Online Appendix of:

Transitory Earnings Opportunities and Educational Scarring of Men

Jósef Sigurdsson

May 19, 2026

A Data Appendix

A.1 Educational Attainment

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

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

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

A.2 Occupation and Sector Classification

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

ILO's website.

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

B General Equilibrium Effects

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

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

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

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

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

Aggregate output in period t is also a CES:

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

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

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

$$\begin{aligned} w^L_{jt} &= \frac{\partial Y_t}{\partial L_{jt}} = \frac{\partial Y_t}{\partial L_t} \times \frac{\partial L_t}{\partial L_{jt}} \\ &= A_{Lt} L_t^{\rho-\eta} \Psi \times \beta_j L_{jt}^{\eta-1} \end{aligned} \tag{13}$$

where

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

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

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

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

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

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

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

$$\Delta \ln\left(\frac{w^L_j}{w^H_j}\right) = -\frac{1}{\sigma_A} \Delta \ln\left(\frac{L_j}{H_j}\right)$$

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

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

B.2 Calibration

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

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

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

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

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

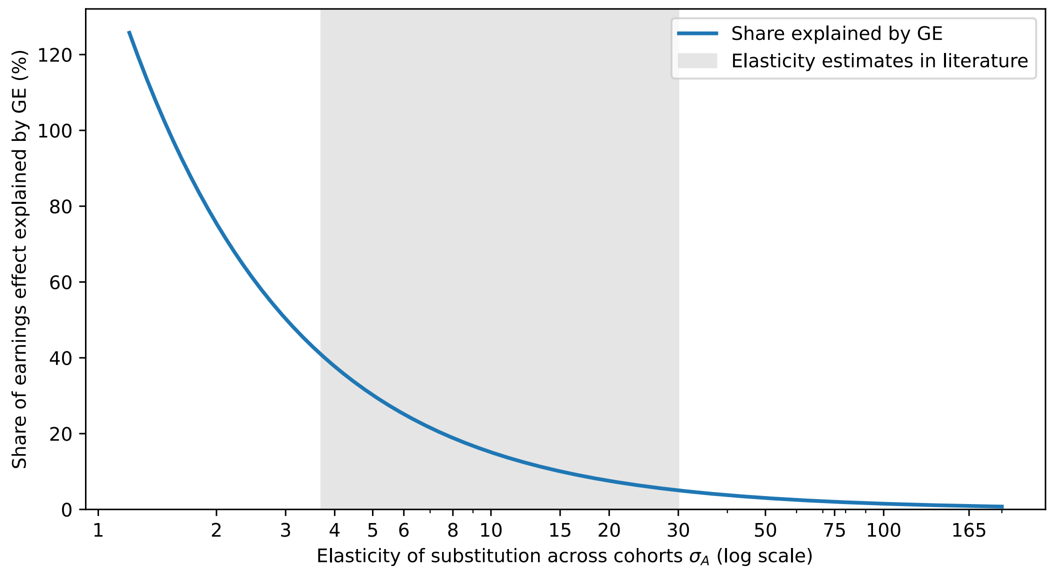


Figure A.1: Calibrated general equilibrium effects under alternative elasticities of substitution

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

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

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

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

C Model Appendix and Extensions

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

C.1 General Setup

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

C.2 Benchmark: Heterogeneous Ability

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

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

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

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

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

C.3 Liquidity Constraints and Psychic Costs

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

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

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

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

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

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

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

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

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

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

C.4 Present Bias and High Discounting

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

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

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

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

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

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

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

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

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

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

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

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

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

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

which is independent of δ.

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

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

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

C.5 Re-entry Frictions

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

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

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

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

Differentiating with respect to τ_0 yields

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

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

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

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

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

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

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

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

Let

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

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

1. **Temporary dropout only.** *If $\gamma \leqslant \gamma_T$, then $\theta_1^*(\gamma) \leqslant \theta_0^B(\gamma)$ and all tax-free-year–induced dropouts have $\theta \geqslant \theta_1^*(\gamma)$; the tax-free year generates only temporary dropout.*

2. **Permanent dropout only.** *If $\gamma \geqslant \gamma_P$, then $\theta_0^{TFY}(\gamma) \leqslant \theta_1^*(\gamma)$ and all tax-free-year–induced dropouts have $\theta < \theta_1^*(\gamma)$; the tax-free year generates only permanent dropout.*

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

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

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

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

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

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

This gap is nonnegative if and only if

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

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

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

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

This gap is nonpositive if and only if

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

C.6 Misperception of Returns

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

D Estimating Misperception of Schooling Returns

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

D.1 Permanent-dropout calibration

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

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

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

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

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

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

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

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

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

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

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

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

and the perceived return required to rationalize dropout:

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

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

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

D.2 Re-entry calibration

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

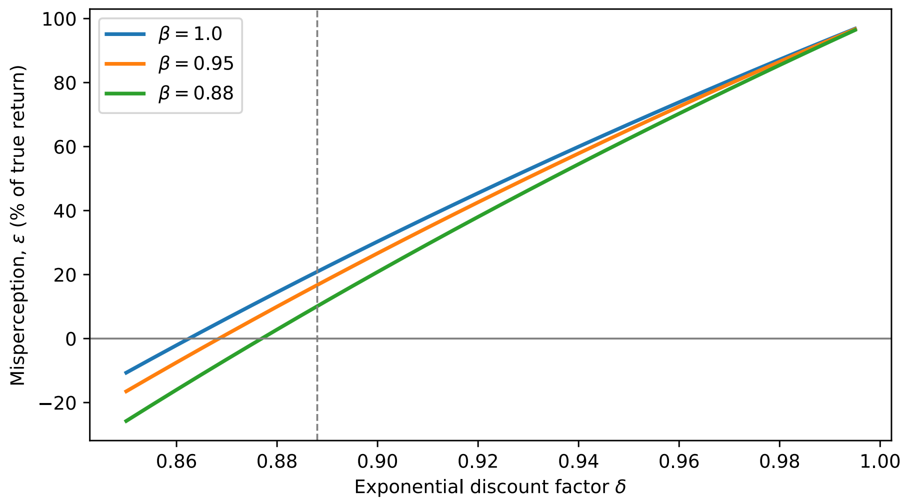
If students plan to return to school after working in $t = 0$, the relevant margin is the re-enrollment decision. From the $t = 1$ vantage point, re-entry is optimal whenever

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

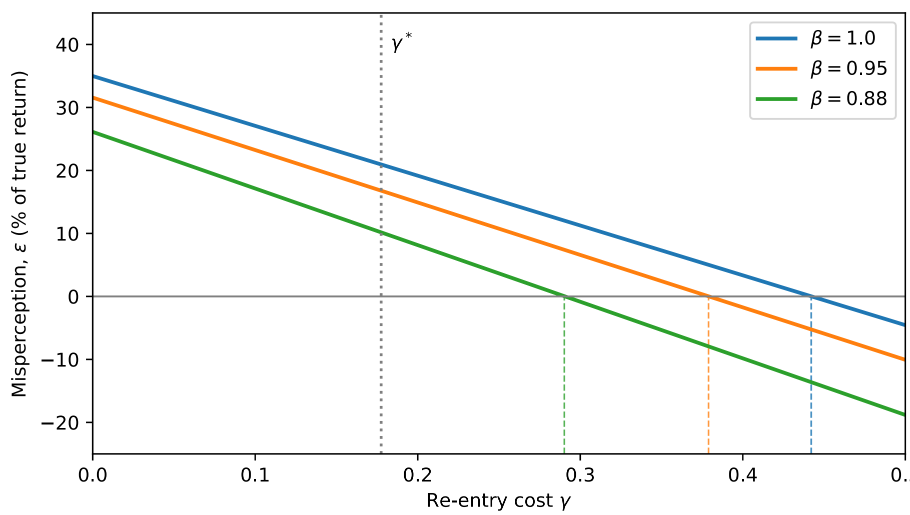
For the marginal student, this condition holds with equality. Let r_1^* denote the perceived net return at re-entry that rationalizes indifference. Solving yields

