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ABSTRACT

Understanding the Effects of Early Motherhood in Britain: The Effects on Mothers*

This paper examines the socio-economic consequences of teenage motherhood for a cohort of British women born in 1970. We employ a number of methods to control for observed and unobserved differences between women who gave birth as a teenager and those who do not. We present results from conventional linear regression models, a propensity score matching estimator, and an instrumental variable estimator that uses miscarriage data to control for unobserved characteristics influencing selection into teenage motherhood. We consider the effects on equivalised family income at age 30, and its constituent parts. We find significant negative effects of teenage motherhood using methods that control only for observed characteristics using linear regression or matching methods. However once unobserved heterogeneity is also taken into account, the evidence for large negative effects becomes much less clear-cut. We look at older and younger teenage mothers separately and find that the negative effects are not necessarily stronger for teenagers falling pregnant before age 18 compared with those falling pregnant between 18 and 20, which could further suggest that some of the negative effects of teenage motherhood are temporary.

JEL Classification: J31

Keywords: teenage pregnancy, miscarriage, instrumental variables

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1. Introduction

The USA's and Britain's worsening record on teenage pregnancies relative to other countries motivates a continued interest in estimating the long-term socio-economic consequences of teenage motherhood. UK teenage birth rates are the highest in Western Europe, although still less than half the rate in the USA. Britain is the only country in Western Europe which has not experienced a significant decline in teenage fertility rates in the last thirty years[1], and has this in common with the USA. This paper is concerned with estimating the effects of early motherhood for a cohort of British women born in 1970, and specifically with calculating how much of the well documented association of early motherhood and negative later-life economic and educational outcomes can be attributed to a causal effect.

The question of whether early motherhood is an indicator of prior disadvantage or a pathway to future disadvantage (or possibly both) is one that has been debated extensively in recent literature. This question has important policy implications - as regards the nature, timing and targeting of interventions to assist young mothers. It has also challenged researchers to find appropriate econometric techniques to distinguish between these two conflicting stories. Existing data and methodologies have lead to disparate evidence. Conventional estimates have indicated large negative socio-economic effects of early motherhood, and so support interventions aimed at reducing the incidence of teenage conceptions. More recent evidence that allows for a separate effect of prior disadvantage has indicated smaller, and in some cases even zero or positive effects, suggesting that the pathway to disadvantage started much earlier in the young woman's life and cannot (entirely) be attributed to early motherhood. If this is the case, policies which are aimed simply at preventing teenage conceptions or births will be less effective in ameliorating the negative outcomes of concern than the raw data would otherwise suggest.

We compare linear regression estimates with non-parametric propensity score matching estimates because of fears that the regression estimates may be sensitive to functional form and because there may be a lack of common support. Moreover, proponents of this method hope to reduce the extent of selection because the "treated" are closely matched to very similar people who did not receive the treatment. That is,

[1] See Social Exclusion Unit (1999).

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using a rich dataset the unobserved heterogeneity responsible for the selection on unobservables allows the researcher to eliminate the bias and estimate the effectf of the treatment on the treated. A recent example of matching estimates used in this context is provided by Levine and Painter (2003) who suggest that the estimated effects of teen motherhood on educational outcomes are approximately halved using this innovation.

Family fixed effects (siblings and cousins)[2] and instrumental variables techniques[3] have traditionally been used to address the problem of unobserved heterogeneity. In this paper, we follow Hotz, McElroy and Sanders (1999) and exploit data on miscarriages to form an instrumental variable that, under certain assumptions, can yield consistent estimates of the effects of early motherhood on those that experienced early motherhood - that is, the effect of the treatment on the treated. The approach is akin to a natural experiment, where the experience of miscarriage can be thought of as a treatment, exogenously delaying age at first birth. This effectively allows the construction of a counterfactual for the outcomes of teenage mothers, had they not given birth as a teenager. Attempts to use this method, such as the Hotz *et al* paper cited above, have been controversial because they have resulted in much smaller effects than traditional estimates. For example, they find that early motherhood tends to raise levels of labour supply, accumulated work experience and labour market earnings by the time a teen mother reaches her late twenties. The use of this method is also controversial because estimates based on this methodology are potentially biased for a number of reasons[4]. In this paper we are interested in whether this bias can account for all of the difference between the IV estimates and conventional estimates, and we apply methods similar to those used in Hotz, Mullins and Sanders (1997) to calculate a bound on the maximum amount of bias introduced by using miscarriages as an instrumental variable.

The contribution of this paper is threefold. As in the US, teen motherhood in the UK remains stubbornly high so the paper takes the opportunity to attempt to replicate, using UK data, the lack of causal effects of early motherhood that have been

[2] See for example Ribar (1999), Hoffman, Foster and Furstenberg Jr (1993) and Geronimus and Korenman (1992).

[3] See for example Klepinger, Lundberg and Plotnick (1998) and Chevalier and Viitanen (2002).

[4] These are discussed in Section 3.

found in the US using techniques that control for unobserved heterogeneity. We make use of the methodology in Hotz, Mullins and Sanders (1997) to explore further the extent to which the well-documented problems with using miscarriages as an instrumental variable can account for the vastly smaller estimates that obtain when using this method, compared with traditional estimates[5]. Second, we also apply propensity score matching that has been shown by Levine and Painter (2003) to suggest much smaller effects of teen motherhood on educational outcomes in the US than linear regression. Finally, we assess the possible pathways through which disadvantageous effects may occur, by breaking down the effect on family equivalised income of the mother at age 30 into its constituent parts – family composition (whether a partner is present and the number of other children), other household income (welfare income and partner's earnings), own labour supply (hours of work and participation), and wages (including the effect of education).

The paper proceeds as follows. In Section 2 we examine the various approaches that have been used to estimate the effects of early motherhood in the existing literature. Section 3 discusses the use of miscarriages as an instrumental variable. In section 4 we discuss the data and in section 5 we present the results of the econometric analyses. Finally, section 6 concludes.

2. Approaches and Findings in the Existing Literature

In the last decade a number of new studies have used a variety of innovative methods to control for unobserved characteristics influencing selection into teenage motherhood. Whereas earlier studies were based on linear models, controlling for observed characteristics only[6], the newer literature has treated this as an evaluation problem, with early motherhood analogous to a treatment that is to be evaluated. The various approaches have differed primarily in the control group that has been used to construct the counterfactual outcome for teen mothers.

These new approaches have generated a debate in the literature as to whether once these unobserved characteristics are controlled for, any negative effects of early

[5] The technical details are outlined in Kaplan and Windmeijer (2003). Recent work by Ermisch (2003) also applies this method to the UK used here.

[6] See for example Hofferth and Moore (1979) for the USA, and Hobcraft and Kiernan (1999) for the UK.

childbearing remain. However, drawing any robust conclusions from this debate has been difficult because of the sensitivity of the results to the empirical methodology chosen and the data set being used.[7]

One group of studies exploit family fixed-effects to compare the outcomes for teenage mothers with those of their sisters. Geronimus and Korenman (1992) used samples drawn from the National Longitudinal Survey of Young Women (NLSYW), National Longitudinal Survey of Youth (NLSY) and Panel Study of Income Dynamics (PSID) and found that fixed-effects estimates were smaller than conventional estimates. In the case of the NLSYW results, the effects were not statistically different from zero, implying that once family-level unobserved characteristics are controlled for, there remains little or no effect on subsequent socio-economic outcomes. However, Hoffman, Foster and Furstenberg Jr (1993b) noted that the NLSYW results are somewhat of an outlier, with the PSID and NSLY results indicating that, while substantially smaller than conventional estimates, the effects of early childbearing are still negative and significant, even in the fixed-effects models. This conclusion was supported by further analysis of the PSID data in Hoffman, Foster and Furstenberg Jr (1993a). One possible explanation for the surprising results in the NLSWY data is the older age at which outcomes are measured (28-31 compared with 21-33 in the PSID and NLSY data), suggesting that there could be a significant temporary effect of early motherhood, but that this effect disappears over time.

However, even if one were to believe the PSID and NLSY results, it is unlikely that family fixed-effects are able to appropriately control for unobserved characteristics influencing selection into teenage motherhood. Maintaining that these characteristics differ only at the family and not the individual level, so that sisters are identical in all unobserved aspects that would influence both the decision to give birth at a young age and later socioeconomic outcomes (such as career motivation) is perhaps an unrealistically strong assumption.

