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Evidence from the Working Families Tax Credit in the UK

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Abstract

The introduction of the 1999 Working Families Tax Credit (WFTC) in the UK encouraged low income families with children to enter the labor market. The tax credit, however, may have had the unintended side effect of increasing the childbearing of these households. While many studies have looked at the importance of WFTC on the female labor supply, only few have estimated the impact it had on fertility decisions of British families. This paper employs the 1995 to 2003 British Household Panel Survey and identifies the policy impact of WFTC by observing the change in the probability of birth as well as the timing of birth using the difference in differences estimator. The main findings of this paper suggest that single women responded to the policy introduction by reducing the probability of birth and prolonging the birth intervals across all birth parity. For women with partners, on the other hand, the estimates indicate that financial incentives did not encourage them to enter motherhood but it rather induced women to have their second birth quicker.

Keywords: Fertility, Welfare policy, Working Families Tax Credit, Difference-in-Differences.
JEL Classification: J13, J18

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1. Introduction

The UK Working Families Tax Credit (WFTC) was introduced in October 1999 and it replaced the former tax credit, Family Credit (FC). Its central aim was to increase the returns generated from work for families with children in order to reduce the poverty level. WFTC was considerably more generous compared to FC in four ways. First, the maximum credit given to families increased compared to FC. Second, the rate at which the tax credit is deducted was reduced from 70 to 55 percent. Third, the income threshold at which the deduction of the tax credits increased from £79 to £90. Moreover, while childcare cost was disregarded from the credit calculation in FC, WFTC subsidized 70 percent of childcare cost that was incurred by using registered childcare. Although the structure of the tax credit was designed to encourage parents into work, larger credit and childcare cost support may have had an unintentional impact on the childbearing decisions of recipient families. The past literature looked at the impact of WFTC on female labor supply but there has been little research into how fertility behavior has been influenced. An investigation on the fertility response would therefore provide a better understanding of the full impact of WFTC. Additionally, parents in the UK have reportedly faced exceptionally high childcare cost relative to families in the rest of the Europe (The Daycare Trust (2003)). The reduction in the cost of childcare may have induced a stronger fertility response in the UK than elsewhere. This makes the UK policy impact on fertility an especially interesting case to study. This paper aims to contribute to the understanding of the effect of WFTC and provide additional insight into the impact of welfare policies on childbearing by presenting an empirical analysis of the UK experience.

Identification of these policy impacts ideally requires comparing individuals' fertility behavior with and without the policy influence. Unfortunately, it is not possible to observe the same individuals in both states. In order to prevent the "missing counterfactual" problem, it is necessary to define a control group of individuals who were not affected by the policy and are very similar in characteristics to treated individuals. Due to the possible systematic level differences in their fertility behavior between the two groups, this paper employs the difference-in-differences estimator which takes the change in differences between these two groups over time.

Whilst majority of studies on the impact of financial incentives on fertility come from the US, evidence is limited for the UK and Europe. The exceptions are the two papers by Francesconi and Van der Klaauw (2007) and Brewer et al. (2007) which also looked at the WFTC impact on fertility. Francesconi and Van der Klaauw (2007) used the British Household Panel Survey (BHPS) and estimated the policy effect on lone mothers' transition to having additional children and found insignificantly negative fertility responses among this group of women. Brewer et al. (2007) on the other hand, estimated the impact of WFTC on women in couples using the Family Resources Survey (FRS) and the Family Expenditure Survey (FES). Their estimates suggest a significantly positive effect of WFTC on the probability of birth particularly on the first birth.

This paper contributes to the existing literature by providing a complementary evidence of the WFTC impact on the probability of having another child. In addition to this, it also studies the policy impact on the timing of birth. The observed fertility response after the policy introduction may not only reflect individuals increasing the number of children but also shortening of their birth intervals. Investigation on the time to birth would thus provide an alternative picture of the policy impact. In addition, this paper proposes a possible solution to overcome the problem of endogeneity faced by studies with cross-sectional data. Since eligibility of WFTC is closely related to the household income level, defining groups based on the level of households' income ensures an accurate split of the sample into those who were affected and unaffected. However, due to the impact of WFTC on both fertility and employment, the level of household income is likely to have been determined simultaneously with fertility (Brewer et al. (2007)). This paper attempts to overcome this problem by using the panel nature of BHPS. Specifically, since BHPS allows the same individuals to be observed over years, household income information prior to the policy introduction is used as a proxy as the income information after 1999.

It is worth noting that the UK, during the period of focus saw several welfare policy reforms. In particular, the generosity of Income Support (IS) payments to unemployed couples and non-working lone parents with dependent child aged below 11 increased at the same time as the introduction of WFTC. Due to the lack of ability of natural experiments to separate the two policy effects, estimates not only include WFTC impacts but also reflect the effect from the increased payment of IS (see Francesconi and Van der Klaauw (2007) for more details). Since WFTC affected families from a wide range of income distribution and also because of the significant increase in generosity from the childcare subsidy component of WFTC, the estimates are assumed to mainly reflect the impact of WFTC introduction.

The rest of the paper is organized as follows. Section 2 and 3 provides background information regarding WFTC such as the detailed explanations of the structure of the tax credit and different features compared to FC and expected impact of WFTC introduction. Section 4 summarizes the past empirical literature. Section 5 describes the econometric specifications while section 6 discusses the data employed. Section 7 looks at the estimated results and section 8 concludes.

2. The structure of Family Credit and the Working Families Tax Credit

The Working Families Tax Credit (WFTC) was introduced in October 1999 approximately 19 months after its announcement in March 1998, replacing the former Family Credit. It was the main in-work tax benefit for families with children in the UK until it was replaced by the new tax credits, Working Tax Credit and Child Tax Credit in April 2003. Since WFTC was preceded by FC, a study of the impact of the policy introduction requires comprehension of the structure of WFTC in relation to FC.

Table 1 summarizes the eligibility conditions and each element of the two tax credits. They were both granted to parents of low income families in which at least one parent was working 16 hours a week or more. The amount of both tax credits was contingent on the number and the age of children in households as well as the household income. Additional credit was given to those families if at least one parent was working longer than 30 hours. For both tax credits, the maximum amounts were given to families earning less than a threshold. Once the level of household income reaches this threshold, the credit was reduced at a specified rate.

(Table 1)Structure of FC and WFTC (Amount of tax credits per week)

FC (1999 rate)	**WFTC (1999 rate)**	**Eligibility condition**
Adult credit £48.80	**Basic tax credit** £52.30	At least one earner working more than 16 hours and has a child
Child credits £15.15(Under 11) £20.90(11-16) £25.95(16-18)	**Child tax credit** £19.85(Under 11) £20.90(11-16) £25.95(16-18)	At least one earner working more than 16 hours and has a child
30 hour tax credit £11.05.	**30 hour tax credit** £11.05	At least one earner working more than 30 hours
Childcare disregard Childcare costs of up to £60 for one child and £100 for two or more children were disregarded from the family's income when the calculation was made.	**Childcare cost subsidy** 70% of eligible childcare cost (£100 for one child and £150 for two or more children) were subsidized.	Both adults working more than 16 hours and has a child (eligibility condition for Childcare tax credit)
Income threshold £79	**Income threshold** £90	N/A
Deduction rate 70%	**Deduction rate** 55%	N/A

Although these two tax credits had similar structures, WFTC was significantly more generous compared to FC. The generosity of WFTC stems from four main components of the WFTC elements. Compared to FC, the amount of maximum credit increased.[2] Moreover, the threshold for the entitlement of maximum credit was raised from £79 to £90. Additionally, the rate at which the tax credit was deducted when weekly income exceeded the threshold was reduced from 70 to 55 percent. Lastly, WFTC provided more financial support towards the childcare cost. The

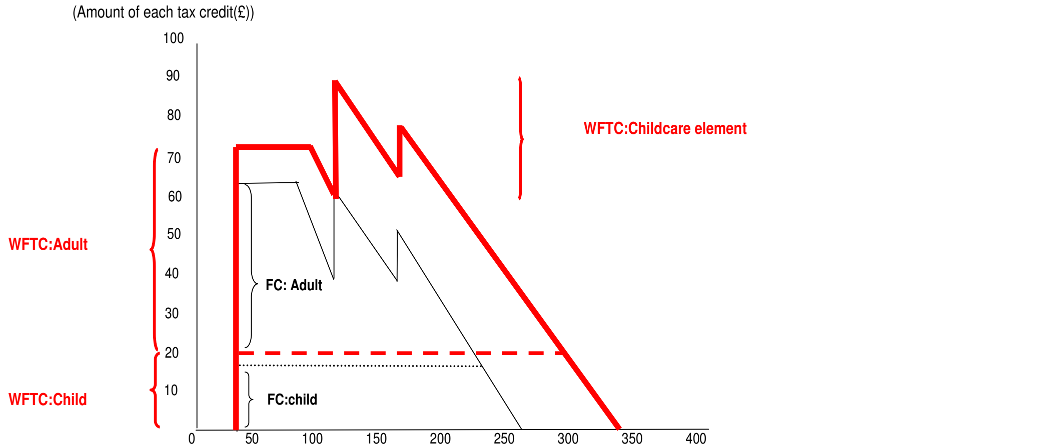
[2] Components of WFTC which increased were mainly the basic tax credit and child credit for children aged under 11 years old.

childcare cost subsidy was given to families if both parents were working more than 16 hours a week. For lone parents, entitlement to WFTC automatically ensured eligibility to apply for the subsidy. Under FC, childcare cost up to £60 could be disregarded from the family's income when the calculation was made. However, it was often criticized since families with maximum tax credits did not attain additional support for their childcare cost. WFTC, on the other hand, supported the cost more actively by providing subsidies for 70% of childcare cost up to £100 a week for families with one child and £150 a week with two children or more. Moreover, the applicable age of children was raised from 11 to 15.

