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Does Welfare Reform Affect Fertility? Evidence from the UK

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Abstract
This paper presents evidence on the fertility effect of welfare from a set of reforms that took place in the UK in 1999 and that substantially increased support for poorer families with children. The reforms, including the introduction of the Working Families Tax Credit and an increase in means-tested income support, raised benefits by up to 10 per cent of household income. We exploit the fact that the reforms were targeted on low-income households and use a differences-in-differences approach to evaluate their impact on fertility. A priori, the fertility effect of the reforms is ambiguous because WFTC has pro-employment effects. In practice, these are more important for lone mothers and we therefore focus on women in couples where we expect the reforms to have a positive effect on births. We find that the reforms raised the probability of birth among women in couples by around 10 per cent (implying an elasticity of 0.22). In line with previous work, the effect is greatest for first births.

Keywords: Welfare reform; Fertility; Working Families Tax Credit

JEL Classification: J13, J18, H53

Electronic version: **http://www.bris.ac.uk/Depts/CMPO/workingpapers/wp177.pdf**

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1 Introduction

There has been considerable interest in the effect of welfare on fertility. Much of the existing evidence comes from the US where studies have typically exploited variation in programme generosity and timing of implementation across states to identify an effect. In general, the US evidence finds that more generous welfare is associated with increased births (see Moffitt, 1998), although the results are sensitive to specification.

This paper presents new evidence on the effect of welfare on fertility from the UK, focusing on a set of reforms to benefits for families with children introduced in 1999. The Working Families Tax Credit (WFTC), similar in design to the US Earned Income Tax Credit (EITC), increased the generosity of benefits for households with children where at least one parent worked 16 hours a week or more,[1] while the generosity of means-tested Income Support (IS) payments to workless households with children also increased.[2] We find that these reforms increased the probability of birth by around 10 per cent among women in couples, equivalent to nearly 20,000 births in the post-reform period for these families.

The UK makes a good case study because of the sheer scale of the reforms. Between 1999 and 2003, government spending per child increased by 50 per cent in real terms, a change that was unprecedented over the previous thirty year period. Most of the additional spending was targeted at low-income households. For the poorest one-fifth of couples with children, the additional amount of benefit they received for their first child was equivalent to a ten per cent increase in their income. Since the main aims of

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[1] The reforms have been extensively documented elsewhere. For further details, see Brewer et al (2006), Francesconi and van der Klaauw (2007), Gregg and Harkness (2003) and Leigh (2007).

[2] Total UK births were 645,000 in 2005

the reforms were to improve work incentives (in the case of WFTC) and to bring about a reduction in child poverty, there was no pro-natalist intention and therefore little concern about policy endogeneity in examining the effect of the reform on fertility (see Besley and Case, 2000). Given the similarity in design between the WFTC and the EITC, the results from our analysis of the UK reforms will be highly relevant to the US.

The employment effects of WFTC have been extensively analysed (see *inter alia* Blundell et al, 2005, Brewer et al, 2006, Gregg and Harkness, 2003, Francesconi and van der Klauuw, 2007 and Leigh, 2007). Similar to the US studies of EITC (see Eissa and Leibman, 1996, Eissa and Hoynes, 2004), these studies find a significant increase in employment among lone parents, but little effect on the employment of women in couples, with a number suggesting that the additional income from WFTC might have caused some women with employed partners to leave work. There has been far less analysis of the impact of the UK reforms on fertility. Yet this is an important, if possibly unintended, consequence of the reforms. Moreover, most existing analyses of the employment effects of welfare-to-work assume that fertility is exogenous without explicitly testing this assumption.

The only previous study of WFTC to consider fertility, Francesconi and van der Klauuw (2007), focused on lone mothers and found a (statistically insignificant) reduction in the probability of lone mothers having another child after the reforms. However, as we argue below, the fertility incentives are not unambiguously positive for this group because the increase in employment could cause a reduction in fertility by raising the opportunity cost of an additional child. In our analysis, we focus on couples where the likely positive fertility effects are stronger.

Since the reforms were nation-wide, we cannot follow the US studies in identifying the policy effect from variation across state and time. Instead, we exploit the fact that the reforms were targeted at low-income households and adopt the commonly-used difference-in-difference approach (see Angrist and Krueger, 1999). We look at the change in fertility before and after the reform for couples who were affected by the reform and use the change in fertility over the same period for couples unaffected by the reform to control for other (unobservable) time-varying effects. While this methodology cannot precisely disentangle individual policy effects, Ellwood (1999) argues that it presents powerful and straightforward evidence on behavioural impacts. We discuss our definition of the two groups, as well as some potential problems with this approach in section 6.

The structure of the rest of the paper is as follows. The following section summarizes the previous literature in this area. Section 3 describes the UK reforms in further detail, and section 4 discusses the possible effect of the reforms on the incentives to have children. Sections 5 and 6 describe the data we use and our empirical strategy. Section 7 presents the results of regression analysis and section 8 offers some conclusions.

2 Previous research

According to a basic economic model of fertility (see Becker, 1991), more generous government support for children would tend to raise the desired number of children through both a positive income effect[3] and a positive own price effect. There is a large US literature that tries to test this prediction, much of it focusing on Aid to

3 Alternatively, it has been argued that higher income is associated with demand for increased quality of children, implying a possible reduction in quantity demanded.

Families with Dependent Children (AFDC), a welfare programme targeted at lone parents (see Moffit, 1998, for an overview). Identification in studies of AFDC typically relies on variation in generosity across states and, although there is clearly a positive statistically significant correlation between welfare generosity and fertility, the results are sensitive to methodology and in particular, the inclusion of state fixed effects and trends (see Hoynes, 1997).

Of particular relevance to the UK 1999 reforms, a recent paper by Baughman and Dickert-Conlin (2003) looks at the effect of EITC on fertility. Focusing on first births and on women with less than college education, who are likely to be more affected by the reforms, they exploit variation in state EITC payments to identify an effect. They control for state fixed effects and time-varying policy and economic variables, but not state trends. Overall, they find that more generous EITC benefits have a *negative* effect on first births, although this is statistically insignificant. But they find a positive effect for married women. As in previous US studies, they find a larger effect for non-whites.

