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Compulsory Schooling in the 21st Century: Short-Term Effects of Finland's 2020 Act on Compulsory Education*

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Abstract

This study examines the short-term effects of Finland's 2021 reform, which extended compulsory schooling to age 18. Using detailed administrative data, I analyze changes in educational outcomes and early labor market outcomes following the reform. The reform is associated with a 20% reduction in dropout rates between ages 15 and 18, with a sharp and temporary increase in dropout rates once the law no longer binds. On average, students in the first affected cohorts are 1.9% (1.5 percentage points) more likely to graduate from secondary school by the age of 20 and complete 1.2 percentage points more courses relative to pre-reform cohorts. Time spent outside of education, employment, or training is reduced by 0.38 months on average, while annual earnings decrease by 86 euros. Heterogeneity analyses indicate relatively stronger responses among students from low-SES and foreign backgrounds and among males.

Keywords: Compulsory schooling, Education policy, Secondary education

JEL Classification: I21, I28, J24, J18

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1 Introduction

Compulsory schooling laws (CSLs) are universal and have been studied extensively in the literature. These laws are designed to raise skill levels and reduce educational inequality while also keeping young people engaged with social programs delivered through the schooling system, such as healthcare and support from social workers. Higher levels of educational attainment are consistently linked to higher earnings, improved health, and better social outcomes (Deming & Silliman, 2025; Oreopoulos & Salvanes, 2011). Policymakers therefore use compulsory schooling requirements to protect young people from setbacks that might otherwise derail their long-term educational progress. However, because most of these laws were enacted in the 20th century, contemporary evidence on their effects is limited.

In Finland, a tenth (10.1%) of the population between the ages 25-34 has not completed an upper secondary degree, which is below the OECD average (12.8%) and at a comparable level to similar systems in Sweden (12.3%) and the UK (11.5%) (OECD, 2023). In an attempt to raise the educational attainment, Finland extended its compulsory schooling requirement to the age of 18 in 2021. Previously, compulsory schooling ended once a pupil completed 9th grade, which typically happens at the age of sixteen. I use the reform to examine how students respond to a compulsory schooling law in a modern context, where many would have remained in school anyway, and monitoring systems are advanced but imperfect. I leverage rich administrative data to study the effects of a CSL on time spent in education and educational achievement, measured by enrollment, course credits, and graduation rates. Examination of various schooling outcomes reveals not only whether the students spent more time in education but also their educational achievement during that time. I match students in the national education registry to their monthly incomes and government benefits to build a comprehensive picture of the short-term dynamics of employment and social exclusion.

The key problem in estimating the effects of compulsory schooling reforms, which are implemented across cohorts, is the lack of a relevant comparison group, making it hard to separate the reform's impact from confounding inter-cohort differences

(Grenet, 2013; Stephens Jr. & Yang, 2014). I cannot account for the problem directly, but I mitigate these concerns by combining multiple complementary empirical strategies to examine the reform and controlling for time confounders using adjacent cohorts, who are still in school during the reform.

The empirical analysis proceeds in two steps. First, I examine how school leaving decisions change across reform cohorts by estimating a Cox regression model. After documenting changes in school leaving rates, I present results from pre-registered approaches that form the main confirmatory analysis[1]. I estimate a two-way fixed-effects model that accounts for time-varying shocks that affect the cohorts similarly and absorbs time-invariant differences related to the stage of studies. I follow the pre-analysis plan with one main exception: I replace age fixed effects with grade fixed effects.[2]

The survival analysis demonstrates a strong reduction in relative dropout rates. On average, students are 20% less likely to drop out between ages 15 and 18. The effect is strongest at ages where students transition from comprehensive school to secondary education, and diminishes as they get older. Conditional on remaining at school until 18, post-reform students are 10% more likely to drop out, as the new law no longer binds. This validates the logic of CSL instruments introduced by Angrist and Krueger (1991), where some students continue their education only because the law requires it and leave once the mandate ends. Nevertheless, even after the law no longer applies at age 18, the overall share of students who leave school at least once remains below pre-reform levels.

Differences across cohorts show that graduation rates for the post-reform group are 1.5 percentage points higher than in the pre-reform group, where 77.9% acquire a degree in the same time frame. Absolute changes in graduation rates for men, low-income students, and immigrants are close to the whole population, but correspond to slightly higher relative effects (2-2.3%) due to a lower baseline. Post-reform students spent on average 0.3 more months in school and face a corresponding decrease in

[1]The pre-analysis plan is available at Open Science Framework: https://osf.io/qyfrz

[2]The pre-specified model suffers from omitted variable bias, as it compares on-time students with those who were held back. The full results from the original specification are included in the Appendix as part of the populated PAP, illustrating the severity of the misclassification problem.

months spent as Not in Education, Employment or Training (NEET). Completed coursework increases on average by 1.2 percentage points.

The vast majority of students attend secondary school regardless of the law, and average outcomes may therefore conceal the policy's impact on those most likely to respond to it. To focus on these students, I employ a pre-specified prediction model to identify individuals at risk of early dropout and compare their outcomes within and across reform groups. Compared to at-risk students in previous cohorts, post-reform at-risk students spend two additional months enrolled in education, complete 5.1 pp more secondary studies, and are 2.5 pp more likely to graduate. The reform also induces a small adverse effect on annual earnings.

The reform extends compulsory schooling from 9th grade graduation (which typically happens at age 16) until a person's 18th birthday. On average, students face an additional 19.6 months of schooling. Since some pupils are a year older, when they finish 9th grade, the length of the compulsory schooling extension varies from 5 to 25 months of instructional time. Shortest extensions are concentrated among students who graduate from 9th grade a year older than their peers. As a result, educationally disadvantaged students also experience less exposure to the reform. This pattern is consistent with Adamecz (2023), who shows that age-based compulsory schooling laws are less effective in systems with grade retention.

Another key element of the reform is that it extends compulsory schooling from one stage of education to another. I show that reductions in early school leaving are driven by people sorting into non-degree education and vocational school for the duration of compulsory schooling, where only the latter yields a secondary diploma. This suggests that the future wage returns induced by the reform may differ across these students, as labor market institutions tend to reward those who obtain formal qualifications due to the reform (Grenet, 2013). Moreover, the reform does not require students to remain in education long enough to complete a degree. Students who finish secondary school on time generally graduate in the year they turn 19, while completing school before age 18 is rare, even among high-achieving students.

My primary contribution to the literature is to bridge the temporal gap in CSL literature. Despite enduring interest in the effects of mandatory schooling (Clark, 2023; Domnisoru, 2021), few studies examine how they function in the 21st century. To my best knowledge, the most recent example is from Cabus and De Witte (2011), who show that a one-year increase in compulsory schooling in the Netherlands reduced dropouts by 2.5 percentage points.

There is an extensive literature studying compulsory schooling laws to identify the causal effect of education on a broad range of outcomes, starting with Angrist and Krueger (1991). Early studies using CSL instruments in the United States find that the estimates on monetary returns to education align with Mincerian approaches (Acemoglu & Angrist, 2000; Oreopoulos, 2006). However, later research demonstrates that these effects disappear once birth year effects are allowed to vary across geographical areas or changes in school quality are accounted for (Stephens Jr. & Yang, 2014). Similarly, regression discontinuity approaches have shown mixed results on labor market outcomes in Britain and France (Clark, 2023; Domnisoru, 2021; Grenet, 2013).

These mixed results highlight the importance of institutional context for both internal and external validity of the estimates. Clark (2023), who finds a limited effect on wages, hypothesizes that the results are explained by the subpar quality of additional education. In the Hungarian setting, similar null effects are explained by the fact that potential compliers are not affected by the laws, due to grade retention (Adamecz, 2023). Pischke and Von Wachter (2008) speculate that observed null results are because an additional year does not teach wage-relevant skills in the German context. Angrist and Krueger (1991) note that compulsory schooling may increase social costs as "students who are compelled to attend school interfere with the learning of other students"

Evidence on the non-pecuniary effects of compulsory schooling is more consistent. A meta-study on the effects of compulsory schooling laws on health outcomes concludes that an increase in education driven by these reforms decreases obesity, mortality, smoking, and hypertension, but increases alcohol use and cancer risk (Hamad et al., 2018). Compulsory schooling laws also reduce crime and alter crime-age profiles (Bell et al., 2016, 2022).

The rest of the paper is structured as follows. In Section 2, I describe the Finnish educational system and Finland's 2020 Act on Compulsory Education. Section 3 presents the data and summary statistics, followed by empirical analysis in Sections 4 and 5. Robustness is discussed in Section 6 and Section 7 concludes.

2 Institutional details

2.1 Compulsory Education in Finland

a Finland's 2020 Act on Compulsory Education

The duration of compulsory schooling was extended in 2021. Under the previous law, students could end their education after either graduating from comprehensive school or when ten years had passed since their compulsory schooling started. The new legislation requires students to continue their education until they turn 18 or graduate with a secondary degree. This reform took effect in January 2021 and applies to students who were in comprehensive school at the time, affecting students born in 2005 or later[3], as well as those born in 2004 who either repeated a grade or started comprehensive school later than usual. I plot the exposure to the reform by month of birth in Figure 1.

The policy's stated goal is for every student to graduate with a secondary degree. Preparation for the law began after a new government took office in the spring of 2019, and a legislative proposal was introduced in November 2020, passing the following month. The relatively swift turnaround is partly due to an earlier proposal, suggested in 2014, which failed to reach a vote due to cost concerns. Additionally, the policy's potential effectiveness had been assessed ahead of the 2019 parliamentary election (Seuri et al., 2018).

The first cohorts affected by the reform applied to secondary education in the spring of 2021. The timeline of secondary education for typical students born in 2005 is depicted in Figure 2. While most students typically complete their secondary degrees in 3-4 years, some enter through preparatory programs that can last up to

[3] Compulsory schooling is not extended for a small number of students born in 2005, who finished 9th grade in 2020.

a year. The extension ends well before a typical student graduates from secondary school and does not require them to finish a degree.

The reform was bundled with additional changes to secondary education. First, the materials and tools for students in secondary education were made costless[4]. Previously, students had to cover items such as books, laptops, software, matriculation exam fees, and tools themselves. Students who live at least seven kilometers from their school are eligible for a transport subsidy. Additionally, labour legislation was slightly altered to forbid employers from scheduling work for times when a student's presence in a secondary school is required.

b Enforcement of the Compulsory Schooling Law

Responsibility for ensuring compliance with the compulsory schooling laws is shared between schools and municipalities. In practice, schools monitor student attendance and notify both the legal guardians and the municipality if a student fails to meet their obligations. If a student is not enrolled in any educational program, their municipality of residence is required to place them in a preparatory education program within two months. Monitoring is enabled by a centralised system for tracking students' presence, study rights, contact information, and admission history. This system is accessible to public sector officials, schools, and legal guardians. Ultimately, the legal liability for compliance lies with the student's legal guardians, and failure to fulfill this duty may result in fines.

2.2 Finnish Schooling System

a Overview

In Finland, compulsory education begins the year students turn seven and enter comprehensive school[5]. School-based learning is the norm in Finland and all com-

[4]In some municipalities, students who graduated comprehensive school in 2020 but started secondary school in 2021 were also entitled to costless secondary education.

[5]Deviations from the starting age require an evaluation by a psychologist and possibly a physician. Pre-school has been mandatory since 2015.

prehensive and secondary education is publicly funded. In 2023, 0.1% of the pupils were homeschooled (Statistics Finland, 2025a). Comprehensive school lasts nine years, and a typical pupil graduates in the spring of the calendar year when they turn 16. Due to delayed starting age, grade retention, migration, and preparatory language training, slightly fewer than one-tenth of the student population graduate a year older. Grade repetition is rare, especially at older ages, with 0.2-0.4% retention rates in the 9th grade from 2020-2024.

Students who graduate from 9th grade are eligible to apply to secondary schools. There are two main secondary tracks: a general secondary education track and a vocational track. Applications are made through a centralized application system in the spring, and students are admitted using the deferred acceptance algorithm. Admissions scores are primarily based on students' grade point averages from comprehensive school, as there is no standardized testing. However, secondary schools may also set additional admission requirements, such as entrance exams or other criteria.

The target time for completing secondary education is three years, but students may extend their studies beyond the target[6]. Students can also apply to preparatory education or to extend their basic studies in a comprehensive school by a year. These transition phase programs are designed for pupils who need additional support or lack the grades to qualify for their preferred secondary degree program.

b Academic Track

Approximately half of the students enter the general academic track after compulsory school. The academic track aims to prepare students for tertiary education and the curriculum consists of studies in natural sciences and humanities. Students must complete the minimum mandatory study credits and pass matriculation exams to graduate. After an admission system reform in higher education in 2018, matriculation exam scores are the main admission criterion for university degree programs.

[6]Study rights in the general track allow for four years of studies and may be extended by the school under special circumstances. Students who qualify for the vocational track have the right to study until they graduate or drop out.

