A Window of Opportunity?

The Impact of Returning to Education on Earnings and Fertiliy*

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Abstract

We leverage high-quality Norwegian register data and a targeted second chance education reform to analyze the importance of the timing of human capital investments for adults who dropped out of high school. Exploiting variation in the age of treatment we find that adult women, by returning to education just a few years earlier, close the substantial intrahousehold education and earnings gaps with their male partners, whose education is found to be unaffected. Fertility also declines significantly post-reform. We find an important 'window of opportunity' for women in the timing of returning to education that points to an effective policy to reduce the gender earnings gap.

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1 Introduction

There exists a large literature estimating the returns to education while there is a paucity of literature on the timing of investments in education from a life cycle perspective. Investing in human capital at younger ages provides a larger return with more employment years to reap the benefits until retirement relative to investments made even a few years later. However, when to target high school drop-outs to return to education can vary by age and, for women, there may be an important window of opportunity related to fertility decisions. Later human capital investment in education can deliver a significant return for mothers who experience large earnings penalties after birth. In this paper, we analyze the importance of the timing of such 'second chance' human capital investments on further human capital investments, labor market outcomes, and fertility. We do so by leveraging a Norwegian policy reform that decreased the opportunity cost of returning to high school for high school drop-outs.

Our research design examines how both women and men respond to the adult education reform, exploiting variation in the age at which different cohorts are treated over the life cycle. This differential exposure to the education reform by age, combined with high-quality Norwegian register data, makes the setting ideal for estimating when returning to high school impacts subsequent higher education, labor market outcomes and future fertility. We use the reform to recover the impact of an education investment made at an *earlier* age, among high school dropouts in their early 30s. As the reform impacts each cohort at a different age, we can use it to exogenously move education investments to younger ages. Even though the comparison group is impacted by the reform when they are older, we are able identify the long-run effect of the change in the *timing* of the investment in human capital.

This paper contributes to the literature on the returns to investing in human capital since, while the labor market returns to education from on-time education are well established, causal evidence on the impacts of second chance education is scarce.[1] The education program we examine is distinct from programs in other countries which are not verified within the formal education system as in the UK (Blanden, Buscha, Sturgis, and Urwin, 2012), from the for-profit college system in the US which offers zero return (Deming, Goldin, and Katz, 2012; Deming, Yuchtman, Abulafi, Goldin, and Katz, 2016), and from the General Education Development (GED) program in the US, which is usually considered to be signalling without any human capital accumulation (Heckman, Humphries, and Mader, 2011; Tyler, Murnane, and Willett, 2000).[2] Indeed, the existing GED literature finds little evidence of GED certification on labor

[1] See Schwerdt, Messer, Woessmann, and Wolter (2012) for the impacts of education vouchers in Switzerland.

[2] It has similarities to the extensive adult education program from 1997 to 2002 in Sweden which funded one year of high school for 25 to 55 year olds in Sweden. However, this program was not within the education system but organized more as a job market program by the municipalities. Moreover, it was not necessarily aimed at a completed high school degree. There are several studies evaluating this either using structural models or matching techniques, and they find some positive employment effect for the lowest skilled Stenberg and Westerlund (2008); Albrecht, van den Berg, and Vroman (2009). The program is also different from an alternative route in Norway where you obtain a vocational degree through workplace training, and the degree externally verified by the education system. See Bratsberg, Nyen, and Raaum (2020) for details on this route.

market outcomes (Heckman and LaFontaine, 2006; Jepsen, Mueser, and Troske, 2016).[3]

We focus on enrolment in education for adults in their late twenties and early thirties. In itself, returning to school as adults in an important policy issue. Across the OECD area, 20–30% of each birth cohort drop out of high school before graduating, and by their mid 30s one third of the drop-outs have completed high school (OECD, 2017). We first document that most men who return to education after dropping out have already done so by the age of 25. In contrast, women return to education far later in the life cycle at ages when low educated women typically have already had their first child, but prior to completed fertility. Given these stark gender differences, we focus on young couples at the time of the reform and assess the importance of the timing of when women and men are treated by the reform. The early thirties represent a key age for women in our sample, given the fertility cycle, and almost all of the women who dropout of high school have had their first child by the age of 30.

Exploiting variation in the age of treatment is key to assess the importance of the timing of both returning to education and future fertility decisions. Specifically, we analyze whether a *window of opportunity* exists for female human capital investments, i.e., whether targeting adult women at relatively young ages enhances high school completion and leads to further human capital investments, better labor market outcomes, and changes in subsequent fertility. Our results show that by returning to education even a few years earlier in the life cycle, treated women are significantly more likely go on to higher education after returning to high school. In contrast to their female partners, however, male education remains unchanged. The results also point to a potential route to reduce the gender earnings gap: the reform increases education investments of women relative to their partners, reducing intrahousehold earnings gaps and impacting future fertility.

Causally estimating the timing of when in the life cycle human capital investments are made when individuals return to complete levels of education they previously dropped out of presents an empirical challenge. First, high school completion and the timing of later life education are clearly endogenous decisions. Second, it is not straightforward to isolate a suitable counterfactual for late high school graduates. Third, the sequential nature of education implies that those who complete high school on-time may return to higher education later in life. Finally, prior research has demonstrated the importance of the age at which earnings are measured when estimating the returns to education (Bhuller, Mogstad, and Salvanes, 2017).

We address each of these concerns, exploiting a policy reform which overhauled the student support system (Lånekassen), increasing the amount of financial support for adults enrolled in high school. This change represents a significant overhaul to the financial support system for adults enrolled in high school

[3]Clark and Martorell (2014) use a RD design to assess the signalling value of a high school diploma, finding little evidence that graduating high school has signalling effects. Relying on the assumption that, conditional on controls, the year an individual decides to return to education is random, Albæk, Asplund, Barth, Lindahl, Strøm, and Vanhala (2019) suggest that adult education in the Nordic countries prevents labor force dropout.

(Norges offentlige utredninger 2018: 13, 2018), with substantial increases in stipends and loans post-reform. Within an event study framework, we measure intrahousehold earnings gaps both prior to and after the completion of education over a long time horizon and examine both short- and long-run impacts of the change in the timing of education on gender earnings gaps. Given that our reform is a one off change affecting everyone at once, we exploit variation in the age at which different birth cohorts are treated by the reform. Specifically, we use older cohorts who were treated at an even later age as a counterfactual for cohorts treated at a younger age.[4] Our empirical design always compares treated cohorts, who get financial support from ages 30–33, to counterfactual cohorts, who are exposed to the same reform three years later. For instance, we estimate the difference in outcomes of being treated at age 33 to being treated three years later at age 36. For the age 33 group, treatment status is always 1 while for the age 36 group, this is always 0. While all cohorts are eventually treated, and we exploit variation in the onset of the reform across cohorts, our approach estimates the evolution in the difference between these two groups over time. As such, issues related to staggered difference-in-differences approaches such as negative weighting and treatment effect heterogeneity are not a major concern for our empirical design, and results are robust to the imputation difference-in-differences estimator developed in Borusyak, Jaravel, and Spiess (2024).

As the event study framework exploits variation across different birth cohorts, changes in demographic characteristics over time may affect the similarity of treated and counterfactual cohorts. Consequently, pre-treatment differences in the composition of the sample between cohorts treated at different ages may affect the dynamics of the outcome variable post-reform, violating the parallel trends assumption. Abadie (2005) and Blundell and Dias (2009) show that weighting the regression by the propensity score estimated as a first step can account for such differences, and we follow a similar approaches taken Mastrobuoni and Pinotti (2015) and Goodman-Bacon and Cunningham (2019). In addition, we control for differences between birth cohorts over time and across areas using data on the entire population.

The paper presents two main sets of results. First, we focus on households, matching both women and men to their partners at the time of the reform. Comparing cohorts treated at younger ages to cohorts treated at even older ages, we find that by reducing the opportunity cost of returning to high school, the reform significantly increases education among female dropouts in their early 30s. In contrast to their female partners, male education remains unchanged post-reform. A majority of women who return to complete high school at younger ages following the reform also continue in the education system to complete higher education, suggesting a relationship between returning to high school younger and the probability of continuing with higher education. By enabling women to return to education, the reform significantly reduces pre-reform intrahousehold earnings gaps. Education increases the rate at which women transition

[4] As a robustness check, we show the same patterns in high school completion using on-time completers. However, we also show that on-time high school graduates are not a suitable counterfactual by exploiting a rich set of characteristics including cognitive test scores.

back into the labor market, and this is key in reducing intrahousehold earnings gaps: while 72% of men are employed full-time pre-reform, just 30% of women are.

Treated women experience a significant decline in fertility as a result of the education reform, suggesting that the joint impact of education on labor market participation and future fertility is an important mechanism. An extensive literature documents considerable gaps in the labor market outcomes by gender and underlying reasons for such differences (Goldin, 2006; Blau and Kahn, 2017). Recent papers have emphasized the importance of the child wage penalty which are considerable in the Scandinavian as well as many other countries (Kleven, Landais, Posch, Steinhauer, and Zweimüller, 2019). By facilitating mothers' transitions back to work (Blundell, Dias, Goll, and Meghir, 2021), returning to education even a few years earlier significantly reduces the pre-existing intrahousehold gaps in earnings as the labor market outcomes of women improve relative to men.

Having established the importance of intrahousehold adjustment, our second set of results examines how the labor market outcomes of all women, both those with and without a partner, are impacted by a second chance in education. We find returning to education earlier in the life cycle improves labor market prospects among all women, not only those in households, with increases in both labor earnings and employment. The observed increase in earnings attributed to later life education is driven primarily by increases in employment rather than increases in hourly wages.

The rest of the paper is organized as follows. Section 2 describes the education system in Norway, describes key attributes of the Norwegian register data that we exploit in this study, and documents the differences in timing of education investments between men and women. In Section 3 we detail the access policy that provided a new financial incentive combined with the removal of barriers to access high school for adults in Norway. We also describe our research design that exploits variation in the age at which high school dropouts are treated by the education reform. Section 4 presents the estimation results of the impact of the reform on adult education investments and intrahousehold gender gaps in these investments. In Section 5 we document the main causal impact on labor market outcomes. Section 6 examines robustness to various directions of heterogeneity including field of study, cognitive ability and labour market conditions. Section 7 concludes.

2 Institutional Setting, Data and Descriptives

2.1 The Norwegian Education System

Following a 1959 reform (see Black, Devereux, and Salvanes (2005) for further details), compulsory schooling in Norway is composed of 6 years of primary school and 3 years of secondary school for our birth cohorts. After the completion of compulsory schooling, a student decides whether or not to continue into high school education or drop out. All birth cohorts considered in this paper are under this compulsory schooling system, such that an individual may drop out at roughly age 16 to join the labor force. Importantly, all students from compulsory schooling are guaranteed to be offered a high school space.

High school is comprised of both academic and vocational high school programs and lasts 3 to 4 years. Academic high school is geared towards future enrollment in university education and lasts 3 years. Vocational high school leads to professional employment in a given vocation after the completion of high school. The vocational route is based on a combination of school and apprenticeship. The main model is 2 years in school, followed by a 2-year apprenticeship. During the apprenticeship period, the apprentice is employed in an approved firm responsible for providing training of sufficient quality.

Tertiary education in Norway includes both university colleges—which specialize in shorter higher education programs such as engineering, nursing, and teaching—and universities. For instance, in order to become a nurse or a teacher, an academic high school degree is required. In addition, technical colleges offer post-secondary education in the vocational track. Such programs are short, spanning a minimum of 6 months to 2 years, and convey the status of a skilled vocational technician. The direct costs of attending are close to zero, as there are no tuition charges and most students qualify for grants and loans from the government.

2.1.1 Adult Education in Norway

The standard route for second chance education in Norway is through the formal education system. The education system offers high school courses for adult students, either as day time or evening classes and was well established at the time period we are analyzing. High schools are administered at the county level, and most classes are offered by the public school system, with a small share also by private schools (about 5% of high schools are private in Norway). Adult education is offered as separate classes for adult students within these schools. Importantly, the qualifications of teachers is the same as in standard high schools, and it is, for the most part, offered in the same school buildings with the same teachers teaching both adult classes and standard high school education. Adult students are also under the same testing regime as on-time high school students. For vocational training, an alternative out of classroom opportunity exists for workers with substantial labor market experience, the Practical Candidate Scheme as discussed in Bratsberg, Nyen, and Raaum (2020). While this Scheme certifies specific vocational skills, it does not necessarily reflect human capital investments.

2.2 Norwegian Register Data

In order to analyze the labor market impacts of later life education, the paper makes use of administrative-based Norwegian Register Data. The data is linked by Statistics Norway across different sources by an anonymized personal identification number. The paper merges information across several different registers to create an individual-level panel following people from birth to age 45. The focus of the empirical analysis are cohorts born between 1964 to 1970 since these cohorts are covered by the educational reform we use for identification. We have earnings data up to 2015, and this is the reason we measure educational attainment and earnings up to the age of 45. In the descriptive analysis below, we extend the cohorts up to 1980 to provide a complete picture of returning to education over time.

We extract population information from the central population register which contains information such as an individual's birth year, gender, age, citizenship, municipality of residence in a given year, and municipality of birth. Information is available for any person who is legally resident in Norway. The central population register also links families across generations, which links parents to children. Such linkage permits the construction of measures for parental education as well as information on any children born to both mothers and fathers.

The Education Register provides information, in each year, on ongoing student status as well as years of education and the exact qualification achieved.[5] Qualifications are measured at the detailed field of study level, and correspond to the International Standard Classification of Education (ISCED) system. Schools have a legal requirement to report any information on enrollment and graduation to Statistics Norway, which minimizes the potential for measurement error. Throughout the paper, educational qualifications at the high school level are grouped into academic or vocational.

Employment data is provided from the Register of Employers and Employees. This provides information on employment status, hours worked, and industry of employment. Hours worked is classified in three categories: (i) less than 20 hours per week, (ii) 20–29 hours per week, and (iii) full-time employment, employed 30 hours or more per week. Data on earnings is extracted from the tax and earnings register, where annual labor earnings are recorded. We use two alternative measures of earnings. First, gross earnings which includes pre-tax total labor earnings, including any earnings from self-employment, and some transfers such as taxable benefits received in a given year including parental leave, unemployment, or sickness benefits. Second, gross market income which excludes any transfers. In addition to these two measures of annual earnings, we also measure hourly wage, calculated as follows. First, we assign an average hours per week employed to one of the three categories of hours worked.[6] Second, we make use of data on days employed to create a measure of annual hours worked. Finally, we measure hourly wage dividing gross market income by annual hours worked.

We also draw on measures of cognitive ability for all males at age 18 from compulsory military testing data. Although not available for women, this does provide some indicative information about the initial relative cognitive distributions between different high school completion groups. Cognitive ability is comprised of three examinations: an arithmetic test similar to the arithmetic test in the Wechsler Adult

[5]Years of education are measured as the number of years it should take a student to achieve a given qualification.
[6]For instance, the average hours worked among those employed less than 20 hours per week is 10.5 hours/week.

Intelligence Scale (WAIS), a word similarities test similar to the same test in the WAIS, and a figures test similar to the Raven Progressive Matrix test. The cognitive ability measure is measured on a 9 point scale, with a mean of 5 and a standard deviation of 2.

2.3 Age-Earnings Profiles

Departing from the notion of on-time completion to later life completion complicates the estimation of the effect of education on lifetime earnings. Understanding to what extent late completers close the gap in earnings to on-time graduates, and to what extent younger late completers fare better than older late completers, is crucial to understand the labor market returns to later life education. To do so requires a comparison of the evolution of earnings prior to study, during study, and after the completion of education. The subsequent figures focus on the evolution of earnings over the life cycle, from the ages of 18–45, for all individuals born 1964–1970 who complete high school at different ages: completed high school at 20 or younger (on-time), 24–26, 30–32, 36–38, and not completed by 45.[7] While all individuals here return to high school, it remains informative to compare the evolution of earnings between on-time graduates and late completers of different ages over time.[8]

Figures 1a and 1b plot the age-earnings profiles among the five groups including those who go onto complete higher education after high school. Prior to completing high school at 24–26 and 30–32, and 36–38, as indicated by the shaded regions of Figure 1, those who complete high school experience a decline in log earnings.

However, the short-run earnings penalties while returning to high school are more than compensated by higher earnings in each subsequent year after completion. Indeed, the slower earnings growth prior to graduating high school is quickly compensated by rapid earnings growth in the years immediately after completing high school. As time goes on, the earnings growth of late completers converges to the slope of the earnings profile of on-time graduates. Despite this strong growth after completion, later completers never fully catch up to earnings levels of on-time graduates: while the earnings growth of late completers reaches the slope of on-time graduates, there remains a persistent levels difference. Additionally, the age an individual returns to complete high school matters for their earnings by age 45, as those who return earlier have earnings which more closely resemble the earnings of on-time graduates while those who never graduate high school have the lowest earnings.

Figures 1c and 1d plot the earnings of those who complete high school at different ages, excluding

[7] Results using other birth cohorts display similar patterns. Appendix C presents results for the completion of higher education across different ages, and show similar patterns of age-earnings profiles.

[8] If one is willing to assume that age of completion is as good as random, then this is causal impact of returning to high school on earnings. However, the extent to which this assumption holds is questionable. Those who complete high school at age 30 had the opportunity to also complete high school at each age from 21–29, but chose not to do so. There are good reasons to believe why one returns to education later in life rather than earlier in life is due to a host of explanations (e.g. changing personal or family situations, changes in employment status, etc).

Figure 1: The Age-Earnings Profiles by Different Ages of High School Completion

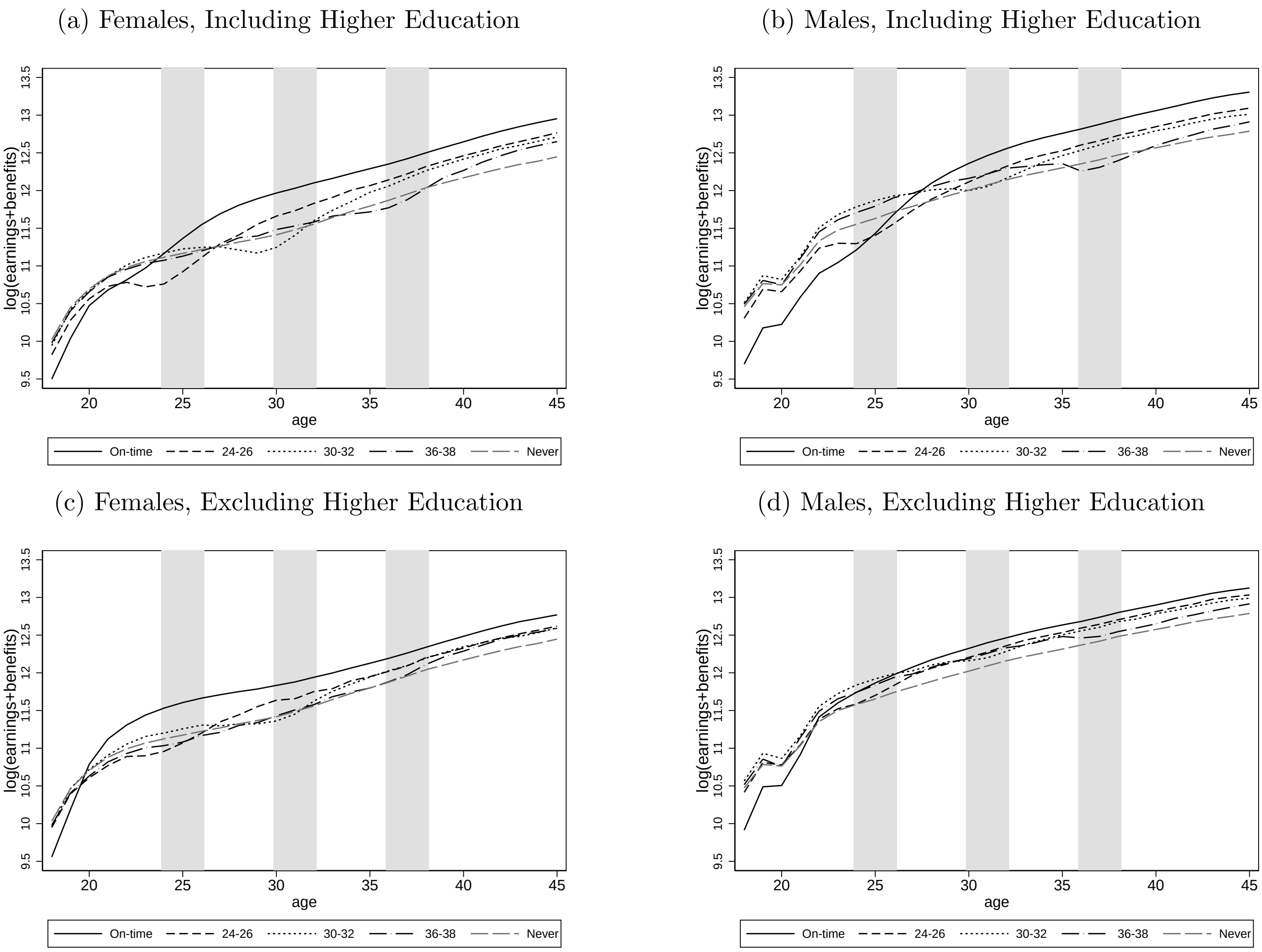


Figure plots, for women and men respectively, average of log earnings for on-time high school graduates (20 or younger), late graduates aged 24–26, 30–32, 36–38, and those who never complete high school by 45. Panels (a) and (b) exclude any person who continues after the completion of high school with a post-secondary degree, while panels (c) and (d) include all high school graduates irrespective of their eventual final degree by age 45. Birth cohorts 1964–1970.

those who continue onto any further education after high school. While excluding higher education clearly ignores the fact that the completion of high school leads to additional higher education, such a sample permits the comparison of the value of a high school degree completed at different ages.[9] Compared to Figures 1a and 1b which include higher education, the differences in age-earnings profiles across different ages of high school completion are much smaller.

Abstracting from endogeneity concerns, the difference between any two curves in Figures 1c and 1d represents the labor market premium to completing high school (and no further education) at a given age relative to another age. For instance, those who complete high school after 20 have higher earnings at younger ages than those who complete high school on-time. However, already by the early 20s, this difference has reversed, and on-time graduates begin to earn higher levels. Relative to each age of completion, there exists a discount rate which equalizes the present value of the two age-earnings profiles.[10] Relative to the female sample, the earnings penalties incurred by male on-time graduates are much larger. While there are clearly selection issues in comparing late completers of different ages and on-time graduates, it is interesting that the discount rates are considerably lower for men compared to women. Among late completers 24–26—an age range when considerably more men return to complete high school relative to women—discount rates of 5–7% equalize the present value of earnings to on-time graduates compared to 27–45% for women. Such low discount rates suggest that more present-biased men may actually find it worthwhile to drop out of high school and return later in life, while the calculated discount rate for women indicates strong incentives to return to education.

