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Transitory Earnings Opportunities and Educational Scarring of Men

Abstract

Men have fallen behind women in education in developed countries. Why? I study the impact of a transitory increase in the opportunity cost of schooling on men's and women's educational attainment. I exploit a reform in Iceland that lowered income taxes to zero for one year and compare teenagers above and below the compulsory schooling age. This earnings opportunity increased the dropout rate and led to a permanent loss in years of education for young men, but had no effect on the education of women. Male dropouts suffer substantial losses in lifetime earnings, slow career progression, and reduced marriage and fertility outcomes. The results cannot be explained by negative selection of dropouts or low returns to education but can be reconciled by gender differences in nonpecuniary costs of school attendance, myopia, or perceived returns to education. The findings suggest that due to these gender differences, economic booms misallocate young men away from school, entrenching the gender gap in education.

JEL-Codes: H240, I210, I260, J160, J240.

Keywords: educational attainment, opportunity cost, gender gap, labor supply, tax reform.

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1 Introduction

The gender gap in education has reversed over recent decades, and now favors women in almost all developed countries (OECD, 2021). In the US, women had overtaken men in college education by the 1980s and since then their education advantage has continued to rise (Goldin, Katz, and Kuziemko, 2006). While women gained ground, men fell behind: high school completion and college attendance among men have declined and men who enroll in college are less likely to graduate than women.[1]

The implications of this gap depend on why men have fallen behind in education. Two competing explanations have been proposed (Goldin, Katz, and Kuziemko, 2006). First, the economic benefits of education may have risen more slowly for men than for women. Second, men may have higher nonpecuniary—or "psychic"—costs of schooling than women. Such differences could lead women to surpass men in educational attainment as returns rise for both genders. Moreover, high psychic costs may weaken school attachment. Consequently, men may abandon education at higher rates than women in response to changes in the opportunity costs of school attendance.

This paper presents novel evidence of the consequences for young men of abandoning school prematurely. I document how a transitory increase in the opportunity cost of school—the earnings students must give up to attend school instead of working—leads to a permanent "educational scarring" of men, but not women. The results cannot be explained by male dropouts being negatively selected or facing low returns to education, as dropping out leads to large earnings losses. However, the results are consistent with gender differences in the psychic costs of school attendance or in perceived returns to education, due to more impatience or erroneous expectations, leading men to leave school for short-term gains and to forgo large returns from completing education.

I exploit a tax reform in Iceland, which led to a year without taxation on earnings (Bianchi, Gudmundsson, and Zoega, 2001; Sigurdsson, 2022). 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. During the transition year 1987, no taxes were collected on the income earned that year to avoid simultaneously collecting taxes on two years of income. The tax-free year offers an ideal natural experiment, since it raised the opportunity costs of school attendance—the take-home pay that students could earn in the labor market—by about 10 percent, but only for a year.

To identify causal effects, I combine this natural experiment with a discontinuity in the compulsory schooling age. Using a regression discontinuity design, I compare teenagers who had turned 16, and therefore completed compulsory schooling and were free to leave school during the tax-free year, to students below the age threshold who were compelled to stay in school that

[1] In the U.S., there were 0.73 men for every woman graduating from a four-year college in 2020, declining from 1.60 in 1960 (U.S. Department of Education, 2021). Heckman and LaFontaine (2010) document that declining high-school graduation rates among men since the 1970s and growing among women can account for half of the growing gender gap in college education. Enrollment in U.S. postsecondary institutions has declined since 2010 with men accounting for the majority of the decline. Men who enrolled in a four-year college in 2014 were 10.3 percentage points less likely than women to graduate within four years (U.S. Department of Education, 2021).

year. Therefore, identification uses two features that are exogenous to schooling decisions and gender: the tax reform and students' dates of birth.

I document that the tax-free year increased dropout rates among men from four-year junior college, which is usually attended between the ages of 16 and 20 following completion of compulsory education. I estimate a 5 percentage point decrease in the completion of post-compulsory education or an 8 percent increase in the dropout rate when compared to the control group that was still in compulsory schooling at the time of the tax-free year. This effect is driven by dropout from academic tracks, which is the qualification channel for university, with no sign of dropout from vocational tracks. The same is not true for women, who were equally exposed to zero tax and already constituted a larger share of junior college students at the time. While men left school to take advantage of the higher wages in the labor market, women stayed in school and completed their education.

If leaving school is not an irreversible decision, it may be tempting to intertemporally substitute schooling for work during a year when working is unusually profitable. Hence, since students can delay schooling for a year or return to school later, dropping out of school to earn tax-free income may not have affected lifetime educational attainment. I find little empirical support for such intertemporal substitution. Tracking students over time, I document that most students who dropped out never returned to school.

The impact of dropping out on individual and social welfare depends on the returns to schooling for students who drop out when opportunity costs rise temporarily. On the one hand, if dropouts have low academic ability and returns to schooling (Willis and Rosen, 1979; Stinebrickner and Stinebrickner, 2012), or a comparative advantage in jobs normally done by non-graduates (Eckstein and Wolpin, 1999), they may fare better in the long run by entering the labor market earlier rather than staying longer in school. On the other hand, if students who drop out would have earned large returns on education, economic booms can have a "scarring effect" on human capital and earnings.

I find that dropping out of school leads to large losses in earnings. While male dropouts have higher earnings in the years after leaving school, they gradually fall behind in adulthood. Young men who had passed the compulsory schooling age at the time of the tax-free year earned on average $2,100, or 5.1 percent, lower incomes at prime age. Given the effect of the tax-free year on school dropout, I translate this reduced-form effect of the tax-free year to a causal effect of dropping out of school on earnings using birth date as an instrument for dropping out. I estimate that men who drop out of school would have increased their annual prime-age income by about $48,000 had they stayed in school, roughly doubling the income of the control group, and raised their lifetime earnings by about 75%.

Gradual earnings losses of dropouts relative to stayers reflect slow career progression in the labor market after entry. Male dropouts start out in occupations that require little education and pay a relatively high starting wage but offer limited opportunities for advancement. These are primarily manual jobs in fishing, manufacturing, and construction. At the prime age, dropouts are still disproportionately likely to hold such jobs. Looking at the quality of first jobs, dropouts start out in relatively productive firms, as measured by their AKM pay premiums (Abowd, Kramarz, and Margolis, 1999). However, they have slower advancement than those who stayed in school, receive fewer promotions, fall behind in pay rank within both their firm and profession, and work at less productive firms during adulthood.

In addition to having reduced economic success, men who drop out of school are also less likely to marry, less likely to have children, and have fewer children in total. These results mirror findings from other contexts. First, college graduates tend to marry college graduates, in part reflecting the role colleges play as local marriage markets (Nielsen and Svarer, 2009; Kirkebøen, Leuven, and Mogstad, 2021). Second, prior work has documented that a reduction in men's earnings, both in absolute terms and relative to women, reduces the pool of "marriageable men", leading to less marriage and fertility (Autor, Dorn, and Hanson, 2019). My study suggests that a growing gender gap in education could influence marriage and fertility rates.

To interpret my findings, I present a simple model of schooling choice that highlights the factors that influence the decision to drop out of school. The model features two dimensions of individual heterogeneity: academic ability and psychic costs of schooling. A model that does not include heterogeneity in psychic costs makes predictions that are inconsistent with the results. Such a model predicts that dropout leads to negligible earnings losses, as the marginal student is negatively selected on ability and returns to education. Instead, under heterogeneity in both academic ability and psychic costs, the model predicts that students who have high psychic costs relative to their returns to education drop out when opportunity costs rise. Moreover, the less correlated psychic costs and returns are, the larger the increase in dropout and the greater the consequential earnings loss.

The model provides a natural interpretation of the results, both in terms of why men, but not women, drop out of school during the tax-free year and why male dropouts incur large losses in earnings relative to their potential. The model implies that psychic costs are, on average, higher for men than for women and less negatively correlated with ability. In line with this interpretation, prior work has highlighted that psychic costs, approximated by non-cognitive skills, are higher for men than for women (Jacob, 2002), that the variance in non-cognitive skills is higher among men than women (Becker, Hubbard, and Murphy, 2010), and documented a lower correlation between cognitive and non-cognitive skills among men than women (e.g. Heckman, Stixrud, and Urzua, 2006b).

I empirically investigate two alternative or complementary explanations for my findings. As the model highlights, psychic costs, broadly defined, can be seen as a stand-in for factors that include myopia and misperceptions about returns to education. First, I document that for my findings to be rationalized only through time preferences, young men would have to be impatient and discount the future at an average annual rate of at least 19%, while young women are patient. Previous studies have documented that boys are more myopic than girls (Bettinger and Slonim, 2007), but also that gender differences in patience are strongly related to differences in school disciplinary and behavioral problems (Castillo et al., 2011), which, in turn, are related to measures of psychic costs (Jacob, 2002). Second, I evaluate whether the findings may reflect misperceptions about returns to education. Students' perceptions are likely to reflect the information available to them, such as that coming from peers or people they know in their community (Jensen, 2010). In line with this explanation, I document that teenage men, including dropouts, work in occupations that require little education but offer higher starting wages relative to jobs worked by young women. Moreover, I estimate that dropout is most prevalent among men from locations where the population is less educated and among those living outside the capital region.

This paper contributes to a literature studying the impact of economic conditions on educational attainment, assessing the impacts of local labor market conditions (Goldin and Katz, 1997; Shah and Steinberg, 2017), expansionary episodes such as WWII (Goldin, 1998), natural resource booms (Black, McKinnish, and Sanders, 2005; Emery, Ferrer, and Green, 2012; Cascio and Narayan, 2020), and business cycles (Gustman and Steinmeier, 1981; Kane, 1994; Betts and McFarland, 1995; Card and Lemieux, 2001; Johnson, 2013). Perhaps closest to my paper are Atkin (2016), who studies the impact of new factories opening in Mexico, and Charles, Hurst, and Notowidigdo (2018), who study the impact of labor market opportunities arising from housing booms across the US. Both studies found permanent negative effects on educational attainment. However, the question that remains largely unanswered is whether the educational attainment decline from such episodes has negative long-term effects on individual and social welfare, which depends on the returns to education for marginal students. My results highlight that evaluating the short-term rather than the long-term impact of negative schooling effects will likely lead to incorrect conclusions about welfare consequences.

More broadly, my findings also contribute to the literature on the growing gender gap in education. While it is well documented that men have fallen behind women in education and that this pattern is found in nearly all OECD countries, there is no consensus on the causes of this and the consequences remain poorly understood (Goldin, Katz, and Kuziemko, 2006).[2] My results are inconsistent with explanations emphasizing that men leave school because of low returns to education. This is in line with prior work finding limited empirical support for a differential education premium for men and women (Hubbard, 2011). Instead, my results are consistent with estimates from structural models suggesting that psychic costs must be large in order to rationalize why many people do not pursue further education despite facing large financial gains from doing so (Heckman, Lochner, and Todd, 2006a). Moreover, my results demonstrate a channel through which gender differences in psychic costs, combined with fluctuations in opportunity costs and other external influences, can contribute to the growing gender gap in education.

My findings have macroeconomic implications. Much of the work on the welfare costs of business cycles has focused on variations in aggregate consumption (Lucas, 1987, 2003), long-run

[2]This open question is well-framed by Goldin (2021): "*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.*"

effects of disruptions caused by recessions (De Long et al., 1988), and long-term earnings losses associated with entering the labor market during downturns (Von Wachter, 2020). This study demonstrates how economic booms, as well as recessions, may be disruptive. Prior theoretical (Rogerson and Wallenius, 2009) and empirical (Sigurdsson, 2022) studies have demonstrated how extensive-margin labor supply adjustment to transitory wage changes is driven by individuals at the two tails of the employment lifecycle. The results of the current paper demonstrate that this adjustment can have negative and permanent side effects, as students who leave school to enter the labor market when wages are temporarily high may never return to complete their education. Back-of-the-envelope calculations suggest that dropout during the 1987 tax-free year resulted in a 0.1-0.2 percent loss in GDP in 2020. Moreover, I estimate that if the compulsory schooling age were one year higher, this loss would have been reduced by almost half.

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 present the effects 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

The Icelandic Finance Minister announced a tax reform on December 6, 1986. The new system, a withholding-based pay-as-you-earn income tax system, replaced the system in which income taxes were collected with a one-year lag. I illustrate this in the top panel of Figure 3. The new system is similar to that of most advanced economies. To ensure that during the transition, workers would not have to pay taxes simultaneously on their 1986 and 1987 earnings, no taxes were collected on 1987 labor income. In other words, all labor income earned in 1987 was tax-free.

The 'tax-free year' generated a strong incentive for intertemporal substitution of work. The average tax rate fell to zero from about 10 percent, sharply but only temporarily increasing the incentive for people to enter the labor market and work longer hours. This incentive was salient. The tax reform and its implications for net-of-tax earnings received much media attention, 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. Therefore, it is likely that most adults, including students, were aware of the reform. As for older cohorts, the tax reform temporarily raised young workers' after-tax wages. As summarized in Appendix Table 1, the average tax rate paid by 16-20 year old workers in the years prior to the reform was 8.3% and the marginal tax rate they faced was 15.8%. In 1987, all labor income taxes were set to zero. For further details about the tax reform, including the background and timeline, see Sigurdsson

(2022).

2.2 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.

As documented in Sigurdsson (2022), the youngest cohort, aged 25 and younger at the time, displayed the strongest labor supply responses to the tax-free year and essentially drove the extensive-margin response. In all likelihood, this group of workers included students at upper secondary schools or universities. These students may have worked longer hours while at school or during teaching breaks, which was not uncommon among teenagers at this time (Rafnsdóttir, 1999), or delayed schooling to enter the labor market as full-time workers. Consistent with this, newspaper coverage in December 1986 and January 1987 indicated that there was an immediate worry that the tax-free year would tempt students to take time off from school. This fear was strengthened in January 1987, when several newspaper articles reported that an unusually high share of junior college students did not return to school after the holiday break. In addition, the enrollment statistics show a reduction in the number of university students enrolled during the tax-free year, as documented in Appendix Figure A.1, and the history of the University of Iceland discusses the reduction in enrollment and the increase in dropout (Hálfdanarson et al., 2011).[3]

2.3 The Education System

The Icelandic education system transitioned into a mature, flexible, and advanced system during the 20th century, similar to the system in other Nordic countries. As outlined in the Constitution, the cornerstone of this system is that Icelandic children and young adults should have equal rights to education at both compulsory and upper-secondary levels.

The Icelandic education system has an elective preschool level, and three main levels. At the age of 6, children enter primary school and complete their compulsory education by finishing lower secondary school 10 years later, at the age of 16. After completing compulsory education, everyone can enter upper secondary school, where essentially no tuition is charged. All students have the right to upper-secondary education irrespective of their academic performance in lower-

[3] As Appendix Figure A.1 documents, during the academic year 1986/1987 there were 0.9% fewer university students registered compared to the year before, and an even further contraction in the number of students in 1987/1988 when the number of students fell by 3.1% from the year before. As I will describe, while the educational system offers a natural control group for people of upper-secondary school age in 1987, there is no such natural control group for university students. Therefore, my analysis is focused on delay and dropout from upper-secondary school.

secondary school, although their options depend on their earlier performance, as upper-secondary schools base their selection of students on scores from tests taken at the end of the 10th grade.

Upper secondary education can be classified into the following three types. Grammar schools offer four-year academic programmes that conclude with matriculation examinations. Completing the matriculation examination is a prerequisite for university enrollment. Vocational schools offer theoretical and practical programs for studying skilled industrial trades, specialized employment, and some non-skilled trades. Comprehensive schools provide academic programs with matriculation examinations, similar to grammar schools, as well as vocational programs similar to those offered by industrial vocational schools. The four-year academic programmes offered by grammar and comprehensive schools are generally referred to as junior colleges. Although these programs lead to further education by law, all upper secondary study programs lead to further education either directly or through defined additional studies (Ministry of Education, Science and Culture, 2002).

At the age of 16, after completing compulsory education, students can choose whether to continue into upper secondary education or leave school. 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). 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.

The upper secondary level mostly includes students aged 16 to 20 who mostly enroll in four-year programs, with some notable exceptions involving shorter programs. This means that most students are expected to complete their studies at the age of 20. However, a significant fraction of students do not complete upper secondary school by the age of 20; many drop out, and many re-enter the education system and finish later, especially those in vocational education (Blöndal et al., 2011). Studies of Icelandic cohorts born in the 1960s, 1970s, and the 1980s documented an average dropout rate of approximately 30% (Blöndal, 2014; Statistics Iceland, 2008).

2.4 Gender Differences in Educational Attainment

As in most other industrialized countries, Icelandic women now complete more education than Icelandic men. Junior college graduation rates were at parity in 1975, and since then, the share of women graduating with junior college degrees has risen steadily faster than that of men. In 2020, there were 0.68 men graduating for every woman who graduated. Similarly, the share of men and women in universities was equal in the mid-1980s; however, in 2020, there were almost twice as many women as men graduating with a bachelor's degree (Statistics Iceland, 2022b).

The widening education gap reflects differences not only in enrollment rates, but also in dropout rates. The share of male students who drop out of post-compulsory education is higher than that of female students, and this difference has remained consistent during recent decades. Among students enrolled in academic programs, the dropout rate among men in 2020 was 21.1 percent compared to 12.9 percent among women. The dropout rate among vocational education students was higher (34.4 percent among men and 32.5 percent among women) (Statistics Iceland, 2022a).

2.5 Empirical Strategy

My empirical strategy uses two features, as I illustrate in Figure 3. 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 age of 16 and students must attend school until they reach that age. This means that those for whom school is no longer compulsory at the time of the tax-free year, who may have begun post-compulsory education, will have the option of delaying school or dropping out to work, and earning tax-free income. Conversely, those still in compulsory education 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:

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

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

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

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 were 16 years or older in December 1986 and therefore had completed compulsory schooling in 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, 16-20. 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).

2.6 Data and Sample

The data I use come from administrative records covering the Icelandic population and consists primarily of three 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, but 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 than one level and each level is registered.[4] 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 B. 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 collected for wealth tax. The third dataset is employer-employee data constructed from digitized payslips, containing information on employment, labor earnings, related compensation, and working time. For further information on these datasets, see Sigurdsson (2022).

My analysis sample consists of all individuals in the birth cohorts who were around the compulsory schooling age of 16 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.

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

3 Effect on School Dropout and Educational Attainment

In this section, I analyze the effect of the tax-free year on school dropout and study the permanent effects on educational attainment.

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

Figure 4a reveals a clear and discrete drop in educational attainment at age 21 among individuals who had passed the compulsory schooling age threshold by the time of the tax-free year. Measuring dropout as not completing post-compulsory education, Figure 4b documents an increased dropout from school during the tax-free year.

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

Figure 5 contains plots, separated by gender, of the age-specific RD estimates of the effect on post-compulsory education and years of schooling.[6] 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

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

[6] 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.

substitution, this reduction in educational attainment is stable, persistent, and not reversed by students returning to junior college the following year or later in life. While there is some evidence that students complete vocational education in their late 20s, this effect is relatively small and not precisely estimated.[7] 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.[8]

Panels (c) and (d) in Figure 5 plot the estimated effects on school completion and years of schooling of women. In stark contrast to the effect on men, the figure reveals no sign that women dropped out of school. Even in the short run, there is no evidence 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 5, 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.

Magnitude The estimated effects on school dropout and educational attainment are sizable. To put these estimates into perspective, I first compute the elasticity of school dropout. I measure the change in the opportunity cost of schooling as the change in the financial incentive to work generated by the tax-free year. Table 1 documents the average and marginal tax rates of employed 16-20-year-olds prior to the reform. For men, the average tax rate was 10.1%. This implies a dropout semi-elasticity of $\frac{-0.050}{\log(1-0.101)} = 0.47$.

The effects I estimate can be compared with estimates from existing studies on the long-run impacts of education policies on schooling. For example, Fredriksson et al. (2013) study the long-run effects of class size in primary schools in Sweden. They estimate that increasing class size by one pupil decreases years of schooling by 0.05 years (Table V). In comparison, I find 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 perform a series of tests to evaluate the validity and robustness of these results. First, I evaluate the sensitivity of the estimates to the choice of bandwidth around the compulsory school age threshold and the degree of polynomials controlled for in the regres-

[7] Appendix Figure A.5 plots the dynamic effect on post-compulsory education and quantifies this small increase in vocational education when individuals are in their late 20s. As I will document, male dropouts disproportionately entered manufacturing and construction jobs. This pattern in Figure 5a might indicate that some of these dropouts return to school to get formal training and certification after having worked in these jobs for several years.

[8] 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.

sions. As a reminder, in the main specifications, I use 48 months on each side, which implies that, to the right of the threshold, I include everyone at the normal upper-secondary school age (age 16-20) and a similar mass of individuals to the left of the threshold. In Appendix Figure A.6 I evaluate the sensitivity of my estimates to this choice by varying the bandwidth from 4 to 60 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 somewhat larger than the main estimates but less precisely estimated than under the chosen bandwidth. At bandwidths beyond one and a half years, the estimates are stable and similar to the estimates using the chosen bandwidth. Estimates using linear or quadratic polynomials are almost identical in most cases. Appendix Table A.3 presents further specification checks for the estimation of equation (1), using the bias-corrected RD estimator of Calonico et al. (2014) and varying controls and kernel weights across specifications. I find that the estimates are generally similar across specifications, both in terms of the size of the point estimates and statistical significance.

