Take-up of Family Credit and Working Families' Tax Credit

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Final report

Executive summary

Many people in the UK do not claim means-tested support to which they seem to be entitled, limiting the programmes' usefulness as a means of helping those on low incomes. This paper examines the determinants of take-up of state support for low-income working parents: Working Families' Tax Credit (WFTC) between October 1999 and March 2003, and its predecessor, Family Credit (FC).

Very little can be said with confidence about couples' take-up behaviour other than that increasing entitlements encourages take-up. Much more can be said about lone parents, however. They were less likely to claim FC or WFTC if they were more educated, home-owners, or lone fathers; earnings, however, had no discernible effect. Entitlement was an important determinant of take-up: for lone parents with the median probability of take-up, the estimated impact of a 10 per cent increase in entitlement on the probability of take-up ranged between 1.5 and 2.1 percentage points under Family Credit and between 0.7 and 1.4 percentage points under WFTC.

 ISBN 1-904938-11-1. JEL classification: H24, H31, I38. Stuart Adam is a Senior Research Economist and Mike Brewer is a Programme Director at the Institute for Fiscal Studies. Part of this paper draws on previous unpublished work by the authors in collaboration with Elaine Kelly, Jonathan Shaw, María José Suárez Fernandez and Ian Walker, to whom the authors extend their gratitude. This paper is part of the project called "Econometric Research: Impact of Working Families' Tax Credit" funded by the Inland Revenue. The authors are grateful to Mike Bielby for comments on earlier drafts. Data from the Families and Children Survey and the Family Resources Survey were made available by the Department for Work and Pensions, and are also available at the UK Data Archive. All errors and omissions remain those of the authors alone. Contact: mike.brewer@ifs.org.uk.

We can neither reject the hypothesis that childcare support incorporated in the programmes was ignored entirely in the take-up decision nor reject the hypothesis that it was fully taken into account.

The greater generosity of WFTC relative to FC meant that take-up of WFTC was higher than we would have expected had FC simply continued unchanged. When we control for rising entitlement (and other observed explanatory factors) to isolate the other aspects of the reform, we find that the first full year of WFTC was associated with a fall in lone parents' probability of take-up of about 3-6 percentage points, assuming that take-up of FC would have continued to rise at the rate it had been. This is true even among lone parents who would have been entitled to FC. However, by 2002, take-up had recovered to the level we would have expected under Family Credit. Using alternative assumptions for the trend in FC take-up reduces, but does not negate, the estimated fall in the first year of WFTC, and means that by 2002 WFTC take-up was above the level we would have expected under FC.

Lone parents' take-up of WFTC was associated with awareness of FC/WFTC: non-recipients were 10 percentage points more likely to claim in the following year (should they become entitled) if they knew the name of the in-work support programme, although awareness in summer 1999 of the forthcoming reform did not seem to matter. Take-up was also strongly correlated with attitudes towards in-work benefits and with knowledge of the income level at which FC/WFTC entitlement ran out, but there is a strong possibility of reverse causation in these cases.

1. Introduction

The current government has substantially increased the use of means-tested tax and benefit programmes to try to help people on low incomes. An important early example of this was the replacement in October 1999 of Family Credit (FC), a benefit providing support for low-income working parents, by Working Families' Tax Credit (WFTC). WFTC was delivered differently from FC, it was described as a tax credit rather than a benefit, and it was also much more generous than its predecessor.

However, the efficacy of using means-testing to help people on low incomes is limited by the fact that many of the people eligible for means-tested programmes do not take them up. Because of this, one of the government's stated aims when introducing WFTC was to encourage take-up, arguing that "as a tax credit rather than a welfare benefit, it will reduce the stigma associated with claiming in-work support, and encourage higher take-up".[2]

In this paper we try to answer the question of whether the replacement of FC by WFTC did indeed encourage take-up. We also try and identify more generally what factors are important in explaining non-take-up of FC and WFTC, in particular quantifying the effect of entitlement level and examining the effects of people's knowledge of, and attitudes towards, in-work support. Our approach is an econometric one, investigating the relationship between take-up of FC/WFTC and a variety of explanatory variables in two micro-data-sets, the Family Resources Survey (FRS) and the Families and Children Survey (FACS). In these regressions we ignore the fact that earnings and therefore entitlement may be endogenous to the take-up decision. A companion paper, Brewer et al (2005), presents results from a structural

[2] Para. 1.04 and para. 2.09 of HM Treasury (1998)

model of parents' joint decisions over how much to work and whether to claim in-work support. This allows for the endogeneity of earnings, but at the cost of imposing additional functional form restrictions.

To some extent this analysis has been overtaken by events: WFTC was itself replaced, along with certain other elements of state support for families, by Child Tax Credit and Working Tax Credit from April 2003.[3] However, a retrospective evaluation of FC and WFTC may be interesting in its own right, and a better understanding of the determinants of take-up should prove valuable for the design of future policy.

This paper supersedes Brewer et al (2003), using additional years of data to focus more on WFTC, and making more use of the attractive features of the Families and Children Study (see especially Sections 7 and 8). Unlike Brewer et al (2003), however, this paper does not use Labour Force Survey data, and does not address the question of whether there was a pre-reform dip in take-up. (Brewer et al, 2003, cannot reject the hypothesis that there was no such dip).

The rest of the paper is structured as follows. Section 2 briefly describes WFTC and how it differed from Family Credit. Section 3 details the data we use and how well they record FC/WFTC receipt, while Section 4 compares these data on receipts with the entitlements that we model. In Section 5 we set out an economic model of take-up and present the results of our baseline regressions; we also attempt to test how far people took into account support for childcare and lost Housing Benefit and Council Tax Benefit when deciding whether to claim FC/WFTC. Section 6 addresses the key question of what impact the move from FC to WFTC had on take-up. In Section 7 we use data from FACS on people's knowledge and attitudes to help explain take-up

[3] See Brewer (2003a) for a brief description of the Child Tax Credit and Working Tax Credit.

behaviour. Section 8 uses the panel element of FACS to examine how robust our earlier findings are to relaxing some of the assumptions of the baseline model. Section 9 summarises the findings and concludes.

2. Family Credit and Working Families' Tax Credit

Between October 1999 and March 2003, families with children in which an adult worked at least 16 hours per week were eligible for Working Families' Tax Credit (WFTC).[4] The amount of credit depended upon weekly earnings, hours worked, the number of qualifying children (but not the number of adults), and savings (savings over £3,000 reduced the award; savings over £8,000 removed eligibility completely). Couples were assessed jointly. Beyond an income threshold (initially £90 a week after tax), the credit was tapered away at 55% of net income, with a small extra credit for families in which someone worked at least 30 hours a week.

WFTC was introduced in October 1999 as a replacement to Family Credit (FC), and was fully phased in by April 2000. Although it owed much to its predecessor, two key differences were the generosity of WFTC and the payment mechanism.[5] WFTC was more generous than FC in three ways: it had higher credits (particularly for young children), families could earn more before the credit was phased out, and it had a lower withdrawal (or taper) rate.[6] WFTC was administered by the Inland Revenue whereas Family Credit was administered by the old Department of Social Security,

[4] For an outline of the entire transfer system affecting families with children in the UK, see Adam and Brewer (2004) and Crawford and Shaw (2004).

[5] See Blundell et al (2000) and Dilnot and McCrae (1999) for a more detailed comparison of WFTC and FC.

[6] These increases were part of a larger set of reforms that increased the generosity of government transfers for all families with children. See Adam and Brewer (2004) and Brewer and Gregg (2003) for more detail.

but there was no structural link between WFTC and the income tax system – as is the case with the earned income tax credit (EITC) in the US, for example – and the vast majority of WFTC payments more than offset claimants' income tax liabilities.[7]

WFTC also significantly changed the system of support for childcare costs. Under FC, childcare costs up to £60 (£100) a week for families with 1 child (2 or more children) could be discounted when assessing income for the means test. Under WFTC, there was a separate childcare tax credit element. This was more generous than the FC childcare disregard, providing a 70% subsidy to the parent on costs up to £100 (£150) a week for families with one child (two or more children). The credit was paid in addition to the rest of families' WFTC payments, rather than being an income disregard, making it worth more to those on the lowest incomes. This change led to a dramatic increase in the number of families benefiting from additional support for childcare costs, albeit from a low base.[8]

In addition to FC/WFTC, Housing Benefit (HB) and Council Tax Benefit (CTB) provided (and still provide) support for low- and moderate-income families. Working families were less likely to be on HB and CTB than non-working families: around 20% of families on WFTC were on Housing Benefit in May 2000, and around 18% were on Council Tax Benefit.[9] Increases in FC/WFTC were worth less for families on HB or CTB because FC/WFTC awards counted as income in the HB and CTB means tests; we discuss the implications of this for our analysis in Section 5.

[7] See Appendix B of Dilnot et al (2001) for a longer discussion on how to classify tax credits.
[8] See Inland Revenue (2003) for more details of WFTC recipients.
[9] See DSS (2000a/b) for out-of-work families, and DWP (2001) for WFTC recipients.

3. Data

Studies of take-up typically require data from a household survey that records both receipt and enough information to allow entitlement to be estimated, usually using a micro-simulation model. We use two such datasets: the Family Resources Survey (FRS) and the Families and Children Study (FACS).[10] Both are annual surveys, with each year's data covering part (FACS) or all (FRS) of a financial year but crossing calendar years; for brevity we refer to years of data by the calendar year in which they start, so that "2001", for example, refers to financial year 2001/2.

The FRS is a cross-sectional survey of around 26,000 representative households per year, comprising almost 30,000 families (of which about 8,000 contain children), drawn from postcode records across Britain. Questions are asked of each household member, and some can be answered 'by proxy' if a household member is absent. The data we use in this report cover the period from 1995 to the last year of WFTC, 2002.

FACS is a panel survey of families with children, drawn from Child Benefit records. To make up for families dropping out of the survey, new families are added to the panel each year. Unlike the FRS, which is collected year-round, FACS data are collected over a few months: initially this was over the summer and autumn, then later over the autumn and winter. In its first two years, 1999 and 2000, FACS contained around 4,500 families; roughly half were a representative sample of lone parents, and half were a sample of low-income couples. The income cut-off point was higher in 2000 than in 1999, but it was high enough in both years to ensure that all those entitled to FC/WFTC were included. In 2001 the income screen was removed altogether and the survey expanded to around 7,500 families, a representative sample

[10] Further information on the FRS and FACS is available at www.data-archive.ac.uk/findingData/frsTitles.asp and www.dwp.gov.uk/asd/asd5/facs respectively.

of all families with children. The main survey is directed at the 'mother figure' in the family whenever possible; some questions are also asked of partners, and a subset of these can be answered by proxy by the main respondent if the partner is not interviewed in person. In this report we use the first four years of FACS data, from 1999 to 2002.

