The impact of children on women's labour supply and earnings in the United Kingdom: evidence using twin births*

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Abstract

The objective of this paper is to investigate the effect of children on women's labour supply and earnings for the United Kingdom. Estimating the causal relationship between family size and economic status is complicated because the same factors that increase family size may also affect the labour market outcomes of women. The birth of twins is used as an instrument to address this problem. Among women with children under age 13, the IV estimates indicate that a larger family induced by a twin birth adversely affects women's labour supply and earnings. Among women with older children, the IV results show no evidence of a causal effect, despite significant OLS relationships. Finally, we compare these results to estimates produced using a twins' sex composition instrument. Estimates using this instrument are very close to the estimates using twin births and imply that the labour market consequences of childbearing disappear over time.

JEL classifications: J22, J13, J12

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1. Introduction

Cross sectional studies generally find a negative correlation between the presence of children in the household and women's labour supply and earnings[1]. As fertility is largely a matter of choice, it is difficult to clearly conclude from simple cross sectional associations that there is a strong causal relationship between fertility and labour supply. It may be just that women select into childbearing on the basis of characteristics that we cannot observe which are also correlated with labour market participation and earnings. Therefore, is it true that having a large family has a causal effect on a woman's labour market activity? Or is it the case that women who choose to have more children are inherently different and would have lower labour market activity in any case? Understanding why children influence women's labour supply and earnings is critical to a number of policy debates. If it is the case that children lead to lower wages for women, then it may be that a policy that focuses on the supply of publicly-provided childcare would reduce the wage gap between women with and without children. But, if there is a selection effect, whereby women who choose to have children are less motivated to work in the labour market, then policies should target other issues (e.g., why women, and not men, have to make choices about children and employment).

The ideal way to address the potential problem of selection would require the random assignment of children to women so that all other differences are controlled. As no such experiment is available, authors have turned to three other methods to tackle the problem of the endogeneity of the fertility decision. The first strategy, largely used for the United States, is to estimate the determinants of fertility and labour supply using simultaneous equation

[1] For the United Kingdom, see for example, Joshi (1990), Joshi *et al*. (1996), Joshi (2002), Harkness and Waldfogel (2003), Sigle-Rushton and Waldfogel (2007).

models (Cain and Dooley, 1976; Fleisher and Rhodes, 1979; Hotz and Miller 1988). The second approach relies on the use of panel data and fixed effects models to address the fertility endogeneity problem. This approach has also been used for both the United States (Waldfogel 1998; Lundberg and Rose, 2000; Anderson *et al*. 2002; Gangl and Ziefle 2009), and the United Kingdom (Waldfogel 1995, 1998; Joshi *et al*. 1999; Gangl and Ziefle 2009). The third strategy, which is the one used in this paper, relies on a natural experiment to determine the economic consequences of childbearing. Multiple births have been used by a number of authors for the United States as an identifying instrument to consistently estimate the causal effects of fertility on labour supply (Rosenzweig and Wolpin 1980a; Bronars and Grogger 1994; Angrist and Evans 1998; Jacobsen *et al*. 1999; Cáceres-Delpiano 2006, 2012; Vere 2011). Sex preference and the sex composition of the first two children have also been used as instruments for family size (Angrist and Evans 1998; Cruces and Galiani 2007; Daouli *et al*. 2009; Ebenstein 2009)[2]. Miscarriage and infertility have been used by other researchers seeking to tackle the problem of fertility endogeneity in this context (Hotz *et al*. 2005; Cristia 2008; Agüero and Marks 2011; Ashcraft *et al*. 2013). In most of these studies, children are shown to have a causal effect on women's labour supply.

We believe that ours is the first attempt to use a large nationally representative dataset for the United Kingdom to investigate the hypothesis that an exogenous increase in the number of children through twinning negatively impacts women's labour supply and earnings. Our analysis is based on a large sample of twins taken from the 1986-2009 General Household Survey (GHS) for the United Kingdom. This source is attractive because as well as containing excellent retrospective information on entire birth histories for each female sample

[2] The same-sex instrument is based on the premise that parents have a preference to have a third child if the first two children are the same sex.

member, it contains information on the month and year of birth for each child that can be used to determine whether children were born as twins. Another advantage of our data is that we are able to generate a sizable twin-birth sample which we use to examine the causal effect of having an unplanned second, third and fourth child on women's labour supply and earnings separately. Earlier studies for the United Kingdom using panel data and fixed effects to control for endogeneity have mainly been concerned with estimating the motherhood 'pay penalty' and understanding the major determinants of this pay gap; there is limited evidence on the impact of children on other labour market characteristics (Waldfogel 1995, 1998; Joshi *et al*. 1999; Gangl and Ziefle 2009). Our work takes a more similar approach to the group of studies that rely on the birth of twins to identify exogenous changes in family size. Unlike previous research, we distinguish between full-time and part-time employment which is the most obvious job characteristic conducive to childcare and far more extensive in the United Kingdom than in the United States. This distinction is interesting not only because it provides deeper insights into women's labour market behaviour following the transition into parenthood, but also because it sheds light on the determinants of part-time status. Moreover, because our data includes adult children, we are able to investigate the long-term effects of childbearing in order to understand how women may eventually adjust over time to having an additional child. This paper also distinguishes itself by using more recent data than previous work as the impact of fertility may have changed over time. In doing so, we provide new and more comprehensive results on how the presence of children is related to women's labour supply and earnings than previous studies. Finally, we examine how the response of single mothers differs from that of married or unmarried cohabiting mothers to the arrival of another child. By looking at the differential response, we improve our understanding of the response of lone parents to an unanticipated child.

For married or unmarried cohabiting women, the evidence reveals that children have a negative effect on labour supply that is not exaggerated by OLS among women with young children (less than age 13). However, for single women, the effect of young children depends on the birth at which we study the increase in family size; specifically a negative impact is only observed as a result of delivering twins at first birth but no effect is observed at higher parities. For all women with mature children (over age 13), our IV estimates suggest that the lower labour market outcomes of women with older children cannot be attributed, in a causal sense, to additional childbearing. Rather, women with larger families who experience poorer labour market outcomes in the long run appear to have other characteristics that decrease their labour market participation and earnings.

