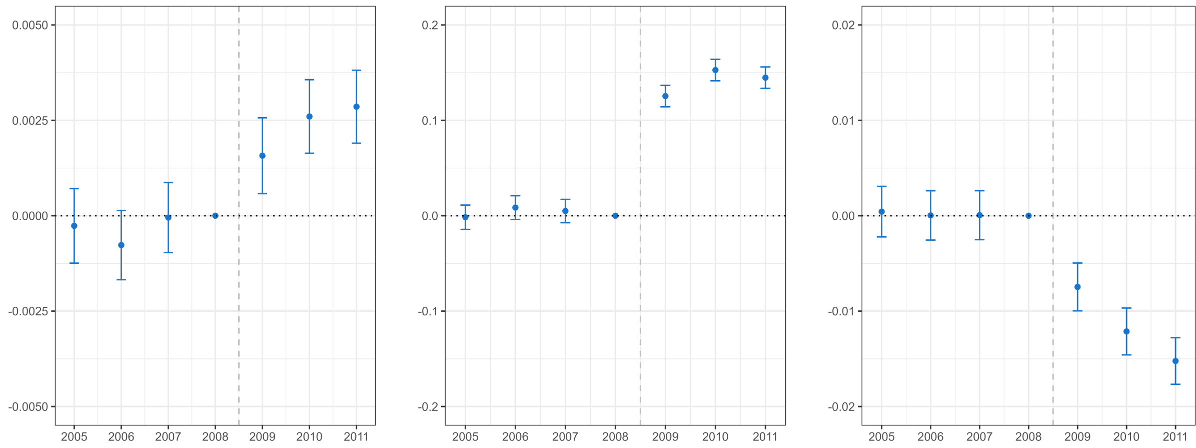
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure 5: Effects on Share of Instructional Hours in Bonus Points Courses by Tercile of Predicted Bonus Points



Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 2, but with a difference: instead of using a continuing treatment variable, we split the students into three discrete terciles of predicted bonus points. The baseline group is the second (median) tercile, whose predicted bonus points vary between 4.96 and 5.7. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure 6: Effects on Course Types



(a) Pre-Reform Relative Grades in Electives (b) Students Taking English Course (c) Share of Advanced Science Courses

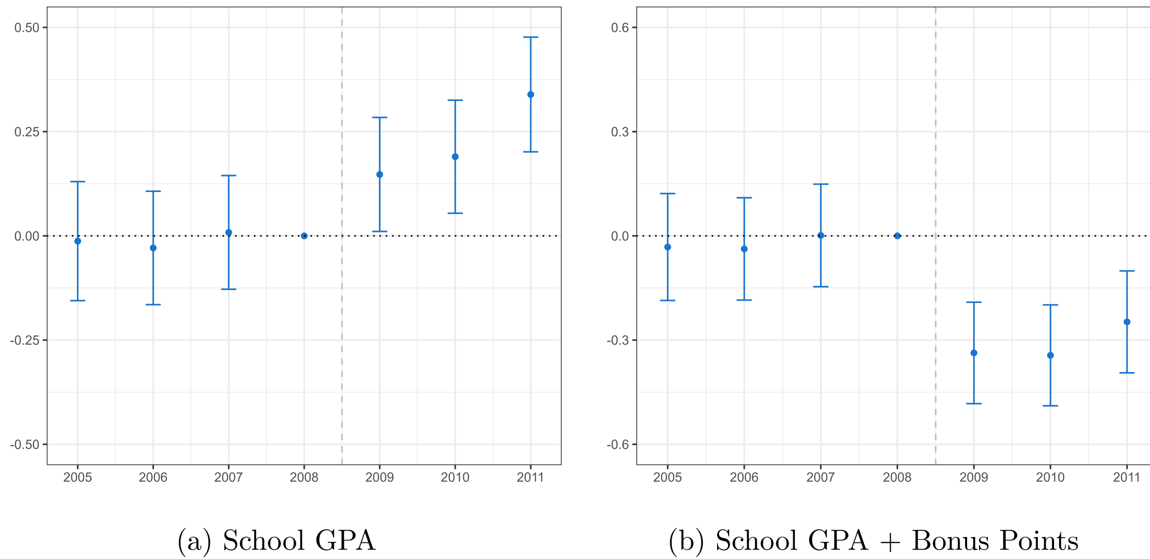
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Table 4: Effects on Course Types

