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The short-term labor market effects of shortening upper secondary school in Iceland.

Name student: Mikael Jóhann Karlsson
Student ID number: 649598

Supervisor: Ana Figueiredo
Second assessor: Laura Hering

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Abstract

This thesis estimates the impact of a school reform in Iceland in 2015, which reduced the number of years necessary to graduate from upper secondary school, from four years to three. I use a difference-in-difference empirical method to explore labor market participation implications of the policy. The results reveal highly statistically significant and positive policy effects on the intensive margin. Meanwhile, on the extensive margin, treated individuals are also significantly less likely to be employed. A closer examination unveils heterogeneity by gender. Wherein, the large positive effect on the intensive margin is driven by females and treated males are 3.23 times less inclined to be employed than their older counterparts. Moreover, treated males show a 46 percent higher tendency to be primarily in education. In contrast, treated females are expected to work more both part-time and full-time, indicating that they deferred higher education.

Contents

1. Introduction

The debate surrounding the effects of education on long run socio-economic outcomes is an ever-expanding literature in social sciences. Opportunities in delivery of the study material with growing technological advancements and ever adapting labor market keep this discussion circulating constantly, both within academia and as a subject of interest to the general population. Questions such as, are we wasting to much time in school? Is the curriculum keeping up with the changes within society? and etcetera. Researchers are continuously searching for evidence on the most optimal structure and design of our education system. Building upon policies yielding natural experiments to identify the effects of for example the length of the school year, class sizes and so on and so forth.

This paper aims to enrich this literature by measuring the effects of one such alteration. The policy examined and discussed in this paper is the shortening of upper secondary school by one year in Iceland, that took effect in the fall of 2015 and its short-term labor market implications. Before the policy, students spent fourteen years in school from the ages of six to twenty years old, afterwards it was reduced to thirteen years. As a result, two cohorts, those born 1998 and 1999 graduated simultaneously in 2018 leading to a quasi-natural experiment exploited in this paper.

Considering the fact that the topic of education is heavily researched, there already exists some literature in different settings on the shortening of upper secondary school. Despite that, the focus has been more centered on performance in university than on labor market outcomes (e.g., Asgeirsdóttir et al. 2022, Krashinsky 2014, Pischke, 2007, Büttner and Thomsen, 2015). Notably, the papers produced by Pischke (2007) and Büttner and Thomsen (2015) also explored some of the labor market dynamics addressed in this paper. Notably, both papers show that treated students are more likely to defer higher education and Pischke (2007) finds no evidence of long-term labor market effects.

Consequently, the contribution of this thesis is to provide further evidence on these labor market dynamics, focusing on the short run. Additionally, Büttner and Thomsen (2015) are unable to demonstrate the choice behavior of males in terms of university enrolment due to military obligations in Germany. This paper provides support for their results in respect to females, uncovering the same trend for university deferral through a measure that estimates they are more prone to full time work, indicating a shift away from school. In sharp contrast, males are 46 percent more likely to be enrolled in education while simultaneously being 52 percent less likely to be primarily working.

Beforehand, I expected the policy effect to exhibit this deferral trend equally for both genders, because I observed that the importance of traveling and experiencing the world was a common view amongst teenagers at the time. It is worth noting that the dataset covers individuals from 2015 to 2021 and thus the onset of Covid-19, in the beginning of 2020 could have influenced this channel somewhat, especially from the fall of 2020 onwards. However, this paper applies a difference-in-difference empirical approach to absorb shocks that affect all cohorts simultaneously. Exploiting publicly available datasets provided by The International Social Survey Programme, I create a repeated cross-sectional dataset from their independent research on various subjects. Each dataset contains important background characteristics to this thesis that allowed me to explore the causal effect of a one year decrease in upper secondary school on labor market outcomes through an exogenous school reform.

Building upon this reform, I find a highly significant increase in number of hours worked on the intensive margin, dominated by female labor input. Despite finding no significant effects for males, the same trend also applied to them when only considering working males. Interestingly, the gender heterogeneity on the extensive margin revealed opposite signs. Females are more inclined to be engaged in work, which is consistent with the German policies on university deferrals for females. In contrast, males have a decreased likelihood of being employed, which was unexplored in the German setting (e.g., Pischke, 2007, Büttner and Thomsen, 2015). In addition, to the traveling channel, the difference might be partially rooted in childbearing, forcing females in a different direction from the males. Exploring this channel further is impossible with the dataset available.

Undoubtedly, finding evidence in support of previous research and supplementing it with additional information that they are unable to show is the main accomplishment of this thesis. However, the reform is also questioned within the Icelandic community, Asgeirsdóttir et al. (2022) find that the initial treated cohorts performed worse in university. Moreover, the National Student Association claims that the policy affected mental health negatively and diminished participation in extracurriculars. Despite these insinuations, dropout rates have gone steadily down while graduation rates, four years after entering upper secondary school have risen since the policy was implemented (*Statistics Iceland: Lower Dropout Rate in Upper Secondary Education*, 2022).

Additionally, Jónsson et al. (2015) demonstrate that, on average, around twenty percent of dropouts left school to persue a job, while also noting that the trend could be higher. Because some expelled students, due to lack of attendance, probably also left at least partly for the same reason. They further underline that males are performing a lot worse than females in upper secondary school with only 38 percent graduating in four years in 2012. This paper finds a 46 percent increased likelihood that males are still primarily in education, suggesting that the reform influenced the trade off between working and finishing school in favor of school.

The organization of this thesis is as follows, Chapter 2 provides a literature review to establish the most relevant ideas and results currently available, supplemented by research more closely related to the topic of this paper. Chapter 3 enhances understanding of the policy that is being addressed as well as giving a brief overview of the reasons and prior development underpinning its realization. The data and methodology are described in Chapters 4 and 5. Meanwhile, results of the main analysis are presented in Chapter 6 with additional heterogeneity and robustness analysis in Chapters 7 and 8. Finally, Chapter 9 concludes with an overview of the results, limitations and recommendations for future research.

2. Literature review

The economic literature researching the effects of schooling on future economic outcomes is vast and well documented from a broad spectrum of policies. For example, Cunha and Heckman (2007) argue that skill formation happens in early childhood and interventions aimed at disadvantaged youth could help reduce the persistent gap between children of different social backgrounds, in terms of long-term economic outcomes.

Moreover, changes within the school structure are well researched as well. For example, lowering the number of students per classroom has shown significant positive returns. The struggle, however, is making such a policy viable in terms of expenses within school infrastructure and by the boundaries set by a limited set of qualified teachers. Fredriksson et al. (2013) find that the long-term benefits of such an intervention in primary school include improved cognitive and non-cognitive skills.

Expanding on the importance of qualified teachers, Jackson (2018) finds that a one standard deviation increase in teacher added non-cognitive skills, raises high school completion rate by 1.47 percent. Highlighted factors include drive, how well the student can adapt to his surroundings and self-restraint. Although, the challenge lies in recognizing important teacher traits that ensure these beneficial outcomes (Jackson, 2018).

Moving the discussion closer to the direct wage impact of education, Acemoglu and Angrist (1999) estimate that the private return to education is an approximately seven percentage rise in average salary for each additional year. In comparison, they note that previous literature has reached a consensus of around six to ten percent returns to schooling. Despite, not finding significant social returns to education, they uncovered that States within the U.S.A. that have higher average education levels also have higher average wages. Thus, indicating positive externality effects of education (Acemoglu & Angrist, 1999). Although Altonji (1995) estimates the return to a year's worth of extra courses only amounts to a 0.3 percent wage increase.

Bearing in mind that education has widespread effects that go far beyond an increase in average wages. Oreopoulos and Salvanes (2011) demonstrate a link to negative non- and pecuniary effects. They find that individuals that complete a higher number of years in school are happier, less likely to be unemployed, more satisfied with their occupation and several other factors correlated with overall life satisfaction. Therefore, policy makers should take stock of how important and extensive legislature on schooling can be to an individual's future outcomes.

Nevertheless, differences in degrees or completed school level might send even stronger signals about prospects than an arbitrary number of school years. Along these lines, the direct link of education years to GDP growth has not held up empirically. Hanushek and Woessmann (2012) addressed this issue by altering the measure of human capital through a metric of cognitive skills as opposed to unilateral comparison in years of schooling. In doing so, they found a strong and significant link between human capital increases and GDP growth in developing countries. Therefore, it suggests that a decrease in school years does not necessarily affect economic growth negatively if the human capital development remains intact. Perhaps, the other negative effects would also be lessened, if somehow schools can maintain the delivery of the material in a shorter timeframe (Hanushek & Woessmann, 2012).

More closely related to this thesis, Büttner and Thomsen (2015) look at the effects of a policy shortening upper secondary school in Germany. They found that treated females were more likely to defer university for a year. Meanwhile, enrollment into the military interfered with policy effects on male decision making.

In the same vein, Pischke (2007) focused on the effects of a shorter school year in Germany, except in a long run analysis. He found no significant effect on wages or employment levels among the treated individuals, the main explanation provided was that degrees have similar signaling power, regardless of the time spent obtaining them. However, he did find a diminished likelihood of enrollment into higher education in accordance with Büttner and Thomsen (2015). The dataset used in this paper has information on whether the individual is enrolled into education or not and thus sheds more light on this link, that both papers have inferred from the German policies.

Meanwhile, researchers have already explored this natural experiment that the Icelandic policy provides in terms of university level performance. They uncovered a statistically significant effect at the 1% level for lower grades, fewer credits finished in the first year and higher dropout rates (Asgeirsdóttir et al., 2022). Indicating that cognitive skills were lower for the treatment group upon entering university.