The samples we analyse have a number of families from the original data removed. Families without children were not eligible for FC or WFTC and are therefore not analysed. We remove families in which an individual does not report their earnings since we cannot calculate their FC/WFTC entitlement,[11] and we remove families containing a self-employed individual since the data on self-employment income are generally considered unreliable. In FACS, the 16 couples in which the man was the main respondent are removed to simplify the empirical analysis. In the FRS, we exclude the 660 observations from the period from October 1999 to March 2000 because WFTC was phased in gradually over this period and we cannot be certain whether a family is receiving FC or WFTC. [12]

Recording FC/WFTC receipt

Figure 1 shows the proportion of lone parents and couples with children in the two surveys who report receiving FC/WFTC, alongside a series for the population as a whole calculated from administrative data on the number of recipients and estimates

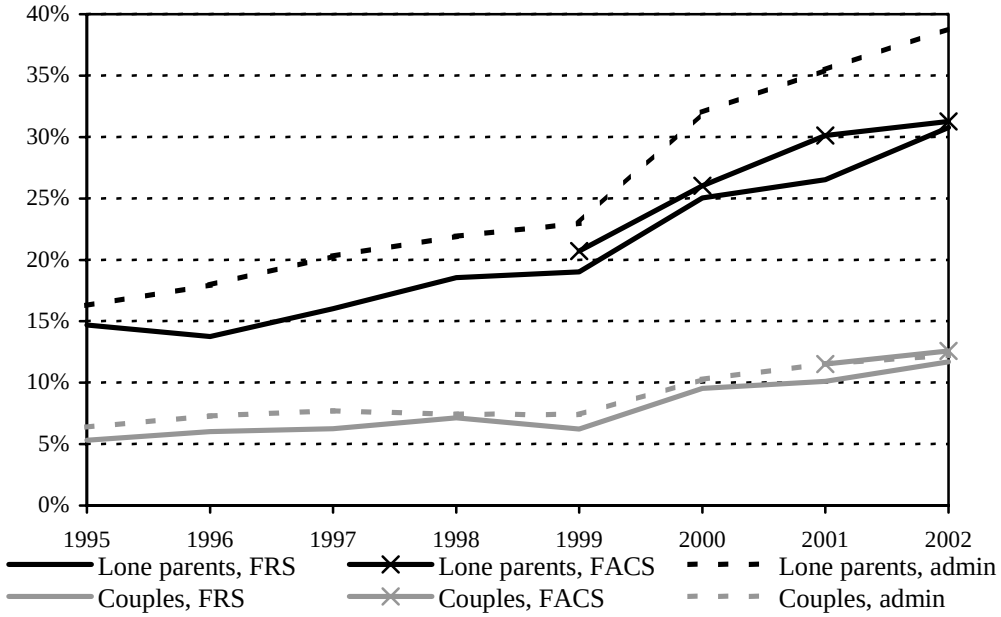
[11] Missing values of earnings and other data are imputed in both datasets. We reverse this imputation where possible in FACS and remove the relevant observations if necessary. We do not reverse imputation in the FRS, but Hancock and Barber (2005) find that, despite the large scale of imputation in the FRS, analysis of pensioners' take-up of Income Support is not much affected.

[12] As mentioned in Section 1, WFTC was abolished in April 2003 as the child and working tax credits were introduced. To ease the transition, there was a short "phase-out" period between December 2002 and March 2003 during which all WFTC awards due for renewal were automatically extended to the end of March 2003 without claimants' circumstances being reassessed. This feature is ignored in our model, and we do not think this will have any substantial impact on our results.

of the numbers of lone parents and couples in the population from the Households Below Average Income series.

The surveys slightly under-report receipt of FC/WFTC, particularly for lone parents, but, on the whole, track administrative figures fairly well, showing a gentle rise under FC and a somewhat faster rise under WFTC, especially in its first year.

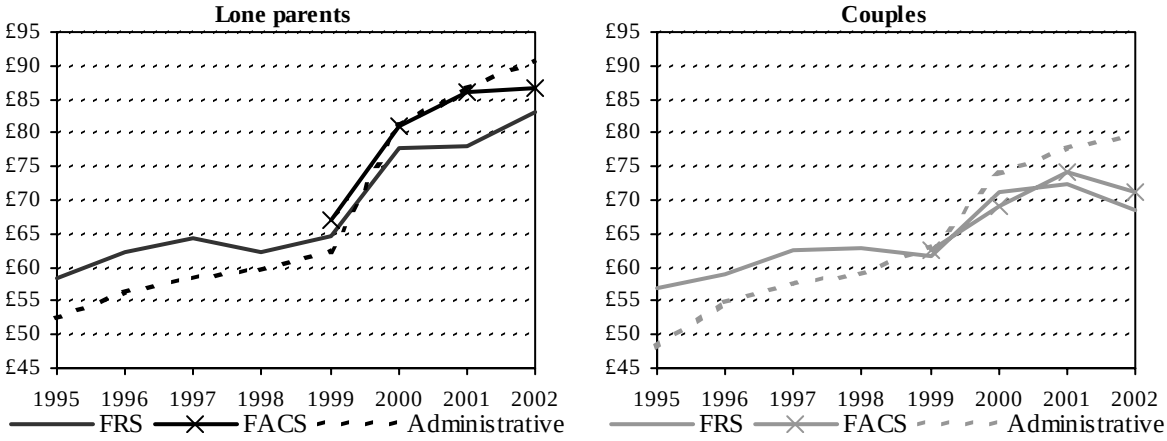
Figure 1. Proportion of families with children receiving FC/WFTC



Note: 'Administrative' figures are for May up to and including 1999 and averages of quarterly figures thereafter, and are calculated by dividing administrative data on the number of recipients (Inland Revenue, 2003a) by FRS data on the number of families with children (scaled up to the population using the FRS's supplied grossing factors). Couples in FACS not shown in 1999 and 2000 because the surveyed sample was not representative.

Figure 2 shows the average level of receipt reported by those receiving, showing a sharp increase in 2000 as WFTC replaced the less generous FC. There is a reasonable match with administrative statistics, although the FRS seems to overstate average FC receipt and understate average WFTC receipt relative to administrative data.

Figure 2. Mean FC/WFTC receipt among recipients



Note: Administrative figures are for May up to and including 1999 and averages of quarterly figures thereafter, from Inland Revenue (2003a).

4. Modelling FC/WFTC entitlement

We estimate entitlement to FC/WFTC by applying the Institute for Fiscal Studies' tax and benefit microsimulation model, TAXBEN, to FRS and FACS data.[13] Table 1 shows the number of families modelled as being entitled to FC/WFTC in each year and how that overlaps with reported receipt in the two surveys.

[13] TAXBEN is described in Giles and McCrae (1995). For FACS we use a simplified version of TAXBEN previously used in Paull et al (2000).

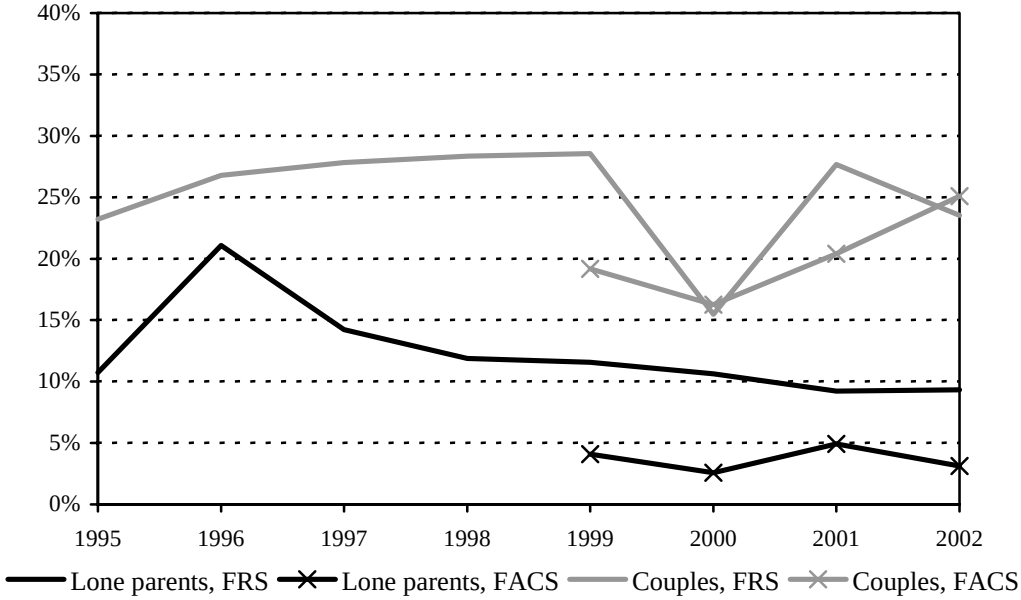
Table 1. Entitlement to and receipt of FC and WFTC in analysed sample

	Entitled		Non-entitled		Total
	Recipients	Non-recipients	Recipients	Non-recipients	
FRS 1995	473	323	96	6,233	7,125
FRS 1996	445	324	141	6,150	7,060
FRS 1997	457	306	122	5,589	6,474
FRS 1998	517	271	127	5,339	6,254
FRS 1999	286	167	67	2,916	3,436
FRS 2000	816	633	121	4,952	6,522
FRS 2001	894	572	201	5,232	6,899
FRS 2002	1,086	564	207	5,558	7,415
FACS 1999	700	316	84	1,579	2,679
FACS 2000	829	599	79	1,220	2,727
FACS 2001	951	558	128	3,158	4,822
FACS 2002	978	455	150	3,135	4,718

Note: Analysed sample restricted as described in Section 3.

Figure 3 shows the proportion of those in our analysed sample that report receiving FC/WFTC that we model as not being entitled. Much of this reflects changes in family circumstances between applying for FC/WFTC and completing the survey, bearing in mind that FC and WFTC both operated on the basis of a six-month award period; it may also reflect errors in the application or the assessment thereof, misreporting in the survey, or modelling error on our part. The higher rate of non-entitled recipients among couples than lone parents probably reflects the extra pieces of information (and therefore potential sources of error) needed to calculate entitlement for couples – in particular, there are two incomes that could change or be mis-measured rather than one.