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

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



(a) Permanent-dropout calibration



(b) Re-entry calibration

Figure A.2: Calibrated misperception

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

re-entry margin is therefore

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

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

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

D.3 Benchmarks for Misperception of Returns

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

D.3.1 Internal Benchmark: Parental Returns to Education

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

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

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

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

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

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

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

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

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

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

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

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

and compute misperception as

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

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

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

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

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

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

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

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

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

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

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

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

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

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

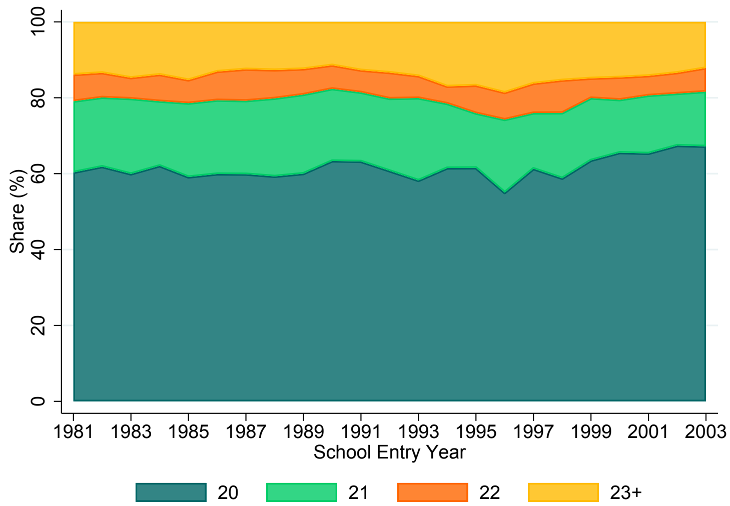


Figure A.3: Age at High School Graduation

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

and would graduate in 1995 if graduating on time.

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

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

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

E.1 Enrollment, Completion, and Dropout Patterns

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

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

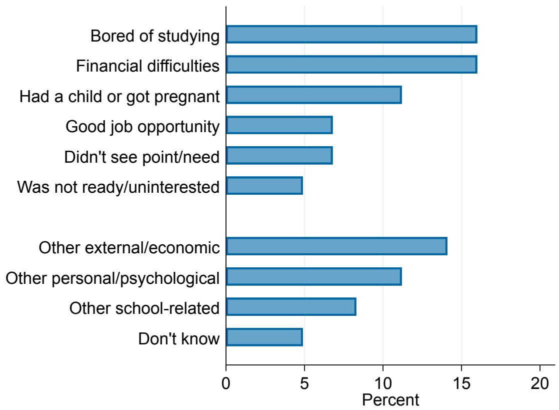
Figure A.3 shows that these patterns extend well beyond the 1975 cohort. Plotting the age at graduation for all high-school graduates from the early 1980s through the 2010s reveals a strikingly stable picture: about 60 percent graduate at age 20, corresponding to on-time completion, while 15–20 percent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

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

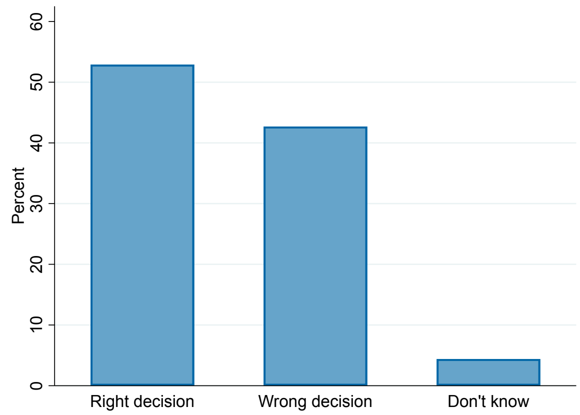
E.2 Dropout Decisions and Re-entry Barriers

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

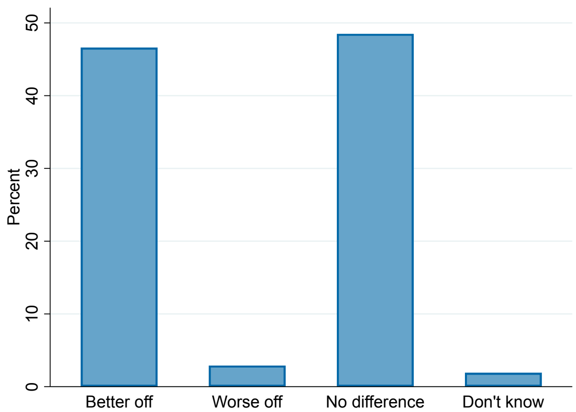
Panel (a) shows the most commonly cited primary reasons for dropping out. Beyond boredom with studying and pregnancy, financial difficulties and employment opportunities stand out: financial difficulties are cited by 16 percent of dropouts and a good job opportunity by 7 percent, together accounting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



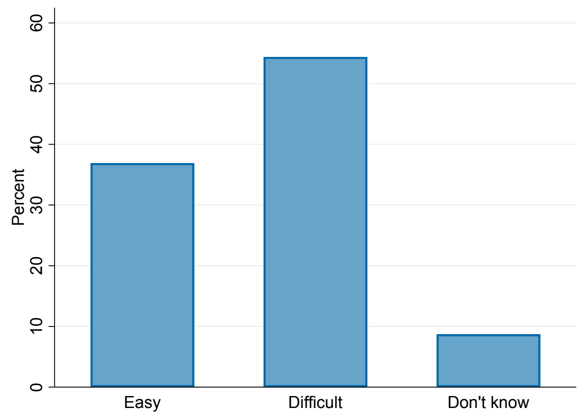
(a) Main reasons for dropping out



(b) Was dropping out the right decision?



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



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

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

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

the findings of this paper.