This paper is structured as follows. The next section discusses the data and the twins instrument. Section 3 outlines our estimation strategy. Section 4 presents our results. Section 5 concludes.

2. Data

Data for this research is drawn from combining the General Household Survey for Great Britain between 1986 and 2009. The primary independent variable of interest is the number of children ever borne. Because retrospective fertility questions contained in the survey cover each female respondent's entire fertility history up until the survey year, information on the total number of children ever borne can be recovered, regardless of whether or not children are still living in their parents' home. This information is recorded for all women age 59 or younger. The GHS also includes information about the mother's age at each birth, birth order, gender, and crucially the month and year of birth of each child[3]. Multiple births can be easily identified as children born in the same month and year, which is precisely recorded in this survey. The empirical analysis is limited to women who have had at least one child by the time of the survey as the effects of fertility using the twins' methodology can only be estimated for women who have at least one child. The summary statistics ascertain that of a total of 77,376 women approximately 1,860 (2.40%) had given birth to twins (in first, second or third parity), which is consistent with the probability of twinning from population statistics.

To characterise women's labour force attachments four dependent variables are defined. The first is a dichotomous variable labelled *working for pay* that takes a value of 1 if an individual is in work and zero otherwise. The second and third dependent variables measure the intensity of work. These are a continuous variable labelled *hours per week* which measures hours worked per week, and an indicator variable labelled *part-time* which is equal to 1 if weekly hours of work are less than 35 hours per week. The fourth is labour market wages which are recorded as gross earnings per week[4]. Dependent variables are set to zero for those who did not work for pay during the survey year.

Education is reported in the GHS as the age the individual completed full-time continuous education. Therefore years of schooling is defined as age left continuous education minus five. Becker and Lewis (1973) predict that an increase in the education of mothers has a negative effect on the number of children as the cost of rearing and bearing children rises

[3] From 1998 onwards information on the date of birth of all children born to each female respondent is contained in the Special License Access files of the GHS.
[4] Wages are deflated to 1997 pounds using the Retail Prices Index.

with women’s wages. Thus years of schooling is introduced as a control variable in our regression models.

Although we expect that the increased value of women’s time at home after child birth negatively affects women’s labour market outcomes regardless of the presence of a partner, we report separate estimates for partnered and single mothers as we expect the effect of an additional child to vary by the presence of a partner in a way that is difficult to predict. Partnered mothers could have lower participation rates and wages than lone mothers for the following reasons. One is that partnered women may be more discouraged than single women from participating in the labour force by the wages of their partners. Single parents, who cannot fall back on an income provided by a partner, may be less likely to withdraw from the labour force, reduce their hours of work, or take part-time employment following the birth of a child. These factors may lead to a lower wage penalty for lone mothers relative to partnered mothers following childbirth. Another reason why the presence of a child may have a more pronounced affect on the wages of partnered mothers is linked to household income maximisation. It may be more difficult to optimise the career development of two individuals than just one if successful career development may require some degree of geographical mobility (Topel and Ward 1992; Himmelweit *et al*. 2013). The presence of a partner may limit mobility and couples may find it optimal to accommodate the careers of men over women. In essence, we expect that the traditional gender division of labour that may exist between couples would be related to the wage cost of having children. Moreover, recent research by Gimenez-Nadal *et al*. (2012) using data for several European countries found that social norms in relation to the gender division of labour deter women (especially highly educated women) with a high opportunity cost of time entering partnerships in the first place.

Thus, we may expect that lone mothers face a lower child pay penalty than partnered mothers if there is an element of selection into single motherhood.

However, it is not out of the question that the participation and wages of partnered women could be higher than those of single mothers. The greater the extent to which fathers share in parenting responsibilities, the more likely that it will be that partnered women whose household responsibilities are reduced by the presence of a partner can have more energy and time to devote to market activities than otherwise similar single mothers (Bryan and Sevilla-Sanz 2011; Becker 1985). The absence of a partner may imply that single mothers assume extra responsibility for their children which creates more conflict for lone mothers who seek to reconcile work and family life. If so, we might hypothesise that the wage penalty among partnered mothers should generally be lower than that experienced by single mothers. Of course, we may find no difference in participation or earnings by partnership status if employer discrimination or some other non-competitive mechanism were at work, unless employers single out one group of mothers for discrimination. It is therefore an interesting empirical question to examine how the effect of an additional child depends on the presence of a partner.

To study potential heterogeneity in the impact of family size, women are further disaggregated into two subsamples by the child's age. Specifically, in the analysis of twins at first birth, the data is broken out into women whose oldest child is younger than age 13, and age 13 or older at the time of the survey. In the analysis of twins at second birth, the data is broken down into women whose second child is younger than age 13, and age 13 or older at the time of the survey. In the case of twins at third birth, the data is similarly disaggregated into women whose third child is younger than age 13, and age 13 or older at the time of the survey. This disaggregation by the child's age (at each parity) reflects how the impact of an unplanned child on women's labour force outcomes changes with time since birth. There are at least three reasons why the effects of children on labour supply are likely to be greatest when children are young and diminish as children mature. The first is that the cost of alternative care falls as children age. The second is that mothers may strongly value personally rearing their children especially when they are infants. The third is that the time spent with a young child may be more exhausting than the same number of hours spent with an older child (Becker 1985). Thirteen is chosen as the horizon as prior research finds that the effect on labour supply of an additional child disappears by the time the child turns 13 (Bronars and Grogger 1994; Angrist and Evans 1998; Jacobsen *et al*. 1999; Paull 2008). Therefore, the interval between 0 and 13 years after the birth of twins seems to be the most appropriate for estimating the total effect of an unplanned birth on women's labour supply and earnings.

