Online Appendix of:

Transitory Earnings Opportunities and Educational Scarring of Young Men

Jósef Sigurdsson

March 17, 2023

Contents

List of Figures

List of Tables

A Model Appendix

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

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

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

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

From the properties of θ and κ we have that:

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

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

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

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

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

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

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

As the RHS is bounded by κ^0 from below, which is assumed positive, this condition also includes (1). Therefore, under condition (2), then:

$$\mathbb{D} \equiv \frac{\partial}{\partial(1-\tau)}\left[\frac{(1-\delta)((1-\tau)+\kappa^0)}{\delta\rho-(1-\delta)b}\right] = \frac{1-\delta}{\delta\rho-(1-\delta)b} > 0 \tag{3}$$

□

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

Assumption 1.

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

Proof of Proposition 1

Proof. From the proof of Lemma 1 we have that

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

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

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

is increasing in b. Solving and taking expectations gives

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

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

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

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

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

which is increasing in b. □

Proposition 2 by example

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

Consider a benchmark dropout propensity $\mathbb{D}$ under correlation b and a higher dropout propensity $\mathbb{D}'$ under correlation b'. This is the example illustrated in Figure 2 in the main text. From (3) we have:

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

where $\Delta b = b' - b$ is the increase in correlation compared to the benchmark. I can then solve for ε

that leads to the same increase in dropout propensity but at the benchmark correlation b:

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

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

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

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

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

which implies that for a hyperbolic discount factor of

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

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

B Data on Educational Attainment

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

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

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

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

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

C Effects on Marriage and Fertility as an Income Effect

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

To evaluate the latter alternative, Table A.2 summarizes estimates from prior studies of the effect of windfall income on marriage and fertility. Cesarini, Lindqvist, Östling, and Terskaya (2021) study the impact of winning a lottery on marital and fertility outcomes among Swedish male and female lottery players. Evaluating outcomes several years after winning a lottery, they find that winning $100,000 increases men's propensity to marry by 30 percent. Exploiting similar variation, Golosovy, Graberz, Mogstad, and Novgorodsky (2021) estimate a smaller but significant average increase in marriage rates among US lottery winners of both genders.[1] Indeed, Cesarini, Lindqvist, Östling, and Terskaya (2021) find that lottery wealth does not significantly alter women's propen-

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

Table A.2: Comparison of Effects on Marriage and Fertility to Prior Studies

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

Notes: The table reports the effects of $100,000 increase/decrease in income or wealth on the outcome specified in the row heading. The table presents estimates for men unless otherwise indicated. Estimates for the impact of school dropout are the coefficient estimates for marriage and fertility reported in Figure 10 but scaled by the estimated effect on lifetime earnings (Figure 11) such that -1% means that a $100,000 reduction in earnings reduces the outcome by 1% compared to the below-threshold average. Cesarini, Lindqvist, Östling, and Terskaya (2021), Golosovy, Graberz, Mogstad, and Novgorodsky (2021), and Hankins and Hoekstra (2011) report responses to windfall income from lottery winnings, and estimates in Lovenheim and Mumford (2013) and Chu, Lin, and Tsay (2020) responses to housing wealth increase.
* The estimates are for both men and women.
** The estimates are in measured as response to a 10% increase in wealth.

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

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

D Supplementary Figures

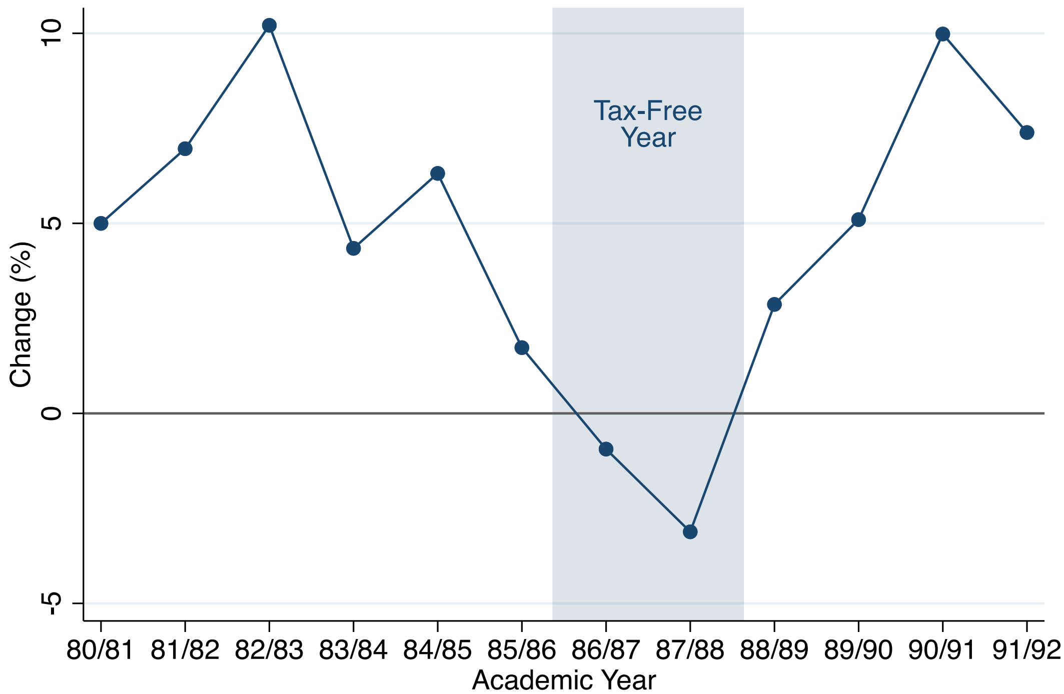
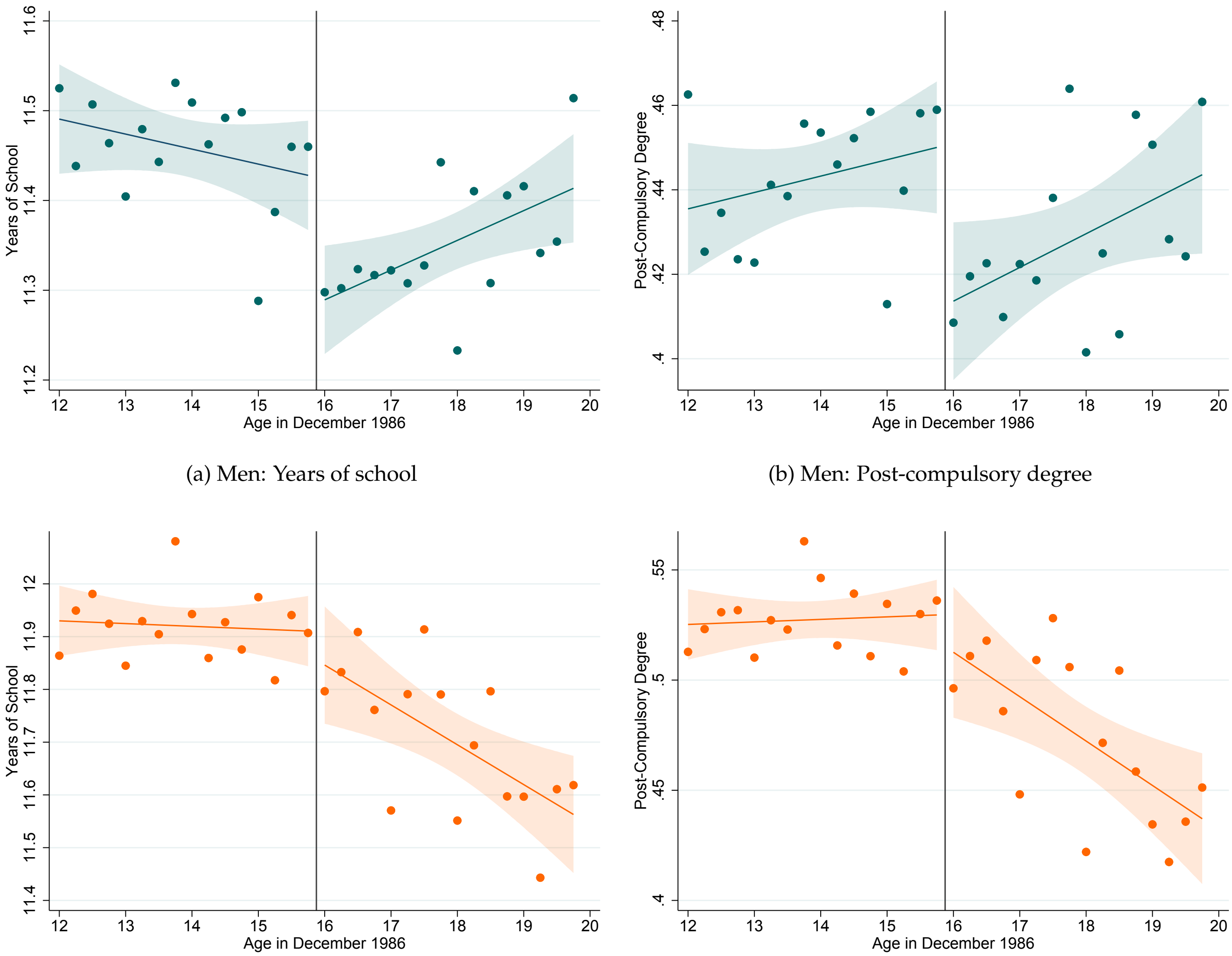


