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From High School to the High Chair: Education and Fertility Timing*

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Abstract

We exploit an expansion of post-compulsory schooling that occurred from the late 1980s to the early 1990s to investigate the effect of education on the timing of fertility in England and Wales. We do not find a significant effect on the probability of having a child as a teenager but instead find that the variation in education led to delays in childbearing. Our estimates suggest that an increase in education by one year led to a 5.3% increase in probability of birth aged 24 or above, 9.4% increase in probability of birth aged 27 or above, and 13.3% increase in probability of birth aged 30 or above. The mechanisms driving these findings are not due to an incapacitation effect – by keeping young people in school or university they have less time or opportunity to have a child – but due to a combination of human capital and signalling effects.

Keywords: Education; Fertility Timing.

JEL classification: I26, J13

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1 Introduction

Teenage fertility rates in the United Kingdom (UK) are the highest in Western Europe, and second only to the United States (US) in the developed world. On average, however, adolescent fertility rates have fallen in many developed countries. For example, Figure 1A shows that there has been a considerable reduction in the variation in teen fertility rates over the last fifty years, with a significant convergence across countries. In the UK, the teen birth rates have also fallen dramatically, from 85 births per 1,000 women aged 15–19 in 1960 to just under 30 in 1980, stabilising over the period until the 1990s when the trend began to fall again. Reduction in the number of births to teenagers has been a significant policy target because of the negative consequences for both the effects on the child (Royer, 2004; Francesconi, 2008) and the mother (Chevalier and Viitanen, 2003).

There is also a concern with delays in fertility. The average mean age of women at first birth has risen by almost three years in the last two decades (OECD, 2016). Women after the age of 35 face a higher risk of having a preterm birth and are more likely to have a child with an abnormal condition as well as suffering other complications (Royer, 2004; Jolly et al., 2000). Delays in fertility may ultimately lead to childlessness, which may then have knock-on effects on replacement rates. For example, Figure 1B shows age-specific fertility rates at selected ages by year of birth in England and Wales between 1920 to 1995. There are many more births to women over 35 compared to forty years ago. Not only has the average age increased (for example, the average age of women at first birth in the UK was 28.6 in 2014 compared to 26.6 in 1995; OECD (2016), Figure 3.6), but the tail of the distribution has become longer as well.

There have been a number of policy interventions in the UK aimed at reducing teenage fertility[1] and improving health during pregnancy.[2] The other side of the policy, other than direct prevention, is to mitigate any potential detrimental effects of teen pregnancy by helping mothers during pregnancy, and once the child has been born. Education is in the policy framework but it is typically focused on getting teenage parents back into education.[3] Indeed, there is a strong correlation between education and maternity rates among under 18-year-olds. Figure 1C shows that the correlation between having no

[1]The teen pregnancy unit was set up in 1999 with a target of reducing the level of teenage pregnancy by half by reducing the probability of social exclusion through increasing education participation and labour market prospects

[2]Introduced in April 2009, the Health in Pregnancy grant gave pregnant women £200 to encourage healthy eating.

[3]In the UK government's Teen Pregnancy Strategy published in June 1999 the word "education" is mentioned 304 times. This is almost three times as many mentions as the word "contraception" (mentioned 120 times).

qualifications and maternity rates of women under 18 is 0.68 (seen in the left panel of Figure 1C). The right panel shows a strong negative connection between maternity rates of women under 18 and proportion of them with a degree. The focus in this paper is on the potential impact of improving education as a mean to prevent teen fertility and postpone first birth, rather than directly tackling the problem once it has occurred. For example, Kearney and Levine (2012), find that the lack of opportunity and being on a low economic trajectory is the cause of teen childbearing, and that the lack of the chance of advancement prevents the investment in teen human capital.

In this paper we examine the effect of education on the reduction in teen fertility and the extent to which fertility is delayed due to more schooling. In order to demonstrate this, we exploit variation in education due to a reform in England and Wales which took place in the late 1980s and early 1990s. This reform was a combination of changes in policies that led to a large expansion in education, which significantly raised education levels across the whole education distribution, thereby considerably reducing the number of individuals with low education levels in birth cohorts exposed to the expansion. Our approach is to think of these cohorts as a 'treated' set of individuals whose education was raised and we can compare their education and the timing of first birth with a 'control' set of cohorts who did not benefit from the expansion. Overall, the proportion of 18-year-olds in full-time education rose from around 17% in 1985 to over 35% in the late 1990s. Further, the proportion of women with a college degree increased from 13% to 30% from the late 1980s to the early 1990s (Walker and Zhu, 2008). We call this period of change the education expansion (EE).

This is the first paper that utilises the large expansion of the UK post-compulsory education system that occurred in the late 1980s and early 1990s in order to investigate the relationship between education and fertility timing. We use an instrumental variables (IV) approach to show that the raise in education levels as a result of the EE did not lead to a statistically significant reduction in the probability of having a child as a teenager. However, we do find that this source of variation in education led to delays in having a first child for women.[4]

[4]In order to calculate the age of the mother we have to observe both the mother and the eldest child within a household at the time of the survey. In the case of parental separation we therefore assume that the child stays with the mother. We can do the same for men and estimate the impact of education on the timing of fatherhood – these results are presented in the Appendix B. However, the assumption of the child staying with the father is more questionable given that around nine in ten children living in lone parent families lived with their mothers in the UK in 2008 (ONS (2009)). Furthermore, the proportion of lone parent households had increased in the period since the education expansion with the proportion of children living with lone mothers increasing from 19% in 1997 to 22% in 2008, while the proportion of children living with just their father remained stable, at around 2% (ONS (2009)). Therefore, while it is possible to examine the impact of education on fatherhood, we would have to rely on stronger set of

The body of evidence points to the effect of education on fertility being driven by a combination of a human capital effect and an incapacitation or incarceration effect: by keeping young people in school or university they have less time or opportunity to have a child (Black et al., 2008). However, we find no evidence that the mechanism driving our results are due to an incapacitation effect because the EE has effects that are beyond the ages that would be binding. Instead the results point to both a direct human capital effect and an improvement in labour market opportunities as a result of the raise in education levels and holding qualifications.

The remainder of the paper is organised as follows. Section 2 provides a literature review which further introduces and motivates the paper's analysis. Section 3 provides the institutional setting for the expansion of the UK post-compulsory education, while Section 4 outlines the empirical strategy. Section 5 describes the data utilised in the paper, and Section 6 provides the estimation results and the robustness checks. Section 7 concludes the paper and provides a discussion of the estimated results.

2 Literature Review

Related literature can be divided into three strands: (i) the literature on private and social returns to education; (ii) the literature which discusses the conceptual relationship between education and fertility timing; and (iii) previous empirical evidence on the effect of education on fertility.

(i) The causal impact of education has been found to have resulted in large private labour market returns (Harmon and Walker, 1995; Oreopoulos, 2006). It is also the case that there are social outcomes that result from a more educated population, such as reductions in crime (Lochner and Moretti, 2004, for the US, and Machin et al., 2011, 2012, for the UK), enhanced political engagement and attitudes in democracy (Milligan et al., 2004; Dee, 2004), improvements in health through reduced mortality (Lleras-Muney, 2005), and better lifestyle behaviours such as more exercise (Park and Kang, 2008). Oreopoulos and Salvanes (2011) provide further evidence on the nonpecuniary benefits of schooling. Timing of fertility is another potential area in which improvement in education could have a further positive spillover effect.

(ii) Conceptually there are two main channels through which education could reduce a birth at an early age and delay childbearing, and these are somewhat analogous to the education and crime literature: (1) a human capital (Black et al., 2008) or income effect

assumptions and treat the results with much more caution. We therefore report the results for fathers in Appendix B.

(Lochner, 2004); and (2) an incarceration effect (Black et al., 2008).[5] The human capital effect implies that increases in the education level for women mean that having a child at an early age becomes much more costly. Higher education levels increase future wages thereby increasing the opportunity cost of teenage pregnancy. Exam and/or curriculum changes may also have similar effects, if not just the quantity of education but also the quality is important (for example, through improvements in human capital; attained education might be more appropriate for the current labour market and the needs of society; there could be relevant signalling effects – Arrow, 1973; Spence, 1973). There are also relevant 'knowledge' effects of prolonged schooling, which give young women (through school material, Internet, and libraries) increased access to information such as family planning and contraception (Thomas et al., 1991). If these mechanisms are in place, a successful educational change should then lead to reduction in teenage pregnancy and to delay in childbearing. In the incarceration effect mechanism it is argued that keeping young women at school increases the cost of being a mother whilst at school, and therefore may lead not necessarily to overall changes in fertility behaviour, but instead to a postponement of fertility.

(iii) Several empirical approaches have been used in order to estimate the causal effect of education on the reduction of teenage pregnancies and postponement of fertility. The most common identification approach is the instrumental variables (IV) approach which uses compulsory school leaving age laws as an instrument for education (Black et al., 2008 for US and Norway; Monstad et al., 2008 for Norway; León, 2004 for US; Silles, 2011, Braakmann, 2011, Wilson, 2012, and Geruso and Royer, 2014 for the UK; Fort et al., 2016 for England and Continental Europe; Cygan-Rehm and Maeder, 2013 for Germany; Kidar et al., 2009 for Turkey; Fort, 2006 for Italy; Lavy and Zablotsky, 2011 for Arabs in Israel). Further examples include variation in the years of education based on the date of birth and age-at-school-entry policies (McCrary and Royer, 2011) or the timing of school construction (Breirova and Duflo, 2004); introduction of universal primary education (Osili and Long, 2008), variation in the content of education by prolonging vocational tracks in the upper secondary school (Grönqvist and Hall, 2013), delayed college enrolment (Humlum et al., 2012), or variation in the costs of education by providing a free uniform

[5] The third potential channel could be patience and risk aversion effects (Lochner and Moretti (2004)). The future returns are discounted depending on the woman's patience. Education may influence patience levels, and therefore the degree to which individuals discount the future. Therefore, those with a lower discount rate are more likely to wait and delay child bearing relative to those with greater discount rates. Many students may downplay the future benefits when they occur later in life, generating time inconsistency problems (Frederick et al. (2002)). Additional education may give greater weight to the future benefits, even without increasing those benefits, thus reducing risky behaviours at an early age. However, we cannot arguably identify this channel using our identification strategy.

(Duflo et al., 2015). Other approaches also include within-sibling (twin) fixed effects (Vikesh and Behrman, 2014; Geronimus and Koreman, 1992; Grogger and Bronars, 1993), instrumentation of age at first birth by age at menarche (Ribar, 1994), and miscarriage as an instrument for teenage mothers (Hotz et al., 2005).

Findings from the previous literature, examining the effect of education on fertility, can be summarised as follows (Table 1): additional years of education result in reduction of teenage pregnancies and postponement of first birth to early 20s or later. The literature finds an ambiguous effect on overall fertility. Very few papers look at the effect of education on the fertility of fathers (Grönqvist and Hall, 2013), finding no significant effect.[6] Using the changes in the compulsory school leaving age laws in the 1960s and 1970s as an instrument for education, previous studies for the UK show that increased schooling reduces the incidence of teenage childbearing and postpones fertility from the early teen years to the late teens and early twenties, and that the effects of schooling are larger following the greater availability of contraception (Silles, 2011; Braakmann, 2011; Wilson, 2012; and Geruso and Royer, 2014). Overall fertility seem to be negatively affected by the additional years of education (Fort et al., 2016).

3 Expansion of the UK Post-Compulsory Education

In the UK the proportion of 18-year olds in full-time education rapidly expanded in the late 1980s and early 1990s. Figure 2 shows the rapid increase in participation over the analysed period, represented by a significant step change. Overall, the proportion of 18-year olds in full-time education rose from around 17% in 1985 to over 35% in the late 1990s. The expansion raised education levels across the education distribution. Figure 2 also shows that the rise occurred for both further education, i.e., post-compulsory schooling (the minimum school-leaving age law in place at the time prohibited leaving school before the age of 16) and higher education. For both measures there was over a doubling in participation over the period. Walker and Zhu (2008) further show that the proportion of women with a college degree increased from 13% to 30% from the late 1980s to the early 1990s.

There were two main causes for the rapid rise in education over the analysed period. First, *supply of higher education* changed dramatically. Second, the *demand for higher education* was altered by a significant change in the high school exam system.

There were two key features that lead to the rise in the availability of university places.

[6]We present the results for the impact of the education expansion on fathers in Appendix B.

The Further and Higher Education Act in 1992 led to an expansion of university education as many polytechnic institutions became universities, which meant that they could then award degrees. Blanden and Machin (2004) and Walker and Zhu (2008) discuss increased university enrolment with respect to alterations in admissions and in financing. In particular, a relaxation in the limits of student places, but also the per university student government grant financing was abandoned. The incentives, therefore, increased for the universities to enrol more students. For the expansion of the post-compulsory sector through increased staying on rates, Blanden and Machin (2004) highlight the change in the school leaving examination system that took place in 1988, as result of the Education Reform Act in 1988. This led to the introduction of the General Certificate of Secondary Education (GCSE). The O-level (General Certificate of Education (GCE)), a higher tier exam, and the Certificate of Secondary Education (CSE), a lower tier exam, were brought together into one exam the General Certificate of Secondary Education (GCSE). A consequence of these changes was an improvement in results. Gray et al. (1993) show that there were big jumps in attainment. Using the Youth Cohort Study (YCS) they find that while only 30% of students obtained 4 or more high grade passes in 1986 (pre-GCSE), this increased to 40% in 1988 – the first year of the GCSE. There was an increase at almost every level. In addition, Gray et al. (1993) show that the most important determinant in predicting post-16 schooling were the received qualifications.

