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DISCUSSION PAPER SERIES

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ABSTRACT

Does Early Timing of First Birth Lead to Lower Earnings in Midlife in Britain?*

Many studies show that motherhood has substantial impacts on women's wages and earnings, but there is less evidence on the effect of the timing of entry into motherhood, particularly over the long term and from contexts other than the US. We analyse a sample of women who became mothers by age 30 from the 1970 British Cohort Study to examine whether the timing of the first birth affects mothers' midlife earnings, as well as the role of potential mediators. In the framework of instrumental variable regression, our preferred specification utilizes the occurrence of contraceptive failure as a source of exogenous variation in the age at first birth. We find tentative evidence that a later first birth leads to a higher probability of having any earnings in midlife. However, a later first birth has a negative earnings effect among mothers with any midlife earnings. Potential mediators of this effect are part-time work and birth spacing, given that a later first birth led to a higher likelihood to work part-time at midlife and to a shorter interval between the first and second birth. These findings suggest that the long-term economic impacts of having an early first birth may not be uniformly negative.

JEL Classification: J13, J16, J30

Keywords: age at first birth, BCS70, earnings, employment, human capital, labour market attachment, midlife, Britain, United Kingdom

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Introduction

While an extensive body of research studies the negative effects of motherhood on women's careers (Cukrowska-Torzewska and Matysiak 2020; Matysiak and Vignoli 2008), the effects of the timing of entry into motherhood have received less attention, despite its growing relevance in light of current trends towards delayed childbearing (Mills et al. 2011). Many previous studies (e.g., Amuedo-Dorantes & Kimmel, 2005; Chandler et al., 1994; Taniguchi, 1999) have investigated the association between later age at first birth (AFB) and higher wages and higher labour force participation rates for mothers, as well as higher levels of family economic well-being (Hofferth 1984). However, a large fraction of the studies on this topic – including those cited above – use data from the United States (Blackburn, Bloom, and Neumark 1993; Buckles 2008; Herr 2016; Miller 2011; Troske and Voicu 2013; Wilde, Batchelder, and Ellwood 2010). In the US, AFB has increased less than in most other high-income countries (Frejka and Sardon 2006) and remains comparatively low (Human Fertility Database 2024). In Europe, there has been a strong trend towards later AFB over the past few decades (Human Fertility Database 2024). In the United Kingdom (UK), the average AFB has increased by three years since 1995 (UNECE 2024), reaching 29.1 in 2020 (Human Fertility Database 2024). There is a positive association between later family formation and women's midlife earnings also in Europe (Muller, Hiekel, and Liefbroer 2020), but our understanding of the mechanism underlying this association is limited. It is important to provide further evidence of the impact of the timing of entry into motherhood in Europe.

Methodologically, it is challenging to identify an effect of the timing of a first birth on earnings due to processes of self-selection. Women with better career prospects might plan to have children later, while women with poorer prospects might choose earlier childbearing. Similar differences can exist depending on women's individual value judgments of childbearing. Instrumental variables (IV) regression utilising biological fertility shocks (i.e., miscarriages or stillbirths) (Bratti and Cavalli 2014; Karimi 2014; Miller 2011; Rosenbaum 2021) and family background characteristics (Blackburn et al. 1993; Chandler et al. 1994; Kind and Kleibrink 2012) as instruments has previously been used to study the effects of the timing of a first birth. However, a limitation of this approach is the weak effects of these instruments on the timing (Bratti 2015; Wilde et al. 2010). Alternatively, studies have used individual fixed effects (FE) models (Amuedo-Dorantes and Kimmel 2005; Buckles 2008; Cantalini, Härkönen, and Dahlberg 2017; Dumauli 2019; Putz and Engelhardt 2014; Taniguchi 1999), which compare the change in earnings following motherhood between groups of women who had a different AFB, and who may differ in other respects too.

The current study aimed to overcome the methodological challenge of finding exogenous variation in first birth timing through an IV approach using contraceptive failure and biological fertility shocks as instruments. We built on previous studies that used biological fertility shocks as a source of exogenous variation in the AFB (e.g. Miller 2011). Using data from the 1970 British Cohort Study (BCS70), we analysed the earnings at midlife of women who had their first child between the ages of 15 and 30. In addition to total earnings as a measure of labour market success, we separately modelled the probability of reporting non-zero earnings and the amount of earnings, conditional on having non-zero earnings to account for selective labour market participation. Results from a naïve OLS regression indicated that a higher AFB between the ages of 15 to 30 was associated with higher earnings across all definitions. We conducted a series of diagnostic checks for instrument validity and found that contraceptive failure was much more closely related to AFB and it was more plausibly random than biological fertility shocks. Results from our preferred IV specification, using only contraceptive failure as an instrument for AFB, suggested that higher AFB was positively (by 2%, although not statistically significantly) related to the probability of reporting non-zero earnings at midlife, but also that postponing a first birth by one year significantly reduced the amount of earnings among employed women (by 6%). We also examined potential mediators of the effect of AFB on midlife earnings.

A limitation of this study is that the results may not be directly generalisable to women who delay their first child until their 30s, as only mothers with an AFB not higher than 30, for whom detailed pregnancy histories were available, could be analysed. Nevertheless, we provide new evidence to the debate on the labour market effects of the timing of entry into motherhood in Europe (Bratti and Cavalli 2014; Cantalini et al. 2017; Fitzenberger, Sommerfeld, and Steffes 2013; Karimi 2014; Kind and Kleibrink 2012; Lundborg, Plug, and Rasmussen 2017; Nisén et al. 2022; Picchio et al. 2021; Putz and Engelhardt 2014; Rosenbaum 2021). We assess the impact of AFB in the British context, where state support for working mothers is relatively weak (Brooks 2012; Sigle-Rushton 2008) and eligibility for job-protected leave was extended only more recently (Gregg, Gutiérrez-Domènech, and Waldfogel 2007). Our results show that later timing does not uniformly lead to better midlife outcomes among mothers with a relatively early AFB. Our focus on mothers' midlife earnings is a strength, given that mothers may need to financially support their children and save for retirement at this stage in their lives (Kahn, García-Manglano, and Bianchi 2014). The existing evidence on the impact of later entry into motherhood on midlife earnings remains mixed (Buckles 2008; Herr 2016; Karimi 2014; Picchio et al. 2021). The focus on midlife earnings deviates from many previous studies that have measured mothers' earnings shortly after their first birth or when they are in their thirties (Amuedo-Dorantes and Kimmel 2005; Bratti and Cavalli 2014; Miller 2011; Nisén et al. 2022; Taniguchi 1999), or estimated effects across a wide age range (Dumauli 2019; Kind and Kleibrink 2012; Putz and Engelhardt 2014; Taniguchi 1999).

The timing of first birth and earnings: a conceptual framework

Education and work experience

Human capital, acquired through formal education or work experience, can contribute to an increase in the future earnings of individuals (Becker 2009). Early childbearing can have a negative impact on young women's opportunities to acquire human capital through formal education. Women who have children before finishing their education face various challenges, such as difficulties in combining parenting responsibilities with the time commitments required for their studies, as well as the high costs of childcare in contexts such as the UK (Lyonette et al. 2015). These challenges can lead student mothers to drop out of higher education, and could have a discouraging effect on mothers who are considering pursuing a higher degree. Early childbearing may consequently impact women's long-term work trajectory and earnings through differences in formal education (Hynes and Clarkberg 2005; Miller 2011; Taniguchi 1999). Such a mechanism is less relevant at higher ages, such as in the late 20s and 30s, when formal education is less likely to be attained.

Child-related career breaks may interrupt human capital accumulation also at work (Mincer and Polachek 1974) or lead to a depreciation of existing knowledge and skills (Mincer and Ofek 1982). Such impacts are likely to increase with the length (and intensity) of the career break (Baum 2002), with a greater potential for a decline in wages following the break (Miller 2011). The timing of a first birth can affect the length of her career break, and thus subsequent earnings. Women who are at an early stage of their career, and who therefore have lower current earnings, might choose to take longer career breaks due to the lower opportunity costs and the lower net earnings relative to the costs of childcare. Thus, a positive effect of higher AFB on women's later earnings could be explained by longer career breaks for women who enter motherhood earlier. Alternatively, young women may be at a critical career stage where they are rapidly accumulating human capital (Leung, Groes, and Santaeulalia-Llopis 2016; Picchio et al. 2021), and where their investment in human capital is important for their later career (Herr 2016; Taniguchi 1999). Early mothers may therefore choose to take a shorter career break, which could have a positive impact on their later earnings. The timing of motherhood may be most critical for women with the potential for steep earnings profiles and when they have entered the labour market but have not yet progressed in their careers. Accordingly, the timing of career breaks may partly explain the motherhood wage gap (Landivar 2020) and the gender wage gap (Light and Ureta 1995).

Work adjustment and work effort

The theory of compensating wage differentials states that in order to meet the care needs of their children, mothers may make adjustments in their work life by choosing to work in jobs that are more easily reconciled with family responsibilities, for instance, by offering a part-time work schedule (Cukrowska-Torzewska and Matysiak 2020). These jobs pay typically lower wages, because workers can be attracted by the working conditions rather than by high wages, and can leave mothers with such jobs with lower earning opportunities. Moreover, work effort theory suggests that mothers might put less effort into their work than childless women, because of the effort required for childcare (Chandler et al. 1994; Cukrowska-Torzewska and Matysiak 2020; Kaufman and Uhlenberg 2000). This is likely to further reduce a mother's earnings and career opportunities. A recent study provided support for these hypotheses, finding that the decline in Danish mothers' earnings after childbirth is driven by lower labour force participation and fewer hours worked, and that Danish mothers are more likely to choose family-friendly working conditions (Kleven, Landais, and Søgaard 2019). An earlier entry into motherhood allows a longer time period for such effects to accumulate, and they may be particularly harmful for women with potentially steep career profiles who have not yet advanced in their careers when their first child is born.

