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 have reduced marriage and fertility. Dropouts predominantly come from low-education—but not low-income—families and neighborhoods, consistent with misperceptions about the returns to education. The findings suggest that temporary economic booms can permanently reduce aggregate human capital accumulation.

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1 Introduction

Economic booms that differentially raise earnings opportunities today versus expected earnings in the future increase the opportunity cost of schooling, potentially reducing enrollment and raising dropout rates (Mincer, 1958; Becker, 1962). Through this channel, temporary booms may lead to permanent declines in educational attainment (e.g., Cameron and Taber, 2004; Atkin, 2016; Charles, Hurst, and Notowidigdo, 2018).

Whether temporary shocks to the opportunity cost of schooling have adverse long-term effects on the earnings and productivity of the labor force remains an open question. Ultimately, these consequences depend on the returns to additional schooling for the marginal students that leave school when opportunity costs are high. If those who drop out possess low academic ability and have limited returns to further education (Willis and Rosen, 1979), or if they hold a comparative advantage in occupations that do not require advanced education (Eckstein and Wolpin, 1999), their lifetime earnings may be largely unaffected by leaving school prematurely. However, if those who drop out during booms would otherwise have completed further education and earned a high return, this implies a 'scarring effect' on human capital and earnings.

In this paper, I answer this question by exploiting a tax reform in Iceland that temporarily increased the take-home pay students could earn in the labor market. In the late 1980s, Iceland transitioned to a pay-as-you-earn tax system from one in which taxes in the current year were based on income in the previous year (Bianchi, Gudmundsson, and Zoega, 2001; Sigurdsson, 2023). During the transition year 1987, no taxes were collected on the income earned that year. The tax-free year offers an ideal natural experiment since it raised the net-of-tax wage that students could earn if they chose to work—the opportunity costs of school attendance—by about 10 percent for a single year, but left the future lifetime returns to education unaffected.

To identify causal effects, I combine this natural experiment with a discontinuity in students' ability to leave school. Specifically, I employ a regression discontinuity design that compares students who, at the time of the tax-free year, had turned 16—making them eligible to leave school—to those just below the age threshold who were required to remain in school that year. This identification strategy relies on two factors that are exogenous to schooling decisions: the timing of the tax reform and students' dates of birth.

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 increase in dropout rates relative to the control group still subject to compulsory schooling. 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.[1]

Students might be tempted to intertemporally substitute schooling for work during a year of unusually high wages. Since students could delay their education or return later, leaving school

[1] At the time, women made up a larger share of high school and university students (Statistics Iceland, 2022b).

to earn tax-free income might not necessarily reduce lifetime educational attainment. However, 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 appears to have been 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 approximately $2,100, or 5.1 percent. This reduced-form effect on earnings can be translated into an effect of lost education on earnings, using the effect of the tax-free year on educational attainment as a first stage. According to this, had the men who dropped out completed their education, their annual prime-age earnings would have roughly doubled. 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 appears to have 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 high school or college. To quantify this general equilibrium effect, I calibrate the model from Card and Lemieux (2001a) using my estimates along with parameter estimates from the literature. This analysis suggests that general equilibrium effects could account for 25 to 40 percent of the estimated earnings loss. Even after accounting for these effects, the results indicate that the direct earnings loss from premature school departure was still substantial.

To interpret my findings, I developed a simple model of schooling choice that highlights the factors influencing the decision to drop out of school. My results contradict the predictions of the standard model of human capital accumulation with heterogeneous returns to education (Willis and Rosen, 1979). According to this framework, students who leave school when opportunity costs increase are negatively selected on ability and returns to schooling, implying no long-term impact on earnings—contrary to the findings. The findings also challenge credit constraints as the primary explanation. Liquidity-constrained students with high returns to education are predicted to leave school only temporarily, using short-term earnings to finance future schooling (Card and Lemieux, 2001b; Emery, Ferrer, and Green, 2012)—a pattern for which I find no support. Instead, the model demonstrates how misperceptions about returns to education or high time discounting can account for the main empirical findings. I then conduct further empirical analysis to distinguish between these potential explanations.

The fact that men left school permanently suggests that they may have been misinformed about the long-run returns to education. I provide corroborating evidence for this interpretation. Previous theoretical and empirical research has shown that children and adolescents form expectations about the returns to education and career opportunities based on information from their parents and by observing individuals in their immediate environment (Manski, 1993; Jensen, 2010). If dropout decisions during the tax-free year were driven by such misperceptions, they would likely be concentrated among students from backgrounds where assessing the true returns to education is especially difficult.

Consistent with this hypothesis, I find that the students who dropped out of school during the tax-free year were predominantly from families with low education levels—but not low income—and from areas where education levels were low, such as rural areas and fishing towns. Inferring returns from the earnings and education of their parents and others in their immediate surroundings, these dropouts may have concluded that completing upper-secondary education would yield little or no increase in future earnings, thereby rationalizing their decision to leave school.[2] However, actual earnings of workers with upper-secondary education were approximately 30-40 percent higher than those of workers with only compulsory education or vocational training. Moreover, young men who dropped out were disproportionately likely to enter the same occupation as their parents, highlighting a direct channel through which low-educated parents may have influenced their children's educational decisions. These findings suggest that the tax holiday reduced intergenerational educational mobility by inducing young men from low-education backgrounds to prematurely exit school due to misperceptions about the true returns to education.

I also empirically assess the alternative explanation that men left school prematurely due to impatience. To do so, I calculate the discount rate required for my findings to be driven solely by time preferences. The results suggest that young men would need to have an annual discount rate of 38 percent, which is substantially higher than those typically reported in the literature, even for adolescents (Kureishi et al., 2021; Frederick et al., 2002; Cohen et al., 2020). Alternatively, under hyperbolic discounting, the findings imply that they would need to have a present-bias factor of 0.23, indicating a degree of present bias far more severe than estimates from prior studies (Imai et al., 2021). These results suggest that, while impatience may have influenced students' decisions,

[2] Jensen (2010) documents that boys in the final year of compulsory schooling in the Dominican Republic perceive returns to schooling to be significantly lower than the actual returns, with 42 percent reporting no expected difference in their own earnings if they completed post-compulsory education. When provided with information about actual returns to schooling, the least poor students were significantly less likely to drop out.

it is unlikely to be the primary explanation.

My findings suggest that the cost of business cycles may be greater than previously recognized. While the macroeconomics literature has extensively examined the welfare costs of business cycles, focusing primarily on the effects of recessions on consumption levels and volatility (e.g., Lucas, 1987, 2003; Krebs, 2007; Krusell et al., 2009), my results suggest that economic booms may also have disruptive effects. To quantify this, I estimate the elasticity of years of schooling to a transitory increase in the wages of young adults. The elasticity estimate suggests that a 10 percent increase in adolescent wages leads to a permanent 1.5 percent loss in years of schooling for the affected cohorts. Using two types of back-of-the-envelope calculations, I estimate that the loss in human capital during the Icelandic tax-free year in the 1980s contributed to a 0.1-0.2 percent reduction in aggregate output in 2020.

These results underscore the potential benefits of policies designed to mitigate school dropout rates during economic booms. Such policies might include raising the compulsory schooling age or providing procyclical stipends to students. For instance, I estimate that increasing the compulsory schooling age from 16 to 17 would have reduced the impact of the tax-free year on school dropout rates by approximately 40 percent.

Lastly, my results have 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. They demonstrate that while low taxes lead to higher labor supply and earnings among the young, thereby expanding the tax base, this comes at a cost. Specifically, it leads to a permanent reduction in educational attainment, which in turn diminishes earnings in adulthood and consequently reduces government tax revenue.

Related Literature This paper contributes to the extensive literature examining how transitory macroeconomic conditions and earnings opportunities influence educational attainment. Previous studies have assessed the impacts of business cycles (e.g., Gustman and Steinmeier, 1981; Kane, 1994; Betts and McFarland, 1995; Card and Lemieux, 2001b; Cameron and Taber, 2004; Johnson, 2013), local labor market conditions (Goldin and Katz, 1997; Shah and Steinberg, 2017; Carrillo, 2020), and expansionary episodes such as natural resource booms (Black, McKinnish, and Sanders, 2005; Emery, Ferrer, and Green, 2012; Cascio and Narayan, 2020), housing booms (Charles, Hurst, and Notowidigdo, 2018), and factory openings (Atkin, 2016).

I extend this literature in two important ways. First, I leverage a natural experiment that provides an exogenous, transitory increase in the opportunity cost of schooling. Unlike economic booms or business cycle fluctuations in employment or wages—which influence opportunity costs but may also affect returns to education—a one-year tax holiday preserves the future lifetime return to education, as tax rates were immediately reverted back.[3]

Second, I follow individuals over their life cycle to examine the long-term effects of school

[3] Extensive research shows that cyclical variation in employment opportunities at graduation has lasting effects on students' labor market outcomes and earnings (Von Wachter, 2020).

dropout on labor income and other enduring outcomes. A central but long-standing question is whether those who leave school prematurely would have benefited from completing further education. This question has proven elusive in most prior studies, with structural estimates often suggesting that dropouts have low ability and low returns to graduation (e.g., Eckstein and Wolpin, 1999; Heckman et al., 2006). In contrast, my findings indicate that dropouts would have earned substantially higher incomes in adulthood had they completed their education, and they likely would have benefited in other ways as well.

More broadly, my findings contribute to the literature on the widening gender gap in education. Men have fallen behind women in educational attainment across nearly all OECD countries (Goldin, Katz, and Kuziemko, 2006). High school completion and college attendance have declined among men, and those who enroll are less likely to graduate (U.S. Department of Education, 2021; Heckman and LaFontaine, 2010). While the causes and consequences of this trend remain debated (Goldin, Katz, and Kuziemko, 2006), my findings on gender differences in dropout responses to short-term earnings gains echo patterns observed during World War II (Goldin, 1998) and the U.S. fracking boom (Cascio and Narayan, 2020).The long-term consequences of these patterns remain unclear, largely due to limited data and direct evidence.[4] My results challenge explanations that attribute men's school dropout to low returns to education. Instead, they suggest that gender differences in exposure to short-term economic opportunities may entrench the gender gap in education.

This paper unfolds 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. The new system, a withholding-based pay-as-you-earn income tax system, replaced the previous system where income taxes were collected with a one-year lag. I illustrate this in the top panel of Figure 1. This system is similar to those in place in most advanced economies. 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. In other words, all labor income earned in 1987 was tax-free.

[4] 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. I worry they'll come to severely regret their choice if they realize the best jobs require a degree they never got.*"

For further details about the tax reform, including its background and timeline, see Sigurdsson (2023).

The tax-free year created a strong incentive for people of all ages to work more or enter the labor market in that year. As summarized in Table 1, the average tax rate paid by workers at upper-secondary schooling age (16-20 years old) before the reform was 8.3% (10.1% for men and 5.6% for women), and the marginal tax rate they faced was 15.8%. This incentive created by the tax-free year was salient. The tax reform and its implications for net-of-tax earnings received extensive media coverage, with newspapers printing headlines highlighting that in 1987 all labor earnings would be tax-free, and the tax authorities sending out advertisements and explanatory flyers about the reform. Consequently, it is likely that most adults, including students, were aware of the reform.

Contemporary media coverage suggests that students took this opportunity to work at higher wages. In December 1986 and January 1987, newspapers reported concerns among school authorities that the tax-free year might encourage students to take time off from school. These fears were amplified in early 1987, as several articles highlighted an unusually high number of high school students not returning after the holiday break. Similarly, reports indicated a decline in university enrollment, with the history of the University of Iceland documenting reductions in enrollment and increased dropout rates during this period (Hálfdanarson et al., 2011).[5]

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 their compulsory education ten years later, in the spring of the year they turn 16. Upon finishing compulsory schooling, students are eligible to enroll in upper secondary education (high school), which is effectively tuition-free. A large fraction of 16-year-olds enroll at the upper secondary level each year, with the enrollment rate rising steadily in the 1990s and the 2000s to above 90% (Blöndal, 2014). While all students have a right to upper secondary education regardless of prior academic performance, their specific options are contingent on their achievements. Admission to upper secondary schools is determined by scores from standardized tests administered at the end of the 10th grade.

Upper secondary education in Iceland is categorized into three types. Grammar schools offer four-year academic programs, attended between ages 16 and 20, culminating in matriculation examinations, which are a prerequisite for university admission. Vocational schools provide both theoretical and practical training, preparing students for skilled industrial trades, specialized occupations, and some unskilled professions. Comprehensive schools combine elements of both, offering academic programs leading to matriculation examinations, akin to those in grammar schools, alongside vocational training similar to that of vocational schools (Ministry of Education,

[5] Appendix Figure A.1 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 exists for university students. Thus, my analysis focuses on delay and dropout from upper-secondary education.

Science and Culture, 2002). The four-year academic programs in grammar and comprehensive schools are commonly referred to as junior colleges or high schools when compared to the U.S. education system. Of the students enrolled in upper secondary education, more than half attended comprehensive schools, roughly one-third attended grammar schools, and approximately 10 percent attended vocational schools.

Since most upper-secondary students pursue four-year programs they typically complete their studies by age 20. However, a significant proportion do not graduate on time, with many dropping out and later re-entering the system, particularly in vocational education (Blöndal et al., 2011). Cohort studies of individuals born in the 1960s, 1970s, and 1980s report an average dropout rate of approximately 30% (Blöndal, 2014; Statistics Iceland, 2008).

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 graduation rates have since risen more rapidly. By 2020, only 0.68 men graduated high school for every woman (Statistics Iceland, 2022b).[6] This growing education gap reflects disparities in both enrollment and dropout rates. Male students consistently exhibit higher dropout rates in post-compulsory education, a trend that has persisted for decades. In academic programs, for instance, the dropout rate among men in 2020 was 21.1%, compared to 12.9% for women (Statistics Iceland, 2022a).

2.3 Youth Employment

In Iceland, almost all students work alongside their schooling, either on weekends or weekdays, outside school hours, or during holidays. More than 90% of Icelandic teenagers, aged 13-17, hold summer jobs, and between one-fourth and one-third work during other holidays and during the school term (Rafnsdóttir, 1999). While summer jobs are somewhat less common in other Nordic countries, the overall share of students who work while attending school or during the holidays is similar. Teenagers work in a variety of jobs, but the most common jobs include childcare, cleaning, shop and supermarket assistance, and manufacturing, such as fish processing.

2.4 Data and Sample

The data I use come from administrative records covering the Icelandic population and consists of four main datasets, all of which are easily linked via a unique personal identifier. The first is the data on educational attainment obtained from Statistics Iceland's Education Register and Degree Register, which include information on educational attainment for the Icelandic population. This is yearly data, dating back to 1981, but contains information on completed education much further back (e.g., university degrees since 1912). These administrative records include information on completed education and in most cases information on education that is started but not completed is not systematically recorded, apart from cases when a final degree consists of more

[6] 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.

than one level and each level is registered.[7] I translate educational attainment into years of schooling using information on the number of years it typically takes to achieve a given level. Further details of this dataset are provided in Appendix A. The second primary data source is a panel of individual tax returns, extending back to 1981. Individual tax returns contain information on all income, including labor income, financial income, pensions, social security, transfer payments, and other sources of income, as well as detailed data on assets and liabilities. The third dataset is employer-employee data constructed from digitized pay slips, containing information on employment, earnings, and working time, as well as occupation and industry. For further information on this dataset, see Sigurdsson (2023). The fourth dataset is the National Register from which I obtain a family identifier allowing me to connect parents and children that live together.

The sample used in my consists of all individuals in the birth cohorts around the compulsory schooling age during the 1987 tax-free year, who completed compulsory education in Iceland, and who were alive and lived in Iceland at the age of 30. The first restriction ensures a sample of individuals who lived in Iceland at the time they were eligible for upper-secondary school studies, and are therefore observed in administrative records such as the education register. The last restriction ensures a sample of individuals who are alive and live in Iceland while of working age and are therefore observed in administrative records, such as the tax register.

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.

2.5 Empirical Strategy

My empirical strategy uses two features, as I illustrate in Figure 1. One, the tax-free year, which was a single-year event that temporarily raised the net-of-tax wages of workers, irrespective of education. Therefore, for students, this provides an exogenous transitory increase in the opportunity costs of schooling. Two, as explained above, in Iceland education is compulsory up to the year students turn 16. This means that those for whom school is no longer compulsory at the time of the tax-free year will have the option of delaying or dropping out of school to work. Conversely, those still at compulsory schooling age do not have the same option and are compelled to stay in school during the tax-free year.

I use a regression discontinuity (RD) design that compares individuals around the compulsory schooling age threshold in the tax-free year. This is a fuzzy RD because the likelihood of school dropout is expected to be higher when crossing the threshold. Specifically, I estimate the following equation:

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

[7] For example, for some vocational education the academic part is registered as one level and the completed vocational degree as another level.

where y_{ia} measures the outcome (e.g., educational attainment or earnings) of individual i. D_i is an indicator function:

$$D_i = \mathbb{1}(m_i \leqslant 0)$$

and the 'assignment' variable m_i is the date of birth in months normalized to zero in December 1971. That is, D_i takes the value of 1 for individuals who had turned 15 years old by December 31, 1986 and therefore had completed or would complete compulsory schooling during the tax-free year, but zero if still of compulsory schooling age. $f^k(\cdot)$ is a k^{th}-order polynomial in the assignment variable that is interacted with D_i such that cohort trends can have different slopes on either side of the threshold. This function accounts for any underlying trend or relationship between the birth cohort and the outcome of interest. Coefficient β identifies the causal effect of the tax-free year from a discrete change in the outcome at the age threshold, under the assumption that the error term ε_i does not change discontinuously at the age threshold. The equation includes a vector of individual-specific characteristics, X_i. These are pre-reform characteristics measured at age 16 including the location of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, as well as an indicator for receiving disability benefits. I present results with and without controls. The inclusion of controls can potentially reduce residual variation in outcomes and improve the precision of the estimates. However, the inclusion of controls should not affect the estimates of β because they should be uncorrelated with being born on one side of the threshold or the other.

To estimate equation (1), I use a local polynomial approximation approach (Lee and Lemieux, 2010), estimating the equation over a narrow set of data. My main estimates are obtained using a linear polynomial (i.e., local-linear approach) and a bandwidth of four years (i.e., 48 birth cohort months). The choice of polynomial degree is motivated by the trends revealed in the nonparametric analysis of the data in Section 3. The bandwidth choice implies that, above the threshold, the sample includes all individuals of normal upper secondary school age and does not include those who would already have completed upper secondary school. I evaluate and document the robustness of the results to varying the chosen bandwidth, polynomial degree, and weights, and to correcting for potential bias using the procedure of Calonico, Cattaneo, and Titiunik (2014). Following Kolesár and Rothe (2018), I present Huber-White heteroskedasticity-robust standard error standard errors, except when presenting time-varying treatment effects, where I present robust standard errors clustered at the individual level.

