Life-Cycle Effects of Comprehensive Sex Education

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Abstract

Sex education can impact pupils' sexual activity and convey the social norms regarding family formation and responsibility, which can have significant consequences to their future. To investigate the life-cycle effects of social norm transmission, this study draws on the introduction of comprehensive sex education in the curriculum of Swedish primary schools during the 1940s to the 1950s. Inspired by social-democratic values, sex education during this period taught students about abstinence, rational family planning choices, and the importance of taking social responsibility for their personal decisions. The study applies a state-of-the-art estimator of the difference-in-differences method to various outcomes of men and women throughout the life cycle. The results show that the reform affected most intended outcomes for men and women, ultimately decreasing gender inequality in earnings. The effects of the reform also extended to the succeeding generation of girls. Both generations created a critical mass that altered social norms in favor of collective engagement and democracy. The findings suggest that social norms, internalized through school-based sex education, persistently affect people's outcomes in significant ways.

JEL codes: I25, J13, Z13, N34, P46.

Key words: social norms; sex education; natural experiment; gender inequality; economic wellbeing; prosociality.

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Acknowledgements:

1 Introduction

Individuals internalize social norms as early as in childhood and subsequently make life choices about schooling and family formation, which have consequences for economic wellbeing and inequality (C. D. Goldin 2021; Kranton 2016; Akerlof and Kranton 2010). In many countries, sex education has long been integrated in the compulsory school curriculum (UNESCO 2021), providing a context for natural experiments. The content and scope of sex education vary, ranging from abstinence-only approaches (as seen in most US states) to comprehensive programs that include ethical components addressing healthy relationships, life skills, and social norms (such as in Sweden). Recent experimental studies have shown that schools have the ability to influence pupils' social and gender attitudes (Dhar, Jain, and Jayachandran 2022; Cappelen et al. 2019), particularly through sexuality education (DeGue et al. 2021; N. A. Constantine et al. 2015; Louise A. Rohrbach et al. 2015). However, the limited duration of these experiments prevents an assessment of the effects of social norms throughout the life cycle.

In this study, we investigate how the transmission of social norms—by means of a sex education reform in primary school—could affect the life choices and economic well-being of men and women. We study the introduction of sex education into the primary school curriculum in 1942–1958 in Sweden, a forerunner of such intervention globally, and follow the affected cohorts and their children across the life cycle. While many developed countries implemented sex education programs in response to the HIV epidemics, Sweden did so to address the population crisis of the 1930s. The Swedish sex education reform promoted abstinence and rational choice in sexual and family matters but was not limited to it. It also served as a pioneering social intervention during the building of the social democratic welfare state: The Swedish state implemented sex education to educate adolescents to become individually and collectively responsible citizens. The reform also targeted gender inequality in self-determination by supporting teenage girls to make their own decisions on sexual behavior.

We find that the reform yielded significant effects for most intended outcomes for both sexes, and some of these effects extended to their children. First, for the first generation, the reform led to a significant reduction in sexual activity and unwanted pregnancies, as evidenced by a decreased likelihood of abortion and cervical cancer (for women), and a decline in marriages of convenience related to out-of-wedlock births (for both men and women). The reform delayed marriage and childbearing, particularly beyond the age of 25 years. Second, the reform significantly increased individuals' earnings by 9.3% to 10.9%, suggesting high returns on non-cognitive skills fostered by the reform, especially responsibility. Third, we find that the reform has created the new identity of an active and socially responsible individual, with women entering the labor force and both men and women choosing educational fields and professions that involved public responsibilities. The reform effects extended even to the next generation of girls, who pursued education with a prosocial focus while also engaging in entrepreneurial activities. Ultimately, the sex education reform has fostered a transformation in collective civic engagement, thereby supporting democracy.

To examine the causal effects of sex education for individual outcomes throughout the life cycle, we obtained various unique and exceptionally rich data and applied a state-of-the-art estimator of the difference-in-differences (DID) approach with differential treatment timing (Callaway and Sant'Anna 2021). Between 1942 and 1958, national and regional authorities facilitated the introduction of sex instruction to the school curriculum across Sweden by organizing local courses for primary school teachers. Such gradual implementation of the courses provides us with the municipality entry to sex education treatment, which we used in the identification of the causal effects: Dependent on where pupils lived at the time of the reform when they were in primary-school ages (cohorts born 1930–1946), they either received sex education or had never done so. We used rich register data on the full population of cohorts under study and their children in a variety of outcomes from 1950 until 2017.

Many studies have been conducted on the effects of sex education, but they have primarily discussed teenage health and pregnancy outcomes. According to a recent review of randomized

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control trials, interventions that simultaneously provide preventive education and contraceptives significantly lower the risk of unintended pregnancies (Oringanje et al. 2016). However, observational studies that applied causal strategies have provided mixed evidence (Kearney and Levine 2012; Sabia 2006). Programs that propagate abstinence yield no desired effects (Paton, Bullivant, and Soto 2020; Carr and Packham 2017). All these studies, however, have assessed the effects of sex education in modern contexts, where pupils' preferences in sex activity have been shaped by various informational channels (Grossman et al. 2014; Reis et al. 2011). Given diversity in contexts, disagreement on the results is not surprising. We expected to contribute to the literature by considering a historical sex education reform in a context in which effective contraception or alternative information channels, like media, were absent and by looking beyond adolescents' outcomes.

Our study makes several contributions to the broader economic literature. First, we explored a school-based sex education reform intended to build and transmit new social norms of family planning and individual and social responsibility among both men and women. The growing body of research on the role of social norms and identities (Guiso, Sapienza, and Zingales 2011; Akerlof and Kranton 2010), and gender identities in particular (Bertrand et al. 2021; C. Goldin 2014), emphasizes how these norms lead to specific educational and occupational choices. Previous quasi-experimental studies have found large impacts of different shocks to norms on the economic outcomes of women (e.g., the introduction of contraceptive pills, in Bailey and Lindo 2018; Guiliano 2018); meanwhile, the effects for men (primarily from studying recent paternity leave reforms) have been inconclusive (Patnaik 2019; Ekberg, Eriksson, and Friebel 2013).

In addition, since our study focused on sex education that aimed to affect individual and social responsibility, our findings add to the related literature on the economic returns to investment in children's non-cognitive skills (Heckman and Mosso 2014). Literature based on randomized educational experiments has demonstrated the possibility of altering pupils' preferences that are linked to future economic success and differ between genders, such as risk attitudes or

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competitiveness (Dhar, Jain, and Jayachandran 2022; Kautz, Jagelka, and Heckman 2019; Shurchkov and Eckel 2018). Several observational studies have assessed how school curriculum affects pupils' political attitudes (Cantoni et al. 2017; Friedman et al. 2016; Clots-Figueras and Masella 2013). Finally, our study adds to the literature on the intergenerational transmission of traits and resources (Falk et al. 2021; Bisin and Verdier 2011), particularly research on the causal effects of such transmission (Black et al. 2020; Dahl, Kostøl, and Mogstad 2014).

Our findings provide direct policy implications, despite the obvious difference of the historical Swedish context studied from today. We draw attention to the fact that the policy effects of sex education are long-lasting because they embed in today's population through different channels, including health behavior, family formation, labor force and political participation, and through their children's lives. To the ongoing debates on sex education in both Europe and the US (UNESCO 2018; Papa and Armfield 2018), we contribute evidence of the long-term potential of school-based sex education that includes both preventive and normative components. In the 1940s, the Swedish state viewed sex education that promoted preferences for autonomy and self-confidence as a tool to protect democracy from the popularity of far-right parties and promote social-democratic views. Today's democratic world faces a similar challenge, one that we propose addressing through the education of citizens in matters of equity, inclusiveness, and responsibility.

2 Context

Sweden entered the 1930s with an economic and demographic crisis. The recession hit working families hard, leaving two-thirds in poverty (Schön 2017). The country also faced simultaneous population issues: birth rates among married couples fell faster than in any other European country, the age at first intercourse fell, and the share of children born out-of-wedlock rose (Lundberg and Åmark 2001). Society was traditional, characterized by a large divide in productive activities: men occupied almost all industrial jobs and women worked in domestic spheres but commonly left their jobs after the birth of the first child (Stanfors and Goldscheider 2017). The government's solutions

to these societal problems were the prohibition of contraceptives and criminalization of abortion. However, such measures proved inefficient in the worsening socio-economic conditions; when faced with a choice between poverty with children or a substantially better living standard without them, most young couples opted not to have children (SOU 1936).

The rise of the Social Democratic Party and its involvement in various governments gave momentum to efforts to solve the population crisis and radically remodel ideals of the individual and the state. Figure 1 illustrates the growth of public support for social democrats and the emergence of the social norm governing sexual life, as measured by the number of newspaper articles on the related topic that was taboo before the 1930s. By the advocacy of Gunnar Myrdal, then head of Sweden's population commission, and Alva Myrdal, a social democratic politician, the party's ideological solution was to implement a program that combined social reform and full sexual enlightenment, and thereby shape new societal identities "with aptitudes for personal independence and for collective cooperation" (A. Myrdal and G. Myrdal 1997 [1934]). The Myrdals emphasized the importance of sex education for young individuals, arguing that targeted education, not the banning of abortions or contraceptives, would better impact teenage sexual activity and build a positive view on the family. The Swedish Association for Sexual Education became another influential entity that shared the Myrdals' views and exerted pressure on the Swedish government to educate the population on sexual matters, eventually leading to the introduction of sex education in primary school (Boethius 1985).

[Figure 1 about here]

As a result of these political efforts, the sex education reform became embedded in the general development of the Swedish welfare state, which intended to instill better hygienic and moral standards in society and give everyone a positive attitude toward the family. In the late 1930s, the Swedish Parliament approved a new abortion law (which allowed abortion on medical and social grounds), a new employees' protection law (which prohibited the dismissal of women because of marriage, pregnancy, or childbearing and provided a 12-week parental leave), and in-kind transfers for childbirth and childbearing (which were intended to help women "become dependent on the state

instead of husbands") (SOU 1954). Soon the Swedish Association for Sexual Education organized courses for adult sexual literacy, but they never proved popular; nevertheless, the expansion of sexual education was stipulated in the early 1940s, after the introduction of a comprehensive sex education in primary school.

As argued by Carlson (1990), Swedish social engineering in population issues yielded a variety of results: a triumph of feminism over socialism, of reason over tradition, and of the state over the family. The "remodeled" cohorts (born in the 1930s–1940s and active in the labor force in the 1960s–1990s) experienced a significantly different life than their parents. The employment rates of married women of these cohorts expanded from 23% to 90%, and the female-to-male hourly wage ratio in manufacturing levelled off at 90% (Stanfors and Goldscheider 2017). A large proportion of people chose different occupations than their parents and the service sector (Schön 2017). Men increased their share in time spent on household and childcare duties from 15% to 40%—a rapid transformation that inspired scholars to name it "a genuine period of equalization" (Esping-Andersen 2009). These cohorts and their children favored prosocial jobs in government that designed policies associated with the Swedish welfare state, including parental leave, childcare, and equal pay legislation.

3 The 1942 royal decree on sex education

On April 10, 1942, the Swedish Parliament issued a royal decree Nr 169 that mandated the implementation of sex education in primary schools. But the decree also stipulated that in case the class teacher did not feel capable of providing sex education the school board could assign another teacher or a medical doctor to give sex education classes, and in case no such teacher could be found, sex education was not to be taught. Prior to the decree, Swedish authorities had introduced biology of sex and race in the girls' upper secondary school but reconsidered such a restricted focus in favor of socio-democratic values after WWII erupted. Before the decree, the primary school curriculum did not include morality topics, except for the historical narratives in the Religious Knowledge subject

where pupils learned the history of Christianity and other religions, read the Bible, and reflected on the values of Christian faith (Skolöverstyrelsen 1939). Sex education became compulsory in 1955, making Sweden the first country to provide comprehensive sex education in school (Zimmerman 2015).

Following the 1942 decree the authorities published instructions for teaching sex education in school—the guidelines in sex education in 1942 (Ecklesiastiskdepartementet 1944) and an instruction booklet as a separate book in 1945 (Royal Board of Education in Sweden 1945). Given the novelty of the subject and the potential reticence by teachers, the booklet provided a detailed account of the reasons for introducing sex education in school and the teaching methods, including sample lectures and answers to pupils' potential questions. The booklet remained unchanged until 1955, the year of the law on compulsory sex education, and even then, the new version refreshed the motivation but left the curriculum and the instructions untouched.

The 1942 decree ordered a change in the curriculum of Swedish primary schools (*folkskola*) in several subjects, where the course content differed by the pupil's age (Royal Board of Education in Sweden, 1945). In the primary grades (7 to 9 years old), the class on Local Geography and Nature separated pupils by sex and taught them about human physiology, hygiene, and the differences between the sexes. Starting from grade four to six in elementary school (ages 11–13 years), the course content included a variety of sex-related topics in the Biology and Religious Knowledge subjects, such as abstinence and masturbation, pregnancy and labor, sexual abuse, and the basics of family formation. In the final grades of elementary school (ages 14–15 years), sex education widened to include topics of sexually transmitted diseases and (low-prevention) contraception in the Biology subject. The History and Civics subject of these grades also included an extensive module on the social norms and public institutions governing sex and family life. The part on the moral and social aspects of sex was repeated in the curriculum for pupils who attended the ninth grade of the comprehensive school or continued their education in a secondary school (aged 16 years and older), but had no major differences compared with the earlier grades.

The most common message in the sex education instructions was abstinence from sexual activity among pupils but with a broader understanding of the practice. Because of the high and previously unmet demand for sex education among pupils of teen ages, schools sought to provide information on sex matters but by no means encouraged teens to have sex. The school curriculum thus began to cover the "firm biological grounds" of the sexes and sex relationships (Royal Board of Education 1964). However, the instruction was not limited to mere biology. The instructions desexualized masturbation (by destroying the myth that this activity may lead to disease and describing it as something that both boys and girls could perform) and encouraged abstinence to prevent potential life crises rooted in sexual activity among teenagers (e.g., cases of sexual abuse or homosexuality). Abstinence was touted as a "source of power that has a developing effect on the personality and on all aspects of intellectual and physical activity" (SOU 1946).

Thus, with introduction of sex education, schools became the venue for providing pupils with information about sex matters. As opposed to parents, schools could cover every pupil and provide uniform and clear information (The Royal Board of Education 1956). With this new role, through sex education classes, teachers could translate information on desirable social norms: "Most young people have certainly got to know in their own homes what their parents and society expect from them; it is the school's responsibility to see that it is made clear to all, without exception, what this is" (Royal Board of Education 1964, p.12).

One social norm concerned family formation. "Love–marriage–sex/children" was translated as the universal formula, in which persons were required to think rationally about having children, particularly in terms of costs. Young people were taught to understand the risk of "children being born without the parents having a home to offer them" (Royal Board of Education 1964, 12). In this period, when contraceptives became legalized but remained inefficacious (the first powerful contraceptive pill was introduced in 1964), a great emphasis in avoiding unintended pregnancies was put on practicing abstinence during adolescence. Importantly, the need for abstinence was motivated purely by rational choice, not moral or religious standards. To illustrate, young people were taught

that unmarried parents were not to be judged morally but viewed as an example of the difficulties that pre-marital sexual intercourse could lead to in life:

> Children born outside marriage have the same social rights as other children… It should be explained that unmarried people, especially if they have children, have special difficulties and problems, which do not exist for complete families with husband, wife, and children. This problem is dealt with in detail, but exaggerated attention should not be given to divorced people, widows, widowers, or unmarried mothers… An unmarried father or mother who does everything possible to overcome these difficulties shows a worthy public spirit, as also do those who give their help to such parents and children. (Royal Board of Education 1964, 15)

Although sex education instruction intended to prevent youth pregnancies, it did not intend to alter the desired number of children. For older pupils, instruction posited intercourse as a cause of birth, which was "a rich source of happiness" (p.50). It also paid attention to pupils' future role of builders of families, with emphasis on responsibility as parents and the questions of late marriage and marriage-partner choice: "The communion of two people is not a private but a community affair" (p.83). The need for abstinence was largely motivated by the intention to inform of the risk of unintended pregnancies in adolescence and outside marriage, not moral standards. Sex instruction for pupils aged 11–13 years also provided pictures with information on the development of the fetus and the course of labor, and for pupils aged 14–15 years, explained the welfare support for childbirth and care. Girls were recommended to observe two- to three-year intervals between births to prevent being worn out in adulthood and abortion of unwanted pregnancies.

