

Earnings and Income Penalties for Motherhood: Estimates for British Women Using the Individual Synthetic Control Method

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Abstract

Using data from the British Household Panel Survey and focusing on women who became mothers between 1995 and 2005, we estimate the motherhood penalty for women's own earnings and for the total income of their household: that is, we consider the extent to which motherhood carries a penalty not only for a mother but also for her family. We adopt an approach that differs from those previously employed in research on motherhood penalties: we follow Hernán and Robins, setting up our data as a 'target trial', and we analyse it using the Individual Synthetic Control method, based on the Synthetic Control approach of Abadie, Diamond and Hainmueller. We find considerable variation in the effect of motherhood on British women's earnings, but the median penalty is a reduction in medium- and long-term earnings by about 45 per cent relative to what women would have earned if they had remained childless. Motherhood has no effect on average on the income of the woman's household, but has substantial negative effects for some households. Focusing on both individual and household penalties yields a more complete picture of the distributional consequences of motherhood than hitherto.

Introduction

In recent decades, there have been many studies of the motherhood wage penalty (MWP), with most finding that women suffer a wage penalty when they become mothers, though the size of the penalty varies between individual women and across countries. In this paper, we revisit the motherhood penalty but we take a somewhat different perspective from previous studies. We also use a different research design and we implement a new method for estimating causal effects, which, as far as we know, has never been used by sociologists but which has several advantages over the fixed effects

approach that has become standard in studies of the MWP. Our empirical analysis deals with Great Britain, which stands out in the literature as having a large MWP.

Research into the MWP goes back to the late 1970s (Hill, 1979; Blau and Kahn, 2017: p. 24). Studies by Waldfogel (1997) and Budig and England (2001) have been particularly influential in sociology. Budig and England used data from the National Longitudinal Study of Youth (NLSY) and a fixed effects model, regressing the log of woman's hourly wage on the number of children she had. Since then fixed effects applied

to panel data has become standard in research on the MWP. We use a different method, which we call Individual Synthetic Control (ISC), based on the Synthetic Control approach of [Abadie, Diamond and Hainmueller \(2010\)](#). We also use a different design, drawing on the ideas of [Hernán and Robins \(2016\)](#) to use observational data to set up a target trial. Lastly, we focus on a different outcome: instead of wages we look at earnings and we consider not only the effect of motherhood on the earnings of mothers but also its effect on the incomes of the households in which they are located.

The goals of our paper are twofold. First, we estimate the effect of becoming a mother on a woman's earnings and investigate how this varies among new mothers and how it develops over the years following the one in which the birth occurred. Second, we estimate the effect of entry into motherhood on the income of the household of the mother. We find that motherhood has a large negative effect on British women's earnings, with an average earnings penalty in the year a woman has her first child of around £306 per month (median = £236), or about 28 per cent relative to what she would have earned had she remained childless. Over the birth year and the following 6 years, the median penalty is 45 per cent. Motherhood has, on average, almost no effect on the income of the woman's household, but has substantial negative effects for some households.

In the next section of the paper, we briefly review previous research on the MWP, then we explain how our approach departs from this. We describe the method we use and the design of our study and we explain the goals of our analysis. We next introduce our data and present our results. The paper concludes with a short summary and discussion. Much of the technical background to our analysis can be found in the [Supplementary Appendix](#).

The Motherhood Wage Penalty

There is a large international literature on the MWP. Studies of the phenomenon in the United States have recently been reviewed by [Gough and Noonan \(2013\)](#) while [Grimshaw and Rubery \(2015\)](#) review studies from a range of different countries. This literature shows that there is considerable heterogeneity in the MWP. Some of this is linked to how many children a woman has: the wage penalty increases with successive births. Some is accounted for by variables capturing human capital or characteristics of the woman's job or her family situation. But all these factors account for no more than half of the penalty. [Budig and England \(2001\)](#) suggest

that this large residual may be due to discrimination against mothers on the part of employers and/or the impact of motherhood on a woman's productivity. Experimental studies have been used to test discrimination against mothers (e.g., [Correll, Benard and Paik, 2007](#)).

Two recent UK studies, both using British Household Panel Survey (BHPS) data, find substantial motherhood penalties. [Costa et al. \(2020\)](#) report a gender wage gap of around 8 per cent at childbirth which increases for the next 10–12 years to just over 30 per cent, then remains constant to the end of their study, 20 years after childbirth. The biggest single explanation of the gap is the difference in experience between men and women, though a large share of it remains unaccounted for. [Kleven et al. \(2019\)](#) find a motherhood penalty in labour income (rather than wages) of 44 per cent, averaged over years 5 to 10 after the birth of the first child. The penalty increases rapidly for the first 3 years after birth, reaching 40 per cent. Both of these studies measure the penalty in terms of the difference between men and women, rather than in absolute terms for mothers. In the [Costa et al.](#) study, the gender gap grows by around 22 percentage points while in the [Kleven et al.](#) study, because childbirth has virtually no effect on men's earnings, the gender gap grows by around 44 percentage points.

The great majority of non-experimental studies follow a common methodological approach: they use panel data and analyse it with fixed effects models. The use of fixed effects in studies of the MWP (in preference to, or as well as, simple OLS) is motivated by the possibility that changes in women's wages after having a child may not, or may only partly be, caused by their having a child. If, after having a child, women give up work or reduce their working hours, if they change their job to one that is more compatible with motherhood, if they exert less effort at work or if firms discriminate against mothers, then having a child can be considered to have a causal effect on mothers' earnings. But if women who have children earn less on average than women who do not (for whom, perhaps, the opportunity costs of children are higher) then what seems like an effect of motherhood may instead be driven by selection into motherhood. Similarly, if parents choose to have children at an opportune moment, such as when the woman has reached a certain point in her career at which her earnings growth is flattening out or when her career is not doing well and this seems like a low-cost time to have a child, then what appears to be an effect of motherhood on earnings may actually be an effect of earnings, or earnings trajectory, on motherhood.

Selection into motherhood as a source of the apparent MWP is widely discussed in the literature: Gough and Noonan (2013: p. 332) and Blau and Kahn (2017) provide summaries.