	(1) Pre-Reform Relative Grades in Electives	(2) Students Taking English Course	(3) Share of Advanced Science Courses
One Predicted Bonus Point x Post Reform	0.003***	0.137***	-0.012***
	(0.0003)	(0.003)	(0.001)
Average Effect	0.0138	0.729	-0.0620
Observations	121106	121594	121594
Pre-policy mean	0.993	0.521	0.0919

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 3. Average Effect is the coefficient multiplied by the predicted bonus points mean (5.2). Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Figure 7: Effects on High School GPA



(a) School GPA

(b) School GPA + Bonus Points

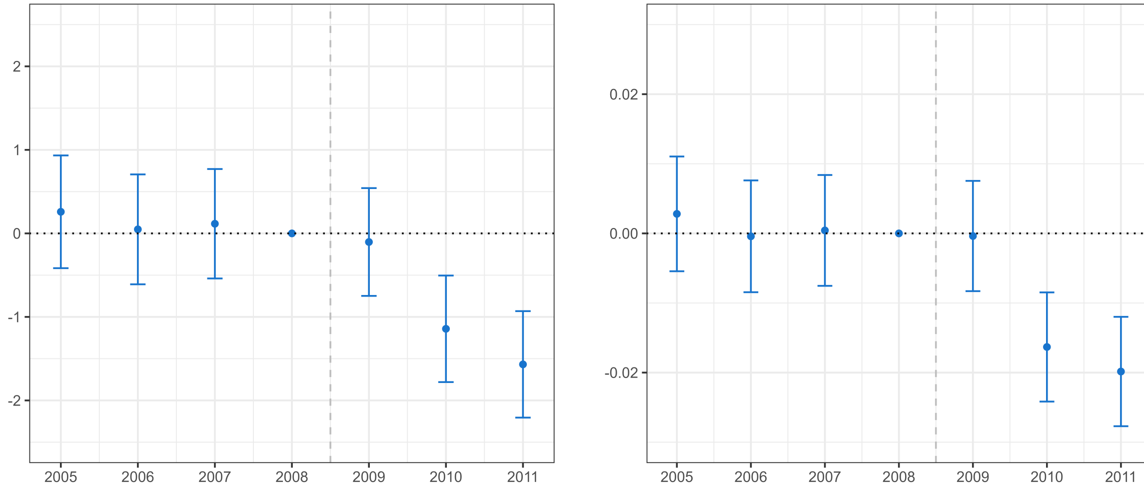
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. School GPA refers to the average of high school grades, which is the primary basis for the admission score, in addition to bonus points. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Table 5: Effects on High School GPA

	(1) School GPA	(2) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	0.231***	-0.286***
	(0.038)	(0.041)
Average Effect	1.24	-1.52
Observations	121986	121986
Pre-policy mean	40.81	46.11

Notes: This table shows authors' estimations from register data generated by Statistics Norway. 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 calculations from equation 3. Average Effect is the coefficient multiplied by the predicted bonus points mean (5.2). Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Figure 8: Effects on College-Program Quality