Moreover, sex education intended to teach about individual and social responsibility, in line with the idea that public education could solve social issues and raise responsible citizens (Lindgren and Backman Prytz 2021). One such issue was to foster understanding that it is the pupils' parents, and often society, too, who bear the economic and other consequences of the pupils' early engagement

in sexual relations. Teachers revealed the unwanted costs of sexual activity, such as sexual abuse, undesired pregnancies, and the spread of sexually transmitted diseases:

> Teaching should aim at showing which things are not morally desirable from a personal and social point of view, and which cannot, therefore, be accepted as norms of behavior. […] Sex instruction must give them a clear understanding of the obligations which accompany sexual relations between a man and a woman. It is necessary to bring the understanding to the home that these are not just a personal matter between a man and a woman, but that they have consequences—especially for children—and are in general a social matter of great importance. (Royal Board of Education 1964, 11–12)

Another issue was to teach pupils about their interaction with society. Following the Myrdals' intention that school sex education must form responsible citizens, sex education provided knowledge on the laws and institutions connected to sexual life, such as sterilization, legal and illegal abortion, prosocial behavior, and welfare support for families (e.g., child allowances, home loans, and job security). In the spirit of its times, the 1942/1945 instructions informed about racial hygiene, encouraging pupils to prevent the birth of children "with hereditary predispositions to physical and mental illness" (Kungl. Skolöverstyrelsen 1945). Only the 1956 version excluded any mention of heritability aspects (Royal Board of Education in Sweden 1956). From 1949, sex instruction explained the employees' protection law, according to which wage-earning women could take a parental leave without losing their job or income. Teaching on the laws and institutions regulating citizens' sexual life was revolutionary, deemed to exemplify the functioning of a democratic society:

> It is a requirement of society that children leave school with enough knowledge to understand what society demands of them as citizens. They should have learnt through the school the broad outlines of the norms which govern the way people live together in a civilized community, and the consequences of following and not

following them. This is an important part of the school's educational activity.

(Royal Board of Education 1964, p. 11–12)

At its culmination, sex instruction in school encouraged pupils to think about the ways they would talk to their own prospective children on sex matters, ideally delivering the messages that they learned as pupils in school and through cooperation with a school medical officer (Royal Board of Education 1964). Sex instruction intended to diminish the influence of parents on pupils' choices in sexual behavior. Pupils were recommended to read on the current scientific and societal knowledge about sex life when they become parents themselves and talk to their own children.

4   Teacher training courses

Although the 1942 sex instruction booklet contained the detailed motivation, instruction, and sample lectures for delivering sex education, the Royal Board of Education (*Kungliga Skolöverstyrelsen*) did not assume that sex education could be introduced in all schools immediately. Many teachers had not received any training in the matter themselves and might have potentially been resistant to teach sex education lessons (Justitiedepartementet 1951). To ensure and support the introduction of the new curriculum and provide training to teachers throughout the country, the Royal Board of Education organized teacher training courses. Courses had already been held in large cities in 1936, but these courses had limited slots, focused on sex and racial biology, and only accepted teachers of girls' higher schools as participants (Justitiedepartementet 1951). These earlier courses played a minor role in the expansion of sex education because only 4% of girls attended upper secondary schools (Ekstedt 1976).

After the 1942 royal decree, the Royal Board of Education, with help of the regional school inspectors, began to organize teacher courses in different locations across the country. Potential locations were numerous: Sweden was divided into 52 school inspection areas, each of which included between five and 120 school districts (2,570 districts in total). Before organizing the course in a particular location, the regional inspectors contacted local authorities regarding the demand for a

course and, in consultation with them, chose the suitable date and location for the course instructors. The course instructors were medical doctors, teaching professionals from the Royal Board of Education, and volunteers through the Swedish Association for Sexual Education. Apart from the courses delivered by the Royal Board of Education, regional inspectors and school districts' principals also arranged their own courses.

We collected data on the teacher courses held between 1936 and 1958 from several national archives, which contain information on the planning and organization of all sex education courses throughout Sweden as well as correspondence with regional school inspectors. The information available includes the school year for the respective course, its location (municipality and school district), and frequently, the school districts covered by the course and number of participating teachers. The annual correspondence contained detailed information on the demand for and implementation of the courses in the previous years, and we were able to verify information and obtain the complete list of courses.

As shown in Figure 2, a total of 230 teacher training courses were held between 1942 and 1958. The number increased from 1942 onwards, indicating that inspection areas and school districts complied with the 1942 royal decree even before the inception of the courses from the Royal Board of Education. Courses organized by the regional inspectors or school districts' principals amounted to 58 in total. For the school years 1945–46, 1946–47, and 1949–50, the Royal Board of Education calculated an average participation of 213 teachers per course, which equated to the full participation of teachers from approximately 18 districts per course. The Royal Board of Education started to organize the courses from 1945 and delivered 172 in total. By 1952, each of the 52 inspection areas had received at least one course from the board.

[Figure 2 about here]

Although courses were conducted in each inspection area, the proportion of participants varied, ranging from less than 20% to 100%. However, since school principals enrolled voluntarily,

the courses did not cover all school districts and municipalities within the inspection area. Owing to the uneven coverage, the Royal Board of Education continued to organize courses in previously uncovered locations even after the enforcement of the law on compulsory sex education in 1955. By 1958, only one inspector expressed a demand for teacher training courses, leading to their eventual discontinuation.

The training courses were closely tied in content to the 1945 instruction booklet distributed to the participating teachers beforehand. Even the earlier courses (between 1942 and 1945) relied on the pre-print of the 1945 booklet in the form of the guidelines, and most school districts followed the guidelines (Justitiedepartementet 1951). The program of a one-day course could, for example, entail three 45-minute lectures and three 35-minute demonstration classes for grades 1 (biology of the sexes), 5 (abstinence and unintended costs), and 7 (social norms). Since sex education was introduced in multiple subjects, primary school teachers from all subject areas participated. A survey conducted by the Royal Board of Education in 1954–55 showed that the majority of teachers were satisfied with the course in terms of the content and number of lectures and demonstration classes (Riksarkivet 1943-1958).

5   Conceptual framework

The 1942 sex education reform is a prime example of how schools can transmit social norms to the minds of young individuals. This reform could directly affect individual economic well-being in three important ways: (1) The reform explicitly educated pupils about social norms regarding sexual activity and translated these norms into their personal utility function, consequently postponing the pupils' first intercourse (or reducing the frequency of intercourse) and decreasing the probability of unintended pregnancies. (2) The reform provided input to the development of pupils' personality skills, such as personal responsibility and internal loci of control—both important components of child human capital that improve future earnings in adulthood. (3) The reform provided a larger cultural framework, in which sex education translated into new identities for pupils—the reform

encouraged pupils to become forward-looking and responsible, and, particularly girls, to exercise the right for protection from sexual abuse; the reform therefore affected the individual's choice of education, entry into the labor market, and occupation.

First, the sex education reform translated social norms to individual preferences, particularly regarding sexual activity. The introduction of sex education aimed at both providing knowledge and value judgments (SOU 1936). The reform tackled a previously taboo subject on sexual activity and family formation and imparted previously undisclosed information to pupils. Consequently, the social norm became personally meaningful to pupils, going beyond abstract moral or religious implications and directly entering pupils' cost-benefit analysis.

The new information provided was expected to alter the perceived costs and/or benefits of sexual activity, as proposed by Oettinger (1999). Individuals could then make the informed decision to engage in sexual activity (as opposed to abstaining) based on a cost-benefit analysis in which the perceived benefits outweighed the perceived costs. These benefits included immediate gratification, the development of an ongoing relationship, an increase in social status among peers, or the acquisition of knowledge. Meanwhile, the inherent costs associated with sexual activity included the deterioration of an ongoing relationship, disapproval from parents or society, and the possibility of illness or undesired pregnancy. The greater the amount of information provided by sex education, the more significant the potential impact on a teenager's sexual behavior.

The utility-altering sex education that reduced the utility of sexual activity was expected to ultimately reduce the number of sexually active individuals and unintended pregnancies (see Figure 3). Such sex education, for example, revealed the benefits of abstinence or the perceived individual and societal costs of sexual activity. These components were implemented by the sex education reformers in 1942–1958 in Sweden. Abstinence was taught to be a social norm and a source of personality growth, and unintended pregnancies and venereal diseases were posited as immense costs for pupils themselves, their families, and society. Moreover, teachers were instructed to by no means encourage teens to engage in any sexual activity.

Second, the sex education reform impacted personality skills. Personality skills, along with preferences, are malleable in adolescent ages and they affect lifetime economic well-being. Children's time preferences and personality changes, such as becoming more conscientious, at ages 13–15 years predict early-career entry, eminence, and various health and labor outcomes in prime working ages (Hoff et al. 2021; Bernstein, Lubinski, and Benbow 2019; Golsteyn, Grönqvist, and Lindahl 2014). Through role models, schools have affected pupils' prosocial inclination (Kosse et al. 2020). Regardless of cognition, such non-cognitive skills as responsibility, grit, self-efficacy, and prosociality are highly valued in the labor market (Kautz, Jagelka, and Heckman 2019). In relation to sex education, evaluations of randomized control trials have shown that sex instruction with abstinence and ethical components improves gender equitable skills, confidence, and self-identity among adolescents shortly after the intervention (N. A. Constantine et al. 2015; Louise A. Rohrbach et al. 2015).

Third, sex education provided a larger cultural framework, in which sex education translated into new identities. Identity (i.e., an individual's assignment to a social category) enters the individual's function and drives behavior (Akerlof and Kranton 2000); institutions such as schools shape norms and identities (Kranton 2016). Through sex education, schools have helped young individuals in Sweden to adapt to ongoing societal change and foster a sense of citizenship and identity (A. Myrdal and G. Myrdal 1997 [1934]). The reform placed significant emphasis on whom the society considers an ideal family member: rational individuals who conceive children when the necessary economic resources to support a family are available (Boethius 1985). It also framed the use of contraceptives inside and outside marriage as legitimate for economic reasons. For girls, it translated the knowledge of own's human rights in relation to engagement in sexual activity and de-stigmatization of working mothers. Finally, the reform explicitly addressed pupils' sense of citizenship by transmitting the ideal of an active and responsible participant in society. The identity economic models predict that new identities do not necessarily lead to higher earnings but instead

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appear in educational, labor-market, and occupational choices, which align with such identities.

6 Methodology

We intended to assess the causal impact of the sex education reform in Sweden on the life-cycle outcomes of pupils and of their children. An ideal experiment for this goal must provide the difference in the pupils' potential outcomes between the municipalities with and without the treatment. To replicate such an experimental setting, our study adopted an identification strategy that relied on the gradual roll-out of sex education across Swedish municipalities in 1942–1958. The institutional context of the reform gave us two sources of variation: (1) in cohorts who—never or ever—attended the sex education classes in the fifth to eighth grades of primary school in a particular year between 1942 and 1958; (2) in municipalities that eventually introduced the reform or had never done it. The combination of these two sources of variation formed a DID approach and allowed us to exclude the influence of the following biases: time (i.e., cohort) bias, which was important because all outcomes we studied changed across cohorts; and selection bias, arising from the potential self-selection of municipalities into treatment.

The most common method to estimate the DID effects is to run a two-way fixed effects regression on the sample. However, the recent methodological literature has cautioned against using regression when the reform is staggering (Roth et al. 2023). In such a case, the regression makes comparisons that a researcher is unwilling to make—it compares later- with earlier- and always-treated municipalities, creating negative weights and biasing the causal effect when the local effects (i.e., across municipalities or years) are heterogeneous. We anticipated the issue in our estimation sample, given that the proportion of earlier- and always-treated municipalities exceeded 50%. Appendix A presents the results of the pre-tests for the presence of negative weights and heterogeneous effects (Roth et al. 2023). We discovered that the effects were indeed heterogenous, and a quarter of the estimation weights for the ever-treated group were negative. Consequently, the use of the two-way fixed effects estimator would provide a DID estimate that would significantly

deviate from the true treatment effect.

We therefore implemented a DID approach by means of the group-time estimator developed by Callaway and Sant'Anna (2021). The group-time estimator is one of the potential (and similar) solutions when heterogeneous effects are present; this estimator is more attractive owing to its weaker assumptions and more conservative inference (for discussion see Roth et al. 2023 or Clement de Chaisemartin and D'Haultfoeuille 2022).

The estimator's building block is the group-time average treatment effect on the treated, $\text{ATT}(g,t) = E[Y_{i,t}(g) - Y_{i,t}(\infty) \mid G_i = g]$, which gives the average treatment effect at time t for the cohort first treated in time g. The estimator defines time as a single cohort (i.e., year of birth in our case) and group as the year of birth (first cohort) when the municipality or a group of municipalities received treatment, with never-treated municipalities assigned into a separate category. Under the staggered versions of the parallel trends and no anticipation assumptions, one can identify $\text{ATT}(g,t)$ by comparing the expected change in the outcome for cohort g between periods $g-1$ and t to that for a control group not-yet-treated at period t. Clearly, the estimator makes all comparisons relative to the *last pre-treatment period*, which is reasonable as we would like to capture sharp changes in the cohorts' outcomes related to the reform. Not-yet-treated units can include the units treated later, never-treated, or both groups, but earlier-treated municipalities are never included into the control group.

To estimate the ATT effects, the estimator employs a canonical two-way fixed-effects regression and conducts estimations separately for each g and t:

$$Y = \alpha_1^{g,t} + \alpha_2^{g,t} \cdot S_g + \alpha_3^{g,t} \cdot 1\{T = t\} + \beta_3^{g,t} \cdot \left(S_g \times 1\{T = t\}\right) + \varepsilon^{g,t} \ (1),$$

where S_g denotes a "group" (in our case, the first year when all primary school teachers attended sex education training courses in the municipality) and t is a year of birth. In our baseline estimations, we only include never-treated municipalities ($S_g = 0$) into the comparison group, because namely the comparison to this group would require a "weak" parallel trend assumption—parallel development

only for one year prior to the treatment. In contrast, when not-yet-treated are included into the comparison group, parallel-trends must hold for longer pre-treatment periods because not-yet-treated participate in both a control group (before treatment) and a treated group. However, we recognized that our results would not be sensitive if we additionally included not-yet-treated municipalities or used only them as a comparison (see Section 8.3 for the results).

After, group-time average treatment effects are aggregated into an overall effect of participating in the treatment. First, single ATT(g,t)'s are aggregated for each group $\tilde{g}$ across all their post-treatment periods $\mathcal{T}$ using the formula: $\theta(\tilde{g}) = \frac{1}{\mathcal{T}-\tilde{g}+1}\sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t)$ (2). Then, these effects are averaged together across groups and weighted according to group sizes to form a summary DID effect, the main aim of the estimation: $\theta^0 = \sum_{g \in G}\theta(g)P(G=g \mid G \leq \mathcal{T})$ (3). Therefore, the aggregated effect θ^0 is the average treatment effect of participating in the treatment experienced by all units that ever participated in the treatment and hence resembles the ATT in the canonical DID setup with two periods and two groups. We obtain more conservative inference by means of a simple multiplier bootstrap procedure (Callaway and Sant'Anna 2021). We also clustered the errors at the municipality level to account for the dependence of groups over time.

The main identification assumptions of the DID with a group-time estimator are the absence of anticipation, stable unit treatment value, and parallel trends (Callaway and Sant'Anna 2021). We collected correct data on reform implementation and hence ensured that anticipation—the behavior of the units mimicking the presence of treatment before the actual treatment occurred—was highly unlikely. The stable unit treatment value assumption was also unlikely to be violated in our work because pupils typically attended the schools in the same municipalities where they resided (Ekstedt 1976). The group-time estimator requires that pre-trends must not exist for one year prior to the reform. To examine the presence of pre-trends and the effects' dynamics, we also estimate the ATT(g,t) effects aggregated by event-years. The group-time estimator estimates single ATT(g,t)'s by group and event years: $\theta^e = \sum_{g \in G} 1\{g + e \leq T\} P(G=g \mid G + e \leq T)ATT(g, g + e)$ (4). For instance,

the effect at *e* = 0 occurs for the first cohort affected by the reform. Then the effects are aggregated by group sizes using *(3)*.

However, the parallel trends assumption may be violated even in the event of no visible pre-trends (Rambachan and Roth 2023). In the context of the sex education reform, our treated and control municipalities might have followed distinct development paths even in the absence of the reform, attributable, for instance, to different social norms or labor-market opportunities. As such, we imposed a relaxed, conditional parallel trends assumption that required a parallel trends assumption to hold conditional on covariates and hence added an extra degree of robustness (Roth et al. 2023). To obtain the conditional ATT(*g*,*t*) values, we used the regression adjustment procedure in conjunction with the group-time estimator (Sant'Anna and Zhao 2020; Heckman, Ichimura, and Todd 1997).

Under the conditional parallel trends, this procedure estimates the conditional expectation of the outcome among untreated units and then averages these predictions using the empirical distribution of covariates among treated units:

$$\hat{\tau} = \frac{1}{N_1} \sum_{i:D_i=1} \left((Y_{i,t} - Y_{i,g-1}) - \widehat{E}[Y_{i,t} - Y_{i,g-1} \mid D_i=0, X_i] \right) \quad (5),$$

where $\widehat{E}[Y_{i,t} - Y_{i,g-1} \mid D_i=0, X_i]$ is the estimated conditional expectation function fitted on the control units but evaluated at X_i for a treated unit. After estimating the conditional ATT(*g*,*t*)'s, they are weighted and averaged as in *(2)* and *(3)*. We identified the conditions as a rich set of covariates at the municipality level: urbanization, primary-school expenditure, sex ratio of the birth cohort, share of votes for social democrats, and the proportion of working women. Since the estimated propensity score based on these covariates had limited overlap among several small treatment groups in the estimation sample, we used binary indicators of the covariates, measured at the median value.

The group-time estimator that we used in the main body of the paper is a generalization of other approaches proposed in the methodological DID literature (Sun and Abraham 2021; Clément de Chaisemartin and D'Haultfœuille 2020). An alternative is to use an imputation approach

proposed by Borusyak, Jaravel, and Spiess (2021), Gardner (2022), Liu, Wang, and Xu (2022). The drawback of imputation approaches (as compared to the group-time estimators) is that they require a researcher to be more confident about parallel trends (Clement de Chaisemartin and D'Haultfoeuille 2022). In the robustness analysis in Section 8.3, we conducted estimations using the imputation approach and obtained estimates that were nearly identical to our group-time estimates.