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 2 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.[8] 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 3 contains plots, separated by gender, of the age-specific RD estimates of the effect on post-compulsory education and years of schooling.[9] 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 in Appendix Figure A.5. 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 evi-

[8] Appendix Figure A.2 plots education by date of birth separately for men and women.

[9] Appendix Figure A.3 plots age-specific RD estimates of the effect on post-compulsory education and years of schooling for men and women jointly.

dence that students complete vocational education in their late 20s, this effect is relatively small and not precisely estimated.[10] 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.[11]

Panels (c) and (d) in Figure 3 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 increased dropout among men, measured by a reduction in post-compulsory education of 5 percentage points or 8 percent when compared to the below-threshold average. As displayed in Figure 3, there is a persistent reduction in men's pre-university years of schooling of 0.145 years or about 1.5 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 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.6 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.

[10] As I will document in Figure 4, male dropouts disproportionately entered manufacturing and construction jobs. This pattern in Figure 3a 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.

[11] 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. Appendix Figure A.4 plots estimated effects on total years of schooling completed, including years of university education.

Appendix Table A.5 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.7 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.8. 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 4 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.12 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.[12]

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 5 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. Appendix Figure A.9 displays a similar discrete jump in employment.[13] For women, there is a small increase in earnings at the threshold, which, in junction 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).

[12] 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.

[13] Employment is defined as earning at least $10,000 per year. As documented in Section 2, most Icelandic students work during summers, and many work alongside school, implying that in normal years most students earn some income. Defining employment in this way roughly corresponds to defining employment as having minimum wage earnings for a low-skilled worker. In Appendix Figure A.10 I explore the robustness of the choice of the earnings threshold by varying the income threshold. The figures show that the estimated employment effect is positive and statistically significant above $6,000, and the point estimates are stable at $9,000 and above.

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 6 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 5, 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. 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 6, 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 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 6, 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 educational attainment, 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 under a set of assumptions, some of which may not hold in this setting. In particular, the tax-free year may have influenced the returns to education by reducing the supply of educated workers, potentially violating the exclusion restriction. Bearing this caveat in mind, I proceed with the IV estimation while investigating such general equilibrium effects in the analysis that follows.

I report the two-stage least squares (2SLS) regression estimates for men in Appendix Table A.7. The results indicate that had the affected young men completed post-compulsory education instead of dropping out, their annual prime-age earnings would have increased by $42,000, effectively doubling their earnings relative to the control group. Similarly, completing post-compulsory education would have increased their lifetime earnings by approximately 70 percent, measured until age 40. These estimates imply a return to an additional year of schooling of 18 percent when measured at prime age and 12 percent when measured over the lifetime. The magnitude of these earnings losses suggests that young men who left school prematurely suffered substantial long-term consequences. While large, these estimates are in line with previous research. Exploiting changes in compulsory schooling laws, Oreopoulos (2007a) estimates that staying in school an additional year instead of dropping out at age 15 increases lifetime income by roughly 17 percent.[14]

Robustness I assess the sensitivity of my results through several robustness checks. Appendix Figure A.11 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.6 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.

General Equilibrium Effects A key concern is that the estimated negative effect on earnings may not solely reflect lower wages among school dropouts but could also capture an increase in the returns to education for individuals who complete post-compulsory schooling. Specifically,

[14] Earlier estimates of the earnings gains from additional compulsory schooling range from 10 to 14 percent (see, e.g., Acemoglu and Angrist, 2000; Harmon and Walker, 1995; Oreopoulos, 2006).

the decline in degree completion within the affected birth cohorts reduces the supply of educated men, potentially widening the wage gap between high school graduates and dropouts. This is an important issue, as such a general equilibrium effect violate the stable unit treatment value (SUTVA) assumption and complicate the interpretation of the labor market consequences of dropping out.

The magnitude of these general equilibrium effects 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 (see Appendix B for a full description of the model). In this 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. 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 equation:

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

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'.[15]

Equation (2) provides a framework for calibrating the magnitude of general equilibrium effects. The left-hand side represents changes in the relative wages of education groups within a cohort, while the right-hand side captures the change in the educational composition of workers, scaled by the across-cohort elasticity of substitution among similarly educated workers. I estimate that the tax-free year led to an 8 percent increase in the dropout rate and a 5.1 percent decline in earnings for affected cohorts.

To quantify the potential magnitude of general equilibrium effects, I draw on existing estimates of the elasticity of substitution across birth cohorts. Using data from the U.S., U.K., and Canada spanning 1959-1996, Card and Lemieux (2001a) estimate this elasticity (σ_A) to range from 3.8 to 6.2, with similar values across countries. Acemoglu and Autor (2011) estimate a comparable elasticity of 3.7 using U.S. data from 1963-2008. Applying these parameter estimates suggests that general equilibrium effects could account for 25-42 percent of the observed earnings decline. Nevertheless, even after accounting for these effects, the direct negative impact of school dropout on earnings remains substantial. Moreover, these estimates likely represent an upper bound for general equilibrium effects, as they are based on five-year birth cohorts rather than adjacent cohorts, where the elasticity of substitution may be smaller.

How large would σ_A need to be for general equilibrium effects to fully explain the observed

[15]See Appendix B for a derivation of equation (2) and underlying assumptions.

earnings effect? A simple calculation based on equation (2) suggests that σ_A would need to be as low as 1.57. This implies that similarly educated workers in adjacent birth cohorts would have to be poor substitutes. To put this into context, this elasticity can be compared to estimates in the literature on the elasticity of substitution between education groups (Katz and Murphy, 1992; Acemoglu and Autor, 2011), which range from 1.4 to 2.9 for high-school versus college-educated workers. Thus, for general equilibrium effects to entirely account for the earnings effect, similarly educated workers from adjacent cohorts would need to be as poor substitutes as workers with vastly different education levels.

4.2 Career Progression

How did dropping out of school and entering manual jobs shape the career trajectories of these young men? As documented in the literature, the majority of lifetime earnings growth for young men occurs through career advancement during the first ten years in the labor market (Topel and Ward, 1992).

To measure career progression, I first rank all workers in the population within firms or industries by their residualized—or 'Mincerized'—earnings, residualizing earnings from the effect of gender and age. Figure A.13 presents the estimated effect on career progression over the life cycle. The figure documents that while young men in the treated cohorts start off on relatively equal footing compared to the control group. However, at prime age, the earnings rank of workers in the treated group is roughly 4 percent below the control group. These results suggest that earnings losses of dropouts reflect them experiencing fewer promotions and slower career advancement than if they had stayed in school.[16]

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 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 statistical, 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.

[16]One alternative is that dropouts entered jobs that were worse in terms of pay compared to jobs they otherwise would have entered. I investigate this in Appendix Figure A.13, where I estimate the effect AKM (Abowd, Kramarz, and Margolis, 1999) pay premiums of firms and industries. The results suggest that dropouts seem to have entered the labor market through firms and industries offering relatively high starting pay, but show no difference in pay premiums at prime age.

4.4 Marriage and Fertility

In addition to its negative impact on careers, dropping out of school may also affect men's family formation in adulthood. There are two main reasons for this. First, economic theories suggest that men's economic prospects influence marriage decisions. In the classic Becker (1973) framework, marriage gains arise from spousal earnings differences and specialization, so a decline in the economic stature of young men reduces women's gains from marriage. Relatedly, a decline in the earnings of less-educated men reduces the pool of economically secure—"marriageable"—men (Wilson and Neckerman, 1986; Wilson, 1987). Second, extensive research documents educational assortative mating (Mare, 1991; Pencavel, 1998) and the role of schools as marriage markets (Blossfeld, 2009; Nielsen and Svarer, 2009; Kirkebøen, Leuven, and Mogstad, 2021).

To explore this, I estimate the effects on marriage and fertility in the affected cohorts. The results, shown in Figure 8, indicate that male dropouts were significantly less likely to marry or have children. Specifically, by age 40, affected men were about 6 percent less likely to be married.[17] Affected men were also 4.8 percent less likely to have children by age 40 and, on average, had 0.10 fewer children, a 5 percent decline. 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 or increased import competition from China, led to reductions in marriage and fertility (Black et al., 2003; Autor et al., 2019). Similarly, Kearney and Wilson (2018) found that the U.S. fracking boom increased fertility among low-educated men, and Schaller (2016) found that improvements in men's labor market conditions and earnings were associated with higher fertility rates.

Could these findings be explained by income effects? Specifically, if marriage and children are "normal goods," a fall in income may lead to lower marriage and fertility rates. To investigate this, I compare my results to prior studies on the effects of windfall income, such as lottery winnings, on marriage and fertility. As discussed in Appendix C, the estimates from this study align with prior research, though they are generally larger in magnitude.

5 Model of School Dropout

To aid in interpreting the empirical results, I present a simple model illustrating how changes in the opportunity cost of schooling influence school dropout decisions. Building on the canonical model of human capital investment (Mincer, 1958; Becker, 1962; Willis and Rosen, 1979), the model examines how individuals trade-off forgoing earnings for schooling today and the future returns to education, while accounting for individual heterogeneity.

There is a unit mass of infinitely-lived individuals who have completed compulsory schooling.

[17] Between 1991 and 2000, when the treated group was in their 20s and early 30s, the average age at first marriage was 31.2 years for men and 29 years for women. During the same period, the average age of first-time fathers was 27.6 years and 24.9 years for mothers, with a fertility rate of 2.1 children.

In period $t = 0$, individuals decide whether to continue their education or exit school to enter the labor market. From period $t = 1$ onward, all individuals work, with those who pursued additional schooling earning a return on their educational investment. Individuals can neither save nor borrow and derive utility from consuming their disposable income every period. Consequently, they choose their level of schooling to maximize the present discounted value of lifetime earnings, net of costs.

Individuals without additional education earn a fixed income in each period, normalized to unity. In contrast, educated individuals earn a return ρ proportional to their economic ability, θ. Ability is assumed to be uniformly distributed, $\theta \sim U[0,1]$. When deciding whether to pursue further schooling, individuals form expectations about returns to education based on observations of those around them (Manski, 1993). Specifically, I assume that $\mathbb{E}(\rho) = (1-\varepsilon)\rho$, where $\varepsilon > 0$ represents misperceptions about the returns to education due to imperfect information, either arising from low education of parents or low level of education in their neighborhood (Jensen, 2010; Bell et al., 2019).

Income is subject to a tax rate, τ, which, for simplicity, is assumed to be zero in all periods $t > 0$. The direct cost of schooling, κ, is expressed in monetary terms and encompasses all financial and non-financial costs. Individuals discount 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 in schooling decisions (Phelps and Pollak, 1968; Laibson, 1997).

The resulting income streams, conditional on the education choice, are as follows:

$$\text{Continue school:} \qquad -\kappa + (1 + (1-\varepsilon)\rho\theta)\sum_{t=1}^{\infty}\beta\delta^t \tag{3}$$

$$\text{Leave school to work:} \qquad (1-\tau) + \sum_{t=1}^{\infty}\beta\delta^t \tag{4}$$

Individuals choose to attend school if the marginal benefit exceeds the marginal cost. The marginal cost consists of the opportunity cost of school—the net-of-tax earnings students must give up to attend school instead of working—and the direct cost of schooling. This provides the following condition for continuing in school:

$$\underbrace{\frac{\beta\delta}{1-\delta}(1-\varepsilon)\rho\theta}_{\text{Marginal benefit}} \geqslant \underbrace{(1-\tau)}_{\text{Opportunity cost}} + \underbrace{\kappa}_{\text{Direct cost}} \tag{5}$$

Equation (5) captures four dimensions of student heterogeneity emphasized in the literature. First, students vary in their economic ability, reflecting the sorting of individuals based on skills and returns to education (Willis and Rosen, 1979). Second, students differ in their perceived returns to education, capturing the role of environmental factors in shaping the information on which education decisions are based (Manski, 1993). Third, students differ in their direct cost of schooling, capturing both the financial costs of schooling, potentially shaped by liquidity constraints, and in non-pecuniary costs (Becker, 1967). Finally, students differ in their discounting of the future, reflecting variations in present bias and time preferences (Levitt et al., 2016).

Although these various dimensions of heterogeneity help explain differences in educational attainment, they generate distinct predictions about who drops out when opportunity costs rise and the consequences of early school departure. Based on these predictions and the empirical evidence presented in earlier sections, some mechanisms can already be ruled out. To illustrate this, I use equation (5) to determine the share of individuals attending school based on their characteristics and the model parameters. Assuming for now that all students are exponential discounters ($\beta = 1$), I define $\bar{\theta}$ as the ability of the marginal individual who attends school under perfect information about returns, and $\bar{\theta}^M$ as the ability of the marginal individual under misperception:

$$\bar{\theta} = \frac{((1-\tau)+\kappa)(1-\delta)}{\delta\rho} \leqslant \frac{((1-\tau)+\kappa)(1-\delta)}{\delta\rho(1-\varepsilon)} = \bar{\theta}^M \tag{6}$$

Figure 9, panel A, illustrates school attendance shares under the two informational assumptions. Taking the derivative of these thresholds with respect to the tax rate reveals how school enrollment shares change in response to tax variations:

$$\frac{\partial\bar{\theta}}{\partial\tau} = -\frac{(1-\delta)}{\delta\rho}, \qquad \frac{\partial\bar{\theta}^M}{\partial\tau} = -\frac{(1-\delta)}{\delta\rho(1-\varepsilon)} \tag{7}$$

Figure 9, panel B, illustrates the share of students who drop out when the tax rate is reduced. The tax reform leads to the same proportion of students leaving school, regardless of their information about returns. However, its effect on lifetime earnings differs. Under perfect information, dropouts are negatively selected on ability and optimally trade off a one-period increase in take-home pay against low future returns to education, resulting in no loss in lifetime earnings—a prediction that contradicts the empirical findings. In contrast, under misperception, dropouts are not perfectly negatively selected on ability and forgo substantial financial gains by leaving school prematurely, aligning with the main empirical results.

What is the effect of present bias? From equation (5), one can see that the effect of misperception is equivalent to the effect of present bias when $\beta = 1-\varepsilon$. Both mechanisms can lead to similar outcomes in terms of school dropout and earnings loss. However, a key distinction is that while myopia can be considered a static individual trait, misperception is shaped by available information and may therefore be more dynamic. One possibility is that the tax reform prompted students to seek information, but the accuracy of the information they received varied depending on their parental background and environment. This variation is captured by the following derivative:

$$\frac{\partial\bar{\theta}^M}{\partial\tau\partial\varepsilon} = -\frac{(1-\delta)}{\delta\rho(1-\varepsilon)^2} \tag{8}$$

This generates two effects, as shown in Figure 9, panel C. First, a larger share of students drop out in response to the tax reform. Second, the average ability of dropouts increases, amplifying lifetime earnings losses.

A final point to note, as seen in (7), is that the share of students who drop out is unaffected by heterogeneity in schooling costs. In a more realistic model with credit constraints, effective schooling costs would be influenced by access to credit. However, in that case, liquidity-constrained students with positive returns to education would intertemporally substitute schooling for work during periods of high take-home wages before returning back to school (Card and Lemieux, 2001b; Emery et al., 2012).[18] Contrary to this prediction, the main results indicate that students who leave school during the tax-free year do so permanently.

This analysis highlights two key mechanisms that can explain the main findings: misperceptions about returns to education and impatience. In the next section, I conduct further empirical analysis to examine these mechanisms and assess their validity.

6 Interpreting the Empirical Results

6.1 Misperceived Returns to Education

Schooling decisions depend on perceived returns. As illustrated by the model in the previous section, some students may have dropped out during the tax-free year because they underestimated the true returns to remaining enrolled. Prior research indicates that children and adolescents often form expectations about education based on information from parents, social networks, and peers (Manski, 1993). Therefore, if misperceptions drove dropout decisions, we would expect dropouts to disproportionately come from low-education environments, where accurately assessing returns to schooling is more challenging.

To investigate this mechanism, I perform a heterogeneity analysis based on parental and neighborhood characteristics. Specifically, I divide students into groups defined by these characteristics and estimate equation (1), interacting group indicators with the discontinuity and age polynomials. When analyzing parental characteristics, I control for municipality fixed effects to account for location-specific factors. The results, presented in Figure 10, reveal several notable patterns.[19]

First, splitting students by whether their parents—mother or father—completed academic upper-secondary education (junior college degree or higher) reveals that the increase in dropout was entirely driven by students from families with low education levels (only compulsory education or vocational training). However, dividing students by parental income shows that dropouts tended to come from families with above-median income. Investigating the interaction between these characteristics by forming four groups based on parental education and income, I find that dropouts predominantly belonged to families with low educational attainment but income above the median.

Turning to neighborhood characteristics, I first classify students by the share of adults in their municipality who completed academic upper-secondary education. The results indicate that

[18] Prior research has found limited evidence that direct costs or credit constraints are the primary drivers of school dropout decisions (Stinebrickner and Stinebrickner, 2003; Cameron and Taber, 2004; Stinebrickner and Stinebrickner, 2008).

[19] Appendix Figure A.14 provides analogous estimates for women.

dropouts were concentrated in municipalities with lower education levels. Next, I split students according to local returns to education, measured by the relative income of adults with upper-secondary education or more compared to those with lower education levels. The estimates suggest that dropout rates were higher in areas with lower returns to education. Lastly, dividing students by rural versus urban residence reveals that most dropouts were mostly from rural areas.[20]

These findings suggest that if students inferred returns to education from observing the relationship between their parents' income and education, or from other workers in their neighborhood, they may have mistakenly concluded that the returns to post-compulsory education were minimal or nonexistent. However, the average annual earnings of workers with upper-secondary education were substantially higher than those of workers with only compulsory education or vocational training—by approximately 40 percent in urban areas and 30 percent in rural areas (Appendix Figure A.15). Furthermore, these returns remained similar, though slightly lower in rural areas, at the end of our sample period when these individuals were in their prime working years.