Other mechanisms

Moreover, the effects of AFB may operate through the occurrence and the timing of subsequent births, which often lead to further career breaks (Karimi 2014; Troske and Voicu 2013). Earlier entry into motherhood is associated with a larger number of subsequent children (Bratti and Tatsiramos 2012), which may lead to amplification of the timing effects discussed earlier. A later entry into motherhood may also encourage a shorter interval between births, with potential implications for a mother's adjustment to work (Karimi 2014). Another partly overlooked aspect are partnerships and the division of work within the family (Ermisch and Pevalin 2005). The possibility for mothers to adjust their working hours or to choose family-friendly jobs with a lower level of compensation depends on the possibility to rely on a partner's income (Muller et al. 2020). For example, in the early 1990s, partner's unemployment led to a faster return to work after childbirth for British mothers who were entitled to maternity leave (Burgess et al. 2008). Employment constraints tend to be most severe for lone mothers, who may find it very difficult to accommodate full-time work despite financial pressures to do so (Joshi 2002). Finally, a motherhood earnings penalty that is not explained by any observable characteristics is often attributed to discrimination. Employers may expect mothers to put less effort into their work, and may thus discriminate against (potential) mothers (Benard and Correll 2010; Correll, Benard, and Paik 2007), especially if they take long career breaks (Albrecht et al. 1999). However, the impact on earnings may only become visible in the long term (Bratti and Cavalli 2014).

The British context

Britain represents a context in which many welfare services are provided by the private market and state interventions are often not universal (Brooks 2012). The motherhood wage penalty and the gender pay gap in the UK are comparatively high (Cukrowska-Torzewska and Matysiak 2020; OECD 2021). In this context, where motherhood is not easily combined with paid work, we would expect the AFB to impact mothers' earnings. Maternity rights were introduced in 1976 to allow women to take a career break in connection with childbirth, with the security of being able to return to their pre-birth job after the break (Gregg et al. 2007; Zabel 2009). In the 1990s, when most of the women in this study had their first birth, eligibility for job-protected leave depended on the woman's pre-birth employment situation. Only since 1994 have all women been entitled to a job-protected leave, regardless of their working hours (Zabel 2009). The length of this unconditional leave was increased from 14 weeks in 1994 to 18 weeks in 2000. During this period, mothers with a history of continuous employment were entitled to additional leave (e.g., in 1994 up to a total of 29 weeks after two years of employment with the same employer). Improvements in maternity rights, including relaxed eligibility conditions for leave and more generous pay during the leave, improved mothers' labour market attachment by facilitating a faster return to work (Burgess et al. 2008). Accordingly, these changes have increased the employment of mothers with young children since 1979, but the increases in the 1980s and 1990s were mainly in part-time work (Gregg et al. 2007). For British women, the low availability of affordable childcare is one of the most challenging aspects of combining motherhood with paid work (OECD 2020). Until the late 1990s, the UK government mainly relied on the private market for the provision of childcare. In 1998, the government began providing free part-time childcare for children aged three to four (Sigle-Rushton 2008). Although this increased the availability of childcare, it did not support full-time employment of mothers. Since the early 2000s, family policies have undergone further changes, including the extension of leave entitlements and improvements in childcare provision (Lewis and Campbell 2007; Sigle-Rushton 2008). The policies implemented since the late 1990s have had a limited impact on the women analysed in this study, as they were born in 1970 and had their first birth by 2000.

Previous empirical evidence

Studies from the US

Much of the research on the impacts of the timing of entry into motherhood on women's labour market outcomes comes from the US. Interest in the question of whether timing matters dates back to the 1970s (Cutright 1973). Special attention, particularly in the US, has been paid to teenage mothers, who often end up with poor educational and labour market outcomes, that appear to be at least partly the result of selection (e.g., Geronimus and Korenman 1992; Hoffman, Foster, and Furstenberg 1993; Hotz, McElroy, and Sanders 2005; Ribar 1999). More generally, Blackburn et al. (1993), Chandler et al. (1994), and Taniguchi (1999) were among the first to identify a positive effect of delayed first birth on women's wages for working women (see Supplementary Table S1 for an overview of the literature). A number of later studies have assessed the impact of the timing of entry into motherhood in the US by analysing data from the NLSY79[1]. Amuedo-Dorantes & Kimmel (2005) found that college-educated women experience a motherhood premium rather than a penalty in their wages, which increases in size with later AFB. Buckles (2008) identified a wage penalty across skills levels and ages, but also found that higher skilled mothers benefited more from a delay. Miller (2011) estimated a positive effect of 3.5% of a one-year delay on mothers' wages at age 34, and positive effects also on wage growth rates and hours worked. Wilde et al. (2010) provided further evidence of the beneficial effects of a delayed entry on US mothers' wages. Troske & Voicu (2013) found that a later entry into motherhood is associated with a smaller reduction in the post-birth propensity to work full time. Herr (2016) advocated for measuring career timing rather than AFB, and showed that the long-run returns to delaying first childbirth on wages are only observed for non-Hispanic white college-educated women who enter the labour market before motherhood. According to the results of Doren (2019), women without a college degree experience a *smaller* motherhood wage penalty when they enter motherhood early in life. Similarly, Landivar (2020) suggested that women in low-wage jobs do not benefit from delaying their first birth. Methodologically, these studies mainly relied on IV regression (Blackburn et al. 1993; Chandler et al. 1994; Miller 2011) or individual FE models (however, see Landivar 2020; Troske and Voicu 2013).

Studies from Europe

In the European context, the evidence on the effects of the timing of entry into motherhood on women's labour market outcomes is more limited, mixed, and methodologically diverse. Using IV regression, Bratti & Cavalli (2014) found that for Italian mothers, a delay has a positive effect on labour force participation and hours worked two years after the birth. According to Picchio et al. (2021), who utilised dynamic modelling with unobserved heterogeneity, a delay of the first childbirth strengthens Italian mother's labour market attachment and earnings, but a very long delay may result in a larger reduction in employment and decreasing returns to earnings. For Germany, using IV regression, Kind and Kleibrink (2012) estimated a 6.4% increase in mothers' wages with each additional year of delay in the first childbirth; Putz and Engelhardt (2014), applying individual FE to the same data, found *larger* wage gaps for women who enter motherhood later. Applying a matching approach, Fitzenberger et al. (2013) also found that in Germany, the effect of motherhood on labour market participation is larger for women who enter motherhood later. In Sweden,

[1] NLSY79 stands for National Longitudinal Survey of Youth 1979.

Karimi (2014) found a negative effect of delayed first birth on mothers' earnings, career earnings, and wages using IV regression as well as individual FE. Based on IV regression on a sample of IVF-treated women in Denmark, Lundborg et al. (2017) detected that earnings losses were larger among later mothers. In Denmark, Rosenbaum (2021), using IV regression combined with siblings FE, found that early timing of motherhood (<25 years) has a negative effect on earnings only until the early thirties, and that this effect is largely attributable to lower participation rates of early mothers. Nisén et al. (2022), utilising longitudinal dynamic modelling, found a positive effect of higher AFB on employment and income among Finnish women in their twenties and early thirties. Applying growth curve analysis, Cantalini et al. (2017) showed that Swedish women's earnings at age 40 do not vary by AFB, although a delay is beneficial for their career earnings.

Methodological considerations

Most previous studies have utilized longitudinal panel data to follow women, often in their thirties, while some of them examined the effects of the timing of entry into motherhood on short-term outcomes in the years following the first childbirth (see Supplementary Table S1). Among the studies utilising an IV approach, Bratti and Cavalli (2014) and Miller (2011) included ages up to 37 and 34. Blackburn et al. (1993) captured longer-term effects by covering ages up to 38 back in the 1980s, and Rosenbaum (2021) more recently by following mothers up to age 40. Studies based on individual FE models have often included higher ages yet in samples with a wide age range (Dumauli 2019), and the mean age in these studies was often below 40 (Amuedo-Dorantes and Kimmel 2005; Kind and Kleibrink 2012; Putz and Engelhardt 2014; Taniguchi 1999). Buckles (2008), Wilde et al. (2010), Cantalini et al. (2017), and Karimi (2014) better captured ages up to 40 and older. Unlike the current study, many previous studies have included women who had their first child after the age of 30. While the majority of these studies included also childless women in their analytic sample, studies using an IV approach typically analysed only mothers. Another observation is that some of the heterogeneity in birth effects on earnings by age in studies based on the FE approach may be due directly to age effects (see Robinson 2003) rather than to AFB.

Previous studies differ also with respect to the inclusion of measures. Some studies did not control for work-related human capital, given that it represents a mechanism of the estimated effect (Cantalini et al. 2017; Herr 2016; Rosenbaum 2021). Meanwhile, many studies focused on the effects net of differences in work experience (Amuedo-Dorantes and Kimmel 2005; Dumauli 2019; Karimi 2014; Kind and Kleibrink 2012; Putz and Engelhardt 2014; Taniguchi 1999; Wilde et al. 2010). Studies that treat work history as a mediator tend to find that education and work experience together explain a significant proportion of the observed variation in US women's wages by the timing of their first birth (Blackburn et al. 1993; Buckles 2008; Chandler et al. 1994). The available direct evidence hints at a partial mediating role of education alone (Blackburn et al. 1993; Chandler et al. 1994; Nisén et al. 2022). A few studies have controlled for other potential mediators, such as the number or spacing of children (Herr 2016; Karimi 2014; Wilde et al. 2010), and the US studies suggest only a small mediating role for number (Amuedo-Dorantes and Kimmel 2005; Buckles 2008; Miller 2011).

Data

The analysis presented in this study used data from the 1970 British Cohort Study (BCS70), which is an ongoing data collection that follows the lives of >17,000 UK residents (Elliott and Shepherd 2006; Sullivan et al. 2023). The study population includes all individuals who were born in the same week in Britain in 1970, and 95-98% of births in that week were captured in the initial data collection (UK Data Archive 2022). The data collection takes place at specific ages, with recent surveys being spaced between four and five years apart. This study incorporated information from throughout the cohort members' life course until 2016, when they were 46 years old. Since both the outcomes and the independent variable of interest (AFB) of this study were only measured at one point in time, no panel data analysis was possible. The final sample included 2,160 women for whom the necessary information was available (see Supplementary Table S2 for more details). The analytic sample was restricted to mothers who reported the birth of their first child (first live birth) in the Age 30 Survey, as this survey included a detailed history of previous pregnancies. Thus, the sample consisted of women with an AFB ≤ 30 and who had participated in the prospective follow-up.