A number of studies have looked at the effect of other forms of government support on childbearing. Whittingdon et al (1990) find a significant fertility effect of changes in the personal tax exemption for dependents in the US, with an implied elasticity of between 0.127 and 0.248. For the UK, Ermisch (1988) finds a significant effect of child benefit payments on third and fourth births. However, in these studies, which exploit variation in levels of support over time, clearly identifying the effect of reforms from the effect of other (unobservable) time-varying factors is a potential issue.

Finally, a number of studies have looked at the effect of explicitly pro-natalist policies.

A recent example, Milligan (2005) studied the effect of the Allowance for Newborn Children (ANC), introduced in Quebec in 1998, which paid 500 Canadian dollars for the first birth, $1,000 for a second birth (split into two annual payments) and up to $8,000 for a third birth (split into twenty quarterly payments). Using a difference-in-difference approach, he found a positive and significant effect on fertility, compared to the rest of Canada, raising fertility by 12% in the case of first births and 25% in the case of third and subsequent births. He estimated that a $Can 1,000 increase in government support in the first year would increase the probability of having a child by 16.9%. Perhaps surprisingly, he found a bigger effect for higher income families. However, in cases such as these, the estimated fertility effects may be biased by possible policy endogeneity.

3 The UK reforms

In 1997, the incoming Labour government initiated a series of policy reforms aimed at reducing child poverty. As discussed in the papers on WFTC cited earlier, a key element was to "make work pay" for low-earning families. Drawing extensively on the experience of welfare-to-work programmes in North America, WFTC was introduced in October 1999 to provide improved work incentives for families with children, together with a number of additional programmes, such as the New Deal for Lone Parents, offering training and other help with finding a job. Alongside this, however, the government also increased the generosity of means-tested income support payments to families with children. In this section we describe first the WFTC reform, and then the contemporaneous changes to welfare benefits and income tax, before finally analysing how the combined package of reforms affected the incomes of families with children.[4]

In essence, WFTC represented a dramatic expansion of an existing, small in-work cash support programme known as Family Credit (FC). Both WFTC and FC required recipients to work for at least 16 hours per week, and the credit was tapered away with household[5] earnings (plus some other forms of income) above a threshold. But WFTC was more generous in five ways: Credits were higher, particularly for those with younger children; families could earn more before the credit began to be withdrawn; the rate at which the credit was withdrawn was lower; support for formal childcare was more generous; and WFTC excluded child maintenance payments from its definition of income.[6] Figure 1 (from Brewer, 2001) compares the WFTC schedule with that of the US equivalent, EITC, for the fiscal year 2000. It shows, compared with EITC, the absence of a phase-in portion for WFTC, the greater generosity of WFTC (at PPP rates) and the steeper phase-out of WFTC.

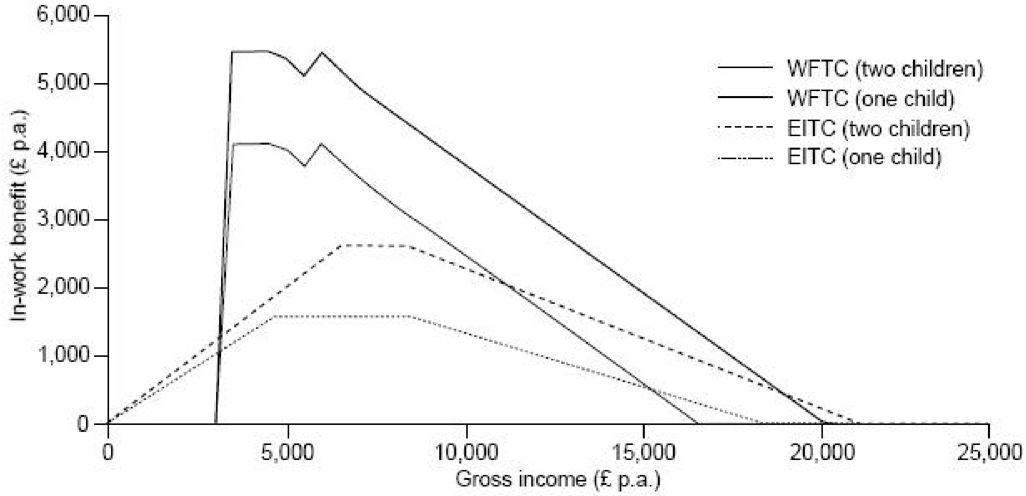
[4] This discussion draws on Brewer and Browne (2006) and Brewer et al (2006). We do not describe the further set of reforms that took place after April 2003 for further details see Brewer (2003).

[5] The assessment was made on the basis of couple's earnings even in the case of cohabiting as opposed to married couples.

[6] There was no attempt to present these reforms as revenue neutral: annual expenditure on FC/WFTC almost doubled between 1998–99 and 2000–01, going from £2.68 billion to £4.81 billion in constant 2002 prices, with a further increase by 2002 to £6.46 billion.

Figure 1

WFTC and EITC schedules compared, 2000



Notes: £1 = $1.50. Assumes 2000 tax system in US. Assumes 2000 tax system in UK plus children's tax credit. Assumes two WFTC awards a year and minimum-wage work in UK, so eligible for 30-hour credit at gross annual income of £5,772 (52×30×£3.70).

But, the introduction of WFTC was not the whole story; there are other income tax allowances and transfer programmes available to families with children during the period under consideration, and most saw some change. Child benefit, a cash benefit available to all families with children regardless of income, saw an increase in the amount paid in respect of the first child; welfare benefits for families on a low income and working less than 16 hours a week saw considerable increases in the amounts paid in respect of children, and a small non-refundable income tax credit for parents was introduced in 2001.