These results are consistent with prior findings in the literature. Studies have shown that in contexts characterized by low educational attainment—such as rural areas or developing countries—perceived returns to education tend to be significantly lower than actual returns, increasing the likelihood of premature school dropout (Jensen, 2010; Nguyen, 2008).[21] For example, Jensen (2010) documents that boys in their final year of compulsory schooling in the Dominican Republic substantially underestimate returns to education, with 42 percent expecting no earnings increase from completing post-compulsory education. Providing accurate information about actual returns significantly reduced dropout rates. The largest effects appeared among the least poor students, possibly because liquidity constraints continued to limit educational investments among the poorest students.

More generally, prior work has documented how children and adolescents make decisions about education and careers based on information from their parents or their environment. For instance, children often follow their parents' educational and career paths, including their choice of college majors (Dahl et al., 2024) and occupations (Blau and Duncan, 1967; Laband and Lentz, 1985; Long and Ferrie, 2013), and they are more likely to enter a profession if it is prevalent in their local environment (Bell et al., 2019).

To further assess the direct influence of parental background on children's choices, I examine

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

[21] Attanasio and Kaufmann (2014) document that in Mexican households, mothers expect significantly higher returns to education than their children. Boys' own expectations predict their schooling decisions, whereas for girls, it is their mothers' expectations—not their own—that matter. Bursztyn and Coffman (2012) conduct an experiment in Brazil and find that parents are willing to pay for improved information and better monitoring of their children's school attendance. These findings suggest that information asymmetries between parents and children, as well as intergenerational conflicts, may contribute to children's underinvestment in education.

the types of jobs students took upon leaving school. Specifically, Appendix Figure A.17 investigates the extent to which children followed their parents' occupational paths. The results provide supporting evidence: although estimates are somewhat imprecise, young men were about 2 percentage points more likely to enter the same broad occupation as their parents, relative to an average tendency of approximately 20 percent. In contrast, no similar pattern emerges among young women.

Together with the evidence in Figure 4, these findings indicate that young men were particularly likely to follow their fathers into fishing, manufacturing, and construction jobs. While these jobs offered relatively high starting wages—especially compared to positions typically available to young women—they likely provided limited career advancement opportunities relative to occupations requiring higher educational attainment. As shown in Appendix Figure A.16, the earnings profile of male dropouts started at a comparatively high level but remained relatively flat over time. In contrast, earnings among more educated men, though initially lower, increased substantially with age.

6.2 High Discount Rates

An alternative explanation for why young men left school permanently is myopia—heavily discounting or disregarding future returns. In this case, marginal students who dropped out during the tax-free year prioritized immediate earnings over the higher wages they could have earned later. As outlined in the model from the previous section, this behavior suggests that these students discount the future using a quasi-hyperbolic discount function, applying a rate of $\beta < 1$ to all future earnings (Phelps and Pollak, 1968; Laibson, 1997), or that their exponential discount factor was particularly high.

To evaluate this mechanism, I calculate the value of β required to equate the immediate earnings gain from dropping out during the tax-free year with the present discounted value of subsequent earnings losses. For this calculation, I assume an exponential discount rate of 5 percent, consistent with Iceland's average five-year indexed interest rate of 4.94 percent in 1986. Under this rate, the present discounted value of the cumulative earnings effect is approximately $14,900 (see Appendix Figure A.18). Using the estimated cumulative gain from dropping out during upper-secondary school age ($3,400), this calculation implies that $\beta = 0.23$.[22]

Such a low value of β suggests that students exhibited extreme present bias—possibly to an unrealistic degree. Indeed, a meta-analysis of experimental studies estimating β finds average values between 0.95 and 0.97, with no studies reporting β below 0.5 (Imai et al., 2021).

An alternative approach to assessing whether high future discounting can explain the main findings is to calculate the exponential discount rate that equates the earnings gain from dropping out of school during the tax-free year with the present discounted value of the subsequent earnings

[22] This estimate likely represents a lower bound, as it assumes no additional earnings gains for school stayers beyond age 40. However, prior research suggests that returns to education may continue to grow beyond this age (e.g., Bhuller et al., 2017).

losses. Specifically, I compute the discount rate δ that solves the following equation:

$$C_{16-20} - \sum_{a=21}^{40} \frac{C_a}{(1+\delta)^{a-20}} = 0 \tag{9}$$

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. The discount rate that satisfies this equation is 18.7 percent (see Appendix Figure A.18). This rate reflects the constant, or average, discount rate over the life cycle.

Prior research finds that children and adolescents exhibit high discount rates (Gruber, 2001; Bettinger and Slonim, 2007; Steinberg et al., 2009; Castillo et al., 2011; Levitt et al., 2016), which tend to decline with age (Kureishi et al., 2021; Tanaka et al., 2010; Warner and Pleeter, 2001; Green et al., 1994). For instance, Kureishi et al. (2021) estimate that discount rates are highest among the youngest individuals and decrease almost linearly by 0.19 percentage points per year. Applying their estimates suggests that individuals aged 16-20, at upper-secondary school age, have an annual discount rate of approximately 38 percent—nearly double the estimated rate for individuals in their early 20s (e.g., Kureishi et al., 2021) and significantly higher than those typically reported in the broader literature (Frederick et al., 2002; Cohen et al., 2020).

In Appendix Figure A.19, I provide additional evidence suggesting that dropouts did not exhibit other clear signs of myopic behavior. Specifically, I find no differences in dropout rates based on whether students had savings or owned a bank account—characteristics previously linked to impatience (Cadena and Keys, 2015). Moreover, in adulthood, dropout rates do not differ based on private pension savings or whether individuals hold accident insurance.

Taken together, these results suggest that while myopia may have played a role, it is unlikely to have been the primary driver of students' decisions to drop out during the tax-free year.

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 first 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 that the average tax rate for employed 16-20-year-olds was 8.3% before the tax-free year. Table 2 reports that the tax-free year increased dropout rates by 2.7 percentage points, or 6.1 percent, in the affected cohorts. This implies that the elasticity of post-compulsory education (i.e., the dropout elasticity) is $\frac{-0.061}{-\log(1-0.083)} = -0.70$. Measured in years of schooling, the tax-free year reduced educational attainment by 0.192 years, or 1.3 percent. The corresponding elasticity of educational attainment is $\frac{-0.013}{-\log(1-0.083)} = -0.15$.

What do these estimates imply for the cost 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. For instance, Charles, Hurst, and Notowidigdo (2018) estimate that the 2000-2006 U.S. housing boom increased wages for 18-25-year-olds by 6 percent for a one standard deviation rise in housing demand. More broadly, the macroeconomics literature has documented that wage fluctuations over the business cycle are much larger for young workers than for middle- and older-aged workers. 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.

Over the business cycle, the effects of booms on educational attainment may be offset by those of recessions. However, recessions influence educational attainment through two opposing channels. On one hand, they reduce the opportunity cost of schooling, encouraging students to stay in school longer during economic downturns, thereby mitigating human capital losses experienced during booms. On the other hand, if students and their families face binding borrowing constraints, they may leave school earlier than they otherwise would (Dellas and Sakellaris, 2003). Determining which force dominates is an empirical question, and the evidence is mixed.

While some studies find that higher local unemployment rates increase school attendance (Betts and McFarland, 1995), several report small or insignificant effects (e.g., Card and Lemieux, 2001b; Raaum and Røed, 2006; Christian, 2007; Kahn, 2010; Oreopoulos et al., 2012; Johnson, 2013). Other research identifies a negative impact of recessions on educational attainment through mechanisms such as parental layoffs (e.g., Oreopoulos et al., 2008; Hilger, 2016; Stuart, 2022).

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. I use the estimated effect on labor earnings from Table 3, which I assume remains constant through men's 40s and into their 50s. To translate this into an effect on GDP, I assume that GDP declines proportionally to the share of labor earnings in GDP. Weighting the impact on the cohort of men at upper-secondary school age in 1987—the treated group—by their share of the working-age population (16-67), I estimate that school dropout during the tax-free year reduced GDP in 2020 by 0.09%.

The second approach applies the growth model estimates from Hanushek and Woessmann (2012) to quantify the impact of losing one year of schooling on GDP per capita growth. I scale this estimate by my working-age-population-weighted effect on years of schooling (Table 2) and use it to adjust the actual annual GDP growth rate from 1987 to 2020. Based on this approach, I estimate that GDP in 2020 was reduced by 0.22%.

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 compulsory schooling 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 high school 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 compulsory schooling age had been 17 instead of 16. I estimate that school dropout during the tax-free year would have been reduced by about 40 percent.[23]

An alternative and more direct approach would be a procyclical education policy aimed at keeping students in school when opportunity costs are high. For instance, policymakers seeking to counteract dropout could increase education subsidies—such as student grants or low-interest loans—during economic booms.

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 a caveat to this proposal. Lowering taxes for young workers increases their labor supply and earnings, thereby raising tax revenue. However, young workers trade off labor not only for leisure but also for education. As they work more, they invest less in education. Since education yields long-term returns, this trade-off leads to lower incomes in adulthood and ultimately reduces tax revenue.

8 Conclusion

This paper provides evidence that transitory increases in the opportunity cost of schooling can have lasting effects on human capital accumulation. I exploit a salient tax reform that, for a single year, increased the prospective earnings of students entering the labor market. Leveraging this natural experiment and the discontinuity in students' ability to leave school to work, I find that men—but not women—left school to seize the temporary wage increase and never returned to

[23] 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.

complete their education. As a result, they suffered substantial lifetime income losses compared to what they would have earned had they stayed and completed their education.

The results suggest that young men who dropped out during the tax-free year were misinformed about their alternatives. Dropouts were primarily from low-education families and resided in rural, low-education areas. In these environments, the immediate earnings from low-skilled jobs likely appeared more attractive than the long-term benefits of education, reinforcing the decision to leave school prematurely. They entered jobs with relatively high starting wages despite requiring limited formal education or training but offered limited career advancement opportunities and relatively flat earnings profiles compared to jobs available to those with further education.

These results highlight the negative long-term consequences of economic booms that temporarily raise earnings opportunities. The marginal students who drop out of school when opportunity costs rise are mainly young men who would otherwise have acquired much more education and earned higher incomes. This implies that economic booms can have a hidden cost by misallocating young men away from school, with negative consequences for long-term economic growth.

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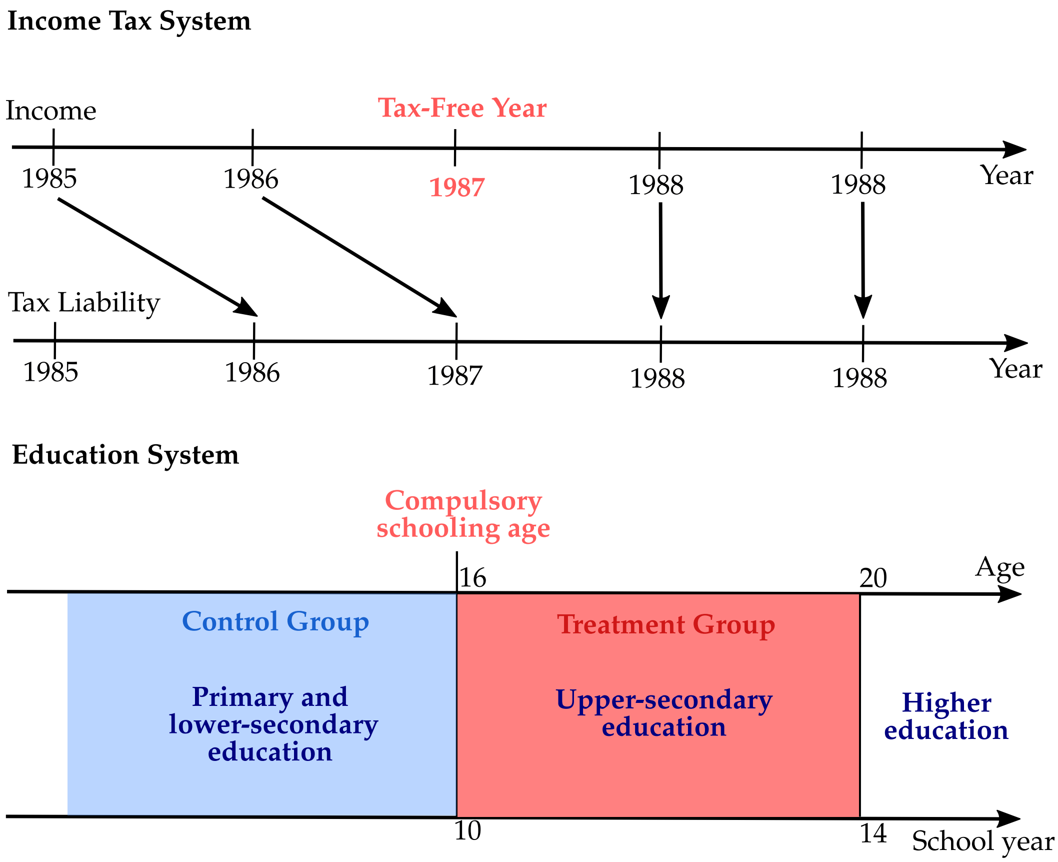
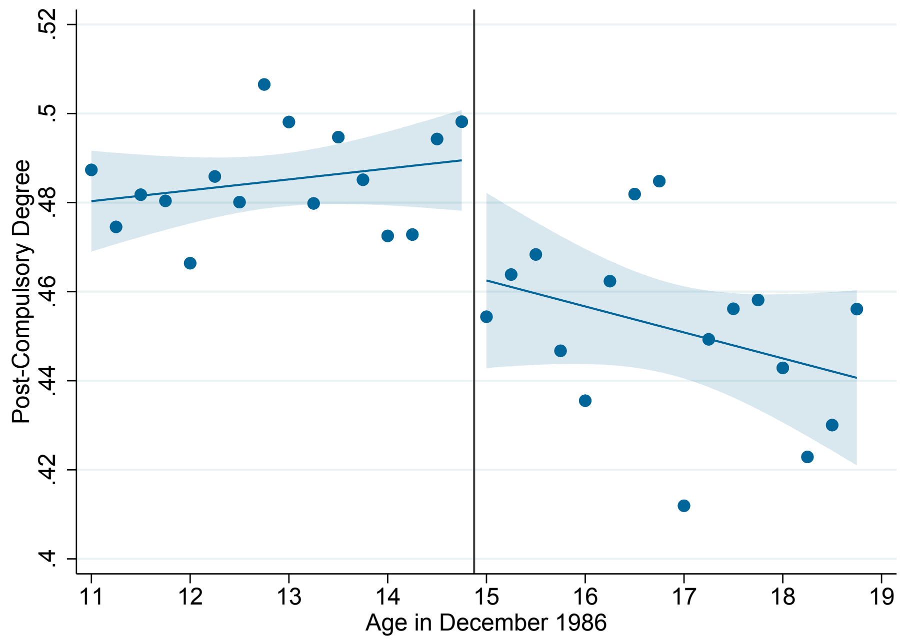
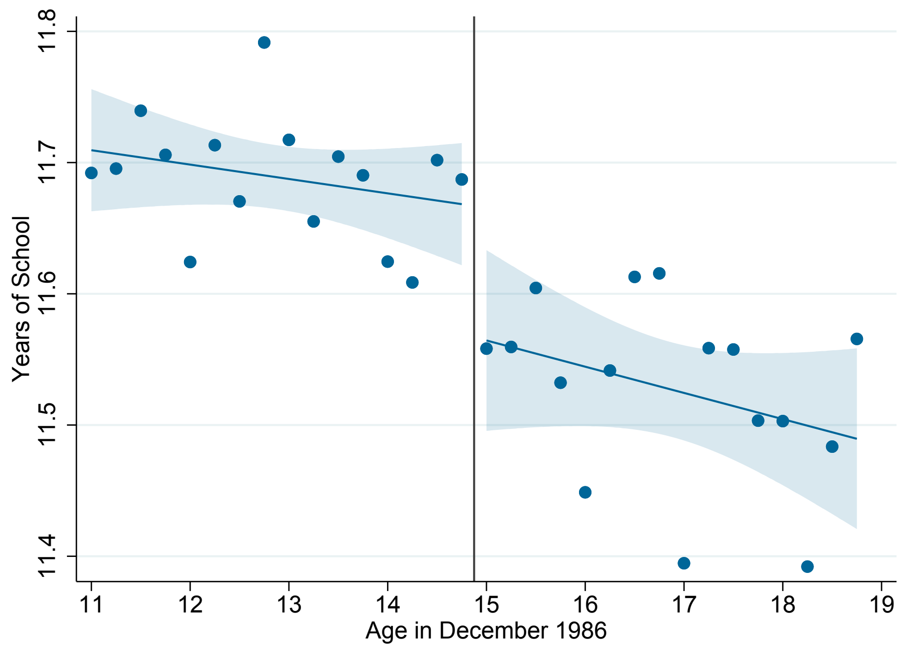


Figure 1: Research Design: Tax-free Year and Compulsory Schooling Age

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 describes the income tax system and transition from retrospective taxation to the pay-as-you-earn system in 1988. This transition led to income earned in 1987 never being taxed. The bottom panel describes the structure of the Icelandic education system in terms of age at each educational level and school year. Students are compelled to stay in school for 10 years until age 16, when they can choose whether to enter upper-secondary education. The research design exploits this discontinuity within the tax-free year. The figure marks the groups whose schooling choice is differently influenced by the tax-free year, that is, the treatment group (age $\geqslant 16$), who are free to choose whether to stay in school, and the control group (age < 16), who are at compulsory schooling age.