Although, they also demonstrate that individuals of higher skill, measured by average GPA in upper secondary school, attended university in 2018 and 2019 in the treatment group compared to the control group (Asgeirsdóttir et al., 2022). Taking into consideration that the policy might have altered teacher grading standards and possibly harder concepts were left out of the curriculum due to time constraints, grade inflations could explain this trend (Sabot & Wakeman-Linn, 1991). Krashinsky (2014) explored a similar policy in Canada, in 2003, where treated students also performed worse on individual courses as well as in overall GPA upon entering university. Indicating as well that an extra year in high school contributes to human capital development.

Unfortunately, less research exists on what schools do that stimulates cognitive skill growth as well as social skills and critical thinking (Oreopoulos & Salvanes, 2011). Thus, a policy shortening school might erode a significant unobserved factor that enhances development of these factors that in turn are correlated with socio-economic outcomes. As explained by Oreopoulos and Salvanes (2011), the neglection of research on this topic comes down to availability of data.

Although, as previously mentioned, Fredriksson et al. (2013) did provide a link between smaller classrooms in primary school and an increase in cognitive skill development. In fact, dropping one whole cohort out of four in upper secondary school should allow for smaller classes, assuming resources are unchanged by the policy. This channel will not be directly explored in this thesis but could be an undiscovered benefit of the policy in the long run.

3. The policy

The discussion on the length and organization of upper secondary school in Iceland had a long prologue before the eventual reform in the so called “White Book” on educational reform was passed by the Icelandic senate in 2014. In 1994 the shortening of the system from four years to three was initially proposed on the grounds that Icelandic students should graduate at the same age as students in other Nordic countries, whose policy throughout history has shaped Icelandic legislation. The idea lost momentum because the Ministry of Education wanted to first make the curriculum in primary school more rigorous, so the adjustment between educational levels would be smaller. Leaving the door open to rethink the shortening of upper secondary school in the future on a stronger foundation. (Oddsdóttir, 2020)

Such plans gained traction again at the onset of the 21st century, this time because the nature of the degree had changed within society. The labor market had developed the need for higher skilled employees, this increased demand was followed by a rise in enrollment into university. It was argued that since a degree from upper secondary school was no longer the primary basis of education before an individual started their occupational career, it was unnecessarily long in comparison to other Nordic countries. Moreover, decreasing the number of years by one could lower the opportunity cost of education for students and their families, while also relieving pressure on school infrastructure due to population increase. Finally, surveys suggested that school length was linked to dropouts. (Oddsdóttir, 2020)

This proposal brought forth by the Ministry of Education was rejected on the grounds that the policy proposal did not include collaboration with key members within the school community, namely teachers and principals. The critique also included concerns that such an intervention would limit the diversity of topics and options within the upper secondary school system and force all students into the same mold. Thus, after this discussion gained momentum every decade and despite a rise in atheism in the world, the old adage, the third time is the charm came true when the bill was finally passed in 2014. (Oddsdóttir, 2020)

The final nail in the coffin came about because only 44.2 percent of Icelandic students finished upper secondary school on time along with high dropout rates. In 2012 twenty percent of people at the age of 18 to 24 had not finished this school level nor were registered for it. The main problem identified was participation in the labor market. From 2003 to 2013, between 65 to 75 percent of people aged 18 to 24 were employed in the first quarter of the year. Therefore, increasing the time demand of schooling per annum over a shorter span of years was supposed to reduce the temptation of dropping out to go work. In other words, dealing with this problem of dropouts was one of the fundamental goals of the policy (*Hvítbók Um Umbaetur í Menntun*, n.d.).

Officially, the policy was implemented in the autumn of 2015, schools then had to adhere to a three-year study program instead of the four-year norm. However, the labor market links became more pronounced later, since working with school was not as common in the first years compared to the final year of school. Therefore, the treatment year is set in 2018 when the first affected cohort graduates. Nevertheless, the policy does not provide a completely clean natural experiment, some schools had previously adjusted to a three-year study program. Menntaskólinn í Borgarfirði in 2007, Framhaldsskólinn í Mosfellsbæ in 2009, Menntaskólinn á Tröllaskaga in 2010 and Kvennaskóli Reykjavíkur in 2011 are these four schools. (Oddsdóttir, 2020)

These schools account for about 6.2 percent of students in the upper secondary school system for the year of 2023 according to the Institution of Education (*Nemendasamsetning í Hefðbundnum Framhaldsskólum | Menntamálastofnun*, n.d.). Taking into consideration that the total number of students was similar in 2015 to 2023, it is appropriate to assume that the ratio was also similar after the policy was put into action. (*Nemendur Eftir Skólastigi, Tegund Náms, Almennu Sviði Og Kyni 1997-2021*, n.d.) Notably, the students in these schools were offered an option to prolong their studies, increasing the study period to 3,5 or 4 years before the policy was implemented in 2015. Statistics from the education reform indicate that about 70% graduated in three years in Kvennaskólinn and 57% in Menntaskólinn í Borgarfirði (*Hvítbók Um Umbaetur í Menntun*, n.d.).

To provide further context on the implementation of the policy, the curriculum was compressed by one year, supposedly keeping the material unchanged by supplementing the policy with lengthening the number of instructional hours in each week. Most likely the overhaul of the entire curriculum brought about some additional changes in direction and requirements imposed on students. Given the smaller timeframe, some assignments must have been cut out and overview of certain topics delivered in a more brief manner. Selection into university based on skill was more prevalent in the treatment group in 2018 and 2019, evident by their higher GPA averages. However, as mentioned before, their overall performance in university was worse than the control group despite this advantage (Asgeirsdóttir et al., 2022).

4. Data

4.1 Data sources

Addressing the research question of this paper requires panel or repeated cross-sectional data that includes year of birth for Icelandic citizens born 1996 to 2002 and their labor market participation between 2015 and 2021. This was achieved by combining datasets provided by The International Social Survey Programme (ISSP). On their website, datasets on various topics are accessible but only one survey each year. Thus, the combined dataset is a repeated cross-sectional dataset from 2015 to 2021 for 558 individuals, showing trends within age cohorts over time, in the key variables. Unfortunately, there were almost no interviews conducted in 2019 in Iceland, leaving that year absent relevant information to this paper (*Modules by Year*, n.d.).

4.2 Treatment allocation

The policy of shortening upper secondary school divides these age cohorts exogenously into the treatment and control group. Those born 1999 to 2002 represent the treatment group while the older cohorts (1996-1998) comprise the control group. The selected treatment year is 2018 because that is when cohorts born 1998 and 1999 graduate simultaneously, in the spring, the first cohort in the new three-year system and the last cohort in the traditional four-year system. It is crucial to mention that teenagers in the treatment group are unlikely to be working at the onset of the policy in 2015 because of youth and the dataset is limited to individuals that have reached the age of eighteen. Therefore, labor data is only available for the treatment group from 2017 onwards and the treatment year of 2018 is also more appropriate for the later cohorts (2000 to 2002).

4.3 Dependent variables

Each dataset has background characteristics concerning employment status, hours worked, nationality and year of birth. Therefore, it is possible to extract the main dependent variables "Work" and "WRKHRS" based on country and birthyear. On the one hand, the former dependent variable is a binary dummy variable indicating whether the individual has a paid occupation or is either unemployed or outside the labor force. Table 4.1 shows that just over two thirds of the individuals observed do in fact work. This trend is also more pronounced for the control group than the treatment group. On the other hand, the latter is a discrete variable that represents the number of hours worked each week by the respondent ranging from 0 to 99 hours a week.

Taking into consideration the potential for measurement error in self-reporting on hours worked, fifteen observations were treated as missing because these individuals reported 96 to 99 hours per week, the maximum amount possible in the questionnaire. I view such reporting as completely unreliable rather than being a slight overestimation that could be scaled down in weight, in the estimations. Table 4.1 shows an average of roughly twenty hours each week for all observations, while the mean in the control group is 5 to 6 hours larger than in the treatment group.

Table 4.1 Descriptive statistics for the dependent variables.

	All		**Control group**		**Treatment group**	
Variable	**N**	**Mean**	**N**	**Mean**	**N**	**Mean**
Hours worked each week	532	20.261	353	22.093	179	16.648
Work						
0. Not in paid work	558	.312	371	.296	187	.342
1. Currently in work	558	.688	371	.704	187	.658
Main status						
1. In paid work	542	.557	358	.595	184	.484
2. Unemployed	542	.054	358	.047	184	.065
3. In education	542	.369	358	.327	184	.451
4. Apprentice or trainee	542	.006	358	.008	184	0
5. Permanently sick	542	.009	358	.014	184	0
7. Domestic work	542	.006	358	.008	184	0

Notes: “Work“ and “Main status“ are categorical variables and the means represent the proportion of each category. Meanwhile, N captures the number of observations in each group. Source: The International Social Survey Programme.

In addition to these dependent variables, the dataset contains information about the momentary main status of the respondent, which allows for an estimation of the differences in choice between education and work for both males and females. Table 4.1 shows all possibilities for this categorical variable but almost 93 percent of observations are either working or in education. “Main status” should give an estimation of the level of full-time employment while “Work” also considers part time occupation. Therefore, it will provide a robustness check in Chapter 8 as well as give insight into the underlying labor participation dynamics. Unfortunately, this variable does not allow for a disentangling of university enrollment from other levels of education. For instance, older individuals primarily in education, might not have finished upper secondary school on time and are therefore mainly in education.

4.4 Control variables

When estimating the relationship between years of schooling and labor market participation, there are some potential confounders that need to be added to the regression to obtain unbiased estimates. Firstly, labor market participation is subject to seasonal fluctuations, especially amongst those primarily still in school. Therefore, the categorical variable “Month of interview” ranging from one to twelve captures this seasonality in the dataset. However, I substitute this variable in the main analysis for a newly created discrete variable “Season”.

The reason for this exchange is that summer work is prominent and commonplace for young people and the created variable could potentially better capture the seasonality in the dataset. It is defined as follows; December to February is the winter season, March to May is spring, June to August is summer and finally September to November is autumn. Each season is assigned a numerical value ranging from 1 to 4 respectively. Table 4.2 shows the distribution of observations in each season with exceptionally low data points in the spring, only 5.7 percent. Meanwhile, the distribution of observations, in terms of season, is similar between the treatment and the control group.

