Transitory Earnings Opportunities and Educational Scarring of Men*

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Abstract

I study how transitory increases in the opportunity cost of schooling affect dropout rates and long-run outcomes. Exploiting a tax-free year in Iceland and comparing teenagers around compulsory schooling age, I document increased dropout rates and a permanent loss in educational attainment for men, but not women. Consequently, they experience substantial lifetime earnings losses, enter occupations with limited career advancement, and are less likely to marry or have children. I show that these findings can be rationalized by a model of schooling decisions in which students either underestimate the returns to education or overestimate how easily they can return to school later. Consistent with the former, dropouts come disproportionately from families where parents earn well despite low education—an environment likely to lead children to underestimate the returns to schooling. These findings show that temporary shocks to the opportunity cost of schooling can leave permanent scars on human capital, with implications for age-dependent taxation and the costs of macroeconomic booms.

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1 Introduction

The opportunity cost of schooling is a central determinant of human capital investment (Mincer, 1958; Becker, 1962). Anything that raises the current return to working relative to the future return to education increases the opportunity cost of school attendance. Whether temporary increases in the opportunity cost—such as macroeconomic booms or age-dependent tax policies—have permanent consequences for human capital remains an open question. Standard theory predicts that a temporary increase in opportunity costs should induce dropout only among students close to completing their optimal level of schooling and for whom the marginal return to further schooling is low (Card and Lemieux, 2001b). However, this prediction requires that students correctly perceive the shock as transitory, that re-enrollment remains costless if they leave, and that they have accurate information about the returns to education. If not, even a short-lived increase in opportunity costs can induce students to drop out permanently and suffer lasting earnings losses.

Answering this question requires a shock that temporarily raises the opportunity cost of schooling while leaving the future return to education intact. In this paper, I exploit a one-year tax holiday in Iceland that constitutes exactly such a shock. In the late 1980s, Iceland transitioned to a pay-as-you-earn tax system from one in which taxes were based on the previous year's income (Bianchi, Gudmundsson, and Zoega, 2001; Sigurdsson, 2025). During the transition year 1987, no taxes were collected on income earned that year. The reform raised students' net-of-tax wages by approximately 10 percent for a single year. I find that this temporary shock had permanent consequences: young men left school to seize the earnings opportunity, never returned, and suffered substantial losses in lifetime earnings.

I identify causal effects using a regression discontinuity design that combines the timing of the tax reform with a legal school-leaving age of 16. Students who had turned 16 by the time of the tax-free year were eligible to leave school, while those just below the threshold were required to remain enrolled. Since both the timing of the reform and students' dates of birth are exogenous to schooling decisions, comparisons across this threshold identify the causal effect of the temporary increase in opportunity costs on dropout and subsequent outcomes.

The tax-free year led to an increase in school dropout rates. Among men, I estimate a 5 percentage point decline in post-compulsory education completion—an 8 percent decline relative to the control-group completion rate. This effect is primarily driven by increased dropout from four-year academic tracks (high school), typically attended between ages 16 and 20, which serve as the main qualification pathway for university. In contrast, women did not exhibit higher dropout rates during the tax-free year. Although students could in principle delay their education and return later, I find little empirical support for such intertemporal substitution. Tracking students over time, I document that most who dropped out never returned to school.

During the tax-free year, young men took jobs in fishing, manufacturing, and construction, while young women worked as shop assistants or in child and elder care. These occupations resemble those typically held by young men and women in normal years. A key distinction, however, lies in job characteristics: men's jobs, though offering limited prospects for career advancement and flat lifetime earnings, paid significantly higher wages to young men and provided more opportunities for full-time work than those available to women. This disparity offers a plausible explanation for why men were more likely than women to leave school for work. The gender gap in pay is also reflected in pre-reform tax rates, which were nearly twice as high for the average young man as for the average young woman. As a result, the effective increase in opportunity costs induced by the tax-free year was larger for men than for women.

Leaving school prematurely resulted in substantial lifetime earnings losses. Although dropouts initially earned higher incomes right after leaving school, their earnings gradually fell behind those of their peers in adulthood. By prime working age, the tax-free year reduced annual earnings for affected men by 5.1 percent. Translating this into a return to schooling using the effect on educational attainment as a first stage implies a return of about 19 percent per year of schooling—inconsistent with the prediction that students induced to drop out by a temporary shock are negatively selected on returns (Willis and Rosen, 1979; Card and Lemieux, 2001b). In addition to the negative effects on labor market outcomes, male dropouts also experienced substantial adverse effects on family formation: they were significantly less likely to marry, less likely to have children, and had fewer children overall. In contrast, for women, the tax-free year had no significant long-run consequences on their labor market outcomes or socioeconomic status, consistent with the absence of effects on their educational attainment.

The negative effect on earnings for the affected cohorts may not solely reflect the earnings loss associated with forgone education. If workers from adjacent birth cohorts are imperfect substitutes, the reduced supply of educated men in the affected cohorts could have influenced the returns to education for those who completed their degree. To quantify this general equilibrium effect, I calibrate the model from Card and Lemieux (2001a) using a range of elasticity estimates from the literature. Across the empirically plausible range of elasticity estimates, general equilibrium wage effects account for a limited share of the estimated earnings losses—below 15 percent under estimates more closely matched to the Icelandic setting. A direct empirical test on unaffected cohorts finds no evidence of positive earnings spillovers, corroborating the calibration evidence.

To interpret the findings, I develop a model of schooling choice that incorporates a range of factors influencing the dropout decision. The model shows that heterogeneity in ability, liquidity constraints, psychic costs, and discounting can all amplify the initial dropout response to a temporary shock to opportunity costs, but mechanisms operating only at the initial dropout margin cannot generate the permanent dropout and associated earnings losses estimated in the data. Permanent dropout requires re-entry costs, misperceptions about the returns to education, or a combination of the two. I use heterogeneity in effects, auxiliary evidence, and structural calibration to assess the empirical plausibility of each mechanism.

The evidence rules out several natural alternatives and points to misperceptions about the returns to education and psychological re-entry costs as the most plausible mechanisms. Children and adolescents form expectations about the returns to education by observing individuals in their immediate environment—parents, peers, and neighbors (Manski, 1993; Jensen, 2010). If dropout decisions during the tax-free year were driven by such misperceptions, they would be concentrated among students from backgrounds where inferring the true returns to education is especially difficult. Consistent with this, dropouts predominantly come from low-education families and neighborhoods whose parents earn more than their education predicts—environments that likely lead students to infer that the returns to schooling are low. Yet high-school graduates earned approximately 25–30 percent more than those without a degree, suggesting that the perceived returns of marginal dropouts may have been substantially below the true returns. A structural calibration based on the absence of temporary dropout among men implies that students underestimate returns by 10–21 percent. An independent estimate constructed from parental earnings data implies underestimation of 17 percent, which falls within that range.

Re-entry costs may take different forms: organizational barriers to re-enrollment, financial or consumption commitments, and psychological switching costs. I find no empirical support for the first two. The Icelandic upper-secondary system is institutionally flexible, and administrative data show no evidence that debt, early parenthood, or other commitments constrained re-entry. The evidence instead points to psychological switching costs as a plausible barrier. Survey evidence from Icelandic upper-secondary school dropouts around this time is informative. Surveyed at age 24, most report that returning to school would be difficult, despite believing they would be better off had they completed their degree—suggesting that even those who recognized the returns to education face barriers to re-entry.

Discounting alone cannot explain the findings. Rationalizing permanent dropout through present bias or high exponential discounting requires parameter values outside the empirically plausible range (Kureishi et al., 2021; Frederick et al., 2002; Cohen et al., 2020; Imai et al., 2021). However, impatient students place less weight on the long-run returns to re-enrollment, making even modest re-entry costs or misperceptions sufficient to generate permanent dropout. Impatience can therefore act as an amplifying factor, but not a primary explanation.

The findings suggest that transitory increases in the opportunity cost of schooling can lead to large losses in human capital and aggregate output. A 10 percent increase in adolescent wages leads to a permanent 1.5 percent loss in years of schooling for the affected cohorts, and back-of-the-envelope calculations suggest that the human capital losses from the tax-free year contributed to a 0.1–0.2 percent reduction in aggregate output in 2020. These costs suggest that policies aimed at reducing dropout during periods of elevated opportunity costs may be warranted. Raising the compulsory schooling age is one natural instrument. I estimate that increasing it from 16 to 17 would have reduced the impact of the tax-free year on dropout by approximately 40 percent.

The findings have direct implications for optimal income taxation. Previous studies have argued that, due to the high elasticity of labor supply among young workers, an optimal labor income tax should be age-based and lower for the young (Kremer, 2002; Weinzierl, 2011). However, my findings introduce a caveat to this proposition. The tax-free year studied in this paper is precisely such a policy, and the results show that even a single year of reduced taxes is sufficient to permanently reduce educational attainment. Young workers trade off labor not only for leisure but also for education, and the human capital losses that result lead to lower incomes in adulthood and ultimately reduce tax revenue. The conventional case for age-dependent taxation should therefore be weighed against these costs, which the present analysis suggests are substantial.

These findings also speak to the cost of business cycles. While the macroeconomics literature has focused primarily on the welfare costs of recessions through consumption volatility (e.g., Lucas, 1987; Krebs, 2007; Krusell et al., 2009), economic booms may impose additional costs through the human capital channel identified in this paper. The elasticity estimates suggest these costs are economically significant and permanent.

Related Literature This paper contributes to the literature on how changes in the opportunity cost of schooling affect educational attainment and long-run outcomes. The most closely related papers are Atkin (2016), Charles et al. (2018), and Carrillo (2020). Atkin (2016) finds that factory openings in Mexico raised the opportunity cost of schooling and reduced educational attainment, though with limited effects on adult earnings. Charles et al. (2018) show that the U.S. housing boom of the 2000s reduced college attendance, with persistent effects on attainment for affected cohorts. Carrillo (2020) exploits coffee price shocks in Colombia and finds that temporary booms permanently reduced schooling and adult earnings, attributing this to present bias. Cleanly identifying the opportunity cost channel, however, is challenging: economic booms raise current earnings opportunities but also alter the perceived lifetime return to education, affect household incomes, and have uncertain duration. The tax holiday, by contrast, raises the net-of-tax wage for a single known year while leaving household incomes and the future return to education unaffected, isolating the opportunity cost channel. Because students knew the shock was transitory, the evidence that most never returned is difficult to reconcile with pure present bias and points instead to re-entry costs and misperceptions about the returns to education. A broader literature has studied the effects of natural resource booms, business cycle fluctuations, and local labor market conditions on educational attainment (Black et al., 2005; Cascio and Narayan, 2020; Gustman and Steinmeier, 1981; Kane, 1994; Betts and McFarland, 1995; Card and Lemieux, 2001b; Cameron and Taber, 2004; Johnson, 2013; Goldin and Katz, 1997; Shah and Steinberg, 2017; Emery et al., 2012).

By following individuals over their full life cycle using administrative data, I can estimate the long-run earnings consequences for the marginal students induced to drop out. These consequences have remained largely elusive in prior studies. Prior evidence presents two contrasting views. One strand finds that dropouts are negatively selected, with low ability and low returns to further schooling (Eckstein and Wolpin, 1999; Heckman et al., 2006a), suggesting those who leave school prematurely lose little by doing so. A second strand finds large returns for students at the margin of dropping out (Oreopoulos, 2007b; Zimmerman, 2014), but attributes these to disadvantage or credit constraints that prevent optimal investment. In either case, the long-run earnings losses from a temporary shock to opportunity costs should be modest—either because those induced to leave have low returns, or because high-return students would not permanently drop out in response to a transitory shock. Yet the dropouts induced by the tax-free year have large implied returns and come predominantly from low-education but above-median earning families—inconsistent with both negative selection and credit constraints.

Finally, the findings contribute to the literature on the widening gender gap in education (Goldin, Katz, and Kuziemko, 2006; U.S. Department of Education, 2021; Heckman and LaFontaine, 2010). The concentration of dropout responses among men echoes patterns from World War II (Goldin, 1998) and the U.S. fracking boom (Cascio and Narayan, 2020), suggesting that differential exposure to short-term economic opportunities may entrench the gender gap in education.[1]

The paper proceeds as follows. In the next section, I describe the setting and empirical strategy. In Section 3, I present the effects of the tax-free year on school dropout and educational attainment. Then, in Section 4, I estimate the impact on the short- and long-run labor market and socioeconomic outcomes. In Section 5, I present the conceptual framework which I then use in Section 6 to guide the interpretation of the empirical results. I discuss the macroeconomic and policy implications of the results in Section 7. Finally, in Section 8, I conclude the paper. Additional background material and auxiliary analyses I relegate to an appendix.

2 Empirical Setting

2.1 The Tax-Free Year

On December 6, 1986, the Icelandic Finance Minister announced a tax reform replacing a system in which income taxes were collected with a one-year lag with a withholding-based pay-as-you-earn system, as illustrated in the top panel of Figure 3. To ensure that workers would not have to pay taxes simultaneously on their 1986 and 1987 earnings during the transition, no taxes were collected on 1987 labor income—making all labor income earned that year tax-free. For further details about the reform and its timeline, see Sigurdsson (2025).

The tax-free year created a strong incentive for people of all ages to work more or enter the labor market. As summarized in Table 1, the average tax rate for workers at upper-secondary schooling age (16–20) before the reform was 8.3% (10.1% for men and 5.6% for women), and the marginal rate was 15.8%. The incentive was highly salient: the reform received extensive media coverage, and the tax authorities distributed advertisements and explanatory flyers, making it likely that most adults, including students, were aware of it. Contemporary newspaper reports confirm that students acted on this incentive, with school authorities expressing concern as early as December 1986 and articles in early 1987 documenting an unusually high number of high school students not returning after the holiday break.[2]

[1] Goldin (2021) summarizes: "*Historically, men have been more likely to drop out of school to work in hot economies, whether it's in the factories of World War II or the fracking mines of the Dakotas. [. . .]. My biggest immediate worry is that men are making the wrong decision.*"

[2] Appendix Figure A.6 shows a 0.9% decline in university enrollment during the 1986/1987 academic year compared to the previous year, followed by a further contraction of 3.1% in 1987/1988. As I will discuss, while the school system provides a natural control group for individuals of upper-secondary school age in 1987, no analogous control group

2.2 The Education System

The Icelandic education system comprises three main levels, supplemented by an optional preschool stage. Children begin school in the fall of the year they turn 6 and complete compulsory education ten years later, in the spring of the year they turn 16 (Figure 3). Upon finishing compulsory schooling, students are eligible to enroll in upper-secondary education (high school), which is effectively tuition-free, with the enrollment rate rising steadily to above 90% by the 2000s (Blöndal, 2014). Admission to upper-secondary schools is determined by scores from standardized tests administered at the end of the 10th grade.

Upper-secondary education is categorized into three types: grammar schools, vocational schools, and comprehensive schools. Grammar and comprehensive schools both offer four-year academic programs culminating in matriculation examinations—a prerequisite for university admission—and together account for roughly 90% of upper-secondary enrollment, with the remainder in vocational schools (Ministry of Education, 2002). These four-year academic programs are commonly referred to as high schools or junior colleges when compared to the U.S. education system. Vocational students have paid practical training in the labor market as part of their education, which gives them more flexibility to temporarily delay the theoretical component of their studies.

Dropout rates are substantial and re-entry is common: cohort studies report an average dropout rate of about 30%, and the Icelandic upper-secondary system is institutionally flexible, accommodating slow progression and re-enrollment at any age (Blöndal, 2014; Statistics Iceland, 2008). Appendix E documents these patterns in detail and shows that they have been stable over time.

As in most industrialized countries, Icelandic women now attain higher levels of education than men. While high school graduation rates were equal in 1975, women's rates have since risen more rapidly: by 2020, only 0.68 men graduated high school for every woman (Statistics Iceland, 2022b).[3] Male students consistently exhibit higher dropout rates in post-compulsory education—21.1% in academic programs in 2020, compared to 12.9% for women (Statistics Iceland, 2022a).

2.3 Data and Sample

The data come from four administrative datasets covering the Icelandic population, all linked via a unique personal identifier. The first is Statistics Iceland's Education Register and Degree Register, which records completed educational attainment for the Icelandic population dating back to 1981, with information on some degrees extending much further back (e.g., university degrees since 1912).[4] I translate attainment into years of schooling using the number of years typically required to achieve each level; further details are provided in Appendix A. The second is a panel of individual tax returns extending back to 1981, containing information on all income sources as

exists for university students. Thus, my analysis focuses on delay and dropout from upper-secondary education.

[3]Similarly, gender parity in university graduation rates observed in the mid-1980s had shifted by 2020, with nearly twice as many women as men earning bachelor's degrees.

[4]In most cases, education that is started but not completed is not systematically recorded, except where a final degree consists of more than one level and each level is registered separately—as is the case for some vocational degrees.

well as detailed data on assets and liabilities. The third is employer-employee data constructed from digitized pay slips, containing information on employment, earnings, working time, occupation, and industry; see Sigurdsson (2025) for further details. The fourth is the National Register, from which I obtain a family identifier linking parents and co-residing children.

The sample consists of individuals in the birth cohorts around the school-leaving age threshold during the 1987 tax-free year, who completed compulsory education in Iceland and were alive and resident in Iceland at age 30. The first restriction ensures the sample was present in Iceland at the time they were eligible for upper-secondary schooling; the second ensures they are observed in the tax register during working age.

All monetary values are winsorized from above at the 0.1% level, converted to 2010 prices using the Icelandic CPI, and then converted to US dollars using an exchange rate of 130 Icelandic króna (ISK) per dollar.

2.4 Empirical Strategy

My empirical strategy exploits two features, illustrated in Figure 3. First, the Tax-Free Year temporarily raised net-of-tax wages for all workers, providing an exogenous transitory increase in the opportunity cost of schooling. Second, Icelandic education is compulsory up to the year students turn 16, so only those above this threshold could delay or drop out of school to work during the Tax-Free Year.

I use a fuzzy regression discontinuity (RD) design comparing individuals around the school-leaving age threshold. Specifically, I estimate:

$$y_i = \alpha + \beta D_i + f^k(m_i) + D_i \times f^k(m_i) + \gamma \mathrm{X}_i + \varepsilon_i \tag{1}$$

where y_i is the outcome of individual i, $D_i = \mathbb{1}(m_i \leqslant 0)$, and m_i is birth month normalized to zero in December 1971. That is, $D_i = 1$ for individuals aged 15 or older as of December 31, 1986—born in 1971 or earlier—and therefore completed compulsory schooling in or before spring 1987, and zero for those still below the school-leaving age. This implies a discrete jump in outside options at the threshold. $f^k(\cdot)$ is a k^{th}-order polynomial in m_i, interacted with D_i to allow different trends on either side of the threshold. Coefficient β identifies the causal effect of the Tax-Free Year from the discrete change in outcomes at the threshold, under the assumption that ε_i does not jump discontinuously there. Controls X_i are pre-reform characteristics measured at age 16: location of residence, and indicators for having a child, receiving social insurance, being fatherless or motherless, and receiving disability benefits. These should be uncorrelated with date of birth and so do not affect the estimate of β, but improve precision.

I estimate equation (1) using a local-linear approach (Lee and Lemieux, 2010) with a bandwidth of four years (48 birth cohort months). The linear polynomial is motivated by the nonparametric analysis in Section 3, and the bandwidth ensures that the above-threshold sample covers individuals of normal upper-secondary school age without including those who would already have completed it. I assess robustness to alternative bandwidths, polynomial degrees, and weights, and apply the bias-correction procedure of Calonico, Cattaneo, and Titiunik (2014). Following Kolesár and Rothe (2018), I report Huber-White heteroskedasticity-robust standard errors, clustered at the individual level for time-varying treatment effects.

3 Effect on School Dropout

In this section, I analyze the effect of the tax-free year on school dropout, estimate the short and long run effects on educational attainment, and study which jobs students take when leaving school.

3.1 Educational Attainment

Nonparametric Graphical Evidence Figure 4 plots education by date of birth around the compulsory schooling age threshold. In panel (a), education is measured by having completed post-compulsory education and in panel (b) by years of schooling, both measured at the age of 21.[5] Since standard upper secondary education programs, such as junior college, take four years to complete, students who complete their studies on time are expected to have completed their degrees by the age of 21. I plot averages over four birth-months within a four-year bandwidth around the age threshold along with fitted linear trends on each side of the threshold and their 95% confidence intervals. The figure represents the graphical counterpart of the regression analysis that follows.

The figure reveals a clear and discrete drop in completion of post-compulsory education—i.e. increased dropout—among individuals who had passed the compulsory schooling age threshold by the time of the tax-free year. This is similarly reflected in years of schooling completed by age 21.

Regression Estimates When faced with a temporary increase in the opportunity cost of schooling induced by the tax-free year, students may decide to intertemporally substitute schooling for work before returning and completing their education after the tax-free year. Hence, the reduction in school completion measured at age 21 years might not reflect a permanent reduction in educational attainment. To investigate the dynamics of the effect on educational attainment, I estimate equation (1) separately for each age 16-40. In essence, this is a regression discontinuity-based event study design, in which the impact of dropout during the tax-free year appears at the expected graduation age of 20 years.

Figure 5 contains plots, separated by gender, of the age-specific RD estimates of the effect on post-compulsory education and years of schooling. For post-compulsory education, I present estimates separately for the two main upper-secondary education tracks—junior college and vocational education—but present the overall effect on completing any post-compulsory education

[5] Appendix Figure A.7 plots education by date of birth separately for men and women.

in Appendix Figure A.8. Panel (a) plots the estimated effect on men's upper secondary education. The figure documents a difference in men's degree completion that emerges at the expected graduation age of 20 years, consistent with men dropping out of school during the tax-free year. This effect is concentrated in about a 5 percentage point drop in the rate of completion of a junior college degree, with no effect on vocational education. This difference may reflect the structural differences between these tracks in how easy it is to combine work and schooling. Vocational students have paid practical training in the labor market as part of their education and have more options to temporarily delay the theoretical part of their studies. In contrast to intertemporal substitution, this reduction in educational attainment is stable, persistent, and not reversed by students returning to junior college the following year or later in life. While there is some evidence that students complete vocational education in their late 20s, this effect is relatively small and not precisely estimated.[6] As presented in panel (b), this dropout from junior college led to a permanent reduction in men's years of completed schooling by almost 0.15 years.[7]

Panels (c) and (d) in Figure 5 plot the estimated effects on school completion and years of schooling of women. In stark contrast to the effect on men, the figure reveals no sign that women dropped out of school. Even in the short run, there is little evidence to suggest that women temporarily took time off from school to work during the tax-free year.

Table 2 quantifies the permanent effect of the tax-free year on educational attainment by presenting the estimates of equation (1) for individuals aged 40. The tax-free year reduced post-compulsory education completion among men by 5 percentage points, or 8 percent relative to the below-threshold average. As displayed in Figure 5, there is a persistent reduction in men's pre-university years of schooling of 0.145 years or about 1.5 school months. At the age of 40, the total reduction in years of schooling was estimated at 0.26 years. This loss in education captures both the dropout from junior college, which is a prerequisite for university enrollment, and a reduction in education beyond upper secondary school. While imprecisely estimated, the point estimate implies a 1.8 percentage point reduction in university education.

To put these estimates into perspective, they can be compared with estimates from studies on the long-run impacts of education policies. For example, Fredriksson et al. (2013) study the long-run effects of class size in primary schools in Sweden. They estimate (Table V) that increasing class size by one pupil decreases years of schooling by 0.05 years. My estimates imply that the long-run impact of the tax-free year on educational attainment is as large as the effect of adding five pupils to the class size.

Robustness and Placebo Tests I conduct a series of tests to assess the validity and robustness of these results. First, I examine the sensitivity of the estimates to the choice of bandwidth around the

[6] As I will document in Figure 6, male dropouts disproportionately entered manufacturing and construction jobs. This pattern in Figure 5a might indicate that some of these dropouts return to school to get formal training and certification after having worked in these jobs for several years.

[7] To maintain the focus on the persistence of dropout from upper-secondary school, which is the relevant level of schooling for the treatment group during the tax-free year, the figure estimates the effect on pre-university years of education. Table 2 presents estimates of effects on total years of schooling completed, including years of university education.

compulsory schooling age threshold. In the main specifications, I use a bandwidth of 48 months on each side of the threshold, capturing individuals within the typical upper-secondary school age range on the right and a comparable group on the left. Appendix Figure A.9 evaluates the sensitivity of the estimates to this bandwidth by varying it from 6 to 62 months, either on one side (control) or both sides (control and treatment) of the threshold. Within a very narrow window of less than a year, the estimates are slightly larger than the main estimates but, as expected, less precisely estimated due to the smaller sample size. Beyond a one-year bandwidth, the estimates stabilize, aligning closely in magnitude and statistical precision with those derived using the chosen bandwidth. These results also include estimates based on MSE-optimal bandwidths, which are indicated in the figure.

Appendix Table A.3 presents additional specification checks for the estimation of equation (1). These include using the bias-corrected RD estimator of Calonico et al. (2014), varying the set of controls, kernel weights, and the degree of polynomials in the regressions. Across these specifications, the estimates remain broadly consistent in terms of magnitude and statistical significance.

A potential threat to the validity of the research design would arise if certain parents systematically timed their children's births to align with the compulsory school-age threshold during the tax-free year. However, this scenario seems highly unlikely in this context, as the implications of being born before or after December 1971 were not known until the tax reform was announced in 1986. Appendix Figure A.10 provides evidence against this concern, showing no abnormal increase in the number of births around the threshold.

To further validate the research design, I conducted placebo tests to check for discontinuities in educational attainment at the relevant age thresholds during placebo tax-free years. Specifically, I tested six placebo thresholds on each side of the actual threshold, resulting in a total of 12 placebo thresholds. For these tests, I used a narrow window of 12 months above and below each threshold, applying the same window to the actual tax-free year for reference. I examined discontinuities in post-compulsory education outcomes, focusing separately on the completion of junior college or vocational degrees for men and women. The results of these placebo tests are presented in Appendix Figure A.11. As shown, these tests revealed no false positives, either before or after the tax-free year.

3.2 Which Jobs Did Dropouts Enter?

Having established that students—primarily young men—left school during the tax-free year, I now examine the types of jobs they entered. Figure 6 shows the estimated effect on employment at upper-secondary school age by occupation. Each bar represents the employment share of 16- to 19-year-olds in a given occupation, along with the estimated increase in employment in that occupation, separately for men and women. Occupations are arranged along the x-axis by their pre-reform average income rank among 16- to 19-year-old employees. For example, prior to the reform, 16- to 19-year-olds earned the highest incomes in fishing and the lowest in service jobs.

The figure shows that, during the tax-free year, young men predominantly entered jobs in fishing, manufacturing, and construction. These jobs were primarily manual labor positions or roles requiring minimal specialized training, although there was also a notable increase in employment among craftsmen in construction and manufacturing. This pattern, in conjunction with the evidence of limited dropout from vocational tracks, suggests that vocational students may have increased their work alongside schooling or prioritized completing their paid practical training while postponing the theoretical components of their studies. In contrast, young women mainly entered jobs as shop assistants or in child and elderly care.

The jobs that young men took during the tax-free year appear to have had a lasting effect on their career trajectories. Appendix Figure A.15 shows that, by prime age, men who were affected by the tax-free year were disproportionately employed in industries similar to those they initially entered, such as manufacturing and construction.[8]

These results provide a plausible explanation for why young men were more likely than young women to leave school to work during the tax-free year. First, the jobs available to men—such as those in fishing, manufacturing, and construction—were, on average, significantly better paid than those available to women. This is reflected in the gender differences in pre-reform tax rates, as shown in Table 1, where young men faced tax rates nearly twice as high as those faced by young women. Consequently, the monetary incentives for men to leave school and work during the tax-free year were much greater than for women. Second, the jobs men entered were more likely to offer full-time, permanent employment, whereas the jobs available to women were more often short-term or part-time. This interpretation aligns with the absence of evidence of increased dropout rates among women, suggesting that although some women took advantage of the tax-free year to work alongside schooling and earn tax-free income, it did not significantly impact their schooling decisions.

4 Effects on Long-Run Outcomes

In the previous section, I documented that for a portion of young men, the tax-free year marked the end of their education—they dropped out of school and never returned. In this section, I examine the long-run consequences of this decision.

4.1 Labor Market Outcomes

Nonparametric Graphical Evidence Figure 7 examines non-parametrically the effect of the tax-free year on labor income. Panels (a) and (b) plot the average labor income by date of birth of men and women, respectively, at ages 16-20. The figure reveals a clear discrete jump in labor earnings of men that were above the compulsory-schooling age threshold at the time of the tax-free year.

[8] Jobs at prime age are classified by industry rather than occupation due to the lack of consistent occupational data for the years when the affected cohorts reached prime working age.

Appendix Figure A.12 displays a similar discrete jump in employment.[9] For women, there is a small increase in earnings at the threshold, which, in conjunction with the results presented in the previous section, is consistent with young women working alongside school during the tax-free year.

In conjunction with the estimates presented in the previous section, these results suggest that young men dropped out of school to work and earn tax-free income. Panels (c) and (d) plot the average labor income by date of birth for men and women, respectively, at prime age (31-40 years). The figure shows a decline in average labor income among men who were above the schooling-age threshold, indicating that men who dropped out of school earn lower incomes in adulthood. For women, there is no visible discontinuity in earnings at the threshold.

Regression Estimates Figure 8 presents estimates of the effects on labor income over the lifecycle. The figure presents estimates of regression equation (1) of the average annual earnings at 5-year age ranges, from age 16 to 40, separately for men and women. Focusing first on the effect on labor earnings at upper-secondary school age, quantifying the discontinuity displayed in Figure 7, panel (a), I estimate that the discontinuity amounts to $838 or about an 8 percent increase in the annual labor income of young men compared to the below-threshold average. For women, the effect is substantially smaller and imprecisely estimated at $96, which corresponds to a 1.3 percent increase. Similarly, I estimate an increase in youth employment. Reported in Table 3, the increase in the employment rate of men at upper-secondary school age amounts to 5 percentage points or about a 12 percent increase compared to the below-threshold average.[10] The table also documents an increase in the employment rate of women by 2 percentage points. When viewed in the light of the results from the previous section, this suggests that women took the opportunity to earn tax-free income, but did so while attending school and did not drop out of school as a consequence.

A priori, the long-term consequences of school dropout are unclear. For some students, the decision to drop out may be rationalized on the grounds of low returns to schooling relative to the transitory increase in the opportunity cost. If true, they will not fare worse in terms of income than if they had stayed (Willis and Rosen, 1979). However, if dropouts have high returns to education, dropping out will lead to long-term earnings losses.

Figure 8, panel (a), shows that while male dropouts gain in the short run, during adulthood their labor income gradually falls behind the earnings of the control group which was still at compulsory schooling age. During the early 20s, there is a positive but not statistically significant

[9] Employment is defined as earning at least $10,000 per year. This threshold is motivated by the near-universal participation of Icelandic teenagers in paid work: more than 90% hold summer jobs—often just a few weeks—and between one-quarter and one-third work during term time or other holidays (Rafnsdóttir, 1999). In normal years most students therefore earn some income, and the $10,000 threshold is set to exclude these short spells, corresponding roughly to minimum wage earnings for a low-skilled worker working full-time. Appendix Figure A.13 explores robustness to this threshold: the estimated employment effect is positive and statistically significant above $6,000, and the point estimates are stable at $9,000 and above.

[10] Employment is defined as annual labor income above a threshold of $10,000, roughly corresponding to minimum wage earnings for a low-skilled worker. I report the robustness of the estimates to this threshold in Appendix Figure A.13.

effect on income, reflecting that those who stay and complete junior college are also more likely to continue to university, as well as possibly reflecting that dropouts are accumulating experience in the labor market which yields a return. However, by the time men reach 40, their loss in annual earnings amounts to about $2,100, or 5.1% when compared to the below-threshold average. This effect is driven by lower earnings during employment, as there is no statistically significant effect on employment during adulthood, reported in Table 3. In comparison, the corresponding estimates for women are close to and not statistically different from zero.

Figure 8, panel (b), further illustrates the dynamics of the effect on earnings, showing the net effect of school dropout on lifetime earnings. First, the bars at age 20 present the cumulative effect on earnings during upper-secondary school age, 16-20. For men, the cumulative increase in earnings is estimated at $3,400 but for women, the corresponding estimate is small and not statistically significant. Next, the connected dots present the effect on cumulative earnings at each age, from age 21 to 40. By the late 20s, the initial gain has disappeared and dropouts gradually fall behind in terms of lifetime earnings during their 30s. At age 40, affected men have lost on average $27,700, or 4.2 percent, in cumulative lifetime income.