The introduction of the GCSE may have led to an improvement in attainment for two primary reasons. First, there was a move from norm-reference grading to criterion-referenced assessment. Norm-reference exams placed emphasis on relative performance compared to criterion-referenced assessment which sets performance based on set criteria and standards that the students had to achieve. This change meant that it was possible for everyone to get the top grades (Blanden et al., 2003) and thus a cap on the number of people who could receive a specific grade was removed. More students could achieve grades A to C, considered to be the passing grades. Second, the assessment of the GCSE introduced a sizeable element of coursework in contrast to the previous assessment criteria of GCEs that were based much more on exam performance. Therefore, someone born in 1972 and after, who had the same ability and other characteristics (such as, for example, similar time preference), would have had a greater opportunity to stay on in education due to the change in the examination system as they would have achieved the grades that would have allowed them to go on to further study, than someone born before 1972. Furthermore, changes to the structure of the economy, moving away from manufacturing and into services and the perceived increases in returns to education was also another significant driver of this increase in education demand, see Blanden and Machin (2004), Kogan and Hanney (2000), Devereux and Fan (2011).

What we therefore consider is a combination of both policy changes (i.e., changes in the high school exam system and changes in the supply of higher education) which did not have independent effects from each other but together had an impact on the distribution of education. The exam changes would have affected the lower end of the education distribution. High school students at the end of their compulsory schooling were in a position to take advantage of these changes by staying in school longer and then moving into higher education with the expansion in higher education occurring alongside changes to the qualifications system. Devereux and Fan (2011) also point out that the improvement in grades would have led to students believing they were good enough to go on to higher education.

A rapid increase in post-compulsory schooling in the UK in the late 1980s and early 1990s has already been used as a source of identification in other areas. Blanden and Machin (2004) study focuses on the education expansion as a key driver of falling intergenerational mobility. Devereux and Fan (2011) have also looked at wage effects associated with the education expansion, showing that on average it caused men and women to gain respectively a year or slightly more than a year of education and that this significantly raised wages. Machin et al. (2012) show that the education expansion reduced both male and female youth crime rates, while James (2015) looks at the effects of education expansion on a range of health outcomes finding significant reductions in body size. None of the existing literature have used the large expansion of the UK post-compulsory education system in order to investigate the relationship between education and fertility timing.

4 Empirical Strategy

As we have argued in the introduction, policy makers have focused on the drivers of both teen fertility and delays in childbearing. What has been examined less is the role of education. In Figure 1C, we have also shown that the correlation between education and maternity rates among under 18-year olds at the regional level is strong. Whether these links are causal and whether we can argue that causality runs from education to reduction of teen births/postponement of fertility is another question. To this end, the identification strategy utilising the education expansion relies on examining cohort-level changes in education and a number of fertility-timing outcomes. To examine the effect of the education expansion we begin by presenting the first stage showing the relationship between the education expansion cohorts and educational achievements:

$$Ed_{ic} = \alpha + \sum_{c=1972}^{1975} \beta_c \text{Cohort}_c + \delta After_c + f(Age_{ic}) + g(Cohort_c) + \varepsilon_{ic} \tag{1}$$

We also estimate the following reduced form regression:

$$F_{ic} = \phi + \sum_{c=1972}^{1975} \gamma_c \text{Cohort}_c + \lambda After_c + h(Age_{ic}) + k(Cohort_c) + \omega_{ic} \tag{2}$$

where the i subscript denotes individuals, and the c subscript denotes cohorts; ε and ω are equation error terms. Ed is a measure of completed education (years of education, having a degree, staying-on in education post-16, and having no qualifications), F is a measure of timing of fertility (age of first birth; probability of becoming a teen mother; probability of delaying having a child), $Cohort$ denotes the during-expansion cohorts between 1972-1975, $After$ is a dummy variable which picks up the effect of post-expansion cohorts (post-1975). The omitted category are the pre-expansion cohorts. The coefficients on the $Cohort$ dummies show the increase in education of each cohort relative to the average education level of the pre-expansion cohorts (pre-1972). The functions of Age and $Cohort$ include a quadratic effect in cohort and a cubic effect in age. In this way, the cohort dummies do not pick up any trend increases in education, just that part of the increase in education that deviates from the underlying trend. Similar to Devereux and Fan (2011), the specification is parsimonious with control variables and only adds a pre-determined dummy variable for whether the person is non-white to all specifications. Including controls for variables such as marital status, number of children, and region of residence are all likely to be affected by the education expansion and would tend to bias the effects of the expansion on fertility.

A nice feature of the reform in question is that it affected a large part of the education distribution and was not just driven by gains at the bottom or at the top, i.e., the reforms did not just result in increases in higher education.[7] Unfortunately, however, this means that it is not feasible to use a portion of the cohort not affected as a control group, as in Etilé and Jones (2011). Additionally, during this period of expansion, Scotland also

[7] This can been seen in Figure 2 as there is a comparable increase in 18-year olds being in further education as well as higher education. Also, there is an 8 percentage point increase in the probability of being awarded a degree relative to the pre-expansion cohorts (Table 2). The raw effect without controls in Table 2 is around 15 percentage points. Therefore, assuming that it takes 3 years to complete a degree, this implies an average increase in years of education of between a quarter of a year to just under a half a year. Given that the overall increase in years of education is around 0.9 years, this again is further evidence that the majority of gains were not taken by those entering higher education.

experienced expansion in the higher education sector, and hence does not make a feasible control group. The strategy employed therefore rests on identifying changes in the cohort trend that cannot be captured using a low-order cohort polynomial. Therefore, there may be underlying differences from cohort to cohort in fertility-timing behaviours, however, there is no reason to think that the other factors that influence fertility-timing do not change smoothly, and would therefore be captured by the cohort trends.

Using equations (1) and (2), we can estimate Two Stage Least Squares (2SLS), giving us the (social) return to education in terms of timing of fertility. The coefficient of interest is the effect of education on fertility timing measured by age at first pregnancy, probability of becoming a teen mother, and probability of delaying motherhood. The interpretation of this estimate, under the assumption of monotonicity, is a local average treatment effect (LATE), i.e., the estimated effect is for those who obtained more education as a result of the expansion. In contrast to changes to compulsory schooling which exclusively affect the bottom end of the distribution, this reform is for a broader part of the population, however, the interpretation remains the same in that the effect is for the compliers of the reform.

While this estimation strategy is not strictly a regression discontinuity design (RDD), it has a similar flavour. Gelman and Imbens (2014) point out that high order polynomials should not be used in RDDs. In any case, we test the robustness of the 2SLS estimates to the specification of both the "running variable" (birth cohort) and how age is put into the model. In Figures A1–A4, bars 8, 9, 10 include age quartic, year of birth cubic, and then both age quartic and year of birth cubic. The results appear robust to these alternative specifications.

The validity of the instruments in this case rests on the assumption that the education expansion cohorts significantly explain the variation in education without being correlated with unobservable characteristics that are correlated with education and fertility timing such as family background, risk aversion, or time preference. Table 2 explicitly tests the first requirement. The second requirement entails that the expansion was not aimed at improving fertility timing or implemented as a reaction to more teenage pregnancies. There is no evidence that this is the case. For example, the Further and Higher Education Act does not explicitly mention fertility-timing outcomes as a reason for the changes either directly or indirectly.[8]

The key identifying assumption in using the set of cohort dummies as instruments for education is that the conditional expectation of the fertility timing outcomes with respect

[8]Further and Higher Education Act 1992:
`www.legislation.gov.uk/ukpga/1992/13/pdfs/ukpga_19920013_en.pdf`

to the birth cohort is that in the absence of the education expansion, the changes could be explained by a low-order cohort polynomial. Therefore, one way in which we can indirectly test this assumption is by examining the effect of the instruments (cohort dummies) on pre-determined or background characteristics. The idea here is to examine whether there is a systematic difference in the background characteristics of those individuals who were affected by the education expansion. If there is then this suggests that what we find might be driven by difference in background characteristics rather than differences in education.

As a check of our identification strategy we use data from the first wave of the Understanding Society data set.[9] The data contains information on the qualifications of the mother and the father. We classify four categories of education separately for the mother and the father: (i) having a degree or a higher degree; (ii) in addition to the first category, we also include having post-school qualifications or certificates; (iii) this group indicates whether the mother or the father left school with some qualifications or above (i.e., we also include category i and ii); and (iv) whether either the father or the mother left without any qualifications or did not go to school at all. The estimation mimics what we do in the reduced form estimation in equations (1) and (2) but replaces the individual qualifications with that of the father or the mother. Table A5 in Appendix A presents the results of this exercise. We do not find any differences in parental education for the EE cohorts. None of the forty coefficients are significant and neither are any of the joint tests of the EE cohort dummies and the post-EE indicator.

Our estimation approach is related to the two strands of previous literature. First, the literature which exploits the before, during, and after design (Devereux and Fan, 2011). Examples include other papers which use the EE as a source of identification when examining the effect of education on intergenerational mobility (Blanden and Machin, 2004), wages (Devereux and Fan, 2011), crime (Machin et al., 2012) and health (James, 2015) outcomes. Further, Ichino and Winter-Ebmer (2004) study the effect of the Second World War on educational attainment and subsequent earnings in Germany and Austria and use pre-war (born before 1930), war-impacted (born 1930-1939), and post-war (born 1940 onwards) groups in analysis. Second, the literature which exploits 'indirect effects' of the education polices, such as the papers by Maurin and McNally (2008) and Nordin (2017). Maurin and McNally (2008) show that the May 1968 student riots in France resulted in abandonment of normal examination procedures and an increase in the pass rate for various qualifications. The lowering of exam thresholds enabled a proportion of

[9] Given that we are interested in the family background (specifically the education level of the parents) we only use one wave (the first) of Understanding Society as these are assumed to be completed and will unlikely change over time.

students to pursue more years of higher education than would otherwise have been possible, which subsequently increased wages and occupational levels of the affected cohorts, and has transmitted across generations into better education performance of their children. Nordin (2017) shows that after a change to a goal- and criterion-referenced grading system in Sweden in 1994, there was a substantial grade inflation, which increased the tertiary education eligibility, and resulted in the crime reduction of the affected cohorts.

5 Data and Descriptive Figures

We use the Labour Force Survey (LFS) in the period from 1975 to 2013. The LFS is a Great Britain household survey covering around 60,000 households, responding each quarter. It is a rotating panel where households are surveyed for five successive quarters. One fifth of the households are undertaking their first interview in each quarter. One fifth of the sample are taking their second interview and so on. The LFS contains information regarding education, including age at which full-time education was completed, as well as the highest education qualification achieved. In order to assign individuals to school cohorts, we used month of birth – individuals born in the first three quarters of a year were assigned to the first school cohort in which they were eligible to start school (i.e., the first academic term following their fifth birthday); individuals born in the last quarter of a year were assigned to the next school cohort. For each individual, we only use information when she first appeared in the data.

Similar to Black et al. (2008) analysis for the US and Wilson (2012) analysis for the UK, age at motherhood is determined from the ages of the mother and the eldest child within a household at the time of the survey. This procedure assumes that a mother-child relationship can be observed only if both individuals are present in the same household at the time of the survey. Allowing for parental separation, this approach assumes that the child resides with the mother and that the mother-child relationship that we observed in the data is then biological. Further, this approach also assumes away child mortality. Although parental separation and child mortality may introduce measurement error, it is likely that any effect would be small, because in the case of parental separation, a child usually stays with the mother, and the childhood mortality rates have been declining over time.[10]

[10] As mentioned earlier, around nine in ten children living in lone parent families lived with their mothers in the UK in 2008 (ONS (2009)). Further, the proportion of lone parent households had increased in the period since the education expansion with the proportion of children living with lone mothers increasing from 19%in 1997 to 22% in 2008. Furthermore, we assume that a potential measurement error in the dependent variable has zero mean; if it does not, then we simply get a biased estimator of the intercept,

Figures 3 and 4 show how education changed by cohort. In order to control for any age effects and a potential secular trend in education, similar to Ichino and Winter-Ebmer (2004) we calculate residuals from fitting a polynomial in age up to a cubic.[11] Figure 3 presents age left full-time education and having a degree education measures after controlling for a cubic age profile. The area in between the two vertical lines represents the cohorts from 1972 to 1975 which were affected by the expansion in education. For both measures of education, the education expansion period is evidently characterised by a strong positive deviation from the secular trend. After the expansion period there is still a positive deviation from the trend, which then has levelled off for the affected cohorts. Figure 4 shows a similar pattern for the post-16 measure of education. There is a sharp, positive, and rapid deviation from the secular trend for the education expansion cohorts. For those having no qualifications, there is a downward trend occurring before the expansion period and carrying on through the expansion period.

Turning to the the timing of fertility measures, Figure 5 presents the residuals of the probability of birth before age of 21, while Figure 6 presents the residuals of the probability of birth at or after age of 21, both after controlling for a cubic age profile. For women younger than 21, throughout the EE period we do not see a deviation away from the secular trend, but there is a slight increase after the expansion. We will test to see whether this deviation is significant in Section 6. In contrast, we do see significant positive deviation in the trend when we examine births after being 21 years old. There is no deviation for births to mothers age 21 and above. However, there is a sharper deviation for all ages 27 and above. For these groups the deviation is much clearer. For the post-expansion cohorts there is still a positive deviation, however, this is declining.

Tables A1–A3 present the descriptive statistics for the cohorts used in the EE reform.

6 Results

6.1 First Stage: The Effect of the EE on Education Outcomes

Table 2 shows the first stage estimates for the EE reform, for four different measures of education. In the first column we show age in which one left full-time schooling, the dependent variable in the second column is an indicator for leaving full-time education after compulsory leaving age (i.e., after the age of 16). In the next column we show the

which is not be of a big concern (Wooldridge, 2009).