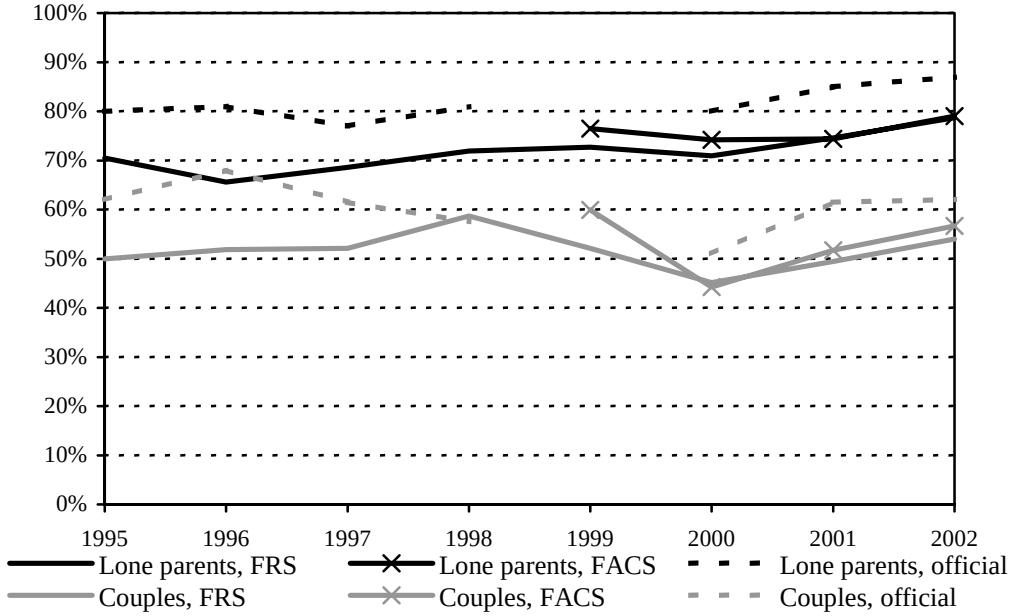
Figure 3. Proportion of FC/WFTC recipients modelled as not entitled



Note: Analysed sample restricted as described in Section 3.

Figure 4 shows the raw take-up rates in the samples we analyse, along with official figures. Official figures use FRS data to estimate the population who are eligible but not receiving, but take the recipient population directly from administrative data. This necessarily involves including apparently non-entitled recipients in both numerator and deonominator, and usually leads to higher estimates of the take-up rate than those that rely exclusively on survey data (see Brewer (2003b)). The trends under FC are ambiguous, but for both lone parents and couples we can see a fall in take-up when WFTC was introduced in 2000 and a rise thereafter. Table 2 shows the sample sizes on which Figure 4 and the analysis to follow are based.

Figure 4. Take-up rate of FC/WFTC among entitled sample



Notes: Analysed sample restricted as described in Section 3. Official figures are mid-points of stated ranges from Inland Revenue (2003b and 2005) for WFTC and DSS (various) for FC. Official figures are not published for 1999.

Table 2. Number of families in the analysed samples entitled to FC/WFTC

	FRS		FACS	
	Lone parents	Couples	Lone parents	Couples
1995	366	430	-	-
1996	337	432	-	-
1997	360	403	-	-
1998	413	375	-	-
1999	242	211	552	464
2000	627	822	659	769
2001	673	793	753	756
2002	789	861	743	690
Total	3,807	4,327	2,707	2,769

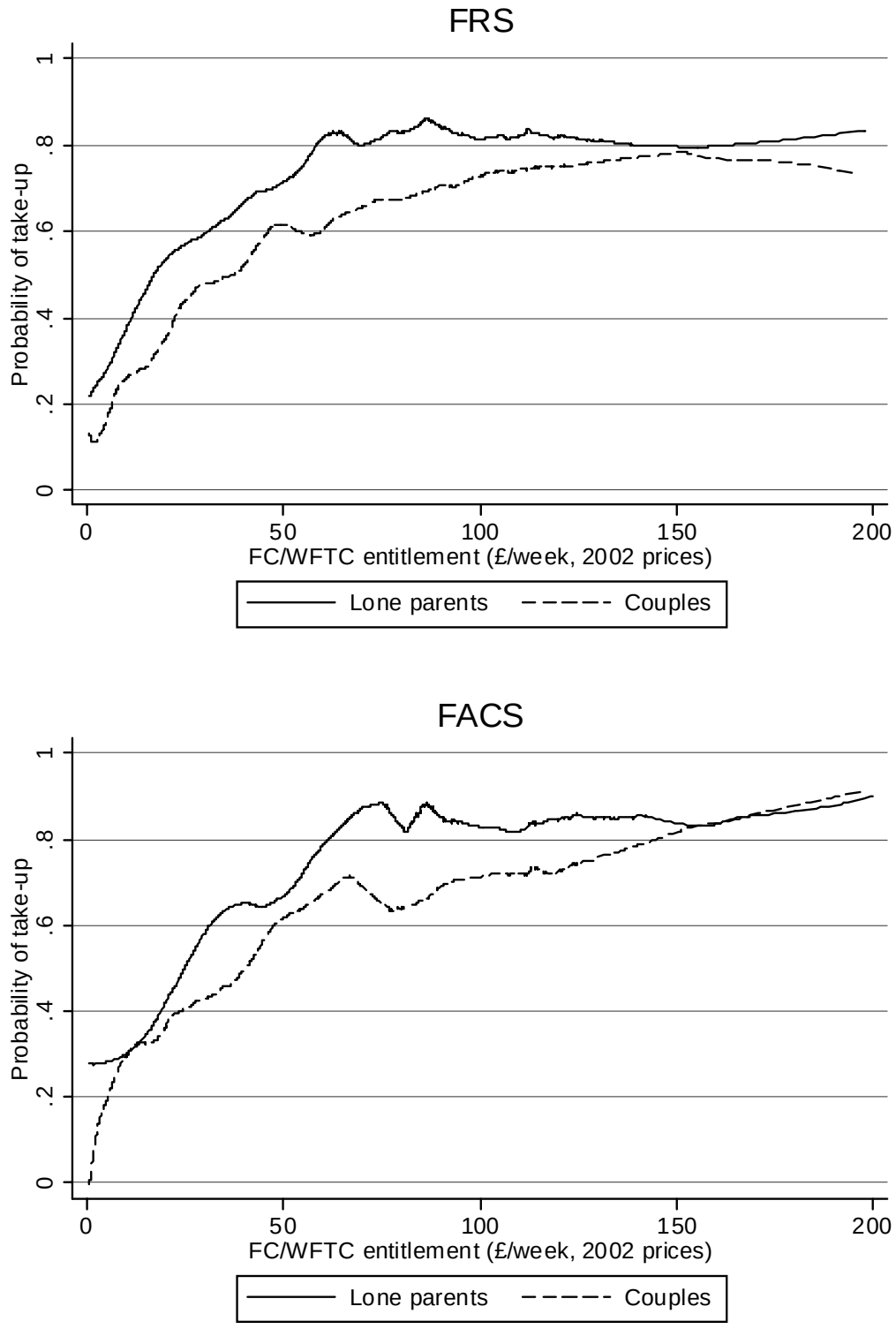
Note: Analysed sample restricted as described in Section 3.

Figure 5 shows the raw relationship between take-up rates and entitlement levels, taking all years together.[14] Unsurprisingly, it shows a positive concave relationship: broadly speaking, we would expect high entitlement to encourage take-up, but with diminishing returns. But to separate the effect of entitlement on take-up from the

[14] Entitlement levels and other monetary variables are reflated to constant 2002 prices throughout this paper.

effect of family characteristics which may be correlated with entitlement, and to identify the effect of the move from FC to WFTC, we must estimate an empirical model of take-up behaviour. That is the subject of the next Section.

Figure 5. Variation in take-up probability with entitlement over all years



Note: Non-parametric regression (lowess) estimates. As is typical with non-parametric regressions, estimates at the extremes of the entitlement distribution are imprecise and should be treated with caution. Analysed sample restricted as described in Section 3.

5. An economic model of take-up

As with cases of apparently non-entitled recipient families (see Section 4 above), families who report not receiving FC/WFTC but whom we model as being entitled may be observed because of errors in survey responses, FC/WFTC applications, application processing, or tax and benefit modelling. However, as is usual in the economics literature since Moffitt (1983), we assume that such cases instead represent optimal decisions by the families not to take up their entitlement because the costs of doing so (time, effort and stigma) outweigh the benefits.

UK studies which have taken this approach include: Blundell et al (1988), which models take-up of Housing Benefit; Fry and Stark (1993), which investigates take-up of the main means-tested benefits from 1984-1990; Dorsett and Heady (1992), which investigates take-up of Family Income Supplement; a series of reports using data from the Programme of Research into Low Income Families which model take-up of Family Credit during the 1990s (see Finlayson and Marsh, 1998); Marsh et al (2001) and McKay (2003), which examine take-up using the FACS survey; and Hancock and Barber (2005), which models take-up of IS amongst pensioners. Brewer (2003b) discusses alternative approaches which may be more desirable but are usually more complicated, including allowing for measurement/modelling error and modelling take-up simultaneously with labour supply (the latter is pursued in Brewer et al, 2005). Currie (2004) gives a partial review of the literature on take-up in the US and the UK.

We specify the take-up decision in a simple way. Following Moffitt (1983), we assume that there is an index I, which depends linearly on a set of observable

explanatory variables X (with an associated vector of coefficients β) and on a random term ε:

$$I = X\beta + \varepsilon$$

If this index is positive, a family claims their entitlement, otherwise they decide not to: I can therefore be thought of as the net utility from claiming. Assuming the random term ε – which reflects unobserved tastes for claiming FC or WFTC – is normally distributed allows us to estimate the probability of claiming using a probit model (see Section 5 of Brewer, 2003b, for more details):

$$\Pr(I > 0 \mid X) = \Pr(\varepsilon > -X\beta \mid X) = \Phi(X\beta)$$

where $\Phi(\cdot)$ is the cumulative standard normal distribution.

We estimate models for lone parents and couples with children separately because the differences in the take-up rates between them suggest different behaviour. The samples consist of those benefits units that are estimated as being entitled to FC/WFTC, and so the estimates are conditional on our modelling entitlement correctly. In this Section, all years of data are pooled together.[15]

Any characteristic which affects either the benefits of additional income or the costs of claiming and receiving FC/WFTC is a candidate for an explanatory variable. Following some exploratory analysis, and in line with previous studies, the explanatory variables included in our preferred specifications of the four regressions

[15] Because FACS is a panel, we calculate standard errors which are robust to clustering on family (ie to unobserved family-specific heterogeneity, uncorrelated with observed characteristics, which influences take-up so that the random term is correlated over time for each family). Section 8 presents a more fundamental treatment which allows for family-specific heterogeneity in estimating the coefficients, not just the standard errors, and which allows for the heterogeneity to be freely correlated with observed characteristics.