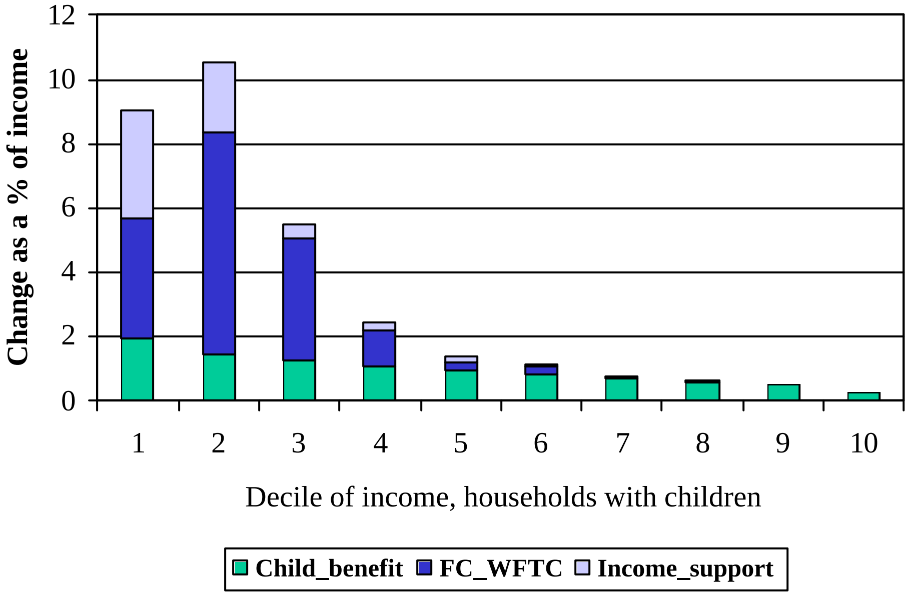
Overall, the combined set of welfare reforms amounted to a big increase in the total package of state-provided child-contingent cash support (whether provided through cash benefits, in-work tax credits or income tax deductions): central government spending on all child-contingent support programmes rose by 50 per cent in real terms between 1999 and 2003 (Adam and Brewer, 2004). But the change was far more important for low-income families than better-off families, because the rise in spending was dominated by WFTC and the higher welfare benefits for families with children.

Figure 2 shows the changes in benefits as a proportion of (pre-reform) net family income, by deciles of income (adjusted for family composition) for couples with one child and for couples with two or more children. This makes it clear that the introduction of WFTC was the most important single element in terms of raising the incomes of families with children. It also highlights how the effects of the changes were concentrated among poorer households. For those in the bottom fifth of the income distribution, the rise in child-contingent support meant increases in net income of around 10 per cent for those with one child, and over 12 per cent for those with two or more children.

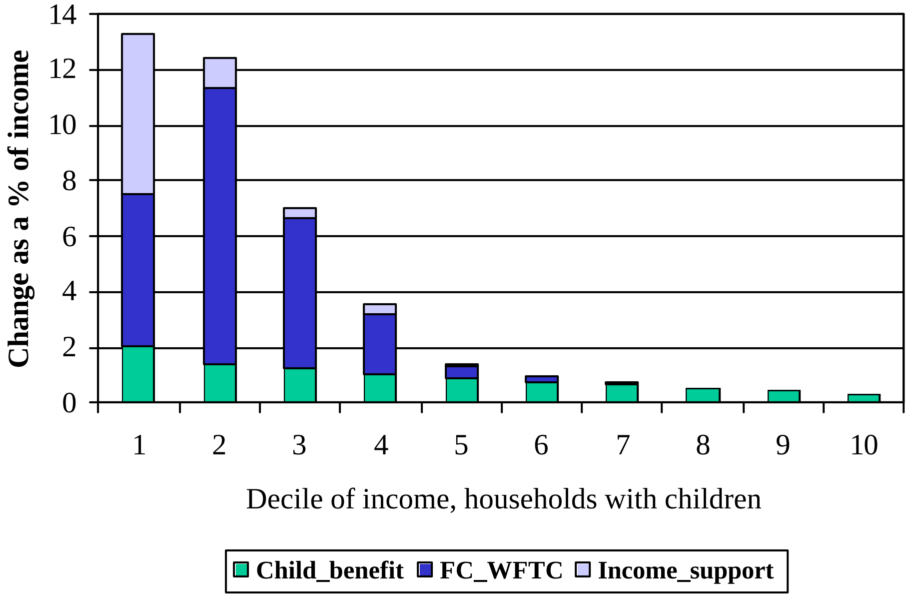
Figure 2

Increase in child-contingent benefits, 1998 – 2002

Couples, one child



Couples, two children



Authors' calculations based on estimated entitlements to welfare programmes, FC and WFTC, calculated using TAXBEN, the IFS' tax and benefit calculator.

4. *The impact on fertility*

In principle, the set of reforms that raised government support for families with children could impact on the desired number of children through the following channels:

- Families who are eligible for WFTC or income support will experience a positive income effect which, if children are a normal good, will increase the demand for the quantity of children.
- The increase in child-contingent benefits[7] will lower the own price of an additional child for eligible women, increasing the demand for children.
- For women on the taper of the WFTC schedule, the fall in net wages will reduce the opportunity cost of an additional child and this will also increase the demand for children.
- Fraser (2001) argues that government support, such as through WFTC or income support, can also act to reduce income volatility and this will tend to increase the demand for children.
- But, for women who are induced to move into, or increase, employment by the introduction of WFTC (potentially anyone below the minimum threshold shown in Figure 1), the opportunity cost of an additional child will be higher[8] and this will reduce the demand for children.

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[7] Including the increase in support with payments for childcare

8 This is likely to be particularly the case since employees typically have fewer maternity rights during their first year of employment at a firm.

In summary, there are positive impacts on fertility through price, income and insurance effects, and ambiguous impacts through the employment (or opportunity cost) effect. It is therefore vital to consider how the direction of the employment effect might vary between different sorts of mothers.

For lone mothers whose eligibility for WFTC is assessed at the individual level, the labour market participation effects of WFTC are unambiguously positive. Lone mothers working more than 16 hours prior to the reform will face a positive income effect and a negative substitution effect, which may cause them to reduce their hours, but the 16-hour condition in WFTC ensures that labour supply does not fall to zero. All of the studies that look at the effect of WFTC on participation, summarized in Table 1, find a positive and significant effect of WFTC on the employment of this group.

For women in couples who are the secondary earner (as is typically the case), the reform also has a positive income effect and a negative substitution effect on participation, but in this case, the woman may reduce her hours below 16 or leave the labour market altogether if the family will continue to be eligible for WFTC on the basis of her partner's participation. Fewer studies have looked at the impact of WFTC on women in couples, but most of those that do, summarized in Table 1, suggest a small reduction in participation among women with employed partners, and a small positive effect for (the small group of) women with unemployed partners. [9]

[9] The reforms other than WFTC mentioned in section 3 would have had very small impacts on participation of mothers in couples, and would have worked in the same direction, ie to discourage labour market participation for women in couples.

