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The Impact of Upper Secondary School Flexibility on Sorting and Educational Outcomes

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ABSTRACT

This paper estimates the causal impact of an upper secondary curriculum reform in Sweden that increased students' course-taking flexibility in year 2000. In the most popular upper secondary program, it led to a significant decrease in mandatory mathematics requirements. Using administrative Swedish data, we estimate the causal impact of the reform on tertiary education outcomes and expected earnings using a differences-in-discontinuity identification strategy. The method compares students born immediately before and after the cutoff date. The inclusion of students born in neighboring non-reform cutoff years enables us to disentangle the school starting age effect from the unconfounded effect of the reform. We find no negative effects of the reduced mathematics requirements. Rather, we find a positive effect of the reform on students' probability of enrolling in, and earning a degree from, tertiary education. Our heterogeneity analysis suggests that relatively disadvantaged students were not negatively affected by the reform.

1. Introduction

A well-educated labor force in science, technology, engineering, and math (STEM) offers a competitive edge in the global economy. Skills in mathematics and science have been shown to be positively associated with economic growth (Hanushek & Kimko, 2000). Policy makers in industrialized countries have shown great interest in improving the accumulation of such skills through curriculum reforms and better preparation of young individuals for tertiary education.[1] When determining the school curriculum, policy makers make choices regarding the overall time devoted to different subjects and what subjects should be compulsory. These choices reflect priorities and preferences concerning what knowledge and skills should be required and, consequently, there is substantial heterogeneity in curriculum priorities across countries (OECD, 2018).

Herein lies a potential trade-off for the policy maker. More advanced education leads to a higher human capital stock, but enforcing a too strict curriculum might also lead less able students to shy away from further investments in human capital. Critics of a rigid curriculum argue that restricting students choices is undemocratic since mandating a fixed curriculum for all students deprives them of the opportunity to take courses they are interested in and comply with their personal aspirations (Noddings, 2011). On the other hand, under a flexible curriculum, students with potentially high returns to more advanced courses may opt out of those courses and hence reduce their tertiary education prospects. This paper sheds light onto the aforementioned trade-off by examining whether students' academic and labor market outcomes are affected by a reform that introduced a more flexible course system while simultaneously decreasing compulsory mathematics requirements which altered university eligibility.

There is quite little empirical evidence on the returns to different course choices. This is quite surprising since every student need to make these decisions and it has been subject to a vast amount of policy discussions (Altonji, Blom, & Meghir, 2012). When students have the flexibility to choose courses in line with their own interest and skills they may obtain higher grades. A higher GPA has been shown to have a positive impact on students' completion beliefs and aspirations to pursue post-secondary education (DesJardins, Toutkoushian, Hossler, &

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[1] See for example Görlitz and Gravert (2018) investigating reform changes in Germany; Ning (2014), Sosa (2016) and Goodman (2017) investigating reform changes in the U.S. and Joensen and Nielsen (2016) investigating curriculum reform changes in Denmark.

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Chen, 2019; Kunz & Staub, 2016).

Opponents of flexibility would argue that the content of the curriculum dictates eligibility to academic subjects and programs. In US for example, a heavily influential report by Gardner (1983) called for more courses in academic subjects, for example mathematics and science. Inspired by the report, there were many reforms increasing the strictness in course choices and academic content. This literature is centred around the importance of specific courses or subjects included in the course curriculum on subsequent outcomes, with a focus on mathematics and science (Altonji, 1995; Goodman, 2017; Görlitz & Gravert, 2018; Joensen & Nielsen, 2016; Levine & Zimmerman, 1995; Ning, 2014; Rose & Betts, 2004; Sosa, 2016). They commonly refer to reforms introducing a stricter curriculum and find that there are significant returns to advanced courses. Furthermore, as established in the literature on tracking there is a risk that students have problems in anticipating future educational performance (Brunello, Giannini, & Ariga, 2007).

In this paper, we explore a reform that increased flexibility but did so at the cost of decreasing mathematics requirements. The cost of stricter curriculum is the loss of flexibility. A relevant question to ask is whether the positive impact of more mathematics knowledge established in the above cited literature is outweighed by the general increase in flexibility? In this respect, the literature is scarce. The paper most closely related to ours is a working paper by Yu and Mocan (2018). They investigate the causal effect of increased upper secondary school curriculum flexibility on student outcomes. The authors exploit a curriculum reform in China launched in 2004 that increased students freedom when selecting courses. The authors find a positive impact on both students academic achievement at university level and their mental well-being. In contrast to Yu and Mocan (2018), who measure outcomes for a representative sample of students while still in tertiary education, we have access to data on the entire student population in Sweden and are able to follow them up to the age of 27. We also focus our analysis on the importance of less strict mathematics requirements. Our detailed data allows for estimation of distributional effects. From the point of view of the social planner, knowing *where* and *how* in the distribution students react to more flexibility is vital information to ensure equity in educational opportunities. Another contribution is our evaluation of a school reform that has never before been evaluated.

The reform was introduced in all Swedish upper secondary schools in year 2000 and the analysis first examines how it alters students course-taking behavior. Second, we ask whether it has a causal impact on tertiary education outcomes and annual expected earnings. Finally, we examine the distributional effects along the dimension of parents socio-economic status (SES). Leveraging detailed Swedish administrative data, containing the entire population of upper secondary school students, we estimate the causal effect in a regression discontinuity (RD) framework. The identification strategy exploits the discontinuity given by student's birth date since it decides whether he/she started upper secondary school after the new curriculum was introduced in autumn 2000. We compare students born in a 3 months window around the cutoff date, i.e., October 1983-March 1984. The RD estimations cannot disentangle the school starting age effect on outcomes from the true effect of the curriculum reform.[2] To tease out the unconfounded effect on outcomes we follow Carneiro, Løken, and Salvanes (2015) and Bertrand, Mogstad, and Mountjoy (2019) and employ a difference-in-discontinuity (RD-DD) design where we augment the RD regression with students born in October-March in neighboring non-reform cutoff years.[3]

The decrease in mathematics requirements was most prominent on the most popular upper secondary school program in Sweden, the Social Science program.[4] In this program, 25 percent of the previously mandatory mathematics coursework was moved to a list of elective courses (GyVux 1994/97:16; GY2000:16). No such change occurred in any other Swedish upper secondary program. We will focus the empirical analysis within this program as it directly altered the pathways to university for the students due to the fact that the now voluntary mathematics course was pivotal for eligibility to, for example, Business and Economics.

The results show that, in line with the aim of the more flexible curriculum, the reform significantly altered students course-taking behavior. We find a significant and large drop in mathematics attainment across males and females, by approximately 37 percent. The decrease was not offset by an increased enrollment in elective STEM-related courses. Rather, we find that students tend to substitute mathematics with non-STEM electives. However, the drop in mathematics does not lead to a significant impact on the probability of completing tertiary education in a field that requires the pivotal mathematics course. Splitting by gender show a marginally significant increase in mathematics-field for women. Nor do we find an effect on the speed at which students enter tertiary education after graduating from upper secondary school. Taken together, these results suggest that students educational prospects, on average, were not limited by the choice to take less mathematics under the more flexible curriculum. On the contrary, our results suggest that the reform increased students probability of enrolling in tertiary education by 3 percent. Furthermore, the reform led to an increase in the probability of exiting tertiary studies with a degree. Splitting the sample by gender shows that the overall effect was driven by a large and positive impact on females, for whom we estimate a 5.6 percent increase in the probability of earning a tertiary degree. Our results are robust to both the choice of bandwidth and other coinciding school reforms. As the students in our sample are too young to allow us to study actual earnings, we estimate the impact on expected earnings based on field of study and gender.[5] We find a small positive effect of the reform on females annual expected earnings. We propose a possible pathway to mediate the positive impact on tertiary education enrollment: an increase in GPA which increased due to the reform.

Treatment heterogeneity is analysed through the distributional impact of the reform along the dimension of parents socio-economic status (SES). We find no evidence that relatively disadvantaged students were negatively affected by the reform. It rather seems that students in the lowest quartile benefited the most from the more flexible curriculum. However, they did so at the cost of the more advantaged students. This group had a decreased probability of attaining mathematics-related education combined with a lower speed to entering university. Taken together, the results suggest that the latter group did substitute a relatively difficult course for fast and easy education. The opposite is true for the disadvantaged students.