More recently, Ribar (1999) developed a simultaneous equation model for sisters' outcomes to calculate the effects of teenage motherhood under different assumptions about the correlation of siblings unobserved characteristics. Maintaining

[7] Hoffman (1998) provides a good synthesis of this debate

the assumption that is equivalent to a family fixed-effects model results in estimates for family income-to-needs ratio[8] and years of education from the NLSY that are significantly negative, and comparable to those in Geronimus and Korenman (1992). However, estimates of effects for family income are not statistically different from zero. Under a different set of assumptions, which are equivalent to allowing each sister's fertility to instrument for the other's childbearing behaviour, he finds implausibly large, negative effects of early childbearing[9].

A different form of fixed-effects analysis is explored in Brien, Loya and Pepper (2002) who control for individual unobserved heterogeneity by looking at changes in mothers' cognitive development over time. Because the authors observe two test scores before a teenager gives birth and one test score after, they are able to control for unobserved factors that influence the level and growth of test scores. Their differences-in-differences analysis indicates that while teenage mothers have lower test scores than teenagers who did not give birth, the direct effects of giving birth on test scores are negligible.

A particularly innovative idea implemented by Bronars and Groggar (1994) was to exploit the random nature of giving birth to twins, conditional on becoming pregnant, to create a natural experiment. The idea rests on the assumption that the effect of giving birth to twins as a teenager on later socioeconomic outcomes is twice that of giving birth to a singleton as a teenager. If this is the case then one can compare outcomes for teenagers who gave birth to twins with outcomes for teenagers who bore singletons to get consistent estimates of the effects of teenage motherhood. The assumed randomness of giving birth to twins accounts for unobserved heterogeneity. They find that there are substantial effects on the short-run labour force participation for all teenage mothers, but lasting effects on the probability of eventual marriage and family earnings only for blacks. However it is unlikely that the necessary assumption for identification holds. Rather, it is probably the case that if effects of teenage motherhood exist, most of the effect is captured by the presence of

[8] The income-to-needs ratio is income divided by the poverty level for the woman's reported family size.

[9] One possible explanation for the unusual IV results is that sisters' fertilities are not strongly correlated, so effectively this is a weak instrument problem.

any children (compared to none), so that the effect on teenagers bearing twins is likely to be less than twice that for teenagers bearing singletons.

Other researchers have searched for appropriate instrumental variables that can explain teenage fertility but are not related to unobserved characteristics that influence later socio-economic outcomes. The most commonly used instruments have been age at menarche, and regional indicators of sexual awareness and access to contraception. For example, Chevalier and Viitanen (2003) use age of menarche as an instrument, whilst Klepinger, Lundberg and Plotnick (1998) used menarche and state/county level information. Studies which use of age of menarche as an instrument for uncovering the effects of teenage motherhood need to be carefully interpreted however, since although age at menarche may exogenously alter the timing of pregnancy, it seems unlikely that it would affect whether or not a young woman gives birth, conditional on becoming pregnant. It is this latter which is required to uncover the effects of early motherhood on later life outcomes.

Finally, a controversial, but potentially helpful methodology has been to exploit the random nature of miscarriages as a mechanism for exogenously delaying age at first birth. This methodology, and the consequences of violations of the assumptions underlying this technique, are discussed in detail in the following section.

Britain and the USA have acute problems with teenage pregnancy[10], and while the studies cited above examine the USA, there is little British evidence on which to base policy prescriptions. The existence of full, retrospective pregnancy histories in the 30 year old sweep of the British Cohort Study (BCS) makes it possible to apply some of the aforementioned techniques to examine the pattern of results for a newer cohort than has previously been analysed in Britain. To our knowledge, Chevalier and Viitanen (2003) is the first UK example and uses age at menarche as an instrument to control for unobserved heterogeneity in an earlier cohort of children born in 1958 - the NCDS. A further analysis is very recent work by Ermisch (2003) which uses the same BCS dataset and the same instrument as we use here. We complement that work by using a propensity score matching method and we consider in more detail the disaggregation of the outcome on family equivalised income into its constitutent parts. Finally, Robinson (2002) constructs synthetic cohorts from cross-section surveys

[10] See Social Exclusion Unit (1999)

pooled over time to estimate the lifecycle evolution of the wage penalty associated with teen motherhood. Her results show that the wage gap between teen mothers and others is largest in the late 20's and early 30's and closes only slowly thereafter[11]. She further shows that the wage penalty appears to be *larger* for recent cohorts. Our data corresponds to this age where the wage difference is at its maximum.

3. Miscarriages as an Instrumental Variable, and Propensity Score Matching

The idea of exploiting miscarriages as a natural experiment to estimate the effects of teenage childbearing was first attempted by Hotz, McElroy and Sanders (1999). The idea is that, if miscarriages occur randomly and are reported correctly, then they represent situations where age at first birth has been exogenously delayed. By comparing outcomes for young women whose first pregnancy ended in a miscarriage with those who gave birth, it is possible to control for all unobserved factors that simultaneously influence the decision to become pregnant as a teenager, the decision to not terminate the pregnancy and the outcome being considered.

However, this methodology has been criticized on various grounds. Importantly, most of the problems with using miscarriages tend, under plausible assumptions, to induce an upwards bias in the estimates, towards zero[12]. This means that it is unclear whether the small effects estimated in Hotz, Mullins and Sanders (1997) and Hotz, McElroy and Sanders (1999) are indicating downward bias in conventional estimates or are being driven by the upward biases inherent in the miscarriage method. It is hence useful to specify the conditions required for miscarriages to provide consistent estimates of the true effects, so that we can get a firm grasp on whether violation of these conditions can explain the discrepancy in results.

[11] While her paper does not address causality, it does examine the results for sensitivity to the inclusion of parental class and country of origin and finds the results to be insensitive to the inclusion of these pre-existing conditions. However this does not, of course, preclude sensitivity to other possible controls or for selection on unobservables.

[12] The socio-economic outcomes being considered are all defined such that a more negative co-efficient represents a stronger negative effect of early motherhood. Hence, when we use the term 'upward bias', we refer to an under-estimate of the effect, whilst a 'downward bias' refers to an over-estimate of the negative effect.

Condition 1 The occurrence of a miscarriage for a pregnant teenager is random with respect to any existing unobserved characteristics that are correlated with the outcome of interest.

Condition 2 All pregnancies and their outcomes are reported correctly.

Condition 3 The occurrence of a miscarriage has no independent effect on the outcome of interest

Numerous researchers have observed that Condition 1 may not be satisfied. For example, there is some evidence that drinking and smoking while pregnant may increase the probability of a young woman experiencing a miscarriage. If the decision to smoke and/or drink while pregnant is correlated with other unobserved factors that impact on future socio-economic outcomes, then this will lead to biased and inconsistent estimates. Another potential source of non-randomness is domestic abuse that results in a miscarriage.

However, the epidemiological literature seems to indicate that the vast majority of miscarriages are random, particularly with respect to future socio-economic outcomes. Regan (2001) notes that approximately 50% of miscarriages are due to foetal chromosomal abnormalities[13] and the remainder are largely due to neural tube defects, viral and bacterial infections in the mother and other foetal genetic defects. All of these causes can be considered as random with respect to *future* socio-economic outcomes, conditional on observed characteristics. Moreover, Regan (2001) also notes that the remaining non-random causes of miscarriages are primarily pre-existing complicating factors, such as diabetes, the occurrence of which one would not expect to be correlated with economic and educational outcomes, after controlling for other background factors.

Hotz, Mullins and Sanders (1997) are able to calculate bounds for the true causal effect of early motherhood, accounting for the extent of violations of Condition 1. For most of their samples and outcomes, they are unable to reject conventional point estimates of the effects, based on the bounds. Two different figures were used for the proportion of miscarriages that occur randomly - an extremely conservative estimate of 38%, and a more realistic estimate of 84%, although the conclusions are

[13] Including monosomies (15%), polyploidies (10%) and trisomies (25%).

not overly sensitive to the estimate used. In this paper, we use a variant on this method to account for violation of Condition 1, and as we will show, to similar effect.

It is important to note that for violation of Condition 1 to induce upward bias in the estimates it is necessary that the correlation between unobserved characteristics and a miscarriage being non-random is negative. In other words, those teenagers experiencing a non-random miscarriage must realise worse outcomes than the teenagers whose miscarriages are random.

