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Comprehensive School Reform and Labor Market Outcomes over the Lifecycle: Evidence from Finland

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- Results are similar for both genders.

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Comprehensive School Reform and Labor Market Outcomes over the Lifecycle: Evidence from Finland

Jani-Petteri Ollikainen

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Abstract

This study focused on the labor market effects of the Finnish comprehensive school reform in 1972–1977 over the lifecycle. The reform increased the age at which students are separated into vocational and general tracks from 11 to 16 as well as updated the curriculum to include more general content instead of vocational. Using longitudinal administrative register data and exploiting the gradual implementation of the reform, I found a negative effect on employment in early career and a positive effect later in the lifecycle. Results for labor earnings are more nuanced and sensitive to different model specifications, but a positive effect was found in the late career. After the reform, the treated were more likely to choose academic secondary education which could mediate the effects on employment. The results were similar for men and women.

Keywords: comprehensive school, vocational education, employment, earnings, lifecycle

JEL Classifications: I21, I28, J24

1. Introduction

Post World War II, many European countries implemented comprehensive school reforms to increase the number of years of compulsory education, introduce national curricula, and/or abolish or delay tracking. Previous studies have found that comprehensive school reforms increase educational attainment and earnings, on average, as well as intergenerational mobility (Meghir and Palme, 2005; Pekkarinen et al., 2009; Aakvik et al., 2010). Little is known about how these reforms affect

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labor market outcomes over the lifecycle.

The main contribution of this paper is to provide one of the first estimates—to the best of my knowledge—of the effects of a comprehensive school reform on lifecycle employment and earnings patterns by studying the Finnish comprehensive school reform of 1972–1977 [1]. The reform increased the age at which students are separated into general and vocational tracks from 11 to 16. The reform also updated the curriculum to include more general content instead of vocational, keeping the number of years of compulsory schooling constant. An attractive feature of the reform is that it was gradually implemented across municipalities. The implementation scheme gave rise to a natural experiment utilized in this paper to compare outcomes across birth cohorts and municipalities using a difference-in-differences approach. The implementation scheme, together with extensive administrative register data on the labor market and educational outcomes, means that the causal effects of the reform may be studied through a nearly 30-year period.

Although studies on the reform have been conducted earlier, its effects on labor market outcomes over the lifecycle remain unknown. Previous studies show that the reform did have an impact on several outcomes. First, the reform increased women's probability of enrolling in academic secondary and tertiary education and decreased the gender wage gap (Pekkarinen, 2008). Second, the reform decreased the intergenerational income elasticity by 23% (Pekkarinen et al., 2009). It also had a slight positive impact on verbal test scores measured by the Finnish Army Basic Skills Test. Among students whose parents had less than a high school education, arithmetic and logical reasoning test scores along with verbal test scores saw an increase as a result of the reform (Pekkala Kerr et al., 2013).

The reform effectively reduced the weight of vocational education in the Finnish school system by increasing the age of tracking into vocational and academic tracks and reducing the amount of vocational education in the curriculum of compulsory schooling. Thus, the present paper's hypothesis of the effects of the reform across the lifecycle is based on the literature on the labor market effects of vocational and general education. First, it has been argued that vocational education offers

[1] Bhuller et al. (2017) use the Norwegian compulsory schooling law reform as an instrument for education to estimate the effect of schooling on lifecycle earnings. They find that additional schooling results in higher lifetime earnings and a steeper age-earnings pattern. Unlike the Norwegian reform which increased minimum compulsory schooling from 7 to 9 years, the Finnish reform did not affect the minimum years of compulsory schooling, making the mechanisms of the two reforms distinct. Thus, this paper does not attempt to estimate the effects of additional schooling but rather focuses on reporting the effects of the reform over the lifecycle.

youths an easier transition from school to work and, thus, may be an effective policy in combating youth unemployment (Ryan, 2001; Wolter and Ryan, 2011; Zimmermann et al., 2013). However, due to technological advancements and structural changes in labor markets, the skills provided by vocational education may become obsolete faster than those provided by general education. Due to this skill depreciation, vocational education may lead to relatively high unemployment rates in later career stages. Krueger and Kumar (2004) claim that the slower adaptation of technology caused by skill-specific education may even help explain the difference in the growth rates of Europe and the US.

The existing empirical literature on the labor market effects of vocational and general education mostly supports a trade-off between early and late labor market advantages. The most notable study in this literature is Hanushek et al. (2017). Using microdata from the International Adult Literacy Survey (IALS), it was found that vocational education indeed leads to better school-to-work transition, but it also becomes a disadvantage in terms of employment as early as the age of 50. Further, they found that the magnitude of the effect depends on the intensity of the treatment i.e., the degree of vocationalization of the country's education system. For example, in Germany, where vocational education is strongly based on apprenticeship programs, the trade-off between early and late labor market advantages is starker than in countries such as Finland, which have school-based vocational programs.

Hampf and Woessmann (2017) and Forster et al. (2016) identified similar effects as Hanushek et al. (2017) using the more recent and richer PIAAC survey. Cörvers et al. (2011) estimated age-earnings patterns consistent with a trade-off between early and late labor market advantages for Germany, the Netherlands, and the United Kingdom. In addition, there are also recent country-specific studies with similar results, such as Brunello and Rocco (2017) for Great Britain, Weber (2014) for Switzerland, and Golsteyn and Stenberg (2017) and Stenberg and Westerlund (2015) for Sweden. A contrasting result from Sweden is by Hall (2016), who found no significant effect on risk of unemployment from a pilot scheme of a reform from 1988 to 1993, which lengthened upper secondary vocational education by an additional year of general education. Silliman and Virtanen (2019) exploited a regression discontinuity design created by a centralized admission system in Finland to show that upper secondary vocational education increases annual income by 7 percent. They found no evidence of the effects diminishing with time. However, their data allows the individuals to be followed only until the age of 31, making it impossible to estimate the effects in

late career stages.

The present paper also makes a secondary contribution to this literature by examining the causal effects of reducing the vocationalization of a school system over the lifecycle. Many previous papers comparing the lifecycle effects of vocational and general education either don't establish causality or base it on rather strong assumptions. For example, as a result of using cross-sectional data, the identification of causal effects in Hanushek et al. (2017) was based on the assumption that the selection of the type of education does not vary over time. Another possible source of bias in their model was that the test scores from IALS used as control variables could have very well been affected by the person's education before taking the test (treatment), resulting in a "bad control" situation (see Angrist and Pischke (2009)).

The results of this paper support the hypothesis of a trade-off between employment early and late in the lifecycle. The results show that the reform harmed employment in the early career but that this gap reduced with age and finally turned into a positive effect from the mid-30s onward. Results for labor earnings are more imprecise and sensitive to different model specifications but a positive effect was found from late 30s onward. Changes in educational outcomes are a likely mediator of the effects of the reform. An analysis of the reform's effect on completed degrees at age 30 showed that the reform increases educational attainment and the probability of choosing the academic track instead of the vocational track. Further, the results related to the labor market and educational outcomes are similar for men and women.