(lone parents and couples in FRS and FACS) are listed in Table 3. Earnings and entitlement are entered in logarithms: this broadly captures the shapes of the relationships in Figure 5 and forces the probability of take-up to tend to 0 or 1 as entitlement and earnings fall to zero. The main results appear to be robust to variation in this and other aspects of the specification.

Including quadratic, cubic, quartic and quintic terms in entitlement, rather than using a logarithmic specification, made a negligible difference. Using a very flexible functional form (ten indicators for deciles of entitlement) marginally improved the fit, but increased the risk of over-fitting and precluded the calculation of marginal effects. Given that it is the norm in the existing literature, and given the theoretical appeal of forcing the probability of claiming towards 0 (or 1 if the coefficient is negative) as entitlement falls towards zero, we therefore prefer to use a log specification. Other variables that we investigated but which proved insignificant were: indicators for month of interview, indicator for vocational qualification, interaction term between London indicator and ethnic minority indicator, indicator for receipt of a child disability benefit (in the FRS) and, in FACS, number of disabled children, indicator for rural, and indicator for same sex couple (variables included only for lone parents or only for couples in a dataset were not significant for the other group). We also tried different functional forms for education, region, local area deprivation (in FACS), disability and number of employees in firm as well as entitlement. We tried interacting each variable with year, and our final specification includes the interaction terms that proved significant.

Since the determinants of entitlement are included in our regressions, identification is achieved through the nonlinearity of the relationship between log entitlement, log

earnings and number and age of children, and through increases in real entitlements over time.

Table 3. Explanatory variables included in our preferred specifications

Lone parents, FRS	**Lone parents, FACS**
Year indicators, indicator for being male, age, age squared, number of children, indicator for pre-school child, indicator for adult non-partner in the household (interacted with year), indicator for ethnic minority, indicator for receiving maintenance, indicator for receipt of a disability benefit, 3 housing tenure indicators, 11 region indicators, 3 indicators for age left education, log earnings, log entitlement (interacted with year).	Year indicators, indicator for being male, age, age squared, number of children (interacted with year), indicator for pre-school child, indicator for adult non-partner in the household, indicator for ethnic minority, indicator for receiving maintenance, 3 indicators for receiving disability benefits, 3 housing tenure indicators, indicator for London, indicator for local area in least deprived decile, 3 indicators for age left education, 4 indicators for number of employees in firm, log earnings, log entitlement (interacted with year).
Couples, FRS	**Couples, FACS**
Year indicators, male age, male age squared, female age, female age squared, number of children (interacted with year), indicator for pre-school child, indicator for adult non-partner in the household, indicator for ethnic minority, indicator for receiving maintenance, 3 housing tenure indicators, 11 region indicators, 3 indicators for age man left education, 3 indicators for age woman left education, indicator for man working, indicator for woman working (interacted with indicator for man working), indicator for man working in a large firm, indicator for woman working in a large firm, male job tenure, female job tenure, indicator for female earnings greater than male, log family earnings, log entitlement.	Year indicators, indicator for man answering the partner questionnaire himself (interacted with year), male age, male age squared, female age, female age squared, number of children (interacted with year), indicator for pre-school child, indicator for adult non-partner in the household, indicator for ethnic minority, indicator for receiving maintenance, 3 housing tenure indicators, indicator for London, indicator for Scotland or Wales, 10 indicators for deprivation of local area, 3 indicators for age man left education, 3 indicators for age woman left education, indicator for man working, indicator for woman working (interacted with indicator for man working), 4 indicators for number of employees in man's firm, 4 indicators for number of employees in woman's firm, male job tenure, female job tenure, indicator for female earnings greater than male, log family earnings, log entitlement (interacted with year).

Marginal effects for these probits (evaluated at the means of all regressors) are shown in Tables 4 to 7. It is important to remember that these associations do not necessarily imply causal effects on take-up: it is possible, for example, that there is some reverse causation, or that the coefficient on a regressor is capturing the effect of some unobserved variable correlated with the regressor in question.

Table 4. Take-up probit with preferred specification: Lone parents, FACS

Sample size: 2704

Variable	Marginal effect	Robust s.e.	P-value	Mean value
D. 2000	0.2497	0.0951	0.056	0.2430
D. 2001	0.2364	0.1100	0.089	0.2785
D. 2002	0.3668	0.0746	0.001	0.2748
No. of dependent children	-0.0269	0.0256	0.292	1.6220
- 2000 interaction	-0.0204	0.0312	0.515	0.3898
- 2001 interaction	-0.0189	0.0330	0.566	0.4534
- 2002 interaction	-0.0800	0.0333	0.016	0.4390
D. child aged 0-4	0.0475	0.0257	0.075	0.2493
D. adult non-partner in HH	-0.0001	0.0533	0.999	0.1916
- 2000 interaction	-0.0830	0.0792	0.258	0.0436
- 2001 interaction	0.0433	0.0577	0.482	0.0577
- 2002 interaction	0.0646	0.0563	0.302	0.0562
D. male	-0.1926	0.0743	0.004	0.0251
Age	-0.0018	0.0101	0.856	36.2204
Age^2	-0.0039	0.0132	0.765	13.7075
D. white	-0.0353	0.0381	0.378	0.9360
D. social renter	0.0532	0.0217	0.016	0.4016
D. private renter	0.0760	0.0251	0.006	0.1128
D. other/unknown tenure	-0.0330	0.0506	0.499	0.0399
D. London	-0.2112	0.0495	0.000	0.0692
D. least deprived decile	-0.1929	0.0941	0.021	0.0163
D. receiving maintenance	0.0339	0.0190	0.077	0.4357
D. left education aged 17-18	-0.0706	0.0250	0.003	0.2237
D. left education aged >18	-0.1767	0.0411	0.000	0.0840
D. 10-24 employees in firm	0.0545	0.0249	0.037	0.2138
D. 25-500 employees in firm	-0.0126	0.0231	0.585	0.4830
D. >500 employees in firm	-0.0710	0.0426	0.076	0.0773
D. receiving IB	-0.0607	0.2020	0.749	0.0018
D. receiving DLA	0.0370	0.0810	0.667	0.0163
Log earnings	0.0248	0.0233	0.285	4.7923
Log entitlement	0.2459	0.0380	0.000	4.1352
- 2000 interaction	-0.0856	0.0457	0.061	1.0108
- 2001 interaction	-0.0815	0.0440	0.064	1.1709
- 2002 interaction	-0.1037	0.0427	0.015	1.1434

Notes: Marginal effects are evaluated at the means of the variables. “Robust standard errors” are robust to clustering on family, as described in footnote 15. Analysed sample restricted as described in Section 3.

Table 5. Take-up probit with preferred specification: Couples, FACS

Sample size: 2673

Variable	Marginal effect	Robust s.e.	P-value	Mean value
D. 2000	-0.4195	0.1146	0.002	0.2877
D. 2001	-0.2328	0.1346	0.097	0.2806
D. 2002	-0.0652	0.1392	0.641	0.2581
No. of dependent children	-0.0488	0.0248	0.049	2.2656
- 2000 interaction	0.0175	0.0304	0.565	0.6543
- 2001 interaction	0.0821	0.0305	0.007	0.6281
- 2002 interaction	0.1076	0.0316	0.001	0.5679
D. child aged 0-4	0.0211	0.0305	0.488	0.5713
D. adult non-partner in HH	0.0149	0.0371	0.688	0.1250
Male age	0.0130	0.0118	0.272	36.9817
Male age^2	-0.0256	0.0144	0.075	14.4212
Female age	0.0016	0.0137	0.907	34.1208
Female age^2	-0.0025	0.0186	0.894	12.2434
D. white	-0.0398	0.0499	0.427	0.8792
D. social renter	0.1810	0.0276	0.000	0.3412
D. private renter	0.2662	0.0420	0.000	0.0718
D. other/unknown tenure	0.0303	0.0733	0.680	0.0299
D. London	-0.2855	0.0459	0.000	0.0707
D. Scotland or Wales	0.0929	0.0495	0.064	0.1452
D. local deprivation decile 2	-0.0619	0.0420	0.142	0.1429
D. local deprivation decile 3	-0.1160	0.0461	0.013	0.1171
D. local deprivation decile 4	-0.0666	0.0619	0.285	0.0673
D. local deprivation decile 5	-0.0872	0.0546	0.114	0.0793
D. local deprivation decile 6	-0.2111	0.0613	0.001	0.0498
D. local deprivation decile 7	-0.1624	0.0780	0.047	0.0299
D. local deprivation decile 8	-0.2275	0.0576	0.000	0.0490
D. local deprivation decile 9	-0.3212	0.0652	0.000	0.0359
D. local deprivation decile 10 (least deprived)	-0.4281	0.0684	0.001	0.0086
D. local deprivation unknown	-0.1812	0.0484	0.000	0.2435
D. receiving maintenance	0.0564	0.0534	0.295	0.0516
D. male left education 17-18	-0.0297	0.0415	0.475	0.1208
D. male left education >18	-0.1397	0.0511	0.008	0.0752
D. male left educ. age unknown	-0.0313	0.0429	0.466	0.1650
D. female left education 17-18	0.0062	0.0314	0.844	0.2263
D. female left education >18	-0.1647	0.0485	0.001	0.0935
D. two-earner couple	0.0175	0.0711	0.806	0.2735
D. only male working	0.0910	0.0721	0.209	0.5608
D. only female working	-0.0331	0.0784	0.673	0.1186
D. 10-24 employees in man's firm	-0.0435	0.0510	0.395	0.0947
D. 25-500 employees in man's firm	-0.0722	0.0423	0.089	0.2911
D. >500 employees in man's firm	-0.1236	0.0716	0.092	0.0355
D. 10-24 employees in woman's firm	0.0365	0.0498	0.465	0.0943
D. 25-500 employees in woman's firm	-0.0078	0.0437	0.859	0.2002
D. >500 employees in woman's firm	-0.1060	0.0786	0.185	0.0254
Male job tenure (months)	-0.0001	0.0002	0.800	38.8249
Female job tenure (months)	-0.0008	0.0004	0.049	20.1074

D. receiving IB	0.0361	0.0741	0.628	0.0314
D. receiving SDA	-0.2834	0.1484	0.112	0.0056
D. receiving DLA	-0.0323	0.0509	0.526	0.0662
D. man didn't answer questionnaire	-0.0507	0.0705	0.473	0.3307
- 2000 interaction	0.0459	0.0726	0.529	0.2016
- 2001 interaction	0.1102	0.0696	0.119	0.1915
- 2002 interaction	0.0936	0.0702	0.189	0.1504
D. female earnings greater than male	-0.1960	0.0597	0.002	0.0382
Log earnings	-0.0075	0.0346	0.829	5.2529
Log entitlement	0.1857	0.0267	0.000	3.5718
- 2000 interaction	0.0554	0.0360	0.123	1.0166
- 2001 interaction	-0.0210	0.0345	0.542	1.0207
- 2002 interaction	-0.0626	0.0332	0.060	0.9224

Notes: Marginal effects are evaluated at the means of the variables. "Robust standard errors" are robust to clustering on family, as described in footnote 15. Omitted category has neither partner working, so must always add in one of the employment status indicators, along with the appropriate firm size if 10 or more employees. Analysed sample restricted as described in Section 3.