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

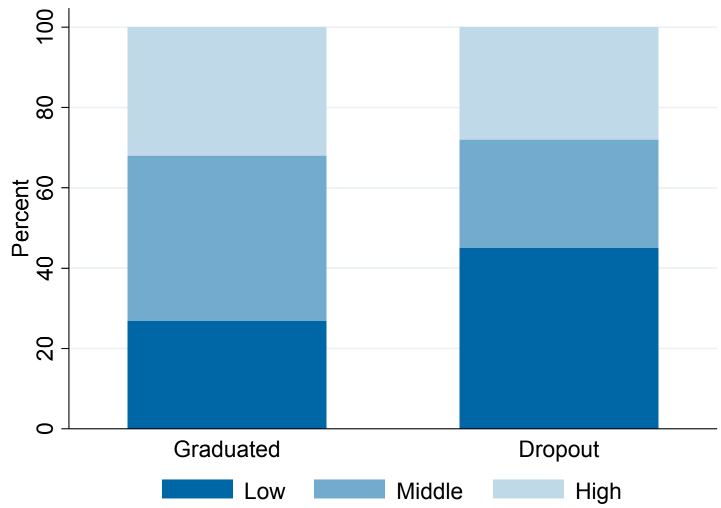


Figure A.5: Self-Esteem by Educational Status

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

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

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

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

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

F Supplementary Figures

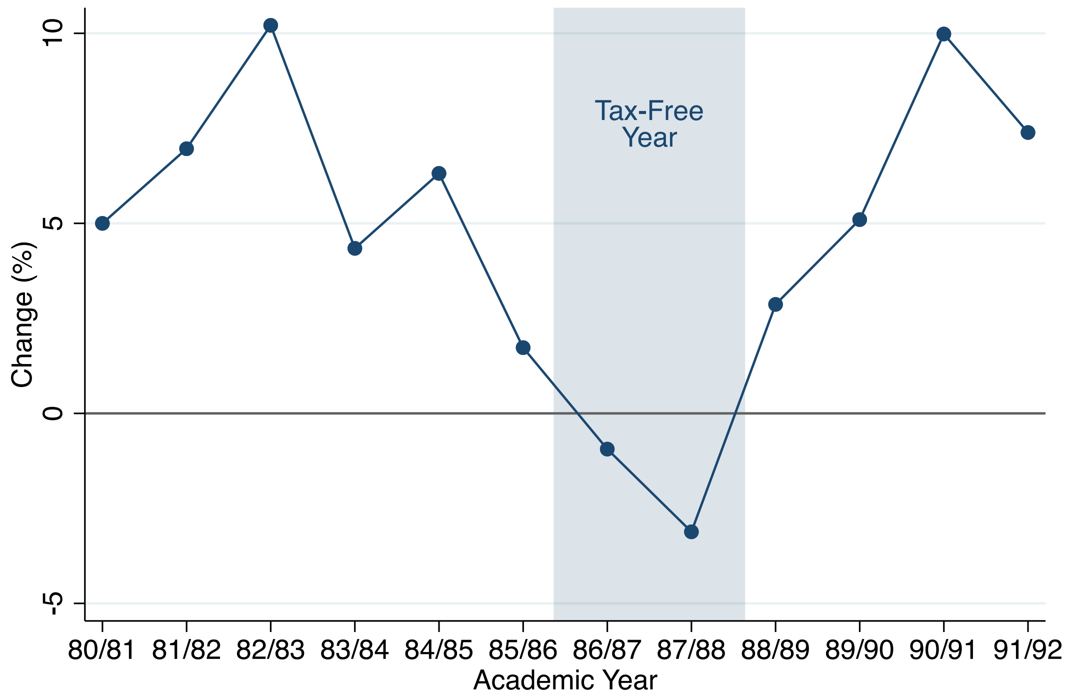
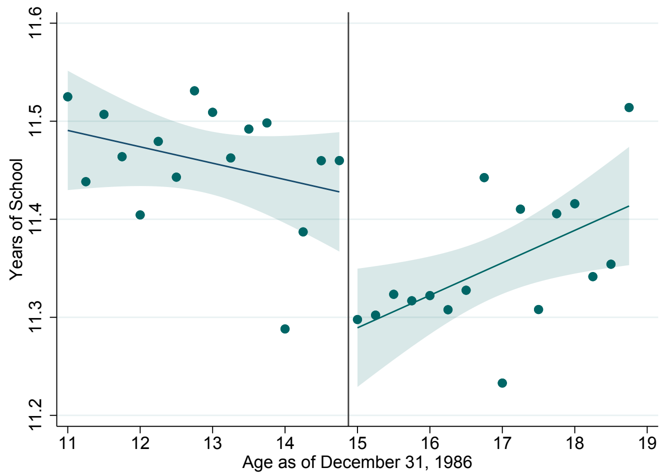
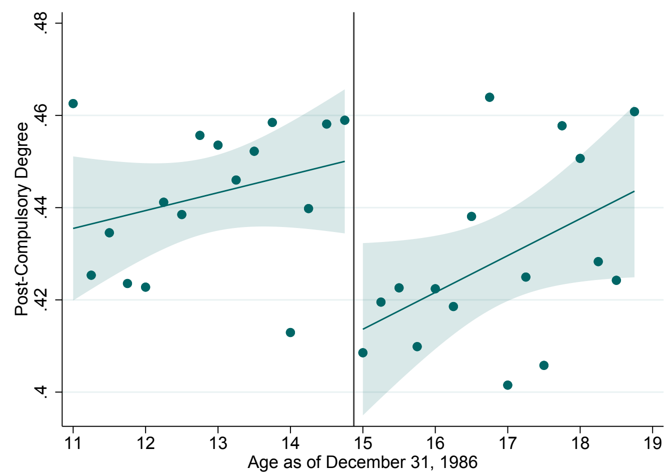


Figure A.6: Change in University Enrollment

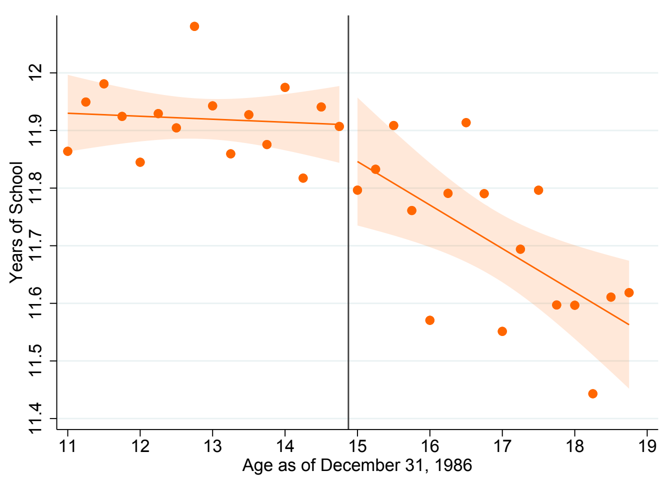
Notes: This figure plots the percentage change in the number of students enrolled in University education each academic year. The shaded area covers the two academic years that the tax-free year influenced.



