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The Causal Effect of Teen Motherhood on Worklessness

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Abstract

Teen motherhood continues to be high in the US and the UK relative to most other western European countries. While recent research has clarified how effective policies to reduce teen motherhood might be (Kearney (2009)), there remains little evidence that quantifies the causal effects of teen motherhood on such mothers and their first born children. This paper provides estimates of the causal effect of teen motherhood on worklessness and does so by exploiting the availability of two sources of exogenous variation in maternal age at first birth, which have not previously been used in this literature. Despite the strength of our instruments, we find no significant causal effects.

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1. Introduction

The UK and the USA have acute problems with teenage pregnancy[1]. Their 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 rate is the highest in Western Europe, although still less than half the rate in the USA. Britain is also the only country in Western Europe which has not experienced a significant decline in teenage fertility rates in the last thirty years, and has this in common with the USA. This paper is concerned with estimating the effects of early motherhood on one key labour market outcome, namely worklessness, measured at the family level, for mothers aged 25-35 in England and Wales, using pooled UK Labour Force Survey (LFS) data which dated back to 1984.

Worklessness has received much attention in the media and the policy circles in recent years. Yet there is no official definition of worklessness in the UK. In this paper, we will follow the Department of Work and Pensions (DWP), which defines a workless household as 'a household where no adults are in paid employment'[2]. There is a strong negative correlation between maternal age at first birth and worklessness, as shown in Figure 1, using the sample on which our econometric analysis is based.

While around one third of women who gave birth at the age of 16 or 17 live in a workless household by the age of 25-35, this rate steadily declines in the maternal age at first birth till it reaches just one-tenth for women who gave first birth at age 25. Moreover, this relationship holds true conditional on observed partnership later in life, despite the expected big difference in the level of worklessness.

In this paper, we focus on worklessness measured at the family level, which is arguably the most important indicator of social exclusion for both the mother and the child(ren) concerned[3]. The contribution of this paper is a rigorous and systematic

[1] See Social Exclusion Unit (1999)

[2] The DWP is actively assessing the progress of the Children in Workless Households target, using the UK LFS data (see. http://www.dwp.gov.uk/ofa/indicators/indicator-1.asp).

[3] Figure A1a in the Appendix shows the proportion of mothers not working, i.e. worklessness measured at the individual level, for the same sample. In contrast, Figure A1b in the Appendix shows the corresponding proportions of mothers and their families who have never had a paid job, which can be regarded as longer-term measures of worklessness. Both figures display very similar patterns in age at first birth.

econometric study of the causal relationship between early motherhood and worklessness, including the identification of the crucial maternal age at first birth at which motherhood begins to affect worklessness. We do this by pooling the LFS data over two decades for the UK and we exploit two sources of exogenous variation in maternal age at first birth. The application to large datasets such as the UK LFS gives the prospect of showing how impact of teen motherhood has changed over time and differs across observable characteristics.

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 led to disparate evidence, much of it rather imprecise. Conventional correlations have indicated large negative socio-economic effects of early motherhood, and so support the use of even expensive interventions aimed at reducing the incidence of teenage conceptions. More recent evidence that allows for the 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. However, much of this recent evidence is not well determined.

Our method is to use instrument variables to isolate exogenous variation in maternal age at first birth. The instruments that we use have wide applicability and are available in many large datasets. In principle, our method would allow us to investigate the impacts on other outcomes for mother and child. Moreover, the method could also be applied to other countries and offers the prospect of being able to make comparisons across countries.

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 our instrumental variables. 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 observable characteristics only[4], this newer literature has treated teen motherhood as an evaluation problem and 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 childbearing remain. However, drawing any robust conclusions from this debate has so far been difficult because of the sensitivity of the results to the empirical methodology chosen and the data set being used.[5]

Family fixed effects (siblings and cousins) and instrumental variables techniques[6] have traditionally been used to address the problem of unobserved heterogeneity. Fixed effect estimation relies on comparing sisters, one of which gives birth as a teen and the other does not. The strong presumption is that such differencing eliminates the endogeneity and therefore relies on the assumption that unobservable heterogeneity can be captured by a family fixed effect – sisters are the same. Moreover, such comparisons inevitably rely on small samples. 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

[4] See for example Hofferth and Moore (1979) for the USA, and Hobcraft and Kiernan (1999) for the UK. Both papers do, of course, express concern about the endogeneity issue but neither address the issue statistically. Robinson (2002) is another notable study. This constructs synthetic cohorts from cross-section surveys 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. She further shows that the wage penalty appears to be *larger* for recent cohorts. Our data corresponds to this age range where the wage difference is at its maximum. 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.

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

[6] See for example Klepinger, Lundberg and Plotnick (1998), Chevalier and Viitanen (2002), Ermisch et al (2003), Goodman et al (2002), and Ashcroft and Lang (2006).

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 NLSYW data is the older age at which outcomes are measured (28-31 compared with 21-33 in the PSID and NLSYW 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.

Ermisch (2003) was able to look at various outcomes, among which was worklessness, using the British Household Panel Survey (BHPS) over the years 1991-2001. More specifically, he found that relative to women starting a family when age 24 or older, women having a teen-birth are less likely to be employed in their thirties and forties, although their pay is not affected once they have a job. Although father's occupation at age 14 and whether or not the woman came from a one-parent family were used to 'control' for family background, it seems unlikely that this would be sufficient for the critical Conditional Independence Assumption (i.e. motherhood is not correlated with unobserved influences on the subsequent outcomes) to be satisfied for these estimates to have a causal interpretation.

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 the assumption that is equivalent to a family fixed-effects model results in estimates for family income-to-needs ratio[7] 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[8].

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.

Bronars and Groggar (1994) exploit the random nature of giving birth to twins, conditional on becoming pregnant, to create a natural experiment. 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, the idea rests on the strong 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 were the case then one could 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. 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 any children (compared to none), so that the effect on teenagers bearing twins is less than twice that for teenagers bearing singletons.

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

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

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 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 channel 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 maternal age at first birth. This second strand of literature include Hotz, McElroy and Sanders (1999), Goodman, Kaplan and Walker (2002), Ermisch *et al* (2003), and Ashcraft and Lang (2006), who all 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 maternal 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. However, Fletcher and Wolfe (2009) suggest that miscarriages might not be random events, and are likely correlated with unobserved community-level factors. Moreover, the comparisons being made typically rely on small samples of teenagers who gave birth compared to pregnant teenagers who spontaneously miscarried. Such work has yielded results that indicate much smaller effects than traditional correlations, but all three papers that adopt this method find results that are largely statistically insignificant.

