Fertility, Partner Choice, and Human Capital

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DISCUSSION PAPER

NHH

Institutt for samfunnsøkonomi
Department of Economics

SAM 11/24
ISSN: 0804-6824
July 2024

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July 1, 2024

Abstract

This paper generates new insights into the effect of education on fertility and partner choice across multiple generations. Using an intensity-of-treatment design, we leverage population-wide panel data for Norway in combination with a school reform in the 1930s, changing the instruction time during the school year in rural municipalities. The reform was binding for most of the rural population and allows us to estimate the effect of education on fertility behavior across the life-cycle, partner choice, and spillover effects on the next generation's fertility. We present robust evidence of reduced total fertility and an increase in the age at first birth driven by increased years of education, better labor market outcomes, and mating with better-educated partners. In addition, the reform also affected the fertility behavior of the children and decreased fertility rates across multiple generations.

1 Introduction

While women are more educated today than fifty years ago, they also have half as many children. Although the differences are decreasing, higher-educated women had, for many decades, fewer children than lower-educated women (Skirbekk, 2008). Today, this negative relationship between women's education and fertility no longer exists in the US and several European countries where fertility rates even increased for women with a postgraduate education (Doepke et al., 2023). On the one hand, the availability of artificial reproductive technologies lowered the risk of infertility for women who delayed childbirth due to investments in university education and careers (Sommer, 2016). On the other hand, also the decrease in unintended births among less-educated women in the US likely contributed to the recent flattening of the relationships between education and

*The authors gratefully acknowledge comments by Katrine V. Løken, Mikko Silliman and seminar participants at Stanford University, Uppsala University, Norwegian School of Economics, EALE, and SOLE. This work was partially supported by the Research Council of Norway through project No. 275800 and through its Centres of Excellence Scheme, FAIR project No. 262675.

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fertility (Buckles et al., 2022). Moreover, fertility is not only determined by women's preferences but often also by the desired fertility of their partners or spouses. As women and men do not always agree on fertility (see, e.g., Field et al., 2016), partner choice might play an essential role in determining whether and how many children a woman has.

The correlations between education and fertility do not necessarily imply that educational attainment causally affects fertility, but these relationships are potentially confounded by reverse causality. In addition, education might affect fertility through a third factor: marriage market outcomes. Hence, education, family formation, and labor force participation might be jointly determined (Willis, 1973; Geruso and Royer, 2018), and the interplay of these decisions is vital to central economic issues such as female labor force participation or fertility delay. It is, therefore, important for policymakers and social scientists to understand the causal effect of education on fertility and the potential mechanism mediating the impact. The literature on the causal relationship between female education and fertility primarily exploits changes in compulsory schooling laws that affect the age at which individuals are allowed to drop out of school. While Monstad et al. (2008) and McCrary and Royer (2011) argue that more education does not lower completed fertility, several papers have reported negative causal effects of education on teenage fertility, on fertility timing, and to some extent on total fertility (Breierova and Duflo, 2004; Aaronson et al., 2014a; Fort et al., 2016; Black et al., 2008). These results do not contradict the finding of the negative unconditional relationship between education and fertility, as women's skills and preferences might confound the correlations. So far, only Geruso and Royer (2018) discuss the causal relationship between education and fertility in connection to marriage market outcomes such as the education or family background of the spouse. However, the literature has long recognized the ties between education, partner choice, and fertility in forward-looking choice frameworks (Willis, 1973). Moreover, the choice of partner might affect not only an individual's fertility but also the fertility preferences of their offspring. However, we lack causal evidence of the effect of education on fertility measures across generations. In addition, most papers focus on cohorts who made fertility decisions after the family planning revolution (1960-1995) that contributed to a decline in fertility in developing countries (e.g., Goldin and Katz, 2002; Bailey, 2006; Myers, 2017). This focus makes it difficult to generalize the findings to historical periods and settings where access to contraceptives is limited.

In this paper, we study how the intensity and quality of schooling affect both fertility decisions and marriage market outcomes. To do so, we exploited a school reform in the 1930s, changing the number of weeks of instruction time during a school year and lowering the student-teacher ratio while keeping the years of compulsory schooling constant. In other words, the reform increases hours and quality of human capital investment without keeping adolescents in school for an additional year (see Acemoglu et al., 2024). This allows us to abstract from the direct effect of being in school until an older age, which is found to reduce peer group interaction and teen fertility. The education reform took place almost 90 years ago, which enables us to look at long-term outcomes such as completed fertility, the exact fertility timing, and partner choice in a context where means of family planning were minimal.[1] Moreover, the long period since the implementation allows us to measure spill-over effects on the next generation's fertility decisions and investigate intergenerational persistence in reform-induced changes. In addition, we analyze potential mechanisms behind changes in fertility, such as the opportunity costs of time and labor market outcomes.

The reform aimed at equalizing the educational attainment between urban and rural areas. Before the reform, the minimum required weeks of teaching was 36 weeks for urban schools and only 12-14 weeks for rural schools. While rural schools had more hours of schooling each week, students in rural areas still received significantly fewer hours of education than students in urban schools. Hence, levels of competencies based on school education differed substantially between rural and urban areas. The reform increased the minimum required weeks of education for rural schools by four weeks per year, corresponding to a sudden 30 percent increase in the required weeks of education during primary school. While the government only mandated the minimum number of weeks of schooling, a substantial share of municipalities offered more than the minimum required before the reform, partly funded by the central government. Hence, there was substantial variation in education provision before the reform. While 27 percent of the rural municipalities provided the required minimum or fewer weeks before the reform was implemented, 14 percent provided one week more, 20 percent two weeks more, and 23 percent three weeks more. 17 percent even offered four or more weeks above the minimum and hence complied already with the new requirement of the reform. This heterogeneity in provided weeks of schooling induces variation in the impact of the reform across municipalities. It permits a treatment and control strategy that allows us to estimate an intensity-of-treatment design similar to that of Card (1992), Bleakley (2007) and Acemoglu and Johnson (2007). In particular, our identification strategy combines the plausibly exogenous school reform with these cross-municipality differences in pre-reform weeks of schooling and cohort variation in the length of the reform exposure while at school. Notably, the reform we study affected a substantial share of the population in rural Norway. This alleviates critiques directed to previous research exploiting school reforms to estimate the effects of only a tiny and potentially non-representative group of students (Card, 2001).

We combine the intensity of treatment design with detailed historical data and rich administrative data on a range of family formation outcomes. The historical data are collected from different central and local archives and provide a detailed account of schooling offered in each municipality

[1]While access to hormonal contraceptives and abortion were instrumental in allowing women to delay their first birth in Western Europe and the US (see, e.g., Bailey, 2006; Mølland, 2016), the use of modern contraceptives remains, for example, low in sub-Saharan Africa due to high demand for children, high costs to access, or the pervasive belief that hormonal contraceptives make women permanently infertile (Zipfel, 2022; Ashraf et al., 2014; Bau et al., 2024). Hence, the context with limited use of modern means of birth control is still relevant today.

before and after the reform. The administrative data allow us to link an individual's birth year and municipality with fertility outcomes (e.g., age at first birth and completed fertility) and educational attainment. Moreover, we can observe mating market outcomes, such as the characteristics of their spouse. Studying changes in spousal characteristics is essential to comprehending the underlying channels, as partner preferences over children likely influence women's fertility outcomes. Moreover, we can link the reform-exposed cohorts to their children's fertility outcomes and study the generational persistence of the policy on fertility behavior.

We present four main findings. First, the reform reduced fertility for both women and men. The estimated decrease in fertility by 0.10–0.14 children is driven by an increase in the share of women with one or no children. Second, we find that treated individuals choose to delay fertility primarily by decreasing their fertility in their 20s. The reform has not altered the spacing of children. Third, we establish that increased education due to the reform and partnering with more educated spouses are likely channels behind our effects. Last, we show that the reform effect persists in the fertility behavior of the next generation and that the children of the treated individuals have about 1 percent fewer children. Our findings are robust to a wide range of sensitivity checks.[2]. In terms of magnitude, our estimates are comparable to the results from the impact of school construction programs. Breierova and Duflo (2004) estimates that a school construction program in Indonesia reduced fertility by 0.1 children per woman, but the result is not statistically significant. Aaronson et al. (2014a) find slightly larger—though insignificant—effects of the exposure to Rosenwald schools in the US. On the other hand, papers looking at the impact of changes in compulsory schooling typically find more minor impacts on total fertility (see, e.g. Monstad et al., 2008; Geruso and Royer, 2018). Our mating market results align with Geruso and Royer (2018), who show that increasing compulsory school education induced both men and women to marry more educated mates.

We add threefold to the literature on the effect of education on family formation.[3] First, we focus on a reform that affected school intensity and quality of mandatory schooling for a large share of the population. This is different from the work that examines the direct effects of being in school or being subject to increased supervision as a teenager (see, e.g., Geruso and Royer, 2018; Jacob and Lefgren, 2003). In this way, our paper expands the work that has studied reforms that increase the length of the school day (Berthelon and Kruger, 2011; Borrero, 2017) or the number of weeks in a school year (Fischer et al., 2019; Heckley et al., 2018) that has mainly focused on short-term outcomes such as teenage fertility. Second, we add to the small literature that considers the effect

[2]These policies include school breakfast programs mostly in urban schools (Bütikofer et al., 2018), access to infant health care centers (Bütikofer et al., 2019), and access to information about birth control through women's clinics in large cities.

[3]Examples include Aaronson et al. (2014a); Black et al. (2008); Currie and Moretti (2003); Cygan-Rehm and Maeder (2013); Fort et al. (2016); Geruso and Royer (2018); Grönqvist and Hall (2013); Lavy and Zablotsky (2015); McCrary and Royer (2011); Monstad et al. (2008).

of school reforms on family formation more broadly and studies consequences on fertility and partner choice simultaneously (see, e.g., Geruso and Royer, 2018). Overall, our results highlight how schooling may influence spousal characteristics and, thereby, inequality on the household level with consequences for their children and grandchildren. Last, by leveraging the long-time dimension of the Norwegian register data and reform that took place in the 1930s, we can assess long-term effects on completed fertility, fertility timing over the life cycle, and spillovers to the next generations. This adds to the literature on generational persistence of access to early child investments (see, e.g., Black et al., 2019; Bütikofer and Salvanes, 2020; East et al., 2022; Barr and Gibbs, 2022) by studying a reform that affected the first generation while they were already at school. In addition, these findings also contribute to the literature on the role of family experience when making fertility decisions (see, e.g., Fernández and Fogli, 2006) and indicate that the causal pathway from education to fertility persists for the next generation.

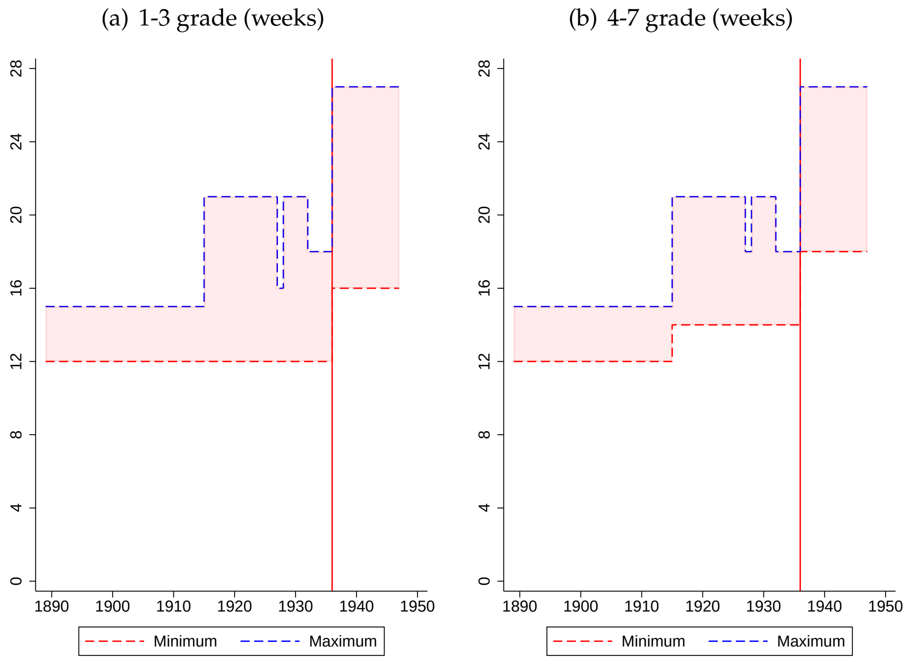
2 The Norwegian Rural Folk School Reform

In 1935, the first stable Labor government in Norwegian history was formed, and it remained unbroken in power for almost four decades, except for the Nazi occupation from 1940 to 1945. Among the first reforms this government initiated was the 1936 primary school reform. This school reform was primarily motivated by a need for a better-educated workforce in an economy undergoing massive structural changes and significant inequalities in teaching weeks between rural and urban schools. The minimum required weeks of teaching were 36 weeks for urban schools and 12 weeks for rural schools. Although rural schools had more hours of schooling each week, children in rural municipalities still received significantly fewer hours of education than children in urban schools. By 1935, the number of teaching days was, on average, 211 in cities, as compared to only 89 in rural districts (Acemoglu et al., 2024).

