Educational Incentives and School Choice*

Daniel Duque Sam Hirshman Kjell G. Salvanes Alexander Willén

October 29, 2024

Abstract

We examine how exogenous changes in external incentives for enrolling in specific high school courses influence students' academic decisions and long-term career trajectories. Using detailed individual-level register data and leveraging a Norwegian reform that removes college admission bonus points for science and advanced specialization courses, we analyze the impacts of these external incentives on students' human capital decisions. Applying a dose-response difference-in-differences approach, we study changes in high school course selection, college enrollment, and labor market outcomes. Our findings reveal that when incentives for taking science and advanced specialization courses decrease, students opt out of these courses, substituting them with easier ones that typically yield higher average grades. However, students perform only slightly better in these courses, resulting in lower overall college application scores and fewer college program options. This leads to enrollment in lower-quality college programs, a reduction in the probability of pursuing STEM degrees, and a sharp decline in the likelihood of pursuing a masters degree. Ultimately, we observe a drop in the predicted likelihood of exposed individuals securing management positions, along with substantial reductions in expected wage premiums at age 35. These results provide valuable insights into how strategic changes in educational incentives can shape the future workforce and affect both students and communities.

JEL Codes: I2; J2
Keywords: Education; College Major Choices; Incentives

*The authors gratefully acknowledge financial support from the Research Council of Norway through its Centres of Excellence Scheme, FAIR project No. 262675. Duque: Department of Economics and FAIR, Norwegian School of Economics (Daniel.duque@nhh.no). Hirshman: Department of Economics, Norwegian School of Economics (Samuel.Hirshman@nhh.no). Salvanes: Department of Economics, Norwegian School of Economics, CESifo, IZA, and CEPR (Kjell.Salvanes@nhh.no). Willén: Department of Economics, Norwegian School of Economics, UCLS, CESifo, and IZA (Alexander.Willen@nhh.no)

1 Introduction

Students often make educational choices based on perceived returns (e.g., Arcidiacono et al. (2012)). However, these choices do not always align with labor market demand or societal needs. Consequently, individual educational decisions can generate negative social externalities, leading to a mismatch between the supply of and demand for skilled labor. This misalignment can hinder economic growth and result in critical shortages in essential occupations and industries.

To reallocate students across majors and better align the supply of human capital with future labor market demands, two primary strategies are typically considered: increasing the returns of specific majors or lowering the relative cost of enrolling in them. While raising returns can be difficult, reducing costs is generally seen as more feasible. However, the effects of lowering costs (financial or non-financial) are theoretically ambiguous. On the one hand, reducing costs may attract more students to specific programs, helping to address skill shortages. On the other hand, it could lead to mismatches between students and programs, potentially reducing student effort, hindering human capital development, lowering overall student quality, and generating negative societal spillovers.

This paper investigates the effect of external incentives for enrolling in particular high school courses on students' course selection, educational attainment, and labor market outcomes. Using unique individual-level register data, and leveraging a Norwegian reform that removes college admission bonus points for certain science and advanced specialization courses, we analyze the long-term effects of changing course incentives on various outcomes, from high school course selection to labor market career outcomes. By addressing both efficiency and equity concerns, we evaluate the short-term effects of shifting incentives as well as the broader implications for human capital distribution and labor market outcomes. This analysis provides valuable insights into how strategic changes in educational incentives can shape the future workforce and affect both students and communities.

We exploit unique features of the Norwegian educational system alongside a reform that changed student incentives to pursue specific courses. The Norwegian admission system for higher education is centralized and based on high school GPA, supplemented with bonus points for taking certain courses (hard science and advanced specialization courses). The bonus point system increases the incentive of taking hard science and advanced specialization courses by rewarding those choices with bonus points applicable in the higher education admission system.[1] Introduced in 1998 to encourage STEM university degrees, the government significantly reduced the number of bonus points for hard science and advanced specialization courses in 2006, thereby lowering the incentive of pursuing these specific courses again.

By leveraging the 2006 points reform and using a dose-response difference-in-differences framework, we compare the outcomes of students differentially exposed to the reform due to their likelihood of receiving additional points. We trace the effects of this policy change on outcomes such as high school course selection, college enrollment decisions, and labor market earnings. By studying how these reduced incentives affected students' educational trajectories and career outcomes, we provide a comprehensive view of the long-term impact of educational incentives on school choice.

The main takeaway from our analysis is that policies which alter the incentive of pursuing specific courses can effectively shape individual educational choices and career trajectories.This conclusion is grounded in three core sets of results.

First, we demonstrate that students respond to the incentive shift by opting out of science and advanced specialization courses, substituting them with easier courses that typically yield higher average grades. However, students perform only slightly better in these easier courses; not nearly enough to fully compensate for the elimination of the specialized bonus points in the admissions system. As a result, the students affected by the policy have worse students' admissions scores for higher education.

Second, we show that this reallocation of courses leads to lower-quality college program enrollment due to the reduced admissions scores, a decreased likelihood of choosing STEM fields in higher education, and a reduction in master's degree completion.

Third, the way students alter their educational choices has strong implications for labor market success. Specifically, the educational outcomes result in substantial negative occupational earnings premium by age 35. Taken together, the removal of incentives leads to a reallocation of students to majors with lower returns and poorer labor market outcomes.

When interpreting our results, it is important to note that the bonus point system we

[1] Earning one additional bonus point provides greater weight in admissions than increasing a grade from a D to an A in a given course.

study operates on the extensive margin for specific courses: students receive bonus points toward higher education simply by passing these courses. This structure maintains effort incentivesstudents still earn admissions points based on their GPAwhile encouraging them to take on more challenging high school courses. Our results suggest that removing these incentives negatively impacts students' long-term human capital development, as many switch to easier high school courses without experiencing a large increase in GPA; subsequently enrolling in lower-tier study programs in higher education and experiencing negative labor market effects.

By utilizing detailed individual-level panel data and leveraging a government reform that changed the incentives for pursuing specific high school courses, this paper provides novel causal evidence on the effect of external incentives on students' educational choices and how those choices subsequently influence their labor market careers. The paper contributes 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 heavily influence students' decisions. The current paper expands on these findings by highlighting the role external incentives not directly related to the perceived benefits of specific courses play in shaping students' human capital decisions and field choices. By examining how changes in educational incentives can shape course selection, our study sheds light on the broader implications for students' long-term labor market outcomes. We argue that external factors, including policy changes and institutional practices, can significantly influence students' academic trajectories, leading to potential mismatches between their educational choices and labor market demands. This perspective enriches the existing literature by emphasizing that understanding major choice requires a comprehensive approach that not only examines individual preferences and long-term returns, but also intermediate incentives used to shape educational environments.