3. Instrumental variables: RoSLA and date of birth as sources of exogenous variation in maternal age at first birth

There are at least two ways in which the date of birth affects the probability of having a first birth at an early age. The first is through a cohort effect – whereby different cohorts of girls were exposed to different legal minima to the school leaving age. In England and Wales those born before (September) 1958 could leave school at 15, later cohorts were required to stay until 16. By forcing girls to remain at school longer they are forced to accept a higher opportunity costs to early motherhood by virtue of the effect of the Raising of School Leaving Age (RoSLA) policy on wage rates (see Harmon and Walker (1995) and many other papers which show that this policy had large effects on wage rates)[9]. Thus, in the face of this higher opportunity cost, teen girls are more likely to postpone motherhood. Usefully, as far as our analysis is concerned, this policy change has a geographical component as well as temporal component to it – the policy was implemented later in Scotland and we would exploit this[10]. The second mechanism is through peer effects: in most countries young girls spend a large proportion of their time mixing with peers who are close to the same age – schools are organised into year cohorts. All children within a 12 month birth window (runs from 1st September to 31st August in England and Wales) are grouped together at school. Within this window there is some age variation and older girls will, on average, become sexually active at an earlier point in calendar than their slightly younger peers. But there is peer pressure, which we expect to be stronger from older to younger than vice versa, and so the youngest girls imitate. Unfortunately the younger girls are likely to be less able to access advice, support, contraception and abortion, and so will be more likely to become teen mothers than older children in the same cohort. Thus, controlling for school year, teen motherhood is expected to be larger for spring/summer born girls than the fall/winter born[11].

Thus our method is effectively one based on "instrumental variables" – RoSLA and spring/summer born are our "instruments". It is important, if our idea is to be used as a basis for research on the effect of teen motherhood, that the effects of

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9 Black *et al* (2008) attempted, without much success, to exploit this idea for Norway (and the US).

10 We also intend to explore the possibility of using the 1967 Abortion Act as a source of exogenous variation. Kahane *et al* (2008) demonstrates the wide variation in abortions by region and time.

11 Crawford *et al* (2007) exploited this idea as an instrument for education achievement.

RoSLA and date of birth on teen motherhood are large and statistically significant. Thus, to support our idea we have constructed a dataset of over 80 thousand mothers aged 25-35 who had their first birth by age 25 (yet young enough that the first born child should still be in the household), by pooling the LFS data from 1984 (the earliest date that the LFS questionnaire becomes stable) to 2007. The data contains month and year of birth of both the mother and the oldest child and so allows us to identify the exact age of the mother at first birth. The age distribution of maternal age at first birth closely matches population level data[12].

We show in the next two figures a graphical depiction of the first stage (i.e. on teen motherhood) and reduced-form (i.e. on worklessness) for the two instruments separately. Figures 2a and 2b focus on the impact of 1973 RoSLA which raised the minimum school leaving age from 15 to 16 for those born from September 1958 onwards, using only mothers born 5 years before and 5 years after the policy change (the Abortion Act 1967 made abortion legal in the UK from October 1967 and so abortion would have been a possibility for all these women)[13]. The upper panel suggests a downward trend in the teen motherhood rate for England and Wales, but with a discontinuity at the critical 1958 date (represented by the vertical bar). The discontinuity is small for teen motherhood at age 16 (or less), large for motherhood at age 17 and 18, and not apparent at all for 19[14]. This pattern is broadly consistent with what we would expect. The small decline in motherhood at age 16 might be due to the likely higher prevalence of contraceptive mistakes at this age rather and the fact that the desire to have a child before the pre-existing minimum school leaving age should not be affected by raising it. That is RoSLA relaxes a non-binding constraint on behaviour at this age and so should not affect behaviour. To the extent that compelling

[12] Of course, the data here are survivors. But early deaths are sufficiently rare that this will make little difference to our analysis. In the wider sample of women in this age range (which includes those whose first births are post 25), 13% of live births occur to teenage (age<20) mothers, with 4% occurring pre-17. These proportions are close to the corresponding figures from NCDS data and ONS population statistics.

[13] Our sample excludes Northern Ireland, which was not covered by the legislation.

[14] The horizontal bars in Table 2a indicate mean proportions having first births at the relevant ages for the pre- and post ROSLA years respectively. Formal statistical tests reject equality of means for motherhood at age 17 and 18 at the 1% significance level, and at age 16 only at the 5% level. However, we fail to reject the null at age 19 even at the 20% level.

girls to have more schooling raises the opportunity cost of early births it should reduce incentives as well as, perhaps, curtailing opportunities to engage in risky sexual behaviours[15]. Thus, we would expect to see the largest effect of RoSLA at age 17 and 18, where all those girls who would have dropped out at 15 in the absence of RoSLA were forced to stay on till at least 16 and some might even be tempted to remain at school to complete A Levels, the next level of qualifications. However, the RoSLA effect is likely to quickly diminish at age 19 or above as it seems likely that few of those who choose to become (or risk becoming) mothers would have expected to go to college even in the absence of a birth.

Conversely, the bottom panel shows little evidence of a discontinuity at 1958 as far as worklessness is concerned. Indeed, there is a smooth upward trend that is steep for motherhood at age 16 through 19, and shallow for motherhood at 20 or older.

In Figures 3a we show the proportion of mothers giving birth at age 16, 17, 18, 19 and 20-25 broken down into mothers born March to August (spring/summer born) relative to those born September to February (autumn/winter born). There is a clear difference, which is highly significant, between spring/summer born and autumn/ winter born for birth at age 16 or less, and this, as we would expect, gets weaker at 17, and vanishes altogether at 18 or later since susceptibility to peer pressure and ignorance both get weaker as girls age. In the bottom panel of Figure 3, the pattern of worklessness by maternal age at first birth is far from clear-cut. Relative worklessness for the spring/summer born is well below parity for motherhood at 16, slightly above parity at 17, and moderately below parity at 18 and 19. For the control group of motherhood at age 20-25, there appears to be no effect of month of birth at all.