Figure A.1: Change in University Enrollment

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



(a) Men: Years of school

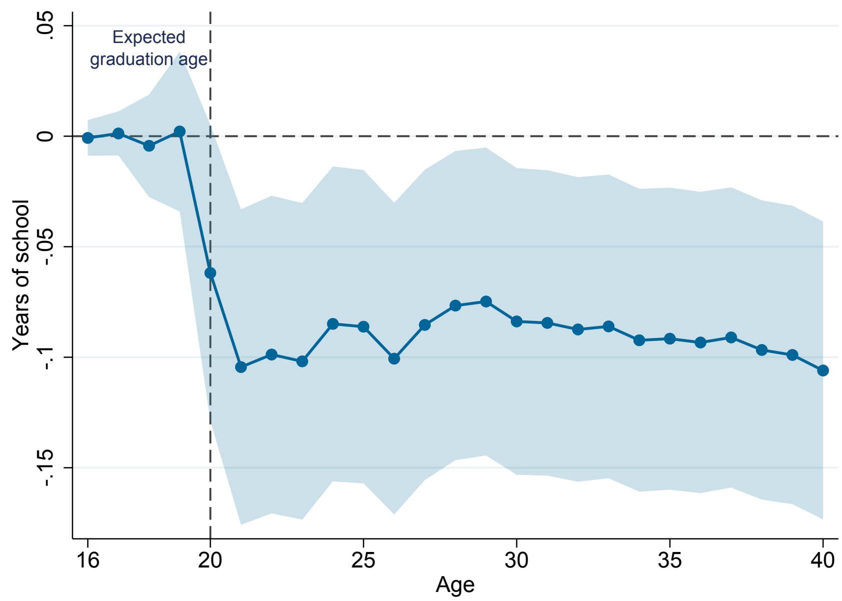
(b) Men: Post-compulsory degree

(c) Women: Years of school

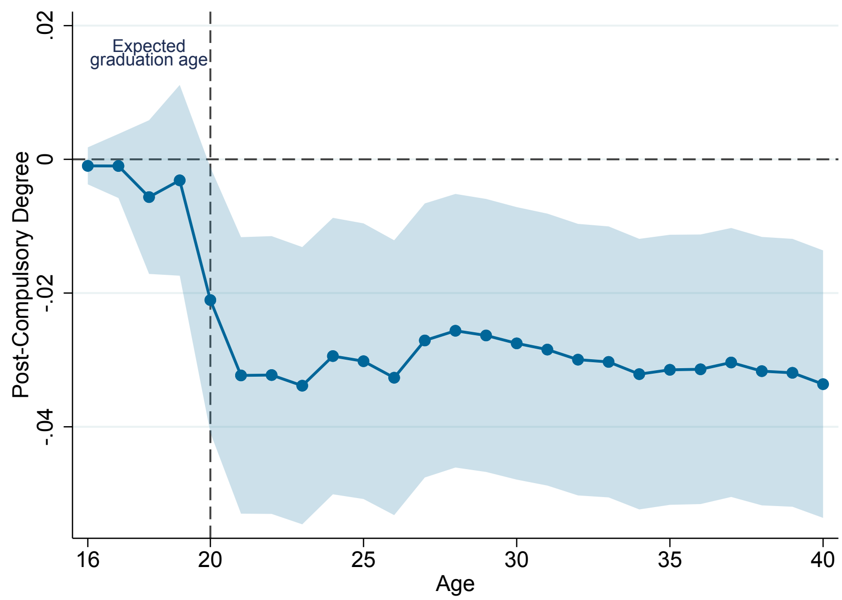
(d) Women: Post-compulsory degree

Figure A.2: Educational Attainment — Men and Women

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



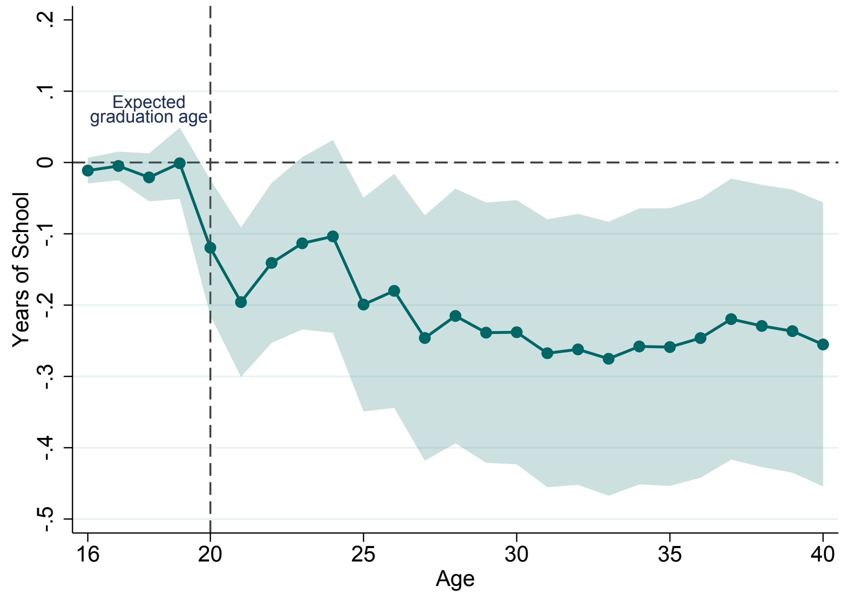
(a) Years of schooling



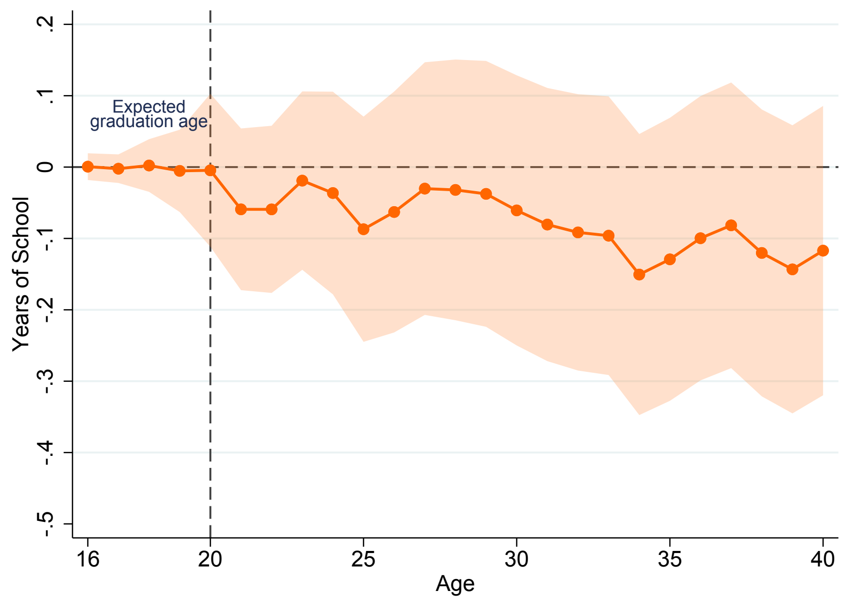
(b) Post-compulsory degree

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

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



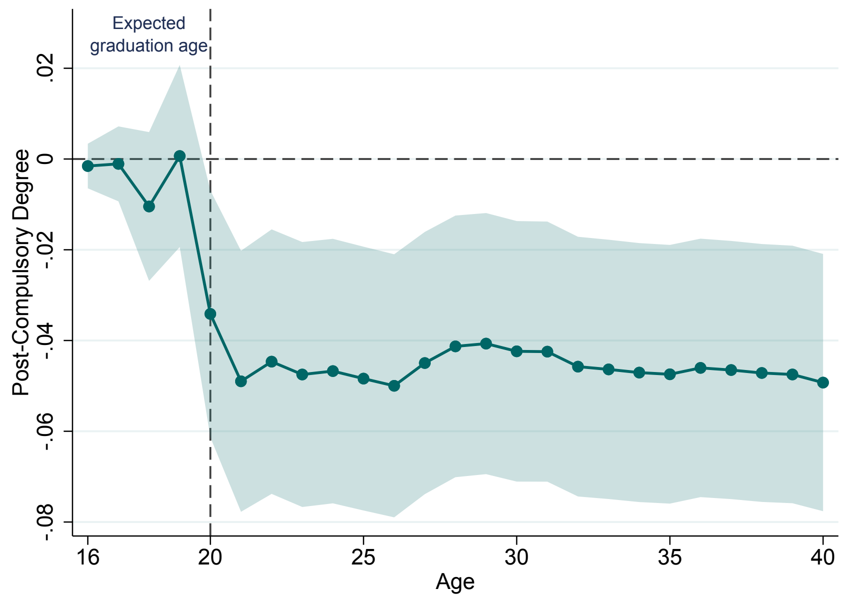
(a) Men: Years of school



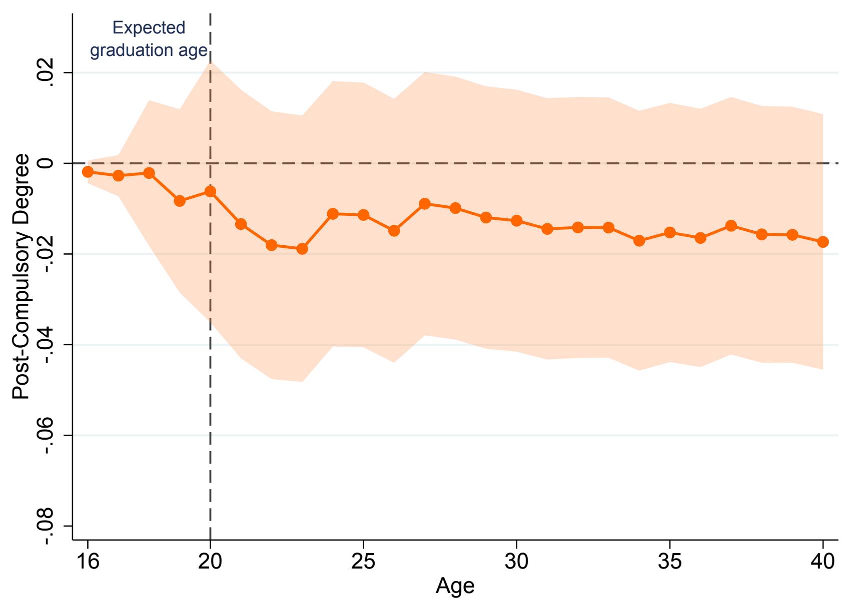
(b) Women: Years of school

Figure A.4: Years of Schooling

Notes: This figure is a plot of estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panel (a) plots estimated effects on years of schooling, including university education, for men, and Panel (b) does the same for women. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The shaded areas show 95% confidence intervals.



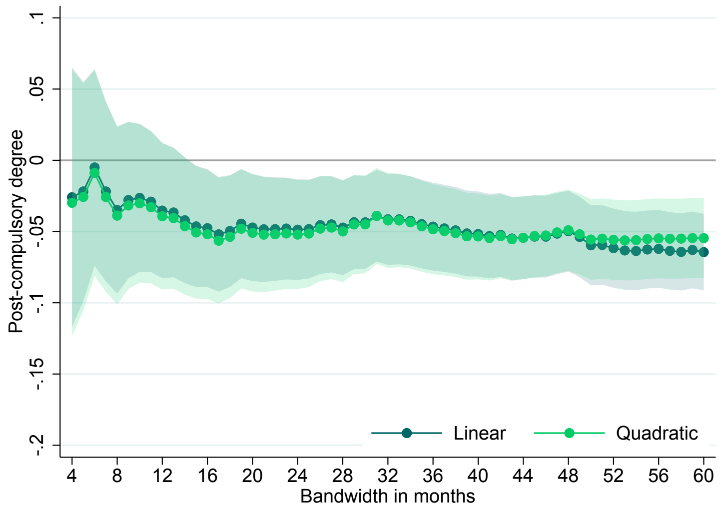
(a) Men: Post-Compulsory Degree



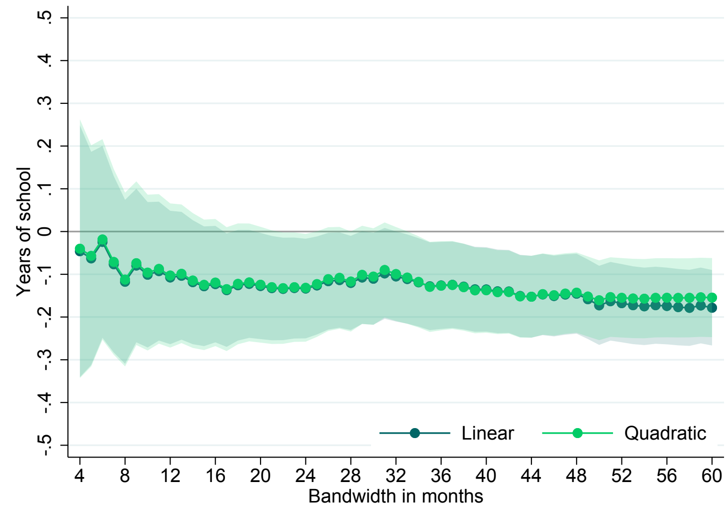
(b) Women: Post-Compulsory Degree

Figure A.5: Post-Compulsory Education

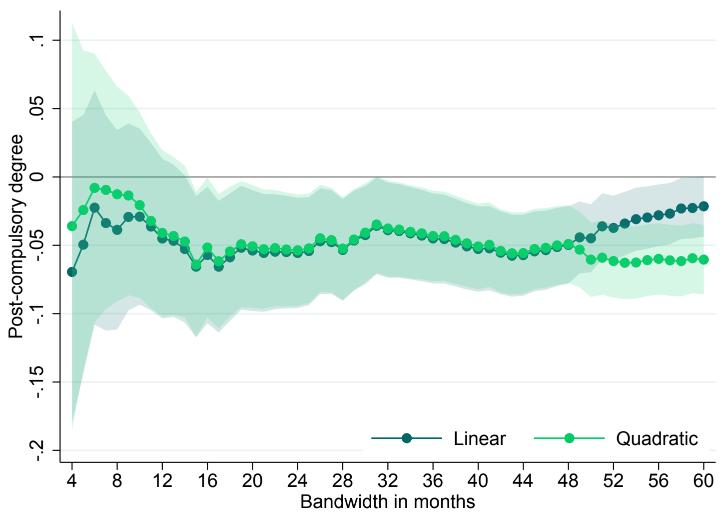
