The Impact of Education on Family Formation: Quasi-Experimental Evidence from the UK[*]

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ABSTRACT

In this paper we examine the fertility and mating market effects of education. We exploit a quasi-experiment generated by a change in UK compulsory schooling laws. This change, introduced in 1972, forced all students to stay in full-time education until at least age 16. The reform was recent enough that access to legal abortion and modern contraception was quite similar to today, granting insight into the fertility effects of education in a modern context. This reform was binding for many girls, inducing around one quarter of the female population of England and Wales in the relevant cohorts to attend an additional year of school. For identification, we leverage the fact that compulsory school requirement was discontinuous with respect to cohort of birth using regression discontinuity methods. We show that the affected girls had significantly lower fertility in their teen years. Instrumental variables estimates imply a 30% reduction in births at ages 16 and 17 caused by the additional year of schooling. The decline was not accompanied by any increase in abortions. We also find that the reform had negligible impacts on completed fertility. Our findings suggest that education-based policies might reduce teen pregnancies without impacting completed fertility rates. On the mating market front, the reform induced both men and women to marry more educated mates and induced women to marry younger mates. The mating effects suggest that educational reforms can have multiplicative effects on household income by changing mate quality.

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1 Introduction

Beginning with Becker (1960), economic analysis of fertility and family formation has been of perpetual interest to researchers. Of particular interest has been the relationship between female education, labor, and fertility decisions. The early literature (e.g. Becker 1960; Mincer 1963; Becker 1965; Willis 1973) was motivated in part by the extremely robust negative correlation between individual-level completed fertility and a woman's educational attainment, with attention to the opportunity cost of childbearing. Most recently, policy interest has focused on two topics related to fertility, education, and labor: First, on fertility trends towards below-replacement levels in high-income countries (e.g. Kalwij 2010); and second, on the levers for moving teen fertility, including and especially education.[1] Teen fertility is an outcome widely considered bad for both teen mothers (Geronimus and Korenman, 1992; Goodman, Kaplan, and Walker, 2004; Chevalier and Viitanen, 2003; Ashcraft and Lang, 2006) and their children (Royer, 2004; Levine and Painter, 2004). The fertility effects are only one of a host of important family formation effects of education, which include effects in the mating market (Behrman, Rosenzweig, and Taubman, 1994).

Despite wide interest in the relationship between education and family formation, evidence of causal effects remains under-developed. While partial correlations between education and various fertility and marital market outcomes raise the possibility that education affects family formation, they do not necessarily imply a causal connection. The omitted variables problems that are pervasive throughout empirical studies are likely prominent here. For example, a female from a disadvantaged family is at higher risk of a teenage pregnancy (Geronimus and Korenman, 1992), as well as at a higher risk of early exit from school.

In light of the strong correlation between education and other characteristics influencing fertility, it is not surprising that the empirical evidence on the education-fertility relationship is inconclusive. Black, Devereux, and Salvanes (2008) uncover large teen fertility effects exploiting compulsory schooling reforms in the US and Norway. But in the long-run, the effects on completed fertility in Norway are much smaller (Monstad, Propper, and Salvanes 2008). In contrast, McCrary and Royer (2011) find no effects when they utilize variation in educational attainment due to school starting rules in the US. Currie and Moretti (2003) estimate significant fertility responses to college openings but have crude measures of fertility.[2]

This literature suffers from several weaknesses. Nearly all of this literature relies on difference-in-difference analyses, which can often be sensitive to the specification of coincident time trends. Other studies of compulsory schooling reforms often involve the estimation of the effects of several

[1] See for instance, the website for the National Campaign to Prevent Teen Pregnancy (http://www.thenationalcampaign.org).

[2] Within the economics literature, empirical work on the fertility effects of education is more complete for developing countries, where recent large national pushes to increase education can be used to identify the fertility effects of education. Examples include Breierova and Duflo (2004), Osili and Long (2008), and Duflo, Dupas, and Kremer (2011). These still tend to focus on teen outcomes, in part because the affected cohorts are too young to have completed their fertility.

educational reforms (like those in Scandinavian countries) as countries often change compulsory schooling requirements along with implementing other educational reforms. In such situations, it is difficult to evaluate the impact of extending the length of schooling in isolation.

In this paper, we estimate the causal effect of education on fertility (timing and quantity) and the mating market. Our analysis can shed valuable new light on this empirical question for several reasons. First, we exploit sharp and plausibly exogenous variation in education. Specifically, we use a 1972 UK education reform that raised the minimum school-leaving age in England and Wales from 15 to 16. The change affected all women born in September 1957 or later. As such, we can rely on regression discontinuity (RD) comparisons of women born very close in time (e.g., September 1957 versus August 1957) who might be otherwise comparable aside from their education. In other quasi-experiments, RD estimators are not often feasible, either because of the resolution of the data or because the implementation details don't permit it (e.g., in the United States, Lleras-Muney 2005).

Second, the reform impacted a large fraction of the population: roughly one quarter of the women born in September 1957 received an additional year of schooling as a result of the reform. The power of this quasi-experiment is observed in Figure 1, which plots month-of-birth averages of the probability of dropping out by certain ages along with regression fits to those averages. The fraction of individuals dropping out by age 15 falls by roughly 30 percentage points as a consequence of the compulsory schooling age increase. The strength of this fall allows us to estimate the effects of this reform very precisely. It also means that our local average treatment effects are estimated for a large and policy-relevant group, somewhat in contrast to settings such as the US, where compulsory schooling laws have been binding for only a few percent of young people (Goldin and Katz, 2003).

Third, the reform was recent and thus the institutional features, such as access to legal abortion and modern contraception, were quite similar to today, allowing us to examine these effects in a modern institutional context. Yet enough time has elapsed since the reform for us to explore the completed fertility effects.

Fourth, in comparison to the prior literature, we can better address the effects of education on conceptions, as opposed to solely on births, by examining impacts on abortions. We combine information on educational attainment from the UK Labor Force Survey with administrative records on the universe of live births and abortions in England and Wales. Ours is the first quasi-experimental study of the fertility effects of schooling that includes abortion outcomes.

Finally, by exploiting a unique feature of a complimentary survey, we can directly identify a subset of compliers affected by the reform. Their self-reported attitudes and partnership histories during their teen years suggest that the mechanisms driving the teen fertility effects may arise in the mating market, rather than as changes in behavior with the same types of partnerships. We investigate this in an RD framework, using Census data on partnerships.

Our analysis reveals four main findings. First, an additional year of schooling had a significant impact on teen fertility, reducing it by around 10% at ages 16 and 17. Second, the additional schooling had at most a negligible effect on teen abortions. This implies that the teen fertility effects reflect a reduction in conceptions. Third, we cannot reject that an additional year of schooling had no impact on post-teen fertility and no impact on completed family size.[3] Fourth, the extra schooling led affected individuals to choose more educated partners, with women marrying men closer to them in age.

We discuss several explanations for why this educational reform could have reduced teen fertility. Crucially, we show that the timing of these teen fertility reductions is consistent with education reducing fertility by reducing the probability that girls conceive while they are in school. We also discuss why this additional education appears not to have changed total fertility. From a theoretical perspective, the most obvious explanation for the zero effect on total (completed) fertility is that the income and substitution effects of the additional earnings enjoyed by more-educated women roughly cancelled out. While Willis (1973) suggests that the substitution effect is more likely to dominate, that analysis ignores the effects of women's education on husband's income, a mechanism discussed by Becker (1981) and documented by Lefgren and McIntyre (2006). With regard to teen fertility, our findings suggest that it was the period of schooling itself, rather than longer-lived human capital effects of education that impacted childbearing behavior.

With the important caveat that we study only one margin of education, the effects of which may be different from those at other margins (e.g., years in college), we conclude that there is no evidence that increased education necessarily leads to lower fertility. Therefore, increasing educational attainment at the population level in the OECD need not imply reductions in future levels of completed fertility. This is a particularly important implication for Western Europe where low fertility is a major concern in light of pension systems and public health systems and where low fertility motivates significant policy intervention (see Kalwij 2010 for an overview). Nevertheless, our study does suggest that schooling can be an effective tool in the fight against teen fertility in developed countries.[4] This is important both because teen fertility reduction is often a difficult policy goal to achieve and because, as discussed below, policy-makers in the UK, US, and other countries are currently considering measures that would compel girls to stay in school for longer. The UK has recently extended the compulsory schooling age to 17 and will increase it to 18 next year. Moreover, extensions of the compulsory schooling age have added benefit of augmenting family resources directly through the direct effect of one's own education but also that of one's mate.

[3] While the teen fertility reductions are large as a percentage of teen births, teen births are such a small part of overall births that complete pass through of the teen effects into completed fertility would fall well within the tight confidence intervals of the completed fertility effects.

[4] Teen fertility reduction is often cited as an important motivation for increasing educational attainment among women in developing countries.

2 Compulsory Schooling Laws and Fertility Trends

2.1 Compulsory Schooling Laws

Compulsory schooling laws in the UK are set at the national level.[5] They include a maximum age by which children must start school and a minimum age at which children can leave school. Officially, children must be age 5 by September 1st of their year of entry; this has been stable since the late 1800's.[6] In contrast, the minimum leaving age has changed four times in the postwar period. These changes, also known as ROSLAs (Raising of the School Leaving Age), were implemented in 1947 and 1972. The ROSLA was further increased in 2013 and is slated to change in 2015.[7]

The 1947 ROSLA increased the minimum leaving age from 14 to 15. Several papers have used this reform to study the effects of education on various outcomes (e.g., Harmon and Walker, 1995; Oreopoulos, 2006; Devereux and Hart, 2010; and Clark and Royer, 2013). The more recent 1972 ROSLA increased the minimum leaving age from 15 to 16. This second ROSLA has received less attention in the literature; Harmon and Walker (1995) and Grenet (2013), Clark and Royer (2013), Machin et al. (2011), and Oreopoulos (2007) use it to study the impact of education on wages, health, crime, and happiness respectively. We focus exclusively on the 1972 ROSLA because fertility data are not available for the women affected by the 1947 ROSLA.

The 1972 ROSLA applied to students in England, Wales and Scotland. We focus on the effects for women born in England and Wales because we do not have fertility data for Scotland. During World War II, the government legislated both the 1947 ROSLA but also recommended a school-leaving age change to 16 when feasible.[8] Later, the Crowther Report and the Newson Report recommended ROSLA to 16 in 1959 and 1963, respectively. Finally in 1964 action was taken to implement the change; the slated date was 1970-1971. By 1968, they postponed the ROSLA for two years. In March 1972, it was officially announced that the school-leaving age would increase by one year. The new school leaving age became effective on 1 September 1972.

Students who turned 15 before September 1972 (born August 1957 or earlier) were subject to the old law; students who turned 15 after this date (born September 1957 or later) were subject to the new law. In the Appendix we discuss in more detail some important aspects of the 1972 ROSLA.

A detail particularly relevant for our study is the treatment of pregnant females within the schooling system. Both before and after the ROSLA, girls who became pregnant during the period of compulsory schooling were required to continue with education through their pregnancy. They

[5] Much of this discussion is adapted from Clark and Royer (2013).

[6] Local Education Authorities have allowed children to enter school before 5, but the frequency of this practice has diminished over time (Del Bono and Galindo-Rueda 2007).

[7] The Education and Skills Act 2008 stipulates that individuals remain in schooling or training until the age of 17 starting in 2013 and until the age of 18 beginning in 2015. See website: http://www.politics.co.uk/reference/education-leaving-age.

[8] These details come from Woodin, McCulloch, and Cowan (2013).

attended school until the later months of their pregnancy, after which they received an in-home, state-provided tutor. Thus a teen pregnancy did not result in a waiver from the compulsory schooling requirement.

2.2 The Impacts of the 1972 ROSLAs

There is a plethora of possible mechanisms behind effects of the 1972 ROSLA on family formation. We highlight a few here. First, an increase in a woman's education could impact tastes for childbearing directly. Second, education could alter the labor market opportunities—through occupation or through wages—which could change the demand for children and the desired timing of childbearing. Third, education could influence the rate of time preference and thereby change the timing of childbearing. Fourth, the extra schooling could impact health and thereby affect fertility through both intensive and extensive margin. Fifth, it could influence the choice of mate, either through the mechanisms described above or directly through tastes.

Prior research, albeit limited, gives us some insights as to which of these channels may be operating as a consequence of the ROSLA.[9] The instrumental variable estimate of the return to an extra year of schooling for the ROSLA-impacted cohorts is 6-7 percent for both men and women (Grenet 2013). These cohorts also are less likely to receive welfare and are happier (Oreopoulos 2007). Health impacts of the additional education were minimal (Clark and Royer 2013).

3 Data

Our study uses several datasets. We describe each of these in turn. The common variables linking our datasets are the month and year of birth of women whose education, fertility, and partnership outcomes we examine. Our analysis is at the level of month-of-birth cohorts, as we describe later. A summary of the data sources and the relevant variables we extract from each is provided in Appendix Table 1.

UK Labor Force Survey. Information on educational attainment comes from the 1975-2002 UK Labor Force Survey (LFS). The LFS samples a large number of households to provide information on labor force participation, employment, and training. Around 60,000 households are interviewed in each wave. Between 1975 and 1983 the LFS was conducted biannually. Between 1983 and 1992 the LFS was an annual survey of a similar number of households. Since 1992, the survey has been a rotating quarterly panel, with the same households interviewed for five consecutive quarters. In the analysis, we include multiple observations per person when available, and cluster all results at the cohort level. Respondents' year and month of birth are available in public release LFS datasets up to 2002.

Since its existence, LFS respondents have been asked about the age at which they first left full-time continuous education. Respondents were also asked about the highest academic qualifications they held, though the exact form of these questions differs across LFS survey years. Our pooled sample

[9] Most of the literature using the UK compulsory schooling laws focuses on the 1947 ROSLA not the 1972 ROSLA.

of 1975-2002 LFS gives us a large sample of over 300,000 respondents born between 1954 and 1962.

Live Birth Records. Live birth data were supplied by the UK Office for National Statistics (ONS). The records cover every birth between 1970 and 2008 in England and Wales to mothers born in England, Wales, or Scotland – roughly 600,000 to 900,000 births per year.[10] Since teen fertility is a relatively rare outcome, it is crucial to have a census of births. The births data are birth counts by mother's month and year of birth and child's month and year of birth but do not include parity information. Given that we rely on across-cohort variation in education, except under special circumstances, it is not problematic that we do not have individual-level birth data. Specifically, our regression framework, described later, does not require the inclusion of covariates aside from controls for cohort trends. Any pre-determined covariates, in principle, should be similar for females born immediately prior to September 1957 and females born immediately following September 1957 because sorting around this threshold would be impossible to do since the exact timing of the ROSLA change was announced in 1972.

The structure of our data implies that we can recover cohort-specific fertility rates but not individual-level fertility rates. For instance, our data cannot distinguish between a scenario in which one woman bears her first child at age 20 and another bears her first child at age 24 and a scenario in which one woman bears her first child at age 20 and then her second at age 24. In practice, since we find that the reform only impacts fertility among girls aged 16 and 17, who are very likely to have been first-time mothers, the missing parity information is of little consequence. Moreover, assuming that education affects fertility behavior mainly in one direction (i.e., either increasing it or decreasing it), the inability to distinguish parity will not cause us to mischaracterize the results.[11]

For each cohort of women, we construct age-specific and total fertility rates according to standard practice (Preston et al. 2001). The numerator in the age-specific rate for birth cohort c is the number of births to women of cohort cover a particular age range (e.g., [16, 17)). The denominator is the number of women alive in that age range. Unfortunately, it is not possible to know exactly how many women are alive at each point in time. Thus, we generate the denominator by multiplying the size of the female cohort at birth (obtained from the Registrar's Annual Reports) with a succession of age- and calendar year-specific survivor rates for the UK (obtained from the Human Mortality Database). We validate these denominator estimates by comparing our predictions for each cohort size in 1991 and 2001 with cohort size as measured by the 1991 and 2001 Censuses. Our estimates and these numbers line up very closely. This correlation ranges between 0.93 and 0.95. The total fertility rate (TFR) aggregates the age specific rates. Cohort TFR reflects the average number of

[10] The birth records from England and Wales do not distinguish between mothers born in England and Wales and mothers born in Scotland. This poses minimal problems for our study, since only a small fraction of the women living in England and Wales during this time were emigrants from Scotland (<1%).

[11] For example, counter to this assumption, suppose that the reform influences one woman to increase her fertility preferences from 2 to 3 children and influences another to reduce her preferences from 1 child to being childless. In this case, the total number of children born is unchanged but this overall effect masks important heterogeneity.

children that women in the cohort bear over their lifetime, conditional on surviving through their fecund years.