In summary, Figure 2 and Figure 3 have demonstrated that both RoSLA and month of birth have a very strong effect on teen motherhood individually. However, the evidence on worklessness in the reduced-form is less clear. In our econometric analysis, we will combine both instruments to maximize our ability to identify the causal effect of teen motherhood.

[15] By exploiting a Chilean school reform that lengthened the school day from half to full-day, Krueger and Berthelon (2009) find that a 20% increase in full-day municipal enrolment reduces the incidence of teen motherhood by 5% among poor girls in urban areas.

4. Data

Much of the existing evidence relies on cohort studies, such as NCDS and BCS for the UK and NLSYW for the US, where teenagers can be observed and can be tracked over their lives. Other work has exploited panel data, such as PSID in the US and BHPS in UK. Previous research has been hampered by imprecise estimates associated with the small size of the samples used. For example, the UK cohort studies yield only a few hundred teen mothers, even if teen is defined as old as 19, and teen miscarriages are substantially less numerous than this.

Conventional cross-sections of data are alternative sources of information. It is common for the age of children in the household to be recorded in cross-section datasets. So providing the data captures first births, we can infer the age of the mother at her first birth. However, most cross-section datasets do not contain a complete fertility history and children leave home from as young as 16. This implies sufficiently tight censoring on such data by age of mother (say, aged 35 and below) is needed to ensure that maternal age at first birth can be inferred from the age of the oldest children present in the household. Inevitably we too are concerned about sample size. Thus, we use the largest UK survey that is available – the Labour Force Survey pooled from 1984 to 2007[16]. The LFS records a variety of outcomes for the mother: her employment, marital status, employment status, earnings and hours of work, and her partner's earnings, hours and employment status if present[17].

The main sample used for analysis in this paper is women aged between 25 and 35 in England and Wales in the UK Quarterly Labour Force Survey pooled from 1984 to 2007, who had their first birth by the age of 25. Our sample contains 80,596 distinct mothers, of which 22,979, or 28.5% gave first birth before their 20th birthday (i.e. the most commonly used definition of teenage mothers). In this paper we use various definitions of 'teen mother' in all of our analysis – those aged up to (and including) 16 years, and those aged 17, 18 and 19. In each case we maintain the same

[16] The LFS data prior to 1983 is not comparable with later surveys because of inconsistencies in measurement, definitions and coverage. The LFS data post 2007 no longer contains information on month of birth of respondents.

[17] Unfortunately, earnings are only available since 1992, when LFS changed from an annual to quarterly survey.

control group: those women whose first birth was at age 20 to 25[18]. We also investigate how adopting an early definition of a teenager affects the results in order to try to identify whether certain ages are critical. This is important for policy purposes since it may suggest how teen motherhood prevention policies might best be timed.

Table 1 presents some key characteristics for each of these various samples defined above. The first 4 columns are mutually exclusive, each consisting of a different treatment group defined by the specific age at which the first child was born, which will be compared against the same control group in the last column. The first column also includes the small number of births at age 14 and 15, and as a whole account for 3.0% of the sample. Motherhood at age 17, 18 and 19 account for 5.8%, 8.8% and 10.9% of the sample respectively. The second last column, denoted 'all teens', is the sum of the first 4 columns and accounts for 28.5% of the sample. The control group, of motherhood at age 20-25, represents the remaining 71.5% of the full sample. It is clear from Table 1 that teen motherhood is associated with higher chance of being cohabiting, divorced/separated or single (hence lower chance of being married), as well as having more children, with a pattern usually monotonic in maternal age at first birth.

Table 2 provides summary statistics of the variables to be used in our econometric analyses in the next section. Consistent with Figure 1, Table 2 shows that there is a striking difference in worklessness, the outcome (i.e. dependent) variable, by maternal age at first birth. Teen mothers as a whole are almost twice as likely to live in a workless household, comparing to women who start motherhood between 20 and 25. As expected, both instruments appear to be independent of the maternal age at first birth. We will control for smooth trends in tastes and technology by including a

[18] Ideally, we would like to classify females based on age at first conception. Unfortunately, date of conception is not available in LFS, as in most cross-section datasets. Instead, we classify based on age at first birth (i.e. the outcome of the first pregnancy). 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.

cubic in normalised continuous measure of birth cohorts in months[19]. Note that there are slight differences in the average ages of each of the groups that will contaminate the outcome information to some degree: the multivariate analysis in the next section will control for age effects by including a quadratic in age. There also appears to be some regional variation in the incidence of teen motherhood, with the rest of England and Greater London over-represented among teen mothers. In the regression, we will allow for differences in composition by including dummies for Wales, London and the Southeast.

5. Results

We begin by examining the correlations between worklessness and teen motherhood using just the Linear Probability Model (LPM) which assume that teen motherhood is exogenous. Probit models produce very similar marginal effects, and are not shown here. Table 3 presents the coefficient on teen motherhood, variously defined, where in each case the control group are mothers whose first birth was 20-25. We successively introduce control variables in a systematic way, from no covariates in panel A, just a cubic in cohort trends in panel B, and cohort trends plus age and age squared in panel C. Finally, in our full specification in panel D, we add regional controls[20]. The LPM estimates are almost invariant to the successive addition of controls. What is really striking in Table 3 is that the effect seems not to vary much with the precise age at which a teen birth occurred. Relative to motherhood at 20-25, the effect of early motherhood on worklessness is around 13% for first birth at age 16 or lower, peaks at 16% for birth at 17, then declines by only 1-2 percentage points with each year of postponement. For all teen mothers as a whole, having an early child birth increases worklessness in the mid 20s to mid 30s by 15 percentage points, comparing to women who have their first child between 20 and 25 (with a mean of 18%). This is almost a doubling of the risk of worklessness.

[19] This is done in practice by first constructing a continuous measure of month of birth which runs from 1, using the information on both year and month of birth in the data. We then normalize the measure, to make sure it is between 0 and 1. The normalization procedure seems critical for the LIML estimation.

[20] One limitation of the Labour Force Survey is the lack of family background controls. Buckles and Hungerman (2008) suggests that months of birth are correlated with family background characteristics in US data and highlights the potential benefits of controlling for family background in IV estimation. Fortunately, our identification strategy does not solely rely on the use of months of birth instruments.