Notes: This figure plots estimates using an RD-based event study design, where each coefficient corresponds to an RD estimate at a given age of 16-40. Vertical lines mark the expected—or normal—graduation age from upper secondary school, which is 20. Panel (a) plots estimated effects on completion of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



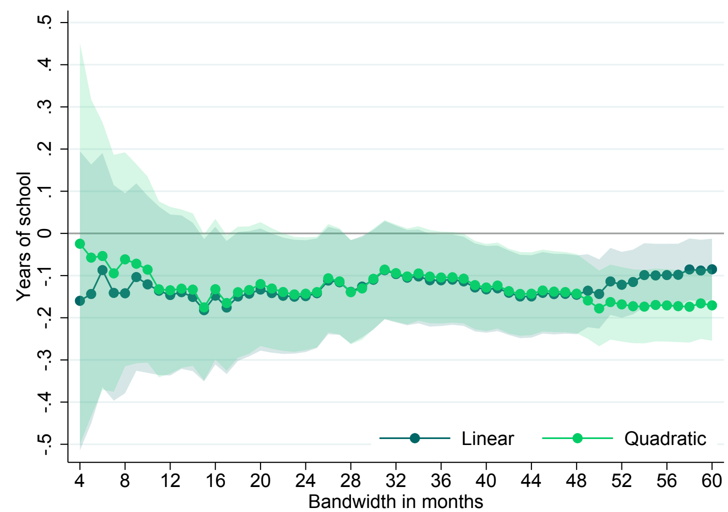
(a) Post-Compulsory Degree — Varying Bandwidth Left of Threshold



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



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



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

Figure A.6: Effect on Educational Attainment: Sensitivity to the Choice of Bandwidth

Notes: This figure plots effects on the educational attainment of men, measured with an indicator for completing a post-compulsory degree and years of school at age 40, using equation (1) for different bandwidths and polynomial order. Each dot is a separate regression estimate and all regressions control for pre-reform municipal fixed effects, number of children, and marital status. Panels (a) and (b) vary the bandwidth to the left of the threshold (i.e. the control group) while maintaining a 48-month bandwidth to the right (i.e. the treatment group). This way it includes everyone at normal upper-secondary schooling age, 16-20. Panels (c) and (d) vary the bandwidth at both sides of the threshold. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits. The shaded areas show 95% confidence intervals.

