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Abstract

Can education policy reduce the incidence of teenage motherhood? This paper uses data from the largest UK household-level survey to investigate the impact of a change in legislation, which increased the duration of compulsory schooling, on the timing of fertility using a regression discontinuity design. The findings indicate strong evidence that the schooling reform induced a downwards impact on fertility not only at the new school-leaving age, but also exerted a non-monotonic effect throughout the teenage years. Overall the analysis suggests that the increase in mandatory education caused a postponement of fertility with the influence of the reform dissipating after age 20.

JEL classification: I21, J13

Keywords: Compulsory Schooling, Fertility, Regression Discontinuity Design, Teenage Motherhood

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1 Introduction

Teenage motherhood is widely regarded as an important socio-economic issue for two key reasons. First, individuals who are restricted in their human capital investment in adolescence may not reach their lifetime's economic potential. Second, there is an important inter-generational dimension associated with early childbearing, as the children born to teenage mothers tend to have poorer outcomes, and also themselves have a higher probability of becoming mothers at an early age (Paniagua and Walker, 2012).

These direct and indirect consequences of teenage motherhood have been widely documented. Analysis based on observed differences between women who gave birth as a teenager and women who became mothers at an older age find substantial adverse effects of early childbearing on a number of lifetime outcomes, such as lower levels of educational attainment (Moore and Waite, 1978; Klepinger, Lundberg, and Plotnick, 1995), and significantly higher rates of poverty, welfare receipt and lower household income (Bronars and Grogger, 1994; Ermisch and Pevalin, 2003). Studies that address the potential endogeneity of the fertility decision to ascribe a causal effect of adolescent motheerhood reveal somewhat disparate results both with respect to the impact on the mother herself (see, *inter alia*, Chevalier and Viitanen, 2003; Hotz, McElroy, and Sanders, 2005; Fletcher and Wolfe, 2009; Ashcraft, Fernández-Val, and Lang, 2013) and the outcomes of the child (Francesconi, 2008). Such analyses with arguably a closer comparison group tend to indicate that teen mothers would have poorer economic outcomes even if they had delayed motherhood. Indeed, Kearney and Levine (2012) suggests that for some individuals the decision to become a young mother is a rational choice in response to low expectations of future economic opportunities, rather than an unintentional consequence. In short, the weight of evidence overwhelmingly points to the existence of negative effects both in the short and long-run. Hence interventions that mitigate adolescent fertility rates are regarded as plausible mechanisms through which to improve the life trajectories of young women whom project a high proclivity toward teenage motherhood.

Given the interdependence of the education and early fertility decision, one potential channel of influence is education policy. In the context of mitigating teenage motherhood interest lies in the ability of the institutional environment to affect the timing of fertility. Exogenous differences in the duration of mandatory schooling have been used to elicit the causal effect of education on the likelihood of becoming a teenage mother. Changes in legislation regarding the minimum age at which an individual becomes eligible to leave school were first used by Black, Devereux, and Salvanes (2008) to investigate the effect of education on teenage fertility.[1] The authors propose two mechanisms through which the legislation changes exert an effect on fertility. First the "incarceration effect", which in the spirit of the findings of Jacob and Lefgren (2003) regarding the impact of schooling on youth crime, can be understood that as individuals are required to remain at school for one year longer, this reduces the opportunity to engage in risky activities, which leads to downward pressure on their fertility. Second, the "human capital effect", whereby individuals reduce their fertility in response to receiving more education and hence better labour market prospects as a result of the legislation change.

Using data for both the US and Norway - two countries with very different institutional environments - they obtain remarkably similar findings.[2] The results indicate only weak evidence for an incarceration effect, and the authors therefore conclude that the observed

[1] A tranche of literature uses this strategy to elicit the causal impact of education on overall fertility, with diverse findings. Using variation in education induced by compulsory schooling laws in 8 European countries, Fort, Schneeweis, and Winter-Ebmer (2011) find an increase in education is associated with a large decrease in childlessness and increase in child parity, whereas Monstad, Propper, and Salvanes (2008) find no overall effect in Norway, and León (2004), using US census data, finds a decrease in the average number of children per woman. In the UK Braakmann (2011) using survey data finds a marginal increase overall fertility, whereas Clark, Geruso, and Royer (2014) using cohort-level administrative data find little effect in completed fertility.

[2] Using General Household Survey data, Silles (2011) finds effects of a similar magnitude for the UK.

significant negative effect of education on teen fertility is driven primarily by the human capital effect. One potential reason for this finding could be that, in both countries, the reforms that were used for identification had a relatively minor "bite", affecting a comparatively small fraction of youth.

But other institutional features also imply variation in the amount of schooling that individuals obtain without varying the age at which they leave school. Two examples include school entry policies and changes to the length of the school day. McCrary and Royer (2011) exploit that in the US different school-entry policies imply that individuals who are born in adjacent months face different required lengths of schooling, but can effectively leave school at the same time. In terms of the mechanisms outlined in Black et al. (2008), the variation in education generated by the school entry policy should only have an impact on teenage fertility via the human capital effect. Using data from California and Texas to investigate the impact of education on a number of socio-economic outcomes they find no effect of education on the timing of fertility, which, in contrast to Black et al. (2008) suggests a relatively minor importance of the human capital effect. Berthelon and Kruger (2010) evaluate a policy intervention in Chile which increased the length of the school day. The policy had been widely criticised as previous evaluations indicated a negligible effect on educational attainment, suggesting no human capital effect. However the analysis shows that the intervention induced a significant impact on non-academic outcomes, specifically an amelioration of risk behaviours such as teen fertility and crime participation. The authors posit that this effect is entirely due to increased incarceration, as adolescents received more adult supervision per day and therefore had less time to engage in risky activities. These aforementioned studies indicate conflicting evidence with regard to the channels through which the mandatory schooling requirement influences the likelihood of early childbearing.

This paper investigates the effect of education policy on adolescent fertility in England and Wales, exploiting exogenous variation in the length of compulsory schooling induced by an institutional change, the Raising of School Leaving Age (RoSLA), implemented by the UK Government in 1972. The contribution of this paper to the literature is threefold. First, the analysis considers a legislative increase to the compulsory school leaving age which, in contrast to those studied in Black et al. (2008), impacted a significant proportion of the population. Second, as eligibility for the reform was determined by a single cut-off date the analysis proceeds using a Regression Discontinuity Design (RDD) and the paper contributes by addressing methodological concerns with implementing RDDs which have been highlighted in the recent econometrics literature. Third, as the UK has one of the highest rates of teenage pregnancy in Western Europe, the paper contributes to the body of international evidence that analyses the influence of education policy on the timing of fertility.

The analysis uses data from the Labour Force Survey, the largest representative UK household survey, exploiting an institutional change which increased the duration of compulsory education by one year. As the legislative change was implemented nationwide at a single point in time, it can be thought of as a natural experiment, which induced exogenous variation in the length of education received by an individual. The variation was determined solely by a discontinuous function of an observed covariate, the individual's month and year of birth, and therefore the estimation proceeds through a regression discontinuity design (RDD), an approach which allows the identification of causal treatment effects in quasi-experimental settings. The analysis employs both parametric and non-parametric methodologies to estimate the direct impact of the reform, and a two-stage 'fuzzy' RDD approach is used to address a pertinent policy question, namely quantifying the consequence of increasing mandatory education by one year on teenage motherhood. The results suggest that the impact of RoSLA varies non-monotonically throughout the teen years and, in contrast to Black et al. (2008), reveals strong evidence of the incarceration effect, as well as the beyond incarceration effect which may be attributable to increased human capital acquisition. The findings are robust to the empirical methodology employed and the sensitivity of the estimates to the choice of bandwidth is explored. In addition, the analysis is extended to examine the extent of the bite of the reform by investigating the extent of the impact of the treatment beyond just the teenage years, the results suggesting that RoSLA essentially caused a postponement of fertility to the late teenage years, with no observed impact of the reform after age 20.

The remainder of the paper is structured as follows: Section 2 summarises the institutional context. Section 3 describes the data used in the analysis. The econometric methodology is outlined in Section 4. Section 5 presents the results and offers interpretations, Section 6 concludes.

2 Institutional Setting

Compulsory schooling was introduced to the UK towards the end of the 19th Century, with separate rules governing school-starting and school-leaving ages. A child is required to commence education no later than the beginning of the academic year[3] after which she reaches the compulsory school-starting age of 5 years, which has remained unchanged since its inception through the Forster Education Act (1870). The first minimum school-leaving age of 10 years was introduced by the Elementary Education Act (1880), with incremental increases to the school-leaving age introduced by subsequent legislation.[4]

[3] In England and Wales the academic year runs from September 1st until August 31st in the next calendar year.

[4] The Elementary (School Attendance) Act (1893) increased the age requirement to initially to 11, and up to 12 with an amendment to the act in 1899; another increase up to age 14 followed the Fisher Act (1918); the Butler Act (1944), enacted in 1947, enabled further rises first to age 15 and subsequently 16; the Education Act (2008) introduced an initial increase to age 17, and from September 2015 requires formal

This paper concentrates on the exogenous variation in the minimum education requirement induced by the Education (Butler) Act (1944), which initially established a minimum compulsory school-leaving age of 15. The act made provision for a further raise of the school leaving age up to age 16, but did not mandate a specific implementation date.[5] In the immediate post-war period implementation was not possible due to acute shortages in capital, material and labour, the latter so extreme that during the 1950's there were calls to reduce the length of compulsory education in order to increase the size of the labour force pool. However following the Crowther Report (1959) there was a distinct shift in attitude in favor of increasing the duration of mandatory schooling, leading to the announcement in 1964 of the government's intention to implement an increased school-leaving age in September 1970. Preparations for the age-rise were extensive and included a revised curriculum, large-scale teacher-training to increase the supply of teachers, and a building initiative enlarging schools to accommodate the increased number of students. These preparations were halted due to fiscal constraints imposed following the 1967 devaluation of sterling, with the government delaying implementation by two years. The new school-leaving age was finally introduced by Statutory Instrument 444 (1972), commonly known as the Raising of School Leaving Age (RoSLA[6]), implemented in September 1972 thus affecting academic cohorts born from 1st September 1957 onwards.