Crucially, then, the positive employment effect of WFTC (which would lead to reduced fertility) is much less prevalent for women in couples than for lone mothers, and hence a positive impact of WFTC on fertility will be stronger for women in couples. We therefore focus on this group in our analysis.

Table 1

The employment effects of WFTC – summary of evidence

Study	Methodology	Lone parents	Couples
Blundell et al (2000)	Use estimates from structural model to simulate likely response; looks only at initial (Oct 99) levels of WFTC	2.2 ppt increase in employment of lone mothers	0.6 ppt reduction in employment for women with employed partners
Gregg and Harkness (2003)	Apply DD methodology (with propensity score matching) to Household Labour Force Survey data 1992 – 2002	5 ppt increase in employment of lone parents	
Leigh (2007)	DD model using panel data from the Quarterly Labour Force Survey 1999 – 2000	1 ppt increase in employment among lone parents	1 ppt increase in employment among women in couples
Blundell et al (2005)	Estimate DD model using Labour Force Survey data 1996 – 2002	3.6 ppt increase in employment of lone mothers working 16+ hours a week	2.6 ppt increase in employment of women with unemployed partners; no effect for women with employed partners
Brewer et al (2006)	Estimate structural model using Family Resources Data 1995 – 2003	5.1 ppt increase in employment of lone mothers	0.6 ppt reduction in employment for women with employed partners, 0.1 ppt increase for women with non-employed partners, 0.6 ppt reduction for all women.
Francesconi and van der Klauuw (2007)	Estimate DD model using British Household Panel Survey data 1991 – 2002	7 ppt increase in employment of lone mothers working 16+ hours a week	

5 *Data*

We combine data from the Family Resources Survey (FRS) and the Family Expenditure Survey (FES) between 1995 and 2003. The main reason for choosing these surveys, as well as the fact that they contain extensive and comparable socio-demographic information, is their size. Both are large repeated cross-section datasets interviewing, respectively, over 20,000 and 7,000 households each year, which together yield over 800 births each year, with interview dates spread roughly equally across the year. While potentially attractive as a panel, the British Household Panel Survey (used by Francesconi and van der Klauw, 2007) has fewer than 150 births a year. The FRS and FES do not explicitly collect information about births or women's fertility. But we derive the probability that a woman had a birth in the previous twelve months from children's date of birth[10] and the date of interview after allocating all children in the household to their natural mothers on the basis of information on household composition. Using this approach, we also determine the number and ages of the children in the household twelve months before interview.

This approach (the so-called "own child method") can be used to derive approximate fertility histories for the women in the sample going back beyond the date of interview (see Murphy et al, 1993). But it is not without problems. First, the estimated birth probabilities are potentially subject to measurement error due to infant mortality and household reconstitution. However, low rates of mortality and the fact that the overwhelming majority of children stay with their natural mother in the event of family break-up reduce the effect of these factors in practice.

10 Where this is not available, we assign a randomly allocated date of birth based on the child's age and the interview date.

Second, an issue that affects information on the number and ages of existing children at the start of the twelve-month period (but not information on births during that period) is that older women may have had children who have now left home. Our solution is to censor the sample of women at age 37, since the problem seems to have a significant effect after this age.[11] We test the sensitivity of our results to this restriction.

As a check on the validity of our data, we compare estimates of the period total fertility rate[12] for all women derived from the FES/FRS with the official measure of total fertility derived by the Office for National Statistics (ONS) from registration data. This is shown in Figure 3 below, using the derived fertility histories from the FES/FRS to estimate total fertility back to 1968. The main long-term trends in fertility can clearly be seen, including the UK's own baby boom peaking in 1964 at 2.94, and the subsequent decline. There is some evidence of rising fertility towards the end of the period. Total fertility estimated using the FES/FRS tracks the official measure quite closely. The average difference is only 0.12 births and the two measures follow similar trends over the period.

For our analysis, we select women aged 20-37 who are in a couple at the time of interview (including married and cohabiting couples). The final sample contains over 53,000 observations. Key summary statistics are presented for this sub-sample, as well as the sample of all women, in Table 2.

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11 We estimated the probability that women had a birth at age 17 using pooled data on women aged 30 or more and including dummies for the age at interview: 38 was the first age at which the age dummy was significant.

12 This is measured as the total number of children a woman would have over her (reproductive) lifetime, if she experienced the age-specific birth rates in a particular year

Figure 3

Comparison of estimated Total Fertility Rate with official TFR

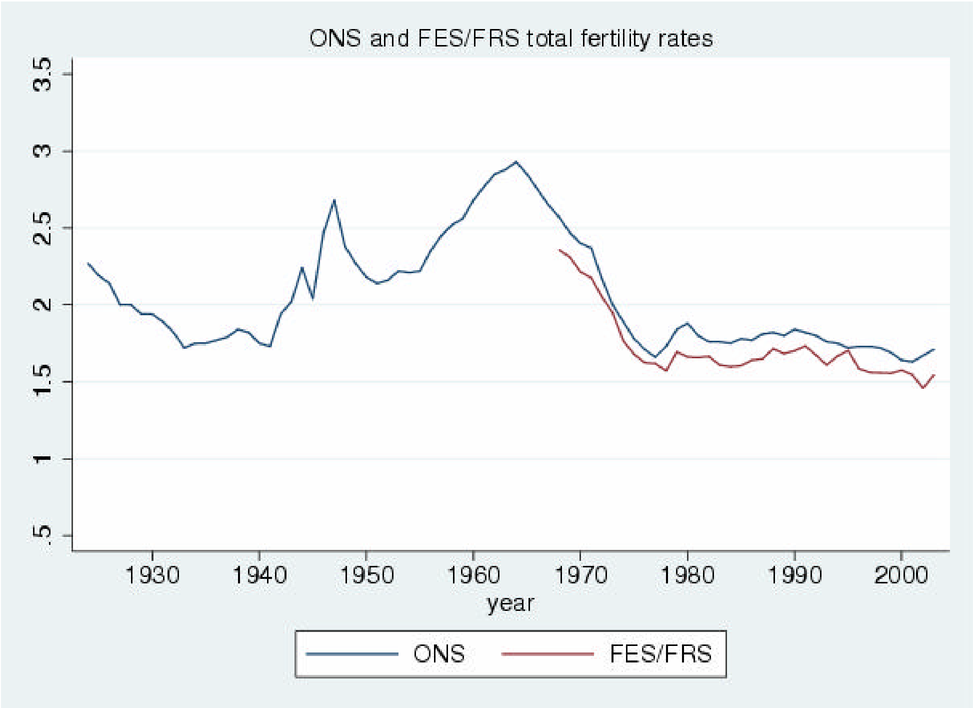

Table 2

Summary statistics

	All women aged 20-37	Women in couples aged 20-37
No. observations	86,234	53,142
Mean age	28.39	29.58
Proportion with birth in previous 12 months	0.088	0.120
Mean number of children	1.03	1.22
Left school at minimum school leaving age	0.467	0.461
Left full-time education after age 18	0.246	0.271
Proportion black	0.027	0.016
Proportion asian	0.043	0.048
Proportion other ethnic group	0.020	0.019