(a) Threshold Analysis

(b) Eligibility Analysis

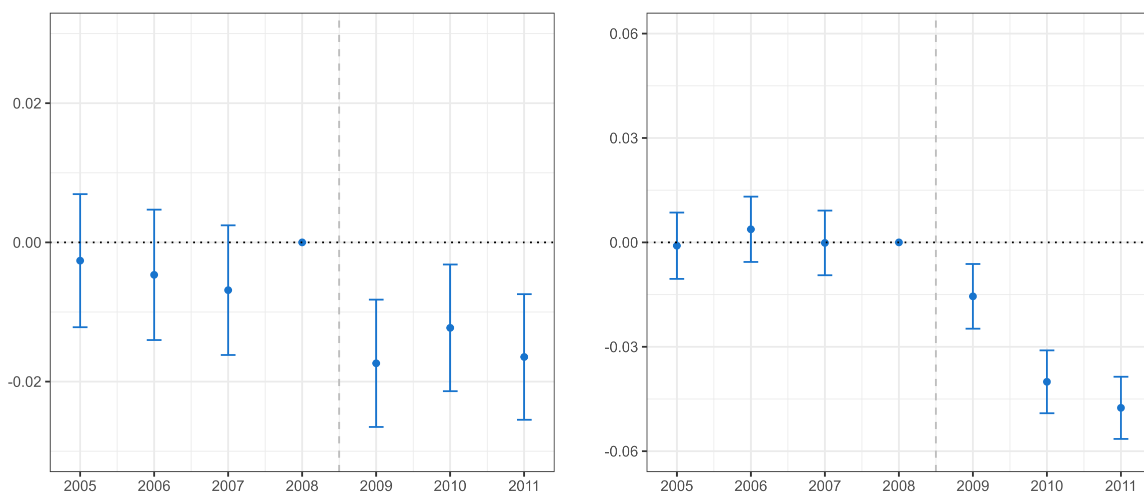
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011 and enrolled in a higher education program in the three years following graduation. The outcome in panel 7a is the pre-reform admission minimum school GPA required to get into the college-program the students were enrolled up to three years after high school graduation. In panel 7b, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Table 6: Effects on College-Program Quality

	(1) College Quality (threshold analysis)	(2) College Quantity (eligibility analysis)
One Predicted Bonus Point x Post Reform	-1.043***	-0.013***
	(0.181)	(0.002)
Average Effect	-5.424	-0.067
Observations	80051	80051
Pre-policy mean	27.892	0.677

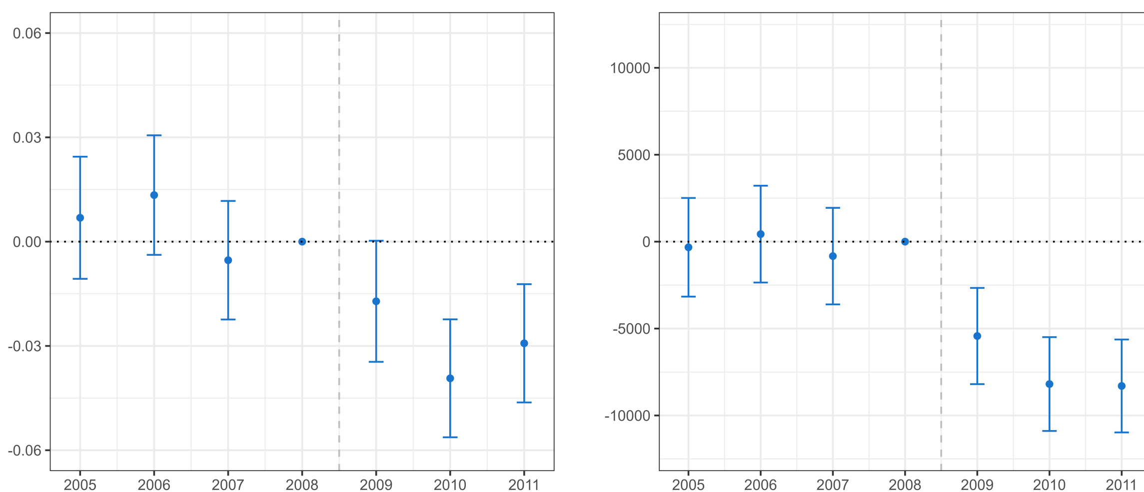
Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011 and enrolled in a higher education program in the three years following graduation. The outcome in Column 1 is the pre-reform admission minimum school GPA required to get into the college-program the students were enrolled up to three years after high school graduation. In Column 2, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. All estimates are calculations from equation 3. Average Effect is the coefficient multiplied by the predicted bonus points mean (5.2). Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Figure 9: Effects on Later Life Outcomes