A threat to the validity of the research design would be if certain parents systematically time their births in such a way that their children's ages coincide with passing the compulsory school-age threshold by the time of the tax-free year. This seems particularly unlikely in this setting, as the implication of being born before or after December 1971 was not revealed until the tax reform was announced in 1986. Appendix Figure A.7 presents evidence that there is no evidence of such behavior, showing that there is no suspicious jump in the number of births at the threshold.

To further evaluate the validity of the research design, I performed placebo tests in which I tested for discontinuities at the relevant age thresholds but during placebo-tax-free years. The results of the placebo tests are presented in Appendix Figure A.8. I report estimates for eight placebo-year thresholds, four before the actual tax-free year and four after, and test for discontinuities in both years of schooling and completion of junior college and vocational degrees. As shown in the figure, these tests revealed no false positives, confirming the validity of the research design. All placebo coefficients are close to zero and never statistically different from zero.

4 Effects on Labor Market and Socioeconomic Outcomes

In the previous section, I documented that for a share of young men the tax-free year marked an end to their education; they dropped out of school and never returned. I now study the consequences of this on their labor market and socioeconomic outcomes in the short and long run.

4.1 Income and Employment

Nonparametric Graphical Evidence Figure 6 examines nonparametrically the effect of the tax-free year on labor earnings at ages 16-20, i.e., when individuals are at upper-secondary schooling age. Panels (a) and (b) present results for men and women, respectively. The figure reveals a clear discrete jump in labor earnings of men that were above the compulsory-schooling age threshold during the tax-free year. For women, there is only a small increase in earnings at the threshold. Appendix Figure A.9 displays a similar discrete jump in employment, defined as individuals earning at least a minimum income.[9] In conjunction with the estimates presented in the previous section, these results are consistent with young men dropping out of school to work and earn tax-free income.

Reduced-form Estimates Figure 6, panel (c), presents estimates of the impact on labor earnings over the lifecycle.[10] The figure presents estimates of regression equation (1) of the average annual earnings at 5-year age ranges, from 16-20 to 36-40, separately for men and women. Focusing first on the effect on labor earnings at upper-secondary school age, quantifying the discontinuity displayed in Figure 6, panel (a), I estimate that the discontinuity amounts to $838 or about an 8 percent increase in the annual earnings of 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. Documented in Appendix Figure A.9, the jump in the employment rate of men amounts to 5 percentage points or about a 12 percent increase compared to the below-threshold average. The figure also documents an increase in the employment rate of women by 2 percentage points. When compared with 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 of this transitory earnings opportunity.

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 the students that drop out during the tax-free year are in this way negatively selected in terms of academic ability and returns, 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 schooling, dropping out will lead to earnings losses.

Figure 6, panel (c), shows that while male dropouts gain in the short run, their earnings gradually fall behind the earnings of the control group during adulthood. During the early 20s, there is a positive but not statistically significant effect on earnings, reflecting that those that stay and complete junior college are also more likely to continue to university, as well as the dropouts are accumulating experience on the labor market which yields a return. However, by the time men reach 40, the loss in annual earnings has reached about $2,100, or a 5.1% loss 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 (Appendix Figure A.9). In

[9] 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 most students earn some income every year. 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.

[10] Appendix Table A.4 presents regression estimates of the effect on labor earnings and employment for men and women, both at upper secondary school age and prime age.

comparison, the corresponding estimates for women are close to and not statistically different from zero.

Figure 6, panel (d), 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 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, men who were above the compulsory schooling age during the tax-free year have on average lost $27,700, or 4.2 percent, in cumulative lifetime earnings.

I refer to the estimates of effects on earnings until age 40 as the effect on lifetime earnings. This notion is in line with prior evidence indicating that the majority of lifetime wage growth occurs during the first ten years on the labor market (Topel and Ward, 1992). However, this is likely to reflect a lower bound. Appendix Figure A.11 plots Mincer earnings profiles by age, education, and gender for cohorts born between 1947 and 1977. It shows that the earnings differences between those who complete post-compulsory schooling and those who do not are increasing, and reach their peak at around age 50.

IV Estimates The estimates reported above reflect the reduced form effect of the tax-free year on earnings. To capture the effect of school dropout during the tax-free year on earnings, I estimate a two-stage least squares (2SLS) regression of educational attainment on earnings using date of birth as an instrument for educational attainment. That is, the first stage is the effect on educational attainment estimated with RD regression equation (1), and the second stage is a regression of earnings on educational attainment instrumented with an indicator for being above the compulsory schooling age at the time of the tax-free year. This estimates the earnings loss caused by dropping out of school during the tax-free year. Under the exclusion restriction that the tax-free year only affected adult earnings through its effect on education, this estimate measures the implied returns to education for those who dropped out.

The results are presented in Table 3. I measure labor earnings in two ways: earnings at prime age, measured at ages 36-40, and lifetime earnings, measured as cumulative earnings from age 21 until age 40. For men, the estimates imply that completing post-compulsory education instead of dropping out would have increased annual prime-age earnings by $48,600, or a doubling of earnings when compared to average earnings for the control group, i.e. below the age threshold. The implied return to an additional year of schooling is a $9,400, or 22.5 percent, increase in annual prime-age earnings. The impact on lifetime earnings is about $500,000, meaning that had the dropouts stayed in school their lifetime earnings would have been about 75% higher than otherwise. For women, the corresponding estimates are much smaller and not statistically significant, in line with the results presented earlier.

These estimates imply that young men suffer large earnings losses from dropping out of school too early. If these compliers were a negatively selected sample of students in terms of abilities and returns to schooling, these estimates should be small or negative. While large, the magnitudes are consistent with some related previous findings. Exploiting changes in compulsory schooling laws, Oreopoulos (2007a) estimates an increase in lifetime income of roughly 17% for individuals who stay in school an extra year instead of dropping out at age 15.[11] Cadena and Keys (2015) study the consequences of impatience and document a more than 50 percent high school dropout rate among impatient individuals and 13 percent lower lifetime earnings. Nakamura, Sigurdsson, and Steinsson (2022) exploit a natural experiment—a volcanic eruption in Iceland—to study the impact of geographic mobility on educational attainment and earnings in adulthood. When interpreted in the same way as above, the results imply that returns to one additional year of school are 23%. However, those results suggest that there are important interactions between location and returns to education and that those that moved as a result of the eruption are likely to have a comparative advantage in activities that require higher education. From the perspective of these results, the results above suggest that students who dropped out of school during the tax-free year are positively rather than negatively selected in terms of academic ability and are likely to have a comparative advantage in occupations that require post-compulsory education.

Wealth 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 Figure 7. 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 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.

Robustness I evaluate the sensitivity of the results in several ways. First, I evaluate the sensitivity of the estimates to the chosen regression specification, including bandwidth around the compulsory schooling age threshold and polynomial degrees. Appendix Figure A.12 plots the estimated effects on labor earnings and employment using a bandwidth around the age threshold ranging from 4 to 60 months. At smaller bandwidths, the estimated effects are somewhat larger but less statistically precise; however, at bandwidths of more than one-and-a-half years, the point estimates are stable. This is independent of whether a linear or quadratic trend is used.

Appendix Table A.5 contains the results of further tests of robustness to specification. It shows the estimates of equation (1) using the bias-corrected RD estimator of Calonico et al. (2014) and varying controls and kernel weights across specifications, demonstrating that the estimates are robust to specification.

To further evaluate the robustness of the estimated effects on earnings and employment, I es-

[11] Earlier estimates of gains in adult earnings from increased compulsory schooling range from 10-14% (see, e.g., Acemoglu and Angrist, 2000; Harmon and Walker, 1995; Oreopoulos, 2006).

timate difference-in-discontinuity between men and women using regression equation (1) with regressors interacting with a male indicator. In light of the results in Section 3 that men, but not women, dropped out of school during the tax-free year, women can be used as a control group for men when evaluating the impact of dropout on earnings and employment. The results presented in Appendix Table A.6 show magnitudes similar to the regression discontinuity estimates presented above.

4.2 Entry Jobs and Career Progression

I now study the impact of school dropout on entry jobs and subsequent career progression to shed further light on the large lifetime earnings losses that result from dropping out of school prematurely.

Entry Jobs and Occupation Figure 8 presents the estimated effect on first jobs, measured as the employment effect in each occupation at upper secondary school age. Each bar represents, separately for men and women, the employment share in each occupation as well as the estimated increase in employment in each occupation. Occupations are ordered along the x-axis by the pre-reform rank of average income among 16-20 year-old employees. For example, prior to the reform, 16-20 year olds earned the highest income if employed in fishing, and the lowest if employed in child and elderly care.

Young men who dropped out of school during the tax-free year took up jobs in fishing, fish processing, manufacturing, and construction. These jobs do not require much formal education and pay a relatively high starting wage compared to other jobs available to teenagers, but offer limited opportunities for a career. Figure 8 shows an increase in the employment of women as shop assistants in retail and in child and elder care. In light of the results documenting no evidence of dropout among women, this indicates that women took the opportunity to work in these occupations alongside school and earn tax-free income.

Figure 8 shows an interesting difference between the employment of male and female teenagers. Men tend to work in relatively well-paying occupations that offer permanent employment opportunities, whereas women tend to work in relatively low-paying occupations with fewer options for permanent employment. While the tax-free year generated the same opportunity for men and women to earn tax-free income, the employment opportunities for men were different, possibly explaining in part the gender difference in school dropout.

Appendix Figure A.13 presents evidence that suggests that these early occupational choices of male dropouts are persistent.[12] At prime working age, they are disproportionately employed in agriculture, fishing, manufacturing, construction, and wholesale and retail trade.

Career Progression and Firm Quality I next study the quality of the first jobs that young men who drop out take and their subsequent career progression. I measure firm quality with the rank

[12]Jobs at prime age are classified by sector of employment, rather than occupation. The reason is that there is not a consistent occupation measure for the years when workers are at prime age.

of the firm AKM (Abowd, Kramarz, and Margolis, 1999) fixed effect, capturing the pay premiums of firms. Similarly, I measure the pay premiums of sectors using the sector AKM fixed effect, i.e. replacing firms of employment with sectors in the AKM regression. To measure individual workers' relative positions and career progression, I used the within-firm and within-sector earnings ranks of individuals.

Figure 9 presents the effects on firms' pay premiums and career progression. The figure shows that dropouts do not seem to enter the labor market through employment in lower-quality firms compared to the control group. At age 21-25, I estimate that they are employed in firms with pay premiums that rank about 1.5 percentiles higher than the firms where the control group is employed. Appendix Figure A.14 documents a similar result for the sector of employment. Moreover, at prime age, dropouts are not employed at lower-wage firms or sectors, indicating that their earnings loss is not a result of them staying at or moving to lower-quality workplaces. Instead, dropouts gradually fall behind in terms of relative earnings, within both their firms and their sectors. At prime age, workers in the treated group rank about 2 percentiles, or 4.3 percent, below the control group in terms of earnings within their firms. This implies that dropouts have experienced fewer promotions and slower career advancement at prime age than if they had stayed in school. These results stand in contrast with the literature on the impact of graduating in a recession, which finds that an important aspect of earnings loss is due to unlucky labor market entrants starting and staying longer at lower-quality firms and lower-paying occupations and sectors (Oreopoulos, Von Wachter, and Heisz, 2012; Von Wachter, 2020). Earnings losses of dropouts seem to reflect, at least in part, reduced career progression, in line with prior empirical work that has documented that the majority of lifetime earnings growth of young men occurs through career advancement during the first ten years in the labor market (Topel and Ward, 1992).

4.3 Marriage and Fertility

There is a widespread idea in economics and sociology that changes in men's economic prospects affect marriage. In the classic Becker (1973) framework, gains from marriage arise from spousal earnings differences and the possibility of specialization, and a fall in the economic stature of young adult men, therefore, reduces women's gains from marriage. Relatedly, the seminal works of Wilson and Neckerman (1986) and Wilson (1987) argue that a decline in employment and earnings of less-educated men reduces the pool of economically secure—"marriageable"—men.[13] More recent work focusing on education has documented educational assortative mating (Mare, 1991; Pencavel, 1998) and found that colleges act as marriage markets (Blossfeld, 2009; Nielsen and Svarer, 2009; Kirkebøen, Leuven, and Mogstad, 2021).

In light of these theories, in Figure 10 I study the impact on marriage and fertility. Male dropouts were less likely to marry. Relative to the below-threshold average marriage rate, men

[13]As thoroughly discussed in Autor, Dorn, and Hanson (2019), while Becker's theory focuses on relative income of couples, Wilson (1987) focuses on absolute income of men. Similar to that in Autor, Dorn, and Hanson (2019), in my setting there is both an absolute fall in earnings of male dropouts as well as relative to women, for whom I find no evidence of permanent school leaving or earnings losses.

above the threshold are roughly 6 percent less likely to be married by the age of 40.[14] However, there is no statistically significant effect on marriage for women. Furthermore, men in the treated group have fewer children and are less likely to have children. By age 40, men are 4.8 percent less likely to have had children, and during their life, they have about 0.10, or 5 percent, fewer children. In clear contrast, there is no reduction in fertility among treated women, as point estimates are very close to and statistically indistinguishable from zero.

These results indicate that young men who drop out of school not only suffer losses in the labor market but also in other areas of adulthood. These results mirror earlier research that found that reduced earnings and employment of young adult men due to a decline in the coal and steel industries or rising import competition from China also reduces their marriage and fertility (Black et al., 2003; Autor et al., 2019). In the same spirit, Kearney and Wilson (2018) document that the US fracking boom increased the fertility of low-educated men, and Schaller (2016) found that improvements in men's labor market conditions and earnings are associated with increased fertility.

Might the effects on marriage and fertility be explained by an income effect? That is, if children and marriage are "normal goods" a fall in income will lead to less fertility and marriage (Becker, 1960), rather than being directly influenced by leaving school earlier. I investigate this in Appendix C by comparing my estimates to prior studies estimating the effect of windfall income, such as lottery winnings, on marriage and fertility. The estimates are qualitatively in line with previous estimates, although somewhat larger in most cases, implying that the income effects from earnings losses may explain the effects on marriage and fertility.

5 Model of School Dropout

To help interpret the empirical results, I now present a simple model of how changes in the opportunity cost of school affect dropout and its long-run consequences. Building on the canonical model of human capital investment (Becker, 1962; Mincer, 1958), this is a model of how individuals forego earnings for schooling today in exchange for returns on education in the future, incorporating both heterogeneous returns and costs of schooling.

5.1 Setup

There is a unit mass of infinitely-lived individuals who have completed compulsory schooling. In period $t = 0$, they choose between continuing schooling or leaving school to work. In periods $t = 1, \ldots \infty$ everyone works, and the educated ones earn a return on their schooling investment. Individuals can neither save nor borrow and derive utility from consuming their disposable income every period. Therefore, they choose their level of schooling to maximize the present discounted value of lifetime earnings, net of costs, discounting the future with a factor $\delta \in (0, 1)$.

[14] During 1991-2000, when the treated group is in their 20s and early 30s, the average age at first marriage among men was 31.2 and 29 among women. During the same period, the average age of parents at the time their first child is born was 27.6 years for fathers and 24.9 years for mothers. The average fertility was 2.1 children.

Individuals are characterized by two features: economic ability, θ, and cost of learning, κ. The cost is measured in monetary values and may include both homogeneous direct costs, such as tuition fees, and heterogeneous costs, such as psychic costs. One interpretation of these two dimensions of heterogeneity is that they reflect heterogeneity in cognitive and non-cognitive skills. Individuals without education earn a fixed income in every period, which I normalize to unity. Educated individuals earn a return proportional to their economic ability, $\rho\theta$. Income is subject to tax, τ, which I assume, for simplicity, to equal zero in periods $t > 0$. This gives the following income streams conditional on education choice:

$$\text{Continue school:} \qquad -\kappa + (1 + \rho\theta) \sum_{t=1}^{\infty} \delta^t \tag{2}$$

$$\text{Leave school to work:} \qquad (1 - \tau) + \sum_{t=1}^{\infty} \delta^t \tag{3}$$

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, including the psychic cost. This provides the following condition for continuing in school:

$$\underbrace{\frac{\delta}{1-\delta}\rho\theta}_{\text{Marginal benefit}} \geqslant \underbrace{(1-\tau)}_{\text{Opportunity cost}} + \underbrace{\kappa}_{\text{Direct/psychic cost}} \tag{4}$$

Figure 1 shows the equilibrium school choices in this model. The figure illustrates how individuals' choices between school and work are determined by their ability (θ) and cost of schooling (κ). Conditional on the cost of schooling, individuals with sufficiently high abilities choose to pursue education. Conditional on their abilities, individuals with a sufficiently high cost of schooling choose to leave school to work.

I now study the impact of an increase in opportunity costs generated by a decrease in the tax rate on dropout. I will study this under different assumptions about heterogeneity in ability and costs, as well as other factors that can influence dropout.

5.2 Heterogeneity in Ability

I first study the impact of a decrease in the tax rate when individuals are heterogeneous in ability, θ, and therefore return to schooling, but the cost of schooling is homogeneous. From (4) we can solve for the ability of the marginal individual who stays in school:

$$\theta^* = \frac{((1-\tau)+\kappa)(1-\delta)}{\delta\rho}$$

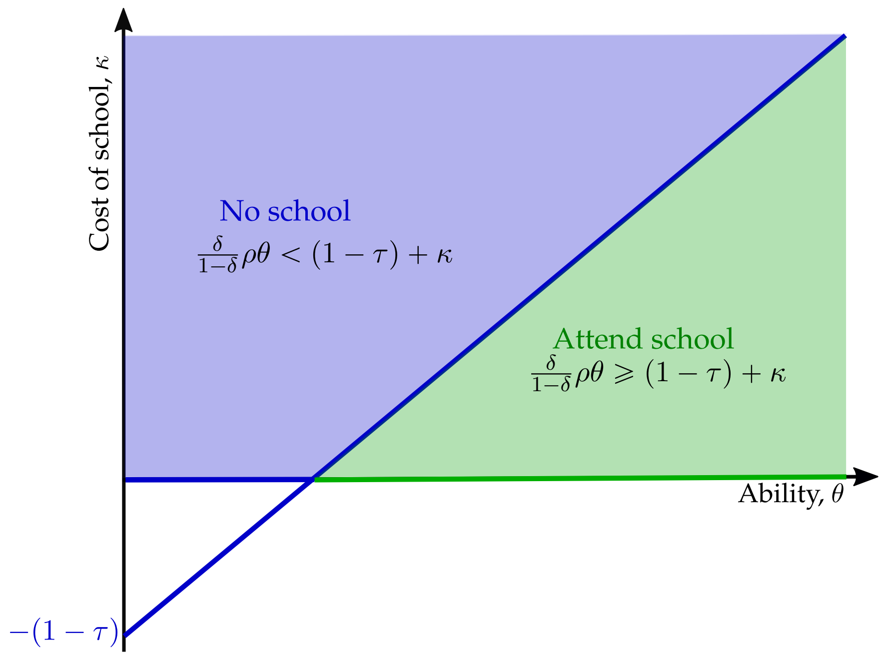


Figure 1: Equilibrium School Choice

Individuals with abilities below θ^* will leave school. Taking the derivative with respect to the tax rate gives the change in the share of such individuals when taxes increase, that is:

$$\frac{\partial\theta^*}{\partial\tau} = -\frac{(1-\delta)}{\delta\rho}$$

The following conclusions can be drawn. First, a decline in the tax rate raises θ^*, leading more individuals to drop out. Second, individuals who drop out of school are negatively selected on ability. They have low returns to education and earn no returns after accounting for direct costs. While the former conclusion is consistent with the empirical results, the latter is inconsistent. Therefore, heterogeneity in ability alone cannot explain my empirical findings.