6 Empirical strategy

We would like to measure the effect of the reforms on the number of children among those women who are affected by the reform, i.e.:

$$E(N_1 - N_1' \mid T = 1) = E(N_1 \mid T = 1) - E(N_1' \mid T = 1)$$

where N_1 is the actual number of children within the group affected by the reform (the treatment group, T = 1) and N_1' is the number of children they would have had in the absence of the reform.

Of course, $E(N_1' \mid T = 1)$ is not known since there is no way of knowing how many children the treatment group would have had in the absence of the reform. The idea behind the difference in difference approach is to use the change in fertility over the same period of a control group to proxy for the change that otherwise would have occurred within the treatment group in the absence of the reform. Applying this change to the initial fertility level of the treatment group gives an estimate of $E(N_1' \mid T = 1)$.

The DD approach therefore measures the following:

$$[E(N_1 \mid T = 1) - E(N_0 \mid T = 1)] - [E(N_1 \mid T = 0) - E(N_0 \mid T = 0)]$$

where N_0 is the number of children prior to the reform and T = 0 contains the control group who are unaffected by the reform.

To identify the effect of the reforms, we exploit the fact that they were targeted at low-income households to define a treatment group who were affected by the reform, and a control group who were not.

Since eligibility for WFTC (and income support) was based on household earnings, an obvious choice is to split the population into treatment and control groups on the basis of earnings (see Table 3 for details of the split used). The advantage is that household earnings are likely to be strongly correlated with the reform's impact, at least in the short-term. But, earnings are likely to be endogenous and affected by the reform because of its impact on both employment and fertility. Furthermore, current earnings may be correlated with transitory shocks, while we would expect the impact of the reform to be greater for households with permanently low earnings.[13]

Table 3
Treatment and control groups – definitions

	Treatment	Control
Household earnings	Bottom third of household earnings distribution	Top third of household earnings distribution
Education	Both male and female partner left school at/before compulsory school leaving age	Both male and female partner left school at 18+
Earnings and education	Education treatment and income treatment	Education control and income control

Instead of earnings, education of both partners (and/or of just the woman) can be used as a time-invariant proxy for income to define treatment and control groups (details of the split given in Table 3). In the short-term at least, we can assume that education choices will be unaffected by the reform. Compared to earnings, the potential disadvantage of using education is that it is less strongly correlated with the impact of the reform. However, as shown in Table 4, the education split does well in picking up the differential impact of the reforms.

13 Of course, this is true only if households expect the reforms to be permanent, or at least to have a permanent effect on the level of child-contingent benefits.

Table 4

Receipt of child-contingent benefits

	Split by education		Split by earnings		Split by education/ earnings	
	Control	Treatment	Control	Treatment	Control	Treatment
Proportion entitled to FC/WFTC or IS						
Before	.098	.237	.125	.354	.000	.441
After	.141	.401	.175	.586	.006	.732
Mean weekly entitlement FC/WFTC, IS + child benefit						
Before	£29.71	£39.00	£32.38	£47.49	£20.92	£54.06
After	£37.27	£56.76	£41.02	£72.02	£24.70	£83.80
Difference	£7.56	£17.76	£8.64	£24.53	£3.78	£29.74
	(25.4%)	(45.5%)	(26.7%)	(51.6%)	(18.1%)	(55.0%)

Authors' calculations based on estimated entitlements to welfare programmes, FC and WFTC, calculated using TAXBEN, the IFS' tax and benefit calculator.

Finally, we also define treatment and control groups on the basis of education *and* household earnings to pick up households who are likely to have permanently low incomes (see Table 3 for details of the split). As shown in Table 4, the difference between the control and treatment groups is most pronounced for this variant.

Identifying the effect of the reform relies crucially on successfully controlling for everything else that might affect the fertility of the treatment group after the reform. In our regression analysis we include a rich set of demographic controls, including age, education, numbers and ages of children in the household, region, housing tenure and ethnicity. In principle, the inclusion of the control group is intended to capture the effect of other (unobservable) time-varying factors. But, the control group may differ to the treatment group, both in the level of their fertility (which the differencing takes care of) and, more problematically, in trends in their fertility (see Ratcliffe and Smith, 2006). Our analysis focuses on a relatively narrow window (four years before and four years after the reform's implementation), which should reduce the bias arising from differential trends, but we also explicitly allow for differential trends in the regression. Using the women's imputed fertility histories, we can extend the before period to get better identification of the differential trends; we also try estimating the effect of spurious reforms to test the sensitivity of our results. As well as allowing for differential linear trends, we also include the 25th and 75th percentile of the female and male wage distributions (matched to the treatment and control groups) to control for (non-linear) differential macro-economic effects.

As already noted, the DD methodology cannot precisely disentangle individual policy effects (such as separating the effect of WFTC from that of changes to income support). It will also include other reforms introduced at the same time that affected the fertility of the treatment group (and not the control group). In fact, a number of changes were made to maternity rights and child-care provision that may have affected fertility, including extensions to maternity leave and increases in free nursery provision.[14] In principle, all women were affected by these reforms, but in practice, the impact may have been greater for women in the low education group if they previously had less generous maternity provisions in their employment, and were less able to afford childcare. If so, then our DD estimate will also include the differential effect of these other reforms. We would argue, however, that the effects of these reforms is likely to be small compared to the impact of WFTC and the changes to income support.

In principle, we would like to measure the effect of the reforms on the total number of children over the fertility lifetime of affected cohorts. This would include older

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[14] See Hills and Waldfogel (2004) for a summary.

cohorts, who are likely to have already started their family formation process at the time of the reform, as well as younger cohorts who make all their fertility decisions facing the post-reform financial incentives. In practice, we do not have sufficient number of years' data after the reform to look at completed fertility for affected cohorts. Also, even if the data were available, it would be hard to attribute changes in fertility across cohorts separated by several years to a discrete policy reform.

