Working Paper No. 229

Locked-in vs. Locked-out: Can Detracked Classes Increase Education Equality?

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October 2024

The Swiss Leading House on Economics of Education, Firm Behavior and Training Policies is a Research Program of the Swiss State Secretariat for Education, Research, and Innovation (SERI).

www.economics-of-education.ch

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Abstract: Do detracked classes affect students from different socio-economic backgrounds differently? In the Swiss education system, students are assigned to one of two tracks based on prior achievements at age twelve: approximately 70% are placed in an advanced track and roughly 30% in a basic track. After this assignment, students may either be grouped into classes based on their track or placed in mixed classes with students from both tracks. While tracking is common in many countries, the evidence on its impact remains inconclusive. Understanding this impact is crucial for optimizing school systems to improve students' labor market outcomes later in life. To evaluate the effect of detracked classes, I exploit a unique detracking reform in one Swiss canton, using a difference-in-differences design. This reform, implemented in 2015, changed only how students were grouped into classes, while track assignments remained the same. Before 2015, classes were tracked, meaning they contained only students from either the advanced or basic track. After the reform, classes were detracked, meaning students from both tracks were placed together, while tracks were still assigned. Using individual-level register data for the entire population of Swiss students from 2012 to 2022, I show that the reform dramatically altered class compositions in terms of peers' background characteristics. Since track assignment is correlated with socio-economic background, advanced track students, on average, had for example fewer native speakers in their classes after the reform, and vice versa for basic track students. The likelihood of being assigned to further education, which enables students to pursue tertiary education, increased for the average student due to detracking. My heterogeneity analysis reveals that the overall positive effects were concentrated among socio-economically disadvantaged students. For students whose parents are not tertiary educated and who are not native in the regional language, the probability of further academic education nearly doubled, while more advantaged students did not experience any negative effects. I can rule out changes in curricula, teacher quantity and quality, and motivational factors as mechanisms for these findings, and interpret my main estimates as the causal effects of detracked classes.

JEL Classification: I24

Keywords: Education, Inequality, Peer Effects, Detracking

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1 Introduction

Tracking students into classes based on prior achievements is common in many countries, but studies estimating the effect of tracked classes on average student outcomes find inconclusive results. On the one hand, studies show that teaching is more efficient with a more homogeneous student body and therefore beneficial for the average student (Duflo, Dupas, and Kremer 2011). On the other hand, there are studies arguing that tracked classes increase education inequality because only higher-achieving students benefit from tracked classes, while lower-achieving students lose (Matthewes 2021). If the latter argument is true, tracking might foster educational inquality because often students' socio-economic background is correlated with educational achievements that determine their track assignment (Van Ewijk 2011). This concern is consistent with findings from cross-country comparisons indicating that early tracking increases education inequality (Hanushek and Woessmann 2006).

Tracking systems differ widely across countries both in the age at which students begin to be tracked and in the degree of tracking (Betts 2011). Therefore, tracking remains one of the most controversially debated issues in how to optimally organize school systems to increase education levels and improve labor market prospects of individuals. Further, despite the importance of tracking for education and earnings inequality, evidence on the heterogeneous effects of tracked classes along socio-economic backgrounds of students is scarce and their impact on long-term outcomes is largely unknown. This paper investigates the heterogeneous effects of detracked classes along socio-economic backgrounds of students on the probability of assignment to further academic education. Further academic education is a prerequisite for tertiary education and is therefore consequential for future earnings. For empirical identification, I exploit a unique detracking reform and leverage variation over time within schools in a difference-in-differences framework. In Switzerland, students are tracked after elementary school at age twelve, when the 70% of higher-achieving students are assigned to an advanced track, while the lower-achieving 30% of pupils are directed to a basic track. Students are not locked-in to these tracks, but can change their initial track assignment if they perform accordingly. Many cantons of Switzerland have a general practice of either tracking lower secondary school classes, such that they contain only advanced or basic track students, or of detracking lower secondary school classes, such that they contain a mix of students of both tracks. In 2015, the canton of Neuchâtel, implemented a detracking reform. Prior to the reform, students were tracked into classes within schools based on

whether they are assigned to the advanced or the basic track. After the reform, schools were instructed to form detracked classes that contained on average 70% advanced track students, and 30% basic track students. Importantly, students are still assigned to tracks after the reform, and the reform only changed the grouping of students into classrooms. Other aspects of the education system, such as teacher quality and quantity, curricula, and financial resources remained unchanged. To estimate the effect of detracked classes on the probability of further academic education, I compare outcomes of students in the reform canton with outcomes of students in cantons that generally track lower secondary school classes over time.

I use register data on the entire Swiss student population, containing students of all ages in any eucational institution in Switzerland from 2012-2022. The data contains student enrollment into detailed types of education programs over time and provide me with a rich set of individual-level covariates, including year and month of birth, gender, nationality, native language, and parents' education. This allows me to focus on the heterogeneous impact of detracked classes for students of different socio-economic backgrounds.

I show that the detracking of classes did not only lead to the mechanical change in the class shares of students on each track, but also changed class shares of students with tertiary educated parents, students of Swiss nationality, native speakers, and females. These effects work in opposite directions for students on the advanced and the basic track because the assignment to tracks is highly correlated with individual characteristics. My results suggest that detracked classes increase the probability of assignment to further academic education enabling students for enrollment to tertiary education by 10% for the average student. I use split sample estimates to reveal considerable heterogeneity of this average effect across students assigned to different tracks and with differential socio-economic backgrounds. The results suggest that prior to the reform, the probability of assignment to further academic education was very close to zero for students on the basic track, while in detracked classes, this probability rises to above zero. Importantly, students assigned to the advanced track did also benefit from the detracked classes by an increase of 8% in the probability of being assigned to further academic education after the reform. Split sample estimates along socio-economic backgrounds of students show an increase of 15% in the probability of assignment to further academic education for students with non-tertiary educated parents, while there is no effect for students with tertiary educated parents, and students who are not native in the regional language tend to benefit more. I show that the gains for the least advantaged students, who neither have tertiary educated parents nor are native

in the regional language, are highest. Their probability of assignment to further academic education increases by over 40% after the reform.

Since the detracking reform exclusively changed class composition but did not alter the assignment to tracks, the proportion of students assigned to each track, curricula, teachers, and other school resources, I interpret my estimates as causal effects of detracked classes and rule out other potential mechanisms. To explore how students of different tracks and socio-economic backgrounds react to different shares of advanced track students within a class, I provide split sample estimates across student characteristics and quartiles of class shares of advanced track students in the post-reform period in the reform canton. The results suggest that students with lower prior achievements and who are socio-economically less advantaged prosper more if their environment in the detracked classes is less competitive when compared to more advantaged students who benefit most from classes with either a very high or a very low share of advanced track students.

I probe the robustness of my findings, in two ways. First, I alter my identification strategy and use the reform as an instrument for detracked classes. Second, I vary the outcome model and apply the doubly-robust estimator proposed by Callaway and Sant'Anna (2021) relying on conditional parallel trends. Both exercises result in estimates of very similar magnitude as my main estimates and therefore confirm their robustness.

This paper contributes to the literature on the effect of tracking on achievements of the average student. Cross-country comparisons of tracking systems mainly find equalizing effects of detracked school systems on educational attainments (Hanushek and Woessmann 2006; Brunello and Checchi 2007; Contini and Cugnata 2016).[1] Even though these studies provide evidence for the equity concern of tracking, the comparability of a wide range of tracked and non-tracked systems as well as outcomes is questionable (Contini and Cugnata 2016), and the results might be related to other unobserved differences between the compared countries (Betts 2011). Approaches focusing on detracking reforms within countries are less prone to that critique. Some studies leveraging detracking reforms find effects on long-term outcomes, such as cognitive army test scores (Pekkala Kerr, Pekkarinen, and Uusitalo

1. Another strand of the literature focuses on the existence of specialized classes and leverages discontinuities around cut-offs on grade scales of prior educational assessments for being assigned to specialized classrooms for high-achievers (Card and Giuliano 2016; Cohodes 2020; Mouganie, Canaan, and Zhang 2022), and for low-achievers (Setren 2019; Figlio and Özek 2020). These studies show that the peer effects are stronger for low-achievers and children with less socio-economic advantaged backgrounds. Even though these results provide interesting insights in the heterogeneity of effects of a grouped classroom on achievements of marginal students, little can be concluded about effect heterogeneity on achievements of average students. Further, in contrast to the tracking system in Switzerland, specialized classrooms for low-achievers in these studies are specifically designed to promote learning for later reintegration into regular classes.

2013) or final educational attainment and earnings (Meghir and Palme 2005; Aakvik, Salvanes, and Vaage 2010). The limited research on effect heterogeneity suggests that the influence of family background is weaker in a detracked system (Aakvik, Salvanes, and Vaage 2010), while students with less advantaged socio-economic background gain most from detracking (Pekkala Kerr, Pekkarinen, and Uusitalo 2013; Meghir and Palme 2005). A caveat to these studies is that the reforms analyzed not only changed if and how students are tracked but included changes related to the minimum school-leaving age, curricula, or teacher quality and quantity. Therefore, the impact of detracked classes is not identified from the impact of these other changes.

The results of my study are most closely related to two recent studies of detracked school systems situated in Germany and France. The first study by Matthewes (2021) uses a triple-difference approach leveraging institutional differences between federal states in Germany to estimate the effect of detracking on student achievements. The results show that the positive average effects on grades are concentrated among low-achievers. While the research question and identification strategy is similar to my setting, three main differences emerge. First, Matthewes (2021) estimates average treatment effects for the bottom 60% of pupils on the distribution of prior achievements. This is because the top 40% of students are tracked into an academic track regardless of the state. Second, the author cannot draw conclusions on the heterogeneity of effects across socio-economic backgrounds of students due to lack of statistical power. Third, the focus on grades provide interesting results for short-term effects but evidence on longer-term outcomes is absent. The second study by Canaan (2020) uses a regression discountinuity design based on birth date to estimate the effect of a reform in France that delayed the age at which students are tracked by two years. Tracks consist of the lowest-achieving 18% of students and the 82% of higher-achieving students. She finds that the reform raised individuals' level of education and increased wages by 6% at ages 40 to 45. The heterogeneity analysis shows that the effects are concentrated among individuals with less advantaged socio-economic backgrounds, but among those only individuals with parents that were born in France gained, while the change did not affect children of immigrants. Even though the tracking system and the reform exploited is similar to my setting, I can advance these findings by three main points. First, the reform exploited by Canaan (2020) did not only affect the tracking system, but also changed the curriculum as well as the teacher quantity and quality. Hence, the effect cannot be exclusively attributed to the later tracking. Second, the reform had the goal of establishing mixed-ability classrooms, but effectively students were still tracked into classrooms according to their prior achievements within schools. Third, the

estimated effects can only be interpreted as intention-to-treat effects, since the actual treatment status of individuals is unobserved. This is in stark contrast to my setting, where I directly observe whether a student is in a tracked or a detracked class. Therefore, my results can be interpreted as average treatment effects on the treated.

In sum, I advance the existing literature in several ways. First, I leverage a within-country detracking reform that only changed the way students are grouped into classes and did not include additional changes such as changes of curricula, teachers, and financial resources. I can therefore interpret my average treatment effects on the treated as causal effects of detracked classes. Second, the detailed register data allow me to not only estimate effects for the average student, but to show that classroom composition in terms of socio-economic backgrounds of students changed dramatically after the reform. Third, I am able to provide split sample estimates along assigned track, parents' education, and native language of the effect of detracked classes on assignment to further education. All in all, these features allow me to contribute to the literature of the effect of detracked classes on assignment to further academic education, an outcome that is consequential for later labor market integration and earnings, and let me draw conclusions of the impact of detracked classes on education inequality.

2 Institutional background and data

2.1 Swiss school system

Education is a cantonal matter in Switzerland and therefore all 26 cantons are autonomous in how to organize their schools. However, since the Intercantonal Agreement on Coordination of Compulsory Education of October 29 1970, the cantons increased their degree of coordination such that overall education in Switzerland has become increasingly harmonized (Fischer, Sciarini, and Traber 2010).[2] Compulsory education lasts eleven years, and starts with two years of kindergarten, starting at the age of four or five depending on birth date cut-offs.[3] Thereafter, they continue with elementary school for six years, followed by three years of lower secondary school.[4] At age 15 or 16, individuals complete

2. https://www.bildung-z.ch/sites/default/files/u189/Konkordat_Schulkoordination.pdf, accessed April 29, 2024.

3. There are cantons where kindergarten is not mandatory or in which only one year of kindergarten is mandatory.

4. There are cantons with elementary schools of only five years and secondary schools of four years

compulsory education by graduating from lower secondary school, but almost all individuals continue with upper secondary education for a maximum of five years. In post-compulsory upper secondary education, students either apply or get assigned to different types of education programs according to their skills and preferences. The vocational education training (VET), into which roughly 49.7% of students enroll, is the most prevalent choice of upper secondary education.[5] It requires individuals to apply for a working contract guaranteeing them a two-to-four-year training period with an employer in their chosen profession. Students typically get on-the-job training on three to four days per week, and follow a curriculum tailored to their professional needs in specialized schools for one or two days per week. VET programs can be either more demanding three-or-four-year programs that lead to a federal certificate of competence (EFZ), or less demanding two-year programs leading to a federal vocational certificate (EBA). Both degrees either prepare students to join the labor market as specialized workers, or to continue with advanced on-the-job training, or other further education programs. Around 21.8% of individuals decide to pursue a more academic upper secondary education for four years, leading to either a specialized or a general baccalaureate with which they are eligible to enroll at all Swiss universities. Since both VET and further academic education are selective, approximately 19.7% of students join a non-selective transitional education option. In these bridge years, students improve their competences and are supported in finding an apprenticeship position that optimally leads to a VET working contract. Only 7.8% of students do not follow any of these options after lower secondary school and are not registered in any educational institution after completion of compulsory school. These cases have to be interpreted as a mixture of dropouts, students with apprenticeship positions, and individuals who leave Switzerland.

In Switzerland, approximately 92% of eligible children are enrolled in public elementary schools, and roughly 90.8% of students are enrolled in public lower secondary schools, while the rest attend private schools. In public schools, children of different gender, ethnic, and socio-economic backgrounds are educated together whenever possible. Importantly, students do not choose their public school but are assigned to schools according to their residence by municipality officials, which reduces potential sorting of students across schools.[6]

5. All statistics in this section are reported for the years 2012-2022 considering the full sample of students in Switzerland.

6. Some cantons offer an academic lower secondary track, which in some cases are located in separate schools. In this case, students are not assigned to schools based on ZIP code, but choose their preferred school and are assigned upon availability.

Most cantons assign students to tracks after elementary school. Depending on the canton, evaluations are based on different indicators, such as students' most recent grades in school reports, performance in cantonal exams, and general social, and cognitive skills. Therefore, track assignment can be interpreted as a proxy for prior achievements or ability of a student. The curricula in lower secondary school are similar for students on both tracks, but requirements are higher for advanced track students. They are more rigorously evaluated when compared to students on the basic track and therefore expected to understand the material more in-depth. Even though there are cantonal differences in explicit track assignment mechanisms, the criteria are similar throughout Switzerland. Consequently, the proportions of students assigned to each track are comparable across cantons and vary only little over time.

Importantly, these track assignments rarely separate individuals across schools and do not necessarily group individuals into different classes. Approximately 19.3% of classes where students are assigned to tracks in the first year of lower secondary school contain students from different tracks. Also, students are not locked-in in their assigned tracks, but are eligible for reassignment at least every school year, depending on their grades.[7] Whether students graduate from lower secondary school on the advanced or the basic track is consequential for their opportunities in post-compulsory education. In some professions, students are only eligible to apply for a VET, if they graduated from the advanced track and students' prior achievements play a crucial role when applying for a position in their preferred occupation. This is because employers can typically choose from several candidates for one VET working contract, even if no specific track requirements apply. Entrance to further academic education depends either on teacher recommendation or on passing a cantonal entry exam (see Appendix section C.1 for more details). A simplified graphic of the Swiss school system including the timing of track assignment and the different possibilities of grouping students into classes is depicted in Appendix Figure A.1.

2.2 Data

To analyze the effect of detracked classes on students' probability of further academic education I draw on the universe of students in Switzerland covered in the LABB data (*Längsschnittanalysen*

7. Roughly 11.2% of pupils initially assigned to the basic track finish lower secondary school on the advanced track, while roughly 5.2% of students initially assigned to the advanced track graduate from lower secondary school on the basic track.

im Bildungsbereich, longitudinal analyses in the field of education) provided by the Swiss Federal Statistical Office. Yearly educational statistics are collected by the Swiss cantons and linked to labor registers via the social security number of students. This results in a comprehensive panel data set available from 2011 onwards, containing students' enrollment from kindergarten to university at all ages, and covers public and private schools. Besides a description on the type of educational program a student is enrolled in, the data contain information on attempts for grade completion and success or failure of an attempt. The data further contain individual, class, and school identifiers, from which I construct cohort identifiers consisting of school-by-grade observations. It is this detailed information on the type of educational program, the class identifiers reported in the LABB data, and the fact that most cantons assign students to an advanced or a basic track for the first year of lower secondary school that provides me with the information of how students are grouped into classes in lower secondary school.[8]

Further, the LABB data include month and year of birth, gender, native language, nationality, and information on the permit of residence. The information on municipality of residence and school allows me to include regional characteristics such as urbanity or language region. From the native language of an individual and the language region of the school municipality I construct an indicator for whether a student is a native speaker of the regionally prevailing language. Additionally, information on the highest educational degree of a household is available for a subsample of students in the LABB data from the Swiss Structural Survey. The survey covers a yearly sample of 10% of the permanent resident population aged 15 years and older. Relying on the assumption that lags or leads of the highest level of education in a household are good proxies for current education within a household, I am able to impute this social background variable for roughly 64% of students observed in the first year of lower secondary school.

Applying for a VET working contract after compulsory school might depend on the situation on the labor market at the point in time when students start to decide on their further education trajectory. To include information on the options on the labor market, I use data of the State Secretariat

8. The cantons of Lucerne, Nidwalden, Basel-Stadt, Thurgau, Ticino, and Valais do not assign or do not report tracks of students for some or all cohorts in the data. These amount to 26% of individuals in the first year of public lower secondary school across Switzerland. Additionally, students in private schools are often not tracked even in cantons that usually track students. These amount to 1.3% of overall individuals in the first year of lower secondary school. Students with special needs due to learning difficulties, disabilities, or recent migration histories are assigned to special educational tracks. This is the case for approximately 0.8% of students between 13 and 16. I will drop these individuals in my final estimation sample but I keep classes which contain them, so they are accounted for when calculating class and cohort size and the corresponding shares of individual characteristics.

for Economic Affairs (SECO) providing the monthly number of registered unemployed individuals for each municipality. These can be linked to the end-of-year number of permanent residents per municipality that is provided by the Federal Statistical Office (FSO) to calculate yearly averages of the share of unemployed individuals in each municipality.[9] I then link these shares to the municipality of residence of students in the LABB data. From this household education variable, I construct an indicator for whether a student has parents with tertiary education.

2.3 Definition of outcomes

My main outcome is the probability of being assigned to further academic education after lower secondary school. I create an indicator for enrollment into education programs that prepare students for tertiary education. Successful completion of such programs enables individuals to enroll in all Swiss universities.[10] Young adults with tertiary education report approximately 50% higher wages relative to the population with upper secondary education in Switzerland, as can be inferred from OECD data.[11] Therefore, enrollment into further academic education is consequential for later labor market integration and earnings of students. I include two additional outcomes concerning other educational trajectories of students. First, I classify a VET as demanding if the share of advanced track students enrolled in the specific VET in the first year of post-compulsory education is higher than the median share of advanced track students enrolled in all VETs. Second, I consider a student a dropout if she is not registered in the LABB data after completing compulsory education.[12]

9. The permanent resident population statistic is only published at the end of each year, hence I am not able to calculate monthly averages. Note that if the number of unemployed individuals in a municipality is less than 5, the data remains unpublished to ensure anonymity. In that case, I use the cantonal average to refrain from systematically excluding small and often rural municipalities.

10. If an individual is registered in such a program in the first year of post-compulsory education at age 15 or 16 but drops out of the education program later, she will still be considered as being assigned to further academic education.

11. Source: https://www.oecd-ilibrary.org/education/data/oecd-education-statistics/education-at-a-glance-earnings-by-educational-attainment-edition-2022_f6608326-en, accessed on June 10, 2024.