Table 6. Take-up probit with preferred specification: Lone parents, FRS

Sample size: 3807

Variable	Marginal effect	Robust s.e.	P-value	Mean value
D. 1995	0.0713	0.1592	0.679	0.0961
D. 1996	-0.1394	0.2349	0.524	0.0885
D. 1997	0.0059	0.1851	0.975	0.0946
D. 1998	0.0817	0.1559	0.633	0.1085
D. 2000	0.1495	0.1296	0.333	0.1647
D. 2001	0.3033	0.0693	0.011	0.1768
D. 2002	0.2836	0.0873	0.028	0.2073
No. of dependent children	-0.0113	0.0111	0.308	1.6199
D. child aged 0-4	0.0057	0.0207	0.784	0.3126
D. adult non-partner in HH	-0.1044	0.0914	0.229	0.1734
- 1995 interaction	0.0981	0.0762	0.271	0.0179
- 1996 interaction	0.1230	0.0687	0.153	0.0160
- 1997 interaction	0.0195	0.0953	0.842	0.0194
- 1998 interaction	-0.1654	0.1243	0.148	0.0158
- 2000 interaction	0.0297	0.0874	0.743	0.0278
- 2001 interaction	-0.0156	0.0947	0.868	0.0310
- 2002 interaction	-0.0509	0.0997	0.595	0.0357
D. female	0.0791	0.0351	0.017	0.9433
Age	0.0228	0.0080	0.004	35.8742
Age^2	-0.0348	0.0106	0.001	13.4361
D. ethnic minority	-0.0705	0.0337	0.028	0.0814
D. private renter	-0.1075	0.0276	0.000	0.1258
D. home-owner	-0.1068	0.0181	0.000	0.4274
D. Yorkshire	0.0583	0.0327	0.094	0.0985
D. North-West	0.0507	0.0309	0.117	0.1547
D. East Midlands	-0.0024	0.0391	0.952	0.0688
D. West Midlands	0.0182	0.0360	0.619	0.0914

D. East Anglia	-0.0376	0.0523	0.458	0.0313
D. Greater London	-0.1353	0.0463	0.002	0.0859
D. South-East	-0.0170	0.0341	0.614	0.1484
D. South-West	-0.0038	0.0384	0.920	0.0764
D. Wales	0.0868	0.0365	0.035	0.0494
D. Scotland	0.0249	0.0338	0.471	0.1219
D. left education aged 17-18	-0.0204	0.0190	0.275	0.2193
D. left education aged >18	-0.0761	0.0298	0.007	0.0841
D. left education age unknown	-0.2850	0.3795	0.424	0.0005
D. receiving a disability benefit	-0.5854	0.0544	0.000	0.0126
D. receiving maintenance	-0.0123	0.0166	0.457	0.3480
Log earnings	-0.0020	0.0174	0.909	4.9136
Log entitlement	0.1994	0.0401	0.000	3.9480
- 1995 interaction	-0.0302	0.0481	0.530	0.3561
- 1996 interaction	0.0082	0.0512	0.873	0.3334
- 1997 interaction	-0.0147	0.0482	0.760	0.3612
- 1998 interaction	-0.0185	0.0480	0.701	0.4120
- 2000 interaction	-0.0586	0.0453	0.196	0.6633
- 2001 interaction	-0.1198	0.0451	0.008	0.7234
- 2002 interaction	-0.0880	0.0443	0.047	0.8541

Notes: Marginal effects are evaluated at the means of the variables. “Robust standard errors” are robust to clustering on family, as described in footnote 15. Analysed sample restricted as described in Section 3.

Table 7. Take-up probit with preferred specification: Couples, FRS

Sample size: 4327

Variable	Marginal effect	Robust s.e.	P-value	Mean value
D. 1995	0.1405	0.0968	0.160	0.0994
D. 1996	0.0666	0.1007	0.511	0.0998
D. 1997	-0.0039	0.1050	0.971	0.0931
D. 1998	-0.0219	0.1067	0.838	0.0867
D. 2000	0.1569	0.0915	0.096	0.1900
D. 2001	-0.0009	0.0957	0.992	0.1833
D. 2002	0.1976	0.0883	0.033	0.1990
No. of dependent children	0.0350	0.0315	0.266	2.2679
- 1995 interaction	-0.0734	0.0388	0.058	0.2235
- 1996 interaction	-0.0133	0.0388	0.732	0.2320
- 1997 interaction	0.0131	0.0400	0.743	0.2172
- 1998 interaction	0.0515	0.0403	0.201	0.2101
- 2000 interaction	-0.0648	0.0367	0.077	0.4225
- 2001 interaction	0.0350	0.0367	0.340	0.4093
- 2002 interaction	-0.0233	0.0362	0.520	0.4356
D. child aged 0-4	0.0236	0.0228	0.300	0.6134
D. adult non-partner in HH	-0.0661	0.0276	0.017	0.1156
Male age	0.0143	0.0091	0.117	36.5147
Male age^2	-0.0201	0.0114	0.077	14.0398
Female age	-0.0019	0.0104	0.856	33.7372
Female age^2	-0.0022	0.0144	0.880	11.9637

D. ethnic minority	-0.0477	0.0270	0.078	0.1676
D. private renter	-0.0630	0.0294	0.033	0.1061
D. home-owner	-0.1603	0.0190	0.000	0.5057
D. Yorkshire	0.0328	0.0405	0.420	0.1026
D. North-West	0.0705	0.0389	0.072	0.1186
D. East Midlands	-0.0469	0.0413	0.257	0.0892
D. West Midlands	0.0036	0.0406	0.929	0.1033
D. East Anglia	-0.0790	0.0550	0.155	0.0337
D. Greater London	-0.1546	0.0429	0.001	0.0887
D. South-East	-0.1040	0.0381	0.007	0.1350
D. South-West	0.0409	0.0421	0.332	0.0867
D. Wales	-0.0815	0.0445	0.070	0.0631
D. Scotland	-0.0760	0.0397	0.057	0.1063
D. male left education 17-18	-0.0868	0.0256	0.001	0.1361
D. male left education >18	-0.0694	0.0330	0.037	0.0985
D. male left educ. age unknown	0.1948	0.1550	0.250	0.0023
D. female left education 17-18	-0.0216	0.0221	0.329	0.2048
D. female left education >18	-0.1184	0.0332	0.000	0.0904
D. female left educ. age unknown	-0.3309	0.0828	0.003	0.0051
D. only male working	0.1596	0.0278	0.000	0.5946
D. only female working	0.1888	0.0467	0.000	0.1498
D. >25 employees in man's firm	-0.0591	0.0189	0.002	0.4920
D. >25 employees in woman's firm	-0.0087	0.0276	0.752	0.2297
Male job tenure (months)	-0.0004	0.0001	0.001	55.8574
Female job tenure (months)	-0.0007	0.0003	0.007	20.2078
D. receiving maintenance	0.0156	0.0427	0.716	0.0416
D. female earnings greater than male	-0.0958	0.0467	0.042	0.1916
Log earnings	-0.0409	0.0219	0.062	5.3127
Log entitlement	0.1510	0.0119	0.000	3.5266

Notes: Marginal effects are evaluated at the means of the variables. "Robust standard errors" are robust to clustering on family, as described in footnote 15. Omitted category has both partners working in firms with 25 employees or fewer. Analysed sample restricted as described in Section 3.

Unsurprisingly, entitlement level was an important determinant of take-up: the marginal effect of log entitlement was large and highly significant for both lone parents and couples in all years. Rather than evaluating the marginal effect of log entitlement at the mean of all regressors (as reported in Tables 4 to 7), a better summary statistic might be the marginal effect evaluated for a family with the median probability of take-up over the whole period (Table 8). For lone parents in FACS, this marginal effect was 0.21 in 1999, meaning that (conditional on all the other variables listed above) a 10 per cent increase in FC entitlement would increase the probability

of take-up by 2.1 percentage points from a base of 82.5%; this marginal effect fell to 0.14, 0.14 and 0.12 respectively in the first three years of WFTC. The marginal effect for the median lone parent (with a take-up probability of 78.7%) was slightly lower in the FRS, fluctuating between 0.15 and 0.19 from 1995 to 1999 and then falling to 0.13, 0.07 and 0.10 respectively in the first three years of WFTC. For couples in FACS with the median take-up probability (55.8%), the marginal effect was 0.15 under FC, rising to 0.20 in the first year of WFTC before falling back to 0.14 and then 0.10. Entitlement was not interacted with year for couples in the FRS, so the marginal effect was estimated at a constant 0.13 on a base of 53.5%.

Table 8. Marginal effect of log entitlement on take-up

	FRS		FACS	
	Lone parents	Couples	Lone parents	Couples
1995	0.15	0.13	–	–
1996	0.19	0.13	–	–
1997	0.16	0.13	–	–
1998	0.16	0.13	–	–
1999	0.18	0.13	0.21	0.15
2000	0.13	0.13	0.14	0.20
2001	0.07	0.13	0.14	0.14
2002	0.10	0.13	0.12	0.10
Baseline take-up rate	*78.7%*	*53.5%*	*82.5%*	*55.8%*

Note: Authors' calculations from the coefficients reported in Tables 4 to 7. Baseline take-up rate is the median probability of take-up over the whole period, at which the marginal effect is evaluated. Analysed sample restricted as described in Section 3.