Abortion Records. Detailed abortion records come from the UK Office of National Statistics. Like the live birth records, these data are counts by women's month and year of birth. Unlike the live birth records, these are limited to a narrower range of cohorts born around the reform threshold. They also cover only abortions performed in the teen years (the only years for which we find any fertility effect). These data allow us to construct teen (16-19) abortion rates for cohorts of women born within a 36-month window of the ROSLA threshold.

UK Census Extracts. Finally, we commissioned several extracts of the 2001 Census that are useful for understanding the mating market effects of the extra education. These extracts allow us to track the age difference between partners as a function of each partner's month of birth and the educational credentials of each spouse in relation to their own month of birth.

National Child Development Study. Finally, we use data from the National Child Development Study (NCDS), a panel that attempted to track 100% of babies born in Great Britain in a particular week of March 1958. This cohort was one of the first to be subject to the 1972 ROSLA. Information collected at ages 7, 11, 16, 23, 33, and 41 offer insight into cohort members' school activities, partnerships, and labor force participation during the extra year of compulsory schooling and beyond. Interestingly and arguably unique to this study is our ability to identify compliers and always takers in these data. When participants were 16, the study asked both the cohort members and their parents when they would have dropped out of school had the 1972 ROSLA not been in place.

4 Empirical Strategy: Regression Discontinuity Framework

Our empirical strategy leverages the difference in the school exit age resulting from the 1972 ROSLA. Specifically, we use a regression discontinuity framework to compare individuals born immediately on either side of the September 1st 1957 cutoff, who we argue are similar except for their relevant ROSLA. Our analysis proceeds in two steps: first, documenting that the 1972 ROSLA indeed did change educational attainment as intended, and second, examining the subsequent effects the ROSLA had on fertility and family formation.

Educational Impacts of the 1972 ROSLA

To estimate the first-stage educational impacts of the 1972 ROSLA, we follow the fuzzy regression discontinuity setup (Lee & Lemieux, 2010; Imbens & Lemieux, 2008). That is, we estimate local linear regressions equations of the following form using data near the September 1957 threshold:

$$E_c = \gamma_0 + \gamma_1 D_c + \gamma_2 R_c + \gamma_3 D_c R_c + u_c \quad (1)$$

where E_c is an average educational attainment measurement for month-of-birth cohort c (e.g., the fraction leaving school at age 15 and the average age at school exit), D_c is an indicator for whether the cohort is affected by the 1972 ROSLA (i.e., equals 1 for cohorts born September 1957 and later and 0 otherwise), R_c is the running or forcing variable (i.e., calendar month of birth), and u_c is the error term.[12] While the Labor Force Survey data on education would allow us to estimate this regression using the individual-level micro-data, all of our outcomes (births, abortions, education and age of mate) are measured at the month by year-of-birth level, so we aggregate our data to the month by year-of-birth level. We weight these regressions by cohort size calculated as described in the data section. The inclusion of R_c captures cohort trends, and the inclusion of D_cR_c allows the cohort trends to differ on either side of the September 1957 although in principle, we have no a priori reason why they should, other than that these cohorts experienced their later teen years during a time of rapidly changing fertility trends throughout the OECD.

Our primary interest is in estimates of γ_1, the effect of the ROSLA on educational attainment. This parameter can be thought of as the "jump" in the dependent variable at the program threshold. This is the sense in which our research design exploits the comparison of cohorts "just unaffected" and "just affected" by the reform.

Fertility and Mating Market Impacts of the 1972 ROSLA

Similarly to evaluate the effects of the ROSLA on our main outcomes of interest—fertility and assortative mating outcomes—we estimate regressions of a form very similar to that of equation (1) except that our dependent variable has changed. That is,

$$Y_c = \beta_0 + \beta_1 D_c + \beta_2 R_c + \beta_3 D_c R_c + v_c \qquad (2)$$

where Y_c is an outcome of interest, β_1 in the regression tables can be directly interpreted as estimates of the ROSLA impact. Note that in none of the employed datasets do we have information on both educational attainment and a fertility or mating market outcome. Thus, to derive the return to an extra year of schooling on any of our outcomes (e.g., fertility at a certain age, educational attainment of mate), we can estimate β_1/γ_1, the standard Wald IV estimate using a two-sample instrumental variables strategy (Angrist and Krueger, 1992).

Choice of a Bandwidth

To estimate these equations, we use data close to the September 1957 threshold. We must decide on how close to the threshold we should be (i.e., bandwidth). To date, there is no universally accepted bandwidth selection procedure. We follow the cross-validation procedure of Ludwig and Miller (2007).[13] The method minimizes the square of the prediction errors from the local linear model.

[12] Note it is not immediately apparent that educational attainment can be immediately derived from age at school exit. However, for most, educational attainment can be derived from age at school exit with little error because retention rates are low (<5%). See http://eacea.ec.europa.eu/education/eurydice/documents/thematic_reports/126EN_HI.pdf
[13] Other bandwidth selector procedures are Imbens and Kalyanaraman (2012) and Calonico et al. (2014).

Because we estimate a variety of effects (births at 16, 17, 18, 19, etc.), for each collection of outcomes (e.g., education or births), we choose one bandwidth for each similar set of outcomes. For example, when looking at teenage fertility, we find the strongest visual evidence of effects at age 17, and choose the bandwidth that performs best in the cross-validation statistic for the age 17 outcome, and then use it for all of the other teenage fertility outcomes. For robustness, we estimate the regression discontinuity models with both narrower and wider bandwidths.

One other common approach in empirical applications of the regression discontinuity design is the global polynomial approach. That is, instead of focusing narrowly around the threshold, this approach uses more flexible functions of the running variable (i.e., higher order terms of R). We show below that cohort patterns in our data are highly irregular away from the threshold, and global polynomials do a poor job of fitting the data near the threshold.

Identifying Assumptions

A key advantage of the regression discontinuity approach is the minimal set of assumptions needed for identification. We must assume that individuals born immediately prior to and immediately following September 1957 are otherwise identical except for their educational attainment. Ex ante, this assumption seems plausible as the policy change was not pre-announced. Moreover, it is testable, at least in terms of observable characteristics. That is, for each observable characteristic, we can test whether the characteristic has a discontinuity at the September 1957 threshold. Clark and Royer (2013) examine number of births and stillbirth rates and find no evidence of discontinuities in these outcomes.[14]

An identifying assumption under which our IV estimates are valid measures of the effect of a year of additional schooling at age 15 is that the 1972 ROSLA does not affect fertility through channels other than its effect on the probability of experiencing a year of full-time education at age 15. This exclusion restriction would be violated, for instance, if the law was accompanied with a change in the accommodations for pregnant students.

5 Education Results

In this section we report our main results. We begin by reporting estimates of the impact of the ROSLA on educational attainment. We then report estimates of the impact of the ROSLA on fertility and mating market behavior.

5.1 Education

To establish that the ROSLA impacted educational attainment, Figure 1 shows the impact of the 1972 ROSLA on age at school exit for women. Specifically, each point in this figure represents the

[14] There is a little set of pre-determined characteristics to examine since education could affect many of them. Moreover, we view behaviors as adults and very few datasets that sample adults have parental information.

average outcome for a cohort of women who share the same month and year of birth. The three sets of points trace the fraction of women who left school by age 15, by age 16, and by age 17. The lines are local linear regression fits to those underlying points.

The compulsory schooling law was binding for a significant share of individuals. Almost one third of women born before September 1957 left school at age 15. The reform had an immediate impact on age at school exit as there is a clear drop in the fraction leaving before age 16 (i.e., before grade 10 in a US context) starting with the September 1957 cohort. This gap is on the order of 25 percentage points. Effects for men (not pictured) are similar. However, there are minimal impacts observed for leaving school by age 16 and 17.

While Figure 1 suggests that most students complied with the new law, there is a non-negligible share of individuals who report not complying (<8% for most cohorts). The most apparent non-compliance is a seasonal pattern of among cohorts born in June, July, and especially August. This is because school was not in session when these women turned 16. Hence, they report leaving school at 15, even though they are likely to have completed grade 10, in compliance with the law, and left school in practice at age 15.7 or later. For non-summer borns, it appears that much of the reported non-compliance is measurement error. Because the LFS is a panel, respondents are asked to recall their school leaving age in multiple waves, and over 10% of respondents who report leaving school at age 15 also report leaving school at 16 in a different survey wave. Further, in the NCDS, which interviewed members of one of the first post-reform cohorts when the respondents were 16 years old—therefore eliciting responses much closer in time to the school-leaving event—less than 1% reported leaving school before age 16.

In Table 1, we quantify the patterns observed in Figure 1. In Panel A, we report discontinuity estimates of γ_1 for different educational outcomes from estimation of equation (1). The estimation sample includes 48 months of birth cohort data on either side of the ROSLA threshold (i.e., 96 months in total). The reported mean of the dependent variable is for the August 1957 birth cohort. The ROSLA led to a 27 percentage point reduction in the fraction of individuals leaving schooling by age 15. This is a considerable change given that the fraction of the last pre-ROSLA cohort leaving at age 15 was 0.44.

As evident in Figure 1, the ROSLA did not induce large fractions of women to stay in school beyond the extra year. Specifically, the effects on leaving at age 16 and 17 are -0.02 and -0.01. Overall the effect on age at left full-time education is 0.31 of a year, which is highly statistically significant with a t-stat of over 5. This discontinuity is noticeable in Figure 2, which plots the average age at school exit by birth cohort. These first-stage estimates are essentially identical to those of Clark and Royer (2013).[15]

[15] In fact, all of the point estimates are the same except for that for left school by age 16, which is -0.037. Clark and Royer (2013) use different data (the General Household Survey and the Health Survey of England) and a different bandwidth.

While these regressions verify the existence of a first-stage relationship, several other points from Panel A are worthy of discussion. First, the size of the impacted population is considerable. Over a quarter of the post-reform cohort had its school leaving age impacted by the 1972 ROSLA. As such, the estimated local average treatment effects (LATE) may be closer to the average treatment effect (ATE) in this context (Oreopoulos 2006). For comparison, compulsory schooling changes only impacted 5 percent of population in the US (Lleras-Muney, 2005) and only around 12 percent in Norway (Black et al. 2008). Second, the set of compliers for whom we will be identifying the effect of a year of education is a policy-relevant subpopulation. Third, we do not have to worry about weak instruments given the strength of the first stage. Such worries have hindered analysis of compulsory schooling in other countries (see Black et al (2008) for a discussion of this).[16] Also, somewhat unique to this context, the strength and robustness of the first stage allows us to use regression discontinuity methods rather than difference-in-difference methods; the regression discontinuity design may be preferable given the relatively weak assumptions needed for identification.

As noted above, one of the regularities in Figure 1 is the seasonal pattern in the probability of leaving school, particularly by age 15. To address this, in panel B, we re-estimate regression equation (1) and include a full set of calendar month-of-birth dummies excluding the September month-of-birth dummy.[17] Given the consistency of this pattern across cohorts, these calendar month-of-birth dummies may adequately control for these regularities. Also, for further robustness, instead of calendar month-of-birth dummies, we exclude the summer borns (i.e., those born in June, July, and August) in estimation of equation (1) in Panel C. The results are similar to those from Panel A assuring us the strength of this research design. We have also tested for sensitivity of the education effects to varying the bandwidth (see Appendix Table 2). These results are consistent with those in Table 1.

6. Fertility-Related Results

6.1 Teenage Fertility

We start with examining the effects of the ROSLA on teen fertility.[18] Figure 3 presents plots of age-specific fertility rates in a format analogous to that in Figure 2. These fertility rates are calculated according to age-last-birthday. Thus the age 16 rate captures births in the exact age range [16,17), and the age 17 rate captures births in the exact age range [17,18). The magnitude and significance of these ROSLA effects is explored in Table 2, which presents reduced-form estimates on teen fertility from ages 15 to 19.[19,20]

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16 Black et al (2008) show that the addition of state-specific trends along with the appropriate calculation of standard errors to difference-in-difference regression models estimating the effect of compulsory schooling on education using US data leads to weak first-stage estimates.

17 We have also estimated models where we allow the effects of calendar month of birth to differ on either side of the September 1957 threshold. The results are similar.

18 General fertility patterns in the UK and Europe are discussed in the Appendix and help to give some context to the underlying cohort trends we observe here.

19 Our data do not allow us to examine fertility at ages below 15. The administrative birth records begin in 1970, which means that the first cohort for whom we could calculate an age 14 birth rate is January 1956. This leaves us with too

Looking at panel A of Table 2, the ROSLA led to significant effects on age 16 and age 17 fertility. Since these effects are somewhat sensitive to specification, it helps to view these estimates in the context of Figure 3. Both the effects on age 16 and 17 fertility are visually apparent in Figure 3. These effects are also economically meaningful. A reduction of -0.0018 in age 16 fertility corresponds to a 10 percent decline. After scaling by the first stage effect on education of 0.31, this represents a 32 percent decline. For age 17 fertility, the reduced-form effect is an 8 percent fall implying a 26.3 percent IV effect.

The effect on age 16 fertility varies a bit across the alternative specifications in Panels B & C although the local linear fits from Panel A apparent in Figure 3 seem to be adequately capturing the patterns in the data. The IV effect for age 16 is still sizable according to these alternative specifications - a 16 percent decline. On the other hand, the age 17 fertility effect is more consistent across the other specifications. These estimates are also robust to different bandwidths (see Appendix Table 2).

Since the ROSLA kept girls in school between ages 15 and 16, and since ROSLA fertility effects are concentrated between ages 16 and 17, it is important to pinpoint the exact timing of the ROSLA fertility effects and, ultimately, the exact timing of the conception effects that precede them. In a later section we address this question by estimating teen fertility effects at narrower age ranges - for example between ages 16 and 16 and one quarter. These analyses also help to determine whether there were statistically significant ROSLA effects on fertility prior to age 17. In relation to this question, the evidence presented in the first three columns of Table 2 is somewhat inconclusive, despite the visual evidence in Figure 3.

Beyond age 17, we observe no ROSLA impacts on teenage fertility; the reduced-form effect sizes are 2.6 percent and 0.3 percent for ages 18 and 19 fertility.[21] These small impacts are evident both in Figure 3 and Table 2. There does, however, appear to be a significant change in the *slope* of the age-19 fertility trend near this threshold. Upon closer inspection the timing for this change would have been 1976-1977. This time effect, which occurs in the age 19 plot among cohorts born in late 1957, is also apparent in the age 18 plot among cohorts born in late 1958, in the age 17 plot among cohorts born in late 1959, and in the age 16 plot among cohorts born in late 1960. Since the graphs plotted in Appendix Figure 1 reveal that both France and Germany shared the same trend break, it seems safe to conclude that this has nothing to do with the ROSLA but more to do with general trends in fertility at that time.

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small a pre-reform sample to do meaningful estimation. However, little is lost by this limitation. Fertility at age 14 is a rare event, even in our dataset of the universe of births. Thus any age 14 estimates would have been associated with large standard errors and driven by just a handful of births.

[20] The Table 2 teen fertility estimates are based on a narrower data window than the Table 1 education estimates. These different windows are those suggested by a cross-validation procedure. Intuitively, the fertility window is narrower because, around the ROSLA threshold, fertility trends are more rapidly changing than education trends. In suggesting a narrower window, the cross-validation procedure implicitly takes account of this feature of the data.

[21] The finding of no ROSLA effects on fertility at ages 18 and 19 extends to ROSLA effects on fertility rates in one-year age intervals up to 45.

In terms of understanding the nature of these teenage fertility effects, we can further divide the outcome into in-marriage and out-of-marriage births. Appendix Figure 2 shows the regression discontinuity plots for in-marriage and out-of-marriage birth rates; Table 3 presents the analogous regression discontinuity estimates with standard errors. Appendix Table 2 examines the robustness of these estimates to varying bandwidths.

The figure clearly shows that the fertility impacts we see for ages 16 and 17 come from a decline in in-marriage fertility. In this population and time period, essentially all marriages among 16-17 year-old girls were preceded by a conception. Because all births in-marriage were indicative of a shotgun marriage in this sense, one implication of the observed drop in in-marriage births is a corresponding decline in shotgun marriages. There is no fall in birth rates for out-of-marriage fertility. Not surprisingly, the magnitudes of the regression discontinuity are similar to those in Table 2 for fertility overall. It is difficult to surmise without additional information whether for this age group, the welfare of the parents and child are better if the birth occurs in or out of marriage. It is also not possible to determine whether, within the context of the observed overall decline in births, there was substitution towards out-of-marriage status.

6.2 Abortions

A missing element in past education and fertility studies is how abortion may mediate the impact of education on fertility. Usually abortion data are difficult to come by. For example, in the United States abortions are not universally reported across states, but in the UK, due to the existence of the National Health Service, which provides universal health insurance, abortion statistics are considered more reliable and comprehensive. We also think that the context of the 1972 ROSLA is unique because unlike other compulsory schooling studies, the quasi-experiment we exploit occurred in the era of modern fertility when abortion and contraceptives were available. Abortions are an important part of fertility; the ratio of abortions to births in our sample varies from 1.7 for 15 year olds to 0.30 for 19 year olds. The teenage abortion rates experienced by the post-ROSLA cohorts are similar to that experienced by cohorts today. Additionally, information on abortions allows us to get closer to understanding how education affects conception and sexual behavior than looking at the effects of education on fertility alone.