Table A1 in the Appendix shows the LPM estimates of early motherhood, variously defined, on the RoSLA variable that captures the introduction of the school leaving age increase, and a dummy variable for born between March and August to capture the month of birth effect, as well as a full set of controls corresponding to panel D in Table 3. These are the first stage results of an over-identified two-stage least squares (2SLS) model which instruments early motherhood on the two instruments. RoSLA has a large negative effect – for example, for the age≤17 definition post RoSLA teens are around 2 percentage points less likely to become teen mothers than pre-RoSLA teens and the effect of being a summer born girl is over half a percentage point more likely for the age≤17 definition, compared to a mean rate of approximately 9%. Note that these are not just sizeable effects, they are well-determined as well – they are statistically significant even at the 1% level[21]. Our conclusion from this exercise is that both of our ideas have considerable merit.

Table 4 presents the 2SLS estimates of teen motherhood, again variously defined, and with various sets of controls as in Table 3, on worklessness. Only in models with no controls at all (i.e. panel A), are the effect of teen motherhood statistically significant. But the marginal effects are well over 100%, which makes them implausible. In panel B which allows for cohort trends, the effect of teen motherhood for all teens is within the 100% limit and statistically significant. However, apart from reversing signs as maternal age at first birth increases, none of the 4 components are statistically significant individually. Once we control for age and/or region effects in panels C and D, none of the estimates remain statistically significant. Indeed, the sizes of the estimated effects also decrease significantly.

In Table 5, we compare various IV estimators, including 2SLS, LIML (Limited Information Maximum Likelihood) and GMM (Generalized Method of Moments), for the sample involving all teen mothers, as well as a subsample of mothers who gave first birth by the age of 17. The rationale for focusing on this particular subsample is based on the first-stage estimates presented in Table A1, which suggest our instruments are likely to be most effective for this subgroup, and

[21] Thus, it seems likely that the projected analysis would not be open to the criticism that the instruments are weak.

hence capture the treatment effect of compliers (see the discussion of the Local Average Treatment Effect in Angrist and Pischke 2009 for instance). This is indeed also in line with the first-stage results and the IV-relevance tests in Table 5. The F-statistics for the joint significance of the instruments are over 23 for the very young teen mother subsample, comparing to a value of around 16 in the full sample. Moreover, the dummy capturing the month of birth effect is never statistically significant when we include all teen mothers in the treatment group, which implies that the model is effectively identified on RoSLA alone (i.e. just identified).

A comparison of the 2SLS estimates with their GMM and LIML counterparts show that there is very little difference in our full-specified model with full controls. In particular, LIML offers the advantage of providing a finite-sample bias reduction, at the cost of higher standard errors (see Davidson and MacKinnon 1993). The fact that 2SLS and LIML estimates are almost indistinguishable in both samples is really reassuring.

The coefficient of teen motherhood is negative but statistically insignificant in all three models for the sample of birth by 17, and this is not through a lack of strong instruments as in many studies. Indeed, formal tests strongly reject the null of weak instruments for both samples. For instance, the relative biases of our LIML estimates are well below 10% of those for LPMs. Moreover, all three models for the very young mum sample also pass the relevant over-identification tests.

So we conclude that despite the strength of our instrumental variables, we haven not been able to find any evidence of a causal effect of early motherhood on worklessness later in life. This finding suggests that the strong negative correlation between teen motherhood and worklessness in the raw data is largely due to unobserved heterogeneity.

6. Conclusions

This paper provides evidence on the effects of teenage motherhood on mother's later life outcomes – here we concentrate on worklessness as our outcome. In line with earlier literature, we find very strong negative correlation in the raw data. However, the focus of the paper is the identification of causal effects. The method is instrumental variables and we have successfully identified two instruments that have not been used in the literature before. Despite the strength of our instruments, we find no significant causal effects.

Further research will be concerned with a wider range of outcomes and an array of datasets, including many from outside the UK. Moreover, it is important to compare the results here with alternative IVs that have been used in the earlier literature and with other methods where possible.