Furthermore, controlling for personal characteristics is important because the dataset is a repeated cross section and not a panel dataset covering the same individuals over time. Therefore, age is included as a control variable to account for differences in working preferences based on age and also gender for heterogeneous policy effects. Evident by Table 4.2, females are 53 percent of the sample and the average age is roughly twenty years old. Naturally, the control group, cosisting of older cohorts exhibits a higher average age than the treatment group. Moreover, “Birthyear” demonstrates the distribution in the sample in each age cohort. Increasing with age because labor data only becomes available at the age of eighteen years.

Additionally, differences in labor market size are considered, because job availability and the trade off between work and education can vary with location. Proximity to university and work could otherwise be confounding factors. This variable is a categorical variable ranging from one to five which represents a decreasing size from a large city to a rural farmhouse. Table 4.2 shows a logical rising frequency of observations with increasing size.

Table 4.2 Descriptive statistics for the independant variables.

	All		Control group		Treatment group	
Variable	**N**	**Mean**	**N**	**Mean**	**N**	**Mean**
Age	558	20.328	371	20.806	187	19.38
Years in education	537	13.743	354	13.975	183	13.295
Sex						
1. Male	558	.471	371	.474	187	.465
2. Female	558	.529	371	.526	187	.535
Birthyear						
1996	558	.233	371	.35	187	
1997	558	.206	371	.31	187	
1998	558	.226	371	.34	187	
1999	558	.129	371		187	.385
2000	558	.088	371		187	.262
2001	558	.063	371		187	.187
2002	558	.056	371		187	.166
Degree						
1. Primary school	513	.004	331	0	182	.011
2. Lower secondary	513	.039	331	.006	182	.099
3. Upper secondary	513	.099	331	.088	182	.121
4. Post secondary	513	.413	331	.412	182	.401
5. Short-cycle tertiary	513	.055	331	.057	182	.049
6. Lower tertiary	513	.366	331	.399	182	.308
7. Upper tertiary	513	.023	331	.030	182	.011
Location						
1. A big city	543	.379	359	.404	184	.332
2. The suburbs/outskirts	543	.263	359	.253	184	.283
3. A town/small city	543	.252	359	.242	184	.272
4. A country village	543	.042	359	.028	184	.071
5. A farm	543	.063	359	.072	184	.043
Season						
Winter	558	.265	371	.261	187	.273
Spring	558	.057	371	.07	187	.032
Summer	558	.299	371	.31	187	.278
Autumn	558	.378	371	.358	187	.417
Spouse						
0. No spouse	551	.601	364	.560	187	.679
1. Working spouse	551	.319	364	.346	187	.267
2. Spouse not working	551	.074	364	.088	187	.048
3. Spouse, never worked	551	.005	364	.005	187	.005

Notes: Age and years in education are continous variables so their means are the actual means of the two variables. Meanwhile, the rest are categorical variables. Thus, the means represent the proportion of each category. Source: The International Social Survey Programme.

Moreover, I control for relationship status that also captures whether the spouse is working or not. Given that individuals in a relationship are more likely to have moved out of their parents house, they might face more financial constraints which in turn encourages them to work. It is a stretch to claim that this also controls for some non-cognitive skill differences, however, it is logical that such traits would make an individual more desirable and thus more likely to be in a relationship. At the bare minimum, it takes self-restraint to be encumbered, which was one of the non-cognitive traits highlighted by Jackson (2018). Table 4.2 shows that most don't have a spouse, around 60 percent while the second most common situation is a working spouse, 32 percent of observations.

Finally, a measure of education is also added through a continuous variable that captures the number of years in education. The average in Table 4.2 is logical based on the average age, almost six years, the starting age separating the two. In that regard, unrealistic answers were treated as missing, such as below five years or within three years of the persons age. This was done because the minimum required years of schooling is ten years and children start school at the age of six. A total of 21 missing observations are reported for this variable. Due to these challenges with self-reported statistics, that variable is also substituted for a variable that reports the highest obtained degree, using an alternative measure that controls for education level.

This categorical "Degree" variable ranges from one to seven in progressing order of higher education with the levels displayed in Table 4.2. The policy effect is robust to these changes which provides evidence that measurement error is not systematically biasing the estimates through these two variables. Both variables should capture the individualistic choice between working and continuing into further education beyond upper secondary school. Expectedly, those with a higher number of years or higher attained degree have completed more education and are thus more free to work. Overall, Table 4.2 presents a logical inverted parabola for all groups with slightly higher obtained degrees for the control group. Which is logical given their higher average age.

5. Methodology

5.1 Empirical specification

The main specification:

$Y_{it} = \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 Treatment_{it} + \boldsymbol{\beta}_2 Post_t + \boldsymbol{\beta}_3 Treatment_{it} \text{ x } Post_t + \boldsymbol{\beta}_4 X_{it} + e_{it}$ (1)

Model 1. uses a difference-in-difference method, whereas Y_{it} is the dependent variable for hours worked and employment status, working or not. Treatment is a dummy variable that takes the value one for cohorts born 1999 to 2002 and zero otherwise (1996 to 1998). The coefficient $\boldsymbol{\beta}_1$ captures the differences between the groups before the policy was implemented. Post is a dummy variable that equals one in the years after policy implementation, 2018 onwards and zero if the observations are from the pre-policy period (2015 to 2017). $\boldsymbol{\beta}_2$ shows the progression of the control group independent of the treatment. $\boldsymbol{\beta}_3$ is then the coefficient of interest, the policy effect obtained by interacting the "Treatment" and "Post" variables. X_{it} is a control variable vector consisting of important individualistic information such as gender, age, education and whether individuals are in a relationship with a working partner. Additionally, controls for location that captures labor market size and time of the interview, because teenager labor market participation fluctuates based on seasons. Finally, e_{it} is the error term.

The shortening of upper secondary school provides a quasi-natural experiment which allows me to overcome endogeneity issues, mainly caused by selection in other settings and in turn estimate the causal effect of the policy on labor market outcomes. Furthermore, to control for potential static confounders or shocks biasing the estimates obtained through the regressions on the cohort or year level, Model 1 uses the benefits of the difference-in-differences approach to absorb such factors that influenced both the treatment and the control group equally. Such shocks include for example economic cycle effects and labor market dynamics.

Bertrand et al. (2002) challenge the validity of many papers using this statistical method due to the potential of serial correlation in the error term. I cluster standard errors at the cohort level in almost all regressions to account for dependence between observations of people born in the same year. Standard errors were similar using clustered errors and clustered robust standard errors. Therefore, heteroskedasticity in the error term is deemed of lesser concern.

However, the one exception is that I use robust standard errors when estimating heterogenous effect for only working individuals in Table 7.3. The reason for this switch is that the sample is much smaller due to both conditioning on gender and working, resulting in a small number of observations within each cluster.

Returning to the potential problem of autocorrelation, the dataset does not cover numerous time periods as the average difference-in-difference research summarized by Bertrand et al. (2002), only six compared to 16.5 periods. Another issue I find unlikely to be present in the analysis is that the dependent variables can have persistence over time, leading to bias. Using different individuals over time should limit this issue as opposed to panel data. The final main issue raised by Bertrand et al. (2002) is that the treatment variable is unchanged across time periods, which is an ingrained part of the statistical approach. Their suggestions to fix this potential problem are better suited to larger samples with more cohorts.

Using a repeated cross-sectional dataset comes with the disadvantage that individual fixed effects cannot be included to account for observable and unobservable time-invariant characteristics on the individual level. Therefore, fixed effects are added on the cohort and calendar year level to account for any time-invariant confounders within each specific cohort and year (Joshua David Angrist & Jörn-Steffen Pischke, 2008).

The regression from Model 1 then becomes:

$Y_{it} = \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 Treatment_{it} \text{ x } Post_t + \boldsymbol{\beta}_2 X_{it} + \alpha_i + \alpha_t + e_{it}$ (2)

Model 2 drops the "Treatment" and "Post" variables to avoid multicollinearity and includes cohort specific fixed effects captured by "α_i" and year specific fixed effects in "α_t". The coefficient $\boldsymbol{\beta}_1$ is now the policy effect and X_{it} is the same control variable vector as in Model 1. This model improves Model 1 by removing potential omitted variable bias but could also absorb some of the policy effect. It is crucial to mention that any measurement error in the independent variables could weaken the estimated relationship between the policy and labor market participation through attenuation bias. Therefore, because of these disadvantages and the type of dataset, this is only included alongside Model 1 in most tables (Joshua David Angrist & Jörn-Steffen Pischke, 2008).

5.2 Identification

Firstly, the main identifying assumption of a traditional difference-in-difference approach is common trends, meaning that in the absence of the treatment, both groups would have continued the same path (Joshua David Angrist & Jörn-Steffen Pischke, 2008). Due to the age of the individuals in the models and nature of the dependent variables, it is hard to obtain pre-trends from before the policy year of 2018. This difficulty lies in the fact that there is only data available for the treatment group from 2017 onwards, once the first cohort turned eighteen years old.

