




Early exposure to foreign language training and students' educational trajectories[☆]

Maurizio Strazzeri^a, Enzo Brox^b , Chantal Oggenfuss^c, Stefan C. Wolter^b ,^{*}

^a Bern University of Applied Sciences, Hallerstrasse 10, CH-3012 Bern, Switzerland

^b University of Bern, Department of Economics, Schanzeneckstrasse 1, CH-3001 Bern, Switzerland

^c Swiss Coordination Centre for Research in Education, Entfelderstrasse 61, CH-5000 Aarau, Switzerland

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ABSTRACT

We estimate the impact of a large curriculum reform in Switzerland that substantially increased the share of foreign language classes in compulsory school on students' subsequent educational choices in upper secondary school. Our analysis focuses on students from German-speaking cantons that introduced English as the first foreign language. Using detailed student register data and exploiting the staggered implementation of the curriculum reform, we find that exposure to more foreign language classes has only minor effects on educational trajectories of the overall student population. However, we find substantial effect heterogeneity: while the reform has no effect on the direct educational progression of low-track female or high-track students, it impedes low-track male students' transition to upper secondary education.

1. Introduction

School curriculum reforms are often considered the primary tool for educational policy makers to influence the skill mix of the future workforce (OECD, 2020). Changes in skill demands resulting from, e.g., skill-biased technological progress (Acemoglu & Autor, 2011; Autor et al., 2003; Katz & Murphy, 1992) or increased competition in globalized markets (Dauth et al., 2014, 2017; David et al., 2013) call on policy makers to regularly review and adjust school curricula.

Prior research suggests that school curriculum reforms that change course requirements or the quantity of course-specific instruction times are effective to address these new demands for the workforce on average as such policy interventions affect students' set of skills (Taylor, 2014) and postsecondary educational choices (Aughinbaugh, 2012; De Philippis, 2021; Görlitz & Gravert, 2018). However, recent studies find that the effect of curriculum changes varies considerably across

groups of students (e.g., across gender, academic ability) and, as a result, these educational reforms have the potential to amplify educational inequality (De Philippis, 2021; Huebener et al., 2017). Moreover, despite the large uncertainties about the specific skill set that today's students need to succeed in the future labor market (OECD, 2018), the main focus of the existing literature is on education reforms that target maths or science classes, whereas other subjects have attracted much less attention.

In this paper, we address both the overall impact of curriculum reforms and their implications for educational inequalities by examining a recent reform in Switzerland's compulsory school system. The reform substantially increased the number of English language classes in the school curriculum with the intended goal of providing students with the necessary skills to compete in a globally interconnected economy.¹ As the overall number of classes did not increase proportionally, the main consequence of the reform was that the fraction of foreign

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^{*} Corresponding author.

E-mail addresses: maurizio.strazzeri@bfh.ch (M. Strazzeri), enzo.brox@unibe.ch (E. Brox), chantal.oggenfuss@skbf-csre.ch (C. Oggenfuss), stefan.wolter@unibe.ch (S.C. Wolter).

¹ The important role of foreign language proficiency in an interconnected economy – today, in particular, English language proficiency – has been extensively studied in the economics literature (see, e.g., Ginsburgh & Prieto-Rodriguez, 2011; Grenier & Vaillancourt, 1983; Melitz, 2016). A related strain of literature focuses on language proficiency of immigrants (see, e.g., Chiswick & Miller, 2007). For a recent literature review on the economics of language, see Ginsburgh and Weber (2020).

language classes in compulsory school (grade 1-9) increased significantly. The effect of the reform on the school curriculum was particularly strong for low-track compulsory school students, who compared to the situation before were now required to study two instead of one foreign language and were exposed to a substantial increase in the fraction of foreign language classes by 63.5% on average (high-track students: 41.0%).² Considering the evidence provided by linguistic research showing that females are better language learners than males in terms of second languages (Van der Slik et al., 2015) and the strikingly large female advantage in language grades in school (Voyer & Voyer, 2014), we expect male and female students to be differently affected by this compulsory school curriculum reform.

In our empirical analysis, we test this hypothesis, after documenting the average effects of the reform, and provide clear evidence that a higher share of foreign language classes in school curricula can have a negative effect on male students' school career. Drawing on administrative data covering the entire student population in Switzerland, we construct a repeated cross-sectional panel data set of students in the last year of compulsory school matched with information about their educational choices in the subsequent year. We focus on German-speaking cantons that implemented this reform by introducing English as the first foreign language, allowing for a homogeneous and policy-relevant analysis. We then exploit the differential timing of canton or municipality adoptions of the curriculum reform as exogenous source of variation in the share of foreign language classes in school curriculum.

Two-way fixed effect models with municipality and cohort fixed effects show that the reform had on average only minor effects on educational trajectories. However, the reform reduced the likelihood of low-track male students to proceed their education in upper secondary school programs one year after compulsory school by 2.3 percentage points. The small group of compulsory school graduates who do not immediately proceed their education in upper secondary school programs typically consists of students who temporarily or permanently drop out of the educational system or enter other non-certifying educational programs. Considering that only around one-fourth of low-track male students fall into this group (ca. 27%), our estimation results reveal a substantial impact of the curriculum reform on the immediate transition to upper secondary school for male low-track compulsory school graduates.

Our estimates are consistent with the hypothesis that male students exposed to a higher share of foreign language classes have more difficulties in school than their female counterparts. Interestingly, low-track male students who do not speak at home the schools' language of instruction saw the biggest decline in immediate transfers to upper secondary school. This result suggests that students who already have to invest in other language skills might be particularly overwhelmed by the foreign language demands of the newly introduced school curriculum. Contrary to the results for low-track students, we do not find robust evidence for a link between the curriculum reform and educational choices of high-track students. In almost all specifications, we obtain very precise null effects of the reform to enter upper secondary school or to choose different tracks within upper secondary school (e.g., vocational education vs general education) for both male and female students.

² Starting in 7th grade, students in Switzerland are assigned to two educational tracks based on their academic ability. For a more detailed discussion of the Swiss education system, see Section 2.1. The fraction of foreign language classes (i.e., the number of foreign language classes divided by the number of all classes during compulsory school) increased on average from 5.8% to 9.5% for this group of students. For high-track compulsory school students, the fraction of foreign language classes increased from 8.8% to 12.4% (or 41.0%). Calculations are based on publicly available school curricula before and after the introduction of the reform. For a detailed description of the implementation of the reform, see Section 2.2.

We examine whether the main results of our paper are robust to various model specifications and show that the interpretation of our results is not affected by adding municipality-specific linear time trends to our baseline model, applying a weighting procedure that accounts for the number of observations per municipality-year cell, and using an event-study design model. Moreover, following Goodman-Bacon (2021), we decompose our estimate of the two-way fixed effect model into separate two-groups-two-periods difference-in-differences (2×2 DD) estimators. The results support our conclusions.

As a final analysis, we study if the curriculum reform had an effect on the occupational track of students who enter a VET program after compulsory school. If the curriculum reform had an impact on students' set of skills, we would expect affected students to be trained for occupations that match their newly acquired skill set.³ To test this hypothesis, we match information on four dimensions of skill requirements (maths, natural science, German language, foreign language) for each occupational track of the Swiss VET program to our data set. Using separately the relative importance of each of the four dimensions of skill requirements as an outcome variable in our two-way fixed effect model, we find a striking effect of the curriculum reform on the occupational track of low-track female students who enter a VET program. This effect is in line with our hypothesis: our estimation results indicate that the curriculum reform increased the share of foreign language skill requirements among all skill requirements of an occupation by around 0.4 percentage points (mean: 15.3%), whereas the share of natural sciences skill requirements of the occupation decreased by an equal amount. Again, we do not find a detectable impact of the reform for high-track students.

Our study has three key implications. First, reforms that increase the share of foreign language instruction in school curricula may negatively affect male students' academic outcomes—especially those with lower academic ability who are already challenged by the language of instruction. Second, as these reforms impose disproportionate demands on male students, they may influence educational gender equality. However, since only a subset of male students is adversely affected—at least in the reform studied—the overall impact on gender equality appears limited. This suggests that targeted interventions, such as remedial support, may suffice to address potential side effects. Finally, our finding that curriculum content influences the occupational tracks of students entering vocational education and training (VET) highlights the broader potential of curriculum reforms as a policy tool to align education with evolving labor market demands.

In addition to the literature mentioned in the beginning of the paper, our study relates to two strands of research. First, a considerable part of the economics of education literature is dedicated to the study of factors in the education production function and an increasing body of literature suggest that classroom instruction time plays a role in explaining students' educational success. Prior research studies the effect of the number of days students go to school on student achievement and the majority of these studies finds positive effects (Aucejo & Romano, 2016; Carlsson et al., 2015; Fitzpatrick et al., 2011; Hermmann & Rockoff, 2012; Marcotte, 2007; Marcotte & Hemelt, 2008; Sims, 2008). Other studies have shown that academic performance is positively affected by the length of the school day or the weekly number of lectures (Bellei, 2009; Huebener et al., 2017; Lavy, 2020). Most closely related to this study, Lavy (2015), Rivkin and Schiman (2015), Cattaneo et al. (2017), Bingley et al. (2018), Mandel et al. (2019) and Wedel (2021) show that subject-specific instruction time is positively associated with grades or student performance in international student achievement tests. However, it has been noted that the effect of increased subject-specific instruction time depends on the

³ Speer (2017) finds that workers' pre-market skills are correlated with the task content of their occupation.

source of that time (Rivkin & Schiman, 2015). An increase in subject-specific instruction time might be compensated for by a reduction of other subjects' instruction time or an increase in overall instruction time; and the magnitude of the link between subject-specific instruction time and student performance will likely depend on the compensation mechanism. As prior research relies on reported cross-subject variation in instruction time from international student achievement tests, they usually do not have information on instruction times of non-tested subjects. Our paper contributes to this literature and provides evidence from a policy-induced change in instruction time that gives a clear picture of the compensation mechanisms (reallocation of instruction time from non-core subjects to foreign language classes).

