Does a 'Teen-birth' have Longer-term Impacts on the Mother?

Evidence from the 1970 British Cohort Study

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NON-TECHNICAL SUMMARY

There has been considerable concern that having a child as a teenager may have longer-term consequences for the mother, in terms of her standard of living and earnings. These are usually thought to arise because having a child disrupts investment in her earning power, by causing her to curtail her formal education and by keeping her out of work for a time, thereby depriving her of valuable work experience.

It is generally difficult to accurately measure the possible consequences of teen births because we do not know what the woman would have done later in life if she had not had a birth as a teenager. Simple comparisons with women who have children later in life may not identify the consequences properly because the women who had a teen birth may have had different outcomes anyway even if they had not given birth as a teenager.

This paper adopts a novel approach to this problem by exploiting information on women's pregnancy histories to obtain better estimates of the consequences using data from the British 1970 Cohort Study. The use of pregnancy information, particularly the incidence and timing of abortions and miscarriages (as well as births) makes it possible to consider three estimators of the average effect of a teen birth on these women later in life.

The results from the last of the three estimators, which is preferred as it uses extra information, suggest that a teen birth has little impact on a woman's qualifications, employment or earnings when they are 30 years of age. These results contrast with previous studies that have not been able to use this estimator. However, this estimator does also suggest that if the woman has a partner at age 30, the partner is less likely to have post-16 education and to be unemployed. The effects are large – about 20 percentage points lower for post-16 education and 20 points lower for being employed. A teen birth also reduces the likelihood of home ownership at age 30.

These results suggest that having a teen birth causes a teenage mother to fare worse in the 'marriage market' in the sense that she partners with men who are more likely to be poorly qualified and more likely to suffer unemployment. This tends to reduce the standard of living of her and her children.

ABSTRACT

The paper uses information on women at birth and ages 5, 10 and 30 along with pregnancy histories, including miscarriages, to estimate average causal effects of having a birth at ages 19 or under on various ‘outcomes’ at age 30 for women who had such a ‘teen-birth’. Following the methods developed by Hotz *et al*, the miscarriage information allows the effects to be bounded under relatively weak conditions, and an instrumental variable estimator exists under stronger conditions. These estimates are compared to other estimators that can be derived from the pregnancy history information. The results suggest little adverse impact of a teen-birth on woman’s qualifications, employment or pay at age 30, but the bounds on the effect indicate that the partner she is with at age 30, if she has one, is more likely to be unemployed. For many outcomes, there is evidence of strong selection into who becomes pregnant as a teenager and who aborts if she becomes pregnant.

Introduction

There has been considerable concern that having a child as a teenager may have longer term consequences for the mother in terms of her earnings and standard of living, and of course these also entail consequences for the children living with her. These are usually thought to arise because having a child as a teenager disrupts her human capital investment, by causing her to curtail her formal education and by keeping her out of employment for a time, thereby depriving her of valuable work experience. It is generally difficult to measure these consequences because we do not know what the woman would have done if she did not have a child as a teenager. A comparison of teenage mothers with women starting childbearing later will usually not identify these consequences because the women who became teenage mothers may have had different outcomes anyway, even if they had not given birth as a teenager. Different methods have been used to address this non-random 'selection into teenage motherhood', including comparisons of sisters (e.g. Geronimus and Korenman, 1992; Hoffman *et al*, 1993), instrumental variable (IV) estimation (e.g. Rosenzweig and Schultz, 1983; Grossman and Joyce, 1990; Olsen and Farkas, 1990); Ribar, 1994), and more general formulations encompassing sibling and IV methods (Rosenzweig and Wolpin, 1995; Ribar, 1999). The present paper uses a particular IV method developed by Hotz et al (1997), which can be applied when there is information about women's pregnancy histories.

The first section discusses what parameter we wish to identify, the issues involved in doing so and three possible estimators of the impact of teenage motherhood. The second section briefly describes the 1970 British Cohort Study (BCS70) and discusses how we implement the estimators derived in the first section with these data and the resulting parameter estimates for a number of outcomes. The third section discusses how we can obtain bounds on the impacts under much weaker conditions and the estimates of these bounds. A final section presents our conclusions.

Measuring the Impact of a 'Teen-birth' on the Mother

In our analysis, we observe a random sample of observations on variables z and x, where z indicates whether (z=1) or not (z=0) a woman had a birth as a teenager (or, more generally, before some age) and x are some control variables (e.g. family background). At some later age a, we also observe outcomes such as educational attainment, earnings, income or receipt of Income Support. Define y_1 as the outcome if a woman were to have a 'teen-birth', and y_0 is the outcome were she to postpone her childbearing beyond her teens (including not having a child by age a). Thus, we observe y_1 when z=1 and y_0 when z=0. The impact of a teen-birth on these outcomes may differ among women. Our objective is to estimate the average impact of a teen-birth, and so one obvious candidate for a measure of it is

$$\beta(x)=E(y_1|x) - E(y_0|x) \quad (1)$$

where $E(y_i|x)$ is the mean of y_i conditional on x. This is the average difference in outcomes resulting from a teen-birth for a woman with characteristics x. An experiment that randomly assigned women to have or not have a teen-birth would provide an estimate for $\beta(x)$, but such an experiment is clearly not feasible

With non-experimental data, the central issue is one of ***identification.*** The sampling process identifies $E(y_1|x, z=1)$, $E(y_0|x, z=0)$ and $E(z|x)=P(z=1|x)$, where $P(z=i|x)$ indicates the probability that $z=i$ conditional on x.. It does *not* identify $E(y_1|x, z=0)$, nor $E(y_0|x, z=1)$, nor therefore

$$E(y_1|x)= E(y_1|x, z=1)P(z=1|x) + E(y_1|x, z=0)P(z=0|x) \text{ and}$$
$$E(y_0|x)= E(y_0|x, z=1)P(z=1|x) + E(y_0|x, z=0)P(z=0|x)$$

Thus, we cannot identify $\beta(x)$. The problem we face is to find restrictions on the population distributions of y_i and z conditional on x that are correct and useful. One assumption that is useful but may be incorrect is to assume $E(y_i|x)= E(y_i|x, z=1) = E(y_i|x, z=0)$, i=0,1. This is the *conditional independence assumption*; that is, the mean of y_i (i=0,1) conditional on x is the same for teen-mothers as for women starting their childbearing later**.** Under this assumption, $\beta(x)=E(y_1|x, z=1)-E(y_0|x, z=0)$. This assumption is often made in the literature on the consequences of teenage births for mothers.