[11] We have tried controlling for linear, quadratic, and quartic polynomials in age, as well as no controls, and we find a similar pattern of results as those presented in the paper.

effect of the expansion on achieving a degree, and finally we present whether someone has no qualifications. We include a set of dummies for each of the 1972 to 1975 inclusive cohorts in order to represent the expansion in education. As shown in the figures in the Appendix, these were the years where the expansion was at its most rapid. We also include a dummy representing the post-expansion cohort, therefore, the estimates we find are relative to the cohorts who experienced the pre-GCSE exam system.

For all of the measures of education there is an increase for each of the cohorts, with each subsequent cohort being greater than the previous. For age left full-time education, the significant step-change monotonically increases. The coefficient on the 1972 cohort is 0.178, this increases to 0.788 for the 1975 cohort, with the post-expansion dummy coefficient being 0.911. The pattern is similar for staying on past 16, with the cohort coefficients increasing from 0.009 in 1972 to 0.164 in 1975, with a plateau at 0.230 in the period post-education expansion. There are also improvements for those achieving a degree. The pattern is similar to the other two measures of education. For those holding no qualifications we see a fall. This fall is predominantly in the latter part of the expansion period. The F-statistic for the joint test of the 1972 to 1975 and a dummy for post-expansion cohorts is not as large as compared to age left school or leaving education after 16. The F-tests for age left education, post-16 and degree are all above 10. For having no qualifications, the F-test is significant but not above 10.

Table 3 summarises first stage results from other literature which uses the EE as a source of identification when examining the effect of education on wages (Devereux and Fan, 2011), crime (Machin et al., 2012) and health (James, 2015) outcomes. First, except for Devereux and Fan (2011), all papers include 1972-1975 (during-expansion cohorts) plus post-expansion cohort dummies as instruments for education, and interpret the results with respect to the pre-expansion cohorts (i.e., pre-1972 cohorts). Therefore, we follow the same specification for the baseline first-stage regression. In the robustness checks, similar to Devereux and Fan (2011), we also include 1970 and 1971 during-expansion cohort dummies as an instrument for education, which does not significantly change the baseline estimation results. Second, first-stage results across the three papers which show first-stage results separately for women (Devereux and Fan, 2011; Machin et al., 2012; and this paper), suggest that post-expansion cohorts of women have between 1-1.5 years more of education, are between 8-13 percentage points more likely to obtain a degree, are between 17-23 percentage points more likely to stay on in education post-16, 14 percentage points more likely to enrol into university, and 2 percentage points less likely to end up having no qualifications in comparison to the pre-expansion cohorts.

6.2 Reduced Form: The Effect of the EE on Fertility Timing

Table 4 shows the reduced form estimates for the EE reform. The first column shows an increase in the age of first birth for the EE cohorts. Those born in 1973 had a child just a quarter of a year later relative to the pre-expansion cohorts. This increases to just over half a year for the post-education expansion cohorts. We do not find much evidence that this increase in the age of first birth was as a result of fewer women having children as a teen, since the point estimates on the cohort dummies do not reveal a pattern that suggests a decline in the probability of a teen birth.

In Table 5 we consider whether the EE reform led to a delay in fertility. We begin by examining births from the age 21 up to after the age of 30. There is little evidence that there was a delay in fertility up to age 23. However, when we consider the probability of births for those aged 24 or above, we then find that the education expansion cohorts are more likely to have births after this age. This occurs mainly for the 1973 to 1975 cohorts, and we do not see a significant effect for the 1972 cohort. This reflects that the 1972 cohort was the least affected by the education expansion. Caused by the education expansion, we estimate an increase in the probability of a birth when aged 24 or above by 2.4 percentage points for the cohort born in 1975. Similar estimates are found for the probability of a birth when aged 25 or above (2.8 percentage points), 26 or above (3.2 percentage points), etc. The F-statistics in columns (4) to (10) all suggest the joint significance of the 1972 to 1975, and the post-education expansion cohorts dummies.

Therefore to summarise, we find a significant effect on delaying fertility as a result of the EE reform. By the end of the EE period, those in the post-expansion cohorts were around half a year older when they had their first child relative to the pre-expansion cohorts. This increase in the age of first birth is not reflected in a reduction in the probability of a birth by a teenager. Instead, we see an increase in the delay of fertility. We see this because there is an increase in the probability of a birth after the age of 24 for those affected by the EE reform. To a certain extent, this reflects the nature of the EE reform. As seen in Table 2, the education expansion led to more time in school but also an increase in post-compulsory schooling and obtaining a degree. Therefore, this is in line with the timing of fertility effects that we find such that they occur after a degree would have been completed.

6.3 Structural Form: The Effect of Education on Fertility Timing

Next we turn to examine the effect of education on fertility timing. Table 6 presents OLS and IV estimates for five different fertility timing measures. First is the age of first birth. Then we consider the probability of a teen birth (defined as having a first birth before the age of 21). The final three measures we use are dummy variables which indicate whether a woman had a child aged 24 or after, 27 or after, and 30 or after.

Panel A presents the estimates for age left full-time education. The OLS estimates suggest that leaving school one year later leads to an increase in the age of first birth by three quarters of a year. The IV estimates are somewhat lower with an effect size of half a year. One interpretation is that women have unobserved characteristics that also make them more likely to delay fertility. Controlling for these characteristics reduces the estimated effect but does not eliminate it. However, the Hausman test implies that these estimates are not significantly different from each other.

Using the same source of identification, the OLS estimates suggest that an additional year of schooling is associated with a reduction in the probability of a teen birth by 3 percentage points. However, as was reflected in the reduced form analysis in Table 2, we do not find a significant result when we estimate the effect of education using 2SLS estimation approach. Similarly when teen birth is the dependent variable, we see a reduction in the point estimate by over 50%, however it is no longer precisely estimated.

In columns (5) to (10), we examine the effect of education (for age left full-time education) on delaying fertility. In most cases the 2SLS estimates are smaller than the OLS estimates. The 2SLS estimate when the dependent variable is an indicator for whether the birth occurred to a woman aged 24 or older is significantly different from the OLS. These estimates suggest an additional year of schooling leads to an increase in probability of a birth aged 24 or after by 3.9 percentage points. Compared to the pre-education expansion mean, this represents a 5.3% increase in probability of birth 24 or above. The IV estimate for being aged 27 or above for a first birth is the equivalent of an 9.4% increase and being aged 30 or above for a first birth is the equivalent of an 13.3% increase.[12]

In Panels B and C we examine two alternative measures of education. Panel B presents the results of having a degree as the margin of education. Panel C uses an indicator as to

[12]We also examine births for older ages, from aged 31, 32 to 33 and above. These are presented in Table A6 in Appendix A. The point estimates are between 7.4 and 6.2 percentage points broadly in line with the estimates for those aged 30 and above.

whether someone stayed on in school after the age of 16. The OLS estimates suggest that those with a degree are almost 4 years older at the time when they have their first child than those who do not. By instrumenting education using the expansion cohorts and a post-expansion dummy, we find that this estimate increases to 5 years. The difference between these two estimates is not statistically significant. The direction of the difference is in contrast to when we use age leaving full-time education as the measure of schooling. First, this could be due to measurement error. As measurement error may bias the OLS estimate towards zero it could be the case that there is more measurement error in recalling having a degree (or there may be greater misclassification as to what constitutes having a degree) compared to the age at which one left school. An alternative explanation is that the marginal effect of schooling for women affected by the education expansion may be larger than the average effect for the population, and this is particularly the case with respect to having a degree. The estimates we find are the effect of obtaining a degree for women who would not have gotten a degree had it not been for the expansion. This is also confirmed by Del Bono and Galindo-Rueda (2007), who find that formal educational degree is a more important driver of employment and participation decisions for women than it is for men.

6.4 Robustness Checks and Additional Results

The Figures A1 – A4 in Appendix A present the robustness estimates for a different set of specifications (age at first birth – Figure A1, birth after age of 24 – Figure A2, birth after age of 27 – Figure A3, and birth after age of 30 – Figure A4), for the measure of education age left full-time education, with the corresponding 95% confidence interval. Specification 1 presents the baseline estimates for comparison. The first set of changes are designed to see how sensitive the estimates are to changing the structure of the instruments. Specification 2 excludes the post-education expansion dummy as an additional instrument and this forces the identification to come from the cohorts that were affected just during the expansion years. Specifications 3 and 4 change the period of education expansion by including earlier cohorts as instruments, similar to Devereux and Fan (2011). Specification 5 reduces the number of instruments by combining the expansion cohorts into one (a single dummy for the 1972-1975 cohorts), and specification 6 into two dummies (a dummy covering the 1972-1973 cohorts, and one for 1974-1975 cohorts). Specifications 7 to 10 revert back to the original instrument set and examine changing the specification of the age variables. In particular, specification 7 replaces the polynomials of age with a set of age dummies. We further test the robustness of the 2SLS estimates to the specification of both the "running variable" (birth cohort) and how age is entered into the model. Therefore, specifications 8, 9, and 10 examine the effect of changing the regression specification with respect to age and year of birth. Specification 8 presents the 2SLS estimates additionally including a cubic for year of birth. Specification 9 additionally includes a quartic in age to the baseline specification, and specification 10 presents the estimates when year of birth cubic and age quartic are both included.

Age effects on fertility may have changed over cohorts, and controlling for age may not be sufficient to control for such effects. In order to address this, specification 11 includes an age cohort interaction as an additional control. We include the square and cubic of this interaction in specification 12 and 13 in order to control for this in the most flexible way, and in specification 14 we additionally include a quartic.

However, in order to examine whether changing the age of the sample matters in specification 15 the sample is restricted to those aged 24 to 34 (main estimation results are done for those aged 20 to 34). The sample is further restricted to include individuals born between; 1965–1979 (specification 16), 1966–1979 (specification 17), and 1967–1979 in specification 18. In specification 19 estimates are presented where limited information maximum likelihood (LIML) is used rather than 2SLS estimation approach. This is because in over-identified models with weak instruments, 2SLS will be biased and this is not the case with LIML. Therefore, a comparison between the 2SLS estimates and those estimated using LIML will also serve as a test of weak instruments. In the final specification 20 we include an indicator for being married as an additional control. As described in the empirical strategy, one might expect that education has a direct impact on marriage and marriage would therefore be considered endogenous. As a result we do not include these types of variables as controls as this may introduce a degree of selection bias. Here we examine what the impact of inclusion of one such control, i.e. an indicator for being married, would do to our estimates.

Taking the three Figures A1 – A4 together we underline two results. First, the baseline estimates are not significantly different than the range of estimates presented that adopt a number of different instrument sets and specifications. Second, the inclusion of age-cohort interactions cause the estimates to fall a little, although this difference is not statistically significantly different from the baseline estimate. For the dependent variable of age of first birth, the inclusion of age cohort up to a quadratic does not significantly change the point estimate, however, it does make the estimate less precise. For the dependent variable age of birth after 27, we find that a flexible specification which includes a quadratic of this age cohort interaction (specification 14) results in a significant result, but less flexible specifications lead to imprecise estimates. When we use age of first birth being above 30, all our estimates remain statistically significant.

7 Discussion and Conclusion

In this paper we have documented the impact of an expansion in post-compulsory education on the timing of fertility. This education expansion was the result of a set of reforms that changed the high school exam system and qualifications, and opened up higher education. We find that an increase in education, either through an increase in age finishing full-time education or obtaining a degree, led to delaying fertility – although we do not find precisely estimated effects on the probability of being a mother as a teenager. Our estimates imply that an increase in education by one year led to a 5% increase in the probability of birth aged 24 and above, a 9% increase in probability of birth when aged 27 or above, and 13% increase in probability of birth when aged 30 or above.

The results we find are from a set of reforms that were more recent than a number of other reforms that have been examined in relation to education and teen motherhood, and the timing of fertility more generally in the UK. For example, Silles (2011), Braakmann (2011), Wilson (2012), Fort et al. (2016) and Geruso and Royer (2014) use reforms that occurred either in 1947 and/or 1972 – the raising of the school leaving age to 15 and 16, respectively. Despite this, it is still the case that the set of reforms we exploit took place around 30 years ago. Therefore, it is an issue as to how generalisable these results are over time. In order to examine external validity, we consider a number of factors surrounding social and sexual norms and the extent to which they have changed since the set of EE reforms occurred.

First, in 1967 the Abortion Act was passed and came into force in 1968 that allowed abortions to be performed lawfully in all of Great Britain (but not Northern Ireland) under specific conditions.[13] Therefore, those affected by the set of reforms we examine were all facing the same legal environment of abortion compared to 2018, which is not the case for those papers that examine the earlier compulsory schooling changes that occurred in 1947. There has been a gradual increase in the number of abortions over the period we

[13]The criteria set out in the 1967 Abortion Act are as follows: Two registered medical practitioners are of the opinion that i) the pregnancy has not exceeded its twenty-fourth week and that the continuance of the pregnancy would involve risk, greater than if the pregnancy were terminated, of injury to the physical or mental health of the pregnant woman or any existing children of her family; ii) that the termination of the pregnancy is necessary to prevent grave permanent injury to the physical or mental health of the pregnant woman iii) that the continuance of the pregnancy would involve risk to the life of the pregnant woman, greater than if the pregnancy were terminated, iv) that there is a substantial risk that if the child were born it would suffer from such physical or mental abnormalities as to be seriously handicapped.

examine. At the beginning of the expansion period, in 1988, the abortion rate was 14.2 per 1000 women residents aged 15-44, and at the end of the expansion in 1994/1995 the rate, at 14, had hardly changed (Department of Health, 2017).[14] Since then the rate has gradually increased and was 16 per 1000 women residents aged 15-44 in 2016, therefore not dramatically different from the rate in the mid-nineties.