3 Access Reform, Choice of Counterfactual and Empirical Approach

In this section, we describe the reform which introduced a new financial incentive combined with the removal of a barrier to access that took place over a three year period from 2000. As we show below, the reform successfully lowered the opportunity cost of returning to education for those who dropped out of high school. We present the counterfactuals and sample used, and lay out our empirical specification.

3.1 The Access Reform

In the 2002/2003 academic year, the Norwegian government introduced a significant overhaul of the student support system (Lånekassen) for adults enrolled in high school (Norges offentlige utredninger 2018: 13, 2018, 6.6.2). These changes were first proposed in August 2001 (Kirke-, utdannings- og forskningsdepartementet, 2001), and the reform resulted in substantial increases in the level of financing available for adult education:

[9] The sample restricts to those who by age 45 still have high school as their highest level of education. Only 36% of those who finish high school on-time still have high school as their highest education level by age 45.

[10] Table D.1 reports more details, and present the discount rates required to equalize the present value of earnings of on-time graduates to later life completers.

it provides a generous monthly stipend to return to education. Post-reform, any adult enrolled in high school was eligible to a stipend of 3,200NOK per month (Lånekassen, 2002, Kap. III).[11] In addition to the stipend, any adult enrolled in high school was eligible for additional support in the form of a monthly loan of 4,800NOK, taking the combined support of the stipend and loan to 8,000NOK per month.[12]

This reform was part of a new agenda to enhance access to adult education in Norway which began in the 2000/2001 academic year when a new law was introduced extending the legal right to enroll in high school education as an adult (*§4A-3, Lov 30 juni 2000 nr. 63*, Kirke-, utdannings- og forskningsdepartementet, 2000). The introduction of the right to upper secondary education (§4A-3 Rett til vidaregåande opplæring for vaksne) required counties to admit any student returning to high school, where their prior inflexibility represented a barrier to enrollment (Kirke-, utdannings- og forskningsdepartementet, 1999). However, until the 2002/2003 academic year, adults exercising their legal right to enroll in high school were entitled to a considerably less generous support means-tested against their income (Lånekassen, 2001).

Figure 2 reveals that take-up of the adult education financing reform increases coinciding with the introduction of the overhaul of the student support system. Panel (a) reports raw averages among treated and counterfactual cohorts, showing an increase in financing among cohorts treated by the reform, and a corresponding increase in the counterfactual group when they are eventually exposed to the same reform later in life. Panel (b) shows that the school reform leads to significant increases in student financing two years after it was first introduced, consistent with the introduction of the change in student support from the 2002/2003 academic year.

Figure 2: The Impact of the Adult Education Reform on Student Support Income

(a) Student Financing, Raw Averages (b) Student Financing, Estimated Treatment Effect

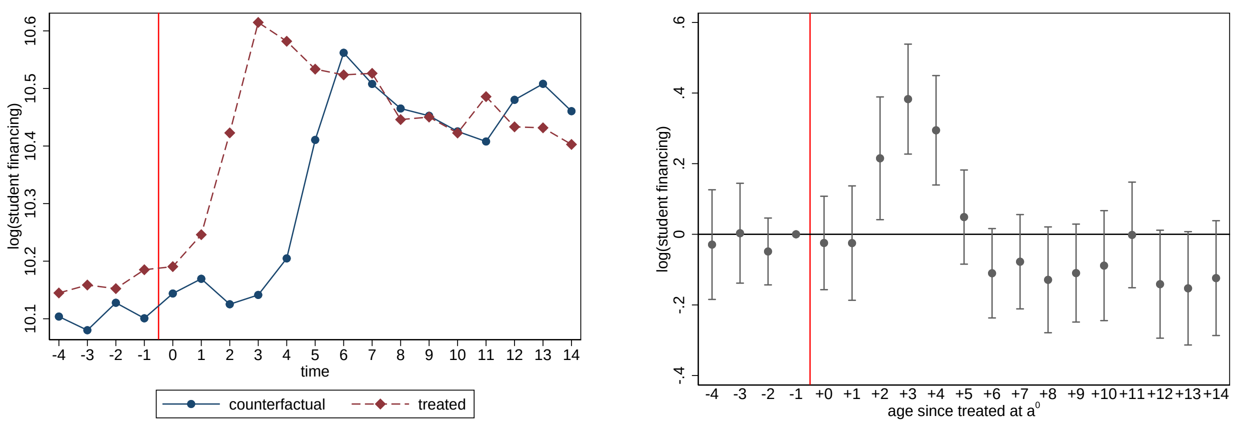


Figure plots raw averages (panel a) and estimates of equation (1), weighted by estimated propensity score in -1 as described in Section 3.3.2 respectively. Figure plots the impact of the reform on student support income, which includes both unconditional stipends and loan based stipends. Sample of females of base ages 30–33. 95% confidence interval reported.

[11]3,200NOK represents a considerable stipend as it corresponds to 12% of median monthly earnings of similar aged workers. Median earnings at the time of the change of women aged 30–34 is roughly 25,000NOK (Statistics Norway, Table 05218). Those with children were eligible for an even more generous stipend, the level of which depends on income.

[12]If an adult failed to graduate with a high school diploma, 1,200NOK of the total amount would be classified as a loan rather than a stipend.

In addition, Figure 3 shows that take-up is greater and lasts for longer among those who are initially unemployed pre-reform relative to those who are initially employed. As there exist considerable gender gaps in rates of employment pre-reform, where females in our sample are roughly 40 percentage points less likely to be employed, we see that the reform pushes women back to education at higher rates relative to men. Strong gender differences are in part driven by the fact that the typical low-educated woman is considerably less attached to the labor force than the typical low-educated man.

Figure 3: The Impact of the Adult Education Reform on Student Support Income, by Initial Employment Status

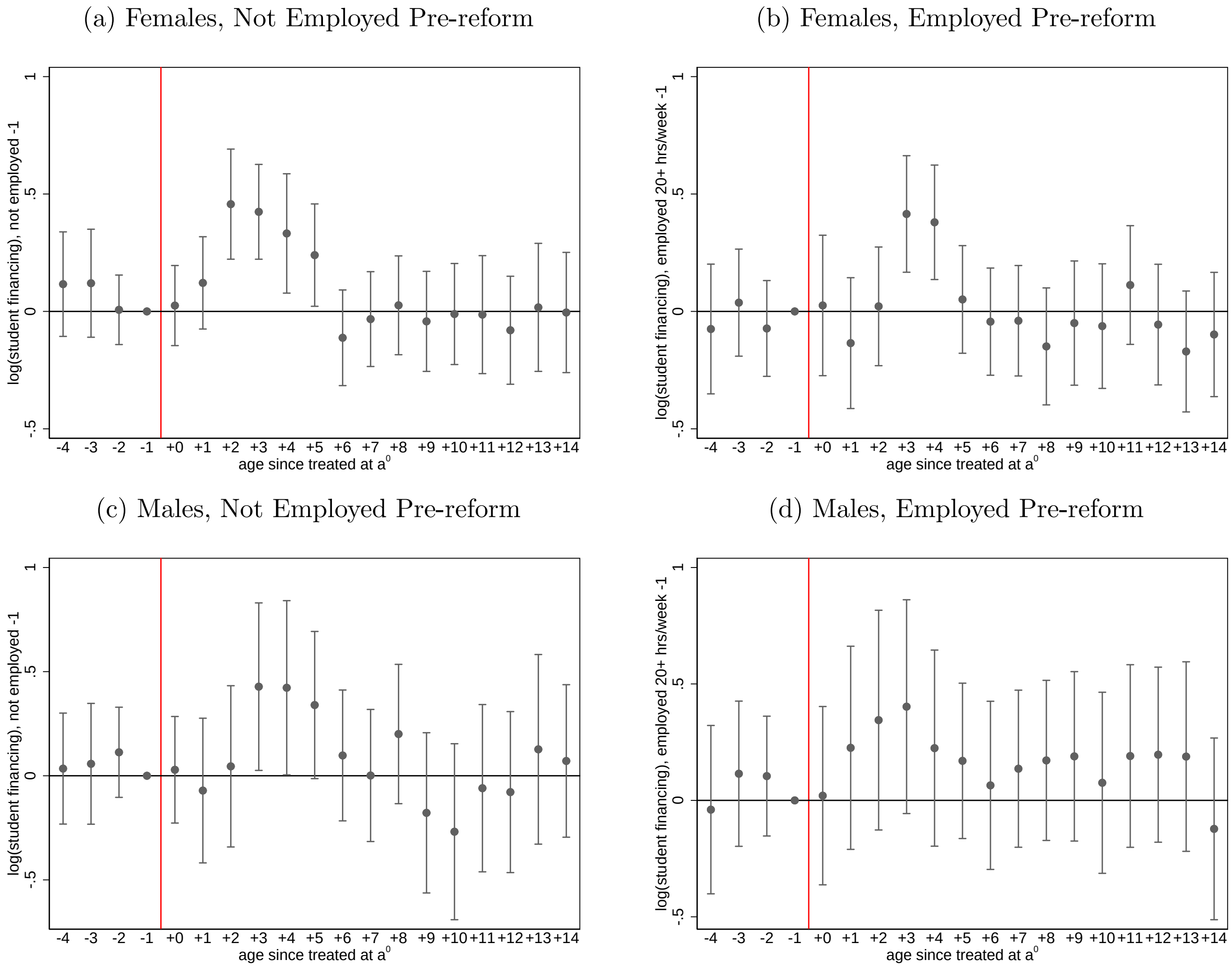


Figure estimates of equation (1), weighted by estimated propensity score in −1 as described in Section 3.3.2 respectively and estimated separately by gender and pre-reform employment status. Figure plots the impact of the reform on student support income, which includes both unconditional stipends and loan based stipends. Sample of females (panels a and b) and males (panels c and d) of base ages 30–33. 95% confidence interval reported.

3.2 Choice of Counterfactual

The reform took place over a three year period and covered the whole economy, consequently we have to take care in isolating a suitable counterfactual in order to estimate the causal impact. We require a similar group who is, at the same time, not impacted by the reform at the comparison age. Figure 1 suggests three potential groups: those who have completed high school on-time, those who never return to high school, and those who return to high school at even later ages.

Although on-time high school graduates may seem a natural comparison group they are unlikely to be a suitable counterfactual for causal analysis. We find substantial differences in cognitive ability between on-time high school graduates and dropouts, see Appendix Figure E.1. There is also a strong socio-economic gradient for on-time graduates and late completers, see Appendix F. Consequently, while on-time graduates have the advantage of not being treated as they have already completed high school, they also will have quite different trends in income compared to high school dropouts in the absence of the educational reform. This can be seen in Figures 1a and 1b. Moreover, those who never complete high school are also unsuitable as they choose not to take up the reform. Using such a group, never returning to high school, would select on post-reform outcomes.

Given the unsuitability of on-time graduates and of never completers, we exploit variation in the age at which different birth cohorts are first treated to compare cohorts treated at younger ages to cohorts treated at even older ages. We define the comparison group as counterfactual cohorts even though they are also eventually treated later in life. Thus, while both treated and counterfactual cohorts are eventually treated, our empirical approach exploits variation in the age at which different birth cohorts are treated. Our choice of counterfactual focuses on the timing of when in the life cycle different birth cohorts return to high school and asks how those who return at younger ages fare compare to those returning at slightly older ages. Such an approach, when combined with the reform, provides a source of exogenous variation in the age at which different cohorts are treated by the reform. The variation exploited is similar to recent papers exploiting the timing of the event in an event study framework (see e.g. Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019), however, with the difference that we use educational reform for identification. For comparison we also provide results using on-time graduates as a comparison and also using different older age groups. Overall the results confirm our main findings.

As a starting point, Figures 4a and 4b plot the change in education for different birth cohorts from the pre-reform levels to 2014, for years of education and the probability of graduating from higher education. The Figures ask how the age at treatment in year 2000 impacts the change in education observed 14 years later. There is a clear pattern where cohorts who are treated at younger ages, for instance the 1970 and 1967 cohorts, see much larger changes in education after treatment than cohorts treated at later ages. The differences in the growth of education from 1999–2014 across different cohorts suggests the age at treatment is crucial for the long run impacts of the intervention.

Figures 4c and 4d plots the raw age-education profiles of different birth cohorts who dropped out of high school. By asking how the evolution of education over the life-cycle differs across these cohorts, the Figures plot changes in education similar to the variation which we exploit in our empirical design. This also turns out to be quite revealing: trends in education are similar pre-reform, and ultimately diverge after the reform depending on when in the life cycle different cohorts are treated. As the 1970 cohort is first treated at age 30, they see the largest increase in education earliest in the life cycle. In contrast, the 1967 cohort is first treated 3 years after at age 33, who see large increases roughly 3 years after the 1970 cohort. Similar patterns are observed for the 1964 cohort and the 1961 cohort. Even though we see some catch up among later cohorts when they are treated, the Figure suggests a long run impact of human capital intervention at younger ages.

Figure 4: The Change in Education and Age-Education Profiles of Different Birth Cohorts

(a) Years of Education (b) Higher Education

(c) Years of Education (d) Higher Education

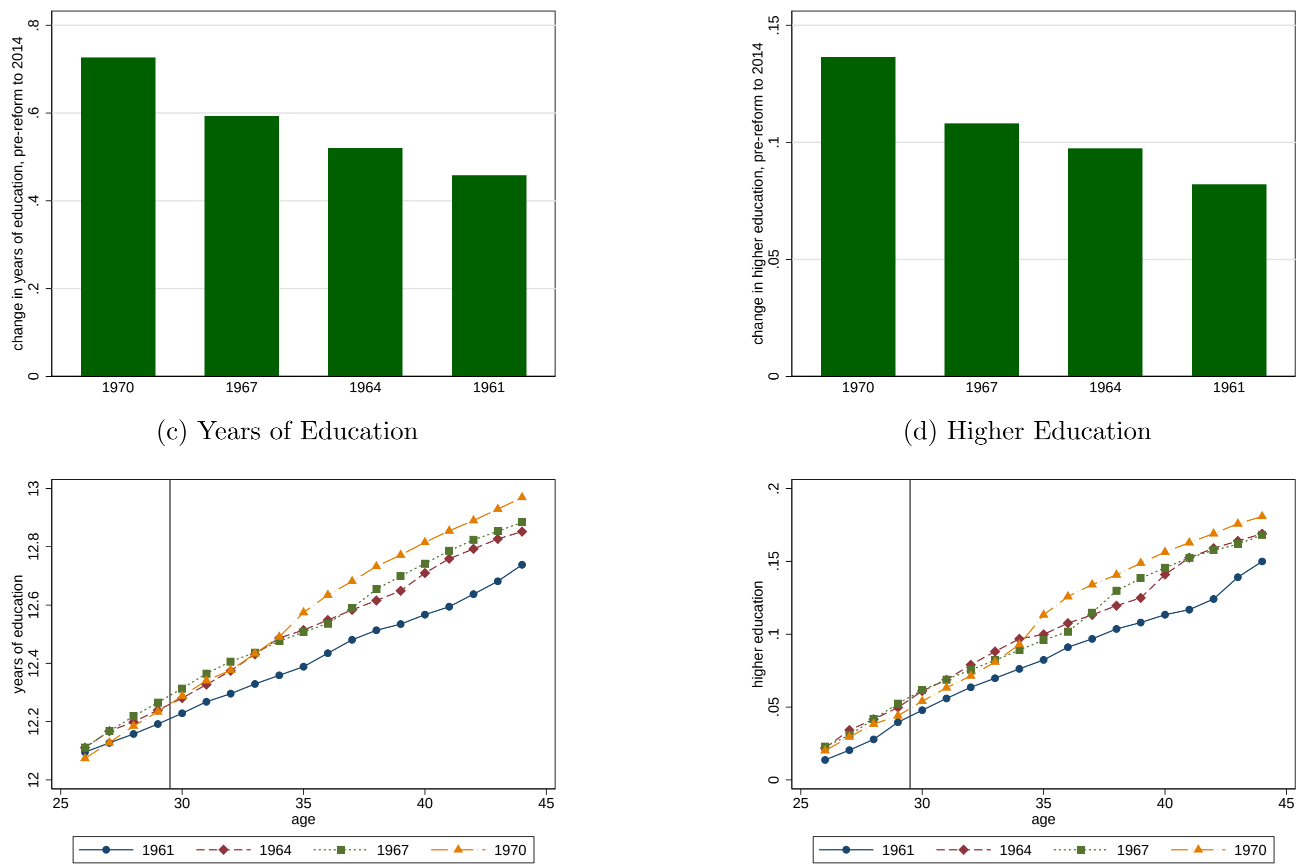


Figure plots the change in years of education (panel a) and higher education (panel b) from 1999–2014; and the age-education profiles for average years of education (panel c) and higher education (panel d) among women. Sample of cohorts born 1961, 1964, 1967, and 1970 who dropped out of high school by age 20. 1970 cohort treated at age 30, 1967 cohort treated at age 33, 1964 cohort treated at age 36, and 1961 cohort treated at age 39. Solid line in panels c and d corresponds to the age just before the 1970 cohort is treated by the reform.

3.3 Empirical Approach: Estimating the Impact of the Educational Reform on Age-Education and Age-Earnings Profiles

To exploit variation in the age at which high school dropouts are treated by the education reform, we define two groups: treated cohorts—those who are treated at younger ages—and counterfactual cohorts—those who are treated at later ages. As noted above, although the treatment begins in 2000, the full implementation does not take place until 2002/2003. Treated cohorts are those aged a^0 in 2000 while counterfactual cohorts are treated by the same reform from age $a^0 + \delta$, that is δ years later. In our baseline specification, $\delta = 3$, and counterfactual cohorts are exposed to the same reform three years after treated cohorts.[13] Treated cohorts c are defined as:

$$treated_{c(i)} = \begin{cases} 1, \text{ if } a^0 = a \text{ in year 2000} \\ 0, \text{ otherwise.} \end{cases}$$

As counterfactual cohorts are assigned 0 throughout the time period, our estimated treatment effects compare differences in exposure to the reform, where those treated at even later ages always serve as the counterfactual for treated cohorts.[14] Variation comes from the fact that while all cohorts are exposed at the same calendar year, the reform affects different cohorts at different ages. By defining treatment from the year 2000, we incorporate the full effect of the reform which began in the 2000/2001 academic year. Thus, we compare treated and counterfactual cohorts at the same ages, and the panel dimension of the data is age, rather than calendar year.

Event time is calculated relative to a^0, a "base age" which indicates the age at which treated cohorts are treated. As described below, our baseline regression focuses on a sample of base ages 30–33.[15] Event time is defined as $time = a - a^0$, which corresponds to the age since treatment. For example, by the end of $time = 3$, treated women aged 33 at the time of the reform have been treated for 4 years (from age 33–36) while the counterfactual cohort is first treated (at age 36).

By defining time with respect to a^0, the event study approach compares the age-education and age-earnings profiles among high school drop outs of different birth cohorts who are exposed to the educational reform at different ages. Thus, time corresponds to the age of an individual relative to 2000 when treatment occurred, that is $t = a - a^0$ where a^0 is the age of individual i in 2000. We estimate the following event study regression:

$$y_{it} = \sum_{k=-4}^{14} \delta_k D_{it}^{k \text{ years after reform}} + \phi treated_{c(i)} + \tau_t + \psi_{j(i)} + \gamma_{l(i)} + u_{it} \tag{1}$$

where the estimated δ_k coefficients correspond to the treatment effect comparing treated and counterfactual cohorts at a given age k years after the reform when individuals are aged a^0. The treatment variable $D_{it}^{k \text{ years after reform}}$ is defined as:

$$\begin{cases} = 1, \text{ if } treated_{c(i)} = 1 \text{ and } time = k \\ = 0, \text{ otherwise.} \end{cases}$$

[13]Section 6.6 examines the robustness of the results to the choices of δ.

[14]Similar approaches are taken in Nekoei and Seim (2018); Malkova (2018); Fadlon and Nielsen (2019), where the counterfactual group is treated in the future.

[15]Section 6.7 details the robustness of the main results to the choice of a^0.

τ_t corresponds to age fixed effects within a given base age a^0. $\psi_j(i)$ corresponds to pre-reform sector fixed effects, where sector is defined as manufacturing, public, employed in any other sector, or non-employed (no sector). $\gamma_{l(i)}$ corresponds to fixed effects for the age at which a student first left the education system and dropped out of high school, where $l = 16, \ldots, 20$. Throughout our empirical analysis standard errors are clustered at the municipality level.

The coefficients δ_k for $k = 0, \ldots, 14$ in (1) represent the impact of the educational reform at a given age k years after the reform, comparing treated and counterfactual cohorts at the same age, on one of each of these outcomes. The pre-reform coefficients $\delta_{-4}, \ldots, \delta_{-1}$ reveal whether treated and counterfactual cohorts had any existing differential pre-reform trends in the relevant outcome variable. The coefficient δ_{-1} is conventionally set to zero, such that the estimated difference is interpreted relative to the difference between treated and counterfactual cohorts in $k = -1$. That is, we fix the difference in outcomes at age $a^0 - 1$ to be constant and ask whether the differences over time are significantly different between the treated and counterfactual groups (relative to the difference between the two groups).

In the main set of results that follow, y_{it} corresponds to one of ten different outcomes of an individual i in the base age sample a^0 in time t: four education measures—a binary variable indicating enrollment in education, a binary variable indicating the completion of high school, years of completed education, and a binary variable indicating the completion of higher education, two fertility measures—the number of children and a binary variable indicating for different numbers of children, two intrahousehold measuring of gaps in education and earnings, and two labor market outcomes—log of annual labor earnings and a binary variable indicating full-time employment.

In addition to the event study regression model (1), we use the same comparison across individuals of different ages at the time of the reform to estimate a simple difference-in-differences specification:

$$y_{it} = \phi treated_{c(i)} + \beta post_t + \delta treated_{c(i)} \times post_t + \tau_t + \psi_{j(i)} + \gamma_{l(i)} + u_{it}. \tag{2}$$

Compared to the event study regression, which allows for the effect of the reform to vary over time, the difference-in-differences regression estimates the average of the post-reform coefficients δ_k in a single coefficient using the specification.

Exploiting the variation in the ages at which different birth cohorts are exposed to the same reform requires the identifying assumption that, in the absence of the reform, the education/labor market outcomes of treated and counterfactual cohorts would have evolved the same over the life cycle. This implies that had those treated at younger ages (and eventually those treated at older ages) not been exposed to the reform, they would have continued to experience the same changes in education/labor market outcomes over the same ages. Given the speed at which the law in 2000 was passed and implemented—the eventual change was proposed 28 April 2000, passed 30 June, and came into force as of 1 August—the age at which high school dropouts of different birth cohorts are treated by the educational reform is likely to be as good as random. The rapid implementation of the law and the inflexibility of the education system prior to the change (*St.meld. nr. 32 (1998-99)*) also limits the scope for anticipation.