2. The Finnish Comprehensive School Reform of 1972–1977

Before the comprehensive school reform, there was a two-track school system in place in Finland. A comparison of the Finnish school systems before and after the reform is depicted in Figure 1. Under the old two-track system, students attended primary school (*kansakoulu*) for four years starting from age 7, after which they chose between applying to a more academic general secondary school (*oppikoulu*) and continuing primary school. Those who were accepted into the five-year general secondary school often continued into a three-year upper secondary school ending in a matriculation examination, the completion of which made them eligible for university.

Students who did not attend general secondary school spent two more years in primary school. After this, they attended practically oriented continuation classes at the primary school level for two

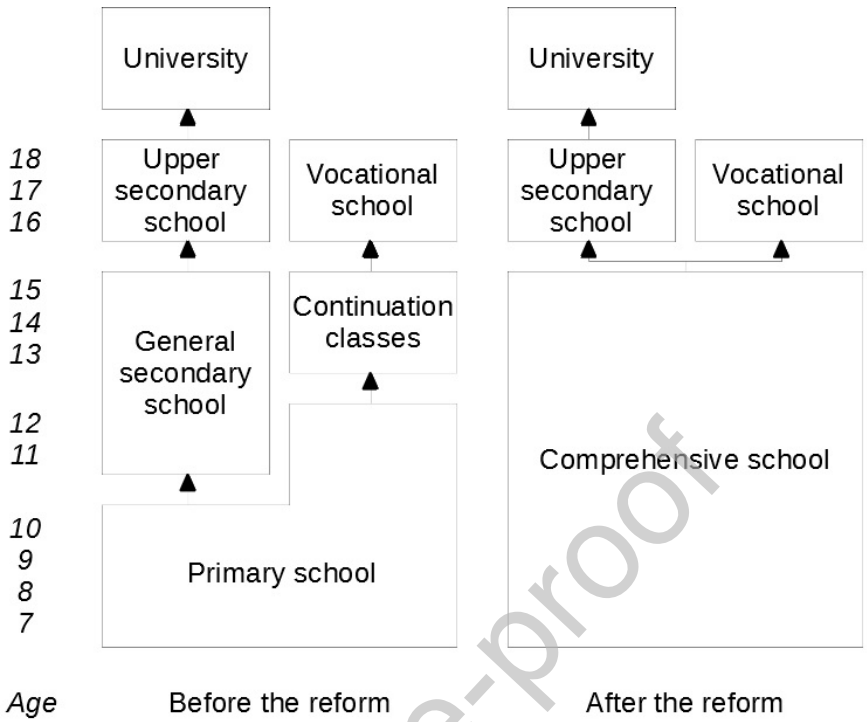

Figure 1: Finnish education system before and after the reform. Source: Pekkarinen (2008)

to three more years. Upon graduating from primary school, they had the possibility of continuing their education in vocational schools. Unlike the general secondary schools, the continuation classes did not make them eligible for upper secondary school or universities. The continuation classes consisted mostly of vocational education. It was required by law for the curriculum to contain subjects and practical exercises relevant to the local industries. For example, in rural areas, the continuation classes could involve agriculture or forestry.

Under the reformed educational system, all students attended the same comprehensive school until the age of 16, after which they could apply to either a vocational or an upper secondary school. The curriculum in comprehensive schools was close to that of the old general secondary schools, which exposed the students, who would have stayed in primary school without the reform, to a more academic curriculum (see Table 1). Compared to primary school, comprehensive school included a greater level of mathematics and science and two compulsory foreign languages. Although the structure of the comprehensive school was similar for all students, ability grouping was not abolished

Table 1: Examples of 7th grade curriculum in primary school, general secondary school, and comprehensive school. Table adapted from Kettunen et al. (2012).

Subject	Continuation classes of primary school (Helsinki 1932)	General secondary school (1941 curriculum)	Comprehensive school (1970 curriculum)
Religion, history of religions, and ethics	1	2	1
Finnish	4	4	3
Civics	3		1
History, social studies, and economics		3	2
Swedish		3	
First foreign language			3
Second foreign language		5	2
Biology	2		
Natural history and geographics		4	2
Physics and chemistry			2
Health education	1		
Singing/Music	2*	2*	1**
Gymnastics and sports/Physical education	2	4	2
Drawing, modelling, and cursive/Art		2	1**
Craft and techical drawing/Craft	14	2	2
Home economics	9*		1
Accounting	2		
Vocational counseling/Student counseling	1		1
Optional subjects			4-6

* Only for girls

** Optional with Music/Art

until the mid-1980s. For example, mathematics and foreign languages had three different levels in grades 7 to 9 (Sahlberg, 2014).

The reform did not increase the minimum school leaving age, which was already 16 before the reform, nor did it increase the length of compulsory schooling (Pekkala Kerr et al., 2013). Thus, the effects of the reform arise from the changes in the tracking age and curriculum. The change in tracking age also caused the selection criteria into the vocational and academic tracks to differ from before the reform, which is also likely to imply changes in peer groups. Unfortunately, as is the case with many real-life reforms, the relative importance of the different factors could not be determined since they tend to change simultaneously.

The reform was implemented gradually across the country (see Figure 2). Beginning in 1972,

the first municipalities to adopt the reform were mainly from Lapland in the north of the country. From there, the reform moved towards the south with the capital region being last to adopt it in 1977. Students who were entering first to fifth grades (ages 7 to 11) by the end of the year in which the reform came into effect in their municipality were affected by it. This gradual implementation is what creates the natural experiment that was exploited in this study.

3. Data and Empirical Strategy

3.1. Data

The complete population register data from Statistics Finland was used for this study. The pertinent information was collected from the Longitudinal Census Files, Longitudinal Employment Statistics Files, and Statistics Finlands Register of Completed Education and Degrees. First, using birth dates, individuals born between 1960 and 1966 were selected for the study. Then, using an individual specific ID code, each individual's municipalities of residence for the years 1972–1977 were gathered. With these two variables and the information on the year in which the reform was adopted in each municipality, I coded an indicator variable for whether or not the individual was exposed to the reform. Further, the number of years that they spent in comprehensive school was also determined. This information is summarised in Table 2.

The treatment status was then linked to labor market and educational outcomes. Data on the employment status of the individuals at the end of the year are available on an annual frequency from 1987 to 2015. Annual labor earnings are available from 1987 to 2016. Annual labor earnings include wages and income from self-employment, which deflated to 2015 euros. Considering the birth cohorts chosen in the data as well, data on employment is available from a minimum age of 21 to a maximum age 55 and data on earnings from a minimum of 21 to a maximum of 56. Finally, data on all completed degrees are available from 1970 to 2015.

Observations had to be excluded for various reasons. First, those who had deceased before the year 1987 were dropped. Second, people whose information on municipality of residence was missing for any of the years between 1972 and 1977 were dropped. Also, those who moved between municipalities that adopted the reform in different years than the period of 1972–1977 were dropped from the data because the treatment for these individuals could not be unambiguously determined.