Fixed effects models are used to try to overcome some of the problems in determining how much of the MWP is causal but they have an important limitation in only dealing with time-invariant sources of unmeasured selection into motherhood (Angrist and Pischke, 2009). But if this does not hold—as would be the case in the scenario outlined in the previous paragraph where selection into motherhood occurs on the basis of earnings growth (or decline)—fixed effects estimates will be biased. Alternatives, such as the fixed effect individual slopes method (Ludwig and Brüderl, 2018: p. 750) could be used to deal with selection into motherhood on the basis of trends in earnings but this requires an auxiliary equation that models earnings growth as a function of predictor variables such as experience. However, the growth function needs to be correctly specified, in addition to the correct specification of the model for the impact of motherhood. The method we use here is, by comparison, very straightforward and intuitive.

Our Approach

The approach we adopt in this paper differs from the usual one both substantively and methodologically. Studies of the MWP focus on gender inequality and the question of who bears the cost of childbearing and child-rearing. Budig and England (2001: p. 204), for example, write that ‘While the benefits of mothering diffuse widely ... the costs of child rearing are borne disproportionately by mothers’. But we ask a further question that has been addressed in the literature less often: how does motherhood affect not only a woman’s own earnings but also the income of her household?¹ If household income declined due to motherhood it would suggest that the costs of child rearing were in fact borne not only by mothers (though they shoulder the greatest burden) but also by others in the same household. This raises the question of which households bear the greatest costs: if, for example, it is households that are poorest to begin with, motherhood may increase existing inequality between families, and, conversely, if the highest-income households lose most from motherhood it could be viewed as ameliorating such inequality. In other words, motherhood penalties may be a source of household inequality as well as gender inequality. Viewed from this perspective, a woman’s wage rate, without also considering how many hours she works, is not a good measure of how much she contributes to the income of the household.

Furthermore, studies of the motherhood wage penalty focus on a ratio: earnings divided by hours worked. While, for many purposes, this can be valuable, a growing number of studies have instead focused on earnings themselves as a better measure of the degree to which motherhood affects a woman’s economic situation. As noted above, Kleven *et al.* (2019) in their study of the United Kingdom use earnings (labour income) rather than wages, as do Angelov, Johansson and Lindahl (2016) in a study of Sweden, and Musick, Bea and Gonalons-Pons (2020) in a study of the United States, United Kingdom, and Germany.

We focus on women’s absolute earnings and thus estimate a motherhood earnings penalty (MEP). We analyse the magnitude of this penalty, how it varies among mothers and over time, and the role it plays in household income. The use of earnings rather than wages has another advantage: we do not need to omit observations of women who did not have a wage since we can treat them as having zero earnings. Omitting them risks introducing bias: for example, women who were not working prior to childbirth may nevertheless suffer a loss of earnings afterwards if they would have worked outside the home if they had not had a child. Methodologically our study differs in two main ways from what has gone before: in the analytical methods we use (ISC rather than fixed effects) and in the design of our research. We discuss each of these in turn.

The Synthetic Control Model

The synthetic control method was developed by Abadie, Diamond and Hainmueller (2010; Abadie and Gardeazabal, 2003) and has since been widely used, particularly in political science and economics (Abadie, Diamond and Hainmueller, 2015; Acemoglu *et al.*, 2016; Hope, 2016). The method was developed for situations in which one case is treated (typically a state or region or country) and we would like to know the effect of treatment on that case. The method assumes that we have observations of the outcome for periods prior to and following treatment for the treated case and for a set of untreated cases that are potential controls. The idea is to form a synthetic control as a weighted sum of the control cases, such that the pre-treatment outcomes for the synthetic control match the pre-treatment outcomes for the treated case. The synthetic control is then used to simulate what the post-treatment outcomes for the treated case would have been had it not been treated.

More formally, time is denoted $t = 1, \dots, T$ with treatment at $1 < \tau < T$. There are N units, indexed $i = 1, \dots, N$. Unit 1 is treated, units 2 through N are the controls. The outcome at each point in time is assumed

to be continuous and is denoted Y_{it} . The binary treatment indicator is D_{it} . We assume that the potential outcomes, $Y_{it}^{D=d}$ are generated by:

$$Y_{it}^{D=d} = X_{it}\beta + \lambda_t\mu_i + \delta_{it}D_{it} + \epsilon_{it} \quad (1)$$

X is a matrix of measured time-varying and time-constant predictors and ϵ is an i.i.d. error term. λ_t is a vector of unobserved time-specific factors and μ_i captures their unit-specific effects. The difficulty in estimating the effect of treatment on the treated case, δ_{it} , is that whether or not a case is treated at t may depend not only on the observed X_{it} but also on $\lambda_t\mu_i$.

Our estimand is the effect of treatment on the treated case: that is

$$(Y_{1t}^{D=1}|D=1) - (Y_{1t}^{D=0}|D=1) \text{ for } t \geq \tau. \quad (2)$$

Here the superscript distinguishes different potential outcomes while the material after the conditioning bar indicates whether or not a case was treated. The estimand is therefore the difference, for the treated case ($D=1$), in its outcomes if it was treated $Y_{1t}^{D=1}$, and if it was not treated, $Y_{1t}^{D=0}$.

We can write $(Y_{1t}^{D=1}|D=1) = (Y_{1t}|D=1)$, but $Y_{1t}^{D=0}|D=1$ is unobserved. Our estimator is:

$$(Y_{1t}|D=1) - (Y_{1t}^*|D=0) \text{ for } t \geq \tau. \quad (3)$$

Y_{1t}^* is the outcome for the synthetic control for unit 1, which is constructed as a weighted combination of the outcomes for the control cases: $Y_{1t}^* = \sum_{i=2}^N w_i Y_{it}$. The w_i are weights chosen to minimize $\left\| Y_{1t} - \sum_{i=2}^N w_i Y_{it} \right\|$ for $t < \tau$ where $\|a-b\|$ is a distance measure (usually Euclidian). The weights are constrained to be non-negative and to sum to 1.

In using the synthetic control outcomes to proxy for the unobserved outcomes for the treated case had it not been treated we are assuming independence of the potential outcomes under non-treatment from the treatment received, conditional on observed covariates and on the history of all outcomes prior to treatment: that is, we assume:

$$Y_{it}^{D=0} \perp D_{it} | X_{it}, \bar{Y}_{it},$$

where \bar{Y}_{it} is all the outcomes for the treated and the control units up to the time of treatment. The intuition behind this is that the pre-treatment outcomes will capture the unobserved $\lambda_t\mu_i$.

...since past outcomes are influenced by unobserved, as well as observed confounders, units with similar past

outcomes over an extended period are likely to also be similar in terms of their unobserved confounders (O'Neill et al., 2016: p. 8).