Condition 2 may be violated in a number of ways. We consider the two most likely possibilities. First, young women may be reluctant to report an abortion that they may have had up to 15 years ago. There is thus the possibility that whilst the teenage pregnancy is correctly reported, the outcome of the pregnancy is misclassified as a miscarriage. This type of misreporting will lead to an understatement of the effects of early motherhood if those women who report abortions as miscarriages are more disadvantaged than the general population of teenage pregnancies. In section 4, we compare the numbers of reported pregnancies, miscarriages and abortions with those from national statistics. In section 5 we present bounds on our IV results, based on the differences between official statistics and our data.

A second type of misreporting is non-reporting of pregnancies. This is a problem in all studies that use retrospective pregnancy history information such as we use here. If the sample of pregnant teenagers who report their pregnancies is not representative of the total population of pregnant teenagers, then this may affect estimates of the effect of early motherhood. In particular, if females who became pregnant as a teenager and experienced a miscarriage but did not report the pregnancy went on to achieve better outcomes on average than teenagers who did report the miscarriage, then this will induce upwards bias in the IV estimates. Once again, under relatively weak assumptions, we are able to bound the effect of this type of misreporting. These bounds are set out in section 5 (the methodology to derive them is described in Kaplan and Windmeijer (2003)).

Finally, violation of condition 3 may also affect our results. This condition is equivalent to the absence of a placebo effect in a controlled laboratory experiment. It states that the only way in which a miscarriage can affect the outcome under consideration is by preventing a birth (and the effects associated with a birth) from having occurred. However, the experience of a miscarriage for a pregnant teenager may be accompanied by feelings of either elation or depression. It is conceivable that the loss of a wanted child could have important lasting effects on the young woman, while it is also possible that the loss of a pregnancy that was likely to be terminated by abortion has a positive impact on the teenager. The question we must ask is whether we think that these effects are important and long-lasting enough to explain differences in socio-economic outcomes ten to fifteen years on.

As well as using miscarriages as an instrumental variable to find the impact of teenage motherhood, we also present results from a propensity score matching estimator. This technique is quite different from the instrumental variables estimator, since it does not allow us to control for all unobserved factors that simultaneously influence the decision to become pregnant as a teenager, the decision to not terminate the pregnancy, and the outcome being considered. Instead, it measures the impact of early motherhood on the assumption that there are no unobserved factors determining selection into early motherhood that also determine later life outcomes. In this respect it is similar to estimates derived using linear regression (also presented here), however it does not require the researcher to specify any particular functional form for the relation between early motherhood and later life outcomes, and this makes the specification completely flexible.

4. Data

Our data comes from the British Cohort Study (BCS), a longitudinal study of a cohort of approximately 17,000 children born in Britain in the week 5-11 April 1970. Surviving members of the cohort have been followed up at ages 5, 10, 16 and 26, and most recently at age 29/30 in 1999/2000. The starting point for our sample is those females who responded to a questionnaire about their past fertility history as part of the age 30 interviews. This provides us with a sample of 5771 females.

We use two definitions of 'teenager' in all of our analysis – those aged up to (but not including) 18 years, and up to 20 years. Ideally, we would like to classify females based on age at first conception. Unfortunately, date of conception is not available in the BCS data. Instead, we classify based on age at the outcome of the first pregnancy.[14] Although the 18 year definition of a teenager may be considered preferable on theoretical grounds - it more closely reflects the time at which a pregnancy is likely to trigger the mechanisms implicated in worsening later life socio-economic outcomes - we focus on the 20 year definition because it provides larger sample sizes, allowing more robust inference. Moreover, this is the definition that has been more commonly adopted in the existing US literature.[15] Where a female became pregnant only once before the relevant cut-off age, we classify the outcome as either a birth, abortion (induced abortion) or miscarriage (spontaneous abortion) [16] [17] .

Previous studies that have exploited miscarriages to estimate the effects of early motherhood have been criticized for their treatment of females experiencing multiple pregnancies as teenagers.[18] The criticism centres on the fact that in these studies, many of the females in the miscarriage sample experienced additional pregnancies as a teenager which ended in either abortions or live births. Table 1 shows the number of teenagers who have had zero, one, two, three and four pregnancies before each cut-off age. No teenagers had more than four pregnancies by age 20 in our sample. Furthermore, many females who experienced a miscarriage as a teenager also experienced an abortion or gave birth before the relevant cut-off age. Table 2 shows the number of females in each of these categories. Including females in the miscarriage sample who also gave birth or had an abortion as a teenager would have a similar effect to contaminating the control group with the treated group in an experimental design, biasing the IV estimates. Moreover, by looking at the outcome

[14] We could choose to impute dates of conception based on the outcome of the pregnancy and the date of the outcome. While this would give us a slightly larger sample of teenagers who became pregnant, it is unlikely that this would significantly affect our results. It also should be noted that aborted and miscarried pregnancies predate births by about 6 months. For this reason our age-cut-offs mean that we could very slightly undercount teenage abortions and miscarriages relative to the number of pregnancies.

[15] For example Ribar (1999).

[16] For the purposes of this paper we refer to induced abortions as "abortions" and spontaneous abortions as "miscarriages".

[17] The BCS data draws a distinction between pregnancies ending in miscarriage and those ending with a stillbirth. There is an argument for reclassifying stillbirths as miscarriages because stillbirths represent situations in which age at first birth has been exogenously delayed, however we exclude observations where the female had a stillbirth but no live birth or abortion by the age cut-off. This is done because Condition 3, discussed in Section 3, is much less likely to hold for stillbirths than for miscarriages. Only 3 females fall into this category and inclusion of these observations in the miscarriage sample does not significantly affect the results.

[18] For example, Hoffman (1998) makes this criticism about Hotz, Mullins and Sanders (1997).

of the other pregnancies we can learn something about the teenager's latent pregnancy resolution decision, had the pregnancy not ended in a miscarriage. In other words, we have information as to whether the teenager would have chosen to abort the pregnancy. In cases where the teenager had a latent abortion preference, we can no longer claim that the miscarriage served to exogenously delay age at first birth.

To overcome this problem, we define our non-pregnant, pregnant, birth, abortion and miscarriage samples as follows:

Non-pregnant Sample	Females who did not report any pregnancy prior to the relevant cut-off age.
Pregnant Sample	Females who reported at least one pregnancy prior to the relevant cut-off age.
Birth Sample	Females who had at least one birth prior to the relevant cut-off age.
Abortion Sample	Females who had at least one abortion and no births prior to the relevant cut-off age.
Miscarriage Sample	Females who had at least one miscarriage and no births or abortions prior to the relevant cut-off age.

Adopting these sample definitions has the effect of ensuring that the birth, abortion and miscarriage samples are mutually exclusive and together comprise the pregnant sample. Figure 1 shows the number of females in each of the samples for the two definitions of teenagers. Although the number of miscarriages is smaller than one would like for statistical purposes, the samples sizes are broadly consistent with those in Hotz, Mullins and Sanders (1997).

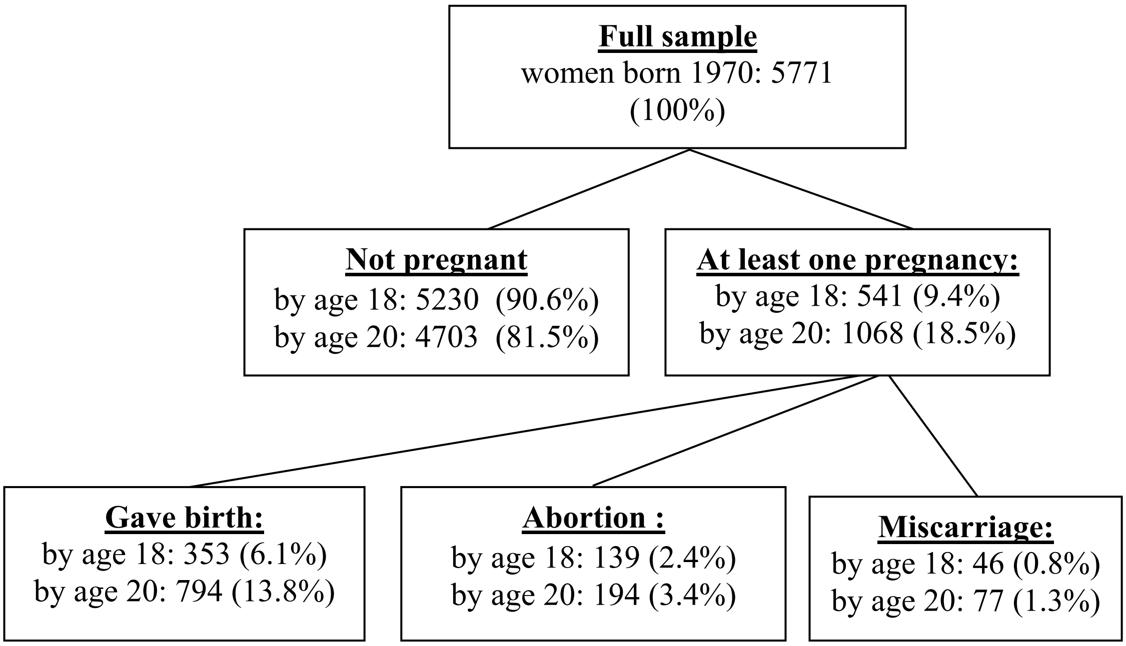
Table 1 Distribution of Number of Pregnancies