12. Note that individuals who potentially return one or more years later will still be considered dropouts.

3 Policy change and empirical design

3.1 Grouping students into lower secondary school classes

Even though curricula are increasingly harmonized, the federal federal structure of the Swiss education system results in considerable heterogeneity in the organization of schools across cantons. To evaluate the effect of detracked classes on the probability of further academic education, I focus on differences of grouping students into lower secondary school classes. I thereby focus on the class composition in the first year of lower secondary school for which students are assigned to tracks. During elementary school, changes in class compositions are minor, but students are grouped into new classes for lower secondary school, which also remain roughly unchanged until the end of compulsory education. There are two possibilities of grouping students assigned to different tracks into classes. Classes can either be tracked, containing only students of either the advanced or the basic track, or they can be detracked, containing students of both tracks. Importantly, in both tracked and detracked classes, students are assigned to tracks, and track assignment is observed for all students. If classes are tracked, students of different tracks follow all subjects in tracked classes. If classes are detracked, all subjects are taught in detracked classes with the exception of main subjects.[13] These are offered in tracked level classes with a more demanding and a less demanding level. In this case, the number of main subjects for which a student is assigned to the more demanding level determines whether the student is on the advanced or on the basic track. Some cantons have a general practice of either only grouping students into tracked or detracked classes, while other cantons leave the decision to the school principals. These cantons show considerable within-canton variation of the two systems, but in both cases the school principals ultimately group the students into classes. Therefore some within-canton heterogeneity remains also in cantons with a general practice.

13. Main subjects typically are mathematics, the regional language, and in bilingual cantons the second cantonal language in the first year of lower secondary school. In most cantons, natural sciences and English are taught in level classes from the second year of lower secondary school onward.

3.2 The detracking reform in the canton of Neuchâtel

I exploit a detracking reform in the canton of Neuchâtel that mainly changed the grouping of students into lower secondary school classes in 2015. Only two minor additional organizational changes were made in the school system in the canton of Neuchâtel at the time of the reform.

First, and most importantly, classes in lower secondary school were detracked, which is explicitly stated in the official report of the reform (Conseil d'État 2013). In the pre-reform period, students are grouped into classes according to their assigned tracks from the first year of lower secondary school onward for all subjects. In the post-refrom period, students were only grouped into classes according to assigned levels in French and mathematics, amounting to one third of overall weekly lessons. For all other subjects, students are grouped into classes regardless of these levels in the first year of lower secondary school. After one year, students are assigned to one of two levels for three additional subjects, and are grouped into classes according to assigned levels in a total of five subjects. These subjects account for two thirds of weekly lessons in the second and third year of lower secondary school. Since the assignment to levels is subject-specific, the post-reform system enables students to follow more individualized education trajectories according to their abilities. This was specifically aspired by the reform.

Second, as a minor organizational change, the timing of tracking and the sub-division of tracks slighlty changed with the reform. In the pre-reform period, tracking started in a diluted form in the last year of elementary school. Students were assigned to provisional tracks based on teachers' recommendations at age ten or eleven, and were grouped into classes according to their provisionally assigned tracks for one year. For the first year of lower secondary school, students were definitively assigned to a track and classes separated students of different tracks thereafter. Students on the advanced track were assigned to either a more demanding or a less demanding subtrack, and grouped into classes by subtrack.[14] In the post-reform period, tracking starts in the first year of lower secondary school. Students are assigned to a more demanding or a less demanding level in French and mathematics. If a student is assigned to the more demanding level in at least one of the two subjects, she is on the advanced track, and on the basic track otherwise.

Third, the exact mechanism of track assignment changed slightly with the reform. Prior to the reform, students were assigned to tracks based on three indicators: the results in cantonal exams in

14. I explain the different ways to further subdivide tracks in Appendix section C.2.

the main subjects, end-of-year grades in all subjects, and teacher recommendations. After the reform, the assignment to either the more or the less demanding level in French and mathematics is based on end-of-year grade cut-offs in the last year of elementary school. If these end-of-year grades of a student were a very close cut, results of the cantonal exams in the respective subject, the opinion of the class council, and the parents' opinion were considered for definitive level assignment. As a consequence, elementary school teachers' opinions mirrored in end-of-year grades are more influential in the post-reform period than prior to the reform. Crucial for the interpretation of my results is that the shares of students on the advanced and the basic track remained roughly stable around the reform and comparable to the shares in control cantons. Further, the individual characteristics of students assigned to each track did not experience major changes after the reform. I will provide further evidence for this claim when discussing my results in section 4.

It is important to note that the reform in the canton of Neuchâtel did not change the resources available for lower secondary schools. The amount of schools and classes remained stable. The policy document explicitly states that the canton wanted to keep the teachers that were already employed and did not foresee hiring more teachers (Conseil d'État 2013).[15] The reform featured some temporary offer of further education for teachers after implementation. This further education prepared all teachers to teach in detracked classes, and instructed formerly basic track teachers to teach students of higher levels, and formerly advanced track teachers to be more sensitive to the needs of basic track students. Other than those temporary offers, no additional resources were devoted to the education system in the canton of Neuchâtel during the reform. In sum, even though the reform featured other minor changes regarding the organization of the school system in the canton of Neuchâtel, the main impact manifested in the composition of lower secondary school classes.

3.3 Empirical design and sample selection

I use a difference-in-differences estimator to identify the causal effect of detracked classes on the probability of assignment to further acdemic education, leveraging the detracking reform. In the canton of Neuchâtel, students enrolled in different cohorts in lower secondary school differ in their probabilities of being grouped into tracked or detracked classes. To disentangle the effect of detracked

15. This was also confirmed by officials of the school administration of the canton of Neuchâtel, who I contacted to acquire more knowledge about the exact implementation of the reform.

classes from general trends in outcomes, I use students enrolled in lower secondary schools in cantons that generally track their students into classes as control group. Additionally, for the validity of my difference-in-differences design, the selected control cantons have to meet two important institutional criteria.

The first criteria is that students should not be able to influence their treatment status. Students could self-select themselves into a tracked or a detracked class by influencing the school in which they are enrolled. Therefore, I only keep individuals with residence in cantons where all public lower secondary schools are instructed to track classes as control observations.[16] Still, students in both the selected control cantons and the canton of Neuchâtel could self-select themselves into tracked or detracked classes by moving across cantons or by enrolling into private schools. Appendix figures A.2 and A.3 show that there are no irregularities in the shares of students moving across cantons and enrolled in private schools at the time of the transition to secondary school. Further, individuals in the canton of Neuchâtel could potentially influence their treatment status by influencing the time of first enrollment into lower secondary school. The year a student is first enrolled in lower secondary school depends on the year of first enrollment in kindergarten or elementary school, which typically depends on birth date cutoffs. After enrollment, school is mandatory and gap years are very rare. To change the year of first enrollment in lower secondary school, students either have to repeat a grade to postpone their transition to lower secondary school, or skip a grade to advance the transition. Repeating a grade to influence the treatment status is possible if performance is poor, but very costly as students lose a year. Skipping a grade is only possible if students perform extremely well and is rare towards the end of elementary school. Appendix figures A.4 and A.5 show that these behaviors do not demonstrate any irregularities at the time of the reform.

The second criteria is that students in the control group should have a similarly limited extent of being able to influence their assigned tracks as individuals in the canton of Neuchâtel. This is to ensure that the assignment mechanism to tracks is not correlated with unobservables that potentially differ between the control and the treated observations. If the possibilities to influence the final track assignment differ between the two groups, students assigned to either track might differ systematically

16. Even though assignment to school is based on students' ZIP code of residence and the cohort is based on school-entry age, I cannot observe potential endogeneity within municipalities because I do not observe students' ZIP code. Hence, I cannot ensure that students are unable to influence the school they attend, and ultimately their treatment status in cantons that do not have a general practice of grouping students into classes.

as well.[17] Therefore, I carefully investigated the mechanisms of assigning students to different tracks and limited my sample to students with residence in cantons where the scope to contest the assignment to tracks is similar as in the canton of Neuchâtel.[18] Second, individuals in both the potential control cantons and the canton of Neuchâtel should not experience contemporaneous other policy changes that are related to the education system and might influence the outcomes. I therefore exclude cantons that underwent reforms regarding the length of elementary school or the track assignment during my observation period.[19]

The above two criteria for valid control cantons leave me with the following ten cantons in which individuals suit as control observations: Appenzell Innerrhoden, Fribourg, Geneva, St. Gallen, Schaffhausen, Solothurn, Schwyz, Glarus, Grisons, and Zug. Appendix Figure A.6 shows my estimation sample on a Swiss map.

The individuals with residence in either my selected control cantons or the canton of Neuchâtel further have to fulfill three main requirements. First, each individual has to be enrolled in the first year of lower secondary school at least once in the years 2012-2018 (see Appendix section C.4 for further explanations regarding the observation period).[20] Second, an individual has to be enrolled in a school located in one of my selected control cantons in the first year of lower secondary school or in the canton of Neuchâtel. Third, an individual has to be assigned to either the advanced or the basic track in the first year of lower secondary school.

Table 1 shows averages of the main covariates for the canton of Neuchâtel in the first column, for the selected control cantons in the second column and the third column serves as a comparison on how representative my selected sample is compared to the entire sample of Swiss students. The probability of assignment to further academic education is higher in the canton of Neuchâtel when compared to the selected control cantons. This is due to the fact that VETs are more popular in

17. I do not observe whether a student or his parents contested track assignment, and hence I do not observe whether a student is assigned to his track by the elementary school teacher directly or whether he influenced the assignment.

18. I explain the scopes to influence the assignment for each canton in detail in Appendix section C.3.

19. From the cantons that advise schools to separate students into classes according to tracks (Aargau, Appenzell Innerrhoden, Basel-Landschaft, Basel-Stadt, Fribourg, Geneva, Glarus, Grisons, St. Gallen, Schaffhausen, Schwyz, Solothurn, Zug) I therefore exclude the cantons Aargau, Basel-Landschaft, and Basel-Stadt.

20. Note that this requires individuals to be registered in any grade in the LABB data. This is often not true for individuals who have special needs who are enrolled in regular lower secondary school classes. I exclude these individuals from my analysis both because they are not registered in a specific grade and because most often they do not have an assigned track. I nevertheless include these individuals into the total amount of students in a class, so they contribute to the calculation of class and cohort shares in the total amount of students.

the German speaking cantons of Switzerland. The shares of students starting a demanding VET is also higher in the canton of Neuchâtel, while the probability of dropout is similar to control cantons. Importantly, the share of students assigned to the advanced track is similar across all columns of Table 1. Additionally, shares of the indicators for individual characteristics are comparable. Partial tracking indicates whether in a school-by-grade observation, at least one class is tracked and at least one class is detracked. Hence, partial tracking is similar to non-compliance in my setting and very low in both the canton of Neuchâtel and the control cantons. Further, class size and the number of first year lower secondary school classes in a school in a year, which is a proxy for average school size, are very similar for Neuchâtel and the control cantons.

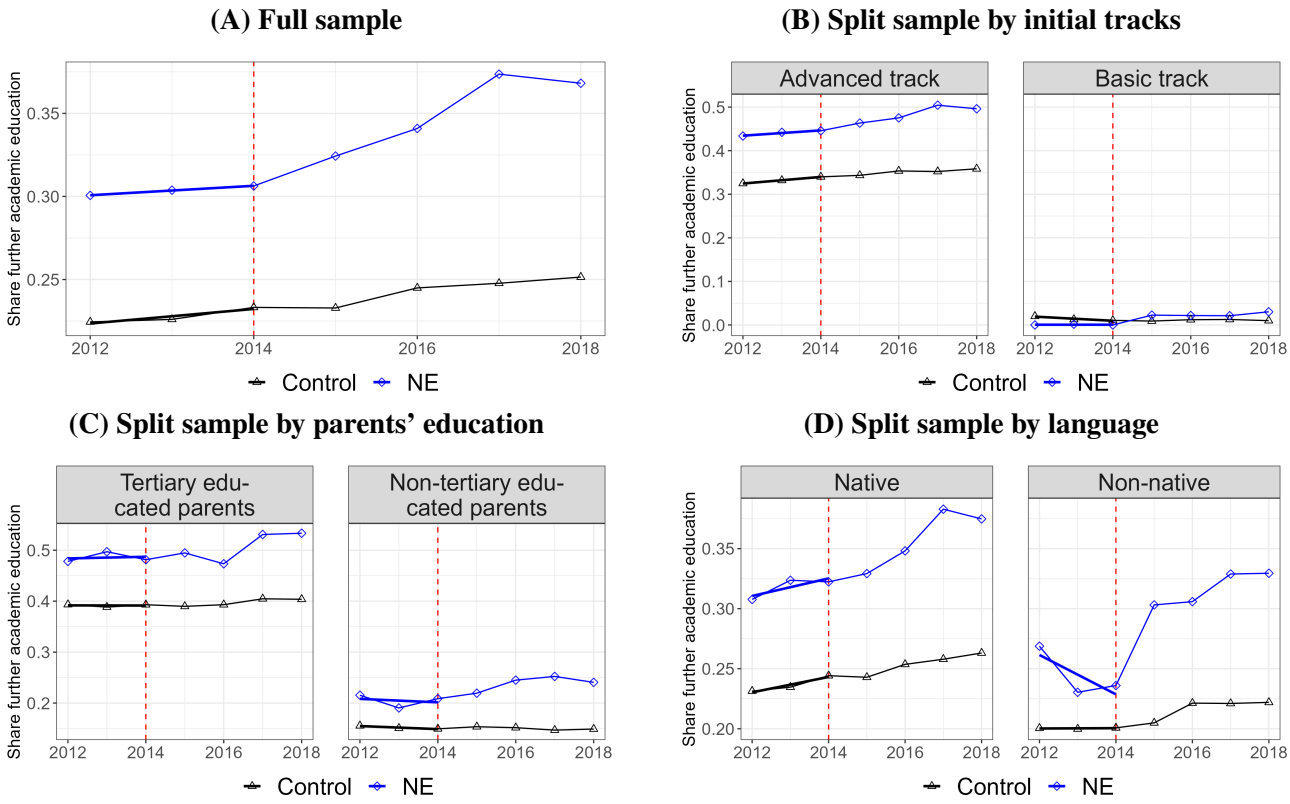
The main identifying assumption for the validity of a difference-in-differences design is the parallel trends assumption. This assumption states that in the absence of the treatment, treated and control groups would have experienced similar trends in the means of their dependent variables. To check its plausibility, it is common practice to plot the trends in outcomes prior to the reform for treatment and control units. Figure 1 shows the parallel trends assumption graphically for the probability of assignment to further academic education in the full sample in panel (A), for split samples by assigned tracks in panel (B), by an indicator for tertiary educated parents in panel (C), and by an indicator for being a native speaker of the regional language in panel (D). Additionally, Appendix Table A.1 shows the plausibility of the parallel trends assumption using a pre-trends test for the full sample and the split samples. Both exercises provide evidence in support of the parallel trends assumption in the full sample. The probability of being assigned to further academic education shows parallel trends for students on the advanced track. For basic track students, the evidence suggests a divergence in trends prior to the detracking reform in panel (B). In the sample splits by an indicator for tertiary education of parents, the parallel trends assumption is plausible when relying on the graphical evidence and on the estimates of the pre-trends tests. For the split sample by language, even though the pre-trends test suggest otherwise, the graphical evidence in panel (D) of Figure 1 shows a divergence in trends prior to the reform for non-native speakers. I therefore interpret the estimates for students assigned to the basic track and for students who are not native speakers of the regional language with caution in section 4. Additionally, I address the divergence in trends and potential concerns regarding the limited number of pre-reform periods in my robustness checks, by varying the identification assumption and by using a doubly-robust estimator relying on conditional parallel trends (Callaway and Sant'Anna 2021).

Table 1: Descriptive statistics

	Neuchâtel	Controls	Switzerland
	Mean (SD)	Mean (SD)	Mean (SD)
Outcomes:			
Academic track	0.331 (0.470)	0.237 (0.425)	0.223 (0.416)
Demanding VET	0.607 (0.489)	0.563 (0.496)	0.539 (0.498)
Dropout	0.075 (0.264)	0.075 (0.264)	0.077 (0.267)
Individual characteristics:			
Advanced track	0.700 (0.458)	0.682 (0.466)	0.689 (0.463)
Female	0.497 (0.500)	0.494 (0.500)	0.491 (0.500)
Age	12.574 (0.455)	12.837 (0.565)	12.850 (0.549)
Swiss citizenship	0.847 (0.360)	0.823 (0.382)	0.842 (0.365)
Native speaker of the regional language	0.821 (0.384)	0.741 (0.438)	0.726 (0.446)
Parents with tertiary education	0.485 (0.500)	0.475 (0.499)	0.486 (0.500)
Rural residence municipality	0.113 (0.317)	0.205 (0.404)	0.178 (0.382)
Unemployment school municipality	0.012 (0.004)	0.014 (0.008)	0.015 (0.007)
Cohort, class and school variables:			
Partial tracking	0.037 (0.190)	0.060 (0.237)	0.229 (0.420)
Class size	19.688 (2.672)	19.108 (4.306)	19.501 (4.024)
Number of classes	6.638 (2.389)	6.426 (3.873)	5.172 (3.023)
	N	N	N
Observations	12,361	143,330	491,705
Classes	669	8,501	29,137
School-by-grade observations	118	2,289	8,761
Schools	18	367	1,625

Notes: This table reports descriptive statistics of outcomes, individual characteristics, and cohort, class and school variables. I use data of the cohorts enrolled for the first time in lower secondary school during the years 2012 to 2018. Standard deviations are reported in parentheses. The first column shows averages for the canton of Neuchâtel, the second column shows the means for the selected control cantons, and the last column reports means for the entire population of pupils enrolled in the first year of lower secondary school.

Figure 1: Parallel trends for the probability of further academic education



Notes: This figure depicts the share of individuals who are assigned to further academic education after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) depicts the shares for the full sample, panel (B) shows the same shares in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language.

My estimation strategy also relies on the assumption that the shares of students assigned to the advanced and the basic track should be similar across the canton of Neuchâtel and the selected control cantons, and crucially should not change after the reform. The results displayed in Appendix Table A.2, checking for changes in cohort characteristics within split samples, and Appendix Table A.3 checking for changes in students' characteristics assigned to each track rule out this concern. Only students with tertiary educated parents are less likely to be assigned to the advanced track after the reform (see Appendix section C.5 for a discussion of potential implications).

Figure 2: Effect of the detracking reform on class shares of advanced track students

Notes: This figure depicts the leave-one-out class shares of advanced track students for students on the advanced and the basic track separately over time. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018. Panel (A) reports the class shares of advanced track students for the reform canton and panel (B) for the selected control cantons. In panel (A), red dots indicate classes that contain students of both tracks. Orange dots indicate non-compliers, defined as containing a mix of students on the advanced and the basic track in the pre-reform period, and containing only students of the same track in the post-reform period. Blue dots are classes containing students of only one track in both panels. In panel (B), non-compliers are defined as in panel (A) in the pre-reform period. The shares that are not exactly one or zero even though the class is tracked emerge due to the way I calculate the leave-own-out shares. If a student is repeating the first year of lower secondary school and was assigned to the basic track when she was enrolled in the first year of lower secondary school for the first time (her treatment year), but is on the advanced track when repeating the first year of lower secondary school, I count her to the total number of students in the class that she is repeating, but I do not count her as a student on the advanced track in that class. This is because she initially was assigned to the basic track and, hence, I consider her a basic track student. The class that she is repeating however is still tracked, since all students in that class, including her, are on the advanced track. Overall, there are 3.59 % (7.79 %) of classes are non-compliers in the canton of Neuchâtel (the control cantons). In the canton of Neuchâtel (the control cantons) 9.18 % (18.66 %) of classes have advanced track shares unequal to 1 or 0 in the tracked system. In the canton of Neuchâtel, 0.56 % of detracked classes in the post-reform period have advanced track shares of 1 or 0.

3.4 Effect of the reform on class shares of advanced track students

The detracking reform implemented for cohorts that started lower secondary school in 2015 changed the grouping of students into lower secondary classes in the canton of Neuchâtel when compared to control cantons. I show the direct effect of the reform graphically in Figure 2 where each point corresponds to one class. The figure depicts leave-own-out class shares of advanced track students over time separately for advanced and basic track students, for the canton of Neuchâtel in panel (A) and for the selected control cantons in panel (B). Prior to the reform, individuals on the advanced track in the canton of Neuchâtel report class shares of advanced track students of around one, while for individuals on the basic track, class shares of advanced track students are close to zero. After the reform, class shares of advanced track students decrease by roughly 30 percentage points for students on the advanced track, while they increase by roughly 70 percentage points for basic track students.[21] Classes are considered non-compliers in the canton of Neuchâtel, if they either contain both students on the advanced and on the basic track before 2015, or if classes contain only students of the same track after 2015. Approximately 1.3% of all first year public lower secondary school classes in the pre-reform period are non-complying, while 0.7% do not comply in the post-reform period.[22] The compliance is also very high in the selected control cantons, where the leave-own-out class shares of students on the advanced track are close to one or zero respectively. Non-compliance is defined as in panel (A) for the pre-reform period and amounts to roughly 1.4% of all first year public lower secondary school classes containing students of both tracks.