The Norwegian Parliament passed the reform in July 1936. It increased rural schools' minimum required weeks of schooling by four weeks per year, corresponding to a 30 percent increase in the required minimum weeks of education during primary school. Moreover, the maximum number of weeks of schooling that these municipalities could receive funding from the central government increased from 18 weeks to 27 weeks. Figure 1 plots rural municipalities' mandated minimum weeks of schooling from 1890 to 1950. The figure also shows the maximum weeks of education for which rural municipalities could receive partial funding from the county and state each year. Except for an increase in 1915 of 2 weeks for grades 4-7, no changes to the minimum number of teaching weeks were made for rural schools for more than 40 years before the reform we study.

In addition, the reform included increased minimum wages for teachers, a decrease in the maximum number of students per teacher from 30 to 35, and the abolishment of physical punishment. These reform components are likely to have improved the quality of schooling (Angrist and Lavy,

Figure 1: Mandatory Schooling Laws 1890-1950



Note: The figures show the minimum and maximum weeks of schooling that rural Norwegian municipalities could provide from 1889 to 1950. Sources: Kyrkje- og undervisningsdepartementet (1936,9).

1999; Fredriksson et al., 2012; Leuven and Løkken, 2018). Moreover, a large share of the funding and the responsibilities for teacher salaries, school buildings, books, inventories, and houses for the teachers were allocated to the central government (Rust, 1989; *Lov om Folkeskolen paa Landet*, 1936). The reform had two main elements: lowering regional inequalities and more centralized funding and administration.

Notably, the number of weeks of schooling varied a lot across rural municipalities before the reform. 26.6 percent of the rural municipalities provided only the minimum or fewer weeks of schooling, while 3.4 percent provided the maximum or more. The average increase in weeks of schooling necessary to meet the new requirements was around 15.6 percent. The main reason for the heterogeneity across municipalities was the local government's willingness or ability to provide school funding in the often dire economic conditions in the 1920s and 1930s. This heterogeneity across municipalities provides variation in the intensity of the reform-induced change in school weeks, which is critical to our identification strategy. Moreover, it is crucial for our identification strategy that the reform was implemented relatively quickly. The Parliament demanded that schools implement the reform for all grades by the school year 1942/43, meaning that schools had five years to implement the reform. Most municipalities implemented the reform in the school year 1937/1938. The swift implementation was also made possible by an abundance of teachers.

Contemporary sources suggest that approximately 1,700 teachers were unemployed during the fall of 1935, while the reform caused a demand for 700–800 new teachers (*Folkeskolens nivå må heves*, 1936). Figure 2 plots the average, quartile, maximum, and minimum weeks of schooling from 1930 to 1950 and documents the pre-reform variation and the rapid implementation after 1936.

3 Data and Measurement

We combine individual-level administrative data and population census data with newly collected municipality-level data on the Norwegian primary school reform roll-out.

3.1 Primary schools

Municipality-level data on Norwegian primary schools from 1930-1950 are collected and digitized from different local archives to construct measures of the change in schooling induced by the primary school reform. As the basis for public funding, municipalities report on a range of topics to the Ministry of Education.[4] We have digitized these reports from the 1930s onward to construct a municipality-level data set on school characteristics, including the actual weeks of schooling each year. In our analysis, we use planned weeks of education in *småskolen* (1-3 grade) and *storskolen* (4-7 grade) to calculate the expected increase in weeks of schooling induced by the reform. Section 4 provides a detailed discussion of how we use these data to construct our treatment variable.

3.2 Individual Level Data

Statistics Norway maintains individual-level administrative data compiled from administrative registries, such as the central population registry, which identifies births and families, the education registry, and the earnings registry. Our primary sample includes individuals affected by the reform born in rural municipalities from 1911 to 1945. We use a sample of children of the directly affected cohorts born 1945-1976 to analyze intergenerational mobility.

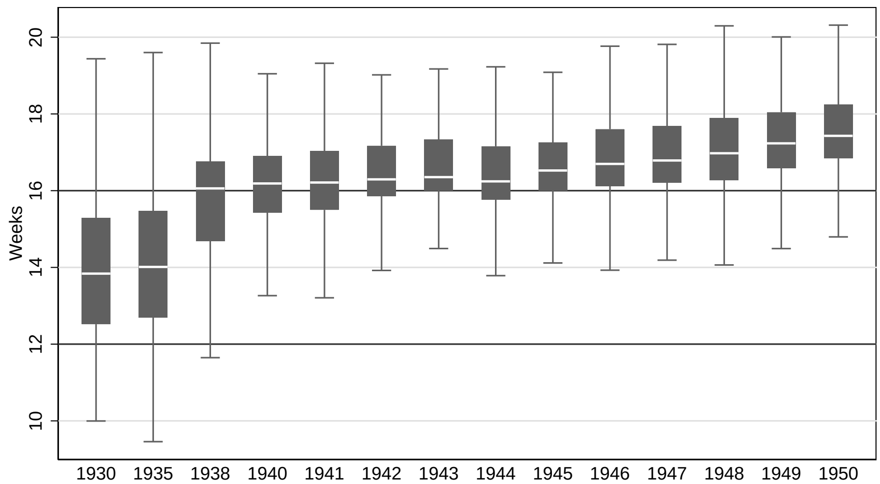
We assign the treatment based on the municipality of birth from the central population registry.[5] This may lead to measurement issues depending on the degree of geographic mobility of families between birth and school entry. However, less than 15 percent of individuals in our sample moved early in their lives, and the geographic reallocation does not change the number of school weeks

[4]The Norwegian primary school system was and still is predominantly public and relied to some degree on funding from the central government. Using data from 1930, we find that 99,5 percent of children of primary school age living in rural areas received their education in the public school system. Note that the law mandated parents to send their children to school and could be fined for not complying.

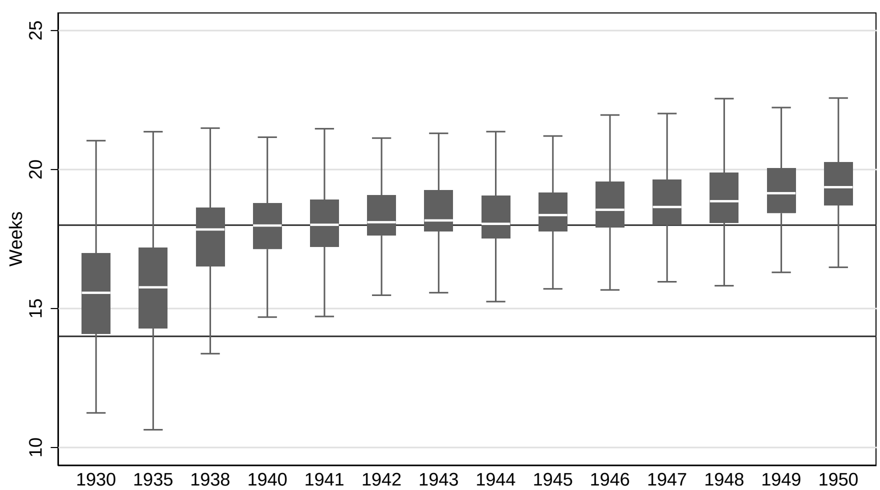
[5]Residency has only been included in the population registry since 1967.

Figure 2: Trends in Weeks of Schooling

(a) 1-3. grade (småskolen)



(b) 4-7. grade (storskolen)



Note: The figures present trends in weeks of schooling during 1-3 grade (småskolen) and 4-7 grade (storskolen) from data at the municipality level. The outer bars represent the upper and lower adjacent values, the upper and lower part of the box is the upper and lower 25 percent, and the white horizontal line is the median value. The sample includes only rural municipalities.

substantially.[6] Hence, the municipality of birth is a reasonable proxy for the municipality of residence during primary school.

We measure fertility by linking parents to their children using the personal identifier in the central population register. We use this link to calculate the number of children for the treated cohorts, their age at first birth, and spacing between births. In addition, we supplement this data with full-count population censuses from 1960, 1970, and 1980. We construct an alternative measure of the total number of children by supplementing the registry data with the census question for the number of children in current marriages.[7] Using the registry and census data, We construct variables for whether an individual was ever married or divorced. For the second generation, the children of the treated cohorts, we use a similar procedure in addition to the medical birth register with detailed birth information since 1967. We link individuals to their partners using data from the population register.

To investigate mechanisms, we exploit information on earnings and educational attainment. We use earnings from the tax registries. The earnings measure is pre-tax labor earnings, including income for self-employment, and it includes some transfers such as unemployment benefits and maternity leave benefits (very minor in this period). Based on this, we calculate the average discounted earnings between ages 50 and 64. We use the same earnings measure for their children at 30-34 years. We define labor force participation as the share of years an individual has income higher than the lowest 10 percent of non-zero incomes. The education register and censuses contain individual-level data on educational attainment both for the treated cohorts and their children.

3.3 Municipality Level Data

In our main specifications, we control for municipality-level characteristics such as the labor force share in agriculture and fishery and the regional income level in 1930 using data from the 1930 population census. We calculate labor shares as the share of adults (older than 14 years) working in agriculture or fishery in the 1930 population census and include both men and women. The municipality-level income data is the average income for the adult population, as measured in the tax records. These income measures were collected as a part of the 1930 census. We also collected municipality-level income data in 1915 and 1920 from yearly publications from Statistics Norway. Summary statistics are presented in Table 1.

[6] The data from military enrollment tests for the 1930 and 1931 male cohorts include information on the municipality of birth and municipality of residence when attending primary school. We find that only 13 percent participate in primary school in a municipality different from their municipality of birth. To infer selective mobility by treatment intensity, we compare the level of schooling in the movers' municipality of birth with their municipality of residence during primary school and find that, on average, movers move to municipalities with slightly more weeks of schooling, the difference is less than three days of education during a year.

[7] This alternative measure entails using the maximum of these two variables. We add children born after 1960 or 1970 using the registry data. We can only construct this measure for mothers, as they did not ask males about the number of children in these censuses.

Table 1: Summary Statistics

	μ	σ	Min.	Median	Max.	N
First generation						
No. of children	2.27	1.56	0.00	2.00	18.00	487,025
Share with child	0.85	0.36	0.00	1.00	1.00	487,025
No. of children (>0)	2.69	1.32	1.00	2.00	18.00	410,728
Year of birth	1929.57	9.88	1911.00	1930.00	1945.00	487,025
Years of Education	9.43	2.58	6.00	10.00	21.00	486,476
Log income	11.70	0.79	9.22	11.92	12.91	419,715
Labor market participation	0.67	0.37	0.00	0.87	1.00	485,730
Age of first birth	26.86	5.39	9.00	26.08	74.75	410,728
Age of last birth	33.32	6.13	11.00	33.08	76.83	410,728
Second generation						
No. of children	1.96	1.22	0.00	2.00	15.00	568,188
Year of birth	1960.38	8.15	1945.00	1961.00	1976.00	587,246
Years of Education	12.30	2.62	5.00	12.00	21.00	566,056
Log income	12.06	0.58	9.90	12.21	12.96	528,240
Labor market participation	0.81	0.33	0.00	1.00	1.00	581,666
Municipality-level variables						
Gini at muni. level 1930	0.43	0.06	0.287	0.422	0.604	343
Av. income 1930	0.51	0.18	0.187	0.477	1.424	362
Av. wealth 1930	2.20	1.43	0.204	1.892	15.420	362
Teacher ratio 1935	36.51	8.28	18.714	35.951	68.000	363
Share agri. 1930	0.49	0.21	0.006	0.537	0.904	367
Share fish 1930	0.16	0.23	0.000	0.031	0.800	367

Note: The table presents basic descriptive statistics of key individual- and municipality-level variables.

4 Empirical Strategy

To study how education affects fertility, we exploit the implementation of the 1936 Norwegian Rural Folk school reform in an intensity-of-treatment design similar to that of Card (1992) and Acemoglu and Johnson (2007) and we rely on the same reform and set-up as Acemoglu et al. (2024).[8] The idea behind the strategy is that the reform led to a larger change in municipalities that had to increase the number of teaching weeks by more to meet the new standard given by the reform and for cohorts with more of their primary school education under the new regime. The identification of the effect of the reform relies on (i) regional variation in treatment intensity, (ii) the reform's swift implementation and convergence in school weeks across municipalities, and (iii) the exogeneity of the reform.

[8]Other examples of the intensity of treatment design are Duflo (2001), Bleakley (2007), and Bütikofer and Salvanes (2020).

