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Upper Secondary Education: Access, Choices and Graduation

Vivika Halapuu

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Vivika Halapuu

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Abstract
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Essay I: We study how Swedish high school students match with programs given their skill endowments at the time of choosing. Using detailed administrative data on high school admissions and earlier school achievement, we construct a multidimensional measure of *program match quality*, reflecting the extent to which students select into programs with skill requirements that align with their skill portfolio. Our results suggest that female students and those from low socioeconomic backgrounds make relatively worse program choices than males and students whose parents have at least some college education. Students with a more appropriate skill set for a given program are more likely to remain in the program, to complete high school on time and they also have higher post-graduation earnings. Better information about how students' relative strengths and weaknesses comply with the skill requirements of programs could prevent costly educational, and consequently occupational mismatch.

Essay II: The paper provides the first causal evidence of how access to education affects disability insurance (DI) claims among low-skilled youths. The research design exploits recent changes in high school eligibility criteria among a set of low-performing compulsory school graduates in Sweden. The results show that the immediate inflow into the DI system increased by 5.1 percentage points among the students who were excluded from standard high school programs. The fact that outflow from DI is very low (half of all young claimants remain in the system after 10 years) together with auxiliary findings indicating that the impact remains high during the short follow-up period suggest that the effect is likely to persist over many years. The results highlight that the design of education systems is a crucial determinant of DI claims among young people and that reforms which limit low-skilled youths' access to education can have lasting detrimental effects on their labor supply.

Essay III: This paper studies the impact of stricter graduation requirements on vocational high school graduates' behavioral responses and early career outcomes exploiting an increase in graduation standards in Swedish vocational high schools. An important feature of the reform is that it increased both general and occupation-specific graduation requirements. Using a unique combination of course-specific grades and detailed administrative data on labor market, I study the incentive effects, and compare job finding rates and job match quality of academically similar students just below and above the two different graduation thresholds using difference-in-differences design. I find no impact of higher general skill requirements on youths' school-to-work transition. Stricter specific skill requirements, on the other hand, come with strong incentive effects, and lead to a separation in job finding rates and job match quality of students at the margin of barely meeting the threshold.

Keywords: Upper secondary education, Vocational education, Access to education, School choice, Skill inputs, High school performance, Graduation standards, School-to-work transition, Job match quality, Disability insurance, Education policy

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To Mirjam and Leo
for their love and patience

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Stockholm, May 2021
Vivika Halapuu

Contents

Introduction

Completed upper secondary education benefits everyone: it increases the chances of obtaining a tertiary degree and employment at well-paid jobs, lowers the risk of unemployment and is associated with better health. In spite of this, a considerable share of students, often from the lower end of the ability distribution, drop out of high school or exit school without meeting diploma requirements. The recent literature on heterogeneity in the returns to education shows at the same time that the marginal returns are particularly strong among low-skilled students (Meghir and Rivkin, 2011; Dearden et al., 2002; Heckman et al., 2018). It suggests that measures that increase the education of these students would be both efficient (high returns) and come with distributional effects (high returns to a more disadvantaged group; Gunderson and Oreopolous, 2020).

Solving the puzzle of low educational investments among those who would benefit the most from these calls for actions in education systems that would improve access to and the successful completion of high school studies among low-skilled students. The design of education systems may matter particularly for low-skilled students with myopic time preferences (Lawrance, 1991; Becker and Mulligan, 1997). The present-bias in youths' time preferences may induce a non-optimal level of effort in studies, and increase the risk of dropping out. Education systems that impose reasonably high eligibility criteria, and incentives for learning and successful completion of studies may help to mitigate these problems by making the heavily discounted future benefits more tangible.

When granted access to education, young people face the first high-stake decision with long-term consequences—the choice of high school program. There is increasing evidence that the returns to high school education vary across different tracks (Altonji, 1995; Altonji et al., 2012; Rose and Betts, 2004). An important underlying question in that field of literature is why students make different educational choices. Economic theory suggests that the choices build on comparative advantage as in Roy (1951), but in practice, various distorting factors such as time-inconsistent preferences, social norms and beliefs, as well as the influence of friends and parents may have an impact on these decisions.

In this thesis I study how the design of upper secondary education affects young people focusing separately on the entry and exit margin of high school studies. I analyze the decision-making process of high school choice, the impact of increased barriers to vocational high school studies on low-skilled youth, and the effect of stricter graduation standards on students' school-to-work transition. Using rich Swedish register data, I aim at understanding whether some population groups are more responsive to the different distortions and how features of education systems affect students. I relate the educational choices to various private costs and benefits of education, e.g. employment outcomes, earnings and social benefit participation.

A special focus is on education-occupation match and interactions between various institutions. Job match quality is of great importance as it has been shown to be central to the career outcomes of workers. Mismatched workers experience smaller returns to occupational tenure and higher job separations probability (Jovanovic, 1984; Fredriksson et al., 2018; Guvenen et al., 2020). In the thesis I show that the foundations to the match quality are laid already before labor market entry through the high school program choice. I also provide some evidence of how the increase in occupation-specific information content of high school diplomas may improve the costly matching. While the thesis builds on topics in economics of education, I show in Essay II that changes in the education system can have potentially long-lasting detrimental effects for some subgroups through the interactions between various institutions; the education system and the social insurance system in this case.

Throughout the thesis, I pay special attention to students enrolled in vocational high school programs (with the exception of chapter I which includes students from academic programs as well). I do so for several reasons. First, in chapters II and III, I exploit a recent Swedish education reform *Upper Secondary School 2011*. The timing of the reform only allows me to observe employment outcomes for students who directly enter the labor market. Second, the changes in the high school eligibility requirements and graduation standards were introduced separately for academic and vocational programs. The variation in eligibility requirements is less noisy at the margin relevant for vocational graduates allowing me to obtain a causal effect of interest. Lastly, the change in graduation standards introduced by the reform set different demands for vocational students' general and specific skills. Thus, the reform created an interesting setup that allows for distinguishing between the importance of the two types of requirements on vocational graduates' early career outcomes, and thereby add a unique contribution to the literature. In what follows, I give an overview of each of the three self-contained essays of the thesis.

In Essay I, **On the Right Track? Match Quality in High School Choice**, co-authored with Lena Hensvik, we study how individuals make

their study choices. Taking off from Roy (1951), we exploit the fact, often ignored in the existing literature, that in many education systems students' compulsory school grade point average is the sole determinant of admission to high school programs that require different skills. Thus, students in the same program may have a different likelihood of succeeding, depending on their skill set. Based on this observation, we construct a novel multidimensional measure of how well a student's skill endowments at the end of compulsory schooling align with the skill requirements of the chosen high school program. The paper focuses on exploring the student-program match quality by gender and parental background, and costs associated with low match quality.

Our findings show that while students, on average, choose programs that fit their skill portfolio relatively well, female students and those with low socioeconomic status (SES) make significantly worse choices than male and high-SES students. In line with other studies (see e.g. Joensen and Nielsen, 2016; Buser et al., 2014; Goldin, 2015), we find that female students are less likely to choose math-intensive high school programs even when comparing male and female students with the same initial math skill endowments. Supplementary survey evidence suggests that part of the distorted behavior is due to lower confidence in own skills. Students from low socioeconomic backgrounds are also less likely to choose math-intensive tracks, but this pattern seems to reflect differences in ability levels rather than differences in confidence levels.

Improving the student-program match quality is associated with several gains. Our analysis suggests that students with higher match quality are less likely to switch track and more likely to complete high school on time. The initial match quality is also positively associated with future earnings.

By using discrepancies between the skill requirements of jobs and the talent-mix among new entrants, as in Fredriksson et al. (2018) and Guvenen et al. (2020), in the education setting, we contribute to the better understanding of the quality of educational choices. Further, the results suggest that occupational mismatch and earning inequalities are established before students even enter the labor market. These findings open up for possibility to prevent costly mismatch in the labor market by early interventions enforced in the school system, for example by informing students on their comparative advantages and guiding their study choices respectively.

In education systems with access barriers, some students have very limited choices when reaching the next level of education. In Essay II, **Access to Education and Disability Insurance Claims** I study the impact of stricter high school eligibility requirements on low-skilled students' labor market outcomes and social insurance participation. I exploit the variation in access to vocational high school programs generated by

the reform Upper Secondary School 2011. By raising the required number of passing grades from the last year of compulsory school from three to eight, the reform excluded a considerable share of low performing students from vocational programs. Instead, the students started their high school studies in introductory programs that are characterized by low graduation rates. Together with the immediate negative effects on employment outcomes, it suggests that the reform may have presented a negative shock to low-skilled youths' labor market prospects.

In line with earlier studies (Black et al., 2002; Rege et al., 2009), I show that such a shock induces the inflow of individuals into the disability insurance (DI) system. Entry into the DI more than doubled among the affected students after the reform. However, declined labor market prospects are just one of the alternative explanations for the change. The analysis suggests that the enforcement of the reform that mechanically prolongs low-skilled youths' time in education in a setting that allows for entry into the DI system for prolonged schooling accounts for parts of the effect.

The existing literature on the increasing DI rolls has primarily focused on controlling the inflow into the system by manipulating the characteristics of DI systems or the role of employers in keeping their workforce active in the labor market (Autor, 2011; Koning and Lindeboom, 2015). I contribute to the literature by focusing on the more recent trend in DI participation—the increasing inflow of youth into the system. Focus on that group is important as the lifetime benefit amounts of young people may exceed those of older awardees (Von Wachter et al., 2011; Ben-Shalom and Stapleton, 2015). My results show that the design of education systems, access barriers that exclude low-skilled youth from regular high school studies in particular, may serve as a tool for controlling the increasing DI participation trend among young people. The context-specific analysis of DI take-up for prolonged schooling underlines the importance of considering joint efficiency when designing different systems.

Essay III, **Stricter Graduation Standards and Labor Market Entry** analyses the graduation margin of high school studies. I ask whether stricter graduation standards alter students' incentives and facilitate youths' school-to-work transition. Many education systems rely on graduation standards in order to uphold the quality of education and to provide students with a tool to signal their observable and unobservable productive attributes to prospective employers. The literature on the effects of stricter graduation standards provides at the same time scant evidence of any effects (see e.g. Holme et al., 2010; Clark and Martorell, 2014). Moreover, existing studies analyze exclusively the impact of stricter general skill requirements on students from comprehensive school

systems, and focus mostly on various educational and labor market outcomes and less on direct incentive effects.

I enrich the literature by exploiting a reform that substantially raised general and occupation-specific graduation standards for vocational graduates. I show that the two margins of graduation standards affect students' behavior differently. Higher general skill requirements do not increase the probability of reaching the stricter threshold. Stricter specific skill requirements lead, on the other hand, to a sharp sizable increase in the fraction of students who meet the demand. There are several explanations to the discrepancy. The results suggest that students (or educators) perceive the specific skill requirement to carry an important signaling value. At the same time, passing this requirement is also less costly. Existing literature further suggests that more conceptual topics may be more difficult to prepare for (Bettinger, 2012) and that extrinsic motivators may be more effective for concrete subjects rather than more conceptual topics (Lepper and Greene, 1978).

In line with theoretical predictions (Betts, 1998; Betts et al., 2001; Levitt et al., 2016), I find a positive effect on youths' school-to-work transition only at the margin that alters students' (or educators') incentives. Students who pass the threshold that signals a certain level of occupation-specific competence experience a higher job finding rate and job match quality than students below the bar. No such effects are evident at the margin of stricter general skill requirements. The findings indicate that the design of graduation standards can have very different impact on behavior and outcomes of students.

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1. On the Right Track? Match Quality in High School Choice

with Lena Hensvik

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1.1 Introduction

Most students in industrialized countries enroll in high school education. In many cases, the choice of high school program is the earliest career choice an individual has to make, and it has important influence over the future education and occupation path (see e.g. Altonji (1995), Altonji et al. (2012), Levine and Zimmerman (1995), Rose and Betts (2004) and Joensen and Nielsen (2016) for evidence on the returns to high school curriculum). Career decisions have traditionally been understood through the lens of the standard Roy model of selection, which predicts that students should base their education choices on their comparative advantages (Roy, 1951). According to this model, systematic differences in choices reflect systematic differences in expected returns.

However, the relatively young age at which students are supposed to make these high-stake decisions has led to concerns that lack of information, parental influence, norms or beliefs may distort education choices. For example, Walters (2014) shows that students with low socioeconomic status (SES), who have the highest gains from attending a charter school, are the least likely to apply. In addition, a growing literature suggests that female students disproportionately select into less math and science intense careers relative to their similarly skilled male peers (Joensen and Nielsen, 2016; Buser et al., 2014; Goldin, 2015).[1] In order to understand the determinants of earnings inequality it therefore seems crucial to document how individuals make their study choices.

In this paper, we describe how young individuals sort into high school programs given their ability endowments at the time of choosing. In our context, nearly all students enroll in high school and the admission to programs is based on the compulsory school grade point average (GPA), ignoring the fact that certain skills are, as we will show, more or less useful across programs.[2] Consequently, students admitted to the same program may have a different likelihood of succeeding, depending on their relative strengths and weaknesses.[3] However, the role of such skill-specific variation has largely been overlooked in the previous literature.

[1] In particular, Buser et al. (2014) show that a substantial portion of the gender differences in choosing more prestigious high school tracks among Dutch students reflects differences in competitiveness. Some of the most competitive boys aim for mathematically heavy tracks despite low math grades. Goldin (2015) studies college admissions to a liberal arts college finding that women are more sensitive than men to low grades, and more likely to gravitate towards other disciplines when receiving a low grade from introductory economics classes.

[2] High school attendance is tuition-free in Sweden. Hence, financial constraints do not enter the high school enrollment decision.

[3] For example, a student may be more successful in a math intensive high school program if she is particularly talented/interested in math, whereas e.g. verbal skills may be more useful in the social science program.

Empirically, we capture this idea by using population-wide administrative data on high school students, matched to information on their compulsory school subject grades, high school program choices and admissions, as well as high school attainment and labor market outcomes. We use these data to construct a multidimensional measure of how well a student's strengths and weaknesses align with the skill requirements of the chosen program, which we denote the student's program *match quality*. Our measure is based on within-program-year comparisons of entrants and graduates, and match quality is higher for entrants who have more of the skills associated with higher predicted program-specific grade returns in older graduating cohorts.

We use our measure to shed light on the differences in relative match quality among students who start in the same high school program in the same year. We are particularly interested in whether there are systematic differences in student-program match quality by gender and parental background. In addition, we study the responses to program match quality in terms of program switches, high school completion and post-education earnings.

Our results support the idea that different skills are differentially useful across high school programs and that there is strong sorting on the predicted payoffs by gender and socioeconomic status. For example, compulsory school math is twice as useful as Swedish in the natural science program, while they are of equal importance in the social science program.[4]

Turning to our multidimensional measure of student-program match quality we first show that students' program preferences are consistent with their skill endowments: predicted match quality is higher for higher ranked programs.

Low match quality is strongly related to the likelihood of switching track. This suggests that students are not fully informed about how well their talents match with the skill requirements of different programs when making their study choices, but they learn about match quality over time (as in the model outlined by Altonji et al., 2012). On average, students who do change program improve their match. Finally, relative to other entrants in the same program, we find that students who sort less on their productive talents have lower high school completion rates and lower earnings in the long run.

Heterogeneity analysis suggests that female and low-SES students, defined as students whose parents lack higher education, are significantly less well matched than their male and high-SES program peers. Thus,

[4]We show that we gain substantial variation by inferring the usefulness of various inputs based on our estimated returns rather than inferring it from the curricular content of the programs.

these groups sort less on their initial relative strengths when choosing high school program. While we are unable to pin down the exact mechanisms behind this result, confidence in own subject ability, which we infer from supplementary survey-data, appears to contribute to the underrepresentation of women in math-intensive high school programs.

Our paper contributes to several strands of the literature. First, it is to our knowledge the first paper attempting to directly measure student-program match effects in the education setting. Our approach is inspired by recent papers by Fredriksson et al. (2018) and Guvenen et al. (2020) who use discrepancies between the skill requirements across jobs/occupations and the talent-mix among new entrants to assess the role of job/occupation match quality in the labor market. By adopting a similar approach in the education setting, we can shed light on the systematic differences in the quality of program choices in a framework that incorporates multiple dimensions of student inputs. We also contribute to the literature on occupational mismatch by highlighting that the foundation for occupational mismatch is laid already before labor market entry.

Our paper is also related to an emerging literature on the payoffs to field of study or college major (Altonji et al. (2012, 2016) and Kirkeboen et al. (2016)). In particular, Kirkeboen et al. (2016) estimate the returns from post-secondary field choice in Norway documenting that students prefer fields in which they have comparative advantage in terms of earnings gains (as in the Roy model). Besides focusing on high school choice, our study complements their work by providing a direct measure of program match quality.

In addition, we contribute to the research about the underrepresentation of women in STEM fields. Our results confirm that women sort less into math-intensive programs and that they are generally sorting less on their comparative advantages than their male program peers. Importantly, these patterns remain even when accounting for differences in inputs. However, we show that students from low-educated households are also less likely to sort into programs based on their initial skill endowments. Given the associated costs of these deviations in terms of program switching, high school completion rates and long-run earnings, a better understanding about the underlying determinants of program choices seems crucial for policy makers who want to close the gender and SES gap in education.

The paper is structured as follows: Section 1.2 provides a brief overview of the education system in Sweden and explains the data used in the study. In Section 1.3 we estimate a prediction model of the returns to a range of skill inputs across programs, and describe in detail how we use these predictions to create a measure of student program match quality. Section 1.4 presents the main results. We first examine if the quality of program choices varies systematically with students' preference rankings

of programs. Then, we assess how student-program match quality is associated with track changes, high school completion and long-run earnings. Finally, we shed light on differences in student-program match quality by socioeconomic characteristics. Section 1.5 concludes.

1.2 Context and data

1.2.1 Context: High school education in Sweden

Sweden has compulsory schooling until the age of 16, which corresponds to nine years of education. After ninth grade, all Swedish students are entitled to enter high school education. High school enrollment is voluntary but almost all students enroll (more than 99 percent). During the time period under study (2001–2010), students could choose between 17 national tracks. These could either be academic tracks, aimed at preparing students for college education or vocational tracks, targeted towards specific segments of the labor market. All tracks are three years long and the main difference is in the amount of theoretical vs. practical content of the curricula.

Students apply to high school programs in spring of the year of compulsory school graduation by ranking their preferred school×program combinations.[5] To become eligible for any high school program, students must meet an eligibility threshold. During the study period, they must have obtained passing grades in compulsory school math, English and Swedish classes. Students below the threshold are referred to a preparatory track (individual program) with the primary aim to help them become eligible. Conditional on eligibility, GPA of the spring semester of grade nine is the sole merit-based criterion used for acceptance. The GPA reflects the sum of final grades in 16 compulsory school subjects. A grading scale with three passing grades was used during the study period. The grades Fail, Pass, Pass with Distinction (PWD) and Excellent are translated into a final GPA as follows: Fail=0, Pass=10, PWD=15 and Excellent=20. This implies that the minimum GPA is 0, and the maximum is 320.[6] The GPA cutoff for admittance to a given program, in a given school and year, is determined by the lowest GPA among admitted individuals.

[5]Up to six preferences are observable in the data. Students make their preliminary choice during January–February and the final list of preferences is submitted by the end of May. Students are informed about the allocation decisions by mid-July. If some seats are not filled, another round of admission will take place in August–September. Some regional differences may occur in the timeline.

[6]Starting from the school year of 2013/2014, students who have attended an elective language course may account for that grade in the GPA calculation. As such, the highest value of the GPA could be 340.

1.2.2 Data and description of high school entrants

We use enrollment and graduation records from Statistics Sweden that include all high school students between 2001–2010. Enrollment register is used to determine the students' initial high school programs, and the graduation register to determine their final high school GPA. We restrict our sample to students in the 17 national programs, which means that we exclude students enrolled in high schools that are exempt from the national grading system[7] (0.02% of the high school entrants), those enrolled in the International Baccaleureate (0.68%) as well as non-national programs.[8]

In addition, we add information on grade nine GPA and subject grades (maintained in the Grade-9 register) as well as demographic information, and data on education and earnings from the Integrated Database for Labor Market Research (LOUISE register).[9] We also identify the students' parents from the Multigeneration register. Students for whom the identity of both of their parents is missing are excluded from the sample.[10] We end our observation period in 2010 due to a substantial reformation of the education system in 2011.[11]

Table 1 shows the distribution of high school entrants across programs (columns 1 and 2) and the average final grade of compulsory school by program (column 3). A similar table for the graduates is presented in Appendix (see Table A1). The four academic tracks (natural science, social science, arts and technology) together account for around half of the students. Not surprisingly, these students are drawn from the upper part of the compulsory school GPA distribution (the average compulsory school GPA is 201.9, see last row). Columns 4 and 5 show the fraction of female students and low-SES students. Among the academic programs,

[7] E.g. schools providing Waldorf education and a few schools with focus on particular languages and cultures, such as German and French.

[8] 14.5% of students were enrolled in the individual program and 18.4% in the specially designed program. Information on the main programs that had been adapted for the students' needs is used given the availability of sufficiently detailed data (74.3% of cases). The cases with insufficient data are excluded from the sample.

[9] The following individual subject grades are used: mathematics, biology, physics, chemistry, technology, geography, history, religion, social science, Swedish or Swedish as a second language, English, home and consumer studies, handicraft, P.E. and health, music, and arts. In some schools a common grade was given in all science related subjects (NO subjects)—biology, physics, chemistry, technology. In those cases the grade in NO was imputed for all of the four individual subjects. Similarly, schools had the chance to give a common grade in social science related subjects (SO subjects)—geography, history, religion, social science. In those cases the grade in SO was imputed for all of the four individual subjects.

[10] The restriction excludes less than 1% (9,227) of observations. Zeros are imputed for missing data on parents' education. In the later analysis, the imputed values are captured by corresponding dummy variables.

[11] The reform changed both the high school admission and graduation requirements.

female students are overrepresented in the social science program and the arts program, and underrepresented in the technology program. The gender segregation is, however, considerably stronger in the vocational programs, where women are heavily overrepresented in the handicraft program, the health and social care program, as well as the child and recreation program; and underrepresented in the electricity program, the building and construction program, the energy program and the vehicle and transport program. The distribution of students is more even in terms of socioeconomic background, but low-SES students are generally somewhat underrepresented in the academic programs and overrepresented in the vocational programs.

Table 1. *High school entrants 2001–2010*

	(1)	(2)	(3)	(4)	(5)
	No. of students	Fraction students	Average CS grade	Fraction female	Fraction low SES
Academic tracks:					
Natural science	145,881	14.73	265.1	47.5	40.0
Social science	273,914	27.67	233.6	60.9	44.2
Arts	64,773	6.54	223.9	71.0	50.6
Technology	67,661	6.83	222.5	16.0	43.1
Vocational tracks:					
Handicraft	24,878	2.51	214.5	87.4	57.0
Media	47,151	4.76	202.9	59.9	52.6
Natural resource use	30,424	3.07	202.2	67.6	60.0
Health and social care	33,938	3.43	193.0	82.2	61.0
Electricity	64,923	6.56	190.7	4.2	54.0
Building and construction	37,824	3.82	190.6	6.7	55.6
Energy	9,192	0.93	189.8	3.4	53.3
Food	4,579	0.46	189.0	75.0	63.0
Business and administration	44,538	4.50	188.3	66.4	59.3
Industrial technology	21,808	2.20	185.9	11.5	56.1
Hotel and restaurant	41,839	4.23	184.9	61.5	59.3
Child and recreation	37,583	3.80	183.9	74.1	60.0
Vehicle and transport	39,132	3.95	172.0	7.3	65.8
Total/Average	990,038	100.00	201.9	47.2	55.0

Notes. The last row shows the sum of all rows for the first two columns and column averages for the last three columns. Low SES refers to students whose neither parent has obtained tertiary education. CS stands for compulsory school.

1.3 Empirical strategy

This section explains how we measure the quality of the match between a student and a specific program. To fix ideas, we assume, and will later show, that the 17 high school programs listed in Table 1 have different skill requirements. The idea is similar to the model of firm-specific capital in Lazear (2009) where all skills are assumed to be general, but used with different weights by firms depending on their production technology. Thus, we can think about these programs as 17 production functions defined as:

$$A^p = f^p(X_1, X_2, ...X_n) \tag{1.1}$$

where A^p is the output in program p and X_s are the various productive skills in dimensions $s = 1$ to n at the time of choosing high school program.

A students' optimal program choice is the one generating the highest output given her skill portfolio (at the time of choosing). As noted previously, students are admitted based on their compulsory school GPA (i.e. $\overline{X}_s$). But if certain skills are relatively more productive, then students may be more or less likely to succeed depending on their particular combination of X_s, holding $\overline{X}_s$ constant. Empirically, we capture this idea by constructing a measure of *program match quality (MQ)* from the entering student's skill portfolio and the program-specific skill returns. The measure is taken from Fredriksson et al. (2018) who use it to measure mismatch in the labor market and is defined as follows:[12]

$$MQ_{ip} = \frac{\sum_{s=1}^{n} (\beta_{ps} - \beta_s) X_{si}}{n} \tag{1.2}$$

where β_{ps} captures the usefulness of skill input s in program p; β_s is the mean return of skill s across all programs, and X_{si} is student i's amount of skill s. According to this measure, a student is considered to be well-matched to a program if she is endowed with skills that are particularly useful compared to other programs and mismatched if she is endowed with skills with relatively low returns. An advantage with this measure is that it directly relates the payoffs of a given program to alternative choices (i.e. the outside options).

To obtain the β_{ps}'s and the β_s's in eq. 1.2, we let the compulsory school grades in subjects $s = 1...16$ proxy for the skill inputs (X_s) and use these skills to predict high school GPA in graduating cohorts (during 2001–2010). More specifically, we estimate the following linear equation separately for each cohort:

[12] Their focus is on job match quality.

$$A_{ip}^{HS} = \beta_0 + \beta_{p1}X_{1i} + \beta_{p2}X_{2i} + ... + \beta_{p16}X_{16i} + \epsilon_{ip} \quad (1.3)$$

where A_{ip}^{HS} is high school achievement measured by the high school GPA of student i who graduated from program p in year t, and X_{si} are student i's compulsory school subject grades. It is important to note that, due to self-selection, the estimated β's do not represent unbiased estimates of the input returns in each program but the relative payoffs conditional on program choice. However, since we aim to use the measure to compare the relative match quality among students who did select into a specific program (through program×cohort fixed effect models), the input payoffs for earlier cohorts that graduated from that program serve as the relevant population for estimation of eq. 1.3.

When assessing the role of MQ_{ip} we always control for the compulsory school GPA as well as for the direct importance of the vector of individual inputs (s_i). That is, we will use the measure to compare students in the *same cohort* who start the *same high school program*, but who have varying match quality stemming from different combinations of X_s, holding $\overline{X}_s$ constant.[13]

1.3.1 Prediction results and validation

Table 2 displays the estimated program-specific returns to each compulsory school subject grade averaged across the observation period (i.e. the estimated β_{sp}'s from eq. 1.3); rows are compulsory school subjects and columns are high school programs. The estimation sample consists of graduates from all national programs in 2001–2010. Appendix Table A1 provides sample statistics.[14] As noted in Section 1.2, enrollment in any of the 17 national high school programs requires passing grades in compulsory school math, English and Swedish. Reassuringly, Table 1 suggests that these subject skills also have by far the highest returns in most programs. However, the estimates in the table also suggest that there is substantial variation in the estimated returns to skill inputs within a program. For example, compulsory school math is twice as useful as compulsory school Swedish in the natural science program (column 2), while relative strength in Swedish seems more important in the child and recre-

[13] In practice, we will include program×year fixed effects in all estimations. The models used for these analyses are presented in conjunction with the results (see equations 1.4, 1.5 and 1.6 in Section 1.4).

[14] For the cohorts graduating in 2002–2010, the information about the program that they graduated comes from the graduation register, but for those who graduated in 2001, the program stands for the track that the students were enrolled for the 5th term in the beginning of the academic year 2000/2001. We exclude 187 cases where information about the high school GPA is missing.

ation program (column 17).[15] Focusing instead on the across-program returns to specific subject skills, we find that compulsory school math has the highest returns in the natural science program and the technology program; Swedish seems most useful in the child and recreation, and the health and social care programs; and English in the social science and the natural science programs.

As a validation exercise we can relate the estimated returns to compulsory school math, Swedish and English grades to the (minimum) amount of required math, Swedish and English courses in each program according to the national high school curriculum. Reassuringly, this relationship is positive (see Appendix Figure A1), suggesting that programs with the highest returns to ninth grade math skills also have the highest fraction of math courses. But we also note that there is considerably more variation in math returns than in the amount of curricula math,[16] which suggests that different skills are used with different weights in the program-specific courses as well.

1.4 Main results on student-program match quality

In this section we present our main results on the role of student-program match quality, calculated from equation 1.2 in Section 1.3. Section 1.4.1 examines the relationship between match quality and students' rank of programs; Section 1.4.2 examines responses to program match effects in terms of program switching, high school completion and long-run earnings and Section 1.4.3 shows how student-program match quality differs by gender and socioeconomic status.

1.4.1 Match quality and program preferences

Do students prefer programs where their skill-mix is more useful? To examine this question we use information from the enrollment records about how students ranked programs upon application. To measure the difference in match quality depending on the rank we estimate the following model:

$$MQ_{ip} = \alpha_i + \delta_1 Rank^1_{ip} + \delta_2 Rank^2_{ip} + \epsilon_{ip} \tag{1.4}$$

[15] All grades are standardized to have mean of zero and standard deviation of one within each cohort. Thus, in column 2, a one standard deviation higher compulsory school math grade is associated with 0.31 standard deviations higher high school GPA.

[16] The main difference is between the academic and the vocational programs.