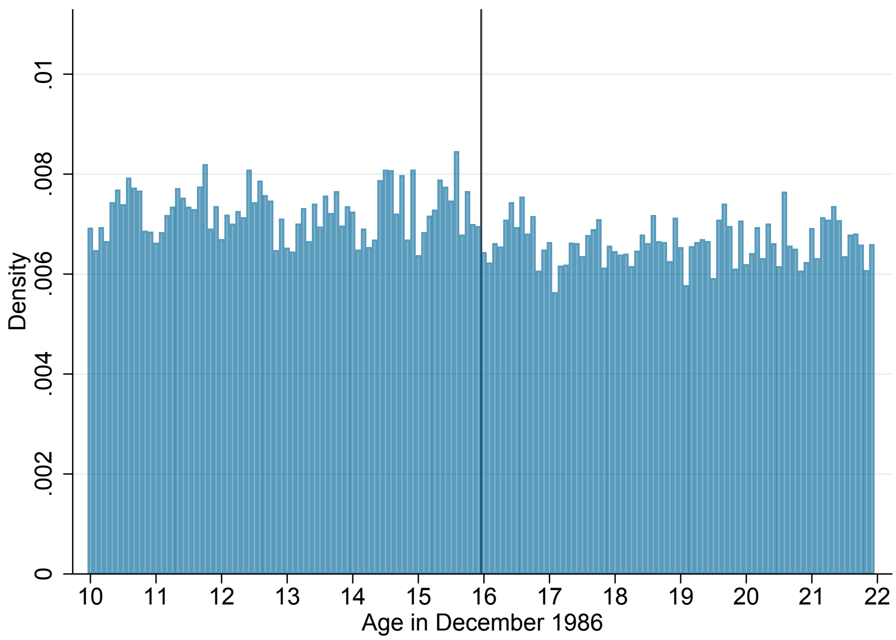
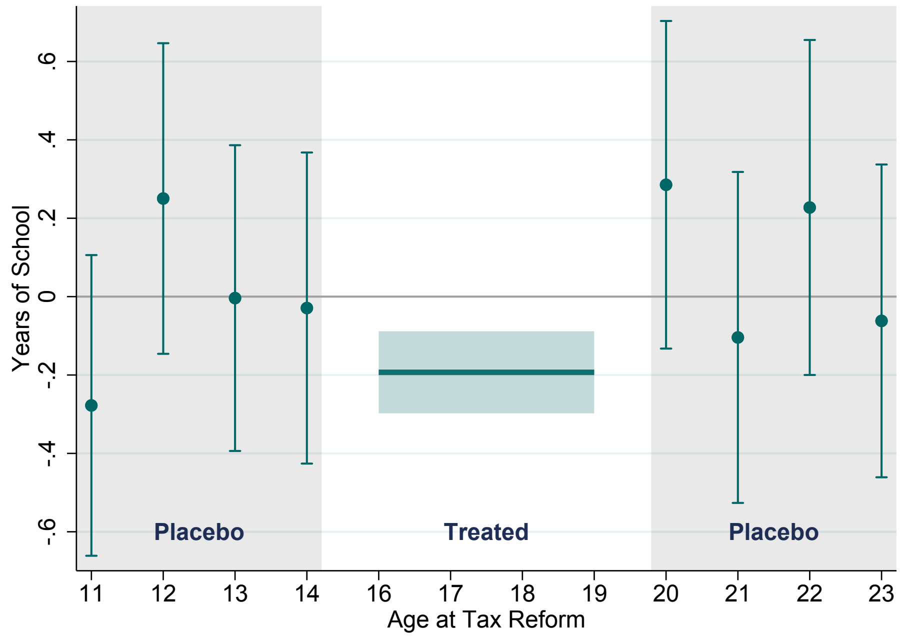
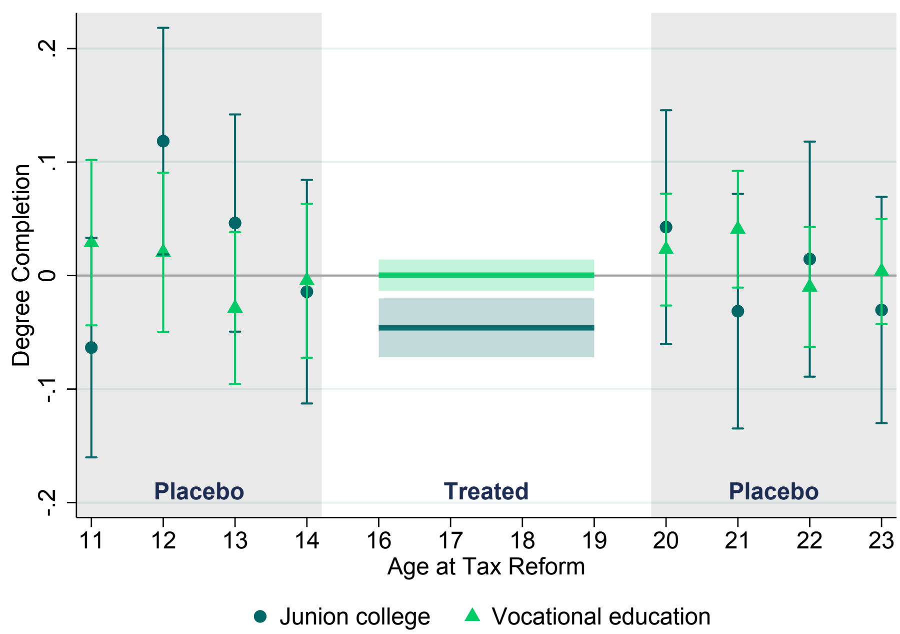


