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 (e.g., individuals born in September 1957 had to leave school at age 16 or later whereas individuals born in August 1957 could leave at age 15) using regression discontinuity methods. We show that the affected girls had significantly lower fertility in their teen years. Instrumental variables estimates imply a 20% 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. On the mating market front, the reform induced individuals to marry more educated and younger mates but had no effect on the probability of marriage. Our findings suggest that education-based policies might reduce teen pregnancies without impacting completed fertility rates.

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1 Introduction

In many OECD countries, completed fertility is low—both by historical standards and relative to the replacement levels needed to maintain stable populations. Within these countries, one of the most robust predictors of individual-level fertility is a woman's educational attainment. If the relationship between education and fertility is causal, then the well-known benefits of further increases in educational attainment could be accompanied by real costs in terms of lower fertility rates. Such a relationship would be particularly troubling for areas such as Europe where an already aging population is expected to have serious impacts on generous pay-as-you-go pension systems and public health systems.

Aside from completed fertility, education may affect fertility timing. One important dimension is teen fertility, an outcome 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). Given the strong inverse correlation between educational attainment and teen fertility, it is not surprising therefore that education is regarded as a policy lever for reducing teen fertility.[1]

These fertility effects are only one of a host of important family formation effects of education. Education can affect the mating market (Behrman, Rosenzweig, and Taubman, 1994). Under positive assortative mating, we expect that a rise in one's own educational level would lead to an increase in the educational attainment of the chosen mate.

While partial correlations between education and various fertility and marital market outcomes raise the possibility that education affects fertility, 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.

Economic theory does not provide clear guidance as to direction of the effect of education on fertility. Theory emphasizes mechanisms pushing in both directions. On the one hand, more-educated women will have better labor-market opportunities (Becker 1960; Mincer 1963; Willis 1973). If participation in the labor market precludes women from bearing and raising children, this implies that more-educated women will have lower completed fertility and likely delayed fertility. On the other hand, to the extent that children represent a normal good, wealthier women will choose to have more of them. More-educated women may be wealthier because education has a direct effect on their own earnings and/or because it has an indirect effect on their husband's earnings via assortative matching in the marriage market (Becker 1981).

[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).

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] However, this literature suffers from several weaknesses. Nearly all of this literature with the exception of McCrary and Royer (2011) relies on difference-in-difference analyses, which require stronger assumptions for identification than needed in a regression discontinuity design as we use here. Other studies of compulsory schooling reforms often involve the estimation of the effects of several 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 not possible to evaluate the impact of extending the length of schooling in isolation.

In this paper, we estimate the causal effect of education on completed and teen fertility 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. This 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.

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Finally, 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.

Our analysis reveals four main findings. First, an additional year of schooling had a significant impact on teen fertility, reducing it by around 20% 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.[4] Fourth, the extra schooling led affected individuals to choose more educated partners.

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 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 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 a trend to increased education in the OECD need imply reductions in future levels of completed fertility. Nevertheless, our study does suggest that schooling can be an effective tool in the fight against teen fertility in developed countries.[5] 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. 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.

[4] 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.

[5] 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.[6] They include a maximum age by which children must start school and a minimum age at which children can leave school. The maximum starting age is five and has been so for over a century. In contrast, the minimum leaving age was changed twice in the postwar period. These changes, also known as ROSLAs (Raising of the School Leaving Age), were implemented in 1947 and 1972. Further increases are slotted for the near future.[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), Clark and Royer (2013), Machin et al. (2011) use it to study the impact of education on wages, health, and crime, respectively. We focus exclusively on the 1972 ROSLA as 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. The government announced the change in March 1972 and on 1 September 1972, the change became effective. 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. Though the change was officially announced in 1972, plans and preparations had begun as early as 1964. 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 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.

In Figure 1, we examine visually the impact of the 1972 ROSLA on age at school exit. Specifically, each scatter point in Figure 1 represents the average outcome for a cohort of women who share the

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

[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.

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.

In this figure, we observe that 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.[8] 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.

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). Analyses of Labor Force Study data suggest that some of the apparent non-compliance likely reflects errors in self-reported school leaving age. Because the LFS is a panel, respondents are asked about school leaving multiple times, and over 10% of respondents who report leaving school at age 15 also report leaving school at 16 in a different survey wave. There is also an apparent seasonal pattern of non-compliance 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.

Figure 1 shows that the reform only induced those at risk of dropping out at age 15 to stay in school an additional year. We do not find any effects of the reform on the probability of attending schooling beyond the age of 16. In the analyses below, we examine these effects within a regression framework.

2.2 Fertility Trends

Fertility declined dramatically between the mid-1960s and the mid-1970s in the UK. Figure 2 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.[9]

Aside from total fertility, we examine teenage births, often an explicit target of fertility policies. As seen in Figure 3, teen fertility also declined through the 1960s and 1970s, although in the UK it fell

[8] The data underlying this figure–the UK Labor Force Survey–are discussed in more detail below.

[9] The 1967 Abortion Act legalized abortion in the UK. This Act covered abortions for teenage girls and did not require parental consent or notification.

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.

Given the oft-mentioned detrimental consequences of teen pregnancy and given the problems associated with an aging population, governments are in search of potential fertility policy levers. The UK government has consistently sought to lower teen fertility and in 1998 launched a £260m (roughly 425m in 2010 US dollars) campaign to halve teen fertility by 2010. Similar efforts and funding commitments have been made in the US.[10] In contrast, governments often view *completed* fertility as being too low since below replacement rates imply slower economic growth and increased burdens on entitlement programs. In response many European governments have introduced policies designed to encourage larger families. These include reforms to maternity pay and paternity leave, more flexible hours, and a family tax break (See, for example, Kalwij 2010 for a discussion of these types of pro-natal policies in Western European countries.) With these divergent policy goals in mind, an interesting question is whether schooling can reduce teen fertility without lowering total fertility.

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 A1.

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

[10]The Healthy People 2010 and Healthy People 2020 initiatives, within the US Department of Health and Human Services, both set targets for reducing the pregnancy rate among adolescent females aged 15 to 17 years.

held, though the exact form of these questions differs across LFS survey years. Our pooled sample of 1975-2002 LFS gives us a large sample of over 300,000 respondents born between 1954 and 1962. These data generate Figure 1, which measures the impact of ROSLA on educational attainment.

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.[11] 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, under most scenarios, 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 covariates, in principle, should be similar for females born immediately prior to September 1957 and females born immediately following September 1957.

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.[12]

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 c over 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 (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 (details available from the authors). The total fertility rate (TFR) aggregates the age specific rates. Cohort TFR reflects the average number of children that women in the cohort bear over their lifetime, conditional on surviving through their fecund years.

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[11] 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%).

[12] 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.

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. Nevertheless, 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 multiple extracts of UK Census data. An extract of the 1991 Census provides information on the marital status of all Census respondents. We also commissioned an extract of the 1991 Census that provides information on the age difference between partners in all marriages in which the partners live in the same household. The first extract allows us to determine whether the ROSLA affected the probability of marriage. The second extract allows us to examine the assortative mating effects of the ROSLA.

National Child Development Study. We will 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 will offer insight into cohort members' school activities, partnerships, and labor force participation during the extra year of compulsory schooling and beyond. We are in the process of obtaining this data extract.

4 Empirical Strategy

To exploit the natural experiment represented by the 1972 ROSLA, we estimate versions of the following equations:

$$E_{ic} = \gamma_0 + \gamma_1 D_c + f(c) + u_{ic}$$

$$Y_{ic} = \beta_0 + \beta_1 D_c + g(c) + v_{ic}$$

where E_{ic} is an indicator for whether individual i in cohort c experienced a year of full-time education at age 15, Y_{ic} is a fertility outcome, D_c is an indicator for whether the cohort is affected by the 1972 ROSLA, and f(c) and g(c) are smooth functions of the birth cohorts (low-order polynomials). Cohorts are groups of women born in the same calendar month of the same year. The error terms, u_{ic} and v_{ic} capture the unobservable determinants of E_{ic} and Y_{ic} respectively. The inclusion of f(c) and g(c) will capture underlying time or cohort trends in education and fertility that evolve smoothly.

Because births and abortions data contain counts by month and year of birth, we aggregate to get month-of-birth means in all datasets. We then estimate cohort versions of these equations, weighted by cohort size. In practice, we normalize the cohort variable c so that it equals zero for women born in September 1957, one for women born in October 1957, and so on. This normalization implies that coefficient estimates of γ_1 and β_1 in the regression tables can be directly interpreted as estimates of the ROSLA impact.

The parameters of interest are γ_1 and β_1. They represent, respectively, the ROSLA effects on the probability that a girl experiences a year of education between age 15 and 16 and on the various fertility outcomes. These parameters 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. The identifying assumption underpinning this strategy is that there would have been no such jump in the absence of the 1972 ROSLA. The data sources for education, births, and abortions are separate, so we separately estimate reduced form effects of ROSLA on these outcomes. The fertility effects can be scaled by the first-stage schooling estimates to provide an instrumental variables estimate of the effect of an additional year of education on various fertility outcomes. The ratio β_1/γ_1 is the standard Wald IV estimate in a two-sample instrumental variables strategy (Angrist and Krueger, 1992).