In the sample, two-thirds (68%) of students who enter academic track graduate secondary school within three years and 92.2% within four.

c Vocational Track

Vocational schooling in Finland is primarily school-based. Applicants can choose from 42 vocational tracks within six broad fields of arts and humanities, business and administration, technology and transport, natural resources, health and welfare, and hotel and catering[7]. Students may also apply for a mixed track, where they can complete part of the courses in general academic track and participate in matriculation exams alongside the vocational training. In addition to the centralized application during the spring, vocational schools have continuous application processes for students not enrolled in a degree programme or who wish to switch tracks. Graduation rates in vocational schools are significantly lower than in academic track. Around two-fifths of each cohort continue to vocational training after 9th grade. From 2019 onwards 63.9% of students graduated within three years and 76.6 % within four.

Basic vocational degrees consist of both occupation-specific training and general academic coursework, with an emphasis on vocational skill courses. Earning a vocational degree grants eligibility to apply for tertiary education. However, vocational graduates who enter tertiary education typically pursue degrees in universities of applied sciences.

d Preparatory Education

The reform mandates attendance in a preparatory education program for youth under 18 who are not enrolled in a secondary school. Preparatory programs are designed for students who lack the language skills, grades, or otherwise find it difficult to continue to a secondary degree programme. The reform formalized several existing options and combined preparatory tracks administered by municipalities.

[7]Vocational schools offer more than 150 occupational degrees, but most require prior training or work experience for enrollment.

Before the reform, municipalities could voluntarily organize preparatory education for the academic track (LUVA) and vocational schools (VALMA). VALMA was a comprehensive mix of language training, life coaching, vocational and comprehensive school courses, as well as labor market training, while LUVA was targeted at immigrants who needed to improve their language skills before applying to the academic track. Alongside the preparatory tracks, pupils could also apply for an additional year of comprehensive school, often referred to as tenth grade. Students with special needs may also attend the TELMA program, which prepares them for work and independent living

LUVA and VALMA were combined into a single preparatory programme, TUVA, in 2022. The separate option to continue comprehensive school was also abolished in 2022, as basic education is also offered in the TUVA education. Municipalities must place the youth outside of education into TUVA.

The youth may also attend liberal education institutes (Kansanopisto) to comply with the law. Liberal education institutes have two distinct features that differentiate them from options offered by municipalities. First, they offer a boarding school-style education, where students may live on the premises for the year. Second, the establishments have more freedom in designing their curricula. National guidelines allow the institutions to allocate around half of the instructional time for activities such as dancing, life stance education, learning an instrument, or gaming. Studying in an accredited liberal education institute is costless, including accommodation and upkeep.

3 Data and Summary Statistics

My primary data source is the KOSKI database, a nationwide registry established in 2018 and maintained by the Finnish National Agency for Education. KOSKI includes detailed records on student enrollment, credits, and course-level grades. I link these data to individual-level administrative records from Statistics Finland's FOLK modules, which allow me to combine information on students' educational attainment with rich background characteristics.

For labor market outcomes, my primary data source comes from the Finnish Tax Administration's income registry, which contains detailed monthly records on wages, pensions, and government benefits reported by employers and other payers. Monthly income data are available beginning in 2019, and benefit records from 2021. Finally, I link these data to the VIRTA higher education achievement register, administered by the Finnish IT Center for Science, which provides information on enrolment in tertiary education.

The key advantage of these data is that I am able to build a comprehensive picture of students' status in a given month. In each month, I observe whether an individual is enrolled in school, employed, or serving in the military. [8] Using this information, I define an indicator for being 'Not in Education, Employment, or Training' (NEET), which I use throughout the paper to denote individuals' NEET status.

Table 1 provides detailed definitions of the variables. My main sample consists of students enrolled in 9th grade during the spring semesters of 2018–2019 through 2021–2022.

3.1 Sample Selection

I restrict the main sample to people who reside in Finland permanently from age 15 to 20 in order to deal with false dropouts from emigrants and exchange students. This choice also excludes refugees arriving from Ukraine, following the Russian invasion of Ukraine in 2022. I additionally exclude people who reside in Åland, an autonomous archipelago region of Finland, because the national education database does not cover this region[9].

Furthermore, I focus on pupils who finished comprehensive school in academic years 2018/2019-2021/2022. These restrictions result in a sample of 236,936 individuals [10] and are made due to two data constraints. First, the KOSKI database

[8]Finnish men must complete mandatory military or civilian service by the age of 29; women may participate voluntarily. Conscripts receive bimonthly allowances during their service.

[9]Åland is also exempt from the reform but may not be used as a credible control due to institutional differences and data availability.

[10]According to official data, 237,724 pupils graduated from comprehensive school between 2019-2022 (Statistics Finland, 2025b), meaning my sample covers roughly 99,6 % of this population. I

was established in its entirety in 2018, meaning I have comprehensive data on primary and secondary education starting from the academic year 2018-2019. Academic records from the spring of 2018 are set aside and mainly used to train the dropout prediction model. Second, I observe student earnings only from 2019 onward and military allowances only from 2021, which limits the number of cohorts for which I can measure NEET status or employment outcomes.

The law applies to students who had not completed their compulsory education by January 2021. I refer to these individuals as the post-reform cohorts. Post-reform group consists primarily of students born in 2005 or later, as well as some born in 2004 who had not yet graduated when the law took effect. The pre-reform cohorts are those who completed comprehensive school before 2021, mainly students born in 2003 and 2004, along with late graduates born in 2002.

Table 2 reports summary statistics for the main sample by comparison group. Background characteristics are measured at the end of the calendar year in which a student turns fifteen, with the exception of age at comprehensive school graduation and grade point average. Differences between the groups are modest. The majority of students speak one of Finland's two official languages, Finnish or Swedish, as their primary language. The share of students speaking another language increases across cohorts by roughly one percentage point (from 6.4% to 7.3%), consistent with broader growth in the immigrant population. Around eight percent of all 9th graders are one year older than the typical student, but those subject to the reform are slightly more likely to complete comprehensive school on time, with 0.6 pp fewer 17-year-old graduates. Average academic achievement, measured by GPA, is nearly identical across groups, as is the share of parents with secondary education. Notably, parental earnings are slightly higher in the reform group, even though income is measured during the COVID-19 pandemic.

Table 3 depicts changes in enrolment status after the reform. Share of students outside of any education, training, or work drops by 1.6 percentage points (41%), and the share of people engaging in work or non-degree[11] training drops by 0.9 per-

also remove 61 people, whose extension status I'm unable to determine.

[11]Non-degree training refers to activities such as evening courses at the academic track or voca-

centage points (28%). Simultaneously, we observe a 1.7 percentage point increase in enrollment in preparatory education and a 0.7 percentage point increase in secondary enrollment. A relatively larger inflow into preparatory tracks suggests that many students who remain in school because of the reform are either unwilling or unable to transition directly to secondary education. Out of the students who are in a preparatory program at the end of the fall semester, 70% enter a secondary degree track a year later.

Admissions remain at similar levels to pre-reform cohorts, although the academic track is slightly less popular among post-reform cohorts, with a 0.7 pp decrease in admission shares. Moreover, Figure A.1 suggests that broader trends in selection into academic or vocational schooling are stable around the reform, though a smaller share of students enters the academic track the year after the reform. In Figure A.2 I show that graduation timing does not exhibit patterns that would suggest manipulation around the extension cut-off.

Because the extension of compulsory schooling is tied to a student's birthday, its exact length varies across individuals. I define the length of the extension as the number of instructional months between the completion of 9th grade (when the previous compulsory schooling mandate would have ended) and the month a student turns 18. Figure 4 depicts the relationship between selected variables and the length of the extension. The extension length is systematically related to student background: it is positively correlated with parental education and household income, while students whose primary language is one of the non-official languages face, on average, a one-month shorter extension. These correlations suggest that linking compulsory schooling to age rather than attainment places lighter demands on students most at risk of leaving school early.

tional training that is not part of a degree program.

4 Student Retention and Dropout Timing

A rich literature has documented that school-leaving laws increase educational attainment, typically measured by the highest completed grade or the age at which individuals leave school (Angrist & Krueger, 1991; Bell et al., 2022; Oreopoulos, 2006; Stephens Jr. & Yang, 2014). Motivated by these results, I begin by examining compliance with the compulsory schooling reform and analyzing the precise timing of school leaving.

The enrolment data includes start and end dates for each student in comprehensive, preparatory, and secondary education. Schools are supposed to flag a student as resigned in the database if they fail to attend for a month, or they may also grant a temporary leave of absence in case of extraordinary circumstances. In the main analysis, dropping out is defined as the first time a person is not enrolled in any compulsory education, for at least four consecutive months, without graduating from secondary school [12]. In this analysis, I use the terms dropout and school leaving interchangeably to refer to students who are no longer enrolled in any form of education.

In practice, I compare school leaving probabilities between students in adjacent birth cohorts. Although the timing of the compulsory schooling reform is arguably unrelated to students' characteristics, the cohorts may not be comparable. One potential concern is that differences between reform cohorts are influenced by unrelated trends in school attendance, such as attitudes towards schooling or educational scarring related to Covid-19. To alleviate this concern, I plot the share of people outside of schooling at age 16 in Figure 5. From Figure 5 we see that enrolment rates are relatively stable at the time when a typical member of the pre-reform group finishes compulsory education. Reassuringly, the figure also depicts a sharp drop across birth cohorts after 2004, suggesting that the reform is the main driver of differences in student retention.

[12] Municipalities must place a person into preparatory education within two months of receiving a notice of their dropout. Defining drop-out as four months is long enough to detect relevant, prolonged dropouts, but also allows me to extend the panel relatively far into secondary school. Results are robust to alternative definitions, which are presented in Table A.5

The compulsory schooling law affects dropout rates in two ways: it brings more students into secondary school initially and prevents those who would have dropped out from leaving. To examine these combined effects, I plot the dropout hazard functions for pre-reform and post-reform cohorts in Figure 6. The graphs show the probability of dropping out at each age, conditional on having remained in school up to that point. For clarity, I scale the empirical hazard rates to dropouts per 1,000 students per month.

The results show that the reform substantially reduces dropout rates until age 18, in both relative and absolute terms. The largest differences between cohorts appear around age 16, when students typically enter secondary school. This timing reflects a key change in how compulsory school graduates are tracked. Post-reform students, who fail to gain admission to secondary school, are directed into preparatory education programs. Before the reform, participation in these programs was voluntary. Differences in exit rates decrease with age and once schooling is no longer mandatory at age 18, dropout hazard spikes above rates observed in the pre-reform group. Although the sharp increase in exits suggests that some students remain in school only due to the law, I cannot rule out the possibility that the schools delay reporting school leavers until their monitoring responsibilities also end. It is important to note that leaving school at these ages is not necessarily final, as some students return to education over time. Nevertheless, fewer than half of those who leave school for four months re-enroll within a year, making dropout a highly consequential decision.

Figure 7 shows the corresponding cumulative drop-out hazard. A month before students turn 18 and the extension ends, cumulative drop-outs are 2.8 percentage points lower, which represents a 34.5% decrease. Despite an acceleration in exits when students turn 18, I observe a 2.3 pp (21.9%) reduction when students turn 19. The reform appears to induce path dependence, with an excess mass of students continuing their studies beyond the new school-leaving age. This suggests that individuals who comply with the law change their behavior in two distinct ways: some remain enrolled until they turn 18 and exit immediately thereafter, while others continue beyond their 18th birthday, presumably to complete a secondary degree.

Despite continuous opportunities to return to education, dropping out is a relatively persistent phenomenon. Figure 8 illustrates return rates conditional on having been out of education for at least four months. On average, fewer than half of dropouts re-enroll within a year, and there are no discernible differences in return rates across cohorts. Table 4 shows that four months after leaving school, fewer than one in five dropouts are employed, while more than half are classified as NEET, suggesting that school leaving is not primarily driven by labor market opportunities. Even post-reform students who exit education relatively young, at age 16, do not return to education any faster than similar pre-reform dropouts,though students who drop out regardless of the law are more negatively selected. Table 4 shows that post-reform students are more likely to be outside of education and employment four and twelve months after dropping out.

4.1 Hazard Ratios

To quantify the effects of the policy on dropout rates, I estimate a semiparametric Cox regression:

$$h(t) = h_0(t) exp(\delta \times Reform + \beta X_i) \tag{1}$$

where $h_0(t)$ is the baseline hazard and *Reform* indicator takes a value of one for people whose compulsory schooling was extended. I also control for pre-determined covariates X_i. Descriptive analysis of hazard rates clearly shows that the effects of the reform are age-varying and the proportional hazard assumption is violated, since the nature of the law changes once students turn 18. As a result, δ represents a weighted average of age-varying effects of the reform. Moreover, this estimate depends on the duration of the follow-up, as censoring at different ages also changes which age effects enter the weighted average. For this reason, I estimate an additional model with age-reform interactions to capture dynamic changes in hazard ratios. Still, this approach does not merit a causal interpretation, since I end up comparing treated students who would have quit earlier if allowed, to a comparison group where the students with a high propensity to leave have already exited. In other words, pre-reform students who remain in secondary school voluntarily have different potential outcomes from those who stay in school at an older age due to the policy.

The average hazard rate from age 15 to 19 is shown in the first column of Table 5. On average, treated students are 19.4% (HR 0.806) less likely to exit education at a given age. Columns 3 and 5 of Table 5 show that this effect increases slightly once I account for differences in parental background across cohorts.