5.3 Heterogeneity in both Ability and Psychic Cost

I now assume that individuals are heterogeneous in both ability, θ, and cost of schooling, κ, owing to heterogeneity in psychic cost. I make two assumptions about the distribution of θ and the relationship between θ and κ. First, I assume that θ is uniformly distributed, as follows:

$$\theta \sim U[0,1] \tag{5}$$

Second, I assume that ability and psychic costs are related, as follows:

$$\kappa = \kappa^0 + b\theta \tag{6}$$

It is useful to give benchmarks for how the correlation between θ and κ depends on parameter values. θ and κ are said to be more (less) correlated the larger (smaller) b is, and perfectly positively correlated when $\kappa^0 = 0, b = 1$:

$$\kappa = \kappa^0 + b\theta \Rightarrow \kappa = \theta$$

and perfectly negatively correlated when $\kappa^0 = 1, b = -1$

$$\kappa = \kappa^0 + b\theta \Rightarrow \kappa = 1 - \theta$$

I now study the effect of a decrease in the tax rate on dropout and earnings loss. As before, a decrease in the tax rate leads to dropout. However, as I show, under heterogeneity in both ability and psychic cost, the dropout propensity and earnings loss of dropouts depend on the correlation between θ and κ. This result is stated in the following proposition:

Proposition 1. *A decrease in the tax rate, τ, leads to a higher rate of school dropout and larger earnings losses among dropouts the higher the correlation is between θ and κ.*

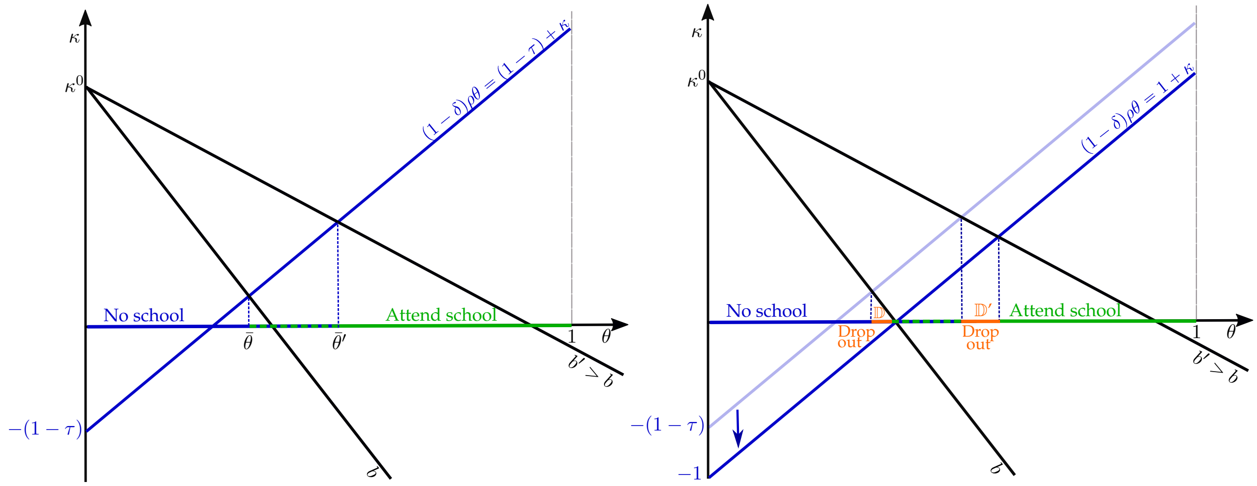
For proofs of propositions and additional model results, see Appendix A. The result summarized in Proposition 1 is illustrated with a graph in Figure 2. In panel (a), I demonstrate the equilibrium school choice under two scenarios: one with a lower correlation b and the other with a higher correlation $b' > b$. Under correlation b the ability threshold for education is $\bar{\theta}$ while under correlation b' it is $\bar{\theta}'$, implying that under higher correlation, the share of workers who stay in school is lower in equilibrium than under lower correlation. Panel (b) demonstrates the effect of a tax-free year. This increases the opportunity cost of schooling, shifting out the school-staying condition and leading a share of students to drop out of school. As the figure demonstrates, the same reduction in tax rates leads to a larger increase in the share of individuals dropping out under a higher correlation ($\mathbb{D}'$) than under a lower correlation ($\mathbb{D}$). Moreover, as the marginal dropout is of higher ability under a higher correlation, earnings loss among dropouts is also higher, since returns to education are $\rho\theta$.

5.4 Misperception and Myopia

I have shown that, under heterogeneity in both ability and psychic cost, a decrease in the tax rate can generate substantial dropout and earnings loss, consistent with my empirical results. I now present two alternative, or complementary, drivers of dropout among students who would have benefitted financially from staying in school.

First, students may *misperceive* their returns to education to be lower than they actually are. That is, students may have incorrect expectations about returns to schooling due to, e.g., youths only being able to observe the earnings of workers around them or form their expectations based on observing workers from the subset of the labor market. To model this, I assume that perceived returns are

$$\mathbb{E}(\rho\theta) = (\rho - \varepsilon)\theta, \qquad \varepsilon \geqslant 0 \tag{7}$$



(a) Equilibrium School Choice

(b) Effect of a Tax-Free Year on School Dropout

Figure 2: Effect on Dropout Depends on the Correlation Between Ability and Psychic Cost

Notes: The figure illustrates in panel (a) the equilibrium school choice and in panel (b) the effect of the tax-free year on dropout, where the tax-free year is modeled as $\tau = 0$. The figure demonstrates results under two cases: low correlation, b, between ability and psychic cost, and high correlation, $b' > b$. The increase in school dropout in response to a reduction in the tax rate is denoted by $\mathbb{D}$ under a low correlation, b, and by $\mathbb{D}'$ under a high correlation, b'. As the figure shows, the effect of the tax-free year on dropout is larger the higher the correlation between ability and psychic cost.

where ε captures the possible misperception of the returns to school.

Second, students may be heterogeneous in how they discount the future. That is, some students may be *myopic* and value the present temporarily more than the future when making the decision about whether to stay in school or enter the labor market. In my model I assume that the future is discounted at the rate $\beta\delta^t$, where some individuals apply a discount rate $\beta < 1$ to all future periods $t > 0$, while others only apply exponential discounting ($\beta = 1$) (Phelps and Pollak, 1968; Laibson, 1997). It is easy to show that such quasi-hyperbolic discounting is analogous to misperception when

$$\varepsilon = \rho(1 - \beta) \tag{8}$$

As shown in Appendix A, in addition to a high correlation between ability and psychic cost, observed dropout and earnings loss may stem from a misperceived of returns to education or high discounting of future gains. This is stated in the following proposition.

Proposition 2. *For a given decrease in the tax rate, τ, an equal dropout propensity and equal-sized earnings loss can result from (i) a high correlation between ability and psychic cost, b, (ii) misperception about returns to education, ε, or (iii) myopia, β.*

This result is derived in Appendix A, showing that the same increase in dropout propensity resulting from increasing the correlation b by $\Delta b = b' - b$, as demonstrated in Figure 2, can result from misperception or present bias. This is true when:

$$\Delta b = \frac{\delta}{1-\delta}\varepsilon = \frac{\delta}{1-\delta}\rho(1-\beta)$$

where the second equality is derived from (8).

6 Interpreting the Empirical Results

6.1 Psychic Costs

The model presented in the previous section explains the empirical results on dropout from school and subsequent earnings loss. The model highlights that the results are consistent with the presence of heterogeneous psychic costs of schooling that are imperfectly correlated with academic ability. This implies that students with a high psychic cost of staying in school will drop out despite facing high returns on completing their education.

An emerging body of literature, primarily based on structural model estimates, has established how large psychic costs can explain why many students do not pursue or complete more education than they do even though they would benefit financially by doing so (see Heckman, Lochner, and Todd, 2006a, for a survey). Prior work has emphasized that psychic costs are likely to be determined by differences in individuals' non-cognitive skills—such as perseverance, motivation, and self-esteem—rather than cognitive skills (IQ) (Jacob, 2002; Heckman, Stixrud, and Urzua, 2006b). While high cognitive skills may lead to greater success in school, low cognitive skills may make it more difficult and less pleasant to stay in school and complete education. In line with this, cognitive ability is found to be a strong predictor of school grades while non-cognitive abilities are a greater predictor of school completion and graduation (Duckworth et al., 2019).

The model presented in Section 5 demonstrates that differences in dropout rates of men and women and subsequent earnings losses reflect differences in not only the level of psychic costs but, importantly, the correlation between psychic costs and academic ability. That is, when measuring academic ability by cognitive skills and psychic costs by non-cognitive skills, the theory implies that to explain the empirical results, men are expected to have lower non-cognitive skills on average and a weaker correlation between non-cognitive and cognitive skills than women. This interpretation is broadly consistent with existing evidence on gender differences in skills. While gender differences in cognitive measures appear to be minor (Hyde, 2014), evidence suggests that women have both higher average non-cognitive skills and less variance than men. Becker, Hubbard, and Murphy (2010) survey evidence on non-cognitive measures related to school performance and participation. They document that girls have higher averages in positive measures (time spent on homework and achievement goals) and lower averages in negative measures (disruptive behavior and behavioral problems) than boys, as well as lower variance in these measures. Moreover, Heckman, Stixrud, and Urzua (2006b) document that correlations across cognitive and non-cognitive tests are almost 50% stronger for women than for men.[15]

[15] In an online appendix to their paper, Heckman, Stixrud, and Urzua (2006b) report correlations between cognitive and non-cognitive scores for men and women. Cognitive skills were measured using five different tests: arithmetic reasoning, word knowledge, paragraph comprehension, mathematical knowledge, and coding speed. Non-cognitive ability is measured by two different scales: the Rotter Internal-External Locus of Control Scale, which measures the extent to which individuals believe that they have control over their lives through self-motivation or self-determination, and

In sum, existing evidence suggests that men have not only lower levels of non-cognitive skills than women but also non-cognitive skills of men are more variable and less correlated with their cognitive skills than women. This suggests that when faced with external influences, such as a transitory opportunity, men are more likely than women to drop out of school, as they find schooling to be more costly than women do. This also suggests that men who drop out of school may have large returns to completing education as their psychic cost is less correlated with their cognitive skills.

To evaluate empirically this explanation of my findings would require having access to proxies for the psychic costs of schooling of Icelandic students at the time of the tax-free year. Unfortunately, proxies such as psychometric measures of non-cognitive skills are not available. To shed some light on this explanation in the Icelandic context, I instead lean on data and estimates from a randomized longitudinal study of Icelandic youth, starting in 2006 (Blöndal et al., 2016). In Appendix Figure A.17, I present the results from an estimation of a hazard model of having completed upper secondary school six years after enrollment (Þorláksson, 2019).[16] The covariates in the model were gender, parental education, cognitive skills measured by standardized test scores in primary school, and non-cognitive skills measured by three psychological factors: school engagement, school ambitions, and bad behavior. The figure shows that women are less likely to drop out of school, as are students with higher cognitive skills. Holding gender and cognitive skills constant, students with higher non-cognitive skills are less likely to drop out of school. These results are broadly in line with Almås et al. (2016), who find that the gender gap in school dropout is partially explained by gender differences in non-cognitive skills as measured by the Big Five personality traits.

6.2 Myopia

While it is clear from my results that gender differences in school dropout cannot be explained by negative selection of male dropouts or them facing low returns to education, the explanation based on gender differences in psychic costs is intrinsically incomplete. Any outcome could be rationalized by psychic costs influencing economic choices. Therefore, what I have referred to as "psychic costs" might stand in for factors such as myopia or erroneous expectations, possibly related to psychometric measures of non-cognitive skills. I do not try to distinguish between these factors but rather view them as complementary.

I evaluate whether gender differences in myopia—i.e., men having a lower discount factor than women—can explain my findings by computing the discount factor that is implied by the

the Rosenberg Self-Esteem Scale. The average correlation between non-cognitive skills measured by self-motivation and the five different cognitive skills is 43% higher for women than men.

[16] The data is from a randomized longitudinal study of 2750 students in upper-secondary school in 2007, interviewed at ages 17 and 18, and then linked to register data from Statistics Iceland on school outcomes at ages 23 and 24 (Blöndal et al., 2016). The figure is based on regression estimates in Þorláksson (2019), who estimates a regression where the outcome variable is an indicator of having completed upper-secondary school at age 23 or 24 on potential factors influencing dropout, including gender, school grades, parental education, and psychological factors. See figure notes for further details.

schooling choices I observe. That is, I compute the discount factor that equates the earnings gain from dropping out of school during the tax-free year and the present discounted value of the subsequent earnings losses. More precisely, I compute the discount rate ρ, which solves the following equation:

$$\beta_{16-20} - \sum_{a=21}^{40} \frac{\beta_a}{(1+\rho)^{a-20}} = 0 \tag{9}$$

where β_{16-20} is the effect on cumulative earnings at upper-secondary school age (16-20), and β_a is the treatment effect on cumulative earnings at age a.[17]

Figure 11 presents the effect on cumulative earnings un-discounted, discounted using a discount rate of 5%, and discounted using the rate ρ that solves equation (9).[18] The discount rate that rationalizes dropout was 18.7%, implying a discount factor of $\frac{1}{1+0.187} = 0.84$.[19] This calculation of the discount factor gives only a lower bound, as it assumes that there are no earnings gains for school stayers beyond the age of 40. Prior work (e.g., Bhuller, Mogstad, and Salvanes, 2017) and lifecycle profiles (Appendix Figure A.11) indicate that returns to education may not peak by the age of 40.

An alternative assumption is that students discount the future according to a quasi-hyperbolic discount function, applying a discount rate $\beta\delta^t$ (Phelps and Pollak, 1968; Laibson, 1997). Figure 11 shows that a discount rate of 5% yields a present discounted value of the cumulative effect on earnings of approximately \$14,900. A discount rate, δ, of 5%, and a cumulative gain from dropping out of \$3,400 imply $\beta = 0.23$. In light of the existing literature, this implies a very severe present bias (Imai et al., 2021).

The literature presents a wide range of discount rate estimates, from negative to infinity (Frederick et al., 2002; Cohen et al., 2020). However, there is substantially more variability in estimates at shorter horizons, and the average discount factor across studies with a horizon beyond one year is approximately 0.8 (Frederick et al., 2002). Therefore, although the discount rate implied by my estimates is large, it falls within the range of estimates reported in the literature. Studies have also documented that boys tend to be more impatient than girls (Bettinger and Slonim, 2007; Castillo et al., 2011), have less self-control and lower ability to delay gratification (Duckworth and Seligman, 2006), and are more sensitive to short-term incentives (Levitt, List, Neckermann, and Sadoff, 2016). Moreover, gender differences in patience have been found to be strongly related to differences in school disciplinary and behavior problems (Castillo et al., 2011), which are related to gender differences in non-cognitive skills (Jacob, 2002). In light of this, school dropout during the tax-free year may reflect gender differences in psychic costs, including factors such as myopia.

[17] The ρ that solves equation (9) is essentially the "internal rate of return" of post-compulsory education (Heckman et al., 2006a).

[18] The average five-year indexed interest rate in Iceland was 4.94% in 1986.

[19] These calculations are based on the reduced-form estimates. An alternative approach is to calculate the interest rate that equalizes the returns to dropping out and the loss in earnings from reduced schooling estimated using 2SLS. This approach yields an interest rate of 13%.

6.3 Misperceived Returns to Education

An alternative explanation for why young men decide to drop out of school when returns would be higher if they stayed is that they perceive returns to be lower than they actually are. As emphasized by Manski (1993), it is not the actual returns to education that determine actual schooling decisions but rather their *perceived* returns.

I present two sets of results that shed light on this explanation. First, as discussed earlier, Figure 8 documents that men and women work different types of jobs during adolescence. Young men, including those who dropped out of school during the tax-free year, primarily work in occupations that pay a relatively high starting wage without requiring much education. In contrast, young women work in occupations that also require little education but pay relatively low salaries. If students form their expectations about the returns to education based on the pay and educational requirements in these occupations, men and women may form very different expectations.

Second, Figure 12 presents the estimates of the effect on men's dropout rates based on their background characteristics. Jensen (2010) documents that perceived returns to education can be smaller in locations where education and income are low. To investigate this explanation, I first classified students according to whether they grew up in a neighborhood with high or low average returns to education. The figure reports a significant effect on dropout among those growing up in low-return neighborhoods, but not in high-return neighborhoods, but there is only a small and not statistically significant difference in point estimates. Next, I classified men by whether they grew up in a neighborhood with high or low dropout rates among their older peers, that is, individuals up to four years older than them. This captures the dropout rate in the closest peer group, whose upper-secondary schooling choices should not be influenced by the tax-free year. The figure shows a somewhat larger effect on dropout among those living in high-dropout neighborhoods. Similarly, I grouped students by education of adults, finding that students growing up around less-educated adults are more likely to drop out during the tax-free year. However, neither of these differences is statistically significant. Finally, I separated students by whether they grew up in the capital region, that is, Reykjavik and the suburbs, versus those living in other areas of Iceland. There is a strong and statistically significant difference between the two groups, where dropout is substantial and significant outside the capital region. Since career opportunities are likely to differ and returns to education to be larger in urban areas than in rural Iceland, these results suggest that young men who dropped out of school may have perceived their returns to completing education to be lower than they actually were. However, as shown in Appendix Figure A.15, there is no indication of similar effects among women.

6.4 Bad Luck

Do the estimated earnings losses from dropping out of school simply reflect bad luck? That is, while the returns to schooling turned out to be high ex-post, things may have looked different exante when students made their decision to drop out. In that case, their decision might have looked more rational than was ex-post. Appendix Figure A.16 plots average earnings by education group over time relative to average earnings of those with only compulsory education. While these ratios vary over the business cycle, the average difference is roughly the same today as it was around the time of the tax-free year.

7 Implications of Results

Welfare Costs Dating back to the seminal contribution of Lucas (1987), the literature quantifying the cost of business cycles has primarily measured this cost with the welfare loss from intertemporal substitution in consumption. The results presented in the current paper highlight another channel through which costs of business cycles can materialize, namely through permanent effects on labor productivity.

According to prior theoretical (Rogerson and Wallenius, 2009) and empirical (e.g. Sigurdsson, 2022) work, employment responses to transitory wage changes—measured with extensive-margin Frisch elasticity—are concentrated in the young cohorts around schooling age and older cohorts around retirement age. For workers close to retirement, intertemporal labor supply adjustment revolves around whether to retire today or tomorrow. If workers close to retirement are less productive or block the career progression of their younger coworkers, delayed retirement may reduce firms' productivity temporarily.[20] For school-age individuals, intertemporal labor supply adjustment revolves around whether to stay in school or to leave school to work. My results document that transitory earnings opportunities lead to permanent losses in education. This implies that employment responses to cyclical increases in wages can lead to permanent productivity losses, reducing output and consumption in the longer run.

I perform two types of calculations to quantify the impact of school dropout during the tax-free year on long-run output. The first approach is a back-of-the-envelope calculation based on my estimated effect on earnings in adulthood. I use the estimated effect on labor earnings reported in Figure 6, panel (c), and assume the effect is unchanged through men's 40s and until their 50s. Moreover, to obtain an effect on GDP, I assume that the effect on GDP is proportional to the share of labor earnings of GDP. Weighting the effect on the cohort of men at upper-secondary school age in 1987, i.e. the treated group, by their share of the working age population 16-67, I estimate that school dropout during the tax-free year led GDP in 2020 to be lower by 0.09%.

For the second approach, I use the growth model estimates from Hanushek and Woessmann (2012) to quantify the impact of the loss of one year of schooling on the growth rate of GDP per capita. I then scale this estimate by my estimate of the working-age-population weighted effect on years of schooling (Table 2). I use this factor to scale the actual annual growth rate of GDP from

[20] Consistent with that notion, Bianchi, Bovini, Li, Paradisi, and Powell (2021) find that delayed retirement among soon-to-retire Italian workers in response to a pension reform reduces the wage growth and promotions of their younger colleagues.

1987 to 2020 to a counterfactual growth rate under no loss in years of schooling. Based on this approach I estimate that GDP in 2020 was reduced by 0.22%.[21]

In sum, I estimate that the loss of human capital incurred in 1987 resulted in sizable losses in output over subsequent decades.

Education Policy I find that the tax-free year had persistent negative effects on the education of men, leading to both worsened individual outcomes and reduced social welfare. Furthermore, these effects likely contributed to the growing gender gap in education that favors women.

The large private and public losses justify policies to reduce dropout. The natural policy instrument is the compulsory schooling age. I estimate that school dropout during the tax-free year would have been reduced by about 40 percent had the compulsory schooling age been 17 instead of 16.[22] Oreopoulos (2007b) studies the experience of several US states that increased the school leaving age to 17 and 18. His results suggest that such policies reduce high-school dropout and increase college enrollment, even though college is not compulsory, and also increase earnings in adulthood. This suggests that students who otherwise would have dropped out from high school, complete further education when high school is compulsory for one or two additional years.

An alternative and more direct policy would be a 'procyclical' education policy, aimed at keeping students in school when opportunity costs are high. For example, policymakers that want to counteract dropout may want to increase education subsidies—e.g. through student grants or low-interest loans—when the economy is booming.[23] As an alternative to developing new policy instruments, raising the school leaving age may be a simple but effective means of reducing dropout.

8 Conclusion

I have presented evidence of how transitory increases in the opportunity cost of schooling can permanently affect the accumulation of human capital and gender gaps in education. I exploit a salient tax reform that, for a single year, raised the prospective income of students if they were to enter the labor market. In tandem with this natural experiment, I leverage the discontinuity in students' ability to leave school to work. I find that men, but not women, leave school to seize the transitory opportunity to work at a higher wage and never return to complete their education. As a consequence, the earnings of these young men gradually fall behind during adulthood, and they suffer large losses in lifetime income.

[21] The calculations assume that cyclicality in educational attainment is not equalized over the business cycle through increased educational attainment during recessions. In support of this assumption, recent work has documented how recessions can *reduce* educational attainment (Stuart, 2020).

[22] To quantify the effect of higher compulsory schooling age on estimated dropout, I use a difference-in-discontinuity estimation strategy where women are the control group for men. This specification relies on my main result that women do not drop out of school. The estimated effect on male dropout using this strategy is 0.036 compared to the main RD estimate of 0.050 reported in Table 2. Estimating the difference-in-discontinuity excluding 16-year-olds reduces the estimated effect on dropout to 0.021, or a 42% decrease.

[23] Additionally, the findings provide support for policies that allow dropouts to return to school later in life. Recent work indicates that such programs can bring large earnings gains (e.g. Bennett et al., 2020; Brough et al., 2021).

These results shed new light on the causes and consequences of the growing gender gap in education. Across developed countries, men have fallen behind women in education due to decreasing enrollment and higher levels of dropout. My results demonstrate that the students who drop out of school when transitory earnings opportunities arise are young men who otherwise would have acquired much more education and earned large returns on that investment. Moreover, my results suggest that gender differences in nonpecuniary costs of school attendance, myopia, or perceived returns to education lead men to be more susceptible to dropping out of school. This calls for policy interventions to counter the factors that lead men to underinvest in education—an underinvestment that leaves permanent scars on output and welfare.