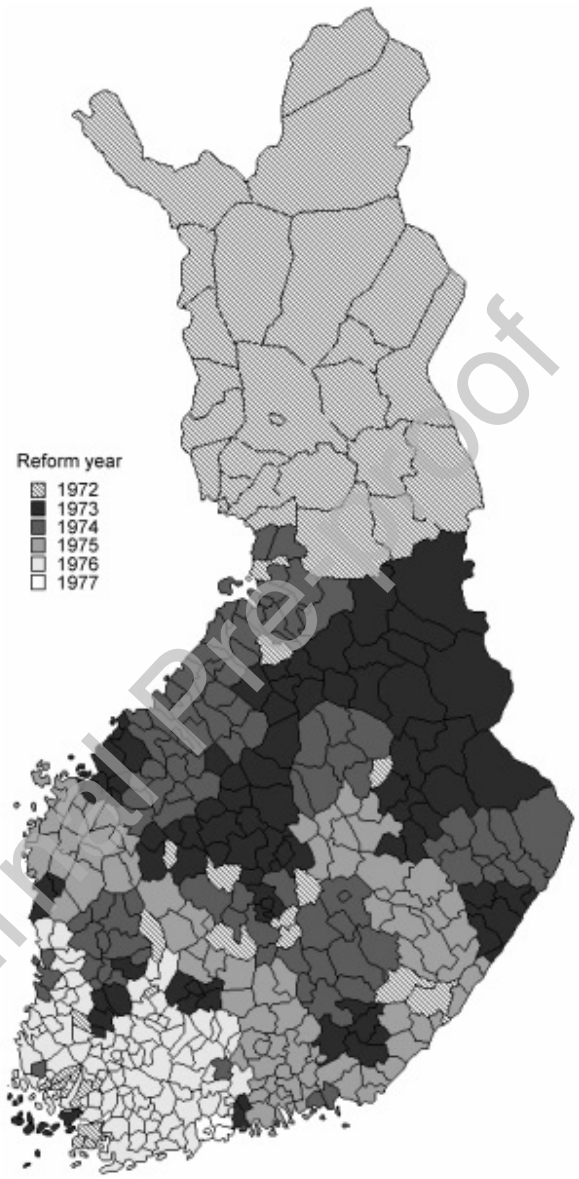

Figure 2: Geographical representation of the implementation of the reform. Source: Pekkarinen (2008)

Table 2: Implementation of the reform, number of observations and years spent in comprehensive school

	Reform year						
Birth cohort	1972	1973	1974	1975	1976	1977	Total
1960	7113	10482	15351	15110	15337	8829	72222
1961	7312 5 years	10353	15362	15034	15393	8821	72275
1962	7091 6 years	10228 5 years	14921	15048	15432	9148	71868
1963	6879 7 years	10009 6 years	14720 5 years	15137	15653	9345	71743
1964	6531 8 years	9607 7 years	14143 6 years	14751 5 years	15527	9591	70150
1965	6068 9 years	9021 8 years	13631 7 years	14425 6 years	14781 5 years	9532	67458
1966	5870 9 years	8717 9 years	13312 8 years	13935 7 years	14925 6 years	9522 5 years	66281
Total	46864	68417	101440	103440	107048	64788	491997

Note: The shaded cells adopted the comprehensive school reform. The first line in a cell reports the number of observations, while the second line in the shaded cells reports the number of years that each cell was exposed to the comprehensive system.

Finally, people living in the autonomous land Islands were dropped. Altogether, these exclusions reduced the sample size by ∼16% from the whole population of individuals born between 1960 and 1966. From the remaining sample, 45.8% belong to the treatment group. The sample contains observations from 496 municipalities. The median number of observations per municipality-birth cohort combination is 247.

An examination of employment and earning patterns over age shows differences in labor market outcomes between those affected and not affected by the reform (see Figures B1 and B2 in Appendix B). The employment rates of the non-treated group were higher than those treated in the earlier years; however, this difference vanishes with age. For earnings, the pattern is not as clear. However, a simple analysis such as this fails to account for the differences between municipalities and birth cohorts which may distort the results, as argued in Section 2. The figures also capture period-specific effects such as depression in the early 1990s in Finland. Thus, a more sophisticated approach should be adopted.

3.2. Empirical Strategy

Following Hanushek et al. (2017), the following difference-in-differences model was estimated to test out the hypothesis of a trade-off between early- and late-career employment. To this end, age-profiles of the outcome variable are allowed to linearly differ between the treated and non-treated[2]:

$$y_{icmt} = \alpha_0 + \alpha_1 Age_{it} + \alpha_2 Age_{it}^2 + \beta_1 Reform_{icm} + \beta_2 Reform_{icm} \times Age_{it} + \gamma_0 D_c + \delta_0 D_m + (\gamma_1 D_c + \delta_1 D_m) Age_{it} + \epsilon_{icmt}, \quad (1)$$

where y_{icmt} is the relevant outcome for individual i belonging to birth cohort c and living in municipality m observed in year t, and $Reform$ is an indicator for having attended comprehensive school. D_c and D_m are dummies for each cohort and municipality, respectively. Cohort- and municipality-specific linear age trends were also added to the regressions[3]. Age was added to the equation as both a linear and squared term to capture the typical age-profile of employment and income. As seen in Figure B1, it was found that the second-degree polynomial may not fit the employment data well until after age 30; therefore, this assumption was later relaxed as a robustness check.

As evident from Figure 2, the implementation of the reform varied geographically. Since the northern and eastern parts of Finland have lower income per capita and level of education than the southern and western parts, the indicator variable for the reform may be endogenous in the absence of the municipality fixed effects and time trends. Further, similar differences may exist among the birth cohorts, which is why fixed effects and time trends were also added for each cohort. Year dummies were omitted from the equation to avoid perfect multicollinearity. With a fixed age, cohort effects effectively control for year effects, but, as a result, they are indistinguishable from each other in the model.[4] However, these estimates are not of importance since the main focus of this paper

[2]In addition to being easier to interpret, a linear model fits the data better than higher polynomial models based on AIC and BIC. In the case of employment, the estimates obtained from the higher polynomial models are qualitatively similar to those of the linear model. Estimated effects on earnings are more sensitive to model specification. In the robustness checks below, estimates from separate regressions for each year of age are reported to further address concerns related to model specification.

[3]Alternatively, one could use dummies and trends for each implementation region instead of municipalities since the variation in reform status within birth cohorts occurs at both levels. Using municipalities instead of the implementation region in the model specification does not affect the estimates but does produce smaller standard errors.

[4]This situation is also known as the age-period-cohort problem. See for example Heckman and Robb (1985).

lies in the effects of the reform.

The parameters of interest in Equation 1 are β_1 and β_2. Together, these parameters describe the lifecycle effects of the reform. β_1 measures the initial impact of the reform on the outcome of interest (normalized to age 21 in this paper). β_2 captures the effect of the reform with each additional year of age. The identification of the causal effect of the reform is based on the usual common trends assumption. In this case, the assumption states that the timing of the reform across regions is not systematically related to other factors that affect labor market outcomes over the lifecycle. Thus, in the absence of the reform, differences in the age profiles of the outcome would have remained constant among the municipalities that adopted the reform at different times. To test the validity of the assumption, Figure B3 plots the average employment and annual labor earnings in the different implementation regions using data on the pre-reform 1960 birth cohort over the lifecycle. A visual inspection suggests that the age profiles of both the employment and annual labor earnings were similar between the regions prior to the reform, which supports the validity of the common trends assumption.