Second, the biggest developments in contraception use occurred in the late 1960s and early 1970s. In particular, the oral contraceptive pill became available in the mid-1960s, and in 1974 family planning services were brought into the National Health Service (NHS), (Bottling and Dunnell, 2000); for example, in 1974 family planning clinics were allowed to prescribe the pill to the single women. Since then, contraception has also been available free of charge through the NHS. The use of the pill was highest in the mid 1970s and early 1980s, peaking at around 28% of 16-49-year-olds, and since then it has remained constant at around 24%. Condom use has on the other hand been increasing. For example, 16% of women aged 16-49 used condoms in 1976 rising to 18% in 1995 and further to 24% in 2008 (Bottling and Dunnell, 2000, Lader and Hopkins, 2008). Therefore, while the proportion using the main method of contraception has remained broadly unchanged there have been some changes in condom use.[15]

Third, there is one major set of surveys that we can refer to in order to examine the changes in sexual activity.[16] The National Survey of Sexual Attitudes and Lifestyles (NATSAL) began in 1991 and it documents sexual behaviour and attitudes. The most relevant behaviour is the age of first intercourse. This has fallen over time. For the 1935-1939 cohort the median age of first intercourse was at age 20, this was 17 for the 1955-1959 cohort where it remained until the cohorts born after 1985, where the age of first intercourse had declined to 16 (NATSAL, 2014). Therefore, although the reforms we examine began around thirty years ago, they occurred when abortion, contraception use, and age of first intercourse were relatively similar to what they are today, than

[14] Rates for all women residents age-standardised using the 2013 European Standard Population for ages 15-44.

[15] Emergency contraception (EC), mostly known as the "morning-after pill," has only became available over the counter (OTC) from pharmacies without prescription to women aged over 16 years in the UK in 2001 (Schenk, 2003). "EC use can be considered a marker of risky sexual behaviour, as it indicates exposure to unprotected sex or a failure in contraceptive method" (Black et al., 2016). Although women in the UK have been among the highest users of EC, availability of EC from the pharmacy in the UK did not result in an immediate increase due to a relatively high cost of £25. The EC contraception has become available free of charge to everyone through pharmacists throughout Scotland in the autumn 2008. Although changes in the use of EC in the UK can be mapped alongside improvements in knowledge and its availability, we consider this a more recent phenomenon and the impact of EC use on unintended pregnancy rates at a population level has not yet been established (Cameron et al., 2012).

[16] The legal age of consent for heterosexual sex in England and Wales is 16 and has not changed since 1885, since the Criminal Law Amendment Act of 1885.

those papers that have used changes to the compulsory schooling laws in the UK as an identification strategy.

We consider two effects that could explain the relationship between education and the timing of fertility, a human capital effect and an incapacitation effect. The body of evidence points to the effect of education on fertility being driven by a combination of these two effects (Black et al., 2008). None of the impacts we find bite at ages that would suggest an incapacitation effect alone given that the expansion have a significant impact on ages that are beyond the ages that would be binding. While we can rule out the incapacitation effect, we cannot distinguish between a direct human capital or signalling effect of education on fertility timing. The effects of the expansion could well be a combination of improvements in human capital and signalling of qualification effects.

From the perspective of the policy makers it is relevant discussing why the effects only take place after the mid-twenties. First, as already mentioned the period of the expansion is where contraception use, abortion access and the onset of sexual behaviour are relatively similar to today. Second, the education expansion led to an improvement in labour market opportunities which subsequently led the affected cohorts of women to postpone having a family and child-rearing activities. The improvement in labour force participation, employment and earnings of the affected cohorts has been well documented in the previous literature (Anderberg and Zhu, 2014; Dickson and Smith, 2011; Del Bono and Galindo-Rueda, 2007).

While we do not show evidence on the effect of education on the overall fertility, a recent paper by Fort et al. (2016), which exploits the variation in education coming from the compulsory schooling reforms in England and Continental Europe, might be informative in this respect. While for Continental Europe the additional education generated by schooling expansions did not lead to a decrease in the number of biological children nor to an increase in childlessness, the authors find a negative relationship between education and overall fertility in England. As an explanation, the authors point to higher teenage fertility rates in England, and the differences in labour and marriage markets between England and Continental Europe. Concluding, although more education has been shown to reduce teen fertility (Silles, 2011; Wilson, 2012; Geruso and Royer, 2014) and result in delays in fertility to after the mid-twenties (this paper), more educated women in England also seem to have lower marriage rates, less children overall, and are more likely to remain childless (Fort et al., 2016). Any policy recommendations should take both of these effects into account.

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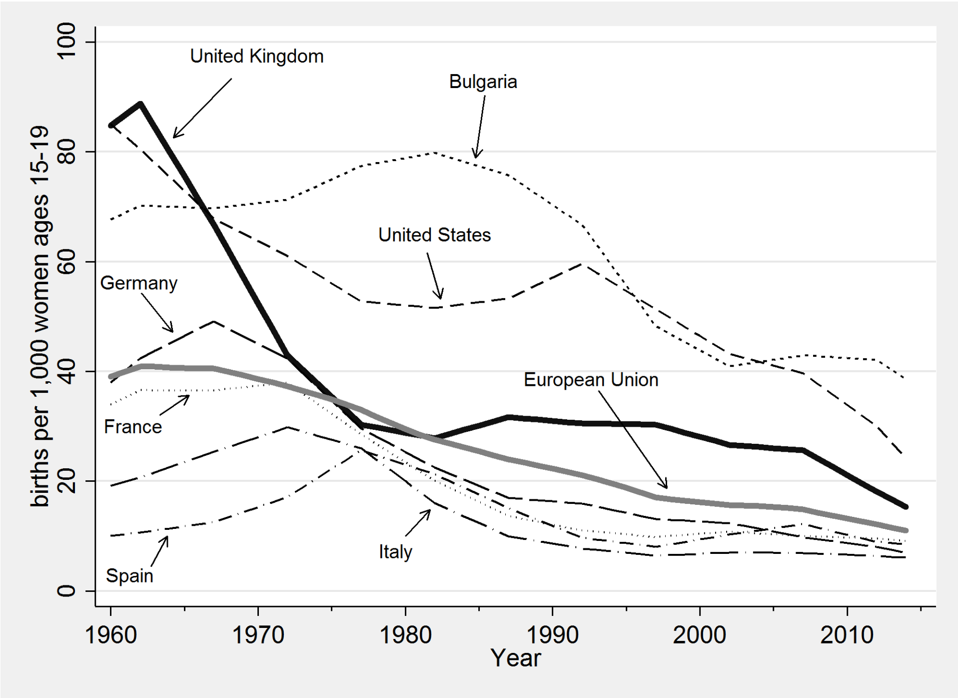
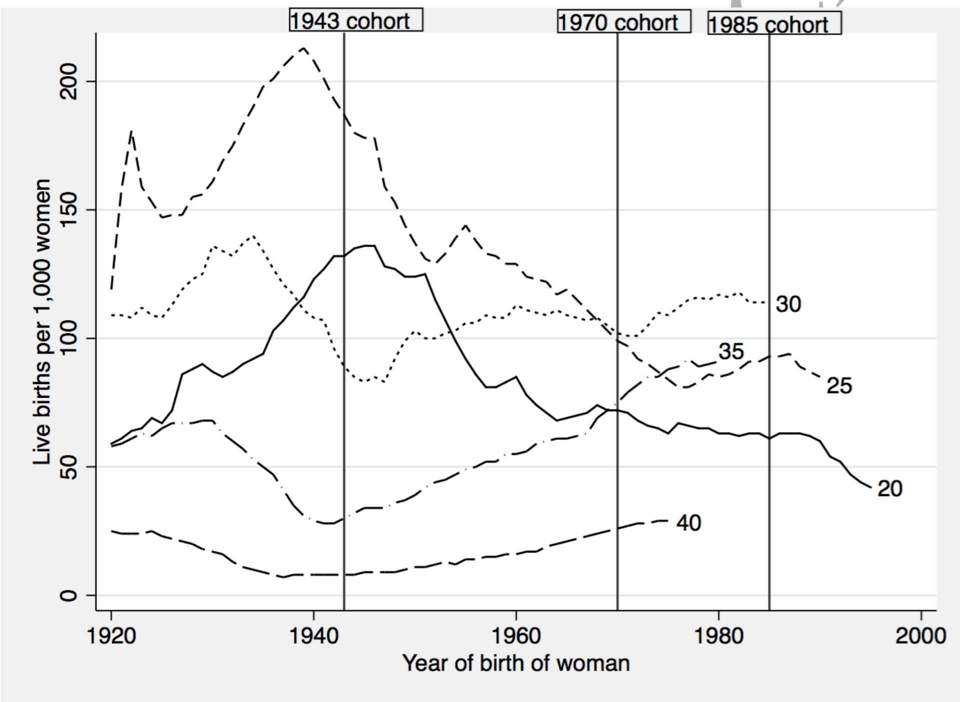
Figures and Tables

Figure 1A: The Trend in Teen Fertility Rates Across Selected Countries, 1960-2014



Source: World Bank, Adolescent fertility rate (births per 1,000 women aged 15-19)

Figure 1B: Age-Specific Fertility Rates at Selected Ages, by Year of Birth, England and Wales, 1920 to 1995



Source: Office for National Statistics (ONS). Live Birth Statistics.

Figure 1C: The Correlation Between the Under 18 Maternity Rate per 1,000 Women Aged 15-17, and Education at the Local Authority Level

Source: Neighbourhood Statistics (`www.neighbourhood-statistics.gov.uk`)
Notes: Each dot represents a local authority. Qualifications data are sourced from the 2011 Census. The under 18 maternity rate is calculated using maternities to women aged under 18 per 1,000 women aged 15-17 resident in the area. The numerator includes conceptions to all women aged under 18, the denominator only uses women aged 15-17 as most maternities occur within this age-group. In each case the line represents a linear fit.

Figure 2: Changes in Post-Compulsory Education Participation From 1985 to 2000

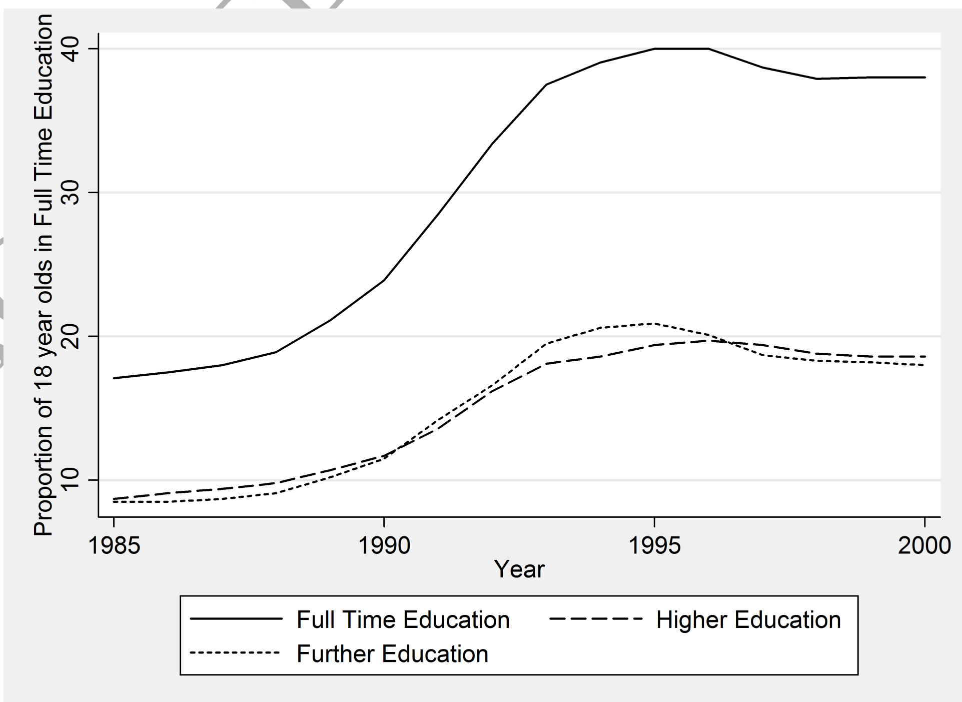


Figure 3: Education (Age Left Full-Time Schooling and Degree) Controlling for a Cubic in Age

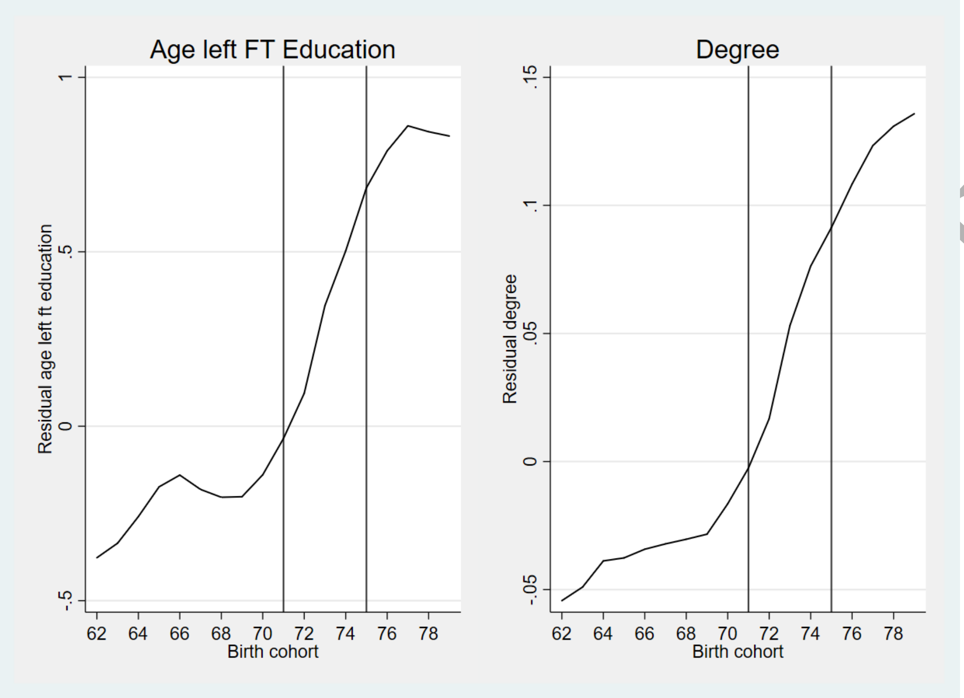


Figure 4: Education (Post-16 schooling and No qualifications) Controlling for a Cubic in Age