In column two, I replace the *Reform* indicator with $Reform \times Age$ interactions, allowing for the effects of the reform to vary with age. Joint test for non-proportional hazards is rejected at confidence level of $p > 0.10$, suggesting hazard ratios within each cell are in fact proportional. Reform is associated with a 45.2% decrease in dropout rates at age 16. The hazard rates converge as students become older, and the reduction recedes by two-thirds as students turn 17. Treated students are 10.8% more likely to drop out after the new law no longer binds. The results are robust to adjusting for background covariates, as shown in columns 3 and 6.

In table 6, I repeat the analysis separately for males, students with a foreign background, and students from low income background. Reduction in exit hazard and age dynamics for males and low-income students is very similar to the full sample, but the average effect is smaller on average. Average dropout rates do not change for students with a foreign background. Dynamic hazard rates reveal that this difference is driven by a drastic increase in hazard ratio once they turn 18.

The survival analysis demonstrates that students are less likely to drop out after finishing 9th grade, and this effect persists even after the law no longer binds. Still, we also observe a significant amount of noncompliance, pointing towards imperfect enforcement[13]. For subgroups, where educational attainment was lower prior to the reform, we observe that exit rates surge above average once they are allowed to leave education. In the next section, we explore what students accomplish during their time in education and quantify the average increases in schooling duration.

[13]Anecdotal evidence tells that teachers may not be motivated to drag some students back to school.

5 Academic and Labor Market Outcomes

One concern related to the reform raised by media and policymakers is that although students are enrolled on paper, they fail to show up or engage in learning activities. In this section, I investigate what students *actually* accomplish while enrolled. To construct a comprehensive picture of educational attainment, I consider several complementary measures of schooling. Specifically, I look at the cumulative months spent enrolled in any education and course completion during that spell. Then I quantify to what extent the reform affects graduation probabilities. Since the compulsory schooling law also changes opportunities to attend work or pursue other interests, I examine the effects on annual earnings and cumulative months spent not in education, employment, or military service.

My goal is to isolate the effect of the reform on outcomes listed above. The main challenge in achieving this target is that I do not observe comparable students with a different CSL status at the same stage of their education, at the same time. The extension of compulsory schooling is based on the stage of a person's comprehensive education at the beginning of 2021 and only applies to pupils who finish 9th grade in 2021 or later.

Since exposure to the reform is based on graduation class, I cannot distinguish the unobserved shocks within $grade \times semester$ cells from effects induced by compulsory schooling. Furthermore, since the policy is essentially implemented across birth cohorts, I cannot include cohort fixed effects due to multicollinearity with grade and time period. In practice, this means that the causal effect of the reform on the post-reform group is only captured under the strong assumptions that 1) potential outcomes are equal across reform cohorts, 2) unobserved time shocks do not impact students differently across their stage of education. Although the reform cohorts are adjacent, I cannot rule out that changes in secular trends may affect the estimates. Examination of the cutoffs between different birth cohorts in Section 6.1 shows negligible differences in schooling outcomes between pre-reform cohorts. These results support the assumption that adjacent cohorts are similar in terms of unobserved characteristics, suggesting that any bias from birth cohort effects is likely to be limited. Still, the results should be interpreted primarily as descriptive rather than causal.

5.1 Differences Across Cohorts

The first set of results compares average differences across reform cohorts using the following specification:

$$Y_{it} = \alpha_t + \alpha_g + Reform_i + \beta(Reform_i \times Post) + \epsilon_{it} \quad (2)$$

where α_t captures common time effects for each semester or year, while α_g represents fixed effects for grade level, defined as academic years relative to graduation from 9th grade. *Reform* is an indicator that takes a value of one, if a person's compulsory schooling was extended and captures any differences in outcomes that accrued before the implementation of the reform. *Post* takes a value of one starting in 2021, and β captures the average differences in outcomes after the compulsory schooling is extended. The residual is denoted by ϵ_{it} and standard errors are clustered at individual level. Except for the revised definition of α_g instead of an age fixed effect [14], the analysis presented in this section follows the pre-specified design.

I trace out the dynamic differences between reform cohorts, conditional on time and the lagged variable in Figure 9. Panel A displays the average differences in months enrolled in education. The difference initially increases by approximately one-quarter of a month, reaching its peak of 0.6 months in the third year following comprehensive school. This confirms that reducing early school leaving has a statistically significant, although modest, effect on average educational duration. Although enrollment effects are immediate, differences in course completion do not appear until one year later. Panel B illustrates the differences in completed studies following 9th grade. Lagged responses are consistent with the fact that reform-induced enrollees typically spend the first year in preparatory education before beginning work towards their degree. By the fourth year, reform cohorts are, on average, 1.7 percentage points closer to completion of a secondary degree.

[14] See Appendix A.1 and the populated PAP for further details.

Bottom panels show that the reform reduces social exclusion and earnings in the middle of secondary school. From Panel C we see that by the time post-reform cohorts begin secondary school, they have experienced less time as NEET, and the gap expands over time. This pattern is symmetric to the enrollment effects shown in Panel A. The fact that pre-reform pupils have spent less time as NEET, conditional on time and grade level, raises some concern. In the main specification, the group indicator captures these differences. However, if future social exclusion is path-dependent, this could bias the results. Since no similar differences appear in average enrollment, the discrepancy likely stems from a few outliers who entered NEET status or home-schooling during the pandemic. Finally, Panel D reveals alternating signs in annual earnings, with positive gains emerging between the second and third years after 9th grade. As more vocational students enter paid apprenticeships, they receive compensation for time spent in education; however, those who remain enrolled beyond the target duration forgo early labor market earnings in favor of completing their degree.

Table 7 reports the estimates for Equation 2. Columns (1)–(4) present average differences in cumulative outcomes, thereby reflecting the average accumulation of effects associated with the reform. In column (1), the first treated cohort exhibits a 1.5 percentage point increase in the probability of graduating by age 20. From column four, we see that individuals in the post-reform cohorts have, on average, accrued 1.2 percentage points more of completed studies. These modest average gains are largely attributable to the high share of students who would have completed secondary school regardless of the compulsory schooling law in place. By the age of 20, graduation rates among the pre-reform cohorts average 77.9%, with individuals having completed approximately 89% of the minimum coursework requirements. Columns two and three indicate that the average time spent as NEET decreases by 0.38 months, while time enrolled in education increases by a similar magnitude (0.3 months), suggesting that additional schooling primarily substitutes for time otherwise spent outside the education and employment system, thereby reducing social exclusion. The dynamic estimates displayed in Figure 9 indicate that these differences emerge almost immediately following the completion of 9th grade and are consequently more pronounced in the early post-compulsory years, when time spent as outside of education remains uncommon. Finally, average annual earnings decline by approximately 100 euros following the reform. P values adjusted for multiple inference are below traditional thresholds for all outcomes at ($p < 0.000$).

To get a sense of the effect size on students who comply with the law, I produce back-of-the-envelope calculations, where I scale the average effects by the change in the share of students who remain in education until the age 18 (0.028). By this logic, remaining in school until age 18 as a result of the reform increases the probability of graduating by the age 20 by approximately 54 percentage points. For this group of "compliers", time spent as NEET decreases by 13.4 months, while time enrolled in education rises by 11 months, which is equivalent to an additional year of schooling. During this period, they complete coursework corresponding to more than two-fifths of the minimum requirements for a secondary degree (0.43). However, this additional time in education entails private costs: these students forgo approximately 3,060 euros in annual earnings. Another way to assess the economic significance of the reform is to consider its aggregate impact. For a cohort of 58,000 students, the estimated effect corresponds to a 1.5 percentage point increase in graduation rates, roughly 1,800 additional academic years spent in secondary school, and 1,800 fewer years spent as NEET during the ages when students are typically expected to attend secondary school.

In terms of heterogeneity across subgroups in Table 8, we find that the differences observed among men before and after the reform largely mirror those in the full sample, though their probability of graduating is about 0.4 percentage points higher. For students with a foreign background, the effects are more than twice as large in terms of enrollment, NEET status, and coursework completed, with no adverse impact on income. However, we cannot rule out the possibility that the reform had no effect on graduation rates. Similarly, for students from low-income backgrounds, the effects on coursework completion are roughly twice as large as in the full sample and more than half as large for enrollment, while the estimated impacts on graduation and earnings are comparable to the overall sample.

Despite larger effect sizes in terms of studies and time spent in education, the reform does not appear to reduce gaps in graduation rates between the full population and students from low-income households or with foreign backgrounds. For context, pre-reform outcomes for each group are summarized in Table A.8. Efficacy of the reform is likely attenuated by two factors. First, membership in these subgroups is negatively correlated with the effective duration of the extension. Since these students are more likely to have been retained during comprehensive school, their exposure to the reform is limited. Second, dropout analyses indicate that these individuals are more likely to exit education upon turning 18, further limiting the reform's potential impact. Despite this shorter period of exposure, the CSL extension induces a stronger behavioral response while binding, suggesting that the reform's intended effects could be strengthened if the extension were tied to grade level rather than chronological age.

5.2 Impact on At-risk Youth

Secondary schooling in Finland is characterised by a large set of students who complete secondary education regardless of the prevailing compulsory schooling regime, which is highlighted by the modest average effects. The target of the policy is to change the behaviour and educational outcomes of people who would not have attained a secondary degree in the absence of the reform. In this section, I identify the students with high dropout risk and contrast their outcomes to pre-reform at-risk students.

The pre-specified empirical strategy proceeds in two steps. First, I estimate a penalized logistic regression model with L1 norm (LASSO), which predicts not having a secondary diploma by 21. I assign a person to the risk group if their predicted probability of not having a secondary degree is in the highest 20% within the reform group. On average, students with a predicted risk above 0.23 are assigned to the at-risk group. The prediction model is trained using pupils who attended 9th grade for the first time in 2017-2018. Data from this graduation class is used solely to train the model. Covariates and interactions used to train the prediction model are listed in the bottom panel of Table 1, and the specification is selected using cross-validation with 10 folds. I then validate the prediction model using students who attended 9th grade in the academic year 2018-2019, since they have already turned 21. Since I utilize extensive background information on both parents and possible siblings, the sample used for this analysis is slightly smaller due to imperfect links in family data. Table 9 shows the background characteristics for the risk sample. Restricting the panel to pupils for whom I observe information on both parents and siblings increases the share of the Finnish-speaking population and decreases the share of single-parent households. Educational attainment of the parent is marginally higher in the risk analysis sample as compared to the full sample, while the balance between comparison groups remains essentially identical.

In the second step of the analysis, modify Equation 2 to isolate the effect on at-risk students:

$$\begin{aligned} Y_{it} = \alpha_t + \alpha_g + Reform_i + Risk_i + (Risk_i \times Post) + (Reform_i \times Risk_i) \\ +(Reform_i \times Post) + \gamma(Reform_i \times Post \times Risk_i) + \epsilon_{it} \end{aligned} \tag{3}$$

where *RISK* is an indicator for belonging to the top risk quintile. The identifying assumption is that in the absence of the reform, outcomes of the post-reform risk cohorts would match those of the previous cohorts.

Predicted dropout probabilities in Figure 10 show that the comparison groups have similar distributions of dropout risk. Only a small share of pupils have a high predicted risk (>0.50). The difficulty of identifying and supporting potential dropouts in Finland is well known, and this challenge was one of the arguments for adopting a universal compulsory schooling law rather than a more targeted policy. In Figure 11, I present a binscatter plot of the predicted and actual shares of individuals without a degree. On average, the LASSO model performs well out of sample, although it slightly underpredicts true dropout risk in the top decile. This also provides indirect evidence supporting the assumption that exposure to COVID-19 at slightly different stages of schooling had little effect on graduation rates. We also see meaningful differences in graduation probabilities between at-risk students and the rest of the student population. Figure 12 shows that students in the top risk quintile are more than twice as likely to drop out as those in the fourth quintile. Moreover, students in the highest-risk group are 20 times (38.8 percentage points) more likely to lack a degree by age 21 than the least risky students.

Table 10 reports the estimates for the risk model specified in Equation 3. The total effect on at-risk individuals is obtained by summing the estimates in the first and second rows of the table. The results indicate that, compared to earlier cohorts, both at-risk and non-risk students are more likely to graduate and complete additional studies. The effect on the graduation rate for at-risk youth is nearly twice as large as that for typical students (2.3 pp vs. 1.3 pp). However, after adjusting for multiple hypotheses testing, the estimated effect of the reform on the at-risk group is not statistically significant (adjusted p-value > 0.1).

Figure 13 presents separate estimates of the probability of graduating from academic and vocational tracks. Among the reform group students, the increases in graduation probability are nearly identical across the two tracks—approximately 0.65 pp for vocational and 0.6 pp for academic programs. For at-risk students, however, the additional improvements in graduation rates stem from students in the vocational track.