To estimate these equations, we must select the specification of the cohort trend functions g and f and the cohort window over which to estimate them. As discussed and debated extensively in this literature, there are two main ways to model the functions g and f. One is to consider a local linear approach where g and f are linear cohort trends but allowed to vary in shape on either side of the regression discontinuity threshold and data near the threshold are used (i.e., data within a bandwidth of the threshold). This is the approach we adopt most in this paper. We choose the cohort window on either side of the reform according to a cross-validation procedure outlined in Lee and Lemieux (2010). This chooses the window that provides the best trade-off between precision (which results from wide windows) and bias (which is minimized with narrow windows). The other approach, the global polynomial approach, models g and f are low-order polynomials while worrying less about the window of data to use. For robustness, we consider alternative specifications of the cohort trends (e.g., quadratic, cubic, etc.) and with both narrower and wider cohort windows.

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. We discuss other possible violations of the exclusion restriction below.

5 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 teen fertility and cumulative fertility.

5.1 Education

The impacts of the 1972 ROSLA on educational attainment were previewed in Figure 1 above. In Table 1, we further assess these impacts by reporting a series of estimates based on data from the Labor Force Survey, 1975-2002. The sample underlying these estimates is restricted to women born in the UK and residing in England or Wales at the time of the interview. Each entry in the table displays the discontinuity estimate of γ_1, from estimation of an equation similar to (1).

The estimate reported in the first column of panel A of Table 1 is obtained from a regression of the fraction of the cohort exiting school by age 15—that is, before age 16—on a ROSLA dummy and a linear cohort trend interacted with the ROSLA dummy. The estimation sample includes 48 months of birth cohort data on either side of the ROSLA threshold (i.e., 96 months in total). This estimate captures the discontinuity apparent in Figure 1. The estimates in columns (2) and (3) are obtained from similar regressions in which the dependent variable is the fraction of women leaving school by ages 16 and 17 respectively. The column (4) estimate is obtained from a regression in which the dependent variable is the average age of school exit. These first-stage estimates are nearly identical to those of Clark and Royer (2013).[13]

The estimates reported in panel B are obtained from models that include month of birth dummies. These are intended to capture seasonality in educational attainment. To allow the seasonality to differ across the pre- and post-ROSLA periods, the panel C estimates are obtained from models that interact these month dummies with the ROSLA dummy. Since the data are collapsed to the cohort level, regressions are weighted by cohort size and robust standard errors are reported. These estimates are numerically identical to those that would be obtained from individual-level regressions in which standard errors are clustered at the cohort level.

The estimate in column (1) of panel A implies that the ROSLA decreased the population of women exiting school before age 16 by around 25 percentage points. This implies fully one quarter of the female population received an additional year of schooling between ages 15 and 16 as a result of the reform. In contrast, the estimates in columns (2) and (3) imply that the same fraction of women continued to exit school by age 16 after the reform. This suggests that there were no spillovers to education beyond the period of compulsory schooling. As noted above, spillovers would have occurred if girls for whom the reform was binding were somehow induced to stay beyond the new compulsory age or if *other* girls were induced to stay longer, perhaps to differentiate themselves from the new mass of students leaving at 16. The column (4) estimates, which measure average age of school exit, are less precise than those reported in the other columns, but yield a similar insight: on average, the ROSLA increased educational attainment among women by a quarter of a year. Comparisons of panels A, B, and C reveal that the estimates are not particularly sensitive to seasonality controls. Estimates based on different data windows also yield similar results (not reported).

[13] The differences in the point estimates are due to slight differences in specification.

Figure 4 provides an illustration of the robustness of these education effects. This figure corresponds closely to the column (4) estimates. The data points represent month of birth cohorts and a vertical line indicates the ROSLA threshold. The fitted lines represent education levels predicted using the trends estimated in the models without controls for month of birth (i.e., those used to generate the panel A estimates).

In the fertility analysis that follows, we interpret our estimates as the average effect of a year of education at age 15 for women who would not have received this education in the absence of the ROSLA (i.e., a local average treatment effect (Imbens and Angrist, 1994)). While effects for this subset of "compliers" may be different from the population-average effects, they identify effects for a group that is both policy-relevant (i.e., affected by compulsory schooling reforms) and large. For example, this fraction is twice as large as the fraction of girls affected by the Norwegian reform analyzed by Black et al. (2008), and much larger than the fraction of adolescents affected by various state-level compulsory schooling reforms in the US. In later sections, we use the value -0.248 (from column (1), Panel C of Table 1) as the denominator in our Wald IV estimates of the fertility impacts of schooling.

5.2 Teen Fertility

We turn next to ROSLA impacts on teen fertility. Figures 5 and 6 plot fertility rates at ages 16 and 17 among cohorts on either side of the reform. 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). Again, each point in the figure represents the outcome for a particular month-of-birth cohort. A discontinuity associated with the ROSLA can be seen in both figures.

The magnitude and significance of these ROSLA effects is explored in Table 2. This presents estimates for fertility over all teen years, 15 through 19.[14] The only significant effects are found for fertility at age 16 and 17. Since these effects are somewhat sensitive to specification, it helps to view these estimates in the context of Figures 5 and 6. Our preferred estimates are those in panel B, which are based on models that include month of birth dummies to remove seasonality. The panel B cohort trends are those plotted in Figures 5 and 6.[15] Even the smaller estimates in panel B correspond to large fertility effects at ages 16 and 17. For example, the age 17 effect in panel B (-.00209) corresponds to a 5.8% decrease in fertility at age 17. After scaling by the first stage effect of ROSLA on schooling (a 25 percentage point increase in the fraction of women attending school for another year at age 15), the implied Wald IV estimate is a 23% reduction in age 17 fertility. The age 16 effect, which is significant in panel A but not in panel B (p-value of 0.14), is of a similar

[14] 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 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.
[15] These trends are estimated with month of birth dummies, but only the trends, not the dummies, are plotted.

magnitude. The coefficient on age 16 fertility in panel B corresponds to a 5.5% reduced form effect and a 22% Wald IV effect.

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.[16]

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 month. 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 5.

Columns (4) and (5) of Table 2 indicate that there were no ROSLA impacts on fertility at ages 18 and 19. Figure 7 provides visual. Consistent with the estimates in Table 2, neither of the panels in Figure 7 (for fertility at ages 18 and 19) suggests a discontinuity at the ROSLA threshold.

There does, however, appear to be a significant change in the *slope* of the age-19 fertility trend near this threshold. Upon closer inspection this represents a calendar time effect occurring around 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 Figure 3 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.

5.3 Cumulative Fertility

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 (see the Appendix Figure A1 for results by single year of age). For a more succinct summary of these effects, and to see how they contribute to effects on completed family size, we now focus on cumulative fertility.

[16] Estimates based on different data windows and polynomial specifications yield similar results. For instance, the regression discontinuity point estimates for a third degree polynomial that includes calendar month-of-birth dummies interacted with the reform dummy are 0.0007, 0.0004, -0.0072, 0.0003, -0.0006 for the ages of 15, 16, 17, 18, and 19, respectively. The estimate for age 17 is the only one that is statistically significant at the five percent significance level.

To begin, Figure 8 plots the RD 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).[17] 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%, we view our cumulative fertility measure as a good proxy for average completed family size.

Figure 8 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. Effect sizes and standard errors are presented in Table 3, which follows the same format as earlier tables. The reduced form effects implied by the graph and shown in column (5) of Table 3 are on the order of +0.002 children. Even after this reduced form effect is scaled by the ROSLA effect on education, it is only +0.008 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 estimate is not only an order of magnitude smaller, but also takes the wrong sign, although these wrong-signed estimates are indistinguishable from zero.

The finding of no completed fertility effect is robust to alternative regression specifications. Our main estimates, reported in Table 3, are based on models that use an interacted linear specification over a 19-month window, with and without dummies for month of birth. For completeness, we also include a cross-validation figure (Appendix Figure A2) that plots 12 different estimates based on alternative data windows, and in no case is the coefficient even marginally significant. In all cases, the standard errors are small.[18]

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 these "third variables" on both education and family size.

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. Yet 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

[17] 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. For example, Figure 2 plots period TFR, which sums age-specific rates of different cohorts in order to give a summary measure of fertility in each year.

[18] In results not shown, these results are fully robust to alternative polynomial controls for cohort trends.

story under which the teen fertility effects lead to a permanent (albeit tiny) reduction in completed fertility.

These possibilities can be seen in Figure 9, which aggregates information from 30 distinct RD estimates. Each point represents an estimate of the cumulative fertility effect from 16 up to the indicated age. The left panel displays levels, the right panel logs. 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 higher number of births at older ages. This is consistent both with teen effects being counteracted by post-teen effects and with teen effects generating permanent reductions in completed fertility.

6 Discussion

In this section we discuss various explanations for our two main findings: sizable ROSLA effects on fertility at ages 16 and 17 and negligible ROSLA impacts on completed fertility. Note first that there is no contradiction between these results. Even if the teen fertility effects were permanent, they would have a tiny effect on completed fertility, one well within the already tight confidence intervals associated with our estimates. That is because teen fertility is such a small component of completed fertility. Put differently, we cannot hope to determine whether the teen fertility effects are permanent or whether positive ROSLA effects on post-teen fertility offset negative ROSLA effects on teen fertility.

6.1 Teen Fertility

At first glance, 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. Therefore, we now examine the timing of fertility effects relative to the timing of the new compulsory schooling period in more detail.