Second, our study relates to the literature on schooling intensity – i.e., the amount of learning material per instruction time – and student performance, as in our setting the increase in the share of foreign language classes likely reflects an increase in learning material in compulsory school curriculum. Prior research that studies a German curriculum reform in the academic track of secondary school suggests that a more compressed school curriculum increases student performance in assessment tests on average (Andrietti, 2016; Andrietti & Su, 2019; Huebener et al., 2017). Interestingly, comparable to our findings regarding low- and high-track students, these studies find a differential effect by student ability, where high-performing students benefit more from the reform than low-performing students potentially due to differences in students' capability to cope with higher schooling intensity. Our paper contributes to this literature by examining a curriculum reform that affected all tracks of compulsory school and investigating a different set of outcome variables (educational choices instead of test scores).

The remainder of this paper is organized as follows. Section 2 describes the Swiss education system and the implementation of the curriculum reform. Section 3 introduces the data and Section 4 explains the empirical approach. Section 5 reports the main findings, robustness tests, the event-study design model, and the decomposition of the two-way fixed effect model. Section 6 reports the findings on occupational choices in the VET program. Section 7 concludes.

2. Background

2.1. The swiss education system

Switzerland is a federal state comprising 26 cantons, with authority over education largely delegated to the cantonal governments.⁴ Education is generally compulsory and free for children aged 4 to 15. Compulsory schooling includes two years of kindergarten (ages 4–6), six years of primary school (ages 6–12), and three years of lower secondary school (ages 12–15). Starting at the lower secondary level, students are placed into one of two educational tracks based on academic ability, determined either by their performance in the final year of primary school or by a separate entrance assessment. Roughly one-third of students enter a track with basic requirements (low track), while the remaining two-thirds follow a track with extended requirements (high track).⁵ Tracking in the Swiss system is relatively rigid, with few students switching tracks during lower secondary school.

After completing compulsory schooling, the vast majority of Swiss students continue to upper secondary education. This level is typically divided into two main pathways: general education programs and

vocational education and training (VET) programs. General education programs, which are mostly free of charge, are school-based and offered by baccalaureate schools or specialized middle schools. Admission is regulated by legal or institutional criteria and is generally reserved for high-track students from lower secondary school who either meet academic performance thresholds or pass an entrance exam. Graduates of these programs earn university entrance qualifications and typically pursue academic degrees at higher education institutions, including universities and universities of applied sciences. The majority of students in upper secondary school are in VET programs. VET programs are selected by both low-track students and high-track students who do not enter a general education program. VET programs prepare students for non-academic careers in the labor market by teaching profession-specific practical and theoretical skills.⁶

A notable number of students do not immediately enroll in upper secondary education, even though they intend to continue their studies. Most of these students are from the low track and either fail to meet the entry requirements for school-based VET programs or are unable to secure an apprenticeship position with a training company. However, a smaller share consists of high-track students who, for instance, aspire to enter general education programs but do not meet the admission criteria of baccalaureate or specialized middle schools. To support these students and prevent disengagement from the education system, various intermediate, non-certifying programs are available. These programs typically last 6 to 12 months and offer tailored support such as additional coursework and guidance on navigating the VET system. Their primary goal is to facilitate a smooth transition to upper secondary education in the following academic year. In general, students who attend these programs have a high likelihood of enrolling in upper secondary education within one year.

2.2. Foreign language classes in compulsory school

Starting in the 1990s, cantonal education authorities began developing new guidelines to harmonize and elevate the role of foreign language instruction in the school curriculum. During these discussions, stakeholders agreed that English should receive greater emphasis. This shift was driven by the growing demands on the Swiss labor force in an increasingly globalized economy. Previously, English was only compulsory for high-track students and typically introduced in lower secondary school. To maintain the prominent role of national languages while also recognizing the benefits of early language acquisition, education authorities ultimately decided to introduce both English and a national language at the primary level.

In June 2007, the guidelines of the education authorities became part of a legally binding agreement among canton governments. This agreement specifies that students should attend training classes in the first foreign language in 3rd grade and training classes in the second foreign language in 5th grade of primary school. Canton governments were given some flexibility for the implementation of the reform as they were able to freely decide whether the first or the second foreign language is an official language of Switzerland, while the other foreign language has to be English.

The reform has been adopted in the course of the following years at canton- or municipality level. When implementing the reform, the higher number of foreign language classes during compulsory school was not entirely compensated for by an increase in the overall number of classes per academic year. Rather, education authorities reduced the number of non-core subjects in the overall school curriculum. As a result, the percentage of foreign language training classes in the school curriculum increased tremendously.

⁴ The autonomy of Swiss cantons in educational matters is comparable to that of U.S. states, Canadian provinces, or German Bundesländer.

⁵ In some cantons, students are assigned to three tracks, with the high track further subdivided by academic performance. In our analysis, we do not distinguish between these subdivisions and treat all students in extended-requirement tracks as high-track. A small number of students attend integrated schools without ability tracking; due to their heterogeneity, these schools are excluded from our analysis.

⁶ VET programs exist today for more than 250 occupations in Switzerland [State Secretariat for Education, Research, and Innovation \(2022\)](#).

3. Data

For the empirical analysis, we use student register data that include the universe of students enrolled in a Swiss educational institution between 2011 and 2018.⁷ The data set provides annual information on students' current education status (e.g., type and location of educational institution, school track, grade) and a limited set of student background characteristics (age, gender, first language, migration status). Individual identifiers included in the data set allow us to identify students across years.

We link information on the educational choice in the subsequent year to each student observation, which leaves us with a repeated cross-sectional panel data set of first-time 9th graders' educational choices, from 2011 to 2017, with schools as the lowest identifiable level of the panel data. We observe the following educational choices of students: non-enrollment in a Swiss educational institution (drop out of Swiss education system), repetition of 9th grade of compulsory school, and enrollment in a (a) non-certifying preparation program, (b) vocational training program, (c) specialized middle school, or (d) baccalaureate school.⁸

3.1. Reform

We received information on the implementation of the curriculum reform from official sources. Specifically, we have access to detailed canton-wide school curricula, covering the years 2003–2020, which canton-level officials are required to report annually to national education authorities (data available upon request). By comparing year-by-year changes in school curricula over time, we are able to identify the year of adoption of the curriculum reform for all cantons in our sample. For the canton of Zurich, which adopted the curriculum reform at the municipality-level, we approached canton-level officials and received a list of all schools located in the canton of Zurich with information on the year in which they implemented the reform.⁹

To identify students who were exposed to the curriculum reform, we calculate the time span between first-time enrollment in 9th grade and the implementation of the reform in the municipality of their current educational institution. We define students as exposed to the curriculum reform if this time span is equal or larger than that of a hypothetical student in the first cohort affected by the reform who regularly passed all grades in compulsory school without grade skipping or grade repetition.¹⁰

3.2. Sample

For our empirical analysis we conduct three sample restrictions. First, as our empirical strategy, which is outlined in Section 4, relies on variation in mean students' educational choices in a municipality over time, we reduce noise in mean outcomes by restricting our sample to municipality-year cells with more than 10 student observation, and restrict our sample to a balanced panel at municipality-level.¹¹

⁷ The data is provided by the Swiss Federal Statistical Office (Office fédéral de la statistique, Division population et formation) under the name "Le Long File longitudinal de l'éducation" (accessed August 2020).

⁸ Around 0.7% (0.5%) of low-track (high-track) students' educational choices cannot be categorized in one of the aforementioned categories. We exclude these observations from our sample.

⁹ Table A1 (Table A2) in the Appendix lists all cantons (municipalities of canton Zurich) included in our study, the number of student observations, and corresponding treatment year.

¹⁰ In case of grade repetition during compulsory school, our approach misclassifies some 9th grade students as being exposed to the curriculum reform even though they were not in the first treated cohort from the beginning. This might slightly bias our results towards zero.

¹¹ In Appendix B Table B3 and Table B4, we show that our estimation results are hardly affected by these restrictions.

Second, we restrict the data to students who enrolled for the first time in the last year of compulsory school (9th grade) and are either in low or high-track of lower secondary school. We mainly do this to ensure the most accurate matching of treatment status and outcomes to student observations we can achieve. This avoid for example that the same student is twice in our sample in two consecutive cohorts of 9th graders.^{12,13}

Third, we restrict our sample to cantons which have a German-speaking majority and introduced English as the first foreign language in compulsory school. Our motivation for these restrictions is twofold. (i) As noted in Section 2, each canton's education system has distinct features and, often, main differences across cantons overlap with linguistic borders in Switzerland. This is true for various economic indicators, which might influence students' educational choices, but also for the education system (e.g., percentage of students entering VET, regulations for entering general education).¹⁴ Restricting our analysis to German-speaking Swiss cantons enhances comparability across regions and allows us to examine, for example, VET choices under similar institutional conditions. (ii) By focusing on cantons that implemented English instead of French as first foreign language, as done by the large majority of cantons with a German-speaking majority, we are able to estimate the effect of a homogeneous policy change.¹⁵ Our final data set includes 14 cantons which cover roughly half of the Swiss population and contributed 47% to the country-wide gross domestic product in 2018.