A number of other variables were significant in explaining take-up.[16] Conditional on other variables, more educated groups were less likely to claim. In FACS, lone fathers were 20 percentage points less likely to take up FC/WFTC than lone mothers, and couples were 20 percentage points less likely to claim if the female was the higher earner; these effects were smaller in the FRS, at 8 percentage points and 10 percentage points respectively. Lone parents, and in later years, couples, in the FRS

[16] Here, and elsewhere unless otherwise stated, significance is assessed at the 5% level.

were less likely to take up if there was another adult in the household, though this made no difference in FACS. Lone parents from ethnic minorities were less likely to take up in the FRS, though this made no significant difference for couples or for lone parents in FACS. Female job tenure had a significant effect for couples: an extra year's tenure reduced the probability of take-up by 0.8 percentage points in the FRS, 0.9 percentage points in FACS. Male job tenure had a smaller effect but was still significant for couples in the FRS: an extra year's tenure reduced the probability of take-up by 0.5 percentage points.

The effect of number of children does not tell a simple story. In FACS, lone parents with more children were significantly less likely to take up WFTC, but not FC; for couples, on the other hand, additional children significantly *increased* the probability of take-up in 2002, but significantly *decreased* it in 1999. In the FRS the number of children made no difference, except that in 1998 and 2001 couples with more children were more likely to take up.

Home-owners were significantly less likely to take up than social renters. This difference was more pronounced amongst couples, especially in FACS: in the FRS, the difference was 11 percentage points for lone parents and 16 for couples, while in FACS the difference was 5 percentage points for lone parents and 18 for couples. The difference in take-up between home-owners and private renters was even bigger in FACS (and the gap between lone parents and couples even more pronounced), at 8 percentage points for lone parents and 27 for couples; but in the FRS, the difference was zero for lone parents and only 10 percentage points for couples.

Geographical variables also proved significant, as did combinations of employment status and firm size, but these results are difficult to summarize because of the

different and complicated ways they enter into the different equations, so the reader is referred to Tables 4 to 7. Interestingly, the coefficient on log earnings was small and insignificant for both lone parents and couples in both datasets. Year indicators were significant throughout but their interpretation is not intuitive because they were interacted with other variables; changes over time are discussed in Section 6.

No individual disability benefit had a significant effect on take-up in the FACS regressions. The indicator for receipt of any disability benefit reduced the probability of take-up by a massive 59 percentage points for lone parents in the FRS, but this is probably an artefact caused by the inclusion of Disability Working Allowance and Disabled Person's Tax Credit – recipients of which are ineligible for FC/WFTC – in the set of disability benefits. An alternative approach which would remove this problem would be to exclude DWA/DPTC recipients altogether from the analysis.

Childcare support, Housing Benefit and Council Tax Benefit

One of the possible reasons why some families entitled to FC/WFTC did not claim it is that they might not have taken full account of the childcare component that was available. Since the changed form and increased generosity of childcare support were among the most important aspects of the WFTC reform, it is worth examining whether people did take account of the childcare support. To identify the impact of the childcare subsidy, we include the childcare element of FC/WFTC separately from the basic entitlement in the regression. Taking into account that entitlement is entered in logarithms, we can write:

$$\log(FC+CC) \;=\; \log\left(\frac{(FC+CC)FC}{FC}\right) \;=\; \log FC + \log\left(1+\frac{CC}{FC}\right)$$

where *FC* is the basic Family Credit or Working Family Tax Credit entitlement excluding childcare support and *CC* is the additional entitlement due to eligible childcare costs. Comparing the coefficients on the two terms on the right hand side of this equation should allow us to check whether the families take account of the childcare subsidy when applying for FC/WFTC. If the coefficient on the second term is zero, families do not value the childcare component at all; if families value the childcare component as much as their basic entitlement, the coefficients should be equal. We perform this analysis using only FACS, since spending on childcare is less well identified in the FRS (see Brewer and Shaw (2004)).[17]

In practice, the coefficients on the childcare component are not well enough determined to allow strong conclusions. We cannot reject the hypothesis that childcare support is fully taken into account, yet at the same time it is borderline whether childcare support is taken into account at all – the estimated coefficient is just significant for lone parents and just insignificant for couples (p-values of 0.04 and 0.06 respectively).

Another possible reason for non-take-up of FC and WFTC is that they acted to reduce entitlements to Housing Benefit and Council Tax Benefit. For HB and CTB recipients, the net financial gain to claiming FC or WFTC would usually be smaller than their calculated entitlement to FC/WFTC. In principle, this could be analysed in a similar way to childcare, testing whether HB and CTB recipients took full account of the reduction in these benefits associated with claiming FC/WFTC – if so, our regressions should use the change in net income (given by the FC/WFTC amount less the change in HB and/or CTB) rather than the amount of FC/WFTC as a determinant

[17] The childcare questions in FACS changed in 2000, leading to an increase in reported childcare spending. However, this proves unimportant since we cannot reach firm conclusions in any case.

of FC/WFTC take-up. However, the viability of such an analysis is severely compromised by the low take-up rates of HB and CTB among low-income workers, and attempting the analysis for HB yielded perverse and implausible results. The ideal solution would be to model joint take-up of FC, HB and CTB, but the complexity of this approach puts it beyond the scope of this paper. We therefore persevere with the simplest approach, using FC/WFTC entitlement as an explanatory variable and ignoring its knock-on effect on entitlement to other benefits. As a result, any effect of HB recipients taking the loss of HB into account will be partly captured in regressions by the indicators for social and private renting, acting to reduce the estimated coefficients on these indicators. Thus the 'true' coefficients on renting will be, if anything, even higher than the large and statistically significant positive values reported above.

6. Did anything change under WFTC?

A central concern of this paper is to determine what effect, if any, the move from FC to WFTC had on take-up. Headline take-up rates, as presented in administrative statistics, are of limited usefulness for this purpose because they do not separate the effect of the reform from the effect of contemporaneous changes in the composition and characteristics of the population (and the entitled population in particular). Nor do headline take-up rates allow us to separate the effect of WFTC's higher entitlement levels from the effect of other aspects of the reform (such as the changes to payment method, recipient, application process and even name) which may affect take-up via potential recipients' knowledge and attitudes and the costs associated with the time and effort of claiming.

The regression framework used above can help to disentangle these factors. But to focus on changes over time, we can improve upon the simple pooled-years probit with year indicators presented in Section 5. Instead, we estimate a similar probit model but using only data from the FC period:

$$I_{FC} = X_{FC}\beta_{FC} + \varepsilon_{FC}$$

$$\Pr(I_{FC} > 0 \mid X) = \Phi(X_{FC}\beta_{FC}) .$$

We then use the estimates coefficients from this probit, $\hat{\beta}_{FC}$, to predict the take-up rate of our sample of families entitled to WFTC in later years if behaviour had remained the same as under FC:

$$\text{Predicted } \Pr(I_{WFTC} > 0 \mid X) = \Phi(X_{WFTC}\hat{\beta}_{FC}) .$$

These counterfactual predictions can then be compared with actual take-up rates under WFTC to see whether take-up of WFTC was higher or lower than we would have predicted under Family Credit.[18] This allows us to control for the changing characteristics of the entitled population (in terms of the observable explanatory variables *X* included in our model, listed in Table 3); we can control for (or ignore) the effect of rising entitlements by including (or excluding) entitlement level as an explanatory variable and thus a basis for prediction.

The description and equations above are slightly simplified, just dividing the data into FC and WFTC periods. In fact we have several years of data and must decide how to deal with them: simply including year indicators in *X*, as in the pooled-years analysis of Section 5, is not an option for the FC-only regression as it would not yield sensible

[18] Note that we are only comparing point estimates here: there are associated standard errors which would allow us to evaluate the statistical significance of changes.

take-up predictions for the WFTC period. With the FRS, our baseline case uses all years of pre-reform data to estimate the FC-only model and allows for a linear time trend in take-up, effectively assuming, for the purposes of our counterfactual, that take-up of FC (conditional on our control variables) would have carried on rising or falling at the same rate after 1999 as it had been in the previous four years. The trend actually estimated is an increase in take-up of just under 1 percentage point per year for lone parents and just under 2 percentage points per year for couples. We also present results estimated under two other assumptions: restricting there to be no time trend at all, and allowing for a linear time trend but forcing it to stop in 1999 (so that we assume that take-up would not have risen any further if FC had continued in place). Of necessity, the course followed for FACS is to assume that take-up would have remained at its 1999 level, since we have no earlier data from which to estimate a trend.

Table 9 compares actual take-up rates of WFTC in our sample with the take-up rates we would have predicted under FC for families with the same characteristics without controlling for entitlement levels. The introduction of WFTC and later reforms were characterised by substantial increases in the generosity of in-work support and we would therefore expect (other things being equal) take-up to rise, since, as shown in Section 5, entitlement is an important determinant of take-up. Accordingly, controlling for household characteristics but not entitlement in Table 9, we find that lone parents' take-up in each year of WFTC is higher than we would have predicted from behaviour under Family Credit – in other words, a lone parent entitled to WFTC in a particular year was more likely to claim it than a similar lone parent was to claim FC in previous years. The same is true for couples in the FRS. For couples in FACS, we would have similarly under-predicted take-up in 2001 and 2002, but in 2000 the

story is slightly different: take-up in the first year of WFTC recorded in FACS was slightly lower than we would have predicted from the last year of FC. This suggests that the positive effect of rising entitlement on take-up is being outweighed by some other change; this may be the effect of lower take-up amongst couples newly entitled as a result of the WFTC reform: take-up did rise amongst couples who would have been entitled to FC anyway (ie if FC had simply been uprated and the WFTC reform not happened).

Table 9. Actual and predicted take-up rates of WFTC without controlling for entitlement

	FRS				FACS			
	Lone parents		Couples		Lone parents		Couples	
	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted
2000	71.0%	65.0%	45.0%	41.8%	74.2%	71.2%	44.6%	47.1%
2001	75.0%	66.0%	49.4%	43.2%	74.3%	69.4%	52.4%	45.7%
2002	78.7%	66.6%	54.0%	42.1%	79.1%	69.4%	56.7%	46.0%

Notes: 'Actual' take-up rates are take-up rates of the WFTC-entitled sample in the relevant year. 'Predicted' take-up rates are out-of-sample predictions of take-up for the WFTC-entitled sample in the relevant year, based on an FC-only probit which excludes entitlement as a regressor and allows for a linear time trend, as described in the text. Analysed sample restricted as described in Section 3.