A potential challenge to the identification strategy is posed by the introduction of a new upper secondary program in Sweden, the Technology program, at the same time as the curriculum reform in autumn 2000. The new program could potentially induce a different sample of

[2] In Sweden, a student's school starting year is based on his or her calendar year of birth. The school starting age effect implies that students born in December differ from students born in January regardless of whether the reform was in place or not since school-wise they are one year younger than their January born peers. See for example Black, Devereux, and Salvanes (2011) and Fredriksson and Öckert (2014) regarding the importance of the school starting age effect.

[3] We include students born in 1982–1983, 1984–1985, 1985–1986, 1986–1987 and 1987–1988.

[4] For a more detailed explanation of the Swedish upper secondary program system, see Section 2.

[5] Students are 27 years old in the most recent data and the differential life cycle trajectories in earnings based on study choice are not yet materialized (Bhuller, Mogstad, & Salvanes, 2017). Field of study is coded in detail and contains 116 education categories.

students to enter the Social Science program after the reform. We estimate the probability to enroll in the Social science program by gender.[6] The reform increased the probability of choosing the Social Science program among women. Importantly for the validity of the design, we show that pre-determined observable characteristics are balanced across the cutoff.

The rest of the paper is structured as follows: Section 2 describes the details of the school reform and the institutional framework of the educational system in Sweden, Section 3 presents the identification strategy, Section 4 describes the data, and Section 5 presents and discusses the main results and heterogeneity analysis. In Section 6, we provide a range of robustness tests, and Section 7 concludes the paper.

2. Institutional background

Attending upper secondary school is not required by Swedish law. Nevertheless, after completing nine years of compulsory education in Sweden, most students choose to continue their education in the Swedish upper secondary school system. In 1999 and 2000, approximately 98 percent of all compulsory school graduates entered upper secondary school in the same year (Skolverket, 2000 ; Skolverket, 2001).[7] Without any grade retention or other discontinuities in prior education, students are expected to enter upper secondary school in the autumn semester of the year in which they turn 16 years old and then graduate after three years. Students apply for enrollment in specific upper secondary programs, either within preparatory or vocational tracks. Students are admitted based on their grades from lower secondary school.[8] In year 2000, the number of available national upper secondary school programs increased from 16 to 17 as a Technology program was officially introduced (Skolverket, 2000).

2.1. The upper secondary school reform GY2000

From 1994 to 2011, the Swedish upper secondary school curriculum was regulated by Lpf 94, although an important revision of the existing program structure and curricula was made as part of the GY2000 reform, implemented in year 2000. A main objective of the reform was to increase the share of elective coursework and therefore also the students course choice flexibility, in particular in the Natural Science and Social Science programs, the Swedish government thought that the course plans for these two programs were too rigid (Skolverket, 1998 ; Prop.1997/98:169).

The GY2000 reform increased upper secondary school students course choice, to various degrees, on existing upper secondary school programs. The percentage of upper secondary school credits devoted to mandatory courses decreased while credits devoted to choice based coursework increased mainly through the introduction of a new package of elective courses from which students choose a number of courses to fill a quota of credits (GY2000:19; GyVux 1994/97:17).[9] While all Swedish upper secondary school programs were affected by the reform, this paper focuses on students enrolled in the Social Science program for the main analysis. The Social Science program is the most popular upper secondary program in Sweden and prior to the reform, the government raised concerns about the strict program curriculum. Before the reform social science students had a quota of 190 course credits, corresponding to 8.8 percent of the total credits, to obtain from individual course choices.[10] After the reform the quota of credits to be earned from choice based course work differed between program tracks, ranging from 18 to 24 percent of total credits. With the exception of the course Mathematics C, described below, each school was to decide what electives to offer.

The reason for focusing the analysis on students in the Social Science program is that one implication of the reform was that a full-year course in intermediate mathematics *Mathematics C*, was made elective as opposed to mandatory. That is, the course was moved from the mandatory course list to the package of elective courses (GY2000:16; GyVux 1994/97:16). Swedish media published articles informing about the increase in curriculum flexibility and the new Technology program, yet no information about the changes regarding the Mathematics C course seems to have been dispersed to the public.[11] If students and parents were poorly informed about this change the risk of student sorting based on changes in mathematics requirements at the Social Science program is attenuated. Prior to the reform, student were required to complete three mathematics courses, Mathematics A, B and C, corresponding to approximately 9.3 percent of the total amount of course credits.[12] After the reform, students were required to complete only the A- and B-level courses in mathematics, corresponding to 6 percent of the total amount of credits in the new curriculum. Although each upper secondary school was free to decide what electives to offer, Mathematics C was made an exception, so that after the reform all upper secondary schools were required to include this course in the elective course package offered to students in the Social Science program. The Swedish National Agency for Education (*Skolverket*) deemed mathematics as particularly important for tertiary education since courses in mathematics is a common entry requirement for many university programs (Skolverket, 1998). For example, the intermediate mathematics course Mathematics C is an entry requirement for popular undergraduate programs in business and economics at Swedish universities as well as for other university programs such as those for future architects and real estate agents (UHR, 2016; SACO, 2018).

A second feature of the GY2000 reform was the introduction of a new higher education preparatory program, the Technology program. Prior to the reform, the Natural Science program offered a technical specialization track. The aim of the new Technology program was to increase the supply of available programs for students interested in the natural sciences and technology since the government at the time deemed that the technical orientation within the current Natural Science program was not sufficient to meet the demand from students interested in technology (Prop.1997/98:169). While we are not explicitly interested in the introduction of the Technology program, it may have induced a different sample of students entering the Social Science program after the reform. In Section 3.1, we discuss this challenge for identification more thoroughly and provide evidence in Section 5.1 that the

[6] 90 percent of the students at the Technology program when first introduced were male, see Table A2

[7] Swedish compulsory education is divided into lower primary school (age 7–10), upper primary school (age 10–13) and lower secondary school (age 13–16). The majority of references in this section is available only in Swedish. The reference list for references in Swedish is found in the Online appendix.

[8] The decision is made prior to lower secondary school graduation.

[9] There are specialization tracks within some of the vocational programs that experienced a small decrease in elective coursework. Choice based coursework within the 15 vocational programs made up 15–56 percent of total credits prior the reform and 22–52 percent after the reform.

[10] The same figure applies to the Natural Science program. Within Social science students were offered extra flexibility within two of the available specialization track; Business Administration and Humanities, but no flexibility within the Social science track (GyVux 1994/95:14).

[11] Tidningarnas Telegrambyrå (1999), "FAKTA: NYA GYMNASIESKOLAN", Tidningarnas Telegrambyrå, September 15; Anna Lena Wallström (1999), "Fler valmöjligheter för gymnasieelever", Borås Tidning, September 16, page 14; Inga-Lill Hagberg (1999), "GYMNASIEFÖRSLAG Teknik och miljö nya val", Svenska Dagbladet, September 16, page 4; Tidningarnas Telegrambyrå (1999), "BRÅTTOM ATT VÄLJA TILL FÖRÄNDRAT GYMNASIUM", Tidningarnas Telegrambyrå, November 4; Lena Hennel (1999), "Lårarkritik mot gymnasiereform", Svenska Dagbladet, November 5, page 5 ;Anna Asker(1999), "Nytt teknikprogram ska avhjålpa teknikerbristen", Svenska Dagbladet, December 7, page 30.

[12] Approximately 5 percent for Mathematics A, 2 percent for Mathematics B and 2 percent for Mathematics C (GyVux 1994/95:16).

introduction of the Technology program should not be of significant concern.

3. Empirical strategy

This study estimates the causal average impact of an upper secondary school curriculum reform in autumn 2000 on students' course taking behavior, tertiary education outcomes and annual expected earnings. The identification explores the discontinuity given by students' birth dates as it dictates whether they started upper secondary school when intermediate mathematics was mandatory or not. We compare students born immediately to the right of the threshold, in January 1984 to students born precisely before, in December 1983. To capture the causal impact of the flexible curriculum, α_{RD}, in the limit, individuals born in December 1983 must be identical to children born in January 1984 such that the only difference comes from curriculum regime.