3.5 Estimation equation

I estimate the following two-way fixed effects model:

$$Y_{i,t} = \alpha_s + \alpha_t + \underbrace{\text{NE}_i \times \text{Post}_t}_{\text{Detracking}_{i,t}} \tau + \gamma \mathbf{X}_i' + \epsilon_{i,t} \tag{1}$$

21. The increase in the class share of advanced track students for students on the basic track is larger than the respective decrease for the advanced track students in percentage point terms because there are overall fewer students on the basic track.

22. A class is also considered non-complying if at least one student is on a track other than the advanced or the basic track. These can be students with special needs who are not assigned to a track, or students that are either still enrolled in elementary school or who are already enrolled in post-secondary education (potentially due to errors in the data).

$Y_{i,t}$ is the binary outcome of interest of individual i in year t, where t indicates the year in which the individual is first enrolled in the first year of lower secondary school. α_s captures school fixed effects for the school at time t, and α_t is a time fixed effect for the year in which a student is enrolled in the first year of lower secondary school for the first time. NE_i is a binary indicator for enrollment in a lower secondary school of individual i in the reform canton, and Post_t indicates whether the year a student is first enrolled in the first year of lower secondary school is after 2014. τ is the coefficient of interest, identifying the causal effect of detracked classes on the outcome of interest, while $\mathbf{X}_i'$ is a vector of time-invariant individual covariates including gender, a binary indicator for whether an individual is a native speaker of the of the school municipality, and a binary indicator for being Swiss in the year of first enrollment in the first year of lower secondary school. To capture a proxy for prior achievements and hence for the abilities of a student, I also include a dummy variable indicating whether an individual is assigned to the advanced or the basic track in the first year of lower secondary school. Additionally, I include the share of registered unemployed individuals per year and municipality if the outcome variables are the probability of enrollment into a demanding VET and the probability of dropout. Finally, $\epsilon_{i,t}$ is the error term, clustered at the school level, which is the level of treatment assignment.[23]

4 Results

4.1 Effects of the detracking reform on class composition

Since track assignment is correlated with individual characteristics of students, the detracking reform had indirect effects on the characteristics of students' peers. These indirect class composition effects differ for students on the advanced and on the basic track. Table 2 displays estimates from estimating equation 1 with leave-own-out class shares of different student characteristics as dependent variables separately for students on each track.[24] Column (1) documents the direct effect of the detracking

23. I cluster the error term at the school level to account for serial correlation in the error term at the school level. Even though the reform takes effect at the cantonal level, the ultimate decision in how to group students into classes is made by the school principal, even in cantons where there is a general practice in how to group students into classes. Therefore, the school level is the level of treatment assignment in the difference-in-differences model.

24. I document split sample estimates in Appendix Table A.2 exchanging the class shares by the respective cohort shares to show that the body of students did not experience large changes after the reform. Importantly, the share of advanced track students in the cohort did not change after the reform for students on both tracks.

reform on the class shares of students on the advanced track with opposing signs across split samples. This is the direct mechanical effect of the reform which is depicted in panel (A) of Figure 2 that changed the grouping of students on different tracks into classes.

Columns (2) to (5) provide evidence that the reform also had large indirect effects on class peer composition. These effects work in opposite directions for advanced and basic track students, because having tertiary educated parents, being Swiss, being a native in the regional language, and being female is correlated with assignment to the advanced track.[25] When comparing the estimates of panel (A) and (B) in Table 2, the detracking reform decreased the class share of students with tertiary educated parents by 6.8 percentage points for students on the advanced track, while it increased the same share for basic track students by 22.7 percentage points. Similarly, the class share of students with Swiss nationality decreased by 4 percentage points for advanced track students in the detracked classes, but did not have a statistically significantly effect on the share of students who are native speakers of the regional language, while it increased the class shares of students with Swiss nationality by 11.8 percentage points and of students who are native speakers of the regional language by 15.4 percentage points for basic track students. Being female is also correlated with track assignment and therefore the class share of females decreases by 3.2 percentage points for advanced track students and increases by 4 percentage points for basic track students with the reform. Further, since tracked classes are smaller for basic track students than for students on the advanced track, class size increases for the basic track students after the reform, and slightly decreases for advanced track students, as can be inferred from column (6) of Table 2. Overall, these split sample estimates show that the reform changed the class peer composition in terms of socio-economic background differently for students assigned to different tracks. The reform increased the share of peers with a less advantaged socio-economic background for students assigned to the advanced track, while students assigned to the basic track are more likely to share a classroom with peers from more advantaged socio-economic backgrounds after the reform. Hence, the effects of the increasingly heterogeneous classes might differ, conditional on own individual characteristics of students.

25. On the advanced (basic) track, 51.6% (44.3%) of the students are females and 56.2% (24.5%) have parents with tertiary education, 79.9% (58.3%) are native speakers of the regional language, and 89.8% (73.4%) are Swiss of all students with assigned tracks in Switzerland.

Table 2: Effects of the detracking reform on class peer composition

Panel (A):	Advanced track students					
Dependent variable:	**Advanced track share**	**Share tertiary educated parents**	**Share Swiss nationality**	**Share native speakers**	**Share females**	**Number of students**
	(1)	(2)	(3)	(4)	(5)	(6)
NE × Post	-0.3019	-0.0680	-0.0399	0.0017	-0.0316	-0.9931
	(0.0135)	(0.0109)	(0.0082)	(0.0108)	(0.0139)	(0.3434)
Pre-reform mean	0.985	0.525	0.906	0.847	0.516	21.000
Observations	106,395	106,372	106,397	106,397	106,396	106,399
R^2	0.6772	0.4216	0.4067	0.6711	0.4285	0.5557

Panel (B):	Basic track students					
Dependent variable:	**Advanced track share**	**Share tertiary educated parents**	**Share Swiss nationality**	**Share native speakers**	**Share females**	**Number of students**
	(1)	(2)	(3)	(4)	(5)	(6)
NE × Post	0.6644	0.2269	0.1182	0.1539	0.0399	2.6201
	(0.0149)	(0.0185)	(0.0119)	(0.0200)	(0.0136)	(0.4475)
Pre-reform mean	0.008	0.232	0.762	0.713	0.451	17.100
Observations	49,278	49,229	49,281	49,281	49,278	49,284
R^2	0.8070	0.2926	0.5294	0.6878	0.2051	0.4189
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Gender, Native, Swiss	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 using class shares of peer characteristics as dependent variables. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level, and split the sample by advanced and basic track students. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table 3: Average treatment effects in the full sample

Dependent variable:	**Further academic education**			
	(1)	(2)	(3)	(4)
NE × Post	0.0306	0.0308	0.0295	0.0295
	(0.0110)	(0.0095)	(0.0100)	(0.0100)
Pre-reform mean	0.304	0.304	0.304	0.304
Effect in percentages	10.07%	10.13%	9.70%	9.74%
Observations	155,691	155,691	155,691	155,691
R^2	0.0039	0.1475	0.3200	0.3200
School FE	No	No	Yes	Yes
Time FE	No	No	No	Yes
Controls:				
Female, Native, Swiss, Advanced track		✓	✓	✓

Notes: This table reports estimates from estimating equation 1 in the full sample. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I do not include controls, in column (2) I add time-invariant individual-level controls, in column (3) and (4) I subsequently add school and time fixed effects. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. Standard errors clustered at the school level are displayed in parantheses.

4.2 Average effect of detracked classes on the probability of further academic education

I first document the causal effect of detracked classes on the probability of further academic education after compulsory school for the average student. The results from estimating equation 1 are displayed in Table 3, where apart from column (1), all specifications include dummy variables for being female, being native in the regional language, being Swiss, and a dummy for being on the advanced track. In column (3), I additionally include school fixed effects and in column (4), I add time fixed effects. The results suggest that detracked classes increase the probability of further academic education by 3 percentage points for the average student and the estimates are robust to the inclusion of controls and fixed effects. The percentage increase corresponding to the effect is roughly 10% when taking into account the pre-reform mean of the probability of further academic education in the reform canton. Appendix Figure A.7 suggests that the average treatment effect on the treated increases over time in the full sample. These findings for the average students are in line with the results of other studies, suggesting that later tracking increases the overall education level (Hanushek and Woessmann 2006; Canaan 2020).

One caveat for the interpretation of the estimates in the full sample is that only one canton implemented the detracking reform. Even though the official report does not mention any aspirations to increase the number of students assigned to further academic education (Conseil d'État 2013), it is possible that the estimated effect is part of the reform and does not increase achievements of students. To explore this further, I check that the number of schools offering further academic education remains stable in the canton of Neuchâtel in my observation period. Similarly, the number of further academic education classes remains constant, while there is an increase in the number of students within further academic education classes after the reform. Therefore, I conclude that no additional teachers were hired for further academic education which might be the case if sending more students to further academic education was aspired by the canton at that time. Still, I cannot rule out that teachers assign students more often to further academic education for other reasons than higher achievements in detracked classes.

From a policy perspective, the ultimate goal should be to assign students to the most rewarding education trajectory in terms of later earnings conditional on their abilities. Therefore, solely increasing the number of students assigned to further academic education does not ensure that these students are necessarily better off in the long run. Further, students could still drop out of further academic education after assignment and potentially be worse off than if they had started a VET directly after compulsory school. To provide evidence on how likely it is that students who were sent to further academic education graduate by receiving a general baccalaureate, I calculate their rate of success, for each year.[26] This success rate is only observable for the students first enrolled in lower secondary school until 2015. The success rate is at around 80% for the pre-reform years, and remains stable for the first post-reform year in 2015.

4.3 Heterogeneous effects of detracked classes on the probability of further academic education

To explore the differential impact of detracked classes on the probability of further academic education, I split my sample based on initially assigned track and socio-economic backgrounds of individuals. To see whether these split sample estimates differ for students of different background is especially

26. Note that I calculate the success rate as graduating after the regular number of years. If an individual graduates later this counts as a fail.

interesting to relate the effect of detracked classes on further academic education to a potential impact on education inequality and intergenerational mobility of education.

Columns (1) and (2) of Table 4 show the effect of detracked classes on the probability of further academic education separately for students assigned to the advanced and to the basic track. The point estimate of 3.6 percentage points corresponds to a rise of roughly 8% in the probability of pursuing further academic education for students on the advanced track. Since the parallel trends assumption does not hold for students on the basic track (see section 3.3), I do not overinterpret this split sample effect. However, the pre-reform mean in the canton of Neuchâtel reported in Table 4 shows that in tracked classes the probability of further academic education is close to zero for these students, while in detracked classes it rises above zero. Another concern with the split sample estimate for the basic track students is the fact that students on the basic track are more likely to have tertiary educated parents after the reform. I discuss implications of this finding for the interpretation of this split sample estimate in Appendix section C.7.

In columns (3) and (4) I split the sample according to an indicator for tertiary education of parents where only the estimate for students with non-tertiary educated parents is statistically significantly different from zero. The effect of the detracked classes corresponds to an increase of 16% in the probability of further academic education for these students. Again, concerns can be raised about the split sample estimate for students with tertiary educated parents, since students with tertiary educated parents are more likely to be assigned to the basic track after the reform in the canton of Neuchâtel. This is likely caused by compositional changes in the availability of information on parents' education in the data (see section C.6). To address concerns about potential differential changes in unobservables for students with tertiary educated parents compared to students with non-tertiary educated parents after the reform in the canton of Neuchâtel, I estimate my split sample estimates in column (3) and (4) again assigning all students with missing parents' education to each of the two split samples. The size of the estimates is very similar as in my main specifications, which confirms the robustness of my results. The detailed implications for the interpretation of this split sample estimate are discussed in Appendix section C.7.

The effects reported in columns (5) and (6) of Table 4 show a larger positive impact of detracked classes on individuals who are not native speakers of the regional language when compared to students who are native speakers. However, even though the pre-trends documented in Appendix Table A.1 support evidence for the parallel trends assumption in these split samples, the graphical evidence in

Table 4: Average treatment effects education in split samples

Dependent variable:	Further academic education					
Sample:	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)
NE × Post	0.0363	0.0258	0.0228	0.0323	0.0255	0.0489
	(0.0129)	(0.0054)	(0.0162)	(0.0112)	(0.0113)	(0.0175)
Pre-reform mean	0.440	0.0006	0.487	0.206	0.318	0.243
Effect in percentages	8.25%	4387.76%	4.68%	15.63%	8.03%	20.11%
Observations	106,404	49,287	45,428	50,224	116,204	39,487
R^2	0.2695	0.0563	0.3465	0.2616	0.3162	0.3513
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Female, Swiss	✓	✓	✓	✓	✓	✓
Advanced track			✓	✓	✓	✓
Native	✓	✓	✓	✓		

Notes: This table reports estimates from estimating equation 1 within split samples. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In columns (1) and (2) I split the sample by assigned track, in columns (3) and (4) I provide split sample estimates by parents' education, and in columns (5) and (6) by a dummy for being a native speaker of the regional language. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Figure 1 raises concerns about a potential divergence in trends for students who are not native speakers of the regional language, so I do not interpret this estimate as causally identified. Appendix Figure A.7 shows no patterns in the dynamics of all split sample treatment effects.

Even though most of the split sample point estimates are not statistically significantly different from each other, my results suggest that the overall positive effect of detracked classes on further academic education is rather concentrated among individuals from socio-economically less advantaged backgrounds. This is in line with other results in the literature showing that school tracking reinforces the impact of family background for education and labor market outcomes (Brunello and Checchi 2007). Similarly, postponing tracking is found to be beneficial for education levels (Meghir and Palme 2005), wages (Canaan 2020), and other achievements (Pekkala Kerr, Pekkarinen, and Uusitalo 2013) of students with parents that have a low income, low education levels, or that are unskilled.

To explore the heterogeneity of the effects of detracked classes on the probability of further academic education in more detail, I further split my sample by two individual characteristics of

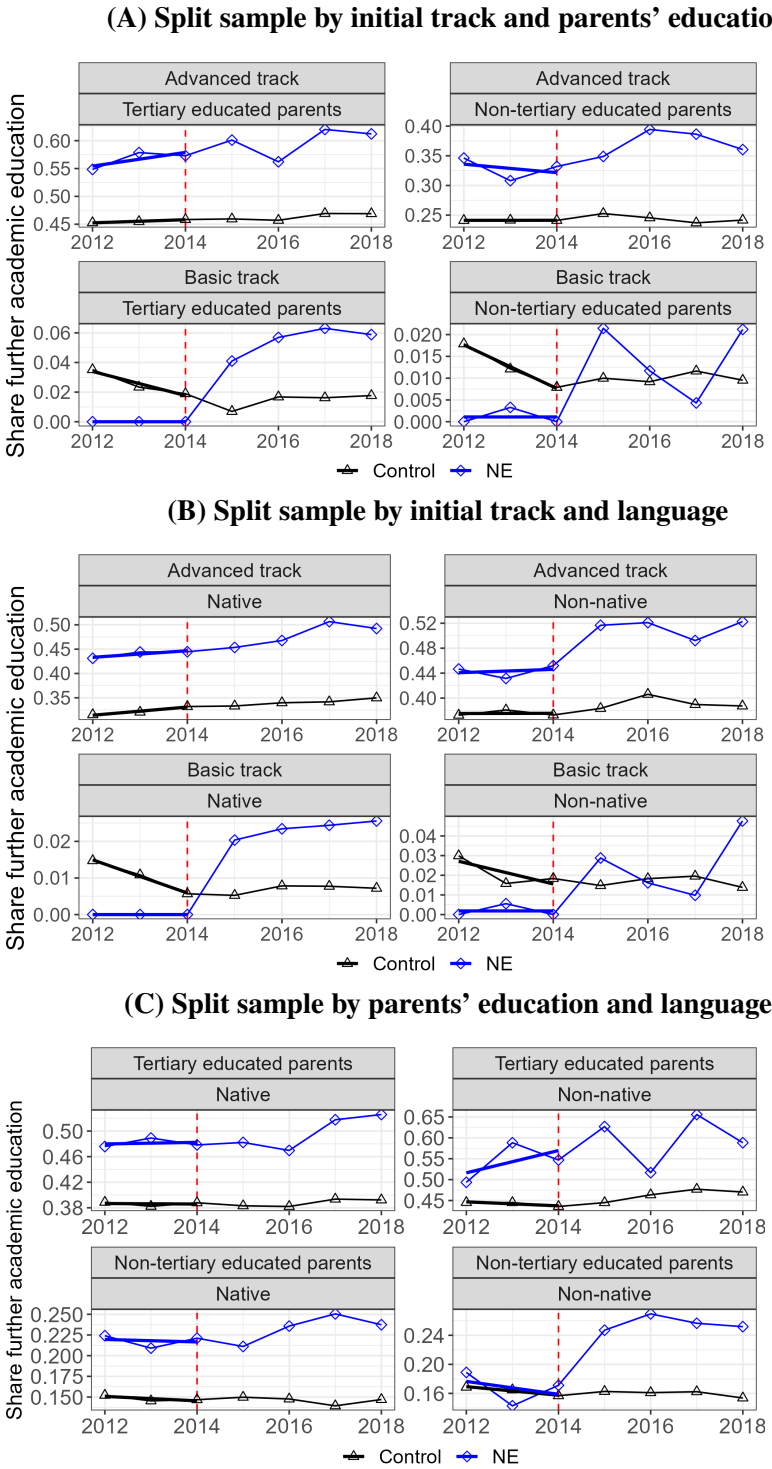
students. In panel (A) of Figure 3, I show the trends in the probability of further academic education for split samples according to initial track assignment and tertiary education of students' parents and I report the estimates of the effect of detracked classes on the probability of further academic education in panel (A) of Table 5.[27] I do not interprete the effects for students on the basic track because the graphical evidence shows a divergence in trends prior to the reform. For students on the advanced track the parallel trend assumption seems to be credible and even though the estimates in columns (1) and (2) in panel (A) of Table 5 are not statistically significantly different from each other, they suggest that advanced track students with non-tertiary educated parents tend to benefit more from detracked classes when compared to advanced track students with tertiary educated parents.

Panel (B) of Figure 3 and Table 5 show the probability of further academic education over time for the split sample by initial track assignment and by an indicator for being a native speaker of the regional language. Again, I only interpret the effects for students on the advanced track since there the identification assumption is fulfilled when judging from graphical evidence. Comparing columns (1) and (2) of panel (B) in Table 5 shows that the positive effect of detracked classes on advanced track students for the probability of further academic education is higher for students who are not native speakers of the regional language than for students who are native speakers. The effects are statistically significantly different from each other and while the estimate for native speakers suggests an increase in the probability of further academic education of only 7%, the percentage increase is more than twice as large for students who are not native speakers of the regional language.

Finally, panel (C) of Figure 3 shows the evolution of the probability of further academic education in split samples by an indicator for tertiary educated parents and for being a native speaker of the regional language, and panel (C) of Table 5 reports the corresponding estimates. Pre-treatment trends seem to only diverge for students with tertiary educated parents who are not native speakers of the regional language, while visual inspection of the parallel trends assumption allows me to interpret the other estimates. From these, only the estimate for individuals with non-tertiary educated parents who are not native speakers of the regional language reported in column (4) in panel (C) of Table 5 is statistically significantly different from zero. It shows that the probability of further academic education increases by 7 percentage points through the detracked classes, which corresponds to an effect size in percentage terms of over 40%. Hence, the detracked classes nearly double the probability of further academic education for these socio-economically least advantaged students.

27. Since the sample size decreases, I only provide graphical evidence and do not report regressions for pre-trends.

Figure 3: Parallel trends for the probability of further academic education in two-way split samples



Notes: This figure depicts the share of individuals who are assigned to further academic education after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level within split samples. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) shows the split sample by initial track and parents' education, panel (B) by initially assigned track and language, and panel (C) by parents' education and language.