7 Data

7.1 Treatment data

Our reform data consisted of the listings of sex education training courses for schoolteachers in Sweden. From the institutional context of the reform, we learned that, although every school inspection area organized at least one course in 1942–1958, school districts/municipalities were covered unequally. The correspondence letters between the regional inspectors and the Royal Board of Education indicated that the course locations were chosen based on either accessibility by the participants or the course instructors (in this case, a course location was a city at the crossroads of several municipalities or regions) or proximity to the school districts that remained uncovered by the reform (i.e., central municipality of the area) (Riksarkivet 1943-1958). Among all the municipalities and inspection areas for which we had participation rate data, only 40%, equivalent to 1,024 municipalities, enrolled all their primary school teachers in the course. The unequal geographical distribution of treatment created a risk of mixing treated and untreated municipalities within a single group, which could potentially negate any reform effects.

We therefore obtained the participation rate for each municipality. We combined the number of participants from the archives' listings with the numbers of teachers by municipality from annual address books (Adresskalender över Sveriges skoldistrikt 1943–1957). Among the municipalities that participated in multiple courses across different years, we found very few cases where the proportion of participants differed; instead, municipalities either participated partially or fully. We then assigned full treatment to the following: locations where the course was organized, locations

with a 100% participation rate, and neighboring municipalities within a 100-kilometer radius, when the archival data state that the course was also held for the buffer area. Through our collection efforts, we compiled a list of municipalities categorized by their entry to treatment for each year between 1942 and 1958. As such, we could ascertain that the implementation of the treatment did not begin before the year of implementation assigned based on full treatment (i.e., no anticipation).

As shown in Figure 4, our treatment data revealed significant variation in the entry to treatment over time, with less than half of the municipalities remaining untreated by 1958. The entry into treatment occurred gradually across different waves of courses, starting with 480 unique municipalities covered by early courses organized by school districts or inspectors themselves. Subsequent waves of courses organized by the Royal Board of Education treated additional municipalities, reaching 409 by 1953 and 443 by the end of 1958. By 1959, 1,211 municipalities (47%) remained uncovered by the reform. When accounting for population size, treated municipalities comprised 65%, whereas untreated municipalities constituted 35%. Although full coverage was not achieved by 1958, contrary to the reform organizers' reports, our findings closely aligned with the treatment self-reported by individuals, as further presented in section 8.1.

[Figure 4 about here]

We investigate the factors that explain the earlier initiation of the sex education reform in Table Appendix B. In the identification phase, we do not assume exogeneity of the reform. In natural experiments like ours, meeting such an assumption is unlikely. However, understanding the driving forces behind the reform at the municipality level will support the selection of pre-treatment variables for the conditional DID estimator. As shown, municipalities that introduced the sex education reform spent more on primary schools and had less conservative social norms, as exemplified by the lower share of unemployed women and higher share of votes for the Social Democrats in the last national election. Population size does not impact the initiation, suggesting that there is no urban-rural divide (and therefore no differential economic development) between the treatment groups of municipalities.

7.2 Microdata

We combined the reform data with microdata (Swedish Interdisciplinary Panel, hosted at the Centre for Economic Demography, Lund University), which contain panel information linked through unique personal identifiers from various administrative registers for the cohorts born since 1930 as well as for their parents, siblings, and children. Lazuka (2020) previously described such data, their sources, and completion. We selected from these data information on individuals who were aged 11–15 years in 1942–1958—or those born between 1930 and 1946 (first generation), their parents (parental generation), and children (second generation). Based on Sweden's Census of Population from 1950 and 1960, we identified the municipalities of residence close to the time when the study cohorts were in primary schools.

A group-time estimator typically requires at least one pre-treatment cohort for each group. Thus, our first treatment group was 1946 instead of 1942, and our estimation sample included 1,939 out of the full pool of 2,503 municipalities. The first available cohort in our microdata was composed of individuals born in 1930 (i.e., they were 16 years old in 1946). Since large and less conservative municipalities implemented the reform earlier, particularly major cities like Stockholm, which organized courses in the early 1940s, we set our final estimation sample to consist of a more homogeneous group of treated and untreated municipalities (see also Table in Appendix B). These municipalities were mostly covered by the courses organized by the Royal Board of Education. The first-generation sample provided information for 1,026,358 individuals, whereas the second-generation sample included data for 2,187,218 individuals.

The variety of the outcomes that we utilized was large: fertility, marriage, cause of hospital admission, education (years, field, and degree), earnings, occupation, and political participation. The estimation sample for the hospitalization outcomes included data for only the years when the county reported full information on hospital records. We constructed an occupational score based on the occupation and socio-economic index (Lambert et al. 2013). Most of the data are available as a panel in 1950–2021. As such, we studied the outcomes across the life cycle, both in terms of averages and

for specific age groups, up to the ages of 70 years for the first generation and 36 years for the second generation.

8 Results

8.1 "First stage"

Before exploring the causal effects of the sex education reform from the DID estimations, we analyze whether teacher training on sex education was connected to the pupils' knowledge of sex education and sexual behavior, for which we used aggregated data from the national survey on sex habits among Swedish adults conducted in 1967. The survey used a random sample of about 2,000 men and women (with a non-response rate of 9.5%) that was deemed representative of the Swedish population (Zetterberg 1969). The survey's microdata have not been kept, but its summary provides a detailed presentation of the cohorts' outcomes, which we used in our analysis.

Panel A in Figure 5 shows that the percentage of pupils who received sex education in school increased dramatically for the cohorts who were in school when the 1942 sex education decree was enacted: 7%–12% of pupils who received primary or secondary education before sex education was introduced and 31–65% for the consecutive cohorts. With such rates of coverage by sex education, the survey has provided support for our own course-based estimations of reform treatment rates. The summary of the 1967 survey concludes that the decision to introduce sex education accelerated a trend toward total dissemination of basic sexual knowledge (Zetterberg 1969).

[Figure 5 about here]

The survey has also demonstrated the development of sex behavior outcomes. As Panel B in Figure 5 shows, the median age at first intercourse had been on a steady decline before the sex education reform, but this age levelled off starting from the reform cohorts. The mean age at first intercourse mimicked the mean age at first birth for the same cohorts, suggesting that abstinence affected the postponement of childbirth. As suggested by Ekstrand et al. (2011), the introduction of sex education was also related to the lower rates of sexually transmitted infections in the 1930s–1950s

in Sweden compared with the US.

8.2 Results for the first generation

8.2.1 *Sexual activity*

Regarding the DID estimations for the health outcomes related to sexual activity or abuse in ages 34–40 years, the youngest possible from the inpatient hospital records, the aggregated reform estimates are presented in Table 1 and the event studies, in Figure 6. We find that, owing to the sex education reform, women were much less likely to abort a pregnancy, by 1.1 percentage points (or 79% of the pre-mean). For this and other outcomes, we present event studies for 5 event-years before and after the reform; for post-reform event-years, they essentially show the reform effects for children exposed at ages 15 to 11 at the earliest. For abortion, the large reform effect emerges for all event-years in the post-treatment period, with no evidence of pre-trends. At the time, women could receive approval for abortion for medical reasons, including risk to the woman's life, potential transmission of a serious disease, sex abuse, or a "weakness," which referred to worn-out mothers who already had children (Sjövall 1972). Illegal abortion was punishable with imprisonment, but women were rarely sentenced, and those who already had a child could receive a certificate for legal abortion from a doctor. Our findings therefore suggest that the reform encouraged women to avoid unwanted pregnancies.

[Table 1 about here]

[Figure 6 about here]

Furthermore, we observe a large negative impact on the probability of cervical cancer and inflammatory diseases of female pelvic organs, amounting to 1 percentage points, or 23.5% of the pre-mean. Since cervical cancer is caused by sexually transmitted diseases (Venkatas and Singh 2020), the effect can be attributed to reduced sexual activity, a smaller number of sex partners, or increased use of protection. However, we observe no effects for sexually transmitted diseases themselves or sexual abuse as a result of the reform. Notably, detecting such effects could be

challenging for these causes owing to the significantly underrepresented rates of related inpatient admissions, unlike those for cervical cancer that could be treated only within a hospital system and hence were registered in full.

We examine the reform effects on sexual activity for subgroups of women with different preferences. Theoretically, utility-reducing sex education should have varying impacts on different subgroups: no effects for individuals who prefer abstinence, no or minimal negative effects for those who prefer sexual activity without fear of pregnancy, and significant negative effects for those who prefer sexual activity but are unwilling to experience pregnancy (Oettinger 1999). In the absence of individual-level preference data, we approximate them using municipality-level youth fertility rates. We then estimate the DD effects on abortion and cervical cancer probabilities for subgroups of women in zero-, medium-, and high-fertility areas, corresponding to preference groups, and present the results in Appendix C. The results align closely with theoretical predictions, with the major reform effects (-1.2 percentage points for abortion and -1.7 percentage points for cervical cancer) observed in the group of women resided in medium-fertility areas in school ages, who on average preferred sexual activity to abstinence but feared pregnancy.

8.2.2 Family formation

The sex education reform was designed to impact pupils' preferences in teenage childbearing and adult family formation. Table 2 and Figure 7 present the results for the impact of the sex education reform on individual decisions to marry across the life cycle. We analyze the reform impact on marriage of convenience, defined as marriages occurring within a year after the childbirth date owing to the limitation that only the year of marriage was available. The results show that the reform significantly reduced the probability of such marriages by age 18 years, which was the legal age for women to marry (0.17 percentage points or 28% of the pre-mean). This reducing effect is twice the average among women, aligning with the expectation that women bore greater of the costs associated with early engagement in sexual activity compared with men. We also note a large and statistically

significant reform effect for the marriage of convenience for the whole life cycle of men and women (-0.4 percentage points or 11.3% of the pre-mean). Therefore, the reform reduced unplanned births outside marriage and fostered marriages based on romantic affection, as intended by the reform.

[Table 2 about here]

[Figure 7 about here]

Turning to traditional marriages, we do not find any reform effects for the probability of marrying by 18 years old. However, the reform effects for the probability of marrying emerge at the age of 25 years, at which the longest schooling available was completed and people embarked on a career. As such, the reform reduced the probability of marriage by 2 percentage points or 4% of the pre-mean. Such reform effects diminished over the life cycle but remained present by the age of 45 years, meaning that the reform delayed marriage and somewhat influenced the proportion of people who never married.

Table 3 and Figure 8 present the results for parenthood outcomes. We find no effects for teenage pregnancies, although we noted that the reform reduced the probability of having a child at the ages of 18–25 years by 1.4 percentage points or 2.8% of the pre-mean. The effect decreased slightly under conditional parallel trends assumptions but remained statistically significant. The median age of childbearing was 30 years, and for later ages, the reform instead encouraged individuals to increase the likelihood of having a child, by 1.8 percentage points (4% of the pre-mean). Therefore, the reform helped increase the total number of children born across the life cycle, although this effect was weak and unclear in the event studies. The reform effects for parenthood did not consistently differ by sex, although they implied that women exhibited larger postponement and catching-up effects compared with men. In sum, across the full life cycle, the reform encouraged individuals to postpone parenthood but did not alter the desired number of children.

[Table 3 about here]

[Figure 8 about here]

8.2.3 *Educational attainment and occupational sorting*

The reform encouraged individuals to pursue higher education by emphasizing the internal loci of control or increasing the perceived opportunity costs of starting a family. It also led individuals to choose specific educational fields associated with occupations involving public responsibilities and greater employability (for women). The respective results for educational outcomes are presented in Table 4 (aggregated) and Figure 9 (event studies).

[Table 4 about here]

[Figure 9 about here]

The effects on years of schooling are positive and statistically significant at the 1% level, albeit relatively small (0.04 years or 0.5% of the pre-mean). However, the positive effects on schooling appear much stronger and more reassuring (according to event studies) for the completion of secondary education and vocational training, each increasing by 1 percentage point or 5% of the pre-mean. Ultimately, 20% of individuals obtained such schooling. Thus, the reform improved the likelihood of pupils successfully completing primary school, given that enrollment in secondary school requires passing a national test and completing all primary school subjects (Skolöverstyrelsen 1955). The same hold true for vocational training, which is provided after the age of 18 years and involves an entrance exam (SOU 1974). The event studies show that the effects on education increase the earlier children enroll in sex education.

Regarding the educational fields, we find that the reform improved the probability of individuals completing specific fields of upper secondary and college education. Our cohorts reached adulthood during a period with labor market opportunities in manufacturing and the private and public sectors. Notably, the implementation of sex education in primary schools stimulated individuals to pursue further education and opt for degrees in social sciences, law, and administration (1 percentage point increase or 10% of the pre-mean). Compared with other fields, the social sciences required a willingness to work for the common good, demanded high skills, and offered employment guarantees

amid the growth of public services. Additional analyses reveal that men chose to pursue careers in health care, whereas women opted for teaching professions. This result suggests that for women who grew up in an era where their mothers were not working, future employability played a great role when choosing an educational field.

The reform should have impacted women's labor-market participation by facilitating abstinence (with greater effects among women), personal responsibility, and the acquisition of education that enhanced employability. Table 5 and Figure 10 present the results for women's employment. We find that the reform allowed women to enter the labor force (from being out of the labor force to receiving employment) at the ages of 30–35 years (1.5 percentage points or 4.8% of the pre-mean). Such effects decrease in size for the later reproductive ages, consistent with the phenomenon of women getting married and having more children during these stages. However, employment effects are also present for married women, as women also became less likely to be employed in a household sector (as maids or in the small businesses of their husbands), equivalent to an increase in the probability of having an independent occupation (1.5 percentage points or 15.4% of the pre-mean). Therefore, the sex education reform contributed to the acceleration of women's entry to paid jobs—a development that distinguishes Sweden from other developed countries in which this trend was staggering.

[Table 5 about here]

[Figure 10 about here]

We also find that the reform increased the interest in studying in fields leading to occupations involving working for the common good, in line with the idea that the reform had effects beyond sex behavior and was central to bringing forward the concept of the social democratic welfare state. Therefore, the reform impacted the occupational structure of the cohorts under analysis. Table 6 and Figure 11 present the results for the reform effects for various occupational outcomes. Consistent with our previous results for educational fields, individuals in the treatment schools are more likely

to be employed in public services (0.8 percentage points or 3% of the pre-mean) rather than in industry or private services. These shifts are driven by the rise in number of not only specialist jobs in health care and teaching but also the positions in government and administration. This reveals a unique path of career development for policy makers—higher professionals who during their prime working ages in the administration became responsible for the design of the welfare state.

[Table 6 about here]

[Figure 11 about here]

The reform effects on occupations did not only emerge for a narrow group of public services— we find a statistically significant increase in overall occupational score (by 0.3 units, 0.5% of the pre-mean). We additionally find that the entire distribution of the occupational score shifted to the right, with somewhat larger effects at the bottom decile (for women) and top decile (for men and women).

8.2.4 Permanent income

Because the reform affected individuals' human capital by fostering non-cognitive skills, we expected to find positive reform effects on individuals' permanent income. Table 7 and Figure 12 present the results for the log earnings, which are age-specific and permanent. Our results show that the sex education reform increased individuals' permanent earnings income by 9.3% and 10.9% under the unconditional and conditional assumptions, respectively. The magnitude of the effect is striking, given that the reform encouraged individuals to choose jobs in the public sector, which, at the time, did not guarantee high wages compared with employment in manufacturing or the private sector. The effect on earnings is most pronounced between ages 48 and 50 years, as earnings for both men and women peaked. At this stage, the effect was significantly larger for women—10.7% versus 6.9% under conditional terms. The reform-driven shift in the employment of women from the unpaid household sector to the paid work and service sector has created significant productivity gains, given that working in the public sector was valued at three to four times as much (Krantz 1987). Consequently, the reform reduced the gender earnings gap and fostered the convergence of women's

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earnings to men's.

[Table 7 about here]

[Figure 12 about here]

In additional analyses, we also study the exit of the affected cohorts from the labor force owing to age. The reform increased both sexes' attachment to the labor market in retirement ages: the reform decreased the probability of early retirement and, additionally for women, of receiving a "guarantee" pension, given in case of no or very low income in pre-pension ages (see Appendix D).

8.3 Robustness analysis: The validity of the estimator and interaction with other social reforms

Methodologically, we ensured that the assumptions of the DID approach were likely to be met. We carefully checked the treatment data for each municipality to avoid anticipation. The event-study analysis revealed no significant pre-trends, and the results obtained were robust to controls for the pre-trends based on various observable regional characteristics measuring the development of the economy and of social norms. However, the DID approach does not control for the biases stemming from the pre-trends in unobservable characterstics and from time-varying effects, such as the influence of overlapping reforms. We thus assess these effects as well.

We first examined whether pre-treatment trends in unobserved municipality characteristics affected the results. To do this, we ran the estimations, using the group of not-yet-treated municipalities instead of never-treated municipalities as a control. Municipalities that eventually introduced the reform likely shared many observed and unobserved characteristics. As shown in Figure E.1-E.7 of Appendix E, the results for all outcomes are almost identical to what we report in the main body of the paper.