Second, this paper provides new insights into STEM shortages and barriers, a topic that has gained substantial attention in recent years (e.g., Arcidiacono et al. (2016); Stinebrickner and Stinebrickner (2014); Black et al. (2021)). These studies highlight the persistent underrepresentation of students in STEM fields, often due to academic and financial barriers. The current study shows how external incentives that alter the cost of STEM participation may exacerbate this problem, leading to fewer STEM graduates and contributing to ongoing labor shortages in these high-demand fields. We also demonstrate the potential for intermediate incentives unrelated to academic quality in shifting student demand for STEM courses.

Third, there is an emerging but rapidly expanding body of literature studying the effects of academic leniency and grading standards (e.g., Bowden et al. (2023); Hvidman and Sievertsen (2021); Ahn et al. (2019); Dee et al. (2019); Figlio and Lucas (2004)). These studies illustrate the significant repercussions of leniency in grading on student behavior and broader educational outcomes. Bowden et al. (2023) explores the implications of academic leniency on student performance, revealing that lenient grading practices can inflate grades without corresponding improvements in actual knowledge or skills and increase performance gaps between high and low performing students. Hvidman and Sievertsen (2021) show how grading leniency impacts students' academic choices and pathways, particularly regarding their selected majors.Ahn et al. (2019) examines the effects of grading standards in STEM courses on female students, suggesting that lenient grading may discourage women from pursuing these fields. Dee et al. (2019) focuses on score manipulation in New York's high school exit exams, demonstrating how inflated scores can improve graduation rates without genuinely preparing students for future challenges. Overall, these findings emphasize that while leniency might appear beneficial, it can have detrimental long-term effects on individual students and broader educational and labor market outcomes. By contrast, our paper investigates an effort-neutral policy that removed the reward for students taking on more challenging courses in high school. Students respond to the intermediate external incentives and switch away from more advanced courses, making them less qualified for higher education and experiencing negative labor market effects. Our results, therefore, suggest that well designed leniency can encourage students to exert extra effort.

Finally, this research furthers the discussion on the allocation of talent and educational match (e.g., Hvide (2003); Nechyba (2006); Dillon and Smith (2020); Black et al. (2023)). It provides evidence that reducing the academic incentives for challenging courses leads to a decline in the supply of specialized human capital. This shift impedes efficient talent distribution and economic growth. Such misallocation has far-reaching implications for labor market dynamics, particularly regarding wages, employment, and STEM participation.

2 Background

To examine the impact of external incentives that increase the cost of certain courses on students' educational choices and labor market outcomes, we leverage a unique natural experiment in Norway. We use a policy change that raised the cost of taking science courses and advanced specialization courses in high school. In this section, we provide a brief overview of the Norwegian education system, focusing on the university application process and, in particular, the 2006 reform that raised the costs associated with hard science and advanced specialization courses.

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

In the academic trackthe focus of this studystudents specialize in one of two programs: 'hard sciences' or 'language, social studies and economics'. While students take some common courses, they also take courses specific to their chosen specialization. Students need a certain amount of study points (instructional hours) within the chosen program, but they can also choose courses from a different program. Students start high school at age 16 and graduate at age 19.

Higher education in Norway is offered by numerous universities and colleges, most of which are public institutions and tuition-free. To gain admission, students must graduate from the academic track of upper secondary school and meet a minimum grade requirement. Additionally, specific majors often require students to have completed certain predefined high school courses (e.g., biology, chemistry, and physics for medical school applicants). Norwegian universities follow the Bologna Process, offering three-year bachelor's degrees and five-year combined bachelor's-master's degrees. When the number of applicants exceeds available spots, selection is based solely on admission score, which we describe in detail below. Education is free at all levels, including the post-secondary level, and most students qualify for financial support from the Norwegian State Educational Loan Fund, which consists of both loans and grants.

The college application process is centralized through the Norwegian Universities and Colleges Admission Service, which manages 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. Offers are made sequentially based on students' application scores, which are calculated using their high school GPA along with a system of bonus points.

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 bonus points were introduced by the central government in 1998 in order to incentivize specific demographic groups to enroll in higher education and to encourage specific major choices. Most of the bonus points relate to demographics that students cannot influence (such as age, military service, and gender). However, one category of bonus points rewards students for taking specific high school courses namely, science and advanced specialization courses.[2] Students earned science points by completing courses in advanced mathematics, biology, physics, and chemistry. For example, taking the course 'advanced physics' generated 1 science point. Students earned advanced specialization points by taking additional in-depth courses beyond their mandatory curriculum, receiving 2 points for each elective subject that have at least two levels. Some courses provided both science points and advanced specialization points. For example, taking the advanced physics course gave students 1 science point and 2 specialization points, while an advanced history course provided 2

[2] Rules for science points can be found in the 2005 regulation for higher education admission for cohort 2006/7, paragraph . See 2005 Forskrift om opptak til universiteter og høyskoler, chapter 7. Rules for specialization points can be found in the 2005 regulation for higher education admission for cohort 2006/7, paragraph . See 2005 Forskrift om opptak til universiteter og høyskoler, chapter 9

specialization points but no science points.

In total, students could earn up to 10 bonus points related to their science and advanced specialization course selection – equivalent to a full point increase in average GPA based on their course selections (6 points for hard science courses and 4 points for specialization courses). That is, gaining one additional bonus point would be akin to raising the average GPA of the student's entire course portfolio with one letter grade. These bonus points, therefore, held greater value than individual course grades, enabling students to significantly boost their admission scores by choosing the right combination of courses.

In 2008, the Norwegian government changed the bonus point system by introducing two major adjustments to the scheme related to bonus points for science and advanced specialisation courses: 1) the complete removal of bonus points for advanced specialization courses, and 2) a 33 percent reduction in bonus points for science courses, decreasing from 6 to 4 points. Those rules were introduced in chapter 7 of the regulations on changes to regulations on admission to higher education, approved in 2008 to apply for the next graduating years, starting from the school year of 2008/9. [3] 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. To illustrate the reform's impact, Figure 1 displays the distribution of science and advanced specialization bonus points for the cohorts graduating around the time the reform was implemented. Panel 1a illustrates the change in bonus points due to the reform by presenting the mean number of bonus points received by students before and after the reform was implemented. The mean decreased from approximately 5 bonus points in the pre-reform period to about 1 bonus point in the post-reform period. Panel 1b further shows that the bonus point distribution changed substantially due to the reform. In the pre-reform period, the distribution ranged from 0 to 10 with a significant concentration in the higher range. In contrast, the post-reform distribution is much more condensed, with no student earning more than 4 bonus points and many receiving none.