Table 5: Average treatment effects in two-way split samples

(A) Split sample by initial track and parents' education

Dependent variable:	Further academic education			
Sample:	**Advanced track Tertiary educated parents**	**Advanced track Non-tertiary educated parents**	**Basic track Tertiary educated parents**	**Basic track Non-tertiary educated parents**
	(1)	(2)	(3)	(4)
NE × Post	0.0301	0.0491	0.0609	0.0166
	(0.0181)	(0.0170)	(0.0133)	(0.0062)
Pre-reform mean	0.566	0.329	0	0.001
Effect in percentages	5.32%	14.92%	–	1492.34%
Observations	38,856	30,526	6,572	19,698
R^2	0.3012	0.2389	0.1080	0.0540
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Swiss, Native	✓	✓	✓	✓

(B) Split sample by initial track and language

Dependent variable:	Further academic education			
Sample:	**Advanced track Native**	**Advanced track Non-native**	**Basic track Native**	**Basic track Non-native**
	(1)	(2)	(3)	(4)
NE × Post	0.0316	0.0709	0.0252	0.0256
	(0.0148)	(0.0278)	(0.0049)	(0.0099)
Pre-reform mean	0.440	0.443	0	0.0021
Effect in percentages	7.17%	16.01%	–	1229.62%
Observations	85,593	20,811	30,611	18,676
R^2	0.2719	0.2758	0.0568	0.0627
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Swiss	✓	✓	✓	✓

(C) Split sample by parents' education and language

Dependent variable:	Further academic education			
Sample:	**Tertiary educated parents Native**	**Tertiary educated parents Non-native**	**Non-tertiary educated parents Native**	**Non-tertiary educated parents Non-native**
	(1)	(2)	(3)	(4)
NE × Post	0.0180	0.0848	0.0197	0.0692
	(0.0177)	(0.0354)	(0.0125)	(0.0188)
Pre-reform mean	0.481	0.541	0.218	0.166
Effect in percentages	3.74%	15.68%	9.04%	41.69%
Observations	39,197	6,231	34,847	15,377
R^2	0.3403	0.4195	0.2536	0.3026
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Swiss, Advanced track	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 within two-way split samples. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In panel (A) I split the sample according to initial track and parents' education, in panel (B) by initial track and native language, and in panel (C) by parents' education and native language. I include time-invariant individual-level controls and school and time fixed effects in all specifications. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" expresses the treatment effect relative to this baseline. Standard errors clustered at the school level are displayed in parantheses.

To check for even more granular effects, I use a three-way sample split and document the results in Appendix Figure A.8 and Table A.11. The results confirm the results in split samples by two student characteristics, even though the parallel trends assumption is not plausible for most of these split samples.

In sum, the split sample analysis along two or even three individual characteristics of students provides support for the suggestive results found for split samples by one individual characteristic, and even expands their scope by the statistically significant differences of the estimates. The effect of detracked classes on students who are less socio-economically advantaged, captured by parents who are not tertiary educated and by not being a native speaker of the regional language on the probability of further academic education is larger, when compared to more advantaged students. These results are especially interesting in terms of the potential of detracked classes to increase equality of opportunity and to decrease intergenerational patterns of tertiary education. They also relate to previous findings in the literature documenting that detracked school systems affect education equality positively using cross-country comparisons (Hanushek and Woessmann 2006; Brunello and Checchi 2007; Contini and Cugnata 2016). Similarly, results from analyses of detracking reforms suggest that intergenerational patterns of education are lower in detracked systems (Aakvik, Salvanes, and Vaage 2010), and that low-achieving students (Matthewes 2021) and students from low socio-economic backgrounds gain most from detracking (Pekkala Kerr, Pekkarinen, and Uusitalo 2013; Meghir and Palme 2005; Canaan 2020).

4.4 Peer Effects

To assess whether class assignment in detracked classes depends on individual-level covariates, I regress different leave-own-out class shares on individual characteristics of students in the canton of Neuchâtel after the reform. Overall, the results documented in Appendix Table A.4 suggest statistical significance of these correlations for only three out of twenty-four estimates, and therefore support the randomness of the class shares of advanced track students in detracked classes.[28]

28. When regressing the leave-own-out class share of Swiss students on student characteristics, the estimate on being Swiss is marginally statistically significant. This means that the average Swiss student experiences a 1 percentage points higher leave-own-out class share of students who are Swiss than foreign students. Regressing the leave-own-out class share of female students on student characteristics shows a statistically significant negative relationship for being female. The average male student experiences a 3 percentage point lower leave-own-out class share of female students than the average female student. Estimating the correlation between student characteristics and class size shows that students on the advanced track experience classes that contain 0.13 more students on average than students on the basic track.

To see whether a higher share of advanced track students has a positive effect on the probability of further academic education in detracked classes, I estimate equation (1) by replacing the post-reform indicator by the leave-own-out class share of advanced track students and restricting my sample to the post-reform period in the canton of Neuchâtel. Appendix Table A.5 does not provide evidence of an effect of a higher class share of advanced track students on the probability of further academic education in detracked classes. To explore a potential non-linearity of the effect of different class shares of advanced track students, I split the post-reform observations in the canton of Neuchâtel into four groups of different leave-own-out class shares of advanced track students. I then estimate equation (1) separately for each group of post-treatment observations, using the full sample of pre-treatment observations in the canton of Neuchâtel and the full sample of control observations. The results documented in Appendix Table A.6 suggest that the average student benefits most from the detracking reform in classes with class shares of advanced track students above 50%. Note however, that there are not many classes with below 50% of advanced track students in the post-reform period in the canton of Neuchâtel.

To check for potential effect heterogeneity, I regress the probability of further academic education on the leave-own-out class shares of advanced track students in the canton of Neuchâtel after the reform within split samples across assigned tracks and indicators for socio-economic backgrounds of students. The results displayed in Appendix Table A.7 are not statistically significantly different from zero.

To further investigate the heterogeneous effects allowing for non-linearity in the effect of different class shares of advanced track students, I again subset the observations in the post-reform period in the canton of Neuchâtel in four groups according to their class shares of advanced track students. The results documented in Appendix Table A.8, A.9, and A.10 do not allow a conclusive interpretation of differences in the effect of different class shares of advanced track students on the probability of further education within split samples. Since there are not many classes with below 50% of advanced track students in the post-reform period in the canton of Neuchâtel, meaningful comparisons between split samples mainly result from comparing columns (3) and (4) of these tables. It becomes evident, that advanced track students tend to benefit more from classes with a higher share of advanced track students when compared to basic track students. These students might be more prone to increased competition and therefore perform worse in classes with a high share of high-achieving peers. The

comparisons based on parents' education and native language of students do not allow to to draw conclusions on which group of classes are more beneficial to different types of students.

4.5 Average effects of detracked classes on additional outcomes

Interesting additional outcomes when estimating the effect of detracked classes are the probability of starting a demanding VET and the probability of dropping out after compulsory school. However, Appendix Figure A.9 shows that the pre-treatment trends are not parallel for the probability of starting a demanding VET when comparing the shares in the canton of Neuchâtel with the shares in the control cantons prior to the reform. I still report the estimates from estimating equation 1 on this outcome variable in Appendix Table A.14, but I do not interpret the effects due to the non-parallel trends.[29] For the effect of detracked classes on the dropout probability, Appendix Figure A.10 suggests that the only interpretable estimate in terms of the parallel trends assumption is the point estimate for students with non-tertiary educated parents. However, for this outcome, I do not find an effect for any split samples reported in Appendix Table A.15.

5 Robustness

The causal interpretation of my difference-in-differences estimates relies on the parallel trends assumption. This assumption is not explicitly testable and can therefore be challenged, especially if not many pre-treatment observations are available. Therefore, I explore the sensitivity of my results in two ways, namely by altering the identification assumption using an instrumental variables approach, and by relaxing the parallel trends assumption by conditioning on pre-determined covariates using a doubly-robust estimator. To alter the identification strategy I use the reform as an instrument for detracked classes. The main identifying assumptions for an instrumental variable estimation are twofold. First, the instrument needs to be relevant. This can be tested empirically by the size of the F-statistic, which is above conventional threshold values in my setting. Second, the exclusion restriction needs to be fulfilled, which cannot be tested. Hence, the instrument, here the reform, should only affect the endogenous independent variable, here the detracked classes, and not influence

29. I also estimate the probability of starting any VET and do not find evidence for an effect of the detracked classes on this probability.

other aspects that might also be correlated with the outcome. The reform therefore should not have affected other features of the school system in the canton of Neuchâtel, that could potentially influence the probability of being assigned to further academic education. Institutional evidence supporting the exclusion restriction is provided by the policy document, which reports that teachers quantity and quality, curricula, and financial resources were not intended to change with the reform (Conseil d'État 2013).[30] I further provide empirical evidence supporting the exclusion restriction by checking that neither the assignment to tracks nor the cohort characteristics changed with the reform. The estimates of the instrumental variable regressions reported in Appendix Table A.16 are very similar in the full sample and within split samples to the estimates of my main specification in Table 3. Therefore, I conclude that my results are robust to altering the identification strategy.

To challenge my outcome model, I apply a version of the difference-in-differences estimator proposed by Callaway and Sant'Anna (2021). It relies on a conditional parallel trends assumption which relaxes the textbook parallel trends assumption on which my main estimates rely. The advantage of using a doubly-robust estimator is that it requires one to correctly specify either the outcome evolution of the comparison group or the propensity score model, but not necessarily both, to ensure appropriate convergence of the variance (for details see section C.8). The results displayed in Appendix Figure A.11 and Table A.17 show that the average treatment effects estimated with this alternative estimator are very similar to my main estimates for the full and the split samples. Additionally, the conditioning on covariates addresses the problems concerning the non-parallel trends in my main specification for the split samples of students on the basic track and of students who are not native speakers of the regional language.

I conclude that my main estimates are robust to altering the main identifying assumption and to applying a different outcome model. Both approaches challenge the identifying assumption of the difference-in-differences model used in my main specification. The similarity of the results in both alternative specifications is especially interesting for the split samples in which the evidence supporting the parallel trends assumption is not given. Hence, the effects of detracked classes for students assigned to the basic track and for students who are not native speakers of the regional language estimated both using the instrumental variable specification and the estimator proposed by Callaway and Sant'Anna (2021) can be interpreted causally. Further, both robustness tests address

30. Officials of the school administration of the canton of Neuchâtel confirmed that the teacher body and the curricula remained unchanged after the reform and no additional financial resources were available.

the concerns related to relying on a parallel trends assumption for which I can only provide evidence using three pre-treatment periods.

Another potential problem is the fact that the canton of Neuchâtel is french-speaking, while most control cantons are german-speaking. Therefore, the interpretation of my results might be problematic if french- and german-speaking cantons are not comparable in terms of non-obervables. In my control cantons, the canton of Geneva is fully french-speaking and the canton of Fribourg is bilingual. To test the sensitivity of my results to the language region, I analyse the detracking reform within the subset of municipalities which are french-speaking. From Appendix Figure A.12 displaying the parallel trends it becomes evident, that the level difference in the share of students assigned to further academic education is smaller, and that the canton of Neuchâtel compared to french-speaking regions has a lower level of students assigned to further academic education. The pre-trends tests are displayed in Appendix Table A.18 and show that as in the main esimation, the pre-trends only diverge for students on the basic track. The results for all students enrolled in french-speaking regions and for the split samples are displayed in Appendix Table **??** and are of very similar size as the main results. I therefore conclude that my results are not sensitive to the language region which further confirms their robustness regarding the institutional context in Switzerland.

6 Conclusion

I use a unique detracking reform to estimate the causal effect of detracked classes on the probability of assignment to further academic education which allows students to enroll in tertiary education. The reform changed the grouping of individuals assigned to different tracks into classes in one Swiss canton, while tracks are still assigned. In contrast to previous studies, the reform did not affect teacher quantity and quality, curricula, and school resources. Therefore, my difference-in-differences estimates do not commingle the effect of detracked classes with the effect of other changes in the school system. I use register panel data for the entire Swiss student population for ten years to show that the reform changed classroom composition dramatically in terms of peer characteristics. Since assignment to either the advanced or the basic track is correlated with socio-economic background of individuals, these effects work in opposite directions for students on each track. Advanced track students are more likely to have class peers with less advantaged socio-economic backgrounds after the reform, and vice versa for students on the basic track. The effect of the detracked classes is positive

for the average student, but the overall increase in the probability of further academic education is difficult to conclusively relate to better performance of students in detracked classes. Interestingly, the average effect exhibits considerable heterogeneity across students of different socio-economic backgrounds. I show that the increase in the probability of further academic education through the detracked classes is higher for students who are less socio-economically advantaged. The percentage increase for students with non-tertiary educated parents and who are not native speakers of the regional language suggests that the probability of pursuing further academic education nearly doubles for those socio-economically most disadvantaged students, while higher-achieving students and students from more advantaged socio-economic backgrounds do not lose.

Several mechanisms are possible. First, teachers might be more inclined to assign students who are socio-economically disadvantaged to further academic education in detracked classes. Second, these students have a higher aspiration to pursue an academic education if class diversity is higher. Third, and related to the first two points, disadvantaged students might be more motivated due to exposure to socio-economically more advantaged peers. The ultimate mechanisms behind the positive effect of detracked classes on the probability of assignment to further academic education remains an avenue for future research.

Clearly, pursuing further academic education is not necessarily the best option for every student in terms of future earnings. This is especially the case in an education system like the one in Switzerland, with a high popularity and a high quality of VET. Nevertheless, my findings are consequential for policy makers who aspire education systems where education inequality is low and intergenerational patterns of tertiary education are minimal. My results provide evidence that education inequality can be reduced by detracked classes. Further, since tertiary education is consequential for labor market participation and earnings, detracked classes could even have the potential to decrease earnings inequality.

Acknowledgments and disclosure

I thank David Card, Beatrix Eugster, Demid Getik, Bryan Graham, Patrick Kline, Fanny Puljic, Lukas Schmid, Petra Thiemann, and Christopher Walters, Ludger Wössmann, as well as participants of the Conference of the European Association of Labour Economists 2024, of the 2nd Workshop on Education Economics and Policy. of the 14th International Workshop on Applied Economics of Education, of the Annual Congress 2024 of the Swiss Society of Economics and Statistics, and seminar participants of the University of California Berkeley, Lucerne, Lund, and of the Swiss Leading House Friday Lunch Talk seminar for their valuable comments and suggestions. I thank the Federal Statistical Office for providing the data for this research project and the school administration of the canton of Neuchâtel for their insights on the implementation of the reform. This project did not receive any funding.

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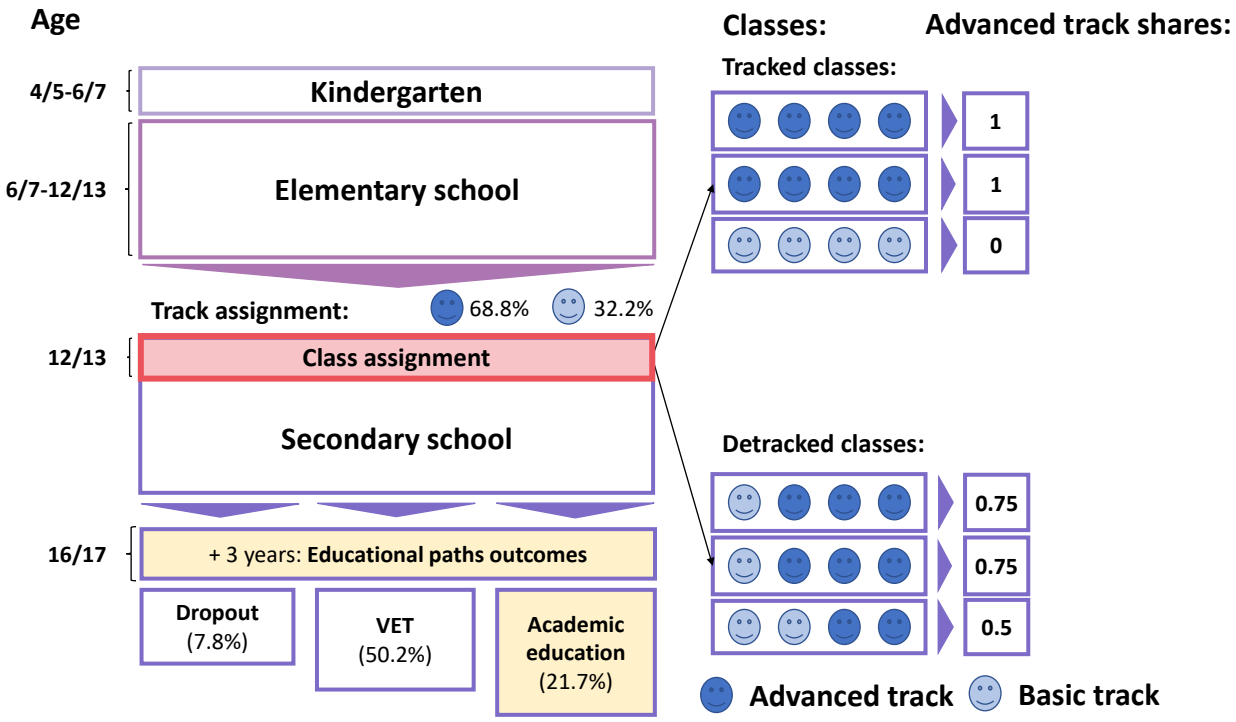
Appendix

A Additional figures

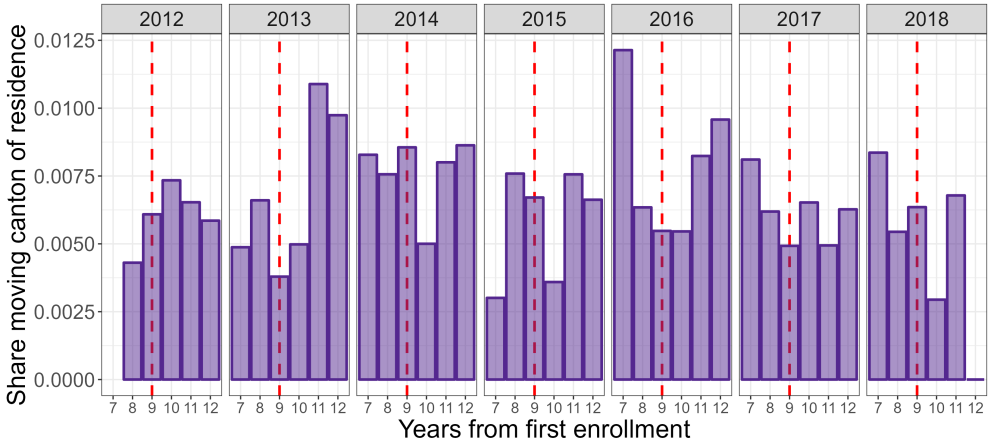
Figure A.1: Swiss education system



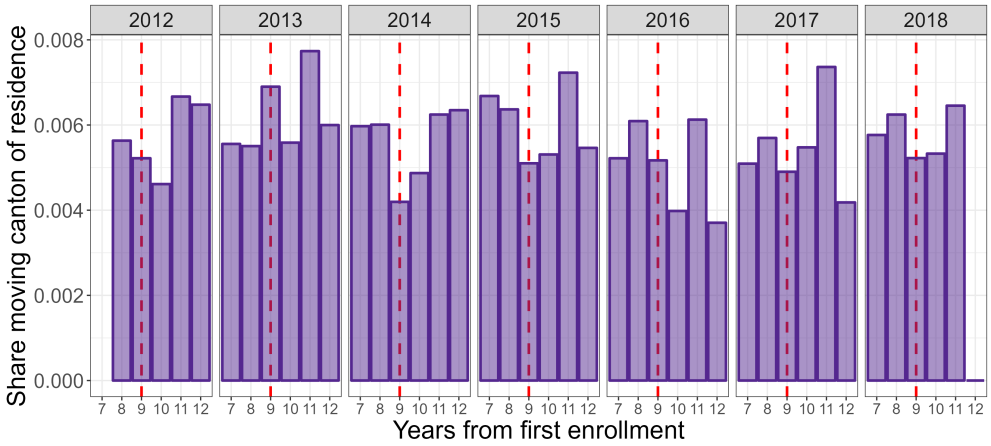
Notes: This figure shows a simplified version of the Swiss education system. It shows different possibilities to group students of different tracks into classes in lower secondary school, and the respective class shares of advanced track students. Additionally it depicts a limited number of options students can follow after compulsory education for upper secondary school. For illustration purposes, I do not include transitional years, apprenticeships, and other schools at the bottom left which together sum up to 20.3%. *Source:* Own illustration.