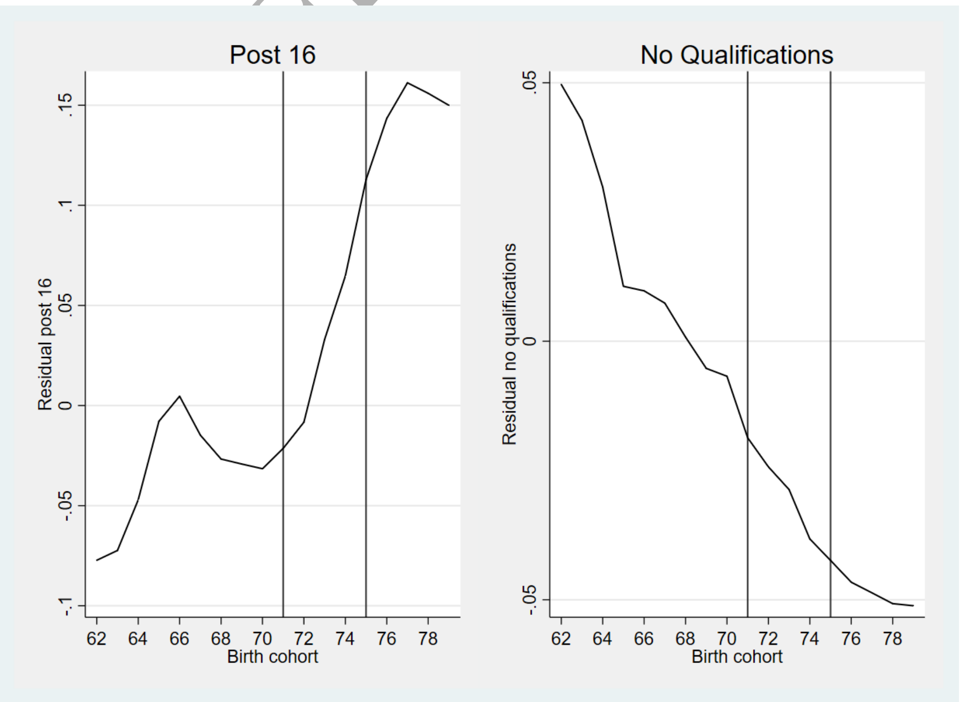


Figure 5: Probability of Teen Birth Controlling for a Cubic Age Profile

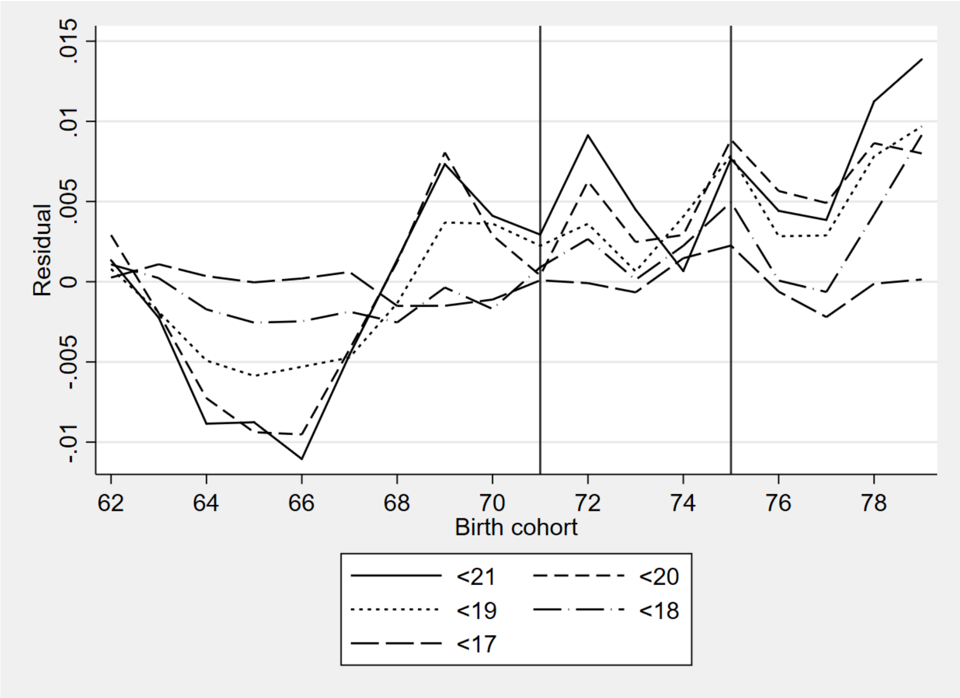


Figure 6: Probability of Birth After Age 20 Controlling for a Cubic Age Profile

Table 1: Overview of the Education and Fertility Literature

Study	Country	Teenage fertility (< 20)	Delayed fertility (≥ 20)	Overall fertility	Methodology
Black et al. (2008)	US and Norway	Negative			Probit regression
Monstad et al. (2008)	Norway	Negative	Positive	No effect	2SLS
Grönqvist and Hall (2013)	Sweden	Negative	Positive	No effect	2SLS
Humlum et al. (2012)	Denmark		Positive		2SLS
León (2004)	US			Negative	2SLS
McCrary and Royer (2011)	California & Texas	No effect	No effect		RDD
Silles (2011)	UK	Negative			2SLS
Braakmann (2011)	Great Britain			Positive	2SLS
Wilson (2012)	England and Wales	Negative	Positive		2SLS
Geruso and Royer (2014)	Great Britain	Negative		No effect	2SLS
Fort et al. (2016)	England			Negative	2SLS
Fort et al. (2016)	Continental Europe			Positive	2SLS
Cygan-Rehm and Maeder (2013)	Germany	Negative	Negative	Negative	2SLS
Kidar et al. (2009)	Turkey	Negative			Duration analysis
Fort (2006)	Italy	Negative	No effect		RDD
Osili and Long (2008)	Nigeria	Negative	Negative	Negative	DID, 2SLS
Lavy and Zablotsky (2011)	Arabs in Israel			Negative	DID, 2SLS

Table 2: Education Expansion (EE) and Education Attainment

	(1) Age left FT Ed	(2) Post-16	(3) Degree	(4) No Quals
Cohort 1972	0.178***	0.009	0.019***	−0.004
	(0.043)	(0.009)	(0.007)	(0.006)
Cohort 1973	0.485***	0.068***	0.057***	−0.005
	(0.051)	(0.010)	(0.008)	(0.006)
Cohort 1974	0.581***	0.096***	0.069***	−0.013*
	(0.057)	(0.012)	(0.009)	(0.007)
Cohort 1975	0.788***	0.164***	0.072***	−0.013*
	(0.066)	(0.013)	(0.011)	(0.008)
Post-EE	0.911***	0.230***	0.083***	−0.021*
	(0.087)	(0.018)	(0.015)	(0.011)
Constant	−17.991***	−7.517***	−3.174***	2.712***
	(3.470)	(0.776)	(0.590)	(0.534)
Observations	103,050	103,050	99,476	99,589
R-squared	0.069	0.030	0.070	0.009
F-test	35.4	44.6	15.6	0.91
p-value	0.00	0.00	0.00	0.47

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, quadratic polynomial in year of birth, and year of survey dummies. The sample contains women aged between 20 and 34, and includes cohorts born between 1962 and 1980. The F-stat is a test for the joint significance of the 1972 to 1975, and post-expansion cohort dummies. The p-value corresponds to this F-test. The dependent variable in column (1) is a variable defining the age the individual left full-time education, column (2) is a dummy equal to 1 if the individual left school after age 16, column (3) is a dummy if the highest qualification achieved is a degree (or equivalent) or above, and column (4) is a dummy equal to 1 representing whether the individual has no qualifications.

Table 3: The First Stage Results Using Education Expansion (EE) Identification Approach

Study	Outcome	Data source, first stage regression specification and results
Devereux and Fan (2011)	Wage	UK Quarterly Labour Force Survey (QLFS) from 1997 to 2009. Cohort dummies included: 1970-1975. Year of birth: 1958-1982; Age: 25-50. Controls: flexible age and cohort polynomials; proportion white and cohort size. Post-expansion cohorts of women have 1.5 years more education and are 13 pp more likely to have a degree than the pre-expansion cohorts.
Machin et al. (2012)	Crime	UK Quarterly Labour Force Survey (QLFS) from 1978 to 2002. Cohort dummies included: 1972-1975. Year of birth: 1962-1982; Age: 16-21. Controls: flexible age and cohort polynomials; proportion non-white, proportion living in London and proportion living in Wales. Post-expansion cohorts of women are 17 pp more likely to stay on in education post-16 and 14 pp more likely to enrol into university than the pre-expansion cohorts.
James (2015)	Health	The Health Survey of England from 1991 to 2012. Cohort dummies included: 1972-1975. Year of birth: 1962-1980; Age: 23-34. Post-expansion cohorts of both men and women have half a year more education, are 18 pp more likely to stay on in education post-16, 9 pp more likely to obtain A-level +, and 8 pp more likely to have a degree than the pre-expansion cohorts.

Table 4: Effect of the Education Expansion on Age at First Birth: Teen Births

	(1) Age of First Birth	(2) < 16	(3) < 17	(4) < 18	(5) < 19	(6) < 20	(7) < 21
Cohort 1972	0.106	0.001	0.000	0.001	−0.001	0.004	0.004
	(0.101)	(0.001)	(0.002)	(0.003)	(0.005)	(0.006)	(0.007)
Cohort 1973	0.248**	−0.001	−0.001	−0.006	−0.009*	−0.006	−0.008
	(0.115)	(0.001)	(0.002)	(0.003)	(0.005)	(0.006)	(0.008)
Cohort 1974	0.202	0.001	0.001	−0.006	−0.009	−0.010	−0.021**
	(0.132)	(0.001)	(0.002)	(0.004)	(0.006)	(0.007)	(0.008)
Cohort 1975	0.208	0.000	0.002	−0.004	−0.008	−0.006	−0.016
	(0.153)	(0.002)	(0.003)	(0.005)	(0.007)	(0.009)	(0.010)
Post-EE	0.507**	−0.001	−0.004	−0.019***	−0.025***	−0.022*	−0.035***
	(0.201)	(0.002)	(0.004)	(0.006)	(0.009)	(0.011)	(0.013)
Constant	4.877	0.047	0.303**	1.035***	1.343***	1.143**	1.418**
	(8.409)	(0.079)	(0.152)	(0.266)	(0.383)	(0.484)	(0.566)
Observations	45,572	103,050	103,050	103,050	103,050	103,050	103,050
R-squared	0.250	0.000	0.000	0.001	0.001	0.001	0.001
F-test	1.60	0.82	1.75	3.54	2.40	1.72	2.62
p-value	0.16	0.53	0.12	0.00	0.04	0.13	0.02

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth, and year of survey dummies. The sample contains women aged between 20 and 34, and includes cohorts born between 1962 and 1979. The F-test tests whether the coefficients on the excluded instruments are jointly equal to zero. The p-value corresponds to this F-test.

Table 5: Effect of the Education Expansion on Age at First Birth: Delaying Fertility

	(1) ≥ 21	(2) ≥ 22	(3) ≥ 23	(4) ≥ 24	(5) ≥ 25	(6) ≥ 26	(7) ≥ 27	(8) ≥ 28	(9) ≥ 29	(10) ≥ 30
Cohort 1972	−0.004	−0.000	0.003	0.001	0.009	0.006	0.008	0.006	0.009	0.013
	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Cohort 1973	0.008	0.009	0.017*	0.025***	0.026***	0.028***	0.038***	0.043***	0.046***	0.047***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Cohort 1974	0.021**	0.019**	0.024**	0.021**	0.023**	0.028**	0.033***	0.040***	0.048***	0.049***
	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Cohort 1975	0.016	0.014	0.025**	0.024**	0.028**	0.032**	0.041***	0.050***	0.049***	0.052***
	(0.010)	(0.011)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Post-EE	0.035***	0.028*	0.032**	0.027*	0.023	0.025	0.032*	0.037**	0.042**	0.042**
	(0.013)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Constant	−0.418	1.803***	4.109***	5.945***	6.736***	5.727***	3.741***	0.937	−1.539**	−3.123***
	(0.566)	(0.623)	(0.664)	(0.693)	(0.714)	(0.728)	(0.737)	(0.741)	(0.742)	(0.740)
Observations	103,050	103,050	103,050	103,050	103,050	103,050	103,050	103,050	103,050	103,050
R-squared	0.001	0.001	0.004	0.011	0.021	0.033	0.049	0.067	0.086	0.105
F-test	2.62	1.26	1.65	2.15	2.35	3.06	4.78	6.75	7.35	7.99
p-value	0.02	0.28	0.14	0.06	0.04	0.01	0.00	0.00	0.00	0.00

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth. The sample contains women aged between 20 and 34 and includes cohorts born between 1962 and 1979. The F-test tests whether the coefficients on the excluded instruments are jointly equal to zero. The p-value corresponds to this F-test.