Our main outcome – earnings – was measured at age 46 at the individual level as the weekly net labour income from a respondent's primary (paid) job. For those women who did not participate in the Age 46 Survey, data on earnings from the last available survey (i.e., Age 42 Survey) were used instead (~17% of observations in the final sample). Earnings were modelled as a continuous variable and transformed to their natural logarithm to normalise the distribution.[2] About 29% of the final sample reported zero earnings (Table 1). It is plausible that AFB would have different effects on the probability of having positive earnings and on the amount of earnings conditional on having positive earnings. We therefore decomposed the total earnings variable into two further outcomes: *(i)* a binary indicator measuring whether a woman reported earnings that were greater than zero; and *(ii)* the log-transformed amount of positive earnings for women who reported non-zero earnings (i.e. employed women). We explored other operationalisations in a series of sensitivity analyses.

We analysed several potential mediators of the effect of AFB on earnings: tertiary education, years in paid work (work experience), working hours, family size, and birth spacing. Tertiary education was assessed using information on the highest academic qualification recorded at age 34. Women who obtained at least an undergraduate degree, a higher education diploma (awarded after two years of full-time study at a university), or a teaching qualification were classified as having tertiary education. The years spent in paid work were derived from the Activities dataset. This measure covered all activities carried out by women from the age of 16 onwards. Work was measured in years and included all types of paid work, regardless of whether it was part-time or full-time work or dependent employment or self-employment. Periods of maternity leave, education, unemployment, illness, or unpaid care work were counted as time not spent in paid work. We assessed working hours using binary indicators for women reporting their employment status as working full-time or part-time. We used a binary indicator for women having two or more children as a measure of family size, and birth spacing is measured as the difference in months between the birth date of the first

[2] To avoid the occurrence of missing values for women without earnings, 1 GBP was added to all of the earnings before the logarithm was calculated.

and second child born to a woman (including births up to age 46). For the analysis of birth spacing only, we removed ~2.5% of respondents in the entire BCS70 sample that reported birth spacing of less than 10 months.

Method

We first examined the association between AFB and earnings in an OLS regression model. Then, we conducted a series of diagnostic checks to assess the validity of two candidate instrumental variables, before estimating our preferred IV specification. Heteroskedasticity-robust standard errors were used in all analyses. Since the BCS70 study includes almost all people who were born in a specific week, neither weighting of observations nor controlling for cohort or period effects was necessary. The analysis was carried out using Stata 18.0 (StataCorp 2023).

OLS regression

The OLS regression model can be written as:

$$Y_i = \beta_0 + A_i\beta_1 + \mathrm{X_i}\beta_2 + \varepsilon_i \qquad (1)$$

Y_i denotes the outcome variable for observation i. A_i represents the AFB and X_i is a vector containing several control variables predetermined before a woman's first birth.[3] We controlled for the age at completion of education of her mother as a proxy for the mother's educational level, her mother's AFB, the parents' occupational class at birth of the woman, whether she was conceived out of wedlock, and the marital status of her parents at the time of her birth. Moreover, the model included the region of birth to account for regional differences in socio-economic status and fertility behaviour. Finally, to control for systematic differences between earnings at age 46 and at age 42, a dummy variable for the survey from which the earnings data were taken was included. Estimates from this model can only be interpreted as causal effects if all relevant factors that may have influenced both AFB and earnings are controlled for. This assumption is likely to be violated by unobserved factors (e.g., career aspirations) that are related to AFB and earnings.

Instrumental variables regression

To address such potential biases, we carried out a two-stage least squares (2SLS) regression analysis. Two instruments were used for this analysis. The first instrument was a dummy variable describing whether a woman experienced a miscarriage or stillbirth in her first pregnancy. In the BCS70 data, a loss of pregnancy before the 26th week of pregnancy was defined as a miscarriage, whereas a loss of pregnancy after the 26th week was defined as a stillbirth. The second instrument used in the analysis was contraceptive failure, measured as a dummy variable describing whether a woman's first child was conceived despite the use of contraception at the time of conception.[4] If the first pregnancy was an abortion, information on the outcome of the first non-aborted pregnancy was used. Here, we

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[3] We modelled AFB using a linear trend, because modelling nonlinear trends in endogenous variables is very challenging in instrumental variable models and requires a different estimation approach. We explored the possibility of modelling a quadratic trend in AFB in a control function regression, however, the results (available on request) were inconclusive and likely affected by misspecification.

[4] Miller (2011) used these instruments together with a third instrument (years from first attempt to conceive to birth).

considered both instruments individually, because, as we show in Supplementary Material S3, they differed in their validity and in the strength of their association with the AFB.

The IV regression analysis followed a two-stage approach, delineated below:

$$A_i = \alpha_0 + Z_i\alpha_1 + X_i\alpha_2 + \nu_i \quad (2)$$

$$Y_i = \beta_0 + \widehat{A}_i\beta_1 + X_i\beta_2 + \varepsilon_i \quad (3)$$

where A_i denotes the AFB for women *i* and Z_i is one of the two possible instruments. In the second stage, $\hat{A}_i$ denotes the predicted values for the AFB from the first stage. Essentially, the IV model identified the causal effect of AFB on the outcome by focusing on the variation in AFB induced by biological fertility shocks or contraceptive failure. A miscarriage or stillbirth may have forced a woman who was about to have a child to delay childbearing until she became pregnant again at a later point in time. Contraceptive failure was expected to affect AFB in the opposite direction, as women who use contraception would usually have their first child at a later point in time after stopping contraception.

We conducted a series of diagnostic checks to examine the required assumptions for IV estimation. Following these diagnostic checks (see Supplementary Material S3 for a detailed discussion), we chose as our preferred IV specification a 2SLS regression using contraceptive failure as an instrument for the AFB, controlling for parental characteristics as shown in Table 1. IV estimation assumes that the instrument is exogenous and only related to the outcome through its effect on AFB. Previous studies have documented that contraceptive failure is more common among women with lower education (Musick et al. 2009) and in less stable partnerships (Wellings et al. 2013). We control for parental characteristics, because they are likely to capture such socioeconomic differences, but are not potential mediators as they are determined before sexual activity begins. We present additional estimates using the biological fertility shock instrument in the supplement (Supplementary Table S4).

We additionally examined potential mechanisms by estimating our preferred IV specification using the potential mediators as outcomes. We also conducted a mediation analysis (see Supplementary Material S7). Since this analysis requires additional, strong assumptions, the results should be considered only as suggestive evidence.

Results

Sample characteristics

The characteristics of the sample are presented in Table 1. On average, the women in the sample earned 222.72 GBP per week at midlife.[5] However, the outcome variable had a high standard deviation. The low mean and the high standard deviation of earnings can be explained by the fact that only 70.9% of the women had any labour earnings at midlife. All women who reported positive earnings were either full-time or part-time employed, whereas women

[5] This amount corresponds to 269 EUR and 302 USD using the exchange rates of 1st July 2016 and 31st May 2012 for the earnings measured in the BSC70 surveys in the respective years.

reporting a different employment status (self-employed, unemployed, in education, temporary or permanent sickness/disability, looking after home/family, and retired) did not report any earnings; only 22 women reported being in full-time or part-time employment, but did not report any employment income (not shown). Put differently, the indicator of having any earnings is essentially analogous with being employed. The characteristics of the women without earnings are presented in Supplementary Table S5. The table shows that compared to women who had some earnings, women with no earnings at midlife came from lower socioeconomic backgrounds, had less education, had worked fewer years, and were much more likely to belong to the lowest household income quintile.

The average AFB of the analysed women was just under 24 years, which was more than two years higher than that of their mothers (Table 1). The mean age was comparable to the figures reported for the 1970s in the UK (i.e., 23.7), and was close to mean age in 1994 (i.e., 26) – or when our cohort would have reached 24 years of age (ONS 2022). Less than 10% of these women experienced a miscarriage or stillbirth in their first pregnancy, and 15.5% conceived their first child while using contraception. A vast majority of these women (86%) had at least two children (Table 3), and the average number of children born to these women was 2.35 (not shown). Approximately a quarter of the women attained a tertiary-level degree (Table 3). The majority left the education system already after completing their O-levels/GCSEs, which were typically taken at age 16, the compulsory school age at the time, and only about a third continued their education after this stage and completed their A-levels or even a tertiary degree (not shown). Thus, these women spent more time in education than their mothers, who, on average, left the education system below the age of 16 (Table 1). The parents of a woman in our sample were typically married at the time of her conception and birth, and the majority of the parents belonged to the working class at the time of the woman's birth.

[Table 1 about here]

Effects on earnings

Supplementary Table S3.1 shows the first-stage regression for both potential instruments. Experiencing a biological fertility shock led to a shift in AFB of about 9.2 months, while contraceptive failure reduced AFB by about 1.5 years. Importantly, the F-statistic on the strength of the instruments suggests that biological fertility shocks are not sufficiently strongly related to AFB, while contraceptive failure is a much stronger predictor. We therefore estimate our preferred IV regressions using only the contraceptive failure instrument (see Supplementary Material S3 for a detailed discussion), controlling for several observable parental characteristics.

Table 2 shows the estimates of the effect of AFB on earnings from the OLS and IV regression models. Full regression tables are available in the supplement (Tables S6 and S7). Consistent with the findings of previous research, the OLS estimates showed a significant positive association, with a one-year delay in AFB being associated with a 7.5% increase in midlife earnings. Columns 2 and 3 suggest that this positive association was primarily driven by the probability of working: for example, delaying childbearing by one year was associated with a 1.3-percentage-point higher probability of reporting any earnings at midlife. In contrast, conditional on reporting any earnings, AFB was not significantly associated with the amount of earnings at midlife.

[Table 2 about here]

The point estimate of the effect of AFB on total earnings in the IV regressions was positive (5.5 percent), but not statistically significant. However, columns 2 and 3 show that this estimate concealed considerable heterogeneity. Column 2 indicates that higher AFB increased the probability of reporting any earnings at midlife by 1.8 percentage points. This was in line with the OLS estimate, although our IV estimate was not statistically significant. In contrast, in the IV regressions a higher AFB reduced the amount of earnings for women who were working by 5.8%,[6] which was in stark contrast to the insignificant (and close to zero) OLS estimate. A possible explanation for these results is that the OLS estimates were biased upwards by factors that were related to a higher AFB and higher earnings, but that were unrelated to labour force participation (e.g., career aspirations).