In order for our IV estimates to be consistent it must be that the occurrence of a twin birth is random and in particular unrelated to unobservable family characteristics that may influence the work effort. Although this is not testable, we examine whether the presence of twins is related to observable characteristics. Table 1 reports means and standard deviations on important characteristics for twin and non-twin mothers by the first child's age group (under or over age 13) for the entire sample of women. The first row of table 1 indicates that twin mothers have on average almost one child more than non-twin mothers. In the sample of young children, twin mothers also appear to be about one year older at their first birth compared to non-twin mothers. These statistics reflect the positive biological relationship between age at birth and the probability of twinning (Mittler 1971). In the sample of older children, the age gap between twin and non-twin mother is about one third of a year on average, reflecting the fact that this is a much older sample of women. On average women are 47.5 years old in the sample of children age 13 or over as compared with 32.12 in the sample of children age 13 or younger. The remaining rows of the table show the other socioeconomic variables. Mean labour force participation, hours of work per week, and earnings are all lower for twin mothers than non-twin mothers with a young first child. However, in the sample with children over age 13, twin and non-twin mothers appear quite similar in terms of these socioeconomic characteristics. Regardless of child's age group, average years of schooling, which may be arguably viewed as a measure of long-term socioeconomic status, are virtually identical for twins and non-twins mothers. These descriptive statistics suggest that the probability of twins is unrelated to parental characteristics, with the exception of mother's age at first birth. This is not a serious problem as it can be addressed by entering mother's age at birth as a control variable in our regression models.

3. Empirical Strategy

The objective of this paper is to distinguish the true causal effect of family size on women's labour market outcomes. OLS estimates may be difficult to interpret since fertility is likely to be endogenous with respect to tastes for work. In the present study the twins natural experiment is used to instrument for family size. There are two conditions for the validity of the twin birth instrument. The first requires that the instrument is correlated with the number of children in a household. Past research and our descriptive statistics indicate that this is indeed the case. The second states that the twins instrument is virtually random and increases with the number of children beyond the desired family size that would have otherwise been achieved (Rosenzweig and Wolpin 1980b). The event of a twin birth is not necessarily independent of desired family size; women who desire larger families would be more likely to have twins. Therefore, simply pooling twins across birth order would tend to oversample women who had several births which in general would result in oversampling women who desired larger families.

Our approach to purging the effect of a twin birth on labour market outcomes from any link between the occurrence of twins and the number of pregnancies is to examine twin births at first, second and third pregnancies separately[5]. Thus, the analysis focuses on three subsets of women who had twins at first, second and third birth to provide a sense of how the effects of an unanticipated child may increase family size at different parities. We therefore estimate the following IV model for the sample of women with at least *k* children:

$$y_i = \beta_0 + \beta_1 n_i^k + \beta_2 X_i + \mu_i \qquad (1)$$

$$n_i^k = \alpha_0 + \alpha_1 twins_{ji} + \alpha_2 X_i + \varepsilon_i \qquad (2)$$

Equation (2) represents the first-stage of the IV estimation, where equation (1) is the second stage. The dependent variable y_i is a measure of labour market activity. The vector of control variables X includes mother's age at the time of birth, age at survey year, years of schooling, and survey year dummies. Maternal age at birth is an important control because the probability of a twin birth increases with age but also because maternal age at first birth is related to labour market participation. The n^k_i denotes the total number of children given birth to by individual i in the subsample of women with at least k children ($k \geq 1$, $k \geq 2$; $k \geq 3$). The instrumental variable is $twins_{ji}$ which is a dummy variable for whether the j^{th} birth was a twin birth for person i. For the sample of women with at least one child, the twins instrument is twins first, $twins_{1i}$ which is set to one if a woman had twins at first birth and zero if a singleton. In this way, we are able to identify the marginal effect of moving from one to two

[5] Alternatively, Rosenzweig and Wolpin (1980b) recommend using the number of twin births divided by the number of pregnancies. This "twins ratio" standardized by parity imposes less severe data requirements.

or more children. For the subsample of women with at least two children, the instrument is redefined to denote twins second $twins_{2i}$ which takes a value of one if the second birth in the family is a twin birth and zero otherwise. The twin-second instrument allows us to examine the marginal effect of going from a family of two to three or more children. By restricting the sample to families with at least two children, we are making sure that preferences over family size on average are the same in families with twins or singletons at the second birth. For the fraction of women who have had at least three children, the instrument is defined as twins-third $twins_{3i}$ and takes a value of one if the third birth is a twin birth and zero if it is a singleton. The twins-third instrument allows us to measure the effect of a fourth child who is born as a consequence of twinning. Once again by focusing on the subsample of women with at least three children we can avoid the possible problem that women who choose to have more pregnancies are more likely to have at least one twin birth.

There are a number of other possible problems with the use of twin births as an instrument. One concern is that the propensity to give birth to fraternal twins runs in families. Hyperovulation, where two eggs instead of one are released at the time of ovulation, is hereditary. Since women are unlikely to know whether or not they carry this gene, this factor is unlikely to have implications for the twin birth instrument.

A second possible objection to the use of twin births as an instrument is that the incidence of having twins is known to be higher in black African women than among women of other races (Myrianthopoulos 1970). In our data, the proportion of black African women is too small to meaningfully disaggregate the data by race. However, as a check on the robustness of our main results, in analysis not reported, we found that limiting our sample to white women did not materially alter our regression results presented below[6].

A third concern with the use of twin births as an instrumental variable for childbearing is the use of assisted reproductive technologies, specifically *in vitro* fertilisation techniques, which are known to increase the likelihood of a twin birth. Unlike the United States, in the United Kingdom *in vitro* fertilization techniques and other fertility-enhancing drugs are covered for all women under the National Health Service (NHS). Thus, a much larger proportion of the fertility variable that is generated by twin births is likely to be random for the United Kingdom than the United States. However, the increased use of fertility treatments particularly among older couples of higher socioeconomic status in both countries may have material implications for the population identifying the instrumental variable estimates in more recent years. Although instrumental exogeneity by definition cannot be tested directly, to try to gauge the importance of this issue as a threat to the internal validity of our results, we can examine whether the probability of twins at first, second and third birth is related to observable characteristics such as mother's education by estimating linear probability models of the likelihood of a twin birth at each point in the birth order with controls for mother's age at first birth and survey year. In each of these regressions the coefficient on mother's education is negligible and not statistically significant at any conventional level of significance[7]. While this is reassuring as socioeconomic status should be highly correlated with years of education, it is important to bear in mind when interpreting our results that the

[6] In the GHS over 95% of women are white with other races making up the balance.
[7] For women with at least one child, a regression of twins at first birth yields a coefficient (standard error) on mother's years of schooling of - 0.0001631 (0.0002018). For women with at least two children, the equivalent coefficient is - 0.000176 (0.000238). Finally, for women with at least three children, the coefficient is - 0.0001846 (0.0004243).

decision to seek fertility treatment may be linked to attitudes to labour supply. Given the absence of information about fertility treatment in the GHS, this issue cannot be solved directly.