An estimate of $\beta(x)$ that uses the sample analogue of $E(y_1|x, z=1)-E(y_0|x, z=0)$ generally estimates something different than $\beta(x)$, as the following shows:

$$E(y_1|x, z=1)-E(y_0|x, z=0)= \beta(x) + \{E(y_0|x, z=1)-E(y_0|x, z=0)\}$$
$$+ P(z=0|x)[E(y_1-y_0|x, z=1)-E(y_1-y_0|x, z=0)] \quad (2)$$

The latter two terms on the right hand size of (2) represent biases from non-random selection into the group of women who have a teen-birth. The first source of *selection bias* may arise because, even if they did not have a teen-birth, women having one may differ in mean outcomes from women who do not. For instance, if y represented a woman's earnings later in life, women with lower earning capacity may be more likely to have a teen-birth, making $E(y_0|x, z=1)<E(y_0|x, z=0)$. This would operate to produce a downward bias in the estimate of $\beta(x)$. The second source of selection bias is that there may also be a difference in expected *differences* in outcomes between women having and not having a teen-birth. For example, women whose earnings would suffer less from having a teen-birth may be more likely to have one, and so $E(y_1-y_0|x, z=1)>E(y_1-y_0|x, z=0)$, which would tend to bias the estimate of $\beta(x)$ upwards. Under the conditional independence assumption, both of these selection bias terms vanish.

An alternative measure of the impact of a teen-birth, which may be easier to identify, is

$$\alpha(x)=E(y_1|x, z=1) - E(y_0|x, z=1) \quad (3)$$

This is the average impact of a teen-birth among those women having one (again allowing for heterogeneity in impacts among women), often called the *effect of treatment on the treated*. This measure should be of particular interest to policy-makers because policies that seek to reduce teenage childbearing are likely to target women who would have become teen mothers in the absence of the policy. Of course, $E(y_0|x, z=1)$ is not identified. Suppose we try to estimate $\alpha(x)$ using, as before, the sample analogue of $E(y_1|x, z=1)-E(y_0|x, z=0)$, denoting this estimator as $\alpha_0(x)$. The expected value of this estimator is

$$E[\alpha_0(x)]=E(y_1|x, z=1)-E(y_0|x, z=0)= \alpha(x) + \{E(y_0|x, z=1)-E(y_0|x, z=0)\} \quad (4)$$

Thus, this difference does not identify $\alpha(x)$ either, because of the first type of selection bias discussed above. Again, selection bias disappears if the conditional independence assumption is correct, and indeed in this case, $\alpha(x)=\beta(x)$; that is, the two types of 'treatment effects' coincide.

We shall focus on trying to estimate $\alpha(x)$ because it is more readily identified from available data and because of its potential policy relevance. That is, we estimate how different would outcomes (y) be if a woman having a teen-birth had postponed the birth beyond her teenage years? It is difficult to believe that the conditional independence assumption, required for α_0 to be an unbiased estimator, is correct, unless, for example, all teenage pregnancies are 'mistakes' and abortion is not an option. While there is evidence that many teenage pregnancies are unplanned (Barrett, 2002), not all of them are, and abortion is freely available from the National Health Service in Britain.[1]

Using pregnancy history information

It could be argued that women who do not become pregnant as teenagers provide little information about the effect of a teen-birth on women having one. An alternative estimator of $\alpha(x)$ can be based on a sample of women who became pregnant as a teenager, from which those having a teen-birth are drawn. Let p be a pregnancy indicator, with p=1 if a woman becomes pregnant as a teenager, and p=0 otherwise. Our alternative estimator of $\alpha(x)$ is the sample analogue of $E(y_1|x, z=1) - E(y_0|x, z=0, p=1)$, denoted as $\alpha_P(x)$, the expected value of which is

$$E[\alpha_P(x)]=E(y_1|x, z=1) - E(y_0|x, z=0, p=1) =\alpha(x) + \{E(y_0|x, z=1)-E(y_0|x, z=0, p=1)\} \quad (5)$$

Again, this estimator may be subject to some selection bias, and freely available abortion provides reason to doubt the assumption needed for α_P to be an unbiased estimate. Among women becoming pregnant as a teenager, women having the child may differ in mean outcomes from women who do not, even if they did not have the child. For instance, if earnings later in life is again the outcome variable, women with higher earning capacity may be more likely to have an abortion when they become pregnant as a teenager; that is, $E(y_0|x, z=1) < E(y_0|x, z=0, p=1)$, thereby leading to downward bias in the estimator $\alpha_P(x)$. In other words, women who abort their child may be 'fundamentally different' in terms of the outcome variable y from those who have the child.

The expected values of the two estimators $\alpha_0(x)$ and $\alpha_P(x)$ are related in the following way:

$$E[\alpha_0(x)]= E[\alpha_P(x)]P(p=1|x) + [1-P(p=1|x)]\{E(y_1|x, z=1) - E(y_0|x, z=0, p=0)\} \quad (6)$$

It is clear from (6) that if $E[\alpha_P(x)]=E(y_1|x, z=1)-E(y_0|x, z=0, p=0)$, then $E[\alpha_P(x)]=E[\alpha_0(x)]$. Probably more informative is the fact that, using (4) and (5),

$$E[\alpha_0(x)] - E[\alpha_P(x)] = [1-P(p=1|x)]\{E(y_0|x, z=0, p=1) - E(y_0|x, z=0, p=0)\} \quad (7)$$

That is, the two estimators are equivalent if the mean outcome in the absence of a teen-birth is the same for women who became pregnant as a teenager as for those

[1] Obviously, abortion is not an option for those who have moral objections to it.

who did not. If it is not, then there is non-random selection into the population who became pregnant as a teenager. For instance, if those who became pregnant would have, on average, worse outcomes than those who did not, then $E[\alpha_0(x)]$ is smaller than $E[\alpha_P(x)]$. Equation (7) suggests a simple test for the difference between these two estimators based on the population of women who did not have a teen-birth. If we cannot reject the hypothesis that these two estimators are different, then the estimator $\alpha_0(x)$ is preferable under the maintained hypothesis that $E(y_0|x, z=1)=E(y_0|x, z=0, p=1)$, because it is more efficient; $\alpha_P(x)$ is less efficient because it discards relevant data.