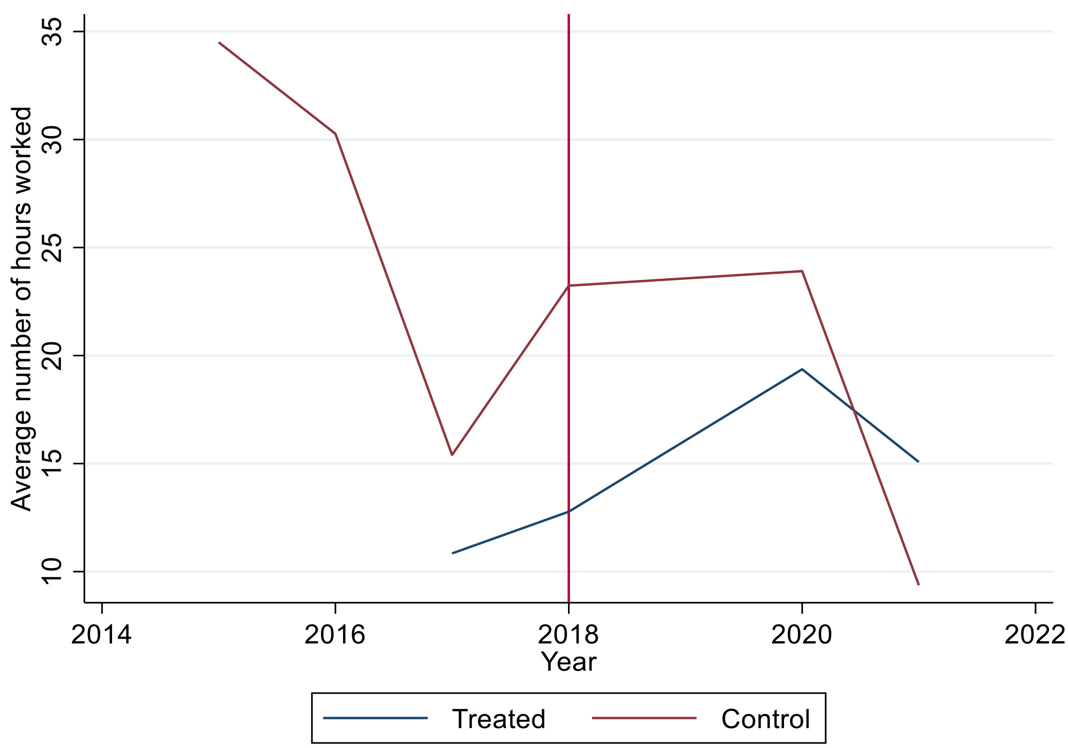

Figure 5.1 The graph depicts the averages for the number of hours worked in each week for the treatment and control group. Source: The International Social Survey Programme.

It is hard to argue that the pre-trends assumption is met, judging from Figure 5.1. Although both groups show an increase from 2017 to 2018, the line is significantly steeper for the control group than the treatment group. Moreover, the large drop in the control group over time is also unexplored but some possible mechanisms are contemplated in the results, Chapters 6 and 7. It is essential to highlight the fact that limitations on pre-policy data for the treatment group are not ideal, but the best option publicly available.

In contrast, Figure 5.2 shows opposite trends in the years 2017 to 2018 on the extensive margin. Nevertheless, in the pre-treatment period, the overall decrease is somewhat comparable between the two groups, so the assumption looks more robust than in Figure 5.1. Unfortunately, due to the age of individuals in the treatment group, there is no data available for their labor participation in 2016 and even if there was, eighteen years of age could be somewhat of a threshold for labor market participation.

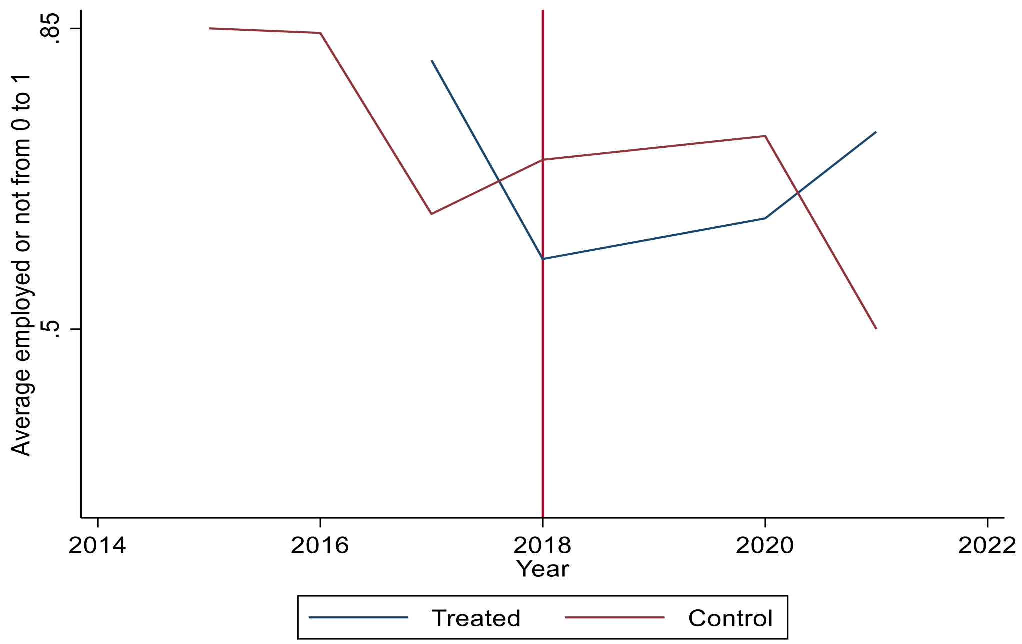

Figure 5.2 The graph outlines the average decision to work or not, within the control and treatment groups. Source: The International Social Survey Programme.

Secondly, an important assumption in this empirical method is a lack of anticipation effects. As previously stated, some schools adjusted to the three-year system, years prior to the policy and due to the quality of the dataset, it is impossible to remove these individuals from the control group. Students from these schools in the control group are expected to bias the estimates downwards (Joshua David Angrist & Jörn-Steffen Pischke, 2008). Because, if the only uncontrolled difference between the groups is the policy, then there should be no difference between individuals in the control group that completed school in three years and those forced to enroll in such a program in the treatment group.

Nevertheless, some selection into three-year schools based on ambition or ability is probable, but at least two of the schools that preemptively changed their system are rural schools. Selection into those schools was likely driven by vicinity to their homes. Notably, these schools also left it up to the student to finish in the given timeframe with the option of finishing school in three and a half or four years (*Hvítbók Um Umbaetur í Menntun*, n.d.). Making up approximately 6.2 percent of a control group with 392 individuals, this applies to between twenty to thirty observations in the dataset.

Furthermore, the policy involved compressing the study material into fewer years by adding additional instructional hours per week. Such increased time constraints might have altered the decision to work alongside upper secondary school, especially on the intensive margin. Taking into account that affected cohorts had similar financial constraints as older cohorts, the decision to work on the extensive margin might have been less affected. Thirdly, the last vital assumption is that group composition remains the same post treatment. This condition is easily met given that the year of birth is not changeable.

5.3 Alternative treatment assignment

Considering the possibility that the 1998 cohort were also affected by the policy. It is worthwhile to examine the differences in the results when these individuals are added to the treatment group. The argument is that the revamp of the education system might have also affected the curriculum of the ones in the final year of the older system. Furthermore, graduating simultaneously with a younger cohort created abnormal competition on the labor market.

Additionally, one cannot completely discount the potential peer effects of such a policy. Feeling behind in the old system could have motivated the 1998 cohort to perform better in university as was shown by Asgeirsdóttir et al. (2022), despite having an overall lower GPA. Moreover, making this adjustment increases the validity of the research design by providing more evidence on existing pre-trends and balances the observational size differences between the two groups. Resulting in higher statistical power in the estimations.

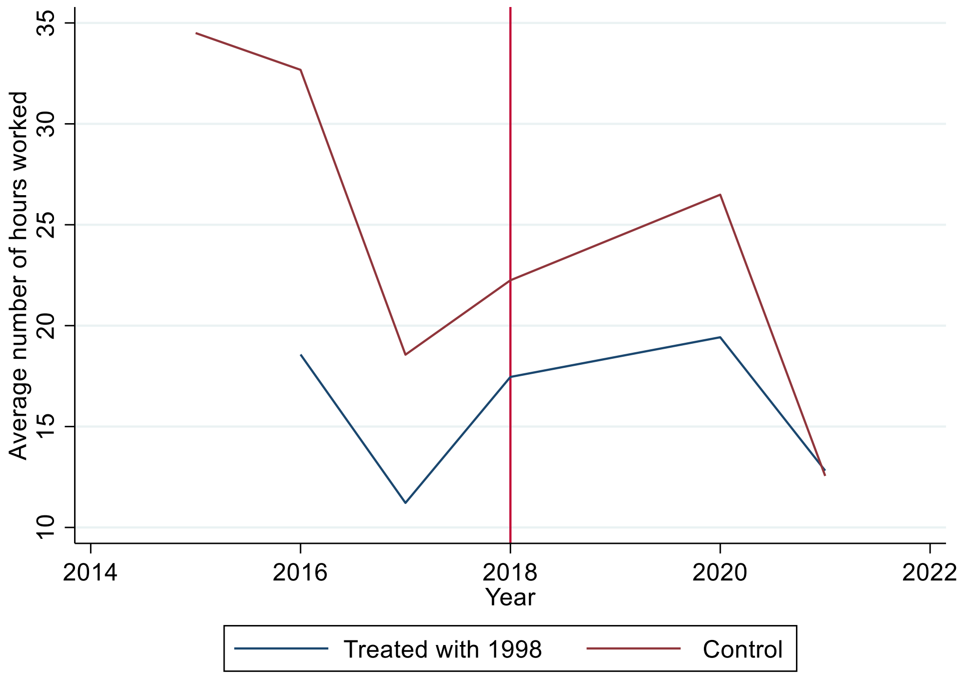

Figure 5.3 The graph shows the average number of hours worked in line with Figure 5.1. Although those born in 1998 have been assigned to the treatment group. Source: The International Social Survey Programme.