3.3.1 Defining the sample

To assess the impacts of the education reform by gender, we focus on households—those married and cohabitating—at the time of the reform. Both household labor supply and the intrahousehold allocation of labor are crucial to understanding the impact of major policies affecting labor supply (Chiappori, 1992; Blundell and Macurdy, 1999). Consistent with this, previous literature shows that household factors are a key determinant of the labor market outcomes of women relative to men (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). By focusing on households, the vast majority of whom have children at the time of the reform, we ask to what extent second chance education impacts intrahousehold gaps in education and earnings.

Despite both being low educated, men and women in households have starkly different labor market outcomes. Although men have larger gaps in education with their partners pre-reform, Table 1 reveals that men have a considerably higher earnings in the household: while male earnings are 0.72 log points *higher* than their partner's, female earnings are 0.88 log points *lower* than their partner's.[16] This is, at least in part, due to large differences in the probability of working full-time pre-reform: while 72% of men are employed full-time, only 30% of women are. Whether returning to education widens or closes these large gaps in household earnings depends precisely on who takes up the reform and how returning to education benefits labor market outcomes.

Unsurprisingly, there also exists a strong relationship between fertility and education, where children are much more detrimental to women's education (see Figure B.1). Table 1 also reveals substantial differences in the importance of having a child before 26: while 66% of female dropouts had a birth by age 25, just 41% of male dropouts do. As the presence of children in the household clearly matters for education, we focus the main treatment group on birth cohorts who are treated in the age range 30–33. Two specific observations support this selection. First, the distribution of age of first birth peaks in the late 20s and the average age of first birth among high school dropouts is 27.5. Focusing on those who return to education from age 30 allows the average dropout's first child to begin kindergarten at the time the Norwegian government reforms the adult education system.[17] These ages represent a key point in the life cycle, and previous literature reveals the importance of child birth for the labor market outcomes of women (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). Second, in contrast with a younger

[16] While some of the men and women presented in Table 1 are in households together, we match dropouts to their partners irrespective of their partner's education and age. Thus, a considerable fraction of the partners are highly educated, reflected in the large intrahousehold gaps in education among both men and women.

[17] Indeed, the take-up of education post-reform is slightly lower among a sample of younger base ages, the vast majority of whom have a young child at home at the time the reform is introduced.

sample, we can measure completed fertility post-reform among those treated from 30–33. As the majority of the sample has children by the time of the reform, such a choice of ages allows us to assess whether later life education impacts the timing of fertility as well as completed fertility.

In addition to the selection of birth cohorts who are treated at ages 30–33, the paper isolates a sample of on-time high school dropouts by imposing three further sample restrictions:

I. The sample is restricted to those who dropped out of high school at age 20 or younger without a high school diploma. We focus on a group that is on the margin of completing high school later in life, dropouts who attended at least one year of high school but did not graduate. Note that the majority of each cohort starts high school even if it is not mandatory. For the cohorts analyzed, the cohorts consist almost exclusively of students born in Norway since it the big immigration wave took place during the 1990s.

II. The sample is restricted to those who, at time -6, have still not completed high school prior to the reform. To the extent that high school dropouts return to education at different levels prior to the 2000 reform, this will be reflected in the estimated pre-reform coefficients δ_{-4}, δ_{-3}, and δ_{-2}.

III. In order to focus on those who make real investments in human capital, we exclude dropouts who go on to complete a vocational degree under the Practical Candidate Scheme discussed in Section 2. The scheme offers an out of classroom opportunity for workers in a specific vocation to document their on the job knowledge and skills and attain a vocational high school diploma. Though certifying vocational skills in this out of classroom scheme may not necessarily reflect investment in human capital, the main results of the paper are similar including practical candidate degrees.

3.3.2 Exploiting the Variation in Treatment Across Cohorts

As the event study framework exploits variation across different birth cohorts, changes in demographic characteristics over time may affect the similarity of treated and counterfactual cohorts. For instance, treated cohorts have slightly higher levels of parental education than counterfactual cohorts (see Table H.1). One potential reason for this is that parental education levels are slowly increasing over time as younger birth cohorts become increasingly educated.[18] As such, pre-treatment differences in the composition of the sample between treated and counterfactual cohorts may affect the dynamics of the outcome variable post-reform, violating the parallel trends assumption. Abadie (2005); Blundell and Dias (2009) show that weighting the regression by an estimated propensity score accounts for such differences.[19]

Focusing on cohorts treated 3 years later limits the scope for major differences, as the cohorts are not born that far apart. However, in order to account for the possibility of minor differences in the composition of the sample of early and later cohorts, we weight the event study regression by an estimated inverse propensity score. We first estimate propensity scores predicting the probability of being in the

[18] Figure G.1 suggests this is the case as the on-time completion rate increases from birth cohorts 1961–1965 to 1976–1980.
[19] Similar approaches are taken in Mastrobuoni and Pinotti (2015); Pohlan (2019); Goodman-Bacon and Cunningham (2019).

Table 1: Describing the Estimation Sample

	Female	Male
	(1)	(2)
Parental Education:		
Frac. at least one parent highly educated	0.302	0.311
	(0.459)	(0.463)
Fertility and Household:		
Frac. first birth age 25 or younger	0.659	0.408
	(0.474)	(0.492)
Number of children	1.990	1.607
	(0.951)	(1.015)
Frac. with children	0.924	0.841
	(0.266)	(0.365)
Demographic:		
Base age	31.61	31.61
	(1.114)	(1.116)
Frac. born in Norway	0.971	0.964
	(0.167)	(0.186)
Labor Market:		
Frac. employed full time	0.301	0.715
	(0.459)	(0.451)
Log of labor earnings	11.54	12.31
	(0.990)	(0.630)
Household gap in log earnings	-0.875	0.716
	(1.158)	(1.194)
Education:		
Years of education	12.16	12.16
	(0.877)	(1.040)
Frac. completed HS	0.0872	0.126
	(0.282)	(0.332)
Frac. completed higher educ.	0.0320	0.0288
	(0.176)	(0.167)
Age first dropped out	17.93	17.85
	(1.137)	(1.146)
Household gap in years of education	-0.184	-0.434
	(2.453)	(2.521)
Individuals	11629	6033

Sample: women & men of base ages 30–33 who dropped out of high school as described in Section 3.3.1 and have partners at the time of the reform. All variables measured at time -1, unless otherwise indicated. Sample of column (1) is female dropouts while sample of column (2) is male dropouts. Table reports the sample average, with standard deviation in parentheses. Household gaps in earnings and education are measured matching women/men to their partners, irrespective of their age or education level. Household earnings gaps are measured only for partners who have earnings in -1.

treated group compared to the counterfactual group. The inverse propensity score is then used to weight the event study regression by

$$treated\frac{p}{P\left(X_i\right)} + \left(1 - treated\right)\frac{1-p}{1-P\left(X_i\right)} \tag{3}$$

where p is the unconditional probability of treatment and $P\left(X_i\right)$ is the conditional probability of treatment (the propensity score), see Mastrobuoni and Pinotti (2015). Intuitively, this method increases the weight of those in the counterfactual group with similar characteristics to the treated group and weights down those in the counterfactual group with differences in the estimated propensity score.

In our application, the propensity score is time-invariant, using data one year prior to the reform in $time = -1$, and then used in all time periods from $time = -4, \ldots, +14$. It is estimated by matching individuals in the treated and counterfactual groups using a rich set of covariates: 3 binary variables indicating at least one highly educated parent, first birth at age 25 or younger, and married (measured at -1); binary variables for age of first drop outs (5 categories, 16–20); binary variables for broad field of study left high school with (10 broad categories); the number of children (measured at -1, including zeros); birth municipality; 2 digit industry dummies (measured at -1); and base age. Appendix Figure H.1 plots the estimated propensity scores, revealing substantial overlap between the two groups. This suggests that, on the whole, the two groups are relatively similar in terms of the matching variables. We report non-bootstrapped standard errors throughout.[20]

A limitation of exploiting variation across ages is that at the same time t, treated and counterfactual cohorts are in different calendar years. For instance, at $t = 0$, the treated cohort is in year 2000 while the counterfactual cohort is in year 1997. This implies that two people who live in the same local area face different economic conditions at the same point in event time. Likewise, two people who are in the same employment sector face different economic conditions. Note that time and year effects cannot be separated, as there is no group in the estimation sample which is never treated. To account for such differences, we make use of data on the entire population and construct, separately for each gender, two controls for labor market conditions which we then merge into our base age sample. We use data on all workers aged 25–54 who have not completed high school from 1993–2014 to construct two controls to account for differences in labor market conditions across years: (a) $municipality \times year$ and (b) $initial\ field\ of\ study \times year$. The calculation of these labor market controls is described in further detail in Appendix H.3. In addition, Section 6.4 designs a robustness check to address the issue that treated and counterfactual groups face different calendar years using a group of individuals who are never treated by the reform: high school

[20] Busso, DiNardo, and McCrary (2014) show that with a large number of observations, non-bootstrapped standard errors are a reasonable approximation when weighting by the inverse propensity score. While typically bootstrapping does not produce valid standard errors in matching, this is not the case when weighting by the inverse propensity score (Abadie and Imbens, 2008). Mastrobuoni and Pinotti (2015) establish the similarity between bootstrapped and non-bootstrapped standard errors.

graduates. Using high school graduates permits the estimation of the impact of the education reform on the education of high school dropouts, who are treated in 2000, to high school graduates, who serve as the counterfactual in 2000.

4 The Impact on Education by Gender and Intrahousehold Gender Gaps

In this section we present the estimation results of the impact of the reform on adult education investments focussing on intrahousehold gender gaps in these investments. In the next section we go on to examine the main causal impact on labor market outcomes for all high school dropouts.

Figure 5: The Estimated Impact of the Educational Reform on Enrollment in Education by Gender

(a) Enrolled in Education—Female

(b) Enrolled in Education—Male

(c) Completion of High School—Female

(d) Completion of High School—Male

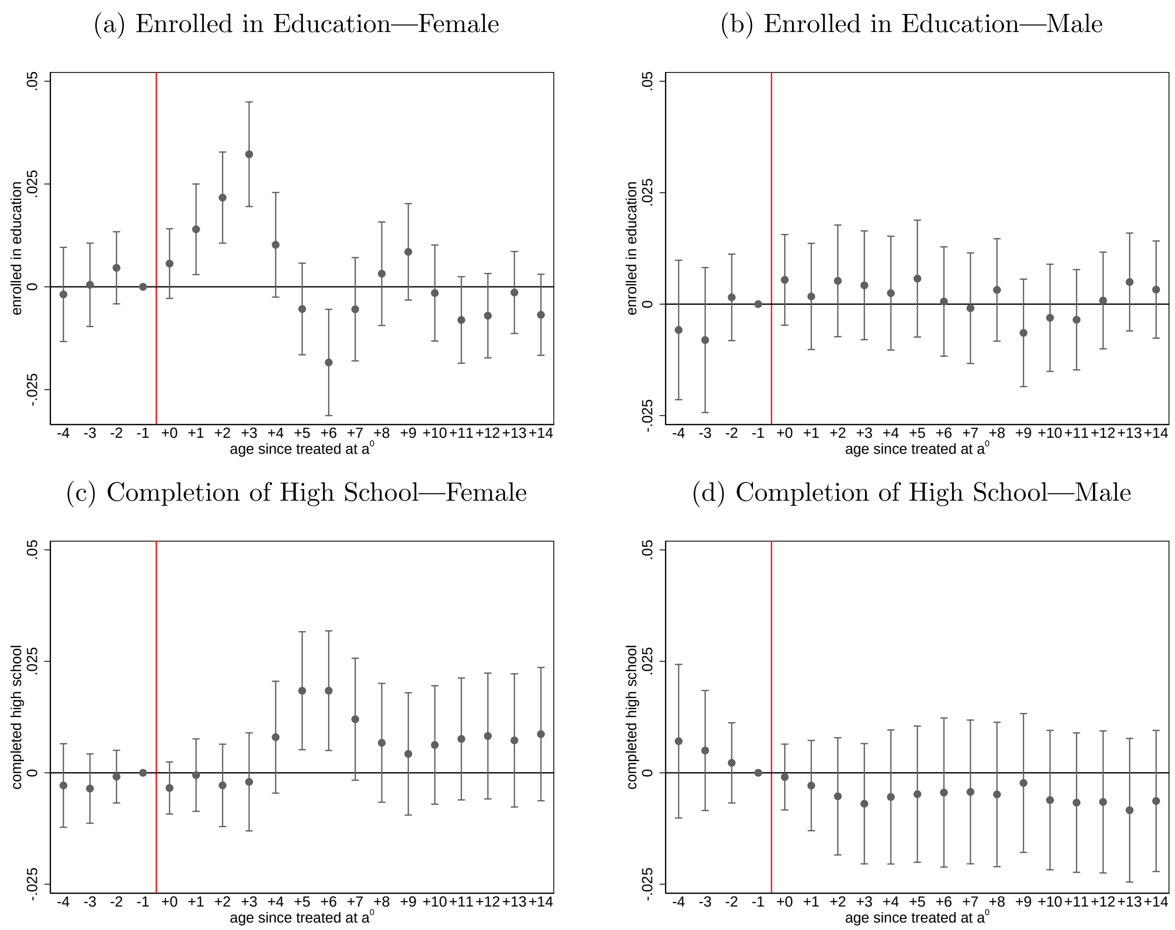


Figure plots estimates of equation (1), weighted by estimated propensity score in -1 as described in Section 3.3.2. Each point represents the difference in the outcome variable between treated (those treated at younger ages) and counterfactual (those treated at older ages) cohorts at a specific point in time, relative to the same difference in -1 (at age $a^0 - 1$). Panels (a)–(b) define education as equal to 1 if enrolled in education. Panels (c)–(d) define education as equal to 1 if completed the final year of high school. Vertical line between -1 and $+0$ corresponds to the age at which treated cohort is treated by the education reform. Sample of female and males of base ages 30–33. 95% confidence interval reported.

Figure 5 plots the estimates of equation (1) for enrollment in education (panels a and b) and high school graduation (panels c and d). Soon after the reform, from time $+1$, enrollment increases among treated women.[21] Enrollment continues to increase and peaks 3 years after treated cohorts are impacted by the reform. In contrast, enrollment among men remains unchanged and is never significantly different from zero. As the counterfactual cohorts are eventually treated by the same reform, enrollment is significantly lower 6 years after treated cohorts are treated among women. Importantly, enrollment is never significantly different pre-reform among women or men, suggesting that before the education reform, both treated and counterfactual cohorts return to education at the same rates. In line with the post-reform increases in enrollment, high school graduation increases post-reform among treated women and peaks 5–6 years after the introduction of the subsidy. Five years after the beginning of reform period, high school completion among treated increases by 1.83 ppt (21% of the mean in −1).[22] In the longer run, the differential impact on high school completion of treated women is not significantly different from zero, suggesting that the timing of treatment does not impact the probability of returning to high school. As with enrollment, there are no significant changes in the high school graduation among treated men.

Figure 6 plots the impact of the reform on years of education (panels a and b) and higher education (panels c and d). Similar to what is seen in Figure 5, years of education increases by 0.08 years (0.7%), an increase which is significant at 1%. Years of education, in contrast to high school, *remains higher* among treated compared to counterfactual individuals, an effect which is significant at the 5% level. The difference between the two sets of results can be explained by differences in the probability of continuing with higher education among the treated cohorts, reported in Figure 6c. While some individuals in the counterfactual group also continue into higher education, as evidenced by the decline in the estimated coefficient from its peak at +6, the probability of continuing past high school is higher among treated women. That is there are are persistent increases in the probability of completing higher education among treated women: 14 years after the reform, treated women are 1.4 ppt more likely to have higher education (44% of the mean in −1). Because years of education and higher education remain unchanged among men, the education levels of low-educated women increase relative to low-educated men. As before, estimated pre-event coefficients are small in magnitude and not significantly different from zero, indicating that we cannot reject that the age-education profiles of treated and future treated cohorts are parallel pre-reform.

Differences in the completion of higher education between women treated by the education reform at different ages reveal that the age of returning to high school matters for the probability of continuing further in the education system. Such results suggest that in order to increase educational attainment later in life, policies encouraging individuals to return to finish high school at younger ages would have

[21] Appendix I offers an alternative empirical approach to estimate the treatment effect of the adult education expansion on enrollment in education. Similar to a regression discontinuity design, there is a sharp jump in enrollment for females at age 31 (one year post-reform) for birth cohorts who are already treated by the reform, relative to cohorts who are not yet treated by the reform. At the same time, there exist no significant differences in the probability of enrollment at age 29, one year prior to the reform, at the cutoff. As seen in Figure 6, there are no corresponding changes in the enrollment of men.

[22] Separating by academic and vocational high school reveals similar patterns, although vocational high school begins to increase slightly earlier.

Figure 6: The Estimated Impact of the Educational Reform on Education by Gender

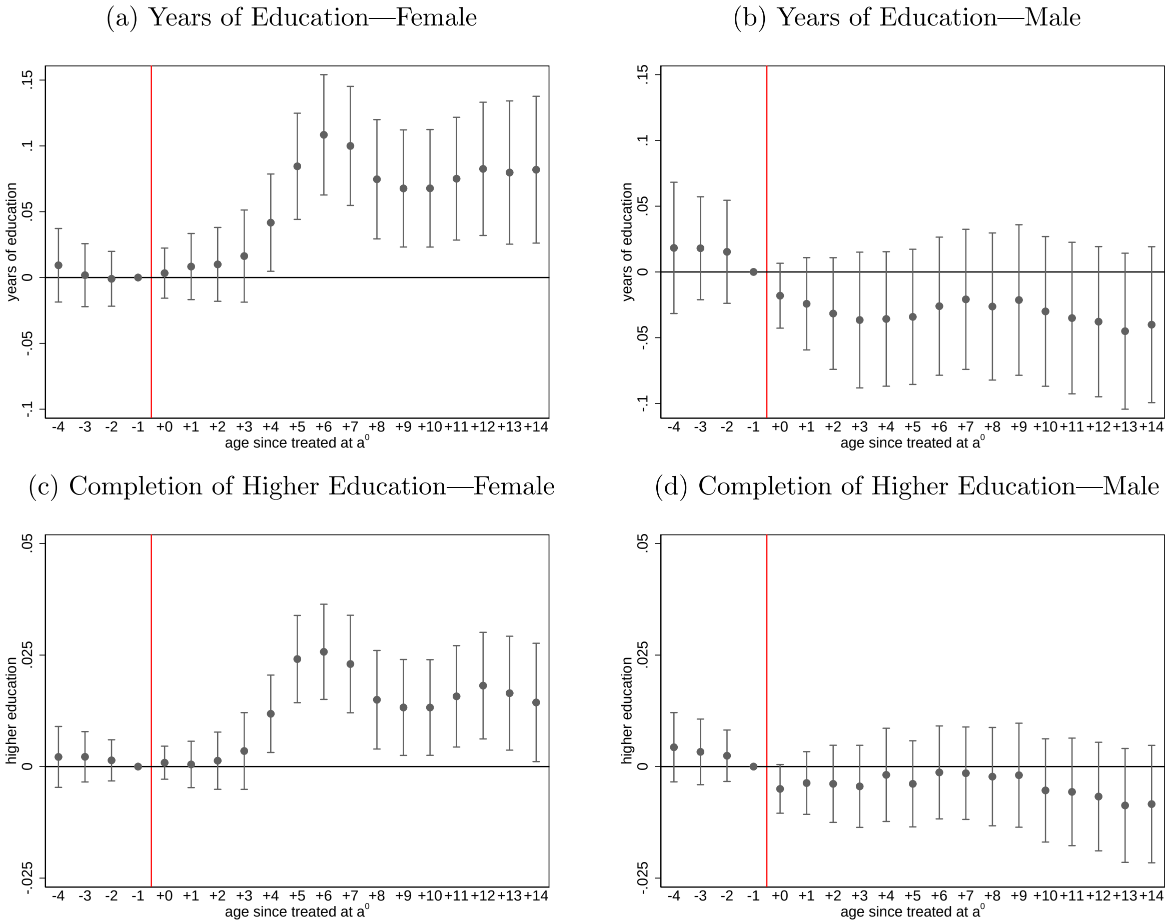


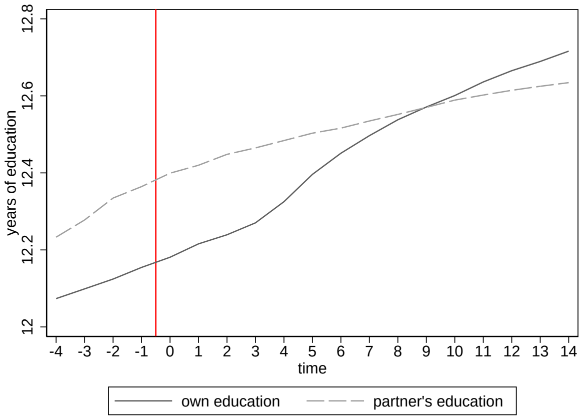
Figure plots estimates of equation (1), weighted by estimated propensity score in -1 as described in Section 3.3.2. Each point represents the difference in the outcome variable between treated (those treated at younger ages) and counterfactual (those treated at older ages) cohorts at a specific point in time, relative to the same difference in -1 (at age $a^0 - 1$). Panels (a)–(b) define education as the number of years of education. Panels (c)–(d) define education as equal to 1 if completed higher education. Vertical line between -1 and $+0$ corresponds to the age at which treated cohort is treated by the education reform. Sample of females and males in a household of base ages 30–33. 95% confidence interval reported.

larger effects on higher education. As counterfactual cohorts are exposed to the same reform 3 years later, they are also 3 years closer to retirement. As such, women in the counterfactual group may find it too costly to forego additional years of earnings to return to higher education while the treated group may not. However, age of exposure seems to matter less for returning to high school, as both early and later treated women complete high school at roughly the same rates.

4.1 Impact of the Educational Reform on Intrahousehold Gender Earnings Gaps

Figure 7: Average Education and Earnings of Treated Females and Their Partners

(a) Education



(b) Labor Earnings

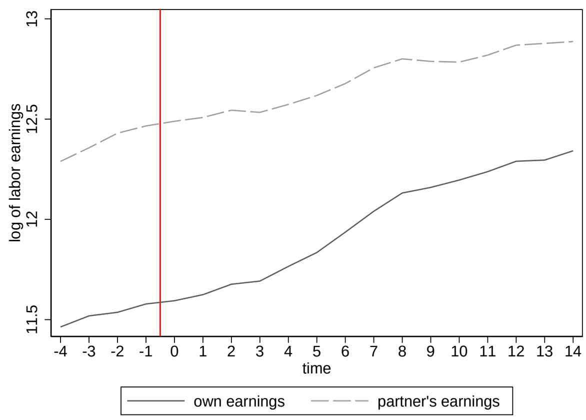


Figure plots the unconditional average difference in education between treated women (own education) and their partner. Couple status measured at the time of reform. Partner of treated women measured as the partner at the time of reform, and followed irrespective of whether the household remains together or not. Panel (a) measures education as years of education while panel (b) measures annual earnings from employment. Vertical line between -1 and $+0$ corresponds to the age at which treated cohort is treated by the education reform. Sample of females of base ages 30–33.