This conjecture that the reduction in pregnancies carried to term is influenced by *current* school attendance is supported by the analysis presented in Figure 10. Each panel plots ROSLA impacts on fertility rates defined over a succession of three-month intervals; the different panels correspond to different regression specifications. 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 sample members, this additional compulsory schooling occurred between exact ages 15.5 and 16.5.[19] 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. All three specifications in the figure yield similar results with regard to the timing of effects.

Fertility reductions that occurred 9-12 months after the new compulsory schooling period must have resulted from some combination of reduced conception and increased termination during the compulsory schooling period. Increased termination is a plausible hypothesis: abortion was legalized by the time the ROSLA was implemented and bringing a pregnancy to term might have been less desirable for girls with several months of compulsory education left to complete.[20]

In fact, the data suggest that there were no ROSLA effects on the rate of termination. This can be seen in Figure 11, which plots abortion rates at ages 16 through 19. Figure 11 reveals no discernable effect on abortion rates of the 1972 ROSLA. Regression results (not presented) confirm the visual evidence in the plots, indicating no significant discontinuity at ages 16 through 19 under a variety of specifications. This stands in contrast to the birth rate effects at 16 and 17.

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. Aggregate statistics from England and Wales show that most marriages among teen girls in this time period (including almost all teen shotgun weddings) are made with men two or more years older. To shed light on this "incarceration" 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 the types of activities that they engaged in. In future work, we plan to explore the NCDS data with a view to gathering this type of evidence. For example, the NCDS includes data on work and partnership histories, and this explanation about peer groups will be less plausible if we find that many ROSLA-affected girls nonetheless worked part-time and/or partnered with older men.

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. 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

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[19] 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.

[20] Cohorts at the threshold of the reform turned 16 in 1973, when abortion rates among teens 16 and 17 in England and Wales were 15 and 18 abortions per thousand women, respectively. By comparison, the 2010 the pooled abortion rate among 16-17 year-olds was 19 abortions per thousand women. Thus, women at the threshold of the reform experienced their teen years when utilization of abortions had already climbed to today's levels.

informational advantage beyond the new compulsory schooling period, resulting in reduced fertility at later ages, including at 18 and 19.

6.2 Completed Fertility

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. On face value, this also 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.

It is easy to explain the difference between our results and least squares estimates of education's impact on total fertility that suggest a strong negative relationship. 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 to their role in society.

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.

6.3 Mating Market Impacts

A second important assumption of classic fertility models is that education does not affect family formation: two-person households form exogenously and education levels influence decision-making within the household. In practice, it seems that women's education might exert a strong influence on whether and to whom they get married. Becker (1981) has since formalized some of these ideas and there is now good evidence to suggest that more-educated women are more likely to marry more-educated and higher-earning men (Lefgren and McIntyre 2006). From the perspective of subsequent fertility, this is likely to exert a second income effect, such that the combined income effects could either cancel out or dominate any opportunity cost effects.

To provide some UK evidence on this question, we commissioned a Census extract that contains details of the education levels of both partners in all marriages recorded in the 1991 Census. First, we estimated ROSLA effects on the probability of being married. Our estimates suggested that the ROSLA had no impact on the probability of being married. Next, we looked for ROSLA impacts on the partner's education level. The results can be seen in Figure 12, which plots the highest educational qualifications attained by the male spouse. As in previous figures, it is the date of birth of each *female* cohort that defines the horizontal axis. The left panel of Figure 12 plots fraction of each cohort of women who married men with no qualifications. The right panel plots the fraction who married men with some O level qualifications, which would be based on national tests at the end of grade 10. The figure shows a jump across the reform. These effects are explored in more detail in Table 4. Column (1) shows that women affected by ROSLA were less likely to marry men with no qualifications. Columns (2) through (5) indicate that they instead married men with some O level qualifications. Note these effects are likely not mechanical as we estimate no change in the probability of marrying someone in one's educational cohort.

These effects on spousal education levels are fairly large. For example, the effect on "no qualifications," once scaled by the first stage, implies that an extra year of compulsory schooling reduces by five percentage points the probability of marrying a man with no 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 women's education on household income.

Since the reform affected men and women, some of this spousal education effect will result from ROSLA-affected women being more likely to marry ROSLA-affected men, irrespective of their preferences for spousal education or opportunities to meet more-educated potential husbands. Policies that change women's education levels without changing those of men 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.

7 Conclusions

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 education. It is not clear what drives this effect, although a leading explanation is that schooling has a type of incarceration effect similar to that documented by Jacob and Lefgren (2003) in relation to crime.

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.

This reform had other effects on family formation through positive assortative mating. The cohort of women forced to stay in school a year longer married more educated mates – likely further augmenting family well-being.

Our findings are important because policy-makers in both the UK and US are considering 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.

Effects of ROSLA on Age of Exit from School

	left school by age 15 (1)	left school by age 16 (2)	left school by age 17 (3)	age left full-time ed (4)
Panel A: Linear cohort trend interacted with threshold				
discontinuity	-0.270***	-0.0246*	-0.0105	0.305***
	(0.0191)	(0.00975)	(0.00677)	(0.0623)
cohorts	96	96	96	96
Panel B: Month of birth indicators included				
discontinuity	-0.244***	-0.0117	-0.00333	0.235***
	(0.00982)	(0.00714)	(0.00476)	(0.0357)
cohorts	96	96	96	96
Panel C: Month of birth indicators interacted with threshold included				
discontinuity	-0.248***	-0.0129	-0.00198	0.255*
	(0.0120)	(0.0103)	(0.0104)	(0.107)
cohorts	96	96	96	96

Table 1: Effects on the age of school-exit of the 1972 ROSLA, which raised the compulsory schooling age to 16. Each column and row is the 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 interacts the month dummies with the threshold to allow this seasonality to differ pre- and post-reform. Column 1 shows that ROSLA decreased the population of girls exiting school before age 16 by around 25 percentage points. There was little spillover to educational attainment beyond age 16, as columns 2 and 3 indicate no significant fraction of girls were induced to stay on beyond age 16. 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. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Effects of ROSLA on Teen Fertility

	age 15 fertility rate (1)	age 16 fertility rate (2)	age 17 fertility rate (3)	age 18 fertility rate (4)	age 19 fertility rate (5)
Panel A: Linear cohort trend interacted with threshold					
discontinuity	-0.000227	-0.00182*	-0.00313***	-0.00133	-0.000168
	(0.000186)	(0.000709)	(0.000766)	(0.00122)	(0.00145)
cohorts	38	38	38	38	38
Panel B: Month of birth indicators included					
discontinuity	-0.000160	-0.000867	-0.00209**	-0.000753	0.000412
	(0.000252)	(0.000568)	(0.000718)	(0.00106)	(0.00129)
cohorts	38	38	38	38	38

Table 2: Fertility effects of the 1972 ROSLA, showing a significant impact on fertility outcomes at 16 and 17. Each column and row is the 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. 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. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Effects of ROSLA on Cumulative Fertility

	Cumulative fertility ages 16-25 (1)	Cumulative fertility ages 16-30 (2)	Cumulative fertility ages 16-35 (3)	Cumulative fertility ages 16-40 (4)	Cumulative fertility ages 16-45 (5)
Panel A: Linear cohort trend interacted with threshold					
discontinuity	-0.00631	-0.000526	0.00220	0.00367	0.00358
	(0.00733)	(0.00725)	(0.00595)	(0.00584)	(0.00578)
cohorts	38	38	38	38	38
Panel B: Month of birth indicators included					
discontinuity	-0.000387	0.00311	0.00315	0.00411	0.00370
	(0.00552)	(0.00588)	(0.00562)	(0.00579)	(0.00585)
cohorts	38	38	38	38	38

Table 3: Effects of the 1972 ROSLA on cumulative fertility at various ages. Each column and row is the discontinuity estimate from a separate regression. There are no significant effects for any of these cumulative measures, indicating the ROSLA had no impact on family size from age 25 to 45. 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. Cumulative fertility at each age x is the sum of age-specific fertility rates from 16 to x, inclusive. 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. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Husband's Highest Qualifications

	No Quals (1)	O-Levels 1 to 4 (2)	O-Levels 5 or more (3)	O-Levels any number (4)	A-Levels or higher (5)
Panel A: Linear cohort trend interacted with threshold					
discontinuity	-0.0122***	0.00588***	0.00638***	0.0123***	0.00360
	(0.00223)	(0.00158)	(0.00145)	(0.00159)	(0.00250)
cohorts	72	72	72	72	72
Panel B: Month of birth indicators included					
discontinuity	-0.0123***	0.00578**	0.00665***	0.0124***	0.00278
	(0.00191)	(0.00184)	(0.00166)	(0.00182)	(0.00208)
cohorts	72	72	72	72	72

Table 4: Effects on husband's qualifications. The window of estimation is 72 months–36 on either side–and was chosen via a cross-validation function. 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. Data come from the 2001 Census. Each observation is a cohort-cell mean. Regressions are weighted by cohort sizes. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Data Source	Sample	Primary Variables	Notes
UK Labour Force Survey (LFS)	Sample of households in England and Wales 1973-2002	age left full-time education	individual-level
Live Birth Records Extract	100% sample of births in England and Wales 1970-2008	live births	counts by mother's dob and child's dob; not an individual-level panel
Abortions Records Extract	100% sample of teen abortions in England and Wales 1970-1979	abortions	counts by mother's dob and date of procedure
UK Census Extracts	100% sample 1991 and 2001	marital status, partner's age, partner's education	
1971 Longitudinal Study	1% sample of 1971 Census, followed through 2001	educational attainment, childbearing	pre-ROSLA Cohorts; forthcoming in future drafts
National Child Development Study (1958 Cohort)	100% sample of children born one week in March 1978	activities in school, partnership histories, activities out of school	one of the first post-ROSLA Cohorts; forthcoming in future drafts

Table A1: Data sources used in the study.