In practice, therefore, we define the variable of interest as the probability of having a birth during the previous year, and compare the changes in these birth probabilities before and after the reform for the treatment and control groups. Since we can define birth probabilities immediately before and after the reform, we will be able to identify any effect more easily (compared to looking at changes in cohort completed family size). However, we cannot separate out changes in the timing of births (ie people choosing to have their children earlier in their lifetimes) from changes in the total number of births, although we investigate whether there have been changes in the age at first birth.

A final issue relates to the definition of the "before" and "after" periods in determining the effect of the reform. WFTC was announced in March 1998 and introduced in October 1999. Assuming no announcement effects, the reform would first have affected births from August 2000. We therefore include women interviewed between 1st April 1995 and 30th June 2000[15] in the before sample, and women interviewed between 1st August 2001 and 31st December 2003 in the after sample. For women interviewed between 1st July 2000 and 31st July 2001, the

15 We choose end of June rather than end of July to allow for premature births

introduction of the reform (plus nine months) occurs in the middle of the twelve month period prior to their interview and so they are omitted from the analysis.

What if there is an effect arising from announcement? This could increase births in the immediate before period if women respond to the announcement of the reforms rather than (or as well as) their implementation. This is not implausible – so long as the increase in benefits is credible, then the loss arising from the gap between announcement and implementation would be relatively small compared to the expected increase in benefits over the child's lifetime. Alternatively, women could decide after announcement to delay childbearing until after the reforms were implemented, which would tend to decrease births in the immediate before period (similar to "Ashenfelter's dip", see Ashenfelter, 1978). We test the sensitivity of our results to announcement effects by trimming the before sample at 31st December 1998 (nine months after the reform was announced).

7 Regression results

The outcome of interest is a binary variable equal to one if the woman had a birth in the previous twelve months, and equal to zero otherwise (*Birth*). The basic difference-in-difference specification includes a binary variable "low" equal to one if the individual belongs to the (low-education, low-income or low-education/income) treatment group, a binary variable "post" equal to one in the post-reform period, and an interaction term, low * post, which captures the difference in the change in the level of fertility after the reform for the treatment group (compared to the control group), our estimate of the effect of the reform. We include an additional interaction with a dummy for whether the woman has children at the beginning of the twelve-month period to allow the effect of the reform to vary by those with and without children. We also allow for the treatment and control groups to have different trends in their fertility. This leads to the following empirical specification:

$$Birth_i = a + b_1 (Low \times Post)_i + b_2 (Low \times Post \times Kids)_i + b_3 Post + b_4 Low$$

$$+ g_1 T + g_2 (T \times Low) + X_i d + u_i$$

X_{it} is a vector of characteristics which are assumed to affect fertility. These include a cubic in the woman's age (at the start of the twelve month period), interacted with education; the number of children in the household (at the start of the twelve month period), interacted with the woman's age and with the woman's age and education and with the age of the youngest child; region and housing tenure; woman's and partner's ethnicity and the 25th and 75th percentiles in the female and male hourly wage distribution. We include ethnicity as a control, but unlike US studies, we do not test for a differential response across ethnic groups, largely because of small sample sizes. We estimate this equation using a probit regression and report the average estimated marginal treatment effects for a number of different specifications in Table 5. A full set of results for the education split, including all the control variables, is given in the Appendix.

The results provide consistent support for an effect of the reforms on the fertility of the treatment group. Controlling for a wide range of other factors, this group experienced a (relative) increase in fertility following the reforms, which is statistically significant in most specifications.

Table 5

Probit regression results, average marginal effects

Dependent variable = had a birth in the previous 12 months

	(1) Education split Women, 20-37		(2) Education split Women, 20-45		(3) Earnings split Women, 20-37		(4) Earnings/ educ split Women, 20-37	
(a) No Announcement effects								
Treatment	.0181	.0294*	.0155*	.0254**	.0206*	.0242	.0312**	.0414*
	(.0134)	(.0169)	(.0084)	(.0111)	(.0119)	(.0151)	(.0179)	(.0265)
Treatment * kids		-.0153		-.0109		-.0048		-.0111
		(.0161)		(.0082)		(.0151)		(.0200)
N	19,950	19,950	30,268	30,268	24,943	24,943	12,782	12,782
(b) Announcement effects								
Treatment	.0194	.0321*	.0158*	.0235**	.0246**	.0310*	.0364**	.0514*
	(.0142)	(.0177)	(.0088)	(.0115)	(.0113)	(.0161)	(.0192)	(.0282)
Treatment * kids		-.0163		-.0098		-.0074		-.0159
		(.0121)		(.0085)		(.0124)		(.0199)
N	18,555	18,555	28,157	28,157	23,108	23,108	11,805	11,805
(c) No announcement effects, excluding trend terms								
Treatment	.0116	.0218	.0117	.0199*	.0139	.0172	.0209	.0298
	(.0122)	(.0157)	(.0079)	(.0106)	(.0106)	(.0140)	(.0152)	(.0239)
Treatment * kids		-.0137		-.0106		-.0045		-.0102
		(.0119)		(.0082)		(.0123)		(.0201)
N	19,950	19,950	30,268	30,268	24,943	24,943	12,782	12,782

Notes to table

** indicates coefficient is statistically significant at the 5% level, * at the 10% level

Sample is women in couples. Treatment refers to the treatment group in the post-reform period. Regressions include a dummy for the post-reform period and a full set of controls for age of mother, education, age and number of children in the household, region, housing tenure and ethnicity. See Appendix for a full set of results.

The magnitude of the estimated effect ranges from 1.2 – 3.6 percentage points depending on the specification. As expected, the effect is greater when the treatment group is defined in terms of earnings and earnings/education interactions since this split is more closely correlated with the reforms' impact. However, as already discussed, there is a concern about the potential endogeneity of household earnings, whereas the split by education is unaffected by the reform.

The results in column (2) confirm that the findings are not driven by selecting on women aged 20 – 37, which might be the case if educated women experienced a stronger trend towards later childbearing. Including women up to age 45, the results are broadly similar and the larger sample size is associated with greater statistical significance. The results allowing for announcement effects, shown in panel (b) are also broadly similar.