Figure A.7: Distribution of Births by Birth-Month Cohorts

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



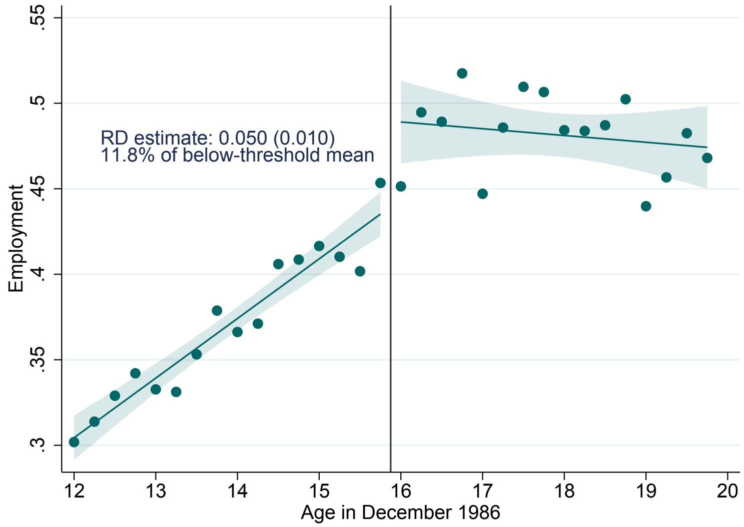
(a) Years of school



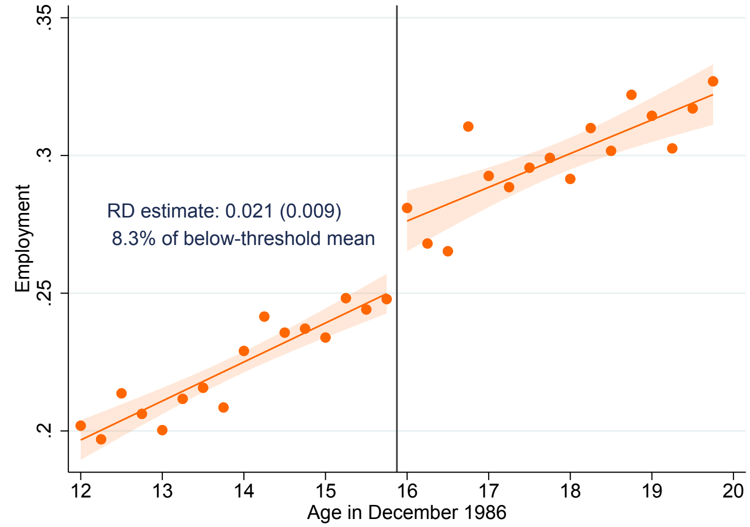
(b) Upper-secondary degrees

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

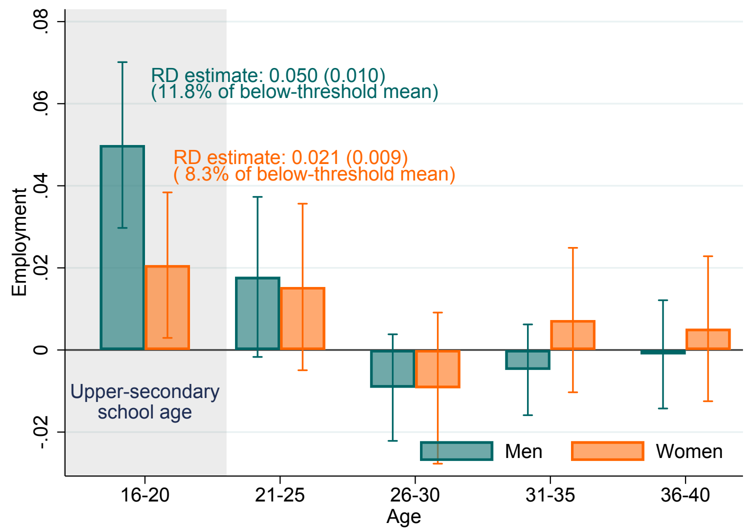
Notes: This figure plots the estimated effects of the actual and hypothetical placebo tax-free years on years of schooling (panel a) and upper-secondary degree completion (panel b) of men. Educational attainment is measured at age 40. Placebo tests are estimates of equation (1) at the actual compulsory school threshold but at placebo tax-free years. For example, the coefficient at age 14 tests for discontinuities in the hypothetical tax-free year of 1989 but around the relevant age threshold (turning 16 by December 31, 1988). The students just to the right of the school-age threshold in 1989 were 14 years old in 1987, which is the age used to label the x-axis. For reference, the figure also plots the estimated treatment effect for those at upper-secondary schooling age during the actual tax-free year of 1987, i.e. 16-20-year-olds. Regressions control for pre-reform characteristics at age 16 including the region of residence, an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and an indicator for receiving disability benefits.



(a) Men: Employment



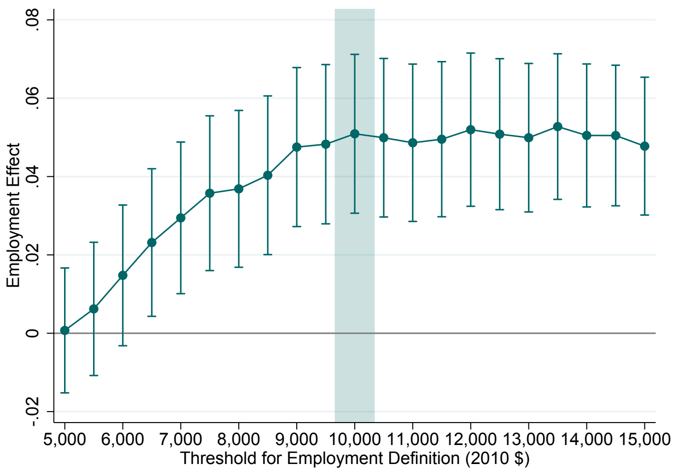
(b) Women: Employment



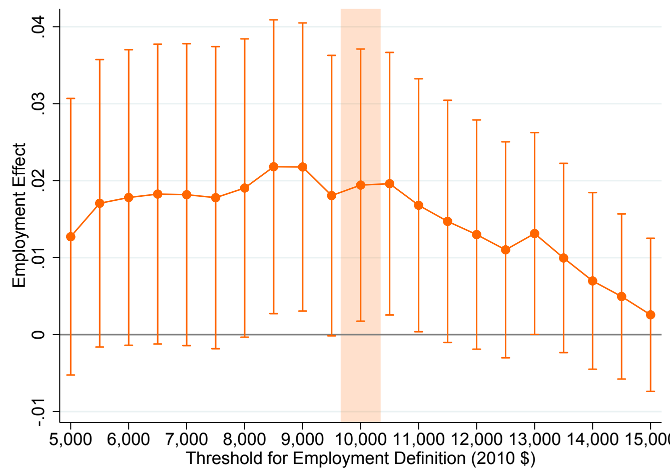
(c) Effect on Employment

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

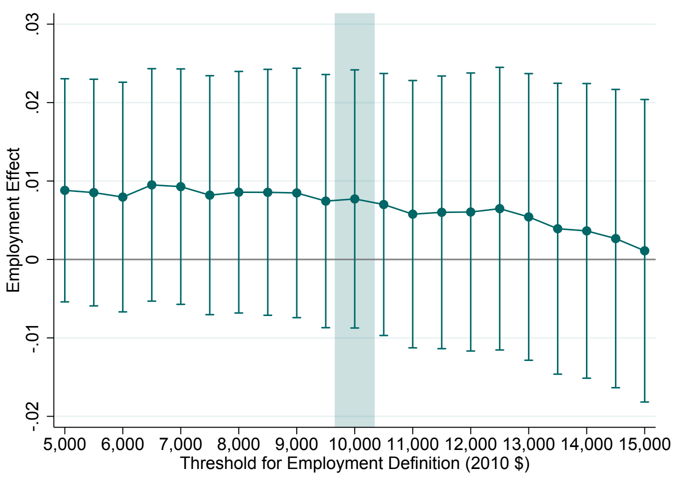
Notes: This figure studies the effect of the tax-free year on employment. Panels (a) and (b) plot the average employment at ages 16-20 around the compulsory schooling age threshold for men and women, respectively. Employment is defined as earning at least $10,000. Panel (c) plots RD estimates using equation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



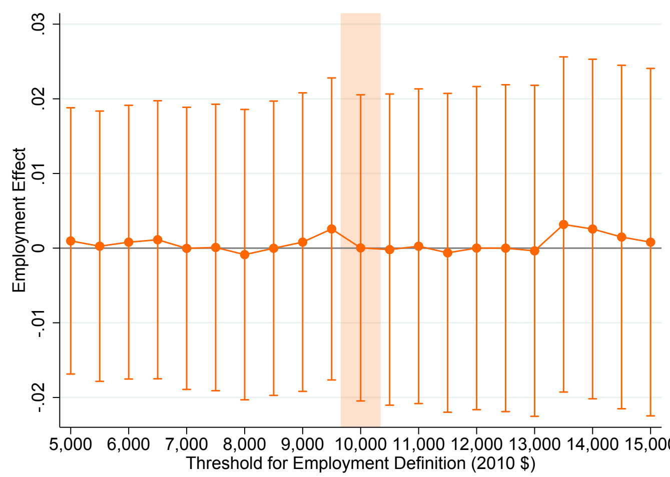
(a) Men: Effect on Employment at School Age



(b) Women: Effect on Employment at School Age



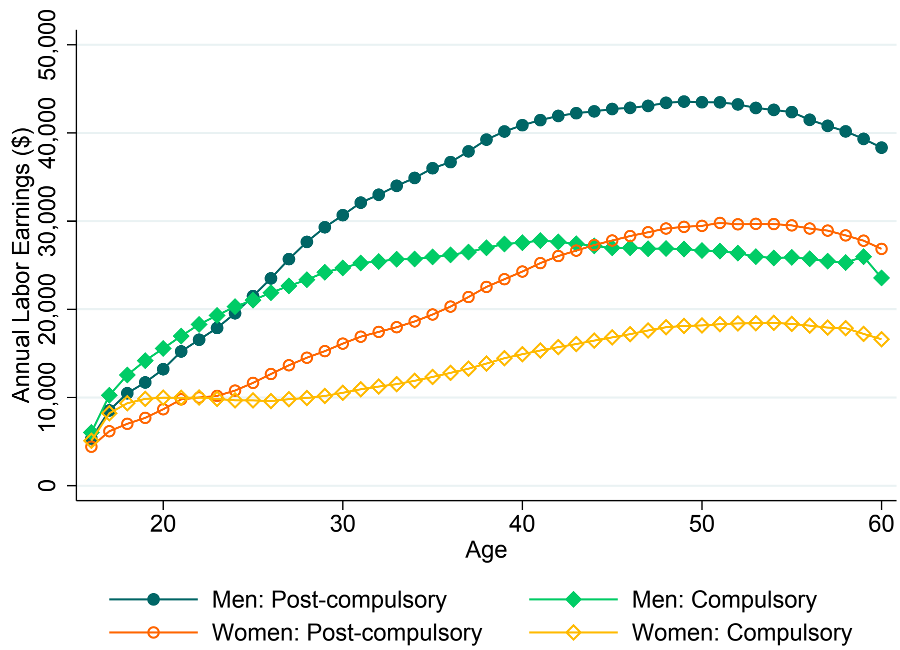
(c) Men: Effect on Employment at Prime Age