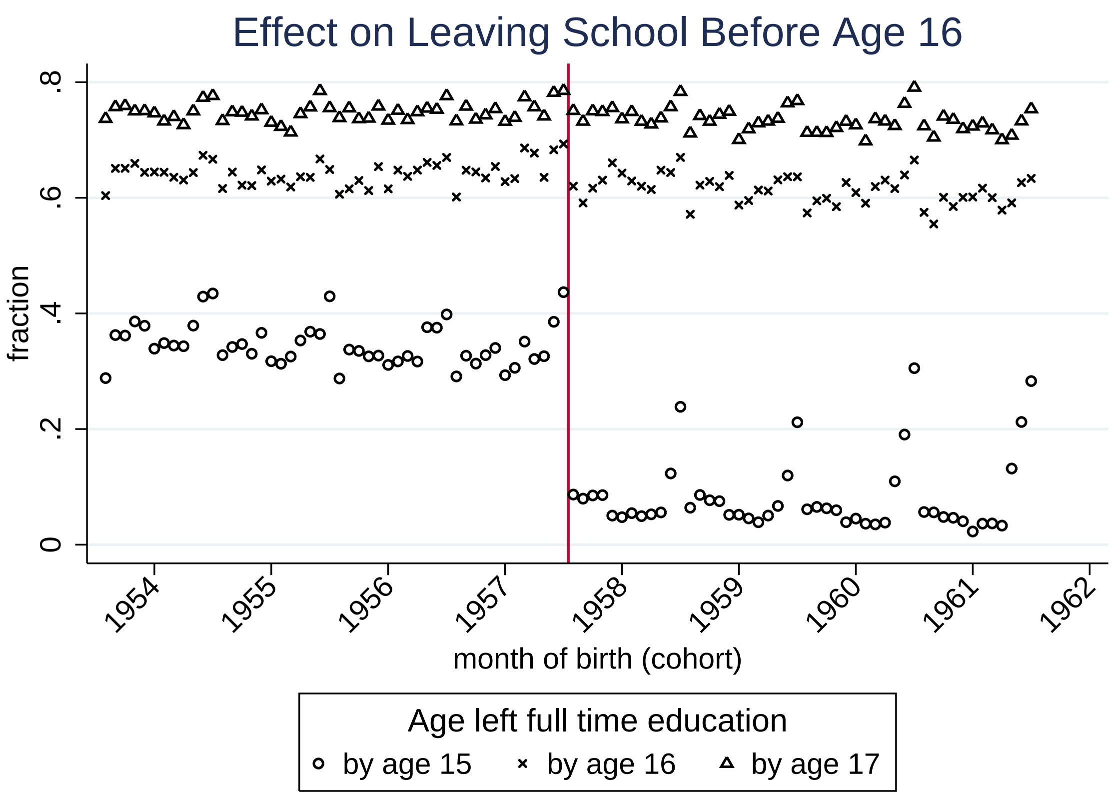

Figure 1: The impact of the 1972 ROSLA on educational attainment. Each point represents a cohort of women sharing the same month of birth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957. The three trends in the figure trace the fraction of women who left school by age 15, by age 16, and by age 17. Age 15 corresponds to grade 9 and age 16 corresponds to grade 10. The discontinuity at the threshold implies that around 25% of the female population received an additional year of schooling between ages 15 and 16 as a result of the reform. 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.

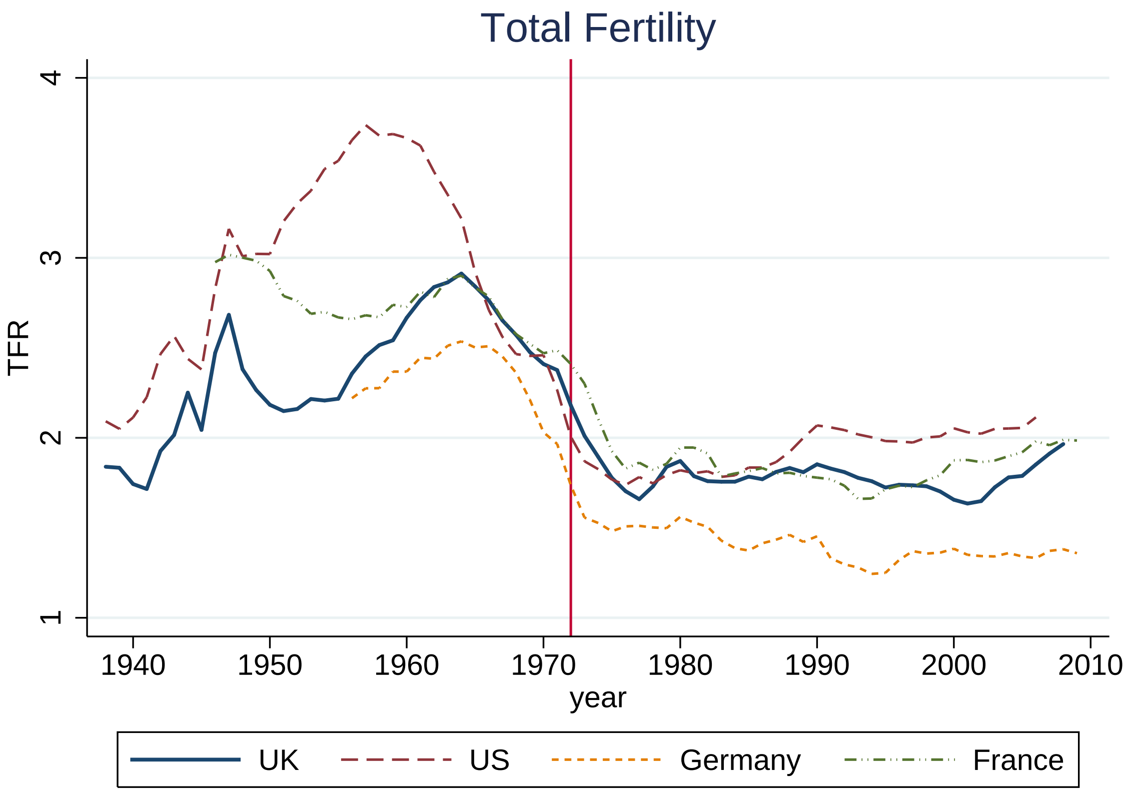

Figure 2: Total fertility rates in the UK, US, France, and Germany. The vertical line indicates the date of the 1972 ROSLA. The raising of the school leaving age in 1972 came at the tail end of a demographic transition among industrialized nations. Women just affected by the reform experienced their 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.

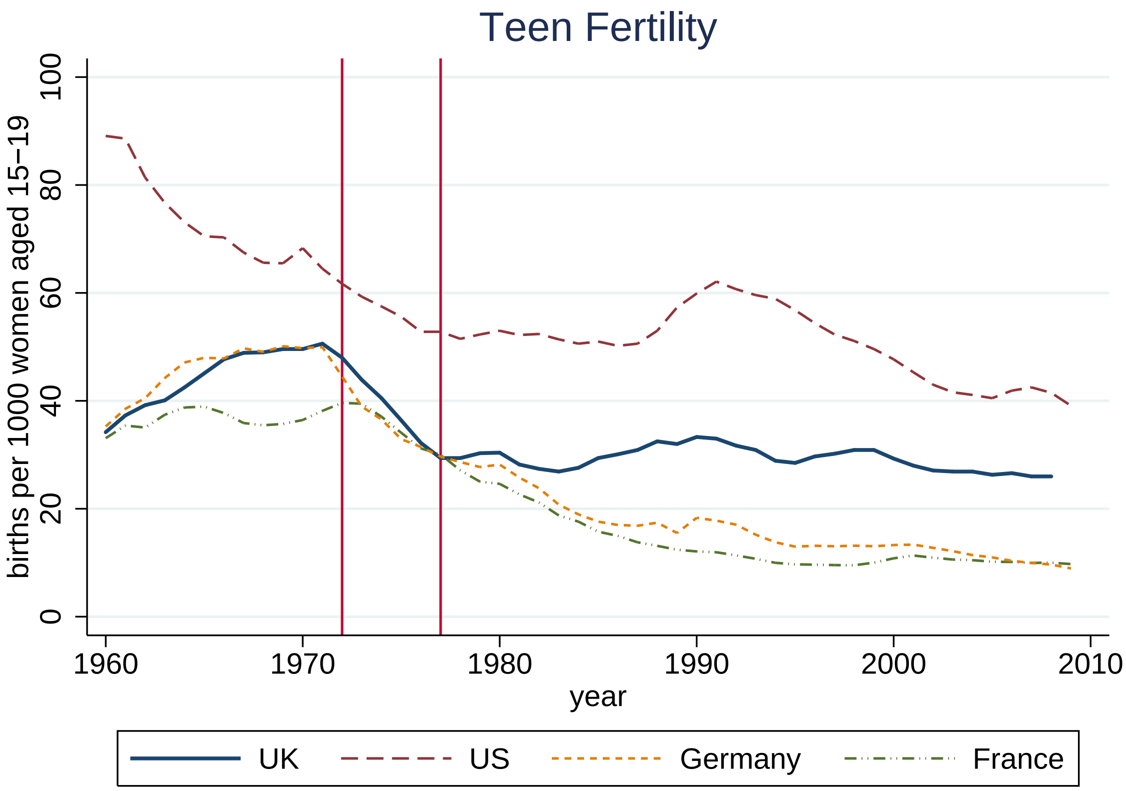

Figure 3: Teen fertility rates in the UK, US, France, and Germany. The vertical axis counts births per 1000 women aged 15 to 19. Comparisons of trends in the UK, US, France and Germany indicates that the steep downward trend in teen fertility through the mid-1970s was not a phenomenon limited to the UK, which experienced an educational reform in 1972. The two vertical lines delimit the years during which women born on the cusp of the educational reform were aged 15 to 19 (1972 to 1977). UK data come from the UK Office of National Statistics. US data come from the US National Center for Health Statistics. French and German data come from the Human Fertility Database.