Panel (c) shows that the magnitude of the coefficients is sensitive to including trend terms. In general, although the effect of the reform is still positive, the coefficients are smaller and typically insignificant. As a further robustness check, we use the derived fertility histories[16] to extend the pre-reform period back to 1985. Table 6, column (1) shows the results using the longer pre-period to capture the differential trends in fertility, while columns (2) and (3) show the results of modelling the effects of spurious reforms in 1995 and 1996. The estimated effect using the longer pre-reform is smaller, but is statistically significant. Moreover, the results from the estimates including the spurious reforms confirm that the change in fertility lines up with the introduction of the actual reforms in 1999.

[16] Based on the number and ages of the children in the household, we estimate birth probabilities for each woman in the sample at each age (and derive information on the number and ages of the children in the household). For further information, see Ratcliffe and Smith (2006)

Table 6

Allowing for a longer pre-reform period

Dependent variable = had a birth in the previous 12 months

Education split	(1)		(2)		(2)	
Women, 20-27	Actual reform, 1999		Spurious reform, 1995		Spurious reform, 1996	
	Coeff	SE	Coeff	SE	Coeff	SE
Treatment	.0069*	.0039	.0013	.0034	.0021	.0035
N	563,929		474,821		479,564	

* indicates coefficient is significant at the 10% level
Sample is women in couples. Treatment refers to the treatment group in the post-reform period.
Regressions include a common dummy for the post-reform period, a trend, interacted with education and with the post-reform dummy, controls for age of mother, education, age and number of children in the household, region and housing tenure.
Standard errors adjusted for clustering since multiple observations on each woman are not independent
Spurious reform, 1995: Before = Apr 85 – Mar 95; After = Apr 96 – Mar 99
Spurious reform, 1996: Before = Apr 85 – Mar 96; After = Apr 97 – Mar 99

The results in table 5 show that the effect of the reforms was stronger for women who did not already have children. Table 7 explores this further by looking at the effects according the number of children already present in the household and the age of the youngest child, both at the start of the twelve months. The results show that the biggest effect was on the first and third births, and also that the reforms had less of an effect for households whose youngest child was aged three or less, or aged eight or over.

A bigger effect for first births is consistent with the fact that benefits increased by more for first births than for second and subsequent births. However, previous studies have also found a stronger effect of financial incentives on first births. Laroque and Salanie (2005), for example, find a stronger effect of the French *Allocation Parentale d'Education* on first births even though the actual financial incentives were triggered by second or higher births. In the case of women with very young children, the lack of an effect may correspond to a natural break between births, and this is supported by the results by age of youngest child. But, it may also reflect the effect of strong underlying preferences for two children. If so, the decision whether to have children (or at least when to begin having them) may be more susceptible to financial incentives than the decision over how many to have, once childbearing has begun.

Table 7

Allowing for differential response by number and age of kids

Dependent variable = had a birth in the previous 12 months

Education split	(1) Presence of kids		(2) Number of kids		(3) Age of youngest	
Women, 20-27	Coeff	SE	Coeff	SE	Coeff	SE
Treatment	0.0294	0.0169*	0.0294	0.0169*	0.0294	0.0169*
T * kids	-0.0153	0.0161				
T * 1 kid			-0.0191	0.0134		
T * 2 kid			-0.0058	0.0154		
T * 3 or more kids			-0.0188	0.0176		
T * youngest_01					-0.0262	0.0144*
T * youngest_23					-0.0177	0.0151
T * youngest_45					-0.0061	0.0189
T * youngest_67					-0.0027	0.0247
T * youngest_89					-0.0183	0.0259
T * youngest_10+					-0.0218	0.0319
N	19,950		19,950		19,950	

** indicates coefficient is significant at the 5% level; * at the 10% level
Sample is women in couples. Treatment refers to the treatment group in the post-reform period. Regressions allow for differential trends and include a common post-reform dummy and a full set of controls for age of mother, education, age and number of children in the household, region, housing tenure and ethnicity.

Finally, we investigate whether treatment impacts vary by the age of the woman. Purely on biological grounds it might be expected that some differences may exist in fertility responses to financial incentives by age. Also, women of different ages will be at different stages in the fertility lifetimes at the time of the reform. However, the results, given in Table 8, show no significant difference between responses among women aged 30+ (the base treatment group), those aged 20-24 and those aged 25-29.

Table 8

Allowing for differential response by woman's age

Dependent variable = had a birth in the previous 12 months

Education split Women, 20-37	Coeff	SE
Treatment	0.0166	.0149
T * 20-24	0.0039	.0157
T * 25-29	0.0027	.0137
N	19,950	

Sample is women in couples. Treatment refers to the treatment group in the post-reform period. Regressions allow for differential trends and include a common post-reform dummy and a full set of controls for age of mother, education, age and number of children in the household, region, housing tenure and ethnicity.

The estimated positive effect of the reforms may reflect women beginning childbearing earlier and bringing forward births they otherwise would have had later on. In this case, the overall increase in births will be smaller than our estimated coefficients suggest. However, we do not find that the reforms had a significant effect on age at first birth. For women who have had children, we regress age at first birth (estimated using information on the number and ages of children in the household) on a set of controls for education, region, housing tenure and ethnicity. We include a full set of date of birth cohorts, and interact these with education, and allow for differential trends. The results are reported in Table 9. For both our selected sample and a larger sample of women up to age 45, the coefficient on the low education group is actually positive, albeit insignificant. This suggests that the reforms were associated with a genuine increase in births, rather than just a change in timing.

Table 9

Effect on age at first birth

Dependent variable = age at first birth

Education split	Women aged 20-37	Women aged 20-45
Treatment	0.2912	0.2876
	(0.4241)	(0.3608)
N	14,267	22,865

Sample is women in couples. Treatment refers to the treatment group in the post-reform period. Regressions allow for differential trends and include a common dummy for the post-reform period, controls for cohort of birth (interacted with education), region, housing tenure and ethnicity

Finally, we have focused on women in couples since that is the group where we expect to find the greatest effect (because of the pro-employment effects of WFTC for lone mothers). We confirm that this is the case by running a regression on the sample of all women. Since partner information is missing for single women, we split the sample on the basis of the woman's education only. We define the treatment group to be women who left school at the compulsory school leaving age and the control group to be women who left school at 18-plus. The results are reported in Table 10. While neither of the estimated treatment effects is significant, they support our argument that the positive effect is more likely for women in couples and indeed the coefficient for single women is negative.