Effectively, we estimate two regressions, one on each side of the threshold:

$$y_i = \delta + \lambda R_{ic} + \gamma f(B_i - c) + \beta f(B_i - c)R_{ic} + \theta \mathbf{X}_{ic} + \pi \mathbf{W}^p_{ic} + \eta_m + v_{ic}. \quad (1)$$

Where y_i is the outcome for student i. Reform exposure, R_i, is and indicator variable equal to 1 if individual i was born in or after January 1984, c, and hence entered upper secondary school in year 2000 when the reform was implemented. Birth month and year, B_i, is normalized around the cutoff such that $c = 0$. α_{RD} is estimated as $\widehat{\lambda}$. Split time trends $f(.)$ are included to allow for different slopes before and after the reform. We include a vector of control variables similar to those used in related work (Kirkeboen, Leuven, & Mogstad, 2016; Malamud & Pop-Eleches, 2010; 2011). Throughout the paper we show results both with and without controls and they are robust to the inclusion of controls.[13] η_m contains controls for municipality fixed effects, the level at which compulsory and upper secondary education is operated in Sweden. Eq. 1 is estimated using a local polynomial regression with a first-order polynomial as suggested by Gelman and Imbens (2018). We use a bandwidth of three months on each side and a triangular kernel since it is shown to be boundary optimal (Cheng, Fan, & Marron, 1997). In practice, the choice of kernel should not significantly alter the results (Lee & Lemieux, 2010).

One concern is that birth month of students is correlated with, for example, educational attainment. Previous research has shown substantial differences in educational achievements depending on month of birth.[14] To account for the effect of school starting age, we follow the identification strategy in Carneiro et al. (2015) and Bertrand et al. (2019) and include cohorts born in neighboring non-reform cutoff years, 1982–1983, 1984–1985, 1985–1986, 1986–1987 and 1987–1988, in order to estimate a difference in regression discontinuity model, the RD-DD.[15] By including the non-reform cutoff years we estimate discontinuities between children born in October-December and January-March. Intuitively, the discontinuity at the cutoff in January 1984 will be a combination of the true effect of the reform and month of birth effects: $\alpha_{RD} = \tau_{reform} + \tau_{b_i}$. Under the assumption that month of birth effects are stable across cutoff years and do not interact with the true reform effect (Carneiro et al., 2015), we can estimate the average discontinuities in outcomes for the five non-reform cutoff years: $\alpha_{RD_{noreform}} = \tau_{b_i}$. By subtracting $\alpha_{RD_{noreform}}$ from α_{RD}, we cancel out the month of birth effect and leave only the true, unconfounded impact of the reform:

$$\alpha_{RD-DD} = \alpha_{RD} - \alpha_{RD_{noreform}} = \left(\tau_{reform} + \tau_{b_i}\right) - \left(\tau_{b_i}\right) = \tau_{reform}$$

The reform effect is thus the difference between the discontinuity in outcomes for students entering school after the reform and the discontinuity for students entering in nearby non-reform years.

3.1. Validity of the RD-DD

We will consistently estimate the impact of reduced mathematics under the crucial assumption that individuals are unable to precisely manipulate the running variable. Use of age-based discontinuities, such as date of birth as the running variable, is common (Lee & Lemieux, 2010), and due to the difference in time between when individuals were born and when they entered upper secondary school, we can be sure that the reform was unknown at the birth date.[16]

Note that we will investigate educational outcomes and annual expected earnings of a restricted part of the full population, namely upper secondary social science students. If the curriculum reform itself, in particular the introduction of the Technology program, caused sorting of students into different upper secondary programs, the comparison between the student samples enrolled in the Social Science program before and after the reform is confounded by selection. As shown in appendix, Table A2, 8 percent of the students born in 1984 enrolled in the new Technology program. To investigate sorting, we estimate the effect of the reform on the probability of enrolling in the Social Science program through estimation of Eq. 1 for a pooled sample and by gender.[17] The plausibility of the RD-DD estimation assumes that unobserved characteristics are similar across the cutoff for the treatment year as well as the control year. We perform a balancing test to ensure that there is no selection on predetermined observables and show the results in Table 4.

A well-known concern with RD-analysis employing a discrete running variable is the fact that there is no continuity at the cutoff. Cattaneo, Idrobo, and Titiunik (2018) recommends supplementing the

[13] Adding controls improves precision and help us reduce any bias due to potential differences in pre-determined characteristics of individuals to the left and right of the cutoff. We add a vector of controls for pre-determined student characteristics $\mathbf{X}_{ic}$, including gender and an indicator variable equal to 1 if the individual obtained a grade of pass with distinction or special distinction in mathematics in lower secondary school. The lower secondary mathematics grade is included as a control for mathematics ability since we hypothesize that this ability is an important determinant of a student's choice of upper secondary courses, in particular whether to substitute the Mathematics C course for another course under the new flexible curriculum introduced as part of the reform. We include a vector of parent characteristics $\mathbf{W}^p_{ic}$, which contains information on whether at least one parent had a low level of education (defined as not having completed three years of upper secondary school), the earnings of the father averaged over age 14–16 of the child, and parents' immigration status (equal to 1 if both parents immigrated to Sweden)

[14] See for example Fredriksson and Öckert (2014) and Black et al. (2011) for good examples of the importance of school starting age.

[15] Stricter entry requirements to upper secondary school programs was introduced in 1998 and hence affecting cohorts born from 1982 onward. The cohort born 1982 was also the first cohort to receive criterion referenced grades when in the 8th grade in elementary school as well as the first cohort that did not receive a course grade in upper secondary school courses if a students absence was high. Therefore, we are not able to include a wider window of non-reform cutoff years.

[16] We include a histogram of the frequency of birth in the relevant years, see Appendix Figure A5. There is a strong seasonality in timing of birth but it is not systematically different across the relevant years.

[17] Boys are more likely than girls to enter the new Technology program. According to Appendix Table A2, the fraction of males in the Technology program was 90 percent in the first cohort after the program was introduced.

analysis with estimations from a local randomization (henceforth LR) framework. The idea is that there exist a window around the cutoff where assignment to treatment can be viewed as a local experiment. When the data is discrete and the cutoff known to the researcher, the smallest window is defined by the two observations closest to the cutoff. Though, as we have discussed previously, comparing children in December and January will yield significant differences irrespective of reform exposure due to the age effect.

Nevertheless, we can make use of the LR-framework to further elaborate on the validity and plausibility of continuity assumption with few mass points.[18] We show Figure A1-A4 in the appendix, containing point estimates from separate RD regressions using a 3 month bandwidth, a 6 month bandwidth and the simple difference in means (the statistics of choice in the Local randomization framework) for women and men respectively. The point estimates are shown for both reform and all non-reform years separately. Regarding the discreteness, the point estimates from the continuity based RD are similar to those adopting the LR framework, simply comparing the difference between the mean of the control (one mass point to the left) and mean of treated (one mass point to the right). This is reassuring since the continuity based approach could mask selection or sorting right at the cutoff but we rule this out due to the similarities between the set of estimators. While the RD and LR estimates are comparable across the years, they are not directly comparable to the main RD-DD results due to the school age starting effect.[19] The latter is the difference in discontinuity between the reform cutoff and the cutoff for the *pooled* control years.

4. Data

We use Swedish registry data provided by Statistics Sweden. Statistics Sweden links several administrative registers by personal identification numbers and we obtain information about individuals' birth month and year, educational attainment, school grades and field of study in upper secondary school as well as in tertiary education. Individuals are linked to their parents (biological or adoptive) and information on the parents' background characteristics. The data set contains the entire population of individuals born in Sweden between January 1982- December 1988 who have completed elementary school. We restrict our main sample to contain first-time enrollees into the Social Science program in upper secondary school.

4.1. Variables

The outcome variables of interest are several measures of tertiary education and annual expected earnings, measured at 27 years as this is the oldest age at which we can observe this information in the dataset. The tertiary education outcomes comprise a set of indicator and discrete

Table 1
Mathematics C choice.

