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# A MICROECONOMETRIC ANALYSIS OF THE TAKE-UP OF INCOME SUPPORT IN BRITAIN 

ROBERT A. CRENIAN

A thesis submitted to the University of Bristol in accordance with the requirements of the degree of Ph.D. in the Faculty of Social Sciences, Department of Economics.


#### Abstract

This thesis deals with the take-up of social security benefits in Britain. It is well documented that not everyone who is entitled to benefits actually claims them. Non-take-up of benefits has been found to be a problem especially for benefits which are means-tested. So, throughout this thesis, we concentrate on Income Support, the main means-tested benefit in Britain. The latest official estimates on the extent of non-takeup (for 1993/94) suggest that up to 1.4 million persons are not receiving close to $£ 1.7$ billion of IS in spite of being entitled to it.

The main question this thesis addresses is what are the factors which determine whether an individual will or will not take-up their benefit entitlement? We consider the problem from an economic perspective by constructing suitable models set in both static and dynamic environments. These models provide some interesting insights about the nature of non-take-up. In turn, they also form the basis to a series of econometric models. Previous empirical evidence has shown that the entitlement level itself is one of the key determinants of whether or not an individual will take-up. In addition, it has long been recognized that - due to the complex nature of the benefit system - determining individual entitlements is, in many cases, error-prone with resulting benefit entitlements that are subject to measurement error. Hence, unlike any other studies thus far, we account for the presence of measurement error in the benefit entitlement when modelling the likelihood of take-up. Finally, we shed new light on the dynamics of take-up by using the information contained in our panel data set. In particular, we consider the effect claiming in the past has on the current decision to take-up and how future changes, expected or known with certainty, influence the decision to take-up or not.


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## Author's Declaration

I hereby declare that the work presented in this thesis was carried out in the Department of Economics at the University of Bristol and is entirely my own.

The views expressed in this thesis are those of the author and not of the University of Bristol.


Robert A Crenian

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## Preface and Outline

This thesis deals with the take-up of social security benefits in Britain. It is well documented that not everyone who is entitled to benefits actually claims them. Non-take-up of benefits has been found to be a problem especially for benefits which are means-tested. So, throughout this thesis, we concentrate on Income Support (IS), the main means-tested benefit in Britain. The latest official estimates on the extent of IS non-take-up (for a twelve month period between 1993/94) suggest that up to 1.4 million persons are not receiving close to $£ 1.7$ billion of in spite of being entitled to it.

The main question this thesis addresses is what are the factors which determine whether an individual will or will not take-up their benefit entitlement? We consider the problem from an economic perspective by constructing suitable models set in both static and dynamic environments. These models provide some interesting insights about the nature of non-take-up. In turn, they also form the basis to a series of econometric models. Previous empirical evidence has shown that the entitlement level itself is one of the key determinants of whether or not an individual will take-up. In addition, it has long been recognized that - due to the complex nature of the benefit system - determining individual entitlements is, in many cases, error-prone with resulting benefit entitlements that are subject to measurement error. Hence, unlike previous studies of take-up, we account for the presence of measurement error in the benefit entitlement when modelling the likelihood of take-up. Finally, we shed new light on the dynamics of take-up by using the information contained in our panel data set. In particular, we consider the effect claiming in the past has on the current decision to take-up and how future changes, expected or known with certainty, influence the decision to take-up or not.

The general outline of this thesis is described briefly in the following. Chapter 1 is relatively short, examining recent trends in IS dependency in Britain. The Social

Security Act of 1986 (which came into effect in April 1988) replaced Supplementary Benefit with IS, an apparently simpler benefit to administer and legislate. We focus on IS and follow the trends in IS recipiency over the relatively short time period since its inception, 1988 to 1994/95 (the most recent figures). Non-take-up of benefits such as IS is often cited as one of the main short-comings and inefficiencies of meanstesting (see, for example, Atkinson (1984)). Thus, it is important to consider, first and foremost, the extent of means-tested benefits in Britain. As such, this chapter provides a backdrop to all subsequent chapters of this thesis.

In Chapter 2 we address the construction of a data set containing all individuals entitled to IS. Such a data set lies at the heart of all future analysis into the take-up of benefits. Our sample of entitled individuals is drawn from the British Household Panel Survey which contains information on benefits and, in particular, on whether or not a respondent is receiving IS at the time of being interviewed. However, for the analysis of take-up this information is necessary but not sufficient. The data required for a take-up analysis must contain not only all individuals currently receiving IS but also all those entitled to IS but currently not in receipt. In order to deduce the latter of these we must compute for each individual in the British Household Panel Survey who is eligible for IS an appropriate entitlement. Chapter 2 describes this process and the resulting output obtained from the IS algorithm.

Next, Chapter 3 turns to the economics of take-up by outlining a basic microeconomic model of the decision to take-up or not to take-up a means-tested benefit. We begin by considering a simple one-period static model with von NeumanMorgenstern uncertainty about the outcome of a claim. Thereafter we construct two types of simple dynamic models in which the current decision to take-up or not is affected by (i) any past experience of claiming the benefit in question, and (ii) expectations about future financial circumstances. Both the static and the dynamic models constructed form the economic backbone to the subsequent econometric analysis in the ensuing chapters 4 and 5 . In particular, Chapter 4 deals with the estimation of binary choice models (notably logit models) where one of the explanatory variables is subject to measurement error. The computation of IS entitlements as described in Chapter 2 is, to some degree, somewhat error-prone, so that it makes sense to incorporate this scope for measurement error into our model of
take-up. Our interest lies in the factors which determine whether an individual will take-up so that an appropriate model would be either a univariate logit or probit model. As will be seen such models are complicated when we can no longer assume that all the covariates are accurate measures of what they are supposed to measure. When at least one of these covariates is measured with error (in our case the IS entitlement) the simple logit/probit model no longer produces consistent estimates of the parameters in the model. The methods proposed in this chapter attempt to overcome this inconsistency.

Finally, Chapter 5 examines some dynamic take-up evidence from the first four waves (A to D) of the British Household Panel Survey, spanning the years 19911994. The main objective of this analysis is find out what happens to non-pensioners entitled to IS as time passes. In particular, our interest falls on individuals entitled to IS but not receiving it (ENRs) and how their benefit status and employment status changes from one wave to another. We are able to follow individuals who are found to be entitled in the first wave through to waves two, three and four. Doing so enables us to gain more than a snap-shot vision provided by a single cross-section. Similarly, we are able to follow all entitled individuals in wave two to waves three and four, and all entitled individuals in wave three through to wave four. The overall representation gained provides a detailed picture of the dynamics of take-up amongst individuals entitled to IS who are not receiving it. We particularly concentrate on changes in employment status and individuals' subjective financial assessments. In addition, we are able to make use of panel data to account for heterogeneity in modelling the decision to take-up IS. In this chapter we apply a random effects probit model and test for the presence of individual heterogeneity. We also test dynamic take-up models in which the current take-up decision is affected firstly, by an individual's past claiming experience, and secondly, by future expectations of employment events.

So, to briefly sum up, this thesis extends current research into take-up in three substantial ways. Firstly, we draw on a new data set (the BHPS) and show how this data set can be utilised in order to yield a meaningful take-up analysis. Secondly, we account for the fact that computed IS entitlements are subject to measurement error which, in turn, affects a statistical analysis of the determinants of take-up. Finally, we
exploit fully the longitudinal aspects of the BHPS by considering the dynamic aspects involved in the take-up of IS.

## CHAPTER 1

## TRENDS IN INCOME SUPPORT DEPENDENCY IN BRITAIN

### 1.1 Introduction

This short chapter examines recent trends in Income Support (IS) dependency in Britain. We continue the descriptive analysis provided by Bradshaw and Huby (1989) on Supplementary Benefit (SB) dependency from 1961 to 1986. The Social Security Act of 1986 (which came into effect in April 1988) replaced SB with IS, an apparently simpler benefit to administer and legislate. Together with Family Credit (FC) and Housing Benefit (HB), IS forms one of the main means-tested benefits in Britain. ${ }^{1}$ Here we focus on IS and follow the trends in IS recipiency over the relatively short time period since its inception, 1988 to 1994/95 (the most recent figures). Although this thesis concentrates primarily on persons who are entitled to IS but who decide, for some particular reason(s), not to take-up their entitlement, it is of interest to consider the wider picture of IS dependency. Non-take-up of benefits such as IS is often cited as one of the main short-comings and inefficiencies of means-testing (see, for example, Atkinson (1984)). Thus, it is important to consider, first and foremost, the extent of means-tested benefits in Britain. As such, this chapter provides a backdrop to all subsequent chapters of this thesis.

Thus far little attention has fallen on IS recipiency trends. Much of this lack of interest is no doubt related to the fact that only short time series are currently available. Nevertheless, concern about IS dependency and, more generally, on benefit dependency as a whole, is very high on the political agenda of many European

[^0]countries. ${ }^{2}$ The recent surge in interest on the welfare state and social security policy stems from both demographic changes (notably the old-age dependency 'crisis') and economic changes (e.g. rising unemployment). However, discussions by economists about the future of social security and the inevitable crisis countries are steering towards have flourished since the late 1970s. For example, the Brookings Institution focused their attention on the social security crisis with a string of related publications aptly named 'The Future of Social Security' (1977), 'Policy Making for Social Security' (1979), and notably Henry Aaron's voluminous output on this topic.

In Britain, the rising cost of the welfare state, and social security in particular, are a permanent source for debate. The social security budget represents the largest share of total government expenditure (currently around 30 percent) with no other constituent part of government expenditure occupying anywhere near as large a part. ${ }^{3}$ Within the social security budget itself, state pensions form the greatest component. Real expenditure on the basic retirement pension, for example, has increased by close to 20 percent in real terms between 1980/81 and 1992/93 (DSS (1993a)). Coupled with the fact that most future forecasts suggest increasing dependency ratios ${ }^{4}$, the future for state pensions in Britain is uncertain to say the least. ${ }^{5}$ Similarly, benefits to non-pensioners have witnessed a plethora of changes since the early 1980s (culminating in the Social Security Act 1986) and, in spite of persistent attempts to 'target' benefits to those in greatest need, have experienced rising real expenditure as well. ${ }^{6}$ Since 1988, IS has been the main means-tested benefit intended for those on low incomes and not in work or working only part-time. It is, as such, the main safety net benefit in Britain (see Atkinson (1991)). As a result, throughout this thesis our emphasis falls primarily on IS.

The structure of this essentially descriptive chapter is as follows: in Section 1.2 we consider the evidence of whether a shift towards greater means testing actually

[^1]occurred throughout the 1980s and early 1990s. This is followed by Section 1.3 where we continue the analysis of Bradshaw and Huby by examining the possible underlying reasons for the recent growth in both IS expenditure and the number of IS recipients. Finally, we turn to the number of IS recipients in the British Household Panel survey (the main data set used throughout this thesis) in Section 1.4 and conclude the chapter in Section 1.5.

### 1.2 A Move Towards Greater Means-Testing?

The Social Security Act 1986 proposed major changes to the social security system based on three main objectives. ${ }^{7}$ One of these primary aims of was to shift benefits to those in greatest need thereby reducing the tax burden and improving work incentives ${ }^{8}$ :
> "the Government believe that resources must be directed more effectively to those areas of greatest need. ... we do not accept that a greater spread of universal benefits is right. If we want to see the money we spend on social security spent to better effect, we must accept that this involves some redistribution between different groups of people. ... We must target the resources we have more effectively." (DHSS (1985, p. 18))

This fundamental objective is still at the forefront of social security policy to this day, although more sharply focused than before:
> "One of the major aims of the Department [of Social Security] is to direct resources to areas of greatest need, in particular, low income families with children." (DSS (1996, p. 38))

The idea behind this section is not to discuss the advantages and pitfalls of targeting (particularly in the form of means-testing benefits) or the relative merits of means-testing versus universal benefit provision but, instead, to examine the evidence over the last one and a half decades as to whether a shift towards greater means-

[^2]testing actually occurred. ${ }^{9}$ To reiterate, since our underlying theme is the take-up or non-take-up of benefits (i.e. one part of the efficacy of means tested benefits), it is important to gain an understanding of the extent of means-testing in Britain. ${ }^{10}$

Social security payments taken as whole have increased in both real terms and as a proportion of GDP over the period 1981 to 1995 as shown in Figure 1.1. Real expenditure (in 1994/95 prices) has increased from $£ 56.7$ billion in 1981/82 to $£ 89.2$ billion in 1994/95, an increase of 57.3 percent. (Viewed from another perspective, the real social security budget cost roughly £978 per capita (1994/95 prices) in 1981/82 and rose to roughly $£ 1,593$ per capita in 1994/95.) Yet it does appear that the period immediately after the 1988 social security reforms actually reduced real social security spending. However, this dip in the social security budget between 1986/87 and the early 1990s is most likely due to the fall in unemployment, which peaked at about 12 percent of the total labour force in 1985 and 1986 and fell gradually to around 4.5 percent by 1990 .

Figure 1.1 Social Security Expenditure, 1981/2-1994/5


Source: HM Treasury (1996).
Note: Real $£$ billion at 1994/95 prices.

[^3]In Figure 1.2 we concentrate on the proportionate expenditure on means-tested benefits. We consider only the three main means-tested benefits SB/IS, FIS/FC and HB. ${ }^{11}$ The pattern which emerges from Figure 1.2 is very similar to that of Figure 1.1. Particularly since the social security reforms came into force (April 1988), meanstested benefits have rapidly taken a greater portion of the total social security budget. However, significant shifts towards means-testing of benefits appears to have occurred prior to the reforms as well. ${ }^{12}$

Figure 1.2 Expenditure on Means-Tested Benefits as Percentage of Total
Benefit Expenditure, 1981/2-1994/5


Source: DSS (1993a) and HM Treasury (1996).
Note: The three main means-tested benefits (MTB) considered are SB/IS, FIS/FC and HB.

The shift towards greater means-testing also becomes apparent when we consider the total number of recipients of the main means-tested benefits. In Table 1.1 we consider first real expenditure on the total social security budget (including and excluding pensioners respectively) and real expenditure on means-tested benefits in

[^4]order to obtain a comparative picture. The growth in expenditure on means-tested benefits is more than three times that on the total social security budget. Especially striking is the relatively large growth rate for FIS/FC. Although not shown here, most of this growth occurred after 1988 and was part of the government's policy of using the new benefit FC to target low income families with children.

Table 1.1 The Relative Growth of Means Tested Benefits

|  | Real Expenditure ( $£$ mill. $1994 / 95$ prices) |  |  |
| :--- | :---: | :---: | :---: |
|  | $1980 / 81$ | $1994 / 95$ | \% Growth 80/81-94/95 |
| Total Social Security | 48,851 | 88,787 | 81.8 |
| All Non-Cont. Benefits $\dagger$ | 16,817 | 47,735 | 183.8 |
| All Means-Tested $\ddagger$ | 8,258 | 29,215 | 255.6 |
| SB/IS | 4,668 | 16,684 | 257.4 |
| FIS/FC | 91 | 1,683 | $1,749.5$ |
| HB | 3,499 | 10,484 | 199.6 |

Total Receipts ('000s)

|  | 1980 | 1995 | \% Growth 80-95 |
| :--- | :---: | :---: | :---: |
| Total Social Security * | $33,149.8$ | $41,539.1$ | 25.3 |
| Excluding Pensioners * | $24,178.8$ | $31,250.1$ | 29.2 |
| All Means-Tested $\ddagger^{*}$ | 6,760 | 11,032 | 63.2 |
| SB/IS | 3,118 | 5,670 | 81.8 |
| FIS/FC | 86 | 602 | 600.0 |
| HB | 3,556 | 4,760 | 33.9 |

$\dagger$ All Non-Contributory Benefits are considered to remove the rising burden of state pensions.
$\ddagger$ Total of SB/IS, FIS/FC and HB.

* Figures include multiple counts (e.g. IS and FC for same person counted twice in total).

A similar picture emerges when we consider the total number of benefit recipients. The total number of means-tested benefit recipients has grown by more than twice as much than all social security recipients between 1980 and 1995. Particularly notable is the expansion in the number of recipients of FIS/FC and SB/IS over this time period. The figures in Table 1.1 do not, however, take account of the fact that there may be multiple receipts for a single individual or head of household claiming for a family. Hence the total figures are somewhat misleading, in the sense
that amongst the 41.5 million receipts of benefit in 1995, for example, there may well be considerably less recipients.

Finally, in Figure 1.3 we give the number of means-tested benefit recipients by benefit from 1983 to $1995 .{ }^{13}$ The figures for HB between 1983 and 1988 are rate rebate recipients which are not directly comparable with the HB scheme from 1988 onwards. Interestingly, we note that the rise in means-tested benefit recipients really only began after 1990. However, since then the total number of means-tested benefit recipients has risen quite considerably.

Figure 1.3 Total Number of Main Means-Tested Benefit Recipients, 1983-1995


Source: Social Security Statistics, DSS (Various Years).
Note: Number of recipients are estimated at a point in time of each year. Graph includes multiple counts (e.g. one benefit unit receiving all three benefits will be counted three times).

So, based on the above evidence, collected in the first place from official sources, there is considerable evidence that greater targeting in the form of meanstesting did in fact occur not only after 1988 but all throughout the 1980s.

[^5]
### 1.3 Influencing Factors

Henceforth we concentrate solely on IS dependency from 1988 to 1995, thus following the work of Bradshaw and Huby cited above. We begin this section by considering the growth in the number of IS recipients by main population groups in Figure 1.4. We follow Bradshaw and Huby by considering four distinct groups consisting of pensioners, the unemployed, lone parents, and all others. Bradshaw and Huby found SB recipient numbers to increase rapidly for all four groups, particularly from 1979 onwards (their analysis ended in 1986).

Figure 1.4 Recipients of IS by Main Group, 1988-1995


Source: Social Security Statistics, DSS (Various Years).

From Figure 1.4 it becomes apparent that this trend no longer holds for IS. Although the unemployed, lone parents and all others experience growing numbers of recipients, the number of pensioners in receipt of IS has remained relatively unchanged throughout the time period considered. The changes in recipient numbers over time are, moreover, quite different for each of these groups: over the period 1988 to 1995 , the average rate of growth in recipient numbers is 0.6 percent for pensioners, 2.9 percent for the unemployed, 6.2 percent for lone parents, and 15.7 percent for all others. Although pensioners and the unemployed still make up the bulk of all
recipients by 1995, the number of lone parent recipients and those recipients falling into the group of all others have been rising continuously. The three main groups (pensioners, the unemployed, and lone parents) made up 90 percent of all IS recipients in 1988 but by 1995 this proportion has fallen to 80 percent of all IS recipients.

The growing number of recipients in the group of all others is primarily due to the increasing number of sick and disabled IS recipients. Government policy since 1988 has deliberately targeted benefits on the sick and disabled, notably with the Disability Living Allowance (DLA) and Disability Working Allowance (DWA) Act of 1991. The benefits introduced with this Act, DLA and DWA, are meant for the disabled who are able to work but who have limited earnings power (for details see Dilnot and Webb (1989)). Furthermore receipt of these benefits entitles recipients to the disability premium of IS thereby perhaps increasing the number of disabled who are entitled to IS.

We now consider the three main population groups in turn and discuss the possible factors which may have influenced the recipiency patterns and trends between 1988 and 1995 outlined above. Four main influences can be considered: (i) financial, (ii) demographic, (iii) economic, and (iv) policy changes. However, one of the major problems here is that the time series we work with are rather short, and consequently some influences will be difficult to detect (for instance, demographic changes).

## Pensioners

There has been little change in the number of pensioner IS recipients over the time period considered. Hence we would expect few changes amongst the influencing factors outlined above.

On the financial side, we consider the level of the basic NI pension, the basic pensioner IS entitlement, and the level of occupational/private pensions. Changes in these may affect the number of IS recipients if the entitlement rates of the latter change in an opposing direction. Bradshaw and Huby, for example, found that the basic state pension has fallen below the SB level since 1966. In Figure 1.5 we consider the basic state pension and the basic IS entitlement for singles and married couples (adult allowance and under 80 pensioner premium) where a similar picture
emerges. However, in recent years both the basic state pension and basic IS allowances are price-indexed and thus the difference between both levels is quite minimal, even though in recent years the discrepancy has been widening slightly.

Figure 1.5 Rates of NI Retirement Pension and IS Personal Allowance, 1988-95


Source: Social Security Statistics, DSS (Various Years).
Note: Basic Pension $=$ Contributory state pension for age under 80. IS Allowance $=$ Basic pensioner allowance with no additional premiums.

This brings us to the second point: the value of the basic state pension relative to net average earnings has declined since the early 1980s. This results directly from a significant government policy change which abolished earnings-indexation in favour of price-indexation. ${ }^{14}$ As Johnson and Stears (1995) point out, the value of the basic state pension was about 20 percent of average net (male) earnings in 1980 but had fallen to 16 percent by 1995 (based on data from the Family Expenditure Survey (FES)). We might therefore expect a greater shift towards dependence on IS which did not, however, occur. One of the reasons might be the rising proportion of pensioners with income from private pensions. Based on FES data for 1992/93, 99 percent of all pensioners had some income from social security (excluding Housing Benefit) and 57 percent had income from private pensions (Johnson and Sears (1995, p. 73)). However, private pension income is strongly negatively skewed, with the

[^6]poorest 20 to 30 percent of the pensioner population receiving only very small proportions of their total income from private pensions.

To reiterate, we must bear in mind that many of the financial changes for pensioners will take time to bring about noticeable changes in behaviour. As such, our time series are perhaps too short to capture any significant changes. This argument applies particularly to demographic changes. The total number of pensioners increased from 10.18 million in 1988 to 10.38 million in 1994 (latest figures), an increase of about 2 percent. The average rate of growth in the pensioner population is about 0.3 percent between 1988 and 1994, and thus only slightly less than the rate of growth in the number of pensioner IS recipients. So, overall, few notable changes occurred throughout the relatively short time period under consideration, reflected in the only minor increase in pensioner IS recipients.

## The Unemployed

The major influence on the number of unemployed IS recipients is likely to be the total number of unemployed individuals. From Figure 1.6 we can clearly see how the number of unemployed IS recipients follows the shape of the total number of unemployed individuals. A further interesting aspect of Figure 1.6 is the large proportion of the unemployed who rely solely on IS. The proportion of the unemployed who rely on contributory Unemployment Benefit (UB) is rather low and still declining, from about 25 percent of all the unemployed in 1988 to around 20 percent in 1994. In contrast, in the early 1960s close to 85 percent of all the unemployed were receiving UB (at that time, however, the number of unemployed did not exceed the half a million mark). Recent changes to UB have renamed it Jobseeker's Allowance, a combined benefit covering both UB and IS to the unemployed. Benefit is payable for only up to 6 months and thus, inevitably, a larger number of long-term unemployed are likely be forced onto IS.

We do not consider, in any depth, financial influences on the changing number of unemployed IS recipients. Relativities in entitlement levels for IS and UB are likely to have changed insignificantly since both are price-indexed. Hence such financial motives are likely to contribute little to explaining the growth in unemployed IS recipients.

Figure 1.6 Total Number of Unemployed and Number of Unemployed in Receipt of IS Only, UB Only, and both IS and UB, 1988-1994


Source: Annual Abstract of Statistics, CSO (Various Years).
Note: U = unemployed, UB = Unemployment Benefit.

A further financial influence might be changes in replacement ratios (i.e. the ratio of total income whilst remaining unemployed to total net income from working). High replacement ratios act as a disincentive to work and thus contribute to the unemployment trap. As noted above, benefit up-ratings are price-indexed rather than earnings-indexed since the late 1980s. The relative performance of these indices are shown in Figure 1.7. We note that average prices exceeded average earnings up to 1988/89, after which the reverse was true until 1993. Since 1993 prices have surpassed earnings once again. So, between 1988 and 1993 one might very well expect decreasing replacement ratios, since the unemployed rely predominantly on benefits. This did in fact occur but primarily as a direct result of policy changes. The 1988 social security reforms attempted to reduce extreme cases of both the unemployment trap and the poverty trap (i.e. both replacement ratios and marginal tax rates of 90 percent and above). Estimates of replacement ratios based on the FES (see DSS (1994a and 1996)), for example, suggest 210,000 benefit units ${ }^{15}$ with replacement ratios of 90 percent and above in 1985 (pre-reform) and a low of 30,000

[^7]benefit units in 1990/91. However, since then, such high replacement ratios have slowly increased again, with 35,000 benefit units in 1995/96 facing replacement ratios of 90 percent and above. Yet, at the lower end of the scale, large numbers still face replacement ratios of 70 percent and above (around half a million throughout the 1990s).

Hence, it appears that the changing number of unemployed IS recipients are mostly driven by the total number of unemployed throughout 1988 to 1995. In view of the fact that roughly 75 percent of all unemployed now rely on IS (solely or in conjunction with UB) the likelihood is that this trend will continue into the future as well.

Figure 1.7 Average RPI vs. Average Earnings, 1980-1995


Source: ONS Economic Trends (1996).
Note: Annual average RPI covers all goods and services. Average earnings are wages and salaries per unit of output for the whole economy.

## Lone Parents

The growing number of lone parents prior to the time period considered here is well documented by the analysis of Bradshaw and Huby. This rise in lone parent IS recipiency continues after the 1988 reforms. One of the obvious driving forces underlying this increase has been the rising number of lone parents (see Milar and Bradshaw (1993)). The most recent estimates for 1991 suggest a total lone parent population of approximately 1.3 million, of which 871,000 were IS recipients. By

1995 the number of lone parent IS recipients has increased to just over 1 million and the DSS predict this number to rise to 1.4 million by 1999/2000. This, in turn, is reflected by the growing number of divorces and the rising number of births outside of marriage. The latest data suggest 215,500 births outside marriage in 1994 and 165,018 divorces in $1993 .{ }^{16}$ The relevant figures for 1988 were 177,400 and 152,633 respectively. So, even over a relatively short time-period of six to seven years, the number of births outside marriage has increased by 21.5 percent and the number of divorces by 8.1 percent.

However, up to 1991 the rate of growth of lone parent IS recipients has been greater than the rate of growth in the lone parent population as a whole, and there is no indication that this trend is likely to be reversed (see DSS (1993a)). Thus, the population trend paints only part of the picture. The increasing dependency on IS by lone parents, and particularly lone mothers (the overwhelming majority of lone parents are female), stems also from a variety of socio-economic changes. ${ }^{17}$ Amongst these are the declining proportion of lone parents who work (particularly full-time), and policy changes which may have increased the number of lone parent IS recipients. For example, Millar and Bradshaw (1993) draw attention to such changes:
"First, successive governments have followed a policy that benefits to lone parents should include some recognition of the extra costs of lone parenthood ... there is a lone-parent premium for those on Income Support. ... Secondly, unlike most other working age benefit claimants, lone parents are not required to register for employment whilst in receipt of Income Support." (Millar and Bradshaw (1993, p.16)).

The latter of these two points, together with the declining rate of Child Support ${ }^{18}$ and more generally child care facilities, have been shown to have significant effects on preventing lone mothers from participating in the labour force (see Bingley et al. (1994)). This is especially the case when the dependent children are young. In

[^8]addition, Ermish and Wright (1991) suggest that better incomes when employed (e.g. by raising levels of One Parent Benefit for example) and stricter enforcements of fathers' maintenance payments could contribute to a gradual decline in the number of lone parent IS recipients.

Thus, it is likely that the steadily rising number of lone parent IS recipients cannot be explained solely by demographic changes. Nor did particularly many policy changes occur, with respect to IS, which contribute to these increasing numbers. The most conceivable explanation lies in the disincentives lone mothers face in joining the labour force. We have briefly cited some such evidence above. Recently much economic work has focused on the issue of how to raise the relatively low living standards of most lone parent families and to reduce their dependency on means-tested benefits such as IS (see, for example, Walker (1990), Jenkins (1992), and Blundell et al. (1992)).

To summarise this section, we note that our descriptive analysis thus far has attempted to highlight some of the causal factors which might underlie the gradual increase in IS recipient numbers. In practice, many of these interact and thus, ideally, an econometric analysis would be helpful. However, due to the relatively short timeseries we are faced with (at most 8 years of annual data), such an exercise would be rather futile. Bradshaw and Huby provide a basic econometric investigation of those factors which determine the number of IS recipients over the somewhat longer time period 1961-1986 with mixed results.

Before we conclude this chapter, we turn briefly to the information on IS recipiency provided by the main data set we will be using throughout the remaining chapters of this thesis, namely the British Household Panel Survey.

### 1.4 IS Recipiency in the British Household Panel Survey

The British Household Panel Survey (BHPS hereafter) is a longitudinal survey of around 10,000 individuals in roughly 5,000 households on an annual basis. The survey began in 1991 (wave A) and it is hoped that it will continue for at least 10 years thus providing a rich new source of information on socio-economic change in

Britain over time. Currently wave D (1994) are the most up-to-date data available and consequently all our ensuing analyses relate to waves A to D of the BHPS.

It is not the aim of this section to describe the BHPS in greater detail. This task is postponed to Chapter 2 of this thesis. ${ }^{19}$ What we aim to do in this short section, is to provide an idea of the number of IS recipients in the BHPS, and how these compare to the official figures on IS recipients by the DSS. It is important to consider this aspect of our data set, since much of the future analysis in this thesis draws inferences from samples drawn from the BHPS.

Like most other data sets used for the purposes of a microeconometric analysis (such as the Family Expenditure Survey (FES) or the General Household Survey (GHS) to name but two), the BHPS is likely to suffer from various survey deficiencies and sampling errors. As a result, certain population groups may be over- or underrepresented in the BHPS. However, since the BHPS is a relatively new data set, research concerning the reliability of the information contained in the BHPS is still at a rudimentary stage. We aim here not to fill this gap by providing a rigorous examination of the BHPS data on IS recipients but attempt to acquire some indication of the general accuracy of the data considered. Clearly, much further work on this aspect of the BHPS is still required. ${ }^{20}$

In contrast, data sets such as the FES have been well-researched over the last ten years. Accordingly, we have a good understanding of the advantages and disadvantages the FES offers for an analysis of IS take-up. For instance, Atkinson and Micklewright (1983) have shown that for non-self-employed non-pensioners, data on incomes in the FES are, on the whole, reasonably accurate (when compared to national accounts). On the other hand, for pensioners and self-employed individuals this is not the case. In addition, Fry and Stark (1989) have shown that there is a serious amount of misreporting of many benefits (including IS) by pensioners surveyed by the FES. The sampling methodology employed by the BHPS (described in Taylor (1996)) does not take into account such problems and so there is, of course,

[^9]a strong possibility that the same (or at least similar) advantages and disadvantages apply.

Bearing the above in mind, we turn first to Figure 1.8 where we have plotted the reported number of IS payments for each of the BHPS cross-section waves on a monthly basis. Each wave provides information from September of the previous year to August of the year of the actual wave (i.e. wave A records benefit receipt from September 1990 to August 1991). Thus, unlike most other variables contained in the BHPS, for benefit payments there are monthly records as to whether a specific benefit payment was received or not.

Figure 1.8 Total Number of IS Recipients in the BHPS, Waves A to D


From Figure 1.8 we can clearly make out the increasing number of IS recipients in any one wave. This rise in recipient numbers is particularly marked at wave A. However, recipient numbers in any one wave display a considerable degree of month-to-month variation. Official DSS counts of the number of benefit recipients for any one year are usually based on a count at one point in time of that year. From Figure 1.8 it becomes apparent that by doing so, we do not capture the quite large numbers who enter and exit the IS caseload. So, for example, in wave C (September 1992 to August 1993) the number of IS recipients in say May 1993 is 726 , whereas the average number of IS recipients is 720.0 (with standard deviation 35.5 ); in wave D
(September 1993 to September 1994) the number of IS recipients in May 1994 is 715, and the average for wave D is 707.1 (with standard deviation 34.6).

Finally, to obtain some indication of how the number of IS recipients in the BHPS compares to the number of IS recipients from official DSS sources, we adopt a rather rough grossing-up procedure. By grossing-up a data set we attempt to make the survey sample (which it is) representative of the entire British population. Precise grossing-up procedures take into account the survey deficiencies and sampling errors briefly touched upon above. ${ }^{21}$ Hence, for well 'established' data sets such as the FES and GHS, grossing-up scales exist that have been tried and tested (see Atkinson et al. (1989)). However, for the BHPS this is not the case and we therefore adopt the following ad hoc procedure: we gross-up each sample of IS recipients (i.e. the samples drawn from each cross-section) by a factor which is the ratio of the approximate total number of households in Britain (about 22 million) to the number of households surveyed at each wave of the BHPS. So, for instance, at wave B the number of households surveyed by the BHPS is 5,227 and thus the grossing-up factor is $4,208.9$. The results of this procedure are given in Figure 1.9 where, for comparison, we also give the actual number of IS recipients from DSS sources. ${ }^{22}$ Note also how we have not connected the time series lines between each wave. If we were to do so, this would provide a misleading picture since we have drawn our samples from each cross-section of the BHPS. In other words, the total number of IS recipients are based on different sample sizes (previously we noted that the BHPS is subject to some degree of attrition so that the actual sample sizes decrease from wave to wave). Of course, a proper grossing-up procedure would account for these changing sample sizes.

[^10]Figure 1.9 Total Number of IS Recipients: DSS Records and Grossed-Up BHPS, Quarterly Records Waves A to D


Source: Social Security Statistics, DSS (Various Years) and author's calculations based on BHPS data. Note: BHPS figures are roughly grossed-up (see main text for a description).

The number of IS recipients in the BHPS falls well below the reported number in official figures. At the same time though, the overall trends of both graphs (ignoring the breaks in the BHPS series) appear to be reasonably similar at times. Of course, some of the shortfall is no doubt the direct result of our grossing-up procedure. Yet it is unlikely that the entire shortfall is solely due to grossing-up; it is quite conceivable that there is actually some degree (most likely quite a substantial one) of under-reporting of IS receipt in the BHPS. Unfortunately we are, at this stage, unable to be more precise about this shortfall. However, if the BHPS does in fact suffer from similar problems as both the FES and the GHS do (and there is no reason to believe that it should not), then one of the most likely explanations for this shortfall is the under-reporting of IS receipt by pensioners. We return to this issue in Chapter 2 where our analysis provides further evidence of the nature of the IS recipiency shortfall in the BHPS.

### 1.5 Conclusions

This short chapter has traced the move towards greater means-testing of benefits in Britain throughout the 1980s and early 1990s. We have drawn primarily on official statistics in describing the developments in real expenditure on means-tested benefits and the total number of means-tested benefit recipients. In line with policy objectives set forth in the Social Security Act 1986 (and implemented in April 1988), we detect a definite shift towards targeting benefits to those in greatest financial need. However, this shift occurred well before the social security reforms of 1988, but did in fact increase in pace especially after 1988. The total number of means-tested benefit recipients (SB/IS, FIS/FC and HB) increased by 3.5 percent between 1983 and 1988 and by close to 30 percent between 1988 and 1995.

Since the emphasis of this thesis is on IS, we have turned to IS in greater detail, examining the possible factors which may have contributed to the quite dramatic increase in IS recipients. The total number of IS recipients expanded by more than 80 percent between 1980 and 1995, and the two largest groups which contributed to this rapid increase are lone parents and the sick and disabled. In contrast, the number of pensioners in receipt of IS has increased only marginally whereas the change in the number of unemployed individuals is strongly dependent on the unemployment rate (although policy changes have also increased notably the proportion of unemployed individuals dependent solely on IS).

Amongst the likely causes of the rise in IS dependency are a variety of economic, financial and demographic changes, as well as government policy changes. Nevertheless, there is still scope for a more extensive econometric analyses which accounts for the interaction of many of these changes on the increasing numbers of IS recipients. For our analysis, this simple inquiry based on relatively short time series, is sufficient for the purposes of introducing the main crux of this thesis: that there are still in 1994/95, in spite of over 5.6 million IS recipients and an IS budget of over $£ 16.5$ billion, a considerable number of individuals who appear not to be in receipt of their IS entitlement. (By 1993/94 (latest estimates) there were between an estimated 720,000 to 1.39 million individuals who, although being entitled to IS, were not receiving it.)

Finally, we have highlighted some aspects of the quality of IS data in the BHPS. In Chapter 2 we provide further evidence of the shortfall in reporting IS recipiency in the BHPS. A full examination of the reliability of BHPS data is still required, and will furthermore necessitate proper grossing-up procedures for the BHPS.

## CHAPTER 2

## AN ALGORITHM FOR COMPUTING INCOME SUPPORT ENTITLEMENTS IN BRITAIN

### 2.1 Introduction

At the heart of all future analysis into the take-up of benefits lies the construction of a data set containing all individuals entitled to Income Support (IS). We draw our sample of entitled individuals from the British Household Panel Survey (BHPS). The BHPS contains information on benefits and, in particular, on whether or not a respondent is receiving IS at the time of being interviewed. However, for the analysis of take-up this information is necessary but not sufficient. The data required for a take-up analysis must contain not only all individuals currently receiving IS but also all those entitled to IS but currently not in receipt. In order to deduce the latter of these we must compute for each individual in the BHPS who is eligible for IS an appropriate entitlement. This chapter describes this process and the resulting output obtained from the IS algorithm.

It is useful at this early stage to clarify, in brief, the distinction between IS eligibility and IS entitlement. In order to compute a monetary amount of IS for an individual claimant (i.e. an entitlement) the very first step is to assess whether they are actually eligible. The rules for eligibility are relatively simple and refer to the hours worked and the amount of savings held (they will be described in more detail later on). Once eligibility is determined, an IS entitlement is computed. This entitlement depends on a variety of factors such as age, marital status, number of dependent children and, above all, income (hence the means-test). The computed entitlement can turn out to be positive or negative. Only when the entitlement is positive do we have what we refer to
as an entitled individual. ${ }^{1}$ Therefore, non-entitled individuals can either be individuals who are simply not eligible for IS, or who are in fact eligible for IS but do not have a positive IS entitlement. Our IS algorithm follows this general procedure and the resulting output consists of a sample of (entitled) individuals who are both eligible for IS and entitled to a positive amount of IS.

As outlined in the previous chapter, our emphasis falls upon IS, the main meanstested benefit in Britain. To restate, IS is a non-contributory benefit aimed mostly at the non-working poor or those working relatively small numbers of hours and is as such the main safety net benefit in Britain. Our IS algorithm attempts to emulate, as best as it can, the rules and regulations concerning IS eligibility and entitlement. However, the legislation with respect to IS eligibility and entitlement is complex and often difficult if not impossible to follow accurately and precisely. The official IS legislation is contained in more than eleven volumes, with annual additions and changes. Even the condensed benefit legislation found in CPAG's National Welfare Benefits Handbook, for example, easily covers more than 500 pages nowadays. ${ }^{2}$ The major problems in an effort to mimic these rules and regulations stems from the fact that, like many other socio-economic data sets, the information in the BHPS is not collected for the purpose of analysing take-up behaviour. As a result, the data do not always contain all the relevant variables or suitable proxies requisite for following particular IS rules.

The problem of accurately following IS rules occurs mostly when special rules apply. For instance, consider the rule concerning savings in a dependent child's name. IS regulations normally state that additional entitlements apply for dependent children; yet a claimant cannot receive any additional IS for a dependent child if the child has savings in excess of $£ 3,000$. From the information contained in the BHPS it is not possible to assess a child's savings so that, in this case, it would be unworkable to take this rule into account. ${ }^{3}$ Besides, many of the special rules and regulations are relatively

[^11]non-binding and are applied at the discretion of the Department of Social Security (DSS), thereby complicating matters even further. For example, a cohabiting couple can be treated as a married couple or as two separate claimants, giving rise to different entitlements.

The above are just a couple of examples to illustrate the possible complexities involved in determining IS eligibility and entitlement. Throughout this chapter we frequently draw attention to such cases when and if they do occur. Nevertheless, such special cases are relatively unusual and arise infrequently in our data set. A further problem in determining eligibility and entitlement relates to the fact that even when we are able to follow IS rules we are not always able to do so accurately. Much of this problem stems from reporting inaccuracies within the BHPS. For example, individuals questioned about their working hours might not always respond truthfully and/or accurately thus giving rise to incorrect eligibility determination. However, we will return to these issues in more detail later in this chapter.