Figure 7 shows how the large intrahousehold gaps in education and labor earnings between treated women and their partners are reduced by the reform. The within household comparison between women and their partners over time resembles an extensive literature assessing the child wage penalty between new mothers and fathers (Angelov, Johansson, and Lindahl, 2016; Bütikofer, Jensen, and Salvanes, 2018; Kleven, Landais, and Søgaard, 2019). Couples are those who were together at the time of the reform, and partner outcomes are measured irrespective of whether or not the couple remains together over time.[23] Initially, there is a large gender gap in educational attainment within treated households, where men are more educated than their female partners (Figure 7a). However, as treated women experience a strong increase in education post-reform while men are largely unaffected, this gender gap reverses: by +14, women are more educated than their partners.

Figure 7b focuses on how labor earnings evolve over time within the household. Prior to the reform, there is also a large gender gap in earnings of 0.89 log points. As treated women return to education post-reform, the intrahousehold earnings gap closes over time to just 0.55 log points by +14. Although far

[23]The probability of divorce is not significantly different over the period between treated and counterfactual women.

from being eliminated, the intrahousehold earnings gap closes by 38% post-reform among treated women. Such stark changes in the within household gender gap suggests that returning to education can improve labor market outcomes of married women after child birth on "the other side of the mountain" (Goldin, Kerr, and Olivetti, 2021).

Table 2: The Estimated Long-Run Impact of the Reform on Intrahousehold Gaps in Education and Labor Earnings, Averaged Over Post-Reform Period from +8–+14

	(1) Log of own earnings− partner earnings	(2) Own education− partner education, years
Treated × Post	0.0488**	0.1266***
	(0.0240)	(0.0457)
N Couples	9949	9949
Avg. Reduced Form Outcome in −1	-0.880	-0.209

Sample of treated and counterfactual women, base ages 30–33, with a defined measure of household gaps in labor earnings & education at any point from +8–+14. Couple status measured at the time of reform. Partner of treated women measured as the partner at the time of reform, and followed irrespective of whether the household remains together or not. Sample period corresponds to the long-run impact of the reform on education/earnings, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Column (1) measures annual earnings from employment and column (2) measures years of education. Coefficients interpreted relative to omitted −1. Regressions include controls for labor market conditions as defined in Section 3.3.2.

While Figure 7 reveal strong impacts of the reform on household gaps in education and labor market outcomes, a remaining question is how treated women fare relative to counterfactual women. Such a comparison asks how the timing of when women return to education impacts intrahousehold gender gaps in education and earnings. Indeed, the timing of when women return to education matters considerably: treated women fare significantly better, as they close intrahousehold gaps in education and earnings even more relative to counterfactual women. Table 2 estimates the difference in the intrahousehold gaps between treated and counterfactual women in the long-run, revealing that returning to education earlier in the life cycle can close both education and earnings gaps between women and their partners. In the long-run, treated women have intrahousehold education gaps which are 0.13 years smaller relative to counterfactual women. These gains in education close 61% of the pre-existing gap in education pre-reform. At the same time, treated women have household earnings gaps which are 0.05 log points smaller relative to counterfactual women, corresponding to 6% of the pre-reform gap. Both differences are statistically significant, revealing that the timing of when married women return to education matters considerably for intrahousehold gaps in both education and earnings.

4.2 How Does Education Close Intrahousehold Earnings Gaps?

Do women treated at younger ages, who close the intrahousehold gaps in education and earnings at significantly higher rates, also experience a decline in fertility? If so, the joint impacts of education on labor market outcomes and future fertility may be an important explanation behind why women treated at younger ages fare better relative to their partners. The education literature estimates sizable reductions in fertility following additional education in addition to increased labor market prospects (Black, Devereux, and Salvanes, 2008). Given that women have two children prior to the reform on average, we estimate whether returning to education impacts the decision to have another child. As both treated and counterfactual cohorts are at least age 44 by +14, the vast majority of births are completed by the end of the sample period.

As described previously, Figure B.1 presents the relationship between fertility and later life education separately for women and men. Figure B.1 reveals that childbirth is much more disruptive to on-time education for women compared to men: there exists a much stronger relationship between age of first birth and age of high school completion for women relative to men. Interestingly, women who complete high school from 26–30 also exhibit a second peak in age of first birth in their late 20s, suggesting that later life education and childbirth may also be linked.

Figure 8 plots the impacts of the education reform on fertility, comparing treated to counterfactual women.[24] Treated women have significantly lower fertility rates as a result of the reform, revealing that less fertility can explain a substantial portion of the closing of intrahousehold earnings gaps. Treated women experience significant declines in the number of children they have in the long-run relative to counterfactual cohorts, a decline of 3% relative to the pre-reform mean. Panel b reveals that returning to education leads women to forgo having a third child, and the probability of having at least 3 children is 8% lower.[25]

Unsurprisingly, there exists a strong negative relationship between the number of children and labor market outcomes among women (see Figure K.1). Consistent with this tradeoff, Appendix K.2 reveals that, in addition to having fewer children in the long-run, treated women are significantly more likely to be employed full-time. As such, the significant declines in fertility, in particular not having a third child, suggest that a substantial portion of the impact of second chance education on intrahousehold earnings gaps is due to the joint decision of fertility and labor force participation.

5 The Labor Market Impacts of the Educational Reform

While Section 4 focuses on households, the results below provide a broader understanding of the labor market impacts of second chance education among all dropouts, both those with and without a partner. While the intrahousehold adjustment is an important margin which is impacted by returning to education, it is equally important to understand how all dropouts are affected by the education reform. Doing

[24]The education reform affects fertility, but not household formation as there are no significant differences in the probability of being married, getting divorced, or remaining single.

[25]As the average woman in the sample has 2 children to the reform, the probability of having any children remains unchanged by the reform. In addition, other margins of having 2–6 children as in panel (b) reveal no significant differences.

Figure 8: The Estimated Impacts of the Educational Reform on Female Fertility

(a) Number of Children (b) Having at Least 3 Children

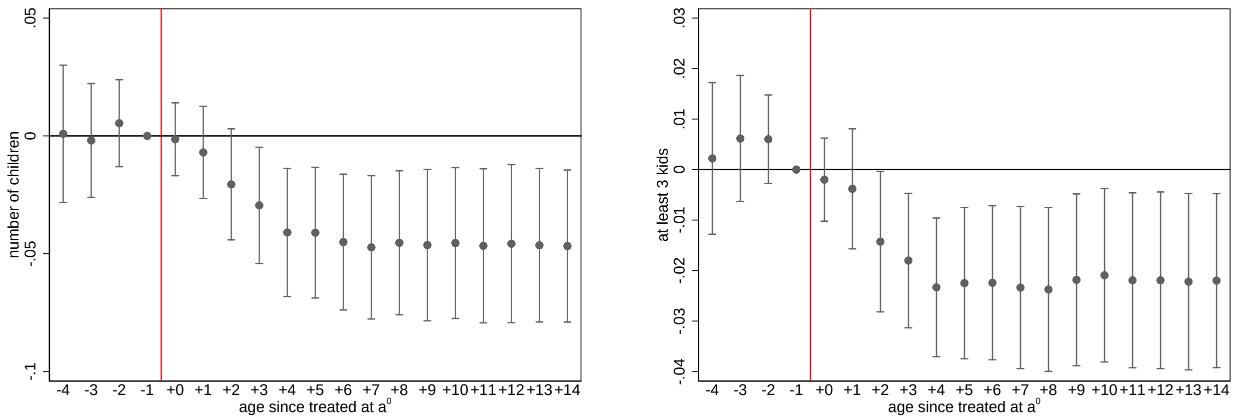


Figure plots estimates of equation (1), weighted by estimated propensity score in -1 as described in Section 3.3.2. Each point represents the difference in the outcome variable between treated (those treated at younger ages) and counterfactual (those treated at older ages) cohorts at a specific point in time, relative to the same difference in -1 (at age $a^0 - 1$). Panels (a) measures the number of children a woman has while panel (b) measures a variable equal to 1 if a woman has at least 3 kids. Vertical line between -1 and $+0$ corresponds to the age at which treated cohort is treated by the education reform. Sample of females in a household of base ages 30–33. 95% confidence interval reported.

so confirms that the previous differences in response to the education reform between men and women are not driven by differences in the composition of dropouts due to selection into household formation. Indeed, Appendix L confirms that while men remain unaffected by the policy change all women return to education. However, the magnitude of the increase in education is slightly smaller among all women, suggesting a slightly greater response of women in households at the time of the reform.

Corresponding with these observed increases in education, labor earnings (Figure 9a) and full-time employment, defined as at least 30 hours per week, (Figure 9b) also increase among women. The timing of the changes in labor market outcomes coincides with changes in education: as treated women complete education, labor market outcomes begin to increase and when counterfactual cohorts return to education, the increases in labor market outcomes stabilize or even decline. In the long run 14 years after the treated cohorts are treated, earnings increase by roughly 5% and employment increases by 3 ppt (8.3%) relative to cohorts treated at even older ages. Increases in earnings are similar irrespective of whether or not benefits are included in the measure of labor earnings.

Table 3 presents the post-reform impacts across four outcomes: annual earnings, hourly labor earnings, a binary variable indicating full-time employment, and years of education. These regressions reflect the labor market responses seen in Figure 9. The results in Table 3 correspond to the difference-in-differences regression of regression (2), where the reported estimates represent the interaction between an indicator for treated and the post-reform period. By looking at the impacts measured from +8 to +14, the difference-in-differences estimates provide the long-run impacts of the reform. As cohorts treated at older ages always serve as a counterfactual for early treated cohorts and their treatment status never changes, the reduced

Figure 9: The Estimated Impact of the Educational Reform on Female Labor Market Outcomes

(a) Log of Labor Earnings

(b) Employed Full-Time

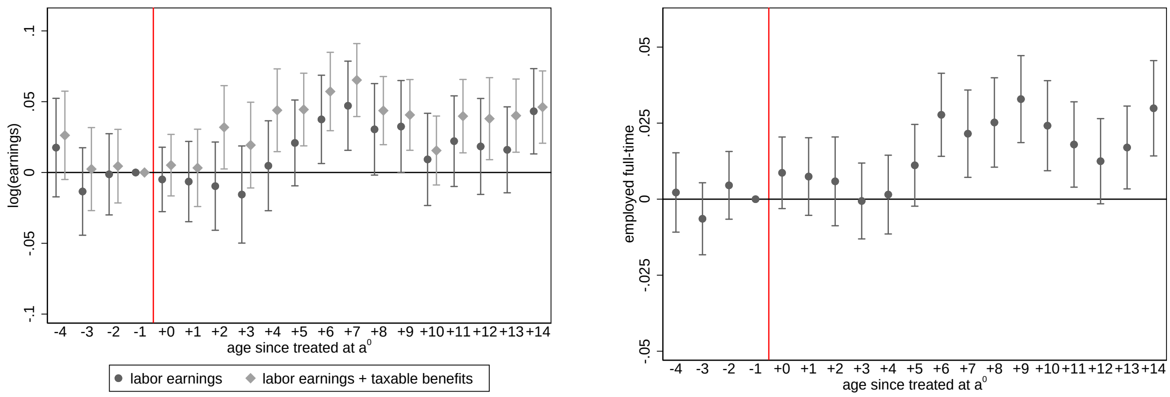


Figure plots estimates of equation (1), weighted by estimated propensity score in -1 as described in Section 3.3.2. Each point represents the difference in the outcome variable between treated (those treated at younger ages) and counterfactual (those treated at older ages) cohorts at a specific point in time, relative to the same difference in -1 (at age $a^0 - 1$). Controls for *municipality* $\times$ *year* and *initial field of study* $\times$ *year* fixed effects as described in Section 3.3.2. Two earnings measures in panel (a) correspond to the log of annual labor earnings and the log of annual labor earnings also including sickness, unemployment, and parental leave benefits. Panel (b) defines employment as equal to 1 if working at least 30 hours per week. Vertical line between -1 and $+0$ corresponds to the age at which treated cohort is treated by the education reform. Sample of females of base ages 30–33. 95% confidence interval reported.

form coefficients of Table 3 estimate the average of δ_k for $k \geq 8$ in equation (1).

Compared to the increase in annual labor earnings in Figure 9a, the increase in *hourly* labor earnings is much smaller, and not significantly different from zero. This suggests that the bulk of the labor market impact of later life education comes through employment response rather than through an increase in wages. Treated women see increases in full-time employment post-reform and, on average over the post-reform period, full-time employment is 6.5% higher relative to a low pre-reform level of 0.36. Indeed, the sample of high school dropouts is not that attached to the labor force. 31% of all women are classified as outside of the labor force in -1 (column (1), Table 4).

To further understand the increase in full-time employment, Table 4 presents the estimated employment response across 4 variables corresponding to different measures of labor market status: outside of the labor force, employed less than 20 hours per week, 20–29 hours per week, and full-time employment for comparison. The results of Table 4 reveal whether the observed increase in full-time employment in Table 3 originates from increasing labor market attachment (a decline in the probability of being outside the labor force) or increasing hours worked (a decline in employment less than full-time). Though the reduction in the probability of being outside the labor force is not significant, increases in full-time employment originate from women joining the labor force and declines in the probability of working less than 20 hours per week (columns (1) and (2) respectively).

Our results point to key responses in full-time employment among treated women. We might also be interested in differences by occupation. Appendix Table M.1 displays the distribution of occupations between treated and counterfactual women in +14 using occupation data available from the mid 2000s.

Table 3: The Estimated Long-Run Impact of the Educational Reform on Female Labor Market and Education Outcomes, Averaged Over Post-Reform Period from +8–+14

	Labor Market Outcomes			Education	
	(1) Log Annual Earnings	(2) Log Hourly Wage	(3) Employed Full Time	(4) Years of Education	(5) Higher Education
Treated × Post	0.0311***	0.0112	0.0234***	0.0414**	0.0104**
	(0.0100)	(0.0111)	(0.0063)	(0.0176)	(0.0043)
N	150654	150654	189301	189301	189301
Avg. Reduced Form Outcome in −1	11.813	4.892	0.361	12.215	0.041

Sample of treated and counterfactual women, base ages 30–33. Sample period corresponds to the long-run impact of the reform on education/earnings, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Column (1) additionally restricts to women who have positive hours worked in a given year. Column (1) measures annual earnings from employment and column (2) measures hourly labor wages, annual earnings in column (1) divided by annual hours worked. Column (3) measure employment as equal to 1 if working more than 30 hours per week. Column (4) measures years of education. Coefficients interpreted relative to omitted −1. Regressions include controls for labor market conditions as defined in Section 3.3.2.

Table 4: The Estimated Long-Run Impact of the Educational Reform on Different Margins of Female Employment, Averaged Over Post-Reform Period from +8–+14

	(1) Outside of L.F.	(2) Employed less than 20 hrs/week	(3) Employed 20–29 hrs/week	(4) Employed Full Time
Treated × Post	-0.0062	-0.0135**	-0.0009	0.0234***
	(0.0070)	(0.0057)	(0.0054)	(0.0063)
N	189301	189301	189301	189301
Avg. Outcome in −1	0.311	0.180	0.148	0.361

Sample of treated and counterfactual women, base ages 30–33. Sample period corresponds to the long-run impact of the reform on employment, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Column (1) measures outside the labor force as equal to 1 if working 0 hours. Columns (2)–(4) measure employment as equal to 1 if working less than 20, 20–29, and more than 30 hours per week respectively. Coefficients interpreted relative to omitted −1. Regressions include controls for labor market conditions as defined in Section 3.3.2.

While there is a sizable causal effect of later life education on labor force participation among women, the *distribution of occupations* between women treated at different ages in +14 is broadly similar. As such, Table M.1 suggests that labor force participation is the primary reason behind the large labor market returns observed in Table 3.

5.1 Differences in the Employment Response by Pre-Reform Labor Market Attachment and Children in the Household

Appendix N reveals important differences in the employment response across those with low, medium, and strong levels of attachment to the labor market pre-reform. While those with low pre-reform attachment see significant declines in the probability of remaining outside of the labor force in the long-run, those with some and strong attachment see signficant gains in full-time employment post-reform (Table N.1). At the same time, increases in employment are concentrated among women with children, in particular those with 1 child, who are less attached to the labor force pre-reform while women with no children see much smaller changes in labor market outcomes (Table N.2).

6 Heterogeneity and Robustness of Results

The sections below detail the fields of study which female high school dropouts return to study (Section 6.1), the heterogeneity of the baseline results (Section 6.2), and further establishes the similarity of economic conditions between the treated and counterfactual cohorts (Section 6.5). Sections 6.6 and 6.7 detail the robustness of the results to the choice of δ, the number of years that the counterfactual cohorts are eventually treated, and a^0, the base ages of the sample.

6.1 Differences by Field of Study

Table O.1 reports the highest attained degree 14 years after the reform among women who have graduated high school. Degrees are presented by the narrow field of study of the degree, for instance, nursing within the broad field of healthcare. Across both bachelors and high school degrees, female dropouts return to finish degrees in healthcare, primarily nursing and carework which represent over 7% degrees, and teaching of young and middle school aged children. Such increases are similar to what is seen in the community college literature in the US, which documents large returns to community college programs in healthcare (Stevens, Kurlaender, and Grosz, 2018; Grosz, 2020). In addition, some return to general high school, which leads to higher education, but do not go onto complete higher education by +14.

Table O.2 asks how the significant increases in higher education among women seen in Figure 6c vary by the most common fields of study. The increase in higher education is primarily driven by increases in the completion of higher education in healthcare. In contrast, the completion of higher education to become a teacher is unchanged post-reform. All other fields besides healthcare and teaching also increase, though the increase is not statistically significant.

6.2 Heterogeneity in Returning to Education Post-Reform

Figure Q.1 examines the importance of two pre-determined factors in the estimated effects of the education reform on education: parental education and age of first birth. Descriptive results in Section F and Section B reveal that parental education matters in returning to education and that childbirth and education are strongly correlated for women. While such differences are not statistically significant, returning to education is stronger among those who have their first child at 25 or younger (Figure Q.1b) while results are similar among high and low educated families (Figure Q.1a).

Figures Q.2 and Q.3 examine the importance of two additional factors: cognitive ability (only available for men) and the importance of oil in the local labor market following the discovery of oil in Norway in 1969.[26] Both cognitive ability and oil seem to matter little for the estimates of returning to education. While the returns to education may differ between high/low oil areas (Cascio and Narayan, 2015), the presence of oil does not matter for returning to education for either women or men.

6.3 Robustness to Alternative Staggered Difference-in-Differences Estimators

Recent papers emphasize the importance of negative weighting in a staggered difference-in-differences approach with heterogeneous treatment effects (Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2024). While equation (1) exploits variation in the age different cohorts are treated by the education reform to estimate the importance of a window of opportunity in returning to education, our empirical strategy always compares treated cohorts (for whom $treated_{c(i)} = 1$ throughout the period) and counterfactual cohorts (for whom $treated_{c(i)} = 0$ throughout the period). As our strategy does not exploit a fully staggered difference-in-differences approach, the estimated δ_k coefficients of equation (1) should be similar to alternative estimators which directly address issues such as negative weighting and heterogeneity of treatment effects. Compared to a TWFE model which fully exploits variation in the timing of treatment, these issues are not as problematic in our empirical strategy which always provides a constant comparison of two different cohorts over time.

Figure P.1 estimates the imputation difference-in-difference method as in Borusyak, Jaravel, and Spiess (2024), which estimates treatment effects which are robust to unrestricted heterogeneity and imputes counterfactual outcomes among all treated observations. Estimates are similar in magnitude, and exhibit similar patterns over time: while the impacts of the adult education reform fade out for high school, there

[26]Following the discovery of oil, there was substantial geographic dispersion in how important oil was to different local labor markets (Løken, Mogstad, and Wiswall, 2012; Bütikofer, Dalla-Zuanna, and Salvanes, 2018) and the shock affected men and women differently (Bennett, Ravetti, and Wong, 2021).

are persistent and statistically significant effects for both years of education and higher education. Because our empirical strategy does not fully exploit variation in the timing of treatment, our results are robust to recent methodological advances in the staggered difference-in-differences literature.

6.4 Robustness to Using On-Time High School Graduates as Counterfactual

Figure 10 reveals how the education reform enables high school dropouts to close the pre-existing education gap with high school graduates who leave the education system after graduating high school. While using high school graduates as a counterfactual has its limitations, for instance Figure E.1 reveals differences in cognitive ability between graduates and dropouts while Section 2.3 reveals age-earnings profiles which diverge over the life cycle, they are unaffected by the introduction of the education reform in 2000 as they have already graduated high school. As high school graduates are likely to be positively selected on both observable and unobservable dimensions, using them as a counterfactual is likely to understate the extent to which dropouts can close the gap in education and earnings with graduates.

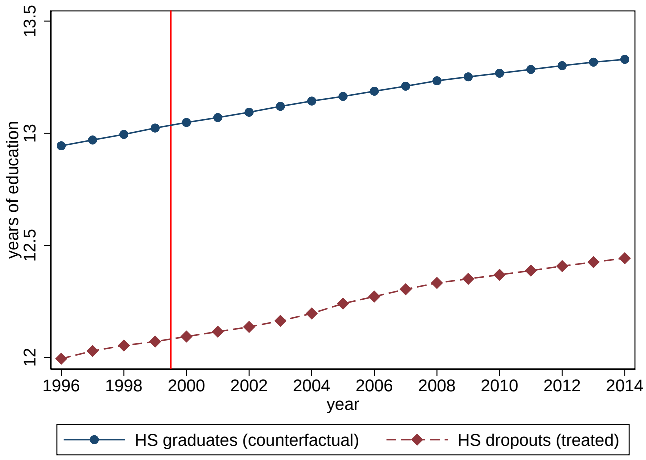
Figure 10 reveals that while on-time graduates and dropouts have different levels of education (Figure 10a), they have similar trends in years of education from 1996–2000, when the reform was introduced. Post-reform, educational attainment of high school dropouts increases considerably more relative to the educational attainment of high school graduates. This increase of education by 0.05 years is stable over time, and is statistically significant (Figure 10c). Using high school graduates as the counterfactual produces remarkably similar increases in years of education as those seen in Figure 6a, which uses high school dropouts treated at even later ages as the counterfactual. Finally, while there are some suggestions that the earnings of high school graduates are on an increasing trend relative to dropouts, as evidenced by the negative slope of the pre-reform coefficients (Figure 10d), high school dropouts close the pre-existing gaps in earnings after returning to finish high school.