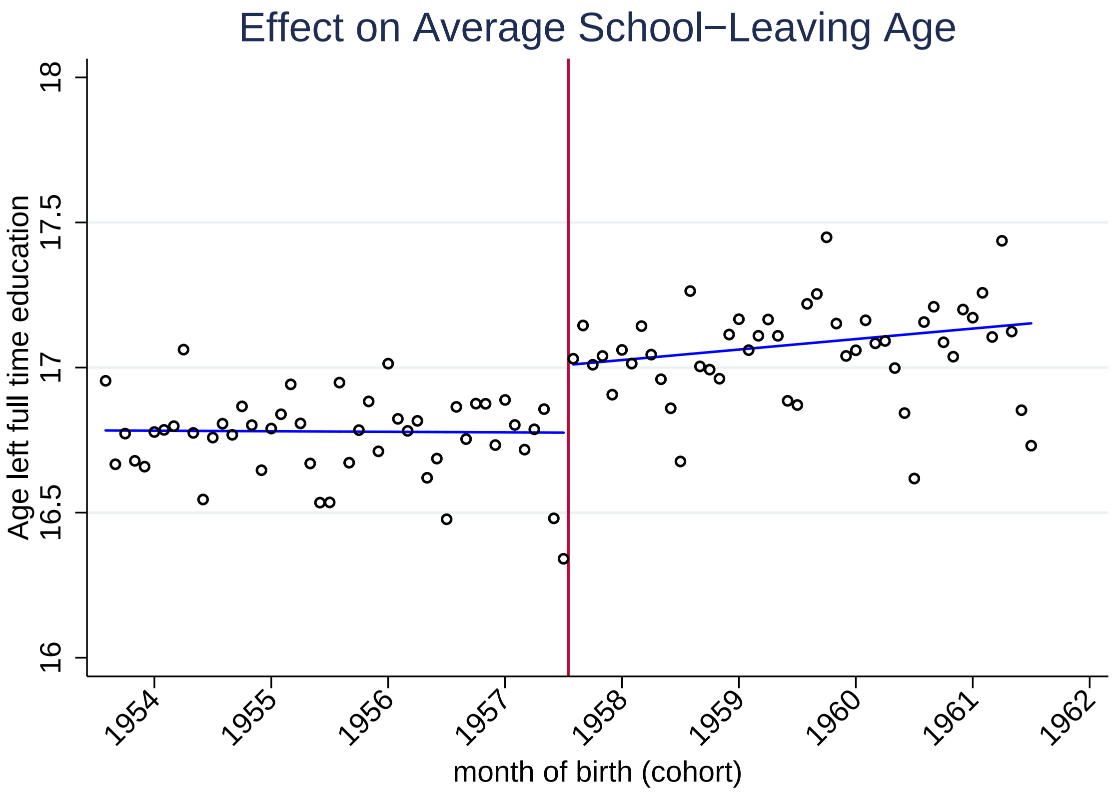

Figure 4: The impact of the 1972 ROSLA on average age of school exit. The discontinuity at the threshold implies that average age of school exit increased by around a quarter year as a result of the reform. Each point represents a cohort of women sharing the same month of birth. The vertical line indicates the ROSLA threshold, with the first post-reform cohort born September 1957. The fitted solid lines control for cohort trends, and are estimated over two 48-month windows on each side of the reform. 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.

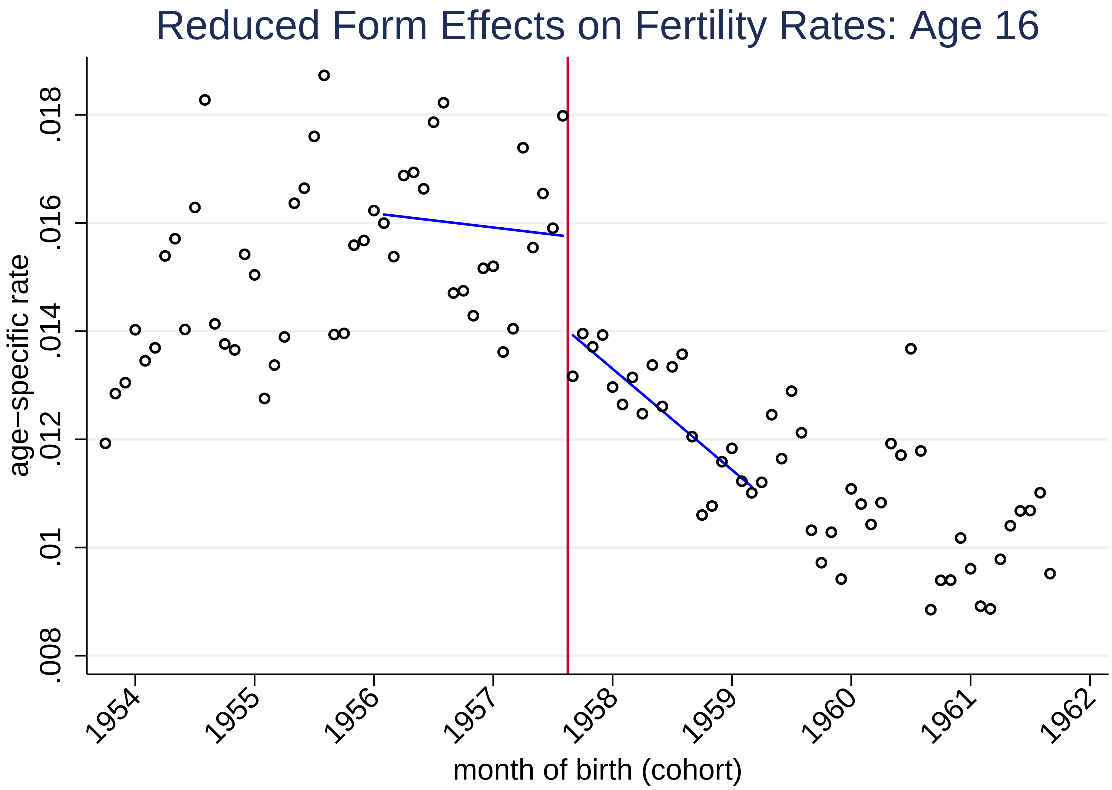

Figure 5: The reduced form impact of the 1972 ROSLA on fertility at age 16. The discontinuity at the threshold corresponds to roughly a 6% reduction in fertility. Once this reduced form effect is scaled by the first stage, the resulting Wald IV estimate indicates that among women kept in school an additional year by the reform, age 16 fertility was reduced by more than 20%. Each point represents a cohort of women sharing the same month of birth. The fitted solid lines control for cohort trends, and are estimated separately in a 19-month window on each side of the reform. The fertility rate at age x is defined as the number of children born to women aged x, divided by the number of women in the cohort alive at age x.