Previous algorithms of benefits in Britain/UK have focused around the construction of tax-benefit models using the Family Expenditure Survey (FES) as their input source. These models allow policy analyses to be performed by simulating the interaction of taxes and benefits and their impact on individual tax/benefit units. Most notably in the UK, the TAXBEN model (see Johnson, Webb and Stark (1990)) and the POLIMOD model (see Sutherland (1991) and Hancock and Sutherland (1992)) have come to the forefront. Such programs encompass a much wider range of policy simulations and are as such not directly suited to our needs. Our model is more specific in its aim in that we concentrate solely on IS. Also, to our knowledge, this is the first model of its kind using the BHPS as its input source.

This chapter as a whole describes the rules and regulations with regard to IS in Britain, thereby outlining the features of the IS algorithm which enable the appropriate calculations to be performed. ${ }^{4}$ In Section 2.2 we outline both the construction of our data set from the BHPS and the structure of our computer program with its constituent

[^12]modules. As noted above, the lack of accurate savings information in the BHPS poses a problem and our method to tackle this issue is discussed in Section 2.3. Section 2.3 also describes the computation of mortgage interest payments required for computing entitlements. The output obtained from our IS algorithm is discussed in Section 2.4, and we finally conclude in Section 2.5.

### 2.1.1 A Brief Overview of the IS Algorithm

In this chapter we describe the IS algorithm by providing an outline of the BHPS data and the way in which we use this data at each step of the algorithm. However, the actual algorithm itself is a mechanical procedure of obtaining IS entitlements and, as such, is entirely independent of the data set we employ as an input. The data simply provide a set of 'case histories' that are used to compute IS eligibilities and entitlements. Hence, before we embark on a detailed description of the BHPS and the way the algorithm uses this data, we begin by providing a brief overview of what the IS algorithm actually is, what it does, and what its inputs and outputs are.

According to The New Shorter Oxford English Dictionary an algorithm is defined as: "A procedure or set of rules for calculation or problem-solving, now especially with a computer". This is precisely what our algorithm does for IS eligibility and entitlement. Given an arbitrary data set as an input source, the data passes through a set of program modules each of which builds on the previous module, culminating in an output data set which consists of individuals eligible for IS and entitled to some positive IS entitlement. This information is subsequently used throughout this thesis in order to assess the degree of take-up/non-take-up which, in turn, enables us to conduct our investigation into the determinants of IS take-up in Britain.

The key advantage of a computer based algorithm is that it works, in principle, for any data set chosen as an input source. In practice, our algorithm has been constructed around the BHPS and consequently the ensuing description of the algorithm in this chapter is based on this particular data set. Nevertheless, being a longitudinal data set with annual releases of new data, our algorithm is easily modified so as to accommodate each new wave of the BHPS as it emerges.

The basic structure of the IS algorithm can be reduced to four key program elements. These elements are written in the $S A S$ programming language (see SAS Institute (1988)) which is particularly suited to the manipulation and preparation of large and complex data sets such as the BHPS. For the purposes of exposition in this section (and not necessarily for any subsequent sections), we can refer to these four program elements as (1.) data extraction and basic data preparation, (2.) data manipulation, (3.) key variable imputations and computations, and finally (4.) eligibility determination and entitlement computation. This sequence corresponds to the order in which a chosen input data set passes through the algorithm. So, the input to program element (2.) of the algorithm is the output produced by element (1.), the output produced by (2.) is the input to element (3.), and so on. The input to the very first program element must be a crosssection data set with information on a large number of socio-economic characteristics (e.g. for the BHPS each new wave must be passed through the program elements separately). Although the algorithm requires information at both the household level and the individual level, the computations are all made at the individual level. In particular, any one household unit can have only one IS claimant. For instance, a couple with or without dependent children (married or cohabiting) can have only one IS claimant, referred to as an 'individual' in our algorithm. The first program element thus draws out all relevant variables from the raw data set and combines these in such a way that all required data manipulations of part (2.) are facilitated. Element (3.) is a BHPS specific element since it lacks information on a number variables critical in determining eligibility and entitlement. Finally, element (4.) forms the largest part of the algorithm. It is here that eligibility is determined (in line with the relevant IS legislation) and, for all those individuals found to be eligible for IS an appropriate IS entitlement is computed.

The output from the algorithm is thus a data set of individuals who are both eligible for and entitled to IS. Recall that each individual in the data set represents one claimant unit. This unit can be a single individual, a couple (with or without dependent children) or a lone parent. In addition, separate output data sets are produced for pensioners and non-pensioners. The important point is that any one of these output data sets contains not only information on various socio-economic characteristics but also a
variable giving the amount of IS (in pounds per week) to which the eligible individual is entitled to.

The remaining sections of this chapter elaborate on the exact workings of the algorithm. Throughout these sections specific reference is given to the BHPS.

### 2.2 The IS Algorithm Layout

The IS algorithm consists of a series of programs each of which has its own purpose. In this section we describe the separate programs and how they combine to function as a complete unit. At the same time, we outline the general IS eligibility and entitlement rules. The starting point consists of a data set (or input source) which in our case is the BHPS. The BHPS is a panel (longitudinal) data set following about 10,000 individuals in roughly 5,000 private households representative of the British population. Household members are re-interviewed on an annual basis. ${ }^{5}$ The basic idea is to follow not only the Original Sample Members (OSMs) chosen at the beginning of the BHPS (wave A or 1991) but also to follow up any OSMs or their off-spring who leave the original household and set up a new household. In addition, the aim is to interview all adult members of the new household as well. This way it is hoped that the exit and entry levels from and into the BHPS roughly balance. At present, having reached wave D, the latter dominate slightly with an approximately 4.5 percent rate of attrition from one wave to another. The aim is thus to obtain a picture of socio-economic changes of the British population throughout the 1990s, and as such the BHPS is the first British data set of its kind.

The actual compositions of each sample at waves A to D are shown in Table 2.1 (see Appendix 2A for this and all subsequent tables). Unlike many other socio-economic data sets, the BHPS will only include consider a household for interview if every adult household member (aged 16 and above) agrees to be interviewed. However, in some cases a household member might be absent at the time of interview or too ill to be

[^13]interviewed, in which case a shorter questionnaire is completed by another household member (usually the head of household if available) on behalf of that respondent. Such cases are referred to as proxy respondents. Note that in general, the information on proxy respondents is insufficiently detailed for them to be included in our IS algorithm so that we draw only on the sample of respondents with full interviews.

The BHPS contains a wealth of information on both the individual level and the household level. In addition to a core set of issues surveyed at each successive wave, special topics are chosen from wave A onwards. The main topics covered in each wave are (i) household organisation, (ii) the labour market, (iii) housing, (iv) income and wealth, (v) health, and (vi) socio-economic values. As noted in the introduction, although the BHPS is not collected with the aim of investigating take-up (or social security for that matter), it is nevertheless a data set very suited to an analysis of take-up since it contains most of the requisite information. It is, however, worthwhile stressing that for wealth and income related variables the Family Expenditure Survey (FES) is considerably more detailed. ${ }^{6}$ The FES has been the recurrent choice in analysing take-up (see Chapter 3 for a survey) and has also been the main input source for tax-benefit models such as TAXBEN and POLIMOD. The main advantage the BHPS has over the FES is, first and foremost, that it is not solely a cross-section data set but instead allows one to gain a picture of change and transformation amongst individuals entitled to IS. Such data also permit some scope for a more dynamic analysis of take-up, an issue which so far has received little attention in the applied literature (mostly as a direct result of a lack of suitable data). ${ }^{7}$ Furthermore, the BHPS contains a much larger number of socioeconomic variables than the FES, and these will be of use in the empirical analysis of take-up later on (see Chapters 4 and 5).

### 2.2.1 Merging and Processing the Data

The general structure of our IS algorithm is summarised in the flow-chart of Figure 2.1 (see Appendix 2B for this and all subsequent figures) where the main stages of the model

[^14]are identified. In addition, Figure 2.2 provides a more detailed picture of the first main stage of the program (data extraction and manipulation). The flow-charts follow roughly the sequence in which data passes through the program. So, for each wave of the BHPS, the program begins by drawing the appropriate data records. Like most large and complex data sets the BHPS data is contained in various records each of which contains different information on the same individuals or households. We make use of four main record types: the first of these is the Income Record containing information on incomes from up to 33 different sources. The majority of these are state benefits and any information related to these benefits, such as the duration of receipt, the last amount received, the number of weeks covered and so on. (This record does not however contain information on income from earnings.) The second record, the Individual Response Record, contains all the information from the full individual interviews and thus constitutes the main data source of the BHPS. For each adult member of a household (aged 16 or above) there are around 750 different variables covering all six main topics of the BHPS. The majority of variables required for our program are to be found in this record type. Third, the All Individual Record holds only a small number of key variables linking individuals within a household and is furthermore the only record to hold some moderate information on children within households. ${ }^{8}$ This record type is crucial in determining relationships between individuals in a household and is used to match dependent children to the corresponding responsible adult. Finally, the fourth record type, the Household Response Record, is similar to the Individual Response Record except that the unit of analysis is the household. This record contains around 200 different variables for each household.

All variables required for the IS algorithm are extracted from each of the above records and appropriate data transformations and adjustments are performed in order to merge them. ${ }^{9}$ Once merged, the resulting data set is split into six sub-samples by DSS

[^15]defined benefit units as follows: (i) single non-pensioners, (ii) non-pensioner couples without children, (ii) non-pensioner couples with children, (iv) single parents, (v) pensioner couples, and (vi) single pensioners. Next, in the case of couples we combine the two members to give one claimant only. ${ }^{10}$ For the purposes of our analysis and in line with DSS rules, a household consisting of a couple can have only one claimant. When this individual claims (for the couple) certain characteristics of the individual and his or her partner are taken into account in assessing eligibility and entitlement. Hence the reference person of the household is identified and the relevant characteristics of his or her lawful spouse or live-in partner (such as all the different incomes, number of hours worked etc.) are added to the reference person. Consequently, our final data set contains only one observation for any one couple, and we therefore refer to individual claimants throughout this thesis. ${ }^{11}$

The final part of Stage 1 of the IS algorithm is the largest and deals mostly with missing values and the creation of new variables. For example, at this stage we compute weekly wages from (gross or net) wages and the number of weeks these wage payments cover. ${ }^{12}$ As concerns missing values, like most micro-level data sets the BHPS contains a number of missing values for non-response, refusal or inapplicability of a particular question (so called item non-response). The BHPS missing value conventions are listed in Table 2.2. At this stage of the program most missing values are reclassified as appropriate. For example, in the case where a variable takes the value 'inapplicable' we can often reclassify to a value of 'zero' or 'no'. Occasionally however, certain missing

[^16]values are deleted since essential computations are performed on them. For example, in the case of hours worked per week, this is a crucial variable in determining eligibility and if it is missing (i.e. it takes a missing value label other than 'inapplicable'), the resulting computations are likely to be incorrect. However, we have confined such practices to cases of only small numbers of missing values for essential variables. If we were to selectively delete large numbers of missing values, the nature and composition of our resulting sample would be far removed from the original BHPS data set and resultant inferences drawn from the sample would no longer be representative of the British population.

Finally, our analysis is confined to individuals aged 18 and above unless they are a dependent child of a claimant (in which case they form part of the reference person's claiming unit). Although there are special IS rules relating to 16 and 17 year-olds their numbers are relatively small in the BHPS so that we have excluded them. Our IS algorithm also currently eliminates all self-employed individuals and those aged over 18 and still in further education. Excluded from the latter category are single parents in further education and married couples where one partner is a full-time student. All other full-time students were until April 1991 also entitled to IS over the long summer vacation but not entitled at all other times. Since wave A interviews occurred between September and December 1991 we can however ignore this rule. The self-employed, on the other hand, may well be entitled to IS but inaccurate income information imperative for computing entitlements is likely to distort conclusions with respect to this group. There is no direct evidence of this in the BHPS but data sets such as the FES (which, to recall, are after all collected for the purpose of accurate income and expenditure information) are well documented for having relatively inaccurate income and earnings information for the self-employed (see for example Atkinson and Micklewright (1983)). ${ }^{13}$

[^17]This completes the first stage of our IS algorithm. The next stages draw on this data set in determining eligibility and in computing appropriate IS entitlements. They are discussed in the following two sections.

### 2.2.2 The Eligibility Stage

The rules regarding eligibility are relatively straightforward, so that a comparatively large number of individuals (compared to the following Entitlement Stage) will be deemed eligible at this stage of the IS algorithm. The eligibility rules are slightly different for each of the six sub-samples described above. Hence, before the data is passed through the module determining eligibility the data set is split into its constituent sub-samples as depicted in Figure 2.1. Note also that the eligibility rules are subject to change on an annual basis and we draw attention to such changes if and when they occurred.

The two main eligibility rules are (i) the number of hours worked per week must not exceed the specified part-time limit and (ii) total savings and assets in the name of the claimant (plus spouse and dependants) must not be greater than a fixed upper limit. Concerning the first rule, the distinction here lies between all those individuals working full-time and those working part-time or not working at all. The full-time/part-time cutoff was set at 24 hours per week in 1991/92 (corresponding to wave A) and was lowered to 16 hours per week from 1992/93 onwards (corresponding to waves B to D). Individuals working 24 hours or more a week ( 16 hours in waves B to D) fall into the full-time category. For couples eligibility is granted only if neither one of the individuals works full-time. In order to compensate for the stricter working hours rule, in 1992/93 the Government introduced transitional provisions for working IS recipients who were affected by this legislative change. These provisions applied mostly to claimants who were in receipt of IS for up to a maximum of twelve weeks prior to the changes being implemented. In essence, they allowed claimants to continue working for more than 16 hours a week (but no more than 24 hours a week) without being instantly penalized. We do not take into account transitional provisions in assessing eligibility since, by the time the BHPS surveyed individuals for wave B (September to December 1992), these transitional provisions applied to only very small numbers of IS recipients. Evidence
from the IS Annual Enquiry for May 1992 suggests that only 9,800 individuals received some form transitional provisions out of a total of more than 5 million IS recipients (i.e. about 0.2 percent of all recipients). ${ }^{14}$

The total number of hours worked per week takes into account not only the hours worked in a main job but also any overtime hours and, if the individual has a second job, any hours worked there. The BHPS variable we use for the hours worked in the main job gives the normal hours of work per week rather than hours worked last week, say. We thereby hope to eliminate some of the problems resulting from fluctuating working hours. In particular, this should minimise the problem of misclassifying individuals on the basis of working hours (i.e. eligible individuals as non-eligible and vice versa). The DSS, when assessing hours of work, usually work with an average over a given time period prior to applying for benefits. There is no equivalent variable in the BHPS which provides information on the variability of working hours, the aforementioned variable coming closest to this assessment.

Individuals found to be part-time workers or not working at all are eligible (provided the second eligibility rule is satisfied) whereas those in full-time employment are generally not eligible. However, full-time workers who satisfy a set of additional conditions can qualify for eligibility. Briefly, for our sample, these apply to the mentally or physically disabled, individuals working for charities or voluntary organisations, those who are looking after individuals in receipt of Attendance Allowance, and to individuals on sick pay or maternity/paternity leave. ${ }^{15}$ These conditions are quite stringent and in cases where they apply it is often impossible to mimic these rules using the information contained in the BHPS. For instance, we can identify whether an individual is disabled or not but, more precisely, the rule states that a disabled individual working full-time will be eligible if her earnings are 75 percent or less of normal earnings, which we are unable to verify. Nevertheless, to reiterate, such rules are the exception rather than the norm and

[^18]apply to only small numbers of individuals. Overall, within our data set only a relatively small number of individuals from the full-time group turn out to be eligible.

The second main eligibility rule relates to the amount of capital held by individuals (and their partner if applicable). Capital includes all savings, investments etc. (we refer hereafter simply to savings) with certain disregards, most notably home ownership. The rules corresponding to waves A to D state that if total savings exceed $£ 8,000$ the claimant is automatically deemed to be non-eligible for IS. Moreover, any savings above $£ 3,000$ are regarded as producing some tariff income, that is, for every additional $£ 250$ in excess of $£ 3,000$ it is assumed that a weekly income of $£ 1$ is generated. Savings in the name of a dependent child are not added to the total of the responsible adult(s) but if the amount exceeds $£ 3,000$ the responsible adult is unable to claim any additional applicable amount for the child.

Unfortunately one of the main weaknesses of the BHPS is the lack of detailed information about savings and investment income. We are unable to determine the level of savings of a dependent child so that (as noted in the Introduction) we are forced to ignore this rule. ${ }^{16}$ More importantly, no precise questions are asked about the total amount of savings held by individuals - the only variable of interest gives the income from dividends/interest in the previous year. The problem with this variable is that the amounts stated are given in very wide bands as follows: (i) nothing, (ii) less than $£ 100$, (iii) $£ 100$ to $£ 1000$, and (iv) more than $£ 1000 .{ }^{17}$ This is clearly inadequate for our purposes and since the capital rule is essential in determining eligibility we impute savings with the aid of another data set. The imputation of savings, using the more detailed savings and investment information in the FES, is outlined in Section 2.3.

Some understanding about the numbers of eligible individuals with some savings can be obtained by considering DSS statistics on the number of IS recipients with capital (see DSS (1992, 1993b, 1994b and 1995a)). The official figures suggest the following percentages of recipients with savings: 15.2 percent in May 1991, 16.5 percent in May

[^19]1992, 15.3 percent in May 1993, and 13.8 percent in May 1994. However, the majority of these savers have savings of less than $£ 1,000$ and, in addition, are pensioners. In fact, the actual proportions with savings in the tariff income generating range of $£ 3,000$ to $£ 8,000$ are considerably smaller: only 1.9 percent of all recipients in May 1991 and May 1992, and 2.2 percent of all recipients in both May 1993 and May 1994 have savings in this range. Therefore, the overwhelming majority of IS recipients do not have any savings at all, and for those who do have some savings, the total amounts are small enough so as not to be considered in their IS eligibility and entitlement assessment.

The situation may, of course, be very different for entitled non-recipients of IS. In fact, one plausible explanation for non-take-up by individuals entitled to IS may be that they have some savings which they are able to draw on. They consequently feel less need to claim benefits. No doubt this is probably true for some of the cases we deal with, particularly in the short term. In general, however, it appears an unlikely reason for non-take-up in the longer term. Later on, our evidence suggests that the largest groups of entitled non-recipients of IS are low income households consisting of the unemployed, those in family care and households where the head is employed part-time in a low paid job. It thus appears unlikely that many of these have accumulated savings. Other factors are more likely to explain the nature of entitled non-recipiency (these are considered in Chapters 4 and 5).

To summarise, at this stage of the program we impute savings and thereby eliminate all individuals with savings in excess of $£ 8,000$. We also create a new variable containing tariff income from savings computed as described above. The overall output at this stage of the program is a sample of individuals eligible for IS, that is all those individuals with no savings in excess of $£ 8,000$ and, in addition, (i) not working, or (ii) working part-time (as defined by the DSS), or (iii) working-full time and satisfying at least one of the special rules. The resulting sample (the eligible sample) constitutes the input source for the next stage of our IS algorithm.

[^20]
### 2.2.3 The Entitlement Stage

The final stage of the program computes an IS entitlement (on a $£$ per week basis) for each individual in our eligible sample described in the previous Section 2.2.2. Eligibility determination is summarized above and here we note briefly some warnings about the composition of the eligible sample. We noted above that, to some extent, we are uncertain about the exact number of hours individuals work per week and, in addition, we are unable to impute savings with a great degree of precision. Hence we are likely to incorrectly include in our entitlement computations some individuals who are not actually eligible for IS and similarly exclude some individuals who are truly eligible for IS. With respect to the savings imputation, we furthermore encounter the problem of possibly incorrect amounts of tariff generating income which in turn affects the entitlement assessment. In this section it will become apparent that such errors are not confined to the Eligibility Stage of the program and are, in fact, even more likely to occur in the Entitlement Stage.

However, we also stress that our emphasis falls on the latter problem, that is we concentrate on errors which occur at the Entitlement Stage and accordingly have the potential to distort the computed IS entitlement. The problems of incorrectly including non-eligible individuals in the eligible sample and discarding truly eligible individuals from it, if assumed to occur in a purely random pattern, will not have a large impact on the composition of our final sample of entitled individuals. ${ }^{18}$ In addition, we stressed above that we attempt to minimise the incidence of this latter problem. Errors which occur in the Entitlement Stage are a recurrent theme in this thesis and the impact of such errors on analysing take-up behaviour are investigated in Chapter 4.

Errors in the Entitlement Stage occur primarily as a direct consequence of the detailed information required about an individual's income. Although most of these

[^21]incomes are provided in the BHPS we cannot always be certain that they are reported accurately and/or truthfully. Hence we may compute no positive entitlement for an individual who is truly entitled to IS (i.e. an individual for whom we would compute a positive benefit entitlement if we were able to observe exactly her income) or a positive entitlement for individuals who are actually not entitled to IS. However, this is likely to occur only at the margin of IS entitlement, that is the cut-off point ( $£ 0.10$ ) where IS is either granted or not. More generally, inaccurate incomes are likely to lead to incorrect IS entitlements. Thus, our final entitled sample (i.e. the sample containing all individuals entitled to IS) may, on the whole, consist of the majority of individuals actually entitled to IS but their computed entitlements may be quite inaccurate.

The actual computation of the IS entitlement proceeds as follows: the entitlement is computed as the difference between the Applicable Amount and any income the individual may possess. The Applicable Amount is supposed to cover all weekly needs (excluding rent) and is laid down by the Government with annual upratings in line with inflation. ${ }^{19}$ It is split into three components consisting of (i) personal allowances, (ii) premiums, and (iii) housing costs. The first of these is a fixed payment depending on an individual's characteristics such as marital status, age, number of children and so on. The premiums are additional fixed payments accounting for special needs arising from disability, pensioner status, and family status. Finally, the housing costs component essentially refers to mortgage holders and covers mortgage interest payments. (The exact computation of mortgage interest is described in Section 2.3 below.) The Applicable Amounts for our sample are given in Table 2.3 and are drawn from various editions of the CPAG's National Welfare Benefits Handbook. So, for example, at wave D (1994) a single mother aged 30 with a son aged 11 has an Applicable Amount of $£ 83.85$ of which $£ 68.70$ is the personal allowance and the remaining $£ 15.15$ are premiums (we have assumed no housing costs covered by IS).

On the income side, most of the income of an individual plus any income of their partner are counted. Income earned by a dependent child still at school is ignored. In

[^22]fact, very little income is disregarded in computing the IS entitlement (e.g. payments in kind, health authority/local council payments for looking after an ill person). ${ }^{20}$ Many of the incomes have certain disregards which are outlined in detail in the National Welfare Benefits Handbook. In the case of couples with separate incomes both partners' incomes must be taken into account. The main income sources are briefly summarised in the following:

1. Net earnings (including earnings from a second job, bonuses and commissions, holiday pay and any back payments) obtained by deducting income tax, class I National Insurance contributions, and one-half of any contributions to a personal/occupational pension scheme from gross pay count in full.
Clearly this will not always be precisely the same as net income reported in the BHPS. If we were to take full account of this calculation we would have to determine for each eligible individual their tax payment and account for pension scheme contributions. For our computations we use an individual's (and spouse's if applicable) last net pay and, in addition, we also check whether this pay was unusual. If it was, we use the usual take-home pay instead. Moreover, we take into account earnings from a second job but do not consider bonuses, commissions, holiday pay and/or back payments. The latter might pose a problem in cases where individuals have been dismissed from employment and have been awarded a lump sum (see point 2. below) or back payments say. In such cases we would compute a higher IS entitlement than would be the case if we were able to account for the aforementioned payment.
However, in general we believe the BHPS variables contain sufficient information for our purposes and as such are a good enough measure of net earnings. Finally note that $£ 5$ of net earnings are always disregarded and $£ 15$ are disregarded for certain individuals (lone parents, singles who are sick or disabled, pensioners with part-time jobs, and the long-term unemployed).

[^23]2. Tariff income generated from savings/capital. As described above, for every $£ 250$ above $£ 3,000$ in total savings (up to $£ 8,000$ are allowed for eligibility), a weekly tariff income of $£ 1$ is assumed to be generated. In other words, tariff income falls into the range $£ 1$ to $£ 20$ per week. Note also that redundancy payments are usually regarded as savings.

Tariff income is derived directly from our savings imputation described in Section 2.3 below. To reiterate, only small numbers of individuals have any tariff income at all and even if they do, the amounts are relatively small in most cases. Hence, in those cases where we do not account for tariff income we will overestimate the IS entitlement.
3. Various other incomes as follows: (i) maintenance payments of any kind, (ii) the majority of other benefit income (with certain exceptions such as, for example, Attendance Allowance, Mobility Allowance, Housing Benefit and Community Charge Benefit) with some benefits having certain disregards (for example, $£ 10$ for any warrelated benefits), (iii) rent from tenants and/or boarders with certain disregards, (iv) occupational pensions and payments from an annuity, and finally (v) any other income not accounted for above.

So, this stage of our IS algorithm computes for every individual in the eligible sample an Applicable Amount (in £ per week) and secondly, a DSS defined income (also in $£$ per week). The weekly IS entitlement is then simply obtained by subtracting the latter from the former. The overall computation of the IS entitlement by our IS algorithm can be summarised as:

$$
B_{C}=\operatorname{Max}\{0.10,[(B+m)-(w+n+s)]\} \quad \text { if } h r s<24 / 16 \text { and } S<8,000
$$

and $B_{C}=0$ otherwise. ${ }^{21}$ Here $B$ is the sum of the applicable amount and any premiums that apply (these will be different for each year), $m$ is weekly mortgage interest, $w$ weekly net earnings, $n$ weekly benefit income from other sources and other non-labour income (with certain exceptions and disregards), and $s$ is tariff income from savings. In addition,
hrs is the number of hours normally worked per week, and $S$ is the total amount of savings. Finally, note that an individual falls into the entitled sample only if $B_{C} \geq £ 0.10$.

The output generated by our IS algorithm is discussed in Section 2.4. To conclude this section, we note again that the computation of $B_{C}$ is subject to error, and consequently our resulting entitled sample can include some individuals who are actually not entitled to IS and exclude some genuinely entitled individuals (i.e. misclassification in the entitled sample might occur). Such misclassification is no doubt a problem in analysing take-up, particularly when it comes to computing take-up rates (see Section 2.4.2). However, we believe that the majority of errors which occur in computing $B_{C}$ show up as 'measurement error' in $B_{C}$ and not as misclassification errors in the entitled sample. The main reason for this is that in many cases, for misclassifications to occur, the degree of errors involved must be quite large or the entitlement itself must be close to the cut-off margin of $£ 0.10$. In other words, misclassification becomes an issue only in those cases where the Applicable Amount is very close to an individual's income. Those most likely to be affected are thus individuals who work part-time with earnings close to the Applicable Amount and individuals with relatively larger amounts of other unearned income. One way to verify this is by comparing our take-up estimates with the official take-up estimates produced by the DSS which we do later in this chapter. In addition, later on we also provide scatter plots of IS entitlements for all individuals in the eligible sample.

In brief, the main reasons for measurement error and thus misclassification errors to arise can be summarised as follows: firstly, the variables required to calculate IS eligibility and entitlement are not available or are not well proxied by other variables present in the BHPS. This is particularly the case when special rules apply. At the eligibility stage misclassification can occur (for example when the special rules for $h r s \geq$ $24 / 16$ need to be applied) and when the IS entitlement is computed it cannot always be established without error. Secondly, even when the required variables are present in the BHPS we cannot always be confident they are reported accurately. This is especially true

[^24]of the incomes used in computing entitlements. These are likely to be reported, at least to some extent, inaccurately. Thirdly, the savings imputation giving rise to $S$, and subsequently $s$, is likely to be a large source for error. In Section 2.3.1 it becomes apparent that we are unable to predict savings with great accuracy. Finally, in calculating mortgage interest, endowment mortgage repayments have been considered as interest only payments (see Section 2.3.2). Thus in calculating $m$ for endowment policy holders no account is taken of insurance policies and other costs included in $m$ which are not covered by the DSS. This will inflate the true applicable amount and consequently increase the number of eligible non-recipients. (These payments though should however only form a small part of the total payment.)

### 2.3 Imputing Savings and Computing Mortgage Interest

The output from Stage 1 of the our IS algorithm is passed through a set of programs which handle the imputation of savings and the computation of mortgage interest (see Figure 2.1). These parts of the program are invoked before the data passes through the Eligibility Stage and Entitlement Stage discussed above. By doing so we ensure that each observation has corresponding variables containing information on savings and mortgage interest respectively. This section describes the procedures adopted in order to deal with these issues.

As noted above, savings are of importance in determining both eligibility and entitlement. The non-existence of detailed information on savings in the BHPS can be regarded as a case of item non-response where none of our observations contain the required information about savings. More precisely, there is insufficiently detailed information within our data set so that, in order to impute the missing values, we resort to a different data set containing the necessary information.

Mortgage interest is crucial in determining the exact IS entitlement since, in those cases where an individual is a mortgage holder, that part of the payment which is solely interest it is added to the Applicable Amount. Here we describe a method adopted from

[^25]Coulter (1991) to compute the interest component for those observations with a mortgage.

### 2.3.1 Estimating Missing Savings

Our aim is to construct a simple model of savings determinants at a microeconomic level with the aid of a data set which contains information about savings. The estimated model can then be used to predict a continuous savings variable for the BHPS, thus providing more detailed information than currently exists in the BHPS. To recapitulate, the existing bands for annual income from savings as reported in the BHPS are: (i) nothing, (ii) less than $£ 100$, (iii) $£ 100$ to $£ 1000$ (from wave C onwards split into $£ 100$ to $£ 500$ and $£ 501$ to $£ 1000$ respectively), and (iv) more than $£ 1000$.

The data set we employ for the savings imputation in the BHPS requires information on savings in a continuous format and, in addition, information on individuals' incomes and socio-economic characteristics. There are currently two UK data sets which contain the required information: the FES and the Financial Research Survey (FinRS). ${ }^{22}$ These two data sets are compared by Banks and Tanner (1996) who also provide a detailed analysis of financial asset holdings in the FES. Like the BHPS, the FinRS contains data on savings only in bands, and although they are more extensive than the four to five bands used in the BHPS, this aspect of the FinRS restricts its usefulness for our purposes. Hence we shall draw on the FES for our savings imputation.

In the FES individuals are questioned about various savings and investments and in particular, about the amount of interest/dividend income per annum received from them. Using average bank and building society interest rates for the appropriate months we can convert these figures into approximate amounts of total savings (the FES suggests that in the period 1991 to 1995 over 90 percent - on average - of all financial assets owned by households in the UK are held in banks and building societies). Our interest falls upon those individuals with savings in the range $£ 3,000$ to $£ 8,000$ only since those who fall outside of this range do not have any tariff income to be dealt with. Therefore

[^26]we can ignore income from savings which falls into the BHPS bands (i) and (iv) above. Those observations with income from savings in range (i) above are eligible since they do not have any savings to be dealt with. However, observations falling into the range (iv) above are not eligible for IS since such a level of income from savings would suggest savings well in excess of $£ 8,000$ (average annual bank and building society interest rates were approximately 7.25 percent for wave A, 5.0 percent for wave $B, 6.5$ percent for wave C , and 6.7 percent for wave D ). ${ }^{23}$ At these interest rates we can also exclude observations with savings in the range (ii) above. So, one approach would be to compute identical bands for the FES and perform an analysis only for those individuals with income from savings in band (iii), $£ 100$ to $£ 1000$. However, many individuals in the FES do not have any savings at all, and amongst those who do hold some financial assets these total less than $£ 1,500$ in the majority of cases (see Banks and Tanner (1996)). Using only observations with income from savings between $£ 100$ and $£ 1000$ would consequently diminish the available information on savings determinants. Hence, in order to obtain as much information as possible from our sample, we use all reported amounts of income from savings in our model. Later when we predict savings in the BHPS, we use the estimates from our imputation model together with the fact that only a certain range of savings are of interest to us.

Modelling savings determinants at a microeconomic level using a single crosssection of data is inevitably of limited use. It is important to realize the inadequacies of this exercise. Standard economic models of savings behaviour are often based on the simple life-cycle hypothesis of Modigliani and Brumberg (1954) who argue that individuals save in order to smooth consumption over time (given an uneven income profile). Even though the model is limited in scope (see for example Deaton (1992)) it remains the predominant choice for economic models of savings and consumption. Whatever form the model takes, it remains intrinsically dynamic in nature and thus a simple cross-section will be unable to capture any dynamic element in the savings decision. Consequently, the majority of work using micro-level data tend to pool data

[^27]over several years thereby exploiting some time-series element of the data. Banks and Blundell (1993), for example, use a twenty-two year pseudo-panel of the FES (1969 to 1990) to analyse savings behaviour at the household level. Similarly, Alessie et al. (1995) use panel data for the Netherlands in their analysis of savings and wealth holdings, and Attanasio (1993) uses a rotating panel to study savings behaviour of US cohorts. ${ }^{24}$

Banks and Blundell stress that a single cross-section can be quite misleading in an analysis of savings. In particular, they find that the hump-shaped savings profiles often associated with age can in fact be primarily ascribed to cohort effects. Nevertheless, for our purposes we believe that there is some (albeit limited) insight to be gained from such a regression imputation based approach. Inevitably our approach is very ad hoc in nature but since only very small numbers of individuals entitled to IS have any savings at all, we believe this approach for overcoming the lack of detailed savings information in the BHPS should suffice.

The model we use for the savings imputation is a standard tobit model. We assume that the underlying process generating the data for income from savings (hereafter simply referred to as savings) is given by an unobserved latent variable, $y_{i}^{*}$, such that

$$
\begin{equation*}
y_{i}^{*}=\beta^{\prime} \mathbf{x}_{i}+\varepsilon_{i} \quad \text { where } \quad \varepsilon_{i} \sim \operatorname{IN}\left(0, \sigma^{2}\right) \tag{2.1}
\end{equation*}
$$

for each individual $i=1, \ldots, n$, where $\mathbf{x}_{i}$ is a vector of explanatory variables (described in more detail later), and $\beta$ is a corresponding parameter vector to be estimated. The latent variable approach captures the notion that 'potential' savings can be a positive or negative amount (the latter can be regarded as credit). Since savings in the FES are reported either as zero or some positive amount we model observed savings, $y_{i}$, as a standard tobit model where

$$
y_{i}= \begin{cases}y_{i}^{*} & \text { if } y_{i}^{*}>0  \tag{2.2}\\ 0 & \text { if } y_{i}^{*} \leq 0\end{cases}
$$

The savings model is described by (2.1) and (2.2). It is well documented that estimation by OLS yields inconsistent estimates (see Amemiya (1981), Dhrymes (1986) or Maddala

[^28](1983)). The standard approach is to estimate the model by maximum likelihood with constituent parts as follows: for the zero observations we have
\[

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=0\right)=\operatorname{Pr}\left(u_{i}<-\beta^{\prime} \mathbf{x}_{i}\right)=1-\Phi(c) \tag{2.3}
\end{equation*}
$$

\]

and for the positive observations

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}>0\right) \times f\left(y_{i} \mid y_{i}>0\right)=\Phi(c) \times \frac{\phi(c)}{\Phi(c)} \tag{2.4}
\end{equation*}
$$

where $c=\beta^{\prime} \mathbf{x}_{i} / \sigma$. We also estimate a tobit model with heteroskedasticity since crosssection surveys such as the FES are likely to suffer from heteroskedasticity. Doing so allows us to perform a simple test for heteroskedasticity based on the likelihood-ratio test-statistic (see Greene (1993)). For the heteroskedastic tobit model we assume the variance to be of the form

$$
\begin{equation*}
\sigma_{i}^{2}=\sigma^{2} \exp \left(\gamma^{\prime} \mathbf{z}_{i}\right) \tag{2.5}
\end{equation*}
$$

where $\gamma$ is the vector of heteroskedasticity coefficients and $\mathbf{z}_{i}$ is vector of explanatory variables.

As Maddala (1983) notes, predictions in the tobit model can be based on three variants of the expected value of the response variable. The expected value of the latent variable - in our case potential savings - is simply given by

$$
\begin{equation*}
E\left(y_{i}^{*}\right)=\beta^{\prime} \mathbf{x}_{i} \tag{2.6}
\end{equation*}
$$

However, if we are interested in predicting observed savings we can either consider the expected value of $y_{i}$ conditional on the censoring (i.e. the mean of all positive $y_{i}$ 's)

$$
\begin{align*}
E\left(y_{i} \mid y_{i}^{*}>0\right) & =\beta^{\prime} \mathbf{x}_{i}+E\left(\varepsilon_{i} \mid \varepsilon_{i}>-\beta^{\prime} \mathbf{x}_{i}\right) \\
& =\beta^{\prime} \mathbf{x}_{i}+\sigma_{i} \times \frac{\phi(k)}{\Phi(k)} \tag{2.7}
\end{align*}
$$

or the unconditional expected value of $y_{i}$ (i.e. the mean of all $y_{i}$ 's, positive and zero)

$$
\begin{align*}
E\left(y_{i}\right) & =\operatorname{Pr}\left(y_{i}>0\right) \times E\left(y_{i} \mid y_{i}>0\right)+\operatorname{Pr}\left(y_{i}=0\right) \times 0 \\
& =\Phi(k) \beta^{\prime} \mathbf{x}_{i}+\sigma_{i} \times \phi(k) \tag{2.8}
\end{align*}
$$

where $k=\beta^{\prime} \mathbf{x}_{i} / \sigma_{i}$. In order to assess the predictive ability of our savings model we compare predicted savings with actual savings in the FES using all three of (2.6) to (2.8) above. Since we are primarily interested in predicting observed savings our natural
choice will be based upon either of (2.7) or (2.8), depending on which of these predicts savings more accurately. However, when we predict savings in the BHPS we draw on neither of these but instead take into account the condition that tariff income applies only between $£ 3,000$ and $£ 8,000$.

To implement the savings model described above we begin by adjusting each cross-section of the FES (1991 to 1994) so as to be comparable to the BHPS data sets at Stage 1 of our IS algorithm (i.e. the data as it is being read into the Eligibility Stage). This primarily involves the deletion of individuals living in Northern Ireland, all selfemployed individuals, those in further education (unless they are lone parents) and those aged less than eighteen. In addition, we also delete a number of observations in each of the FES cross-section samples with extreme outlying savings values (approximately 10 to 15 observations for each sample with weekly income from savings in excess of $£ 1,500$ ).

As concerns our choice of explanatory variables, we stress here that the aim is to keep the savings model as simple as possible. In addition to the problems of using crosssection data noted above, a major limitation stems from the fact that we are confined to a set of explanatory variables which are common to both the FES and the BHPS. In view of these problems our choice of explanatory variables is to some extent arbitrary. ${ }^{25} \mathrm{We}$ include age to capture perhaps some life-cycle consumption element and also various income elements (earnings, pensions income from a private scheme only, and rental income for property owners) all of which we expect to have a positive effect on savings. In addition, we include mostly socio-economic indicators of ownership of durable goods (if all three of a video recorder, freezer and washing machine are owned), home ownership, and the number of rooms which might act as a very rough proxy to housing value. ${ }^{26}$ Finally, we include the total number of persons in a household (larger families

[^29]might act as a greater incentive to save but might on the other hand represent a greater strain on resources) and widow status. Taken as a whole we might expect pensioners to have a greater stock of savings, particularly in the early years of pensioner status. Widow status on the other hand might be an indicator of greater hardship and thus a lower level of savings.