6.5 Labor Market Conditions Between Treated and Counterfactual Cohorts

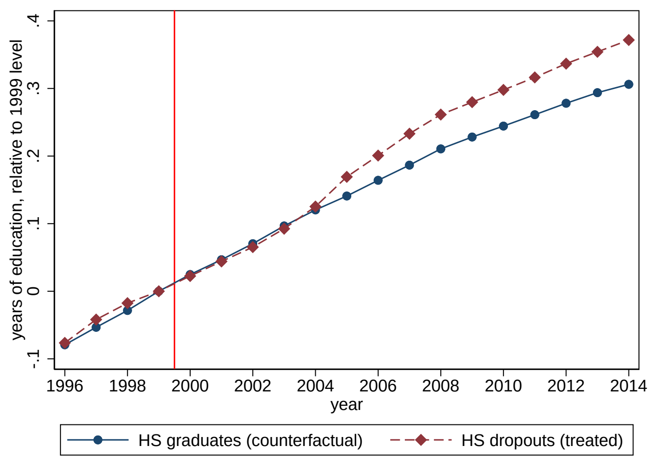
While women return to education following the reform, the reform has no discernible impact on the education of men and effects for men are imprecisely estimated (see Appendix L). Table R.1 replicates the results of Table 3 for men. While the fixed effects described in Section 3.3.2 capture differences in labor market conditions between treated and counterfactual cohorts, it may be that other changes over time are not captured by the two included controls. If this were the case, and education was not the driving force behind the increased labor market outcomes of treated women, then similar increases in earnings and employment would be observed for men whose education is largely unchanged. Reassuringly, there are no significant changes in any measure of earnings or employment status for men, reinforcing that the increases in earnings and employment among women are due to increases in later life education.

Figure 10: The Estimated Impact of the Educational Reform on Education and Labor Market Outcomes, Using On-Time Graduates as the Counterfactual

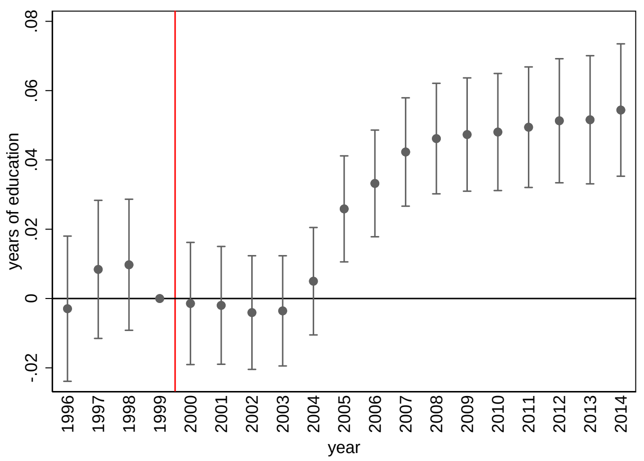
(a) Years of Education



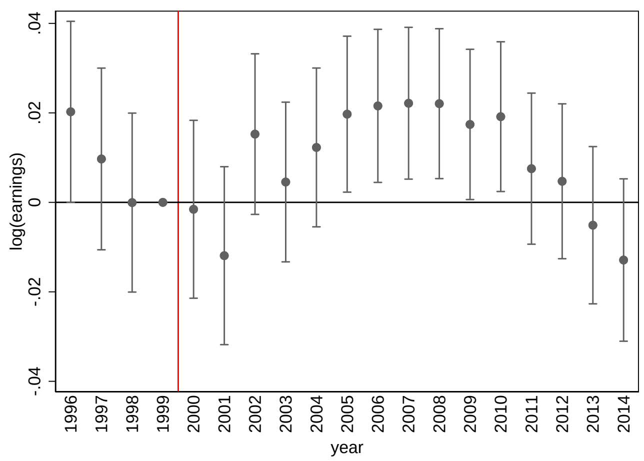
(b) Years of Education, Fixing the Level in 1999



(c) Event Study Estimates, Years of Education



(d) Event Study Estimates, Log of Earnings



Sample of females aged 30–33 in 2000. Panels (a) and (b) plot the (unconditional) average years of education of high school dropouts and high school graduates who leave the education system after graduating high school from 1996–2014. Panel (b) takes the level of education in 1999 as fixed for both groups respectively. Panels (c) and (d) plot the estimated difference between dropouts and graduates, with an estimating equation given by: $y_{iy} = \sum_{k=-4}^{14} \delta_k (HSdropout_i \times \tau_y)^{k\ years\ after\ reform} + \phi HSdropout_i + municipality_{m(i)} \times \tau_y + married_i \times \tau_y + any\_children_i \times \tau_y + educated\_parent_i \times \tau_y + u_{iy}$, for individual i in year y. Panels (c) and (d) plot δ_k coefficients, the average difference over time between high school dropouts and graduates. Battery of controls interacted with year including municipality fixed effects, married (= 1 if married), any children (= 1 if have children), and having an educated parent (= 1 if at least one parent graduated high school)—where the fixed effect is measured pre-reform in 1999—included to account for pre-reform differences between dropouts and graduates. Vertical line between 1999 and 2000 corresponds to the year at which treated cohort (high school dropouts) is treated by the education reform. 95% confidence interval reported in panels (c) and (d).

6.6 Robustness of Results to Varying δ

Figure R.1 examines how the results change by varying δ, the number of years which have passed until the counterfactual cohorts are exposed to the same reform. The Figure examines how using a counterfactual group who is exposed even earlier ($\delta = 2$) and a counterfactual group who is exposed even later ($\delta = 5$) affects the results on high school completion and higher education compared to the baseline case ($\delta = 3$).[27] In the longer run, the estimated effects on high school completion are indistinguishable when using the three different levels of δ. However, the timing of when increases in high school completion fade out changes: using a counterfactual group treated even earlier corresponds to an earlier decline in the completion of high school while using a counterfactual group treated even later corresponds to a longer positive impact on high school completion. A similar picture emerges for the estimated effects on higher education.

Such variation in the timing of the estimated effects is consistent with the strength of the identification strategy: as δ increases, the peak of education moves later in time as the counterfactual cohorts are increasingly later exposed to the educational reform. As high school completion fades out at similar rates, this suggests that counterfactual cohorts return to high school at roughly similar rates despite the fact they are older at the time they are treated. Interestingly, the longer run impact on higher education is substantially lower for the sample of $\delta = 2$, reinforcing the idea that the returning to high school at younger ages increases the probability of completing additional higher education.

To provide a further understanding of the long-run effects of the reform, Figure R.2 presents estimates of the impact of the reform varying different levels of δ, the number of years until the counterfactual cohorts are also treated, from $\delta = 3$ to $\delta = 14$. Increasingly varying δ gives an indication of the span of the long-run effect, comparing the baseline specification to different levels of δ. Figure R.2 suggests that long-run impacts of the education expansion are roughly 2–3 times as large relative to the estimates of the reform on education in our baseline specification.[28]

6.7 Robustness of Results to Varying Base Ages

Appendix R.3 reveals how the average post-reform increases in years of education (Table R.2) and higher education (Table R.3) vary based on the ages of the estimation sample. Specifically, the Tables compare the post-reform education of baseline estimation sample, those aged 30–33 at the introduction of the education reform, to those aged 34–37 and those aged 38–41. Clearly, age matters in returning to education: those aged 34–37 are roughly half as likely to complete higher education, an effect which is not statistically significant, while the education of those aged 38–41 is unchanged. The strong relationship between take up of the education reform and age suggests that policies designed to encourage dropouts to return to high

[27] Including even more alternatives for δ resembles the patterns seen in Figure R.1.

[28] As increasing δ compares birth cohorts who are born increasingly further apart, the inverse propensity score reweighting exercise of Section 3.3.2 becomes increasingly important.

school are more effective at younger ages.

Appendices R.4– R.6 further examine the robustness of the baseline results to varying the base ages of the sample. Three different samples are used: (i) a slightly older sample, those aged 30–37 at the time of reform; (ii) an even older sample, those aged 38–41, who have mostly completed fertility at the time of reform; and (iii) a younger sample, those aged 26–33 at the time of the reform.

Figure R.3 replicates the results on education extending the sample of workers to $a^0 = 30, ...37$. Including workers who are even older at the time of the reform produces similar results, with significant increases in high school which fade out over time and persistent increases in higher education. However, the post-reform increases in education are slightly smaller when compared to the baseline sample, suggesting that older workers are less responsive to the education reform.

To further examine the relationship between age and returning to education post-reform, Figure R.4 compares results on education using base ages $a^0 = 38, ..., 41$ to results using $a^0 = 30, ..., 33$. The post-reform increases in high school are comparable between younger and older base age samples. However, results for higher education (Figure R.4d) differ substantially: the increase in higher education among the younger sample is at least twice as large. Indeed, there is no significant increase in higher education for the older sample by +14. This is consistent with the fact that while take up of high school completion is similar irrespective of age, returning to high school younger increases the probability of completing higher education relative to returning to high school older.

Finally, Figure R.5 examines the robustness of the results to including even younger workers, $a^0 = 26, ..., 33$. Including even younger workers produces similar results, with significant increases in higher education post-reform and an increase in high school completion which fades out as the counterfactual cohorts become treated.

7 Conclusion

In a setting of a large policy reform for a second chance education option, we leverage high-quality Norwegian register data to analyze the life cycle timing of human capital investment. We focus on adult women and find an important impact of the timing of second chance educational investments. Exploiting the variation in the age at which different birth cohorts in Norway are exposed to a major reform in the 2000s that enables greater access to high school for adults, we establish a causal link between the timing of a second chance education and labor market outcomes. Expanding access at younger ages before women have completed fertility, results in large and significant increase in education investments with a strong rise in the rate of college completion, leading to higher earnings, increased employment, and decreased fertility.

We look at enrolment in education by women in their late twenties and early thirties. While most men who return to education have done so by this age, female high school dropouts are much more likely to return to education after having their first child and we estimate a significant impact of the reform on women returning to education at these ages. In particular, we find a sizeable impact on the probability of women going on to complete higher education, suggesting a second chance in the formal education system provides a key route to enhancing college completion later in life. Importantly, we show that the timing of such second chance investments is crucial: returning to education even a few years earlier significantly increases the incentive to invest in additional higher education.

Our results reveal that second chances in the formal education system are key to enabling low-educated mothers' transition back to the labor force at critical points in life cycle. We find that the impact on female labor earnings operates primarily through increases in employment. Before the reform, women who are high school dropouts are, on average, weakly attached to the labor force. Fertility is shown to be an important underlying mechanism: in addition to increasing employment, returning to education also leads to a reduction in fertility. Given the strong relationship between children and employment, a portion of the increase in employment can be attributed to joint decisions over fertility and employment. The results also suggest that enhanced access to adult education, and the subsequent increase in years of education, can be an effective policy to reduce the gender earnings gap as such education enables low-educated married women to close the pre-reform gaps in earnings with their spouses.

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A Window of Opportunity?

The Impact of Returning to Education on Earnings and Fertility*

Appendix

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July 12, 2024

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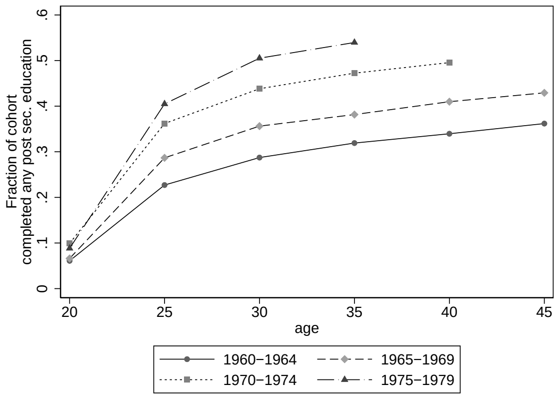
A Field of Study, Higher Education, and Years of Education Over the Life Cycle

A.1 Returning to Education Over the Life Cycle—Higher Education

Figure A.1: Age-Education Profile for the Completion of Higher Education Over the Life Cycle

(a) Females



(b) Males

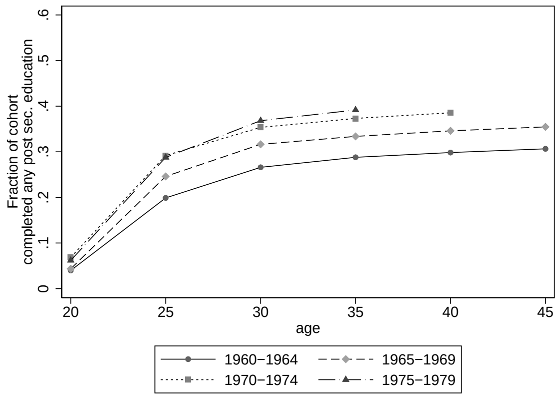


Figure plots the higher education completion rate over the life cycle for birth cohorts 1961–1980 for women and men respectively. Sample is balanced to include those observed at all points from age 20 onward. 1976–1980 cohorts are followed until age 35 and 1971–1975 cohorts are followed until age 40 in the data.

A.2 Returning to Education Over the Life Cycle—Years of Completed Schooling

Figure A.2: Evolution of Years of Education Over the Life Cycle

(a) Female (b) Male

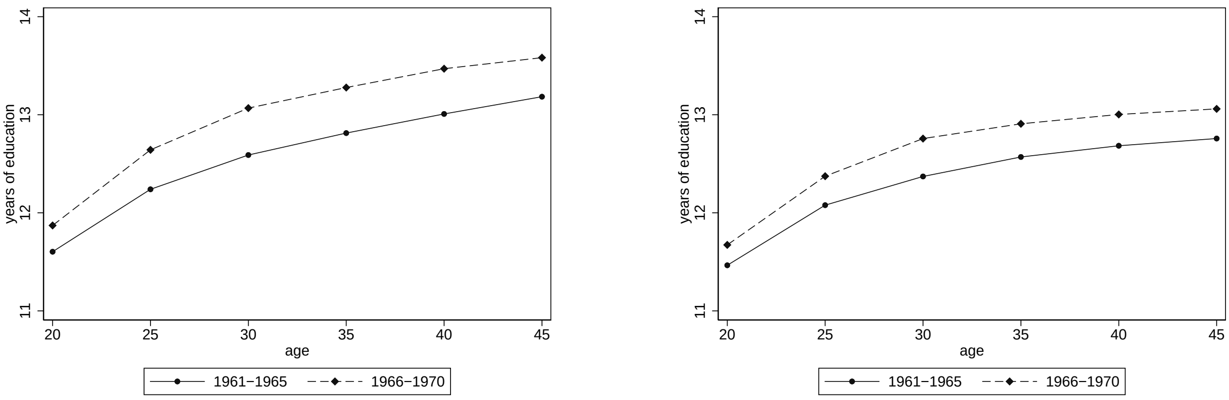


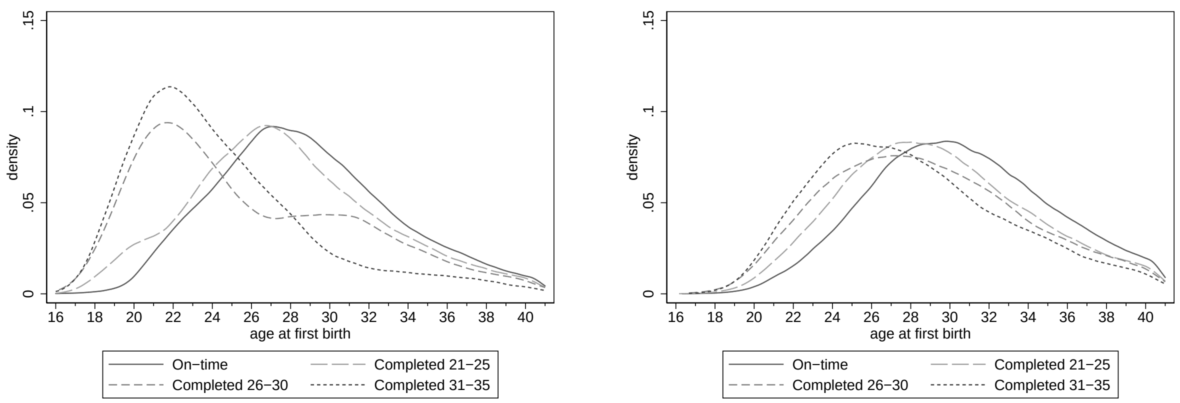
Figure plots average years of education over the life cycle for birth cohorts 1961–1970 for women and men respectively. Years of education measured as the typical number of years it takes a student to complete a specific qualification.

B The Relationship Between Fertility and Age of High School Graduation

The figure presents the distribution of age at first birth for four separate samples: on-time high school graduates (age 20 or less) and those who completed high school from 21–25, 26–30, and 31–35. In particular, fertility during the teenage years and the early 20s is much more common among women who return to education later in life.[1] For women who complete high school from 21–25, there is a clear mass who have their first child in their late teenage years and early 20s. The same is not true for men. The distributions of age at first birth differ even more among those who complete high school from 26–30 and 31–35: the vast majority of women who complete education at these ages had their first child in their teens and early 20s. Again, the same cannot be said of men.

Figure B.1: Distribution of Age at First Birth for Different Ages of High School Completion

(a) Females (b) Males

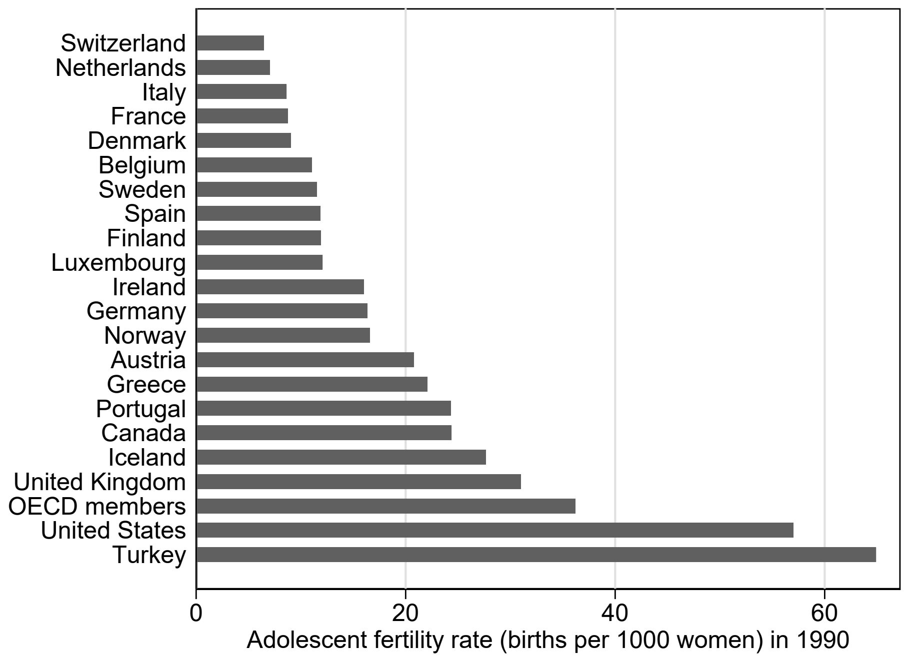


Figures plots the age distribution of age at first birth separately for females and males by four groups of age of high school completion: on-time (age 20 or younger), 21–25, 26–30, and 31–35. Includes all first-births between 16 and 41, birth cohorts 1961–1970.

B.1 Adolescent Fertility Across OECD Founding Member States & Finland

[1] Figure B.2 presents adolescent fertility rates among the OECD founding member states and Finland in 1990. Norway ranks in the middle of the distribution of teenage fertility across the founding member states with 16.5 births per 1000 women aged 15–19.

Figure B.2: Fertility Rates of 15–19 Year Olds in 1990



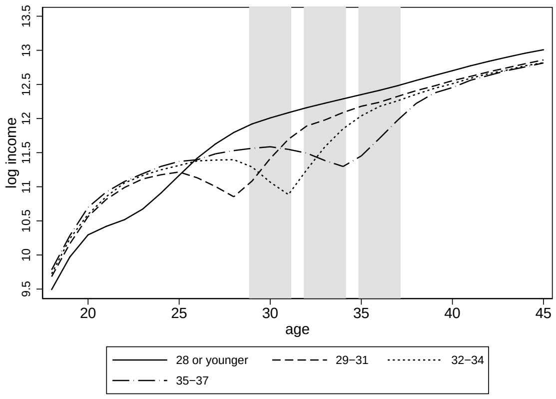
Births per 1000 women aged 15–19 in 1990. Source - World development indicators, World Bank, (https://datacatalog.worldbank.org/dataset/world-development-indicators). World bank defines adolescent as ages 15–19. Founding OECD member countries and Finland reported, OECD average calculated as average across all OECD members in 1990.

C Late Completion of Higher Education and Lifetime Earnings

Figures C.1a and C.1b present the evolution of earnings for those who complete any post-secondary education from age 28 or younger, 29–31, 32–34, and 35–37 for women and men respectively. The dip in earnings among later life completers relative to those who graduate post-secondary education at 28 or younger is quite large. As with the completion of high school, late completers catch up in terms of the slope of lifetime earnings, but not the level.

Figure C.1: Evolution of average earnings by different ages completed any post-secondary education

(a) Female



(b) Male

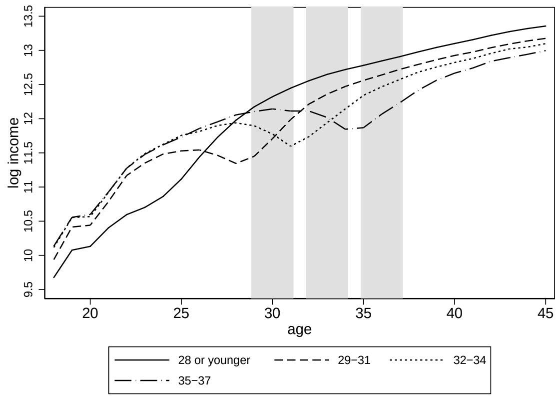


Figure plots, for women and men respectively, average of log earnings for on-time higher education graduates, late graduates aged 29–31, 32–34, and 35–37. Birth cohorts 1964–1970.

D Calculating the Discount Rates to Equalize Age-Earnings Profiles of On-Time and Later Life High School Graduates

The table calculates the discount rate over ages 18–64, assuming a stable difference in earnings from age 46 on. We calculate the discount rate as in Bhuller, Mogstad, and Salvanes (2017): $\sum_{a=18}^{45} \frac{\beta_a}{(1+\rho)^{a-17}} = 0$.

First, we calculate the discount rate which equalizes the present value of earnings of on-time graduates and late completers 24–26. For females, a large discount rate is required to equalize the earnings of the two groups: 27–45%. Compared to women, the earnings penalty incurred by male on-time graduates is much larger and the lifetime gains compared to late completers are much smaller. As such, a much lower discount rate is required to equalize the present value of earnings of the two groups for men: 5–7%.