Table 10 shows results for the FRS if we impose the restriction that there is no time trend or that it stops in 1999. Unsurprisingly, we then make lower predictions than in our baseline case for what take-up would have been had FC continued beyond 1999, and actual take-up of WFTC looks even higher relative to our predictions, especially in later years.

Table 10. Actual and predicted take-up rates of WFTC without controlling for entitlement: alternative time trend assumptions in the FRS

	Lone parents				Couples			
	Actual	Predicted: linear time trend (baseline)	Predicted: no time trend	Predicted: time trend stops in 1999	Actual	Predicted: linear time trend (baseline)	Predicted: no time trend	Predicted: time trend stops in 1999
2000	71.0%	65.0%	62.0%	64.0%	45.0%	41.8%	37.3%	40.4%
2001	75.0%	66.0%	62.2%	64.2%	49.4%	43.2%	37.2%	40.4%
2002	78.7%	66.6%	61.9%	63.9%	54.0%	42.1%	34.7%	37.8%

Notes: 'Actual' take-up rates are take-up rates of the WFTC-entitled sample in the relevant year. 'Predicted' take-up rates are out-of-sample predictions of take-up for the WFTC-entitled sample in the relevant year, based on an FC-only probit which excludes entitlement as a regressor, as described in the text. Analysed sample restricted as described in Section 3.

We also predict take-up under WFTC based on previous years of WFTC (take-up in 2001 based on behaviour in 2000, and take-up in 2002 based on behaviour in both 2000 and 2001). Take-up in each year was higher than would have been predicted from any previous year's data, presumably reflecting continued increases in the generosity of WFTC.

Behavioural changes can be more clearly identified if we control for (and therefore abstract from) rising entitlement levels: by stripping out the effect of rising entitlements (as well as other observed explanatory factors), we can isolate the effect of other aspects of the reform. Table 11 compares actual take-up rates of WFTC in our sample with the take-up rates we would have predicted for families with the same characteristics had they had the same entitlement to FC as they did to WFTC again using our baseline assumption that there is a linear trend in take-up of FC which would have continued beyond 1999 had FC not been abolished. Table 12 shows results under the two alternative time trend assumptions (no time trend, and a time trend that stops in 1999).

Both tables show that take-up, conditional on entitlement and the other variables in our regressions, fell on the introduction of WFTC, especially for couples: for example, in 2000, the actual take-up in our sample was 74% for lone parents and 45% for couples in FACS, yet the behaviour of families entitled to FC would have led us to predict take-up rates of 80% for these lone parents and 59% for these couples under our baseline set of assumptions. The FRS reveals a similar but less pronounced pattern: actual take-up was 71% for lone parents and 45% for couples in 2000, compared with predicted take-up (under our baseline assumption of a linear time trend) of 74% and 51% respectively. This fall is not primarily due to the extension of support to a new group: if we remove the newly entitled, the gap between predicted and actual take-up in 2000 is almost as big: 87% predicted versus 83% actual for lone parents in FACS (82% versus 79% in the FRS), and 76% predicted versus 65% actual for couples in FACS (69% versus 65% in the FRS). This means that families were less likely to take up WFTC than families with the same characteristics would have been to take-up the same amount of FC; the implication is that people – even people who had previously been entitled to FC – were initially less likely to know about WFTC, or less keen to claim it, than had been the case with Family Credit.

Table 11. Actual and predicted take-up rates of WFTC controlling for entitlement

	FRS				FACS			
	Lone parents		Couples		Lone parents		Couples	
	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted
2000	71.0%	74.2%	45.0%	50.7%	74.2%	80.1%	44.6%	59.1%
2001	75.0%	76.0%	49.4%	52.1%	74.3%	80.6%	52.4%	60.4%
2002	78.7%	76.5%	54.0%	51.8%	79.1%	80.0%	56.7%	60.5%

Notes: 'Actual' take-up rates are take-up rates of the WFTC-entitled sample in the relevant year. 'Predicted' take-up rates are out-of-sample predictions of take-up for the WFTC-entitled sample in the relevant year, based on an FC-only probit which includes entitlement as a regressor and allows for a linear time trend, as described in the text. Analysed sample restricted as described in Section 3.

Table 12. Actual and predicted take-up rates of WFTC controlling for entitlement: alternative time trend assumptions in the FRS

	Lone parents				Couples			
	Actual	Predicted: linear time trend (baseline)	Predicted: no time trend	Predicted: time trend stops in 1999	Actual	Predicted: linear time trend (baseline)	Predicted: no time trend	Predicted: time trend stops in 1999
2000	71.0%	74.2%	72.2%	73.5%	45.0%	50.7%	46.7%	49.4%
2001	75.0%	76.0%	73.4%	74.7%	49.4%	52.1%	46.8%	49.6%
2002	78.7%	76.5%	73.3%	74.6%	54.0%	51.8%	45.1%	53.9%

Notes: 'Actual' take-up rates are take-up rates of the WFTC-entitled sample in the relevant year. 'Predicted' take-up rates are out-of-sample predictions of take-up for the WFTC-entitled sample in the relevant year, based on an FC-only probit which includes entitlement as a regressor, as described in the text. Analysed sample restricted as described in Section 3.

However, this negative effect of the WFTC reform on take-up faded over time. For both lone parents and couples, take-up in 2001 and 2002 was higher than we would have predicted from previous years of WFTC, and by 2002 take-up was almost as high as (in FACS) or even slightly higher than (in the FRS) we would have predicted under FC, as shown in Table 11.

This qualitative result is robust to our three assumptions about the time trend, but, as Table 12 shows, the extent to which actual take-up of WFTC in 2002 exceeds what we would have predicted had FC not been abolished does depend on whether we restrict there to be no time trend, or force it to stop in 1999.

Thus, over the medium term, the general increase in entitlement under WFTC did increase take-up by broadly what we would have expected given the pattern of take-up under Family Credit. But there is little evidence here that replacing FC with WFTC persuaded any more people to claim WFTC by the end of its life-span than would have claimed FC had it simply been made more generous.

7. Knowledge and attitudes in FACS

To understand the causes of non-take-up, it is desirable to investigate the relationship between take-up and families' knowledge of, and attitudes towards, WFTC and state support in general. FACS is particularly useful in this regard because it specifically asks respondents (and in some cases their partners) about their knowledge and attitudes. This Section reports the results of including these extra explanatory factors in the regressions.

We run the same probits as in Section 5, but with some additional regressors:

- We include a set of indicators for how strongly the respondent agreed or disagreed with the statement that "people with jobs should not get social security benefits", and a corresponding set of indicators for the partner in couples. Respondents were not asked about their attitudes to tax credits, so we do not know if tax credits were perceived differently.

- Respondents (but not partners) were asked what they thought the maximum level of net earnings was at which their family could receive FC/WFTC. We include indicators for their getting this right (within 5%), overestimating it, and underestimating it; we include a separate indicator for their guessing a run-out point below their actual income (which is necessarily an underestimate of the run-out point, because only people who we think are entitled appear in our sample).

- In 1999, respondents (not partners) were also asked whether they had heard anything about the forthcoming change to WFTC; we include an indicator for this in subsequent years.

- Respondents (and partners where appropriate) in non-recipient families were asked to name the in-work support programme (ie either “Family Credit” or “Working Families’ Tax Credit”) from a description. Since the question was only asked of non-recipients, we include this indicator in regressions describing take-up in the following year. In other words, we add a set of indicators for whether, in the previous wave, the respondent was

    - not present in the survey
    - present and receiving FC/WFTC
    - not receiving but could name it
    - or not receiving and could not name it (and for non-recipient couples, indicators for whether the partner could name it).

  Note that this necessitates including an indicator for lagged receipt, which we do with some reluctance since we have not specified a full dynamic model; unsurprisingly, the coefficient on this variable is always large and significant.

Including these additional regressors changes the estimated marginal effects of the other variables. However, this is less a result of including knowledge or attitudinal variables than of including an indicator for lagged receipt. Since, as mentioned above, the inclusion of lagged receipt is rather suspect without a proper dynamic model, we do not attach much weight to these changes in coefficients. We are more interested in the coefficients on the knowledge and attitudinal variables themselves, and our key findings are as follows.

First, awareness in summer 1999 of the forthcoming replacement of FC by WFTC had no significant effect on the probability that an eligible family would claim in any year.

Secondly, non-recipient lone parents who could correctly name FC/WFTC from a description were 10 percentage points more likely to take up in the following year, if entitled, than those who did not know the name. Non-recipient couples in which the woman could name FC/WFTC were 12 percentage points more likely to take it up in the following year; the man's ability to name it had no significant impact on take-up: although the coefficient on this was large, the associated standard error was too large for the estimate to be significant. Experimenting with interactions between whether the man and women knew the name resulted in a set of coefficients all estimated with large standard errors.

Thirdly, there was no significant difference in take-up between those who accurately guessed the income level at which FC/WFTC ran out, those who overestimated it, and those who underestimated it. However, people who were entitled to FC/WFTC but who incorrectly thought that the run-out point was below their actual income – meaning that they considered themselves not to be entitled – were much less likely to claim than those who guessed accurately: 25 percentage points less likely in the case of lone parents, 18 percentage points less likely in the case of couples.

Finally, lone parents who strongly approved of workers' receiving benefits were 10 percentage points more likely to take up than those who were uncertain; those who strongly disapproved were not significantly different from either. In couples, the view of the man had no significant impact on take-up. Couples in which the woman strongly approved of in-work benefits were 11 percentage points more likely to claim than if the woman was uncertain, and also 9 percentage points more likely than if she

only weakly approved and 15 percentage points more likely than if she weakly disapproved; as with lone parents, couples in which the woman strongly opposed in-work benefits were not significantly different from any other group.

It is important to remember, though, that regressions models can only ever estimate correlations between variables, and that the coefficients do not necessarily represent causal relationships. Of course, the main reason for including variables about families' attitudes towards and knowledge of in-work support programmes in a take-up regression is to investigate the causal link from attitudes towards and knowledge of in-work support programmes to the decision to take-up FC/WFTC. But whether one approves of in-work benefits, and how well one knows the FC/WFTC run-out point, are both endogenous to the take-up decision, because applying for and receiving FC/WFTC will probably directly influence claimants' knowledge of and attitudes towards in-work support. This means that the coefficients we estimate for these two factors will reflect not just the causal link from attitudes and knowledge of in-work support programmes to the take-up decision (the coefficient of interest to us), but also the causal link from the take-up decision to attitudes and knowledge of in-work support programmes (the other two knowledge variables we include – awareness in 1999 of the forthcoming reform and non-recipients' being able to name the in-work support programme – cannot exhibit reverse causation, however, since the variables relate to knowledge prior to the take-up decision in question).

