The Unintended Consequences of Shortening High School: Evidence from Ontario*

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Abstract

Can schooling be shortened without harming students? We study a policy in Ontario, Canada, which cut secondary schooling from five years to four while redesigning the curriculum to preserve instructional intensity. Using difference-in-differences supported by birth-date regression discontinuity designs, we find that affected students graduated high school and enrolled in postsecondary education earlier, but were less likely to complete either, and earned less in early adulthood. This was not driven by academic difficulty: course failure and measured learning were unchanged. Earlier educational decisions, interrupted postsecondary enrollment, slower work-experience accumulation, and sorting into lower-paying occupations explain these results. Effects were larger for lower-income students, and earnings penalties more persistent for women.

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1 Introduction

Do youth spend too many years in school? Can removing some non-essential material allow students to graduate earlier without harming their long-term educational and career prospects? These questions are increasingly relevant in settings where competitive labor markets push young people to accumulate more credentials and stay in school longer, while aging populations and low fertility rates create pressure for earlier labor market entry. More schooling can raise human capital and improve outcomes related to earnings, health, and crime,[1] but extended curricula also impose private and public costs, delay labor market entry, and do not always translate into higher productivity or stronger foundational skills.[2] Moreover, if only high-SES families can absorb the direct and opportunity costs of long schooling pathways, extended programs may widen existing gaps in education and mobility. These tradeoffs raise a simple but important policy question: can shortening educational programs accelerate entry into postsecondary programs and the labor market without unintentionally reducing educational attainment or harming early career outcomes?

Unfortunately, there is limited causal evidence on the impact of accelerating school curricula to facilitate early graduation. This is primarily due to two reasons. First, the dominant policy trend in recent decades has been to increase years of schooling, and very few jurisdictions[3] have attempted to shorten time spent in school. Second, in the rare cases where such policies have been implemented, they typically compress an existing curriculum into a shorter time frame, increasing learning intensity and making the program more demanding for lower-achieving students. In such settings, it is difficult to disentangle the effects of reduced time in school from the effects of intensified coursework. As such, most of this literature has found negative effects on schooling outcomes from these types of curriculum compression reforms.[4]

We provide new evidence on whether shortening time to high school completion harms students' educational and labor market outcomes by studying a reform that, unusually, separated the duration of schooling from its intensity. In 1999, Ontario, Canada eliminated the last year of high school (grade 13), and the curriculum was redesigned with the explicit goal of maintaining comparable learning intensity (e.g., weekly class and homework hours). This

[1]See, for instance, Card (1999) and Lleras-Muney (2005), among many others.

[2]See, for example Hanushek and Woessmann (2008), Hanushek and Woessmann (2012), Pritchett (2013), and Hanushek et al. (2015).

[3]Some German states attempted a curriculum compression in the early 2000s, and Ghana rolled back an earlier policy lengthening high school in the late 2000s. Egypt shortened schooling by one year in the 1980s. Other countries and states have reduced compulsory schooling, including a few US states in the late 1800s to early 1900s.

[4]See Pischke (2007) and Büttner and Thomsen (2015) in Germany, Parinduri (2014) in Indonesia, and Morton et al. (2024) in the United States, among others.

was achieved by eliminating duplicated content across courses, adjusting prerequisites to give earlier access to advanced classes, and expanding credit recovery options such as summer, online, and partial-credit retakes. The aim was thus to enable earlier graduation without reducing content, lowering standards, or increasing difficulty, allowing us to estimate the effects of a shorter curriculum separately from increased curriculum difficulty.

We estimate the effects of this reform using a difference-in-differences design that compares Ontario cohorts born in 1985 and after (subject to the new 12-year curriculum) to earlier Ontario cohorts and to cohorts in other Canadian provinces that follow a 12-year system. When datasets contain exact birthdates and sample sizes permit, we supplement this with a difference-in-regression-discontinuity (DRDD) design around the December 31 school-entry cutoff, comparing students born just before and after January 1, 1985 while netting out relative-age effects. We draw on a rich set of restricted Canadian administrative and survey data (labor force surveys, census data, longitudinal tax records, and youth surveys) to track affected students from high school through their early 30s, measuring outcomes across learning, attainment, labor market activity, and earnings.

Our findings show that eliminating Grade 13, even when the curriculum was redesigned to preserve comparable learning intensity, had broad-ranging and unintended consequences. First, as expected, the reform reduced time spent in high school by about 0.44 years and sharply accelerated entry into postsecondary schooling, increasing postsecondary enrollment by roughly 18 percentage points at age 18 and 8 percentage points at age 19. However, at the same time, it lowered high school graduation rates by 1.6 to 4.2 percentage points and led to persistent declines in postsecondary attainment, including a 1.8 percentage point reduction in bachelor's degree completion, despite the overall increase in postsecondary enrollment. These educational disruptions translated into delayed labor market entry, with cumulative earnings roughly 15 percent lower at ages 21 to 22, slower accumulation of work experience, shorter early job tenure, and weaker employment and earnings throughout the early career. These effects are heterogeneous: students from above-median income households are better positioned to transition into postsecondary schooling early, while students from lower-income households bear a larger share of the declines in attainment and early career earnings.

On the mechanisms, the reform did not reduce learning (measured by standardized numeracy tests at ages 16–17 and 20–21) or increase course failure rates, ruling out heightened academic difficulty as the primary channel leading to lower educational attainment. Instead, the effects likely operate through changes in the timing and sequencing of schooling, by forcing students to make educational and career decisions one year earlier. Indeed, affected cohorts entered postsecondary education about 0.34 years earlier but were more likely to interrupt their studies, with interruptions of one year or more rising by 1.8 percentage points;

they accumulated about 0.74 additional months of full-time-equivalent postsecondary enrollment, with the calendar-time spread between first enrollment and final exit increasing by only 0.11 years and without completing more degrees. At the same time, this fragmented postsecondary enrollment picture is not offset by more frequent work-study behavior or by more labor market engagement. Instead, the policy led to higher economic inactivity rates (during which students are not in school or in the labor market). These longer and more fragmented enrollment spells delayed labor market attachment, reduced early work experience, and led to working more frequently in occupations with low educational requirements, ultimately contributing to lower early career earnings. Together, these results show that the structure and sequencing of schooling, not only instructional content, play a central role in shaping long run educational and labor market trajectories.

This setup allows us to make three contributions. First, we provide causal evidence on the consequences of shortening high school in a setting where the reform was explicitly designed to streamline, rather than compress, the curriculum. Prior evidence on curriculum length reforms typically reflects settings in which the same material was taught in fewer years, mechanically confounding shorter schooling with higher instructional intensity (Büttner and Thomsen, 2015, Parinduri, 2014, Pischke, 2007). The Ontario reform provides a distinct case because it reduced the time structure of schooling while aiming to preserve comparable learning expectations and the value of the high school credential. This allows us to study whether reducing the time available for course-taking, credit recovery, academic planning, and postsecondary decision-making can affect educational and labor market trajectories, even when the formal curriculum is not made more demanding. Earlier work on this policy has focused mainly on the "double cohort" (Morin, 2013, 2015). While useful for studying the transition year, this comparison cannot fully answer our question because the double cohort faced unusual postsecondary supply conditions and congestion from two cohorts entering postsecondary institutions at the same time. In contrast, we study broader and longer-run effects across multiple cohorts.

Second, we use linked administrative and survey data to examine the mechanisms through which the shortened curriculum affected students' pathways. Existing research emphasizes the importance of educational attainment for earnings and other adult outcomes (Card, 1999, Galama et al., 2018, Lleras-Muney, 2005, Lochner and Moretti, 2004, Oreopoulos et al., 2012), but less is known about how the timing and sequencing of educational transitions shape those outcomes. We show that the reform accelerated postsecondary entry but also increased interruptions, extended enrollment spells, delayed work experience, and weakened early labor market attachment. At the same time, we find little evidence that the policy increased course failure or reduced average measured learning in high school. This pattern suggests that the adverse effects operated primarily through disrupted sequencing and earlier educational decision-making rather than increased academic difficulty.

Third, we trace the longer-run consequences of these pathway disruptions into early adulthood. The reform lowered high school and postsecondary attainment, reduced early-career earnings, and shifted some affected students toward occupations with lower skill requirements. These effects were larger among students from lower-income households, suggesting that accelerated educational timelines may widen inequality when students differ in their academic, financial, and informational resources. More broadly, the findings show that curriculum structure and sequencing, independent of instructional content, can have persistent effects on human capital accumulation and early career trajectories.

In summary, the findings speak directly to current policy debates about accelerating secondary education. They show that shortening high school, even when the curriculum is redesigned to preserve comparable learning intensity, can reshape educational trajectories in ways that policymakers may not anticipate. In particular, earlier graduation does not necessarily translate into faster or more successful postsecondary progression and may instead delay labor-market integration. These results highlight that curriculum structure and sequencing, rather than instructional content alone, should be central considerations when designing policies aimed at speeding up the transition from school to work.

The rest of our article is organized according to the following structure: section 2 describes the Ontario institutional background and policy details, in section 3 we use a conceptual framework to illustrate possible mechanisms through which the policy might affect student outcomes, in section 4 we describe the data sources used in this study, section 5 describes the empirical approach, section 6 presents the results, section 7 presents robustness checks and section 8 concludes.

2 Institutional Background

In 1999, the provincial government of the province of Ontario, Canada, eliminated grade 13, thus replacing the existing five-year high school program with a new four-year curriculum. The reform was motivated by several objectives: reducing system-wide costs, facilitating earlier entry into the labor market, and aligning Ontario with the 12-year secondary systems used across most Canadian provinces and the United States (Morin, 2013). Prior to the reform, Ontario students typically spent 13 years in primary and secondary school. Even students who could complete all the required credits in twelve years often continued to take a voluntary "victory lap" year, typically to improve grades, refine postsecondary plans, or try out more elective classes (Brady and Allingham, 2010).

Beginning with the cohort entering high school in 1999 (i.e., those born in 1985) the province mandated a new 30-credit, four-year program to replace the old 36-credit, five-year program.[5] The curriculum was thus deliberately redesigned to maintain comparable learning intensity, defined as the total number of hours of lectures, homework and study per week, because of concerns that simply compressing the 5-year curriculum into 4 years would lead to increases in course failure and high school dropout rates. Several other measures were taken to ensure that learning outcomes would not be affected. First, the 6-credit reduction was achieved by analyzing the entire curriculum, including elective classes, and identifying and eliminating redundancies by merging courses. Second, summer course offerings were expanded to help students stay on track for on-time completion (King et al., 2004). Lastly, the emphasis on class failure was shifted towards learning goals. In other words, in the new system, when a student failed a class, they were allowed to receive partial credits for the modules in which they performed well. Meanwhile, teachers and administrators identified particular modules on which the student struggled and created customized learning tasks that the student could complete to receive full credits without retaking the entire class. The intent was thus to streamline the pathway to graduation without compressing instructional content or increasing academic difficulty.

The reform also produced a so-called "double cohort": students born in 1984 (the last eligible for grade 13) and students born in 1985 (the first required to complete high school in four years) graduated in the same year. As in Morin (2013) and Zhang (2022), we leverage these adjacent cohorts to corroborate our main estimates. The double cohort faced unusual postsecondary supply conditions: colleges expanded first-year capacity, universities adjusted admissions thresholds, and students encountered a temporarily overcrowded system. These features distinguish this transition year from typical pre- or post-reform cohorts. Moreover, there is evidence that some students born in 1984 attempted to graduate from high school earlier to avoid being part of the double cohort. Our main empirical strategy therefore compares multiple pre-1985 cohorts to multiple post-1985 cohorts in Ontario, keeping the 1984 and 1985 cohorts in the sample, using contemporaneous cohorts in other Canadian provinces as a control group rather than relying directly on the December 31st, 1984 cutoff. We confirm in Section 7.1 that excluding the 1984 and 1985 cohorts as a robustness check leaves the main results essentially unchanged.

[5] A credit corresponds to a course with at least 110 instructional hours both before and after the reform.

3 Conceptual Framework

The elimination of grade 13 shifted students' timing and sequencing of major educational decisions, with potential implications for postsecondary enrollment, attainment, and early-career labor market outcomes. Three broad channels motivate our empirical analysis.

First, removing Grade 13 reduced the opportunity cost of entering postsecondary schooling. By advancing high school exit by one year, the reform lowered forgone earnings and shortened the time required to begin college or university. Standard human capital models predict that lower opportunity costs should raise enrollment, especially for students on the margin of attending. This channel motivates our examination of earlier and higher postsecondary entry.

Second, the reform required students to make key educational choices at a younger age. Students in the post-reform cohorts selected programs, institutions, and course loads roughly one year earlier than previous cohorts. Earlier decision-making may increase the risk of sub-optimal program choice, weak planning, or the need to revise enrollment decisions midstream. These adjustments can produce longer total time enrolled, more interruptions, or slower progression even if initial enrollment rises. This motivates our analysis of enrollment spells, interruptions, and the timing of exit from postsecondary schooling.

Third, shifting prerequisites earlier changed the academic content students encountered at younger ages. Exposure to advanced material earlier might raise learning for some students or overwhelm others. Whether the reform increased or reduced academic intensity is therefore an empirical question, motivating our study of math performance, numeracy, and course-failure outcomes.

While these channels do not yield sharp theoretical predictions for all outcomes, they imply a coherent set of empirical tests: (i) changes in the timing and quantity of postsecondary enrollment, (ii) changes in the structure of enrollment spells, (iii) potential effects on academic performance in high school, and (iv) downstream effects on work experience, earnings trajectories, and early career outcomes. More specifically, these mechanisms generate a set of observable empirical implications. If the reform reduced the opportunity cost of postsecondary schooling by allowing students to leave high school earlier, we should observe earlier postsecondary enrollment and, potentially, higher enrollment among marginal students. If the reform required students to make program, institution, and course-sequencing decisions at a younger age, we should observe more fragmented postsecondary pathways, including interruptions, delayed completion, or lower degree attainment. Since the reform exposed students to advanced academic material earlier, we may observe changes in measured learning, course failure, or high school graduation, with effects that may differ by prior achievement.

Finally, if earlier transitions and disrupted sequencing weakened students' accumulation of work experience or altered their educational credentials, we should observe delayed labor market attachment, lower early-career earnings, and sorting into occupations with lower skill requirements. These implications guide the empirical analysis that follows. In the next section, we describe the data used to evaluate these predictions.

4 Data

Our analysis combines several large-scale Canadian administrative and survey datasets that together allow us to follow individuals from high school through postsecondary schooling and into early labor market experiences.

4.1 Labour Force Survey (LFS)

First, we use the Labour Force Survey (Statistics Canada, 2022), henceforth the LFS, spanning 2000 to 2022 to study the labor market outcomes and educational enrollment and attainment of individuals exposed to the policy from late adolescence to mid-thirties. The LFS is a monthly survey conducted by the Canadian government with a rotating panel design, in which households are interviewed for up to six consecutive months. We primarily use the LFS as a series of repeated cross-sections to study population-level outcomes for individuals aged 15 and above, but we exploit the panel structure to link teenagers to co-resident parents and recover household socioeconomic status.[6] Our analysis focuses on education levels, labor market status, hours worked, occupation and industry, job tenure, and earnings.

We only use LFS surveys between October and February, because the LFS does not contain birth dates (only ages), which is central to our identification strategy. Using survey months from the beginning (or end) of each calendar year allows us to infer birth years more accurately. For example, an individual aged 20 in January 2020 was almost certainly born in 1999, whereas a 20-year-old surveyed in July 2020 could have been born in either 1999 or 2000. This is similar to the procedure used by Morin (2013).

4.2 2021 Canadian Census of Population

We also use the 2021 Canadian Census of Population (Statistics Canada, 2021). A randomly selected 25% sample of households must complete the long-form census, which includes rich information on demographics, geography, education, field of study, and income. Compared to the LFS, the Census provides us with a single snapshot of the Canadian population. However,

[6] Standard errors in our regressions are clustered at the province-by-cohort level, which is a coarser partition that nests individuals.

it i) includes individuals' birth dates, which we exploit in a regression discontinuity design around the school enrollment cutoff, and ii) provides us with a very large sample allowing us to study long-term outcomes almost twenty years after high school graduation.

4.3 Longitudinal Administrative Databank (LAD)

The Longitudinal Administrative Databank (Statistics Canada, 2019), henceforth the LAD, is a longitudinal dataset comprising individual and family tax returns from 1982 onwards, covering 20% of Canadians. We use LAD years 1993 to 2019. This offers rich demographic details such as birth dates, census area, and immigration status. While lacking data on educational attainment or hours worked, the LAD provides precise and detailed income measures, including labor market earnings and benefits, facilitates longitudinal tracking of individuals, and offers a large sample size, making it invaluable for assessing the policy's impact on earnings. Moreover, the LAD tax returns enable us to creatively assess various other outcomes. By leveraging the Federal tuition tax credit claimed by parents for their children's postsecondary schooling attendance between 1996 and 2016, we can accurately gauge individuals' school attendance histories.

4.4 National Longitudinal Survey of Children and Youths (NLSCY)

The NLSCY (Statistics Canada and Human Resources Development Canada, 2009) was a longitudinal study of Canadian children that follows respondents from birth to early adulthood, running from 1994 to 2009. It is designed to provide information regarding the factors influencing a child's social and behavioral development. The NLSCY contains information about school performance at ages 14 and 15, which allows us to document the heterogeneous effects of the policy by baseline academic achievement.

4.5 Survey of Labour and Income Dynamics (SLID)

Lastly, the SLID (Statistics Canada, 2011) was an overlapping longitudinal study of the economic well-being of Canadians, running from 1994 to 2011. For our purposes, we only use the SLID to track Ontario teenagers to determine the number of years they spent in high school as a result of the reform.

5 Empirical Framework

5.1 Difference-in-Differences

The baseline identification strategy compares the post-reform cohorts in Ontario with the same birth cohorts in the rest of Canada. For outcomes measured at a single age or over a fixed age range (e.g., high school graduation, highest attainment, long-run earnings at ages 30–35), we estimate equation 1:

$$Y_i = \beta_p \text{Ontario}_i \times \mathbb{1}(c_i \geq 1985) + \beta_x^T X_i + \theta_j + \theta_c + \theta_y + \varepsilon_i \tag{1}$$

Here, Y_i is the outcome for individual i in province j, birth cohort c, observed in the administrative data in year y. The parameter of interest, β_p, compares the post-reform Ontario cohorts with the same cohorts elsewhere. Province (θ_j), cohort (θ_c), and survey-year (θ_y) fixed effects absorb structural differences across regions and time, and X_i includes standard demographic controls available in the survey data.[7] We do not include separate Ontario_i or $\mathbb{1}(c_i \geq 1985)$ main effects because they are perfectly collinear with the province and cohort fixed effects, respectively, and would be dropped from the regression; the interaction β_p therefore captures the full difference-in-differences contrast.

This specification identifies the average effect of exposure to the new curriculum, conditional on common trends between Ontario and comparison provinces.[8] As shown in Section 7, the common-trend assumption holds closely for most outcomes, including schooling and many labor market outcomes, but is weaker for earnings and hourly wages. For this reason, we complement the DiD with an age-profile estimator for those variables. We also estimate a synthetic-control-weighted difference-in-differences model, described in more detail in section 5.3, to address this issue. Lastly, standard errors are clustered at the province–birth cohort level.[9]

[7] In the LFS, these include parental education and gender, in the LAD these include gender, in the NLSCY these include household socioeconomic score at age 14–15, single parent status at age 14–15 and gender, and in the SLID, these include gender and immigrant status.

[8] We exclude respondents in the province of Québec from our analyses, despite its size, which accounts for about a quarter of the Canadian population, for two reasons. First, the Québec education system is unique, making it difficult to compare to other provinces. It follows a 6+5 primary and secondary system, plus a two-year upper secondary or postsecondary degree (called CEGEP) that is a prerequisite for university-bound students and also offers vocational tracks. Further complicating matters, university-bound students from Québec are exempt from the freshman year of university if they graduated from CEGEP. Second, and perhaps more importantly, the school enrollment cutoff date in Québec is September 30th, which makes it difficult to apply both our difference-in-differences and DRD designs, especially in datasets when birth year must be imputed from ages. All other provinces use a January 1st enrollment cutoff date.

[9] Although the data are observed at the individual level and, in some cases, constitute a census, the identifying variation in equation 1 arises at the province–cohort level, since treatment status is constant within each province–birth cohort cell. As emphasized by Abadie et al. (2023), clustering is warranted

5.2 Age-Profile Estimates (Triple Differences)

To study outcomes whose levels exhibit province-specific pre-trends (especially yearly earnings and hourly wages), we estimate policy effects on the *shape* of the life-cycle profile rather than on levels at a given age.

Specifically, we estimate equation 2:

$$\begin{aligned} Y_i = {} & \beta_p \text{Ontario}_i \times \mathbb{1}(c_i \geq 1985) + \beta_{p\times a}\, \text{Ontario}_i \times \mathbb{1}(c_i \geq 1985) \times \text{Age}_i + \\ & \beta_x X_i + \beta_{x\times a} X_i \times \text{Age}_i + \\ & \theta_j + \theta_c + \theta_y + \theta_{j\times a} + \theta_{c\times a} + \theta_{y\times a} + \varepsilon_i \end{aligned} \tag{2}$$

This specification is effectively a triple-difference, where we interact the terms in Equation 1 with respondents' ages. We also include age-specific cohort (c), province (j) and year (y) fixed effects θ in addition to the fixed effects in the previous equation. Identification is driven by *within-age and survey year comparisons.* In other words, we compare individuals who have the same ages and who are observed in the same survey years, but whose treatment status - defined by birth year and birth province - varies. As in Equation 1, we omit the Ontario_i, $\mathbb{1}(c_i \geq 1985)$, and Age_i main effects, as well as the $\text{Ontario}_i \times \text{Age}_i$ and $\mathbb{1}(c_i \geq 1985) \times \text{Age}_i$ interactions, because they are perfectly collinear with the province, cohort, year, and age-interacted fixed effects $(\theta_j, \theta_c, \theta_y, \theta_{j\times a}, \theta_{c\times a}, \theta_{y\times a})$. The level DiD coefficient β_p and the slope DiD coefficient $\beta_{p\times a}$ are the only treatment parameters separately identified.

This approach does not require parallel trends in outcome levels. Instead, it requires that Ontario and the comparison provinces exhibit similar *changes in the slope* of the age profile absent the reform. This assumption is more plausible than full parallel trends for earnings and wage outcomes, and the estimated $\beta_{p\times a}$ coefficients provide a transparent view of how the policy reshapes life-cycle trajectories.

5.3 Additional Specifications and Robustness Checks

Beyond the main DiD and age-profile estimators, we implement a set of supplementary specifications that help verify the stability of the results. These checks are described briefly here, with more details presented in Section 7.

when score contributions are correlated within groups in ways that do not vanish asymptotically, regardless of whether individuals are randomly sampled. In this difference-in-differences design, individuals within the same province–cohort cell may share unobserved shocks relevant for identification, while individuals across cells provide independent identifying variation. Clustering at the province–cohort level therefore yields inference that is consistent with the underlying assignment and dependence structure of the design.