(a) STEM Degree at 25 years old

(b) Master Degree at 25 years old



(c) Wage Premium at 25 years old (log)

(d) Wage Premium at 25 years old (NOK)

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All outcomes are measured at the age of 25 years old. Outcomes in panel **??** and **??** are the expected wage (at age of 35) based on individuals' level-specialization. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Table 7: Later Life Effects

	(1) STEM Degree	(2) Master degree	(3) Wage Premium (log)	(4) Wage Premium (NOK)	(5) Prob. of Manag. Occup.
One Predicted Bonus Point	-0.012***	-0.035***	-0.032***	-7233***	-0.00014***
x Post Reform	(0.003)	(0.003)	(0.005)	(764.1)	(0.00004)
Average Effect	-0.061	-0.182	-0.168	-37055	-0.001
Observations	121986	121986	121986	121986	121986
Pre-policy mean	0.135	0.133	12.00	483074	0.015

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All outcomes are measured at the age of 25 years old. Outcomes in columns 3 and 4 are the expected wage (at age of 35) based on individuals' level-specialization. All estimates are calculations from equation 3. Average Effect is the coefficient multiplied by the predicted bonus points mean (5.2). Standard errors are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: 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	0.662***	0.542***	0.656***	0.526***	0.558***	0.631***
	(0.018)	(0.014)	(0.058)	(0.017)	(0.015)	(0.016)
Observations	28340	39693	20949	47084	34180	33853

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008. The outcome of observed bonus points. All estimates are calculations from equation 3. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Results Excluding Outliers

VARIABLES	(1) Share of Bonus Points Instructional Hours	(2) College Quality (Threshold Analysis)	(3) College Quality (Eligibility Analysis)	(4) Graduated in STEM Program	(5) Master Graduate
One Predicted Bonus Point x Post Reform	-0.015***	-0.930***	-0.011***	-0.015***	-0.028***
	(0.001)	(0.195)	(0.002)	(0.003)	(0.003)
Observations	117,054	78,450	78,450	119,548	119,548
Pre-policy mean	.334	28.00	.679	.136	.134

VARIABLES	(6) Education Premium (log)	(7) Education Premium (nok)	(8) Prob. of Management Occupation	(9) School GPA	(10) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	-0.034***	-7670***	-0.0001**	0.217***	-0.307***
	(0.005)	(823.9)	(0.0001)	(0.042)	(0.044)
Observations	119548	119548	119548	119548	119548
Pre-policy mean	12.00	483865	.015	39.31	44.61

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011, excluding those whose predicted bonus points are below percentile 1 or above percentile 99. In columns 2 and 3, the sample is restricted further to students who were enrolled in a higher education program in the three years following graduation. The outcome in Column 2 is the pre-reform admission minimum school GPA required to get into the college-program the students were enrolled up to three years after high school graduation. In Column 3, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. In Column 4, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. Outcomes in columns 6 and 7 are the expected wage (at age of 35) based on individuals' level-specialization. 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 calculations from equation 3. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Effects Excluding Controls

Panel A: No Mother Employment Status

VARIABLES	(1) Share of Bonus Points Instructional Hours	(2) College Quality (Threshold Analysis)	(3) College Quality (Eligibility Analysis)	(4) Graduated in STEM Program	(5) Master's Degree
One Predicted Bonus Point x Post Reform	-0.015***	-1.044***	-0.014***	-0.012***	-0.035***
	(0.001)	(0.181)	(0.002)	(0.003)	(0.003)

VARIABLES	(6) Education Premium (log)	(7) Education Premium (nok)	(8) Prob. of Manag. Occup.	(9) School GPA	(10) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	-0.032***	-7119***	-0.0001***	0.234***	-0.292***
	(0.005)	(764.2)	(0.00004)	(0.039)	(0.041)