Table 10

All woman sample

Dependent variable = had a birth in the previous 12 months

Education split Women, 20-37	Coeff	SE
Treatment	-0.0232	.0310
T*in_couple	0.0353	.0452
N	47,329	

Sample is all women. Treatment refers to the treatment group in the post-reform period. Regressions allow for differential trends by education and for women in couples. They include dummies for the post-reform period, and a full set of controls for age of mother, education, age and number of children in the household, region, housing tenure and ethnicity.

8 *Discussion and conclusions*

The reforms that took place in the UK in 1999 present an excellent case study for addressing the question of whether government support for families with children affects fertility, largely because of the scale of the increases.

We argue that when looking at the impact of welfare-to-work programmes such as WFTC and EITC, the overall impact on fertility is ambiguous because of the pro-employment effects of these programmes. In practice, however, the effect on fertility is likely to vary between women in couples and lone mothers, and is more likely to be positive for women in couples. We provide evidence that this is the case, consistent with earlier findings from the US. These findings imply that it is crucially important to estimate fertility effects separately for these different groups of women.

When we focus on couples, we find evidence that the increase in payments to families with children increased the probability of a birth. This is an important, if unintended consequence, of increasing benefits to families with children. Taking the smallest estimated effect, our results indicate that the probability of having a birth increased by 1.2 percentage points among the low education group, equivalent to a 10 per cent increase, or nearly 20,000 additional births. Since entitlement to benefits increased by 45 per cent among this group, the implied elasticity is around 0.22.[17] This is towards the upper end estimated by Whittingdon, but much smaller than the recent findings of Milligan (2005) for the pro-natalist Allowance for Newborn Children in Quebec. He found that the probability of birth increased by 17 per cent for a $Can 1,000 total

[17] The implied elasticities based on the income and income/education splits are 0.15 and 0.25 respectively. These estimated elasticities assume a zero response among the control group. Since benefits did increase for the control group, but by less, the implied elasticity could be greater in practice.

increase in support, whereas, in the UK, the increase in benefits for the low-education group was equal to an additional £1,000 *each year*. The implied elasticity is greater than Baughman and Dickert-Conlin found for EITC in the US. The difference may be attributable to the greater magnitude of the UK reforms.

Finally, our findings add to the growing body of evidence that the effect of financial incentives is likely to vary by birth order, and is typically stronger for first births than for subsequent births. This implies that the reforms had an effect on the fertility decisions of households who were not (yet) receiving the benefits (ie those with no children). However, there is supporting evidence of high levels of awareness of the new benefits even among those who were not receiving it, which may have come about as a result of the extensive television advertising and/or through word-of-mouth. A survey carried out for the UK Department for Work and Pensions in summer 2000 (repeated in summer 2001) found that 33 per cent (42 per cent in 2001) of low/moderate-income couples were aware of WFTC although they had never received it (McKay, 2000 and 2001). Given this evidence, we would argue that the first-birth effect is not implausible.

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Table A1

Probit regression results

Dependent variable = birth in last 12 months

	Coeff	SE
Age_highed	-2.554	0.837
Age_lowed	-0.972	0.572
Age_1kid_highed	2.712	1.951
Age_2kids_highed	3.501	3.334
Age_3+kids_highed	-1.611	9.789
Age_1kid_lowed	0.711	0.848
Age_2kids_lowed	2.640	1.118
Age_3+kids_lowed	1.476	1.757
Agesq_highed	0.976	0.300
Agesq_lowed	0.357	0.213
Agesq_1kid_highed	-1.008	0.675
Agesq_2kids_highed	-1.288	1.118
Agesq_3+kids_highed	0.232	3.181
Agesq_1kid_lowed	-0.247	0.312
Agesq_2kids_lowed	-0.969	0.399
Agesq_3+kids_lowed	-0.524	0.612
Agecubed_highed	-0.118	0.035
Agecubed_lowed	-0.044	0.026
Agecubed_1kid_highed	0.121	0.077
Agecubed_2kids_highed	0.151	0.124
Agecubed_3+kids_highed	0.003	0.342
Agecubed_1kid_lowed	0.028	0.038
Agecubed_2kids_lowed	0.115	0.047
Agecubed_3+kids_lowed	0.060	0.070
1kid_highed	-23.090	18.582
2kids_highed	-30.366	32.887
3+kids_highed	25.853	99.745
1kid_lowed	-6.741	7.558
2kids_lowed	-23.904	10.306
3+kids_lowed	-14.112	16.656
1kid,youngestage23	0.497	0.054
1kid,youngestage45	0.364	0.071
1kid,youngestage67	0.034	0.093
1kid,youngestage89	-0.078	0.117
1kid,youngestage10+	-0.251	0.108
2kids,youngestage23	0.313	0.067
2kids,youngestage45	0.171	0.082
2kids,youngestage67	0.049	0.103
2kids,youngestage89	-0.033	0.127
2kids,youngestage10+	-0.159	0.139
3+kids,youngestage23	0.057	0.099
3+kids,youngestage45	0.229	0.110
3+kids,youngestage67	-0.345	0.173
3+kids,youngestage89	0.063	0.171
3+kids,youngestage10+	-0.338	0.295
Region = North East	-0.066	0.072
Region = North West	0.025	0.054
Region = Yorks&Humbs	-0.029	0.059
Region = EastMidlands	0.001	0.058
Region = WestMidlands	0.013	0.055
Region = Eastern	0.044	0.058
Region = SouthEast	0.055	0.052
Region = SouthWest	0.064	0.057

Region = Wales	0.048	0.068
Region = Scotland	-0.002	0.055
Region = Northern Ireland	0.195	0.102
Tenure = social housing	0.159	0.033
Tenure = Private rented	-0.156	0.042
Tenure = other	0.176	0.173
Black	0.113	0.136
Asian	0.094	0.133
Other ethnic group	-0.147	0.124
Partner = Black	0.096	0.132
Partner = Asian	0.235	0.133
Partner = other ethnic group	0.156	0.126
Female wage	0.069	0.205
Male wage	-0.116	0.139
Trend	-0.008	0.014
Trend * Lowed	-0.006	0.004
Post	0.007	0.099
Post * Lowed	0.096	0.068
N	19,950	