Number of Pregnancies	**by age 18**		**by age 20**	
	Number	**Percentage**	**Number**	**Percentage**
0	5230	90.6	4703	81.5
1	469	8.1	786	13.6
2	65	1.1	239	4.1
3	6	0.1	39	0.7
4	1	0.0	4	0.1
Total	5771	100	5771	100

Table 2 Other Pregnancies for Teenagers who Miscarried

	Miscarriage by 18	**%**	**Miscarriage by 20**	**%**
Also gave birth	18	26.9	63	41.7
Also had abortion	2	3.0	6	4.0
Also gave birth and had abortion	1	1.5	4	2.6
Only had miscarriages	46	68.7	77	51.0
Total	67	100	151	100

Figure 1 *Sample sizes*



Note: the Pregnant sample includes 3 more women in total than the birth, miscarriage, and abortion samples combined because of the stillbirths discussed in footnote 17.

To give an indication of the possible extent of under-reporting and misreporting of pregnancies in the BCS data, Table 4 compares information from our data to the total number of conceptions, births, abortions and miscarriages per 1000 women aged 15-19 for the cohort born in 1970 based on official ONS population statistics (where available). It also shows – based on these figures - the proportion of all conceptions ending in birth, abortion or miscarriage. Official statistics are not available for miscarriage rates, however it is commonly accepted[19] that between 10% and 15% of clinically recognised pregnancies (births, abortions and miscarriages) end in miscarriage, with this proportion increasing with age. Thus a reasonable estimate for 15-19 year old females is somewhere in the vicinity of 10% to 12%.

There is clearly a substantial amount of under-reporting of pregnancies amongst the BCS sample, with around 75-80 pregnancies per 1000 women going unreported. As would be expected, a disproportionate amount of this under-reporting is among those pregnancies ending in abortions, with only 21% of pregnancies being reported as ending in abortion for the BCS sample, compared to 29-30% for the ONS statistics. Moreover, the fact that the BCS data show 12% of pregnancies ending in

[19] See Regan (1997)

Table 4 Fertility rates for 1970 Cohort, aged 15-19

	ONS per 1000	BCS70 Per 1000	ONS (%)	BCS70 (%)
Births	152	114	59-60	67
Abortions	74	36	29-30 [a]	21
Miscarriages	25-31	24	10-12[b]	12
	251-257	176	100	100

Notes: Birth rates refer to the number of registered live births in England and Wales. Abortion rates refer to the number of recorded abortions in England and Wales.
a: Abortion rates for the 1970 cohort are not available directly. The earliest year for which age-specific ONS abortion data is available is 1991, when the 1970 cohort would have been 21 years old. To calculate the abortion rates in this table, we use information on abortion rates of women from more recent cohorts, applying the percentage of conceptions (births and abortions) ending in abortion for each age 15-19, averaged over the years 1991-95, to the relevant birth rates for the 1970 cohort.
b: by assumption – see text above.

Sources: ONS Series FM1 no. 30 (revised) Table 10.1. and Table 12.2 and authors' calculations.

miscarriage, combined with the apparent higher proportion of pregnancies ending in births in the BCS sample (67% compared with 59%), suggest that indeed some abortions could be erroneously reported as miscarriages in the BCS data. These figures support both the notion that the unreported pregnancies are more likely to end in abortions or miscarriages than reported pregnancies, and the belief that some abortions are being misreported as miscarriages.

The outcomes we investigate cover a range of economic and educational outcomes, all measured at age 29 or 30. Our primary outcome of interest is the natural logarithm of equivalised family income[20], however, in order to more fully understand what is driving the effects of this broad outcome, we break it down into its component parts. First, we investigate the cohort members' family size as measured by the equivalence scale, as well as the cohort members' own labour market outcomes, including the natural logarithm of their hourly and weekly net wages[21] and their total hours worked. Next, we look at the natural logarithm of the cohort member's partner's weekly wages. We also examine two outcomes related to the dependency of the female on Government benefits - the logarithm of real benefits received per week and an indicator variable for whether the cohort member was in receipt of means-tested benefits. Finally, we are also interested in education outcomes. We present results for

[20] Equivalised family income comprises cohort member's real net weekly income, partner's real net weekly income, real benefits received per week and real net weekly income form other sources (interest payments etc), adjusted to take account of household composition and size.
[21] Where net wage data was missing, net wages were imputed from gross wages.

age left full-time education and whether or not the female continued in post-compulsory schooling. Results for the impact of early motherhood on these outcomes are presented in Section 5.

Table 5 displays summary statistics for each of these outcomes for the various samples defined above. To conserve space, summary statistics are only shown for the 20-year definition of a teenager. The descriptive results are qualitatively similar for the other age groups in that for all outcomes and age definitions, the birth sample has a substantially lower (“worse”) average outcomes than for the not-pregnant, abortion and miscarriage samples. All regressions we report control for a range of background characteristics. The controls included are: age mother and father left FT education; maths, reading and ability test scores at age 10; mother's age at birth; father's social class; banded family income at age 10 and age 16; and indicators at age 16 for whether the family had experienced financial hardship in the last year, and whether the girl's mother thinks sex education is important, whether her daughter will do A-levels, and whether her daughter will continue in full time education past age 18. The propensity score matching estimates we report are based on this same vector of observed characteristics. Table 6 displays descriptive statistics for the background variables for the various samples.

Three points are immediately clear from Table 6, emphasizing the selection problem that we are faced with when trying to estimate the causal effect of early motherhood. First, the birth sample comes from substantially more disadvantaged backgrounds on average than both the full sample and the not-pregnant sample. Those individuals who gave birth as a teenager have test scores at age 10 that are on average between 7.44 and 11.24 percentage points lower than teenagers who did not become pregnant. For each dimension, there is evidence that teenage birth is to some extent an indicator of prior disadvantage. Second, there is a remarkable similarity between the background characteristics for teenagers in the abortion sample and those in the not-pregnant sample. The income distributions at age 10 and 16, and the distribution of father's social class are almost identical for the two groups. This point, and the one noted above, provide a further warning against simply comparing the outcomes for teen mothers with non-teen mothers in order to assess the impact of teenage motherhood, even after conditioning on becoming pregnant as a teenager. Moreover, the vastly different background characteristics between the birth sample and the