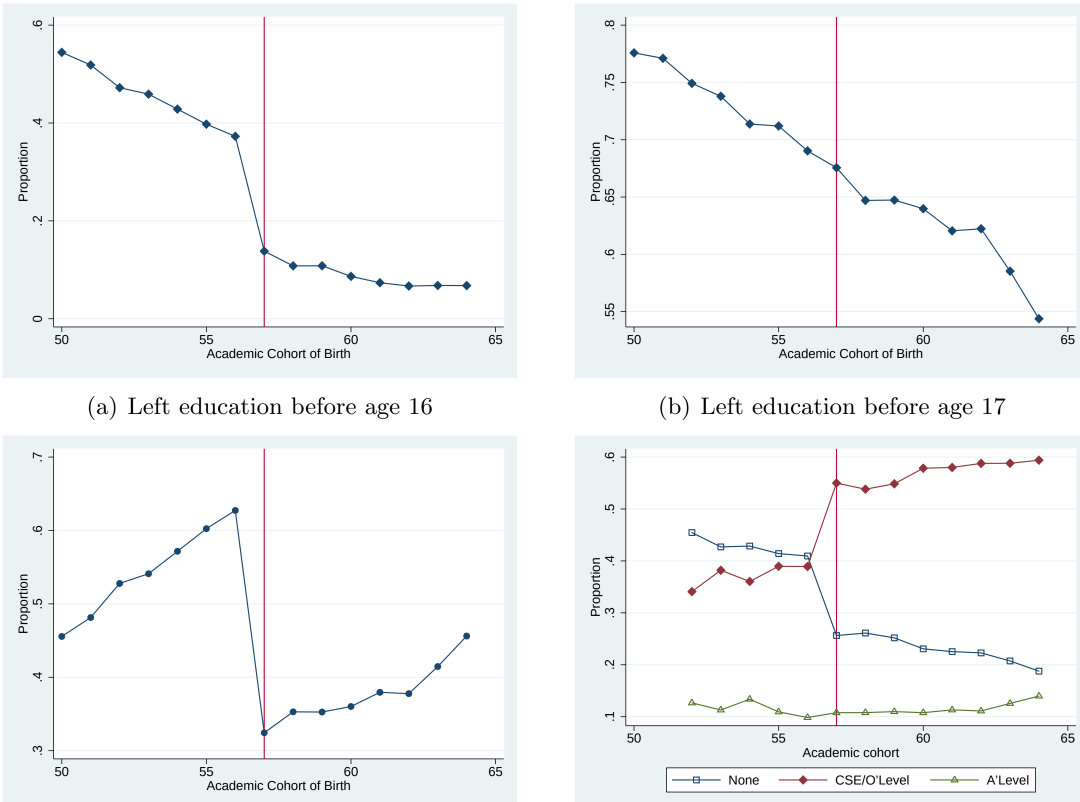
The reform impacted the leaving decisions of individuals in the lower tail of the education distribution only. Figure 1(a) depicts the fraction of individuals leaving education before the age of 16 by their academic cohort of birth. This proportion was steadily declining prior to the implementation of RoSLA, but there is an immediate drop of approximately 20 percentage points exactly coinciding with the introduction of the new minimum school-

participation in education or training of individuals in England and Wales until their 18th birthday.

[5]Section 35 of the Act states that the subsequent raise should occur 'as soon as the Minister is satisfied that it has become practicable to raise to sixteen the upper limit of the compulsory school age'

[6]A comprehensive history of the RoSLA can be found in Woodin, McCulloch, and Cowan (2013).

Figure 1: Participation in Education



(a) Left education before age 16

(b) Left education before age 17

(c) Post-compulsory

(d) Highest qualification level

Notes: The graphs display the proportion of individuals by academic cohort of birth. The vertical line depicts the implementation of RoSLA.

leaving age, indicating that the RoSLA reform constituted a binding constraint for this proportion of the school age population. Compliance with the increased mandatory age was almost ubiquitous. Since the Education Act (1962) an individual did not become eligible to leave school on the exact day he attained the compulsory school-leaving age, instead two school exit dates were imposed - the end of the Spring term (at Easter) for individuals within an academic cohort whose birthday lay between September and January, and the last day of the Summer term for those attaining school-leaving age between February and

August. The implication of this 'Easter Leaving Rule' was that summer-born children born at the end of the academic year would become eligible to leave school just before the birthday where they reached compulsory school-leaving age. Specifically as the end of the Summer term usually falls around the end of June, one sixth of the first cohort directly affected by RoSLA (those born in July and August 1958), could leave school at age 15 and still be compliant with the minimum school-leaving age requirements, and therefore in Figure 1(a), the proportion of individuals leaving education by age 15 does not fall to exactly zero after the implementation of the increased schooling requirement.

Consistent with previous studies (see e.g., Chevalier, Harmon, Walker, and Zhu (2004); Dickson and Smith (2011)), the data indicate that there were no ripple-upwards effects of the RoSLA throughout the duration of education distribution. Figure 1(b) shows that there is no discontinuity in the downward trend of the proportion of individuals leaving education by age 17, indicating that the RoSLA did not induce an increase in the proportion of students participating in post-compulsory education. Indeed as verified in Figure 1(c), prior to implementation over 60% of students already participated in post-compulsory education, but approximately half of these individuals remained in school to age 16 only.[7] As a consequence it can be observed that the post-compulsory education rate actually fell approximately 30 percentage points coincidental to the introduction of the reform, afterwhich it reaches a relatively stable level consistent with the RoSLA reform inducing an increase in schooling for those individuals in the lower tail of the years of education distribution up to the new minimum school leaving age but not beyond. This is further supported by examining qualifications obtained: Figure 1(d) illustrates the trends in the highest academic qualification obtained by individuals. In the RoSLA year there is a drop

[7]The first tier of academic qualifications in England and Wales are taken at age 16, which prior to RoSLA may have been the inducement for these individuals to remain in education beyond the minimum requirement

in the proportion of individuals without academic qualifications of almost 15 percentage points, approximately equal to the increase in the proportion of individuals obtaining either a Certificate of Secondary Education (CSE) or Ordinary Level (O'Level) qualification, examinations which are sat in the academic year in which an individual turns 16. In contrast, there is no impact of the RoSLA on the proportion of individuals with an Advanced Level (A'Level), an examination taken at age 18.

3 Data

The analysis combines data from the 1975-2006 Labour Force Surveys (LFS). The survey, which is the largest representative household-level survey in the UK, contains detailed information on each individual within a household including month and year of birth, ethnicity, age at leaving full-time education, area of residence, and country of birth.

The outcome of interest in the analysis, the age at which an individual entered motherhood, is determined from the ages of the mother and the eldest child within a household at the time of the survey using the "own-children methodology" developed by Grabill and Cho (1965). This reverse-survival technique has been shown to generate age-specific fertility rates from LFS survey data which are consistent with those calculated from administrative data (Murphy and Berrington, 1993). Implicit in this procedure is that a mother-child relationship can be observed only if both individuals are present in the same household at the time of the survey. Thus in the case of parental separation the child is assumed to be resident with the mother, so that the observed mother-child relationship is biological. The determination also assumes away child mortality, and therefore the eldest child observed is primogeniture. Although these two factors may induce measurement error, it is likely that any effect would be small.[8] As the LFS contains measures of both month and year of birth

[8]The proportion of multi-family households has declined from 3% in 1961 to approx 1% in 2001 (Social Trends 32, Office of National Statistics (2002)), with over 90% of stepfamilies in 1990 being comprised of

it is possible to determine maternal age to within one month, a more accurate calculation than is possible with census data.[9] A further advantage of the detailed reporting of date of birth in the LFS is that it enables precise assignment of individuals to their academic cohort of birth, which would not be possible if only calendar year of birth was reported.

To avoid truncation of the distribution of teenage mothers, the sample is restricted to women aged between 20 and 30; the lower bound reflects that to determine whether an individual is a teen mother or not the observation must be taken after adolescence, the upper bound reflects the fact that during this period individuals started to leave the parental home from age 16 onwards, so above the age of 30 it may not be possible using information on individuals residing in a household to accurately identify whether a woman became a mother in her teenage years.

Although the LFS does report country of birth, for all but the latest surveys this measure is aggregated to the national level for UK-born individuals, as constituent countries of the UK are measured from only the 2nd quarter of 2001 onwards. Additionally, the LFS reports contemporaneous region of residence only at the time of the survey, and therefore does not have information on where an individual spent her childhood. This is problematic as the education system in Northern Ireland and Scotland differs from that in England and Wales, and in particular education in Scotland is governed by separate rules and legislation. For this reason the sample is restricted to those women who were born in the UK, but were resident in England and Wales at the time of the survey, with the implicit assumption that these individuals would have been subject to the English education system. It is therefore

children from a previous relationship of the mother (Social Trends 38, Office of National Statistics (2008)). There has been an upward trend in single-parent families, but a fairly constant proportion of these (circa 85%) are lone-mother families (Social Trends 38, Office of National Statistics (2008)). Childhood mortality rates have been declining over time - the under-15 mortality rate stood at 31 per 100,000 in 1980, falling to 15 per 100,000 by 2000 (Child Mortality Statistics, Office of National Statistics (2010a)).