The share of coursework completed by at-risk students rises by 5.1 pp, while coursework completion for typical post-reform students remains largely unchanged. In a given year, at-risk students have spent 2.3 months less outside of education or employment since 9th grade, compared to their older counterparts. Surprisingly, members of the non-risk group appear to have spent more time as NEET overall. This difference likely reflects how students seek different types of education after the reform. Non-risk students who adjust their behavior in response to the policy are more likely to enter apprenticeship contracts, which accelerate degree completion. In Figure 13, I explore this further and confirm that any students who are admitted to the vocational track are more likely to sort into apprenticeships after the reform. There is a 1.6 pp increase in the share of post-reform students who graduate with any apprenticeship training after the reform.

Consequently, these students transition into the labor market earlier, and in this case, the transition to labor market happens during a period of economic downturn, leading to increased time spent as NEET following graduation. At-risk students, however, start investing more effort in education and stay enrolled longer than previous cohorts [15]. As a result, we observe a great reduction in social exclusion among at-risk students, and as expected, we confirm that the average results are driven by at-risk students.

6 Robustness

6.1 Differences-in-Discontinuities

Here, I assess the robustness of the results on educational outcomes using an alternative identification strategy based on cohort discontinuity. Specifically, I focus on students born in the months immediately before and after 2005, which largely determines whether their period of compulsory schooling was extended. Figure 1 depicts the share of students for whom the CSL was extended. As discussed above—and illustrated in Figure 5—the reform introduces a discontinuity in CSL exposure between students born just before and just after the 2005 cutoff.

Naturally, the season of birth may generate other discontinuities, particularly when the cutoff crosses a calendar year. To account for such seasonality, I estimate the corresponding discontinuity in previous years and difference it out from the discontinuity of interest. If these time-of-birth effects remain constant across cohorts, this differences-in-discontinuities approach identifies the local average treatment effect on the treated.

This strategy, however, has two important caveats. First, some students born in 2004 who were held back in comprehensive school received an effective one-year extension, implying that the estimated effect is likely downward-biased. Second, the confounding discontinuity may not be fully stable over time, although this assump-

[15] Alternatively, if we assume that the reform had no effect on non-risk students, the model would correspond to a triple-differences specification, in which, absent the CSL extension, degree attainment would have increased independently, accompanied by shorter time in education and greater time spent as NEET.

tion can be evaluated using pre-reform cohorts. Finally, although students born in adjacent months are nearly identical in age, those born in different years enter the labor market at different times. For this reason, I focus primarily on schooling outcomes, which are less sensitive to short-term labor market fluctuations. The estimation equation is as follows,

$$\begin{aligned} Y_{it} = \alpha + f(\textit{month of birth}) + Reform + f(\textit{month of birth}) \times Reform + \\ + Early + f(\textit{month of birth}) \times Early + \epsilon_i \end{aligned} \tag{4}$$

where *month of birth* $\in [-6, 5, ...5, 6]$ denotes the running variable, re-centered around the turn of the year. *Early* is an indicator for being born in the first half of the year and *Reform* captures the local effect of the CSL reform. Month of birth effects are controlled by a linear function $f(\textit{month of birth})$, and I allow for different slopes across the cutoff. Standard errors are heteroskedasticity-robust.

Figure 14 shows estimated discontinuities at the start of the year from 2003 to 2005. We see that discontinuities are similar in magnitude before the reform, lending credibility to the assumption that adjacent cohorts are similar in terms of unobservables. Panels A and B show that individuals born early in the year are less likely to be enrolled in education in the fall when they turn 16, yet more likely to have graduated by the age of 20. Both the probability of enrollment and the likelihood of completing a degree exhibit discontinuous increases between the students born in 2004 and 2005. Consistent with these patterns, Panel C shows a sharp rise in the share of completed studies. Changes in time spent enrolled are presented in Panel D. These changes are less pronounced but in line with the main analysis.

Differences in discontinuities are averaged by estimating Equation 4, and the results are presented in Table 11. Point estimates are similar to the main results in terms of magnitude for degree attainment and studies, with 2.1 and 1.9 pp increases, respectively. By contrast, the estimate for time spent enrolled is smaller and statistically insignificant. This likely reflects the fact that as students born early in the year are more likely to graduate on target time and also experience a shorter effective extension on average. Accordingly, the point estimates for months enrolled rise with the bandwidth in Figure 15, consistent with each additional month being associated with a longer period of CSL exposure. In contrast, estimates for degree attainment and coursework completion remain stable across bandwidths.

6.2 Additional Robustness Checks

I conclude this section by summarizing the robustness of the main results to additional exercises. First, in Table A.6 I show that the estimates are unaffected by the inclusion of region specific time fixed effects. Since responses to COVID-19 differed across regions, this finding helps alleviate the concerns that the educational outcomes are confounded by differences in schooling environment. This finding is consistent with previous research showing that the shift from in-person to online teaching during the COVID-19 pandemic had no effect on final exam performance in the academic track (Riudavets-Barcons & Uusitalo, 2024).

Next, I estimate a pre-specified dose–response model. The results, presented in Table A.7, confirm that educational outcomes improve with longer extensions of compulsory schooling. Although the model does not account for labor market fluctuations, multiplying the main coefficient by the average dosage yields effects on educational outcomes similar to those in the main estimates. However, the effect on enrollment is considerably smaller.

7 Conclusion

This study shows that extending compulsory schooling continues to raise educational attainment. Using variation from Finland's compulsory schooling reform in 2021, I find that dropout rates fell by 20% and graduation rates increased by 1.5 pp after the reform. On average, the time spent outside of education, work, or military service decreased by 0.38 months. Back-of-the-envelope calculations suggest that students who changed their behavior in response to the law enrolled 11 months longer in education and completed coursework corresponding to more than two-fifths of the minimum requirements for a secondary degree (0.43). Analysis of at-risk students reveals that even for students with the highest propensity of school leaving, the effect on graduation rates is limited. Nevertheless, the reform keeps these students engaged with the educational system for a considerably longer period than before.

Heterogeneity analysis shows that while the reform generates larger gains in enrollment and course completion, the gap in graduation rates between pre-determined subgroups and the general population remains stable. Survival analysis helps explain this pattern: students in these low-achieving groups tend to leave school at higher rates once legally permitted, and they experience shorter effective extensions under the reform, which further limits its impact.

More broadly, this study provides new evidence on how compulsory schooling laws extending to age 18 operate in practice. The findings highlight that even when education is mandatory and flexible study options exist, a subset of students remains reluctant to continue schooling. Finally, this analysis focuses on outcomes between ages 15 and 20, meaning that any potential effects on labor market outcomes or non-pecuniary returns to schooling are likely to appear only in the years ahead.

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Figures and Tables

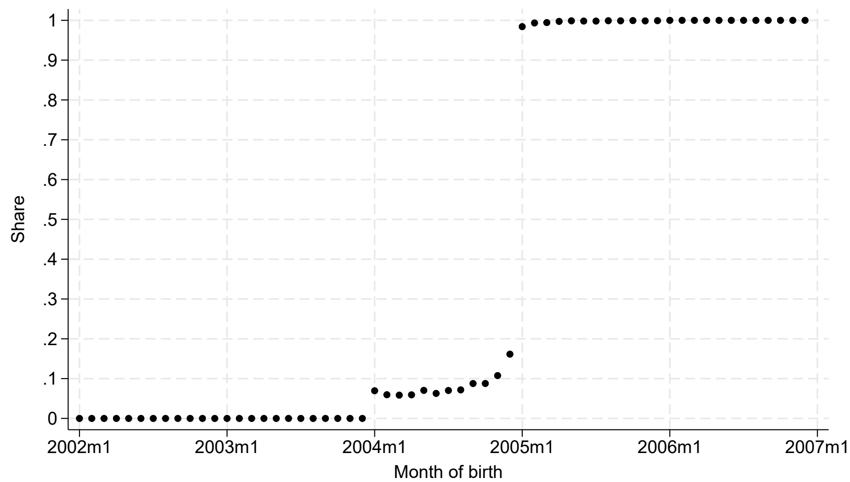

Figure 1: Exposure to the compulsory schooling extension by month of birth.

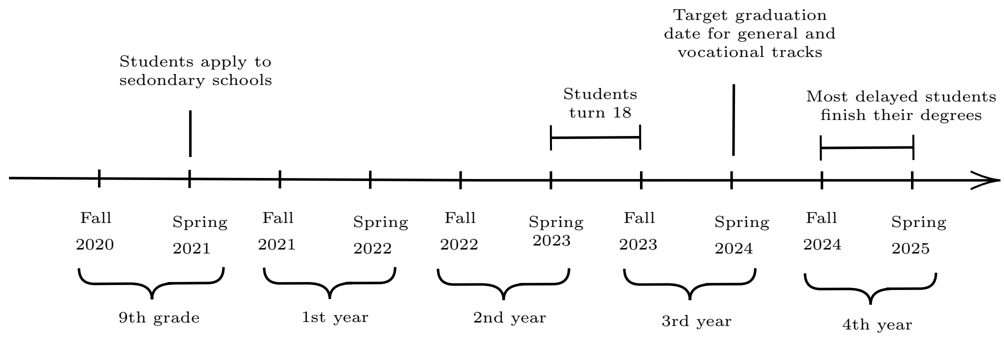

Figure 2: Timeline for the first birth cohort impacted by the reform (2005 born).

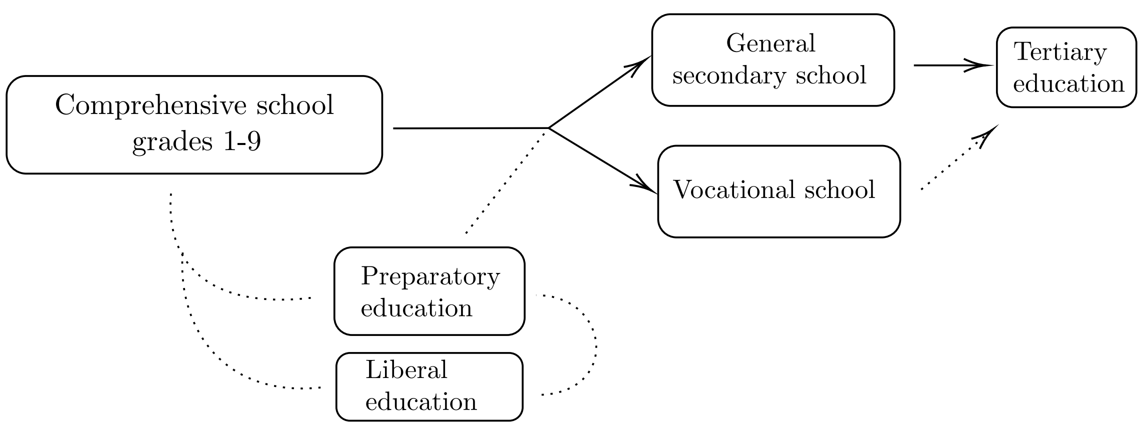

Figure 3: Illustration of the Finnish education system.

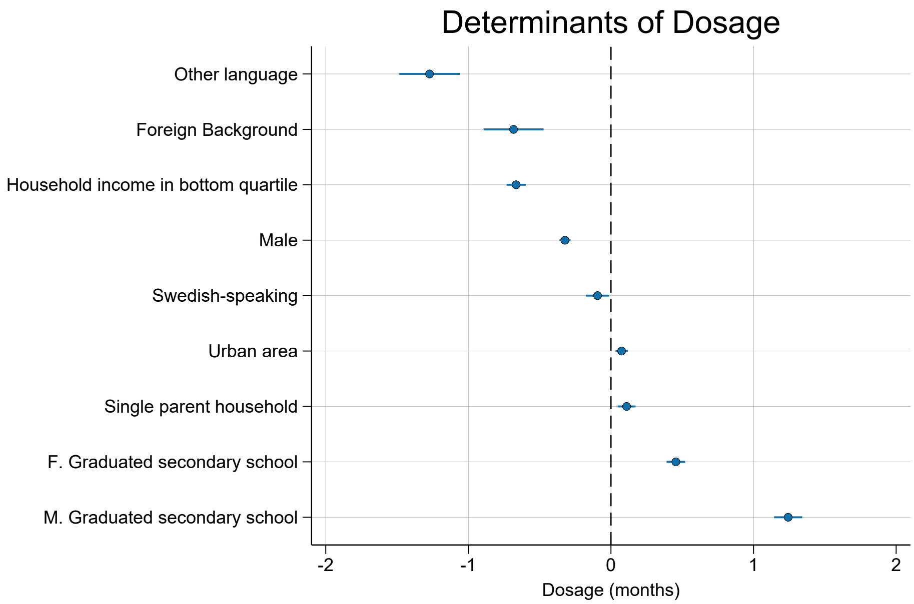

Figure 4: Coefficient estimates of selected variables from regressing the length of the extension (dosage) on background characteristics. Mean extension for the estimation sample is 19.6 months of instructional time.