A final concern with the use of twin births to instrument for family size is that the increased occurrence of twins in recent years has been linked to the use of bovine growth hormone, or bST (Steinman 2006). This hormone is largely used in the United States to increase cattle yields of milk and meat. Consuming dairy products, particularly milk, containing this hormone is associated with increased rates of multiple ovulation. This hormone was banned in the UK in 1990, though our data extends to children born prior to this legislation. Since almost everyone consumes dairy products, this factor should not lead to biases in the twins-based IV estimates.

In addition to IV estimates we also provide OLS estimates of the relationship between women's labour market outcomes and family size. In general, the OLS regressions only yield consistent estimates of increments to family size if there is no correlation between the error term and family size in equation (1), conditional on the set of control variables. OLS estimates will be biased if there is a correlation between family size and some characteristic (e.g. career ambition) excluded from the control vector that also affects labour market outcomes. If this omitted variable were positively correlated with labour market success and negatively correlated with the number of children, excluding it from the OLS regression would bias estimates upwards (in absolute terms) as part of the estimated negative effect of children is not in fact due to family size. The IV approach should provide a solution to this problem if it can be successfully argued that a twin birth is exogenous to the error term and correlated with family size.

4. Results

Tables 2 through 4 set out our main results for women with at least one child, at least two children, and at least three children, respectively. The first and second columns report the estimates for partnered and single women with a child younger than age 13. The corresponding effects for the sample of women with a child older than age 13 are shown in the last two columns. In order to save space, the tables present only the coefficients on the number of children in equation (1) along with the first-stage coefficients on the twins instrument in equation (2).

The estimates presented in the first row of table 2 show the impact of twins at first birth on the number of children ever born. For partnered women whose first pregnancy has resulted in twins the coefficient (standard error) is 0.697(0.039), which gives a t-statistic of 17.9. For single women, the equivalent estimate is somewhat smaller 0.613 (0.088), with a t-statistic of 6.9[8]. A t-statistic less than 5 is indicative of the weak instrument problem, therefore our IV models should be powerful enough to detect the labour market consequences of child-bearing. For women whose oldest child was born more than 13 years ago, the effect of twins at first birth drops considerably to about 0.5 additional children[9]. This effect is virtually identical for partnered and single women but it is noteworthy that the standard errors for the samples of single women are about twice as large as those in the sample of partnered women. The finding that a twin birth in the first pregnancy results in a much smaller impact on family size in the long-run is related to the fact that families in time adjust their fertility in response

[8] The first-stage regressions were initially run without controls for survey year, mother's age at the time of survey, and years of schooling. The results from this specification are virtually identical to those reported here, suggesting that twinning is not correlated with other covariates in the model.
[9] The associated t-statistics are 10.2 for the partnered sample and 5.4 for the single sample.

to the impact of a twin birth. While not directly comparable these first-stage results are within the range of estimates provided by Jacobsen *et al*. (1999) for the United States. These authors found that the effect of twins at first birth steadily declines with the age of the first child but level off at about 0.6. The more pronounced decline in the impact of twins at first birth on family size for the UK may reflect the more recent data used in this study and the fact that fertility has been generally declining over time.

We now turn to our labour market estimates of the effects of family size which are listed in the remaining rows of table 2. Inspection of the results for all labour market indicators shows that the effect of an increase in the number of children differs considerably by whether the additional child is younger or older than age 13. Consider the decision to work for pay among women in the sample of children less than age 13. For partnered women, the OLS results suggest that additions to family size reduce employment by 14.9 percentage points. The equivalent IV estimate that instruments for fertility using twinning at first birth is approximately 8.7 percentage points. For single women, the OLS and IV point estimates are of the same order of magnitude and reveal that the probability of working for pay is reduced by approximately 14 percentage points as a result of an unplanned child.

Among both partnered and single women with an older child, the OLS estimates on labour market participation are between 5.5 and 6.9 percentage points, implying that as children mature the influence on women's participation is reduced. The equivalent IV estimates are the reserve direction of what is expected and statistically insignificant. These findings suggest that there is no persistent adverse effect of increased family size on labour market participation, once fertility endogeneity is taken into account.

Other labour market indicators give further evidence of the difference that an extra child makes. Focusing on work intensity, the OLS results show that an extra young child on average reduces work by 5.2 hours per week for partnered women and 4.4 hours per week for single women. An additional young child increases the probability of holding part-time work (less than 35 hours per week) by 9.1 percentage points for partnered women and 8.2 percentage points for single women. For women with mature children these effects are generally somewhat smaller in magnitude though not always statistically different from those for young children.

For partnered women, the IV results suggest that having an unanticipated child who is younger than age 13 results in a statistically significant reduction in work of 3.8 hours a week. For single women, the equivalent IV estimate is higher at 4.7 hours a week. It is noteworthy that these results for women with young children are almost identical in magnitude to the corresponding OLS coefficients, suggesting that in this sample omitted variables do not matter. In relation to part-time employment, the IV estimates suggest that an additional child increases the likelihood of working part-time by approximately 9 percentage points for partnered women. The equivalent IV estimate for single women is similar in magnitude to that for partnered women but not statistically significant. The positive effect of children on part-time employment is consistent with the explanation that mothers of young children are more likely to demand jobs that fit within the responsibility of child care.

Although our results for the sample of women with young children are not directly comparable they are in broad agreement with those found by Vere (2011) for the United States. Using the dummy variable ‘more than two children’ as opposed to the number of children, he found that the causal effect of an additional child induced by twins at first birth reduced the probability of employment by approximately 13 percentage points and work by 6 hours per week.