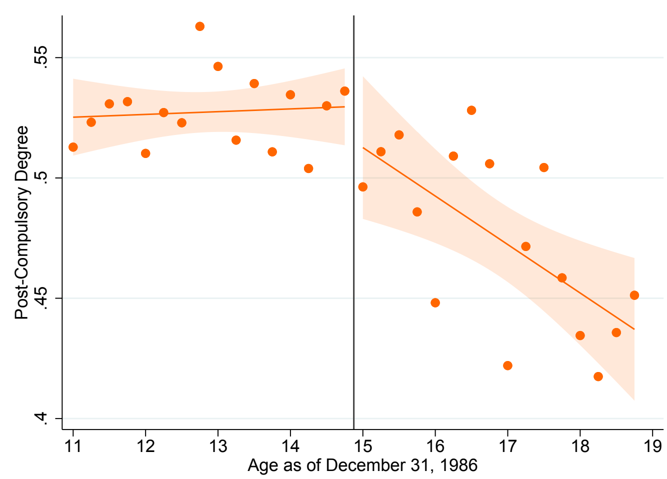
(a) Men: Years of school



(b) Men: Post-compulsory degree



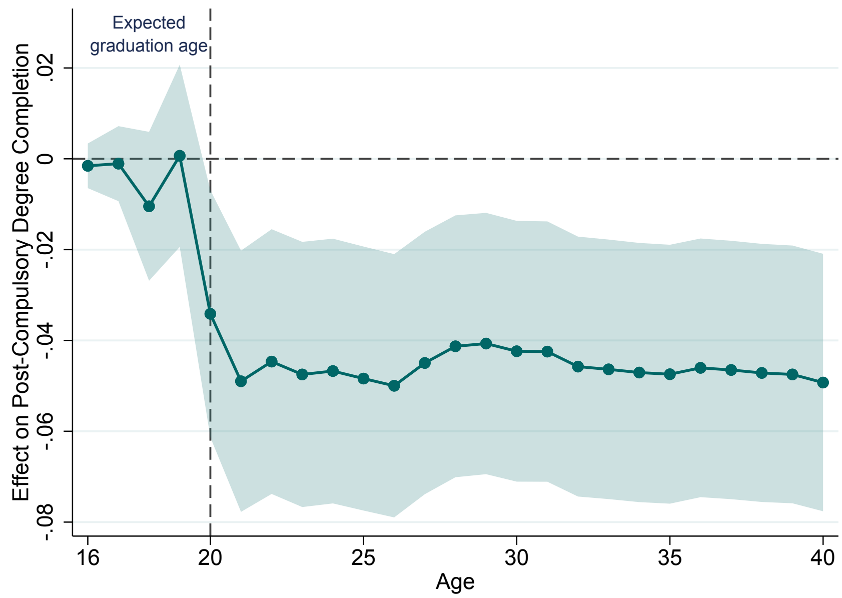
(c) Women: Years of school



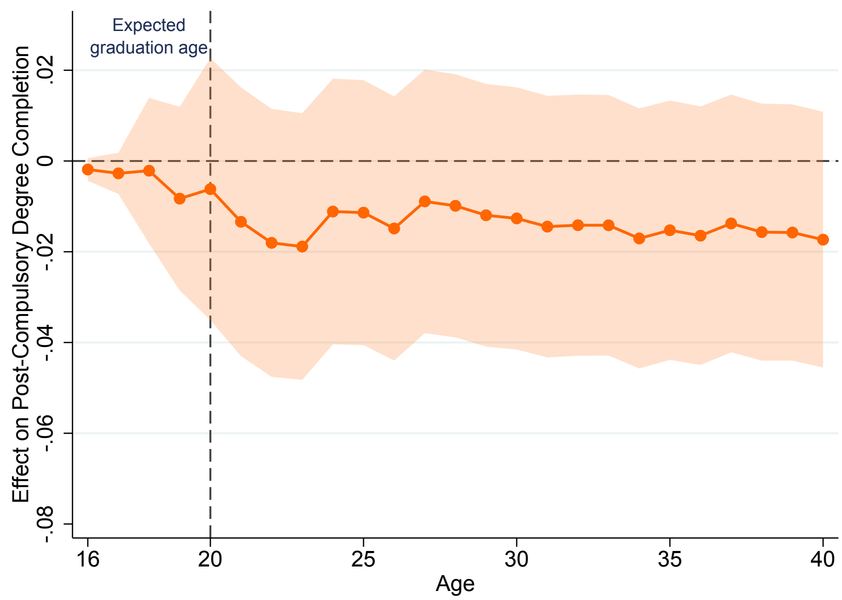
(d) Women: Post-compulsory degree

Figure A.7: Educational Attainment — Men and Women

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



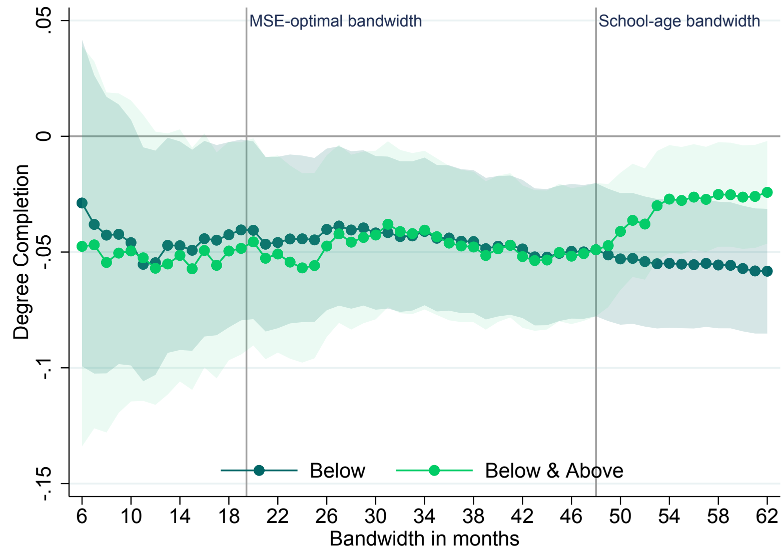
(a) Men: Post-Compulsory Degree



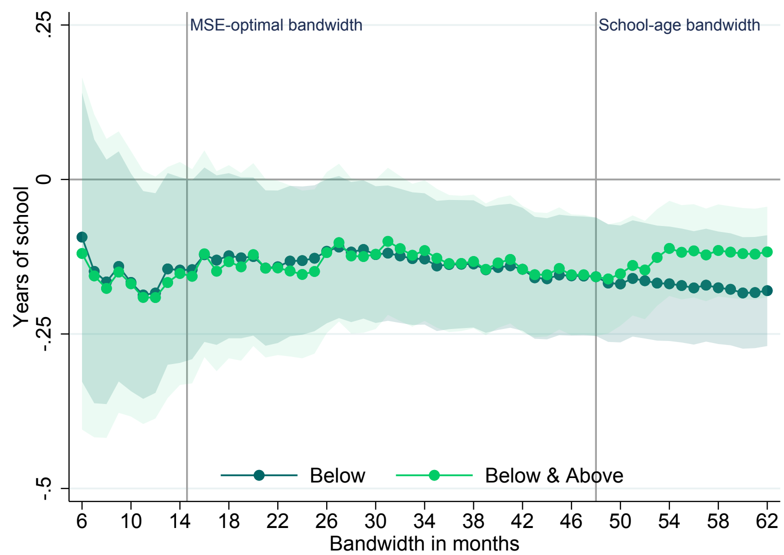
(b) Women: Post-Compulsory Degree

Figure A.8: Post-Compulsory Education

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



(a) Post-Compulsory Degree



(b) Years of school

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

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

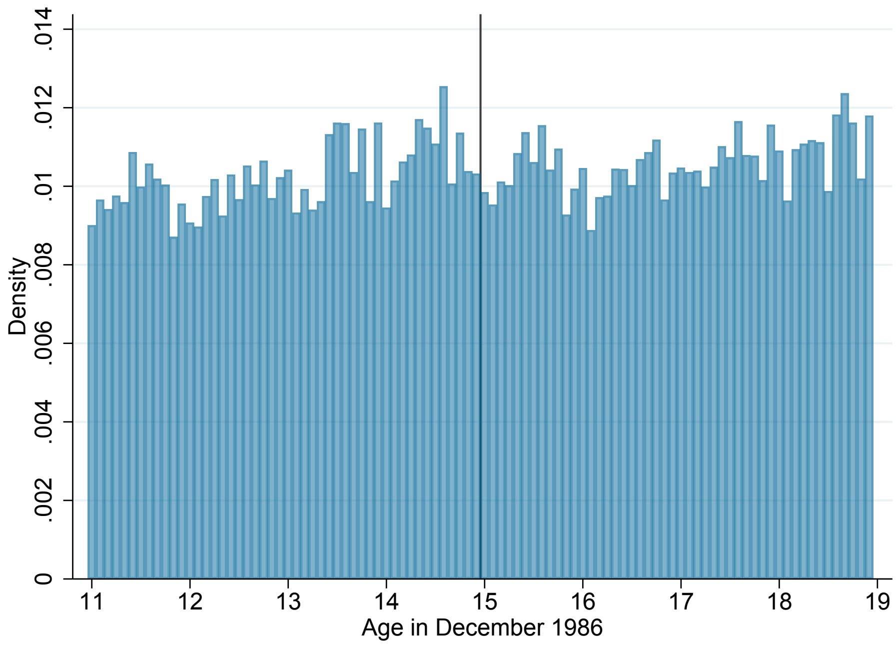
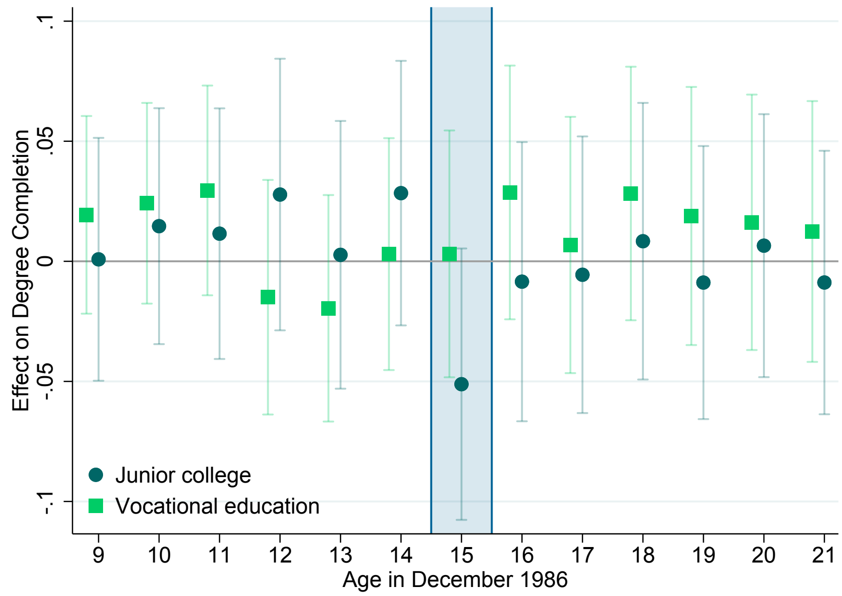
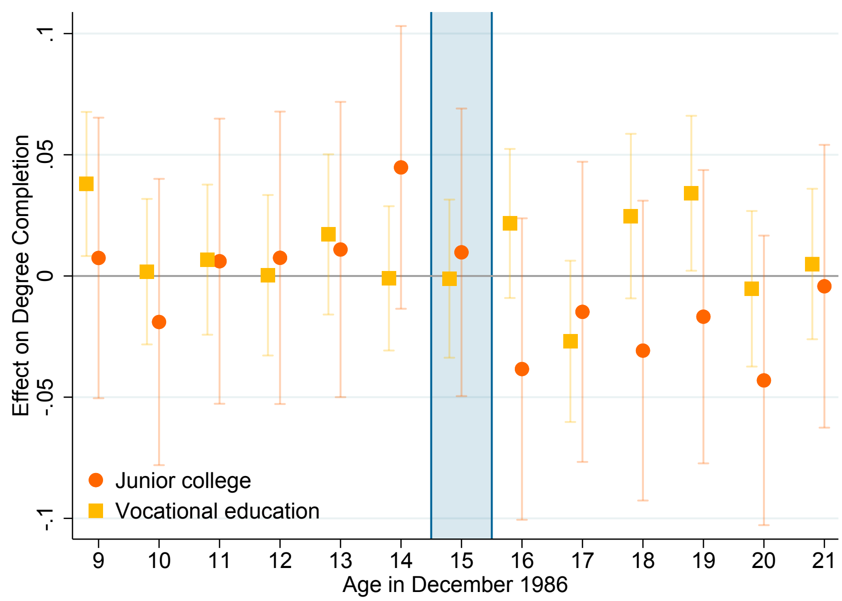