	High Returns	Low Returns
Enroll	(1) +	(2) -
Not enroll	(3) -	(4) +

[18] Note that the three mass points to the left and the right cutoff in our main specification contains information from a large number of students, the efficient number of observations at every cutoff is approximately 4500 to the left and 5800 to the right of the cutoff. The difference is due to the fact that the cohort size is larger in January-March relative to October-December.

[19] As the LR simply compares the mean across the cutoff, we have tested the sensitivity of the RD-estimates using a polynomial of degree 0 instead of 1 for a more similar comparison. We have done the same check for the RD-DD estimates. Neither the RD, nor the RD-DD estimates are sensitive to the choice of polynomial.

Table 2
Summary statistics for the Social Science Program.

	Before reform cohort 1983			After reform cohort 1984		
	Mean	Std. dev	Obs.	Mean	Std. dev	Obs.
Tertiary Education Outcomes						
Math C-field	0.16	0.37	20,721	0.17	0.37	20,704
Speed	2.57	1.27	12,777	2.60	1.28	12,898
Any tertiary education	0.63	0.48	22,487	0.64	0.48	22,277
Degree	0.31	0.46	22,487	0.33	0.47	22,277
Labor Market Outcome						
Annual expected earnings (SEK)	308 330	106 056	22,487	309 996	104 877	22,277
Upper Secondary School						
GPA	14.20	2.86	19,594	14.38	3.10	19,834
Mathematics C enrollment	0.64	0.48	22,185	0.39	0.49	21,973
Mathematics B enrollment	0.93	0.26	22,185	0.93	0.26	21,973
STEM enrollment	0.03	0.17	23,278	0.03	0.18	23,079
Non-STEM enrollment	0.08	0.28	23,278	0.18	0.38	23,079
Background Characteristics						
High math ability	0.43	0.49	23,278	0.43	0.50	23,079
Male	0.37	0.48	23,278	0.36	0.48	23,079
Immigrant	0.12	0.32	21,943	0.13	0.33	21,693
$LowEducation_p$	0.66	0.48	21,759	0.64	0.48	21,522
$LogAvgWage_f$	10.99	3.80	22,140	11.03	3.80	21,927

variables capturing educational attainment on both the extensive and the intensive margin.

For impacts on the intensive margin, we construct an indicator variable, *MaC-field*, which is equal to 1 if an individual has her or his highest attained education in the field of business, economics, architecture or real estate management. Entry to all of these university programs requires prior completion of Mathematics C in upper secondary school.[20] Inclusion of this outcome variable is motivated by its direct dependence on students' mathematics choices in upper secondary school.

Given students' potential returns to mathematics studies, one could roughly define one group of students who *should* (high returns) enroll in the Mathematics C course and one group who *should not* (low returns). A strict, non-flexible, course curriculum ensures that all students with potentially high returns enroll in the course, but it also forces students with low returns to take the course even if they would be better off studying something else; cells (1) and (2) in Table 1. Introducing choice under a flexible curriculum may lead to the desirable outcome that low-return students opt out, i.e., cell (4), while high-return students continue to enroll, i.e., cell (1). If this is the case, we expect no impact of the reform on the outcome variable *MaC-field*. However, introducing choice raises the concern that students with low potential returns who ideally should not enroll in the course continue to do so, i.e., cell (2). An even greater concern is that students with potentially high returns may refrain from taking the course under the flexible curriculum, i.e., cell (3), and forego the eligibility to enter mathematics-intensive post-secondary academic fields they would have pursued absent the reform. Under such circumstances we expect to find a negative impact on *MaC-*

[20] Obviously, there are other university fields, for example in the natural sciences, that also require Mathematics C or more. However, graduating from the upper secondary Social Science program does not make individuals eligible for these fields independent of whether they chose to take Mathematics C. Hence, the course choice is not pivotal for eligibility, in contrast to the fields of study included in *MaC-field*.

field.

We also include a discrete variable, *Speed*, measuring the speed to entry into tertiary education. The variable ranges from 0 to 5. It is equal to 0 if an individual started tertiary education in the same year as she or he graduated from upper secondary school and 5 if she or he started tertiary education five years after completing upper secondary school.[21] We expect to find an impact here if students regret their choices induced by the reform and therefore have to take adult education classes to gain the desired eligibility for certain study fields in tertiary education.

For general tertiary education outcomes, we have constructed the indicator variable *AnyTE*, equal to 1 if the individual ever attended any tertiary education, to capture the impact of the reform on the extensive margin. We further include the indicator variable *Degree*, which is equal to 1 if an individual exited tertiary education with an academic or vocational degree. This variable does not distinguish between the different durations of tertiary education programs needed to earn a certain degree.

Given the time span of our data, the students are too young for us to study actual earnings (Bhuller et al., 2017). Students born in 1988 are at most 27 years old in the most recent data - an age at which the differential life cycle trajectories in earnings based on study choice have not yet materialized. For the full sample we estimate the impact of the reform on expected returns to education, through imputing an outcome variable for an individuals annual expected earnings in middle age.[22] Table 2 summarizes the mean and standard deviation of the main variables for the sample of upper secondary social science students born in the pre- and post-reform years 1983 and 1984, respectively.

Before the reform, 64 percent of social science students took Mathematics C. After the reform, the share shrunk to 39 percent. 63 percent of the students enrolled in any tertiary education both before, relative to 64 percent after the reform. 31 percent of the students who started upper secondary school before the reform went on to complete a higher education degree while the corresponding number figure after the reform is 33 percent. The mean of speed to enter tertiary education is approximately 2.6 years for both groups, which implies that the average student enters tertiary education 2–3 years after graduating from upper secondary school. The fraction of males in the sample is approximately 37 percent before and 36 percent after the reform. The low fraction of males is due to the sample restriction to include only students in the Social Science program, which traditionally has a high share of female students. Background characteristics are similar in both groups.

5. Results

5.1. Sorting

As discussed in Section 2, the reform introduced a third higher education preparatory program. To separate the effect of increased course flexibility from the effect of the introduction of the new program, we must find out whether the sample of students in the Social Science program was similar in terms of background characteristics before and after the reform. We estimate the impact of the reform on the probability of enrolling in the Social Science program, using both the RD and the RD-DD estimator. Recall that the difference between the two is that the RD-DD is augmented with neighboring non-reform years to enable us to subtract a possible month of birth effect from the reform effect. Note that the entire population of potential upper secondary school students is included in this estimation, i.e. all students graduating from 9th grade in high school. We also estimate the regression separately by gender since the new Technology program is strongly male dominated.[23]

Table 3
Probability of enrolling in Social Science.

Social Science	RD	RD	RD-DD	RD-DD
		All		
Reform	0.011***	0.008***	0.013***	0.012***
Standard Error	0.001	0.001	0.002	0.002
Observations	42,288	42,288	268,835	268,835
R^2	0.026	0.063	0.023	0.054
Pre-reform Mean	0.237	0.237	0.237	0.237
		Females		
Reform	0.041***	0.032***	0.027***	0.025***
Standard Error	0.002	0.003	0.005	0.005
Observations	20,636	20,636	131,286	131,286
R^2	0.028	0.052	0.034	0.046
Pre-reform Mean	0.307	0.307	0.307	0.307
		Males		
Reform	-0.013***	-0.012***	-0.000	-0.000
Standard Error	0.003	0.003	0.006	0.005
Observations	21,652	21,652	137,549	137,549
R^2	0.034	0.043	0.023	0.032
Pre-reform Mean	0.170	0.170	0.170	0.170
Controls		✓		✓

The table reports the impact of the reform on the probability of enrolling in the Social Science program for the full universe graduates from 9th grade. The first two columns show the RD regression results using a 3-month bandwidth on each side of the cutoff and a triangular kernel. The discontinuity in outcomes is estimated with a local linear regression with separate trends on each side of the cutoff. We present the RD-DD estimates where we augment the regression with students born in October-March in the neighboring non-reform years 1982–1983, 1984–1985, 1985–1986, 1986–1987, and 1987–1988. The pairwise difference across columns is the inclusion of control variables.

The results in Table 3 reveal that the reform affected the probability of students choosing the Social Science program, at least for females.[24] The pairwise difference across columns is the inclusion of control variables. In the RD-DD specification, the results are robust with respect to inclusion of different controls.