While this reform significantly reduced the incentive of pursuing science and advanced

[3] See 2008 Forskrift om endring i forskrift om opptak til høyere utdanning, chapter 7

specialization courses by limiting the bonus compensation for these subjects, it's 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 prerequisites.

3 Data

Our core data come from comprehensive administrative registers that include all Norwegian residents. A unique personal identifier allows us to track students over time and across different registers. In terms of analysis period, the first cohort affected by the points reform graduated high school in 2009. To ensure sufficient data coverage both before and after the reform, we focus on cohorts of students who graduated from high school between 2005 and 2011. This provides us with four cohorts pre-reform and three cohorts post-reform.[4]

For education data, we use information from the high school, primary school, and university registers. The high school register provides detailed information on which school the student attended, each course taken, and the grades received. This allows us to see whether students enrolled in courses qualifying for bonus points and if they met the prerequisites for specific college programs at the time of application. We merge these data with the primary school register, which provides us with the students' primary school GPAs used for high school admission. This provides us with a measure of baseline ability prior to high school attendance and represents a key variable in our dose response estimation framework. We also incorporate data from the university register, which includes information on college enrollment, major choices, and college locations.

We merge the education data with information from the demographic, tax, and intergenerational 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. This dataset allows us to construct a thorough panel covering the universe of Norwegian students and much of their demographic, educational, and labor market information.

Our extensive data allow us to examine a broad range of outcomes, thereby capturing all the margins of adjustments that the students may engage in due to the reform. First,

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

we examine immediate behavioral responses to the reform by studying students' enrollment in science and advanced specialization courses. These outcomes help us understand whether students respond to the changes in external incentives when selecting courses, which could reflect shifts in academic focus.

Second, we consider performance effects by looking at changes in high school GPA and college admissions scores, key indicators of academic success in the Norwegian education system. A priori, it is theoretically ambiguous what effects we should expect to see on these dimensions. Specifically, if the change in external incentive led students to pursue on average easier courses — as measured by the average grade in the course—, then their GPA may go up as a consequence of the reform, enabling them to offset some of the drop in the admissions score driven by the elimination of the bonus points. However, a change in courses may also change the quality of the course match for the student, something that could either pull down the GPA (if the new courses are a worse match for the student) or bring it up (if the new courses are a better match for the student).

Third, we examine the higher education implications of the reform, studying how the policy impacts college-program availability (programs available to the student based on the admission score applied with) and quality (proxied by the school GPA of the student's peers in the program ultimately chosen), as well as the likelihood of pursuing STEM degree, or advancing to a master's program.

Finally, we assess labor market outcomes, focusing on the expected wage premiums that students may earn as a result of these educational shifts. Since the reform took place in 2009 and our data end in 2019, we cannot directly examine their earnings at prime working age (35). As such, we use the population-wide labor market and education register to predict the earnings premium at age 35 for each program-university combination available in Norway through a Mincer wage equation that includes cohort and municipal fixed effects. We use this imputed earnings premium as a measure of anticipated labor market effect of the reform. We take the same approach to predict the probability of the individual having a management occupation by the age of 35.

Together, these outcomes provide a comprehensive view of both the immediate academic responses and the long-term education and labor market implications of the reform.

4 Empirical Specification

To examine the impact of external incentives in shaping the course selection of students, and how that subsequently influences educational attainment and labor market outcomes, we exploit the Norwegian points reform and leverage a dose-response difference-in-differences specification.

The core idea behind our identification strategy is that the reform will have a differential impact on students as a function of the number of science and advanced specialization courses that the students would have taken in the absence of the reform. We can use this differential bite in a dose-response difference-in-differences design, leveraging within cohort variation in exposure over time across the bonus point distribution. This allows us to examine the effect of explicit targeted and effort-neutral incentives on student human capital investments and subsequent labor market performance.[5]

The core challenge that we face is that we do not observe the number of bonus points that the students would have received post the reform had the reform not been implemented. To overcome this challenge and estimate the number of points that students would have received post-reform had the reform not occurred, we exploit the richness of the Norwegian register data and predict the number of bonus points students receive based on a detailed set of demographic and educational background variables using the pre-reform cohorts: 2005 to 2008. We then use these predictions for cohorts in the post-reform period to generate a measure of how many points they likely would have received had the policy not been implemented. 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, middle school GPA, parental education (level and program), parental employment status, household income (per capita), birth month, municipality of birth, middle school identifier, and the share of both science

[5]The bonus point system we study operates on the extensive margin for specific courses: students receive bonus points toward higher education simply by passing these courses. This structure maintains effort incentivesstudents still earn admissions points based on their GPAwhile encouraging them to take on more challenging high school courses.

and specialization courses instructional hours in 12th grade.

The results from this exercise are shown in Table 9, demonstrating that many of the individual coefficients are strongly related to the eventual bonus points of students in the pre-reform cohorts, and that the overall prediction is strong with an F-statistic of 68.

We validate this treatment intensity variable by examining the relationship between actual bonus points and predicted bonus points among students who graduated before the reform. The results from this validation exercise are shown in Figure 3, and reveal a correlation of more than 0.9. In other words, the bonus point prediction obtained through the use of the demographic characteristics discussed above are able to closely anticipate the number of bonus points that a student receives in the pre-period.

Once we have constructed our dosage measure, we use the following event study specification to examine the impact of the reform:

$$Y_{ict} = \alpha + \sum_{q=2005}^{2011} \pi_q (1[c = q]\widehat{BonusPoints}_{ic}) + \gamma X'_{ic} + \widehat{\phi}_{pre}(2008 - c)\widehat{BonusPoints}_{ic} + \epsilon_{ict}, \quad (2)$$

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 we study effects across cohorts, we also incorporate $\widehat{\phi}_{pre}(2008 - c)\widehat{BonusPoints}_{ic}$ as a linear pre-trend control (e.g., Jakobsen et al. (2019)).$\widehat{\phi}_{pre}$ is a linear differential pre-trend identified based on the four pre-reform years.

To summarize these results in an easily-interpretable way, we also show results from a conventional difference-in-differences (DiD) specification, estimating the average treatment effect in the post period through the following specification:

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

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 akin to an instrumented dose-response difference-in-differences specification, and the identifying variation in our setting comes from differences in exposure to the point reform based on the number of predicted points that students would have taken in the post-period had the policy not been implemented. In order to interpret our results as the causal effect of changing the incentive to pursue specific course in high school, four assumptions need to be met: common trends, monotonicity, relevance, and exclusion.