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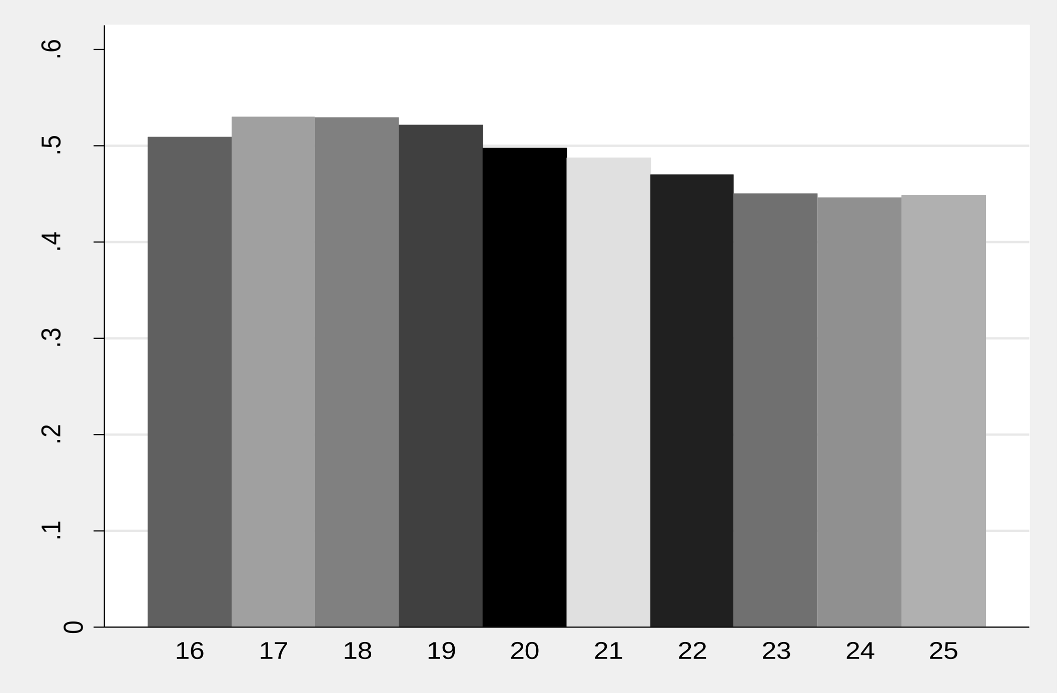
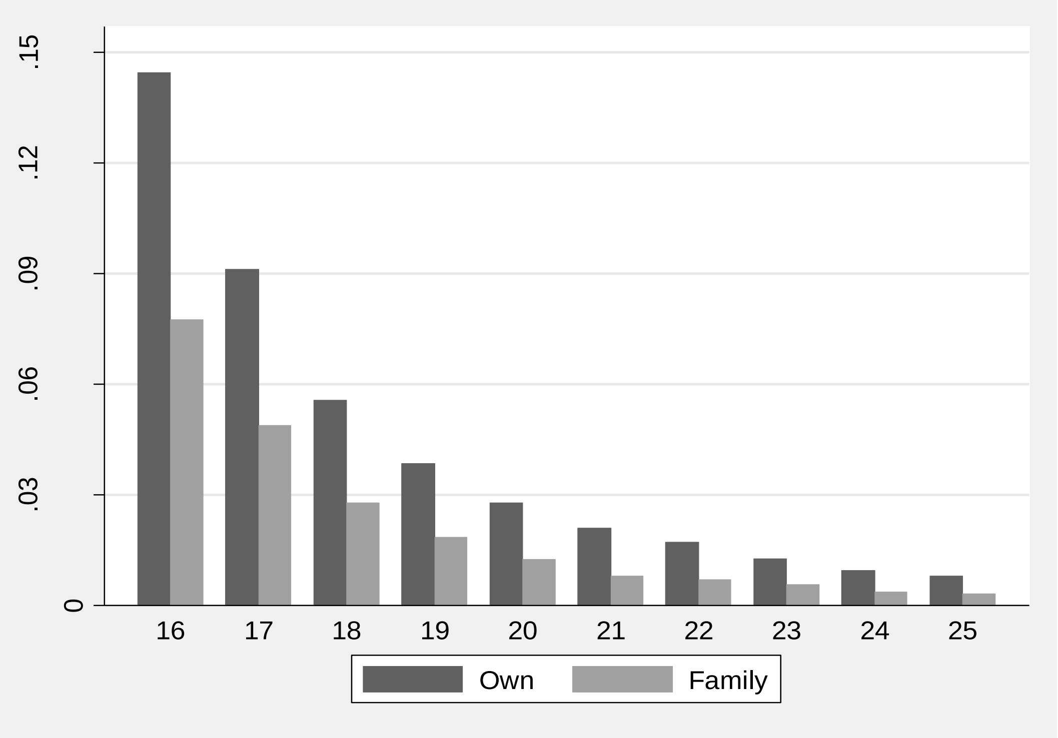
Appendix

Figure A1a: Proportion of Women Aged 25-35 Not in Paid Work in England and Wales, by Age at 1st Birth



Notes: Authors' own calculation using pooled Labour Force Survey 1984-2007.

Figure A1b: Proportion of Women Aged 25-35 and Their Families Who Have Never Had A Paid Job in England and Wales, by Age at 1st Birth



Notes: Authors' own calculation using pooled Labour Force Survey 1984-2007.

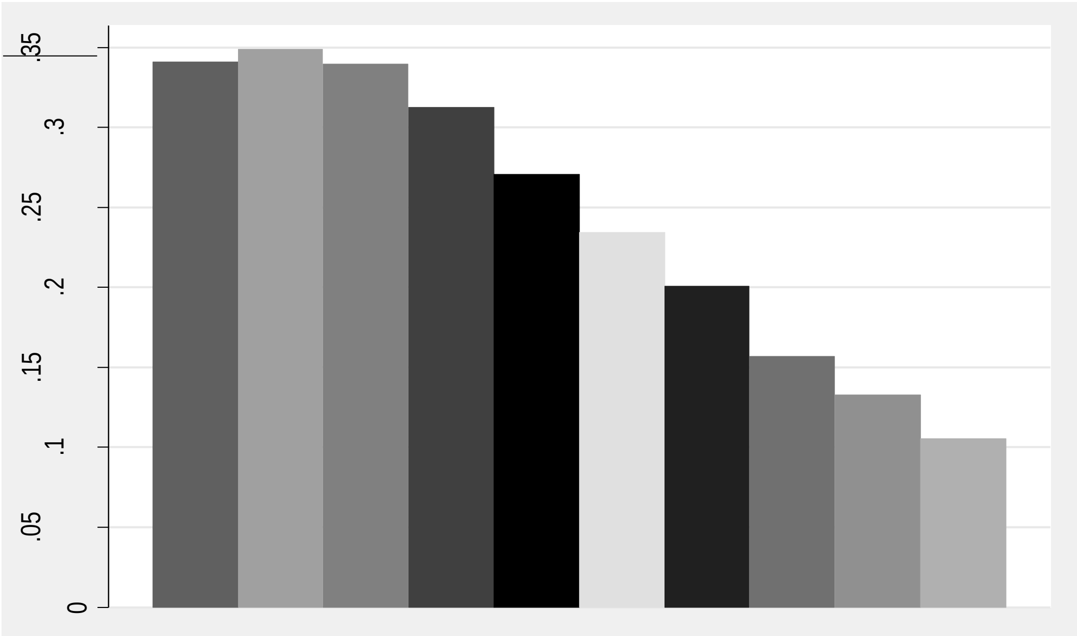
Table A1 *Linear Probability Model Estimates of Teen motherhood on IVs and a Full Set on Controls (Model D), by Age at 1st Birth*

Age at 1st birth:	≤16	17	18	19	All teens
Excl. Instruments:					
(post-) RoSLA	**-0.011**	**-0.019**	**-0.029**	-0.004	**-0.041**
	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)
Born Mar-Aug	**0.006**	**0.006**	-0.002	**-0.007**	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Covariates:					
Cohort	**0.255**	0.140	-0.129	-0.015	0.109
	(0.090)	(0.119)	(0.138)	(0.147)	(0.179)
Cohort squared	*-0.200*	-0.179	0.191	0.135	0.086
	(0.109)	(0.144)	(0.168)	(0.179)	(0.216)
Cohort cubic	**0.194**	0.161	-0.070	0.046	0.108
	(0.076)	(0.100)	(0.116)	(0.124)	(0.149)
Age * 10	**0.423**	0.010	-0.040	-0.112	0.193
	(0.061)	(0.081)	(0.094)	(0.100)	(0.121)
Age squared * 10^2	**-0.065**	-0.017	0.001	0.024	-0.029
	(0.009)	(0.013)	(0.015)	(0.016)	(0.019)
Wales	0.000	0.003	**0.014**	**0.017**	**0.022**
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)
London	-0.002	-0.000	-0.001	-0.001	-0.002
	(0.003)	(0.004)	(0.004)	(0.005)	(0.006)
Southeast England	**-0.010**	**-0.023**	**-0.032**	**-0.028**	**-0.061**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Sample size	60,016	62,308	64,737	66,386	80,596

Notes: Standard errors in parentheses. **Bold** and *italic* cases indicate statistical significance at the 5% and the 10% levels respectively.