The estimates reported above capture the reduced-form, or intention-to-treat, effect of the tax-free year on earnings. To further interpret these results, it is useful to quantify the effect of school dropout on earnings—that is, the local average treatment effect for compliers who left school due to the reform. A natural approach is to use an instrumental variables (IV) strategy, where the first stage leverages the RD estimate of the tax-free year's impact on years of schooling, and the second stage estimates the effect of education on earnings using an RD-IV regression with an indicator for the compulsory schooling age threshold as an instrument. However, this approach identifies a causal effect only if the tax-free year affects earnings solely through its impact on educational attainment. This exclusion restriction may not hold if the reform had direct effects on earnings through channels other than schooling—for instance, by shaping the early career trajectories of those who dropped out in ways that persist independently of their educational attainment, or by altering labor market conditions through general equilibrium effects that affected the returns to education for those who stayed. Bearing these caveats in mind, I proceed with the IV estimation while investigating both of these potential violations in the analysis that follows.

I report the two-stage least squares (2SLS) regression estimates for men in Appendix Table A.5. Each additional year of schooling lost to the reform is estimated to reduce annual prime-age earnings by approximately 19 percent and lifetime earnings through age 40 by 17 percent. The first-stage F-statistics are in the range of 7–9, suggesting some caution is warranted in interpreting the precision of these estimates. These implied returns are somewhat higher than conventional IV estimates of the average return to compulsory schooling, which range from 10 to 14 percent (see, e.g., Card, 1999; Acemoglu and Angrist, 2000; Harmon and Walker, 1995), but are consistent with IV estimates that identify the effect of schooling specifically for students at the margin of dropping out. A well-established finding is that such IV estimates exceed OLS estimates in part because instruments tend to identify returns for students with particularly high gains from schooling (Oreopoulos, 2006; Carneiro et al., 2011). Exploiting changes in compulsory schooling laws across the US, Canada, and the UK, Oreopoulos (2007a) estimates that staying in school an additional year increases lifetime wealth by roughly 17 percent under IV estimation. Using a regression discontinuity around college admission thresholds in Florida, Zimmerman (2014) finds earnings gains of 22 percent, with even larger effects for men and students of low-income backgrounds. In both cases the returns are largest for disadvantaged students, for whom credit constraints are a plausible explanation (see also Oreopoulos, 2007b). By contrast, as I will document, the young men who dropped out in response to the tax-free year came predominantly from families with above-median incomes, making liquidity constraints an unlikely explanation for their high implied returns. Moreover, even for students who were credit-constrained, theory predicts that a transitory increase in take-home wages should have increased rather than reduced permanent schooling: a constrained student who drops out temporarily to accumulate savings would rationally return to school once the tax holiday ends, raising completed attainment if anything. The high implied returns therefore point instead to mechanisms such as misperceptions about the returns to schooling or barriers to re-entry that cause students who would benefit financially from continued education to drop out permanently. I examine these mechanisms directly in the analysis that follows.[11]

General Equilibrium Effects A key concern is that the estimated effects on earnings may partly reflect general equilibrium responses in the labor market rather than the direct consequences of dropping out. Specifically, the decline in degree completion within affected cohorts reduces the supply of educated workers, potentially widening the wage gap between high school graduates and dropouts and violating the stable unit treatment value (SUTVA) assumption. I assess the magnitude of this channel through a calibration exercise and a direct empirical test. A related but distinct general equilibrium effect concerns the dropout decision itself: the tax-free year may have expanded labor demand, facilitating entry for young workers and amplifying the dropout response beyond what individual incentives alone would predict. I address each channel in turn.

The magnitude of the wage spillover channel depends on the elasticity of substitution between workers across different birth cohorts. In a seminal contribution, Card and Lemieux (2001a) develop a model incorporating imperfect substitutability between workers of different ages but similar education levels. I apply their framework to calibrate the extent to which these effects influence my estimated effects on earnings. In the model, aggregate output depends on two CES sub-aggregates of low-educated (L) and high-educated (H) workers, where low-educated workers are those with only compulsory schooling (dropouts), and high-educated workers are high school graduates (see Appendix B for a full description of the model). The relationship between the change in the wage gap between low-educated workers in cohort j, w^L_{jt}, and high-educated workers in the same cohort, w^H_{jt}, relative to the adjacent cohort j', is given by the following equa-

[11] It is worth noting that the earnings losses I estimate do not reflect post-1987 trends in returns to education or in the prospects of blue-collar workers. Appendix Figure A.19 shows that Mincerian returns to schooling were relatively stable throughout the sample period in both urban and rural areas, and that there was no systematic deterioration in the earnings premia of fishing, manufacturing, and construction in the decades following the reform.

tion:

$$\Delta \ln \left(\frac{w_{jt}^L}{w_{jt}^H} \right) = -\frac{1}{\sigma_A} \Delta \ln \left(\frac{L_{jt}}{H_{jt}} \right) \tag{2}$$

where σ_A is the elasticity of substitution between workers in different birth cohorts but with the same education, and Δ denotes the difference between an affected cohort j, subject to the tax-free year, and an adjacent, unaffected cohort j'.

Equation (2) provides a framework for calibrating the magnitude of wage spillovers on the affected cohorts. I estimate that the tax-free year led to an 8 percent decline in post-compulsory education completion and a 5.1 percent decline in earnings for affected cohorts. Figure A.1 plots the share of the estimated earnings effect that can be explained by general equilibrium wage adjustment as a function of σ_A. Drawing on estimates from across the literature for the U.S. and European labor markets, the implied contribution of wage spillovers is small—at 13.6% for $\sigma_A = 11.1$ and as little as 5% for $\sigma_A = 30$. A full discussion of the relevant estimates of σ_A and the robustness of this conclusion is provided in Appendix B.

As a direct empirical test of wage spillovers, Figure A.21 examines the earnings effects of the tax-free year on cohorts that were too young to respond to the reform—those still of compulsory schooling age at the time of the reform and therefore unable to adjust their educational attainment. Any earnings effects on these cohorts must operate through labor market spillovers rather than human capital accumulation. Under imperfect substitution, the increase in low-educated labor supplied by the affected cohorts should raise the relative wages of high-educated workers in nearby cohorts, generating positive earnings effects for non-affected cohorts on net. I find no evidence of this: the estimated earnings effects for non-affected cohorts are small and statistically indistinguishable from zero. This result corroborates the calibration evidence that wage spillovers account for only a limited share of the estimated earnings losses of the affected cohorts.[12]

A distinct general equilibrium effect concerns the dropout decision itself. As documented in Sigurdsson (2025), the tax-free year generated a strong labor supply response among adults across the labor market. Appendix Figure A.22 shows that these responses were stronger in rural than urban areas and in manual occupations—precisely the segments where school dropout was concentrated (see Figures 6 and 11). If older and younger workers are complements in production, this expansion in labor market activity may have facilitated entry for young workers, making jobs easier to find and amplifying the dropout response beyond what individual incentives alone would predict. In this sense, the estimated dropout effect should be interpreted as an equilibrium outcome, reflecting both the direct incentive effects of the tax-free year and this labor demand channel. Importantly, however, Sigurdsson (2025) documents that the labor supply responses of older workers were entirely transitory, reverting to pre-reform levels in the year following the tax-

[12] A further potential spillover operates through school quality: if male dropout reduced class sizes, remaining students may have benefited from improved learning conditions, potentially boosting the earnings of both men and women who stayed in school. This channel would therefore predict positive earnings effects among women, who stayed in school while class sizes fell, as well as among men who completed their education. The absence of any such effect among women, documented in Figure 8, argues against this mechanism being quantitatively important.

free year.[13] The same conditions that facilitated entry during the tax-free year did not persist, and so this channel cannot account for the permanent character of the dropout effect.

Robustness I assess the sensitivity of my results through several robustness checks. Appendix Figure A.14 examines how the estimated effects on labor income at both upper-secondary schooling age and prime age vary with the bandwidth, ranging from 6 to 62 months, applied either to one side (control) or both sides (control and treatment) of the threshold. With the exception of very narrow bandwidths around the compulsory schooling age, the estimates remain stable in magnitude and statistical precision, closely aligning with those obtained using the chosen bandwidth. This includes estimates based on MSE-optimal bandwidths, which are marked in the figure.

Appendix Table A.4 further tests robustness by estimating equation (1) using the bias-corrected RD estimator of Calonico et al. (2014). The table presents results across different specifications, varying controls, kernel weights, and polynomial orders. The consistency of estimates across these specifications confirms the robustness of my findings.

4.2 Career Progression

To measure career progression, I rank all workers within firms or industries by their residualized earnings, controlling for gender and age. Figure 9 presents the estimated effect on within-firm or within-industry rank over the life cycle. Workers in affected cohorts start, if anything, at a higher relative pay than the control group, but by prime age, their earnings rank is approximately 4 percent lower. The earnings losses thus reflect fewer promotions and slower career advancement rather than lower starting wages. This finding aligns with evidence that most lifetime earnings growth for young men occurs through career advancement during the first decade in the labor market (Topel and Ward, 1992).[14] Consistent with this, Appendix Figure A.15 shows that affected men remain disproportionately employed in fishing, manufacturing, and construction at prime age, suggesting that early career exposure to these industries during the tax-free year had lasting effects on occupational trajectories, with potential direct earnings implications.

4.3 Wealth and Non-Labor Income

Is the financial impact of dropout mitigated through the accumulation of wealth, e.g. by dropouts beginning to save or purchasing real estate earlier than those that stay longer in school? To investigate this I estimate the effect on wealth and non-labor income at prime age. The results are presented in Table 3. For men at prime age, I estimate a negative effect on total wealth of $15,500 or a loss of 8.3 percent when compared to the below-threshold average. This effect is primarily

[13] Firm investment responded similarly: Sigurdsson (2025) documents elevated growth in the capital stock—primarily machines, equipment, and buildings—in 1987 relative to surrounding years, consistent with firms front-loading investment in anticipation of increased labor supply. This investment expansion was likewise transitory, reverting to pre-reform levels the following year.

[14] Appendix Figure A.16 examines whether dropouts entered firms or industries with systematically different pay premiums using the AKM decomposition (Abowd, Kramarz, and Margolis, 1999). Dropouts appear to have entered through firms and industries offering relatively high starting pay, with no difference in pay premiums at prime age.

driven by less real-estate wealth with a smaller and not statistically significant effect on financial wealth. In addition, I estimate a negative, but not statistically significant, effect on non-labor income, including capital income. In sum, this implies that while the impact of school dropout on future finances is primarily mediated through the labor market, this impact seems to have been enlarged through reduced saving and wealth accumulation.

4.4 Marriage and Fertility

In addition to its negative impact on careers, dropping out of school may also affect men's family formation. There are two main channels. First, economic theory suggests that men's economic prospects influence marriage decisions: in the Becker (1973) framework, marriage gains arise from spousal specialization, so a decline in men's earnings reduces women's gains from marriage and shrinks the pool of "marriageable" men (Wilson and Neckerman, 1986). Second, extensive research documents educational assortative mating (Mare, 1991; Pencavel, 1998) and the role of schools as marriage markets (Blossfeld, 2009; Kirkebøen, Leuven, Mogstad, and Mountjoy, 2021).

To explore this, I estimate the effects on marriage and fertility in the affected cohorts. The results, shown in Figure 10, indicate that male dropouts were significantly less likely to marry or have children.[15] By age 40, affected men were about 6 percent less likely to be married, 4.8 percent less likely to have children, and had on average 0.10 fewer children—a 5 percent decline.[16] In contrast, there was no reduction in marriage or fertility among affected women. These findings align with earlier research showing that declines in men's earnings and employment due to external shocks—such as downturns in coal and steel industries, increased import competition from China, and the U.S. fracking boom—led to reductions in marriage and fertility (Black et al., 2003; Autor et al., 2019; Kearney and Wilson, 2018; Schaller, 2016). Comparing the estimated effects on marriage and fertility to prior studies of windfall income effects (Cesarini et al., 2021; **?**; Lovenheim and Mumford, 2013) suggests that income effects from permanent earnings losses may partly account for the findings, though the estimated effects are generally larger in magnitude.

5 Model of School Dropout

Building on the human capital investment framework (Mincer, 1958; Becker, 1962), I develop a model of schooling choice that incorporates ability heterogeneity, quasi-hyperbolic discounting, liquidity constraints, psychic costs, re-entry frictions, and misperceptions about returns to education. The model identifies which combinations of these ingredients can generate the permanent dropout and associated earnings losses observed in the data, and which cannot.

[15] Figure A.17 in the Appendix plots the RD graphs for marriage and fertility by gender.

[16] Between 1991 and 2000, the average age at first marriage was 31.2 years for men and 29 years for women, and the average age of first-time fathers was 27.6 years, with a fertility rate of 2.1 children.

5.1 Setup

There is a unit mass of infinitely-lived young adults who have completed compulsory schooling. In period $t = 0$, they choose between continuing schooling for an additional year or leaving school to work. If they stay in school, they graduate in a year and then enter the labor market in $t = 1$. If they drop out, they can choose to re-enter in $t = 1$ or leave school permanently and stay in the labor market. In periods $t = 2, \ldots, \infty$ everyone works. Individuals derive utility from consuming their disposable income every period. Therefore, they choose their level of schooling to maximize the present discounted value of lifetime earnings, net of costs, discounting future income at the rate $\beta\delta^t$, where $\delta \in (0, 1)$ represents the exponential discount factor, and $\beta \leqslant 1$ accounts for potential myopia or present bias (Phelps and Pollak, 1968; Laibson, 1997).

Attending school entails a cost κ, which captures both direct monetary costs—such as tuition and living expenses—and non-monetary or psychic costs.[17] Individuals without post-compulsory education earn a fixed income in each period, normalized to unity. In contrast, individuals with post-compulsory education earn a return, ρ, proportional to their economic ability, θ. Ability is assumed to be uniformly distributed, $\theta \sim U[0, 1]$. Income is subject to tax, which is τ_0 in the initial period and assumed constant, $\tau_t = \tau$, in all periods $t > 0$.

In the initial time period, individuals therefore face the choice between three lifetime streams of utility:

$$\text{Stay in school:} \quad -\kappa + (1-\tau)\rho\theta \sum_{t=1}^{\infty} \beta\delta^t \tag{3}$$

$$\text{Temporary dropout:} \quad (1-\tau_0) - \beta\delta\,\kappa + (1-\tau)\rho\theta \sum_{t=2}^{\infty} \beta\delta^t \tag{4}$$

$$\text{Permanent dropout:} \quad (1-\tau_0) + (1-\tau) \sum_{t=1}^{\infty} \beta\delta^t. \tag{5}$$

Individuals choose to attend school if the marginal benefit exceeds the marginal cost. Comparing utility streams (3) and (5) provides the following schooling condition:

$$\underbrace{\frac{\beta\,\delta}{1-\delta}\,(1-\tau)\,(\rho\theta - 1)}_{\text{Marginal benefit}} \geqslant \underbrace{(1-\tau_0)}_{\text{Opportunity cost}} + \underbrace{\kappa}_{\text{Direct cost}} \tag{6}$$

which shows that young adults choose to attend school if the earnings gain exceeds the marginal cost, which consists of the opportunity cost—the net-of-tax earnings students must give up to attend school—and the direct cost of schooling, including the psychic cost.

Using the three utility streams, we can derive thresholds that split the population in three

[17] In the benchmark model, schooling costs are assumed to be constant over time and students do not face liquidity constraints. Appendix C relaxes these assumptions and shows that while liquidity constraints affect who drops out initially, they do not generate permanent dropout, because all induced leavers optimally return to school once the tax-free year ends.

segments based on their schooling decisions. First, from (3) and (4), we get a threshold for young adults to stay in school in $t = 0$:

$$\theta_0^* = \frac{(1-\tau_0) \ + \ \kappa(1-\beta\delta)}{\beta\,\delta\,(1-\tau)\,\rho} \tag{7}$$

Second, viewed from the $t = 1$ vantage point after working in $t = 0$, when the re-entry decision is made, the individual compares returning to school with remaining in the labor market. Returning to school entails the opportunity cost of forgone earnings, $(1-\tau)$, as well as the schooling cost κ. This yields the re-entry threshold:

$$\theta_1^* = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau)+\kappa\big]}{\beta\,\delta\,(1-\tau)\,\rho}. \tag{8}$$

The two thresholds partition the population of young adults into three segments based on their ability level, as illustrated in Figure 1. First, students with $\theta \geqslant \theta_0^*$ stay in school in $t = 0$. Second, students with $\theta_1^* \leqslant \theta < \theta_0^*$ leave school in $t = 0$, but return in $t = 1$. Third, students with $\theta < \theta_1^*$ leave school in $t = 0$ and never return.

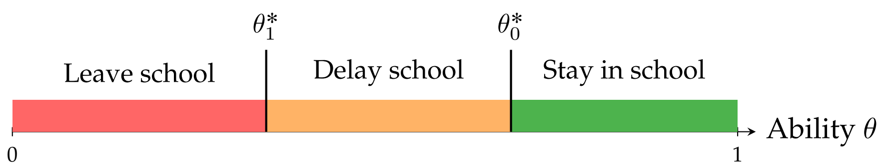

Figure 1: Schooling decisions by ability

5.2 Temporary Dropout

To evaluate the effect of the tax-free year ($\tau_0 < \tau$) on school dropout, we study the comparative statics of the stay-in-school threshold (7) and the re-entry threshold (8). Differentiating θ_0^* and θ_1^* with respect to τ_0 yields

$$\frac{\partial\theta_0^*}{\partial\tau_0} = -\frac{1}{\beta\delta(1-\tau)\rho} < 0, \qquad \frac{\partial\theta_1^*}{\partial\tau_0} = 0.$$

As a result, the tax-free year leads a share of students to drop out temporarily in $t = 0$ and return in $t = 1$, but does not affect the share of individuals who permanently leave school.

Appendix C extends this benchmark model and investigates the implications of liquidity constraints, psychic costs, time-discounting, and present bias. Liquidity constraints and psychic costs shift the set of marginal students, while impatience and present bias amplify the initial dropout response to the tax-free year. Importantly, in the absence of frictions at the re-entry margin, these extensions do not alter the central prediction of the benchmark model: the tax-free year generates only temporary dropout. As discussed below, future discounting matters primarily through its interaction with re-entry frictions, reinforcing their effect in generating permanent dropout.

Figure 2, panel (a), illustrates the effect of the tax-free year on temporary dropout and the associated effect on earnings. To show the implied earnings consequences, the figure plots the earnings gain (or loss) from temporary dropout relative to staying in school:

$$\Delta\text{Earnings}^T(\theta) \equiv (1-\tau_0) + \kappa(1-\beta\delta) - \beta\delta(1-\tau)\rho\,\theta. \tag{9}$$

The figure shows that individuals with $\theta \in [\theta_1^*,\, \theta_0^*]$ drop out of school temporarily during the tax-free year and experience a positive earnings gain, with the total earnings gain corresponding to the shaded blue area, reflecting the integral of (9) over the range $[\theta_1^*, \theta_0^*]$.

5.3 Permanent Dropout

The benchmark model and its extensions predict that, absent frictions at the re-entry margin, a tax-free year generates only temporary dropout: students respond to the short-run incentive by leaving school in $t = 0$ but optimally return once the incentive expires. This subsection shows how distortions at the re-entry stage—through re-entry costs or misperceived returns—can rationalize the permanent dropout and associated large earnings losses observed in the data.

Re-entry costs Now let us assume, as before, that students who leave school in $t = 0$ can return in $t = 1$, but returning to school is subject to a cost $\gamma > 0$. This cost captures possible financial costs, consumption commitments, psychological costs, or organizational barriers to re-enrollment. Moreover, let us first assume γ is known at the time of the initial schooling decision.

As before, the tax-free year affects the initial enrollment cutoff but not the re-entry cutoff. Anticipated re-entry costs also affect the level of both cutoffs: they reduce the incentive to leave school initially, because students internalize the cost of returning, and they raise the cutoff for re-entry. However, whether the students induced to leave drop out temporarily or permanently depends on the position of the re-entry cutoff θ_1^* relative to the set of students who leave school as a result of the tax-free year, $[\,\theta_0^*(\tau),\, \theta_0^*(\tau_0)\,)$. As shown in Appendix C, this comparison yields two critical threshold values for the re-entry cost:

$$\gamma_T \equiv \frac{\delta(1-\beta)\big[(1-\tau)+\kappa\big]}{1-\delta(1-\beta)}, \qquad \gamma_P \equiv \frac{(\tau-\tau_0)+\delta(1-\beta)\big[(1-\tau)+\kappa\big]}{1-\delta(1-\beta)}.$$

These thresholds imply that: (i) if $\gamma \leqslant \gamma_T$, all induced leavers re-enter in $t = 1$ and the tax-free year generates only temporary dropout; (ii) if $\gamma_T < \gamma < \gamma_P$, the tax-free year induces a mix of temporary and permanent dropout; and (iii) if $\gamma \geqslant \gamma_P$, none of the induced leavers return and the tax-free year generates only permanent dropout. Permanent dropout therefore does not arise because the tax-free year changes the value of schooling per se, but because it interacts with frictions that prevent students from returning once they have left. The tax-free year determines who leaves initially, while re-entry costs determine whether that initial response becomes permanent. Both thresholds depend on discounting: lower values of δ or β reduce the value of future schooling returns and can amplify the effect of re-entry frictions, while the exact thresholds also reflect

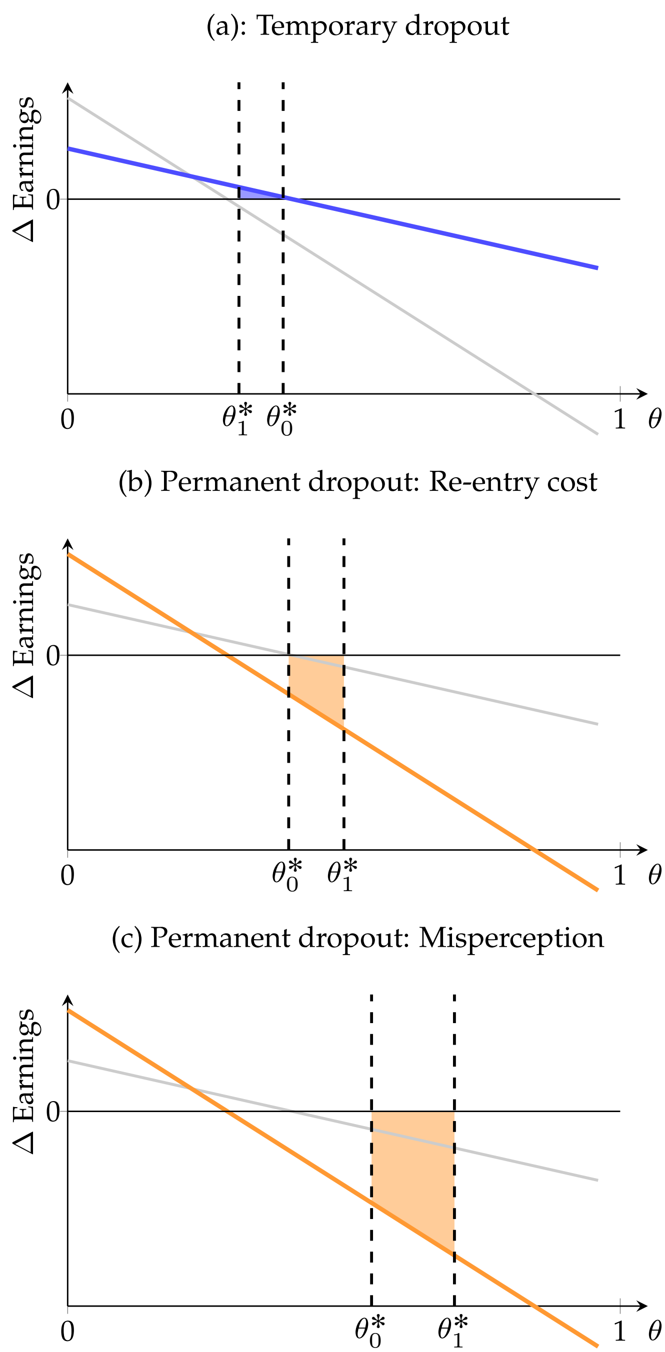

Figure 2: The Effect of Tax-Free Year on Dropout and Earnings

Note: This figure illustrates the effect of a tax-free year on schooling decisions and earnings. Panel (a) shows the benchmark case, in which the tax-free year induces only temporary dropout: some students leave school in $t = 0$ but return in $t = 1$, generating a short-run earnings gain. The solid blue line shows earnings from temporary dropout relative to staying in school, while the solid gray line shows earnings under permanent dropout for reference. The shaded blue area represents the aggregate earnings gain from temporary dropout. Panel (b) introduces re-entry costs, which can turn initial dropout into permanent dropout. In the case shown, some students who leave school in response to the tax-free year do not return in $t = 1$. The solid orange line shows earnings from permanent dropout relative to staying in school, and the shaded orange area represents the associated long-run earnings loss. Panel (c) introduces misperception about returns to schooling. When students perceive returns to schooling as lower than they truly are, the relevant dropout and re-entry thresholds rise. As a result, the students induced to leave school by the tax-free year are of higher ability, and permanent dropout generates larger long-run earnings losses.

the composition of students induced to leave. In the benchmark case with $\beta = 1$, the thresholds simplify to $\gamma_T = 0$ and $\gamma_P = \tau - \tau_0$: the tax-free year determines who leaves school initially, while even modest incremental re-entry costs can turn those initial exits into permanent dropout.

While some components of the re-entry cost γ—such as organizational frictions—are likely to be known in advance, others—such as consumption commitments or the psychic costs of returning to school—may not be fully anticipated. We therefore also consider the case in which re-entry costs are unanticipated at the time of the initial schooling decision. This assumption captures both the possibility that students hold incorrect beliefs about re-entry costs and that their circumstances change between $t = 0$ and $t = 1$ in ways that make returning to school costly. In this case, students who leave school in $t = 0$ discover in $t = 1$ that re-entering entails a cost $\gamma > 0$, which raises the re-entry cutoff to

$$\theta_1^*(\gamma) = \theta_1^*(0) + \frac{\gamma(1-\delta)}{\beta\delta(1-\tau)\rho} \; > \; \theta_1^*(0).$$

Unanticipated re-entry costs are particularly potent because they distort decisions at the re-entry stage without being internalized at $t = 0$. As shown formally in Appendix C, this mechanism strictly increases permanent dropout relative to the anticipated-cost case for any given γ, since some students leave school expecting to return but are later deterred by unexpectedly high re-entry barriers.

Figure 2, panel (b), illustrates how the tax-free year affects dropout and earnings when students face re-entry costs. The figure shows the case in which the re-entry cutoff lies inside the set of students induced to leave school by the tax-free year. Induced leavers below the re-entry cutoff do not return and therefore become permanent dropouts. To characterize the earnings consequences, we plot the function for the earnings gain or loss from permanently dropping out relative to staying in school:

$$\Delta\text{Earnings}^P(\theta) \equiv (1-\tau_0) + \kappa + (1-\tau)\frac{\beta\delta}{1-\delta} - (1-\tau)\frac{\beta\delta}{1-\delta}\rho\,\theta. \tag{10}$$

As the figure shows, individuals who drop out permanently incur an earnings loss from permanent dropout. The magnitude of this loss corresponds to the shaded orange region, obtained by integrating the permanent-dropout earnings function over the induced leavers who remain below the re-entry cutoff.

Misperceived Returns to Education I now extend the model to allow individuals to hold incorrect beliefs about the return to schooling. Following the subjective-expectations framework in Manski (1993), individuals form beliefs about the earnings return ρ based on the educational and labor market outcomes they observe in their reference group—parents, peers, coworkers, or others in their social environment. Such misperception generates a distortion of the true return:

$$\mathbb{E}_t[\rho] = (1-\varepsilon_t)\rho$$

where ε_t is a (possibly time-varying) misperception parameter. If $\varepsilon_t = 0$, beliefs are correct; if $\varepsilon_t > 0$, the return is underestimated; and if $\varepsilon_t < 0$, it is overestimated.

Misperception about returns to schooling affects both the initial school-leaving decision and the re-entry margin. Underestimating returns raises the stay-in-school and re-entry thresholds,

$$\frac{\partial \theta_0^*}{\partial \varepsilon_0} = \frac{\theta_0^*}{1-\varepsilon_0} > 0, \qquad \frac{\partial \theta_1^*}{\partial \varepsilon_1} = \frac{\theta_1^*}{1-\varepsilon_1} > 0,$$

making schooling less attractive at both stages. Pessimistic beliefs also amplify the absolute response to the tax-free year at enrollment. Since

$$\frac{\partial^2 \theta_0^*}{\partial \tau_0 \, \partial \varepsilon_0} = -\frac{1}{\beta\delta(1-\tau)\rho(1-\varepsilon_0)^2} < 0,$$

the derivative $\partial \theta_0^* / \partial \tau_0$ becomes more negative as ε_0 rises. Equivalently, a reduction in τ_0 raises θ_0^* more when perceived returns are lower. Misperception also increases the sensitivity of re-entry decisions to re-entry frictions:

$$\frac{\partial^2 \theta_1^*}{\partial \gamma \, \partial \varepsilon_1} = \frac{1-\delta}{\beta\delta(1-\tau)\rho(1-\varepsilon_1)^2} > 0.$$

Misperception can therefore generate permanent dropout through two related channels. If students evaluate the initial decision as one between staying in school and leaving permanently, sufficiently pessimistic beliefs can rationalize permanent dropout. If instead students initially plan to return, permanent dropout can arise if perceived returns at the re-entry margin are sufficiently low relative to the beliefs that governed the initial dropout decision, or if re-entry is costly. For example, after leaving school, students may form or update their beliefs based on exposure to environments where returns to education are perceived to be low. Such pessimistic beliefs at the re-entry margin raise the perceived re-entry threshold ex post and can convert an initially temporary response into permanent dropout.

Figure 2, panel (c), shows the effect of the tax-free year on schooling decisions and earnings when students hold pessimistic beliefs about returns to schooling. By lowering the perceived return to schooling, misperception raises the relevant dropout and re-entry thresholds. As a result, the students induced to leave school by the tax-free year can include higher-ability individuals, for whom the long-run earnings loss from permanent dropout is larger.

Empirical implications The model helps restrict the set of mechanisms that can explain the permanent dropout and associated earnings losses observed in the data. Permanent dropout arises only when the initial response to the tax-free year interacts with distortions that impede re-entry. Two such distortions are central: re-entry costs and misperception about returns to schooling. These mechanisms operate on the same margin and are complementary: pessimistic beliefs make re-enrollment less attractive and expand the set of students for whom re-entry frictions can turn initial dropout into permanent dropout. Discounting does not by itself generate permanent dropout in response to a temporary shock, but it can amplify these mechanisms by increasing the weight placed on short-run gains relative to long-run returns. These implications guide the empirical analysis that follows.

6 Interpreting the Empirical Results

Guided by the model, this section evaluates the mechanisms that can explain why the tax-free year generated permanent school dropout and sizable earnings losses. The model highlights three candidates: misperception of returns to schooling, re-entry costs, and future discounting. I examine each mechanism in turn, using heterogeneity in effects, auxiliary outcomes, and model-based calibration to assess their empirical plausibility.

6.1 Misperceived Returns to Education

Misperception about the returns to schooling is a natural candidate mechanism in this setting. Students do not observe the true returns to education directly. Instead, they infer them from the outcomes of people around them—parents, peers, and neighbors (Manski, 1993). Because the information available from this local environment may be incomplete or systematically biased, perceived returns can diverge substantially from actual ones (Jensen, 2010; Nguyen, 2008). I assess this channel in two ways. First, through heterogeneity in dropout responses across parental background and neighborhood characteristics that plausibly shape beliefs about returns. Second, through a model-based calibration that quantifies the degree of misperception needed to explain the observed dropout and earnings effects, which I validate against both internal and external benchmarks.