Table 5 Summary statistics – Outcome variables, 20 year definition of teenager

Outcome	Full Sample	Not Pregnant	Pregnant	Birth	Abortion	MisCarriage
Family Income						
Log Equivalised	5.76	5.84	5.41	5.30	5.84	5.58
Family Income	(0.77)	(0.77)	(0.65)	(0.60)	(0.66)	(0.74)
	5515	4489	1026	768	181	74
Log Family	5.78	5.81	5.63	5.59	5.81	5.63
Income	(0.77)	(0.79)	(0.66)	(0.64)	(0.71)	(0.75)
	5515	4489	1026	768	181	74
McClements	1.06	1.00	1.29	1.38	1.00	1.10
Equivalence	(0.30)	(0.26)	(0.34)	(0.30)	(0.29)	(0.29)
Scale[a]	5771	4703	1068	794	194	77
Work						
In Work?	0.68	0.72	0.51	0.46	0.66	0.69
	(0.47)	(0.45)	(0.50)	(0.50)	(0.48)	(0.47)
	5771	4703	1068	794	194	77
Log Weekly	5.14	5.21	4.73	4.56	5.21	4.78
Wage	(0.79)	(0.77)	(0.80)	(0.73)	(0.74)	(0.93)
	3907	3360	547	365	128	53
Log Hourly	1.73	1.76	1.56	1.49	1.77	1.55
Wage	(0.41)	(0.41)	(0.42)	(0.39)	(0.40)	(0.54)
	3891	3348	543	361	128	53
Hours Worked	35.15	36.10	29.25	26.53	35.66	32.32
per Week	(12.97)	(12.56)	(13.85)	(12.92)	(13.97)	(14.35)
	3938	3389	549	366	129	53
Partner						
Partner	0.70	0.71	0.69	0.70	0.63	0.69
in Household?	(0.46)	(0.45)	(0.46)	(0.46)	(0.48)	(0.47)
	5771	4703	1068	794	194	77
Log Weekly	5.64	5.66	5.51	5.45	5.65	5.66
Wage	(0.65)	(0.64)	(0.71)	(0.73)	(0.68)	(0.59)
	3372	2810	562	406	110	44
Post-	0.57	0.60	0.48	0.44	0.60	0.55
Compulsory	(0.49)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)
Schooling?	5771	4703	1068	794	194	77
Benefit variables						
Log Weekly	3.69	3.49	4.17	4.25	3.77	3.79
Benefit	(1.02)	(0.97)	(0.99)	(0.98)	(0.96)	(0.92)
Income	3266	2329	937	772	109	54
On Means-	0.79	0.85	0.50	0.42	0.77	0.69
Tested	(0.41)	(0.35)	(0.50)	(0.49)	(0.42)	(0.47)
Benefits?	5757	4689	1068	794	194	77
Education						
Age Left	17.48	17.72	16.48	16.26	17.28	16.78
Full-Time	(2.26)	(2.35)	(1.43)	(1.09)	(2.20)	(1.51)
Education	5607	4552	1055	791	185	76
Post-	0.50	0.56	0.25	0.19	0.48	0.31
Compulsory	(0.50)	(0.50)	(0.43)	(0.39)	(0.50)	(0.47)
Schooling?	5771	4703	1068	794	194	77

Table 6: Summary Statistics – Background variable, 20 year definition of a teenager

Background variables	**Full**	**Not pregnant**	**Pregnant**	**Birth**	**Abortion**	**Miscarriage**
Age father left	16.00	16.12	15.45	15.29	16.07	15.55
FT education	(2.25)	(2.35)	(1.56)	(1.32)	(2.25)	(1.39)
	5208	4270	938	695	167	73
Age mother left	15.74	15.83	15.35	15.24	15.78	15.42
FT education	(1.65)	(1.73)	(1.12)	(0.93)	(1.58)	(1.30)
	5208	4273	935	692	167	73
Maths Score	61.77	63.08	56.10	54.69	61.15	58.08
Age 10	(16.17)	(15.91)	(16.05)	(16.07)	(15.91)	(14.02)
	4327	3513	814	601	144	66
Reading Score	63.28	64.87	56.42	53.63	65.21	61.84
Age 10	(19.44)	(18.94)	(20.05)	(19.69)	(19.16)	(18.96)
	4651	3773	878	646	160	69
Ability Score	52.98	54.06	48.31	46.62	53.94	51.34
Age 10	(13.34)	(13.13)	(13.26)	(12.95)	(13.04)	(12.96)
	4563	3703	860	634	153	70
Mother's Age	25.97	26.20	24.95	24.67	25.97	25.33
at birth	(5.35)	(5.24)	(5.74)	(5.75)	(5.12)	(6.78)
Father’s class:						
- I	6%	7%	3%	2%	7%	5%
- II	24%	26%	15%	12%	28%	16%
- III.manual	9%	10%	7%	6%	12%	5%
- III.nonmanual	44%	43%	51%	52%	43%	61%
- IV	12%	11%	17%	20%	7%	9%
- V	4%	3%	7%	8%	3%	4%
Income at 10:						
<£50pw	6%	5%	10%	12%	4%	10%
£50-£100	30%	27%	39%	43%	26%	31%
£100-£150	35%	35%	32%	31%	36%	34%
£150-£200	16%	17%	13%	10%	21%	16%
>£200pw	13%	15%	6%	4%	12%	9%
Income at 16:						
<£100pw	16%	12%	31%	35%	18%	19%
£100-£150	14%	14%	16%	17%	12%	21%
£150-£200	15%	15%	14%	15%	9%	17%
£200-£250	12%	13%	8%	8%	7%	7%
£250-£300	10%	10%	7%	6%	14%	7%
£300-£350	6%	6%	4%	4%	5%	2%
>£350pw	12%	13%	7%	5%	15%	7%

[a] Equivalence scales provide the means of adjusting a household's income for size and composition so that incomes can be sensibly compared across different households. Official income statistics use the McClements (1977) equivalence scale, in which an adult couple with no dependent children is taken as the benchmark with an equivalence scale of one. The equivalence scales for other types of households can be calculated by adding together the implied contributions of each household member . The scale used is: Head, 0.61; Partner/Spouse, 0.39; Other second adult, 0.46; Third adult, 0.42; Subsequent adults, 0.36; Each child aged 0-1, 0.09; Each child aged 2-4, 0.18; Each child aged 5-7, 0.21; Each child aged 8-10, 0.23; Each child aged 11-12, 0.25; Each child aged 13-15, 0.27; Each child aged 16-18, 0.36.

abortion and not-pregnant samples, suggest that simply controlling for these characteristics in a linear model may not be sufficient to identify effects for the birth sample. The problem of there existing only a narrow region of common support amongst these background characteristics suggest that a more flexible framework, such as propensity score matching, may be more appropriate. Accordingly, we present results for our matching estimator[22] alongside the OLS and IV results in Section 6.

Finally, Table 6 indicates that the characteristics of the miscarriage sample lie somewhere between the birth and not-pregnant samples, but closer to the birth sample. This is supportive of the idea that the miscarriage sample comprises a mixture of latent birth type women and latent abortion type women, with a higher proportion of the miscarriage having a latent-preference for birth.

5. Results

The results from our analysis cover five broad areas – family income, receipt of means-tested benefits, employment and wages, partnership, and education. The aim is to understand both how early motherhood affects the mother's socio-economic status and living standard at age 30 (captured by equivalised family income), and what the pathways between this and teen motherhood are.

Following the methodologies discussed above, for each outcome we present six sets of estimates. First, we show OLS estimates of the effects of early motherhood for the full sample of females and for the sample of those who became pregnant as a teenager (columns 1 and 2). These are the 'conventional' linear models that control for observed characteristics only.

Next we use propensity score matching to compare outcomes for teenage mothers with similar non-teenage mothers (columns 3 and 4). This also controls for observed characteristics only, but within a more flexible framework that does not restrict the effects of the control variables on the outcomes to be the same for the two groups. With propensity score matching we are also able to impose common support, by restricting the individuals to whom we compare teenage mothers to those with similar background characteristics. We use Barbara Sienesi's *psmatch2* routine and

[22] We use Barbara Sienesi's psmatch2 routine and present estimates from Kernel density matching with two bandwidths of 1% and 0.1% to examine the sensitivity of the results to this arbitrary choice.

present estimates from Kernel density matching with two bandwidths, one imposing common support within a propensity score bandwidth of 0.01, the other within a bandwidth of 0.001, to examine the sensitivity of the results to this arbitrary choice.

Next, we present the first set of estimates that control for unobserved heterogeneity (column 5). These are the IV estimates, β_{IV} using miscarriages as an instrument for teenage births. Finally, in column 6, we give estimates for a lower bound of β_{IV}, accounting for non-randomness and misreporting of miscarriages. The details of the derivation of this bound can be found in Kaplan and Windmeijer (2003). Results are shown for estimates of the bounds where the proportion of non-random miscarriages among the set of reported miscarriages (k_{NR}) is assumed to be 0.15. All regressions include the background variables discussed above as controls.

We also present results for three definitions of teenagers. In all five tables, panel A shows our baseline results using the 20-year definition of a teenager, while Panels B and C then split this group into those females whose first pregnancy was before age 18 and those whose first pregnancy was between 18 and 20 years old respectively.