Potential mechanisms

Table 3 shows the estimated effect of AFB on eight potential mediators, broadly capturing women's education, previous and current employment, subsequent fertility, and current partnership status. For most outcomes, OLS and IV estimates were qualitatively similar, but effect sizes were larger in IV regressions with three exceptions (years in paid work, tertiary education, and having 2 or more children). This might indicate that the OLS estimates are biased towards zero. The IV estimates indicated that a delayed first birth led women to be 5.4 percentage points less likely to work full-time, and 6.8 percentage points more likely to work part-time. For years in paid worked, we estimated a positive yet insignificant coefficient of AFB. We did not observe any effects on education or the occurrence of a second birth. However, the delay of a first birth by one year led to a 5.5 months shorter interval between the first and second birth, and a higher probability to live with the father of the first child (+8.4 percentage points) as well as to live in any cohabiting partnership (+2.8 percentage points, significant at the 10 percent level).

[Table 3 about here]

A decomposition of the total effects of AFB on earnings into direct effects (not explained by the mediators) and indirect effects (explained through changes in the mediators) suggests that the total effects in the OLS regressions effects on earnings are almost completely explained through changes in the mediators (see Supplementary Tables S8.1 and S8.2). When the mediators are included in the IV regressions, they explain most of the effect of the AFB on the amount of earnings (i.e., the only outcome that was significantly affected by fertility timing in our main specification). However, due to missing values on the mediators the sample size in this mediation analysis is substantially smaller

[6] At the sample average of 314.2 GBP per week, a decrease of 6% corresponded to about 19 GBP per week. To put our point estimates in perspective, we note that the impact of an additional year of education on the earnings of women in the UK has been estimated to be 7.7% (Bonjour et al. 2003).

than the sample in Table 2, and consequently the estimated total effects do not match those reported in Table 2. We do not test the mediation by individual variables separately, as this would require making assumptions about sequential ignorability (Imai, Keele, and Tingley 2010) that are unlikely to hold.

Sensitivity analyses

We conducted a series of sensitivity tests to assess the robustness of our findings from the IV regression models (see Supplementary Table S9). First, we considered alternative operationalisations of earnings. We examined whether our results were sensitive to analysing (self-reported) net rather than gross earnings or to the inclusion of self-employment income in addition to income from the main job. The effects remained qualitatively similar with these alternative specifications. Results were also robust to controlling for tertiary-level education or the age of the youngest child in the household. Second, we re-estimated our IV regression models using different sample selection criteria. Our working sample did not condition on a woman having entered the labour market before first childbirth, but it has been suggested that a delay in first birth would only increase earnings of women who engaged in paid work prior to having their first birth (Herr 2016; Karimi 2014). Research focusing on teenage mothers suggests a negative impact on labour market outcomes up to the early thirties in Britain (Chevalier, Viitanen, and Viitanen 2003; Ermisch 2003). Our results remained similar with the exclusion of (i) women without work experience before their first birth, or (ii) women who had their first birth before age 20. With the first specification, the negative coefficient on the amount of positive earnings was significant at 10% level. We also used inverse probability weights (IPW) to correct for the selection of women whose AFB was ≤30 in our working sample. We first estimated a probit regression in which we regressed an indicator for our sample selection on the same covariates included in our regression models. Based on this probit regression, we constructed inverse probability weights and re-estimated our IV regressions using these weights to correct for sample selection. Standard errors for these IPW-adjusted IV estimates were derived by repeating this two-step procedure for 1000 bootstrap replications. The results from these regressions were very similar to our baseline specification, even if the negative effect on the amount of positive earnings was only significant at 10% level.

Discussion

Main findings and implications

This study investigated the impacts of the timing of entry into motherhood on women's midlife earnings in Britain, using BCS70 data on women with a detailed history of pregnancies recorded up to the age of 30. We drew on an IV regression approach, which assumed that the occurrence of contraceptive failure predicted the AFB but was not correlated with earnings through any other mechanism. Our rigorous testing provided support for this assumption. Our results provided only tentative evidence of a positive (~ 2%, non-significant) effect of a one-year increase in the AFB on women's likelihood of having any labour earnings at midlife (i.e., being employed). However, we found that among women with any positive earnings, a one-year increase in the AFB had a significant negative impact (~ 6%, equivalent to 19 GBP per week at the sample average) on the amount of midlife earnings. We further showed that AFB affected potential mediators of this negative impact, including the likelihood to work part-time at midlife and birth spacing. Altogether, these findings provide new evidence on the role of the timing of entry into motherhood on women's earnings – especially in a context, where access to job-protected maternity leave and affordable childcare is constrained. However, as the study sample only included women whose AFB was no higher than 30, the results are not directly generalisable to differences in timing to and among women who delay their first birth beyond age 30, who are often highly educated (Berrington, Stone, and Beaujouan 2015; Joshi 2002).[7]

Our findings are similar to some previous findings from Europe. For instance, using an IV regression with biological fertility shocks, Karimi (2014) estimated a negative effect of delayed motherhood on wages in Sweden. Rosenbaum (2021) combined IV regression with biological fertility shocks and siblings FE and found that early motherhood has no impact on earnings beyond the early thirties in Denmark. US studies have again typically found positive wage effects of later entry into motherhood, although some effects may be limited to women with higher levels of education (e.g., Doren, 2019; Herr, 2016). Such heterogeneity may partly explain why the present study does not find a positive impact of timing on earnings. Given the educational differences in the AFB, highly educated women are under-represented in the present study, which is based on British women whose AFB was ≤30. Moreover, the results could be related to differences in the motherhood penalty along the earnings distribution. Cooke (2014) found that the motherhood earnings penalty in the UK is largest for women in the middle of the earnings distribution, while women at the bottom actually receive a premium. Since, on average, earnings increase with age (Robinson 2003), early mothers are likely to be situated lower in the earnings distribution at the time of their first birth than later mothers. They may therefore experience less of a motherhood penalty or even a premium, supporting our finding of a positive effect of early entry into motherhood on midlife earnings for employed British mothers.

The negative effect of higher AFB on the amount of employed mothers' midlife earnings was not due to differences in human capital (i.e. education or work experience) or the occurrence of a subsequent birth. Rather, other mechanisms need to be considered. The positive effect of later timing on the likelihood of working part-time is a possible mechanism. A later first birth also led to mothers being more likely to be partnered in midlife, in which case they could rely on the income of a coresidential partner. The severe constraints on the employment choices of lone mothers are well known (Joshi 2002). At the same time, lone mothers may have stronger incentives to work full-time rather than part-time in order to support themselves and their children (Muller et al. 2020), especially given the high direct costs of children in the British context (e.g. childcare, education). Full-time work also becomes a more viable option for some British single mothers as their children grow older (Zagel 2014). Early motherhood among British women has also been found to increase the likelihood of partnering with a man who is at risk of unemployment (Ermisch and Pevalin 2005), which may be another factor discouraging part-time work for early mothers (Gregg et al. 2007). Secondly, the negative earnings effect of later motherhood, as mediated by part-time work, may be related to a more closely spaced second birth (see also Karimi 2014). A short interval between births may require a mother to take one long career break instead of two shorter ones and to make greater adjustments to her work in order to accommodate the care of young children, possibly leading to long-term adjustments in her working hours. The child-related career

[7] In the present study, 25.5% of women attained a tertiary-level degree. For the BCS70 female cohort as a whole, this proportion was 32%. Of all women, 54% had become mothers by age 30, with variation by education (see Joshi 2002).

breaks of British mothers have shortened over time (Joshi 2002). This trend coincides with an increase in part-time employment among mothers with young children in the 1980s and 1990s, alongside improvements in maternity rights, which led to only marginal increases in mothers' wages (Gregg et al. 2007). Future research should pay attention to potential mechanisms other than human capital – such as partnership history, birth spacing, and maternal work adjustment – in explaining differences in earnings between mothers with different AFBs.

Study limitations

The available sample size decreased the statistical power of our IV regression analysis, despite the significant association of the instrument with motherhood timing and no violations found of the IV assumptions. Future studies would benefit from larger datasets including information on contraceptive histories. Our tests of instrument validity suggested that biological fertility shocks might not be valid instruments for assessing the effect of motherhood timing, likely because of unobserved differences between women who do and do not experience such fertility shocks (see also Herr 2016; Wilde et al. 2010). However, using large administrative data, Karimi (2014) found that women who miscarried differed little from other women with respect to their prior health-related characteristics, while Rosenbaum (2021) suggested that complementing the IV estimation with sibling FE to control for family heterogeneities could produce estimates with higher internal validity. Future research should carefully assess instrument validity and also consider alternative approaches to establishing causality (see also Bratti 2015).

Our IV regressions identify the effect of AFB for women who gave birth earlier than planned, and therefore these effects may not be informative for women who were able to achieve their desired timing or who gave birth later than planned. Previous evidence shows that unplanned births often occur due to contraceptive failure rather than non-use (ESHRE 2018), and they are more common among women with lower levels of education (Musick et al. 2009; Wellings et al. 2013). Such differences may be due to lower economic and relationship stability, or lower efficacy and self-regulation, of the less educated. An unplanned pregnancy is also associated with a higher risk of depression and poorer health behaviors in the following year (Wellings et al. 2013), and possibly worse long-term mental health outcomes (Gipson, Koenig, and Hindin 2008). Furthermore, it can be argued that the ability to plan the timing of a first birth may allow a woman to reduce the career costs of motherhood (Bailey, Hershbein, and Miller 2012). As a result, an unplanned birth may be more damaging to a mother's career than a planned birth. Alternatively, an unplanned birth may be associated with fewer work adjustments and therefore less damaging to a career (Bearak et al. 2021). Moreover, anticipating the costs of having a child, such as career costs, may be associated with the degree of birth planning (Simoni, Mu, and Collins 2017) and the likelihood of having an unplanned birth (Brzozowska, Buber-Ennser, and Riederer 2021). Contraceptive use among British women is almost universal, but there is some evidence of socioeconomic, particularly educational, differences (Wellings et al. 2001).[8]

[8] In our study sample, at age 30, around 35% of women report currently taking the contraceptive pill, and 9% report never having taken any oral contraceptives.

Our sensitivity analysis showed that the main findings held even when educational attainment in addition to parental characteristics was controlled for in the IV models. Despite our statistical testing, we cannot completely rule out the possibility that unobserved differences between mothers who had a contraceptive failure and those who did not could affect our results. However, methodological considerations suggest that direct effects of contraceptive failure related to such factors, i.e. effects not attributable to lower AFB, should bias our estimates of the negative effect of AFB on employed mothers' earnings mainly towards zero. Based on the link between low education and unplanned births, we might expect contraceptive failure to be linked to a lower probability of employment and lower earnings. In such a case, our estimate of a true negative effect of timing on the amount of earnings would be conservative.