Converting the composition of the treatment group to include the cohort born in 1998 offers an alternative angle on the pre-trends assumption. This is captured in Figure 5.3 on the intensive margin. Overall, there is a similar trend year over year in the observed period. There is a considerable decrease in the pre-treatment period for the control group while the treatment group decreases only slightly. It is an improvement from Figure 5.1 but does not convey the same information as before. However, it does strengthen the internal validity of the empirical approach to see that the trend for the 1998 group is identical to older cohorts between 2016 to 2017. Because it shows that different age groups follow similar trends besides the treatment.

Meanwhile, Figure 5.4 shows pre-trends on the extensive margin when including the 1998 cohort in the treatment group. The year over year trend is not similar but the trend between 2016 to 2018 is quite comparable. A significant decrease on the extensive margin is observed in both the treatment and control group. In coherence with Figure 5.2, there is still a considerable decrease in the control group between 2020 and 2021. Possibly the onset of Covid-19 drove people in the control group increasingly back to school.

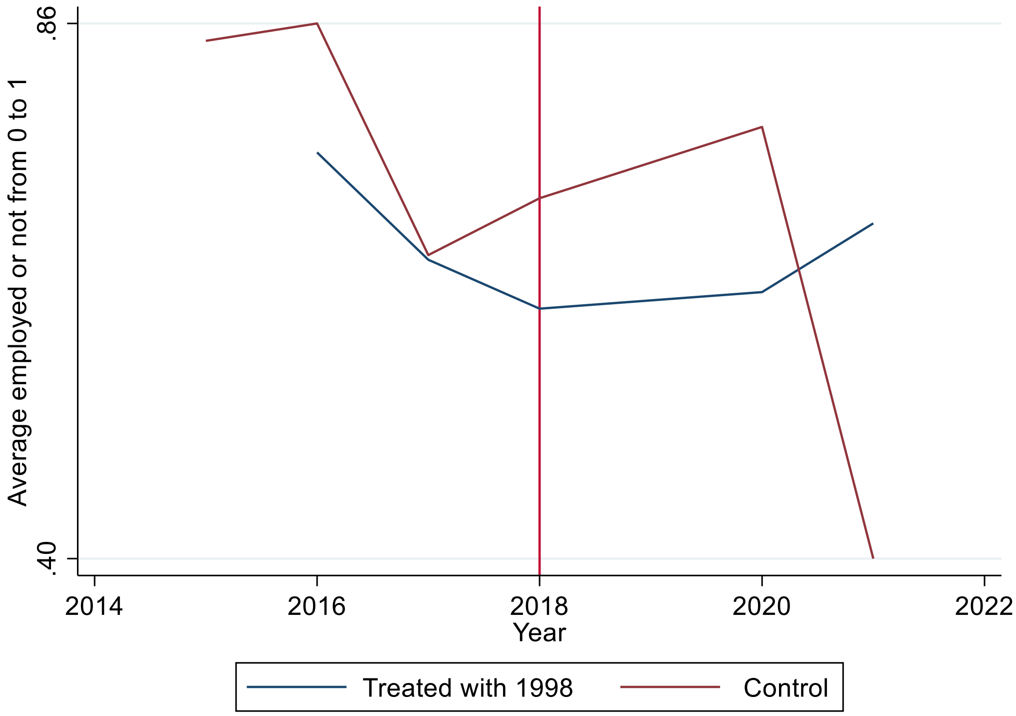

Figure 5.4 The graph shows the estimated average decision to work in line with Figure 5.2. The difference being that those born in 1998 have been assigned to the treatment group. Source: The International Social Survey Programme.

6. Results

Table 6.1 analyzes the policy effect on the intensive margin, showing a statistically significant policy effect for all specifications. Column 1 represents the relationship absent controls, a policy effect of 6.7 hours per week statistically significant at the ten percent level. In Column 2, controls are introduced to the model, the individualistic variables “AGE” and “SEX” are interacted to capture some heterogenous effects in gender while letting age vary in Columns 2 to 4. To keep the table smaller these interaction estimates are omitted from Table 6.1, a more detailed accounting of these effects is provided in Table i in the appendix.

Due to expected sensitivity in the dependent variable to seasonality, the variable “Month of interview” is added to control for monthly trends in Column 2 but is then substituted for a created “Season” variable, neither has a statistically significant coefficient. Furthermore, “Years in education” is also switched out for another measure of education “Degree” in Columns 3 and 4. The policy effect is robust to these changes but the value increases from 10.4 to 13.4 hours per week of additional work in Column 3 and then further to 16.8 once cohort specific and year fixed effects are added in Column 4. After controlling for time-invariant observables and unobservables, the estimate in Column 4 is also the most statistically significant, at the 1% significance level.

Table 6.1 Policy effect on the intensive margin.

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Treatment	-11.35**	-12.98**	-13.79**	
	(4.014)	(4.237)	(4.657)	
Post	-0.218	-1.461	-3.732	
	(2.935)	(2.238)	(4.083)	
Policy effect	6.714*	10.38**	13.40**	16.80***
	(3.095)	(4.124)	(5.081)	(4.012)
Month of interview		0.0885		
		(0.377)		
Location		1.717**	2.232***	2.206***
		(0.690)	(0.459)	(0.449)
Years in education		-0.0179		
		(0.870)		
Spouse		1.488	1.999	1.927
		(1.100)	(1.246)	(1.321)
Season			0.767	0.353
			(0.654)	(0.656)
Degree			3.031**	2.956*
			(1.153)	(1.210)
Constant	22.19***	18.87	3.726	0.352
	(4.014)	(10.06)	(6.045)	(4.802)
Observations	532	509	480	480
R-squared	0.021	0.092	0.136	0.145

Notes: Standard errors are clustered at the cohort level and presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. The table shows the relationship between treatment assignment and hours worked per week using a difference-in-difference method. The columns vary in choices of controls and the policy effect is robust to these changes. Column 4 uses controls for cohort specific and year fixed effects. Variables "Treatment" and "Post" were dropped to address potential multicollinearity. In Columns 2-4, variables "AGE" and "SEX" are interacted together to capture some heterogenous effects in gender while letting age vary. Source: The International Social Survey Programme.

Moreover, the variable "Location" is highly significant across all specifications, it captures the effects of size in labor markets where the individual resides. Unsurprisingly, those in smaller regions work more, which is intuitive, since most major schools are in larger municipalities. Therefore, the trade-off between work and education is more favorable to work in smaller regions, ceteris paribus. As was mentioned above, "Degree" is an individual level control variable for education level. For each higher level of education, an individual works approximately three more hours each week. Finally, the coefficient for spouses' work effort is not significant.

Unfortunately, the validity of a survey questionnaire in terms of measurement error is always a concern. Because such an error could bias the estimates obtained in the regressions (Joshua David Angrist & Jörn-Steffen Pischke, 2008). However, the robustness of the estimates between Column 2 and 3, regardless of which measure is used, increases the validity of these control variables. Alternative combinations of these substitutable control variables were checked and yielded similar estimates and significance levels.

Table 6.2 Policy effect on the extensive margin.

VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Treatment	0.502**	0.351	0.255		
	(0.204)	(0.281)	(0.359)		
Post	-0.171	0.502*	0.472***		
	(0.143)	(0.299)	(0.160)		
Policy effect	-0.707**	-1.079***	-0.866***	-0.939	-1.104*
	(0.323)	(0.316)	(0.180)	(0.654)	(0.654)
Age		-0.281***	-0.242***	-0.117*	-0.164**
		(0.0830)	(0.0936)	(0.0675)	(0.0804)
Sex		0.156	0.127	0.132	0.178
		(0.280)	(0.271)	(0.200)	(0.195)
Month of interview		-0.0359			-0.0235
		(0.0410)			(0.0290)
Location		-0.0511	-0.0440	-0.0398	-0.0493
		(0.0896)	(0.0689)	(0.0875)	(0.0832)
Years in education		0.113*			0.108
		(0.0684)			(0.0786)
Spouse		0.313**	0.333***	0.319**	0.287**
		(0.140)	(0.117)	(0.149)	(0.144)
Season			0.0156	0.0471	
			(0.0562)	(0.0907)	
Degree			0.118	0.144	
			(0.0829)	(0.0933)	
Constant	0.944***	4.828**	4.712**		
	(0.204)	(2.213)	(2.198)		
Observations	558	527	499	499	527

Notes: Standard errors are clustered at the cohort level and presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. The table shows the relationship between treatment assignment and the decision to work using a difference-in-difference method. Fixed effects are included in Columns 4 and 5. Source: The International Social Survey Programme.

Moving onto the estimated policy effect on the extensive margin shown in Table 6.2. The policy effect is highly significant in Columns 1-3 and robust to changes in controls. In fact, the magnitude of the policy effect in Columns 2 and 3 translates to approximately 18 to 23 percent less likelihood of being employed for the treatment group compared to the control group. Overall, these significant results on the extensive margin are not in line with the results of Pischke (2007) in the German setting of 1967. Although, he estimated long run effects, while these reflect short run trends.

Despite that, once cohort specific and year fixed effects are included, the estimate remains similar in size but no longer statistically significant in Column 4. There could be an unobserved confounder within cohorts that is correlated with the decision to work that once accounted for, the policy effect becomes insignificant. However, the value is similar, and the estimate is close to the 10% significance level and when swapping out “Season” and “Degree” for “Month of interview” and “Years in education”, the estimate is significant at the 10% level, as can be seen in Column 5. Additionally, including the fixed effects might also absorb some of the policy effects when it potentially removed unwanted confounders.

Interestingly, the regressions consistently show a negative coefficient for age, the older an individual becomes, the less likely he is to be employed. Logical explanations for this phenomenon might include that working with university is more demanding than alongside upper secondary school. This could explain the decrease in the control group over the sample period. Another possible scenario is that some students deferred higher education after graduating from upper secondary school to work and travel, returning to education later. This channel could be more prolific for the treatment group, who were already ahead of schedule and therefore did not have to rush into university. The findings of Büttner and Thomsen (2015) and Pischke (2007) suggest that this could be the reality.