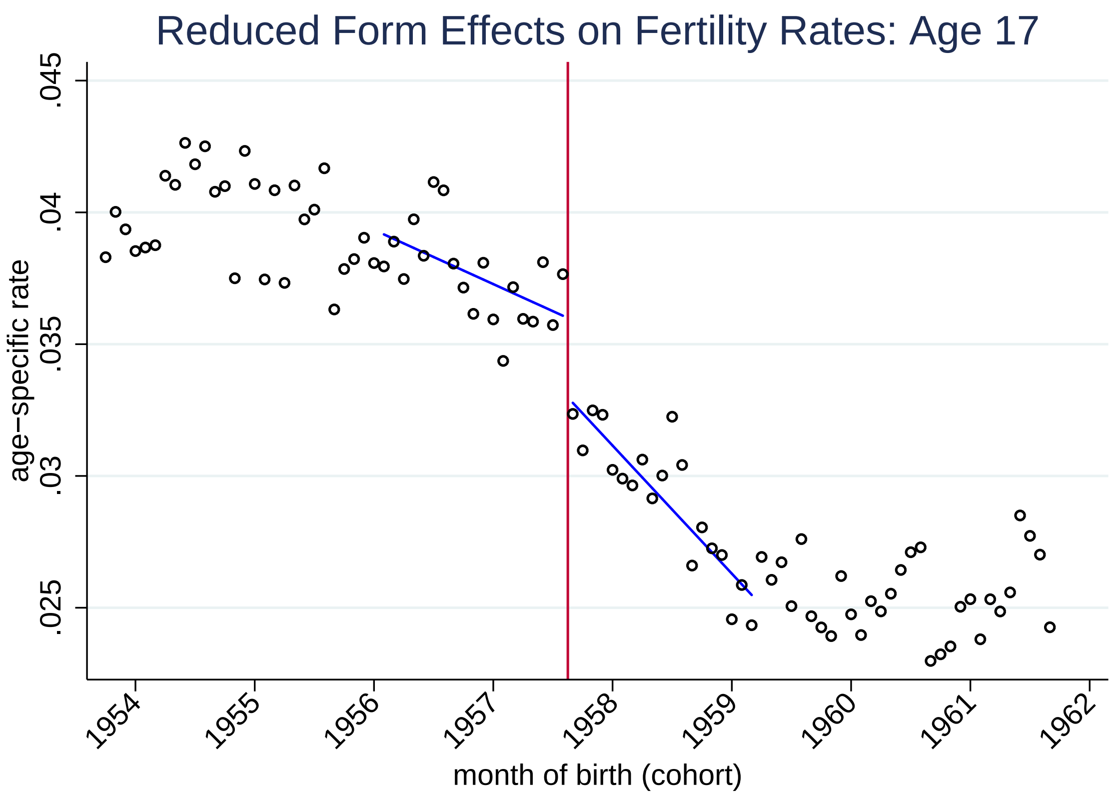

Figure 6: The reduced form impact of the 1972 ROSLA on fertility at age 17. The discontinuity at the threshold corresponds to roughly a 6% reduction in fertility. Once this reduced form effect is scaled by the first stage, the resulting Wald IV estimate indicates that among women kept in school an additional year by the reform, age 17 fertility was reduced by more than 20%. Each point represents a cohort of women sharing the same month of birth. The fitted solid lines control for cohort trends, and are estimated separately in a 19-month window on each side of the reform. The fertility rate at age x is defined as the number of children born to women aged x, divided by the number of women in the cohort alive at age x.

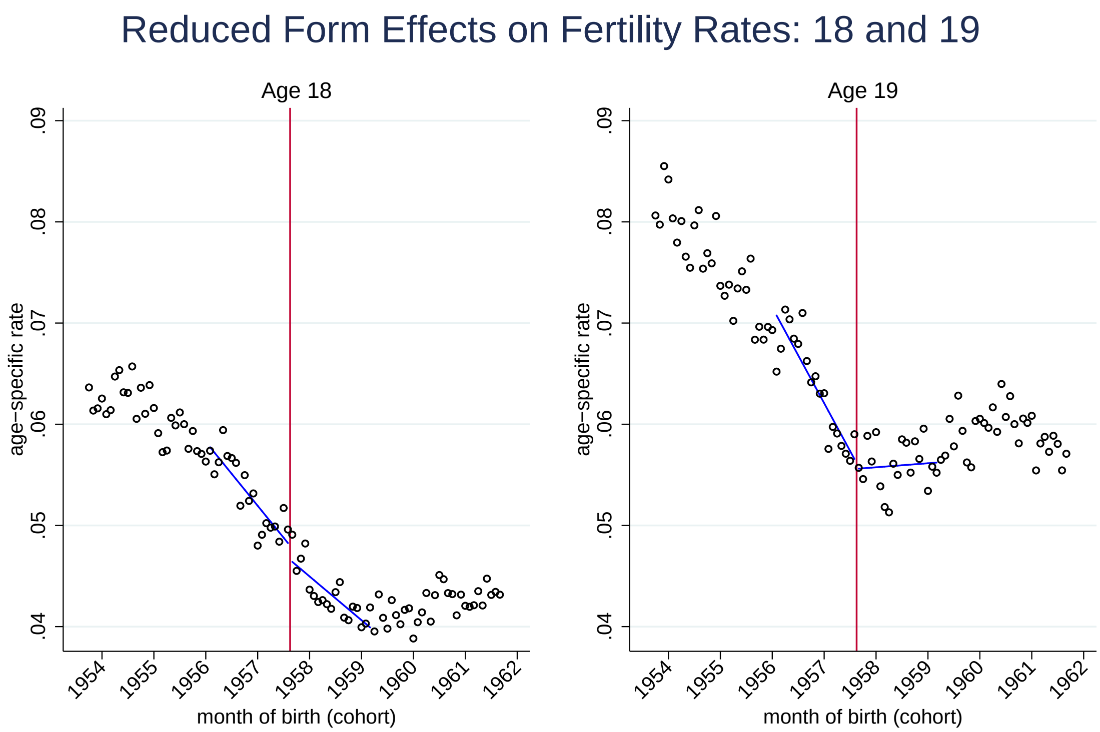

Figure 7: The reduced form impact of the 1972 ROSLA on fertility at ages 18 and 19. The lack of discontinuity at the thresholds in both panels suggests the 1972 ROSLA had no effects on fertility at ages 18 and 19. Each point represents a cohort of women sharing the same month of birth. The fitted solid lines control for cohort trends, and are estimated separately in a 19-month window on each side of the reform. The fertility rate at age x is defined as the number of children born to women whose age-last-birthday was x, divided by the number of women in the cohort alive at age x.

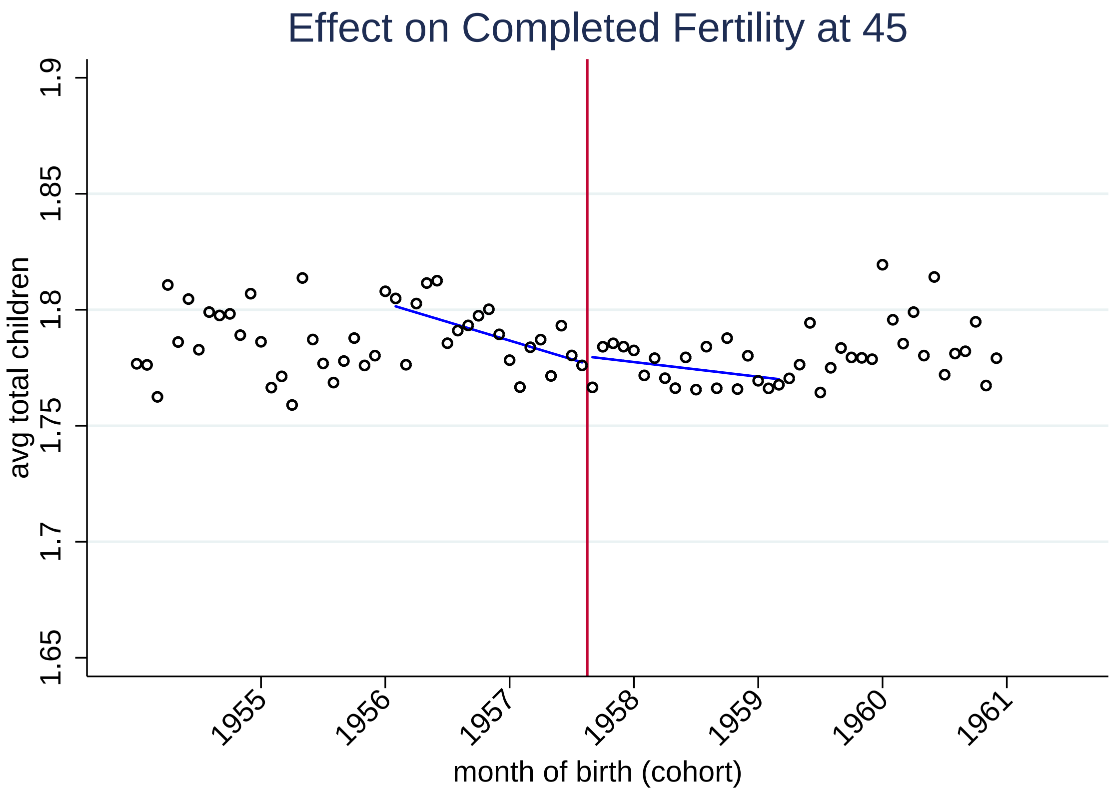

Figure 8: The reduced form impact of the 1972 ROSLA on cumulative fertility to age 45. The implied discontinuity and its associated standard error rule out even small effects of the reform on completed fertility. Each point represents a cohort of women sharing the same month of birth. The fitted solid lines control for cohort trends, and are estimated separately in a 19-month window on each side of the reform. The completed fertility rate at age 45 is defined as the sum of age specific rates from 16 to 45.