4.1 Treatment

The treatment variable defines the treatment intensity for each cohort in each rural municipality. The new law required schools to provide 16 weeks of schooling per year during 1-3 grade and 18 weeks during 4-7 grade. To fulfill the requirements of the new law, municipality j had to increase weeks of schooling from their pre-reform level of $pre_j^{\text{små}}$ and pre_j^{stor} to 16 and 18 weeks, respectively, thus creating regional variation in the intensity of the reform.[9] This increase is equal to $max(16 - pre_j^{\text{små}}, 0)$ for *småskolen* and $max(18 - pre_j^{stor}, 0)$ for *storskolen*, denoted as $b_j^{\text{små}}$ and b_j^{stor}. We define a measure of the intensity of treatment in each municipality j as:

$$P_j = \frac{3 \times b_j^{\text{små}} + 4 \times b_j^{stor}}{28}, \tag{1}$$

where P_j is a weighted average of the increase in school weeks for the different grades and normalized by the maximum increase in weeks of schooling for an individual fully exposed to the reform.[10] P_j can be interpreted as the proportion of the full reform experienced in municipality j by cohorts that attend primary school fully in the reformed system. A value of 1 indicates that municipality j had to increase weeks of schooling by four weeks, while a value of zero indicates that no increase was necessary to fulfill the new law's requirements.

To test this relationship, we plot the reform exposure P_j in each municipality against the change in weeks of schooling from 1935 to 1942 in Figure 3. We find a clear positive relationship between the treatment intensity in each municipality and the reform-induced increase in weeks of schooling. Increasing the municipality-level treatment intensity P_j by one is associated with a rise of four extra weeks of education.

In addition to spatial variation in the exposure to the reform, treatment is also determined by the years an individual spends in the reformed school system. We assume that schools implemented the reform in the 1938/1939 school year and that all individuals attend primary school between the ages of 7 and 14.[11] Hence, cohorts born in 1923 or earlier are not directly affected by the reform. The 1924 birth cohort attended 7th grade during the 1938/39 school year, meaning they attended one year of schooling in the reformed system. Younger cohorts received proportionally more years of treatment. Cohorts born in 1931 or later attended primary school from the fall of 1938 and thus experienced the full effect of the reform. We denote the years each cohort t is affected by the reform as $treat_t$.

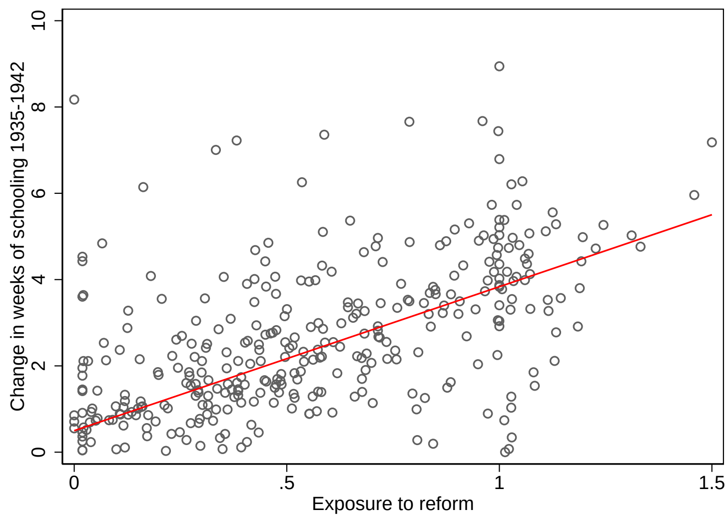
Our measure of the municipality-cohort-level intensity of treatment Z_{jt} is the sum of increases in weeks of schooling induced by the reform for cohort t in municipality j. Specifically, we calculate

[9] As weeks of schooling in *småskolen* and *storskolen* can differ within a municipality and across grades, we calculate the pre-reform level as the mean number of planned weeks in 1935.

[10] The increase in yearly weeks of schooling was 4 in *småskolen* (16-12 weeks) and 4 in *storskolen* (18-14 weeks). The increase in minimum weeks of schooling across all seven years in primary school is therefore $4 \times 3 + 4 \times 4 = 28$.

[11] For the sample we consider, the school starting age was seven years.

Figure 3: Treatment Intensity and Change in Weeks of Schooling 1935-1942



Note: This figure plots the municipality-level change in mean weeks of schooling from 1935 to 1942 against the treatment intensity of the 1936 primary school reform (as measured by P_j, see Equation 1).

it as:

$$Z_{jt} = \frac{\sum_{a=5}^{7} b_j^{\text{små}} \mathbb{1}(treat_t = a) + \sum_{a=1}^{4} b_j^{stor} \mathbb{1}(treat_t = a)}{28}, \tag{2}$$

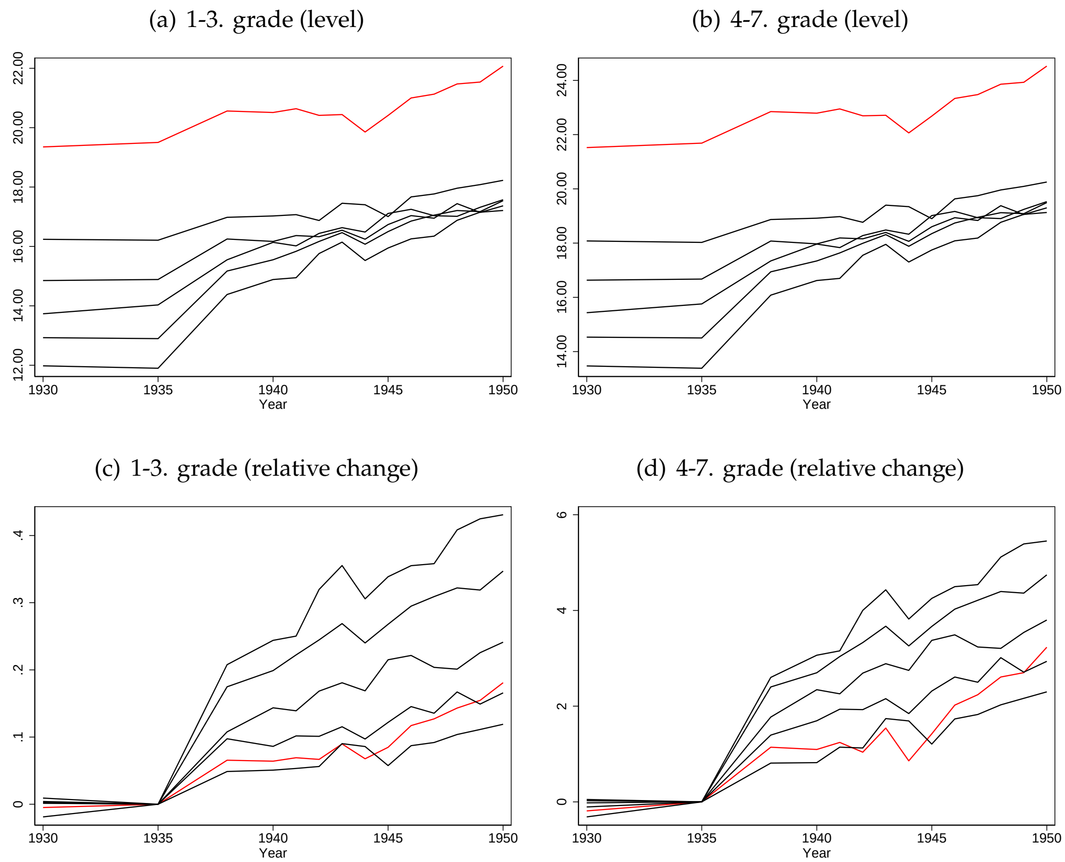
where $\mathbb{1}(treat_t = a)$ indicate whether cohort t experienced a years of schooling in the reformed primary school system. Importantly, the variation in the intensity of treatment variable Z_{jt} comes from two sources: the variation in the intensity of treatment across municipalities and the variation in years of schooling a cohort is affected by the reform. This variation will allow us to capture the causal effect of the reform.

To visualize the speed of the reform implementation and the convergence in the number of school weeks across regions, we plot absolute weeks of schooling and the percentage change in school weeks by treatment intensity in Figure 4. All four panels show a very high degree of early reform implementation and a quick convergence in weeks of schooling across municipalities with virtually flat pre-trends—all central to our identification strategy. However, municipalities with a higher treatment intensity value took somewhat longer to implement the reform as they had to change their schooling system the most.

4.2 Empirical Specification

We estimate the event-study relationship between reform exposure P_j of individual i and fertility outcomes. We exploit the variation in treatment intensity at the municipality level:

Figure 4: Trends in Weeks of Schooling by Reform Intensity



Note: The figures show trends in weeks of schooling by treatment intensity quantiles using municipality-level data. Figures (a) and (b) show absolute weeks of schooling per year in 1-3 grades and 4-7 grades. Figure (c) and (d) show the percentage change from the base year 1935. Higher quantiles are treated more. The red line marks municipalities with weeks of schooling above what we would expect from rural municipalities, and we suspect that these municipalities have gained permission to follow the urban schooling law. These municipalities are, therefore, excluded from our main analysis.

$$Y_{ijt} = \alpha + \sum_{l \neq 1924} \beta_l (d_l \times P_j) + \theta_j + \eta_t + \varepsilon_{ijt}, \tag{3}$$

where Y_{ijt} denotes the fertility or other outcomes we analyze for individual i, born in year t in municipality j. θ_j are the municipality of birth fixed effects and η_t are cohort dummies. We cluster the standard errors at the municipality level. The key coefficients of interest are in the vector β_t, capturing the reduced-form impact on cohorts $t \in [1917, 1945]$. We omit $t = 1924$, the last cohort unaffected by the reform. This makes β_t effect sizes relative to the 1924 cohort. Our sample includes only individuals born in rural municipalities, implying that we do not consider convergence between rural and urban areas. Crucially, to recover the causal effect, we invoke a «strong» parallel trends assumption as proposed by Callaway et al. (2021). This differs from the standard parallel trends assumption by requiring that the average outcome for units with different levels of treatment intensity develop similarly as those treated with a particular treatment intensity P_j had they received the same treatment intensity.

Second, we estimate a specification assuming that the impact of the reform is proportional to years spent in the reformed primary school system. Specifically, we estimate:

$$Y_{ijt} = \alpha + \beta Z_{jt} + \sum_{l} \gamma_l (d_l \times \mathbf{X_j}) + \theta_j + \eta_t + \varepsilon_{ijt}, \tag{4}$$

where Z_{jt} is the estimated fraction of the entire reform experiences by individual i born in year t in municipality j. $d_l \times \mathbf{X_j}$ captures year-of-birth fixed effects interacted with a set of municipality-level control variables, meaning we allow the effect of the control variables to differ by cohort. β is the coefficient of interest, capturing the estimated impact of moving from the old minimum requirement of the law ($Z_{jt} = 1$) to the new minimum requirements ($Z_{jt} = 0$). The linearity assumption implies that the impact of going from 1 to 2 years of schooling in the reformed system is the same as going from 6 to 7. The takeaway from Figure 7 is that this specification fits the data well. We find little or no signs of non-linearities in the impact of the reform on our outcomes of interest; thus, we will use Equation 4 as our preferred specification.

We use a similar setup for the second generation—the offspring of the directly affected cohort. First, we use an event-study setup by linking municipality-level reform intensity P_j to the outcome of interest Y_{ijtc}:

$$Y_{ijtc} = \alpha + \sum_{l \neq 1924} \beta_l (d_l \times Z_j) + \theta_j + \eta_t + \iota c + \varepsilon_{ijt}, \tag{5}$$

where t is the year of birth of the first generation, and c is the year of birth of the second generation. The observation unit is the children of those directly affected by the reform. Nevertheless, we use the same control variables for the directly exposed generation and add fixed effects for the birth year of the second generation to remove any mechanical effects from delays in fertility. Regressions are weighted such that each first-generation parent gets equal weight.

Similar to the setup for the first generation, we estimate an alternative specification for the full effect of the reform, assuming that the reform impact is linear in years spent in the reformed primary school system:

$$Y_{ijtc} = \alpha + \beta Z_{jt} + \sum_{l} \gamma_l (d_l \times \mathbf{X_j}) + \theta_j + \eta_t + \iota c + \varepsilon_{ijt}, \tag{6}$$

where β is the coefficient of interest. We interpret this as the effect of one parent going from the old minimum required weeks of schooling to the new minimum. We weight the regressions such that each first-generation parent gets equal weight.

Finally, we analyze the short-term impact of the primary school reform. Note that these regressions are based on municipality-level rather than individual-level data. We estimate the following specification:

$$Y_{jt} = \alpha + \beta \left(P_j \times \text{Post}_t\right) + \sum_l \gamma_l (d_l \times \mathbf{X_j}) + \theta_j + \eta_t + \varepsilon_{ijt}, \tag{7}$$

where $Post_t = 1 \, \forall \, t > 1937$ and t is the school year $t/t+1$. We mainly use this specification to investigate the short-run effects of the reform on contemporary outcomes that could impact our long-run results and to measure the actual increase in weeks of schooling that our identification can pick up. Observations are weighted by the size of the population older than 14 years in 1930.

Our identification strategy relies on (i) regional variation in reform exposure, (ii) swift convergence in school weeks across municipalities, and (iii) the exogeneity of the reform. We have demonstrated the swift convergence above. Figure 5 illustrates the regional variation in the exposure. In particular, Panel (a) plots the raw average weeks of schooling for each municipality in 1935, while Panel (b) plots our measure of treatment intensity P_j. In Panel (b), darker colors indicate a higher treatment intensity than lighter colors, and black indicates city status. A municipality-level treatment intensity P_j of zero indicates that a municipality has already fulfilled the minimum requirements of the new legislation pre-reform. Municipalities with a P_j greater than one did not meet the requirements of the old standard. Although municipalities with the lowest reform exposure were primarily located in the southern parts of Norway around cities and municipalities with the highest reform exposure were in the North and areas with high fishing and farming activity, there is significant variation in school days across the country.