First, the common trends assumption requires that the outcomes of students with varying predictions of bonus points would have trended in parallel had the reform not occurred. Although we cannot directly test this assumption, we can estimate event studies to analyze common trends during the pre-policy period. We do this through Equation 2, noting that the absence of pre-trends provides strong suggestive support for the parallel trends assumption. We present results consistent with this assumption in the next section.

Second, the monotonicity assumption states that an increase in predicted bonus points must lead to an increase in the actual number of points across the entire distribution. While we cannot evaluate this for the treated cohorts due to the lack of counterfactual data, we can demonstrate that this assumption holds for the pre-reform cohorts, where we observe both the predicted and actual points. The results of this analysis, shown in Figure 2, provide robust support for the monotonicity assumption.

Third, the relevance assumption requires a strong correlation between the predicted number of bonus points and the actual number of bonus points students would receive. Again, we cannot examine this for the treated cohorts, but we can obtain supportive evidence by examining the pre-reform cohorts. The results of this validation are found in the first stage F-statistic of 68 (shown in Table 9).[6]. This indicates that the predictions derived from demographic characteristics effectively anticipate the number of bonus points students received in the pre-reform period.

Finally, the exclusion restriction posits that the only pathway through which predicted bonus points affect student outcomes is via the change in incentives driven by the reform. While we cannot test this assumption directly, we are unaware of any contemporaneous shocks or trends that could potentially confound our treatment effect.

[6]The strong correlation of the predicted points and actual points in the pre-period, as shown in Figure 3, serves as further visual support for this assumption

5 Results

In this section, we present our core results on the effect of external incentives that reduce the cost of taking specific courses on students' course selection, educational attainment, and labor market outcomes. We first show the immediate behavioral response to the policy, examining the impact on the probability of pursuing science and specialization courses that traditionally yielded bonus points. We then examine the impact on performance, studying both high school GPA as well as overall admission score effects. Then, we study college quality, probability of pursuing STEM programs, probability of attending elite schools, and the probability of pursuing masters degrees. Finally, we study the overall impact on earnings.

After we have presented our main findings, we introduce a series of robustness and sensitivity analyses that probe the data further in an effort to rule out alternative explanations and provide additional support for our causal interpretation of the results we provide.

Behavioral Response. Figure 4 shows our event study results based on Equation 2 using the number of courses taken that would have yielded bonus points prior to the reform as the outcome variable. The figure shows two important findings. First, there is no evidence of differential pre-trends as a function of our dosage variable, providing strong support in favor of the common trends assumption required for causal inference in our setting. Second, there is an immediate drop in the number of science and specialization courses taken at the time of the points reform, suggesting that respond immediately to the changing course incentive.[7]

To facilitate the interpretation of this result, Table 2 provides the coefficient from our simple difference-in-differences specification, showing the effect of the point reform on (1) the number of courses taken that would have yielded bonus points prior to the reform, (2) the number of hard science courses taken that would have yielded bonus points prior to the reform, and (3) the number of specialization courses taken that would have yielded bonus points prior to the reform.

The table demonstrates that students moved away both from science as well as advanced specialization courses in response to the reform. Scaling the point estimate with the pre-policy mean, the magnitude of the effect is approximately 5 percent per predicted bonus

[7]Interestingly, Appendix Table 8 shows that there is no differential effect across men and women in our sample, which may have been expected due to differences in risk preferences, confidence, and behavior (e.g., Croson and Gneezy (2009); Niederle and Vesterlund (2007); Exley and Kessler (2022)).

point. Given that the average predicted bonus points is 5.2, the average effect is about 25% of the pre-policy mean.

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 5. 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 5. 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 strong support in favor of 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 score.

To facilitate the interpretation of the performance results, Table 3 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 0.09 per predicted bonus point and a drop in admission score of 0.43.

The relatively weak effect on grade point average (GPA) may stem from several explanations. First, the match between students' abilities and courses may shift, potentially resulting in lower grades. Alternatively, enrolling in less challenging courses might lead to diminished effort, which could also contribute to poorer performance. While our analysis does not allow us to disentangle the relative importance of these two mechanisms, we view this as an intriguing avenue for future research.

Educational Quality and Attainment. The results up until now have demonstrated that the reduced incentive for studying science and advanced specialization courses caused students to substitute these classes for others. However, the course switch was not sufficient to offset the drop in admission scores caused by the reduction in potential bonus points. This may subsequently influence the college opportunities and choices that students make, a set of outcomes to which we now turn.

First, Panel 6a of Figure 6 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 the education opportunities at the college level. Panel 6b 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 4.

Both panels in Figure 6 show flat and stable trends in the outcomes as a function of treatment status in the pre-shock years, providing strong evidence in favor of the common trends assumption. Both panels also show a sharp drop in these quality measures in the second post-reform year, strongly indicating that there is a substantial reduction in both the quality of fields and college availability as a consequence of the reform.

To further probe the data and better understand the implications of these quality effects, we also estimate our event study specification using (a) the probability of holding a masters degree, (b) the probability of holding a STEM degree, and (c) the probability of attending an elite institution.

The results from these analyses are shown in Panels 7a and 7b of Figure 7, showing that the incentive reform led to a substantial drop in receiving a STEM degree and a reduction in the probability of holding a masters degree. Part of the STEM effect that we identify likely operates through the fact that these programs tend to have considerable course prerequisites related to advanced math and hard science courses. As the affected students shift out of these courses once the bonus incentive is removed, it is likely that they no longer qualify for these programs. Results from our simplified difference-in-differences specification are provided in Table 5.

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, this course switch 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 ageage 35 (e.g.,Haider and Solon (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 7c and 7d of Figure 7 (event study specification) and Table 4 (Difference-in-Differences specification).

There are three main take-aways from these anayses. 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.

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 Table 6 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 7 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 8 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.

7 Discussion

Students frequently base their educational choices on perceived returns. These choices often do not 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 impede economic growth and lead to critical shortages in essential occupations and industries.

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.

In this paper, we provide the first empirical analysis on the effect of external incentives for enrolling in particular high school courses on students' course selection, educational attainment, and labor market outcomes. Using unique individual-level register data, and leveraging a Norwegian reform that removes college admission bonus points for certain science and advanced specialization courses, we analyze the long-term effects of changing course incentives on various outcomes, from high school course selection to labor market career outcomes.

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 substantial reductions in expected wage premiums at age 35.

By examining how changes in educational incentives can shape course selection, our analysis demonstrates that external factors, including policy changes and institutional practices, can significantly influence students' academic trajectories, leading to potential mismatches between their educational choices and labor market demands. These results provide valuable insights into how strategic changes in educational incentives can shape the future workforce and affect both students and communities. In addition, this perspective enriches the existing literature by emphasizing that understanding major choice requires a comprehensive approach that not just examines individual preferences and long-term returns, but also intermediate incentives used to shape educational environments.