Second, we calculate the discount rate which equalizes the age-earnings profiles of on-time graduates and late completers aged 30–32. For both women and men, the discount rates which equalize the earnings of the two groups are lower compared to those which equalize on-time graduates and late completers 24–26. For women, a discount rate 14–30% equalizes the age-earnings profiles. For men, a discount rate of 3–4% equalizes the age-earnings profiles.

Table D.1: Required Discount Rates to Equalize Earnings of Different Age of HS Completion Groups

	Discount Rate to Equal Age Group On-Time HS Graduates			
	(1)	(2)	(3)	(4)
	Females		Males	
	Excluding Post-Secondary (Figure 1c)	Including Post-Secondary (Figure 1a)	Excluding Post-Secondary (Figure 1d)	Including Post-Secondary (Figure 1b)
	Earnings 18–64 (assuming stable difference 46–64)			
On-time graduate	ref.			
24-26	0.4507	0.2746	0.0674	0.0552
30-32	0.3044	0.1441	0.0348	0.0304
Observations On-Time	37195	120322	37734	108021
Observations 24–26	2506	6685	7393	12745
Observations 30–32	2722	5003	4797	6196

Table calculate the discount rate required to equalize the lifetime earnings of late high school graduates aged 24–26 and 30–32 to the lifetime earnings of on-time graduates (the reference group). Columns (1)–(2) present results for females while (3)–(4) present results for males, birth cohorts 1964–1970. Columns (1) and (3) exclude any individual who completed post-secondary education beyond high school, while columns (2) and (4) include post-secondary education. Assumes that the earnings difference at 45 persists in each future age from 46–64. Discount rate calculated as in Bhuller, Mogstad, and Salvanes (2017): $\sum_{a=18}^{45} \frac{\beta_a}{(1+\rho)^{a-17}} = 0$.

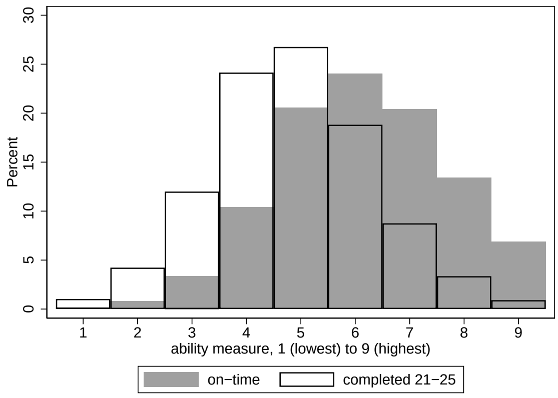
E The Relationship Between Cognitive Ability and Returning to Education

Using data available for men from compulsory military testing at age 18, Figure E.1 plots the distribution of cognitive ability measures across different ages of high school completion for the birth cohorts 1961–1970. The Figure compares the distribution of cognitive ability between on-time graduates and four groups: those who completed high school from 21–25, 26–30, 31–35, and those who never complete high school. Figure E.1 reveals clear differences in the underlying cognitive ability of on-time graduates, late completers, and never completers. On-time graduates are positively selected from the IQ distribution, with a median of cognitive ability of 6 and very few with cognitive ability score below the national mean of 5. As age of high school completion increases, the cognitive ability distribution shifts leftwards. While there are clear differences in the distributions of on-time and late completers aged 21–25 (Figure E.1a), where late completers have lower IQ scores, these differences are even more prevalent comparing on-time and late completers aged 26–30 and 31–35 (Figures E.1b and E.1c respectively).

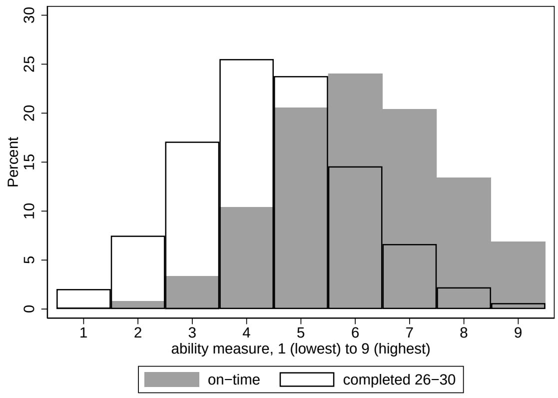
Figure E.1d compares the cognitive abilities of those who never complete high school to those who graduate on-time. While on-time graduates are positively selected, those who never complete high school by age 45 are negatively selected from the cognitive ability distribution. Such stark differences in cognitive ability complicate the comparison of late high school completers to on-time and never completers, and Section 3 discusses the choice of counterfactual for late high school completers in estimating the returns to later in life education.

Figure E.1: Distribution of IQ for Different Ages of High School Completion

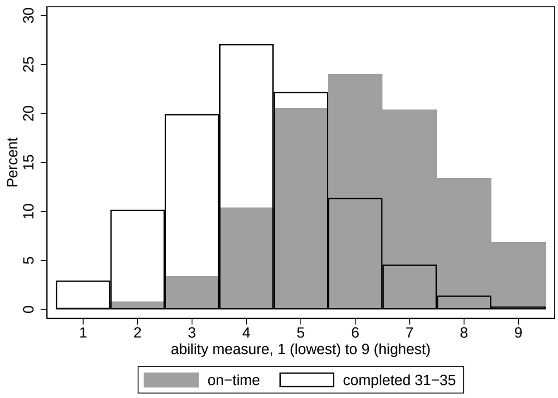
(a) Completed HS On-time and 21–25



(b) Completed HS On-time and 26–30



(c) Completed HS On-time and 31–35



(d) Completed HS On-time and Never Completed

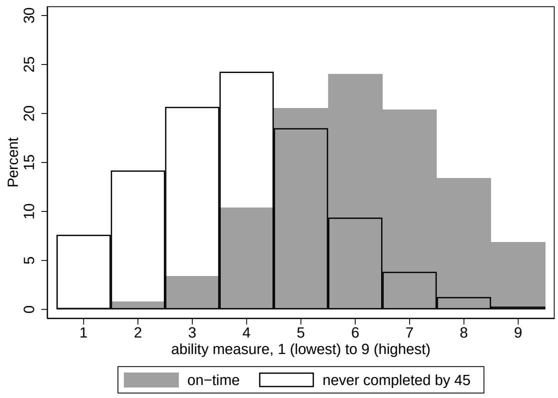


Figure compares the distribution of cognitive ability for men for on-time high school completers to four different groups: high school completers from 21–25, from 26–30, from 31–35, and those who never complete high school by age 45. Cognitive ability measured for all males at age 18 from compulsory military testing. Cognitive ability is measured on a 9 point scale, mean of 5 and standard deviation of 2. Measure is comprised of 3 examinations: an arithmetic test, a word similarities test, and a figures test as described in Section 2. Birth cohorts 1961–1970.

F The Relationship Between Socioeconomic Status and Returning to Education

Given the intergenerational link in education and earnings (see Black and Devereux, 2011, for an overview), socioeconomic status (SES) may be an important factor in returning to education over the life cycle. Do gaps in high school completion between high and low SES families persist over the life cycle, or do lower SES families catch up over time? Figures F.1 and F.2 plot the gaps in education by socioeconomic status (SES) over the life cycle for women and men respectively. SES is measured by parental education, where parental education is defined as the highest level of parental education ever attained, such that either the mother, the father, or both parents may have the indicated level of education. Figures F.1 and F.2 separate the sample into two groups of parental education: low-educated families, where the highest parental education is less than high school, and high-educated families, where the highest parental education is post-secondary education.[2]

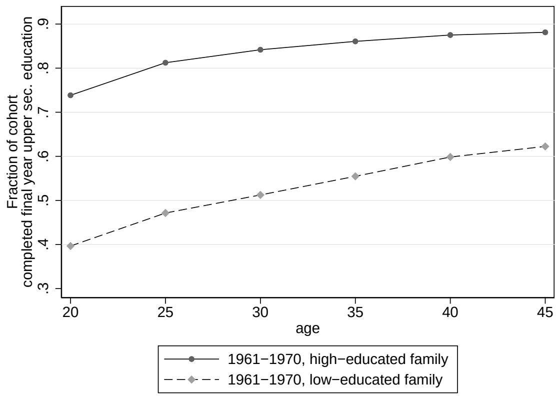
Unsurprisingly, there are sizable gaps in on-time high school completion by SES: those from low-educated families complete high school by age 20 at much lower rates. This is true for both men and women. SES gaps in high school completion decrease over time, as the completion gap is wider for the 1961–1970 cohorts compared to the 1971–1980 cohorts. Over the course of the life cycle, the SES gap in high school completion decreases, as those from low-educated families complete high school at higher rates than those from higher educated families later in life. For women born 1961–1970 (Figure F.1a), the SES gap in high school completion is 34 p.p. at age 20, 31 p.p at age 35, and 26 p.p by age 45. As seen before, men tend to return to education at younger ages relative to women.

While high school completion is key, it is informative to also understand how SES gaps evolve over the life cycle including all margins of education. As such, panels (c) and (d) in Figures F.1 and F.2 plot the SES gaps measured in terms of years of education. Unlike high school completion, SES gaps measured in years of education widen over the life cycle, as those from high-educated families continue to complete higher education beyond the high school level.

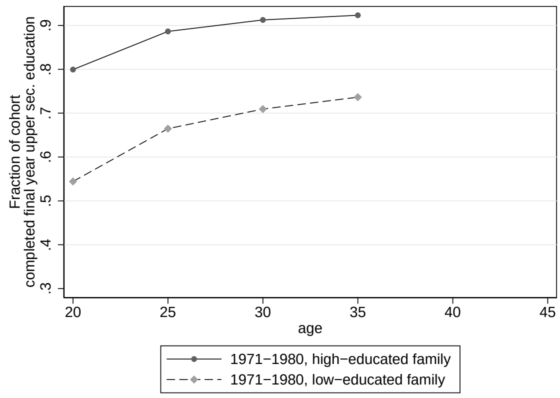
[2]Omitted are families where the highest level of parental education is the completion of high school. The high school completion of children in such families lies in between the high- and low-SES families.

Figure F.1: Completion of High School/Years of Education Females, by Highest Parental Education

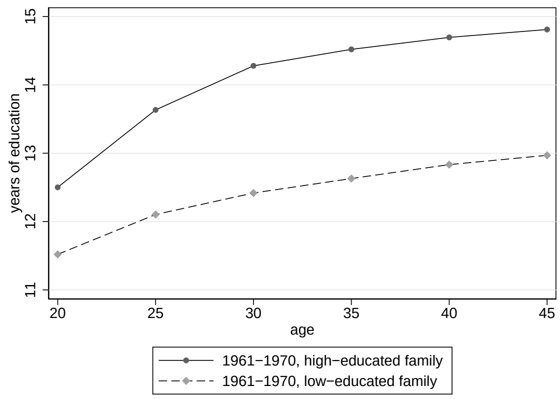
(a) 1961–1970



(b) 1971–1980



(c) 1961–1970



(d) 1971–1980

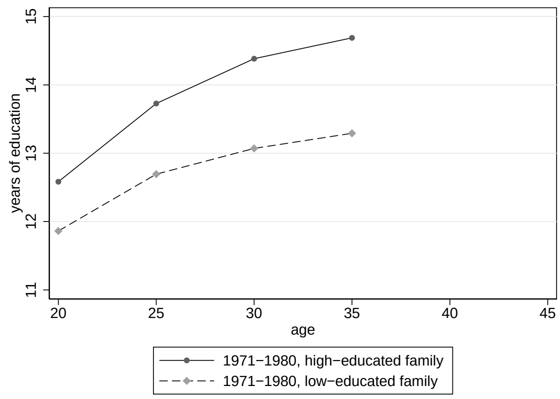
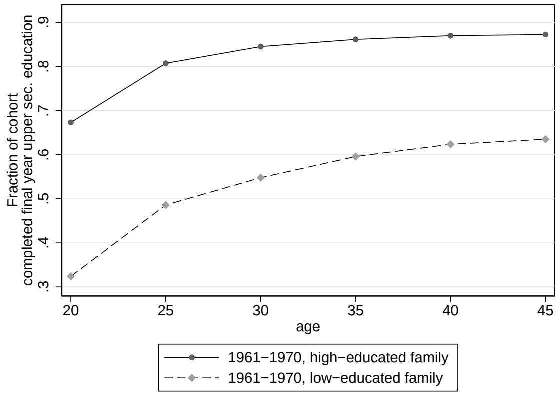


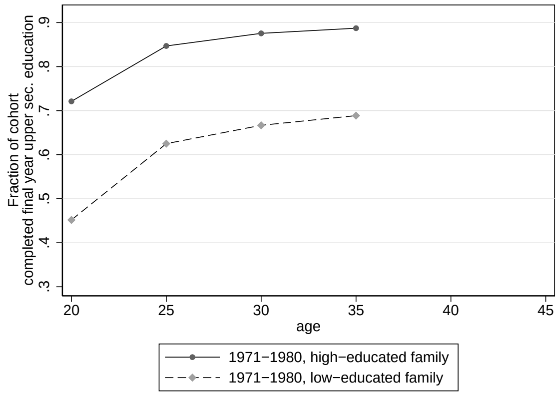
Figure plots the high school completion rate/years of education over the life cycle for birth cohorts 1961–1980 for females by the level of family education. Low-educated family: highest parental education is less than high school. High-educated family: highest parental education is any post-secondary education.

Figure F.2: Completion of High School/Years of Education Males, by Highest Parental Education

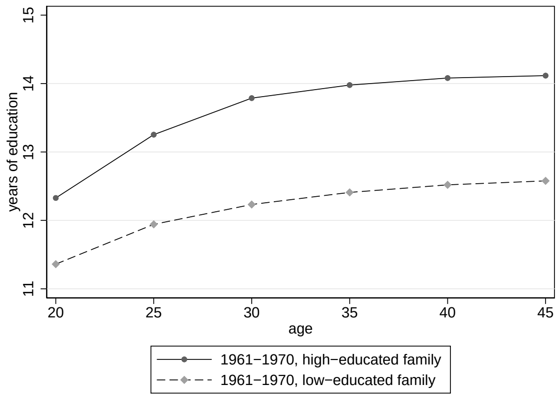
(a) 1961–1970



(b) 1971–1980



(c) 1961–1970



(d) 1971–1980

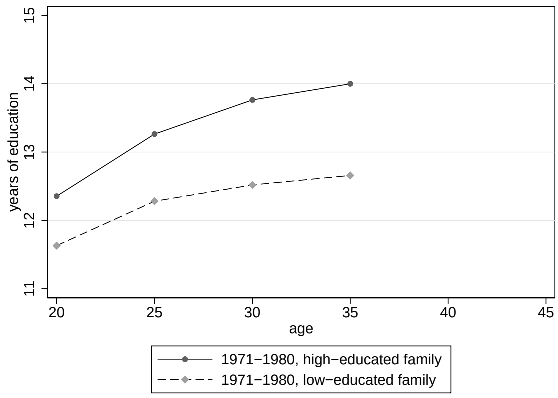


Figure plots the high school completion rate/years of education over the life cycle for birth cohorts 1961–1980 for males by the level of family education. Low-educated family: highest parental education is less than high school. High-educated family: highest parental education is any post-secondary education.

G The Evolution of Returning to Education Across Genders and Cohorts

Figures G.1a and G.1b plot the high school completion rate from ages 20–45 for females and males respectively. Figure G.1 reveals returning to education is an important phenomenon over the course of the life cycle: across 20 years of birth cohorts in Norway from 1961-80, the high school completion rate increases by 6–17 percentage points between the ages of 20–25, by an additional 7–10 p.p. between the ages of 25–35, and by an additional 2–7 p.p. from 35–45. While the high school completion rate begins to flatten over the course of the life cycle, individuals continue to complete high school through age 45, well after an "on-time" student would.[3] Among those born 1961–1970 who drop out of high school and do not graduate on-time, 39% of women and 47% of men go onto complete high school by age 45.

Figure G.1: Age-Education Profile for the Completion of High School Over the Life Cycle

(a) Female (b) Male

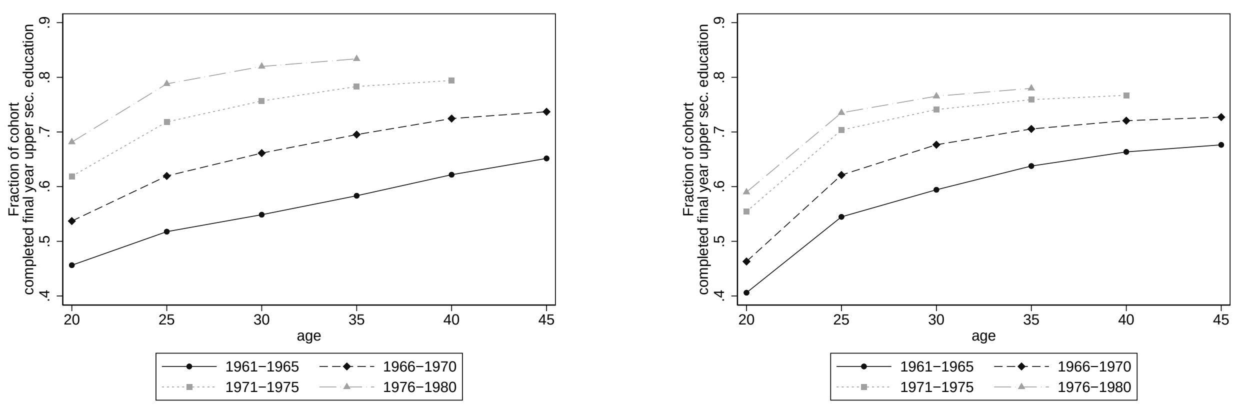


Figure plots the high school completion rate over the life cycle for birth cohorts 1961–1980 for women and men respectively. Sample is balanced to include those observed at all points from age 20 onward. 1976–1980 cohorts are followed until age 35 and 1971–1975 cohorts are followed until age 40 in the data.

Across different birth cohorts, on-time high school completion increases from older to younger cohorts. Most of the differences in eventual high school completion across birth cohorts are due to the level shift in on-time completion. However, age-education profiles are not parallel across cohorts: as on-time completion has increased, returning to high school over the life cycle has declined. This appears to be more true for men than for women, though high school completion continues into older ages even for men. As the cohorts 1961–1970 closely correspond to the cohorts who are impacted by the reform described in Section 3, we focus mainly on these birth cohorts.

There exist considerable gender differences in high school completion from on-time completion up

[3]The figures throughout follow individuals until age 45, where the 1971–1975 cohorts are missing data for age 45 and the 1976–1980 cohorts are missing data for age 40 and 45.

until age 45. Across all birth cohorts, females complete high school on-time at a higher rate than males. Irrespective of gender, high school education continues to increase over the life cycle, but at a slightly decreasing rate.

However, the age at which women and men return to education differs greatly. From 20–25, the high school completion rate of men increases rapidly relative to women such that the large gap in high school completion at age 20 between men and women is reversed by age 25. Women, on the other hand, return to high school at higher rates than men at later stages in the life cycle. For instance, from ages 30 onwards, women return to high school substantially more than men. This is especially true from 35–45, where the high school completion rate of women is nearly double that of men.

Completing academic high schools also provides the option of attending higher education. Figures A.1a and A.1b plot the fraction of each birth cohort from 1961–1980 who complete any higher education at different ages for women and men respectively. Females complete higher education at a much higher rate than males. Consistent with Figure G.1, where women returned to high school at much higher rates relative to men, females also return to higher education later in life at much higher rates. This suggests there may be a window of opportunity for women, where returning to high school earlier in life leads to a much steeper age-education profile in higher education for women relative to men. In order to present a complete picture, Appendix Figures A.2a and A.2b replicate Figure G.1 for years of education rather than high school completion. For females, increases in years of education later in life do not flatten off compared to high school completion while increases in years of education begin to flatten for men. Between 30–45, after the typical student has had the opportunity to complete university education, years of education increases by roughly 0.5 years for women and 0.25 years for men. Women complete more years of education than men at age 20, and this gender gap is expanding over the life cycle as years of education increases more for women than men.

Important differences in when in the life cycle women and men return to education suggest different considerations in the decision of women and men at the margin of returning to high school. Relative to men, returning to high school is more common for women in their 30s and early 40s. Appendix B examines differences in the importance of childbearing for women relative to men as a potential reason behind this. Relative to women, on-time high school dropout is more problematic for men. This suggests that the returns to high school education may not be large enough to justify investments in human capital for young boys or that the labor market opportunities available in youth may be more appealing to men than women. However, by age 25, a large fraction of male dropouts have returned to complete high school, suggesting that some of these dropouts quickly see the need for additional human capital.

H Comparing Treated and Counterfactual Cohorts

H.1 Pre-Reform Characteristics of Treated and Counterfactual Cohorts, Female Sample

Table H.1: Describing the Estimation Sample

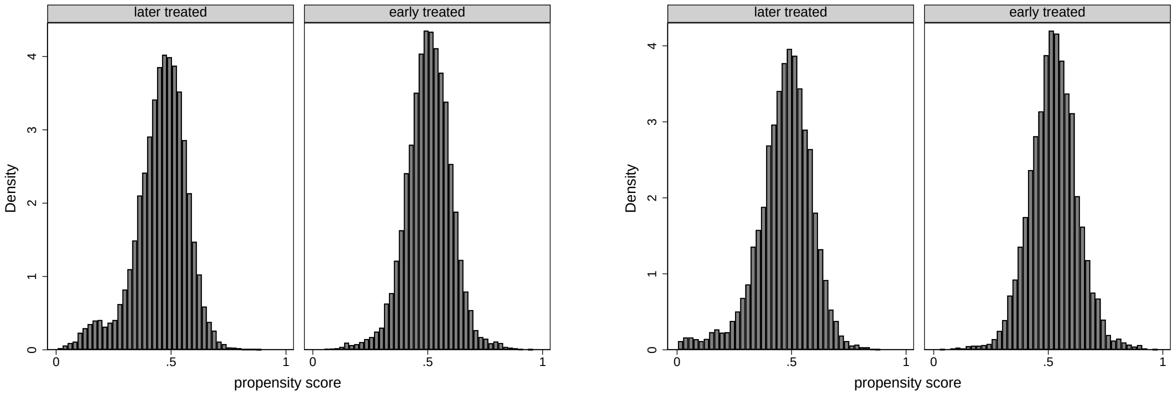
	Female		
	(1) Treated Cohorts	(2) Counterfactual Cohorts	(3) Both
Parental Education:			
Frac. at least one parent highly educated	0.327	0.288	0.306
	(0.469)	(0.453)	(0.461)
Fertility and Household:			
Frac. first birth age 25 or younger	0.563	0.546	0.554
	(0.496)	(0.498)	(0.497)
Number of children	1.559	1.550	1.554
	(1.053)	(1.055)	(1.054)
Frac. with children	0.802	0.798	0.800
	(0.398)	(0.402)	(0.400)
Frac. married	0.436	0.475	0.457
	(0.496)	(0.499)	(0.498)
Demographic:			
Base age	31.55	31.51	31.53
	(1.116)	(1.116)	(1.116)
Frac. born in Norway	0.973	0.977	0.975
	(0.163)	(0.149)	(0.156)
Labor Market:			
Frac. employed full time	0.345	0.376	0.361
	(0.475)	(0.484)	(0.480)
Log of labor earnings	11.63	11.57	11.60
	(1.003)	(1.015)	(1.010)
Log of labor earnings & benefits	11.66	11.64	11.65
	(0.976)	(0.924)	(0.949)
Education:			
Years of education	12.21	12.22	12.22
	(1.033)	(0.942)	(0.987)
Frac. completed HS	0.128	0.115	0.121
	(0.334)	(0.319)	(0.326)
Frac. completed higher educ.	0.0389	0.0425	0.0408
	(0.193)	(0.202)	(0.198)
Age first dropped out	17.90	17.89	17.90
	(1.142)	(1.178)	(1.161)
Individuals	11358	12565	23923

Sample: women of base ages 30–33 who dropped out of high school as described in Section 3.3.1. All variables measured at time −1, unless otherwise indicated. Sample of column (1) corresponds to birth cohorts who are treated early in life (treatment cohorts). Sample of column (2) corresponds to birth cohorts who are treated later in life (counterfactual cohorts). Column (3) combines both samples. Table reports the sample average, with standard deviation in parentheses.