All the models were separately estimated by gender. This approach was adopted because earlier papers on the reform indicate heterogeneous effects for men and women. All the models were estimated by OLS, which means that linear probability models were used for binary outcome variables. A linear probability model was used to simplify the results since the marginal effects for linear probability models corresponded to the estimates[5].

4. Results

4.1. Main results

Table 3 reports regression estimates of Equation 1 with employment status as the dependent variable. Columns 1 and 2 present the estimates from a model including municipality and cohort fixed effects for men and women separately. The results show that the reform had a negative initial effect on employment (normalized to age 21). The estimated effect is -4.8 percentage points for men and -6.1 percentage points for women. However, the reform's interaction with age was found to be

[5]For robustness, the models were also estimated using a probit model. The marginal effects from probit models were similar to those from the linear probability models

Table 3: Effects of the reform on employment over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Reform	-0.048***	-0.061***	-0.002	-0.004
	(0.002)	(0.004)	(0.002)	(0.003)
Reform × (Age/10)	0.028***	0.036***	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.001)
Observations	6,931,132	6,691,826	6,931,132	6,691,826
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Age in the sample ranges from 21 to 55. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

positive, meaning that the employment gap between the treated and non-treated cohorts narrows by 2.8 percentage points for men and 3.6 percentage points for women with every decade. These estimates imply that, in terms of employment, attending comprehensive school goes from being a disadvantage to an advantage at approximately age 38 for both men and women. Columns 3 and 4 include municipality and cohort-specific linear age trends with the model. After the addition of these trends, the effects of the reform can be seen to practically vanish. Although the estimates still show a negative initial impact and positive linear interaction with age, the estimates are close to zero and statistically insignificant.

As argued by Wolfers (2006), a model using a single dummy to capture the full effect of the reform may be misspecified if the actual treatment effect is not constant. This misspecification might result in the model being unable to separate the effect of the reform from the effects of the cohort and municipality. Since the students were exposed to the reform for varying numbers of years depending on which grade they were in when the reform was adopted in their municipality, the effects of the reform are likely heterogeneous with respect to treatment intensity.

To circumvent this issue, a richer variation in the treatment intensity was exploited instead of assuming a constant treatment effect. To this end, the treatment effects were allowed to linearly depend on the number of years spent in comprehensive school.[6] The variable *Intensity* in Table

[6]Linearity was assumed to simplify the results. Using dummies for the number of years spent in comprehensive school produced similar estimates and particularly showed that treatment effects grow with treatment intensity, so the assumption of linearity here was not too restricting.

Table 4: Effects of the reform on employment by treatment intensity over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Intensity	-0.016***	-0.023***	-0.005***	-0.008***
	(0.001)	(0.002)	(0.001)	(0.002)
Intensity × (Age/10)	0.011***	0.014***	0.003***	0.005***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	6,931,132	6,691,826	6,931,132	6,691,826
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Employed (=1, otherwise 0). Employment status is measured at the end of the year. The age range of the sample is from 21 to 55. The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4 takes on the integer values from 0 (not treated) to 5 (spent the full nine years in comprehensive school). The estimates in Table 4 are based on an OLS estimation of Equation 1, with the only difference being that the reform indicator is replaced by a measure of treatment intensity. Finally, using treatment intensity instead of a single indicator requires no further identifying assumptions, and the discussion on the identification strategy in Section 3.2 still applies.

Table 4 reports the reform's effect on employment by the intensity of treatment. Columns 1 and 2 show the estimations of the model without the municipality- and cohort-specific trends, separated by gender. Without the trends, the obtained estimates are similar to those in Table 3. Specifically, they show that treatment intensity has a negative initial impact and then a positive interaction with age. The estimates indicate that the initial impact of the reform on employment ranges from -1.6 to -8 percentage points for men and from -2.3 to -11.5 percentage points for women, depending on treatment intensity. The interaction terms show that the gap in employment narrows every ten years by 1.1 to 5.5 percentage points for men and 1.4 to 7 percentage points for women. Consequently, the crossover age is around 36 for men and 37 for women.

In Columns 3 and 4, the municipality and cohort-specific trends were added to the model. Adding the trends had an attenuating impact on the estimates but not to the degree as in Table 3. The estimates show that the reform had an initial effect on employment of -0.5 to -2.5 percentage points for men and -0.8 to -4 percentage points for women, depending on the number of years they spent in comprehensive school. With every ten years, the probability of employment increases by

Table 5: Effects of the reform on annual labor earnings over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Reform	-399.06	-1279.49***	95.39	-37.48
	(416.61)	(251.71)	(148.67)	(79.51)
Reform × (Age/10)	227.95***	719.24***	-105.08	-14.88
	(223.95)	(104.20)	(110.86)	(69.18)
Observations	7,137,671	6,891,804	7,137,671	6,891,804
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Age in the sample ranges from 21 to 56. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

0.3–1.5 percentage points for men and 0.5–2.5 percentage points for women. With this specification, the crossover in the employment probability occurs approximately at age 37 for men and age 36 for women.

Tables 5 and 6 report estimates of the reform's effect on annual labor earnings. For women, the estimates from models without trends show a trade-off between early and late-career earnings. Column 2 in Table 5 shows that initially, the reform decreased annual earnings by around 1300 euros, but every ten years the annual earnings increased by around 700 euros. A similar effect is reported in Column 2 of Table 6 using treatment intensity as the explanatory variable. However, these estimates were not robust to adding the municipality- and cohort-specific trends. For men, the estimates in Tables 5 and 6 are even more sensitive to the model specification. The initial effect of the reform was found to be positive and the interaction with age to be negative in most models. However, none of these results are statistically significant.

4.2. Robustness checks

A possible concern related to the main results is that they may be sensitive to the model specification which was especially seen in the earnings results. Thus, to allow for a more flexible model, the reform's effect on employment and earnings were separately estimated for each year of age. The regressions included either the reform indicator or the measure of treatment intensity and fixed effects for the birth cohorts and municipalities. The estimates from these regressions are presented

Table 6: Effects of the reform on annual labor earnings by treatment intensity over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Intensity	235.16	-267.22***	315.63***	-40.18
	(197.08)	(92.42)	(112.50)	(39.79)
Intensity × (Age/10)	-62.45***	251.78***	-87.00	65.10**
	(85.64)	(34.76)	(65.78)	(28.60)
Observations	7,137,671	6,891,804	7,137,671	6,891,804
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age in the sample ranges from 21 to 56. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

in Appendix B.

The results show a similar age–employment pattern as suggested by the main specification. The estimates support the hypothesis of a trade-off between early- and late-career employment, although some non-linearities are present in the early career period. As with the linear specification, using the simple reform indicator (Figure B4) failed to produce significant results. However, as shown in Figure B5, treatment intensity has a negative effect on the probability of employment until the late 30s (with some exceptions in the late 20s), after which the effect is positive. This crossover age is close to the one suggested by the main specification.