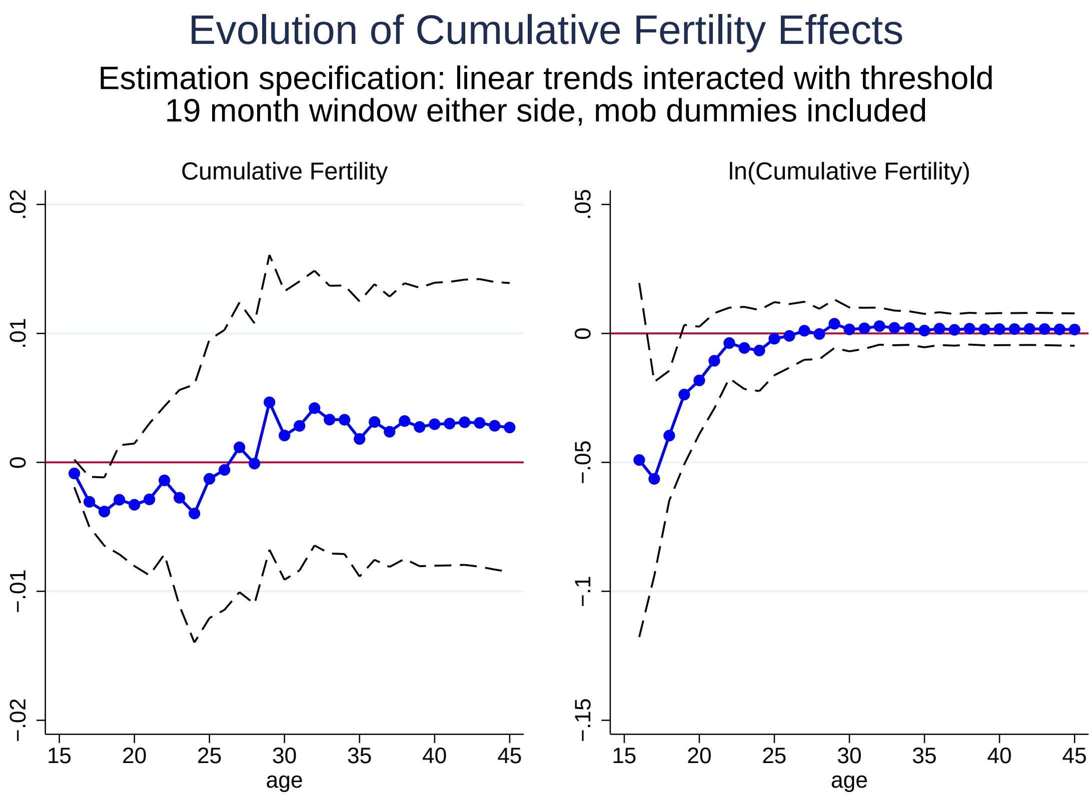

Figure 9: Effects of the 1972 ROSLA on cumulative fertility. Each point in the plots represents a distinct RD estimate of the reduced form effect of ROSLA on cumulative fertility from 16 up to and including the indicated age. Pointwise 95% confidence intervals are plotted as dotted lines. The left panel plots level effects, which are measured in counts of children per woman. The right panel plots effects on log of cumulative fertility. The left panel shows that the the confidence interval generally rules out effects larger than +.015 births per woman and smaller than -.010 birth per woman, and is significant only at ages 17 and 18. The right panel, which scales effects as percentage changes, shows that large percentage effects occur in the teen years and then die out. Large teen effects are consistent with very small effects on cumulative fertility at age 45 because the levels of the teen effects are small compared to completed 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 discontinuity estimate of the reform on the age 16 fertility rate.

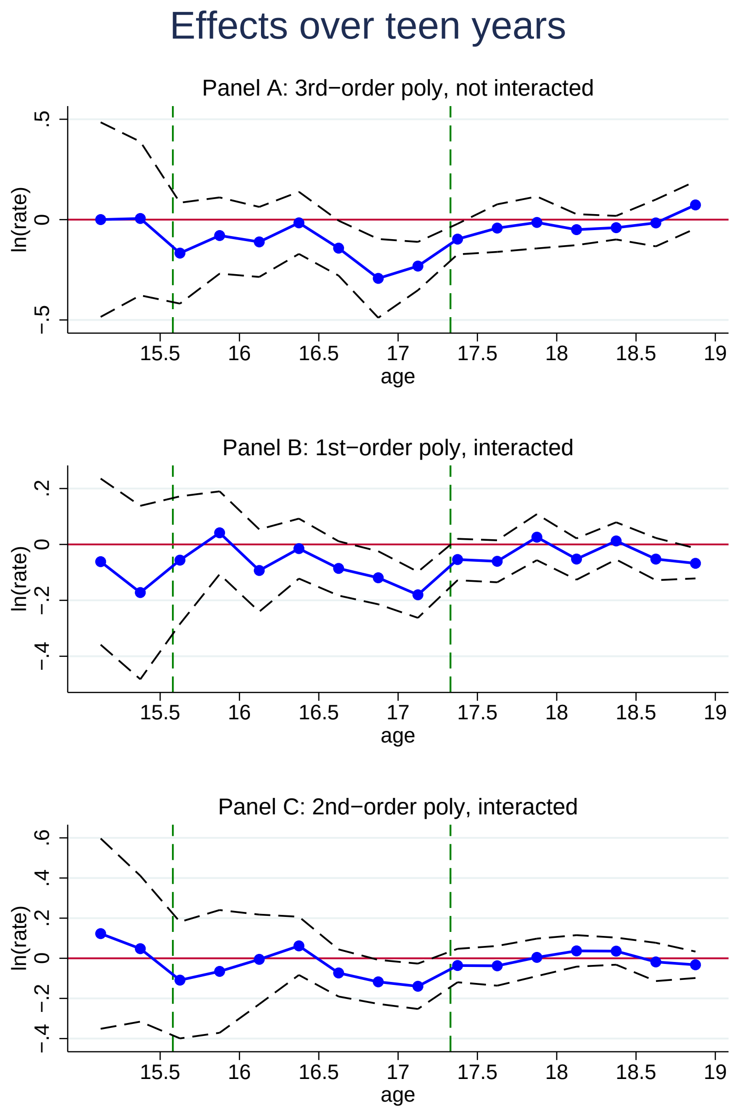

Figure 10: Detailed timing of birth effects. Fertility rates are calculated over quarter-years of age. Each point represents a distinct RD estimate of the effect of ROSLA on fertility in a quarter-year age interval. The first point represents the RD estimate of the reform on births between ages 15.00 and 15.25, the second point represents the RD estimate of the reform on births between ages 15.25 and 15.50, and so on. The vertical dotted lines denote the time during which the effected cohorts were newly in school, shifted right by 9 months. Pointwise 95% confidence intervals are plotted as dotted lines. The three panels show results under alternative specifications of the trend control. The window of estimation of the trend control was determined using a cross-validation procedure described in the text.

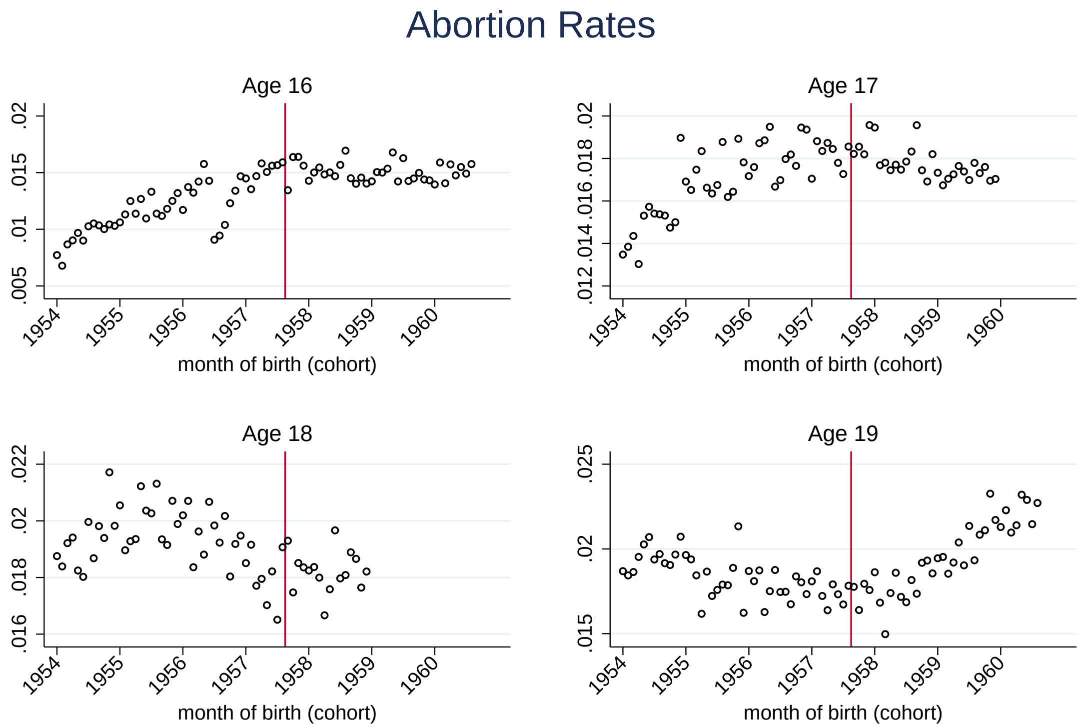

Figure 11: The reduced form impact of the 1972 ROSLA on abortions. The lack of discontinuity at the thresholds across all panels indicates that the 1972 ROSLA had no effects on abortion rates at ages 16 through 19. The abortion rate at age x is defined as the number of abortions performed on women whose age-last-birthday was x, divided by the number of women in the cohort alive at age x.