H.2 Estimated Propensity Scores from Pre-Reform Reweighting Exercise

Figure H.1: Distribution of the estimated propensity score from weighting exercise of Section 3.3.2

(a) Female (b) Male



We estimate the propensity score by matching treated (those treated at younger ages) and counterfactual (those treated at older ages) cohorts in $time = -1$ on: 3 binary variables indicating at least one highly educated parent, first birth at age 25 or younger, and married; binary variables for age of first drop outs (5 categories, 16–20); binary variables for broad field of study left high school with (10 broad categories); the number of children (including zeros); birth municipality; 2 digit industry dummies; and base age. Reweighting exercise described in Section 3.3.2.

H.3 Controlling for Differences in Labor Market Conditions Across Years

In order to account for the fact that treated and counterfactual cohorts in regression equation (1) face different economic conditions, different years at the same point in event time, the paper creates two labor market controls using data on the entire Norwegian population. These controls are measured as follows.

First, we use data on all workers aged 25–54 who have not completed high school from 1993–2014, measuring one of two labor market outcomes: (a) labor earnings and (b) full-time employment. Second, we regress this labor market outcome on *municipality* × *year* fixed effects and *initial field of study* × *year* fixed effects. Finally, we merge these estimated fixed effects for earnings/employment from the entire population into the sample of high school dropouts of base ages 30–33 and control for them in their respective regression. Specifically, we make use of these controls estimated on the entire population of dropouts in the estimation for earnings (Figure 9a) and employment (Figure 9b), and all subsequent Figures and Tables which use earnings and employment as an outcome variable.

These two controls for labor market conditions account for differences in local economic conditions and time-varying shocks which affect treated and counterfactual cohorts who dropped out of education in the same field of study and live in the same municipality differently. Excluding these controls leads to slightly larger impacts of the reform on income and employment, thus the controls capture differences in economic conditions which are quantitatively important.

We define field of study at high school as the field of study an individual in the base age sample first dropped out of education with, as the entire sample all completed some high school yet did not finish.

I The Impact of the Reform on Short Term Enrollment in Education

Figure I.1: Enrollment in Education at Different Ages Across Birth Cohorts, Females

(a) Enrollment at age 29 (b) Enrollment at age 31

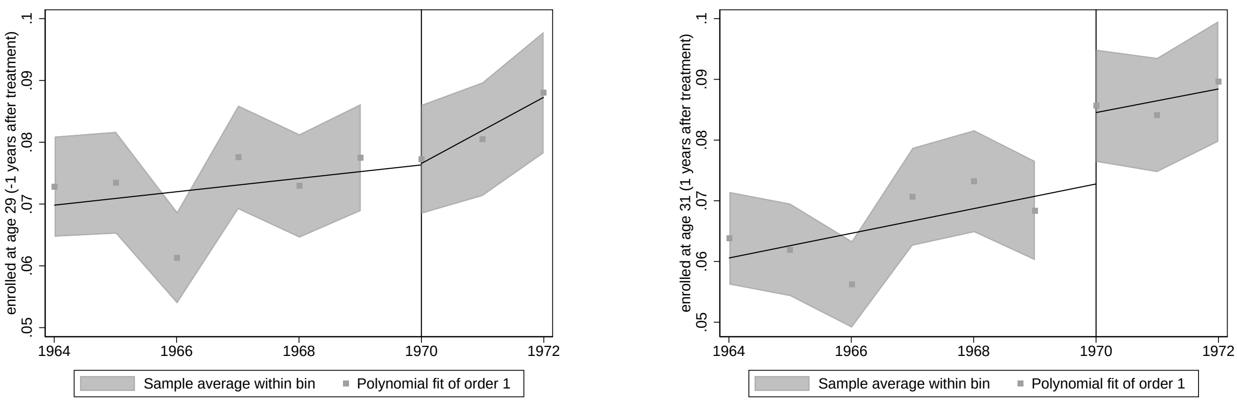


Figure plots the rates of enrollment at different ages across different birth cohorts. Vertical line at 1970 cohort corresponds to a cutoff in the age at which cohorts are treated, where younger cohorts are treated and older cohorts are not yet treated. Sample of females as described in Section 3. 95% confidence interval reported.

Figure I.2: Enrollment in Education at Different Ages Across Birth Cohorts, Males

(a) Enrollment at age 29 (b) Enrollment at age 31

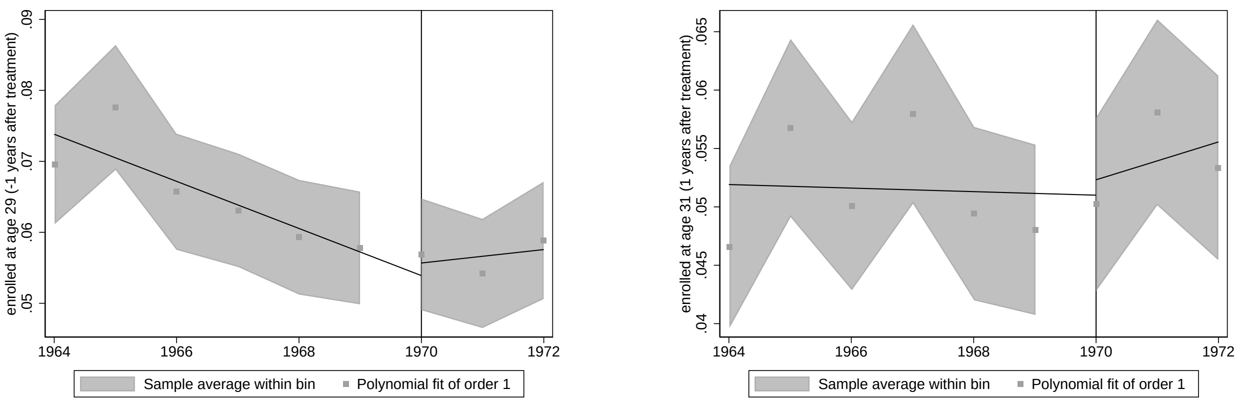
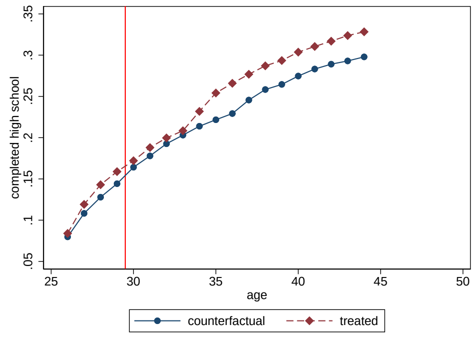


Figure plots the rates of enrollment at different ages across different birth cohorts. Vertical line at 1970 cohort corresponds to a cutoff in the age at which cohorts are treated, where younger cohorts are treated and older cohorts are not yet treated. Sample of males as described in Section 3. 95% confidence interval reported.

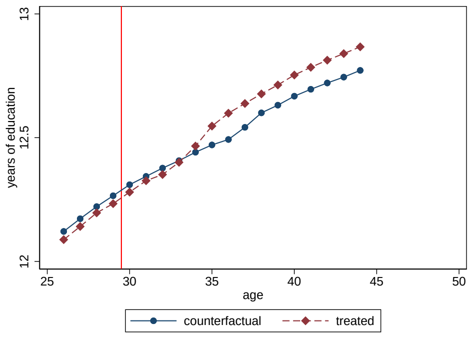
J The Impact of the Reform on Education—Raw Averages

Figure J.1: Age-Education Profile of Estimation Sample, Comparing Treated and Counterfactual Cohorts

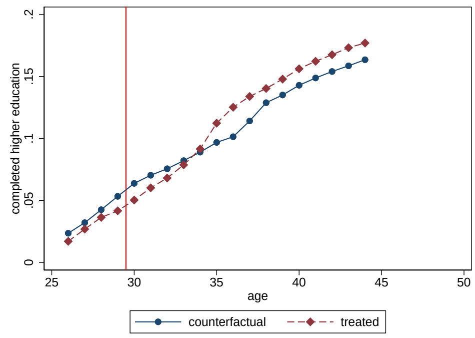
(a) High School, 1970 (treated) and 1967 (counterfactual)



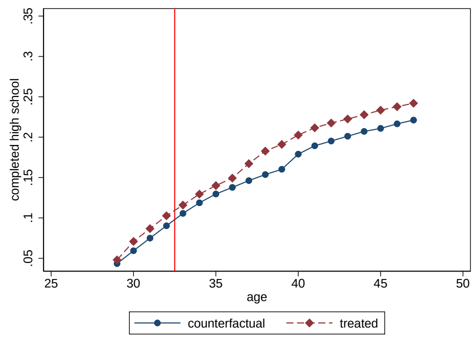
(b) Years of Education, 1970 (treated) and 1967 (counterfactual)



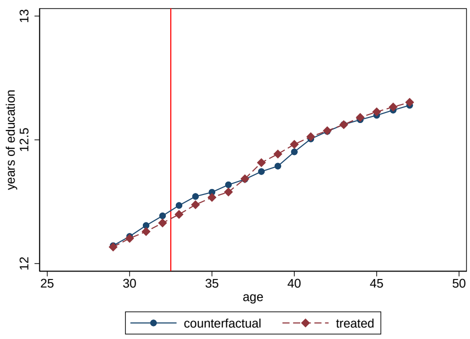
(c) Higher Education, 1970 (treated) and 1967 (counterfactual)



(d) High School, 1967 (treated) and 1964 (counterfactual)



(e) Years of Education, 1967 (treated) and 1964 (counterfactual)



(f) Higher Education, 1967 (treated) and 1964 (counterfactual)

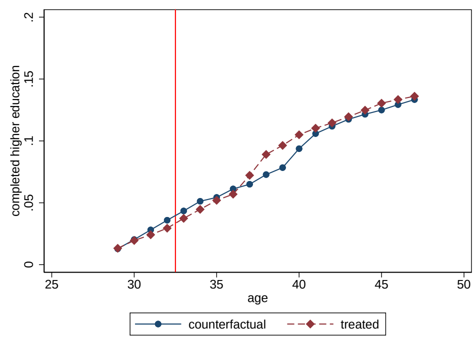


Figure plots the age-education profiles of two different treated birth cohorts (1970 and 1967) and their respective different counterfactual birth cohorts (1967 and 1964) for the completion of high school, years of education, and higher education. Raw averages reported. Vertical line indicates age at which treated cohort is treated by the education reform (age in year 2000), while counterfactual cohort is 3 years older when treated by the same reform.

K The Relationship Between Fertility and Labor Market Outcomes

K.1 The Correlation Between Pre-Reform Employment and Children

Figure K.1: Relationship Between Employment and Children for Women

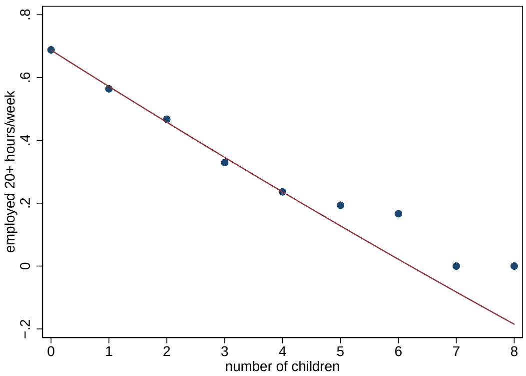


Figure plots the correlation between pre-reform employment (time −1) and the number of children. Sample of women as described in Table H.1. Employed defined as employment of 20 or more hours per week.

K.2 The Impact of the Reform on Labor Market Outcomes, Sample of Women in Households at the Time of Reform

Table K.1: The Estimated Long-Run Impact of the Educational Reform on Female Labor Market and Education Outcomes, Averaged Over Post-Reform Period from +8–+14

	Labor Market Outcomes			Education	
	(1) Log Annual Earnings	(2) Log Hourly Earnings	(3) Employed Full Time	(4) Years of Education	(5) Higher Education
Treated × Post	0.0329**	0.0011	0.0325***	0.0757***	0.0153***
	(0.0153)	(0.0166)	(0.0100)	(0.0233)	(0.0056)
N	74498	74498	91662	91692	91692
Avg. Reduced Form Outcome in −1	11.755	4.898	0.301	12.161	0.032

Sample of treated and counterfactual women, base ages 30–33, who have a partner at the time of the reform. Sample period corresponds to the long-run impact of the reform on education/earnings, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Column (1) additionally restricts to women who have positive hours worked in a given year. Column (1) measures annual earnings from employment and column (2) measures hourly labor wages, annual earnings in column (1) divided by annual hours worked. Column (3) measure employment as equal to 1 if working more than 30 hours per week. Column (4) measures years of education. Coefficients interpreted relative to omitted −1. Regressions include controls for labor market conditions as defined in Section 3.3.2.

L The Estimated Impact of the Reform on Education, Men and Women

Table L.1 presents the estimated impacts of the education reform on men and women. Sample corresponds to all men and women, that is, both those with and without households at the time of the reform. The estimated effects are small in magnitude and imprecisely estimated for men. Figure G.1 provides insight into why: a substantial fraction of male high school dropouts already return to high school at earlier ages in the life cycle compared to women. As men are substantially more likely to return to high school from 20–30 compared to women, it is unsurprising that men are not induced to return to high school education in their early 30s. For instance, for those born 1966–1970, the early treated birth cohorts, the high school completion rate of men is 7.4 percentage points less than that of women at age 20. However, by age 30, the gap has reversed and men are 1.6 percentage points more likely to have completed high school.

Table L.1: The Estimated Impacts of the Reform on Educational Attainment, all Men and Women

	Male Sample			Female Sample		
	(1) Completed High School	(2) Years of Education	(3) Higher Education	(4) Completed High School	(5) Years of Education	(6) Higher Education
Treated × Post	-0.0074	-0.0303	-0.0023	0.0017	0.0416**	0.0105**
	(0.0054)	(0.0214)	(0.0031)	(0.0052)	(0.0176)	(0.0042)
N	145560	145368	145560	189720	189395	189720
Avg. Reduced Form Outcome in −1	0.143	12.161	0.028	0.121	12.215	0.041

Sample of treated and counterfactual men and women, base ages 30–33. Male sample in columns (1)–(3), female sample columns (4)–(6). Sample period corresponds to the long-run impact of the reform on education/earnings, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Columns (1) and (4) measure outcome variable as equal to 1 if graduated high school. Columns (2) and (5) measure outcome variable as years of education. Columns (3) and (6) measure outcome variable as equal to 1 if graduated higher education. Coefficients interpreted relative to omitted −1. Regressions include controls for labor market conditions as defined in Section 3.3.2.

M Does the Education Reform Impact Occupational Choice?

Table M.1: Distribution of Occupations in +14 by Treated/Counterfactual

	Counterfactual	Treated
Managers	3.72	3.93
Professionals	11.66	12.04
Technicians and associate professionals	12.17	11.47
Clerical support	10.33	8.87
Service and sales	33.14	35.50
Skilled agriculture, forestry, and fishery	0.29	0.29
Craft and related trades	0.45	0.45
Plant and machine operators	1.66	1.66
Elementary occupations	2.75	2.70
No occupation (not employed)	23.83	23.08
Total	100.00	100.00
N	12565	11358

Table M.2: Distribution of Public/Private Sector in +14 by Treated/Counterfactual

	Counterfactual	Treated
private sector	42.16	43.36
public sector	57.84	56.64
Total	100.00	100.00
N	9571	8737

Occupations grouped according to Norwegian standard classification of occupations. Sample of those who are employed in an occupation in Table M.1.

N The Impact of the Educational Reform on Employment by Pre-Reform Labor Market Attachment & Children

N.1 Impact of the Reform on Employment by Pre-Reform Labor Market Attachment

Table N.1 examines the importance of pre-reform labor market status for the impacts on employment, separating the sample into low, some, and strong attachment to the labor force based on the number of hours worked in -1. Women with different levels of pre-reform attachment to the labor market see very different changes in employment outcomes post-reform.

Table N.1: Long-Run Post-Reform Employment Response by Pre-Reform Labor Market Attachment

	(1) Outside of L.F.	(2) Employed less than 20 hrs/week	(3) Employed 20–29 hrs/week	(4) Employed Full Time
Low attachment in -1:				
Treated × Post	-0.0410***	0.0053	0.0226***	0.0127
	(0.0131)	(0.0093)	(0.0075)	(0.0102)
N	62587	62587	62587	62587
Avg. Outcome in -1	0.809	0.106	0.031	0.054
Some attachment in -1:				
Treated × Post	-0.0115	-0.0241*	-0.0016	0.0450***
	(0.0102)	(0.0124)	(0.0127)	(0.0108)
N	62530	62530	62530	62530
Avg. Outcome in -1	0.106	0.409	0.350	0.135
Strong attachment in -1:				
Treated × Post	0.0036	-0.0243***	-0.0263***	0.0471***
	(0.0075)	(0.0065)	(0.0071)	(0.0116)
N	64509	64509	64509	64509
Avg. Outcome in -1	0.026	0.030	0.065	0.879

Sample of treated and counterfactual women, base ages 30–33. Sample period corresponds to the long-run impact of the reform on employment, including the time periods +8–+14 and the pre-reform reference period -1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Top panel corresponds to women with low attachment to the labor force prior to the reform in time -1, defined as having worked less than 477 hours in -1. Middle panel corresponds to women with some attachment to the labor force in -1, defined as having worked 477–1505 hours in -1. Bottom panel corresponds to women with strong attachment to the labor force in -1, defined as working more than 1505 hours in -1. Entire sample divided into 3 quantiles shown in each of 3 panels. Column (1) measures outside the labor force as equal to 1 if working 0 hours. Columns (2)–(4) measure employment as equal to 1 if working less than 20, 20–29, and more than 30 hours per week respectively. Regressions include controls for labor market conditions as defined in Section 3.3.2.

Among women with low attachment—who are primarily outside of the labor force pre-reform—the probability of remaining outside of the labor force significantly declines post-reform. This translates into significant increases in part-time employment. Among women with some attachment—who are primarily employed part-time—the probability of working less than 20 hours per week decreases post-reform. This decrease translates into significant increases in the probability of working full-time. Among those with strong labor force attachment—who are predominantly employed full-time in -1—treated women are significantly more likely to continue to work full-time relative to counterfactual women, with significant declines observed in employment less than full-time. Relative to women treated at older ages, the labor market responses of early treated women are markedly different across those with different pre-reform levels of labor market attachment: labor force participation increases among those with low labor market attachment while hours worked increase among those with some and strong of labor market attachment.

N.2 Impact of the Reform on Employment by Pre-Reform Number of Children

Table N.2 examines the importance of the number of children pre-reform in the impacts on employment. The four panels of Table N.2 correspond to women who had 0, 1, 2, and 3 or more children in time -1 respectively. As with pre-reform labor market status, women with different number of children also see markedly different changes in post-reform labor market outcomes.

Though the average woman in the sample has 1.5 children in -1, there are still some women who have no children pre-reform. Employment of women with no children, who also have the strongest attachment to the labor force, is largely unchanged, with slight declines in part-time employment. Increases in full-time employment are driven by women with children, in particular women with one child. Increases in employment are concentrated among women with children who, prior to the reform, are less attached to the labor force while women with no children see small changes in labor market outcomes.

Table N.2: Long-Run Post-Reform Employment Response by Pre-Reform Number of Children

	(1) Outside of L.F.	(2) Employed less than 20 hrs/week	(3) Employed 20–29 hrs/week	(4) Employed Full Time
0 Children in −1:				
Treated × Post	0.0028	-0.0155*	-0.0040	0.0151
	(0.0131)	(0.0093)	(0.0096)	(0.0156)
N	37930	37930	37930	37930
Avg. Outcome in −1	0.211	0.101	0.101	0.586
1 child in −1:				
Treated × Post	-0.0291**	-0.0171	-0.0093	0.0552***
	(0.0134)	(0.0108)	(0.0101)	(0.0124)
N	46189	46189	46189	46189
Avg. Outcome in −1	0.289	0.147	0.145	0.418
2 children in −1:				
Treated × Post	0.0068	-0.0160	0.0055	0.0090
	(0.0108)	(0.0101)	(0.0089)	(0.0112)
N	73371	73371	73371	73371
Avg. Outcome in −1	0.315	0.218	0.174	0.294
3+ children in −1:				
Treated × Post	-0.0102	0.0002	-0.0055	0.0209
	(0.0176)	(0.0154)	(0.0140)	(0.0152)
N	32136	32136	32136	32136
Avg. Outcome in −1	0.451	0.233	0.148	0.168

Sample of treated and counterfactual women, base ages 30–33. Sample period corresponds to the long-run impact of the reform on full-time employment, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Four panels correspond to women who have 0, 1, 2, and 3 or more children in −1 respectively. Column (1) measures outside the labor force as equal to 1 if working 0 hours. Columns (2)–(4) measure employment as equal to 1 if working less than 20, 20–29, and more than 30 hours per week respectively. Regressions include controls for labor market conditions as defined in Section 3.3.2.

O Fields of Study Which Increase as a Result of Education Reform

Table O.1: Highest Attained Degree by Narrow Field of Study Post-Reform

	Most common degrees, percent of sample
Bachelor, Nursing and carework	5.17
High School, General (leading to higher education)	2.32
High School, Nursing and carework	2.25
Bachelor, Pre-school/kindergarten teacher education	1.63
Bachelor, Social services	1.42
Bachelor, Primary/middle school teacher education	1.14
Bachelor, Health and welfare, other	1.14
High School, Health and welfare, other	0.99
Bachelor, Business and administration	0.98
Preparatory course for higher education	0.74
Bachelor, Vocational teacher training	0.67
High School, Manufacturing and extraction	0.52
High School, Business and administration	0.51
Bachelor, Supplementary education for teachers	0.48
High School, Therapy	0.42

Table reports the 15 most common narrow field of study degrees among sample of women who have attained (at least) a high school diploma by time +14.