The methodological DID literature has proposed several approaches to estimate ATT effects for the case of staggered treatment timing that are robust to heterogeneity over time and space. However, Callaway and Sant'Anna's group-time estimator is a generalization of two other popular approaches. The estimator by Sun and Abraham (2021) produces the same aggregated and weighted ATT, using never-treated or last-to-be-treated units as controls. The same is true for Clément de Chaisemartin and D'Haultfœuille (2020)'s estimator, with the only difference being a particular choice of weights.

Instead of the group-time estimator, we consider the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021), which offers a valid alternative. Other imputation approaches are analogous (Gardner 2022; Liu, Wang, and Xu 2022). The imputation etsimator fit a TWFE regression with year and unit fixed effects only for the not-yet-treated units, infer the never-treated potential outcome for each treated unit using the predicted value from that TWFE regression, and then aggregates the unit-specific ATT estimates into the summary ATT parameter with similar weights as in the group-time etsimator (i.e, group sizes). The estimator relies on the "long" parallel trends (i.e., for all pre- and post-treatment periods) and no anticipation assumptions. While the group-time estimator makes all comparisons relative to the last pre-treatment period, the imputation etsimator makes comparisons relative to the average of the pre-treatment periods (see Roth et al. 2023 for more details). In Table E.8 Appendix E, we perform estimations for women's sexual activity outcomes based on the imputation approach and obtain the estimates nearly identical to the group-time estimates.

We further assess the impact of overlapping reforms. The sex education reform was planned and implemented together with a set of social reforms intended to reduce the costs of childbearing (SOU 1954). Most reforms (e.g., the employees' protection law and childbearing cash transfers) were introduced abruptly across the country and consequently affected all women (i.e., mothers of the first generation). Hence, the effects of such reforms had been absorbed by the birth cohort dummies. One reform—opening of maternity wards with a 10-day laying-in period—occurred gradually across the

cohorts and municipalities and had lasting economic effects for the cohorts in their adulthood (Lazuka 2023). We thus study the impact of new wards' openings on our reform estimates.

Table 8 and Figure 13 present the aggregated (DID) and event-study (DDD) estimates for the interaction effects of the childbirth reform with the sex education reform for the marriage of convenience in ages 18–45 years. The results for the other outcomes provide a similar pattern. Because we had to exclude municipalities for which the assignment of treatment by social reforms was imprecise (as in Lazuka 2023), we first ascertain that the sex education reform effects remained unchanged for the restricted estimation sample. We find that the sex education reform effects do not differ economically and statistically between the subsamples with municipalities that introduced another reform and those that never or had not done so. In relation to the childbirth reform, the absence of interaction effects suggests that maternal empowerment, which was the reform's target, did not affect the first generation's outcomes.

[Table 8 about here]

[Figure 13 about here]

Arguably, two other events could have influenced the estimates of the sex education reform. First, starting in the 1940s, the authorities gradually extended mandatory schooling to 9 years (Holmlund 2020). In the context of our study design, this schooling reform implied that cohorts at age 16 years, which we considered untreated, could potentially have received sex education. However, our analysis revealed that only 3.9% of pupils within our cohorts were affected by the schooling reform. This minimal impact suggested that any potential bias on the effects would be negligible. Additionally, our results from the event studies did not provide any evidence of anticipatory effects, which would manifest as a spike in the first year before the reform. Second, in other European countries that participated in WWII, adult women (i.e., the parental generation) experienced favorable labor market conditions in the 1940s–1950s owing to wartime casualties among men. Notably, Sweden did not participate in WWII. In summary, our analysis indicated that

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no other events influenced the outcomes of the sex education reform.

8.4   Results for the second generation

We also study the outcomes of the offspring generation of the individuals affected by the sex education reform, for three primary reasons. First, the sex instruction in school explicitly educated pupils on how to inform their own future children on sex matters, by encouraging pupils to rely on contemporary scientific knowledge and school instruction instead of private experiences in sex matters (The Royal Board of Education 1956). Second, research has shown that social and gender norms exercised by parents are transmitted to the economic preferences of their children and persist in children's labor supply outcomes in adulthood (Falk et al. 2021; Bredtmann, Höckel, and Otten 2020; Bertrand 2019; Farré and Vella 2013; Fernández, Fogli, and Olivetti 2004). Third, our previous findings for the first generation showed positive reform effects for men and women in favor of prosociality and egalitarianism. We therefore expected that the reform would affect the related outcomes of the next generation.

We applied the same methodology as for the first generation—unconditional and conditional group-time estimator in two samples, where the second-generation individuals were linked to their mother's and father's treatment variables. To avoid uncertainty in the treatment status of the family, we excluded the cases where either a mother or father was unknown or parents had a different treatment under the sex education reform, such as one parent being ever-treated and the other, never-treated. Appendix F shows the second generation's results while controlling for the first generation's income and educational transmission. The cohort span of the data (including cohorts born in 1948–1985) allowed us to observe individuals until they turned 36 years old. We then selected a narrower set of outcomes than for the first generation, focusing on prosociality and gender-equal roles.

Table 9 gives the results for the second generation's girls and boys. We derive two main findings from the analysis. First, the educational and occupational sorting of girls shows maternal treatment reform effects: we observe a higher probability (1.1 percentage points or 39% of the pre-

mean) of choosing law and administration as an educational field and a higher probability (1.5 percentage points or 52% of the pre-mean) of running their own business. Moreover, in the case of girls, the effects are not driven by parental transmission of education or income. Girls did not choose a job in public services, as their mothers did. Thus, the sorting effects suggest that sex education impacted children through maternal empowerment rather than prosociality. Such effects are gender-specific, as they show up only with the randomization of the mother's treatment, with no similar effects for the father's treatment. Second, we find no effects for boys for the same outcomes or those that measured gender-equal roles in their own families, such as an uptake of parental leave. As shown in Figure 14, conditional event-studies support our findings for girls on the mother's side.

[Table 9 about here]

[Figure 14 about here]

Consequently, the second generation's effects for girls were driven by cultural transmission— social norms and gender roles exercised within the family influenced children's decisions on career paths. These findings hold significant importance, especially when considering that both the treated and control groups of the second generation received their own sex education in school and benefited from various policies promoting gender equality in areas such as individual taxation, childcare, and employment.

8.5 Results for political outcomes and the critical mass

In the last section, we examine the impact of sex education reform on the political outcomes of both the first and second generations. We utilized data from the European Social Survey for Sweden, which includes information on individuals' region of residence (eight regions), year of birth, and political attitudes spanning from 2001 to 2022. While aggregating the municipality reform treatment at the regional level, we encountered a loss of treatment variability. Consequently, we included individuals from the first generation, born between 1925 and 1947 in Sweden, as well as their children, with the last cohort being from the year 1985. In selecting the outcomes, we focused on

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variables that do not introduce bias due to differences in age across the cohorts. Our analysis involved estimating models using both unconditional and conditional DID group-time estimators (as described in section 6).

The aggregated reform estimates are presented in Table 10. We find that the reform increased trust in politicians by 1.1 units (equivalent to 25% of the pre-mean), enhanced interest in politics by 0.2 units (or a 13% increase of the pre-mean), and boosted the inclination to vote for the left relative to the right government by 0.3 units (a 6% rise of the pre-mean). There are no effects on the preference of the Social Democratic Party specifically. The sex education curriculum never explicitly mentioned the leading party but instead endorsed specific attitudes (active citizenship and the combination of individual and social responsibility) that are better aligned with the preferences of central-left and left governments. Event studies, as detailed in Appendix G, indicate that trust in politicians emerges immediately for the first post-reform cohorts (i.e., the first generation), whereas other outcome responses appear to aggregate across both the first and second generations.

[Table 10 about here]

In the 1940s, advocates of sex education reform introduced it in primary schools with the aim of cultivating individuals with new aptitudes, such as "personal independence and collective cooperation" (A. Myrdal and G. Myrdal 1997 [1934]). Their focus rested on schoolchildren, a demographic sizeable enough to create a critical mass that will alter societal norms in the future. But did this transformation materialize? To address this question, we turn to the DID estimate for left-leaning party preferences, which we can rescale to preference voting shares. In Sweden, between 2002 and 2022, a substantial 69.3 percent of the population voted for left and center-left parties (Statistik Centralbyrån 2023). When we apply the mean value of 4.98 to this estimate, it translates to a modest 4.7 percent impact. This figure suggests that the reform likely did not suffice to support the left-leaning parties' majority. However, political interest and trust in politicians reveal a significantly greater impact—four and two times larger, respectively. Thus, the sex education reform has fostered a transformation in collective civic engagement, thereby supporting democracy.

9   Conclusions

In the 1940s–1950s, during Sweden's pivotal period of welfare state construction based on social-democratic values, the government introduced sex education in primary schools, making Sweden a global pioneer for mandatory sex education. The Swedish sex education reform promoted abstinence and rational decision-making in sexual and family-formation matters but was not limited to these principles. It also served as a pioneering social intervention, emphasizing the notion of responsible citizenship. At today's point, the majority of the pupils who were affected by the reform have completed their working careers, and their children have entered active working age. We assessed the causal effects of sex education for a variety of outcomes of these individuals throughout the life cycle, based on a state-of-the-art DID estimator applied to exceptionally rich microdata. We estimated the reform effects under both unconditional and conditional parallel trends assumption and tested for the potential influence of other social reforms.

As our first major finding, we confirmed that the reform led to a significant reduction in sexual activity and unwanted pregnancies. This was evidenced by the decreased likelihood of abortion and cervical cancer (for women) and decline in marriages of convenience related to out-of-wedlock births (for both men and women), with the effects emerging already by age 18 years and lasting for all reproductive ages. The reform also delayed marriage and childbearing, particularly beyond the age of 25 years, but did not alter the desired number of children born. In light of the mixed evidence for youth outcomes from the evaluations of contemporary abstinence-based sex education policies (Paton, Bullivant, and Soto 2020; Carr and Packham 2017), our findings suggested that previous results may be confounded by the influence of information channels other than the school curriculum. Future research should also look beyond the short term, as the school may permanently affect preferences in sexual activity and family formation (Oettinger 1999).

Second, the sex education reform was successful in fostering responsibility as a personality trait. We found that the reform significantly increased individuals' permanent earnings by 9.3% to 10.9%, suggesting the importance of responsibility as an important economic trait and its malleability

in adolescence (cf. Kautz, Jagelka, and Heckman 2019; Heckman and Mosso 2014). However, our results also suggested that the reform created a new identity of an active and socially responsible individual, as supported by the effects observed for women who were more likely to enter the labor force and for both men and women who became more likely to choose educational fields and professions that involved prosocial behavior. A gender-neutral notion on personal and social responsibility in the study curriculum ultimately decreased gender-based inequality in earnings. Moreover, the reform-induced educational and occupational preferences were transmitted to the next generation's girls. This indicates that identity is formed in childhood and central to an individual's and family's utility and decision-making (Akerlof and Kranton 2000; Kranton 2016).

Overall, our findings supported the notion that norms and behavior can be influenced by school curricula (Oettinger 1999; see also Cantoni et al. 2017). Such effects shape the nation—the life of pupils across the entire life cycle and of their children which relative share is sizable to alter social norms in the society. Although the scope of comprehensive sex education varies across different countries' curricula, most nations include information on sexual activity and its prevention, relationships, as well as discussions on gender, social norms, and life skills. What sets Sweden apart in the introduction of sex education is its early implementation, which provided an opportunity to study the effects of sex education that other countries can anticipate. Further study on the longer-term effects of comprehensive sex education in other settings can make valuable contributions to the ongoing debates regarding sex education in school in the US and other countries in Europe.

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Table 1 – Effects on sexual activity and abuse, ages 34–40 years.

	Abortion (women)	Cervical cancer (women)	Sexually transmitted diseases (both)	Sexual abuse (both)
(a) Unconditional DD	-1.061***	-1.024***	0.014	0.004
	(0.282)	(0.400)	(0.028)	(0.003)
DDD, women minus men			-0.014	-0.009
			(0.040)	(0.011)
(b) Conditional DD	-1.079***	-0.986***	0.016	0.002
	(0.264)	(0.391)	(0.029)	(0.003)
DDD, women minus men			-0.004	-0.013
			(0.041)	(0.014)
Pre-mean	1.358	4.342	0.053	0.017
Individuals	379,494	379,494	776,930	776,930

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 2 – Effects on marriage, ages 18–45 years.

	Marriage of convenience (both)		Married (both)			
	by 18	18-45	by 18	by 25	by 30	by 45
(a) Unconditional DD	-0.172***	-0.384***	-0.012	-2.131***	-1.709***	-0.885***
	(0.063)	(0.142)	(0.134)	(0.465)	(0.395)	(0.265)
DDD, women minus men	-0.271***	-0.053	0.077	0.707	0.476	0.317
	(0.100)	(0.225)	(0.196)	(0.610)	(0.526)	(0.397)
(b) Conditional DD	-0.169***	-0.302**	0.065	-1.811***	-1.225***	-0.631**
	(0.064)	(0.150)	(0.135)	(0.499)	(0.409)	(0.266)
DDD, women minus men	-0.249**	-0.101	0.213	0.168	-0.544	-0.075
	(0.104)	(0.244)	(0.199)	(0.672)	(0.581)	(0.433)
Pre-mean	0.608	3.534	2.366	54.741	77.176	86.904
Individuals	1,026,358	989,655	1,026,358	1,020,490	1,015,396	989,655

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 3 – Effects on parenthood, ages 18–45 years.

	Having a child (both)				Total number of children (both)
	by 18	18-25	25-30	30-45	
(a) Unconditional DD	0.096	-1.436***	-0.420	1.771***	0.022*
	(0.147)	(0.458)	(0.431)	(0.398)	(0.012)
DDD, women minus men	0.087	0.942	-0.652	0.872	0.024
	(0.227)	(0.611)	(0.591)	(0.585)	(0.015)
(b) Conditional DD	0.059	-0.920***	-0.531	1.643***	0.025**
	(0.140)	(0.440)	(0.451)	(0.420)	(0.012)
DDD, women minus men	0.093	0.357	-1.199*	1.361***	0.024
	(0.242)	(0.630)	(0.650)	(0.462)	(0.016)
Pre-mean	3.462	50.188	45.277	43.242	2.008
Individuals	1,026,358	1,020,490	1,015,396	989,655	989,655

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 4 – Effects on completed education.

	Years of schooling (both)	Secondary (both)	Vocational training (both)	College (both)	Social sciences, law, and administration (both)	Natural sciences (both)
(a) Unconditional DD	0.043***	0.900**	1.020***	0.636	0.978***	0.138
	(0.021)	(0.442)	(0.449)	(0.406)	(0.342)	(0.215)
DDD, women minus men	-0.013	0.198	0.078	-0.572	0.486	-0.013
	(0.026)	(0.592)	(0.664)	(0.503)	(0.472)	(0.376)
(b) Conditional DD	0.039**	0.820*	0.818*	0.496	1.092***	-0.110
	(0.018)	(0.476)	(0.455)	(0.333)	(0.319)	(0.209)
DDD, women minus men	-0.028	-0.278	-0.457	-0.843	0.125	0.006
	(0.027)	(0.658)	(0.743)	(0.516)	(0.514)	(0.376)
Pre-mean	7.933	19.209	19.496	8.417	9.954	4.301
Individuals	969,247	969,247	969,247	969,247	969,247	969,247

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 5 – Effects on women's employment.

	Out of labor force			Employed in household production
	ages 30-35	ages 35-45	ages 45-55	ages 35-45
(a) Unconditional DD	-1.436***	-0.572*	-0.685*	-1.483***
	(0.395)	(0.324)	(0.408)	(0.277)
(b) Conditional DD	-1.350***	-0.595	-0.562	-1.117***
	(0.406)	(0.368)	(0.403)	(0.218)
Pre-mean	31.881	18.349	15.903	10.318
Individuals	492,074	490,729	483,308	397,757

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 6 – Effects on sector of employment and occupation, ages 30–50 years.

	Sector of employment (both)					Occupational score (both)
	Manufacturing	Private services	Public services	Teaching, research, and healthcare	Government and administration	
(a) Unconditional DD	-0.652***	-0.439*	0.786***	0.491***	0.459***	0.293***
	(0.318)	(0.243)	(0.320)	(0.236)	(0.164)	(0.083)
DDD, women minus men	1.110**	-0.065	-0.372	0.201	-0.277	0.167
	(0.469)	(0.390)	(0.466)	(0.356)	(0.299)	(0.149)
(b) Conditional DD	-0.353	-0.486*	0.829***	0.487*	0.321*	0.326***
	(0.373)	(0.264)	(0.368)	(0.260)	(0.179)	(0.116)
DDD, women minus men	0.723	-0.187	-0.706	0.421	-0.192	-0.094
	(0.478)	(0.415)	(0.518)	(0.387)	(0.337)	(0.171)
Pre-mean	28.271	16.269	28.879	17.105	14.579	58.145
Individuals	1,010,713	1,010,713	1,010,713	1,010,713	1,010,713	1,006,639

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 7 – Effects on earnings, ages 48–70 years.