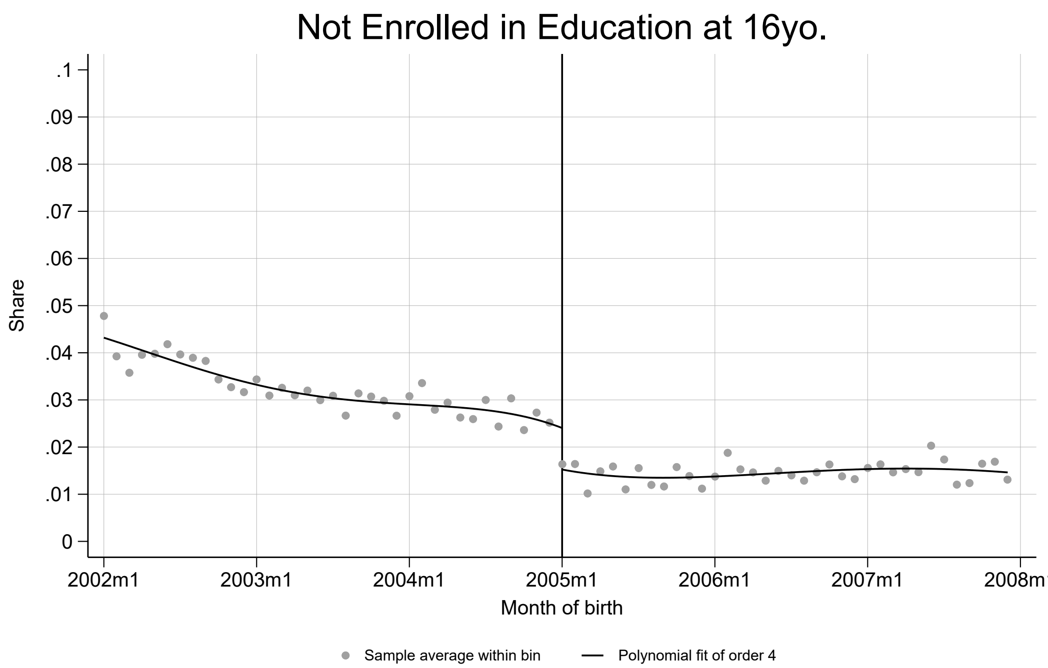

Figure 5: The figure shows the share of individuals who are not enrolled in any form of education at the end of the year in which they turn 16. Each dot represents students born in the same month.

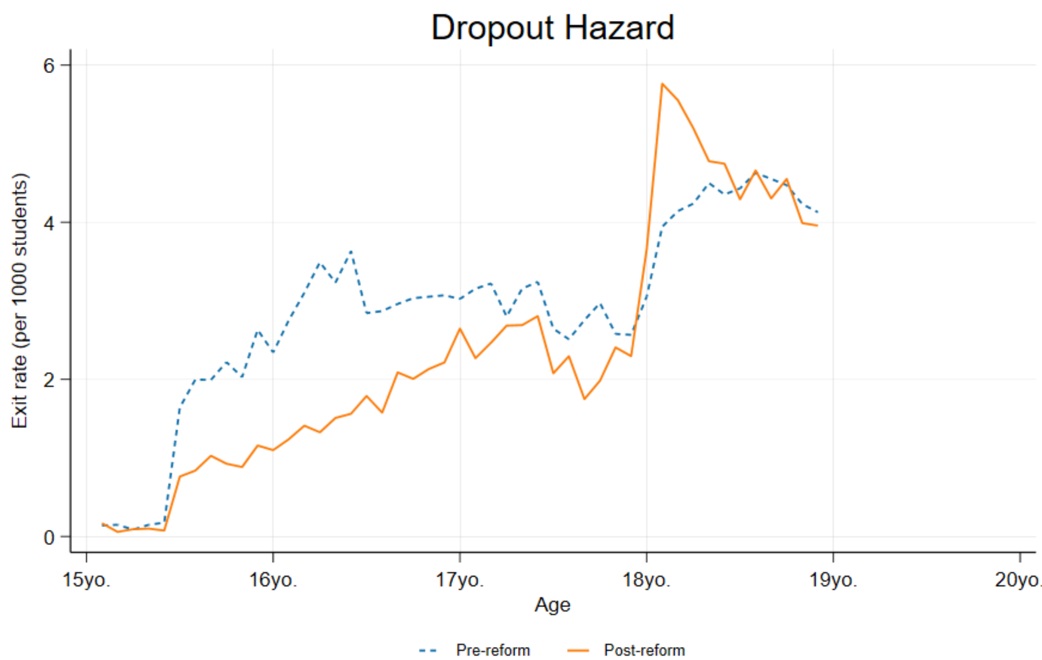

Figure 6: Exit rates per 1000 students. The dotted line depicts the exit rates at monthly intervals for pre-reform cohorts, and the solid line depicts them for post-reform cohorts.

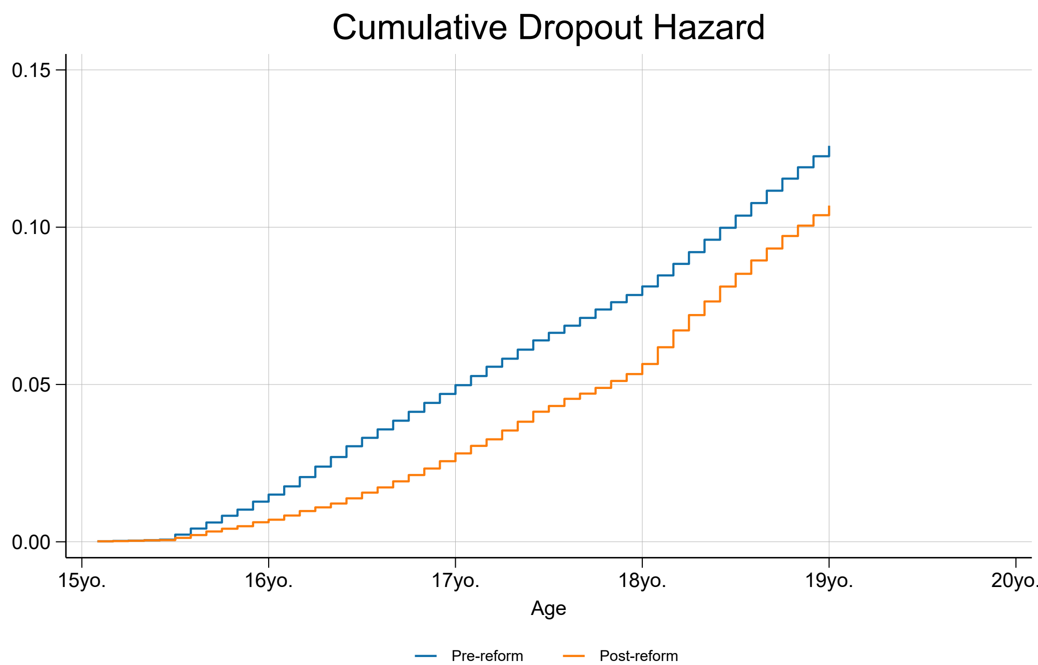

Figure 7: Cumulative hazard of dropping out of education. The curves show the proportion of each reform group who have left education for at least four months by a given age.

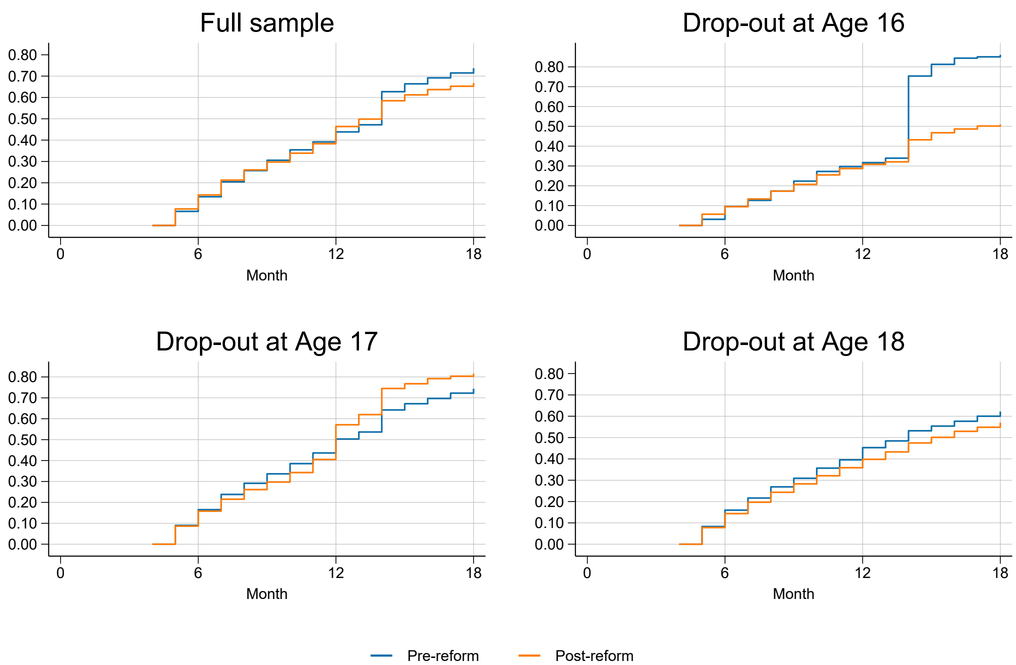

Figure 8: Cumulative hazard of re-enrolling in any education, after dropping out for at least four months. The curves show the proportion of each reform group who have left education and re-enrolled at least once. Pooled panel includes all individuals from age 15 to 19. Age-specific figures include students who drop out at a given age.

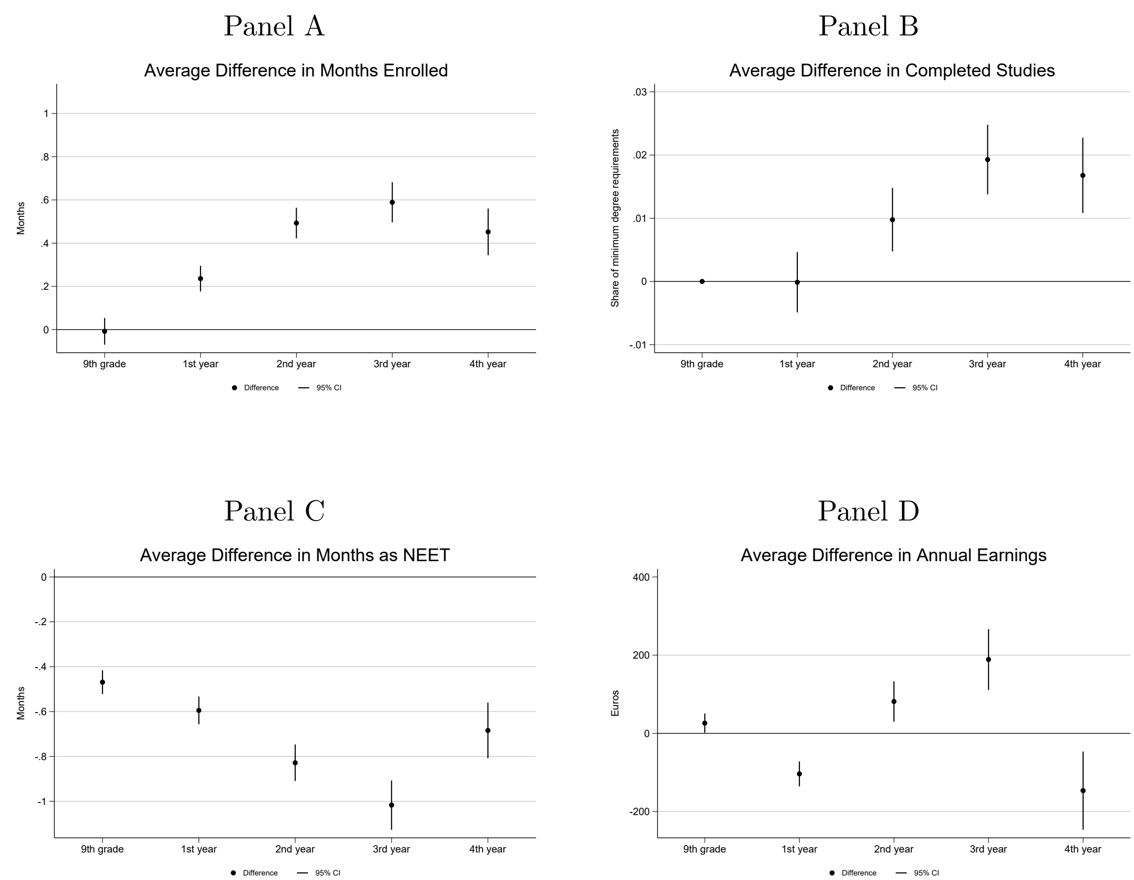

Figure 9: Dynamic estimates of differences between reform cohorts. Each point represents the estimated coefficient for an interaction between *Reform* and grade level, with standard errors clustered at the individual level. Panel B omits the first coefficient because comprehensive school pupils cannot, by definition, complete secondary school studies during that period. Time fixed effects are semester-level for Panels A and B, and year-level for Panels C and D. Definitions of all outcomes are provided in Table 1.