Figure A.10: Distribution of Births by Birth-Month Cohorts

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



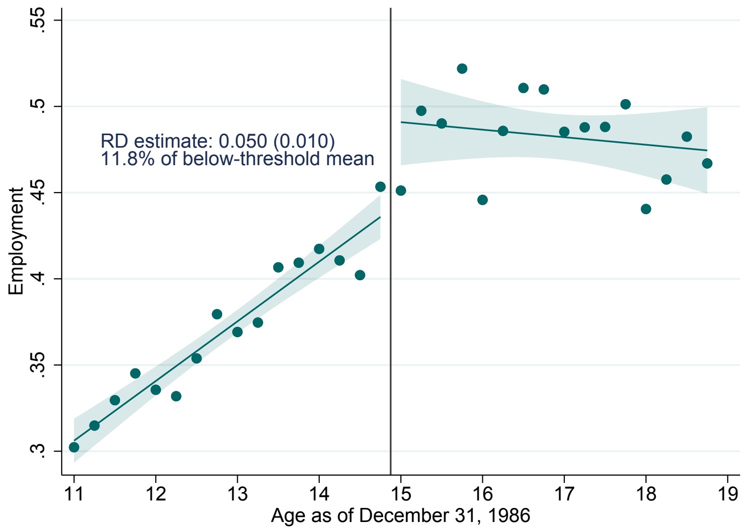
(a) Men



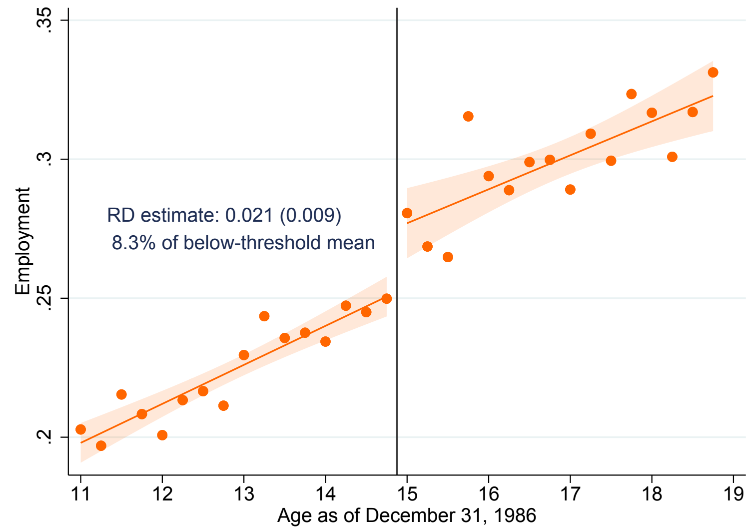
(b) Women

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

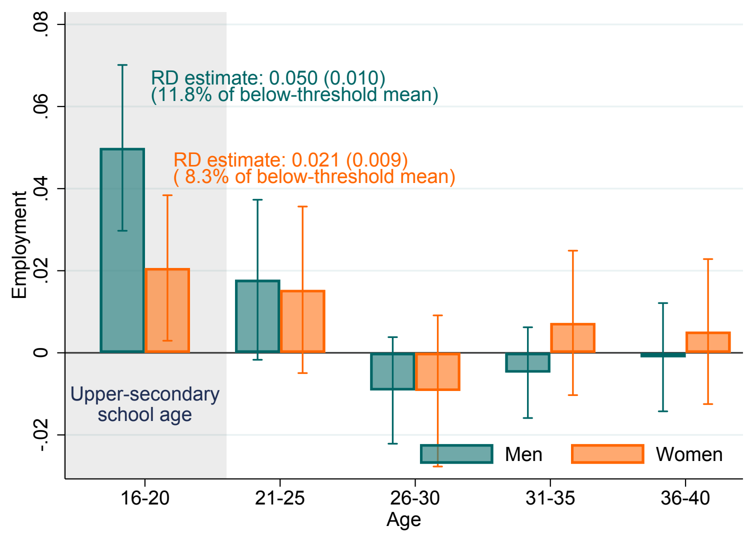
Notes: This figure plots tests of discontinuities in the educational attainment of men (panel a) and women (panel b) at the actual compulsory schooling age threshold in the tax-free year and at placebo thresholds. Educational attainment is measured as the completion of post-compulsory education, either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