In the case of the MEP, this method will capture unobserved earnings variation over time because the synthetic control will closely resemble the earnings trajectory of the treated unit. This does not mean that the choice of having children is random: unobserved factors can influence fertility decisions but we are assuming that these unobserved factors are unrelated to a mother's post-childbirth earnings trajectory. The synthetic control method, unlike fixed effects, will capture important biases such as selection into different earning trajectories or fertility decisions influenced by earnings growth. In principle, the only bias that the method does not capture would come from an unobserved factor that affected both the decision to have a child and the post-childbirth earnings trajectory but not the pre-childbirth earnings trajectory.

The Individual Synthetic Control Method

Robbins, Saunders and Kilmer (2017) extended the synthetic control approach to apply when there is more than one treated case, but in their set-up, all the units are treated at the same time and they use the synthetic control method to estimate the average causal effect. In this paper, we extend the method further to what we call ISC: we have many treated units, with treatment occurring at different times. Because our data come from a panel survey based on a sample of the population with known sampling probabilities, we can not only estimate individual treatment effects (as the synthetic control method does), but we can also compute average treatment effects (the average treatment effect on the treated or ATT) and we can generalize these to the population. Below and in the [Supplementary Appendix](#), we explain how we addressed the problems of statistical inference using ISC. Moreover, synthetic control methods typically analyse only one treated case, making it impossible to regress the synthetic control estimates on explanatory variables. But because we have a relatively large sample, we can combine traditional regression techniques with our individual synthetic estimates and analyse which factors explain variation in the causal effects. Furthermore, because we generate the distribution of individual treatment effects, we can look at how these effects vary at quantiles of the distribution (such as the median). We are not limited to a focus on average treatment effects.

Now we index treated cases $i = 1, \dots, n$ and control cases by $j = n + 1, \dots, N$. We index time in the same way as before but define the treatment time for case i by $\tau(i)$. Then we have weights, $w(i)$ which minimize

$$\left\| Y_{it} - \sum_{j=n+1}^N w_j(i) Y_{jt} \right\| \text{ for } t < \tau(i) \text{ for each } i. \quad (4)$$

Each treated case can have its own set of control cases with its own set of weights (see [Supplementary Appendix B](#) for more details). Our distance measure is the square root of the sum of squared differences between the observed outcome for the treated case and its synthetic control. Treatment effects for a treated case are the difference, in the treatment and each post-treatment period, between the outcome for the treated case and the outcome for its synthetic control:

$$(Y_{it}D = 1) - (Y_{it}^*D = 0), \text{ for } t \geq \tau. \quad (5)$$

We want to estimate not only the average treatment effect among the treated (the ATT) but also the effect for each treated unit. This allows us to explore heterogeneity in the MEP much more easily than we could with fixed effects models. In the latter, heterogeneity is investigated by interacting the treatment dummy with covariates that are thought to moderate the effect of motherhood. Using ISC, however, we can see the distribution of treatment effects across all mothers without having to hypothesize how this variation comes about, and we can then investigate the sources of this variation.

Standard Errors and Weights

When we use the individual estimates to compute quantities relating to the whole sample, such as the ATT, or we estimate regression coefficients, we need to take into account that our counterfactual outcome is an estimate, not an observed quantity.

Define δ_{it} as the causal effect for the i th case at time t :

$$\delta_{it} \equiv Y_{it} - \sum_j \hat{w}_{jt(i)} Y_{jt}. \quad (6)$$

The average treatment effect for the treated at time t is the average of the above:

$$ATT_t = \frac{1}{n} \sum_{i=1}^n \hat{\delta}_{it} \equiv \hat{\delta}_t, \quad (7)$$

where j indexes the n treated cases. The weights are themselves estimates, as shown by the circumflex in

[equation \(6\)](#) and so the standard error of the ATT must take this into account. The standard error thus has two parts: a ‘between’ part that comes from the variation of the individual treatment effects around the average, and an additional ‘within’ part that comes from the variation in the estimated weights round their mean. [Supplementary Appendix B](#) details how we estimated the standard errors of the ATT and, similarly, the standard errors of regression coefficients.

Design

We follow [Hernán and Robins \(2016\)](#) by asking how we could design a study of the MEP that, as far as possible, mimicked an experiment. Of course, we cannot assign women randomly to treatment, but we can consider what causal effect we are most likely to be able to estimate adequately and how we should do that. Like most previous studies of the MWP, we use panel data, but, in contrast to previous studies that have usually used fixed effect models to contrast pre- and post-birth wages for all births, we define our treatment more narrowly as entry into motherhood, that is the birth of the first child.

In our study, we seek to define clearly both the treatment and control groups. Our treatment group is made up of women aged 16–30 when they enter the panel and who have a first birth during the period of our panel. Our control group is defined as women of the same age who remained childless during the duration of the whole panel.² We follow our treatment and control cases for at least 8 years: a minimum of 4 years pre-childbirth and a minimum of 3 years post-childbirth. To estimate the weights for the synthetic controls, we use the outcome (earnings or income) in the pre-childbirth years together with three covariates: age, age left school, and working hours (see [Supplementary Appendix C](#) for details). We estimate the effect of motherhood in the year of birth and the succeeding years. Our causal estimand is thus the effect of motherhood on the earnings of women relative to what their earnings would have been if they had never had a child.

We estimate the effect of motherhood on earnings at each post-birth time point, t , as $Y_{it} - Y_{it}^*$. We also report the counterfactual penalty in percentage terms, calculated as $\frac{100 \times (Y_{it} - Y_{it}^*)}{Y_{it}^*}$. In words, we express the effect of motherhood as a percentage of the earnings that a mother would have had if she had not had a child. To the extent that women enter motherhood at a time in their life when their earnings are on an upward trajectory, these effects, both absolute and in percentage terms, will be large because their counterfactual earnings will be growing throughout the post-childbirth years.

Table 1. Number of treated cases: mothers

Timing of first birth	Number of treated cases
-7	150
-6	188
-5	245
-4	318
-3	318
-2	318
-1	318
0	318
1	318
2	318
3	318
4	282
5	247
6	222

Data

Our data come from the BHPS, which ran as a stand-alone panel survey from 1991 until 2008. We select all women aged 16–30 at the first time they were observed in the data and who had not yet had a child. Because we allow for a minimum of four pre-treatment waves the women who became mothers had their first child when they were aged between 20 and 42. Our control group is those women who remained childless throughout the life of the panel. Our outcome variable for all women is their monthly earnings in each year (set to zero if they had no earnings because they did not work outside the home) and our outcome for the households they were in is its monthly income (defined as the sum of the incomes, from all sources, of all people in the household). All money amounts are deflated to GB pounds in the year 2000 and they refer to pre-tax earnings and incomes.