Fig. 1 illustrates the time and cross-sectional variation used in our analysis. The left-hand side sub-figure of Fig. 1 illustrates the Swiss-German cantons included in the data (upper left corner) as well as the year when the first treated cohort entered 9th grade for each municipality (main map). Gray areas reflect municipalities for which we do not have data.¹⁶ The sub-figure on the right shows the percentage of municipalities whose 9th grader cohort were exposed to the curriculum reform by year as well as the years covered by our final data set (white area).

As shown in the right-hand plot of Fig. 1, at the beginning of our panel data set, around 15% of students were located in municipalities where 9th graders were already exposed to the curriculum reform. For these municipalities, we do not observe both 9th grader cohorts affected by the curriculum reform and 9th grader cohorts not affected by the reform, so that these municipalities can only act as control

¹² We address concerns regarding grade repetition before grade 9 in our robustness checks.

¹³ Around 3% of students in our final sample are in school programs without tracks. These school programs are very diverse in terms of student ability level and are excluded from our analysis.

¹⁴ For recent studies on the economic consequences of the Swiss language border, see Aepli et al. (2021), Eugster et al. (2017), Herz et al. (2021).

¹⁵ There are also several institutional reasons that warrant us from using a larger sample. First, multilingual cantons have either not introduced the policy or the effect of a language reform is very difficult to classify in these cantons. Second, for one cantons we are not able to obtain information on a precise starting date at the canton level (Bern). Third, French speaking cantons have introduced the reform relatively late, such that the first 9th grade cohort would be observed after 2017, which warrants us from estimating an effect for these cantons. We would therefore be able to enlarge our sample by two (German speaking) cantons Basel-Stadt and Solothurn, who opted for French instead of English as first foreign language. However, Basel-Stadt had another structural policy implemented at the same time, which affected the length of primary and secondary schooling and therefore may have an impact on the outcomes as well and is therefore problematic for interpretation. However, adding the two cantons does not change our results. Results are available upon request.

¹⁶ The main reason for this is that not all municipalities have schools with 9th graders. Another reason could be that these municipalities were excluded from the data set because of the panel restriction explained above. However, in Figure B1 Appendix B we show that only very few municipalities were excluded because of this restriction.

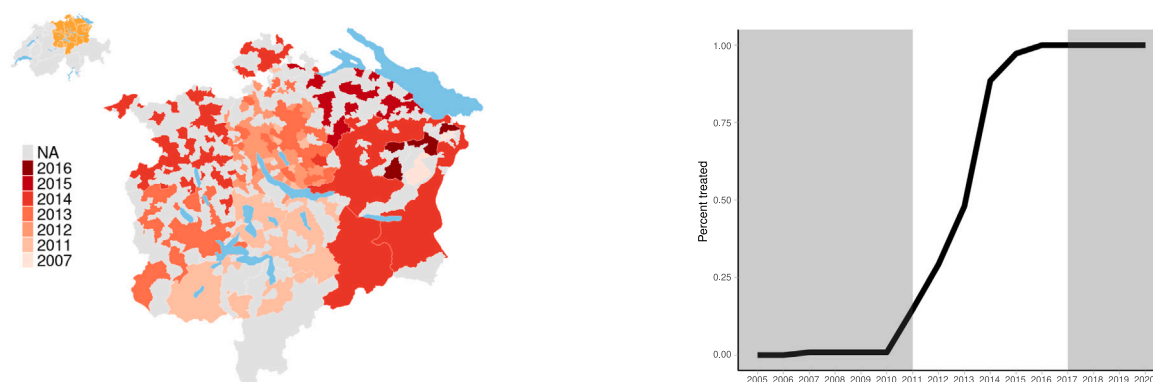


Fig. 1. Time variation in the implementation of the curriculum reform.

Note: Figure on the left shows in the upper left corner a map of Switzerland where the colored area marks cantons included in the final data set. The larger map of the left figure shows a magnified map of the cantons included in the data. Colors of municipalities indicate years when the first 9th grader cohort was exposed to the curriculum reform (see small legend next to the map, blue areas are lakes). Figure on the right illustrates the percentage of municipalities whose 9th grader cohort were exposed to the curriculum reform by year. White area (shaded area) shows years included (not included) in the final data set. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

units in our empirical analysis. By 2017, the last observation period in our data, 9th graders of all other municipalities were affected by the curriculum reform. The variation in exposure to the curriculum reform across cohorts in these municipalities is used as identifying variation for the effect of the curriculum reform on educational choices in the empirical analysis.

3.3. Descriptive statistics

Table 1 illustrates the consequences of the reform for the content of the school curriculum in Swiss cantons which have a German-speaking majority and chose English as the first foreign language in school curricula. Using information from official canton-level school curricula, Table 1 reports separately for low and high-track students the total number of classes in compulsory school by subject (or groups of subjects) and the percentage of each subject (or group of subjects) from all classes in compulsory school before and after the implementation of the reform. Prior to the adoption of the reform, both low-track and high-track students started to attend French classes in 5th grade while high-track students additionally began attending English classes in 7th grade. After the adoption of the curriculum reform, all students receive English learning classes starting in 3rd grade and French learning classes starting in 5th grade.

Table 1 shows a substantial increase in foreign language classes for both types of students. Prior to the reform, only 5.8 percent of total classes in compulsory school were dedicated to foreign language training of low-track students and 8.8 percent of total classes of high-track students. After the implementation of the reform, this number increased for low-track students to 9.5 (12.4 for high-track students), which implies an increase of 63.5 percent (41.0 percent for high-track students). The increase in the share of foreign language classes was compensated for by both a reduction in other subjects – most pronounced for non-core subjects such as Arts, Music, or elective classes (Others) – and an increase in the total number of classes. For both types of students, the increase in the total number of classes compensated for roughly 50% of the increase in the number of foreign language classes.¹⁷

Summary statistics of the background variables by school track and gender are shown in the upper part of Table 2. Low-track students

¹⁷ Increase in total number of classes: 7,539.4-7,402.7=136.7 (low-track) and 7,661.2-7,520.1=141.1 (high-track). Increase in foreign language classes: 716.5-430.3=286.2 (low-track) and 947.6-659.5=288.1 (high-track).

are slightly older than high-track students, which suggest that grade repetition is more common for students in the lower track. Table 2 also shows that the percentage of foreigners and students who do not speak at home either German or another official language is noticeably larger in the group of low-track students than in the group of high-track students. Overall, background characteristics of male and female students are rather similar within school type.

The lower part of Table 2 reports educational choices of students one year after compulsory school. Low-track students proceeding their education in upper secondary school almost exclusively enter a vocational training program.¹⁸ While vocational training programs are also an important education path for high-track students, a large share of high-track students enters specialized middle or baccalaureate schools. For both low and high-track students, Table 2 reveals a large gender discrepancy with respect to enrollment in upper secondary school, with low-track female students being 20 percentage points less likely than low-track male students to proceed their education immediately after compulsory school (gender gap in high-track: 7 percentage points). Instead, female students are more likely to drop out of the Swiss education system—at least temporary—or enroll in a non-certifying preparation class.¹⁹

4. Empirical approach

To assess the impact of increased foreign language classes in compulsory school on educational choices of students after compulsory school, we estimate the following two-way fixed effects (FE) model on students in the last year of compulsory school for all students (and separately for low and high-track students):

$$y_{i,c,t+1} = \alpha_c + \alpha_t + \beta^{DD} Treated_{c,t} + \epsilon_{i,c,t} \quad (1)$$

¹⁸ In general, only high-track students can enter a specialized middle school or baccalaureate school. However, we cannot exclude the possibility that exemption are made for a small number of high ability students in low-track of compulsory school. Another explanation could be measurement error in the administrative data.

¹⁹ To assess if there are any noticeable trends in our data, we report separately for low-track male and female students in Table A3 and A4 in Appendix A background characteristics and educational choices by year (for high-track students: Table A5 and A6). For all four groups, we find a remarkably decrease in the percentage of students that speak at home German or any other Swiss official language, but educational choices appear to be stable over time.

Table 1
Compulsory school (Grade 1–9) curriculum before and after the reform.