Panel B: No Household Income

VARIABLES	(1) Share of Bonus Points Instructional Hours	(2) College Quality (Threshold Analysis)	(3) College Quality (Eligibility Analysis)	(4) Graduated in STEM Program	(5) Master's Degree
One Predicted Bonus Point x Post Reform	-0.015***	-1.062***	-0.013***	-0.012***	-0.025***
	(0.001)	(0.182)	(0.002)	(0.003)	(0.003)

VARIABLES	(6) Education Premium (log)	(7) Education Premium (nok)	(8) Prob. of Manag. Occup.	(9) School GPA	(10) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	-0.032***	-7090***	-0.0003***	0.184***	-0.340***
	(0.005)	(764.3)	(0.00004)	(0.039)	(0.041)

Panel C: No Mother Employment Status nor Household Income

VARIABLES	(1) Share of Bonus Points Instructional Hours	(2) College Quality (Threshold Analysis)	(3) College Quality (Eligibility Analysis)	(4) Graduated in STEM Program	(5) Master's Degree
One Predicted Bonus Point x Post Reform	-0.015***	-1.109***	-0.014***	-0.012***	-0.026***
	(0.001)	(0.189)	(0.002)	(0.003)	(0.003)
Observations	119442	80051	80051	121986	121986

VARIABLES	(6) Education Premium (log)	(7) Education Premium (nok)	(8) Prob. of Manag. Occup.	(9) School GPA	(10) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	-0.033***	-7187***	-0.0003***	0.186***	-0.334***
	(0.005)	(793.6)	(0.00005)	(0.039)	(0.041)
Observations	121986	121986	121986	121986	121986

Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. In columns 2 and 3, the sample is restricted further to students who were enrolled in a higher education program in the three years following graduation. The outcome in Column 2 is the pre-reform admission minimum school GPA required to get into the college-program the students were enrolled up to three years after high school graduation. In Column 3, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. In Column 4, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. Outcomes in columns 6 and 7 are the expected wage (at age of 35) based on individuals' level-specialization. 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 calculations from equation 3, each panel excluding one or two controls that are present in the main specification. Standard errors are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: 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: Masters or Greater	-0.004
	(0.150)
Father: Less than university	-0.148
	(0.140)
Father: Masters 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 authors' estimations from the survey data. 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 hold out group is prefer not to answer). Standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 12: 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 authors' estimations from the survey data. 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 13: 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: Masters or Greater	-0.252	-0.251
	(0.161)	(0.158)
Father: Less than university	0.146	0.181
	(0.150)	(0.147)
Father: Masters 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 authors' estimations from the survey data. 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 hold out group is prefer not to answer) in column one. In column two, self-reported knowledge of the University Admission Points system on a 1-5 scale is included as a control as well. Standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

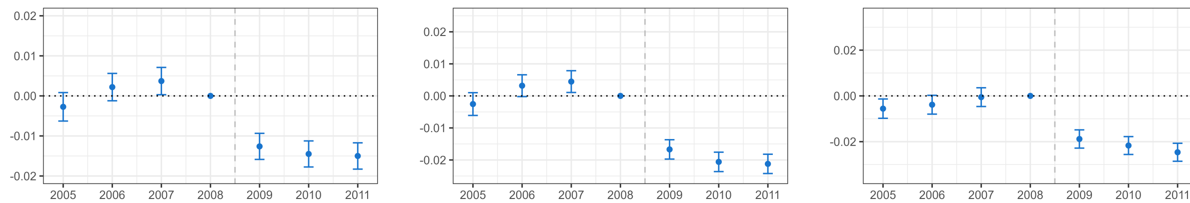
Table 14: Conjoint Analysis of Interest in Switching Courses to Earn an Bonus Point

	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 authors' estimations from the survey data. 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. 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 hold out 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