	Log earnings, in ages				
	48-50	51-55	56-60	61-70	In total, 48-60
(a) Unconditional DD	0.093***	0.070	0.074	-0.017	0.089***
	(0.045)	(0.050)	(0.053)	(0.064)	(0.043)
DDD, women minus men	0.053	0.083	-0.041	-0.009	0.022
	(0.069)	(0.076)	(0.087)	(0.087)	(0.073)
(b) Conditional DD	0.124***	0.105***	0.092	-0.004	0.116***
	(0.044)	(0.047)	(0.061)	(0.059)	(0.046)
DDD, women minus men	0.044	0.034	-0.082	-0.022	-0.025
	(0.071)	(0.078)	(0.092)	(0.091)	(0.075)
Pre-mean	10.684	10.266	9.002	0.599	9.809
Individuals	981,934	973,637	954,009	928,078	983,658

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

Table 8 – Interaction effects with a social reform, marriage of convenience in ages 18–45.

	Sex education reform on a restricted sample	Childbirth care reform	
		In place	Not in place
(a) Unconditional DD	-0.534***	-0.506***	-0.525*
	(0.172)	(0.203)	(0.310)
(b) Conditional DD	-0.489***	-0.472***	-0.524*
	(0.170)	(0.205)	(0.314)
Pre-mean	3.637	3.374	3.932
Individuals	881,444	607,520	273,924

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the table reports the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

– Effects on the second generation's outcomes, ages 30–35 years.

	Sexual abuse	Married	Has a child	Log parental leave	Years of schooling	Law and admi- nistration	Log earnings	Public services	Occupa- tional score	Runs own business
				(A) GIRLS, mother's side						
(a) Unconditional DD	0.040	-1.152	2.360*	0.107	-0.003	1.100***	0.013	-0.648	0.403	1.485***
	(0.051)	(1.25)	(1.372)	(0.143)	(0.064)	(0.342)	(0.084)	(0.975)	(1.153)	(0.516)
(b) Conditional DD	0.020	-0.599	1.877	0.166	0.012	1.333***	-0.022	-0.891	0.230	1.594***
	(0.030)	(1.275)	(1.190)	(0.200)	(0.069)	(0.385)	(0.110)	(1.096)	(1.214)	(0.546)
Pre-mean	0.060	57.406	20.994	19.017	13.703	39.809	10.471	74.800	56.935	2.903
Individuals	227,396	232,955	232,955	233,482	231,153	231,153	233,482	228,179	125,790	228,179
				(B) BOYS, father's side						
(a) Unconditional DD	0.005	0.174	-0.226	0.028	-0.092	0.214	0.015	0.376	0.302	0.569**
	(0.035)	(0.676)	(0.504)	(0.100)	(0.047)	(0.205)	(0.058)	(0.589)	(0.420)	(0.189)
(b) Conditional DD	0.021	-0.226	0.331	-0.060	-0.045	0.246	-0.026	-0.263	0.278	0.423*
	(0.036)	(1.003)	(0.579)	(0.135)	(0.052)	(0.227)	(0.085)	(0.699)	(0.470)	(0.219)
Pre-mean	0.054	44.942	12.605	10.213	12.793	17.603	10.840	44.988	54.371	5.631
Individuals	239,665	246,218	246,218	246,218	242,649	242,649	246,218	240,279	132,750	237,849

*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the unconditional and conditional parallel trends assumptions, with clustering at the municipality level and merging variables through the mother's or father's identifier. For presentation purposes, we multiplied binary outcomes by 100. Conditional group-time estimates applied the regression-outcome DD estimator of Sant'Anna and Zhao (2020) combined with a group-time estimator with pre-treatment variables for the mother/father as controls (levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, the share of working women in the region or municipality). Inference procedures used bootstrapped standard errors.

Table 10 – Effects on political outcomes, first and second generation.

	Trust in politicians (continuous)	Not interested in politics (continuous)	Voted in the last national election (binary)	Voted for the left party (continuous)
(a) Unconditional DD	1.091**	-0.291***	0.025	0.319**
	(0.534)	(0.125)	(5.156)	(0.158)
(b) Conditional DD	1.135**	-0.205	0.319	0.343***
	(0.524)	(0.395)	(4.713)	(0.129)
Pre-mean	4.363	2.351	90.430	4.975
Individuals	5,982	6,018	6,018	6,018

*** p<0.01, ** p<0.05, * p<0.1

Note: For difference-in-differences (DID), the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. Models used post-stratification weights including design weights. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates applied the regression-outcome DD estimator of Sant'Anna and Zhao (2020) combined with a group-time estimator with the share of working women in the region as a control. Inference procedures used bootstrapped standard errors.
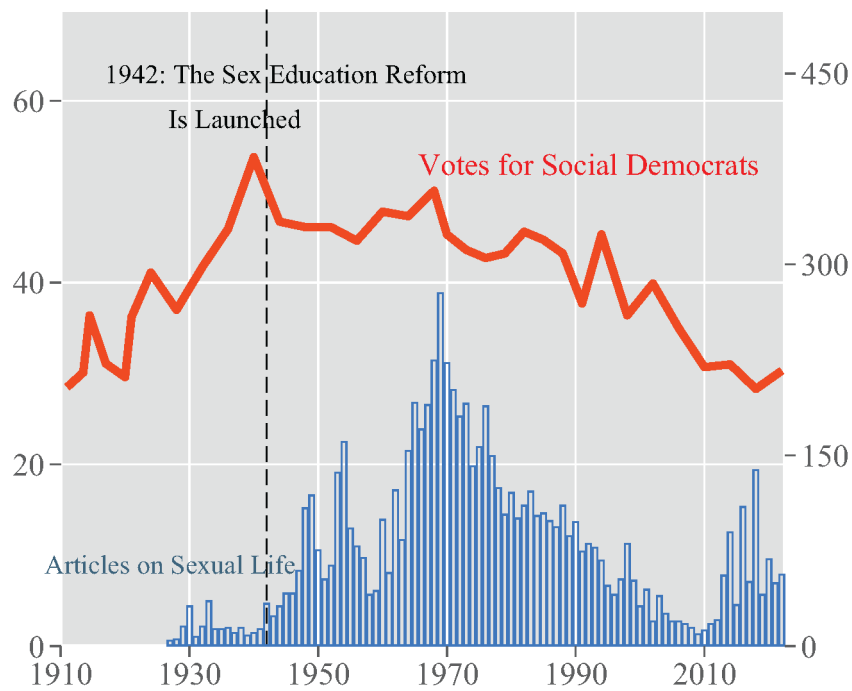
Figure 1 – Social democrats' dominance in the share of votes (left axis, %) and the newspaper articles on sexual life (right axis, totals) during the sex education reform.

Note: The share of votes for social democrats is sourced from Statistics Sweden (2023). The number of newspaper articles on sexual life is derived from data containing all newspapers from 1900 to 2020 in Sweden, as listed in the Royal Library (2023).
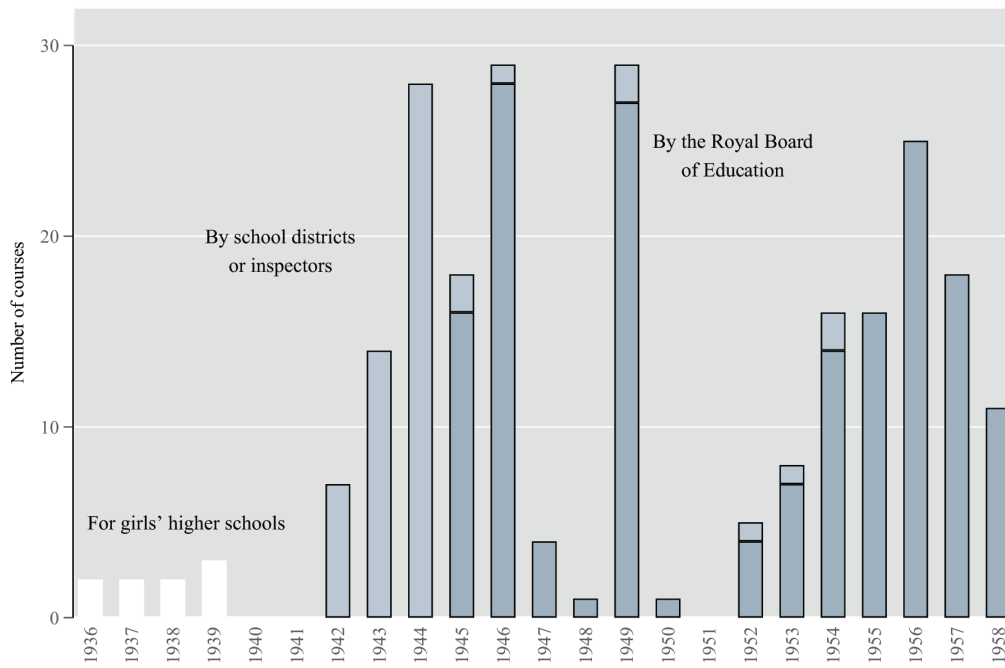
Figure 2 – Number of teacher training courses during the sex education reform, in total per year.

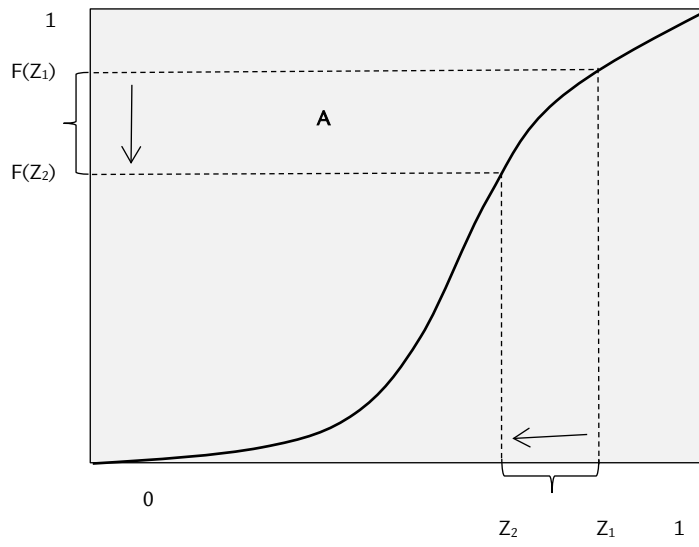*Source*: Data on reform implementation gathered from the national archives.

Figure 3 – Reduction in sexual activity with utility-reducing sex education

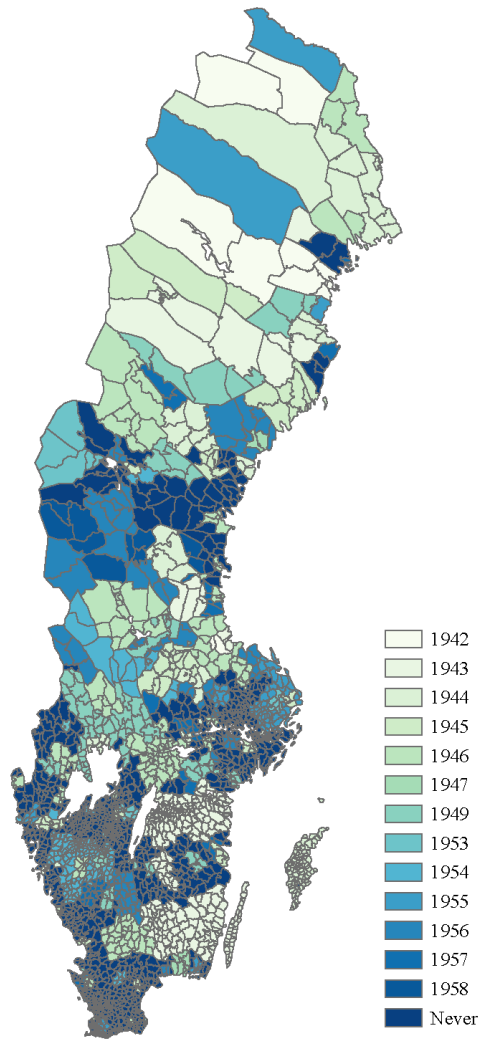*Source*: Based on Oettinger (1999).

Figure 4 – Gradual entry to the treatment under the sex education reform across municipalities in Sweden.

Source: Data on reform implementation gathered from the national archives.

(A) % received sex education in schools
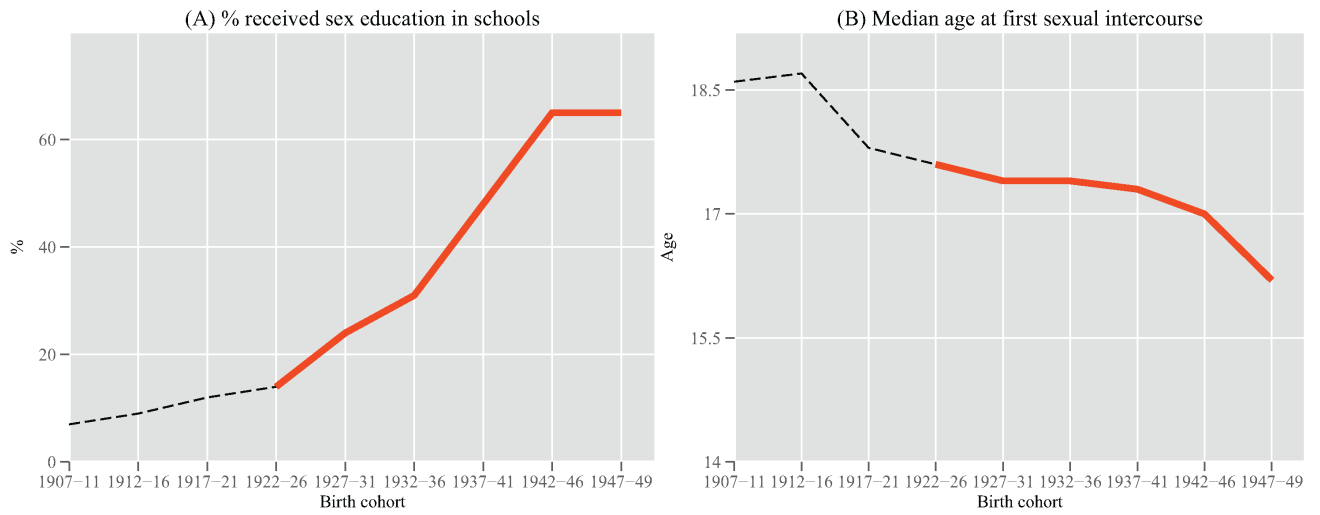
(B) Median age at first sexual intercourse

Figure 5 – "First-stage" outcomes for sex education and sexual activity aggregated by cohort, born in 1907–1949.

Source: Calculations based on Zetterberg (1969).

Figure 6 – Event studies for sexual activity and abuse, ages 34–40 years.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
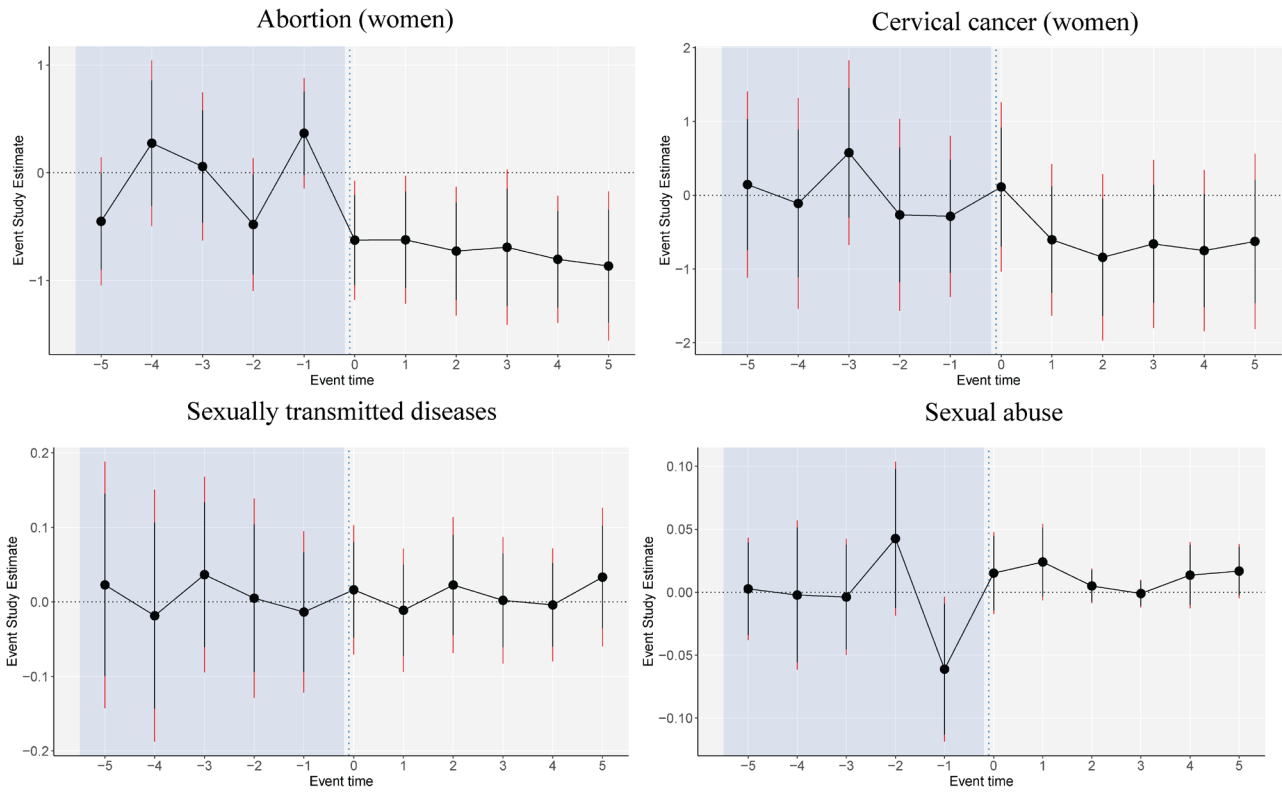
Figure 7 – Event studies for marriage outcomes, ages 18–45 years.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.