Finally, the restriction of the study sample to women who had their first child by the age of 30 limits the generalisability of our findings. A sensitivity analysis using inverse probability weights suggested that our results remain robust. However, that analysis required further assumptions, and observing births at older ages would provide stronger evidence than our sensitivity analysis could. Due to methodological limitations, we assumed that the marginal (monetary) returns to delaying childbearing by one year are constant. In practice, they may depend on the duration of the delay, and might therefore also differ for women giving birth before or after age 30. There is limited evidence on the shape of the "dose-response" relationship between timing of entering motherhood and earnings. A Danish study suggested larger gains to AFB before age 30 (Leung et al. 2016), while an Italian study found decreasing returns to earnings of very long delays (Picchio et al. 2021). A higher AFB may even lead to more diverse labour market trajectories (Frühwirth-Schnatter et al. 2016). We call for more evidence on the heterogeneity in the effects of motherhood timing by life stage. In theory, the timing may be most critical for women with potentially steep career profiles (e.g., highly educated) who have entered the labour market but have not yet progressed in their careers.

Conclusion

The findings of this study suggest that early motherhood has long-term effects on earnings, which may not always be uniformly negative. Our results show that for British mothers born in 1970 who were employed in midlife and did not delay their first birth until their thirties, earlier entry into motherhood led to higher earnings and a lower likelihood of working part-time. This may have been out of necessity, as early mothers were less often able to rely on the income of a co-resident father in midlife. Future research should disentangle the life course processes through which the long-term effects of the timing of entry into motherhood unfold.

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Table 1: Sample characteristics: British women with a first birth at age ≤30

	N	Mean	SD[1]	Min	Max
Net earnings	2,160	222.72	243.13	0	4,158.46
Net earnings>0[2]	1,531	314.22	233.76	0.77	4,158.46
Age at first birth (AFB)[3]	2,160	24.00	3.63	15	30
Mother's age at completion of education[4]	2,160	15.58	1.59	0	25
Mother's age at first birth[3]	2,160	21.64	3.65	12	43
	N	**%**			
P(Net earnings>0)					
0	629	29.1			
1	1,531	70.9			
First non-abortion pregnancy was a stillbirth/miscarriage					
No	1,955	90.5			
Yes	205	9.5			
First live birth was conceived while using contraception					
No	1,825	84.5			
Yes	335	15.5			
Parents' occupational class at woman's birth					
V unskilled	126	5.8			
IV partly skilled	397	18.4			
III manual	1,010	46.8			
III non-manual	286	13.2			
II managerial and technical or I professional	341	15.8			
Woman was conceived out of wedlock					
No	1,830	84.7			
Yes	330	15.3			
Parents' marital status at woman's birth					
Single, widowed, divorced or separated	158	7.3			
Married	2,002	92.7			

Notes: Region of residence at woman's birth consisting of ten categories is not shown in the table. [1]SD = Standard deviation. [2]Total amount of earnings among women who had non-zero earnings. [3]AFB refers to first live birth. [4]A value of zero for the mother's age at completion of education implies that the mother has never attended school. *Source:* BCS70, own calculations.

Table 2: Effects of the age at first birth (AFB) on earnings at midlife

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
OLS estimates			
Age at first birth[1]	0.075****	0.013****	0.007
	(0.016)	(0.003)	(0.005)
IV estimates			
Age at first birth[1]	0.055	0.018	-0.058**
	(0.097)	(0.017)	(0.027)
KP F-statistic	60.878	60.878	46.977
Number of observations	2,160	2,160	1,531
Mean of dependent variable	222.72	0.709	314.22

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1 The table shows estimates of an OLS and a 2SLS regression of earnings on the age at first (live) birth using contraceptive failure as an instrumental variable. The three outcome variables are defined as: log of total net earnings + 1 (Column 1), a binary indicator for whether a woman reported non-zero net earnings (Column 2), and the log of total net earnings conditional on reporting non-zero earnings (Column 3). Robust standard errors are shown in parentheses. Models control mother's age at completion of education, for mother's age at first birth, parents' occupational class at woman's birth, whether woman was conceived out of wedlock, parents' marital status and region of residence at woman's birth, and the survey at which the most recent earnings data was collected. [1]AFB refers to first live birth. *Source:* BCS70, own calculations.

Table 3: Effects of the age at first birth (AFB) on potential mediators

	Tertiary-level degree	N. of years in paid work	Full-time employed	Part-time employed	Two (or more) children	Birth spacing (1^{st} to 2^{nd})	Living with father	Living with partner
OLS estimates								
Age at first birth[1]	0.019****	0.572****	-0.008**	0.020****	-0.007****	-1.381****	0.040****	0.012****
	(0.003)	(0.052)	(0.003)	(0.003)	(0.002)	(0.249)	(0.003)	(0.003)
IV estimates								
Age at first birth[1]	0.003	0.475	-0.054***	0.068****	0.022	-5.453****	0.084****	0.028*
	(0.017)	(0.291)	(0.019)	(0.017)	(0.014)	(1.646)	(0.020)	(0.017)
N of observations	1,882	2,051	2,155	2,155	2,160	1,833	1,843	2,160
Mean of dep. variable	0.255	21.26	0.466	0.361	0.861	47.31	0.543	0.762

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. The table shows estimates of the effect of age at first (live) birth on the potential mediators specified in the columns. Estimates come from an OLS and a 2SLS regression using contraceptive failure as an instrumental variable. Robust standard errors are shown in parentheses. Models control mother's age at completion of education, for mother's age at first birth, parents' occupational class at woman's birth, whether woman was conceived out of wedlock, parents' marital status and region of residence at woman's birth, and the survey at which the most recent earnings data was collected. [1]AFB refers to first live birth. *Source:* BCS70, own calculations.

SUPPLEMENT

Supplementary Table S1: Overview of the literature on "Impact of the timing of entry into motherhood on labor market outcomes"

Study nr.	Authors (year)	Country	Data source	Mothers only[1]	Longi-tudinal[2]	Years	Main outcome[3]	Max. AFB[4]	Age at outcome measurement
1	Amuedo-Dorantes and Kimmel (2005)	US	NLSY79		X	1979-2000	wages	42	14-42
2	Blackburn et al. (1993)	US	NLS		(X)	1968-1982	wages	38	28-38
3	Bratti and Cavalli (2014)	Italy	Birth Sample Survey	X	X	2003-2005	LFP, hours worked	34	AFB ≤ 34 + 18-26 months of follow-up
4	Buckles (2008)	US	NLSY79		X	1979-2004	wages	48	20-48
5	Cantalini et al. (2017)	Sweden	Register data		X	1990-2012	earnings, cumulative earnings	40	18-40
6	Chandler et al. (1994)	US	Nat. Survey of Families and Households	X		1987-1988	wages	n.a	n.a.
7	Doren (2019)	US	NLSY79		X	1979-2014	wages	50	18-57
8	Dumauli (2019)	Japan	Japan Household Panel Survey		X	2004-2015	wages	40	18-40
9	Fitzenberger et al. (2013)	Germany	SOEP		X	1991-2008	employment	33	24-38
10	Herr (2016)	US	NLSY79	X	X	1979-2008	wages	n.a [5]	44-51
11	Karimi (2014)	Sweden	Register data	X	X	1985-2007	earnings, wages	n.a.[5]	16-64
12	Kind and Kleibrink (2012)	Germany	SOEP	X		2000-2010	wages	65	20-65
13	Landivar (2020)	US	American Community Survey			2011-2015	wages	44	35-44
14	Lundborg et al. (2017)	Denmark	Register data		X	1991-2009	earnings (for timing)	40	mean AFB 32 + ≤ 10 years of follow-up
15	Miller (2011)	US	NLSY79	X	X	1979-2000	earnings, wages	33	21-34
16	Nisén et al. (2022)	Finland	Register data		X	1990-2007	employment, earnings, educ.	32	15-32
17	Picchio et al. (2021)	Italy	AD-SILC (survey + admin. data)		X	1977-2005/2011	earnings, working days / year	45	26-45
18	Putz and Engelhardt (2014)	Germany	German Socio-Economic Panel		X	1984-2010	wages	45	17-45
29	Rosenbaum (2020)	Denmark	Register data	X	X	1980-2014	earnings, LFP, wages, educ.	40	20-40
20	Taniguchi (1999)	US	NLS		X	1968-1992	wages	44	14-44
21	Troske and Voicu (2013)	US	NLSY79		X	1979-2004	LFP, hours worked	46	15-46
22	Wilde et al. (2010)	US	NLSY79	X	X	1979-2006	wages	49	14-49

Supplementary Table S1 continues

Study nr. 1	Statistical method	Main finding
1	FE with selection bias adjustment	AFB ≥30 decreases the motherhood wage penalty among college-educated women.
2	OLS and IV (family background characteristics)	Higher AFB increases wages (in part due to differences in human capital).
3	IV (miscarriage/stillbirth)	Higher AFB increases mothers' after-birth LFP and working time, but not wages.
4	FE	Higher AFB is related to a lower motherhood wage penalty for highly educated mothers. [7]
5	FE (growth curve analysis)	Small differences in earnings at age 40 by AFB, but earlier mothers have lower cumulative earnings.
6	OLS, IV (religious attendance/number of siblings)	Predicted AFB is positively related to wages, but the relationship weakens at longer durations since childbirth.
7	FE	Higher AFB is related to a lower wage penalty for highly educated mothers, but not for less educated mothers; among less educated mothers, the wage penalty is smaller among those who entered motherhood at ages 20-22 than at ages 23-27. [7]
8	FE	AFB is not related to the magnitude of the motherhood wage penalty.
9	IPW-matching	Higher AFB is not related to smaller employment effects of motherhood. [7]
10	OLS (+ IV)	Later timing is related to higher wages among Non-Hispanic white college-educated women who have their first child after entering the labour market; in other groups, later timing has no effect or a negative effect on wages. [6]
11	IV (miscarriage), FE	Later timing decreases career earnings and career mean wages, and increases the motherhood wage penalty in a sample of college-educated women. [6, 7]
12	IV (AFB of the mother-in-law)	Higher AFB and later career timing increase mothers' wages.[6]
13	OLS with random effects	Delay of first birth is beneficial for wages of women in high-wage occupations, but not for other women.
14	IV (IVF treatment)	Women with an AFB of <32 suffer a larger motherhood penalty than mothers with an earlier AFB.
15	IV (miscarriage/ stillbirth/contraceptive failure), FE	Higher AFB has positive effects on all outcomes, including on wage growth after entering motherhood. [7]
16	Longitudinal G-formula	Higher AFB has positive effects on employment and earnings.
17	Factor analytical dynamic model with unobserved heterogeneity	Having a first birth ≤6 years after graduation leads to slower recuperation of earnings than having a first birth 7-9 years after graduation, but later timing has more negative effects on earnings and on the days worked.[6]
18	FE	Higher AFB and later career timing are related to a larger motherhood wage penalty.[6]
19	IV (miscarriage), combined with siblings FE	Early childbearing (<25 years) does not affect earnings, LFP, or wages at age 40 (but does have a small impact on education).
20	FE	Higher AFB is related to a lower motherhood penalty, but teenage mothers experience no penalty.
21	Markov Chain Monte Carlo simulation	Later timing leads to higher pre-birth LFP, and therefore to a weaker post-birth reduction in LFP.[7]
22	FE (+ IV)	Higher AFB is related to a lower motherhood penalty, but only among highly skilled women over the long run.[7]