Some descriptive statistics for the explanatory variables in each FES data set are given in Table 2.4. There is little change in the composition of the samples between different years although we note a sudden increase in the average weekly income from private pension schemes in 1994. The results from the tobit regression are given in Table 2.5 for the standard tobit model and in Table 2.6 for the tobit model with heteroskedasticity. The parameter estimates are similar for different years with positive and significant effects for age, all income variables and ownership of durables (including house ownership). However, individuals who rent (from the private sector) and increasing numbers of persons in a household have a negative effect on savings. When we account for heteroskedasticity the effect on the parameter estimates is large although the general findings for the standard tobit still apply. In order to test the hypothesis of no heteroskedasticity against the alternative of heteroskedasticity being present (i.e. test $H_{0}: \gamma=0$ in the model described by (2.1), (2.2) and (2.5)) we perform a simple likelihood-ratio test based on the following test statistics: $L R(10)=1,818.2$ for the FES 1991 sample, $\operatorname{LR}(10)=1,893.4$ for the FES 1992 sample, $\operatorname{LR}(10)=1,720.2$ for the FES 1993 sample, and finally $\operatorname{LR}(10)=1,711.2$ for the FES 1994 sample. All of these test statistics are asymptotically chi-squared distributed (with degrees of freedom equal to the number of restrictions) and since $\chi_{10}^{2}=18.31$ ( 5 percent rejection level) they clearly fall well into the rejection region. Therefore we reject the hypothesis of no heteroskedasticity in favour of heteroskedasticity and consequently use the heteroskedastic tobit estimates for prediction purposes.

Before we turn to the prediction of savings in the BHPS, one immediate question we may want to address in brief, is how well our estimated models predict savings within the FES. Using the heteroskedastic parameter estimates of Table 2.6 we predict weekly income from savings using equation (2.6) to (2.8) above. In Table 2.7 we report the
actual and predicted proportions of individuals with savings and, in addition, some basic descriptive statistics for each FES cross-section. For actual savings we note that for each year the mean is considerably greater than the median so that the distribution of savings is positively skewed (as expected). Predictions based on (2.6) appear to be closest to actual savings but the predicted proportion of savers is far too small. In contrast, predictions based on observed savings (equations (2.7) and (2.8)) tend to overestimate the percentage of savers and savings on average. However, they still predict the distribution of savings to be positively skewed. ${ }^{27}$ Hence the FES evidence suggests that by basing our predictions on observed savings and particularly when conditioning on positive observations only, we overpredict somewhat not only the proportion of savers but also the average amount of savings held. This, of course, is of interest when predicting savings in the BHPS. If these findings hold in the BHPS we will, as a result, overestimate tariff income (in those cases where it applies) and accordingly underestimate the IS entitlement.

The method adopted for predicting weekly income from savings in the BHPS is as follows: instead of making direct use of one of the predictor equations (2.6) to (2.8) above, we condition total savings to lie in the tariff income generating range $£ 3,000$ to $£ 8,000$. Let total savings be $S=y(S) \times 52 / r$ and tariff income be $s=(S-3,000) / 250$, where $s$ is rounded to an integer, $y(S)$ is weekly income from savings, and $r$ is the average bank and building society interest rate. Since an individual is not eligible for IS if $S>£ 8,000$ and since $s=0$ when $S<£ 3,000$, our prediction of tariff income for each individual is based upon:

$$
\begin{align*}
E(s \mid S<8,000) & =\operatorname{Pr}(S<3,000) \times 0 \\
& +\operatorname{Pr}(3,000<S<8,000) \times E(s \mid 3,000<S<8,000) \tag{2.9}
\end{align*}
$$

and since

$$
E(s \mid 3,000<S<8,000)=\beta^{\prime} \mathbf{x}_{i}+\sigma_{i}\left[\frac{\phi\left(S_{l}\right)-\phi\left(S_{u}\right)}{\Phi\left(S_{u}\right)-\Phi\left(S_{l}\right)}\right]
$$

we have

$$
\begin{equation*}
E(s \mid S<8,000)=\left[\Phi\left(S_{u}\right)-\Phi\left(S_{l}\right)\right] \beta^{\prime} \mathbf{x}_{i}+\sigma_{i}\left[\phi\left(S_{l}\right)-\phi\left(S_{u}\right)\right] \tag{2.10}
\end{equation*}
$$

[^30]where $S_{l}=3,000-\beta^{\prime} \mathbf{x}_{i} / \sigma_{i}$ and $S_{u}=8,000-\beta^{\prime} \mathbf{x}_{i} / \sigma_{i}$.
Applying equation (2.10) to our BHPS samples and converting the figures into the four bands above produces the results in Table 2.8. In line with the FES evidence we tend to overpredict the proportion of savers in our samples. However, although we overpredict the proportion of savers in band (ii), we underpredict those in band (iii) which, to recall, is after all the band where tariff income applies. We cannot, with the available data, comment on whether the actual predicted savings are greater or smaller than those in the BHPS sample (since the BHPS contains only the four bands of savings income) and thus whether we underpredict or overpredict tariff income. Evidence from the FES suggests the latter, thus underpredicting the resultant IS entitlement. Nevertheless, when we turn our attention to the final entitled sample we find that in each wave between about 7 to 12 individuals have some tariff income according to our predictions. This translates to roughly 1.5-2.5 percent of the entitled samples which is remarkably similar to the official figures quoted in Section 2.2.2 above.

To conclude this section, we must accept that we are unable to model savings on an individual level with great accuracy. Despite these inaccuracies we believe our technique makes the most of the available information. However, to reiterate, official figures for actual recipients of IS suggest only a very small proportion of individuals with savings in the tariff income generating range and our savings imputation suggests the same for our BHPS sample. Any errors occurring in the computation of savings will result in an inaccurate figure for tariff income and hence an imprecise IS entitlement. Since tariff income cannot exceed $£ 20$, in those cases where earnings from work and other non-labour income are small or non-existent (as is the case for the majority of individuals entitled to IS) the resulting IS entitlement may well be distorted, but since Applicable Amounts are considerably greater than $£ 20$ it is unlikely that many misclassifications will result from inaccurately predicted tariff income.

### 2.3.2 Calculating Mortgage Interest

For those individuals who are mortgage holders at the time of applying for IS, the Applicable Amount covers only the interest part of a mortgage. The BHPS contains quite
detailed information on mortgages, such as the type, length and original amount of mortgage taken out, as well as the monthly repayment. However, the situation is complicated by the fact that only the interest part is covered and not any capital payment (in the case of repayment mortgages) nor any payments towards mortgage protection schemes etc. So, in the case of endowment mortgages (or interest only mortgages) the procedure is relatively straightforward: mortgage payments are interest payments and so these are covered excluding any insurance payments (such as towards a mortgage protection policy). The BHPS provides the means for us to identify whether an individual pays for certain mortgage insurance policies or not but we cannot ascertain the amount of the payment. The amount is unlikely to be particularly large, so that presently we simply add the weekly mortgage payment towards the applicable amount for endowment mortgage holders. Hence, we are likely to overestimate the Applicable Amount and (other factors unchanged) overestimate the IS entitlement for individuals with endowment mortgages.

For repayment mortgage holders (or principal/interest mortgages) the procedure of computing the amount of mortgage interest is more complex. We follow the approach of Coulter (1991) by firstly finding the outstanding mortgage debt for the year in question. This debt, in turn, depends on how large the outstanding debt is relative to the limit on the size of a mortgage eligible for tax relief (the MIRAS scheme). Throughout this section we adopt the following notation for the mortgage interest computation, denoted $m$ : the monthly mortgage repayment is $M$, the outstanding mortgage debt in year $l$ is $D_{l}$, the total length of the mortgage is $L, r$ is the average mortgage rate, $t$ the basic tax rate, $T$ is MIRAS tax relief, and finally $B$ is the limit on the size of the mortgage eligible for relief. The values for $B$ and $t$ can be found from the CSO's Inland Revenue Statistics (Table A.2, pp. 42) at $£ 30,000$ and 25 percent respectively for waves A to D. Since we can identify the month the household was interviewed we use the average mortgage rate one month prior to the interview from the CSO's Financial Statistics as shown in Table 2.9.28

[^31]The mortgage interest calculation occurs at the same stage of our IS algorithm as the savings imputation. Within these samples, between about 40 to 45 percent of households at each wave are mortgage holders, as shown in Table 2.10.29 Of these mortgages, roughly 70 percent are endowment mortgages and 25 percent repayment mortgages. The remaining 5 percent are either a combination of both types of mortgages or some other type of mortgage/loan. In these latter cases we adopt the same approach as for a pure endowment mortgage.

For repayment mortgages we compute the interest component according to the following principle. The mortgage payment can be expressed as the sum of the interest repayment and the principal repayment less any tax relief as:

$$
\begin{equation*}
M=r D_{l}+\left(D_{l}-D_{l+1}\right)-T \tag{2.11}
\end{equation*}
$$

We can thus compute the following value for $D_{l}$ :

$$
\begin{equation*}
D_{l}=(M / r+t B)\left[1-(1+r)^{l-L}\right] \tag{2.12}
\end{equation*}
$$

for the case where $D_{l} \geq B$. In those cases where $D_{l}<B$, the correct value for $D_{l}$ is given by

$$
\begin{equation*}
D_{l}=\frac{M}{(1-t) r}\left[1-(1+(1-t) r)^{l-L}\right] . \tag{2.13}
\end{equation*}
$$

The amount of tax relief, $T$, is given by the larger of the product $\operatorname{tr} D_{l}$ or $\operatorname{tr} B$, and the weekly mortgage interest payment is then

$$
\begin{equation*}
m=\frac{1}{4.33}\left(r D_{l}-T\right) \tag{2.14}
\end{equation*}
$$

in either of these two cases ( 4.33 is the standard BHPS week to month ratio). Finally, $m$ is added to the Applicable Amount in the Entitlement Stage.

In all of the above cases we have inherently assumed that mortgage holders are always MIRAS participants. In the BHPS we are unable to determine without additional procedures whether a mortgage holder is a MIRAS participant or not. In contrast, from the FES it is possible to determine the extent of MIRAS participation. For example, the FES 1991 suggests the majority of mortgage holders to be MIRAS participants with only

[^32]9 percent of mortgage holders not participating. Furthermore, CSO Inland Revenue Statistics suggest that for 1991-92 there were 9.55 million beneficiaries of MIRAS whereas the Council of Mortgage Lenders' Housing Finance Statistics suggest a total of 9.815 million outstanding mortgage loans at the end 1991. Although these figures are not directly comparable (the Inland Revenue Statistics consider single individuals or one member of a couple whereas the Housing Finance Statistics consider only the total number of outstanding mortgages) they give a rough indication that overall, the majority of mortgage holders by 1991/92 and onwards appear to be MIRAS participants.

This completes our savings imputation and mortgage interest calculation and thus the description of our IS algorithm. In the subsequent sections we discuss and attempt to assess the validity of the output generated by the IS algorithm. To summarise, both the savings imputation and the mortgage interest calculation are sources of error in computing the IS entitlement. However, the evidence suggests that, on average, the former is likely to underestimate the IS entitlement whereas the latter is likely to overestimate the IS entitlement in those cases where the computations apply.

### 2.4 The IS Algorithm Output

The various stages of our IS algorithm give rise to distinct samples and these are shown in Table 2.11. As the data pass through successive stages of the IS algorithm the sample sizes are gradually reduced. In waves C and D the eligible and entitled samples are rather less than at waves A and B. As a proportion of the main samples at Stage 1, roughly 35 percent of the wave $A$ and $B$ samples are retained for the eligible sample and 18-19 percent for the entitled sample, whereas for the wave C and D samples the respective figures are lower, at about 28 percent for the eligible sample and 16-17 percent for the entitled sample. Much of this decline in sample sizes can perhaps be attributed to the more stringent eligibility rules from wave $B$ onwards (the full of effect of these may have taken some time to become perceptible). Nevertheless, for all four waves between 50 to 60 percent of the eligible samples are judged to be entitled to IS.

In Table 2.12 we turn our attention to the eligible samples with corresponding positive and negative IS entitlements (recall that the IS entitlement cut-off occurs at
 reported in the BHPS. For non-pensioners with positive IS entitlements the majority are recipients (i.e. entitled recipients) whereas for those with negative IS entitlements only relatively small numbers report IS receipt (i.e. non-entitled recipients). ${ }^{30}$ For pensioners though, the number of entitled non-recipients is considerably greater than the number of entitled recipients at each wave. This, together with additional evidence we cite later, strongly suggests that we are unable to generate particularly accurate results for pensioners. Consequently the ensuing investigation of take-up amongst individuals entitled to IS will centre primarily on the entitled non-pensioner samples.

In the remainder of this section we discuss the output generated by our IS algorithm principally in terms of take-up rates. Therefore we begin our discussion with a brief exposé on computing take-up rates and follow this with a description of the output.

### 2.4.1 Take-Up Rates

The caseload take-up rate has become the standard measure for assessing the proportion of IS recipients to the total number actually entitled to IS. ${ }^{31}$ As such it provides a convenient standard for measuring the extent of participation in a social security program such as IS. Moreover, it allows us to compare the computed take-up rates derived from our IS algorithm with officially produced estimates by the DSS (and other estimates for that matter) thereby providing some indication of the accuracy of our model.

The basic caseload take-up rate is defined by

$$
\begin{equation*}
T U 1=\frac{R}{R+E N R} \tag{2.15}
\end{equation*}
$$

where $R$ gives the total number of IS recipients and $E N R$ gives the total number of entitled non-recipients. The recent literature, however, has begun to highlight the shortcomings of this simple take-up rate. Take-up as measured by (2.15) is likely to be

[^33]inaccurate since both $R$ and ENR are subject to errors (as discussed throughout this chapter). Fry and Stark (1989 and 1993) claim that by including non-entitled recipients in $R,(2.16)$ is misleading, particularly since it overestimates the true take-up rate. ${ }^{32}$ They suggest a more 'natural' measure of true take-up given by
\[

$$
\begin{equation*}
T U 2=\frac{E R}{E R+E N R} \tag{2.16}
\end{equation*}
$$

\]

where $E R$ gives the total number of entitled recipients. However, Duclos (1992a\&b) argues that neither (2.15) nor (2.16) are appropriate measures of true take-up. In fact, both underestimate the true take-up rate: (2.15) since its denominator includes nonentitled recipients and (2.16) even more so since it also makes the status of recipiency conditional on being entitled to IS. Duclos suggests what he believes to be a better estimate of take-up defined by

$$
\begin{equation*}
T U 3=\frac{R}{E R+E N R} . \tag{2.17}
\end{equation*}
$$

In the case where one does not have the benefit of an econometric model of take-up Duclos argues that (2.17) is the best measure of true take-up. In those cases where one does have a (specific) econometric model of take-up, the most accurate measure of takeup is obtained by using this model to predict (i) the number of claimants and (ii) the number of entitled individuals, and to thereupon compute a take-up rate as the ratio of (i) to (ii). One problem with this latter approach is that it relies on an econometric model specifically constructed for the task in hand. Since we do not replicate the econometric model of Duclos we are unable to compute these latter take-up rates.

Throughout the remainder of this chapter we will base our take-up rates on equation (2.15) above. For our purposes either of (2.15) to (2.17) would suffice since we are primarily interested in relative take-up rates between different groups of individuals in our sample and not in precise absolute measures of take-up. However, when we come to

[^34]compare our take-up estimates with official DSS estimates we make use of all three of the above measures.

### 2.4.2 IS Take-Up Evidence From the BHPS

The computed IS take-up rates using (2.15) from above are given in Table 2.13 by family type and are reproduced graphically (for non-pensioners only) in Figure 2.11. In line with the existing evidence (Fry and Stark (1993) and DSS (1994c, 1995b\&c)) we find the highest take-up rates for lone parents. However, these studies suggest the lowest take-up rates for singles but in our case couples without children have the lowest take-up rates. Our findings for this latter group are based on very small samples and therefore must be treated with caution.

We are unable to detect a time trend for take-up. For singles, for example, take-up increases for the first three waves but then falls by the fourth wave. Note also that takeup varies considerably more for couples than either for lone parents or singles. Most of this variation in take-up is probably due to the relatively smaller sample sizes for these groups. In addition, we must bear in mind that for part of our samples we are in fact pursuing the same individuals over time. Increasing take-up rates from one wave to another might simply capture a greater likelihood to claim as time progresses. ${ }^{33}$

Pensioner IS take-up based on our estimates is very low indeed. According to our algorithm we estimate a substantial number of entitled non-recipients, particularly when compared to the number of entitled recipients. The problems associated with estimating take-up for pensioners using survey samples is well documented (see, for example, Atkinson (1984)). The root of the problem can be traced to the fact that the number of pensioners who report IS receipt falls well below the official recipient figures. One of the reasons cited for this is benefit confusion: since the NI state pension and IS are paid in the same order book there is often a degree of confusion about the composition of the total payment, and when questioned about benefit receipt pensioners are thus more liable to misreport.

[^35]The shortfall in pensioner recipient numbers in the FES has been so acute in recent years that the Institute for Fiscal Studies, in its latest take-up report (Fry and Stark (1993)), do not provide any analyses at all for pensioners. The DSS on the other hand who produce the official take-up estimates - make special adjustments to their take-up estimates. These adjustments also centre around the fact that the number of IS recipients in the FES falls short of those recorded in their administrative statistics (often by up to 50 percent, or close to 500,000 individuals). Although there is no direct evidence of the quality of data (with respect to IS receipt amongst pensioners) in the BHPS there is, of course, no reason to believe that the situation is any better for the BHPS. However, further work is still required. In order to assess BHPS data by comparison with official statistics, the appropriate samples need to be grossed-up. To our knowledge no such work exists yet and thus the strongest evidence of pensioner under-reporting of IS receipt is provided by our very low take-up rates. ${ }^{34}$ As a result of these problems we exclude hereafter pensioners from our discussion of the take-up evidence provided by our IS algorithm and present results for non-pensioners only .

In Table 2.14 take-up rates by tenure type are shown (see also Figure 2.12 for a graphical display). Individuals who own their homes outright or mortgaged have notably lower take-up rates than those who rent their homes. However, there is very little noticeable difference in take-up between those who are local authority (or housing association) tenants and those who rent from the private sector. The lower take-up rates for home owners might reflect the greater stigma associated with claiming for this group of individuals.

Take-up rates by relationship to the head of household are given in Table 2.15 (and also in Figure 2.13). Heads of household and non-relatives have, on average, the largest take-up rates whereas (non-dependent) children have the lowest. This probably reflects some element of benefit sharing between family members within a household.

[^36]Finally we consider take-up by employment status in Table 2.16 (and Figure 2.14). We observe particularly high take-up rates for the unemployed and those in family care and comparatively low take-up rates for the early retired. Take-up rates for the latter also tend to vary quite considerably across waves. One of the reasons for lower take-up rates for the early retired may be that they have small sums of redundancy payments, lump sum payments or some savings which they might prefer to draw on in the short term before becoming dependent on IS. This could also account for the somewhat lower takeup rates for the sick and disabled. Likewise, lower take-up amongst the employed might be explained by a more widespread perception amongst this group that their are able to cope without benefits. In addition, the early retired, sick and disabled with some capital and the employed might not actually be aware of being entitled to IS despite their limited assets.

The above findings based on the output generated by our IS algorithm compare favourably with the existing evidence, particularly that of Fry and Stark (1993) who provide the most extensive take-up evidence to date. However, their analysis of take-up concentrates mostly on Supplementary Benefit and extends only to the first year of the IS program. ${ }^{35}$ As such, their findings are not always comparable to our evidence on more recent IS take-up. Finally, we have speculated on the factors which may explain relatively lower or higher take-up rates amongst some of the groups considered above. A full understanding of the factors which determine take-up can only be obtained from an econometric investigation of take-up. Such an investigation follows in Chapters 4 and 5 of this thesis.

### 2.4.3 Checking the Output

Throughout this chapter we have repeatedly drawn attention to the fact that our computed IS entitlement is subject to various 'measurement' errors and as a consequence, our entitled sample may contain individuals who are not actually entitled to IS. Similarly, we may exclude from our entitled sample individuals who are actually truly entitled to IS but

[^37]who, as a result of possibly inaccurate or insufficient data, are incorrectly deemed to be not entitled to IS. Moreover, various reporting errors in the BHPS may give rise to inexact and misleading figures with respect to the number of IS recipients. The majority of these errors arise for reasons beyond our control, and are an inevitable problem in an exercise such as ours, since we must work with a data set which was not constructed for the purposes of analysing take-up behaviour. These errors are of interest to us since they have the potential to affect the output generated by our IS algorithm in such a way that any results based upon the model can be highly misleading about the nature of IS take-up in Britain.

In Section 2.4.2 above we have already concluded that data on pensioners in the BHPS is too imprecise for us to deduce any meaningful conclusions on pensioner take-up behaviour. Here we return to this issue again by providing further evidence. We assess the output generated by our IS algorithm both for pensioners and non-pensioners by comparing our computed take-up rates with those from official sources. In addition, we highlight the scope for measurement error in the IS entitlement, an important issue to which we devote an entire chapter later in this thesis (see Chapter 4).

When comparing our take-up estimates with the official figures produced by the DSS there are two problems of incompatibility which must be borne in mind. Firstly, the DSS use different data sources in constructing their estimates and, secondly, these estimates are based on figures which are differentially grossed-up and thus corrected for a variety of sampling errors. Grossing-up data is a standard technique for adjusting results based on survey samples so as to be representative of the entire population (see our discussion on pensioners in Section 2.4.2 above). Some sections of the target population are likely to be under-represented or over-represented and differential grossing-up techniques take this survey deficiency into account. Since, to our knowledge, no work on grossing-up has yet been done for the BHPS we are unable to gross-up our samples. However, the fact that we do not gross-up our samples should not affect our computed take-up rates to a large extent, except in the case of pensioners.

With respect to first point above, the DSS make use of two data sources for their take-up estimates: (i) the FES and, more recently, the Family Resources Survey (FRS) to
estimate the number of entitled non-recipients, and (ii) DSS administrative data (namely the IS Annual Enquiry) for the total number of recipients. They claim that since this procedure uses survey data only to estimate the number of entitled non-recipients, it provides more accurate take-up figures as compared to methods which rely solely on survey data sets. ${ }^{36}$ This is, of course, debatable since two data sources might actually increase the extent of errors to occur in any calculations of take-up (see Atkinson (1984)).

The standard caseload take-up rate as calculated by the DSS is based on $T U 1$ of equation (2.15) above where $R$ is estimated from the IS Annual Enquiry and ENR from either the FES or the FRS. However, in recent years the DSS has become increasingly concerned about the impact of many of the errors discussed above on simple take-up rates such as $T U$ 1. In particular, the work by Duclos cited above - which has paid specific attention to the way in which errors affect take-up rates - has influenced the approach the DSS now adopts for the estimation of take-up rates. For estimates from the early 1990s onwards they report broad ranges of take-up reflecting the impact of a variety of errors on take-up rates (see Harris (1994) for a detailed exposition of the new DSS method). The new take-up ranges produced by the DSS give a lower bound (i.e. a 'worst case' scenario) and an upper bound (i.e. a 'best case' scenario) with the possibility that the true take-up rate can lie anywhere between the two limits. The use of these ranges is rather limited by the fact that it is not possible to say where within the take-up range the true take-up rate precisely falls.

Nevertheless, in spite of these apparent incompatibilities between the DSS take-up estimates and our own take-up rates, we believe that meaningful comparisons are still obtained by comparing basic caseload take-up rates as in (2.15) to (2.17) above with the DSS take-up ranges. ${ }^{37}$ The whole issue of computing take-up rates is of interest in itself and has received much attention in recent years (particularly the seminal work both by Atkinson and Duclos cited above). However, it is not the aim of this thesis to provide more accurate take-up measures or to contribute to the analysis of take-up figures per se.

[^38]What we aim to do is to accept the figures as they stand and to incorporate the measurement error in their computation into an analysis of the factors which determine take-up. Further work on take-up estimation using the BHPS is no doubt still required and is likely to provide a further useful source of take-up estimates.

Before we compare take-up rates, consider the graphs in Figures 2.3 to 2.10 where we have plotted for each wave all computed IS entitlements (in £ per week) for the eligible samples of our IS algorithm (i.e. each individual in the eligible sample has one observation). ${ }^{38}$ The solid line indicates the IS entitlement cut-off at $£ 0.10$ so that those individuals who fall above the line are entitled to IS and those who fall below it are not. For non-pensioners we note a clustering of observations around the $£ 40$ to $£ 50$ mark. This corresponds to the IS entitlement for a single individual with no mortgage interest to cover and with no other income or very small amounts thereof. In stark contrast, for pensioners there are large clusters of observations around and especially just above the IS entitlement cut-off. In other words, for many of the pensioners in the eligible sample the Applicable Amount is very close to their income (this is because the basic pensioner IS allowance is very close to the basic NI state pension level). Hence even relatively small measurement errors can easily tip the balance between being entitled to IS and not being entitled to IS. According to the evidence of Tables 2.12 and 2.13 discussed previously (see Section 2.4.2) it appears that our IS algorithm program overestimates the number of entitled pensioners who are non-recipients. One of the reasons suggested was that, in line with the FES, pensioners tend to underreport IS receipt. A further reason re-enforcing this argument may well be that in a notable number of cases the measurement errors act in such a way that they tend to tip the balance in favour of entitlement (for example, by underestimating tariff income from savings for pensioners). This, together with the

[^39]evidence cited previously, once again suggests strongly that our results on pensioners are likely to be misleading and inaccurate in many cases.

A number of non-pensioners also have IS entitlements very close to the IS cut-off (i.e. their total income is close to their Applicable Amount). As stated above, the situation for non-pensioners is by no means as sensitive to measurement errors as that for pensioners but nevertheless some individuals are likely to be misclassified according to the IS entitlement.

Finally, we turn to the comparison of our take-up rates with the official DSS takeup ranges discussed above. In Table 2.17 we present three different take-up rates according to equations (2.15), (2.16) and (2.17) respectively and the corresponding DSS take-up ranges. The estimates are reproduced in Figure 2.15 for non-pensioners and Figure 2.16 for pensioners. As foreseen, for both non-pensioners and pensioners, TU 2 yields an estimate of take-up that is less than $T U 1$, whereas $T U 3$ is greater than either of the other two. Compared to the official take-up ranges, our estimates fare rather well for non-pensioners but, in line with all our previous evidence, our pensioner take-up estimates fall a good 20 to 30 percentage points below even the lower end of the DSS take-up range. In comparison, for non-pensioners $T U 1$ and $T U 2$ fall only about 5 to 10 percentage points below the lower end of the DSS range whereas TU 3 actually falls within the DSS range for each of the years considered. In Section 2.4.1 above it was shown that - based on the work of Duclos - TU 3 is the best take-up estimate in our exercise, in the sense that it is the estimate of take-up which comes closest to the true take-up rate. Therefore, it is reassuring to find that this estimate actually falls into the official take-up range.

So, in spite of the methodological differences in computing take-up rates, the overall impression gained is encouraging, especially for non-pensioners. For pensioners, on the other hand, we are forced to follow the trend of Fry and Stark (1993) who, for similar reasons, altogether abandon any analysis of take-up amongst pensioners in the FES. It thus remains doubtful whether future research into take-up behaviour by pensioner households will be fruitful unless a greater effort is made to improve the available data at the primary collection stage.

### 2.5 Conclusions

In this chapter we have shown, in some detail, how we have gone about constructing a IS algorithm for IS in Britain. Thereby we have outlined the key rules relating to IS eligibility and the final selection of individuals into the entitled IS sample. This has been an extensive exercise: IS is one of the social security benefits in Britain administered by a rather lengthy and often complex set of rules and regulations which are frequently subject to subtle changes. As a result, one of the main deficiencies of any attempt to mimic these rules and regulations is the difficulty involved in following all of them in every aspect. However, in the majority of cases determining an individual's eligibility and entitlement are comparatively straightforward. The required information is generally contained in the BHPS, even though it may be subject to some degree of measurement error. Provided these measurement errors occur at random and are relatively small in magnitude the major consequence is a computed IS entitlement which is also subject to measurement error.

Problems arise in those cases where special rules are invoked and, due to a lack of suitable variables or proxies thereof, we are unable to follow these rules. Such cases though are unlikely to occur frequently in our samples. Nevertheless, both measurement errors and misclassifications are issues of concern when computing IS entitlements for a sample of individuals. Throughout this chapter we have argued that the former represents more of problem than the latter and therefore, in this thesis, we pay attention to the impact of measurement error on modelling IS take-up in Chapter 4.

Our work represents the first analysis of take-up using the BHPS as its data source. In spite of its imperfection as a suitable data set for the analysis of take-up (most notably the lack of detailed information on savings and also its, as yet, unexplored reliability as a data source) the BHPS provides two clear advantages over the FES: firstly, we have at our disposal a much larger set of socio-economic variables on individuals' characteristics and, more importantly, it allows one to introduce a dynamic element to the take-up decision by providing the researcher with a longitudinal element. The latter of these provides a particularly exciting and new avenue for research on take-up. We exploit the panel data structure of the BHPS together with the output from our IS
algorithm in Chapter 5 where we attempt to shed new light on the hitherto static evidence on take-up.

Finally, we have shown the output generated by our IS algorithm to be much in line with the existing data on take-up. The take-up estimates produced from the output of our IS algorithm measure up well to the officially produced estimates by the DSS. Clearly, further work is still required on the BHPS estimates, especially on grossing-up the BHPS data and on the various problems associated with pensioner receipts. However, on the whole, the BHPS has shown itself to be a satisfactory data source for an analysis of take-up, and as such provides an interesting alternative data source to the FES providing not only alternative take-up estimates but also the potential for a dynamic take-up analysis.

## CHAPTER 3

## MODELLING THE TAKE-UP OF INCOME SUPPORT: AN ECONOMIC APPROACH

### 3.1 Introduction

This chapter outlines a basic microeconomic model of the decision to take-up or not to take-up a means-tested social security benefit. We begin by considering a simple oneperiod static model with von Neuman-Morgenstern uncertainty about the outcome of a claim. Thereafter we construct two types of simple dynamic models in which the current decision to take-up or not is affected by (i) any past experience of claiming the benefit in question, and (ii) expectations about future financial circumstances. Both the static and the dynamic models constructed form the economic backbone to the subsequent econometric analysis in the ensuing chapters 4 and 5.

There has been considerable work in analysing the decision to participate in a social security program both from economists and (usually in the form of special surveys) from sociologists/psychologists. ${ }^{1}$ To gain a better understanding of the problem of non-take-up it is important to consider the work of both economists and sociologists/psychologists which we attempt to do in what follows. However, we are particularly interested in the way that economic theory can provide a better understanding of the non-take-up problem. Hence our emphasis will fall on microeconomic approaches to the problem of non-take-up by focusing on the individual decision-maker and the way they decide about making a claim for benefit or not. Such models are able to explain
why, in the presence of what we currently refer to as transaction costs, some individuals might refuse an increase in their disposable income. ${ }^{2}$ The models developed in this chapter, in turn give rise to econometric models that utilise the data from the previous chapter. The aim of such an econometric analysis is to examine the factors that determine take-up or non-take-up.

The decision to claim a social security benefit by an individual and the factors that affect this decision can be regarded as a sequence of passing a certain set of key criteria. Figure 3.1 (see Appendix 3) roughly illustrates these criteria: to begin with, an individual has to be aware of the actual existence of the benefit. Only then can she decide whether or not to claim. In the case where an individual decides to claim the government agency will assess the benefit application and determine whether the claimant will be granted the benefit or not. For means-tested benefits (which are of particular interest in our analysis), the government agency must decide on two key decision criteria: first, whether the individual is eligible to the benefit and if so, whether a positive benefit entitlement can be established for that individual. Only when both criteria are satisfied will the government agency grant the means-tested benefit to the individual. ${ }^{3}$

Hence, in this analysis three main agents are involved: the individual claimant who decides whether or not to claim, the government agency (the DSS in the UK) who decide whether or not an individual should be granted the benefit, and finally the analyst who models these decisions (particularly those of the individual claimant) and estimates these models with the aid of a suitable data set. This chapter concentrates on the individual claimant's take-up decision and how this decision can be accommodated within a rational utility maximising framework. ${ }^{4}$ Throughout our analysis we assume that

[^40]individuals have some basic knowledge of the benefit they are entitled to and, in addition, we also assume that if they decide to claim the government agency will grant the benefit (since they are entitled to it). The analyst is unable to directly observe all stages of the claiming process and is thus forced to use available data, often subject to considerable inaccuracies. Hence we will highlight the difficulties in modelling the take-up decision in view of the scope for errors in computing the benefit entitlement. These issues will ultimately have to be taken into account when constructing an econometric model of the take-up decision.

The chapter as a whole addresses general issues encountered in modelling take-up. Section 3.2 briefly reviews the contributions of the socio-psychological works on take-up with particular emphasis on the threshold model by Kerr. Thereafter we review the main microeconomic contributions to the take-up problem in section 3.3. The emphasis is on a handful of models which have formed the theoretical foundations to most econometric studies. In section 3.4 we briefly consider the three agents involved in the analysis of take-up building up a very basic framework for the decision-making process. This is followed by section 3.5 in which we expand the notion of transaction costs of claiming. We construct a model of benefit stigma in an attempt to capture the effect changes in various exogenous variables have on the level of stigma. The model of take-up follows in section 3.6. The static and both dynamic models of take-up are presented. The static model underlies the ensuing econometric analysis using cross-section aspects of the BHPS only (Chapter 4) whereas the two-period model underlies a panel data approach (Chapter 5). We end with concluding remarks in section 3.7.

### 3.2 Take-Up Studies Outside of Economics

In this section we consider socio-psychological models that have been formulated in an attempt to single out those decisive factors that make an individual a claimant or a nonclaimant. The work on these models stems mostly from special surveys on particular groups of claimants. The models are not built into a strict economic framework but instead, try to analyse individual behaviour on a socio-psychological level. Special surveys are particularly important in analysing certain groups of individuals that are
underrepresented in the Family Expenditure Survey (FES) which thus far has been the main source in analysing take-up behaviour (e.g. the self-employed, those not living in households).

The main model we shall focus upon is Kerr's (1982) threshold model. To our knowledge, this model is one of the earlier attempts at modelling individual take-up behaviour in a socio-psychological setting. Kerr relates the model to the claiming behaviour of pensioners with respect to supplementary pensions. He proposes that in order to make a claim an individual must pass a consecutive set of thresholds. Each threshold in turn represents a distinct reason not to claim the benefit. Thus, if for any reason an individual should fail to pass any one of the thresholds no claim will proceed. The thresholds which form the core of Kerr's model, represent what to him are the decisive factors influencing an individual's claiming decision. They are as follows:

1. an individual's perceived need,
2. a basic knowledge of the existence/name of the benefit,
3. an individual's perceived eligibility to the benefit,
4. an individual's perceived utility gain as a result of claiming and receiving the benefit,
5. an individual's beliefs and feelings about the benefit/application procedure,
6. an individual's perceived stability of his/her situation at the time of applying and in future.
The important point is that each stage of the model must be passed in successive order and failure at any one of 1 . to 6 . implies no claim from that particular individual. The core of the model is formed by the fifth threshold. That is, an individual's decision to claim crucially depends firstly, on the strength of his or her belief that applying for a benefit will lead to certain consequences and secondly, on the feelings about these consequences. These consequences take the form of the 'hassle' and 'stigma' attached to claiming so that these factors appear to be the key elements in the model.

Kerr's model formed a decisive step forward in analysing claimants' behaviour by firstly constructing an analytical framework that incorporated the different stages of decision making and secondly, by providing greater understanding of the underlying
process of making a claim. Hence an insight is provided as to why, for example, certain individuals who decide to apply for a benefit do not necessarily take-up the benefit in question. However, innovative as it may be, the model has a number of shortcomings. Craig (1991) points to the fact that the practical evaluation of the model strongly depends on the belief that participants have replied truthfully and correctly. It is a known fact that pensioners surveyed in data sets (such as the FES, for example) are a particularly difficult group to extract reliable data from (see Atkinson and Micklewright (1983)). The model is also restrictive in that Kerr constructed and tested it with pensioners in mind and the result applicable to this subgroup of the population cannot necessarily be extrapolated so as to apply to other groups of persons. The question that obviously springs to mind is how well the model would perform with persons other than pensioners. Furthermore, Kerr assumes that the questions in themselves closely address the issues represented by each threshold. In other words, the questions are thought to mirror the thresholds. Both of these issues are directly addressed in a quite substantial literature that builds on the threshold model concept devised by Kerr (see Craig (1991) and more recently Corden (1995) for summaries of the literature). Table 3.1 (see Appendix 3) presents the essential results as summarized by Craig. These results suggest that the key factors in determining an individual's decision to claim are either the perceptions of needs/eligibility or a combination of basic knowledge of the system and perceived eligibility.

A further extension would be to take into account the fact that many claiming decisions are not necessarily made on an individual level but rather as a household unit thereby placing a stronger emphasis on the social surroundings of claimants. Buckland and Dawson (1989) provide such an extension in addition to considering the time-lags involved in claiming procedures.

One study we shall focus on is Davies and Ritchie (1988) who attempt to judge the predictive strength of the model, for means-tested benefits other than supplementary pensions, by subjecting the model to a 'real-life' test. 5 Whereas Kerr assumed that basic knowledge and perceived eligibility existed amongst the sample, Davies and Ritchie do

[^41]not make this assumption. ${ }^{6}$ The test was conducted on a sample of 119 current entitled non-claimants: 20 were predicted to be claimants and 99 non-claimants. With the aid of a series of questionnaires their findings suggest the main obstacle to a claim being an individual's perceived eligibility to the benefit. Subsequent examination of the 119 individuals in the test revealed a total of 99 correct predictions. However, the model is better at predicting non-claimants ( 91 percent correct predictions) than claimants (45 percent correct predictions). In addition, the model's predictive strength appeared to be worse for housing benefit (HB) claims than for either of the other two benefits (only 30 percent of HB claims were correctly predicted). Consequently Davies and Ritchie conclude that although the model has specific strengths in predicting non-claimants better than claimants, the six threshold factors, as suggested by Kerr, do capture the key decision-making hurdles faced by individuals in the claiming process. In fact, their study manifests the key obstacles to claiming as being (i) the lack of perceived need, (ii) individuals' uncertainty about their eligibility, and (iii) the stigma (in the form of negative feelings/beliefs) attached to the claiming process. The analysis of the claiming process is complicated in that, in many cases, various factors tend to interact so that the decision not to take-up a benefit does not solely depend on one factor alone. Hence, any attempts to increase the take-up of means-tested benefits will have to focus on tilting the balance between the positive and negative factors resulting from a claim in such a way, so as to ensure that the claimant will experience an overall positive outcome.

The study finally suggests practical measures that could encourage take-up. These include the provision of further information and advice (in the form of publicity campaigns or welfare agencies) not only from government agencies but also in the shape of informal networks (for example, through neighbours, colleagues, doctors). Such measures would increase awareness and certainty of benefits. These kind of measures would also appear to be particularly effective if they were targeted at those population groups that suffered from very low take-up rates.

[^42]The more recent literature outside of economics has begun to concentrate on the supply side of the claiming process. That is, rather than placing the emphasis of non-take-up on the decision made by an individual claimant (i.e. the demand side) as most other studies have, recent work by Corden (1995) considers non-take-up as a result of failing the first step of Figure 3.1 above. She considers the effects of the administrative system on information provision, service provision and the application procedure itself. However, as noted in the introduction, the economic framework which we develop to study the non-take-up problem does not account for these supply side aspects. The lack of information about benefits as a determinant of non-take-up is an issue we will return to in the next section.

Finally, an interesting study about non-take-up of Family Credit using a unique data set is described by Marsh and McKay (1993). ${ }^{7}$ Unlike many other studies outside of the economics literature the data set they utilise is rather large ( 2,200 low income families in Britain surveyed in 1991). In addition, they re-interviewed a small subsample (122 couples) of the original sample two years after the original interviews were performed (the detailed findings from the re-interviewed sample are described in McKay and Marsh (1995)). Their findings are generally optimistic about the Family Credit scheme. In spite of finding that a point estimate of take-up at a single point in time is rather low ( 64 percent at the 1991 survey), only a small proportion of the re-interviewed sample remained eligible but not in receipt of Family Credit (on average about 15 percent). These results are particularly interesting in view of the fact that they are, to our knowledge, the only published longitudinal data on take-up behaviour. The data set we utilise later on in this thesis (see Chapter 5) are the first to provide longitudinal evidence of Income Support (IS hereafter) take-up patterns.