Regression discontinuity design: First, we exploit Ontario's January 1 school-entry cutoff to estimate two cohort-based RD designs to address the relative age effect, akin to a difference in regression discontinuity strategy (DRDD). These strategies are akin to the ones used in Malamud et al. (2023). The first method effectively isolates the policy effect by contrasting outcomes for individuals born after the January 1, 1985 cutoff in Ontario (where both policy and relative age effects are present) with outcomes for those born after cutoff dates in other "control" years in Ontario (where only the relative age effect is expected). The second strategy compares Ontario students born around the January 1985 cutoff to those in other provinces born around the same time. These approaches help mitigate the influence of relative age, facilitating the estimation of the policy effect. For more details on these approaches, please see Appendix B.3. The RDD results are presented along with our main results, in the next section.

Omitting the double cohort: Because the 1984 and 1985 birth cohorts graduated as part of Ontario's double cohort, we re-estimate all main specifications excluding these groups to ensure that the results are not driven by the unusual fact that two cohorts graduated from high school and entered university at the same time. The results of these estimations are discussed in section 7.1.

Placebo tests: We reestimate our models using 1978 as a placebo policy birth year. The reason behind this choice was to split the 1975–1980 cohorts into two equally-sized groups, and allowing us to omit the 1981–1984 cohorts, which are pre-treatment cohorts, but may exhibit anticipation effects. The results of these estimations are discussed in section 7.2.

Parallel Trends: We show event-study plots for all outcomes for which this is possible, documenting the absence of pre-reform differential trends and thereby validating the parallel-trends assumption underlying our DiD estimates. These estimates are discussed in Section 7.3.

Synthetic-Control-Weighted Difference-in-Differences: To address potential issues with non-parallel pre-trends in the outcomes, we implement a synthetic-control-weighted difference-in-differences design that constructs province weights using the synthetic control approach of Abadie and Gardeazabal (2003) and then estimates a difference-in-differences regression on the reweighted panel. More specifically, for each outcome, we aggregate our data at the province and birth cohort levels and create a "synthetic Ontario" by assigning weights to the other Canadian provinces.[10] These weights are assigned to minimize the differences in pre-trends in the outcomes between Ontario and the "synthetic Ontario". Using these weights, we estimate a difference-in-differences regression that compares the control "synthetic Ontario" to Ontario cohorts pre- and post-1985. The main drawback to this analysis, and the reason we do not use it as our main specification, is the fact that these regressions are estimated at the province-by-birth year cell, and thus we cannot include individual controls. The results of these estimations are discussed in section 7.4.

5.4 Assessing Heterogeneity in Outcomes

For many of the outcomes presented in this paper, we are interested in heterogeneity in particular by socioeconomic status or baseline academic ability. Not all data sets used to estimate DiD models in this paper measure these dimensions. The NLSCY, which we use to measure learning outcomes mostly in high school, is the only dataset providing baseline academic ability. In the LAD, which is a panel tracking individuals and households across many years, we can assess the socioeconomic background of young adults by measuring their household incomes when they were aged between the ages of 14 and 17 and very likely to be cohabiting with their parents.

However, for the LFS, which is a rotating 6-month panel, we can assess the socioeconomic status of young adults only to the extent that they are still cohabiting with their parents. For this reason, we measure heterogeneity across three different groups: young adults cohabiting with their parents whose household incomes are above-median, their counterparts whose household incomes are below-median, and young adults who are not living with their parents. While this is an imperfect way to assess socioeconomic status, it is the only way we can do so in the LFS. Because of issues of selection into cohabiting with their parents, we only explore heterogeneity on these dimensions in the LFS at ages 18 to 20, when cohabitation is still common enough to permit such an analysis.

In addition to socioeconomic status and baseline academic ability, we also assess heterogeneity in the policy effects by gender. Unlike socioeconomic status, gender is observed without measurement error in every dataset used in the paper, so heterogeneity by gender can be estimated in the LFS, LAD, and Census without restricting attention to the subsample of young adults still cohabiting with their parents. We re-estimate our main DiD specifications interacting the policy variable with a Female indicator and report results in Appendix B.2. A consolidated summary of heterogeneity findings across income, cohabitation, and gender

[10] We exclude Québec, which, as mentioned previously, is excluded due to its unique education system and different school enrollment cutoff dates, and the three territories - Yukon, the Northwest Territories, and Nunavut - due to their small populations.

is presented in Section 6.9.

6 Results

6.1 High School Completion and Timing

The policy's most direct objective was to eliminate the final (fifth) year of high school. In Table 1, we examine the immediate impact of the policy, using SLID and LFS data. Overall, it moderately succeeded, trimming Ontario youths' total school time by 0.44 years (column 1).[11] This reduction primarily stems from a shift from graduating high school in five years to graduating in four years.[12] Despite this large shift, about 20% of students still opt for a fifth year. Termed a "victory lap," this tendency persists for several reasons, such as meeting graduation criteria, improving postsecondary prospects, or taking additional courses (King et al., 2004). From an empirical standpoint, these dynamic responses do not pose problems for identification, since our estimates capture intent-to-treat effects at the cohort level. In fact, this means that our estimates are lower bounds for the effects on the treated.

Table 1: Effects on High School Timing and Age 18–19 Enrollment

	Dependent Variable:						
	Years	HS Enrollment (pp)		2-yr College Enrollment (pp)		4-yr Univ. Enrollment (pp)	
	in HS	Aged 18	Aged 19	Aged 18	Aged 19	Aged 18	Aged 19
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	-0.437***	-20.19***	-5.49***	4.41***	3.04**	17.74***	5.36***
	(0.004)	(1.35)	(0.95)	(0.93)	(1.23)	(1.45)	(1.37)
Dataset	SLID	LFS	LFS	LFS	LFS	LFS	LFS
N	24,397	1,343,800	1,343,800	1,343,800	1,343,800	1,343,800	1,343,800

This table shows the policy effect on total years spent in high schools and enrollments into high school, college, and university at ages 18 and 19. The coefficient for column (1) is from estimating equation 1 on SLID data. Coefficients for columns (2)–(7) are from estimating versions of equation 2 on LFS data and reporting coefficients of the policy effects at ages 18 and 19. Control variables for (1) include gender, immigrant status, year, cohort, and province of high school fixed effects. Control variables for (2)–(7) include gender, year, birth cohort, and province of high school fixed effects, and their interaction with birth cohort. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These changes in high school duration translate directly into enrollment patterns at ages 18 and 19. Columns 2 and 3 of Table 1 show a substantial decrease in high school attendance at age 18 (about 20.2 pp), followed by a moderate decrease of 5.5 pp at age 19. These results corroborate the 0.44-year reduction in time spent in high school reported.

[11] In Table A.1 of the appendix, we show that this is driven mainly by shifts from 5-year high school completion to 4-year completion.

[12] Figure A.1 in the appendix shows the corresponding shift in the distribution of time spent in high school.

At the same time, the policy increased enrollment in postsecondary institutions at ages 18 and 19. Two-year college enrollment rises by 3–4 pp at these ages (columns 4–5), while four-year university enrollment surges by nearly 18 percentage points at age 18 and continues to be elevated at age 19 (5.4 pp). These increases reflect both the mechanical effect of earlier high school graduation, as students who previously would have still been in grade 13 at age 18 now enter college or university one year sooner, but also an increase in postsecondary enrollment, as the total surge in postsecondary enrollment at age 19 (roughly 8.4 pp) is larger than the corresponding decrease in high school enrollment at age 19 (5.5 pp).

6.2 High School Outcomes: Learning, Course Failure and Graduation Rates

Table 2: Policy Effect on High School Graduation Rates

	HS Graduation Rate (pp)				
	(1)	(2)	(3)	(4)	(5)
Policy	-7.0***	-4.4	-1.59***	-2.99***	-4.18***
	(2.9)	(3.2)	(0.32)	(0.81)	(1.10)
Low achiever		-12.8***			
		(2.4)			
Policy × Low achiever		-3.8			
		(5.0)			
Dataset	NLSCY	NLSCY	LFS	Census	Census
Method	DiD	DiD	DiD	DRDD	DRDD
Ages	20–21	20–21	30–35	33–46	35–36
N	2,801	2,577	381,200	51,500	7,800

This table shows the policy effects on high school graduation rates. Each column represents a different regression. Columns (1)–(3) use a DiD strategy, and columns (4)–(5) use a DRDD strategy (estimating equations A.1 and A.2, respectively). Low achievers are students whose overall academic performance was rated "Average" or "Below Average" in Grades 7 or 8 (ages 14–15). Control variables for LFS estimation include gender, parents' educational attainments, survey year, cohort, and province of residence fixed effects. Control variables for NLSCY estimations include gender, household socioeconomic score at age 14–15, single parent status at age 14–15, year, cohort, and province of high school fixed effect. Census estimations include gender as a control. Standard errors are clustered at the province × cohort level for DiD estimates and at the running variable level for DRDD estimates. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Despite policymakers' goal to maintain high school graduation rates, the curriculum redesign led to a decline in graduation rates ranging from 1.6 to 7.0 percentage points across datasets and identification strategies (Table 2). Columns 1 to 3 use the NLSCY and LFS in a difference-in-differences framework to document this decline. Columns 4 and 5 rely on census data and a DRDD design around the January 1, 1985 "double cohort" enrollment cutoff, confirming a reduction of 3.0 to 4.2 percentage points in graduation rates that cannot be attributed to secular trends in high school completion.[13]

Table 3: Policy Effect on Course Failure Rates

	Ever Failing a High School Class in:							
	Overall		Math		English		Science	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy	-0.016	0.009	0.026	0.04	-0.057***	-0.046***	-0.003	0.015
	(0.033)	(0.043)	(0.024)	(0.029)	(0.015)	(0.012)	(0.022)	(0.020)
Low achiever		0.207***		0.101***		0.100***		0.096***
		(0.032)		(0.020)		(0.018)		(0.015)
Policy ×		0.008		0.072		-0.054***		-0.054***
Low achiever		(0.060)		(0.049)		(0.019)		(0.020)
N	4,552	4,078	4,539	4,067	4,540	4,068	4,539	4,067
R^2	0.082	0.123	0.045	0.074	0.034	0.056	0.033	0.053

This table shows the effect of the policy on course failure rates. Each column represents a different estimation of equation 1, using NLSCY Cycles 1–8 (1994–2008). The dependent variable is an indicator of students ever failing a certain course in high school. Low achiever means students whose overall academic performance was rated "Average" or "Below Average" in Grades 7 or 8 (ages 14-15). Other control variables include gender, household socioeconomic score at age 14–15, single parent status at age 14–15, year, cohort, and province of high school fixed effect. Standard errors are clustered at the province × cohort level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Why did high school graduation rates fall? An obvious hypothesis is that, despite policymakers' intentions, the elimination of Grade 13 increased learning intensity, making it more difficult for students to earn the credits required for graduation. However, three findings contradict this explanation. First, there is no statistically significant evidence that the policy disproportionately affected low-achieving students (column 2). While this result is somewhat surprising, the small sample size of the NLSCY, which is the only dataset containing baseline achievement measures, may limit statistical power.

Second, there is little evidence that the policy increased failure rates (Table 3). This holds for overall failure rates (columns 1–2) as well as for specific subjects: mathematics (columns 3–4), English (columns 5–6), and science (columns 7–8). If anything, failure rates in English declined by 5.7 percentage points following the reform. Moreover, there is no evidence that low-achieving students—who are most at risk of failing—were more likely to fail courses after the elimination of Grade 13. Again, if anything, low achievers failed English and science courses 5.4 percentage points less often after the reform. These findings are consistent with

[13] In Table B.26 of the appendix, a 1.3 pp decrease in high school graduation rate is already observable at ages 18–20, using an Synthetic-control-weighted difference-in-differences estimation.

policymakers' efforts to prevent increases in course failure and rule out academic difficulty as the primary mechanism behind the decline in high school graduation rates.

Third, we examine how the high school curriculum redesign impacted student learning. Notably, it facilitated earlier graduation by adjusting course prerequisites, allowing students to take advanced classes sooner. We analyze this impact using two measures: a standardized math test taken at ages 16–17 and a numeracy test taken at ages 20–21 (Columns 1–4 of Table 4). The policy increased math skills by 0.16 SD, driven mostly by high-achieving students, and low-achievement students also score higher post-reform. These gains are consistent with students being exposed to more advanced material earlier in high school, which may have raised achievement for higher-performing students without imposing measurable learning losses on lower-performing students.

Table 4: Effect of Policy on Learning in and after High School

	Math Ability (16–17)		Numeracy (20–21)	
	(1)	(2)	(3)	(4)
Policy	0.158*	0.208***	-0.032	0.139
	(0.079)	(0.080)	(0.065)	(0.087)
Low achievement		-0.675***		-0.718***
		(0.052)		(0.069)
Policy $\times$ Low achievement		-0.093		-0.227**
		(0.117)		(0.229)
N	3,130	2,803	2,487	2,288

This table shows the policy effect on numeracy and literacy. Each column represents a different estimation of equation 1, using the NLSCY. Math ability is measured at ages 16–17 using the PISA math questionnaire (columns 1–2), and numeracy is measured at ages 20–21 via a test (3–4). Control variables include gender, household socioeconomic score at ages $14-15$, single parent status at ages 14–15, year, cohort, province of high school fixed effects and grade fixed effects (1–2). Low achievement means students whose overall academic performance was rated "Average" or "Below Average" in Grades 7 or 8 (ages 14–15). Standard errors are clustered at the province $\times$ cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Consistent with the early exposure theory (as well as the aggregate decrease in educational attainment), positive effects on learning do not persist across time. Columns 3-4 show null effects for numeracy at ages 20–21. High achievers maintain higher numeracy skills, but there is evidence of a substantial learning loss (0.23 s.d.) among low achievers. This pattern is consistent with the decline in high school graduation documented earlier - and the negative effects on postsecondary attainment that we document in the next section.

In conclusion, the policy had no aggregate negative effects on average learning or on course failure rates, although later numeracy declines among low-achieving students suggest weaker skill persistence in some subgroups. Shifts in high school graduation behavior are therefore more likely to reflect behavioral or scheduling constraints than academic difficulty in the average case, while leaving open the possibility that some vulnerable groups were affected through a learning channel as well. The transition from a flexible five-year structure to a compressed four-year pathway may have tightened course sequencing, restricted opportunities to recover missing credits, or increased the need to take multiple demanding courses simultaneously. These logistical pressures can reduce graduation rates even in the absence of declines in academic ability or increases in course failure. Moreover, the new curriculum pressures students into making crucial decisions for their educational career, such as enrolling into university-preparatory courses or designing a path to graduation, one year earlier, at a stage when preferences, aspirations, and academic identities are still forming, and at this age even a single year constitutes a substantial share of development.

6.3 Long-Run Educational Attainment

Contrary to its goals, the policy also had lasting negative impacts on educational attainment beyond high school, as shown in Table 5. In the top panel, we show how the proportion of young Ontarians in different educational attainment categories changed as a result of the policy. As previously highlighted in Table 2, the high school dropout rate increased by 1.6 pp. The proportion of students who attained exactly a high school degree remained unchanged, however. Finally, while there was a rise in the proportion of respondents who attended some postsecondary schooling without earning a degree (0.9 pp), it was coupled with a decrease in college completion (-0.5 pp, insignificant) and bachelor's degree completion (-1.8 pp), indicating an increase in postsecondary dropout behavior.

Therefore, we can conclude that the policy led to a negative shift in educational attainment, more easily visible in the bottom panel. The decrease in high school graduation (1.6 percentage points) was accompanied by declines in those who enrolled at least in some form of postsecondary schooling (1.5 percentage points), completion of at least a 2-year college degree (2.3 percentage points), or at least a bachelor's degree (1.8 percentage points). We further validate these findings using a DRDD design, leveraging the January 1, 1985 school enrollment cutoff (Tables B.12 and B.13 in the appendix). This confirms our main findings. Overall, the new curriculum led to negative effects on educational attainment.[14]

What explains this negative effect of the policy on educational attainment? These effects are unlikely to be driven solely by high school dropouts, as students on the margin of not

[14]Unfortunately, we cannot show heterogeneity in these results in the LFS due to not being able to accurately measure the socioeconomic background of individuals not cohabiting with their parents.

Table 5: Policy Effect on Long-Run Educational Attainment

	(1) HS Dropout	(2) HS Graduate	(3) Some PSE	(4) College	(5) University
Attained Exactly					
Policy	1.59***	-0.14	0.87***	-0.53	-1.84***
	(0.32)	(0.48)	(0.31)	(0.55)	(0.69)
Attained At Least					
Policy		-1.59***	-1.45***	-2.32***	-1.78**
		(0.32)	(0.53)	(0.61)	(0.81)
N	381,200	381,200	381,200	381,200	381,200

This table shows the effect of the policy on long-run educational attainment. Each column represents a different estimation of equation 1, using the LFS. The top panel shows results using exact educational attainment as outcome variables. The bottom panel shows results using minimal educational attainment as outcome variables. Controls include gender, parents' educational attainments, survey year, cohort, and province of residence fixed effects. Standard errors are clustered at the province $\times$ cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

graduating from high school are unlikely to be on the margin of enrolling in postsecondary education, and even less likely to be on the margin of completing it. Moreover, the magnitude of the negative effects on postsecondary attainment (1.8 to 2.3 percentage points) exceeds that observed for high school graduation (1.6 percentage points).

As previously noted, one possible explanation is that requiring teenagers to make postsecondary enrollment decisions one year earlier leads to suboptimal choices. In addition, exposure to more demanding postsecondary coursework at a younger age may be more difficult to manage, even if learning outcomes in high school are not adversely affected by the policy. These factors may have persistent consequences for students' postsecondary trajectories.

Postsecondary Enrollment Histories To tease out the mechanisms leading to lower educational attainment, we study individuals' postsecondary enrollment histories throughout their late teens and twenties (Table 6) with the help of tuition tax credit data from the LAD. These data record all tuition payments claimed for tax purposes and therefore allow us to track the exact timing, duration, and intensity of postsecondary enrollment for every individual in the LAD.[15] This provides a detailed view of how the curriculum change reshaped

[15] Eligible tuition payments were claimed using a tax slip (T2202), which postsecondary institutions issued to all enrolled students and filed directly with the Canada Revenue Agency. Claiming the tuition credit was trivial and required little more than accepting the reported amount. While students received tax slip

students' trajectories after high school and helps us understand the increases in postsecondary enrollment coupled with decreases in graduation rates.

Table 6: Postsecondary Attendance Histories (LAD)

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy	-1.65***	-0.34***	-0.29***	-0.08***	1.76***	0.74***	0.11***
	(0.35)	(0.04)	(0.02)	(0.03)	(0.38)	(0.24)	(0.04)
N	391,900	277,300	269,600	277,300	277,300	277,300	277,300

This table shows the effect of the policy on educational enrollment histories. Each column represents a separate estimation of equation 1 using the LAD. Controls include gender, year of birth, survey year and province of residence at age 18 and interactions between gender and year of birth and survey year and year of birth. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Exposure to the shortened curriculum increased enrollment into postsecondary schooling by 1.65 pp (column 1) and lowered the age of first attendance by 0.34 years (column 2), which were goals of the policy. These increases are consistent with the earlier spikes in enrollment at ages 18 and 19 documented in Table 1: by reducing time spent in high school and lowering the opportunity cost of entering postsecondary programs, the reform encouraged more students to enroll and to do so earlier. The age at which students first interrupt their postsecondary studies (column 3) also falls slightly, but the age of final exit remains nearly unchanged (column 4), implying longer overall time spent in postsecondary schooling. Earlier entry therefore did not accelerate completion; instead, it lengthened the time students remained enrolled.

Interruptions, defined as being out of school for an entire calendar year before re-enrolling, increased by 1.76 pp (column 5), and individuals accumulated 0.74 more months of full-time equivalent postsecondary enrollment (column 6). The calendar time between first enrollment and final exit, however, increased only marginally — by 0.11 years (column 7), consistent with lower degree completion. These results are confirmed by the DRDD checks in Appendix Tables B.10 and B.11 and are thus not driven by pre-trends in enrollment patterns: students in the double cohort exposed to the new curriculum are more likely to experience these enrollment patterns than their cohort mates who followed the old curriculum and graduated at the same time.

To recap, the tuition credit data indicate that students responded to the compressed high school curriculum by entering postsecondary schooling earlier but progressing through it in a

T2202 and initially claimed the tax credit, it was possible for the credit to be subsequently partially or fully transferred to their parents. This allowed families to lower their tax burden even if the children were low-income earners with very low marginal tax rates, and gave strong incentives to claim the credit.

more fragmented and extended manner. They spaced out their studies over a longer period without taking more courses and accumulated more interruptions. Evidence from postsecondary settings shows that such interruptions reduce academic performance and credit accumulation even among otherwise similar students, likely reflecting skill decay or the need to re-learn material after time away from structured coursework.[16] Thus, the more frequent and longer gaps in enrollment documented here may have directly hindered academic progression and contributed to lower completion rates.

This pattern also has implications for public finances. Students exposed to the reform spend similar or greater total time enrolled in postsecondary institutions but do not translate this additional time into higher completion. In practice, this means that institutions incur comparable instructional costs while producing fewer graduates. Moreover, to the extent that postsecondary enrollment is more costly than high school enrollment,[17] the education system as a whole is incurring higher costs without leading to higher educational attainment. These enrollment histories help explain the declines in educational attainment documented earlier. In the next section, we examine how these altered educational pathways translated into slower accumulation of work experience and weaker early-career labor market outcomes.