For partnered women with a mature child, the OLS results suggest that women with one additional older child work 2.4 fewer hours per week and are 5.6 percentage points more likely to work part-time. The analogous estimates for single women are also statistically significant but somewhat greater (in absolute terms) in magnitude. However, failure to account for endogenous fertility in these regressions leads one to overstate (in absolute terms) the size of the effect of children on labour supply. The insignificant IV estimates on both hours and part-time status are now opposite in sign to the ones obtained for women with a young child and reflect no reduction in labour supply as a result of increased fertility. In summary, this suggests that omitted variables impart considerable upward bias (in absolute terms) to OLS estimates for women with older children. Moreover, the results imply that the detrimental effects of an exogenous increase in family size are transitory and disappear as children mature.

Turning now to labour market wages, in general the OLS estimates show that more children in a family have a negative effect on labour market wages when children are young. These coefficients are very precisely estimated and quite similar for partnered and single women. However, the equivalent IV estimates diverge significantly by partnership status. For partnered women, the IV estimate is roughly half that of the OLS estimate and not statistically significant[10]. For single women, the IV estimate implies a remarkably large wage

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[10] The previous literature for the UK generally documents a significant wage penalty for motherhood. For example, Waldfogel (1998) using the NCDS and fixed effects analysis finds that the pay penalty for one child is 11% and for two or more children 19%.

penalty of £59 per week as a consequence of an unplanned child, which is at least marginally statistically significant with a t-statistic of 1.64. This amounts to about half of average weekly earnings for single women with two or more children. One explanation for these results is that an extra child may lower weekly earnings for single women who may bring less energy to the market work relative to partnered women who can rely on their partners for child care (Becker 1985).

For women whose first child is more than age 13, the OLS estimates suggest a pay penalty of £18 for partnered women and £21 for single women. For both partnered and single women, the IV estimates are entirely statistically insignificant indicating no long-term wage ramifications of increased family size. Overall, it appears that we need to be careful about interpreting the OLS results for women whose first child is older than 13 as revealing the true long-run response to an unplanned birth. The fact that children neither decrease labour supply nor negatively impact women's wages in the long run is in line with research for the United States by Jacobsen *et al*. (1999). These authors found that the effect on labour supply and earnings of an exogenous fertility shock caused by the occurrence of twins in the first birth were transitory and had vanished by about the time the first child was 10 years old.

Table 3 replicates the regressions of table 2, but uses data for the subsample of women with more than 2 children and twins at second birth as the instrument to generate exogenous variation in the number of children. Estimates from the first-stage regressions linking twinning at second birth to family size are shown in the top row of table 3. The effect of twinning at second birth is to increase family size by 0.897 (0.036) children for partnered women and 0.859 (0.080) children for single women. For both partnered and single women whose second child is over age 13 the corresponding increment to family size is virtually identical at approximately 0.78 children. These long-run first-stage effects are much steeper than the corresponding estimates for twins at first birth presented in table 2, presumably because twins at second birth are more likely to push families above their desired number of children. The t-statistics for the first stage range from 7.7 to 26.9, suggesting that the twins-second birth is not a weak instrument for family size in our IV models. Moreover, these estimates are in line with those reported by Angrist and Evans (1998) using twins at second birth to instrument for additional childbearing for the United States. They found that having twins at second birth increase the number of children by 0.809 (0.014).

The labour market effects are reported in subsequent rows. The OLS and IV results for families with two or more children generally parallel the pattern of results in table 2. Across all samples, the OLS results show that an additional child has a negative and precisely determined effect on women's labour supply and earnings. The estimated coefficients are close in magnitude to the equivalent results presented in table 2, and there are no notable differences in OLS coefficients by partnership status.

For women with a young child, the IV results for partnered and single women differ somewhat. For the sample of partnered women, the IV estimates indicate that the presence of an extra child induced by twins at second birth reduces the probability of working by 6.0 percentage points, causes hours of work per week to fall by 2.26 hours, raises the likelihood of part-time work by 5.0 percentage points, and results in an wage penalty of £19.62 per week. Except for wages, these estimates are about half that of the equivalent estimates presented in table 2 and imply that the causal effect of fertility declines with child parity. The smaller marginal effect of an unplanned child at second birth may arise if decreases in labour supply are largest following the first child. It is worth noting that our estimates are comparable in magnitude to those for the United States by Angrist and Evans (1998) who report a coefficient on ‘worked for pay’ of -0.063 (0.018) and on hours per week of -2.83 (0.73). In addition our results in relation to wages are generally consistent with the previous literature for the UK which has consistently revealed that the wage losses of motherhood tend to increase with the number of children (see, for example, Waldfogel 1995, 1998; Harkness and Waldfogel 2003).

For the sample of single women with a young child, the IV estimates are attenuated across all labour market indicators and never larger than their standard errors. These relatively small and statistically insignificant results are contrary to our priors, which predict that young children negatively impact women’s labour supply and earnings. Nevertheless these results are in line with the argument that single mothers have a larger incentive to work than partnered women who can rely on their husband’s earnings. Our findings for single women are also in broad agreement with the direction of results reported by Vere (2011) for the United States. He found that the effects of fertility on single women’s labour supply had declined markedly over time while remaining stable for partnered women. Our estimates are also consistent with earlier estimates for Britain by Gangl and Ziefle (2009) who report a statistically insignificant wage penalty per child for single mothers compared to 13.5% wage penalty faced by married women.

Regardless of partnership status, for women whose second child is more than age 13, IV estimates on all indicators of labour market activity tend to be smaller than OLS and never statistically significant. Thus, adding another child to a family as a result of a second twin birth has no impact on women’s labour supply and earnings in the long run. This is also true for both partnered and single women who experienced twins at first birth. Thus, these results appear to confirm our earlier finding that OLS estimates are upward biased by failure to account for endogenous fertility in the long-run impact of children on women's labour market outcomes.