	German	English & French	Maths	NATSCI & HIS	Arts & Music	Sports	Others	Total
High-track students								
Total number of classes								
Before reform	1307.4	659.5	1409.1	1326.6	1438.2	808.0	571.2	7520.1
After reform	1284.4	947.6	1383.4	1313.4	1379.7	806.7	546.1	7661.2
Relative change in %	-1.8	43.7	-1.8	-1.0	-4.1	-0.2	-4.4	1.9
Percentage of total classes								
Before reform	17.4	8.8	18.7	17.6	19.1	10.7	7.6	
After reform	16.8	12.4	18.1	17.1	18.0	10.5	7.1	
Relative change in %	-3.6	41.0	-3.6	-2.8	-5.8	-2.0	-6.2	
Low-track students								
Total number of classes								
Before reform	1331.4	430.3	1430.8	1317.3	1484.5	808.0	600.5	7402.7
After reform	1308.3	716.5	1404.8	1301.2	1426.1	806.7	575.8	7539.4
Relative change in %	-1.7	66.5	-1.8	-1.2	-3.9	-0.2	-4.1	1.8
Percentage of total classes								
Before reform	18.0	5.8	19.3	17.8	20.1	10.9	8.1	
After reform	17.4	9.5	18.6	17.3	18.9	10.7	7.6	
Relative change in %	-3.5	63.5	-3.6	-3.0	-5.7	-2.0	-5.9	

Note: Table illustrates the effect of the implementation of the curriculum reform in Swiss cantons with a German-speaking majority that selected English as first foreign language in the new school curriculum on (a) the total number of subject-specific classes in compulsory school and (b) the percentage of subject-specific classes from total classes in compulsory school. Each column refers to a specific class or groups of classes (except of the last column, which refers to the total number of all classes). NATSCI & HIS includes natural science and history classes. Others includes other elective non-core subjects that vary across cantons. Numbers are calculated based on information from canton-level compulsory school curricula before and after the introduction of the reform (see Section 3). Numbers reported are unweighted average values over all cantons.

Table 2
Summary statistics.

	Low track students		High track students	
	Females	Males	Females	Males
<i>Student characteristics</i>				
Age	15.1	15.1	14.8	14.9
Migration status				
Swiss born in CH	63.8	66.6	83.7	84.4
Non-Swiss born in CH	20.2	18.7	7.3	7.0
Swiss not born in CH	3.2	3.0	3.1	3.1
Non-Swiss not born in CH	12.6	11.4	5.6	5.3
First language				
German	53.3	56.4	81.4	82.8
Official language of CH	58.3	61.3	84.2	85.5
<i>School characteristics</i>				
Located in urban area	67.5	66.4	60.9	61.0
Private school	2.1	2.9	4.7	4.4
<i>Educational choice</i>				
Drop-out of Swiss education system	21.1	9.6	8.5	3.5
Grade repetition	1.7	1.2	6.1	4.9
Non-certifying preparation class	23.9	16.0	7.3	6.2
Vocational training program	53.2	73.1	43.2	60.5
Specialized middle school	0.0	0.0	5.8	1.3
Baccalaureate school	0.1	0.1	29.1	23.6
Observations	30,682	39,088	84,866	77,354

Note: Mean values of student and school characteristics and students' educational choices in the next year. Sample includes students in the last year of compulsory school (9th grade). Binary variables: Migration status, first language, school characteristics and educational choices.

where $y_{i,c,t+1}$ is a variable measuring the observed educational choice in year $t + 1$ of student i enrolled in the last year of compulsory school in municipality c in year t , and $Treated_{c,t}$ is a binary variable indicating whether students in the last year of compulsory school in municipality c were affected by the curriculum reform that increased foreign language classes through introducing English language training in primary school. Eq. (1) includes dummies for each municipality (α_c) and graduation year (α_t). We include cross-sectional FE (α_c) to control for unobserved heterogeneity in educational choices specific to a municipality that might be caused by, e.g., persistent differences in local labor markets, student population compositions, or school environments across municipalities. Time FE (α_t) are intended to capture unobserved heterogeneity in educational choices across years that might be driven by, e.g., time-varying shocks or trends in labor demand

or students' preferences that affect educational choices of the entire student population in our sample in a specific graduation year.

Our coefficient of interest in Eq. (1), β^{DD} , measures the effect the implementation of the curriculum reform on educational choices of students. In our setting of a staggered adoption of the curriculum reform, β^{DD} is the weighted average of all possible 2×2 difference-in-differences (DD) estimates that compare groups of municipalities treated at different points in time with each other (Goodman-Bacon, 2021).²⁰ If the effect of the curriculum reform on educational choices

²⁰ Our research designs provides 25 2×2 DD estimates: 5 comparisons between always-treated municipalities (2011 and before) and groups of municipalities that introduced the reform at different points in time (2012, 2013, 2014, 2015, 2016), 10 comparisons between early adopters of the reform and

Table 3
Effect of curriculum reform on educational choices.

	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.001 (0.004)	0.001 (0.004)	-0.002 (0.005)	-0.002 (0.004)	-0.005 (0.005)	-0.003 (0.005)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	No	Yes	No	Yes
Municipality-specific trend	No	No	Yes	Yes	No	No
Population weights	No	No	No	No	Yes	Yes
Student observations	231,990	231,990	231,990	231,990	231,990	231,990
Municipalities	331	331	331	331	331	331

Note: Least squares regressions of binary variables measuring educational choices one year after students enter the last year of compulsory school (DV) on a binary variable indicating if a student was exposed to the curriculum reform (Treated). Observed educational choices: not enrolled in any Swiss educational institution, grade repetition or enrolled in non-certifying preparatory class, vocational training program, specialized middle school (only high-track students), baccalaureate school (only high-track students). DV: Upper secondary school is equal to 1 if student is either enrolled in vocational training program, specialized middle school, or baccalaureate school one year after compulsory school and 0 otherwise. Sample includes students in the last year of compulsory school between 2011–2017. Control variables: Age, first language (German, non-German), migration status (Swiss-born national, Swiss-born foreigner, non-Swiss-born national, non-Swiss-born foreigner), school location (urban, rural, intermediary), type of school (public, private). Reported standard errors in parentheses are cluster-robust at municipality-level.

is constant over time and across municipalities (constant treatment effects), β^{DD} estimates the average treatment effect of the curriculum reform on students' educational choices for those municipalities that introduced the curriculum reform during the years studied in this paper. The identifying assumption is that, for all possible 2×2 DD models, in the absence of the implementation of the curriculum reform students' educational choices in a municipality that implemented the curriculum reform in a given year would have evolved similarly to students' educational choices in municipalities of the control group (common trends). We assess the assumption of common trends and potential implications of violations of the constant treatment effect assumption in Sections 5.3 and 5.4.

$Treated_{c,t}$ varies at municipality-year level. Ordinary least squares estimates of Eq. (1) are equivalent to weighted estimates of the same equation based on data collapsed at municipality-year level using mean values of the outcome variable as dependent variable and weights proportional to the student population in each municipality-year cell. In Section 5.2, we discuss to what extent our results are affected by these weights and adjust our estimates for the number of students in each municipality-year cell by inverse population weighting, i.e., the sum of all weights within a municipality-year cell is equal to 1.

We estimate Eq. (1) on the full sample of students, for low and high-track students and on sub-samples by gender to assess potential effect heterogeneity. We additionally assess effect heterogeneity of the curriculum reform by gender in a double DD (2-DD) model. Formally, we estimate separately for low and high-track students the equation:

$$y_{i,c,t+1} = \alpha_{c,t} + \alpha_c Female_{i,c,t} + \alpha_f Female_{i,c,t} + \beta^{2-DD} Treated_{c,t} \times Female_{i,c,t} + \xi_{i,c,t}, \tag{2}$$

where $\alpha_{c,t}$ is a municipality-by-year FE and $Female_{i,c,t}$ a binary variable indicating female students. Municipality-by-year FE control for unobserved heterogeneity in educational choices across municipality-year-cells that affect students of both genders simultaneously. $\alpha_c Female_{i,c,t}$ ($\alpha_f Female_{i,c,t}$) are municipality-specific (time-specific) FE for female students that take out any unobserved heterogeneity in educational choices of females specific to a municipality (year). Using appropriate weights, estimates of Eq. (2) are equivalent to estimates of Eq. (1) on collapsed data at municipality-year level and using the difference in the mean outcome variables of females and males for each municipality-year cell as dependent variable. Thus our coefficient of interest in Eq. (2), β^{2-DD} , measures the average treatment effect of the

late adopters of the reform, and 10 comparisons between late adopters of the reform and early adopters of the reform.

curriculum reform on the female-male difference in educational choices of students.

5. Results

5.1. Baseline models

Table 3 summarizes our estimation results for the full sample. In column 1 we report results of the baseline two-way FE model. Columns 2-6 present extensions of the model in which we add control variables (columns 2,4 and 6), municipality-specific linear time trends (columns 3-4), and change the weighting procedure of observations to give each municipality-year cell the same weight instead of weighting each cell by the number of student observations per cell (columns 5-6). To take into account serial correlation of error terms, Table 3 reports cluster-robust standard errors at municipality-level in parentheses (Bertrand et al., 2004).

Table 3 shows that the curriculum reform did not affect the likelihood to enter upper secondary school one year after compulsory school. The estimated coefficients are small in comparison to the sample mean of 0.76 in all specifications and we cannot reject the hypothesis that our estimated coefficient is equal to 0. In the remainder of the paper we therefore focus on a second policy relevant question, which is whether the reform had a heterogeneous impact on specific groups and may therefore exacerbate educational inequalities by favoring certain groups of students, such as those who already perform better in these subjects or who are predisposed to them due to stereotypes. We first present results for male and female students. We present these results by track, as treatment intensity differs across tracks (see Table 1).²¹

Table 4 summarizes our estimation results for low-track (Panel A) and high-track (Panel B) students. Columns 1-2 report results of the two-way FE model estimated on the entire sample, columns 3-4 (columns 5-6) report estimation results of the same model for the sub-sample of females (males), and columns 7-8 report estimation results of the 2-DD model. In columns 2, 4, and 8, we add a set of control variables to the baseline models. Standard errors are clustered at the municipality level.²²

In general, low-track students who immediately proceed their education in upper secondary school enter VET programs. Students in

²¹ We address potential selection concerns due to the endogenous track choice in Section 5.2.