(a) Men: Employment



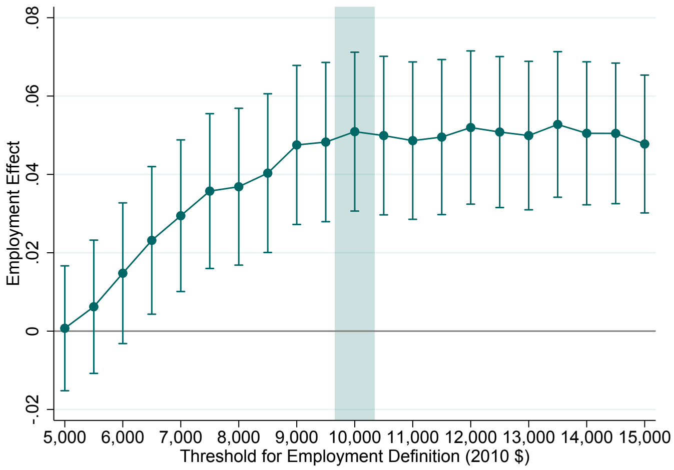
(b) Women: Employment



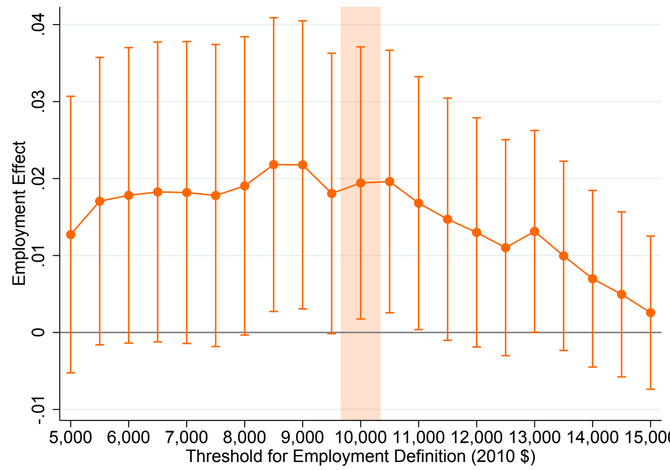
(c) Effect on Employment

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

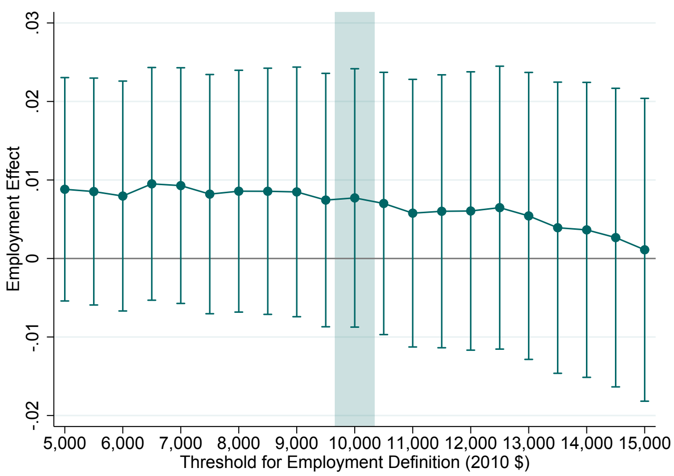
Notes: This figure studies the effect of the tax-free year on employment. Panels (a) and (b) plot the average employment at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Employment is defined as earning at least $10,000. Panel (c) plots RD estimates using equation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



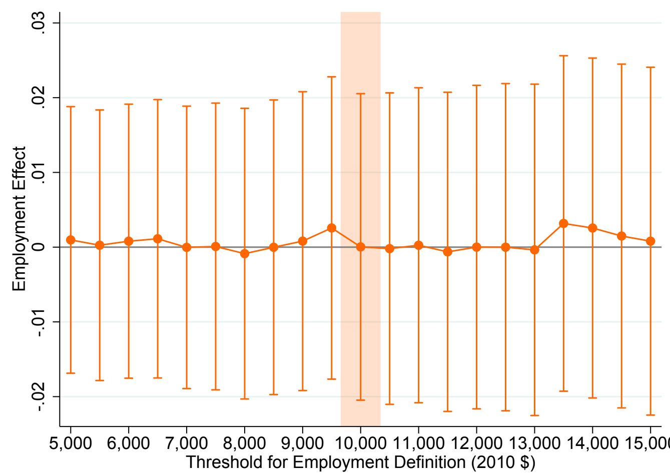
(a) Men: Effect on Employment at School Age



(b) Women: Effect on Employment at School Age



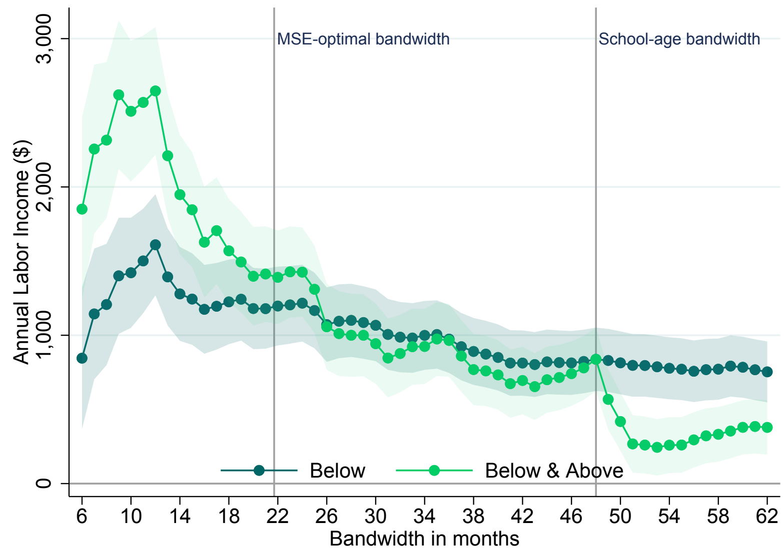
(c) Men: Effect on Employment at Prime Age



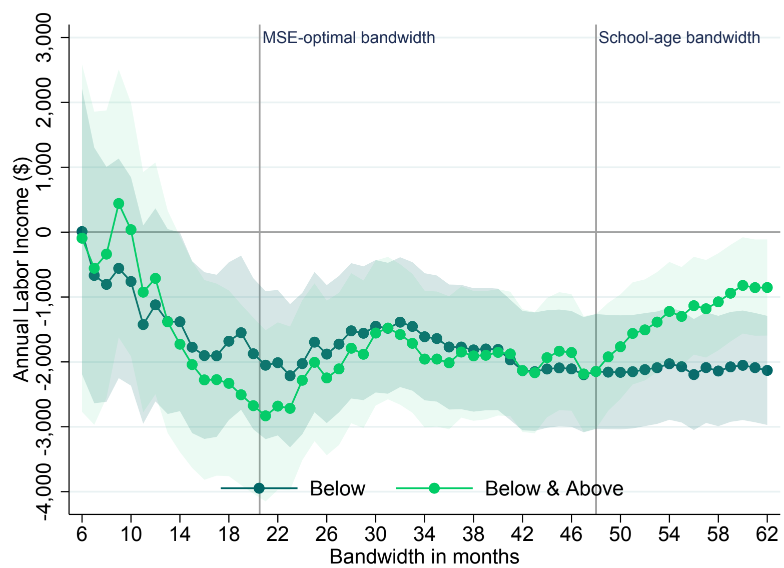
(d) Women: Effect on Employment at Prime Age