Figures

Figure 1: Proportion of Women Aged 25-35 Living in Workless Households in England and Wales, by Age at 1st Birth



Notes: Authors' own calculation using pooled Labour Force Survey 1984-2007.

Figure 2a   Minimum school leaving age and the maternal age at first birth

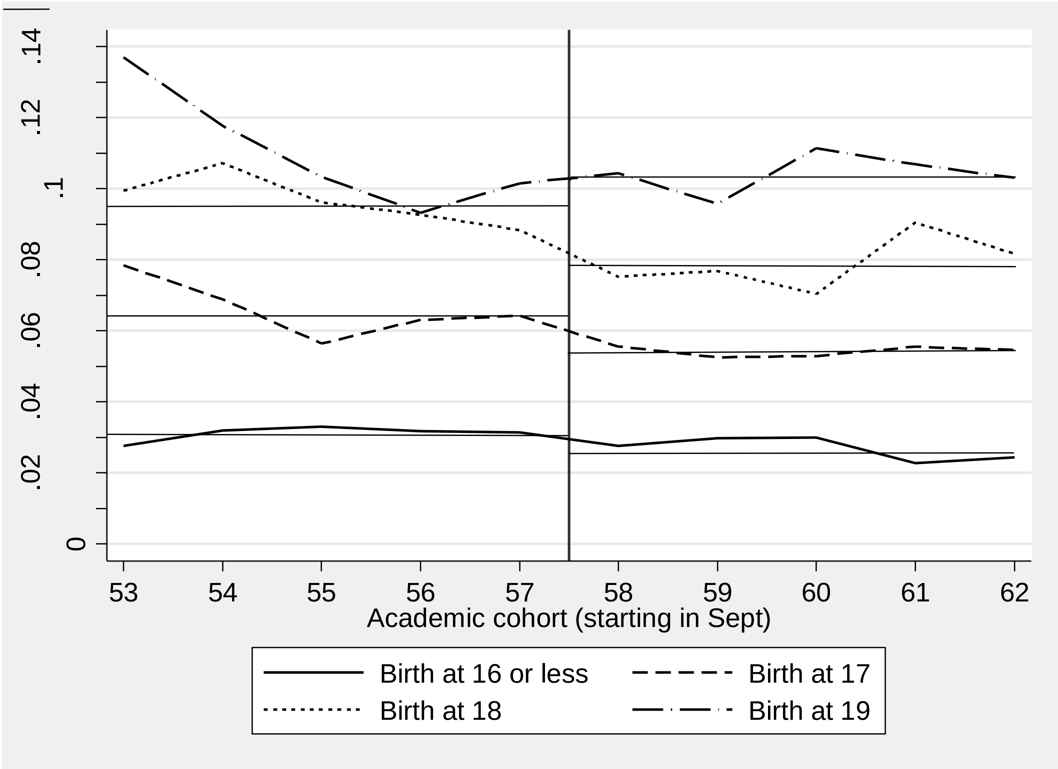


Figure 2b   Minimum school leaving age and Worklessness

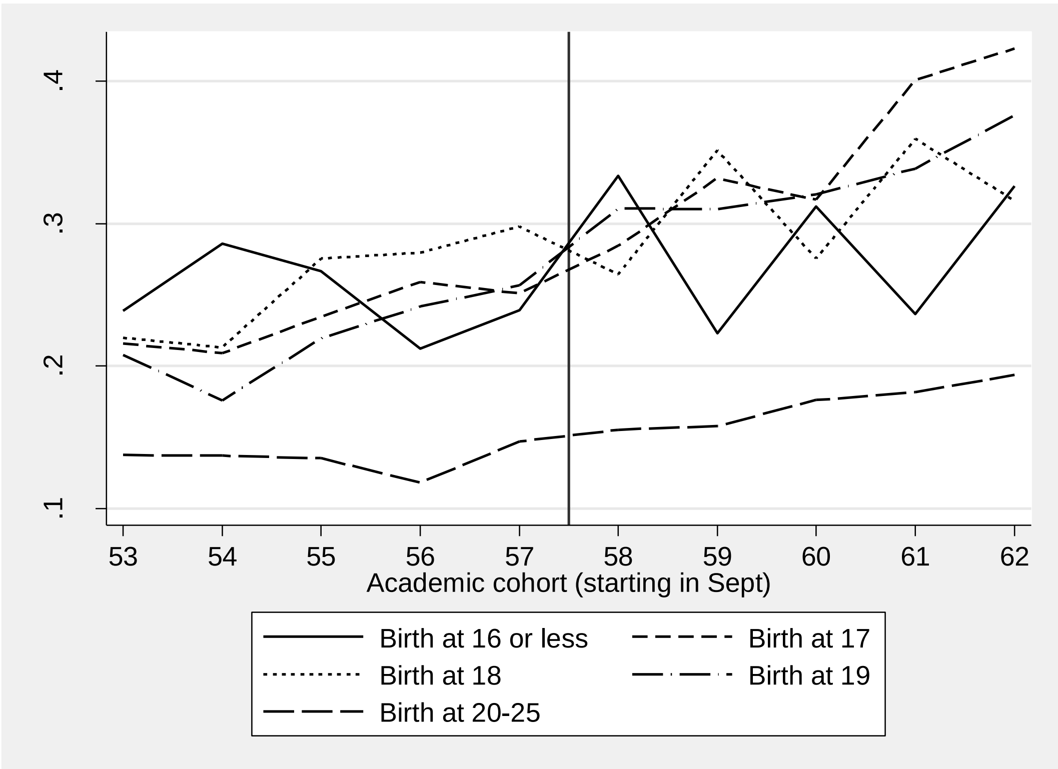


Figure 3a Month of birth and the maternal age at first birth, Ratio of March-August births relative to September-February births