²² In the Section 5.2, we show that the interpretation of our results is not affected if we cluster standard errors at canton-level and apply a wild cluster bootstrap-t procedure as suggested in Cameron et al. (2008) in case of small number of clusters.

Table 4
Effect of curriculum reform on educational choices.

	All		By gender				2-DD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Low-track students								
DV: Upper secondary school								
Treated	-0.009 (0.007)	-0.009 (0.006)	0.004 (0.012)	0.007 (0.011)	-0.023*** (0.008)	-0.024*** (0.008)		
Treated x Female							0.030** (0.015)	0.033** (0.015)
Mean outcome ^a	0.64		0.53		0.73		-0.20	
Student observations	69,770	69,770	30,682	30,682	39,088	39,088	69,770	69,770
Municipalities	253	253	253	253	253	253	253	253
Panel B: High-track students								
DV: Upper secondary school								
Treated	0.004 (0.005)	0.005 (0.005)	0.007 (0.006)	0.008 (0.006)	0.001 (0.008)	0.001 (0.008)		
Treated x Female							0.006 (0.010)	0.006 (0.009)
Mean outcome ^a	0.82		0.78		0.85		-0.07	
DV: Baccalaureate school								
Treated	0.000 (0.005)	-0.000 (0.005)	0.002 (0.006)	0.001 (0.007)	-0.003 (0.006)	-0.003 (0.006)		
Treated x Female							0.007 (0.008)	0.006 (0.008)
Mean outcome ^a	0.26		0.29		0.24		0.05	
Student observations	162,220	162,220	84,866	84,866	77,354	77,354	162,220	162,220
Municipalities	326	326	326	326	326	326	326	326
Model specifications								
Restricted to:								
Females	No	No	Yes	Yes	No	No	No	No
Males	No	No	No	No	Yes	Yes	No	No
Variables added:								
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Municipality x Year FE	No	No	No	No	No	No	Yes	Yes
Municipality x Female FE	No	No	No	No	No	No	Yes	Yes
Year x Female FE	No	No	No	No	No	No	Yes	Yes

Note: Least squares regressions of binary variables measuring educational choices one year after students enter the last year of compulsory school (DV) on a binary variable indicating if a student was exposed to the curriculum reform (Treated). Observed educational choices: not enrolled in any Swiss educational institution, grade repetition or enrolled in non-certifying preparatory class, vocational training program, specialized middle school (only high-track students), baccalaureate school (only high-track students). DV: Upper secondary school is equal to 1 if student is either enrolled in vocational training program, specialized middle school, or baccalaureate school one year after compulsory school and 0 otherwise. DV: Baccalaureate school is equal to 1 if student is enrolled in baccalaureate school one year after compulsory school and 0 otherwise. Sample includes students in the last year of compulsory school between 2011–2017. Control variables: Age, first language (German, non-German), migration status (Swiss-born national, Swiss-born foreigner, non-Swiss-born national, non-Swiss-born foreigner), school location (urban, rural, intermediary), type of school (public, private). Reported standard errors in parentheses are cluster-robust at municipality-level.

^a Column (7) reports the mean value of the female-male difference of the corresponding outcome variable.

low-track who do not proceed their education in upper secondary school after compulsory school either enroll in non-certifying preparation classes, repeat the last year of compulsory school or drop out of the educational system for at least one year. Panel A of Table 4 reports estimates of a binary variable equal to one if a student proceeded its education one year after compulsory school and zero otherwise on a binary variable indicating if a student was imposed to the curriculum reform for the sample of low-track students in the last year of compulsory school or not.

The first two column of Table 4 suggest that the curriculum reform did not affect the likelihood to enter upper secondary school one year after compulsory school for the combined sample of males and females. The estimated coefficient of -0.009 is small in comparison to the sample mean of 0.64 and we cannot reject the hypothesis that our estimated coefficient is equal to 0. However, we find a striking difference between male and female students. While our estimates based on the sample of females are again close to 0, we estimate a significant negative effect of the curriculum reform for male students. Our estimates in columns 5–6 indicate that the curriculum reform reduced the probability of male students to attend upper secondary school one year after compulsory

school by around 2.3 percentage points or 3.2% ($0.023/0.73$) relative to the sample mean.

The effect heterogeneity of the curriculum reform by gender is confirmed by the estimates of the 2-DD model in columns 7–8. We estimate that the curriculum reform decreased the probability of male students to enter upper secondary school one year after compulsory school by 3 percentage points relative to the probability of female students. As low-track male students have a higher participation rate in upper secondary school one year after compulsory school, our results suggest that the introduction of the reform reduced the gender gap in immediate transfer to upper secondary school by around 15% ($0.03/0.20$).

Panel B of Table 4 reports estimation results for the sub-sample of high-track students. High-track students have a broader set of educational choices after compulsory school. Most students either enter a baccalaureate school, a specialized middle school, or VET. High-track students who do not select one of these 3 educational tracks enter a preparation year, repeat the last year of compulsory school or drop out of the educational system—equivalent to low track students who do not enter upper secondary school.

The upper part of Panel B in Table 4 presents estimation results for a binary outcome variable indicating if a student entered upper secondary school one year after compulsory school. Contrary to the results based on the sample of low-track students, the estimated coefficients are small and not significantly different from 0 for the entire sample as well as for the sub-samples of male and female students. We also check if the curriculum reform changed high-track students' educational choices within upper secondary school. Using a binary variable indicating if a student entered baccalaureate school and zero otherwise as outcome variable (lower part of Panel B), we, again, do not find any evidence that the curriculum reform affected educational choices of high-track students.²³

To sum up, we find substantial heterogeneity by gender for low-track students, while there no such evidence for high-track students. Potential reasons for this difference could be the larger *treatment-dose*, which low-track students receive compared to high-track students or ability difference, which make lower ability students more vulnerable to the shift in the curriculum (Huebener et al., 2017). We therefore add a second indicator for students that may potentially have higher difficulties with the new curriculum, which is whether students speak at home the language of instruction. We hypothesize that these students have more difficulties with the new curriculum as they already have to invest in other language skills (i.e., the language of instruction) to be successful in school.

In Table 5, we report split-sample estimates for students who speak at home the language of instruction, i.e., German, (columns 4-6) and those who do not (columns 1-3). Again, we show estimates for the overall sample (columns 1, 4), for female students (columns 2, 5), and for male students (column 3, 5) separately for low-track (Panel A) and high-track (Panel B) students.

The estimation results reported in Table 5 do not provide evidence for an effect of the curriculum reform for any subgroup of high-track students, and the negative effect of the curriculum reform is centered among low-track male students. While both male students from German and non-German speaking households in low-track are negatively affected by the curriculum reform, the effect is more than twice as large for students from non-German speaking households. The gender difference among non-German speaking students is also substantially larger.

In contrast to our previous results, which showed very little impact on female students, we also find that low-track female students from non-German speaking household are weakly positively affected by the reform. In general, a positive effect of female students is in line with previous results by De Philippis (2021), showing that an increase in STEM education had a positive impact on male students. We show related findings in Section 6, when studying occupational choices. It is also worth noting that a potentially harmful impact of a decrease in German classes as a side effect of this reform for this group is unlikely as the reduction in German classes was marginal (see Table 1).

5.2. Robustness tests

We provide various robustness tests to evaluate the strength of our findings. In Table A7 in the Appendix we report estimation results based on standard errors clustered at canton-level. As the curriculum reform was introduced at the same time for all municipalities in a canton in 13 out of 14 cantons in our sample, a potential concern is that standard errors clustered at municipality-level do not account for potential within canton correlation of the error term. As shown in

²³ We also do not find an effect of the curriculum reform on high-track students' educational choices if we use a binary variable indicating enrollment in baccalaureate school or specialized middle school as outcome variable or if we restrict the sample to students who enter upper secondary school. Results are available upon request.

Table A7 in the Appendix, standard errors clustered at canton-level are similar and in some specifications even smaller than standard errors clustered at municipality-level, which suggest that within canton correlation of the error term is rather small. However, we only have a small number of cantons in our sample in which case cluster-robust standard errors are more likely to over-reject the null-hypothesis (Cameron et al., 2008). Hence, we also report p-values resulting from a wild cluster bootstrap-t procedure in squared brackets in Table A7 in Appendix A. It is comforting to see that inference based on standard errors from the bootstrap-t procedure does not change the interpretation of our baseline results.²⁴

We add municipality-specific linear time trends to our baseline specifications. Intuitively, including municipality-specific linear time trends resembles a regression discontinuity design with years as the running variable. In these modified versions of the two-way FE and 2-DD model, the identifying variation of β^{DD} and β^{2-DD} relies more strongly on the discontinuity of the outcome variable at the time of the policy change which potentially relaxes the common trend assumption by allowing treatment and control cantons to be on different trends (Angrist & Pischke, 2008; Lee & Solon, 2011).²⁵ Results for these modified versions of our baseline models are reported in Table A8 in Appendix A. Overall, we find that our estimation results are remarkably robust to the inclusion of municipality-specific time trends. We interpret this finding as supporting evidence for the gendered effect of the curriculum reform on low-track students' educational choices.