6.4 Wages and Earnings

Finally, we study the policy effects on wages and earnings. Before turning to these effects, it is important to note that several labor market variables exhibit non-parallel pre-trends, as illustrated in the event study figures in Section 7.3. In particular, hourly wages and yearly earnings show province-specific trends during the 1990s and early 2000s that are unrelated to the curriculum reform.[18] For this reason, we do not interpret level differences across cohorts using a standard DiD approach. Instead, we focus on how the policy shifted the shape of age profiles in earnings and wages, using equation 2. This approach isolates changes in the timing and slope of labor market trajectories and allows us to study how earlier high school exit and altered postsecondary pathways translated into long-run labor market outcomes.[19]

The earnings data reveal a clear steepening of the cumulative earnings profile for the affected cohorts (first panel of Figure 1). In the LAD tax data, cumulative earnings are

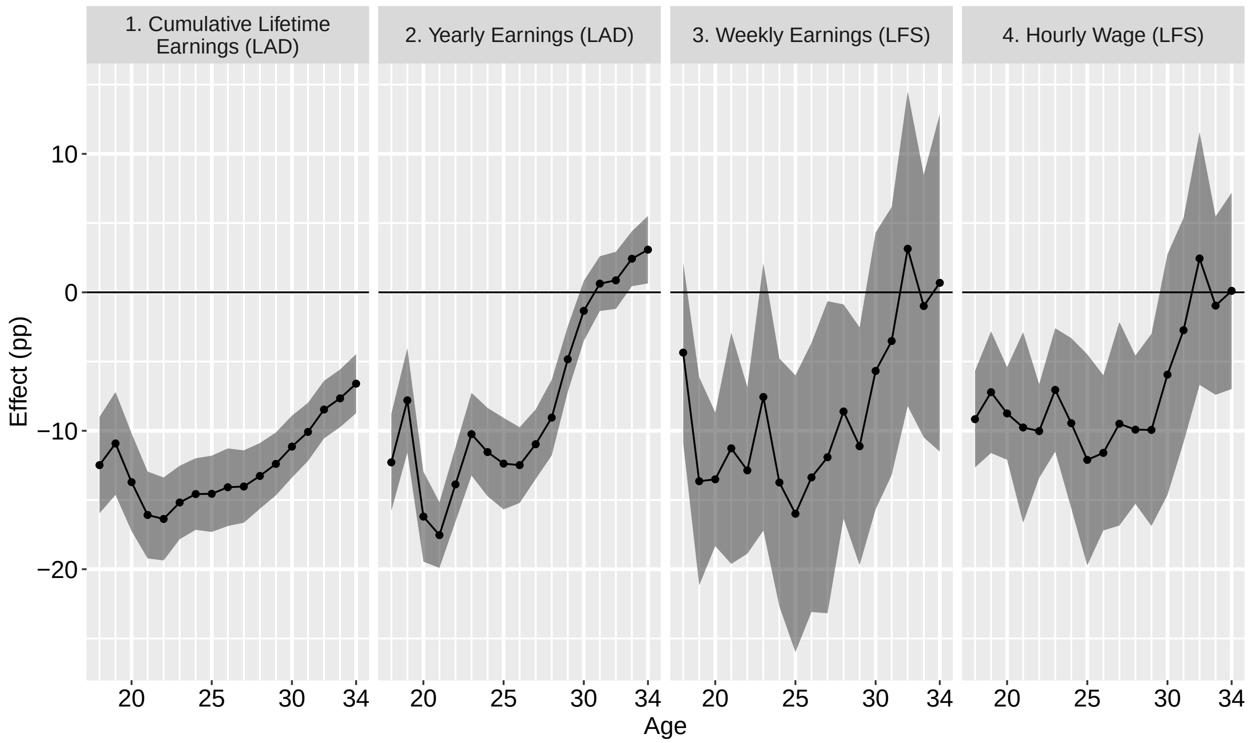
[16] See, for example DesJardins et al. (2006)

[17] See, for example Statistics Canada (2025). For Ontario, secondary schooling costs per pupil per year in 2022–2023 were less than half that of the corresponding costs for postsecondary schooling.

[18] These trends are most likely related to an economic convergence between other provinces and Ontario, historically the richest Canadian province. These trends have been documented for Québec (Fortin, 2001) and other provinces, in particular the Prairie provinces which have benefited from a natural resource boom (Brown and Macdonald, 2015).

[19] Given the issues with pre-trends, we pay particular attention to synthetic-control-weighted difference-in-differences estimates for wages and earnings. A more detailed discussion of the results yielded by this methodology is presented in Section 7.4.

Figure 1: Earnings Age Profiles



This figure shows estimates of the policy effect on earnings profiles. Each panel represents a different estimation of 2 using the LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

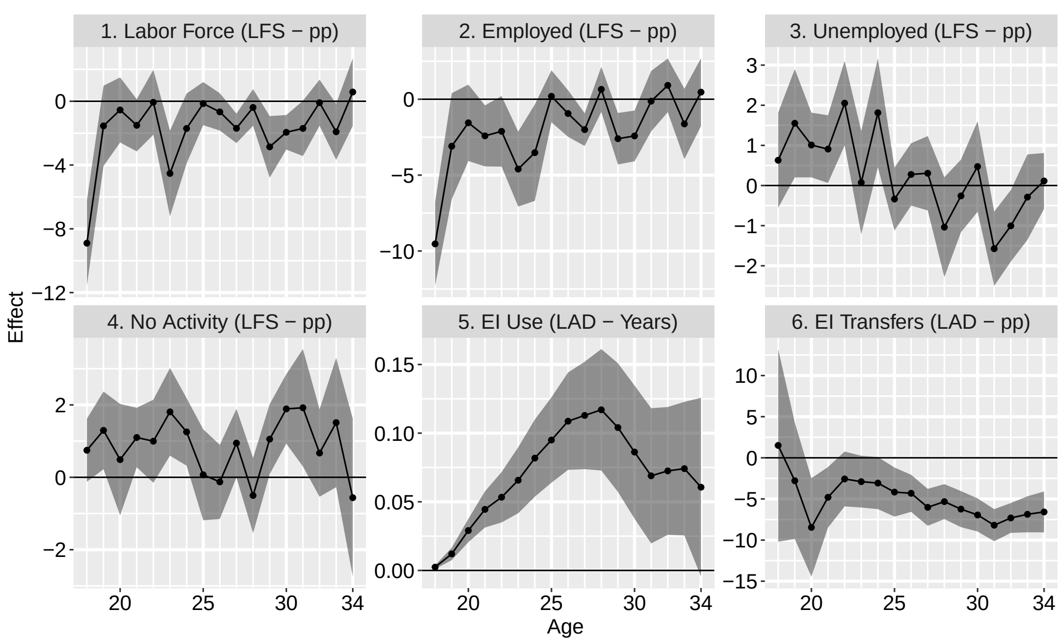
noticeably lower at younger ages: roughly 15 pp when measured at ages 21–22. This gap narrows as individuals reach their early thirties, shrinking to 6 pp. This pattern reflects sizeable negative effects on yearly earnings at ages 18 to 25 followed by a gradual recovery with age (second panel of Figure 1). These yearly earnings profiles provide the clearest view of how the reform shifted the timing of earnings accumulation. Discontinuity-design (DRDD) estimates on lifetime (age-30) cumulative earnings (Tables B.14 and B.15) confirm the negative direction on log cumulative earnings but show small positive coefficients on the cumulative number of years with positive earnings. This is not at odds with the age-profile result: the policy delays labor-market entry and depresses earnings in the early career, but by age 30 affected cohorts have accumulated comparable (and in some specifications slightly more) years of positive earnings as their lifetime profile catches up. The age-profile of yearly earnings, not the lifetime cumulative total, is what carries the policy effect.

The LFS data show the same pattern. Weekly earnings exhibit early negative effects that diminish over time (third panel), consistent with the tax data. These differences could arise from reduced hours worked, lower wage rates, or both. To separate these factors, we examine hourly wages in the LFS. The wage profile also shows negative effects at younger ages and a steeper trajectory later in the life cycle (fourth panel). This confirms that the results are not driven only by hours worked: the affected cohorts also earned lower wage rates during their early careers.

How can we explain this steepening of the wage and earnings profiles of young Ontarians following the policy? We examine two broad mechanisms that may shape these outcomes: labor market participation, which affects the accumulation of work experience, and occupational choice. On the one hand, because high school graduation rates declined and some students exited high school earlier, we would expect earlier entry into the labor market among students who are not bound for postsecondary education. For postsecondary-bound students, however, the age at final exit from postsecondary schooling remains unchanged after the reform. As a result, we would not expect systematic changes in labor market participation for this group, unless students use interruptions in their studies to accumulate additional work experience. At the same time, occupational choice may be adversely affected by lower educational attainment.

6.5 Labor Market Activities

Figure 2: Labor Market Activities



This figure shows estimates of the policy effect on labor market activities. Each panel represents a different estimation of 2 using the LFS or LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure 2 documents effects on labor market activities and shows a consistent downward shift in labor market attachment for affected cohorts. Panel 1 shows persistent reductions in labor force participation, while panel 2 shows corresponding declines in employment. Panels 3 and 4 indicate increases in unemployment and inactivity that extend into the late twenties and early thirties.

At age 18, labor force participation drops by about 8 pp (panel 1), which may partly reflect increased enrollment into postsecondary schooling. However, the negative effects extend well beyond the schooling margin. Between ages 18 and 24, employment is 1 to 10 pp lower (panel 2), and unemployment and inactivity rise by 1–2 pp (panels 3–4). These patterns align closely with the work–study evidence and with the slower accumulation of work experience documented above: students exposed to the reform enter the labor market later, and once they enter, they do so less stably. There is little heterogeneity in these results by achievement level. Splitting the sample by high school graduates and dropouts, we see similar patterns (Figures A.2 and A.3), the only substantive difference being that high school graduates' increases in unemployment occur later than those of high school dropouts', given the different timing of their entry in the labor market.

Panels 5 and 6 of Figure 2 reinforce this interpretation through Employment Insurance (EI) outcomes. Affected cohorts accumulate more years with positive EI benefits (panel 5), consistent with more frequent job separations, peaking between ages 25 to 30. Conditional on receiving EI, however, total EI transfers are lower (panel 6), a pattern more consistent with shorter job tenure and lower wages than with faster re-employment. Together, these results point to a labor market trajectory characterized by delayed entry, more instability, and weaker early-career outcomes for the post-reform cohorts.

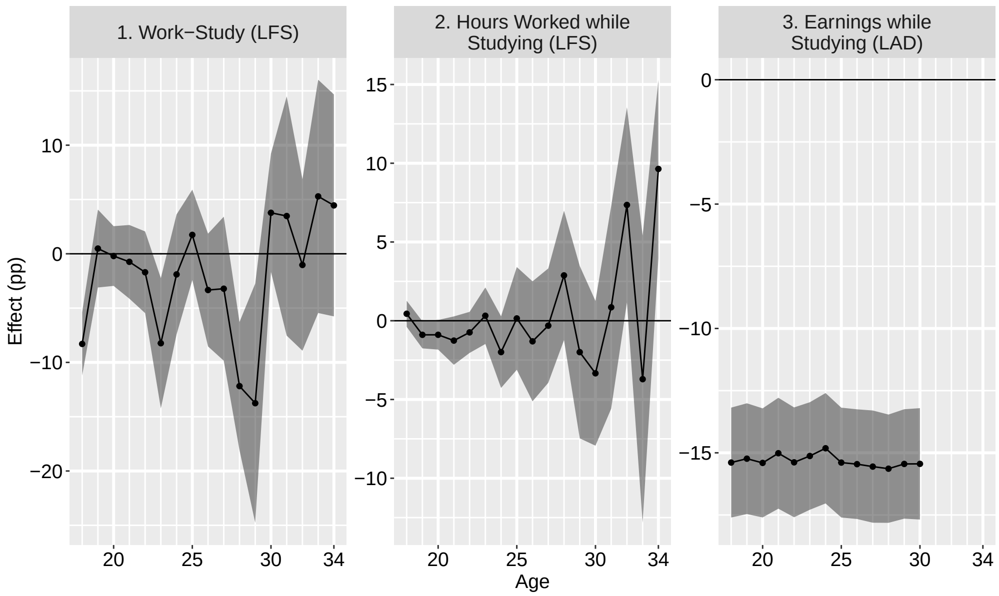
6.6 Work-Study Behavior

Next, we study work-study behavior. This is an important mechanism linking the fragmented postsecondary pathways to weaker labor market preparation. If students extend their time in postsecondary schooling and accumulate more interruptions, one potential offsetting channel would be increased work experience during these gaps or lighter academic periods.

Instead, the evidence points in the opposite direction. Figure 3 shows that students exposed to the policy are less likely to work while enrolled in school (panel 1), particularly at ages 18, 23, and 28–29. At other ages, the estimates are smaller and insignificant, but generally negative. Among those who do work while studying, total hours do not increase (panel 2), and earnings during school decline (panel 3). These lower in-school earnings are consistent with reduced hours, shorter job spells, or shifts toward lower-paying part-time positions.

In summary, students study for longer, interrupt their studies more often, but do not accumulate more work while in school. As a result, the longer and less structured enroll-

Figure 3: Work-Study Patterns



This figure shows estimates of the policy effect on work-study behavior. Each panel represents a different estimation of 2 using the LFS or LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

ment histories documented earlier do not translate into additional labor market experience, reinforcing the mechanism behind the slower buildup of work experience observed in the next section. These results also heavily imply that the policy led to more spells of inactivity among Ontario youth, a pattern we document in the next sections.

6.7 Work Experience

Consistent with previous findings, Figure 4 shows how these altered schooling and work–study patterns translate into the accumulation of work experience. Cumulative years with positive labor earnings fall at every age from 18 to 30 for affected cohorts (panel 1). Meanwhile, we document an increase of roughly 0.5 pp in the probability of never having entered the labor market between ages 20 and 25, pointing to delayed labor market entry (panel 2). By the early thirties, job tenures for the currently employed are 2.3 to 5.8 months shorter for Ontario youth exposed to the new curriculum (panel 3), which is consistent with the overall pattern of reduced and/or delayed labor market participation.

In summary, these patterns reveal that compressed high school combined with more drawn-out postsecondary enrollment reduces opportunities to accumulate early work experience. This slower buildup of experience is consistent with the earnings profiles documented earlier, where wage penalties are concentrated at younger ages.

Figure 4: Work Experience Age Profiles

This figure shows estimates of the policy effect on work experience profiles. Each panel represents a different estimation of 2 using the LFS or LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

6.8 Occupational Sorting

A final mechanism behind the earnings penalties is sorting into lower-skill occupations. Table 7 documents the policy effect on the gap between an individual's own years of schooling and the years of schooling required by their current occupation (as classified by the National Occupational Classification code). At ages 18–19, the effect is small and statistically insignificant, consistent with the bulk of this cohort still being in school or entering the labor market for the first time. From age 20 onward, however, affected cohorts are systematically employed in occupations requiring less education: the gap between own schooling and job requirement rises by 0.12 percentage points at ages 20–25 and by approximately 0.14–0.15 percentage points at older age groups, both statistically significant. This pattern is consistent with lower educational attainment channeling graduates into occupations with reduced qualification requirements, generating an educational mismatch that persists throughout early adulthood and contributes to the wage penalties documented above.[20]

[20]These patterns are not sensitive to excluding the double cohort. Table B.20 shows similar patterns when excluding these cohorts from the analysis.

Table 7: Policy Effect on Overqualification

	(1) Overqualification (Years)
Ages ≤ 19	-0.06
	(0.09)
Ages 20–25	0.12***
	(0.04)
Ages 26–29	0.15**
	(0.07)
Ages 30–35	0.14***
	(0.05)
N	1,187,500

This table shows the effect of the policy on the difference between own years of schooling and the years of schooling required by the occupation currently employed in, according to the NOC occupational codes. The coefficients are from estimating a version of equation 1, with the policy indicator interacted with different age groups in the LFS. The reference group is those aged under 18. Controls include gender, year of birth, survey year, province of residence at age 18, interactions between gender and year of birth, and survey year and year of birth. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.9 Heterogeneity in Policy Effects

We summarise here heterogeneity in the policy effects along three dimensions — household income, cohabitation status, and gender — following the methodology described in Section 5.4.[21]

Household income. The policy's effects on enrolment timing and postsecondary trajectories are larger for students from above-median income households. These students are more likely to transition to postsecondary schooling early, more likely to spend additional time enrolled, and more likely to experience study interruptions (Tables B.2 and B.3). The earn-

[21] For a table containing the complete list of heterogeneity and robustness checks, please see Table B.1 in the Appendix. Detailed tables and figures are reported in Appendices B.1 and B.2.

ings penalty, however, runs in the opposite direction: it is concentrated among low-income students, consistent with their lower postsecondary enrolment rates and weaker recovery in the early career (Figure B.1). Effects on labor market activity at ages 18–20 mirror the enrolment pattern: the declines in labor force participation and employment are concentrated among high-income students, who are reallocating from work to postsecondary schooling, while increases in inactivity are observed for both groups (Table B.4; Figure B.2). Work-experience losses are similarly concentrated among low-income youth; high-income individuals see an initial gain from early high-school graduation before negative effects emerge from age 22 onwards (Figure B.3). Cumulative EI receipt does not differ meaningfully by income. Taken together, these patterns suggest that the policy widened existing inequalities in access to postsecondary schooling while imposing the largest early-career earnings and work-experience costs on low-income youth.

Cohabitation status. Because the LFS measures parental household income only for young adults still cohabiting with their parents, we additionally split the LFS sample by cohabitation status to assess whether the income result generalises beyond the cohabiting subsample. Effects on high-school timing and age 18–20 enrolment are qualitatively similar for cohabiting and non-cohabiting young adults (Table B.5), suggesting that the headline timing results are not an artefact of selection into cohabitation.

Gender. Effects of the reform vary moderately by gender. At ages 18–20, female cohorts experience slightly larger short-term declines in postsecondary enrollment (Table B.6). Long-run educational attainment effects are similar across genders (Table B.7): both subsamples show comparable declines in attaining at least high school, some PSE, college, and university, with attainment-exactly effects close to zero for both groups. Policy effects on LAD-based postsecondary attendance histories are largely similar across genders, with the exception that females exit their first postsecondary spell roughly 0.09 years earlier than males under the policy (Table B.8). Females also experience slightly larger increases in economic inactivity at ages 18–20 (Table B.9). Earnings and labor-market trajectories show more pronounced heterogeneity. Yearly earnings paths diverge in the early thirties: males recover into positive territory while females remain below pre-reform trend (Figure B.4). Cumulative employment-insurance receipt rises more sharply for females than males after age 25 (Figure B.5). Cumulative work-experience losses are concentrated among females in the early twenties, with the male profile staying close to zero before the two groups converge by age 30 (Figure B.6).

7 Robustness Checks

We subject each of the main results to a battery of robustness checks. Table B.1 in the appendix summarizes which checks are performed for which outcome and, for the empty cells, the reason a given check is not feasible or not pursued. The subsections below describe each class of check; the heterogeneity analyses are summarised in Section 6.9 and reported in full in Appendix B.1 and B.2, and the regression-discontinuity validations in Appendix B.3.

7.1 Excluding the 1984 and 1985 Double Cohorts

As previously noted, a distinctive feature of the Ontario reform is the 2003 "double cohort," when both the final Grade 13 class (born in 1984) and the first four-year cohort (born in 1985) graduated simultaneously. Postsecondary capacity was temporarily expanded to accommodate this surge in demand, creating exceptional conditions for postsecondary entry. At the same time, many students from these cohorts completed high school and entered postsecondary education concurrently, generating a short-run oversupply of entrants. As shown in Figure 5, this also appears to have induced some students in the 1984 cohort to graduate early, likely to avoid the increased competition associated with the larger cohort size. To the extent that the 1984 cohort likely displays anticipation effects, the estimates presented in our paper so far are lower-bounds of the true treatment effects of this policy.

To further ensure that our results are not driven by these transitional cohorts, we re-estimate all main specifications after excluding the 1984 and 1985 cohorts. The estimates, reported in Appendix B.4, are virtually unchanged. Effects on high school completion, post-secondary attainment, and the timing of school-to-work transitions retain the same sign, magnitude, and statistical significance. The labor market age-profile results also exhibit the same steepening and early-career penalties documented in the main analysis. These findings confirm that the core results are not an artifact of the double cohort. Specifically, Tables B.16, B.17, B.18, B.19, and B.20 replicate the main HS-timing, HS-graduation, attainment, PSE-history, and overqualification tables; Figures B.7, B.8, B.9, B.10, and B.11 replicate the earnings, labor-activity, work-study, work-experience, and event-study figures.

7.2 Placebo Cohort Tests

To assess whether the estimated effects reflect spurious cohort trends rather than the policy itself, we conduct placebo difference-in-differences analyses in which treatment is artificially assigned to cohorts several years prior to the actual reform. Specifically, we reestimate equations 1 and 2 using the 1975 to 1980 birth cohorts and define the placebo birth year cutoff of the reform to be 1978.

Across all placebo specifications (shown in section B.5 of the Appendix), the estimated effects are typically zero, with some evidence of modest negative effects in postsecondary graduation rates for the post-1978 cohorts. Likewise, placebo labor market age-profile tests show no evidence of the steep earnings or wage shifts observed in the actual treated cohorts. Two qualifications are worth noting. First, some of the age-related postsecondary-history outcomes (age at first entry and first exit, total school months) show small but statistically significant placebo coefficients in Table B.24, indicating modest pre-trend variation in these timing measures; the magnitudes are several times smaller than the policy coefficients and do not reverse the qualitative pattern, but the precise PSE-timing estimates should be interpreted with this caveat in mind. Second, the placebo specification for overqualification (Table B.25) shows a sizable negative coefficient at ages ≤ 19 that mirrors the main result in the same age band, suggesting the youngest-age overqualification estimate may partly capture secular trends rather than the reform alone; the estimates for ages 20–35 remain clean in the placebo and provide more reliable evidence of the policy's occupational-sorting effect.

These placebo tests demonstrate that the empirical patterns documented in the main analysis arise only when treatment is assigned at the true reform boundary, reinforcing the interpretation that the curriculum change, not coincidental cohort differences, drives the observed effects. Tables B.21, B.22, B.23, and B.24 report the placebo DiD estimates for HS timing, HS graduation, attainment, and PSE histories; the placebo for overqualification is reported in Table B.25. The corresponding labor-market and event-study placebo figures are Figures B.12, B.13, B.14, B.15, and B.16.

7.3 Event Studies

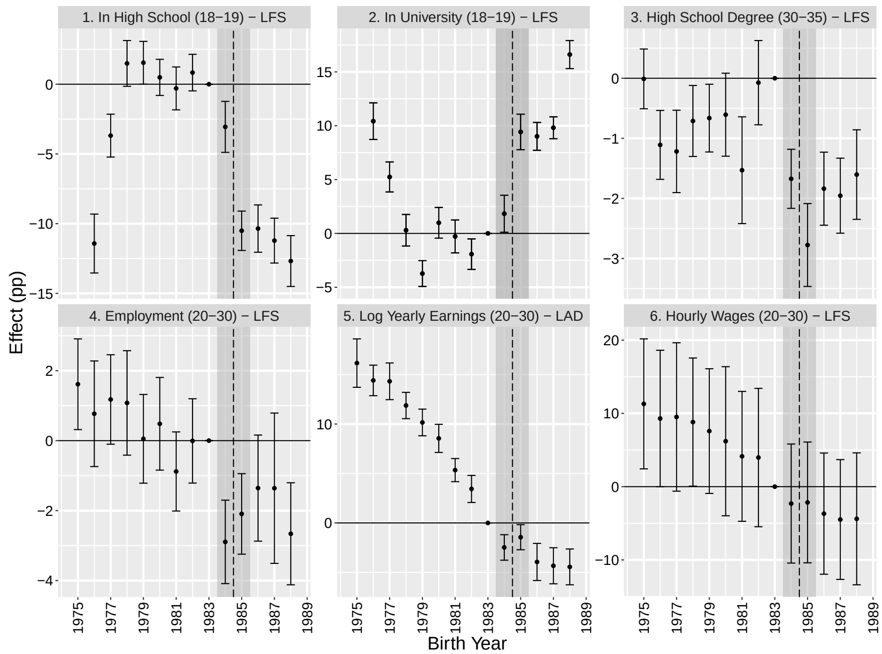
Figure 5 presents select event-study estimates for several key outcomes used to assess the validity of our empirical approach. The top 3 panels of the figure show key education outcomes, and the bottom 3 panels show key labor market outcomes.[22] The 1984-1985 double cohort is highlighted in grey.