In the next part of the analysis we remove families with one or two children from the sample and repeat our regression models for families with at least three children. These results are contained in table 4. Interestingly, the pattern of estimates for the effect of twins at third birth on the number of children is virtually the same as for the effect of twins at second birth. In the sample of partnered women with children younger than age 13, the first row of the table indicates that on average women with twins at third birth have 0.882 (0.045) more children than those with singletons. The equivalent estimate for single women is approximately the same at 0.866 (0.110). For partnered women with an older set of twins, the estimate is 0.814 (0.054) and for single women the estimate is 0.770 (0.108). The t-statistics for the significance of the instrument range from 7.1 to 20.9.

The remaining rows of the table display the OLS and IV estimates of the impact of children on labour market status. In generally the estimates reveal a similar pattern to the results presented earlier. For both child age samples, the OLS point estimates show that the effect of having another child is similar in sign and magnitude across all four labour market outcomes to those produced in tables 2 and 3. These estimates show that an extra child has a negative effect on women's labour supply and wages, regardless of partnership status and child's age.

In contrast to the OLS estimates, the IV estimates differ by partnership status. For partnered women, the IV estimates show that an exogenous additional child that arises from twinning at the third birth lowers employment rates by14.4 percentage points, raises the average probability of being in part-time work by roughly 5.7 percentage points, and reduces mean hours of work by 3.24 hours per week. Although the IV estimates suggest that another child lowers wages by approximately £24.83 a week, the coefficient is not statistically significant because of the large standard error. For single women, the analogous IV results are small and insignificantly different from zero. These results are quite similar to those reported in table 2. For women whose third child was born more than 13 years ago, the statistically insignificant IV estimates suggest that the initial decline in labour market activity due to a twin birth has no permanent influence on market activity. This finding is in consensus with our analogous results for twins at lower parities.

In interpreting our results it should be borne in mind that IV estimates do not identify the average causal effect, but identifies a Local Average Treatment Effect for the subpopulation influenced by the instrument (Imbens and Angrist 1994; Ebenstein, 2009). Therefore our IV estimates identify the average causal effect for those individuals who were forced to have an extra child due to the occurrence of twins. Angrist *et al*. (1996) refer to this group as the subpopulation of *compliers*. Differences in the size of the first stage across parity points measure differences in the probability of compliance between groups of women who experience twins. For the samples of women with at least two children or at least three children, the large first stage coefficients for the twin instruments (0.8 - 0.9) do not differ significantly from 1, implying that the twins instrument has almost perfect compliance. That is, the birth of twins at second and third parity leads to one extra child in the vast majority of households in both the short and the long run. However, for twins-first mothers the first-stage coefficient is 0.7 in the sample of young children and only 0.5 in the sample of mature children, which are significantly different from unity indicating that compliance is less than complete. Angrist and Evans (1998) find that parents of same-sex children are more likely to go on to have an additional child. Therefore, fertility adjustments among twin-first mothers may differ by the sex composition of the twins.

In the case of first-born twins, because the sex of first-born twins is virtually randomly assigned, an indicator variable for whether the sex of the twins is identical or mixed provides an additional plausible instrument for further childbearing. This instrument is based on the premise that twins-first parents have a preference for a mixed sibling-sex composition. Thus, parents of identical twins at first birth will be more likely to try for a third child than parents of opposite sex children.

One possible problem with the exclusion of children's sex from labour supply equation is the existence of secular effects of child sex on family life. Reasons for such effects include the possibility of male children increasing the father's commitment to the family (Morgan *et al*. 1988). It is also possible that there could be secular impacts of sex mix generated by the fact that boys are more likely than girls to be born with disabilities. For families with at least one child, to avoid the possibility of bias arising from any secular effects of child sex on family life, twins are grouped by three dummy variables: *twins first – two boys*, *twin first – two girls*, and *twin first - mixed sex*. This in effect constitutes a model with variable treatment intensity, where the effects of twins on family size are greatest when they have the same sex.

The first stage effects reported in table 5 appear to confirm expectations that parental preference for balanced families leads them to be more likely to increase family size when twins are the same sex. The coefficients on *twins first – two boys* and *twins first – two girls* are generally larger than the coefficients on *twins first – mixed sex*. There is also suggestive evidence that having twin boys increases family size more than twin girls. The most notable result is for single women in the sample of mature children. For these women the *twins first – mixed sex* coefficient is not statistically significant implying no additional children following the initial birth of opposite sex twins.

Table 6 reports the second-stage 2SLS estimates. We also include for comparison our earlier IV estimates based on twins at first birth. As it turns out, separating the components of the twins-first instrument by sex does not change the coefficient estimates or lead to an increase in precision in either the sample of women with children younger or older than age 13. In the sample of young children the only important exception is for single women where the coefficient on earnings no longer carries even a marginally statistically significant penalty. In the sample of mature children, the only exception for partnered women is that the coefficient on part-time employment is now statistically significant indicating that these women are more likely to move into full-time employment as their children mature. In all other cases, the IV estimates are almost completely invariant to the choice of instrument.

This analysis was repeated for families with at least two children where twins at second birth were grouped into three dummy variables: *twins second – three boys*, *twin second – three girls*, and *twin second - mixed sex*. In this case, *twins second – three boys* is an indicator variable that takes a value of 1 if male twins at second birth were part of a family where the first child was a boy, *twins second – three girls* is a dummy variable that takes a value of 1 if female twins at second birth were part of a threesome of girls, and *twins second – mixed sex* is a dummy variable that is set to 1 if twins are part of a mixed-sex sibling composition. The premise is that parents who have same sex children will keep having children to pursue a more balanced sex constellation. The unreported results from this analysis, compared with those presented in table 3 using the standard twins-second instrument, provide no evidence that it matters which instrument is used. This is unsurprising given that the first-stage results in table 3 using the standard twins-second birth instrument illustrate almost perfect compliance.

5. Conclusion

We have attempted in this paper to provide causal estimates of the effect of increments to family size on women's labour market outcomes that are free of the major sources of bias presented in earlier cross-sectional studies. There are three principal findings that warrant emphasis.

First, our IV estimates generally confirm our OLS estimates that find a negative effect of larger families on women's labour market outcomes among women whose children are younger than age 13. The fact that our IV results are based on the incidence of twins gives us considerable confidence that the effects of parenthood on women's labour supply and earnings are causal rather than arising from some unobserved factor, such as lower-productivity women having larger families. Our second instrumental approach that utilised the sex composition of twins provides corroborating evidence on this point.