Another highly significant factor in the regressions is whether an individual has a spouse. A plausible explanation is that individuals who are in a relationship, could be more likely to live with their spouse, instead of with their parents. These individuals might face more financial constraints, for example in the form of rent and increased cost of living in general. Therefore, they are more likely to work to meet these financial obligations.

7. Heterogeneity analysis

Continuing in accordance with Büttner and Thomsen (2015), Pischke (2007) and social studies literature in general, I disentangle the policy effects by gender. Observing that the results on the intensive margin are primarily driven by females while it is mostly the opposite on the extensive margin. Column 1 of Table 7.1 shows a statistically significant treatment effect for females, the estimate is robust to controls added in Column 2 and inclusion of fixed effects in Column 3. Meanwhile, the treatment effect for males is statistically insignificant in all specifications, Columns 4 to 6 in Table 7.1. However, it seems that decreasing labor market size has a positive effect on male labor market participation. The effect of "location" is highly significant and shows an approximately 2.5 hours increase per week.

Table 7.1 Policy effect on the intensive margin. Heterogeneity by gender.

	Females			Males		
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	-12.13**	-13.47*		-10.18**	-9.317	
	(4.337)	(6.268)		(3.665)	(7.781)	
Post	-5.998*	-6.853*		4.856	9.821	
	(2.943)	(2.986)		(3.399)	(5.620)	
Policy effect	13.17**	15.48**	17.37***	0.715	-2.549	0.804
	(3.783)	(4.537)	(4.581)	(3.891)	(4.806)	(5.643)
Age		-0.854	-3.094*		-1.795	0.267
		(1.217)	(1.467)		(1.769)	(1.559)
Season		0.736	-0.105		0.504	1.205
		(1.313)	(1.361)		(1.187)	(1.245)
Location		1.585	1.229		2.428***	2.575***
		(0.857)	(0.841)		(0.528)	(0.505)
Degree		2.583	2.074		3.439	3.423
		(1.729)	(1.707)		(3.005)	(3.273)
Spouse		0.324	0.381		2.725	2.599
		(2.099)	(2.496)		(1.727)	(1.770)
Constant	22.63***	22.06	63.33**	21.61***	31.67	-10.55
	(4.337)	(26.30)	(24.69)	(3.665)	(42.28)	(40.03)
Observations	275	252	252	257	228	228
R-squared	0.029	0.090	0.087	0.042	0.111	0.080

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. Table 7.1 shows the relationship between treatment assignment and hours worked per week using a difference-in-difference method. The first three columns show female policy effect with and without controls, in all specifications, the policy effect is highly significant and large. The latter columns reveal the policy effect for males that is not signifcant with or without controls. Source: The International Social Survey Programme.

Comparatively, Column 1 in Table 7.2 shows a statistically signifcant policy effect for females at the 10% level, which represents a 14.2 percentage point higher likelihood of being employed. This effect is insignificant when controls are added in Columns 2 and 3. Meanwhile, the policy effect is highly significant and economically large for males in Columns 4 and 5. Using controls, Column 5 suggests that males exposed to the policy are 3.23 times less likely to have a paid job than males in the control group. Robustness check added in Column 3 of Table 8.1 tells a similar story. Males in the treatment group are also less likely to have work as their main status at the time of taking the survey questionnaire.

Table 7.2 Policy effect on the extensive margin. Heterogeneity by gender.

	Females			Males		
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	-0.288*	-0.534		14.69***	14.71***	
	(0.172)	(0.427)		(1.110)	(1.185)	
Post	-0.679**	-0.131		0.379	1.363	
	(0.344)	(0.546)		(0.384)	(0.969)	
Policy effect	0.712*	0.551	0.367	-15.49***	-16.29***	-15.73
	(0.409)	(0.337)	(0.771)	(1.400)	(1.496)	(752.7)
Age		-0.224	-0.268***		-0.333*	0.00532
		(0.165)	(0.101)		(0.186)	(0.0983)
Season		-0.0814	-0.109		0.0947	0.199
		(0.104)	(0.129)		(0.164)	(0.138)
Location		-0.194	-0.222*		0.176	0.205
		(0.144)	(0.117)		(0.120)	(0.145)
Degree		0.0379	0.0208		0.291	0.327**
		(0.130)	(0.134)		(0.296)	(0.141)
Spouse		0.217	0.212		0.456**	0.439**
		(0.268)	(0.220)		(0.190)	(0.223)
Constant	1.204***	5.816**		0.622**	4.908	
	(0.172)	(2.853)		(0.255)	(3.254)	
Observations	295	267	267	263	232	232

Notes: Clustered standard errors are presented in parentheses, except with the fixed effects models since. Significance levels are expressed as follows *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Table 7.2 shows the relationship between treatment assignment and the decision to work using a difference-in-difference method. The first three columns show female policy effect with and without controls. The latter three columns reveal the policy effect for males with and without controls. Cohort specific and year fixed effects are included in Columns 3 and 6. Column 6 does not offer a lot of information due to inflated standard errors. Assumptions of the model likely do not hold in Column 6, perhaps due to small number of observations or cohorts. Source: The International Social Survey Programme.

Furthermore, the fixed effects model estimate in Column 6 shows similar coefficient value, but nonsensical inflated standard errors. Undoubtedly, resulting from a violation in the assumptions of the model. Interestingly, having a spouse has a significant positive effect on male labor participation. Potentially, it captures some individuals that have moved out of their parents house or that having an income and more independence makes them more attractive as a spouse. Meanwhile, age still has a negative correlation with the decision to work for both genders.

At first glance, the large policy effect for females in number of hours worked accompanied by almost no signifcant difference in the decision to work, suggests that females indeed deferred university, as was the result in the German case (Büttner & Thomsen, 2015). Because such a large increase in work effort translates to an increase of about 39 to 43 percent, in terms of full employment of 40 hours per week. Intuitively, one would expect that increase to be too large for part time occupation alongside university. However, from looking at the means for hours worked and taking into account the highly negative and significant treatment variable. It seems that it is more of a catch up effect blended with changes within the control group.

From the perspective of an increase for the treatment group then the more demanding curriculum in the new system might have inhibited females from working alongside school to the same magnitude as the control group. Then, once enrolled into university with the same time constraints as the older students, they began to work more similar amount of hours. On the flip side of the coin, females in the control group might be returning to education after deferring higher education in favor of working or due to family obligations such as bearing children or getting married. However, only two observations indicate marriage in the whole sample and controlling for the work effort of spouses, should account for some of these familial trends.

Table 8.1 provides further insight into this argument, there is a statistically significant difference in main status for treated females. Meaning, that they are more likely to be primarily working than the control group. Therefore, this difference on the extensive margin could be a key driver in the difference on the intensive margin. Indicating that, females deferred higher education to work full time and perhaps travel, as was the initial expectation. Comparatively, the treatment group could also have been affected by child bearing interfering with initial education plans and forced them sooner onto the labor market than initially planned. Unfortunately, there is no control variable for pregnancy but, as in the case of the control group, these effects might be somewhat captured in the spousal control variable.

Although, the previous argument is valid, it assumes that the assumptions of the model hold. This is not necessarily the case, it is hard to rule out anticipatory effects of the policy completely. Due to more time constraints in the new curriculum, females might have worked less alongside school leaving them at a much lower average hours worked with room for increase once graduated. In the absence of the policy one would expect the pre-treatment averages to be closer in value. However, why these effects are completely nonexistent for males needs further exploration. Although in their case, they are less likely to be working, so even if there are anticipatory effects also present for them, they could be counteracted by them being less likely to work.

To substantiate this channel, Column 3 in Table 7.3 shows the effect for males on the intensive margin, only for working males. It uncovers a similar pattern to the females with a treatment effect of 15.33 hours increase in each week. Therefore, it is reasonable to suspect that the policy changed the dynamic of working with upper secondary school. Supposedly, this is a positive policy effect since the primary reason for dropping out of school, before the implementation, was to work. Around twenty percent of dropouts left school to persue a job, this number is likely underestimated because some expelled students, due to lack of attendance, probably also left at least partly for the same reason (Jónsson et al., 2015).

Table 7.3 Policy effect on the intensive margin only considering working individuals. Heterogeneity by gender.

VARIABLES	**All (1)**	**Females (2)**	**Males (3)**
Treatment	-15.51***	-14.02***	-17.93***
	(3.778)	(3.874)	(4.641)
Post	-3.288	-6.621	1.596
	(3.826)	(4.571)	(4.893)
Policy effect	14.77***	14.20***	15.33***
	(4.902)	(5.006)	(5.704)
Season	0.453	1.375	-0.599
	(0.909)	(1.064)	(1.519)
Location	3.400***	4.439***	2.011
	(0.800)	(1.017)	(1.338)
Degree	2.470***	3.091***	2.258*
	(0.875)	(1.012)	(1.308)
Spouse	0.281	-0.553	-0.0985
	(1.493)	(1.967)	(2.176)
Age		0.401	0.740
		(1.107)	(1.335)
Constant	10.97**	-3.507	4.486
	(5.443)	(22.77)	(27.52)
Observations	320	171	149
R-squared	0.205	0.219	0.130

Notes: Robust standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. Table 7.3 shows the relationship between treatment assignment and hours worked per week using a difference-in-difference method. However, all individuals not working are excluded. The decision to use robust standard errors in this table is because the sample size is a lot smaller and therefore, few individuals within each cluster. As before, age is interacted with gender to account for heterogeneity in gender while letting age vary and detailed interaction effects are reported in the appendix. Source: The International Social Survey Programme.

Bringing back the problem of not being able to disentangle education from being registered into university. It is hard to know whether males lagged behind in finishing upper secondary school on time in the new and more demanding curriculum or that they enrolled increasingly into university. Regardless, the findings indicate that treated males are more likely to be primarily in school and likely work less on the intensive margin while in upper secondary school than previous cohorts.