[9]For instance, the US census records year of birth only for the 1940 and 1950 censuses, thereafter also quarter of birth allowing a calculation of maternal age to within 3 months at best (Black et al., 2008).

possible that the sample is affected by random mobility, however internal migration between constituent countries of the UK is assumed small.[10]

Table 1: Descriptive Statistics

Variable	Mean	Std Dev	Variable	Mean	Std Dev
Academic Cohort	57.75	4.876	Age at survey	25.38	3.063
Age left F/T Education	16.61	1.824	Subject to RoSLA	0.605	0.489
White	0.974	0.160	No of children	1.787	0.811
Mother at 15	0.003	0.053	Mother by 15	0.003	0.051
Mother at 16	0.012	0.107	Mother by 16	0.005	0.073
Mother at 17	0.028	0.166	Mother by 17	0.017	0.129
Mother at 18	0.042	0.200	Mother by 18	0.045	0.208
Mother at 19	0.050	0.218	Mother by 19	0.087	0.282
Mother at 20	0.051	0.221	Teen Mother	0.137	0.344
Number of Observations			137,502		

Table 1 displays the descriptive statistics for the main sample used in the analysis. The individuals were all subject to the Butler Act (1944), thus facing a minimum school-leaving age of either 15 or 16. Academic cohorts range from 1947/48 to 1964/65, with 61% of individuals within the sample subject to the post-RoSLA schooling regime (minimum school-leaving age of 16). The sample is predominantly white;[11] 13.7% of the sample are teenage mothers, 8.7% are mothers before the age of 19, 4.5% before age 18, 1.7% before age 17, 0.5% before age 16 and 0.3% before age 15, proportions reflective of those recorded in administrative data. Amongst mothers in the sample, the number of children per mother is 1.78. This is lower than official estimates of total fertility rates, but reflects that the sample measures fertility only up to a maximum age of 30 rather than completed fertility per woman.[12]

[10]Internal migration statistics are not available prior to 1991, however Stillwell, Boden, and Rees (1990) using doctor registration data from 1975-1986 estimate that the bulk of internal migration over this period was within rather than between countries of the UK.

[11]The under-reporting of ethnic minority groups is well-known in the LFS. In an attempt to address this issue, 'boost' samples, which over-sample in areas with a high population density of under-represented groups, have been taken since 1984.

[12]The average number of children per woman in the sample is 1.18, which is comparable to cohort fertility

4 Empirical Methodology

As the RoSLA reform was implemented nationwide at a single point in time, it can be thought of as a natural experiment inducing exogenous variation in the length of education received by an individual. As this variation was determined solely by a discontinuous function of an observed covariate, the individual's birth date, the estimation proceeds through a regression discontinuity design (RDD), an approach which allows the identification of causal treatment effects in quasi-experimental settings. The method dates back to Thistlethwaite and Campbell (1960), who introduced the approach analyzing the impact of winning a scholarship on subsequent academic outcomes. More recently RDDs have gained popularity in applied economics and have been used to investigate, *inter alia*, the impact of impact of class sizes on scholastic achievement (Angrist and Lavy, 1999), voting shares (Lee, 2001) and labour market discrimination (Hahn, Todd, and van der Klaauw, 1999).

The RDD approach is based on the idea that a discontinuity in the assignment function to a treatment is induced in situations where individuals are deterministically assigned to the treatment based on whether the value of an observed covariate, the running variable, Z_i, falls on either side of a specific threshold value $Z_i = z^*$. The intuition is that individuals in the neighbourhood of the threshold value are identical in all other characteristics, apart from whether or not they are assigned to the treatment. Therefore by comparing individuals 'close' to the discontinuity from either side of the threshold, a causal effect of the treatment can be identified. As there is local randomization additional covariates are not necessary, but may improve precision of the estimates (Lee and Lemieux, 2010).

The assumptions to achieve identification in this context are hence twofold: a) that

rates in administrative data. ONS estimates of children per woman range from 0.99-1.62 for women aged 30 between birth years 1947-1983 (ONS, Cohort Fertility, England & Wales, Office of National Statistics (2010b)).

individuals are randomly selected into the RoSLA 'treatment'; b) that the timing of the introduction of RoSLA is not related to unobserved characteristics that determine teenage motherhood. Whether an individual was subject to the increased school-leaving age can be considered to be as good as randomly assigned for two reasons. Firstly, individuals are assigned to academic cohorts according to their date of birth, which cannot be perfectly controlled. Second, there is no possibility of announcement effects, whereby forward-looking parents could time the birth of their children according to RoSLA eligibility, as detailed in Section 2 plans to raise the school leaving age were not made public before 1964, by which time the first individuals who would be impacted by RoSLA had already been born.

Formally the RDD estimate α^{RDD} is calculated by taking the difference in the expected values of the outcome variable either side of the threshold of the observed running variable:

$$\begin{aligned} E[\alpha^{RDD}|z] &= E[Y_1 - Y_0|Z = z^*] \\ &= \lim_{z^* \leftarrow z^+} E(y_i^1|z^*) - \lim_{z^- \rightarrow z^*} E(y_i^0|z^*) \\ &= \lim_{e \rightarrow 0} E(y_i^1|z^* + e) - \lim_{e \rightarrow 0} E(y_i^0|z^* - e) \end{aligned} \tag{1}$$

where Y_1 and Y_0 are respectively the 'treated' and 'untreated' population means; y_i^1 and y_i^0 are observations of individuals respectively to the right or the left of the discontinuity; the threshold level of the running variable is denoted $Z = z^*$. When the support of the running variable is continuous, e can be infinitely small close to the discontinuity so that the limits in (1) exist, and it is appropriate to use non-parametric methods in the estimation (Hahn, Todd, and van der Klaauw (2001)). As eligibility for the reform is deterministic, this representation is a 'sharp' RDD.

4.1 Non-parametric Estimation

The analysis uses kernel-weighted local polynomial smoothing to estimate the expectations either side of the threshold value of Z_i, with the treatment effect calculated as the difference between the predicted values calculated at the discontinuity. Although triangular kernels, by assigning larger weights to observations at the threshold in principle have better boundary properties (Fan and Gijbels, 1996), in practice kernel choice does not exert a significant impact on the magnitude of the estimates and rectangular kernels have become the *de facto* standard (Imbens and Lemieux, 2008). The order of polynomial smoothing is guided by the Bayesian Information Criterion.[13] Bootstrapped coefficients and standard errors are calculated.

The running variable in the analysis is the distance in time between an individual's birth and the implementation of the RoSLA reform. Time is clearly continuous, however a practical issue arises because the data contains only discrete measures, so that the lowest granularity that this distance can be calculated is in months. Lee and Lemieux (2010) argue that as long as the running variable, Z_i, is finely distributed the econometric complication is limited, as in practice data will always contain discrete measures (Imbens and Lemieux, 2008). In essence the concern with a discretely measured running variable is that it is not possible to allow e to become infinitely small in the neighbourhood of the discontinuity. Thus there is an irreducible gap between observations on either side of the threshold, and the casual effect of the programme is only identified with a parametric assumption regarding the assignment function (Lee and Card, 2008).

[13] The Bayesian Information Criterion (BIC) indicated that a linear polynomial was appropriate over all outcome variables. The BIC applies a larger penalty for higher order terms than the Akaike Information Criterion, which proved to be less definitive, but indicated either a linear or quadratic polynomial according the outcome variable in question. As the role of the polynomial is to reflect the underlying data generating process that governs fertility, rather than fertility at a specific age as measured by the relevant outcome variable, the same order of polynomial was applied across all specifications.

4.2 Parametric Estimation

Recall from equation (1), the estimate of interest is $E[Y_1 - Y_0|Z = z^*]$. The issue at hand with discretely measured data is that it is possible to observe $E[Y_1|Z \geq z^*]$, the outcome of the set of individuals at precisely the threshold or above who are subject to the treatment, and $E[Y_0|Z = z^* - e]$, the outcomes of the set of individuals strictly below the threshold who are not treated. With discrete Z_i, e takes on a finite number of values over the range $Z = z_j, j = (1, .., J)$, which implies that the limits in equation (1) do not exist. Specifically, the closest realisation below the threshold, where $z^* = z_k$, is $E[Y_0|Z = z_{k-1}]$ and therefore to predict $E[Y_0|Z = z_{k-0}]$ a parametric approach is required. As the outcome variable is binary, probit regressions are estimated using a treatment dummy, T, indicating whether the individual was subject to the RoSLA reform, and include a polynomial function of the running variable, z_j. Including interaction terms between the treatment dummy and the polynomial allows the polynomial coefficients to differ either side of the discontinuity.[14]

The estimation equation thus becomes:

$$Y_{ij} = \alpha_0 + \beta_0 T_{ij} + \gamma_0 P_j^l + \delta_0(T_i \times P_j^l) + a_j + \epsilon_{ij} \tag{2}$$

where Y_{ij} is the outcome for individual i born at a distance of j, in months, from the relevant threshold; T_{ij} is a dummy variable indicating whether an individual born in month j was subject to the RoSLA reform, thus β_0 captures the impact of the treatment, and is hence the parametric estimate of α^{RDD}; P_j^l is a vector of polynomial functions of z_j, with $(l \in \mathbb{N})$

[14] Although the polynomial is allowed to have different coefficients either side of the discontinuity, the same order of polynomial is applied, reflecting that the polynomial is capturing the underlying data-generating process. Lee and Lemieux (2010) note that constraining the coefficients of the polynomial to be the same on both sides of the discontinuity is inconsistent with the intuition behind the RDD approach as data from above the threshold would be used to estimate $E[Y_0|Z = z^*]$ and data from below the cutoff would be used in the calculation of $E[Y_1|Z = z^*]$. However this approach is often seen in the literature, see for example Silles (2011), as imposing this constraint will lead to more efficient estimates.

denoting the order of the polynomial; a_j is a specification error term that describes the difference between the true value at each z_j and the estimated polynomial function; ϵ_{ij} is an idiosyncratic error term.

The magnitude of the coefficient estimates of interest can be sensitive to the choice of polynomial in the running variable. A certain degree of smoothing may be desirable to minimise the influence of outliers and seasonality, although at a cost of deterioration in the model's fit. Higher degree polynomials follow the data more accurately, but may overstate outliers. With small bandwidths the number of higher degree polynomials is limited as J constrains the total parameters that can be estimated. The optimal order of the polynomial is again guided chosen according to the Bayesian Information Criterion. With this approach it is necessary to include more conservative standard errors to reflect modeling uncertainty. Lee and Card (2008) advocate inflating standard errors in relation to their goodness of fit statistic G,[15] and therefore (2) includes the specification error term a_j, which is assumed identical either side of the discontinuity and to be random and orthogonal to Z. The estimation computes robust standard errors with random, identical specification errors by clustering on z_j.

In practice both the parametric and non-parametric approaches should yield similar estimates of the RDD parameter as long as the discretisation of Z is not too coarse. Therefore Section 5 presents results utilising both methodologies in order to illustrate that the analysis does not rely on one particular method or specification.