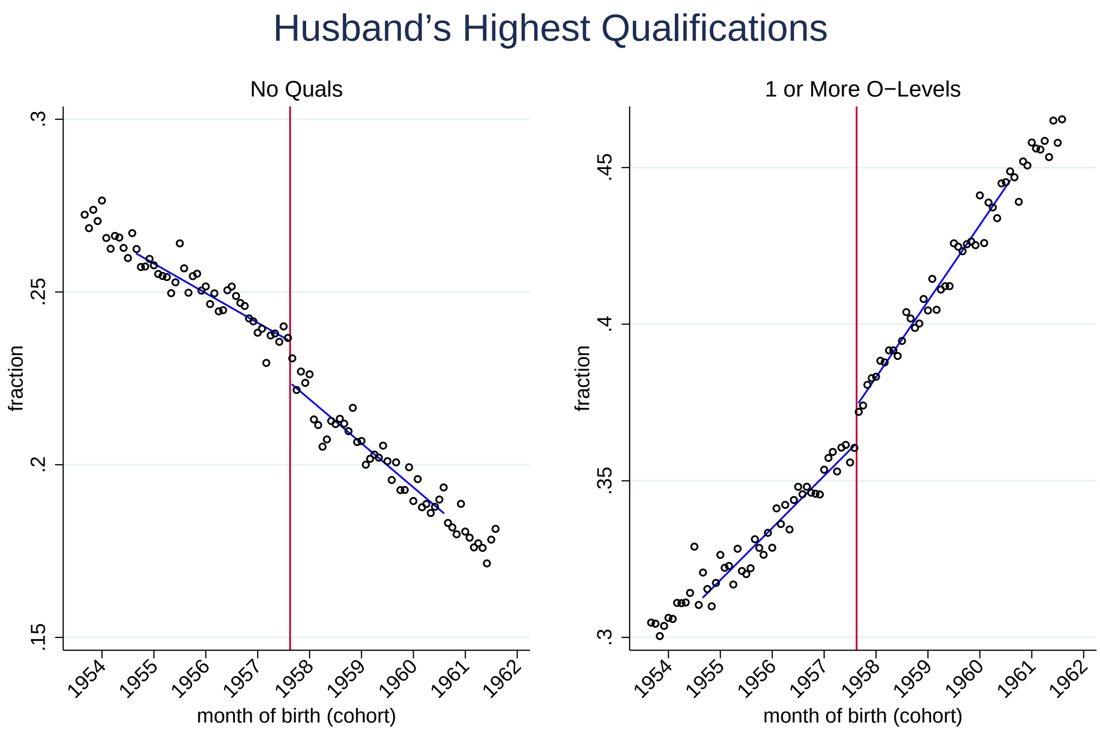

Figure 12: Effects on husband's educational qualifications. The horizontal axis is the wife's date of birth. Data come from the 2001 Census. Each observation is a cohort-cell mean.

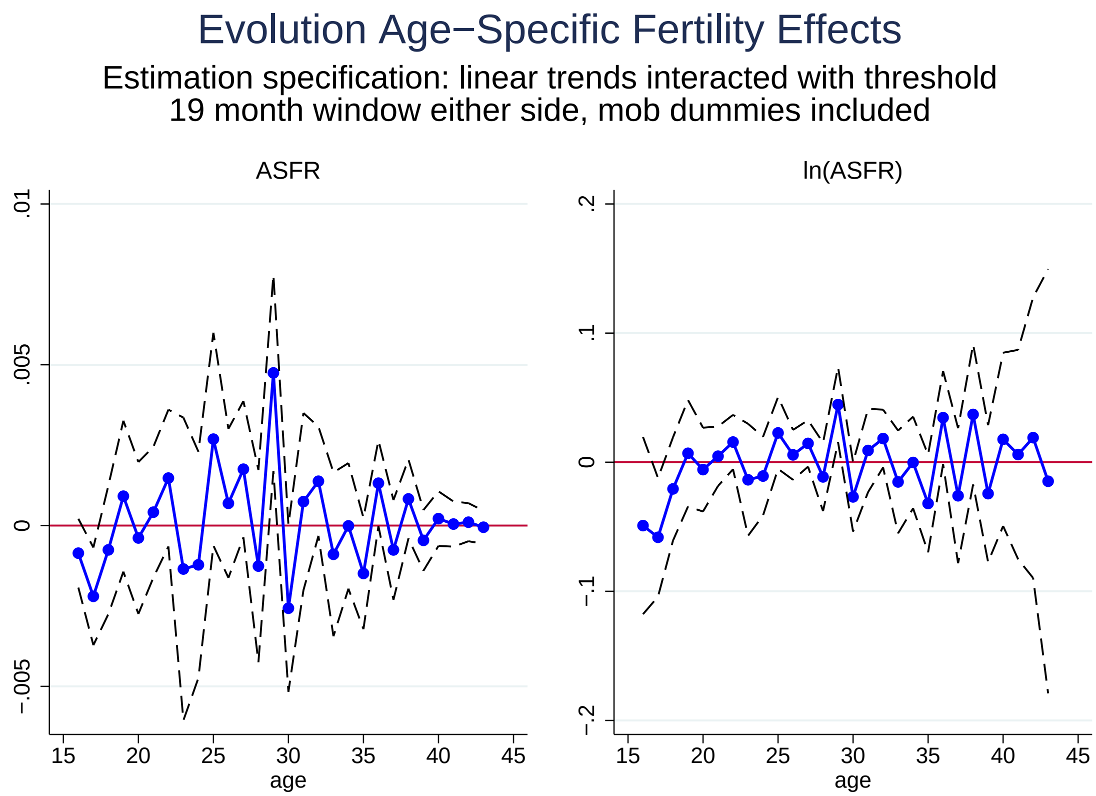

Figure A1: Effects of the 1972 ROSLA on age specific fertility at each age. Each point in the plots represents a distinct RD estimate of the reduced form effect of ROSLA on an age-specific fertility rate. Pointwise 95% confidence intervals are plotted as dotted lines. The left panel plots level effects on rates. The right panel plots effects on log of of these rates.

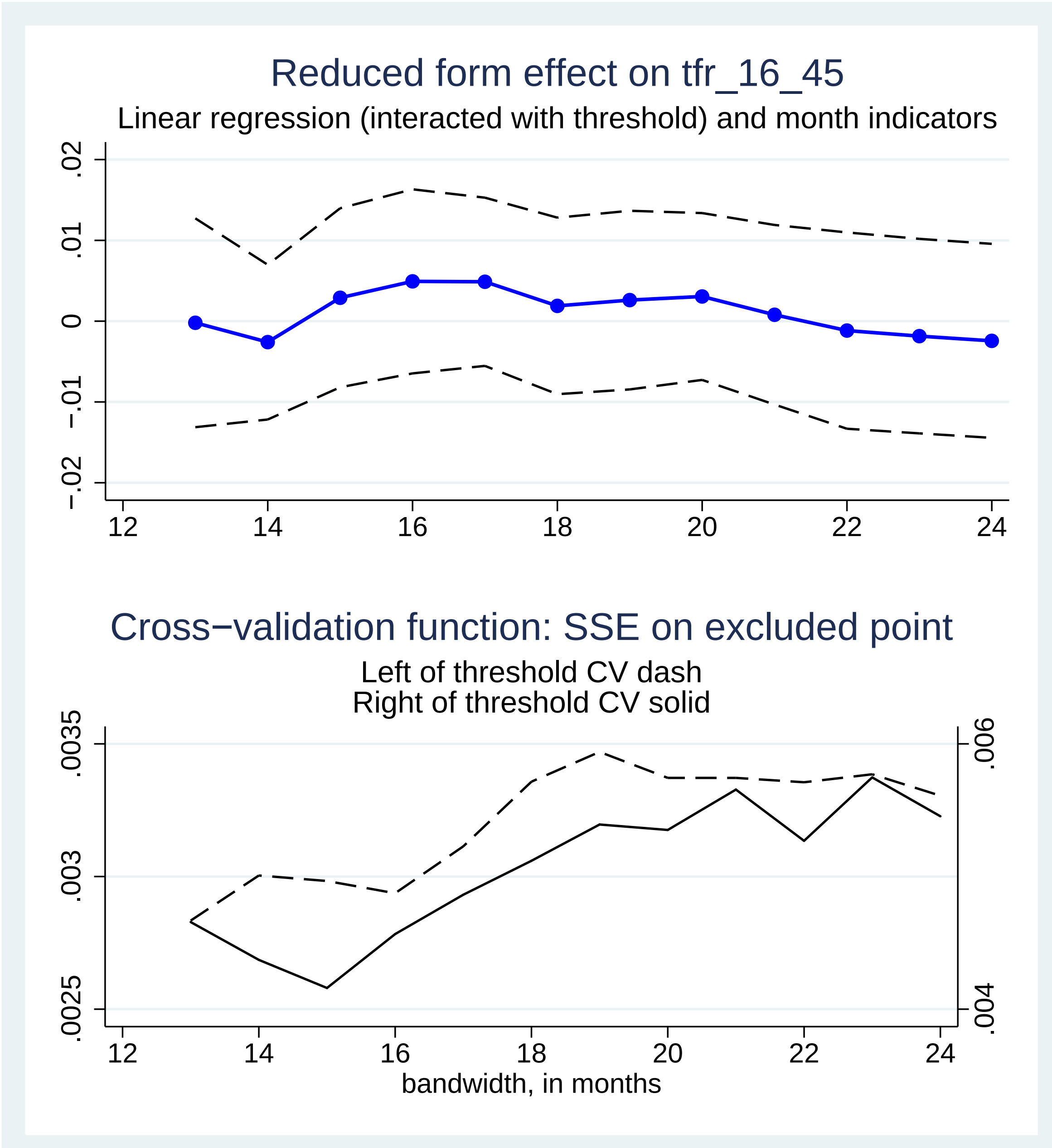

Figure A2: ROSLA's effect on TFR under different specifications of the cohort trends. The top panel plots RD estimates for the effect of ROSLA on cumulative fertility at age 45 for a range of windows used to estimated the cohort trend. In all cases, the cohort trend is linear, estimated separately on either side of the reform, and includes month of birth dummies to capture seasonality. The bottom panel plots the sum of squared residuals from a cross-validation function calculated for the same set of trend specifications. The cross-validation procedure is described in detail in Lee and Lemieux (2010). Lower values of the function indicate a better fit of the trend.