Consistent estimates of α(x) using miscarriage data

As shown above, both α_0(x) and α_P(x) may be biased estimates of α(x). This cannot be tested without additional information. While $E(y_0|x, z=1)$ is not identified, the distribution of y_0 is identified for the population who have a miscarriage. If we assume that random miscarriages do not affect the non-birth outcomes (y_0) of women who would have had a birth if they did not have the miscarriage, then, following Hotz et al (1997),

$$\alpha(x) = E(y_1|x, z=1) - E(y_0|x, z_L=B) \qquad (8)$$

where z_L is an unobserved latent pregnancy resolution indicator for women having a random miscarriage, with z_L=B, A, or NR, indicating birth, abortion or 'non-random miscarriage'. Non-random miscarriages occur through behaviour like smoking and drinking during pregnancy, which may be correlated with outcomes. Unfortunately, $E(y_0|x, z_L=B)$ is not identified from the data unless additional conditions are imposed.

Suppose that the following conditions are satisfied. (a) Miscarriages are observable events. (b) The occurrence of a random miscarriage precludes a teen-birth, and in the absence of a random miscarriage, latent-birth women have a birth. (c) Random miscarriages do not affect non-birth outcomes of latent-birth women (i.e. $E(y_0|x, z^*=RM, z_L=B) = E(y_0|x, z_L=B)$, where z^*=RM indicates a random miscarriage). (d) Random miscarriages are independent of a woman's latent type; that is, $P(z_L=j|z^*=RM)= P(z=j|x,z^*=\sim RM)=P_j(x)$, j=B, A, NR, where z=0 has been sub-divided into abortion (z=A) and non-random miscarriage (z=NR); $P_{NR}(x)$ is the probability that the latent pregnancy resolution for a woman having a random miscarriage would have been a non-random miscarriage; $P_B(x)$ is the probability that it would have been a birth and $P_A(x)$ is the probability that the pregnancy resolution would have been an abortion ($P_{NR}(x) + P_B(x)+ P_A(x)=1$); (e) Random miscarriages do not affect non-birth outcomes of latent-abortion women (i.e. $E(y_0|x, z^*=RM, z_L=A) = E(y_0|x, z_L=A)$). (f) Random miscarriages do not affect non-birth outcomes of latent-non-random-miscarriage women (i.e. $E(y_0|x, z^*=RM, z_L=NR) = E(y_0|x, z_L=NR)= [P_B(x)/(1-P_{NR}(x)]E(y_0|x, z_L=B) + [P_A(x)/(1-P_{NR}(x)]E(y_0|x, z_L=A)$.

If these six conditions are satisfied, then it follows from (8) that

$$\alpha(x)= [1-P_{NR}(x)]\{E(y|x, p=1, M=0) - E(y|x, M=1)\}/P_B(x) \qquad (9)$$

where M=1 indicates a miscarriage (see Hotz et al 1997).

If all miscarriages are random (i.e. $P_{NR}(x)=0$), then (9) becomes

$$\alpha(x)=\{E(y|x, p=1, M=0) - E(y|x, M=1)\}/P_B(x) \qquad (10)$$

Both of these conditional means can be estimated from a random sample of pregnancies, and $P_B(x)$ is estimated as the proportion of women having a teen-birth among those who became pregnant as a teenager but did not have a miscarriage. Note that the estimator based on the sample analogue of (10) is equivalent to an Instrumental Variable estimator of $\alpha(x)$ with M being the instrument:

$$\alpha(x)=\{E(y|x, p=1, M=0)-E(y|x, M=1)\}/\{P(z=1|x, p=1, M=0)-P(z=1|x,M=1)\} \qquad (11)$$

because $P(z=1|x, p=1, M=0)=P_B(x)$ and $P(z=1|x, M=1)=0$.[2]

Thus, under the assumption that all miscarriages are random, the IV estimator, denoted as $\alpha_{IV}(x)$, based on the sample analogue of $\{E(y|x, M=0) - E(y|x, M=1)\}/P_B(x)$, is a consistent estimator. The intuition for the inclusion of women having abortions in the computation of the difference in conditional means in the numerator is that the proportion of miscarrying women who would have had an abortion if they had not miscarried is assumed to be the same as the proportion of abortions among those who did not miscarry (see particularly conditions (d) and (e)). A Hausman test of whether $\alpha_{IV}(x)$ is different from $\alpha_P(x)$ provides a test of the bias in $\alpha_P(x)$, and as an alternative, a test is implemented by bootstrapping the standard error of the difference $\alpha_P(x)- \alpha_{IV}(x)$.

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[2] *Heckman (1997) shows that instrumental variable methods fail when* $P(z=1|x)$ *depends on* u_1-u_0, *where* $u_i=[y_i-E(y_i|x)]$, $i=0,1$, *because* $\alpha(x)$ *is* functionally dependent *on the instrument's value in this case. For example, people who "choose the treatment" despite having values for the instrumental variable that suggest they should not, would tend to have high values of* u_1-u_0. *But in our case,* $E(u_1-u_0|x, z_L=B, M=1) = E(u_1-u_0|x, z_L=B, M=0)$ *when all miscarriages are random; thus, women who have a miscarriage do not have systematically different values of* u_1-u_0 *from those who do not, and* $\alpha(x)$ *is not functionally dependent on the instrument's value.*

In sum, we have three estimators of the average effect of treatment on the treated. They differ with respect to the assumptions under which they are consistent estimates of the average effect. In experimental language, each could be viewed as incorporating a different ‘control group’: all women not having a teen-birth for α_0, women having an abortion or a miscarriage for α_P and women having a miscarriage as teenager who would have had a teen-birth in the absence of the miscarriage for the IV estimator. The IV estimator can also be interpreted as a *Local Average Treatment Effect*, or LATE (Imbens and Angrist 1994; Angrist *et al* 1996) corresponding to the miscarriage instrument.[3] According to this interpretation, it estimates the average effect of having a teen-birth for women who would have had a teen-birth if they did not have a miscarriage as a teenager. In our case, the LATE estimator corresponding to the miscarriage instrument estimates the average effect of treatment on the treated.