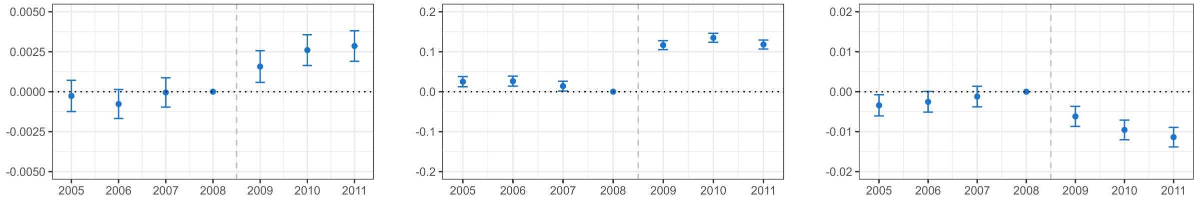
Figure A1: Effects on Course Allocation (Share of Total Instructional Hours) - No Linear Pre-Trend Correction



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

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure A2: Effects on Course Types - No Linear Pre-Trend Correction



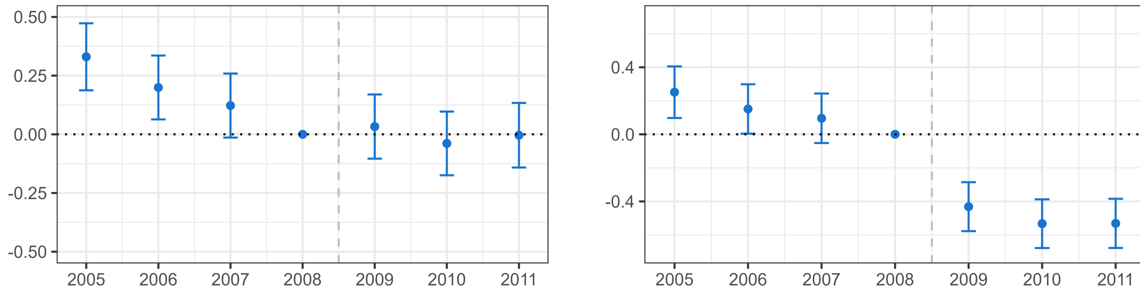
(a) Pre-Reform Relative Grades in Electives

(b) Students Taking English Course

(c) Share of Advanced Science Courses

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure A3: Effects on High School GPA - No Linear Pre-Trend Correction

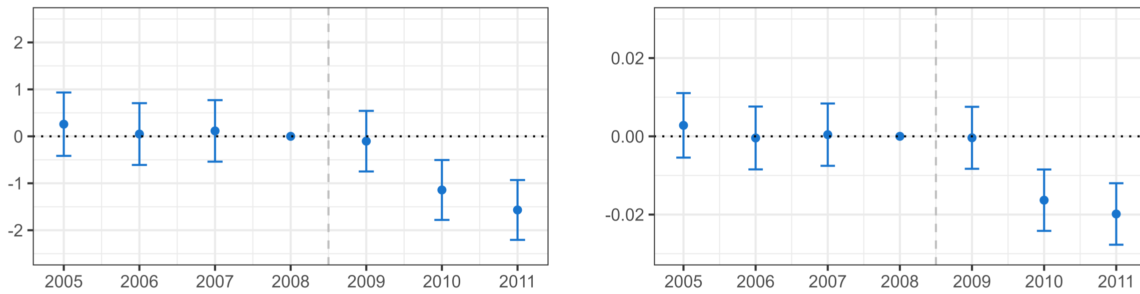


(a) School GPA

(b) School GPA + Bonus Points

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. 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 calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure A4: Effects on College-Program Quality - No Linear Pre-Trend Correction