[15] The Lee and Card (2008) G-statistic is calculated as:

$$G \equiv \frac{(RSS_R - RSS_{UR})/(J-K)}{RSS_{UR}/(N-J)}$$

where RSS_R is the residual sum of squares for the model using polynomial functions and RSS_{UR} for the unrestricted model using dummies respectively. Under the assumption of normality, G follows an $F_{(J-K,N-J)}$ distribution, with K the number of parameters estimated in the restricted model, N the number of observations and J the total number of values in the support of Z. The null hypothesis is that there is no systematic difference in the residual sum of squares in the restricted and unrestricted estimations.

4.3 Bandwidth Choice

A key issue in both the parametric and non-parametric approaches is the determination of the appropriate size of the window around the discontinuity to use in the estimation. From a theoretical perspective, by taking the limits either side of the threshold the smallest window width around the discontinuity yields unbiased estimates of the true treatment effect. However such an estimation would use only a paucity of data points and therefore have little statistical power. Wide bandwidths use a greater number of observations and will produce more efficient estimates, however a degree of bias may be introduced by including observations far from the discontinuity, the concern being that there may be unobserved changes over the bandwidth period, for instance to legislation or benefit entitlement[16], which could independently impact the proclivity toward teen motherhood, potentially confounding the analysis. It might also be expected that the magnitude of the treatment effect is different for those cohorts closer to the timing of the implementation. In addition, too great a window size may indicate a sizable treatment effect even when the data is smoothly distributed around the discontinuity. There is therefore an inherent trade-off between bias and efficiency in choosing the appropriate window of observations to include in the estimation.

Ludwig and Miller (2007) propose an optimal bandwidth selection procedure specific to a RDD context. For each candidate bandwidth, h, the cross-validation function is computed via a leave-one-out procedure, whereby for each observation, i, a regression is estimated omitting observation i and the difference is calculated between the predicted value for observation i from this regression, $\hat{y}(z_i)$, and the actual value y_i. To reflect that RDD estimates are estimated at the boundary, if the value of the running variable for

[16]The Child Benefit Act (1975), enacted 1977, replaced family and child tax allowances paid to the household with child benefit paid directly to the primary child caretaker (usually the mother). Therefore estimates using a window width larger than 5 years may reflect the introduction of this benefit entitlement.

observation i is to the left of the threshold, then the regression uses only observations where $z_i - h \leq z < z_i$. If observation i has a value of Z to the right of the threshold then the regression uses only observations where $z_i < z \leq z_i + h$. Repeating this procedure for each observation i with every possible bandwidth h yields the cross-validation function $CV_Y(h) = \frac{1}{N}\sum_{i=1} N(y_i - \hat{y}(z_i))^2$. The optimal bandwidth is then the value of h that minimises $CV_Y(h)$, the mean square difference of the predicted value to the true value of Y (Imbens and Lemieux, 2008).

4.4 Fuzzy RD

The methodology presented thus far allows the estimation of the impact of an increase in mandatory education from age 15 to age 16 on adolescent motherhood. However a more general determination of the impact of schooling duration on fertility behaviour may be pertinent to policy formation. As the education and fertility decisions are interrelated,[17] a simple estimation of the impact of schooling on fertility using Ordinary Least Squares (OLS) may produce biased estimates.

Using an instrumental variable (IV) approach is a standard method to address such endogeneity. In the context of regression discontinuity design, the IV approach is a 'fuzzy' (FRD) regression discontinuity (Trochim, 1984). The FRD differs from the sharp design, described by (1), insofar that treatment assignment is not required to be a deterministic function of Z_i. Instead the *probability* of receiving treatment as a function of the running variable, $\Pr(T_i = 1|z_i)$, is discontinuous at the threshold, $Z_i = z^*$, as there are factors unobserved by the econometrician that can influence assignment to treatment, such that

[17] Specifically there may be non-observed characteristics that affect both the fertility and education decision. The specification may also suffer from reverse causality: an individual with low academic attainment may choose to become a mother early. This was described by Harris, Duncan, and Boisjoly (2002) as the 'Nothing-to-lose' hypothesis, as such an individual would be likely to have poor economic opportunities regardless of the timing of her fertility. However it is also plausible that an individual who experiences early fertility may elect to curtail her education prematurely in response to motherhood.

treatment participation is not perfectly predicted by the cohort rule. Hahn et al. (2001) argue that the FRD allows the determination of a Wald estimator even when the standard IV assumption is violated. As the estimates are applicable only to the sub-population of individuals, for whom the RoSLA reform actually induced an increase in the schooling (the 'compliers'), the estimated coefficients therefore describe a Local Average Treatment Effect (Angrist and Imbens, 1994).[18]

As in (2) the estimation allows for random, identical specification errors in the estimation and receive robust standard errors by clustering on z_j. The two step approach can be written as:

$$AGELEFT_{ij} = \alpha_1 + \beta_1 T_{ij} + \gamma_1 P_j^l + \delta_1 (T_i \times P_j^l) + a_{1j} + \nu_{1ij} \tag{3}$$

$$Y_{ij} = \alpha_2 + \xi \widehat{AGELEFT}_{ij} + \gamma_2 P_j^l + \delta_2 (T_i \times P_j^l) + a_{2j} + \nu_{2ij} \tag{4}$$

In the first stage (3), the impact of the RoSLA treatment on school-leaving age for individual i born at a distance of j months from RoSLA implementation is estimated, and then included in the second stage equation (4). Thus the Wald estimate, ξ^{FRD}, describes the causal effect of one year of schooling on the fertility outcome of interest Y_{ij}, and is thus equivalent to the ratio of the sharp RDD estimate from equation (2) and the first stage estimate, β_1, so that $\xi^{FRD} = \frac{\alpha^{SRD}}{\beta_1}$. This has an intuitive interpretation: as not everybody responds to the treatment, the reduced form estimate has to be multiplied by the inverse of the proportion of the affected population.

[18]Individuals (the 'always takers') who would always stay in school until age 16 would not have been affected by the increase in school leaving age. As the reform mandated compulsory attendance, the population of 'never-takers' should not exist. Key to identification is the monotonicity assumption that RoSLA had a non-negative effect on an individual's duration of schooling, so that individuals who in absence of the reform would have remained at school after age 16 reduce their duration of education in response to the RoSLA legislation (the 'defiers') are ruled out.

5 Results

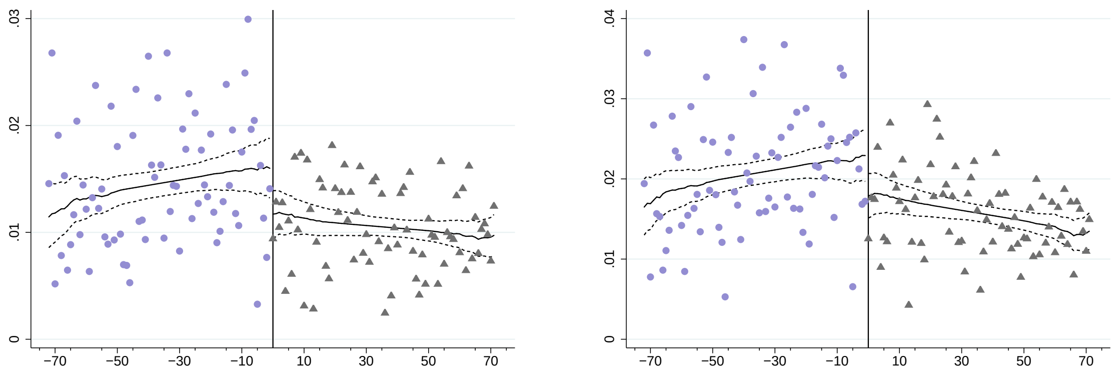
In Section 5.1 the main results explore the impact of the RoSLA reform first over each of the individual teenage years, and also the cumulative effect over the years of adolescence. To examine the extent to which RoSLA bites, the analysis is extended by investigating the extent of any impact of the treatment beyond just the teenage years. The robustness and sensitivity of the analysis is explored in Section 5.2. In Section 5.3 the analysis is extended to examine at the policy relevant question, the impact of years of education on the timing of entry to motherhood.

5.1 Main Results

To illustrate the transparency of the sharp RDD approach, the results are first presented graphically. Figure 2(a) depicts the impact of the reform on the probability of becoming a mother at age 16, whereas the cumulative of becoming a mother before the age of 17 is shown in figure 2(b). These graphs are estimated using the local polynomial smoothing approach, as described in Section 4.1, with a bandwidth of 48 months and a smoothing polynomial of degree 1. In each case the timing of the implementation of the RoSLA reform has been normalised to 0. Appendix A displays the full set of results over each of the outcome variables.

Considering fertility at each of the individual teen years, the graphs in Appendix A are indicitive of a clear difference in fertility before and after the reform, for all but mother at age 17. As RoSLA raised the age of compulsory schooling from age 15 to age 16, the observed effect at age 16 reflects the immediate 'bite' of RoSLA and can be interpreted as the direct incarceration effect associated with the requirement to complete one year of additional schooling. At ages beyond 16, the RoSLA constraint is not binding, and therefore any observed effect cannot be attributed to incarceration alone. The graphs

Figure 2: Graphical Results - Sharp RDD



(a) Mother at age 16

(b) Mother by age 17

Notes: The graphs display local-linear polynomial smooths, as described in Section 4.1, using a bandwidth of 48 months, a smoothing polynomial of degree 1, and a rectangular kernel, of the probability of becoming a mother a) at age 16 and b) before age 17. The horizontal axis measures the distance, in months, of individuals' births to the RoSLA cutoff. The scatterplot indicates the proportions of mothers in each month-bin. The dashed lines are 95% confidence intervals of the local polynomial.

illustrate a non-monotonic impact of the reform over the teenage years, with negative effects for motherhood at age 16 and at age 18, a negligible effect at age 17, and positive effects at age 15 and age 19.

Analytical results are presented in Tables 2 and 3. Panel A displays results estimated using the parametric procedure as detailed in section 4.2, for the probability of becoming a mother at a specific year of age, or before a certain age respectively (thus teenage motherhood is defined as entering motherhood before the age of 20). The first estimation uses the preferred bandwidth of 48 months, then estimates using half and double the preferred bandwidth are displayed to illustrate the robustness of the results to the choice of bandwidth (Imbens and Lemieux, 2008). Panel B shows the bootstrapped estimates and coefficients from the non-parametric method described in Section 4.1.