The impact on the probability of choosing the Social Science program among the entire population of female students is increased, as is evident in column 1 to 4. Regarding the male students, the comparison between RD and RD-DD estimates reveals difference between the RD and RD-DD estimates.[25] After the reform, females were on average 2.5 percentage points more likely to choose the Social Science program. In relative terms, the fraction of female students was 8 percent higher after the reform.

The sorting in to the Social Science program may change the sample

[21] We cannot extend the time to more than five years due to data restrictions. However, approximately 50 percent of graduating upper secondary students in Sweden enter university within five years (Holmlund, Liu, & Nordstrom Skans, 2007). Note that this is a lower bound since less than 100 percent of students ever enter university.

[22] We take the average earnings for individuals aged 43–45 in 2015, stratified by gender and detailed information on field of tertiary education. Our data includes 116 detailed tertiary education fields. We impute this value to the individuals in the relevant sample as the annual expected mean income, in Swedish kronor (SEK). We further stratify by level of education, in addition to field and gender, to capture the quantity of tertiary education in a separate measure of annual expected earnings.

[23] As seen in the extended summary statistics in Appendix Table A2, 90 percent of the students in the Technology program were males.

[24] Regression results of the impact of the introduction of the reform on other upper secondary programs than Social Science are presented in Appendix Table A3.

[25] When comparing with the same estimation on the probability of attending Vocational and Natural Science programs, we find similar differences between the RD and RD-DD estimates, see Table A3. We interpret this as evidence of fluctuating probabilities of choosing specific programs for all cutoff years, not exclusively for the reform cutoff.

Table 4
Balancing test of pre-treatment characteristics Social Science.

	$HighMath_i$	$Male_i$	$Loweduc_p$	$Foregin_p$	$\mathrm{Ln}\overline{Earnings}_f$
	(1)	(2)	(3)	(4)	(5)
			All		
RD	0.101***	-0.022*	0.002	0.009***	-0.056*
Standard Error	0.005	0.010	0.011	0.001	0.024
Observations	10,359	10,359	10,359	10,359	10,359
RD-DD	-0.010	-0.022**	-0.005	0.001	0.004
Standard Error	0.012	0.011	0.008	0.003	0.036
Observations	60,026	60,026	60,026	60,026	60,026
Pre-reform Mean	0.412	0.367	0.647	0.113	11.077
			Females		
RD	0.095***		0.012	0.008**	0.104**
Standard Error	0.008		0.010	0.003	0.034
Observations	6,552		6,552	6,552	6,552
RD-DD	-0.008		-0.009	0.004	0.033
Standard Error	0.010		0.012	0.006	0.070
Observations	37,779		37,779	37,779	37,779
Pre-reform Mean	0.437		0.669	0.110	11.06
			Males		
RD	0.117***		0.006	0.014*	-0.306
Standard Error	0.012		0.028	0.007	0.122
Observations	3,807		3,807	3,807	3,807
RD-DD	-0.012		0.003	-0.004	-0.021
Standard Error	0.017		0.013	0.008	0.136
Observations	22,247		22,247	22,247	22,247
Pre-reform Mean	0.368		0.608	0.120	11.106

The table reports the impact of the reform on pre-determined characteristics. In the first panel we present the RD regression results using a three months bandwidth on each side of the cut-off and a triangular kernel. The discontinuity in outcomes is estimated with a local linear regression with separate trends on each side of the cut-off. In the second panel we present the RD-DD estimates where we augment the regression with students born in October to March in nearby non-reform years 1982–1983, 1984–1985, 1985–1986, 1986–1987 and 1987–1988.

before and after the reform. Therefore, it is crucial to address whether the sample selection led to a compositional change among the students enrolled in the Social Science program. For any RD design to be credible, i.e., to separate the treatment effect from any effects of the change in composition, we need to investigate the impact of the reform on pre-determined covariates. To put it differently, even though students cannot manipulate the running variable they can sort themselves into the program. As would manipulation of the running variable, sorting results in predetermined covariates being unbalanced across the threshold. We test the balance of predetermined covariates in Table 4. Note that the sample is different in Table 3 versus Table 4. The latter contains the group of first time enrollees in Social Science students while the former contained all students that finished elementary school.

The results reveals a strong selection on the mathematics grade in lower secondary school.[26] However, from Table 4 it is clear that in our preferred specification, the RD-DD, we have no such selection suggesting that the RD was picking up school starting age effects.[27] We interpret this as evidence of school starting age effects that will confound comparisons of children born in January to children born in December. Henceforth we present only the RD-DD estimates in the main analysis. All corresponding RD-estimates are available upon request.

[26] In Appendix Table A4, we present an additional balancing test of pre-determined characteristics for the full population of upper secondary students where similar discontinuities in the RD-estimates are found.

[27] In particular with respect to controlling for final lower secondary grade in mathematics. For example, McEwan and Shapiro (2008) show that test scores are significantly affected by school starting age.

Besides gender, the results in Table 4 suggest no evidence of a compositional change since the covariates balance before and after the reform. The pairwise comparison shows that the estimates are robust to adding control variables. The probability of being male within the Social Science program is slightly lower after the reform. In sum, besides the change in gender composition, we cannot reject that there was no systematic selection to the Technology program, with respect to the other observable characteristics. However, recent developments in the RD-literature recommends additional robustness checks when basing the analysis on the continuity assumption when the running variable is discrete (Cattaneo et al., 2018). Figure A1 and A2 elaborates on the sensitivity to choice of bandwidth and shows the sensitivity to the assumption of continuity. Overall, the point estimates from three different estimations are similar and not significantly different from each other which provides support for using the local linear approach. Importantly for the validity and credibility of the RD design, there is no evidence of a specific jump at the reform cutoff. The figure clearly shows the non-randomness in mathematics grade across the cutoff but is it similar for the reform and control years.

5.2. Course-taking behavior

Did the increase in course selection flexibility significantly alter social science students course-taking pattern? Table 5 presents the regression estimates corresponding to the descriptive results presented in Fig. 1. The pairwise difference across columns is the inclusion of control variables. Even though the control variables increase R^2, they make little difference to the point estimates.

As is evident from Table 5, the reform did have a significant impact on students course-taking behavior. In particular, there was considerable substitution of Mathematics C after the reform. That is, we find a highly statistically significant post-reform decrease in the fraction of students enrolled in this course.[28] The estimates suggest a decrease by 23.4 percentage points after the reform, approximately equivalent to a 37 percent decrease given the pre-reform enrollment rate of 64 percent. From a policy point of view, one may worry that the overall decrease in mathematics attainment is driven by a relatively larger course substitution among female subjects after the reform. Researchers have started to search for underlying explanations to observed gender differences in choices of a more mathematics/science intensive curricula. The answer may be attributed to a lower taste for competitiveness (Buser, Niederle, & Oosterbeek, 2014) and/or lower valuation of own mathematics abilities among girls (Rapoport & Thibout, 2018). As previously mentioned the mathematics C course is pivotal for a number of study fields in tertiary education with high earning prospects. Therefore, heterogeneous responses in course selection across gender after the reform could potentially induce/exacerbate a gender gap in these mathematics related fields. However our point estimate differs only marginally across genders: compared with the baseline, females were 36 percent less likely to take Mathematics C after the reform; the corresponding decrease for males was 37 percent. Adding controls for pre-determined characteristics in column 4 barely affects the magnitude of the coefficient estimates.

To ensure that this drop is not driven by a general decline in mathematics attainment, we also estimate the impact on the preceding math course, Mathematics B. We do not find any changes on the preceding mathematics course. More course choice flexibility did not increase students probability of enrolling in STEM related courses. Instead, under the flexible curriculum, students chose to enroll in non-STEM elective such as arts and humanities and media. We estimate a 8.1 percentage point increase in the probability of enrolling in non-STEM electives after

[28] Our register data contains a complete list of grades from each course in upper secondary school and we define attainment as having a grade from Mathematics C. The actual grade does not matter, so students who received a failing grade for the course are still defined as having attained the course.

Table 5
Course-taking behavior.