[^43]
### 3.3 The Economic Literature on Take-Up

The micro-economic literature on social security participation has primarily focused on the individual claimant. ${ }^{8}$ In such a framework the decision of an individual to take-up a benefit can be viewed as choice under uncertainty. An individual applying for a benefit faces (i) the uncertainty as to whether the government agency (the DSS) will judge them to be entitled to the benefit and (ii) the uncertainty about the actual level of entitlement. Given these forms of uncertainty, an individual will either claim or not claim the benefit depending on the utility of one decision outweighing the utility from the alternative. ${ }^{9}$

Whatever course of action the individual decides to pursue, either act gives rise to certain consequences: the claim is either successful or unsuccessful. The DSS decides upon the outcome of the act on the basis of the information provided by the individual. It will either judge the claimant as being entitled or as not being entitled, correctly or incorrectly. These decisions by the DSS can be viewed as the states, $S$, unknown to an applicant at the time of applying. Hence we can think of an act, $A$, as a function that assigns to each state a consequence such that $A=1$ if the claim occurs and $A=0$ if no claim occurs. The resulting states, are then $S=1$ if the DSS accepts the claim correctly, $S$ $=2$ if the DSS rejects the claim correctly, $S=3$ if the DSS accepts the claim incorrectly, and $S=4$ if the DSS rejects the claim incorrectly. The consequences can then be illustrated as follows:

|  |  | State |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  |  | $\mathbf{1}$ | $\mathbf{2}$ | $\mathbf{3}$ | $\mathbf{4}$ |  |
|  |  | $\mathbf{0} t$ | $N$ | $N$ | $N$ |  |
| $N$ |  |  |  |  |  |  |
| $\mathbf{1}$ | $B$ | $N$ | $B$ | $N$ |  |  |

where $B$ denotes the receipt of benefit and $N$ denotes no receipt. An individual receives the benefit, $B$, only when a claim has occurred $(A=1)$ and the DSS has granted that

[^44]person a positive entitlement ( $S=1$ or 3 ), correctly or incorrectly. All other consequences are denoted $N$ since no benefit is paid in these cases.

A general model can be formalized further if we consider the utility from the claiming process in the following way: index the states by $S=1,2,3,4$ and assume that if $S$ occurs then a vector of consequences, given by $C(., S)$ results. The particular element of $C(., S)$ will depend on the act, $A$, chosen by the individual. Henceforth we can specify the utility function

$$
\begin{equation*}
u=f[C(1,1), C(1,2), C(1,3), C(1,4)] \tag{3.1}
\end{equation*}
$$

giving the utility of the outcome of making a claim.
Furthermore, the utility function can be extended by attaching probabilities, given by $\pi_{1}, \ldots, \pi_{4}$ such that $\sum_{i=1}^{4} \pi_{i}=1$, corresponding to each state. These are subjective probabilities attached to the outcome of each state by the individual. We then have the expected utility from a claim given by

$$
\begin{equation*}
E u=g\left[\sum_{i=1}^{4} \pi_{i} u\right] . \tag{3.2}
\end{equation*}
$$

It is important that the model can account for the fact that the decision to make a claim by an individual involves an element of transaction cost or disutility. This cost/disutility arises from those factors that are likely to cause a loss in utility as a result of claiming (particularly in the case where a benefit is subject to a means-test). These cost factors can be viewed essentially as the claiming hurdles in Kerr's model discussed above and can be classified as falling into one of the following: (i) the stigma resulting from an individual's feelings and beliefs about the benefit, (ii) the hassle of claiming itself (in the form of having to fill in application forms, presenting oneself to an assessment interview and so on), and (iii) a lack of knowledge of the actual benefit in question or being unaware of an entitlement to that benefit. Any factor that presents some form of utility loss to an individual deciding to claim for a benefit should be captured by the model. These factors are succinctly summarised in the following:
"There are several possible reasons for non-take-up. People may be unaware of the benefit. They may be aware of its existence but believe that they are not
eligible. This may happen, for example, when they had previously claimed and been deemed 'ineligible', but there has been a subsequent uprating which makes them eligible. People may be aware of their eligibility but not claim on account of the costs of doing so, ... including any loss of dignity ('stigma'). They may claim but their claim may be rejected through administrative error." (Atkinson (1984, p.192))

Underlying most micro-models of take-up is the simple concept that for a claim to occur, the utility from claiming less any transaction costs must exceed the utility from not claiming. The advantage of such simple models is that they readily yield themselves to an econometric investigation in the form of a random utility model. In the following we review the main contributions to this basic model which we too will adopt later on.

### 3.3.1 A Review of the Literature

To our knowledge there is no complete review of the take-up literature to date. The subject matter covers a range of disciplines and we have attempted to give a brief outline of the socio-psychological literature above (as noted, a more detailed exposition can be found in Craig (1991) and Corden (1995); see also van Oorschot (1991) for a Europeanwide review). As outlined in the introduction our emphasis falls on economic modelling of take-up, so that we concentrate here on a small number of major economic contributions which have lead the way in this field. The basic models of the take-up decision are simple and as such adequately explain the decision to take-up. We believe that such models are sufficient in our analysis and many of the ensuing empirical works on take-up, including our own, draw on the main models presented below. ${ }^{10}$

One of the earliest microeconomic models of take-up, adopting the above approach, was presented by Moffitt (1983) in his analysis of the stigma resulting from a benefit claim. Uncertainty on behalf of the claimant as to their eligibility for the benefit and uncertainty about the level of benefit are not accounted for in the model. The key emphasis is on the way that stigma enters as a transaction cost in deciding to take-up a benefit. The stigma from claiming can be independent of the level of benefit receipt (the

[^45]flat component of stigma) or it can vary with the benefit level (the variable component). Moffitt's approach is to consider functional forms of utility before and after a claim has occurred. Thus non-benefit utility is given by a function $u(y)$, where $y$ is income other than benefit income. Utility from the benefit depends on whether a flat or variable component of stigma exists: if a flat component exists then utility is given by
\[

$$
\begin{equation*}
u_{f}=u(y+A b)-A s \tag{3.3}
\end{equation*}
$$

\]

and, in addition to this, a variable component exists if

$$
\begin{equation*}
u_{v}=u(y+\sigma A b)-A s \tag{3.4}
\end{equation*}
$$

where $b$ is the value of the benefit, $A$ is as above (i.e. a binary variable indicating whether the individual does or does not claim), and $\sigma$ and $s$ are stigma parameters such that $0<\sigma<1$ and $s>0$. Therefore, if $\sigma=1$ only the flat component of stigma exists and an individual will take-up if

$$
\begin{equation*}
u(y+b)-s>u(y) .{ }^{11} \tag{3.5}
\end{equation*}
$$

Similarly, when both a flat component and a variable component of stigma are present take-up will occur if

$$
\begin{equation*}
u(y+\sigma b)-s>u(y) \tag{3.6}
\end{equation*}
$$

From this formulation we can immediately infer that the likelihood of making a claim is increased by raising the value of $b$ or by taking measures to reduce both forms of stigma associated with the benefit (i.e. minimising $s$ and bringing $\sigma$ close to one). However, it is possible that $s$ itself is an increasing function of the benefit level, $b$, in which case an increase in $b$ need not necessarily lead to an increased likelihood of participating. ${ }^{12}$ We will return to this difficulty later on in this chapter when we consider our model of takeup.

The model is extended by incorporating a linear labour supply equation (a function of net wages and other non-wage income) and this micro-framework is used as a

[^46]starting point for an econometric analysis. ${ }^{13}$ Using a data set of female heads of household (taken from the 1976 Michigan Panel on Income Dynamics) he concludes that of the two stigma components the flat component is far more important in deciding whether an individual participates or not. Individuals thus appear to be deterred from claiming as they feel the stigma associated with claiming. This finding emphasizes the importance that stigma has in preventing people from making a claim in the first place. Stigma though does not have a particularly strong effect on those individuals already claiming a benefit (insignificant variable component). We will not consider the econometrics of Moffitt's work in any greater detail. The paper's significant contribution lies in its innovative approach in setting take-up analysis into a microeconomic structure.

Stigma as a transaction cost in claiming is also the focus of Cowell (1986) who adopts a very similar approach to Moffitt. He concentrates not only on stigma but also on the hassle arising from a claim. In addition, he introduces uncertainty only as to whether a claim will be successful or not with no account for uncertainty about the level of entitlement. Cowell tackles both cost factors separately. Hassle, $h$, is treated in the same way as Moffitt's flat stigma component. Thus, given the subjective probability that a claim will be successful as $\pi$, an individual will take-up if

$$
\begin{equation*}
\pi u(y+b)+(1-\pi) u(y)-h>u(y) \tag{3.7}
\end{equation*}
$$

that is, if the expected utility from the claim (that might turn out to be successful or unsuccessful) exceeds the (certain) utility from income alone. This basic model of the decision to claim is the one we will adopt later in this chapter.

Stigma is modelled in a slightly different manner and not treated as a simple transaction cost. In particular, Cowell considers stigma arising from other peoples' perceptions about a claimant. Thus the environment in which an individual makes a decision is crucial to his stigma analysis. The uncertainty in this case refers only to whether other people know if the individual has claimed the benefit. The probability that they know of this is given by $\theta$. Hence, uncertainty about eligibility for the benefit no longer applies so that the assumptions are (i) individuals know their entitlement to the

[^47]benefit and (ii) if they should claim they will receive the benefit. The disutility the individual experiences, should others discover that she is a claimant, is given by the stigma, $s$. The model is thus slightly different from the hassle model. Cowell shows that the decision to claim is now determined by the condition
\[

$$
\begin{equation*}
\dot{u}(y)>s^{e}[y, a(y), r(y)] \tag{3.8}
\end{equation*}
$$

\]

where $\dot{u}(y)$ gives the utility gain from the extra income provided by the benefit, i.e. $u[y+b(y)]-u(y)$, and $s^{e}[$.$] is the expected stigma (=\theta s) . \quad a(y)$ is a measure of aggregate claimant activity, that is the overall proportion of people in the population considered that are claimants as well, and $r(y)$ gives a 'reference group'. Note how all of these, including the benefit level, are now dependent on the income level, $y$.

It seems more likely that an individual living in an area or community with a large number of claimants will feel less stigma about claiming (such as, for example, in a housing estate where a sizeable proportion of residents are benefit claimants). Hence the chances are that $s^{e}[$.$] is decreasing (possibly strictly) in a(y)$. But it also appears likely that $s^{e}[$.$] is increasing with income. If we then consider the entire community/population$ group with different individuals on varying incomes, we can model any individual with income $y$ as part of a reference group, $r(y)$, such that expected stigma is $s^{e}=s^{e}[y, a(y), r(y)]$ specific to that reference group. (A problem arises from the definition of the exact reference group for an individual with a certain income (see Cowell (1986, pp. 11-19) for details).)

With our aim of constructing an econometric model of take-up, Cowell's stigma approach is of somewhat limited use. The main advantage it presents over the transaction costs approach is to introduce uncertainty about others' perceptions of oneself (at the expense of excluding uncertainty about eligibility and levels of entitlement). In addition, empirical counter-parts of specific reference groups would be difficult if not impossible to determine. However, the idea of modelling stigma as a result of how other people within a certain population group view benefit claimants is extended by Besley and Coate

[^48](1992). Their model of benefit stigma does not deal with the individual decision-making process but, instead, considers the interaction of a two-person type economy. We will return to this model in depth in section 3.5. For the moment we note that understanding benefit stigma is of great importance in this chapter. At the heart of all of the work in the remainder this chapter is the general decision-making model of Cowell (see equation (3.7)) and the model of benefit stigma based on the work of Besley and Coate.

The final issue to be addressed is models that account for non-take-up as a result of a lack of information about the benefit. Essentially, if an individual is unaware of the existence of the benefit she does not enter the decision-making stage above. An individual in such a position is unable to base decisions on the utility of one act outweighing the utility from an alternative. The basic utility maximisation approach thus cannot accommodate non-take-up due to lack of knowledge of the benefit. However, from a different approach, being unaware of a benefit's existence can be regarded as a transaction cost in itself. In particular, making the effort to acquire information relating to the benefit in question is costly to the individual (a 'hassle') and thereby enters as a cost in the decision-making process. Our approach will adopt this latter reasoning thereby treating a lack of information as a transaction cost, similar to hassle and stigma.

Unfortunately, there are virtually no UK data sources that provide details about an individual's reasons for non-take-up, so that it is extremely difficult to assess the extent of non-take-up due to a lack of information. McKay and Marsh (1995) provide some limited evidence (based on very small samples) on Family Credit. They find very little evidence of a complete lack of knowledge of Family Credit. It is more common (in their sample) that individuals had heard of Family Credit but - often due to a lack of suitable information - did not think they were eligible for it. Empirical evidence for the lack of information as a determinant of non-take-up in the US is provided in an early paper by Strauss (1977). He considers the presence of a Federal Eligibility Determination Office (FEDO) in 100 counties of North Carolina as a proxy for information availability. Simple OLS estimates are provided as to the effect of two different types of FEDO on the number of individuals who take-up supplemental security income and on the growth of such take-up rates (only the blind, elderly and disabled are considered). The results
suggest that the presence of a FEDO branch office only has a significant positive effect on take-up. ${ }^{14}$

More recently Anderson and Meyer (1994) discuss the take-up of unemployment insurance in the US. Utilising an extremely large data set (the Continuous Wage and Benefit Project with a total sample size of $1,117,000$ ) covering six states in the US with information on reasons for non-take-up, only 5.64 percent of the sample of entitled nonrecipients cited a lack of knowledge as the main reason for non-take-up (this response also included those who knew about the benefit but were unsure about how to claim for it). The responses that featured most predominantly were (i) the expectation of obtaining a new job in the near future ( 37.06 percent), (ii) the hassle and/or stigma of claiming (12.53 percent), and (iii) not knowing what one's reason for non-take-up was (16.74 percent). Of course, these results apply to the US welfare system and as such do not necessarily translate to means-tested benefits in Britain.

### 3.4 The Agents in an Analysis of Take-Up

Throughout this chapter we concentrate on the individual decision-maker when analysing the take-up problem. However, in this section we briefly outline the three agents involved in a take-up analysis: the government agency, the individual claimants, and the analyst. We begin with a brief description of the government agency. The decisions made by the government agency are regarded as exogenous to our model. We assume that individual claimants who are entitled to benefit will be granted the benefit if they decide to apply for it. Our main interest centres on the individual claimant and the analyst who considers the decision by such claimants. Nevertheless, it is important to draw attention to the government agency in the framework of analysing take-up. ${ }^{15}$

[^49]
### 3.4.1 The Government Agency

The government agency is represented by the DSS in the UK and it ultimately decides on whether the benefit is granted or not. This decision is made on the basis of information provided by the individual claimant. Thereby the government agency follows a set of rules and regulations that determine whether a claimant is eligible for the benefit and if so, what the level of entitlement to the benefit is. We assume that the underlying quest is to determine the true eligibility and entitlement level of an individual as given by the benefit legislation. The level of entitlement to the benefit (IS in this case) as computed by the government agency is determined according to

$$
\begin{align*}
b & =\max \left\{0,(B+m)-\left(w+y_{o}+y_{t}\right)\right\} & & \text { if } h r s<24 / 16 \text { and } S<8000 . \\
& =0 & & \text { otherwise } \tag{3.9}
\end{align*}
$$

where $B$ is benefit entitlement as laid down by legislation, $m$ is weekly mortgage interest, $w$ is weekly net earnings, $y_{o}$ weekly benefit income from other sources and other nonlabour income (with certain exceptions and disregards), and $y_{t}$ is tariff income from savings. Furthermore, hrs denotes the number of hours normally worked per week, and $S$ is the total amount of savings. ${ }^{16}$

In computing the benefit entitlement according to (3.9) the government agency is unlikely to establish for each individual applicant the precise entitlement. These computational errors arise for two main reasons: firstly, the government agency is likely to commit some administrative error. It might be unable to observe exactly $m, w, y_{o}, y_{t}$, hrs and/or $S$. This is also more likely to occur for means-tested benefits governed by complex legislation, such as IS in the UK. Secondly, an individual might not necessarily provide truthful information when making a claim for a benefit. In doing so the individual might provide false information on purpose (e.g. a working claimant with several jobs not revealing all earnings) or by accident (e.g. a pensioner claimant confusing income receipts and misreporting these as a result). Suppose that the true level of benefit (if all the individual claimant's characteristics could be observed without error)

[^50]is given by $b^{*}$. Then $b^{*}$ is likely to differ from the amount of benefit as calculated by the government agency, $b$, such that $b=b^{*}+\varepsilon$ for each individual claimant, where $\varepsilon$ is some stochastic error term. ${ }^{17}$ An individual is granted IS by the government agency only if $b$ exceeds zero. Consequently, there is some scope for the government agency to incorrectly determine whether an individual claimant is actually entitled to IS. From the perspective of the analyst the following conditional probabilities are thus of particular interest: $\operatorname{Pr}\left(b>0 \mid b^{*} \leq 0\right)$ and $\operatorname{Pr}\left(b \leq 0 \mid b^{*}>0\right)$. The first of these probabilities simply gives the probability of the government agency incorrectly granting benefit when in fact the claimant is not actually entitled, whereas the latter gives the reverse situation.

If one of the government agency's aims were to accurately asses benefit eligibility the goal would be to minimise the probability of these types of errors occurring. In other words, the extent of possible administrative error should be minimised as should the extent of providing incorrect information (for whatever reason) on behalf of the applicant. Possible ways of dealing with both of these two issues would be to simplify the rules and regulations to the benefit, such as by improving the available information referring to the benefit, or more generally taking any measures to decrease the transaction costs an individual faces when making a claim. ${ }^{18}$ However, such policies are likely to reduce the probability of granting no benefit to claimants who are actually entitled, i.e. reduce $\operatorname{Pr}\left(b>0 \mid b^{*} \leq 0\right)$, but might, on the other hand, increase the probability of awarding benefit to claimants who are not actually entitled, i.e. increase $\operatorname{Pr}\left(b \leq 0 \mid b^{*}>0\right)$. The government thus faces a trade-off between taking measures to improve the accuracy of information provided by applicants and the possible scope for fraudulent abuse of the system.

[^51]
### 3.4.2 The Individual Claimant

In this section we outline the basic decision-making framework based on the models outlined in section 3.3.1 above. We assume individuals to be risk-averse rational utility maximisers who will claim and thus take up a benefit if the utility from claiming the benefit - less any transaction costs that arise - exceeds the utility from not claiming. The individual will not claim otherwise. Furthermore assume that individuals have at least some basic knowledge of the existence of the benefit so that they are able to enter the decision-making stage of claiming for the benefit. ${ }^{19}$ Strictly speaking, we therefore rule out the possibility of non-take-up due to a lack of knowledge of the benefit in question. However, the acquisition of even some basic knowledge about the benefit can be regarded as being costly and as such can then be regarded as a cost of claiming.

Suppose now that the individuals we consider are unemployed at the time of making a claim. An individual who does not claim derives utility from non-benefit income, $y$, only (non-benefit income is determined exogenously). If she does claim and is granted the benefit she derives additional utility from the benefit income, $b$. Claiming involves a cost and this cost can be composed into two parts: the hassle, $h$, from claiming and any stigma, $s$, associated with claiming. We can view the hassle from claiming as a one-off cost, encountered only at the time of claiming. Stigma, on the other hand, can be regarded as being experienced not only when claiming but also when in receipt of the benefit. Besides, we can relax the assumption of a fixed stigma level and, instead, consider a stigma function that varies with income and benefit level so that $s=s(b, y)$. (We will elaborate the cost structure in section 3.5 for the case that stimga varies with $b$ and $y$.)

Now assume that an individual claimant is uncertain only about whether a claim will turn out to be successful or not. For the moment suppose that she is not uncertain about the entitlement level itself. The individual claimant then attaches subjective probability $\pi$ to the claim being successful, yielding utility $u(y+b)$, and thus probability $1-\pi$ to the claim being unsuccessful with utility $u(y)$. We assume $u($.$) to be$

[^52]increasing, smooth and concave in all its arguments. Hence an individual will take-up only if
\[

$$
\begin{equation*}
\pi u(y+b)+(1-\pi) u(y)-[h+s]>u(y) \tag{3.10}
\end{equation*}
$$

\]

which is essentially the same condition as equation (3.7) of Cowell's model of benefit hassle. Equation (3.10) differs from (3.7) in that we also account for the stigma arising from claiming. In the subsequent sections we also consider the possibility that $s=s(b, y)$.

The simple condition of equation (3.10) underlies all our models of benefit takeup. It captures the notion that an individual will take-up only if the expected utility from claiming - taking into account the cost associated with being a benefit recipient - exceeds the utility from not taking-up the benefit. If for the moment we ignore that the stigma level depends on $b$ and $y$, then we can instantly infer that (for fixed costs of claiming) increasing $b$ or $\pi$ will increase the likelihood of take-up whereas a rise in the cost of claiming will reduce the likelihood to take-up. We shall return to equation (3.10) in the remaining sections of this chapter.

As a further extension to the individual's claiming decision we could introduce uncertainty about the benefit level itself. In order to do this, assume that each individual forms some subjective belief about a distribution of benefit payments as awarded by the government agency. Such a distribution is likely to be formed conditional on the government agency's publicised benefit level being positive and is thus given by the conditional density function $f(b \mid b>0)$. Then each individual forms an expectation about the benefit level they believe to be entitled to, according to say

$$
\begin{equation*}
b^{e}=E(b \mid b>0)=\int_{b} b \times f(b \mid b>0) d b \tag{3.11}
\end{equation*}
$$

However, the conclusions based on any of the models developed in this chapter do not change as a result of introducing uncertainty in the form of equation (3.11). Hence, throughout the remainder of this chapter we shall assume no uncertainty of the benefit entitlement.

### 3.4.3 The Analyst

From the perspective of the analyst, only the outcome of the decision made by the individual claimant can be observed. That is, we can detect only whether the individual does or does not take-up the benefit. This observation is in essence the same as observing whether or not the government has decided to grant the benefit. However, these observations are made by inspecting whether benefit receipt is being reported or not in the data set. As a result problems emerge primarily due to having to work with such survey data in which errors are likely to occur. So when an individual reports receipt (or non-receipt) of a benefit this might not necessarily mean that they are actually receiving the benefit (or not receiving it). This potential for misclassification of entitled individuals as non-entitled and vice versa is an issue not addressed in this thesis. We assume that in the data set we employ, the reported IS entitlement by individual respondents is correct and misclassification is only a major issue amongst pensioners (see Chapter 2 for a more detailed discussion). The closely related issue of measurement error in the IS entitlement is however an issue we will analyse in some depth in Chapter 4.

The first step for an analyst looking at the issue of benefit take-up is to compute the set of all entitled individuals. This is done using an IS algorithm which computes for each eligible individual in a particular data set an entitlement level (as outlined in Chapter 2). The computation aims to mimic the benefit formula (3.9) above and will be denoted $b_{A}$ hereafter. (Note that since the analyst computes this entitlement using the data reported in the data set there is some scope for errors to occur. This theme will be takenup in Chapter 4.) Using both the computation of $b_{A}$ and whether an individual reports benefit receipt we can establish the following sets of Figure 3.2 (see Appendix 3): the shaded set gives all those individuals who report benefit receipt in the data set (assuming, as we do, this is correctly reported this set gives all those for whom $b>0$ ) whereas the unshaded set gives all those for whom $b_{A}>0$. Of particular interest are those who are entitled according to our program but who are not reporting receipt of the benefit, i.e. the set $\left(b_{A}>0\right)-\left[\left(b_{A}>0\right) \cap(b>0)\right]$, indicated by ENR in Figure 3.2. Also note that the data we use in our analysis suggest that the number of these entitled non-recipients is
considerably greater than the number of recipients who are not entitled, i.e. the set $(b>0)-\left[\left(b_{A}>0\right) \cap(b>0)\right]$ indicated by NER in Figure 3.2.

As in the case for the government agency, errors are likely to occur in the computation of $b_{A}$ since (i) we are unable to mimic all the complex rules and regulations relating to benefit entitlements, and (ii) even when we are able to mimic these rules the data given in the BHPS are not always correctly reported (respondents do not always respond truthfully and/or accurately). Hence, for many individual cases there will be a discrepancy between the analyst's computation of entitlement and the entitlement level as computed by the government agency, that is $b_{A}=b_{G}+\varepsilon_{A}$ where, as before, $\varepsilon_{A}$ is a stochastic (or possibly systematic) error term. This error in computing the IS entitlement we regard hereafter as measurement error, and the next chapter is devoted to studying the impact of this measurement error on an econometric analysis of take-up.

When modelling the factors that determine whether an individual participates in a social security program, it is the entitlement level as computed by the analyst, $b_{A}$, which is used as one of the main explanatory levels. Most previous studies (for example, Altman (1981), Blundell et al. (1988), Fry and Stark (1989 \& 1993) and Dorsett and Heady (1991)) do not take into account the likelihood that the computation of $b_{A}$ is subject to measurement error. An exception to this is Duclos (1992a, 1992b \& 1995) who accounts for a variety of modelling errors (measurement errors and, above all, systematic misclassification) in his analysis. Our analysis differs to that of Duclos in that we do not account for misclassification (as given by the above probabilities) but instead focus more on measurement error and its impact on econometric models of take-up.

This section has provided us with an insight to the general issues involved in modelling the take-up of social security benefits. We have shown that many problems arise from what may at first sight appear to be a relatively straightforward problem. However, not all of these problems can be tackled from a single modelling approach. As discussed in the previous section, economic theory may not be ideally suited to analysing the role of the government agency when considering the take-up problem. Recent developments in
the socio-psychological literature emphasizing the supply side of the take-up problem may provide more understanding about the government agency. Nevertheless, our emphasis falls primarily on the individual claimant and we believe that economic theory has much to offer in this respect. A simple model as outlined above (equation (3.7)) encapsulates the key issues in determining whether an individual will take-up or not. Therefore it serves as the foundation to all ensuing models encountered in this chapter. In the next section we draw our attention to the costs of claiming and how these too can be placed into an economic framework.

### 3.5 An Economic Model of the Cost of Claiming Benefits

Previously we noted the presence of some form of transaction costs which deter individuals entitled to benefit from claiming their entitlement. These costs take the form of the hassle involved in claiming and the stigma associated with a claim. The hassle of a claim is the general term adopted to describe the process of applying for benefits. As such it includes the effort in obtaining some basic information about the benefit, the actual process of applying by filling in forms (and having to reveal a large amount of personal information about the individual and their family circumstances), but also the possible impracticalities involved in getting to a job centre to make the claim in the first place. The stigma associated with a claim describes any 'psychic costs' associated with claiming benefits. This concept includes any feelings of inferiority on behalf of the claimant, not wanting to be seen as living on charity and so on.

Some evidence of stigma amongst claimants can be obtained by considering other persons' perceptions of benefit claimants. The 1989 British Social Attitudes Survey suggests that 67.5 percent of persons questioned agreed with the statement 'Large numbers of people these days falsely claim benefits' but, perhaps somewhat surprisingly, at the same time 83.7 percent agreed with the statement 'Large numbers of people who are eligible for benefit these days fail to claim them' (see SCPR (1992)). The more recent 1994 British Social Attitudes Survey found 51 percent of persons questioned agreed with
the statement 'People receiving social security are made to feel like second-class citizens' (see SCPR (1995)). ${ }^{20}$

The aim of this section is to get to grips with these somewhat vague cost concepts by modelling them in an economic framework. In particular, we attempt to determine the effect of changing the benefit entitlement and changing the level of any unearned income on the cost functions in our above models. Our particular emphasis falls on the stigma function $s=s(b, y)$. The following model endogenizes the concept of stigma into a simple two-person type economy and by doing so we are able to perform comparative statics which determine the sign of our stigma function, namely $\partial s(.) / \partial b$ and $\partial s(.) / \partial y$. Although the results derived from this model relate to the economy as a whole, we use them as a guideline for the individual claimant deciding to take-up a benefit.

We treat both the hassle and the stigma arising from a claim separately. The assumption made is that any 'transaction' costs of claiming consist of a fixed one-off cost, referred to as the hassle of claiming and denoted $h$ hereafter, and a variable element which depends upon the benefit entitlement and any other income that the claimant may receive. This variable cost is the stigma, $s$, attached to being a benefit claimant. The stigma associated with a claim acts in a more subtle way than the hassle from claiming. Thus, unlike the hassle from claiming, we must elaborate the structure of stigma. In particular, we want to know how the level of stigma changes with varying levels of both the benefit entitlement and unearned income and we consequently take stigma to be a function of these variables.

In order to examine stigma we draw on the work of Besley and Coate (1992). They construct a simple model of a two-person economy, consisting of rich persons and poor persons only, and rest their notion of stigma (or 'psychic cost' of claiming) on the sociological developments of Goffman (1963). Stigma is viewed as the outcome of a society that regards certain characteristics as desirable and individuals dependent on social security benefits are perceived as lacking these characteristics. Thus if an

[^53]individual is known to be on benefits, others will infer some "blemish of character" which in turn gives rise to unfavourable treatment and thus to stigma. An alternative view of stigma results from the fact that rich individuals pay for the benefits. These rich individuals have their own subjective view as to the optimal benefit level and as to whether a benefit recipient is deserving or undeserving. Those rich individuals who feel that benefit levels are too high and that benefits reach a large proportion of undeserving claimants will treat poor individuals unfavourably, thereby inducing stigma amongst poor individuals. ${ }^{21}$

In the following we examine both views of stigma and the implications of changing certain socio-economic characteristics on the resulting level of stigma. Our model differs only slightly from that of Besley and Coate in that we assume individuals to have more than a single source of income (i.e. we allow for unearned income as well). Nevertheless, as will be seen later, the key results of the Besley and Coate model still apply.

### 3.5.1 The Basic Model

Suppose we have an economy with total population $\Omega$, consisting of two types of individuals: poor individuals of which there are in total $P$, and rich individuals of which there are in total $R(=\Omega-P)$. Furthermore assume that amongst the $P$ poor individuals a certain proportion, $k$, are genuinely unable to work (the involuntarily unemployed, the sick and disabled etc.) and, in addition, they have no other unearned income to support themselves (or only very small amounts of unearned income which does not make them ineligible for benefit). Hereafter we refer to these individuals as the needy poor. The remaining ( $1-k$ ) poor individuals can either work if they choose to do so, or live of unearned income which is sufficient to support themselves (it is a level of income which makes them ineligible for benefit but is not, however, a level of income which places them in the rich section of the population). These individuals can be regarded as the non-

[^54]needy poor. Stigma in this economy arises from the way in which rich individuals view poor individuals, specifically the non-needy poor. Rich individuals feel some degree of concern about needy poor individuals only but, since they cannot distinguish between the needy and the non-needy poor, stigma is experienced by all claimants.

Now consider the various populations groups in turn. The rich have consumption given by $c_{R}=w_{R} l_{R}+y_{R}$ where $w_{R}$ is the net wage, $l_{R}$ are the units of labour supplied, and $y_{R}$ is unearned income with the subscript $R$ referring to rich individuals. A rich person has utility

$$
\begin{equation*}
U\left(c_{R}\right)-\mu \times k P \times \psi\left(c_{R}\right) \tag{3.12}
\end{equation*}
$$

where $\mu$ is some weight giving the degree of concern about the needy poor (varying continuously across $\Omega$ ), $k P$ is the toal number of needy poor individuals, and $\psi\left(c_{R}\right)$ is a smooth, decreasing and convex function of poverty/distress. The utility function $U\left(c_{R}\right)$ is assumed to be smooth, increasing and strictly concave.

The rich have some concern only about the needy poor who are genuinely unable to work and/or who have insufficient unearned income to support themselves. They thus pay taxes from their income which in turn finance benefit payments, $b$, to the poor. The tax faced by each rich individual is given by

$$
\begin{equation*}
t=\frac{C b}{\Omega-P} \tag{3.13}
\end{equation*}
$$

where $C$ is the total number of poor individuals receiving benefit. ${ }^{22}$
The benefit payment, $b$, is available to any poor individual subject to the following conditions being satisfied: (i) they must be unemployed, and (ii) their unearned income must not exceed a certain threshold, given by $\bar{y}$. Let the net wage paid to the poor be $w_{P}$, the units of labour supplied be $l_{P}$, and the level of unearned income be $y_{P}$. Now the needy poor have no earnings from work and if they have any unearned income it is such that $y_{P}<\bar{y}$. On the other hand, suppose that the non-needy poor either have

[^55]earnings $w_{P} l_{P}$ (and no unearned income) or, if they cannot work, some unearned income such that $y_{P} \geq \bar{y}$. Denote this unearned income by $y_{P}^{*}$. Hence a simple means-test operates whereby eligibility is determined by the employment status and entitlement depends on unearned income such that
\[

$$
\begin{array}{llll}
b=0 & \text { iff } & w_{P} l_{P}>0 & \text { or } \\
b>0 & \text { iff } & w_{P}^{*} l_{P}=0 \text { and } y_{P} \geq \bar{y} \\
y_{P} & { }^{23}
\end{array}
$$
\]

The needy poor have consumption given by $c_{P}=b+y_{P}$ assuming they take-up their benefit entitlement, whereas the non-needy poor have consumption $c_{P}^{*}=w_{P} l_{P}$ or $c_{P}^{*}=y_{P}^{*}$ depending on whether they work or not. A needy poor individual has utility $V\left(c_{P}\right)-s$ where $s$ gives the stigma from being a benefit claimant (for simplicity assume $s$ to be uniformly distributed over the interval $[0,1])$. The non-needy poor can cheat the system in either of the following ways: if they are actually able to work, they can conceal this ability to the government agency by pretending to be a needy poor individual; if they are not able to work but have unearned income $y_{P}^{*}$, they can again mislead the government agency by feigning their unearned income to be $y_{P}<\bar{y}$. Note though that they cannot cheat the government agency by working and claiming benefit at the same time. Thus, non-needy poor individuals who are able to work can either do so with utility $V\left(w_{P} l_{P}\right)-d$ where $d$ is the disutility from working (for simplicity assume $d$ also to be uniformly distributed over the interval [ 0,1$]$ ), or they can claim benefits with utility $V(b)-s$ where $s$ is as before. Likewise non-needy poor individuals who are unable to work can either live of their unearned income, with utility $V\left(y_{P}^{*}\right)$, or claim for benefit with utility $V\left(y_{P}^{*}+b\right)-s$. The utility function $V($.$) is assumed to be smooth, increasing$ and strictly concave, and $V(.) \rightarrow-\infty$ as the consumption level of poor individuals goes to zero (i.e. some positive consumption is neccessary).

Throughout this section we ignore the take-up decision of individuals. So, needy poor individuals who are entitled to benefit are assumed to claim their entitlement. Nonneedy individuals do make choices though. In particular, a non-needy poor individual
who is able to work must decide whether she is going to do so or whether she will hide the fact that she can work and claim for benefits instead. Thus, for each non-needy poor individual who can work, a critical level of disutility from working exists. Let this level be $d^{*}$ at which

$$
\begin{equation*}
V\left(w_{P} l_{P}\right)-d^{*}=V(b)-s \tag{3.14}
\end{equation*}
$$

i.e. the level of disutility at which the utility from working equates with the utility from being a benefit recipient. Now if for some non-needy poor individual the disutility from working exceeds this critical level, $d>d^{*}$, then she will decide to beguile the system by claiming instead of working.

Similarly, a non-needy poor individual who has sufficient unearned income, $y_{P}^{*}$, must decide whether she will be satisfied with this income or whether she will conceal her true income and, instead, report an income which entitles her to benefit. In this case, each non-needy poor individual has a critical level of stigma, denoted $s^{*}$, such that

$$
\begin{equation*}
V\left(y_{P}^{*}\right)=V\left(y_{P}^{*}+b\right)-s^{*} \tag{3.15}
\end{equation*}
$$

Hence, a non-needy poor individual will betray the system only if the stigma level they experience is less than the critical value, $s<s^{*}$.

Since we have assumed that both $d \in[0,1]$ and $s \in[0,1]$, we can now derive the total number of poor individuals who receive the benefit as

$$
\begin{equation*}
C=P k+P(1-k)\left[\left(1-d^{*}\right)+s^{*}\right] \tag{3.16}
\end{equation*}
$$

where $P k$ gives the total number of needy poor individuals (deserving claimants), $(1-k)\left(1-d^{*}\right)$ gives the proportion of non-needy poor individuals who decide not to work since $d>d^{*}$, and $(1-k) s^{*}$ gives the proportion of non-needy poor individuals who decide not to live of their unearned income since $s<s^{*}$. These later two groups are regarded as undeserving claimants.

It follows from (3.14) that $d^{*}$ is decreasing in $b$ and increasing in both $w_{P}$ and $s$. Consequently the number of benefit claimants, $C$, increases as $b$ rises and decreases as

[^56]$w_{P}$ and $s$ rise. Similarly, from (3.15) we note that $s^{*}$ is increasing in $b$ so that once again, $C$ increases as $b$ rises. However, we cannot say from (3.15) what effect changing $y_{P}^{*}$ has on $s^{*}$.

This simple two-person type economy model forms the basis to our analysis of the two views of stigma briefly outlined in the introduction to this section. For unemployed non-needy poor individuals (i.e. with unearned income $y_{P}^{*}$ ) we have also shown that increasing benefit levels will increase the amount of stigma felt by these undeserving claimants, thereby reducing the number of total benefit claimants. So far, we have been unable to comment on the effect changes in the benefit level have on the stigma level experienced by non-needy individuals who could work if they chose to do so. In order to address the latter we turn to two models of benefit stigma developed by Besley and Coate. As noted in the introduction to this section, the basic model developed by Besley and Coate differs from our model only in so far that they provide no account for unearned income. Therefore, the general results derived for their model apply to our model as well. In the following section we briefly summarise their main findings.

### 3.5.2 Reputational Externalities and Taxpayer Resentment

Reputational Externalities: The first view of stigma considered is based on the sociological concept of stigma first proposed by Goffman (1963). According to his view, society regards certain characteristics amongst individuals as desirable such as, for example, the desire to work hard, self-reliance, independence etc.. Individuals who are seen to be claiming benefits are regarded as lacking these characteristics. As a result, claimants are treated less auspiciously compared to other members of society and this in turn gives rise to stigma.

According to our model above, poor individuals can either be deserving (i.e. the needy poor) or undeserving of benefit (i.e. the non-needy poor). Rich individuals, on the other hand, pay for these benefits and the problem arises from the fact that they are often unable to distinguish between both types of claimants. They thus treat all benefit claimants less favourably so that in effect the undeserving claimants impose a
reputational externality on the deserving claimants (i.e. stigma affects all benefit claimants).

In order to model the concept of stigma suppose that the disutility from working, $d$, incorporates those characteristics upon which social judgements about individuals are made. As such, individuals with large values of $d$ are regarded as lazy or work-shy and do not, therefore, deserve the benefit. It is precisely these individuals who impose the reputational externality on all claimants collectively. By considering the average value of $d$ amongst all $P$ poor individuals (the social norm) relative to the average value of $d$ amongst all $C$ benefit claimants, we can define stigma as an increasing function of this difference. The greater this difference, the more reason there is to think that benefit claimants are (on average) lazier than the poor population as a whole. In order to formalize this concept, define an equilibrium level of stigma given by

$$
\begin{equation*}
\hat{s}=f\left[\bar{d}_{C}(b, \hat{s})-\bar{d}\right] \tag{3.17}
\end{equation*}
$$

where $f($.$) is some smooth increasing function such that f(0)=0, \bar{d}_{C}($.$) is the average$ $d$ amongst all benefit claimants, and $\bar{d}$ is the average $d$ amongst all poor individuals. ${ }^{24}$ From (3.17) it can then be shown that

$$
\begin{equation*}
\frac{\partial \hat{s}}{\partial b}=\frac{f^{\prime} \partial \bar{d}_{C} / \partial b}{1-f^{\prime} \partial \bar{d}_{c} / \partial s} \tag{3.18}
\end{equation*}
$$

(since applying the implicit function rule to (3.17) gives $\partial \hat{s} / \partial b=-F_{b} / F_{\hat{s}}$ where $F=s^{*}-f\left[\bar{\theta}_{C}\left(b, s^{*}\right)-\bar{\theta}\right]$, and the subscripts denote partial derivatives). As long as the denominator exceeds zero, the sign of (3.18) depends on the sign of $\partial \bar{d}_{c} / \partial b$. In turn, it can be demonstrated that $\partial \bar{d}_{c} / \partial b$ is in fact positive only when $d^{*}>(1-\sqrt{k}) /(1-k)$ and negative when the inequality is reversed. So, provided the condition $d^{*}>(1-\sqrt{k}) /(1-k)$ is satisfied, an increase an $b$ will in fact increase the level of stigma experienced by all claimants in our economy. If, however, this condition is not satisfied stigma decreases with rising levels of $b$.