We have, in total, 318 treated cases and 630 control cases. For a woman who had her first child in a given year, we use a minimum of four preceding years and three post-treatment waves' individual earnings and household income to compute weights for the synthetic control using the treated case and the controls. We ran sensitivity analyses with more pre-treatment observation periods and found no significant differences in the results (this is reported in [Supplementary Appendix D](#)).

[Table 1](#) shows the distribution of treated cases according to their number of pre- and post-treatment years. So, 150 mothers had 7 waves of pre-childbirth earnings and income, while 222 had 6 post-childbirth waves.³ [Supplementary Appendix A](#) contains more details of the sample construction.

[Table 2](#) summarizes the characteristics of the sample by showing, for the control and treated groups in each year of the BHPS, the means of all the variables used in our later analyses (the sizes of these groups in each wave are shown at the bottom of the table). The control group is made up of women who never had a child while the treated group is all women who had a child between 1995 and 2005 (thus many of them will not yet have had a child in a particular year even though they are included in the treated group for that year). We use five variables to explain variation in the motherhood penalty. We have one time-constant variable: whether or not the woman had a University degree prior to the birth of her child (University degree), and four time-varying covariates: the woman's age, how many children she had, her working hours, and whether or not she was unmarried.

1991 through to 1994 are all pre-first birth years (see the variable 'Nb children') and the table shows that our attempt to generate a target trial from the BHPS data yielded treated and control groups that are very similar on average, even before we calculate the synthetic controls ([Table C.3](#) in [Supplementary Appendix C](#) shows how the synthetic control further improves the matching of treated and control cases). Differences start to emerge from 1995 onwards. In 1995, for example, the mean earnings gap between the treated and controls was around £40 per month. In 2004, when the treated women had, on average, 1.2 children, the gap was around £450. However, differences in household income remain small throughout all 18 waves. This is a first indication that motherhood may have only a small average effect on household incomes.

Results

The upper-left panel of [Figure 1](#) shows the difference between the average earnings of the treated women and those of their synthetic control for each year, pre- and post-treatment, together with the 95 per cent confidence intervals. The upper-right panel shows the trend in both treated and synthetic units across all years. In the upper-left panel, we see that, before the treatment year, the 95% confidence interval for the difference between the synthetic and controls always includes zero, while in the treatment year (0 in the figure) and afterwards the gap is large and negative. In the upper-right panel, we see how the two trends track each other prior to treatment and then diverge: the upward trend does not persist for women who become mothers. The treatment effect at any post-treatment time is measured by the gap between the lines for the treated and control at that point.

Table 2. Descriptive statistics

Treated	Variable	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	
0	Nb children	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
1	Nb children	0	0	0	0	0.2	0.3	0.5	0.6	0.7	0.8	0.9	0.9	0.9	1.1	1.2	1.4	1.5	1.6	1.7
0	Age	23	23.8	24.3	24.9	25.5	26	26.5	27.2	27.7	28.2	28.6	29.2	29.7	30.3	30.9	31.5	32.1	32.5	32.5
1	Age	24.4	24.1	25	26.1	26.6	27.2	27.9	28.6	29.3	29.9	30.4	31	31.8	32.3	32.9	33.6	34.4	34.4	35
0	University	0.1	0.1	0.1	0.1	0.1	0.1	0.1	0.2	0.2	0.2	0.2	0.3	0.3	0.3	0.3	0.3	0.4	0.4	0.4
1	University	0.1	0.1	0.1	0.1	0.2	0.2	0.2	0.2	0.2	0.3	0.3	0.3	0.3	0.4	0.4	0.4	0.4	0.4	0.5
0	Working hours	28.3	27.6	26.4	28.2	27.5	28.6	30.3	28.9	30.4	29.8	29.4	31.6	31.6	30.7	31.4	31.4	31.1	31.5	31.1
1	Working hours	30.6	32.3	29.9	29.9	29.6	27.8	25.7	24.8	26.1	23.7	23.1	22.8	22.8	21.5	20.1	18	16.9	16.8	16.7
0	Unmarried	0.8	0.8	0.8	0.8	0.7	0.8	0.8	0.8	0.7	0.7	0.7	0.8	0.8	0.7	0.7	0.7	0.7	0.7	0.6
1	Unmarried	0.6	0.6	0.4	0.4	0.4	0.3	0.3	0.3	0.3	0.3	0.3	0.3	0.3	0.2	0.2	0.2	0.2	0.2	0.2
0	Earnings	818.6	858.8	879.2	930.2	988.8	1,004.4	1,038.9	1,098.6	1,191.3	1,198	1,244.6	1,330.8	1,284	1,373.2	1,395.8	1,460.7	1,439.4	1,393.8	1,393.8
1	Earnings	894.6	938.7	951.3	1,031.4	1,027.1	961.8	965.4	993.2	1,034	1,009.6	1,010.2	1,044.6	1,008.6	953.8	923.1	832.5	925	958.1	958.1
0	Household income	2,376	2,439.3	2,557.4	2,616.7	2,754.7	2,764	2,778.4	2,841.4	3,021.2	3,024.6	3,005.7	3,011.4	2,967.7	3,130.4	3,080.8	3,021.5	3,068.8	3,030.2	3,030.2
1	Household income	2,640.6	2,361.1	2,383.3	2,506.3	2,599.2	2,695.5	2,761.5	2,761.9	2,916.1	2,863.9	2,973.9	3,147.6	3,093.2	3,091.7	3,193.2	2,982.6	3,318	3,403.9	3,403.9
0	N cases	165	193	229	251	288	318	354	374	478	512	603	592	586	579	559	553	539	512	512
1	N cases	199	219	235	241	252	259	269	274	300	307	316	316	315	315	310	309	306	304	304
	Total	364	412	464	492	540	577	623	648	778	819	919	908	901	894	869	862	845	816	816