Figure 11 reports estimates of the effect of the tax-free year on school dropout among men, allowing the response to vary by parental and neighborhood characteristics.[18] The results reveal pronounced heterogeneity consistent with misperception about returns to schooling. The dropout response is concentrated among sons of parents without a high school degree—who either dropped out of high school themselves or never enrolled. This suggests that these students grew up in households with limited firsthand information about the gains from post-compulsory education. By contrast, splitting by parental income shows that dropout responses are not driven by low-income families and if anything are larger among students from above-median-income households. This pattern directly contradicts a liquidity constraints explanation, under which we would expect the response to be concentrated among poorer families.

A more direct test of the information channel uses parents' realized returns to education. I estimate cohort-specific Mincerian returns to schooling for parents and classify them according to whether their earnings exceed or fall short of what their education would predict. The dropout response is concentrated almost entirely among sons of parents with low realized returns—precisely

[18] Appendix Figure A.18 presents corresponding results for women.

the families whose observed outcomes would convey pessimistic signals about the payoff to schooling, leading children to underestimate returns.

The neighborhood-level results point to the same conclusion. Dividing students by rural versus urban residence shows that the dropout response is driven primarily by individuals growing up in rural areas.[19] Classifying students by the educational composition of their municipality yields the same pattern: dropout effects are concentrated in areas with lower adult educational attainment. Splitting by local returns to education reveals substantially larger dropout responses in municipalities where returns to schooling are low.[20]

These patterns align closely with experimental evidence on information frictions in schooling decisions. Providing students with accurate information about returns reduces dropout and improves school performance, with the largest effects occurring among relatively better-off students rather than the poorest (Jensen, 2010; Nguyen, 2008). This mirrors the parental income finding above: the dropout response to the tax-free year was larger among students from above-median-income households, consistent with information frictions rather than liquidity constraints driving the response. To assess how large misperception about returns would need to be to explain the observed dropout and earnings effects, I now turn to a model-based calibration.

To quantify the degree of misperception consistent with the observed dropout and earnings effects, I calibrate the model using estimated treatment effects and parameter values from the data and the literature. Appendix D describes the procedure and parameter choices in detail.

The calibration depends on how the marginal student evaluates the schooling decision, and I consider two specifications of the marginal student's choice problem. In the first, the marginal student compares staying in school to leaving permanently, without expecting to return. This specification is supported by the empirical finding that the tax-free year did not generate temporary dropout among men (Figure 5): students who left school during the tax-free year did not return, suggesting they may not have anticipated doing so. This calibration implies underestimation of returns of 10–21 percent, depending on parameter values.

In the second, students initially plan to return but subsequently fail to do so, so that permanent dropout arises from the interaction between misperceived returns and re-entry frictions. If there is no re-entry cost beyond the ordinary opportunity cost of returning to school, the calibration implies underestimation of returns of 26–35 percent. As the re-entry cost rises, the required misperception falls: when the re-entry cost equals the change in the opportunity cost induced by the tax-free year, the second calibration coincides with the first, implying underestimation of 10–21 percent. At the other extreme, setting misperception to zero implies that re-entry costs alone

[19]These patterns may also reflect social spillovers in schooling decisions. If a critical mass of peers with low expected returns to education leaves school for work, other students—including those with high underlying returns—may follow due to peer pressure, conformity, or concerns about short-run income or status. Such socially driven exits operate through the same information channels emphasized here, reinforcing pessimistic beliefs about the returns to schooling.

[20]A substantial portion of Iceland's rural population resided in small towns dominated by fishing, fish processing, and manufacturing industries, collectively accounting for roughly 40 percent of local income. These sectors typically offer high wages but low returns to formal education, resulting in lower educational attainment than in urban regions (Nakamura, Sigurdsson, and Steinsson, 2022).

can rationalize permanent dropout only if they are 29–44 percent of annual earnings for workers without post-compulsory education. Thus, permanent dropout can be rationalized by moderate misperception, by substantial re-entry costs, or by a combination of the two.

I benchmark the calibration against both internal and external evidence (see Appendix D for details). The internal benchmark constructs an independent estimate of misperception from parental earnings. The key idea is that children observe the outcomes of their own parents, not the full population. A parent with high earnings relative to their education level conveys a pessimistic signal about the returns to schooling, even if the true population return is high. This is precisely the type of family background from which the dropout response is concentrated: as shown in Figure 11, the effects are strongest among students whose parents earn more than their education would predict. To quantify the magnitude of the resulting belief distortion, I estimate cohort-specific Mincerian returns to schooling using parental earnings data, and then re-estimate the return among fathers whose earnings exceed what their education would predict. The ratio of the restricted to the full-sample return yields an estimate of misperception. This exercise implies that students underestimate returns by approximately 17 percent, which falls within the 10–21 percent range implied by the calibration when students do not consider re-entry or face moderate re-entry costs.

The external benchmark draws on Jensen (2010), who directly elicits expected earnings by schooling level among boys at the end of compulsory schooling in the Dominican Republic and contrasts these beliefs with observed earnings differences. Mapping his estimates into the misperception parameter of the model implies underestimation of returns of 74–78 percent. These estimates are substantially larger than the misperception required by the calibration, suggesting that the belief distortions needed to rationalize the findings are well within the range observed in settings where beliefs are directly measured.

6.2 Re-entry Costs

The model predicts that permanent dropout may arise if students who leave school during the tax-free year overestimate how easily they will be able to return. The model formalizes this as a re-entry cost, which may be anticipated at the time of exit or realized only afterward, and which can take several forms: organizational frictions in re-enrollment, financial or consumption commitments that raise the cost of returning, or psychological costs associated with resuming studies. I assess the empirical relevance of each channel in turn.

Organizational frictions The Icelandic upper-secondary system is institutionally flexible: it accommodates slow progression, permits re-entry after dropout, and formally accepts students of any age (Blöndal et al., 2010). Organizational frictions are therefore unlikely to be the primary barrier preventing dropouts from returning to school.

This is borne out by the evidence. Appendix E documents upper-secondary school trajectories for the 1975 birth cohort using administrative records on enrollment spells, course-taking, and credits earned, collected directly from all Icelandic upper-secondary schools by Jónasson and Blöndal (2002). Students frequently take breaks or progress at a slower pace than required for on-time completion, and among those who eventually graduate, roughly half of the delay beyond four years is accounted for by semesters on break. That leaving and returning is common even among graduates confirms that the system poses few institutional obstacles to re-enrollment.

Appendix Figure A.3 shows that these patterns generalize well beyond the 1975 cohort. The distribution of age at graduation has been stable from the early 1980s through the 2010s: roughly 60 percent graduate on time at age 20, while 15–20 percent complete at ages 21 or 22 and roughly 10 percent at age 23 or older. The consistency of these patterns across four decades confirms that the 1975 cohort evidence is representative of the broader Icelandic context.

Financial or consumption commitments A second potential source of re-entry costs is financial or consumption commitments undertaken after leaving school. Students who leave school and begin working may take on obligations—debt, a car, or early parenthood—that make the income loss associated with returning to school increasingly costly over time. Even if they initially intended to return, such commitments could lock them into continued employment. Appendix Table A.6 tests this channel by estimating the effect of the tax-free year on early-life outcomes at ages 16–20: indicators of having a child, owning a car, holding any debt, and holding high debt. The estimates provide little support for this mechanism: the tax-free year does not increase the likelihood of early parenthood or car ownership, and while there is a small and marginally significant effect on the probability of holding any debt, there is no effect on high debt and the magnitudes are small throughout.[21] Financial commitments therefore appear unlikely to account for the low rate of return among dropouts.

Psychological costs The absence of organizational barriers and limited evidence for consumption commitments suggests that re-entry costs, if operative, are most plausibly psychological. To assess this channel directly, I draw on survey evidence from upper-secondary school dropouts in the 1975 birth cohort (Jónasson and Blöndal, 2002).

Appendix Figure A.4 presents the survey responses on dropout decisions and re-entry barriers. The reasons dropouts cite for leaving school are heterogeneous, but one in four cite good job opportunities or financial difficulties, consistent with opportunity costs playing an important role in the dropout decision. When asked to retrospectively assess their decision, 43 percent say dropping out was the wrong choice given the circumstances they faced at the time, and 47 percent say they would be better off in the labor market today had they completed upper-secondary school. Yet when asked whether returning to school would be easy or difficult, 54 percent report it would be difficult. This gap—between recognizing the value of a degree and acting on that recognition—points toward psychological switching costs as a primary barrier to re-entry.

Complementary evidence from the postal survey points in the same direction. Appendix Figure A.5 shows that among dropouts, 45 percent fall in the lowest third of the self-esteem distri-

[21] I also examine real estate ownership, but very few individuals own real estate at these ages and I find no treatment effect.

bution, compared to only 27 percent among graduates. While the cross-sectional nature of the data makes it difficult to establish whether low self-esteem precedes dropout or is partly a consequence of it, the pattern is consistent with a broad literature showing that noncognitive traits independently predict schooling attainment beyond cognitive ability (Jacob, 2002; Heckman et al., 2006b).

These findings connect naturally to models of schooling that allow for heterogeneous psychic costs. Such models find that psychic costs are quantitatively important in explaining why individuals with positive financial returns to education nevertheless do not pursue further schooling (Cunha et al., 2005; Heckman et al., 2006a). Interruption of schooling may lower academic self-confidence or shift identity away from the student role, raising the subjective cost of re-entry even in the absence of formal barriers. Such mechanisms generate path dependence in schooling choices: once individuals exit, returning becomes psychologically costly—and the longer they remain out, the more costly it becomes.

6.3 The Role of Future Discounting

An alternative explanation for permanent dropout is that students heavily discount future returns, prioritizing immediate earnings over higher future wages. As outlined in the model, individuals may discount the future through a quasi-hyperbolic discount function with present-bias parameter $\beta < 1$ or through a high exponential discount factor. While the model shows that impatience alone does not lead to permanent dropout in response to the tax-free year, it amplifies the behavioral response and reinforces the effect of re-entry costs. A natural question is whether discounting alone—or in combination with re-entry frictions—can explain the observed permanent dropout, even in the absence of misperception about returns to schooling.

To assess this possibility, I calculate the discount rates required to rationalize permanent dropout when students correctly perceive returns to schooling. I compare the decision to stay in school against permanent dropout, abstracting from the possibility of temporary exit.[22] This comparison already incorporates re-entry frictions, as it assumes students who leave never return. The question is whether realistic discount rates can make permanent dropout optimal given the actual returns to education.

Implied present bias I calculate the present-bias parameter β required to equate the immediate earnings gain from dropping out with the present discounted value of subsequent earnings losses, assuming an exponential discount rate of 5 percent.[23] This calculation implies $\beta = 0.23$.

This value lies well outside the empirically plausible range. A meta-analysis of experimental studies finds average values of β between 0.95 and 0.97, with no studies reporting values below 0.5 (Imai et al., 2021).

Implied exponential discount rate An alternative approach calculates the exponential discount

[22]This calculation is similar in nature to the permanent-dropout calibration of misperception in Appendix D.

[23]This rate is consistent with Iceland's average five-year indexed interest rate of 4.94 percent in 1986.

rate δ that equates the earnings gain from dropping out with the present discounted value of subsequent losses, assuming no present bias ($\beta = 1$).[24] The discount rate that satisfies this condition is 18.7 percent (see Appendix Figure A.20).

While adolescents exhibit higher discount rates than adults (Gruber, 2001; Bettinger and Slonim, 2007; Steinberg et al., 2009; Castillo et al., 2011; Levitt et al., 2016), the implied rates substantially exceed typical estimates. If discount rates decline linearly by 0.19 percentage points per year as estimated by Kureishi et al. (2021), the constant rate of 18.7 percent from age 20 to 40 would correspond to approximately 20.6 percent at age 20, and approximately 21.4 percent at ages 16-18 during upper-secondary school. These rates lie well above those typically found in the empirical literature on adolescent time preferences (Andersen et al., 2008; Epper et al., 2020; Frederick et al., 2002; Cohen et al., 2020).

These calculations suggest that discounting—whether through present bias or high exponential discount rates—cannot fully explain permanent dropout on its own, even when combined with complete re-entry frictions. While discounting likely played a supporting role by amplifying the behavioral response, it cannot serve as the primary explanation for permanent dropout.

Stepping back, the evidence presented in this section points to a combination of misperception about returns to schooling and psychological re-entry costs as the primary explanation for permanent dropout. Organizational frictions and financial commitments play at most a minor role, and discounting alone cannot rationalize the observed patterns at empirically plausible parameter values. The two remaining mechanisms are complementary, with misperception explaining why students were willing to leave school in response to a temporary earnings opportunity, while psychological re-entry costs explain why so few returned once they had left. Together, these mechanisms can account for why a temporary policy produced lasting consequences.

7 Implications of Results

Cost of Business Cycles Since the seminal contribution of Lucas (1987), the literature quantifying the cost of business cycles has primarily measured this cost through the welfare loss from intertemporal substitution in consumption (e.g., Krebs, 2007; Krusell et al., 2009). The findings in this paper highlight an additional channel through which business cycle costs can materialize. Economic booms increase the opportunity cost of schooling, diverting students away from education and permanently scarring human capital.

To quantify this channel, I compute the elasticity of educational attainment with respect to the opportunity cost of schooling. I measure changes in the opportunity cost as the financial incentive to work created by the tax-free year, captured by the change in the average tax rate, which reflects the increase in take-home pay for students entering the labor market. Table 1 shows

[24] Specifically, I compute the discount rate that solves $C_{16-20} - \sum_{a=21}^{40} \frac{C_a}{(1+\delta)^{a-20}} = 0$, where C_{16-20} represents the effect on cumulative earnings during upper-secondary school age, and C_a denotes the treatment effect on cumulative earnings at age a, both taken from Figure 8, panel (b).

that the average tax rate for employed 16–20-year-olds was 8.3 percent before the tax-free year. Measured in years of schooling, the tax-free year reduced educational attainment by 0.192 years, or 1.3 percent. This implies an elasticity of educational attainment of $\frac{-0.013}{-\log(1-0.083)} = -0.15$.

These elasticity estimates have direct implications for the human capital costs of business cycles. The elasticity suggests that a transitory boom that raises teenage wages by 10 percent leads to a 1.5 percent permanent loss in years of schooling for the affected cohorts. Wage increases of this magnitude are common in typical booms. Jaimovich et al. (2013) find that between 1964 and 2010, the cyclical volatility of real wages for 15–19-year-olds was twice as high as that for prime-age workers, implying that young workers are particularly exposed to boom-induced increases in opportunity costs.

Over the business cycle, the effects of booms on educational attainment may be offset by those of recessions. Recessions reduce the opportunity cost of schooling, which should encourage students to stay in school longer, and some studies find exactly this (Betts and McFarland, 1995). However, recessions may also reduce attainment through borrowing constraints and parental job loss (Dellas and Sakellaris, 2003; Oreopoulos et al., 2008; Hilger, 2016; Stuart, 2022), and several studies find small or insignificant net effects (e.g., Card and Lemieux, 2001b; Johnson, 2013). More broadly, these estimates should be interpreted with caution when extrapolating to economic booms: unlike the tax holiday, booms may alter both current earnings opportunities and the perceived lifetime return to education simultaneously, and their duration is unknown ex ante.

Aggregate Output Loss The loss of human capital during booms likely leads to long-run declines in productivity and output. To quantify this effect, I perform two calculations.

The first is a back-of-the-envelope estimate based on the impact of reduced schooling on adult earnings. The permanent effect on labor earnings in men's 40s is 5.1 percent (Table 3). The affected cohorts of men represent approximately 3.9 percent of the working-age population aged 16–67. Multiplying this share by the estimated 5.1 percent earnings loss and the labor share of GDP, 46.9 percent in 2020, implies that school dropout during the tax-free year reduced GDP in 2020 by about 0.09 percent.

As a second approach, I use the estimates from Hanushek and Woessmann (2012). Their estimate implies that one standard deviation in cognitive skills raises annual GDP per capita growth by 1.98 percentage points, and one year of schooling corresponds to one-third of a standard deviation in cognitive skills. I scale this by the working-age-population-weighted reduction in years of schooling, 0.010 years—the product of the estimated reduction of 0.262 years and the affected cohorts' 3.9 percent share of the working-age population. This implies a reduction in annual GDP per capita growth of 0.0067 percentage points. Applying this adjustment to annual GDP growth from 1988 to 2020 implies that GDP in 2020 was reduced by about 0.22 percent.

Age-Dependent Taxation The literature on optimal taxation has proposed age-dependent labor income taxes (Kremer, 2002; Weinzierl, 2011). The rationale is that young workers have a more elastic labor supply than middle-aged workers. They tend to have greater flexibility, are still selecting career paths, and often work part-time or remain outside the labor market. Building on the principle that taxing the most elastic sources of supply less is advantageous (Ramsey, 1927), an optimal age-dependent tax policy would set lower taxes for young workers.

However, my findings introduce an important caveat. Although an age-dependent tax reduction is permanent in calendar time, it generates a temporary increase in opportunity costs from the perspective of the individual—young workers simply age out of the preferential rate, leaving the future return to education unchanged. The tax-free year studied in this paper is precisely such a shock, and the results show that even a single year of reduced taxes is sufficient to permanently reduce educational attainment, as young workers trade off labor not only for leisure but also for education. The resulting human capital losses lead to lower incomes in adulthood, ultimately reducing tax revenue.

The findings in this paper suggest that the conventional case for age-dependent taxation should therefore be weighed against these human capital costs, which the present analysis suggests are substantial.

Education Policy The large private and public losses from reduced human capital accumulation justify policies aimed at reducing dropout rates. One natural policy instrument is the school-leaving age. Oreopoulos (2007b) examines U.S. states that increased the school-leaving age to 17 or 18 and finds that these reforms reduced high school dropout rates, increased college enrollment—even though college was not compulsory—and boosted adult earnings. This suggests that students who would otherwise have dropped out of high school are more likely to complete further education when attendance is mandated for an additional year or two.

To assess the potential effectiveness of such a policy in reducing dropout during booms, I estimate how the impact of the tax-free year would have changed if the school-leaving age had been 17 instead of 16. I estimate that dropout during the Tax-Free Year would have been reduced by about 40 percent.[25] More direct procyclical policies—such as student grants or low-interest loans during periods of elevated opportunity costs—could complement a higher school-leaving age in reducing dropout.

8 Conclusion

This paper provides evidence that transitory increases in the opportunity cost of schooling can permanently reduce human capital accumulation. Exploiting a one-year tax holiday in Iceland that raised students' net-of-tax wages for a single year while leaving the future return to education intact, I find that men—but not women—left school to seize the temporary earnings opportunity and never returned. As a result, they suffered substantial lifetime earnings losses, entered occupations with limited career advancement, and were less likely to marry or have children.

[25] To quantify this, I use a difference-in-discontinuity estimation strategy where women serve as a control group for men, based on my main finding that women did not drop out of school. The estimated effect on male dropout using this strategy is 0.036 (SE 0.021). When excluding 16-year-olds, the estimated effect falls to 0.021 (SE 0.031), a reduction of 42 percent.

The findings are consistent with two complementary mechanisms. First, students underestimated the returns to education—dropout responses were concentrated among those from low-education families and neighborhoods where inferring true returns is especially difficult. Second, students overestimated their ability to return to school later, facing psychological barriers to re-entry that converted what may have begun as a temporary response into a permanent one.

More broadly, the results highlight that temporary shocks to economic incentives can leave lasting scars that far outlast the shock itself. Whether the trigger is an economic boom that raises earnings opportunities for young workers or an age-dependent tax policy that lowers their tax burden, the human capital costs can be large and irreversible.

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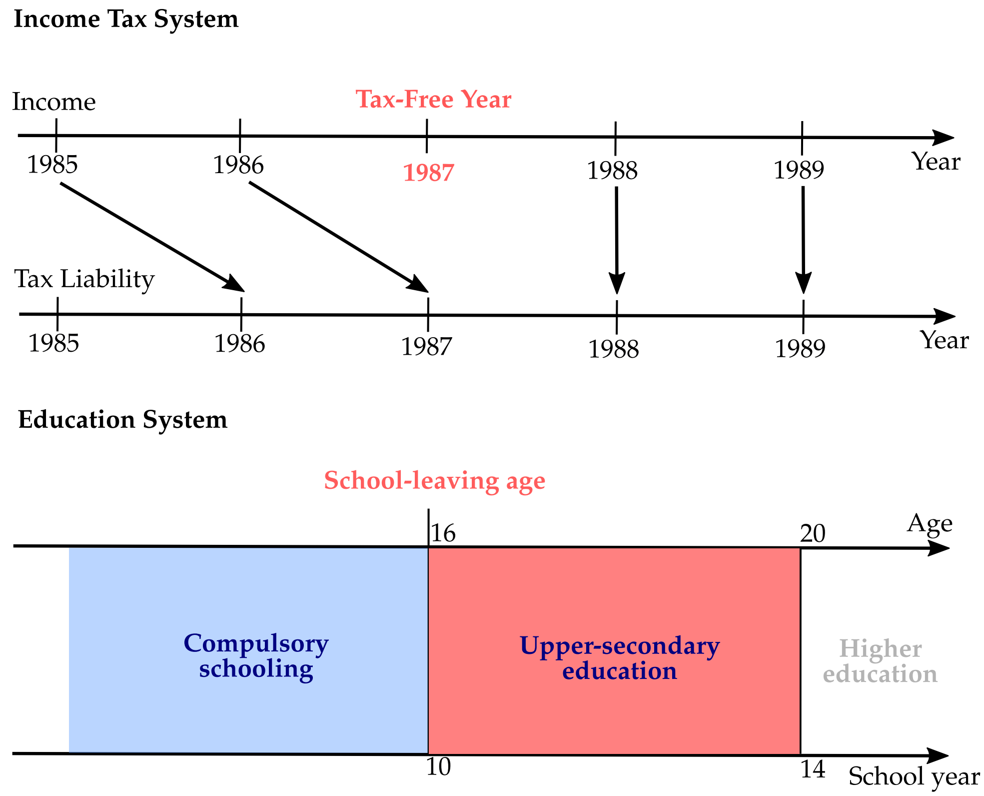
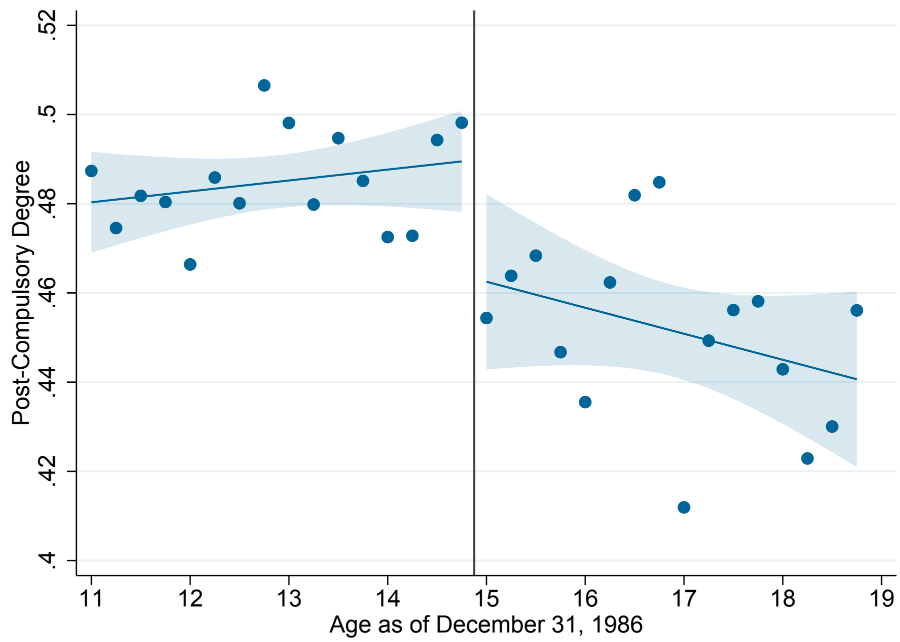
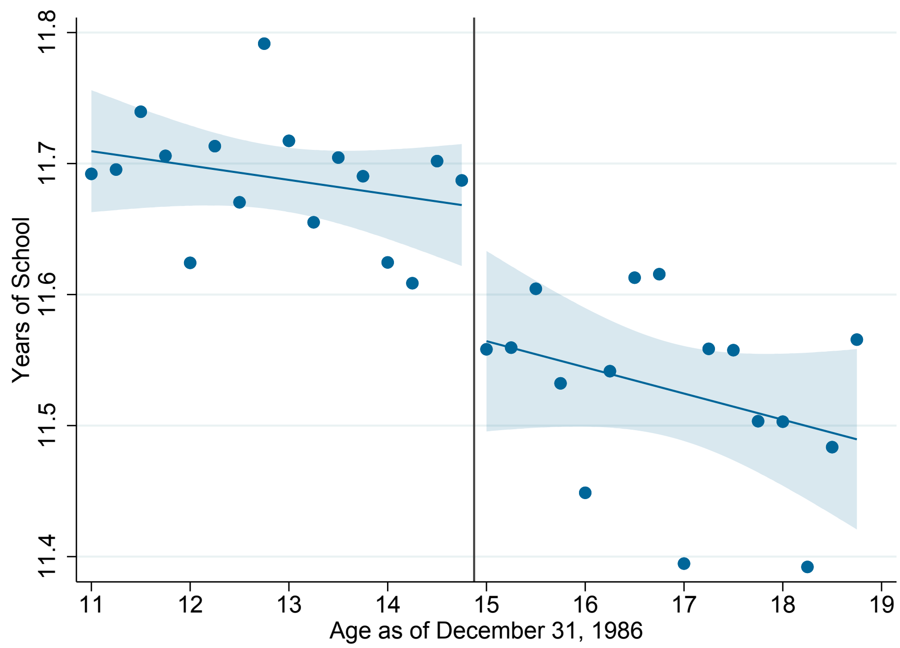


Figure 3: Research Design: Tax-free Year and School-leaving Age

Notes: The figure is a diagram of the research design, which leverages a tax-free year and discontinuity in the compulsory schooling age. The top panel illustrates the transition from a retrospective income tax system to a pay-as-you-earn system. Under the old system, taxes on income earned in year t were collected in year $t + 1$; under the new system taxes are withheld contemporaneously. The transition implied that income earned in 1987 was never taxed. The bottom panel shows the structure of the Icelandic education system by age. The research design exploits the discontinuity at the school-leaving age of 16: students below this threshold are required to remain in compulsory schooling, while those at or above it are free to choose whether to continue into upper-secondary education. Higher education lies outside the estimation bandwidth.



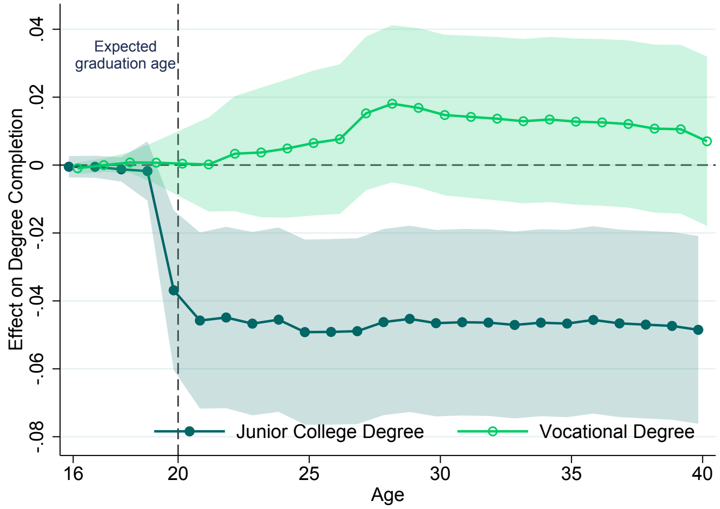
(a) Post-compulsory degree



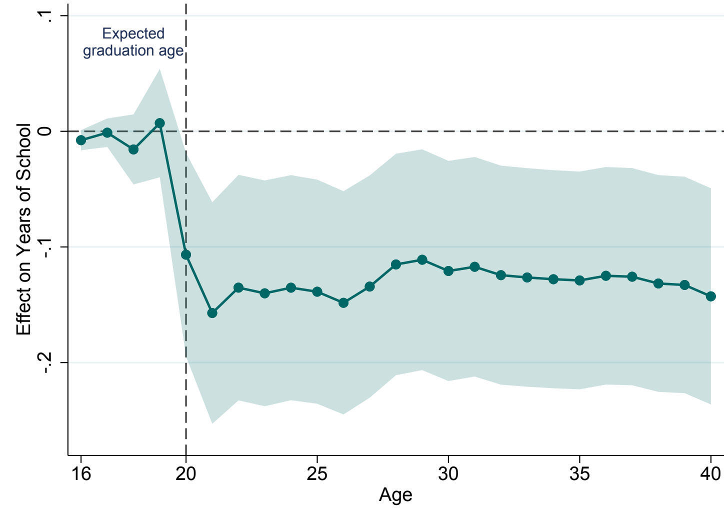
(b) Years of school

Figure 4: School Dropout

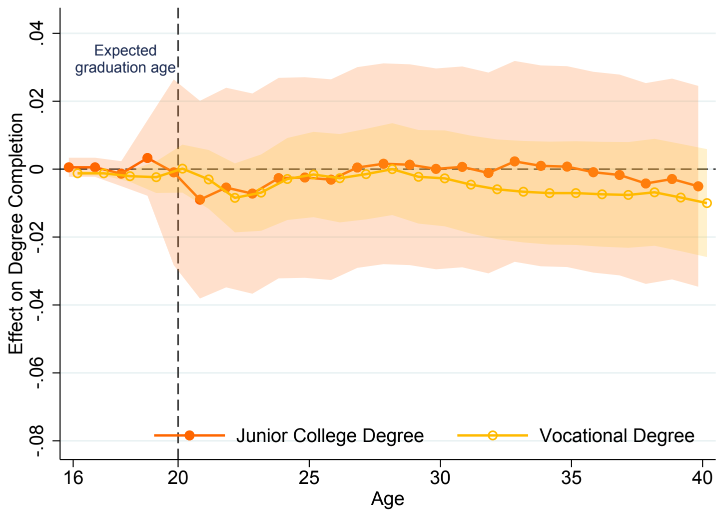
Notes: The figure plots average educational attainment at age 21 on each side of the compulsory-school age threshold. Panel (a) plots the average share with post-compulsory education. Panel (b) plots the average number of years of school completed. The vertical line denotes the compulsory schooling age threshold. The vertical line denotes the compulsory schooling age threshold. Dots are four-month age bins through. I plot fitted linear trends through the dots on each side of the compulsory schooling age threshold and their 95% confidence intervals.



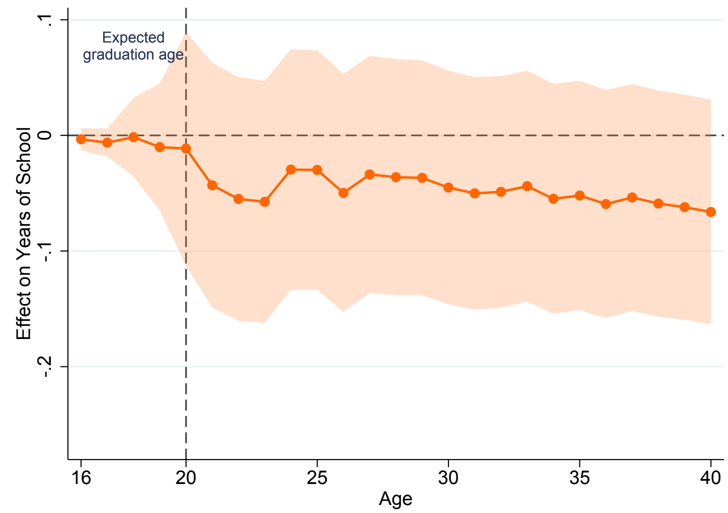
(a) Men: Junior college and vocational degree



(b) Men: Years of school



(c) Women: Junior college and vocational degree



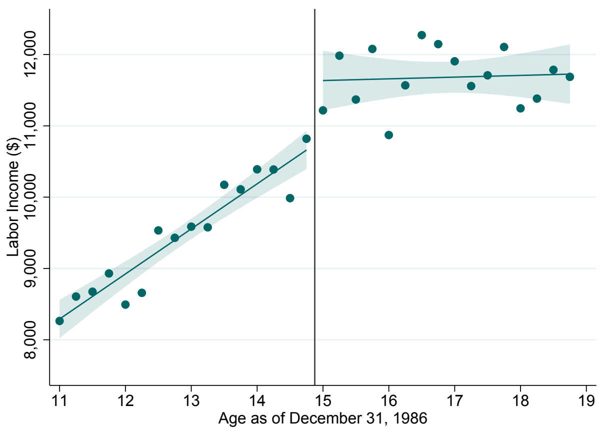
(d) Women: Years of school

Figure 5: Dynamics of the Effect of Tax-Free Year on School Dropout and Educational Attainment

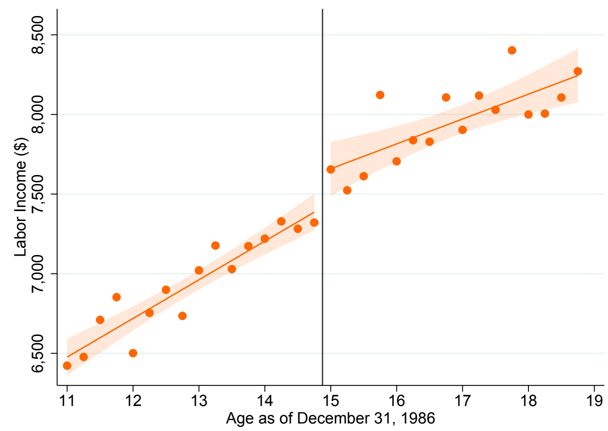
Notes: The figure plots estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panels (a) and (c) plot the estimated effects of the tax-free year on completing junior college and vocational degrees among men and women, respectively. Panels (b) and (d) plot the estimated effects of the tax-free year on years of schooling completed among men and women, respectively. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The shaded areas show 95% confidence intervals, where the standard errors are clustered at the individual level.