Figure 1 illustrates the increased generosity of in-work benefit from FC to WFTC in 1999. The thin line shows the structure of FC while the thick line indicates WFTC. The initial difference in the amount of tax credit is due to the larger amount of each element of WFTC compared to FC. Both credits are deducted after a threshold. As noted earlier, this threshold level was higher under WFTC compared to FC and thus the deduction starts later for the new tax credit. The slower rate of deduction under WFTC is reflected in the less steep slope at the taper. The large jump in the amount of tax credits around the level of £115.2 is due to the childcare disregard or the childcare cost subsidies which became available when both parents work more than 16 hours a week. Finally, a smaller jump seen around £158.94 is due to the additional awards given to families working 30 hours or more.

Tables 2 and 3 present marginal subsidies of having a child separately by marital status as well as the number of earners in the household. Table 2 does not take account of the childcare cost whilst table 3 assumes that the family spends £45 per week on childcare. The entitled amounts of tax credits are calculated assuming that the working parents receive minimum wage at the 1999 rate and work 16 hours a week. The first number in each cell shows the amount under FC while those in parenthesis are the amounts under WFTC. From Tables 2 and 3, two earners couples experienced more increase in tax credits compared to single earner couples, providing incentives for families to become two earners couples. Additionally, childcare cost subsidies contributed to a large proportion of the increase in tax credits.

(Figure 1) Comparison of the structure of FC and WFTC



- * A family with one child under 11 years old.
- * Parents work at hourly wage of 3.6 pounds.
- * Childcare cost assumed to be 45 pounds per week.
- * Housing benefit, coucil tax benefit ignored.
- * Both parents are assumed to be working 16 hours or more when the net household income reaches £115.2 per week and hence becomes eligible to receive childcare element of FC and WFTC .
- * One parent is assumed to be working 30 hours or more when the net household income is over £158.94.

(Table 2): Marginal subsidy without childcare cost

	Two earners	Single earner
Couple	£38.61 (£58.29)	£63.95 (£72.15)
Single	- (-)	£63.95 (£72.15)

N.B. Individuals are assumed to be working at the 1999 rate of minimum wage (£3.6) for 16 hours a week.
First value in each cell shows the FC amount given to families with a child aged under 11. Values in parenthesis show the amount of WFTC.

(Table 3): Marginal subsidy with childcare cost (£45/week)

	Two earners	Single earner
Couple	£63.95 (£89.79)	£63.95 (£72.15)
Single	- (-)	£63.95 (£103.65)

N.B. Individuals are assumed to be working at the 1999 rate of minimum wage (£3.6) for 16 hours a week.
First value in each cell shows the FC amount given to families with a child aged under 11. Values in parenthesis show the amount of WFTC.

3. Potential Impact of WFTC

Economic theoretical framework provides us with three possible channels through which WFTC could have affected the fertility decisions of the UK women. The eligible individuals experienced a positive income effect through the increased entitled amount of WFTC compared to FC. Provided that children are normal goods, this positive effect raised the demand for children. Moreover, the generous childcare cost subsidy reduced the cost of having additional children and hence increased the demand for children. Finally, the changes in the level of threshold and the deduction rate increased incentives for individuals to enter the labor market. This in turn raised the opportunity cost of mothers' time and thus reduced the demand for children. The understanding of the overall effect on fertility, however, must consider the interactions between these three channels. In particular, the interweaving interactions between the negative labor supply effect and the positive effects from the raised credit and childcare cost support is a vital element in the determination of the policy impact.

In order to comprehend the policy impact on fertility, the WFTC effect on the employment and labor supply effects need to be considered. Ex-ante analysis of the impact of WFTC on female labor supply includes studies by Blundell et al. (2000), Blundell et al. (2005) and Brewer et al. (2005). Blundell et al. (2000) applies a discrete choice structural approach on the 1994-1995 and 1995-1996 Family Resources Survey (FRS). Their simulation suggests a positive policy impact on lone mothers' number of hours worked as well as their participation rate. They also observed that the policy increased both the number of working hours and participation of women with unemployed partners. On the other hand, women with employed partners are estimated to have reduced their hours and participation rate. Brewer et al. (2005) also use a structural model of labor supply using FRS data pre and post reform and find very similar results to those in Blundell et al. (2000). Blundell et al. (2005) apply the difference-in-differences method using FRS and the Labour Force Survey to carry out an ex-post analysis. According to their estimation, lone mothers experienced a positive employment effect while married mothers saw little change in their participation rates. In summary, there seem to be strong positive impacts of WFTC on lone mothers' and women with unemployed partners' labor supply and participation, while evidence shows little impact for mothers with employed partners.

Given the above effects of WFTC for the labor supply and employment, combined effects of the three channels on the British women's fertility behavior are as follows. Single women as well as women with unemployed partners experienced an ambiguous impact from WFTC, since they were affected by all three channels. Single women may not respond strongly to financial incentives in the short observation period available to this paper, since these individuals are not in stable relationships. Lastly, women with employed partners are expected to have experienced an unambiguous positive impact. This group of women were mainly affected by the positive income effect, since they were influenced little by the negative labour supply effect.

An alternative theory suggests a possible negative impact of the larger amount of tax credit on fertility incentives (Becker (1991)). According to the theory, the parents not only care about the family size but also the quality of children. In this framework, higher household income would lead to negative substitution effect due to the increased cost of improving the quality of children by devoting more goods. This additional negative substitution effect may have offset the positive income effects, leaving the overall impact to be ambiguous for all women regardless of their marital status and their partners' employment status.

4. Previous Literature

The majority of past literature investigating the impact of family policies on fertility comes from the US while a limited number of studies are carried out in the rest of the world, including the UK.

4.1. US Evidence

4.1.1. Evidence from 1970s to mid 1980s

The US literature focuses on the impact of policies on out-of-wedlock birth. This is due to the historical background that policies favorably supported single parents. For example, the main welfare system for families with children of 1970s to mid 1980s was the Aid to Families with Dependent Children (AFDC). This was a cash support program which targeted children living without at least one of their biological parents. Summary by Moffitt (1997) reveals the mixed conclusions drawn from various studies in the 1970s to mid 1980s. According to Moffitt (1997), studies from the 1970s find almost no impact of the Aid to Families with Dependent Children (AFDC). This consensus is changed when various papers in the 1980s found positive impact of such policies on out-of-wedlock births but with varying degrees of effect. Through meta-analysis, Moffitt (1997), suggests that the choice of data and methodologies affected the size of effects estimated and concludes that the extent of variations in the estimates makes it difficult for him to derive any decisive judgment on the effectiveness of cash benefit policies such as AFDC.

4.1.2. Evidence from 1990s

The 1990s in the US saw several reforms in family policies due to the introduction of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). PRWORA replaced AFDC with the Temporary Assistance for Needy Families (TANF) and allowed each state a greater flexibility to implement state specific cash assistance programs. As part of a response to this reform, states imposed family cap policies in the hope to reduce out of wedlock births. Family cap policies restricted or abolished the incremental increase in the amount of cash benefit given to families with a new born child, reducing the incentive for families to give additional birth. States individually experimented on the level of the restrictions, thus presenting an ideal environment for empirical investigations. Studies that employed the experimental design by Jagannathan et al. (2004) and Jagannathan (2003) as well as a natural experimental study by Horvath and Peters (1999) find negative impact of these policies on fertility. Using data from New Jersey's Family Development Program, both Jagannathan (2003) and Jagannathan et al.(2004) find lower birth rates for treated individuals. Although their evidence suggests that the family cap policies were effective in reducing the lone motherhood, these studies are not without problems. Loury (2000) reveals problems associated with the design of the experimental data, New Jersey's Family Development Program, used by Jagannathan et al. (2004), Jagannathan (2003). In particular, he suggests problems with the confusion among the participants of the experiments regarding the policies that applied to them and the non-randomization of the participant allocation to each group. Joyce et al (2004), on the other hand, suggests that the short period of post reform observations available from the state-level panel data between 1984 and 1996 used by Horvath and Peters (1999) may not have allowed enough responses to be observed, particularly when 20 out of 27 states implemented family cap policies after 1995 and the data was only collected up until 1996. The majority of other natural experimental studies (Kearney (2004), Levine (2002), Dyer and Fiarlie (2003) and Ryan et al (2006)) find an insignificantly positive impact of the family cap policies, contrary to the theory. Other studies by Kaushal and Kester (2001) and Joyce et al (2004)) also find either mixed or insignificant results. A possible reason for the contradicting results is the difficulty of natural experimental methods to separate policies that were implemented at the same time as the family cap. For example, the Earned Income Tax Credit, which will be discussed later, significantly expanded their tax credit in 1991 and made the entitlement more generous. The studies of the impact of family cap, however, seem to suggest a small and insignificant impact of cash benefits on fertility decisions.