Figure A.2: Moving residence across canton

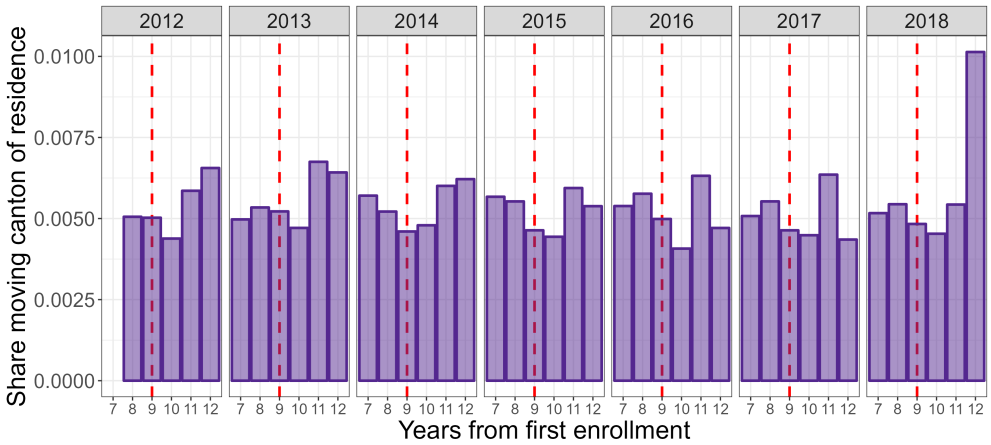
(A) Share of students moving residence across canton in the next grade for Neuchâtel



(B) Share of students moving residence across canton in the next grade for control cantons



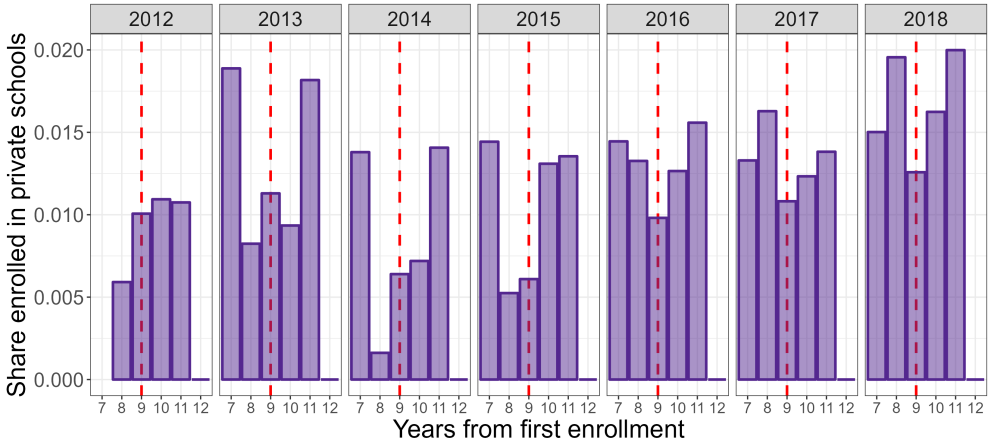
(C) Share of students moving residence across canton in the next grade for Switzerland



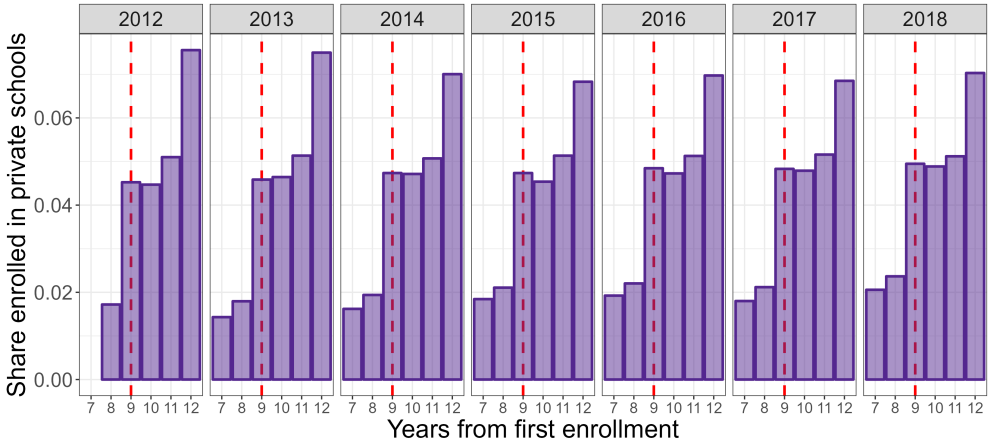
Notes: This figure shows the share of students moving residence across cantons in the next year for the reform canton in panel (A), the selected control cantons in panel (B), and for the entire population of pupils in Switzerland in panel (C). Shares are calculated per years and grades from first enrollment. The red dashed line marks the time of treatment assignment, the first year of lower secondary school. Hence, students who potentially distort their treatment by movement across cantons in the next grade would be registered as moving in the first year of lower secondary school in this figure.

Figure A.3: Private school enrollment

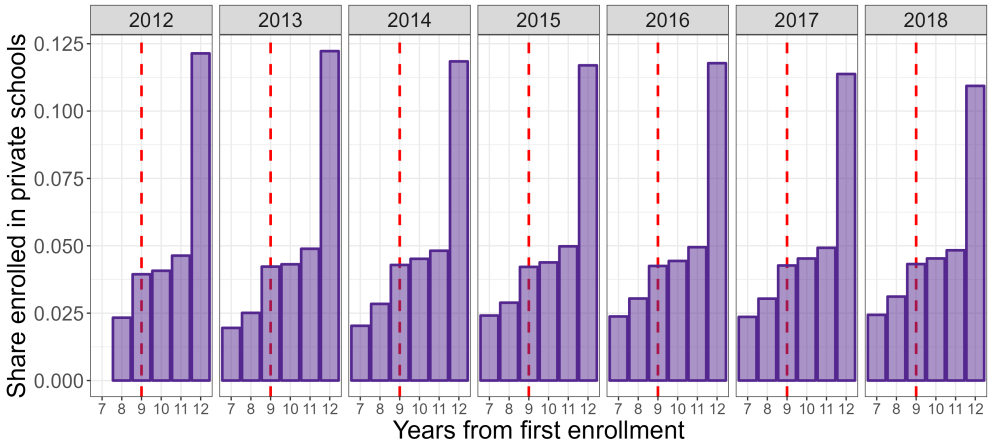
(A) Share of students enrolled in a private school in the canton of Neuchâtel



(B) Share of students enrolled in a private school in the control cantons



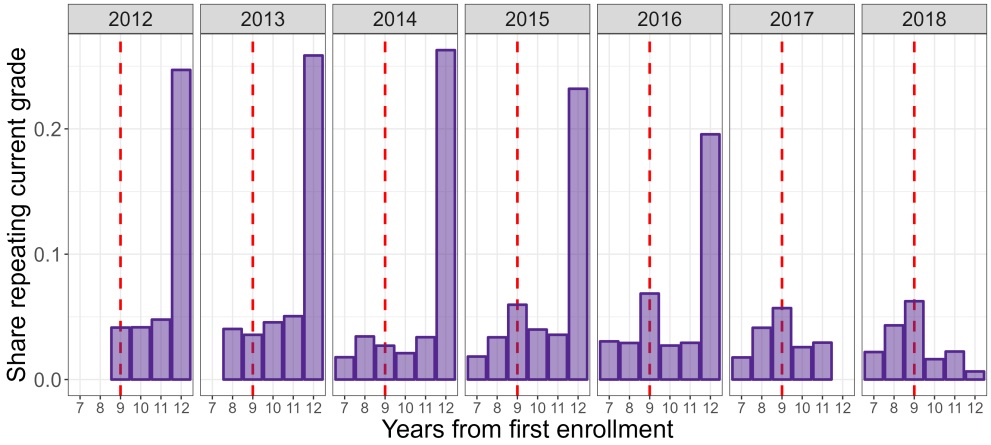
(C) Share of students enrolled in a private school in Switzerland



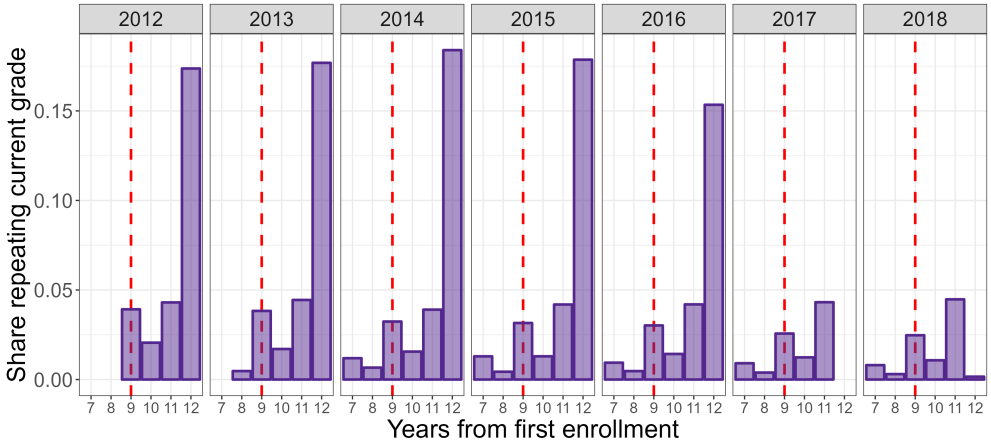
Notes: This figure shows the share of students enrolled in private schools for the reform canton in panel (A), the control cantons in panel (B), and for the entire population of pupils in Switzerland in panel (C). Shares are calculated per years and grades from first enrollment. The red dashed line marks the time of treatment assignment, the first year of lower secondary school. Hence, students who potentially distort their treatment by enrolling in private schools would do this before the first year of lower secondary school in this figure.

Figure A.4: Repeating current grade

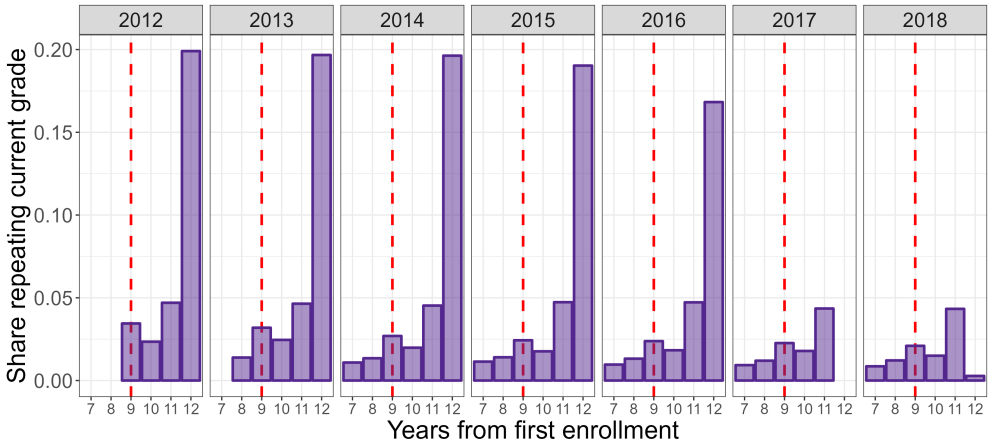
(A) Share of students repeating the current grade in the canton of Neuchâtel



(B) Share of students repeating the current grade in the control cantons



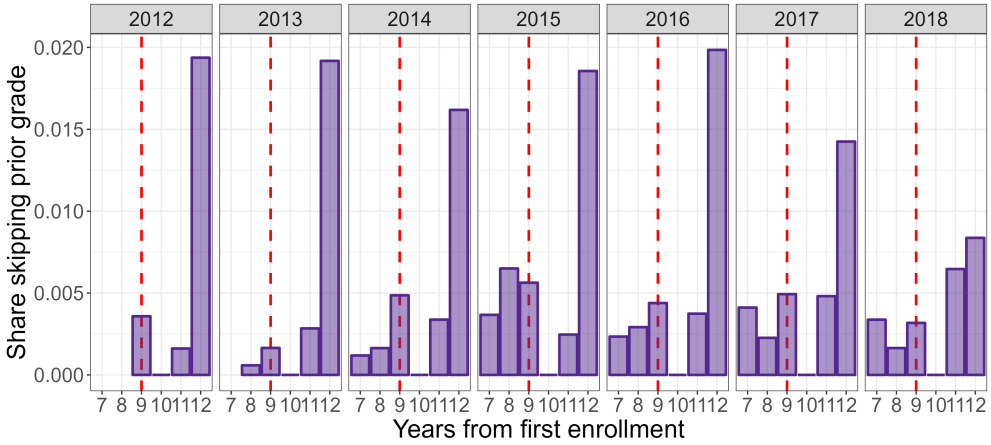
(C) Share of students repeating the current grade in Switzerland



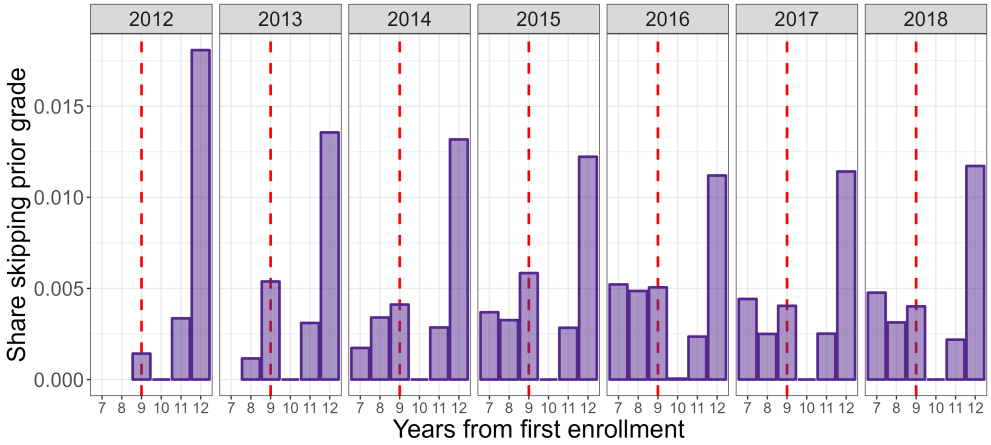
Notes: This figure shows the share of students repeating the current grade for the reform canton in panel (A), the control cantons in panel (B), and for the entire population of pupils in Switzerland in panel (C). Shares are calculated per years and grades from first enrollment. The red dashed line marks the time of treatment assignment, the first year of lower secondary school. Hence, students who potentially distort their treatment by enrolling in private schools would be registered as repeaters in the first year of lower secondary school in this figure.

Figure A.5: Skipping prior grade

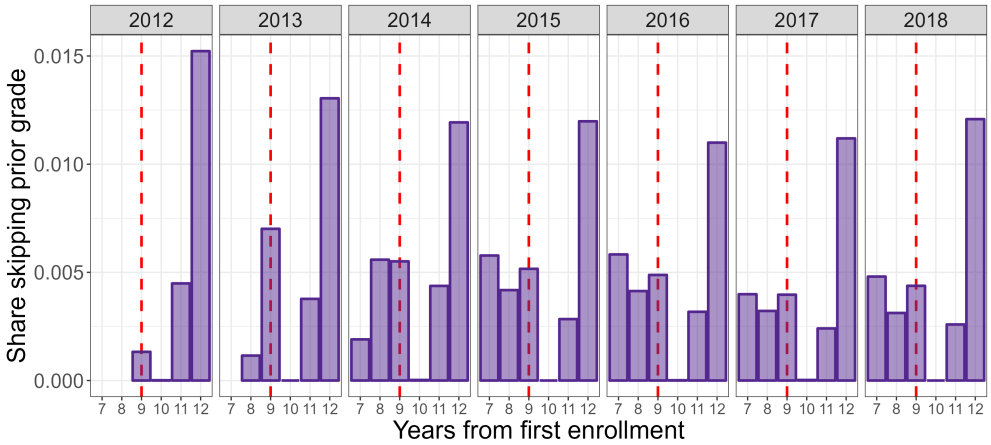
(A) Share of students skipping prior grade in the canton of Neuchâtel



(B) Share of students skipping prior grade in the control cantons

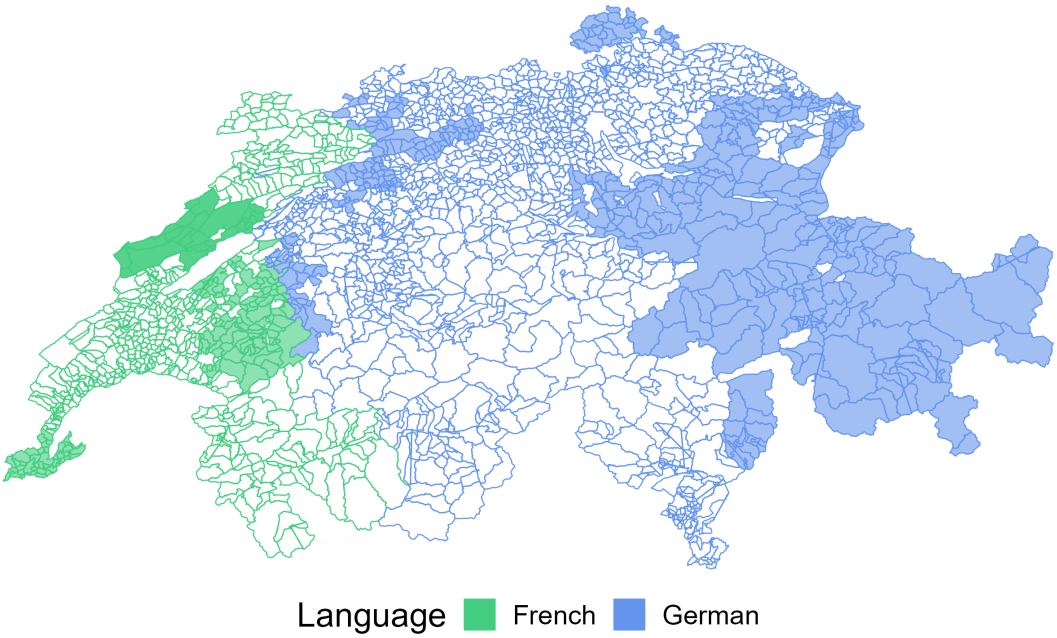


(C) Share of students skipping prior grade for Switzerland



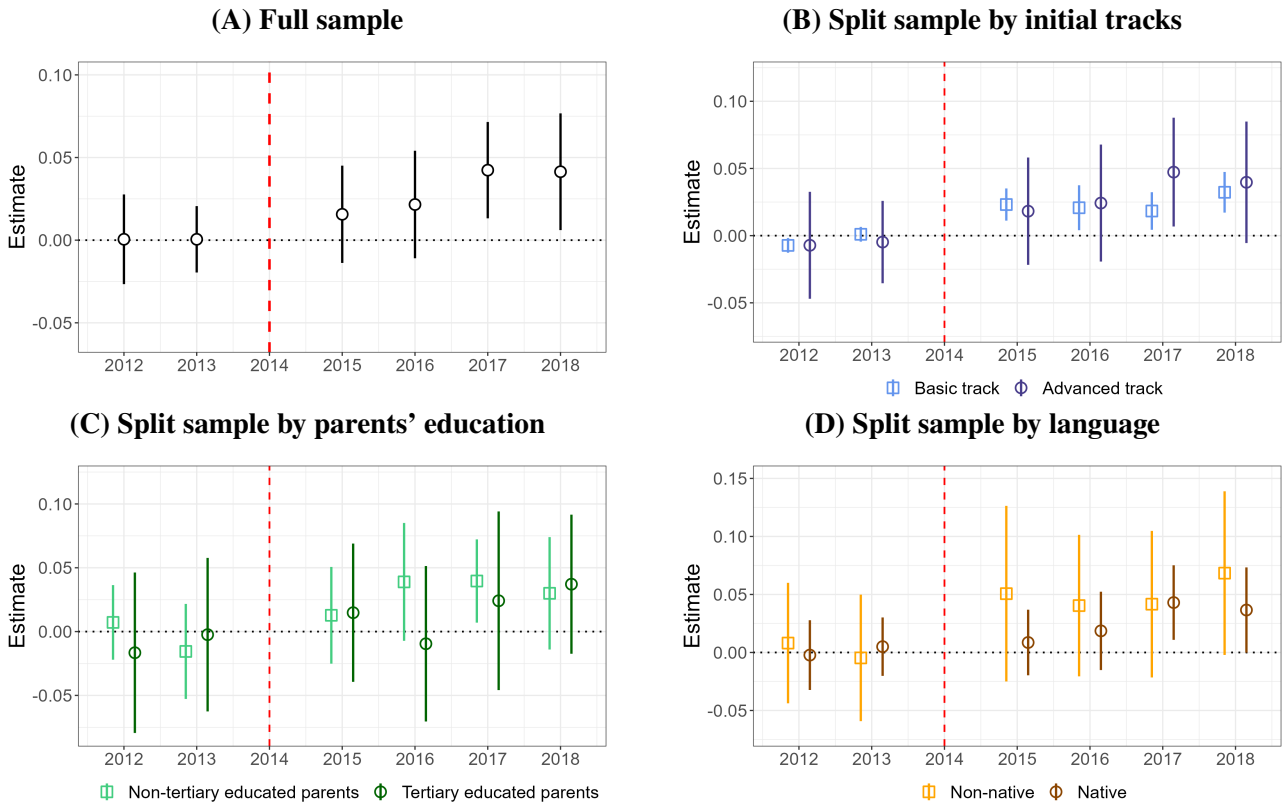
Notes: This figure shows the share of students skipping the prior grade for the reform canton in panel (A), the control cantons in panel (B), and for the entire population of pupils in Switzerland in panel (C). Shares are calculated per years and grades from first enrollment. The red dashed line marks the time of treatment assignment, the first year of lower secondary school. Hence, students who potentially distort their treatment by skipping the prior grade would be registered as skipping the prior grade in the first year of lower secondary school in this figure.