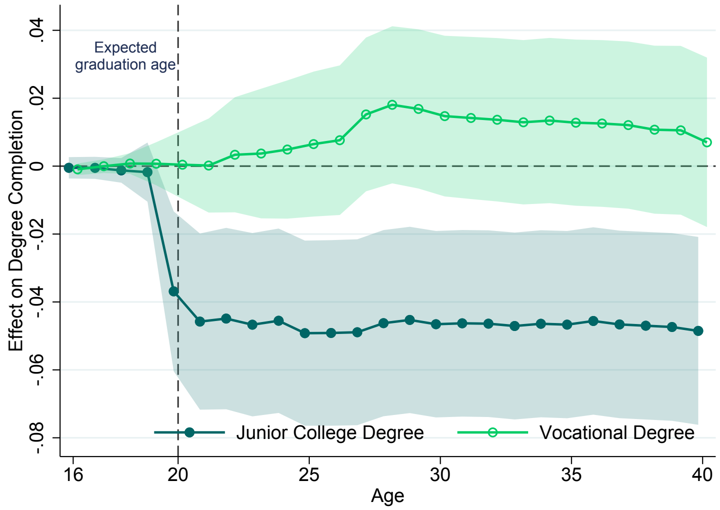
(a) Post-compulsory degree



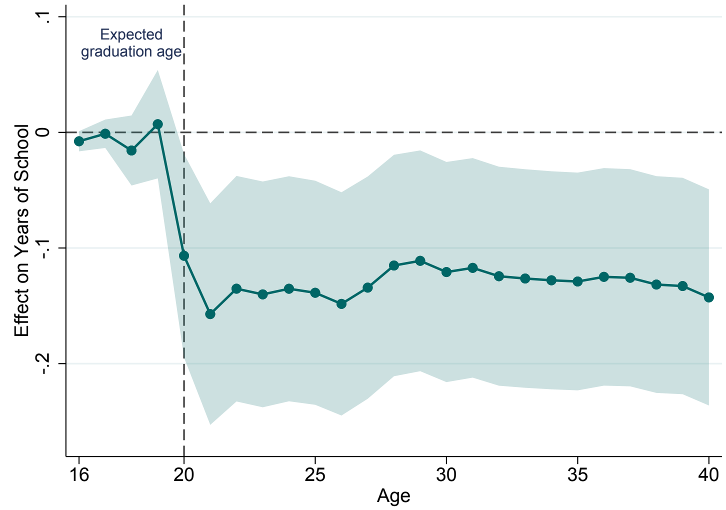
(b) Years of school

Figure 2: School Dropout

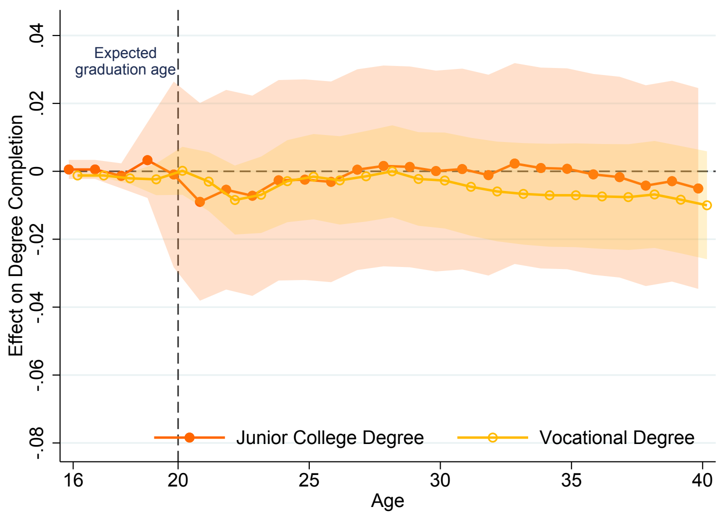
Notes: The figure plots average educational attainment at age 21 on each side of the compulsory-school age threshold. Panel (a) plots the average share with post-compulsory education. Panel (b) plots the average number of years of school completed. The vertical line denotes the compulsory schooling age 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.



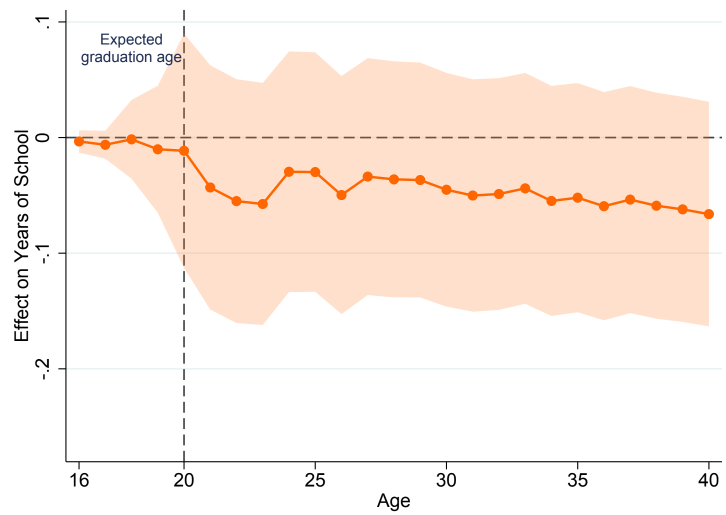
(a) Men: Junior college and vocational degree



(b) Men: Years of school



(c) Women: Junior college and vocational degree



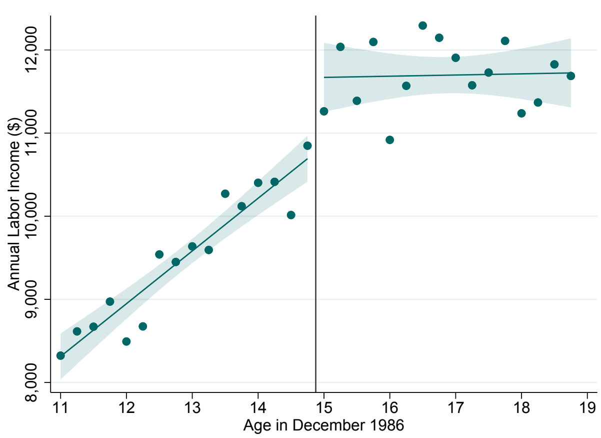
(d) Women: Years of school

Figure 3: Dynamics of the Effect of Tax-Free Year on School Dropout and Educational Attainment

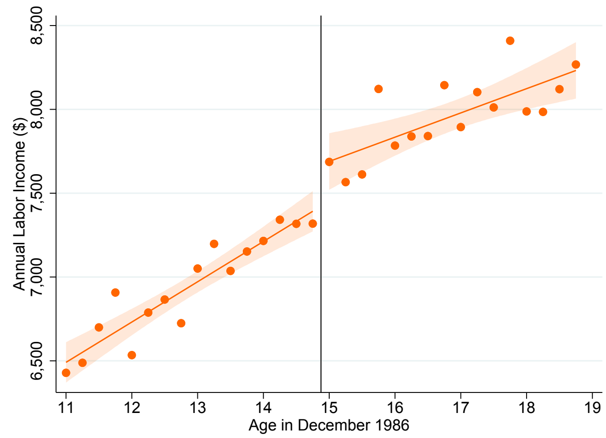
Notes: The figure plots estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panels (a) and (c) plot the estimated effects of the 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 4: Entry Jobs of School Dropouts

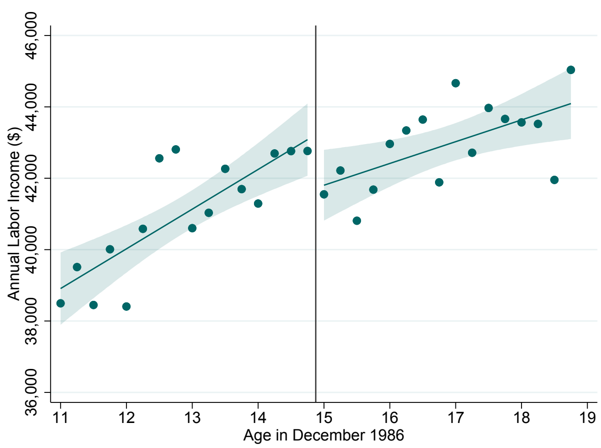
Notes: The figure presents the estimated effect on employment by occupation. The lower part of each bar represents the employment share in that occupation among 16-19-year-olds, separately for men and women, in the pre-reform year 1986. At the top of each bar, I plot the estimated effect on employment 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.



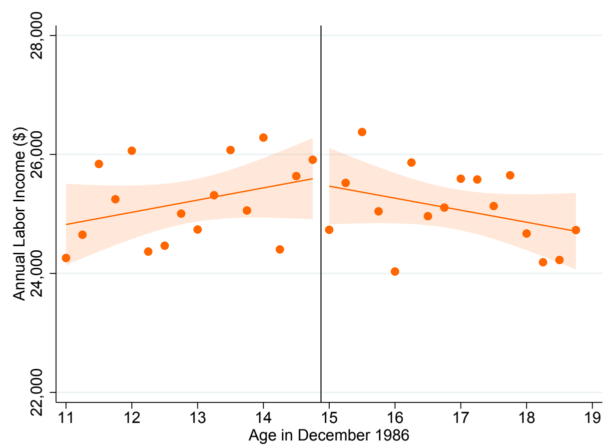
(a) Men at Upper-Secondary School Age



(b) Women at Upper-Secondary School Age



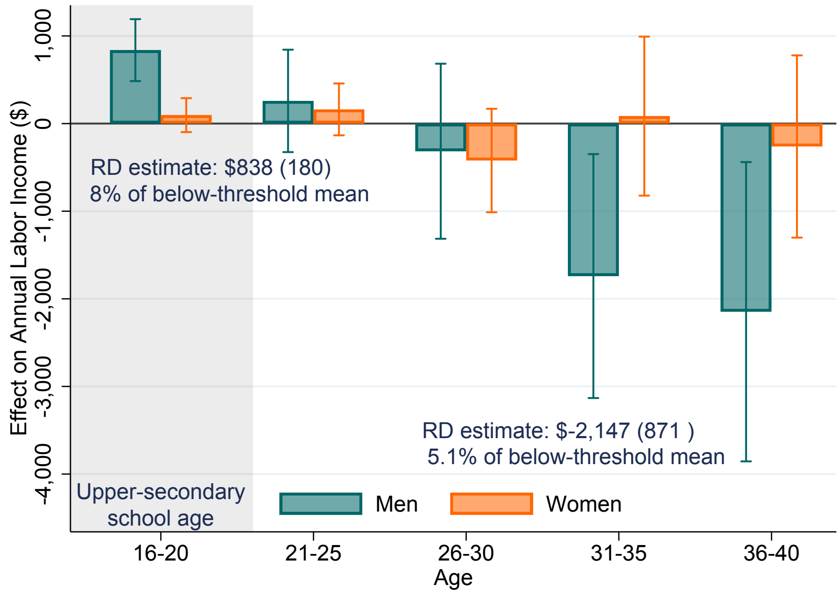
(c) Men at Prime Age



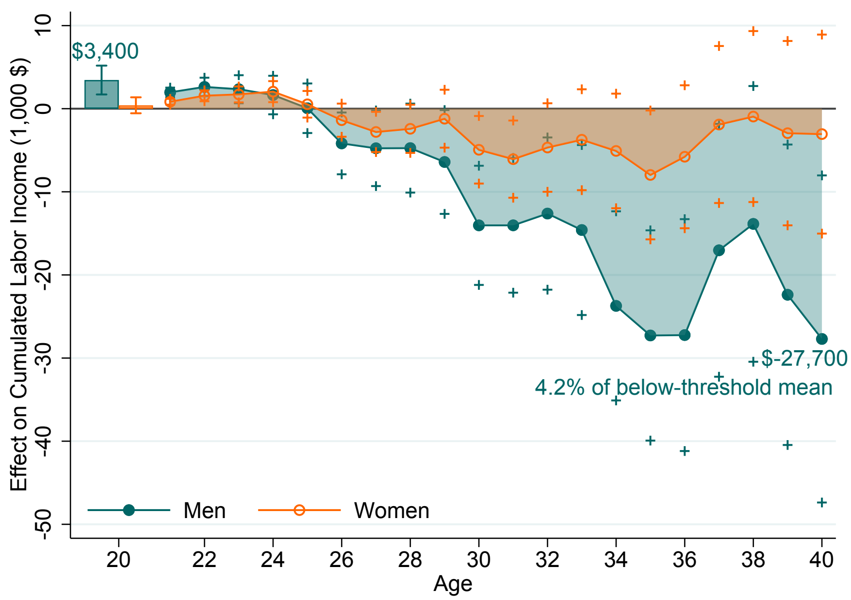
(d) Women at Prime Age

Figure 5: 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 6: 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.

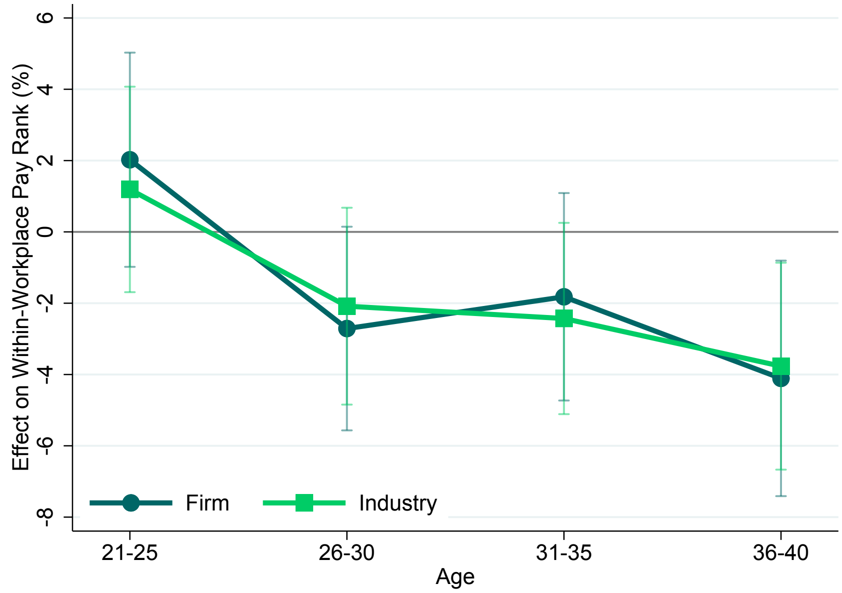


Figure 7: Effect on Career Progression

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.

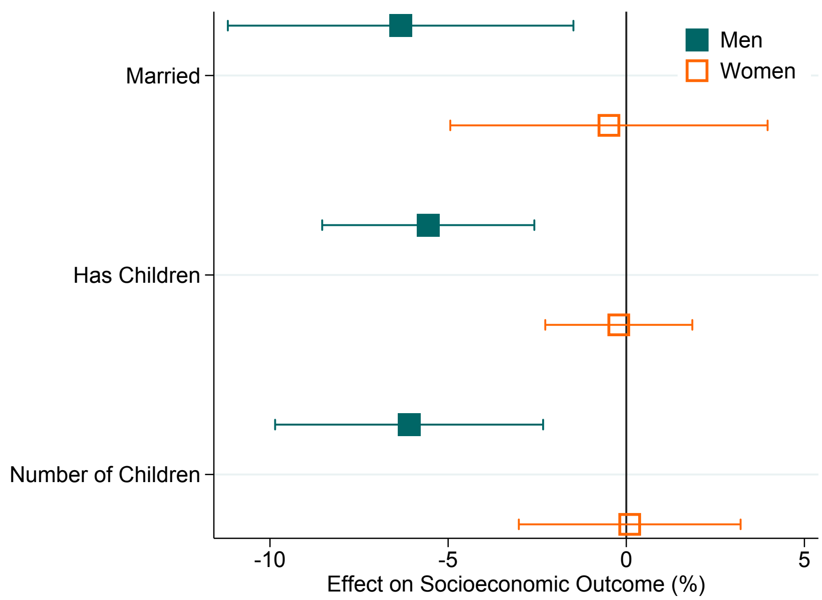


Figure 8: Effect of School Dropout on Marriage and Fertility

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.

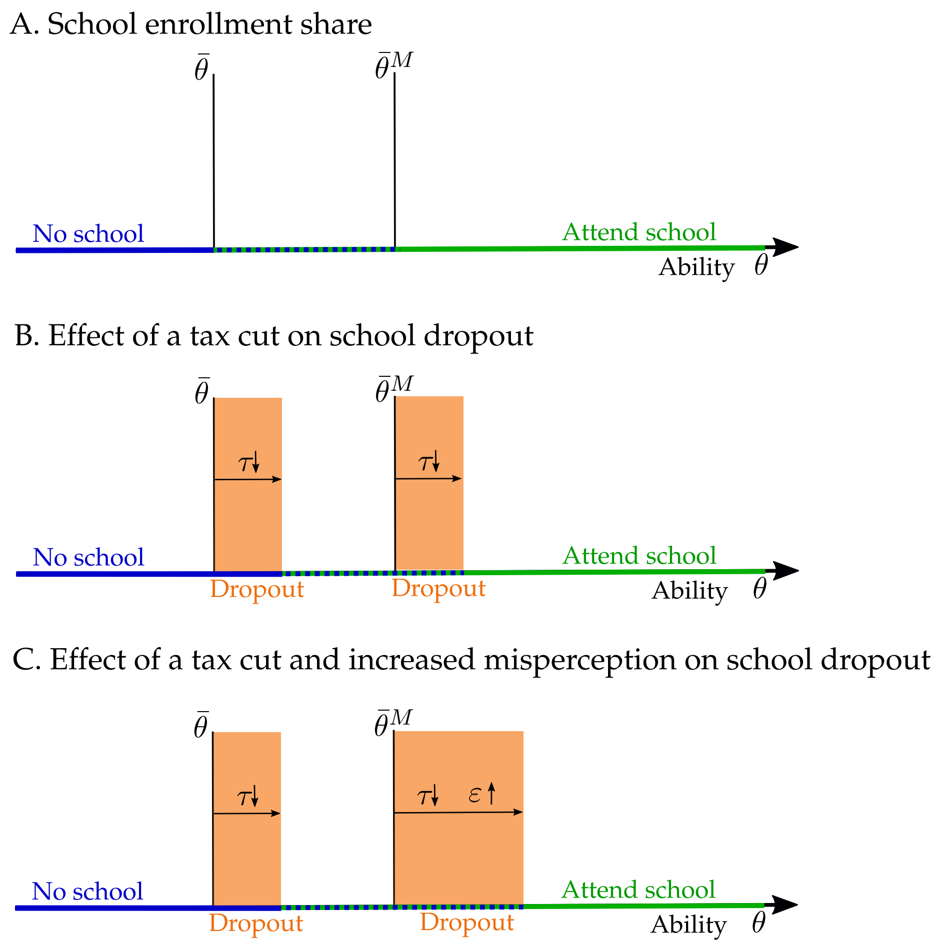


Figure 9: Model of School Dropout

Notes: This figure illustrates the equilibrium school attendance decisions as a function of individual ability. Panel A depicts the ability threshold for school attendance under perfect information about the returns to education, denoted as $\bar{\theta}$, and under misperception, denoted as $\bar{\theta}^M$. Panel B shows the effect of a tax cut on school dropout rates under the two assumptions regarding information accuracy—perfect information and misperception of returns. Panel C examines the combined impact of a tax cut and increased misperception on school dropout rates.