The identification relies on the exogeneity of the reform concerning the number of school weeks prior to the reform. To measure the relationship between the treatment intensity and municipalities' industry structure or location, we present results from a regression of pre-determined municipality-level characteristics and reform exposure in Table 2. Several municipality-level characteristics strongly correlate with treatment intensity. These characteristics include the agricultural share and fishery share. To account for this, we include them as flexible controls, interacting the control variables with cohort-fixed effects.

Our intensity of treatment measure captures expected increases in weeks of schooling. If this aspect of the primary school reform is correlated with the quality component of the reform, then our estimate will be a bundle of the effect of both the quantity and quality of schooling. One of the quality components of the reform was a reduction in the highest allowed student-teacher ratio. We show in Table 2 that the treatment intensity correlates with the student-teacher ratio in 1935, meaning that our estimates will also capture parts of the quality component of the reform (Acemoglu et al., 2024).

Table 2: Determinants of Reform Intensity

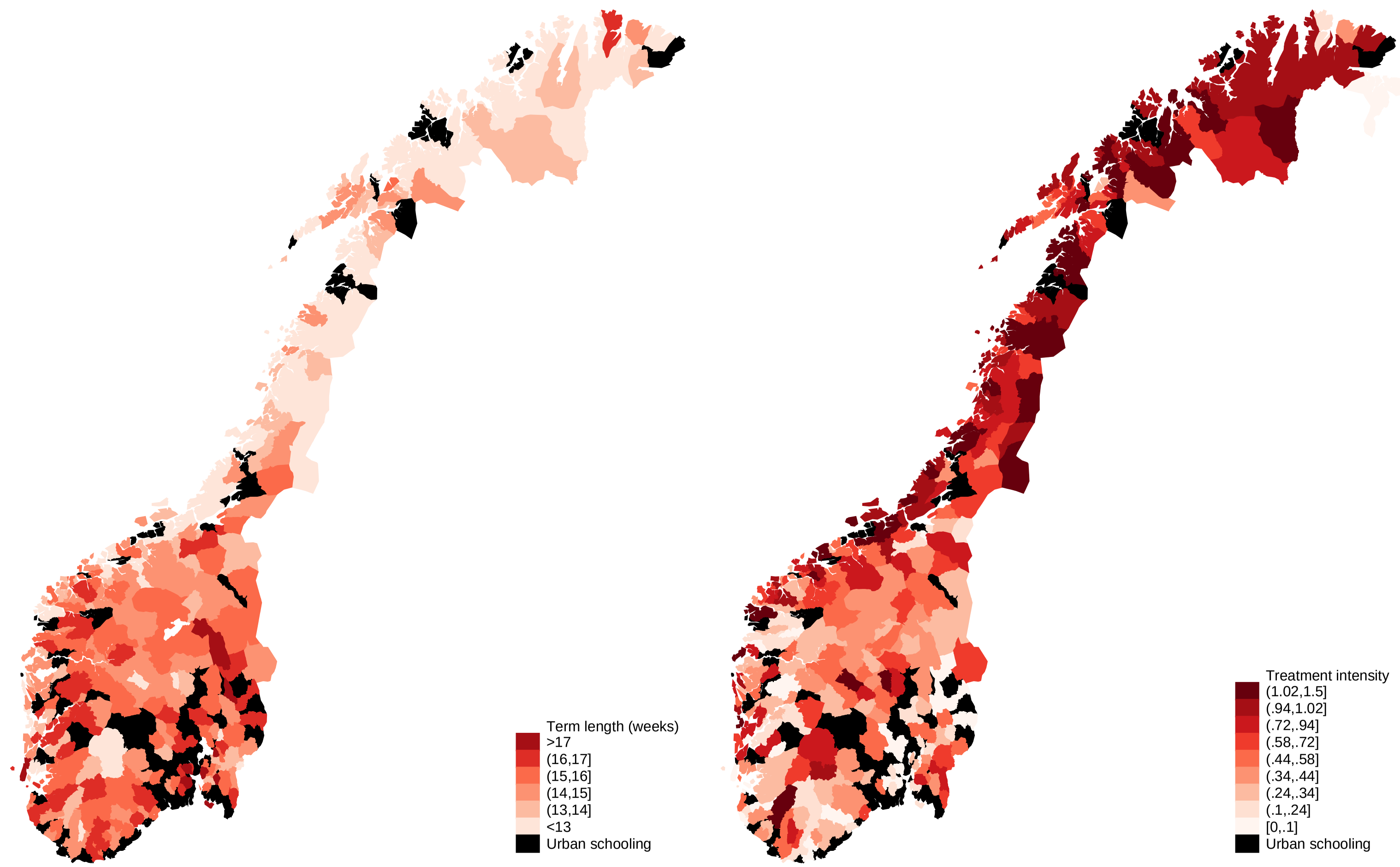
	Reform Exposure	Reform Exposure
Teacher ratio 1935	0.576***	0.448***
	(10.93)	(7.67)
Share qualified teacher	37.98***	21.15***
	(11.00)	(5.69)
Income 1930		-13.74***
		(-4.65)
Fishery share 1930		14.78***
		(3.85)
Agriculture share 1930		12.84***
		(3.54)
Female		0.722
		(0.06)
North		-0.328
		(-0.17)
N	338	338

Note: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table presents results from two regressions where we regress the measure of treatment intensity on a range of municipality-level variables. Data include rural municipalities only.

Figure 5: Spatial Variation in Schooling and Reform Intensity

(a) Weeks of Schooling in 1935

(b) Intensity of treatment



Note: The two maps plot spatial variation in 1935 schooling and intensity of the 1936 primary school reform. Figure (a) plots the average weeks of schooling during primary school in Norwegian municipalities based on data from 1935. Figure (b) plots the treatment intensity of Norwegian municipalities. Municipalities that follow the urban schooling law have been grouped with urban municipalities.

A further possible concern is that our results are impacted by mean reversion. We might, for example, expect that municipalities with higher fertility rates or little schooling would have converged to the mean even without the primary school reform. To account for this concern, we use data from the 1930 census to control for the municipality income level interacted with the year of birth.

5 Results

This paper investigates how education affects fertility using a school reform that increased the number and quality of school days during the year. We interpret our results as an intention-to-treat effect of an education reform. We use the variation in reform-induced change in instruction weeks as a measure of treatment intensity in our empirical specifications.[12] Before focusing on fertility measures, we first analyze the impact of the reform on actual school days to demonstrate that the reform had a real impact on schools. We present the contemporaneous effects of the reform on weeks of schooling. Second, we present our estimates for the reform's impact on fertility, looking at both total fertility along the intensive and the extensive margin. Last, we investigate the timing and spacing of fertility.

5.1 Contemporaneous Effects of Schooling

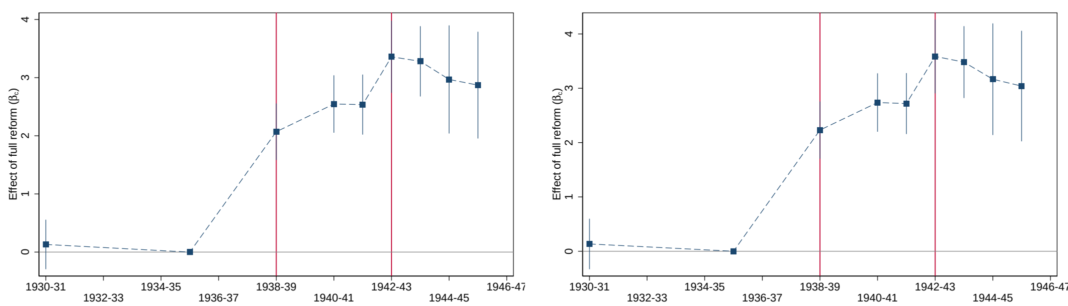
To ensure that our identification strategy captures the bulk of the reform-induced increase in completed education, we first investigate the contemporaneous effects of the school reform on weeks of schooling provided by municipalities. We use an event-study approach similar to that outlined in Equation 3 to estimate the relationship between the actual weeks of schooling provided by the municipalities and reform intensity each year. Figure 6 presents the results separately for the first three grades and the four remaining grades. Notably, the figure shows no significant pre-trend in weeks of schooling before the reform is passed. In addition, we find that the law change from the old to the new minimum number of weeks—a one-unit change— led to an immediate increase of about two weeks of schooling in 1938. The results document that municipalities further away from the new requirement had to increase weeks of education to a greater extent than municipalities already in compliance with the new regulations. These findings are also reflected in results presented in Table 3 where we estimate Equation 7 for weeks of schooling. A change from the old to the new minimum of weeks requirement increases schooling by 2.7 and 2.9 weeks, respectively. These estimates are statistically significant across all specifications, and the estimated increase is slightly more important for older children than younger children. The estimated increase suggests

[12] Note that we could use the treatment intensity measure Z_{jt} as an instrument for instruction time. This would require assuming that the reform only works through instruction time. However, the education reform we study—as most educational reforms—affected several educational quality dimensions which are highly correlated (Acemoglu et al., 2024). We, therefore, limit our analysis to reduced form specifications.

that children who benefitted from first to seventh grade from the reform received about 18-20 more weeks of mandatory schooling, which resulted in more than one year of education according to the 1940s standards. The difference between our estimate of about 2-3 weeks and the 4-week change between the new and the old minimum requirement is likely explained by municipalities already in compliance with the new regulations and increased their school weeks voluntarily above the required minimum or noise in our measurement.

Figure 6: School Year-Specific Relationships Between Weeks of Schooling and Reform Exposure

(a) 1-3. grade (b) 4-7. grade



Note: The figures plot the coefficients from a regression of actual weeks of schooling on our index of the expected increase in weeks of schooling induced by the primary school reform. The left-hand y-axis is the estimated impact on the weeks of schooling, β_t (similar to Equation 3). The x-axis is the school year $t/t+1$. The control variables included are municipality of birth dummies and year of birth dummies. The solid red lines are the beginning and end of the implementation period of the reform. The error bars are the 95 percent confidence interval for each coefficient. The standard errors are clustered at the municipality level.

Table 3: Contemporaneous Effects in Weeks of Schooling

	Weeks småskolen	Weeks storskolen	Average weeks
Reform	2.726***	2.930***	2.797***
	(0.270)	(0.294)	(0.286)
Pre-mean	14.466	16.194	15.494
% change	18.846	18.093	18.051
P-value	0.000	0.000	0.000
N	9,751,049	9,751,049	9,751,049

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality level are in parentheses. Each coefficient is the result of estimating Equation 7 and weighted by the 1930 population. Control variables included are municipality and year-fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

5.2 Effects on Fertility

Access to more schooling and better school quality could increase the opportunity cost of having children and thereby affect fertility both at the intensive and extensive margin (see, e.g., Aaronson et al., 2014b). First, we investigate the effect of the school reform on the extensive margin of fertility by estimating Equation 3 for men and women separately. The event-study estimates from Equation 3 on the probability of having at least one child are plotted in Panels (a) and (b) of Figure 7. The results from Equation 4 are presented in Panel A of Table 4. Figure 7 suggests that there is no visible pre-trend, and the plots indicate that the effect on the extensive margin is small and not very precisely estimated. The summary results in Table 4 for the difference-in-difference specification show that about 1.6 percentage points decrease in fertility along the extensive margin for women (significant on the 5 percent significance level). For men, the estimated decrease is not significant. When excluding the northernmost counties, the estimated effect for women is slightly smaller and only significant on the 10 percent significance level. Overall, this suggests that the reform had little to no impact on whether men and women had at least one child.