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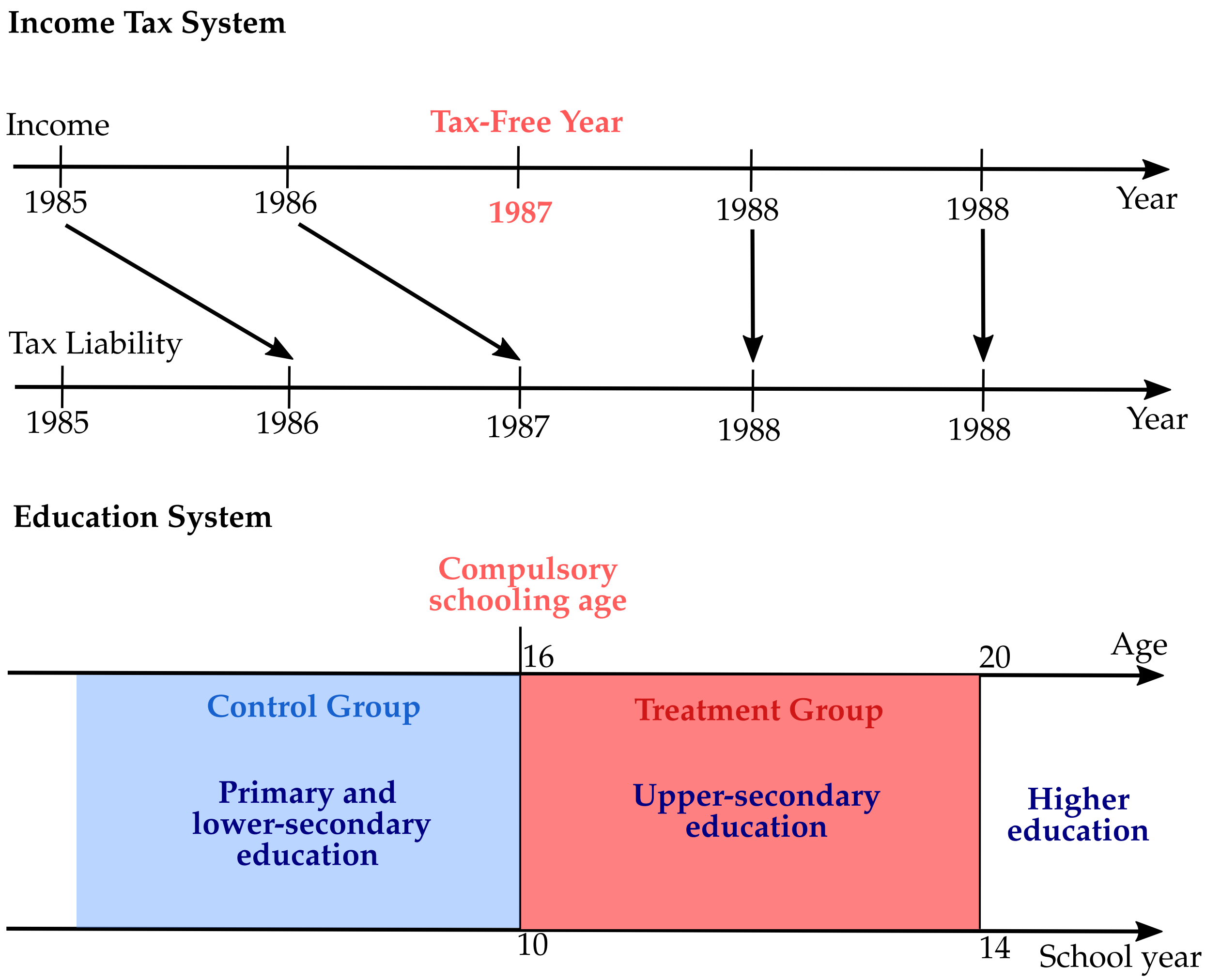
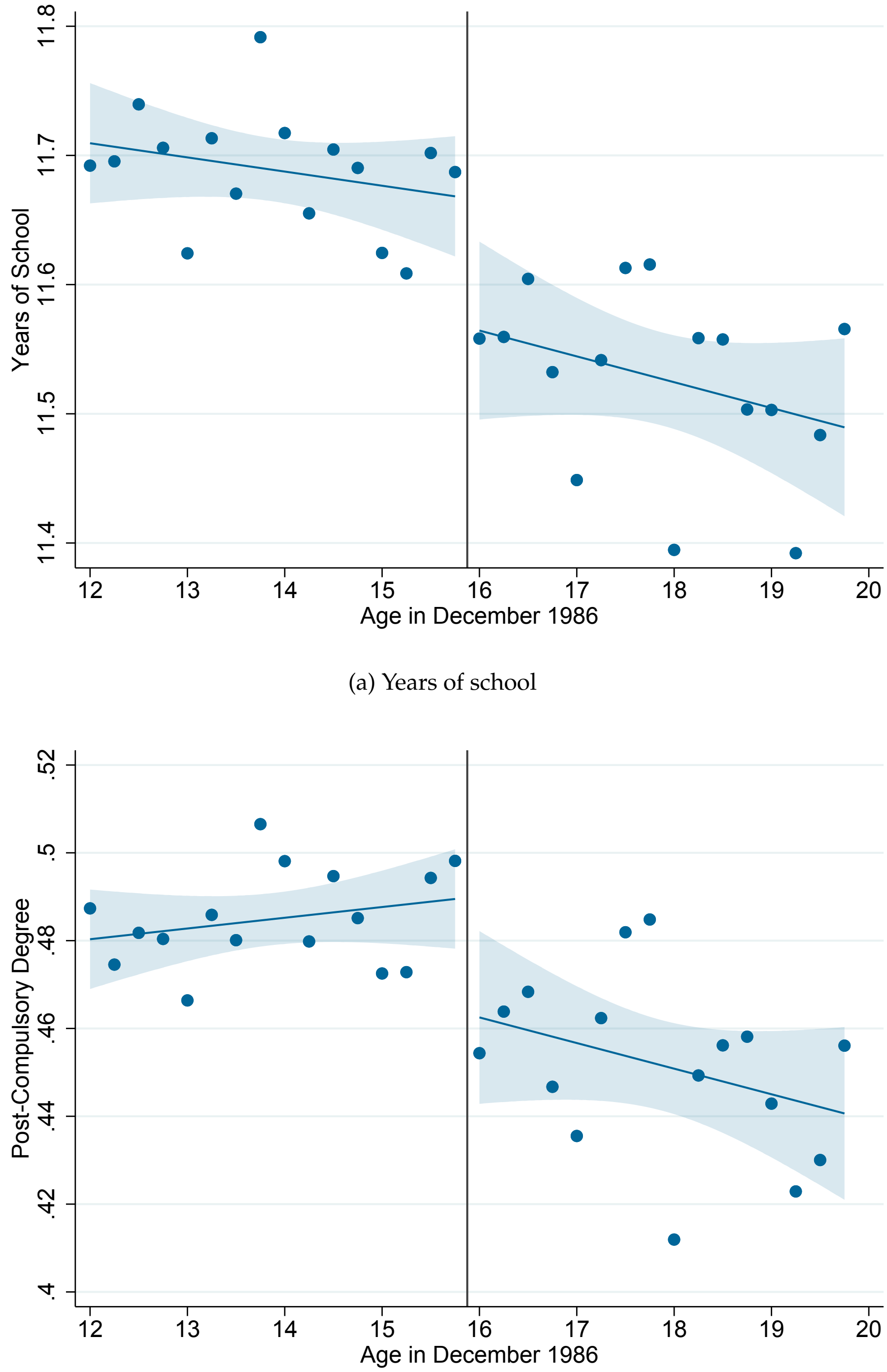


Figure 3: Research Design: Tax-free Year and Compulsory Schooling Age

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.

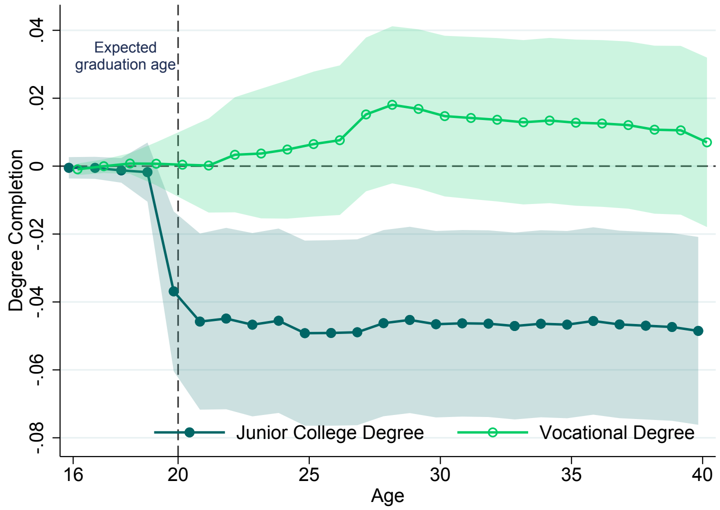


(a) Years of school

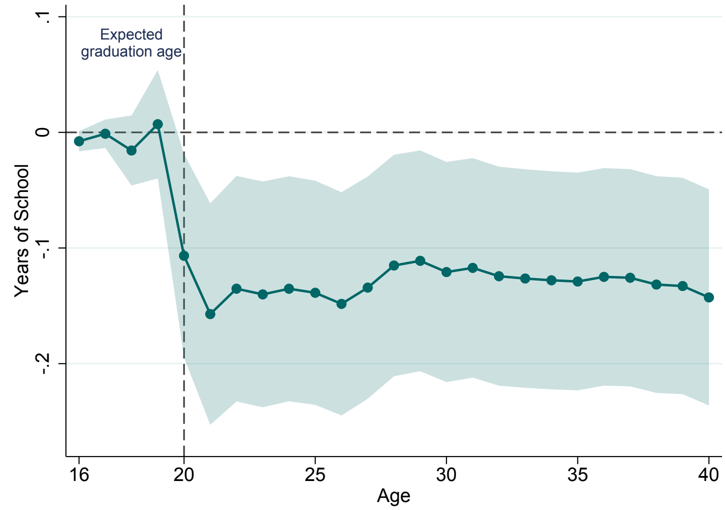
(b) Post-compulsory degree

Figure 4: School Dropout

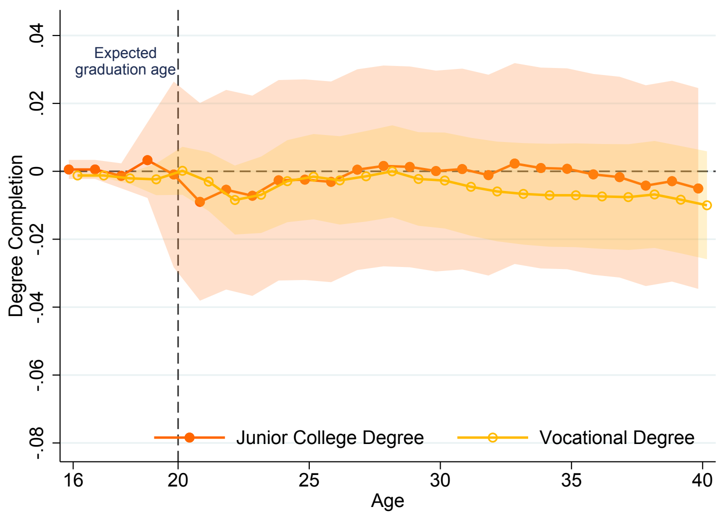
Notes: The figure is a plot of average educational attainment at age 21 on each side of the compulsory-school age threshold. Panel (a) plots the average number of years of school completed. Panel (b) plots the average share with post-compulsory education. The vertical line denotes the compulsory schooling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



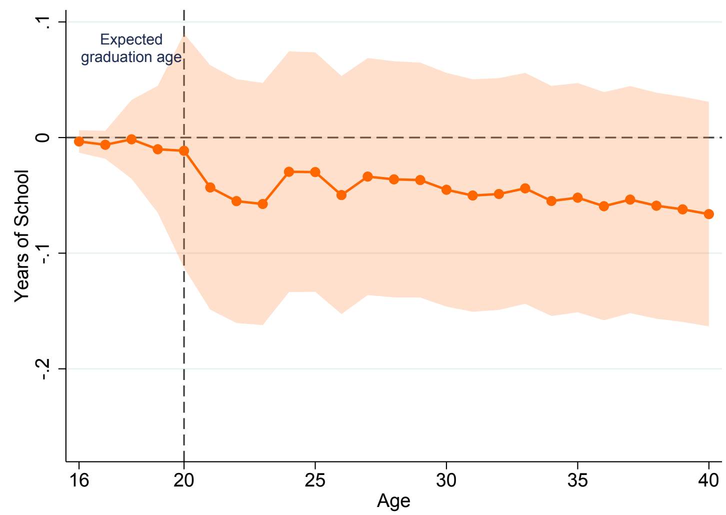
(a) Men: Junior college and vocational degree



(b) Men: Years of school



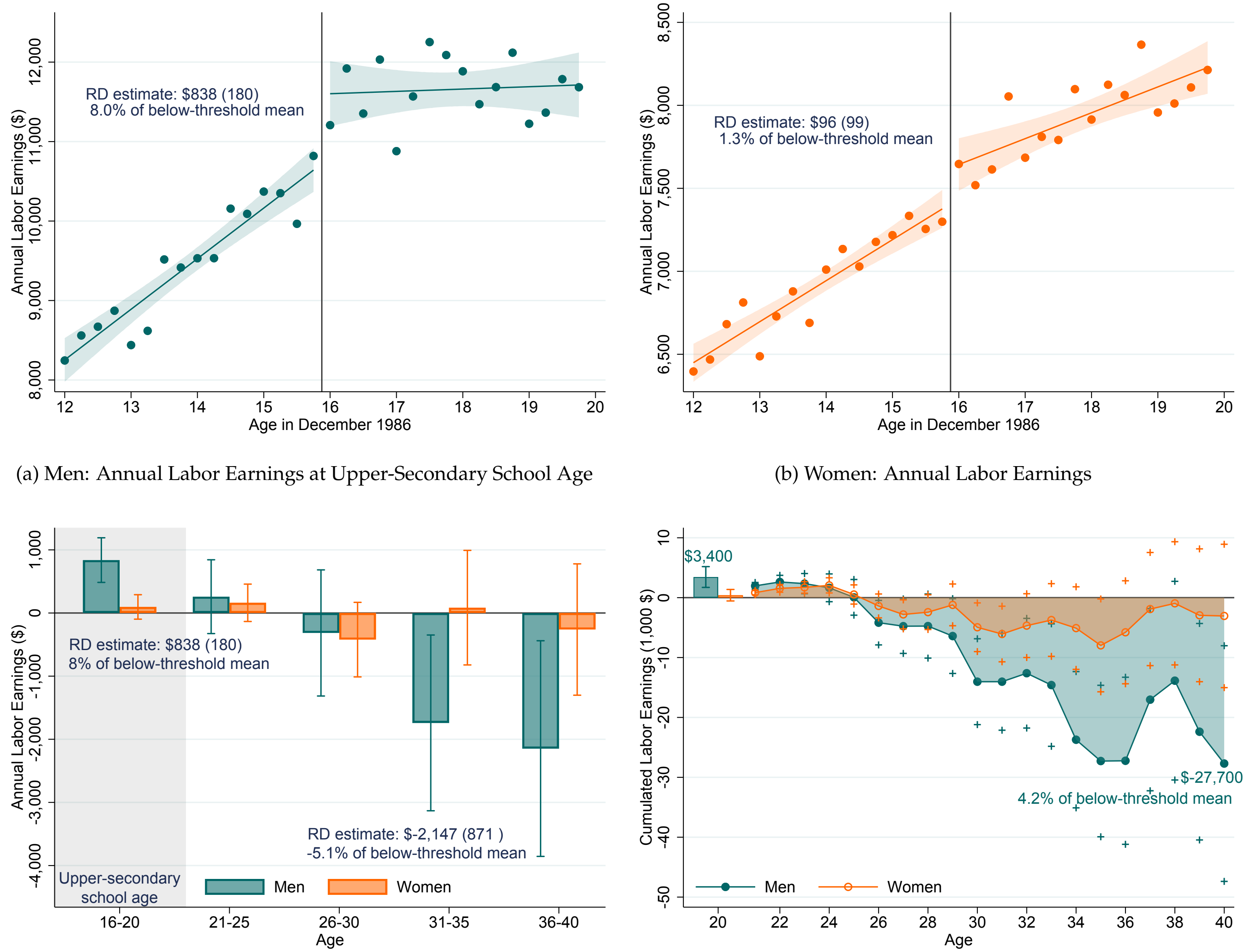
(c) Women: Junior college and vocational degree



(d) Women: Years of school

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

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



(a) Men: Annual Labor Earnings at Upper-Secondary School Age

(b) Women: Annual Labor Earnings

(c) Effect on Annual Labor Earnings

(d) 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. Panels (a) and (b) plot the average annual labor earnings at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Panel (c) 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 (d) 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 (c) and crosses in panel (d) display the 95% confidence intervals based on robust standard errors clustered at the individual level.

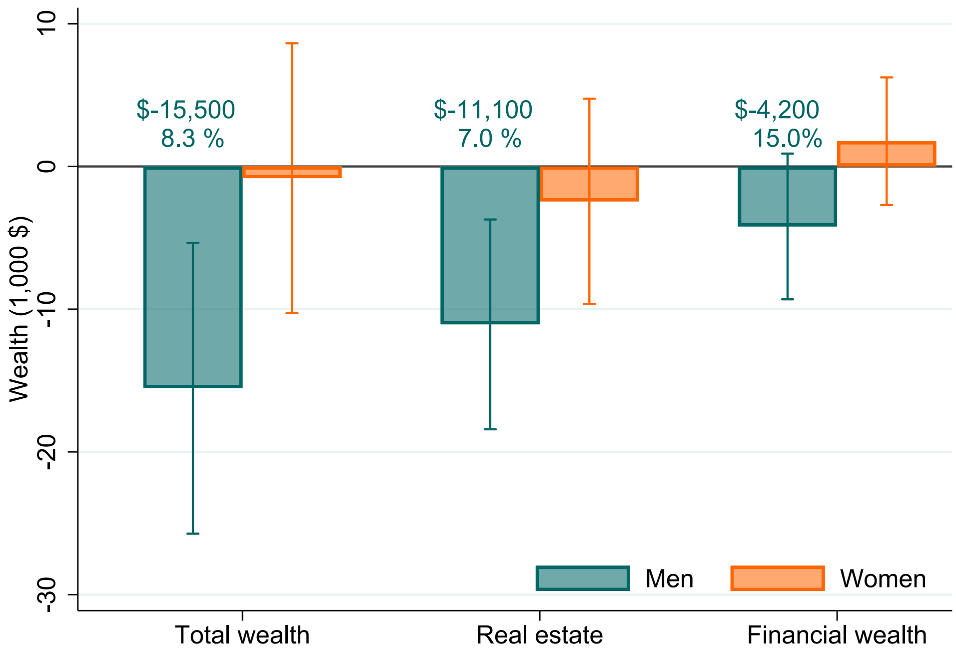


Figure 7: Effects on Wealth at Prime Age

Notes: This figure reports treatment effects on wealth estimated with regression equation (1). Estimates are averages over the prime age of 36-40. Each bar represents a single regression estimate for the outcome measure specified on the horizontal axis. Numbers above the bars present the point estimate as a percent of the outcome mean, where the outcome means are measured as 12-month averages below the threshold. *Total wealth* is all assets, financial and non-financial assets. *Real estate* is the tax-base 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. The whiskers display the 95% confidence intervals based on robust standard errors clustered at the individual level.