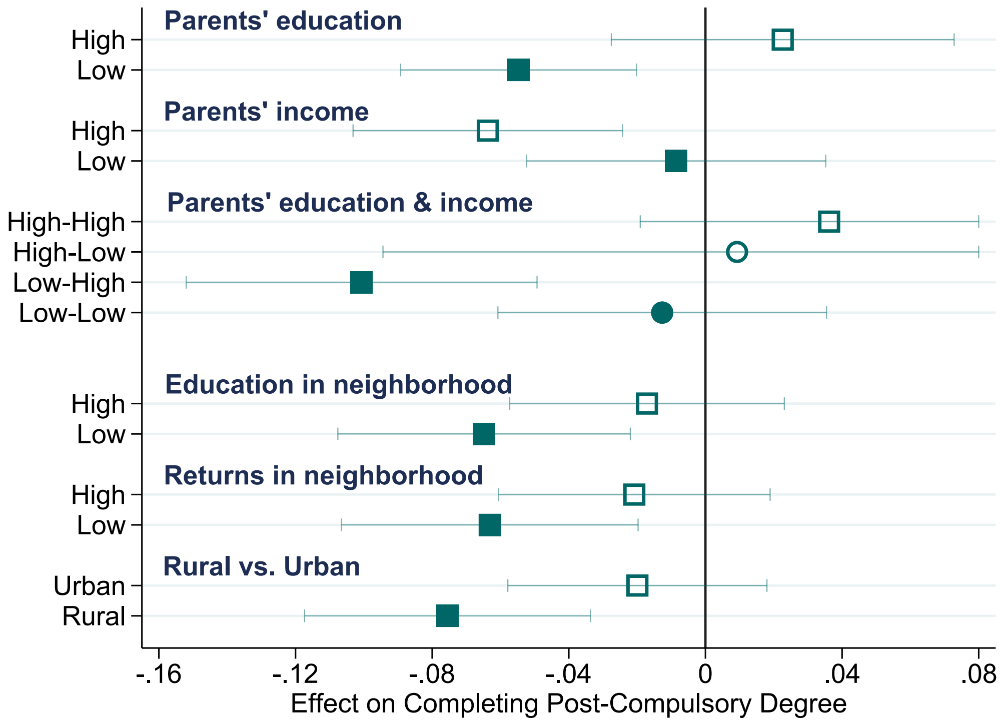


Figure 10: School Dropout by Parental Background and Neighborhood

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 (mother or father) has completed an upper-secondary degree (junior college or higher). 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 40-55. Each student is assigned the rank of the higher-earning parent, and students are split at the median parental income rank. When splitting jointly by parental education and income, I use the indicators described above. For example, "High-Low" refers to students with at least one parent holding an upper-secondary degree, but whose higher-earning parent has income below the median. For neighborhood education, I calculate, for each municipality, the share of adults aged 25-64 with at least an 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 men aged 30-60, separately by education level (at least upper-secondary vs. less). Returns are defined as the ratio of these averages, and students are split at the median return. Municipalities are classified as urban or rural based on official municipality codes. All regressions control for individual characteristics measured before the reform. Regressions by parental background additionally include municipality fixed effects. Whiskers denote 95% confidence intervals, which are top-coded at 0.08.

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

April 14, 2025

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*), while taking into account the education attained by Icelandic students from early 20th century. As summarized in Table A.1, the standard divides attained education into nine levels, out of which six are further subdivided. In all, educational attainment is classified into 31 educational classes. The Register records completed education. Education is considered completed once the student can transition to next level, as is the case at lower levels, or completed with sufficient qualification and degree.

In my analysis, my main measure of educational attainment is education measured as years of school. One year refers to the school year, is normally 8-10 months (2-3 terms). For university education there are two semesters, where each semester refers to 30 credits according to the European Credit Transfer and Accumulation System (ECTS) or equivalent before introduction of the ECTS system. I translate education attained into years of school based on the time required to complete a given level or degree. For example, a junior college degree translates to 4 years of school and a bachelor's degree (180 ECTS) translates to 3 years.

Table A.1: Education Classification in Statistics Iceland's Education Register

Level	Description	Broad Category	Nr. of sub-categories
0	Less than primary education		1
1	Primary education	Compulsory education	1
2	Lower secondary education	Compulsory education	8
3	Upper secondary education	Junior college and vocational education	8
4	Post-secondary non-tertiary education	Junior college and vocational education	5
5	Short-cycle tertiary education	Higher education	2
6	Bachelor's or equivalent level	Higher education	3
7	Master's or equivalent level	Higher education	2
8	Doctoral or equivalent level	Higher education	1
			31

A.2 Occupation and Sector Classification

The pay slip data records occupation according to a two-digit classification. There are 74 separate occupation classes recorded. The occupation classification is an Icelandic version of the International Labor Organization's (ILO) International Standard Classification of Occupations (ISCO), version ISCO-88. More details on the classification are provided in documentation on ILO's website. Table A.2 documents the structure of the classification and lists the broader occupation groups. Subcategories within the broader occupation categories generally refer to sector specific groups. For example, within *Elementary occupations* there are manufacturing laborers and construction laborers.

The pay slip data also record the sector for each firm. In total there are 189 separate sector classes recorded. The sector classification is based on the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC). Details about the classification are provided in documentation on UN's website. Table A.3 documents the structure of the sector classification.

Table A.2: Occupation Classification

Group	Occupation category	No. of subcategories
1	Legislators, senior officials and managers	17
2.	Professionals	5
3.	Technicians and associate professionals	8
4.	Clerks	7
5.	Service workers and shop and market sales workers	9
6.	Plant and machine operators and assemblers	1
7.	Skilled agriculture and fishery workers	7
8.	Craft and related trades workers	11
9.	Elementary occupations	9
0.	Armed Forces	0
		74

Notes: The occupation classification is based on the International Labor Organization's (ILO) International Standard Classification of Occupations (ISCO), version ISCO-88. For a detailed description of the classification, see ILO's website.

Table A.3: Sector Classification

Group	Sector category	No. of subcategories
1	Activities of extraterritorial organizations and bodies	2
2	Agriculture and forestry	10
3	Fishing	6
4	Manufacturing	64
5	Mining and quarrying	2
6	Construction	16
7	Other service activities	6
8	Electricity, gas, steam, and air conditioning supply	2
9	Water supply; sewage, waste management and remediation activities	2
10	Wholesale and retail trade; repairs of motor vehicles and motorcycles	19
11	Financial and insurance activities	5
12	Real estate activities	2
13	Rental and leasing activities	2
14	Transportation and storage	10
15	Public administration and defense; compulsory social security	6
16	Education	4
17	Human health and social work activities	11
18	Arts, entertainment and recreation	8
19	Professional, scientific and technical activities	9
20	Activities of households as employers	1
21	Accommodation and food service activities	2
		189

Notes: The sector classification is 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 A Model of Aggregate Production with Cohort Specific Supplies

In this section, I outline the theoretical framework developed by Card and Lemieux (2001a) and derive equation (2) presented in the main text. For a more comprehensive description of the model and its estimation, refer to the original paper.

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 (L_t) and low (H_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 (2001a) add a lower level where supplies of each education group are themselves two 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:

$$H_t = \left(\sum_j \alpha_j H_{ij}^{\eta}\right)^{\frac{1}{\eta}} \qquad L_t = \left(\sum_j \beta_j L_{ij}^{\eta}\right)^{\frac{1}{\eta}} \tag{10}$$

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

Aggregate output in period t is also a CES:

$$Y_t = (A_{Ht}H_t^{\rho} + A_{Lt}L_t^{\rho})^{\frac{1}{\rho}} \tag{11}$$

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:

$$\begin{aligned} w_{jt}^{L} &= \frac{\partial Y_t}{\partial L_{jt}} = \frac{\partial Y_t}{\partial L_t} \times \frac{\partial L_t}{\partial L_{jt}} \\ &= A_{Lt}L_t^{\rho-\eta}\Psi \times \beta_j L_{jt}^{\eta-1} \end{aligned} \tag{12}$$

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

$$w_{jt}^{H} = A_{Ht}H_t^{\rho-\eta}\Psi \times \beta_j H_{jt}^{\eta-1} \tag{13}$$

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

Using equations (12) and (13), 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

$$\ln\left(\frac{w_{jt}^{L}}{w_{jt}^{H}}\right) = \ln\left(\frac{A_{Lt}}{A_{Ht}}\right) + \ln\left(\frac{\beta_j}{\alpha_j}\right) + \left[\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right]\ln\left(\frac{L_t}{H_t}\right) - \frac{1}{\sigma_A}\ln\left(\frac{L_{jt}}{H_{jt}}\right) \tag{14}$$

In the context of this paper, the focus is on quantifying how changes in the relative supply of low-educated and high-educated workers within a cohort influence the relative wages of these two education groups. To align with the empirical setting, consider two adjacent birth cohorts, j and j', where cohort j is affected by the tax reform, and cohort j' is not. Equation (14) describes the relative wages in cohort j, with an analogous equation for cohort j'. Taking the difference between the two equations yields:

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

assuming that the relative efficiency parameters are the same for the two cohorts. This is equation (2) in the main text.

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, particularly from Card and Lemieux (2001a), we can quantify the effect on the relative wages of education groups. In Section 3, I estimate an 8 percent increase in the dropout rate. Card and Lemieux (2001a) 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). Their estimates of σ_A range from 3.8 to 4.9 for the U.S., 3.8 to 4.3 for the U.K., and 6.1 to 6.2 for Canada (see Table 3 in Card and Lemieux (2001a)). Acemoglu and Autor (2011) estimate a comparable elasticity of 3.7 using U.S. data from 1963-2008. Combining these estimates with the calculated effect on educational attainment, we find that the relative wage of low-educated workers decreases by between 1.3 and 2.1 percent. This general equilibrium effect accounts for 25 to 40 percent of the total estimated earnings effect for the affected cohorts, as discussed in Section 4. Moreover, using equation (2), we can calibrate the value of σ_A that would equate the estimated effects on education and earnings, which yields $\sigma_A = 1.57$. As noted in the main text, this would imply that similarly educated workers from adjacent birth cohorts are as poor substitutes as high school-educated and college-educated workers (Katz and Murphy, 1992; Acemoglu and Autor, 2011).

C Effects on Marriage and Fertility as an Income Effect

The findings in Section 4 reveal that dropouts suffer large income losses in adulthood as well as reduced marriage and fertility. This effect might run through two alternative channels. On the one hand, dropping out of school may directly affect marriage and fertility. For example, much prior work has documented educational assortative mating (Mare, 1991; Pencavel, 1998) and recent studies document how schools act as marriage markets (Blossfeld, 2009; Kirkebøen, Leuven, and Mogstad, 2021). On the other hand, reduced socioeconomic success might reflect an income effect where earnings loss leads to reduced marriage and fertility. For example, as first emphasized by Becker (1960), if children are "normal goods" a fall in income will lead to less fertility.

To evaluate the latter alternative, Table A.4 summarizes estimates from prior studies of the effect of windfall income on marriage and fertility. Cesarini, Lindqvist, Östling, and Terskaya (2021) study the impact of winning a lottery on marital and fertility outcomes among Swedish male and female lottery players. Evaluating outcomes several years after winning a lottery, they find that winning $100,000 increases men's propensity to marry by 30 percent. Exploiting similar variation, Golosovy, Graberz, Mogstad, and Novgorodsky (2021) estimate a smaller but significant average increase in marriage rates among US lottery winners of both genders.[24] Indeed, Cesarini, Lindqvist, Östling, and Terskaya (2021) find that lottery wealth does not significantly alter women's propensity to marry, which may partly explain the difference. Cesarini, Lindqvist,

[24] Chu, Lin, and Tsay (2020) estimate the marriage response to an increase in housing wealth and find that a 10% increase in housing wealth induces a 4% increase in marriage rates. In contrast to the other studies, Hankins and Hoekstra (2011) find no effect on marriage among male winners in the Florida Lottery, but a reduction among female winners.

Table A.4: Comparison of Effects on Marriage and Fertility to Prior Studies

	Marriage (1)	Has children (2)	Number of children (3)
Windfall Income: $100,000 ↑			
Cesarini et al. (2021)	29.9%	13.4%	17.0%
Golosovy et al. (2021)	5.5%*	—	—
Chu et al. (2020)	3.9%**	—	—
Hankins and Hoekstra (2011)	No effect	—	—
Lovenheim and Mumford (2013)	—	16.4%	18.8%
School Dropout: $100,000 ↓	-13.5%	-16.0%	-42.4%

Notes: The table reports the effects of $100,000 increase/decrease in income or wealth on the outcome specified in the row heading. The table presents estimates for men unless otherwise indicated. Estimates for the impact of school dropout are the coefficient estimates for marriage and fertility reported in Figure 8 but scaled by the estimated effect on lifetime earnings (Figure A.18) such that -1% means that a $100,000 reduction in earnings reduces the outcome by 1% compared to the below-threshold average. Cesarini, Lindqvist, Östling, and Terskaya (2021), Golosovy, Graberz, Mogstad, and Novgorodsky (2021), and Hankins and Hoekstra (2011) report responses to windfall income from lottery winnings, and estimates in Lovenheim and Mumford (2013) and Chu, Lin, and Tsay (2020) responses to housing wealth increase.
* The estimates are for both men and women.
** The estimates are in measured as response to a 10% increase in wealth.

Östling, and Terskaya (2021) also estimate that winning the lottery increases the likelihood of having a child by roughly 13% and the number of children by 17%. Exploiting increases in housing wealth, Lovenheim and Mumford (2013) find even stronger effects of wealth on fertility.

To compare my estimates to those in these studies, I present the estimated effect on marriage and fertility scaled by the estimated effect on lifetime earnings such that -1% means that a $100,000 reduction in earnings following school dropout reduces the outcome by 1% compared to the below-threshold average. A comparison of my estimates to prior studies demonstrates that my estimates are qualitatively and quantitatively in line with their estimates. While substantially smaller than estimates in Cesarini, Lindqvist, Östling, and Terskaya (2021), the effect on marriage is similar but somewhat larger than those in Golosovy, Graberz, Mogstad, and Novgorodsky (2021) and Chu, Lin, and Tsay (2020). The effect on fertility is broadly in line with prior estimates. For example, a $100,000 loss in lifetime earnings due to dropout is associated with as much reduction in fertility as Cesarini, Lindqvist, Östling, and Terskaya (2021) estimate that winning $100,000 increases fertility. This indicates that income effects from large losses in earnings may explain the estimated effects on marriage and fertility.

D Supplementary Figures

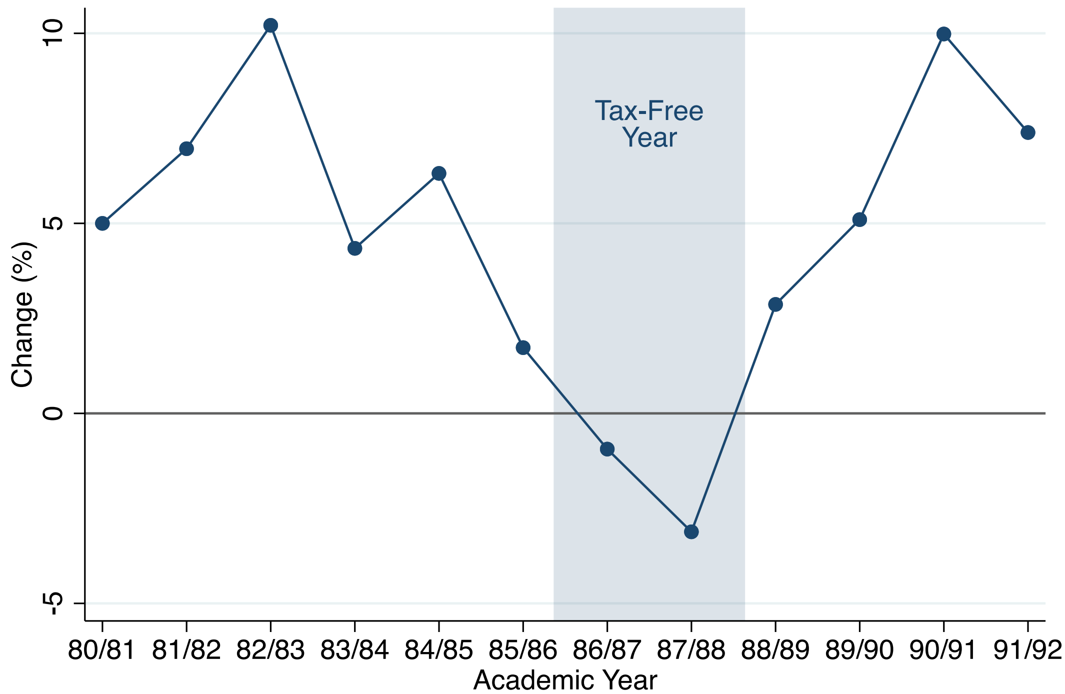
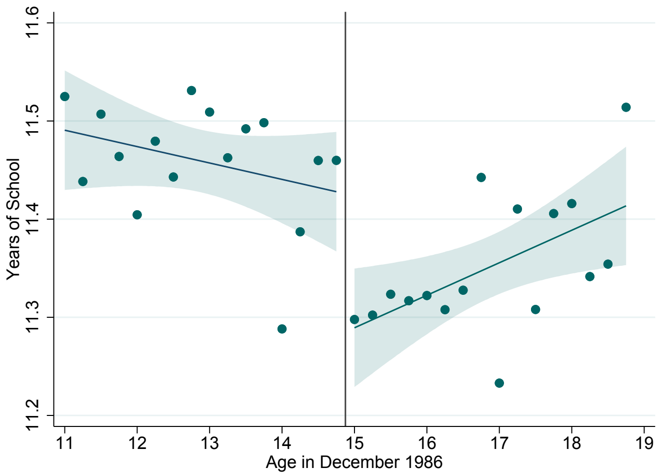
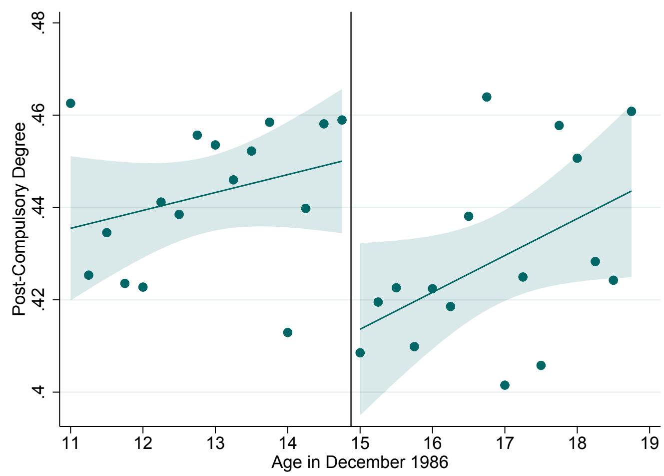


Figure A.1: Change in University Enrollment

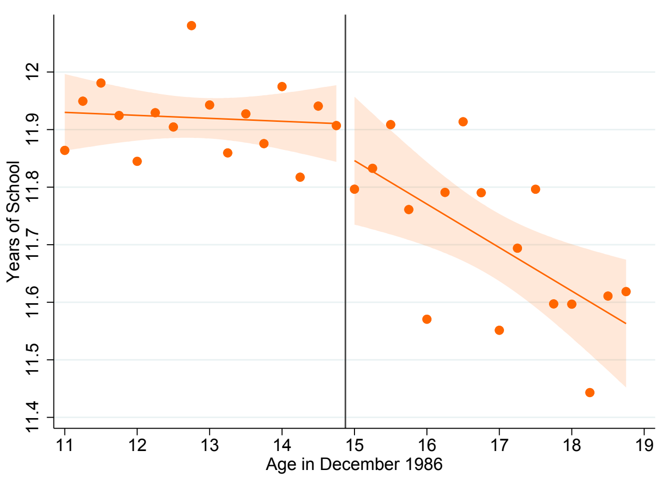
Notes: This figure plots the percentage change in the number of students enrolled in University education each academic year. The shaded area covers the two academic years that the tax-free year influenced.