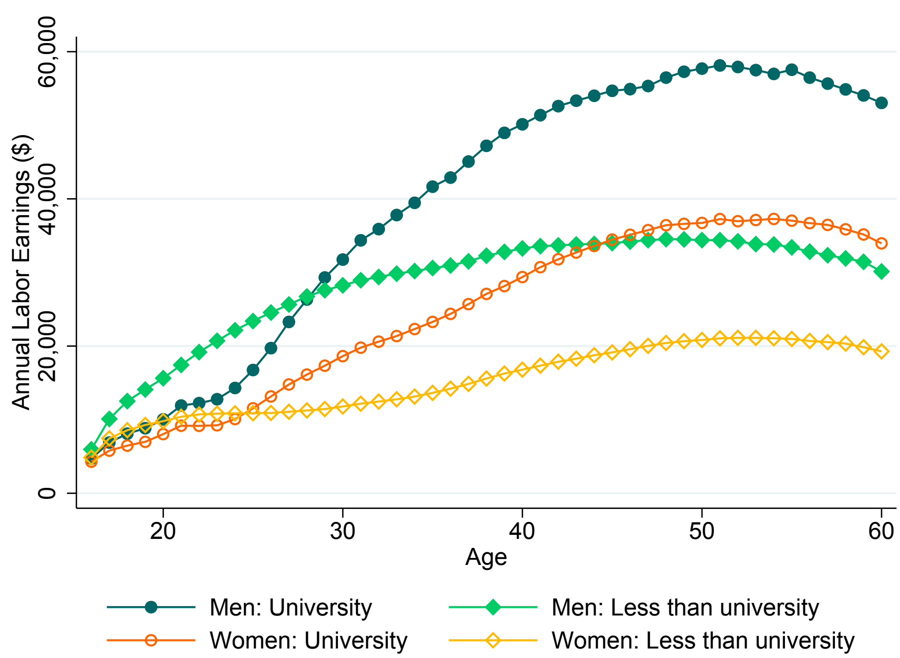
(d) Women: Effect on Employment at Prime Age

Figure A.10: Robustness to Varying the Earnings Threshold to Define Employment

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



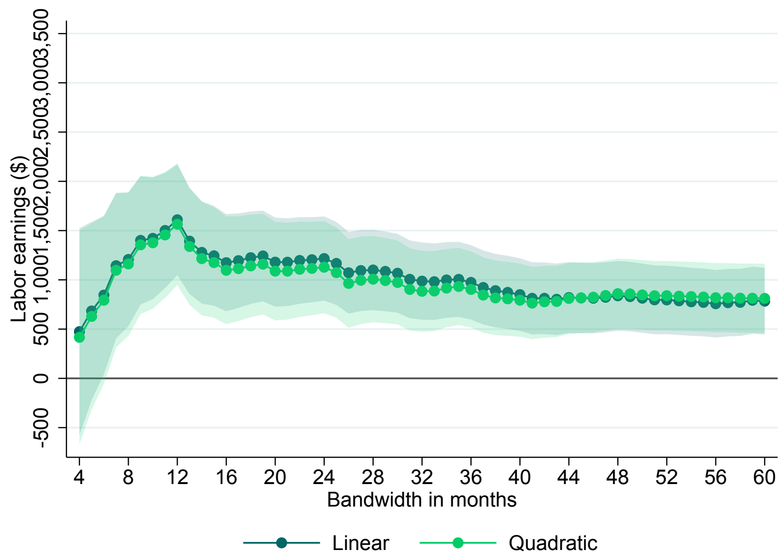
(a) Post-Compulsory Degree



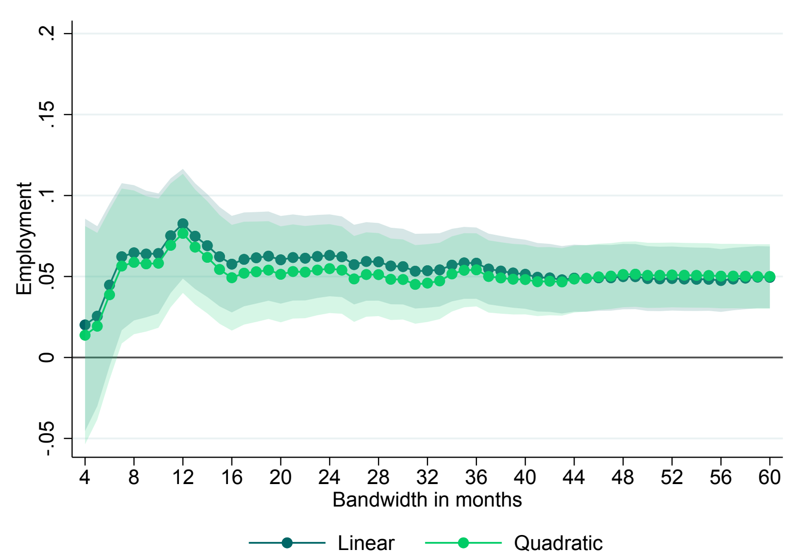
(b) University Degree

Figure A.11: Lifecycle Earnings Profiles by Education

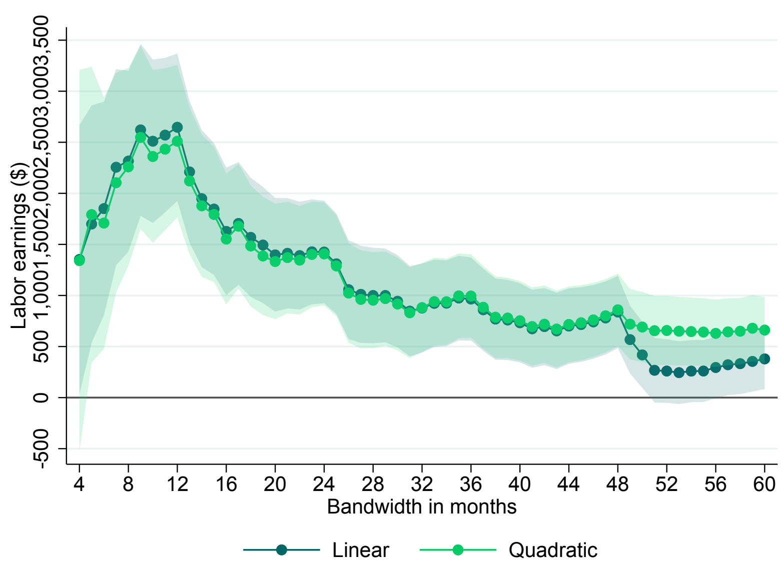
Notes: The figure plots earnings profiles by education for men and women in the cohorts born between 1947 and 1977. Panel (a) plots average earnings separately for those who complete a post-compulsory degree compared to those who only complete compulsory education. Panel (b) plots average earnings separately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



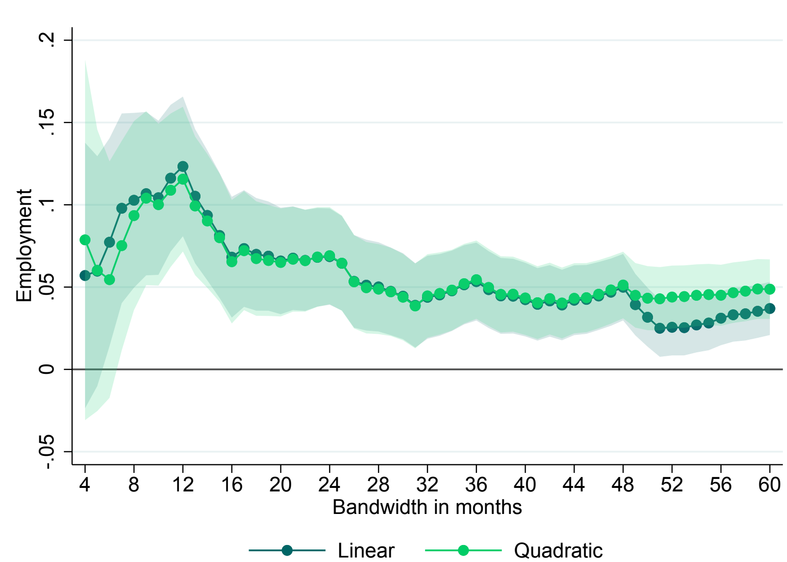
(a) Labor Earnings — Varying Bandwidth Left of Threshold



(b) Employment — Varying Bandwidth Left of Threshold



(c) Labor Earnings — Varying Bandwidth on Both Sides



(d) Employment— Varying Bandwidth on Both Sides

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

Notes: This figure plots effects on labor market outcomes at ages 16-20, measured with labor earnings in 2010 $ and an indicator for being employed, using equation (1) for different bandwidths and polynomial order. Panels (a) and (b) vary the bandwidth to the left of the threshold (i.e. the control group) while maintaining 48-month bandwidth to the right (i.e. the treatment group). This way it includes everyone at normal upper-secondary schooling age, 16-20. Panels (c) and (d) vary the bandwidth at both sides of the threshold. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The shaded areas show 95% confidence intervals.