5.1 The impact of teenage motherhood on family income at 30

We start by presenting the impact of teenage motherhood on family income at age 30. Our baseline measure of the overall economic welfare of the teenage mother at age 30 is net weekly family income, equivalised using the McClements equivalence scale to account for the number and ages of family members. We later go on to consider the possible pathways, through which the impact of teenage motherhood on equivalised family income may be operating.

Considering first Panel A – those who gave birth before the age of 20: in accordance with the existing literature, we see large negative effects of teenage motherhood on equivalised family income at 30 when the impact of teenage motherhood is derived using conventional OLS estimation. Compared to women from the same age group who did not become teenage mothers, those who gave birth as a teenager had on average 42 per cent lower family income, after controlling for the background characteristics set out in section 4. These effects are slightly smaller (38% compared with 42%) when we restrict estimation to the sample who reported a pregnancy whilst a teenager (the pregnant sample) only. The PSM estimates are only slightly smaller again, 34%-35%, indicating that a more flexible framework that controls for observed heterogeneity only does *not* reduce the estimated effect significantly.

However, as we pointed out in our introductory sections, unless we effectively take account of any unobserved heterogeneity between women who give birth as a teenager, and those who do not, the above estimates are likely to be biased. Our central IV results in column (5) suggest that unobserved heterogeneity may well be an important factor in driving the above findings. Becoming a teenage mother results in a considerably smaller cut - of around 16% - in family income at age 30 on this estimate, and this is not significantly different from zero at the 5% level[23].

But as we have also pointed out, these IV estimates are not without their potential problems. Once we calculate bounds on our IV estimate to take into account possible misreporting and non-randomness of miscarriages, we see that the OLS and PSM estimates lie within our lower IV bound. This indicates that problems with our instrument *could* account for the discrepancy between the conventional estimates and those based on IV.

On balance, therefore, we are unable to conclude that teenage motherhood does not have strongly negative effects on the family income of the mother at age 30. But depending upon how much faith we are willing to place in the use of miscarriage as an instrument, our IV results provide evidence that the impact may well not be as negative as the conventional estimates suggest.

These headline results do not tell us what the drivers of the effect might be. In an attempt to understand this, throughout the rest of the paper, we break down net equivalised family income into its constituent parts. Our aim is to uncover the tranmission mechanism that leads from teenage motherhood to lower living standards for teenage mothers in early-middle age (and their children).

Table 7 shows that the effects of teenage motherhood on *un*equivalised family income are significantly smaller than the effects on equivalised family income. Conventional OLS estimates show the effect of being a teenage mother is to reduce

[23] It should be noted, however, that the lack of significance of this estimate is due to the poor precision of our IV estimates, rather than a point estimate particularly close to zero.

family income, unadjusted for family composition, by around just 12 percent compared to all women of a similar age, and by around 9 per cent compared to all women who became pregnant as teenagers – though this latter estimate is not significantly different from zero. Again the IV estimates show that this negative effect can be eliminated altogether once unobserved heterogeneity is taken into account, although the bounds we have calculated also suggest that this estimated reduction in the effect could again be entirely due to problems with our instrument.

This relatively small effect on household income before it has been adjusted for household composition shows the importance of household composition - both the presence of a partner and the number and age of dependent children at the age of 30 - in explaining the apparent drop in living standards at age 30 associated with teenage motherhood. The third row of results in Table 7 shows this more clearly: teenage motherhood is associated with an increase in the equivalence scale – i.e. in the cost of attaining a given standard of living - of around 35 per cent on most estimates, and 24 per cent on the IV estimate[24].

It is important to realise that this increase in costs, and hence the negative impact on living standards at age 30, associated with teenage motherhood may in part be temporary, and may be simply be the result of bringing childbearing forward in time. This is because these equivalence scales (which are used for adjusting official household incomes statistics in the UK, see DWP, 2003) assume older children cost more than younger children. Since those who give birth as teenagers will have older children by age 30 than women who delay childbirth until their 20s or beyond, they will have lower living standards for any given income level at this age. This position could reverse in time, as the children of teenage mothers leave home, and the children of the older mothers become more costly. But this phenomenon does not entirely explain the difference in living standards between teenage mothers and non-teenage mothers, since alternative equivalence scales which weight all children equally, regardless of age are also estimated to be higher for teenage mothers[25].

[24] See Note a to Table 5.

[25] Results based on these alternative equivalence scales are available from the authors.

Before going on to consider the determinants of household income in more detail, it is also interesting to consider whether the effects of teenage motherhood on income at age 30 differ, according to the age at which the teenager became a mother. Panels B and C show the separate impact of becoming a teenage mother on family income when the first birth occurs before 18, and when first birth occurs at 18 or 19. The results in these panels, though showing a similar pattern to Panel A across estimation strategies, also suggest that the impact of being a teenage mother on family income is less detrimental the *younger* the age of the mother when she first gave birth. This is because the estimated effects of teenage motherhood at age 30 are almost uniformly larger for the 18-20 year old sample, who gave birth for the first time more recently, rather than for the under 18 sample, who gave birth for the first time at a younger age.

Table 7 – Impact of Teenage Motherhood on Family Income Variables

	(1) OLS	(2) OLS	(3) PSM bw = 0.01	(4) PSM bw=0.001	(5) IV	(6) IV – Bound 85% random
	Full Sample	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**
A: 20yr definition						
Log Equivalised	-0.419	-0.381	-0.367	-0.403	-0.160	-0.468
Family Income	(0.025)	(0.049)	(0.073)	(0.082)	(0.108)	(0.082)
Log Family	-0.117	-0.088	-0.051	-0.087	0.048	-0.239
Income	(0.027)	(0.053)	(0.080)	(0.089)	(0.115)	(0.092)
McClements	0.347	0.339	0.359	0.325	0.243	0.362
Equivalence Scale	(0.012)	(0.022)	(0.028)	(0.038)	(0.043)	(0.043)
B: 18yr definition						
Log Equivalised	-0.409	-0.297	-0.253	-0.252	-0.017	-0.345
Family Income	(0.033)	(0.054)	(0.078)	(0.125)	(0.150)	(0.114)
Log Family	-0.105	-0.035	-0.009	-0.015	0.143	-0.164
Income	(0.034)	(0.058)	(0.080)	(0.130)	(0.154)	(0.120)
McClements	0.356	0.298	0.262	0.252	0.174	0.336
Equivalence Scale	(0.018)	(0.032)	(0.048)	(0.069)	(0.086)	(0.080)
C: 18-20 definition						
Log Equivalised	-0.344	-0.444	-0.384	-0.363	-0.238	-0.532
Family Income	(0.032)	(0.072)	(0.107)	(0.170)	(0.133)	(0.117)
Log Family	-0.103	-0.189	-0.140	-0.040	-0.103	-0.375
Income	(0.035)	(0.080)	(0.122)	(0.176)	(0.139)	(0.128)
McClements	0.271	0.300	0.272	0.292	0.170	0.301
Equivalence Scale	(0.015)	(0.030)	(0.045)	(0.074)	(0.052)	(0.051)

Two hypotheses could explain this phenomenon. First, this could be taken as further evidence that any negative effects of teenage motherhood at age 30, are, at least in part temporary. If the effects of being a teenage mother were permanent and distinctive, we might expect to see larger effects for the under 18 sample in Panel B. Second, this phenomenon could also suggest that the youngest mothers are in general more protected by their families from the negative effects of early motherhood than those who give birth slightly later. These considerations suggest that further work is required in unravelling these two (possibly competing) hypotheses[26].

5.2 The impact of teenage motherhood on benefit receipt at 30

Another indicator of socio-economic well-being which early motherhood may impact upon is the likelihood of receiving means-tested benefits at age 30. This outcome is of course closely related to the family income variables we considered above, because of the means-test.

Table 8 shows the impact of teenage motherhood on two variables related to the receipt of state benefits, first the probability of the subject's family being on means tested benefits at age 30, and second the level of weekly benefit income (this latter including child benefit). Once again, the results for the 20-year definition of a teenager are shown in Panel A of Table 8. For both benefit variables the effects are strong and significant in all specifications, including the result derived from our IV estimator. The results indicate that a female who gave birth before age 20 is on average likely to receive 34% to 39% more benefit income and is 21% to 27% more likely to be receiving a means-tested benefit at age 30, compared to if she had not given birth by age 20.