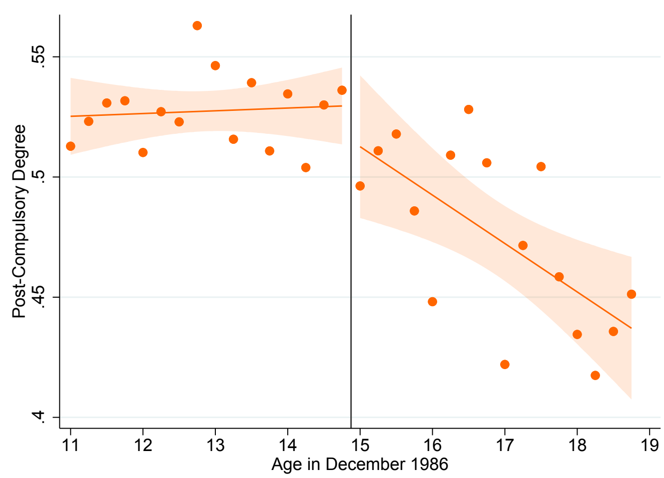
(a) Men: Years of school



(b) Men: Post-compulsory degree



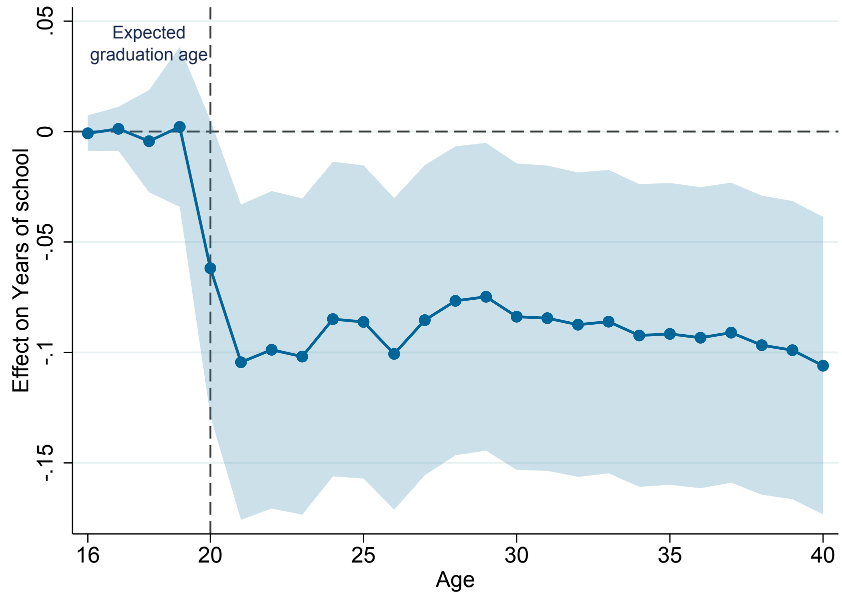
(c) Women: Years of school



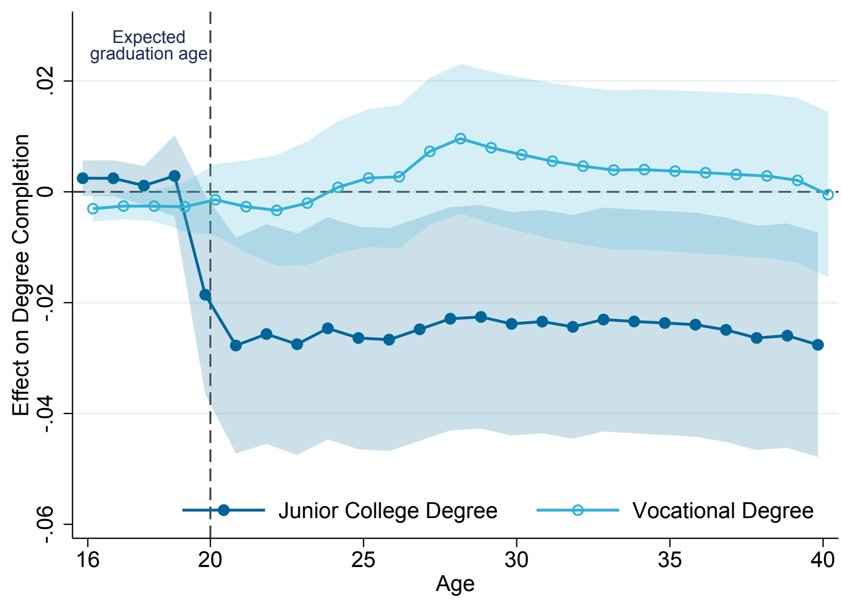
(d) Women: Post-compulsory degree

Figure A.2: 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.



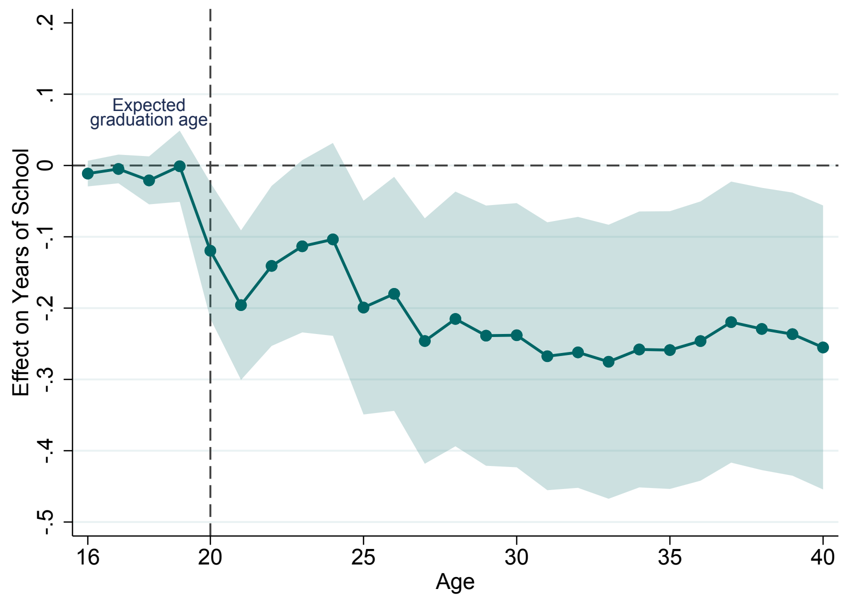
(a) Years of schooling



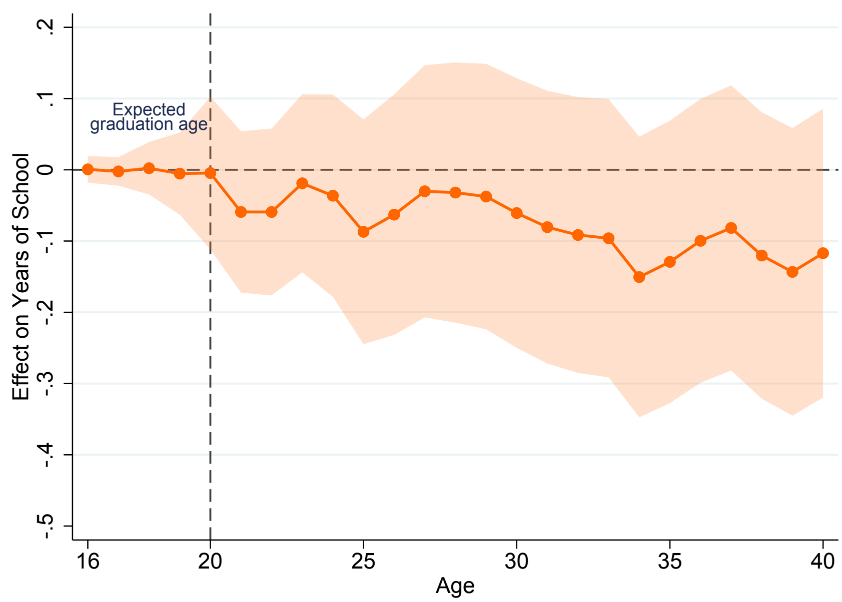
(b) Junior college and vocational degree

Figure A.3: Dynamics of the Effect of Tax-Free Year on Educational Attainment

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 the estimated effects of the tax-free year on years of schooling completed. Panel (b) plots the estimated effect on completing junior college and vocational degrees. 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, where the standard errors are clustered at the individual level.



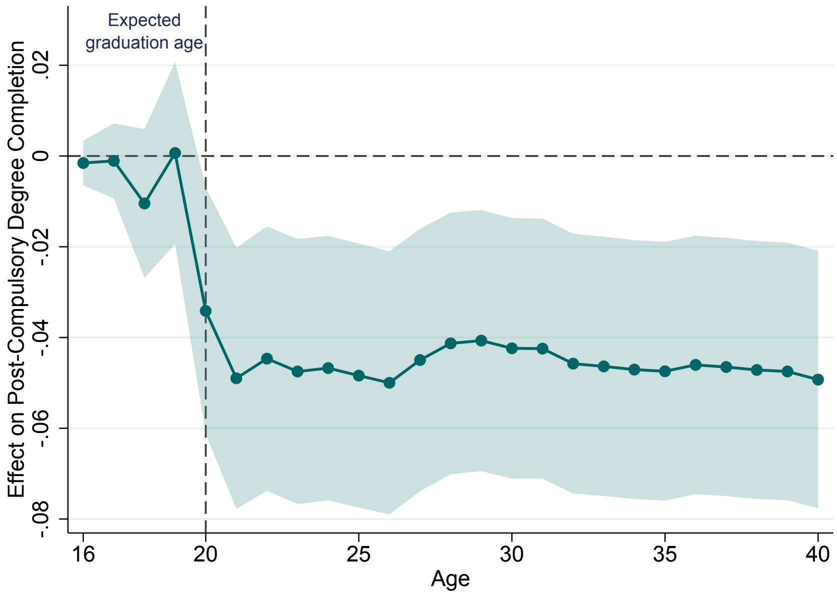
(a) Men: Years of school



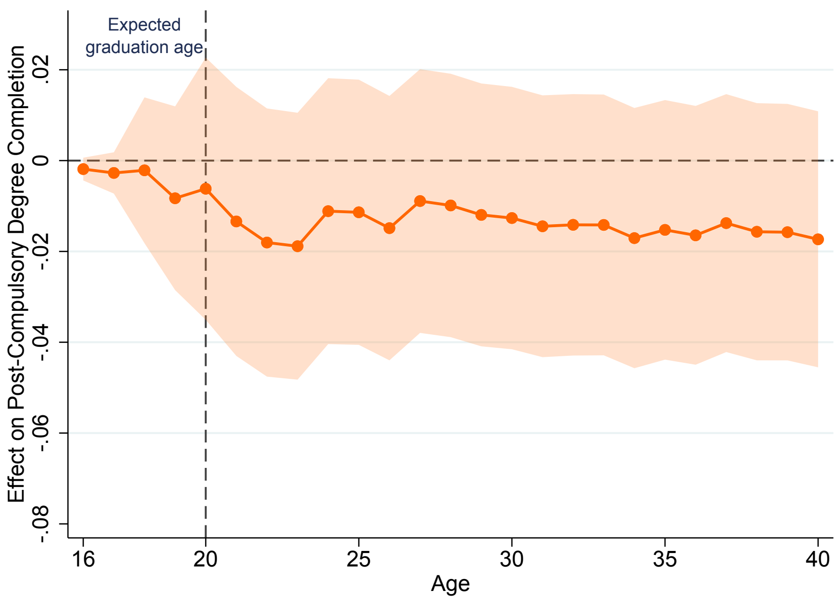
(b) Women: Years of school

Figure A.4: Years of Schooling

Notes: This figure is a plot of 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 years of schooling, including university education, 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.



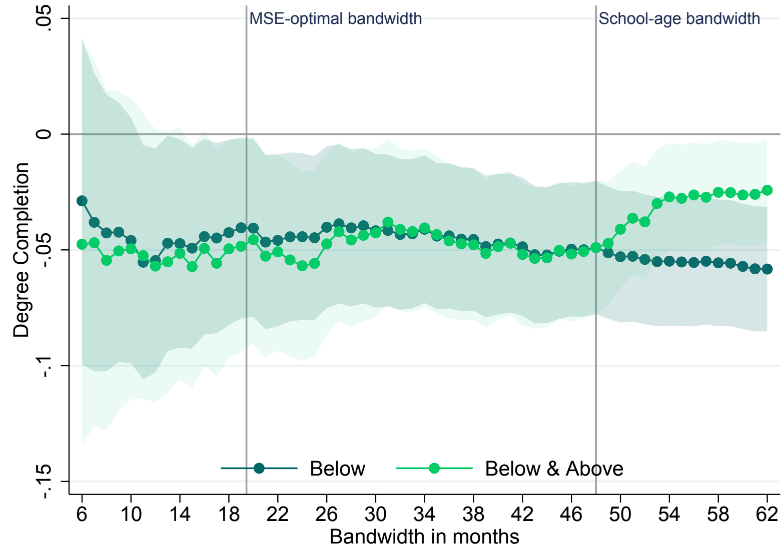
(a) Men: Post-Compulsory Degree



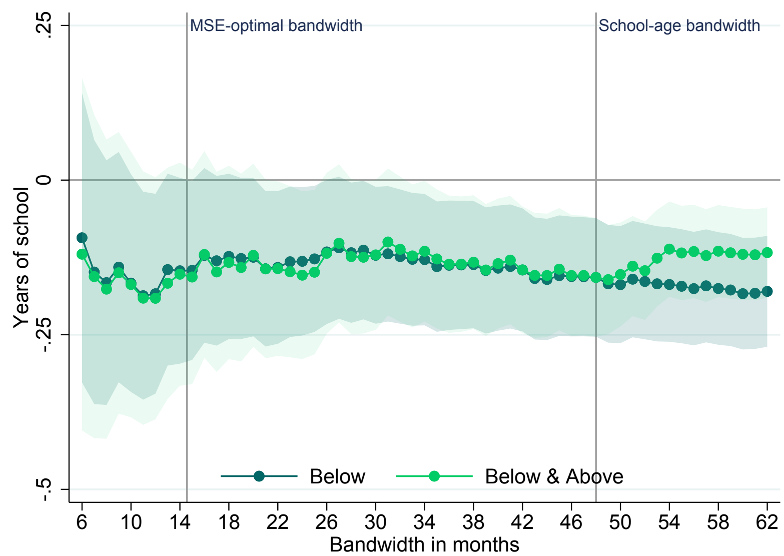
(b) Women: Post-Compulsory Degree

Figure A.5: 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.6: 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.