Figure A.6: Sample selection



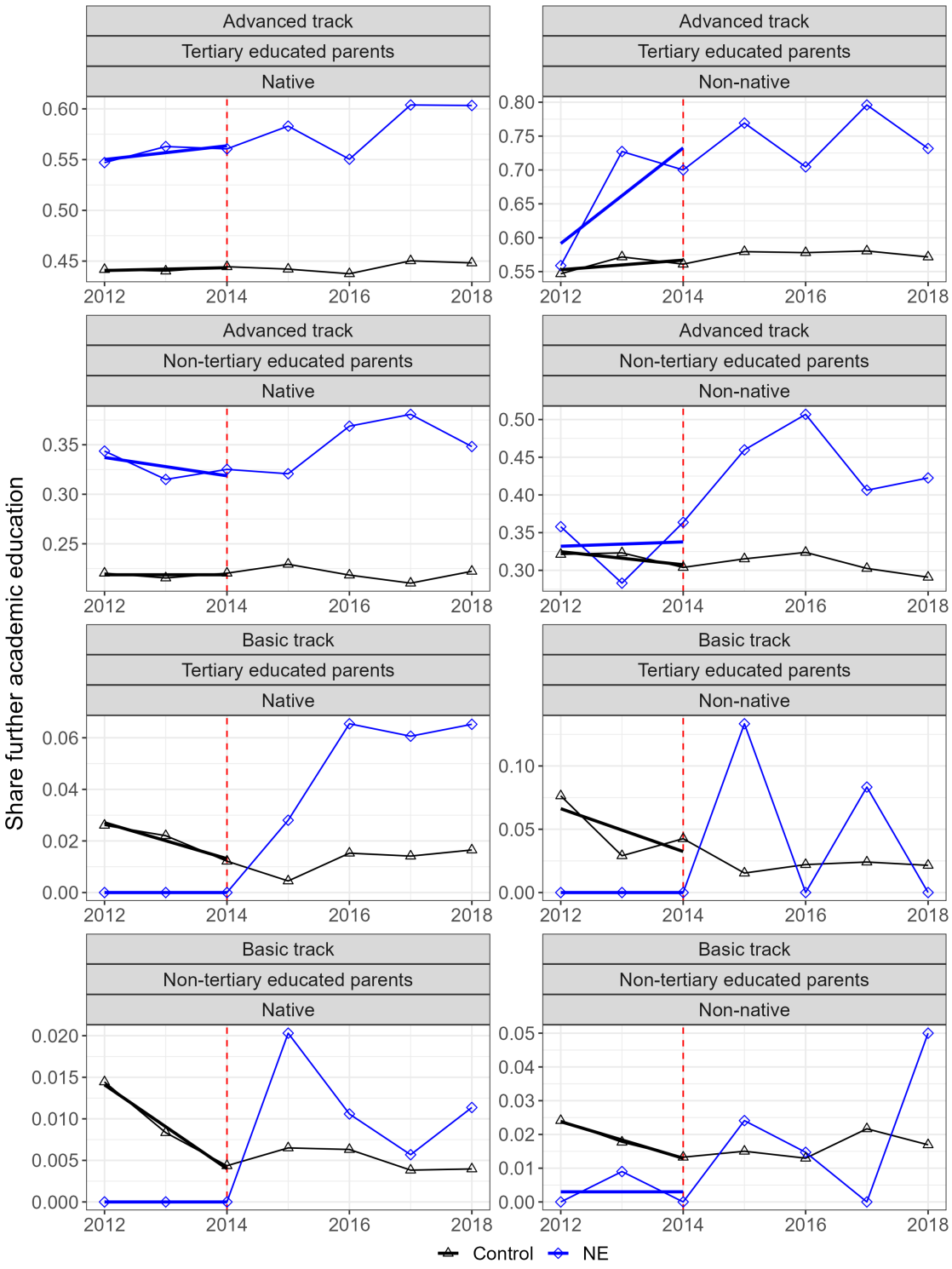
Notes: This map shows the sample selection, including the language regions in Switzerland. French-speaking regions are colored in green, and German-speaking regions are colored in blue. I neglect italian-speaking regions and regions where the first language is retoromanic to facilitate readability. Light-shaded are the control cantons and darker colored in green is the canton of Neuchâtel. *Source:* BFS, and own illustration.

Figure A.7: Dynamics of the average treatment effects



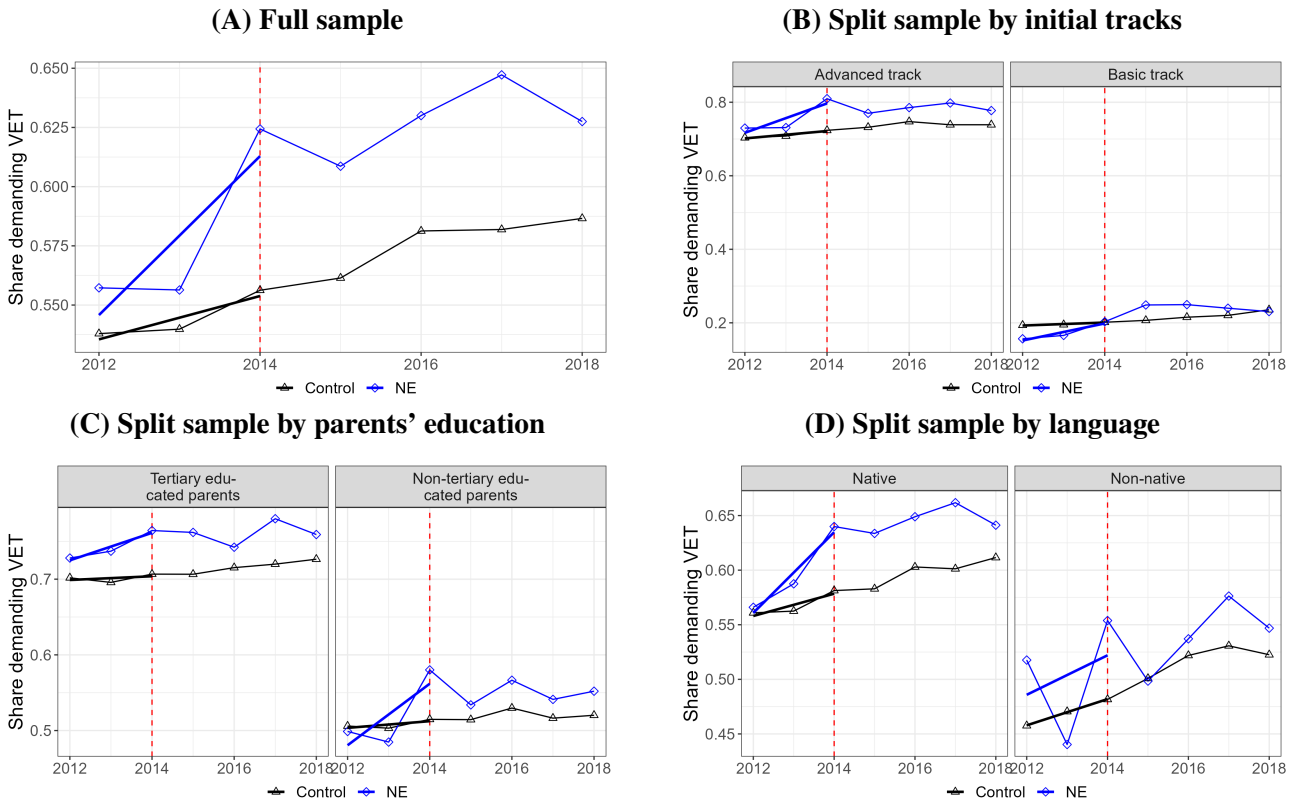
Notes: This figure shows the event-study type estimates from estimating equation 1 including 95% confidence intervals. 2014 serves as a reference year and is marked with a red dashed line. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. All specifications include time-invariant individual-level covariates, fixed effects for school and year, and cluster standard errors at the school-level. Panel (A) depicts the estimates for the full sample, panel (B) shows the same estimates in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language.

Figure A.8: Parallel trends for the probability of further academic education in three-way split samples



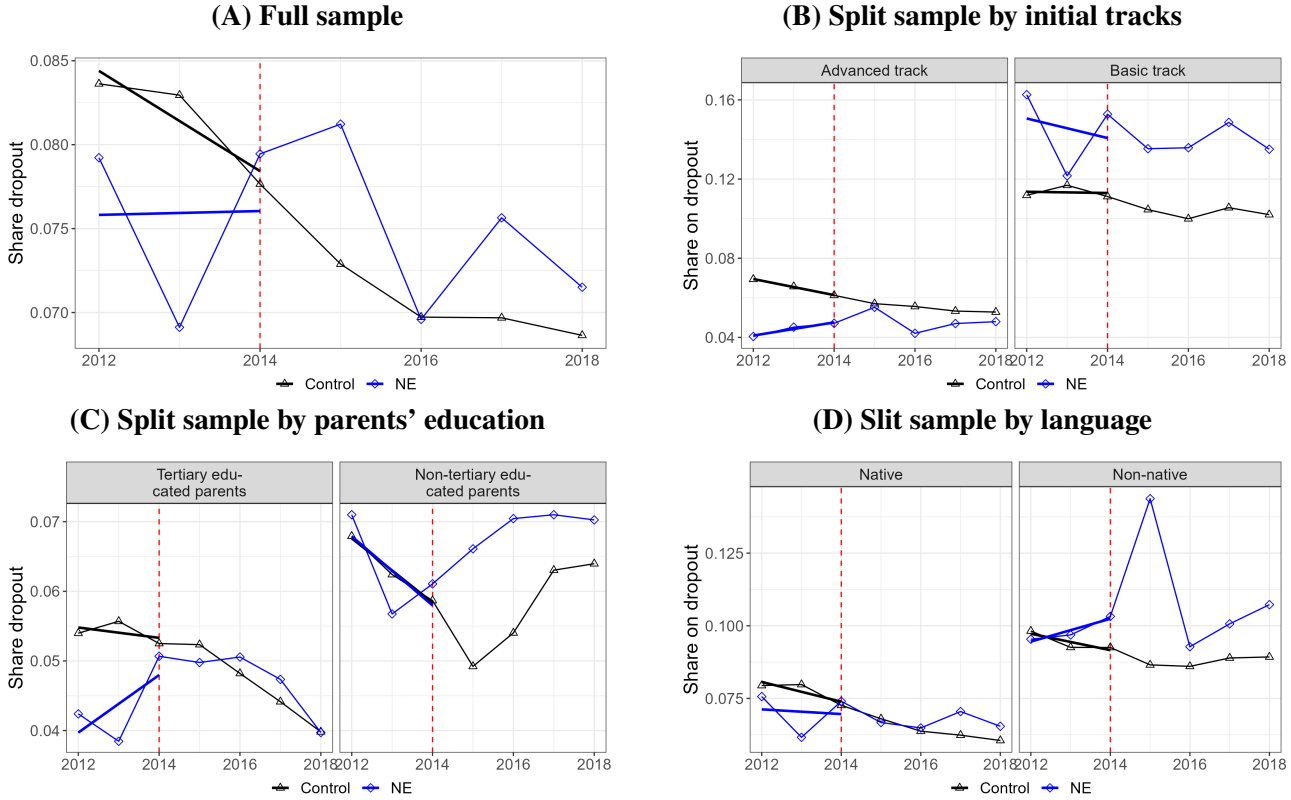
Notes: This figure depicts the share of individuals who are assigned to further academic education after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level within split samples. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line.

Figure A.9: Parallel trends for the probability of starting a demanding VET



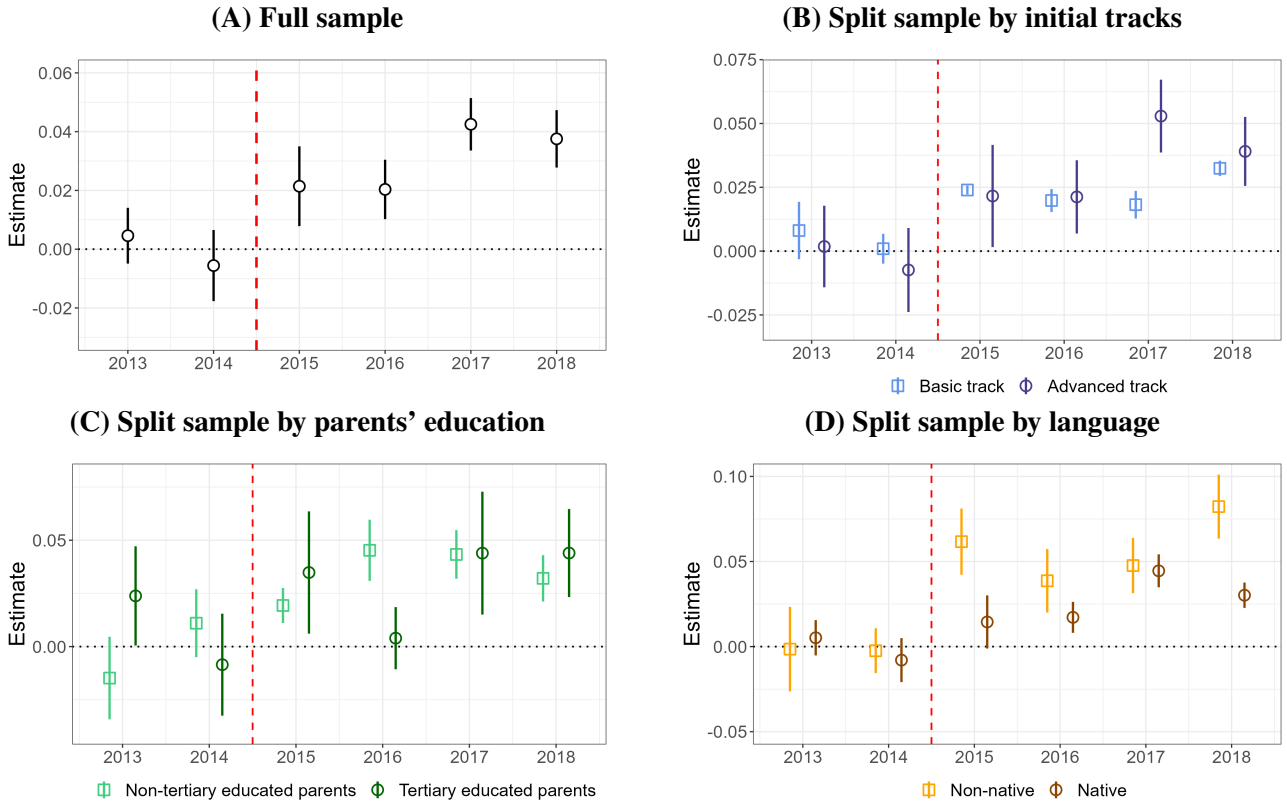
Notes: This figure depicts the share of individuals who start a demanding VET after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) depicts the shares for the full sample, panel (B) shows the same shares in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language.

Figure A.10: Parallel trends for the probability of dropout



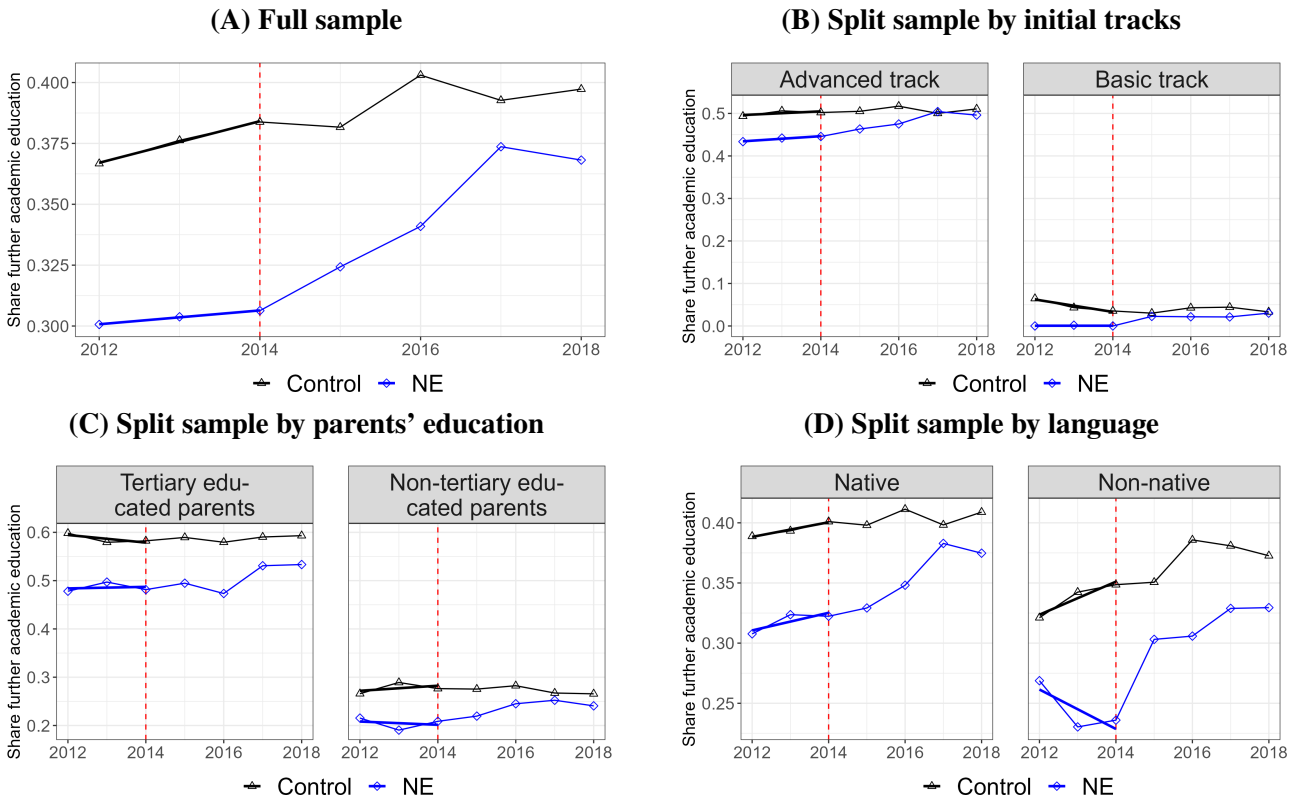
Notes: This figure depicts the share of individuals who drop out after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) depicts the shares for the full sample, panel (B) shows the same shares in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language.

Figure A.11: Doubly-robust estimator



Notes: This figure shows estimation results applying the doubly-robust estimator proposed by Callaway and Sant'Anna (2021), using 2015 as first treatment year. The dashed red line divides the plot into a pre- and a post-treatment period. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. All specifications include time-invariant individual-level covariates, fixed effects for canton and year, and cluster the standard errors at the canton level. Panel (A) depicts the estimates for the full sample, panel (B) shows the same estimates in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language. I cluster the standard errors at the canton level.

Figure A.12: Parallel trends for the probability of further academic education for french-speaking regions



Notes: This figure depicts the share of individuals who are assigned to further academic education after compulsory school. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I only use the subset of students who are enrolled in schools located in french-speaking municipalities. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) depicts the shares for the full sample, panel (B) shows the same shares in split samples along initially assigned tracks, panel (C) along a dummy variable indicating whether students have tertiary educated parents, and panel (D) along an indicator for individuals being native speakers of the regional language.