8. Robustness checks

To further support these claims, it is valuable having an alternative metric to explain the relationship between the treatment assignment and choices on the extensive margin. Moreover, this dependent variable, “Main status”, captures the decision to work as a full time employee, rather than both full time and part time. Most observations are either primarily in work or education, therefore not much information can be gathered from the other options. Overall, Column 1 shows a statistically significant and positive policy effect. The magnitude of the coefficient estimates a 13.6 percent less likelihood of being primarily employed and a 12 percent increased probability of being mainly in education.

Columns 2 and 3 show that these effects vary by gender, reflected in the opposite signs of the policy effects for treated males and females. Interestingly, the policy effect for females is more statistically significant than in Table 7.2 but also of the opposite sign when only considering full time employment. The policy effect coefficient represents a 14.5 percentage point inclination towards full time employment for treated females compared to the control group and a 12.4 percentage diminished likelihood of being primarily in education. These results for females are in line with the trends in the German policies explored by Büttner and Thomsen (2015) and Pischke (2007).

As previously stated, Büttner and Thomsen (2015) could not explore male decision making due to military obligations. Importantly, this is not an issue in this paper and Table 8.1 shows that treated males are more likely to be enrolled into education than working a full-time job. In more detail, the policy effect coefficient estimates that treated males are 52.4 percent less likely to be in full time work and have a 47.4 percent higher probability of being mainly in education.

Table 8.1 Different measure of the policy effect on the extensive margin. Heterogeneity by gender.

VARIABLES	**All** (1)	**Females** (2)	**Males** (3)
Treatment	-0.206	0.246	-0.990*
	(0.316)	(0.291)	(0.589)
Post	-0.0260	0.784***	-1.203
	(0.286)	(0.283)	(0.749)
Policy effect	0.569**	-0.618**	2.308***
	(0.273)	(0.283)	(0.704)
Age	0.0933	0.0897	0.178
	(0.0862)	(0.113)	(0.123)
Sex	0.208		
	(0.210)		
Season	0.0427	-0.0183	0.145
	(0.0866)	(0.129)	(0.171)
Location	-0.181**	-0.202	-0.187**
	(0.0798)	(0.132)	(0.0824)
Degree	-0.205**	-0.150	-0.335
	(0.0961)	(0.132)	(0.302)
Spouse	-0.183*	-0.0167	-0.300***
	(0.0958)	(0.169)	(0.0875)
Observations	487	258	229

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. Table 8.1 shows the relationship between treatment assignment and self-reported main status using a difference-in-difference method. Policy effect for males is still highly significant and a positive value expresses an inclination away from employment. Meanwhile, females show a tendency to be more primarily working. Source: The International Social Survey Programme.

Relying on the results on the extensive margin, they suggest that treated males are in fact less likely to have a part time job than a full time occupation in comparison to the control group, but also less inclined to work full time. As before, having a spouse is correlated with the choice of working. Additionally, living in more rural areas increases labor market participation and for the whole sample there is also a significant inclination towards work with higher level of obtained degrees.

Additional robustness check of this paper involves moving the 1998 cohort from the control to the treatment group in coherence with the argument provided in Chapter 5.3. The estimated effects on the intensive margin are smaller in value and significance but show the same pattern as before. However, the heterogeneous policy effect for males is now statistically significant but much lower in value than for females. Since including the 1998 cohort drags the estimates down in value, it is likely that the trends for this cohort are more similar to the control group than the treatment group. Despite that, obtaining highly siginficant and positve effects with more statistical power is encouraging.

Table 8.2 Policy effect on the intensive margin after moving the cohort born 1998 into the treatment group.

		All		Female	Male
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Treatment	-14.53***	-16.33***		-18.55***	-10.49*
	(0.997)	(2.888)		(1.596)	(5.011)
Post	-2.715	-0.904		-5.034	2.320
	(2.126)	(4.300)		(6.591)	(2.905)
Policy effect	8.672***	8.634	10.36**	12.70*	5.124**
	(2.230)	(4.889)	(4.104)	(6.510)	(1.992)
Season		0.219	0.199	0.345	0.698
		(0.633)	(0.626)	(1.018)	(1.078)
Location		2.052***	2.079***	0.989	2.657***
		(0.473)	(0.476)	(1.035)	(0.457)
Degree		2.862**	2.856*	1.940	3.246
		(1.102)	(1.189)	(1.382)	(2.945)
Spouse		1.836	1.913	0.0840	2.801
		(1.298)	(1.328)	(2.097)	(1.738)
Age				-2.502	-0.509
				(1.339)	(1.554)
Constant	26.43***	10.87*	1.518	65.50*	9.851
	(0.912)	(5.396)	(5.204)	(27.77)	(41.27)
Observations	532	480	480	252	228
R-squared	0.063	0.179	0.145	0.155	0.108

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Table 8.2 shows the relationship between treatment assignment and self-reported hours worked using a difference-in-difference method. The robustness check mainly entails the movement of the 1998 cohort from the control to the treatment group. Column 3 includes cohort specific and year fixed effects. Source: The International Social Survey Programme.

Meanwhile, the effects on the extensive margin disappear completely, both in general but also the highly statistically significant male effect is no longer significant, these estimates are reported in Table 8.3. This result indicates a potential peer effect, that those born 1998 increasingly took part time work alongside upper secondary school. Being in school with younger cohorts that have more time intensive weekdays, might have encouraged them to sacrifice some of their leisure for work. To reiterate, competitive nature might have devalued leisure amongst those graduating simultaneously with a younger cohort.

Table 8.3 Policy effect on the extensive margin after moving the cohort born 1998 into the treatment group.

VARIABLES	(1)	All (2)	(3)	Female (4)	Male (5)
Treatment	-0.483*	-0.563***		-0.950***	-0.309
	(0.255)	(0.202)		(0.195)	(0.558)
Post	-0.260	0.416		0.634	0.113
	(0.178)	(0.310)		(0.858)	(0.506)
Policy effect	0.133	-0.121	0.0583	-0.117	-0.0144
	(0.431)	(0.430)	(0.382)	(0.703)	(1.026)
Age		-0.256***	-0.152**	-0.410***	-0.109
		(0.0929)	(0.0768)	(0.146)	(0.157)
Sex		0.147	0.129		
		(0.275)	(0.200)		
Season		0.0167	0.0486	-0.152*	0.190
		(0.0464)	(0.0903)	(0.0800)	(0.140)
Location		-0.0484	-0.0396	-0.219	0.162
		(0.0683)	(0.0876)	(0.158)	(0.131)
Degree		0.102	0.136	0.0187	0.230
		(0.0828)	(0.0938)	(0.123)	(0.263)
Spouse		0.346***	0.329**	0.205	0.481**
		(0.129)	(0.150)	(0.287)	(0.191)
Constant	1.176***	5.295**		10.11***	0.882
	(0.00519)	(2.167)		(2.715)	(3.704)
Observations	558	499	499	267	232

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. Table 8.3 shows the relationship between treatment assignment and the decision to work using a difference-in-difference method. The robustness check mainly entails the movement of the 1998 cohort from the control to the treatment group. Column 3 includes cohort specific and year fixed effects. Source: The International Social Survey Programme.

9. Conclusion and limitations

In this paper, the relationship between years of schooling and labor market participation was explored exploiting a quasi-natural experiment that resulted from an education reform in Iceland. The estimation uncovered a higly significant policy effect on the intensive margin. The estimates ranged from ten to seventeen hours per week. Moreover, the policy effect was driven by treated females, a rise of thirteen to seventeen hours per week. Even though, this effect was not significant for the males, there was a similar trend when only working males were analysed. Pischke (2007) found no significant long term labor market effects of school shortening but this paper establishes evidence on significant short run effects.

Meanwhile, the policy also altered behavior on the extensive margin. The policy effect estimates an eighteen to twenty three percent diminished likelihood of being employed for the treatment group. Interestingly, treated females show a 14.2 percentage point higher inclination towards employment, at the ten percent significance level. Meanwhile, treated males are 3.23 times less likely to have a job compared to males in the control group. This estimate is robust to controls and highly significant.

In the same vein, estimating the likelihood of full time work versus being primarily in education revealed the same trend. Overall, the treatment group is 13.6 percent less prone to work full time and 12 percent more likely to be in education. In line with the estimates on the extensive margin for work, part or full time, treated females are 14.5 percent more likely to work full time and have a 12.4 percent diminished likelihood of being primarily in education. Contrary to the other measure, these estimates are highly significant. Meanwhile, treated males were 51 percent less inclined to be in full time occupation and exhibit a 46 percent higher likelihood of being mainly in education.

Therefore, I provide evidence in accordance with Büttner and Thomsen (2015) and Pischke (2007) on females deferring education in favor of working. In contrast, I find that males are more likely to stay in education which the former paper was unable to explore due to military obligations for males in Germany. Thus, this result for males is unexpected and indicates a successful aspect of the policy. Especially given the fact that work was the primary reason for dropping out of school (Jónsson et al., 2015). This evidence suggests that males withdrew from the labor market and either enrolled on a larger scale into university or kept going in upper secondary school, instead of dropping out. Undoubtedly, both cases can be true at once.

Despite that, these results have to be taken with a grain of salt. The presence of anticipation effects both in terms of some schools offering the three-year program prematurely and allowing students some autonomy over their pace in completing school. Furthermore, given the nature of the variables and age of the individuals in the sample, it is plausible that the policy altered behavior in the pre-treatment period, especially on the intensive margin. Support for this critique is mostly evident in the lower average hours worked in a week in the treatment group. Leaving a large gap between the groups that then converges once the pressure of the more time intensive curriculum is gone.