Table 2. *Estimated returns to compulsory school grades by program*

	Program-specific returns, β_{ps}																	Mean returns, β_s
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Math*	.20	.31	.14	.29	.17	.15	.18	.20	.17	.14	.24	.20	.18	.23	.13	.13	.12	.19
Swedish*	.20	.17	.18	.12	.11	.18	.17	.13	.11	.26	.12	.10	.23	.15	.18	.15	.27	.17
English*	.16	.16	.11	.13	.07	.11	.05	.07	.07	.09	.10	.10	.10	.12	.08	.04	.08	.10
Biology	.11	.09	.11	.06	.08	.09	.10	.03	.05	.15	.05	.04	.11	.04	.10	.06	.10	.08
Physics	.08	.10	.06	.12	.06	.08	.09	.12	.06	.06	.14	.09	.07	.08	.04	.07	.05	.08
Chemistry	.08	.11	.05	.11	.06	.04	.07	.06	.05	.07	.09	.04	.07	.05	.05	.05	.05	.07
Technology	.03	.03	.04	.04	.05	.05	.05	.10	.08	.03	.09	.09	.04	.08	.05	.07	.05	.06
Geography	.04	.04	.03	.05	.03	.05	.06	.03	.03	.04	.03	.04	.05	.03	.06	.01	.05	.04
History	.09	.08	.06	.05	.05	.06	.04	.07	.03	.04	.05	.04	.04	.06	.03	.01	.04	.05
Religion	.08	.05	.06	.03	.03	.06	.03	.04	.01	.09	.03	.01	.05	.01	.06	.07	.10	.05
Civics	.08	.07	.06	.07	.03	.05	.07	.05	.04	.07	.06	.06	.05	.04	.05	.04	.05	.05
Home studies	.10	.05	.08	.08	.11	.10	.12	.12	.09	.13	.08	.08	.14	.10	.17	.13	.15	.11
Crafts	.04	.04	.05	.07	.12	.07	.09	.09	.12	.07	.09	.12	.08	.12	.10	.15	.07	.09
P.E. and health	.05	.04	.08	.03	.09	.05	.10	.05	.08	.06	.03	.06	.04	.07	.05	.11	.07	.06
Music	.07	.07	.06	.07	.06	.08	.06	.06	.04	.07	.05	.03	.08	.06	.07	.10	.08	.07
Art	.05	.04	.07	.06	.09	.10	.06	.02	.05	.06	.04	.02	.07	.04	.06	.09	.07	.06

Notes. The returns are obtained by regressing high school GPA on grade nine subject grades for the compulsory school graduation cohorts of 2001–2010 using equation 1.3. All grades are standardized to have mean of zero and standard deviation of one. (*) indicates the subjects required for eligibility for any program. Each row reports the returns to a grade nine subject grade, across programs (columns). The programs refer to the following (1)=Social science, (2)=Natural science, (3)=Arts, (4)=Technology, (5)=Handicraft, (6)=Media, (7)=Natural resource use, (8)=Energy, (9)=Building and construction, (10)=Health and social care, (11)=Electricity, (12)=Vehicle and transport, (13)=Business and administration, (14)=Industrial technology, (15)=Hotel and restaurant, (16)=Food, (17)=Child and recreation, (18)=Average of all programs. The number of observations for each program is given by column 1 of Table 1.

where MQ_{ip} is the index of how well student i is matched to program p (see eq. 1.2 for the definition of MQ); $Rank^r_{ip}$ is a dummy taking the value of one if student i ranked program p as her r^{th} alternative. Furthermore, we include student (and implicitly year) fixed effects, α_i.[17]

Table 3, column 1 displays the relative difference in program match quality within the students' choice sets. The most preferred program has on average 0.13 standard deviations higher match quality than the third- or lower-ranked programs (the reference category). Thus, students do indeed prefer programs where their skill-mix is more useful. In column 2, we estimate a slightly different model contrasting students who start the same program. Here, we replace the student fixed effects in eq. 1.4 with a vector of program by cohort fixed effects α_p. This model is informative about the relative match quality among students who start the same program in the same year, but who had the program as a higher- or lower-ranked alternative. In these within-program comparisons, we also account for the students' average compulsory school grades, GPA^{CS}_i, and the vector of the specific compulsory school subject grades, $g(S^{CS}_i)$. The estimates in column 2 are consistent with the student fixed effects estimates: students who were admitted to their first program choice have significantly higher match quality than program-peers who had other preferred (higher-ranked) alternatives.

Table 3. *Differences in match quality by program rank*

	(1)	(2)
	Dep var: Match quality	
1st rank	0.128***	0.094***
	(0.003)	(0.021)
2nd rank	0.078***	0.095***
	(0.003)	(0.019)
Observations	681,729	671,872
R^2	0.500	0.447
Student×Year FE	Yes	No
Program×Year FE	No	Yes
Compulsory school GPA	-	Yes
Compulsory school grades by subject	-	Yes
Dummies for missing grades	-	Yes

Notes. Match quality, compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one within each cohort. Data for cohorts who started in high school during the period of 2001–2007 are used. Reference category is "3rd or lower rank". In column 2 we control for gender and socioeconomic status. In column 1 we cluster standard errors at the student level to account for the fact that we have multiple observations per student. In column 2 robust standard errors are reported. *** p<0.01, ** p<0.05, * p<0.1.

[17] We only use the first year of application for each student.

1.4.2 Relationship between initial program match quality and subsequent outcomes

Besides documenting the extent of student-program mismatch, it is interesting to analyze its relationship to program turnover, high school completion and long-run earnings. This analysis will inform us about the potential costs associated with this kind of mismatch, over and above the impact of the high school GPA. We estimate the following equations:

$$Y_{ip} = \alpha_{pt} + \gamma_1 MQ_{ip} + \gamma_2 Female_i + \gamma_3 lowSES_i + GPA_i^{CS} + g(S_i^{CS}) + \epsilon_{ip} \tag{1.5}$$

where Y_{ip} are the outcomes that we are interested in, MQ_{ip} is the index of how well student i is matched to program p, $Female_i$ is an indicator for female students, $lowSES_i$ is an indicator for students whose neither parent has obtained any tertiary education, GPA_i^{CS} stands for compulsory school GPA, and $g(S_i^{CS})$ is the vector of the specific compulsory school subject grades. We focus on three different outcomes: (i) the probability of switching high school program between the first and second year, (ii) the probability of completing high school on time and (iii) long-run earnings. In our main specifications we include program×year dummies, α_{pt}, but as a robustness check we also estimate models with program, year and maternal fixed effects, implying that we compare the relative match quality among siblings who start their high school studies during the period of 2001–2010.

Program switching and high school completion

Table 4 shows the association between our measure of student-program match quality and the probability of switching high school program. If students were fully aware about their match quality, we would not expect that program peer variation in MQ would predict program changes as the potential costs of being less well matched would be fully internalized at high school entry. Hence, this outcome is particularly interesting, as it speaks to the amount of information about match quality available at the time of choosing program.

In column 1 of Table 4, we show that higher match quality is associated with a quite substantial decrease in the probability of switching program: one standard deviation higher match quality is associated with around 2 percentage points lower likelihood of switching track during the first two years since entry (a 25 percent decrease). Thus, better program choices lead to less disruption of one's study path. The estimate is robust to the inclusion of specific subject skills (column 2) as well as to maternal fixed effects (column 3).

As an additional exercise, we look at the sample of students who do switch program and assess the quality of the new match. To this end,

we compute the initial and subsequent match quality. The results are shown in Appendix Table A3. These suggest that there is a positive and significant difference in the quality between the new and initial match. Hence, on average, students who change programs improve their match.

Table 4. *Initial match quality and the probability of switching high school program*

	(1)	(2)	(3)
	Dep var: Program switch between term 1 and 3		
Match quality	-0.020***	-0.019***	-0.018***
	(0.005)	(0.004)	(0.001)
Female	0.009	0.008	0.009***
	(0.006)	(0.007)	(0.002)
Low SES	-0.001	0.001	-0.004
	(0.003)	(0.003)	(0.006)
Observations	990,038	990,038	988,881
R^2	0.038	0.040	0.691
Mean dependent variable	0.079	0.079	0.079
Program×Year FE	Yes	Yes	No
Program FE	No	No	Yes
Compulsory school GPA	Yes	Yes	Yes
Compulsory school grades by subjects	No	Yes	Yes
Maternal FE	No	No	Yes

Notes. Match quality, compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one for each cohort. Low SES refers to students whose neither parent has obtained tertiary education. All models control for missing information on compulsory school grades and high school enrollment at term 3. Standard errors in columns 1 and 2 and clustered at the program level, and those in columns 3 at the mother level. *** p<0.01, ** p<0.05, * p<0.1.

In Table 5, we change the outcome in eq. 1.5 to an indicator for failing to complete high school on time (within three years). Compared to the baseline probability of changing track, the share of students who do not obtain upper secondary education during the nominal time is much higher. In our sample, 18 percent of students who started their high school studies in 2001–2010 had not obtained a high school diploma within the estimated three years. The results in Table 5 point to the role of initial match quality: one standard deviation higher match quality is associated with 1.3 percentage points (or 7 percent) lower likelihood of failing to graduate from high school in nominal time.

Table 5. *Initial match quality and the probability of failing to complete high school on time*

	(1)	(2)	(3)	(4)
	Dep var: Student did not complete high school on time			
Match quality	-0.013*	-0.013***	-0.010***	-0.013***
	(0.007)	(0.002)	(0.001)	(0.001)
Female	0.020***	0.007	0.004	0.006**
	(0.007)	(0.008)	(0.003)	(0.003)
Low SES	-0.017***	-0.009***	0.025***	0.021**
	(0.002)	(0.002)	(0.008)	(0.008)
Observations	990,038	990,038	988,881	988,881
R^2	0.139	0.151	0.743	0.746
Mean dependent variable	0.182	0.182	0.181	0.181
Program×Year FE	Yes	Yes	No	No
Program FE	No	No	No	Yes
Compulsory school GPA	Yes	Yes	Yes	Yes
Compulsory school grades by subjects	No	Yes	Yes	Yes
Maternal FE	No	No	Yes	Yes

Notes. Match quality, compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one for each cohort. Low SES refers to students whose neither parent has obtained tertiary education. All models control for missing information on compulsory school grades and lacking information about highest level of education three years after compulsory school graduation. Standard errors in columns 1 and 2 and clustered at the program level, and those in columns 3 and 4 are clustered at the mother level. *** p<0.01, ** p<0.05, * p<0.1.

Long-run earnings

Finally, in Table 6, we look at the long-run earnings response to high school match quality. For this analysis, we focus on the cohort of students who started their studies at the upper secondary level in 2001 and 2002, and observe their labor market outcomes ten years after the expected graduation year (i.e. in 2014 and 2015). Our estimates suggest that one standard deviation higher match quality is associated with 1.2 percent higher income ten years after graduation. The relationship can be regarded as non-trivial as we condition on program fixed effects, average compulsory school grade and grades by subject.

Table 6. *Initial match quality and earnings ten years upon graduation*

	(1)	(2)
	Dep var: log(earnings) 10 years later	
Match quality	0.021***	0.012***
	(0.003)	(0.004)
Observations	152,493	152,493
R^2	0.066	0.071
Program×Year FE	Yes	Yes
Compulsory school GPA	Yes	Yes
Compulsory school grades by subjects	No	Yes

Notes. Match quality, compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one for each cohort. Data for the cohorts who enrolled in high school in 2001 and 2002 are used. Both models include controls for gender, socioeconomic status and missing compulsory school grades. Robust standard errors are reported. *** p<0.01, ** p<0.05, * p<0.1.

1.4.3 Heterogeneity in match quality: gender and parental background

A number of studies have documented systematic differences in program choice by gender and family SES. Therefore, it is interesting to explore how these background characteristics are related to program match quality. We assess match quality by gender and socioeconomic status using the following model:[18]

$$MQ_{ip} = \alpha_{pt} + \delta_1 Female_i + \delta_2 lowSES_i + GPA_i^{CS} + g(S_i^{CS}) + \epsilon_{ip} \quad (1.6)$$

The results, presented in Table 7 suggest that female/low-SES students make relatively worse education choices than their male/high-SES program peers. On average, match quality among girls is 0.24 standard deviations below that of boys, conditional on their average grades from

[18]This model is similar to the one used in Table 3, column 2.

compulsory school (column 1). The SES difference is considerably smaller but significant. Interestingly, accounting for the grades in each compulsory school subject reduces the gender differences significantly, while the SES difference remains unchanged. Thus, part but not all of the strong gender difference in match quality across program peers seems to reflect actual differences in skill inputs.

Table 7. *Differences in match quality by gender and socioeconomic status*

	(1)	(2)
	Dep var: Match quality	
Female	-0.244***	-0.043***
	(0.002)	(0.002)
Low SES	-0.040***	-0.040***
	(0.002)	(0.002)
Observations	990,038	990,038
R^2	0.370	0.417
Program×Year FE	Yes	Yes
Compulsory school GPA	Yes	Yes
Compulsory school grades by subjects	No	Yes

Notes. Match quality, compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one for each cohort. Low SES refers to students whose neither parent has obtained tertiary education. Both models control for missing compulsory school grades. Robust standard errors are reported. *** p<0.01, ** p<0.05, * p<0.1.

1.4.4 Discussion of results and the role of beliefs

There are, of course, multiple reasons for why women and low-SES students choose programs which are less well aligned with their skills. Our results point to the role of asymmetric information. To complement the picture, we explore a subsample for which we can extract measures of beliefs about own math, English and Swedish abilities. These data come from a survey (*the UGU Survey*) collected in grade nine for a random sample of students born in 1987. Surveyed students were asked to rank how good they thought they were in math, English and Swedish on a 1–5 scale, ranging from very good to very bad. In the following analyses we use these scales in a reversed order.[19]

It should be noted that this analysis will only be partial, as we do not have information about beliefs for all subjects. However, Panel A of Table 8 shows that students' beliefs in their own subject skills vary substantially by gender. Female students have substantially lower beliefs in their math and English skills, but higher beliefs in their Swedish

[19]Distributions of the confidence measures used in the analyses are plotted in Appendix, see Figure A2.

skills, conditional on their average and subject specific compulsory school grades. In contrast, there is no difference between high- and low-SES students. Panel B further shows the association between beliefs in own math, English and Swedish skills and the program-specific returns to the same subject. These results suggest that stronger belief in own subject-specific skills raises the probability of entering programs requiring those skills. Taken together, these results suggest that confidence in own skills may contribute to the sorting of women into less math-intensive programs and, in turn, to the skill-program mismatch documented in Table 7.

Table 8. *Beliefs in own skills inferred from survey data*

	(1)	(2)	(3)
Panel A:	*Dep var: Beliefs in own skills in:*		
	Math	English	Swedish
Female	-0.184***	-0.136***	0.096***
	(0.024)	(0.022)	(0.019)
Low SES	0.017	-0.026	0.004
	(0.023)	(0.021)	(0.019)
Observations	5,983	5,983	5,983
R^2	0.348	0.339	0.278
Panel B:	*Program returns to:*		
	Math	English	Swedish
Beliefs in own skills	0.144***	0.076***	0.054***
	(0.014)	(0.018)	(0.014)
Female	-0.513***	0.126***	0.418***
	(0.026)	(0.030)	(0.020)
Low SES	-0.079***	-0.172***	-0.083***
	(0.025)	(0.030)	(0.020)
Observations	5,983	5,983	5,983
R^2	0.273	0.158	0.120
Compulsory school GPA	Yes	Yes	Yes
Compulsory school grades by subjects	Yes	Yes	Yes

Notes. Program returns to math, English and Swedish as well as compulsory school GPA and compulsory school subject grades are standardized to have mean of zero and standard deviation of one for each cohort. Low SES refers to students whose neither parent has obtained tertiary education. All models control for missing compulsory school grades. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.5 Conclusions

In this paper, we have documented how students sort into high school programs, and how this sorting process varies by gender and socioeconomic status. We proposed a new empirical measure of the relative quality of program choice among program peers, which incorporates multiple dimensions of skills and program skill requirements. The match quality measure is based on how the portfolio of initial subject skills among entrants corresponds to the returns to those skill endowments among older program peers.

Our analysis suggests that initial program match quality significantly predicts the probability of switching track, completing high school on time and long-run earnings. The fact that student program match quality predicts track changes lends support to the notion that students are not fully informed about how well their talents match the skill requirements of programs when making their choices, but that they learn about how apt they are for a particular program over time.

We also document that female and low-SES students make significantly worse program choices than men and high-SES students conditional on their initial endowments. Subjective math ability is significantly lower among females, and it also predicts the probability of choosing a math-intensive track. Students from low socioeconomic backgrounds are also less likely to choose math-intensive tracks, but this pattern seems to reflect differences in ability levels rather than differences in confidence levels.

Given the potential costs of switching programs as well as the substantial cost of occupational mismatch suggested in the literature, our findings highlight that such mismatch could potentially be prevented already prior to labor market entry through better information about which tracks students are the most apt for given their specific skills. More generally, our results call for further studies elucidating how high school programs are chosen.

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Appendix

Table A1. *High school graduates 2001–2010*

	(1) No. of students	(2) Fraction students	(3) Average CS grade	(4) Fraction female	(5) Fraction low SES
Academic tracks:					
Natural science	124,844	17.47	265.1	45.5	51.1
Social science	216,923	30.36	236.5	63.6	65.4
Arts	43,082	6.03	228.4	72.9	62.7
Technology	40,992	5.74	228.8	14.7	68.3
Vocational tracks:					
Handicraft	13,736	1.92	221.8	87.8	84.6
Media	32,533	4.55	212.6	59.3	75.0
Natural resource use	19,883	2.78	206.3	64.4	82.4
Health and social care	22,147	3.10	198.7	86.7	86.5
Electricity	39,818	5.57	196.2	3.0	82.3
Building and construction	24,366	3.41	189.9	4.1	87.6
Energy	5,491	0.77	190.7	2.6	83.6
Food	3,158	0.44	190.2	72.7	88.6
Business and administration	29,865	4.18	193.2	66.2	87.3
Industrial technology	14,759	2.07	194.8	10.4	83.6
Hotel and restaurant	30,650	4.29	193.5	63.0	86.3
Child and recreation	27,937	3.91	190.2	77.4	87.7
Vehicle and transport	24,380	3.41	177.9	5.0	90.2
Total/Average	714,564	100.00	206.8	47.0	79.6

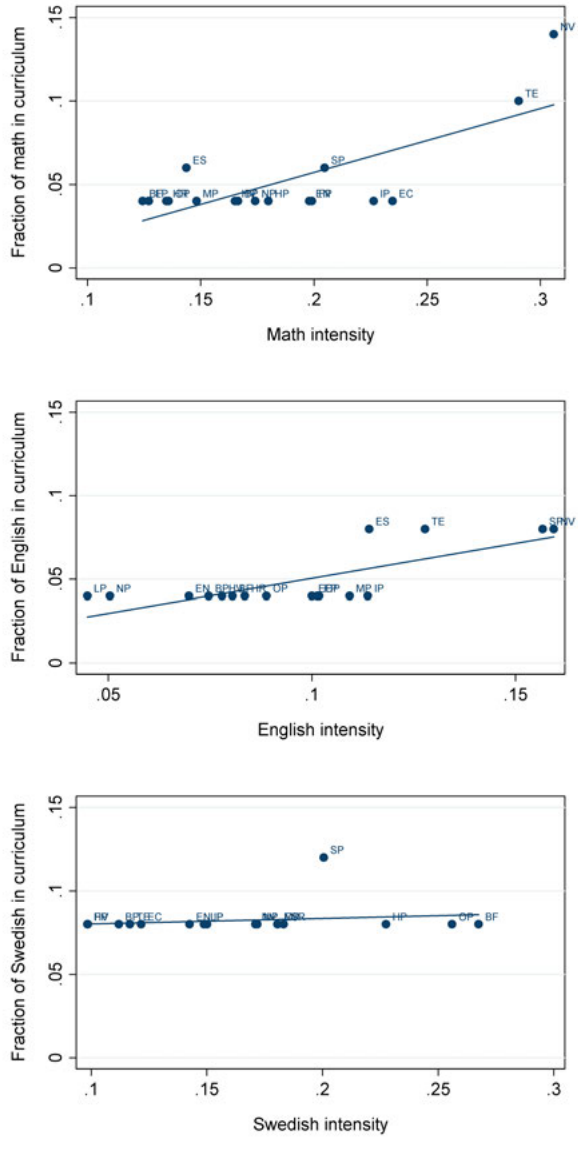
Notes. The last row shows the sum of all rows for the first two columns and column averages for the last three columns. Low SES refers to students whose neither parent has obtained tertiary education. CS stands for compulsory school.

Table A2. *Correspondence between estimated skill returns and curricula content*

	(1)	(2)	(3)
		Program-specific returns:	
	Math	English	Swedish
Curricula content			
Fraction math courses	1.479***		
	(0.213)		
Fraction English courses		1.379***	
		(0.297)	
Fraction Swedish courses			0.910**
			(0.335)
Observations	17	17	17
R^2	0.566	0.575	0.030

Notes. The table shows the estimated relationship between the program-specific high school returns to compulsory school math/English/Swedish grades and each subject's share of the total course load. We focus on the mandatory courses in each program. Curricula information has been collected from the website of the National Board of Education. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1. Returns to compulsory school math and the minimum share of math courses in curricula



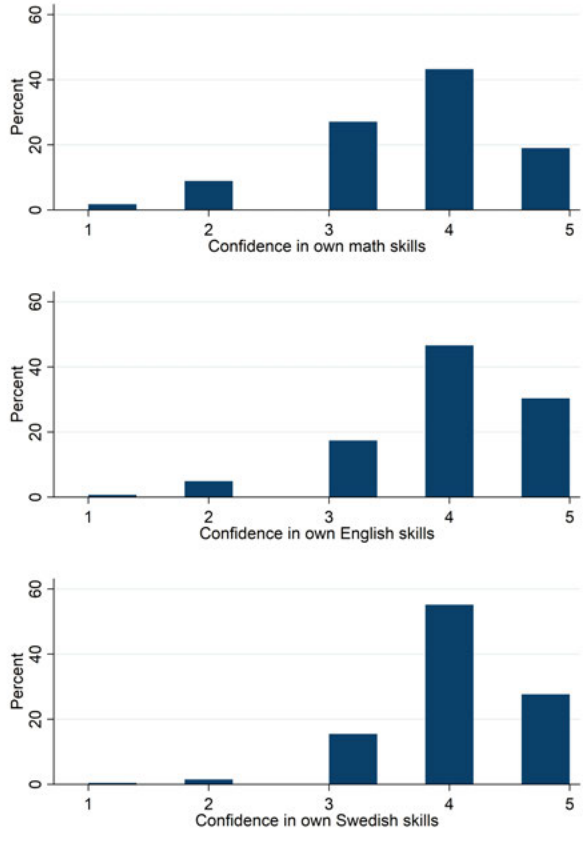
Notes. The labels refer to the following programs: BF - Child and recreation, BP - Building and construction, EC - Electricity, EN - Energy, ES - Arts, FP - Vehicle and transport, HP - Business and administration, HR - Hotel and restaurant, HV - Handicraft, IP - Industrial technology, LP - Food, MP - Media, NP - Natural resource use, NV - Natural science, OP - Health and social care, SP - Social science, TE - Technology.

Table A3. *Initial and subsequent match quality among program switchers*

	(1) Initial	(2) Subsequent	(3) Diff.
Standardized MQ	-0.334	-0.285	0.049***
	(0.004)	(0.004)	(0.006)
Observations	78,195	78,195	156,390
R^2			0.644
Mean dependent variable			-0.310
Student FE			Yes

Notes. The table shows the initial and subsequent match quality for students who switch program between the first and the third term. Standard errors in column 3 are clustered at the student level. *** p<0.01, ** p<0.05, * p<0.1.

Figure A2. Distributions of the skill confidence measures



Notes. Students' confidence in their own skills is measured by the following question: How good do you think you are in the following subjects? Confidence in own skills in mathematics, English and Swedish is shown in the figure. While the value 1 indicates the highest and 5 the lowest level of confidence according to the original scale, a reversed variable is plotted in the figure, i.e. (1)=Very bad ... (5)=Very good.

2. Access to Education and Disability Insurance Claims

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2.1 Introduction

Inflow of youth into disability insurance (DI) benefit schemes has been increasing in several OECD countries, opposing the average trend among the total working-age population (see e.g. OECD, 2010, 2014, 2015, 2019). While the proportion of youth on DI is generally still low,[1] the increasing enrollment trend raises concerns. Along with the spendings on cash benefits, long benefit persistence adds to the costs. The DI exit rate is low and even if claimants leave the DI benefit, they are far more likely to move onto another benefit scheme (Pearson and Prinz, 2005; OECD, 2010, 2012). As youth disability spells start earlier and are more often tied to diagnoses with lower mortality rates,[2] their lifetime benefit amounts may exceed those of older awardees (Von Wachter et al., 2011; Ben-Shalom and Stapleton, 2015). Lastly, early DI take-up adds to the social costs via negative association with employment prospects and labor earnings, and positive association with unemployment and poverty (see e.g. OECD, 2010).

The key in controlling the youth DI rolls, similarly to the DI rolls among the full working-age population, lies in controlling the *inflow* into the system as it has been proved to be ineffective to help people return to the labor market after an absence (Autor, 2011; Koning and Lindeboom, 2015).[3] Among others, more stringent screening systems and employer incentives that encourage the rehabilitation of potential DI beneficiaries before their entry into the system have been used in different countries. The employer incentives are, however, less relevant for young people who, without any work experience, transfer directly from the education system into benefit dependency. Instead, changes within the education system may matter for that group.

The literature on causes and consequences of the rising DI rolls points at the link between DI take-up and low level of education (Autor and Duggan, 2003; Autor, 2011; Kostol and Mogstad, 2014; Korkeamäki and Kyyrä, 2012; Prins, 2013; Banks et al., 2015). Furthermore, the education difference between beneficiaries and non-beneficiaries is shown to have increased over time (OECD, 2010). Yet, this paper is the first to present quasi-experimental evidence on how access to education affects the inflow into DI system.[4] Establishing the causal link is important

[1] On average, 1.8% of 16–29-year-olds were awarded a DI benefit in OECD in 2013 (OECD, 2016a,b).

[2] Mental ill-health is the most common underlying cause for the DI benefits among young people (OECD, 2014, 2019; Banks et al., 2015).

[3] This holds particularly for people suffering from mental illnesses (OECD, 2015).

[4] Closest in spirit is the paper by Poterba et al. (2017) which, relying on a selection-on-observables strategy, shows that men with a high school degree are 0.23 percentage points less likely, and women 0.34 percentage points less likely to participate in DI programs than people without a high school degree.

for avoiding unintended (long-term) social costs and effects on public finances that education reforms may come with. It would also improve projections of future trends in DI participation considering the changes in the educational composition of the population (Poterba et al., 2017).

For estimating the causal effect of education on youth DI enrollment, I exploit an exogenous change in *access*[5] to vocational high school education introduced by a Swedish high school reform in 2011. The reform raised high school eligibility requirements. For cohorts graduating from compulsory school in 2010 or earlier, passing grades in compulsory school math, English and Swedish classes were required for qualifying for vocational high school programs. In 2011, demand for five additional passing grades was introduced. I use a difference-in-differences identification strategy to analyze the impact of the reform on students' DI claims and other economic outcomes. To this end, I compare the students who were eligible for vocational high school studies before but not after the reform to other low-performing students who remained eligible throughout the sample period.

My results show that the immediate inflow into the DI system increased by 5.1 percentage points among the students who were excluded from standard high school programs. The results are robust to a range of different falsification tests and different sample restrictions. For example, the new eligibility requirements did not have any effect on DI take-up among the students who failed compulsory school math, English or Swedish classes and were, therefore, never eligible for high school studies. Lack of effect on DI take-up by older siblings further shows that the affected students do not come from families that are more likely to participate in the DI system.

Supplementary analyses instead show that labor supply of the affected students fell dramatically: their employment probability declined by 36% and they were 12% less likely to be registered unemployed three years after compulsory school graduation. Additionally, labor earnings of the affected students fell by 37%. In line with previous studies (e.g. Black et al., 2002; Rege et al., 2009; Bratsberg et al., 2010), the findings suggest that adverse shocks to employment prospects may induce the inflow into the DI system. Follow-up studies on longer time-series are needed to understand if the short-term effects on labor market outcomes persist or reflect the fact that the affected students stayed in school system longer; most of them started high school studies in preparatory programs that prolong study period. Heterogeneous results by gender show that the reform had a negative effect on boys along all the studied dimensions.

[5]The focus is on the access to education as this is the margin that matters for people with low ability, and that group, in turn, is the one that is overrepresented among the DI beneficiaries.

I evaluate the effects of limited access to education on youth DI take-up in a context where, in addition to declined work capacity, young people can be awarded the DI benefit for completing their education in case of disability-related prolongation of studies. That aspect of the system deserves special attention as it opens up another channel for the inflow of youth into the DI system. While specific to the Swedish system, the DI take-up for prolonged schooling is very similar to the DI take-up for declined work capacity leading to a long benefit dependence. On average, about a third of the youth awarded DI for prolonged schooling remain in the DI system 10 years later. My findings show that the students who did not qualify for vocational high school studies after the reform were more likely to enter the DI system for prolonged schooling. Thus, implementation of the reform that prolonged low-skilled students' studies in the institutional setup that allows for entry into the DI system for prolonged schooling might have magnified the unintended effect of the reform. The finding emphasizes on the importance of taking joint efficiency into consideration when designing different systems.

The reform that increased the high school eligibility standards affected a limited part of population. Hence, the absolute magnitude of the effect is small and alleviated by the strong demographic decline in the relevant age groups coinciding with the reform. In spite of this, it is important to consider the huge relative effects when reforming education systems. In 2016, a report commissioned by the Swedish Government suggested that the entry requirements for vocational high school programs were to be increased to the level of nine additional passing grades (SOU, 2016). In the light of the findings of the paper, such a change could have increased the inflow of youth into the DI system even further.