Figure 6: Entry Jobs of School Dropouts

Notes: The figure presents the estimated effect on employment by occupation. The lower part of each bar represents the employment share in that occupation among 16-19-year-olds, separately for men and women, in the pre-reform year 1986. At the top of each bar, I plot the estimated effect on employment in that occupation among 16-19-year-olds. Occupations are ordered along the horizontal axis by the pre-reform rank of average income among 16-19-year-old employees. For example, prior to the reform, 16-19-year-olds earned the highest income if employed in fishing but the least if employed as service workers. Occupations are classified based on the Icelandic version of the ISCO-88 code, where occupations are organized within industry. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The whiskers display the 95% confidence intervals based on robust standard errors clustered at the individual level.

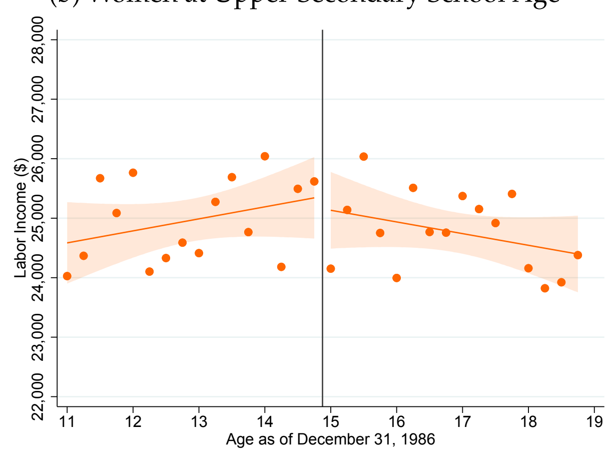


(a) Men at Upper-Secondary School Age



(b) Women at Upper-Secondary School Age

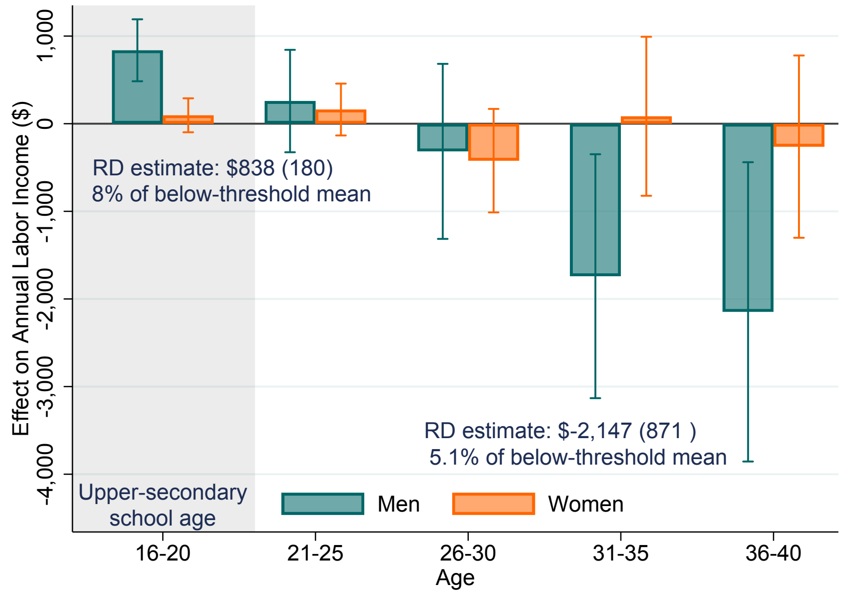
(c) Men at Prime Age



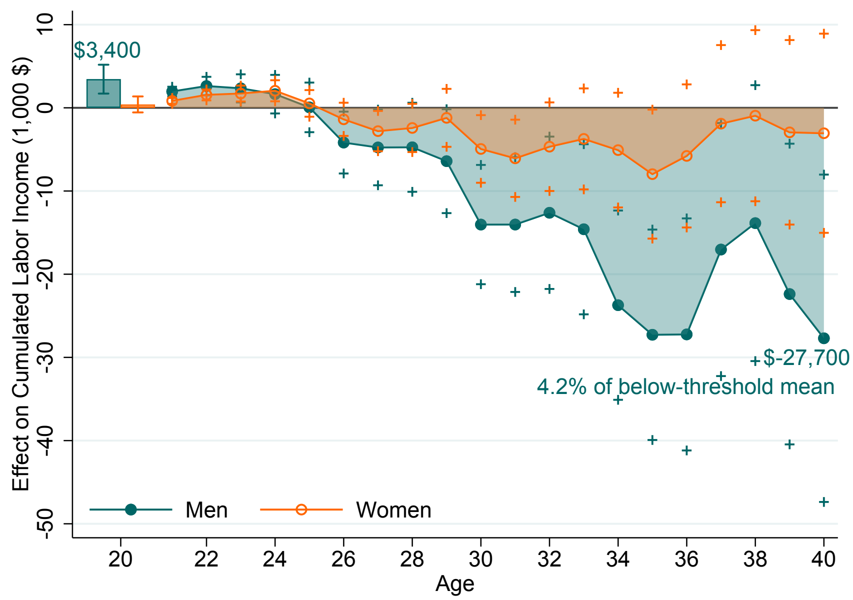
(d) Women at Prime Age

Figure 7: Labor Income at Upper-Secondary School Age and Prime Age

Notes: The figure reports the effect of the tax-free year on labor income. Panels (a) and (b) plot the average annual labor income at upper-secondary school age (16-20) around the compulsory schooling age threshold for men and women, respectively. Panels (c) and (d) plot the average annual labor income at prime age (31-40) around the compulsory schooling age threshold for men and women, respectively. The vertical line denotes the compulsory schooling age threshold. Dots are four-month age bins through. I plot fitted linear trends through the dots on each side of the compulsory schooling age threshold and their 95% confidence intervals.



(a) Effect on Annual Labor Earnings



(b) Effect on Cumulative Labor Earnings

Figure 8: Effects of Tax-Free Year on Labor Earnings in Short and Long Run

Notes: The figure reports the effect of the tax-free year on labor earnings in the short and long run. Panel (a) plots RD estimates using equation (1) of the effect of the tax-free year on annual labor income. The bars correspond to average effects at each age interval. Panel (b) plots RD estimates using equation (1) of the effect of the tax-free year on cumulative labor income. The bars correspond to estimates of equation (1) of the effect on accumulated labor earnings over upper-secondary school age 16-20 and the dots correspond to effects on accumulated labor earnings over time from age 21 to 40. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers in panel (a) and crosses in panel (b) display the 95% confidence intervals based on robust standard errors clustered at the individual level.

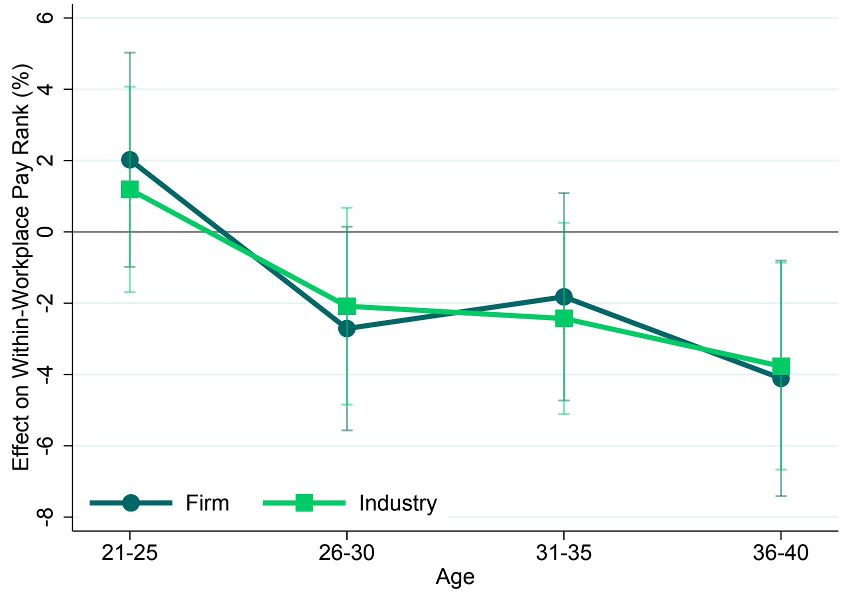


Figure 9: Effect on Career Progression

Notes: The figure plots the effect on the worker's relative position within the firm or industry of employment. Career progression is measured by the rank of the worker's residualized, or 'Mincerized', earnings within the firm or industry. That is, earnings are regressed on gender, age, and interaction of age and gender, and ranks are based on residuals from this regression. The dots/squares correspond to estimates of equation (1) on earnings rank, which I divide by the below-threshold, or control group, mean rank, measured as 12-month averages below the age threshold, to measure the percentage change in ranks. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence interval based on robust standard errors clustered at the individual level.

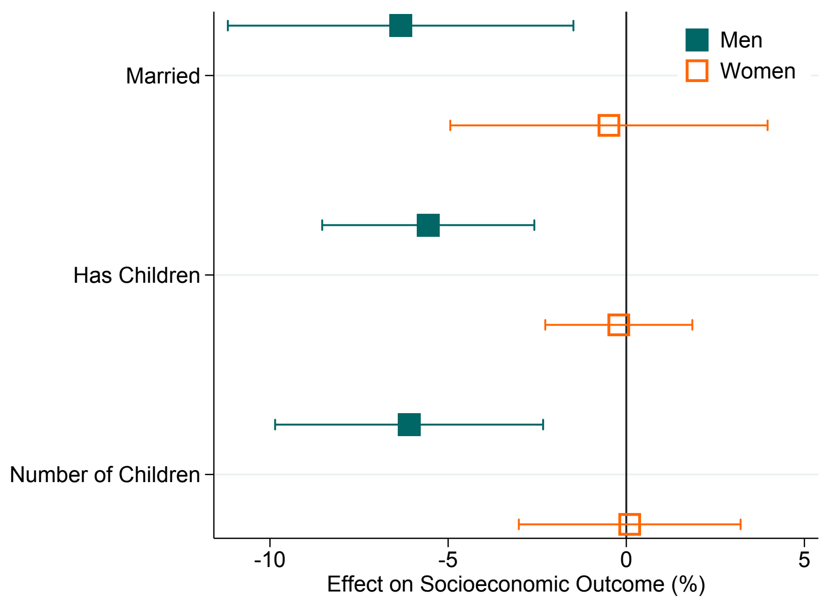


Figure 10: Effect of School Dropout on Marriage and Fertility

Notes: The figure presents estimates of equation (1) of the effect on marriage and fertility. *Married* is an indicator of ever having been married by age 40. The below-threshold mean is 0.59 (0.64) for men (women). *Has children* is an indicator of having had children by age 40. The below-threshold mean is 0.80 (0.89) for men (women). *Number of children* is the number of children the individual has had by age 40. The below-threshold mean is 1.92 (2.18) for men (women). Estimates are reported as the ratio of the treatment effect to the below-threshold, or control group mean, measured as 12-month averages below the age threshold, to measure the percentage change in outcome. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The whiskers display the 95% confidence interval where standard errors are calculated using the Delta method.

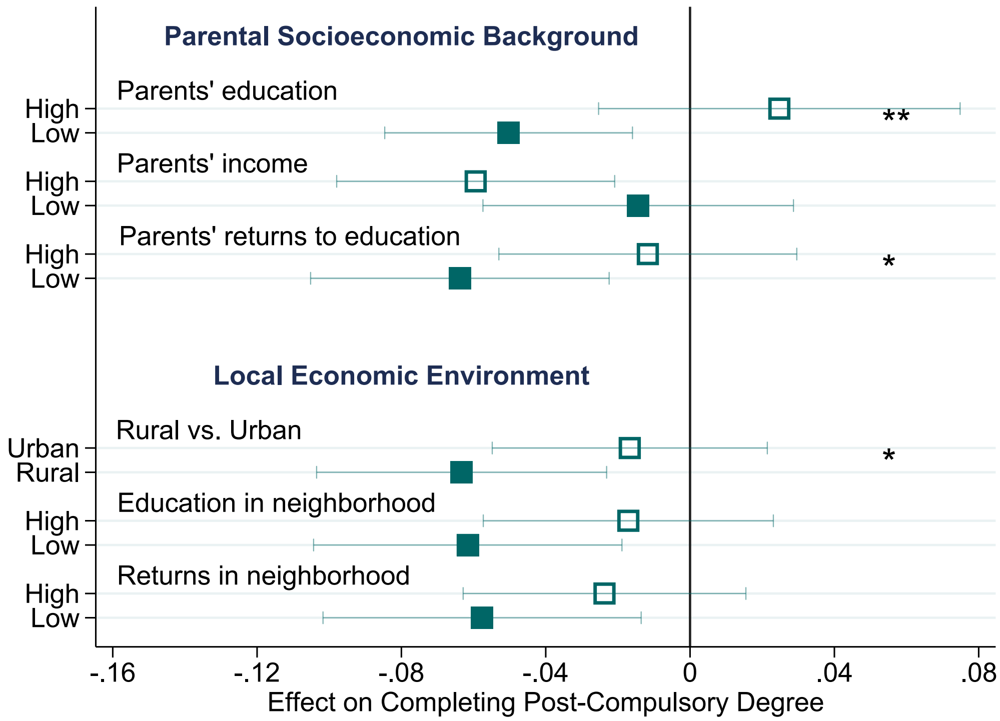


Figure 11: Dropout Responses to the Tax-Free Year by Parental and Neighborhood Characteristics

Notes: The figure shows the estimated effects of the tax-free year on school dropout among men, by parental background and neighborhood characteristics. For each characteristic, students are divided into two groups, and I estimate equation (1), interacting group indicators with the discontinuity and age polynomials. For parental education, students are split according to whether at least one parent has completed an academic upper-secondary degree (high school or more). The share of parents with this level of education is 29 percent. For parental income, I rank all individuals in the population by labor income within each birth cohort, gender, and calendar year. I then compute the median income rank of parents at ages 40-60. Each student is assigned the rank of the higher-earning parent, and students are split at the median parental income rank. To split by parental returns to education, I regress earnings on birth cohort indicators, interaction of those indicators and years of schooling, and control for year and location fixed effects. I then distinguish between parents with positive residual (low returns) or negative residual (high returns). The former group is 46 percent of parents. Municipalities are classified as urban or rural based on official municipality codes. 30 percent of parents reside in urban municipalities. For neighborhood education, I calculate, for each municipality, the share of adults (aged 25-64) with an academic upper-secondary degree in the year before the tax-free year (1986), and split students at the median of this distribution. For neighborhood returns to education, I compute, for each municipality, average labor income in 1986 for adult men, separately by education level (academic upper-secondary degree vs. less). Municipal returns are calculated as the ratio of these averages, and students are split at the median return. All regressions control for individual characteristics measured before the reform. Regressions by parental background additionally include municipality fixed effects. Whiskers denote 95% confidence intervals. Asterisks indicate the significance of tests for equality of coefficients across groups (** $p<0.05$, * $p<0.1$).

Table 1: Tax Rates for Workers Aged 16-20

	All (1)	Men (2)	Women (3)
Average tax rate	8.3%	10.1%	5.6%
Marginal tax rate	15.8%	18.8%	11.5%

Notes: This table reports average and marginal tax rates for 16-20-year-olds working at least 36 weeks (9 months) during the year. Numbers are averages over the years 1984-1986.

Table 2: Effect on School Dropout and Educational Attainment

	Post compulsory degree		Junior college degree		Vocational degree		Pre-university Years of school		University degree		Years of school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All												
Treatment effect	-0.033***	-0.034***	-0.030***	-0.027***	0.003	-0.002	-0.106***	-0.108***	-0.016	-0.014	-0.200***	-0.192***
	(0.010)	(0.010)	(0.011)	(0.010)	(0.008)	(0.008)	(0.035)	(0.034)	(0.011)	(0.010)	(0.074)	(0.073)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	0.650	0.650	0.446	0.446	0.159	0.159	12.25	12.25	0.449	0.449	14.36	14.36
Observations	34,654	34,654	34,654	34,654	34,654	34,654	34,650	34,650	34,654	34,654	34,654	34,654
B. Men												
Treatment effect	-0.048***	-0.050***	-0.048***	-0.049***	0.008	0.007	-0.135***	-0.145***	-0.015	-0.017	-0.244**	-0.262**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.048)	(0.048)	(0.014)	(0.014)	(0.102)	(0.102)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	0.631	0.631	0.365	0.365	0.239	0.239	12.08	12.08	0.350	0.350	13.82	13.82
Observations	17,694	17,694	17,694	17,694	17,694	17,694	17,694	17,694	17,694	17,694	17,694	17,694
C. Women												
Treatment effect	-0.016	-0.018	-0.006	-0.005	-0.009	-0.010	-0.064	-0.068	-0.010	-0.010	-0.119	-0.119
	(0.014)	(0.014)	(0.015)	(0.015)	(0.008)	(0.008)	(0.050)	(0.050)	(0.015)	(0.015)	(0.104)	(0.104)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	0.668	0.668	0.525	0.525	0.0812	0.0812	12.42	12.42	0.545	0.545	14.88	14.88
Observations	16,960	16,960	16,960	16,960	16,960	16,960	16,956	16,956	16,960	16,960	16,960	16,960

Notes: This table reports the estimated effect of the tax-free year on school dropout and educational attainment, estimated using the RD regression equation (1). Each cell represents a single a regression estimate for the education outcome specified in the row heading. The estimates are based on local-linear regressions for individuals at age 40 and allow for different coefficients on each side of the cutoff. Outcome mean refers to averages of the dependent variable over 12 months below the threshold, i.e., a control group. Regressions control for gender and pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Effect on Labor Market Outcomes and Wealth

	Age 16-20		Age 36-40					
	Earnings (1)	Employment (2)	Earnings (3)	Employment (4)	Non-labor Income (5)	Total Wealth (6)	Real Estate (5)	Financial Wealth (6)
				A. All				
Treatment effect	484***	0.036***	-1,095**	0.003	-354	-8,078**	-6,690**	-1,175
	(105)	(0.007)	(517)	(0.006)	(283)	(3,546)	(2,625)	(1,730)
Outcome mean	8,906	0.336	33,995	0.829	6,124	195,103	167,670	27,087
Observations	153,131	153,131	150,543	150,543	150,543	150,543	150,543	150,543
				B. Men				
Treatment effect	838***	0.050***	-2,147**	-0.001	-626	-15,539***	-11,065***	-4,202
	(180)	(0.010)	(871)	(0.007)	(417)	(5,199)	(3,750)	(2,605)
Outcome mean	10,487	0.425	41,927	0.863	5,405	186,704	158,362	27,976
Observations	78,247	78,247	76,269	76,269	76,269	76,269	76,269	76,269
				C. Women				
Treatment effect	96	0.021**	-262	0.005	-51	-823	-2,443	1,770
	(99)	(0.009)	(531)	(0.009)	(381)	(4,824)	(3,669)	(2,282)
Outcome mean	7,342	0.248	26,247	0.796	6,826	203,306	176,760	26,220
Observations	74,884	74,884	74,274	74,274	74,274	74,274	74,274	74,274

Notes: This table reports the coefficient of the treatment indicator according to the regression equation (1), where each cell represents a single regression estimate for the outcome measure specified in the row heading. Outcome mean refers to 12-month averages at the left of the threshold. Total wealth is the sum of all assets, financial and non-financial. Real estate is the tax-based value of the real estate. Financial wealth is the difference between total wealth and real estate wealth.Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. Robust standard errors, clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Online Appendix of:

Transitory Earnings Opportunities and Educational Scarring of Men

Jósef Sigurdsson

May 19, 2026

A Data Appendix

A.1 Educational Attainment

Data on educational attainment is drawn from Statistics Iceland's *Education Register*. This register is based largely on Statistics Iceland's *Degree Register*. For this register data on completed education is collected twice a year from all schools in the formal education system, in May-June and December after graduations, and in some cases directly from the Ministry of Education, as in the case of the journeyman's examination. The Education Register also builds on various other additional sources, including university graduates back to 1912, certified masters' of trades (some without attending the masters' school) back to 1937, graduations from upper secondary schools before the start of regular data collection, information on licenses for particular occupations, information from Statistics Iceland's census, records from the Immigration office, and information from various surveys conducted by Statistics Iceland.

In the Education Register, educational attainment is classified according to the *ÍSMENNT* standard, which is based on the international standard classification of education (*ISCED*). The standard divides attained education into nine levels, out of which six are further subdivided, yielding 31 educational classes in total. The Register records completed education, defined as education completed with sufficient qualification and degree to transition to the next level.

In my analysis, I use years of schooling as one measure of educational attainment, where one year refers to the school year of normally 8–10 months. For university education, each semester corresponds to 30 credits under the European Credit Transfer and Accumulation System (ECTS). I translate educational attainment into years of school based on the time required to complete a given level or degree, according to the *ÍSMENNT* standard. For example, a junior college degree translates to 4 years of school and a bachelor's degree (180 ECTS) translates to 3 years.

A.2 Occupation and Sector Classification

The pay slip data records occupation according to a two-digit classification with 74 separate occupation classes, based on the International Labor Organization's International Standard Classification of Occupations (ISCO), version ISCO-88. For a detailed description of the classification, see

ILO's website.

The pay slip data also record the sector for each firm, with 189 separate sector classes based on the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC). For a detailed description of the classification, see UN's website.

B General Equilibrium Effects

This appendix outlines the theoretical framework of Card and Lemieux (2001) and derives equation (2) in the main text, which links changes in the relative supply of low- and high-educated labor to changes in their relative wages. I then combine this expression with parameter estimates from the literature to calibrate the potential magnitude of general equilibrium wage effects arising from the change in educational attainment induced by the tax-free year.

B.1 A Model of Aggregate Production with Cohort Specific Supplies

The model relaxes the assumption of perfect substitution across cohorts assumed in conventional models of educated-related wage differentials, extending the model of Katz and Murphy (1992) which allows for imperfect substitution of workers depending on level of education. In the model, aggregate output depends on a nested CES aggregate with two-levels. The upper-level is identical to the model of Katz and Murphy (1992), where output is a function of labor with high (H_t) and low (L_t) education. In the context of the current paper, these are workers with only compulsory education (dropouts) and workers with post-compulsory education (high-school graduates). Card and Lemieux (2001) add a lower level where supplies of each education group are themselves CES subaggregates of the labor supply of different age groups (j). Aggregate education supplies therefore depend on age-group specific supplies. Education supplies of each group are:

$$H_t = \left(\sum_j \alpha_j H_{ij}^{\eta}\right)^{\frac{1}{\eta}} \qquad L_t = \left(\sum_j \beta_j L_{ij}^{\eta}\right)^{\frac{1}{\eta}} \tag{11}$$

where $\sigma_A = 1/(1-\eta)$ is the elasticity of substitution across age groups j with the same education. As $\eta \rightarrow 1, \sigma_A \rightarrow \infty$ and groups are perfect substitutes. α_j and β_j are efficiency parameters, assumed to be cohort-specific and time-invariant.

Aggregate output in period t is also a CES:

$$Y_t = (A_{Ht} H_t^{\rho} + A_{Lt} L_t^{\rho})^{\frac{1}{\rho}} \tag{12}$$

where $\sigma_E = 1/(1-\rho)$ is the elasticity of substitution between education groups, as in Katz and Murphy (1992). A_H, A_L are time-varying efficiency parameters.

The marginal product of labor for a given education-cohort group is determined by two factors: the labor supply of that specific education-cohort group and the aggregate labor supply of workers with the same education level. Under the assumption of competitive wage setting, wages are equal to marginal products. Accordingly, the wages for low-educated workers in cohort j are given by:

$$w_{jt}^{L} = \frac{\partial Y_t}{\partial L_{jt}} = \frac{\partial Y_t}{\partial L_t} \times \frac{\partial L_t}{\partial L_{jt}}$$
$$= A_{Lt} L_t^{\rho-\eta} \Psi \times \beta_j L_{jt}^{\eta-1} \tag{13}$$

where

$$\Psi = (A_{Ht} H_t^{\rho} + A_{Lt} L_t^{\rho})^{\frac{1}{\rho}-1}$$

Similarly, the wages for high-educated workers in cohort j are

$$w_{jt}^{H} = A_{Ht} H_t^{\rho-\eta} \Psi \times \alpha_j H_{jt}^{\eta-1} \tag{14}$$

Provided that $\eta < 1$, the age-specific wage by education group is declining in age-specific labor supply for that education group.

Using equations (13) and (14), we get that the relative wage of low-educated workers in cohort j, w_{jt}^{L}, to the wage of high-educated workers in the same cohort, w_{jt}^{H}, is

$$\ln\left(\frac{w_{jt}^{L}}{w_{jt}^{H}}\right) = \ln\left(\frac{A_{Lt}}{A_{Ht}}\right) + \ln\left(\frac{\beta_j}{\alpha_j}\right) + \left[\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right] \ln\left(\frac{L_t}{H_t}\right) - \frac{1}{\sigma_A} \ln\left(\frac{L_{jt}}{H_{jt}}\right) \tag{15}$$

The objective is to quantify how cohort-specific changes in the relative supply of low-educated and high-educated workers affect their relative wages. To mirror the empirical setting, consider two adjacent birth cohorts, j and j', where cohort j is exposed to the tax reform and cohort j' serves as the comparison group. Because wages and labor supply are observed at the same point in calendar time for both cohorts, and because each cohort represents only a small share of the overall labor force, the aggregate supplies L_t and H_t can be treated as approximately fixed across cohorts. Let Δ denote the difference between the affected and adjacent cohorts, i.e., $\Delta X \equiv X_j - X_{j'}$. Applying this operator to equation (15) yields:

$$\Delta \ln\left(\frac{w_{j}^{L}}{w_{j}^{H}}\right) = -\frac{1}{\sigma_A} \Delta \ln\left(\frac{L_j}{H_j}\right)$$

assuming that the relative efficiency parameters are the same for the two adjacent cohorts.[26] This equation relates the tax-induced change in the relative wage gap between low- and high-educated workers to the reform-induced change in their relative cohort supply and corresponds to equation (2) in the main text.

[26]This assumption is plausible for adjacent birth cohorts, which grew up in the same economic environment, attended the same schools, and entered the labor market at nearly the same time. Any differences in average cohort quality or ability are unlikely to be systematic at this frequency.

B.2 Calibration

Using empirical estimates of the effect of the tax-free year on educational attainment in the affected cohorts, along with estimates of σ_A from the literature, we can quantify the general-equilibrium effect on the relative wages of education groups. I estimate in Section 3 that the tax-free year reduced post-compulsory education completion by 8 percent, and in Section 4 that it reduced earnings by 5.1 percent.

Figure A.1 plots the share of the estimated earnings effect of the tax-free year that can be explained by general-equilibrium wage adjustment according to equation (2) as a function of the elasticity of substitution within education groups across cohorts, σ_A. As $\sigma_A \to \infty$, workers of different cohorts but the same education level are perfect substitutes and cohort-specific changes in labor supply have no effect on relative wages.

The shaded region in the figure summarizes empirical estimates of σ_A from the literature. In their seminal paper, Card and Lemieux (2001) estimate σ_A for five-year birth cohorts using data from the U.S. (1959–1996), the U.K. (1974–1996), and Canada (1980–1995), obtaining estimates between 3.8 and 4.9 for the U.S., 3.8 and 4.3 for the U.K., and 6.1 and 6.2 for Canada (see Table 3 in Card and Lemieux (2001)). Acemoglu and Autor (2011) estimate a comparable elasticity of 3.7 using U.S. data over an extended period from 1963–2008. A key assumption underlying these estimates is that workers within an education–cohort cell are homogeneous, so that changes in cohort size affect wages only through quantities and not through composition. Carneiro and Lee (2011) relax this assumption by allowing the average quality of workers within education–age cells to vary with educational attainment. Correcting for these composition effects substantially raises the estimated elasticity of substitution across age groups. For high-school graduates, their estimate of the elasticity of substitution across age groups is 11.1. This adjustment is particularly relevant in settings, such as the present one, where policy-induced changes in schooling affect the margin of participation and therefore the composition of education groups.[27]

European evidence based on age- or experience-group variation suggests a wide but informative range of values for σ_A. For France, Verdugo (2014) report inverse elasticities between -0.08 and -0.11, corresponding to σ_A values of roughly 9–13. For Germany, estimates of substitution across experience groups within education categories tend to be high. Using regional immigration variation and a nested CES labor-demand framework, Brücker and Jahn (2008) estimate elasticities of substitution between experience groups on the order of 20–30 and cannot reject perfect substitution. Similarly, Felbermayr et al. (2010) are unable to reject perfect substitution across experience groups and effectively treat σ_A as very large (100) in their calibration. Taken together, these studies suggest meaningful but imperfect substitution across cohorts within education groups, with plausible values of σ_A ranging from 3.7 to 30 or even higher.

[27] A related issue in comparing estimates across studies is terminology. While Card and Lemieux (2001) define cohorts by birth year, many subsequent papers estimate substitution elasticities across age or potential-experience groups within education categories. Although the grouping differs, these approaches identify the same underlying object for the purpose of equation (2): the degree of imperfect substitution between workers of different cohorts observed at the same point in time.

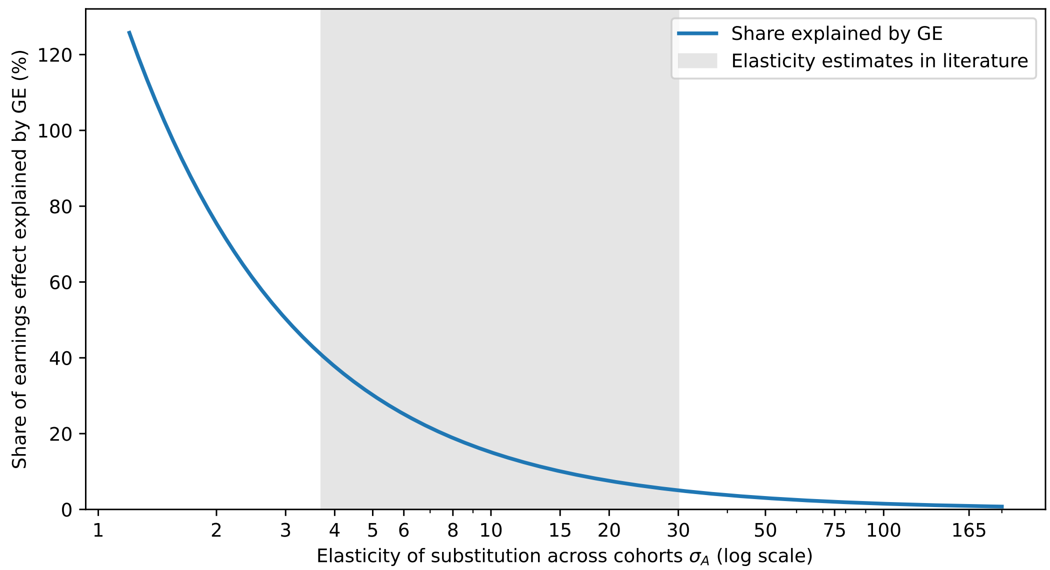


Figure A.1: Calibrated general equilibrium effects under alternative elasticities of substitution

Notes: The figure plots the fraction of the estimated earnings decline for cohorts exposed to the tax-free year that can be explained by general-equilibrium wage adjustment, as implied by equation (2), for alternative values of the elasticity of substitution across cohorts within education groups, σ_A. The shaded area corresponds to the range of σ_A estimates reported in the literature. Lower values of σ_A imply stronger imperfect substitution and larger wage responses to cohort-specific supply shifts, while higher values imply near-perfect substitution and negligible wage effects.