The estimated effects on earnings are noisier than the effects on employment, which may explain the poor fit of the linear model in this case, especially for men (see Figures B6 and B7). The preferred point estimates using treatment intensity as the explanatory variable (Figure B7) are mostly positive for men and women from the late-30s onward, although the estimates for men are not as precise as for women. For women, the point estimates are mostly negative prior to this age but for men, there is no clear pattern. The sensitivity of these results to different model specifications makes it difficult to conclude that a trade-off is present in the data related to earnings, although the estimates from this model point to possible earnings gains beginning from late-30's as a result of the reform[7].

[7]The findings here are in line with Pekkarinen (2008) and Pekkarinen et al. (2009) who find the average effect of the reform on taxable income in the year 2000 (which corresponds to ages 34 to 40) to be insignificant. In my

Several additional robustness checks were used to evaluate the estimated effects on employment. The results can be found in Appendix B. First, given that the reform increased the probability of attending higher education, it is likely that the treated would have entered the labor market later. Similarly, the non-treated were less likely to obtain higher education, which could have led them to enter the labor market earlier and even exit it earlier by retiring. To ensure that the results were not driven by this kind of behavior, the model was estimated using a sample of only those individuals who were either employed or unemployed. Second, observations from the Helsinki region were dropped; the reform was met with fierce resistance and most of the students were already attending general secondary school, making the reform largely redundant (Pekkarinen, 2008; Pekkarinen et al., 2009). Third, the data was balanced such that only persons observed in each year were retained. Finally, the model was estimated using a sample that included all the birth cohorts from 1960 to 1970. In the original sample, all treatment intensities were not observed in all municipalities, leading to the estimates reflecting differences between the municipalities. By including the birth cohorts from 1967 to 1970, all treatment intensities were observed in all the municipalities. All of the aforementioned robustness checks left the results qualitatively unchanged.

4.3. Effects of the reform on post-compulsory education

Since the reform increased the tracking age and updated the curriculum to include more academic content, an interesting question arises of whether the reform also affected post-compulsory schooling. Changes in educational attainment or the share of students choosing the academic track over the vocational track are likely to affect labor market outcomes over the lifecycle. To investigate these effects, Table 7 presents estimates of the reform's effect on education at age 30. The reported estimates are only from a model of treatment intensity because using the reform indicator produced statistically insignificant estimates close to zero, as with the previous results.

The estimates show, on average, an increase in the probability of attaining an upper secondary degree. This effect was found to be 0.6–3 percentage points (significant at 1% level) for men and 0.7–3.5 percentage points for women (significant at 5% level), depending on the treatment intensity. Relative to the mean of the outcome, these estimates correspond to increases of around 2–11 percent and 1–7 percent for men and women, respectively.

results, the point estimates for ages 34–40 include both negative and positive values.

Table 7: Effects of the reform on education at age 30

	Upper secondary degree		Vocational degree		Tertiary degree	
	(1)	(2)	(3)	(4)	(5)	(6)
	Men	Women	Men	Women	Men	Women
Intensity	0.006***	0.007**	-0.002	-0.006**	0.006***	0.008***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Outcome mean	0.279	0.477	0.556	0.516	0.267	0.383
Observations	250,660	241,337	250,660	241,337	250,660	241,337

Note: The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All columns control for municipality and cohort fixed effects.

Conversely, the reform's estimated effects on the probability of completing a vocational degree were found to be negative, ranging between -0.2 and -1 percentage points for men based on the treatment intensity. However, the estimate was not significantly different from zero. For women, the resulting effect was between -0.6 and -3 percentage points (around -1 to -6 percent relative to the mean), and it was significant at the 5% level.

Finally, the reform also had a positive effect on the probability of completing a tertiary degree. In the Finnish context, these include bachelor's and master's degrees from universities and vocational universities. The reform increased the probability of completing a tertiary degree by 0.6–3 percentage points for men (around 2–11 percent relative to the mean) and 0.8–4 percentage points for women (around 2–10 percent relative to the mean), depending on the treatment intensity. Both the estimates were found to be significant at the 1% level.

The reform's effects on post-compulsory education are likely to contribute to its effect on labor market outcomes over the lifecycle. Based on previous literature on general and vocational education (e.g., Hanushek et al. (2017)), the shift from upper secondary vocational to general education found here is in line with the estimated negative effect on employment early in the career and a positive effect later on. Similarly, the increase in the probability of completing tertiary education might explain the estimated age-employment pattern: Participating in tertiary education is likely to postpone labor market entry, thus decreasing employment in early career. However, the increased educational attainment could provide a better employment probability later in the individual's career.

5. Conclusion

This paper focused on the labor market effects of the Finnish comprehensive school reform over one's lifecycle using complete population administrative registry data. The reform transitioned the Finnish education system towards more general education by increasing the age at which students were tracked, into academic and vocational tracks, from 11 to 16 and by updating the curriculum to include more academic content. The municipalities' gradual implementation of the reform was exploited as a natural experiment to estimate the causal effects of the reform.

The results show that the reform decreased the affected population's employment probability in early career stages, but from their mid-30s onward, having attended comprehensive school turned into an employment advantage. Results for labor earnings are sensitive to different model specifications, but a positive effect was found in the late career by running separate regressions for each year of age. These results resemble findings from earlier literature comparing the lifecycle effects of general and vocational education. Changes in post-compulsory educational outcomes are likely mechanisms through which the reform affected labor market outcomes. The reform increased the probability of completing an upper secondary degree, instead of a vocational degree, as well as the probability of completing a tertiary degree. The obtained results related to the labor market and educational outcomes over the lifecycle were similar for men and women.

This paper provides one of the first estimates of the effects of a comprehensive school reform on labor market outcomes over the lifecycle. A limitation of the study is that it was impossible to disentangle the relative importance of the tracking age change from the curriculum changes. However, I argue that, in practice, comprehensive school reforms are likely to change both the curriculum and tracking age, making the results here relevant for evaluating the lifecycle effects of past and future reforms.

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Appendix A. Identification Strategy

Without loss of generality, consider two municipalities, m_j, where $j \in \{0, 1\}$ denotes the year in which the municipalities adopt the reform. Within both these municipalities, there are two cohorts, c_k, whose birth years are denoted by $k \in \{0, 1\}$. Assume that the reform r_{jk} is implemented such that it affects only cohort c_1 and only in municipality m_0.

Suppose we are interested in estimating the effect of the reform β at age a on an outcome y. Assume that there is a linear relationship between municipality, birth cohort, year, reform status, and y:

$$y_{ijkl} = a + m_j + c_k + t_l + \beta r_{jk} + \epsilon_{ijkl}, \tag{A.1}$$

where $E[\epsilon_{ijkl}|j, k, l] = 0$. t_k denotes year effects. Since there are two cohorts observed at the same age, there must also be two years in which they are observed, so that $k \in \{0, 1\}$. This exercise also highlights the age-period-cohort problem mentioned in Section 3. With age held constant, it impossible to disentangle the cohort and year effects. Thus, from now on, only age- and cohort-related effects are included, and the year effects are omitted, although these are implicitly included in the cohort effects. The model now simplifies to:

$$y_{ijk} = a + m_j + c_k + \beta r_{jk} + \epsilon_{ijk}, \tag{A.2}$$

Note that it is also assumed that the common trends assumption holds. This assumption states that the outcome y would solely be determined by the age, cohort, and municipality effects in absence of the reform, i.e., $E[y_{0ijk}|j, k] = a + m_j + c_k$, where $y_{0ijk} = y_{ijk}$ when $r_{jk} = 0$.