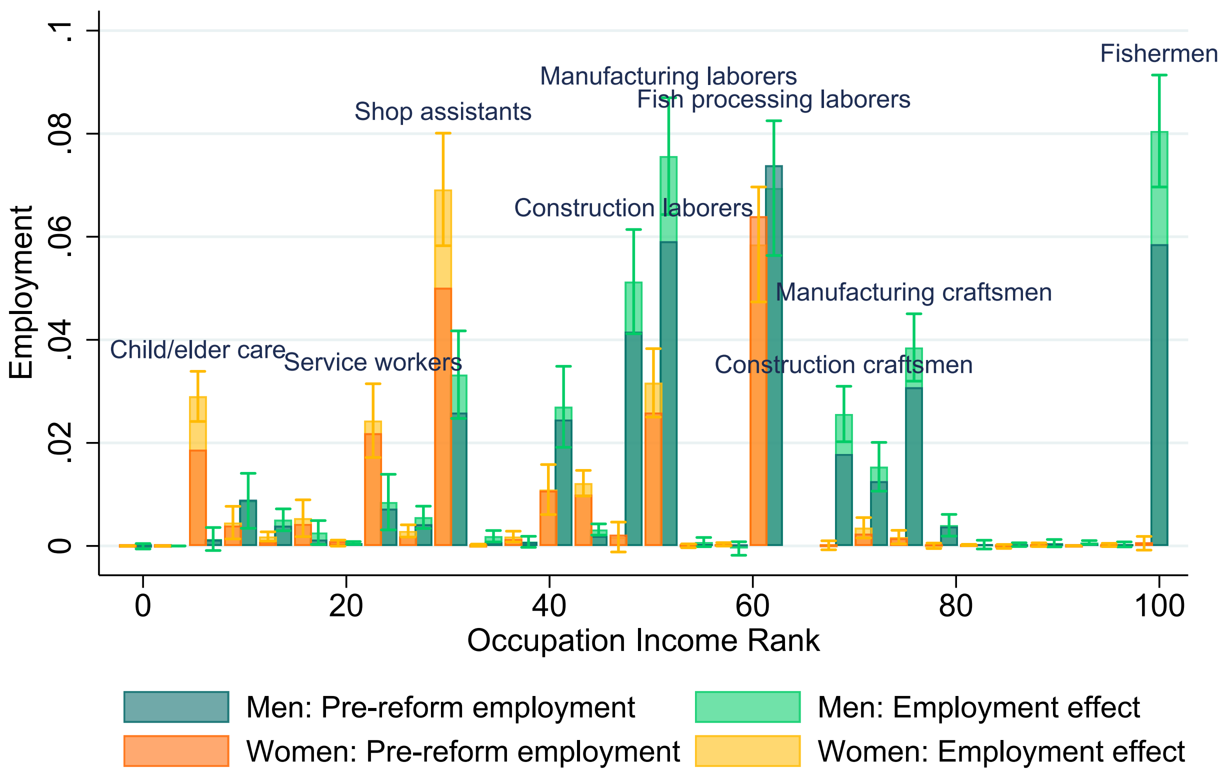


Figure 8: Entry Jobs of School Dropouts

Notes: The figure presents the estimated employment effect by occupation. The lower part of each bar represents the employment share in that occupation among 16-20-year-olds, separately for men and women. At the top of each bar, I plot the estimated effect on employment in that occupation among 16-20-year-olds. Occupations are ordered along the horizontal axis by the pre-reform rank of average income among 16-20-year-old employees. For example, prior to the reform, 16-20-year-olds earned the highest income if employed in fishing but the least if employed in child- and elderly care. 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.

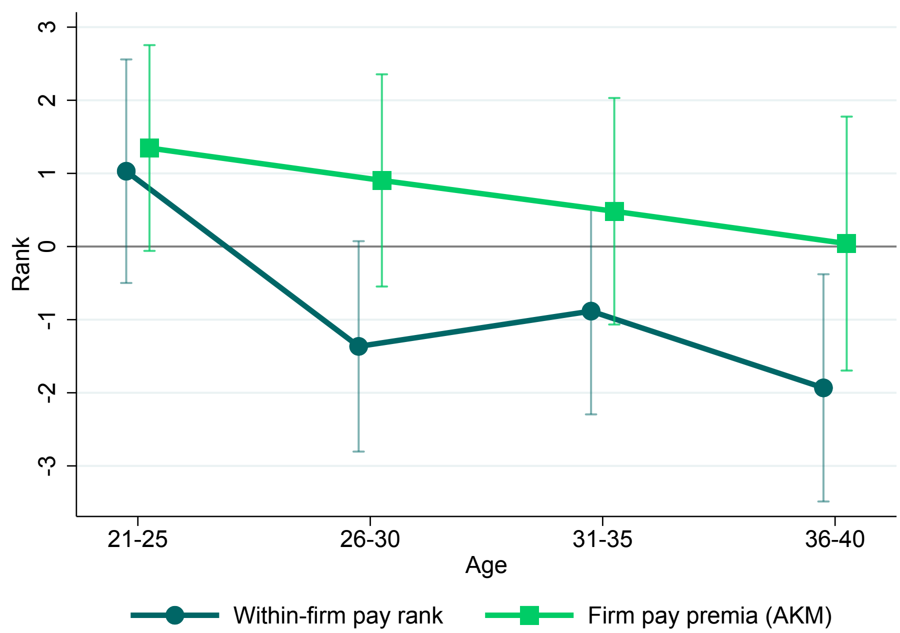


Figure 9: Firm Pay Premia and Career Progression

Notes: The figure plots the estimated treatment effects on the pay premia at the worker's employer and the worker's relative position within the firm of employment. Firms' pay premia are measured by AKM firm fixed effects estimated in the population of firms. Career progression is measured by the rank of the worker's residualized ('Mincerized') earnings within the firm. 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. 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. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence interval based on robust standard errors clustered at the individual level.

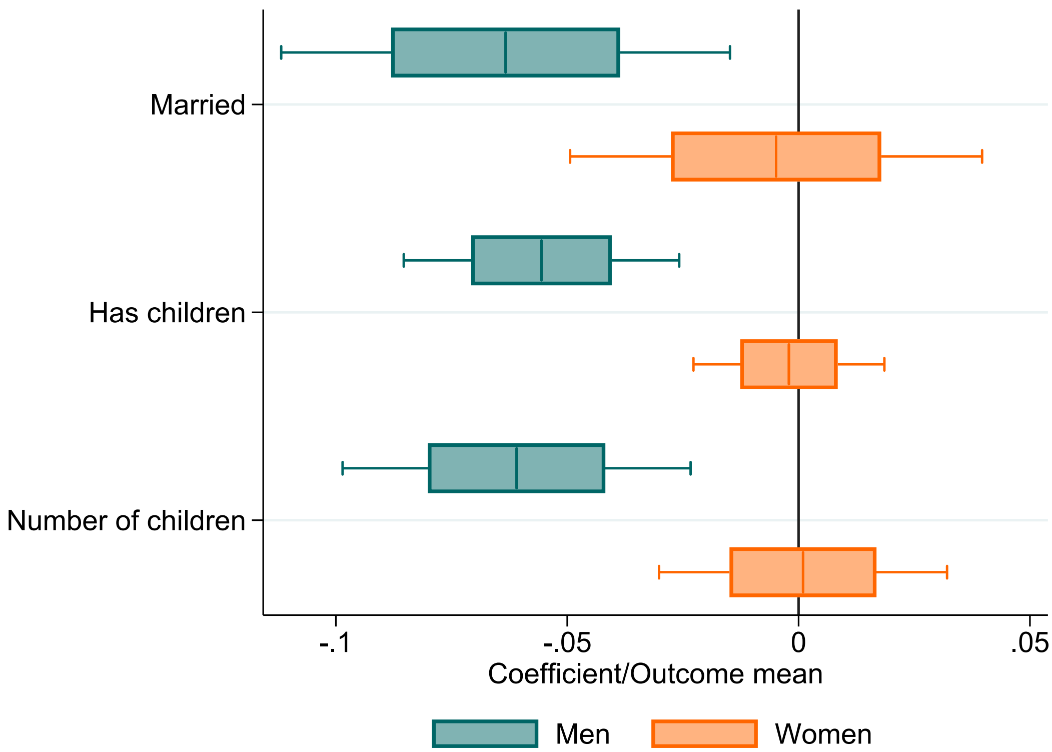


Figure 10: Effect of School Dropout on Marriage and Fertility

Notes: The figure presents estimates of equation (1) of the effect on marriage and fertility. *Married* is an indicator of ever having been married by age 40. *Has children* is an indicator of having had children by age 40. *Number of children* is the number of children the individual has had by age 40. Estimates are reported as the ratio of the treatment effect to the outcome means, measures as 12-month averages below the age threshold. The treatment effect, measured as this ratio, is presented as the center (vertical line) in each box. The top and bottom of each box represent effects that are one standard error above and below. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The whiskers display the 95% confidence interval where standard errors are calculated using the Delta method.

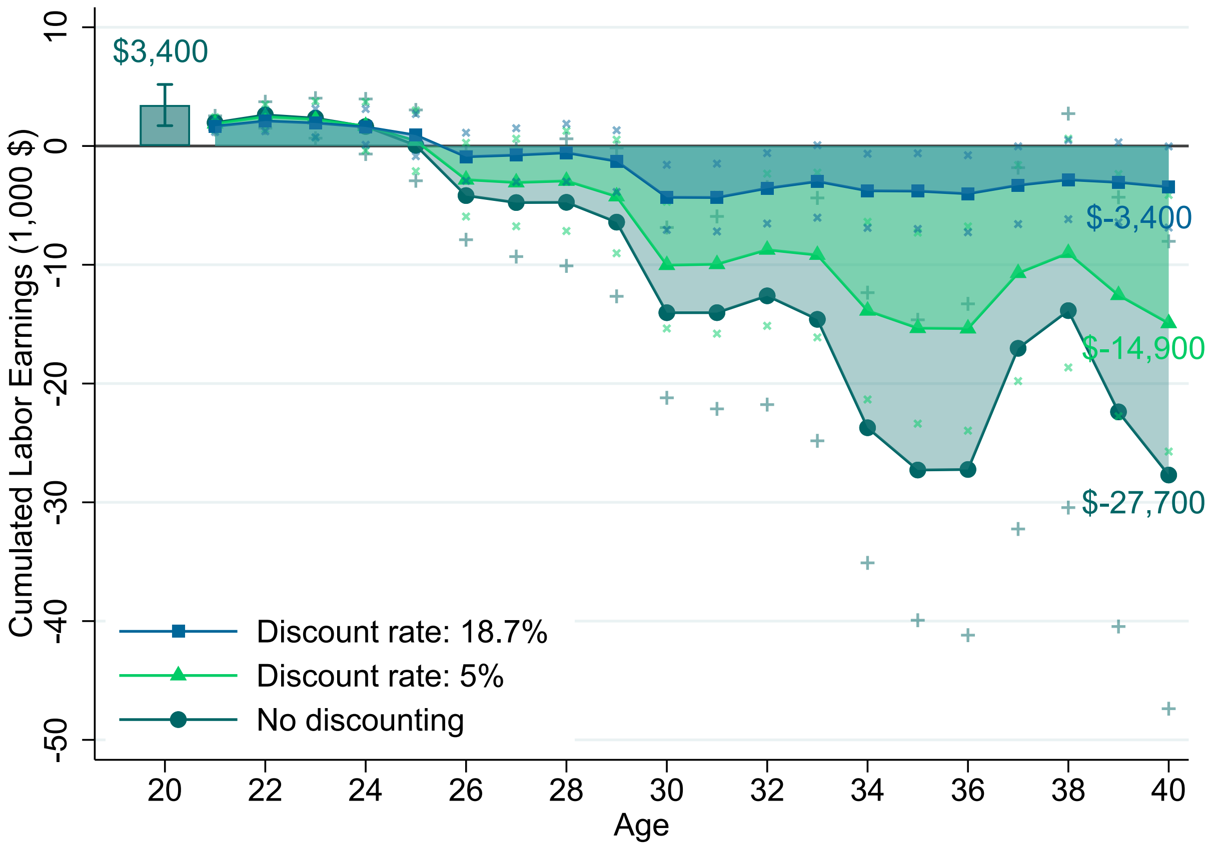


Figure 11: Cumulative Labor Earnings and Implied Discount Rate

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.

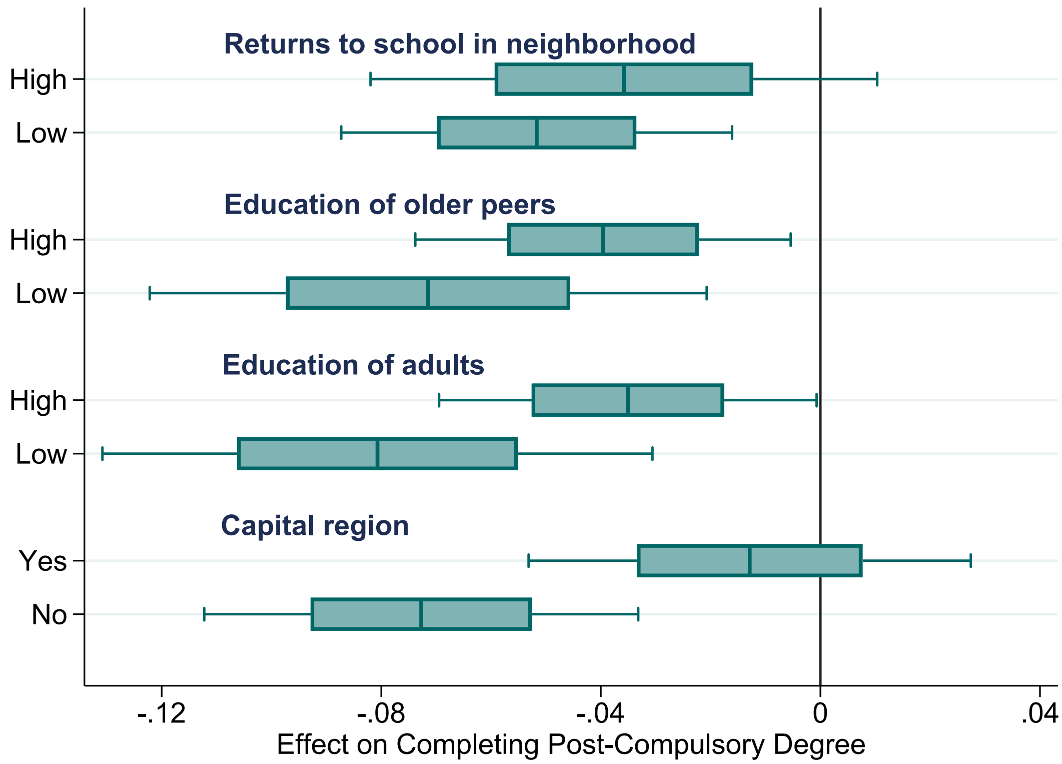


Figure 12: School Dropout by Background

Notes: The figure plots the estimated effects of the tax-free year on school dropout of men by background characteristics. *Returns to school in neighborhood* is the average ratio of earnings to years of school in the area. That is, I first measure the return in each municipality as the average ratio of earnings to years of school among adults in the three years prior to the tax reform, and then split students into two groups based on their location of residence. *Education of peers* is the share of 21-24-year-olds with post-compulsory education in the area. *Education of adults* is the share of individuals of age 35 and older with post-compulsory education in the area. *Capital region* denotes whether the individual lived in the Reykjavik area at age 16. The bars correspond to estimates of equation (1) where the outcome variable is an indicator of completing post-compulsory schooling, which is interacted with a group indicator splitting the sample in two based on these background characteristics. 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 whiskers display the 95% confidence interval.

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 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: Effects of School Dropout on Earnings

	Men				Women			
	Prime-Age		Lifetime		Prime-Age		Lifetime	
	Degree	Years	Degree	Years	Degree	Years	Degree	Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS Estimate	48,643**	9,433**	501,794**	86,764**	51,827	5,138	425,417	36,667
	(22,826)	(4,721)	(206,313)	(38,470)	(164,755)	(11,417)	(1,103,849)	(75,303)
Implied Return	1.160	0.225	0.765	0.132	1.975	0.196	1.125	0.0970
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome mean	41,927	41,927	656,154	656,154	26,247	26,247	378,028	378,028
Observations	76,269	76,269	15,026	15,026	74,274	74,274	14,784	14,784

Notes: This table reports estimates of reduced educational attainment due to school dropout on earnings. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Online Appendix of:

Transitory Earnings Opportunities and Educational Scarring of Young Men

Jósef Sigurdsson
March 17, 2023

A Model Appendix

This appendix to Section 5 presents proofs of propositions and additional model results. Before presenting a proof of proposition 1, I present a proof of the following lemma that states under which conditions a decrease in the tax rate leads to school dropout.

Lemma 1. *A decrease in the tax rate, τ, leads to increased dropout from school if* $\frac{\delta\rho-(1-\delta)b}{1-\delta} > (1-\tau)+\kappa^0$.

Proof. I denote the change in the fraction of individuals that drop out of school when the tax rate changes with $\mathbb{D}$. I want to show that:

$$\mathbb{D} \equiv \frac{\partial}{\partial(1-\tau)} \mathrm{P}\left(\frac{\delta}{1-\delta}\rho\theta \leqslant (1-\tau)+\kappa\right) > 0$$

From the properties of θ and κ we have that:

$$\mathrm{P}\left(\frac{\delta}{1-\delta}\rho\theta \leqslant (1-\tau)+\kappa\right)$$

$$= \mathrm{P}\left(\theta \leqslant \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}\right)$$

$$= \begin{cases} 1 & \text{if} \quad \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} > 1 \\ \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} & \text{if} \quad 0 \leqslant \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} \leqslant 1 \\ 0 & \text{if} \quad \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} < 0 \end{cases}$$

I focus on the case where $0 < \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} < 1$ as $\mathbb{D}$ is not well defined at 0 or 1. I first show when $\frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} > 0$. The numerator is always weakly positive assuming κ^0 is positive. Therefore, the condition holds true when:

$$\frac{\delta\rho}{1-\delta} > b \tag{10}$$

Now I show when $\frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b} < 1$. This is true when:

$$\frac{\delta\rho-(1-\delta)b}{1-\delta} > (1-\tau)+\kappa^0 \tag{11}$$

As the RHS is bounded by κ^0 from below, which is assumed positive, this condition also includes (10). Therefore, under condition (11), then:

$$\mathbb{D} \equiv \frac{\partial}{\partial(1-\tau)}\left[\frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}\right] = \frac{1-\delta}{\delta\rho-(1-\delta)b} > 0 \tag{12}$$

□

The lemma states that a decrease in the tax rate leads to dropout if the probability of a dropout is strictly positive and strictly less than unity. Since the empirical results document that a decrease in the tax rate increases dropout, I make the following assumption:

Assumption 1.

$$\frac{\delta\rho-(1-\delta)b}{1-\delta} > (1-\tau)+\kappa^0$$

Proof of Proposition 1

Proof. From the proof of Lemma 1 we have that

$$\mathbb{D} \equiv \frac{\partial}{\partial(1-\tau)}\left[\frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}\right] = \frac{1-\delta}{\delta\rho-(1-\delta)b} > 0$$

which is increasing in b. Next we want to show that

$$\mathbb{E}[\rho\theta|\mathbb{D}] = \frac{\partial}{\partial(1-\tau)}\rho\mathbb{E}\left[\theta|\frac{\delta}{1-\delta}\rho\theta \leqslant (1-\tau)+\kappa\right]$$

is increasing in b. Solving and taking expectations gives

$$\begin{aligned}
&\rho\mathbb{E}\left[\theta|\frac{\delta}{1-\delta}\rho\theta \leqslant (1-\tau)+\kappa\right] \\
&= \rho\mathbb{E}\left[\theta|\theta \leqslant \frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}\right] \\
&= \frac{1}{2}\frac{\rho(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}
\end{aligned}$$

Differentiating with respect to the net-of-tax rate gives

$$\frac{\partial}{\partial(1-\tau)}\frac{\rho(1-\delta)((1-\tau)+\kappa^0)}{2(\delta\rho-(1-\delta)b)} = \frac{\rho(1-\delta)}{2(\delta\rho-(1-\delta)b)}$$

which is increasing in b. □

Proposition 2 by example

Proposition 2 states that for a given decrease in the tax rate, τ, dropout of the same propensity can result from a high correlation between ability and psychic cost, b, misperception about returns to education, ε, or a present bias, β. Rather than providing a formal proof of the proposition, I will show this with an example.

Consider a benchmark dropout propensity $\mathbb{D}$ under correlation b and a higher dropout propensity $\mathbb{D}'$ under correlation b'. This is the example illustrated in Figure 2 in the main text. From (12) we have:

$$\mathbb{D}' = \frac{1-\delta}{\delta\rho - (1-\delta)(b+\Delta b)}$$

where $\Delta b = b' - b$ is the increase in correlation compared to the benchmark. I can then solve for ε that leads to the same increase in dropout propensity but at the benchmark correlation b:

$$\mathbb{D}' = \frac{1-\delta}{\underbrace{\delta\rho - (1-\delta)(b+\Delta b)}_{\varepsilon=0}} = \frac{1-\delta}{\underbrace{\delta(\rho-\varepsilon) + (1-\delta)}_{\Delta b=0}}$$

which gives misperception required to generate the same increase in dropout as Δb

$$\varepsilon = \frac{1-\delta}{\delta}\Delta b$$

Furthermore, from the assumption of $\beta\delta$ preferences, I have shown in the main text that

$$\varepsilon = \rho(1-\beta)$$

which implies that for a hyperbolic discount factor of

$$\beta = 1 - \frac{1-\delta}{\delta\rho}\Delta b$$

then present-bias yields the same dropout propensity as an increase in correlation of Δb and misperception of ε.

B Data on 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

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		8
3	Upper secondary education	Junior college and vocational education	8
4	Post-secondary non-tertiary education		5
5	Short-cycle tertiary education	Higher education	2
6	Bachelor's or equivalent level		3
7	Master's or equivalent level		2
8	Doctoral or equivalent level		1
			31

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.

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.2 summarizes estimates from prior studies of the

Table A.2: 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 10 but scaled by the estimated effect on lifetime earnings (Figure 11) 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.

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, 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

[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.