In panels 1 and 2, we show that there is a 10–15 pp shift from high school to university enrollment at ages 18–19. Meanwhile, panel 3 highlights the 2 pp drop in high school graduation rates. There is no evidence of differential pre-trends in these outcomes. The 1984 cohort shows a small anticipation effect, which is consistent with students' incentives to finish high school early in order to avoid graduating as part of the double cohort, when competition for postsecondary seats and entry-level jobs was expected to be unusually high.

We now turn to the lower labor market-related event study plots. We show that the policy led to a 2–3 pp decrease in employment when measured at ages 20 to 30. Like other outcomes

[22] Section B.6 presents event studies for all outcomes explored in this paper, when possible.

Figure 5: Event Study Plots: Key Educational and Labor Market Outcomes



This figure shows event study plots for selected educational and labor market outcomes. The plotted coefficients represent the estimated effects of interactions between birth cohorts and residing in Ontario. The LFS regressions (panels 1–4 and 6) control for province and several variables interacted with birth cohorts: survey year, father's and mother's education, and gender. The LAD regression (panel 5) controls for province of residence at age 18 and several variables interacted with birth cohorts: gender and survey year. 90% confidence intervals are shown using error bars. The 1984 and 1985 double cohorts are highlighted in grey.

related to labor market activities, there is no presence of pre-trends in this outcome. Earnings and wages outcomes, however, do show the presence of pre-trends. More specifically, there is catch-up in wages and earnings by individuals from other provinces relative to Ontarians.[23] This diagnostic finding motivates our strategy for the labor market outcomes. Instead of interpreting effects in levels, we examine how the reform changed the shape of age profiles. That is, we trace how the earnings and wage trajectories of affected cohorts differ across the life cycle. This allows us to study timing and slope differences without requiring parallel pre-reform levels.

Overall, the event studies support our empirical strategy. The education outcomes satisfy the assumptions of the DiD approach, while the labor market outcomes require an age-profile specification. In both cases, the timing and direction of the shifts align with the expected consequences of the reform. Appendix B.6 provides event studies for the remaining outcomes, including those examined in the mechanism sections. Figures B.17, B.18, B.19,

[23]As mentioned in Section 6.4, this is probably mainly due to the natural resource boom in the Prairie provinces.

and B.20 report the event-study plots for HS timing, HS graduation, long-run attainment, and earnings; Figures B.21, B.22, B.23, and B.24 report the corresponding placebo event studies, which show no analogous pre-trend or jump at the placebo cutoff.

7.4 Synthetic-Control-Weighted Difference-in-Differences

As a final robustness check, we re-estimate our main results using a synthetic-control-weighted difference-in-differences estimator, which constructs province weights using the synthetic control method of Abadie and Gardeazabal (2003) and then runs a difference-in-differences regression on the reweighted panel; results are reported in Appendix B.7. Rather than relying on parallel trends across all comparison provinces, this approach constructs a synthetic Ontario by reweighting other Canadian provinces[24] to match Ontario's pre-reform outcome trajectory. The data-driven weighting ensures that the pre-treatment paths of Ontario and its synthetic counterpart align as closely as possible before the reform. Figures B.25–B.31 show the pre-trend validation plots for each outcome, confirming close pre-reform alignment between Ontario and synthetic Ontario.

The synthetic-control-weighted DiD estimates, reported in Appendix Tables B.26–B.32, are broadly consistent with the main DiD results across all outcomes. The sign, magnitude, and statistical significance of the effects on time spent in high school, postsecondary enrollment and attainment, earnings, and work experience are preserved under this approach. This consistency supports the robustness of our findings to alternative identification assumptions and confirms that the main results are not an artifact of the composition of the comparison group or of province-level trends unrelated to the reform. The pre-trend validation plots are reported in Figures B.25, B.26, B.27, B.28, B.29, B.30, and B.31; the corresponding SDID estimates appear in Tables B.26, B.27, B.28, B.29, B.30, B.31, and B.32.

8 Conclusion

This paper studies the effects of Ontario's 1999 elimination of Grade 13 — a reform that compressed five years of high school into four while explicitly redesigning the curriculum with the goal of maintaining comparable learning intensity. This feature allows us to study the effect of curriculum length without the mechanical increase in required instructional hours per credit that confounds prior reforms in other countries. Using difference-in-differences and age-profile estimators on rich Canadian administrative and survey data, we find that the reform had broad and largely unintended consequences.

[24]As in our other analyses, we exclude Québec and the territories from the control group.

The reform achieved its immediate goal of reducing time in high school: affected cohorts spent about 0.44 fewer years in high school and entered postsecondary institutions roughly a third of a year earlier. However, these earlier transitions came with costs. High school graduation rates fell by 1.6 to 7.0 percentage points. Postsecondary enrollment rose sharply at ages 18 and 19, but completion rates fell, with bachelor's degree attainment declining by approximately 1.8 percentage points. Affected cohorts also accumulated work experience more slowly, spent more time economically inactive, and faced persistently lower earnings throughout their twenties. These labor market disruptions are explained, in part, by increased sorting into occupations requiring lower levels of education.

We find little evidence that these effects operated through broadly reduced learning. Course failure rates did not rise, and math ability at ages 16–17 was, if anything, slightly higher for the post-reform cohort. We do, however, observe a numeracy decline among low-achieving students at ages 20–21, suggesting that some vulnerable groups may have experienced weaker skill formation or persistence. Instead, changes in the timing and sequencing of schooling are likely the primary mechanism: tighter prerequisite structures forced students to make consequential educational decisions one year earlier, and the resulting postsecondary enrollment spells were more fragmented, more likely to be interrupted, and longer without producing more graduates. Heterogeneity analysis shows that students from lower-income households were more exposed to these adverse effects on attainment and early earnings, while students from higher-income households were better positioned to navigate the accelerated timeline.

These results carry broad lessons for education policy and offer a cautionary tale for policymakers considering similar reforms. They show that the structure and sequencing of a curriculum, not just its content, can substantially shape students' educational trajectories and labor market outcomes. Even when curricula are redesigned to preserve comparable instructional intensity, compressing the high school timeline can generate persistent disruptions by forcing earlier decision-making, tightening course pathways, and leaving students with fewer opportunities to course-correct before key transitions. Policies aimed at accelerating graduation should therefore account carefully for sequencing constraints and the readiness of students to make high-stakes decisions at younger ages.

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Online Appendix

A Supplemental Results

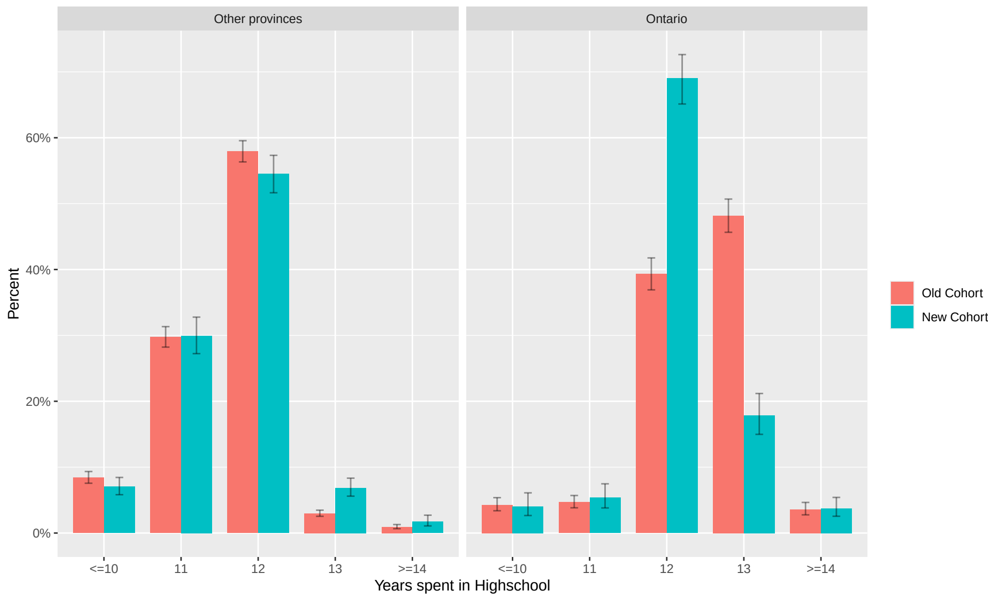


Figure A.1: Years Spent in High School

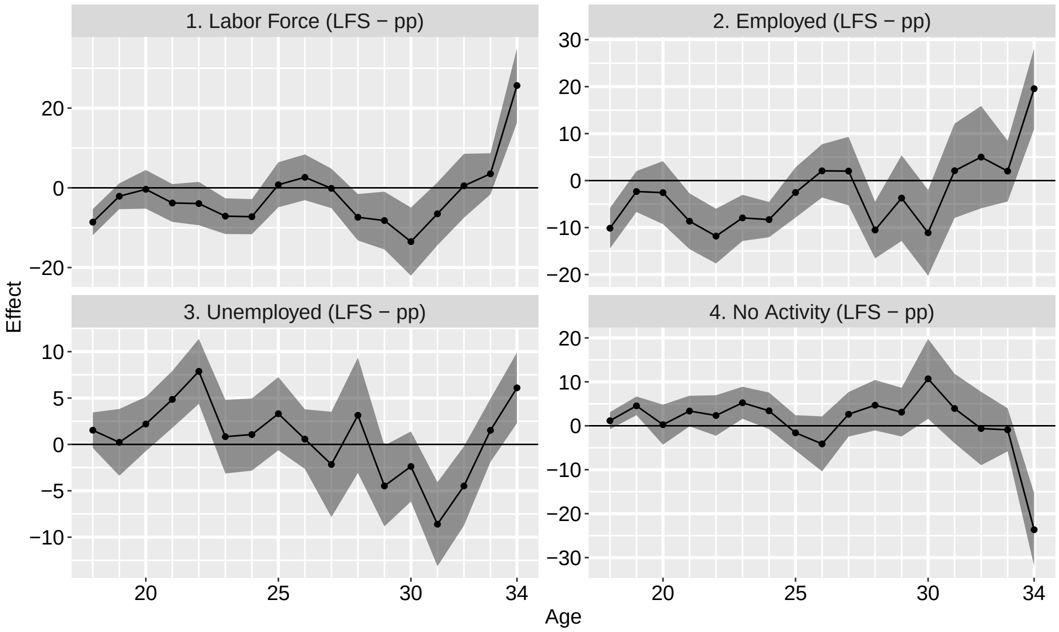
This figure shows the distribution of years spent in high school for all high school students in Ontario versus the rest of Canada using the SLID (1993–2011). Sample includes all the cohorts born between 1975 and 1988 who attended high schools in provinces other than Québec.

Table A.1: Years Spent in High School

	Total Years	*Time Spent in High School (Years)* 1	2	3	4	5	6
Policy	-0.437***	0.004	0.008	0.034***	0.325***	-0.370***	-0.004
	(0.004)	(0.004)	(0.005)	(0.010)	(0.018)	(0.017)	(0.006)
N	24,397	24,397	24,397	24397	24,397	24,397	24,397
R^2	0.004	0.005	0.006	0.018	0.181	0.237	0.019

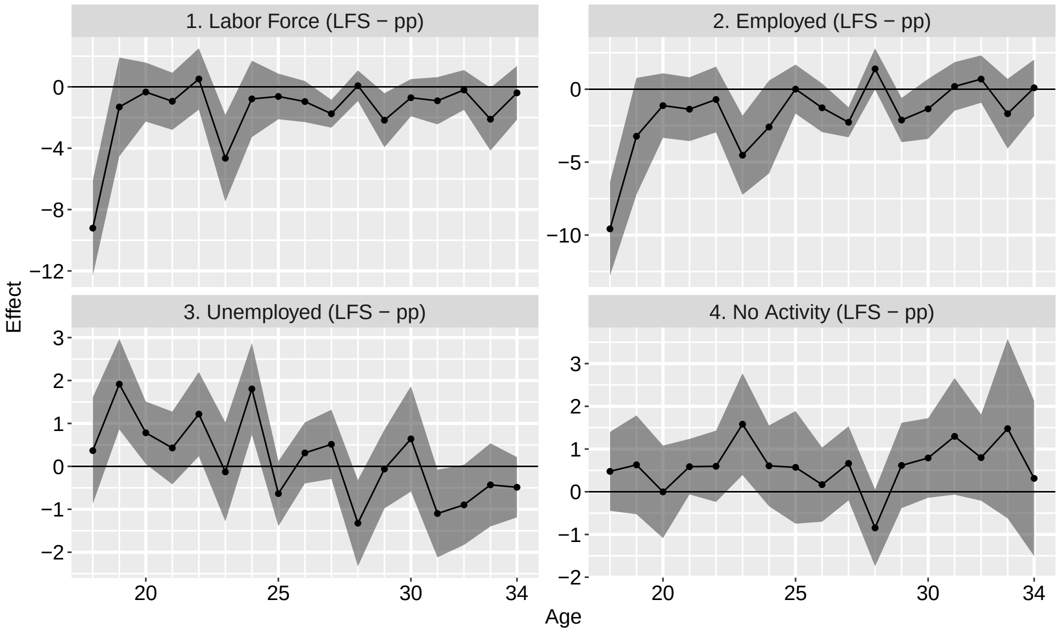
This table shows estimates of the policy effect on years spent in high school. Each column represents a separate estimate of equation 1, using the SLID (1993–2011). Sample includes all the individuals born between 1975 and 1988, aged 22–24, and who were not attending high school in the reference year. Other control variables include gender, immigrant status, year, cohort, and province of high school fixed effect. Standard errors are clustered at the province $\times$ cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.2: Labor Market Activities: High School Dropouts



This figure presents a reestimate of Figure 2 for high school dropouts only. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure A.3: Labor Market Activities: High School Graduates



This figure presents a reestimate of Figure 2 for high school graduates only. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

B Heterogeneity and Robustness Checks

Table B.1 summarizes which robustness checks are performed for each main result; the subsections that follow present the underlying tables and figures.

Table B.1: Robustness Coverage Across Main Results

	Dataset(s)	Excl. Double Cohort	Placebo Cohort	SDID	Het. Income	Het. Cohab.	Het. Gender	DRDD	Event Study
T1. HS timing	LFS, SLID	✓	✓	✓[i]	✓[i]	✓[i]	✓[i]	—[a]	✓[i]
T2. HS graduation	NLSCY, LFS, Census	✓[b]	✓[b]	—[b]	—[b]	—[b]	—[b]	✓[b]	✓[b]
T3. Course failure	NLSCY	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]
T4. Learning	NLSCY	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]	—[c]
T5. Attainment	LFS, Census, LAD	✓[i]	✓[i]	✓[i]	—[d]	—[d]	✓[i]	✓	✓[i]
T6. PSE histories	LAD	✓	✓	✓	✓	—[d]	✓	✓	—[e]
T7. Overqualification	LFS	✓	✓	—[f]	—[f]	—[f]	—[f]	—[a]	—[e]
F1. Earnings	LAD	✓	✓	✓	✓	—[d]	✓	✓[g]	✓
F2. Labor activities	LFS, LAD	✓	✓	✓	✓	—[d]	✓	—[h]	—[e]
F3. Work-study	LFS, LAD	✓	✓	✓	✓	—[d]	✓	—[h]	—[e]
F4. Work experience	LAD	✓	✓	✓	✓	—[d]	✓	✓[g]	—[e]

Notes: "✓" indicates the check is reported; "—" indicates it is not.

[a] LFS/SLID lack birth-month, so DRDD around the Jan 1985 cutoff is infeasible.

[b] T2 combines NLSCY, LFS, and Census; only the LFS-DiD column is re-estimated for DC/placebo, and only the Census column supports DRDD. The LFS HS-graduation effect also appears as column 2 of Table B.7.

[c] NLSCY (sole source for T3–T4) is too small for sub-sample slicing or alternative identification.

[d] Parental-income heterogeneity uses an indicator observed only at ages 14–17 in the LFS rotating panel; T5 attainment is measured at 30–35, so the two age windows do not overlap. Cohabitation heterogeneity is an LFS-only design and applies only to T1.

[e] Event studies are reported for the main outcomes (HS timing, HS graduation, attainment, earnings).

[f] Overqualification is a secondary finding; SDID and heterogeneity were not extracted from the RDC.

[g] DRDD applies to lifetime (age-30) summaries; the age-profile main-text figures are not re-estimated as DRDDs.

[h] F2/F3 are age-by-age outcomes; the DRDD at the 1985 birth-date cutoff does not apply.

[i] In multi-dataset rows, the check covers only the dataset that supports it (T1: LFS enrollment columns; T5: LFS-DiD portion of the main table).

B.1 Heterogeneity by Household Income and Cohabitation

Table B.2: Effects on High School Timing and Age 18–20 Enrollment: By Household Income (LFS, Cohabiting Students)

	(1) HS	(2) 2-yr College	(3) 4-yr Univ.
Policy	-10.95***	3.11**	9.02***
	(0.89)	(1.21)	(0.92)
Below Median Income	2.20***	0.67	1.98***
	(0.38)	(0.49)	(0.42)
Policy × Below Median Income	0.04	-1.24	-2.67***
	(0.44)	(1.54)	(0.95)
N	145,900	145,900	145,900

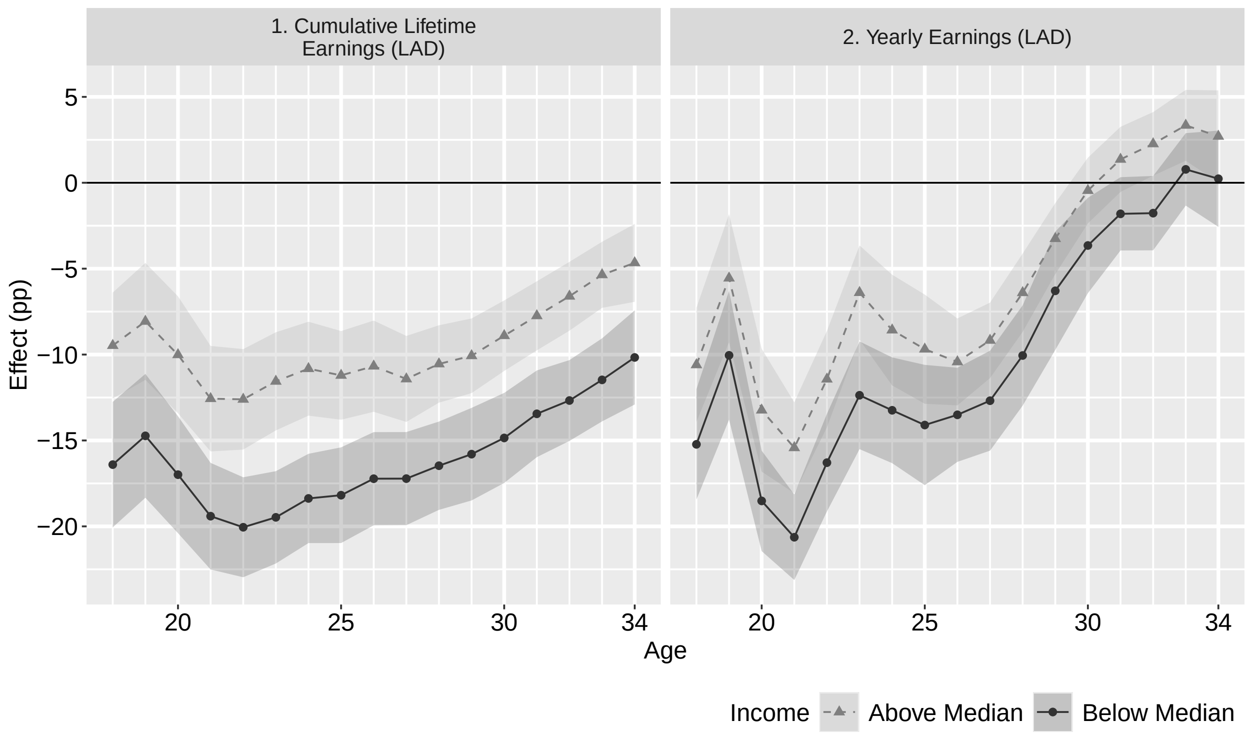
This table shows heterogeneity in the policy effect on enrollment at ages 18–20 by household income, using the LFS (restricted to cohabiting students). Each column represents a separate estimation of equation 2. The table reports the main policy effect, a Below Median Income indicator, and their interaction. Controls include gender, year of birth, survey year, and province of residence at age 18, and their interactions with birth cohort. Standard errors are clustered at the province × cohort level. This table is a companion to Table 1. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Postsecondary Attendance Histories (LAD): by Household Income

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy	-0.90**	-0.45***	-0.42***	-0.16***	1.67***	0.65**	0.11***
	(0.41)	(0.03)	(0.03)	(0.03)	(0.48)	(0.26)	(0.04)
Policy × Below Median Income	-0.49	0.25***	0.27***	0.14***	-0.33	-0.28	-0.10
	(0.55)	(0.05)	(0.04)	(0.05)	(0.60)	(0.33)	(0.06)
N	250,400	185,200	180,600	185,200	185,200	185,200	185,200

This table shows heterogeneity in the policy effect on postsecondary enrollment histories by household income, using the LAD. Each column represents a separate estimation of equation 1. The table reports the main policy effect and its interaction with a Below Median Income indicator. Controls include gender, year of birth, survey year, and province of residence at age 18, and interactions between gender and year of birth and survey year and year of birth. This table is a companion to Table 6. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.1: Earnings Age Profiles: Heterogeneity by Household Income



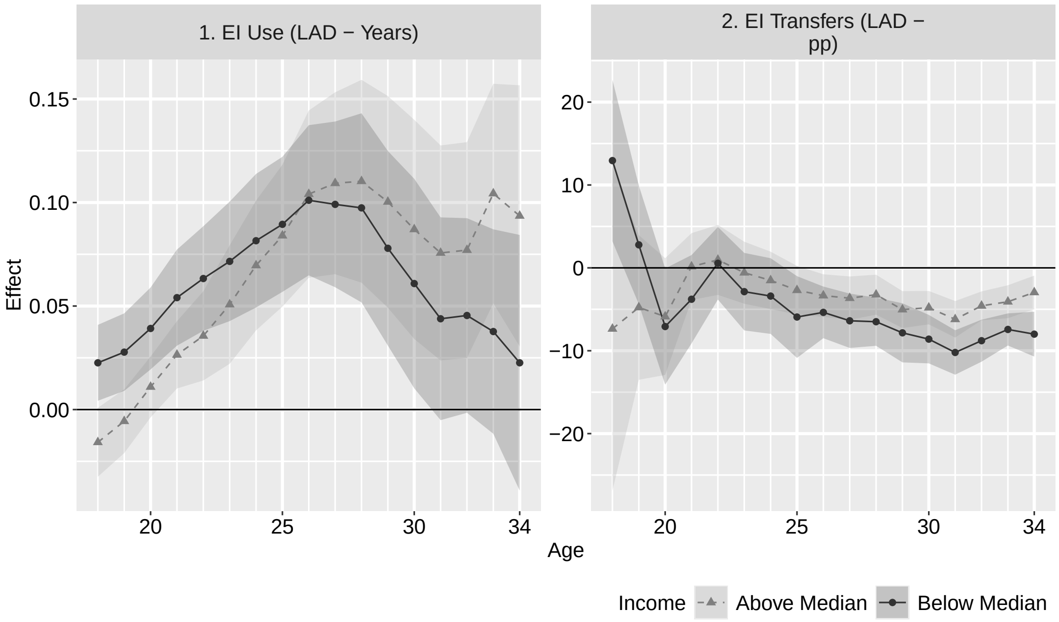
This figure shows estimates of the policy effect on earnings profiles by household income. Each panel represents a different estimation of equation 2 using the LAD, interacted with a Below Median Income indicator. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Table B.4: Labor Market Activities: Heterogeneity by Household Income