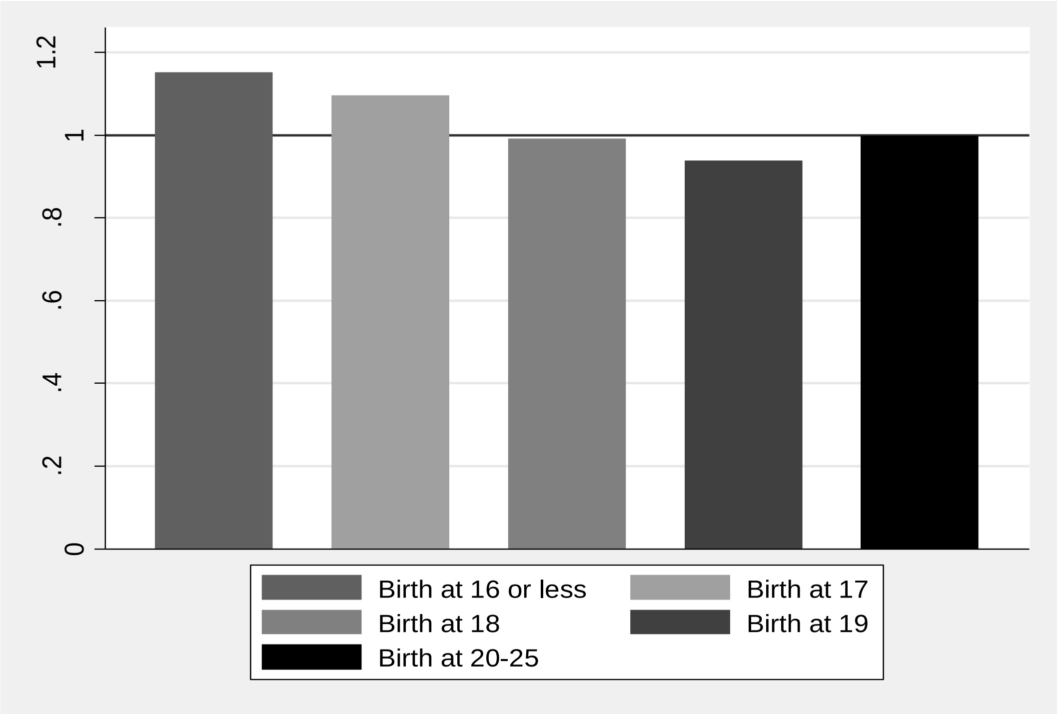
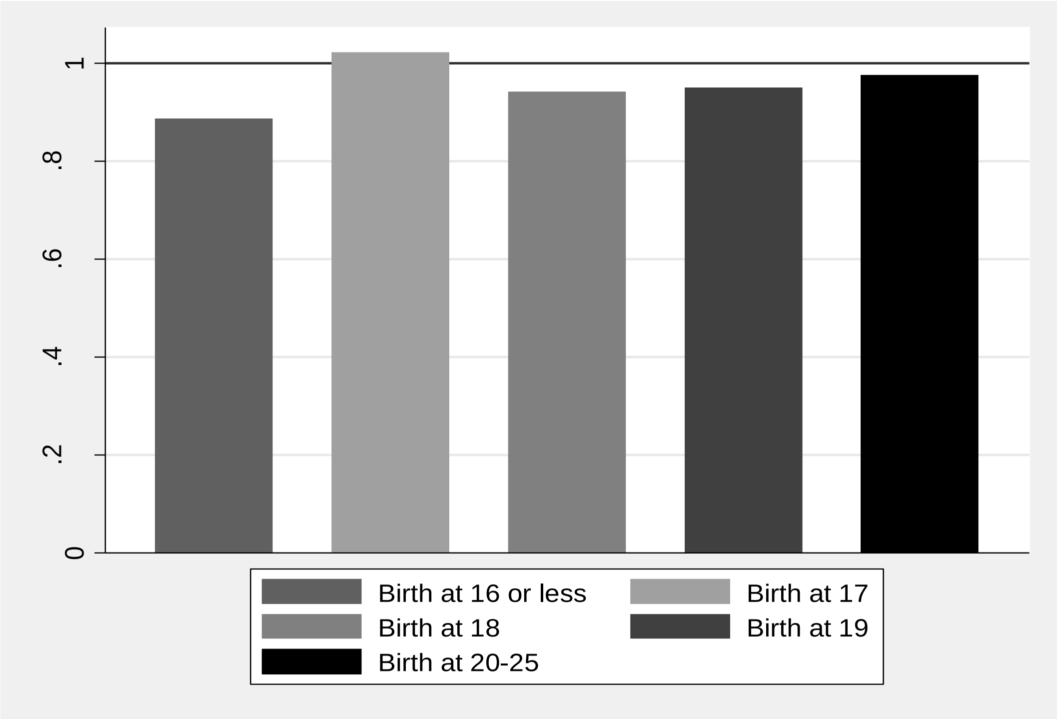


Figure 3b Month of birth and Worklessness, Ratio of March-August births relative to September-February births



TABLES

Table 1 Sample Characteristics: Pooled LFS 1984-2007

Age at 1st birth:	≤16	17	18	19	All teens	20-25
Married	0.596	0.591	0.600	0.618	0.605	0.738
Cohabiting	0.100	0.083	0.081	0.081	0.083	0.071
Divorced/separated	0.182	0.181	0.187	0.174	0.180	0.116
Single	0.122	0.145	0.133	0.127	0.132	0.075
Number of children	2.67	2.62	2.56	2.44	2.54	2.02
Age oldest child	13.33	12.46	11.68	10.77	11.67	7.28
Age youngest child	6.85	6.67	6.32	5.95	6.30	4.24
% of all 1st births	2.98	5.82	8.83	10.88	28.51	71.49
Sample size	2,399	4,609	7,120	8,769	22,979	57,617

Note: Fractions reported for all binary variables.