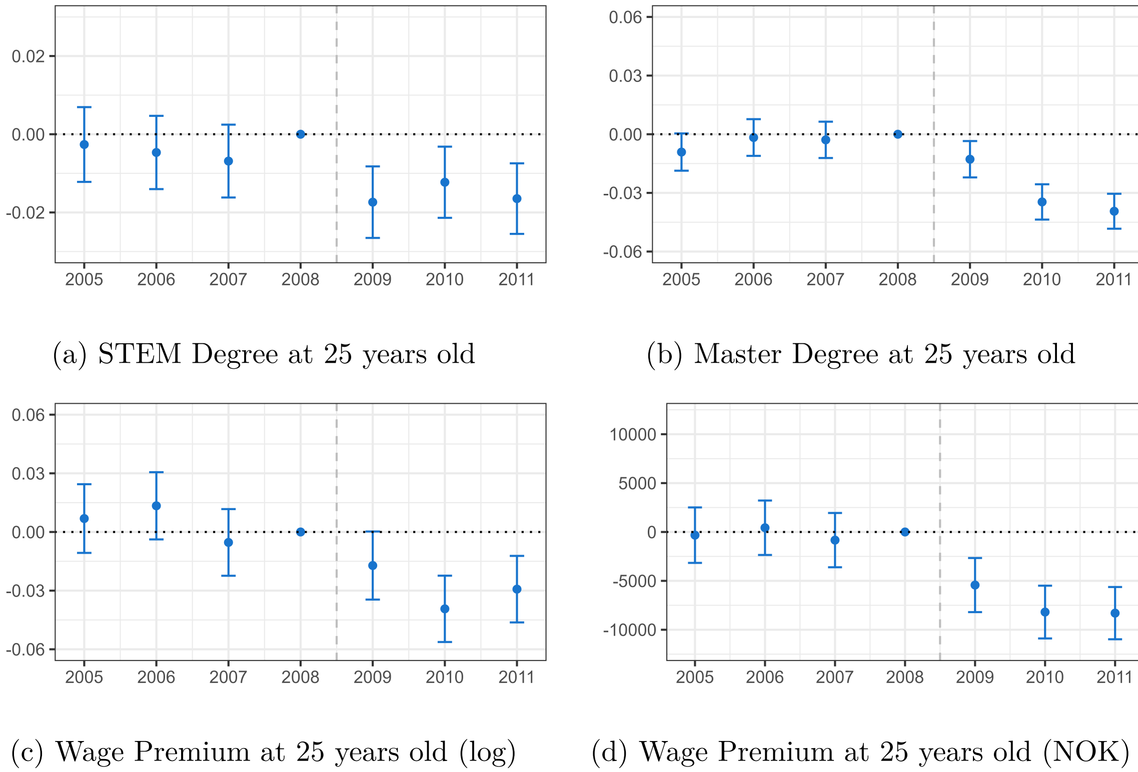


(a) Threshold Analysis

(b) Eligibility Analysis

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011 and enrolled in a higher education program in the three years following graduation. The outcome in panel 7a is the pre-reform admission minimum school GPA required to get into the college-program the students were enrolled up to three years after high school graduation. In panel 7b, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the College-program the students were enrolled up to three years after high school graduation. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Figure A5: Effects on Later Life Outcomes - No Linear Pre-Trend Correction



(a) STEM Degree at 25 years old

(b) Master Degree at 25 years old

(c) Wage Premium at 25 years old (log)

(d) Wage Premium at 25 years old (NOK)

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. All outcomes are measured at the age of 25 years old. Outcomes in panel **??** and **??** are the expected wage (at age of 35) based on individuals' level-specialization. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.

Table A1: 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 then University (N)	244
Bachelors (N)	113
Masters or Greater (N)	98
Mother's Education	
Less then University (N)	194
Bachelors (N)	153
Masters or Greater (N)	108