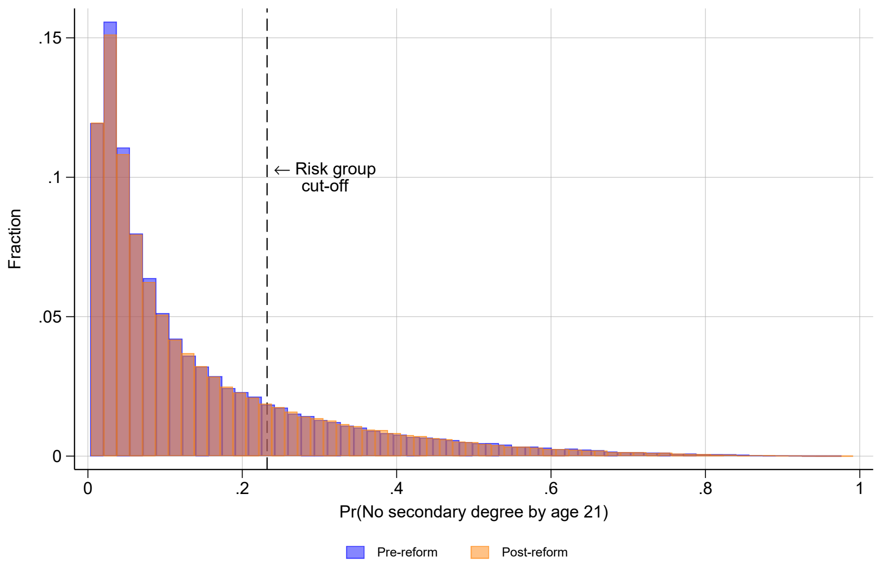

Figure 10: Predicted dropout risk by reform group.

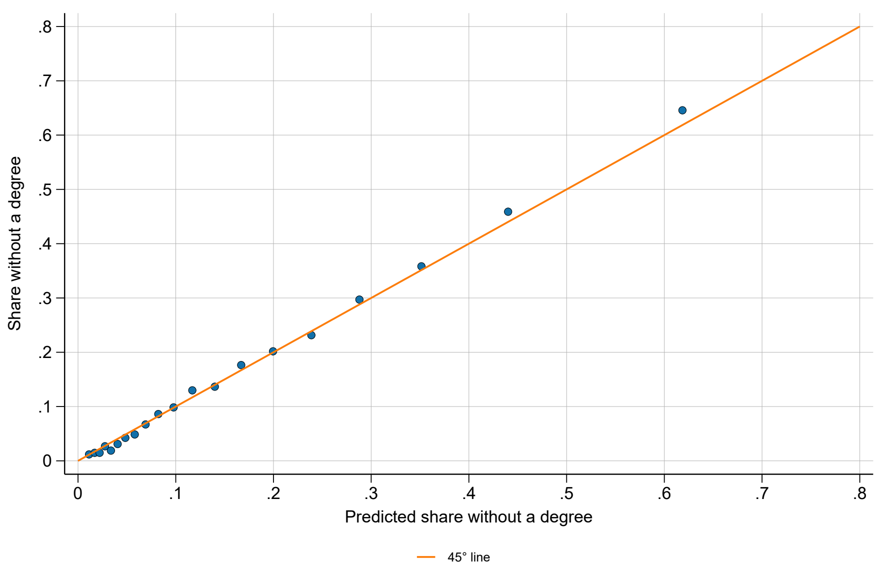

Figure 11: Predicted and realized graduation rates at age 21 for the 2019 comprehensive school graduation class.

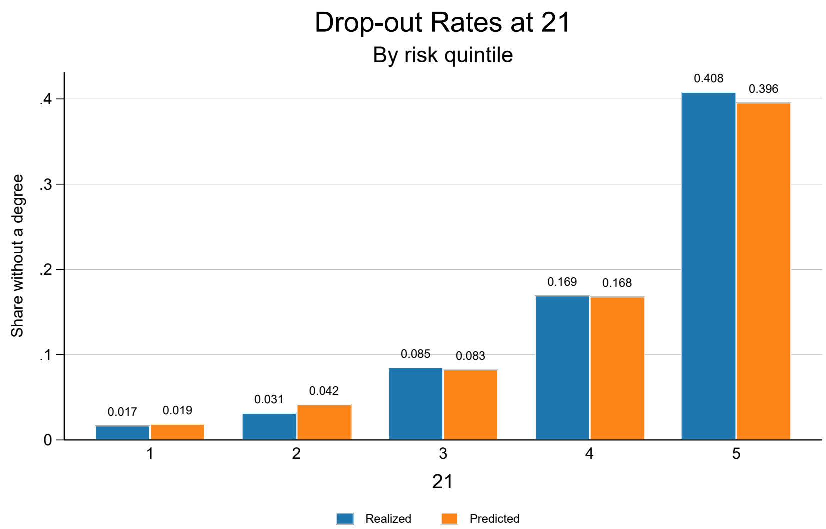

Figure 12: Predicted and realized shares of students without a degree, at age 21, by risk quintile, for the 2019 comprehensive school graduation class. Shares are calculated by subtracting the probability of graduating from one.

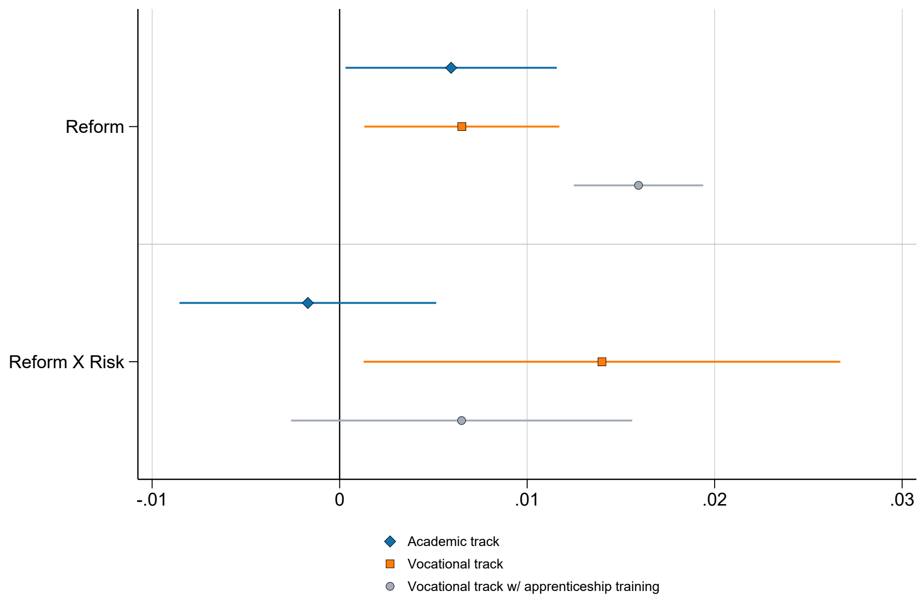

Figure 13: Changes in graduation probabilities by track, after the reform. Blue diamonds show the changes in the probability of graduating from the academic track in the spring when a person turns 20. Orange squares show the changes in the probability of graduating from the vocational track. Gray dots describe the combined probability of 1) graduating from the vocational track and 2) having spent any time under an apprenticeship contract during vocational training. Standard errors are clustered at the individual level.

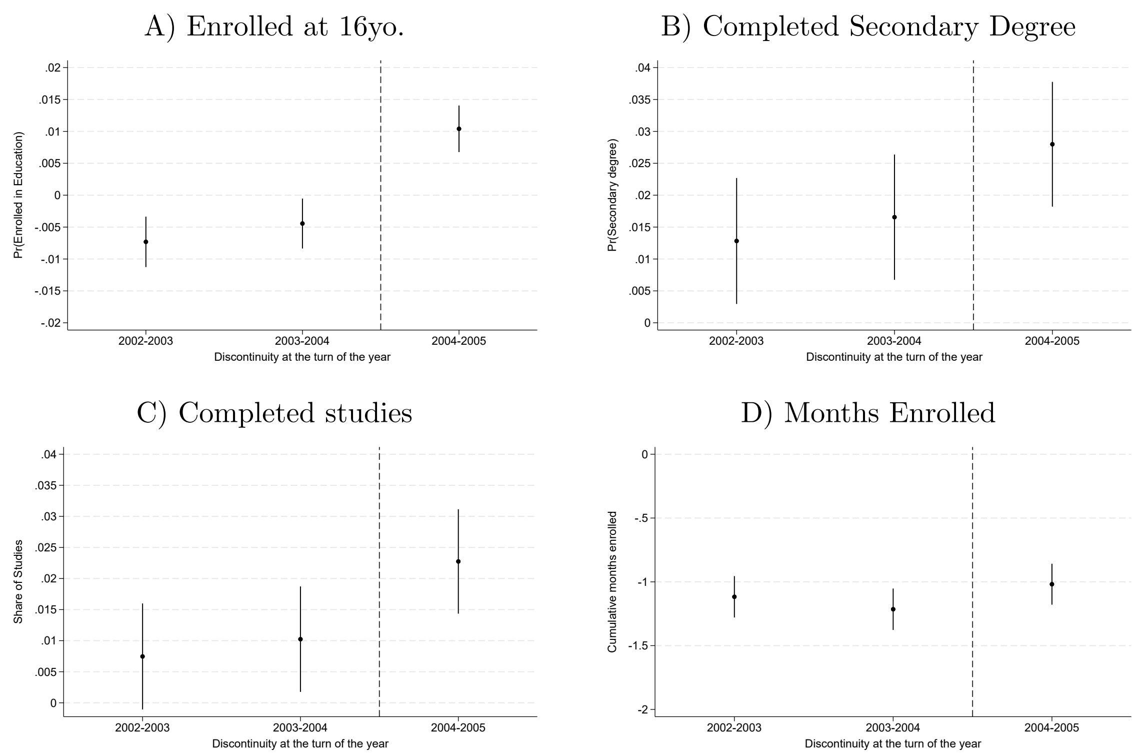

Figure 14: Estimated discontinuities at the turn of the year, controlling for a linear function of the running variable. Standard errors are heteroskedasticity-robust.

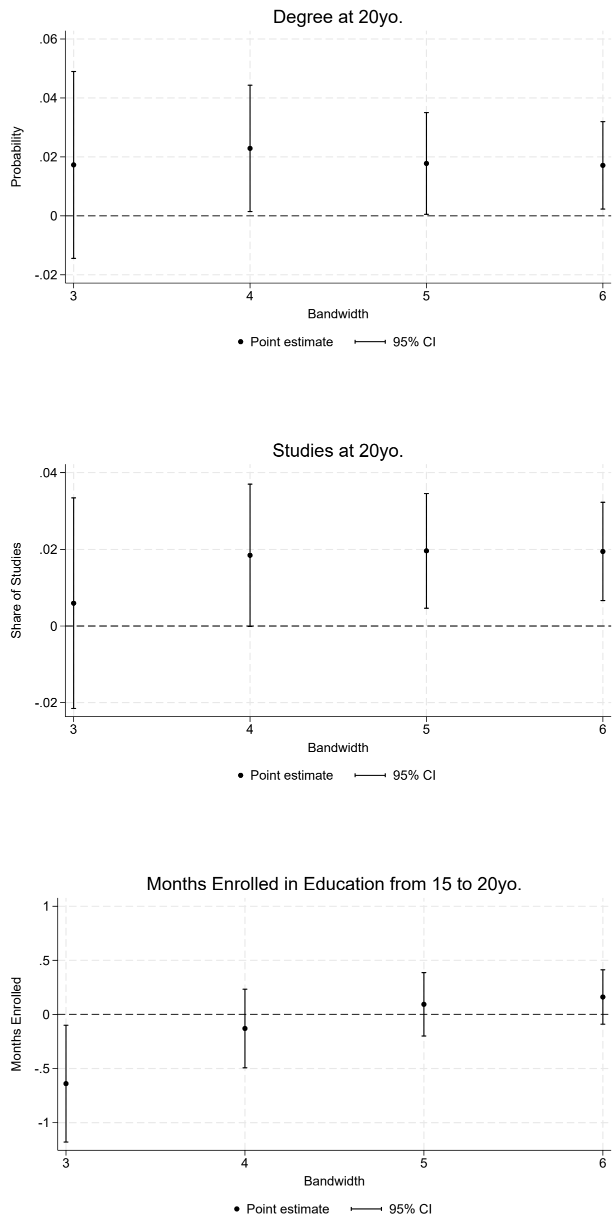

Figure 15: Differences-in-discontinuities estimates with various bandwidths. Standard errors are heteroskedasticity-robust.

Table 1: Data Descriptions for Selected Variables

Schooling Outcomes	
Degree completion	Indicator of completing a secondary degree by the year of turning 20
NEET status	Cumulative amount of months in NEET from the year a pupil turns 15
Earnings	Annual earned income in real euros
Completed education	Cumulative share of mandatory studies completed in a secondary track leading to a degree
Time spent in any education	Cumulative number of months enrolled in education
Subgroups	
Household income rank	Indicator for the household total earned income being in the bottom quartile in the year when the student turns 15
Gender	Binary indicator for males
Foreign background	Pupils' parents born abroad
LASSO covariates	
Student characteristics	Gender, foreign background, language, and interactions between all possible pairs of the variables
Parents' educational attainment	Father's highest degree, Mother's highest degree
Older sibling's educational attainment	Indicators for an older siblings' highest degree and being the eldest sibling
Parents' NEET status	Number of months in NEET separately for each parent in the year a student turns 15
Household variables	Single parent household, income transfers received by the household, and either parent dead in the year a student turns 15.
Comprehensive school (9th grade)	GPA , months not attended school
Other (at age 15)	Urban-rural classification for place of residence.