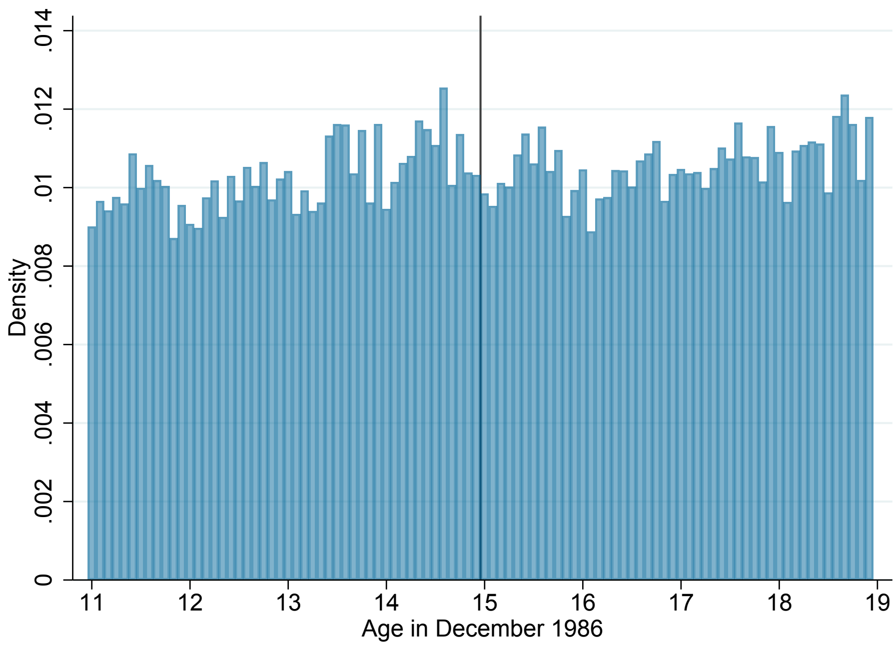
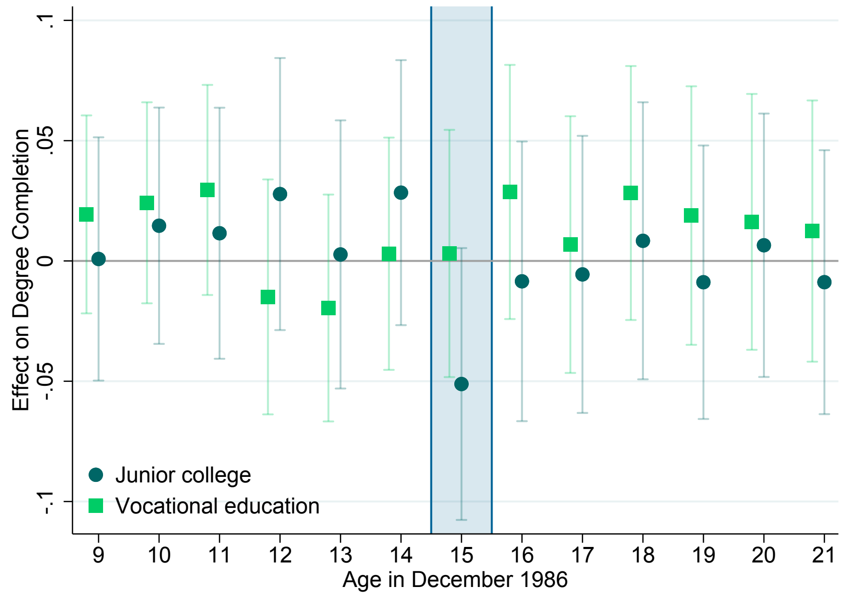
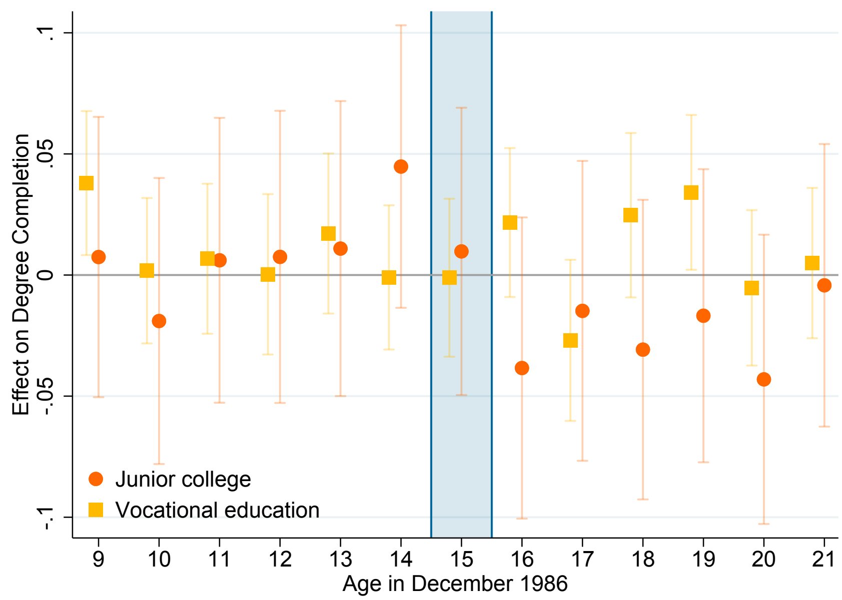


Figure A.7: Distribution of Births by Birth-Month Cohorts

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.



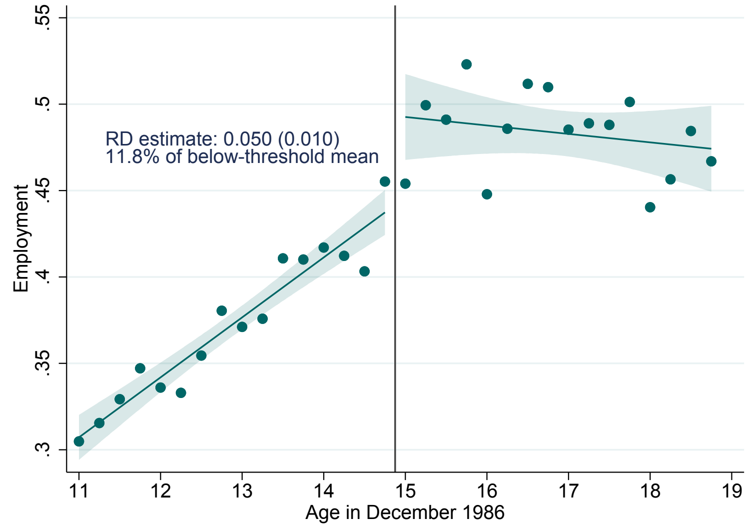
(a) Men



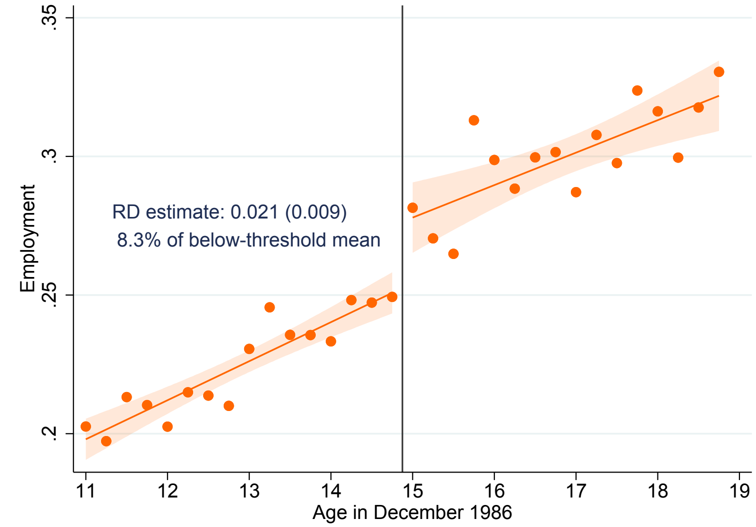
(b) Women

Figure A.8: Placebo Tests of Effects on Educational Attainment

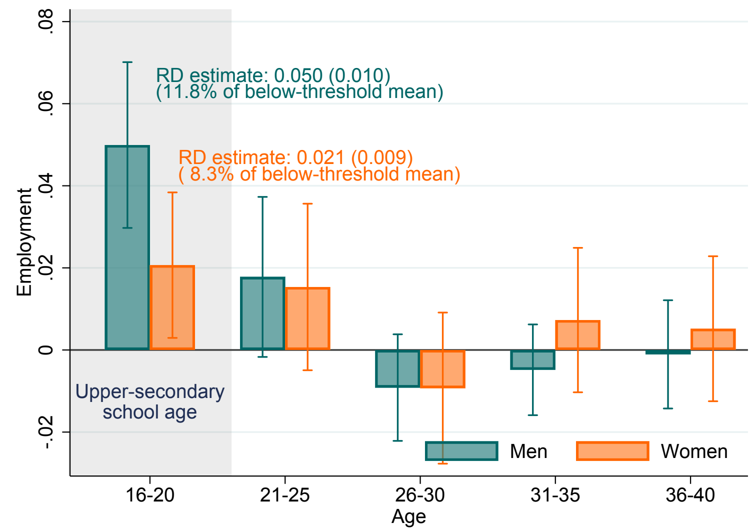
Notes: This figure plots tests of discontinuities in the educational attainment of men (panel a) and women (panel b) at the actual compulsory schooling age threshold in the tax-free year and at placebo thresholds. Educational attainment is measured as the completion of post-compulsory education, either 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



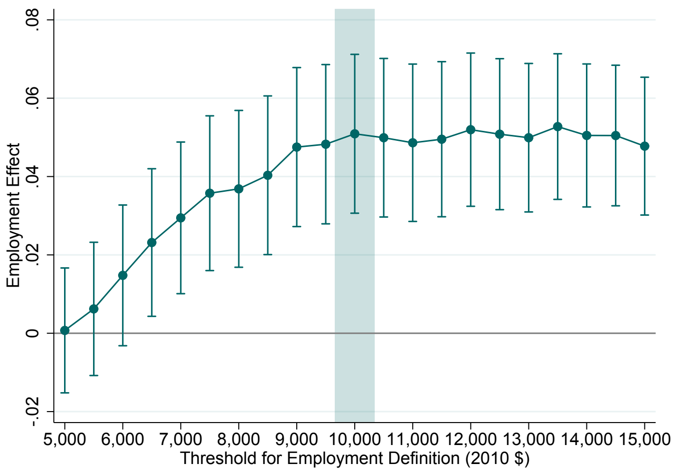
(b) Women: Employment



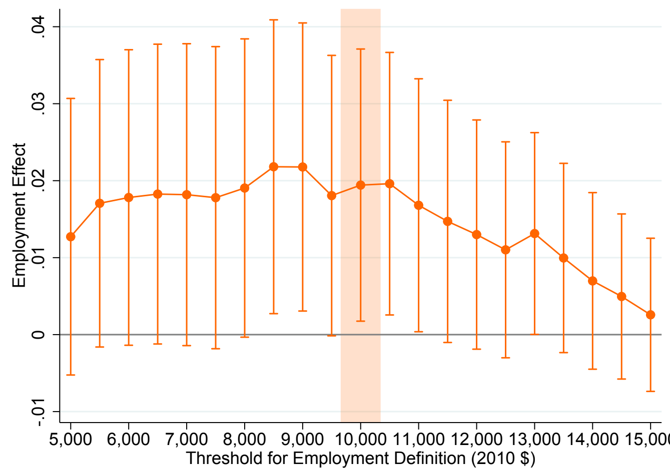
(c) Effect on Employment

Figure A.9: Effects of Tax-Free Year on Employment

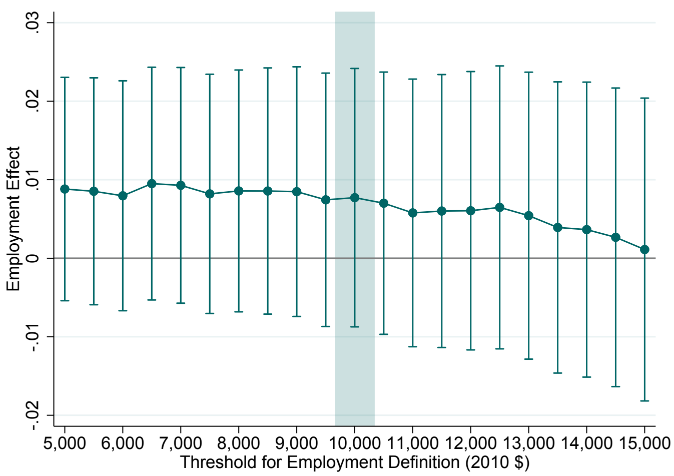
Notes: This figure studies the effect of the tax-free year on employment. Panels (a) and (b) plot the average employment at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Employment is defined as earning at least $10,000. Panel (c) plots RD estimates using equation (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.



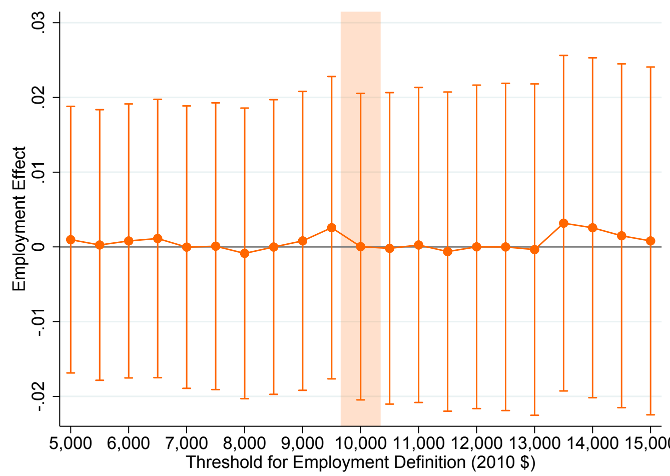
(a) Men: Effect on Employment at School Age



(b) Women: Effect on Employment at School Age



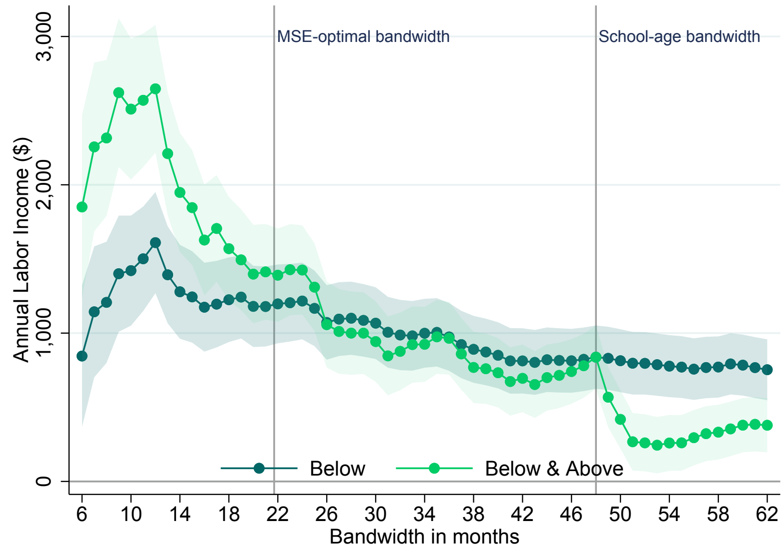
(c) Men: Effect on Employment at Prime Age



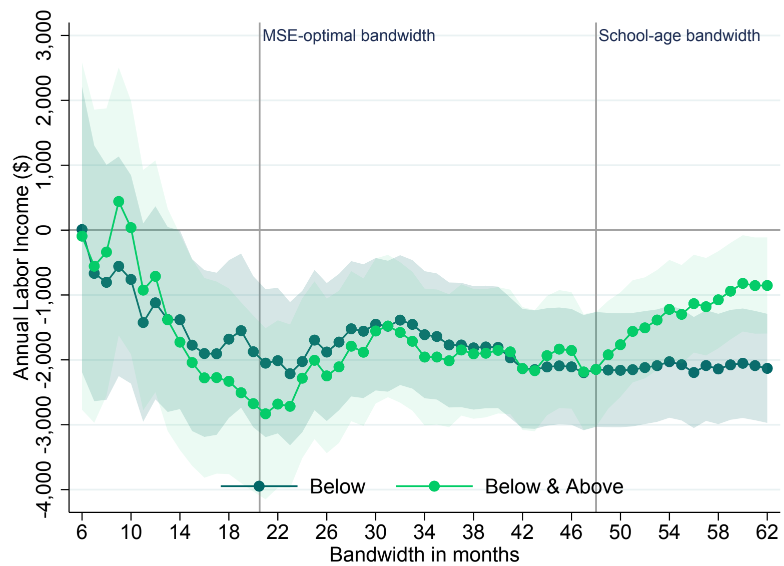
(d) Women: Effect on Employment at Prime Age

Figure A.10: 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.11: 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.

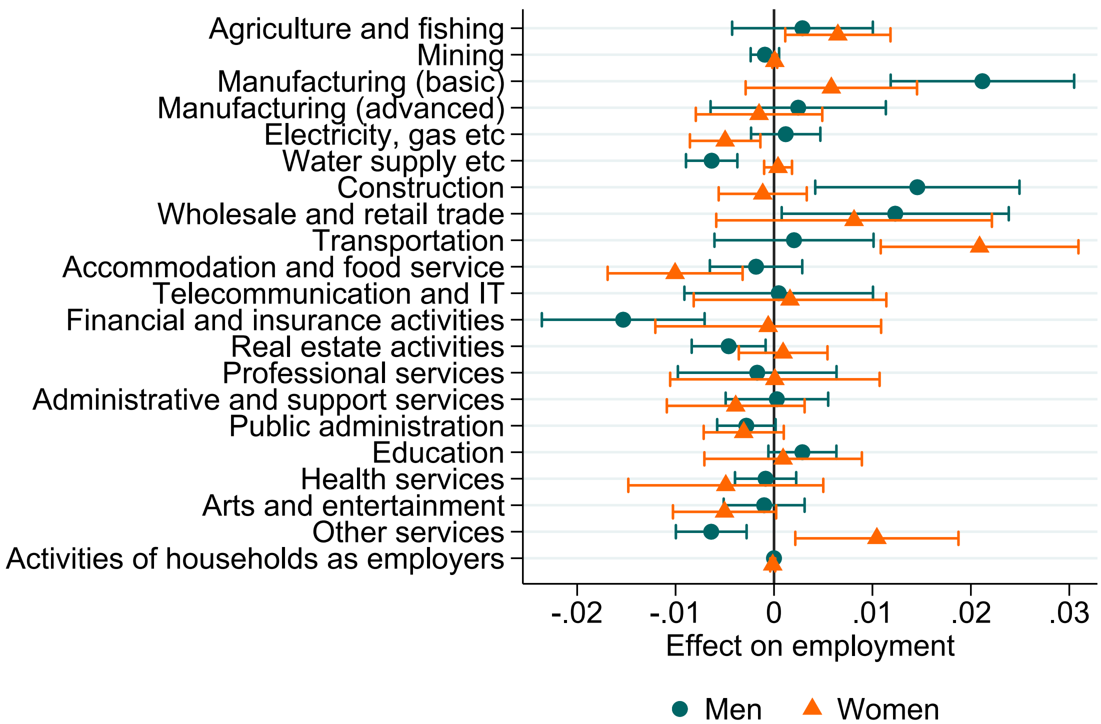


Figure A.12: Jobs at Prime Age

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.

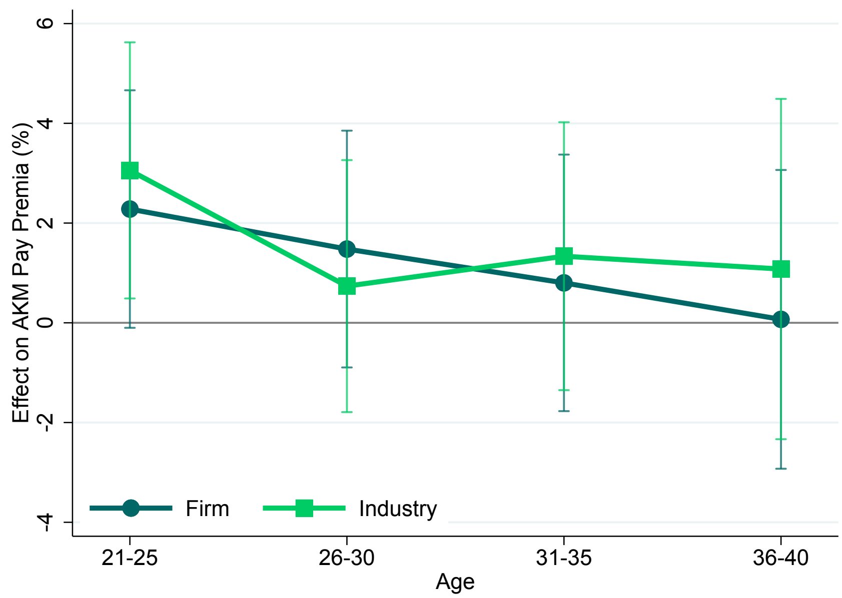


Figure A.13: Effect on Pay Premia in Firms and Industries

*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) on earnings rank, which I divide by the mean rank for the control group 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.