To see the importance of observing abortions, consider that the negligible effects of education on fertility for the older teenagers may reflect similar rates of contraception of pre- and post-ROSLA cohorts or higher rates of conception among pre-ROSLA cohorts with higher rates of abortion for these cohorts. Similarly, the education-related reductions in fertility among 16 and 17 year olds would be consistent with either with a difference in conception rates among pre- and post-reform cohorts or with comparable conception rates but a higher rate of abortions among the post-reform cohorts due to the higher opportunity cost of having a birth for these cohorts.

Now we turn to the abortion data to test for these intervening effects. In Table 4, we present the reduced-form regression discontinuity estimates for abortions at age 15 through 19; Figure 4 shows the analogous figures. Across all of the ages and all of the specifications, there is no strong evidence of an impact of the ROSLA on abortion rates. These conclusions are robust to the alternative bandwidths (see Appendix Tables 2). Just examining the point estimates, most of the reduced-form estimates range 1 to 3 percent. The exception is the age 15 abortion rate, which implies a 5 to 9 reduction in abortion rates, but because the incidence rate is so low, the estimates are imprecise.

6.3 Conceptions

Rather than examining abortions and births separately as a means of estimating the effects on conceptions, we can combine these data together to estimate a conception rate. To do this, we adjust the timing of abortions to be consistent with that of births. For example, an abortion in September 1974 would have likely resulted in a birth in the first half of 1975 if the abortion had not occurred. To do so, we move the timing of abortions forward by 7 months (most abortions occur before 12 weeks of gestation). Then, we simply add abortions and births together to form our "conception" rate. Of course, this measure misses miscarriages.

Appendix Table 3 presents the ROSLA effects on our measure of conceptions. The increase in school exit age reduces the conception rate at ages 16 and 17 (12 percent and 9 percent reduced-form effects). These effects are statistically significant at the 1 percent level in Panel A. These magnitudes align with those for birth rates. Effects for ages 18 and 19 are much smaller and statistically insignificant.

6.4 Cumulative Fertility

Since the earliest of the ROSLA-impacted cohorts have completed their fertility by the early 2000's, we can also examine the impacts on completed family size. To begin, Figure 5 plots the regression discontinuity figure for cumulative fertility from ages 16 to 45 (inclusive). Cohort-specific cumulative fertility is often referred to as the cohort total fertility rate (TFR).[22] This cumulative rate is constructed as the sum of each cohort's age-specific rates. Because of the nature of our births data, we do not observe completed fertility at the person level. However, cohort TFR would exactly equal the average completed family size found in individual data provided that all women in the cohort survived through their childbearing years. Since the probability of survival from 16 to 45 among these cohorts was above 98 percent, we view our cumulative fertility measure as a good proxy for average completed family size.

Figure 5 suggests that there was no ROSLA effect on completed fertility. The scale of the vertical axis measures average births per cohort member, and the apparent zero effect of the reform is contained within a very narrow band of births per woman. Table 5 presents regression discontinuity effect sizes and standard errors for a expanded set of cumulative fertility outcomes. Fertility between 16 and 25, 16 and 30, 16 and 35, and 16 and 40, and 16 and 45 are considered as outcomes. The reduced form effects implied by the graph and in Table 5 range from -0.0055 to 0.0073 children. Even after this reduced form effect is scaled by the ROSLA effect on education, this range is only -

[22] Cohort TFR, which is derived from age-specific rates for a fixed cohort, differs from period TFR, which is derived from age-specific rates for a fixed point in time.

0.018 to 0.023 children. To put this effect size in context, note that for cohorts of women born near the reform threshold, an additional year of education at age 16 is associated with a decline in total childbearing of 0.248 children in the cross section (see Geruso 2010). Our IV estimates are an order of magnitude smaller. The finding of no completed fertility effect is robust to alternative regression specifications; see Appendix Table 2 for results under alternative bandwidths.

In even the simplest economic models of fertility, fertility and educational attainment are governed by the same joint decision process. These depend, among other things, on preferences, family background and peer influences, all forces that researchers cannot easily measure. These results suggest that the strong correlation found between education and completed family size reflects the impacts of this basic heterogeneity.

At first glance, it may seem that our findings of large negative teen fertility effects and small completed fertility effects imply positive "catch up" effects beyond the teenage years. This is not necessarily true. Instead, because teen fertility is such a small component of completed fertility, the confidence intervals on our fertility effects include both this catch-up story and the polar opposite story under which the teen fertility effects lead to a permanent (albeit tiny) reduction in completed fertility.

These possibilities can be seen in Figure 6, which aggregates information from 30 distinct regression discontinuity estimates from local linear regressions that include month of birth dummies. Each point represents an estimate of the cumulative fertility effect from 16 up to the indicated age. The left panel displays the regression discontinuity estimates when cumulative fertility is measured in levels, while the right panel uses cumulative fertility measured in logs. As fertility rates vary considerably with age, the log graph estimates can be interpreted as percentage change. Since the first point is degenerate, representing the cumulative effect from 16 to 16, it corresponds to the ROSLA effect on the age 16 fertility rate (from column (2) and panel B of Table 2). The figure shows that the cumulative effects begin at age 16 and then die out, as the averted teen births become swamped by the larger number of births at older ages. This is consistent both with teen effects being counteracted by post-teen effects and with alternatively, teen effects generating permanent reductions in completed fertility.

7 Understanding the Mechanisms Behind the Fertility Effects

Classical theories linking fertility and education emphasize the role of opportunity costs (Becker 1960 and Willis 1973). Increases to a woman's education raise the opportunity cost of her time and lead to the combination of income and substitution effects influencing fertility in opposite directions. To explain the strong cross-sectional correlation between education and fertility, Willis (1973) argues that the substitution effect trumps the income effect. Becker (1965) and Becker and Lewis (1974) discuss a substitution from quantity towards quality with ambiguous effects on family size. Note that under any such theories, one would expect the influence of education to be permanent (i.e., impact fertility throughout the life cycle) and not short-lived in the teen years as we observe here.

The temporary nature of the effect we find is more consistent with an alternative role of schooling for impacting fertility. In particular, the teen fertility reductions appear to follow the compulsory schooling period with a lag of about one year, suggesting a connection between the fertility effects and the new period of schooling itself. It seems as though being enrolled in school changes the opportunity cost of time, the ability to find a mate, or other factors related to being in school. The findings of other studies confirm this role for education. For example, in Chile, the lengthening of the school day is associated with fertility reductions (Berthelon and Kruger 2011).

7.1 Detailed Timing

To explore the idea of this incapacitation effect of schooling, we now examine the timing of fertility effects relative to the timing of the new compulsory schooling period in more detail. Figure 7 tests this conjecture that the reduction in pregnancies carried to term is influenced by *current* school attendance. Each panel plots ROSLA impacts on fertility rates defined over a succession of three-month intervals. The leftmost point on each plot corresponds to ages 15-15.25, the second point to ages 15.25-15.5 and so on. The associated confidence intervals are represented by the dotted lines around each of these points. The vertical dashed lines depict the new compulsory schooling period shifted forwards by nine months to indicate where to expect a reduction in births if conceptions were reduced during the new compulsory enrollment. For most individuals, this additional compulsory schooling occurred between exact ages 15.5 and 16.5.[23] Although the correspondence is not exact, the figure shows that roughly speaking, the ROSLA effects are largest 9-12 months after the new schooling period.

To explain the teen fertility effects then, we must explain why girls were less likely to become pregnant during the new compulsory schooling period. An obvious explanation is that they had fewer opportunities to become pregnant: less contact and less time with older men that they might have met in other contexts, such as the workplace. We can observe very little about the partnerships of teen girls, but aggregate national statistics from England and Wales show that most marriages among teen girls in this time period (which would be comprised primarily of shotgun weddings for 16 and 17 year olds) are made with men two or more years older. To shed light on the possible incapacitation story in which schooling kept girls separated from older potential partners, it would be useful to know more about the types of people that ROSLA-affected girls associated with and their preferences over older partners. For that, we turn briefly to a complimentary panel survey that followed one of the first post-reform cohorts.

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[23] Unlike in the US, students in the UK wishing to leave school at the earliest permissible date could not do so immediately upon their 16th birthday. In most cases, they had to complete a school term that ended some time after their 16th birthday.

7.2 Direct Evidence Partnerships from Complier Self-Reports

We gain insight into preferences over mates and fertility using the age-16 and age-23 waves of the NCDS. All NCDS respondents were born in March 1958, making their cohort one of the first to be affected by ROSLA. The NCDS data is unique in that it contains self-reports of the desired school-leaving age. At the time they were 16, cohort members were asked: "Do you wish that you could have left [school] when you were 15?" We use the answer to determine the desired age for leaving school. In the case of missing or uncertain responses to this question, we use the parents' answer to the question: "In the study child's case do they wish that he/she had been able to leave school at fifteen?" We use this information, combined with other survey responses to provide intuition for the potential mechanisms behind our results.

Adopting the classic instrumental variables language, we call respondents who wished to leave school at 15 but stayed on until 16 the compliers, and the respondents who left at 16, but didn't wish to leave at 15, the always-takers. Because these are hypothetical and not actual responses, we assess the possibility validity of these responses by comparing the fraction of NCDS compliers (0.27), to the fraction of LFS respondents in the pre-reform cohorts who report actually leaving school at 15 (0.34). As further evidence that self-reports are informative of the counterfactual in which the school-leaving age had not been raised, we show that regional variations in stated preferences (post-reform) and realized schooling outcomes of earlier cohorts (pre-reform) are strongly correlated. Appendix Figure 3 is a scatterplot showing that the fraction of NCDS respondents wishing to leave school at age 15 in each region highly correlates with the fraction of LFS respondents actually leaving school at age 15 in that region in the pre-reform period. The correlation is among female respondents is 0.72, despite that the LFS measures region of current residence and the NCDS measures region of birth.

Panel A of Table 6 lists some descriptive statistics of the female NCDS cohort members, stratified by complier status. First, it is interesting to note that at age 16, after the additional year of schooling, the compliers and always takers differ in their preferences over mates and childbearing, with compliers preferring earlier marriage and earlier children. Almost 20% of compliers preferred to be married by 18, compared to just 11% of always-takers. We do not know whether the additional year of schooling had *no* effect on the childbearing preferences of the compliers, but we do know from these statistics that experiencing the additional year of schooling did not cause to their preferences to converge to those of the always-takers, who also completed their schooling at age 16.

Panel A also shows that a significant fraction of girls in both groups worked during full-time schooling. Compliers were slightly more likely to do so and worked more hours. Work is a plausibly important context for defining peers. Because the respondents in the NCDS are all post-reform, compliers and always takers do not differ in the compulsory schooling age they faced. However, the high prevalence of work in both groups, even during full-time education, suggests that education could have been a binding constraint on the labor force activities of compliers. If so, compulsory schooling could have altered compliers' exposure to a different set of peers and potential mates.

We can look more closely at certain kinds of partnerships in the NCDS to gain insight into mate characteristics. Panel A shows that while essentially all women lived with their guardians on their 16th birthday, a substantial minority left home at 16 or 17, many to begin cohabitating with a mate. In Panel B of Table 6, we explore the characteristics of cohabitating partnerships that began at these ages. Among both compliers and always-takers, the men these women partnered with were substantially older. Among 16 year-old women, the 25th percentile of the partnering man's age was 18 and the median was 20. A full third of the women cohabitating 16 had a birth 16, and over one half had a birth by 17. Similar patterns are found for women who began cohabitating at 17. These descriptive statistics show that women who were in serious relationships at 16 and 17 were uniformly partnering with much older men.

The fact that both compliers and always-takers lived at home until the end of schooling, after which a substantial minority began cohabitating with older men, suggests that the reform may have been delayed the initiation of cohabitating partnerships for complier women. Thus one potential reason that fertility declined at ages 16 and 17 is that the additional year of schooling limited women's exposure to older men at age 16. For a more systematic analysis of this hypothesis, we use census data below to explore the effect of the reform on the probability of marrying a similarly-aged classmate, which could serve as a substitute for an older man. We also investigate effects on the age of spouse. Estimates of both effects are consistent this hypothesis, with women marrying men closer to them in age.

There are several other factors that could explain why girls were less likely to become pregnant during the new schooling period, although none are entirely convincing because they do not predict temporary decreases in fertility. First, the extra schooling may have weakened girls' preferences for motherhood. It is not hard to imagine why education might have such an effect, but it is hard to explain why this type of effect would be temporary. That is, why would it not reduce fertility at later ages? Second, the extra schooling may have provided girls with better information about birth control. Again though, one would expect girls to retain this informational advantage beyond the new compulsory schooling period, resulting in reduced fertility at later ages, including at 18 and 19.

8 Mating Market Results

Aside from fertility, another aspect of family formation potentially impacted by education is assortative mating (Lefgren and McIntyre 2006). We will look at assortative mating over the dimensions of education and age. In the presence of assortative mating, the ROSLA could have a multiplier effect – for example, affecting both the education of the woman and the spouse she chooses.[24]

8.1 Spouse's Education

For some UK evidence on this question, Figures 8 & 9 and the corresponding Table 7 provide the ROSLA impacts on partner's educational level. Figure 8 shows the probability of the male spouse

[24] The ROSLA could also impact a man's education and that of his spouse.

having no qualifications, 1 or more O-level exam or 1 A-level exam, and 2 or more A-level exams or higher as a function of the wife's date of birth. These categories are mutually exclusive. O-level and A-level exams are sets of national exams covering different subjects. O-level exams happen after grade 9 (the grade in which most will turn 16). A-level exams are administered two years later. Figure 9 is the analogous figure for men. It is important to keep in mind that we observe these individuals in 2001, many years after the ROSLA. As such, we interpret these effects as the long-run impacts on mate choice. Since all of these figures are conditional on marriage, in analysis not reported, we also look at the probability of marriage as a function of date of birth. There are no impacts on marriage probabilities for either gender.

Both women and men affected by ROSLA were less likely to marry men or women with no qualifications. The regression discontinuity estimates are similar for men and women – a statistically-significant reduction of 1-1.5 percentage points. Figures 8 & 9 visually confirm these estimates. These estimates are remarkably stable across specifications, including varying bandwidths (Appendix Table 2). When scaled by the first stage estimates, these effects amount to a 15 to 20 percentage point reduction for women and men, respectively, in the probability of marrying an individual with no educational qualifications. Given conventional estimates of the rate of return to schooling, and assuming that the difference between no qualifications and some qualifications corresponds to a year of schooling, this spousal education channel could lead to important effects of education on household income.

It should be noted that the population affected on the mating market dimension is not necessarily the population affected on the fertility dimension. Many more individuals have their mating market decisions impacted than their fertility decisions (i.e., 1 percentage point mating market effect versus 0.03 percentage point age-16 fertility effect).

Instead of marrying mates with no credentials, the ROSLA-impacted cohorts married men or women with some O-level qualifications/1 A-level qualification; we observe small and insignificant impacts on marrying someone with 2 or more A-level qualifications or higher.

8.2 Spouse's Age

Schooling could impact the educational attainment of one's mate in several ways. An extension of schooling could influence the composition of their age 15 peer group and make one more likely to marry someone whom they met through school. Additional education could change spousal preferences or opportunities to meet more-educated potential husbands or wives (e.g., through changes in occupation or employment status).

For further insight into the mechanisms behind the education effects, we now turn to examining how the ROSLA impacted spousal age. On average, men in this context marry women who are roughly 2 years younger. Thus, if the increase in required schooling increases the probability of marrying a schoolmate, we would expect that men and women would marry individuals closer in age.

In Figure 10, we display the regression discontinuity graphs for the age difference. In the left panel we show the husband's age minus wife's age as a function of the woman's month of birth. In the right panel we show the wife's age minus husband's age as a function of the man's month of birth. These plots are seasonally-adjusted by removing month-of-birth effects.[25] For women, there is a sharp decrease in the average age gap at the threshold consistent with an effect of the ROSLA. That is, women who had to stay until 16 marry younger men. The corresponding regression discontinuity estimate is -0.87 of a month (roughly a 3 percent reduced-form effect), which is statistically significant at the 1 percent level.[26] On the other hand, there is not a similar effect for men.

The average age graphs obfuscate interesting underlying distributional effects. In Figure 11, we plot the regression discontinuity estimates for each integer age gap along with 95 percent confidence intervals, adjusting for seasonality via the inclusion of month-of-birth controls. The top panel displays that for the age gap of husband's age-wife's age as a function of wife's date of birth and the bottom panel displays that for the age gap of wife's age-husband's age as a function of husband's date of birth. For the top panel, the 0.004 at age gap at 0 implies that the women born immediately following September 1957 are 0.4 percentage points more likely to marry a man between 0 and 1 year older than them than women born immediately prior to this date.