Figure 8 – Event studies for parenthood outcomes, ages 18–45 years.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
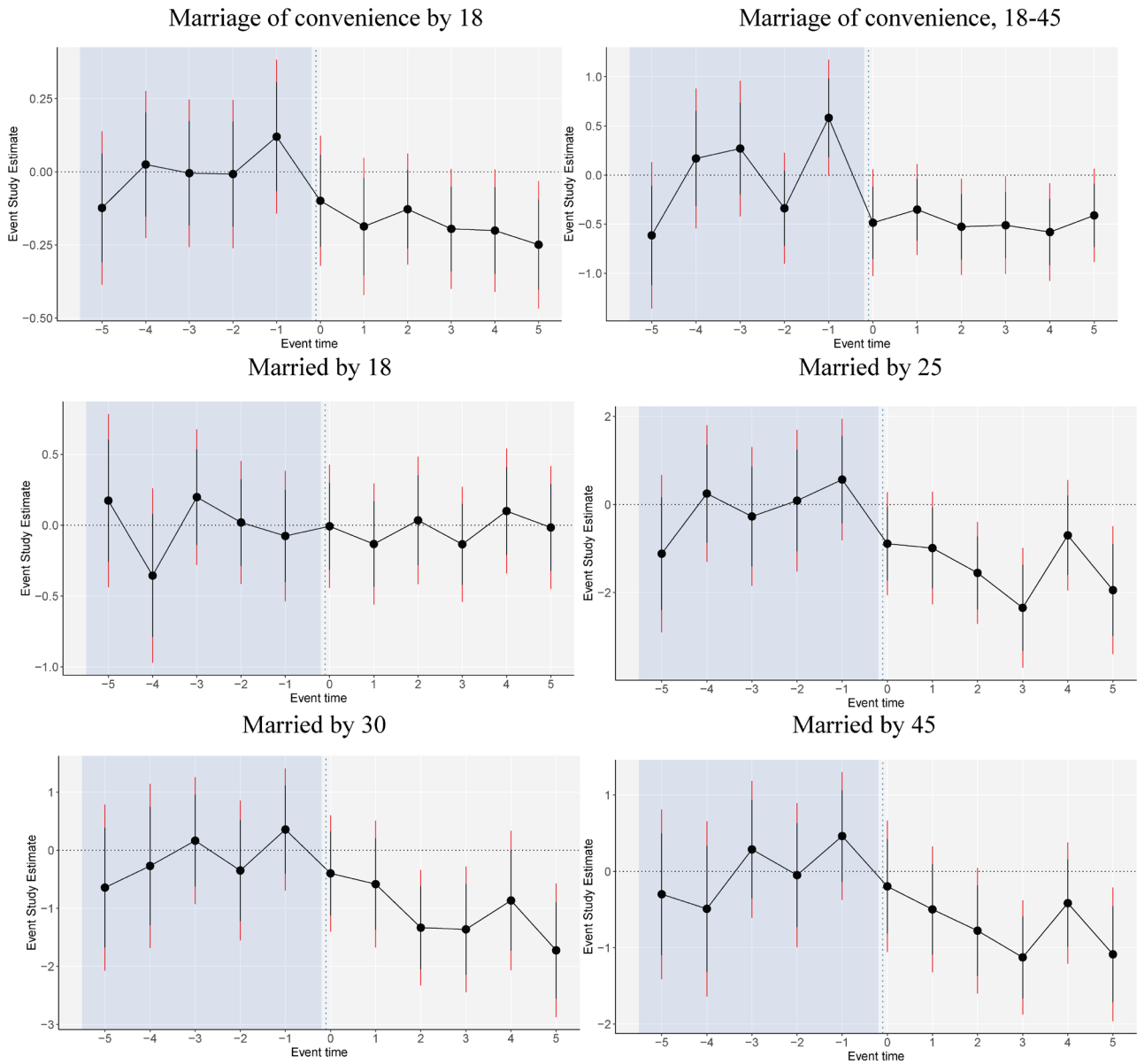
Figure 9 – Event studies for educational outcomes.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
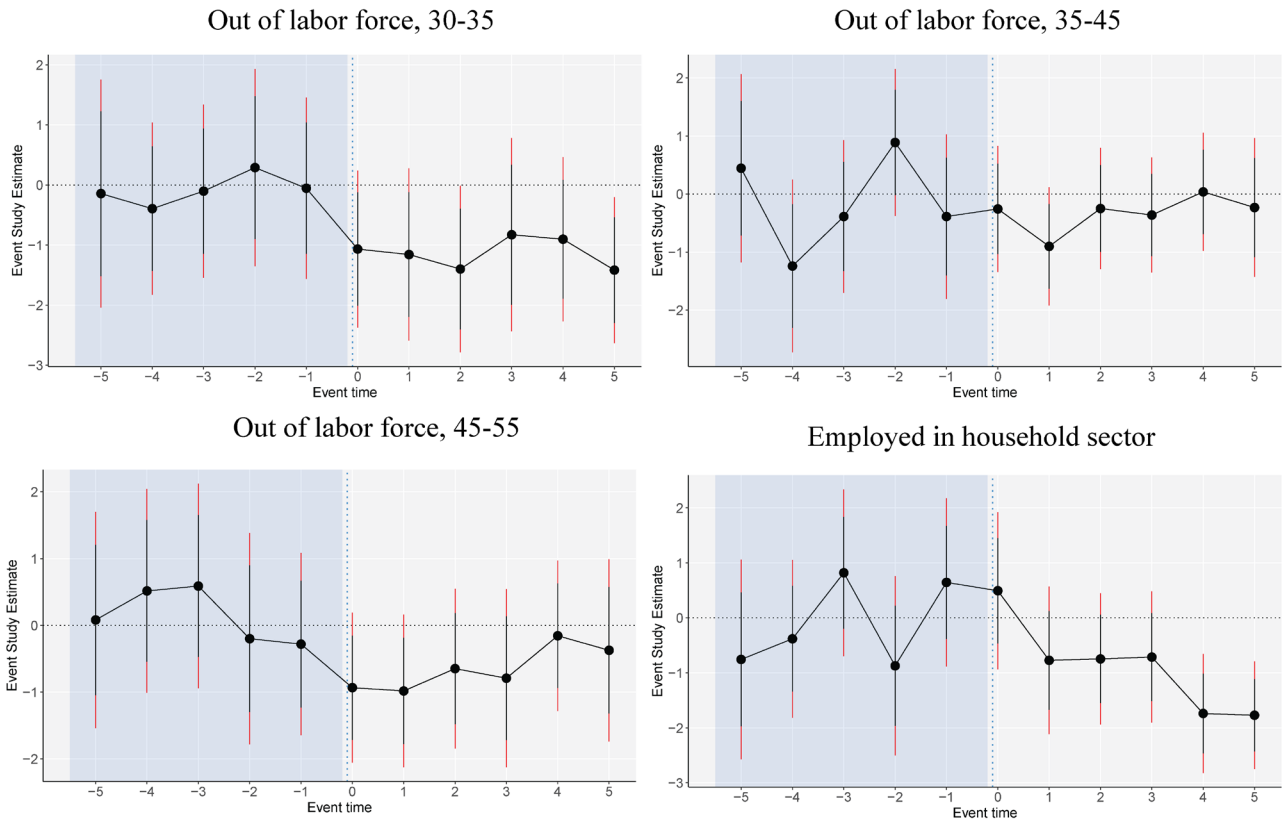
Figure 10 – Event studies for women's employment.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
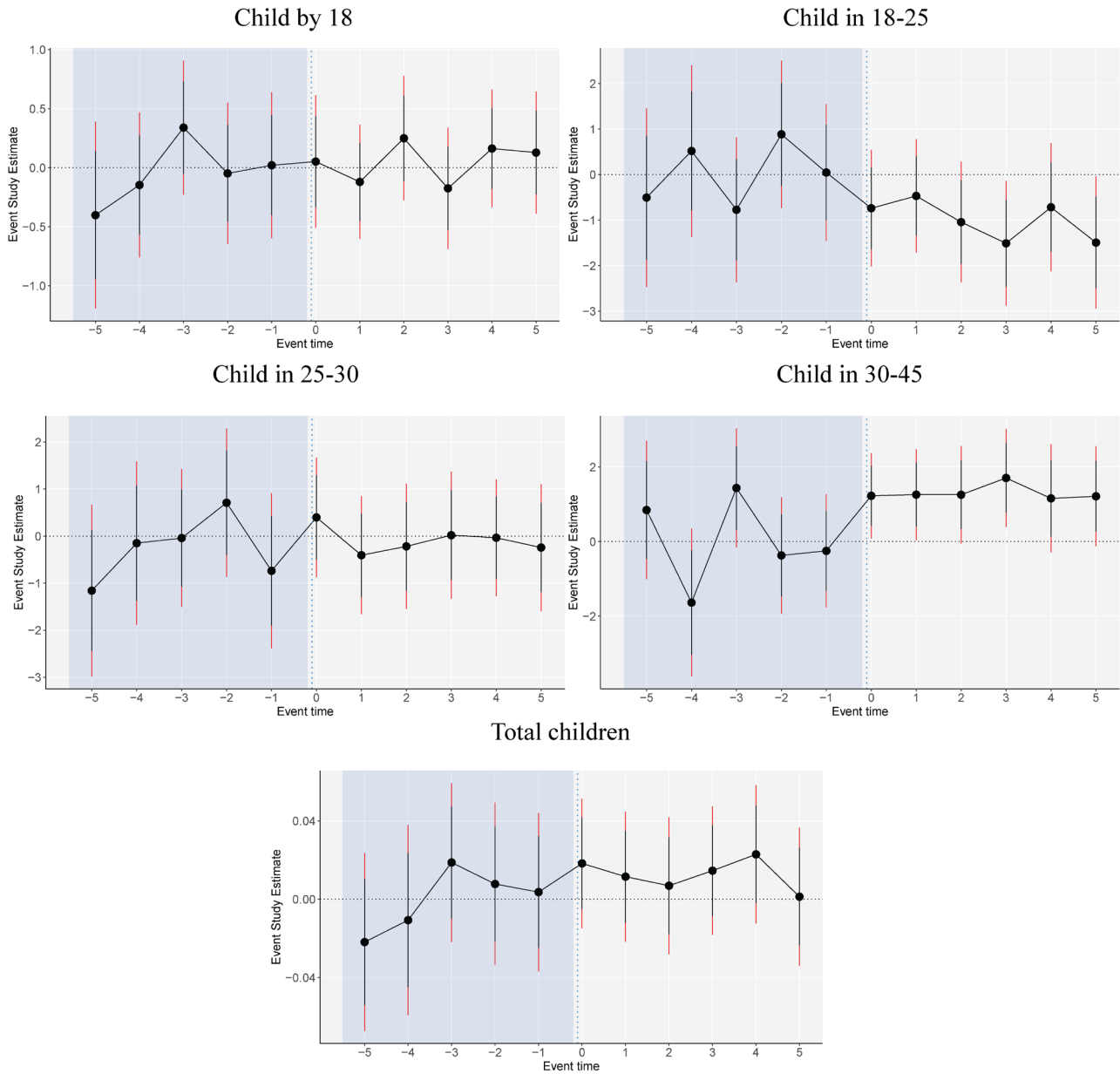
Figure 11 – Event studies for the sector of employment and occupational score.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
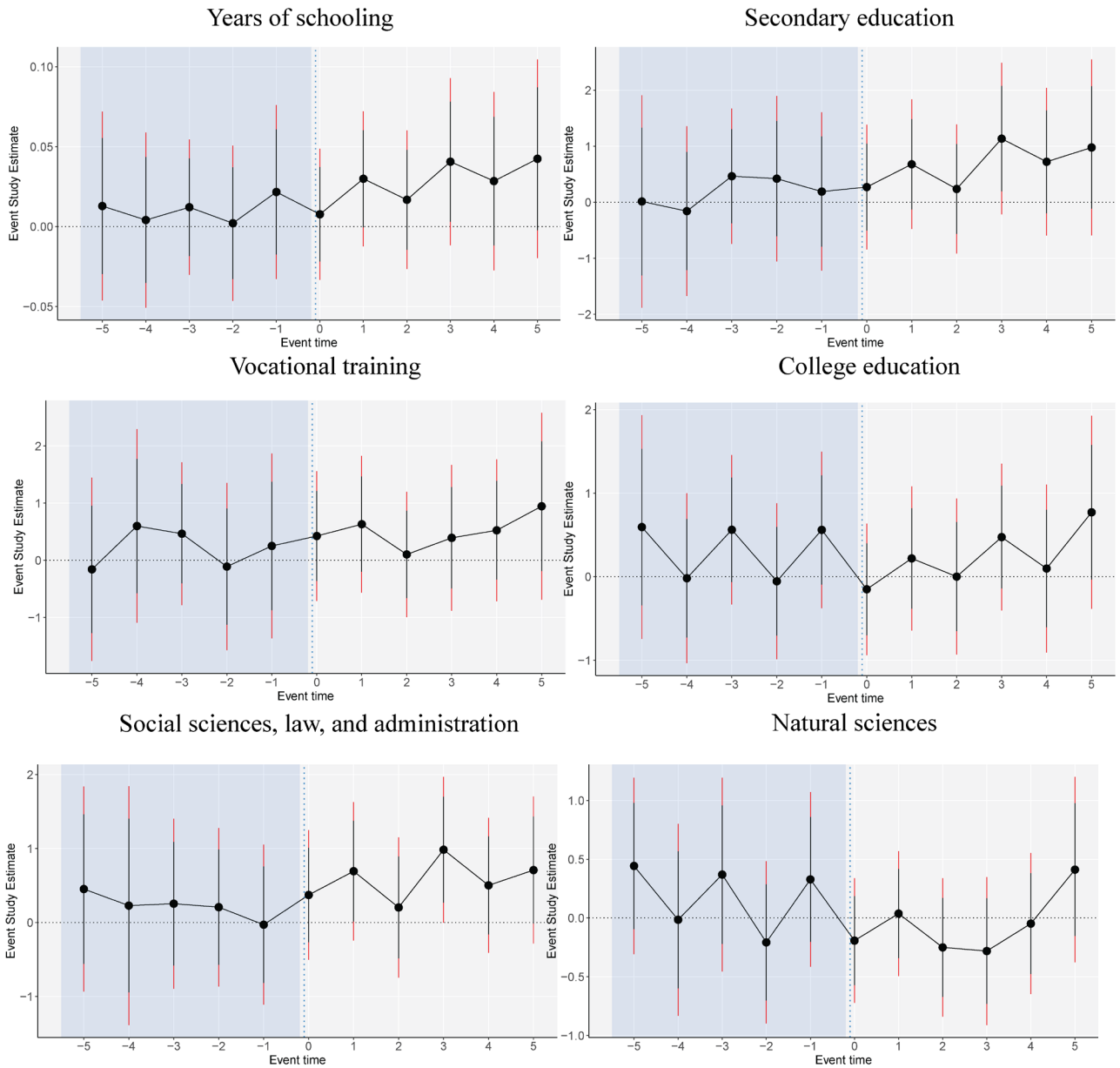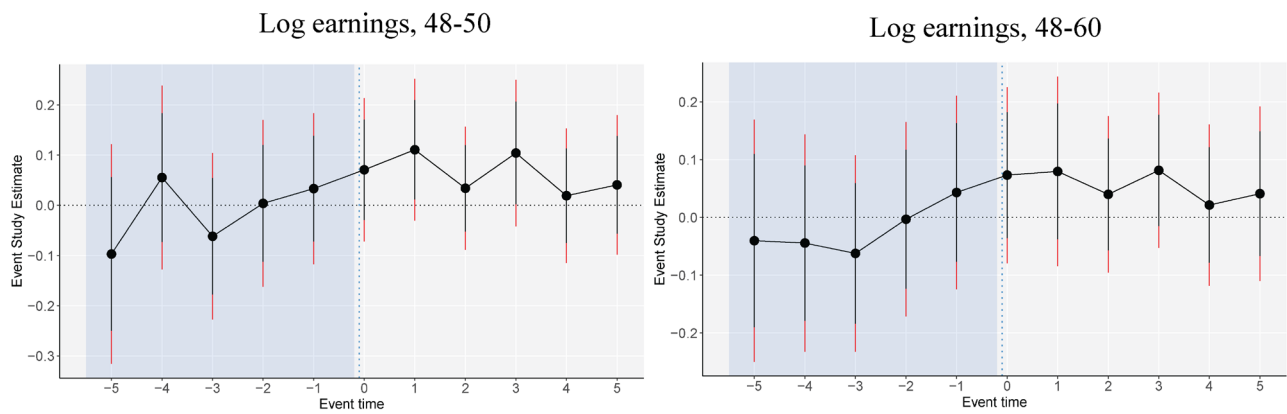
Figure 12 – Event studies for permanent income.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
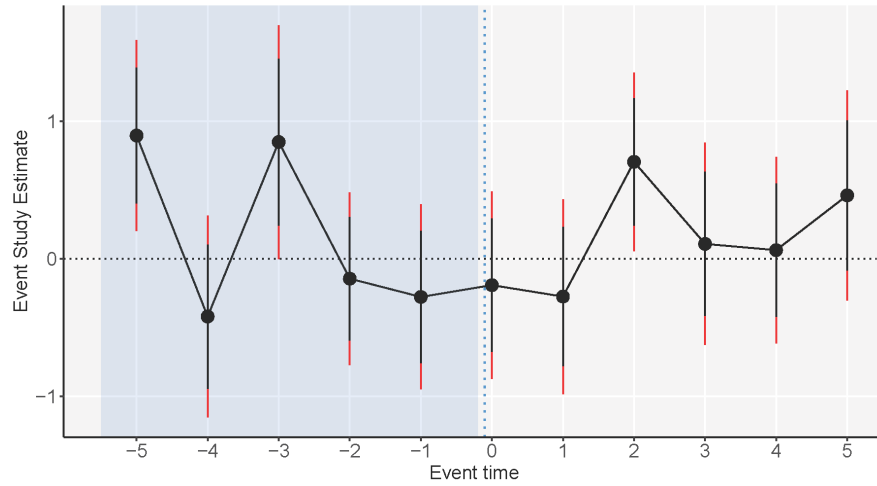
Figure 13 – Event studies for the interaction effects of the sex education reform with the childbirth reform.