Second, a comparison of IV estimates across parity points indicates that the effect of young children on labour supply and earnings is largely concentrated at first parity in our sample of single mothers. Among single women there is no statistically significant relationship between additional children and labour outcomes for births at second and third parity. This implies that gains in family size at these parities do not translate into poorer labour market outcomes for single women. In contrast, among partnered women, we observe statistically significant effects of children on labour market outcomes at all parity points considered in this study.

Third, the estimates we obtain from the twins approach appear to suggest that OLS seriously overestimates (in absolute terms) the long-run negative effects of an extra child on female labour supply and earnings. In general the OLS estimates suggest that more children in a family have a negative effect on women's labour supply and earnings that does not diminish as children mature. However, IV estimates, which find no negative effect of larger families on any indicator of labour market status among women whose children are older than age 13, indicate that these effects are transitory. This result holds for both single and partnered women.

In considering the design of policy, four conventional reasons have been advanced to explain why children reduce women's labour supply and earnings[11]. First, the presence of children may damage the accumulation of human capital through a reduction in work experience, tenure and training, and a reduced incentive to invest in human capital that may depend upon future work. A second explanation is that children may interfere with the earnings capacity of women through the diversion of energy away from market activities to childrearing responsibilities (Becker 1985). A third possibility is that wage differences linked to children may reflect compensating wage differentials associated with the adaptation of market work to family life. Finally, even if childcare has no real effect on productivity, employers may discriminate against women by the number of children if they perceive that there is a conflict between work and family size.

[11] Although little is known about the effects of parenthood on men's labour market outcomes, previous work suggests no detrimental relationship exists between parenthood and labour market outcomes for men (Angrist and Evans 1998; Joshi 1998; Lundberg and Rose 2000, Paull 2008, Vere 2011).

Neoclassical models of labour market and fertility behaviour suggest that social policies targeted at reducing the opportunity cost of children would moderate the negative impact of children on women's labour supply and earnings. This may happen through family allowances, parental leave schemes, and publicly-provided child care. Since some policy interventions designed to protect mothers may have harmful consequences, future research should carefully investigate the possible consequences of various policy interventions for the career development of women with children (Blau and Ehrenberg 1997; Ruhm 1998). However, it is possible to speculate that compared to most other policies, publicly-provided childcare would probably have the most powerful and unambiguous effect on reducing the duration of work time lost due to childbearing. Reliable childcare may also help to increase the productivity of women at work and reduce statistical discrimination. How efficacious such a policy would actually be depends on how responsive the labour supply of women is to the price of child care. A strong case can also be made for other and related forms of support especially the strengthening of flexible working rights for women with children (Goldin 2014). Yet, it is still unlikely that these forms of support alone will be sufficient to completely surmount the difficulties women with children face in competing in the labour market without a transition towards more egalitarian parenthood policies that give both men and women equal facilities to combine employment and family commitments (de Laat and Sevilla-Sanz 2011; Gimenez-Nadal and Sevilla 2012). Such a development, we believe, would have major consequences for the traditional gender division of labour, the intrafamily allocation of time, and decisions concerning fertility.

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Table 1.
Summary statistics for all women
Variable means (standard deviations)

	All children		First child's age <= 13		First child's age > 13	
	Non-twin mothers	Twin mothers	Non-twin mothers	Twin mothers	Non-twin mothers	Twin mothers
Number of children	2.224	3.181	1.839	2.850	2.457	3.375
	(0.982)	(0.921)	(0.807)	(0.787)	(1.005)	(0.939)
Mother's age at first birth	23.780	24.408	25.253	26.389	22.887	23.245
	(4.702)	(4.985)	(5.185)	(5.534)	(4.135)	(4.222)
Mother's age at survey	41.724	42.461	32.122	33.804	47.540	47.543
	(10.032)	(9.445)	(5.919)	(5.915)	(7.121)	(7.157)
Working for pay	0.615	0.590	0.539	0.469	0.662	0.661
	(0.487)	(0.492)	(0.498)	(0.499)	(0.473)	(0.473)
Hours per week	16.745	15.518	13.362	10.776	18.794	18.301
	(16.158)	(15.586)	(15.054)	(13.554)	(16.456)	(16.028)
Part-time status	0.764	0.795	0.838	0.895	0.719	0.735
	(0.424)	(0.404)	(0.368)	(0.306)	(0.449)	(0.441)
Gross earnings per week	130.189	127.222	117.627	109.778	137.799	137.462
	(191.707)	(183.960)	(193.495)	(188.668)	(190.215)	(180.436)
Years of schooling	11.593	11.669	12.227	12.228	11.209	11.341
	(2.028)	(2.001)	(2.089)	(2.046)	(1.889)	(1.900)
Sample size	75,516	1,860	28,487	688	47,029	1,172

Table 2.
OLS and IV estimates of labour supply models.
Women with 1 or more children

	First child's age <= 13		First child's age > 13	
	(1)	(2)	(3)	(4)
	Partnered	Single	Partnered	Single
First-stage estimates of family size				
Twins - first	0.697***	0.613***	0.502***	0.506***
	[0.039]	[0.088]	[0.049]	[0.094]
OLS and 2SLS estimates				
Worked for pay (OLS)	-0.149***	-0.136***	-0.055***	-0.069***
	[0.004]	[0.008]	[0.003]	[0.004]
Worked for pay (IV - twins first)	-0.087**	-0.145*	0.031	0.068
	[0.038]	[0.084]	[0.049]	[0.089]
Hours/week (OLS)	-5.187***	-4.404***	-2.418***	-3.073***
	[0.133]	[0.235]	[0.086]	[0.156]
Hours/week (IV - twins first)	-3.819***	-4.690*	1.414	1.511
	[1.138]	[2.532]	[1.681]	[3.116]
Part-time (OLS)	0.090***	0.082***	0.056***	0.080***
	[0.003]	[0.006]	[0.002]	[0.004]
Part-time (IV - twins first)	0.091***	0.070	-0.069	-0.087
	[0.029]	[0.065]	[0.047]	[0.088]
Earnings/week (OLS)	-35.868***	-31.874***	-17.530***	-20.607***
	[1.709]	[2.761]	[0.957]	[1.800]
Earnings/week (IV - twins first)	-17.165	-58.546*	14.184	56.526
	[14.670]	[29.943]	[18.440]	[37.343]
Observations	23,624	5,551	37,626	10,575