For women, the impacts imply that the ROSLA increased the probability of marrying a man 1 and 11 years older and 3 years younger and reduces the probability of marrying a man 2, 3, 8, and 9 years older. One plausible characterization of these patterns is that the extra compulsory schooling raises woman's likelihood of marrying a much older man but also shifts some women who have married men 2 to 3 years older to marrying men 0 to 1 years older.

For men, the shifting of the age gap is much more modest. As a result of the ROSLA, men are less likely to marry women who are 2 years younger but more likely to marry women who are 3 years younger or the same age as themselves.

We can test more directly whether the education assortative mating effects are due to the fact that extending the length of schooling changes the peer group that one is exposed to. We do this in two ways. First, in Appendix Figure 4, we compute the fraction marrying someone in their academic cohort and see whether we observe a regression discontinuity in that outcome. An academic cohort starts with September births and ends with August births of the following year. There is considerable but systematic seasonality in the probability of marrying a classmate. For women, the oldest in class are less likely to marry a classmate than the youngest in the class (a 0.11 probability versus a 0.13 probability). For men, the pattern is the reverse with a slightly larger difference

[25] We regress the age gap on a set of month-of-birth dummies, take the residuals, and add back the mean age gap to obtain these seasonally-adjusted numbers.
[26] This estimate comes from a local linear regression with month-of-birth dummies.

between the oldest and the youngest (a 0.15 probability versus a 0.11 probability). This seasonality is unaffected by the ROSLA. Due to this seasonality, the Appendix Figure 4 plots the de-seasonalized patterns to be able to visually discern the ROSLA effect. Both for men and women, the probability of marrying a classmate increases because of the ROSLA. Using the specification that controls for month of birth (i.e., Panel B specification in the regression tables), the regression discontinuity point estimates are 0.0042 and 0.0032, respectively for women and men – both are statistically significant but small.

Second, it might be possible that the assortative mating effects arise due not to marrying someone in one's own class but someone who was also subjected to the 1972 ROSLA. In Appendix Figure 5, we calculate the fraction marrying someone born September 1957 or later. This raw figure exhibits little seasonality so we present the figure unadjusted for seasonality. For both men and women, there is an apparent discontinuity in the probability of marrying an individual whose compulsory schooling age was 16. Local linear regression regression discontinuity estimates (not reported) including month-of-birth dummies are 0.069 for women and 0.054 for men. At least some of the spousal education effects result from ROSLA-affected women being more likely to marry ROSLA-affected men and vice versa, irrespective of their preferences for spousal education or opportunities to meet more-educated potential husbands or wives. Policies that change one gender's education levels without changing those of the other would likely have weaker effects. Nevertheless, theory and evidence suggests education effects on spousal education and it is interesting to see those effects in this context.

9 Discussion

Given the strong negative correlation between education and fertility often found in observational data, it is somewhat surprising that we find no evidence of an education effect on completed fertility. It is unsurprising that OLS estimates are biased towards large fertility effects - OLS estimates are unlikely to control for all of the factors that might generate a spurious correlation between education and fertility. This is particularly true since many of these are hard to measure, such as women's attitudes towards their role in society. But our results and standard errors for completed fertility (through age 45) rule out even a small fraction of the OLS effects. On face value, this contradicts some theories of the education-fertility relationship, which predict that education reduces fertility via its impact on wage rates and hence on the opportunity cost of raising children.

In relation to theory, note that if children are a normal good, then other things equal, we would expect more-educated women to demand more of them. Willis (1973) acknowledges this, but argues that these positive effects are likely smaller than the negative effects operating via the opportunity cost mechanism. In the context of his model, this conclusion seems reasonable. This model does, however, impose some strong assumptions. Once these are relaxed, the positive income effects may be as strong as the negative opportunity cost (substitution) effects.

One important assumption is that women must choose between working and raising children. An alternative formulation might have women facing a labor market penalty for raising children, where this penalty varies across time, across countries and across occupations. If public policies and work practices have become more family-friendly over time, this penalty—and thus the strength of the opportunity cost mechanism—will have weakened.

The problem of teen fertility is one that attracts a lot of attention. Discussions of how to reduce it have focused on sex education, the availability of contraception and the way the welfare system treats teenage mothers. In this paper we have shown that it might be reduced by policies that result in girls spending more time in school. Such educational policies have other very positive benefits including improving labor market opportunities and thus likely provide a high return on investment in comparison to other policies aimed at decreasing teenage pregnancy.

The decline in completed fertility is a related problem that policy-makers have grappled with. Education does not feature in this policy discussion because, if anything, the conventional wisdom is that more-educated women have fewer children. Our analysis suggests that there is no causal relationship between education and completed fertility in the modern, developed country context we examine: Our estimates are statistically indistinguishable from zero and our confidence intervals restrict the possible effects to a small range.

Aside from fertility decisions, this reform had other effects on family formation through positive assortative mating. ROSLA-impacted women and men married more educated mates. For women, the age gap between spouses fell with women marrying younger mates. At least some of these mating market effects are attributable to being in school as both men and women became more likely to marry classmates and more generally others who were also impacted by the 1972 ROSLA. This education assortative result is particularly important as the target population of the ROSLA includes those who are risk of dropping out of school and thus, those who may have reduced means as adults. The ROSLA has a multiplier effect through impacting both the husband's and wife's education level, further reducing the likelihood that this at-risk population lives in poverty.

Our findings are important as policy-makers in both the UK and US are consider measures that would result in girls spending more time in school. The UK Coalition Government increased the compulsory schooling age to 17 in September 2013 and will further augment it to 18 by 2015. Our results suggest that these measures could reduce teen fertility. Moreover, they suggest that these reductions could be achieved with little or no change in completed fertility. If the costs of teen fertility are as high as analysts claim, then these policies will generate important social benefits beyond those typically considered along dimensions such as crime and the labor market.

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APPENDIX

1972 ROSLA

We define the treatment of interest as experiencing an extra year of full-time education at age 15. We have characterized the 1972 ROSLA as increasing the minimum leaving age from 15 to 16. It seems natural to suppose that the ROSLA did, therefore, push significant fractions of students into this treatment.

While this intuition is approximately correct, it is worth noting that the school leaving rules were slightly more complicated. In particular, there were two possible points at which students could leave school at the minimum leaving age: Easter (i.e., the end of the spring term) and June/July (i.e., the end of the summer term). Students born between 1 September and 31 January were eligible to leave at Easter; students born 1 Febuary-31 August were required to remain in school until the summer. For a student born in September 1957, the 1972 ROSLA meant that the earliest leaving point was Easter 1974 rather than Easter 1973. For a student born in February 1958, the 1972 ROSLA meant that the earliest leaving point was summer 1974 rather than summer 1973.

Because students born between February and August could leave in June/July, some would actually leave before the minimum school leaving age (both before and after the 1972 ROSLA). This can account for some of the apparent non-compliance seen in Figure 1. Because the ROSLA required students to remain until age 16, this meant the earliest academic year they could leave was the academic year in which students took O Level and CSE exams. Since these exams are taken in May/June, this meant that some students, such as those born in early July, would technically have left at the minimum age under the old system (i.e., age 15) but below the minimum age under the new system (i.e., also at age 15, albeit almost one year later). This can account for some of the increased non-compliance that appears to follow the 1972 ROSLA.

Appendix: Fertility Trends in Europe

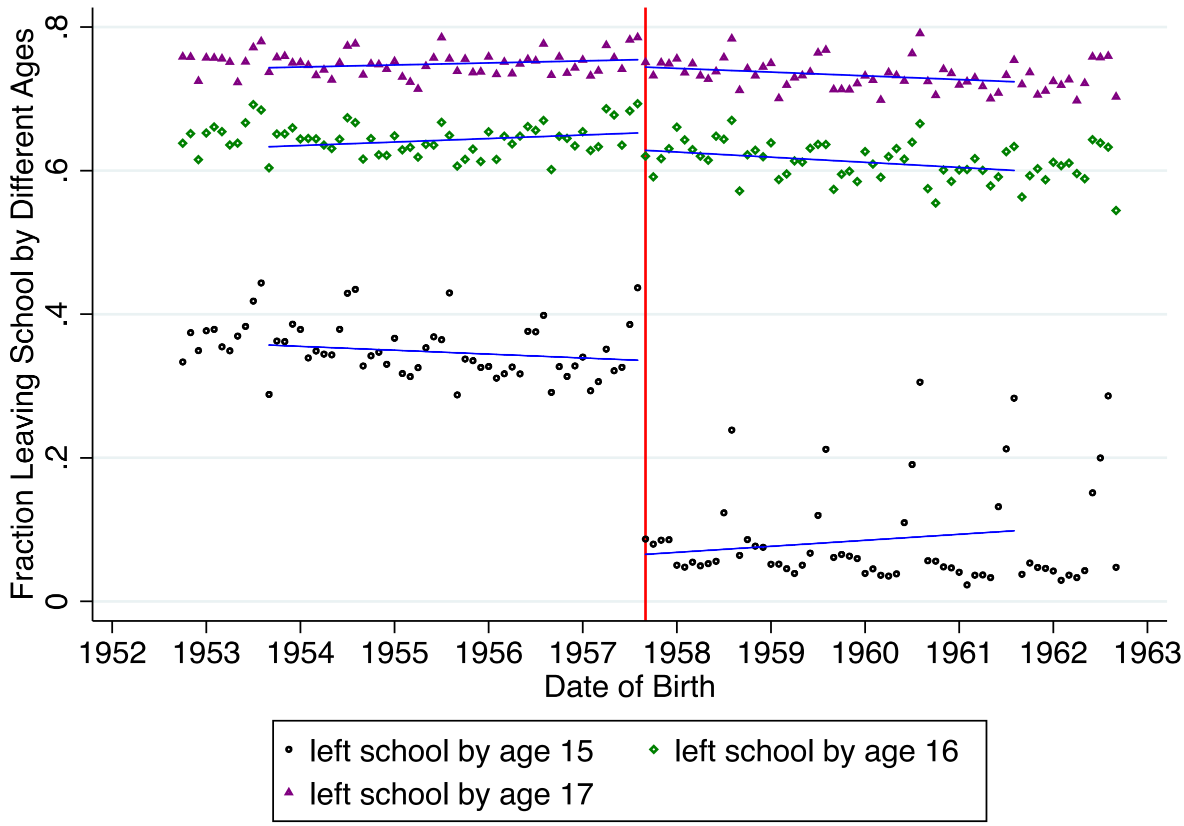
It is important to understand the environment in which this change occurred. Fertility declined dramatically between the mid-1960s and the mid-1970s in the UK. Appendix Figure 1 displays the trend, along with trends in the US, France and Germany for comparison. In the UK, by 1975, total fertility had fallen to modern, below-replacement levels.

The post-1972 ROSLA period in the UK, marked by the vertical line, is a period of low and stable fertility (1972-2002). Modern fertility rates in the UK are quite comparable to those of the US and France. Given the similarity of fertility today and at the time of the enactment of the 1972 ROSLA, the 1972 ROSLA is almost ideal for studying the education-fertility relationship: early enough for the affected women to have now completed their fertility, but late enough for them to be informative about childbearing in a modern setting. Moreover, the cohorts just affected by the reform have shared the same access to modern contraception and abortion services throughout their fecund lives as women today.[27]

Aside from total fertility, we examine teenage births, often an explicit target of fertility policies. As seen in Appendix Figure 1, teen fertility also declined through the 1960s and 1970s, although in the UK it fell slightly slower and slightly later than total fertility. The timing of the fertility decline is consistent with a possible effect of the 1972 ROSLA. However, all countries depicted in the graph exhibit a similarly-timed decline, raising the possibility that wider trends could be confounded with a ROSLA program effect. This highlights the importance of using a research design that can credibly control for these cohort and time trends. Teen fertility in the UK stabilized at a level higher than those seen elsewhere in Western Europe but below that seen in the US. As of 2009, about one in eight girls in the UK bore a child before age 20.

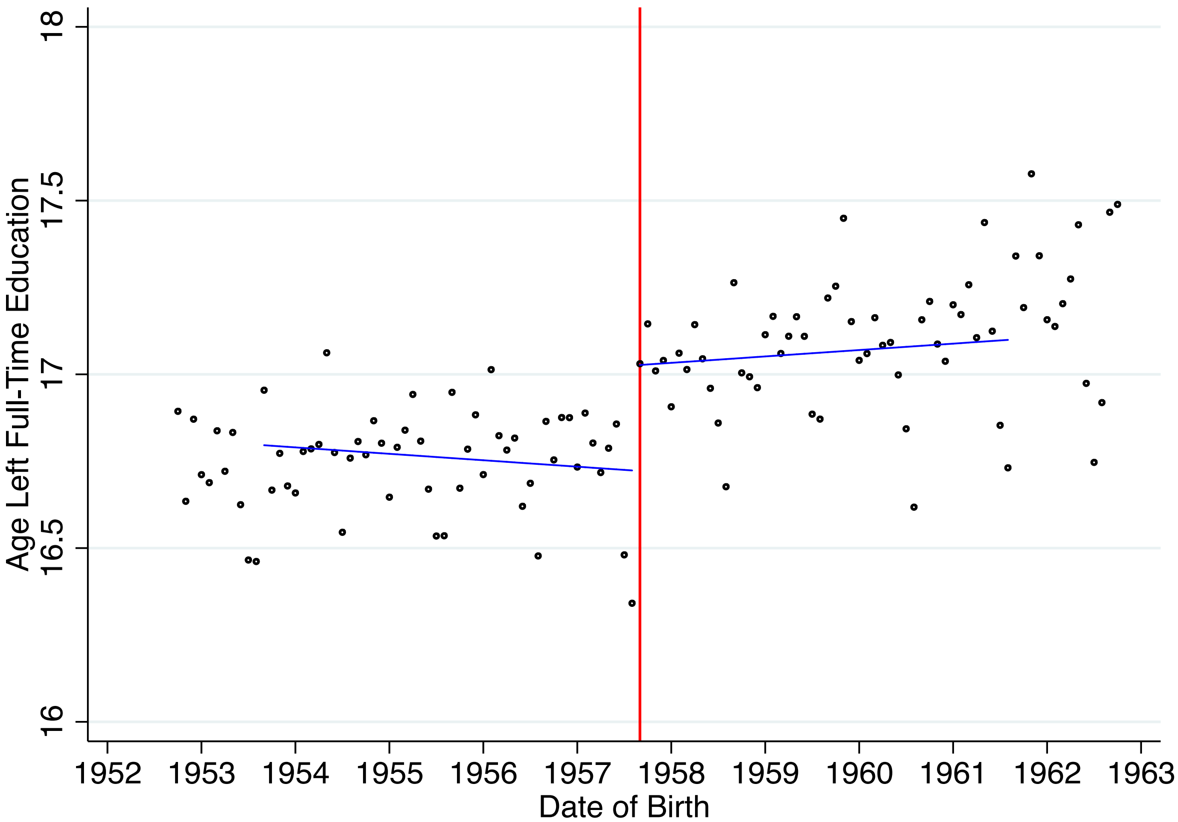
[27]The 1967 Abortion Act legalized abortion in the UK. This Act covered abortions for teenage girls and did not require parental consent or notification. Oral contraceptives became available in 1961 through the National Health Service (http://news.bbc.co.uk/onthisday/hi/dates/stories/december/4/newsid_3228000/3228207.stm).