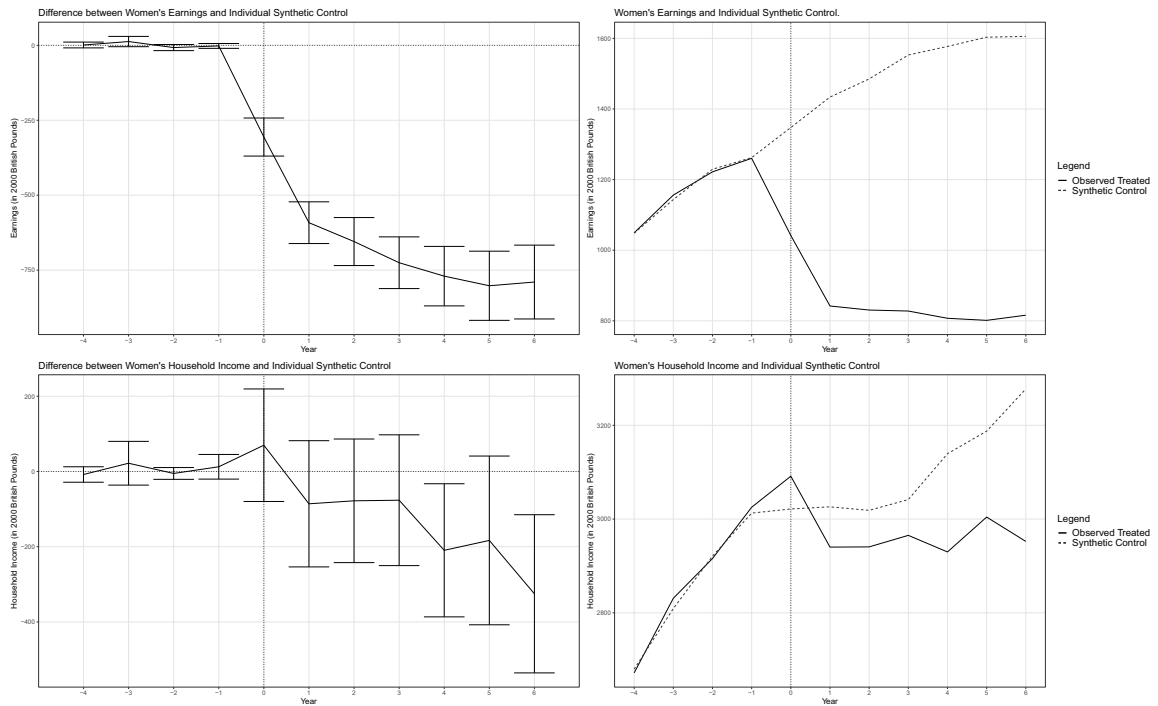


Figure 1. Average treatment effects. ISC estimation of earnings and household income.

In the lower panels, we present the same measures but this time for household income. In the lower-left panel, the difference between the treated and synthetic cases during the pre-treatment period is larger than for earnings but nevertheless never significantly different from zero. After the birth of the child, the difference between the treated and the controls is in the expected direction but imprecisely estimated, being not statistically except in post-birth years 4 and 6. The gap can be seen more clearly in the lower-right panel. This suggests, once again, that, on average, entry into motherhood has little effect on household incomes.

Figure 2 shows how having a first child reduces a woman's contribution to the income of the household. Before the birth, women's earnings account for, on average, just less than 45 per cent of household income but this declines to less than 30 per cent one year after the birth. We estimate that, in the absence of children, the contribution of women's earnings would have increased to just over 50 per cent, so having a child leads to a reduction of about 20 percentage points.⁴ This provides further grounds for thinking that motherhood will affect household income.

Table 3 shows the distribution of the estimated effects in each pre- and post-treatment period. We report the estimates at each decile of the distribution. At year 0 (the year of childbirth), the median effect for earnings is $-\pounds 236$ and $+\pounds 37$ per month for household income. Five years later, the median effect is $-\pounds 861$ for mothers' earnings and $-\pounds 212$ for their household income. However, we can see large differences in the earnings penalty between mothers at the 1st decile (10th percentile), where the effect is $-\pounds 927$ in the year she gives birth and $-\pounds 1,664$ 5 years afterwards, and the 9th decile, where the effect is positive: $\pounds 191$ and $\pounds 93$, respectively. The table suggests that 20 per cent of mothers experience no MEP and may even experience a positive effect of first birth. There is, therefore, a great deal of variation between mothers in their earnings penalties.

The effect of motherhood on household income is mostly negative at the median and below but more than 40 per cent of households in which a woman had a child experience no income penalty or experience a positive effect. Households at the 90th percentile show a large positive effect of $\pounds 1,208$ per month in the birth year and $\pounds 1,268$ 5 years later. On the other hand, households at the 10th percentile experience a loss of $\pounds 1,168$ in the