References

Ahn, Thomas, Peter Arcidiacono, Amy Hopson, and James R Thomas. 2019. “Equilibrium grade inflation with implications for female interest in stem majors.”Technical report, National Bureau of Economic Research.

Arcidiacono, Peter, Esteban M Aucejo, and V Joseph Hotz. 2016. “University differences in the graduation of minorities in STEM fields: Evidence from California.” *American Economic Review*, 106(3): 525–562.

Arcidiacono, Peter, V. Joseph Hotz, and Songman Kang. 2012. “Modeling college major choices using elicited measures of expectations and counterfactuals.” *Journal of Econometrics*, 166(1): 3–16, URL: https://www.sciencedirect.com/science/article/pii/S0304407611001151, DOI: http://dx.doi.org/https://doi.org/10.1016/j.jeconom.2011.06.002, Annals Issue on “Identification and Decisions”, in Honor of Chuck Manski’s 60th Birthday.

Bhuller, Manudeep, Gordon B. Dahl, Katrine V. Løken, and Magne Mogstad. 2020. “Incarceration, Recidivism, and Employment.” *Journal of Political Economy*, 128(4): 1269–1324, URL: https://doi.org/10.1086/705330, DOI: http://dx.doi.org/10.1086/705330.

Black, Sandra E, Jeffrey T Denning, and Jesse Rothstein. 2023. “Winners and losers? the effect of gaining and losing access to selective colleges on education and labor market outcomes.” *American Economic Journal: Applied Economics*, 15(1): 26–67.

Black, Sandra E, Chandra Muller, Alexandra Spitz-Oener, Ziwei He, Koit Hung, and John Robert Warren. 2021. “The importance of STEM: High school knowledge, skills and occupations in an era of growing inequality.” *Research policy*, 50(7): , p. 104249.

Bordon, Paola, and Chao Fu. 2015. “College-major choice to college-then-major choice.” *The Review of economic studies*, 82(4): 1247–1288.

Bowden, A Brooks, Viviana Rodriguez, and Zach Weingarten. 2023. “The Unintended Consequences of Academic Leniency.”

Croson, Rachel, and Uri Gneezy. 2009. “Gender differences in preferences.” *Journal of Economic literature*, 47(2): 448–74.

Dee, Thomas S, Will Dobbie, Brian A Jacob, and Jonah Rockoff. 2019. “The causes and consequences of test score manipulation: Evidence from the New York regents examinations.” *American Economic Journal: Applied Economics*, 11(3): 382–423.

Dillon, Eleanor Wiske, and Jeffrey Andrew Smith. 2020. “The consequences of academic match between students and colleges.” *Journal of Human Resources*, 55(3): 767–808.

Exley, Christine L, and Judd B Kessler. 2022. “The gender gap in self-promotion.” *The Quarterly Journal of Economics*, 137(3): 1345–1381.

Figlio, David N, and Maurice E Lucas. 2004. “What’s in a grade? School report cards and the housing market.” *American economic review*, 94(3): 591–604.

Haider, Steven, and Gary Solon. 2006. “Life-cycle variation in the association between current and lifetime earnings.” *American economic review*, 96(4): 1308–1320.

Hvide, Hans K. 2003. “Education and the Allocation of Talent.” *Journal of labor Economics*, 21(4): 945–976.

Hvidman, Ulrik, and Hans Henrik Sievertsen. 2021. “High-stakes grades and student behavior.” *Journal of Human Resources*, 56(3): 821–849.

Jakobsen, Katrine, Kristian Jakobsen, Henrik Kleven, and Gabriel Zucman. 2019. “Wealth Taxation and Wealth Accumulation: Theory and Evidence From Denmark*.” *The Quarterly Journal of Economics*, 135(1): 329–388, URL: https://doi.org/10.1093/qje/qjz032, DOI: http://dx.doi.org/10.1093/qje/qjz032.

Kirkeboen, Lars J, Edwin Leuven, and Magne Mogstad. 2016. “Field of study, earnings, and self-selection.” *The Quarterly Journal of Economics*, 131(3): 1057–1111.

Nechyba, Thomas J. 2006. “Income and peer quality sorting in public and private schools.” *Handbook of the Economics of Education*, 2 1327–1368.

Niederle, Muriel, and Lise Vesterlund. 2007. "Do Women Shy Away From Competition? Do Men Compete Too Much?" *Quarterly Journal of Economics* 1067–1101.

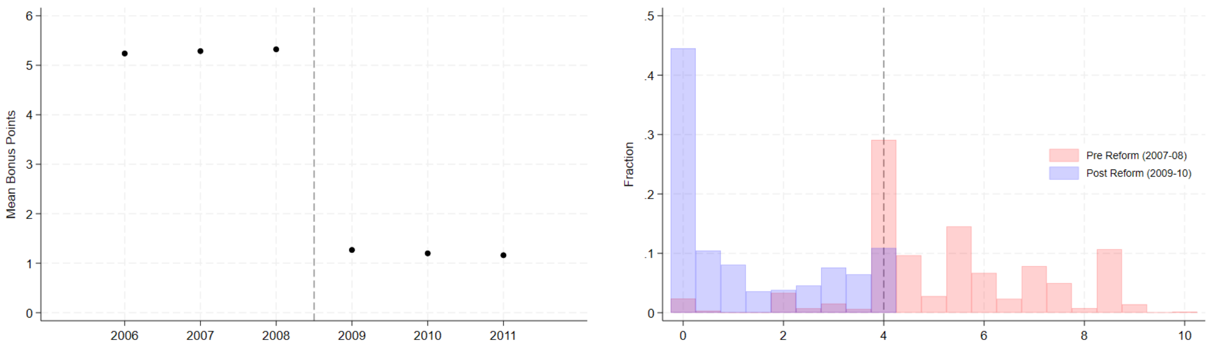
Stinebrickner, Ralph, and Todd R Stinebrickner. 2014. "A major in science? Initial beliefs and final outcomes for college major and dropout." *Review of Economic Studies*, 81(1): 426–472.

Wiswall, Matthew, and Basit Zafar. 2015. "Determinants of college major choice: Identification using an information experiment." *The Review of Economic Studies*, 82(2): 791–824.

Zafar, Basit. 2013. "College major choice and the gender gap." *Journal of Human Resources*, 48(3): 545–595.