To identify the effects of the reform, I use a difference-in-differences estimator (DiD). To see that this estimator indeed identifies the effect of the reform, first, the difference between the expectations of the two cohorts in each municipality was taken, beginning with m_0:

$$\Delta_0 \equiv E[y_{ijk}|j = 0, k = 1] - E[y_{ijk}|j = 0, k = 0] = (c_1 - c_0) + \beta \tag{A.3}$$

Similarly, for m_1:

$$\Delta_1 \equiv E[y_{ijk}|j=1,k=1] - E[y_{ijk}|j=1,k=0] = (c_1 - c_0) \tag{A.4}$$

Finally, taking the difference between Equations A.3 and A.4, we have the DiD-estimator:

$$\Delta_0 - \Delta_1 = (c_1 - c_0) + \beta - (c_1 - c_0) = \beta \tag{A.5}$$

which is an unbiased estimator of the reform's effect. In practice, the estimator is implemented by including municipality and cohort fixed effects as regressors. This simple exercise can also be generalised to consider age-dependent effects. To comply with the model in Equation 1, interactions with age were added. The common trends assumption now states that $E[y_{0ijk}|j,k] = a + m_j^0 + c_k^0 + a(m_j^1 + c_k^1)$, and the outcome at age a can be written as:

$$y_{ijk} = a + m_j^0 + c_k^0 + \beta r_{jk} + a(m_j^1 + c_k^1 + \delta r_{jk}) + \epsilon_{ijk} \tag{A.6}$$

Δ_0 and Δ_1 can now be expressed as:

$$\begin{aligned} \Delta_0 &\equiv E[y_{ijk}|j=0,k=1] - E[y_{ijk}|j=0,k=0] \qquad \text{(A.7)} \\ &= (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) + \beta + \delta a \\ \Delta_1 &\equiv E[y_{ijk}|j=1,k=1] - E[y_{ijk}|j=1,k=0] \qquad \text{(A.8)} \\ &= (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) \end{aligned}$$

The DiD-estimator is the difference between Equations A.7 and A.8:

$$\Delta_0 - \Delta_1 = (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) + \beta + \delta a - (c_1^0 - c_0^0) - a(c_1^1 - c_0^1) = \beta + \delta a \tag{A.9}$$

which, again, is unbiased. It is worth noting that in Equation A.9, the effect of the reform at age a consists of the initial impact β and the age-dependent effect δa.

Appendix B. Figures and Tables

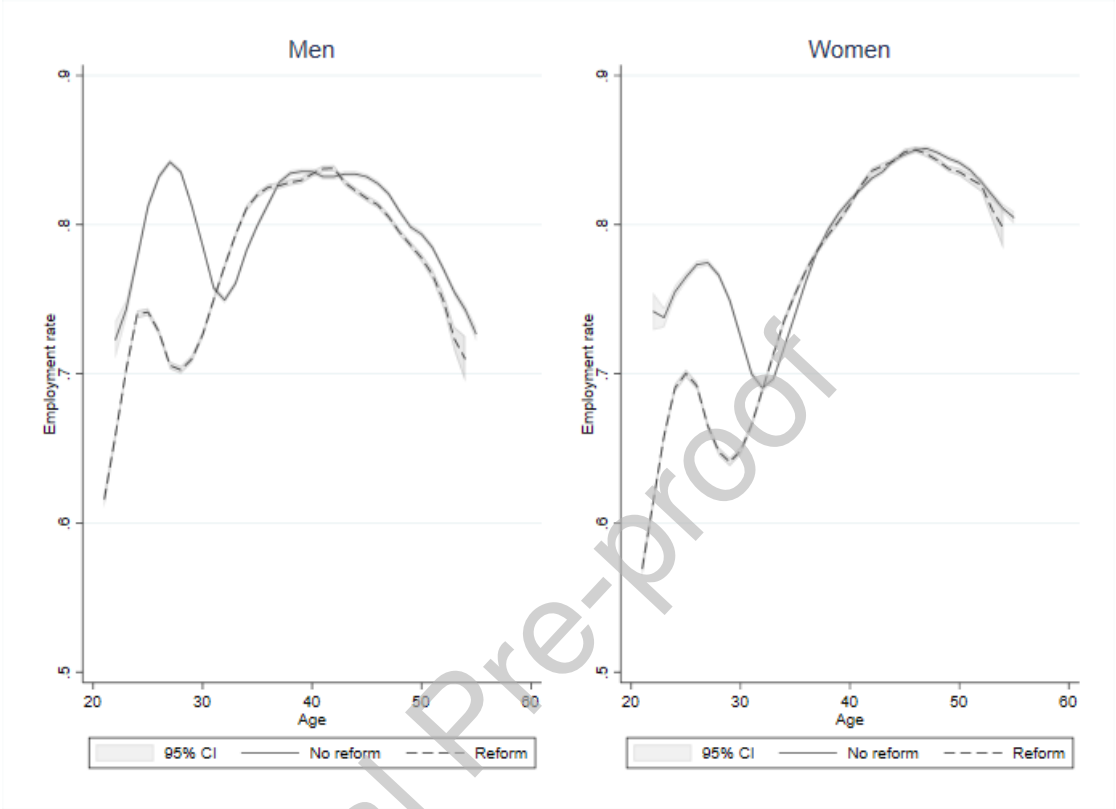

Figure B1: Employment by age and reform status.