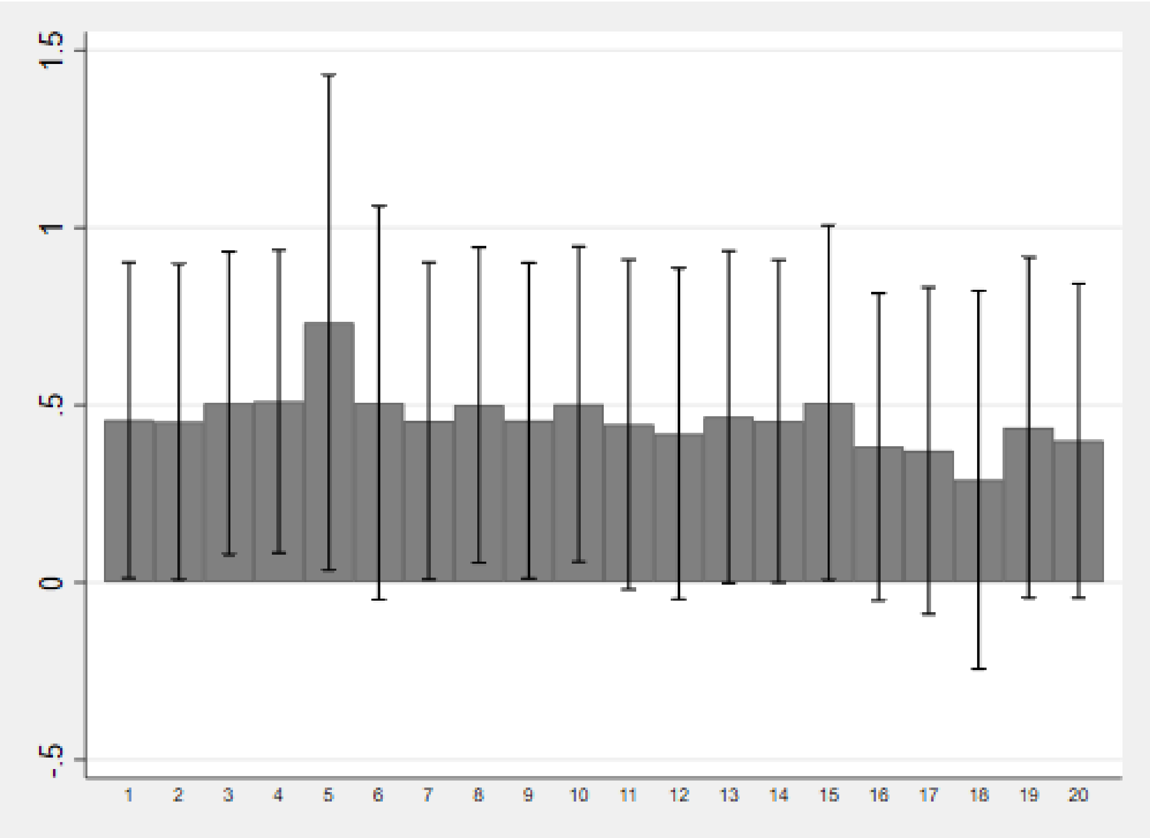
Table 6: OLS and IV Estimates of Education on Fertility Timing: Evidence from the Education Expansion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Age of 1st Birth		Teen Birth		Aged 24 or above		Aged 27 or above		Aged 30 or above	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A										
Age left FTE	0.723***	0.457**	-0.030***	-0.012	0.065***	0.039***	0.075***	0.061***	0.072***	0.079***
	(0.010)	(0.227)	(0.000)	(0.010)	(0.001)	(0.013)	(0.001)	(0.014)	(0.001)	(0.014)
Observations	45,572	45,572	103,050	103,050	103,050	103,050	103,050	103,050	103,050	103,050
Pre-EE mean	22.8		0.098		0.737		0.648		0.595	
Hansen J (p-value)		0.29		0.13		0.58		0.09		0.01
Hausman Test (p-value)		0.25		0.07		0.04		0.29		0.64
Panel B										
Degree	3.863***	4.998*	-0.117***	-0.099	0.307***	0.387***	0.387***	0.618***	0.382***	0.816***
	(0.064)	(2.571)	(0.002)	(0.088)	(0.003)	(0.122)	(0.003)	(0.131)	(0.004)	(0.139)
Observations	44,706	44,706	99,476	99,476	99,476	99,476	99,476	99,476	99,476	99,476
Pre-EE mean	22.9		0.097		0.735		0.643		0.587	
Hansen J (p-value)		0.30		0.16		0.82		0.43		0.37
Hausman Test (p-value)		0.64		0.85		0.51		0.08		0.00
Panel C										
Post-16	2.304***	1.718**	-0.133***	-0.089**	0.259***	0.115**	0.275***	0.138**	0.258***	0.166***
	(0.034)	(0.796)	(0.002)	(0.041)	(0.003)	(0.058)	(0.003)	(0.062)	(0.003)	(0.063)
Observations	45,572	45,572	103,050	103,050	103,050	103,050	103,050	103,050	103,050	103,050
Pre-EE mean	22.8		0.098		0.737		0.649		0.595	
Hansen J (p-value)		0.41		0.35		0.11		0.00		0.00
Hausman Test (p-value)		0.48		0.29		0.01		0.03		0.14

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth. The sample contains women aged between 20 and 34, and includes cohorts born between 1962 and 1979. The pre-EE mean is the average taken for the cohorts before the first education expansion cohort of 1972. The slight differences in the pre-EE mean are due to small differences in the sample sizes across the panels.

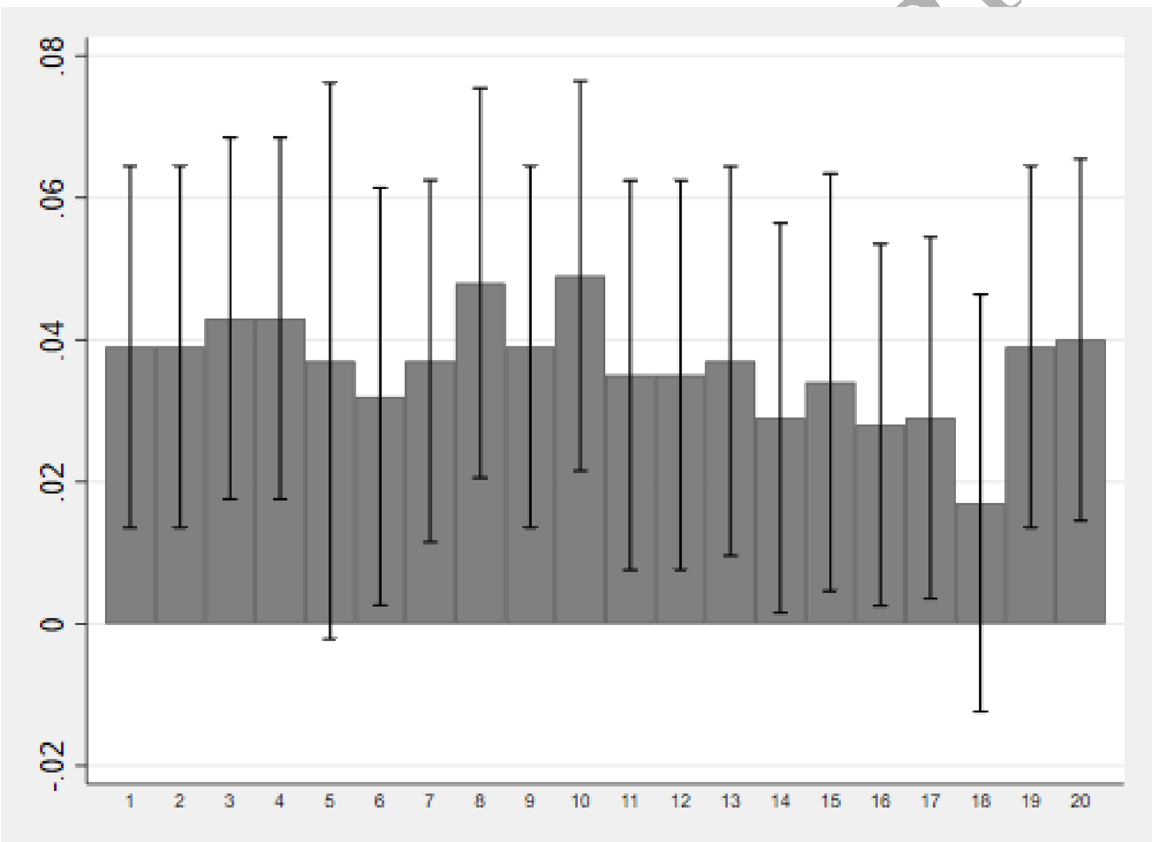
Appendix A: Additional Figures and Tables

Figure A1: Robustness Checks: Age at First Birth



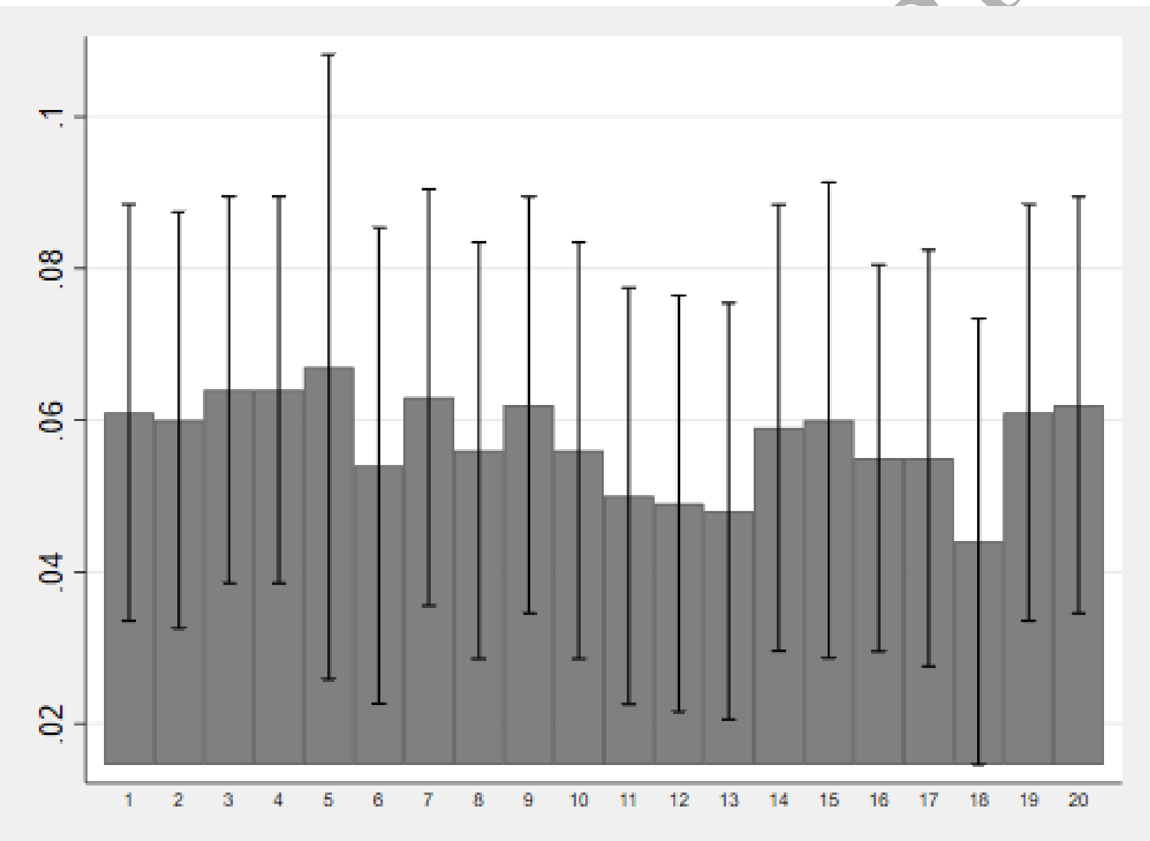
Notes: Each specification corresponds to a 2SLS estimate and the wiskers represent the 95% confidence interval. 1. Is the 2SLS baseline specification. 2. Excludes post-expansion dummy from the instrument set and instead includes it as a control variable. 3. Baseline plus 1970 and 1971 cohort dummies are included in the instrument set. 4. Baseline plus 1971 cohort dummy is included in the instrument set. 5. A single 1972-75 chort dummy plus post-expansion dummy form the instruments set. 6. We use three instruments, one for cohorts 1972-73, one for cohorts 1974-75, and one for a post-expansion dummy. 7. Age dummies replace age specification in the baseline specification. 8. Age cubic additionally included to the baseline specification. 9. Age quartic additionally included to the baseline specification. 10. Year of birth cubic and age quartic both additionally included. 11. Cohort age interaction included. 12. Cohort age interaction and its square included. 13. Cohort age interaction, its square and cubic included. 14. Cohort age interaction, its square, cubic and quartic included. 15. The sample restricted to those aged 24 to 34. 16. The sample restricted to the cohorts 1965–1979. 17. The sample restricted to the cohorts 1966–1979. 18. The sample restricted to the cohorts 1967–1979. 19. LIML is used instead of 2SLS. 20. Baseline plus an indicator for being married.