By providing a causal estimate of limited access to education on the inflow of youth into the DI system, the paper enriches the literature of causes and consequences of increasing DI rolls (see e.g. Black et al., 2002; Autor and Duggan, 2003; Autor, 2011; Kostol and Mogstad, 2014). The findings on the interaction between the education system and the DI system contribute to the literature on the interactions between DI and other institutions, such as unemployment insurance systems, dismissal policies and (early) retirement systems (Hassink et al., 1997; Koning and van Vuuren, 2007; Rege et al., 2009; Bratsberg et al., 2010; Korkeamäki and Kyyrä, 2012). Through the focus on the margin of becoming eligible for vocational high school programs, the paper further informs the discussion about the returns to career and technical education (Kemple and Willner, 2008; Jacob, 2017; Dougherty, 2018; Brunner et al., 2019). Lastly, the paper is related to the literature on the effects of post-compulsory education on various other outcomes, such as crime (Lochner and Moretti, 2004; Hjalmarsson et al., 2015; Åslund et al., 2018; Huttunen et al., 2019).

The paper is organized as follows. Section 2.2 gives an overview of the Swedish DI system, including the most recent trends in youth DI participation, and the Swedish education system. After describing the changes in access to high school studies enforced by the high school reform in 2011, Section 2.3 proceeds with information on the data and sample construction and Section 2.4 with the methodological approach. Section 2.5 presents the main findings together with a list of robustness and falsification tests. Section 2.6 concludes.

2.2 Institutional setting

2.2.1 The Swedish disability insurance system

The DI system serves as a part of the social safety net that helps people with limited working ability to alleviate poverty, and enables them to participate in society. In Sweden, different tax-funded benefit schemes are in place for that purpose for younger and older people. Throughout the study period of 2007–2012, 19–29-year-olds who, due to sickness, accident or disability, were not able to work full time at any job available in the labor market for at least a year were eligible for an activity compensation (*aktivitetsersättning*). 30–64-year-olds suffering from permanently reduced work capacity were eligible for a more permanent benefit scheme—sickness compensation (*sjukersättning*). As this paper studies the inflow of youth into the DI system, the focus of the paper is on the activity compensation.

The activity compensation provides youth suffering from a lasting reduced work capacity with replacement of foregone earnings. A majority of the beneficiaries are eligible for the tax-funded guaranteed compensation.[6] People who due to sickness, accident or disability are not able to graduate from high school on time could apply for the benefit for completing their studies (DI benefit for prolonged schooling). For that, it has to be shown that the inability to graduate from school on time indeed depends on a disability or declined health, and not on other problems such as social issues, tiredness of school etc.

People first qualify for the activity compensation in July of the year when they turn 19, and the benefit can be assigned for 1–3 years at once. The temporary nature of the benefit is to combat the lock-in effect in the social insurance system affecting especially young people (ISF, 2013). Despite the fact, about 50% of the beneficiaries are still in the DI system 10 years later (shown in Figure 2 in the next section). The benefit is

[6]In 2014, about 93% of all activity compensation beneficiaries received the guaranteed level of the benefit (Riksrevisionen, 2015). Only a small share of all beneficiaries qualify for the income compensation funded by social insurance contributions.

supposed to be accompanied with different rehabilitation activities to improve the work capacity of the beneficiaries and, hence, enhance their return to the labor market. If still suffering from declined work capacity by the age of 30, the beneficiaries can transfer to the more permanent benefit scheme for 30–64-year-olds.[7]

The activity compensation is fairly generous. In 2011, individuals with no previous work experience were entitled to guaranteed compensation at the average amount of 7,900 SEK (approx. 900 EUR) per month.[8] In addition to that, DI beneficiaries may receive a non-taxable housing supplement. Single people with low income could in 2011 get at most 5,000 SEK (550 EUR) per month to cover their housing costs. For cohabiting people, the housing subsidy is half of the amount. To set the figures into perspective, the unemployment subsidy available for people under 25 years of age with no previous work experience (*utvecklingsersättning*) amounted to 1,050 SEK (approx. 100 EUR) per month and the monthly minimum wage[9] was approx. 20,000 SEK (2,200 EUR) in 2011.

The DI system is managed by the Swedish Social Insurance Agency (SSIA). In order to be awarded the benefit, an application together with relevant health-related information must be sent to the SSIA. Each application is assigned to a case manager—an employee of the SSIA who determines a claimant's right for the benefit. The case managers may obtain additional information by consulting the doctor who diagnosed the medical cause underlying the application, and other relevant parties for decision making. In case of a declined application, the applicant may request that the SSIA reconsider the decision and may thereafter appeal the decision to an administrative court.

When awarding activity compensation for prolonged schooling, different rules apply for students in regular schools and those enrolled in schools for students with learning disabilities.[10] The case managers have to decide

[7] A majority (about 70%) of the youth who transfer to the permanent benefit scheme suffer from various mental disorders and 8% have diagnoses related to various nerve system disorders (Försäkringskassan, 2017). Since 2017, people aged 19–29 who are permanently disabled qualify for the permanent benefit scheme as well. During the period 2003–2016 the only possible DI benefit for the latter group was, however, the activity compensation.

[8] This is a pre-tax benefit level. The guaranteed compensation level does not depend on individual's work history, but it is age dependent. People with previous work experience before getting sick were entitled to income compensation at the amount of 64.7% of the average monthly salary during the past year, with a ceiling set at 17,120 SEK (approx. 1,900 EUR).

[9] As there is no statutory minimum wage in Sweden, it is proxied by the 10^{th} percentile of the actual wage distribution in 2011.

[10] Students with developmental disorders may enroll in schools for students with disabilities (*särskola*). Students with deafness or impaired hearing can attend special needs schools (*specialskola*) with ten years of education.

in each case whether an applicant's need for prolonged schooling is tightly linked to disability. For that purpose, a medical certification proving a disability must be issued by a healthcare specialist. In addition, necessary information has to be obtained from the applicant and the school they are enrolled in. No medical certificates are needed for the decision for students enrolled in schools for students with learning disabilities. In these cases, a document confirming enrollment in such a school is enough to prove the existence of an underlying disability.

The decision making is complex and has become more so as the set of diagnoses has expanded (Försäkringskassan, 2018b). The case managers find it particularly difficult to determine the right for the activity compensation for prolonged schooling for students enrolled in regular schools. It is also considered difficult to assess when a student's right for the DI benefit for prolonged schooling starts and how long it should last. The potential for higher quality of the DI award decisions for prolonged schooling has been addressed in the SSIA's legal quality monitoring (Försäkringskassan, 2018a). DI benefits awarded for declined work capacity suffer from similar problems (Försäkringskassan, 2016).

Swedish youth on disability benefits

While DI participation among younger people is well below the participation rate of older people,[11] the trend in youth DI enrollment has been increasing over time. In 2003, when the activity compensation scheme was first introduced, 0.4% of 19–29-year-olds were enrolled in the benefit scheme. The share had increased by a factor of 7.5 to 3% in 2017 (see Figure 1). DI participation among 30–64-year-olds has, in contrast, declined from its peak of 12.2% in 2006 to 6.1% in 2017.

Early DI take-up is highly persistent. The Kaplan-Meier survival estimates in Figure 2 indicate that 69% of all people who at the age of 19 were awarded the activity compensation were still on DI benefits five years later and 50% 10 years later. Furthermore, about 38% of the initial pool had transferred to the permanent benefit scheme for 30–64-year-olds by the age of 31.

Falling into the benefit dependency is associated with various costs. Table 1 shows descriptive associations between education, earnings and early DI take-up. People who are awarded DI benefit at the age of 19 are very unlikely to obtain any post-secondary education. While 31.4% of all 19-year-olds in 2003–2007 had obtained some post-secondary education by age 23, the figure was 29.1 percentage points lower for those on DI benefits. Virtually none of the early DI beneficiaries had obtained a three year college degree by age 23. Secondly, people on DI benefits have

[11] About 3% of all 19–29-year-olds were awarded a DI benefit in 2017. The figure is twice as high among 30–64-year-olds.

Figure 1. Trends in DI participation among 19–29- and 30–64-year-olds during the period 2003–2017

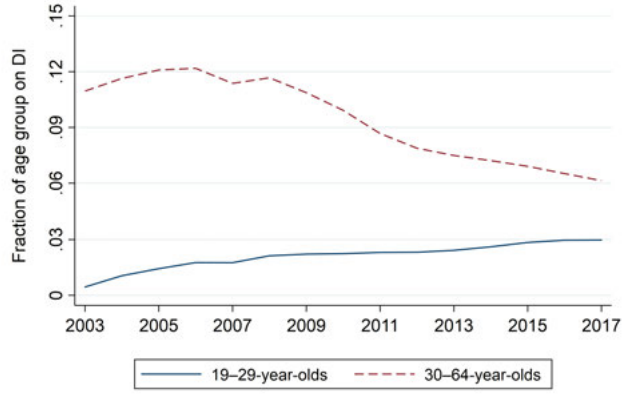
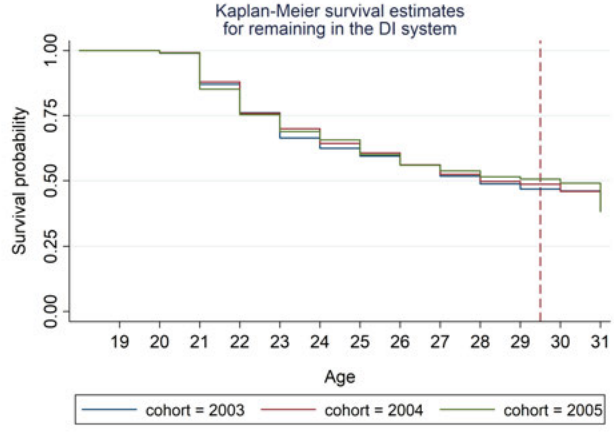


Figure 2. Survival estimates for adolescents' DI participation



Notes. The figure plots 13 year survival estimates for remaining in the DI system for people who were awarded the activity compensation at the age of 19. Data for 19-year-olds in 2003–2005 are used. Only people with one continuous DI spell during the time period are included in the analysis (about 72% of all the 19-year-old DI beneficiaries in 2003–2005). The vertical dashed line marks the end of the eligibility for the activity compensation and the beginning of the eligibility for the sickness compensation scheme for 30–64-year-olds.

worse labor market outcomes. Labor income (measured at the age of 29) of the early DI beneficiaries was about 70% below that of the non-beneficiaries and their disposable income was about 37% lower than that of the non-beneficiaries.

Table 1. *Associations between post-compulsory education, earnings and early DI take-up*

	(1) Any post-secondary education	(2) 3 year college	(3) Labor earnings	(4) Disposable income
DI take-up	-0.291***	-0.075***	-163.073***	-85.921***
	(0.002)	(0.001)	(1.414)	(0.761)
Observations	559,264	559,264	531,117	524,738
R^2	0.005	0.001	0.020	0.017
Mean dep. var.	0.314	0.077	234.448	230.238
Cohort dummies	Yes	Yes	Yes	Yes

Notes. Dependent variables are given by the column headers. Data for 19-year-olds in 2003–2007 are used for the analysis. DI take-up is measured at the age of 19, highest level of education at the age of 23, annual labor earnings and disposable income at the age of 29. Observations with no data on the highest level of education, all negative values of the income variables and the observations with income above the 99$^{\text{th}}$ percentile within each cohort are excluded from the analysis. Labor earnings and disposable income are presented in thousands of Swedish kronor. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

2.2.2 The Swedish school system

The Swedish compulsory school system and transition to high school until the school year 2010/2011

The Swedish school system stipulates nine years of compulsory schooling starting at the age of 7 and ending in grade nine at the age of 16. Grade repetition is very uncommon. After compulsory school graduation, most students continue their studies at the post-compulsory level. The Swedish high school system provides students with a selection of various programs. Academic high school programs prepare students for further studies at the tertiary level, whereas a majority of the graduates from vocational tracks enter the labor market directly. Before 2011, students could choose between 17 national programs[12]—four academic and 13 vocational—which were often provided within the same schools. High school admission is determined by students' final grades from compulsory school.

During most of the compulsory school, students do not receive official grades.[13] Until the academic year of 2010/2011, official grades were assigned in each of the compulsory school subjects in the end of each semester from grade eight onward, and the grades could take the values of Pass, Pass with Distinction, Excellent or Fail, where the first three

[12]In addition to these, a few non-national programs (e.g., International Baccalaureate and individual program) were available.

[13]Instead, individual development plans follow students' development.

were considered as passing grades with numeric values of 10, 15 and 20, respectively, and the latter (with numeric value of 0) indicated failure in a subject. In the end of the spring term of grade nine, students receive their school leaving certificates with final grades.

To become eligible for high school studies, students must have obtained passing grades in grade nine math, English and Swedish classes. Most students met the eligibility criterion: the share of those below the threshold varied from 11.6% to 12.3% during the period of 2007–2010 (see Appendix Table A2). Students below the threshold could obtain passing grades in the missing courses in summer school, and after that apply for any of the national programs. Alternatively, they could start their high school studies in an individual program with a low probability of high school graduation.[14] Out of the approximately 12,000 students who started in the individual program in 2002, only 19% had graduated from high school in four years and 23% in five years (Regeringen, 2009). For comparison, about 80% of the students who enrolled in vocational programs graduated from high school in four years (Skolverket, 2014a).

In order to enroll in any of the national high school programs, students list their school×program preferences. In smaller municipalities, the choice often reduces to the choice of a program only. Conditional on meeting the eligibility criterion, seats are allocated based on the final grades from compulsory school.

Stricter high school entry requirements since 2011

On July 1st, 2011 a high school reform "Upper Secondary School 2011" (*Gymnasieskola 2011*, GY2011 hereafter) that tightened high school eligibility requirements was enforced in Sweden. The purpose of the reform was to guarantee that (i) the prospective students would be better prepared for high school studies and the dropout rates from high school would shrink, and (ii) high school graduates would be better prepared for further studies at the tertiary level or for direct entry to the labor market (Skolverket, 2011).

To reach the target, a set of changes was introduced. Most importantly for this study, GY2011 tightened high school entry requirements to ensure that the students enrolled in different programs would indeed have the necessary prerequisites for coping with these.[15] The students

[14] This program could be adapted to the particular needs of each student who lacked passing grades in some or all of the three key subjects with the main purpose of preparing them for the education in standard high school programs. It could also be combined with studies in the program that a student pursued to get enrolled in.

[15] Additionally, the set of high school programs was expanded to six academic and 12 vocational programs. The difference between the academic and vocational programs was increased. GY2011 removed direct eligibility to studies at the tertiary level from vocational programs. In order to become eligible for such studies, vocational students

graduating from compulsory school in 2011 or later must have obtained passing grades not only in grade nine math, English and Swedish classes as earlier,[16] but in additional five subjects as well for qualifying for vocational high school studies. There are no restriction on the set of the five additional courses.[17] The new rules apply for both public and private schools.

Students below the threshold can start their high school studies in one of the five new introductory programs.[18] These programs were to replace and improve the earlier individual program, and to provide students with skills necessary for transferring to some national program or familiarizing them with working life. The probability of transferring to a national high school program and graduating from any such program remained, similarly to the earlier individual program, low among the students enrolled in the introductory programs. 38.5% of all students who started in an introductory program in 2011 dropped out of high school without ever transferring to any national program (Skolverket, 2014b). Labor market outcomes of the students who only have been enrolled in introductory programs are shown to be weak (Skolverket, 2019).

2.3 Data

The study relies on student level Swedish register data for the cohorts who graduated from compulsory school in 2007–2012.[19] Compulsory school records of these students are linked to data on students' high school enrollment, their employment and unemployment records, DI participation data, as well as data on their parents.[20] Data on the results of national

must adjust their high school curriculum with additional English and Swedish courses since 2011. A new grading system was introduced as well. The earlier scale was replaced with the one ranging from A to F, where A–E are passing grades and F a failing one.

[16] As an exception, newly arrived students from countries where no tuition was provided in English were allowed to enroll in national high school programs even without passing grades in compulsory school English classes.

[17] Students aiming for academic programs must have obtained passing grades in nine additional subjects, whereas program-specific requirements apply.

[18] Alternatively, the students could stay in compulsory school for another year for obtaining the missing passing grades.

[19] Due to changes in compulsory education that coincided with GY2011, the post-reform cohorts of 2010/2011 and 2011/2012 are the only ones that went to compulsory school under the same rules as the pre-reform cohorts and, thus, provide me with comparable data. The changes in compulsory education affected both curriculum and grading system. The new system was first implemented on the cohorts who were at most in grade eight in 2011/2012 (SOU, 2016).

[20] For a small share of students with several compulsory school graduation records, the earliest one is used.

tests in math, Swedish and English conducted in grade nine are also used but due to different data sources, these data cannot be merged to the main data set that contains information on DI take-up.

I focus on students with passing grades in grade nine math, English and Swedish classes. All these students would have been eligible for vocational high school studies both before and after 2011 without the reform. Furthermore, while GY2011 tightened the entry requirements for both academic and vocational programs, I only focus on the students who are at the margin of making it into vocational ones. For that purpose, all students with at least nine additional passing grades from the final year of compulsory school are excluded from the sample as they could potentially be eligible for academic programs.[21] The reason for focusing on the vocational graduates is twofold. First, labor market outcomes are not determined for the students enrolled in academic programs shortly after high school graduation as most of the students continue their studies at the tertiary level. Vocational students enter the labor market faster, providing relevant data when studying the effects on labor market outcomes. Secondly, the less precisely determined requirements for enrollment in academic programs (due to the track-specific demands) would undermine the identification at that threshold.

The main sample of the study contains data for 10,795 students who graduated from compulsory school during the period 2007–2012 with passing grades in grade nine math, English and Swedish classes. Additionally, all of these students had 1–8 passing grades in different compulsory school subjects. The cohorts graduating from compulsory school at the latest by the end of the academic year 2009/2010 are treated as *pre-reform cohorts* and those graduating in 2011 or later as *post-reform cohorts*. Students with less than five additional passing grades from grade nine form the group of *ineligible students*—the students who are no longer eligible for vocational high school programs after the reform, even though students with the same results graduating during the earlier years were. Since 2011, ineligible students could start their high school studies in some of the introductory programs with much lower graduation probability or choose not to enroll in high school at all. Students with 5–8 additional passing grades form the group of *eligible students* who remained eligible for vocational high school studies throughout the study period. In the empirical estimation, that group serves as a control group and the group of ineligible students as a treatment group.

The sample is, as expected, different from the full pool of students as shown in Appendix Table A3, which limits the external validity of the

[21] Whether they were or not depends on the exact set of passing grades they obtained and their study choices as the entry requirements were stated at a more detailed level for the academic programs.

study. However, the indications that DI take-up is higher among the low-educated (Black et al., 2002; Autor and Duggan, 2003; Korkeamäki and Kyyrä, 2012; Kostol and Mogstad, 2014; Karnehed et al., 2015) and low-ability population (Gravseth et al., 2008; Johansson et al., 2012) clearly support the focus on the lower end of the ability distribution.

2.4 Empirical strategy

2.4.1 Main model

For estimating the effect of tighter high school eligibility requirements on adolescents' inflow into the DI system, I employ the difference-in-differences (DiD) estimation technique. Under the parallel trend assumption, it enables me to estimate the parameter of interest by comparing the DI take-up rates among the eligible and ineligible students before and after the implementation of the reform. The model is given by eq. 2.1, where $Ineligible_s$ is a dummy variable that takes the value of one for students who do not meet the stricter high school eligibility standards, $Post_y$ is a dummy for post-reform period and β_3 denotes the main parameter of interest that captures the effect of limited access to vocational high school studies on DI take-up. A set of background characteristics X_{iy}, such as gender, migration status, and parental characteristics (DI take-up, income and education), is used in some specifications.

$$\begin{aligned} Y_{iy} = \beta_0 + \beta_1 Ineligible_s + \beta_2 Post_y + \beta_3 Ineligible_s Post_y \\ + X'_{iy}\varphi + \epsilon_{iy} \end{aligned} \tag{2.1}$$

The main outcome variable (Y_{iy}) of interest is DI take-up three years after compulsory school graduation. Young people could be awarded activity compensation at the earliest in July of the year when they turn 19. This corresponds to the year $t+3$ (where t is the year of compulsory school graduation) in most of the cases. In some of the robustness analyses, I measure DI take-up at $t+2$ and $t+4$ to study how the effect evolves over time. In terms of the DI benefit, the paper focuses on the take-up of the guaranteed level of activity compensation as very few students qualify for the income related part.

In addition to DI take-up, I study the effects of the reform on various student outcomes such as enrollment in vocational and academic high school programs, starting high school studies in any of the introductory programs and not enrolling in high school at all (see Section 2.5.1). As the total high school enrollment rate decreased by less than one percentage point after the reform (see Appendix Figure A1), these models mostly study the reform effect on the reallocation of students between different

high school programs.[22] I also ask how the reform affected graduation probability over time, the share of students still enrolled in high school four years after initial enrollment (I call this prolonged schooling), and the high school dropout rate. Students are defined as dropouts if they neither graduated from high school three years after compulsory school graduation nor were enrolled in high school four years after compulsory school graduation.

In order to assess the channels of the reform effect on youth DI take-up, I estimate the reform effect on various labor market outcomes. The following outcome variables are used for that purpose: probability of employment (to be classified as employed, people must have been working for at least one hour a week in November), probability of unemployment (people are defined as unemployed if they were registered as open unemployed and/or participants of any labor market programs for at least a day in a particular year), length of unemployment conditional on being unemployed (that indicates the total number of days a person was registered as open unemployed and/or a participant of some labor market program), annual labor earnings and disposable income.

2.4.2 Threats to validity

The DiD estimates are unbiased under a few assumptions. Most importantly, identification of the parameter of interest requires that in the absence of the reform, the actual trend of DI take-up among the eligible students and the counterfactual trend of DI take-up among the ineligible students would have been the same. In other words, both groups should have experienced the same average change in the outcome variable had the reform not taken place. The parallel trend assumption is crucial, because the DiD approach eliminates time-invariant differences between the groups of ineligible and eligible students, but the time-dependent variation remains intact.

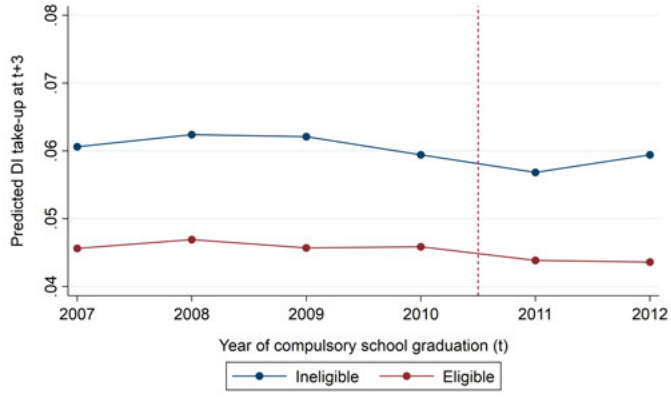
Since the counterfactual trend among the group of ineligible students is not observable, I cannot test the assumption directly, but Figure 5 (shown together with the main results in Section 2.5) provides rather compelling evidence in favor of it. Panel A of the figure illustrates that the DI take-up rate evolved rather similarly among the ineligible and eligible students during the pre-reform era (2007–2010). Panel B shows further that the effect of stricter high school eligibility requirements on DI take-up in 2007–2009 did not differ statistically significantly from the

[22]Students enrolled in the International Baccalaureate, specially designed programs (up to 2011) or programs with nationwide admission cannot be classified into any of these categories unless there is enough information for re-classifying them into some of the national programs. All of the students are kept in the sample for the analysis, but no high school category has been assigned for them.

effect in 2010 (the last pre-reform year). However, DI take-up among the ineligible students increased significantly in 2011 when the reform was introduced. The DI take-up rate among the eligible students does not show any sharp changes after the reform.

Related to the parallel trend assumption, it is important that the composition of the groups of eligible and ineligible students did not change together with the reform. Behavioral changes leading to differential sorting into programs could have occurred since GY2011 removed direct eligibility for tertiary education from vocational programs. If academically stronger students were interested in having the direct eligibility and responded to the reform by exerting extra effort to qualify for academic programs, the reform might have led to negative selection in the control group. I analyze the composition of the groups by studying the balance of predetermined covariates in the sample. Table 2 shows that the groups of eligible and ineligible students experienced similar changes in terms of most of the pre-determined characteristics.[23]

Figure 3. Predicted DI take-up rate among the ineligible and eligible students three years after compulsory school graduation



Notes. The predicted DI take-up is estimated as a function of students' gender, migration status, compulsory school GPA, parents' DI take-up, sum of parents' income, their employment status and education. The vertical dashed line marks the introduction of the stricter high school eligibility requirements. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011.

The stability of the composition of the groups is further assured by Figure 3 which plots the predicted DI take-up among the groups of ineligible and eligible students. The predicted DI take-up is first estimated for

[23]In Section 2.5.4 I present a set of robustness tests, including one addressing the concern of potential negative selection in the control group by expanding the sample to students just above the eligibility threshold for academic tracks (those with nine and 10 additional passing grades).

each individual as a function of their pre-determined background characteristics (gender, migration status, compulsory school GPA, parents' DI take-up in the year when the student graduated from compulsory school, sum of parents' income, their employment status and education). Figure 3 shows that both groups experienced very similar trend in the predicted outcome throughout the sample period. Thus, the baseline characteristics of the groups cannot explain any sharp changes in the actual DI take-up among the ineligible students after the reform.

Table 2. *Balance of covariates*

	(1) Effect of Ineligible×Post	(2) Std. Err.	(3) Pre-reform mean
Students' characteristics:			
Female	-0.033	0.031	0.414
Not born in Sweden	-0.028*	0.015	0.072
Math test score$^{\diamond}$	0.065	0.046	-0.428
English test score$^{\diamond}$	-0.004	0.041	-0.294
Swedish test score$^{\diamond}$	0.011	0.043	-0.431
National test score in math missing$^{\diamond}$	0.083***	0.030	0.185
National test score in English missing$^{\diamond}$	0.062**	0.028	0.146
National test score in Swedish missing$^{\diamond}$	0.031	0.026	0.152
Parents' characteristics:			
Mother on DI	0.005	0.021	0.153
Father on DI	-0.007	0.017	0.095
At least one parent on DI	0.007	0.025	0.224
Mother not working	0.026	0.027	0.194
Father not working	-0.007	0.023	0.166
Sum of parents' income (hundreds of SEK)	141.3	193.4	3657.1
Mother - less than high school	0.021	0.026	0.213
Mother - high school/some post-secondary	-0.031	0.030	0.662
Mother - tertiary education	0.010	0.020	0.098
Father - less than high school	0.017	0.026	0.246
Father - high school/some post-secondary	-0.024	0.029	0.615
Father - tertiary education	0.007	0.018	0.058
F-test	0.753		
p-value	0.711		

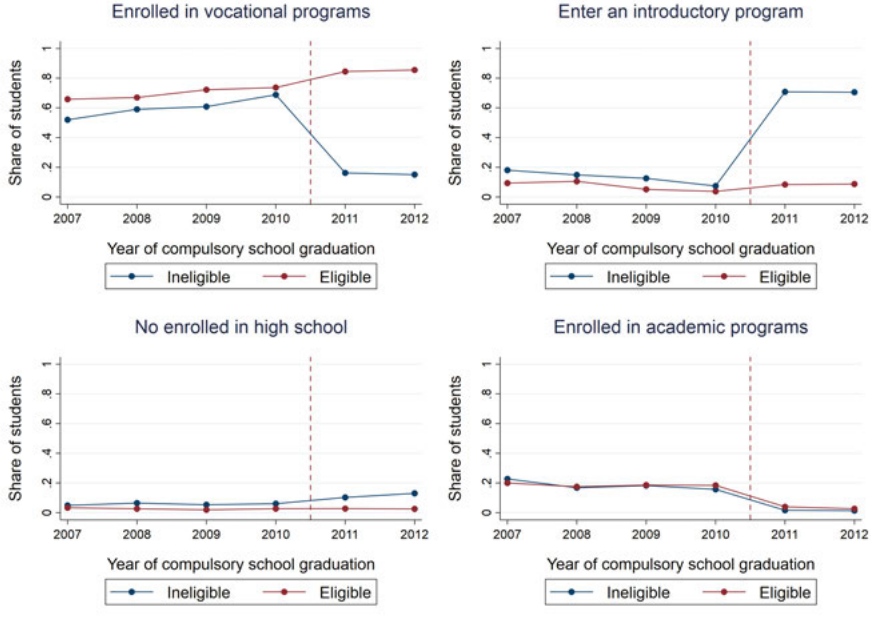
Notes. The results in column 1 are estimated by equation 2.1, where Y_{iy} stands for the variables presented in the first column. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. For estimating the effect of the reform on students' national test scores, actual grade in a corresponding subject is imputed for all of the missing values. ($^{\diamond}$) Data on national tests come from a different data source and cannot be linked to the rest of the data. Due to that, the joint significance test reported in the end of the table excludes these variables. A separate joint significance test testing whether all of the coefficients of the national test scores and the indicators of missing test data are simultaneously equal to zero report a F-statistic of 1.533 with a p-value of 0.163. *** p<0.01, ** p<0.05, * p<0.1.

2.5 Results

2.5.1 Effects on high school enrollment

I start by presenting the results on the effects of GY2011 on various student outcomes. Figure 4 and Table 3 suggest that the increased entry barriers come with a strong reallocation of students across different programs, the effects being just at the margin where students were targeted by the reform (similar results for the full student population are presented in Appendix Figure A2). Students just below the stricter high school eligibility threshold are 59.5 percentage points less likely to enroll in vocational programs. Instead, the probability of starting high school studies in any of the introductory programs (with a low probability of high school graduation) increases by 55.9 percentage points. As expected, no significant change between the groups occurs in the enrollment in academic high school programs.

Figure 4. Share of students enrolled in different high school programs before and after the reform



Notes. Until the school year of 2011/2012, the introductory programs were not organized in the same way but an individual option existed. The vertical dashed lines mark the introduction of the stricter high school eligibility requirements. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011.