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

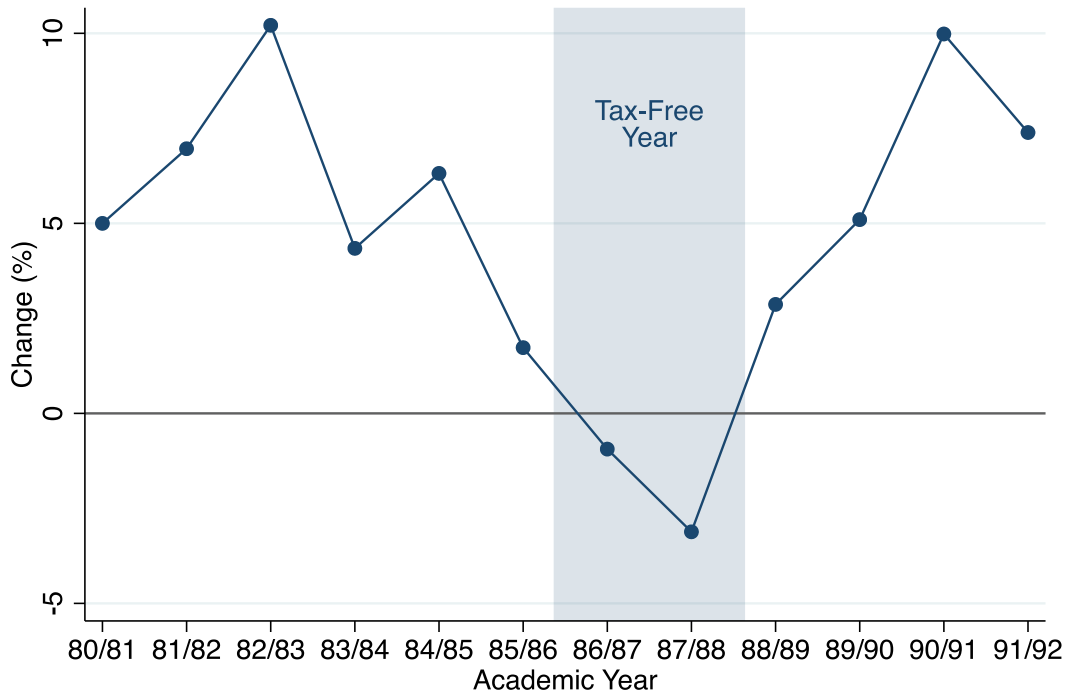
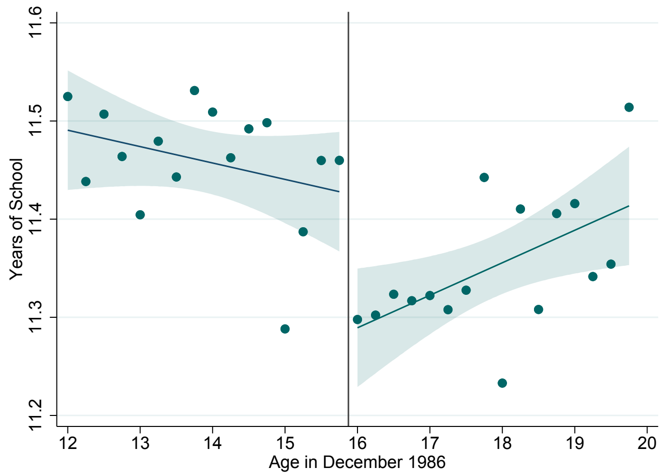
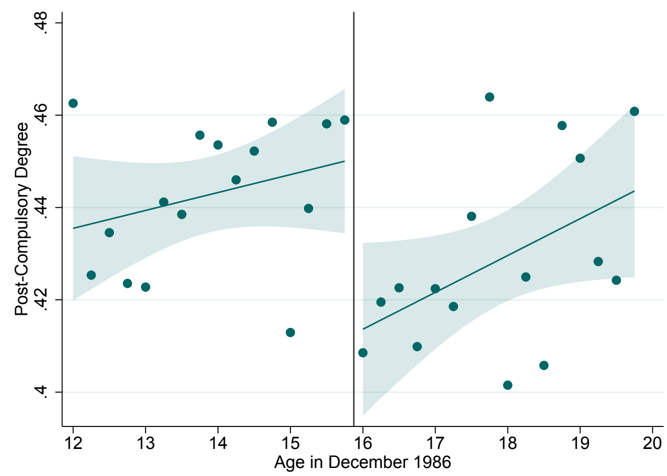


Figure A.1: Change in University Enrollment

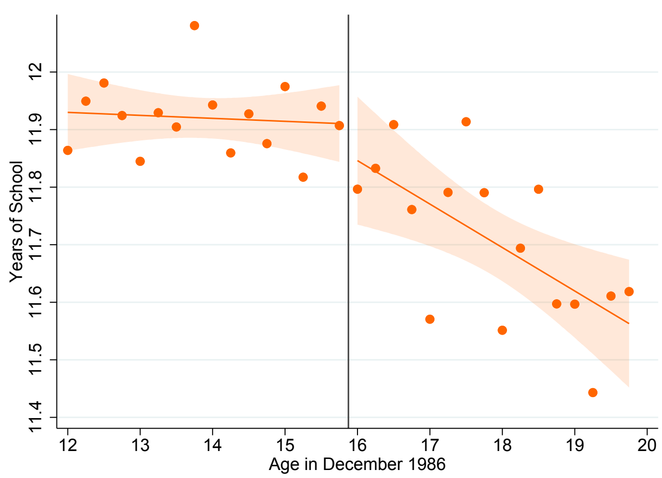
Notes: This figure plots the percentage change in the number of students enrolled in University education each academic year. The shaded area covers the two academic years that the tax-free year influenced.



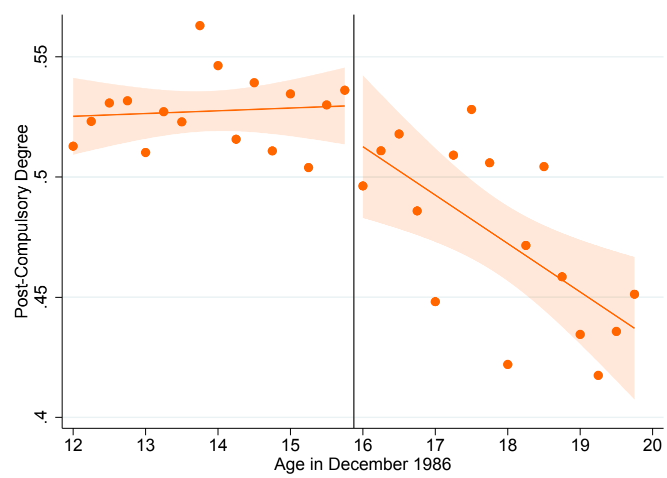
(a) Men: Years of school



(b) Men: Post-compulsory degree



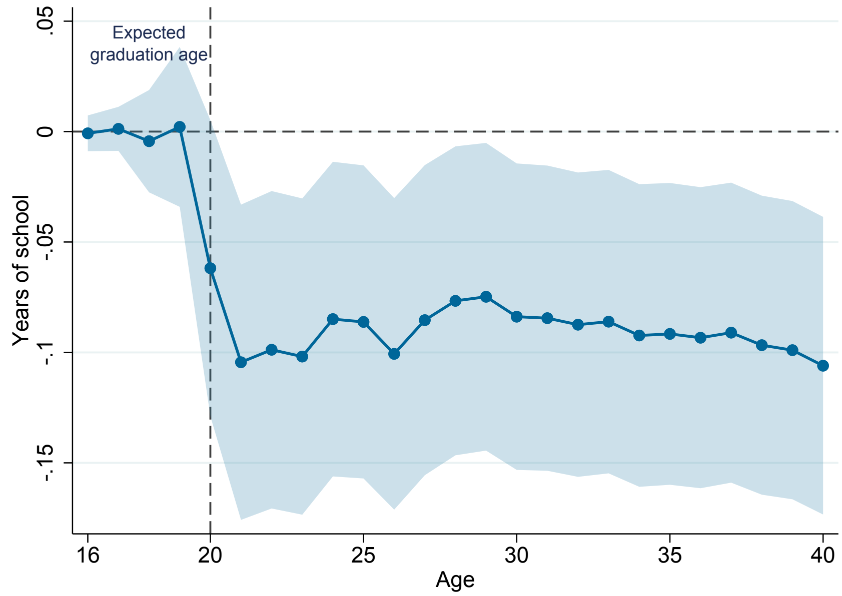
(c) Women: Years of school



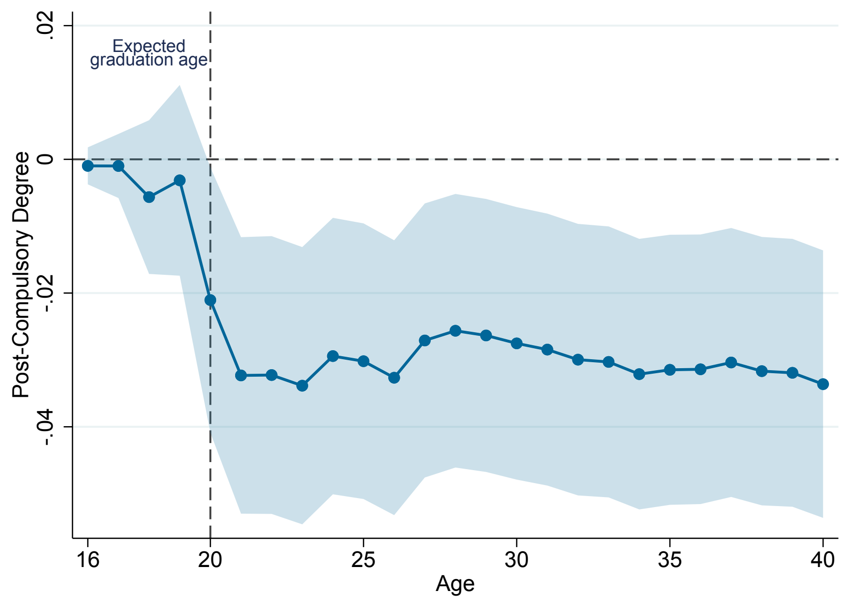
(d) Women: Post-compulsory degree

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.



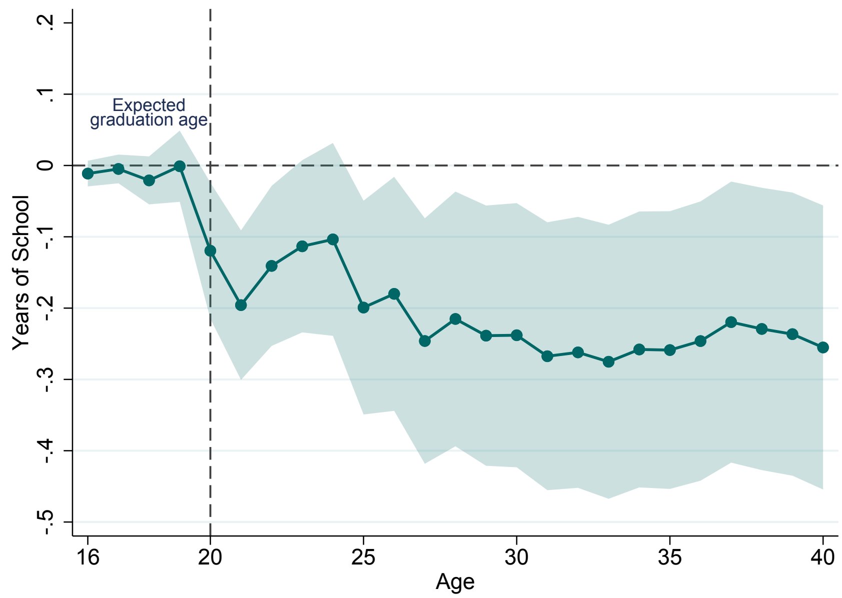
(a) Years of schooling



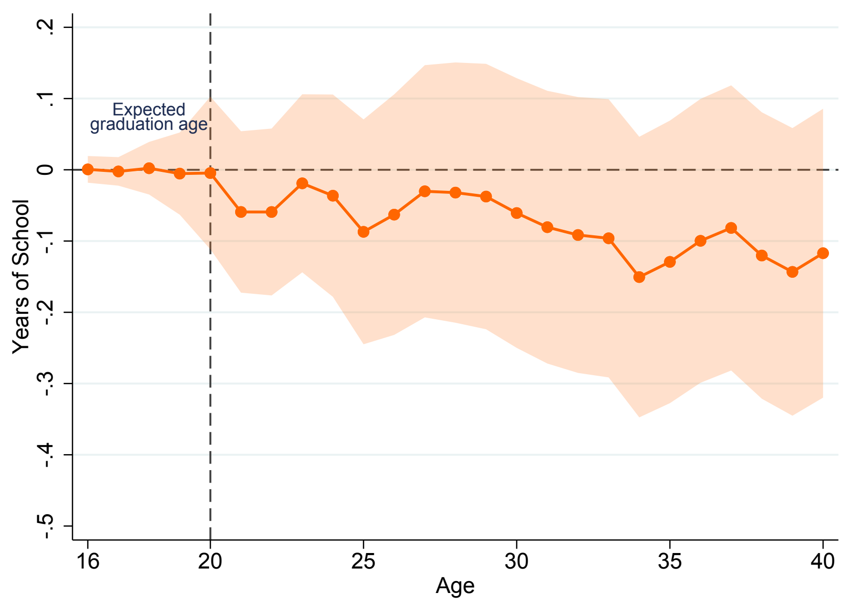
(b) Post-compulsory degree

Figure A.3: Dynamics of the Effect of Tax-Free Year on Years of School

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 a post-compulsory degree. 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.



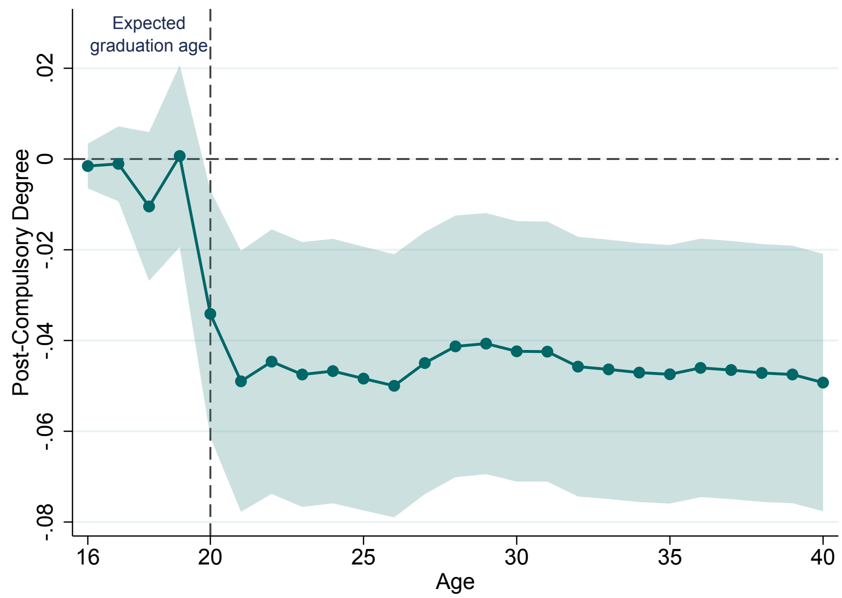
(a) Men: Years of school



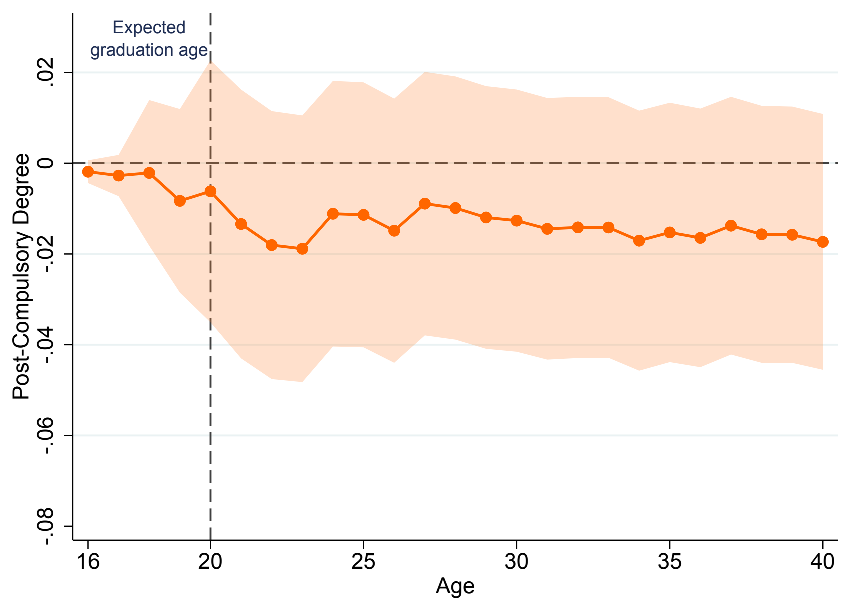
(b) Women: Years of school

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.



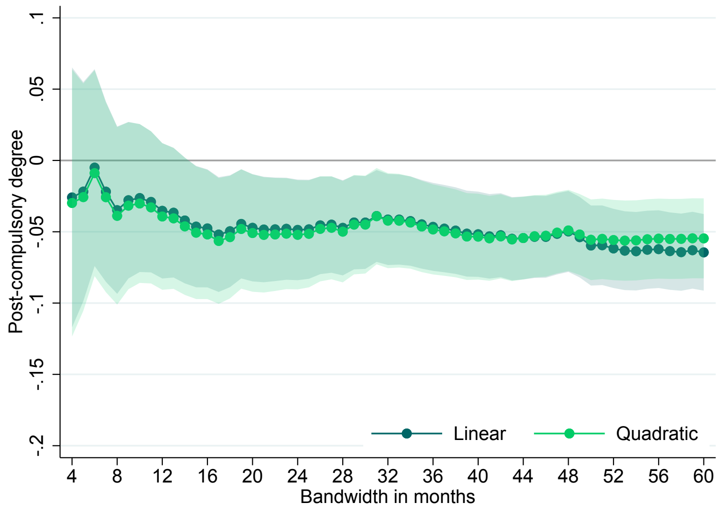
(a) Men: Post-Compulsory Degree



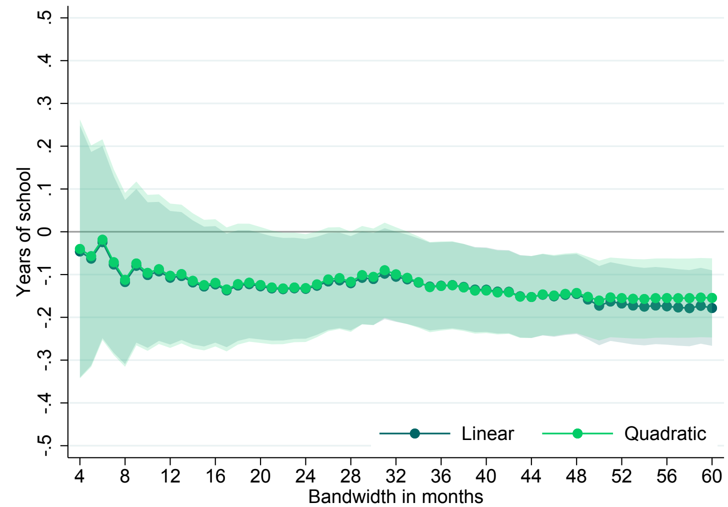
(b) Women: Post-Compulsory Degree

Figure A.5: Post-Compulsory Education

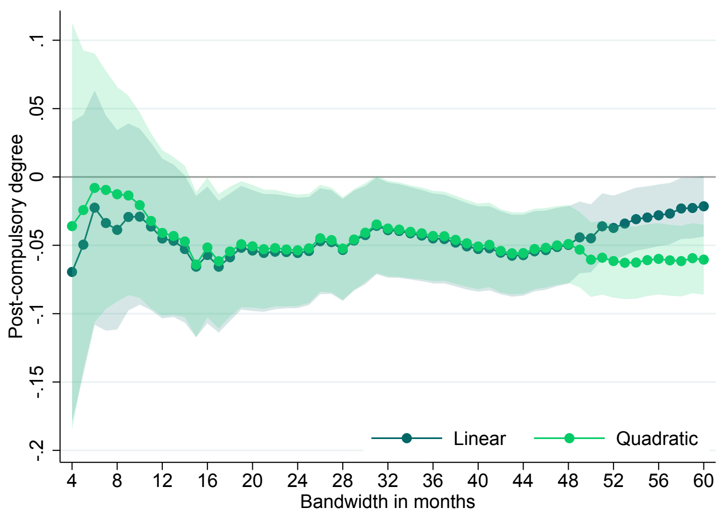
Notes: This figure plots estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panel (a) plots estimated effects on completion of 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.



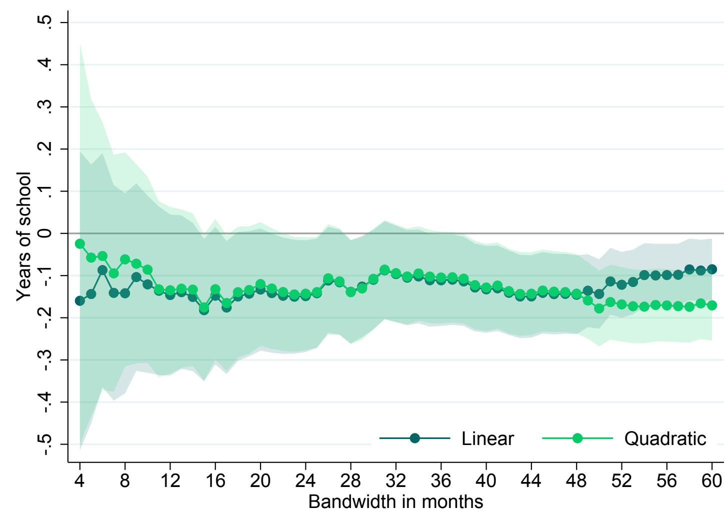
(a) Post-Compulsory Degree — Varying Bandwidth Left of Threshold



(b) Years of school — Varying bandwidth left of threshold



(c) Post-Compulsory Degree — Varying Bandwidth on Both Sides



(d) Years of School — Varying Bandwidth on Both Sides

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 with an indicator for completing a post-compulsory degree and years of school at age 40, using equation (1) for different bandwidths and polynomial order. Each dot is a separate regression estimate and all regressions control for pre-reform municipal fixed effects, number of children, and marital status. Panels (a) and (b) vary the bandwidth to the left of the threshold (i.e. the control group) while maintaining a 48-month bandwidth to the right (i.e. the treatment group). This way it includes everyone at normal upper-secondary schooling age, 16-20. Panels (c) and (d) vary the bandwidth at both sides of the threshold. 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.