In our baseline specification, we give each municipality-year observation weight equal to the number of student observations in each municipality-year cell. In case that the effect of the curriculum reform on students' educational choices is homogeneous for municipalities, we would expect our results to not differ if we run our analysis when giving each municipality-year cell equal weights (Solon et al., 2015). In Table A9 in the Appendix, we show estimation results of our baseline regressions using weights to adjust for the number of students in each year- municipality-cell. Our point estimates become smaller for low-track male students (from -0.023 to -0.030) and the significant negative effects of entering upper secondary school after compulsory school remains significant for male students and we do not find a corresponding effect for female students. The results of the 2-DD model is not affected by the inclusion of the inverse population weights, which suggests that the effect of the curriculum reform on educational choices is similar among students in smaller and larger municipalities. However, Table A9 shows a noticeable difference of the effect of the curriculum reform on educational choices of high-track students if we weight our regressions. Specifically, we find that the curriculum reform significantly reduced the probability to enter a baccalaureate school for male students in high-track in the unweighted regressions. While the point estimate of the 2-DD estimate is less affected by the population weights, this result still suggests that our baseline estimates for high-track students are mainly driven by larger municipalities that receive relatively more weight in our baseline regressions.

In Table A10 in the Appendix, we show that the effect of the reform for low-track male students is persistent and remains visible also two years after graduation. We re-run our estimation Eqs. (1) and (2) using the educational status two years after graduation as outcome

²⁴ We also conduct a separate analysis for the canton of Zurich, which is the only canton for which we have municipality level information of the implementation of the reform and can therefore rely on within canton variation. In addition, we conduct a leave-one-out analysis to show that our results are robust to the choice of the sample. Our result can be found in Appendix B Table B6, Table B7, Table B8 and Table B9. The negative effect on low-track male students is insensitive to these tests.

²⁵ For a more detailed discussion, see Goodman-Bacon (2018).

Table 5
Effect heterogeneity, students who do not speak at home the language of instruction.

	Non-German speaking			German speaking		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Low-track students						
DV: Upper secondary school						
Treated	-0.001 (0.011)	0.031* (0.016)	-0.036** (0.015)	-0.014 (0.009)	-0.011 (0.017)	-0.018** (0.009)
Mean outcome	0.55	0.47	0.63	0.72	0.59	0.81
Observations	31,379	14,324	17,055	38,391	16,358	22,033
Cluster	252	252	252	253	253	253
Panel B: High-track students						
DV: Upper secondary school						
Treated	0.009 (0.014)	0.025 (0.016)	-0.012 (0.021)	0.005 (0.004)	0.006 (0.006)	0.003 (0.006)
Mean outcome	0.73	0.71	0.74	0.84	0.80	0.88
DV: Baccalaureate school						
Treated	-0.008 (0.012)	-0.012 (0.011)	-0.002 (0.015)	0.003 (0.005)	0.005 (0.007)	-0.002 (0.006)
Mean outcome	0.18	0.19	0.17	0.28	0.31	0.25
Observations	29,053	15,777	13,276	133,167	69,089	64,078
Cluster	325	321	323	326	326	326
Model specifications						
Restricted to:						
Females	No	Yes	No	No	Yes	No
Males	No	No	Yes	No	No	Yes
Non-German speaker	Yes	Yes	Yes	No	No	No
German speaker	No	No	No	Yes	Yes	Yes
Variables added:						
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Note: Least squares regressions of binary variables measuring educational choices one year after the last year of compulsory school (DV) on a binary variable indicating if a student was exposed to English language training in primary school (Treated). Observed educational choices: not enrolled in any Swiss educational institution, grade repetition or enrolled in non-certifying preparatory class, vocational training program, specialized middle school (only high-track students), baccalaureate school (only high-track students). DV: Upper secondary school is equal to 1 if student is either enrolled in vocational training program, specialized middle school, or baccalaureate school one year after compulsory school and 0 otherwise. DV: Baccalaureate school is equal to 1 if student is enrolled in baccalaureate school one year after compulsory school and 0 otherwise. Sample includes students in the last year of compulsory school between 2011–2017. Control variables: Age, migration status (Swiss-born national, Swiss-born foreigner, non-Swiss-born national, non-Swiss-born foreigner), school location (urban, rural, intermediary), type of school (public, private). Reported standard errors in parentheses are cluster-robust at municipality-level.

variable.²⁶ To obtain the information on the educational status two years after graduation, we have to shorten our panel by one year. As shown in Table A10, the estimated coefficient shrinks by around 32% for the estimates by gender and by around 14% for the 2-DD estimates. Overall, we interpret these estimates as supportive evidence that the curriculum reform has a persistent effect on the school career of low-track male students.

A potential concern regarding the validity of our estimation strategy based on repeated cross-sectional data of 9th grader is that the reform might have induced a compositional change in 9th grader cohorts in treated municipalities by affecting the likelihood to graduate from low- or high-track. To address this concern, in Table A15 in the Appendix, we report results of regressions of Eqs. (1) and (2) using a binary variable measuring whether a student is enrolled in high-track in the last year of compulsory school as outcome variable. The estimated coefficients for β^{DD} (and β^{-2DD}) are not significantly different from zero and suggest that the likelihood to attend either low- or high-track is not affected by the reform. It is worth noting here that we do not

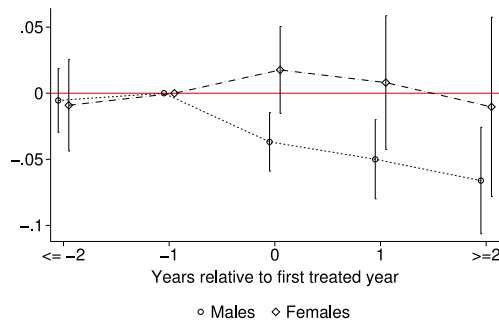
find any evidence for sorting in the full sample as well as in the sample of male and female students due to the reform.

Another concern is that the reform increased the likelihood to repeat a certain grade in compulsory school, again leading to a compositional change in the 9th grader cohort in treated municipalities. Unfortunately, we do not have information on the number of times a student has repeated a certain grade. However, we can address this concern indirectly by using students' age as outcome variable in our estimation equation. Table A16 in the Appendix reports results of such an estimation equation. As the estimated coefficients are insignificant and close to zero, we do not find any evidence that grade repetition might be a concern in our setup.²⁷ As we also present results based on the language spoken at home, we conduct a similar robustness check and show that the share of students who do not speak the language of instruction at home is not affected by the reform. The results can also be found in Table A16.

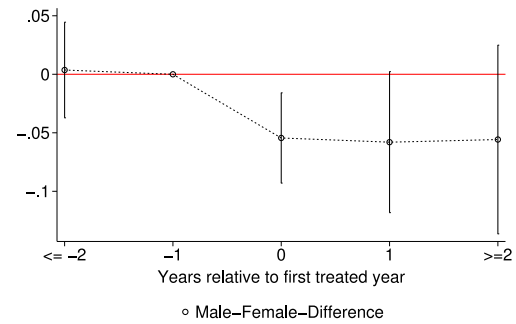
A final concern relates to the specific nature of the reform. As standard in curriculum reforms, the relative increase in foreign language courses has to be compensated. These simultaneous changes might all affect the outcome jointly and therefore it not ex-ante clear whether the observed effect is a result of the increase in foreign language classes or the compensation mechanism (Rivkin & Schiman, 2015). In our case part of the compensation is a decrease in instruction time in other subjects and part of the compensation is also an increase in instruction time overall. We acknowledge, in line with the related literature, that

²⁶ Tables A11–A14 in the Appendix show frequency tables of educational outcomes one and two years after enrollment in 9th grade. There is a high persistence of educational choices over this period. However, as expected, a large share (between 60–80%) of students who entered preparation classes immediately after graduation start a vocational education in the next year. We also find that a substantial part (around 50%) of students who drop out of the education system immediately after graduation find their way back to the education system.

²⁷ Table 2 provides further intuition for this by showing that the share of grade repeaters is rather low, especially in the low-track (below 2%).