B Additional tables

Table A.1: Pre-trend testing for the probability of further academic education

Dependent variable:	Further academic education						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Natives**	**Non-natives**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	-0.0003	0.0038	0.0039	0.0074	-0.0034	0.0014	-0.0073
	(0.0069)	(0.0100)	(0.0013)	(0.0158)	(0.0074)	(0.0076)	(0.0133)
Observations	67,799	45,865	21,934	19,567	23,424	51,650	16,149
R^2	0.3197	0.2769	0.0735	0.3505	0.2724	0.3182	0.3567
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports regression coefficients of an interaction term between the linear time trend and the treatment indicator including school and time fixed effects. I use data for cohorts first enrolled in lower secondary school in the years 2012-2014 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy on being native in a regional language. I include time-invariant individual-level controls in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.2: Effects of the detracking reform on cohort composition

Panel (A):	**Advanced track students**					
Dependent variable:	**Advanced track share**	**Share tertiary educated parents**	**Share Swiss nationality**	**Share native speakers**	**Share females**	**Number of students**
	(1)	(2)	(3)	(4)	(5)	(6)
NE × Post	-0.0084	0.0074	0.0087	0.0477	-0.0111	0.3805
	(0.0129)	(0.0108)	(0.0055)	(0.0097)	(0.0120)	(5.5341)
Pre-reform mean	0.683	0.453	0.860	0.805	0.496	131.000
Observations	106,391	106,357	106,392	106,392	106,392	106,400
R^2	0.8337	0.7839	0.7900	0.8800	0.6232	0.9627

Panel (B):	**Basic track students**					
Dependent variable:	**Advanced track share**	**Share tertiary educated parents**	**Share Swiss nationality**	**Share native speakers**	**Share females**	**Number of students**
	(1)	(2)	(3)	(4)	(5)	(6)
NE × Post	-0.0132	0.0113	0.0090	0.0504	-0.0053	-1.2073
	(0.0135)	(0.0117)	(0.0059)	(0.0097)	(0.0118)	(5.5484)
Pre-reform mean	0.673	0.437	0.861	0.805	0.494	129.000
Observations	49,282	49,273	49,282	49,282	49,282	49,286
R^2	0.8732	0.7051	0.8336	0.8971	0.3500	0.9673
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Gender, Native, Swiss	✓	✓	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 using cohort shares of peer characteristics as dependent variables. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level, and split the sample by advanced and basic track students. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.3: Characteristics of students on the advanced track

Dependent Variable:	Advanced track			
Characteristic:	**Tertiary educated parents**	**Native speaker**	**Swiss**	**Female**
	(1)	(2)	(3)	(4)
NE × Post × Characteristic	-0.0417	-0.0281	-0.0516	-0.0077
	(0.0107)	(0.0226)	(0.0307)	(0.0158)
Observations	95,652	155,691	155,691	155,691
R^2	0.1689	0.1550	0.1631	0.1303
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

Notes: This table reports estimates from estimating the regression $Y_{it} = \alpha_s + \alpha_t + \mathbf{X}_i'\gamma + \mathbf{X}_i' \times \text{NE}_i\nu + \mathbf{X}_i' \times \text{Post}_t\eta + \mathbf{X}_i' \times \text{NE}_i \times \text{Post}_t\tau + \epsilon_{i,t}$. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I use an indicator for tertiary educated parents as $\mathbf{X}_i'$, in column (2) I insert a dummy variable for being a native speaker of the regional language, in column (3) an indicator for being a Swiss citizen, and in column (4) a dummy for being female. I include school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.4: Check randomness of class assignment in post-reform period in Neuchâtel

Dependent variable:	**Advanced track share**	**Share tertiary educated parents**	**Share Swiss nationality**	**Share native speakers**	**Share females**	**Number of students**
	(1)	(2)	(3)	(4)	(5)	(6)
Advanced track	0.0029	0.0040	0.0019	−0.0019	−0.0005	0.1283
	(0.0087)	(0.0048)	(0.0026)	(0.0025)	(0.0025)	(0.0605)
Native speaker	−0.0045	0.0085	−0.0042	0.0054	−0.0015	-0.0549
	(0.0042)	(0.0058)	(0.0039)	(0.0051)	(0.0033)	(0.0626)
Swiss	0.0056	0.0049	0.0106	−0.0001	−0.0006	-0.0128
	(0.0042)	(0.0055)	(0.0057)	(0.0035)	(0.0032)	(0.0715)
Female	−0.0025	0.0013	−0.0006	0.0011	−0.0314	0.0035
	(0.0021)	(0.0023)	(0.0016)	(0.0015)	(0.0020)	(0.0247)
Observations	6,882	6,882	6,882	6,882	6,882	6,882
R^2	0.1297	0.3321	0.2726	0.6267	0.1256	0.2474
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from regressing leave-own-out shares of class characteristics in the detracked classes on individual characteristics including school and time fixed effects. I use data for cohorts first enrolled in lower secondary school in the canton of Neuchâtel in the years 2015-2018 at the yearly level, and split the sample by advanced and basic track students. Standard errors clustered at the school level are displayed in parantheses.

Table A.5: Average treatment effects of class shares of advanced track students in Neuchâtel in the full sample

Dependent variable:	**Further academic education**			
	(1)	(2)	(3)	(4)
Class share of advanced track students	0.1325	0.0555	0.0444	0.0339
	(0.0752)	(0.0552)	(0.0462)	(0.0461)
Observations	6,882	6,882	6,882	6,882
R^2	0.0011	0.2103	0.2322	0.2329
School FE	No	No	Yes	Yes
Time FE	No	No	No	Yes
Controls: Female, Native, Swiss, Advanced track		✓	✓	✓

Notes: This table reports estimates from estimating equation (1) and replacing the treatment indicator $\text{NE}_i \times \text{Post}_t$ by the class share of advanced track students. I use data for cohorts first enrolled in lower secondary school in the canton of Neuchâtel in the years 2015-2018 at the yearly level. In column (1) I do not include controls, in column (2) I add time-invariant individual-level controls, in column (3) and (4) I subsequently add school and time fixed effects. Standard errors clustered at the class level are displayed in parantheses.

Table A.6: Average treatment effects of quartiles of class shares of advanced track students in the full sample

Dependent variable:	**Further academic education**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	−0.1460***	0.0288	0.0282**	0.0385**
	(0.0128)	(0.0256)	(0.0114)	(0.0176)
Pre-reform mean	0.304	0.304	0.304	0.304
Effect in percentages	−48.03%	9.47%	9.28%	12.66%
Observations	148,846	149,095	153,658	150,519
Treated bservations	76	286	4,849	1,710
R^2	0.3234	0.3231	0.3193	0.3225
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Native, Swiss, Advanced track	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation (1) in the full sample. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1), I only include post-reform observations in the reform canton that report class shares of advanced track students between 0 and 0.25, in column (2) between 0.25 and 0.5, in column (3) between 0.5 and 0.75 and in column (4) between 0.75 and 1. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. Standard errors clustered at the school level are displayed in parentheses.

Table A.7: Average treatment effects of class shares of advanced track students in Neuchâtel in split samples

Dependent variable:	**Further academic education**					
Sample:	**Advanced track**	**Basic track**	**High parental education**	**Low parental education**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)
Class share of advanced track students	0.0057	-0.0033	0.1171	-0.0439	0.0460	0.0001
	(0.0752)	(0.0240)	(0.0772)	(0.0694)	(0.0502)	(0.1103)
Observations	4,875	2,007	2,734	2,685	5,717	1,165
R^2	0.0668	0.0208	0.2161	0.1990	0.2168	0.3382
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Female, Swiss	✓	✓	✓	✓	✓	✓
Advanced track			✓	✓	✓	✓
Native	✓	✓	✓	✓		

Notes: This table reports estimates from estimating equation 1 and interchanging the treatment indicator $\text{NE}_i \times \text{Post}_t$ by the class share of advanced track students. I use data for cohorts first enrolled in lower secondary school in the canton of Neuchâtel in the years 2015-2018 at the yearly level. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. In columns (1) and (2) I split the sample according to assigned track, in columns (3) and (4) I provide split sample estimates by an indicator for having tertiary educated parents, and in columns (5) and (6) by a dummy on being a native speaker of the regional language. Standard errors clustered at the class level are displayed in parantheses.

Table A.8: Average treatment effects of quartiles of class shares of advanced track students in split samples by tracks

Panel (A):	**Advanced track students**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	—	0.0363***	—	0.0489**
	—	(0.0129)	—	(0.0211)
Observations	—	106,404	—	102,763
R^2	—	0.2669	—	0.2762
Panel (B):	**Basic track students**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	0.0280***	0.0269	0.0296***	0.0187**
	(0.0015)	(0.0228)	(0.0061)	(0.0076)
Observations	47,317	47,366	48,688	47,756
R^2	0.0531	0.0530	0.0507	0.0591
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Native, Swiss	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 within split samples by initially assigned tracks. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. In column (1), I only include post-reform observations in the reform canton that report class shares of advanced track students between 0 and 0.25, in column (2) between 0.25 and 0.5, in column (3) between 0.5 and 0.75 and in column (4) between 0.75 and 1. Panel (A) reports estimates for individuals initially assigned to the advanced track individuals, and panel (B) shows coefficients for individuals initially assigned to the basic track. Standard errors clustered at the school level are displayed in parentheses.

Table A.9: Average treatment effects of quartiles of class shares of advanced track students in split samples by parents' education

Panel (A):	**Tertiary educated parents**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	−0.2179***	0.0481	0.0182	0.0403
	(0.0191)	(0.0279)	(0.0164)	(0.0289)
Observations	42,702	42,800	44,597	43,411
R^2	0.3495	0.3490	0.3429	0.3533
Panel (B):	**Non-tertiary educated parents**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	−0.0679***	0.0044	0.0356**	0.0256
	(0.0108)	(0.0440)	(0.0132)	(0.0179)
Observations	47,556	47,650	49,477	48,158
R^2	0.2615	0.2611	0.2573	0.2604
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Advanced track, Female, Native, Swiss	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 within split samples by parents' education. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. In column (1), I only include post-reform observations in the reform canton that report class shares of advanced track students between 0 and 0.25, in column (2) between 0.25 and 0.5, in column (3) between 0.5 and 0.75 and in column (4) between 0.75 and 1. Panel (A) reports estimates for individuals with tertiary educated parents, and panel (B) shows coefficients for individuals with non-tertiary educated parents. Standard errors clustered at the school level are displayed in parentheses.

Table A.10: Average treatment effects of quartiles of class shares of advanced track students in split samples by language

Panel (A):	**Natives**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	−0.1827***	0.0195	0.0252**	0.0326
	(0.0118)	(0.0248)	(0.0114)	(0.0214)
Observations	110,518	110,740	114,515	111,892
R^2	0.3204	0.3200	0.3153	0.3214
Panel (B):	**Non-natives**			
Quartile of class share:	**[0,0.25]**	**(0.25,0.5]**	**(0.5,0.75]**	**(0.75,1]**
	(1)	(2)	(3)	(4)
NE × Post	0.0287*	0.0855	0.0431*	0.0625**
	(0.0159)	(0.1000)	(0.0220)	(0.0229)
Observations	38,328	38,355	39,143	38,627
R^2	0.3466	0.3465	0.3452	0.3529
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Advanced track, Female, Swiss	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 within split samples by language. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. In column (1), I only include post-reform observations in the reform canton that report class shares of advanced track students between 0 and 0.25, in column (2) between 0.25 and 0.5, in column (3) between 0.5 and 0.75 and in column (4) between 0.75 and 1. Panel (A) reports estimates for individuals who are native speakers of the regional language, and panel (B) shows coefficients for individuals who are not native speakers of the regional language. Standard errors clustered at the school level are displayed in parentheses.

Table A.11: Average treatment effects in three-way sample splits

Dependent variable:	Further academic education			
Sample:	**Advanced track Tertiary educated parents Native**	**Advanced track Tertiary educated parents Non-native**	**Advanced track Non-tertiary educated parents Native**	**Advanced track Non-tertiary educated parents Non-native**
	(1)	(2)	(3)	(4)
NE × Post	0.0244	0.1075	0.0305	0.1232
	(0.0197)	(0.0411)	(0.0191)	(0.0302)
Pre-reform mean	0.557	0.653	0.328	0.332
Effect in percentages	4.38%	16.47%	9.30%	37.10%
Observations	33,946	4,910	22,955	7,571
R^2	0.2992	0.3340	0.2325	0.2674
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Swiss	✓	✓	✓	✓

Dependent variable:	Further academic education			
Sample:	**Basic track Tertiary educated parents Native**	**Basic track Tertiary educated parents Non-native**	**Basic track Non-tertiary educated parents Native**	**Basic track Non-tertiary educated parents Non-native**
	(1)	(2)	(3)	(4)
NE × Post	0.0555	0.0741	0.0167	0.0176
	(0.0134)	(0.0253)	(0.0056)	(0.0100)
Pre-reform mean	0	0	0	0.0034
Effect in percentages	–	–	–	520.59%
Observations	5,251	1,321	11,892	7,806
R^2	0.1219	0.1470	0.0595	0.0575
School FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls:				
Female, Swiss	✓	✓	✓	✓

Notes: This table reports estimates from estimating equation 1 within three-way split samples. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I include time-invariant individual-level controls and school and time fixed effects in all specifications. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. Standard errors clustered at the school level are displayed in parantheses.

Table A.12: Pre-trends testing for the probability of starting a demanding VET

Dependent variable:	Demanding VET						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Natives**	**Non-natives**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0260	0.0309	0.0159	0.0207	0.0310	0.0262	0.0234
	(0.0050)	(0.0067)	(0.0145)	(0.0107)	(0.0077)	(0.0062)	(0.0192)
Observations	67,312	45,771	21,541	19,530	23,254	51,370	15,942
R^2	0.3053	0.0875	0.0954	0.2572	0.3048	0.2863	0.3647
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced Track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports regression coefficients of an interaction term between the linear time trend and the treatment indicator including school and time fixed effects. I use data for cohorts first enrolled in lower secondary school in the years 2012-2014 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. I include time-invariant individual-level controls in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.13: Pre-trends testing for the probability of dropout

Dependent variable:	Dropout						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Natives**	**Non-natives**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0032	0.0071	-0.0053	0.0043	-0.0003	0.0019	0.0084
	(0.0034)	(0.0043)	(0.0085)	(0.0053)	(0.0062)	(0.0034)	(0.0110)
Observations	67,799	45,865	21,934	19,567	23,424	51,650	16,149
R^2	0.0497	0.0396	0.0639	0.0492	0.0541	0.0494	0.0772
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports regression coefficients of an interaction term between the linear time trend and the treatment indicator including school and time fixed effects. I use data for cohorts first enrolled in lower secondary school in the years 2012-2014 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. I include time-invariant individual-level controls in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.14: Average treatment effect for the probability of starting a demanding VET

Dependent variable:	Demanding VET						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0116	0.0016	0.0367	0.0143	0.0133	0.0191	-0.0135
	(0.0106)	(0.0109)	(0.0201)	(0.0150)	(0.0142)	(0.0115)	(0.0188)
Pre-reform mean	0.579	0.757	0.175	0.744	0.520	0.598	0.498
Effect in percentages	1.81%	2.56%	0.21%	1.93%	20.97%	3.19%	-2.72%
Observations	153,971	105,922	48,049	45,230	49,580	115,187	38,784
R^2	0.3014	0.0927	0.0803	0.2528	0.2956	0.2798	0.3576
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports estimates from estimating equation 1. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by for being a native speaker of the regional language. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.15: Average treatment effect for the probability of dropout

Dependent variable:	Dropout						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0138	0.0150	0.0151	0.0156	0.0130	0.0122	0.0216
	(0.0048)	(0.0054)	(0.0131)	(0.0081)	(0.0082)	(0.0070)	(0.0136)
Pre-reform mean	0.0757	0.0442	0.146	0.0434	0.0633	0.0702	0.0990
Effect in percentages	18.30%	33.91%	10.32%	36.05%	20.60%		
Observations	155,691	106,404	49,287	45,428	50,224	116,204	39,487
R^2	0.0466	0.0341	0.0614	0.0374	0.0449	0.0438	0.0653
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports estimates from estimating equation 1. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.16: Instrumental variables estimates

Dependent variable:	Further academic education						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0318	0.0390	0.0278	0.0248	0.0344	0.0278	0.0504
	(0.0112)	(0.0145)	(0.0063)	(0.0182)	(0.0119)	(0.0129)	(0.0181)
Observations	155,691	106,404	49,287	45,428	50,224	116,204	39,487
R^2	0.3199	0.2694	0.0559	0.3464	0.2615	0.3161	0.3512
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports estimates from estimating an instrumental variable estimation using the reform as an instrument. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. The F-statistic of the first stage in the full sample is 355,324.7. Standard errors clustered at the school level are displayed in parantheses.

Table A.17: Doubly-robust estimates

Dependent variable:	Further academic education						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0305	0.0337	0.0236	0.0317	0.0350	0.0266	0.0576
	(0.0043)	(0.0063)	(0.0017)	(0.0117)	(0.0040)	(0.0042)	(0.0083)
Pre-reform mean	0.304	0.363	0.0006	0.487	0.206	0.318	0.243
Effect in percentages	10.03%	9.28%	4013.61%	6.51%	16.99%	8.36%	23.70%
Observations	155,691	106,391	49,282	45,428	50,224	116,204	39,487
Conditional parallel trends:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table shows estimation results applying the doubly-robust estimator proposed by Callaway and Sant'Anna (2021). I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. I include time-invariant individual-level controls and canton and time fixed effects in all specifications. Standard errors clustered at the canton level are displayed in parantheses.

Table A.18: Pre-trends testing for the probability of further academic education for french-speaking regions

Dependent variable:	Further academic education						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Natives**	**Non-natives**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0030	0.0011	0.0134	0.0204	−0.0051	0.0033	−0.0017
	(0.0079)	(0.0111)	(0.0037)	(0.0158)	(0.0102)	(0.0092)	(0.0146)
Observations	26,509	19,000	7,509	8,188	9,247	18,511	7,998
R^2	0.2973	0.1806	0.0478	0.2929	0.2418	0.2822	0.3382
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced Track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports regression coefficients of an interaction term between the linear time trend and the treatment indicator including school and time fixed effects. I use data for cohorts first enrolled in lower secondary school in the years 2012-2014 at the yearly level. I only use the subset of students who are enrolled in schools located in french-speaking municipalities. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by a dummy for being a native speaker of the regional language. I include time-invariant individual-level controls in all specifications. Standard errors clustered at the school level are displayed in parantheses.

Table A.19: Average treatment effect for the probability of further academic education for french-speaking regions

Dependent variable:	**Further academic education**						
Sample:	**Full sample**	**Advanced track**	**Basic track**	**Tertiary educated parents**	**Non-tertiary educated parents**	**Native**	**Non-native**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NE × Post	0.0329	0.0428	0.0315	0.0398	0.0422	0.0291	0.0504
	(0.0116)	(0.0146)	(0.0074)	(0.0182)	(0.0140)	(0.0133)	(0.0186)
Pre-reform mean	0.579	0.757	0.175	0.744	0.520	0.598	0.498
Effect in percentages	1.81%	2.56%	0.21%	1.93%	20.97%	3.19%	-2.72%
Observations	61,745	45,337	16,408	19,415	19,953	43,312	18,433
R^2	0.2838	0.1608	0.0318	0.2752	0.2310	0.2674	0.3281
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:							
Female, Swiss, Unemployment	✓	✓	✓	✓	✓	✓	✓
Advanced track	✓			✓	✓	✓	✓
Native	✓	✓	✓	✓	✓		

Notes: This table reports estimates from estimating equation 1. I use data for cohorts first enrolled in lower secondary school in the years 2012-2018 at the yearly level. I only use the subset of students who are enrolled in schools located in french-speaking municipalities. In column (1) I use the full sample, in columns (2) and (3) I split the sample by assigned track, in columns (4) and (5) I provide split sample estimates by parents' education, and in columns (6) and (7) by for being a native speaker of the regional language. The row "Pre-reform mean" shows the mean of the respective dependent variable in the pre-treatment period in the reform canton. The row "Effect in percentages" express the treatment effect relative to this baseline. I include time-invariant individual-level covariates and school and time fixed effects in all specifications. Standard errors clustered at the school level are displayed in parantheses.