Table 3. *Effects of GY2011 on high school enrollment*

	(1) Enrolled in vocational programs	(2) Enter introductory programs	(3) Not enrolled in high school	(4) Enrolled in academic programs
Ineligible×Post	-0.595***	0.559***	0.058***	-0.017
	(0.025)	(0.027)	(0.019)	(0.013)
Ineligible	-0.098***	0.062***	0.030***	-0.001
	(0.012)	(0.008)	(0.005)	(0.010)
Post	0.155***	0.012*	-0.000	-0.153***
	(0.010)	(0.007)	(0.004)	(0.006)
Observations	10,795	10,795	10,795	10,795
R^2	0.072	0.126	0.010	0.033
Mean dep. var.	0.671	0.088	0.034	0.186

Notes. Dependent variables are given by the column headers. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Perhaps less intuitive is the fact that the enrollment in vocational programs increases somewhat even for the control group. This is not too surprising, though, as these are the students who in the absence of the reform would have been eligible for any national high school program. The stricter requirements for academic tracks force some students with true preference for academic tracks into vocational programs.

A small fraction of students below the new eligibility threshold respond to the reform by not enrolling in high school at all immediately after compulsory school graduation. The probability increases by 5.8 percentage points among the affected students. I show later in the paper that the effect dominates among boys. Appendix Table A4 shows that the effect is temporary: most of the students who do not enroll immediately, do it one year later. In line with that pattern, the fraction of students who stay in high school for a fourth or fifth year increases (12.5 and 4.1 percentage points, respectively; see Appendix Table A5). The changes reflect (partly) a mechanical effect on the prolongation of studies due to the need to spend additional time in preparatory programs before transferring to any standard high school program. Appendix Table A6 shows that the probability of graduating from high school three years after compulsory school exit declines by 13.8 percentage points, whereas the probability of graduating four years later increases by 9.4 percentage points. The probability of graduating at any time within five years declines, but the effect is statistically insignificant (see Appendix Table A7).

2.5.2 Inflow into the disability insurance system

This section answers the central research questions of the role of limited access to education on low-skilled youths' entry into the DI system. The main results are summarized in columns 1 and 2 of Table 4 and illustrated in Figure 5. The results show that the immediate inflow into the DI system increases by 5.1 percentage points among the students who do not meet the stricter eligibility requirements. Considering that on average 4.4% of the pre-reform cohorts were awarded the DI benefit at that time, the effect translates into more than a two-fold increase in the inflow of marginal students into the DI system.[24]

Table 4. *Effect of limited access to vocational high school programs on DI take-up*

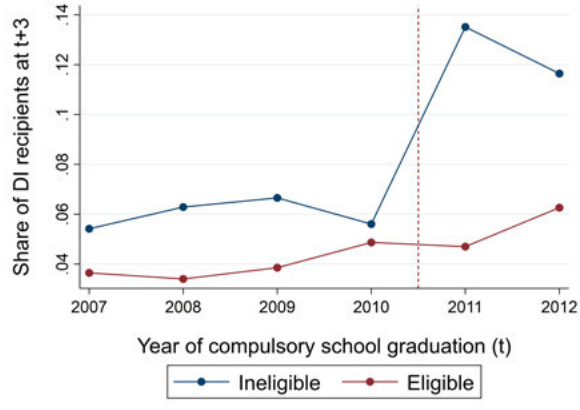
	(1) DI take-up at $t+3$	(2) DI take-up at $t+3$	(3) DI take-up at $t+4$
Ineligible×Post	0.051***	0.052***	0.068***
	(0.020)	(0.020)	(0.023)
Ineligible	0.021***	0.019***	0.038***
	(0.006)	(0.006)	(0.007)
Post	0.015***	0.017***	0.022***
	(0.006)	(0.006)	(0.007)
Observations	10,795	10,795	10,795
R^2	0.006	0.019	0.009
Mean dep. var.	0.044	0.044	0.068
Controls	No	Yes	No

Notes. Dependent variables are given by the column headers. The results in column 2 are adjusted for gender, migration status and parental characteristics (DI take-up, income and education). As the data on national tests cannot be linked to the rest, the data are not used as controls. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

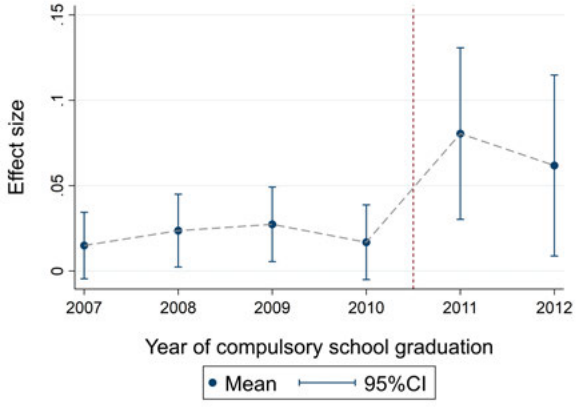
[24]The effect is much larger than the estimates reported by Poterba et al. (2017) who show that men with a high school degree are 0.23 percentage points less likely, and women 0.34 percentage points less likely to participate in DI programs than people without a high school degree. Besides the fact that their selection-on-observables identification strategy differs from the one used in this paper, the studies focus on very different samples and margins of interest. Poterba et al. (2017) study the data on all 50–62-year-olds. I focus on much younger cohorts at the entry margin of (vocational) high school studies. Due to the nature of the new eligibility standards I focus on students from the lower end of the ability distribution, where the effects are thought to be larger.

Figure 5. Average DI take-up rate by eligibility status (Panel A) and the effect of not meeting the stricter high school eligibility standards on DI take-up (Panel B), by cohorts

(a) Average DI take-up at $t+3$ among the eligible and ineligible students



(b) Effect of not meeting the stricter high school eligibility standards on DI take-up



Notes. The vertical dashed lines mark the introduction of the stricter high school eligibility requirements. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011.

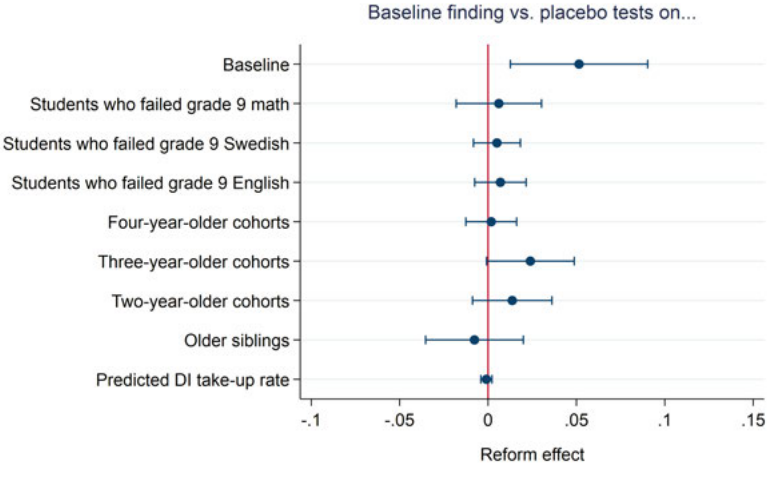
Adding covariates to the analysis changes the point estimates just marginally, which confirms again that the predetermined background characteristics are balanced across the groups. As the covariates increase the precision of the estimates very little, the estimates from column 1 are treated as the main findings. In the following tables I refer to these as the baseline results.

The results in column 3 estimate the effect of the stricter high school eligibility standards on youth DI take-up at the age of 20 (i.e. four years after compulsory school graduation). The point estimate of 6.8 percentage points suggests that the effect remains large over the short follow-up period. Together with the high persistence of youth DI participation the results point at the potentially long-lasting effects of the reform on the affected students. It further underlines the importance of providing low-skilled youth with access to vocational education for avoiding both short- and long-term consequences.

2.5.3 Placebo tests

The baseline findings are supported by a set of placebo tests which are summarized in Table 5 and illustrated in Figure 6. In the analysis presented in columns 2–4 of Table 5, I rely on the pre-reform eligibility requirement. Even if students had obtained at least five additional passing grades from grade nine but failed one of the key subjects, i.e. math, English or Swedish, they did not qualify for high school studies in vocational programs regardless of cohort. Thus, I should not see any difference in DI take-up at $t+3$ around the threshold of five additional passing grades when focusing on the students who fail any of these key subjects. The results are very convincing, indicating that GY2011 did not affect the DI enrollment among any of the groups.

Figure 6. The baseline estimate in comparison with the placebo estimates



The placebo tests in columns 5–7 investigate whether the sudden increase in the ineligible students' DI enrollment rate in 2014 and 2015 may reflect a more general increase in the DI awards during these years rather than the true effect of GY2011. To study that, I create three new samples of students who graduated from compulsory school in 2005–2010 (two-year-older cohorts, column 7), 2004–2009 (three-year-older cohorts, column 6) and 2003–2008 (four-year-older cohorts, column 5), which I further split into comparable groups by eligibility status.[25] As neither the group of eligible nor ineligible students in any of these cohorts were affected by GY2011, a similar effect as observed for my main sample would call the main results in question. Reassuringly, the results show that the ineligible students did not experience any significant increase in the DI take-up rate (measured at $t + 5$ and $t + 7$, respectively) neither in the two- nor four-year-older cohorts. The placebo test run on the sample of compulsory school graduates of 2004–2009 (the three-year-older cohorts, DI take-up is measured at $t + 6$) is the only placebo test out of the total of eight which indicates that there might have been some other factors in 2014 and 2015 that could have affected the increase in the DI enrollment.

To understand whether the students who did not meet the stricter high school eligibility standards after the reform come from families that are more likely to use DI than similar students before the reform, column 8 studies the DI take-up rate among older siblings of the people in the main sample. A sudden increase in the DI enrollment in 2014 and 2015 among the older siblings would indicate that my main findings originate from changes in students' background rather than the effect of GY2011. The results confirm that the baseline findings reflect the response to GY2011.

Lastly, column 9 of Table 5 shows the results that were already depicted in Figure 3. In that placebo test, I use the predicted DI take-up rate as an outcome instead of the actual one.[26] The results are in line with Figure 3 showing no effect of the limited access to vocational high school studies on the affected students' predicted DI take-up. This and all of the other placebo tests, apart from one, lend support to the baseline findings.

[25] I.e. I only focus on people from the cohorts who graduated from compulsory school with passing grades in math, English and Swedish. Those with 1–4 additional passing grades are defined as ineligible students and those with 5–8 additional passing grades as eligible students.

[26] The DI take-up rate is predicted as a function of gender, migration status, compulsory school GPA, parents' DI take-up, sum of parents' income, their employment status and education. National test scores cannot be used in the analysis due to different data identifiers.

Table 5. *Summary of the placebo tests*

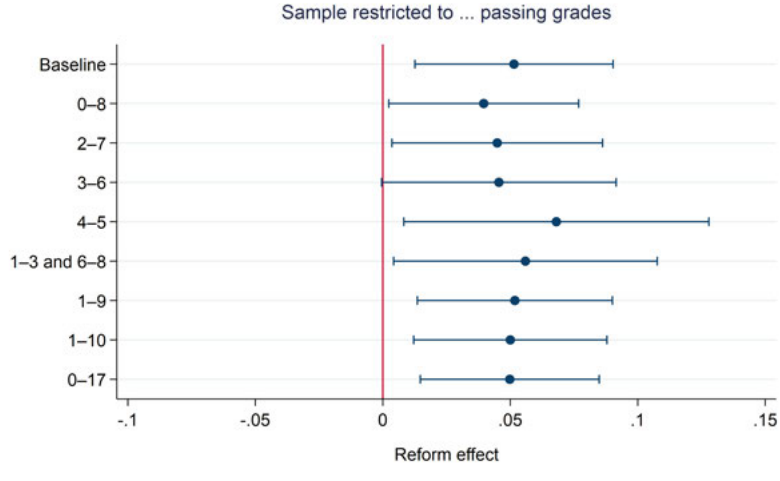
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Failed grade nine ...			Older cohorts			Older	Predicted
		Math	*Swedish*	*English*	*+4 yrs*	*+3 yrs*	*+2 yrs*	siblings	DI take-up
Ineligible×Post	0.051***	0.005	0.007	0.002	0.006	0.024*	0.014	-0.008	-0.001
	(0.020)	(0.007)	(0.007)	(0.007)	(0.012)	(0.013)	(0.011)	(0.014)	(0.002)
Ineligible	0.021***	0.002	0.001	0.000	0.039***	0.035***	0.032***	0.014**	0.015***
	(0.006)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)	(0.007)	(0.006)	(0.001)
Post	0.015***	0.021***	0.017***	0.016***	0.019***	0.016***	0.020***	0.009	-0.002***
	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.001)
Observations	10,795	28,242	23,255	22,802	13,788	13,383	15,113	11,042	10,795
R^2	0.006	0.002	0.002	0.001	0.007	0.008	0.006	0.001	0.064
Mean dep. var.	0.044	0.059	0.058	0.057	0.058	0.056	0.062	0.044	0.050

Notes. Dependent variable is DI take-up three years after compulsory school graduation for columns 1–8 and predicted DI take-up three years after compulsory school graduation for column 9. Models in different columns are estimated on different samples of students (columns 2–8) or using another outcome variable (column 9). Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

2.5.4 Additional robustness tests

Table 6 and Figure 7 present the results of additional robustness tests to confirm that the sample construction is not driving the findings. In column 2 of the table I estimate the effect of limited access to high school education on early DI take-up on the sample of students who obtained 0–8 additional passing grades from grade nine. The main sample did not include the people who met the persistent eligibility requirement only (i.e. those with 0 additional passing grades) because it is not completely clear whether these students might have been enrolled in special schools. In columns 3–5, the main sample is restricted further by narrowing the "bandwidth" of the additional passing grades in order to better capture the unobservable characteristics that could bias the estimate. The students with only one additional passing grade may arguably be rather different from those with eight additional passing grades. As such, limiting the sample as done in the baseline analysis may not capture the unobservable characteristics well enough. In column 3, I limit the sample to 2–7, in column 4 to 3–6 and in column 5 to 4–5 additional passing grades. In the latter case, the students may be considered much more similar to each other than in the main analysis. The results show that the reform effect on DI take-up has very similar magnitude in all of the cases. Due to loss of observations, the estimates in columns 3–5 become less precise.

Figure 7. Robustness of the main finding to different sample restrictions



In column 6, the students just around the threshold of becoming eligible for vocational high school studies (i.e. students with four and five additional passing grades) are excluded from the sample. The incentives for

Table 6. *Robustness tests on different samples*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Sample restrictions based on the number of additional passing grades								
	Baseline 1–8	0–8	2–7	3–6	4–5	1–3 and 6–8	1–9	1–10	0–17
Ineligible×Post	0.051***	0.040**	0.045**	0.046*	0.068**	0.056**	0.052***	0.050***	0.049***
	(0.020)	(0.019)	(0.021)	(0.023)	(0.031)	(0.026)	(0.020)	(0.019)	(0.018)
Ineligible	0.021***	0.030***	0.010	0.002	-0.018*	0.034***	0.023***	0.028***	0.064***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.011)	(0.007)	(0.006)	(0.005)	(0.005)
Post	0.015***	0.015***	0.014**	0.002	-0.019	0.022***	0.015***	0.017***	0.004***
	(0.006)	(0.006)	(0.007)	(0.009)	(0.014)	(0.006)	(0.004)	(0.003)	(0.000)
Observations	10,795	11,101	7,442	4,703	2,254	8,541	15,168	21,146	606,326
R^2	0.006	0.007	0.004	0.002	0.003	0.009	0.005	0.006	0.004
Mean dep. var.	0.044	0.047	0.048	0.053	0.056	0.041	0.041	0.036	0.006

Notes. Dependent variable is DI take-up three years after compulsory school graduation. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

manipulating the system, e.g. by being graded more leniently or asking teachers to provide them with a missing passing grade, were presumably the highest for these students. Excluding the cases at the margin does not affect the baseline findings much.

In columns 7 and 8, the upper threshold is slightly increased to control for the potential bias of the baseline estimate stemming from a potentially negatively selected control group. If the control group were in fact negatively selected, my baseline findings could potentially underestimate the true effect. Including students with nine or 10 additional passing grades enables me to capture the marginal students who, by exerting extra effort, managed to obtain 1–2 additional passing grades in order to qualify for academic high school studies. The estimates are very similar to the baseline, which is a good indication that negative selection is not a concern. Column 9 finally expands the sample to full student population eligible for high school studies under the old requirements. The effect of not meeting the stricter eligibility requirements on DI take-up at the age of 19 estimated on the broad sample is still very close to the baseline estimate.

2.5.5 Inference

Even though the DiD estimation technique enables me to obtain the effect of GY2011 on youth DI take-up, there is a substantial literature pointing at its weaknesses when it comes to the estimation of unbiased standard errors (Bertrand et al., 2004; Cameron and Miller, 2015; Abadie et al., 2010; Brewer et al., 2013). The two main concerns that have caught attention are grouped error terms and the serial correlation common for the data used in the DiD setup. The first of the issues often arises as the unit of observation differs from the level of variation. The serial correlation tends to be especially severe as the reforms that are often used in the DiD framework (such as GY2011) tend to represent an absorbing state. The reform indicator contains the value of zero for a set of units until the reform is enforced, and switches to the value of one from that point onward. For the control group, the reform indicator is zero throughout the sample period.

In what follows, I study the robustness of the main inference by implementing different standard error corrections. Columns 2 and 3 in Table 7 first deal with the issue of potentially grouped errors terms and report two different types of clustered standard errors. In column 2, clustering is implemented at the level of cohort $\times$ number of passing grades, in column 3 only 16 clusters (clustered at the level of post-reform dummy $\times$ number of additional passing grades) are used. In both of the cases, the clustered

Table 7. *Standard error corrections*

	(1) Baseline	(2) Clustered s.e	(3) Clustered s.e.	(4) Bootstrapped s.e.	(5) Collapsed data
Ineligible×Post	0.051***	0.051**	0.051**	0.051**	0.051**
	(0.020)	(0.024)	(0.021)	(0.025)	(0.017)
Ineligible	0.021***	0.021***	0.021**	0.021*	0.021*
	(0.006)	(0.006)	(0.009)	(0.011)	(0.011)
Post	0.015***	0.015*	0.015*	0.015*	0.015
	(0.006)	(0.009)	(0.008)	(0.009)	(0.008)
Observations	10,795	10,795	10,795	10,795	16
R^2	0.006	0.006	0.006	0.006	0.602
Mean dep. var.	0.044	0.044	0.044	0.044	0.044
# of clusters	NA	48	16	16	8
# of bootstrap reps.	NA	NA	NA	1000	NA

Notes. Dependent variable is DI take-up three years after compulsory school graduation. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

standard errors prove to be slightly larger than the baseline ones. In spite of this, the main inference holds.[27]

To correct for the potential issue of serial correlation I rely first on the block bootstrap method suggested by Bertrand et al. (2004) (column 4, Table 7) which leads to the largest standard errors. In column 5, I ignore the time-series information altogether by collapsing the data at the pre- and post-reform level. The standard error estimates found on the collapsed data also support the inference drawn in Section 2.5.2.

2.5.6 Effects on labor market outcomes

Existing studies and quality reports of the DI awards indicate that not all of the beneficiaries may be truly disabled (ISF, 2011; Försäkringskassan, 2016, 2018a). While the case managers making the DI award decisions should always investigate whether the need for prolonged schooling is strongly linked to a disability or not, it has been proved to be difficult to assess (Försäkringskassan, 2018b). The literature on the substitutability between the unemployment insurance (UI) system and the DI system further suggests that at least part of the reform effect on adolescents' inflow into the DI system may occur as a response to an adverse shock to one's employment prospects (Black et al., 2002; Rege et al., 2009;

[27] Application of the wild cluster bootstrap-t method suggested by Cameron and Miller (2015) for dealing with small number of clusters is undermined by the binary dependent variable.

Bratsberg et al., 2010). The level of the UI benefit available for young people with no previous working experience being considerably below the level of the DI benefit underlines the potential concern about the role of declined economic prospects on DI take-up even further.

GY2011 presents a negative shock to the affected students' chances in the labor market by negatively affecting their educational outcomes. The reform effects on youth high school enrollment (see Table 3) showed that the students affected by the reform were much more likely to start high school studies in introductory programs that are described by much lower graduation rate. About half of the students who started in introductory programs in 2011 dropped out of high school during the following three years, many changed between the introductory programs, and only a small share graduated from national programs in four years (Skolverket, 2014b). My results indicate a negative, albeit statistically insignificant, effect of the probability of graduating from high school within five years.

To understand whether economic incentives may have contributed to the increase in DI take-up as a response to the reform, I study the effects of GY2011 on several labor market outcomes (see Table 8, as well as Appendix Tables A10 and A11). I find a strong negative effect of failing to meet the new eligibility requirements on students' employment probability, the effect being as large as 10.6 percentage points (36% of mean employment). This result is in line with the fact that (at least) three-year high school education is important for increasing competitiveness in the labor market (see e.g. SOU, 2016). My results show further that the probability of being registered as unemployed three years after compulsory school graduation declined among the affected students as well (by 5.2 percentage points, 11.8% of mean unemployment). Conditional on being unemployed, the length of unemployment remains at the same time unaffected. The declined probability of unemployment together with the large drop in employment probability indicates a sizable decline in the labor supply of the students ineligible for vocational high school studies due to the new rules.

The reform decreased annual labor earnings (measured three years after compulsory school graduation) of the students below the new eligibility threshold as well. The affected students' earnings fell by 37%.[28] The income variables measured four years after compulsory school graduation (see Table A11) show, similarly to the main results, that the effect remains large during the short follow-up period.

The effects on labor market outcomes, while in line with the literature (Black et al., 2002; Rege et al., 2009; Bratsberg et al., 2010), are estimated

[28]The results on labor earnings shown in tables 8, A10 and A11 include observations with zero earnings. Using log earnings and, hence, conditioning on positive earnings leads to estimates of very similar magnitude; -0.391 with robust standard error of 0.137 at $t+3$ and -0.351 with robust standard error of 0.137 at $t+4$.

Table 8. *Effects of GY2011 on various labor market outcomes at $t+3$*

	(1) P(Employment)	(2) P(Unemployment)	(3) Length of unemployment	(4) Labor earnings	(5) Disposable income
Ineligible×Post	-0.106***	-0.052*	6.303	-10.159***	-8.069***
	(0.027)	(0.031)	(11.089)	(2.789)	(2.589)
Ineligible	-0.043***	-0.009	12.223***	-4.774***	-0.832
	(0.011)	(0.013)	(3.815)	(1.055)	(0.983)
Post	0.060***	-0.055***	-5.966*	8.173***	6.419***
	(0.012)	(0.012)	(3.570)	(1.253)	(1.077)
Observations	10,795	10,795	4,628	10,688	10,578
R^2	0.006	0.003	0.004	0.009	0.005
Mean dep. var.	0.293	0.442	147.251	27.583	45.060

Notes. Dependent variables are given by the column headers. Employment is defined as having worked for at least one hour a week in November. Registered unemployment is defined by being registered as open unemployed or participating in some labor market program in a particular year. Length of unemployment stands for the total number of days registered in the two labor market schemes. All negative values of labor earnings and disposable income are treated as missing. Both of the variables (in thousands of Swedish kronor) are trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

in a short time window. More post-reform data are needed in order to study whether the effects persist, or reflect the increased risk of prolonged schooling among the affected students.

2.5.7 Different paths into the disability insurance system

The effect of GY2011 on youth DI take-up is evaluated in a context where young people could apply for the DI benefit for either declined work capacity or for completing their studies in case of permanent disability hindering their on-time graduation. While context-specific, the DI take-up for prolonged schooling is very similar to the DI take-up for declined work capacity as it tends to lead to a long benefit dependence. On average, about one third of the youth awarded the DI for prolonged schooling remain in the DI system 10 years later. At the same time, it serves as an alternative path for youth into the DI system and presents, thus, a channel for controlling the inflow.

Table 9 focuses more closely on DI take-up four years after compulsory school graduation. At $t+4$, I can further analyze the effect of GY2011 on DI take-up among those who did not graduate from high school in three years and were not enrolled in high school at $t+4$ (high school dropouts,

see column 2 of Table 9), and the students who did not graduate from high school at $t+3$, but were still enrolled in school at $t+4$ (students with prolonged schooling, see column 3 of Table 9). I use the groups of high school dropouts and students with prolonged schooling as proxies for the types of DI awards. I assume that the dropouts were more likely to be awarded the benefit for declined work capacity and students still in school at $t+4$ for prolonged schooling.

Table 9. *Underlying reasons for the DI participation at $t+4$*

	(1) DI take-up at $t+4$	(2) Dropout × DI	(3) Prolonged schooling × DI	(4) Graduate at $t+3$ × DI
Ineligible×Post	0.068***	0.026	0.043**	-0.001
	(0.023)	(0.016)	(0.018)	(0.007)
Ineligible	0.038***	0.013***	0.023***	0.002
	(0.007)	(0.005)	(0.005)	(0.002)
Post	0.022***	0.011**	0.008*	0.004
	(0.007)	(0.005)	(0.004)	(0.003)
Observations	10,795	10,795	10,795	10,795
R^2	0.009	0.003	0.007	0.000
Mean dep. var.	0.068	0.029	0.031	0.008

Notes. Dependent variables are given by the column headers. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Dropouts are the students who did not graduate from high school in three years and were not enrolled in high school at $t+4$. Prolonged schooling is an indicator for the students who did not graduate from high school at $t+3$, but were enrolled in school at $t+4$. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Virtually none of the students who graduated from high school at $t+3$ were on DI a year later, but GY2011 increased DI take-up for both the dropouts and the students with prolonged schooling. Although the point estimate is slightly larger and statistically significant only for DI take-up among the students still in high school at $t+4$, the estimates do not differ statistically significantly from each other. The findings suggest that the exclusion of low-skilled youth from vocational high school studies in the presence of the specific path into the DI system may have contributed additionally to the increase in the youth DI take-up. However, it is worth noting that the choice of applying for DI for prolonged schooling is endogenous to the system. Without the additional channel, the youth awarded the DI benefit for prolonged schooling might have entered the system for declined work capacity. With that caveat in mind

the findings emphasize on the importance of taking joint efficiency into consideration when designing different systems.

2.5.8 Gender heterogeneity

Lastly, the effects of restricted access to education are studied by gender. Table 10 shows that a large share of both boys and girls who did not meet the stricter high school eligibility requirements were excluded from vocational high school programs due to the reform. No change appeared in the high school enrollment rate for the affected girls. Most of them started their high school studies in introductory programs.

While starting high school studies in an introductory program is a negative outcome as it leads with much lower probability to high school graduation than enrollment in vocational programs, the negative effects of the reform on boys seem to be more immediate. Boys are significantly more prone to opt out of school as a response to the reform. On average, 3.4% of all the boys in the main sample never enrolled in high school directly after compulsory school graduation during the pre-reform period, but for the boys excluded from regular high school programs after the reform, the share increased by 9.4 percentage points. The effect is at the same time temporary: most of the boys enroll in high school one year later. The share of students enrolled in high school for four years increased by the same magnitude for both boys and girls, whereas the high school dropout rate was unaffected by the reform for both groups (see Appendix Table A7).

Table 11 summarizes the findings of the reform effects on different economic outcomes by gender. The table suggests that the reform affected boys more severely than girls. The employment probability fell by 14.1 percentage points (48% of the mean employment before the reform) for boys. The decline in unemployment was 4.4 percentage points (10% of average unemployment before the reform). Although the latter estimate is imprecise, the findings suggest a large decline in the labor supply of the affected boys. The finding goes in hand with the 7.2 percentage point increase in the probability of being on DI at the age of 19 for boys. In accordance with the results, boys' earnings were also more affected by the reform.[29] The effects on boys' outcomes at $t+4$ (see Appendix Table A13) point at persistently large effects during the short follow-up period.

While having the same signs, albeit of lower magnitude, the reform effects on girls' outcomes are not statistically significant. Due to the noisy estimates, the differences between the outcomes of boys and girls are also insignificant. Despite the fact, the results suggest that boys

[29]The effect of the reform on log earnings is 49.4% (s.e. 0.187) for boys and 27.9% (s.e. 0.198) for girls.

Table 10. *Effects of GY2011 on various student outcomes, by gender*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Enrolled in vocational programs			Enter introductory programs			Not enrolled in high school		
	Girls	Boys	Diff.	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	-0.610***	-0.584***	-0.026	0.636***	0.512***	0.124**	-0.001	0.094***	-0.094***
	(0.025)	(0.026)	(0.036)	(0.040)	(0.036)	(0.054)	(0.022)	(0.015)	(0.027)
Ineligible	-0.083***	-0.109***		0.056***	0.067***		0.031***	0.029***	
	(0.019)	(0.016)		(0.012)	(0.011)		(0.008)	(0.007)	
Post	0.137***	0.165***		0.025**	0.004		0.008	-0.005	
	(0.016)	(0.012)		(0.011)	(0.009)		(0.007)	(0.005)	
Observations	4,378	6,417		4,378	6,417		4,378	6,417	
R^2	0.061	0.079		0.157	0.109		0.005	0.016	
Mean dep. var.	0.666	0.675		0.077	0.096		0.034	0.034	

Notes. Dependent variables are given by the column headers. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11. *Effects of GY2011 on various outcomes, by gender*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	DI take-up at $t+3$			Labor earnings		
	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	0.017	0.072***	-0.055	-4.733	-13.647***	8.914
	(0.030)	(0.026)	(0.039)	(4.917)	(3.338)	(5.943)
Ineligible	0.021**	0.020***		-5.528***	-4.026***	
	(0.009)	(0.007)		(1.437)	(1.496)	
Post	0.014	0.017**		12.733***	5.208***	
	(0.009)	(0.007)		(2.016)	(1.603)	
Observations	4,378	6,417		4,348	6,340	
R^2	0.003	0.009		0.019	0.006	
Mean dep. var.	0.051	0.040		25.130	29.323	
Panel B.	P(Employment)			P(Unemployment)		
	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	-0.050	-0.141***	0.091	-0.067	-0.044	-0.023
	(0.048)	(0.033)	(0.058)	(0.048)	(0.039)	(0.063)
Ineligible	-0.054***	-0.035**		-0.005	-0.010	
	(0.017)	(0.015)		(0.019)	(0.017)	
Post	0.095***	0.040***		-0.069***	-0.047***	
	(0.020)	(0.015)		(0.020)	(0.016)	
Observations	4,378	6,417		4,378	6,417	
R^2	0.010	0.005		0.005	0.002	
Mean dep. var.	0.288	0.296		0.433	0.449	

Notes. Dependent variables are given by the column headers. Employment is defined as having worked for at least one hour a week in November. Registered unemployment is defined by being registered as open unemployed or participating in some labor market program in a particular year. All negative values of labor earnings are treated as missing and the variables (in thousands of Swedish kronor) is trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

were more affected by the reform both in the short-run through worsened employment prospects and (potentially) in the long-run through increased entry into the DI system which often leads to long benefit dependence. However, as noted above, more data are needed to study the extent to which the effects may be explained by prolonged schooling.