Additionally, the common trends assumption, essential to the difference-in-difference approach applied, is hard to prove due to lack of pre-treatment data. It is more convincing on the extensive margin and getting a larger and more detailed dataset, from 2016 and 2019 especially, could address these limitations. Exploring further the policy effects on males in terms of staying in school is warranted and could provide further evidence of a positive impact, both questioned by Asgeirsdóttir et al. (2022) and negative effects implied by the National Student Association ("Vilja Mat á Styttingu Framhaldsskólanna," 2023). Therein, they mention the need for research into student mental health and other negative non-pecuniary effects of the policy they expect to exist, which are questions still unanswered and left to future research.

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Appendix

Table i. This table shows the detailed interaction effects between variables age and sex left out of Table 6.1 in the main text.

VARIABLES	(1)	(2)	(3)	(4)
Treatment	-11.35**	-12.98**	-13.79**	
	(4.014)	(4.237)	(4.657)	
Post	-0.218	-1.461	-3.732	
	(2.935)	(2.238)	(4.083)	
Policy effect	6.714*	10.38**	13.40**	16.80***
	(3.095)	(4.124)	(5.081)	(4.012)
19.AGE		-4.662	-6.066	-7.952
		(5.397)	(5.522)	(5.326)
20.AGE		2.770	0.892	-0.0386
		(5.003)	(5.515)	(6.782)
21.AGE		-6.805	-8.830*	-16.32**
		(4.683)	(4.170)	(5.559)
22.AGE		11.70**	9.565	6.439
		(3.908)	(6.737)	(3.888)
23.AGE		3.638	5.895	-0.251
		(7.696)	(8.605)	(6.990)
24.AGE		-1.945	-9.198*	-20.44***
		(4.228)	(4.723)	(4.166)
25.AGE		-7.257	-3.783	-16.02**
		(6.381)	(4.227)	(4.515)
2.SEX		-0.661	-0.596	-0.961
		(6.780)	(6.340)	(6.137)
18b.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
18b.AGE#2o.SEX		0	0	0
		(0)	(0)	(0)
19o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
19.AGE#2.SEX		3.113	3.453	4.651
		(7.419)	(7.121)	(6.837)
20o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
20.AGE#2.SEX		-3.444	-4.485	-6.176
		(7.021)	(5.834)	(6.081)
21o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
21.AGE#2.SEX		6.512	5.577	6.313
		(6.956)	(6.106)	(5.968)
22o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
22.AGE#2.SEX		-22.41**	-19.79**	-21.64**
		(7.178)	(7.273)	(7.441)
23o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
23.AGE#2.SEX		-1.874	-3.288	-2.868
		(6.635)	(8.049)	(7.959)
24o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)

24.AGE#2.SEX		2.321	-2.175	-1.376
		(5.987)	(6.399)	(6.322)
25o.AGE#1b.SEX		0	0	0
		(0)	(0)	(0)
25.AGE#2.SEX		-12.67	-14.75*	-14.14*
		(6.778)	(6.254)	(6.009)
Month of interview		0.0885		
		(0.377)		
Location		1.717**	2.232***	2.206***
		(0.690)	(0.459)	(0.449)
Years in education		-0.0179		
		(0.870)		
Spouse		1.488	1.999	1.927
		(1.100)	(1.246)	(1.321)
Season			0.767	0.353
			(0.654)	(0.656)
Degree			3.031**	2.956*
			(1.153)	(1.210)
Constant	22.19***	18.87	3.726	0.352
	(4.014)	(10.06)	(6.045)	(4.802)
Observations	532	509	480	480
R-squared	0.021	0.092	0.136	0.145
Fixed effects				x

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. This table is homogenous to Table 6.1 in the main text except for the interaction effects of Age and gender. See additional details in Table 6.1 in the main text.

Table ii. More detailed version of Table 7.3 in the main text.

VARIABLES	**(1)**	**(2)**	**(3)**
Treatment	-15.51***	-14.02***	-17.93***
	(3.778)	(3.874)	(4.641)
Post	-3.288	-6.621	1.596
	(3.826)	(4.571)	(4.893)
Policy effect	14.77***	14.20***	15.33***
	(4.902)	(5.006)	(5.704)
18b.AGE#2.SEX	-1.339		
	(3.681)		
19.AGE#1b.SEX	-1.718		
	(4.032)		
19.AGE#2.SEX	-0.596		
	(3.765)		
20.AGE#1b.SEX	6.014		
	(4.247)		
20.AGE#2.SEX	1.530		
	(4.624)		
21.AGE#1b.SEX	6.151		
	(6.655)		
21.AGE#2.SEX	0.752		
	(4.529)		
22.AGE#1b.SEX	9.912*		
	(5.602)		

22.AGE#2.SEX	-6.627		
	(4.818)		
23.AGE#1b.SEX	8.535		
	(6.237)		
23.AGE#2.SEX	-1.643		
	(5.701)		
24.AGE#1b.SEX	-2.205		
	(8.306)		
24.AGE#2.SEX	3.133		
	(6.990)		
25.AGE#1b.SEX	19.06***		
	(5.386)		
Season	0.453	1.375	-0.599
	(0.909)	(1.064)	(1.519)
Location	3.400***	4.439***	2.011
	(0.800)	(1.017)	(1.338)
Degree	2.470***	3.091***	2.258*
	(0.875)	(1.012)	(1.308)
Spouse	0.281	-0.553	-0.0985
	(1.493)	(1.967)	(2.176)
Age		0.401	0.740
		(1.107)	(1.335)
Constant	10.97**	-3.507	4.486
	(5.443)	(22.77)	(27.52)
Observations	320	171	149
R-squared	0.205	0.219	0.130

Notes: Robust standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. See additional details in Table 7.3 in the main text.

Table iii. Reports additional coefficients omitted from Table 8.1 in the main text.

VARIABLES	**All (1)**	**Females (2)**	**Males (3)**
Treatment	-0.206	0.246	-0.990*
	(0.316)	(0.291)	(0.589)
Post	-0.0260	0.784***	-1.203
	(0.286)	(0.283)	(0.749)
Policy effect	0.569**	-0.618**	2.308***
	(0.273)	(0.283)	(0.704)
Age	0.0933	0.0897	0.178
	(0.0862)	(0.113)	(0.123)
Sex	0.208		
	(0.210)		
Season	0.0427	-0.0183	0.145
	(0.0866)	(0.129)	(0.171)
Location	-0.181**	-0.202	-0.187**
	(0.0798)	(0.132)	(0.0824)
Degree	-0.205**	-0.150	-0.335
	(0.0961)	(0.132)	(0.302)
Spouse	-0.183*	-0.0167	-0.300***
	(0.0958)	(0.169)	(0.0875)
/cut1	1.154	1.092	1.792
	(1.890)	(1.973)	(2.025)
/cut2	1.380	1.276	2.086

	(1.908)	(1.972)	(2.017)
/cut3	4.840***	4.525**	6.095***
	(1.671)	(1.794)	(1.710)
/cut4	5.166***	4.823***	6.506***
	(1.561)	(1.701)	(1.553)
/cut5	6.158***	5.529***	
	(1.599)	(1.887)	
Observations	487	258	229

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. See additional details in Table 8.1 in the main text.

Table iv. Showing interaction effects omitted in Table 8.2 in the main text.

VARIABLES	(1)	All (2)	(3)	Female (4)	Male (5)
Treatment	-14.53***	-16.33***		-18.55***	-10.49*
	(0.997)	(2.888)		(1.596)	(5.011)
Post	-2.715	-0.904		-5.034	2.320
	(2.126)	(4.300)		(6.591)	(2.905)
Policy effect	8.672***	8.634	10.36**	12.70*	5.124**
	(2.230)	(4.889)	(4.104)	(6.510)	(1.992)
18b.AGE#2.SEX		-0.519	-1.085		
		(6.344)	(6.309)		
19.AGE#1b.SEX		-2.797	-4.464		
		(4.859)	(4.580)		
19.AGE#2.SEX		0.942	-0.566		
		(4.539)	(4.320)		
20.AGE#1b.SEX		-0.663	-0.465		
		(5.404)	(7.096)		
20.AGE#2.SEX		-6.350	-7.448		
		(3.337)	(3.947)		
21.AGE#1b.SEX		-12.14*	-14.92*		
		(6.245)	(7.032)		
21.AGE#2.SEX		-6.949	-9.621		
		(6.635)	(7.872)		
22.AGE#1b.SEX		7.959	4.453		
		(5.311)	(5.149)		
22.AGE#2.SEX		-12.87*	-16.69*		
		(6.527)	(7.590)		
23.AGE#1b.SEX		0.825	-1.293		
		(9.503)	(8.638)		
23.AGE#2.SEX		-3.111	-5.300		
		(7.209)	(6.465)		
24.AGE#1b.SEX		-16.45**	-19.65***		
		(6.170)	(4.956)		
24.AGE#2.SEX		-19.09*	-22.01**		
		(8.702)	(8.782)		
25.AGE#1b.SEX		-12.24	-15.59**		
		(6.433)	(5.240)		
25.AGE#2.SEX		-27.17***	-30.50***		
		(6.445)	(5.078)		
Season		0.219	0.199	0.345	0.698
		(0.633)	(0.626)	(1.018)	(1.078)
Location		2.052***	2.079***	0.989	2.657***

		(0.473)	(0.476)	(1.035)	(0.457)
Degree		2.862**	2.856*	1.940	3.246
		(1.102)	(1.189)	(1.382)	(2.945)
Spouse		1.836	1.913	0.0840	2.801
		(1.298)	(1.328)	(2.097)	(1.738)
Age				-2.502	-0.509
				(1.339)	(1.554)
Constant	26.43***	10.87*	1.518	65.50*	9.851
	(0.912)	(5.396)	(5.204)	(27.77)	(41.27)
Observations	532	480	480	252	228
R-squared	0.063	0.179	0.145	0.155	0.108

Notes: Clustered standard errors are presented in parentheses. Significance levels are expressed as follows *** p<0.01, ** p<0.05, * p<0.1. See additional details in Table 8.2 in the main text.