Figure 1: Effect of ROSLA on Age Left Full Time Schooling for Women



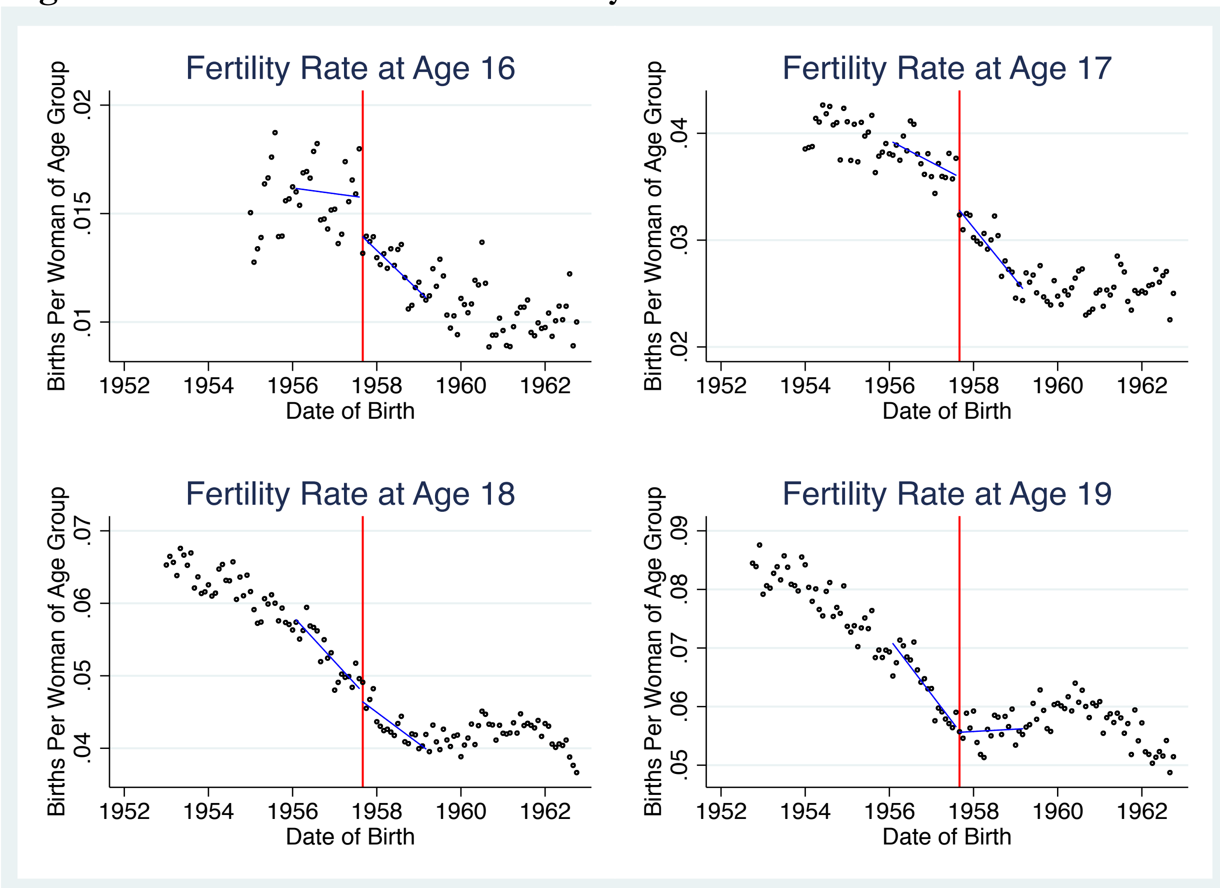
Notes: The figure depicts the impact of the 1972 ROSLA on educational attainment. Each point represents a mean for a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957. Data come from the UK Labor Force Survey, pooled 1975-2002 and restricted to women who were born in the UK and resident in England or Wales at the time of the interview. Respondents were asked the age at which they first left continuous, full-time education.

Figure 2: Effect of ROSLA on Age at Which Left Full Time Schooling



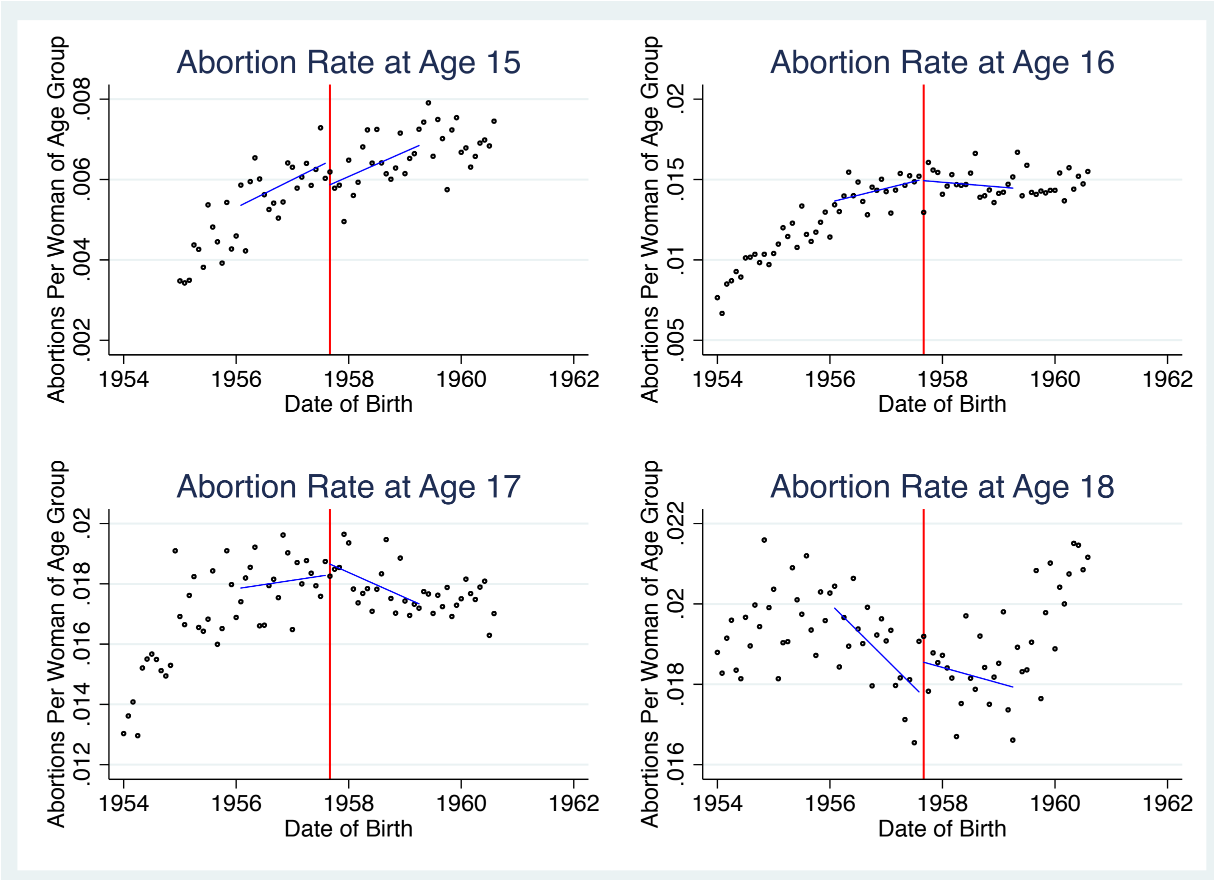
Notes: The impact of the 1972 ROSLA on educational attainment. Each point represents the mean average age at school exit for a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957. Data come from the UK Labor Force Survey, pooled 1975-2002 and restricted to women who were born in the UK and resident in England or Wales at the time of the interview. Respondents were asked the age at which they first left continuous, full-time education.

Figure 3: Effects of ROSLA on Fertility



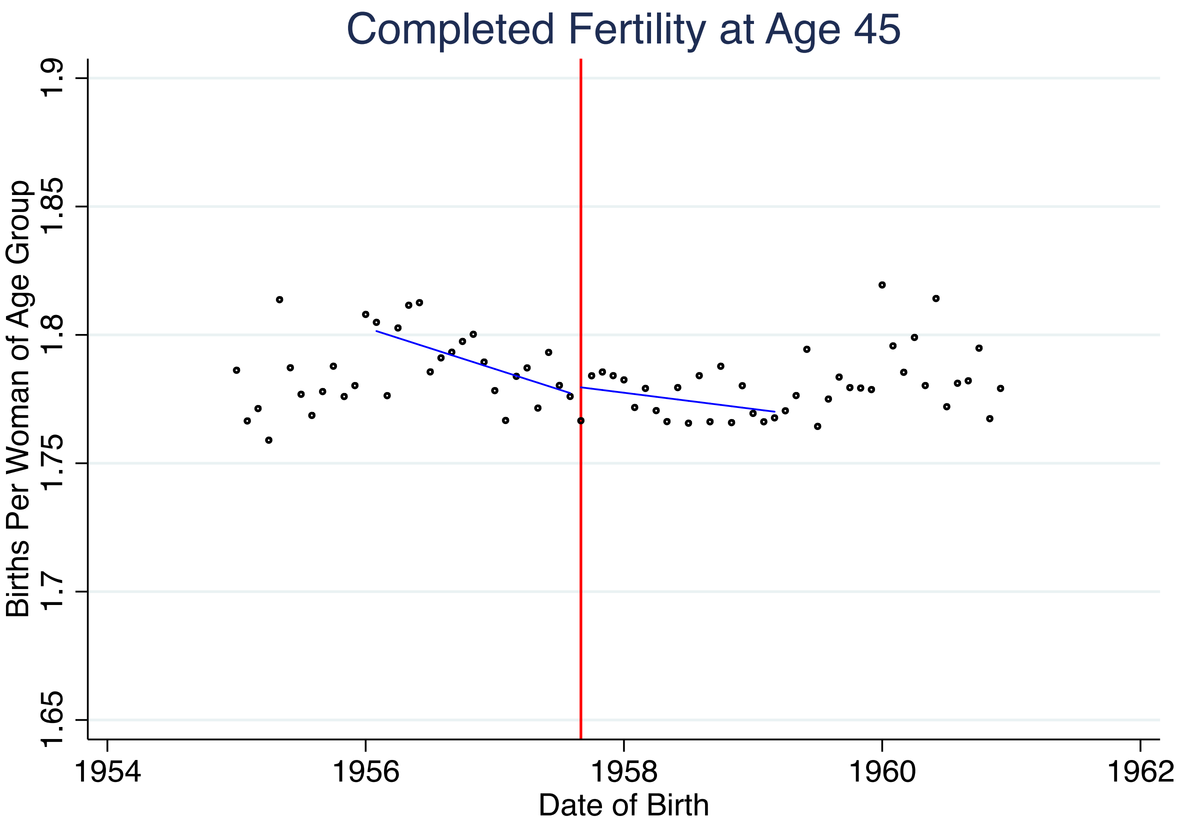
Notes: The impact of the 1972 ROSLA on fertility. Each point represents the fertility rate (number of births/size of population) for a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 4: Effects of ROSLA on Abortion



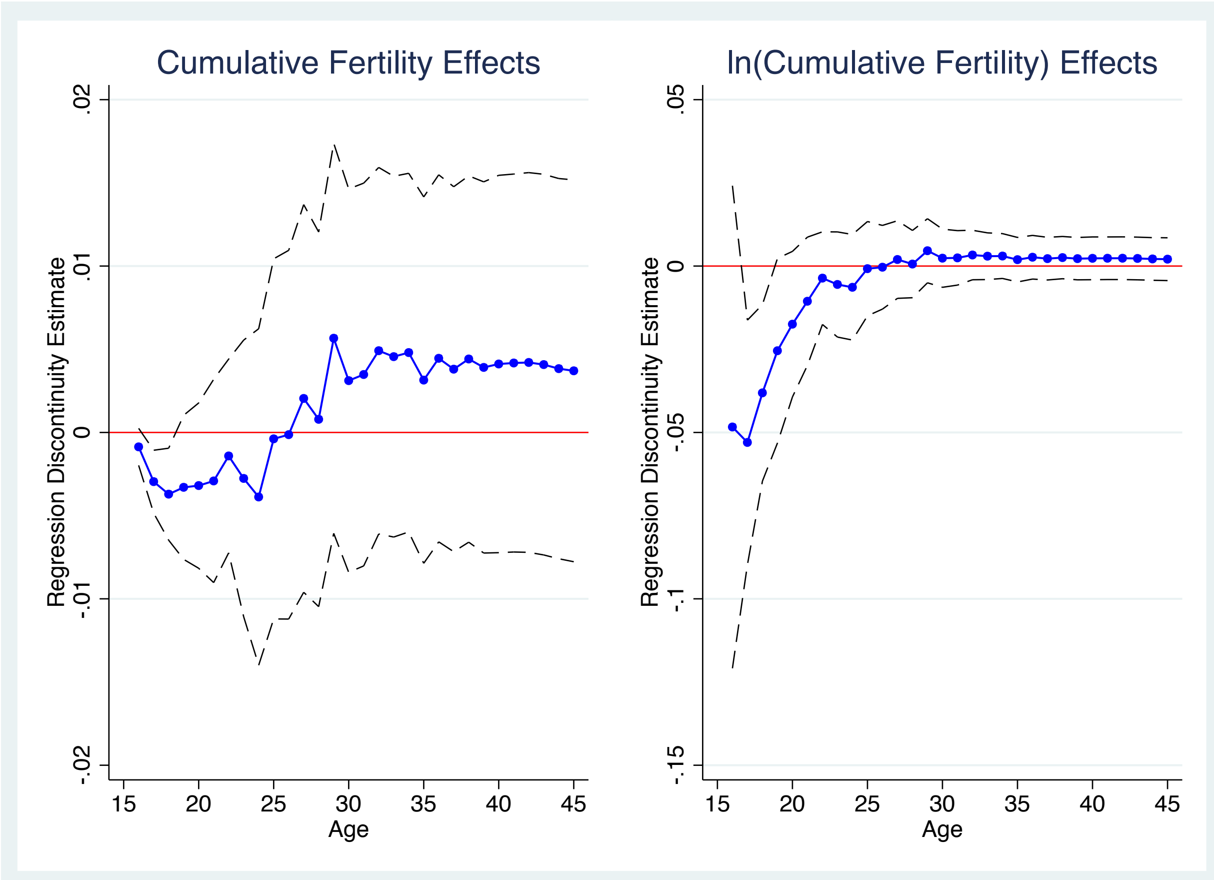
Notes: The impact of the 1972 ROSLA on abortion rates. Each point represents the abortion rate (number of abortions/size of population) for a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 5: Effect of ROSLA on Completed Fertility



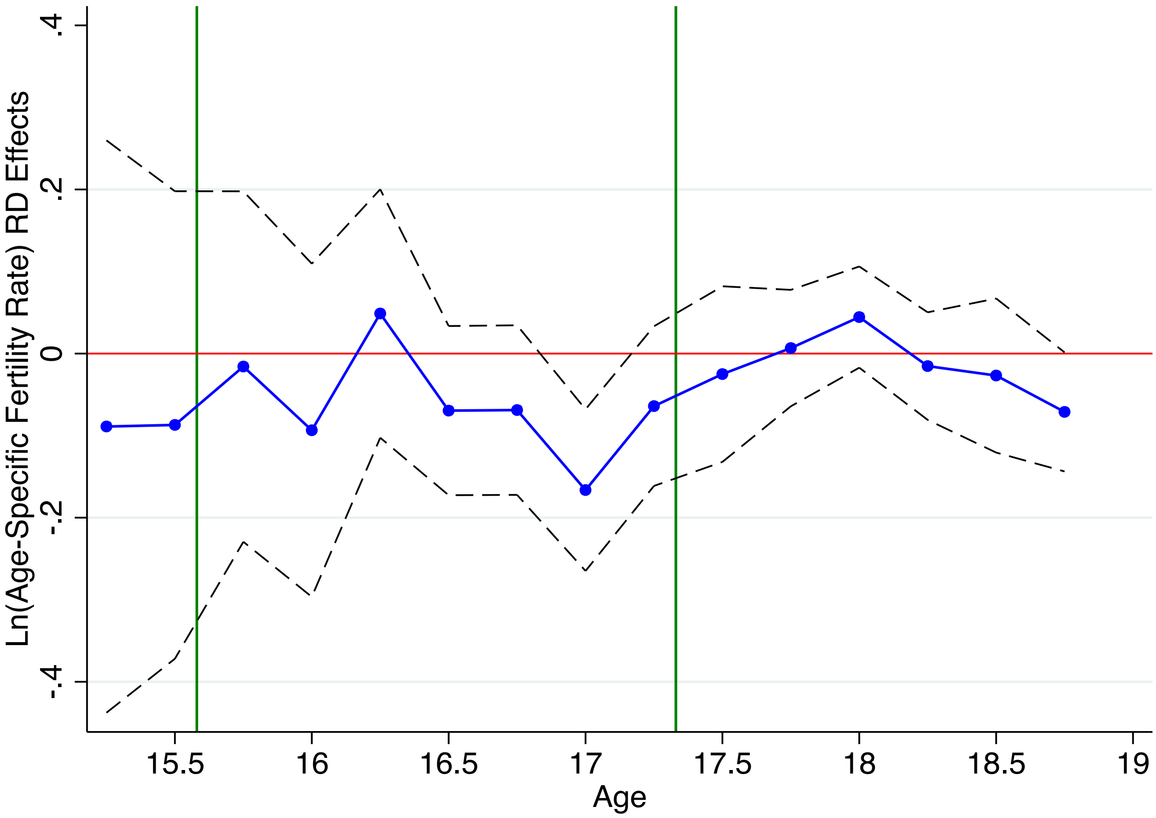
Notes: The impact of the 1972 ROSLA on completed fertility (i.e, the sum of age specific fertility rates from 16 to 45). Each point represents the average number of children born to a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 6: Evolution of Cumulative ROSLA Fertility Effects