Implementing the estimators with BCS70 data

The 1970 British Cohort Study is made up of all British people born during 5-11 April 1970. It collected data from the cohort members or their parents at birth, ages 5, 10, 16, 23 and 30. In this last sweep, at age 30, 5,790 women were interviewed and 3,670 reported having ever been/being pregnant. Of these, 844 women reported a pregnancy before age 20 (14.6% of the sample of women at age 30). Including multiple pregnancies before 20, a total of 1,002 teenage pregnancies were reported. From these pregnancies, 582 women (10.0% of the sample of women at age 30[4]) had 664 live births. Of the remaining pregnancies, 220 were aborted and 118 ended in a miscarriage or still birth. Thus, condition (a) of the previous section is satisfied, although miscarriages are probably under-reported. If women have both a miscarriage and a teen-birth as a teenager, then in our analysis they are designated as having a teen-birth. Those defined in our analysis as having a miscarriage as a teenager include only those who had a miscarriage(s) but did not have a birth as a teenager. Thus, by construction, condition (b) of the previous section is satisfied. In a companion paper (Ermisch and Pevalin, 2003), we show that a number of personal and family background factors measured at birth, age 5 and age 10 of the

[3] As Heckman (1997) shows, different instruments define different LATE parameters.
[4] Birth registration statistics indicate that 13% of the women in the 1970 birth cohort had a first live birth before their 20th birthday; see *Birth Statistics*, 1999, Table 10.3.

cohort member are both associated with the probability that a woman becomes a mother as a teenager and with outcomes measured at age 30. Given this evidence of ‘selection into teenage motherhood’ in terms of observable characteristics of a woman and her family that also affect outcomes, it is not unlikely that there is also ‘selection on unobservables’, which the estimators described above address in different degrees.

We do not allow $\alpha(x)$ to vary with x, but include control variables in a linear specification. The estimate of $\alpha(x)$ comes from the estimate of α in a regression, $y=\alpha z+ x\gamma +e$, where γ is a vector of parameters associated with the control variables x and e is a residual error term. The outcome variable may be continuous or dichotomous, and in the latter case we are estimating a linear probability model. Robust standard errors are calculated. For the $\alpha_P(x)$ and $\alpha_{IV}(x)$ estimators, the sample of women is restricted to those who were pregnant as teenagers. The IV estimate of α treats z as endogenous and uses M and x as the set of instruments.[5] Its standard error is obtained by bootstrapping the IV estimator because of concerns about the small sample properties of the IV estimate.

Table 1 shows the three estimators of the average impact of postponing a teen-birth for a number of women’s outcomes measured at the age of 30.[6] Appendix Table A1 reports estimates of α without control variables x. As might be expected, these bivariate estimates almost always indicate larger adverse impacts of a teen-birth (or less favourable ones) than the multivariate estimates in Table 1, irrespective of the estimator. This reflects the fact that many of our control variables, measured when the woman was aged 10, significantly affect both outcomes at age 30 and the probability of having a teen-birth.

In order to illustrate the reporting of the results, consider the impact of a teen-birth on the probability of being a ‘regular smoker’ at age 30. While being a regular smoker has a number of health implications, it is unclear what mechanisms would produce a ‘causal’ impact of a teen-birth on the chances of being a regular smoker

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[5] An alternative computation of the IV estimate of α is the estimate of b in the regression $y=b(1-M)+ xc +e^*$ for the sample of women who were pregnant as teenagers divided by the average probability of a birth in this sample (i.e. the sample average of $P_B(x)$ computed from a logit model for a birth among the sample of women pregnant as a teenager). The results are very similar.

[6] Confidence intervals for the $\alpha_0(x)$ and $\alpha_{IV}(x)$ estimators are given in Appendix Table A3.

ten or more years after the birth.[7] According to the α_0 estimator, having a teen-birth increases the probability of being a regular smoker by 0.225, but the α_P estimator indicates that the impact is only 0.059, which is not significantly different from zero. The test of whether these two estimators are significantly different from each other, suggested by (7), is a simple t-test that the parameter *a* is zero in the following regression estimated from a sample of women who did not have a teen-birth: $y=ap+xd+u$. Table 2 shows the estimate of *a* for each of the outcomes considered in Table 1. It indicates that α_0 is significantly greater than α_P in the case of the regular smoker outcome (the estimate of *a* is 0.189).[8]

In words, even if they did not have the child, women who became pregnant as teenagers are more likely to be a regular smoker at age 30 than women who did not. Clearly, this difference cannot be interpreted as the impact of a teen-birth. Among women who became pregnant as a teenager, those having a birth are not significantly more likely to be a regular smoker at 30. The IV estimator for this outcome in Table 1 indeed indicates that having a teen-birth *reduces* the probability of being a regular smoker by 0.024, but this impact is not statistically significant. This pattern suggests that the α_0 estimator is a strongly upward biased estimator of $\alpha(x)$, while the α_P estimator may also be upward biased, but not significantly so, as indicated by the test of α_P-α_{IV}. Thus, the evidence strongly suggests that having a teen-birth does not have a causal impact on the probability of being a regular smoker.

Given the concern (expressed at the outset of the paper) that having a child as a teenager disrupts a woman's human capital investment, a teen-birth is more likely to have a causal impact on the qualifications a woman obtains. For instance, consider the impact on obtaining 2 or more A-levels. According to the α_0 estimator, a teen-birth reduces the probability of having 2 or more A-levels by the age of 30 by 0.145, while the α_P estimator indicates that the reduction is 0.115. The latter estimate is significantly less than the former according to the t-test reported in Table 2. Furthermore, the IV estimator is significantly different from the α_P estimator, and it indeed suggests that having a teen-birth *increases* the probability of obtaining 2 A-

[7] It is, of course, possible that additional stress that may be associated with teen childbearing may encourage smoking in subsequent years, but lower income may discourage it.
[8] Table A2 in the Appendix shows the estimates of *a* in the bivariate model. Note that more of the differences between the α_0 and α_P estimators are statistically significant in the bivariate model.

levels by 0.042, although this estimate is not significantly different from zero. The IV estimator is also significantly different from the α_P estimator for the other qualification or education variables in Table 1, and the IV estimator suggests that the impacts of a teen-birth on the probabilities of having no education beyond the age of 16, of obtaining 'any qualifications' and of obtaining 'at least O-level qualifications' are virtually zero. In the case of each of these impacts, the α_P and α_0 estimators are not significantly different.