(a) Split sample estimate



(b) Full sample estimate

Fig. 2. Event-study design, effect on direct transfer to upper secondary school, low-track students

Note: Plot on the left shows coefficient estimates of β_{-2} , β_0 , β_1 , and β_2 based on Eq. (3), estimated separately for male and female low-track students. Plot on the right shows estimates of the coefficients interacted with a binary variable indicating male students, estimated on the entire sample of low-track students, based on a modified version of Eq. (3) with interaction terms for all predictors. Data restricted to municipalities in which students in the last year of compulsory school were affected by the policy change between 2013 and 2015. Number of municipalities: 176. 90% confidence intervals are calculated based on robust standard errors clustered at municipality-level.

we cannot empirically distinguish them. However, it is worth noting here that all other changes are remarkably small and therefore unlikely to have a major impact on student outcomes. This is illustrated in Table 1.²⁸

5.3. Event-study design model

As noted in Section 4, the credibility of our preferred empirical model relies on the common trend and the constant treatment effect assumption. In order to examine a potential violation of one of the two assumptions, we follow standard practice in DiD literature and estimate an event-study design model. In the event-study design model, we replace $Treated_{c,t}$ in Eq. (1) with a set of dummy variables indicating the distance between the treatment year, i.e., the year in which the first student cohort in municipality c was affected by the curriculum reform, and t .²⁹

Formally, we estimate on sub-samples of male and female students the following model:

$$y_{i,c,t+1} = \beta_{-2} \mathbb{1}[-2 \leq t - T_c] + \sum_{j \in \{0,1\}} \{\beta_j \mathbb{1}[j = t - T_c]\} + \beta_2 \mathbb{1}[2 \geq t - T_c] + \alpha_c + \alpha_t + \epsilon_{i,c,t} \tag{3}$$

where T_c is the year the first student cohort exposed to the language reform entered the last year of compulsory school (treatment year), and $\mathbb{1}[\cdot]$ is an indicator variable equal to 1 if the condition in the squared brackets is fulfilled. To avoid collinearity, we exclude the dummy variable for the year prior to the treatment year from the estimation equation. Thus, the coefficients of interest in Eq. (3) (β_{-2} , β_0 , β_1 , and β_2) measure the time-varying effect of the exposure to the curriculum reform on students' subsequent educational choices relative to one year before the first student cohort entered the last year of compulsory school.

²⁸ E.g. The decrease in German classes of 1.7% in the low-track captures a decrease by 23 units over the course of 9 years of compulsory schooling (less than 3 units per year).

²⁹ Due to the leads and lags of the treatment variable, we estimate the event-study design model only on a sub-sample of the municipalities in our sample (Schmidheiny & Siegloch, 2019). Specifically, we focus on municipalities which implemented the curriculum reform between 2013 and 2015. Given the observation window of students' educational choices for the years 2011 to 2017, we can construct two pre-treatment dummy variables (two or more years before treatment, one year before treatment), two post-treatment dummy variables (one year after treatment, two or more years after treatment), and one event dummy variable (treatment year).

The coefficients of interest of Eq. (3), estimated on the sample of low-track students and using enrollment in upper secondary school as outcome variable, are illustrated in the left plot of Fig. 2.³⁰ The right plot of Fig. 2 illustrates coefficient estimates that measure the time-varying effect of the reform on the female-male difference in educational choices that come from an estimate of a modified version of Eq. (3) where all predictors are additionally interacted with a binary variable indicating male students.

The estimation results shown in the left plot of Fig. 2 do not indicate pre-treatment trends in educational choices for the sub-sample of males (dotted line) nor for the sub-sample of females (dashed line), which supports the common trend assumption of our empirical approach. While we see a significant drop in immediate enrollment in upper secondary school programs for male students in the treatment year and the years after, we do not find a corresponding effect for female students. The effect for male students appears to increase over time, which suggests the presence of time-varying treatment effect. Such time-varying treatment effect might be caused by the fact that over time the percentage of wrongly defined treated students due to grade repetition in grade 3 to 9 decreases.³¹ However, we see a similar negative trend for female students, which suggest that instead of time-varying treatment effects, treatment-independent factors correlated with the timing of the introduction of the curriculum reform could explain these trends. This is confirmed in the right plot of Fig. 2, which shows that the male-female differential effect of the curriculum reform remains remarkably constant over time.

5.4. Goodman-bacon decomposition

A growing literature has shown that estimations of two-way FE models in a staggered treatment adoption design can lead to misleading results. Particularly, if the treatment effect varies over time, two-way fixed effect models are biased and the estimated effect might even have the opposite sign of the true effect. The reason for this is that when already treated units act as a control group in one of the 2×2 DD estimates, changes in the outcome variable over time due to the dynamic treatment effect affect the DD estimate.

While the results of our event-study design model do not provide clear evidence in favor of dynamic treatment effects, we evaluate the

³⁰ For brevity, we focus on educational choices of low-track students. Event-study design estimates for high-track students are available upon request.

³¹ See discussion on potential mis-measurement of the treatment variable in Section 3.

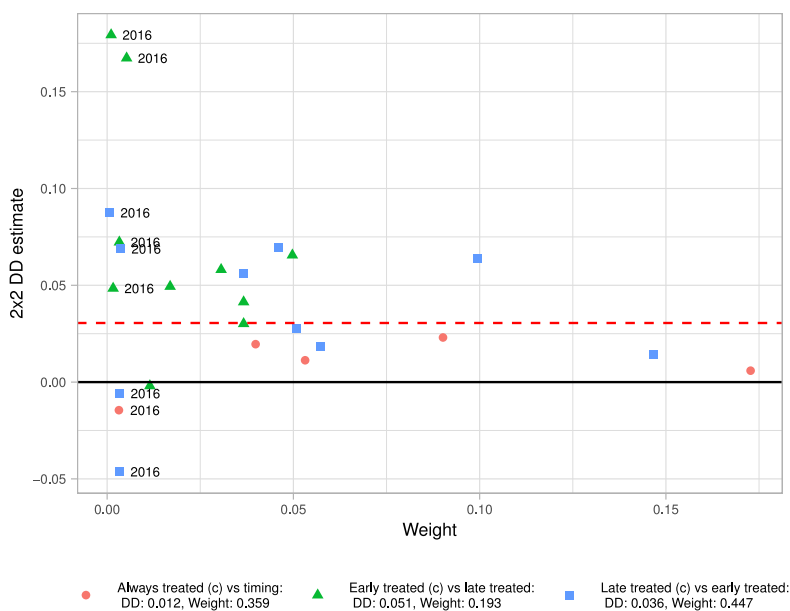


Fig. 3. Goodman-Bacon (2021) decomposition, low-track students, 2-DD model

Note: The figure plots each 2×2 DD estimate against their weight given in the baseline 2-DD model for low-track students (dependent variable: Attends upper secondary school). The (red) dotted line indicates the DD estimate of the baseline model, which is equal to the average of all plotted 2×2 DD estimates weighted by the value of the x-axis. The decomposition is based on models weighted by the inverse number of students in each municipality-year-cell as discussed in Section 5.2. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

influence of potential biases of our two-way FE model using the decomposition proposed by Goodman-Bacon (2021). This decomposition estimates all 2×2 DD models and their corresponding weights that they receive in the two-way FE model. These weights increase with (a) the number of observations in a particular 2×2 DD model and (b) the treatment variance, which is highest for 2×2 DD models in which the treatment year is in the middle of the panel and lowest for treatment years at the beginning or the end of the panel.

Our research designs provides 25 2×2 DD estimates that can be categorized in three groups: (a) 5 comparisons between always-treated municipalities (2011 and before), which act as control, and groups of municipalities that introduced the reform at different points in time (2012, 2013, 2014, 2015, 2016), which act as treatment group, (b) 10 comparisons between early adopters of the reform, which act as control group, and late adopters of the reform, which act as treatment group, and (c) 10 comparisons between late adopters of the reform, which act as control group, and early adopters of the reform, which act as treatment group. The first two cases (a and b) are problematic in case of dynamic treatment effects as they involve treated municipalities as control groups. Our main motivation for applying the Goodman-Bacon decomposition is to separately estimate the treatment effect for all three categories, using information on the 2×2 DD estimates and their corresponding weights, and to evaluate to what extent our baseline estimate is driven by the potentially two problematic categories.

In Fig. 3 we plot for low-track students each 2×2 DD estimate (dependent variable: Attends upper secondary school) and their corresponding weights for the 2-DD model as well as the baseline estimate from the 2-DD model (red dotted line).³² In Fig. 3, (red) dots represent

³² In Figure A1 in the Appendix we plot for low-track students each 2×2 DD estimate and their corresponding weights for the two-way FE models (separately for males and females) as well as the baseline estimate from the two-way FE model (red dotted lines) respectively. To ease comparability between the models, the decomposition is based on models weighted by the inverse number of students in each municipality-year-cell as discussed in Section 5.2 (results reported in Table A9 in the Appendix). By doing so, the weights resulting from the Goodman-Bacon decomposition for each 2×2 DD

2×2 DD models of always-treated municipalities, which act as control group, and groups of municipalities that introduced the reform at different points in time, (green) triangles represent 2×2 DD models of early adopters of the reform, which act as control group, and late adopters of the reform, and blue squares represent 2×2 DD models of late adopters of the reform, which act as control group, and early adopters of the reform. The Goodman-Bacon decomposition for the 2-DD model shows a very homogeneous effect of the reform on the female-male difference in our outcome variable and almost all of the 2×2 DD estimates correspond in sign to our baseline estimates.³³ The estimate for the group of 2×2 DD models that is not affected by potential dynamic treatment effects (blue squares in Fig. 3) is 0.036 and very close to our baseline estimate for the 2-DD model (0.03).³⁴

In sum, we interpret the results of the Goodman-Bacon decomposition as supporting evidence that potential dynamic treatment effects are not driving our baseline estimates. However, we also present an additional robustness check based on a novel estimator suggested by Gardner et al. (2024) for settings with treatment effect heterogeneity and variation in treatment timing.³⁵ The results can be found in

estimate are the same for the two two-way FE models (male and female) as well as for the 2-DD model.

³³ We find an anomaly only for 2×2 DD estimates generated with observations from the treatment year 2016. However, as the treatment year 2016 is at the end of our observation period and includes only observations from a small canton with only 25 municipality (see Table A1 in Appendix A), those 2×2 DD estimates receive only little weight in our baseline estimates and have only little effect on our overall estimate.