Notes:

This table lists studies that are most relevant for the current study; i.e. studies that apply a causal study design to analyze the effects of the timing of entry into motherhood on earnings, wages, or other closely related labour market outcomes. The table does not include important earlier studies focusing on the same topic with a descriptive rather than a causal study design (e.g., Frühwirth-Schnatter et al. 2016; Leung, Groes, and Santaeulalia-Llopis 2016; Muller, Hiekel, and Liefbroer 2020), or studies focusing on the effects of teenage births (e.g., Ermisch 2003; Geronimus and Korenman 1992; Ribar 1999). References to the studies listed in the table can be found in the reference list of the article.

AFB = Age at first birth
FE = Fixed effects regression
IPW = Inverse probability weighting
IV = Instrumental variables regression
LFP = Labour force participation
NLS = National Longitudinal Survey of Young Women
NLSY79 = National Longitudinal Survey of Youth 1979
OLS = Ordinary least squares regression
SOEP = German Socio-Economic Panel

[1] The column indicates whether the analytic sample included only mothers (i.e. excluded childless women).
[2] The column indicates whether the analysed data were longitudinal rather than cross-sectional.
[3] The column indicates the main outcomes measures analysed with respect to timing of the entry into motherhood.
[4] The column indicates the highest age at first birth eligible in the study. Note that in some of the studies this maximum age applied only for a part of the sample (i.e. later-born women were measured at a younger age).
[5] These studies measured timing in terms of the career stage rather than in terms of age. Herr (2016) analysed women with a first birth no later than in their 17th career year, and dropped 2% of latest mothers. In her supplementary analysis, utilising FE models on the who sample of women (incl. childless women) Karimi (2014) restricted her sample to women who had their first birth at ages 35 and younger.
[6] Analyses career timing instead of or in addition to AFB.
[7] Excluded teen mothers.

Supplementary Table S2: Sample selection criteria

BCS70 cohort at birth (Birth Survey)	17,196
Exclusion criteria	
Male or unknown sex	8,917
Lost to follow-up between Birth Survey and the Age 30 Survey	2,894
No information on age at first birth (incl. childless) in the Age 30 Survey	2,489
Missing values on all outcomes (lost to follow-up since the Age 30 Survey)	607
Missing values on one of the covariates	78
Abortion after contraceptive failure prior to the first live birth	51
Final sample	2,160

Notes: Table S2 describes the selection process of the analytical sample for this study. The initial BCS70 sample was restricted to women who reported the birth of their first child in the Age 30 Survey of data collection, since this survey included very detailed questions on all previous pregnancies. The group of women who were excluded due to no information on their age at first birth in the Age 30 Survey was mostly composed of women who were childless. All women who were included in the analytical sample had their first live birth at the age of 30 or below. Moreover, only women who provided complete information on the outcomes and all covariates were included. Lastly, women who had an abortion of a child conceived while they were using contraception before they had their first live birth were excluded to improve the validity of the analysis. Source: BCS70, own calculations.

Supplementary Material S3: IV assumptions and diagnostic checks

For valid identification of a causal effect of the timing of entry into motherhood, we had to make three assumptions: reliability, validity, and monotonicity. Reliability implies that the instrumental variable (i.e., biological fertility shocks or contraceptive failure) should have a non-zero effect on the timing. Even if the instruments had a significant effect on the age at first birth, the precision of our estimates depended crucially on the strength of the instruments. The strength of an instrument depends on the sample size as well as the amount of variation in the endogenous treatment variable explained by the excluded instruments, and it is typically assessed by comparing an F-statistic to established threshold values (Stock and Yogo 2005). Table S3.1 shows first-stage estimates for both instruments, both jointly and individually. Estimates for both instruments showed the expected sign and were highly statistically significant, which implies that experiencing a miscarriage or stillbirth led to a postponement of fertility by 8-9 months, whereas women who experienced contraceptive failure tended to give birth about 1.5 years earlier. However, the Kleibergen-Paap F-statistic indicated that contraceptive failure was much more closely linked to the timing of entry into motherhood than to biological fertility shocks. The F-statistic for biological fertility shocks used individually as an instrument for the timing was below the threshold values commonly used to identify weak instrument problems (~16.4 following Stock & Yoko (2005); or ~23.1 following Olea & Pflueger (2013), whereas the F-statistic for contraceptive failure was much stronger, at around 60.

Instrument validity required that biological fertility shocks and contraceptive failures were as good as random, and that these shocks affected earnings only by changing the age at first birth. For pregnancy loss, one concern might be that there were behavioural factors that influenced both the likelihood of experiencing pregnancy loss and socio-economic status. However, a recent review concluded that lifestyle factors play only a minor role in predicting miscarriages, and that most miscarriages can be attributed to structural malformations or genetic aberrations of the foetus, which are likely to be randomly distributed (Larsen et al. 2013). Another potential concern is the possibility of reverse causality, because the risk of pregnancy loss is higher at higher maternal ages. However, this association does not seem to occur before the age of 30 (Andersen et al., 2000; de La Rochebrochard & Thonneau, 2002; Magnus et al., 2019). It is also possible that a miscarriage or stillbirth might have negatively affected some women's health in the long run, with negative consequences for earnings, e.g., due to extended sick leave. In this scenario, the exclusion restriction for the fertility shock instrument would be violated, leading to a downward bias in the IV estimates. Similarly, a woman's age might also determine her risk of contraceptive failure. However, the empirical literature on this question found that there are no major age-related differences in this risk below the age of 30 (Kost et al. 2008; Moreau et al. 2007). Finally, women's attitudes towards a potential pregnancy may also influence their risk of contraceptive failure. Ambivalence about pregnancy has been shown to be associated with less consistent use of contraception, which might increase the risk of contraceptive failure (Frost and Darroch 2008). Moreover, women with a strong preference not to have a child at the time of the contraceptive failure might have been more likely to undergo an abortion than women who were more ambivalent. To avoid self-selection by abortion, women who had an abortion after a contraceptive failure prior to their first live birth were excluded from the analysis (n=51). We note, however, that underreporting of abortions was documented in an earlier study of the BSC70 data (Ermisch and Pevalin 2005). In general, abortion is often under-reported in surveys (ESHRE 2018).

We also conducted several diagnostic checks to identify potential violations of this assumption. First, we examined whether a range of predetermined variables were balanced across values of the instrument (Tables S.3.2.1 and S.3.2.2). We considered the covariates included in our regression models, which referred to characteristics of the woman's parents. Additionally, we considered several variables on risky behaviour (alcohol consumption, smoking, partnership status, sexual activity, contraceptive use) measured at age 16. These variables are measured before the first birth and can therefore not be influenced by either the instrument itself or the treatment. We would thus expect the average levels of these covariates to be similar for women who experienced a pregnancy loss or a contraceptive failure and women who did not. We estimate linear regression models of the chosen covariates on each of the instruments separately to examine whether the instruments are significantly associated with these predetermined characteristics Table S3.2 shows that there were significant differences in parental socio-economic status between women who experienced a pregnancy loss and those who did not. Moreover, women who experienced a pregnancy loss were less likely to have used a contraceptive gel at age 16. In contrast, the parental characteristics of women who experienced a contraceptive failure were very similar to those of women who did not. While we found a significant difference in the average age of a mother at her first live birth, the magnitude of this difference was relatively small, at about half a year. Importantly, women who experienced contraceptive failure were not significantly more likely to exhibit risky behaviour at age 16. On the contrary, they were significantly more likely to have used the pill for contraception.

Second, we implemented a recently proposed test of instrument validity and monotonicity (Mourifié and Wan 2017). Monotonicity was the third assumption required for causal inference with instrumental variables, and it implied that the instrument should affect treatment status in the same direction for all units in our sample. In our case, this meant that a pregnancy loss should not lead to a lower age at first birth for any women in our sample, and contraceptive failure should not lead to a later age at first birth. Taken together, instrument validity and monotonicity imply that the conditional distributions of the untreated and treated outcomes are nested. The test proposed by Mourifié and Wan (2017) builds on these implications and makes it possible to detect certain violations of either validity and monotonicity. The test only works for cases in which both the treatment and the instrument are binary. Therefore, we constructed a binary treatment indicator for women who first gave birth at age 25 or older. We ran the test separately for both instruments and all three definitions of earnings: the log of total net earnings; the probability of reporting non-zero earnings; and the log of net earnings, conditional on reporting non-zero earnings. We tested both unconditional and conditional instrument validity by implementing the test both without and with covariates. For the fertility shock instrument, both the unconditional and the conditional test failed to reject the null hypothesis of joint instrument validity and monotonicity. For contraceptive failure, the results of the unconditional test indicated concerns about instrument validity and monotonicity. The test rejected the null hypothesis at the 5% level for earnings, at the 1% level for the probability of reporting any earnings, and at the 10% level for earnings conditional on reporting any earnings. Once we accounted for parental characteristics as covariates, the test failed to reject the null hypothesis, suggesting that the instrument is valid.

While the test of instrument validity and monotonicity suggested that both instruments were valid, conditional on the selected covariates, we decided that based on the covariate imbalances documented in Table S3.2, contraceptive failure was a more valid instrument for assessing the timing of entry into motherhood than pregnancy loss. Moreover, in our sample, fertility shocks were a weak instrument, whereas contraceptive failure was much more closely related to the timing.