Across this empirically plausible range of elasticities, Figure A.1 shows that general-equilibrium wage effects can account for only a limited share of the estimated earnings losses for the affected cohorts. Even under the lowest estimates in the literature ($\sigma_A = 3.7$), general equilibrium effects explain at most 40% of the observed earnings decline. However, this figure likely overstates the true contribution. As discussed above, the estimate of 3.7 does not correct for composition effects, and European estimates for small, open labor markets consistently point to substantially higher elasticities. The quality-adjusted and European estimates are therefore more appropriate benchmarks for the present setting, under which the implied contribution is substantially smaller—at 13.6% for $\sigma_A = 11.1$ and as little as 5% for $\sigma_A = 30$.

Using equation (2), we can also compute the elasticity required for general equilibrium wage effects alone to fully account for the observed earnings losses. This calculation yields $\sigma_A = 1.57$, a value far below even the lowest estimates reported in the literature. Such a low elasticity would imply that workers with the same education but from adjacent birth cohorts are nearly as imperfect substitutes as workers with only a high-school degree and college-educated workers (Katz and Murphy, 1992; Acemoglu and Autor, 2011).

Two considerations suggest that the elasticities used in the calibration likely overstate the magnitude of general-equilibrium wage effects in this setting. First, most estimates of σ_A are identified using comparisons across broad age or experience groups rather than adjacent birth cohorts. Since workers who are close in age are much closer substitutes than workers who differ substantially in experience, elasticities estimated from wide age bins are likely to understate substitutability between adjacent cohorts and therefore overstate the wage response to a cohort-specific supply shift. Second, existing estimates are typically obtained at the national labor-market level. Elasticities of substitution are likely higher within smaller and narrower labor markets. Both of these forces would suggest that the calibration could be interpreted as an upper bound.

C Model Appendix and Extensions

This appendix complements Section 5 by formalizing the results presented in the main text and by developing a set of model extensions. We first present a general version of the model that nests the different types of heterogeneity and frictions considered in the paper. We then analyze the model under alternative assumptions and extensions.

C.1 General Setup

There is a unit mass of infinitely-lived young adults who have completed compulsory schooling. In period $t = 0$, they choose between continuing schooling for an additional year or leaving school to work. If they stay in school, they graduate in a year and then enter the labor market in $t = 1$. If they drop out, they can choose to re-enter in $t = 1$ or leave school permanently and stay in the labor market. In periods $t = 2, \ldots, \infty$ everyone works. Individuals derive utility from consuming their disposable income every period. Therefore, they choose their level of schooling to maximize the present discounted value of lifetime earnings, net of costs, discounting future income at the rate $\beta\delta^t$, where $\delta \in (0, 1)$ represents the exponential discount factor, and $\beta \leqslant 1$ accounts for potential myopia or present bias (Phelps and Pollak, 1968; Laibson, 1997).

Schooling entails a cost, κ_t, which includes both financial costs, such as tuition fees and living expenses, as well as psychic costs. Students cannot borrow and need to finance costs out of pocket. If their liquid resources at enrollment are limited, students need to pay a liquidity premium, which raises the effective cost of schooling. Students who drop out of school face a cost, $\gamma \geqslant 0$, of re-entry. This cost may capture financial costs, consumption commitments, psychological costs, or organizational barriers to re-enrollment.

Individuals without post-compulsory education earn a fixed income in each period, normalized to unity. In contrast, individuals with post-compulsory education earn a return, ρ, proportional to their economic ability, θ. Ability is assumed to be uniformly distributed, $\theta \sim U[0, 1]$. Income is subject to tax, τ_t, which is assumed constant, $\tau_t = \tau$, in all periods $t > 0$.

In the initial time period, individuals therefore face the choice between three lifetime streams of utility:

$$\text{Stay in school:} \quad -\kappa_0 + (1-\tau)\rho\theta \sum_{t=1}^{\infty} \beta\delta^t \tag{16}$$

$$\text{Temporary dropout:} \quad (1-\tau_0) - \beta\delta\,(\kappa_1+\gamma) + (1-\tau)\rho\theta \sum_{t=2}^{\infty} \beta\delta^t \tag{17}$$

$$\text{Permanent dropout:} \quad (1-\tau_0) + (1-\tau) \sum_{t=1}^{\infty} \beta\delta^t. \tag{18}$$

Individuals choose to attend school if the marginal benefit exceeds the marginal cost. Comparing utility streams (16) and (18) provides the following schooling condition:

$$\underbrace{\frac{\beta\,\delta}{1-\delta}\,(1-\tau)\,(\rho\theta-1)}_{\text{Marginal benefit}} \geqslant \underbrace{(1-\tau_0)}_{\text{Opportunity cost}} + \underbrace{\kappa_0}_{\text{Direct cost}} \tag{19}$$

which shows that young adults choose to attend school if the earnings gain exceeds the marginal cost, which consists of the opportunity cost—the net-of-tax earnings students must give up to attend school—and the direct cost of schooling, including the psychic cost.

Students also face the option of whether to stay in school in $t = 0$ or drop out temporarily and return in $t = 1$. Comparing utility streams (16) and (17) yields a condition for staying in school in $t = 0$:

$$\underbrace{\beta\delta\,(1-\tau)\rho\theta}_{\text{Marginal benefit of staying now}} \geqslant \underbrace{(1-\tau_0)}_{\text{Opportunity cost}} + \underbrace{\kappa_0 - \beta\delta\,(\kappa_1+\gamma)}_{\text{Net direct cost now vs. later}}. \tag{20}$$

Students who leave school in $t = 0$ have the option of returning to school in $t = 1$ or leaving school permanently. Viewed from the $t = 1$ vantage point after working in $t = 0$, the student compares returning to school with remaining in the labor market. Returning to school entails the normal-period opportunity cost of forgone earnings, $(1 - \tau)$, as well as the schooling cost κ_1 and any re-entry cost γ. The re-entry condition is therefore:

$$\underbrace{\frac{\beta\delta}{1-\delta}\,(1-\tau)\,(\rho\theta-1)}_{\text{Marginal benefit of re-entering}} \geqslant \underbrace{(1-\tau)}_{\text{Opportunity cost}} + \underbrace{\kappa_1+\gamma}_{\text{Re-entry costs}}. \tag{21}$$

Using these schooling conditions, we can derive three thresholds that partition the population based on their schooling decisions. First, from (20), we can derive a threshold for the share of young adults who stay in school in $t = 0$:

$$\theta_0^* = \frac{(1-\tau_0) + \kappa_0 - \beta\delta\,(\kappa_1+\gamma)}{\beta\,\delta\,(1-\tau)\,\rho}. \tag{22}$$

Second, from (21), we can derive a threshold for the share of young adults who leave school in $t = 0$ but return in $t = 1$:

$$\theta_1^* = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau)+\kappa_1+\gamma\big]}{\beta\,\delta\,(1-\tau)\,\rho}. \tag{23}$$

Finally, from the enrollment condition (19), we can derive a permanent-dropout threshold

$$\theta^*_{\text{perm}} = \frac{\beta\delta(1-\tau) + \big((1-\tau_0)+\kappa_0\big)(1-\delta)}{\beta\delta(1-\tau)\rho}, \tag{24}$$

such that individuals with $\theta < \theta^*_{\text{perm}}$ strictly prefer leaving school permanently to completing an additional year of schooling.

The three thresholds θ^*_0, θ^*_1, and θ^*_{perm} govern enrollment, re-entry, and permanent-exit incentives, respectively. The thresholds θ^*_0 and θ^*_1 characterize the enrollment and re-entry margins. When $\theta^*_1 \leqslant \theta^*_0$, they partition the population into three segments: students with $\theta \geqslant \theta^*_0$ stay in school in $t = 0$; students with $\theta^*_1 \leqslant \theta < \theta^*_0$ leave school in $t = 0$ but return in $t = 1$; and students with $\theta < \theta^*_1$ leave school in $t = 0$ and never return. When $\theta^*_1 > \theta^*_0$, the temporary-dropout interval is empty, and individuals who leave school in $t = 0$ do not return.

The threshold θ^*_{perm} summarizes the counterfactual completion-versus-permanent-exit margin at $t = 0$, abstracting from re-entry. Because students who leave in $t = 0$ retain the option to re-enter in $t = 1$, observed choices are governed by θ^*_0 and θ^*_1. Depending on the relative position of the cutoffs, the temporary-dropout region may be empty or non-empty.

C.2 Benchmark: Heterogeneous Ability

We begin with a benchmark in which the only source of heterogeneity is ability, setting $\beta = 1$, $\kappa_0 = \kappa_1 = \kappa > 0$, and $\gamma = 0$. Using (22) and (23), the schooling thresholds simplify to

$$\theta^*_0 \;=\; \frac{(1-\tau_0)+\kappa(1-\delta)}{\delta(1-\tau)\rho}, \qquad \theta^*_1 \;=\; \frac{(1-\tau)+\kappa(1-\delta)}{\delta(1-\tau)\rho}.$$

The effect of the Tax-Free Year on dropout. Differentiating θ^*_0 and θ^*_1 with respect to τ_0 yields

$$\frac{\partial\theta^*_0}{\partial\tau_0} = -\frac{1}{\delta(1-\tau)\rho} < 0, \qquad \frac{\partial\theta^*_1}{\partial\tau_0} = 0.$$

As a result, the Tax-Free Year expands the temporary-dropout region but does not affect the share of individuals who permanently leave school.

C.3 Liquidity Constraints and Psychic Costs

We now allow schooling costs to differ across periods. In particular, an interesting case is when $\kappa_0 \geqslant \kappa_1$, capturing possible short-term liquidity constraints. A higher κ_0 reflects that enrolling immediately requires access to liquid funds (e.g., tuition payments or foregone earnings), whereas students who work in $t = 0$ accumulate resources and face a lower effective cost κ_1 if they re-enter in $t = 1$. For simplicity, we maintain the assumption of no present bias, $\beta = 1$. Using (22) and (23), the enrollment and re-entry thresholds are

$$\theta_0^* = \frac{(1-\tau_0) + \kappa_0 - \delta\,\kappa_1}{\delta(1-\tau)\rho}, \qquad \theta_1^* = \frac{(1-\tau) + \kappa_1(1-\delta)}{\delta(1-\tau)\rho}.$$

Liquidity constraints affect dropout because they change the relative cost of attending school today versus returning later. Differentiating the thresholds with respect to κ_0 and κ_1:

$$\frac{\partial\theta_0^*}{\partial\kappa_0} = \frac{1}{\delta(1-\tau)\rho} > 0, \qquad \frac{\partial\theta_0^*}{\partial\kappa_1} = -\frac{1}{(1-\tau)\rho} < 0, \qquad \frac{\partial\theta_1^*}{\partial\kappa_1} = \frac{1-\delta}{\delta(1-\tau)\rho} > 0.$$

The first derivative shows that reduced access to liquidity in $t = 0$ (a higher κ_0) raises the enrollment cutoff and makes students more likely to drop out initially. The second derivative shows that lowering the cost of schooling in $t = 1$ (a lower κ_1) raises the enrollment cutoff, making initial dropout more likely because returning in $t = 1$ becomes more attractive relative to staying in school in $t = 0$. Comparing these two derivatives, raising κ_0 and lowering κ_1 by the same amount—a liquidity premium—unambiguously increases θ_0^*, making initial dropout more likely.

The third derivative shows that reducing the cost of schooling in t=1 also lowers the re-entry threshold θ_1^*, making it easier for students who left in $t = 0$ to return. Together, these effects widen the gap $\theta_0^* - \theta_1^*$. A liquidity premium therefore expands the set of young adults who leave school in $t = 0$ but find it optimal to re-enter in $t = 1$, increasing temporary (but not permanent) dropout.

To evaluate whether liquidity constraints modify the impact of the tax holiday, differentiate the thresholds with respect to τ_0:

$$\frac{\partial\theta_0^*}{\partial\tau_0} = -\frac{1}{\delta(1-\tau)\rho} < 0, \qquad \frac{\partial\theta_1^*}{\partial\tau_0} = 0.$$

These derivatives do not depend on κ_0 or κ_1. Thus, while liquidity constraints shape who is pushed into temporary dropout (through their effects on θ_0^* and θ_1^*), they do not change the way the tax-free year affects schooling decisions.

Psychic costs of schooling Another explanation for dropout is that some students face high psychic costs of schooling—disutility from studying, anxiety about academic performance, or a general dislike of the school environment. In the model, psychic costs enter symmetrically by increasing both κ_0 and κ_1, shifting both cutoffs upward in parallel without widening the gap $\theta_0^* - \theta_1^*$. As with liquidity constraints, the comparative statics with respect to τ_0 are unaffected: psychic costs raise the overall dropout rate but do not amplify or attenuate the response to the tax-free year.

C.4 Present Bias and High Discounting

We now allow students to be present biased, $\beta \in (0, 1)$, while maintaining symmetric schooling costs ($\kappa_0 = \kappa_1 = \kappa$) and no liquidity constraints. Using (22) and (23), the enrollment and re-entry thresholds become

$$\theta_0^* = \frac{(1-\tau_0) + \kappa(1-\beta\delta)}{\beta\delta(1-\tau)\rho}, \qquad \theta_1^* = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau) + \kappa\big]}{\beta\delta(1-\tau)\rho}.$$

Present bias creates both temporary and permanent dropout. Present-biased individuals undervalue future returns to schooling, reducing the perceived gain from remaining in school. Differentiating the two thresholds with respect to β yields

$$\frac{\partial \theta_0^*}{\partial \beta} = -\frac{(1-\tau_0)+\kappa}{\beta^2\,\delta\,(1-\tau)\,\rho} \;<\; 0, \qquad \frac{\partial \theta_1^*}{\partial \beta} = -\frac{(1-\delta)\big[(1-\tau)+\kappa\big]}{\beta^2\,\delta\,(1-\tau)\,\rho} \;<\; 0.$$

Thus, lower β (stronger present bias) raises both thresholds. Present bias also generates a wedge between enrollment and re-entry cutoffs, which at constant tax rate $\tau_0 = \tau$ is:

$$\theta_0^* - \theta_1^* = \frac{(1-\beta)\big[(1-\tau)+\kappa\big]}{\beta(1-\tau)\rho} \;>\; 0.$$

This wedge implies that $\beta < 1$ increases the region of *temporary dropout*. At the same time, because θ_1^* rises as β falls, present bias increases the mass of types below θ_1^*, who drop out *permanently*. Present bias therefore increases both temporary and permanent dropout.

Present bias amplifies temporary dropout during the tax-free year. To evaluate the effect of present bias on responses to the tax-free year, we differentiate the thresholds with respect to τ_0:

$$\frac{\partial \theta_0^*}{\partial \tau_0} = -\frac{1}{\beta\delta(1-\tau)\rho} < 0, \qquad \frac{\partial \theta_1^*}{\partial \tau_0} = 0.$$

To see how present bias influences this response, the relevant cross-partial derivative is

$$\frac{\partial^2 \theta_0^*}{\partial \tau_0\,\partial \beta} = \frac{1}{\beta^2\,\delta(1-\tau)\rho} \;>\; 0.$$

implying that a given reduction in τ_0 shifts θ_0^* more when β is smaller. Thus, the more present-biased students are, the stronger is the temporary behavioral response to the tax-free year. However, present bias alone does not induce students to leave school permanently in response to the tax-free year.

Heterogeneity in discount rates. In addition to present bias, individuals may differ in their exponential discount factor δ. A lower δ raises both θ_0^* and θ_1^*, making impatient individuals less willing to invest in schooling. With symmetric schooling costs and a constant tax rate, however, exponential discounting alone does not create a temporary-dropout wedge when $\beta = 1$: in that case, $\theta_0^* = \theta_1^*$ for all δ. More generally, for fixed $\beta < 1$, the normal-times temporary-dropout region is

$$\theta_0^* - \theta_1^* = \frac{(1-\beta)\big[(1-\tau)+\kappa\big]}{\beta(1-\tau)\rho},$$

which is independent of δ.

Discounting nevertheless affects the response to the tax-free year. Since

$$\frac{\partial \theta_0^*}{\partial \tau_0} = -\frac{1}{\beta\delta(1-\tau)\rho}, \qquad \frac{\partial^2 \theta_0^*}{\partial \tau_0\, \partial \delta} = \frac{1}{\beta\delta^2(1-\tau)\rho} > 0,$$

a given reduction in τ_0 shifts θ_0^* more when δ is smaller. Since θ_1^* does not depend on τ_0, the tax-free year generates a stronger initial dropout response among more impatient students.

C.5 Re-entry Frictions

We now introduce a friction in re-entering school, modeled as a cost, $\gamma \geqslant 0$, incurred if a student who has left school in $t = 0$ decides to return in $t = 1$. This cost may represent financial commitments, psychological costs, or organizational barriers to re-enrollment. The key distinction lies in whether this cost is *anticipated* or *unanticipated* at the time of the initial schooling decision.

In contrast to earlier sections, the presence of a re-entry cost implies that permanent exit may dominate both completion and temporary dropout for some types, making the completion–versus–permanent-exit margin behaviorally relevant.

Anticipated re-entry cost. Assume that the re-entry cost $\gamma \geqslant 0$ is known at $t = 0$. Moreover, for simplicity, we assume that the direct cost of schooling is constant over time, $\kappa_0 = \kappa_1 = \kappa > 0$. Under these assumptions, the enrollment and re-entry thresholds become

$$\theta_0^*(\gamma) = \frac{(1-\tau_0) + \kappa - \beta\delta(\kappa + \gamma)}{\beta\delta(1-\tau)\rho}, \qquad \theta_1^*(\gamma) = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau) + \kappa + \gamma\big]}{\beta\delta(1-\tau)\rho}. \tag{25}$$

Differentiating with respect to τ_0 yields

$$\frac{\partial \theta_0^*(\gamma, \tau_0)}{\partial \tau_0} = -\frac{1}{\beta\delta(1-\tau)\rho} \ < \ 0, \qquad \frac{\partial \theta_1^*(\gamma)}{\partial \tau_0} = 0.$$

Hence, a reduction in τ_0 raises the enrollment cutoff θ_0^* (more students leave school in $t = 0$), while the re-entry cutoff is unaffected. The tax-free year therefore unambiguously increases school dropout in $t = 0$. However, whether students leave school temporarily or permanently depends on the size of the re-entry cost and other parameter values. We summarize the potential effect in the following proposition.

Proposition 1 (Tax-free year with anticipated re-entry costs)**.** *Assume $\kappa_0 = \kappa_1 = \kappa > 0$, and that the re-entry cost $\gamma \geqslant 0$ is known at $t = 0$. Using*

$$\theta_0^*(\gamma, \tau_0) = \frac{(1-\tau_0) + \kappa - \beta\delta(\kappa + \gamma)}{\beta\delta(1-\tau)\rho}, \qquad \theta_1^*(\gamma) = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau) + \kappa + \gamma\big]}{\beta\delta(1-\tau)\rho},$$

define the enrollment cutoffs in the baseline (B) and tax-free year (TFY) as

$$\theta_0^B(\gamma) \equiv \theta_0^*(\gamma,\tau), \qquad \theta_0^{TFY}(\gamma) \equiv \theta_0^*(\gamma,\tau_0^{TFY}),$$

where $\tau_0^{TFY} < \tau$*. The set of tax-free-year–induced dropouts is*

$$\mathcal{D}(\gamma) = [\theta_0^B(\gamma),\, \theta_0^{TFY}(\gamma)).$$

Let

$$\gamma_T \equiv \frac{\delta(1-\beta)\big[(1-\tau)+\kappa\big]}{1-\delta(1-\beta)}, \qquad \gamma_P \equiv \frac{(\tau-\tau_0^{TFY})+\delta(1-\beta)\big[(1-\tau)+\kappa\big]}{1-\delta(1-\beta)}.$$

Then the tax-free year has three possible effects on dropout:

1. **Temporary dropout only.** *If* $\gamma \leqslant \gamma_T$*, then* $\theta_1^*(\gamma) \leqslant \theta_0^B(\gamma)$ *and all tax-free-year–induced dropouts have* $\theta \geqslant \theta_1^*(\gamma)$*; the tax-free year generates only temporary dropout.*
2. **Permanent dropout only.** *If* $\gamma \geqslant \gamma_P$*, then* $\theta_0^{TFY}(\gamma) \leqslant \theta_1^*(\gamma)$ *and all tax-free-year–induced dropouts have* $\theta < \theta_1^*(\gamma)$*; the tax-free year generates only permanent dropout.*
3. **Temporary and permanent dropout.** *If* $\gamma_T < \gamma < \gamma_P$*, then* $\theta_0^B(\gamma) < \theta_1^*(\gamma) < \theta_0^{TFY}(\gamma)$ *and the tax-free year induces both permanent and temporary dropout: individuals with* $\theta \in [\theta_0^B(\gamma),\, \theta_1^*(\gamma))$ *drop out permanently, while those with* $\theta \in [\theta_1^*(\gamma),\, \theta_0^{TFY}(\gamma))$ *return to school in* $t = 1$*.*

Proof. With $\kappa_0 = \kappa_1 = \kappa$ and re-entry cost γ, the gap between the enrollment and re-entry thresholds, for a generic initial tax rate τ_0, is

$$\theta_0^*(\gamma,\tau_0) - \theta_1^*(\gamma) = \frac{(\tau-\tau_0)+\delta(1-\beta)\big[(1-\tau)+\kappa\big]-\gamma\big[1-\delta(1-\beta)\big]}{\beta\delta(1-\tau)\rho}.$$

Since $\delta \in (0,1)$ and $\beta \in (0,1]$, we have $1-\delta(1-\beta) > 0$.

Evaluating this gap at $\tau_0 = \tau$ (no tax-free year) gives

$$\theta_0^B(\gamma) - \theta_1^*(\gamma) = \frac{\delta(1-\beta)\big[(1-\tau)+\kappa\big]-\gamma\big[1-\delta(1-\beta)\big]}{\beta\delta(1-\tau)\rho}.$$

This gap is nonnegative if and only if

$$\gamma \leqslant \frac{\delta(1-\beta)\big[(1-\tau)+\kappa\big]}{1-\delta(1-\beta)} \equiv \gamma_T.$$

Thus, $\gamma \leqslant \gamma_T$ is equivalent to $\theta_1^*(\gamma) \leqslant \theta_0^B(\gamma)$.

Evaluating the same gap at $\tau_0 = \tau_0^{TFY}$ yields

$$\theta_0^{TFY}(\gamma) - \theta_1^*(\gamma) = \frac{(\tau-\tau_0^{TFY})+\delta(1-\beta)\big[(1-\tau)+\kappa\big]-\gamma\big[1-\delta(1-\beta)\big]}{\beta\delta(1-\tau)\rho}.$$

This gap is nonpositive if and only if

$$\gamma \geqslant \frac{(\tau - \tau_0^{TFY}) + \delta(1-\beta)\big[(1-\tau) + \kappa\big]}{1 - \delta(1-\beta)} \equiv \gamma_P.$$

Thus, $\gamma \geqslant \gamma_P$ is equivalent to $\theta_0^{TFY}(\gamma) \leqslant \theta_1^*(\gamma)$.

The tax-free year changes only the initial tax rate, so the induced leavers are exactly those with

$$\theta \in \mathcal{D}(\gamma) = [\theta_0^B(\gamma), \theta_0^{TFY}(\gamma)).$$

By definition of $\theta_1^*(\gamma)$, individuals with $\theta < \theta_1^*(\gamma)$ never return to school, while those with $\theta \geqslant \theta_1^*(\gamma)$ do return. The three cases follow by comparing $\theta_1^*(\gamma)$ to the endpoints of $\mathcal{D}(\gamma)$. □

Unanticipated re-entry cost. Now suppose students believe $\gamma = 0$ at $t = 0$ but discover at $t = 1$ that the true re-entry cost is $\gamma > 0$. The perceived thresholds at $t = 0$ are $\theta_0^*(0)$ and $\theta_1^*(0)$ as defined above, while the realized re-entry threshold becomes

$$\theta_1^*(\gamma) = \theta_1^*(0) + \frac{\gamma(1-\delta)}{\beta\delta(1-\tau)\rho} \; > \; \theta_1^*(0).$$

We focus on the economically relevant case where the tax-free year induces a non-empty set of dropouts $\mathcal{D}(0) = [\theta_0^B(0), \theta_0^{TFY}(0))$ and $\theta_1^*(0) < \theta_0^{TFY}(0)$, so that some of these dropouts expect to return. Once the true cost $\gamma > 0$ is realized, the re-entry bar rises, converting part of this expected temporary dropout into permanent dropout. Intuitively, a student who leaves school expecting to return faces a higher effective barrier than one who anticipated the cost from the outset—the unanticipated cost raises the re-entry threshold without having reduced the initial dropout response.

The effect of the tax-free year on dropout under unanticipated re-entry costs is summarized in the following proposition.

Proposition 2 (Tax-free year with unanticipated re-entry costs)**.** *Assume $\kappa_0 = \kappa_1 = \kappa > 0$ and that at $t = 0$ students believe $\gamma = 0$, while the true re-entry cost realized at $t = 1$ is $\gamma > 0$. Suppose the tax-free year induces a non-empty set of dropouts $\mathcal{D}(0) = [\theta_0^B(0), \theta_0^{TFY}(0))$ and that $\theta_1^*(0) < \theta_0^{TFY}(0)$, so that some of these dropouts expect to return under their initial beliefs. Then:*

1. ***Permanent dropout.*** *For any $\gamma > 0$ such that $\theta_1^*(\gamma) > \max\{\theta_0^B(0), \theta_1^*(0)\}$, the tax-free year generates a non-empty set of permanent dropouts among $\mathcal{D}(0)$:*

$$\{\theta : \; \theta \in \mathcal{D}(0), \; \theta_1^*(0) \leqslant \theta < \theta_1^*(\gamma)\} \; \neq \; \varnothing.$$

   These are individuals who expected to re-enter when $\gamma = 0$ but no longer find it optimal to return once $\gamma > 0$ is realized.

2. ***Temporary versus permanent dropout.***

(a) *If* $\theta_1^*(\gamma) \geqslant \theta_0^{TFY}(0)$*, then all tax-free-year–induced dropouts in* $\mathcal{D}(0)$ *have* $\theta < \theta_1^*(\gamma)$ *and never return: the response to the tax-free year is* purely permanent*.*

(b) *If* $\theta_1^*(\gamma) < \theta_0^{TFY}(0)$*, then the tax-free year induces both permanent and temporary dropout: individuals with* $\theta \in [\theta_0^B(0),\, \theta_1^*(\gamma))$ *drop out permanently, while those with* $\theta \in [\theta_1^*(\gamma),\, \theta_0^{TFY}(0))$ *return to school in* $t = 1$*.*

Proof. Under the belief $\gamma = 0$, the tax-free year lowers τ_0 and shifts the enrollment cutoff from $\theta_0^B(0)$ to $\theta_0^{TFY}(0)$, generating the dropout set $\mathcal{D}(0) = [\theta_0^B(0), \theta_0^{TFY}(0))$. Students also believe that re-entry requires $\theta \geqslant \theta_1^*(0)$.

At $t = 1$, they learn that the true re-entry cost is $\gamma > 0$, so the re-entry threshold rises to

$$\theta_1^*(\gamma) = \theta_1^*(0) + \frac{\gamma(1-\delta)}{\beta\delta(1-\tau)\rho} \;>\; \theta_1^*(0).$$

By assumption, $\theta_1^*(0) < \theta_0^{TFY}(0)$, so there is a subset of $\mathcal{D}(0)$ with $\theta \geqslant \theta_1^*(0)$ who expected to return under their beliefs. For any $\gamma > 0$ such that $\theta_1^*(\gamma) > \max\{\theta_0^B(0), \theta_1^*(0)\}$, the intersection

$$[\theta_0^B(0), \theta_0^{TFY}(0)) \cap [\theta_1^*(0), \theta_1^*(\gamma))$$

is non-empty. Types in this intersection satisfy $\theta \in \mathcal{D}(0)$, $\theta \geqslant \theta_1^*(0)$ (so they intended to re-enter) but $\theta < \theta_1^*(\gamma)$ (so they do not return in $t = 1$). They are permanent dropouts, proving part (1).

For part (2), note that whether there are any temporary dropouts depends on the position of $\theta_1^*(\gamma)$ relative to $\mathcal{D}(0)$. If $\theta_1^*(\gamma) \geqslant \theta_0^{TFY}(0)$, every $\theta \in \mathcal{D}(0)$ satisfies $\theta < \theta_1^*(\gamma)$ and no one returns: all induced dropouts are permanent. If instead $\theta_1^*(\gamma) < \theta_0^{TFY}(0)$, then $\mathcal{D}(0)$ splits at $\theta_1^*(\gamma)$ into a lower segment $[\theta_0^B(0), \theta_1^*(\gamma))$ of permanent dropouts and an upper segment $[\theta_1^*(\gamma), \theta_0^{TFY}(0))$ whose members exceed the realized re-entry cutoff and therefore return to school in $t = 1$. □

C.6 Misperception of Returns

This extension allows individuals to hold incorrect beliefs about the return to schooling. Following the subjective-expectations framework in Manski (1993), individuals form beliefs about the earnings return ρ based on the educational and labor market outcomes they observe in their reference group—parents, peers, coworkers, or others in their social environment. We parameterize misperception as a multiplicative distortion of the true return:

$$\mathbb{E}_t[\rho] = (1 - \varepsilon_t)\rho,$$

where ε_t may differ across $t = 0$ and $t = 1$ if individuals update their expectations after observing new signals. If $\varepsilon_t = 0$, individuals correctly perceive the true return; if $\varepsilon_t > 0$ they underestimate it; and if $\varepsilon_t < 0$ they overestimate it.

Replacing ρ by $\mathbb{E}_t[\rho]$ in (22)–(23) yields the perceived enrollment and re-entry thresholds

$$\theta_0^* = \frac{(1-\tau_0)+\kappa_0-\beta\delta(\kappa_1+\gamma)}{\beta\delta(1-\tau)\rho(1-\varepsilon_0)}, \qquad \theta_1^* = \frac{\beta\delta(1-\tau)+(1-\delta)\big[(1-\tau)+\kappa_1+\gamma\big]}{\beta\delta(1-\tau)\rho(1-\varepsilon_1)}.$$

Enrollment depends on the return perceived at $t=0$, $(1-\varepsilon_0)\rho$, while re-entry depends on the return perceived at $t=1$, $(1-\varepsilon_1)\rho$.

Fixed misperception with no re-entry cost: temporary dropout only. We begin by setting $\gamma = 0$ and considering fixed beliefs, $\varepsilon_1 = \varepsilon_0 \equiv \varepsilon$. In this case, misperception scales both cutoffs upward through the common factor $1/(1-\varepsilon)$ but does not create a wedge between re-entry and permanent exit. In particular, differentiating with respect to the initial tax rate yields

$$\frac{\partial\theta_0^*}{\partial\tau_0} = -\frac{1}{\beta\delta(1-\tau)\rho(1-\varepsilon)} < 0, \qquad \frac{\partial\theta_1^*}{\partial\tau_0} = 0.$$

Thus, the tax-free year affects only the enrollment margin and increases initial school leaving in $t=0$. However, when $\gamma = 0$ temporary dropout strictly dominates permanent exit for all types who would ever return; hence, fixed misperception alone cannot generate permanent dropout in response to the tax-free year.

Fixed misperception does, however, amplify the temporary response. The relevant cross-partial derivative is

$$\frac{\partial^2\theta_0^*}{\partial\tau_0\,\partial\varepsilon} = -\frac{1}{\beta\delta(1-\tau)\rho(1-\varepsilon)^2} < 0,$$

so the absolute magnitude of the response of θ_0^* to τ_0 is larger when perceived returns are lower. Intuitively, students who underestimate returns view schooling as less valuable and are therefore more sensitive to the short-run financial incentive created by the tax-free year.