	MaC (1)	MaC (2)	MaB (3)	MaB (4)	STEM (5)	STEM (6)	non-STEM (7)	non-STEM (8)
				All				
RD-DD	-0.237***	-0.234***	-0.001	-0.001	0.004	0.004	0.082***	0.081***
S.E.	0.004	0.004	0.012	0.011	0.003	0.003	0.005	0.005
Observations	57,668	57,668	57,668	57,668	60,026	60,026	60,026	60,026
R^2	0.095	0.201	0.022	0.056	0.044	0.047	0.093	0.100
Pre-reform $\bar{y}$	0.641	0.641	0.925	0.925	0.035	0.035	0.095	0.095
				Females				
RD-DD	-0.227***	-0.226***	-0.002	-0.001	0.006*	0.006*	0.083***	0.082***
S.E.	0.012	0.012	0.004	0.004	0.003	0.003	0.006	0.006
Observations	36,436	36,436	36,436	36,436	37,779	37,779	37,779	37,779
R^2	0.089	0.198	0.025	0.060	0.044	0.046	0.105	0.111
Pre-reform $\bar{y}$	0.628	0.628	0.925	0.925	0.030	0.030	0.095	0.095
				Males				
RD-DD	-0.250***	-0.245***	-0.002	-0.001	0.002	0.002	0.080***	0.080***
S.E.	0.017	0.013	0.010	0.010	0.005	0.005	0.009	0.010
Observations	21,232	21,232	21,232	21,232	22,247	22,247	22,247	22,247
R^2	0.120	0.220	0.032	0.061	0.066	0.071	0.093	0.097
Pre-reform $\bar{y}$	0.663	0.663	0.924	0.924	0.045	0.045	0.094	0.094
Controls		✓		✓		✓		✓

The table reports the impact of the reform on enrollment in selected courses: Mathematics C, Mathematics B, STEM courses (i.e., courses in science, technology, engineering, and mathematics), and non-STEM courses. We present the RD-DD estimates where we augment the regression with students born in October-March in the neighboring non-reform years 1982–1983, 1984–1985, 1985–1986, 1986–1987, and 1987–1988. The discontinuity in outcomes is estimated with a local linear regression with separate trends on each side of the cutoff, using a 3-month bandwidth on each side of the cutoff and a triangular kernel. The pairwise difference across columns is the inclusion of control variables.

the reform.[29] From Table 5, we conclude that the students experienced a large decrease in mathematics attainment and, importantly, this was not compensated by selecting other STEM-related courses.

5.3. Tertiary education outcomes and expected earnings

Next, we proceed to estimate the impact of the reform on tertiary educational outcomes and annual expected earnings for students in the Social Science program.

Table 6 presents the impact of the schooling reform on tertiary education outcomes. The RD-DD approach enables us to disentangle the school starting age effect on y_i from the reform effect under the mild assumption that school starting age effects are constant across the neighboring cutoff years (Carneiro et al., 2015). This assumption cannot be explicitly tested but we complement the analysis with an RD regression identical to Eq. 1 for all of our control years.[30] For women, the point estimates on tertiary education variables for pre- and post-reform control years have similar magnitudes. For men the control cutoff prior to the reform has a slightly higher point estimate for the probability of taking a field requiring Mathematics C, relative to the control cutoffs after the reform. However, this leads to an underestimation of the magnitude of the RD-DD estimate.

We estimate no impact of the reform on the students probability of choosing a field in tertiary education that requires Mathematics C for eligibility (columns 1 and 2) for the pooled sample. We find a marginal increase in this probability for women, by 1 percentage points. The result indicates that students opting out of Mathematics C under the flexible curriculum were students who would not have continued their academic career in fields where the course is pivotal under a strict curriculum where the course was mandatory. The positive impact on females showed that some women even continued with higher education in math-related fields which they would not have done prior to the reform. Nor do we find a clear effect on the speed of entering higher education after graduating from upper secondary school (columns 3 and 4). We hypothesized that we would find an impact on these two variables if students opted out of mathematics under the flexible curriculum and then regretted their choice when transferring to the tertiary education cycle. However, the results do not support this hypothesis.

We find a positive and statistically significant effect of the reform on the probability of attending tertiary education (columns 5 and 6). The estimated size of the effect is robust to the inclusion of controls. The control variables are included for two reasons: first, to increase precision, and secondly, they allow us to assess the possible presence of a sorting bias based on observable characteristics. Adding the control variables increases the explained variation in outcomes, R^2, but does not significantly alter the magnitude of the point estimates. If females and males responded differently to a more flexible curriculum and/or to the larger share of female peers in the program after the reform, the estimates are expected to differ. We estimate, on average, a 1.9 percentage point increase in the probability of enrolling in tertiary education, which is equivalent to a 3 percent increase given the pre-reform mean of 63 percent. We lose precision when we split the sample by gender. The estimated coefficient suggest a positive impact of the reform on the probability to enroll in tertiary education for both women and men. However, the effect is only precisely estimated in our female sample. Relative to the baseline, the reform induced a 2.5 percent increase in the probability of enrolling in tertiary education for females.

The reform also led to an increase in the probability of exiting tertiary education with a degree (columns 7 and 8). Here, we can conclude that this increase is entirely driven by women, and after the reform, females were 5.6 percent more likely to exit the tertiary education cycle with a degree.

Taken together, the results regarding the effect of the reform on tertiary education outcomes show a positive impact on students probability of entering tertiary education. The increase in tertiary education enrollment translates into a higher fraction of students earning a degree.

[29] Note that STEM and non-STEM are a subset of *electives* offered at the majority of schools. Core content is not included in any of these categories. see Table A1 for a lengthier discussion on what is included in these two categories.
[30] The RD estimate per control year is plotted in Appendix Figure A3 and A4 for women and men respectively.

Table 6
Tertiary education outcomes.

	MaC-field (1)	MaC-field (2)	Speed (3)	Speed (4)	Any TE (5)	Any TE (6)	Degree (7)	Degree (8)
				All				
RD-DD	0.001	0.003	0.025	0.028	0.020	0.019*	0.014*	0.012**
Standard Error	0.004	0.004	0.028	0.028	0.013	0.011	0.007	0.006
Observations	53,555	53,555	34,038	34,038	58,126	58,126	58,126	58,126
R^2	0.015	0.040	0.026	0.048	0.020	0.112	0.020	0.079
Pre-reform Mean	0.162	0.162	2.591	2.591	0.626	0.626	0.306	0.306
				Females				
RD-DD	0.009	0.010*	0.055*	0.052	0.016**	0.017***	0.019***	0.020***
Standard Error	0.006	0.005	0.032	0.032	0.008	0.006	0.006	0.007
Observations	34,422	34,422	23,465	23,465	36,553	36,553	36,553	36,553
R^2	0.016	0.034	0.032	0.048	0.023	0.091	0.026	0.059
Pre-reform Mean	0.151	0.151	2.524	2.524	0.683	0.683	0.359	0.359
				Males				
RD-DD	-0.013	-0.010	-0.042	-0.036	0.020	0.023	-0.001	0.001
Standard Error	0.009	0.009	0.055	0.057	0.025	0.023	0.013	0.012
Observations	19,133	19,133	10,573	10,573	21,573	21,573	21,573	21,573
R^2	0.039	0.064	0.042	0.059	0.033	0.110	0.025	0.061
Pre-reform Mean	0.181	0.181	2.747	2.747	0.527	0.527	0.214	0.214
Controls		✓		✓		✓		✓

The table reports the impact of the reform on tertiary education outcomes. We present the RD-DD estimates where we augment the regression with students born in October-March in the neighboring non-reform years 1982–1983, 1984–1985, 1985–1986, 1986–1987, and 1987–1988. The discontinuity in outcomes is estimated with a local linear regression with separate trends on each side of the cutoff, using a 3-month bandwidth on each side of the cutoff and a triangular kernel. The pairwise difference across columns is the inclusion of control variables.

Our results show that the impact is largely driven by a positive impact on females who are significantly more likely to both enter tertiary education and exit tertiary education with a degree after the reform. With respect to males, we have too low precision to make a conclusion about the effect of the reform on their probability to enroll in tertiary education. However, our results show no impact of the reform on their probability to earn a degree. Hence, our results may signal that females benefit more than males from a flexible curriculum and/or from being in an even more female-dominated group of peers.