Tables and Figures

Figure 1: Pre- and post reform bonus point distribution



(a) Mean Bonus Points by Year

(b) Distribution of Bonus Points

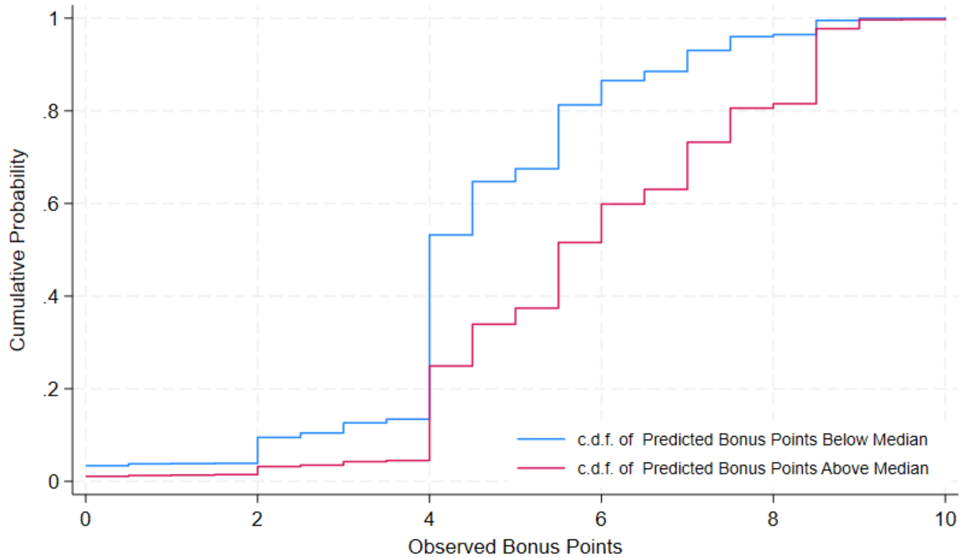
Notes: This figure shows authors' calculations from register data generated by Statistics Norway. Panel 1a shows the mean bonus points by graduating cohort, spanning from 2006 to 2011. Panel 1b shows You are showing a hisogram of the bonus points for cohorts that graduated before the reform (2007-2008) 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	39.3	7.1	40.7	7.1
School GPA + Bonus Points	44.6	7.9	41.9	7.8

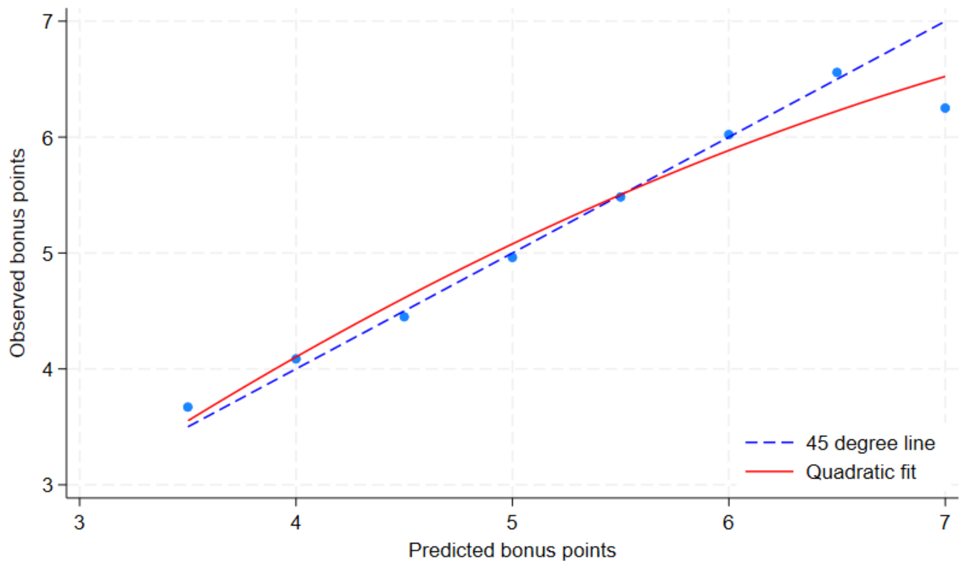
Notes: This table shows authors' calculations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008 (Pre Reform) and 2009 to 2011 (Post Reform).

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



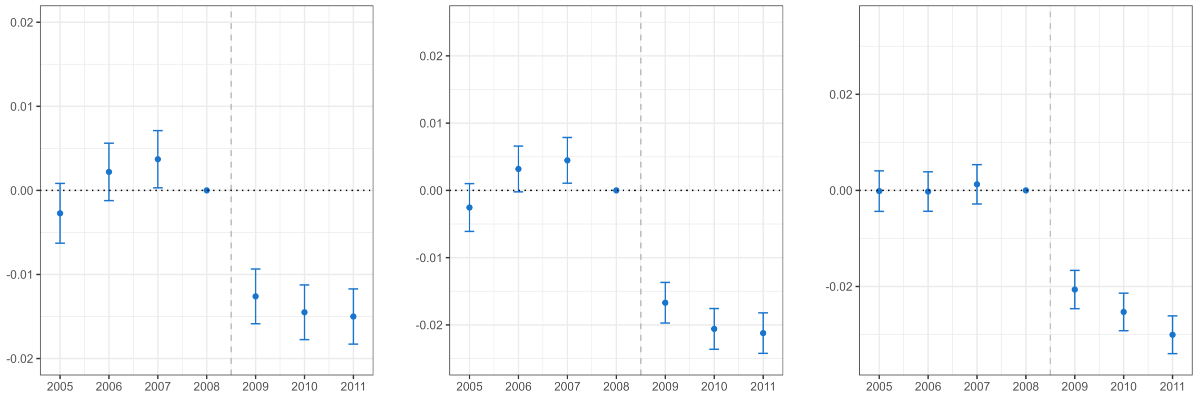
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2008, before the reform. Predicted points are estimated in Equation 1. Dots are pooled in 0.5 intervals. Predicted bonus points are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