Table 2 Summary statistics: Pooled LFS 1984-2007

Age at 1st birth:	≤16	17	18	19	All teens	20-25
Dependent variable:						
workless	0.322	0.349	0.340	0.313	0.329	0.182
Excl. Instruments:						
(post-) RoSLA	0.749	0.720	0.717	0.730	0.726	0.739
Born Mar-Aug	0.552	0.540	0.515	0.501	0.519	0.517
Covariates:						
Cohort	0.461	0.444	0.440	0.437	0.442	0.429
Cohort squared	0.260	0.246	0.242	0.238	0.243	0.226
Cohort cubic	0.166	0.156	0.151	0.147	0.152	0.135
Age	29.60	29.97	30.16	30.25	30.09	30.27
Age squared	884.34	907.59	919.51	924.80	915.42	925.81
Wales	0.069	0.072	0.079	0.078	0.076	0.065
(Greater) London	0.083	0.087	0.085	0.084	0.085	0.082
Southeast England	0.159	0.149	0.149	0.162	0.155	0.202
Rest of England	0.689	0.692	0.688	0.676	0.684	0.651
% of all 1st births	2.98	5.82	8.83	10.88	28.51	71.49
Sample size	2,399	4,609	7,120	8,769	22,979	57,617

Note: Cohort variables normalised. Fractions reported for all binary variables.

Table 3 *Linear Probability Model Estimates of Teen motherhood on Worklessness, by Age at 1st Birth*

Age at 1st birth:	≤16	17	18	19	All teens
A. No Covariates					
	0.140	**0.167**	**0.157**	**0.130**	**0.147**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.003)
B. Cohort Effects					
	0.135	**0.165**	**0.157**	**0.130**	**0.146**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.003)
C. Cohort Effects; age and age squared					
	0.133	**0.164**	**0.156**	**0.130**	**0.145**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.003)
D. Cohort Effects; age and age squared; survey year and region dummies					
	0.132	**0.162**	**0.154**	**0.128**	**0.143**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.003)
Sample Size	60,016	62,308	64,737	66,386	80,596

Notes: Standard errors in parentheses. **Bold** and *italic* cases indicate statistical significance at the 5% and the 10% levels respectively. The cohort effect variables comprise a cubic in normalised continuous measure of birth cohorts in months.

Table 4 *Two Stage Least Squares (2SLS) Estimates of Teen motherhood on Worklessness, by Age at 1st Birth*

Age at 1st birth:	≤16	17	18	19	All teens
A. No Covariates					
	3.133	**-5.524**	**-6.718**	**-3.803**	**-6.525**
	(0.982)	(1.434)	(1.735)	(1.256)	(1.786)
B. Cohort Effects					
	-0.770	-0.544	-0.235	0.332	**-0.398**
	(0.407)	(0.299)	(0.245)	(0.399)	(0.167)
C. Cohort Effects; age and age squared					
	-0.721	-0.405	-0.038	0.377	-0.208
	(0.452)	(0.289)	(0.232)	(0.390)	(0.153)
D. Cohort Effects; age and age squared; survey year and region dummies					
	-0.352	-0.143	0.240	0.679	-0.026
	(0.413)	(0.296)	(0.244)	(0.494)	(0.162)
Sample Size	60,016	62,308	64,737	66,386	80,596

Notes: Standard errors in parentheses. **Bold** and *italic* cases indicate statistical significance at the 5% and the 10% levels respectively. The cohort effect variables comprise a cubic in normalised continuous measure of birth cohorts in months.

Table 5 Alternative IV Estimates of Teen motherhood on Worklessness, by Age at 1st Birth, Selected Models

Age at 1st birth:	All teens			≤17		
	2SLS	LIML	GMM	2SLS	LIML	GMM
Second Stage Results:						
Teen Motherhood	-0.026	-0.048	-0.025	-0.085	-0.086	-0.079
	(0.162)	(0.173)	(0.152)	(0.188)	(0.188)	(0.179)
Cohort	0.080	0.077	0.089	0.169	0.169	0.172
	(0.160)	(0.161)	(0.158)	(0.172)	(0.172)	(0.171)
Cohort Squared	**0.658**	**0.664**	**0.651**	*0.378*	*0.378*	*0.372*
	(0.201)	(0.203)	(0.196)	(0.217)	(0.217)	(0.215)
Cohort Cubic	**-0.467**	**-0.465**	**-0.463**	-0.245	-0.245	-0.242
	(0.138)	(0.139)	(0.141)	(0.158)	(0.159)	(0.162)
Age * 10	-0.148	-0.141	-0.146	0.028	0.028	0.026
	(0.120)	(0.121)	(0.120)	(0.152)	(0.152)	(0.151)
Age Squared * 10^2	0.020	0.019	0.020	-0.007	-0.007	-0.007
	(0.019)	(0.019)	(0.019)	(0.024)	(0.024)	(0.024)
Wales	**0.025**	**0.026**	**0.025**	**0.018**	**0.018**	**0.018**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
London	**0.091**	**0.090**	**0.091**	**0.087**	**0.087**	**0.087**
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
South-east England	**-0.058**	**-0.059**	**-0.058**	**-0.050**	**-0.050**	**-0.050**
	(0.011)	(0.011)	(0.010)	(0.007)	(0.007)	(0.007)
Survey Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
First Stage Results:						
RoSLA	**-0.041**	**-0.041**	**-0.041**	**-0.028**	**-0.028**	**-0.028**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Born in March-August	0.001	0.001	0.001	**0.011**	**0.011**	**0.011**
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
F-stat for excl. IVs	**16.035**	**16.035**	**16.322**	**23.271**	**23.271**	**23.655**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
C.V. for 10% rel. bias	19.93	8.68	-	19.93	8.68	-
C.V. for 15% rel. bias	11.59	5.33	-	11.59	5.33	-
Sargan (Anderson-Rubin	*3.645*	*3.629*	*3.610*	0.176	0.176	0.184
/Hansen) $\chi^2_{(1)}$ (P-value)	(0.056)	(0.057)	(0.057)	(0.675)	(0.675)	(0.668)
Sample Size	80,596	80,596	80,596	62,308	62,308	62,308

Notes: Standard errors in parentheses. **Bold** and *italic* cases indicate statistical significance at the 5% and the 10% levels respectively. The cohort effect variables comprise a cubic in normalised continuous measure of birth cohorts in months. Omitted region is the rest of England.