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

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



(a) Labor Income at Schooling Age



(b) Labor Income at Prime Age

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

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

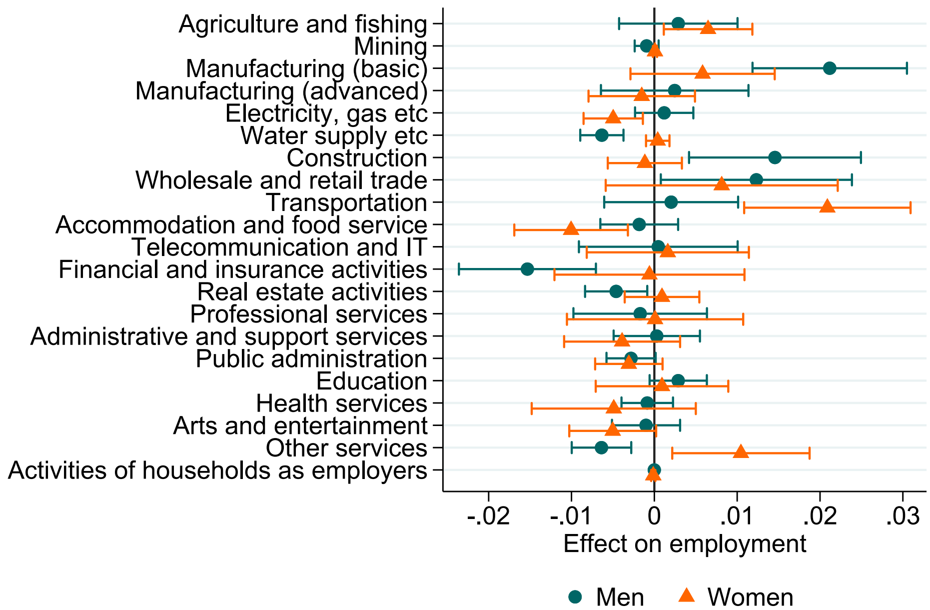


Figure A.15: Jobs at Prime Age

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

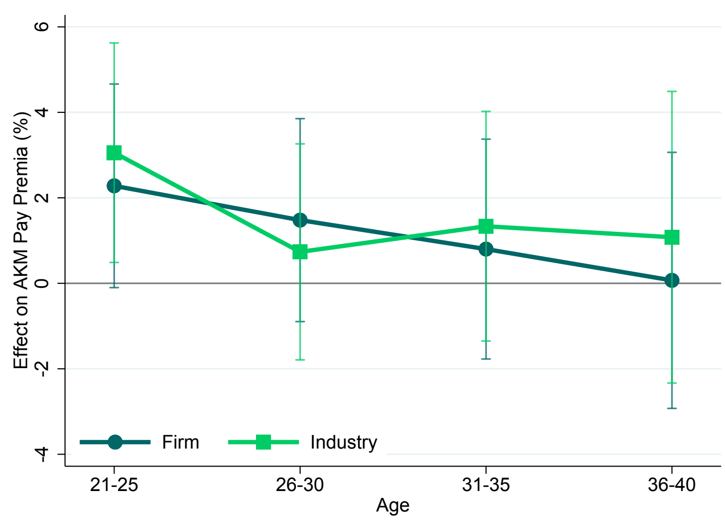
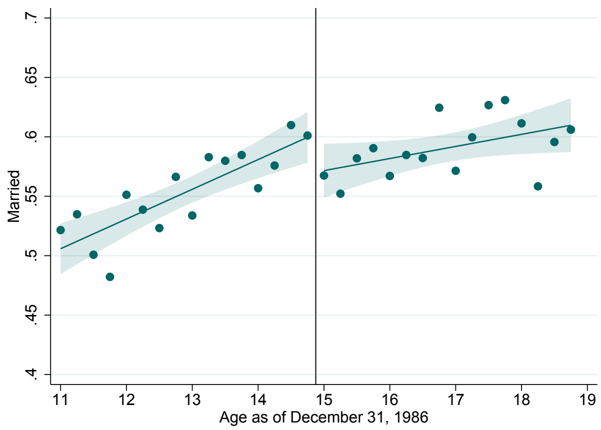
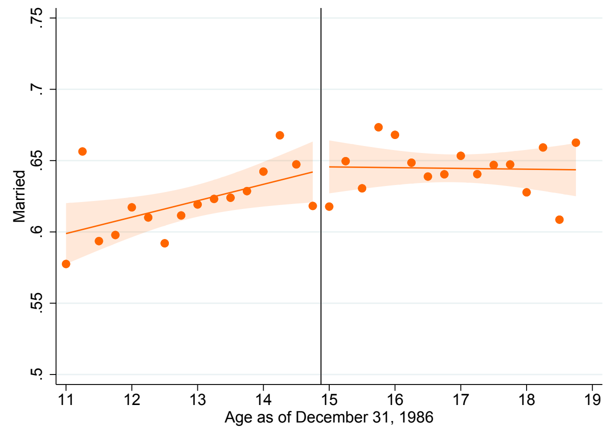


Figure A.16: Effect on Pay Premia in Firms and Industries

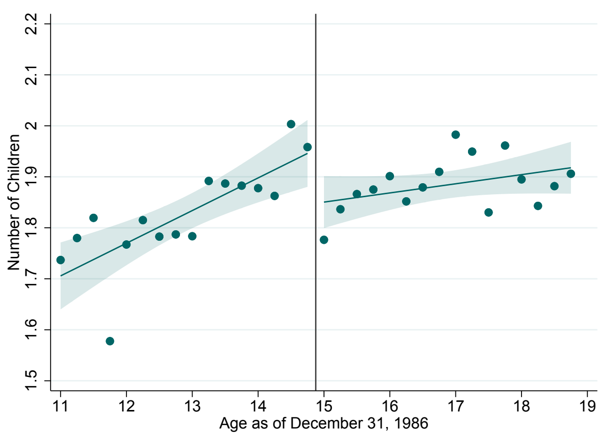
Notes: The figure plots the estimated treatment effects on the firm and industry pay premia at the worker's employer. Pay premia are measured by AKM firm or industry fixed effects estimated in the population of firms and industries in a regression on individual fixed effect, firm or industry fixed effects,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



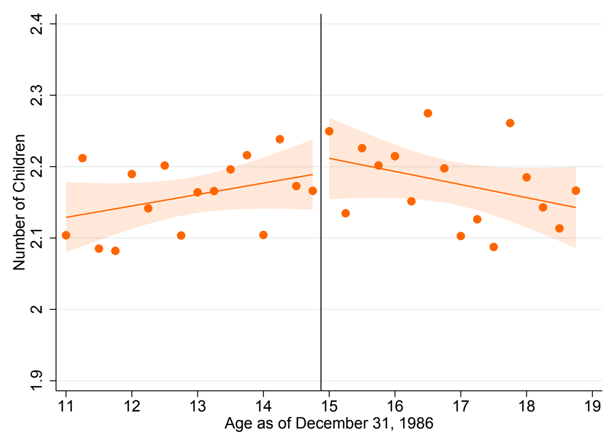
(a) Marriage — Men



(b) Marriage — Women



(c) Fertility — Men



(d) Fertility — Women

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

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

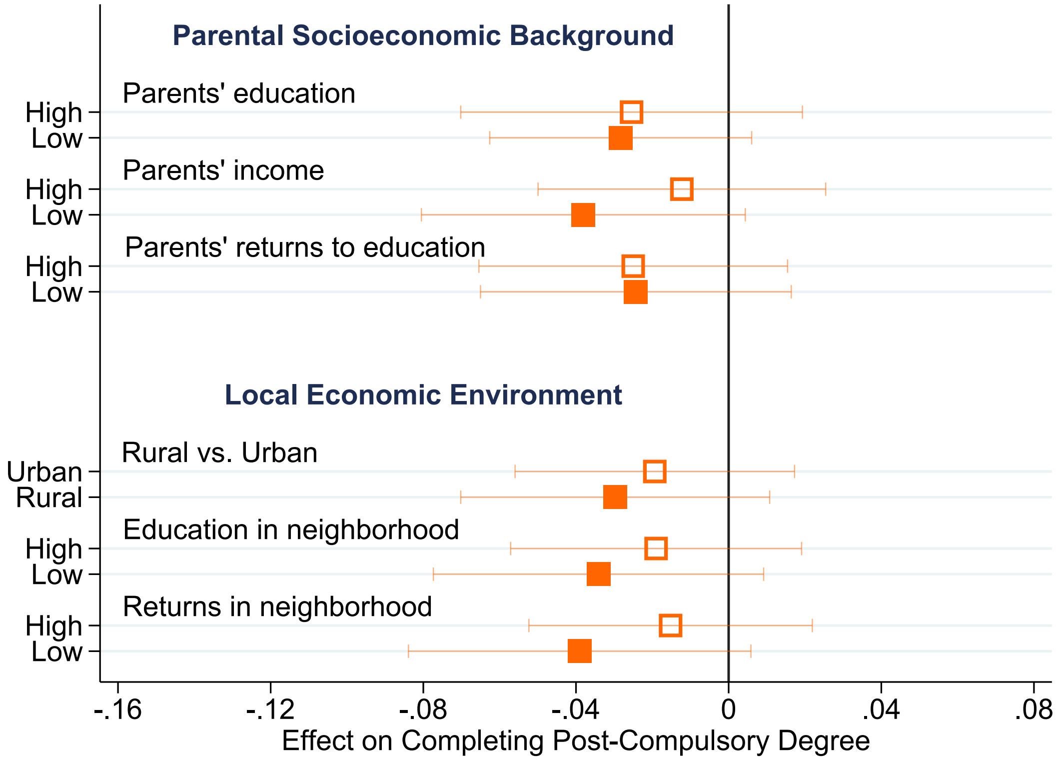
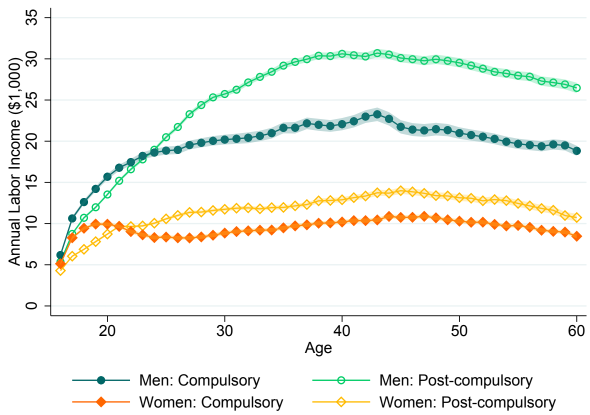
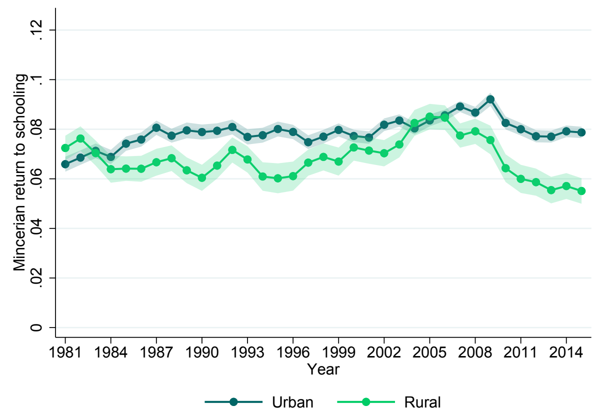