Supplementary Table S3.1: First stage regression

	Both instruments	Only fertility shocks	Only contraceptive failure
First non-abortion pregnancy was a stillbirth/miscarriage	0.665***	0.774***	
	(0.241)	(0.243)	
First live birth was conceived while using contraception	-1.577****		-1.607****
	(0.206)		(0.206)
Mother's age at first birth	0.151****	0.160****	0.153****
	(0.023)	(0.024)	(0.023)
Parents' occupational class at woman's birth			
V unskilled (ref.)			
IV partly skilled	0.328	0.291	0.361
	(0.381)	(0.385)	(0.383)
III manual	1.231****	1.214****	1.278****
	(0.354)	(0.357)	(0.356)
III non-manual	1.686****	1.695****	1.698****
	(0.394)	(0.398)	(0.396)
II managerial and technical or I professional	1.982****	1.980****	2.002****
	(0.397)	(0.401)	(0.400)
Mother's age at completion of education	0.180****	0.176****	0.180****
	(0.047)	(0.048)	(0.047)
Region of residence at woman's birth			
North (ref.)			
Scotland	-0.661*	-0.709**	-0.692**
	(0.347)	(0.355)	(0.347)
Northern Ireland	1.485***	1.770***	1.401**
	(0.555)	(0.543)	(0.559)
Yorkshire and Humberside	-0.672**	-0.635*	-0.695**
	(0.329)	(0.339)	(0.327)
East Midlands	-0.688*	-0.644*	-0.720*
	(0.381)	(0.386)	(0.382)
East Anglia	-0.490	-0.345	-0.472
	(0.478)	(0.479)	(0.481)
South East	-0.341	-0.323	-0.373
	(0.271)	(0.278)	(0.270)
South West	-0.047	-0.034	-0.057
	(0.326)	(0.331)	(0.325)
West Midlands	-1.090***	-1.022***	-1.108***
	(0.345)	(0.350)	(0.346)
North West	-0.786**	-0.775**	-0.811**
	(0.314)	(0.320)	(0.315)
Wales	-1.007**	-1.047***	-1.042***
	(0.398)	(0.404)	(0.397)
Woman was conceived out of wedlock			
No (ref.)			
Yes	-0.256	-0.225	-0.229
	(0.253)	(0.257)	(0.252)
Parents' marital status at woman's birth			
Single, widowed, divorced or separated (ref.)			
Married	0.528	0.564	0.536
	(0.349)	(0.351)	(0.351)
Survey at which the most recent earnings data were collected			
46 (ref.)			
42	-0.239	-0.273	-0.246
	(0.200)	(0.202)	(0.200)
KP F-statistic	34.254	10.137	60.878
Number of observations	2,160	2,160	2,160

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01, **** p < 0.001. The table shows estimates from the first stage regression of the age at first (live) birth on the instruments and control variables. Standard errors are shown in parentheses. *Source*: BCS70, own calculations.

Supplementary Table S3.2.1: Covariate balance for instrumental variable assignment – socioeconomic characteristics

	Fertility shock	Contraceptive failure
Mother's age at first birth	0.466	-0.581***
	(0.299)	(0.215)
Mother's age at completion of education	-0.022	-0.039
	(0.135)	(0.085)
Parents' marital status at woman's birth	0.005	-0.012
	(0.019)	(0.016)
Woman was conceived out of wedlock	0.031	0.003
	(0.028)	(0.022)
Parents' occupational class at woman's birth: V unskilled	-0.038***	0.002
	(0.012)	(0.014)
Parents' occupational class at woman's birth: IV partly skilled	-0.009	0.033
	(0.028)	(0.024)
Parents' occupational class at woman's birth: III manual	0.114***	-0.002
	(0.036)	(0.030)
Parents' occupational class at woman's birth: III non-manual	-0.044**	-0.015
	(0.022)	(0.019)
Parents' occupational class at woman's birth: II managerial	-0.024	-0.017
and Technical or I professional	(0.025)	(0.021)
Number of observations	2,160	2,160

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. The table shows estimates of an OLS regression of each outcome (specified in the rows) on the two instruments. Robust standard errors in parentheses. *Source:* BCS70, own calculations.

Supplementary Table S3.2.2: Covariate balance for instrumental variable assignment – risky sexual behaviour

	Fertility shock	Contraceptive failure
Alc. drinks yesterday (age 16)	-0.052	0.018
	(0.081)	(0.078)
N	835	835
Cigarettes last week (age 16)	-0.995	1.880
	(2.623)	(2.220)
N	855	855
Had partner at age 16	0.045	0.064
	(0.059)	(0.047)
N	858	858
Felt ready for sex at age 16	-0.064	0.039
	(0.049)	(0.042)
N	1,061	1,061
Contraception at 16: withdrawal	-0.040	0.002
	(0.034)	(0.031)
N	1,061	1,061
Contraception at 16: condom	0.007	0.034
	(0.050)	(0.041)
N	1,061	1,061
Contraception at 16: safe period	0.017	0.061**
	(0.032)	(0.029)
N	1,061	1,061
Contraception at 16: pill	0.022	0.105**
	(0.051)	(0.042)
N	1,061	1,061
Contraception at 16: jelly cream	-0.018****	-0.011
	(0.004)	(0.008)
N	1,061	1,061
Contraception at 16: other method	0.033	0.019
	(0.023)	(0.016)
N	1,061	1,061
Contraception at 16: abstinence	-0.011	0.011
	(0.049)	(0.041)
N	1,061	1,061
Contraception at 16: trusting luck	0.008	0.023
	(0.029)	(0.025)
N	1,061	1,061
Contraception at 16: none of listed	-0.003	0.010
	(0.024)	(0.021)
N	1,061	1,061
Contraception at 16: don't know	0.001	-0.013
	(0.018)	(0.012)
N	1,061	1,061

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. The table shows estimates of an OLS regression of each outcome (specified in the rows) on the two instruments. Robust standard errors in parentheses. N refers to the number of observations. *Source*: BCS70, own calculations.

Supplementary Table S4: Comparison of IVs

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Only contraceptive failure			
Age at the first live birth	0.055	0.018	-0.058**
	(0.097)	(0.017)	(0.027)
KP F-statistic	60.878	60.878	46.977
Only fertility shocks			
Age at the first live birth	0.093	0.011	0.056
	(0.241)	(0.042)	(0.071)
KP F-statistic	10.137	10.137	7.384
Both instruments			
Age at the first live birth	0.059	0.017	-0.047*
	(0.092)	(0.016)	(0.025)
KP F-statistic	34.254	34.254	26.280

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Standard errors are shown in parentheses. The table shows 2sls regression results for the effect of the age at first birth on earnings using different instruments. All regressions include the same full set of control variables as shown in in Table 2 (see also Supplementary Table S7). The KP F-statistic is the Kleibergen-Paap F-statistic on the strength of the excluded instrument. *Source:* BCS70, own calculations.

Supplementary Table S5: Sample characteristics of women with and without non-zero earnings at midlife

	Net earnings=0	Net earnings>0	t-statistic
Attainment of a tertiary degree	0.232	0.272	-1.67*
Number of years in paid work	17.16	24.25	-15.27****
Number of children	2.58	2.27	5.48****
Household income Above median	0.382	0.565	-7.08****
Household income: Bottom quintile	0.338	0.151	7.95****
Household income: 2nd quintile	0.215	0.189	1.21
Household income: 2nd quintile	0.145	0.237	-4.65****
Household income: 4th quintile	0.121	0.223	-5.47****
Household income: Top quintile	0.197	0.213	-0.73
Mother's age at first birth	21.53	21.78	-1.18
Mother's age at completion of education	15.59	15.65	-0.65
Woman was conceived out of wedlock	0.181	0.145	1.81*
Parents' marital status at woman's birth	0.889	0.937	-3.06***
Parents' occ. class at woman's birth: V unskilled	0.068	0.056	0.96
Parents' occ. class at woman's birth: IV partly skilled	0.179	0.165	0.68
Parents' occ. class at woman's birth: III manual	0.455	0.474	-0.72
Parents' occ. class at woman's birth: III non-manual	0.135	0.140	-0.27
Parents' occ. class at woman's birth: II managerial and technical or I professional	0.163	0.165	-0.12

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1, p<1. The table shows the average values of the covariates for women with and without earnings at midlife. Column 3 show the results from a t-test for the equality of means with unequal variances. Region of residence at woman's birth consisting of ten categories is not shown in the table.
Source: BCS70, own calculations.

Supplementary Table S6: Full estimates for the OLS regression

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Age at first birth (AFB)[1]	0.075****	0.013****	0.007
	(0.016)	(0.003)	(0.005)
Mother's age at first birth	0.019	0.003	0.003
	(0.017)	(0.003)	(0.005)
Parents' occ. class at woman's birth			
V unskilled (ref.)			
IV partly-skilled	0.184	0.022	0.086
	(0.265)	(0.048)	(0.071)
III manual	0.266	0.039	0.086
	(0.243)	(0.044)	(0.062)
III non-manual	0.344	0.044	0.154**
	(0.280)	(0.050)	(0.076)
II managerial and Technical or I professional	0.152	0.013	0.119
	(0.279)	(0.050)	(0.076)
Mother's age at completion of education	0.054	0.003	0.049****
	(0.036)	(0.006)	(0.011)
Region of residence at woman's birth			
North (ref.)			
Yorks and Humberside	0.018	0.019	-0.107
	(0.268)	(0.047)	(0.083)
East Midlands	0.200	0.043	-0.051
	(0.285)	(0.050)	(0.079)
East Anglia	-0.426	-0.053	-0.182*
	(0.367)	(0.065)	(0.107)
South East	-0.230	-0.037	-0.021
	(0.231)	(0.040)	(0.070)
South West	-0.366	-0.055	-0.082
	(0.279)	(0.049)	(0.085)
West Midlands	0.184	0.038	-0.031
	(0.264)	(0.046)	(0.077)
North West	-0.042	-0.007	-0.008
	(0.252)	(0.044)	(0.073)
Wales	-0.288	-0.037	-0.124
	(0.304)	(0.054)	(0.112)
Scotland	0.066	0.009	0.031
	(0.276)	(0.048)	(0.081)
Northern Ireland	2.559****	0.351****	0.642
	(0.447)	(0.059)	(0.395)
Woman was conceived out of wedlock			
No (ref.)			
Yes	0.030	-0.009	0.109**
	(0.181)	(0.032)	(0.056)
Parent's marital status at woman's birth			
Single, widowed, divorced or separated (ref.)			
Married	0.565**	0.084*	0.136
	(0.258)	(0.046)	(0.093)
Survey at which the most recent earnings data was collected			
Age 46 (ref.)			
Age 42	-0.551****	-0.065**	-0.299****
	(0.147)	(0.027)	(0.049)
Intercept	0.315	0.199	4.411****
	(0.786)	(0.139)	(0.245)
Number of observations	2,160	2,160	1,531

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Robust standard errors are shown in parentheses. The table shows estimates of an OLS regression of earnings on age at first birth. The outcome variables are defined as: log of total net earnings from main job + 1 (column 1), a binary indicator for women reporting non-zero net earnings from their main job (column 2), and the log of total net earnings from main job conditional on reporting non-zero earnings. [1]First live birth. *Source*: BCS70, own calculations.