Note: The figure reports differences in the ATT(g,t) effects parameters (DDD effects) with versus without the hospital childbirth reform, aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the municipality level. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
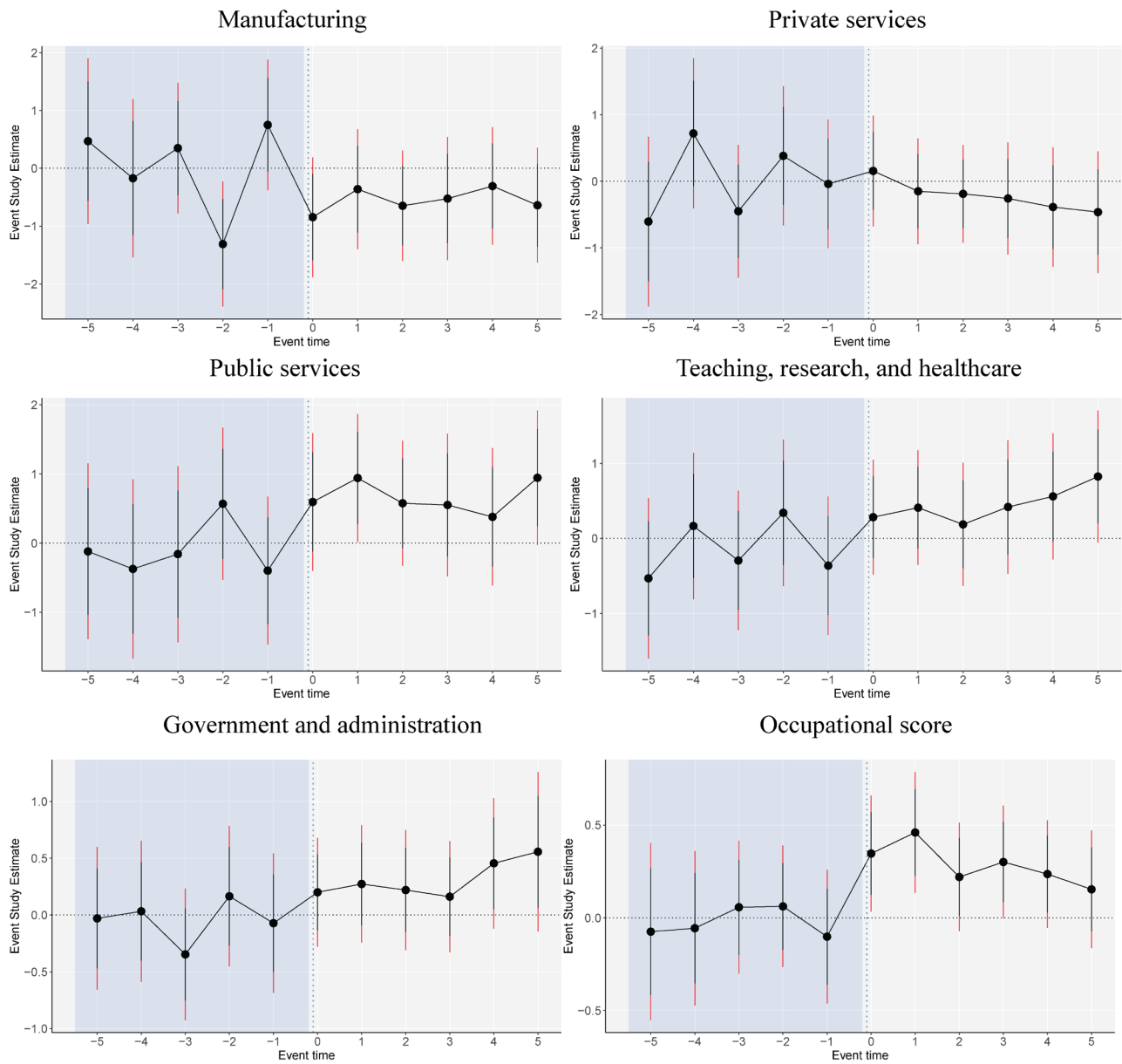
(A) **GIRLS**, mother's side
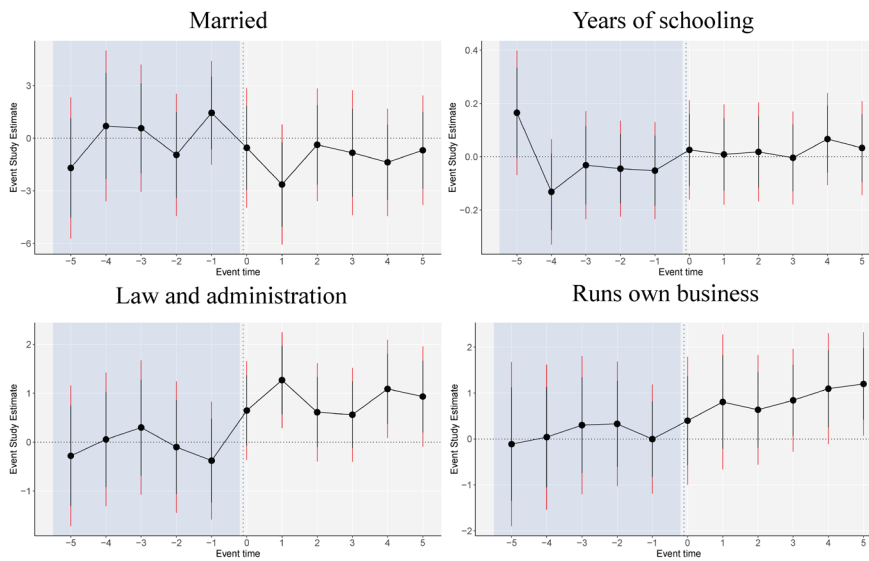
(B) **BOYS**, father's side

Figure 14 – Event studies for the second generation's outcomes.

Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the conditional parallel trends assumption and with clustering at the municipality level. To ease interpretation, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.
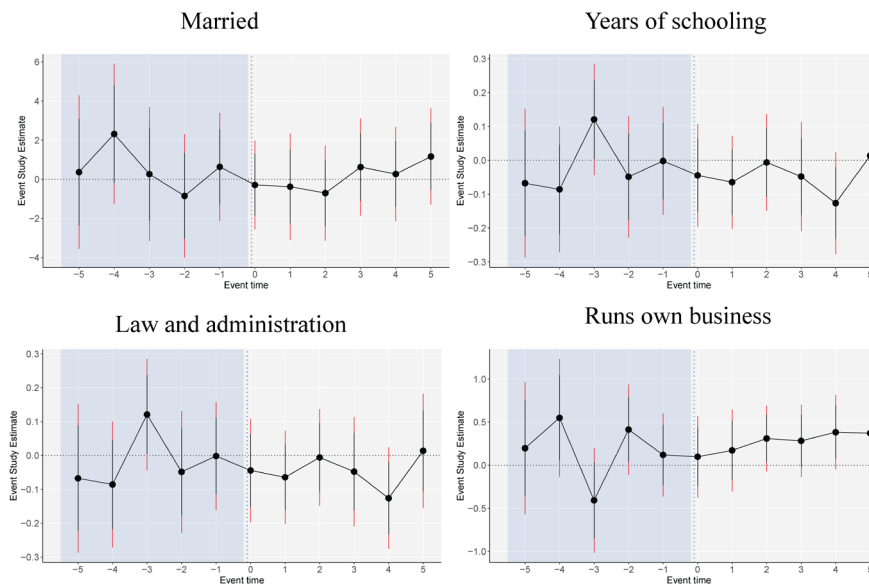
In the case of differential treatment timing in the reform implementation, a presence of both negative weights and heterogeneous treatment effects leads to a bias in the DD estimate, if the estimate is obtained from a two-way fixed-effects regression (Roth et al. 2023; Clement de Chaisemartin and D'Haultfoeuille 2022; Goodman-Bacon 2021; Jakiela 2021). We expect that negative weights appear in our sample: a substantial portion of municipalities are treated in the first years of the reform implementation. Below we provide the results of the tests for a presence of both negative weights and heterogeneous treatment effects, based on the marriage of convenience by age 18 as an outcome. We aggregate the sample by year of birth X municipality cells and weight observations by a number of individuals in each cell.

The two-way fixed-effects OLS estimator provides a sex education reform estimate equal to -0.288 (standard error is 0.083), a highly statistically and economically significant effect. However, we first detect that the treatment effects are heterogenous between combinations of treatment and comparison groups. A Goodman-Bacon (2021)'s decomposition shows that an estimate for the comparison of the ever- to never-treated municipalities amounts to -0.172 [negative weights cannot appear in this group], while it is -0.720 for the comparison of later- to earlier-treated municipalities, a source of negative weights. Therefore, an estimate from a subgroup of municipalities, which create so-called "forbidden" comparison in DD, suggests a much larger and potentially biased reform effect.

Second, following Clément de Chaisemartin and D'Haultfœuille (2020) and Jakiela (2021), we calculate weights as residuals from a regression of treatment on two-way fixed effects and plot them as fractions in Figure. As can be seen, a fraction of municipality-year of birth observations with negative weights is unbalanced between treatment groups. In total, observations for 358,336 individuals who were treated receive a negative weight, 25 percent of the whole sample.

The presence of both such weights and heterogenous treatment effects implies that a two-way fixed-effects OLS estimator provides an DD estimate that differ from the one representing an average treatment effect on the treated. In the case of marriage for convenience, the effect is biased downwards.
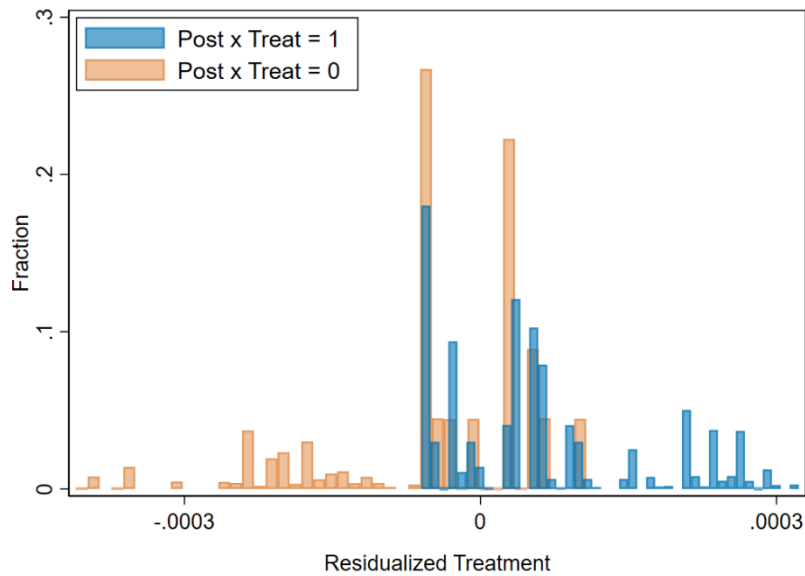
Figure — Two-way fixed-effects weights, by treatment status.

Note: The figure presents a histogram of weights used to calculate the two-way fixed-effects estimates of the impact of sex education on marriage of convenience by age 18. The weights are the residuals from a regression if treatment on municipality and year of birth fixed effects, scaled by the sum of squared residuals across all observations.

Appendix B – Determinants of the sex education reform timing

	(A) Years 1942–1958		(B) Years 1946–1958	
	Ever-treated	**Treatment year**	**Ever-treated**	**Treatment year**
	(1)	(2)	(3)	(4)
Share of unemployed women in 1940	-0.142***	5.138***	-0.004	1.064**
	(0.021)	(0.469)	(0.022)	(0.424)
School investments in 1940	0.177***	-3.719***	0.165***	-2.603***
	(0.048)	(1.100)	(0.052)	(0.979)
Population in 1940	-0.051	-0.456	-0.054	-0.558
	(0.069)	(1.583)	(0.073)	(1.377)
Share of votes for Social Democrats in 1940	0.178***	-3.325**	0.201***	-3.484**
	(0.069)	(1.573)	(0.071)	(1.352)
Sex ratio of 1930's birth cohort	-0.012	0.430	-0.006	0.273
	(0.021)	(0.484)	(0.023)	(0.439)
Constant	0.442***	1,960.860***	0.340***	1,964.052***
	(0.017)	(0.399)	(0.019)	(0.353)
Mean of dep.var.	0.526	1958.515	0.388	1962.884
Observations	2,503	2,503	1,939	1,939
R-squared	0.059	0.069	0.057	0.070

Note: Table reports the estimates of the municipality-level covariates in 1940 (binary variables divided at the median) for treatment initiation, binary ("Ever-treated") and continuous ("Year of treatment", with never-treated set to 1970). Standard errors are in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix C – Sex education reform effects on abortion at different levels of sexual activity

Table – Effects on abortion and cervical cancer in ages 34–40 years at sub-populations of women with different preferences in sexual activity.

(A) ABORTION	Prefer abstinence	Prefer sexual activity but not pregnancy	Prefer sexual activity and pregnancy
(a) **Unconditional parallel trends**	-0.316	-1.251***	-1.042
	(0.375)	(0.327)	(0.751)
(b) **Conditional parallel trends**	-0.321	-1.289***	-1.091
	(0.374)	(0.329)	(0.751)
Pre-mean	1.336	1.433	1.345
(B) CERVICAL CANCER			
(a) **Unconditional parallel trends**	0.845	-1.689***	-0.652
	(0.526)	(0.519)	(1.020)
(b) **Conditional parallel trends**	0.856	-1.824***	-0.758
	(0.533)	(0.489)	(1.013)
Pre-mean	4.136	4.564	4.648
Individuals	82,505	216,862	80,127

*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. Preferences in sexual activity are approximated with the municipality-level under-18 fertility rates: no births (prefer to abstain from sex), low rates (above 0 and below 6 percent of youth: prefer sexual activity but do not want to become pregnant), and high rates (above 6 percent of youth: prefer sexual activity and are not afraid of becoming pregnant). For presentation purposes, binary outcomes were multiplied by 100. Unconditional group-time estimates were obtained following the Callaway and Sant'Anna (2021)'s estimator. Conditional group-time estimates in addition apply the Sant'Anna and Zhao (2020) doubly robust DD estimator with pre-treatment levels of urbanization, healthcare and primary-school expenditures, sex ratio of the birth cohort, and the share of working women in the region or municipality as controls. Inference procedures used bootstrapped standard errors.

Appendix D – Sex education reform effects for pension outcomes of the first generation

In Table, we present the results for the sex education reform's effects on pension outcomes. We find that both sexes the reform has increased both sexes' attachment to the labor market at the retirement ages, as individuals have 0.7 percentage points lower probability to be enrolled in the early retirement (4.6 percent of pre-mean). Our cohorts enter the eligibility age for the early retirement, the age of 60, in 1990, when early retirement schemes were provided for both health and labor market reasons, such the unemployment status of the worker (Hagen 2013); our results show that the early-retirement effect is driven by the decline in share of workers retiring namely due to labor-market reasons. Moreover, we find that the reform has reduced the individuals' probability of receiving a "guarantee" pension, given in case on no or very low income in pre-pension ages. These effects are large and statistically significant for women, with 0.3 percentage points decrease (or 18.7 percent of pre-mean) which aligns with our results for the reform-driven improvements in employment and earnings in women's adulthood.

Table – Effects on pension in ages 60–70 years.