2.6 Conclusions

Although many studies point at the association between low level of education and enrollment in DI system, the paper is the first to estimate the causal effect of changes in the education system on DI participation. For that purpose, the effects of a Swedish high school reform that raised the high school eligibility requirements in 2011 were evaluated. Better understanding of the interactions between DI systems and educational institutions may provide policy makers with tools for controlling the inflow of youth into the DI system.

The findings first confirm that the reform prevented a set of low-skilled students from entering standard high school programs. Those with worse final grades from compulsory school were, as stipulated by the new rules, remarkably less likely to continue their studies in vocational high school programs than students meeting the new eligibility requirements. Instead, the students below the new eligibility threshold became more likely to start their high school studies in introductory programs that are characterized by much lower high school graduation rates. Boys responded relatively more to the reform by not enrolling in high school at all immediately after compulsory school graduation, but the effect is temporary; most of the boys enroll one year later.

The main results of the paper show that limited access to education affects the inflow of low-skilled youth into the DI system. The probability of being awarded the DI benefit more than doubled among the students who did not meet the stricter eligibility standards after the reform. Supplementary analyses suggest that the short-term labor supply of the affected students declined considerably due to the reform. The students left out of the vocational high school programs due to the stricter eligibility requirements are less likely to be both employed and registered unemployed. Their annual earnings are significantly harmed as well. Since most of the affected students prolong their time in education due to the reform, it is important to follow up their outcomes in the long-run to see if these effects persist, or are explained by prolonged schooling.

All in all, the findings show that excluding low-skilled youth from vocational high school studies leads to potentially long-lasting detrimental consequences. Importantly, the effects are likely to go beyond the short-term effects on employment outcomes. Unlike previous studies, the paper highlights the increased probability of entering the DI system. While the results are analyzed within a short time-horizon, descriptive findings show that the early DI participation is highly persistent. Together with the auxiliary findings showing that the effect remains large during the short follow-up period, the immediate increase in DI take-up is likely to lead to a long-term benefit dependence. The adverse shock to the employment prospects may, as suggested by earlier studies, account for parts

of the inflow into the DI system. Alternatively, the interaction between the institutional setup that allows for entry into the DI system for prolonged schooling and the reform that mechanically increased low-skilled students' time in education is likely accountable for (at least) parts of the change. Taken together, the paper shows unintended and potentially long-term effects of limiting low-skilled youths' access to vocational high school studies, and the findings emphasize on the importance of taking joint efficiency into consideration when designing different systems.

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Appendix

Table A1. *Average DI take-up three years after compulsory school graduation among students enrolled in different high school programs or not enrolled in high school at all, full student population*

	(1) Mean DI take-up	(2) S.e.	(3) N
Academic	0.006	0.000	409,169
Vocational	0.008	0.000	276,262
Introductory	0.056	0.001	69,485
No HS	0.092	0.002	13,480
Some other track	0.006	0.001	6,920

Table A2. *Fraction of compulsory school graduates not meeting the persistent eligibility requirement, by cohort*

Compulsory school graduation cohort	Fraction of graduates	Standard error
2007	0.117	(0.001)
2008	0.116	(0.001)
2009	0.117	(0.001)
2010	0.123	(0.001)
2011	0.124	(0.001)
2012	0.125	(0.001)

Table A3. *Predetermined background characteristics in the sample and full student population*

	(1)	(2)	(3)	(4)
	Sample		Population	
	Mean	S.e.	Mean	S.e.
Students' characteristics:				
Female	0.406	(0.005)	0.488	(0.001)
Not born in Sweden	0.076	(0.003)	0.077	(0.000)
Standardized result of national test in math	-0.413	(0.008)	0.002	(0.001)
Standardized result of national test in English	-0.295	(0.006)	0.003	(0.001)
Standardized result of national test in Swedish	-0.434	(0.007)	0.002	(0.001)
National test score in math missing	0.191	(0.004)	0.078	(0.000)
National test score in English missing	0.146	(0.003)	0.066	(0.000)
National test score in Swedish missing	0.144	(0.003)	0.058	(0.000)
Parents' characteristics:				
Father on DI	0.088	(0.003)	0.048	(0.000)
Mother on DI	0.142	(0.003)	0.078	(0.000)
At least one parent on DI	0.208	(0.004)	0.117	(0.000)
Father not employed	0.162	(0.004)	0.088	(0.000)
Mother not employed	0.197	(0.004)	0.105	(0.000)
Sum of parents' income (in hundreds SEK)	3,754.2	(26.3)	5,410.1	(4.8)
Mother – less than high school	0.209	(0.004)	0.109	(0.000)
Mother – high school or some post-secondary	0.666	(0.005)	0.649	(0.001)
Mother – tertiary education	0.098	(0.003)	0.216	(0.000)
Father – less than high school	0.241	(0.004)	0.152	(0.000)
Father – high school or some post-secondary	0.618	(0.005)	0.634	(0.001)
Father – tertiary education	0.060	(0.002)	0.157	(0.000)
Observations	10,795		689,069	

Table A4. *Effects on probability of starting in high school at different points in time*

	(1) All	(2) Boys	(3) Girls
Panel A.	*First enrolled at t+0*		
Ineligible×Post	-0.058***	-0.094***	0.001
	(0.019)	(0.026)	(0.025)
Ineligible	-0.030***	-0.029***	-0.031***
	(0.005)	(0.007)	(0.008)
Post	0.000	0.005	-0.008
	(0.004)	(0.005)	(0.007)
Observations	10,795	6,417	4,378
R^2	0.010	0.016	0.005
Mean of dependent variable	0.966	0.966	0.966
Panel B.	*First enrolled at t+1*		
Ineligible×Post	0.047***	0.075***	0.000
	(0.015)	(0.021)	(0.018)
Ineligible	0.009**	0.008	0.011*
	(0.004)	(0.005)	(0.006)
Post	-0.000	-0.005*	0.009
	(0.003)	(0.003)	(0.006)
Observations	10,795	6,417	4,378
R^2	0.006	0.012	0.002
Mean of dependent variable	0.016	0.016	0.016
Panel C.	*First enrolled at t+2*		
Ineligible×Post	-0.000	-0.001	0.002
	(0.005)	(0.005)	(0.009)
Ineligible	0.005***	0.005**	0.004
	(0.002)	(0.002)	(0.003)
Post	0.000	-0.000	0.001
	(0.001)	(0.001)	(0.002)
Observations	10,795	6,417	4,378
R^2	0.002	0.002	0.001
Mean of dependent variable	0.002	0.002	0.003
Panel D.	*Never enrolled in high school*		
Ineligible×Post	0.008	0.019	-0.010
	(0.011)	(0.016)	(0.013)
Ineligible	0.017***	0.017***	0.016***
	(0.004)	(0.005)	(0.006)
Post	0.000	0.001	-0.001
	(0.003)	(0.004)	(0.004)
Observations	10,795	6,417	4,378
R^2	0.004	0.005	0.003
Mean of dependent variable	0.016	0.015	0.016

Notes. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean of dependent variable indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. *Effects on high school enrollment over time*

	(1)	(2)	(3)
Panel A.		*In school at t+0*	
	All	Boys	Girls
Ineligible×Post	-0.058***	-0.094***	0.001
	(0.019)	(0.026)	(0.025)
Ineligible	-0.030***	-0.029***	-0.031***
	(0.005)	(0.007)	(0.008)
Post	0.000	0.005	-0.008
	(0.004)	(0.005)	(0.007)
Observations	10,795	6,417	4,378
R^2	0.010	0.016	0.005
Mean of dependent variable	0.966	0.966	0.966
Panel B.		*In school at t+1*	
	All	Boys	Girls
Ineligible×Post	0.000	-0.025	0.036
	(0.020)	(0.026)	(0.033)
Ineligible	-0.047***	-0.032***	-0.067***
	(0.007)	(0.009)	(0.012)
Post	-0.018***	-0.009	-0.032***
	(0.007)	(0.008)	(0.011)
Observations	10,795	6,417	4,378
R^2	0.006	0.004	0.011
Mean of dependent variable	0.929	0.933	0.924
Panel C.		*In school at t+2*	
	All	Boys	Girls
Ineligible×Post	0.002	0.022	-0.031
	(0.028)	(0.035)	(0.047)
Ineligible	-0.089***	-0.090***	-0.086***
	(0.010)	(0.014)	(0.016)
Post	-0.046***	-0.048***	-0.044***
	(0.010)	(0.012)	(0.016)
Observations	10,795	6,417	4,378
R^2	0.011	0.010	0.011
Mean of dependent variable	0.830	0.835	0.823

Table A5. *Effects on high school enrollment over time (continues)*

	(1)	(2)	(3)
Panel D.		*In school at t+3*	
	All	Boys	Girls
Ineligible×Post	0.125***	0.127***	0.125**
	(0.031)	(0.039)	(0.050)
Ineligible	0.003	-0.011	0.023
	(0.012)	(0.016)	(0.018)
Post	-0.075***	-0.081***	-0.065***
	(0.011)	(0.014)	(0.018)
Observations	10,795	6,417	4,378
R^2	0.005	0.005	0.005
Mean of dependent variable	0.328	0.333	0.321
Panel E.		*In school in t+4*	
	All	Boys	Girls
Ineligible×Post	0.041**	0.033	0.056*
	(0.018)	(0.022)	(0.032)
Ineligible	-0.000	-0.003	0.003
	(0.007)	(0.009)	(0.010)
Post	-0.023***	-0.024***	-0.022**
	(0.006)	(0.008)	(0.010)
Observations	10,795	6,417	4,378
R^2	0.001	0.001	0.002
Mean of dependent variable	0.079	0.080	0.077

Notes. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Enrollment at $t + 3$ means that a student is enrolled in high school for fourth year. Mean of dependent variable indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6. *Effects on graduation probability over time*

	(1)	(2)	(3)
Panel A.		*Graduate at t+3*	
	All	Boys	Girls
Ineligible×Post	-0.138***	-0.139***	-0.140***
	(0.026)	(0.034)	(0.041)
Ineligible	-0.113***	-0.108***	-0.120***
	(0.011)	(0.015)	(0.017)
Post	0.053***	0.054***	0.051**
	(0.012)	(0.016)	(0.020)
Observations	10,795	6,417	4,378
R^2	0.017	0.016	0.018
Mean of dependent variable	0.346	0.350	0.340
Panel B.		*Graduate at t+4*	
	All	Boys	Girls
Ineligible×Post	0.094***	0.087***	0.105***
	(0.024)	(0.031)	(0.041)
Ineligible	-0.031***	-0.029**	-0.033**
	(0.009)	(0.012)	(0.014)
Post	-0.043***	-0.047***	-0.037***
	(0.009)	(0.011)	(0.014)
Observations	10,795	6,417	4,378
R^2	0.003	0.003	0.003
Mean of dependent variable	0.162	0.165	0.159
Panel C.		*Graduate at t+5*	
	All	Boys	Girls
Ineligible×Post	0.015	0.007	0.029
	(0.014)	(0.016)	(0.025)
Ineligible	-0.002	-0.002	-0.002
	(0.005)	(0.007)	(0.008)
Post	-0.012**	-0.015**	-0.007
	(0.005)	(0.006)	(0.008)
Observations	10,795	6,417	4,378
R^2	0.001	0.001	0.000
Mean of dependent variable	0.047	0.048	0.045

Notes. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean of dependent variable indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7. *Effects on other educational outcomes*

	(1)	(2)	(3)
Panel A.	*Probability of graduating within five years*		
	All	Boys	Girls
Ineligible×Post	-0.022	-0.038	0.002
	(0.032)	(0.040)	(0.052)
Ineligible	-0.147***	-0.138***	-0.157***
	(0.013)	(0.017)	(0.019)
Post	0.003	-0.004	0.015
	(0.012)	(0.016)	(0.020)
Observations	10,795	6,417	4,378
R^2	0.016	0.014	0.018
Mean dep. var.	0.543	0.552	0.531
Panel B.		*P(Dropout)*	
	All	Boys	Girls
Ineligible×Post	0.008	0.008	0.008
	(0.032)	(0.040)	(0.051)
Ineligible	0.109***	0.116***	0.099***
	(0.012)	(0.016)	(0.019)
Post	0.012	0.019	0.003
	(0.012)	(0.015)	(0.019)
Observations	10,795	6,417	4,378
R^2	0.009	0.010	0.008
Mean dep. var.	0.341	0.331	0.356

Notes. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Dropouts are defined as students who did not graduate from high school three years after compulsory school graduation and who were not enrolled in any high school program four years after compulsory school graduation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8. *Effects on DI take-up at different points in time*

	(1) DI take-up at $t+2$	(2) DI take-up at $t+3$	(3) DI take-up at $t+4$	(4) DI take-up at $t+3$
Ineligible×Post	0.019*	0.051***	0.068***	0.034*
	(0.010)	(0.020)	(0.023)	(0.018)
Ineligible	0.002	0.021***	0.038***	0.019***
	(0.002)	(0.006)	(0.007)	(0.005)
Post	0.002	0.015***	0.022***	0.014***
	(0.002)	(0.006)	(0.007)	(0.005)
Observations	10,795	10,795	10,795	10,795
R^2	0.002	0.006	0.009	0.169
Mean dep. var.	0.008	0.044	0.068	0.044
Controls for lagged dep. var.	No	No	No	DI take-up at $t+2$

Notes. Dependent variables are given by the column headers. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9. *Event study specification of the main model*

	(1) DI take-up at $t+3$	(2) DI take-up at $t+3$	(3) DI take-up at $t+4$
Ineligible×2007	-0.002	-0.003	-0.004
	(0.015)	(0.014)	(0.018)
Ineligible×2008	0.007	0.005	-0.014
	(0.015)	(0.015)	(0.018)
Ineligible×2009	0.010	0.009	0.029
	(0.015)	(0.015)	(0.020)
Ineligible×2011	0.064**	0.064**	0.100***
	(0.028)	(0.028)	(0.034)
Ineligible×2012	0.045	0.043	0.034
	(0.029)	(0.029)	(0.033)
Ineligible	0.017	0.016	0.036**
	(0.011)	(0.011)	(0.014)
Post	0.015***	0.017***	0.022***
	(0.006)	(0.006)	(0.007)
Observations	10,795	10,795	10,795
R^2	0.006	0.019	0.011
Mean of dependent variable	0.008	0.044	0.068
Controls	No	Yes	Yes

Notes. Dependent variables are given by the column headers. The results are estimated by the model $Y_{iy} = \alpha + \sum_{y=2007, y\neq 2010}^{2012} \beta_y Ineligible_s \mathbb{1}\{year = y\} + \delta Ineligible_s + Post_y + X'_{iy}\varphi + \epsilon_{iy}$. Model in column 2 controls for gender, migration status, and parental characteristics (DI take-up, income and education). As the data on national tests cannot be linked to the rest, the data are not used as controls. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10. *Event study specification of the models studying the effects of GY2011 on other outcomes at $t+3$, full sample*

	(1) P(Employment)	(2) P(Unemployment)	(3) Length of unemployment	(4) Labor earnings	(5) Disposable income
Ineligible×2007	-0.034	0.005	11.416	-2.953	0.545
	(0.028)	(0.032)	(9.573)	(2.628)	(2.508)
Ineligible×2008	-0.008	-0.022	13.675	-0.397	0.098
	(0.029)	(0.032)	(10.177)	(2.688)	(2.506)
Ineligible×2009	-0.025	0.011	18.867*	-4.638*	-1.623
	(0.028)	(0.032)	(9.631)	(2.521)	(2.463)
Ineligible×2011	-0.138***	-0.015	19.002	-11.177***	-8.254**
	(0.038)	(0.044)	(15.046)	(4.089)	(3.672)
Ineligible×2012	-0.106**	-0.102**	15.665	-13.621***	-8.376**
	(0.043)	(0.046)	(17.634)	(3.839)	(3.887)
Ineligible	0.060***	-0.055***	-5.966*	8.173***	6.419***
	(0.012)	(0.012)	(3.572)	(1.253)	(1.077)
Post	-0.025	-0.007	0.780	-2.688	-0.593
	(0.022)	(0.024)	(7.297)	(2.018)	(1.920)
Observations	10,795	10,795	4,628	10,688	10,578
R^2	0.006	0.003	0.005	0.009	0.005
Mean dep. var.	0.293	0.442	147.251	27.583	45.060

Notes. The results are estimated by the model $Y_{iy} = \alpha + \sum_{y=2007, y\neq 2010}^{2012} \beta_y Ineligible_s \mathbb{1}\{year = y\} + \delta Ineligible_s + Post_y + \epsilon_{iy}$. Dependent variables are given by the column headers. Employment is defined as having worked for at least one hour a week in November. Registered unemployment is defined by being registered as open unemployed or participating in some labor market program in a particular year. Length of unemployment stands for the total number of days registered in the two labor market schemes. All negative values of labor earnings and disposable income are treated as missing. Both of the variables (in thousands of Swedish kronor) are trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11. *Effects of GY2011 on various labor market outcomes at $t+4$, full sample*

	(1)	(2)	(3)	(4)	(5)
Panel A.	P(Employment)	P(Unemployment)	Length of unemployment	Labor earnings	Disposable income
Ineligible×Post	-0.104***	0.011	10.493	-21.861***	-12.858***
	(0.030)	(0.042)	(12.407)	(4.916)	(3.757)
Observations	10,795	9,627	5,132	10,634	10,562
R^2	0.011	0.003	0.011	0.014	0.010
Mean dep. var.	0.368	0.543	185.718	61.088	78.946
Panel B.	P(Employment)	P(Unemployment)	Length of unemployment	Labor earnings	Disposable income
Ineligible×2007	-0.050*	0.010	6.341	-3.495	-1.280
	(0.030)	(0.032)	(10.065)	(4.853)	(3.621)
Ineligible×2008	-0.065**	0.001	21.697**	-4.881	-1.551
	(0.030)	(0.032)	(10.102)	(4.857)	(3.671)
Ineligible×2009	-0.053*	0.032	17.063*	-8.725*	-5.015
	(0.030)	(0.032)	(9.934)	(4.633)	(3.518)
Ineligible×2011	-0.159***	0.023	22.199	-22.723***	-10.031*
	(0.042)	(0.047)	(14.038)	(6.968)	(5.183)
Ineligible×2012	-0.132***			-30.941***	-21.309***
	(0.046)			(7.084)	(5.675)
Observations	10,795	9,627	5,132	10,634	10,562
R^2	0.011	0.003	0.012	0.015	0.011
Mean dep. var.	0.368	0.543	185.718	61.088	78.946

Notes. The results in panel B are estimated by the model $Y_{iy} = \alpha + \sum_{y=2007, y\neq 2010}^{2012} \beta_y Ineligible_s \mathbb{1}\{year = y\} + \delta Ineligible_s + Post_y + \epsilon_{iy}$. Dependent variables are given by the column headers. Data on unemployment was available until 2015. Employment is defined as having worked for at least an hour a week in November. Registered unemployment is defined as being registered as open unemployed or participating in some labor market programs. Length of unemployment denotes the total number of days registered in the labor market schemes. Negative values of labor earnings and disposable income are treated as missing, and the variables (in thousands of Swedish kronor) are trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12. *Effects of GY2011 on additional labor market outcomes at $t+3$, by gender*

	(1)	(2)	(3)	(4)	(5)	(6)
	Length of unemployment			Disposable income		
	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	28.241	-7.741	35.983	-5.058	-9.681***	4.623
	(21.000)	(12.959)	(24.673)	(4.616)	(3.013)	(5.512)
Observations	1,823	2,805		4,317	6,261	
R^2	0.002	0.009		0.011	0.003	
Mean dep. var.	147.162	147.311		46.983	43.688	

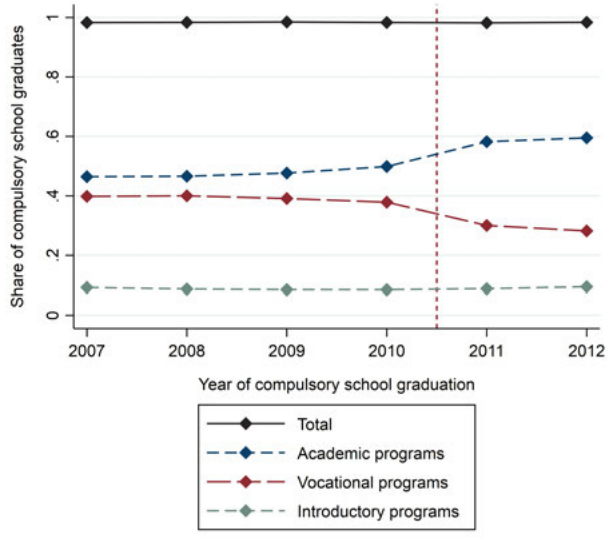
Notes. Dependent variables are given by the column headers. Registered unemployment is defined by being registered as open unemployed or participating in some labor market program in a particular year. Length of unemployment stands for the total number of days registered in the two labor market schemes. All negative values of disposable income are treated as missing and the variable (in thousands of Swedish kronor) is trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A13. *Effects of GY2011 on various labor market outcomes at $t+4$, by gender*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A.	P(Employment)			P(Unemployment)			Length of unemployment		
	Girls	Boys	Diff.	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	-0.029	-0.149***	0.120*	-0.018	0.029	-0.047	25.535	2.126	23.409
	(0.051)	(0.036)	(0.062)	(0.066)	(0.054)	(0.085)	(21.415)	(15.132)	(26.219)
Observations	4,378	6,417		3,940	5,687		1,954	3,178	
R^2	0.014	0.010		0.002	0.003		0.007	0.014	
Mean dep. var.	0.366	0.370		0.505	0.569		184.021	186.781	
Panel B.	Labor earnings			Disposable income			DI take-up		
	Girls	Boys	Diff.	Girls	Boys	Diff.	Girls	Boys	Diff.
Ineligible×Post	-5.865	-32.049***	26.184**	-5.236	-17.263***	12.027	0.028	0.091***	-0.063
	(8.307)	(6.061)	(10.282)	(6.410)	(4.576)	(7.875)	(0.036)	(0.030)	(0.047)
Observations	4,343	6,291		4,323	6,239		4,378	6,417	
R^2	0.018	0.013		0.014	0.009		0.005	0.014	
Mean dep. var.	56.453	64.395		81.452	77.153		0.076	0.063	

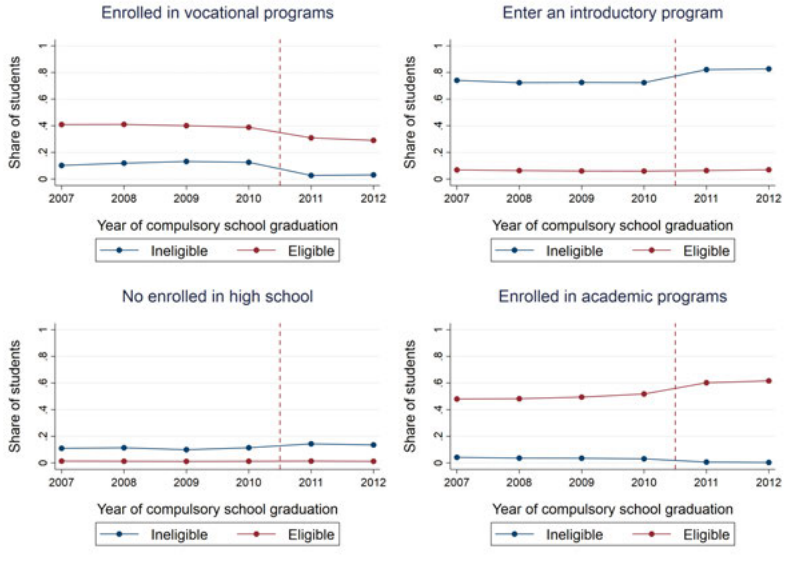
Notes. Dependent variables are given by the column headers. Employment is defined as having worked for at least one hour a week in November. Registered unemployment is defined by being registered as open unemployed or participating in some labor market program in a particular year. Length of unemployment stands for the total number of days registered in the two labor market schemes. All negative values of labor earnings and disposable income are treated as missing. Both of the variables (in thousands of Swedish kronor) are trimmed by 1% from both ends of the distributions. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011. t denotes the year of compulsory school graduation. Mean dep. var. indicates mean of the dependent variable for pre-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1. Changes in high school enrollment during the period 2007–2012



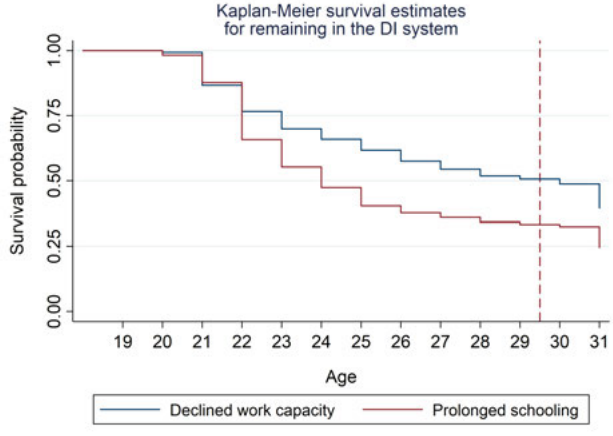
Notes. The figure shows the share of compulsory school graduates enrolling in high school (and in different high school programs) directly after compulsory school graduation. The figure (as well as the whole study) builds on data on students who obtained compulsory school education in Sweden and excludes, therefore, the majority of the students enrolled in the language introduction after the reform. The vertical dashed line marks the introduction of the stricter high school eligibility requirements.

Figure A2. Changes in high school enrollment during the period 2007–2012, full student population



Notes. The vertical dashed lines mark the introduction of the stricter high school eligibility requirements. Ineligible is an indicator for not meeting the stricter high school eligibility requirements introduced in 2011.

Figure A3. Survival estimates for adolescents' DI participation by the underlying reason of the DI award



Notes. The figure plots 13 year survival estimates for remaining in the DI system for people who were awarded the activity compensation at age 19. Data for 19-year-olds in 2003–2005 are used. Only people with one continuous DI spell during the time period are included in the analysis (about 72% of all the 19-year-olds DI beneficiaries in 2003–2005). The vertical dashed line marks the end of the eligibility for the activity compensation and the beginning of the eligibility for the sickness compensation scheme for 30–64-year-olds. Students who did not graduate from high school in three years and were not enrolled in high school at $t+4$, where t denotes the year of compulsory school graduation, are defined as high school dropouts. Students who did not graduate from high school at $t+3$, but were enrolled in school at $t+4$ are treated as students with prolonged schooling. Due to the lack of data on the underlying causes of the DI awards, these two categories are used as proxies for the causes. The dropouts are assumed to be more likely awarded the benefit for declined work capacity and students still in school at $t+4$ for prolonged schooling.

3. Stricter Graduation Standards and Labor Market Entry

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3.1 Introduction

Many education systems rely on graduation standards in order to uphold the quality of education. These graduation standards also provide students with a tool to signal their skills and abilities to prospective employers. Yet, the existing, largely U.S. based evidence on the effects of stricter graduation standards is mixed, showing no sizable gains in academic achievement, an increase in dropout rates among low-achieving students and racial minorities, and little or no positive effects on labor market outcomes (see a summary of the studies in Holme et al., 2010). Clark and Martorell (2014) conclude that reaching standards and earning a diploma does not carry any signaling value. The literature and the conclusion rest on data from comprehensive school systems and tests that measure students' general skills in domains such as reading, mathematics, science and social science. In practice, students' occupation-specific skills are presumably of greater importance for employers. The literature on the effects of specific skill requirements and the effects of stricter skill requirements on vocational graduates is, however, scant. Only Bishop and Mane (2005) have shown that stricter standards have a relatively larger effect for vocational students.

In this paper I study how stricter high school graduation requirements affect students' behavior and early career outcomes among Swedish vocational high school graduates[1] focusing on job finding rates and job match quality. I exploit a Swedish high school reform that substantially raised high school graduation standards by requiring passing grades in a large share of general and program-specific subjects. To clearly denote the changed underlying graduation standards, the old high school diploma was replaced with a new graduation document (*new diploma*). The purpose of that was to provide employers with a more informative certification of general and program-specific competence of high school graduates.

Using detailed administrative data on students' educational records linked to a merged employer-employee data set, I explore two of the primary changes in the graduation standards—the increased threshold of total number of passing grades and the requirement of passing a program-related diploma project. Theoretically, the stricter standards could affect students through two different channels: via signaling to prospective employers and via enhanced effort by students and educators.

In the signaling theory framework (Spence, 1973), increased costs of earning a diploma would concentrate diploma acquisition among a smaller group of more able students increasing the value of the signal. As I

[1]Sweden has a school-based system of vocational education with field-specific programs, such as building and construction program, electricity program, vehicle and transport program, hotel and tourism program, and health and social care program.

illustrate, the new diploma strongly distinguishes between students from the upper and lower part of the grade distribution. Students with the new diploma have higher high school grade point average (GPA) and the distribution of GPA is more compressed than the GPA distribution among students without it. Moreover, the new diploma is a better indicator of program-specific skills in that it distinguishes between stronger and weaker students in terms of the GPA of program-specific courses as well.