	(1) Labor Force	(2) Employed	(3) Unemployed	(4) No Activity
Policy	-3.19***	-3.65**	0.45	0.95**
	(0.93)	(1.40)	(0.69)	(0.41)
Below Median Income	-8.07***	-9.64***	1.57***	1.38***
	(0.44)	(0.50)	(0.29)	(0.21)
Policy × Below Median Income	3.72***	3.29**	0.43	-0.10
	(1.02)	(1.38)	(0.61)	(0.43)
N	145,900	145,900	145,900	145,900

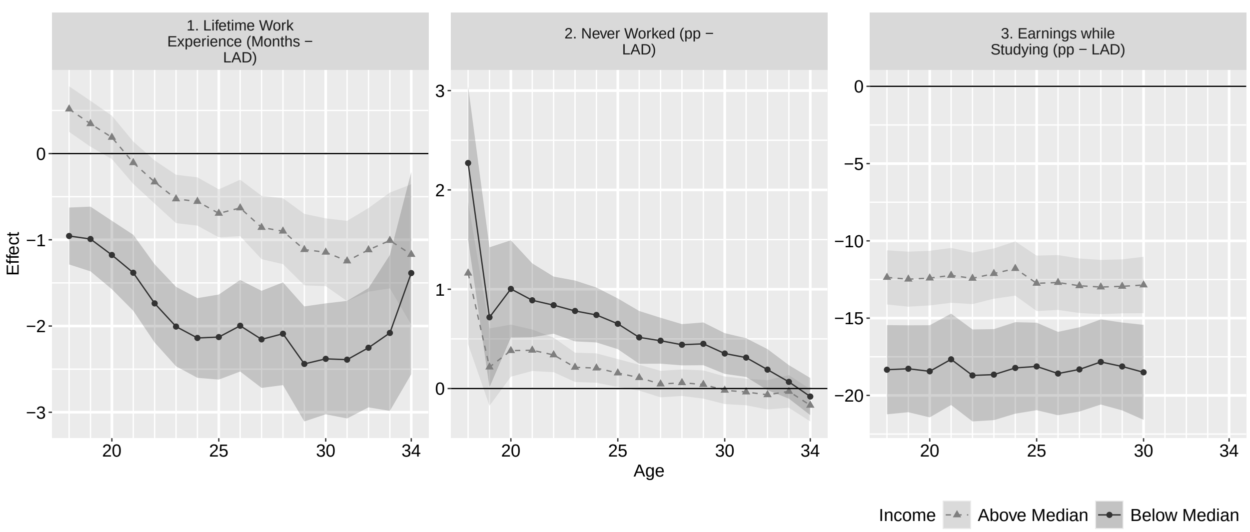
This table shows heterogeneity in the policy effect on labor market activities by household income, using the LAD ($N = 145{,}900$). Each column represents a separate estimation of equation 1. The table reports the main policy effect, a Below Median Income indicator, and their interaction. Controls include gender, year of birth, survey year, and province of residence at age 18, and interactions between gender and year of birth and survey year and year of birth. This table is a companion to Figure 2. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.2: Labor Market Activities: Heterogeneity by Household Income



This figure shows estimates of the policy effect on labor market activities by household income. Each panel represents a different estimation of equation 2 using the LAD, interacted with a Below Median Income indicator. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.3: Work-Study Patterns: Heterogeneity by Household Income



This figure shows estimates of the policy effect on work–study behavior by household income. Each panel represents a different estimation of 2 using the LFS or LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Table B.5: Effects on High School Timing and Age 18–20 Enrollment: By Cohabitation Status

	(1) HS	(2) 2-yr College	(3) 4-yr Univ.
Policy	-10.08***	2.89***	8.27***
	(0.76)	(0.66)	(1.22)
Without Parents	4.06***	1.65***	6.92***
	(0.50)	(0.54)	(0.49)
Policy × Without Parents	4.30***	-1.94**	-0.03
	(0.66)	(0.85)	(2.15)
N	249,900	249,900	249,900

This table shows heterogeneity in the policy effect on enrollment at ages 18–20 by cohabitation status, using the LFS. Each column represents a separate estimation of equation 2. The table reports the main policy effect, a Not Cohabiting indicator, and their interaction. Controls include gender, year, birth cohort, and province of high school fixed effects, and their interaction with birth cohort. Standard errors are clustered at the province × cohort level. This table is a companion to Table 1. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Heterogeneity by Gender

This appendix reports heterogeneity in the policy effects by gender. We re-estimate equations 1 and 2 interacting the policy with a Female indicator and present the resulting tables and figures below. A brief summary of the findings is provided in Section 6.9.

Table B.6: Effects on High School Timing and Age 18–20 Enrollment: By Gender

	(1) HS	(2) 2-yr College	(3) 4-yr Univ.
Policy (Male)	0.01	0.13	-0.28
	(0.06)	(0.23)	(0.35)
Policy (Female)	0.09	-0.41**	-0.15
	(0.07)	(0.20)	(0.34)
N	381,200	381,200	381,200

This table shows heterogeneity in the policy effect on time in high school and on enrollment at ages 18–20 by gender, using the LFS and SLID. Each column represents a separate estimation of equation 2. The table reports the main policy effect, a Female indicator, and their interaction. Controls include year, birth cohort, and province of high school fixed effects, and their interaction with birth cohort. Standard errors are clustered at the province $\times$ cohort level. This table is a companion to Table 1. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Policy Effect on Long-Run Educational Attainment: By Gender

	(1) HS Dropout	(2) HS Graduate	(3) Some PSE	(4) College	(5) University
Attained Exactly					
Policy (Male)	1.59***	0.33	0.54	0.12	-1.85**
	(0.33)	(0.74)	(0.39)	(0.93)	(0.83)
Policy (Female)	1.60***	-0.65	1.20***	-1.18	-1.89*
	(0.44)	(0.55)	(0.43)	(0.87)	(1.00)
Attained At Least					
Policy (Male)		-1.59***	-1.92***	-2.46***	-2.58***
		(0.33)	(0.72)	(0.64)	(0.94)
Policy (Female)		-1.60***	-0.95	-2.14**	-0.96
		(0.44)	(0.74)	(0.93)	(1.02)
N	381,200	381,200	381,200	381,200	381,200

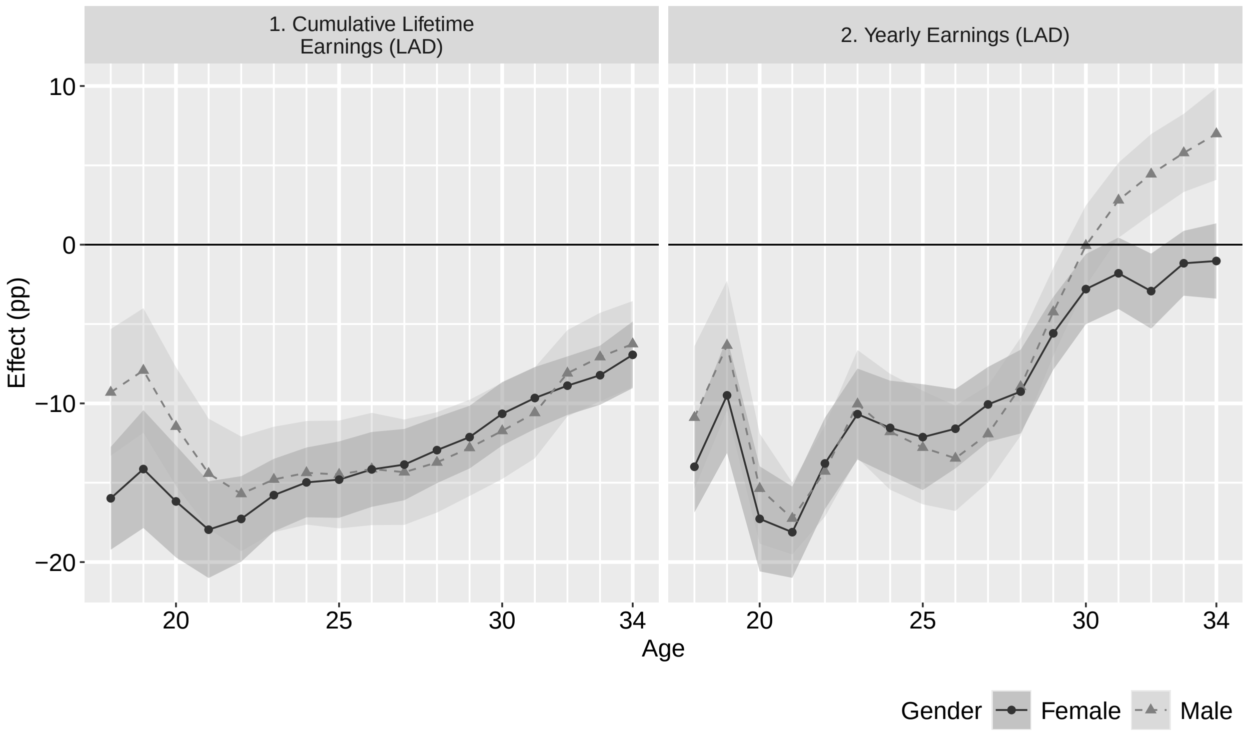
This table shows heterogeneity in the policy effect on long-run educational attainment by gender, using the LFS. Each column represents a separate stratified estimation of equation 1 on the male and female subsamples. This table is a companion to Table 5. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Postsecondary Attendance Histories (LAD): by Gender

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy (Male)	-1.65***	-0.35***	-0.25***	-0.05	1.30***	1.15***	0.14***
	(0.46)	(0.04)	(0.03)	(0.03)	(0.49)	(0.23)	(0.04)
Policy × Female	0.03	0.02	-0.07**	-0.07	0.82	-0.76*	-0.05
	(0.45)	(0.03)	(0.03)	(0.05)	(0.69)	(0.40)	(0.05)
N	391,900	277,300	269,600	277,300	277,300	277,300	277,300

This table shows heterogeneity in the policy effect on postsecondary enrollment histories by gender, using the LAD. Each column represents a separate estimation of equation 1. The table reports the main policy effect, a Female indicator, and their interaction. Controls include year of birth, survey year, and province of residence at age 18, and interactions between survey year and year of birth. This table is a companion to Table 6. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.4: Earnings Age Profiles: Heterogeneity by Gender



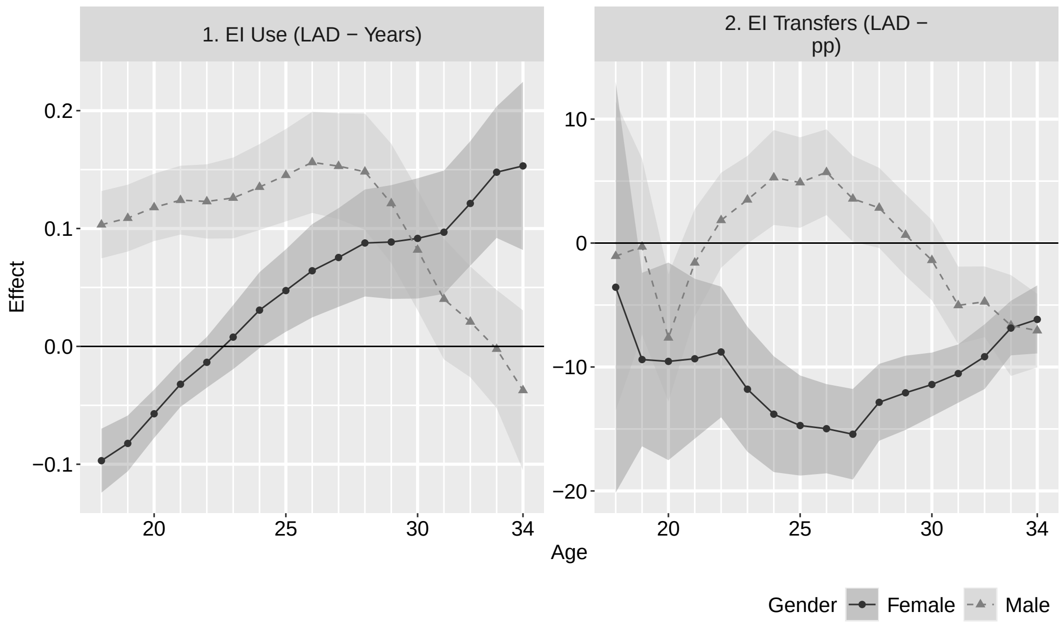
This figure shows estimates of the policy effect on earnings profiles by gender. Each panel represents a separate estimation of equation 2 using the LAD, interacted with a Female indicator. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p \times a}$.

Table B.9: Labor Market Activities: Heterogeneity by Gender

	(1) Labor Force	(2) Employed	(3) Unemployed	(4) No Activity
Policy (Male)	-1.12*	-0.30	-0.82**	1.06**
	(0.60)	(0.88)	(0.41)	(0.50)
Policy (Female)	-1.33*	-1.11	-0.22	1.59*
	(0.78)	(1.12)	(0.47)	(0.86)
N	381,200	381,200	381,200	381,200

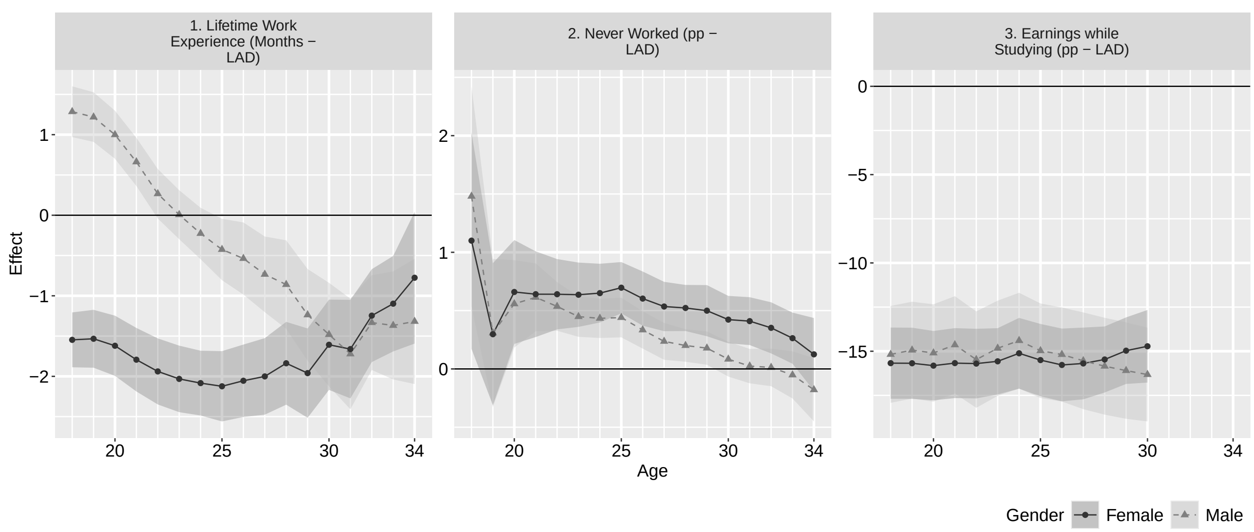
This table shows heterogeneity in the policy effect on labor market activities by gender, using the LAD. Each column represents a separate estimation of equation 1. The table reports the main policy effect, a Female indicator, and their interaction. Controls include year of birth, survey year, and province of residence at age 18, and interactions between survey year and year of birth. This table is a companion to Figure 2. Standard errors are clustered at the province × cohort level. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.5: Labor Market Activities: Heterogeneity by Gender



This figure shows estimates of the policy effect on labor market activities by gender. Each panel represents a separate estimation of equation 2 using the LAD, interacted with a Female indicator. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.6: Work-Study Patterns: Heterogeneity by Gender



This figure shows estimates of the policy effect on work–study behavior by gender. Each panel represents a separate estimation of equation 2 using the LFS or LAD. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

B.3 Difference in Regression Discontinuity Design (DRDD)

The second methodological approach employed is the Difference in Regression Discontinuity Design (DRDD) centered around the school enrollment cutoff date. This strategy leverages the consistent January 1 enrollment cutoff date across Canadian provinces (excluding Québec), where children born in a given year typically begin first grade a year later than those born in December of the previous year.

Specifically, we examine the cohorts born in December 1984 (exposed to the 5-year high school system) and January 1985 (exposed to the 4-year high school system) in Ontario, which are nearly identical ex-ante. This allows for the application of a DRD model around this cutoff, comparing children born just after January 1, 1985, to those born just before, to discern the policy effect.

However, an additional consideration arises: children born in January tend to be relatively older within their grade, potentially influencing academic performance and long-term outcomes due to a phenomenon known as the relative age effect. Thus, the observed differences in outcomes between those born after the January 1, 1985 cutoff and those born in the final months of 1984 may stem from both the relative age effect and the policy itself.

We adopt the method employed, for example, by Malamud et al. (2023) to address the relative age effect, akin to a difference in regression discontinuity strategy. This method effectively isolates the policy effect by contrasting outcomes for individuals born after the January 1, 1985 cutoff (where both policy and relative age effects are present) with outcomes for those born after cutoff dates in other "control" years (where only the relative age effect is expected). This approach helps mitigate the influence of relative age, facilitating the estimation of the policy effect:

$$Y_i = \gamma_0 + \gamma_a \text{After}_i + \gamma_T \text{Treat}_i + \gamma_p \text{After}_i \times \text{Treat}_i + f(\text{day}_i) + \gamma_x X_i + \theta_k + \varepsilon_i \qquad \text{(A.1)}$$

Individuals are indexed by i and k stands for birth year cohort. Here, Treat is an indicator for being born on either side of the treatment cutoff of interest (January 1, 1985), After is an indicator for being born on or soon after January 1 (of any year), X is a matrix of individual demographic characteristics, θ_k are birth year fixed effects and finally, $f(day)$ is a polynomial in the running variable (the day of birth). The coefficient of interest is γ_p, which captures the impact of being born after the treatment school enrollment cutoff over and above the relative age effect observed in other cohorts. Lastly, we use different bandwidths around the enrollment cutoffs for robustness and conduct a test to rule out possible manipulations in the running variable (McCrary, 2008).

We restrict our analysis to individuals born solely in Ontario, excluding those born around the 1983 and 1985 enrollment cutoffs. These exclusions are made to avoid comparisons with the unique 1984–1985 double cohort, which may have influenced factors such as labor supply and university capacity. Our control cohorts consist of individuals born around January 1 from 1976 to 1983 and from 1987 to 1990, while the treatment cutoff is set at January 1, 1985.

We establish an alternative difference-in-regression-discontinuity design to validate our results. While the previous specification compares Ontario students across different cohorts, this new approach compares Ontario students born around the January 1985 cutoff to those in other provinces born around the same time. This setup helps eliminate the possibility of the results being influenced by a nationwide policy shock that affected students born in early 1985 but not those born in late 1984.

$$Y_i = \gamma_0 + \gamma_a \text{After}_i + \gamma_p \text{After}_i \times \text{Ontario}_i + f(\text{day}_i) + \gamma_x X_i + \theta_j + \theta_k + \varepsilon_i \tag{A.2}$$

In this analysis, we include all individuals born around January 1, 1985, across Canada (except for Québec). The variable "Ontario" indicates whether an individual attended school or resided in Ontario, depending on the dataset. Additionally, we incorporate θ_j, representing province fixed effects. The coefficient of interest, γ_p, captures the policy's impact on Ontarians born on or after January 1, 1985, accounting for the relative age effect observed in the same cohorts across other Canadian provinces.

The paper employs two distinct methodologies, each with its strengths and limitations. The Difference in Regression Discontinuity Design (DRDD) relies exclusively on the 1984–1985 double cohort, a unique group facing adjusted university seat availability and heightened labor market competition due to a surge in graduates. This method compares outcomes of cohorts born just days apart, minimizing the influence of external factors, and providing a more robust causal interpretation. However, it necessitates a sizable dataset with detailed birth date information, precluding its use in surveys like the LFS where such data is unavailable.

Conversely, the difference-in-differences (DiD) approach is more versatile, applicable to smaller datasets, and requires only birth year information. While less susceptible to confounding factors, it depends heavily on the parallel trends assumption and may be influenced by concurrent policies affecting the new cohorts. Unlike the DRDD, the DiD approach analyzes cohorts from 1975 to 1988, offering a broader perspective beyond the double cohort phenomenon.