Notes: Huber-White's robust standard errors are reported in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 3
OLS and IV estimates of labour supply models.
Women with 2 or more children

	Second child's age <= 13		Second child's age > 13	
	(1)	(2)	(3)	(4)
	Partnered	Single	Partnered	Single
First-stage estimates of family size				
Twins - second	0.897***	0.859***	0.789***	0.787***
	[0.036]	[0.080]	[0.048]	[0.102]
OLS and 2SLS estimates				
Worked for pay (OLS)	-0.173***	-0.164***	-0.072***	-0.087***
	[0.006]	[0.010]	[0.003]	[0.006]
Worked for pay (IV - twins second)	-0.060*	-0.053	0.013	0.000
	[0.032]	[0.064]	[0.034]	[0.070]
Hours/week (OLS)	-4.992***	-4.451***	-2.852***	-3.858***
	[0.163]	[0.303]	[0.115]	[0.218]
Hours/week (IV - twins second)	-2.258**	-1.181	-0.338	0.177
	[0.931]	[1.894]	[1.167]	[2.502]
Part-time (OLS)	0.065***	0.060***	0.058***	0.099***
	[0.004]	[0.008]	[0.003]	[0.006]
Part-time (IV - twins second)	0.050**	0.034	0.031	-0.018
	[0.023]	[0.047]	[0.032]	[0.068]
Earnings/week (OLS)	-31.326***	-33.479***	-20.458***	-24.987***
	[2.061]	[3.781]	[1.277]	[2.386]
Earnings/week (IV - twins second)	-19.618*	-7.793	-11.699	-13.398
	[11.706]	[23.441]	[12.839]	[26.869]
Observations	19,738	4,265	27,224	7,430

Notes: Huber-White's robust standard errors are reported in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 4
OLS and IV estimates of labour supply models.
Women with 3 or more children

	Third child's age <= 13		Third child's age > 13	
	(1)	(2)	(3)	(4)
	Partnered	Single	Partnered	Single
First-stage estimates of family size				
Twins - third	0.882***	0.866***	0.814***	0.770***
	[0.045]	[0.110]	[0.054]	[0.108]
OLS and 2SLS estimates				
Labour force participation (OLS)	-0.176***	-0.111***	-0.080***	-0.097***
	[0.010]	[0.017]	[0.007]	[0.012]
Labour force participation (IV - twins third)	-0.144***	-0.062	-0.031	0.171
	[0.047]	[0.103]	[0.048]	[0.105]
Hours/week (OLS)	-4.797***	-2.775***	-2.829***	-3.877***
	[0.294]	[0.493]	[0.233]	[0.411]
Hours/week (IV - twins third)	-3.244**	-0.458	-1.311	4.742
	[1.356]	[2.965]	[1.601]	[3.572]
Part-time (OLS)	0.053***	0.021*	0.048***	0.094***
	[0.007]	[0.011]	[0.006]	[0.011]
Part-time (IV - twins third)	0.057*	-0.014	0.023	-0.025
	[0.032]	[0.068]	[0.042]	[0.088]
Earnings/week (OLS)	-30.450***	-20.498***	-20.510***	-27.519***
	[3.393]	[5.632]	[2.294]	[3.951]
Earnings/week (IV - twins third)	-24.837	-8.594	-20.198	10.226
	[15.621]	[33.761]	[15.760]	[32.711]
Observations	7,925	2,168	10,517	3,325

Notes: Huber-White's robust standard errors are reported in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 5
First-stage estimates: Twins-sex composition instrument
Women with 1 or more children

	First child's age <= 13		First child's age > 13	
	(1)	(2)	(3)	(4)
	Partnered	Single	Partnered	Single
First-stage estimates of family size				
Twins first - two boys	0.691***	0.677***	0.567***	0.692***
	[0.068]	[0.141]	[0.081]	[0.151]
Twins first - two girls	0.669***	0.530***	0.472***	0.546***
	[0.064]	[0.174]	[0.086]	[0.164]
Twins first - mixed sex	0.696***	0.449**	0.462***	-0.111
	[0.089]	[0.185]	[0.109]	[0.241]
F-statistic	89.560	12.620	32.210	10.770
Observations	23,624	5,551	37,626	10,575

Notes: Huber-White’s robust standard errors are reported in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 6
2SLS estimates using different instruments
Women with 1 or more children

	First child's age <= 13		First child's age > 13	
	(1)	(2)	(3)	(4)
	Partnered	Single	Partnered	Single
Worked for pay (IV1 - twins first)	-0.087**	-0.145*	0.031	0.068
	[0.038]	[0.084]	[0.049]	[0.089]
Worked for pay (IV2 - twins first - sex)	-0.092**	-0.101	0.036	0.055
	[0.042]	[0.095]	[0.051]	[0.083]
Hours/week (IV1 - twins first)	-3.819***	-4.690*	1.414	1.511
	[1.138]	[2.532]	[1.681]	[3.116]
Hours/week (IV2 - twins first - sex)	-4.038***	-3.084	2.283	0.840
	[1.252]	[2.865]	[1.771]	[2.916]
Part-time (IV1 - twins first)	0.091***	0.070	-0.069	-0.087
	[0.029]	[0.065]	[0.047]	[0.088]
Part-time (IV2 - twins first - sex)	0.093***	0.058	-0.100**	-0.065
	[0.032]	[0.073]	[0.050]	[0.082]
Earnings/week (IV1 - twins first)	-17.165	-58.546*	14.184	56.526
	[14.670]	[29.943]	[18.440]	[37.343]
Earnings/week (IV2 - twins first - sex)	-16.417	-39.403	14.743	51.15
	[16.155]	[33.530]	[19.195]	[34.958]
Observations	23,624	5,551	37,626	10,575

Notes: Huber-White's robust standard errors are reported in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.