Notes: Outcomes of interest for confirmatory analysis, following the pre-analysis plan.

Table 2: Background characteristics

	(1) Pre-reform	(2) Post-reform	(3) Difference	(4) Observations
Student				
Finnish-speaking	0.886	0.877	0.010	236,936
	(0.317)	(0.329)		
Swedish-speaking	0.050	0.051	-0.001	236,936
	(0.217)	(0.219)		
Other language	0.064	0.073	-0.009	236,936
	(0.244)	(0.259)		
GPA (4-10)	7.979	8.001	-0.023	233,713
	(0.993)	(0.993)		
Graduated CS after 16	0.082	0.076	0.006	236,936
	(0.275)	(0.265)		
Single parent household	0.240	0.245	-0.005	236,936
	(0.427)	(0.430)		
Urban area	0.691	0.706	-0.016	236,936
	(0.462)	(0.455)		
Mother				
Earnings	27,727	28,890	-1,173	231,469
	(21,300)	(22,077)		
M. Graduated secondary school	0.896	0.900	-0.004	236,936
	(0.305)	(0.300)		
Months as NEET	1.571	1.200	0.371	230,149
	(3.631)	(3.311)		
Father				
Earnings	37,792	38,797	-1,005	226,965
	(30,308)	(30,763)		
F. Graduated secondary school	0.828	0.832	-0.005	236,936
	(0.378)	(0.373)		
Months as NEET	1.140	1.038	0.102	221,551
	(3.176)	(3.096)		
Missing variables	0.082	0.082	0.000	236,936
	(0.275)	(0.274)		
Observations	116,807	120,129	236,936	236,936

Notes: Student background characteristics at age 15. Incomes in 2010 euros. GPA is the grade point average for all subjects in 9th grade. NEET is defined as not in education, training, military service or employment.

Table 3: Main type of activity after graduating 9th grade

	(1) Pre-reform	(2) Post-reform
Vocational	0.369	0.383
Academic	0.521	0.514
Preparatory	0.039	0.056
NEET	0.039	0.023
Other	0.032	0.023
N	116,807	120,129

Notes: Main type of activity is measured in the following December, after finishing 9th grade. NEET is defined as not in education, training, military service or employment.

Table 4: Main type of activity after leaving school

	4 months		12 months	
	(1)	(2)	(3)	(4)
	Pre-reform	Post-reform	Pre-reform	Post-reform
Panel A.				
Vocational	0.037	0.038	0.144	0.110
Academic	0.010	0.011	0.045	0.086
Preparatory	0.007	0.020	0.015	0.030
Comprehensive	0.001	0.002	0.017	0.023
NEET	0.637	0.725	0.457	0.529
Non-degree	0.013	0.017	0.047	0.043
Other	0.096	0.055	0.105	0.091
Working	0.200	0.135	0.188	0.112
Panel B.				
Re-enrolled at least once	0.068	0.087	0.407	0.434
N	22,479	13,938	20,086	11,020

Notes: Main type of activity in a given month, after leaving education, before graduating secondary school. Person is defined as working if they received any salary in a given month. NEET is defined as not in education, training, military service or employment. Ever re-enrolled denotes the share of people who have enrolled at least once after leaving education.

Table 5: Cox proportional hazards

	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.806		0.803		0.811	
	(0.010)		(0.010)		(0.010)	
Reform ×15		0.450		0.449		0.454
		(0.021)		(0.021)		(0.021)
Reform ×16		0.549		0.548		0.554
		(0.015)		(0.015)		(0.015)
Reform ×17		0.848		0.846		0.855
		(0.021)		(0.021)		(0.021)
Reform ×18		1.107		1.101		1.110
		(0.022)		(0.022)		(0.022)
Household income in bottom quartile			2.124	2.123	1.648	1.647
			(0.027)	(0.027)	(0.028)	(0.028)
Male			1.156	1.156	1.158	1.158
			(0.014)	(0.014)	(0.014)	(0.014)
Foreign Background			1.080	1.078	0.783	0.782
			(0.023)	(0.023)	(0.043)	(0.043)
Single parent household					1.177	1.177
					(0.019)	(0.019)
F. Graduated secondary school					0.645	0.645
					(0.010)	(0.010)
M. Graduated secondary school					0.563	0.563
					(0.010)	(0.010)
Swedish-speaking					0.746	0.745
					(0.025)	(0.025)
Other language					0.936	0.935
					(0.051)	(0.051)
Obs	10,740,732	10,740,732	10,740,732	10,740,732	10,740,732	10,740,732
Reform PH test	0.000		0.000		0.000	
Global PH test	0.000	0.107	0.000	0.000	0.000	0.000

Notes: Standard errors (shown in parentheses) are clustered at the individual level. Tests for proportional hazards evaluate whether the scaled Schoenfeld residuals for each covariate have a constant slope over time.

Table 6: Subgroup analysis: Cox proportional hazards

	(1) Men	(2) Men	(3) Foreign	(4) Foreign	(5) Low income	(6) Low Income
Reform	0.845		0.998		0.881	
	(0.014)		(0.039)		(0.016)	
Reform ×15		0.561		0.771		0.531
		(0.036)		(0.104)		(0.042)
Reform ×16		0.582		0.795		0.578
		(0.021)		(0.069)		(0.024)
Reform ×17		0.834		0.860		0.899
		(0.029)		(0.067)		(0.034)
Reform ×18		1.110		1.340		1.190
		(0.028)		(0.086)		(0.035)
Obs	5,512,619	5,512,619	729,886	729,886	2,684,572	2,684,572
Reform PH test	0.000		0.000		0.000	
Global PH test	0.000	0.345	0.000	0.886	0.000	0.372

Notes: Standard errors (shown in parentheses) are clustered at the individual level. Tests for proportional hazards evaluate whether the scaled Schoenfeld residuals for each covariate have a constant slope over time.

Table 7: Estimated Differences on Educational and Labor Market Outcomes

	(1) Degree at 20yo.	(2) NEET	(3) Months Enrolled	(4) Studies	(5) Annual earnings
Reform $\times Post$	0.015	-0.376	0.307	0.012	-85.765
	(0.002)	(0.030)	(0.019)	(0.002)	(20.255)
Obs	175,998	1,297,376	2,595,455	2,122,289	1,297,350
p-value	0.000	0.000	0.000	0.000	0.000
q-value	0.000	0.000	0.000	0.000	0.000
Semester FE			✓	✓	
Year FE		✓			✓
Class FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001), taking into account each p-value within the table.

Table 8: Subgroup Analysis on Educational and Labor Market Outcomes

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Panel A: Men					
Reform $\times Post$	0.019	-0.354	0.299	0.009	-63.296
	(0.003)	(0.044)	(0.027)	(0.003)	(29.440)
Obs	90,325	665,763	1,331,759	1,088,870	665,750
p-value	0.000	0.000	0.000	0.011	0.032
q-value	0.000	0.000	0.000	0.051	0.121
Panel B: Foreign background					
Reform $\times Post$	0.015	-1.011	0.702	0.029	-11.078
	(0.010)	(0.190)	(0.119)	(0.011)	(76.941)
Obs	11,565	87,071	174,223	141,969	87,062
p-value	0.118	0.000	0.000	0.008	0.886
q-value	0.421	0.000	0.000	0.041	1.000
Panel C: Low income households					
Reform $\times Post$	0.014	-0.598	0.537	0.024	-99.380
	(0.005)	(0.079)	(0.049)	(0.006)	(40.884)
Obs	43,165	318,239	636,620	520,523	318,230
p-value	0.004	0.000	0.000	0.000	0.015
q-value	0.021	0.000	0.000	0.000	0.063
Semester FE			✓	✓	
Year FE		✓			✓
Grade FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Estimation results for pre-specified subgroups. Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001) and taking into account each p-value within the entire table.

Table 9: Background Characteristics for Risk Sample

	(1) Pre-reform	(2) Post-reform	(3) Difference	(4) Observations
Student				
Finnish-speaking	0.901	0.891	0.010	216,362
	(0.299)	(0.312)		
Swedish-speaking	0.051	0.052	-0.001	216,362
	(0.220)	(0.223)		
Other language	0.048	0.057	-0.009	216,362
	(0.214)	(0.231)		
GPA (4-10)	8.009	8.032	-0.023	216,362
	(0.987)	(0.987)		
Graduated CS after 16	0.062	0.056	0.005	216,362
	(0.240)	(0.230)		
Single parent household	0.223	0.228	-0.005	216,362
	(0.416)	(0.420)		
Urban area	0.697	0.714	-0.017	216,362
	(0.460)	(0.452)		
Mother				
Earnings	28,334	29,549	-1,216	216,362
	(21,217)	(21,994)		
M. Graduated secondary school	0.925	0.930	-0.005	216,362
	(0.263)	(0.255)		
Months as NEET	1.486	1.121	0.364	216,362
	(3.531)	(3.204)		
Father				
Earnings	38,908	39,888	-980.167	216,362
	(30,050)	(30,519)		
F. Graduated secondary school	0.862	0.869	-0.006	216,362
	(0.344)	(0.338)		
Months as NEET	1.119	1.015	0.104	216,362
	(3.145)	(3.061)		
Missing variables	0.000	0.000	0.000	216,362
	(0.000)	(0.000)		
N	106,629	109,733	216,362	216,362

Notes: Student background characteristics at age 15 for the risk sample. The risk sample consists of individuals with complete records for all LASSO covariates. Incomes in 2010 euros. GPA is the grade point average for all subjects in 9th grade. NEET is defined as not in education, training, military service or employment.

Table 10: Risk Model Estimates on Educational and Labor Market Outcomes

	(1) Degree at 20yo.	(2) NEET	(3) Months Enrolled	(4) Studies	(5) Annual earnings
Reform $\times$*Post*	0.013	0.305	-0.220	0.000	-78.271
	(0.002)	(0.023)	(0.017)	(0.002)	(22.774)
Reform $\times$*Post* $\times$ *Risk*	0.012	-2.635	2.111	0.051	-140.364
	(0.006)	(0.059)	(0.056)	(0.003)	(46.934)
Obs	160,695	1,184,996	2,370,463	1,938,210	1,184,985
p-value	0.048	0.000	0.000	0.000	0.003
q-value	0.110	0.000	0.000	0.000	0.008
Semester FE			✓	✓	
Year FE		✓			✓
Grade FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Model estimates include group indicators and interactions shown in Equation 3, except column (1), where *Reform* and *Risk* and their interaction are unidentifiable by construction. Analogously, *Reform* and *Reform*$\times$ *Risk* is omitted from Column (4), since the post-reform group cannot have completed secondary studies before the reform took place. Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate p-values and false discovery rate q-values following Benjamini and Yekutieli (2001) for *Reform* $\times$ *Post* $\times$ *Risk* interaction term.

Table 11: Diff-in-Disc

	(1) Degree	(2) Enrolled	(3) Studies
Reform	0.021	0.120	0.019
	(0.007)	(0.113)	(0.006)
N	174,545	174,545	174,545

Notes: Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Estimation results for 4, where month of birth effects are controlled by a linear function $f(\textit{month of birth})$ and standard errors are robust to heteroskedasticity.

A Appendix

A.1 Held Back Pupils and Omitted Variable Bias

The main specification in the pre-analysis plan (PAP) was the following,

$$Y_{it} = \alpha_t + \alpha_a + Treated_i + \beta(Treated_i \times Post) + \epsilon_{it} \tag{5}$$

where α_t is a common time-fixed effect, α_a captures age-fixed effects and ϵ_{it} is the residual. The plan was based on the assumption that after conditioning on age and time, if comparison groups were otherwise similar, we could recover the effect the reform. Due to institutional details, this is not the case.

The comprehensive school graduating class of 2021 includes two main groups of students. Most were born in 2005 and completed 9th grade on time. A smaller group was born in 2004 and had been held back at some point during their schooling. Because of this, when we compare students conditional on age and time period, being a member of the post-reform group is correlated with grade retention.

This biases the estimates, since much of the treatment effect is identified from differences between students born in 2004, who were held back, and those who were not. Simply adding a control for grade retention does not fully fix this issue, since being held back can result from many unobserved factors, such as language proficiency, motivation, developmental timing, or random health shocks that also affect later outcomes. Finally, I cannot include both age and grade fixed effects due to multicollinearity.

I solve this problem by replacing age fixed effects with a grade level, defined as academic years relative to graduation from 9th grade. I argue that this choice achieves the same goal as controlling for age, since we want to keep fixed any unobservable factors that students face as they become older. Grade level is arguably even more fit to this purpose in my setting, since it is more precise in capturing where a student is in their educational path.

A.2 Populated PAP

Here I report the results following the pre-analysis plan (https://osf.io/qyfrz), without modifications.