Table B.10: Postsecondary Attendance Histories (LAD): DRD Ontario vs Other Provinces

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Post-Reform	2.59***	-0.31***	-0.25*	-0.03***	1.27***	-0.37***	-0.10***
	(0.03)	(0.00)	(0.03)	(0.00)	(0.01)	(0.00)	(0.00)
Ontario	-7.29***	-0.20***	0.27**	0.22***	-2.44***	1.58***	-0.03**
	(0.06)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Post-Reform × Ontario	0.78*	-0.17***	-0.16	-0.29***	-1.11***	0.76***	0.16***
	(0.09)	(0.00)	(0.03)	(0.00)	(0.01)	(0.00)	(0.00)
N	6,900	4,900	4,800	4,900	4,900	4,900	4,900

This table shows the effect of the policy on educational enrollment histories. Each column represents a different estimation of equation A.1 using the LAD. We use 30-day bandwidths around the enrollment birth date. Using 60-day and 90-day bandwidths (unreported) yields similar results. Controls include gender and province of residence. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: Postsecondary Attendance Histories (LAD): DRD Double Cohorts vs Other Ontarian Cohorts

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Post-Reform	2.52***	-0.17***	-0.17**	-0.13***	-0.22*	-0.62**	-0.09**
	(0.01)	(0.00)	(0.00)	(0.00)	(0.02)	(0.03)	(0.00)
Cutoff 1985	-6.35***	-0.76***	-0.02	0.03**	0.88**	4.90***	0.81***
	(0.01)	(0.00)	(0.02)	(0.00)	(0.02)	(0.03)	(0.00)
Post-Reform $\times$ Cutoff 1985	0.84***	-0.33***	-0.26*	-0.19***	0.41**	1.10**	0.16**
	(0.01)	(0.00)	(0.03)	(0.00)	(0.02)	(0.03)	(0.00)
N	28,400	20,900	20,400	20,900	20,900	20,900	20,900

This table shows the effect of the policy on educational enrollment histories. Each column represents a different estimation of equation A.2 using the LAD. We use 30-day bandwidths around the enrollment birth date. Using 60-day and 90-day bandwidths (unreported) yields similar results. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: Policy Effect on Long-Run Educational Attainment (LAD): DRD Ontario vs Other Provinces

	(1) Below HS	(2) HS Graduate	(3) Trade	(4) College	(5) University
Attained Exactly					
Policy	2.99***	-2.79***	-0.06	-2.06	0.32
	(0.81)	(0.92)	(0.77)	(1.46)	(1.63)
Attained At Least					
Policy		-2.99***	-2.94**	-2.89**	-0.57
		(0.81)	(1.44)	(1.42)	(1.67)
N	209,565	209,565	209,565	209,565	209,565

This table shows the effect of the policy on long-run educational attainment. Each column represents a different estimation of equation A.1 using the LFS. We use 30-day bandwidths around the enrollment birth date. Using 60-day and 90-day bandwidths (unreported) yields similar results. Controls include gender and province of residence. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Policy Effect on Long-Run Educational Attainment (LAD): DRD Ontario Double Cohorts vs Other Ontarian Cohorts

	(1) Below HS	(2) HS Graduate	(3) Trade	(4) College	(5) University
Attained Exactly					
Policy	4.18***	-4.62***	-0.32	-0.93	1.21
	(1.10)	(1.20)	(1.30)	(1.99)	(2.01)
Attained At Least					
Policy		-4.18***	-2.76	-2.44	-0.89
		(1.10)	(1.73)	(1.86)	(2.03)
N	30,770	30,770	30,770	30,770	30,770

This table shows the effect of the policy on long-run educational attainment. Each column represents a different estimation of equation A.2 using the LFS. We use 30-day bandwidths around the enrollment birth date. Using 60-day and 90-day bandwidths (unreported) yields similar results. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.14: Lifetime Labor-Market Outcomes (LAD): DRD Ontario vs Other Provinces

	(1) Log Cum. Earnings	(2) Years Earning > 0	(3) Log Cum. EI Receipts	(4) Years on EI	(5) Age First Worked	(6) Age Last Non-Worker
Post-Reform × Ontario	-8.94*	0.02**	32.02	0.00	0.10	0.03
	(1.02)	(0.00)	(14.48)	(0.00)	(0.06)	(0.02)
Post-Reform	19.51**	0.01*	19.17	0.01**	-0.45**	0.03
	(1.27)	(0.00)	(21.86)	(0.00)	(0.02)	(0.01)
N	5,800	6,900	100	6,900	6,400	6,800

This table shows the policy effect on lifetime labor-market outcomes (cumulative earnings, years with positive earnings, cumulative EI receipts, years on EI, age first worked, and age last as a non-worker) measured at age 30 in a discontinuity design comparing Ontarians born around the January 1, 1985 cutoff to individuals born in other provinces around the same date, using the LAD. We use 30-day bandwidths around the enrollment birth date. Companion to Figures 1 and 4. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15: Lifetime Labor-Market Outcomes (LAD): DRD Double Cohorts vs Other Ontarian Cohorts

	(1) Log Cum. Earnings	(2) Years Earning > 0	(3) Log Cum. EI Receipts	(4) Years on EI	(5) Age First Worked	(6) Age Last Non-Worker
Post-Reform × Cutoff 1985	-2.95	0.02***	7.38	0.01***	-0.15	0.06*
	(1.49)	(0.00)	(19.81)	(0.00)	(0.06)	(0.01)
Post-Reform	12.10***	0.00**	31.51*	0.00***	-0.20**	0.01
	(0.15)	(0.00)	(4.07)	(0.00)	(0.01)	(0.00)
Cutoff 1985	1.96	-0.01***	-9.30	-0.02***	-0.11	-0.22**
	(1.65)	(0.00)	(17.80)	(0.00)	(0.07)	(0.01)
N	33,100	39,400	400	39,400	36,400	39,000

This table shows the policy effect on lifetime labor-market outcomes measured at age 30 in a discontinuity design comparing Ontarian double-cohort (1985) births to surrounding Ontarian birth cohorts, using the LAD. We use 30-day bandwidths around the enrollment birth date. Companion to Figures 1 and 4. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Omitting the Double Cohort

Table B.16: Effects on High School Timing and Age 18–19 Enrollment

	(1) HS, Age 18	(2) HS, Age 19	(3) 2-yr College, Age 18	(4) 2-yr College, Age 19	(5) 4-yr Univ., Age 18	(6) 4-yr Univ., Age 19
Policy	-20.74***	-6.55***	5.15***	4.42***	19.25***	6.25***
	(1.42)	(1.15)	(0.98)	(1.06)	(1.62)	(1.85)
N	1,146,300	1,146,300	1,146,300	1,146,300	1,146,300	1,146,300

This table presents a reestimate of Table 1, excluding the double cohort.

Table B.17: Policy Effect on High School Graduation Rates

	(1) HS Graduation
Policy	-1.55***
	(0.31)
N	324,600

This table presents a reestimate of column 3 (LFS) of Table 2, excluding the double cohort. Only the LFS difference-in-differences specification supports this exclusion: the NLSCY columns (1–2) are too small once the double cohort is dropped, and the Census discontinuity columns (4–5) identify the policy effect at the 1985 cutoff and so cannot be re-estimated after removing the double-cohort birth years.

Table B.18: Policy Effect on Long-Run Educational Attainment

	(1) HS Dropout	(2) HS Graduate	(3) Some PSE	(4) College	(5) University
Attained Exactly					
Policy	1.55***	-0.13	0.68*	-0.46	-1.02
	(0.31)	(0.77)	(0.35)	(0.83)	(0.80)
Attained At Least					
Policy		-1.55***	-1.41*	-2.10***	-1.64
		(0.31)	(0.77)	(0.76)	(1.06)
N	324,600	324,600	324,600	324,600	324,600

This table is a reestimate of Table 5, excluding the double cohort.

Table B.19: Postsecondary Attendance Histories (LAD)

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy	-1.67***	-0.37***	-0.26***	-0.01	2.15***	1.21***	0.17***
	(0.55)	(0.05)	(0.02)	(0.02)	(0.52)	(0.22)	(0.04)
N	303,700	214,200	208,200	214,200	214,200	214,200	214,200

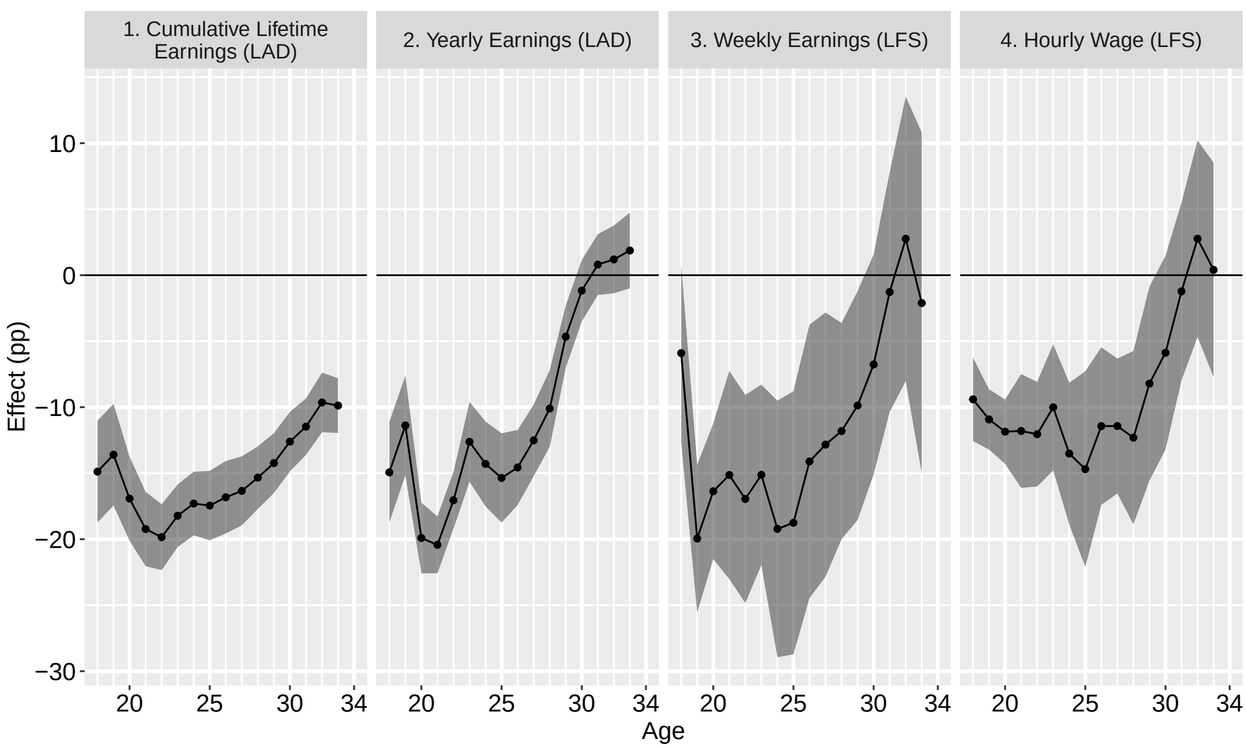
This table is a reestimate of Table 6, excluding the double cohort.

Table B.20: Policy Effect on Overqualification

	(1) Overqualification (Years)
Ages $\leq$ 19	-0.01
	(0.07)
Ages 20–25	0.15***
	(0.04)
Ages 26–29	0.16**
	(0.07)
Ages 30–35	0.14**
	(0.06)
N	1,011,400

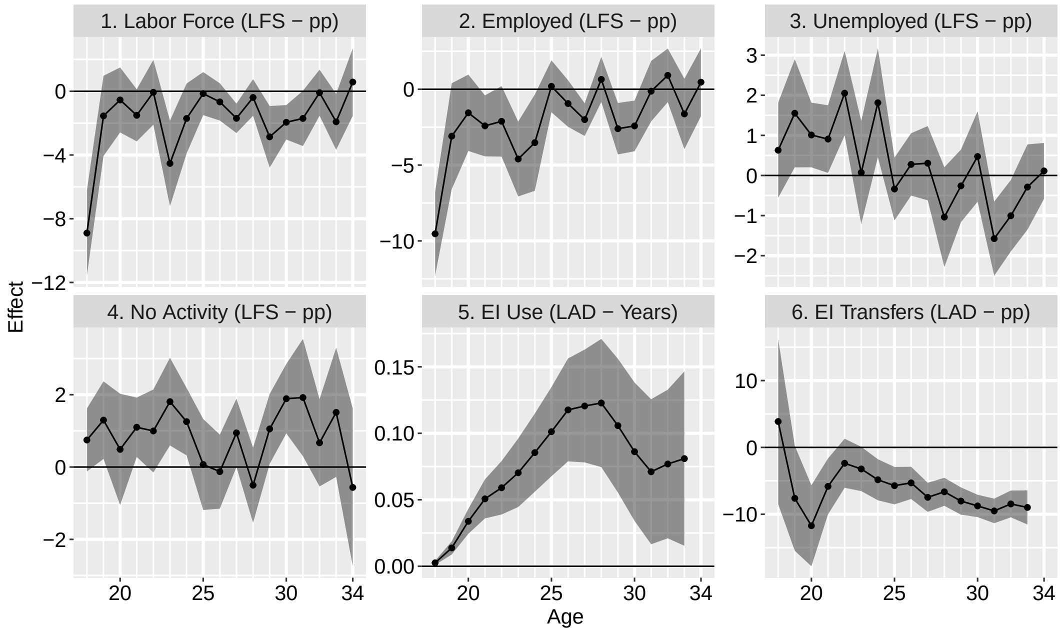
This table is a reestimate of Table 7, excluding the double cohort.

Figure B.7: Earnings Age Profiles



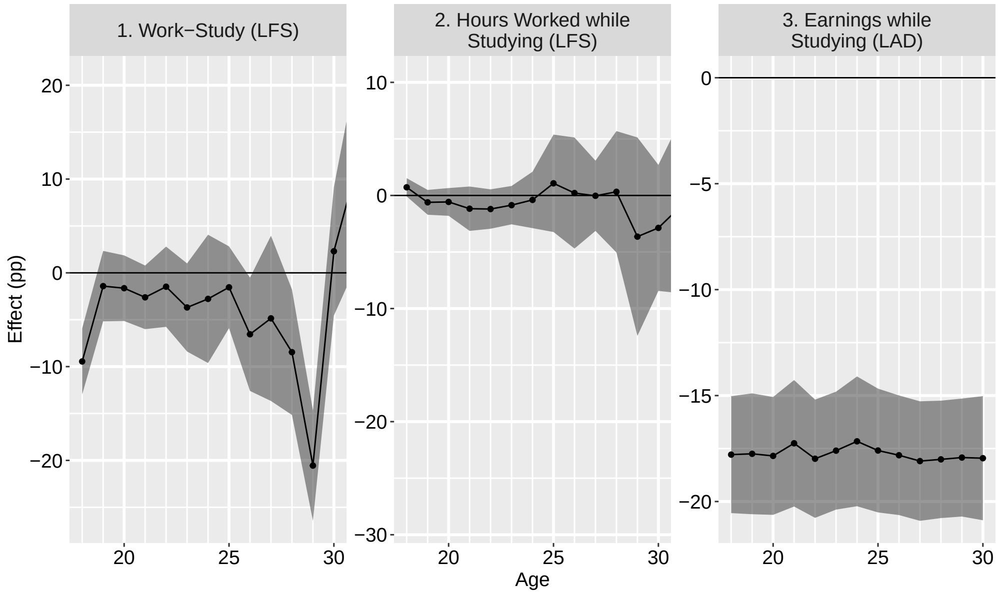
This figure is a reestimate of Figure 1, excluding the double cohort. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p \times a}$.

Figure B.8: Labor Market Activities



This figure is a reestimate of Figure 2, excluding the double cohort. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.9: Work-Study Patterns

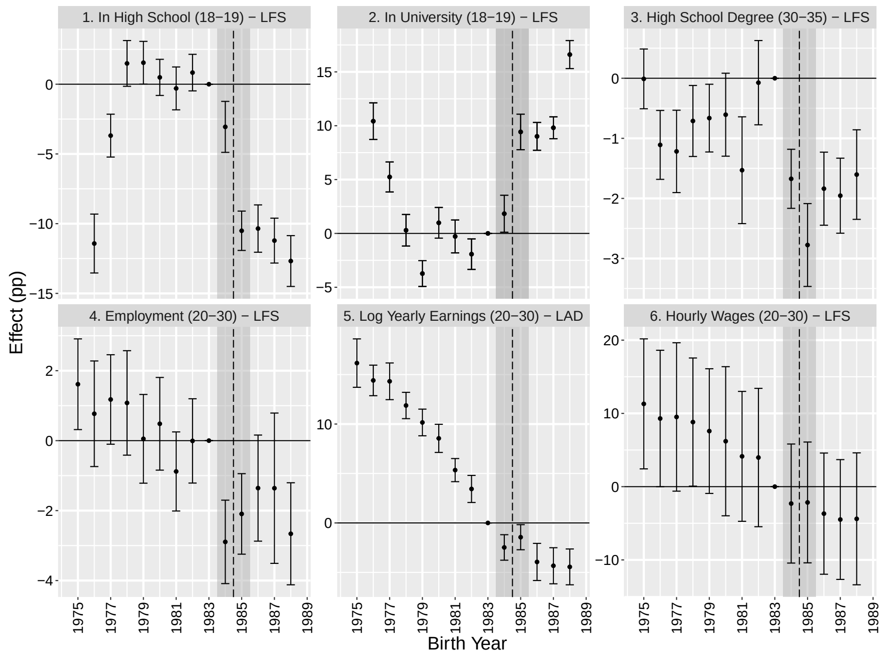


This figure is a reestimate of Figure 3, excluding the double cohort. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.10: Work Experience

This figure is a reestimate of Figure 4, excluding the double cohort. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.11: Event Study Plots: Key Educational and Labor Market Outcomes



This figure is a reestimate of Figure 5, excluding the double cohort. 90% confidence intervals are shown using error bars.

B.5 Placebo Checks

Table B.21: Effects on High School Timing and Age 18–19 Enrollment

	(1) HS, Age 18	(2) HS, Age 19	(3) 2-yr College, Age 18	(4) 2-yr College, Age 19	(5) 4-yr Univ., Age 18	(6) 4-yr Univ., Age 19
Policy	-1.01	-2.65**	2.68***	-1.80	2.18	-3.33***
	(1.21)	(1.16)	(0.49)	(1.46)	(1.97)	(1.14)
N	595,500	595,500	595,500	595,500	595,500	595,500

This table presents a reestimate of Table 1, using the 1975–1980 birth cohorts and a placebo policy year of 1978.

Table B.22: Policy Effect on High School Graduation Rates

	(1) HS Graduation
Policy	0.18 (0.29)
N	188,100

This table presents a reestimate of column 3 (LFS) of Table 2, using the 1975–1980 birth cohorts and a placebo policy year of 1978. Only the LFS difference-in-differences specification supports the placebo exercise: the NLSCY (columns 1–2) and Census discontinuity (columns 4–5) columns of Table 2 cannot be re-estimated with a placebo cutoff.

Table B.23: Policy Effect on Long-Run Educational Attainment

	(1) HS Dropout	(2) HS Graduate	(3) Some PSE	(4) College	(5) University
Attained Exactly					
Policy	-0.18	1.87***	0.65	-1.42*	-1.85***
	(0.29)	(0.45)	(0.59)	(0.76)	(0.57)
Attained At Least					
Policy		0.18	-1.69***	-2.33**	-0.91
		(0.29)	(0.59)	(0.88)	(1.12)
N	188,100	188,100	188,100	188,100	188,100

This table is a reestimate of Table 5, using the 1975–1980 birth cohorts and a placebo policy year of 1978.

Table B.24: Postsecondary Attendance Histories (LAD)

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy	0.24	0.14^{**}	0.14^{***}	0.07^{**}	-0.38	0.48^{*}	0.10^{*}
	(0.62)	(0.05)	(0.04)	(0.03)	(0.57)	(0.24)	(0.05)
N	135,400	94,100	91,400	94,100	94,100	94,100	94,100

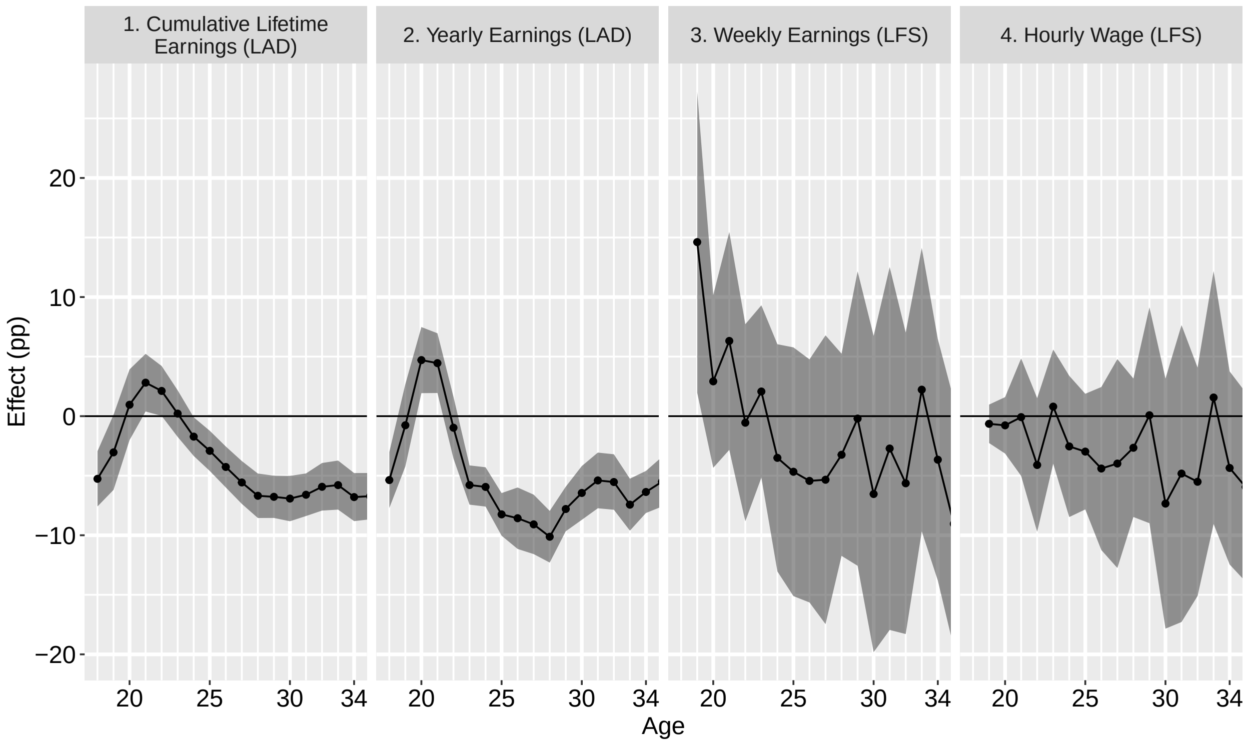
This table is a reestimate of Table 6, using the 1975–1980 birth cohorts and a placebo policy year of 1978.

Table B.25: Policy Effect on Overqualification

	(1) Overqualification (Years)
Ages $\leq$ 19	-0.68***
	(0.10)
Ages 20–25	-0.11
	(0.08)
Ages 26–29	0.06
	(0.09)
Ages 30–35	0.06
	(0.07)
N	521,800

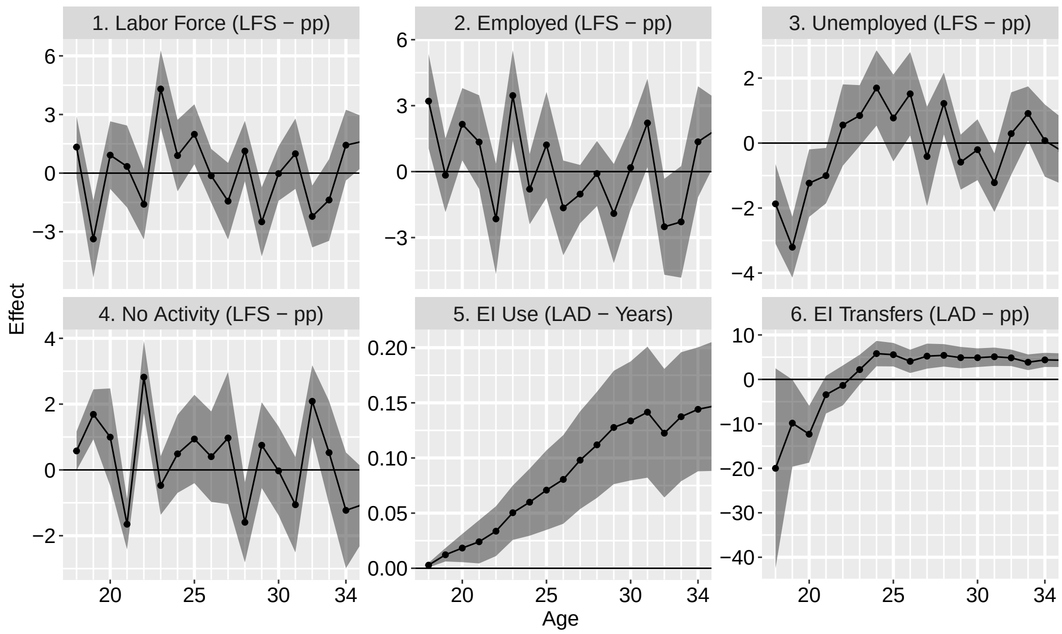
This table is a reestimate of Table 7, using the 1975–1980 birth cohorts and a placebo policy year of 1978.