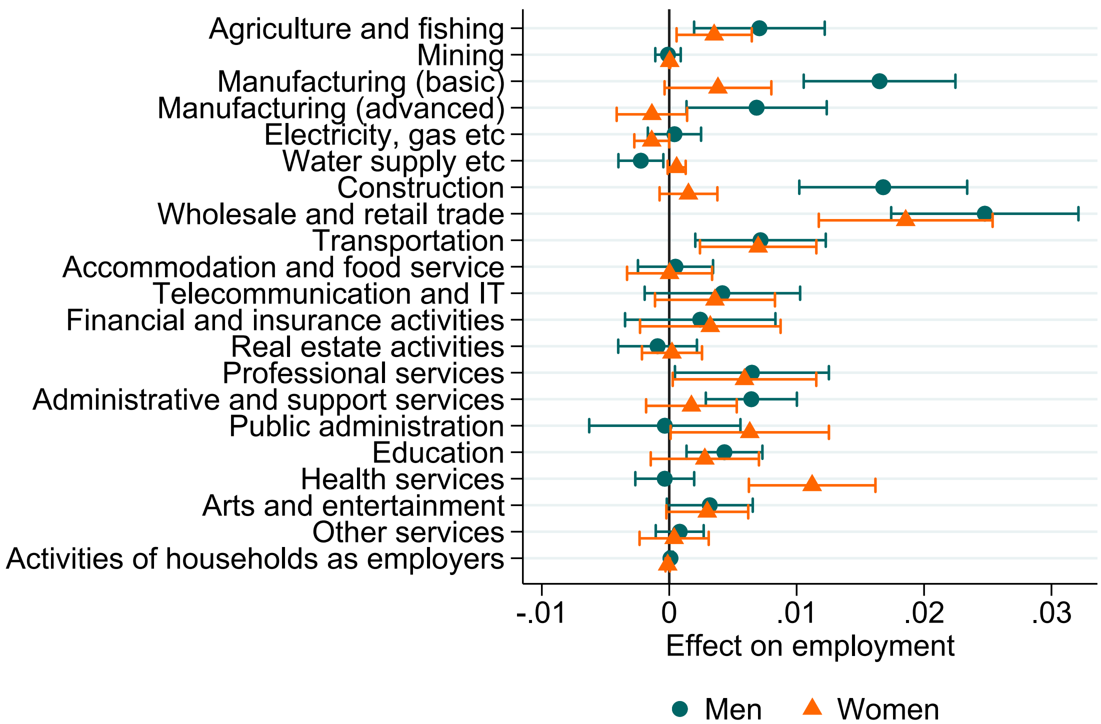


Figure A.13: Jobs at Prime Age

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

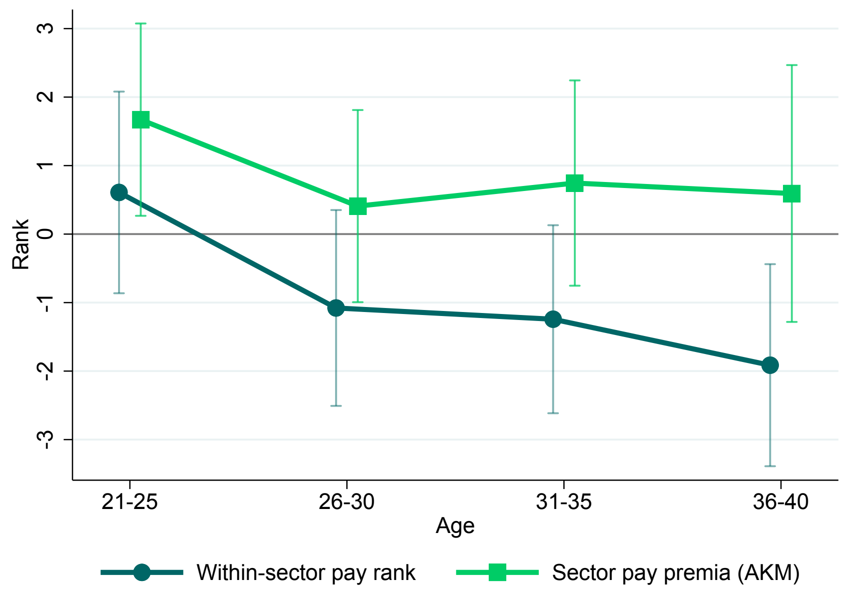


Figure A.14: Sector Pay Premia and Career Progression within Sectors

Notes: The figure plots the estimated treatment effects on the pay premia at the worker's sector of employment and the worker's relative position within the sector of employment. Pay premia is measured by AKM sector fixed effects. Career progression is measured by the rank of a worker's residualized ('Mincerized') earnings within the sector. That is, earnings are regressed on gender, age, and interaction of age and gender, and ranks are based on residuals from this regression. The dots/squares correspond to estimates of equation (1) on earnings rank. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. The whiskers display the 95% confidence interval based on robust standard errors clustered at the individual level.