³⁴ For completeness, we also conduct a Goodman-Bacon decomposition to test the concern of track choices being affected by the reform. The results can be found in Appendix B Table B5. Our conclusion regarding compositional changes are unchanged.

³⁵ For a detailed discussion of the estimator see Gardner et al. (2024). To summarize, the approach consists of two stages. In the first stage the outcome is regressed on group and period fixed effects using the subsample of untreated observations. Stage two subtracts the estimated group and period effects from the observed outcomes. The residualized outcome is regressed on the treatment status.

Appendix A Table A17 for the main sample and separately for female and male students and in Figure A2 for the gender difference across low-track students. The alternative estimation approach qualitatively and quantitatively supports our main results.

6. Effect on occupational choices

In this section, we investigate whether the curriculum reform was effective in changing students' set of skills. Unfortunately, we do not have access to repeated large-scale achievement tests that would allow us to directly estimate the consequences of increased foreign language classes on students' skills in a setting of a staggered implementation of the curriculum reform. However, the Swiss education system, which includes a large vocational education and training (VET) component in upper secondary school, gives us the opportunity to approach this question indirectly. VET programs prepare students for non-academic careers in specific occupations. These training occupations differ from each other by their task content and the skills that they require to successfully graduate from the VET program. As previous literature suggests that workers' pre-market skills covary with their occupation's task content (Speer, 2017), we would expect to see changes in the selected training occupations of students starting a VET program after compulsory school. More specifically, if exposure to increased foreign language classes affects students' set of skills, we would expect students affected by the reform to be more likely to select into training occupations with a higher share of tasks that require foreign languages than not affected students.

To test this claim, we restrict our sample to students who start a VET program immediately after compulsory school and match our data with information on the skill requirements of training occupations. We obtain this unique information on skill requirements of training occupations from <https://www.anforderungsprofile.ch>, a website that is administrated by the Swiss trade association and the Swiss Conference of Cantonal Ministers of Education and is partially financed by the Swiss Secretariat for Education, Research, and Innovation.³⁶ Overall, the data includes information on a comprehensive set of 20 different skill measures that can be linked to four main categories: maths, natural science, native language, foreign language.

Based on the skill requirement measures of the four main categories, which are calibrated to a scale of 0–100, we construct a training occupation-specific variable that represents the *relative* importance of each skill dimension by dividing each skill requirement measure by the sum of the skill requirement measures of all four categories. These relative measures of skill requirements are used as outcome variables in our baseline model outlined in Eq. (1). If (pre-market) skills and the skill requirements of a training occupation are correlated and the exposure to the curriculum reform changed students' foreign language skills, we expect to see a positive effect of the curriculum reform on the relative importance of foreign language requirements of training occupations.³⁷

Table 6 reports estimation results separately for low-track (Panel A) and high-track students (Panel B) based on the sample of students who

³⁶ The information available on this website is intended to help students—or persons who advise students such as parents, teachers, etc.—to choose a training occupation that fits a students' profile by providing information on the skills that are required to successfully graduate from the VET program. The skill requirement measures were constructed from a systematic comparative rating procedure that relates each occupation to a reference group of selected occupations. The rating procedure was conducted by experts and practitioners in the field such as teachers at vocational schools and human resource managers of training companies.

³⁷ In Table A18 in the Appendix, we replicate the findings of Speer (2017) and show that students' pre-market skills (measured by achievement test scores) are correlated with our relative skill requirement measure using data from the Swiss subsample of 2012 PISA survey.

start a VET program one year after compulsory school (the dependent variable is measured in percent, i.e., multiplied by 100). Columns 1–2 summarize estimation results for the outcome variable math skill requirements; columns 3–4 for the outcome variable school language requirements; columns 5–6 for the outcome variable natural science requirements; columns 7–8 for the outcome variable foreign language requirements. We run all estimations separately for female (columns 1, 3, 5, 7) and male students (columns 2, 4, 6, 8).

Panel A suggest that the curriculum reform changed the skill requirement content of training occupations for low-track female students, while we do not find a corresponding effect for low-track male students. As expected, we find a significant positive effect of the reform on the relative skill requirement measure for foreign language skills. Relative to the sample mean of 15.3%, the curriculum reform increased the relative foreign language skill requirement measure by around 2.6%. It is also worth noting that the increase in relative foreign language requirements was mainly compensated for by a reduction in natural science skills, rather than the more closely linked category of native language requirements.³⁸

The estimates for high-track students, as reported in Panel B, do not show an effect of the curriculum reform on the skill requirement of the training occupation. While the estimated coefficients are larger for female than for male students, we cannot reject the hypothesis that the coefficients are equal to zero based on conventional significance levels.

A potential concern of this estimation approach is that the curriculum reform induced a compositional change in the sample of students who start a VET program immediately after compulsory school and this compositional change might confound our estimates. However, the estimation results reported in Section 5 suggest that the curriculum reform does not affect the transition to upper secondary school for the majority of students (i.e., low-track female and high-track students), rendering the assumption of compositional changes unlikely.

7. Conclusion

In this paper, we analyze the effects of a large curriculum reform in Switzerland on educational choices and trajectories at the end of compulsory schooling using administrative data on all students in the German speaking part of the country. The curriculum reform substantially increased the share of foreign language instruction in compulsory school. The increase in foreign language teaching was mainly compensated for by a reduction of instruction time in non-core subjects and partly by increasing the overall instruction time. Exploiting the staggered introduction of the reform, we are able to estimate the average treatment effect of the curriculum reform on student outcomes. We analyze the transfers to post-compulsory education at upper secondary level, namely whether the transfer is immediate or delayed, to which type of education the transfer is made, and for students starting a VET program, which is the majority of Swiss students, which skill requirement profile the found training occupation has. Overall, we find that the vast majority of students, i.e., low-track female and high-track students, which represent together around 83% of the student population, were not affected by the reform in terms of the immediate transfer to upper secondary school or the type of education (general education or VET) they select in upper secondary school.

Our estimates, however, show significant effects for two groups of students. First, low-track male students, in particular those not speaking the school language at home, are more likely to delay the entrance in upper secondary school. We find that the likelihood of low-track male students to delay the immediate entrance in upper secondary

³⁸ For completeness and comparability, we also conduct a Goodman-Bacon decomposition for this outcome variable. The results can be found in Appendix B Table B10. Our conclusion regarding the impact of the reform on relative skill requirements of occupations are unchanged.

Table 6
Effect of the reform on the relative skill requirements of training occupations.

	Math		School language		Natural sciences		Foreign language	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Low-track students								
Treated	-0.191 (0.190)	0.165 (0.202)	0.220 (0.147)	-0.087 (0.123)	-0.432*** (0.165)	0.118 (0.107)	0.403* (0.205)	-0.197 (0.181)
Mean outcome	18.47	31.82	36.94	29.78	29.28	31.96	15.32	6.44
Student observations	15,963	26,637	15,963	26,637	15,963	26,637	15,963	26,637
Municipalities	253	253	253	253	253	253	253	253
Panel B: High-track students								
Treated	0.122 (0.122)	-0.108 (0.139)	-0.109 (0.085)	0.044 (0.081)	-0.190 (0.167)	0.020 (0.141)	0.177 (0.163)	0.044 (0.196)
Mean outcome	20.86	30.37	34.40	28.48	25.38	28.16	19.36	12.98
Student observations	34,627	42,488	34,627	42,488	34,627	42,488	34,627	42,488
Municipalities	326	326	326	326	326	326	326	326
Model specifications								
Restricted to:								
Females	Yes	No	Yes	No	Yes	No	Yes	No
Males	No	Yes	No	Yes	No	Yes	No	Yes
Variables added:								
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Least squares regressions of a variable measuring the relative skill requirement (in percent) of training occupations on a binary variable indicating if a student was exposed to the curriculum reform (Treated). Sample includes 9th grade students who selected into a VET program in the subsequent year. Control variables: Age, migration status (Swiss-born national, Swiss-born foreigner, non-Swiss-born national, non-Swiss-born foreigner), school location (urban, rural, intermediary), type of school (public, private). Reported standard errors in parentheses are cluster-robust at municipality-level.

school increases by 2.3 percentage points or 8.5% relative to the sample mean. We do not find a corresponding effect for female students in low-track nor for high-track students. Second, we find that low-track female students who start a VET program immediately after compulsory school are significantly more likely to opt for a training occupation with higher demands in foreign language skills but at the expense of cognitive demands in the field of natural science. We estimate for this group of students that the share of foreign language skill requirements of training occupations increases by around 2.6% relative to the sample mean. This would be a result consistent with the expectation that the reform increased students' foreign language skills. However, our data do not allow us to examine the effect of the reform on students' foreign language proficiency, since standardized proficiency tests for foreign languages are not regularly administered in Switzerland. Therefore, we cannot know to what extent and for which groups of students the reform has achieved the primary intended goal, namely that of improving foreign language proficiency.

What we can say, however, is that even if the reform has not had any disadvantages for the vast majority of students in terms of further educational progress, negative side effects have occurred for a small group of students. The reform would therefore need to be accompanied by targeted interventions for this group of students without compromising the potential benefits of the reform for the majority of students.

Appendix A. Additional results and supplementary material

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.econedurev.2025.102684>.

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