[^57]In order to clarify this condition from a practical perspective, Besley and Coate suggest the following example: if 10 percent of poor individuals in our economy are needy ( $k=0.1$ ), then the critical value of $d^{*}=0.75$. So, if less than 25 percent of the non-needy poor claim benefit the average disutility amongst all claimants is increasing in $b$, and thus stigma is increasing in $b$ as well. If more than 25 percent of the non-needy poor claim the opposite holds.

What about changes in the level of unearned income of the poor? By changing $y_{P}$ the number of undeserving claimants is affected. The effect is similar to that of decreasing $b$ : both the proportion of deserving claimants and the average disutility of labour increase. Hence, using the arguments above for changes in $b$, we note that increasing $y_{P}$ has an effect in the opposite direction to that of changing levels of $b$. That is, the level of stigma increases if $d^{*}<(1-\sqrt{k}) /(1-k)$ and decreases if the inequality is reversed. ${ }^{25}$

Taxpayer Resentment: The second view of stigma considered is based on the concept that social security benefits need to be financed by some form of taxation. Within our twoperson type economy it is the rich individuals who pay taxes (see equation (3.13)) which in turn finance benefit payments to the poor. Unlike the previous view of stigma this view has a somewhat stronger foundation in economics. The fundamental concept here is that rich individuals form their own view about the appropriate and just benefit entitlement level. If they believe that the benefit level is too altruistic they will feel some element of resentment towards benefit claimants. As a result, benefit claimants experience some stigma.

As in the basic model discussed above, we assume each rich individual to have some degree of concern for the needy poor only, measured by some index $\mu$. Given their individual $\mu$, each rich individual chooses an optimal benefit level, denoted $b^{*}(\mu, s)$. Then rich individuals for whom $b^{*}(\mu, s)>b$ believe that benefit payments are not

[^58]sufficient, whereas those for whom $b^{*}(\mu, s)<b$ believe that benefit payments are too generous. The latter of these are the cause of stigma amongst all benefit claimants since they feel some degree of resentment towards benefit claimants (not being able to distinguish between deserving and undeserving claimants) and therefore treat them less favourably.

Now suppose that we can actually quantify the extent to which rich individuals resent benefit claimants in the form of some resentment function for each $\mu$ such that

$$
\begin{equation*}
r\left(\mu, s, b, y_{P}\right)=g\left[b-b^{*}(\mu, s)\right] \tag{3.18}
\end{equation*}
$$

where $g($.$) is some smooth increasing function such that g(0)=0$. The greater the difference between $b$ and $b^{*}(\mu, s)$, the larger the degree of resentment felt by taxpayers. Suppose both $r($.$) and s$ are measured on the same scale and, as before, define an equilibrium level of stigma by:

$$
\begin{equation*}
\hat{s}=(\Omega-P) \int_{0}^{\mu} r\left(\mu, \hat{s}, b, y_{P}\right) d F(\mu) \tag{3.19}
\end{equation*}
$$

where $F(\mu)$ is some distribution function of $\mu .^{26}$ From (3.19) it follows that

$$
\begin{equation*}
\frac{\partial \hat{s}}{\partial b}=\frac{(\Omega-P) \int_{0}^{\mu} r_{b} d F(\mu)}{1-(\Omega-P) \int_{0}^{\mu} r_{s} d F(\mu)} \tag{3.20}
\end{equation*}
$$

where are $r_{b}$ and $r_{s}$ are the partial derivatives of $r($.$) with respect to the particular$ subscript. The degree of resentment is increasing in $b$ (i.e. $r_{b}>0$ ) so that, as long as resentment is decreasing in $s$ (i.e. $r_{s}<0$ ), equation (3.20) will in fact be positive. It can be shown that these conditions are in fact easily satisfied. Hence under the taxpayer resentment model stigma is unambiguously increasing in $b$.

The effect of changes in unearned income of the poor, $y_{P}$, on the level of stigma can be analysed in a similar way. Computing $\partial \hat{S} / \partial y_{P}$ and using similar arguments to the ones above, we can conclude that an increase in $y_{P}$ will in fact decrease the level of stigma. Intuitively, when unearned income rises the proportion of non-needy individuals

[^59]who claim benefit falls and consequently, the cost of increasing benefits decreases. Hence, as the level of unearned income amongst the poor increases stigma levels will fall.

In conclusion to this section, we note that we have been able to construct an economic model of stigma which allows us to determine the direction of change of varying the exogenous variables in our model of take-up. The model draws primarily on the stigma model of Besley and Coate, specifically with respect to the two views of stigma presented above. According to the model, rising benefit levels will increase stigma whereas rising levels of unearned income will decrease stigma. These results hold unambiguously under the taxpayer resentment model presented above. Under the reputational externalities model however, these results apply if and only if the critical value of the disutility from working satisfies certain conditions (hereafter we shall assume that it does).

Unlike the stigma model developed in this section, the simple models of take-up of the next section are not based around a model of the economy. Instead we consider individual level decision-making. The results derived from the stigma model do however give a general conception of the effect of changing benefit levels and unearned income levels on stigma. On an individual level, such changes are likely to be interlinked with a variety of other socio-economic characteristics, so that the effect on any one individual might be quite different to the average effect on all individuals as a whole. Nevertheless, throughout the remainder of this chapter we assume that, on average, the results of the stigma model apply to the individual decision-making claimants considered. This enables us to impose some economic structure on the stigma function in our models of take-up. Thus, in all subsequent models of take-up we consider a general stigma function $s(b, y)$, taken to be a smooth function increasing in benefit entitlement, $b$, and decreasing in unearned income, $y$, such that $s(0, y)=0$.

In the next section we return to the basic model of take-up in view of the findings from this section. In addition, we extend the basic static model to account for a more dynamic structure in the decision-making process.

### 3.6 Economic Models of Take-Up

This section begins by returning to the static one period model of take-up considered in section 3.4.2. We draw on the results of section 3.5 to determine the effect of changing the benefit level, $b$, and levels of unearned income, $y$, on the likelihood to take-up. Intuitively, an increase in $b$ (holding all other variables fixed) should make claiming more attractive and thus increase the probability to take-up. At the same time though, if stigma is assumed to vary with $b$ (we have shown that it increases in $b$ ), this in turn might counter-act the utility gain of an increase in $b$. On the other hand, an increase in $y$ (holding all other variables fixed) should enable the individual to cope better without benefit so that with increasing $y$ we might expect a decrease in the take-up probability. However, contrary to this effect, if stigma is assumed to vary with $y$ (similarly we have shown that it decreases with $y$ ) the probability to take-up might actually increase. Consequently, trying to assess the effects of changing $b$ and $y$ on the probability to takeup are difficult to assess a priori. In the following section 3.6.1 we attempt to impose more structure on the problem in order to gain a better understanding of these effects.

Subsequently we extend the static model and present two simple dynamic models of the individual take-up decision. In the first model we account for the fact that when an individual makes a claim for a benefit she is likely to be influenced by past claiming experiences. In particular, whether a claim at time $t-1$ turned out to be successful or unsuccessful is likely to influence the decision to apply for benefits again at time $t$. In the second model expectations of some future event or state at time $t+1$ affect current decision-making at time $t$. Hence, both dynamic models considered here provide an extension to the static model by presenting a more accurate portrayal of the individual claimant's decision-making process.

### 3.6.1 The Static Model

The basic model is the same as above (see section 3.4.2). We assume uncertainty only about whether a claim will be successful (with probability $\pi$ ) or not (with probability $1-\pi)$. Suppose that individuals entitled to benefit can derive income either from the benefit itself or from some other unearned source or from both. Denote the benefit
entitlement by $b$ and any unearned income by $y$, both measured on the same scale. These are the only sources of income so that we also assume that entitled individuals cannot work. Hence an individual's consumption is given by $c=y+A b$ where $A=1$ if she receives benefit and $A=0$ if she does not receive benefit. Each individual then has utility $u(c)$ where $u($.$) is assumed to be smooth, increasing, concave and twice$ continuously differentiable such that $u^{\prime}>0$ and $u^{\prime \prime}<0$.

From section 3.5 we also know that the cost of claiming consists of the hassle, $h$, which is assumed to be fixed, and the stigma of claiming, which is either fixed as well or a function of $b$ and $y$. If stigma is taken as a function of $b$ and $y$ then $s=s(b, y)$ applies which, under the assumptions made, is such that $\partial s(b, y) / \partial b>0$ and $\partial s(b, y) / \partial y<0$.

Now recall that a claim will occur only if the expected utility from claiming less the cost involved exceeds the utility from unearned income alone, given by equation (3.10) above. In other words we can state that the expected net utility from claiming must exceed zero, i.e.

$$
\begin{equation*}
u_{N}(\pi, y, b, h, s)=\pi[u(y+b)-u(y)]-[h+s(b, y)]>0, \tag{3.21}
\end{equation*}
$$

and no claim occurs if the inequality is reversed. Also assume that the greater is the net utility, $u_{N}$, the more likely an individual is to claim. ${ }^{27}$ Now if we assume $s$ to be fixed, we can immediately infer from (3.21) that increasing $b$ or $\pi$ increases $u_{N}$ and, as a result, an individual is more likely to claim. We cannot, however, deduce the effect of changing $y$ on the likelihood to take-up. ${ }^{28}$

When we no longer assume $s$ to be independent of $b$ and $y$ we cannot necessarily deduce unambiguously the effects changes in these variables have on the likelihood to take-up. From (3.21) it follows that

$$
\frac{\partial u_{N}}{\partial b}=\pi u^{\prime}-\partial s(b, y) / \partial b>0 \quad \text { iff } \pi>\frac{\partial s(b, y) / \partial b}{u^{\prime}}
$$

[^60]$$
\frac{\partial u_{N}}{\partial y}=-\partial s(b, y) / \partial y>0
$$

Thus increasing levels of $b$ will increase the likelihood to take-up only if the expected marginal utility of benefit exceeds the marginal increase in stigma (resulting from a change in $b$ ). Alternatively, what amounts to the same, $u_{N}$ is increasing in $b$ only if $\pi$ is strictly greater than the marginal monetary increase in stigma. Whether this holds cannot be determined a priori and remains to be established in the following empirical chapters. Moreover, note that with our particular stigma function, increasing levels of unearned income actually raise the likelihood to take-up as well.

Cowell (1986) shows that (3.21) can be approximated by the net expected monetary benefit from claiming, given by

$$
\begin{equation*}
N(\pi, b, y, h, s)=\frac{u_{N}}{u^{\prime}}=\pi b-\frac{[h+s(b, y)]}{u^{\prime}}>0 \tag{3.22}
\end{equation*}
$$

where the marginal utility of income approximates the discrete utility change such that

$$
u^{\prime}=\frac{\partial u(.)}{\partial y} \cong \frac{u(y+b)-u(y)}{b} .
$$

The intuitive appeal of equation (3.22) lies in the fact that we can interpret the decision to claim in monetary terms: a claim will occur only if the expected monetary benefit outweighs the monetary cost of claiming. As before, we assume that the greater is the expected net monetary value of a claim, the greater the likelihood that an individual will take-up.

Now let us investigate whether (3.22) allows us to be more precise about the effect changing $b$ and $y$ have on the likelihood to take-up. In the introduction to this section we noted that, intuitively, we would expect $N($.$) to be increasing in b$. From (3.22) we have

$$
\frac{\partial N(.)}{\partial b}=\pi-\left[\frac{\partial s(b, y) / \partial b}{u^{\prime}}-\frac{u^{\prime \prime}[h+s(b, y)]}{\left(u^{\prime}\right)^{2}}\right]
$$

which will exceed zero only if

$$
\begin{equation*}
\pi u^{\prime}>\frac{\partial s(b, y)}{\partial b}+r[h+s(b, y)] \tag{3.23}
\end{equation*}
$$

where $r=-u^{\prime \prime} / u^{\prime}$ is the coefficient of absolute risk aversion (which is increasing in its arguments). Similarly, we noted that $N($.$) should be decreasing in y$, and from (3.22) we have

$$
\frac{\partial N(.)}{\partial y}=-\left[\frac{\partial s(b, y) / \partial y}{u^{\prime}}-\frac{u^{\prime \prime}[h+s(b, y)]}{\left(u^{\prime}\right)^{2}}\right]
$$

which is strictly less than zero only if

$$
\begin{equation*}
\frac{\partial s(b, y)}{\partial y}>r[h+s(b, y)] \tag{3.24}
\end{equation*}
$$

with $r$ as before.
Equation (3.23) states that the expected marginal utility of benefit must be strictly greater than the sum of (i) the marginal stigma of benefit and (ii) the product of the absolute risk aversion and the total cost of claiming. In monetary terms, this condition is equivalent to $\pi$ exceeding the sum of the monetary values of (i) and (ii). Likewise, equation (3.24) asserts that the marginal (monetary) stigma of income must be strictly greater than the product of (i) the absolute risk aversion and (ii) the total (monetary) cost of claiming.

So, the simple static model of take-up developed above allows us to impose some structure and deduce some general conditions about the effects of changing certain variables of interest on the likelihood of take-up. In the case where the stigma of claiming is taken to be fixed, the effect of varying $b$ is straightforward to verify. We cannot say anything about the effect of changing $y$ though. In a more realistic setting, stigma is a function of both $b$ and $y$ and provided certain conditions are satisfied, increasing the benefit level increases the likelihood of take-up. The probability of takeup is also increased the greater is the subjective probability that a claim will turn out to be successful. In contrast, the probability of take-up decreases with rising levels of unearned income and the greater is the hassle of claiming. Nevertheless, verifying whether these conditions are in fact satisfied remains an empirical issue to be addressed in Chapters 4 and 5.

In the next two sections we consider the dynamic structure involved in claiming for a benefit. We deal first with the effect of past experience on current claiming
decisions (state dependence) and thereafter consider the impact of future events directly related to the take-up decision.

### 3.6.2 State Dependence

The key issue to be addressed in this section is whether a claim in the past - which may have turned out to be either successful or unsuccessful - affects the current take-up decision. Such state dependence could be captured by a simple two period model where in the first period individuals decide to claim on the basis of equation (3.21). In the second period individuals know the outcome of the decision in the first period and once again make a decision to claim based on equation (3.21). However, when claiming in the second period, we can account for the previous claiming experience by including an additional term which enters the right-hand side of (3.21). This term could take the form of a smooth increasing function of the previous net utility. Then individuals who claimed in the past (i.e. their net utility exceeded zero) will be more likely to claim at present, whereas individuals who did not claim (i.e. their net utility was less than zero) are less likely to claim at present.

The model we develop here adopts a very similar mechanism to analyse the effect a previous claim may have on current decision-making. Unlike the model described above however, we do not consider the net utility of a previous claim entering the current claiming decision. Instead, an individual is more likely to make the current decision to claim in view of the actual monetary outcome of a previous claim. Previous claims for benefit are made under the same uncertainty as current claims for benefit, namely the uncertainty of whether the claim will be successful or not. However at the time of making the current claim the outcome of the previous claim is known. The difference between what the individual expected from the previous claim and what the outcome of the claim actually is, captures some degree of (dis)contentment with the outcome. In turn, this factor of (dis)contentment enters the current decision to take-up or not. ${ }^{29}$

[^61]We assume discrete time periods and at some point in time, $t$, each entitled individual decides to claim or not to claim for benefit. Individuals who decide to claim will continue to receive the benefit for the following time periods provided they are still entitled to it. Suppose also that the time lag between claiming and receiving the benefit occurs within the same time period considered. ${ }^{30}$ An individual claimant who has claimed in the past (call this time period $t-1$ ) is able to observe the outcome of that claim when making a claim in the current time period, $t$. In the previous time period, $t-1$, the individual decided to claim depending on whether equation (3.21) from above was satisfied. The outcome of that claim was known within the same time period. Let the outcome of that claim be some benefit entitlement $b_{t-1}^{*}$. (In the case where no benefit is granted we simply have $b_{t-1}^{*}=0$.)

Suppose now that at the time of claiming the individual had some expectation of whether the claim would be successful, given by the subjective probability $\pi_{t-1}$. Then the expected entitlement at the time of claiming was $b_{t-1}^{e}=\pi_{t-1} \times b_{t-1}$, so that the quantity of interest is given by the (monetary) difference $\Delta_{t-1}^{b}=b_{t-1}^{*}-b_{t-1}^{e} \cdot{ }^{31}$ Hence $\Delta_{t-1}^{b}>0$ if the outcome of the claim turned out to be better than expected, and $\Delta_{t-1}^{b}<0$ if it turned out to be worse than expected. Let the difference $\Delta_{t-1}^{b}$ give rise to some loss or rise in utility measured by the function $d\left(\Delta_{t-1}^{b}\right)$ which is smooth, increasing and concave. The function $d\left(\Delta_{t-1}^{b}\right)$ captures the outcome of the previous claim and, as such, enters the current decision to claim at time $t$. Hence the decision to take-up or not at time $t$ now depends on whether

$$
\begin{align*}
u_{N}\left(\pi_{t}, y_{t}, b_{t}, h_{t}, s, d\right)=\pi_{t}\left[u\left(y_{t}+b_{t}\right)-u\left(y_{t}\right)\right]+d\left(\Delta_{t-1}^{b}\right) \\
-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]>0 \tag{3.25}
\end{align*}
$$

[^62]or, using Cowell's approximation from above (equation (3.22)), whether
\[

$$
\begin{equation*}
N\left(\pi_{t}, b_{t}, y_{t}, h, s\right)=\pi_{t} b_{t}+\frac{d\left(\Delta_{t-1}^{b}\right)}{u^{\prime}}-\frac{\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]}{u^{\prime}}>0.32 \tag{3.26}
\end{equation*}
$$

\]

In monetary terms, a claim will thus occur only if the sum of the expected monetary benefit and the monetary value of $d\left(\Delta_{t-1}^{b}\right)$ exceeds the monetary cost of claiming.

Now since $d\left(\Delta_{t-1}^{b}\right)$ is an increasing function of $\Delta_{t-1}^{b}$, it becomes clear from (3.25) or from (3.26), that if the previous claim turned out to be better than anticipated $\left(d\left(\Delta_{t-1}^{b}\right)>0\right)$ the net utility from claiming is greater than if the claim turned out to be worse than foreseen $\left(d\left(\Delta_{t-1}^{b}\right)<0\right)$. Hence the former are more likely to take-up than the latter.

So, by accounting for the outcome of a previous claim we can introduce the concept of state dependence into the decision to take-up a benefit. This section has shown how our simple model of take-up can be expanded to incorporate past claiming experiences into the decision-making framework. Later on we will estimate and test models which account for state dependence in the take-up decision.

### 3.6.3 Future Events

An individual who claims for benefit is likely to take into account future events. In particular, events that will improve or deteriorate individual financial circumstances are prone to affect current decision-making. An individual who say expects her financial situation to worsen in the near future, is more likely to claim as compared to an individual who expects an improvement in her financial situation. Individuals who expect better financial circumstances may feel that they are more able to cope financially as the situation is likely to improve in the near future.

In this section we develop a simple dynamic model which accounts for such future events. The aim of such a model is to address the question of what effect such future events have on the current probability to take-up. If we assume that an individual
claimant is currently unemployed, non-take-up may be the result of the cost of claiming outweighing the expected benefit. However, in addition, if the individual either knows that she will be employed in the near future, or alternatively, expects a job in the near future, the (expected) future utility from employment will enter current decision-making with respect to claiming.

As in the state dependence model of section 3.6.2 we assume discrete time periods in any one of which an individual decides to claim. The time period in which an individual decides to claim is denoted $t$. Assume that at each time period a new benefit application needs to be made (i.e. benefit lasts only for one time period). Also assume there is no time lag between the act of claiming and the corresponding outcome.

Individuals can be either employed or unemployed only, with the former not being entitled to benefit. The unemployed are entitled to benefit and must decide whether or not to claim. Then if a claimant has no uncertainty about her future employment status we can consider two types of individuals: (i) those who know they will be employed at $t+1$, supplying $l_{t+1}$ units of labour with net wage $w_{t+1}$, and (ii) those who know they will remain unemployed at $t+1$, having to live of their unearned income and the benefit if they are awarded it. Both types of individuals make their decision to claim at time $t$ whilst taking into account their known employment and financial circumstances at time $t+1$. They have current consumption

$$
c_{t}=y_{t}+A_{t} b_{t}
$$

where $A_{t}$ is as before, giving the outcome of the claim at time $t\left(A_{t}=1\right.$ if the benefit is awarded and $A_{t}=0$ otherwise). The uncertainty when claiming for benefit at time $t$ relates to $A_{t}$ : an individual does not know whether the benefit will be granted or not.

Future consumption is given by

$$
c_{t+1}=y_{t+1}+A_{t+1} b_{t+1}+B_{t+1}\left(w_{t+1} l_{t+1}\right)
$$

[^63]where $B_{t+1}=1$ if the individual is employed at $t+1$ and $B_{t+1}=0$ otherwise. Now assume that, at time $t$, each individual knows with certainty the future value $B_{t+1}$. Assume furthermore, for simplicity, that the following holds:
\[

$$
\begin{aligned}
& \text { if } B_{t+1}=0 \Rightarrow A_{t+1}=1 \\
& \text { if } B_{t+1}=1 \Rightarrow A_{t+1}=0
\end{aligned}
$$
\]

i.e. employment and benefit receipt are mutually exclusive. So, each individual knows their future consumption, $c_{t+1}$, with certainty. ${ }^{33}$ Consequently, individuals who know they will be employed at time $t+1$ have utility from not currently claiming the benefit given by
where

$$
u\left(y_{t}\right)+u\left(c_{t+1}^{E}\right)
$$

the consumption level from being employed at time $t+1$. On the other hand, individuals who know they will remain unemployed at time $t+1$ have utility from not currently claiming the benefit given by
where

$$
u\left(y_{t}\right)+u\left(c_{t+1}^{U}\right)
$$

the consumption level from being unemployed at time $t+1 .{ }^{34}$
As before, the decision to claim for benefits is based on equation (3.21) above (i.e. claim only if the net utility from claiming exceeds zero). When taking into account their future employment status, individuals who know they will be employed at $t+1$ will claim at $t$ only if

$$
\pi_{t} u\left(y_{t}+b_{t}\right)+\left(1-\pi_{t}\right) u\left(y_{t}\right)-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]>u\left(y_{t}\right)+u\left(w_{t+1} l_{t+1}+y_{t+1}\right)
$$

or, in terms of the net utility, if

$$
\begin{equation*}
u_{N}=\pi_{t}\left[u\left(y_{t}+b_{t}\right)-u\left(y_{t}\right)\right]-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]-u\left(w_{t+1} l_{t+1}+y_{t+1}\right)>0 \tag{3.27}
\end{equation*}
$$

On the other hand, those who know that they will remain unemployed at $t+1$ will claim at $t$ only if

[^64]$$
\pi_{t} u\left(y_{t}+b_{t}\right)+\left(1-\pi_{t}\right) u\left(y_{t}\right)-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]>u\left(y_{t}\right)+u\left(y_{t+1}+b_{t+1}\right)
$$
or, again in terms of the net utility, if
\[

$$
\begin{equation*}
u_{N}=\pi_{t}\left[u\left(y_{t}+b_{t}\right)-u\left(y_{t}\right)\right]-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]-u\left(y_{t+1}+b_{t+1}\right)>0 . \tag{3.28}
\end{equation*}
$$

\]

Now if we consider (3.27) and (3.28) separately, the results which apply to the static model of section 3.6 .1 also hold for these dynamic models. Besides, it clearly follows from (3.28) that the greater is $y_{t+1}$ and/or $b_{t+1}$, the less likely an individual is to claim at time $t$. Equally, the same result applies to (3.27), for either increasing $y_{t+1}$ or $w_{t+1}$. For fixed $y_{t+1}$, we can see that by comparing (3.27) and (3.28), an individual who knows they are going to be employed at $t+1$ is less likely to claim at time $t$ than an individual who knows they will remain unemployed at $t+1$, provided that $w_{t+1} l_{t+1}>b_{t+1}$. In addition, the greater the earnings an individual faces at time $t+1$, the lower the likelihood of claiming at time $t$.

We can extend this model to consider the case where individuals who are currently unemployed, are no longer certain about their future employment status. Suppose that individuals are uncertain about (i) whether the current claim will turn out to be successful or not (as before), and (ii) their employment status and thus earnings at time $t+1$. At $t+1$, an individual can either be employed with expected wage $\hat{w}_{t+1}$, or remain unemployed with zero earnings from employment. Each individual forms her future expected earnings conditional on the information set available at time $t$, such that

$$
\hat{w}_{t+1}=E\left(w_{t+1} \mid I_{t}\right)
$$

where $I_{t}$ denotes the information set. Now assume that a claimant attaches some subjective probability to being employed at time $t+1$. This probability is formed at time $t$ and is given by $\varphi_{t}$. The expected utility from working at $t+1$ is then

$$
E\left\{u\left(\operatorname{work}_{t+1}\right)\right\}=\varphi_{t} u\left(y_{t+1}+\hat{w}_{t+1} l_{t+1}\right)
$$

where we assume that the future units of labour supplied, $l_{t+1}$, are known at time $t$.

The probability of remaining unemployed at $t+1$ is $\left(1-\varphi_{t}\right)$. Assume, for simplicity, that there is no uncertainty about $A_{t+1}$, i.e. whether the future benefit claim will be successful or not. ${ }^{35}$ The expected utility from not working at $t+1$ is thus

$$
E\left\{u\left(\text { unemployed }_{t+1}\right)\right\}=\left(1-\varphi_{t}\right) u\left(y_{t+1}+A_{t+1} b_{t+1}\right)
$$

The decision to claim can now be expressed in words as follows: a claim will occur only if

$$
\begin{equation*}
E\left\{u\left(\text { claim }_{t}\right)\right\}-\operatorname{cost}_{t}+E\left\{u\left(\text { unemployed }_{t+1}\right)\right\}>u\left(\text { income }_{t}\right)+E\left\{u\left(\text { work }_{t+1}\right)\right\} \tag{3.29}
\end{equation*}
$$

where all the terms of equation (3.29) are the same as before except that (i) the expected utility from not working at $t+1$ enters the left-hand side, and (ii) the expected utility from working at $t+1$ enters the right-hand side. Substituting the various components of (3.29) and rearranging gives the following net utility condition for a claim at time $t$ to occur:

$$
\begin{align*}
& u_{N}=\pi_{t}\left[u\left(y_{t}+b_{t}\right)-u\left(y_{t}\right)\right]-\left[h_{t}+s\left(b_{t}, y_{t}\right)\right]+u\left(y_{t+1}+A_{t+1} b_{t+1}\right) \\
&-\varphi_{t}\left\{u\left(y_{t+1}+\hat{w}_{t+1} l_{t+1}\right)+u\left(y_{t+1}+A_{t+1} b_{t+1}\right)\right\}>0 . \tag{3.30}
\end{align*}
$$

Clearly, from (3.30) it follows that individuals with large future expected earnings and/or large probabilities of being employed at $t+1$ will be less likely to take-up at time $t$. So, increasing either $\varphi_{t}$ or $\hat{w}_{t+1}$ (or both) decreases the likelihood of claiming in the current time period. Note also that this finding has interesting policy implications: if the government were to send out signals or adopt a policy which increases future employment/wage expectations amongst the unemployed, then current take-up should decline according to our model.

Hence, by taking into account the future employment status of an individual (be it certain or uncertain) we can adopt our simple static model of take-up to account for future events. Individuals who know they will be employed in the near future (or are more certain that they will be) are less likely to take-up at present according to the models developed above. In the ensuing empirical chapters we estimate and test for models with both state dependence and future events.

[^65]
### 3.7 Conclusions

In this chapter we have shown the motivation underlying a series of microeconomic models which attempt to explain what might be regarded as somewhat paradoxical behaviour: why rational utility-maximising individuals might refuse an increase in their disposable income. We have begun by tracing the origins of this work to the sociopsychological literature. For all the insights such an approach provides, it is of limited use in constructing an economic framework that is to form the basis of a subsequent econometric analysis. What we have shown is that simple economic models can be constructed that provide an understanding about the non-take-up problem. Furthermore, such models can be extended relatively easily in order to account for a more dynamic decision-making environment.

By introducing the notion of some form of transaction cost faced in the decisionmaking process, individuals are deterred from taking-up benefits for one or several of the following reasons: a lack of knowledge of the benefit, the hassle of lodging a claim, and the stigma of being a benefit recipient. We have constructed an economic model of the cost of claiming that provides a deeper understanding of how, in particular, the stigma associated with a claim fits into a wider model of take-up.

Throughout the economic analysis we have paid particular attention to various issues that might arise when considering empirical models of take-up. We believe that one of these issues (measurement error) is of prime importance in an econometric analysis and the issue will be dealt with in the next chapter (Chapter 4). Furthermore we have provided an economic framework for constructing an empirical take-up model in which (i) the current take-up decision is dependent upon past success or failure in takingup and (ii) future events and expectations affect current decision-making. This analysis will form the underlying theory to the majority of models presented in the chapter dealing with panel aspects of take-up (Chapter 5).

## CHAPTER 4

## THE IMPACT OF MEASUREMENT ERROR IN AN ECONOMETRIC MODEL OF BENEFIT TAKE-UP

### 4.1 Introduction

In this chapter we consider static empirical models of take-up based on the underlying micro-theory of Chapter 3. In particular, this chapter deals with the estimation of binary choice models (notably logit models) when one of the explanatory variables is subject to measurement error. In the previous chapters much attention has been paid to the possibility of errors entering the computation of one of our main explanatory variables, the entitlement level of Income Support (IS). Our IS algorithm of Chapter 2 computes for each of the individuals in our sample an IS entitlement. Since this computed entitlement is to some degree somewhat error-prone (see Chapter 2 for details), it makes sense to incorporate this scope for measurement error into our model of take-up.

The model we will consider is simple, the complications arising from the introduction of measurement error in the explanatory variables. We assume there is no uncertainty in claming so that the basic decision-making model from Chapter 3 applies. Hence, the model is motivated by a utility maximising individual who will take-up the benefit if her net utility from doing so exceeds zero and will not take-up otherwise. The response variable is thus binary, taking a value of one if the individual does take-up and a value of zero if she does not take-up. Our interest lies in the factors which determine whether an individual will take-up so that an appropriate model would be either a univariate logit or probit model. As will be seen, such models are complicated when we
can no longer assume that all the covariates are accurate measures of what they are supposed to measure. When at least one of these covariates is measured with error (in our case the IS entitlement) the simple logit/probit model no longer produces consistent estimates of the parameters in the model. The methods proposed in this chapter attempt to overcome this inconsistency.

This chapter is structured as follows: in Section 4.2 a brief review of the empirical take-up literature is presented. The majority of this work is based on the univariate logit or probit model with no consideration for measurement error. A discussion of the basic theory of linear, non-linear, and binary choice measurement error models follows in Section 4.3. This basic theory is useful in that it highlights many of the results that are both applicable and at times at odds with more complicated measurement error models. This is followed by Section 4.4 where we apply two measurement error techniques for binary choice models to the take-up problem: (i) approximation estimators and (ii) simulation extrapolation. As will be seen, both methods give rise to estimates that are quite different to those resulting from not accounting for measurement error. Finally, some concluding remarks are made and the scope for further work is highlighted in Section 4.5.

### 4.2 Empirical Studies of Take-Up

As noted in the introduction to this chapter, most of the empirical work on benefit take-up has centred around the univariate logit/probit model with no account for covariate measurement error. The aim of such an econometric analysis is to single out those factors which determine whether an individual entitled to IS will take-up her entitlement. Covariate measurement error is an issue of interest since it has been found in previous studies that the level of entitlement is one of the key determinants of take-up, and we know that our computation of this entitlement is subject to various errors. ${ }^{1}$ In turn, it will become apparent that covariate measurement error has an effect on a simple logit/probit

[^66]analysis. However, so far, apart from the work by Duclos (1992a\&b and 1995), there are to our knowledge no studies that take into account this scope for measurement error.

Consider first the simple logit/probit model which takes no account of measurement error. Such models have become standard analytical tools in econometrics and are well reviewed in Amemiya (1981), Maddala (1983), Dhrymes (1984) or Pudney (1989). ${ }^{2}$ To illustrate the use of such models in the analysis of take-up, suppose we have data with observations on $n$ individuals entitled to IS, concerning their socio-economic characteristics and their take-up decisions. Henceforth let the dependent variable be

$$
y_{i}= \begin{cases}1 & \text { if the } i-\text { th individual takes }-u p  \tag{4.1}\\ 0 & \text { otherwise }\end{cases}
$$

where $i=1, \ldots, n$. Let $\mathbf{x}_{\mathbf{i}}$ be a vector of covariates that are thought to determine the decision to take-up (including socio-economic characteristics). Now suppose that the $i$-th individual derives the following utilities:

$$
\begin{array}{ll}
u_{i 1}=\beta_{1}^{\prime} \mathbf{x}_{i 1}+\varepsilon_{i 1} & \text { if she decides to claim } \\
u_{i 0}=\beta_{0}^{\prime} \mathbf{x}_{\mathbf{i} 0}+\varepsilon_{i 0} & \text { otherwise }
\end{array}
$$

where $\beta_{1}$ and $\beta_{2}$ are unknown parameter vectors and $\varepsilon_{i 1}$ and $\varepsilon_{i 0}$ are i.i.d. error terms. For simplicity assume no transaction costs and no uncertainty in the take-up decision. A claim for IS will occur only if $u_{i 1}>u_{i 0}$, i.e. if the following holds:

$$
\begin{equation*}
\varepsilon_{i 0}-\varepsilon_{i 1}<\beta_{1}^{\prime} \mathbf{x}_{i 1}-\beta_{0}^{\prime} \mathbf{x}_{i 0} \tag{4.2}
\end{equation*}
$$

Hence the probability of making a claim is given by

$$
\begin{align*}
\operatorname{Pr}\left(y_{i}=1 \mid \mathbf{x}_{\mathbf{i}}\right) & =\operatorname{Pr}\left(\varepsilon_{i 0}-\varepsilon_{i 1}<\beta_{1}^{\prime} \mathbf{x}_{\mathbf{i 1}}-\beta_{0}^{\prime} \mathbf{x}_{\mathbf{i} 0}\right) \\
& =\int_{-\infty}^{\beta^{\prime} \mathbf{x}_{\mathbf{i}}} f_{i}(\varepsilon) d \varepsilon  \tag{4.3}\\
& =F\left(\beta_{1}^{\prime} \mathbf{x}_{\mathbf{i 1}}-\beta_{0}^{\prime} \mathbf{x}_{\mathbf{i} 0}\right) \\
& =F\left(\beta^{\prime} \mathbf{x}_{\mathbf{i}}\right)
\end{align*}
$$

[^67]where $f(\varepsilon)$ is the density function of $\varepsilon_{i 0}-\varepsilon_{i 1}, F($.$) the corresponding distribution$ function and $\beta$ is the parameter vector to be estimated. If $F($.$) is normal we have the$ probit model
\[

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=1 \mid \mathbf{x}_{\mathbf{i}}\right)=\Phi\left(\beta^{\prime} \mathbf{x}_{\mathbf{i}}\right) \tag{4.4}
\end{equation*}
$$

\]

whereas if $F($.$) is logistic we have the familiar logit model$

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=1 \mid \mathbf{x}_{\mathbf{i}}\right)=\frac{\exp \left(\beta^{\prime} \mathbf{x}_{\mathbf{i}}\right)}{1+\exp \left(\boldsymbol{\beta}^{\prime} \mathbf{x}_{\mathbf{i}}\right)} \tag{4.5}
\end{equation*}
$$

It is well documented that since the logistic distribution and the cumulative normal distribution are quite similar (except in the tails of the distribution), results based on either the logit or the probit will be alike. In fact, over the approximate interval $0.1 \leq \operatorname{Pr}\left(y_{i}=1 \mid \mathbf{x}_{\mathbf{i}}\right) \leq 0.9$, Amemiya (1981) suggests that the logit parameters are linearly related to the probit parameters by a factor of 0.625 . For our purposes the logit model will be the preferred choice since, as will be seen, it is arithmetically less burdensome for the measurement error models proposed.

Both the naive logit and the naive probit model have been the usual choice for empirical models of take-up. ${ }^{3}$ One of the earliest empirical take-up studies is provided by Altman (1981) for the means-tested Supplementary Benefit (SB). ${ }^{4}$ She considers only male pensioners drawn from the Family Expenditure Survey (FES) 1970-77 and presents results from a variety of naive logit models (these differ mostly in the choice of covariates). Her findings suggest that only age and whether an individual is a private tenant have a significant positive effect on the probability of take-up. The SB level itself is found to have a positive effect as well but this effect is statistically insignificant. However, pensioners are a particularly difficult sample to consider in a take-up analysis and the potential scope for measurement error is large. Fry and Stark (1993) though point out that the FES provided quite accurate data for pensioner SB receipt prior to 1983, deteriorating only thereafter.

[^68]Pensioners as well as non-pensioners are considered by Blundell et al. (1988) for Housing Benefit (HB) take-up. ${ }^{5}$ They use a sample from the 1984 FES and present probit results for pensioners and non-pensioners respectively. In both samples the HB entitlement level is found to have a significant positive effect on the probability of takeup. Moreover, their analysis recognises the scope for measurement error in the computation of the HB entitlement but they do not attempt to correct for the effects of such measurement error.

Similarly, Fry and Stark (1989) also adopt the naive probit model for an analysis of SB take-up amongst pensioners and non-pensioners using the 1984 FES, with results very similar to those of Blundell et al. (1988). In a subsequent and more comprehensive study (Fry and Stark (1993)), their analysis is restricted to non-pensioners only using pooled data from the FES 1984-87 for both SB and HB (with a separate analysis for men and women). But as before, and in spite of providing kernel density plots of reported and computed benefit entitlement levels which highlight the scope for measurement error, no account is taken of the potential effects of measurement error on the analysis. Nevertheless, one interesting facet of the Fry and Stark (1993) study is that they exclude a take-up analysis for pensioners on the grounds that the data for this group is simply too unreliable. Later on we will argue in the same vein based largely on the conclusions reached in Chapter 2.

Recent extensions to the basic models for means-tested benefits in the UK have also ignored the issue of measurement error and, instead, have centred on the endogeneity of multiple participation decisions. Thus, attempts have been made to model the joint participation in HB and Family Income Supplement (FIS) ${ }^{6}$ using pooled FES data from the years 1984-87 (see Dorsett and Heady (1991)), and more recently the joint participation in the labour force and the FIS/FC programme using a large pooled FES

[^69]sample of lone mothers from 1978-92 (Bingley and Walker (1995)). ${ }^{7}$ Dorsett and Heady's results suggest that joint take-up of HB and FC rises with increasing amounts of HB entitlements but not with rising FC entitlements. Furthermore, take-up of one benefit also increases take-up of the other. Bingley and Walker detect that an increase in FC entitlements has a strong positive effect on part-time participation in the labour force but somewhat off-setting this, they detect and measure a significant stigma effect of being on FC. ${ }^{8}$

The overall empirical results of the main UK studies are summarised in Table 4.1 (see Appendix 4A for this and all subsequent tables). The table provides a very general picture of those variables which frequently appear as statistically significant in the various studies. The variables are largely chosen to mimic the transaction costs of claiming. For example, private tenants (as compared to local authority tenants) are perhaps more prone to feel some stigma when claiming benefits and are thus less likely to take-up. However, it must be kept in mind that these figures give only a rough measure since no distinction is drawn between the various groups considered (such as pensioners/non-pensioners or men/women). Moreover the table gives no indication of the size of the effects, which are of importance as well. So, from Table 4.1 we note that more than one out of six studies suggest the probability of SB take-up is increased by spells away from work and decreased by being an owner-occupier. The probability of HB takeup is increased with additional children, and decreased by being a private-tenant and with additional household income. As expected the most notable result though remains that both the take-up of SB and HB are reported as being increasing in the level of entitlement itself (in four out of the six studies).