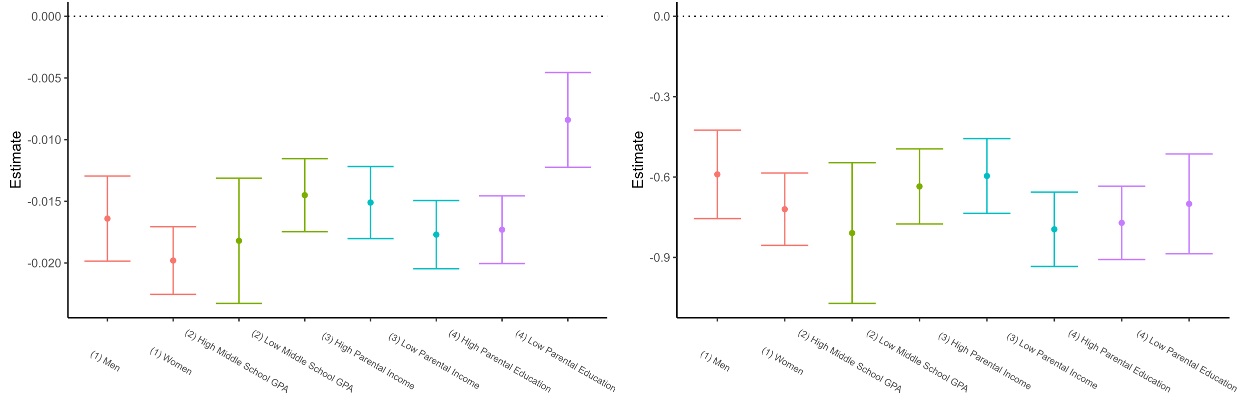
Notes: This table shows authors' estimations from the survey data. The data presented are descriptive statistics about the survey sample.

Table A2: Conjoint Analysis of Interest in Switching Courses to Earn an 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

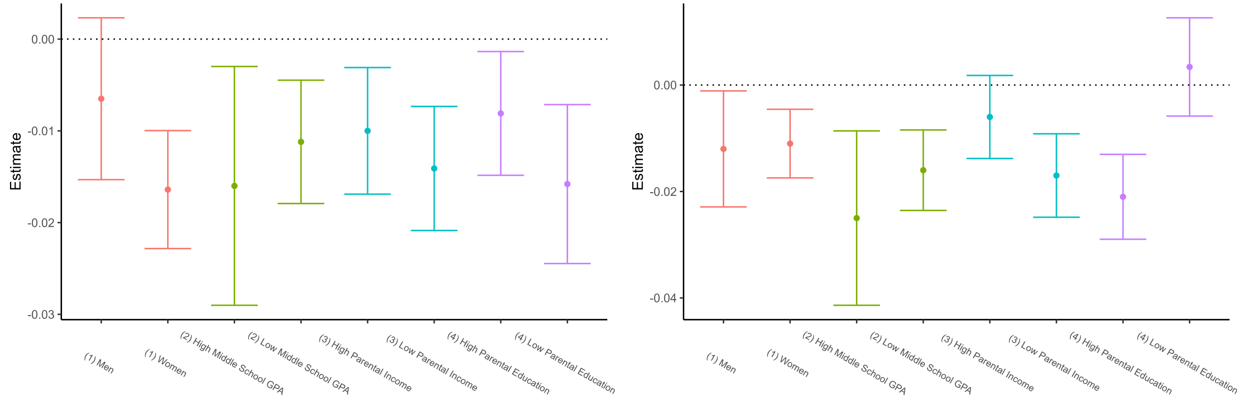
Notes: This table shows authors' estimations from the survey data. The outcome variable is a 10 point likert scale indicating respondents' interest in switching in to a randomly selected course for one additional bonus point with a randomly assigned grade cost of 0, 1 or 2 grade points. The scale ranged from 1-Not very interested to 10-Very interested. The analysis shows the analysis from column 1 in Table 14 excluding language courses. Standard errors clustered at the respondent level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure A6: Effects on Later Life Outcomes



(a) Share of Bonus Courses Instructional Hours

(b) High School GPA + Bonus Points



(c) College Quality (Eligibity Analysis)

(d) STEM Degree at 25 years old

Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. In panel A6c, the sample is restricted further to students who were enrolled in a higher education program in the three years following graduation. In the same panel, the outcome is the share of college-programs the students would be eligible for, considered the minimum pre-reform school GPA of the college-program the students were enrolled up to three years after high school graduation. Outcome in panel A6d is measured at the age of 25 years old. All estimates are calculations from equation 2. Dots represent the π_q estimates; bars represent 95% confidence intervals, with standard errors clustered at the school level.