Figure B.12: Earnings Age Profiles



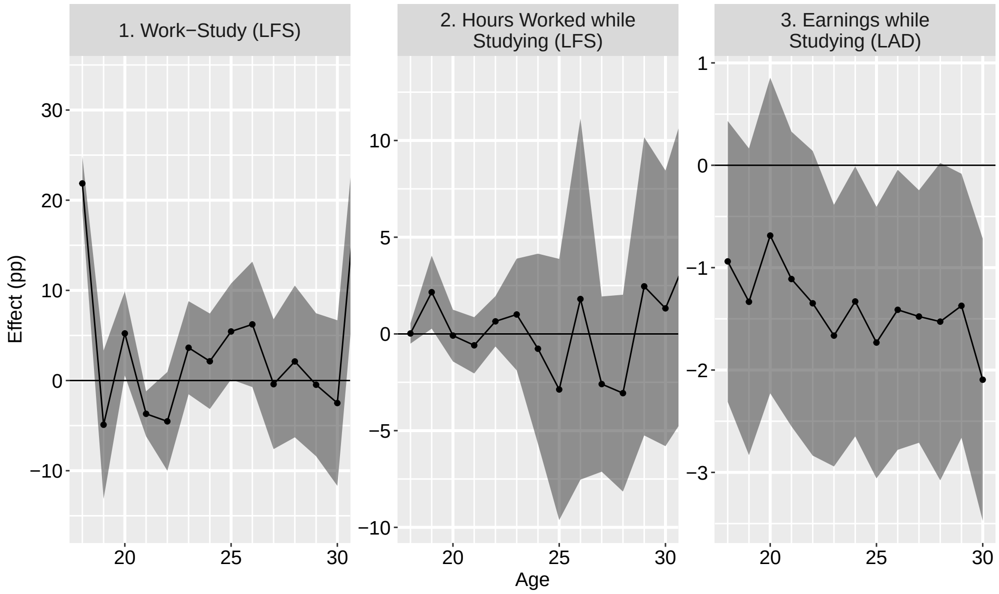
This figure is a reestimate of Figure 1, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.13: Labor Market Activities



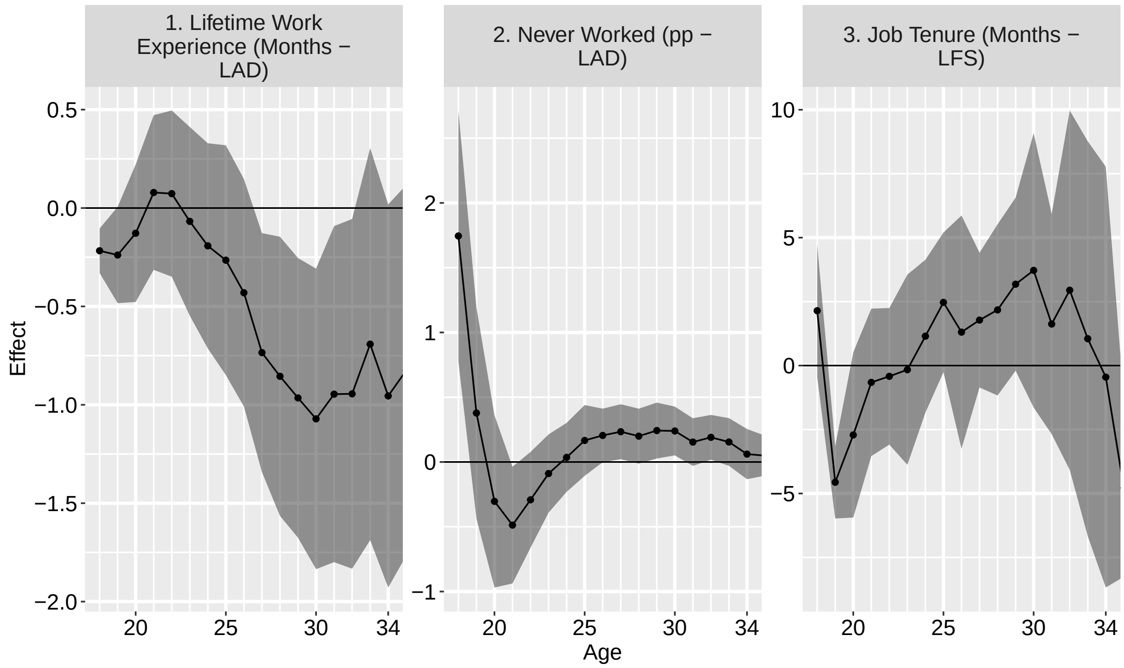
This figure is a reestimate of Figure 2, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p \times a}$.

Figure B.14: Work-Study Patterns



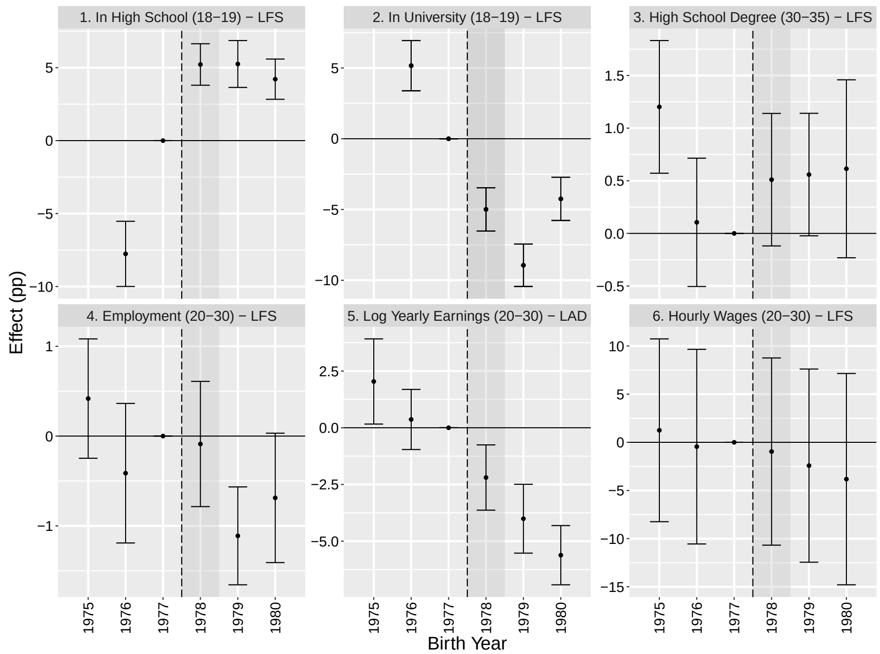
This figure is a reestimate of Figure 3, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

Figure B.15: Work Experience



This figure is a reestimate of Figure 4, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The gray areas represent 90% confidence intervals. The plotted estimates represent $\beta_{p\times a}$.

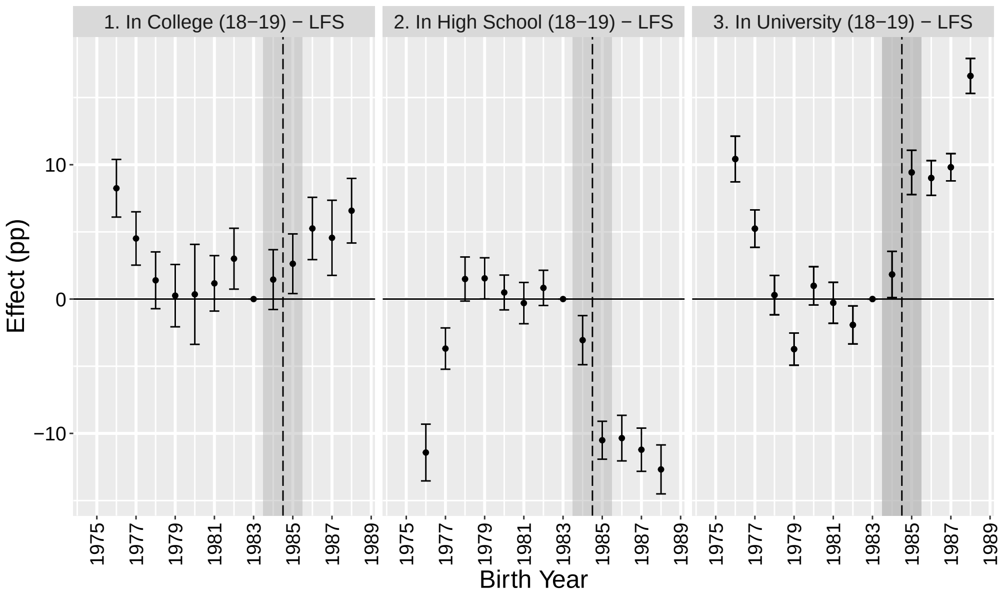
Figure B.16: Event Study Plots: Key Educational and Labor Market Outcomes



This figure is a reestimate of Figure 5, using the 1975–1980 birth cohorts and a placebo policy year of 1978. 90% confidence intervals are shown using error bars.

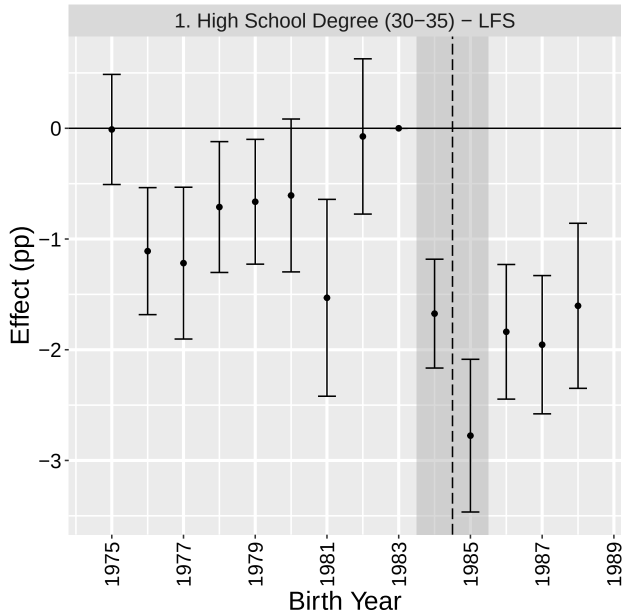
B.6 Additional Event Studies

Figure B.17: Event Study: High School Timing and Age 18–19 Enrollment



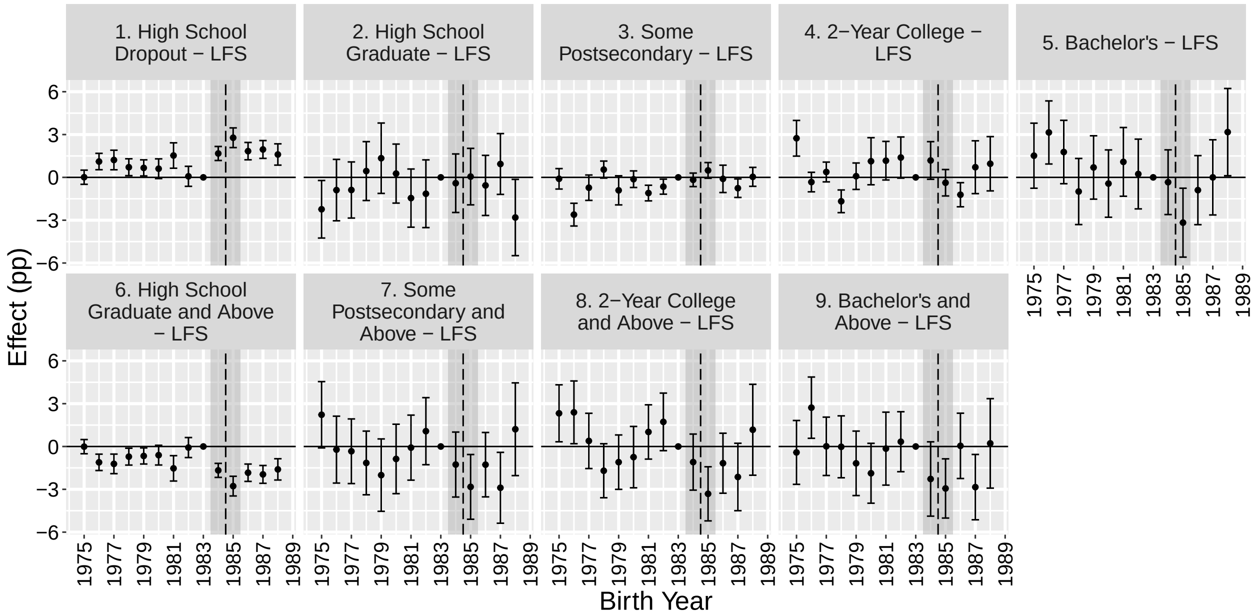
Event study plot for high school graduation rates. This plot is a companion to Table 1. The area shaded in grey represents the “double cohort”. Error bars represent 90% confidence intervals.

Figure B.18: Event Study: High School Graduation



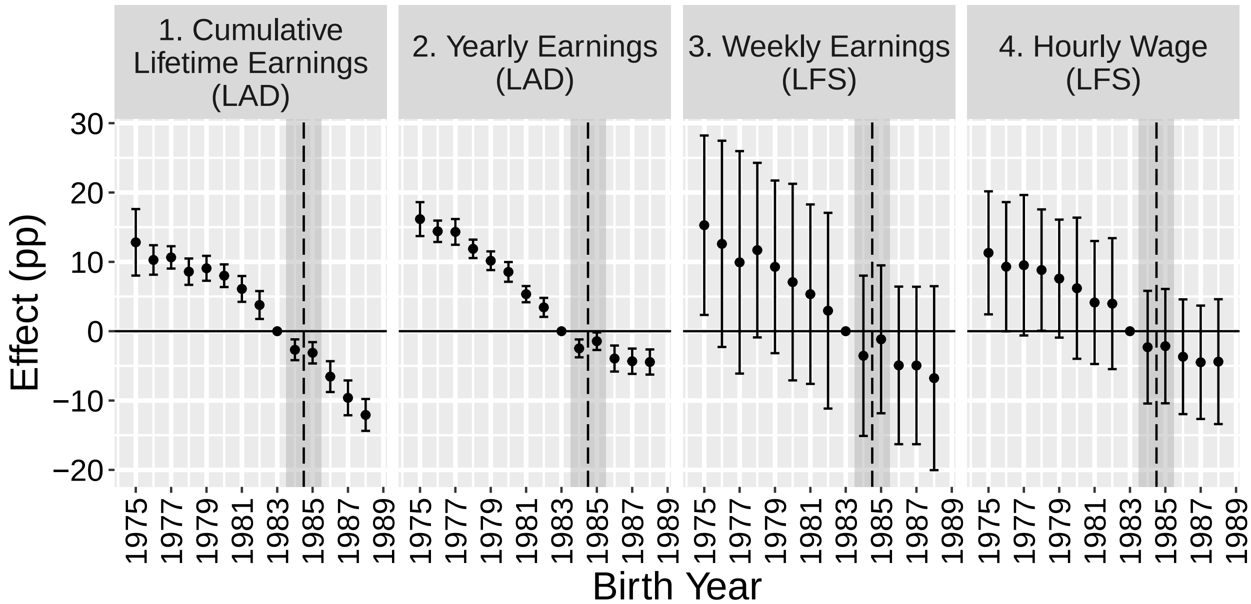
Event study plot for high school graduation rates. This plot is a companion to column 3 of Table 2. Other estimates in that table are for the NLSCY, where the sample size is small to conduct event studies and the Census, where we employ a DRD approach. The area shaded in grey represents the "double cohort". Error bars represent 90% confidence intervals.

Figure B.19: Event Study: Long-Term Educational Attainment



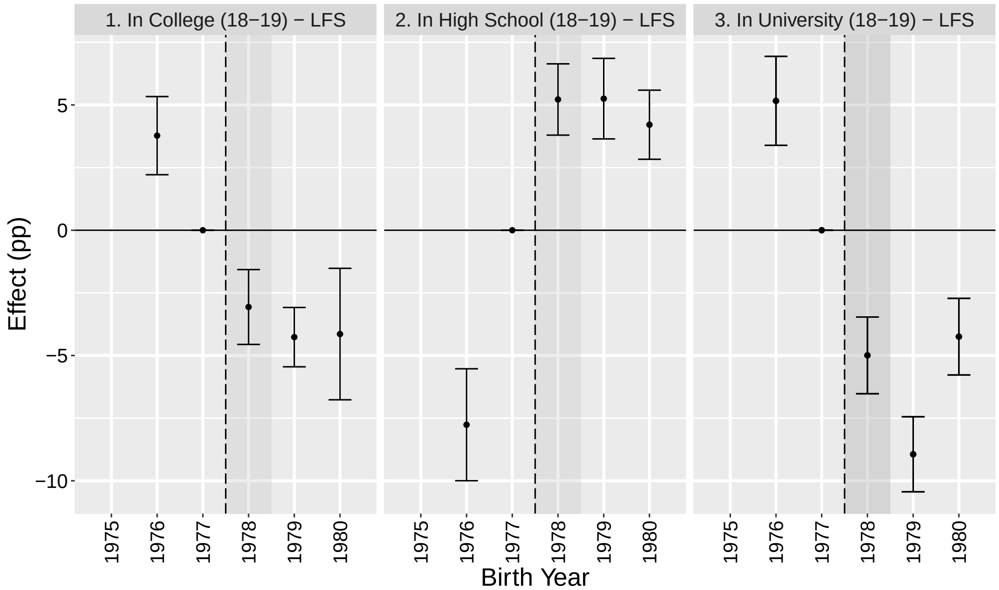
Event study plot for long-term educational attainment. This plot is a companion to Table 5. The area shaded in grey represents the "double cohort". Error bars represent 90% confidence intervals.

Figure B.20: Event Study: Earnings and Wages



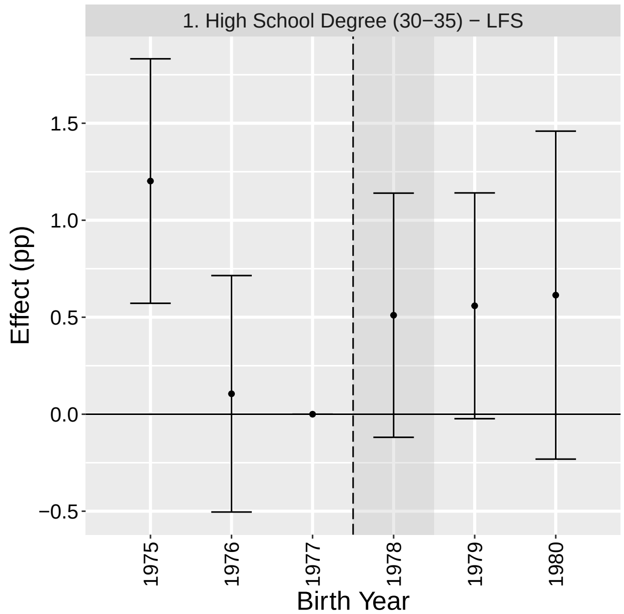
Event study plot for earnings and wages. This plot is a companion to Figure 1. The area shaded in grey represents the "double cohort". Error bars represent 90% confidence intervals.

Figure B.21: Event Study: High School Timing and Age 18–19 Enrollment (Placebo)



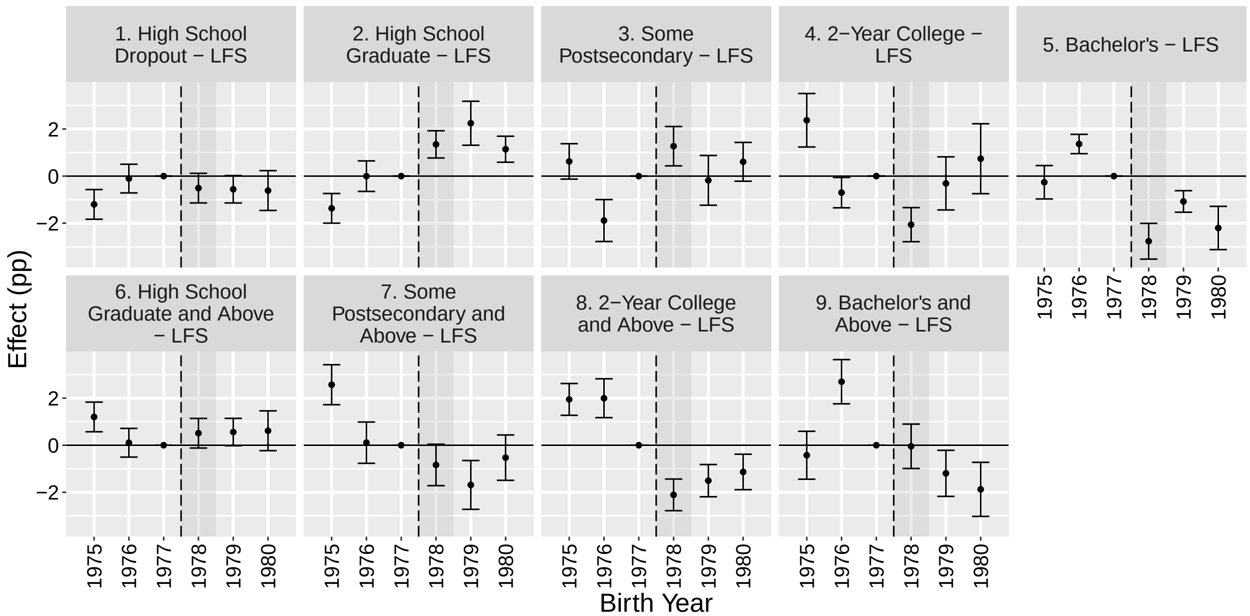
Placebo event study companion to Figure B.17, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The area shaded in grey represents the placebo cohort window. Error bars represent 90% confidence intervals.