Figure A2: Robustness Checks: Birth After the Age of 24



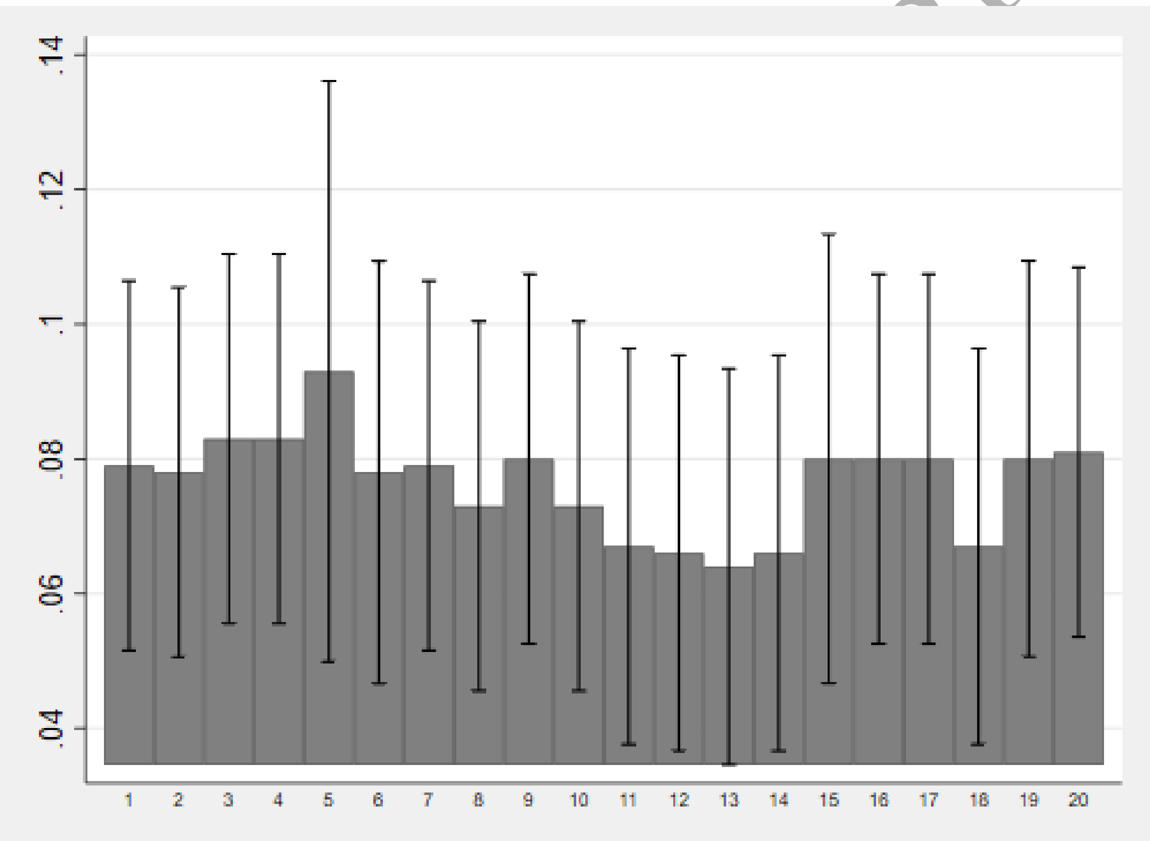
Notes: See notes to Figure A1.

Figure A3: Robustness Checks: Birth After the Age of 27



Notes: See notes to Figure A1.

Figure A4: Robustness Checks: Birth After the Age of 30



Notes: See notes to Figure A1.

Table A1: Summary Statistics of Age and Education Variables by Cohort

Cohort	Age	Min Age	Max Age	Age left full-time education	Post-16	Degree	No Qualifications
1962	25.4	20	34	16.9	0.393	0.082	0.188
1963	25.0	20	34	16.9	0.388	0.074	0.182
1964	25.0	20	34	17.0	0.413	0.077	0.172
1965	25.3	20	34	17.1	0.461	0.077	0.146
1966	25.4	20	34	17.1	0.474	0.082	0.150
1967	25.8	20	34	17.1	0.449	0.087	0.146
1968	26.5	20	34	17.1	0.441	0.099	0.137
1969	26.6	20	34	17.1	0.442	0.100	0.129
1970	26.4	20	34	17.2	0.433	0.109	0.132
1971	27.2	20	34	17.4	0.456	0.138	0.113
1972	27.6	20	34	17.5	0.466	0.162	0.108
1973	27.6	20	34	17.8	0.519	0.207	0.105
1974	27.5	20	34	18.0	0.541	0.227	0.093
1975	27.3	20	34	18.2	0.598	0.238	0.092
1976	27.2	20	34	18.2	0.624	0.254	0.087
1977	27.0	20	34	18.3	0.643	0.266	0.086
1978	27.0	20	34	18.3	0.632	0.272	0.083
1979	26.6	20	34	18.2	0.625	0.273	0.085
Total	26.1	20	34	17.3	0.470	0.130	0.137

Notes: Data: LFS 1975-2013. Women aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table A2: Summary Statistics of Age at First Birth and Birth Before a Specific Age (16 to 21) by Cohort

Cohort	Age of	Age first birth before					
	first birth	16	17	18	19	20	21
1962	22.2	0.005	0.011	0.030	0.062	0.107	0.150
1963	22.1	0.006	0.012	0.029	0.059	0.101	0.147
1964	22.3	0.005	0.010	0.026	0.055	0.095	0.137
1965	22.6	0.004	0.010	0.025	0.055	0.093	0.140
1966	22.8	0.006	0.010	0.025	0.056	0.092	0.134
1967	22.8	0.005	0.011	0.026	0.056	0.099	0.144
1968	23.1	0.004	0.008	0.024	0.059	0.102	0.148
1969	23.0	0.004	0.009	0.029	0.066	0.113	0.157
1970	23.1	0.003	0.009	0.026	0.065	0.105	0.152
1971	23.2	0.004	0.011	0.030	0.063	0.101	0.149
1972	23.2	0.005	0.011	0.032	0.066	0.112	0.160
1973	23.3	0.004	0.010	0.028	0.060	0.104	0.152
1974	23.2	0.005	0.013	0.030	0.065	0.104	0.146
1975	23.0	0.004	0.014	0.035	0.071	0.114	0.158
1976	23.1	0.002	0.010	0.027	0.063	0.108	0.152
1977	23.3	0.003	0.008	0.027	0.063	0.107	0.150
1978	23.1	0.005	0.011	0.032	0.070	0.112	0.160
1979	22.7	0.003	0.011	0.039	0.071	0.111	0.162
Total	22.8	0.004	0.011	0.028	0.061	0.103	0.148

Notes: Data: LFS 1975-2013. Women aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table A3: Summary Statistics of Age at First Birth After a Specific Age (21-30) by Cohort

Cohort	Age of birth by or after									
	21	22	23	24	25	26	27	28	29	30
1962	0.893	0.850	0.798	0.755	0.709	0.675	0.645	0.623	0.603	0.586
1963	0.899	0.853	0.810	0.771	0.736	0.702	0.675	0.655	0.635	0.625
1964	0.905	0.863	0.818	0.778	0.740	0.708	0.682	0.659	0.643	0.632
1965	0.907	0.860	0.821	0.783	0.746	0.714	0.687	0.666	0.648	0.635
1966	0.908	0.866	0.828	0.793	0.760	0.727	0.700	0.679	0.662	0.647
1967	0.901	0.856	0.812	0.772	0.738	0.707	0.680	0.653	0.633	0.616
1968	0.898	0.852	0.811	0.771	0.738	0.705	0.674	0.651	0.626	0.604
1969	0.887	0.843	0.801	0.762	0.727	0.696	0.665	0.635	0.609	0.582
1970	0.895	0.848	0.813	0.777	0.746	0.717	0.690	0.661	0.636	0.611
1971	0.899	0.851	0.814	0.777	0.744	0.708	0.674	0.642	0.614	0.588
1972	0.888	0.840	0.803	0.763	0.724	0.694	0.657	0.624	0.592	0.567
1973	0.896	0.848	0.808	0.772	0.745	0.709	0.677	0.652	0.627	0.603
1974	0.896	0.854	0.812	0.774	0.737	0.705	0.677	0.649	0.627	0.607
1975	0.886	0.842	0.802	0.769	0.737	0.711	0.684	0.662	0.641	0.614
1976	0.892	0.848	0.802	0.765	0.733	0.703	0.678	0.655	0.629	0.606
1977	0.893	0.850	0.811	0.773	0.741	0.713	0.687	0.664	0.644	0.624
1978	0.888	0.840	0.794	0.756	0.729	0.706	0.682	0.663	0.637	0.615
1979	0.889	0.838	0.801	0.765	0.739	0.712	0.690	0.666	0.640	0.622
Total	0.897	0.852	0.810	0.772	0.737	0.705	0.677	0.653	0.631	0.612

Notes: Data: LFS 1975-2013. Women aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table A5: Family Background Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Degree		Post-School Quals		Some Quals		No quals or schooling	
	Father	Mother	Father	Mother	Father	Mother	Father	Mother
Cohort 1972	0.018	0.010	−0.018	0.005	0.004	0.022	0.017	0.008
	(0.024)	(0.020)	(0.035)	(0.031)	(0.037)	(0.037)	(0.035)	(0.035)
Cohort 1973	0.033	−0.013	0.049	0.014	−0.030	−0.049	0.023	0.043
	(0.025)	(0.018)	(0.036)	(0.032)	(0.037)	(0.037)	(0.035)	(0.036)
Cohort 1974	−0.012	−0.000	−0.045	−0.008	−0.047	−0.040	0.023	0.032
	(0.025)	(0.022)	(0.037)	(0.034)	(0.040)	(0.040)	(0.038)	(0.038)
Cohort 1975	−0.015	−0.019	0.037	−0.015	0.070*	0.006	−0.063*	−0.003
	(0.027)	(0.022)	(0.041)	(0.036)	(0.041)	(0.041)	(0.037)	(0.039)
Post-EE	0.019	0.006	0.013	−0.020	−0.006	−0.037	−0.049	0.004
	(0.037)	(0.032)	(0.051)	(0.046)	(0.052)	(0.051)	(0.047)	(0.048)
Constant	5.049*	4.512*	8.179*	9.480**	11.236**	12.061***	−4.410	−7.067
	(3.016)	(2.477)	(4.254)	(3.967)	(4.412)	(4.461)	(4.704)	(4.440)
Observations	4,135	4,134	4,135	4,134	4,135	4,134	4,135	4,134
R^2	0.024	0.016	0.004	0.009	0.010	0.019	0.009	0.026
F-test	0.84	0.47	1.22	0.13	1.56	0.80	1.21	0.43
p-value	0.52	0.80	0.30	0.98	0.17	0.55	0.30	0.83

Notes: Data: The first wave of Understanding Society. Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. The sample includes women born between 1962 and December 1979. Degree (= 1) if the parent had a degree, 0 otherwise. Post-School Quals (= 1) if the parent had some post-school qualifications or above (i.e., including degree), 0 otherwise. Some Quals (= 1) if the individual indicates that the parent had some qualifications or above, 0 otherwise. No quals or schooling (= 1) if the parent left school with no qualifications or did not go to school at all, 0 otherwise. All specifications include a indicator for being non-white, a cubic polynomial in age and quadratic in year of birth.

Table A6: OLS and IV Estimates of Education on Fertility Timing for ages 31 and above

	Aged 31 and above		Aged 32 and above		Aged 33 and above	
	OLS	IV	OLS	IV	OLS	IV
Age Left FTE	0.070***	0.074***	0.068***	0.063***	0.066***	0.062***
	(0.001)	(0.014)	(0.001)	(0.014)	(0.001)	(0.014)
Pre-EE mean	0.584		0.577		0.571	
Observations	103,050	103,050	103,050	103,050	103,050	103,050
Hansen J (p-value)		0.00		0.00		0.00
Hausman Test (p-value)		0.79		0.70		0.76

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth. The sample contains women aged between 20 and 34, and includes cohorts born between 1962 and 1979. The pre-EE mean is the average taken for the cohorts before the first education expansion cohort of 1972.

Appendix B: Results of the impact of the Education Expansion on the Timing of Fatherhood

In this appendix we document and present the impact of the set of reforms we call education expansion on the timing of fatherhood. The LFS allows us to construct a variable that indicates the age of becoming a father based on observing the household and seeing the age of the father and eldest child, in the same way that we construct that age of motherhood. However, the assumptions required for this to be an accurate measurement of the age of becoming a father are stronger as they require that the child and the father are observed in the household. In the event of parental separation, given the high probability that the child stays with the mother, this is not as likely to be the case and is therefore not likely to be as accurate. Notwithstanding this caveat, we present the same tables as we do for the impact on mothers. Tables B1-B3 show the summary statistics.

In Table B4 we document the first stage effect of the education expansion on the educational attainment of men. The pattern is broadly similar to the effects found for women. We see the age left full-time schooling increases although not completely monotonically and the overall magnitude is slightly smaller for men. This is probably due to men starting from a higher base before the expansion in education. Similarly, we find positive and significant effects in staying on in school after the age of 16 and in achieving a degree. However, the impact of the expansion on not having any qualifications was not as strong.