Examining first the estimations with fertility at a specific age as the outcome variable, the regression coefficients reveal evidence of both an 'incarceration' and a 'beyond

Table 2: Sharp RDD - Mother at specific ages

	At 15	At 16	At 17	At 18	At 19
Panel A:					
BW = 48	0.0015	-0.0040*	-0.0019	-0.0081**	0.0058*
N = 64,359	(0.0011)	(0.0022)	(0.0026)	(0.0034)	(0.0031)
% change	49.67%	-36.73%	-6.85%	-19.63%	11.41%
BW = 24	0.0000	-0.0059*	-0.0021	-0.0038	0.0049
N = 31,566	(0.0015)	(0.0030)	(0.0035)	(0.0051)	(0.0036)
BW = 96	0.0008	-0.0047***	-0.0032	-0.0072***	0.0025
N = 124,458	(0.0008)	(0.0014)	(0.0020)	(0.0026)	(0.0025)
Panel B:					
BW = 48	0.0014*	-0.0043**	-0.0020	-0.0076**	0.0055
N=64,359	(0.0007)	(0.0018)	(0.0028)	(0.0031)	(0.0035)

Notes: Panel A displays estimates from the parametric estimations, as described in Section 4.2, of each dependent variable over columns, with different bandwidths over rows. Robust standard errors, which allow for random and identical specification errors, are reported in parentheses. Panel B shows bootstrapped coefficients and associated standard errors from the local-linear polynomial smoothing procedure described in Section 4.1, using 1,000 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

incarceration' effect. The negative significant effect of 0.40 percentage points at age 16 reflects the direct impact of the increase in the schooling requirement, and can therefore be interpreted as the incarceration effect of RoSLA. This implies that the effect of requiring young women to stay an additional year at school is to reduce the incidence of pregnancy at the age of 16 by 36.73% relative to the sample mean. Although a positive effect at age 15 of 0.15 percentage points is observed, translating to a large increase in the incidence of pregnancy at this age, the estimate is imprecise due to the very small fraction of individuals who experience such early motherhood. A back-of-an-envelope calculation indicates approximately one quarter of the decrease in incidence of motherhood at age 16 may be attributed to individuals bringing fertility forward to age 15.[19]

[19] This quantitatively small effect may be attributed to individuals with preferences for extreme early fertility, who in absence of the RoSLA would have postponed motherhood to age 16, due solely to the social norm of not having a child whilst still in education. However with the increased schooling requirement these individuals find that the perceived cost of delaying fertility one more year is so great that the reform actually induces them to enter motherhood earlier than they would have done in absence of the increase in mandatory schooling.

The pertinent question is whether the remainder of the decrease in incidence of motherhood at age 16 is due to individuals delaying fertility by one year only (a pure incarceration effect) or by more than one year. If pure incarceration only is present, then fertility should shift by one year, which would induce a positive impact of 10% at age 17. However, the coefficient in Table 2 suggests that there is no significant impact of the reform at age 17, in turn implying that some individuals who were not directly constrained by the RoSLA reform also delayed their fertility. This consequently should induce a positive impact at age 18, but the coefficient reveals that there is also a significant decrease in fertilty at age 18 of 0.81 percentage points, almost double the level impact seen at age 16, but implying a lesser decrease of the incidence of motherhood of 19.63% due to the larger number of individuals entering motherhood at this age. Therefore the results provide strong evidence of both incarceration and an additional downward impact of the reform on fertility that cannot be explained purely by incarceration. Furthermore, at age 19 there is a significant positive impact on fertility of 11.41%, which suggests that overall RoSLA induced a postponement of fertility to late teen years.

Table 3: Sharp RDD - Cumulative effect over teen years

	By 16	By 17	By 18	By 19	By 20
Panel A:					
BW = 48	-0.0007	-0.0048*	-0.0067*	-0.0145***	-0.0088*
N = 64,359	(0.0013)	(0.0024)	(0.0037)	(0.0046)	(0.0053)
% change	-14.52%	-29.69%	-15.18%	-17.06%	-6.57%
BW = 24	-0.0018	-0.0076**	-0.0096*	-0.0133*	-0.0082
N = 31,566	(0.0019)	(0.0036)	(0.0051)	(0.0074)	(0.0078)
BW = 96	-0.0002	-0.0049***	-0.0082***	-0.0154***	-0.0132***
N = 124,458	(0.0009)	(0.0016)	(0.0027)	(0.0036)	(0.0041)
Panel B:					
BW = 48	-0.0007	-0.0050**	-0.0070**	-0.0146***	-0.0090
N = 64,359	(0.0012)	(0.0021)	(0.0035)	(0.0046)	(0.0056)

Notes: See notes to Table 2.

The estimates in Table 3 reflect the cumulative effect of the individual year impacts displayed in Table 2. The coefficient for mother by age 16 captures the impact of the RoSLA treatment on the probably of entering motherhood for all ages up to but not including the individuals 16th birthday. Thus the coefficient for mother by age 17 cumulates the 'by 16' effect with the 'at 16' effect from Table 2. Here the clear evidence of the incarceration effect is indicated by the coefficient on mothers by age 17, whereas the beyond incarceration effect is evident from the increasing magnitude of the coefficients for older teenage mothers.

To investigate the extent and duration to which the overall effect of RoSLA on fertility bites,[20] the analysis is extended to investigate fertility outcomes beyond the teenage years. In order to determine the effect on cumulative motherhood 'by' a particular age the sample must be restricted to individuals strictly above that age, that is to observe whether an individual became a mother at any age before her 25th birthday, we must observe her at age 25 or above. Table 4 presents the estimates of cumulative fertility by year up to by age 25. Results for each year of motherhood before age 25 use the sample of individuals aged 25-30; before age 24 use the sample of individuals aged 24-30 and so on.

The estimates in Table 4 confirm that the treatment exerted a significant impact over the teenage years only. The coefficients for each outcome in each of the sub-samples are consistent in sign and magnitude, displaying the same pattern of an increasing magnitudes for ages before 19, and a decrease in the size of the effect before age 20 (consistent with the positive impact at age 19 as shown in Table 2). After age 20 the impact of RoSLA on fertility is quantitatively small relative to the sample mean, and statistically indistinguishable

[20]Note that the analysis of the impact of RoSLA on fertility is restricted to the incidence and timing of fertility. To investigate quantum fertility requires knowledge of completed fertility, which is generally measured as the number of children per woman at age 45. However, as previously discussed, in order to accurate determine teenage motherhood it is necessary to restrict the sample to individuals aged between 20 and 30, and therefore it is not possible to investigate the impact of RoSLA on the number of children per woman. Administrative data indicates that there is no difference in completed fertility between pre-RoSLA and post-RoSLA cohorts beyond the long-run (downward) trend (ONS, Cohort Fertility, England & Wales, 2010), a result also found in the cohort analysis of Clark et al. (2014).

Table 4: Sharp RDD - Extended results - cumulative years

	By 16	By 17	By 18	By 19	By 20	By 21	By 22	By 23	By 24	By 25
25 - 30 sample	-0.0009	-0.0019	-0.0062	-0.0164***	-0.0132*	-0.0040	0.0011	-0.0017	-0.0052	-0.0110
N = 39,912	(0.0015)	(0.0029)	(0.0043)	(0.0058)	(0.0067)	(0.0070)	(0.0078)	(0.0088)	(0.0084)	(0.0094)
24 - 30 sample	-0.0015	-0.0036	-0.0066	-0.0151***	-0.0095	-0.0016	0.0037	-0.0012	-0.0059	
N = 45,621	(0.0014)	(0.0028)	(0.0041)	(0.0053)	(0.0061)	(0.0067)	(0.0080)	(0.0085)	(0.0080)	
23 - 30 sample	-0.0011	-0.0040	-0.0066*	-0.0150***	-0.0090	-0.0028	0.0034	-0.0014		
N = 51,164	(0.0014)	(0.0027)	(0.0039)	(0.0049)	(0.0057)	(0.0065)	(0.0082)	(0.0084)		
22 - 30 sample	-0.0007	-0.0046*	-0.0071*	-0.0163***	-0.0108*	-0.0030	0.0029			
N = 56,204	(0.0014)	(0.0027)	(0.0039)	(0.0049)	(0.0056)	(0.0064)	(0.0079)			
21 - 30 sample	-0.0008	-0.0048**	-0.0060*	-0.0154***	-0.0106*	-0.0029				
N = 61,023	(0.0013)	(0.0025)	(0.0036)	(0.0047)	(0.0055)	(0.0060)				
20 - 30 sample	-0.0007	-0.0048**	-0.0067*	-0.0145***	-0.0088*					
N = 64,359	(0.0013)	(0.0024)	(0.0037)	(0.0046)	(0.0053)					

Notes: The table shows estimates from local parametric estimations, as described in Section 4.2, of each dependent variable over columns, using different sub-samples over rows as indicated. Robust standard errors, which allow for random and identical specification errors, are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

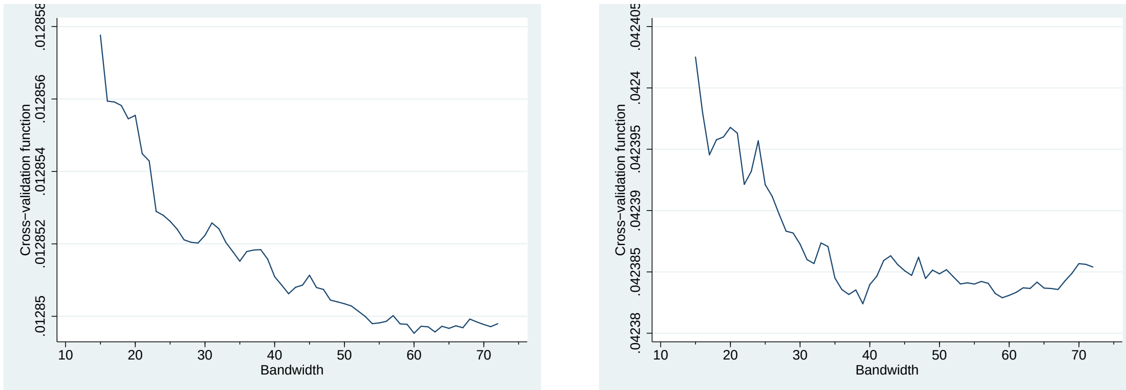
from zero.