4.1.3. Evidence on the Earned Income Tax Credit

Two very relevant studies to this paper from the US literature are on the impact of The Earned Income Tax Credit (EITC) on fertility by Baughman and Dickert-Conlin (2003), (2006). EITC was introduced in 1975 and its structure and eligibility conditions are very similar to that of WFTC. Although it has gone through various reforms, the one of particular interest is the 1990 Omnibus Budget Reconciliation Act (OBRA). Prior to OBRA, the maximum EITC did not depend on the number of children. This was changed and the maximum EITC became higher for families with two or more children compared to only one child and hence it gave an incentive for women to have more children. Using the US birth certificate data between 1990 and 1999, Baughman and Dickert-Conlin (2003, 2006) study the impact of the OBRA reform on fertility behavior. Baughman and Dickert-Conlin (2003) estimate their linear model separately for married and single women as well as white and non-white women and show that for unmarried white women, an increase in EITC is negatively correlated with first births while for non-white women, the relationship is positive. On the other hand, a positive impact is observed for white and non-white married women but the result is only significant for non-white women. A similar regression is estimated by Baughman and Dickert-Conlin (2006) but the impact is estimated for first and higher-order birth of white and non-white women. Their results suggest that while white women generally reduced their fertility, non-white women responded to the increase in the Base EITC by increasing their birth rates.

4.2. Canadian Evidence

Zhang et al. (1994) investigates the impact of three main policies throughout 1920s to 1980s on Canadian women's childbearing decisions. These three main tax policies were the personal tax exemption, the family allowance benefit and the child tax credit. The personal tax exemption was introduced in 1918 and it deducted a lump sum amount from the taxable income. On the other hand, the family allowance benefit, introduced in 1945, was an allowance which was distributed to all families with children under 18 years old. Finally, child tax credit, which was introduced in 1977, was a financial support for families that were in receipt of the family allowance. Using 1921-1988 time series data from Canada, he finds that the family allowance had a positive and significant effect. The personal tax exemption and child tax credit had positive effects but the statistical significance varied according to the specifications employed. While the study shows evidence that Canadian pro-natalist policies positively impacted the fertility behavior of women, the use of time series data may be problematic. In particular, time series variation is not sufficient to identify the impact of the policy if unobserved characteristics which affect the childbearing decision vary over time (Milligan (2005)).

Studies by Duclos et al. (2001) and Milligan (2005) explore the effect of a more recent policy, the Allowance for Newborn Children (ANC) which was introduced in Quebec. ANC was a transfer policy that operated between 1988 and 1997 and paid a lump sum cash benefit up to C$8000 for every additional child a woman had. The amount of cash transfer differed according to the birth rank within the family. The paper by Duclos et al (2001) estimates the conditional transition probabilities of moving from zero to first parity from first to second parity by age group. Their estimates from the difference-in-differences model show that ANC increased the conditional probability of transition to first parity by 21% and to second parity by 15%. Additionally, ANC increased the conditional probability of transition to third parity by 26 to 35%. Using 1980-1997 vital statistics and micro-data derived from the public use files of the Canadian Census, Milligan (2005) identifies the impact of the same policy using the difference-in-difference-in-differences method. From vital statistics, he finds that ANC increased the fertility rate by 9% while he estimates an impact ranging from 5.6% to 25% using the census data. The evidence, therefore, indicates a positive impact of ANC on fertility.

4.3. European Evidence

European evidence is scarce, possibly due to the lack of exogenous variations in the price of having a child. One exception to this is France. France has recently received attention from the rest of the world due to their improved fertility rate. The increase has often been associated with the extension of Allocation Parentale d'Education (APE) which was implemented since 1995. Prior to 1994, APE only provided financial assistance for the third child. The eligibility was extended to include second child and provided around 500 euros every month for three years, thereby providing a positive fertility incentives. Laroque and Salanie (2004, 2005) investigate the impact of the reform on the fertility activity in France. Their identification strategy involves the structural modeling of female fertility and participation which takes account of the French tax and benefit system. The specifications of the two studies are very similar to each other, but while Laroque and Salanie (2004) focus on a myopic setting; their paper in 2005 builds a dynamic model. Additionally, Laroque and Salanie (2005) include over 75 variables to correctly capture the fertility incentives while their estimation in their 2004 paper contained a smaller number of covariates. Using the 1999 French Labor Force Survey, Laroque and Salanie (2004) suggest that APE only affected low parity birth. As APE was structured to influence the fertility incentives of second birth, their result is contradictory to their hypothesis. On the other hand, through simulation, their 2005 paper concludes that APE increased births of second parity by 10.9% but reduced the third parity by 2.4%. The total effect of APE is estimated as a 3.7% higher birth rate.

4.4. UK Evidence

Two papers from the UK are of particular interest to this study as they have also investigated the impact of WFTC on childbearing decisions. Francesconi and Van der Klaauw (2007) devote a section of their paper on the issue of the impact of WFTC on lone mothers' fertility behavior using 1991-2001 annual data from BHPS. The data is arranged as a pooled interview year panel and their dependent variable takes the value one if a woman gave birth in a specific interview year. They use the linear probability model to estimate the transitions to birth provided that the lone mothers are observed for at least two consecutive years. The transition probability is estimated from the second year of observations and onwards. The result indicates an insignificant reduction in the risk of having another child among lone mothers by 1.5 percentage points.

Brewer et al. (2007), on the other hand, present evidence on the impact of WFTC on women in couples using the difference-in-differences estimator. They employ 1995-2003 FRS and the Family Expenditure Survey (FES) which are both repeated cross sectional data. Due to the characteristics of cross sectional data, it is not possible to observe the same individual for consecutive years. This implies that any birth after the interview date cannot be retrieved. In order to overcome this problem, births that occurred within 12 months prior to the interview date are collected. One thing to note, however, is that they do not have the demographic information of the same individual before and after the policy introduction or the birth. Moreover, FES does not contain complete information on the date of birth and, hence, they rely on the age of children at the interview date to identify the birth year and allocate the birth month randomly. They attempt to reveal the policy impact by making three choices of treatment and control groups: 1. Women from bottom and top third of earnings distribution 2. Both partners left school at or before compulsory school leaving age versus both partners left school after 18 and over. 3. Interaction between educational attainment and earnings. Although the level of household income is the ideal information to divide individuals into the treatment and control groups, the use of this variable would cause endogeneity bias due to the features of WFTC which affected both the labor supply and fertility behavior. In order to reduce the extent of the bias, they use the educational variable to identify those who were affected. Their results indicate positive and significant impact of WFTC on the probability of birth especially for first birth.

This paper attempts to contribute to the existing UK literature in the following four aspects. First, it provides an additional evidence of the WFTC impact on the probability of birth of both single women and women in couples. Second, it also investigates a possible policy impact on the timing of birth. Third, by using the advantageous features of a longitudinal dataset, it proposes a solution to the problem of endogeneity faced by studies with cross sectional data. The method proposed here is to proxy for the post reform income information with pre-policy information. This paper, as a result, separate those who were eligible and ineligible on the basis of pre-reform information collected in wave 8.

5. Econometric specification and Estimation method

Comprehension of the impact of WFTC requires calculating the outcome of interest for affected individuals with and without the introduction. This is referred to as the average treatment effect on the treated and is denoted as follows.

$$E[y_1 - y_0 \mid T = 1] = E[y_1 \mid T = 1] - E[y_0 \mid T = 1] \tag{1}$$

where
y_1 …Outcome of eligible individuals affected by a policy.
y_0 …Outcome of eligible individuals in the absence of a policy.
T…Individuals who would be affected in the presence of a policy.

In the case of natural experiments, however, we do not observe $E[y_0 \mid T = 1]$, namely the outcome of treated individuals in the absence of the policy. This is the well known problem of the missing counterfactual. In the attempt to recover the policy impact, we define a group of unaffected individuals who have similar characteristics to those who are treated as the control so as to mimic $E[y_0 \mid T = 1]$. One way to estimate the policy impact is to take the difference between $E[y_1 \mid T = 1]$ and $E[y_0 \mid T = 0]$. A problem with this approach is that the two groups may be systematically different from each other by a constant. In order to eliminate this disparity, the difference-in-differences estimation (DID), as shown in equation (2), takes the difference in the outcomes of the two groups before and after and evaluates the change in differences over time.

$$E[y_1 - y_0 \mid T = 1] = \{E[y_A \mid T = 1] - E[y_A \mid T = 0]\} - \{E[y_B \mid T = 1] - E[y_B \mid T = 0]\} \tag{2}$$