Betts (1998) and Betts et al. (2001) argue that the main rationale for standards is to alter incentives of students, parents and teachers to enhance learning. The models with incentive effects show that behavioral responses should occur particularly among students just below higher thresholds, for whom the costs of exerting extra effort are low relative to the benefits (Betts, 1998; Betts et al., 2001; Levitt et al., 2016). Lepper and Greene (1978) further argue that the margins of general and specific skill requirements could affect students' labor market outcomes differently as the behavioral responses may depend on the types of extrinsic motivators.

The study provides reduced form evidence concerning both the impact on early career outcomes and on the incentive effects in school. My empirical strategy relies on a difference-in-differences design comparing students just above and below the new thresholds. I test if the number and composition of students satisfying the criteria changes with the introduction of the new graduation standards.

The results show that higher *general* skill requirements do not increase the probability of reaching the stricter threshold. The incentive effects are very close to zero and statistically insignificant across various subgroups. Stricter *specific* skill requirements lead, on the other hand, to a sharp sizable increase in the fraction of students who pass the threshold. The response is stronger among male students, those with low socioeconomic background, and students with higher level of motivation of finding a job in an industry related to the field of studies. The difference in the responses may originate from a higher expected payoff at the margin of stricter specific skill requirements. The findings are also in line with the arguments that more conceptual topics may be more difficult to prepare for (Bettinger, 2012) and that extrinsic motivators may be more effective for concrete subjects rather than more conceptual topics (Lepper and Greene, 1978; Rouse, 1998; Gneezy et al., 2011).

In terms of labor market outcomes I find no impact of passing stricter *general* graduation standards on employment in general nor on the probability of finding a matching job. Studying the impact of higher *specific* graduation standards requires a greater leap of faith due to the documented increase in the number of passing students. With this caveat in mind, I find that students above the bar are more likely to find a stable job. Supplementary analyses show that they are also more likely to find

a job faster and their job match quality is higher. Before the reform, there was no difference in the probability of being employed at a job that survives the probation period between the students above and below the stricter graduation threshold. After the reform, the gap increased to 6 percentage points.

Overall, the results are the first that build on a quasi-experiment that drastically raised general and specific graduation requirements in a setup of a selective high school system.[2,3] The analysis is very data demanding and is made possible through a unique combination of course-specific grades and detailed register data on labor market. Using rich data on students' employment spells and characteristics of the workplaces I further contribute to the literature by studying the effects on job match quality.[4] Spence (1973) argues that signaling costs that in his model are assumed to be negatively correlated with productive capability—a prerequisite for distinguishing between different applicants—may be negatively correlated with one type of productive capability but not with another. Hence, being able to signal a particular kind of productivity may be more important for employment outcomes at jobs that value that particular ability rather than employment outcomes on average. As vocational high school programs (e.g. the health and social care program, the electricity program and the vehicle and transport program) prepare students for particular occupations, a signal of graduates' program-specific skills is presumably of special interest for employers. In addition, the gains from stricter graduation standards are presumably particularly important for vocational

[2]The existing U.S. based studies on the effects of increased graduation standards (for an overview of various studies see e.g. Holme et al., 2010) analyze mostly the effect of minimum competency exams (MCE) and course graduation requirements (CGR) that both underline the importance of academic content of curriculum.

[3]There is very little evidence on the effect of increased graduation standards on vocational students. Only Bishop and Mane (2005) have studied the impact of graduation standards on vocational students.

[4]A bulk of the literature has focused on students' educational outcomes, dropout rate in particular (Holme et al., 2010). The literature on labor market outcomes is more limited and shows few (if any) positive effects. Bishop and Mane (2001, 2005) show that state MCEs are positively associated with earnings, and that the effect is larger for vocational students (employment probability of vocational students was found to be 5.1% higher, and annual earnings 9.6% higher in the MCE states). Dee (2003) and Dee and Jacob (2006) exploit within-state variation in examination policies and find that stricter graduation requirements have no substantial impact on average employment outcomes, but affect various subgroups differently. They conclude that the heterogeneity of the findings suggests that exit exams might have unique incentive effects by race and ethnicity. Martorell (2005) shows that passing more rigorous graduation exams has a temporary positive effect on employers' beliefs of job-seekers' productivity, whereas Clark and See (2011) and Clark and Martorell (2014) find no effects of stricter graduation standards on earnings.

students who cannot distinguish themselves from others with help of a college degree (Betts et al., 2001).

The estimates of the effect of eligibility for a stronger diploma on students' post-graduation employment outcomes contribute to the literature on the signaling value of education (Tyler et al., 2000; Clark and Martorell, 2014). The fact that stricter graduation standards divide students into narrower groups which may reduce firms' hiring costs and allow them to better discriminate between weaker and stronger candidates relates the paper to the literature on screening discrimination (Arrow, 1971; Wolpin, 1977; Aigner and Cain, 1977; Oettinger, 1996; Cornell and Welch, 1996; Pinkston, 2003). Lastly, strong responses to the change in specific skill requirements suggest that students and educators perceive these as carrying an important value. This result contributes to the understanding of the mechanisms of the effects of stricter graduation standards on various outcomes and informs the literature on interventions aimed at boosting student achievement (see e.g. Jackson et al., 2015; Stinebrickner and Stinebrickner, 2008; Hvidman and Sievertsen, 2019; Haraldsvik, 2012).

The paper is organized as follows. Section 3.2 describes the Swedish vocational high school system and the graduation requirements before and after 2014. Section 3.3 presents the data and defines the main outcome variables of interest. Sections 3.4 and 3.5 describe the empirical strategy for estimating the effects of stricter high school eligibility standards on students' employment outcomes at the margins of new general and specific skill requirements, respectively. Results are presented in the same sections. Section 3.6 concludes.

3.2 Institutional setting

3.2.1 Vocational high school studies in Sweden

The Swedish school system stipulates nine years of compulsory schooling starting at the age of 7 and ending at the age of 16. After that, all students are entitled to high school studies and nearly all students continue their studies at the post-compulsory level. Students can choose between a selection of academic and vocational high school programs. While academic programs prepare students for further studies at the tertiary level, a majority of graduates from vocational tracks enter the labor market directly. Throughout the paper, I focus on graduates from vocational high school programs. In order to qualify for high school studies, students must meet an eligibility threshold.[5] Conditional on that, students are

[5]For cohorts graduating from compulsory school in 2010 or earlier, passing grades in compulsory school math, English and Swedish classes were needed to qualify for the

allocated between their preferred tracks and schools based on their final grades from compulsory school.

As part of the curriculum, all vocational students take a set of courses that are common to all high school students. These foundation subjects (*core general subjects* hereafter) are to lay a ground for the most important skills in order to cope with a changing labor market, to be prepared for further studies, and to be able to actively participate in society. A second group of classes consists of core program-specific courses which give each track its special focus. In addition to these, courses leading to specialization within the tracks and a set of elective courses are provided. Lastly, all students are required to complete a diploma project. The program-related project aims at demonstrating students' proficiency within the field and is perceived by the industry as the only channel for confirming the quality of graduation standards (Svenskt Näringsliv, 2016). The total course load sums up to 2500 high school credit points.

3.2.2 GY2011 and increased graduation requirements

Until 2013, the completion of vocational high school studies was certified by a high school diploma called *slutbetyg* (the old diploma hereafter). In order to earn the diploma, students must have obtained a grade in all classes[6] and a *passing* grade in core math, Swedish and English classes (persistent graduation requirement hereafter). Students who obtained passing grades in at least 90% of the total course load (2250 credit points) earned additionally direct eligibility for studies at the tertiary level. The fairly low requirements together with the opportunity to adapt programs into a specially designed one implied that students had a great degree of discretion in adjusting their curriculum. That, in turn, made the high school education fragmented and complicated prospective employers understanding of graduates' skill levels and profiles. The possibility to earn the diploma by obtaining the highest grades in all subjects or by barely passing math, Swedish and English classes and failing all the others made the old diploma a noisy signal of skill levels as well.

The ambiguity of the signal was addressed by a Swedish high school reform Upper Secondary School 2011 (*Gymnasieskola 2011*, GY2011 hereafter). As part of the reform, the high school graduation requirements were raised (see Table 1).[7] The reform was enacted in 2011 and the first graduation cohort affected by the new graduation requirements is the one of 2014. Since 2014, students must meet the persistent graduation

vocational high school programs. In 2011, demand for five additional passing grades (in any subjects) was introduced.

[6]Grades indicating failure in a subject were accepted.

[7]The reform increased high school eligibility requirements as well. The effects of that change are studied in Chapter 2 of this thesis.

requirement, pass at least 90% of the total course load, obtain passing grades in the core program-specific subjects worth at least 400 points and obtain a passing grade in the diploma project in order to earn a high school diploma. The requirement of passing at least 400 points of the core program-specific courses translates into a requirement of 100% of such courses being passed for nine programs out of 12, 57% for two programs and 36% for one program (see Appendix Table A2). While before the reform, the diploma project was graded using a scale with three passing grades—Pass, Pass with Distinction and Excellent—only a Pass/Fail distinction is used since the enactment of the reform.

The new graduation requirements can be grouped into two categories. The requirements of passing a certain amount of core track-specific courses and the diploma project address graduates' vocational proficiency and lack any pre-reform counterpart. Although the block of core track-specific courses and the diploma project were part of the curriculum also before, grades in these subjects did not matter for graduation.[8] The demand for passing grades for 2250 credit points signals a general skill level rather than specific vocational competence as the total course load is a mix of general and vocational courses. Although this requirement is also new, an incentive for reaching that threshold was in place already before the reform—the students who met the margin earned eligibility for studies at the tertiary level. In practice, only a small share of vocational graduates continue their studies at the tertiary level (on average less than 5% of the pre-reform cohorts), though. In this paper, I study separately (i) the effect of stricter general skill requirements by focusing on the students at the margin of passing 2250 credits points and (ii) the effect of stricter specific skill requirements by focusing on the margin of passing the diploma project, conditional on meeting all the other graduation requirements.

The stricter and more complex graduation requirements were developed in collaboration with various interest groups and their purpose was to better guide high school studies in different programs towards a commonly known goal and guarantee that students who earn a graduation document meet a certain commonly agreed level of skills within a field (Regeringen, 2008). For achieving the common agreement, national and local program councils (*programråd*) consisting of representatives of industry, labor market organizations and government agencies were established for each vocational program. Their task is to advise the National Agency for Education e.g. in terms of professional competence needed at jobs in the fields, curriculum content, study outcomes and graduation

[8] As part of GY2011, the set of core track-specific courses was redefined for each track to ensure that it consists only of the subjects really necessary for all of the students in a program despite their specializations. As a result, the total amount of such courses declined for most programs, but the passing requirement was raised from 0 to 100% in most cases (see Appendix Table A2).

Table 1. *Vocational graduation requirements before and after the enactment of GY2011*

Before GY2011	**After**
Take courses for at least 2500 credits	Take courses for at least 2500 credits
Pass math	Pass math
Pass English	Pass English
Pass Swedish	Pass Swedish
–	Pass 2250 credits
–	Pass 400 points of core track-specific courses
–	Pass the diploma project

requirements. Learning outcomes, course plans and program structures have been changed slightly during recent years, but the fact that the amount of core track-specific courses has been increased only for one of the vocational programs indicates that the initial design of the programs done in collaboration with different employer organizations is considered adequate for various interest groups.[9]

To clearly denote that students graduating from vocational high school programs since 2011 have met a higher graduation threshold, the old diploma was replaced with a new document—a vocational diploma (*yrkesexamen*, the new diploma hereafter).[10] Students below the new graduation threshold are issued a study certificate (*studiebevis*)—a document listing all taken courses and obtained grades.

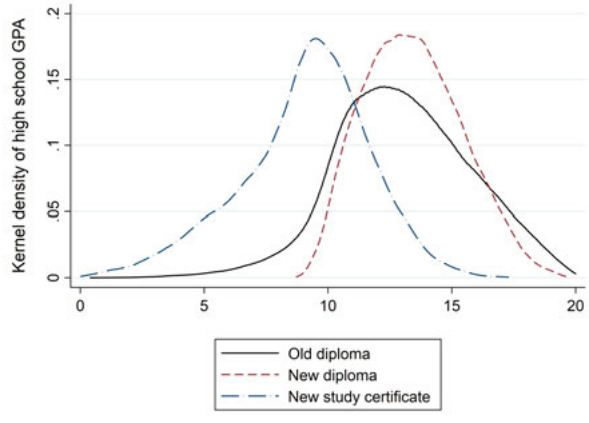
The new graduation documents with the stricter underlying graduation standards limit the ambiguity the old diploma came with, and reduce firms' costs on obtaining information on students' skill set and ability. In order to earn the new diploma students must not only put more effort in obtaining more passing grades but are also required to demonstrate a certain level of program-specific proficiency. In Panel A of Figure 1 I plot the distribution of high school GPA of students with different graduation documents. The figure shows that the GPA of students with the new diploma is higher than the GPA of students who earned the old diploma, and much higher than the GPA of students who do not meet the stricter graduation requirements and earn, thus, the new study certificate. Moreover, the variance of the GPA distribution of students who earn the new

[9]The amount of core track-specific courses increases from 1100 to 1400 credit point for the health and social care program in July, 2021. More changes have taken place in terms of specializations within the tracks, content of some courses, and learning outcomes.

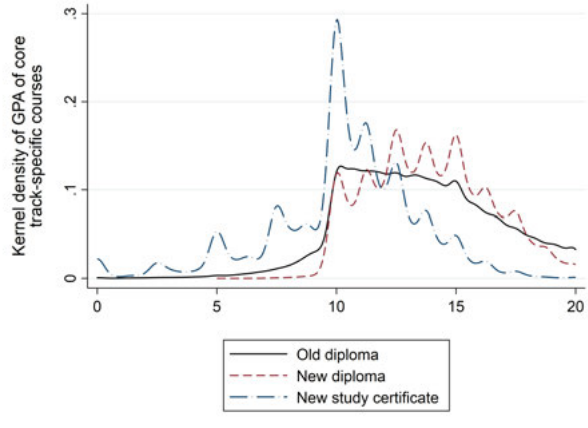
[10]An academic diploma (*högskoleförberedande examen*) with different graduation requirements was introduced for the graduates from the academic tracks.

Figure 1. Distribution of high school GPA and the GPA of program-specific courses by type of graduation document

(a) High school GPA



(b) GPA of core program-specific courses



Notes. Data for all students graduating from the vocational high school programs in 2010–2016 are used in the analysis. The category "Old diploma" shows the grade distribution of the graduates of 2010–2013. "New diploma" and "New study certificate" show the grade distributions of the graduates of 2014–2016 distinguishing between those who met the new high school graduation requirements and those who did not.

diploma is much smaller than the variance of the other distributions indicating that the signal is more precise. In Panel B I plot the distribution of GPA of core program-specific courses. Importantly, the figure shows similar patterns as panel A suggesting that the new documents differentiate between students in terms of their program-related proficiency as well.

3.3 Data

Data sources

The study relies on Swedish register data on vocational students graduating from high school in 2010–2016.[11] Students' high school graduation records provide information on the high school programs they graduated from, their subject grades, high school GPA, year of graduation and the type of document earned. Compulsory school records are used to account for high school eligibility. Data from a merged employer-employee data set that provide firm and establishment level data on employment spells and earnings are further linked to students' educational records and the data on firms' and establishments' characteristics. Data from the Integrated Database for Labor Market Research (LOUISE) and the Multigeneration register are used for obtaining information on students' background characteristics, including their parental background.

Definitions of the key variables

Throughout the paper, students graduating from high school in 2010–2013 are treated as pre-reform cohorts and those graduating in 2014–2016 as post-reform cohorts. In sections 3.4 and 3.5 I explore two different margins of the reform. In section 3.4 I focus on the general skill requirements and define treatment as passing at least 2250 credit points of the total course load. In Section 3.5 I study the effect of stricter specific skill requirements and define treatment as passing the diploma project. In both cases, the students who met the new standards are labeled as *Eligible* and those below the new thresholds as *Ineligible* for the new diploma.

Employment outcomes

The outcome variable in focus of the study is the probability of employment at a stable job. I define a *stable job* as the first post-graduation job that lasts for at least four months and produces total earnings larger than three times the monthly minimum wage.[12] These criteria aim at excluding summer jobs. To identify stable jobs, I start tracking students' employment spells in August of the year of high school graduation and follow them through 29 months. I focus on the first job that meets the

[11] In case graduation records existed for several years, the earliest records were used. The programs were somewhat differently designed before and after the reform. See Appendix Table A1 for an overview of the mappings.

[12] There is no statutory minimum wage in Sweden. Instead, the minimum wage is proxied by the 10^{th} percentile of the actual wage distribution in the year of high school graduation, following Kramarz and Skans (2014) and Hensvik et al. (2017).

criteria. If several jobs meet the criteria and start at the same time, I focus on the one generating the largest income or lasting the longest.

Matching jobs

Since the reform increased both general and specific skill requirements, I ask whether it affected job match quality. For defining matching jobs, I first identify the most common industries where graduates from pre-reform cohorts ended up working in five years after graduation.[13] *Stable matching job* stands for a stable job in an industry that corresponds to any of the two most common industries among pre-reform graduates from the same vocational high school program. In a few cases where the difference in frequencies between the second, third (and fourth) most common industry were negligible, I expanded the list of matching industries respectively. For some tracks manual matching was allowed to match obvious but less common matches accurately and thus minimize the type II error of misclassifying the matches.

Summer jobs

Studying the effects of stricter graduation requirements on students' employment outcomes may suffer from self-selection of more motivated students into the group of eligible students. As it is reasonable to believe that students who look for and take up the opportunities of in-school work experience are more motivated to find a job (for some evidence, see Silva et al., 2020), I rely on a proxy variable of having a summer job experience in such an industry to capture the level of motivation of finding a matching job. The variable is created by tracking students' employment spells throughout their high school studies[14] and defining all of their jobs as matching or non-matching as described above. 55–56% of the students in my study had such in-school work experience. A variable of any summer job experience is defined analogously ignoring the match component of the job. Nearly all students (91–92%) had obtained some work experience during their high school studies. Appendix Figure A1 shows a strong positive correlation between the summer job experience and the probability of post-graduation employment.

Analysis samples

I use two different samples to facilitate the analyses of the stricter general and specific skill requirements. To study the effects of the increase

[13] I rely on the Swedish Standard Industry Classification at the two-digit level.
[14] August of the first year (t) through June in $t + 3$.

in general skill requirements on students' early career outcomes, I focus on students who met all of the new graduation requirements apart from passing 90% of the total course load. To minimize the effect of alternative factors than eligibility for the new diploma on the outcomes, I rely on a set of sample restrictions. First, I study the effects among students who graduated from high school in three years. This excludes a considerable share of students who graduated in four or five years, but the restriction is necessary for focusing on the post-reform graduates who entered high school under the new rules. Along with the stricter graduation standards, high school eligibility requirements were raised in 2011. To account for that, the sample is restricted to students who were eligible for high school studies throughout the sample period.[15] Lastly, I limit the sample to students who met the persistent graduation requirement, passed the diploma project and the core program-specific courses for at least 400 points, and passed in total courses for 2100–2400 credit points.[16] The latter restriction aims at better capturing unobservable characteristics that could affect the results. A robustness analysis confirms that my results are robust to alternative approaches to the sample construction. I exploit the remaining variation in the total number of passing grades. There are 20,963 students in the sample.

To study the effects of the increase in specific skill requirements on students' early career outcomes, I focus on students who pass or fail the diploma project. Similarly to the approach at the previously described margin, I adopt a set of sample restrictions to minimize the effect of other factors, such as differences in various observable and unobservable characteristics, on the main findings. As the only difference from the previous strategy, I now relax the requirement of obtaining a passing grade for the diploma project, and constrain the attention to the intensive margin of the general skill requirement. This leads to a sample of students who met the persistent graduation requirement, passed program-specific courses for at least 400 points, and passed in total courses for 2250–2400 credit points. I exploit the remaining variation in grades in the diploma project. There are 21,403 students in that sample.

The samples are very similar to each other in terms of the composition of students (see Appendix Table A4). 40% of the students are female, about 5% are not born in Sweden and 78% of students are of low socioeconomic background as defined by neither of the parents having a tertiary degree. The students in both samples are of similar ability, but

[15] That is students who had obtained passing grades in grade nine math, English and Swedish classes together with at least five additional passing grades.

[16] Additionally, students from the media program are excluded from the study because the program only existed until 2011, and students from Waldorf schools, international Swedish schools and those enrolled in the International Baccalaureate as different grading systems apply for them.

the students at the margin of meeting the stricter specific graduation requirements are, by construction, from a slightly higher part of the grade distribution. The distribution of students across different vocational programs reflects to a large extent the distribution in the full population of vocational graduates.

3.4 Stricter general skill standards

This section shows how stricter general skill requirements affect students' behavior and labor market outcomes. I start by showing that there were no strong behavioral responses to the new incentives at that margin. After describing the identification strategy and the validity of the identifying assumptions, I show the effects of stricter general graduation requirements on students' early career outcomes.

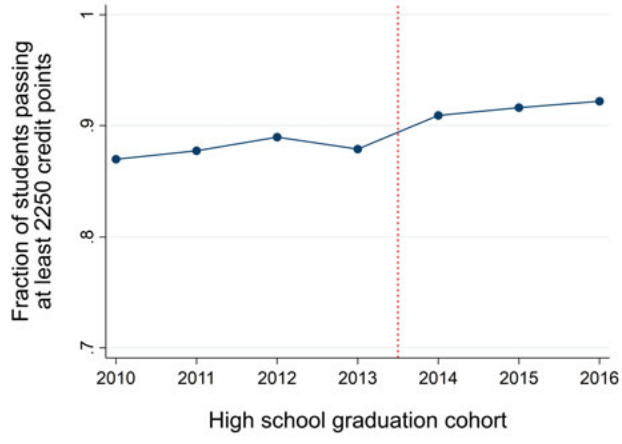
3.4.1 Behavioral responses

The introduction of the new graduation standards was motivated by increasing the value of high school diploma for employers, whereas there could be two sources for the increase. In a pure sorting model students' behavior remains unaffected and the increased value of a diploma is achieved by relabeling the group of students who would have earned the diploma under the old rules as failures under the new standards. The assumption of no behavioral effects implies no effects on human capital accumulation and productivity. A more realistic model builds on the rationale for standards to alter incentives. As Betts (1998), Betts et al. (2001) and Levitt et al. (2016) show, the behavioral responses should occur particularly among students just below the higher threshold, for whom the costs of exerting extra effort are low relative to the benefits.

Figure 2 shows an upward trend in the probability of passing at least 2250 credit points throughout the study period. On average, the probability of passing the threshold is 3.7 percentage points higher among the post-reform cohorts, but controlling for the time-trend diminishes the effect. The remaining effect is completely driven by the cohort of 2013.

The balance table, Table 2, further shows that the sample is not affected by selection in terms of gender, socioeconomic background and in-school work experience. The fact that most of the estimates on compulsory school subject grades in column 1 of Table 2 are negative suggests, however, that the eligible students might be negatively selected after GY2011. This is also reflected in Appendix Table B1 which shows that students with weaker compulsory school results reacted slightly stronger to the incentive of passing the new general skill requirements. I account

Figure 2. Fraction of students who passed at least 2250 credit points



for the compulsory school subject grades in the analyses presented in the next section.

3.4.2 Effects of stricter general skill requirements on employment outcomes

Empirical strategy

In order to study the effects of increased graduation requirements on students' school-to-work transition, I employ the difference-in-differences (DiD) approach. I compare the outcomes of students who were eligible for the stronger high school diploma after the reform but not before to the same outcomes among students who were ineligible throughout the study period.

$$Y_{ipt} = \alpha + \beta Eligible_s Post_t + \delta Eligible_s + \lambda_t + \lambda_p + X'_{ipt}\gamma + \epsilon_{ipt} \quad (3.1)$$

The main model of interest is given by equation 3.1, where Y_{ipt} denotes employment outcomes for student i graduating from program p in year t in the main reduced form analysis and the diploma attainment when analyzing the effect of stricter standards on diploma attainment. $Eligible_s$ is an indicator variable equal to one for students who passed courses for at least 2250 credits, $Post_t$ is an indicator that takes the value of one for post-reform cohorts, λ_t stands for cohort fixed effects, λ_p for track fixed effects and X_{ipt} for a set of control variables. In some specifications I add local labor market fixed effects, LLM_m, a track, cohort

Table 2. *Changes in the composition of eligible students*

Dependent variable	(1) Diff-in-diff	(2) s.e.	(3) Pre-reform mean
Female	-0.013	(0.023)	0.428
Sum of parents' income	0.001	(0.027)	-0.197
Low SES	-0.004	(0.016)	0.779
Compulsory school GPA	-0.035*	(0.019)	-0.300
Number of passing grades	-0.052	(0.073)	13.048
Arts	-0.025	(0.034)	-0.184
Biology	-0.018	(0.027)	-0.276
Chemistry	0.009	(0.031)	-0.257
Civics	-0.044*	(0.026)	-0.300
Crafts	0.031	(0.034)	-0.102
English	-0.045*	(0.024)	-0.222
Geography	-0.038	(0.027)	-0.288
History	-0.040	(0.027)	-0.304
Home and consumer studies	-0.024	(0.033)	-0.184
Mathematics	0.000	(0.015)	-0.199
Music	-0.092***	(0.033)	-0.207
P.E. and health	-0.047	(0.040)	-0.113
Physics	-0.004	(0.029)	-0.258
Religion	-0.044*	(0.026)	-0.299
Swedish	-0.026	(0.021)	-0.283
Technology	-0.031	(0.030)	-0.190
No. of voc. courses taken	30.1**	(14.5)	1,085.6
No. of voc. courses passed	66.0***	(14.0)	1,045.2
Summer job	0.014	(0.014)	0.909
Summer job in a relevant industry	0.007	(0.023)	0.538
Independent school	-0.040**	0.020	0.186

Notes. Results in column 1 are estimated by the model $Y_{ipt} = \alpha + \beta Eligible_s Post_t + \delta Eligible_s + \lambda_t + X'_{ipt}\gamma + \epsilon_{ipt}$, where Y_{ipt} stands for the variables presented in the first column of the table and β-s are the parameters of interest shown in the table. Low SES refers to students whose neither parent has obtained tertiary education. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

and local labor market specific proxy for labor market tightness,[17] and adjust the results for compulsory school GPA and subject grades, as well as high school GPA. The local labor market (LLM) fixed effects control for permanent regional differences in employment and cohort fixed effects

[17]The track, cohort and local labor market specific labor market tightness measure is used as a control variable to account for the fact that the employment prospects of graduates from different cohorts and tracks in different regions are among other things affected by the excess or shortage of the workforce in the field in the area. The (inverse of) tightness is proxied by the fraction of new hires from non-employment following Manning (2003).

for general time trends in employment rate. By using the LLM_m fixed effects together with track fixed effects I estimate the effects of stricter requirements on students' employment outcomes by comparing students graduating from the same track and facing the same labor market before and after the reform. As the employment opportunities of graduates from different programs in different local labor markets might be affected by common shocks, all standard errors are clustered at the level of $LLM\times$ *high school program*.

At the margin of the new general skill requirements, I estimate the model on a sample of students who are very similar in terms of their high school GPA and who all meet most of the stricter graduation requirements. However, only the students who demonstrate a marginally higher general skill level by passing the requirement of 2250 passed credit points earn a high school diploma after the reform. The coefficient β is the reduced form parameter of interest that shows how the separation of students just above and below the new general skill requirement affects the gap in student outcomes.

Identifying assumptions

The DiD method leads to an unbiased reform effect under a few identifying assumptions. First, the employment outcomes should have evolved similarly among the eligible and ineligible students without the intervention. I validate the assumption by estimating an event study specification of eq. 3.1. Appendix Figure B3 shows that the probability of employment at different jobs evolved similarly among the eligible and ineligible students during the pre-reform period (in fact, throughout the whole study period). The results of a placebo test (see Appendix Table B2) also suggest that the parallel trend assumption is likely valid.

Second, various concurrent changes in the high school system introduced with the reform raise the concern of potential compositional changes among the eligible and ineligible students before and after the reform.[18] Table 2 shows, though, that the groups experienced similar changes in terms of gender, socioeconomic background or summer job experience. The primary cause of concern when interpreting the main findings as a causal effect of the reform is the indication of negative selection among the eligible students. I implement a set of robustness tests where I limit the sample to students even closer around the threshold of 2250 credit points to address the issue. I also study the reform effect on a set of students who passed a majority of compulsory school courses and for whom the new eligibility requirements were far from binding. The findings show that the main results are robust to the alternative sample restrictions that

[18]The reform changed not only the graduation requirements but even the high school eligibility standards. It also removed direct eligibility to further studies from the vocational programs potentially affecting the attractiveness of these tracks.

diminish the concern of negative selection. When estimating the effect of stricter graduation requirements on students' early career outcomes, I control for the unbalanced covariates.

Results

The results first show that passing courses for at least 2250 credit points strictly distinguishes between the students who earn the new diploma and those who earn the study certificate (see Appendix Table A6). Students above the threshold are 96.9 percentage points more likely to earn the new diploma than students below the bar. In what follows, the central question is whether the separation of more and less able students leads to a larger separation in the students' labor market outcomes. According to the results in Table 3, this is not the case. In terms of the probability of any stable employment, all model specifications suggest an effect that is very close to zero. Controlling for the unbalanced covariates does not change the conclusion. The results in panel B show the effect of reaching the threshold of 2250 passed credit points on the probability of stable employment in a relevant industry. The point estimates suggest that the probability is lower among the eligible students after the reform, but all of the estimates are noisy and statistically insignificant.

The negative point estimates likely reflect the potential negative selection in the group of eligible students after the reform as suggested by the balance table. To better capture unobservable characteristics, I conduct a set of robustness tests where I limit the sample closer to the threshold of 2250 credit points (see Appendix Table B3). On another dimension, I limit the sample to students who passed more grades in grade nine and whose study choices were, thus, less affected by the introduction of the new eligibility standards. In particular, the sample of students who started their studies in the vocational high school programs having passed a majority of mandatory compulsory school courses (more than 12) should reflect those with true preference for these programs. The sample is more balanced, but suffers from a smaller number of observations. The results based on the most restricted sample suggest that negative selection is embedded in the results shown in Table 3. When analyzing the reform effect on probably more similar students, the effects remain insignificant but get smaller in magnitude, and turn positive in the most narrowly defined set of students.