Belief updating with no re-entry cost: permanent dropout through pessimistic updating. Next, continue to set $\gamma = 0$ but allow beliefs to update between $t=0$ and $t=1$. A natural case is pessimistic updating, $\varepsilon_1 > \varepsilon_0$, which corresponds to a decline in perceived returns after working during the tax-free year:

$$\varepsilon_1 > \varepsilon_0 \qquad \Longleftrightarrow \qquad \mathbb{E}_1[\rho] < \mathbb{E}_0[\rho].$$

Because $\theta_1^*(\varepsilon_1)$ is increasing in ε_1, pessimistic updating raises the re-entry cutoff:

$$\theta_1^*(\varepsilon_1) = \frac{\beta\delta(1-\tau)+(1-\delta)\big[(1-\tau)+\kappa_1\big]}{\beta\delta(1-\tau)\rho(1-\varepsilon_1)} > \frac{\beta\delta(1-\tau)+(1-\delta)\big[(1-\tau)+\kappa_1\big]}{\beta\delta(1-\tau)\rho(1-\varepsilon_0)} = \theta_1^*(\varepsilon_0).$$

Consequently, some students who left school in $t=0$ expecting to return—those with $\theta \geqslant \theta_1^*(\varepsilon_0)$—no longer find it optimal to re-enroll once beliefs become more pessimistic, i.e. if $\theta < \theta_1^*(\varepsilon_1)$. Thus, even when re-entry is costless, belief updating can convert part of the initial, tax-induced dropout into permanent dropout.

Fixed misperception with re-entry frictions: complementarity and permanent dropout. We now combine fixed misperception with re-entry frictions. Suppose beliefs are fixed over time, $\varepsilon_1 = \varepsilon_0 \equiv \varepsilon$, but re-entry is costly, $\gamma > 0$. As before, define baseline and tax-free-year enrollment cutoffs under perceived returns as

$$\theta_0^B(\gamma, \varepsilon) \equiv \theta_0^*(\varepsilon; \tau_0 = \tau), \qquad \theta_0^{TFY}(\gamma, \varepsilon) \equiv \theta_0^*(\varepsilon; \tau_0 = \tau_0^{TFY}),$$

and let the set of individuals induced to leave school in $t = 0$ by the tax-free year be

$$\mathcal{D}(\gamma, \varepsilon) = [\theta_0^B(\gamma, \varepsilon),\, \theta_0^{TFY}(\gamma, \varepsilon)).$$

Conditional on leaving school in $t = 0$, re-entry is optimal if and only if $\theta \geqslant \theta_1^*(\gamma, \varepsilon)$, where

$$\theta_1^*(\gamma, \varepsilon) = \frac{\beta\delta(1-\tau) + (1-\delta)\big[(1-\tau) + \kappa_1 + \gamma\big]}{\beta\delta(1-\tau)\rho(1-\varepsilon)}.$$

Hence, whether tax-free-year–induced leavers return to school or drop out permanently depends on the position of $\theta_1^*(\gamma, \varepsilon)$ relative to $\mathcal{D}(\gamma, \varepsilon)$.

To characterize how misperception and re-entry frictions interact, recall from Proposition 1 that under correct beliefs ($\varepsilon = 0$) there exist cutoff values γ_T and γ_P such that the tax-free year generates (i) only temporary dropout if $\gamma \leqslant \gamma_T$, (ii) both temporary and permanent dropout if $\gamma_T < \gamma < \gamma_P$, and (iii) only permanent dropout if $\gamma \geqslant \gamma_P$.

With fixed misperception, $\varepsilon_1 = \varepsilon_0 \equiv \varepsilon$, perceived returns scale the return to schooling by $(1-\varepsilon)$. Hence the baseline enrollment cutoff, the tax-free-year enrollment cutoff, and the re-entry cutoff are all multiplied by the common factor $1/(1-\varepsilon)$ relative to the correct-beliefs case:

$$\theta_0^B(\gamma, \varepsilon) = \frac{\theta_0^B(\gamma, 0)}{1-\varepsilon}, \qquad \theta_0^{TFY}(\gamma, \varepsilon) = \frac{\theta_0^{TFY}(\gamma, 0)}{1-\varepsilon}, \qquad \theta_1^*(\gamma, \varepsilon) = \frac{\theta_1^*(\gamma, 0)}{1-\varepsilon}.$$

Because this is a common positive rescaling, it leaves the pairwise comparisons among the three cutoffs unchanged. Thus fixed misperception does not change the critical re-entry-cost values that separate the temporary-dropout, mixed-dropout, and permanent-dropout regimes:

$$\gamma_T(\varepsilon) = \gamma_T, \qquad \gamma_P(\varepsilon) = \gamma_P.$$

Within a given regime, however, fixed pessimistic beliefs expand the relevant interior ability intervals by the factor $1/(1-\varepsilon)$ whenever these intervals remain interior. Since ability is uniformly distributed, the corresponding masses are scaled by the same factor. Thus, with beliefs fixed over time, pessimistic beliefs expand the set of students affected by the tax-free year but do not by themselves change the re-entry-cost thresholds separating temporary from permanent dropout. Re-entry costs and misperceived returns are nevertheless complementary in their effects on the mass of permanent dropout: pessimistic beliefs make schooling less attractive and expand the relevant ability intervals, while re-entry costs create the wedge that can prevent initial leavers from returning.

D Estimating Misperception of Schooling Returns

A question that arises from the theoretical model presented in Section 5 is how large does misperception need to be in order to be able to explain the estimated effects on dropout and earnings loss? This section seeks to provide an answer to this question using a simple structural estimation. Taking the model as the underlying structure, this implies using the empirical estimates and parameter estimates from the literature to calibrate the implied misperception.

D.1 Permanent-dropout calibration

The first approach builds on the empirical finding that the tax-free year does not generate temporary dropout among men. I therefore calibrate the model under the assumption that the marginal individual compares staying in school to permanent dropout, abstracting from temporary exit. I will then relax this assumption.

The starting point is the enrollment condition in (6), which holds with equality for the marginal student:

$$\frac{\beta\,\delta}{1-\delta}\,(1-\tau)\,(\rho\theta-1)=(1-\tau_0)+\kappa.$$

We interpret the IV estimate of the long-run earnings return to schooling as the proportional earnings gain from schooling in the model,

$$\rho\theta-1\equiv\hat{r}=0.194\ (\text{SE }0.092).$$

To quantify misperception, I parameterize beliefs directly over the net return to schooling and write

$$\mathbb{E}[\rho\theta-1]=(1-\varepsilon)\,(\rho\theta-1),$$

where $\varepsilon>0$ corresponds to underestimation of returns.[28]

If students permanently exit school in response to the tax-free year, the marginal student must be indifferent between staying and dropping out when the perceived return at enrollment, r^*, satisfies

$$r^*=\frac{(1-\tau_0)+\kappa}{(1-\tau)}\cdot\frac{1-\delta}{\beta\delta}.$$

The implied degree of underestimation is therefore the proportional gap between the true return

[28] While the main text models misperception over the return parameter ρ, the calibration parameterizes misperception directly over the net return to schooling, $r=\rho\theta-1$. This is the object estimated empirically and avoids having to separately identify ρ and the marginal student's ability θ. A proportional misperception over ρ would imply a perceived net return of $(1-\varepsilon)\rho\theta-1$, so the calibration should be interpreted as a reduced-form mapping from the model to the empirical return estimate.

and the perceived return required to rationalize dropout:

$$\varepsilon(\beta, \delta) = \frac{\hat{r} - r^*}{\hat{r}}.$$

Given values of τ_0, τ, and κ, this expression pins down for each pair of parameters β and δ the degree of underestimation of return required to rationalize the observed permanent dropout response. I set τ to 0.178, corresponding to the average marginal tax rate in the population, as reported in Sigurdsson (2025). This reflects the expected average lifetime tax rate for the marginal student. I set τ_0 to 0 consistent with the tax-free year. I then set κ to 0 to reflect that there are no or very low tuition fees in Iceland. Naturally, there are some costs associated with schooling, both financial and psychic costs. I make this conservative choice to obtain an upper bound on the implied misperception, since setting $\kappa > 0$ yields lower implied misperception.

Figure A.2 plots ε as a function of δ for $\beta \in \{1, 0.95, 0.88\}$. The three values for β correspond to a reference point of no present bias, the average estimate across all studies reviewed in a meta-analysis by Imai et al. (2021), and the average across estimates in studies of effort cost as opposed to monetary rewards, as reported in Imai et al. (2021). The figure marks the estimates at a discount factor of 0.888, which corresponds to the mean curvature-adjusted discount rate estimate of 12.59 in estimates of age-varying discount rates over the life cycle from age 25 to 80 in Kureishi et al. (2021). For these parameter values, the calibration implies that individuals underestimate returns by between 10% ($\beta = 0.88$) and 21% ($\beta = 1$).

D.2 Re-entry calibration

The calibration above implicitly assumes that students who drop out during the tax-free year do not intend to return to school. This assumption is motivated by the empirical finding that the tax-free year generates little evidence of temporary dropout among men. However, it does not rule out the possibility that students initially planned to return but subsequently chose not to. I therefore present a calibration that allows for intended re-entry and shows how misperception and re-entry costs jointly generate permanent dropout.

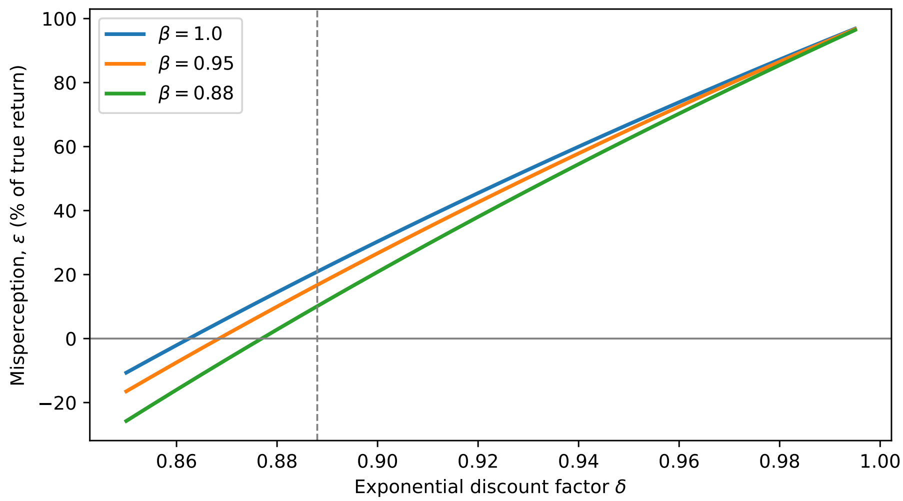
If students plan to return to school after working in $t = 0$, the relevant margin is the re-enrollment decision. From the $t = 1$ vantage point, re-entry is optimal whenever

$$\frac{\beta\delta}{1-\delta}(1-\tau)(\rho\theta - 1) \geqslant (1-\tau) + \kappa + \gamma.$$

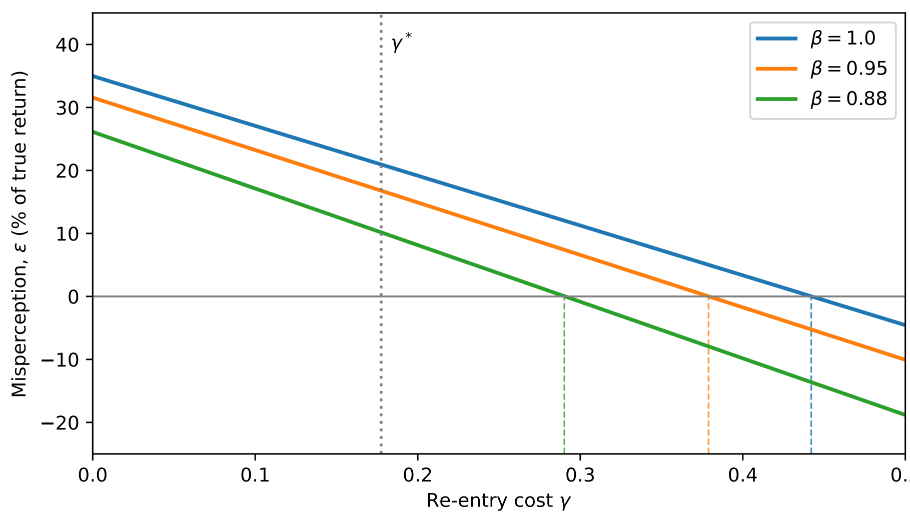
For the marginal student, this condition holds with equality. Let r_1^* denote the perceived net return at re-entry that rationalizes indifference. Solving yields

$$r_1^*(\gamma; \beta, \delta) = \frac{(1-\tau) + \kappa + \gamma}{1-\tau} \cdot \frac{1-\delta}{\beta\delta}.$$

Given the empirical estimate $\hat{r}$ of the net return to schooling, the implied misperception at the



(a) Permanent-dropout calibration



(b) Re-entry calibration

Figure A.2: Calibrated misperception

Notes: Panel (a) shows the implied misperception of the return to schooling required to rationalize permanent dropout as a function of the exponential discount factor δ and the present-bias factor β. The calibration assumes no temporary dropout, in line with the empirical estimates. The misperception parameter is defined as $\varepsilon = (\hat{r} - r^*)/\hat{r}$, so positive values indicate underestimation of the return to schooling relative to the empirical return, while negative values indicate overestimation. The vertical dashed line marks the reference value of δ used in the text. Panel (b) shows the implied misperception at the re-entry margin as a function of the re-entry cost γ. The horizontal line marks zero misperception. The colored dashed vertical lines mark the values of γ at which the re-entry calibration requires no misperception, so that re-entry costs alone rationalize permanent dropout. The gray dotted vertical line marks the re-entry cost γ^* at which the re-entry calibration coincides with the permanent-dropout calibration for the benchmark case $\beta = 0.95$.

re-entry margin is therefore

$$\varepsilon_1(\gamma; \beta, \delta) = \frac{\hat{r} - r_1^*(\gamma; \beta, \delta)}{\hat{r}}.$$

Figure A.2, panel (b), plots $\varepsilon_1(\gamma; \beta, \delta)$ as a function of the re-entry cost γ for the same values of β as in panel (a), setting $\delta = 0.888$. The figure illustrates the trade-off between misperceived returns and re-entry costs. Larger re-entry costs reduce the degree of misperception needed to rationalize non-return. The horizontal line marks zero misperception. The colored dashed vertical lines mark the values of γ at which the re-entry calibration requires no misperception, so that re-entry costs alone rationalize non-return.

The calibration yields three useful reference points. First, when $\gamma = 0$, returning to school entails no additional re-entry cost beyond the normal opportunity cost of schooling. In this case, the implied underestimation of returns ranges from 26 to 35 percent across the values of β considered. Thus, even without additional re-entry costs, the re-entry margin can be rationalized by moderate misperception. Second, the re-entry calibration coincides with the permanent-dropout calibration at $\gamma^* = \tau - \tau_0 = 0.178$. At this value, the re-entry cost exactly offsets the difference between the tax-free-year opportunity cost of leaving school and the normal-period opportunity cost of returning to school. Hence, the re-entry margin and the permanent-dropout margin imply the same degree of misperception, which ranges from 10 to 21 percent across the values of β considered. Third, if misperception is set to zero, the re-entry cost required to rationalize non-return ranges from 0.29 to 0.44. Thus, permanent dropout can result from moderate misperception, substantial re-entry costs, or a combination of the two. Even modest re-entry costs substantially reduce the amount of misperception required.

D.3 Benchmarks for Misperception of Returns

This appendix provides benchmarks for the magnitude of misperception of returns to schooling implied by the calibration in the main text. I first construct an internal benchmark using parental earnings data, and then compare these magnitudes to direct evidence on subjective beliefs from Jensen (2010).

D.3.1 Internal Benchmark: Parental Returns to Education

As an internal benchmark, I use parental earnings data to construct an independent estimate of misperception based on the information environment faced by students. The goal is to approximate the returns to schooling that children might infer from observing their parents' realized educational and labor-market outcomes.

Specifically, I estimate cohort-specific Mincerian returns to schooling in the full population using the regression

$$\log y_p = \alpha_{c(p)} + \beta S_p + \beta_{c(p)} S_p + u_p,$$

where y_p denotes parental earnings, S_p years of schooling, and $c(p)$ the parent's birth cohort. The interaction terms allow the return to schooling to vary flexibly across cohorts. Using the residuals from this regression, I classify parents according to whether their earnings are above or below what their education would predict within their cohort.

I then re-estimate the Mincerian regression restricting the sample to fathers with positive residuals. Conditioning on such outcomes alters the schooling gradient, reflecting the return to education that would be inferred by children observing only their own parents' realized educational and labor-market outcomes. I denote this estimate by $\hat{\beta}^{+}$. For comparison, I denote the corresponding estimate from the full sample of men in fathers' cohorts by $\hat{\beta}$.

Interpreting $\hat{\beta}^{+}$ as the perceived return and $\hat{\beta}$ as the actual return, misperception is measured as

$$\varepsilon^{\text{parent}} = 1 - \frac{\hat{\beta}^{+}}{\hat{\beta}}.$$

This exercise implies that students underestimate returns to schooling by 17.2 percent (1-0.072/0.087).

D.3.2 External Benchmark: Evidence from Jensen (2010)

Direct evidence on misperceptions of the returns to schooling is rare. A clear benchmark is provided by Jensen (2010), who elicits subjective expectations about earnings at different schooling levels among boys in their final year of compulsory schooling in the Dominican Republic and compares them to observed earnings differences.

Jensen reports, in Section II.D and Table III, both realized and perceived monthly earnings differences (in Dominican pesos, RD$) for adjacent schooling levels, separately for beliefs about own earnings ("Self") and beliefs about others' earnings ("Others"). I map these differences into the belief specification used in the model,

$$\mathbb{E}[\rho\theta - 1] = (1 - \varepsilon)(\rho\theta - 1),$$

and compute misperception as

$$\varepsilon = \frac{\hat{r} - r^{*}}{\hat{r}},$$

where $\hat{r}$ denotes the realized earnings difference and r^{*} the perceived earnings difference.

Table A.1 summarizes the implied degree of misperception, which ranges from about 74 to 78 percent across specifications. Jensen further notes that instrumental-variables estimates imply true returns that are approximately 10–20 percent larger than the observed earnings differences reported in Table III, which would imply slightly larger underestimation in magnitude. Accounting for this adjustment yields implied misperception in the range of roughly 75–80 percent.

Table A.1: Implied Misperception of Returns to Schooling from Jensen (2010)

	Secondary vs. Primary		Tertiary vs. Secondary	
	Self	Others	Self	Others
Realized earnings difference $\hat{r}$	1299	1299	5202	5202
Perceived earnings difference r^{*}	329	287	1282	1334
Implied misperception ε (%)	74.6	77.9	75.4	74.4

Notes: The table reports realized and perceived monthly earnings differences by schooling level from Table III of Jensen (2010). Implied misperception is calculated as $\varepsilon = (\hat{r} - r^{*})/\hat{r}$. Positive values indicate underestimation of returns.

Interpreted through the lens of the model, these magnitudes substantially exceed the degree of misperception required when the marginal student compares schooling directly to permanent

Table A.2: Upper-Secondary School Enrollment, Completion, Breaks, and Dropout

Upper-secondary school enrollment					
Enrolled	92.7				
Never enrolled	7.3				
	100.0				
Among those who enrolled					
Graduation timing (cumulative shares)					
Graduated by age 20	34.8				
Graduated by age 21	47.4				
Graduated by age 22	53.9				
Graduated by age 23	58.3				
Graduated by age 24	61.7				
		Semesters			
Status at age 24	Share (%)	Completed	Uncompleted	On break	Total
Graduated	61.7	8.0	0.7	0.7	9.4
Dropped out	31.1	2.2	2.3	1.8	6.4
Enrolled	7.1	3.4	3.1	3.1	9.6
	100.0				

Notes: The sample is the cohort born in 1975 who resided in Iceland on December 1, 1990. Outcomes and enrollment status are measured as of fall 1999, when the cohort was age 24. Enrollment therefore measures the share of the cohort that has ever enrolled by fall 1999, dropout is the share of enrolled students who have not graduated and are not enrolled in school in fall 1999, and enrolled is the share who is enrolled and has not graduated at that time. On-time graduation is at age 20, which for this cohort corresponds to spring 1995. A semester equals half a school year. Semester counts are averages within each group."Completed" counts number of semesters in which a student is registered and completes 9 or more credits, which is the requirement is pass a semester. "Uncompleted" counts number of semesters in which a student is registered but completes less than 9 credits, including semesters with no credits completed. "On break" counts semesters since first enrollment in which the student is not registered. "Total" is the number of semesters from first enrollment through the end of studies or fall 1999, whichever comes first.

exit, but correspond to the misperception implied by the calibration in which permanent dropout arises from the interaction of misperceived returns and re-entry frictions, especially when re-entry costs are low.

E Evidence on Upper-Secondary School Dropout and Re-entry Barriers

This appendix presents evidence on upper-secondary school trajectories, dropout, and re-entry barriers. The primary source is a study of the 1975 birth cohort by Jónasson and Blöndal (2002), which combines administrative school records with telephone and postal surveys conducted when the cohort was age 24. I complement this with evidence from the education register on graduation timing from a broader set of cohorts.

The study of the 1975 birth cohort samples individuals born in 1975 who were registered in the Icelandic national registry as residents of Iceland on December 1, 1990—the fall when the cohort was of age to begin upper-secondary school. This yields a base sample of 4,180 individuals. This cohort began primary school in 1981, would enroll in upper-secondary school in the fall of 1991,

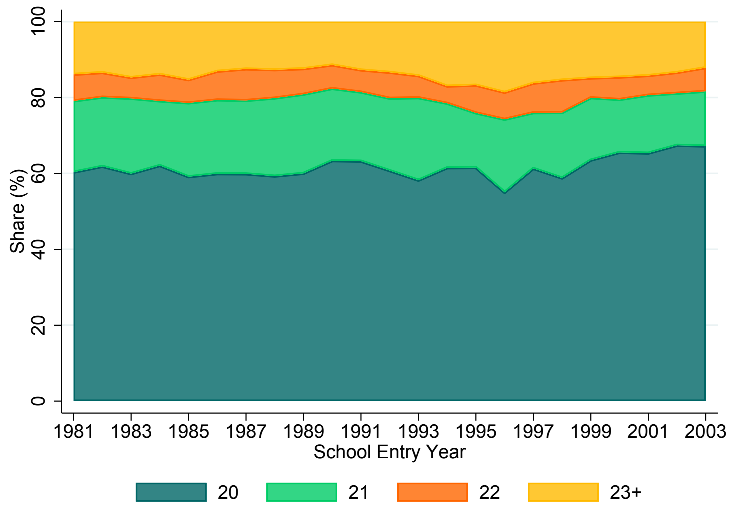


Figure A.3: Age at High School Graduation

Notes: The figure plots the share of students who graduate from high school by their age of graduation. School entry year is the year that a given birth cohort would at the earliest enter high school, which is at age 16.

and would graduate in 1995 if graduating on time.

The study combines three data sources. The first source is administrative records on upper-secondary school enrollment and completion, collected directly from all Icelandic upper-secondary schools and a number of specialized public institutions, such as agricultural and gardening schools. These records cover enrollment spells, course-taking, credits earned, and qualifications obtained, and were subsequently linked to records from Statistics Iceland on educational attainment and graduation. The school records were collected through spring 1998, and graduation and enrollment status is available through the end of 1999, when the cohort was age 24.

The second source is a telephone survey conducted in October 1999 with a random subsample of 1,000 individuals from the cohort. The response rate was 75 percent. The survey asked about education and employment, attitudes toward schooling, and, for those who had dropped out, the reasons for doing so. The dropout rate among the respondents was 29 percent, close to the rate observed in the cohort as a whole.

The third source is a postal survey conducted in November 1999, reaching the same random subsample of 1,000 individuals as the telephone survey. The response rate was 56 percent. It collected information on self-esteem, vocational interests, and self-assessed ability.

E.1 Enrollment, Completion, and Dropout Patterns

Table A.2 documents high-school enrollment, completion, breaks, and dropout among the 1975 cohort. About 93 percent of individuals enroll in upper-secondary school, and most—more than 80 percent—enroll in academic tracks, with the remainder enrolling in vocational tracks. Students who enroll in high school immediately after finishing compulsory education and progress on time graduate at age 20. About 35 percent of students do so. A large share of students takes longer to graduate, and by age 24 the share of students who have graduated from upper-secondary school reaches 62 percent. Of the remainder, 31 percent have dropped out and 7 percent remain enrolled, having enrolled late, studied at a slow pace, or taken breaks from their studies.

The table also reports further information on the pace of completion, as measured by semesters completed, semesters not passed, and semesters on break, i.e. semesters where students where not enrolled. Each school year comprises two semesters, and a high school degree at full pace takes eight semesters, or four years. Those who graduate complete eight semesters on average, where a completed semester is one in which the student earns nine or more credits—the passing threshold. In addition, they have on average 0.7 semesters in which they do not meet the credit threshold, and 0.7 semesters on break from school. Those who drop out complete on average about one year of school before leaving, in addition to roughly one year of breaks and an additional year without sufficient credits to pass. Those still enrolled at age 24 have taken approximately one and a half years of breaks since first enrolling and another three uncompleted semesters. The break semesters recorded even among graduates show that leaving and returning to school is common on the path to completion, and is not limited to those who ultimately drop out.

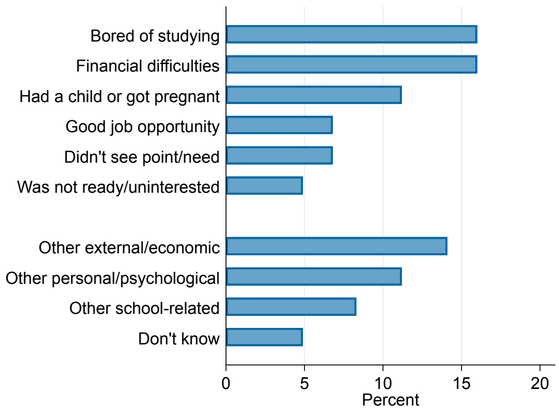
Figure A.3 shows that these patterns extend well beyond the 1975 cohort. Plotting the age at graduation for all high-school graduates from the early 1980s through the 2010s reveals a strikingly stable picture: about 60 percent graduate at age 20, corresponding to on-time completion, while 15–20 percent complete at ages 21 or 22 and roughly 10 percent at age 23 or older. The consistency of these patterns across four decades suggests that the detailed evidence from the 1975 cohort is broadly representative of the Icelandic upper-secondary system more generally.

This evidence highlights the flexibility of the Icelandic upper-secondary system. Students can progress slowly, take breaks, and re-enroll after dropping out without facing significant institutional obstacles. This makes the low rate of return among those who dropped out during the tax-free year harder to explain on organizational grounds alone, and points instead toward financial or psychological barriers as the primary obstacles to completion. The next subsection turns to survey evidence on this question.

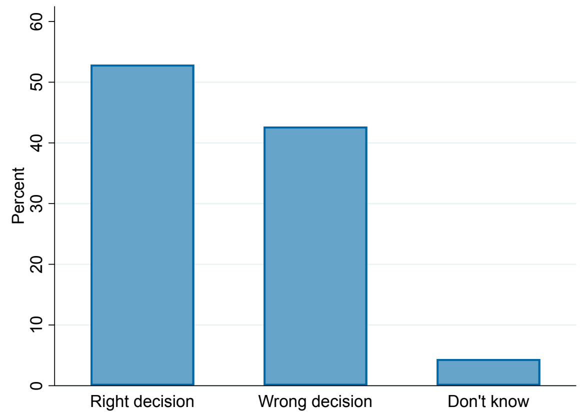
E.2 Dropout Decisions and Re-entry Barriers

Figure A.4 presents evidence from the telephone survey on the reasons for dropout and attitudes toward schooling among the 1975 cohort who had dropped out of upper-secondary school by age 24.

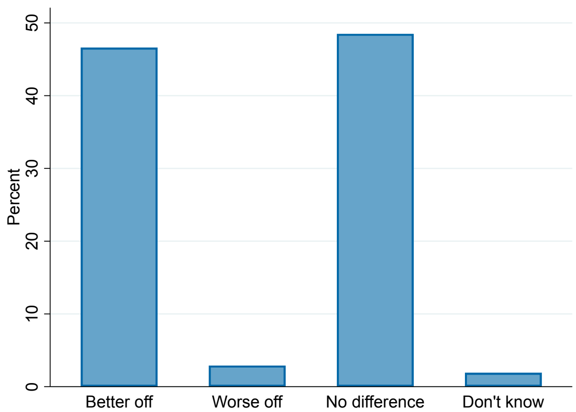
Panel (a) shows the most commonly cited primary reasons for dropping out. Beyond boredom with studying and pregnancy, financial difficulties and employment opportunities stand out: financial difficulties are cited by 16 percent of dropouts and a good job opportunity by 7 percent, together accounting for 23 percent of responses. Including other external and economic reasons raises this share to 37 percent. The prominence of financial difficulties and job opportunities suggests that labor market pull factors play an important role in the dropout decision, consistent with



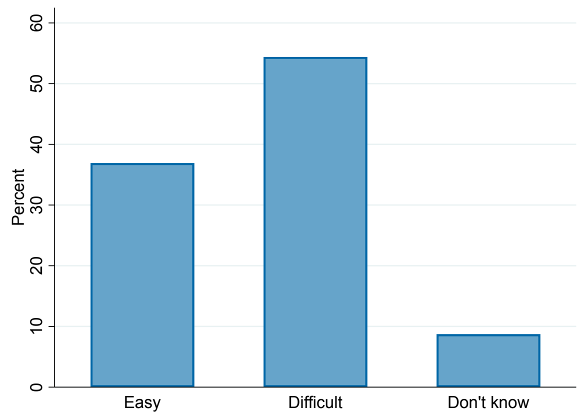
(a) Main reasons for dropping out



(b) Was dropping out the right decision?



(c) Better off on the labor market with degree?



(d) Would returning to school be easy or difficult?

Figure A.4: Survey Evidence on Dropout Decisions and Re-entry Barriers

Notes: Figures show responses from phone survey of upper-secondary school dropouts from the 1975 birth cohort, surveyed at age 24. The dropout sample consists of individuals who either did not continue to upper-secondary school after completing compulsory education or started but dropped out of upper-secondary school. *Panel A:* "Hver telur þú að hafi verið mikilvægasta ástæðan fyrir því að þú fórst ekki í framhaldsskóla eða hættir í framhaldsskóla?" (What do you think was the most important reason you did not start or dropped out of upper-secondary school?) *Panel B:* "Ef þú lítur til baka, finnst þér þú hafa tekið rétta eða ranga ákvörðun um að byrja ekki í eða halda ekki áfram framhaldsskólanámi miðað við þær aðstæður sem þú varst í þá?" (Looking back, do you think you made the right or wrong decision not to start or continue upper-secondary education given the circumstances you were in at the time?) *Panel C:* "Telur þú að þú værir betur sett(ur) á vinnumarkaðinum í dag, verr sett(ur) eða það myndi engu breyta ef þú hefðir próf úr framhaldsskóla?" (Do you think you would be better off, worse off, or would it make no difference in the labor market today if you had completed upper-secondary school?) *Panel D:* "Telur þú að það yrði erfitt eða auðvelt fyrir þig að hefja nám að nýju innan formlega skólakerfisins?" (Do you think it would be difficult or easy for you to start studying again in the formal school system?)

the findings of this paper.