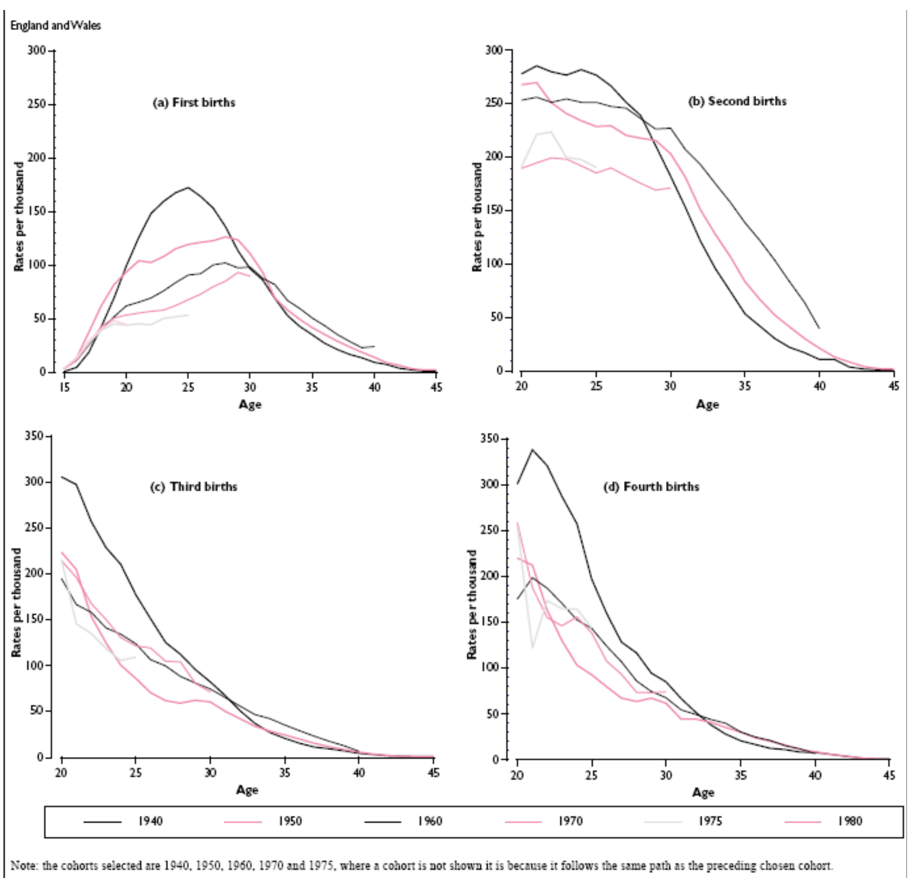
There are two assumptions required to ensure that estimates correctly identify the policy impact of the interests (Blundell and MaCurdy (1999)). Firstly, it is necessary that the time trends experienced by the affected and the control groups do not differ systematically. Secondly, there should be no compositional changes between the two groups over time. Satisfactory fulfillment of these assumptions is one of the hardest hurdles that the natural experimental studies face. Nevertheless, the treatment and control groups are defined in the next section in an attempt to reduce the risk of violating these two assumptions.

Given the above framework, the policy effect on the probability of having additional children is investigated using the probit model while the timing of birth is studied using the random effect probit model. One thing to bear in mind is the difficulty associated with separating the policy impact on the number of children from the timing of having a child. In particular, due to the short observation period, the probability of birth model estimates would reflect a combined policy influence on the family size and the timing of birth. It is, therefore, hoped that the second model illustrates a better picture of the policy impact on the timing of birth.

Both estimation models look at the policy impact on the birth of first, second and third children. This is because the profiles of birth probabilities across parity are very different from each other as shown in Figure 2 which shows fertility rates by parity. Differing profiles observed in Figure 2 justify the investigation of the policy impacts on each parity.

(Figure 2)

'At risk' age specific fertility rates by parity, selected cohorts, 1940–1980



(Source) Smallwood (2002)

5.1. Probability of Birth Model

The underlying latent model for the first specification is;

$$y_{it}^* = x_{it}'\beta_t + \varepsilon_{it} \tag{3}$$

Given the latent model (3), it is assumed that the ith individual has a child at time t when the difference in utility exceeds a threshold of zero. Let y be an indicator variable that equals one (y*>0) if the observed individual has a child in a specific year and 0 ($y^* \leq 0$) otherwise.

$$y_{it} = 1\{y^*>0\} \tag{4}$$

Then the empirical specification is given by

$$P(y_{it} = 1) = \Phi(\ x_{it}'\beta + \beta_1 Treatment_t + \beta_2 Post_t + \beta_3 Treatment_t * Post_t) \tag{5}$$

where $Treatment_t$ is a dummy that equals one if the individual is from the treatment group at time t, $Post_t$ is a dummy to indicate that observations are from the after policy introduction period and x_{it} includes demographic characteristics. The coefficient of particular interest to this paper is β_3, which shows the impact of the policy on those who were affected.

5.2. Timing of Birth model

The second specification is concerned with the timing of births which is investigated using the empirical framework of the event-history analysis. BHPS is an annual interview year panel. This implies that the fertility information is provided in the discrete time intervals although the underlying process of birth in reality is occurring in continuous time. Furthermore, since the observation period is terminated exogenously in year 2002, it is not possible to follow all individuals until they complete their fertility cycle. This is the problem of the right censoring. These issues separately present crucial implications on the empirical specification which are considered each in turn.

The interval censored data is organised in the following form (Jenkins (2004)),

$$[t_1, t_2], (t_2, t_3], (t_{j-1}, t_j], \ldots\ldots\ldots\ldots (t_{k-1}, t_k] \tag{6}$$

The survivor function at the end of the jth interval is

$$\Pr(T > t_j) = 1 - F(t_j) = S(t_j) \tag{7}$$

where the cumulative distribution function $F(t_j)$ is the failure function.

Since the probability of exit within the jth period is given as

$$\Pr(t_{j-1} < T \leq t_j) = S(t_{j-1}) - S(t_j) \tag{8}$$

The discrete hazard rate, which is the probability of exit within the interval j-1 and j conditional on survival until period j-1 is defined as

$$\begin{aligned}\lambda^d(t_j) &= \Pr(t_{j-1} < T \le t_j \mid T > t_{j-1}) \\ &= \frac{\Pr(t_{j-1} < T \le t_j)}{\Pr(T > t_{j-1})} \\ &= \frac{S(t_{j-1}) - S(t_j)}{S(t_{j-1})}\end{aligned} \tag{9}$$

If the probability of survival up until the end of jth interval is denoted as S(j), then $S(t_{j-1}) - S(t_j)$ can be rewritten as

$$S(t_{j-1}) - S(t_j) = F(t_j)S(j-1) \tag{10}$$

which implies that the probability of exit within the jth period is the product of the probability of survival until j-1th period and the exit in jth period. Given equation (10), equation (9) now becomes

$$\lambda^d(t_j) = \frac{S(t_{j-1}) - S(t_j)}{S(t_{j-1})} = \frac{F(t_j)S(j-1)}{S(j-1)} = F(t_j) \tag{11}$$

Equation (11) reveals that the discrete hazard function is equivalent to the failure function. This in turn implies that the hazard function can be defined as any cumulative distribution function.

The right censoring, on the other hand, alters the shape of the likelihood function. Let d_i be the indicator variable that takes the value one if the individual is observed to have given birth and zero otherwise. The ith individual's likelihood contribution for a complete spell (d_i=1) is

$$L_i = \Pr(T_i > t_j) = S_i(j) = \prod_{k=1}^{j}(1 - F_{ik}) \tag{12}$$

The likelihood contribution for an incomplete spell (d_i=0) is given as

$$L_i = \Pr(T_i > t_j) = S_i(j) = \frac{F_{ij}}{1 - F_{ij}}\prod_{k=1}^{j}(1 - F_{ik}) \tag{13}$$

The log likelihood function for the whole sample is

$$\log L \ = \sum_{i=1}^{n} d_i \log(\frac{F_{ij}}{1-F_{ij}}) + \sum_{i=1}^{n}\sum_{k=1}^{j} \log(1-F_{ik}) \qquad (14)$$

The indicator variable d_i can be redefined as a variable y_{ik} which equals one if the ith individual gives birth in period k and zero otherwise. Using this new variable, equation (14) can be rewritten as

$$\log L \ = \sum_{i=1}^{n}\sum_{k=1}^{j} y_{ik} \log(F_{ik}) + \sum_{i=1}^{n}\sum_{k=1}^{j} (1-y_{ik}) \log(1-F_{ik}) \qquad (15)$$

which implies that the duration to additional child can be investigated by using the binary regression models.

Take the underlying latent model,

$$y_{it}{*} = x_{it}'\beta_t + c_i + \varepsilon_{it} \qquad (16)$$

where c_i indicates an unobserved individual effect which may be thought of as summarizing individual history that is not controlled by other demographic characteristics. For example, the number of siblings the woman has may affect her notion of ideal family size. Given the indicator variable y_{it} defined in equation (4), the cumulative distribution function is assumed to be normal.

$$F_{ij} = \ P(y_{it} = 1 \mid x_{it}, c_i) = \Phi(x_{it}'\beta_t + c_i) \qquad (17)$$

Equation (17) implies that the duration to mth birth can be estimated using the random effect probit model. For this estimation, data should take the form of an interview year panel, observing the same individuals over consecutive years from a common starting point until they give birth to their mth child at period t. Once birth is observed, the individual is discarded from the sample.