C Additional explanations

This section provides further insights on institutional details regarding cantonal differences in assignment to tracks, further subdivision of tracks, and potential influences on track assignment in Switzerland. It also discusses the choice of the observation period and potential implications of changes in student characteristics.

C.1 Assignment to tracks

Teacher recommendations for the assignment to tracks count in the cantons of Aargau, Bern, Basel-Landschaft, Basel-Stadt, Fribourg, Geneva, Jura, Lucerne, Neuchâtel, Nidwalden, Obwalden, Solothurn, Ticino, Uri, Valais, and Zug. In the cantons of Aargau, Bern, Basel-Stadt, Fribourg, Nidwalden, Solothurn, Ticino, Valais, and Zug students have the option to take an exam if they did not receive a recommendation. In the cantons of Appenzell Innerrhoden, Appenzell Ausserrhoden, Glarus, Grisons, St. Gallen, Schaffhausen, Schwyz, Thurgau, Waadt, and Zurich, students have to pass a cantonal entry exam. If individuals in these cantons have already been on the academic lower secondary school track and have received appropriate grades, they are directly referred to the academic track.

C.2 Subdivision of advanced and basic tracks

Note that if and how to subdivide the advanced and basic tracks further into more granular subtracks is a cantonal matter. Therefore some differences exist among my selected control cantons and the canton of Neuchâtel. In the pre-reform period the canton of Neuchâtel subdivides the advanced track into a most demanding subtrack with 2/3 of the students, and a demanding subtrack with 1/3 of the students. Of my selected control cantons, the cantons Appenzell Innerrhoden, Fribourg, and Solothurn subdivide the advanced track into a most demanding subtrack with roughly 1/3 of the students, and a demanding subtrack with 2/3 of the students. The cantons of St. Gallen and Schwyz have a lower secondary academic track on which roughly 2% and 7% of the entire student population in the first year of lower secondary school are enrolled. Except for the canton of Geneva that subdivides students on the basic track further into a less demanding subtrack with 2/3 of basic track students, and a least demanding subtrack with 1/3 of the basic track students, all other selected control cantons do not subdivide the basic track further. I check for changes in proportions of students assigned to these subtracks during my observation period by consulting yearly cantonal questionnaires for institutional evidence and by checking the evolution of shares of students on each canton-specific subtrack in the data.[31] Both the questionnaires and the evolution of shares do not show any evidence of major changes during my observation period.

C.3 Potential influence on track assignment

The scope to influence the assignment differs substantially from the canton of Neuchâtel in the cantons of Aargau, Bern, Basel-Landschaft, Basel-Stadt, and Thurgau. Students in these cantons are eligible to enroll for voluntary exams if they want to contest the assignment to a track by the elementary school

31. Source: https://edudoc.ch/search?ln=de&p=kantonsumfragen+ides&f=&action_search=Suchen&rm=&sf=&so=d&rg=10&c=Archivierte+Dokumente&c=Film&c=Monographien&c=Offizielle+Dokumente&c=Parlamentarische+Dokumentation&c=Zeitschriften&c=Zeitschriftenartikel&c=&of=hb&fti=1&fti=1, accessed on June 22, 2023.

teacher. In the cantons Grisons, St. Gallen, Solothurn, Zug, and Zurich, students have the possibility to enter a lower secondary academic track directly after elementary school if and only if they pass an entry exam. This is unlike the system in the canton of Neuchâtel, but it is not problematic for my analysis since even though technically all children have the right to take these exams irrespective of the track assignment by the elementary school teacher, students who take these very selective exams would have been assigned to the advanced track by their elementary school teacher with very high probability. Therefore, these students do not take the exam to contest a potential assignment to the basic track and if they do, their probability of passing these exams is very low. Hence, these students are surrounded by peers on the advanced track either way in my selected control cantons, since all classes in lower secondary school are tracked in these cantons. This is because students on both the academic lower secondary track and the demanding subtrack are categorized as advanced track students by the Swiss education system.

C.4 Observation period

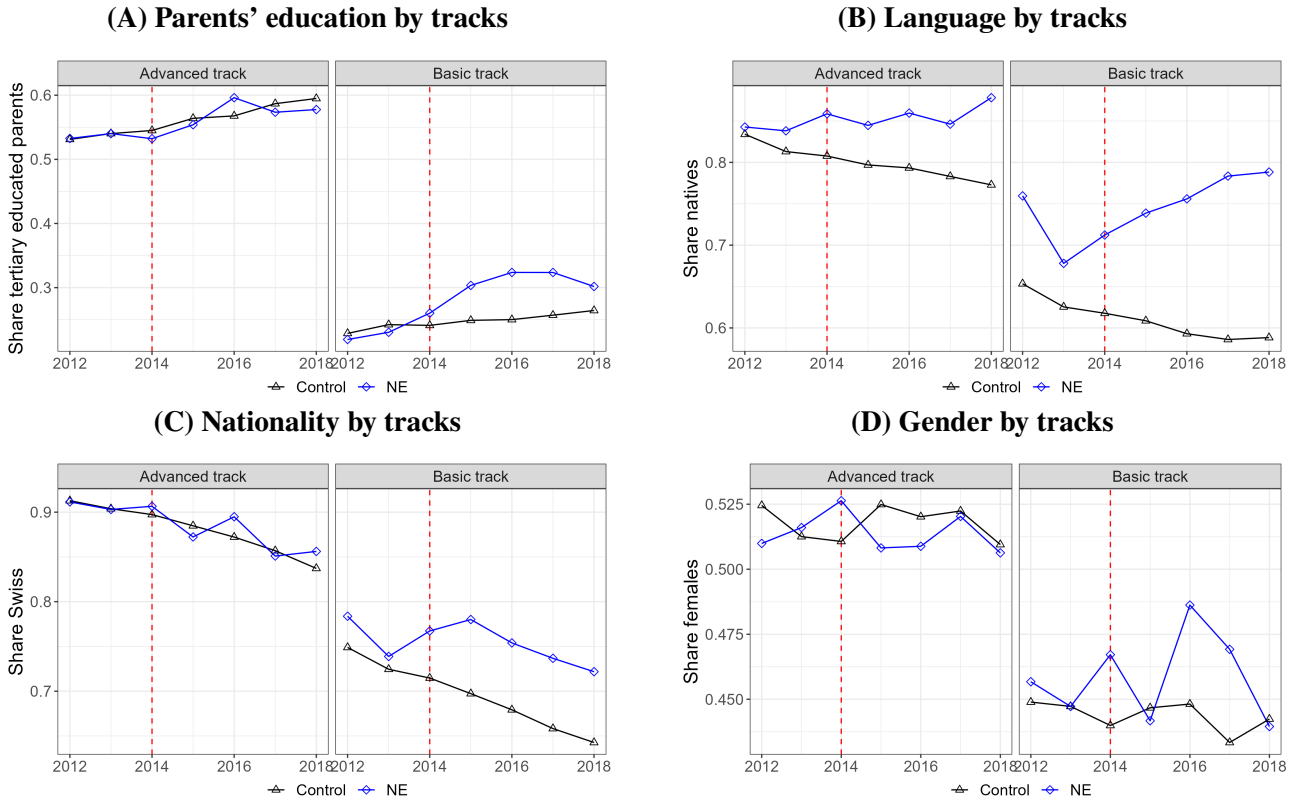
I exclude data recorded in the first available year in the LABB data, the year 2011 because I define the treatment assignment of a tracked or a detracked class to take place when an individual transitions from elementary to lower secondary school, hence at first enrollment in the first year of lower secondary school. For students registered in the first year of lower secondary school in the year 2011, I cannot ensure that they are enrolled in the first year of lower secondary school for the first time or whether they repeat the grade. I plot the share of students repeating the current grade in Appendix Figure A.4, which documents that students in the canton of Neuchâtel repeat the first year of lower secondary school slightly more often than students in the selected control cantons. Therefore, students enrolled in the first year of lower secondary school in the year 2011 might systematically differ across the treatment and the selected control cantons if repeaters differ in their unobservables from non-repeaters. The reason to exclude students enrolled in the first year of lower secondary school for the first time after 2018 is that outcome data is not available for those students.

C.5 Implications of changes in student characteristics

In Figure A.13, I plot the evolution of shares of individual characteristics separately for students on the advanced and the basic track over time for the canton of Neuchâtel and the selected control cantons. Panel (A) of Figure A.13 reveals that of the students with non-missing parents' education, relatively more students with tertiary educated parents are assigned to the basic track after the reform in the canton of Neuchâtel. If students with tertiary educated parents have a higher probability to contest the decision of the track assignment by the elementary school teacher, there are two potential explanations for this change. First, it could have been easier to protest against the decision of the teacher in the pre-reform period and therefore parents whose children were assigned to the basic track succeeded in contesting the decision of the teacher. In that case, children with tertiary educated parents were assigned to the advanced track relatively more often than children with non-tertiary educated parents of equal abilities. Second, it is probable that parents protested more often against a potential assignment to the basic track prior to the reform because track assignment might have been considered more consequential in tracked classes than after the reform in detracked classes. If either of the two is the case, assignment to the advanced track is a better proxy for actual abilities of students with tertiary educated parents in the post-reform period in the canton of Neuchâtel when compared to the pre-reform period and the selected control cantons. Consequently, the average student on the

advanced track in the canton of Neuchâtel would be higher-achieving in the post-reform period than prior to the reform because less lower-achieving students with tertiary educated parents are assigned to the advanced track. Additionally, students on the basic track would then be more likely to have parents with tertiary education in the post-reform period in the canton of Neuchâtel when compared to the pre-reform period and the control cantons, which is confirmed by the empirical evidence. In Appendix section C.6 and Figure A.14, I provide evidence, that the effect found in Table A.3 is rather caused by compositional changes. If there are behavioral changes after the reform of students or their parents these are minor.

Figure A.13: Student characteristics by tracks



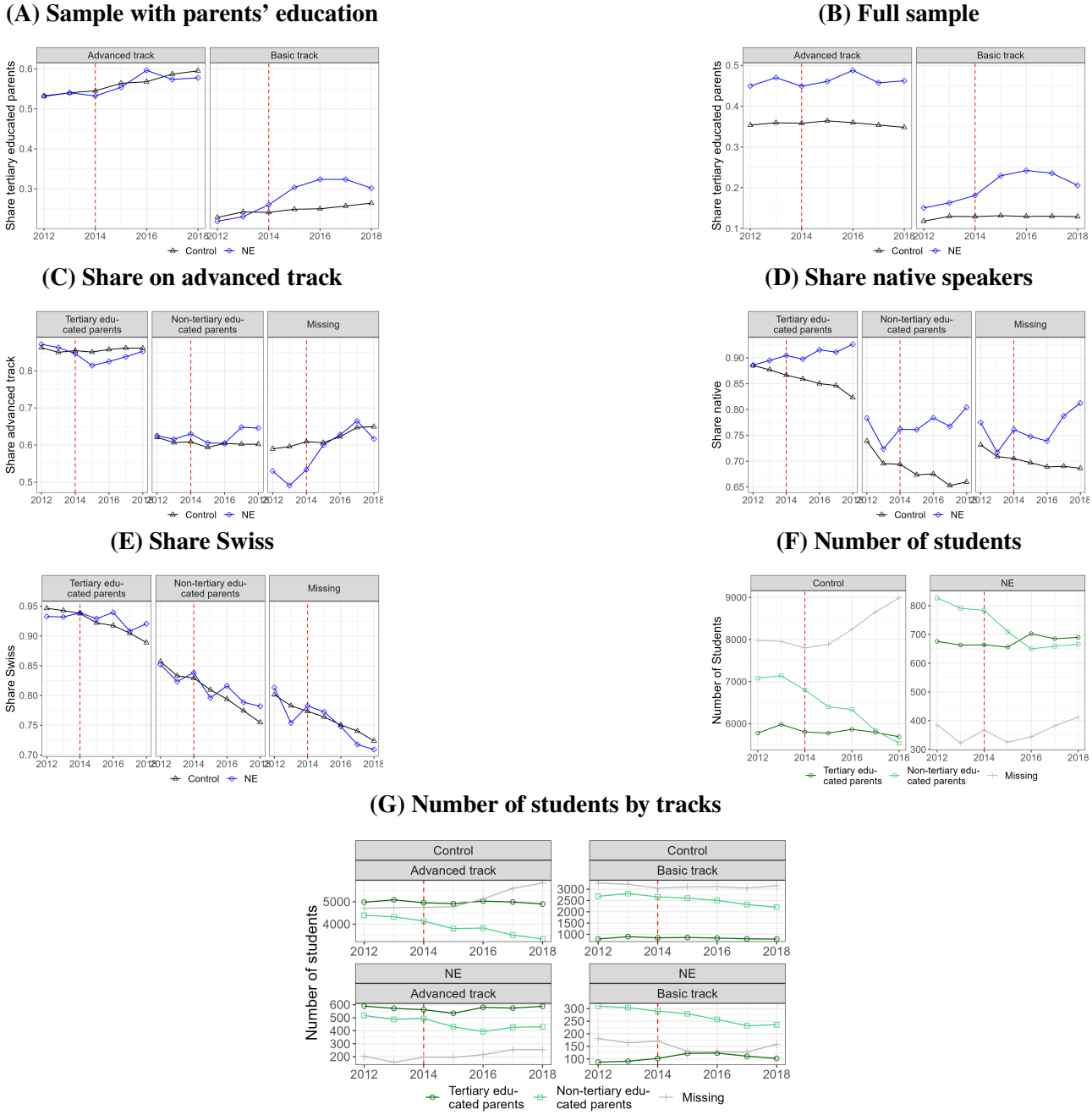
Notes: This figure depicts the share of the respective individual characteristic for advanced and basic track students over time. The treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. The last year prior to the treatment is marked with a red dashed line. Panel (A) shows the share of students with tertiary educated parents, panel (B) depicts the share of individuals who are native speakers of the regional language, panel (C) shows the share of Swiss individuals, and panel (D) reports female shares.

C.6 Parents' education across student characteristics

With Appendix Figure A.14 I aim to shed more light into whether the concerns related to behavioral changes that affect track assignment are justified in panel (A). I plot the share of students with tertiary educated parents of all students for whom parents' education is non-missing separately for students on the advanced and the basic track for the canton of Neuchâtel and the selected control cantons. Note that this is the same sample as used in the regression of which the estimates are reported in Appendix Table A.3 and this is the same figure as displayed in panel (A) of Appendix Figure A.13. Panel (B) of

Appendix Figure A.14 is similar as panel (A), but I plot the share of students with tertiary educated parents of all students, hence including the students for whom parents' education is missing in the sample when calculating the shares. Comparing panels (A) and (B) reveals that the evolution of the shares of students with tertiary educated parents is flatter than in panel (A) and the levels of the shares are lower especially for the selected control cantons. In panel (C) of Appendix Figure A.14, I plot the share of students assigned to the advanced track separately for students with or without tertiary educated parents and for students for whom information on parents' education is missing. Prior to the reform, students for whom parents' education is missing were much less likely to be assigned to the advanced track than after the reform in the canton of Neuchâtel. The students without information on parents' education seem to be similar in terms of their track assignment to students with non-tertiary educated parents. To further investigate this observation, I plot the share of students who are native speakers of the regional language and who are Swiss in panels (D) and (E) of Appendix Figure A.14 by parents' education. Again, students for whom the information on parents' education is missing seem more similar to students with non-tertiary educated parents than to students with tertiary educated parents in terms of these background characteristics. I next investigate the absolute number of students with tertiary and non-tertiary educated parents and for students for whom parents' education is missing separately for the canton of Neuchâtel and my selected control cantons in panel (F). Two observations can be made. First, the number of observations for whom information on parents' education is missing is much lower in the canton of Neuchâtel than in the selected control cantons and increases less over time. Second, only the number of students with non-tertiary educated parents declines over time in the canton of Neuchâtel, while the number of students with tertiary educated parents or without information on parents' education remains stable or increases. In panel (G) of Appendix Figure A.14, I check whether this decline is different for students on the advanced and on the basic track. It can be seen that the number of students on the basic track with non-tertiary educated parents declines more in the canton of Neuchâtel than in the selected control cantons, and the number of students with tertiary educated parents on the basic track rises somewhat. Hence, the rise in the share of students with tertiary educated parents who are assigned to the basic track in the canton of Neuchâtel after the reform in panels (A) and (B) of Appendix Figure A.14 can be partly attributed to the decline in the number of students with non-tertiary educated parents on the basic track, and not solely to the rise in the number of students with tertiary educated parents on the basic track. I conclude, that the effect found in Table A.3 is rather caused by these compositional changes. If there are behavioral changes after the reform, these are minor.

Figure A.14: Evolution of parents' education across student characteristics



Notes: This figure depicts the parents' education in the sample by different student characteristics. In panels (A) to (E) the treatment group (blue squares) includes individuals living in the reform canton, the control group (black triangles) includes individuals living in the selected control cantons. In panels (F) and (G) the number of students with tertiary educated parents are marked with dark green points, the number of students with non-tertiary educated parents with light green squares, and the number of students with missing parents' education with grey crosses. The last year prior to the treatment is marked with a red dashed line in all panels. Panel (A) depicts the shares of students with tertiary educated parents for the sample of students for whom parents' education is non-missing, panel (B) shows the same shares for the full sample. Panel (C) depicts the share of students on the advanced track by parents' education, panel (D) shows the share of individuals who are native speakers of the regional language by parents' education, and panel (E) depicts the shares of Swiss students by parents' education. Panel (F) and (G) show the number of students with tertiary educated parents, with non-tertiary educated parents, and for whom parents' education is missing in the selected control cantons and in the canton of Neuchâtel, where panel (G) additionally splits the sample by initially assigned tracks.

C.7 Implications for split sample estimates by parents' education

As pointed out in Appendix section C.5, initial track assignment might be a bad proxy for abilities of students with tertiary educated parents in the pre-reform period in the canton of Neuchâtel and in the selected control cantons, when compared to the post-reform period in the canton of Neuchâtel. Therefore, the estimated effect for students with tertiary educated parents might be attributed to the fact that students with tertiary educated parents on the advanced track are higher-achieving after the reform in the canton of Neuchâtel due to behavioral changes at track assignment. If this is the case, the reported estimate for students with tertiary educated parents in column (3) of Table 4 would be too high, since part of the treatment effect then has to be attributed to the higher overall ability of students in this split sample after the reform. The fact that my treatment effect is not statistically significantly different from zero supports the evidence in Appendix section C.6 that the observed negative correlation of tertiary educated parents and assignment to the advanced track is rather due to compositional changes in the sample than to behavioral changes of parents. Additionally, as discussed in Appendix section C.5 and C.6, the share of students on the basic track whose parents are tertiary educated increases in Neuchâtel after the reform. If parents with tertiary education support their children more and push them more to pursue further academic education the treatment effect reported for these children has to be attributed to the increase in parents' support and less to the detracked classes. To test my results to the sensitivity of the availability of the information of parents' education, I estimate all regressions using only observations for which I observe parental education. The parallel trends, the pre-trends tests, and the effects are of very similar size as the ones in the main sample and are available upon request.

C.8 Doubly-robust estimator

The conditional parallel trends assumption of the doubly-robust estimator proposed by Callaway and Sant'Anna (2021) only differs from the textbook difference-in-differences parallel trends assumption in conditioning on a vector of individual covariates X in my setting:

$$\mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, D = 1] = \mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, D = 0] \tag{2}$$

I estimate the following regression:

$$Y_{i,t} = \alpha_t + \alpha_g + \beta \mathrm{DC}_{i,t} + \gamma \mathbf{X}_i' + \epsilon_{i,t} \tag{3}$$

$Y_{i,t}$ is the binary outcome of interest of individual i in year t, where t indicates the year in which the individual is first enrolled in the first year of lower secondary school. α_t is a fixed effect for the year and α_g is a fixed effect for the canton. $\mathrm{DC}_{i,t}$ indicates whether an individual is in a detracked class in year t and β is the coefficient of interest identifying the causal effect of detracked classes on the outcome of interest, while $\mathbf{X}_i'$ is a vector of time-invariant individual-level covariates on which parallel trends condition. I include the same covariates as in my main specification to condition the parallel trends on. Finally, $\epsilon_{i,t}$ is the error term, clustered at the cantonal level.

Callaway and Sant'Anna (2021) propose a doubly-robust difference-in-differences estimator that estimates a nuisance function for each group g and each time period t in a first step and plugs the fitted values into the sample analogue of the group-time average treatment effect on the treated $ATT(g, t)$ in a second step. To calculate the average treatment effect on the treated (ATT), the group-time average treatment effects are weighted with weights according to their number of observations.