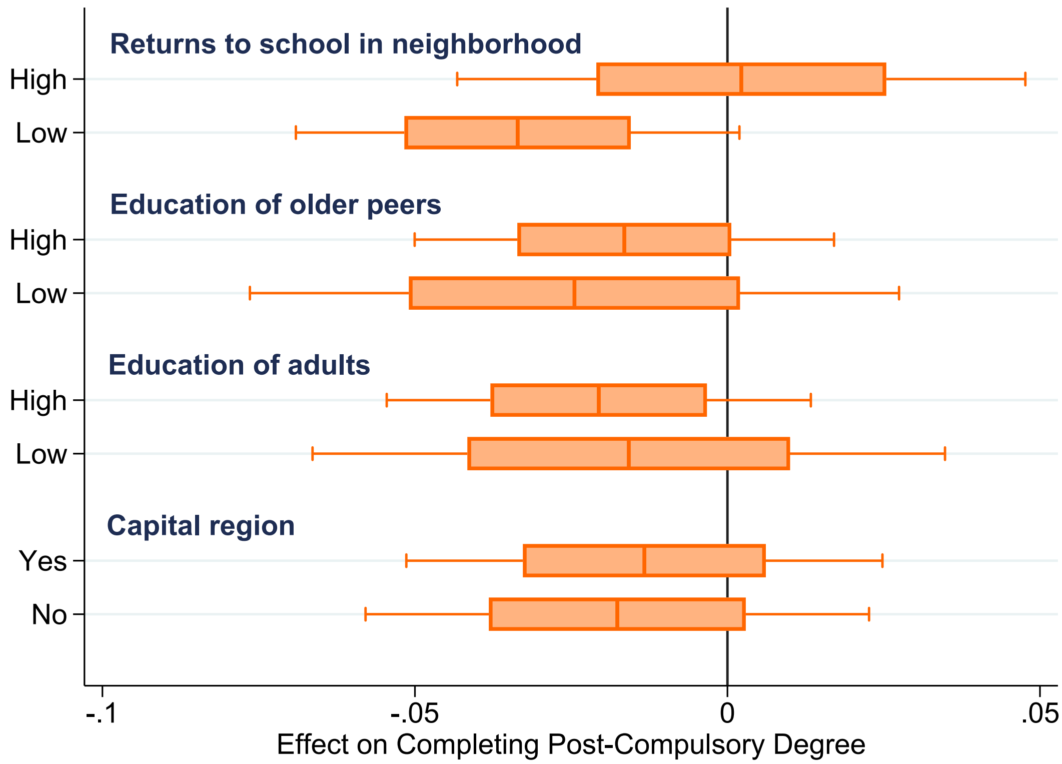


Figure A.15: School Dropout by Background — Women

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

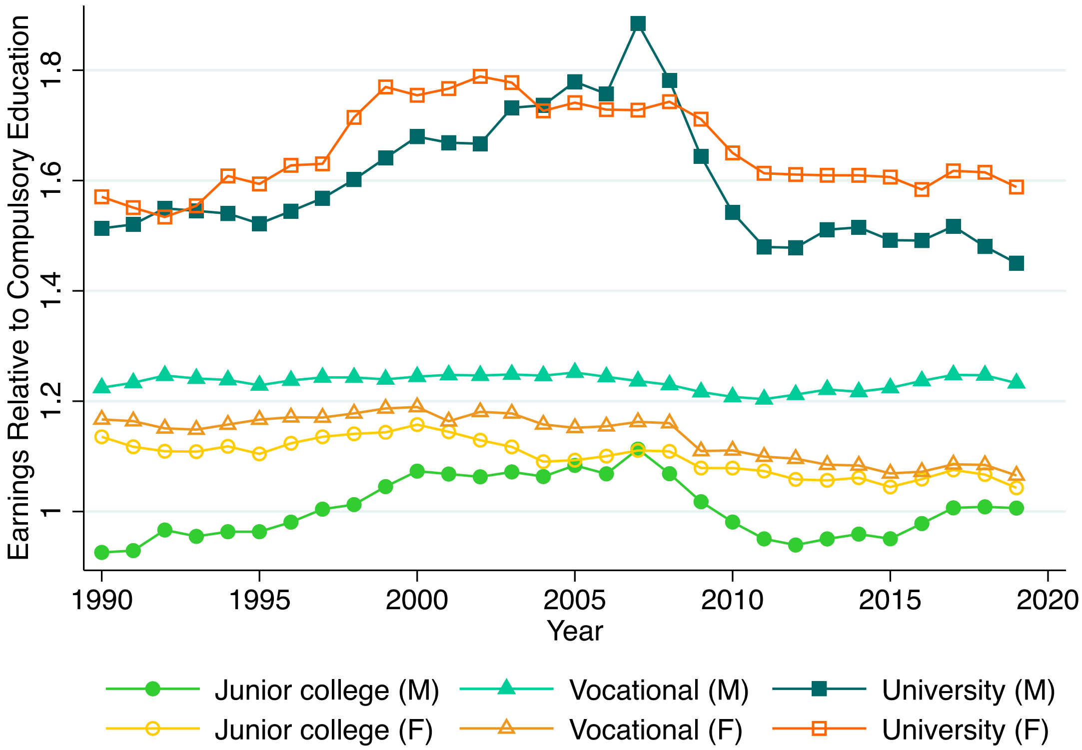


Figure A.16: Returns to Education

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

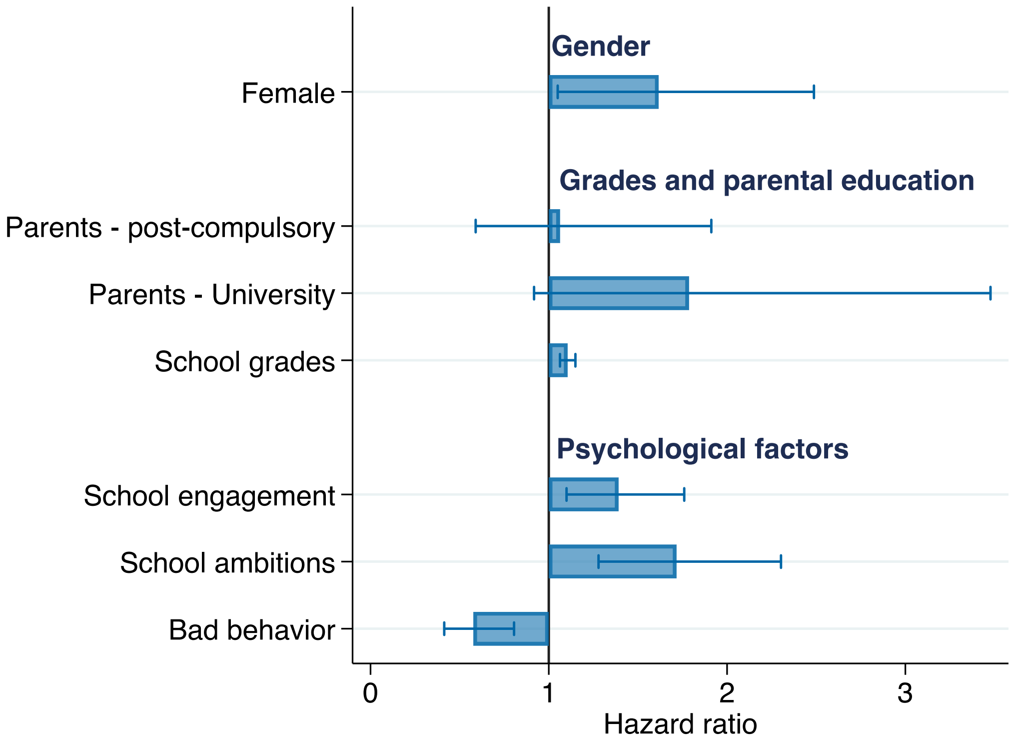


Figure A.17: Factors Influencing School Completion

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

E Supplementary Tables

Table A.3: Effect on Educational Attainment — Robustness

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

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

Table A.4: Effect on Labor Market Outcomes

	Labor Earnings ($)				Employment			
	16-20		36-40		16-20		36-40	
	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)
A. Men								
	886***	838***	-1,597*	-2,147**	0.053***	0.050***	0.007	-0.001
	(191)	(180)	(918)	(871)	(0.011)	(0.010)	(0.008)	(0.007)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	10,487	10,487	41,927	41,927	0.425	0.425	0.863	0.863
Observations	78,247	78,247	76,269	76,269	78,247	78,247	76,269	76,269
B. Women								
	85	96	-446	-262	0.019**	0.021**	0.000	0.005
	(102)	(99)	(574)	(531)	(0.009)	(0.009)	(0.010)	(0.009)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	7,342	7,342	26,247	26,247	0.248	0.248	0.796	0.796
Observations	74,884	74,884	74,274	74,274	74,884	74,884	74,274	74,274

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

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

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

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

Table A.6: Men-Women Differences: Labor Market Outcomes

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

Notes: This table reports difference-in-discontinuity estimates, which are obtained using a version of regression equation (1) that is fully interacted with a male indicator. The table reports the coefficient on the treatment indicator interacted with the male indicator, reflecting the difference in the discontinuity between men and women. Each cell represents a single regression estimate for the outcome measure specified in the row heading. Outcome mean refers to 12-month averages at the left of the threshold. Regressions control for year and region fixed effects and pre-reform characteristics at age 16 including an indicator for having a child, an indicator for receiving social insurance, an indicator for being fatherless or motherless, and disability status. Robust standard errors clustered at the individual level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix References

BECKER, G. S. (1960): "An economic analysis of fertility," in *Demographic and economic change in developed countries*, Columbia University Press, 209–240.

BLÖNDAL, K. S., J. T. JÓNASSON, AND S. SIGVALDADÓTTIR (2016): *Sérkenni námsferils starfsnámsnemenda í framhaldsskóla: afstaða og skuldbinding til náms, líðan og stuðningur foreldra og skóla*, Menntavísindastofnun, Menntavísindasvið, Háskóli Íslands.

BLOSSFELD, H.-P. (2009): "Educational assortative marriage in comparative perspective," *Annual review of sociology*, 35, 513–530.

CALONICO, S., M. D. CATTANEO, AND R. TITIUNIK (2014): "Robust nonparametric confidence intervals for regression-discontinuity designs," *Econometrica*, 82, 2295–2326.

CESARINI, D., E. LINDQVIST, R. ÖSTLING, AND A. TERSKAYA (2021): "The Eects of Unearned Wealth on Marital and Fertility Outcomes," Working paper, Stockholm School of Economics.

CHU, C. C., J.-C. LIN, AND W.-J. TSAY (2020): "Males' housing wealth and their marriage market advantage," *Journal of Population Economics*, 33, 1005–1023.

GOLOSOVY, M., M. GRABERZ, M. MOGSTAD, AND D. NOVGORODSKY (2021): "How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and Unearned Income," Tech. rep., University of Chicago.

HANKINS, S. AND M. HOEKSTRA (2011): "Lucky in life, unlucky in love? The effect of random income shocks on marriage and divorce," *Journal of Human Resources*, 46, 403–426.

KIRKEBØEN, L., E. LEUVEN, AND M. MOGSTAD (2021): "College as a Marriage Market," Tech. rep., National Bureau of Economic Research.

LOVENHEIM, M. F. AND K. J. MUMFORD (2013): "Do family wealth shocks affect fertility choices? Evidence from the housing market," *Review of Economics and Statistics*, 95, 464–475.

MARE, R. D. (1991): "Five decades of educational assortative mating," *American sociological review*, 15–32.

ÞORLÁKSSON, Á. Ö. (2019): "Framhaldsskólanemendur sem gera hlé á námi: Einkenni og námsframvinda," MSc thesis, University of Iceland.

PENCAVEL, J. (1998): "Assortative mating by schooling and the work behavior of wives and husbands," *The American Economic Review*, 88, 326–329.