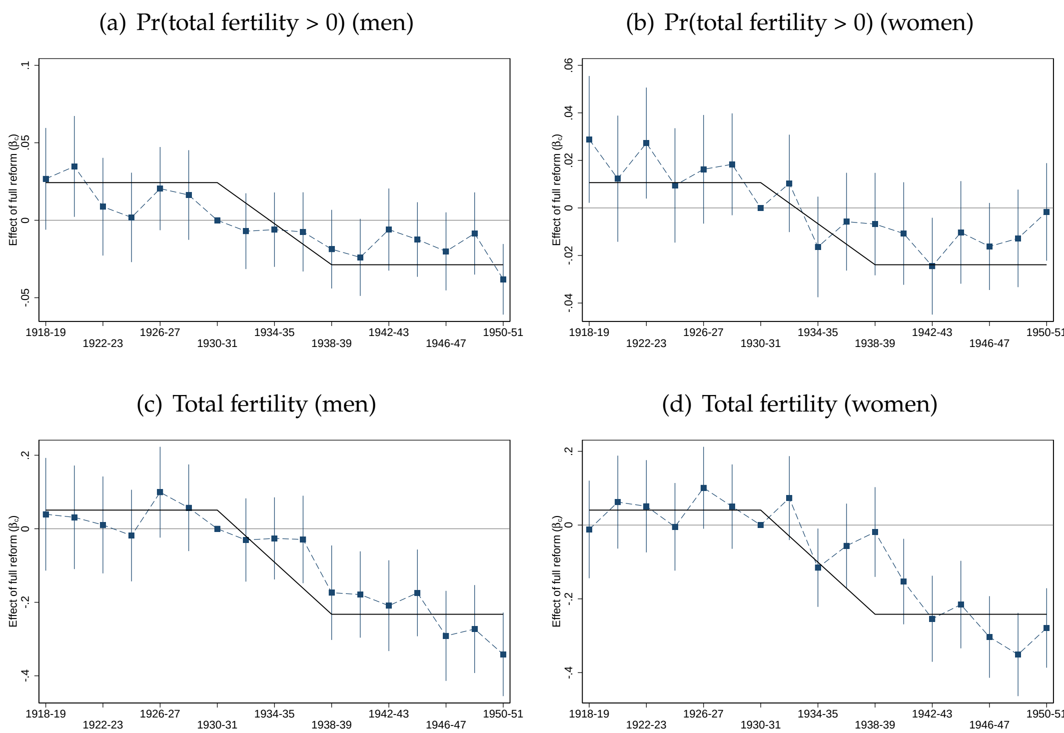
Table 4: Long-Term Effect of School Reform Exposure on Fertility

				Without North	
	Whole sample	Men	Women	Men	Women
		Panel A: Probability of Having Children			
Reform	-0.016***	-0.013	-0.016**	-0.008	-0.015*
	(0.006)	(0.008)	(0.007)	(0.009)	(0.008)
Pre-mean	0.813	0.791	0.831	0.792	0.828
% change	-1.944	-1.630	-1.940	-0.992	-1.826
P-value	0.008	0.118	0.030	0.369	0.068
N	478,966	227,709	251,257	198,870	218,870
		Panel B: Number of Children			
Reform	-0.133***	-0.120**	-0.135***	-0.086**	-0.098**
	(0.042)	(0.050)	(0.045)	(0.039)	(0.040)
Pre-mean	2.246	2.048	2.404	2.021	2.348
% change	-5.906	-5.864	-5.613	-4.235	-4.166
P-value	0.002	0.016	0.003	0.029	0.015
N	478,966	227,709	251,257	198,870	218,870

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are the municipality of birth and cohort fixed effects, as well as controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

Nevertheless, the reform could impact the number of children. We, therefore, study the effect of reform on the intensive margin. The results are displayed in Panels (c) and (d) of Figure 7 and

Figure 7: Cohort-Specific Relationships Between Fertility and Reform Exposure



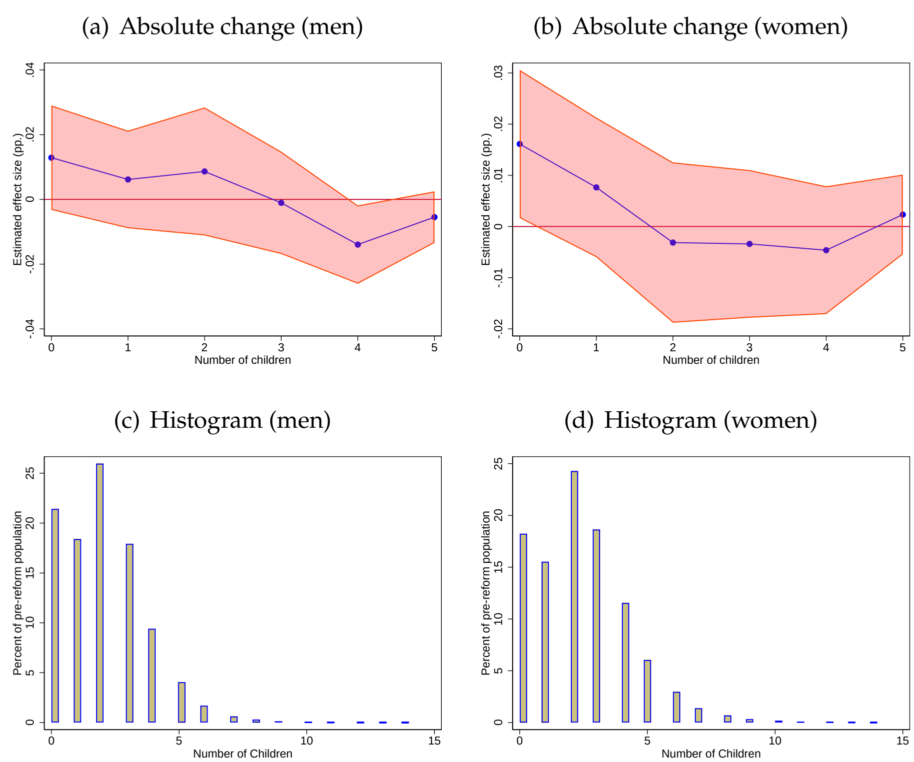
Note: The figures plot the coefficients from a regression of the probability of having at least one child and the total fertility for men and women on the expected increase in weeks of schooling induced by the primary school reform (Equation 3). The left-hand y-axis is the estimated impact on the long-term outcome β_t. The x-axis is the year t that the cohort would begin primary school. We include municipality of birth dummies and year of birth dummies. The solid black line is the potential increases in weeks of schooling from the primary school reform that a cohort is exposed to multiplied by −1 to illustrate how we, in theory, would expect our results to look. The error bars are the 95 percent confidence interval for each coefficient. The standard errors are clustered at the municipality of birth.

indicate a decline in the number of children of about 0.2 for cohorts old enough to be affected by the reform. The estimated effects are similar for men and women, and there is no pre-trend in the estimated cohort-specific relationship between overall fertility and reform exposure. Panel A in Table 4 summarizes these results for the number of children from Equation 4 with and without controls of pre-reform municipality characteristics. Individuals with total reform exposure have between 0.1 and 0.3 fewer children. The effect is similar for men and women and is about a 6–14 percent decrease compared to the pre-reform mean of 2.05 for men and 2.41 for women. When excluding municipalities in the two northernmost counties—a less developed region than the South—-the reduction in total fertility is somewhat smaller but still significant at the 5 percent significance level.

Overall, our results suggest that the number of children men and women have is a more critical margin by which schooling intensity affects fertility. Our estimated effect of a 6 percent decrease in overall fertility relative to the mean fertility rate is smaller than, for example, the effects of the Rosenwald schools described by Aaronson et al. (2014b).

To determine where in the fertility distribution the effect is most considerable, we estimate the impact on the probability of having a specific number of children. We visualize the results in Figure 8 for a more straightforward interpretation. The plots indicate that for men, the decrease in the number of children comes primarily from decreases in the fraction of families with more than three children. For women, we see a shift to more childlessness and one-child families. Note, however, that the effects are not very precisely estimated.

Figure 8: Distributional Effects on Total Fertility



Note: Panels (a) and (b) plot the coefficients from regressions (Equation 4) of dummy variables indicating if the long-run outcome is equal to b on the expected increase in weeks of schooling induced by the primary school reform. The left-hand y-axis is the estimated impact on the long-term outcome β. The x-axis is the value b for which the dummy variable is constructed. Control variables include municipality of birth dummies and year of birth dummies and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930. The red area represents the 95 percent confidence interval for each coefficient. In Panel (c) and (d), we show the pre-reform fractions of individuals with various values of b.

5.3 Effects on Timing and Spacing of Fertility

Lower fertility could result from delayed fertility. We, therefore, study the impact of the reform on the age at first birth and the spacing of the two first children in Table 5. The estimated impact on the age at first birth is positive. Reform exposure during all seven years of mandatory education increases the age at first birth by about four months. Birth spacing between different births is not significantly affected.

Table 5: Long-Term Effect of School Reform Exposure on Timing and Spacing of Children

	Whole sample	Men	Women
Panel A: Age at first birth			
Reform	0.290**	0.271*	0.250**
	(0.121)	(0.160)	(0.121)
Pre-mean	28.921	30.423	27.751
% change	1.003	0.889	0.900
P-value	0.017	0.092	0.040
N	403,935	187,596	216,339
Panel B: Panel B: Spacing between 1st and 2nd child			
Reform	-0.139	-0.313	0.198
	(0.766)	(0.897)	(0.886)
Pre-mean	51.184	49.411	52.656
% change	-0.273	-0.633	0.375
P-value	0.856	0.727	0.824
N	335,837	159,201	176,636

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are the municipality of birth and cohort fixed effects, as well as controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

Although the effect on the age at first birth is only significant on the 10% significance level for men, the effect sizes are similar across genders. Relative to the mean age at first birth of 28 years for women and 30 years for men this is an increase of about 1 percent. Importantly, individuals have, on average, their first child long after finishing primary school, and although we find an increase in the age at birth, the size of the change is relatively small. Hence, these results suggest that delayed fertility is not the primary explanation for the reduced fertility.

6 Mechanisms

We have established that the primary school reform affected fertility, mainly the number of children and, to some degree, the timing of the first birth. Importantly, we find a similar effect for men and women. While we have established that there is a contemporaneous effect on more school weeks being offered in the most intensely treated areas (see Section 5.1), a reform-induced increase in years of education may be an essential channel for our fertility results. In his seminal work, Becker (1960) discussed that increasing women's labor market opportunities, for example, through higher education, will reduce childbearing. Hence, more education may increase the opportunity cost of work for women (and men) and lead to a reduction in total fertility and a change in fertility timing. Education may also lead, among others, to changes in preferences and information, knowledge about contraception, and changing discount rates, which may affect fertility decisions. Moreover, education may affect the marriage market outcome, both entry into marriage, the timing of marriage, the quality of marriage as measured by divorce rates, and assortative mating.

We first document the reform's effect on investment in human capital, participation in the labor market, and earnings. Second, we present results on adjustment in the marriage market.

6.1 Years of Education

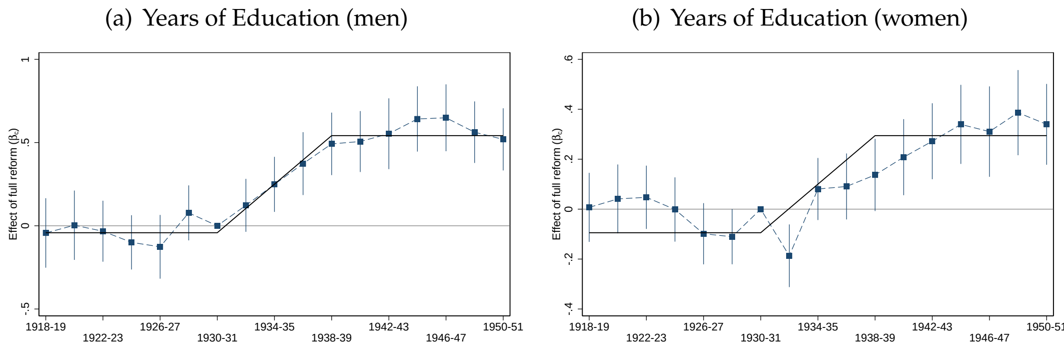
First, we analyze whether the reform has a reduced form impact on completed years of education. We investigate whether the more intense and better-quality teaching in primary school increased individuals' schooling beyond the mandatory educational level.

The event-study estimates in Figure 9 show no pre-trends for years of education and that the impact on years of education follows a linear trajectory before leveling out once the reform is implemented. The impact is stronger for men than women. This difference is unsurprising as it was more common for men to invest in human capital above the mandatory seven years of education in the 1930s and 1940s. Panel A of Table 6 presents the results from estimating Equation 4 for years of education. We find a significant increase in years of schooling for both men and women. The estimated impact of the reform is 0.33 years for men and 0.29 years for women. This is equivalent to a 3.7 and 3.5 percentage increase in years of education from pre-treatment means of 9 years for men and 8.3 years for women. Note that this is not a mechanical effect of the reform, as required years in primary school remain constant throughout the reform. The estimated effects are similar and still significant when excluding the northernmost counties.[13]

To analyze where in the educational distribution attainment is increased, we estimate Equation 4 on indicator variables equal to 1 if years of schooling is equal to b years and zero otherwise where b ranges from 7 to 21 years of education. Figure 10 presents the estimated coefficients for men and women. We find that the estimated increase in years of education is driven by a decrease in the fraction of individuals with seven years of education and an increase in the fraction with 10–12 years of education. This means we see an increase in completed middle school and even some impact on high school education. There are no apparent differences between the genders.

[13]Note that these results stand in contrast to Fischer et al. (2013), which do not find an increase in the years of schooling when they analyze a similar Swedish reform around the same time.

Figure 9: Cohort-Specific Relationships Between Years of Education and Reform Exposure



Note: The figures plot the coefficients from a regression of education for men and women on the expected increase in weeks of schooling induced by the primary school reform (Equation 3). The left-hand y-axis is the estimated impact on the long-term outcome β_t. The x-axis is the year t that the cohort would begin primary school. The control variables that are included are municipality of birth dummies and year of birth dummies. The solid black line is the potential increase in weeks of schooling from the primary school reform that a cohort is exposed to. The error bars are the 95 percent confidence interval for each coefficient. The standard errors are clustered at the municipality of birth.