This evidence again suggests that the effects of teenage motherhood on socio-economic status at age 30 are significantly negative, even when we take into account the fact that selection into teenage motherhood may be based on unobserved attributes of the mothers. The IV results present the possibility that the effects are not as negative as simple estimation methods might suggest. But in this case, whichever estimation technique is used, teenage motherhood appears to increase the likelihood of receiving means-tested benefits.

[26] We intend to investigate this by extending our empirical work to BCS cohort members at age 26, and also to compare simple OLS estimates from the NCDS at ages 23, 33 and 42.

Splitting the sample into those who gave birth before age 18 (Panel B) and those who gave birth between 18 and 20 years of age (Panel C), we find similar results. However, again we find the surprising result that the effects appear larger for those falling pregnant between the ages of 18 and 20 than those before 18. As before we find that the PSM estimates are similar to the OLS results for the pregnant sample.

Table 8 Impact of teenage motherhood on benefit income variables

	(1) OLS	(2) OLS	(3) PSM bw = 0.01	(4) PSM bw=0.001	(5) IV	(6) IV - Bound 85% random
	Full Sample	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**
A: 20yr definition						
Log Weekly Benefit	0.631	0.345	0.381	0.338	0.384	0.669
Income	(0.042)	(0.082)	(0.124)	(0.143)	(0.152)	(0.156)
On Means Tested	0.365	0.268	0.270	0.238	0.211	0.380
Benefits?	(0.019)	(0.034)	(0.047)	(0.056)	(0.075)	(0.079)
B: 18yr definition						
Log Weekly Benefit	0.592	0.224	0.194	0.279	0.160	0.538
Income	(0.058)	(0.104)	(0.148)	(0.264)	(0.238)	(0.225)
On Means Tested	0.346	0.185	0.122	0.118	0.062	0.227
Benefits?	(0.027)	(0.047)	(0.069)	(0.108)	(0.116)	(0.119)
C:18-20 yr definition						
Log Weekly Benefit	0.471	0.277	0.330	0.218	0.343	0.609
Income	(0.050)	(0.126)	(0.214)	(0.320)	(0.199)	(0.203)
On Means Tested	0.308	0.240	0.196	0.151	0.172	0.339
Benefits?	(0.025)	(0.050)	(0.083)	(0.123)	(0.096)	(0.100)

5.3. The impact of teenage motherhood on employment and wages at 30

One important reason why teenage mothers fare worse, both in terms of their family income, and in terms of dependence on means tested benefits at age 30 is because teenage motherhood has a detrimental impact on a woman's labour market status at age 30. Table 9 shows that teenage motherhood significantly reduces the probability of being in employment at age 30, on all estimation techniques we have adopted. For those who do work, conventional estimates suggest that it significantly reduces the number of hours worked. Not surprisingly, these shorter hours mean that teenage motherhood leads to a reduction in weekly earnings. Additionally, Table 9 shows that hourly earnings are also significantly reduced (though by less than weekly earnings). Our 'conventional' estimates which control for observed heterogeneity only suggest that hourly wages are around 15-24 percentage points lower as a result of giving birth as a teenager. In the case of these outcomes the PSM estimates are again similar to the OLS on the pregnant sample. Again our IV estimate suggests that the true value of this effect on hourly wages is considerably smaller, and not significantly different from zero; however the bounds we have calculated on this estimate suggest that problems with our instrument could be driving the elimination of this effect. In the case of family income and benefit receipt, teenage motherhood appeared more detrimental if the age at which the mother first gave birth was 18 or 19, rather than under 18. However there is no such consistent pattern for the labour market variables shown in Table 9.

Table 9 *Impact of teenage motherhood on employment and wage variables*

	(1) OLS	(2) OLS	(3) PSM bw =0.01	(4) PSM bw=0.001	(5) IV	(6) IV - Bound 85% random
	Full Sample	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**
A: 20yr definition						
In work?	-0.211	-0.146	-0.216	-0.208	-0.185	-0.465
	(0.019)	(0.036)	(0.051)	(0.065)	(0.071)	(0.074)
Log Weekly Wage	-0.510	-0.388	-0.424	-0.441	-0.007	-0.524
	(0.040)	(0.076)	(0.111)	(0.168)	(0.190)	(0.161)
Log Hourly Wage	-0.187	-0.151	-0.185	-0.237	0.062	-0.175
	(0.022)	(0.044)	(0.067)	(0.100)	(0.119)	(0.079)
Hours Worked per	-8.053	-6.414	-8.145	-7.848	-3.817	-10.425
Week	(0.722)	(1.323)	(1.847)	(2.962)	(2.865)	(2.677)
B:18yr definition						
In work?	-0.194	-0.087	-0.195	-0.187	-0.185	-0.096
	(0.027)	(0.047)	(0.067)	(0.109)	(0.105)	(0.109)
Log Weekly Wage	-0.478	-0.316	-0.415	-0.449	-0.073	-0.451
	(0.061)	(0.091)	(0.150)	(0.311)	(0.302)	(0.253)
Log Hourly Wage	-0.204	-0.101	-0.154	-0.349	-0.034	-0.174
	(0.029)	(0.051)	(0.101)	(0.213)	(0.159)	(0.154)
Hours Worked per	-6.036	-4.447	-5.810	-5.863	-2.405	-8.036
Week	(1.142)	(1.835)	(2.995)	(6.541)	(4.887)	(4.560)
C:18-20 yr definition						
In work?	-0.183	-0.193	-0.178	-0.188	-0.248	-0.399
	(0.025)	(0.051)	(0.078)	(0.119)	(0.087)	(0.091)
Log Weekly Wage	-0.471	-0.415	-0.249	-0.177	-0.012	-0.594
	(0.050)	(0.114)	(0.280)	(0.469)	(0.243)	(0.204)
Log Hourly Wage	-0.152	-0.113	0.055	-0.085	0.066	-0.213
	(0.030)	(0.078)	(0.237)	(0.330)	(0.165)	(0.108)
Hours Worked per	-8.629	-6.652	-8.505	-8.934	-1.071	-8.296
Week	(0.858)	(1.796)	(3.395)	(7.468)	(3.602)	(3.456)

5.4 The impact of teenage motherhood on partnership at 30

There is little evidence in Table 10 that teenage motherhood affects the probability of having a partner[27] at 30. This means that lone parenthood can be ruled out as an important contributor through which teenage motherhood confers disadvantage at this age. However teenage motherhood *is* associated with having a partner who is less well qualified, and who has a lower weekly wage compared to those who did not become mothers as a teenager, when we consider our OLS and matching estimates alone.

However our IV estimates again raise the question of whether it is teenage motherhood *per se* which leads to lower-earning partners, or whether it is other, unobservable attributes of the mother determining this outcome. But again the consequent bounds calculated on the IV estimates show that that we cannot rule out that it is biases in our IV that generate this result.

5.5 The impact of teenage motherhood on educational attainment by 30

The final set of outcomes that we consider relate to the cohort members' educational attainment. This is likely to be an important mechanism through which teenage motherhood confers later life disadvantage, and one which is likely to create permanent, rather than temporary differences between teenage, and non-teenage mothers.

Our 'conventional' estimates in Table 11 show that those who gave birth as teenagers are considerably less likely to go on to post-compulsory education than those who do not. This could be an important mechanism through which teenage childbearing leads to the negative effects that we have already seen.

However, inferring a causal interpretation for this is complicated by the fact that the most important decisions made by young people about their education are likely to take place around the same time, or even before the pregnancy and motherhood decisions we are considering. Hence the decision to become a young mother may in part be a direct result of leaving school young, and not the other way round. Our IV approach should mitigate such problems since, in principle, the educational outcomes

[27] Defined as a cohabitee or legal spouse.