The estimated impact of a teen-birth on the probability of receiving Income Support (the primary means-tested welfare benefit in Britain) at age 30 varies considerably with the estimator used. The α_0 estimator significantly exceeds the α_P estimator, and the α_P estimator is much larger than the IV estimator, which itself is not significantly different from zero. But the difference between the IV estimator and α_P is not statistically significant at the 0.05 level.

The variation among the estimators is similar for the outcomes 'in-employment', 'in-the-top-two social classes', 'being an owner-occupier' and the logarithm of pay (all at age 30). While the α_0 estimator is larger in size than the α_P estimator for these outcomes, we cannot reject the hypothesis that the two estimators are the same. That is, women who became pregnant as teenagers are similar in average outcomes to women who did not. For the three labour market outcomes, the IV estimator is not significantly different from zero. It is noteworthy that the IV estimator suggests that women who had a teen-birth tend to have *higher* pay and are *more* likely to be in the top two social classes in terms of their occupation (among women in employment at age 30), and we can reject the hypothesis that the IV and the α_P estimators are equal. The direction of these impacts is similar to that found by Hotz et al (1997), and they could be interpreted as indicating that being forced to postpone the start of their childbearing by a miscarriage would actually reduce these women's pay and occupational status. That is, women having a teen-birth acted in their own best interests in having a teen-birth rather than postponing.

The IV estimate indicates that a teen-birth reduces the probability of being an owner-occupier by 0.19, and this effect is statistically significant. Whether or not a person is an owner-occupier is indicative of more choice in housing consumption, thereby tending to be associated with higher living standards, and also of wealth accumulation, or at least the potential for it. Thus, the large negative effect suggests that teen-mothers have lower living standards. The IV estimate is not significantly different from the α_P estimator.

Exceptions to the general pattern of results are the impact of a teen-birth on the probability that a woman's partner has no education beyond the age of 16 and the probability that he has a job, conditional on her having a partner at age 30. In each case the IV estimator is larger in size than the other two estimators, and it is significantly different from zero. It suggests that, given that she has a partner, having a teen-birth reduces the probability that he has a job by 0.20. While not as well-determined, her partner also tends to have lower pay if he has a job. This suggests that having a teen-birth constrains a woman's opportunities in the 'marriage market' in the sense that she partners with more unemployment-prone and poorly qualified and lower earning men. Note that a teen-birth does not affect the probability of having a partner at age 30.

The comparison group for these estimates has been all women at age 30, including childless women. As having a teen-birth forecloses the option of being childless at 30, we believe that this is a good 'control group' for evaluating the impact of a teen-birth. But as we are estimating the impact of postponing a birth beyond a woman's teen-years, a case could be made for including only mothers at age 30 in the control group. Table 3 presents the same three estimators for this narrower definition. Comparing Tables 1 and 3, the estimated effects of a teen-birth are usually smaller with the mothers-only control group, particularly for the outcomes associated with a mother's employment, occupation and pay, and the IV estimate is significantly different from the α_P estimator in only one case. For the three outcomes in which the IV estimator is significantly different from zero in Table 1, concerning the partner's education and employment and owner-occupation, the IV estimates are significant and similar for both control groups of women. Indeed, the IV estimate of the impact of a teen-birth on the probability of being an owner-occupier is much larger for the mothers-only control group, and it exceeds the other two estimators.

Unfortunately, the IV estimator, which is less prone to bias than the other two estimators, is usually not very precise. This is partly because the estimate of $E(y|x, M=1)$ is based on only 74 miscarriages. Nevertheless, the smaller size of the IV estimator compared to α_P, or the change in the sign, suggests that α_P is likely to overstate the size of adverse impacts of having a teen-birth. Furthermore, the only estimator that is feasible in the absence of the pregnancy history information, α_0, usually overstates the size of the impacts by more than α_P.

As most data do not contain information on pregnancy histories, it is worth considering further how biased the α_0 estimator is relative to the imprecise IV estimator. Tables A3 and A4 in the Appendix show the 95% confidence intervals for these two estimators the two respective control groups. When including the women who are childless at age 30 in the control group (Table A3), the confidence interval of the α_0 estimator lies fully outside the confidence interval of the IV estimator for five of the outcomes, thereby suggesting a large bias in these cases. It is only contained within that for the IV estimator for 5 outcomes, those associated with the presence of a partner, his employment and pay, her own employment and owner-occupation. For all but these 5 outcomes, the confidence intervals suggest that the α_0 estimator tends to overstate adverse outcomes.

Using the control group containing only mothers at age 30 (Table A4), the confidence interval of the α_0 estimator lies within the confidence interval of the IV estimator for 9 outcomes, and never lies fully outside it. This suggests that there is less selection bias associated with the α_0 estimator when the control group only contains mothers at age 30. There also appears to be less selection bias for the 5 outcomes associated with the presence of a partner, his employment and pay, her own employment and owner-occupation, irrespective of the control group. This information is potentially helpful in estimating the effects of teen-motherhood using other data for which the α_0 estimator is the only feasible one.

Bounding causal effects under weaker conditions

The IV estimator above made the assumption that all miscarriages are random. Epidemiological studies have found, however, that smoking and drinking during pregnancy significantly increase the probability of a miscarriage, and this behaviour is likely to be correlated with subsequent outcomes, such as those examined in the preceding section. For this reason, miscarriages may not be a valid instrumental variable; some of them may affect, or be associated with, the outcome variable directly, rather than only through their effect on the probability of a teen-birth. Fortunately for our purposes, some miscarriages are random, resulting for example from abnormal formation of fetal chromosomes that occurs randomly. Thus, the group of women who miscarry constitute a mixture of random and non-random miscarriages, but we do not know who experienced which type. In the terminology of Horowitz and Manski (1995), our data on miscarriages is a *contaminated* sample of random miscarriages. Using estimates of the proportion of miscarriages that are random, we can, however, obtain lower and upper bounds on $\alpha(x)$. These bounds may allow us to reject some of the point estimates of $\alpha(x)$ discussed above, and they may allow us to determine whether $\alpha(x)$ is positive or negative under relatively weak assumptions.