Supplementary Table S7: Full estimates for the IV regression

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Age at first birth (AFB)[1]	0.055	0.018	-0.058**
	(0.097)	(0.017)	(0.027)
Mother's age at first birth	0.022	0.002	0.015**
	(0.023)	(0.004)	(0.007)
Parents' occ. class at woman's birth			
V unskilled (ref.)			
IV partly-skilled	0.190	0.021	0.084
	(0.267)	(0.048)	(0.079)
III manual	0.292	0.032	0.151**
	(0.270)	(0.048)	(0.072)
III non-manual	0.379	0.035	0.248***
	(0.325)	(0.058)	(0.089)
II managerial and Technical or I professional	0.193	0.003	0.228**
	(0.336)	(0.060)	(0.094)
Mother's age at completion of education	0.057	0.002	0.059****
	(0.040)	(0.007)	(0.011)
Region of residence at woman's birth			
North (ref.)			
Yorks and Humberside	0.004	0.022	-0.161*
	(0.275)	(0.048)	(0.090)
East Midlands	0.186	0.046	-0.127
	(0.292)	(0.051)	(0.088)
East Anglia	-0.433	-0.051	-0.248**
	(0.366)	(0.066)	(0.114)
South East	-0.237	-0.035	-0.051
	(0.233)	(0.041)	(0.072)
South West	-0.367	-0.055	-0.082
	(0.278)	(0.049)	(0.088)
West Midlands	0.163	0.044	-0.103
	(0.284)	(0.050)	(0.087)
North West	-0.059	-0.003	-0.066
	(0.263)	(0.046)	(0.078)
Wales	-0.310	-0.032	-0.198*
	(0.320)	(0.056)	(0.119)
Scotland	0.051	0.013	-0.002
	(0.287)	(0.050)	(0.084)
Northern Ireland	2.593****	0.342****	0.706*
	(0.468)	(0.065)	(0.386)
Woman was conceived out of wedlock			
No (ref.)			
Yes	0.026	-0.008	0.110*
	(0.181)	(0.032)	(0.060)
Parent's marital status at woman's birth			
Single, widowed, divorced or separated (ref.)			
Married	0.577**	0.081*	0.165
	(0.264)	(0.047)	(0.103)
Survey at which the most recent earnings data was collected			
Age 46 (ref.)			
Age 42	-0.556****	-0.063**	-0.335****
	(0.149)	(0.027)	(0.053)
Intercept	0.654	0.115	5.541****
	(1.767)	(0.310)	(0.515)
KP F-statistic	60.878	60.878	46.977
Number of observations	2,160	2,160	1,531

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Robust standard errors are shown in parentheses. The table shows estimates of a 2SLS regression of earnings on age at first birth using contraceptive failure as an instrumental variable. The outcome variables are defined as: log of total net earnings from main job + 1 (column 1), a binary indicator for women reporting non-zero net earnings from their main job (column 2), and the log of total net earnings from main job conditional on reporting non-zero earnings. [1]First live birth. *Source*: BCS70, own calculations.

Supplementary Material S8: Mediation analysis

A mediation analysis was performed to explore the role of potential mediators in the effect of the timing of entry into motherhood on earnings. This analysis aimed to decompose the total effect of the timing on earnings (as estimated using the models described above) into an indirect effect and a direct effect. The indirect effect measured the change in earnings that could be traced back to changes in education (tertiary-level degree), years in paid work, working hours (full-time vs. part-time), family size, family status, and birth spacing caused by the timing of entry into motherhood. The direct effect then measured the effect of the timing on earnings net of any changes in these mediators. For the analysis, we followed an approach suggested by VanderWeele and Vansteelandt (2014). Their approach allows for the evaluation of the joint impact of more than one mediator. First, we estimated a regression model for earnings while controlling for all mediators. For example, in the OLS model we would estimate the equation:

$$Y_i = \beta_0 + A_i\beta_1 + \mathrm{X_i}\beta_2 + \sum_{j=1}^{8} \mathrm{M_{ji}}\upgamma_\mathrm{j} + + \varepsilon_i$$

where M_{ji}, j=1,...,8 are the eight potential mediators considered here: number of years in paid work, employed full-time, employed part-time, having a tertiary-level degree, having two or more children, the distance between the birth dates of the first and second child, living with the father of the first child, and living with a cohabiting partner. In this regression, β_1 captures the direct effect of the timing of entry into motherhood on earnings (i.e., the effect net of any changes in the three mediating variables). The indirect effect depends on both the effect of the mediators on earnings (captured by γ_j) as well as the effect of the timing on these mediators. In a second step, we therefore estimate a separate regression model of each mediator *j* on age at first birth:

$$M_{ji} = \delta_{\mathrm{j0}} + A_i\delta_{\mathrm{j1}} + \mathrm{X_i}\delta_{\mathrm{j2}} + \nu_{ji}$$

Here, δ_{j1} captures the effect of the timing of entry into motherhood on mediator *j* (e.g., education). The indirect effect explained by all mediators jointly is then given by $IDE = \sum_{j=1}^{8} \gamma_j \delta_{j1}$. Conceptually, this means that we multiplied the effect of age at first birth on, e.g., education with the effect of education on earnings to measure how large the effect of the timing of entry into motherhood on earnings operating through changes in education is. The total effect of the timing of first birth on earnings is given as $TE = DE + IDE$. Standard errors around these estimates are derived using 1000 bootstrap replications.

Mediation analysis requires the assumption of sequential ignorability (Imai, Keele, and Tingley 2010) – essentially we assumed that the mediators were exogenous, conditional on the age at first birth. This is a very strong assumption that is unlikely to hold. For example, unobserved factors that are related to both earnings and one of the mediators would violate this assumption. Therefore, the results shown below should be interpreted as suggestive evidence only. In addition, the chosen approach to mediation analysis allowed us only to measure the impact of all mediators jointly. Quantifying the impact of an individual mediator required much stronger assumptions, in particular a clear sequencing of the *j* mediators, and we therefore decided against these additional analyses.

The results of the mediation analysis are shown in Tables S8.1 and S8.2. A decomposition of the total effects of AFB on earnings into direct effects (not explained by the mediators) and indirect effects (explained through changes in the mediators) suggests that the total effects in the OLS regressions effects on earnings are almost completely explained through changes in the mediators.

Supplementary Table S8.1: Mediation effects - OLS

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Direct effect	0.003	0.001	-0.001
	(0.015)	(0.003)	(0.005)
Indirect effect	0.057****	0.010****	0.012***
	(0.015)	(0.003)	(0.004)
Total effect	0.061***	0.011***	0.012**
	(0.020)	(0.003)	(0.006)
Number of observations	1,599	1,599	1,599

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Bootstrapped standard errors are shown in parentheses. All regressions are estimated including same control variables as in Table 2 (see also Supplementary Table S7). *Source*: BCS70, own calculations.

Supplementary Table S8.2: Mediation effects - IV

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Direct effect	0.054	0.007	0.009
	(0.114)	(0.020)	(0.032)
Indirect effect	0.016	0.008	-0.023
	(0.089)	(0.015)	(0.021)
Total effect	0.070	0.016	-0.014
	(0.119)	(0.021)	(0.030)
Number of observations	1,599	1,599	1,599

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Bootstrapped standard errors are shown in parentheses. All regressions are estimated including same control variables as in Table 2 (see also Supplementary Table S7). *Source*: BCS70, own calculations.

Supplementary Table S9: Robustness checks

	ln(Net earnings)	P(Net earnings>0)	ln(Net earnings>0)
Baseline IV	0.055	0.018	-0.058**
	(0.097)	(0.017)	(0.027)
Gross earnings	0.035	0.016	-0.072**
	(0.102)	(0.017)	(0.029)
Incl. self-employed income	0.015	0.014	-0.084***
	(0.083)	(0.014)	(0.031)
Controlling for tertiary-level degree	0.051	0.017	-0.055**
	(0.110)	(0.019)	(0.028)
Controlling for age of youngest child	0.072	0.023	-0.073**
	(0.132)	(0.023)	(0.037)
Excluding women without work experience before birth	0.101	0.024	-0.048*
	(0.104)	(0.018)	(0.028)
Excluding women who gave birth before age 20	0.015	0.018	-0.115**
	(0.166)	(0.029)	(0.045)
IPW-adjusted IV estimates	0.065	0.018	-0.048*
	(0.100)	(0.018)	(0.029)

Notes: **** p<.001, *** p<.01, ** p<.05, * p<.1. Standard errors are shown in parentheses. The table shows the estimated effects for the age at first (live) birth from several different specifications. All regressions are estimated using our preferred IV specification as in Table 2 (see also Table S7). Full regression results are available upon request. Row 1 replicates our baseline specification from Table 2. Row 2 uses gross earnings from the main job to define the earnings outcomes. Row 3 uses net earnings from the main job as well as net earnings from self-employment to define the outcome variables. In Row 4, we additionally control for education (i.e. whether the woman had attained a tertiary-level degree. In Row 5, we additionally control for the age of the youngest child in the household. For women with missing values on this variable (e.g., women without children in the household), we set the age of the youngest child to zero and include a separate binary indicator for observations with missing values. Row 6 excludes women who gave birth before age 20. Row 7 excludes women if they never worked or if their first spell of work (employment or self-employment) began in the year following the birth of their first child. Row 8 reports estimates using inverse probability weights to correct for selection into the sample of women who gave birth before age 30. Weights are constructed based on a probit regression using a binary indicator for giving birth before age 30 as the outcome, and use the same set of covariates as in Table S7 as controls. *Source:* BCS70, own calculations.

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[9] References to the studies listed in Supplementary Table S1 can be found in the reference list of the article.