Figure B.22: Event Study: High School Graduation (Placebo)



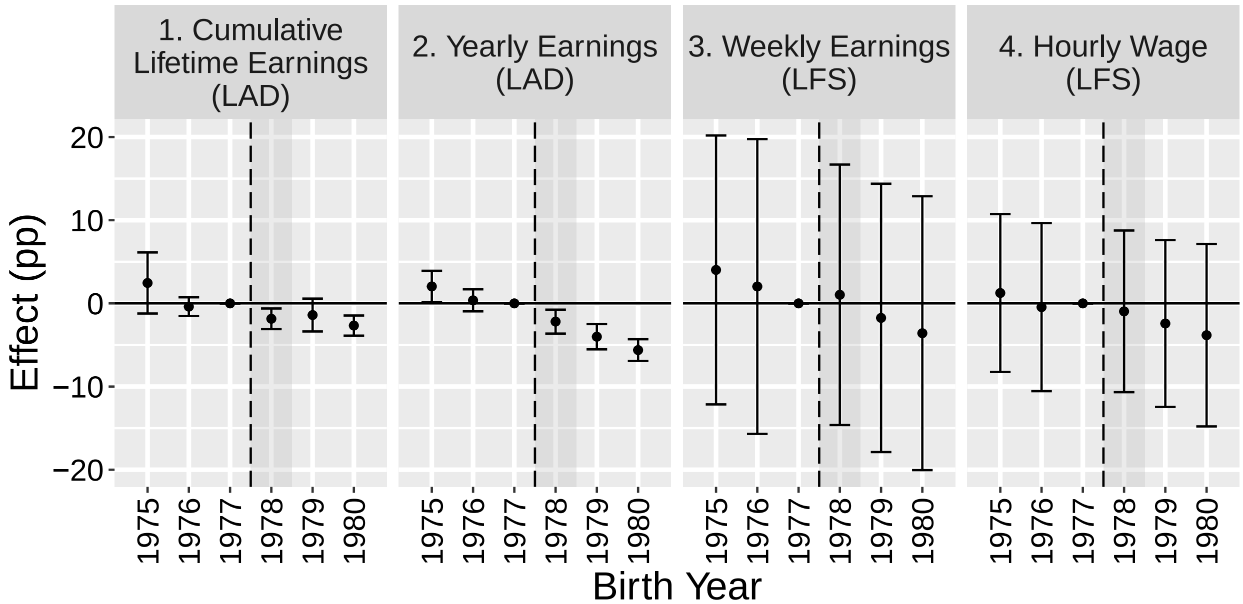
Placebo event study companion to Figure B.18, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The area shaded in grey represents the placebo cohort window. Error bars represent 90% confidence intervals.

Figure B.23: Event Study: Long-Term Educational Attainment (Placebo)



Placebo event study companion to Figure B.19, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The area shaded in grey represents the placebo cohort window. Error bars represent 90% confidence intervals.

Figure B.24: Event Study: Earnings and Wages (Placebo)

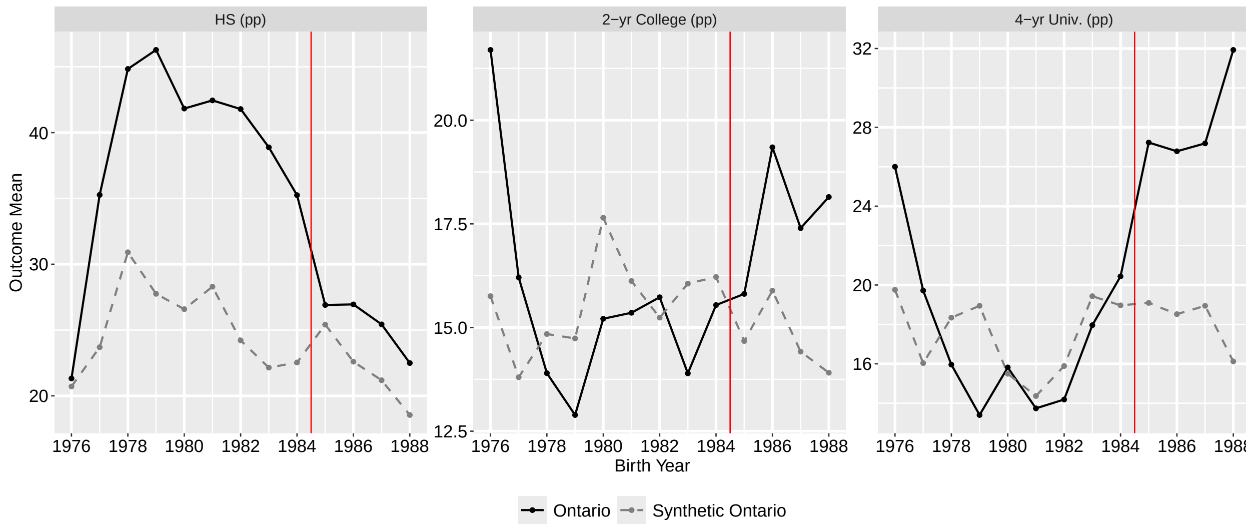


Placebo event study companion to Figure B.20, using the 1975–1980 birth cohorts and a placebo policy year of 1978. The area shaded in grey represents the placebo cohort window. Error bars represent 90% confidence intervals.

B.7 Synthetic-Control-Weighted Difference-in-Differences

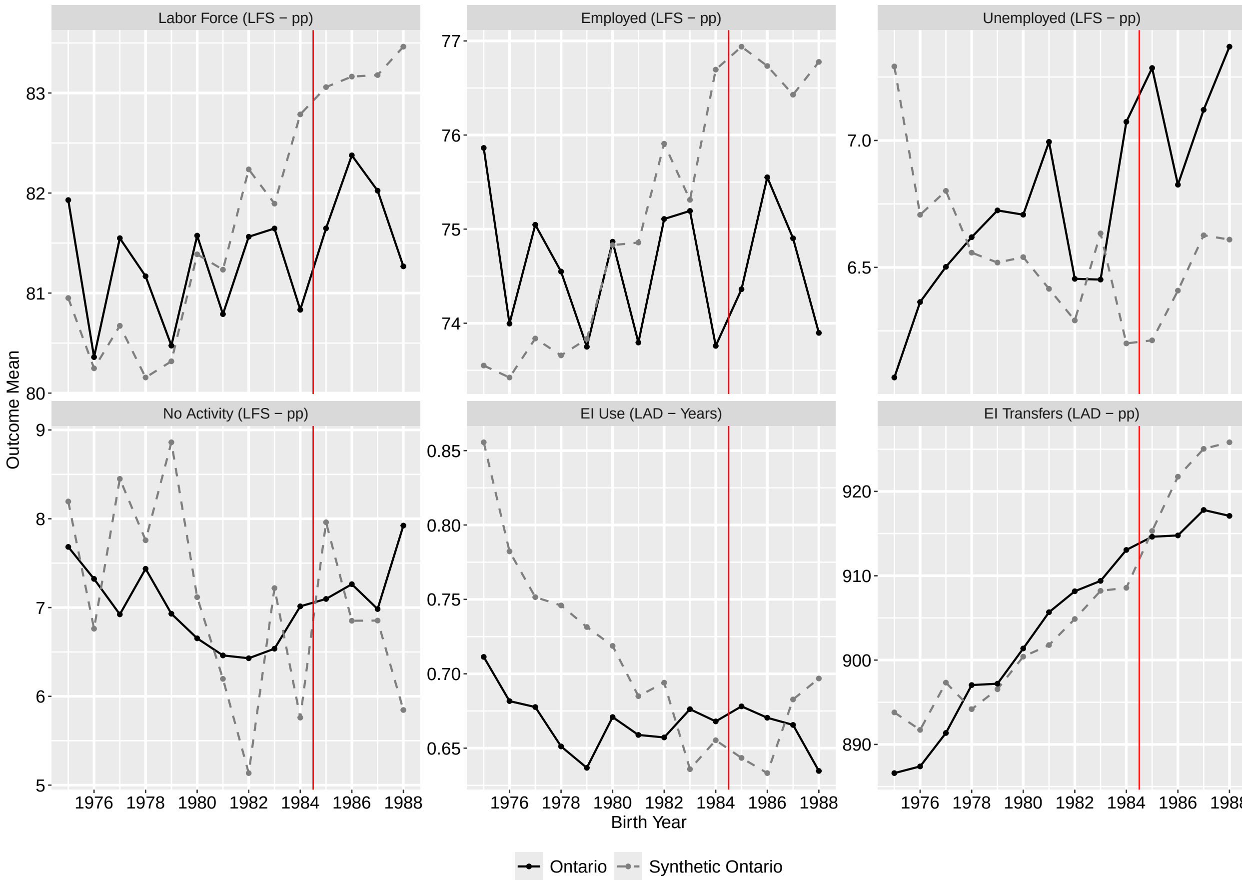
B.7.1 Pre-Trends

Figure B.25: Time Spent in High School: Ontario vs. Synthetic Ontario



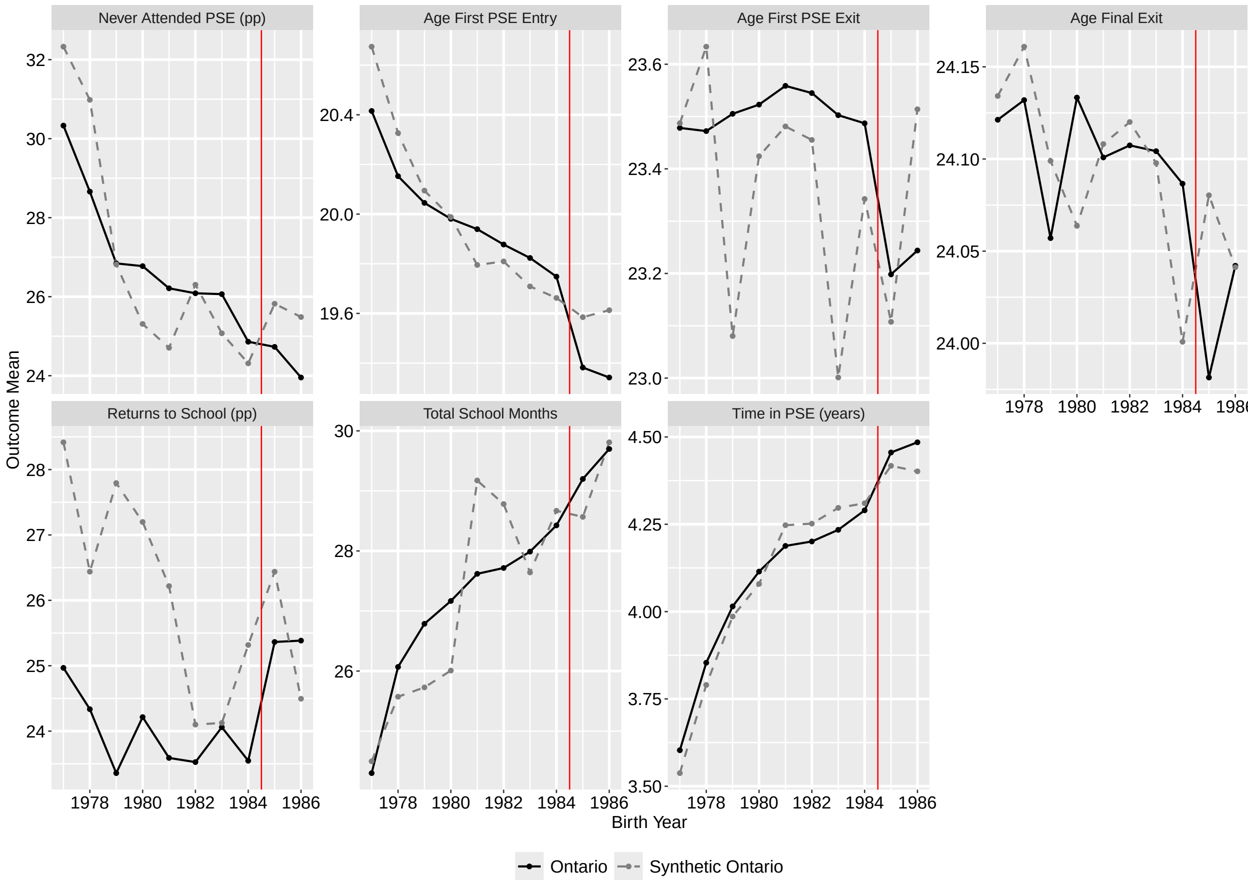
Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.26.

Figure B.26: Post-Secondary Activities and Labor Market: Ontario vs. Synthetic Ontario



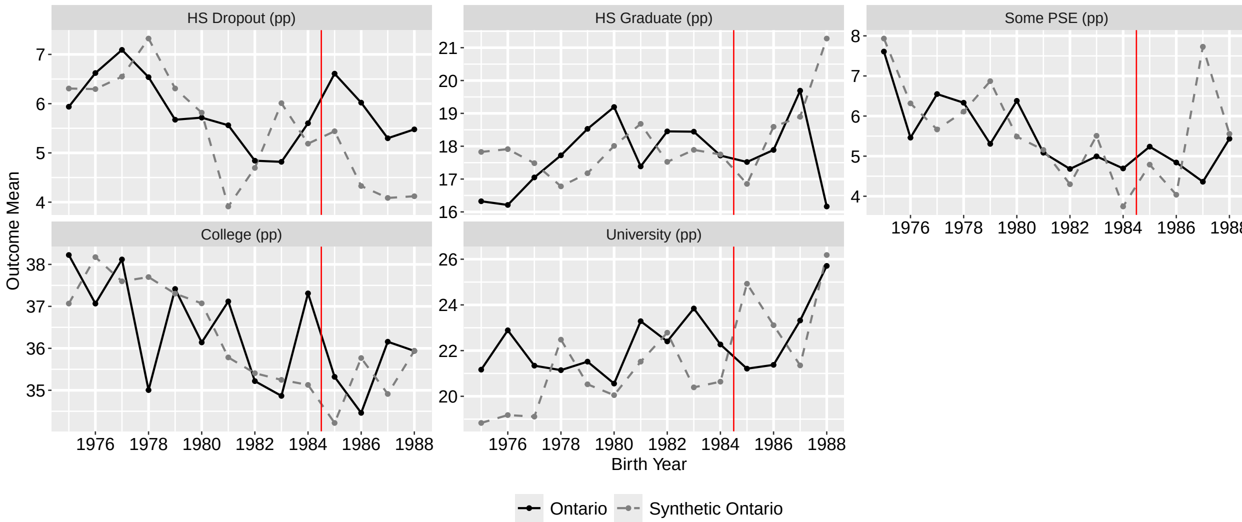
Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.27.

Figure B.27: Enrolment Histories: Ontario vs. Synthetic Ontario



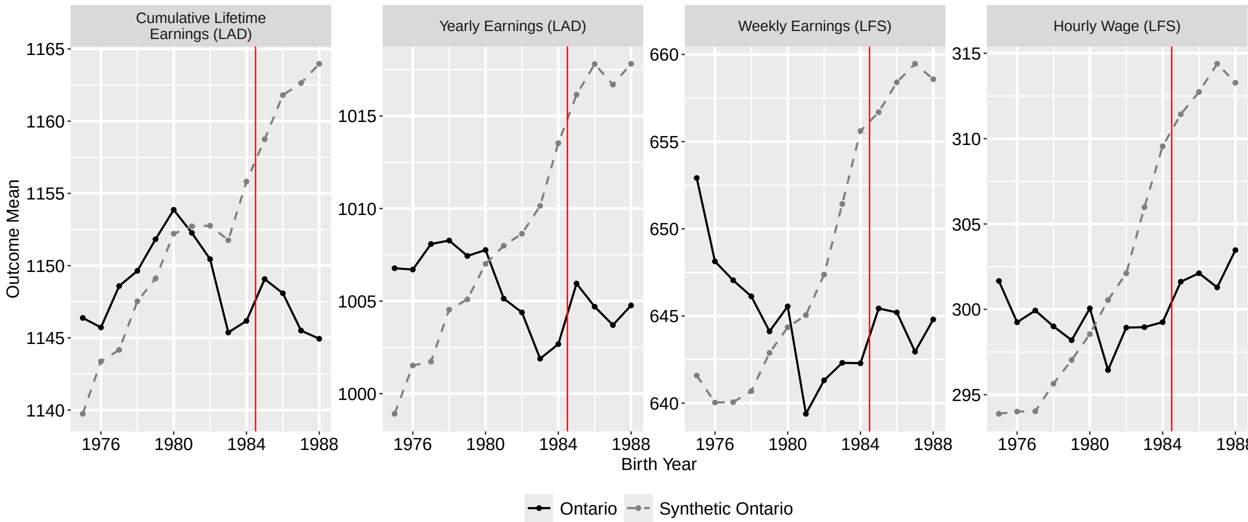
Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.28.

Figure B.28: Long-Run Educational Attainment: Ontario vs. Synthetic Ontario



Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.29.

Figure B.29: Earnings: Ontario vs. Synthetic Ontario

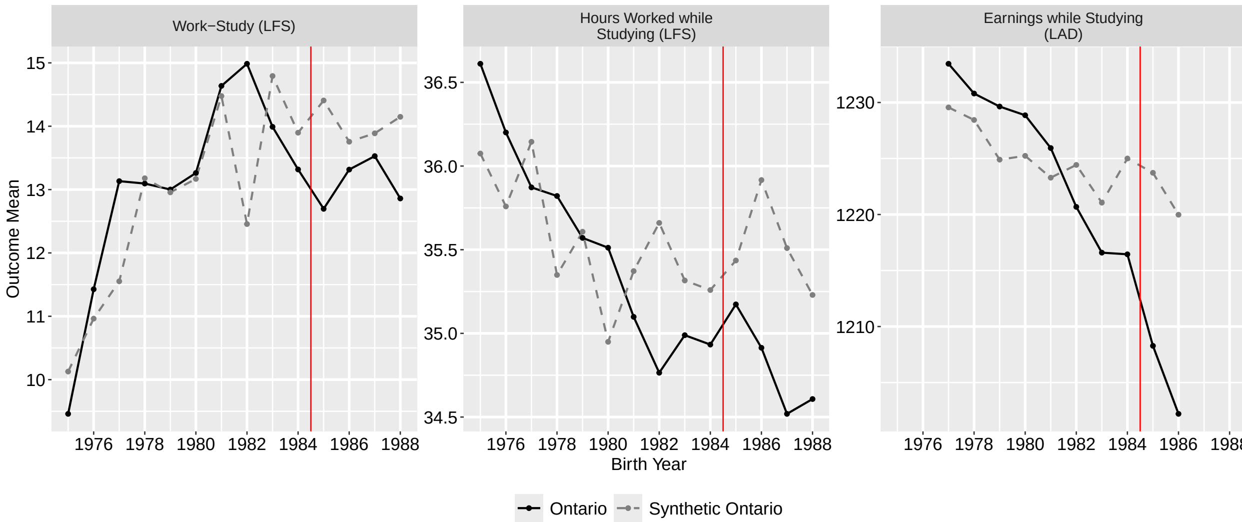


Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.30.

Figure B.30: Work Experience: Ontario vs. Synthetic Ontario

Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.31.

Figure B.31: Work and Study: Ontario vs. Synthetic Ontario



Pre-trend validation. Solid black line: Ontario. Dashed grey line: synthetic Ontario, constructed from a donor pool of provinces (excluding Québec). The red vertical line marks the 1985 treatment cohort. Companion to the synthetic-control-weighted difference-in-differences estimates in Table B.32.

B.7.2 Synthetic-Control-Weighted Difference-in-Differences Estimates

Table B.26: Synthetic-Control-Weighted Difference-in-Differences Estimates: Time Spent in High School

	(1) HS	(2) 2-yr College	(3) 4-yr Univ.
Policy	-9.94***	2.95***	10.11***
	(0.00)	(0.51)	(0.47)
N	26	117	117
Pre-Treatment Mean	40.43	14.98	16.57
% Change	-24.59	19.72	61.06

Synthetic-control-weighted difference-in-differences version of Table 1. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.27: Synthetic-Control-Weighted Difference-in-Differences Estimates: Post-Secondary Activities and Labor Market

	(1) Labor Force (LFS - pp)	(2) Employed (LFS - pp)	(3) Unemployed (LFS - pp)	(4) No Activity (LFS - pp)	(5) EI Use (LAD - Years)	(6) EI Transfers (LAD - pp)
Policy	-1.39**	-2.04**	0.69**	0.65***	0.05***	-5.89***
	(0.53)	(0.83)	(0.22)	(0.00)	(0.00)	(1.00)
N	126	126	126	28	28	126

Synthetic-control-weighted difference-in-differences version of Figure 2. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.28: Synthetic-Control-Weighted Difference-in-Differences Estimates: Enrolment Histories

	(1) Never Attended PSE	(2) Age First PSE Entry	(3) Age First PSE Exit	(4) Age Final Exit	(5) Returns to School	(6) Total School Months	(7) Time in PSE
Policy	-1.31	-0.23**	-0.24***	-0.06**	2.16***	0.26	0.06
	(1.15)	(0.07)	(0.00)	(0.02)	(0.00)	(0.28)	(0.04)
N	90	90	30	90	20	90	90

Synthetic-control-weighted difference-in-differences version of Table 6. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.29: Policy Effect on Educational Attainment (Ages 18–20)

	(1) HS Dropout	(2) HS Graduate	(3) Some PSE	(4) College	(5) University
Attained Exactly					
Policy	1.36***	-1.09**	-0.56	0.26	-2.48***
	(0.05)	(0.40)	(0.33)	(0.97)	(0.00)
N	112	126	126	126	28
Pre-Treatment Mean	5.83	17.71	5.70	36.63	22.04
% Change	23.28	-6.15	-9.81	0.71	-11.26
Attained At Least					
Policy		-1.27***	0.76	1.46*	0.92***
		(0.17)	(0.59)	(0.70)	(0.00)
N		126	126	126	28
Pre-Treatment Mean		94.17	76.46	70.76	34.13
% Change		-1.35	1.00	2.06	2.69

Synthetic-control-weighted difference-in-differences version of Table 5. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.30: Synthetic-Control-Weighted Difference-in-Differences Estimates: Earnings

	(1) Cumulative Lifetime Earnings (LAD)	(2) Yearly Earnings (LAD)	(3) Weekly Earnings (LFS)	(4) Hourly Wage (LFS)
Policy	-15.00***	-12.34***	-13.70***	-10.87***
	(1.65)	(0.97)	(1.85)	(0.96)
N	126	126	126	126

Synthetic-control-weighted difference-in-differences version of Figure 1. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.31: Synthetic-Control-Weighted Difference-in-Differences Estimates: Work Experience

	(1) Lifetime Work Experience (LAD)	(2) Never Worked (LAD)
Policy	-0.11**	0.29
	(0.03)	(0.21)
N	126	126

Synthetic-control-weighted difference-in-differences version of Figure 4. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.32: Synthetic-Control-Weighted Difference-in-Differences Estimates: Work and Study

	(1) Work-Study (LFS)	(2) Hours Worked while Studying (LFS)	(3) Earnings while Studying (LAD)
Policy	-1.22***	-0.71***	-16.67***
	(0.00)	(0.08)	(1.51)
N	28	126	90

Synthetic-control-weighted difference-in-differences version of Figure 3. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.