Overall, the reform impacted the years of education, and the increase in human capital might, therefore, be a mechanism through which the reform affected fertility outcomes.

6.2 Labor Market Outcomes

The 1936 primary school reform positively affected schooling and likely improved labor market opportunities. To investigate this potential mechanism, we present estimates of the impact of the primary school reform on labor market outcomes later in life. Indicators of labor market outcomes are constructed from the tax registers, where we have data on individual income for our sample between ages 50 and 64. Panels B and C of Table 6 show estimates from Equation 4 on log income and mean labor force participation. We find positive effects on labor market outcomes for women. Income increases by 9.4 percent. The labor force participation, however, increased by 4 percent relative to the pre-reform mean on a 10 percent significant level. Men's labor market outcomes are positively affected by the reform. The estimated effects are, however, smaller for men than for women on absolute and relative levels. Hence, the increase in labor market opportunities for women is another mechanism through which the reform could reduce childbearing.

6.3 Marriage Market

Marriage market outcomes are a further potential mechanism for our fertility results. The reform could affect the probability of marriage and the type of spouse an individual chooses. Table 7 presents the results. Our estimates imply a slight reduction in the number of married women.

Table 6: Long-Term Effect on Years of Education and Labor Market Outcomes

	Whole sample	Men	Women
	Panel A: Years of Education		
Reform	0.321***	0.334***	0.290***
	(0.057)	(0.073)	(0.058)
Pre-mean	8.639	9.020	8.333
% change	3.714	3.700	3.482
P-value	0.000	0.000	0.000
N	478,427	227,464	250,963
	Panel B: Panel B: Log Income		
Reform		0.040**	0.094***
		(0.016)	(0.021)
Pre-mean		11.940	11.012
% change		0.339	0.854
P-value		0.012	0.000
N		215,720	196,902
	Panel C: Labor Market Participation		
Reform		0.013**	0.019*
		(0.006)	(0.011)
Pre-mean		0.868	0.461
% change		1.453	4.206
P-value		0.039	0.066
N		227,550	250,135

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

Women decrease their probability of marrying by 1.4 percent from a pre-reform level of 91.8 percent. While the effect is significant on the 5% significance level, it is relatively small and unlikely to be the only driver of our results.

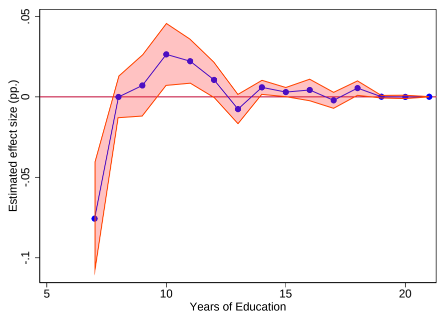
On the other hand, we show that the number of years of education of the partners the affected individuals chose increased significantly for both men and women. Overall, partners' years of schooling increased by 2.4–2.6 percent relative to the pre-reform mean. On the other hand, we do not find that individuals' partners are from better-selected families that we proxy by the partner's fathers' education. For men, the effect on the socioeconomic background of their partner is even negative; the effect is insignificant for women.[14] Moreover, men's partners are more likely to be born in urban areas where schooling levels are higher, irrespective of the reform.[15] Hence, this

[14]Note that we only observe parental education for a subset of individuals because family linkages for these early generations are incomplete and parents need to be observed in the 1960 census.

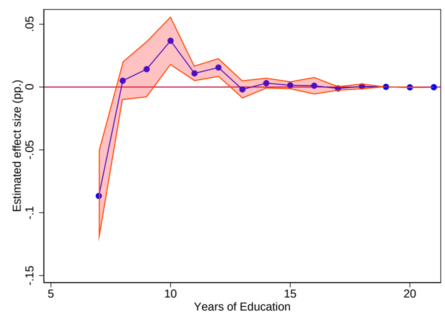
[15]We code individuals who do not get married with zero because of selection into marriage.

Figure 10: Distributional Effects on Years of Education

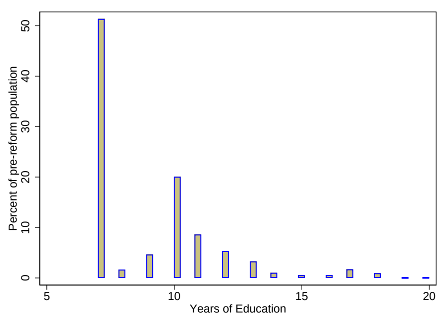
(a) Absolute change (men)



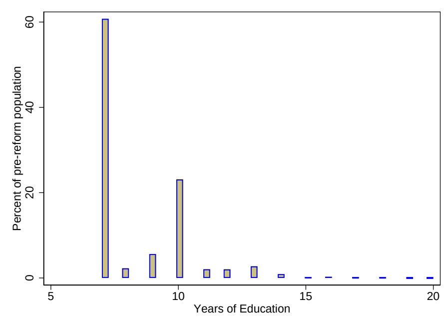
(b) Absolute change (women)



(c) Relative change (men)



(d) Histogram (women)



Note: Panels (a) and (b) plot the coefficients from regressions (Equation 4) of dummy variables indicating if the long-run outcome is equal to b on the expected increase in weeks of schooling induced by the primary school reform. The left-hand y-axis is the estimated impact on the long-term outcome β. The x-axis is the value b for which the dummy variable is constructed. Control variables include municipality of birth dummies and year of birth dummies and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930. The red area represents the 95 percent confidence interval for each coefficient. The standard errors are clustered at the municipality of birth. In Panels (c) and (d), we show the pre-reform fractions of individuals with various values of b.

increase in the partner's education level likely comes from reform-affected individuals marrying other reform-affected, higher-educated individuals.

These results suggest that the reform increased the education of the directly affected individuals and their partners. Hence, the total increase in household-level education is substantial, indicating that the reform likely affected the fertility decision to a large degree through its effect on the human capital of both couple members.

Table 7: Long-Term Effect of School Reform Exposure on Marriage and Assortative Mating

	Ever married	Partners' education	Partners' father's education	Partner born in city
		Panel A: Men		
Reform	-0.011	0.224***	-0.214**	0.044***
	(0.007)	(0.055)	(0.092)	(0.008)
Pre-mean	0.885	8.554	7.708	0.090
% change	-1.191	2.620	-2.782	-18.569
P-value	0.114	0.000	0.021	0.034
N	227,709	197,298	48,049	227,709
		Panel B: Women		
Reform	-0.013**	0.216***	0.128	-0.007
	(0.006)	(0.068)	(0.090)	(0.006)
Pre-mean	0.918	8.803	7.639	0.069
% change	-1.394	2.450	1.673	-9.462
P-value	0.029	0.002	0.158	0.280
N	251,257	222,270	42,295	251,257

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

7 Intergenerational Effects

When forming preferences about the ideal family size, children's family experience may be important (Fernández and Fogli, 2006). Hence, the reform might not only affect directly affected individuals but also the fertility of their offspring. Importantly, the offspring generation did not face the same geographic differences in school days as their parents. The school system became even more unified across regions and municipalities before the offspring generation attended school. This allows us to study the generational persistence of the school reform.

In this section, we analyze the multi-generational effect of the reform. Table 8 presents the results from estimating Equation 6 on fertility for the second generation. Note that the regressions for the offspring generation are weighted so that each parent gets equal weight in the affected generation. Hence, our results for the offspring generation are therefore not biased by differences in fertility across different groups.

Although only significant on a 10% level, the reform decreases the number of children of the second generation by 0.03—about 1 percent relative to the pre-reform mean. Compared to the effect on the first generation's number of children, this is an intergenerational persistence of the reform effect of about 22%. Interestingly, the intergenerational fertility effect is driven by children of

Table 8: Intergenerational Effects on Fertility and Education

	Whole sample	Exposed fathers	Exposed mothers
	Panel A: Number of children		
Reform	-0.029**	-0.042**	-0.017
	(0.014)	(0.019)	(0.017)
Pre-mean	2.058	2.047	2.066
% change	-1.431	-2.054	-0.826
P-value	0.039	0.029	0.309
N	1,191,630	554,248	637,380
	Panel B: Panel B: Years of education		
Reform	0.028	0.035	0.024
	(0.042)	(0.046)	(0.049)
Pre-mean	11.787	11.969	11.636
% change	0.235	0.290	0.206
P-value	0.510	0.454	0.625
N	1,187,270	551,947	635,321

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 6. Control variables included are the municipality of birth, cohort, and cohort of parent fixed effects, and controls for heterogeneity across municipalities such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

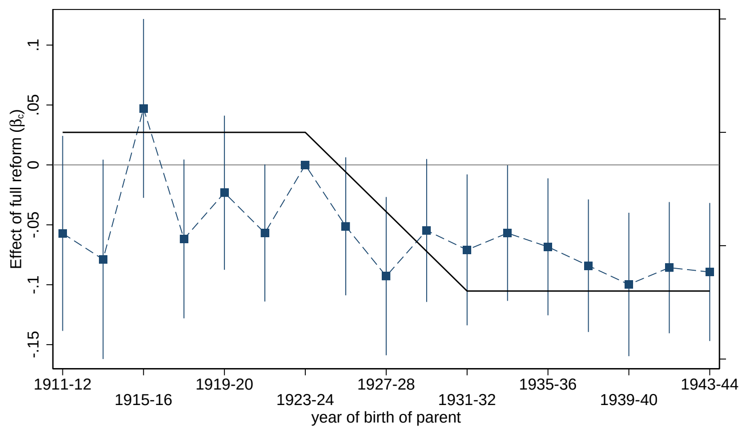
affected men. That is, individuals with an exposed father have a 2 percent lower fertility (significant on a 5 percent significance level). Children of affected mothers do not have a significantly different number of children. Note that this does not necessarily mean that only fathers are important in carrying over the fertility preferences to the next generation. As shown in Section 6.3, exposed men are more likely to partner with better-educated women and women from urban areas. Hence, the intergenerational persistence of the effect might be, at last, partly determined by the father's choice of a spouse.

Figure 11 presents event-study results on the fertility of the second generation by estimating Equation 5 separately for the offspring of affected men and women and shows that there are no significant pre-trends. Taken together, we find some evidence for an intergenerational effect of the reform on fertility, mostly on the father's side. The effect size is, however, substantially smaller than for the directly affected generation.

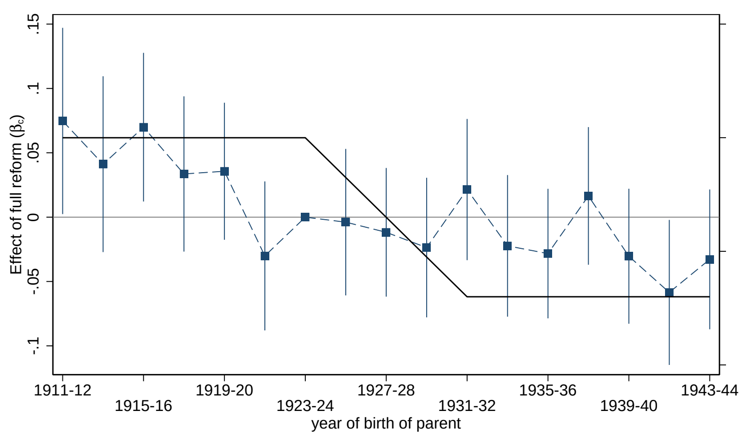
Panel B of Table 8 shows that there are no significant effects on the second generation's years of education. This is in line with Black et al. (2005), who find little or no evidence for intergenerational effects of parents' education on children's education using a school reform in Norway in the 1950s and 1960s. Hence, for the offspring generation, increases in education cannot be the main mechanism for the smaller family sizes.

Figure 11: Cohort-Specific Relationships Between Fertility and Reform Exposure of Parents

(a) Father's side



(b) Mother's side



Note: The figures plot the coefficients from a regression of years of education on the expected increase in weeks of schooling of their parents as induced by the primary school reform (Equation 5). The left-hand y-axis is the estimated impact on the long-term outcome β_t. The x-axis is the year t that the cohort would begin primary school. Control variables include municipality of birth, year of birth and parents' year of birth fixed effects. The solid black line is the potential increase in weeks of schooling from the primary school reform that a cohort is exposed to. The error bars are the 95 percent confidence interval for each coefficient. The standard errors are clustered at the municipality of birth.