I interpret the stability of the findings of stricter general skill requirements on average job finding probability as confirming that the new graduation requirements at this margin did not affect students' employment outcomes.[19] The findings are in line with the previous literature (Jacob, 2001; Bishop and Mane, 2001, 2005; Martorell, 2005; Dee and Jacob,

[19]Alternatively, the fact that the threshold of 2250 passed credit points was used to differentiate between academically more and less able students for awarding eligibility

Table 3. *Effects of stricter general skill requirements on employment outcomes*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.			P(stable job)			
Eligible×Post	0.005	-0.003	-0.002	0.004	-0.003	-0.002
	(0.019)	(0.019)	(0.017)	(0.019)	(0.019)	(0.018)
Observations	20,963	20,963	20,963	20,963	20,963	20,963
R^2	0.024	0.033	0.092	0.035	0.043	0.101
Mean dep. var.	0.814	0.814	0.814	0.814	0.814	0.814
Gap in outcomes	0.032	0.032	0.032	0.032	0.032	0.032
Panel B.			P(stable matching job)			
Eligible×Post	-0.012	-0.031	-0.025	-0.013	-0.032	-0.026
	(0.023)	(0.023)	(0.021)	(0.023)	(0.023)	(0.021)
Observations	20,963	20,963	20,963	20,963	20,963	20,963
R^2	0.062	0.088	0.212	0.069	0.094	0.216
Mean dep. var.	0.436	0.436	0.436	0.436	0.436	0.436
Gap in outcomes	0.069	0.069	0.069	0.069	0.069	0.069
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	No	No	No	Yes	Yes	Yes
HS GPA	No	Yes	Yes	No	Yes	Yes
No. of passed voc. courses	No	Yes	Yes	No	Yes	Yes
Unbalanced covariates	No	No	Yes	No	No	Yes

Notes. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry and low SES. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

2006; Clark and See, 2011; Tyler et al., 2000; Clark and Martorell, 2014). I find no effect of stricter general skill requirements on the probability of stable matching employment either. However, the fact that the reform effects on stable matching employment are more sensitive to the sample construction leaves room for bias in these estimates. The bias, especially in the most narrowly defined specifications, should in this case originate from the selection of more ambitious students out of vocational programs due to the removal of direct eligibility to tertiary education introduced by the same reform. This, in turn, would suggest that the results on matching employment might underestimate the true effect.

to tertiary studies before the reform may also explain the findings. That would be the case if students exerted effort for reaching the threshold before the reform not for eligibility for further studies but for distinguishing themselves from other vocational graduates, and employers relied on the signal for distinguishing between more and less productive workers. The data are not sufficient for testing that channel.

3.5 Stricter specific skill standards

Most of the existing literature on the effects of higher graduation standards on students' achievement and labor market outcomes has focused on changes in general skill requirements. This paper expands the literature to changes in specific skill requirements. By using a sample of students at the margin of just meeting the stricter program-specific graduation requirements, I show that an increase in specific skill requirements comes with strong incentive effects and leads to a distinct separation in early career outcomes of students who meet the new requirements and those who do not.

3.5.1 Behavioral responses

The introduction of the requirement of passing the diploma project came with strong behavioral responses. 80.8% of the students in the sample passed the diploma project before the reform. In 2014, the share increased sharply by 16.6 percentage points. The magnitude of the incentive effect is much larger than found in Hvidman and Sievertsen (2019), but similar to the results in Angrist and Lavy (2009); Kremer et al. (2009); Jackson (2010) and Guryan et al. (2015). The increase may originate both from the behavioral effects on students and the incentives provided for teachers/schools for changing grading practices.

Table 4 shows that the incentives affect various groups of students differently. In line with Guryan et al. (2015), I find that more motivated students are 2.3 percentage points more likely to obtain a passing grade post reform than less motivated students (see Table 4 and Appendix Figure C1). Female students react less to the incentives than male students. Similarly to Angrist and Lavy (2009), I find, however, that girls also respond strongly. The relatively lower response is driven by a higher baseline value for girls. Low-SES students are 1.7 percentage points more likely to obtain a passing grade after the reform than students from more advantaged backgrounds.

The contrasting behavioral responses at the two different margins are in line with the psychological (Lepper and Greene, 1978) and economic literature (Rouse, 1998; Gneezy et al., 2011) suggesting that extrinsic motivators may be more effective for concrete subjects rather than more conceptual topics. The more concrete subjects allow students to better prepare for tests as the tests often build on a set of facts. Tests in more conceptual topics may be more difficult to prepare for (Bettinger, 2012). In addition, the expected payoff to extra effort or manipulation are less predictable and less manipulable at the margin of the new general skill requirement. In order to reach the threshold, passing one single subject is not necessarily enough. Instead, it requires more effort from students

Figure 3. Fraction of students who passed the diploma project

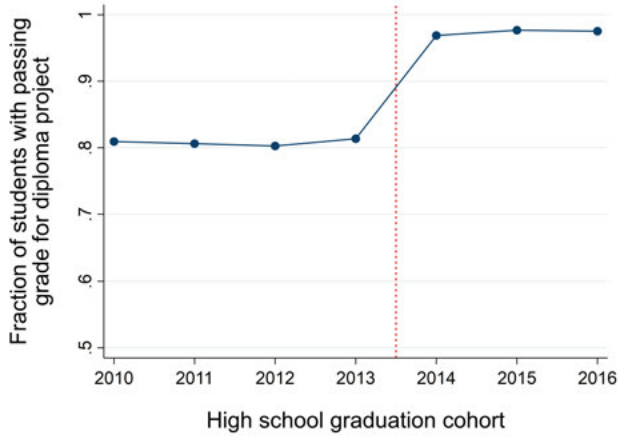


Table 4. *Heterogeneity of passing the threshold*

	(1)	(2)	(3)
Dependent variable	Diff-in-diff	Diff-in-diff	Diff-in-diff
Female	-0.059***	-0.059***	-0.058***
	(0.008)	(0.008)	(0.008)
Had a summer job	0.019	0.019	0.019
	(0.015)	(0.015)	(0.015)
Had a matching summer job	0.026***	0.026***	0.023***
	(0.008)	(0.008)	(0.008)
Low SES	0.018*	0.018*	0.017*
	(0.009)	(0.009)	(0.009)
Sum of parents' income	-0.007	-0.007	-0.008
	(0.006)	(0.006)	(0.006)
CS GPA	-0.009	-0.009	-0.006
	(0.010)	(0.010)	(0.010)
Independent school	-0.002	-0.002	-0.001
	(0.011)	(0.011)	(0.011)
Linear time trend	No	Yes	Yes
HS GPA	No	No	Yes

Notes. The results are estimated by the model $Eligible_{ipt} = \alpha + \beta X_{ipt} Post_t + \delta X_{ipt} + \lambda_t + \epsilon_{ipt}$, where $Eligible_{ipt}$ is a dummy variable equal to one for students who meet the stricter requirement and X_{ipt} stands for the variables presented in the first column of the table. Each cell presents a value for β from a different regression. Results in columns 2 include a control for a linear time trend and the results in column 3 for standardized high school GPA. Low SES refers to students whose neither parent has obtained tertiary education. The model with *Independent school* as the variable in focus is estimated on the cohorts of 2011–2016. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

or a collaboration of various teachers. The diploma project is specific in nature and, conditional on meeting all the other graduation requirements, comes with higher expected returns to the extra effort.

3.5.2 Effects of stricter specific skill requirements on employment outcomes

Empirical strategy

Similarly to the analysis in Section 3.4.2, I employ the DiD approach to study the effects of stricter specific skill standards on students' school-to-work transition. After restricting the sample as described in Section 3.3, I exploit the variation in grades in the diploma project. I compare various labor market outcomes of students with similar academic ability who all met the general skill requirement and passed at least 400 points of the core track-specific courses, but only students who passed the diploma project after the reform became eligible for the new high school diploma.

The main model of interest is still given by equation 3.1. In this case, β is the reduced form parameter of interest that shows how meeting stricter specific skill requirements affects students' labor market outcomes.

Identifying assumptions

Any study of the impact of passing the stricter specific skill requirements on labor market outcomes needs to rest on strong assumptions due to the large behavioral responses documented above. With this caveat in mind I will proceed and estimate the effects relying on a very detailed set of covariates to handle the potential selection.

Appendix Table C1 analyzes the balance of pre-determined covariates among the eligible and ineligible students and shows that the groups experienced similar changes in terms of students' compulsory school grades and parental background. However, the eligible students are more likely to have a summer job experience and much more likely to have an in-school work experience in a relevant industry. The main findings presented in the next section account for unbalanced background characteristics. In addition, I validate the parallel trend assumption using an event study specification of eq. 3.1. Appendix Figure C3 shows that the probability of employment at different jobs evolved very similarly among the eligible and ineligible students throughout the pre-reform period even at this margin. Results of a placebo reform on the high school graduates of 2006–2012 (Appendix Table C2) confirm the same.

Results

Similarly to the stricter general skill requirement, passing the diploma project strictly separates between the students who earn the new high

school diploma and those who do not (see Appendix Table A6). The findings presented in Table 5 further show that the employment gap between the students who met the stricter graduation requirements and those who did not increased from zero to 6.9 percentage points after the reform. The figure falls to 6.1 percentage points when controlling for grades (column 2) and declines even more when controlling for the unbalanced background characteristics. This suggests that selection explains part of the effect, but the results in column 3 are still positive and do not differ statistically significantly from the those in columns 1 and 2.

Table 5. *Effects of stricter specific skill requirements on employment outcomes*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A.			P(stable job)				>6m
Eligible×Post	0.069**	0.061**	0.043	0.067**	0.059**	0.042	0.060**
	(0.030)	(0.030)	(0.027)	(0.030)	(0.030)	(0.027)	(0.028)
Observations	21,402	21,402	21,402	21,402	21,402	21,402	21,402
R^2	0.025	0.035	0.095	0.036	0.044	0.102	0.120
Mean dep. var.	0.818	0.818	0.818	0.818	0.818	0.818	0.616
Gap in outcomes	0.000	0.000	0.000	0.000	0.000	0.000	-0.006
Panel B.			P(stable matching job)				>6m
Eligible×Post	0.091**	0.072**	0.039	0.084**	0.064*	0.032	0.062**
	(0.037)	(0.036)	(0.031)	(0.037)	(0.036)	(0.031)	(0.028)
Observations	21,402	21,402	21,402	21,402	21,402	21,402	21,402
R^2	0.061	0.089	0.211	0.068	0.095	0.216	0.190
Mean dep. var.	0.443	0.443	0.443	0.443	0.443	0.443	0.616
Gap in outcomes	0.005	0.005	0.005	0.005	0.005	0.005	-0.003
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	No	No	No	Yes	Yes	Yes	Yes
HS GPA	No	Yes	Yes	No	Yes	Yes	Yes
No. of passed voc. courses	No	Yes	Yes	No	Yes	Yes	Yes
Unbalanced covariates	No	No	Yes	No	No	Yes	Yes

Notes. Dependent variables are given by the column headers. In column 7, an indicator for employment that lasts longer than six months is used as an outcome variable. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry and low SES. Mean dep. var. stands for the mean dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

The previous results included track fixed effects estimating the effect among students from the same high school program. To account for the regional variation in labor market conditions, I estimate the same three model specifications with local labor market fixed effects. The results in columns 4–6 are nearly identical to those in columns 1–3. Moreover,

when focusing on the probability of stable employment that exceeds the usual probation period of six months (column 7), the estimate adjusted for all the unbalanced covariates and compulsory school grades is both large and significant (see a full table of the results on employment at such jobs in Appendix Table C3). While the eligible and ineligible students were equally likely to be employed at a stable job lasting longer than six months before the reform, the gap between the groups increased to 6 percentage points after the reform. This suggests that the probability of stable employment increased among the eligible students (as compared to the ineligible students) after the reform.

The results in panel B focus on employment in industries related to the field of studies. The findings suggest that meeting the stricter specific skill requirements leads to a relative increase in job match quality (as compared to the outcome among those below the threshold). The gap in the probability of employment at matching jobs declines when controlling for grades and even more so (by a factor of 2) when controlling for the compositional changes within the groups, but the estimates are all of comparable magnitude. Local labor market fixed effects do not affect these results either. The effect on employment at matching jobs lasting longer than the probation period (column 7) shows particularly explicitly that students eligible for the new diploma are relatively more likely to be employed at matching jobs. Importantly, the effect is estimated by keeping the motivation of finding such a job, as proxied by the indicator for having a summer job in a relevant industry, fixed.[20]

In Appendix Table C4 I study the robustness of the findings to various sample restrictions. In columns 2 and 3 I limit the sample more narrowly in terms of the total number of passed credit points in order to better capture various unobservables. In column 4, I expand the sample to students who graduated from high school in four years. By doing so I add an indicator for prolonged studies as a control when estimating the models. On another dimension I limit the sample tighter in terms of the total number of passing grades from grade nine to compare students with true preference for vocational studies. The students are probably less

[20]Appendix Tables C6 and C7 show the results of various supplementary analyses focusing on outcomes such as time to first job, probability of employment at a longer lasting job and income from the employment as well. The findings suggest that the reform did not only have a positive impact on the gap between the eligible and ineligible students employment probability, but also on the job finding speed and job match quality as measured by the probability of employment at a longer lasting job. Consistent with previous literature (Clark and See, 2011; Clark and Martorell, 2014), I find no effects on income from stable jobs. Additional results (not shown) also suggest that the relative effect of becoming eligible for the more informative diploma is stronger in the economically better times—students who meet the stricter specific skill requirements are experiencing relatively better labor market outcomes especially when facing a slack labor market.

affected by the removal of direct eligibility to tertiary studies. Furthermore, the new high school eligibility requirements were not binding for these students.

The results confirm a strong positive separation in the eligible and ineligible students' employment outcomes after the reform. Only the most narrowly defined samples that suffer from a low number of observations show small or no positive effects on matching employment. Appendix Table C5 shows the same analysis for stable and stable matching employment including employment spells that do not exceed the probation period. Comparison of the tables suggests again that increase in the specific skill requirements improves job match quality: the positive effects are particularly evident for longer lasting employment spells.

The findings suggest that the introduction of program-specific graduation requirements provides employers with information value in terms of graduates' relevant skills. This is in line with the perception of the industry that the diploma project provides them with an opportunity to confirm the quality of graduation standards (Svenskt Näringsliv, 2016). The information value may originate from accumulated human capital and from the pure diploma value. The findings of the paper do not aim at distinguishing between the two.

Interpreting the findings as information value rests on a strong assumption. The strong incentive effect together with an increased share of highly motivated students among those above the new graduation threshold suggest that some unobservable characteristics may affect the results. The 16.2 percentage point increase in the fraction of students with relevant summer job experience among the eligible students post reform indicates that the students were much more determined in finding a relevant post-graduation job. Controlling for the relevant summer job experience aims at capturing the effect of motivation, and the robustness analysis on more narrowly defined samples various other unobservables.

3.6 Conclusions

This paper provides evidence of how stricter high school graduation standards and eligibility for a new diploma alter incentives and affect students' employment outcomes at an early stage of the school-to-work transition. I exploit an exogenous change in both general *and* specific graduation standards, and study how these affect overall job finding rates and job match quality. The effects are estimated on two different sets of vocational high school graduates.

I first illustrate that the new diploma distinguishes well between more and less able students. High school grade point average (GPA) of students who graduate from high school with the new diploma is above the GPA of

students who graduate without it. Moreover, the new diploma is a better indicator of program-specific skills by distinguishing between stronger and weaker students in terms of the GPA of program-specific courses as well. This suggests that the new diploma with stricter underlying graduation standards can reduce employers' costs of obtaining information about newly graduated job-seekers' general and specific skills.

Secondly, I show that increase in the specific graduation standards leads to 21% increase in the probability of reaching the higher threshold. This suggests that the specific skill requirements are perceived to carry an important signaling value. The responses are stronger among male students, those with lower socioeconomic background and students with in-school work experience in a relevant industry.

Resting on strong assumptions due to the large behavioral responses, I find a strong impact of stricter specific skill requirements on employment outcomes. While the eligible and ineligible students were equally likely to be employed at a stable job lasting longer than six months before the reform, the gap between the groups increased to 6 percentage points after the reform. Students above the new threshold are also relatively more likely to find a job in a relevant industry. The effect may originate from human capital accumulation given the strong behavioral response. The incentive effect may, at the same time, reflect changes in grading practices. Thus, the pure diploma effect is also plausible. The paper does not aim at distinguishing between the two. Stricter general skill requirements affect neither students' (and/or educators') behavior nor their school-to-work transition.

All in all, the results suggest that the design of graduation standards can have very different impact on behavior and outcomes of students. My findings show that vocational students respond to and benefit from specific skill requirements more than general skill standards. More research is, however, needed to pin down to what extent employers value specific vs. general skill certification.

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Appendix A. General additional results

Table A1. *Mapping between the old and new vocational programs*

New programs	**Old programs**
a) Child and recreation	Child and recreation
b) Building and construction	Building and construction
c.1) Electricity and energy	Electricity
c.2) Electricity and energy	Energy, specialization on operation and maintenance
d) HVAC and property maintenance	Energy, specialization on HVAC
e) Vehicle and transport	Vehicle and transport
f) Business and administration	Business and administration
g) Handicraft	Handicraft
h) Hotel and tourism	Hotel and restaurant program, specialization on hotels
i.1) Restaurant management and food	Food program
i.2) Restaurant management and food	Hotel and restaurant program, specialization on restaurant and food service
j) Industrial technology	Industrial technology
k) Natural resource use	Natural resource use
l) Health and social care	Health and social care

Notes. The table shows the mapping between the old and new vocational high school programs. The programs were somewhat differently designed before and after GY2011. To make the programs comparable throughout the study period, I rely on specializations within the tracks where needed. HVAC stands for heating, ventilation and air conditioning.

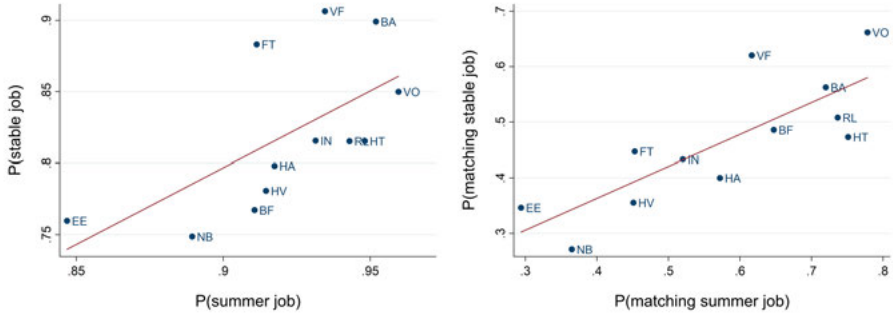
Table A2. *Total number of statutory core track-specific courses and the share that must be passed for meeting the diploma requirements, by program*

	(1)	(2)	(3)	(4)
	Before 2014		Since 2014	
	Credits	% passed	Credits	% passed
Child and recreation	700	0	700	57
Building and construction	500	0	400	100
Electricity and energy	400	0	400	100
HVAC and property maintenance	450	0	400	100
Vehicle and transport	500	0	400	100
Business and administration	600	0	400	100
Handicraft	700	0	400	100
Hotel and tourism	650	0	400	100
Restaurant management and food	600	0	700	57
Industrial technology	400	0	400	100
Natural resource use	450	0	400	100
Health and social care	650	0	1100	36

Notes. Some of the programs were restructured after the reform. In the cases where other programs were present before GY2011, students from the pre-reform period are mapped into the new programs based on their specializations within the programs as shown in Appendix Table A1. HVAC stands for heating, ventilation and air conditioning.

Figure A1. Correlation between a summer job experience and post-graduation employment probability, full population of vocational graduates

(a) Any job *(b)* Matching jobs



Notes. Media program is excluded from the analysis as it only existed before GY2011. BF - Child and recreation, BA - Building and construction, EE - Electricity and energy, VF - HVAC and property maintenance, FT - Vehicle and transport, HA - Business and administration , HV - Handicraft, HT - Hotel and tourism, RL - Restaurant Management and food, IN - Industrial technology, NB - Natural resource use, VO - Health and social care.

Table A3. *Vocational curriculum before and after GY2011, by program*

Vocational programs	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Core general courses		Core track-specific		Specializations		Program deepening		High school project		Electives		Total	
	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
Child and recreation	750	600	700	700	300	300	350	600	100	100	300	200	2500	2500
Building and construction	750	600	500	400	300	400–900	550	300-800	100	100	300	200	2500	2500
Electricity and energy														
c.1)	750	600	450	400	250	400–500	650	700–800	100	100	300	200	2500	2500
c.2)	750	600	400	400	300–350	400–500	600–650	700–800	100	100	300	200	2500	2500
HVAC and property maintenance	750	600	450	400	250	300–400	650	800–900	100	100	300	200	2500	2500
Vehicle and transport	750	600	500	400	200–550	400–500	300–650	700–800	100	100	300	200	2500	2500
Business and administration	750	600	600	400	450	500	300	700	100	100	300	200	2500	2500
Handicraft	750	600	700	400	0	500	650	700	100	100	300	200	2500	2500
Hotel and tourism	750	600	650	700	400	400	300	500	100	100	300	200	2500	2500
Restaurant management and food														
i.1)	750	600	600	400	0	300–600	X	600–900	100	100	300	200	2500	2500
i.2)	750	600	650	400	300	300–600	400	600–900	100	100	300	200	2500	2500
Industrial technology	750	600	350	400	0	300–400	X	800–900	100	100	300	200	2500	2500
Natural resource use	750	600	450	400	0	300	X	900	100	100	300	200	2500	2500
Health and social care	750	600	650	1100	0	0	700	500	100	100	300	200	2500	2500

Notes. The industrial technology, the natural resource use, and the food program did not have any national specializations before 2011. The specializations were to be designed locally to better meet the local labor market needs. Some of the old programs do not map 1:1 to the new vocational programs. In those cases, the mapping has been conducted based on the specializations within the tracks. See Appendix Table A1 for the description of (c.1), (c.2), (i.1) and (i.2). HVAC stands for heating, ventilation and air conditioning.

Table A4. *Descriptive statistics, vocational graduates 2010–2016*

	(1)	(2)	(3)	(4)	(5)	(6)
	All vocational graduates		Sample 1		Sample 2	
	Mean/ Share	s.e.	Mean/ Share	s.e	Mean/ Share	s.e
Child and recreation	0.087	0.001	0.105	0.002	0.104	0.002
Building and construction	0.127	0.001	0.123	0.002	0.125	0.002
Electricity and energy	0.156	0.001	0.145	0.002	0.144	0.002
HVAC and property maintenance	0.024	0.000	0.031	0.001	0.031	0.001
Vehicle and transport	0.102	0.001	0.103	0.002	0.113	0.002
Business and administration	0.103	0.001	0.158	0.003	0.148	0.002
Handicraft	0.081	0.001	0.035	0.001	0.034	0.001
Hotel and tourism	0.029	0.000	0.034	0.001	0.033	0.001
Restaurant management and food	0.067	0.000	0.080	0.002	0.082	0.002
Industrial technology	0.058	0.000	0.036	0.001	0.036	0.001
Natural resource use	0.077	0.001	0.060	0.002	0.061	0.002
Health and social care	0.091	0.001	0.090	0.002	0.089	0.002
Independent school	0.266	0.001	0.207	0.003	0.206	0.003
Female	0.417	0.001	0.403	0.003	0.392	0.003
Not born in Sweden	0.070	0.000	0.049	0.001	0.047	0.001
Sum of parents' income	526.596	0.582	536.411	2.038	536.235	2.011
Low SES	0.749	0.001	0.776	0.003	0.779	0.003
Compulsory school GPA	186.200	0.093	182.580	0.185	183.289	0.183
High school GPA	12.291	0.006	11.353	0.010	11.466	0.010
GPA of track-specific courses	12.810	0.006	12.182	0.015	12.259	0.014
Passed diploma project	0.877	0.001	1.000	0.000	0.877	0.002
Old diploma	0.580	0.001	0.547	0.003	0.583	0.003
New diploma	0.276	0.001	0.403	0.003	0.394	0.003
New study certificate	0.077	0.001	0.050	0.002	0.023	0.001
Summer job	0.915	0.001	0.913	0.002	0.915	0.002
Matching summer job	0.549	0.001	0.553	0.003	0.561	0.003
Stable job	0.816	0.001	0.844	0.003	0.845	0.002
Stable matching job	0.451	0.001	0.468	0.003	0.471	0.003
Observations	286,933		20,963		21,402	

Notes. HVAC stands for heating, ventilation and air conditioning. Low SES refers to students whose neither parent has obtained tertiary education. Parents income is presented in thousands of SEK. Sample 1 is used to study the effects of stricter general skill requirements. Sample 2 is used to study the effects of stricter specific skill requirements.

Table A5. *Variation of labor market tightness, by program and year*

	2010	2011	2012	2013	2014	2015	2016
Health and social care	0.306	0.276	0.279	0.268	0.266	0.268	0.263
Child and recreation	0.314	0.291	0.281	0.281	0.284	0.286	0.279
Vehicle and transport	0.373	0.356	0.332	0.337	0.343	0.347	0.331
HVAC and property maintenance	0.407	0.380	0.361	0.357	0.358	0.357	0.333
Natural resource use	0.433	0.405	0.397	0.332	0.350	0.343	0.339
Electricity and energy	0.429	0.400	0.380	0.375	0.378	0.373	0.345
Building and construction	0.442	0.414	0.392	0.385	0.387	0.383	0.350
Industrial technology	0.462	0.435	0.392	0.377	0.395	0.384	0.366
Business and administration	0.435	0.409	0.396	0.387	0.385	0.384	0.369
Handicraft	0.481	0.459	0.451	0.440	0.438	0.434	0.416
Hotel and tourism	0.523	0.506	0.498	0.487	0.484	0.483	0.472
Restaurant management and food	0.532	0.513	0.505	0.491	0.488	0.487	0.474

Notes. The table shows the average fraction of new hires from non-employment in the industries related to the programs. For defining the industries, I identify the most common industries where the graduates from pre-reform cohorts ended up working in five years after graduation relying on the Swedish Standard Industry Classification at the two-digit level. Labor market tightness is inversely related to the measure (Manning, 2003), i.e. larger values in the table refer to lower tightness. HVAC stands for heating, ventilation and air conditioning.

Table A6. *Effects of stricter high school graduation requirements on students' educational credentials*

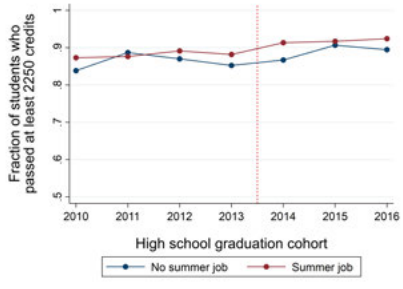
	(1)	(2)	(3)	(4)
	General skill requirements			
	New diploma		New study certificate	
Eligible×Post	0.970***	0.969***	-0.970***	-0.969***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	20,963	20,963	20,963	20,963
R^2	0.951	0.951	0.752	0.753
Mean dep. var.	0.889	0.889	0.111	0.111
	Specific skill requirements			
	New diploma		New study certificate	
Eligible×Post	0.970***	0.968***	-0.970***	-0.968***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	21,402	21,402	21,402	21,402
R^2	0.951	0.951	0.482	0.484
Mean dep. var.	0.945	0.945	0.055	0.055
Cohort FE	Yes	Yes	Yes	Yes
Treatment FE	Yes	Yes	Yes	Yes
HS GPA	No	Yes	No	Yes

Notes. Mean dep. var. stands for the mean of dependent variable for the post-reform cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

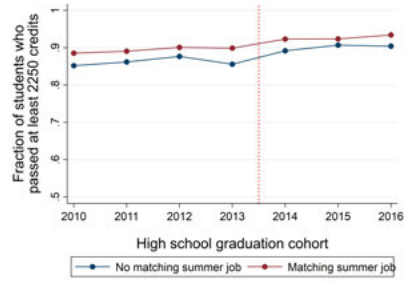
Appendix B. Additional results at the margin of increased general skill requirements

Figure B1. Fraction of students who passed courses for at least 2250 credit points

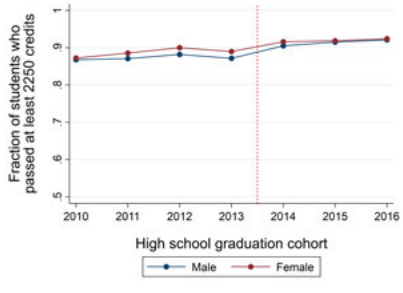
(a) By summer job experience



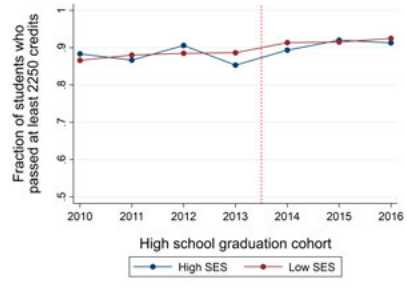
(b) By relevant summer job experience



(c) By gender



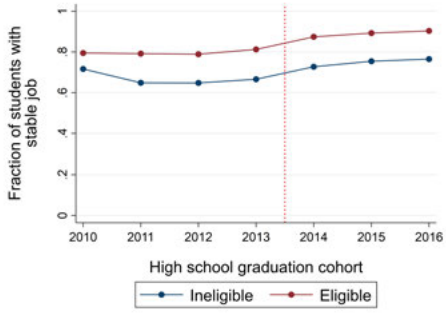
(d) By socioeconomic status



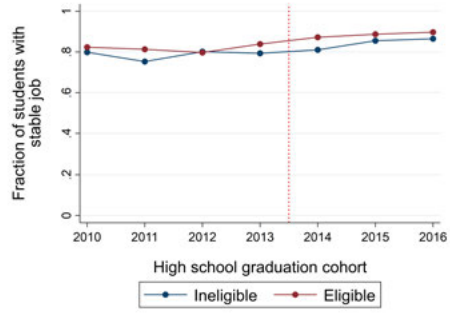
Notes. Vertical dashed line marks the introduction of the new graduation requirements.