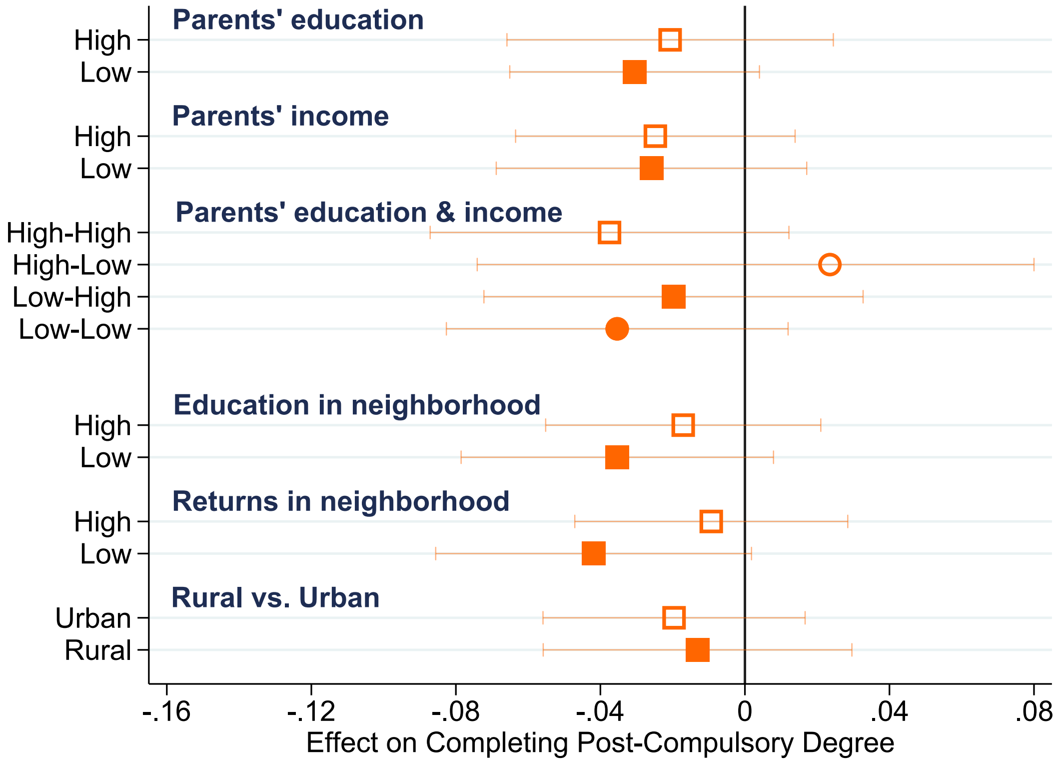


Figure A.14: School Dropout by Parental Background and Neighborhood — Women

Notes: The figure plots the estimated effects of the tax-free year on school dropout of women by parental background and neighborhood. 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, the sample is split according to whether at least one parent (father or mother) has completed an upper-secondary degree (junior college or higher). For parental income, I first rank all individuals in the population by labor income within each birth cohort, gender, and calendar year. I then calculate the median income rank for parents at ages 40-55. For each student, the parental income rank is assigned based on the higher-earning parent's rank, and the student population is divided at the median parental income rank. When splitting by both parental education and income, I use the indicators described above, i.e., having at least one parent with an upper-secondary degree and having parental income above the median. To split the sample by neighborhood education levels, I first compute, for each municipality, the share of adults aged 25-64 with at least an upper-secondary degree in the year preceding the tax-free year (1986). I then divide students at the median of this share. To split by neighborhood returns to education, I calculate for each municipality the average labor income in 1986 for men aged 40-55 separately by education level (at least upper-secondary vs. less education). The returns to education are then defined as the ratio of average incomes between the educated and less educated groups. Students are divided at the median of these returns. Lastly, I classify municipalities into urban and rural areas using municipality codes. All regressions control for individual characteristics measured before the reform, and estimations by parental background further include municipality fixed effects. Whiskers in the figure represent 95% confidence intervals.

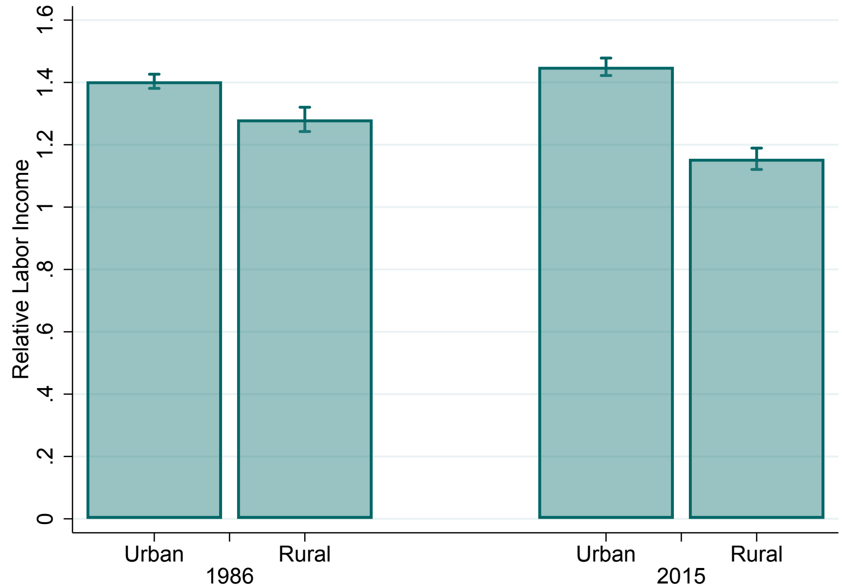


Figure A.15: Returns to Education

Notes: The figure plots the average labor earnings of men at ages 30-60 with upper-secondary education relative to men at the same age without upper-secondary education, separately by those living in urban and rural areas.

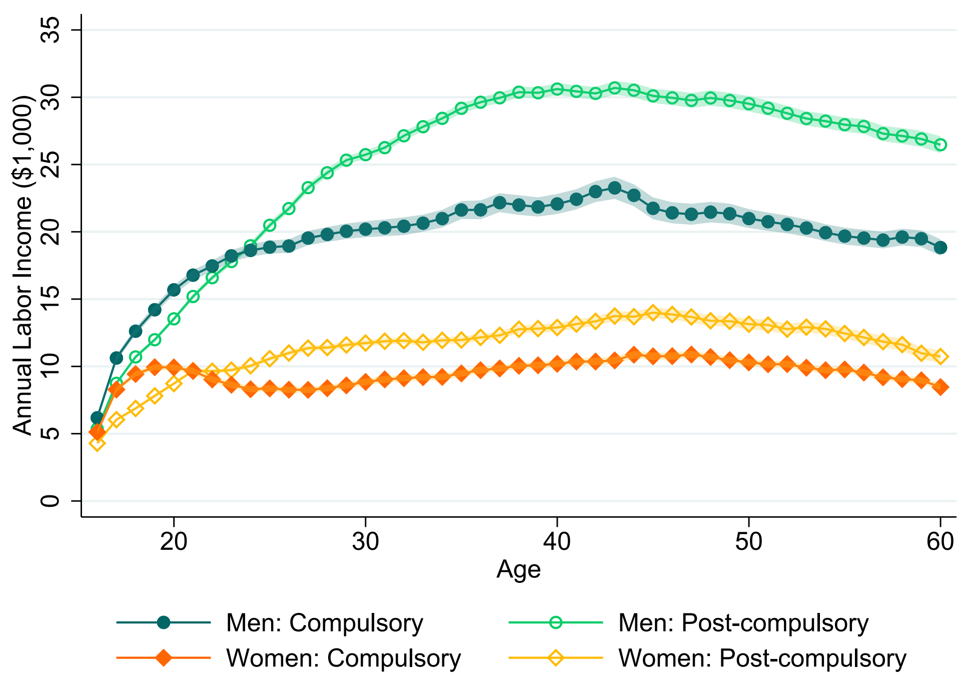


Figure A.16: Lifecycle Earnings Profiles by Education

Notes: The figure plots earnings profiles by education for men and women, separately for those who complete a post-compulsory degree compared to those who only complete 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. The shaded areas around each series display the 95% confidence intervals.

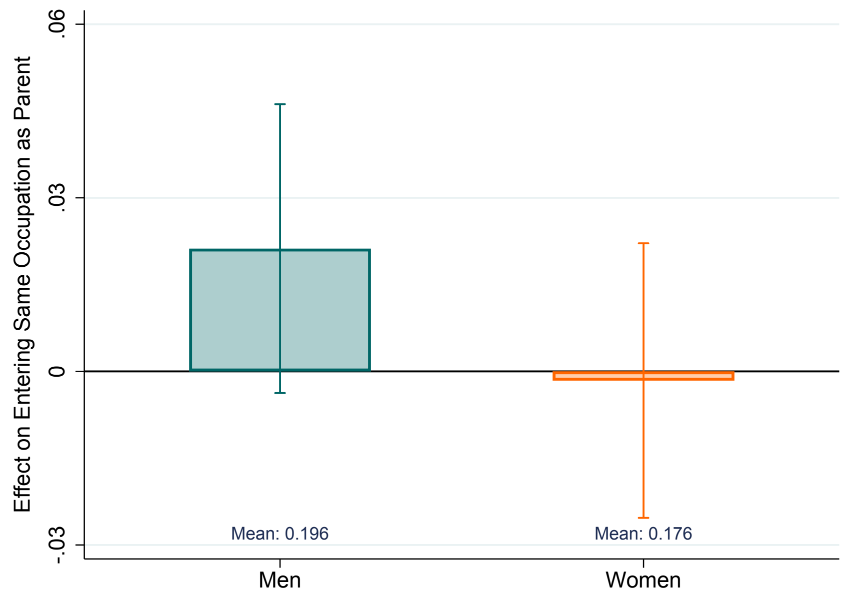


Figure A.17: Occupational Following

Notes: The figure plots the estimated effects of the tax-free year on children entering the same occupation as their parents. Estimates are based on equation (1) where the outcome is an indicator for a child holding the same occupation as either its father or mother during ages 16-19. Occupations are classified based on the Icelandic version of the ISCO-88 code, where occupations are organized within industry. I group together managers and foremen (occupations exclusively held by adults) with elementary or manual workers, which is the occupation primarily held by adolescents. That is, I assign children as working in the same occupation as their parents if they are elementary or manual workers within the industry where their parents are managers or foremen. Regressions control for pre-reform individual characteristics, occupation fixed effect, and municipality fixed effects. Below-threshold means of the outcome are reported in the graph. The whiskers display the 95% confidence interval.

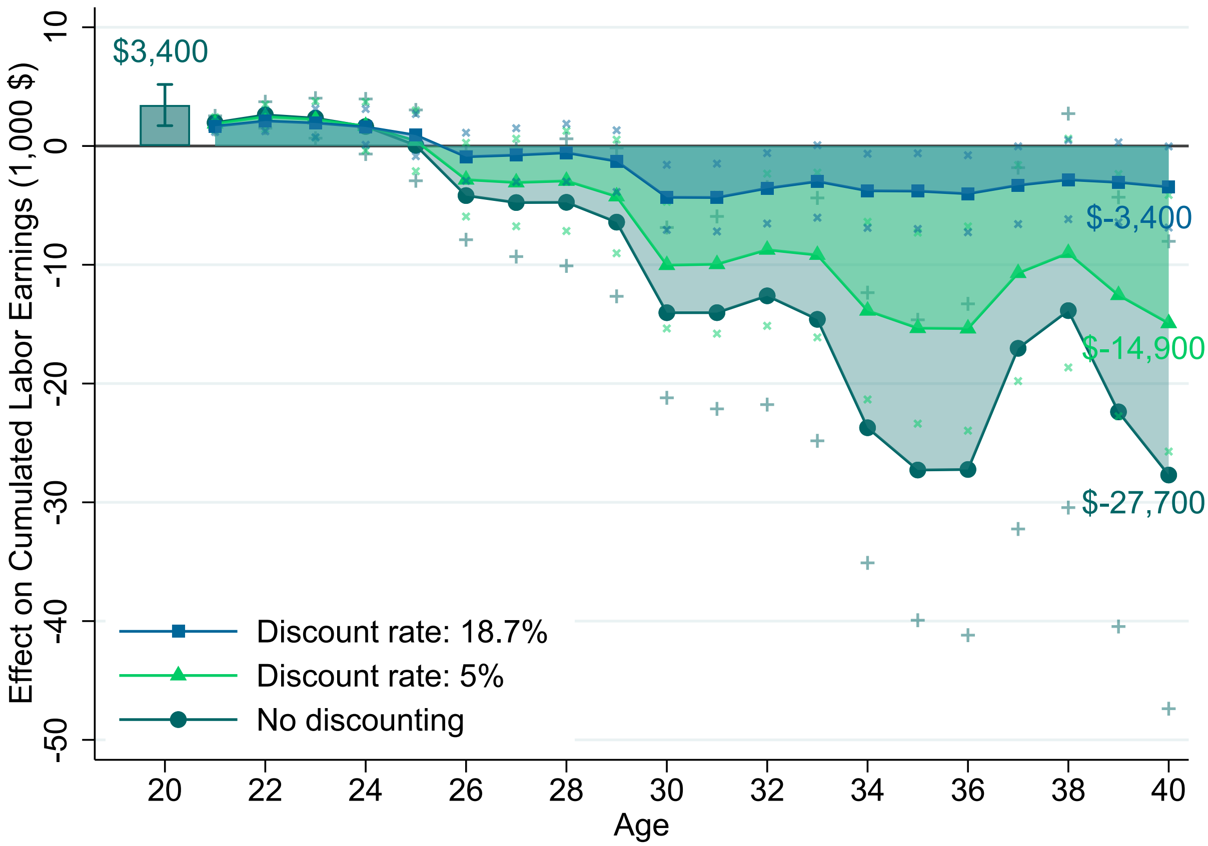


Figure A.18: Cumulative Labor Earnings and Implied Discount Rate

Notes: The figure plots the estimated treatment effect on cumulative labor earnings of men. The bar correspond to estimates of equation (1) on cumulative labor earnings over upper-secondary school age 16-20. The dots correspond to estimates of equation (1) 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 on accumulated labor earnings, discounted to age 21 using a discount rate that solves equation (9). 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.

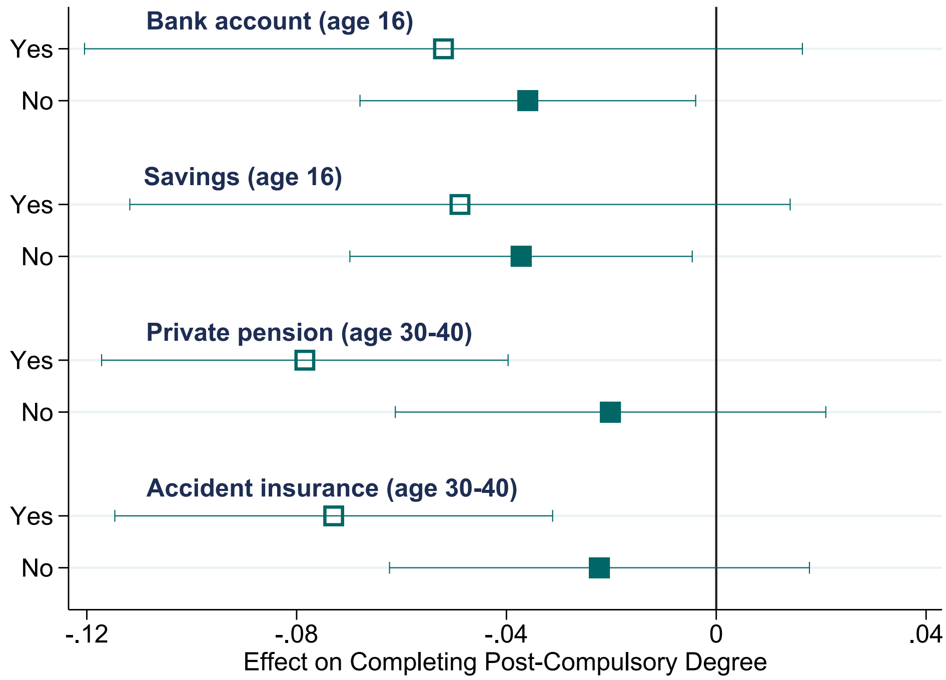


Figure A.19: School Dropout by Characteristics

Notes: The figure plots the estimated effects of the tax-free year on school dropout of men by characteristics associated with myopia. The characteristics are: (1) indicator for owning a bank account at the age of 16, (2) indicator for having some savings at the age of 16, (3) indicator for contributing to a private-pension plan at some point at age 30-40, and (4) indicator for purchasing accident insurance at some point at age 30-40. For each characteristic, students are split into two groups and equation (1) estimated separately for each group. Regressions control for pre-reform individual characteristics and municipality fixed effects. The whiskers display the 95% confidence interval.

E Supplementary Tables

Table A.5: 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.6: 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.7: Effects of School Dropout on Earnings

	Prime Age		Lifetime	
	Degree	Years	Degree	Years
	(1)	(2)	(3)	(4)
2SLS Estimate	42,214**	7,598**	461,409**	78,066**
	(20,795)	(3,737)	(190,401)	(33,906)
Implied Return	1.007	0.181	0.703	0.119
F-statistic	9.8	6.6	14.3	10.5
Outcome mean	41,927	41,927	656,154	656,154
Observations	76,269	76,269	15,026	15,026

Notes: This table reports estimates of reduced educational attainment due to school dropout on labor income of men. Educational attainment is measured either by the propensity of completing a post-compulsory degree (*Degree*), i.e. to not drop out, or by years of school completed (*Years*). The effect is estimated using a two-stage least squares (2SLS) version of RD regression equation (1) where the compulsory schooling age threshold indicator is used as an instrumental variable for the two schooling outcomes. Earnings are measured either as labor earnings at *prime-age* (average at ages 36-40) and *lifetime* earnings (cumulative from age 21 until age 40). *Implied Return* is measured by $\frac{\text{2SLS Estimate}}{\text{Outcome mean}}$, where outcome mean refers to 12-month below-threshold averages in the outcome variable. Therefore, this gives an estimate of the implied return in earnings to completing post-compulsory education or one additional year of schooling. 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