It is likely that the negative relationship between guessing an FC/WFTC run-out point below actual family income and receiving FC/WFTC occurs partly because those who think they are not entitled consider there to be no point in applying, but also because those who have successfully applied for FC/WFTC can presumably deduce that the

cut-off point is greater than their own income. Similarly, the relationship between attitudes towards in-work benefits and receiving FC/WFTC might arise both because those who think that workers should not receive benefits might chose not to apply for in-work support, and because the act of receiving FC/WFTC might make families feel more positively about in-work benefits. Because of this endogeneity (or reverse causation), all that we can say is that the coefficients that we have estimated on these endogenous variables are upper bounds on the magnitudes of the effects of interest to us (the causal effects of attitudes and knowledge of in-work support programmes on the decision to claim FC/WFTC).

8. Panel analysis of FACS

The probits in Section 5 simply pool all FACS observations into a single regression for lone parents and one for couples. Year indicators are included, but the fact that some of the observations from different years are in fact the same families observed more than once is recognized only in the calculation of standard errors, not in the estimation of coefficients. This means that our estimates are at best inefficient, and at worst biased and inconsistent, if there is unobserved family-specific heterogeneity influencing take-up.

A more general model for the take-up of FC/WFTC is given by:

$$I_{it} = X_{it}\beta + f_i + \varepsilon_{it}$$

where i and t index families and years respectively and f_i is the part of family i's unobserved taste for claiming FC/WFTC which is constant over time.

All the models estimated so far in this paper assume that $f_i = 0$ for all families, so that of two families with the same observed characteristics, the family more likely to claim in one year is no more likely to claim in the next year. If f_i is non-zero but uncorrelated with the observed explanatory variables, then estimation which ignores it will produce consistent (albeit inefficient) estimates of β, but the associated standard errors will be underestimated; accordingly, in Sections 4 to 6 we calculate standard errors for FACS which are robust to clustering on family (see footnote 15), although no such correction can be made for the FRS since the adjustment itself requires panel data. If, however, f_i is correlated with the observed explanatory variables, then all the coefficients reported in Sections 4 to 7 are estimated inconsistently and should be considered unreliable: we may be wrongly attributing variation in take-up to particular family characteristics when in fact families who are likely to take up for other reasons (indicated by a high f_i) just tend to have those characteristics as well.

It is possible to allow for such family fixed effects in the estimation of coefficients. A fixed-effects logit exploits the within-family variation provided by panel data to estimate the coefficients: using only families whose take-up behaviour changes over time, it relates these changes in take-up to changes in the families' observed characteristics, thus stripping out the unchanging effect of each family's unobserved propensity to claim regardless of any correlation with observed variables.[19]

Relying exclusively on changes over time within families avoids the problem of unobserved family characteristics influencing take-up, so it is extremely robust. On the other hand, restricting attention to families whose take-up behaviour changes over time, and explaining take-up behaviour only by reference to changes over time in

[19] The fixed-effects logit model is due to Chamberlain (1980). For details see eg Baltagi (2001) or Greene (2003).

characteristics, means ignoring altogether the relationship between characteristics and take-up behaviour *across* families. Discarding the data on variation across families in this way means that the technique is unnecessarily inefficient in the absence of fixed effects. A further disadvantage of the fixed-effects methodology in a binary choice setting is that the estimated coefficients cannot be translated into marginal effects. Thus the fixed-effects logit can tell us which of the explanatory variables that change over time are statistically significant (and with what signs) under more parsimonious assumptions than before, but it cannot tell us the magnitude of their effects on take-up.

Table 13 for lone parents and Table 14 for couples therefore show which regressors are significant and with what signs in a simple logit with all years pooled and in a fixed-effects logit. We use a logit rather than a probit specification for the pooled regression here to be consistent with the fixed-effects regression, which can only be easily estimated (without bias) as a logit. The difference is that the error term ε_{it} is assumed to have a logistic, rather than a normal, distribution. In practice, the choice of probit or logit specification makes little difference to the results. Note that the set of explanatory variables used here is different from that used in Section 3. This is because the fixed-effects logit can estimate coefficients only for regressors which vary enough over time; while regressors with no variation at all are dropped automatically from the regression and pose no problem, regressors with slight variation over time (such as a London indicator) must be removed 'artificially'.

Because of the relative inefficiency of the fixed-effects logit, we find very few significant explanatory variables. One results is clear, however: log entitlement remains significant at the 1% level throughout for both lone parents and couples,

meaning that increases in entitlement are unequivocally associated with increases in the probability of take-up.

Table 13. Pooled-years and fixed-effects logit results: lone parents, FACS

Variable	Pooled logit (Sample size: 2707 observations from 1481 families)	Fixed-effects logit (Sample size: 567 observations from 201 families)
D. 2000		
D. 2001		
D. 2002	+ + +	+ +
No. of dependent children		
- 2000 interaction		
- 2001 interaction		
- 2002 interaction	– – –	– –
D. child aged 0-4		
D. adult non-partner in HH		
- 2000 interaction		
- 2001 interaction		
- 2002 interaction		
Age	– – –	
D. social renter	+ +	
D. private renter	+ +	
D. other/unknown tenure		
D. receiving maintenance	+ +	
Log earnings		
Log entitlement	+ + +	+ + +
D. white		n/a
D. male	– – –	n/a
D. in least deprived decile in any year	– – –	n/a
D. left education aged 17-18	– – –	n/a
D. left education aged >18	– – –	n/a
Constant	– –	n/a

Note: +, + + and + + + denote positive effects significant at the 10%, 5% and 1% levels respectively; –, – – and – – – similarly denote negative significance. Variables marked n/a were dropped from the fixed-effects logit as they did not vary over time. Analysed sample restricted as described in Section 3; fixed-effects logit also removes all observations with no change in take-up behaviour over time.

Table 14. Pooled-years and fixed-effect logit results: couples, FACS

Variable	Pooled logit (Sample size: 2677 observations from 1661 families)	Fixed-effects logit (Sample size: 542 observations from 202 families)
D. 2000	– – –	–
D. 2001	– – –	
D. 2002	– – –	
No. of dependent children	– –	
- 2000 interaction		+
- 2001 interaction	+ + +	+ + +
- 2002 interaction	+ + +	
D. child aged 0-4	+ + +	
D. social renter	+ + +	
D. private renter	+ + +	+
D. other/unknown tenure		
D. two-earner couple		– –
D. only female working	– –	
Male job tenure (months)		
Female job tenure (months)	– – –	
D. female earnings greater than male	– – –	
Log earnings		
Log entitlement	+ + +	+ + +
D. white		n/a
D. in least deprived decile in any year	– – –	n/a
D. male left education aged 17-18		n/a
D. male left education aged >18	– – –	n/a
D. male left education age unknown	– – –	n/a
D. female left education aged 17-18		n/a
D. female left education aged >18	– – –	n/a
Constant	–	n/a

Note: +, + + and + + + denote positive effects significant at the 10%, 5% and 1% levels respectively; –, – – and – – – similarly denote negative significance. Variables marked n/a were dropped from the fixed-effects logit as they did not vary over time. Omitted category has only the male working. Analysed sample restricted as described in Section 3; fixed-effects logit also removes all observations with no change in take-up behaviour over time.

We can test the assumption made in the pooled logit that there are no family fixed effects influencing take-up, by testing whether the coefficients from the pooled logit (which is inconsistent if there are fixed effects) are equal to those from the fixed-effects logit (which is consistent whether or not there are fixed effects but less efficient than the pooled logit if there aren't).[20] For lone parents we cannot reject the hypothesis that the coefficients are equal (a p-value of 0.927), meaning that there is no

[20] This is Hausman's (1978) test: see eg Baltagi (2001) or Greene (2003) for more details.

such heterogeneity and the more efficient pooled logit can be used. However, for couples the test produces a p-value of 0.002, confirming the existence of family fixed effects. That being the case, for couples we should rely only on the results from the fixed-effects logit shown in Table 14, not on the results described in Sections 4 to 7.

9. Conclusion

The panel analysis of Section 8 has important implications for our study of take-up behaviour. It shows that we can actually draw very few conclusions about couples' take-up behaviour, the main one being that increasing entitlement levels definitely increases the probability of take-up. However, it also validates the findings of earlier sections about the take-up behaviour of lone parents, which are summarised below.

Lone parents were less likely to claim if they had more years of education, were home-owners, or were lone fathers; earnings, however, had no discernible effect. Entitlement was an important determinant of take-up: for lone parents with the median probability of take-up, a 10 per cent increase in entitlement increased the probability of take-up by between 1.5 and 2.1 percentage points under Family Credit and by between 0.7 and 1.3 percentage points under WFTC. We attempted to examine the effect of childcare support separately, but could neither reject the hypothesis that childcare support was ignored entirely in the take-up decision nor reject the hypothesis that it was fully taken into account.

The greater generosity of WFTC relative to FC meant that take-up of WFTC was higher than we would have expected had FC simply continued unchanged. When we control for rising entitlement (and other observed explanatory factors) to isolate the

other aspects of the reform, we find that the introduction of WFTC was associated with a fall in lone parents' take-up rate of about 3-6 percentage points, assuming that take-up of FC would have continued to rise at the rate it had been. This is true even among lone parents who would have been entitled to FC. However, by 2002, take-up had recovered to the level we would have expected under Family Credit. Using alternative assumptions for the trend in FC take-up reduces, but does not negate, the estimated fall in the first year of WFTC, and means that by 2002 WFTC take-up was above the level we would have expected under FC. The change from 2000 to 2002 is presumably due to awareness of WFTC spreading and/or any stigma associated with claiming it falling: our data are not directly informative about the precise causes.

We find quantitative evidence that awareness of in-work support programmes did influence take-up of WFTC: non-recipients were 10 percentage points more likely to claim in the following year (should they become entitled) if they could name FC/WFTC from a description, although awareness in 1999 of the imminent reform to in-work support did not seem to matter to take-up at any point between 1999 and 2002. We also found that take-up was strongly correlated with attitudes towards in-work benefits, and with placing the run-out point below the person's actual income, but in these cases, there is a strong possibility of reverse causation.

These findings are broadly consonant with those of Brewer et al (2005), which takes a more restrictive structural approach and models take-up jointly with labour supply. That paper finds that the utility cost of claiming in-work support initially rose when WFTC was introduced but then fell back to the same level as (for couples) or slightly lower than (for lone parents) under FC.

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