Figure A.18: School Dropout by Parental Background and Neighborhood — Women

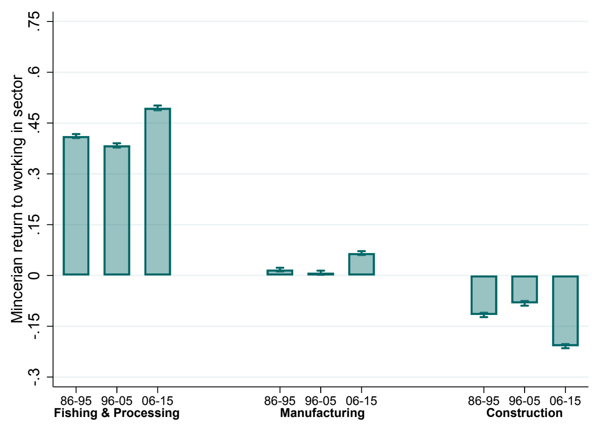
Notes: The figure shows the estimated effects of the tax-free year on school dropout among women, by parental background and neighborhood characteristics. For each characteristic, students are divided into two groups, and I estimate equation (1), interacting group indicators with the discontinuity and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



(a) Average earnings by education level



(b) Returns to schooling by region



(c) Sector premia

Figure A.19: Returns to Education and Sector Premia

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

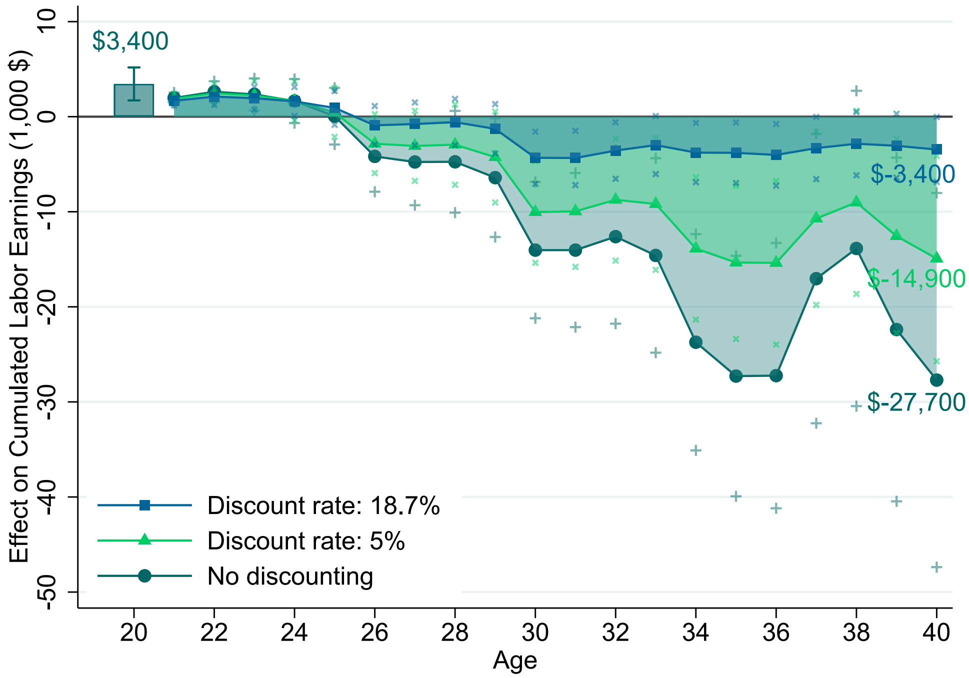


Figure A.20: Cumulative Labor Earnings and Implied Discount Rate

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

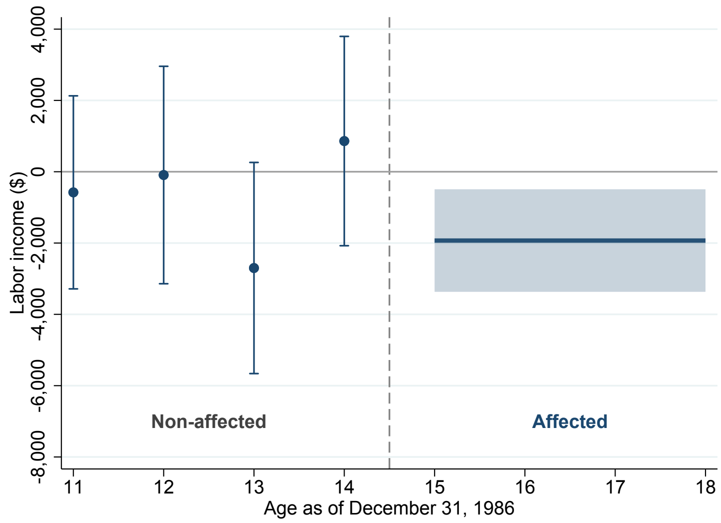


Figure A.21: Evaluation of Cohort Spillover Effects

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

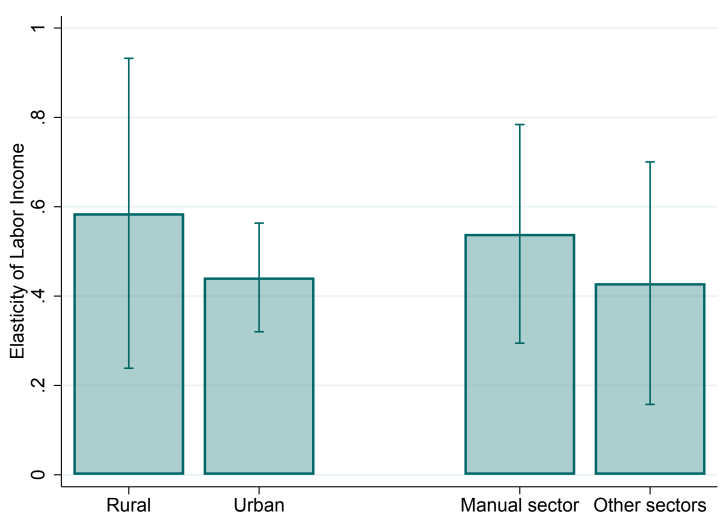


Figure A.22: Labor Supply Responses by Region and Sector

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

G Supplementary Tables

Table A.3: Effect on Educational Attainment — Robustness

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

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

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

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

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

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

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

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

Table A.6: Effect on Consumption Commitments

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

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

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