Table O.2: The Estimated Impact of the Reform on Higher Education by Narrow Field of Study, Averaged Over Post-Reform Period

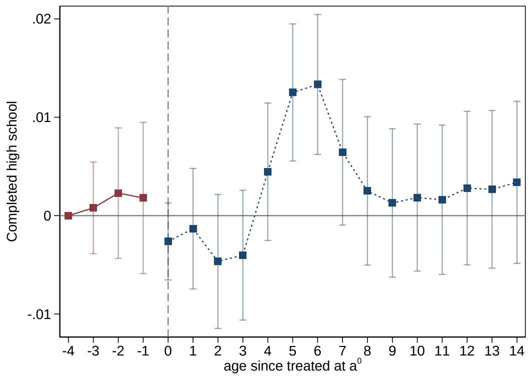
	(1) Higher Education Any Field	(2) Higher Education Health	(3) Higher Education Teacher	(4) Higher Education All Other Fields
Treated × Post	0.0105**	0.0067**	0.0004	0.0034
	(0.0043)	(0.0031)	(0.0019)	(0.0022)
N	189720	189720	189720	189720
Avg. Outcome in −1	0.041	0.021	0.011	0.009

Sample of treated and counterfactual women, base ages 30–33. Sample period corresponds to the long-run impact of the reform on field of higher education, including the time periods +8–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Column (1) measures the completion of higher education, irrespective of field of study. Column (2) measures the completion of higher education in healthcare. Column (3) measures the completion of higher education in teaching. Column (4) measures the completion of higher education in all other fields of study. Coefficients interpreted relative to omitted −1.

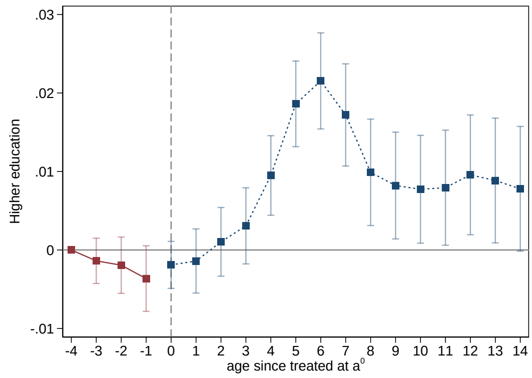
P Imputation Difference-in-Differences as in Borusyak, Jaravel, and Spiess (2024)

Figure P.1: Alternative Imputed Difference-in-Differences Estimator as in Borusyak, Jaravel, and Spiess (2024)

(a) High School



(b) Higher Education



(c) Years of Education

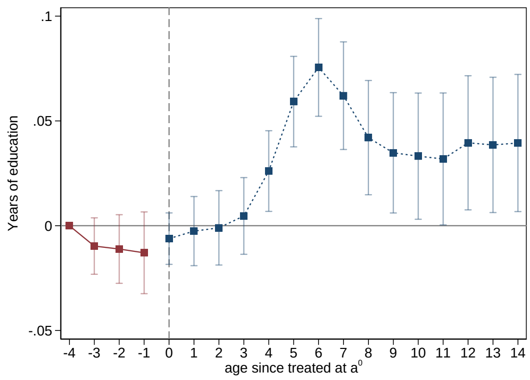


Figure plots estimates of imputation difference-in-differences as in Borusyak, Jaravel, and Spiess (2024) for outcome variables high school (panel a), higher education (panel b), and years of education (panel c). Imputation method estimates dynamic treatment effects which are robust to heterogeneity in the treatment effects, imputing counterfactual outcomes among all treated observations as defined in Section 3.

Q Heterogeneity of the Impact of the Education Reform on Education

Q.1 The importance of SES and early fertility

Figure Q.1: Estimated Impact on Years of Education by Subgroups

(a) Parental Education

(b) Age of First Birth

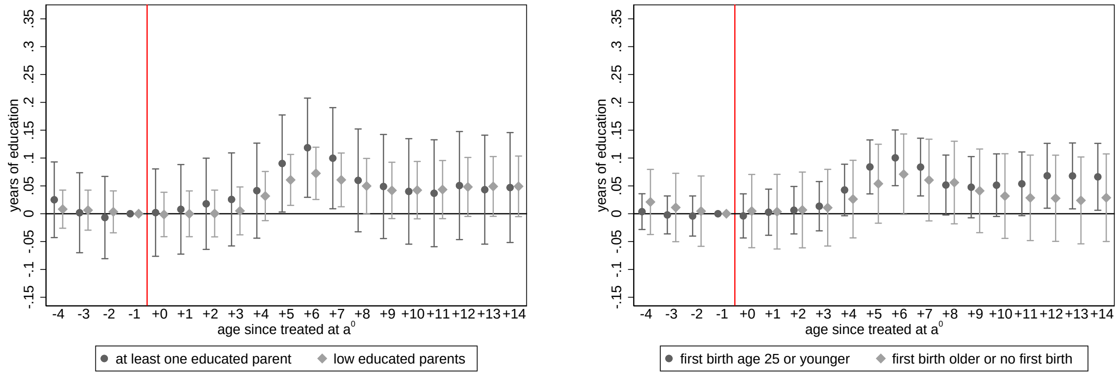
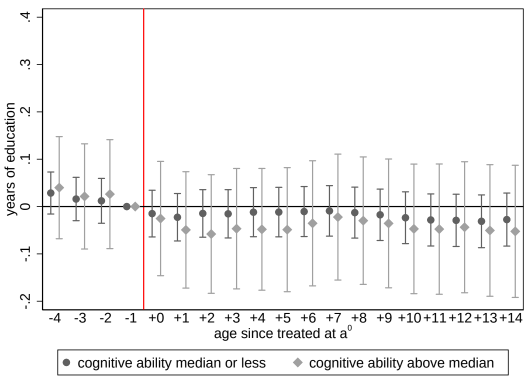


Figure plots estimates of equation (1), weighted by estimated propensity score in −1 as described in Section 3.3.2. Panel (a) plots separate estimates by educated/low educated parents for females. Panel (b) plots separate estimates by age of first birth for females. Sample of females of base ages 30–33. 95% confidence interval reported.

Q.2 The importance of cognitive ability

Figure Q.2: Years of Education, Separating by Cognitive Ability

(a) Male

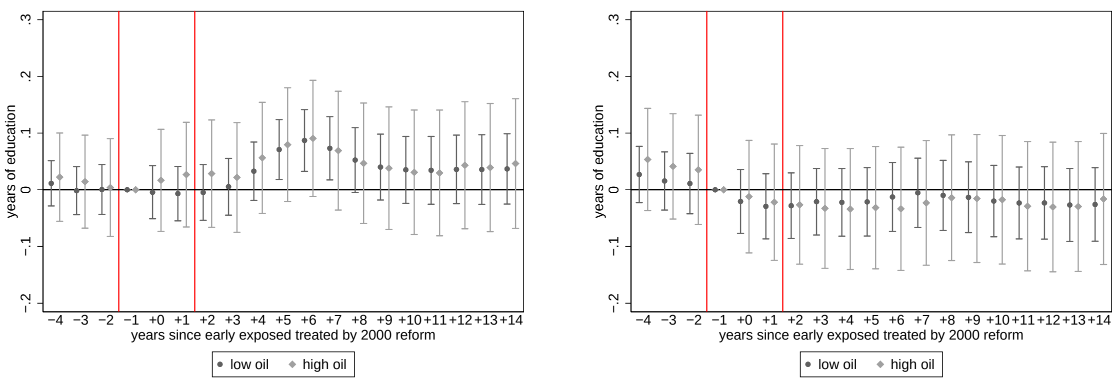


Panel (a) plots separate estimates by above/below cognitive ability for males. Data only available for males.

Q.3 The importance of the Norwegian oil boom

Figure Q.3: Years of Education, Separating by Oil

(a) Female (b) Male



Panel (a) plots separate estimates by oil exposure at birth local labor market for females. Panel (b) plots separate regression estimates by oil exposure at birth local labor market for males. High, middle, and low oil defined as in Bütikofer, Dalla-Zuanna, and Salvanes (2018) as a local labor market with employment in oil industry in 1980 greater than 10%, between 7.5 and 10%, and less than 7.5% respectively. 46 local labor markets as defined in Bhuller (2009).

R Robustness of Main Results

R.1 Baseline Results for Men—The Importance of Controls for Differences in Labor Market Outcomes

Table R.1: The Estimated Impact of the Reform on Labor Market Outcomes, Averaged Over Post-Reform Period

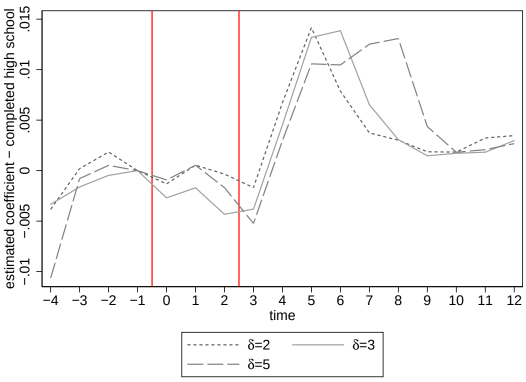
	Labor Earnings		Employment			
	(1) Log Annual Earnings	(2) Log Hourly Wage	(3) Outside of L.F.	(4) Employed less than 20 hrs/week	(5) Employed 20–29 hrs/week	(6) Employed 30+ hrs/week
Reduced-Form Regression:						
Treated × Post	0.0081	0.0027	-0.0001	0.0007	-0.0020	0.0028
	(0.0120)	(0.0091)	(0.0058)	(0.0030)	(0.0029)	(0.0059)
N	271876	221456	291042	291042	291042	291042
Avg. Reduced Form Outcome in −1	12.162	5.015	0.255	0.045	0.031	0.670

Sample of treated and counterfactual men, base ages 30–33. Sample period corresponds to the long-run impact of the reform on full-time employment, including the time periods +0–+14 and the pre-reform reference period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). First-stage regresses years of education on the same interaction using the sample from the reduced-form regressions. Column (1) measures labor earnings as the total annual earnings from employment. Column (2) measures hourly labor earnings, $(\frac{\text{annual earnings}}{\text{annual hours worked}})$. Column (3) measures outside the labor force as equal to 1 if working 0 hours. Columns (4)–(6) measure employment as equal to 1 if working less than 20, 20–29, and more than 30 hours per week respectively. Coefficients interpreted relative to omitted −1. Employment outcome variables measured as hours worked per week in a worker's main employment relationship at end of November.

R.2 Robustness to Varying Delta

Figure R.1: The Estimated Impacts of the Reform on Education, Varying Delta

(a) High School



(b) Higher Education

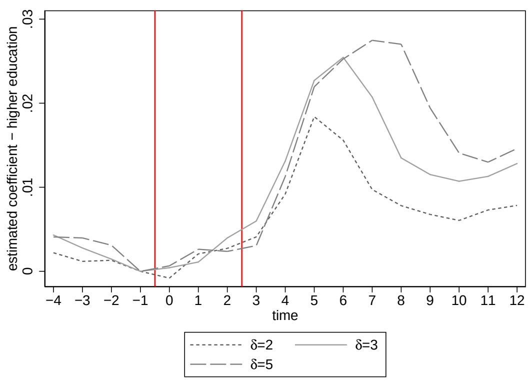
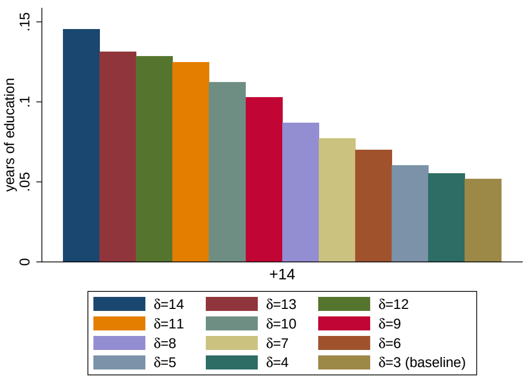


Figure plots estimates of equation 1 varying the level of δ, the number of years between when treated and counterfactual cohorts are treated for $\delta = 2$, $\delta = 3$ (the baseline used throughout the paper), and $\delta = 5$. Each point represents the difference in the outcome variable between treated and counterfactual cohorts at a specific point in time, relative to the same difference in -1. Panel (a) defines education as the completion of high school, panel (b) defines education as the completion of higher education. Sample of females of base ages 30–33. 95% confidence intervals reported.

Figure R.2: The Impacts of the Education Reform on Education 14 years Later, Comparing Different Counterfactual Cohorts

(a) Years of Education



(b) Higher Education

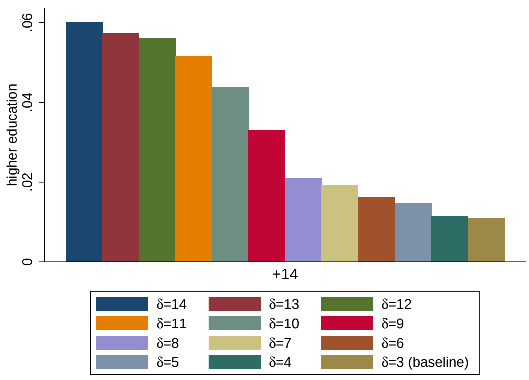


Figure plots estimates of the equation: $y_{it} = \sum_{k=-4}^{14} \delta_k D_{it}^{k\ years\ after\ reform} + \phi treated_{c(i)} + \tau_t + \psi_{j(i)} + \gamma_{l(i)} + u_{it}$, varying the number of years after which the counterfactual group is treated. Figure plots the impacts on education 14 years after the reform, where each bar represents the difference in the outcome variable between treated and counterfactual cohorts at $+14$, relative to the same difference in -1. Sample of females of base ages 30–33. 95% confidence intervals reported.

R.3 Comparing takeup of reform among different age groups

Table R.2: The Estimated Long-Run Impact of the Reform on Years of Education, Averaged Over Post-Reform Period from +8–+14 and Varying Ages of Sample

	(1) Years of Educ. 30–33	(2) Years of Educ. 34–37	(3) Years of Educ. 38–41
Treated × Post	0.0413**	0.0059	-0.0185
	(0.0177)	(0.0146)	(0.0277)
N	189301	193150	291330

Sample of treated and counterfactual women, base ages 30–33 (column 1), 34–37 (column 2), and 38–41 (column 3), from +8–+14, also including the pre-reform period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Outcome variable: years of education. Coefficients interpreted relative to omitted −1. Sample size of column (3) is larger due to data limitations in restricting the ages of dropout of the sample, which is not observed for older cohorts.

Table R.3: The Estimated Long-Run Impact of the Reform on Higher Education, Averaged Over Post-Reform Period from +8–+14 and Varying Ages of Sample

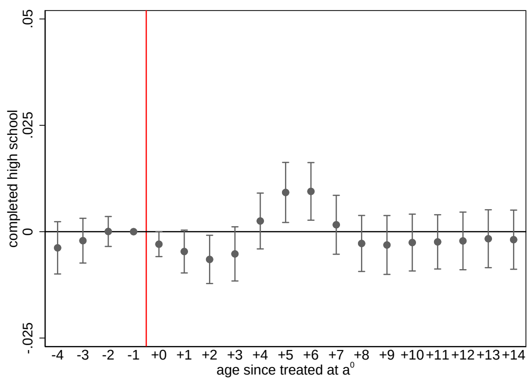
	(1) Higher Educ. 30–33	(2) Higher Educ. 34–37	(3) Higher Educ. 38–41
Treated × Post	0.0104**	0.0040	0.0001
	(0.0043)	(0.0034)	(0.0055)
N	189301	193336	291368

Sample of treated and counterfactual women, base ages 30–33 (column 1), 34–37 (column 2), and 38–41 (column 3), from +8–+14, also including the pre-reform period −1. Each column presents the interaction between treated and a post-reform indicator equal to 1, as defined in regression equation (2). Outcome variable: higher education. Coefficients interpreted relative to omitted −1. Sample size of column (3) is larger due to data limitations in restricting the ages of dropout of the sample, which is not observed for older cohorts.

R.4 Robustness to varying base ages of the estimation sample: 30–37

Figure R.3: The Estimated Impacts of Reform on Education, Ages 30–37

(a) Completion of High School



(b) Completion of Higher Education

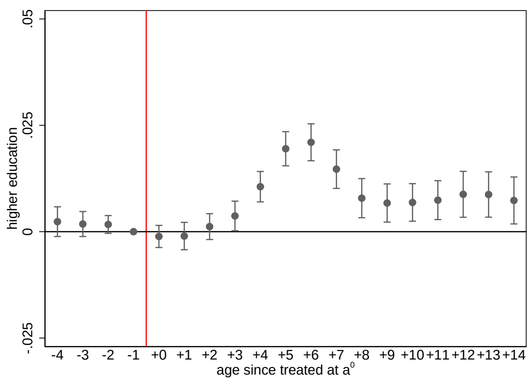
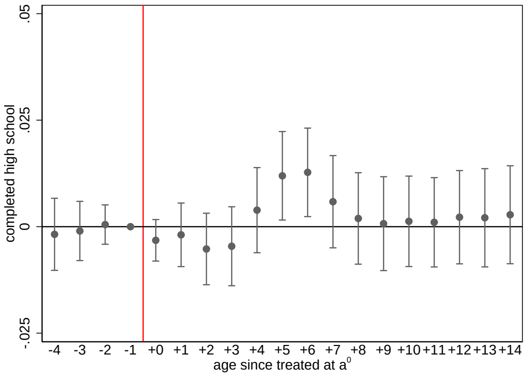


Figure plots estimates of equation 1. Each point represents the difference in the outcome variable between treated and counterfactual cohorts at a specific point in time, relative to the same difference in −1. Panel (a) defines education as the completion of high school, panel (b) defines education as the completion of higher education. Sample of females of base ages 30–37. 95% confidence intervals reported.

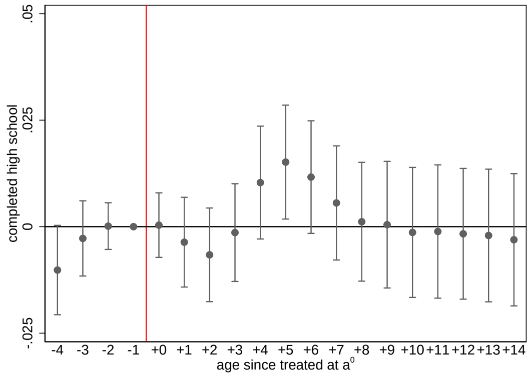
R.5 Robustness to varying base ages of estimation sample: comparing 30–33 to 38–41

Figure R.4: The Estimated Impacts of Reform on Education, Comparing Ages 30–33 to 38–41

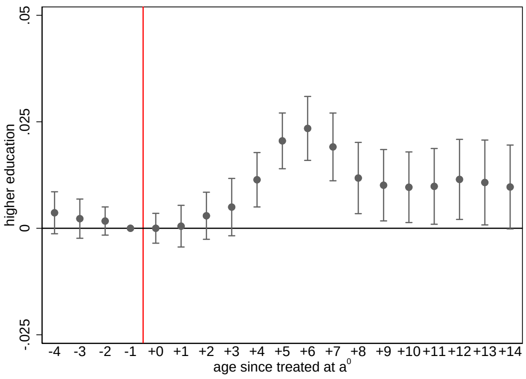
(a) Completion of High School, 30–33



(b) Completion of High School, 38–41



(c) Completion of Higher Education, 30–33



(d) Completion of Higher Education, 38–41

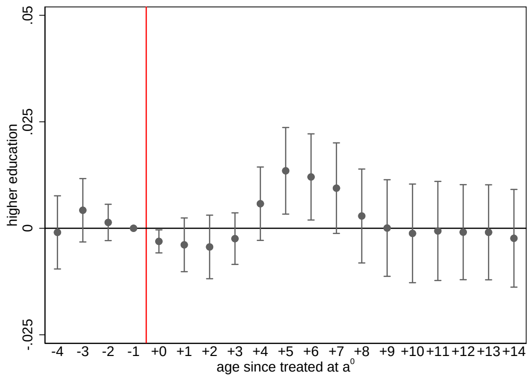


Figure plots estimates of equation 1. Each point represents the difference in the outcome variable between treated and counterfactual cohorts at a specific point in time, relative to the same difference in -1. Panels (a)–(b) define education as the completion of high school, panels (c)–(d) define education as the completion of higher education. Sample of females of base ages 30–33 (panels a and c) and base ages 38–41 (panels b and d). 95% confidence intervals reported.

R.6 Robustness to varying base ages of the estimation sample: 26–33

Figure R.5: The Estimated Impacts of Reform on Education, Ages 26–33

(a) Completion of High School (b) Completion of Higher Education

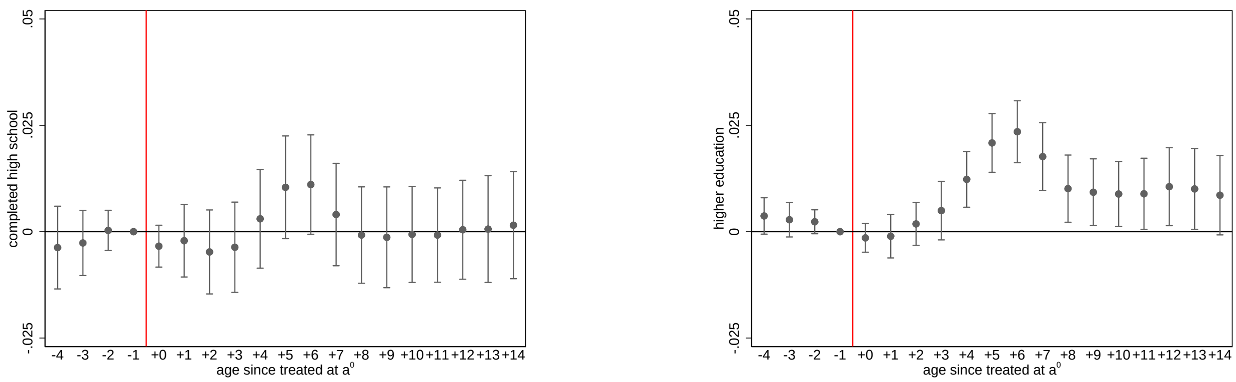


Figure plots estimates of equation 1. Each point represents the difference in the outcome variable between treated and counterfactual cohorts at a specific point in time, relative to the same difference in −1. Panel (a) defines education as the completion of high school, panel (b) defines education as the completion of higher education. Sample of females of base ages 26–33. 95% confidence intervals reported.

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