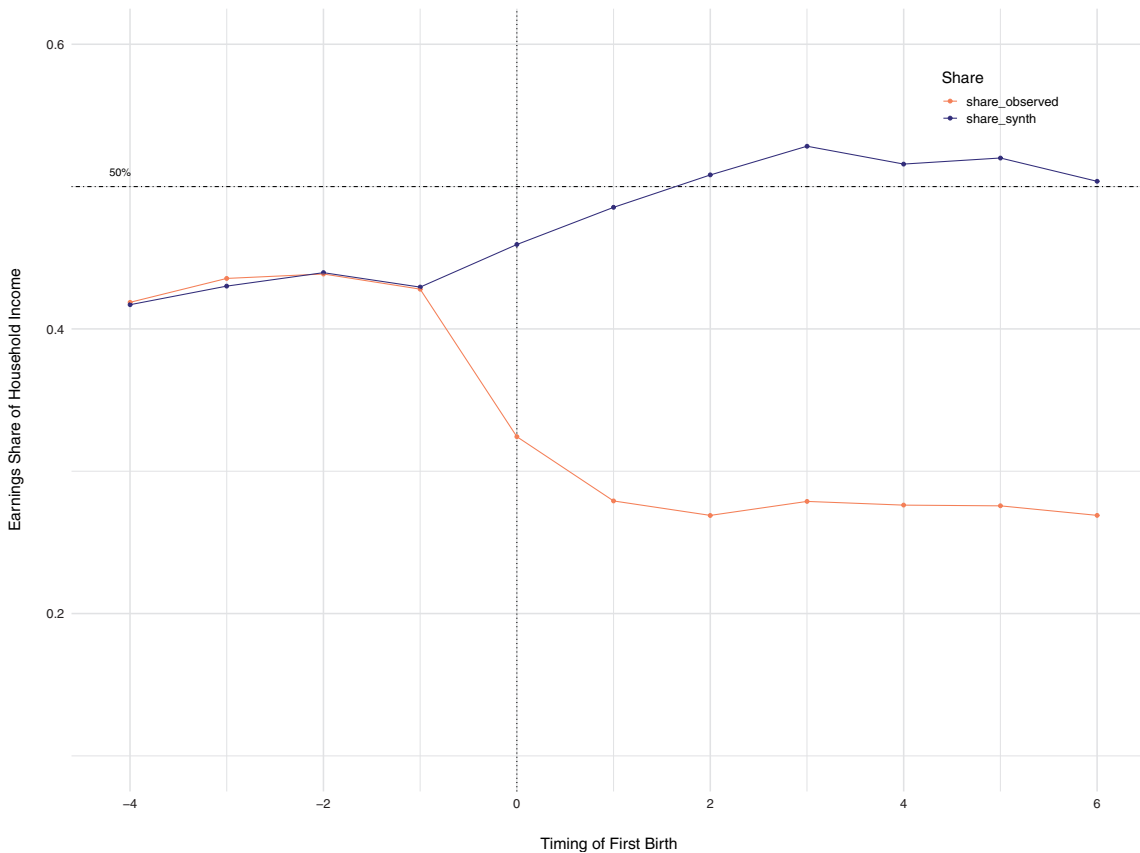


Figure 2. Share of women’s earnings on household income.

Table 3. Distribution of women’s ISC estimation on earnings and household income (in GB pounds deflated to year 2000)

Deciles	Earnings						Household income					
	0	T + 1	T + 2	T + 3	T + 4	T + 5	0	T + 1	T + 2	T + 3	T + 4	T + 5
1th decile	-927	-1,290	-1,363	-1,515	-1,656	-1,664	-1,168	-1,495	-1,516	-1,709	-1,654	-1,733
2th decile	-667	-966	-1,110	-1,202	-1,319	-1,369	-791	-931	-1,024	-1,223	-1,155	-1,242
3th decile	-472	-813	-943	-1,060	-1,126	-1,149	-501	-662	-723	-751	-826	-963
4th decile	-354	-717	-811	-917	-999	-1,034	-203	-436	-434	-488	-578	-542
5th decile	-236	-622	-702	-747	-862	-861	37	-261	-146	-152	-372	-212
6th decile	-129	-523	-550	-644	-660	-741	238	36	127	121	-20	96
7th decile	-19	-344	-341	-488	-490	-530	453	250	402	476	234	396
8th decile	55	-136	-188	-190	-176	-236	744	538	769	892	652	901
9th decile	191	22	61	21	159	93	1,208	1,216	1,354	1,718	1,349	1,268
10th decile	810	695	931	1,022	1,226	1,125	4,625	5,094	3,890	3,967	4,309	4,773

Note: 0 refers to the Year of Birth, 1 to the year of birth + 1, etc.

birth year and £1,733 5 years later. The variation in the impact of motherhood on family income is thus greater, in absolute terms, than the variation in the effect on women’s earnings.

Multilevel Regression

In the literature, investigations of heterogeneity usually focus on variation between mothers, and several sources of such variation have proved important in the MWP

(Gough and Noonan 2013). Of these, age at birth, marital status, education, and the level of pre-childbirth earnings are relevant to us. But because we follow mothers for up to six years after the birth of their child we can also investigate how the MEP varies through time. Whether or not the woman had a second child⁵ and changes in her labour force participation are likely to be important factors in explaining temporal variation.

To explore both sources of heterogeneity—between mothers and within mothers over time—we turn to multilevel, or hierarchical, linear models (see [Supplementary Appendix](#)). Here years (birth year and the six subsequent years) are level-1 units, nested within mothers (level-2 units). Letting i index individual mothers and t years at and after the birth of her first child, our model is:

$$Y_{it} = \alpha_0 + \sum_t \delta_t + \sum_m \beta_m X_{im} + \sum_q \gamma_q Z_{iqt} + \varepsilon_i + \nu_{it},$$

and

$$\alpha_0 = \alpha + u_i.$$

We regress the estimate of the individual MEP in each post-treatment year, Y_{it} , on dummy variables, δ , for each post-childbirth year, plus time-constant variables, X , and time-varying variables, Z . The X variables are a dummy variable for the woman's education (whether she had a University degree or not 1 year prior to giving birth) and her age when she gave birth, distinguishing those aged less than 25, who are the reference category, from those aged 25–30 and those 30 or more. The time-varying variables are whether the woman was married or not in each year, her average weekly hours spent in paid employment in each year (distinguishing mothers working less than 10 hours per week, who are the reference group, from those working 10–30 hours and those working more than 30 hours per week), and whether she had more than one child in each year, distinguishing one child (the reference) from two, or more than two children.

We fit three models for women's MEP, as shown in [Table 4](#). In the first model, we include only the intercept and the dummies for post-childbirth years: this allows us to see how the average effect evolves. In model 2, we add the time-constant variables, which allow us to explore how the variation in the MEP is linked to measured characteristics of the mother. Notice that, by definition, these variables are orthogonal to the dummies for post-childbirth years and so their addition will only affect the estimated intercept. Finally, in model 3, we add the time-varying variables. These can be seen as, in part, possible consequences of the entry into

motherhood (e.g., hours worked and number of children) and thus as mediators. Including them in the model points to some pathways through which motherhood affects earnings.⁶

In model 1 of [Table 4](#), the constant is our estimate of the ATT for year of birth: –£306 per month. The earnings penalty grows over time, reaching an estimated –£772 (= –£306–£466) six years after childbirth (these are similar to the median effects reported in [Table 3](#)). In model 2, neither the woman's education nor her age when she had her first child has significant effects and their inclusion barely changes the estimate of the intercept. In model 3, we see that there are some important pathways that shape the size of the earnings penalty. The crucial thing is how much a woman works. Working 10–30 hours a week reduces the penalty by £468 per month compared to mothers working less than 10 hours a week. Working more than 30 hours a week reduces the penalty even more, by £1,050: in other words, for these women, there is no MEP. Once we control for working hours, we find an age effect, with women 25–30 having a larger penalty than those younger or older. Having more children worsens the penalty by around £114 per month for those who go on to have a second child but subsequent children seem to have no effect on the penalty. We find no effect of marital status.