8 Robustness and Sensitivity Analysis

We perform a wide range of sensitivity tests. First, we add controls for county- and municipality-specific pre-trends to alleviate concerns of differential trends by treatment exposure. Second, we restrict our sample by dropping municipalities with exceptionally high or low treatment exposure. Third, we control for mean reversion using different income data and specifications. Fourth, we

Table 9: Sensitivity Analysis: Pre-Trends

	No. of Children		Prob(no. children > 0)	
	Men	Women	Men	Women
	Panel A: Linear municipality-specific time trends			
Reform	-0.124***	-0.138***	-0.013	-0.016**
	(0.048)	(0.045)	(0.008)	(0.007)
Pre-mean	2.048	2.404	0.791	0.831
% change	-6.075	-5.754	-1.685	-1.908
P-value	0.010	0.002	0.106	0.034
N	227,692	251,257	227,692	251,257
	Pane B: Linear county-specific time trends			
Reform	-0.110**	-0.132***	-0.012	-0.018**
	(0.047)	(0.044)	(0.008)	(0.007)
Pre-mean	2.048	2.404	0.791	0.831
% change	-5.370	-5.473	-1.528	-2.115
P-value	0.021	0.003	0.145	0.017
N	227,709	251,257	227,709	251,257

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are the municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

investigate the sensitivity of our results to varying definitions of fertility measures. Fifth, we do randomization inference by randomly reallocating the treatment exposure of municipalities. Finally, we loop over many possible combinations of these specifications in a specification chart. We focus on the primary outcome variables: fertility along the extensive and intensive margin. Our main results are robust to all these sensitivity tests.

We begin by investigating the sensitivity of our results to various specifications. To alleviate the concerns that our results are driven by trends correlated with our measure of treatment intensity, Table 9 presents estimates where we control for municipality-specific (Panel A) and county-specific pre-trends (Panel B), respectively. Neither municipality-specific nor county-specific pre-trends impact our results meaningfully, and all results hold.

Second, we might be concerned that municipalities with a treatment intensity of zero are not a good control group. These areas provide weeks of schooling before the reform equal to or even more than the new required weeks. In total, these are 13 percent of municipalities and 17 percent of individuals. To investigate the sensitivity of our results to these concerns, we re-run the analysis on our primary outcome variables without municipalities with a zero or close to zero treatment intensity and present results in Panel A in Table 10. The results do not change meaningfully, indicating that these municipalities are not driving our results. Similarly, we might be worried

Table 10: Sensitivity Analysis: Excluding Non-Treated and Non-Complying Municipalities

	No. of Children		Prob(no. child>0)	
	Men	Women	Men	Women
	Panel A: Without non-treated			
Reform	-0.116**	-0.144***	-0.017*	-0.020**
	(0.056)	(0.049)	(0.009)	(0.008)
Pre-mean	2.076	2.438	0.791	0.832
% change	-5.600	-5.907	-2.161	-2.353
P-value	0.038	0.003	0.056	0.011
N	204,188	225,070	204,188	225,070
	Panel A: Without earlier non-compliers			
Reform	-0.129***	-0.139***	-0.014	-0.019**
	(0.042)	(0.044)	(0.009)	(0.008)
Pre-mean	2.038	2.392	0.792	0.830
% change	-6.315	-5.800	-1.720	-2.306
P-value	0.002	0.002	0.150	0.013
N	216,362	238,561	216,362	238,561

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Non-treated is defined as a municipality having to increase schooling across seven years by less than two weeks, while non-compliant is defined as having to increase by more than 30 weeks. Control variables included are the municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930.

that a few municipalities with too few weeks of schooling before the reform might be driving our results. We drop these municipalities in Panel B in Table 10 and find that the coefficients for the number of children are still statistically significant from zero.

Third, we might be worried that controlling for the 1930 income level does not sufficiently capture the potential mean reversion. We present estimates for various income measures to investigate the basis for this concern. Table 11 presents estimates replacing the 1930 income level with the municipality-level income from 1915 and 1920 and the percentage change from 1915 to 1920. The coefficients for the number of children remain similar and statistically significant; some of the coefficients for the probability of having at least one child are more precisely estimated but not much more prominent and still constitute a relatively small increase in fertility on the extensive margin.

Fourth, we combine data on fertility outcomes from various sources. We test whether our results depend on the definition of the fertility variables. We use three alternative fertility measures for women in Table 12: First, we use data from the population censuses on the number of kids

Table 11: Sensitivity Analysis: Controlling for Mean Reversion

	Men				Women			
	Income 1915	Income 1920	Income 1930	% change 1915-1920	Income 1915	Income 1920	Income 1930	% change 1915-1920
	Panel A: Number of children							
Reform	-0.121**	-0.123**	-0.120**	-0.152***	-0.136***	-0.131***	-0.135***	-0.125***
	(0.049)	(0.049)	(0.050)	(0.042)	(0.044)	(0.044)	(0.045)	(0.039)
Pre-mean	2.048	2.048	2.048	2.048	2.404	2.404	2.404	2.404
% change	-5.928	-6.013	-5.864	-7.406	-5.656	-5.434	-5.613	-5.208
P-value	0.013	0.013	0.016	0.000	0.002	0.003	0.003	0.001
N	222,463	223,114	227,709	221,766	245,514	246,350	251,257	244,721
	Panel B: Probability of having children							
Reform	-0.016**	-0.016*	-0.013	-0.015**	-0.016**	-0.016**	-0.016**	-0.011
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Pre-mean	0.791	0.791	0.791	0.791	0.831	0.831	0.831	0.831
% change	-2.058	-1.991	-1.630	-1.943	-1.871	-1.972	-1.940	-1.307
P-value	0.045	0.057	0.118	0.049	0.038	0.026	0.030	0.126
N	222,463	223,114	227,709	221,766	245,514	246,350	251,257	244,721

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are the municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. The way of controlling for mean reversion differs across specifications, as indicated in the table.

in current marriages.[16] Second, we supplement the census data with the register data for all years after the census was completed. Third, we use only the register data to count the number of children registered with an individual as a parent. Our results are not sensitive to how we construct our fertility measures.

Next, we relax the assumption that the t-statistic is normally distributed and present results from randomization inference, which does not assume any such specific distribution. We randomly reshuffle the treatment intensity of municipalities and re-estimate our preferred regression model on the number of children and the probability of having children. This procedure is iterated over 1,000 times, and the results for all two primary outcome variables are presented in Figure 12, together with our preferred estimates (red line). We find that our main findings are robust to this test.

Finally, Figure 13, 14, and 15 are specifications charts showing estimates from a wide range of alternative specifications. We do this to alleviate concerns about the sensitivity of our results on fertility to particular combinations of controls and sample restrictions. Each coefficient is from a separate regression, and the coefficient in red is our preferred specification. The control variables

[16]Note that we only have this variable for women.

Table 12: Sensitivity Analysis: Fertility Measures (Only Women)

	1960 census	1970 census	1960 census and registry	1970 census and registry	Population register
Reform	-0.129**	-0.109**	-0.135***	-0.139***	-0.120***
	(0.056)	(0.049)	(0.045)	(0.042)	(0.044)
Pre-mean	2.145	2.109	2.404	2.333	1.993
% change	-6.005	-5.174	-5.613	-5.975	-6.003
P-value	0.021	0.026	0.003	0.001	0.007
N	251,257	251,257	251,257	251,257	251,257

Note: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors adjusted for clustering at the municipality of birth level in parentheses. Each coefficient is the result of estimating Equation 4. Control variables included are the municipality of birth and cohort fixed effects and controls for heterogeneity across municipalities, such as labor market share of agriculture and fishery. We control for mean reversion by controlling for the municipality-level income level in 1930. Men are excluded in this table as the 1960 and 1970 population censuses did not collect information on the number of children for men.

added, gender restriction of the sample, and other sample restrictions are indicated by the black dots below the figure. Reassuringly, we find that all specifications for women are still negative and statistically significant from zero at the 5 percent significance level. Most of our specifications for men are also statistically different from zero at the 5 percent level, but few are not. Importantly, our preferred specification produces a less extreme coefficient than most alternative specifications.

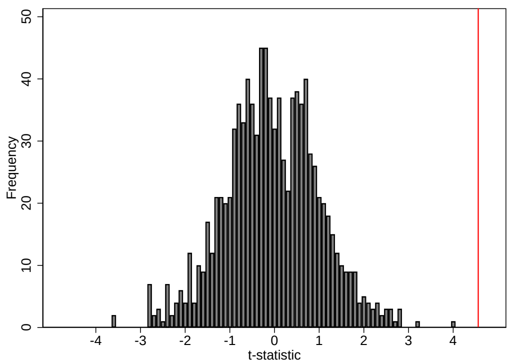
9 Conclusion

Understanding how primary school quality affects fertility and the potential mechanism mediating the impact is essential for policymakers and social scientists in developing countries and more developed economies. The central questions are whether there is a causal mechanism going from educational quality to decisions on fertility, whether the intensive or extensive margins of fertility and the fertility timing are affected, and whether the changes persist across multiple generations. Using research designs to avoid bias due to self-sorting into education, several papers have reported negative effects of education on fertility, especially for teenagers, on the timing of fertility, and to some extent on total fertility and family formation (Breierova and Duflo, 2004; Aaronson et al., 2014a; Geruso and Royer, 2018; Fort et al., 2016; Black et al., 2008; Monstad et al., 2008). Most of the papers use compulsory schooling laws affecting the length of education.

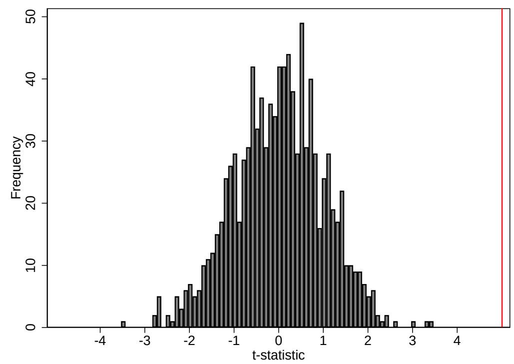
In this paper, we focus on a primary school reform in 1936, changing the length of the school year and the educational quality, keeping the compulsory years of schooling constant. Hence, the reform changed the academic content for individuals before age 14, when teenage pregnancies were relatively infrequent. Thus, we can abstract from the more mechanical or direct effect of being in school for more years, which is found to reduce teenage fertility (Black et al., 2008). Instead,

Figure 12: Imputation Test on Main Outcomes

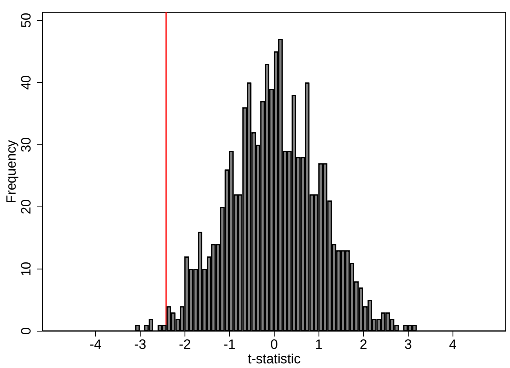
(a) Education (men)



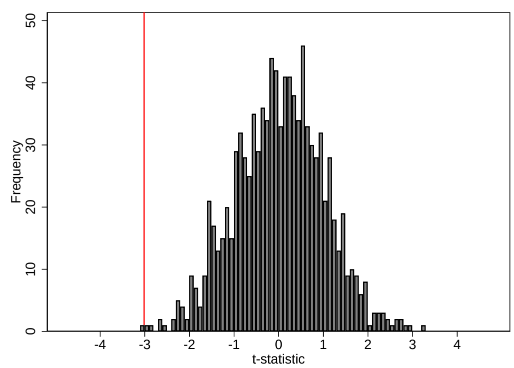
(b) Education (men)



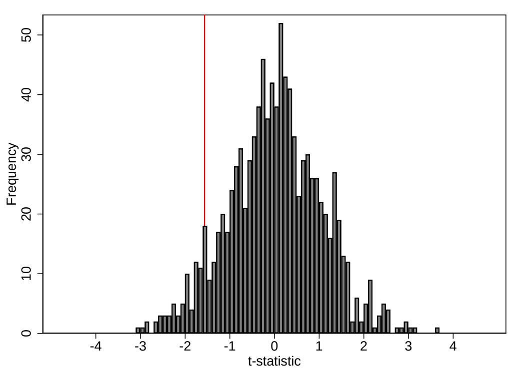
(c) Total fertility (men)



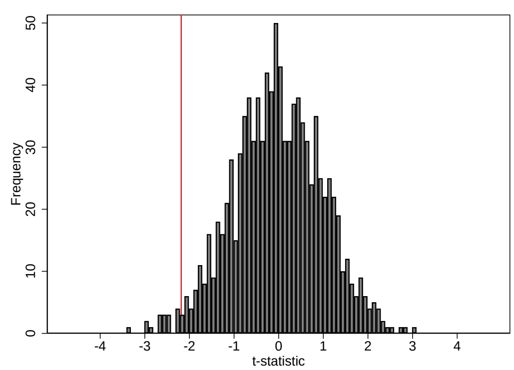
(d) Total fertility (women)



(e) Pr(total fertility>0) (men)