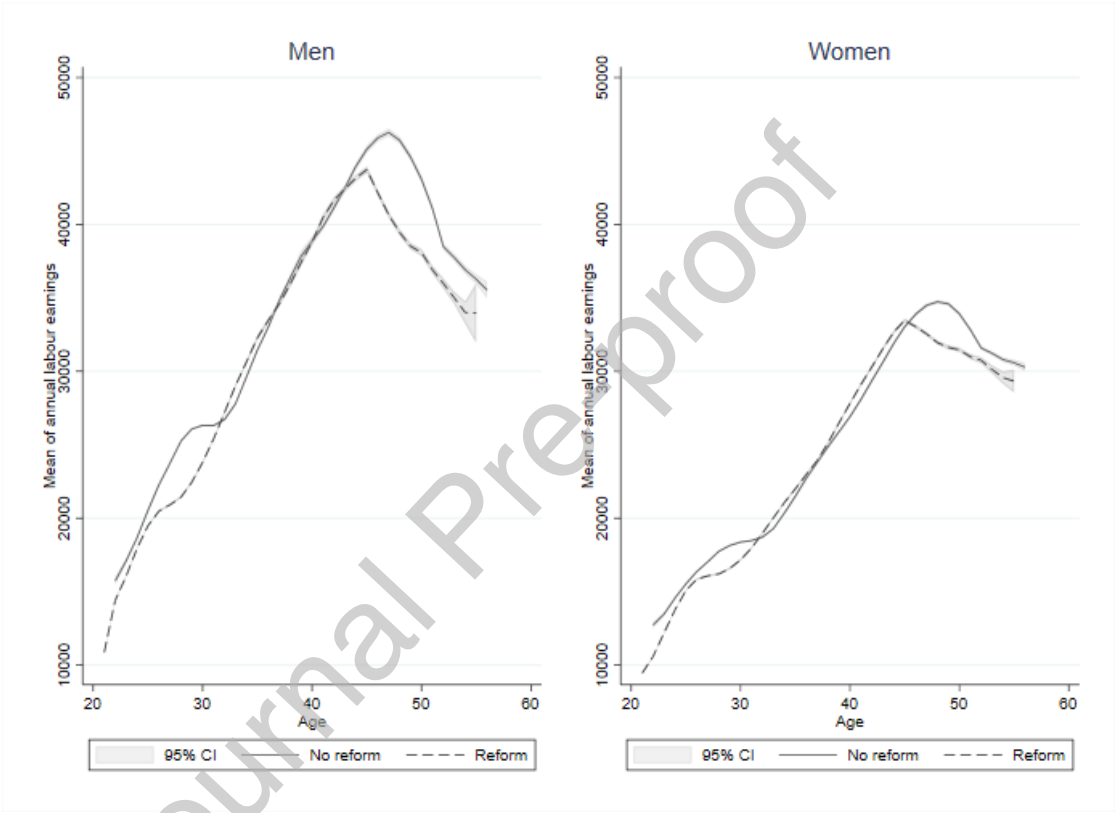

Figure B2: Annual labor earnings (2015 euros) by age and reform status.

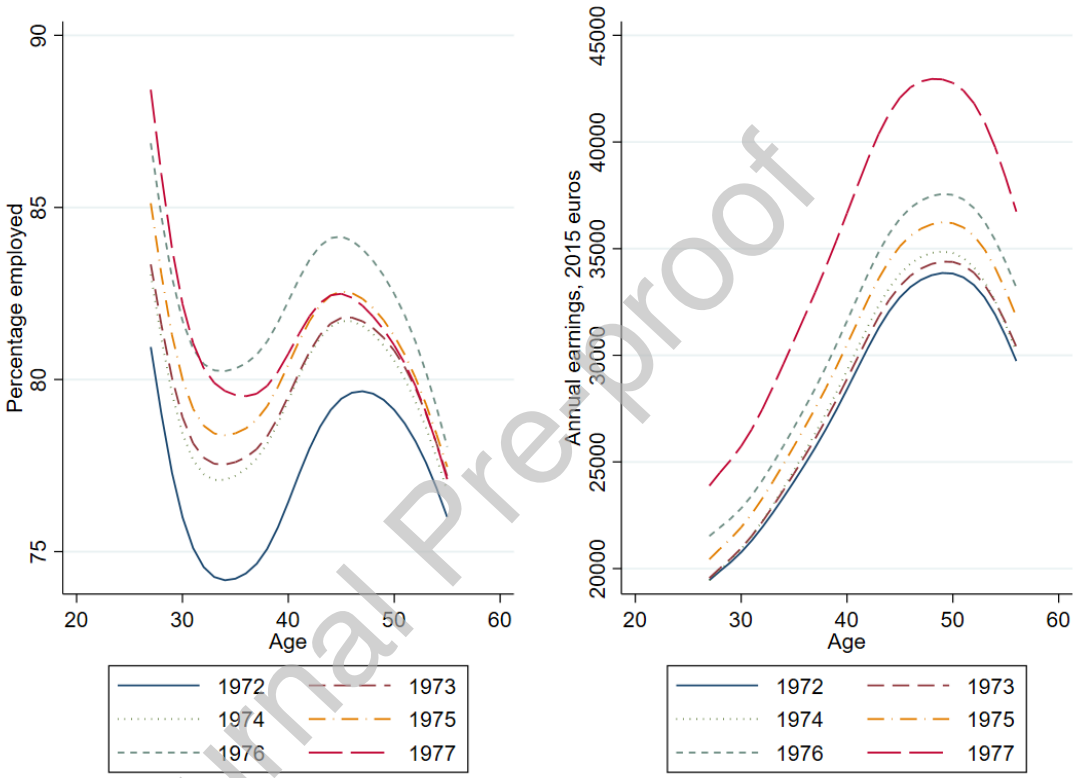

Figure B3: Employment and annual labor earnings by age and implementation region, 1960 birth cohort.
Note: Smoothed scatter plots using locally weighted regressions.

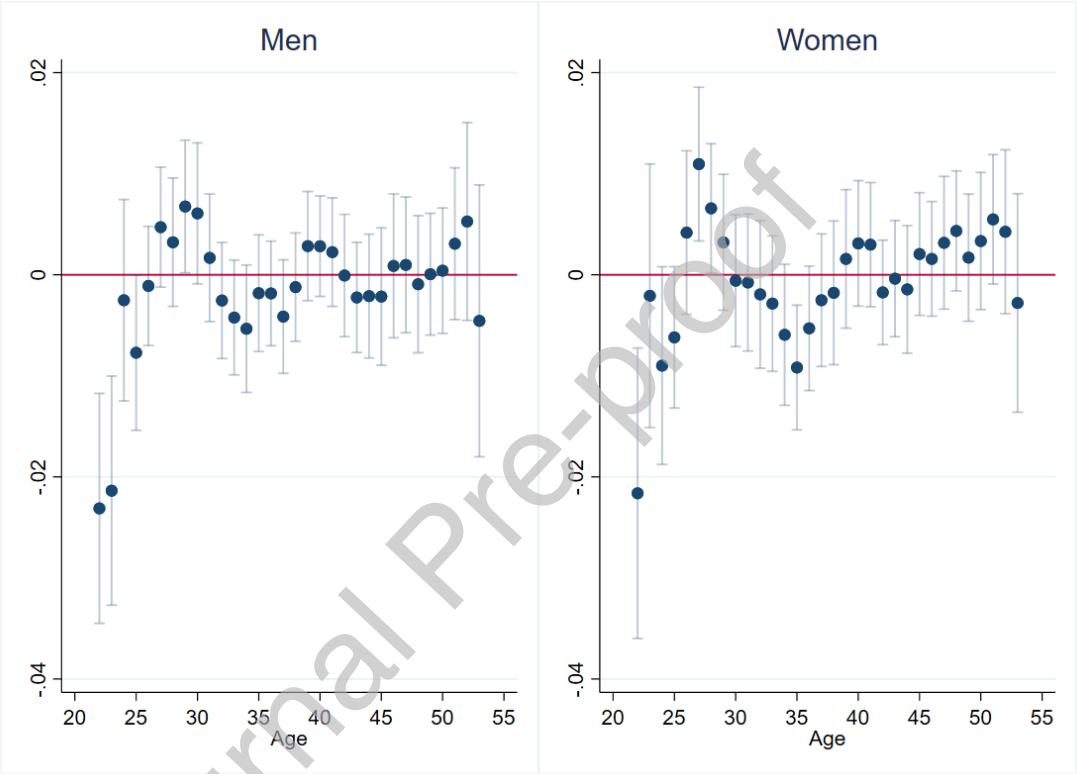

Figure B4: Effect of the reform on employment over the lifecycle.
Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