The estimator based on the sample analogue of (9) allows for non-random miscarriages, but it must still satisfy the six conditions specified in the paragraph preceding (9). Some may consider these conditions to be strong, especially condition (f). Suppose instead that only the first three conditions are satisfied. Given these three conditions, the fact that the distribution of y_0 for the miscarriage population is a mixture of y_0 for women of the various latent pregnancy resolution types who experience random and non-random miscarriages implies that

$$E(y_0|x, z^*=RM, z_L=B) = \{E(y_0|x, M=1) – (1-\lambda)E(y_0|x, M=1, z_L\neq B)\}/\lambda \qquad (12)$$

where λ is the proportion of miscarriages that occur randomly to latent birth type women ($z_L=B$). More formally, $\lambda=P_B(x)P_{RM}/P_M$, where P_{RM} is the probability that a miscarriage is random and P_M is the probability of a miscarriage, with $P_M= P_{RM} + (1-P_{RM})P_{NR}(x)$. While $E(y_0|x, M=1)$ is identified from the data, $E(y_0|x, M=1, z_L\neq B)$ is not, and so $E(y_0|x, z^*=RM, z_L=B) = E(y_0|x, z_L=B)$ is not identified. From (8), $\alpha(x)$ is also not identified, but it can be bounded using (12).

These bounds are most easily expressed when the outcome variable y is dichotomous, which was quite common in the empirical analysis of the previous section. Because y_0 can then only take on the values of 0 and 1, and by condition (c), $E(y_0|x, z^*=RM, z_L=B) = E(y_0|x, z_L=B)$, equation (12) implies that

$$\{E(y_0|x, M=1) – (1-\lambda)\}/\lambda \leq E(y_0|x, z_L=B)\leq \{E(y_0|x, M=1)\}/\lambda \qquad (13)$$

From (8) and (13), we obtain the following bounds for $\alpha(x)$ when y is a dichotomous outcome variable:

$$A_{LD}(\lambda) = E(y_1|x, z=1) – min\{1, E(y_0|x, M=1)/\lambda\} \qquad (14a)$$

$$A_{UD}(\lambda) = E(y_1|x, z=1) – max\{0, [E(y_0|x, M=1) – (1-\lambda)]/\lambda\} \qquad (14b)$$

From the analysis of Hotz et al (1997), equation (12) implies that the the lower and upper *Horowitz-Manski* (HM) *bounds* on $\alpha(x)$ when y is a continuous variable are given, respectively, by

$$A_{LC}(\lambda) = E(y_1|x, z=1) - \{E(y_0|x, M=1) - (1-\lambda)E(y_0|x, M=1, y \leq y_{M,1-\lambda})\}/\lambda \quad (15a)$$

$$A_{UC}(\lambda) = E(y_1|x, z=1) - \{E(y_0|x, M=1) - (1-\lambda)E(y_0|x, M=1, y \geq y_{M,\lambda})\}/\lambda \quad (15b)$$

where $y_{M,1-\lambda}$ is the (1-λ)-quantile of the distribution of outcome y among the miscarriage population; and $y_{M,\lambda}$ is the λ-quantile of this distribution. Given an estimate of λ (or a lower bound of it), all of the terms on the right hand sides of (14a), (14b), (15a) and (15b) are identified from the data. These bounds are "sharp" in the sense that they exhaust all of the information about $\alpha(x)$ that is available from the sampling process and the three maintained assumptions (a ,b and c).

The intuition for the bounds in (15a) and (15b) is as follows. Suppose that 60% of the miscarriage population are random miscarriages to latent-birth types. Thus, the population of non-latent-birth types could not have a distribution of outcomes below that of the bottom 40% of the miscarriage population, and so the mean outcome for the bottom 40% of the miscarriage population is a lower bound on the average outcome for non-latent-birth types. In general, the fraction 1-λ of miscarriages are non-latent-birth types, and the expected value of the (1-λ)-quantile of the distribution of outcome is a lower bound on the mean outcome for non-latent-birth types ($E(y_0|x, M=1, z_L \neq B)$). Similarly, the smallest upper bound on $E(y_0|x, M=1, z_L \neq B)$ is the expected value of the top λ-quantile of the distribution of outcomes.

Implementing (14a) and (14b), or (15a) and (15b), requires an estimate of λ. As long as its estimate does not overstate λ, then the bounds will contain the true α(x), because a lower value of λ reduces $A_{LD}(\lambda)$ and $A_{LC}(\lambda)$, and raises $A_{UD}(\lambda)$ and $A_{UC}(\lambda)$. Following Hotz et al (1997), we consider an estimate of a lower bound on P_{RM}/P_M, based on the assumption that non-random miscarriage types (e.g. those who smoke) are the least likely to report their miscarriages and on American epidemiological studies, $P_{RM}/P_M=0.84$.[9] To obtain corresponding estimates of λ, we need an estimate of P_B that does not overstate it.

In our data, 66% of teenage pregnancies result in a live birth, 22% in an abortion, 11% in a miscarriage and 1% in a still-birth. If underreporting of

[9] They also estimate the bounds under the assumption that $P_{RM}/P_M=0.38$, which would produce wider bounds.

pregnancies is similar across latent types, this suggests that $P_B \leq 0.75$. But official data on abortions suggest that these are underreported in the BCS70. The official data indicate that about 40% of pregnancies not ending in a miscarriage are aborted. Taking 0.6 as a conservative estimate of P_B, our estimate of λ is 0.50. Using this value, we can calculate the sample analogues of $[E(y_0|x, M=1)]/\lambda$ and $[E(y_0|x, M=1)-(1-\lambda)]/\lambda$, or of $E(y_0|x, M=1, y \leq y_{M,1-\lambda})$ and $E(y_0|x, M=1, y \geq y_{M,\lambda})$, from our miscarriage sample, and thereby estimate the bounds on $\alpha(x)$ from (14a) and (14b), or (15a) and (15b).

Tighter bounds may be obtained by imposing additional conditions. For instance Hotz et al (1997) derive the bounds that result from imposing condition (e) in addition to (a), (b) and (c); that is, it is also assumed that *random* miscarriages do not affect non-birth outcomes of latent-abortion women ($E(y_0|x, z^*=RM, z_L=A) = E(y_0|x, z_L=A)$). In this case the bounds on $\alpha(x)$ if the outcome variable is dichotomous, are

$$B_{LD}(\lambda,\theta) = E(y_1|x, z=1) - min\{1, [E(y_0|x, M=1) - \theta E(y_0|x,z=A)]/\lambda\} \quad (16a)$$

$$B_{UD}(\lambda,\theta) = E(y_1|x, z=1) - max\{0, [E(y_0|x, M=1) - \theta E(y_0|x,z=A) - (1-\lambda-\theta)]/\lambda\} \quad (16b)$$

Where $\theta=P_A P_{RM}/P_M$ is the proportion of miscarriages that occur randomly to latent-abortion type women.