Panel (b) shows that, looking back, a slim majority of dropouts—53 percent—consider their decision to have been the right one given the circumstances at the time, while 43 percent consider it to have been the wrong decision. This suggests that for a large share of dropouts, the decision

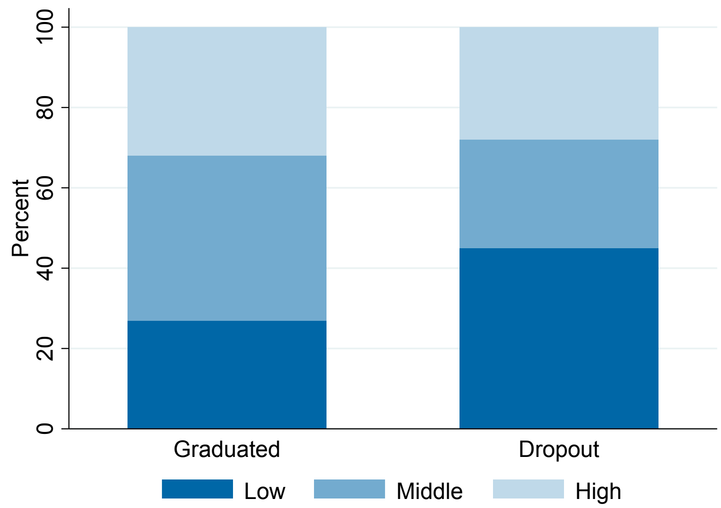


Figure A.5: Self-Esteem by Educational Status

Notes: The figure shows the distribution of self-esteem separately for upper-secondary school graduates and dropouts from the 1975 birth cohort, measured at age 24. Self-esteem is measured using the Rosenberg (1965) scale, which consists of 10 items rated on a four-point scale from strongly agree to strongly disagree. The average of the ten items is computed, with higher values indicating better self-esteem (scale range 1–4). *Low*, *Middle*, and *High* refer to the bottom, middle, and top thirds of the self-esteem distribution.

to leave school may have been driven by transitory circumstances rather than a lasting disinterest in education or low returns.

Panel (c) further illuminates how dropouts evaluate their decision. Nearly half—47 percent—believe they would be better off in the labor market today had they obtained an upper-secondary degree, while about the same share, 49 percent, believe it would have made no difference, and just 3 percent think they would be worse off. Together with Panel (b), this suggests that a substantial share of dropouts believe, in hindsight, that they would have benefited from staying in school.

Panel (d) turns to the question of re-entry. Despite the flexibility of the Icelandic system documented above, a majority of dropouts—54 percent—report that returning to school would be difficult, compared to 37 percent who say it would be easy. This is notable given that institutional barriers to re-enrollment are low. Together with the previous panels, it suggests that many dropouts regret their decision and believe they would benefit from returning, yet perceive the costs of doing so as prohibitively high—pointing toward financial or psychological barriers as the primary obstacles to degree completion.

Figure A.5 provides complementary evidence on the psychological characteristics of dropouts, based on responses to a postal survey. The figure shows the distribution of self-esteem, measured using the Rosenberg (1965) scale, separately for graduates and dropouts at age 24. The figure shows a clear contrast: among dropouts, 45 percent fall in the lowest third of the self-esteem distribution, compared to only 27 percent among graduates. Conversely, graduates are considerably more concentrated in the middle of the distribution—41 percent versus 27 percent among dropouts—while the two groups are broadly similar in the share with high self-esteem. This pattern suggests that low self-esteem is substantially more prevalent among dropouts than among those who complete their degree. While the cross-sectional nature of the data makes it difficult to establish whether low self-esteem precedes dropout or is partly a consequence of it, the result is consistent with the view that psychological barriers contribute to the low rate of re-entry among dropouts. Combined with the evidence in Figure A.4, this points to a picture in which many dropouts would benefit from returning to school, but low self-confidence and other psychological barriers leave many dropouts unlikely to return.

F Supplementary Figures

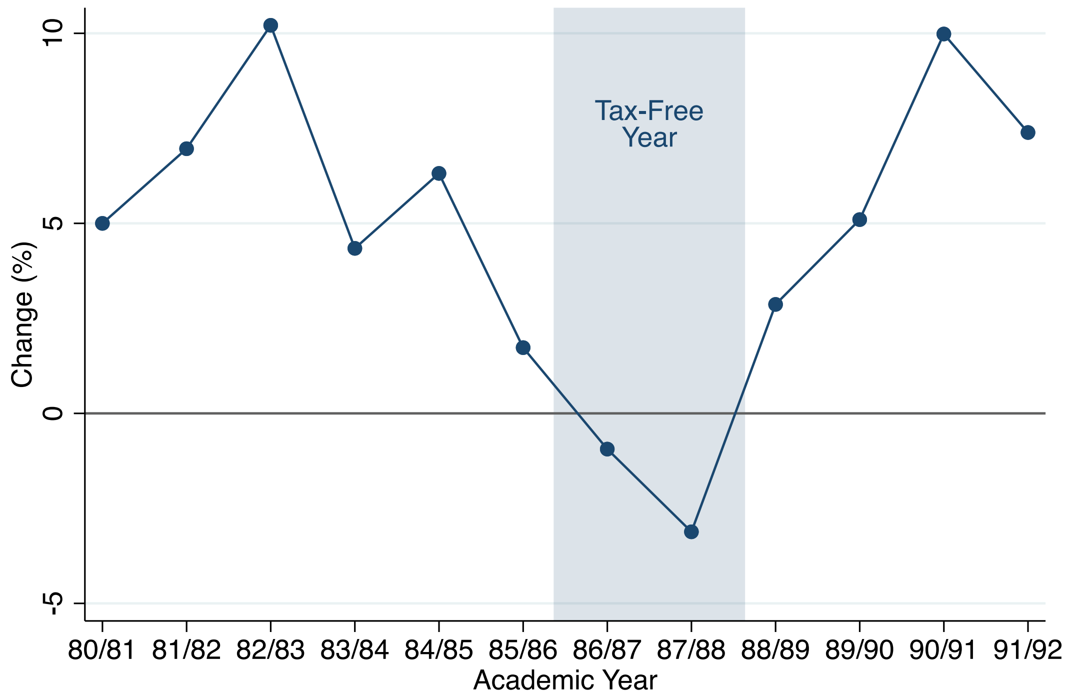
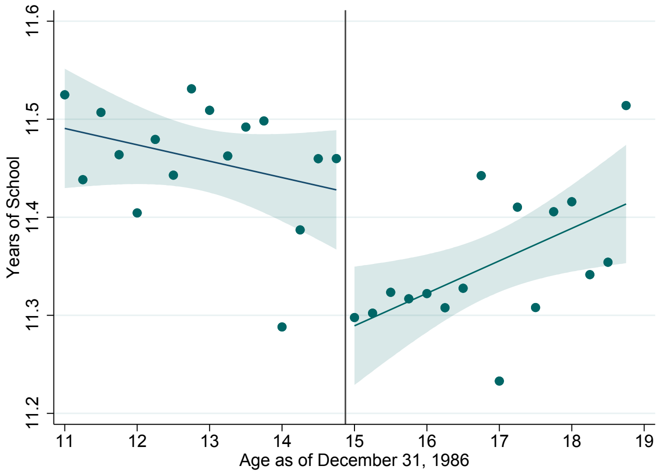
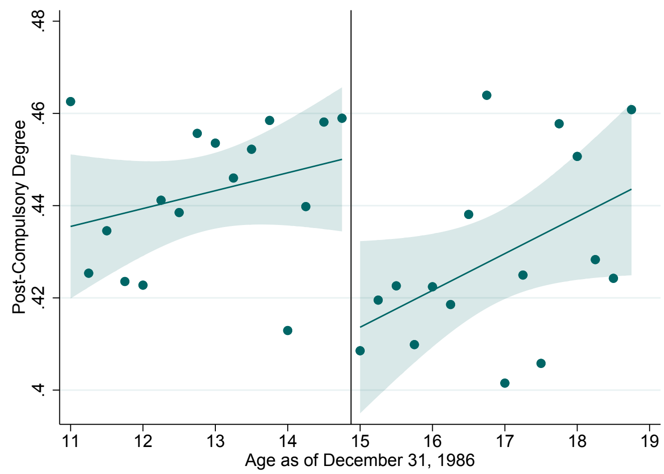


Figure A.6: Change in University Enrollment

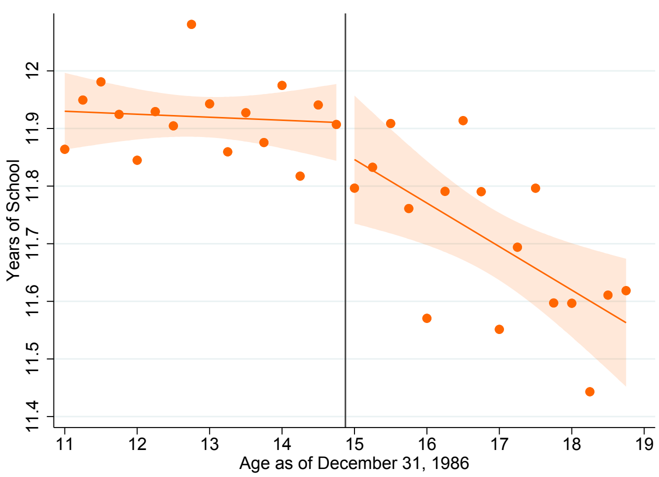
Notes: This figure plots the percentage change in the number of students enrolled in University education each academic year. The shaded area covers the two academic years that the tax-free year influenced.



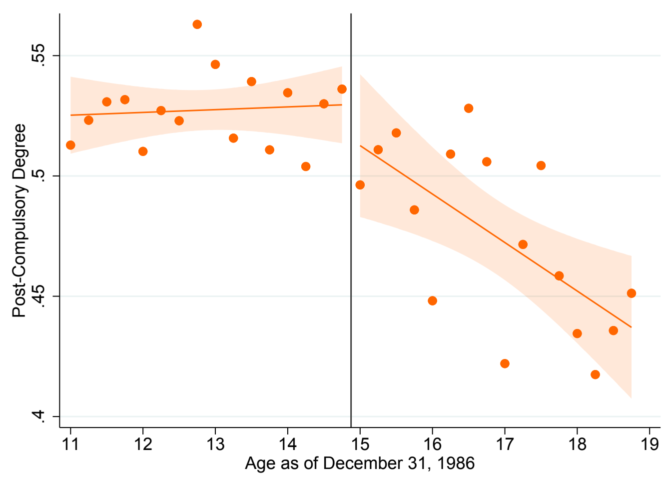
(a) Men: Years of school



(b) Men: Post-compulsory degree



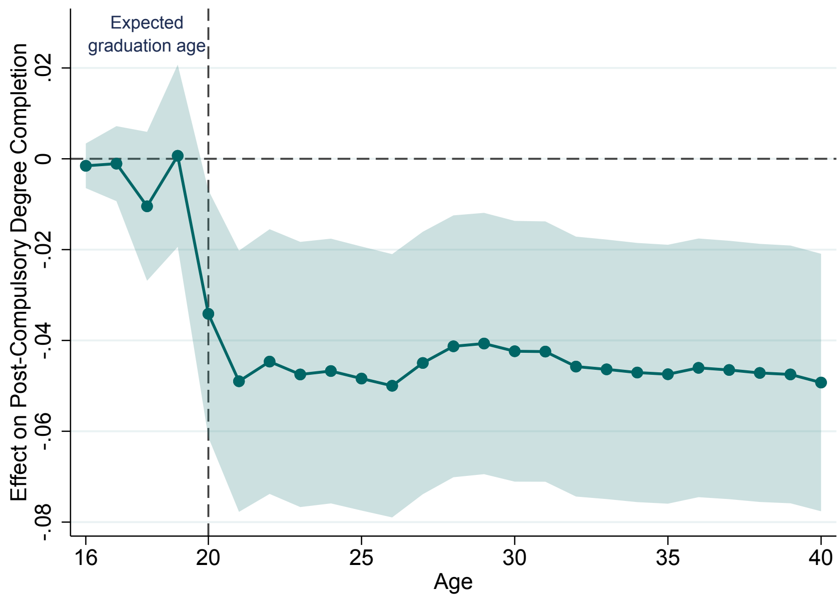
(c) Women: Years of school



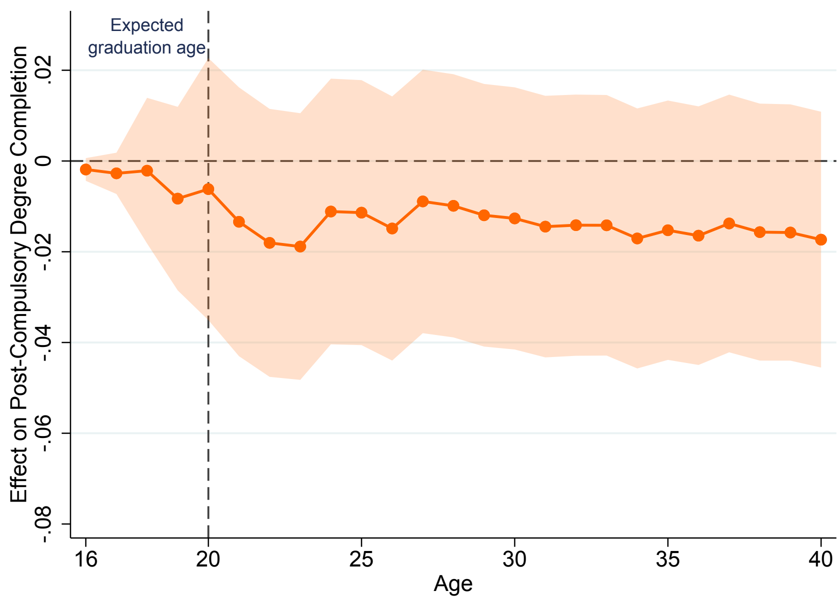
(d) Women: Post-compulsory degree

Figure A.7: Educational Attainment — Men and Women

Notes: This figure is a plot of average educational attainment at age 21 for four years on each side of the age threshold. Panels (a) and (c) plot the average number of pre-university years of school completed by men and women, respectively. Panels (b) and (d) plot the average share with a post-compulsory degree by men and women, respectively. The vertical line denotes the compulsory schooling age threshold. Dots are four-month age bins through which linear trends are fitted and their 95% confidence intervals.



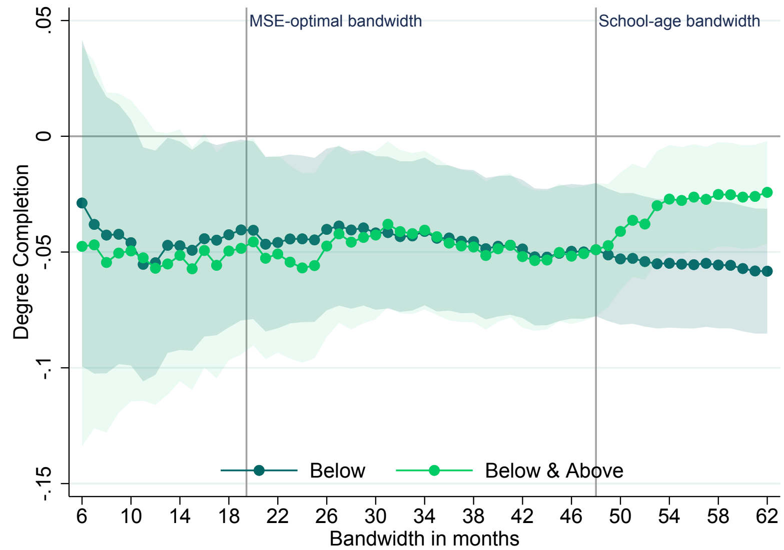
(a) Men: Post-Compulsory Degree



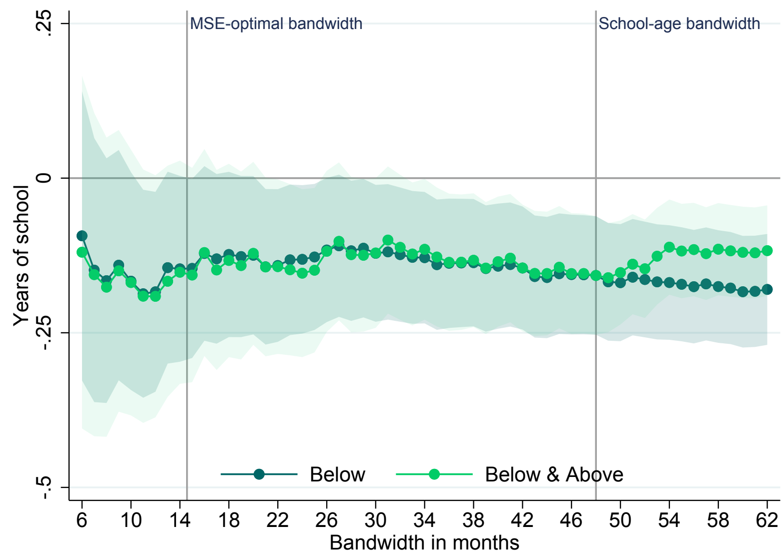
(b) Women: Post-Compulsory Degree

Figure A.8: Post-Compulsory Education

Notes: This figure plots estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panel (a) plots estimated effects on completion of a post-compulsory degree, i.e. of not dropping out, for men, and Panel (b) does the same for women. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The shaded areas show 95% confidence intervals.



(a) Post-Compulsory Degree



(b) Years of school

Figure A.9: Effect on Educational Attainment: Sensitivity to the Choice of Bandwidth

Notes: This figure plots effects on the educational attainment of men, measured in panel (a) with an indicator for completing a post-compulsory degree and, in panel (b), by years of school using equation (1) for different bandwidths. Each dot is a separate regression estimate. Both figures plot coefficients from two sets of regressions. In one I vary the bandwidth below the schooling age threshold (i.e. the control group) while maintaining a 48-month bandwidth above (i.e. the treatment group). This way the treatment group includes everyone at normal upper-secondary schooling age. In the other set of regressions, I vary the bandwidth both below and above the threshold. Vertical lines mark the estimated MSE-optimal bandwidth and the school-age bandwidth, i.e. the bandwidth that includes those at normal upper-secondary schooling age during the tax-free year. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The shaded areas show 95% confidence intervals.

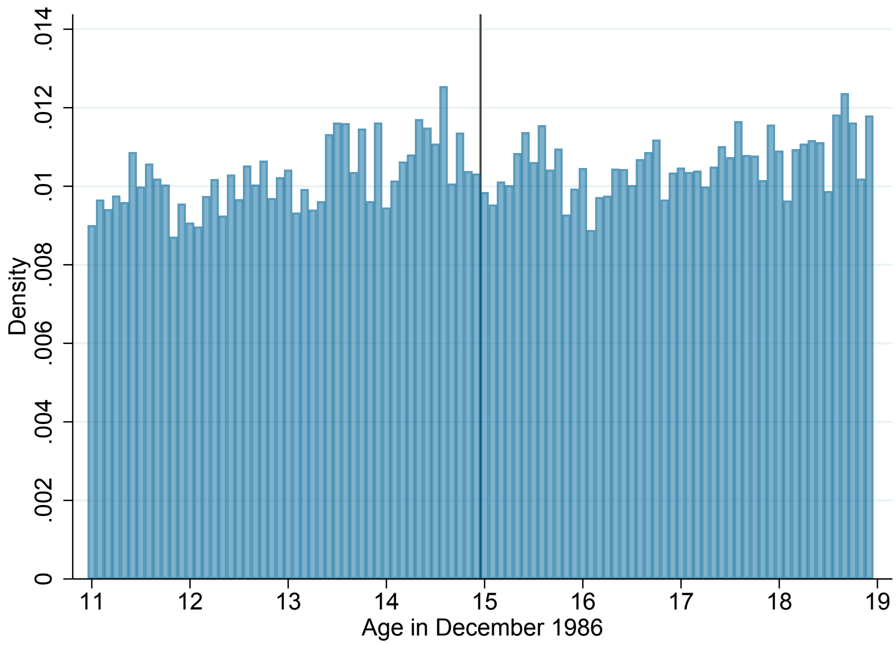
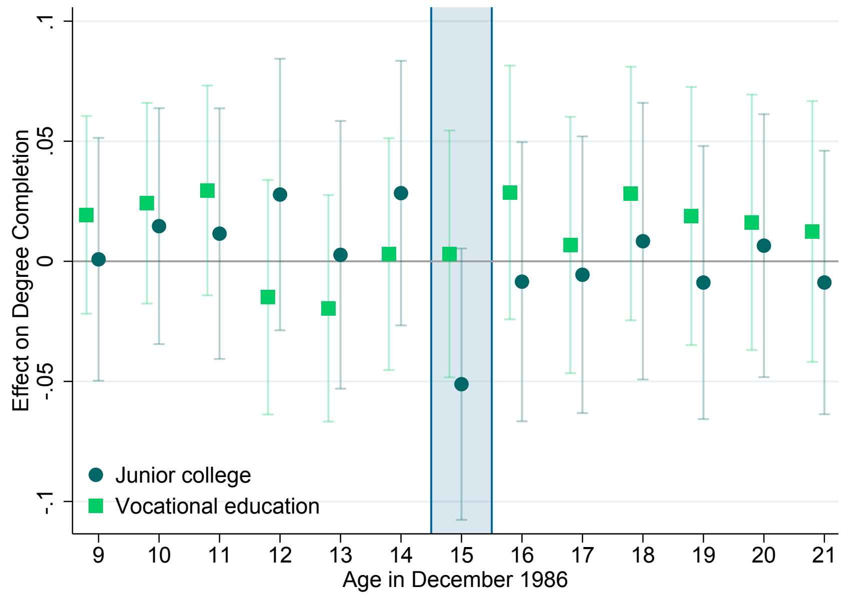
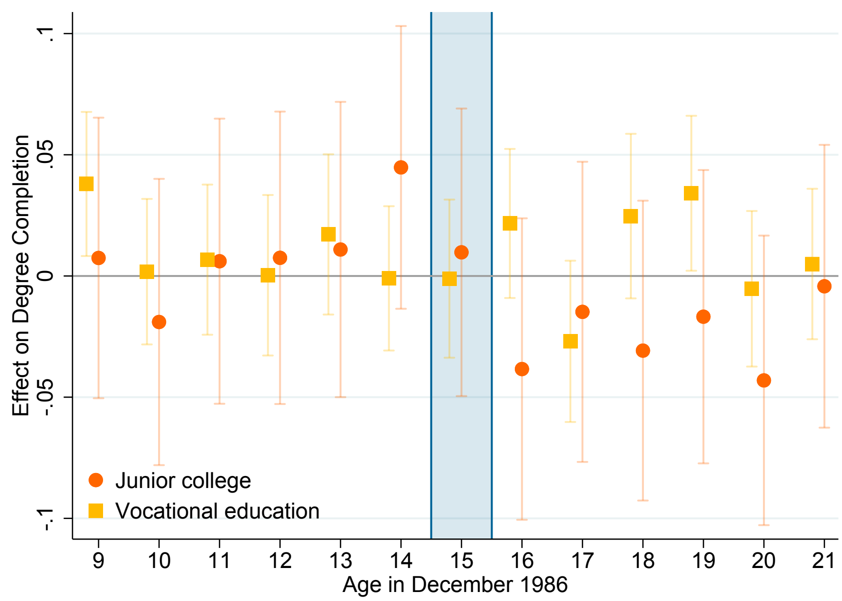


Figure A.10: Distribution of Births by Birth-Month Cohorts

Notes: This figure plots the distribution of births by birth-month cohorts of Icelanders who are between ages of 11 and 19 in December 1986. That is, cohorts born between January 1968 and December 1975.



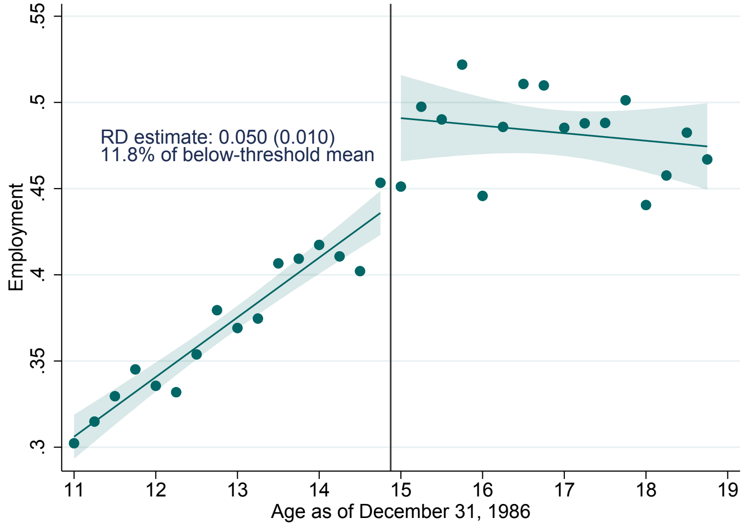
(a) Men



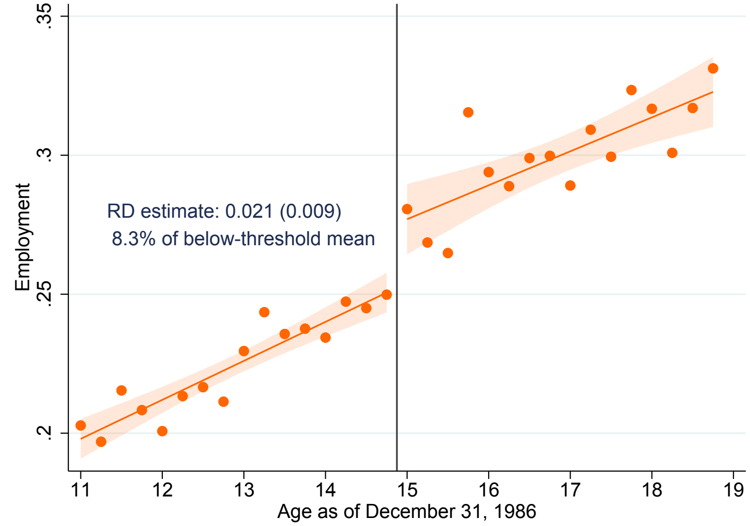
(b) Women

Figure A.11: Placebo Tests of Effects on Educational Attainment

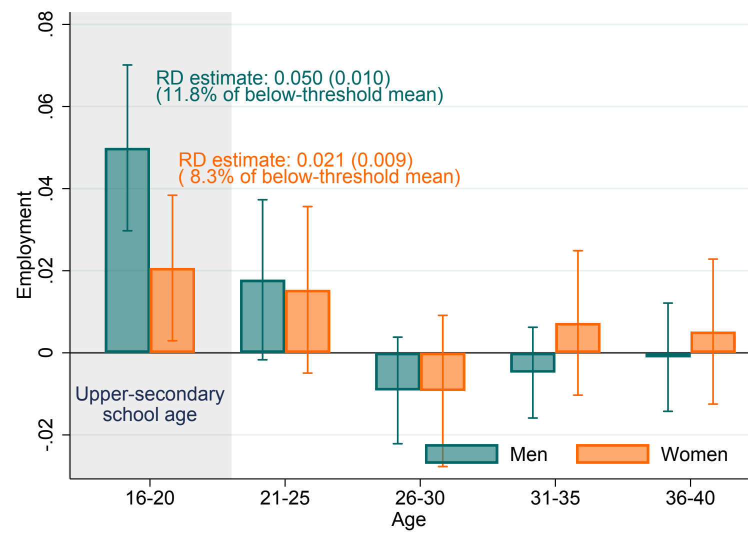
Notes: This figure plots tests of discontinuities in the educational attainment of men (panel a) and women (panel b) at the actual compulsory schooling age threshold in the tax-free year and at placebo thresholds. Educational attainment is measured as the completion of post-compulsory education, either junior college or vocational education. The bandwidth around the threshold is 12 months on each side. The figure plots the coefficient on an indicator for being above the relevant (actual or placebo) age threshold. The coefficient at age 14, for example, tests for discontinuities in the hypothetical tax-free year of 1989 but around the relevant age threshold (turning 16 by December 31, 1988). The students just above the school-age threshold in 1989 were 14 years old in 1987, which is the age used to label the x-axis. Regressions control linearly for date of birth in months and for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits.



(a) Men: Employment



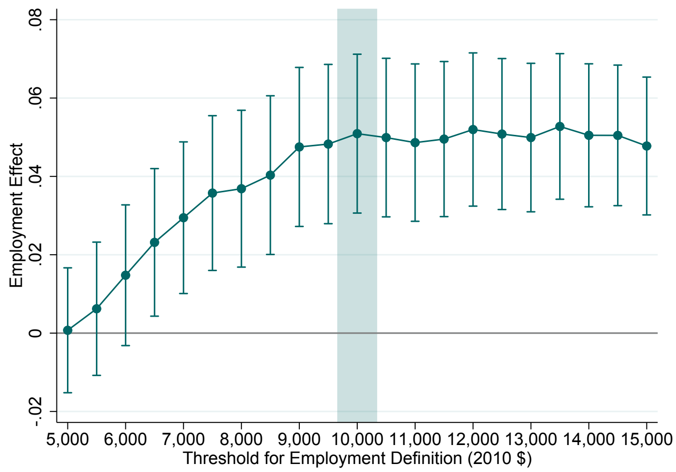
(b) Women: Employment



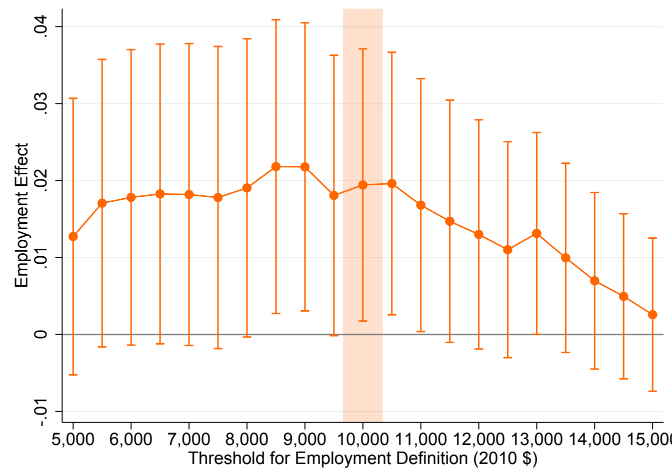
(c) Effect on Employment

Figure A.12: Effects of Tax-Free Year on Employment

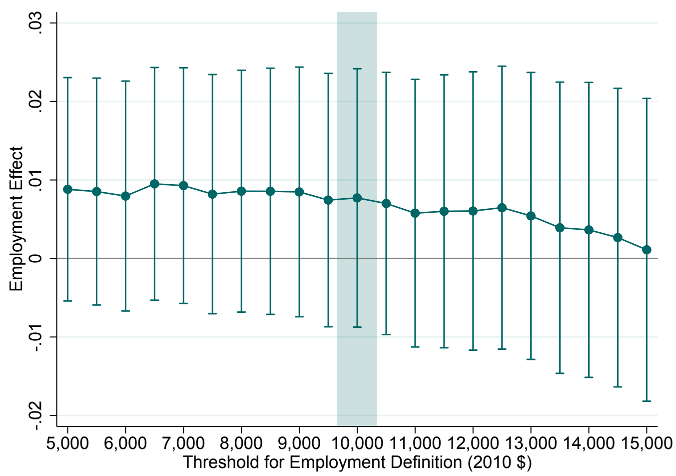
Notes: This figure studies the effect of the tax-free year on employment. Panels (a) and (b) plot the average employment at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Employment is defined as earning at least $10,000. Panel (c) plots RD estimates using equation (1) of the effect of the tax-free year on employment. The bars correspond to average effects at each age interval. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence intervals based on robust standard errors clustered at the individual level.