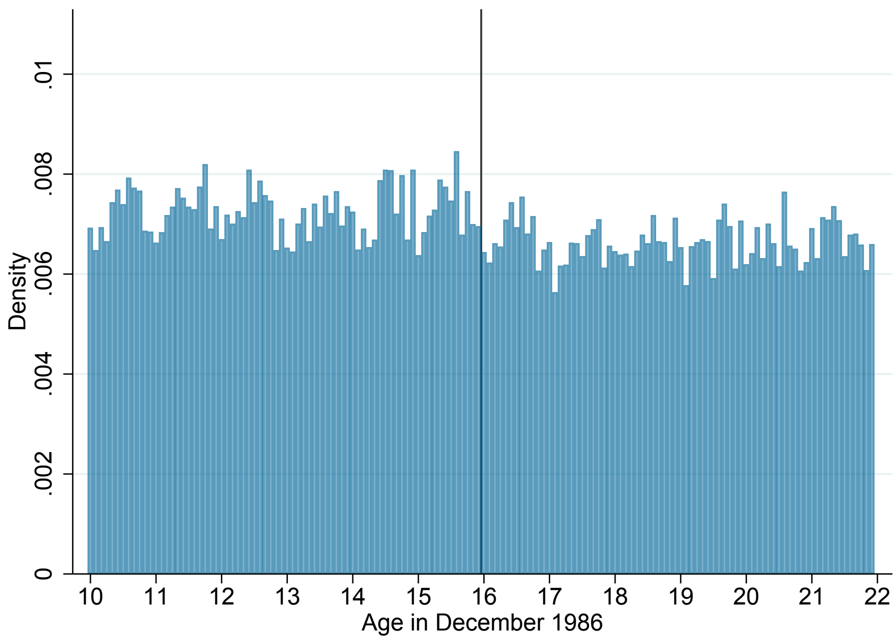
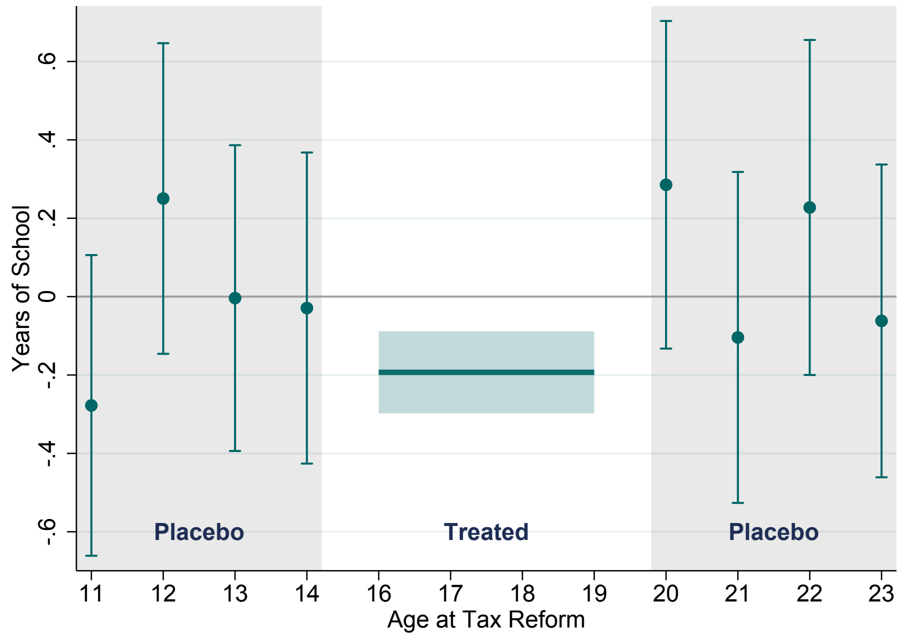
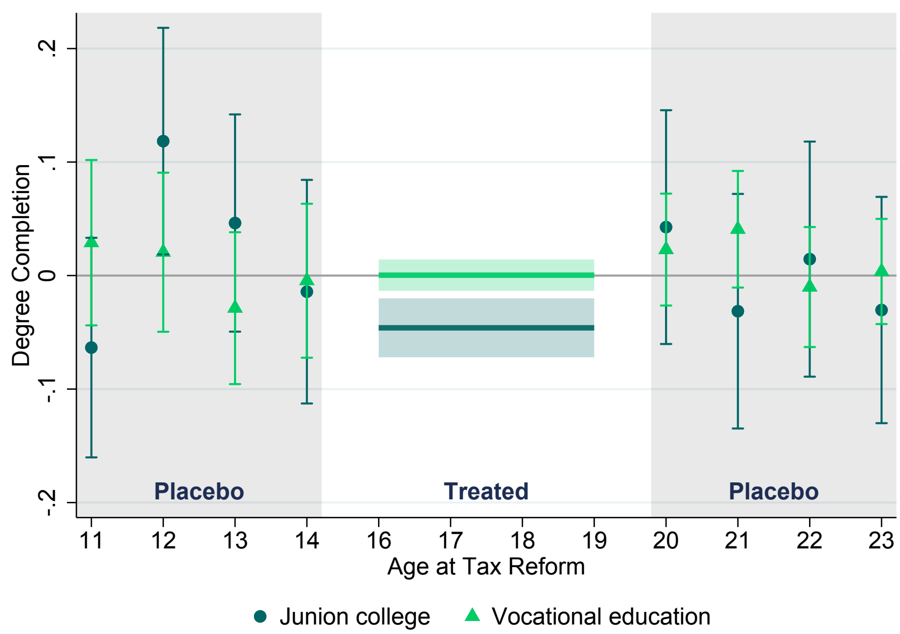


Figure A.7: Distribution of Births by Birth-Month Cohorts

Notes: This figure plots the distribution of births by birth-month cohorts of Icelanders who are between ages of 10 and 22 in December 1986. That is, cohorts born between January 1966 and December 1977.



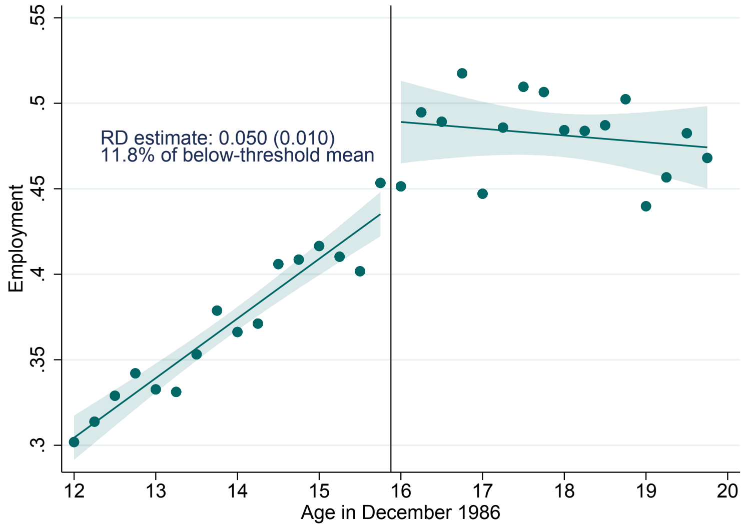
(a) Years of school



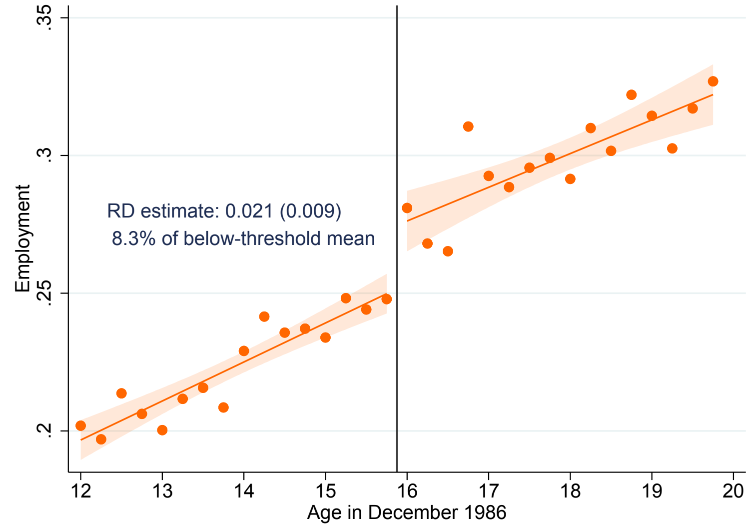
(b) Upper-secondary degrees

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

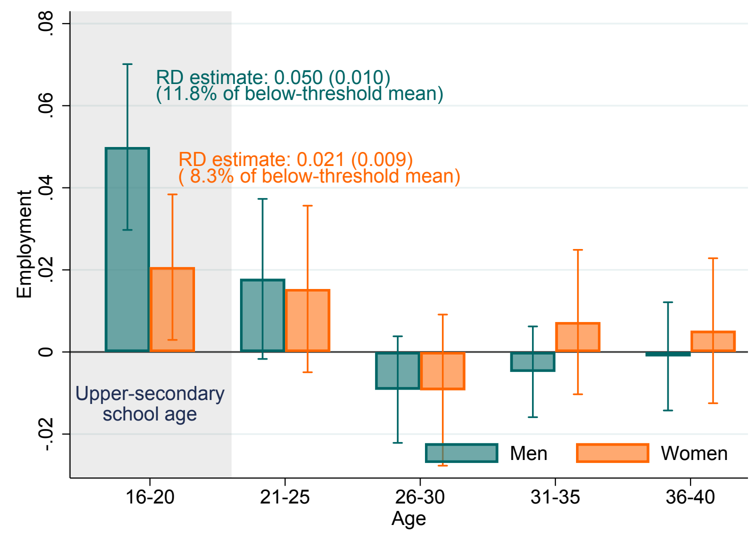
Notes: This figure plots the estimated effects of the actual and hypothetical placebo tax-free years on years of schooling (panel a) and upper-secondary degree completion (panel b) of men. Educational attainment is measured at age 40. Placebo tests are estimates of equation (1) at the actual compulsory school threshold but at placebo tax-free years. For example, the coefficient at age 14 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 to the right of the school-age threshold in 1989 were 14 years old in 1987, which is the age used to label the x-axis. For reference, the figure also plots the estimated treatment effect for those at upper-secondary schooling age during the actual tax-free year of 1987, i.e. 16-20-year-olds. 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.



(a) Men: Employment



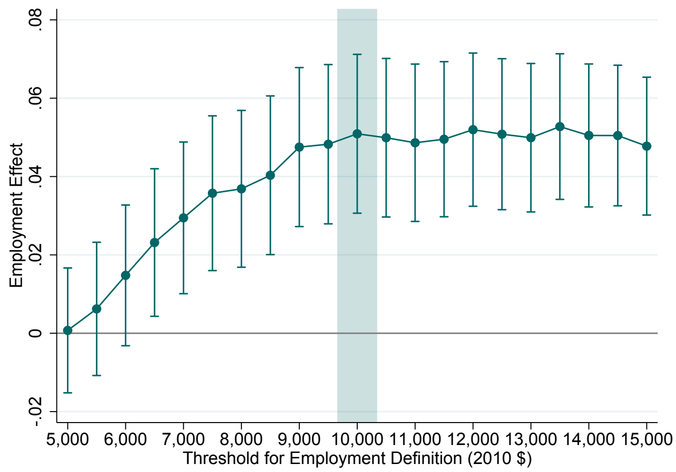
(b) Women: Employment



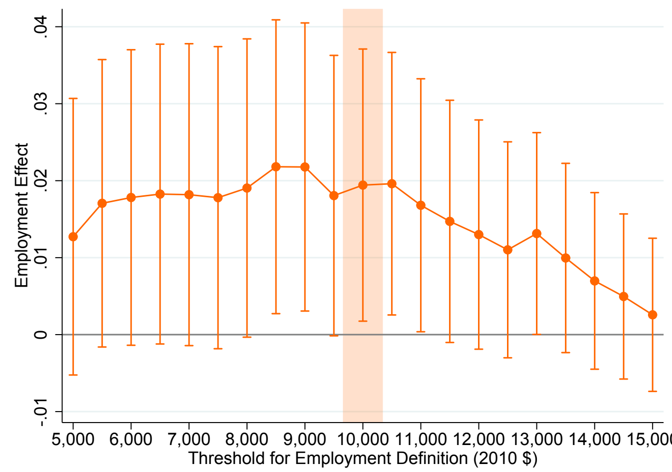
(c) Effect on Employment

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

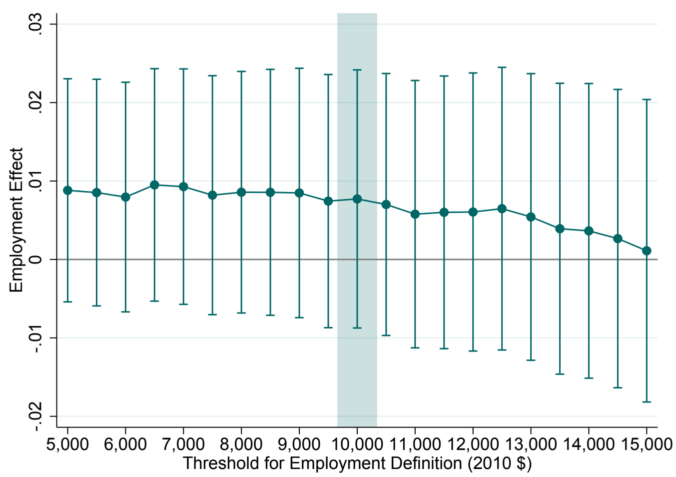
Notes: This figure studies the effect of the tax-free year on employment. Panels (a) and (b) plot the average employment at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Employment is defined as earning at least $10,000. Panel (c) plots RD estimates using equation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



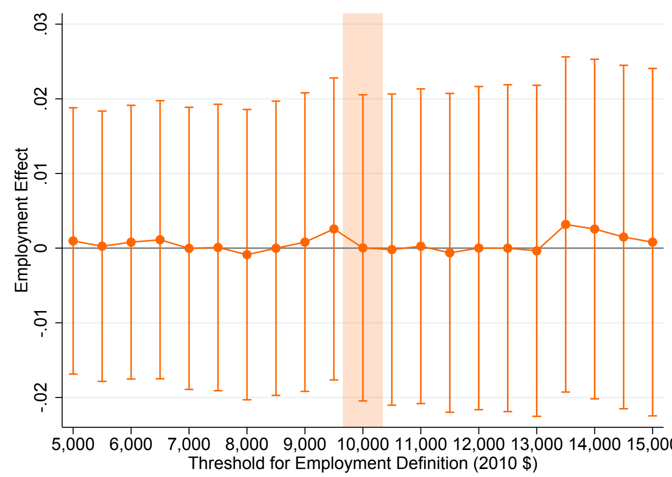
(a) Men: Effect on Employment at School Age



(b) Women: Effect on Employment at School Age



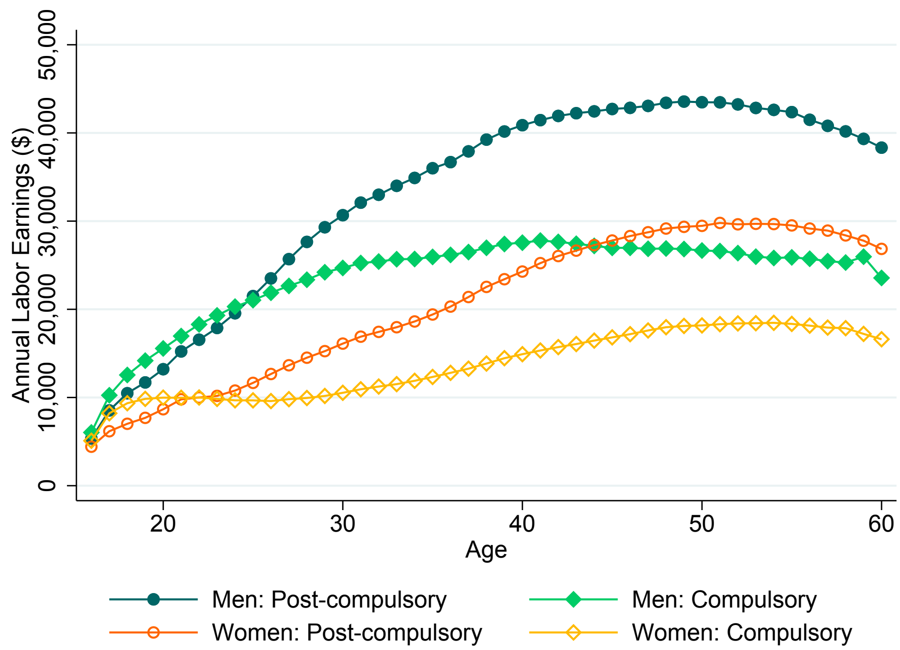
(c) Men: Effect on Employment at Prime Age



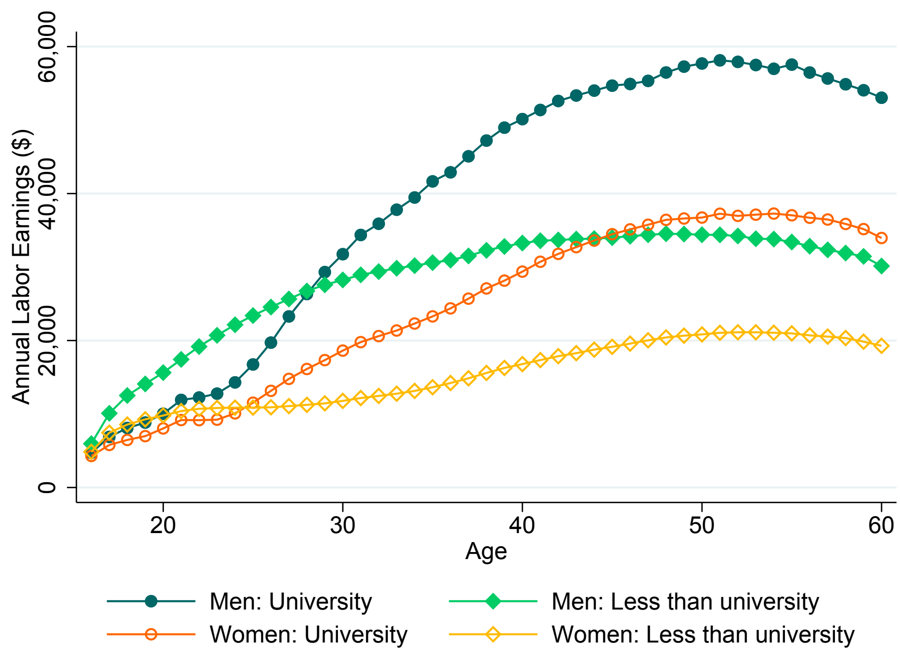
(d) Women: Effect on Employment at Prime Age

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.