For the estimation of the random effect probit model, it is assumed that the common starting point or the entry into the initial state is exogenous. This assumption avoids the problem of initial conditions and circumvents the complications caused by having to take account of the differential probabilities for individuals to be found in the initial state. It is also assumed that the error terms are strictly exogenous i.e.) $E(u_{it} \mid x_{it-2}, x_{it-1}, x_{it}, x_{it+1} ....) = 0$. Together with this strict exogeneity assumption, assuming that $y_{i1}, y_{i2},......y_{iT}$ are independent conditional on x_{it} and c_i allows conditional joint densities of $y_{i1}, y_{i2},......y_{iT}$ to be written as

$$f(y_1,.......y_T \mid x_i, c_i; \beta) = \prod_{t=1}^{T} f(y_t \mid x_{it-1}, c_i; \beta)$$
$$where \qquad \prod_{t=1}^{T} f(y_t \mid x_{it-1}, c_i; \beta) = \Phi(x_{it-1}\beta + c)^{y_{it}} [1 - \Phi(x_{it-1}\beta + c)^{1-y_{it}}] \tag{18}$$

One crucial problem with the above density is that the unobserved variable c_i cannot be estimated. This is partly due to c_i not being observed and also because the number of this unobserved parameter (c_i) increases as N→∞. Assuming that the distribution of c_i is independent from the covariates'[3], c_i is integrated out in order to obtain the following likelihood function.

$$f(y_1,.......y_T \mid x_i, c_i; \beta, \sigma_c) = \int_{-\infty}^{\infty} [\prod_{t=1}^{T} f(y_t \mid x_{it-1}, c_i; \beta)](1/\sigma_c)\phi(c/\sigma_c)dc \tag{19}$$

The above equation is often estimated using a numerical approximation such as the Gaussian-Hermite quadrature because it usually does not have a closed-form. Since this paper is interested in the partial effect, it instead estimates the average partial effect (APE) using the probit model. APE is obtained by averaging the partial effects across the population distribution of c_i. Wooldridge (2002) indicates that the marginal effect estimated by the probit model consistently estimates the average partial effects of the random effect probit model. Let v_{it} be a composite of the error term and the unobserved individual effect c_i as shown below.

$$\begin{aligned} & v_{it} = c_i + \varepsilon_{it} \\ & where \qquad v_{it} \mid x_{it} \sim Normal(0, \sigma_v^2) \\ & \qquad\qquad \sigma_v^2 = 1 + \sigma_c^2 \\ & \qquad\qquad \because \sigma_\varepsilon^2 = 1 \end{aligned} \tag{20}$$

Then the conditional probability can be written as

$$P(y_{it} = 1 \mid x_{it}) = \Phi(\frac{x_{it}\beta}{\sigma_v}) = E(y_{it} = 1 \mid x_{it}) \tag{21}$$

Using the iterated expectations,

[3] This assumption is rather restrictive and one way to relax this assumption is to estimate the Chamberlain's random effect probit model (Mundlak (1978), Chamberlain (1980)) which allowed for correlation between unobserved effect c_i and x_i although it only estimates time-variant covariates. In this paper, however, Chamberlain's random effect probit is not estimated as the *policy* variable is time-invariant.

$$E(y_{it} = 1 \mid x_{it}) = E_c[E(y_{it} = 1 \mid x_{it}, c_i)] = E_c[\Phi(x_{it}\beta + c_i)] \tag{22}$$

which is the expected value with respect to c_i. Calculating the average partial effect at a constant value x_0 yields the expression in equation (23), which is equivalent to the marginal effect from the probit model.

$$\frac{\partial E_c[\Phi(x_{it}\beta + c_i)]}{\partial x_j} = (\frac{\beta_j}{\sigma_v})\phi(\frac{x_{it}\beta}{\sigma_v}) \tag{23}$$

As a result, the present paper uses the probit model for the estimation of the marginal effects as it consistently estimates the average partial effect even when unobserved heterogeneity is present.

6. Data

This paper uses the waves 5 to 13 British Household Panel Survey (BHPS). BHPS is a nationally representative longitudinal data set collected annually since 1991. The panel consists of over 10,000 individuals from 5000 UK households. The interviews were carried out on household and individual level and include individuals over the age of 16, who reside in the same house. Information regarding the children of these adults is also collected provided that their children are living within the same household.

While repeated cross-sectional data have attractive features such as a large number of births as well as the sample size, longitudinal data present us with several advantages. One of the major merits is the solution provided by the panel to overcome the endogeneity problem. Due to the eligibility condition of WFTC, individual characteristics such as household income are likely to be determined simultaneously with fertility after the policy introduction. Fertility and marital status are also prone to be jointly determined. For instance, the event of pregnancy may encourage individuals to form a stable partnership. In such cases, the use of demographic characteristics observed in each interview year would thus bias the estimates. Employing the panel data allows information collected prior to the policy introduction or birth to be used as a proxy to overcome this problem. Additionally, due to the structure of the panel data, birth information of children who left households can reasonably be recovered from early waves. This is particularly important in the context of fertility analysis as the incentive to have additional children is likely to depend on the number of existing children. Lastly, unlike several other datasets such as FES, BHPS reports both the birth year and the month of children.

In this paper, fertility activities during the period of 1995 and 2002 are used. Due to the concern regarding differences in time trends which are caused by including too many years prior to the policy introduction, the year 1995 is selected as the starting year of the observation. The cut off year 2002 was chosen in order to avoid including the impact of the two new credits, the Working Tax Credit and the Child Tax Credit which were widely advertised since September 2002. The year that separates pre and post policy introduction period is defined as 1999. January, 1999 is approximately nine months after the policy announcement date, March 1998. Assuming that individuals changed their behavior since the announcement date, any births after this date would reflect the responses to the policy introduction. For the purpose of sensitivity analysis, the separation year is also defined to be 2000 to see if it causes any change in the size and direction of estimates. The sample is restricted by dropping all women who gave birth before 1995 but did not give birth between 1995 and 2002 as these women are most likely to have finished their fertility activities.[4] Lastly, the individual marital status is defined according to their status in 1995. This ensures that the selection of women in each group is independent from their fertility behavior. The total number of observations during the period is 5958 (approximately 1766 women observed over the duration of eight years).

Data is structured differently according to the models studied. For the probability of birth model, data is organized as a pooled panel and the dependent variable is defined as a dummy that takes the value one if the observed individual gave birth within the interview year. In other words, each observation is considered as a separate individual. On the other hand, the time to birth model recognizes the same individuals and follows them over time. In particular, it is organized as an interview year panel and individuals are observed from a common starting point and are followed until they give birth. Once birth is observed, these individuals are discarded from the sample. Individuals who did not give birth during the period of observation are followed until 2002. The common starting point can be defined either as a common starting year or when each individual is situated in the same state. In this paper, the starting point is defined differently according to the birth parity. For the estimation of the time to first birth, the starting point is year 1995 for everyone, assuming that each individual woman is in the initial state of having no child exogenously. For timing to mth child (i.e. m=2,3), the common starting point is defined as one year after when the woman gave birth to her m-1th child conditional on the mother giving m-1th child after 1995. The dependent variable for the time to birth model is also a dummy that takes the value zero for all years apart from the year a woman gave birth.

Reflecting this difference in data organization, the selections of the sample of women included in each model vary. In particular, the sample used for the probability of birth model contains women who were aged between 20 and 40 in any interview years. On the other hand, women aged between 20 and 35 in 1995 are selected for the time to birth model which implies that the age profile of the sample of women in 2002 was between 28 and 42. The age selection criteria are chosen so as to ensure that women in the sample had not finished their fertility cycle when they were interviewed and were able to respond to the policy during the period of analysis.

[4] For sensitivity analysis, the observation period is reselected to include years between 1991 and 2002. Estimates from the new sample suggest the same conclusion.

6.1. Treatment groups

As noted in section 5, the common trend and no compositional change assumptions required for correctly identifying the policy effect present us with real difficulties. Year 1999 in the UK saw a number of other policies introduced. Major policies include the introduction of minimum wage and other policies such as New Deal to improve the work incentives of low income families. Moreover the maternity allowance paid to mothers with some previous record of employment became more generous. Specifically, the period of coverage increased to 26 weeks from the previous length of 18 weeks (Hills and Woldfogel (2004)). These policies primarily affected individuals from the low income families. The selection of the control group must be done to ensure similarities in the household income background between the two groups.

The treatment and control groups are selected on the basis of the household income due to the eligibility condition of WFTC. Use of income information after the policy introduction, however, would be endogenous. This is because income is simultaneously determined with birth due to the WFTC impact on labor supply. In order to circumvent this problem, income information in wave 8 is used as a proxy for later years when defining the groups.[5] WFTC had an extensive media advertisement since March, 1998. Assuming that individuals started adjusting their behaviors after the announcement, the 1997 characteristics are the best predictors of the post policy information that are unaffected by the introduction.[6]

The first treatment group includes women whose 1997 household income is below £23,000 while the control group includes individuals whose household income is between £25,000 and £35,000.[7] In an effort to satisfy the common trend assumption, individuals who have similar levels of income to those in the treatment group but not eligible to receive WFTC are chosen as the first control group. While the first treatment and control groups attempt to separate those eligible from non-eligible individuals, it does not ensure that women from the two groups have similar levels of education or labor supply. Studies suggest negative correlation between female labor supply or educational attainment and fertility (Browning (1992), Blossfeld and Huinink (1991), Martin (2000)). Moreover, investigations on the causal relationship between education and fertility by Bloemen and Kalwij (2001) and Skirbekk et al. (2004) report evidence that an additional year of schooling delays birth. It is, therefore, important to select women with similar level of educational

[5] Each year, individuals in BHPS are asked to provide their income information of the past 12 months. The 1997 income information, therefore, is reported in the wave 8 data set collected in 1998.