Modelling the joint participation in a social security programme and the labour force has also been a recurrent theme in the US literature on take-up. In the UK the main means-tested benefit IS is aimed at the non-working poor. In contrast, in the US a much larger proportion of benefit programme recipients participate in the labour force.

[^70]Therefore, the incentive effects of such social security programmes (particularly Aid to Families with Dependent Children (AFDC)) on labour force participation are of particular interest in US studies (see Moffitt (1992) for a comprehensive review). ${ }^{9}$

One of the earliest US take-up studies is Hosek (1980) who considers the take-up of the AFDC - Unemployed Fathers programme (AFDC-UF). Such a study clearly considers only benefit programme participation as in the case of an analysis of participation by pensioners only (for a study of the latter see, for example, Coe (1985)). The first studies to analyse the joint labour force/benefit programme participation decision are by Moffitt (1983) on AFDC and Ashenfelter (1983) using the Seattle and Denver Income Maintenance Experiments (SIME \& DIME). Moffitt estimates an hours equation (tobit model) with endogenous AFDC participation whereas Ashenfelter estimates a separate hours equation and probit participation equation. Also drawing on the SIME/DIME experiments Plant (1984) concentrates on long-term participation in social security programmes, a similar theme adopted by Blank (1989) and Moffitt (1987). Blank adopts a duration analysis of AFDC participation using monthly data and, in particular, how the duration of a spell on benefit affects the probability of participation ending, whereas Moffitt presents a simple analysis of AFDC participation trends between 1967-82. Other social security programmes apart from AFDC are studied by Halpern and Hausman (1986) on Disability Insurance and Anderson and Meyer (1994) on Unemployment Insurance.

More recently, US studies have paid increasing attention to multiple program participation. Many low income families receiving AFDC (particularly lone parent heads of household) are also entitled to Medicaid (health insurance), Food Stamps (food subsidies) and subsidised public housing. Fraker and Moffitt (1988) were the first to consider such multiple program participation by modelling the take-up of AFDC and Food Stamps jointly with the labour supply decision. The interactions of Medicaid on labour force participation and the take-up of AFDC are examined by Moffitt and Wolfe (1992) and Yelowitz (1995), the latter of whom provides a good review of the general

[^71]issues involved. A novel approach (estimation by simulation) for multiple participation in three programmes is provided by Keane and Moffitt (1994) for AFDC, Food Stamps, public housing and their interaction with the labour supply decision of female heads of household.

In spite of the now quite vast literature on benefit take-up in the US, none of these studies addresses the issue of measurement error. ${ }^{10}$ One of the main reasons for this deficiency is that most US studies are based either on controlled data sets (e.g. SIME and DIME) or on data sets which are specifically constructed for the purposes of analysing program participation. Thus, unlike UK data sets, frequently used data sets for the analysis of AFDC take-up, such as the Panel Survey of Income Dynamics (PSID) and the Survey of Income and Programme Participation (SIPP), actually contain detailed records of benefit entitlements even though an individual might not be a recipient. As a result there is often no need to compute benefit entitlements thereby reducing the main source for measurement error at source. In addition, since these data sets are collected with the purpose of analysing social security issues, a large proportion of relevant information is usually included in them.

So far, the only study which has concentrated on measurement error in an analysis of take-up is by Duclos (1992a\&b and 1995). He tackles a variety of issues related to take-up beginning with a basic analysis of the computation of take-up rates themselves. ${ }^{11}$ Above all, his work attempts to highlight the systematic and random biases that result in computing percentage take-up figures from data sets such as the FES. Taking into account the scope for general modelling errors, an econometric analysis attempts to provide a better understanding of the magnitude of the claiming (transaction) costs faced by individuals. Using a sample from the 1985 FES his approach allows one to obtain estimates of actual claiming costs to SB claimants. ${ }^{12}$

[^72]Our approach to modelling take-up differs from Duclos's in several important ways. First, we use a more recent and different data set (the British Household Panel Survey). Second, our modelling approach to take-up will follow the 'naive' logit approach as presented by Blundell et al. (1988) and Fry and Stark (1993) above with corrections for biases resulting from measurement error. We accept the general validity of a simple binary choice model, with complications arising solely from the presence of covariate measurement error. Third, our analysis explicitly accounts for the fact that modelling errors (in the form of measurement error) arise from our computation of the required IS entitlement level. Duclos considers modelling errors due to the government agency only, i.e. the analyst's entitlement computation is taken to be correct. Finally, by using a panel data set, we are able to present an analysis of longitudinal take-up issues in Chapter 5.

### 4.3 The Effects of Measurement Error

Before we turn to our estimation of various empirical take-up models it is helpful to begin with a brief summary of the main results relating to the simple linear measurement error model and the more general non-linear model. Such models apply in situations where at least one of the covariates is subject to measurement error. Our prime interest falls on logit models where one of the explanatory variables is measured with error and such models will be discussed later.

Much of the of the work on measurement error models emanates from the statistical literature which still dominates the field, particularly with regard to non-linear models. ${ }^{13}$ Nevertheless, economists have long recognised that by ignoring measurement error spurious conclusions regarding economic relationships can occur. Hence a vast number of applications of the linear errors-in-variables model can be found in economics and more recently econometricians have been paying attention to measurement error in

[^73]non-linear models. ${ }^{14}$ As Hausman et al. (1995) note, interest in measurement error models by econometricians dates to at least the 1930's. More recent applications of measurement error models can be found in a great variety of economic disciplines. (To our knowledge no complete literature review exists to date.) Examples are provided in labour economics where measurement error in earnings data have shown to produce misleading results in labour supply and unemployment duration analyses (see Duncan and Hill (1985), Griliches and Hausman (1986), Bound and Krueger (1991), Christensen and Kiefer (1994) and Pischke (1995)). In the field of health economics it is often the case that self-reported health variables are subject to considerable measurement error (e.g. Butler et al. (1987)) as are many explanatory variables in assessing health productivity (e.g. Headen (1991) and Atkinson and Crocker (1992)). Other interesting applications of the impact of measurement error can be found in explaining wage differentials as a result of sex discrimination (Schafer (1987a)), managing portfolio returns in finance (Rahman et al. (1991)), the analysis of intergenerational income transfers (Solon (1992) and Zimmerman (1992)), the estimation of various forms of demand functions (Brester and Wohlgenant (1993) and Uri (1994)), the assessment of poverty rates (McGarry (1995)), human capital studies of the return to schooling (Blackburn and Neumark (1995)), and finally the estimation of Engel curves (Aasness et al. (1993) and Hausman et al. (1995)).

For a recent outline of the many current techniques for both linear and non-linear measurement error models the interested reader is referred to the American Mathematical Society Conference Proceedings (Brown and Fuller (1990)). Linear measurement error models are extensively reviewed by Fuller (1987) whereas a very up-to-date review of non-linear measurement error models (including logit/probit models) can be found in Carroll, Ruppert and Stefanksi (1995).

### 4.3.1 Linear Measurement Error Models

We now turn to a brief discussion of the impact of measurement error in linear models (or the 'errors-in-variables' model). Suppose the data of interest are given by $\left\{y_{i}, x_{i}\right\}_{i=1}^{n}$ and

[^74]our aim is to estimate the parameter $\beta_{x}$ in a simple scalar model with no intercept and a single explanatory variable such that
\[

$$
\begin{equation*}
y_{i}=\beta_{x} x_{i}+\varepsilon_{i} \quad \varepsilon_{i} \sim I N\left(0, \sigma_{\varepsilon}^{2}\right) \tag{4.6}
\end{equation*}
$$

\]

Instead of observing the true covariate, $x_{i}$, we actually observe a surrogate covariate

$$
\begin{equation*}
z_{i}=x_{i}+v_{i} \quad v_{i} \sim \operatorname{iid}\left(0, \sigma_{v}^{2}\right) \tag{4.7}
\end{equation*}
$$

where $\operatorname{Cov}\left(\varepsilon_{i}, x_{i}\right)=\operatorname{Cov}\left(\varepsilon_{i}, z_{i}\right)=\operatorname{Cov}\left(\varepsilon_{i}, v_{i}\right)=\operatorname{Cov}\left(y_{i}, v_{i}\right)=0$. Hence $x_{i}$ is said to be subject to (random) additive measurement error and is often referred to as the unobserved latent variable. ${ }^{15}$ The observed data are $\left\{y_{i}, z_{i}\right\}_{i=1}^{n}$ so that the model we actually estimate is described by

$$
\begin{equation*}
y_{i}=\beta_{z} z_{i}+\xi_{i} \quad \text { where } \xi_{i}=\varepsilon_{i}-\beta v_{i} \tag{4.8}
\end{equation*}
$$

The OLS (slope) estimator of $\beta_{z}$ from (4.8), denoted $\hat{\beta}_{z}$, is not consistent for $\beta_{x}$ since $\operatorname{Cov}\left(\xi_{i}, z_{i}\right) \neq 0$. It is in fact a consistent estimate of

$$
\begin{array}{ll} 
& \beta_{z}=\beta_{x}\left(\frac{\sigma_{x}^{2}}{\sigma_{z}^{2}}\right)=\beta_{x}-\beta_{x}\left(\frac{\sigma_{v}^{2}}{\sigma_{z}^{2}}\right) \\
\text { i.e. } & \operatorname{plim}\left(\hat{\beta}_{z}\right)=\frac{\beta_{x}}{\left(1+\sigma_{v}^{2} / \sigma_{x}^{2}\right)} . \tag{4.9}
\end{array}
$$

This is the well-known attenuation effect of using OLS in the errors-in-variables model. The attenuation effect describes the bias towards zero for the parameter estimates in the observed model.

The key point is that when we estimate the simple model of (4.6) the true OLS estimator, $\hat{\beta}_{x}$, yields consistent estimates for $E\left(y_{i} \mid x_{i}\right)$. However, the OLS estimator from the model of (4.8), $\hat{\beta}_{z}$, produces consistent estimates for $E\left(y_{i} \mid z_{i}\right)$. Therefore, unless the two conditional expectations are identical, simple OLS in our model will produce inconsistent estimates for the true model of interest.

In addition to the attenuation effect there are two other noteworthy consequences of measurement error. First, the observed data are more noisy so that the relationship

[^75]between $y_{i}$ and the surrogate $z_{i}$ is likely to be weaker than that between $y_{i}$ and the true $x_{i}$. This effect becomes apparent as an increase in the residual variance
\[

$$
\begin{equation*}
\operatorname{Var}\left(y_{i} \mid z_{i}\right)=\sigma_{\varepsilon}^{2}+\beta_{x}^{2}\left(\frac{\sigma_{v}^{2} \sigma_{x}^{2}}{\sigma_{x}^{2}+\sigma_{v}^{2}}\right) . \tag{4.10}
\end{equation*}
$$

\]

Second, measurement error causes the error in the OLS estimator to be non-linear. The error in the observed OLS estimator is given by

$$
\begin{equation*}
\hat{\beta}_{z}-\beta=\frac{\sum_{i=1}^{n}\left(x_{i}-\bar{x}\right)\left(\xi_{i}-\bar{\xi}\right)+\left(v_{i}-\bar{v}\right)\left(\xi_{i}-\bar{\xi}\right)}{\sum_{i=1}^{n}\left(x_{i}-\bar{x}\right)^{2}+2\left(x_{i}-\bar{x}\right)\left(v_{i}-\bar{v}\right)+\left(v_{i}-\bar{v}\right)^{2}} \tag{4.11}
\end{equation*}
$$

where a bar over a variable indicates the sample mean. Therefore, the bias in $\hat{\beta}_{z}$ is a result of the correlation between $v_{\mathrm{i}}$ and $\xi_{\mathrm{i}}$. If, in addition, the assumptions about the measurement error do not hold, so that $v_{\mathrm{i}}$ is correlated with $x_{i}$ and $\varepsilon_{\mathrm{i}}$, then additional terms will contribute to this bias expression.

In the multivariate case the bias effects become more complicated and depend on the correlations between the explanatory variables. Even when only one of the covariates is measured with error and all others are error-free, the bias effects can be transmitted to all parameter estimates. Consider, for example, a simple model with two covariates such that

$$
\begin{equation*}
y_{i}=\beta_{\mathbf{x}}^{\prime} \mathbf{x}_{\mathbf{i}}+\varepsilon_{i} \quad \text { and } \mathbf{x}_{\mathbf{i}}=\left[\mathrm{x}_{1 \mathrm{i}}, \mathrm{x}_{2 \mathrm{i}}\right]^{\prime} \tag{4.12}
\end{equation*}
$$

Suppose that only one of the covariates is measured with error so that we observe $z_{i}=x_{1 i}+v_{i}$ as in (4.7) and an error-free $x_{2 i}$ with corresponding parameters $\beta_{1}$ and $\beta_{2}$ respectively. It can be shown that the parameter estimates for both explanatory variables are inconsistent, provided the explanatory variables are correlated. In fact, the bias on the coefficient for the variable measured with error, $x_{1 i}$, is of magnitude $-\beta_{1}\left(\sigma_{v}^{2} / \sigma_{x}^{2}\right) /\left(1-\rho^{2}\right)$ whereas the bias on the coefficient for the error-free variable, $x_{2 i}$, depends on the bias of the coefficient for the variable measured with error and is of magnitude $-\rho\left[\operatorname{bias}\left(\beta_{1}\right)\right]$, where $\rho$ is the correlation coefficient between the two variables.

In order to consistently estimate linear measurement error models certain assumptions have to be made without which the model parameters are not identifiable. One common assumption is that the variance of the measurement error, $\sigma_{v}^{2}$, is known directly or, alternatively, the ratio of error variances, $\sigma_{\varepsilon}^{2} / \sigma_{v}^{2}$ with $\sigma_{v \varepsilon}=0$ (zero covariance) is known. In practice, this often means having access to a replication or validation data set providing information on the true covariate, $x_{1 i}$. Obtaining such data is often difficult if not impossible in econometric studies.

A more widely used alternative is instrumental variables estimation where a further variable, say $w_{i}$, is introduced which is correlated with $x_{i}$ and uncorrelated with both $v_{i}$ and $\varepsilon_{\mathrm{i}}$. Furthermore, $w_{i}$ should not in itself have an effect on the response variable, $y_{i}$. Under these conditions the instrumental variables estimator (which is consistent for $\beta_{x}$ ) is given by

$$
\begin{equation*}
\hat{\beta}_{I V}=\frac{\sum_{i=1}^{n}\left(y_{i}-\bar{y}\right)\left(w_{i}-\bar{w}\right)}{\sum_{i=1}^{n}\left(x_{i}-\bar{x}\right)\left(w_{i}-\bar{w}\right)} \tag{4.13}
\end{equation*}
$$

where we assume that $\sigma_{x w} \neq 0$ (note though that no assumptions need to be made about $\sigma_{\varepsilon v}$, nor the assumption that $\sigma_{x v}=0$ ).

One of the major drawbacks of using instrumental variables is that the process of finding a suitable instrumental variable satisfying all three conditions is not an easy task. Even when a suitable instrumental variable has been found there is no guarantee that it will be particularly accurate: if the relationship between $x_{i}$ and its instrument $w_{i}$ is weak then not much information will be shed on the relationship of interest, that of $y_{i}$ and $x_{i}$ (see Chesher (1991b)). ${ }^{16}$

[^76]
### 4.3.2 Non-Linear Measurement Error Models

In this section we briefly consider the impact of measurement error in non-linear measurement error models. In recent years both the econometrics literature and the statistics literature have concentrated on a much wider class of measurement error models. In the econometrics literature ${ }^{17}$, the focus has been on general non-linear measurement error models of the form

$$
\begin{equation*}
y_{i}=h\left(\beta ; x_{i}\right)+\varepsilon_{i} \quad \text { with } \varepsilon_{i} \sim I N\left(0, \sigma_{\varepsilon}^{2}\right) \tag{4.14}
\end{equation*}
$$

where, as before, we observe $z_{i}=x_{i}+v_{i}$ and $h($.$) denotes a real-valued continuous non-$ linear function in either $x_{i}$ or $\beta .{ }^{18}$ The same assumptions as in the linear model above apply to the distribution of the measurement error.

In parallel development, the statistics literature on non-linear measurement error models has centred on generalized linear models (GLMs). ${ }^{19}$ The statistics literature has been more substantial and, unlike the econometrics literature, methods have been specifically developed for (cross-section) binary choice models. In this chapter we will use two different estimation methods for measurement error logit models so that we shall consequently concentrate primarily on the contributions of the statistics literature.

In order to consider non-linear measurement error models we focus on densities of the variates and covariates (since likelihood functions can be derived from them). In general non-linear measurement error models we assume that a surrogate covariate $z_{i}$ is observed in place of $x_{i}$, with a conditional density function $f_{Z \mid X}(z \mid x)$. We also assume the measurement error to be additive, random and uncorrelated as in the linear case. ${ }^{20}$ More specifically, to capture the notion that $v_{i}$ is measurement error we assume that

[^77]$f_{Y X, Z}(y \mid x, z)=f_{Y \mid X}(y \mid x)$ so that the distribution of $y_{i}$ given $x_{i}$ is independent of $z_{i}$ (where, for convenience, we have suppressed $i$-subscripts in the density functions). This assumption is often referred to as measurement error being non-differential, i.e. the surrogate covariate contains no additional information about the conditional distribution of the response variable that is not already contained in the true covariate.

As before, our interest centres on estimating the true parameters in some unobserved model described by a density $f_{Y \mid X}(y \mid x)$. The model we observe is a based on the density of $y_{i}$ conditional on the surrogate variable, i.e. $f_{Y \mid}(y \mid z)$. This density can be expressed as

$$
\begin{equation*}
f_{Y \mid Z}(y \mid z)=\int f_{Y \mid X}(y \mid x) f_{X \mid Z}(x \mid z) d x \tag{4.15}
\end{equation*}
$$

To elucidate the effects of measurement error on conditional densities, Chesher (1991a) suggests a small variance approximation for equation (4.15): ${ }^{21}$ The approximation is based on a second-order Taylor series expansion with respect to $x_{i}$ about the mean of $x_{i}$ conditional on $z_{i}$, i.e. $E\left(x_{i} \mid z_{i}\right)=\mu_{X \mid Z}$, and is given by

$$
\begin{align*}
& f_{Y \mid Z}(y \mid z) \cong \int\left\{f_{Y \mid X}(y \mid x)+\left(x-\mu_{X \mid Z}\right) f_{Y \mid X}^{\prime}\left(y \mid \mu_{X \mid Z}\right)\right. \\
&\left.+\frac{1}{2}\left(x-\mu_{X \mid Z}\right)^{2} f_{Y \mid X}^{\prime \prime}\left(y \mid \mu_{X \mid Z}\right)\right\} f_{X \mid Z}(x \mid z) d x . \tag{4.16}
\end{align*}
$$

Expression (4.16) simplifies to

$$
\begin{equation*}
f_{Y \mid Z}(y \mid z) \cong f_{Y \mid X}\left(y \mid \mu_{X \mid Z}\right)+\frac{1}{2} \operatorname{Var}(x \mid z) f_{Y \mid X}^{\prime \prime}\left(y \mid \mu_{X \mid Z}\right) \tag{4.17}
\end{equation*}
$$

where $f_{Y \mid X}^{\prime}($.$) and f_{Y X X}^{\prime \prime}($.$) are the first- and second-partial derivatives with respect to x_{i}$ respectively. Note that the first-order term equals zero after taking expectations.

From (4.17) we see that the distribution of $y_{i}$ conditional on $z_{i}$ consists of (i) a term giving the distribution of $y_{i}$ conditional on the conditional mean of $x_{i}$ and (ii) a term involving the variance of $x_{i}$ conditional on $z_{i}$ together with a term indicating the concavity/convexity of the density of $y_{i}$ given the conditional mean of $x_{i}$. An attenuation effect is still present but the effect of measurement error in non-linear models

[^78]is more complex compared to the linear case. We cannot simply detect a bias towards zero as in simple linear models. In particular, note that the conditional density $f_{Y Z}(y \mid z)$ will be flattened if $f_{Y \mid X}^{\prime \prime}\left(y \mid \mu_{X \mid Z}\right)<0$ (concave) and raised if $f_{Y \mid X}^{\prime \prime}\left(y \mid \mu_{X \mid Z}\right)>0$ (convex). ${ }^{22}$

Having outlined the main points with regard to linear and non-linear measurement error models we will now turn our attention to binary choice measurement error models (logit/probit models in particular). These models can be regarded as a specific class of non-linear models or as a specific form of GLM.

### 4.3.3 Binary Choice Measurement Error Models

It is well documented that maximum likelihood estimation in logit and probit models yields consistent estimates (see, for example, Dhrymes (1986)). However, in the presence of covariate measurement error these 'naive' estimators are no longer consistent (see Michalek and Tripathi (1980), Stefanski and Carroll (1985), and Yatchew and Griliches (1985)). To date, a considerable number of different techniques have been proposed to overcome this inconsistency and this section briefly surveys some of these methods. ${ }^{23}$

One of the earliest papers (Michalek and Tripathi (1980)) performs a simulation study of the logit model and its behaviour in the presence of covariate measurement error. The estimated naive parameters are found to be biased and this bias becomes worse the greater is the measurement error. However, the paper itself suggests no particular method to correct for the bias. The first proposed method to deal with covariate measurement error in binary choice models uses a pseudo-maximum likelihood estimator (Carroll et al. (1984)). The method is suggested for the computationally more tractable probit model with additive measurement error and the appropriate likelihood function is given. Strong assumptions are required though: in addition to the standard assumptions about the measurement error, the unobserved true variable is assumed to be normally distributed

[^79]with known mean and variance. In practice the mean and variance are usually unknown so that a simple replication method is suggested to estimate these unknowns.

Likelihood based methods are also suggested by Stefanski and Carroll (1985) who propose three maximum likelihood estimators based on small measurement error asymptotics (a technique we will make use of later). Unlike the pseudo-maximum likelihood estimator none of the three methods makes any distributional assumptions about the underlying true covariate and one of the three methods makes no assumptions about the distribution of the measurement error either (the other two methods assume measurement error to be normally distributed).

In recent years many new approaches for estimation in binary choice measurement error models have emerged. Schafer (1987 and 1993), for example, suggests treating the underlying true covariate as missing data and thus apply a standard but involved missing data technique to maximise the full likelihood function (the expectation-maximisation algorithm). Carroll and Wand (1991) suggest the use of a semi-parametric estimator particularly useful when the model assumptions are questionable. Instrumental variables estimation, usually restricted to the linear model, has also recently come forth as a further method (Carroll and Stefanski (1994) and Stefanski and Buzas (1995)). These methods are similar to instrumental variable estimation in linear models as outlined in Section 4.3.1.24

So far, applications of these methods have been confined mostly to the biometrics and medical statistics literature, especially medical epidemiology (see Whittemore (1990) for a survey), with to our knowledge, no applications in econometrics. ${ }^{25}$ This is perhaps somewhat surprising given the widespread use of logit and probit models (and variants thereof) in applied microeconometrics and the scope for measurement error in economic data. One of the major hindrances for using these techniques is most likely to be the lack of suitable validation data. Most techniques and methods suggested in the literature

[^80]surveyed above require some data with accurate information on certain aspects of the measurement error (usually its mean and variance). We will show how we are able to construct and use a validation data set and, as a result, apply two general types of measurement error logit models to an economic problem, namely the take-up of IS in Britain.

### 4.4 Modelling the Take-Up of IS in Britain

In this section we consider first the naive estimates of a simple empirical take-up model. Starting from this fully specified model we are able to reduce the choice of covariates before we estimate models that account for measurement error. As noted in Section 4.3.1 above, the effects of measurement error become more complex when a greater number of covariates is introduced. Hence our aim is to estimate a relatively parsimonious empirical model of take-up. For this parsimonious model two measurement error methods will be used: (i) models based on approximations similar to the one in equation (4.17), also known as regression calibration techniques, and (ii) the very recent technique of simulation extrapolation, or SIMEX for short.

### 4.4.1 Choice of Covariates

The reference point for the purpose of making comparisons is a naive logit model as specified in equation (4.5) above. The choice of covariates in our basic model is largely determined by the underlying theory of Chapter 3 and by previous studies of take-up as reviewed above. Thus, in addition to the IS entitlement level and any other income, the other explanatory variables can be categorised as capturing the effects of one or several of (i) information, (ii) stigma, and (iii) hassle. However, in many cases we must rely on proxy variables since it is often difficult if not impossible to find variables that capture any of the above effects. A brief discussion of our choice of covariates/dummies and the anticipated signs on their estimated parameters follows.
of heart disease and the factors associated therewith). Palca (1990) provides a short intuitive explanation of the biases that arise from not accounting for measurement error in an analysis of heart disease.

In line with other empirical studies and based on some of the evidence of Chapter 3, we expect the IS entitlement to have a strong positive effect on the probability to takeup (see Table 4.1). We follow Blundell et al. (1988) by using the natural logarithm of the IS entitlement since, although we believe the IS entitlement to increase take-up, we expect this to occur at a diminishing rate the greater the entitlement level is. On the other hand, any other income the claimant may receive is likely to reduce the probability of take-up. This is particularly true for a means-tested benefit such as IS where roughly speaking, every additional pound earned is deducted from the entitlement. ${ }^{26}$ Thus, if claimants are aware of this, it may lead to their own perception as being non-eligible for IS.

The sign of the parameter estimates for the other covariates are often more difficult to predict since many of these possibly capture more than a single effect. For example, holding a degree might suggest a better knowledge of the IS system and thus a greater probability to take-up but, at the same time, a degree holder might feel a greater sense of stigma. Being a couple and thus having an additional adult in a household might make a claimant feel more deserving and in need of IS. At the same time though, additional adults might be indicative of the claiming unit being more able to cope with hardship. Similarly, we would also expect a greater number of children to increase the probability to take-up but, on the other hand, a larger number of children might conceivably increase the hassle of lodging a claim. ${ }^{27}$ We also try to capture frictional effects arising from the duration of unemployment. Individuals who have only recently become unemployed might be in the process of applying for IS (so that it is not reported in our survey data set) or might believe they are able to obtain a job in due course before the need for benefit dependency arises. Thus, we would expect the longer the duration of unemployment the more likely that an individual will take-up. However, a better

[^81]
## Chapter 4

understanding of such dynamic aspects will be obtained in Chapter 5 where we explicitly account for the dynamics in take-up.

### 4.4.2 The Naive Logit Model

The data we use for our estimation comes from the British Household Panel Survey (BHPS) and is described in Chapter 2. For the analysis in this chapter we utilise the cross-sectional properties of the BHPS using a pooled data set from the first four waves A to $D$. In addition to all entitled individuals of wave $A$ we use all newly entitled individuals of wave B, C and D. ${ }^{28}$ Furthermore, we restrict our attention to nonpensioners only. The reason we exclude an analysis for pensioners is that our results from Chapter 2 strongly indicate our inability to obtain accurate information about pensioner take-up. ${ }^{29}$

From a cross-section perspective, one of the main advantages the BHPS has over traditionally used data sets for a take-up analysis (notably the FES) is a greater variety of socio-economic characteristics. Hence we have included some relevant dummy variables based on BHPS subjective measures (see the description of variables in Appendix 4A). We would expect a greater likelihood of take-up among individuals who feel they are worse off than they were a year ago, whereas this might not be the case for those who anticipate worse financial circumstances in the coming year. The latter may believe they are still able to cope better with current circumstances before the need for benefit dependency arises. ${ }^{30}$

The main pooled data set we work with has 1,199 observations for whom a positive benefit entitlement was established. These observations are comprised as

[^82]follows: 510 observations are found to be entitled in wave A. ${ }^{31}$ In wave $B$ we can identify a further 281 observations which are 'newly' entitled, that is, they are found to be entitled in wave B and have not been entitled in wave A. Similarly, we find a further 213 newly entitled observations in wave C and 195 newly entitled observations in wave D. Thus, by pooling these observations our resulting data set contains 1,199 entitled observations of which 887 are IS recipients. In other words, the take-up rate in our sample is roughly 74 percent. Some basic descriptive statistics for the main data set are given in Table 4.2. The average amount of the IS entitlement is $£ 52.13$ and just over half of our sample are unemployed and have been so for, on average, just over 15 weeks. The majority of entitled individuals are single (with lone parents and couples thereafter forming roughly equal proportions) and close to half of the sample live in local authority rented accommodation. More than half of the sample questioned about current financial difficulties responded positively to the questions.

The parameter estimates from our fully specified naive logit model are presented in Table 4.3.32 In addition, we also calculate the marginal effects evaluated at the means of the explanatory variables. These allow a meaningful interpretation of the magnitude of these parameter estimates, especially when compared to one another. Most of the signs on the parameters are as expected and our hypothesis of an increasing IS entitlement leading to a greater probability of take-up is confirmed. However, the magnitude of this effect is rather small. For every one percent increase in the IS entitlement level the likelihood to take-up increases by approximately only 0.03 percent (all other variables remaining unchanged). Likewise, our hypothesis of decreasing take-up with increasing levels of non-benefit income is also validated. A one percent increase in other income decreases the likelihood to take-up by 0.1 percent (all other variables remaining unchanged). The largest marginal effects apply to the level of other income, and the dummy variables for lone parenthood, tenancy status, and whether an individual is sick or

[^83]not. When interpreting these marginal effects it is noteworthy that they are evaluated at the means of the explanatory variables. Later on in this chapter we will see that the likelihood to take-up is particularly sensitive to changes at the lower level of IS entitlement (i.e. between $£ 10$ to $£ 30$ ) which is considerably less than the mean IS entitlement of $£ 52.13$ in our sample.

Overall, there are a greater number of covariates that have a positive effect on the probability to take-up, with only four covariates having a negative effect. For many of the variables for which the expected signs were indeterminate it appears that we are able to resolve the ambiguity. So, at first sight, one could argue that a greater number of dependent children does in fact increase the probability of take-up, being a couple induces a stronger feeling of deserving the benefit, and being in possession of a degree allows one to make a more informed decision. However, many of these variables are most inaccurately determined in the sense that the standard error for the parameter estimate is often greater than the parameter estimate itself (in addition, the marginal effects for these variables are very small as well). In fact, for about a third of the covariates this is the case. Thus, using our data we can reject many of the hypotheses previously suggested and thereby estimate a more parsimonious model. Henceforth, we drop the covariates no. kids, female, head, degree, owner, and subject 2 and estimate what we refer to as a reduced logit model. This reduced model then serves as a basis for comparison with the two measurement error corrected models considered in Section 4.4 .3 and 4.4.4 below. In addition, dropping the insignificant variables has the added advantage of presenting us with a simpler and more convenient model to estimate. (We noted above that the effects of measurement error are complicated in models with more than one covariate. So, reducing the number of covariates should be of benefit.)

[^84]
### 4.4.3 Approximation Estimators

In this section we consider the application of two estimators based on small measurement error variance approximations similar to the expansion of equation (4.17) above. Two stages are involved for this technique: first, a calibration step whereby data on a validation sample are used to obtain predicted values for the covariate subject to measurement error. This is followed by a logit regression step which in its simplest form takes no account for measurement error and in a more complex form does account for measurement error. In this second step the logit regression is run on the covariate predicted in the first stage. The two estimators will be referred to as the zero-order approximation estimator and the second-order approximation estimator respectively. The former was first suggested by Rosner et al. $(1989,1990$ and 1992) whereas a secondorder approximation estimator similar to ours was first applied by Kuha (1994).

We begin with a comment on notation. Consider the case of a single scalar covariate subject to measurement error, i.e. $z_{i}$ is observed instead of the true covariate $x_{i}$. In addition, we observe a further scalar covariate which is not (for all practical purposes) subject to measurement error, denoted $x_{i}^{*} \cdot{ }^{33}$

For the first step we must make use of a validation data set containing accurate information about the true covariate. A validation data set is often a subset of the main data set (internal validation) or it might be an external data set containing the relevant information of interest (external validation). ${ }^{34}$ Whereas the main data set contains data on $\left\{y_{i}, z_{i}, x_{i}^{*}\right\}_{i=1}^{n}$, the validation data set must contain information on $\left\{y_{i}, x_{i}, x_{i}^{*}, z_{i}\right\}_{i=1}^{n}$, i.e. data on both the true covariate and the surrogate covariate. Consequently such a data set

[^85]allows evaluation of the measurement error variance and (as is necessary for this method) it permits estimation of a relationship between the true and observed covariate.

Previously we noted that in the data sets used by micro-econometricians validation data is often unavailable. ${ }^{35}$ We attempt to overcome this problem by using the amount of IS as reported by individuals in our data set as the true covariate, $x_{i}$, and the IS entitlement obtained by our computer program as the surrogate covariate, $z_{i}$. This approach is clearly open to criticism, specifically since it inherently assumes (i) that the IS entitlement as reported in the BHPS is error-free, and (ii) that measurement error is non-differential, i.e. $f_{Y \mid X, Z}(y \mid x, z)=f_{Y X}(y \mid x)$. With respect to point (ii) we note that our response variable is partly generated by our surrogate covariate so that non-differential measurement error does not necessarily hold. Similarly, as concerns point (i) this is clearly not always the case either, but nevertheless we believe that for our purposes it is the closest we can get to a validation data set. As an alternative consider the following two methods of obtaining a validation data set:

- Manually computing an IS entitlement for a small random sub-sample of individuals (say, approximately $n=50$ ) from the BHPS;
- Obtaining information from an external data set such as the IS Annual Enquiry which provides information on recipients of IS only, or alternatively, the FES with perhaps more accurate data on incomes and benefit receipt.
The problems with the first method would be much the same as those encountered with our computation of IS as outlined in Chapter 2 (e.g. lack of suitable variables in the BHPS, imprecise variables when they do exist etc.) whereas the IS Annual Enquiry contains no information about the computed IS entitlement (the observed covariate, $z_{i}$ ) and cannot be used in our analysis without making further restrictive assumptions. ${ }^{36}$ Consequently there would not appear to be any overwhelming reasons to use either a small subset of the BHPS or the IS Annual Enquiry instead of the reported IS

[^86]entitlements in the BHPS itself. However, later in this section we will examine in detail the underlying assumptions of the approximation estimators and how these may impair our conclusions.

The method itself is implemented as follows: in the first step we specify a model for the relationship between $x_{i}, z_{i}$ and $x_{i}^{*}$ in the validation data set. Suppose we assume a linear relationship such that

$$
\begin{equation*}
x_{i}=\gamma z_{i}+\delta x_{i}^{*}+v_{i} \quad \text { where } \quad v_{i} \sim \operatorname{iid}\left(0, \sigma_{v}^{2}\right) \tag{4.19}
\end{equation*}
$$

For estimation purposes it is often convenient to assume that $v_{i}$ is normally distributed with mean zero and variance $\sigma_{v}^{2}$. Then we can estinıate the parameters $\gamma$ and $\delta$ by OLS and use these estimates (denoted $\hat{\gamma}$ and $\hat{\delta}$ ) in the main data set to predict the true covariate, i.e. $\hat{x}_{i}=\hat{\gamma} z_{i}+\hat{\delta} x_{i}^{*}$. In the second step we use the main data set and estimate a logit model of take-up on the predicted covariate, $\hat{x}_{i}$, and the covariate not subject to measurement error, $x_{i}^{*}$. The resulting maximum likelihood logit estimates are biascorrected in the sense that they are less biased than the naive estimates.

The bias-correction of the approximation estimators can be justified in at least three different ways (see Rosner et al. (1989)). First, from equation (4.15) we can express the probability of take-up conditional on the surrogate covariate as

$$
\begin{align*}
\operatorname{Pr}\left(y_{i}=1 \mid z_{i}, x_{i}^{*}\right) & =\int \operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right) f_{X \mid Z, X^{*}}\left(x_{i} \mid z_{i}, x_{i}^{*}\right) d x \\
& =E\left\{\left.\frac{\exp \left(\beta x_{i}+\beta^{*} x_{i}^{*}\right)}{1+\exp \left(\beta x_{i}+\beta^{*} x_{i}^{*}\right)} \right\rvert\, z_{i}, x_{i}^{*}\right\}  \tag{4.20}\\
& =\int \frac{\exp \left(\beta x_{i}+\beta^{*} x_{i}^{*}\right)}{1+\exp \left(\beta x_{i}+\beta^{*} x_{i}^{*}\right)} \times f_{X \mid \bar{X}, X^{*}}\left(x_{i} \mid z_{i}, x_{i}^{*}\right) d x
\end{align*}
$$

[^87]A closed form solution to (4.20) does not exist. However, we can approximate it by using the Taylor series expansion of (4.17) above, i.e. replace $\operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right)$ by its Taylor expansion about $\hat{x}_{i}=\hat{\gamma} z_{i}+\hat{\delta} x_{i}^{*}$ to give

$$
\begin{align*}
\operatorname{Pr}\left(y_{i}=1 \mid z_{i}, x_{i}^{*}\right) \cong & \frac{\exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)}{1+} \exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right) \\
& \quad+\left(\frac{\sigma_{v}^{2}}{2}\right) \frac{\beta^{2} \exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)\left[1-\exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)\right]}{\left[1+\exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)\right]^{3}} \tag{4.21}
\end{align*}
$$

Second, we can replace $\operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right)$ in (4.20) by its Taylor series expansion around $\beta=0$ to give

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=1 \mid z_{i}, x_{i}^{*}\right) \cong \frac{\exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)}{1+\exp \left(\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}\right)}+\left(\frac{\sigma_{v}^{2}}{2}\right) \frac{\beta^{2}\left[1-\exp \left(\beta x_{i}^{*}\right)\right]}{1+\exp \left(\beta x_{i}^{*}\right)} \tag{4.22}
\end{equation*}
$$

Finally, a third way to justify the approximation estimators is to assume that the conditional distribution of $x_{i}$ given $z_{i}$ and $x_{i}^{*}$ is multivariate normal and that

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right)=F\left(\beta x_{i}\right) \cong \exp \left(\beta x_{i}\right) . \tag{4.23}
\end{equation*}
$$

Equation (4.23) applies particularly in those cases where $\operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right)$ is small. Then the conditional distribution of $\exp \left(\beta x_{i}\right)$ given $z_{i}$ and $x_{i}^{*}$ is log-normal with parameters $\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}$ and $\beta^{2} \sigma_{v}^{2}$, and

$$
\begin{align*}
\operatorname{Pr}\left(y_{i}=1 \mid z_{i}, x_{i}^{*}\right) & =E\left\{\operatorname{Pr}\left(y_{i}=1 \mid x_{i}\right) \mid z_{i}, x_{i}^{*}\right\} \\
& \cong \exp \left\{\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}+\frac{1}{2} \beta^{2} \sigma_{v}^{2}\right\}  \tag{4.24}\\
& \cong F\left\{\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}+\frac{1}{2} \beta^{2} \sigma_{v}^{2}\right\}
\end{align*}
$$

We shall concentrate on the expansion of equation (4.21). By using only the first term of expansion (4.21) (or the first term of (4.22)) we obtain the zero-order approximation estimator. The key argument here is that measurement error is so small that terms beyond the first term are very close to zero and thus the approximation is sufficient. However, if higher-order terms are not close to zero, inclusion of the second term might yield a more accurate approximation. The resulting estimator is termed the
second-order approximation estimator, and the argument is that terms beyond secondorder are $O\left(\sigma_{v}^{2}\right)$ which passes to zero.

Finally note that for the second-order approximation estimator we must estimate the model of equation (4.21). As the model stands there is no guarantee that we obtain a proper probability in the interval [0, 1]. However, Chesher and Santos Silva (1995) suggest a suitable transformation for the multinomial logit model based on a transformation for densities such as the one in equation (4.17). ${ }^{37}$ Applied to (4.21) we can rewrite the second-order approximation estimator as

$$
\begin{equation*}
\operatorname{Pr}\left(y_{i}=1 \mid z_{i}, x_{i}^{*}\right) \cong \frac{\exp \left\{\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}+\left(\frac{\beta^{2} \hat{\sigma}_{v}^{2}}{2}\right) \frac{\left(1-e^{\beta \hat{x}_{i}+\beta^{*} x_{i}}\right)}{\left(1+e^{\beta \hat{x}_{i}+\beta^{*} x_{i}}\right)^{2}}\right\}}{1+\exp \left\{\beta \hat{x}_{i}+\beta^{*} x_{i}^{*}+\left(\frac{\beta^{2} \hat{\sigma}_{v}^{2}}{2}\right) \frac{\left(1-e^{\beta \hat{x}_{i}+\beta^{*} x_{i}}\right)}{\left(1+e^{\beta \hat{x}_{i}+\beta^{*} x_{i}}\right)^{2}}\right\}} \tag{4.25}
\end{equation*}
$$

where $\hat{\sigma}_{v}^{2}$ is an estimate of the measurement error variance obtained from the validation data set. The probability in (4.25) does in fact lie in the [0, 1] interval. In addition, finding the MLEs for the second-order approximation estimator is made easier by using the form in (4.25) rather than the one in (4.21). We use an iteratively reweighted least squares algorithm and implement the procedure in SAS/IML (see Appendix 4C for a detailed description).