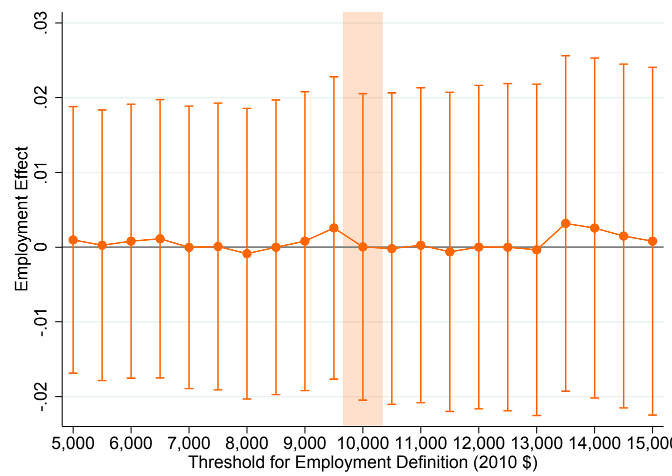
(a) Men: Effect on Employment at School Age



(b) Women: Effect on Employment at School Age



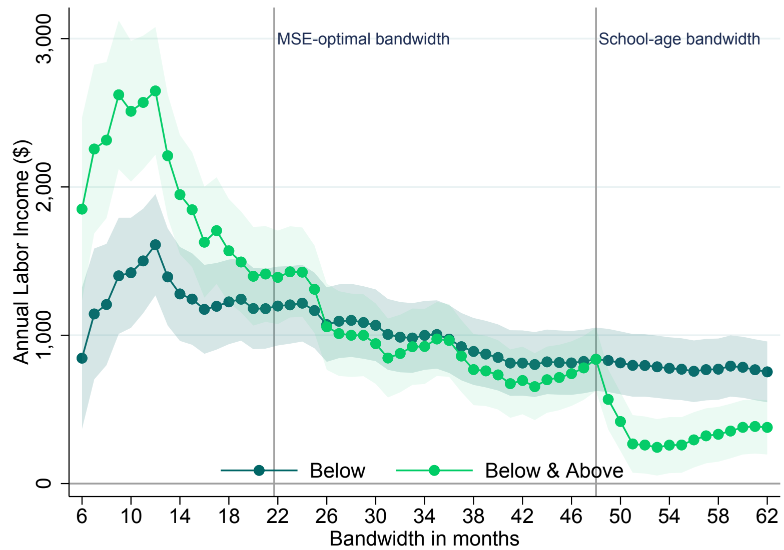
(c) Men: Effect on Employment at Prime Age



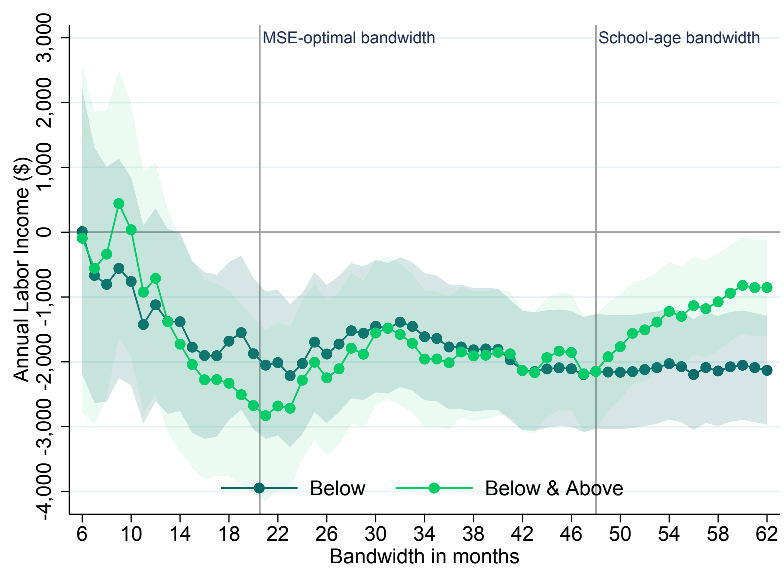
(d) Women: Effect on Employment at Prime Age

Figure A.13: Robustness to Varying the Earnings Threshold to Define Employment

Notes: This figure plots estimates of (1) where the outcome variable is employment defined as labor earnings exceeding a certain threshold. Panels (a) and (b) plot estimates at upper-secondary school age (16-20) for men and women, respectively. Panels (c) and (d) plot estimates at prime age (36-40) for men and women, respectively. Each point reflects one estimate, where the earnings threshold, defined in real terms (2010 US dollars) is varied from 5,000 to 15,000. Estimates in the main text are based on a threshold of $10,000, which is highlighted in the figure. The figure shows that the employment effects I obtain are robust to this definition.



(a) Labor Income at Schooling Age



(b) Labor Income at Prime Age

Figure A.14: Effect on Labor Market Outcomes: Sensitivity to the Choice of Bandwidth

Notes: This figure plots the estimated effects on labor income using equation (1) for different bandwidths around the compulsory schooling age threshold. Panel (a) plots estimates at upper-secondary schooling age, i.e. 16-20, and panel (b) at prime age, i.e. 36-40. Each dot is a separate regression estimate. Both figures plot coefficients from two sets of regressions. In one I vary the bandwidth below the schooling age threshold (i.e. the control group) while maintaining a 48-month bandwidth above (i.e. the treatment group). This way the treatment group includes everyone at normal upper-secondary schooling age. In the other set of regressions, I vary the bandwidth both below and above the threshold. Vertical lines mark the estimated MSE-optimal bandwidth and the school-age bandwidth, i.e. the bandwidth that includes those at normal upper-secondary schooling age during the tax-free year. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The shaded areas show 95% confidence intervals.

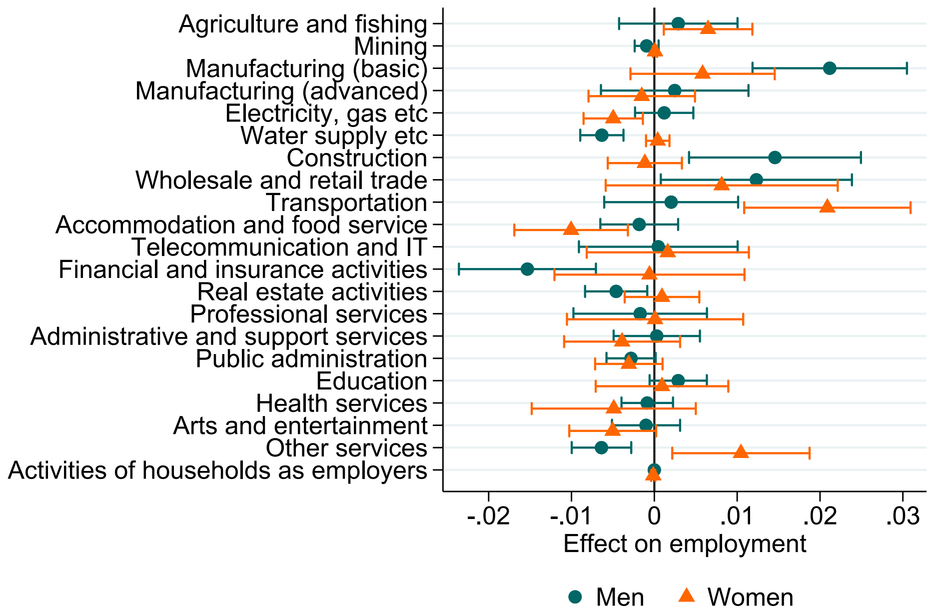


Figure A.15: Jobs at Prime Age

Notes: This figure plots the estimated effects on the sector of employment at prime age. The points are estimates of equation (1) where the outcome is an indicator of employment in a given sector at ages 36-40. The whiskers display the 95% confidence interval based on robust standard errors clustered at the individual level.

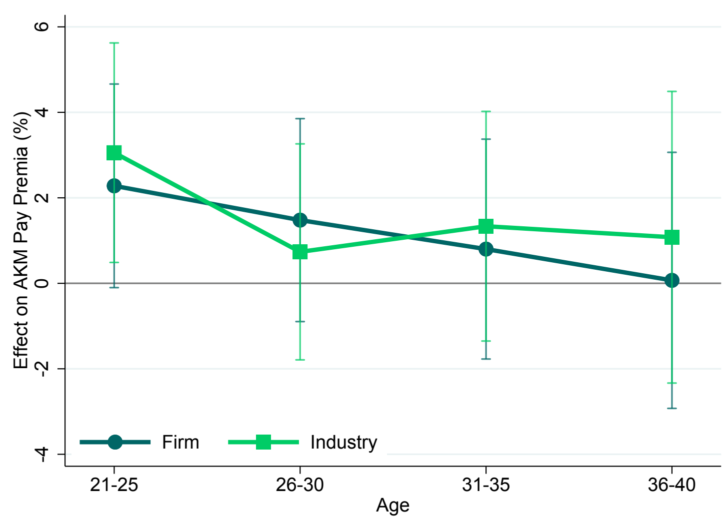
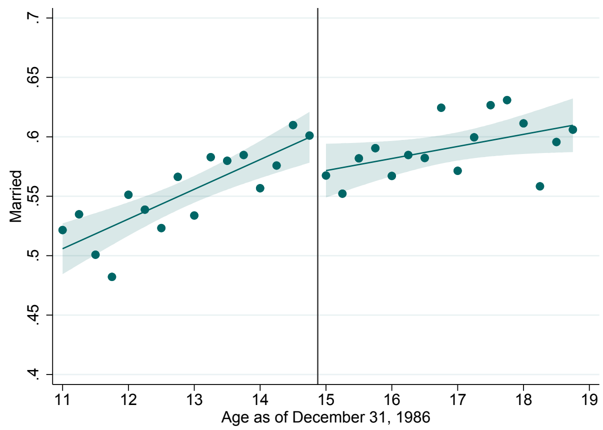
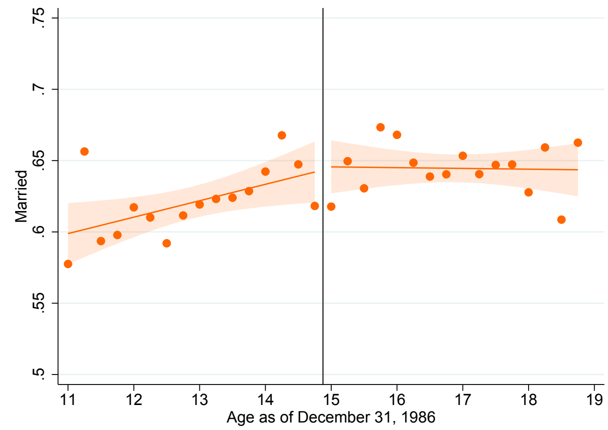


Figure A.16: Effect on Pay Premia in Firms and Industries

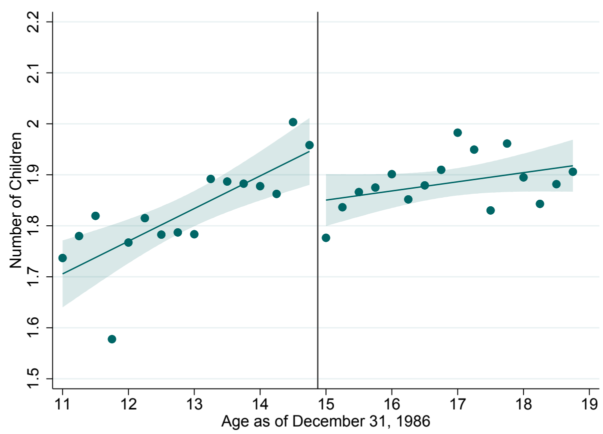
Notes: The figure plots the estimated treatment effects on the firm and industry pay premia at the worker's employer. Pay premia are measured by AKM firm or industry fixed effects estimated in the population of firms and industries in a regression on individual fixed effect, firm or industry fixed effects, and a polynomial in age. The dots/squares correspond to estimates of equation (1) where the outcome is the AKM firm or industry pay premium at the worker's employer, expressed as a percentage change relative to the control-group mean. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence interval based on robust standard errors clustered at the individual level.



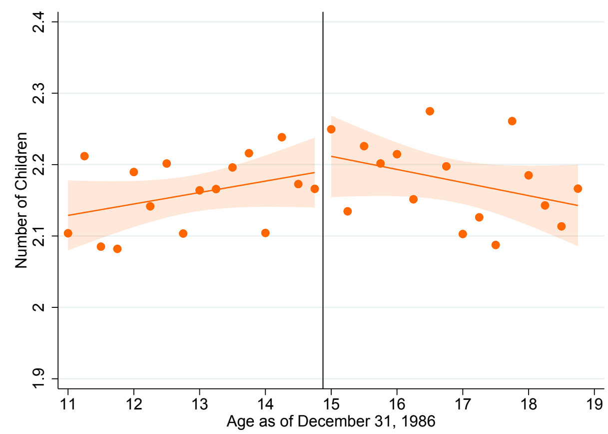
(a) Marriage — Men



(b) Marriage — Women



(c) Fertility — Men



(d) Fertility — Women

Figure A.17: Effect of the Tax-Free Year on Marriage and Fertility

Notes: The figure shows reduced-form effects of the tax-free year on marriage and fertility. Panels (a) and (b) plot the share ever married by age 40, and Panels (c) and (d) plot the number of children by age 40, for men and women respectively, by birth quarter relative to the compulsory schooling cutoff. The vertical line marks the cutoff. Dots are four-month birth-quarter bins. Lines are fitted linear trends estimated separately on each side of the cutoff, with 95% confidence intervals.

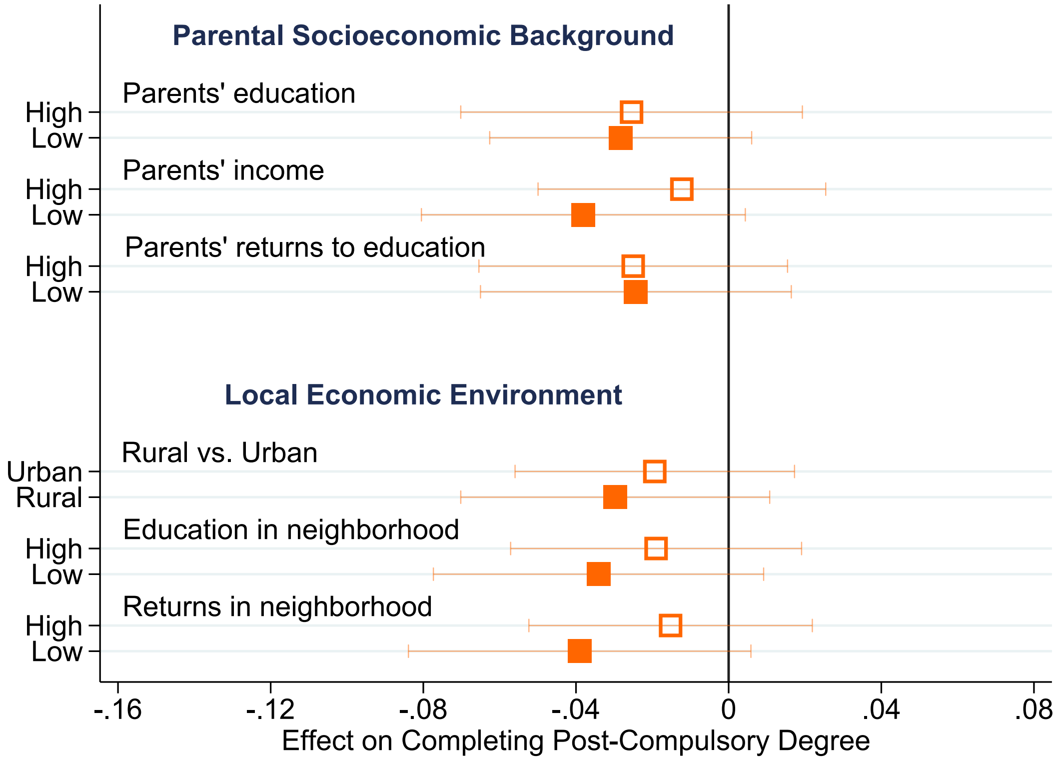
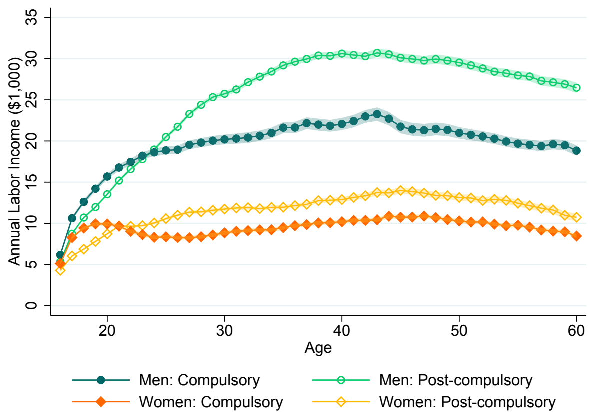
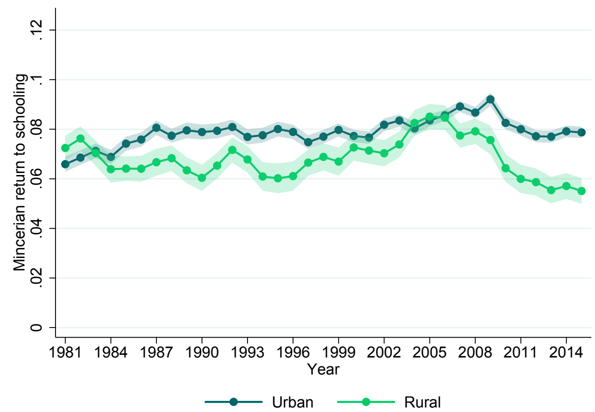


Figure A.18: School Dropout by Parental Background and Neighborhood — Women

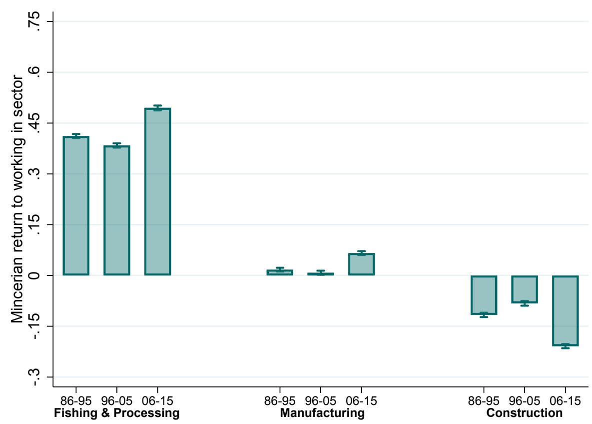
Notes: The figure shows the estimated effects of the tax-free year on school dropout among women, by parental background and neighborhood characteristics. For each characteristic, students are divided into two groups, and I estimate equation (1), interacting group indicators with the discontinuity and age polynomials. For parental education, students are split according to whether at least one parent has completed an academic upper-secondary degree (high school or more). The share of parents with this level of education is 29 percent. For parental income, I rank all individuals in the population by labor income within each birth cohort, gender, and calendar year. I then compute the median income rank of parents at ages 40-60. Each student is assigned the rank of the higher-earning parent, and students are split at the median parental income rank. To split by parental returns to education, I regress earnings on birth cohort indicators, interaction of those indicators and years of schooling, and control for year and location fixed effects. I then distinguish between parents with positive residual (low returns) or negative residual (high returns). The former group is 46 percent of parents. Municipalities are classified as urban or rural based on official municipality codes. 30 percent of parents reside in urban municipalities. For neighborhood education, I calculate, for each municipality, the share of adults (aged 25-64) with an academic upper-secondary degree in the year before the tax-free year (1986), and split students at the median of this distribution. For neighborhood returns to education, I compute, for each municipality, average labor income in 1986 for adult men, separately by education level (academic upper-secondary degree vs. less). Municipal returns are calculated as the ratio of these averages, and students are split at the median return. All regressions control for individual characteristics measured before the reform. Regressions by parental background additionally include municipality fixed effects. Whiskers denote 95% confidence intervals.



(a) Average earnings by education level



(b) Returns to schooling by region



(c) Sector premia

Figure A.19: Returns to Education and Sector Premia

Notes: Panel (a) plots the annual earnings profiles by education for men and women, separately by whether individuals completed post-compulsory schooling or only compulsory education. The sample consists of those aged 16 to 60 and averages are computed for the 5 years before the tax-free year, 1982–1986. Panel (b) plots Mincerian returns to an additional year of schooling estimated separately for urban and rural areas for each year from 1981 to 2015. Regressions are estimated for men aged 25–65 and control for potential experience, experience squared, and experience cubed, where potential experience is defined as age minus years of schooling. Panel (c) plots the estimated return to working in the fishing and fish-processing, manufacturing, and construction sectors relative to other sectors, estimated separately for three periods: 1986–1995, 1996–2005, and 2006–2015. Sector regressions are estimated for men aged 25–65 and control for years of schooling, potential experience, experience squared, experience cubed, and an indicator for rural versus urban employment. All monetary values are in real USD.

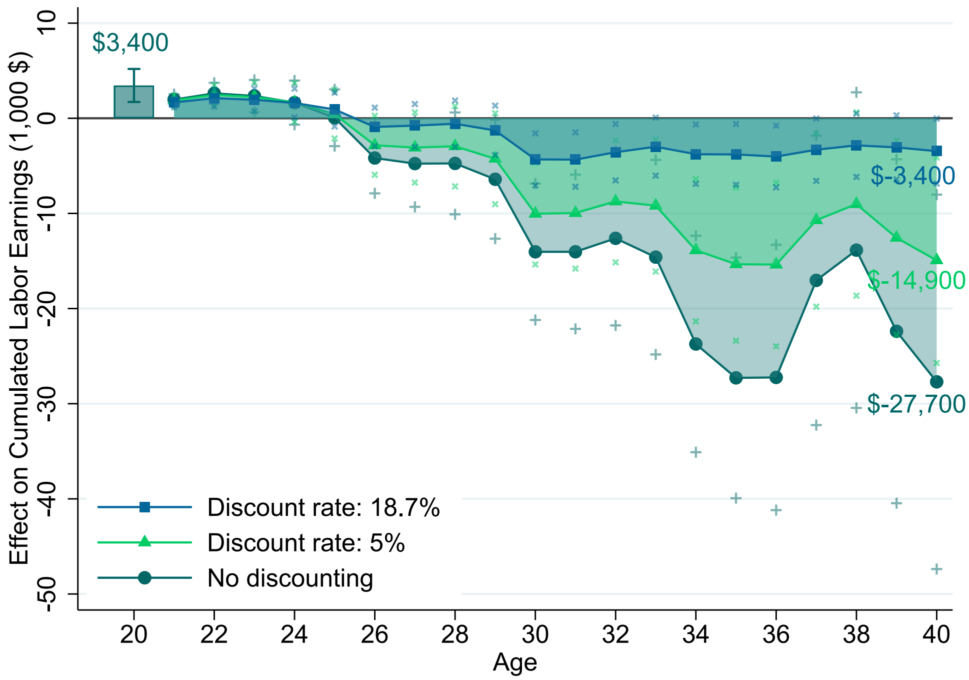


Figure A.20: Cumulative Labor Earnings and Implied Discount Rate

Notes: The figure plots the estimated treatment effect on cumulative labor earnings of men. The bar corresponds to estimates of equation (1) on cumulative labor earnings over upper-secondary school age 16–20. The dots correspond to estimates of the same equation on cumulative labor earnings over time from age 21 to 40. The triangles are present discounted values of estimated effects on accumulated labor earnings, discounted to age 21 using a discount rate of 5%. The squares are present discounted values of estimated effects on accumulated labor earnings, discounted to age 21 using a discount rate that equates the present discounted value of the short-run earnings gain to the long-run earnings loss (see footnote 24). Regressions control for region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The crosses display the 95% confidence interval where robust standard errors are clustered at the individual level.

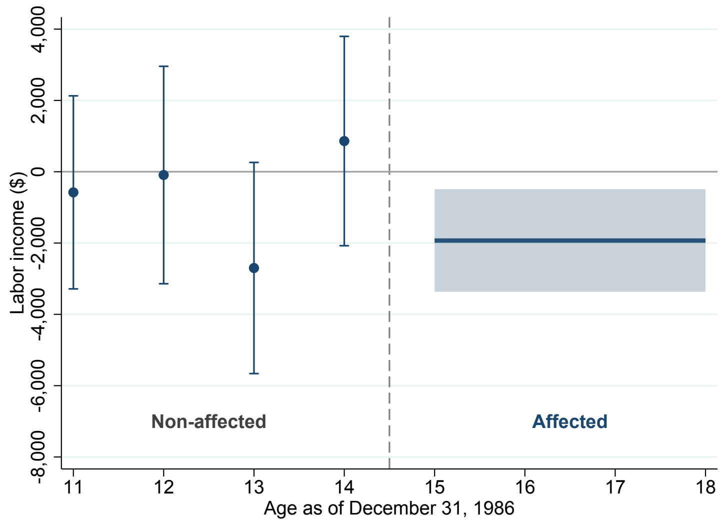


Figure A.21: Evaluation of Cohort Spillover Effects

Notes: The figure plots RD estimates using equation (1) of the effect of the tax-free year on annual labor income at prime age (31-40) for affected and non-affected cohorts. Non-affected cohorts are cohorts that were still at compulsory schooling age at the time of the tax-free year. The estimate for the affected cohorts corresponds to the prime-age earnings estimate reported in Figure 8. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence intervals based on robust standard errors clustered at the individual level.

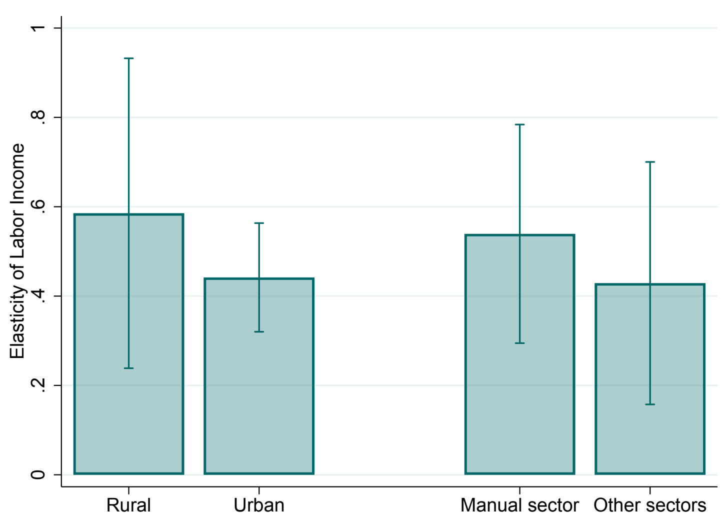


Figure A.22: Labor Supply Responses by Region and Sector

Notes: The figure plots estimates of the elasticity of labor income to the tax-free year separately by region and sector of employment, for workers aged 21 and older. Estimates are obtained using the tax-bracket difference-in-differences design of Sigurdsson (2025), described in Section III, where I estimate their equation (2) separately for workers employed in rural and urban areas and in the manual sector and other sectors, defined based on pre-reform employment. Rural areas are defined according to postal codes. The manual sector comprises workers employed in fishing, fish-processing, manufacturing, and construction. Estimates by subgroups are obtained by interacting group indicators with the log of the net-of-tax rate and the respective instrumental variable. The whiskers display the 95% confidence intervals based on robust standard errors clustered at the tax-bracket by municipality level.

G Supplementary Tables

Table A.3: Effect on Educational Attainment — Robustness

	Post compulsory degree					Years of school				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						A. All				
Treatment effect	-0.031***	-0.024	-0.028**	-0.024*	-0.025**	-0.128***	-0.068	-0.120***	-0.090*	-0.091**
	(0.011)	(0.016)	(0.011)	(0.013)	(0.012)	(0.039)	(0.059)	(0.042)	(0.048)	(0.046)
Outcome mean	0.462	0.462	0.462	0.462	0.462	11.77	11.77	11.77	11.77	11.77
						B. Men				
Treatment effect	-0.049***	-0.047**	-0.047***	-0.045**	-0.046***	-0.193***	-0.111	-0.190***	-0.154**	-0.158**
	(0.015)	(0.022)	(0.016)	(0.018)	(0.017)	(0.053)	(0.081)	(0.057)	(0.065)	(0.062)
Outcome mean	0.420	0.420	0.420	0.420	0.420	11.52	11.52	11.52	11.52	11.52
						C. Women				
Treatment effect	-0.013	-0.001	-0.009	-0.002	-0.002	-0.061	-0.024	-0.046	-0.021	-0.020
	(0.015)	(0.023)	(0.016)	(0.019)	(0.018)	(0.058)	(0.087)	(0.062)	(0.071)	(0.068)
Outcome mean	0.503	0.503	0.503	0.503	0.503	12.00	12.00	12.00	12.00	12.00
Specification	Linear Uniform	Quadratic Uniform	CCT Triangular	CCT Epanechnikov	CCT Uniform	Linear Uniform	Quadratic Uniform	CCT Triangular	CCT Epanechnikov	CCT Uniform

Notes: This table reports the coefficient of the treatment indicator (age above compulsory-schooling age threshold) according to the regression equation (1). The specification in columns (1) and (6) corresponds to my benchmark specification reported in Table 2. "Quadratic" refers to a specification with a second-degree polynomial in age. "CCT" refers to estimates based on the biased correction method of Calonico et al. (2014), using uniform, triangular, or Epanechnikov kernel weights. Each cell represents a single regression estimate for the education outcome specified in the row heading. The estimates are based on local-linear regressions for individuals at age 21 and allow for different coefficients on each side of the cutoff. Outcome mean refers to the averages of the dependent variable for 12 months below the threshold (control group). Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Effect on Labor Market Outcomes — Robustness

	Labor Earnings ($)					Employment				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					A. Men — 16-20					
Treatment effect	838***	1,008***	751***	805***	711***	0.050***	0.050***	0.048***	0.047***	0.043***
	(109)	(159)	(111)	(129)	(122)	(0.007)	(0.010)	(0.007)	(0.008)	(0.008)
Outcome mean	10,487	10,487	10,487	10,487	10,487	0.425	0.425	0.425	0.425	0.425
					B. Women — 16-20					
Treatment effect	96	314***	0	54	5	0.021***	0.013**	0.035***	0.018**	0.014*
	(65)	(93)	(68)	(77)	(73)	(0.007)	(0.009)	(0.006)	(0.008)	(0.007)
Outcome mean	7,342	7,342	7,342	7,342	7,342	0.425	0.425	0.425	0.425	0.425
					C. Men — 36-40					
Treatment effect	-2,147***	-1,673**	-1,891***	-1,560***	-1,621***	-0.001	-0.014**	0.001	-0.003	-0.003
	(451)	(664)	(466)	(537)	(509)	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)
Outcome mean	41,927	41,927	41,927	41,927	41,927	0.863	0.863	0.863	0.863	0.863
					D. Women — 36-40					
Treatment effect	-262	-536	-92	-148	-195	0.005	-0.003	-0.001	-0.008	-0.006
	(279)	(405)	(294)	(341)	(322)	(0.006)	(0.008)	(0.006)	(0.007)	(0.006)
Outcome mean	26,247	26,247	26,247	26,247	26,247	0.796	0.796	0.796	0.796	0.796
Specification	Linear Uniform	Quadratic Uniform	CCT Uniform	CCT Triangular	CCT Epanechnikov	Linear Uniform	Quadratic Uniform	CCT Uniform	CCT Triangular	CCT Epanechnikov

Notes: This table reports the coefficient of the treatment indicator according to the regression equation (1). The specification is either "Benchmark" which refers to my main estimate, or "CCT" which refers to estimates based on the biased correction method of Calonico et al. (2014), using uniform, triangular, or Epanechnikov kernel weights. Each cell represents a single regression estimate for the outcome specified in the row heading. The estimates are based on local-linear regressions and allow for different coefficients on each side of the cutoff. Outcome mean refers to the averages of the dependent variable for 12 months below the threshold (control group). Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Effects of Years of Schooling on Labor Income

	Prime Age		Lifetime	
	Levels	Log	Levels	Log
	(1)	(2)	(3)	(4)
2SLS Estimate	8,126**	0.194**	86,495**	0.170**
	(3,686)	(0.092)	(38,296)	(0.074)
F-statistic	6.2	6.8	9.2	9.3
Outcome mean	41,927		656,154	
Observations	155,710	149,354	15,026	15,011

Notes: This table reports 2SLS estimates of the effect of an additional year of schooling on labor income of men, where the compulsory schooling age threshold indicator serves as an instrumental variable for years of schooling completed. Earnings are measured either as average annual labor earnings at *prime age* (ages 31–40) or as *lifetime* earnings (cumulative from age 21 to 40), and in each case as either levels ($US) or logs. *Outcome mean* refers to the 12-month below-threshold average. The log coefficients give a direct estimate of the percentage return to an additional year of schooling; for level outcomes, dividing the 2SLS estimate by the outcome mean yields the corresponding percentage return. The lower number of observations in log specifications reflects the exclusion of individuals with zero earnings in a given year or interval. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. Robust standard errors, clustered at the individual level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Effect on Consumption Commitments

	Has Child	Has Car	Has Debt	Has High Debt
	(1)	(2)	(3)	(4)
Treatment effect	0.000	0.004	0.011*	0.005
	(0.002)	(0.010)	(0.006)	(0.006)
Outcome mean	0.011	0.348	0.114	0.102
Observations	78,247	78,247	78,247	78,247

Notes: This table reports the coefficient on the treatment indicator from equation (1) in the sample of men. Each column represents a separate regression. Outcomes are binary indicators measured at age 16-20. "Has high debt" indicates individuals with debt of more than two months of minimum full-time income at individual's age. Outcome mean refers to the mean of the dependent variable at the left of the threshold. All regressions include year and region fixed effects and control for pre-reform characteristics measured at age 16. Robust standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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