	Early retirement			Retirement due to labor-market reasons			Guarantee pension		
	(both)	(men)	(women)	(both)	(men)	(women)	(both)	(men)	(women)
(a) **Unconditional parallel trends**	-0.722***	-0.855***	-0.602**	-1.462***	-1.888***	-1.015**	-0.197*	-0.127	-0.282**
	(0.209)	(0.304)	(0.276)	(0.392)	(0.576)	(0.507)	(0.109)	(0.176)	(0.141)
(b) **Conditional parallel trends**	0.565**	-0.744**	-0.443	-1.189***	-1.860***	-0.721	-0.183	-0.054	-0.274*
	(0.231)	(0.349)	(0.301)	(0.452)	(0.653)	(0.565)	(0.115)	(0.189)	(0.147)
Pre-mean	15.083	14.212	15.962	12.998	12.145	13.734	1.921	2.263	1.525

*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For presentation purposes, binary outcomes were multiplied by 100. Unconditional group-time estimates were obtained following the Callaway and Sant'Anna (2021)'s estimator. Conditional group-time estimates in addition apply the Sant'Anna and Zhao (2020) doubly robust DD estimator with pre-treatment levels of urbanization, healthcare and primary-school expenditures, sex ratio of the birth cohort, and the share of working women in the region or municipality as controls. Inference procedures used bootstrapped standard errors.

In this Appendix, we perform (1) estimations with alternative control groups and (2) with alternative DD estimators. Our results are robust to the use of an alternative control group and the use of other estimators for heterogeneous treatment effects (i.e., imputation estimator).

(1) First, we perform group-time DID estimations using a group of not-yet-treated municipalities as a control. For difference-in-differences (DID), tables E.1–E.7 below report aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the conditional and unconditional parallel trends assumptions, with clustering at the municipality level. For difference-in-difference-in-differences (DDD), the tables below report the difference between aggregated ATT(g,t) effects parameters (DD) estimated on the sample of women and men. For presentation purposes, we multiplied binary outcomes by 100. Unconditional group-time estimates were obtained following the estimator of Callaway and Sant'Anna (2021). Conditional group-time estimates in addition apply the regression-outcome DD estimator of Sant'Anna and Zhao (2020) with pre-treatment levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, and share of working women in the municipality as controls. Inference procedures used bootstrapped standard errors.

(2) Second, we apply an imputation estimator Borusyak, Jaravel, and Spiess (2021). Table E.8 shows the estimates for women's sexual activity outcomes based on the imputation approach. Conditional estimates use the interactions between the same observable characteristics as in (1) and year-of-birth dummies. Additionally, we present the estimates for the pre-treatment years, as a test for parrelel trends. As in the case of group-time estimator, pre-treatment estimates based on the imputation approach are never statistically significant. Finally, in Table E.8, we show the TWFE estimates for informative purposes—TWFE estimates are severely biased.

Table E.1 – Effects on sexual activity and abuse, ages 34–40 years.

	Abortion (women)	Cervical cancer (women)	Sexually transmitted diseases (both)	Sexual abuse (both)
(a) Unconditional DD	-1.085***	-1.012***	0.004	0.004
	(0.273)	(0.393)	(0.027)	(0.003)
DDD, women minus men			-0.017	-0.010
			(0.040)	(0.011)
(b) Conditional DD	-1.092***	-1.007***	0.008	0.001
	(0.250)	(0.404)	(0.029)	(0.004)
DDD, women minus men			-0.013	-0.013
			(0.041)	(0.014)
Pre-mean	1.358	4.342	0.053	0.017
Individuals	379,494	379,494	776,930	776,930

*** p<0.01, ** p<0.05, * p<0.1

Table E.2 – Effects on marriage, ages 18–45 years.

	Marriage of convenience (both)		Married (both)			
	by 18	18-45	by 18	by 25	by 30	by 45
(a) Unconditional DD	-0.168***	-0.385***	-0.041	-2.022***	-1.634***	-0.839***
	(0.064)	(0.147)	(0.126)	(0.476)	(0.412)	(0.253)
DDD, women minus men	-0.291***	-0.084	0.015	0.583	0.315	0.175
	(0.100)	(0.225)	(0.197)	(0.601)	(0.518)	(0.390)
(b) Conditional DD	-0.165***	-0.307**	0.048	-1.885***	-1.295***	-0.637**
	(0.064)	(0.148)	(0.130)	(0.477)	(0.389)	(0.256)
DDD, women minus men	-0.261**	-0.121	0.174	0.386	-0.331	-0.162
	(0.104)	(0.244)	(0.198)	(0.656)	(0.575)	(0.427)
Pre-mean	0.608	3.534	2.366	54.741	77.176	86.904
Individuals	1,026,358	989,655	1,026,358	1,020,490	1,015,396	989,655

*** p<0.01, ** p<0.05, * p<0.1

Table E.3 – Effects on parenthood, ages 18–45 years.

	Having a child (both)				Total number of children (both)
	by 18	18-25	25-30	30-45	
(a) Unconditional DD	0.045	-1.328***	-0.361	1.707***	0.021*
	(0.144)	(0.488)	(0.392)	(0.414)	(0.012)
DDD, women minus men	-0.030	0.776	-0.662	0.709	0.017
	(0.225)	(0.607)	(0.581)	(0.583)	(0.015)
(b) Conditional DD	0.061	-0.957***	-0.395	1.731***	0.025**
	(0.149)	(0.436)	(0.412)	(0.425)	(0.012)
DDD, women minus men	0.091	0.233	-1.056	1.253**	0.020
	(0.237)	(0.619)	(0.637)	(0.650)	(0.016)
Pre-mean	3.462	50.188	45.277	43.242	2.008
Individuals	1,026,358	1,020,490	1,015,396	989,655	989,655

*** p<0.01, ** p<0.05, * p<0.1

Table E.4 – Effects on completed education.

	Years of schooling (both)	Secondary (both)	Vocational training (both)	College (both)	Social sciences, law, and administration (both)	Natural sciences (both)
(a) Unconditional DD	0.036*	0.739*	0.837**	0.480	0.890***	0.049
	(0.021)	(0.426)	(0.431)	(0.394)	(0.331)	(0.205)
DDD, women minus men	-0.010	0.344	0.085	-0.527	0.432	0.142
	(0.026)	(0.589)	(0.664)	(0.499)	(0.470)	(0.377)
(b) Conditional DD	0.033**	0.664	0.627	0.335	0.905***	-0.136
	(0.017)	(0.442)	(0.461)	(0.332)	(0.297)	(0.207)
DDD, women minus men	-0.029	-0.211	-0.482	-0.826	-0.058	0.357
	(0.027)	(0.655)	(0.737)	(0.506)	(0.505)	(0.391)
Pre-mean	7.933	19.209	19.496	8.417	9.954	4.301
Individuals	969,247	969,247	969,247	969,247	969,247	969,247

*** p<0.01, ** p<0.05, * p<0.1

Table E.5 – Effects on women's employment.

	Out of labor force			Employed in household production
	ages 30-35	ages 35-45	ages 45-55	ages 35-45
(a) Unconditional DD	-1.528***	-0.466***	-0.600***	-1.350***
	(0.387)	(0.227)	(0.211)	(0.256)
(b) Conditional DD	-1.366***	-0.591***	-0.627***	-1.048***
	(0.393)	(0.242)	(0.247)	(0.216)
Pre-mean	31.881	18.349	15.903	10.318
Individuals	492,074	490,729	483,308	397,757

*** p<0.01, ** p<0.05, * p<0.1

Table E.6 – Effects on sector of employment and occupation, ages 30–50 years.

	Sector of employment (both)					Occupational score (both)
	Manufacturing	Private services	Public services	Teaching, research, and healthcare	Government and administration	
(a) Unconditional DD	-0.570*	-0.403*	0.761***	0.479**	0.457***	0.286***
	(0.311)	(0.229)	(0.321)	(0.247)	(0.169)	(0.084)
DDD, women minus men	1.116**	-0.031	-0.399	0.229	-0.272	0.152
	(0.464)	(0.388)	(0.467)	(0.353)	(0.295)	(0.148)
(b) Conditional DD	-0.282	-0.462*	0.723***	0.419	0.327**	0.286***
	(0.334)	(0.244)	(0.362)	(0.268)	(0.152)	(0.099)
DDD, women minus men	0.785	-0.189	-0.773	0.344	-0.199	-0.103
	(0.471)	(0.410)	(0.512)	(0.384)	(0.333)	(0.169)
Pre-mean	28.271	16.269	28.879	17.105	14.579	58.145
Individuals	1,010,713	1,010,713	1,010,713	1,010,713	1,010,713	1,006,639

*** p<0.01, ** p<0.05, * p<0.1

Table E.7 – Effects on earnings, ages 48–70 years.

	Log earnings, in ages				
	48-50	51-55	56-60	61-70	In total, 48-60
(a) Unconditional DD	0.104***	0.081	0.082	-0.024	0.101***
	(0.046)	(0.049)	(0.054)	(0.058)	(0.046)
DDD, women minus men	0.059	0.097	-0.035	-0.008	0.028
	(0.068)	(0.075)	(0.087)	(0.086)	(0.073)
(b) Conditional DD	0.125***	0.105***	0.087	-0.009	0.119***
	(0.046)	(0.046)	(0.060)	(0.060)	(0.046)
DDD, women minus men	0.040	0.052	-0.071	-0.036	-0.019
	(0.070)	(0.078)	(0.092)	(0.091)	(0.075)
Pre-mean	10.684	10.266	9.002	0.599	9.809
Individuals	981,934	973,637	954,009	928,078	983,658

*** p<0.01, ** p<0.05, * p<0.1

Table E.8 – Effects on sexual activity (women) with different estimators, ages 34–40 years.

|	Abortion (women)			Cervical cancer (women)		
	TWFE	Group-time estimator (baseline)	Imputation estimator	TWFE	Group-time estimator (baseline)	Imputation estimator
(a) Unconditional DD	-0.293***	*-1.061****	-1.091***	-0.0721	*-1.024****	-1.048**
	(0.0997)	*(0.282)*	(0.410)	(0.156)	*(0.400)*	(0.405)
(b) Conditional DD	-0.305***	*-1.079****	-1.067***	-0.0360	*-0.986****	-1.043**
	(0.0948)	*(0.264)*	(0.407)	(0.161)	*(0.391)*	(0.405)
(c) Pre-trends						
Event-year (-5)	-0.00181	*-0.449*	-0.00311	0.00146	*0.144*	0.00239
	(0.00192)	*(0.241)*	(0.00228)	(0.00394)	*(0.447)*	(0.00406)
Event-year (-4)	-0.000175	*0.275*	0.000545	-0.00299	*-0.112*	0.000344
	(0.00248)	*(0.282)*	(0.00346)	(0.00333)	*(0.497)*	(0.00368)
Event-year (-3)	-0.0000653	*0.0592*	0.000812	0.000329	*0.576*	0.00361
	(0.00212)	*(0.265)*	(0.00417)	(0.00356)	*(0.470)*	(0.00381)
Event-year (-2)	-0.00326	*-0.478*	-0.000752	0.00246	*-0.265*	0.00601
	(0.00196)	*(0.241)*	(0.00423)	(0.00392)	*(0.454)*	(0.00444)
Event-year (-1)	0.00232	*0.368*	0.0317	0.00137	*-0.285*	0.00133
	(0.00202)	*(0.202)*	(0.00542)	(0.00332)	*(0.429)*	(0.00396)
Pre-mean	1.575	1.358	1.578	5.028	4.342	5.133
Individuals	588,750	379,494	106,656	588,750	379,494	106,656

*** p<0.01, ** p<0.05, * p<0.1

Appendix F – Sex education reform effects for second generation, controlling for parental transmission of income and education

	Sexual abuse	Married	Has a child	Log parental leave	Years of schooling	Law and admi-nistration	Log earnings	Public services	Occupa-tional score	Runs own business
(A) GIRLS, mother's side										
(a) Unconditional DD	0.039	-1.245	0.216	0.008	0.011	1.111***	0.041	-1.286	0.701	1.375***
	(0.040)	(1.300)	(1.537)	(0.167)	(0.065)	(0.345)	(0.085)	(1.058)	(1.127)	(0.580)
(b) Conditional DD	0.095	-1.768	-0.351	0.034	-0.021	1.341***	0.001	-0.544	0.427	1.499***
	(0.070)	(1.321)	(1.338)	(0.198)	(0.069)	(0.415)	(0.087)	(1.120)	(1.221)	(0.560)
Pre-mean	0.060	57.406	20.994	19.017	13.703	39.809	10.471	74.800	56.935	2.903
Individuals	227,396	232,955	232,955	233,482	231,153	231,153	233,482	228,179	125,790	228,179
(B) BOYS, father's side										
(a) Unconditional DD	0.005	0.325	-1.228**	-0.090	-0.098**	0.205	-0.014	0.792	0.805*	0.363
	(0.034)	(0.674)	(0.562)	(0.097)	(0.042)	(0.209)	(0.053)	(0.648)	(0.434)	(0.186)
(b) Conditional DD	0.001	0.258	-1.073	-0.089	-0.069	0.235	-0.003	0.135	0.678	0.251
	(0.035)	(0.705)	(0.645)	(0.113)	(0.043)	(0.234)	(0.059)	(0.657)	(0.489)	(0.210)
Pre-mean	0.054	44.942	12.605	10.213	12.793	17.603	10.840	44.988	54.371	5.631
Individuals	239,665	246,218	246,218	246,218	242,649	242,649	246,218	240,279	132,750	237,849

*** p<0.01, ** p<0.05, * p<0.1

Note: *Note*: the table reports aggregated (average, weighted by group size) ATT(g,t) effects parameters, under the unconditional and conditional parallel trends assumptions, with clustering at the municipality level and merging variables through the mother's or father's identifier. For presentation purposes, binary outcomes were multiplied by 100. Conditional group-time estimates apply the Sant'Anna and Zhao (2020) regression-outcome DD estimator combined with a group-time estimator with pre-treatment variables for the mother (levels of urbanization, primary-school expenditures, sex ratio of the birth cohort, the share of working women in the region or municipality) and the mother's (father's) log earnings and education as controls. Inference procedures used bootstrapped standard errors.

Appendix G – Event studies for political outcomes, first and second generation
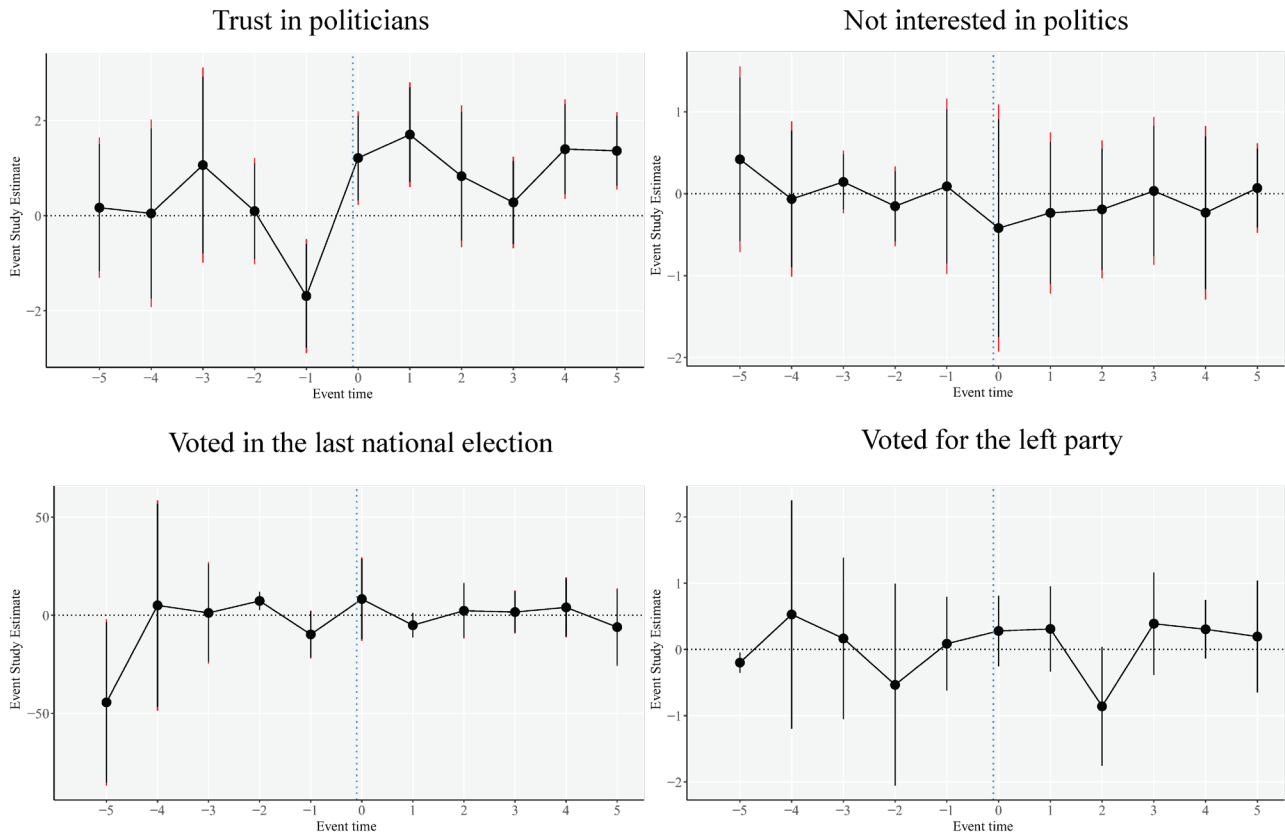
Note: The figure reports ATT(g,t) effects parameters aggregated by event time and weighted by group size, under the unconditional parallel trends assumption and with clustering at the region level. For presentation purposes, we multiplied binary outcomes by 100. Inference procedures used bootstrapped standard errors and confidence intervals adjusted (wider) and not adjusted to multiple hypothesis testing.

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