While models 1–3 relate to the absolute earnings penalty, models 4 through 6 of [Table 4](#) show the estimates relating to the counterfactual penalties—that is, the percentage loss of earnings relative to the synthetic control. In model 4, the constant is –0.28: mothers experience, on average, a counterfactual loss of 28 per cent of potential earnings in the year they have their first child: this increases over the post-childbirth period to –48 per cent 6 years after childbirth. Model 5 includes education and age. The constant of the model is now –0.48. This applies to non-University educated mothers aged less than 25 years old when they had their first child. Women with a degree have, on average, an 11 percentage point smaller penalty than those without. Age has significant effects: the older the woman when she became a mother, the smaller her earnings penalty. In model 6, we see that, unsurprisingly, working hours explain an important share of the counterfactual earnings penalty. Indeed, this model indicates that all the average time trend in the penalty is accounted for by subsequent fertility (having another child leads to a slightly larger penalty) and, especially, working hours. Working more than 30 hours a week reduces the penalty by 80 percentage points while working between 10 and 30 hours reduces it by 39 points. [Figure 1](#) helps to explain why

Table 4. Effect of first birth on women's earnings

Term	Earnings (in 2000 GB pounds)											
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Est.	se	Est.	se	Est.	se	Est.	se	Est.	se	Est.	se
Timing +1	-285.7	46.7	-285.7	46.7	-94.3	43.7	-0.173	0.03	-0.173	0.03	-0.041	0.028
Timing +2	-348.7	48.6	-348.7	48.6	-121.8	47	-0.192	0.031	-0.192	0.031	-0.04	0.03
Timing +3	-419.3	48.8	-419.3	48.8	-142.3	48.9	-0.205	0.031	-0.205	0.031	-0.022	0.03
Timing +4	-465.3	52.1	-465.6	52.1	-161	54.7	-0.213	0.033	-0.214	0.033	-0.013	0.034
Timing +5	-483	54.5	-482.4	54.6	-155.6	57.4	-0.202	0.035	-0.202	0.035	0.011	0.038
Timing +6	-466	55.6	-465.6	55.6	-154.8	62.1	-0.204	0.036	-0.203	0.036	-0.007	0.038
University			-21.3	82.2	-17.4	69.3			0.114	0.053	0.107	0.035
25–30 years old			-47.3	102.3	-193.7	85.1			0.147	0.068	0.02	0.044
More than 30 years old			117.7	106.3	-80.9	88.6			0.265	0.07	0.101	0.046
Work 10–30 hours					468.2	44					0.392	0.024
Work 30 hours or More					1,049.7	47.9					0.796	0.029
2 children					-113.7	45.9					-0.054	0.027
More than 2 children					-55	119.9					0.013	0.07
Unmarried					32.9	62.2					-0.036	0.034
Intercept	-306	45	-327.2	93	-873.5	85.5	-0.28	0.031	-0.484	0.062	-0.877	0.046
sd intercept	508.1		504.8		391.2		0.37		0.354		0.201	
N-spells	1,954		1,954		1,954		1,954		1,954		1,954	

Note: Individual synthetic multilevel regression. *p<0.05; **p<0.01; ***p<0.001.

these counterfactual penalties are large: on average, not only do women who become mothers earn less than they did before they had a child, they also miss out on the earnings growth they would have experienced if they had remained childless.

Table 5 shows the estimates for the effect of motherhood on total household income in absolute terms and as counterfactual penalties. In the models (1 and 4) with only dummies for years, the coefficients are much smaller than for earnings and rarely statistically significant. There is no significant average penalty in the year a woman gives birth to her first child. In the post-child-birth years, the penalty is zero or small, never exceeding about 3 per cent. Model 2 suggests that households in which the mother has a degree fare significantly better than others but her age at birth has no effect. In model 3, we now see a large and significant intercept: this applies to women who do not have a degree and work less than 10 hours per week and who have one or two children. This negative effect is offset if the mother works more than 10 hours per week. The same variables are important when we turn to the counterfactual household income penalty. Women without a degree who work less than 10 hours per week and who have less than two children suffer a 21 percentage point reduction in their family income: working 10–30 hours per week reduces this to about 3 percentage points. If she works more than 30 hours per week the penalty turns into a premium of around 10 percentage points.⁷

Discussion

Ours is the first study to use what we have called the ISC method. One advantage is that, because ISC explicitly estimates individual causal effects, we can see the variation in motherhood penalties (as in Table 3). We have shown that motherhood has a substantial, but widely varying, negative effect on British women's earnings. In the year a woman has her first child, the median MEP is £236 per month, rising to £861 pounds per month 5 years later with an average over the whole 6 years of £746. In percentage terms, the median loss of women's earnings is around 45 per cent relative to what they would have earned if they had remained childless. Motherhood has, on average, no effects on household income. But this average effect is misleading insofar as some households do experience a significant negative impact of motherhood on their income. Our ability to see and investigate this is another advantage of the ISC method.

A recent study decomposing the effect of children on the gender gap found a 44 per cent child penalty for the

earnings of women who became mothers (Kleven *et al.*, 2019) relative to men who became fathers. In our study, we compared mothers to childless women but our estimated penalty is roughly the same, suggesting that childless women are in fact similar to men in their earnings trajectories (Gough and Noonan, 2013). This supports the idea that the most important source of gender inequality in pay is parenthood status (Waldfoegel, 1998; Kleven, Landais and Sogaard, 2019).

Estimates of average effects mask substantial heterogeneity, both among individual mothers and among the households in which they are located. Around 20 per cent of mothers experience no MEP, while, for those at the 10th percentile of the distribution, the average earnings penalty in the year of birth is over £900; this is more than three times the median penalty. More than 40 per cent of households containing a mother experience no motherhood household income penalty, but here, although its magnitude is smaller, variation between households is much greater. There is also considerable variation over time: the MEP grows in the years following the birth, flattening out at the end of our observation period. By this time, the median has more than trebled. This growth is explained by reference to Figure 1. Most women who become mothers do so at a time when their earnings are increasing: for women who do not become mothers, this increase continues while, for mothers, at best it continues on a less steep trajectory. The median household income penalty, in contrast, grows for the first couple of years, but then remains approximately constant. This suggests that some households can compensate (in part or full) for the loss of the mother's earnings.