5.2 Sensitivity Analysis

Panel A from Tables 2 and 3 included estimates using three different bandwidths, the preferred, as well as double and half this bandwidth, to illustrate the robustness of the estimates to the choice of bandwidth. The preferred bandwidth was chosen according to the cross-validation procedure as described in Section 4.3, calculated and examined for each of the outcome variables in turn. This analysis did not yield a unique optimal bandwidth appropriate for all outcome variables, however a bandwidth between 36 and 60 months was consistently indicated.

As an illustration, Figure 3(a) displays the cross-validation function for mother at age 16 over bandwidths ranging from 15 to 72 months. The function decreases in value as the size of bandwidth increases, but the graph suggests that increases in bandwidth above 40 exert little difference in the magnitude of the function. The cross-validation function for mother at age 18 is displayed in Figure 3(b). In this case the function does suggest a

Figure 3: Cross-Validation



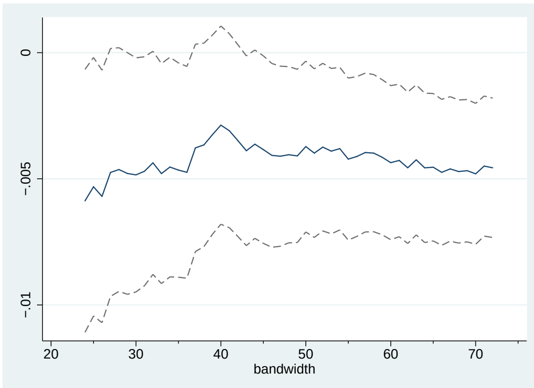
(a) Mother at age 16 (b) Mother at age 18

Notes: The graphs display the cross-validation function calculated as described in Section 4.3. The optimal bandwidth is given by the minimand of the function $CV_Y(h) = \frac{1}{N}\sum_{i=1} N(y_i - \hat{y}(z_i))^2$.

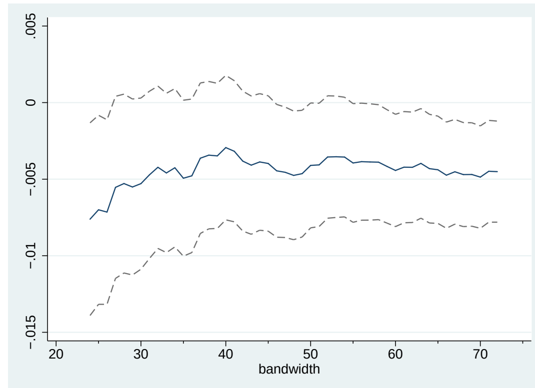
clear minimand, at approximately 40 months. The cross-validation functions for each of the outcome variables are displayed in Appendix A.

A corollary to the cross-validation procedure is to directly examine the sensitivity of the estimates to bandwidth choice. Figure 4 displays the magnitude of the coefficients estimated using bandwidths ranging between 18 and 72 for fertility at (a) age 16 and (b) up to, but not including age 17. The estimated impact displays some sensitivity to smaller bandwidths, but the magnitude of the estimates is essentially stable for bandwidths greater than 40. This is a reflection of what was seen in Figure 3(a), that increases in bandwidth exert little effect on the cross-validation function for bandwidths greater than 40. Appendix A includes the full set of results displaying the sensitivity of the estimates to bandwidth choice over each of the outcome variables. The graphs generally indicate stability in the estimated coefficients for all outcome variables at bandwidths from approximately 40 onwards, apart from the estimates for mother at age 18 (also affecting cumulative fertilty by ages 19 and 20), which achieve stability after approximately 60 months.

Figure 4: Sensitivity of Estimates to bandwidth choice



(a) Mother at age 16



(b) Mother by age 17

Notes: The graphs display the magnitude of the estimates, along with the 95% confidence interval, over different bandwidths based on the parametric regression discontinuity design as described in Section 4.2.

At the boundary the comparison is between individuals born at the end (August) of one academic cohort with individuals who are born at the beginning (September) of the next academic cohort. The key identifying assumption is that individuals in the neighbourhood of the discontinuity are identical in characteristics apart from their assignment to the treatment. However there may be fundamental differences in individuals according to their relative and social age within an academic cohort and therefore the RDD estimation, which is essentially a between-cohort comparison at the boundary, may just reflect compositional differences of those born at the beginning versus the end of a cohort. For instance, Crawford, Dearden, and Meghir (2010) find that relative age within a cohort exerts an important influence on academic outcomes, younger individuals in a cohort perform on average significantly worse than their older peers in assessments, which the authors attribute to the absolute age of the individual when taking the test. In the context of fertility behaviour, *a priori* it may be expected that older individuals within a cohort would have higher fertility due to their higher emotional and physical maturity, as forging a relationship requires a set of social skills that are likely to be more developed in individuals born earlier within a cohort. In addition because fecundability increases over the period of adolescence (Wood and Weinstein, 1988), older individuals are more able to conceive. However, analyzing the fertility outcomes within academic cohorts in Sweden, Skirbekk, Kohler, and Prskawetz (2004) find that individuals born at the beginning of a cohort actually enter motherhood up to 4.9 months later than those born at the end of the academic cohort, which the authors attribute to the 'social age' effect.

Table 5: Placebo Analysis

Panel A	At 15	At 16	At 17	At 18	At 19
1951	-0.0032**	0.0011	0.0002	0.0010	-0.0005
N=42,803	(0.0014)	(0.0026)	(0.0031)	(0.0043)	(0.0043)
RoSLA	0.0015	-0.0040*	-0.0019	-0.0081**	0.0058*
N=64,359	(0.0011)	(0.0022)	(0.0026)	(0.0034)	(0.0031)
1964	-0.0023**	0.0013	-0.0032	0.0000	-0.0032
N=73,021	(0.0009)	(0.0017)	(0.0025)	(0.0033)	(0.0034)

Panel B	By 16	By 17	By 18	By 19	By 20
1951	-0.0019	-0.0009	-0.0006	0.0004	-0.0002
N=42,803	(0.0025)	(0.0040)	(0.0035)	(0.0068)	(0.0106)
RoSLA	-0.0008	-0.0049**	-0.0068*	-0.0147***	-0.0089*
N=64,359	(0.0013)	(0.0024)	(0.0037)	(0.0046)	(0.0053)
1964	-0.0016	-0.0002	-0.0034	-0.0033	-0.0066
N=73,021	(0.0012)	(0.0020)	(0.0036)	(0.0040)	(0.0050)

Notes: The table shows estimates from parametric estimations, as described in Section 4.2, of each dependent variable over columns using the preferred bandwidth of 48 months, with the discontinuity defined in different years over rows. Robust standard errors, which allow for random and identical specification errors, are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In order to confirm that the results presented in Section 5.1 are indeed driven by the reform rather than inherent between cohort effects two further robustness checks are undertaken. Firstly a falsification exercise is undertaken, placebo regressions are estimated under the assumption that RoSLA was implemented prior to or after actual implementation. The results of the placebo analysis are displayed in Table 5, which are not consistent with the estimates that use the correct RoSLA assignment. The sign, magnitude and significance of the coefficients differ non-systematically, suggesting that observed effect on fertility is in fact driven by the implementation of RoSLA.

Table 6: RD-DiD estimates

Panel A	At 15	At 16	At 17	At 18	At 19
Pre-RoSLA DiD	0.0013	-0.0066***	-0.0116***	-0.0188***	0.0006
N = 79,852	(0.0010)	(0.0017)	(0.0029)	(0.0036)	(0.0035)
Pre-Post RD-DiD	0.0008	-0.0043***	-0.0037*	-0.0091***	0.0007
N = 137,502	(0.0008)	(0.0012)	(0.0022)	(0.0025)	(0.0028)

Panel B	By 16	By 17	By 18	By 19	By 20
Pre-RoSLA DiD	0.0003	-0.0062***	-0.0179***	-0.0363***	-0.0355***
N = 79,852	(0.0012)	(0.0022)	(0.0037)	(0.0051)	(0.0061)
Pre-Post RD-DiD	0.0000	-0.0043***	-0.0080***	-0.0169***	-0.0165***
N = 137,502	(0.0009)	(0.0015)	(0.0026)	(0.0036)	(0.0042)

Notes: The table shows estimates from the Regression Discontinuity difference in difference procedure, as described in Section 5.2 of each dependent variable over columns, using non-overlapping windows of observations and a bandwidth of 36 months. The Pre-RoSLA RD-DiD is estimated over the 47/48 - 52/53 and 53/54 - 59/60 windows. The Post-RoSLA RD-DiD is estimated over the 53/54 - 59/60 and 60/61 - 64/65 windows. The Pre-Post RD-DiD is estimated over all three windows. Robust standard errors, which allow for random and identical specification errors, are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Second, following Danzer and Lavy (2013), a difference in difference approach is applied in the context of the regression discontinuity design (RD-DID). This procedure explicitly nets out any inherent between cohort differences at the August-September threshold by using three non-overlapping windows of observations[21]- the pre-RoSLA period (academic cohorts 1947/48 - 1952/53), the post-RoSLA period (1960/61 - 1964/64) and the period around the RoSLA discontinuity (1953/54 - 1959/60). For each sub-period the running variable is defined as the distance in months from the relevant August-September threshold.

[21] Distinct windows are required to form the counterfactual observations. In order to accommodate the total observation window a bandwidth of 36 months is used in the estimations

Two versions of following specification are then estimated:

$$Y_{ij} = \beta_0 + \beta_1 Right_{ij} + \beta_2 RoslaRight_{ij} + \Sigma_{k=1}^{3} Period_k + \gamma_0 P_j^l + \delta_0 (T_i \times P_j^l) + a_j + \epsilon_{ij} \quad (5)$$

where Y_{ij} is the outcome of interest for individual i born at a distance of j from the relevant threshold; *Right* is an indicator variable for an observation being on the right-hand side of the relevant discontinuity; *Period* are period dummies for each window of observations; *RoslaRight* is a dummy equal to 1 if the observation is on the right-hand side of the discontinuity in the period around the RoSLA discontinuity, thus β_2 describes the RD-DiD estimate. The γ and δ capture the polynomial smooth in the running variable.