In order to implement the approximation estimators we use the data set described above in Section 4.4.2. The main data set consists of 1,199 pooled observations from waves A to D of the BHPS. To reiterate, these observations consist of all those individuals for whom a positive benefit entitlement is computed by our micro-simulation program, whether or not they are receiving IS. Previously we noted that the take-up rate for the main data set is roughly 74 percent. In other words, about 887 individuals in the main data set report receipt of IS when being interviewed for the BHPS. ${ }^{38}$ It is precisely

[^88]these individuals who (i) report IS receipt and (ii) are entitled to IS according to our IS algorithm, which make up the validation data set. Hence the validation data set is a subsample of the main data set. However, our validation data consists of only 739 observations since we do not use a number of outlying cases and cases for whom covariates are missing.

We assume that the only covariate subject to measurement error is the observed IS entitlement as computed by our program. All other covariates are assumed to be measured without error. ${ }^{39}$ For the first step we use the validation data set and estimate the following model (the choice of covariates derives from the reduced logit model discussed above):

$$
\begin{align*}
\log I S_{R} & =\beta_{0}+\beta_{1} \log I S_{C}+\beta_{2} \log I S_{C}^{2}+\beta_{3} \text { income }+\beta_{4} \text { income }^{2} \\
& +\beta_{5} \text { age }+\beta_{6} \text { age }^{2}+\beta_{7} \text { lonepar }+\beta_{8} \text { tenant }+\beta_{9} \text { couple }  \tag{4.26}\\
& +\beta_{10} U+\beta_{11} \text { weeks } U+\beta_{12} \text { sick }+\beta_{13} \text { subject } 1+\varepsilon
\end{align*}
$$

where $I S_{R}$ is the IS entitlement (in $£ / \mathrm{wk}$ ) as reported by individuals (i.e. the true covariate $x_{i}$ ), $I S_{C}$ is the IS entitlement (in $£ / \mathrm{wk}$ ) computed by our IS algorithm (i.e. the observed covariate, $z_{i}$ ), and $\varepsilon$ is an i.i.d. error term. Income is measured in pounds per week ( $£ / \mathrm{wk}$ ) and age in years. The functional form of (4.26) is chosen on the basis of 'best-fit' as measured by $\bar{R}^{2} .{ }^{40}$ The OLS estimates of (4.26) are given in Table 4.4 and it is these estimates which are used to compute the predicted IS entitlement used in the second step.

Table 4.4 also provides some basic descriptive statistics for the main data set and the validation data set in comparison. Based on these, the two data sets appear very similar. Individuals in the validation data set have slightly lower average incomes from other sources and the proportion of lone parents is greater. In addition, the length of unemployment spells is slightly greater in the validation data. This is to be expected,

[^89]since entitled non-recipients who are excluded from the validation set are predominantly recently unemployed singles with small amounts of other income. In other words, since the validation data set contains only entitled recipients and since (i) these often have no other income at all and (ii) lone parents have particularly high take-up rates, we find a lower average income and a greater proportion of lone parents in the validation set.

The second step uses the predicted IS entitlement from (4.26) as a covariate in the reduced form logit regression. We estimate both the zero-order approximation estimator and the second-order approximation estimator using in both cases the predicted variable $\hat{\log } I S$ as a covariate. In Table 4.5 we compare the parameter estimates from the approximation estimators with those from the naive logit regression (i.e. the logit model which takes no account of measurement error in the IS entitlement). The standard errors for the approximation estimators are corrected standard errors obtained by using the bootstrap method. ${ }^{41}$ As expected (see Section 4.3), all parameter estimates are affected by measurement error even though only one of the covariates was measured with error. For the eleven explanatory variables (including the intercept term) the approximation estimators produce parameter estimates that are (in absolute value) greater in seven cases and smaller in four cases. The change in the parameter values is particularly large for the intercept term and for the parameter on the covariate measured with error (the IS entitlement). In both cases the corrected estimates suggest that the naive estimates underestimate the parameter value by almost a third of their actual value. In comparison between the two approximation estimators we note that the direction of change is the same for the zero-order approximation and the second-order approximation. Furthermore

[^90]the difference between the approximation estimates is relatively small. This latter finding suggests that we are in fact dealing with a situation of relatively small measurement error for which the zero-order approximation estimator is perhaps quite sufficient.

A further effect we would expect to see is an increase in the estimated standard errors. This turns out to be the case for the zero-order approximation (with a couple of exceptions) but not so for the second-order approximation. In fact, for the second-order approximation, the estimated standard errors for all the covariates that are measured without error actually decrease. One of the reasons for this finding might be the fact that the standard errors obtained from the naive logit regression are in fact obtained from a mis-specified model. Thus, these too could be corrected for the sake of comparison. However, for our purposes the main point of interest is that the corrected standard errors indicate all our coefficients as being statistically significant at conventional levels.

To graphically illustrate the differences between the naive estimates and the approximation estimates we consider the interaction of the biases on all parameter estimates in Figures 4.1 to 4.3 (see Appendix 4B for these and all subsequent figures). Here we plot the predicted take-up probability for each of the estimators against an increasing IS entitlement holding all other covariates constant. We plot separate graphs for typical entitled non-recipients of IS. ${ }^{42}$ From Figures 4.1 to 4.3 we note the attenuation effect of both approximation estimators, that is, what becomes clear from these figures is the flattening effect of measurement error when no account is taken of covariate measurement error. At low levels of IS, the naive estimates overestimate the probability of take-up whereas at higher levels of IS the reverse is true. On average, non-take-up relates to small IS entitlements, so that for such cases we are overestimating the probability to take-up when our analysis does not take account of measurement error. Finally also note that the differences between the two approximation estimators is

[^91]minimal, reflecting the small differences between the zero-order and second-order estimates in Table 4.5.

### 4.4.4 Checking the Approximation Estimator Assumptions

So far, our analysis has highlighted the effects of measurement error in our model of IS take-up. In particular we have identified the biases arising from a naive analysis when a single covariate is subject to measurement error. In order to correct for these biases we have implemented two approximation estimators as described above. However, in order for these estimators to work several assumptions had to be made. In this section we highlight these assumptions and assess whether we have been able to satisfy them in our above application. We consider the assumptions in turn below:
(i) Measurement error is non-differential, i.e. $f_{Y \mid X, X^{*}, Z}\left(y \mid x, x^{*}, z\right)=f_{\eta X, X^{*}}\left(y \mid x, x^{*}\right)$. This assumption implies that the surrogate covariate (z) contains no additional information about the conditional distribution of the response variable $(y)$ not already described by the true covariate ( $x$ ). In other words, if we were able to compute the IS entitlement exactly for each individual then we would find that the conditional distribution of $y$ would be the same as its conditional distribution in our actual data set containing the errorcontaminated IS entitlement. ${ }^{43}$ However, this assumption is difficult if not impossible to verify since we have no information about the distribution of $y$ given only the true covariate, $x$, and any covariates not subject to measurement error, $x^{*}$.
(ii) The reported IS entitlement in the validation data set is the true IS entitlement, $x$. Clearly this assumption is likely to be flawed to at least some extent. Micro data sets such as the BHPS are prone to suffer from problems encountered in similar data sets such as accidental misreporting and/or incorrect reporting of important variables such as incomes. To our knowledge no work exists at present that examines the reliability of
consider a single male aged 30 with no other income and unemployed for close to 17 weeks. His unclaimed computed entitlement is $£ 31.00$ per week.
incomes data in the BHPS as a whole. ${ }^{44}$ Nevertheless, as noted above, we feel that the reported IS entitlement is, on average, a reasonably accurate measure of the true entitlement and therefore serves the purposes for our calibration step. Ideally, an accurate measure of the true entitlement would be obtained for a small random subsample of the BHPS sample by careful and meticulous examination at the primary data collection stage.
(iii) The calibration model of equation (4.26) is correct and holds in the validation data set. As noted above, we have decided on the model of (4.26) on the basis of it being the model that best fits the data in the validation data set. ${ }^{45}$ This conclusion was reached after examining the scatter plot of computed IS (z) against reported IS $(x)$ in Figure 4.4 and by considering other specifications of the model in (4.26). (In Figure 4.5 we also show the more accurate IS entitlement as predicted by equation (4.26) in comparison with the computed IS entitlement.)

To illustrate the effects on the parameter estimates of using a calibration model that does not fit the data as well as the model in (4.26) does, we have - for the first stage of the approximation estimators - predicted IS using a simple linear model. Following the above steps for the approximation estimators we then computed the zero-order approximation estimates and compared these to the actual estimates of Table 4.5. The results are given in Table 4.6. The direction of change (as compared to the naive estimates) is the same for both approximation estimators. Furthermore, most of the approximation estimates based on calibration model II (the model that fits the data less well) suggest a greater bias of the naive estimates when compared to the estimates based on calibration model I (the model of equation (4.26)). This might suggest that if we were able to use a calibration model which fits our validation data very well, the biases in the naive estimates might be less than suggested by our approximation estimates.

[^92](iv) The information obtained from the calibration model can be passed on to the main data set, i.e. the calibration model also holds in the main data set. This assumption essentially requires that the validation data set is a representative sample of the main data set. As noted in point (ii) above, in an ideal situation we would have access to an internal validation data set that consisted of a random subsample of the main data set. Such a sample would have to be arranged at the primary data collection stage. In our application, this is not the case. The sample we have used as the validation data set consists of all those individuals who report receipt of IS. Hence, as such it is not a random sample of the main data set and consequently is likely to differ somewhat. However, the basic descriptive statistics in Table 4.4 suggests that the differences between the two data sets are not particularly large so that, in general, the validation data set appears to be quite representative of the main data set.
(v) The approximations used in deriving the estimators hold, i.e. measurement error is small. Our validation data set suggests relatively small measurement error with $\mu_{v}=0.06, \sigma_{v}^{2}=0.416$. In particular, if $\beta \sigma_{v}^{2}$ is small, then the zero-order approximation should be sufficient and the difference between the zero-order approximation and the second-order approximation should be small. This is precisely the case in our application. In addition, Rosner et al. (1989) use evidence from a simulation study to suggest that the approximation estimators perform well when the product $\beta^{2} \sigma_{v}^{2}<0.50$ (provided, of course, the other assumptions are satisfied). For our estimated values of $\beta$ and $\sigma_{v}^{2}$, the product $\beta \sigma_{v}^{2}=0.392$. This compares quite favourably with similar epidemiological studies by Carroll et al. (1995) who estimate $\beta \sigma_{v}^{2}=0.16$, and Kuha (1994) who estimates $\beta \sigma_{v}^{2}=0.20$. Note however that the approximations can be good when measurement error is large. This occurs when the approximations produced by the Taylor series expansions are globally accurate, such as in normal based models (e.g. probit models). As noted above, for computational ease we have chosen the logit model over the probit model in our measurement error applications.

As Kuha (1994) comments, of the assumptions (i) to (v) above, (iii) and (v) are the only ones we can assess directly. The other assumptions are more difficult (if not impossible) to verify without the use of additional information. However, the key assumption must be that we are able to actually measure the true covariate accurately in the validation data set. As noted in (ii) above, some doubt about the accuracy of the reported IS entitlement exists. Nevertheless, we believe it to be a more accurate measure of the true entitlement than our computed IS entitlement. The validity of the approximation estimators stems not so much the fact that they might yield the truth about the parameter estimates, but more in that they indicate the direction of bias that arises from not correcting for measurement error. As such, they provide a step towards the truth about the parameter estimates and, as shown in Figures 4.1 to 4.3 , the true parameter values are likely to lead to quite different conclusions than those suggested by the naive estimates.

In the next sections we consider a more recent technique for correcting for measurement error in statistical models which makes different modelling assumptions and thus provides an interesting comparison to the approximation estimators discussed above.

### 4.4.5 Estimation by Simulation Extrapolation

The recent technique of simulation extrapolation (SIMEX) is a novel method of obtaining estimates of the true parameters of interest in a statistical model in which at least one of the covariates is subject to measurement error. The method is computationally more intensive than the approximation estimators discussed above but is universally applicable to a variety of different statistical models. SIMEX was first suggested by Cook and Stefanski (1994) and the method is also described in detail by Carroll et al. (1995). Theoretical foundations are provided by Stefanski and Cook (1995) and Carroll et al. (1996). ${ }^{46}$

[^93]So far, very few applications of the SIMEX method exist and to our knowledge none can be found in the econometrics literature. In this and the following sections we describe and implement the SIMEX procedure using our take-up data. In doing so, we carefully examine the SIMEX assumptions and methodology, and finally compare the SIMEX estimates to those obtained from the approximation estimators discussed above.

SIMEX relies on different assumptions than the approximation methods of Sections 4.4.3 and 4.4.4. The method requires an estimate of the measurement error variance and, as will become apparent, the way SIMEX is implemented affects the resulting corrected parameter estimates. The basic idea behind SIMEX is to simulate consecutively larger amounts of measurement error which are added to the data set of interest. Subsequently, for each of the resulting 'contaminated' data sets the naive parameter estimates are computed. By plotting these parameter estimates against the known additional amounts of measurement error we can (i) graphically display the effects of measurement error on the parameter estimates, and (ii) establish a trend of parameter estimate bias against increasing measurement error. Finally, in an extrapolation stage the SIMEX parameter estimate is obtained by extrapolating from the bias trend to the point of no measurement error.

The SIMEX procedure is best described by considering a simple errors-invariables model. (The method transfers easily to other scalar and multivariate linear and non-linear measurement error models.) As before, suppose we have a simple scalar model $y_{i}=\beta x_{i}+\varepsilon_{i}$ for each $i=1, \ldots, n$. Instead of the true $x_{i}$, we observe $z_{i}=x_{i}+v_{i}$ where $v_{i} \sim \operatorname{iid}\left(0, \hat{\sigma}_{v}^{2}\right)$, independent of $y_{i}$ and $x_{i}$. We begin with the simulation step whereby additional increasing amounts of measurement error are added to the original data set to give a total of $D$ contaminated data sets. ${ }^{47}$ Each of these new data sets contains the covariate $z_{i}$ with measurement error of magnitude $\left(1+\lambda_{d}\right) \hat{\sigma}_{v}^{2}$ where $d=1, \ldots, D$ and $0=\lambda_{1}<\lambda_{2}<\ldots<\lambda_{D}\left(\lambda_{1}=0\right.$ is the original data set). The naive OLS estimate (i.e.

[^94]uncorrected for measurement error) from each of these data sets is given by $\hat{\beta}_{z, d}$ which is not a consistent estimate of $\beta_{x}$ (see equation 4.9).

For the simulation step we assume that $\sigma_{v}^{2}$ is known. An estimate of $\sigma_{v}^{2}$ can be obtained by using our validation data set (as was done for the previous regression calibration techniques). In each of the newly created data sets the surrogate covariate, $z_{i}$, has increasing amounts of measurement error added to it, so that for any $\lambda_{d}>0$ we have

$$
\begin{equation*}
z_{b, i}\left(\lambda_{d}\right)=z_{i}+\sqrt{\lambda_{d}} v_{b, i} \tag{4.27}
\end{equation*}
$$

for individual $i=1, \ldots, n$ and simulation $b=1, \ldots, B$. The pseudo-error terms, $\left\{v_{b, i}\right\}_{i=1}^{n}$, are generated as

$$
\begin{equation*}
v_{b, i} \sim I N\left(0, \sigma_{v}^{2}\right) \tag{4.28}
\end{equation*}
$$

independent of all observed data. This simulation step is performed a large number of times for each given $\lambda_{d}$ thus yielding a total of $D \times B$ contaminated data sets. ${ }^{48}$ The next step is to obtain the naive OLS estimates for each of these contaminated data sets. Let $\hat{\beta}_{b}\left(\lambda_{d}\right)$ be the naive OLS estimate from the data set with covariates $\left\{z_{b, i}\left(\lambda_{d}\right)\right\}_{i=1}^{n}$. The average of these naive estimates for any given $\lambda_{d}$ is then simply

$$
\begin{equation*}
M\left[\hat{\beta}\left(\lambda_{d}\right)\right]=\frac{1}{B} \sum_{b=1}^{B} \hat{\beta}_{b}\left(\lambda_{d}\right) . \tag{4.29}
\end{equation*}
$$

The first step of the SIMEX method thus results in a total of $D$ averaged parameter estimates.

The second main stage is the extrapolation step. For this step we start by plotting the points $\left\{M\left[\hat{\beta}\left(\lambda_{d}\right)\right], \lambda_{d}\right\}_{d=1}^{D}$ and following this, model each of the averages, $M\left[\hat{\beta}\left(\lambda_{d}\right)\right]$, as a function of $\lambda_{d}$ using a suitable extrapolation function. Subsequently, by extrapolating back to $\lambda=-1$ the SIMEX estimate with no measurement error, i.e. where $(1+\lambda) \hat{\sigma}_{v}^{2}=0$, is obtained. Clearly, the choice of the extrapolant function is important as the resulting SIMEX parameter estimate depends on it. Cook and Stefanski (1994)

[^95]consider three general types of extrapolant function: linear, quadratic and non-linear (or rational linear) extrapolation. The linear $(c=0)$ and quadratic forms are
\[

$$
\begin{equation*}
K_{Q}(\lambda, a, b, c)=a+b \lambda+c \lambda^{2} \tag{4.30}
\end{equation*}
$$

\]

whereas the non-linear extrapolant is given by

$$
\begin{equation*}
K_{R L}(\lambda, a, b, c)=a+\frac{b}{c+\lambda} \tag{4.31}
\end{equation*}
$$

where $a, b$ and $c$ are unknown parameters to be estimated. The quadratic form has its advantages, such as in cases where there are only small amounts of curvature in the extrapolant function. When the extrapolant function is almost constant the non-linear form can become unstable as $c$ is not determined. However, in most practical applications Cook and Stefanski (1994) have found the quadratic extrapolant and particularly the non-linear extrapolant to be the most suitable, in the sense that the resulting bias correction is best. However, in our application SIMEX we use two additional extrapolant functions based on (4.30) that include a cubic term and quartic term in $\lambda$ respectively.

For practical purposes note also that a good indication of a suitable extrapolant function is often obtained from the plot of $\left\{M\left[\hat{\beta}\left(\lambda_{d}\right)\right], \lambda_{d}\right\}_{d=1}^{D}$ itself. As concerns the choice of $\lambda$, Carroll et al. (1995) suggest that from their practical applications and simulation studies of SIMEX an appropriate choice of $\lambda$ is $\left[0, \lambda_{\text {max }}\right]$ such that $1 \leq \lambda_{\max } \leq 2$ with increments of magnitude 0.2 and a grid given by $\Lambda=\left(\lambda_{1}, \lambda_{2}, \ldots, \lambda_{D}\right)$, where in a typical situation $\lambda_{1}=0$ and $\lambda_{D}=\lambda_{\max }=2$. To test how sensitive the resulting corrected parameter estimates are to the grid range of $\lambda$, we compute corrected SIMEX parameter estimates not only for $\lambda \in[0,2]$ but for $\lambda \in[0,3]$ and $\lambda \in[0,4]$ as well.

These two steps complete the SIMEX method and the resulting parameter estimates are approximately consistent (see Cook and Stefanski (1994)) which we formally state in the following theorem.

THEOREM: The SIMEX parameter estimate, $\hat{\beta}_{s}$, is approximately consistent for $\beta$, i.e. $\operatorname{plim} \hat{\beta}_{s}=\beta^{*}$ where $\beta^{*} \cong \beta$.

PROOF: Assume that

$$
\begin{align*}
& \operatorname{plim} \hat{\beta}_{x}=E\left[\hat{\beta}_{x}\right]  \tag{4.32}\\
& \operatorname{plim} \hat{\beta}_{z}=E\left[\hat{\beta}_{z}\right]  \tag{4.33}\\
& \operatorname{plim} \hat{\beta}_{b}\left(\lambda_{d}\right)=E\left[\hat{\beta}_{b}\left(\lambda_{d}\right)\right]  \tag{4.34}\\
& \operatorname{plim} \hat{\beta}\left(\lambda_{d}\right)=E\left[\hat{\beta}\left(\lambda_{d}\right)\right] \tag{4.35}
\end{align*}
$$

where $\hat{\beta}_{x}$ is the estimator of $\beta$ if we were able to observe the true $x_{i}, \hat{\beta}_{z}$ is the naive estimator, and $\hat{\beta}\left(\lambda_{d}\right)=E\left[\hat{\beta}_{b}\left(\lambda_{d}\right) \mid\left\{y_{i}, z_{i}\right\}_{i=1}^{n}\right]$. It follows that $E\left[\hat{\beta}\left(\lambda_{d}\right)\right]=E\left[\hat{\beta}_{b}\left(\lambda_{d}\right)\right]$. From (4.34) we assume that $\hat{\beta}_{b}\left(\lambda_{d}\right)$ converges in probability to its expectation. In turn, this expectation can be regarded as a function of the true parameter $\beta$ and the variance of the total measurement error in $z_{i}\left(\lambda_{d}\right)$, i.e. $\operatorname{Var}\left(v_{i}+\sqrt{\lambda_{d}} z_{b, i}\right)=\sigma_{v}^{2}\left(1+\lambda_{d}\right)$. Suppose this function is given by $g\left(\beta, \sigma_{v}^{2}(1+\lambda)\right)$, so that by assumption we have

$$
\begin{aligned}
& \operatorname{plim} \hat{\beta}_{b}\left(\lambda_{d}\right)=g\left(\beta, \sigma_{v}^{2}\left(1+\lambda_{d}\right)\right) \\
& \operatorname{plim} \hat{\beta}\left(\lambda_{d}\right)=g\left(\beta, \sigma_{v}^{2}\left(1+\lambda_{d}\right)\right) .
\end{aligned}
$$

Now, if $\sigma_{v}^{2}=0$ we have $\hat{\beta}_{x}=\hat{\beta}_{z}=\hat{\beta}_{b}\left(\lambda_{d}\right)=\hat{\beta}\left(\lambda_{d}\right)$ almost surely. Hence, if $\hat{\beta}_{x}$ is a consistent estimator of $\beta$ then it follows that $\beta=g(\beta, 0)$. Finally, provided $g($.$) is$ continuous, it holds that

$$
\lim _{\lambda \rightarrow-1} E\left[\hat{\beta}\left(\lambda_{d}\right)\right]=\lim _{\lambda \rightarrow-1} g\left(\beta, \sigma_{v}^{2}\left(1+\lambda_{d}\right)\right)=g(\beta, 0)=\beta .
$$

The extrapolant function which we fit to $\hat{\beta}\left(\lambda_{d}\right)$ for $\lambda>0$ is an approximation for $E\left[\hat{\beta}\left(\lambda_{d}\right)\right]$, so that by extrapolating the function to the point $\lambda=-1$ (which gives $\hat{\beta}_{s}$ ) we approximate $\beta$.

Standard errors for the SIMEX estimates can be obtained by a similar extrapolation technique. If the variance estimator for each $\hat{\beta}_{b}\left(\lambda_{d}\right)$ (say, the estimate based on the inverse of the information matrix) is denoted $\hat{V}_{b}\left(\lambda_{d}\right)$ then its average is

$$
\begin{equation*}
M\left[\hat{V}\left(\lambda_{d}\right)\right]=\frac{1}{B} \sum_{b=1}^{B} \hat{V}_{b}\left(\lambda_{d}\right) \tag{4.36}
\end{equation*}
$$

Let $s^{2}\left(l_{d}\right)$ be the sample variance of $\left\{\hat{\beta}_{b}\left(\lambda_{d}\right)\right\}_{b=1}^{B}$. By extrapolating the difference

$$
\begin{equation*}
\Delta^{V}\left(\lambda_{d}\right)=M\left[\hat{V}\left(\lambda_{d}\right)\right]-s^{2}\left(\lambda_{d}\right) \tag{4.37}
\end{equation*}
$$

to the point where $\lambda=-1$, the variance of the SIMEX estimate is obtained.
The theoretical motivation for obtaining the SIMEX standard errors in this manner is provided by Stefanski and Cook (1995). To begin with define, as above, $\hat{\beta}_{x}$ as the estimator of $\beta$ when the true covariate, $x_{i}$, is actually observable and $\hat{\beta}_{z}$ as the naive estimator computed from the observed data $\left\{y_{i}, z_{i}\right\}_{1}^{n}$. Similarly, let $\hat{V}\left(\hat{\beta}_{x}\right)$ be the variance estimator of the true variance $V\left(\hat{\beta}_{x}\right)$ (i.e. obtained if the actual data $\left\{y_{i}, x_{i}\right\}_{1}^{n}$ were observable) and $\hat{V}\left(\hat{\beta}_{z}\right)$ the variance estimator of the naive variance. By making use of suitable approximations Stefanski and Cook (1995) show that conditional on the true data, the SIMEX estimator is approximately unbiased, i.e.

$$
\begin{equation*}
E\left[\hat{\beta}_{s} \mid(y, x)_{1}^{n}\right] \approx \hat{\beta}_{x} \tag{4.38}
\end{equation*}
$$

where $\hat{\beta}_{s}$ is the SIMEX estimator. In terms of conditional expectations the variance of the SIMEX estimator is then

$$
\begin{equation*}
V\left(\hat{\beta}_{s}\right)=V\left\{E\left[\hat{\beta}_{s} \mid(y, x)_{1}^{n}\right]\right\}+E\left\{V\left[\hat{\beta}_{s} \mid(y, x)_{1}^{n}\right]\right\} \tag{4.39}
\end{equation*}
$$

where

$$
E\left\{V\left[\hat{\beta}_{s} \mid(y, x)_{1}^{n}\right]\right\}=E\left\{E\left[\hat{\beta}_{s}^{2} \mid(y, x)_{1}^{n}\right]\right\}-E\left\{E\left[\hat{\beta}_{s} \mid(y, x)_{1}^{n}\right]\right\}^{2}
$$

By substituting (4.38) and simplifying the expression in (4.39) we obtain

$$
\begin{equation*}
V\left(\hat{\beta}_{s}\right) \approx V\left(\hat{\beta}_{x}\right)+V\left(\hat{\beta}_{s}-\hat{\beta}_{x}\right) . \tag{4.40}
\end{equation*}
$$

Equation (4.40) consists of two parts: the first part can be estimated from $\hat{V}\left(\hat{\beta}_{z}\right)$, calculated at each simulation $b$ and then averaged as in equation (4.36). Each of these
averaged variance estimates is then plotted against $\lambda_{d}$, i.e. we plot $\left\{M\left[\hat{V}\left(\lambda_{d}\right)\right], \lambda_{d}\right\}_{d=1}^{D}$ and, using a suitable extrapolant function, we extrapolate to the point $\lambda=-1$. For the second part we require, for every given $d$, the differences

$$
\begin{equation*}
\Delta_{b}^{\beta}\left(\lambda_{d}\right)=\hat{\beta}_{b}\left(\lambda_{d}\right)-M\left[\hat{\beta}\left(\lambda_{d}\right)\right] \tag{4.41}
\end{equation*}
$$

for $b=1, \ldots, B$. Note that the sample variance of $\left\{\hat{\beta}_{b}\left(\lambda_{d}\right)\right\}_{b=1}^{B}$ is given by

$$
\begin{equation*}
s^{2}\left(\lambda_{d}\right)=\frac{1}{(B-1)} \sum_{b=1}^{B}\left[\Delta_{b}^{\beta}\left(\lambda_{d}\right)\right]^{2} . \tag{4.42}
\end{equation*}
$$

Next draw on the following three properties of $s^{2}\left(\lambda_{d}\right)$ (see Stefanski and Carroll (1995) for further details):
(i) It is an unbiased estimator for the conditional variance $V\left\{\Delta_{b}^{\beta}\left(\lambda_{d}\right) \mid(y, z)_{1}^{n}\right\}$;
(ii) $E\left\{\Delta_{b}^{\beta}\left(\lambda_{d}\right) \mid(y, z)_{1}^{n}\right\}=0$;
(iii) $\quad V\left(\hat{\beta}_{S}-\hat{\beta}_{x}\right)=-\lim _{\lambda \rightarrow-1} V\left\{\Delta_{b}^{\beta}\left(\lambda_{d}\right)\right\}$.

From (i)-(iii) above and using the expression of (4.39) again we know that

$$
\begin{align*}
& V\left[\Delta_{b}^{\beta}\left(\lambda_{d}\right)\right]=V\left\{E\left[\Delta_{b}^{\beta}\left(\lambda_{d}\right) \mid(y, x)_{1}^{n}\right]\right\}+E\left\{V\left[\Delta_{b}^{\beta}\left(\lambda_{d}\right) \mid(y, x)_{1}^{n}\right]\right\} \\
& \Rightarrow E\left[s^{2}\left(\lambda_{d}\right)\right]=V\left\{\Delta_{b}^{\beta}\left(\lambda_{d}\right)\right\} . \tag{4.43}
\end{align*}
$$

Hence estimating the second part of (4.41) becomes a problem of estimating

$$
\begin{equation*}
V\left(\hat{\beta}_{s}-\hat{\beta}_{x}\right)=-\lim _{\lambda \rightarrow-1} E\left[s^{2}\left(\lambda_{d}\right)\right] . \tag{4.44}
\end{equation*}
$$

The limit of (4.44) can be approximately estimated by plotting the elements of $\left\{s^{2}\left(\lambda_{d}\right), \lambda_{d}\right\}_{d=1}^{D}$ and extrapolating to $\lambda=-1$. However, instead of extrapolating each part separately, extrapolation of $\left\{M\left[\hat{V}\left(\lambda_{d}\right)\right]-s^{2}\left(\lambda_{d}\right), \lambda_{d}\right\}_{d=1}^{D}$ to the point where $\lambda=-1$ yields the same corrected standard errors for the SIMEX estimator.

In order to implement the SIMEX procedure for our logit model of take-up we use the same pooled data set as discussed in Section 4.4 .3 (i.e. 1,199 individuals entitled to IS according to our IS algorithm using the BHPS). For the first stage, the simulation step, we generate independently and identically normally distributed measurement error such that $v_{i} \sim I N(0,0.416)$. (The estimate of the measurement error variance is obtained from our validation data set as discussed in Section 4.4.3.) This generated measurement error
is then added to the surrogate covariate such that each new contaminated data set contains the contaminated covariate, given by

$$
z_{b, i}\left(\lambda_{d}\right)=z_{i}+\sqrt{\lambda_{d}} v_{b, i}
$$

in addition to all other covariates which we assume to be measured without error. The contaminated covariates (and thus data sets) are generated for $\lambda \in[0,2], \lambda \in[0,3]$ and $\lambda \in[0,4]$ respectively in increments of 0.2.49 Consequently, logit parameter estimates are obtained for each of these contaminated data sets. For each value of $\lambda$ this step is repeated 250 times and the average for each of the logit parameter estimates, given by $M\left[\hat{\beta}\left(\lambda_{d}\right)\right]$, is computed. ${ }^{50}$ Therefore, at the end of the first stage we have $D$ parameter estimates (averaged over 250) for each of our covariates in our reduced specification logit model of take-up (there are ten covariates in addition to an intercept term).

For the second stage, the extrapolation step, we begin by plotting the averaged parameter estimates against the corresponding values of $\lambda$, i.e. we plot $\left\{M\left[\hat{\beta}\left(\lambda_{d}\right)\right], \lambda_{d}\right\}_{d=1}^{D}$. Figure 4.6 gives the plots for all eleven parameter estimates (including the intercept term) obtained from using the grid range $\lambda \in[0,2]$. For each of the parameter estimates the plots clearly show a relatively smooth convex or concave relationship between the parameter estimate and the values of $\lambda$. In some cases the changes in the parameter values as a result of increasing measurement error are quite large indicating that the naive estimate is quite severely biased; in other cases the plots suggest that the naive estimate is biased to only a very small extent. Next we the fit the different extrapolation functions discussed above: the linear, quadratic and non-linear forms of (4.30) and (4.31) as suggested by Cook and Stefanski (1994) and in addition, (4.30) with a cubic and quartic term in $\lambda$ respectively. Finally, the estimated extrapolant functions are then used to predict the corresponding SIMEX estimates at the point $\lambda=-1$. At this point we obtain the extrapolated SIMEX estimates and these are given in

[^96]Table 4.7 for the different extrapolant functions (the grid range used for these estimates is $\lambda \in[0,2])$. Similarly, the estimated standard errors are obtained by using the method discussed above and the extrapolant function used for the standard errors is the same as the one used to obtain the SIMEX estimate.

The SIMEX results suggest similar findings to the approximation estimates. The naive estimates are all biased, some to a greater and some to a lesser extent. As expected, the bias correction that SIMEX provides depends on the choice of the extrapolant function (we will discuss this issue in the following Section 4.4.6). However, for all of the parameter estimates in Table 4.7 the direction of absolute change is the same, irrespective of the extrapolant function used. Also, as expected, the estimated SIMEX standard errors are larger than the naive estimates but most explanatory variables are still significant at the standard 5 percent or 10 percent significance level.

Finally, as for the approximation estimators of Section 4.4.3, we illustrate the predicted take-up probabilities for changing IS entitlement levels - whilst holding all other covariates constant - in Figures 4.7, 4.8 and 4.9. As before, we use the same three cases of a lone parent, a couple with one child and a single male. The predictions based on all five extrapolant functions demonstrate the attenuation effect of measurement error on the naive estimates in the order non-linear $>$ quartic $>$ cubic $>$ quadratic $>$ linear. ${ }^{51}$

### 4.4.6 Checking the SIMEX Methodology

The only explicit assumption required for the SIMEX technique is an estimate of the measurement error variance. In our application we estimate this variance from the same validation data set used for the approximation estimators. As a result, the accuracy of this estimate can be questioned on the same grounds as above, notably the assumption that the true IS entitlement is measured without error in the validation data set. Apart from this assumption the precision of the SIMEX estimates can be questioned on three further

[^97]issues: (i) how sensitive are the resulting SIMEX estimates to changes in the grid range of the simulation step, (ii) which extrapolant functions provide the best corrected SIMEX estimates, and (iii) what occurs if we generate non-normal measurement error in the simulation stage? We do not examine the last point (iii) here. Carroll et al. (1995, p.82) maintain that for SIMEX "the assumption of normality is not critical in practice" but do not supply any evidence to consolidate this point. Neither Stefanski and Cook (1995) nor Carroll et al. (1996) provide any evidence of the impact of non-normality in the simulation stage either. The former do however argue that normality is highly useful in demonstrating unbiasedness of the SIMEX estimates, whereas the latter provide the asymptotic distribution of the SIMEX estimators and thereby show that (although normality is desirable) non-normal measurement error is admissible. We continue by assuming normality of measurement error but stress that further work on this issue is required. We do however turn our attention to points (i) and (ii) above. Thus in this section we attempt to address the two former issues by considering the SIMEX results based on (i) different extrapolant functions and (ii) larger grid ranges.
(i) The choice of extrapolant function. Returning to Table 4.7 it becomes clear that for any one of the parameter estimates, the absolute change relative to the naive estimate is in the same direction for all extrapolant functions. The difference between the functions lies in the magnitude of bias correction provided, which differ quite significantly. For example, for our explanatory variable measured with error (the IS entitlement) the extrapolant functions suggest a downward bias in the naive estimate as follows: 17.9 percent for the linear extrapolant, 50.8 percent for the quadratic extrapolant, 75.4 percent for the cubic extrapolant, 97.3 percent for the quartic extrapolant, and 124.6 percent for the non-linear extrapolant. This pattern is mostly repeated for the other parameter estimates with the non-linear extrapolant suggesting the largest bias in the naive estimates and the linear extrapolant providing the most 'conservative' bias correction. (The overall degree of bias correction and the attenuation effect becomes clear from the predicted takeup probabilities in Figures 4.7 to 4.9 as well.)

For two of the parameter estimates ( $\log I S$ and lonepar) we demonstrate how the five different extrapolant functions give rise to distinct SIMEX estimates in Figures 4.10 and 4.11. For the log IS parameter the SIMEX correction suggests the expected bias downwards of the naive estimate. However, for the lonepar parameter the reverse is true so that the bias corrections are all negative. The accompanying tables highlight the increasingly better fit of the extrapolant functions as the degree of the polynomial increases. It is these functions that are used to predict the estimate at $\lambda=-1$.

The choice of extrapolant function is clearly important and it could be argued that it makes up the crucial step in the SIMEX method. As discussed above, the different extrapolation functions give rise to quite different bias corrections (in magnitude). Cook and Stefanski (1994) note that in ideal simulation conditions the non-linear extrapolant function provides the best bias correction. For practical situations they suggest that if only cautious bias corrections are what the researcher aims for a simple extrapolant such as the linear or quadratic form will suffice. In more complex situations the parameters for the non-linear extrapolant might be more difficult to estimate, particularly as the parameter $c$ in (4.31) could be close to zero. In our application there were no problems in estimating the non-linear extrapolant functions and, as noted above, the resulting predictions for $\lambda=-1$ are in line with those from the other extrapolant functions. In addition, by including the cubic and quartic terms for the extrapolant function we have shown that a better fit can be obtained and thus perhaps a more accurate bias correction. Hence, if in fact the non-linear extrapolant provides the best bias correction, then in our application the linear (or quadratic for that matter) do not provide much bias correction at all. Nevertheless, the important point is that the SIMEX method can give us a better idea of the true parameter estimates in our take-up model but since the extrapolant stage is an approximation we have no guarantee that the true unbiased parameter estimates are actually obtained.
(ii) The choice of the simulation grid range. Cook and Stefanski (1994) in their original paper on the SIMEX method propose a grid range for $\lambda$ that falls into the range [0, 2]. They argue that simulation evidence and practical applications of SIMEX suggest this
range to be sufficient for the purpose of establishing a bias trend. However, they do not provide any evidence to support their claim. Thus in order to examine the effects larger grid ranges have on the SIMEX estimates we implement SIMEX for the grid ranges $\lambda \in[0,3]$ and $\lambda \in[0,4]$ in increments of 0.2 . Larger grid ranges affect the SIMEX estimates through the extrapolant function since a larger number of data points can improve or deteriorate the fit of the extrapolant function used.

In Table 4.8 we consider the SIMEX estimates for the quadratic extrapolant and the non-linear extrapolant for all three grid ranges. For each of the extrapolant functions, as the grid range increases the parameter estimates change in the same direction. The changes are relatively small, particularly when compared to the different estimates obtained from the various extrapolant functions. For example, for the $\log I S$ parameter, the quadratic extrapolant suggests the following downward biases in the naive estimate: 50.8 percent for $\lambda \in[0,2], 40.2$ percent for $\lambda \in[0,3]$, and 31.6 percent for $\lambda \in[0,4]$. Using the non-linear extrapolant we have a downward bias in the naive estimate of 125.6 percent for $\lambda \in[0,2], 119.3$ percent for $\lambda \in[0,3]$, and 116.6 percent for $\lambda \in[0,4] .{ }^{52}$

Figures 4.12 and 4.13 give the quadratic, quartic and non-linear extrapolant functions for the parameters on $\log I S$ (biased downwards) and lonepar (biased upwards) respectively, and the differences between the grid ranges are seen to be small. In addition, the accompanying tables highlight the fact that the larger is the grid range, the less well the fit of the extrapolant function albeit by only small amounts. Finally, the interaction of the biases are shown in Figures 4.14 and 4.15 for a single unemployed male, where once again the relatively small differences in predicted take-up probabilities arising from the varying grid ranges becomes apparent. Hence, unlike the choice of extrapolant function, changes in grid range do not have a large effect on the resulting SIMEX estimates. This is particularly the case when the non-linear extrapolant function is used.