Table A.1: Estimation Results for Equation 1 of The PAP

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Reform $\times Post$	0.015	1.101	-0.301	-0.193	-848.629
	(0.002)	(0.032)	(0.036)	(0.002)	(20.308)
Obs	175,998	1,311,867	2,798,330	2,798,330	1,311,841
p-value	0.000	0.000	0.000	0.000	0.000
q-value	0.000	0.000	0.000	0.000	0.000
Semester FE			✓	✓	
Year FE		✓			✓
Age FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001).

Table A.2: Subgroup Estimation Results for Equation 1 of The PAP

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Panel A: Men					
Reform $\times Post$	0.019	1.247	-0.164	-0.206	-828.134
	(0.003)	(0.045)	(0.050)	(0.003)	(29.130)
Obs	90,325	674,679	1,438,455	1,438,455	674,666
p-value	0.000	0.000	0.001	0.000	0.000
q-value	0.000	0.000	0.004	0.000	0.000
Panel B: Foreign background					
Reform $\times Post$	0.015	0.741	0.524	-0.291	-1024.070
	(0.010)	(0.144)	(0.160)	(0.007)	(74.317)
Obs	11,565	91,307	193,905	193,905	91,298
p-value	0.118	0.000	0.001	0.000	0.000
q-value	0.393	0.000	0.004	0.000	0.000
Panel C: Low income households					
Reform $\times Post$	0.014	1.443	-0.261	-0.242	-1243.409
	(0.005)	(0.074)	(0.080)	(0.004)	(40.142)
Obs	43,165	325,235	692,177	692,177	325,226
p-value	0.004	0.000	0.001	0.000	0.000
q-value	0.014	0.000	0.004	0.000	0.000
Semester FE			✓	✓	
Year FE		✓			✓
Age FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Estimation results for pre-specified subgroups. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001) and taking into account each p-value within the entire table.

Table A.3: Estimation Results for Equation 2 of the PAP

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Reform $\times Post$	0.013	0.756	-0.037	-0.109	-632.919
	(0.002)	(0.026)	(0.023)	(0.002)	(22.574)
Reform $\times Post \times Risk$	0.012	-2.442	1.809	-0.003	-119.207
	(0.006)	(0.058)	(0.048)	(0.002)	(47.394)
Obs	160,695	1,194,762	2,550,039	2,550,039	1,194,751
p-value	0.048	0.000	0.000	0.132	0.012
q-value	0.137	0.000	0.000	0.301	0.045
Semester FE			✓	✓	
Year FE		✓			✓
Age FE		✓	✓	✓	✓
Measurement unit	Probability	Month	Month	Share of a degree	Euro

Notes: Estimation results for pre-specified risk-group analysis. Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001) and taking into account each p-value within the entire table.

Table A.4: Estimation Results for Equation 3 of the PAP

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Dosage	0.00059	-0.00029	0.00733	0.00063	-30.66265
	(0.00009)	(0.00165)	(0.00159)	(0.00008)	(1.65216)
Obs	180,627	180,627	180,627	180,627	180,627
p-value	0.000	0.859	0.000	0.000	0.000
q-value	0.000	1.000	0.000	0.000	0.000

Notes: Estimation results for pre-specified dose-response analysis. Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors clustered at the individual level. I calculate the false discovery rate q-values following Benjamini and Yekutieli (2001) and taking into account each p-value within the entire table.

A.3 Robustness

Table A.5: Alternative drop-out definitions

	(1) 4 months (main)	(2) 4 (temp)	(3) No immortality	(4) 6 months
Reform	0.806	0.746	0.824	0.783
	(0.010)	(0.011)	(0.005)	(0.010)
N	10,740,732	10,740,732	10,740,732	10,740,732
Reform PH test	0.000	0.000	0.000	0.000
Global PH test	0.000	0.000	0.000	0.000

Notes: First column reproduces the main estimates for Equation 1, where exit is defined as leaving school for at least four consecutive months, from age 15 until 19th birthday. In column two, I record students who have made an agreement to leave school temporarily as enrolled. Column three reports results without granting immortal time for secondary school graduates, and column four reports results for students who spend at least six months outside of education. Standard errors clustered at the individual level. Tests for proportional hazards evaluate whether the scaled Schoenfeld residuals for each covariate have a constant slope over time.

Table A.6: Estimation Results with Regional Trends

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Reform × Post	0.017	-0.377	0.310	0.013	-87.658
	(0.002)	(0.030)	(0.019)	(0.002)	(20.161)
Obs	175,998	1,297,375	2,595,453	2,122,287	1,297,349
p-value	0.000	0.000	0.000	0.000	0.000
Semester × Region FE			✓	✓	
Year × Region FE		✓			✓
Grade FE		✓	✓	✓	✓
Region FE	✓				
Measurement unit	Probability	Month	Month	Share of a degree	Euro

This table presents results from Equation 2, allowing for flexible regional differences. Columns 2–5 include flexible time trends that vary across the 19 regions of mainland Finland. Column 1 reports estimates from a first-difference model for degree attainment at age 20, including region fixed effects. Degree attainment in Column (1) is measured at the end of the spring semester in the year a student turns 20, which is the most recent point at which data are available for the first reform cohort. Standard errors are clustered at the individual level.

A.4 Dose-response Analysis

Table A.7: Estimates of Dose-response Model Estimates at Age 20

	(1) Degree at 20yo.	(2) Months as NEET	(3) Months Enrolled	(4) Studies completed	(5) Annual earnings
Dosage	0.00059	-0.00029	0.00733	0.00063	-30.66265
	(0.00009)	(0.00165)	(0.00159)	(0.00008)	(1.65216)
Obs	180,627	180,627	180,627	180,627	180,627
p-value	0.000	0.859	0.000	0.000	0.000
q-value	0.000	1.000	0.000	0.000	0.000
$Dosage \times \delta$	0.012	-0.006	0.144	0.012	-602.085

Notes: Results for regressing the main outcomes on lenght of the extension, controlling for month of birth and whether a student was held back during comprehensive school. The bottom row displays results for multiplying the dosage coefficient with average dosage. Standard errors are robust to heteroskedasticity.

A.5 Additional Summary Statistics

Table A.8: Pre-reform Outcomes Four Years after 9th Grade

	(1) Full sample	(2) At-risk	(3) Men	(4) Foreign Background	(5) Low Income
Degree	0.791	0.511	0.767	0.616	0.659
	(0.407)	(0.500)	(0.423)	(0.486)	(0.474)
Studies	0.900	0.644	0.869	0.773	0.787
	(0.367)	(0.446)	(0.367)	(0.430)	(0.424)
Months Enrolled	42.398	41.384	41.542	44.887	42.159
	(7.229)	(8.837)	(7.296)	(10.166)	(8.724)
Months as NEET	3.988	6.906	4.290	6.580	6.165
	(8.601)	(10.582)	(9.006)	(12.181)	(11.117)
Degree w/ apprenticeship	0.097	0.146	0.134	0.049	0.092
	(0.297)	(0.360)	(0.341)	(0.216)	(0.290)
Observations	116,807	21,325	59,976	7,403	28,652

Notes: Baseline outcomes measured four years after a person finishes 9th grade. NEET is defined as not in education, training, military service, or employment. Degree w/ apprenticeship refers to students who have graduated from secondary school and spent any time in an apprenticeship during their studies.

Table A.9: Background Characteristics for Risk Groups

	(1) Non-risk group	(2) Risk group
Student		
Finnish-speaking	0.906	0.855
	(0.292)	(0.352)
Swedish-speaking	0.058	0.026
	(0.234)	(0.159)
Other language	0.036	0.119
	(0.186)	(0.324)
GPA (4-10)	8.356	6.678
	(0.772)	(0.486)
Graduated CS after 16	0.032	0.166
	(0.176)	(0.372)
Single parent household	0.179	0.411
	(0.383)	(0.492)
Urban area	0.706	0.702
	(0.456)	(0.458)
Mother		
Earnings	31,493.560	18,776.414
	(21,960.882)	(16,712.979)
M. Graduated secondary school	0.965	0.778
	(0.183)	(0.416)
Months as NEET	0.947	2.715
	(2.874)	(4.624)
Father		
Earnings	42,892.468	25,455.453
	(30,870.563)	(23,088.252)
F. Graduated secondary school	0.907	0.698
	(0.290)	(0.459)
Months as NEET	0.731	2.410
	(2.559)	(4.438)
Missing variables	0.000	0.000
	(0.000)	(0.000)
N	173,091	43,271

Notes: Student background characteristics at age 15 for the risk sample. The risk sample consists of individuals with complete records for all LASSO covariates. Incomes in 2010 euros. GPA is the grade point average for all subjects in 9th grade. NEET is defined as not in education, training, military service or employment.

A.6 Additional Figures

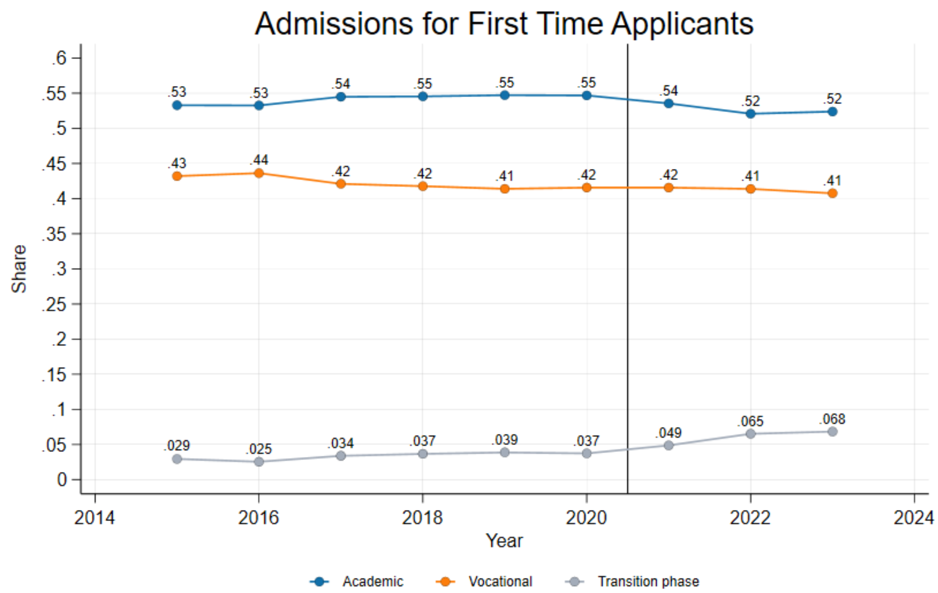

Figure A.1: Trends in admissions to secondary education for first-time applicants.

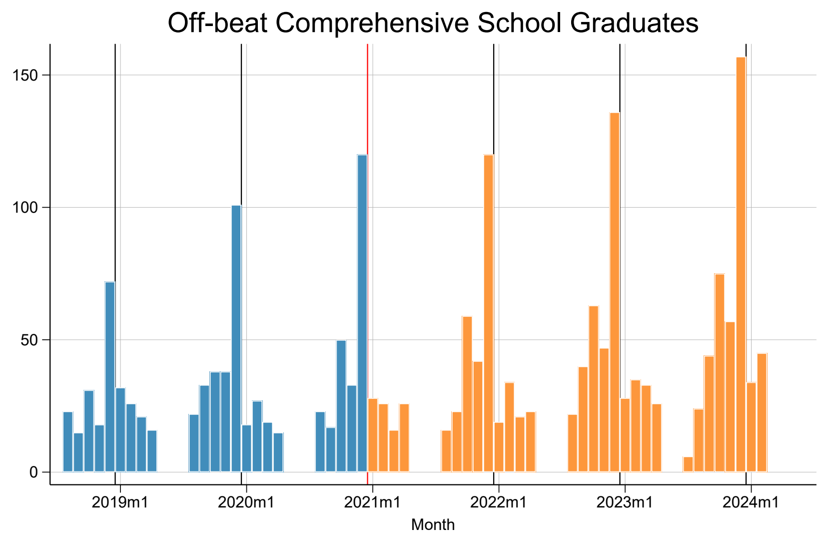

Figure A.2: Number of pupils who graduate from comprehensive school after the end of the spring semester at age 16.

B Misc

B.1 Major choices made during data cleaning

1. Vacation time, such as summer or winter vacation, is recorded as attendance if the student is enrolled in education before and after the vacation.

2. Reform status is inferred from the date when a student graduates 9th grade. For a handful of students for whom this information is missing, I check whether they have enrolled in secondary education, and deduce their graduation date, since secondary enrollment requires graduating comprehensive school. I exclude 61 people whose extension status I'm unable to determine.

3. Registering credits for the same exact course with the same confirmation date in a different school does not count as a completed course, since it is most likely a credit transfer.

4. If a student has credits in multiple tracks, for example, after switching from the academic track (22% complete) to vocational school (12% complete), the maximum of the two is chosen as the share of completed studies.

5. Employment is determined by the months for which an individual receives earnings.

6. In the case of overlapping enrolment periods, I prioritize studies towards a secondary degree over non-degree training.

7. Spells marked as temporary suspension of enrollment are interpreted as "not in education" in the main specification.

8. In the survival analysis student is considered enrolled in education if they spend at least half of a given month participating in any form of education, including combined enrollments across programs.