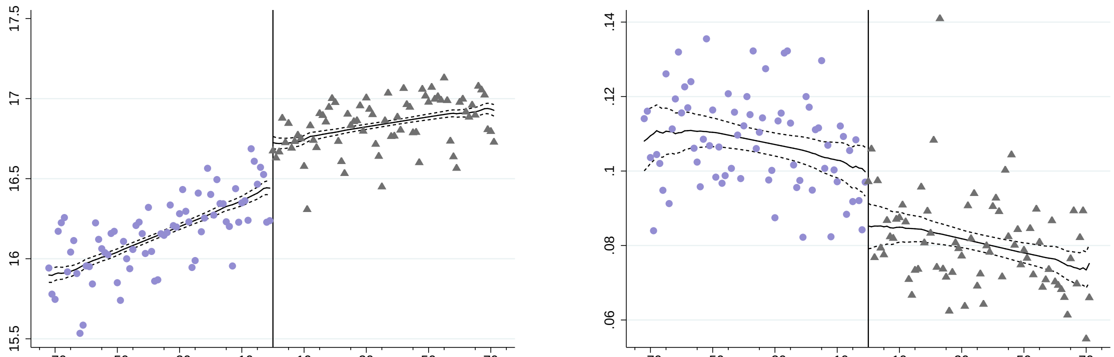
Table 6 presents the results of the difference-in-difference analysis considering first the pre-RoSLA period only as counterfactual observations, and second using both pre and post-RoSLA periods for comparison. The estimates are qualitatively similar to those presented in the main analysis and therefore adjusting the original RoSLA coefficients to account for any inherent between-cohort discontinuities does not an induce a significant impact on the sign or magnitude of the RDD estimates.

5.3 Further Estimations

Finally the analysis considers the impact of education as measured by years of schooling on adolescent fertility in a two-stage approach. In the first stage the impact of the RoSLA reform on schooling duration, measured by the age at which an individual finished full-time education is measured. This prediction is used in the second stage to analyse the effect on the probability of entry to motherhood. Figure 5 presents these two stages graphically.

The analytical results are reported in Tables 7 and 8. The top panel presents the Wald Estimates using the preferred bandwidth of 48 months, as well as estimates produced using half and double the preferred bandwidth. The middle panel displays results of simple OLS

Figure 5: Graphical results - Fuzzy RDD



(a) Age left full-time education

(b) Mother by age 19

Notes: The graphs display local-linear polynomial smooths, as described in Section 4.1, using a bandwidth of 48 months, a smoothing polynomial of degree 1, and a rectangular kernel, for a) age an individual left school (first-stage of the fuzzy RDD) and b) the probability of becoming a mother before age 19 (second-stage of the fuzzy RDD) . The horizontal axis measures the distance, in months, of individuals' births to the RoSLA cutoff. The scatterplot indicates the proportions of mothers in each month-bin. The dashed lines are 95% confidence intervals of the local polynomial.

regressions of the impact of years of schooling on the probability of teen motherhood, and the bottom panel presents the reduced form and first stage of the estimation (for expositional convenience only the preferred bandwidth estimates are reported in these latter panels).

The OLS coefficients consistently indicate that there is a negative relationship between an individual's propensity of early motherhood and the age at which she left full-time education. However, as discussed in Section 4.4, there may be omitted variables which imply that the residual term is correlated with years of education. If the unobserved heterogeneity is such that it asserts a positive impact on the propensity for early motherhood and a negative impact on schooling, then the OLS coefficients will be biased upwards. Conversely if the unobserved heterogeneity impacts both teen motherhood and years of schooling in the same direction, then the OLS estimates will be understated. This potential endogeneity is addressed using the FRD procedure described in Section 4.4. Recall this is analogous to an

Table 7: Fuzzy RDD - Impact of years of education - individual years

	At 15	At 16	At 17	At 18	At 19
Wald Estimates					
BW = 48	0.0047	-0.0147	-0.0064	-0.0250*	0.0202*
N= 64,359	(0.0035)	(0.0082)	(0.0086)	(0.0124)	(0.0100)
BW = 24	-0.0010	-0.0207	-0.0079	-0.0126	0.0179
N = 31,566	(0.0050)	(0.0124)	(0.0120)	(0.0184)	(0.0126)
BW = 96	0.0024	-0.0176**	-0.0132*	-0.0244**	0.0091
N = 124,458	(0.0025)	(0.0054)	(0.0065)	(0.0092)	(0.0081)
OLS					
Years of Education	-0.0004**	-0.0030***	-0.0069***	-0.0088***	-0.0096***
	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
IV					
Reduced Form	0.0014	-0.0043*	-0.0019	-0.0073**	0.0059*
	(0.0010)	(0.0023)	(0.0027)	(0.0033)	(0.0030)
First stage	0.2934***	0.2934***	0.2934***	0.2934***	0.2934***
	(0.0586)	(0.0586)	(0.0586)	(0.0586)	(0.0586)

Notes: The table shows estimates from local parametric estimations, as described in Section 4.2, of each dependent variable over columns, using a bandwidth of 48 months. First-stage F-statistic = 25.07. Robust standard errors, which allow for random and identical specification errors, are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

IV approach, where the RoSLA treatment is applied as an instrument for schooling. The identification assumption is that the timing of the RoSLA implementation is orthogonal to unobserved determinants of motherhood, and therefore the effect of the reform on fertility can be understood as operating only through its impact on years of education. The first stage reveals that the reform had a significant positive impact on years of schooling, raising it on average by approximately 3 months, which reflects that prior to implementation of RoSLA a substantial proportion of the school age population already stayed at school until at least age 16, as depicted in Figure 1(c). Considering the wald estimates over the individual years, Table 7, of the effect of the duration of education on teen motherhood, these differ from the OLS estimates non-systematically: the coefficients on mother at age 15 and mother at age 19 change sign (from positive to negative) indicating that the OLS estimates of these coefficients are downwardly biased. The coefficients on mother at age 16 and at age 18 are the same sign (negative) as the OLS coefficients, and are larger in magnitude indicating that the OLS estimates are understated. The coefficient on mother at age 17 also has the same sign (negative) but is smaller in magnitude than the OLS coefficient. These observations imply that not only does RoSLA have a varying impact on fertility depending on the age of the mother, but also that the correlation between unobserved factors and years of schooling varies throughout the teen years.

Table 8: Fuzzy RDD - Impact of years of education - cumulative years

	By 16	By 17	By 18	By 19	By 20
Wald Estimates					
BW = 48	-0.0026	-0.0173	-0.0237	-0.0487**	-0.0226
N= 64,359	(0.0047)	(0.0094)	(0.0122)	(0.0164)	(0.0187)
BW = 24	-0.0058	-0.0248	-0.0296	-0.0398	-0.0207
N = 30,338	(0.0065)	(0.0150)	(0.0188)	(0.0269)	(0.0297)
BW = 96	-0.0011	-0.0193**	-0.0346***	-0.0575***	-0.0519**
N = 118,388	(0.0031)	(0.0062)	(0.0098)	(0.0132)	(0.0161)
OLS					
Years of Education	-0.0007***	-0.0037***	-0.0106***	-0.0194***	-0.0290***
	(0.0001)	(0.0002)	(0.0003)	(0.0004)	(0.0006)
IV					
Reduced Form	-0.0008	-0.0051*	-0.0070*	-0.0143**	-0.0084
	(0.0014)	(0.0026)	(0.0038)	(0.0045)	(0.0053)
First stage	0.2934***	0.2934***	0.2934***	0.2934***	0.2934***
	(0.0586)	(0.0586)	(0.0586)	(0.0586)	(0.0586)

Notes: see notes for Table 7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Considering the cumulative fertility estimates, Table 8, the first stage of course is identical to that in Table 7. A comparison of the OLS and the wald estimates reveals that they all share the same sign, in contrast to the results in Table 7 for the estimations at each of the individual teen ages. The cumulative estimates thus suggest that any positive correlation between the measure of education and unobservables (such as at age 16) is offset by negative correlation (for instance at age 15).

To reconcile the differences between the reduced form (sharp RDD) and the wald (fuzzy RDD) estimates, recall that the SRD measures the causal effect of the reform, which is the average effect of being subject to the RoSLA regime in comparison to the pre-RoSLA regime (on average an extra three months of schooling). In contrast, the FRD approach rescales the reduced form results so that the Wald estimates reflect the effect on the propensity for motherhood of an additional year of education for the sub-population of individuals who were induced to increase the duration of schooling by the RoSLA reform.

6 Conclusion

This paper has investigated the impact of an increase in the minimum compulsory school-leaving age on teenage fertility rates, using data from the UK Labour Force Survey, the largest representative UK household survey. The findings indicate a non-monotonic impact over the individual teenage years. In contrast to previous research, the results provide strong evidence of a large incarceration effect. This discrepancy may be explained by the proportion of individuals directly affected by the institutional change to mandatory education. The Norwegian reform analysed by Black et al. (2008) increased the duration of schooling by two years, yet the estimated increase to individuals' education was just 0.122 years, indicating that only a small fraction of the population were impacted. In contrast the UK's RoSLA, compelling an increase to compulsory schooling of just one year, increased the average years of schooling by 0.293 years due to the higher proportion of individuals affected. Hence although the incarceration effect, by capturing the shift in fertility for the age at which the legislation bites, may be thought of as just a mechanical response to the extra year of schooling induced by the legislation change, the evidence suggests that if mandating a higher school graduating age raises the schooling durations of a large share of the school-age population, teenage fertility rates will be substantially affected.

Unfortunately, the data used in this analysis does not allow examination of the mechanism that results in the beyond incarceration effect, the question therefore remains to what extent this is attributable to the impact of education on human capital acquisition. Extending the analysis beyond the teenage years revealed that the impact of RoSLA was to essentially induce a postponement of fertility from early teen to the late teenage years, with a large increase in the incidence of fertility at age 19, and the impact of the increase in compulsory education tailing off after age 20. Given that these individuals continued to bear children at a relatively young age, a question for future research is whether this postponement of fertility positively impacted outcomes for these mothers and their children.