[6] If many women decide to leave the labor market in the event of pregnancy, their household income would go down after the birth is observed. The implication of this would be that the treatment group may include more women who gave birth in 1996 or 1997 and thus the income information is endogenous. Given that British women on average give birth with intervals of two to three years, fewer women in the treatment group would have given birth after the year 1999, which in turn implies that the estimates would be negatively biased. When proportions of birth are computed separately for the treatment and the control group, no sudden increase in the proportion of birth around 1996 and 1998 are observed. The bias from this endogeneity problem seems to be limited.

[7] Typical families in the sample have two children. Allowing for these families to receive the maximum tax credit, the annual household income of approximately £23,000 is the maximum amount they can earn before losing their entitlement.

attainment. Motivated by this reason, the second treatment and control groups both select women with less than or equal to o-level education but who have different levels of household income.

-T1: Women whose household income in 1997 is below £23,000.
-C1: Women whose household income in 1997 is between £25,000 & £35,000.[8]

-T2: Women with less than or equal to o-level education and whose household income in 1997 is below £23,000.
-C2: Women with less than or equal to o-level education and whose household income in 1997 is between £25,000 & £35,000.

6.2. Variables used in the analysis

The demographic characteristics used in the analysis of both probability of birth and time to birth models include age of women and its square and cube terms, housing tenure, ethnicity of the women and their partners, women's level of educational attainment and the region of residences. A dummy that takes the value one if each family already has a boy is included ('boy' in Table 4). In addition, another dummy is included in order to take account of families with two children that are of opposite sex. ('difsex' in Table 4). The motivation for the inclusion of these two dummies is as follows. The dummy for boys are included to take account of a possible reduction in fertility incentives due to the effect of son preferences. Similarly, parents who already have children of opposite sex may have less incentive to have additional children compared to those who only have children of one sex. Additionally, the treatment dummy is interacted with the number of other children in the household in order to identify differential impacts across birth parity.

Table 4 shows summary statistics of variables used in the analysis. The first column shows statistics of the total sample. The second and the third columns show statistics for women with partners by treatment status (treatment1/control1) while the third and the fourth columns include statistics of single women. Table 4 presents evidence that both single women and women with partners who were affected by WFTC tend to be marginally younger, have lower income and more children. They are also more likely to live in either a council or a rented house compared to those in the control group. One thing to note from Table 4 is that when the division into treatment and control group is based solely on the household income level, women's level of educational attainment between the two groups differs greatly. Due to this possible influence of education on fertility as discussed earlier, it is important to restrict the wide disparity in the educational background between the two groups. Table 5, therefore, presents summary statistics of the employed sample split by the second treatment and control groups. As the table illustrates, the

[8] Individuals with household income below £15,000 are also estimated for sensitivity analysis. In addition, estimates are computed using a control group including individuals whose household income ranges between £30,000 and £45,000. These alternative treatment and control groups, however, do not alter the conclusion.

extent of differences in the educational attainment between the two groups is reduced when the second treatment/control group is employed.

Just as the household income or the marital status of individuals, other demographic characteristics may have been affected by the policy introduction. Use of post-reform characteristics in this case would cause endogeneity bias. In order to check this issue, summary statistics of variables from the year 1997 is presented in Table 6. Comparing the statistics between Tables 4 and 6, it is possible to confirm that the changes of characteristics over time are relatively small. The size of the bias, therefore, is likely to be small.

(Table 4) Summary statistics of variables from pre and post reform period (first treatment/control group)

	(1)	(2)	(3)	(4)	(5)
	Total	Women with partners		Single women	
		Treatment 1	Control 1	Treatment 1	Control 1
1 if birth given in the interview year	0.107	0.144	0.144	0.063	0.049
No. of children	0.596	0.975	0.676	0.349	0.140
	(0.826)	(0.898)	(0.829)	(0.680)	(0.414)
Age at date of interview	28.014	29.300	30.770	26.446	27.097
	(5.508)	(5.239)	(4.636)	(5.339)	(5.420)
Annual household income in 1997 (from wave 8)	18943.869	14744.387	29655.093	12402.593	29648.381
	(9166.288)	(5076.411)	(2808.455)	(6055.998)	(2979.104)
Ethnicity: White	0.956	0.976	0.970	0.936	0.952
Ethnicity: Asian	0.017	0.017	0.011	0.015	0.021
Spouses' ethnicity: White	0.571	0.847	0.898		
Spouses' ethnicity: Asian	0.007	0.007	0.010		
Housing tenure: Rent	0.174	0.137	0.072	0.274	0.121
Housing tenure: Council	0.181	0.178	0.030	0.237	0.103
Housing tenure: Owned	0.643	0.684	0.898	0.481	0.775
1 if already has a boy	0.199	0.358	0.226	0.122	0.019
1 if already has children of different sex	0.073	0.140	0.079	0.031	0.003
Region: London	0.095	0.042	0.085	0.128	0.151
Region: South East	0.174	0.155	0.220	0.172	0.231
Region: South West	0.087	0.089	0.077	0.093	0.073
Region: East Anglia	0.052	0.081	0.059	0.059	0.027
Region: East Midlands	0.092	0.122	0.117	0.073	0.030
Region: West Midlands	0.078	0.082	0.040	0.065	0.110
Region: North West	0.095	0.090	0.082	0.109	0.055
Region: York	0.095	0.148	0.091	0.064	0.076
Region: Region of north	0.068	0.072	0.065	0.080	0.051
Region: Wales	0.048	0.036	0.055	0.052	0.066
Region: Scotland	0.109	0.085	0.110	0.105	0.130
Academic qualification: First or higher degree	0.163	0.093	0.157	0.221	0.255
Academic qualification: Teaching, nursing or other higher qualification	0.269	0.244	0.322	0.262	0.346
Academic qualification: A-level	0.188	0.174	0.173	0.187	0.182
Academic qualification: O-level	0.242	0.279	0.271	0.227	0.163
Academic qualification: Less than o-level qualification	0.087	0.141	0.058	0.050	0.049
Academic qualification: No educational attainment	0.050	0.070	0.019	0.052	0.004
N	5958.000	1435.000	1054.000	1666.000	670.000

(Table 5) Summary statistics of variables from pre and post reform period (second treatment/control group 2)

	(1)	(2)	(3)	(4)	(5)
	Total	Women with partners		Single women	
		Treatment 2	Control 2	Treatment 2	Control 2
1 if birth given in the interview year	0.147	0.172	0.153	0.100	0.069
No. of children	0.863	1.159	0.687	0.587	0.214
	(0.883)	(0.872)	(0.831)	(0.798)	(0.529)
Age at date of interview	28.530	29.277	31.624	27.237	26.903
	(5.636)	(5.496)	(5.053)	(5.459)	(5.166)
Annual household income in 1997 (from wave 8)	17314.007	14046.479	29241.499	11944.305	29778.951
	(8858.125)	(5260.511)	(2831.406)	(5601.242)	(2832.789)
Ethnicity: White	0.970	0.986	0.989	0.965	0.966
Ethnicity: Asian	0.017	0.014	0.000	0.022	0.000
Spouses' ethnicity: White	0.624	0.865	0.926		
Spouses' ethnicity: Asian	0.003	0.003	0.000		
Housing tenure: Rent	0.119	0.105	0.054	0.166	0.069
Housing tenure: Council	0.277	0.236	0.033	0.381	0.138
Housing tenure: Owned	0.602	0.659	0.913	0.448	0.786
1 if already has a boy	0.284	0.411	0.199	0.209	0.069
1 if already has children of different sex	0.098	0.151	0.065	0.036	0.014
Region: London	0.065	0.036	0.090	0.035	0.214
Region: South East	0.160	0.181	0.199	0.160	0.159
Region: South West	0.097	0.081	0.079	0.129	0.117
Region: East Anglia	0.061	0.077	0.112	0.055	0.041
Region: East Midlands	0.112	0.132	0.106	0.086	0.076
Region: West Midlands	0.080	0.092	0.060	0.044	0.069
Region: North West	0.108	0.098	0.060	0.131	0.124
Region: York	0.095	0.121	0.112	0.080	0.028
Region: Region of north	0.079	0.085	0.071	0.098	0.034
Region: Wales	0.035	0.034	0.027	0.056	0.014
Region: Scotland	0.098	0.063	0.084	0.126	0.124
Academic qualification: O-level	0.637	0.569	0.779	0.690	0.752
Academic qualification: Less than o-level qualification	0.229	0.289	0.166	0.151	0.228
Academic qualification: No educational attainment	0.130	0.142	0.054	0.158	0.021
N	2264.000	703.000	367.000	549.000	145.000