Table 2: 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

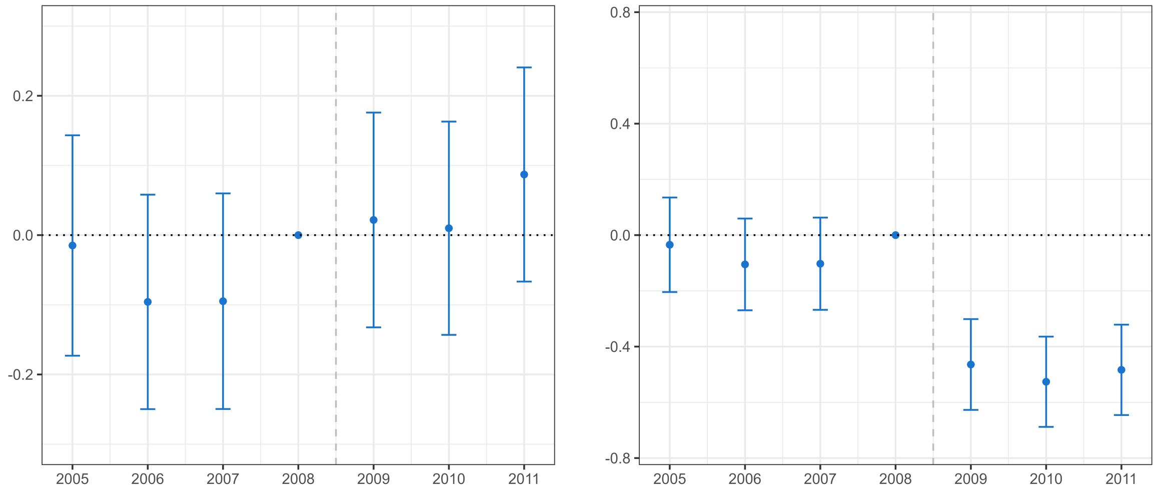
Figure 4: Effects on Course Allocation (Share of Total Instructional Hours)



(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 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.

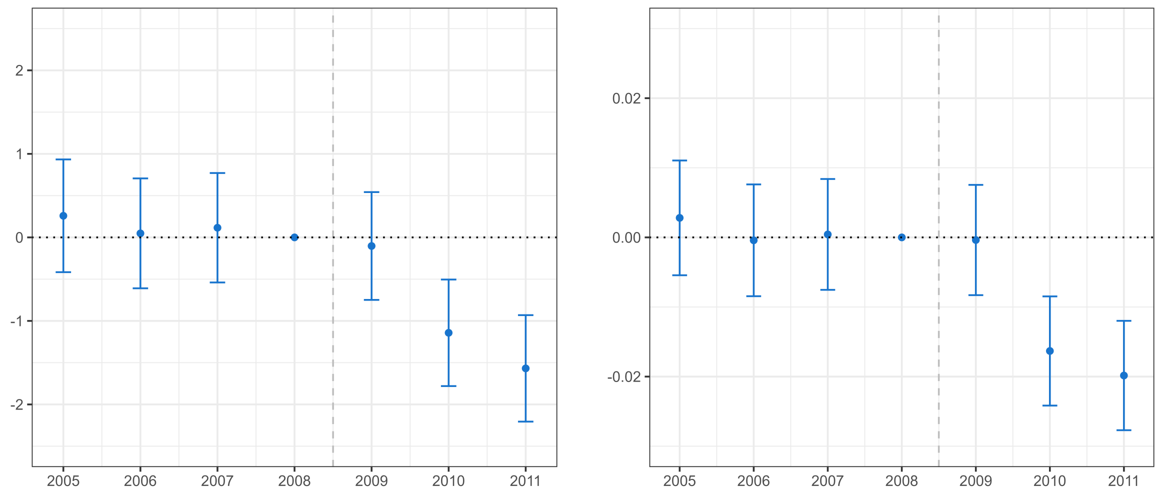
High School GPA

Table 3: Effects on High School GPA

	(1) School GPA	(2) School GPA + Bonus Points
One Predicted Bonus Point x Post Reform	0.091**	-0.431***
	(0.043)	(0.045)
Average Effect	0.473	-2.24
Observations	119442	119442
Pre-policy mean	39.28	44.56

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 6: Effects on College-Program Quality



(a) Threshold Analysis

(b) Eligibility Analysis

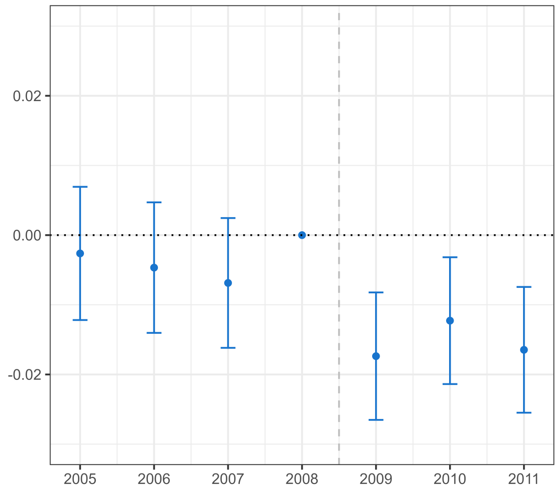
Notes: This figure shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011 and enrolled in a higher education program in the three years following graduation. The outcome in panel 6a is the pre-reform 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 6b, 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 4: Effects on College-Program Quality

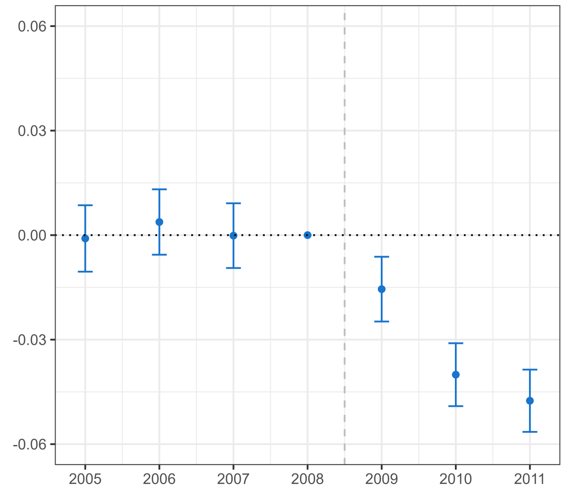
	(1) College Quality (threshold analysis)	(2) College Quantity (eligibility analysis)
One Predicted Bonus Point x Post Reform	-1.043***	-0.013***
	(0.181)	(0.002)
Average Effect	-5.424	-0.067
Observations	80051	80051
Pre-policy mean	27.892	0.677

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

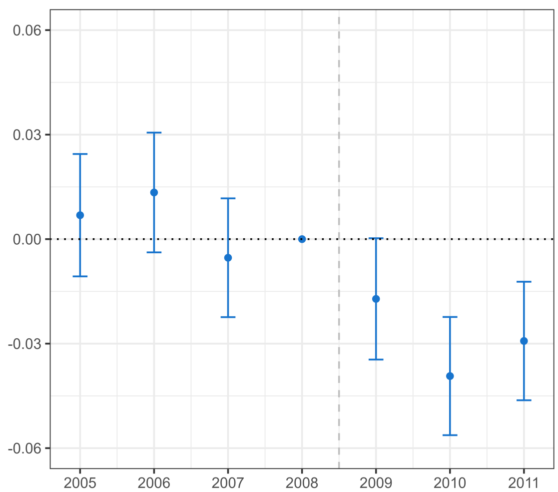
Figure 7: Effects on Later Life Outcomes