When the outcome variable is continuous, these bounds become

$$B_L(\lambda,\theta) = E(y_1|x, z=1) - \{E(y_0|x, M=1) - \theta E(y_0|x,z=A) - (1-\lambda-\theta)E(y_0|x, M=1, y \leq y_{M,1-\lambda-\theta})\}/\lambda \quad (17a)$$

$$B_U(\lambda,\theta) = E(y_1|x, z=1) - \{E(y_0|x, M=1) - \theta E(y_0|x,z=A) - (1-\lambda-\theta)E(y_0|x, M=1, y \geq y_{M,\lambda+\theta})\}/\lambda \quad (17b)$$

where $y_{M,1-\lambda-\theta}$ is the $(1-\lambda-\theta)$-quantile of the distribution of outcome y among the miscarriage population; and $y_{M,\lambda+\theta}$ is the $(\lambda+\theta)$-quantile of this distribution. Taking 0.35 as a conservative estimate of P_A, our estimate of θ is 0.29. Note that, taken together, our estimates entail $P_A+P_B=0.95$ which is consistent with the American evidence (Hotz et al 1997) that $P_A+P_B \leq 0.97$.

The estimates of these bounds from the BCS70 data are usually not informative in the sense that they contain zero, particularly when the lower (upper) bounds are taken to be the lower (upper) 95% confidence interval around the estimate of the bound. One important exception is the bounded interval on the impact of a teen-birth on the probability that a woman's partner has a job, conditional on her having a partner at age 30. The first set of bounds above is –0.218, -0.163 ignoring the standard errors around the bounds, and –0.266, -0.043 using their 95% confidence interval. The tighter, second set of bounds is –0.218, -0.212, and –0.266, -0.090 including the confidence interval. Thus, with a high degree of confidence we can say that having a teen-birth reduces the chances of having a working partner at age 30, given that she has a partner. Women having teen-births appear to suffer in the 'marriage market' in the sense that they partner with men who are more likely to suffer unemployment.

Conclusions

We have used information on women's pregnancy histories, including miscarriages, to estimate average causal effects of having a birth at ages under 20 on various 'outcomes' at age 30 for women who had a 'teen-birth'. Following the methods developed by Hotz *et al*, the miscarriage information allows the average effects to be bounded under relatively weak conditions, but with an important exception, these bounds were not informative with the data used in the study. An instrumental variable estimator exists under stronger conditions, and the IV estimates are compared to other estimators that can be derived from the pregnancy history information. The results suggest little adverse impact of a teen-birth on woman's qualifications, employment or pay at age 30, but the estimated bounds indicate that the partner she is with at age 30, if she has one, is more likely to be unemployed. That is, women having a teen-birth appear to fare worse in the 'marriage market' in the sense that they partner with men who are more likely to suffer unemployment. Having a teen-birth also tends to reduce the probability that a woman is a homeowner at age 30. For many outcomes, there is evidence of strong selection into who becomes pregnant as a teenager and who aborts if pregnant.

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Table 1: Average Effect of a Teen-birth among Women having a Teen-birth**

Outcome	α_0	α_P	α_{IV}	**Test**[a] $\alpha_P-\alpha_{IV}$
Pr(Regular Smoker at 30)	0.225*	0.059	-0.024	N/s
Pr(2+ A-levels)	-0.145*	-0.115*	0.042	Sig.
Pr(Any qualifications)	-0.167*	-0.155*	0.027	Sig.
Pr(At least O-level)	-0.179*	-0.181*	0.014	Sig.
Pr(No educ. beyond 16)	0.242*	0.186*	0.005	Sig.
Pr(Inc. Supp. Recpt. at 30)	0.194*	0.125*	0.037	N/s
Pr(In employment at 30)	-0.220*	-0.169*	-0.108	N/s
Pr(Top 2 social class at 30)	-0.193*	-0.162*	0.097	Sig.
Log(pay at 30)	-0.284*	-0.218*	0.189	Sig.
Pr(Has partner at 30)	0.001	0.054	0.033	N/s
Pr(Partner no post-16 educ.	0.169*	0.137*	**0.220***	N/s
Pr(Partner in employment at 30)	-0.155*	-0.143*	**-0.203***	N/s
Log(Partner's pay at 30)	-0.168*	-0.257*	-0.168	N/s
Pr(Owner-occupier at 30)	-0.302*	-0.262*	**-0.190***	N/s

*Statistic at least twice its standard error

**From multivariate models that control for the age of the woman's mother in 1970, her household's social class at age 10, her mother's education and a summary scale of teacher's ratings at age 10.

[a] 0.05 significance level

Table 2: Test of difference between α_0 and α_P**

Outcome	***a***
Pr(Regular Smoker at 30)	0.189*
Pr(2+ A-levels)	-0.069*
Pr(Any qualifications)	-0.012
Pr(At least O-level)	-0.016
Pr(No educ. beyond 16)	0.073
Pr(Inc. Supp. Recpt. at 30)	0.059*
Pr(In employment at 30)	-0.055
Pr(Top 2 social class at 30)	-0.030
Log(pay at 30)	-0.076
Pr(Partner in employment at 30)	-0.011
Log(Partner's pay at 30)	0.075
Pr(Owner-occupier at 30)	-0.037

*Statistic at least twice its standard error

**From multivariate models that control for the age of the woman's mother in 1970, her household's social class at age 10, her mother's education and a summary scale of teacher's ratings at age 10.