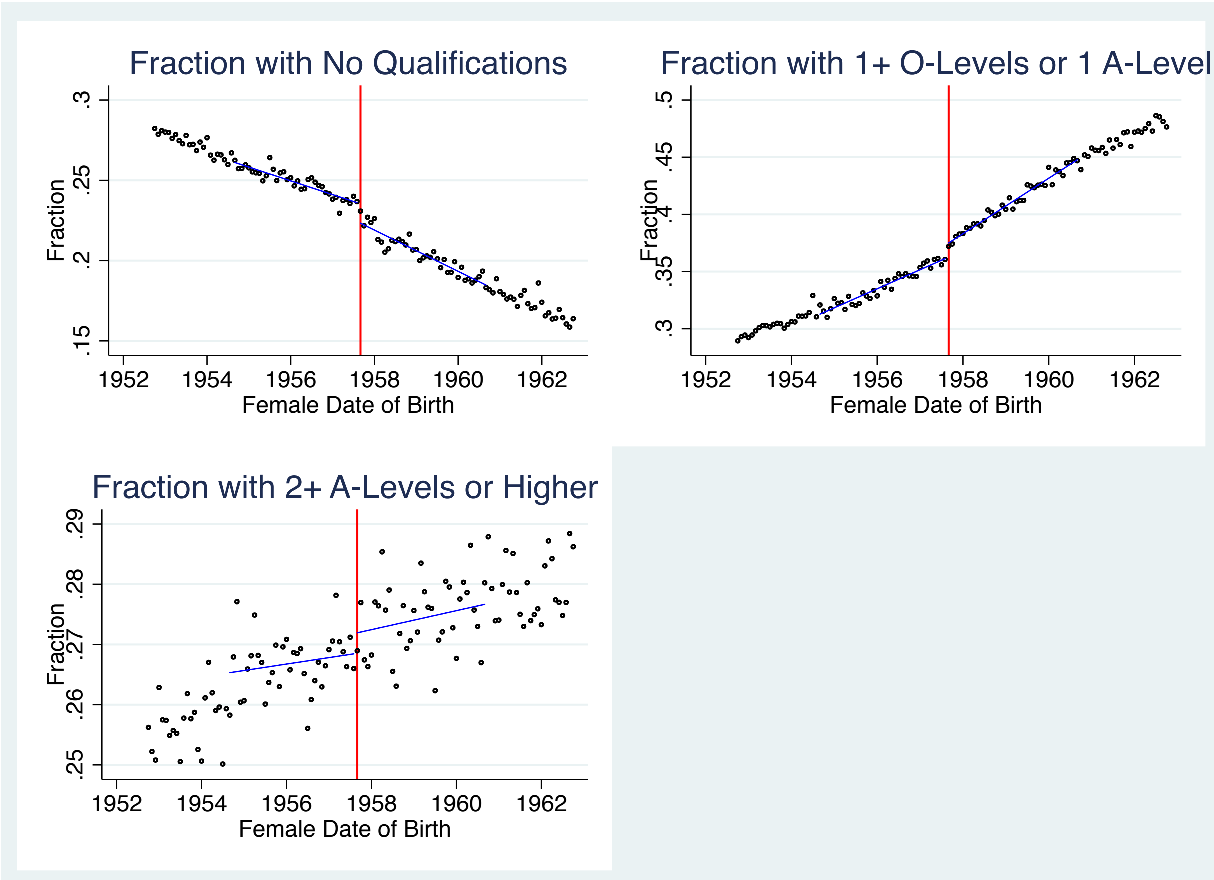
Notes: The impact of the 1972 ROSLA on cumulative fertility. Each point in the plots represents a regression discontinuity estimate of the reduced form effect of ROSLA on cumulative fertility from 16 up to and including the indicated age. The regression discontinuity regressions include the month of birth running variable, a dummy for the post-ROSLA cohorts, and their interaction along with month of birth dummies. Pointwise 95% confidence intervals are plotted as dotted lines. The left panel plots level effects, which are measured in counts of children per woman. The right panel plots effects on log of cumulative fertility. Cumulative fertility at each age x is the sum of age-specific fertility rates from 16 to x, inclusive. Since the first point in each panel is degenerate, representing the cumulative effect from 16 to 16, it corresponds exactly with the regression discontinuity estimate of the reform on the age 16 fertility rate. Robust standard errors are estimated.

Figure 7: Effects Over Teen Years



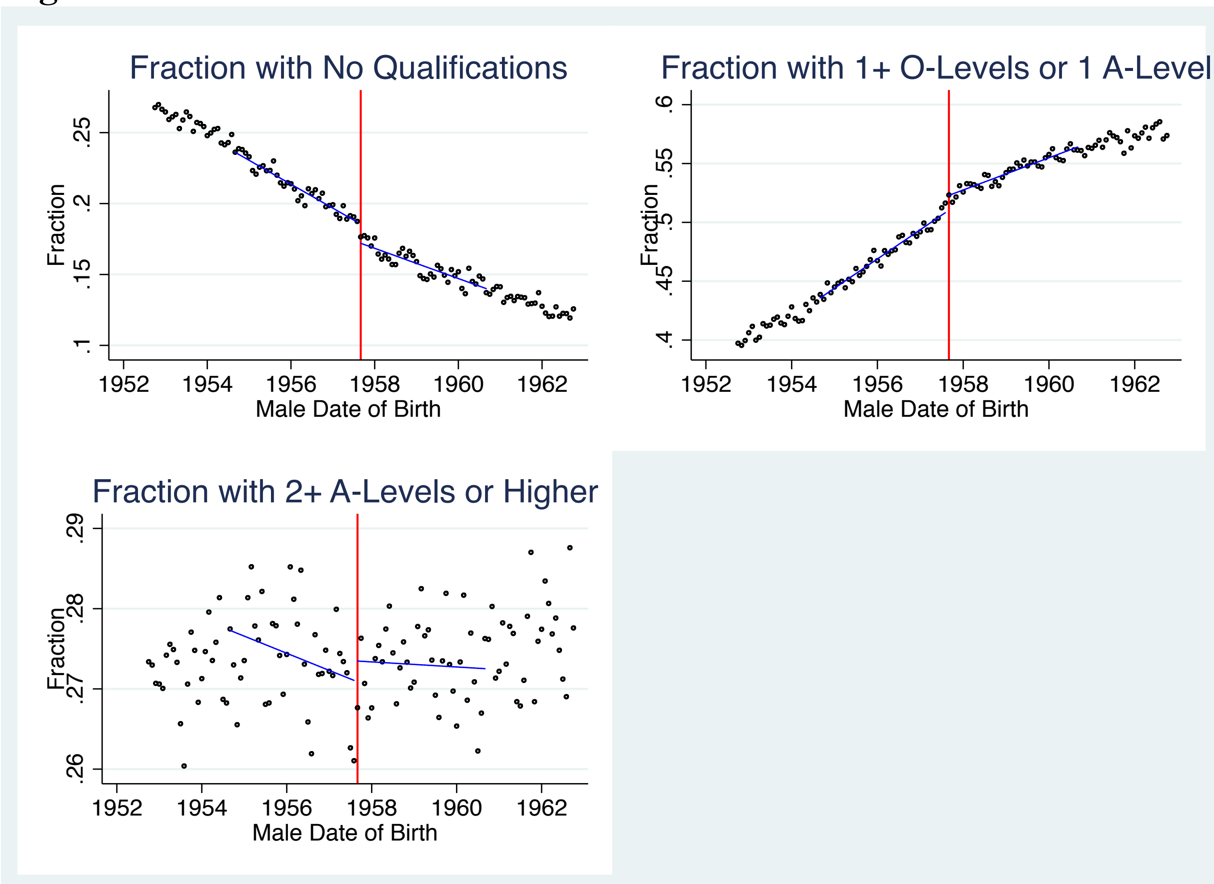
Notes: The impact of the 1972 ROSLA on fertility at certain ages. Each point in the plots represents a regression discontinuity estimate of the reduced form effect of ROSLA on ln(fertility rate) for that age. The regression discontinuity regressions include the month of birth running variable, a dummy for the post-ROSLA cohorts, and their interaction along with month of birth dummies. The green lines mark the start and end of the extra schooling due to the ROSLA (students can experience it between ages of 14.9 and 16.6 (translating into a birth effect by adding on 40 weeks of ages 15.6 to 17.3) . Pointwise 95% confidence intervals are plotted as dotted lines. Robust standard errors are estimated.

Figure 8: Effect of ROSLA on Educational Attainment of Woman's Mate



Notes: The impact of the 1972 ROSLA on the educational attainment of a woman's mate. Each point represents the fraction of women of the same month of birth cohort who have a mate of a certain education level. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 9: Effect of ROSLA on Educational Attainment of Man's Mate



Notes: The impact of the 1972 ROSLA on the educational attainment of a man's mate. Each point represents the fraction of men of the same month of birth cohort who have a mate of a certain education level.The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 10: Effect of ROSLA on Age Gap of Spouses

Notes: The impact of the 1972 ROSLA on age gap of spouses; women's mates are represented by the left panel and men's mates are represented by the right panel. Each point represents the average age gap adjusted for monthly seasonality between husband and wife (left panel) and the average age gap between wife and husband (right panel) for women of the same month of birth cohort (left panel) or men of the same month of birth cohort (right panel). Seasonality adjustments are formed by regressing the average age gap on a set of month dummies and taking the residuals and adding the average age gap back. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Figure 11: Effect of ROSLA on Age Gap Between Spouses

Notes: The impact of the 1972 ROSLA on the age gap between spouses; women's mates are represented by the left panel and men's mates are represented by the right panel. Each point represents a regression discontinuity estimate of the reduced-form effect of ROSLA on the probability of marrying a mate with a certain age gap (left panel: age difference between wife and husband for women and right panel: age difference between husband and wife for men). Regression discontinuity estimates control for seasonality through the inclusion of calendar month of birth dummies. Pointwise 95% confidence intervals are plotted as dotted lines.

Table 1. Effect of ROSLA on Age of School Exit

	Dependent Variable:			
	left school by age 15	left school by age 16	left school by age 17	age left full-time ed
Panel A: Linear cohort trend interacted with threshold				
Discontinuity	-0.27**	-0.02*	-0.01	0.31**
	(0.02)	(0.01)	(0.01)	(0.06)
Observations	96	96	96	96
Bandwidth	48	48	48	48
Mean Dependent Variable	0.437	0.693	0.785	16.34
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies				
Discontinuity	-0.24**	-0.01	-0.00	0.24**
	(0.01)	(0.01)	(0.00)	(0.04)
Observations	96	96	96	96
Bandwidth	48	48	48	48
Mean Dependent Variable	0.437	0.693	0.785	16.34
Panel C: Linear cohort trend interacted with threshold with summer borns dropped				
Discontinuity	-0.24**	-0.02	-0.01	0.21**
	(0.01)	(0.01)	(0.01)	(0.04)
Observations	72	72	72	72
Bandwidth	48	48	48	48
Mean Dependent Variable	0.437	0.693	0.785	16.34

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from UK Labour Force Surveys, 1975-2002. The sample is restricted to women born in the UK and resident in England or Wales at the time of the interview. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. *: p-value < 0.05, **: p-value < 0.01, ***: p-value < 0.001

Table 2. Effect of ROSLA on Teenage Fertility

	Dependent Variable: Fertility Rate at Age				
	15	16	17	18	19
Panel A: Linear cohort trend interacted with threshold					
Discontinuity	-0.0002	-0.0018*	-0.0031**	-0.0013	-0.0002
	(0.0002)	(0.0007)	(0.0008)	(0.0012)	(0.0014)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.0036	0.0180	0.0377	0.0496	0.0590
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies					
Discontinuity	-0.0005	-0.0001	-0.0034**	0.0022	-0.0004
	(0.0005)	(0.0008)	(0.0010)	(0.0026)	(0.0014)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.0036	0.0180	0.0377	0.0496	0.0590
Panel C: Linear cohort trend interacted with threshold with summer borns dropped					
Discontinuity	-0.0002	-0.0008	-0.0024**	-0.0001	-0.0001
	(0.0003)	(0.0009)	(0.0007)	(0.0013)	(0.0018)
Observations	29	29	29	29	29
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.0036	0.0180	0.0377	0.0496	0.0590

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from live birth records supplied by the UK Office of National Statistics, containing all births in England and Wales to mothers born in England, Wales, and Scotland, 1970-2008. These are supplemented with data on cohort sizes generated by registrar reports of cohort sizes at birth and mortality rates from the Human Mortality Database. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Table 3. Effect of ROSLA on In/Out of Marriage Teenage Fertility

	Dependent Var: In-Marriage Fertility Rate at Age					*Dependent Var: Out-Marriage Fertility Rate at Age*				
	15	16	17	18	19	15	16	17	18	19
Panel A: Linear cohort trend interacted with threshold										
Discontinuity	0.0000	-0.0015**	-0.0031**	-0.0010	-0.0004	-0.0002	-0.0003	-0.0001	-0.0003	0.0003
	(0.0000)	(0.0004)	(0.0006)	(0.0009)	(0.0011)	(0.0002)	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Observations	38	38	38	38	38	38	38	38	38	38
Bandwidth	19	19	19	19	19	19	19	19	19	19
Mean Dep Var	0	0.00842	0.0231	0.0333	0.0453	0.00360	0.00956	0.0146	0.0163	0.0137
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies										
Discontinuity	0.0000	-0.0015**	-0.0031**	-0.0010	-0.0004	-0.0002	0.0002	0.0001	-0.0002	0.0003
	(0.0000)	(0.0004)	(0.0006)	(0.0009)	(0.0011)	(0.0002)	(0.0003)	(0.0006)	(0.0005)	(0.0005)
Observations	38	38	38	38	38	38	38	38	38	38
Bandwidth	19	19	19	19	19	19	19	19	19	19
Mean Dep Var	0	0.00842	0.0231	0.0333	0.0453	0.00360	0.00956	0.0146	0.0163	0.0137
Panel C: Linear cohort trend interacted with threshold with summer borns dropped										
Discontinuity	0.0000	-0.0011	-0.0023**	-0.0007	-0.0005	-0.0002	0.0003	-0.0001	0.0006	0.0004
	(0.0000)	(0.0006)	(0.0006)	(0.0011)	(0.0014)	(0.0003)	(0.0005)	(0.0004)	(0.0005)	(0.0007)
Observations	29	29	29	29	29	29	29	29	29	29
Bandwidth	19	19	19	19	19	19	19	19	19	19
Mean Dep Var	0	0.00842	0.0231	0.0333	0.0453	0.00360	0.00956	0.0146	0.0163	0.0137

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from live birth records supplied by the UK Office of National Statistics, containing all births in England and Wales to mothers born in England, Wales, and Scotland, 1970-2008. These are supplemented with data on cohort sizes generated by registrar reports of cohort sizes at birth and mortality rates from the Human Mortality Database. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Table 4. Effect of ROSLA on Abortion Rates

	Dependent Variable: Abortion Rate at Age				
	15	16	17	18	19
Panel A: Linear cohort trend interacted with threshold					
Discontinuity	-0.00055	-0.00002	0.00033	0.00071	-0.00055
	(0.00036)	(0.00057)	(0.00042)	(0.00058)	(0.00058)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.00603	0.0152	0.0187	0.0191	0.0178
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies					
Discontinuity	-0.00036	0.00045	0.00008	0.00057	-0.00063
	(0.00038)	(0.00046)	(0.00049)	(0.00047)	(0.00060)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.00603	0.0152	0.0187	0.0191	0.0178
Panel C: Linear cohort trend interacted with threshold with summer borns dropped					
Discontinuity	-0.00043	0.00010	0.00033	0.00058	-0.00045
	(0.00041)	(0.00072)	(0.00056)	(0.00064)	(0.00078)
Observations	29	29	29	29	29
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.00603	0.0152	0.0187	0.0191	0.0178

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from abortion records supplied by the UK Office of National Statistics, containing all terminations in England and Wales to 15-19 year old mothers born in England, Wales, and Scotland, 1970-2008. These are supplemented with data on cohort sizes generated by registrar reports of cohort sizes at birth and mortality rates from the Human Mortality Database. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Table 5. Effect of ROSLA on Cumulative Fertility

	Dependent Variable: Cumulative Fertility Rate for Ages				
	16-25	16-30	16-35	16-40	16-45
Panel A: Linear cohort trend interacted with threshold					
Discontinuity	-0.0063	-0.0005	0.0022	0.0037	0.0036
	(0.0073)	(0.0073)	(0.0059)	(0.0058)	(0.0058)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.777	1.327	1.639	1.756	1.776
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies					
Discontinuity	0.0009	-0.0011	-0.0075	-0.0050	-0.0055
	(0.0049)	(0.0062)	(0.0070)	(0.0084)	(0.0089)
Observations	38	38	38	38	38
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.777	1.327	1.639	1.756	1.776
Panel C: Linear cohort trend interacted with threshold with summer borns dropped					
Discontinuity	-0.0004	0.0058	0.0075	0.0074	0.0073
	(0.0093)	(0.0092)	(0.0080)	(0.0080)	(0.0080)
Observations	29	29	29	29	29
Bandwidth	19	19	19	19	19
Mean Dependent Variable	0.777	1.327	1.639	1.756	1.776