In Figure A3 and A4 we show that the results are robust to using estimates with a large bandwidth of 6 months as well as taking the continuity-based assumption into account by adopting the LR framework and show estimates from difference in means comparisons. After the introduction of a new grading system, first applied for the cohort born in 1982, there is a trend of teachers using higher grades in lower secondary school in the first years after introducing the new grading system.[31] This results in a distributional shift both at the 82/83 and 83/84 cutoffs which can be seen in Figure A5. As a robustness check, we re-run the results using only the pre-treatment cutoff 82/83 as a control cohort since they experience a similar shift in distribution but are unexposed to the curriculum reform. The results are shown in Table A5 in the Appendix. The magnitude of the estimated coefficients are in general slightly larger using the restricted control group but the main results remain unchanged. Out of all estimations, only one estimate change from being insignificant using the pooled control years to significant using the pre-treatment control year only.[32] The results are also robust to excluding two municipalities from the analysis, due to a reform change in admission rules to oversubscribed upper secondary schools in a handful of municipalities.[33] In Table A6 we exclude the affected municipalities, Stockholm and Malmö. Since these are two large municipalities, the exclusion leads to lower precision. Overall, the results are qualitatively similar to the main results presented in Table 6.

We proceed by estimating the impact of the reform on expected earnings in middle age.[34] In the two first columns of Table A7 annual expected earnings are based on gender and field of tertiary education studies. We find a modest increase in expected earnings by field for women by approximately 1.2 percent, significant at the 5 percent level.

5.4. Treatment heterogeneity

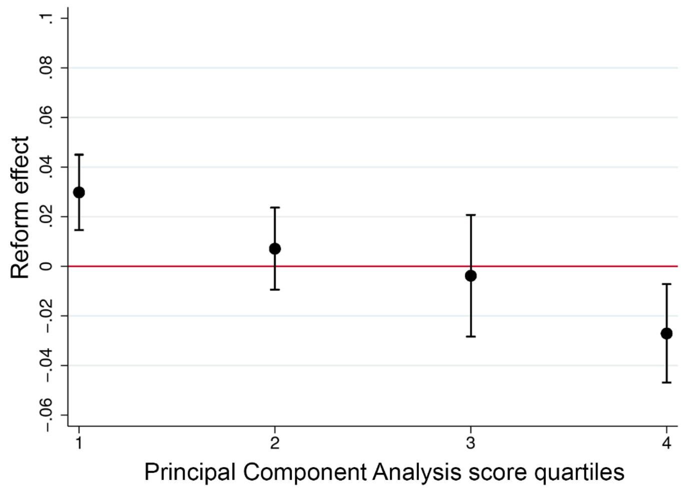
Existing research finds a strong and robust association between an individuals educational outcomes and parents SES (Björklund & Salvanes, 2011). According to Björklund and Salvanes (2011) parents' location in the SES distribution may affect a child's educational outcomes through differences in parents' choice of investments in child education and the quantity and quality of information provided to the child about educational prospects. Educational policies and school reforms have the capacity to reduce or reinforce the association between family background and students' educational outcomes and earnings. To investigate the distributional impact of the reform on students educational outcomes along the dimension of SES, we construct an index

[31] See Holmlund et al. (2014) for a thorough analysis of the Swedish school reforms in 1990s. Major reforms include 1991: Municipalities take over the main responsibility over schools "The municipalication of Swedish schools"; 1992: Charter School reform 1992–1995: Swedish upper secondary school get 16 national programs and one additional program for students who do not get into any of the national programs because of bad grades; 1994: New curriculum (Lpf 94) and criterion referenced grades in upper secondary school; Possibility to choose another school than the one closest to a students home; 1995: New curriculum in elementary school (Lpo 94); 1996: Criterion references grades in elementary school; 1998 Stricter entry requirements to enter upper secondary school.

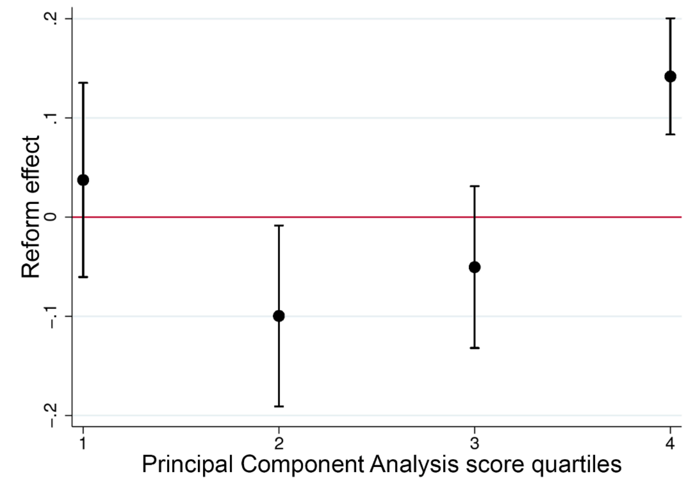
[32] The probability of having higher education in a mathematics related field becomes significantly lower for boys after the reform.

[33] Söderström and Uusitalo (2010) studies the impact of this reform in the municipality including the capital of Sweden, Stockholm, and find that the new admission rule increased sorting in inner city schools. Molin (2019) expands the analysis and finds that the admission reform changed the socio-demographic composition of students only in two municipalities: Malmö and Stockholm.

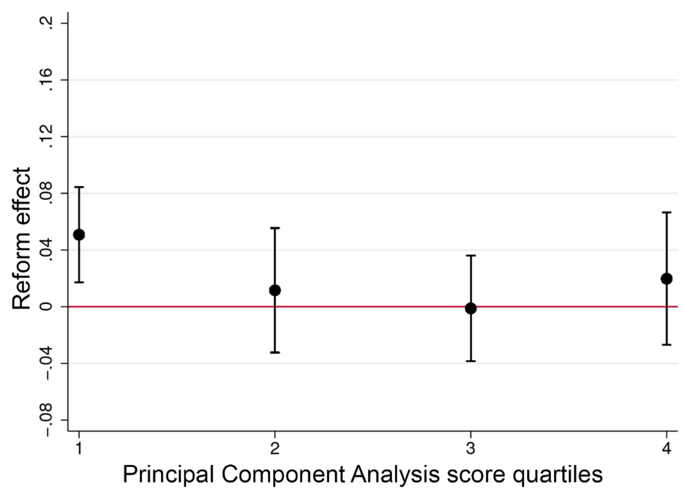
[34] Earnings are measured in 2015 values and the exchange rate per December 31, 2015.



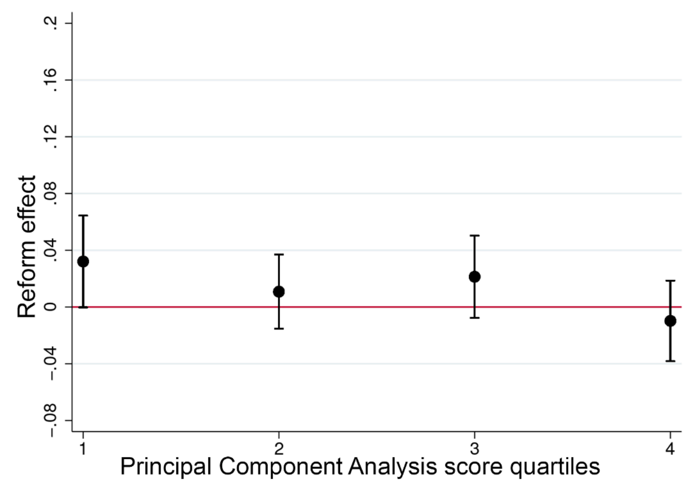
(a) MaC-field



(b) Speed to Tertiary Education



(c) Any Tertiary Education



(d) Degree

Fig. 1. Educational Outcomes by Socio-Economic Status Quartile The figure show the reform coefficient per quartiles from the baseline RD-DD specification with a 3 month bandwidth. The upper and lower bound is calculated at the 90 percent level of significance. The quartiles are based on the distribution of a SES-index constructed based on principal components.

based on a principal component analysis.[35] The results presented in Fig. 1 suggest some treatment heterogeneity on outcomes based on parents' SES.