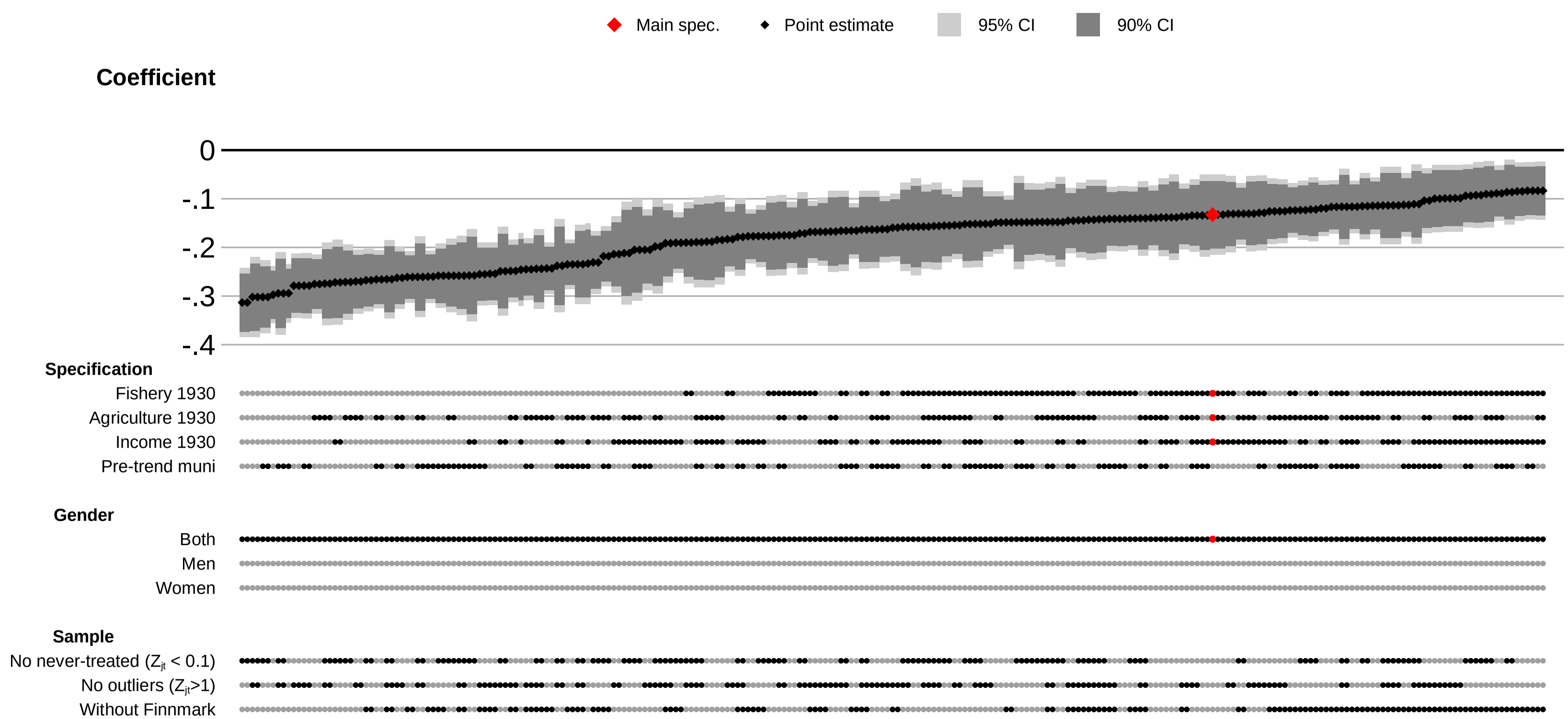


(f) Pr(total fertility>0) (women)



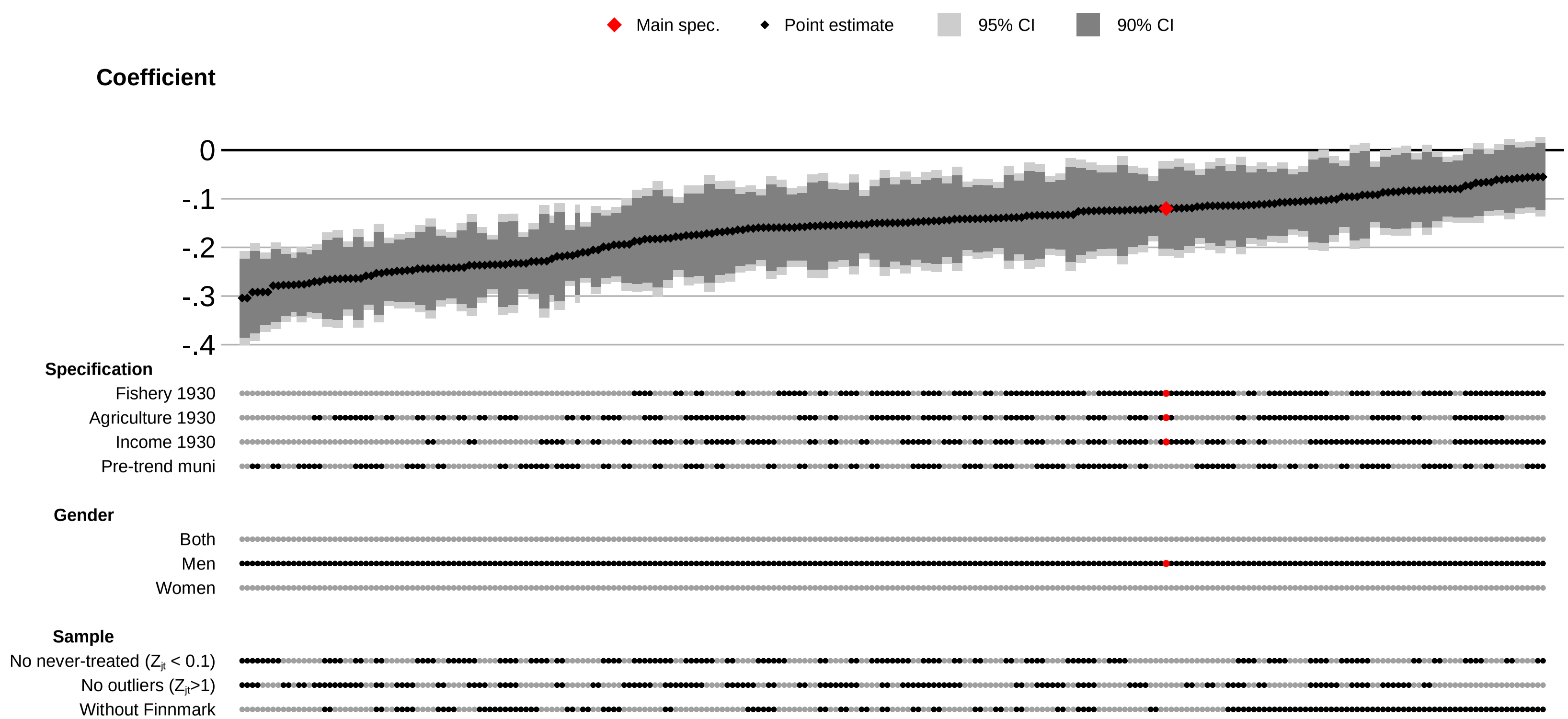
Note: The figures plot the t-statistics from the permutation test with 1,000 repetitions. The treatment intensity of each municipality is reshuffled randomly between each iteration. The regression controls for birth cohort and municipality of birth fixed effects, income level in 1930 and labor shares in agriculture and fishing in 1930. The red lines are the t-statistics when using the actual reform intensity for each municipality.

Figure 13: Specification Chart for Men and Women



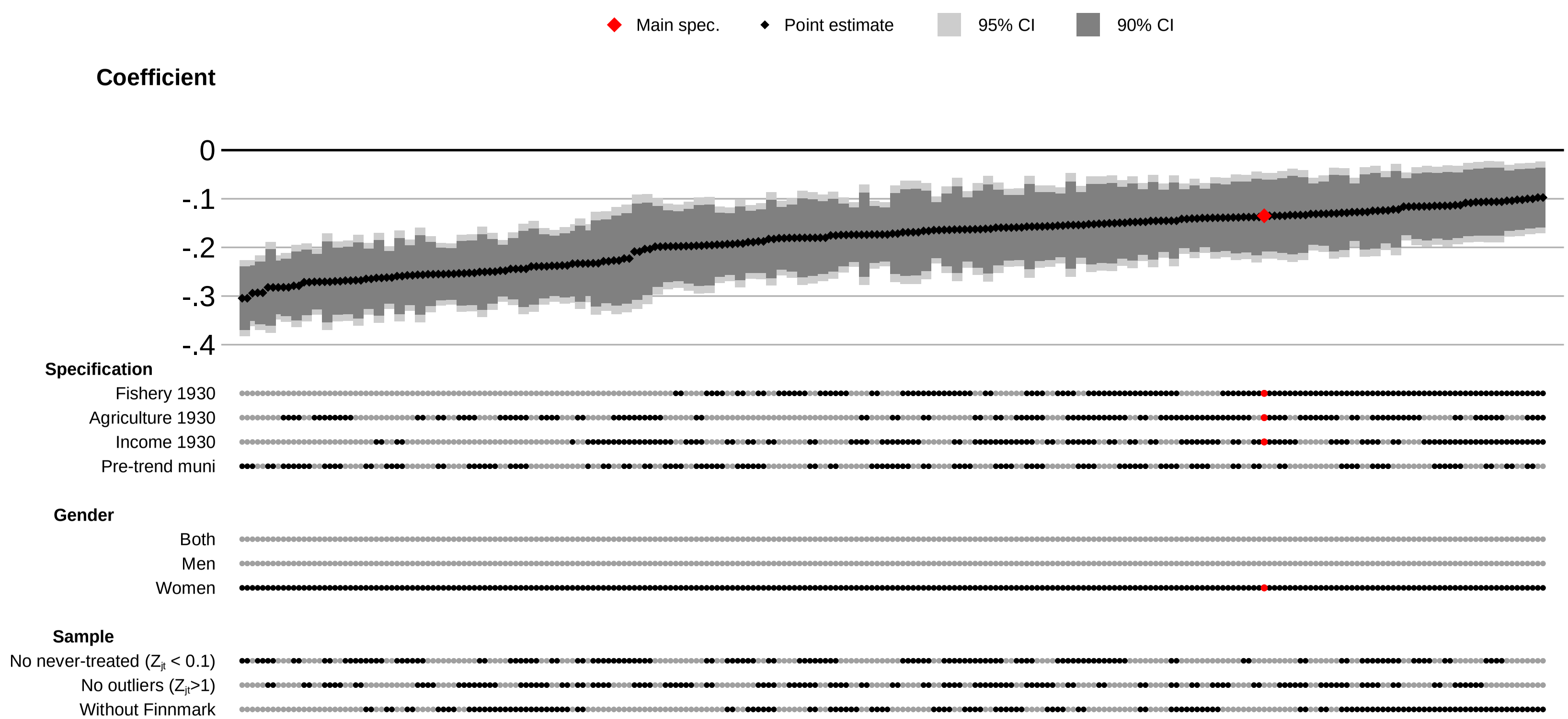
Note: The specification chart plots the estimated causal effect of the reform on total fertility for 757 different regression specifications. Standard errors are clustered at the municipality of birth. As proposed by Simonsohn et al. (2015). Dots indicate the specification used, the gender of the sample and other sample restrictions.

Figure 14: Specification Chart for Men



Note: The specification chart plots the estimated causal effect of the reform on total fertility for 757 different regression specifications. Standard errors are clustered at the municipality of birth. As proposed by Simonsohn et al. (2015). Dots indicate the specification used, the gender of the sample and other sample restrictions.

Figure 15: Specification Chart for Women



Note: The specification chart plots the estimated causal effect of the reform on total fertility for 757 different regression specifications. Standard errors are clustered at the municipality of birth. As proposed by Simonsohn et al. (2015). Dots indicate the specification used, the gender of the sample and other sample restrictions.

we aim to estimate the impact driven by human capital investment and preference formation. In addition, our data allow us to investigate whether education affects fertility through marriage market outcomes. Moreover, the education reform took place almost 90 years ago. It has the advantage that we can look at long-term consequences, such as completed fertility for the directly affected cohorts and their children, and not only investigate fertility at young ages.

Exploiting an intensity-of-treatment design (see, e.g., Duflo, 2001; Acemoglu et al., 2024), we emphasize four main findings. First, the reform reduced the number of children per woman. This effect is robust to a wide range of robustness checks and amounts to about 0.10-0.14 children per woman. Second, we find that treated individuals choose to delay fertility. Third, we document that more years of education, higher earnings, and partnering with better-educated spouses are potential channels through which the reform affects fertility. Fourth, we show that the fertility decrease persists for the children of the directly affected individuals, particularly if the father was exposed to the reform.

Compared to the results in the literature, our findings are quite large and most closely match the results from the impact of school construction programs on less developed settings and settings with little access to modern family planning methods. Breierova and Duflo (2004), for example, estimates that a school construction program in Indonesia reduced fertility by 0.1 children per woman, but the result is not statistically significant. Compared to our estimates, Aaronson et al. (2014a) find slightly larger but less precisely estimated effects of exposure to the Rosenwald schools in the US compared to our findings. On the other hand, papers looking at the impact of changes in the number of compulsory schooling years typically find tiny or no effect on total fertility, but a change in the timing of fertility (see, e.g., Monstad et al., 2008; Geruso and Royer, 2018). Hence, primary school reforms that change the educational intensity and quality in a setting with limited access to modern contraception seem to reduce fertility sustainably—in our case, even for multiple generations.

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