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Cumulative fertility at each age x is the sum of age-specific fertility rates 16 to x, inclusive. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from live birth records supplied by the UK Office of National Statistics, containing all births in England and Wales to mothers born in England, Wales, and Scotland, 1970-2008. These are supplemented with data on cohort sizes generated by registrar reports of cohort sizes at birth and mortality rates from the Human Mortality Database. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Table 6. Partnerships and Preferences at Age 16

	Left School at 16		Left at 17+
	Preferred 15	Preferred 16	
Panel A			
Lived with guardians on 16th birthday	0.99	0.99	1.00
Left home at age 16	0.10	0.08	0.02
Left home at age 17	0.23	0.19	0.09
First cohabitating partnership at 16	0.06	0.02	0.00
First cohabitating partnership at 17	0.11	0.07	0.02
Obs	1039	1323	1842
Panel B			
Respondents who cohabitated at 16			
Parnter age: 25th percentile	18	18	46
Parnter age: Median	20	19.5	46
Duration of partnership	17.07	13.99	12.35
Eventual marriage	0.84	0.79	0.60
Age at first birth	18.33	20.59	26.96
Birth by 16	0.36	0.32	0.00
Birth by 17	0.66	0.54	0.00
Birth by 18	0.79	0.54	0.00
Obs	61	33	5
Respondents who cohabitated at 17			
Parnter age: 25th percentile	19	19	19
Parnter age: Median	20	21	21
Duration of partnership	15.89	17.23	14.78
Eventual marriage	0.96	0.88	0.69
Age at first birth	18.30	19.87	22.08
Birth by 16	0.05	0.06	0.00
Birth by 17	0.36	0.41	0.23
Birth by 18	0.64	0.59	0.45
Obs	117	86	32

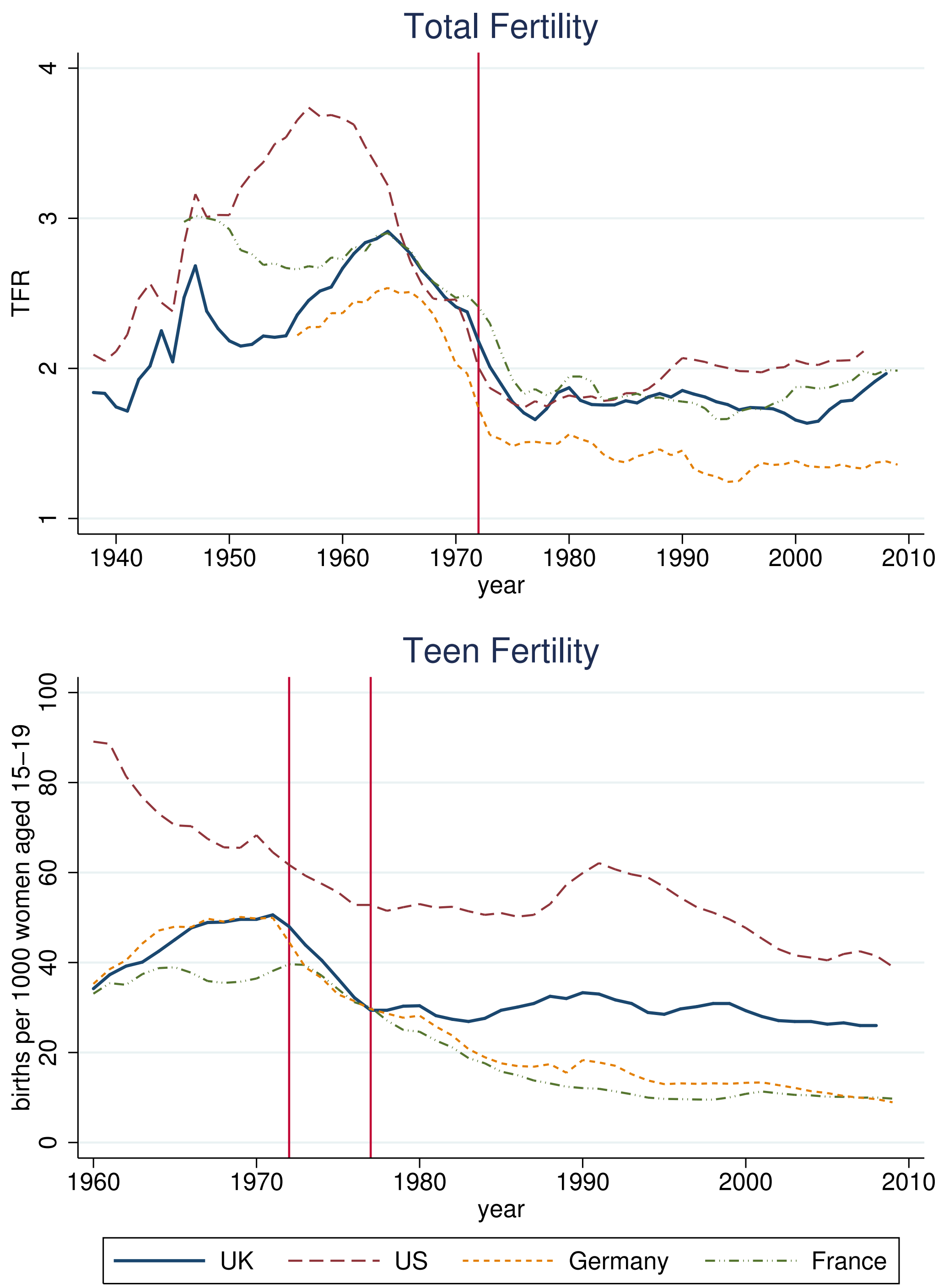
Notes: Sample includes NCDS female respondents. Columns sub-divide sample by preferred school-leaving age, as reported by the NCDS cohort member at the time they were 16. Respondents were asked: "... do you wish that you could have left when you were 15?" Missing cohort member responses are filled in with their parents' responses to the question: "In the study child's case do they wish that he/she had been able to leave school at fifteen?" If both parent and respondent answered "unsure," respondent is coded with a preference for leaving at age 15.

Table 7. Effect of ROSLA on Partner's Educational Credentials

	Woman's Mate			*Man's Mate*		
	No Qualifications	1+ O-levels or 1 A-level	2+ A-Levels or Higher	No Qualifications	1+ O-levels or 1 A-level	2+ A-Levels or Higher
Panel A: Linear cohort trend interacted with threshold						
Discontinuity	-0.0122**	0.0123**	0.0036	-0.0137**	0.0125**	0.0028
	(0.0022)	(0.0016)	(0.0025)	(0.0022)	(0.0021)	(0.0026)
Observations	72	72	72	72	72	72
Bandwidth	36	36	36	36	36	36
Mean Dep Var	0.237	0.361	0.266	0.187	0.516	0.261
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies						
Discontinuity	-0.0123**	0.0124**	0.0028	-0.0143**	0.0140**	0.0017
	(0.0019)	(0.0018)	(0.0021)	(0.0019)	(0.0017)	(0.0019)
Observations	72	72	72	72	72	72
Bandwidth	36	36	36	36	36	36
Mean Dep Var	0.237	0.361	0.266	0.187	0.516	0.261
Panel C: Linear cohort trend interacted with threshold with summer borns dropped						
Discontinuity	-0.0101**	0.0131**	0.0016	-0.0104**	0.0155**	-0.0034
	(0.0026)	(0.0020)	(0.0027)	(0.0026)	(0.0020)	(0.0023)
Observations	54	54	54	54	54	54
Bandwidth	36	36	36	36	36	36
Mean Dep Var	0.237	0.361	0.266	0.187	0.516	0.261

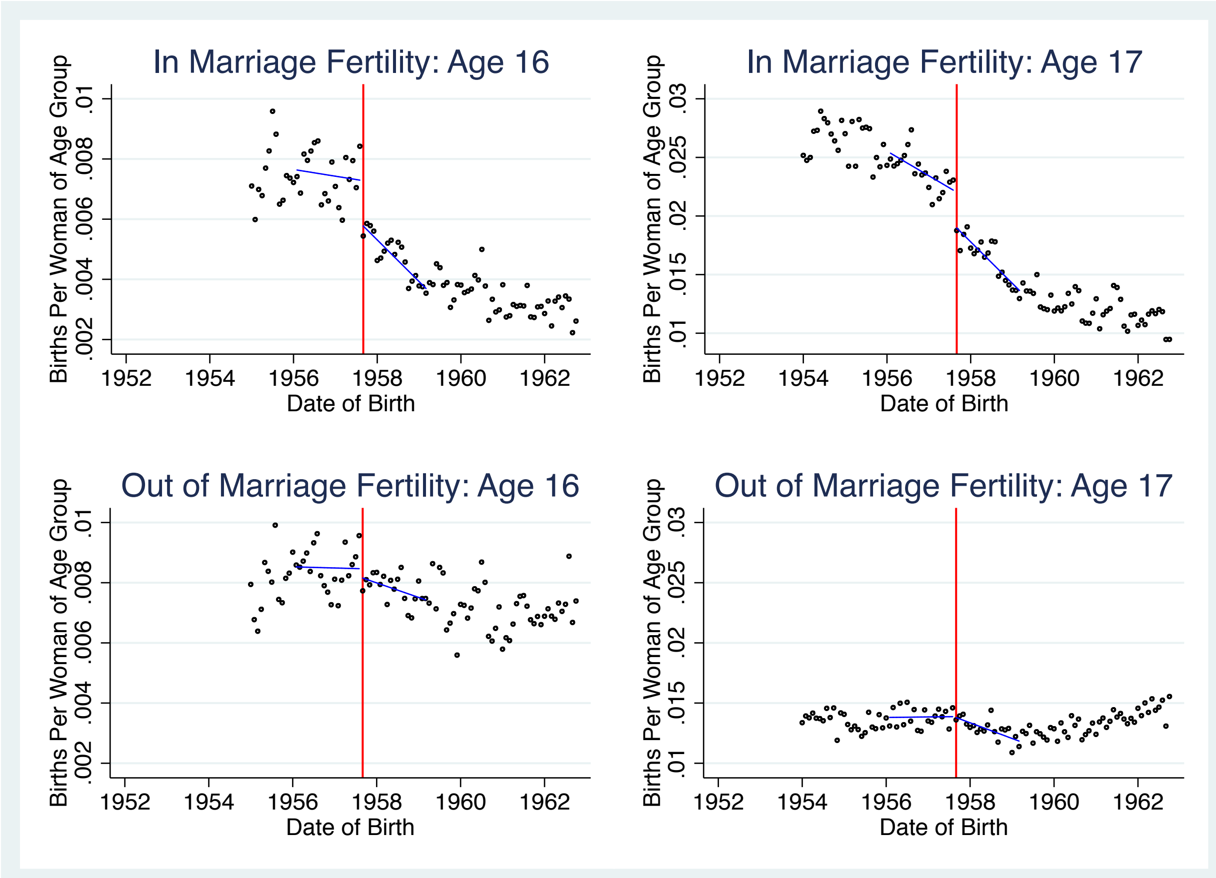
Notes: Each column and row is the regression discontinuity estimate from a separate regression. Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable (Mean Dep Var) is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from the 2001 Census. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. Bandwidth is measured in months. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Appendix Figure 1: Global Trends in Fertility During Study Period



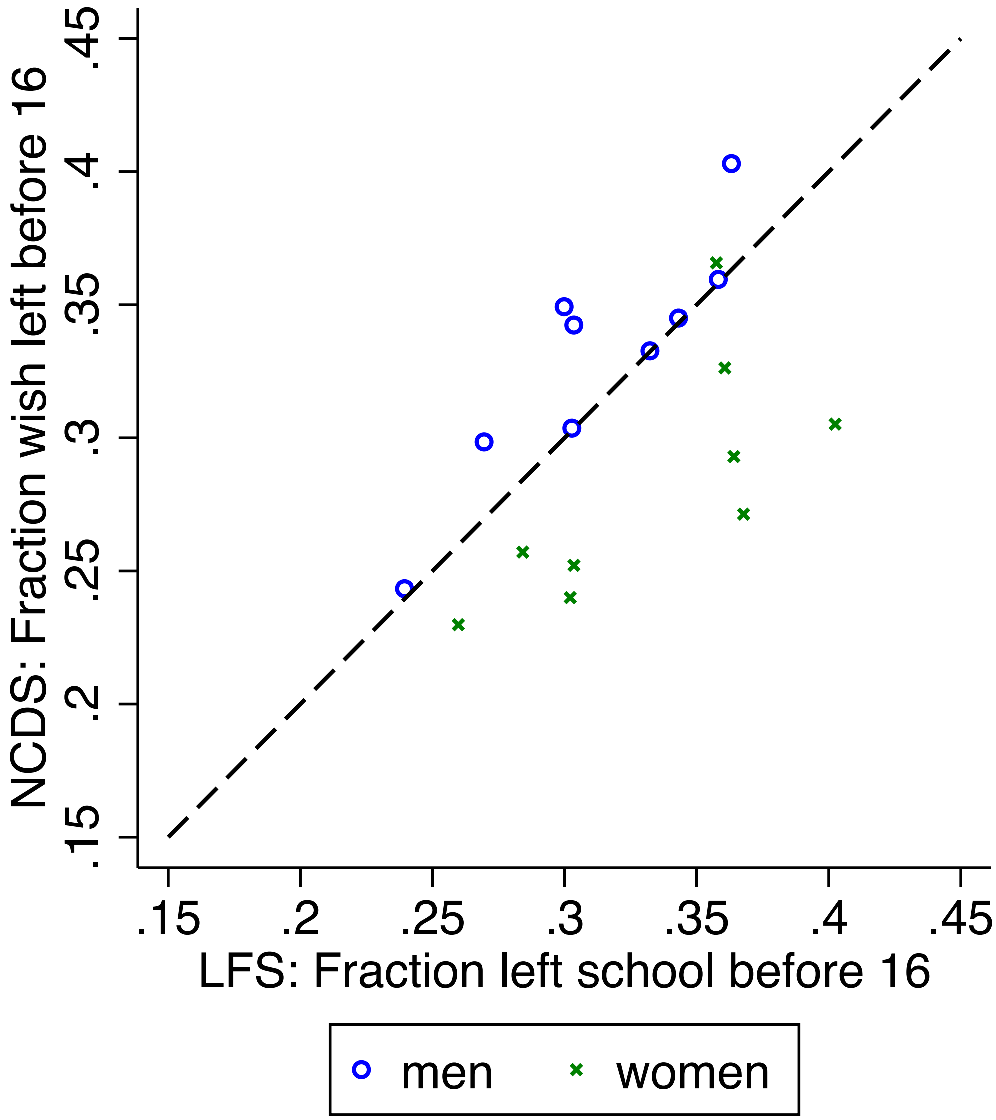
Notes: Total fertility rates in the UK, US, France, and Germany. The vertical line indicates the date of the 1972 ROSLA. The raising of the school leaving age in 1972 came at the tail end of a demographic transition among industrialized nations. Women just affected by the reform experienced their childbearing years (roughly 1972 through 2002) in a period of low, stable fertility. UK data come from the UK Office of National Statistics. US, French, and German data come from the Human Fertility Database.