Table 10 Impact of teenage motherhood on partnership variables

	(1) OLS	(2) OLS	(3) PSM bw =0.01	(4) PSM bw=0.001	(5) IV	(6) IV - Bound 85% random
	Full Sample	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**	**Pregnant Sample**
A: 20yr definition						
Partner in Household?	-0.006	0.063	-0.005	0.005	0.012	-0.116
	(0.018)	(0.035)	(0.047)	(0.062)	(0.074)	(0.077)
Log Partner's Weekly	-0.151	-0.174	-0.183	-0.123	-0.148	-0.365
Wage	(0.040)	(0.067)	(0.084)	(0.161)	(0.126)	(0.109)
Partner Post-Compulsory	-0.099	-0.131	-0.097	-0.097	-0.091	-0.217
Schooling?	(0.020)	(0.036)	(0.048)	(0.062)	(0.075)	(0.079)
B: 18yr definition						
Partner in Household?	-0.003	0.072	-0.007	-0.009	-0.043	-0.189
	(0.026)	(0.045)	(0.063)	(0.105)	(0.112)	(0.114)
Log Partner's Weekly	-0.274	-0.225	-0.029	-0.073	-0.160	-0.431
Wage	(0.067)	(0.104)	(0.159)	(0.375)	(0.253)	(0.248)
Partner Post-Compulsory	-0.088	-0.064	-0.088	-0.082	0.044	-0.076
Schooling?	(0.028)	(0.047)	(0.068)	(0.107)	(0.113)	(0.117)
C: 18 to 20 definition						
Partner in Household?	-0.006	0.016	-0.001	-0.005	-0.013	-0.146
	(0.023)	(0.050)	(0.079)	(0.129)	(0.090)	(0.094)
Log Partner's Weekly	-0.043	-0.178	-0.246	-0.154	-0.136	-0.385
Wage	(0.045)	(0.090)	(0.130)	(0.318)	(0.143)	(0.135)
Partner Post-Compulsory	-0.088	-0.142	-0.086	-0.075	-0.065	-0.201
Schooling?	(0.025)	(0.052)	(0.081)	(0.120)	(0.101)	(0.106)

Table 11 Impact of teenage motherhood on educational attainment

	(1) OLS	(2) OLS	(3) PSM bw = 0.01	(4) PSM bw=0.001	(5) IV	(6) IV - Bound 85% random
	Full Sample	Pregnant Sample	Pregnant Sample	Pregnant Sample	Pregnant Sample	Pregnant Sample
A: 20yr definition						
Age Left Full-Time	-0.731	-0.439	-0.263	-0.714	-0.174	-0.652
Education	(0.052)	(0.105)	(0.111)	(0.161)	(0.196)	(0.197)
Post-Compulsory	-0.221	-0.128	-0.131	-0.210	-0.026	-0.127
Schooling?	(0.016)	(0.031)	(0.046)	(0.053)	(0.062)	(0.065)
B: 18yr definition						
Age Left Full-Time	-0.728	-0.330	-0.232	-0.767	0.203	-0.226
Education	(0.061)	(0.114)	(0.145)	(0.227)	(0.193)	(0.189)
Post-Compulsory	-0.220	-0.112	-0.076	-0.207	0.120	0.033
Schooling?	(0.021)	(0.038)	(0.053)	(0.090)	(0.074)	(0.075)
C:18-20 definition						
Age Left Full-Time	-0.586	-0.402	-0.274	-0.545	-0.206	-0.742
Education	(0.065)	(0.159)	(0.213)	(0.351)	(0.276)	(0.282)
Post-Compulsory	-0.178	-0.086	-0.037	-0.165	-0.034	-0.143
Schooling?	(0.020)	(0.045)	(0.073)	(0.113)	(0.083)	(0.086)

of teenage mothers will only be compared to those who, except by for random chance, would otherwise have become teenage mothers too.

In fact, IV estimates of the impact of teenage motherhood on educational attainment show that those who give birth before they are 18 are *more likely* to remain in post-compulsory schooling than had they not given birth – although not significantly so. Moreover the IV bound we have calculated suggests that this *positive* result is unlikely to be due to any biases introduced by our IV estimator. Of course this later school leaving age may not imply extra years of school overall, but could be driven by the fact that schooling is interrupted for those who have a child before they reach 18 – meaning the age at which they finally leave school is delayed. In contrast the IV estimate of the effect on education is negative for the late-teen motherhood group, although insignificantly so.

Finally is should be noted that Table 11, for educational outcomes, is the first time there have been sizeable differences between the linear regression results for the pregnant sample and the propensity score matching estimates. As in Levine and Painter (2003) we find that the PSM estimates with fairly wide bandwidth (column (3)) are somewhat more modest than linear regression and close to the IV results. Levine and Painter (2003) report that their results are insensitive to the bandwidth used while we find that tightening the bandwidth considerably to 0.001 (column (4)) we estimate much larger effects than linear regression – indeed the PSM estimates are now very close to the linear regression results for the whole sample (column (1)).

6 Conclusions

This paper provides evidence on the effects of teenage motherhood on women's later life outcomes, by considering the impact of becoming a teenage mother on a cohort of British women observed at age 30. In line with the recent literature, we have employed a number of methods to account for both observed and unobserved characteristics influencing selection into teenage motherhood. Our results confirm that when observed characteristics only are taken into account, the effects of teenage motherhood on a woman's socio-economic status at age 30 appear to be large and negative. The pathways are numerous, from early educational attainment, to later life labour market status, hours of work, and pay, as well as the labour market status of the partner. Family size and composition also appears to be a very important driver of subsequent disadvantage.

However, our analysis suggests that once we take unobserved characteristics determining selection into teenage motherhood into account, the evidence for strong negative effects on later life outcomes becomes less clear cut. Our IV results - which exploit data on miscarriages as a source of exogenous variation in teenage motherhood - suggest that many of the negative effects may be significantly reduced or even disappear once such unobserved heterogeneity is taken into account. This is in line with the results from many of the other papers cited in section 3. As in other work the size of our treatment group is a problem that undermines precision – we have only 46 (77) miscarriages by age 18 (20) compared to 353 (794) births.

However it is also important to take into account the possible biases inherent in using miscarriages as an instrument for fertility because of misreporting and non-randomness, before concluding that teenage motherhood has few ill effects. We show that for most outcomes we consider, the apparent lack of strong negative effects using IV *could* be driven by biases in our IV estimator. This is shown by the fact that our estimates of lower bounds for our IV results are again large and significantly negative, and indeed are broadly in line with the conventional OLS estimates of the impact of teenage motherhood. This means that we are unable to conclude that the conventional estimates (i.e. those shown in columns 1, 2, and 3 of Tables 7 to 11) could not be in fact the true estimates of the impact of teenage motherhood. Rather, a cautious interpretation of our results would conclude that these conventional OLS estimates probably represent the worst possible effects of early motherhood, whereas the IV estimates probably represent the best.

What does all this mean for the policymaker, trying to decide if teenage motherhood is simply an indicator of prior disadvantage, or a pathway to future disadvantage? From the evidence in this paper alone, we cannot rule out that teenage motherhood itself leads to lower socio-economic status later in life, rather than earlier disadvantage alone. However, our own IV estimates – though potentially biased – do add to the growing body of evidence, amassed using a variety of different, and all imperfect, methods, which suggests that the importance of teenage motherhood may in fact be small compared to the role that prior disadvantage plays.

Our results also shed some light on some other important issues. First we have shown some of the contributing factors to the lower economic status at age 30 experienced by teenage mothers. In particular, those who become teenage mothers are less likely to be in work, work fewer hours, and earn a lower hourly wage than those who do not. There is no difference in the likelihood of having a partner, but the partners of teenage mothers have lower educational qualifications and labour market status than the partners of those who did not become teenage mothers. We have also shown that teenage mothers' families have greater needs - as determined by their family size and composition - for any given level of income. We also have presented some evidence that the educational attainment of teenage mothers could be impaired through having a child at a young age. However the extent to which this is in fact a contributory factor in the decision to become a teenage mother, rather than a result of it is still unclear.

Our analysis has also highlighted the importance of disentangling timing issues from any long-term permanent disadvantage that might be incurred by teenage motherhood. For example, we showed that for most outcomes, the effects of early motherhood at age 30 are larger for females falling pregnant between 18 and 20 years old than those falling pregnant before age 18. One explanation is that the effects of early pregnancy diminish over time and do not persist into later periods in life. This could be because teenage motherhood tends to bring forward in time some of the disadvantage incurred by most mothers when they raise children. Another explanation could be that for those who give birth at a younger age, the young mother's own family typically provides more support, and protects the teenager from some of the more negative effects of becoming a mother at a young age. More research – following individuals over a longer timespan – is required to ascertain the extent to which more permanent disadvantage also ensues.

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