There are no clear patterns in the heterogeneity, the exception being a negative trend with regard to the probability of enrolling in a university program with Mathematics C as an entry requirement. Somewhat surprisingly, our estimates show a negative impact in the higher part of the SES distribution while the impact is positive in the lower quartiles. The most advantaged students have a reduced probability of attending aforementioned programs, combined with a lower speed to tertiary education. This group seem to have opted out of mathematics C and substituted more difficult programs with fast, and easier ones. Somewhat surprisingly, in the first quartile, the magnitude of the effect is sizeable. We estimate a 2.4 percentage point increase in the probability of choosing a post-secondary academic field that requires Mathematics C, equivalent to an increase of 19 percent given the low baseline of 12.4 percent.[36] The lowest SES quartile also drives the increase in the probability of pursuing any tertiary education and the probability of earning a degree. We conclude that low SES students did not fare worse after the reform. Instead, these students are found to benefit the most from the flexibility induced by the reform. On the other hand, the drop in mathematics requirements led the most advantaged students to substitute the relatively difficult education for fast and easy education.

5.5. Possible mediator

One possible pathway to the higher enrollment in tertiary education is through an effect on students upper secondary GPA. For example, Graetz, Öckert, and Nordström Skans (2020) employ Swedish register data to estimate students' responses to higher scholastic aptitude test (SAT) and find that higher SAT scores causally increase enrollment and graduation rates, especially among low SES students. Receiving a higher GPA in upper secondary school may also have a positive effect on students' level of aspirations for tertiary education as well as subjective completion beliefs (DesJardins et al., 2019; Kunz & Staub, 2016). Such completion beliefs has further been shown to be predictive of actual completion of tertiary education (Kunz & Staub, 2016). Hence, students receiving a higher GPA after the reform may also mediate some of the positive impact of the reform on the estimated probability of earning a degree from tertiary education. Prior to the reform, more than a fifth of all social science students failed the course, and we take this as evidence of the course being particularly difficult for this group of students. A failing grade naturally decreases a students grade point average (GPA) when applying for tertiary education. Hence, after the reform, students had the option to replace Mathematics C with a course from which they expected to receive a higher grade and thus boost their overall GPA. In general, more overall flexibility in course selection may induce students to act strategically by taking relatively simpler courses or choose courses based on innate ability and preferences. Either of these behavioral responses can be expected to increase students overall GPA and make them more competitive in the tertiary education application process.

We estimate the impact of the reform on individual students final upper secondary school GPA. This is a continuous variable ranging from 0 to 20. During the period of interest, Swedish upper secondary school students received the grade *fail, pass, pass with distinction* or *pass with special distinction*. If students received a passing grade in all courses, they obtain a final upper secondary GPA of 10. The regression results presented in Table 7 confirm a post-reform increase in average GPA.[37] From the RD-DD estimation, we find an approximate 0.34 increase in average GPA. In terms of magnitude, this is approximately equivalent to replacing a grade of pass in Mathematics C with a grade of pass with special distinction in another course. The point estimate is similar for males and females but females come from a slightly higher baseline, resulting in a marginally lower relative increase in GPA. Column 3 and 4 are restricted to students that graduated from the Social Science program (as opposed to first time enrollment). The results are similar when focusing on students that did not change program.

Table 7
Grade point average.

	GPA (1)	GPA (2)	GPA (3)	GPA (4)
		All		
RD-DD	0.348***	0.343***	0.378***	0.390***
Standard Error	0.110	0.081	0.067	0.038
Observations	54,511	54,511	42,490	42,490
Pre-reform Mean	14.134	14.134	14.296	14.296
		Females		
RD-DD	0.368***	0.373***	0.344***	0.393***
Standard Error	0.103	0.085	0.109	0.096
Observations	34,843	34,843	27,711	27,711
Pre-reform Mean	14.534	14.534	14.648	14.648
		Males		
RD-DD	0.270	0.308**	0.401**	0.416***
Standard Error	0.174	0.149	0.174	0.151
Observations	19,668	19,668	14,779	14,779
Pre-reform Mean	13.371	13.371	13.584	13.584
Controls		✓		✓

The table reports the impact of the reform on students grade point average. We present the RD-DD estimates where we augment the regression with students born in October-March in the neighboring non-reform years 1982–1983, 1984–1985, 1985–1986, 1986–1987, and 1987–1988. The discontinuity in outcomes is estimated with a local linear regression with separate trends on each side of the cutoff, using a 3-month bandwidth on each side of the cutoff and a triangular kernel. The pairwise difference across columns is the inclusion of control variables.

6. Conclusion

A rigid and non-flexible curriculum regime provides a tool for policy makers to ensure a desirable level of human capital accumulation and keep the level of acquired knowledge fixed among students, especially within growth enhancing STEM skills (Hanushek & Kimko, 2000). However, it also denies individuals the freedom to take courses they are interested in and that are in line with their personal aspirations. Ultimately, ignoring individual heterogeneity may even cause less able students to shy away from further education.

Our paper contributes to understanding this curriculum trade-off by exploring an upper secondary school reform in Sweden implemented in year 2000. A main feature of the reform involved an increase in the share of elective course work. However, it did so at the cost of reduced

[35] See Appendix Table 8 for details on the construction of the index.

[36] See Appendix Table A9.

[37] One concern is the possibility that GPAs trended upwards due to factors unrelated to the reform, e.g., grade inflation. While we cannot assess such inflation concerns by looking at upper secondary school GPAs, we plot the distribution of lower secondary GPAs in Figure A6 for the 1982–1988 cohorts. Except for the changes in the distribution for early cohorts, discussed in Section 5.3, there is no clear evidence of grade inflation and we conclude that the increase in upper secondary school GPA was mainly driven by substitution of courses.

mandatory mathematics course load in the most popular upper secondary school program, the Social Science program.

Using detailed register data, we provide evidence that students course-taking behavior changed after the reform. In particular, mathematics attainment experienced a sharp and robust decrease while enrollment in elective courses in non-STEM fields increased dramatically. Our results show that female and male students discard mathematics at a similar rate after the reform. Among neither females nor males was the decrease in mathematics attainment compensated by an increase in STEM-field related coursework.

We estimate a positive causal impact of the reform on students probability of ever enrolling in tertiary education, an increase of 3 percent. The positive impact on Social Science students enrollment in tertiary education translates into an increase in the probability of students exiting tertiary education with a degree. Estimating the effect by gender shows that the positive impact on the probability of earning a degree was driven by a large and positive impact for females. Interestingly, a marginally significant effect for women and no impact for men is found on the probability of having the highest degree in a relatively mathematics-intensive field. Nor does the reform affect the speed of students entering tertiary education after graduating from upper secondary school, on average. However, the average outcome masks the distributional effects of the reform. Our heterogeneity analysis reveals that relatively disadvantaged students (measured along a socio-economic status index) were not negatively affected by the curriculum reform. Rather, students in the lowest SES quartile seem to have benefited the most from the more flexible curriculum and have a large increase of 19 percent in the probability of entering a mathematics intensive program. On the other hand, the most advantaged students had a reduced probability of attending the same program as well as a lower speed to enter tertiary education. To the extent that majors in Business and Economics give relatively higher earnings, this group were harmed by the reform.

We provide evidence that the decreased required course load in mathematics and the increase in GPA can explain part of the increase in transmission from upper secondary school to tertiary education. The increase in GPA is in line with results found in Yu and Mocan (2018), the paper most closely related to our work, explicitly investigating curriculum flexibility. They, too, find a positive impact on GPAs when students in China were given more course choice flexibility.

Our results are informative for policy makers speculating about the optimal level of flexibility and mathematics content. Increasing flexibility had a positive impact on academic outcomes. The decline in mathematics attainment lead relatively more disadvantaged students in particular to choose more advanced programs than their peers. In particular, the most advantaged students were negatively affected by the reform in terms of chosen programs in higher education. As such, the reform possibly lead to a dismantling of the socio-demographic gradient in educational attainment.

Conflict of Interest

Declarations of Interest: none

CRediT authorship contribution statement

Andrea Berggren: Conceptualization, Methodology, Software, Data curation, Visualization, Formal analysis, Writing - original draft, Writing - review & editing. **Louise Jeppsson:** Conceptualization, Methodology, Software, Data curation, Visualization, Formal analysis, Writing - original draft, Writing - review & editing.

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Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2021.102080

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