Appendix Figure 2: Effect of ROSLA on In and Out of Marriage Fertility



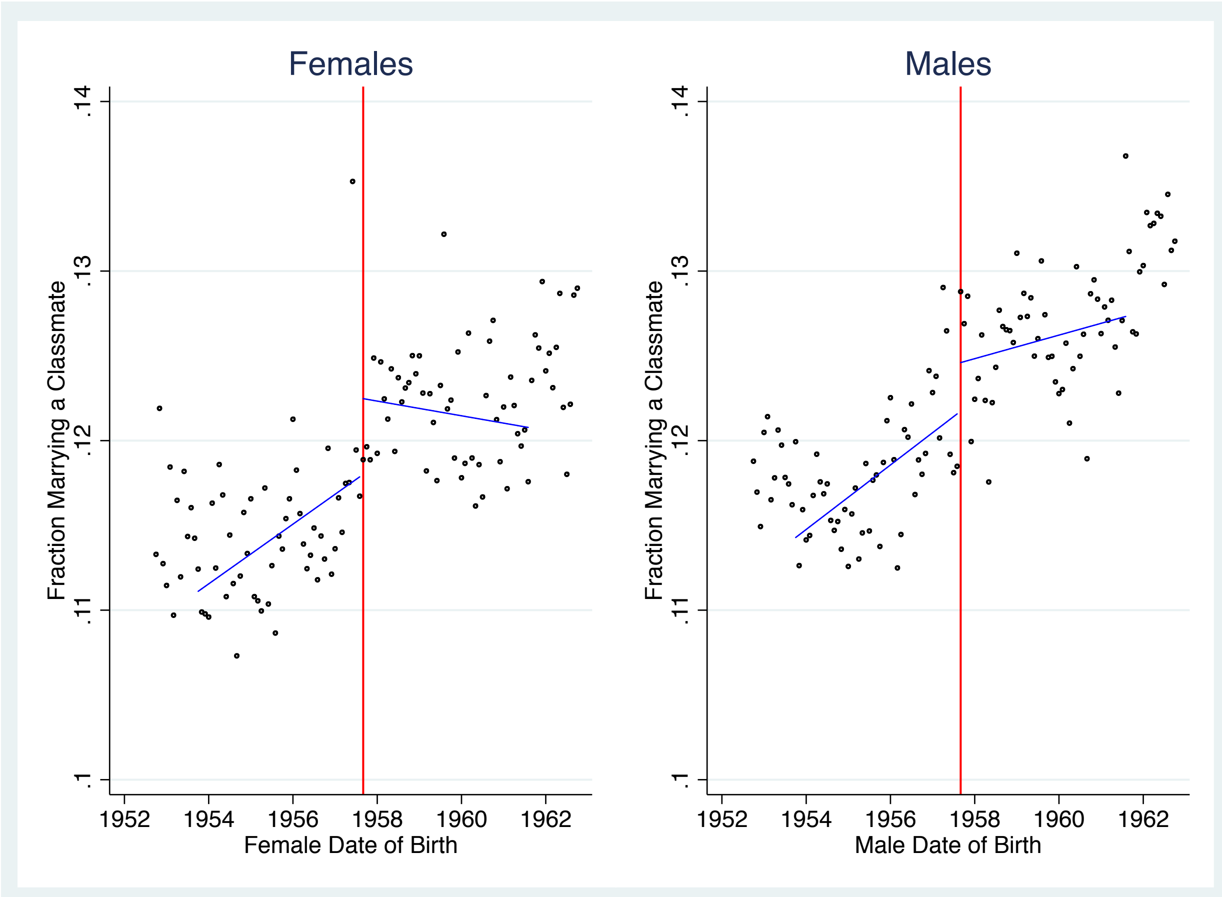
Notes: The impact of the 1972 ROSLA on in marriage and out of marriage fertility. Each point represents the fertility rate (number of births/size of age group) for a cohort of women sharing the same month of birth. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Appendix Figure 3: Self-Reports of Complier Status



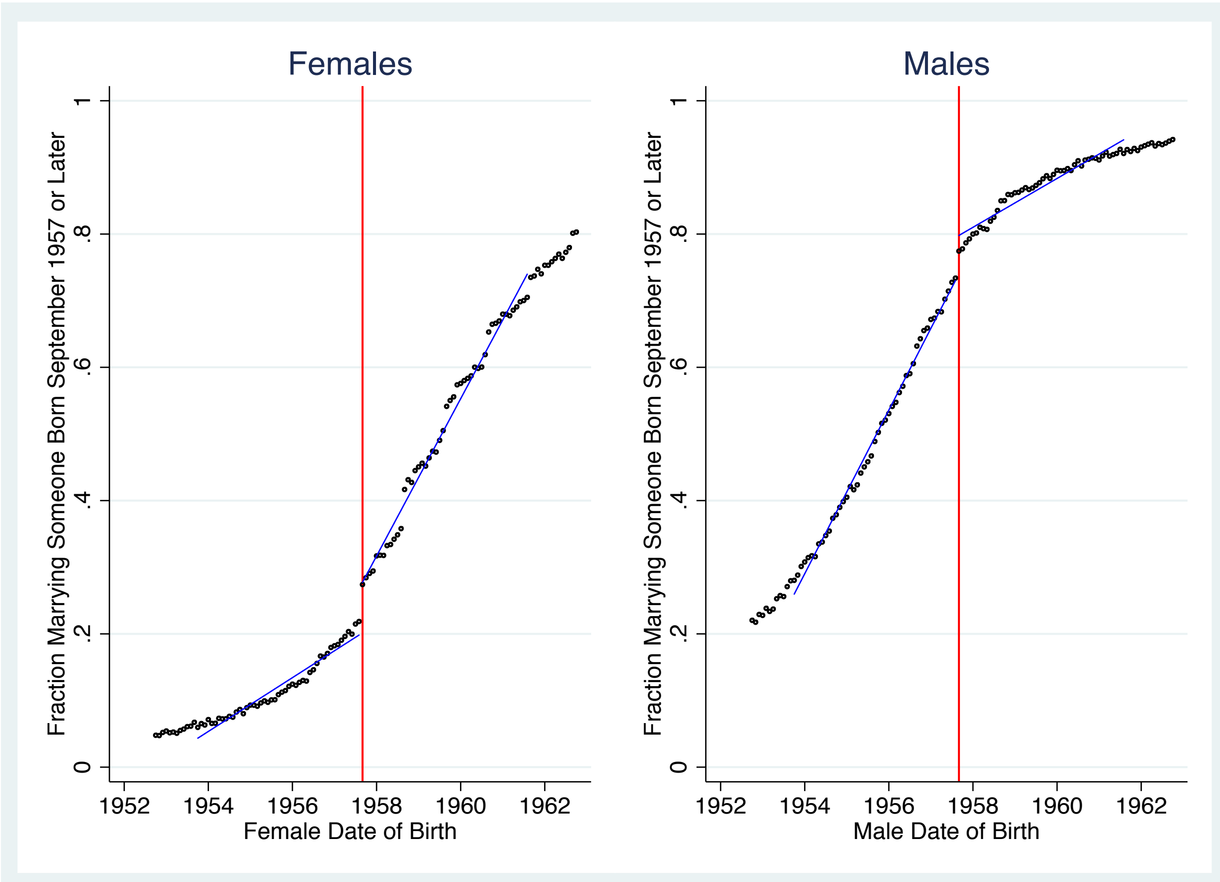
Notes: The vertical axis plots the fraction wishing to leave school before age 16, self-reported by NCDS respondents (born in the post-ROSLA period). The horizontal axis plots the fraction actually exiting from school before age 16, self-reported by LFS respondents who were born in the pre-period 1954-1957. Each point corresponds to a region of the UK interacted with gender.

Appendix Figure 4: Effect of ROSLA on Probability of Marrying Someone in the Same Academic Cohort



Notes: The impact of the 1972 ROSLA on probability of marrying someone in the same academic cohort; women's mates are represented by the left panel and men's mates are represented by the right panel. Each point represents the fraction marrying a classmate for women of the same month of birth cohort (left panel) or men of the same month of birth cohort (right panel). Seasonality adjustments are formed by regressing the cohort fraction on a set of month of birth dummies and taking the residuals and adding the mean fraction marrying a classmate. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Appendix Figure 5: Effect of ROSLA on Probability of Marrying Someone in the Post-ROSLA Cohort



Notes: The impact of the 1972 ROSLA on probability of marrying someone born September 1957 or later; women's mates are represented by the left panel and men's mates are represented by the right panel. Each point represents the fraction marrying someone born September 1957 of later for women of the same month of birth cohort (left panel) or men of the same month of birth cohort (right panel). Seasonality adjustments are formed by regressing the cohort fraction on a set of month of birth dummies and taking the residuals and adding the mean fraction marrying someone born September 1957 or later. The plotted lines are local linear regression fits to those points over the chosen bandwidth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957.

Appendix Table 1: Data Sources

Data Source	Sample	Primary Variables	Notes
UK Labour Force Survey (LFS)	Sample of households in England and Wales	age left full-time education	individual-level
Live Births Records Extract	100% sample of births in England and Wales 1970-2008	live births	counts by mother's date of birth (measured in months) and child's date of birth (measured in months); not an individual-level panel
Abortion Records Extract	100% sample of teen abortions (15-19 years old) in England and Wales	abortions	counts by mother's date of birth (measured in months) and date of procedure (measured in months)
UK Census Extracts	100% sample for the 2001 Census	marital status, partner's age, partner's education	
National Child Development Study (1958 Cohort)	100% sample of children born one week in March 1978	age at which would have dropped out of school if ROSLA had not occurred, activities in school, partnership histories, activities out of school	one of the first post-ROSLA cohorts

Appendix Table 2. Sensitivity of Regression Discontinuity Estimates to Varying Bandwidths

Outcome	Chosen Bandwidth in Months	Chosen Bandwidth - 6	Chosen Bandwidth	Chosen Bandwidth + 6
Left School by Age 15	48	-0.24**	-0.24**	-0.24**
		(0.01)	(0.01)	(0.01)
Left School by Age 16	48	-0.02*	-0.01	-0.01
		(0.01)	(0.01)	(0.01)
Left School by Age 17	48	-0.01	-0.00	-0.00
		(0.01)	(0.00)	(0.00)
Age Left Schooling	48	0.26**	0.24**	0.21**
		(0.04)	(0.04)	(0.03)
Fertility at Age 15	19	-0.0002	-0.0002	-0.0001
		(0.0003)	(0.0003)	(0.0002)
Fertility at Age 16	19	-0.0013	-0.0009	-0.0012*
		(0.0010)	(0.0006)	(0.0005)
Fertility at Age 17	19	-0.0011	-0.0021**	-0.0030**
		(0.0008)	(0.0007)	(0.0006)
Fertility at Age 18	19	-0.0002	-0.0008	-0.0021
		(0.0012)	(0.0011)	(0.0011)
Fertility at Age 19	19	0.0021	0.0004	-0.0015
		(0.0011)	(0.0013)	(0.0012)

Notes: Notes: Each column and row is the regression discontinuity estimate from a separate regression. All regression estimates come from local linear regressions which allow the slope with respect to the running variable to vary pre and post reform. These regressions also include month-of-birth dummies. Robust standard errors in parentheses. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Appendix Table 2 Con't. Sensitivity of Regression Discontinuity Estimates to Varying Bandwidths

Outcome	Bandwidth in Months	Chosen Bandwidth - 6	Chosen Bandwidth	Chosen Bandwidth + 6
In-Marriage Fertility at Age 15	19	0.0000	0.0000	0.0000
		(0.0000)	(0.0000)	(0.0000)
In-Marriage Fertility at Age 16	19	-0.0014*	-0.0011**	-0.0013**
		(0.0006)	(0.0003)	(0.0003)
In-Marriage Fertility at Age 17	19	-0.0014*	-0.0022**	-0.0028**
		(0.0005)	(0.0005)	(0.0005)
In-Marriage Fertility at Age 18	19	0.0014	-0.0005	-0.0018
		(0.0011)	(0.0010)	(0.0009)
In-Marriage Fertility at Age 19	19	0.0021	0.0001	-0.0013
		(0.0010)	(0.0011)	(0.0010)
Out-of-Marriage Fertility at Age 15	19	-0.0002	-0.0002	-0.0001
		(0.0003)	(0.0002)	(0.0002)
Out-of-Marriage Fertility at Age 16	19	0.0001	0.0002	0.0002
		(0.0005)	(0.0003)	(0.0003)
Out-of-Marriage Fertility at Age 17	19	0.0002	0.0001	-0.0002
		(0.0008)	(0.0006)	(0.0004)
Out-of-Marriage Fertility at Age 18	19	-0.0016**	-0.0002	-0.0004
		(0.0005)	(0.0005)	(0.0005)
Out-of-Marriage Fertility at Age 19	19	-0.0000	0.0003	-0.0002
		(0.0003)	(0.0005)	(0.0004)

Notes: Notes: Each column and row is the regression discontinuity estimate from a separate regression. All regression estimates come from local linear regressions which allow the slope with respect to the running variable to vary pre and post reform. These regressions also include month-of-birth dummies. Robust standard errors in parentheses. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Appendix Table 2 Con't. Sensitivity of Regression Discontinuity Estimates to Varying Bandwidths

Outcome	Bandwidth in Months	Chosen Bandwidth - 6	Chosen Bandwidth	Chosen Bandwidth + 6
Abortion at Age 15	19	-0.00031	-0.00036	-0.00047
		(0.00041)	(0.00038)	(0.00031)
Abortion at Age 16	19	-0.00087	0.00045	-0.00024
		(0.00056)	(0.00046)	(0.00049)
Abortion at Age 17	19	-0.00105	0.00008	-0.00001
		(0.00065)	(0.00049)	(0.00041)
Abortion at Age 18	19	0.00018	0.00057	0.00020
		(0.00066)	(0.00047)	(0.00044)
Abortion at Age 19	19	-0.00043	-0.00063	-0.00094
		(0.00079)	(0.00060)	(0.00057)
Cumulative Fertility Ages 16-25	19	-0.0017	-0.0004	-0.0108
		(0.0062)	(0.0055)	(0.0057)
Cumulative Fertility Ages 16-30	19	0.0003	0.0031	-0.0094
		(0.0070)	(0.0059)	(0.0065)
Cumulative Fertility Ages 16-35	19	0.0036	0.0031	-0.0087
		(0.0070)	(0.0056)	(0.0062)
Cumulative Fertility Ages 16-40	19	0.0025	0.0041	-0.0079
		(0.0064)	(0.0058)	(0.0062)
Cumulative Fertility Ages 16-45	19	0.0006	0.0037	-0.0084
		(0.0066)	(0.0059)	(0.0064)

Notes: Notes: Each column and row is the regression discontinuity estimate from a separate regression. All regression estimates come from local linear regressions which allow the slope with respect to the running variable to vary pre and post reform. These regressions also include month-of-birth dummies. Robust standard errors in parentheses. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Appendix Table 2 Con't. Sensitivity of Regression Discontinuity Estimates to Varying Bandwidths

Outcome	Bandwidth in Months	Chosen Bandwidth - 6	Chosen Bandwidth	Chosen Bandwidth + 6
Woman's Mate: No Qualifications	36	-0.0112**	-0.0123**	-0.0118**
		(0.0021)	(0.0019)	(0.0017)
Woman's Mate: 1+ O-levels or 1 A-level	36	0.0117**	0.0124**	0.0148**
		(0.0020)	(0.0018)	(0.0017)
Woman's Mate: 2+ A-Levels or Higher	36	0.0029	0.0028	0.0007
		(0.0022)	(0.0021)	(0.0018)
Man's Mate: No Qualifications	36	-0.0130**	-0.0143**	-0.0119**
		(0.0019)	(0.0019)	(0.0018)
Man's Mate: 1+ O-levels or 1 A-level	36	0.0124**	0.0140**	0.0139**
		(0.0018)	(0.0017)	(0.0015)
Man's Mate: 2+ A-Levels or Higher	36	0.0019	0.0017	0.0000
		(0.0018)	(0.0019)	(0.0016)

Notes: Notes: Each column and row is the regression discontinuity estimate from a separate regression. All regression estimates come from local linear regressions which allow the slope with respect to the running variable to vary pre and post reform. These regressions also include month-of-birth dummies. Robust standard errors in parentheses. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001

Appendix Table 3. Effect of ROSLA on Conception Rates

	Dependent Variable: Conception Rate at Age of Time of Birth			
	16	17	18	19
Panel A: Linear cohort trend interacted with threshold				
Discontinuity	-0.0033**	-0.0050**	-0.0021	-0.0020
	(0.0007)	(0.0008)	(0.0012)	(0.0016)
Observations	48	48	48	48
Bandwidth	24	24	24	24
Mean Dependent Variable	0.0275	0.0548	0.0684	0.0772
Panel B: Linear cohort trend interacted with threshold + month-of-birth dummies				
Discontinuity	-0.0011	-0.0028**	-0.0014	-0.0010
	(0.0007)	(0.0009)	(0.0012)	(0.0016)
Observations	48	48	48	48
Bandwidth	24	24	24	24
Mean Dependent Variable	0.0275	0.0548	0.0684	0.0772
Panel C: Linear cohort trend interacted with threshold with summer borns dropped				
Discontinuity	-0.0023*	-0.0042**	-0.0014	-0.0012
	(0.0010)	(0.0009)	(0.0014)	(0.0018)
Observations	36	36	36	36
Bandwidth	24	24	24	24
Mean Dependent Variable	0.0275	0.0548	0.0684	0.0772

Notes: Each column and row is the regression discontinuity estimate from a separate regression. Conception rates are defined as the sum of birth rates and abortion rates (adjusted to take into account that timing of abortion and births are different, see text for more details). Column headers list the dependent variable. Panel A includes linear controls for cohort trends, interacted with the threshold to allow different pre- and post-reform trends. Panel B adds dummies for month of birth to capture seasonality. Panel C drops summer borns to deal with the fact that they could drop out of school at the end of May of the year they reached the compulsory schooling age. Mean of dependent variable is measured for the cohort born in August 1957, the last pre-reform cohort. Data come from live birth records and abortion records supplied by the UK Office of National Statistics, containing all births in England and Wales to mothers born in England, Wales, and Scotland, 1970-2008. These are supplemented with data on cohort sizes from the Human Mortality Database. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. * :p-value < 0.05, ** :p-value < 0.01, ***: p-value < 0.001