Figure B5: Effect of the reform on employment by treatment intensity over the lifecycle.
Note: Point estimates of the effect of treatment intensity with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Treatment intensity measures the number of years spent in comprehensive school (ranging from 0 to 5). Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

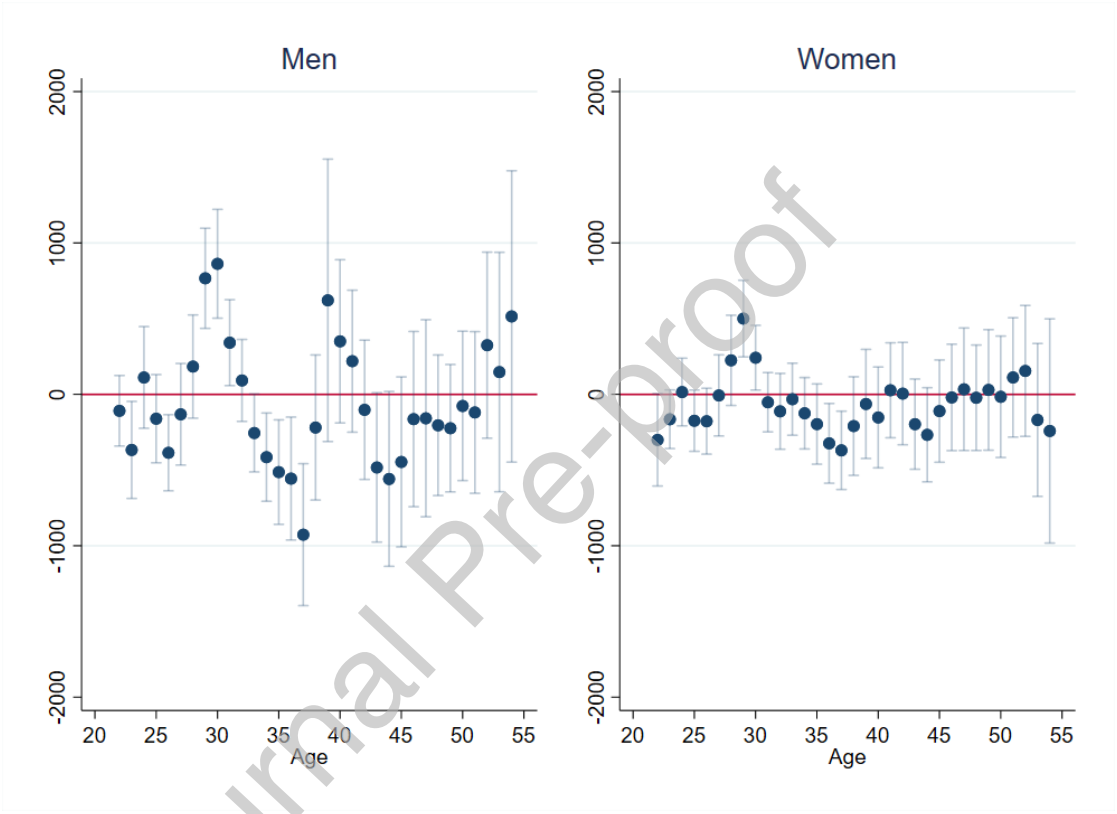

Figure B6: Effect of the reform on labor earnings over the lifecycle.
Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

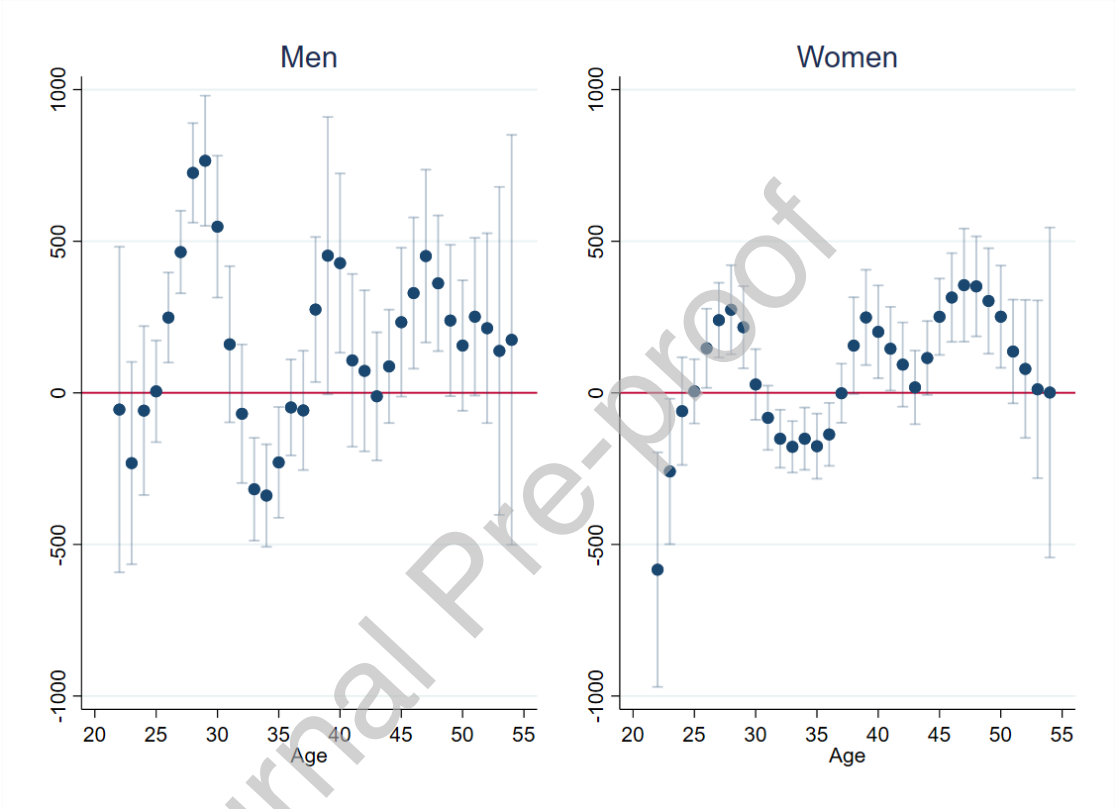

Figure B7: Effect of the reform on labor earnings by treatment intensity over the lifecycle.
Note: Point estimates of the effect of treatment intensity with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Treatment intensity measures the number of years spent in comprehensive school (ranging from 0 to 5). Standard errors clustered at the municipality level.

Table B1: Robustness checks

	Employed and unemployed		No capital region		Observed in all years		1960-1970 cohorts	
	(1) Men	(2) Women	(3) Men	(4) Women	(5) Men	(6) Women	(7) Men	(8) Women
Intensity	-0.004***	-0.003**	-0.003*	-0.005***	-0.006***	-0.009***	-0.002**	-0.003***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Intensity × Age	0.002***	0.002***	0.003***	0.003***	0.004***	0.005***	0.002***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	6,246,957	5,742,807	6,021,906	5,821,638	6,437,681	6,361,237	10,266,447	9,894,291

Note: The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.