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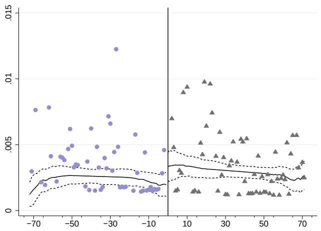
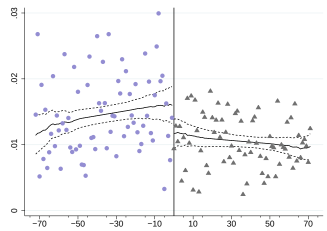
Appendices

A Full Graphical Results

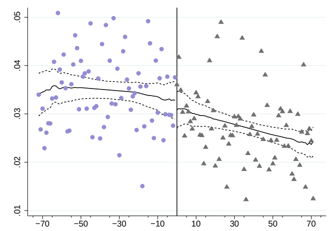
Figure A.1: Sharp RDD for all outcome variables



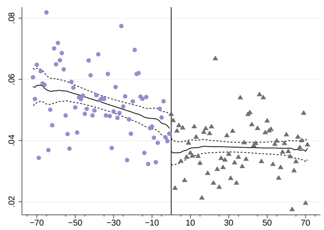
(a) Mother at age 15



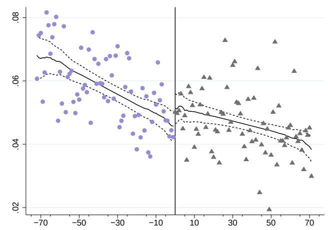
(b) Mother at age 16



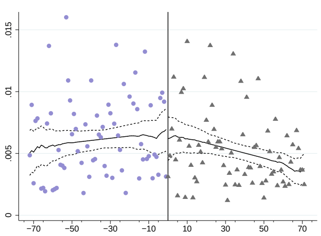
(c) Mother at age 17



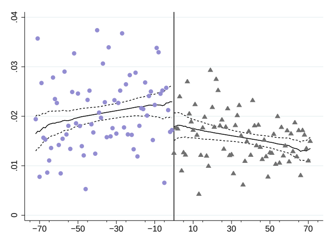
(d) Mother at age 18



(e) Mother at age 19



(f) Mother by age 16



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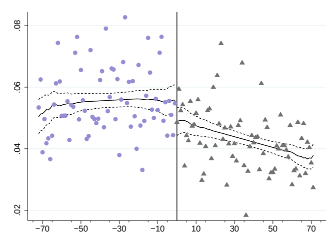
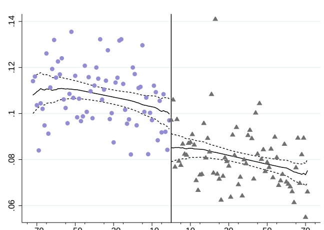
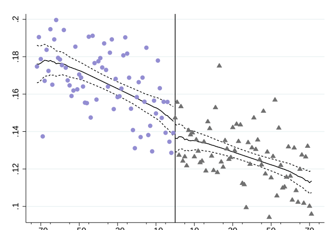


(h) Mother by age 18



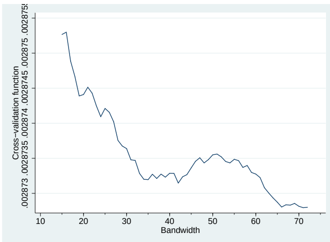
(i) Mother by age 19



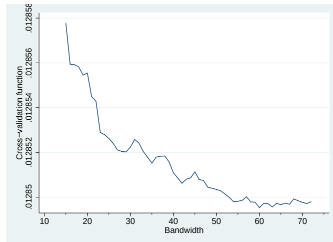
(j) Mother by age 20

Notes: The graphs display local-linear polynomial smooths, as described in Section 4.1, using a bandwidth of 48 months and a rectangular kernel, for the probability of becoming a mother at age 15 - age 19 (graphs a - e) and by age 16 - by age 20 (graphs f - j). The horizontal axis measures the distance, in months, of individuals' births to the RoSLA cutoff. The scatterplot indicates the proportions of mothers in each month-bin. The dashed lines are 95% confidence intervals of the local polynomial.

Figure A.2: Cross-Validation functions



(a) Mother at age 15



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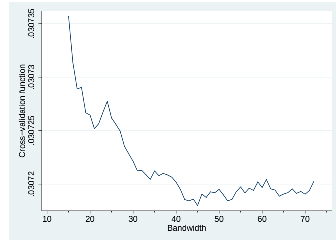
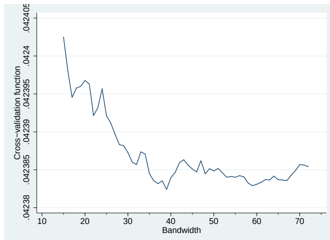
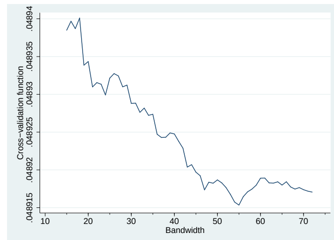


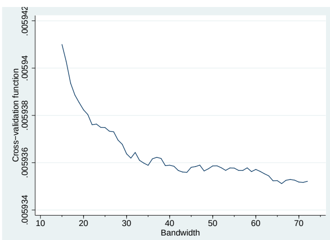
(c) Mother at age 17



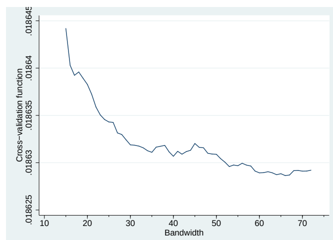
(d) Mother at age 18



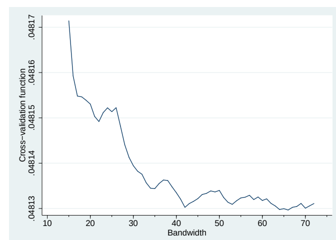
(e) Mother at age 19



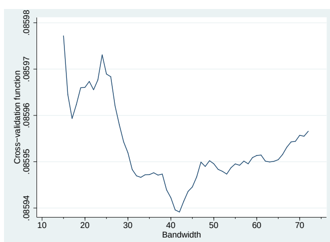
(f) Mother by age 16



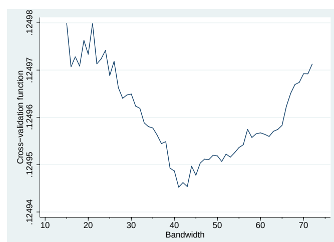
(g) Mother by age 17



(h) Mother by age 18



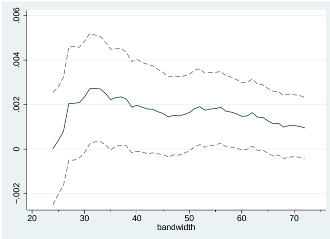
(i) Mother by age 19



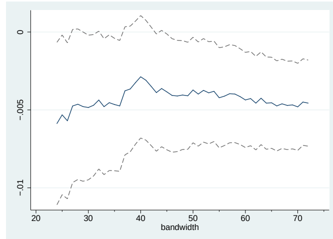
(j) Mother by age 20

Notes: The graphs display the cross-validation function for each of the outcome variables over the range of bandwidths following the procedure described in Section 4.3. The optimal bandwidth is defined as the value of the bandwidth, h, that minimizes the cross-validation function, $CV_Y(h)$, which is computed as the mean square difference of the predicted value to the true value of Y

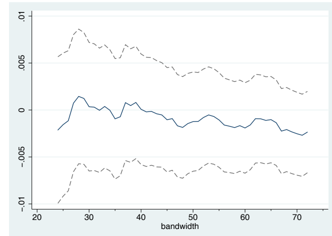
Figure A.3: Sensitivity of Estimates to bandwidth choice



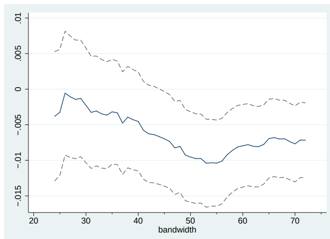
(a) Mother at age 15



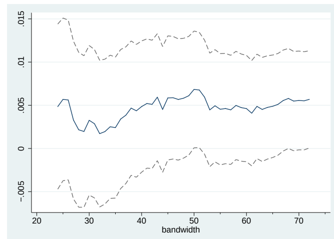
(b) Mother at age 16



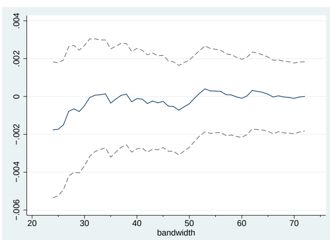
(c) Mother at age 17



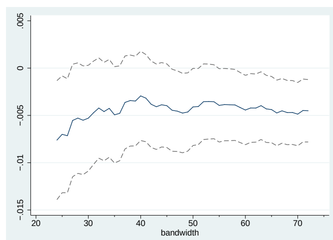
(d) Mother at age 18



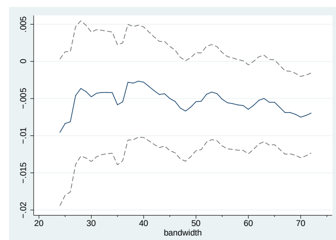
(e) Mother at age 19



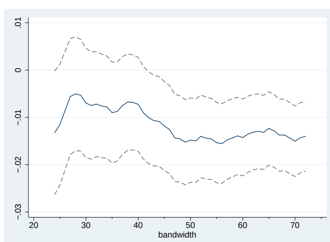
(f) Mother by age 16



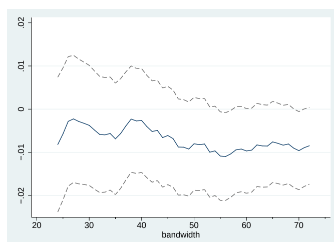
(g) Mother by age 17



(h) Mother by age 18



(i) Mother by age 19



(j) Mother by age 20

Notes: The graphs display the magnitude of the estimates, along with the 95% confidence interval, over different bandwidths based on the parametric regression discontinuity design as described in Section 4.2.