In the regressions reported in Table 5, we account for about 23 per cent of the variance in the absolute earnings penalty and 46 per cent of the counterfactual penalty.⁸ For the household income penalty the figures are 6 per cent in both cases. These results shed some light on who suffers the greatest penalties. For earnings, being a younger mother and not having a University education are linked to a large counterfactual earnings penalty. For household income, only education has an effect. When we turn to the mediators, much the most important is working hours. Attachment to the labour market is important for both kinds of penalty: mothers who manage to stay employed or return to full-time employment experience a much smaller penalty (or even no penalty) in earnings and income compared to other mothers. This finding supports studies (Gangl and Ziefle, 2009) showing that labour market attachment explains most of the motherhood wage penalty in Britain. Understanding why women choose to return to

Table 5. Effect of first birth on women's household income

Term	Household income (in 2000 GB pounds)																
	Model 1			Model 2			Model 3			Counterfactual penalties							
	Est.	se		Est.	se		Est.	se		Est.	se						
Timing +1	-155.7	98.4		-155.7	98.5	100	-29.8	100		-0.047	0.034		-0.047	0.034		-0.003	0.034
Timing +2	-147.8	99.6		-147.8	99.7	102.9	-34.4	102.9		-0.031	0.035		-0.031	0.035		0.006	0.035
Timing +3	-146.2	100.8		-146.2	100.8	109.6	-37.6	109.6		-0.029	0.034		-0.029	0.034		0.004	0.036
Timing +4	-244	104.3	*	-244.4	104.4	117.1	-139.8	117.1		-0.067	0.035	+	-0.067	0.035	+	-0.036	0.039
Timing +5	-209.4	109.2	+	-205.7	109.3	124.8	-119.8	124.8		-0.052	0.036		-0.05	0.036		-0.025	0.041
Timing +6	-313	114.7	**	-308.4	114.7	129.9	-265.2	129.9	*	-0.081	0.039	*	-0.079	0.039	*	-0.068	0.044
University				602.5	152.4	151.5	545.8	151.5	**				0.176	0.05	***	0.156	0.05
25-30 years old				45.2	202.2	201.9	-93.4	201.9					0.035	0.067		-0.017	0.066
More than 30 years old				262.7	207.4	207.4	104.1	207.4					0.119	0.069	+	0.06	0.068
Work 10-30 hours							493	97.3	***							0.178	0.032
Work 30 hours or More							911.5	107.4	***							0.327	0.036
2 children							86.4	98.2								0.045	0.032
More than 2 children							659.8	247.2	**							0.191	0.084
Unmarried							-176.6	144.7								-0.06	0.048
Intercept	69.8	91.3		-221	179.1	195.5	-642.9	195.5	***	0.049	0.031		-0.063	0.06		-0.213	0.065
sd intercept	1,005			956.2		945.1				0.32			0.304				0.298
N-spells	1,997			1,997		1,997				1,997			1,997				1,997

Note: Individual synthetic multilevel regression. *p<0.05; **p<0.01; ***p<0.001

the labour market after childbirth is beyond the scope of this paper: however, we conducted supplementary analysis and found that most women, in the years after the first birth, did not return to their pre-motherhood working hours. Even when disaggregating mothers in different pre-birth working hour trajectories, we found a long-lasting reduction in working hours.

Having a university degree is a strong protection against an earnings penalty and household income penalty. This stands in contrast to US studies reporting that women with high skills experience the highest penalty (Anderson, Binder and Krause, 2002; England *et al.*, 2016). The difference might be explained in part by the fact that we studied earnings and household income rather than individual wages or simply by country differences in the way in which women with different levels of education manage to combine work and motherhood.

Motherhood has substantial negative effects on a woman's earnings: our results show that lost earnings can quickly accumulate even after just a few years out of the labour market. This has important consequences for mothers' economic independence and lifetime wealth accumulation, and can contribute to vulnerabilities later in life. But we also found a household income penalty for some mothers. This is smaller, but nevertheless persistent, with certain kinds of households (notably those where the mother's labour force attachment is weak) particularly badly affected. The correlation between the earnings penalty and the household income penalty, averaged across birth year and the six post-birth years, is 0.32, suggesting that the largest earnings penalties tend to be found together with the largest income penalties. We conclude that the motherhood penalty has important consequences not only for gender inequality but also for inequality between households. This dimension could be included in further studies of the motherhood wage or earnings penalty in order to yield a more complete picture of the distributional consequences of motherhood.

Supplementary Data

Supplementary data are available at *ESR* online.

Notes

- 1 A related question is addressed by Musick, Bea and Gonalons-Pons (2020) who consider how entry into motherhood affects the proportion of household income contributed by the mother.
- 2 We could have defined our control group differently and then our estimand would have been different. One possibility would be to take as the control group women who did not have a child until after the follow-up period in which we estimate the effect of entry into

motherhood. Then we would be estimating the effect of entry into motherhood in a particular year relative to delaying entry into motherhood. We believe this would confuse the motherhood effect with a timing of motherhood effect. But it might also be objected that our control group (women who never had a child) is a very selected set of women. This is true but the selection of women into motherhood relative to remaining childless is what we are trying to capture through the use of the synthetic cohort method. The women in our data were born between 1960 and 1988. Of women born in Great Britain in 1970, 17 per cent were childless at the age of 44 so the women in our control group could not be considered unusual (<https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/conceptionandfertilityrates/bulletins/childbearingforwomenbornindifferentyearsenglandandwales/2015#by-the-end-of-their-childbearing-years-17-of-women-born-in-1970-remained-childless>).

- 3 The number of treated cases in each year can be read off from Table 1: 318 in year 0 and years $T + 1$ to $T + 3$, 282 in $T + 4$, and 247 in $T + 5$.
- 4 This average is calculated including households in which the women are the only earner and so it does not reflect the contribution of women's earnings to the income of two-earner households.
- 5 We consider having a second child to be a consequence of having a first child: it is thus on the causal path between treatment (first child) and outcome (earnings).
- 6 This part of our analysis is descriptive rather than causal. If there are unmeasured confounders of the relationship between a woman's MEP and a mediating variable such as hours worked this could introduce bias into the estimate of the direct, partial effects of variables such as education on the MEP.
- 7 The positive effects for women with more than two children shown in Table 5 are most likely the result of selection into having more than one subsequent birth. This is, in any case, a rare event in our data: only 27 out of 318 (8 per cent) first-time mothers continue to have more than two children
- 8 Based on comparing the residual standard deviation of model 3 with 1 and model 6 with 4 in Tables 4 and 5. So they tell us how much variance in the outcome we explain around the post-birth year means.

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