Table B1: Summary Statistics of Age of Fathers and Education Variables by Cohort

Cohort	Age	Max Age	Min Age	Age left full-time education	Post-16	Degree	No Qualifications
1962	25.8	21	34	17.0	0.328	0.103	0.184
1963	25.2	20	34	17.0	0.314	0.096	0.186
1964	24.8	20	34	16.9	0.322	0.089	0.179
1965	25.0	20	34	17.0	0.350	0.087	0.169
1966	25.2	20	34	17.0	0.379	0.093	0.163
1967	25.7	20	34	17.0	0.364	0.093	0.161
1968	26.4	20	34	17.1	0.369	0.108	0.152
1969	26.4	20	34	17.1	0.362	0.112	0.137
1970	26.3	20	34	17.0	0.357	0.110	0.152
1971	27.2	20	34	17.3	0.396	0.144	0.120
1972	27.6	20	34	17.6	0.424	0.183	0.118
1973	27.5	20	34	17.6	0.430	0.184	0.127
1974	27.5	20	34	18.0	0.498	0.218	0.102
1975	27.0	20	34	18.0	0.529	0.224	0.113
1976	27.1	20	34	18.1	0.569	0.232	0.092
1977	26.7	20	34	18.1	0.573	0.233	0.093
1978	26.6	20	34	18.1	0.580	0.239	0.087
1979	26.3	20	34	18.1	0.580	0.228	0.099
Total	26.0	20	34	17.3	0.396	0.133	0.148

Notes: Data: LFS 1975-2013. Men aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table B2: Summary Statistics of Age of Fathers at First Birth and Birth Before a Specific Age (16 to 21) by Cohort

Cohort	Age of first birth	Age first birth before					
		16	17	18	19	20	21
1962	23.4	0.009	0.012	0.017	0.029	0.044	0.063
1963	23.3	0.007	0.013	0.018	0.025	0.036	0.056
1964	23.6	0.008	0.012	0.015	0.024	0.034	0.051
1965	23.6	0.007	0.010	0.015	0.022	0.035	0.052
1966	24.1	0.008	0.011	0.014	0.021	0.031	0.046
1967	24.3	0.007	0.010	0.016	0.023	0.036	0.052
1968	24.7	0.007	0.010	0.015	0.025	0.035	0.054
1969	24.7	0.007	0.012	0.016	0.025	0.038	0.056
1970	24.5	0.006	0.011	0.017	0.028	0.041	0.056
1971	24.7	0.011	0.014	0.020	0.028	0.041	0.057
1972	24.9	0.008	0.013	0.019	0.027	0.036	0.056
1973	24.6	0.014	0.019	0.026	0.036	0.046	0.057
1974	24.7	0.010	0.013	0.019	0.026	0.033	0.047
1975	24.5	0.010	0.018	0.024	0.031	0.041	0.055
1976	25.0	0.005	0.010	0.013	0.021	0.033	0.047
1977	24.6	0.011	0.016	0.021	0.026	0.033	0.050
1978	24.9	0.007	0.009	0.017	0.022	0.032	0.043
1979	24.1	0.011	0.014	0.022	0.030	0.042	0.059
Total	24.2	0.008	0.012	0.017	0.026	0.037	0.054

Notes: Data: LFS 1975-2013. Men aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table B3: Summary Statistics of Age of Fathers at First Birth After a Specific Age (21-30) by Cohort

Cohort	Age of birth by or after									
	21	22	23	24	25	26	27	28	29	30
1962	0.956	0.937	0.916	0.887	0.859	0.831	0.802	0.779	0.760	0.744
1963	0.964	0.944	0.923	0.900	0.875	0.849	0.827	0.807	0.790	0.778
1964	0.966	0.949	0.925	0.907	0.889	0.869	0.848	0.831	0.814	0.805
1965	0.965	0.948	0.929	0.908	0.887	0.868	0.850	0.834	0.822	0.811
1966	0.969	0.954	0.936	0.916	0.894	0.872	0.852	0.838	0.823	0.812
1967	0.964	0.948	0.932	0.912	0.892	0.872	0.856	0.838	0.822	0.806
1968	0.965	0.946	0.926	0.906	0.886	0.868	0.849	0.831	0.811	0.794
1969	0.962	0.944	0.928	0.910	0.892	0.872	0.851	0.832	0.809	0.789
1970	0.959	0.944	0.928	0.910	0.890	0.873	0.851	0.834	0.812	0.790
1971	0.959	0.943	0.926	0.907	0.891	0.871	0.850	0.827	0.801	0.780
1972	0.964	0.944	0.929	0.911	0.888	0.865	0.842	0.818	0.791	0.765
1973	0.954	0.943	0.926	0.911	0.893	0.876	0.854	0.838	0.816	0.794
1974	0.967	0.953	0.935	0.919	0.896	0.879	0.857	0.836	0.818	0.801
1975	0.959	0.945	0.929	0.915	0.898	0.877	0.855	0.841	0.819	0.803
1976	0.967	0.953	0.939	0.925	0.907	0.890	0.875	0.857	0.839	0.825
1977	0.967	0.950	0.934	0.920	0.906	0.886	0.873	0.856	0.844	0.827
1978	0.968	0.957	0.941	0.923	0.908	0.891	0.875	0.859	0.844	0.829
1979	0.958	0.941	0.925	0.907	0.891	0.876	0.864	0.851	0.837	0.822
Total	0.963	0.946	0.928	0.908	0.888	0.867	0.847	0.829	0.810	0.795

Notes: Data: LFS 1975-2013. Men aged between 20 and 34 are included in the sample and those born between 1962 and 1979.

Table B4: Education Expansion and Education Attainment of Men

	(1) Age left FT Ed	(2) Post-16	(3) Degree	(4) No Quals
Cohort 1972	0.270***	0.022**	0.037***	−0.008
	(0.050)	(0.010)	(0.008)	(0.006)
Cohort 1973	0.259***	0.026**	0.032***	0.007
	(0.054)	(0.011)	(0.008)	(0.007)
Cohort 1974	0.588***	0.088***	0.059***	−0.015**
	(0.064)	(0.012)	(0.010)	(0.008)
Cohort 1975	0.648***	0.123***	0.066***	−0.003
	(0.071)	(0.014)	(0.011)	(0.009)
Post-EE	0.724***	0.174***	0.067***	−0.024**
	(0.094)	(0.019)	(0.015)	(0.012)
Constant	−9.765**	−5.421***	−3.205***	1.771***
	(3.857)	(0.832)	(0.610)	(0.581)
Observations	90,021	90,021	88,564	88,673
R-squared	0.066	0.041	0.056	0.012
F-test	22.6	22.7	10.6	3.08
p-value	0.00	0.00	0.00	0.01

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, quadratic polynomial in year of birth, and year of survey dummies. The sample contains men aged between 20 and 34, and includes cohorts born between 1962 and 1980. The F-stat is a test for the joint significance of the 1972 to 1975, and post-expansion cohort dummies. The p-value corresponds to this F-test. The dependent variable in column (1) is a variable defining the age the individual left full-time education, column (2) is a dummy equal to 1 if the individual left school after age 16, column (3) is a dummy if the highest qualification achieved is a degree (or equivalent) or above, and column (4) is a dummy equal to 1 representing whether the individual has no qualifications.

In Table B5 we show the reduced form effect of the education expansion on the timing of fatherhood. In the first column we document the increase in the age of first birth. Relative to the pre-EE cohorts, men born in 1972 had children around a third of a year later, this increases to just over a year later for the post-EE cohort. In columns 2 through to 7 we show the reduced form impact of the education expansion on the timing of fatherhood for specific ages. We do find some evidence of a reduction in the likelihood of teen fatherhood. This is shown mostly in the older ages, that is, we find stronger effects for births before the age of 21 relative to births before father was 17, for example. We also find, as shown in Table B6, increases in the probability of delaying fertility for men as was found for women.

Table B5: Effect of the Education Expansion on Age of Fathers at First Birth: Teen Births

	(1) Age at First Birth	(2) < 16	(3) < 17	(4) < 18	(5) < 19	(6) < 20	(7) < 21
Cohort 1972	0.366**	−0.003	−0.001	−0.003	−0.005	−0.009**	−0.007
	(0.148)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)
Cohort 1973	0.199	0.003	0.004	0.004	0.003	−0.000	−0.008
	(0.182)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)
Cohort 1974	0.399*	−0.002	−0.002	−0.005	−0.010**	−0.016***	−0.021***
	(0.206)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)
Cohort 1975	0.469**	−0.003	0.002	−0.002	−0.007	−0.012**	−0.016**
	(0.238)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
Post-EE	1.102***	−0.007**	−0.006	−0.013**	−0.020***	−0.026***	−0.032***
	(0.313)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)	(0.009)
Constant	−6.168	0.401***	0.483***	0.853***	1.069***	1.129***	1.129***
	(13.633)	(0.148)	(0.178)	(0.217)	(0.265)	(0.320)	(0.382)
Observations	23,129	90,021	90,021	90,021	90,021	90,021	90,021
R-squared	0.258	0.002	0.002	0.003	0.003	0.002	0.002
F-test	3.18	2.74	2.54	3.56	4.27	4.60	3.55
p-value	0.01	0.02	0.03	0.00	0.00	0.00	0.00

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth, and year of survey dummies. The sample contains men aged between 20 and 34, and includes cohorts born between 1962 and 1979. The F-test tests whether the coefficients on the excluded instruments are jointly equal to zero. The p-value corresponds to this F-test.

Table B6: Effect of the Education Expansion on Age of Fathers at First Birth: Delaying Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	≥ 21	≥ 22	≥ 23	≥ 24	≥ 25	≥ 26	≥ 27	≥ 28	≥ 29	≥ 30
Cohort 1972	0.007	0.010**	0.016***	0.014**	0.014**	0.016**	0.014*	0.014*	0.013	0.011
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Cohort 1973	0.008	0.010*	0.020***	0.023***	0.028***	0.031***	0.037***	0.040***	0.043***	0.048***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Cohort 1974	0.021***	0.023***	0.032***	0.031***	0.034***	0.037***	0.036***	0.044***	0.050***	0.057***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Cohort 1975	0.016**	0.020***	0.033***	0.035***	0.034***	0.034***	0.039***	0.040***	0.047***	0.046***
	(0.007)	(0.007)	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)
Post-EE	0.032***	0.037***	0.050***	0.051***	0.049***	0.056***	0.055***	0.062***	0.070***	0.072***
	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)	(0.015)	(0.015)	(0.015)
Constant	−0.129	0.475	1.380***	2.515***	3.087***	2.236***	0.324	−1.977***	−4.334***	−6.001***
	(0.382)	(0.434)	(0.485)	(0.530)	(0.568)	(0.597)	(0.622)	(0.642)	(0.658)	(0.670)
Observations	90,021	90,021	90,021	90,021	90,021	90,021	90,021	90,021	90,021	90,021
R-squared	0.002	0.003	0.005	0.010	0.018	0.029	0.041	0.057	0.075	0.093
F-test	3.55	3.31	5.00	4.35	4.38	4.71	5.01	5.95	7.11	8.75
p-value	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth. The sample contains men aged between 20 and 34 and includes cohorts born between 1962 and 1979. The F-test tests whether the coefficients on the excluded instruments are jointly equal to zero. The p-value corresponds to this F-test.

Table B7 replicates Table 6 using the sample of men rather than women. The pattern of results is somewhat similar between the two. The 2SLS estimates are always larger than those estimated using OLS. We find that an additional year of education leads to just over half a year delay in the age of first child, however, the 2SLS estimates are not precisely estimated. Unlike the estimates for women, we do find a precisely estimated effect of years of schooling on the probability of being a parent whilst being a teenager. Relative to the pre-EE mean, however, the estimate of a 2.3 percentage is very large. We find precisely estimated effects of increases in years of schooling on delays in fertility, as we did for women. The estimates are in a similar range as those found for women. We find an additional year of schooling delays the probability of fatherhood until after age of 24, 27 or 30 by 5.4%, 6.4% and 9.4%, respectively. Furthermore, we examine different margins of education. In panel B we find that having a degree led men to delaying fertility as did leaving school after the age of 16.

Table B7: OLS and IV Estimates of Education on Fertility Timing of Men: Evidence from the Education Expansion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Age of 1st Birth		Teen Birth		Aged 24 or above		Aged 27 or above		Aged 30 or above	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A										
Age left FTE	0.433***	0.598	−0.008***	−0.023***	0.023***	0.048***	0.032***	0.053***	0.034***	0.073***
	(0.012)	(0.393)	(0.000)	(0.007)	(0.000)	(0.012)	(0.000)	(0.014)	(0.001)	(0.015)
Observations	23,129	23,129	90,021	90,021	90,021	90,021	90,021	90,021	90,021	90,021
Pre-EE mean	24.2		0.035		0.886		0.822		0.774	
Hansen J (p-value)		0.01		0.01		0.28		0.03		0.00
Hausman Test (p-value)		0.55		0.04		0.03		0.12		0.01
Panel B										
Degree	2.642***	5.928	−0.039***	−0.176**	0.119***	0.427***	0.174***	0.501***	0.182***	0.655***
	(0.079)	(3.761)	(0.001)	(0.071)	(0.002)	(0.118)	(0.003)	(0.137)	(0.003)	(0.156)
Observations	22,944	22,944	88,564	88,564	88,564	88,564	88,564	88,564	88,564	88,564
Pre-EE mean	24.2		0.035		0.886		0.821		0.774	
Hansen J (p-value)		0.02		0.01		0.19		0.04		0.00
Hausman Test (p-value)		0.26		0.04		0.00		0.01		0.00
Panel C										
Post-16	1.610***	4.525***	−0.034***	−0.136***	0.101***	0.216***	0.133***	0.194***	0.136***	0.271***
	(0.056)	(1.752)	(0.001)	(0.038)	(0.002)	(0.058)	(0.002)	(0.068)	(0.003)	(0.076)
Observations	26,272	26,272	117,519	117,519	117,519	117,519	117,519	117,519	117,519	117,519
Pre-EE mean	24.2		0.035		0.886		0.822		0.774	
Hansen J (p-value)		0.10		0.06		0.11		0.00		0.00
Hausman Test (p-value)		0.07		0.01		0.04		0.33		0.06

Notes: Robust standard errors in parenthesis. *, ** and *** respectively denote significance at the 10, 5 or 1 percent level. All specifications include a cubic polynomial in age, a quadratic polynomial in year of birth. The sample contains men aged between 20 and 34, and includes cohorts born between 1962 and 1979. The pre-EE mean is the average taken for the cohorts before the first education expansion cohort of 1972.