(Table 6) Summary statistics of variables in 1997

	(1)	(2)	(3)	(4)	(5)
	Total	Women with partners		Single women	
		Treatment 1	Control 1	Treatment 1	Control 1
Housing tenure: Rent	0.199	0.147	0.090	0.368	0.118
Housing tenure: Council	0.217	0.218	0.042	0.219	0.145
Housing tenure: Owned	0.582	0.635	0.867	0.404	0.737
Region: London	0.082	0.046	0.072	0.114	0.105
Region: South East	0.170	0.147	0.235	0.184	0.211
Region: South West	0.089	0.102	0.090	0.075	0.079
Region: East Anglia	0.044	0.081	0.048	0.048	0.026
Region: East Midlands	0.092	0.112	0.096	0.066	0.066
Region: West Midlands	0.084	0.091	0.042	0.079	0.118
Region: North West	0.097	0.096	0.078	0.114	0.066
Region: York	0.092	0.152	0.084	0.061	0.053
Region: Region of north	0.072	0.066	0.060	0.101	0.039
Region: Wales	0.046	0.051	0.042	0.039	0.105
Region: Scotland	0.111	0.056	0.151	0.118	0.132
Academic qualification: First or higher degree	0.130	0.081	0.127	0.180	0.224
Academic qualification: Teaching, nursing or other higher qualification	0.210	0.198	0.301	0.224	0.224
Academic qualification: A-level	0.207	0.193	0.175	0.259	0.224
Academic qualification: O-level	0.283	0.299	0.295	0.246	0.224
Academic qualification: Less than o-level qualification	0.111	0.162	0.084	0.035	0.105
Academic qualification: No educational attainment	0.059	0.066	0.018	0.057	0.000
N	900.000	197.000	166.000	228.000	76.000

7. Estimation Results

7.1. The probability of birth

The results for the probability of birth estimated by the probit model are presented in Tables 7 to 10. Tables 7 and 8 indicate the marginal effects of single women while Tables 9 and 10 show those of women with partners. Tables 7 and 9 defined the post-reform period as after 1999.On the other hand, Tables 8 and 10 used the year 2000 as a cut off year. In order to test the robustness of the results, both regressions with and without demographic covariates are estimated. Every odd number column shows results from base regressions without any demographic covariates while every even number column shows those with covariates. Comparing any of the corresponding pair of columns from Tables 7 to 10, the size and sign of estimates can be seen as relatively stable even when individual characteristics are included. The coefficients of our interests are *Treatment_after1999* as well as its interaction terms with *kid1* and *kid2*. Since *kid1* and *kid2* are indicating the number of other children in the household, the coefficient on *kid1_after1999_policy*, for example, shows the impact of the policy on the second birth. The base group shown as *Treatment_after1999* includes individuals without any other children.

Tables 7 and 8 both indicate that single women experienced negative policy impact across birth parity. Taking the smallest estimates for birth parity separately, WFTC reduced the probability of the first, second and third birth by 2, 6, and 3 percentage points respectively (reduced by -29, -35 and -20 percent).[9] When the year to separate pre and post reform policy is defined as 2000, probabilities of second birth become significantly negative. The choice of treatment and control groups seem to have little impact on the size of the estimates.

Estimates for women with partners (Tables 9 and 10) indicate that the choice of treatment/control groups has a substantial impact on the size of the estimates for women with partners. In particular, when women with similar educational backgrounds are compared with each other, the magnitude of the estimated policy impact becomes larger. Because of the possible influence of educational attainment towards the fertility behavior, the estimates for the second treatment and control group (column (3) and (4) of each table) are more likely to identify the policy impact without the influence of the labor supply effect. Even when the smallest estimate is taken from regressions with treatment2, the impact of WFTC on the first birth is large and significant (-10 percentage points or -55 percent).[10] Evidence on the probability of second birth, again from estimates with treatment2, indicates insignificantly positive impact of the policy introduction (approximately ranging between 7 and 10 percentage points or 29 and 42 percent). Lastly, estimates for the probability of the third birth are insignificant and mixed.

(Table 7) Probability of birth model. Marginal effects for single women. Post reform period defined as after 1999

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after1999	-0.04**	-0.05***	-0.06	-0.07*
	(0.02)	(0.02)	(0.05)	(0.04)
kid1_after1999_Treatment	-0.02	-0.01	-0.04	-0.03
	(0.01)	(0.02)	(0.03)	(0.03)
kid2_after1999_Treatment	-0.02	0.02	-0.02	-0.02
	(0.02)	(0.04)	(0.05)	(0.04)
Observations	2336	2336	694	694

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

[9] The proportion of single women giving birth in a given year before the policy introduction (i.e. base probability) for each birth parity are 7, 17 and 15 percentage respectively.
[10] The proportion of women with partners giving first, second and third birth before the policy introduction is 18, 24, and 12 percent respectively.

(Table 8) Probability of birth model. Marginal effects for single women. Post reform period defined as after 2000

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after2000	-0.03*	-0.03**	-0.02	-0.04
	(0.02)	(0.01)	(0.06)	(0.04)
kid1_after2000_Treatment	-0.04***	-0.03**	-0.07***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.01)
kid2_after2000_Treatment	-0.03*	-0.00	-0.06**	-0.06***
	(0.02)	(0.03)	(0.03)	(0.01)
Observations	2336	2336	694	694

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

(Table 9) Probability of birth model. Marginal effects for women with partners. Post reform period defined as after 1999

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after1999	-0.04	-0.02	-0.13*	-0.10*
	(0.04)	(0.04)	(0.07)	(0.05)
kid1_after1999_Treatment	0.11*	0.07	0.20	0.17
	(0.06)	(0.06)	(0.14)	(0.14)
kid2_after1999_Treatment	0.01	-0.01	0.07	0.05
	(0.05)	(0.04)	(0.11)	(0.10)
Observations	2489	2489	1070	1070

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

(Table 10) Probability of birth model. Marginal effects for women with partners. Post reform period defined as after 2000

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after2000	-0.04	-0.02	-0.13*	-0.11*
	(0.04)	(0.04)	(0.07)	(0.05)
kid1_after2000_Treatment	0.11	0.07	0.22	0.21
	(0.08)	(0.07)	(0.19)	(0.20)
kid2_after2000_Treatment	0.02	-0.01	0.12	0.11
	(0.06)	(0.05)	(0.16)	(0.16)
Observations	2489	2489	1070	1070

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

7.2. Time to birth model

Tables 11 and 13 both show the average partial effects for the timing of birth estimated by the probit model where the post policy periods are defined as after 1999. Tables 12 and 14, on the other hand, show estimates with year 2000 as the cut off year. Similarly to the probability of birth model, base regressions without any covariates as well as with covariates are separately estimated. Just as before, the inclusion of covariates does not drastically change the signs of the estimates. The estimates in these tables imply how the exit rate from having no additional state changed across time. In other words, negative (positive) estimates indicate that the time to birth is prolonged (shortened).

The result in Tables 11 and 12 illustrate a similar picture as that of the probability of birth model. Single women reduced the exit rate from having no birth across birth parity. In other words, all single women regardless of the number of existing children increased the time to an additional birth.

Tables 13 and 14 present estimates for women with partners. Just as the results for single women, findings for women with partners also enforce the conclusion drawn from the probability of birth model. These estimates, however, indicate a very strong impact of WFTC on the time to the first and second birth of women with partners. While the majority of the estimates from the probability of birth model (in Tables 9 and 10) are insignificant for this group of women, estimates reported in Tables 13 and 14 are highly significant, particularly for the first and the second birth. The risk of exit for first birth is estimated to have been reduced while it is significantly increased for second birth. From Tables 13 and 14, even when the smallest estimates are taken, the exit rates for the first and second birth are -7 and 7 percentage points respectively (-24 and 41 percent).[11] The evidence for the time to third birth is again mixed and insignificant.

(Table 11) Time to birth model. Average Partial Effect for single women. Post reform period defined as after 1999

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after1999	-0.07***	-0.05***	-0.06	-0.04
	(0.02)	(0.02)	(0.06)	(0.06)
kid1_after1999_Treatment	-0.00	-0.00	-0.00	0.01
	(0.02)	(0.02)	(0.05)	(0.05)
kid2_after1999_Treatment	-0.02	-0.01	-0.00	-0.03
	(0.03)	(0.02)	(0.06)	(0.04)
Observations	2261	2261	683	683

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

(Table 12) Time to birth model. Average Partial Effect for single women. Post reform period defined as after 2000

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after2000	-0.06***	-0.04***	-0.01	-0.00
	(0.02)	(0.01)	(0.06)	(0.06)
kid1_after2000_Treatment	-0.02	-0.01	-0.05	-0.04**
	(0.02)	(0.02)	(0.03)	(0.02)
kid2_after2000_Treatment	-0.03	-0.02	-0.06**	-0.05***
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	2261	2261	683	683

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

[11] The base probability (before 1999) is 29 and 17 % for the first and second birth respectively.