Table 3: Average Effect of a Teen-birth among Women having a Teen-birth who are mothers at aged 30**

Outcome	α_0	α_P	α_{IV}	**Test**[a] α_P-α_{IV}
Pr(Regular Smoker at 30)	0.212*	0.094	0.099	N/s
Pr(2+ A-levels)	-0.085*	-0.060	0.027	N/s
Pr(Any qualifications)	-0.137*	-0.107*	0.079	Sig.
Pr(At least O-level)	-0.137*	-0.128*	0.017	N/s
Pr(No educ. beyond 16)	0.200*	0.168*	0.106	N/s
Pr(Inc. Supp. Recpt. at 30)	0.144*	0.056	-0.015	N/s
Pr(In employment at 30)	-0.077*	-0.037	-0.041	N/s
Pr(Top 2 social class at 30)	-0.106*	-0.097	0.054	N/s
Log(pay at 30)	-0.202*	-0.082	0.082	N/s
Pr(Has partner at 30)	-0.088*	-0.038	0.002	N/s
Pr(Partner no post-16 educ.	0.127*	0.118*	**0.244***	N/s
Pr(Partner in employment at 30)	-0.152*	-0.142*	**-0.184***	N/s
Log(Partner's pay at 30)	-0.154*	-0.230*	-0.280	N/s
Pr(Owner-occupier at 30)	-0.304*	-0.267*	**-0.330***	N/s

*Statistic at least twice its standard error

****From multivariate models that control for the age of the woman's mother in 1970, her household's social class at age 10, her mother's education and a summary scale of teacher's ratings at age 10.

Appendix

Bivariate Models

Table A1: Average Effect of a Teen-birth among Women having a Teen-birth

Outcome	α_0	α_P	α_{IV}	**Test**[a] α_P-α_{IV}
Pr(Regular Smoker at 30)	0.265*	0.075*	-0.019	N/s
Pr(2+ A-levels)	-0.270*	-0.176*	-0.026	Sig.
Pr(Any qualifications)	-0.263*	-0.214*	-0.080	N/s
Pr(At least O-level)	-0.298*	-0.230*	-0.112	N/s
Pr(No educ. beyond 16)	0.353*	0.211*	0.000	Sig.
Pr(Inc. Supp. Recpt. at 30)	0.229*	0.166*	0.074	N/s
Pr(In employment at 30)	-0.269*	-0.199*	-0.099	N/s
Pr(Top 2 social class at 30)	-0.254*	-0.180*	0.072	Sig.
Pr(Has partner at 30)	-0.003	-0.057	-0.011	N/s
Pr(Partner no post-16 educ.	0.223*	0.091*	0.151	N/s
Log(pay at 30)	-0.365*	-0.244*	0.099	Sig.
Pr(Partner in employment at 30)	-0.157*	-0.159*	**-0.204***	N/s
Log(Partner's pay at 30)	-0.249*	-0.252*	-0.241	N/s
Pr(Owner-occupier at 30)	-0.367*	-0.319*	**-0.194***	N/s

*Statistic at least twice its standard error

[a] 0.05 significance level

Table A2: Test of difference between α_0 and α_P

Outcome	***a***
Pr(Regular Smoker at 30)	0.200*
Pr(2+ A-levels)	-0.099*
Pr(Any qualifications)	-0.051
Pr(At least O-level)	-0.071*
Pr(No educ. beyond 16)	0.150*
Pr(Inc. Supp. Recpt. at 30)	0.066*
Pr(In employment at 30)	-0.073*
Pr(Top 2 social class at 30)	-0.077*
Log(pay at 30)	-0.126*
Pr(Partner in employment at 30)	0.002
Log(Partner's pay at 30)	0.003
Pr(Owner-occupier at 30)	-0.050

*Statistic at least twice its standard error

Table A3: 95% Confidence Intervals of Average Effect of a Teen-birth among Women having a Teen-birth

Outcome	α_0*		α_{IV}	
	Lower	**Upper**	**Lower**	**Upper**
Pr(Regular Smoker at 30)	***0.17***	***0.28***	-0.23	0.16
Pr(2+ A-levels)	***-0.18***	***-0.11***	-0.09	0.18
Pr(Any qualifications)	-0.22	-0.12	-0.15	0.20
Pr(At least O-level)	-0.23	-0.13	-0.17	0.20
Pr(No educ. beyond 16)	***0.20***	***0.29***	-0.18	0.19
Pr(Inc. Supp. Recpt. at 30)	0.15	0.24	-0.12	0.20
Pr(In employment at 30)	**-0.27**	**-0.17**	-0.29	0.08
Pr(Top 2 social class at 30)	***-0.25***	***-0.14***	-0.12	0.32
Log(pay at 30)	***-0.36***	***-0.20***	-0.04	0.42
Pr(Has partner at 30)	**-0.05**	**0.05**	-0.15	0.22
Pr(Partner no post-16 educ.	**0.12**	**0.22**	0	0.44
Pr(Partner in employment at 30)	-0.21	-0.10	-0.29	-0.12
Log(Partner's pay at 30)	**-0.27**	**-0.07**	-0.52	0.18
Pr(Owner-occupier at 30)	**-0.35**	**-0.25**	-0.37	0

* **Bold** indicates that the confidence interval of α_0 lies fully within the α_{IV} confidence interval, and ***Italics*** indicates that the confidence interval of α_0 lies outside the α_{IV} confidence interval.

Table A4: 95% Confidence Intervals of Average Effect of a Teen-birth among Women having a Teen-birth who are mothers at aged 30

Outcome	α_0*		α_{IV}	
	Lower	**Upper**	**Lower**	**Upper**
Pr(Regular Smoker at 30)	**0.15**	**0.27**	-0.11	0.31
Pr(2+ A-levels)	-0.12	-0.05	-0.09	0.14
Pr(Any qualifications)	-0.19	-0.08	-0.12	0.28
Pr(At least O-level)	**-0.19**	**-0.08**	-0.19	0.23
Pr(No educ. beyond 16)	**0.15**	**0.25**	-0.08	0.29
Pr(Inc. Supp. Recpt. at 30)	0.10	0.19	-0.19	0.17
Pr(In employment at 30)	**-0.13**	**-0.02**	-0.25	0.17
Pr(Top 2 social class at 30)	-0.17	-0.05	-0.13	0.24
Log(pay at 30)	-0.28	-0.12	-0.17	0.34
Pr(Has partner at 30)	**-0.14**	**-0.04**	-0.18	0.19
Pr(Partner no post-16 educ.	**0.07**	**0.18**	0	0.49
Pr(Partner in employment at 30)	**-0.20**	**-0.10**	-0.28	-0.08
Log(Partner's pay at 30)	**-0.26**	**-0.05**	-0.68	0.12
Pr(Owner-occupier at 30)	**-0.36**	**-0.25**	-0.52	-0.13

* **Bold** indicates that the confidence interval of α_0 lies fully within the α_{IV} confidence interval; there are no cases for which the confidence interval of α_0 lies outside the α_{IV} confidence interval.