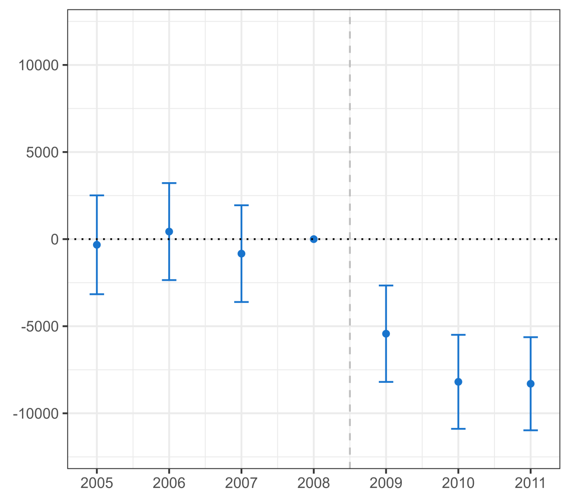
(a) STEM Degree at 25 years old



(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 7c and 7d 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 5: 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 6: 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 7: 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	-0.015***	-0.930***	-0.011***	-0.015***	-0.028***
x Post Reform	(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	-0.034***	-7670***	-0.0001**	0.068	-0.450***
x Post Reform	(0.005)	(823.9)	(0.0001)	(0.046)	(0.049)
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 8: 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*** (0.001)	-1.044*** (0.181)	-0.014*** (0.002)	-0.012*** (0.003)	-0.035*** (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*** (0.005)	-7119*** (764.2)	-0.0001*** (0.00004)	0.092** (0.043)	-0.430*** (0.045)

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*** (0.001)	-1.062*** (0.182)	-0.013*** (0.002)	-0.012*** (0.003)	-0.025*** (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*** (0.005)	-7090*** (764.3)	-0.0003*** (0.00004)	0.049 (0.043)	-0.471*** (0.045)

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*** (0.001)	-1.109*** (0.189)	-0.014*** (0.002)	-0.012*** (0.003)	-0.026*** (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*** (0.005)	-7187*** (793.6)	-0.0003*** (0.00005)	0.064 (0.045)	-0.454*** (0.047)
Observations	121986	121986	121986	121986	121986

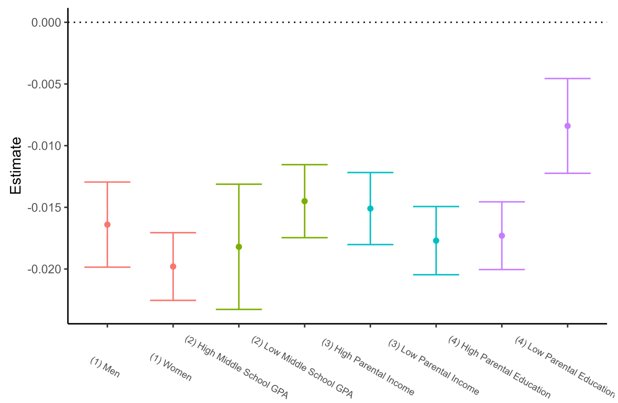
Notes: This table shows authors' estimations from register data generated by Statistics Norway. Sample is restricted to students who graduated in high school from 2005 to 2011. In columns 2 and 3, the sample is restricted further to students who were enrolled in a higher education program in the three 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 9: First Stage - Predicting Bonus Points

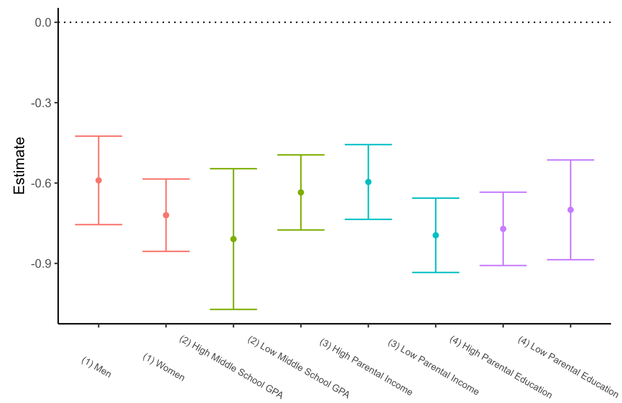
Variable	Estimate
Mother Employment Status	-0.022
	(0.028)
Father Employment Status	0.108***
	(0.030)
Middle School GPA (1-1.5]	0.773
	(0.769)
Middle School GPA (1.5-2]	0.334
	(0.316)
Middle School GPA (2-2.5]	0.556*
	(0.323)
Middle School GPA (2.5-3]	0.900**
	(0.315)
Middle School GPA (3-3.5]	1.202***
	(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

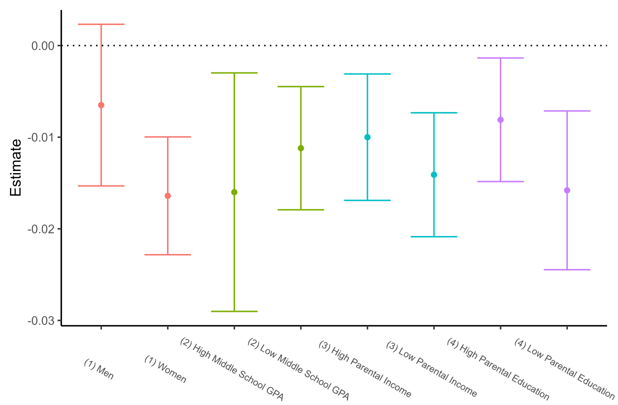
Figure 8: Effects on Later Life Outcomes



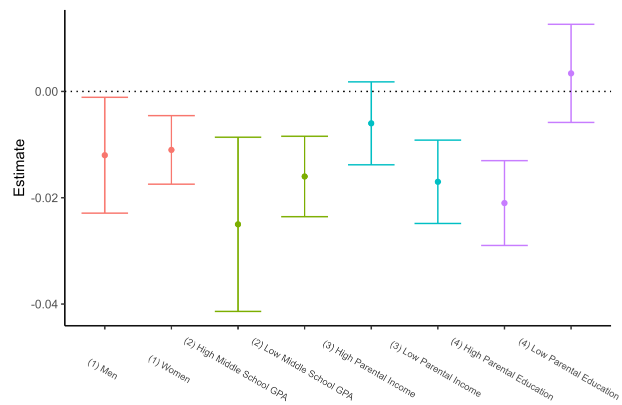
(a) Share of Bonus Courses Instructional Hours



(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 8c, 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 8d 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.