(Table 13) Time to birth model. Average Partial Effect for women with partners. Post reform period defined as after 1999

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after1999	-0.11***	-0.09***	-0.26***	-0.24***
	(0.03)	(0.03)	(0.05)	(0.05)
kid1_after1999_Treatment	0.19***	0.16**	0.38***	0.38**
	(0.07)	(0.07)	(0.14)	(0.15)
kid2_after1999_Treatment	0.09	0.03	0.30	0.22
	(0.06)	(0.05)	(0.13)**	(0.13)
Observations	2051	2051	925	925

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

(Table 14) Time to birth model. Average Partial Effect for women with partners. Post reform period defined as after 2000

	(1)	(2)	(3)	(4)
	Treatment1/Control1		Treatment2/Control2	
Treatment_after2000	-0.09***	-0.07*	-0.22***	-0.20***
	(0.03)	(0.03)	(0.05)	(0.05)
kid1_after2000_Treatment	0.17**	0.15**	0.40**	0.41**
	(0.08)	(0.08)	(0.17)	(0.19)
kid2_after2000_Treatment	0.08	0.03	0.34**	0.27
	(0.07)	(0.06)	(0.16)	(0.17)
Observations	2051	2051	925	925

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

7.3. Estimates comparisons with past evidence

Although the finding from this paper enforces that of Francesconi and Van der Klaauw (2007), the estimates found in this paper and Brewer et al. (2007) illustrate a very different picture of the policy impact for women with partners. While women in couples in this paper are observed to have reduced their probability of first birth significantly, they find significantly positive results for the same birth parity. There are several potential causes of such disparities between the estimates which are discussed below.

The repeated cross sectional data such as FRS or FES has a comparative advantage over panel data over two issues. Firstly, it is usually easier to collect larger samples. As a result, Brewer et al. (2007) include over 50,000 individuals in their sample. Secondly, the age profile of individuals is constant over years in the repeated cross section while individuals become older in the panel. As age is closely related with fertility activities, the probability of birth in the panel data is not only affected by the policy and other macro effects but also by the aging effect. If such aging effect affects the fertility behavior of two groups in a similar manner, this is not a problem. However, due to the eligibility conditions of WFTC, groups are defined using measures related to labor supply and household income. Since a high level of female labor supply is often associated with delayed fertility, if women in the control group on average give birth in later years of their lives than those in the treatment group, the age effect would cause differential trends and in turn the results in this paper would tend to be more negative.

Although the repeated cross sectional data possess the positive elements explained above, BHPS has accurate birth date information which FES does not have. Such mismeasurement would potentially cause bias in the estimates.[12] Moreover, longitudinal data gives a solution to the endogeneity problem faced by the cross sectional data by allowing data from the pre-policy period to be used as a proxy for information in latter years. The cross sectional data only provide personal information collected in a specific interview year. Since the eligibility rule under WFTC was related to the levels of household income, use of the post-reform information would cause endogeneity bias. Marital status is also another factor that is likely to be jointly determined with fertility behavior. Since couples are more likely to consider forming a stable partnership after conceiving a child, using marital status from each year to determine the sample of women with partners would positively bias the results.

Another possible explanation for the differing estimates is the choice of treatment and control groups. All the treatment and control groups used in Brewer et al. (2007) have large disparities in the characteristics from each other. In particular, their treatment group consists of women from the bottom range of the income distribution while women from the top range of household income are used as a control. While this selection ensures clear separation between those who were affected and unaffected, the fertility patterns of the two groups may be dissimilar. This issue is particularly crucial since various policies other than WFTC heavily affected the labor supply and the household income of low income families.[13]

In order to evaluate the above arguments, the base model from Brewer et al. (2007) is replicated using BHPS. The first column of Table 15 shows results estimated using the treatment and control groups defined by Brewer et al.[14] The second column presents estimates with the treatment and control groups defined using the 1997 educational information. The third column shows estimates when the marital status is taken from year 1995. Finally, the last column indicates results with the treatment and control 2 used in this paper (see section 6).[15]. The first column suggests that when the specification from Brewer et al (2007) is applied, the probability of birth for the first birth is now positive although insignificant. It seems that the aging effect discussed earlier negatively biases the estimates reported in this paper. There is almost no change in the signs and the

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[12] For example, if a child is born in December 1998 but the interview was carried out in September 1999, it is not possible to determine whether the child was born in 1998 or 1999 from the age of the child reported in the data. Lack of birth month information is also problematic due to the non random nature of the birth months. Data from the National statistics, "Births: 1938-2004, Live births: month of occurrence", indicate less births in February and December while more births are observed during the summer months during the period of 1995 and 2003.

[13] Examples include increased payment of Income support in 1999 and the introduction of the New Deal policy in 1998 as well as the increased generosity in the maternity allowance paid to mothers with some previous record of employment in 1999.

[14] Their treatment equals one if both the women and their partners left at or before the compulsory school leaving age and zero if both of them left school after the leaving age.

[15] A selection of women with partners is made using the marital status from each interview year for the column 4 regression.

sizes of estimates even when the treatment and control groups are defined using the 1997 information. Since educational attainment cannot change drastically in the limited number of years, the endogeneity bias caused by the use of contemporary information seems to be small. The third column illustrates the fact that defining the sample of women with partners using marital status at the beginning of the observation period corrects for the positive bias discussed above. From the last column, the use of a similar control group has a significant impact on the level of estimates. Combining these four possible causes of the disparity, the aging effect of panel data is likely to have affected the estimates from this paper to be more negative. However, the primary causes of the differences in the estimated impact for the first birth between the two papers are the use of marital status at the beginning of the observation period and the differential definitions of the treatment and control groups between the two papers.

(Table 15) Comparisons with Brewer et al. (2007)

	(1)	(2)	(3)	(4)
	Brewer et al. (2007) treatment/control	Brewer et al. (2007) 97 treatment/control	Marital Status in 1995	Treatment1/ Control1
Treatment_after2000	0.01	0.01	-0.02	-0.09*
	(0.05)	(0.04)	(0.07)	(0.06)
Kid1_after2000_treatment	-0.00	0.02	0.06	0.02
	(0.06)	(0.05)	(0.12)	(0.07)
Kid2_after2000_treatment	-0.06	-0.07**	-0.08	-0.05
	(0.05)	(0.03)	(0.05)	(0.06)
Observations	2202	2272	2031	1476

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

8. Conclusion

This paper investigated the impact of the 1999 WFTC introduction on the fertility behavior of the UK women. It attempts to provide complementary evidence to the previous fertility studies on WFTC by presenting the findings for the policy impact on both the probability of birth as well as the timing of birth. It proposed a possible solution to the problem of the endogeneity bias faced by using the post-reform individual information.

The interpretation of the estimates reported in this paper, however, should be done with some caution. Due to the construction of the longitudinal data, individuals age over years. As age is closely related with fertility activities, estimates may be negatively biased due to the differing fertility trends between groups caused by such aging effects. For example, if the control group of women on average has a stronger attachment to the labor market, it may be associated with delayed fertility compared to those women in the treatment group.

Economic theory predicts ambiguous impacts for single women (see section 3). Due to the large increase in their labor supply reported by previous studies (e.g. Blundell et al (2000)) as well as the need for this group of women to find a partner in order to conceive a child, it is expected that they were most likely to be exposed to a negative WFTC impact on their fertility behavior.

Evidence provided in this paper validates the theoretical predictions. In particular, estimates for single women indicate that they reduced the probability of birth and prolonged their birth intervals for all birth parity.

On the other hand, the results indicate that the financial support from WFTC did not encourage women with partners into motherhood but they had their second birth earlier. Even after taking account of the possible negative bias due to the aging effect of the panel data, it is a little surprising to reach the conclusion that WFTC did not encourage the first birth as these families were awarded the most generous amount of the tax credit. However, various other policies were introduced during this period in order to encouraged low income families to enter the labor market.. If women without children were more responsive to these policies due to the limited restrictions from their household and childcare responsibilities, they might have increased their labor supply more than those in the control group. The increased labor supply may, therefore, have driven women to have less incentives to give birth. Another possible cause is the employment status of the partners. Women in couples from low income families are on average more likely to be with a partner who is unemployed. Since the UK labor supply literature (Blundell (2000)) report a positive labor supply impact of WFTC for these women, the increased labor market attachment may also have reduced these women's incentive to have another child.[16]

On the other hand, the shorter birth interval to the second birth for women with partners may be due to the average UK women giving birth to two children. These women with one child may have planned to have another child even in the absence of the policy introduction. In other words, financial support from the WFTC introduction did not seem to increase the demand for children for women with partners, but it rather worked as a stimulant for those who were planning to have an additional child even in the absence of the policy impact.

Although the estimated impact of the timing of birth for lone mothers is quite similar to those reported by Francesconi and Van der Klaauw (2007), this paper's findings of the probability of birth for women with partners are contradictory to the results given by Brewer et al (2007). The cause of this variation in estimates for women in couples is identified as partially due to the aging effect but also because of the way women's marital status is identified as well as the differential definition of the treatment and control groups in each paper.

Future investigations on the implications of the use of the panel data as opposed to the cross sectional data are needed in order to comprehend the differences in estimates more rigorously. One way of achieving this is by weighting the age of women in BHPS in order to make estimates from the two datasets comparable to each other. Difficulties in separating various policies introduced during the same period may be reduced by the use of structural modeling. It may also help to look at the impact of Child Tax Credit introduced in 2003 which was provided to families from much wider ranges of income distribution and does not require specific working hours by parents. Such an

[16] Although Milligan (2005) reported a positive impact from the Allowance for Newborn Children, this policy did not have any requirements that parents be in the labor market.

eligibility condition would allow us to overcome the problem faced in this paper to separate the labor supply effect from the fertility effect.

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