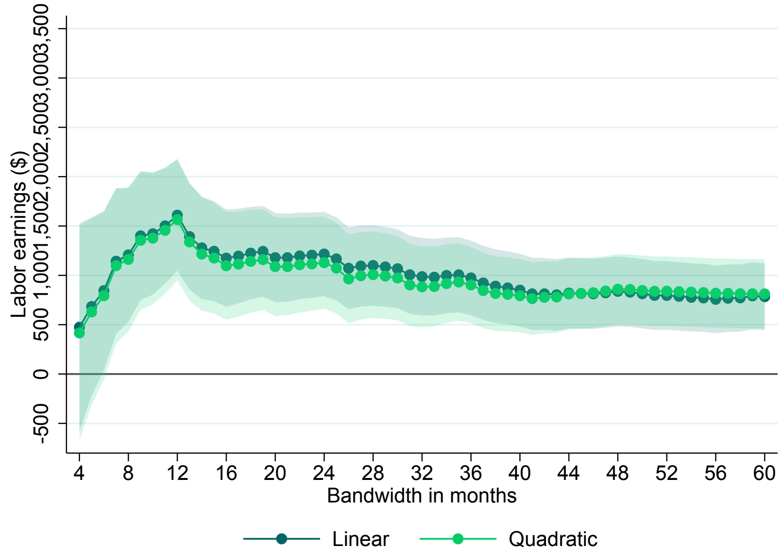
(a) Post-Compulsory Degree



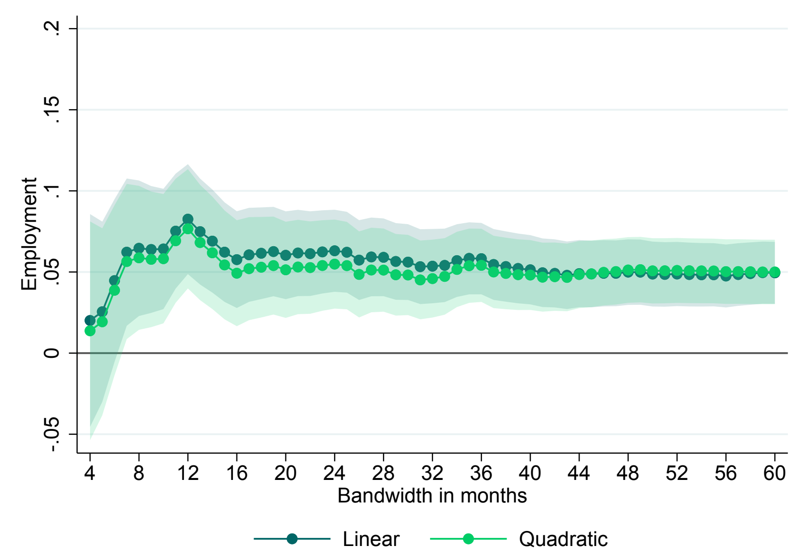
(b) University Degree

Figure A.11: Lifecycle Earnings Profiles by Education

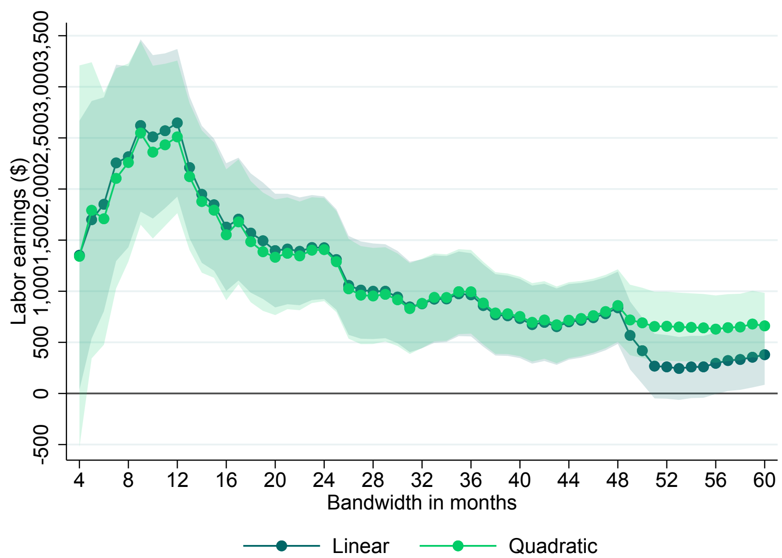
Notes: The figure plots earnings profiles by education for men and women in the cohorts born between 1947 and 1977. Panel (a) plots average earnings separately for those who complete a post-compulsory degree compared to those who only complete compulsory education. Panel (b) plots average earnings separately for those who complete a university degree compared to those who complete less education.



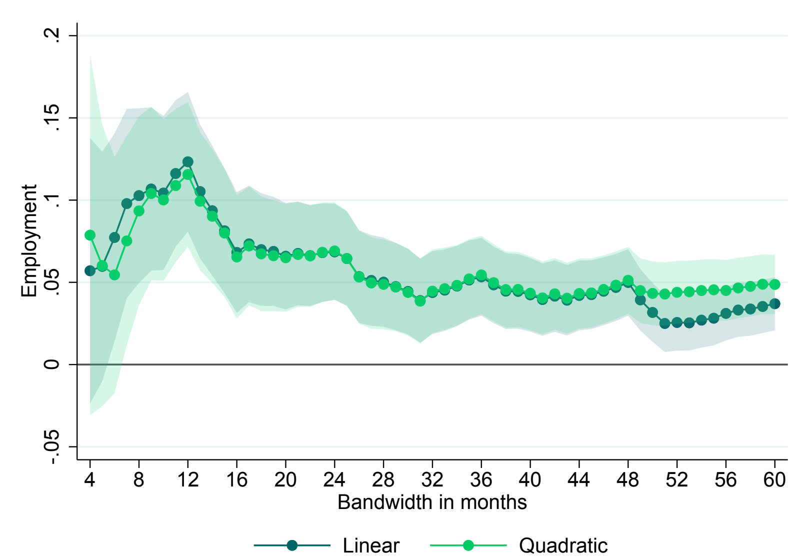
(a) Labor Earnings — Varying Bandwidth Left of Threshold



(b) Employment — Varying Bandwidth Left of Threshold



(c) Labor Earnings — Varying Bandwidth on Both Sides



(d) Employment— Varying Bandwidth on Both Sides

Figure A.12: Effect on Labor Market Outcomes: Sensitivity to the Choice of Bandwidth

Notes: This figure plots effects on labor market outcomes at ages 16-20, measured with labor earnings in 2010 $ and an indicator for being employed, using equation (1) for different bandwidths and polynomial order. Panels (a) and (b) vary the bandwidth to the left of the threshold (i.e. the control group) while maintaining 48-month bandwidth to the right (i.e. the treatment group). This way it includes everyone at normal upper-secondary schooling age, 16-20. Panels (c) and (d) vary the bandwidth at both sides of the threshold. 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.

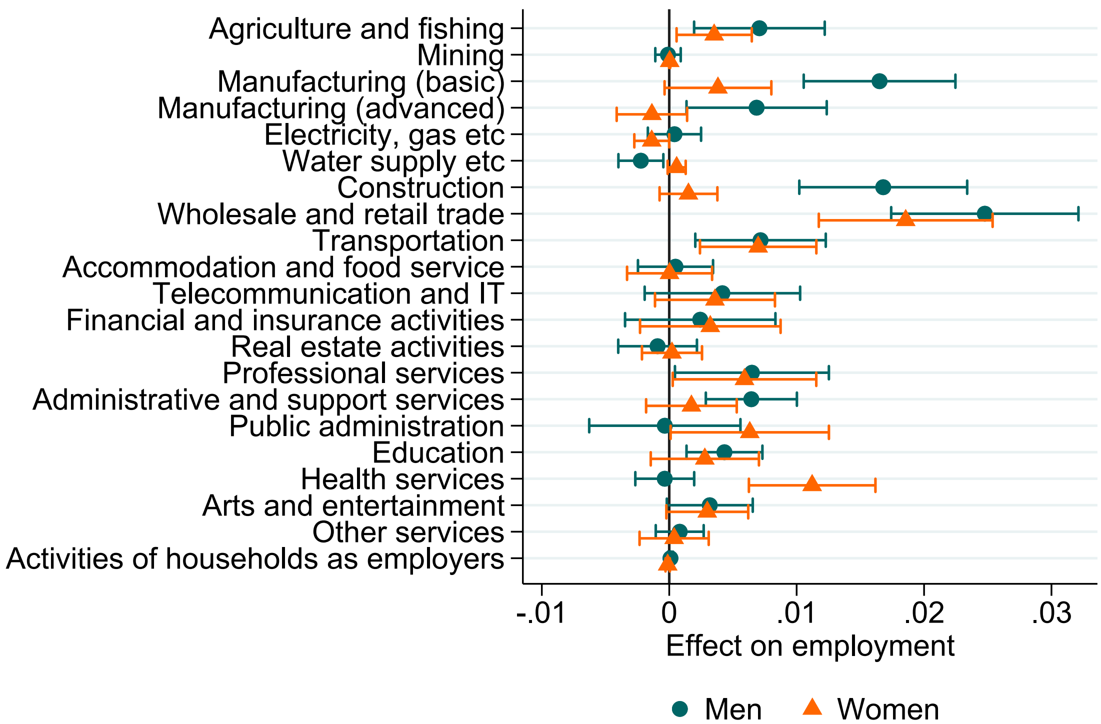


Figure A.13: Jobs at Prime Age

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.

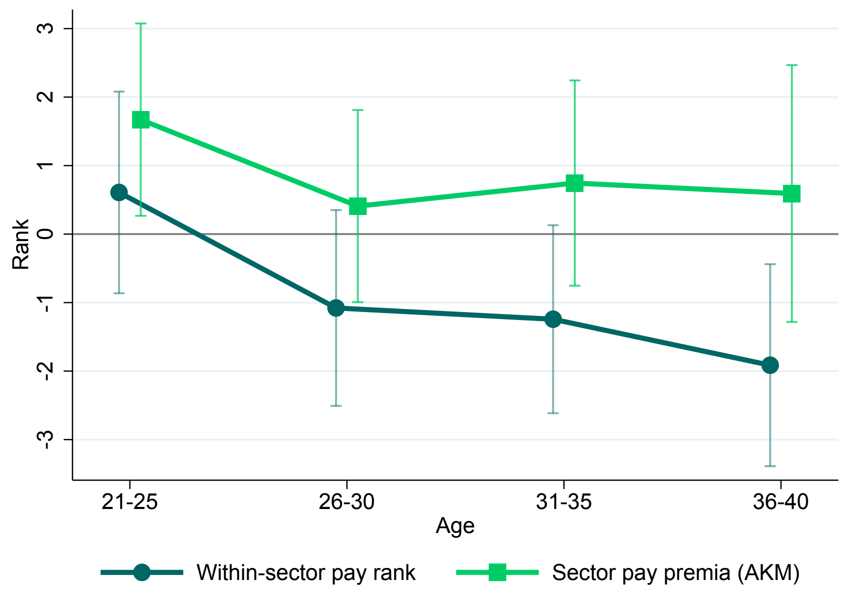


Figure A.14: Sector Pay Premia and Career Progression within Sectors

Notes: The figure plots the estimated treatment effects on the pay premia at the worker's sector of employment and the worker's relative position within the sector of employment. Pay premia is measured by AKM sector fixed effects. Career progression is measured by the rank of a worker's residualized ('Mincerized') earnings within the sector. 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. 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.

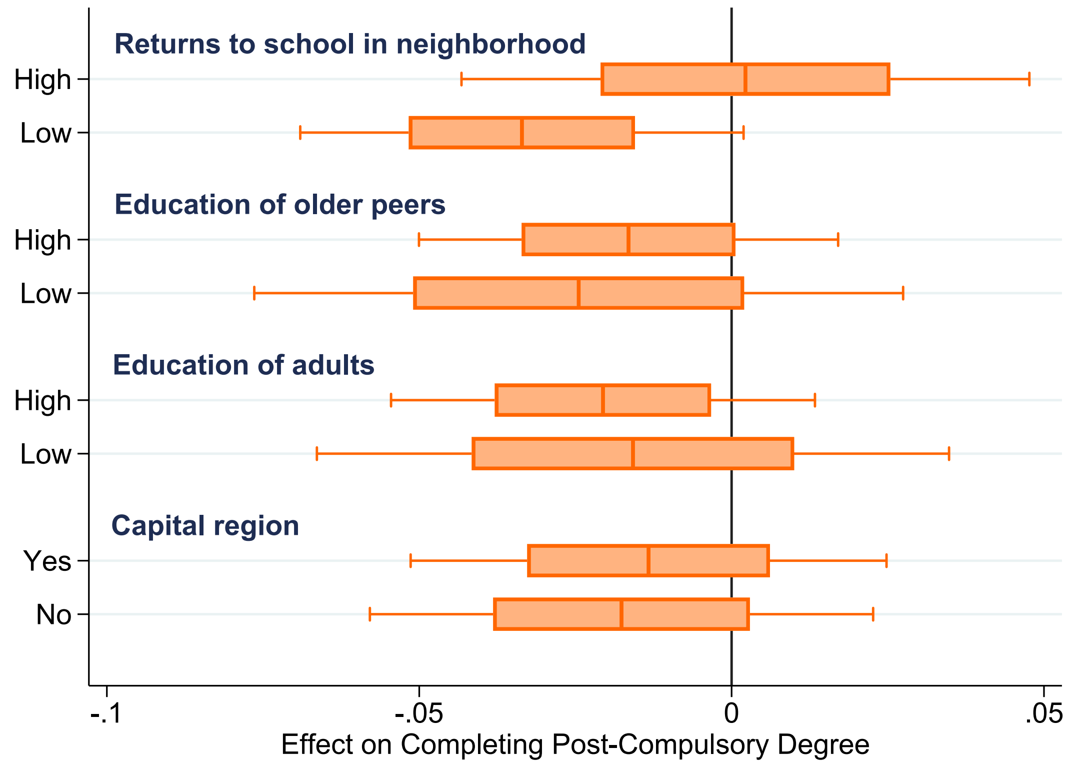


Figure A.15: School Dropout by Background — Women

Notes: The figure plots the estimated effects of the tax-free year on school dropout of women by background characteristics. *Returns to school in neighborhood* is the average ratio of earnings to years of school in the area. That is, I first measure the return in each municipality as the average ratio of earnings to years of school among adults in the three years prior to the tax reform, and then split students into two groups based on their location of residence. *Education of peers* is the share of 21-24-year-olds with post-compulsory education in the area. *Education of adults* is the share of individuals of age 35 and older with post-compulsory education in the area. *Capital region* denotes whether the individual lived in the Reykjavik area at age 16. The bars correspond to estimates of equation (1) where the outcome variable is an indicator of completing post-compulsory schooling, which is interacted with a group indicator splitting the sample in two based on these background characteristics. 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 whiskers display the 95% confidence interval.

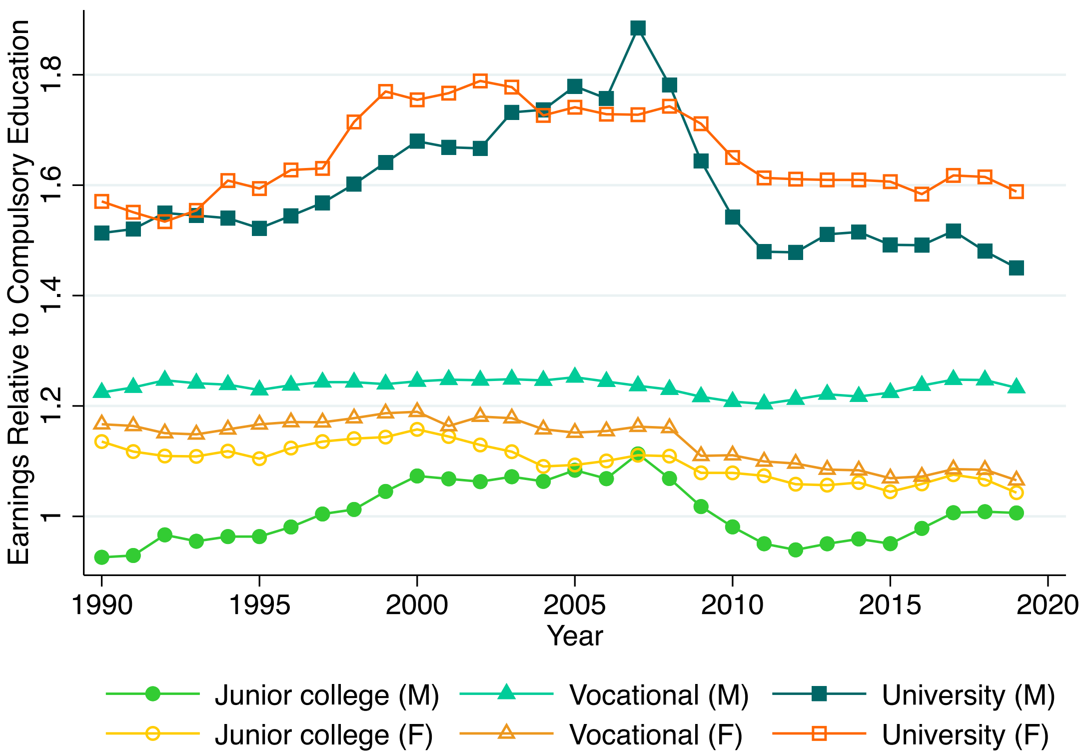


Figure A.16: Returns to Education

Notes: This figure plots the average labor earnings of male (M) and female (F) workers by education level. Numbers are relative to the average earnings of workers with compulsory education.

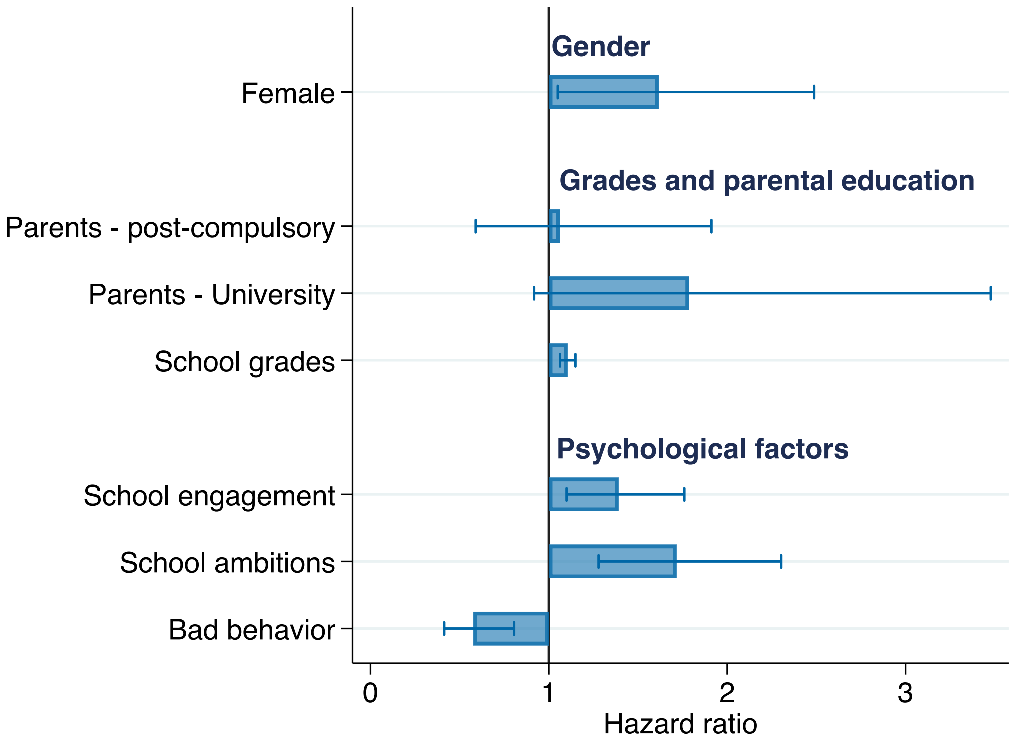


Figure A.17: Factors Influencing School Completion

Notes: This figure reports the hazard ratios from a Cox proportional hazard model of factors influencing school completion. A hazard ratio higher than 1 means students with (more of) a given characteristic are more likely to complete upper-secondary school than those without (with less) that characteristic. The data is from a randomized longitudinal study of 2750 students in upper-secondary school in 2007 (Blöndal et al., 2016). The students were interviewed at ages 17 and 18. The data was then linked to register data from Statistics Iceland on school outcomes at ages 23 and 24. The figure is based on regression estimates in Þorláksson (2019), who estimates a regression where the outcome variable is an indicator of having completed upper-secondary school at age 23 or 24 on potential factors influencing dropout, including gender, school grades, parental education, and psychological factors. The variables on parental education are indicators. *School grades* is a standardized GPA from national-level exams in Icelandic and Math at the end of compulsory education. *School engagement* aggregates four factors measuring students' well-being when they are at school and their connection to the school itself. *School ambitions* aggregates five factors measuring students' ambitions and vision for future education. *Bad behavior* aggregates five factors measuring participation and activity in school. In the figure above I plot the coefficients from this regression as hazard ratios. The whiskers display the 95% confidence intervals.

E Supplementary Tables

Table A.3: Effect on Educational Attainment — Robustness

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

Notes: This table reports the coefficient of the treatment indicator (age above compulsory-schooling age threshold) according to the regression equation (1). The specification is either "Benchmark" which refers to my estimates reported in Table 2, 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 education outcome specified in the row heading. The estimates are based on local-linear regressions for individuals at age 21 and allow for different coefficients on each side of the cutoff. Outcome mean refers to the averages of the dependent variable for 12 months below the threshold (control group). Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Effect on Labor Market Outcomes

	Labor Earnings ($)				Employment			
	16-20		36-40		16-20		36-40	
	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)
					A. Men			
	886***	838***	-1,597*	-2,147**	0.053***	0.050***	0.007	-0.001
	(191)	(180)	(918)	(871)	(0.011)	(0.010)	(0.008)	(0.007)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	10,487	10,487	41,927	41,927	0.425	0.425	0.863	0.863
Observations	78,247	78,247	76,269	76,269	78,247	78,247	76,269	76,269
					B. Women			
	85	96	-446	-262	0.019**	0.021**	0.000	0.005
	(102)	(99)	(574)	(531)	(0.009)	(0.009)	(0.010)	(0.009)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	7,342	7,342	26,247	26,247	0.248	0.248	0.796	0.796
Observations	74,884	74,884	74,274	74,274	74,884	74,884	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. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. Robust standard errors, clustered at the individual level, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.5: Effect on Labor Market Outcomes — Robustness

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

	Labor Earnings ($)		Employment	
	16-20	36-40	16-20	36-40
	(1)	(2)	(3)	(4)
	762***	-1,844*	0.031**	-0.008
	(207)	(1,027)	(0.014)	(0.011)
Controls	Yes	Yes	Yes	Yes
Outcome mean	10,487	41,927	0.425	0.863
Observations	153,131	150,543	153,131	150,543

Notes: This table reports difference-in-discontinuity estimates, which are obtained using a version of regression equation (1) that is fully interacted with a male indicator. The table reports the coefficient on the treatment indicator interacted with the male indicator, reflecting the difference in the discontinuity between men and women. 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. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$