Figure B2. Fraction of students with stable employment and stable matching employment among the eligible and ineligible students at the margins of stricter general skill requirements, full population and sample

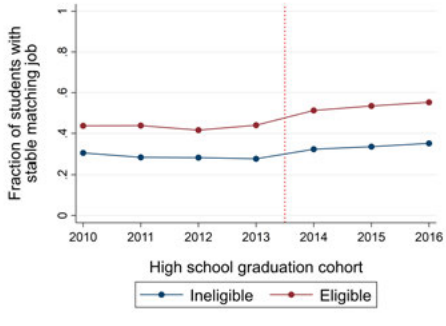
(a) Stable employment - population



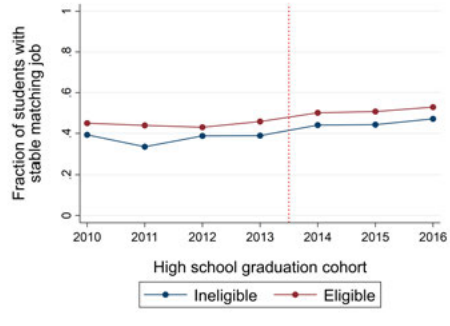
(b) Stable employment - sample



(c) Stable matching employment - population



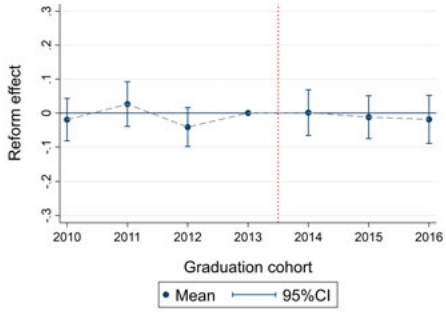
(d) Stable matching employment - sample



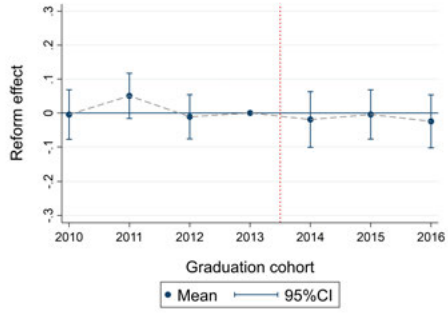
Notes. Vertical dashed line marks the introduction of the new graduation requirements.

Figure B3. Reform effects on employment probabilities, by cohort

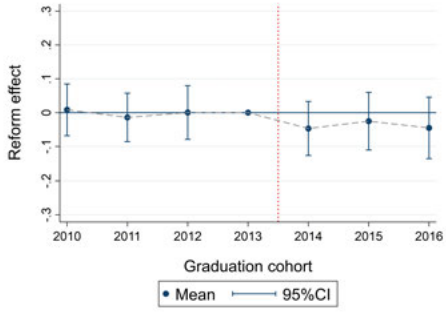
(a) P(stable job)



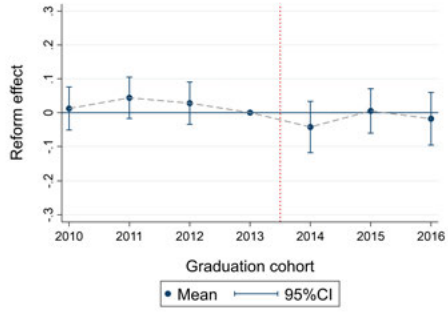
(b) P(stable matching job)



(c) P(stable job>6m)



(d) P(stable matching job>6m)



Notes. Panels C and D show the reform effects on employment probability at jobs that survive the probation period (i.e. the spells that last for longer than six months). The results are estimated by the model $Y_{ipt} = \alpha + \sum_{y=2010, y\neq 2013}^{2016} \beta_t Eligible_s \mathbb{1}\{year = t\} + \delta Eligible_s + \lambda_t + \lambda_p + X'_{ipt}\gamma + \epsilon_{ipt}$. The estimates are adjusted for migration background, labor market tightness, gender, compulsory school GPA and subject grades, high school GPA, indicators for summer job, summer job in a relevant industry, low SES, as well as cohort, treatment, track and LLM fixed effects. Vertical dashed line marks the introduction of the new graduation requirements. Standard errors are clustered at the level of *local labor market* × *high school program*.

Table B1. *Heterogeneity of passing the stricter general skill requirements*

	(1)	(2)	(3)
Dependent variable	Diff-in-diff	Diff-in-diff	Diff-in-diff
Female	-0.008	-0.008	0.001
	(0.008)	(0.008)	(0.008)
Had a summer job	0.011	0.010	0.008
	(0.016)	(0.016)	(0.015)
Had a matching summer job	-0.006	-0.006	-0.017**
	(0.009)	(0.009)	(0.008)
Low SES	0.007	0.007	0.003
	(0.010)	(0.010)	(0.010)
Sum of parents' income	0.001	0.002	-0.002
	(0.006)	(0.006)	(0.006)
CS GPA	-0.038***	-0.039***	-0.024**
	(0.011)	(0.011)	(0.010)
Independent school	-0.022*	-0.021*	-0.013
	(0.011)	(0.011)	(0.011)
Linear time trend	No	Yes	Yes
HS GPA	No	No	Yes

Notes. The results are estimated by the model $Eligible_{ipt} = \alpha + \beta X_{ipt} Post_t + \delta X_{ipt} + \lambda_t + \epsilon_{ipt}$, where $Eligible_{ipt}$ is a dummy variable equal to one for students who meet the stricter requirement and X_{ipt} stands for the variables presented in the first column of the table. Each cell presents a value for β from a different regression. Low SES refers to students whose neither parent has obtained tertiary education. The model with *Independent school* as the variable in focus is estimated on the cohorts of 2011–2016. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B2. *Effects of a placebo reform on various outcomes, vocational graduates of 2006–2012*

	(1)	(2)	(3)	(4)	(5)
Panel A.	*Stable job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.009	0.010	0.000	0.011	214.0
	(0.020)	(0.022)	(0.020)	(0.022)	(328.6)
Observations	11,881	11,881	11,881	11,881	9,408
R^2	0.085	0.101	0.019	0.053	0.152
Mean dep. var.	0.794	0.436	0.268	0.359	13998.8
Gap in outcome	0.016	0.033	-0.006	0.021	-1.2
Panel B.	*Stable matching job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.014	0.023	-0.007	0.030	429.1
	(0.022)	(0.020)	(0.014)	(0.019)	(502.7)
Observations	11,881	11,881	11,881	11,881	4,104
R^2	0.171	0.148	0.037	0.110	0.213
Mean dep. var.	0.322	0.177	0.096	0.148	13737.2
Gap in outcome	0.042	0.031	0.008	0.026	278.5
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
HS GPA	Yes	Yes	Yes	Yes	Yes
No. of passed voc. courses	Yes	Yes	Yes	Yes	Yes
Unbalanced covariates	Yes	Yes	Yes	Yes	Yes

Notes. Placebo reform introduced in 2010. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry, low SES, compulsory school GPA and subject grades. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program.* *** p<0.01, ** p<0.05, * p<0.1.

Table B3. *Robustness to the sample construction*

Panel A.	(1)	(2)	(3)	(4)
	P(stable job)			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	-0.002	0.008	0.010	-0.005
	(0.018)	(0.023)	(0.028)	(0.017)
Observations	20,963	10,980	6,264	23,698
> 10 passing grades	-0.007	0.010	0.003	-0.011
	(0.018)	(0.024)	(0.030)	(0.018)
Observations	19,768	10,242	5,829	22,233
> 12 passing grades	-0.011	0.003	-0.012	-0.014
	(0.018)	(0.024)	(0.032)	(0.018)
Observations	17,339	8,810	4,969	19,363
Panel B.	*P(stable matching job)*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	-0.026	-0.019	0.008	-0.026
	(0.021)	(0.025)	(0.032)	(0.020)
Observations	20,963	10,980	6,264	23,698
> 10 passing grades	-0.019	-0.007	0.016	-0.019
	(0.022)	(0.026)	(0.034)	(0.021)
Observations	19,768	10,242	5,829	22,233
> 12 passing grades	-0.023	-0.008	0.020	-0.019
	(0.025)	(0.029)	(0.036)	(0.024)
Observations	17,339	8,810	4,969	19,363

Notes. Each cell shows results from a different regression run on a sample that is restricted to the total number of passed courses in grade nine as shown in the row title and the total number of passed credit points in high school as shown in the column header. All models include the same set of control variables (see Table 3, column 6). The results in column 4 are estimated based on a sample that consists of both the students who graduated from high school in three or four years, but excludes the students with four years of studies who graduated in 2014. When estimating the effects an additional dummy variable indicating the length of studies is used as a control. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table B4. *Robustness to the sample construction, jobs that last longer than six months*

	(1)	(2)	(3)	(4)
Panel A.	*P(stable job > 6m)*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	-0.038	-0.030	-0.017	-0.036
	(0.025)	(0.030)	(0.036)	(0.023)
Observations	20,963	10,980	6,264	23,698
> 10 passing grades	-0.040	-0.028	-0.021	-0.040*
	(0.025)	(0.031)	(0.039)	(0.024)
Observations	19,768	10,242	5,829	22,233
> 12 passing grades	-0.043	-0.033	-0.027	-0.049*
	(0.026)	(0.032)	(0.041)	(0.025)
Observations	17,339	8,810	4,969	19,363
Panel B.	*P(stable matching job > 6m)*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	-0.041**	-0.036	0.004	-0.044**
	(0.021)	(0.024)	(0.030)	(0.019)
Observations	20,963	10,980	6,264	23,698
> 10 passing grades	-0.036	-0.026	0.010	-0.038*
	(0.022)	(0.026)	(0.031)	(0.020)
Observations	19,768	10,242	5,829	22,233
> 12 passing grades	-0.052**	-0.036	0.004	-0.057**
	(0.024)	(0.028)	(0.033)	(0.022)
Observations	17,339	8,810	4,969	19,363

Notes. Each cell shows results from a different regression run on a sample that is restricted to the total number of passed courses in grade nine as shown in the row title and the total number of passed credit points in high school as shown in the column header. All models include the same set of control variables (see Table 3, column 6). The results in column 4 are estimated based on a sample that consists of both the students who graduated from high school in three or four years, but excludes the students with four years of studies who graduated in 2014. When estimating the effects an additional dummy variable indicating the length of studies is used as a control. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table B5. *Summary of the results on various outcomes*

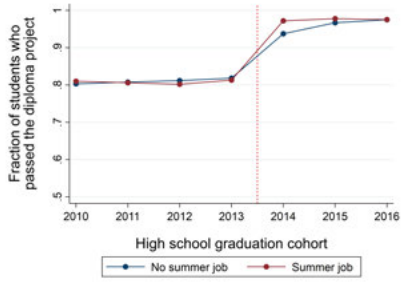
	(1)	(2)	(3)	(4)	(5)
Panel A.	*Stable job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	-0.002	-0.010	-0.001	0.028	246.3
	(0.018)	(0.020)	(0.018)	(0.021)	(309.5)
Observations	20,963	20,963	20,963	20,963	17,701
R^2	0.101	0.119	0.016	0.057	0.156
Mean dep. var.	0.814	0.435	0.283	0.376	15081.8
Gap in outcomes	0.032	0.050	0.003	0.052	325.3
Panel B.	*Stable matching job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	-0.026	-0.023	-0.012	-0.022	-137.2
	(0.021)	(0.017)	(0.016)	(0.018)	(428.0)
Observations	20,963	20,963	20,963	20,963	9,810
R^2	0.216	0.216	0.026	0.141	0.228
Mean dep. var.	0.436	0.253	0.131	0.212	15018.6
Gap in outcomes	0.069	0.065	0.015	0.071	663.6
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
HS GPA	Yes	Yes	Yes	Yes	Yes
No. of passed voc. courses	Yes	Yes	Yes	Yes	Yes
Unbalanced covariates	Yes	Yes	Yes	Yes	Yes

Notes. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry, low SES, compulsory school GPA and subject grades. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program.* *** p<0.01, ** p<0.05, * p<0.1.

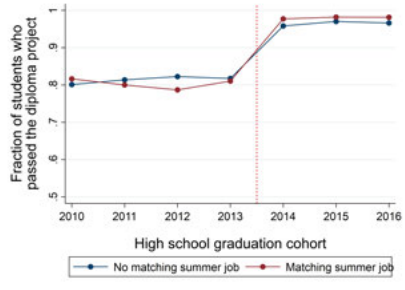
Appendix C. Additional results at the margin of increased specific skill requirements

Figure C1. Fraction of students who passed the diploma project

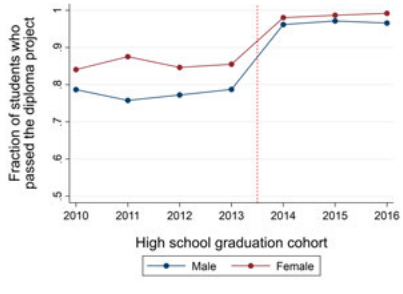
(a) By summer job experience



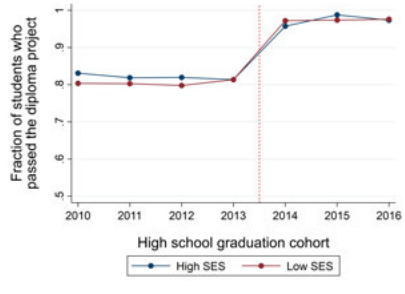
(b) By relevant summer job experience



(c) By gender



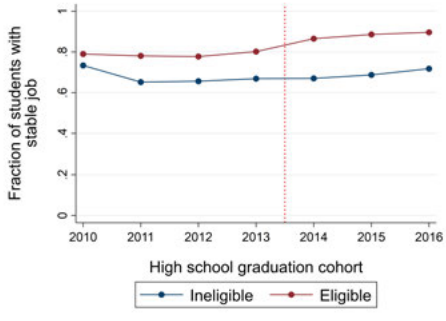
(d) By socioeconomic status



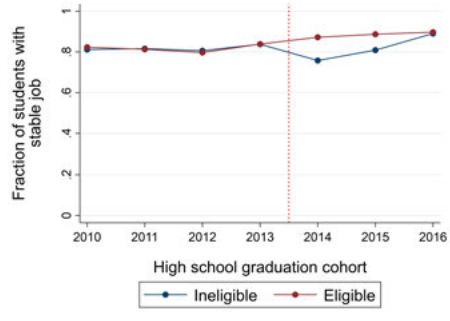
Notes. Vertical dashed line marks the introduction of the new graduation requirements.

Figure C2. Fraction of students with stable employment and stable matching employment among the eligible and ineligible students at the margins of stricter specific skill requirements, full population and sample

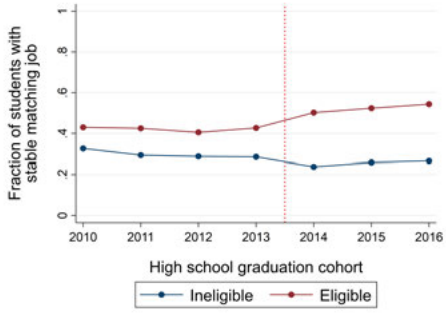
(a) Stable employment - population



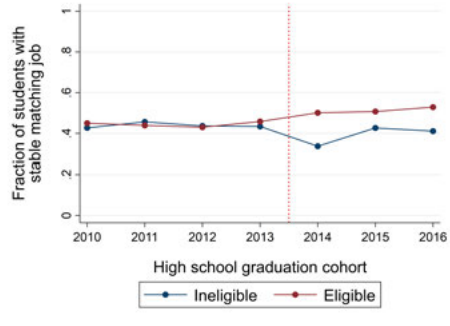
(b) Stable employment - sample



(c) Stable matching employment - population



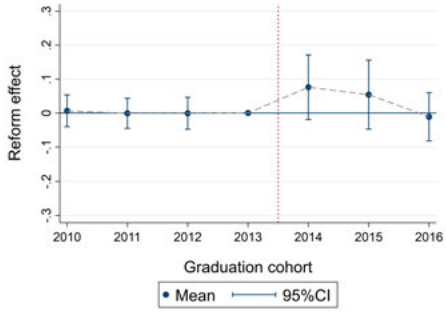
(d) Stable matching employment - sample



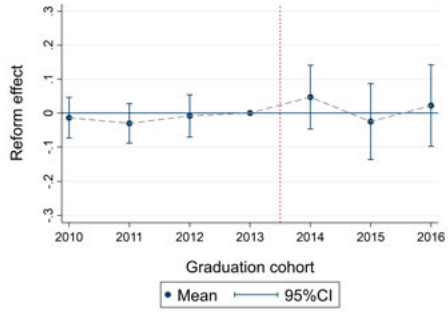
Notes. Vertical dashed line marks the introduction of the new graduation requirements.

Figure C3. Reform effects on employment probabilities, by cohort

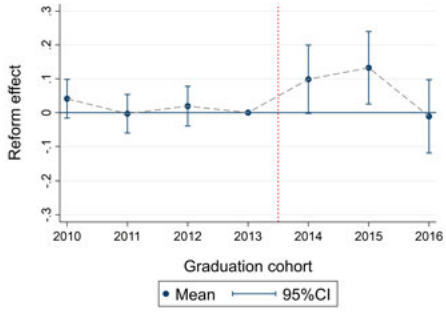
(a) P(stable job)



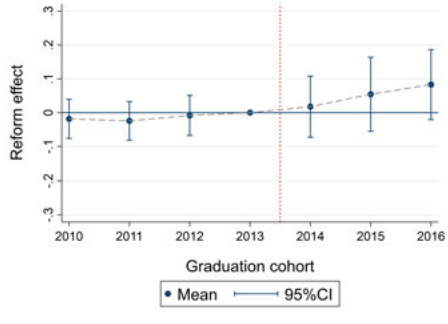
(b) P(stable matching job)



(c) P(stable job>6m)



(d) P(stable matching job>6m)



Notes. Panels C and D show the reform effects on employment probability at jobs that survive the probation period (i.e. the spells that last for longer than six months). The results are estimated by the model $Y_{ipt} = \alpha + \sum_{y=2010, y\neq 2013}^{2016} \beta_t Eligible_s \mathbb{1}\{year = t\} + \delta Eligible_s + \lambda_t + \lambda_p + X'_{ipt}\gamma + \epsilon_{ipt}$. The estimates are adjusted for migration background, labor market tightness, gender, compulsory school GPA and subject grades, high school GPA, indicators for summer job, summer job in a relevant industry, low SES, as well as cohort, treatment, track and LLM fixed effects. Vertical dashed line marks the introduction of the new graduation requirements. Standard errors are clustered at the level of *local labor market×high school program*.

Table C1. *Changes in the composition of eligible students*

Dependent variable	(1) Diff-in-diff	(2) s.e.	(3) Pre-reform mean
Female	0.058**	(0.028)	0.408
Sum of parents' income	-0.007	(0.044)	-0.196
Low SES	0.005	(0.025)	0.783
Compulsory school GPA	0.006	(0.029)	-0.29
Number of passing grades	0.02	(0.105)	13.078
Arts	-0.072	(0.049)	-0.188
Biology	0.05	(0.045)	-0.269
Chemistry	0.02	(0.045)	-0.245
Civics	0.022	(0.038)	-0.29
Crafts	0.034	(0.05)	-0.092
English	-0.063	(0.039)	-0.211
Geography	-0.006	(0.04)	-0.279
History	0.035	(0.04)	-0.293
Home and consumer studies	0.044	(0.044)	-0.183
Mathematics	-0.001	(0.027)	-0.192
Music	-0.001	(0.048)	-0.198
Physics	0.021	(0.044)	-0.244
P.E. and health	0.029	(0.052)	-0.091
Religion	0.037	(0.039)	-0.291
Swedish	-0.008	(0.033)	-0.283
Technology	-0.046	(0.046)	-0.181
No. of voc. courses taken	-26.7	(17.7)	1,104.2
No. of voc. courses passed	-14.4	(18.5)	1,075.9
Summer job	0.049**	(0.023)	0.911
Summer job in a relevant industry	0.162***	(0.035)	0.550
Independent school	-0.028	0.031	0.188

Notes. Results in column 1 are estimated by the model $Y_{ipt} = \alpha + \beta Eligible_s Post_t + \delta Eligible_s + \lambda_t + X'_{ipt}\gamma + \epsilon_{ipt}$, where Y_{ipt} stands for the variables presented in the first column of the table and β-s are the parameters of interest shown in the table. Low SES refers to students whose neither parent has obtained tertiary education. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C2. *Effects of a placebo reform on various outcomes, vocational graduates of 2006–2012*

	(1)	(2)	(3)	(4)	(5)
Panel A.	*Stable job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.008	-0.009	0.023	0.020	-421.1
	(0.019)	(0.025)	(0.021)	(0.022)	(329.1)
Observations	12,037	12,037	12,037	12,037	9,618
R^2	0.086	0.099	0.017	0.055	0.154
Mean dep. var.	0.801	0.443	0.268	0.362	14057.9
Gap in outcome	-0.014	-0.000	-0.008	0.008	-260.7
Panel B.	*Stable matching job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	-0.009	-0.020	0.012	0.004	-287.5
	(0.021)	(0.018)	(0.014)	(0.018)	(440.0)
Observations	12,037	12,037	12,037	12,037	4,277
R^2	0.171	0.151	0.036	0.110	0.230
Mean dep. var.	0.328	0.182	0.096	0.151	13844.3
Gap in outcome	0.013	0.007	0.009	0.012	-240.6
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
HS GPA	Yes	Yes	Yes	Yes	Yes
No. of passed voc. courses	Yes	Yes	Yes	Yes	Yes
Unbalanced covariates	Yes	Yes	Yes	Yes	Yes

Notes. Placebo reform introduced in 2010. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry, low SES, but I also control for the compulsory school GPA and subject grades. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program.* *** p<0.01, ** p<0.05, * p<0.1.

Table C3. *Effects of stricter specific skill requirements on employment at jobs that last longer than six months*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	P(stable job > 6m)					
Eligible×Post	0.104***	0.091***	0.066**	0.099***	0.086***	0.060**
	(0.031)	(0.031)	(0.028)	(0.030)	(0.030)	(0.028)
Observations	21,402	21,402	21,402	21,402	21,402	21,402
R^2	0.044	0.057	0.111	0.055	0.068	0.120
Mean dep. var.	0.616	0.616	0.616	0.616	0.616	0.616
Gap in outcomes	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006
Panel B.	P(stable matching job > 6m)					
Eligible×Post	0.119***	0.101***	0.069**	0.112***	0.094***	0.062**
	(0.032)	(0.032)	(0.028)	(0.032)	(0.031)	(0.028)
Observations	21,402	21,402	21,402	21,402	21,402	21,402
R^2	0.056	0.082	0.184	0.064	0.089	0.190
Mean dep. var.	0.331	0.331	0.331	0.331	0.331	0.331
Gap in outcomes	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	No	No	No	Yes	Yes	Yes
HS GPA	No	Yes	Yes	No	Yes	Yes
No. of passed voc. courses	No	Yes	Yes	No	Yes	Yes
Unbalanced covariates	No	No	Yes	No	No	Yes

Notes. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry and low SES. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table C4. *Robustness to the sample construction, jobs that last longer than six months*

Panel A.	(1)	(2)	(3)	(4)
	P(stable job > 6m)			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	0.060**	0.111***	0.099**	0.068**
	(0.028)	(0.039)	(0.045)	(0.027)
Observations	21,402	10,953	6,306	24,114
> 10 passing grades	0.057**	0.103**	0.086*	0.065**
	(0.028)	(0.042)	(0.048)	(0.028)
Observations	20,285	10,269	5,894	22,731
> 12 passing grades	0.045	0.091**	0.075	0.060*
	(0.031)	(0.045)	(0.056)	(0.031)
Observations	17,895	8,876	5,053	19,935
Panel B.	*P(stable matching job > 6m)*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	0.062**	0.065*	0.034	0.059**
	(0.028)	(0.037)	(0.043)	(0.027)
Observations	21,402	10,953	6,306	24,114
> 10 passing grades	0.061**	0.067*	0.039	0.062**
	(0.029)	(0.038)	(0.046)	(0.028)
Observations	20,285	10,269	5,894	22,731
> 12 passing grades	0.045	0.052	0.007	0.056*
	(0.031)	(0.039)	(0.050)	(0.030)
Observations	17,895	8,876	5,053	19,935

Notes. Each cell shows results from a different regression run on a sample that is restricted to the total number of passed courses in grade nine as shown in the row title and the total number of passed credit points in high school as shown in the column header. All models include the same set of control variables (see Table 3, column 6). The results in column 4 are estimated based on a sample that consists of both the students who graduated from high school in three or four years, but excludes the students with four years of studies who graduated in 2014. When estimating the effects an additional dummy variable indicating the length of studies is used as a control. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table C5. *Robustness to the sample construction*

	(1)	(2)	(3)	(4)
Panel A.	*Stable job*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	0.042	0.058	0.058	0.053**
	(0.027)	(0.038)	(0.042)	(0.027)
Observations	21,402	10,953	6,306	24,114
> 10 passing grades	0.046	0.061	0.068	0.053*
	(0.029)	(0.039)	(0.045)	(0.027)
Observations	20,285	10,269	5,894	22,731
> 12 passing grades	0.025	0.036	0.042	0.034
	(0.029)	(0.042)	(0.050)	(0.029)
Observations	17,895	8,876	5,053	19,935
Panel B.	*Stable matching job*			
No. of passing grades from grade nine	Sample restricted to ... passed credit points 2100–2400	2150–2350	2200–2300	Extended sample
>= 5 passing grades	0.032	0.008	-0.011	0.029
	(0.031)	(0.041)	(0.050)	(0.029)
Observations	21,402	10,953	6,306	24,114
> 10 passing grades	0.027	0.008	-0.003	0.025
	(0.031)	(0.042)	(0.054)	(0.030)
Observations	20,285	10,269	5,894	22,731
> 12 passing grades	0.018	-0.002	-0.025	0.026
	(0.032)	(0.043)	(0.057)	(0.030)
Observations	17,895	8,876	5,053	19,935

Notes. Each cell shows results from a different regression run on a sample that is restricted to the total number of passed courses in grade nine as shown in the row title and the total number of passed credit points in high school as shown in the column header. All models include the same set of control variables (see Table 3, column 6). The results in column 4 are estimated based on a sample that consists of both the students who graduated from high school in three or four years, but excludes the students with four years of studies who graduated in 2014. When estimating the effects an additional dummy variable indicating the length of studies is used as a control. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table C6. *Summary of the results on various outcomes*

	(1)	(2)	(3)	(4)	(5)
Panel A.	*Stable job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.042	0.092***	-0.032	0.076**	577.9
	(0.027)	(0.035)	(0.036)	(0.032)	(471.9)
Observations	21,402	21,402	21,402	21,402	18,084
R^2	0.102	0.117	0.016	0.056	0.152
Mean dep. var.	0.818	0.443	0.281	0.382	15248.8
Gap in outcomes	0.000	-0.006	0.011	0.003	-670.8
Panel B.	*Stable matching job*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.032	0.052**	-0.015	0.042	838.5
	(0.031)	(0.025)	(0.023)	(0.026)	(815.6)
Observations	21,402	21,402	21,402	21,402	10,070
R^2	0.216	0.213	0.027	0.140	0.230
Mean dep. var.	0.443	0.263	0.131	0.221	15267.6
Gap in outcomes	0.005	-0.008	0.013	-0.000	-943.4
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
HS GPA	Yes	Yes	Yes	Yes	Yes
No. of passed voc. courses	Yes	Yes	Yes	Yes	Yes
Unbalanced covariates	Yes	Yes	Yes	Yes	Yes

Notes. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry, low SES, compulsory school GPA and subject grades. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market* × *high school program*. *** p<0.01, ** p<0.05, * p<0.1.

Table C7. *Summary of the results on various outcomes, jobs that last longer than six months*

	(1)	(2)	(3)	(4)	(5)
Panel A.	*Stable job > 6m*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.060**	0.070**	-0.004	0.095***	337.1
	(0.028)	(0.032)	(0.031)	(0.031)	(480.0)
Observations	21,402	21,402	21,402	21,402	14,174
R^2	0.120	0.104	0.014	0.080	0.181
Mean dep. var.	0.616	0.302	0.227	0.337	17883.9
Gap in outcomes	-0.006	-0.009	0.011	-0.009	-885.4
Panel B.	*Stable matching job > 6m*				
	Job finding rate	P(start in the year of graduation)	P(start a year after graduation)	P(spell >18m)	Income
Eligible×Post	0.062**	0.050**	0.008	0.054**	126.0
	(0.028)	(0.023)	(0.020)	(0.025)	(776.3)
Observations	21,402	21,402	21,402	21,402	7,736
R^2	0.190	0.166	0.033	0.145	0.246
Mean dep. var.	0.331	0.185	0.106	0.196	17601.9
Gap in outcomes	-0.003	-0.008	0.005	-0.006	-1104.6
Cohort and treatment FE	Yes	Yes	Yes	Yes	Yes
Track FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
HS GPA	Yes	Yes	Yes	Yes	Yes
No. of passed voc. courses	Yes	Yes	Yes	Yes	Yes
Unbalanced covariates	Yes	Yes	Yes	Yes	Yes

Notes. Dependent variables are given by the column headers. All models include controls for migration background and labor market tightness. The unbalanced covariates are gender and indicators for summer job, summer job in a relevant industry, low SES, compulsory school GPA and subject grades. Mean dep. var. stands for the mean of dependent variable for the pre-reform cohorts. Gap in outcome indicates the difference in outcome variable between the eligible and ineligible students before the reform. Standard errors are clustered at the level of *local labor market × high school program.* *** p<0.01, ** p<0.05, * p<0.1.

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