[^98]
### 4.5 Comparing the Approximation Estimators and the SIMEX Method

Thus far we have considered two separate estimation methods that correct for the biases arising in naive logit models which do not take account of measurement error. We have applied these methods to the analysis of social security take-up where a single covariate is subject to measurement error. Both methods, approximation estimators and SIMEX, have highlighted the need for bias correction since even though only one covariate is measured with error all resulting parameter estimates are biased. As a result, conclusions based on a naive analysis are misleading. In this section we briefly compare both estimation methods and draw attention to the results obtained from them.

Table 4.9 compares both approximation estimates and the SIMEX estimates (based on the non-linear extrapolant function with different grid ranges) to the naive logit estimates. Of the eleven parameter estimates the two methods indicate the same direction of absolute change in only five cases. In all other cases the two methods suggest opposing bias directions of the naive estimates. Turning to the covariate subject to measurement error (the IS entitlement), we find that all methods suggest a downward bias in the naive estimate. However the bias correction suggested by the approximation estimates is considerably greater than that suggested by SIMEX. One of the reasons for this might be due to the over-correction provided by the approximation estimates. Previously we provided some evidence (see Section 4.4.4) that in comparing calibration models used in the first step of the approximation estimators, models which fit the data less well yield approximation estimates for the $\log I S$ parameter that are greater in magnitude. Thereby the fit of the calibration model need not be that much worse for the effect to become apparent. Alternatively the SIMEX estimates with the largest degree of bias correction (i.e. using the non-linear extrapolant) might still be quite misleading in the sense that they do not provide sufficient bias correction. Previously we noted that greater bias corrections were obtained with more complex extrapolant functions. There might exist more suitable extrapolant functions which give rise to even greater bias corrections. Hence, if this is in fact the case, the approximation estimates might actually bring us closer to the truth than the SIMEX estimates.

Furthermore, the calibration model forms a crucial step in approximation estimation and a model that fits the data better might give rise estimates that are closer to the SIMEX estimates. In addition, as noted above, the approximation estimators rely on a number of assumptions not all of which are likely to be satisfied. The effects on the parameter estimates of violating these assumptions have not yet been explored. Carroll et al. (1995) do provide some simulation evidence that the approximation methods work well in logit models, provided all the assumptions the method makes are satisfied. Furthermore, as noted in the introduction, measurement error in non-linear models such as the logit model has complex effects. An attenuation effect is present but, as becomes apparent from our estimates, the bias effects on individual parameter estimates are intertwined. In combination, these effects can interact and produce bias corrected estimates that may not necessarily go in the right direction.

In comparison, SIMEX estimation although computationally more demanding relies (on first inspection) on less assumptions. Besides, various simulation studies and practical applications (Carroll et al. (1995 \& 1996), Cook and Stefanski (1994), and Stefanski and Cook (1995)) provide ample evidence that SIMEX works very well in logit models, particularly SIMEX utilising the non-linear extrapolant. However, the resulting SIMEX estimates strongly depend on the choice of extrapolant function and as such the method gives rise to an element of doubt. We have shown that the differences in the parameter estimates resulting from the various extrapolant functions demonstrate a clear pattern of increasing bias correction but nevertheless they differ considerably. Also, as noted, the effects of non-normal measurement error in the simulation stage still need to be investigated for practical applications of the method.

Hence in this chapter we have shown two quite different methods to deal with measurement error in a logit model of IS take-up. Deciding between either of the methods is complicated in that both methods have their advantages and disadvantages as discussed above. The approximation estimators are the more established of the two methods and have been well documented, particularly for logit models. In addition, they have also been successfully applied in various studies apart from our own (see above for references). Besides, upon closer investigation the approximation estimators rely on less
stringent assumptions than SIMEX. SIMEX, being a relatively new technique, still requires further examinations and practical applications of SIMEX are thus far sparse in nature. Hence, if we were asked to decide between both techniques our preference would lie with the approximation estimators since we have reason to believe that, on the whole, the approximation estimators provide us with a more accurate picture of what the true logit model of take-up actually is. Nevertheless, Figures 4.16 to 4.18 are somewhat reassuring in that even though they clearly show differences between the predicted takeup probabilities of the two methods, the attenuation pattern that emerges is not altogether divergent. A closer look is obtained from Table 4.10 which considers predicted take-up probabilities for small and large IS entitlements. For a single unemployed male, for example, doubling the IS entitlement from $£ 10$ to $£ 20$ raises the probability to take-up by 4.4 percentage points, based on a naive analysis. The second-order approximation estimates suggest an increase of 15.7 percentage points whereas the SIMEX estimates (grid range [0,2]) put the estimate at a more modest 7.1 percentage points. Similar findings apply to increases at the higher end of the IS scale.

### 4.6 Conclusions

In this chapter we have demonstrated the impact of measurement error on a simple econometric analysis of IS take-up in Britain. Our results based on a naive analysis (which does not account for measurement error) suggests that whether an individual will take-up can be explained by the individual's entitlement level, their income, the duration of their unemployment spell, and a variety of other socio-economic characteristics. In addition, we find that lone parents and couples are more likely to take-up than single individuals. More specifically, increasing benefit levels increase the probability to takeup (in an almost one-to-one ratio) whereas increasing levels of other income decrease considerably the probability to take-up. We thus have some empirical grounding to the ambiguous nature of these changes in our theoretical models of Chapter 3. The majority of these findings are confirmed by incorporating the presence of measurement error in our analysis. However, measurement error in one of the main explanatory variables biases all
parameter estimates in a simple take-up model and thus the resulting predictions based upon a naive model can be misleading.

Two techniques that attempt to correct for the biases resulting from measurement error have been considered in this chapter: approximation estimators and SIMEX estimation. Our evidence suggests that both methods suggest the expected attenuation effect in naive models that is not altogether dissimilar. Nevertheless we have reason to believe that the results based on the approximation methods are closer to the true parameter values than those provided by the SIMEX estimation technique. However, further work examining the relative performance of both estimation methods is still required.

From the policy perspective the conclusions are two-fold. First, certain groups of those entitled to IS are more likely to take-up the benefit than others. In particular, single individuals with relatively short durations of unemployment are less likely to take-up IS than any other group. In addition, those individuals who find greater financial difficulty as compared to a year ago are more likely to take-up. Thus the evidence suggests that much non-take-up is likely to be attributed to short-term hardship with anticipated improvements in the near future. Individual claimants who expect their financial situation to change for the better in the near future (such as perhaps by finding a job) are likely to find the costs of claiming to outweigh the perceived benefits. Expectations are thus of importance in a take-up analysis and such insights cannot be gained from a simple cross-section. A better handle on these dynamic issues will be obtained in Chapter 5 where we employ a short panel of data on take-up.

Second, for all three groups (singles, couples and lone parents) a naive analysis overestimates the probability to take-up for small IS entitlements and underestimates the probability to take-up for larger IS entitlements. From a different perspective, decreasing IS entitlements which are already at low levels (such as might be done in an attempt to increase labour force participation) reduces the probability to take-up to a greater extent than predicted by a naive analysis. On a wider scale, the DSS (1994) predictions suggest in a worst case scenario (for non-pensioners) an unclaimed $£ 930$ million and a total

550,000 entitled non-recipients. Therefore, on such a scale, errors in computing take-up predictions that might appear relatively small (around 5-10 percent) would roughly translate into figures of about 740,000-915,000 entitled non-recipients, or an unclaimed £1,300-£1,710 million.

Finally, this chapter has considered only two measurement error correcting methods. As noted above, many other techniques have emerged in recent years and further work on the impact of measurement error in a take-up model would benefit from a comparison with such methods. In addition, all methods require some information about the nature of the measurement error and consequently better results could be obtained if more precise validation data were available. In our case, a very small random sub-sample within the BHPS which collects very detailed and accurate information would suffice for such purposes.

The overall conclusion that remains is that there is considerable scope for applications of measurement error logit/probit models in the econometrics literature much of which has remained unexplored so far. The methods discussed in this chapter are relatively simple to implement and the corrections provided yield interesting results. Nevertheless, since the methods rely on a number of assumptions results based on them need to be treated with some degree of caution. Measurement error corrections as discussed and applied in this chapter bring the researcher a step closer the truth but do not yield unbiased estimates for the model of interest.

## CHAPTER 5

## THE TAKE-UP OF INCOME SUPPORT IN BRITAIN: EVIDENCE FROM PANEL DATA

### 5.1 Introduction

This chapter examines some dynamic take-up evidence from the first four waves (A to D) of the British Household Panel Survey (BHPS hereafter), spanning the years 19911994. The main objective of this analysis is find out what happens to non-pensioners entitled to income support (IS) as time passes. ${ }^{1}$ In particular, our interest falls on individuals entitled to IS but not receiving it (ENRs) and how their benefit status and employment status changes from one wave to another. We are able to follow individuals who are found to be entitled in the first wave through to waves two, three and four. Doing so enables us to gain more than a snap-shot vision provided by a single cross-section (or a pooled data set as used in Chapter 4). Similarly, we are able to follow all entitled individuals in wave two to waves three and four, and all entitled individuals in wave three through to wave four. The overall representation gained provides a detailed picture of the dynamics of take-up amongst individuals entitled to IS who are not receiving it. We particularly concentrate on changes in employment status and individuals' subjective financial assessments.

One of the major criticisms of means-tested benefits are their relatively low take-up rates. ${ }^{2}$ In view of this, social security programs such as IS in Britain are not achieving their objectives. On top of this, since the early 1980s a growing number of

[^99]individuals have been dependent on IS with a particularly steep increase from the early 1990s. ${ }^{3}$ Based on the evidence in this chapter we are able to reach conclusion with regard to the efficacy of poverty alleviation programmes such as IS in Britain. For example, if it turns out to be the case that ENRs remain in such a state for shorttime periods only, then a high measure of non-take-up based on an estimate at one point in time can be misleading. In other words, if the dynamics of non-take-up are high and, in addition, non-take-up is focused heavily on certain population groups, then non-take-up may prove to be less of a problem than previously thought.

In addition, we are able to make use of panel data to account for heterogeneity in modelling the decision to take-up IS. In the previous chapter only the crosssectional properties of the BHPS were exploited. Thus a finding that the probability to take-up IS is say, 75 percent amongst non-pensioners may be interpreted in either of two ways: in a homogenous population any one individual has a 75 percent chance of take-up, whereas in a heterogeneous population 75 percent will always take-up and 25 percent will never take-up. ${ }^{4}$ Panel data will allow us to discriminate between these two hypotheses by accounting for differences amongst individuals. Thus, rather than modelling the average behaviour of a group of individuals (as is done in a crosssection analysis) we are able to model individual behaviour when utilising panel data.

In this chapter we apply a random effects probit model and test for the presence of individual heterogeneity. We also test dynamic take-up models in which the current take-up decision is affected firstly, by an individual's past claiming experience, and secondly, by future employment events.

This chapter is in two parts. The first part addresses some basic evidence on the status of ENRs as we follow them from one wave to another. Thus, in section 5.2 we descriptively analyse the path followed by entitled individuals in each of the waves. The second part of this chapter returns to the key issue addressed in this thesis, namely analysing the factors which determine take-up. Hence, in section 5.3 we outline the various panel data models and in section 5.4 we discuss the results

[^100]obtained. Finally, we end with some concluding remarks and policy implications in section 5.5.

### 5.2 A Descriptive Analysis of the Entitled Sample

We begin this section with a brief summary of the distinction between the various categories of individuals entitled to IS. The data used for computing IS entitlements is drawn from the first four waves of the BHPS. For each wave, our IS algorithm mimics the rules and regulations which determine eligibility for and entitlement to IS. The model computes for each eligible individual an IS entitlement and generates as output a sample of individuals who are not only eligible for IS but who are, furthermore, entitled to some positive amount of IS. ${ }^{5}$ This sample is referred to as all entitled individuals and the analysis presented in this chapter (and the preceding chapter for that matter) draws on the entitled sample of each wave of the BHPS.

The income record of the BHPS holds information about whether a respondent is currently (i.e. at the time of interview) in receipt of IS and if so, the amount of IS they are receiving. By using this information in conjunction with our sample of entitled individuals, we are able to establish the following four categories for respondents in the BHPS: (i) individuals entitled to IS and receiving it, (ii) individuals entitled to IS and not receiving it, (iii) individuals not entitled to IS but receiving it, and finally (iv) individuals not entitled to IS and not receiving it. Clearly, for any one wave of the BHPS category (iv) is not of interest to us. Similarly, our previous evidence (see Chapter 2) suggests that category (iii) consists of a relatively small number of individuals, particularly if compared to category (ii). ${ }^{6}$ Hence our emphasis falls on categories (i) and (ii) with specific attention to the latter. For convenience, we hereafter abbreviate all entitled recipients (category (i)) by ER, all entitled nonrecipients (category (ii)) by ENR, and all individuals not entitled to IS (categories (iii) and (iv)) by NE. Finally, the take-up rate is computed as the ratio of all recipients of

[^101]IS (entitled and not entitled) to the sum of that figure plus all ENRs. This proportion gives the caseload take-up rate. ${ }^{7}$

In Table 5.1 (see Appendix 5A for this and all subsequent tables) we show the sample sizes of all entitled individuals for each wave separately and for the panel data set as a whole. The panel data set is obtained by merging all entitled individuals in each wave across all four waves. We thereby retain individuals who can be entitled to IS for anything between one wave to all four waves. The number of entitled individuals changes relatively little from one wave to another, the decrease in the latter two waves probably being attributable to general attrition in the BHPS. Similarly, the take-up estimates for each wave are reassuring at between 75 to 80 percent for each wave. The officially produced estimates of IS take-up by the Department of Social Security (DSS 1994, 1995a\&b) suggest higher take-up rates for non-pensioners: 84 percent in 1991, 84 percent (at worst) in 1992, and 87 percent (at worst) in 1993/94. ${ }^{8}$

### 5.2.1 Take-Up Evidence from Panel Data

Within our panel data set most individuals remain entitled for only one time period (59.1 percent), with increasingly smaller proportions experiencing two spells of entitlement ( 23.5 percent), three spells ( 11.5 percent), and finally four spells ( 5.9 percent). There are only a relatively small number of individuals ( 71 in total) who actually remain entitled to IS over all four years. For the majority of individuals, entitlement to IS is a relatively short-term experience, lasting no more than one year at

[^102]most. However, we must bear in mind that the true dynamics of benefit dependency are captured only by considering monthly data since, within any one year, there are likely to be large number of individuals who are dependent on IS for only a few months at a time. Walker (1996) has analysed the number of IS recipients using monthly BHPS data from the first two waves. His findings suggest a large degree of dynamics in benefit dependency with a high number of movements on and off the IS caseload. In contrast to our work, Walker's analysis is based only on the data already provided in the BHPS and as such does not reveal any information about the number of entitled individuals. The process of estimating the number of entitled individuals is a lengthy one and with the limitations of the data we have available, can only be performed on an annual basis. ${ }^{9}$ Thus, even though our analysis is unable to focus on the very short-term changes in entitlement to IS, we are nevertheless able to pursue ENRs for up to three years after first being entitled to IS. A further drawback arises since we are able to assess entitlement only at one point in time for each of the years. So, for cases in which an individual experiences several changes in circumstances within one year (and thus possibly in entitlement status), our analysis is unfortunately unable to detect such variations.

Within our sample of entitled individuals it is of interest to consider the changes in take-up behaviour from one wave to another. In Table 5.2 we give the spell runs experienced by all entitled individuals in wave A who are also entitled in at least one other subsequent wave (the spell runs from wave B onwards and from wave C onwards, although not shown here, display similar patterns). It becomes clear that the majority of ERs of IS at wave A retain this status in the waves thereafter. However, ENRs exhibit greater changes in their spell runs. For example, amongst the 218 entitled individuals in wave A who remain entitled at wave B, there are some 30 ENRs. Exactly one-third of these become ERs by wave B. Similarly, amid the 12 ENRs in wave A - who remain entitled at both waves B and C - more than one-half become ERs by wave B; and amongst the 6 ENRs in wave A who remain entitled in the following three waves B, C and D, precisely one-half become ERs by wave B.

[^103]It thus appears that even for those individuals who continuously remain entitled for several waves, non-take-up is a phenomenon that lasts for only up to one year for quite a significant proportion of entitled individuals. This is not surprising however, since as time progresses we would expect entitled individuals to experience greater hardship as their means for supporting themselves are gradually depleted thereby developing a greater propensity to claim (assuming they are aware of their entitlement). Previous cross-section evidence also suggested a significantly greater likelihood to take-up with increasing lengths of unemployment. ${ }^{10}$ In addition, the figures in Table 5.2 are based on very small samples and should thus be treated with an element of caution. Any errors in computing incorrect entitlements which in turn can give rise to misclassified individuals, can lead to potentially large misleading percentages. ${ }^{11}$ As noted above, by far the greatest number of entitled individuals in our sample experience only a single spell of entitlement throughout the four waves in which we follow them. The majority of ENRs at any one wave are no longer entitled to IS within the next wave. A sizeable proportion of entitled individuals with two or three spells also experience intermittent entitlement spells ( 54 cases of 282 entitled for two spells, and 22 cases of 138 entitled for three spells) and these are not considered in Table 5.2.

In order to observe the exact changes in take-up status from one wave to another we have computed cross-tabulations in Tables 5.3a-c for the entire (entitled) panel data set. These tables provide the exact destination in terms of take-up status of all entitled individuals for each wave. In addition, these tables allow us to determine the previous take-up status of an entitled individual. As before, our interest falls on ENRs, and we observe an increasingly smaller number of these at waves A, B or C remaining in this state in subsequent waves. In fact, the largest proportion of ENRs at any one wave become NEs as time progresses. For example, amongst the 115 ENRs at wave A (see Table 5.3a) only about 7 percent remain ENRs at wave B, and by the time these individuals reach wave D only 3.5 percent remain ENRs. In contrast, a

[^104]much larger proportion of ERs remain in that state when observed in the subsequent wave (approximately 40-45 percent at each wave). Even for ERs at wave A, almost 30 percent remain ERs when followed through to wave D.

The question that springs to mind is what happens to these ENRs? Do they become ERs with time? Based on our evidence in Tables 5.3a-c, only around 10-15 percent of ENRs actually become ERs within one of the next waves. Similarly, within any one wave, the largest proportion of the inflow into the status of ENR comes from the status of NE. (Note though that there exists some evidence for take-up status association across waves in the form of the chi-square statistics in Tables 5.3a-c.) Hence there must be other reasons for ENRs to loose their status as time progresses. The question that arises is what change in circumstances ENRs experience in order for them to lose their entitlement to IS. This issue is considered in the next section where we concentrate on ENRs only.

### 5.2.2 What Happens to Entitled Non-Recipients?

It was previously noted that relatively low take-up rates are observed for short-term unemployed singles (see Chapter 2). According to our theory of rational utility maximising individuals, non-take-up occurs as a result of the costs of claiming outweighing the (expected) benefit resulting from the claim. However, in a more dynamic setting the prospect of future changes in circumstances will also affect the current decision to take-up or not. Thus for the short term unemployed, anticipated changes for the better, reflected particularly in their employment status are likely to affect the current take-up decision. ${ }^{12}$ In this section we follow the path of ENRs firstly by employment status, and secondly by a variety of subjective measures of financial well-being.

Before we embark on comparing changes to ENRs, consider Figure 5.1 (see Appendix 5B for this and all subsequent figures) where we compare ENRs and ERs for each wave. Turning to ERs first, we note that the majority ( 50 percent and more) are unemployed, followed by those in family care (about 30 percent), with less than 10 percent being employed. The composition of the ER samples changes little from one wave to another. An exception is the proportion formed by all others at wave $D$,
where the increase from roughly 10 percent at waves $A$ to $C$ to 15 percent is attributable to a sudden increase in the number of sick and/or disabled (not shown in Figure 5.1). Compared to this, the majority of ENRs are also unemployed but the group containing all others features quite dominantly as well. Apart from those in family care, all groups show much greater wave to wave variation. Finally note that the employed constitute a greater proportion of ENRs than ERs. The most likely explanation for this is that employed ENRs might not believe they are entitled to IS; alternatively, they might feel that their earnings are sufficient for them to cope without the need for social security benefits.

In Tables 5.4a-c we follow all ENRs at each wave from employment status into employment status. The first striking result is that by moving from one wave to another, all ENRs increasingly progress into paid employment. This finding is particularly discernible for unemployed ENRs. With time, only a small proportion remain unemployed. For example, for wave A unemployed ENRs, only about 40 percent continue to be unemployed by wave $B$ and less than 25 percent by the time we reach wave $D$. It is reassuring to find that a large proportion of ENRs who are unemployed find themselves in paid employment when observed a year later. Such individuals provide credence to the view that expected employment changes in the near future appear to prevent entitled individuals from claiming at present.

Nevertheless, at the same time, close to 25 percent of the unemployed ENRs at wave A, for example, remain unemployed three years on. Similarly, for all wave B unemployed ENRs, more than 30 percent stay unemployed two years on. This much smaller sample of 'long-term' ENRs (as compared to those ENRs who move into employment) are of prime concern with respect to the efficacy of IS. The decision not to take-up by these individuals cannot be explained solely by anticipated future improvements. However, in Tables $5.5 \mathrm{a}-\mathrm{c}$ we consider ENRs by employment status and their subsequent movements into take-up status. The evidence provided suggests that, irrespective of their employment status, the overwhelming majority of ENRs become NEs within one year. Thus, even though a sizeable proportion of unemployed ENRs stay unemployed, they are no longer entitled to IS, even one year after first observing them.

[^105]Next we consider some subjective measures of the level of financial hardship experienced at present, in the past, and as expected for the future. Such measures are useful indicators of changes individuals experience that may be indicative of the relatively short-term nature of entitled non-recipiency. Assuming they are aware of their entitlement, we would expect ENRs who contemplate a positive improvement in their financial situation to be less likely to claim now, as compared to those who anticipate a worsening in their future financial situation. Similarly, we would expect ENRs to have less difficulty coping financially at present than ERs do. However, such subjective measures need to be treated with some degree of caution. A variety of factors are likely to influence the response to questions about individuals' perceptions of their personal financial situation. Nevertheless, when used as a rough guideline, they yield some interesting and useful insights to the nature of entitled non-recipiency.

To start with we consider the cross-sectional differences between ERs and ENRs in each wave. In Figure 5.2 we chart the responses to questions about an individual's current financial situation. For ERs the more positive responses ('living comfortably' and 'doing all right') gradually increase from one wave to another whereas the more negative responses ('finding it quite difficult' and 'finding it very difficult') display the opposite trend. The majority of individuals respond with 'just about getting by', and negative feelings about the current financial situation are more prevalent than positive feelings. For ENRs there is more of a balance between positive and negative replies, although 'just about getting by' is still the most common reply. Compared to the ERs, there is also greater wave-to-wave variation amongst the replies. However, in contrast to ERs, a larger proportion of ENRs reply with positive attitudes and less with negative attitudes. Thus, based on this evidence, it does appear as though ENRs have less financial difficulty when compared to ERs.

When asked to consider present financial circumstances with those of a year ago (see Figure 5.3), the responses by ERs and ENRs are quite similar. The majority (ERs and ENRs) reply with 'worse off' although this is closely followed by the reply 'about the same'. As before, the responses of ENRs show more variation when compared to the responses of ERs. The same applies to individuals' responses to questions about their financial expectations for the coming year (see Figure 5.4). On
average, however, it does appear that ENRs have slightly better expectations than ERs have.

The actual changes in subjective financial assessment for all ENRs at each wave are given in Tables 5.6a-c for the current situation, in Tables 5.8a-c for the situation relative to that of a year ago, and in Tables $5.10 \mathrm{a}-\mathrm{c}$ for future expectations.

In terms of the current financial situation (see Tables $5.6 \mathrm{a}-\mathrm{c}$ ), we find that ENRs who class themselves in the two top categories ('living comfortably' and 'doing all right') have a slight inclination to class themselves in lower categories in the following waves. Similarly, at the other end of the classification, ENRs who class themselves in the two bottom categories ('finding it quite difficult' and 'finding it very difficult') have a tendency to class themselves in higher categories as we follow them. In particular, there appears to be a gradual drift from both negative and positive responses towards the response 'just about getting by'. For example, of the 115 ENRs at wave A, approximately 32 percent classed themselves in the two bottom categories. When the same individuals were questioned at wave $B$, only around 12 percent of respondents continued to class themselves into the two bottom categories. ${ }^{13}$ Moreover, about 17 percent responded with 'just about getting by' and another 3 percent classed themselves in the two top categories. By the next wave, the corresponding figures had fallen to about 8 percent of all respondents in the two bottom categories, roughly 7 percent responding with 'just about getting by', and around 12 percent in the two top categories. Hence, what we seem to be observing here is the well-known concept of 'regression towards the mean'. ${ }^{14}$ In other words, respondents who express strong opinions about their financial situation at any one wave are likely to 'regress' towards less extreme responses when questioned at the next occasion.

When we consider the movements of ENRs from their current subjective financial situation into take-up status we note the overwhelming majority become NEs

[^106]when observed at the next wave (see Tables $5.7 \mathrm{a}-\mathrm{c}$ ). Individuals who responded with 'just about getting by' were the most likely to remain ENRs in subsequent waves.

Comparing the current financial situation with that of one year ago (see Tables $5.8 \mathrm{a}-\mathrm{c}$ ), we observe greater variability in responses from one wave to another. Nevertheless, amongst those ENRs who report being worse off compared to a year ago, an increasing proportion report their financial situation as having improved. If we turn to the take-up status which ENRs move into, a slightly greater proportion of individuals who responded positively become NEs as compared to those who responded negatively (see Tables $5.9 \mathrm{a}-\mathrm{c}$ ).

Finally, when asked about their future financial expectations (see Table 5.10ac), the majority of ENRs continued at ensuing waves with the same responses they gave at the initial waves. ${ }^{15}$ This is particularly true for individuals who expected a positive improvement and for those who expected financial circumstances to remain the same. A more interesting finding is revealed in Tables 5.11a-c, where we consider movements from future financial expectations into actual take-up status. Here we note that a larger proportion of individuals who had positive future expectations become NEs as compared to those who had negative expectations, or who expected financial circumstances to remain the same. However, by the time two to three years have passed this difference is only marginal. For example, of all wave A ENRs who had positive expectations, just over 82 percent became NEs by wave B; for those with negative expectations nearly 67 percent became NEs by wave B. Yet once we have reached wave $D, 90$ percent of the former are now NEs and just over 87 percent of the latter are NEs as well.

### 5.2.3 Summary of Main Findings

In summary, our descriptive analysis provides an interesting insight to the phenomenon of non-take-up amongst ENRs. As such, a simple descriptive analysis is unable to comment on those factors that determine take-up or non-take-up. This issue was dealt with in the previous chapter using cross-section evidence, and will be dealt

[^107]with in the remaining sections of this chapter by making full use of the panel nature of our data. However, the following points relating to this section are noteworthy:

- Non-take-up of IS amongst individuals who are entitled to it is a relatively shortterm phenomenon. The majority of individuals we observe as being ENRs are no longer entitled to IS when we observe them one year on. Thus, rather than becoming claimants of IS, ENRs experience some change which makes them no longer entitled to IS.
- The largest proportion of ENRs are unemployed. However, within one year of first observing them, the majority of unemployed ENRs have found some form of employment. Furthermore, the vast majority of unemployed ENRs are no longer entitled to IS. In terms of subjective financial assessment, ENRs appear to have less difficulty (compared to ERs) coping with their current financial situation. In addition, a sizeable proportion of ENRs appear to have optimistic perceptions about their future financial circumstances.
- All findings in this section are based on small (and often very small) samples and as such must be treated with some degree of caution. Nevertheless, there is at present no other data set for Britain that allows such an analysis to be performed. Our results are the first of their kind, and as such provide a new perspective on the non-take-up problem. They are thus of use and interest to the current debate about the efficacy of means-tested benefits in Britain.


### 5.3 Econometric Models of IS Take-Up Using Panel Data

In this section we return to the main issue of identifying those factors which determine whether an individual will take-up her IS entitlement. As noted in the intrúuction to this chapter, with the aid of panel data we are able extend our empirical model of takeup by taking into account individual-level heterogeneity. Probit/logit models for analysing panel data with binary response variables in which we want to account for unobserved effects specific to each individual have become a common tool in recent years. However, depending on whether we want to treat the individual-specific effects
as fixed constants (i.e. fixed effects model) or as a random variable (i.e. random effects model) the appropriate choice of model differs. ${ }^{16}$ It can be shown that in the fixed effects case maximum likelihood estimation of probit/logit models yields inconsistent estimates (see Hsiao (1986 \& 1992) for a proof). Chamberlain (1980) has suggested a conditional maximum likelihood estimator for the logit model which conditions on the individual-specific effects and gives rise to consistent estimates. In the probit case such conditioning is not possible and it appears that there exists no consistent probit estimator in the fixed effects case. In contrast, in the random effects case the reverse is true. The random effects probit model yields consistent estimates whereas the random effects logit model does not. ${ }^{17}$

This brings us to our next problem: should we use the fixed effects logit model or the random effects probit model? Maddala (1987) has addressed this issue in detail and suggests the following three reasons in favour of random effects over fixed effects models:

1. In a random effects model only the mean and variance of the individual-specific effects are estimated whereas in a fixed effects model a total of $N$ individualspecific effects need to be estimated. This leads to a large loss in degrees of freedom in the latter case.
2. The individual-specific effect measures an effect we do not know anything about in the same way that the general error term in a panel data model (or any other statistical model for that matter) captures an effect we are ignorant about. Now since the general error term is treated as a random variable there is no reason why the individual-specific effect should not be treated as one as well.
3. If the aim of our analysis is to draw inferences only about the sample we are working with then the individual-specific effects should be treated as fixed constants. If, however, we want to draw inferences about the population from

[^108]which this sample was drawn (as is the case in our application) the individualspecific effects should be treated as random variables.

In view of the above arguments the individual-specific effects are treated as random variables throughout this chapter so that the appropriate model is the random effects probit model. Note that when we assume random effects our error term (consisting of the general model error term and the individual-specific effect) is correlated across cross-sectional units even in the case where our general model error term is assumed to be independently and identically distributed (see below for a formal exposition). If we were to use a standard probit model on pooled waves of our data these correlations are being ignored and the resulting estimates are consistent but inefficient (see Guilkey and Murphey (1993) for a formal proof). These pooled probit estimates are often used as the starting values for the appropriate random effects probit algorithm.

Recent surveys of the random effects probit model can be found in Baltagi (1995), Chamberlain (1984), and particularly Hsiao (1986 \& 1992) and Maddala (1987). One of the earliest applications of the model is by Heckman and Willis (1975) in a model of reproduction. More recent applications include an analysis of the relationship between health and retirement decisions (Sickles and Taubman (1986)), and the effects of imports and inward foreign direct investment on innovations by domestic firms (Bertschek (1995)). An interesting non-economic application can be found in a model for predicting medical malpractice amongst a sample of physicians (Gibbons et al. (1994)).

In the following we outline the random effects probit model and methods of consistent parameter estimation. The model we consider is described by

$$
\begin{equation*}
y_{i t}^{*}=\beta^{\prime} \mathbf{x}_{i t}+\varepsilon_{i t} \quad \text { where } \varepsilon_{i t} \sim \operatorname{iid}\left(0, \sigma^{2}\right) \tag{5.1}
\end{equation*}
$$

for individuals $i=1, \ldots, N$ and time periods $t=1, \ldots, T$, where $\mathbf{x}_{i t}$ is a vector of explanatory variables and $\beta$ is a vector of parameters to be estimated. We observe the response variable

$$
y_{i t}= \begin{cases}1 & \text { if } y_{i t}^{*}>0  \tag{5.2}\\ 0 & \text { if } y_{i t}^{*} \leq 0\end{cases}
$$

where $y_{i t}^{*}$ is some unobserved latent variable. As for our cross-section models we can assume $y_{i t}^{*}$ to represent the net utility from a claim: the individual will take-up if it exceeds zero and will not take-up otherwise. In order to capture the individual differences we assume the error term to consist of an individual-specific effect and an effect that varies with both $i$ and $t$, that is $\varepsilon_{i t}=\mu_{i}+u_{i t}$ with assumptions ${ }^{18}$

$$
\begin{aligned}
& E\left(\mu_{i}\right)=E\left(u_{i t}\right)=E\left(\mu_{i} u_{i t}\right)=0 \\
& E\left(\mu_{i} \mu_{j}\right)= \begin{cases}\sigma_{\mu}^{2} & \text { if } i=j \\
0 & \text { if } i \neq j .\end{cases}
\end{aligned}
$$

Since we consider random effects models we also assume that $\mu_{i}$ is treated as a random variable just like $u_{i t}$.

Unlike pure cross-section models standard maximum likelihood techniques will not yield consistent estimates of the unknown parameters in the model described by (5.1) and (5.2). The inconsistency arises from the fact that the error term is serially correlated such that

$$
E\left(\varepsilon_{i t} \varepsilon_{i s}\right)=\sigma_{\mu}^{2}+E\left(u_{i t} u_{i s}\right)
$$

where $t \neq s$. Thus, even when $E\left(u_{i t} u_{i s}\right)$ is zero, the error term is still correlated across i. As a result, the standard cross-section technique of writing the joint likelihood of $\left(y_{1 t}, y_{2 t}, \ldots, y_{N_{t}}\right)$ as the product of the marginal likelihoods of $y_{i t}$ is no longer possible. The joint likelihood function is now a function involving the integration of a multidimensional distribution function.

The likelihood function to be maximised depends upon the assumptions made about the relationship between the individual-specific effect and the vector of explanatory variables. In the simplest case we assume the $\mu_{i}$ to be independent of the vector $\mathbf{x}_{i t}$. In addition, if we assume the $\mu_{i}$ to be a random sample from a univariate distribution $G($.$) , indexed by the parameter vector \delta$, then the log-likelihood function can be written as

$$
\begin{equation*}
\log L=\sum_{i=1}^{N} \log \int_{-\infty}^{+\infty} \prod_{t=1}^{T} \Phi\left(\mu_{i}+\beta^{\prime} \mathbf{x}_{i t}\right)^{y_{i t}}\left[1-\Phi\left(\mu_{i}+\beta^{\prime} \mathbf{x}_{i t}\right)\right]^{1-y_{i t}} d G(\mu \mid \delta) \tag{5.3}
\end{equation*}
$$

[^109]where $\Phi($.$) is the standard normal distribution function. If, however, we assume the$ $\mu_{i}$ to be correlated with the $\mathbf{x}_{i t}$, then we must specify a relationship between the individual-specific effect and the vector of explanatory variables. Chamberlain (1984), for example, considers a linear relationship between $\mu_{i}$ and $\mathbf{x}_{i t}$. The resulting log-likelihood function is similar to that of (5.3) except that $\Phi($.$) is evaluated$ inclusive of the linear relationship specified.

Maximising (5.3) yields consistent and efficient estimates of $\beta$ as $N \rightarrow \infty$ (for a proof see Hsiao (1992)). However, due to the integral in (5.3), maximum likelihood computation is involved and simpler methods have been proposed. In the case where $\mu_{i}$ is uncorrelated with $\mathbf{x}_{i t}$, if we condition on the individual-specific effects, $\mu_{\text {: }}$, then the error terms are $\varepsilon_{i t} \sim I N\left(\mu_{i}, 1\right)$ described by the density function $\phi\left(\varepsilon_{i t} \mid \mu_{i}\right)$. It follows that the likelihood function

$$
L=\prod_{i=1}^{N} \operatorname{Pr}\left(y_{i 1}, \ldots, y_{i T}\right)
$$

has component

$$
\begin{align*}
\operatorname{Pr}\left(y_{i 1}, \ldots, y_{i T}\right) & =\int_{a_{i 1}}^{b_{i 1}} \ldots \int_{a_{i T}}^{b_{i T}} \int_{-\infty}^{+\infty} \prod_{t=1}^{T} \phi\left(\varepsilon_{i t} \mid \mu_{i}\right) f\left(\mu_{i}\right) d \mu_{i} d \varepsilon_{i 1} \ldots d \varepsilon_{i T} \\
& =\int_{-\infty}^{+\infty} f\left(\mu_{i}\right) \prod_{i=1}^{T}\left[\Phi\left(b_{i t} \mid \mu_{i}\right)-\Phi\left(a_{i t} \mid \mu_{i}\right)\right] d \mu_{i} \tag{5.4}
\end{align*}
$$

where

$$
\begin{array}{ll}
a_{i t}=-\beta^{\prime} \mathbf{x}_{i t}, b_{i t}=\infty & \text { if } y_{i t}=1 \\
a_{i t}=-\infty, b_{i t}=-\beta^{\prime} \mathbf{x}_{i t} & \text { if } y_{i t}=0,
\end{array}
$$

$f($.$) is the normal density function with variance \sigma_{\mu}^{2}$, and $\Phi($.$) is as before. Hence,$ this method reduces a $T$-dimensional integral to a single integral which is much simpler to evaluate. In fact, the integrand consists of the product of a normal density and a total of $T$ differences of normal cdfs. Butler and Moffitt (1982) have suggested approximating (5.4) by Gaussian quadrature using Hermite integration and making use of the BHHH algorithm. ${ }^{19}$

Finally note that throughout this chapter we compute probit estimates based on the method by Butler and Moffitt described above. That is, we consider the simplest

[^110]case of (i) no correlation between the individual-specific effect and the explanatory variables, and (ii) no equicorrelation amongst the disturbance terms, i.e. $E\left(u_{i t} u_{i s}\right)=0 \quad \forall t \neq s$. However, methods have been proposed for situations in which these assumptions are thought to be violated. We will not discuss these methods in any detail. The interested reader is referred to Chamberlain (1984) who considers a restricted minimum distance estimator for the case where the assumption of no relationship between $\mu_{i}$ and $\mathbf{x}_{i t}$ is dropped, and Avery et al. (1983) who propose a method of moments estimator which accounts for equicorrelation of the disturbance terms.

### 5.3.1 Does State Dependence Exist in the Decision to Take-Up?

In Chapter 3 we presented a model of state dependence whereby an individual who decides to take-up IS in the current time-period is influenced by any past experience of her decision to take-up or not. In particular, our model accounted for an increased probability to take-up at present if the past take-up decision yielded an outcome greater than expected, and a decreased probability to take-up at present if the outcome of a past take-up decision turned out to be less than expected. This model was termed a state dependence model since, as a result of having made a take-up decision in the past, current preferences are altered in such a way that individuals who have experienced take-up or non-take-up in the past behave differently to those who have not experienced it. Thus, there is a genuine shift in preferences as a direct consequence of a previous claiming experience. In contrast, if there is no state dependence in the decision to take-up IS, then any previous take-up experience has no effect on current preferences to take-up or not.

However, we must beware of observing what Heckman (1978, 1981a \& 1981b) terms 'spurious' state dependence. Our sample of entitled individuals may be subject to unobserved heterogeneity so that individuals may differ in some characteristics which we are unable to observe. These unobserved characteristics may have an effect on the probability to take-up, particularly if they are correlated over time. If this turns out to be the case, then past experience of the take-up decision will act as proxy for these correlated unobservables and, as a result, past experience will appear as a determinant of the current take-up decision. In this case preferences have
not actually changed as a consequence of any previous take-up experience but we are incorrectly lead to believe that they have, since the individuals in our sample differ in some unobserved way.

Heckman (1981a) tests true state dependence against its spurious counter-part by using a panel of US women aged 45-59 and analysing their labour force participation decisions. His findings suggest that past employment experience has a strong effect on the current decision to participate even when the effects of unobserved heterogeneity are controlled for. In addition, controlling for this heterogeneity is of importance since the effect of past experience on the current participation decision is severely over-estimated when it is not properly controlled.

Hence, testing for state dependence versus heterogeneity is an important issue and panel data allow relatively simple tests to be performed. We can write the general random effects state dependent probit model as

$$
\begin{equation*}
y_{i t}^{*}=\gamma y_{i, t-1}^{*}+\beta^{\prime} \mathbf{x}_{i t}+\mu_{i}+u_{i t} \tag{5.5}
\end{equation*}
$$

where the observed response variables are given by

$$
y_{i t}= \begin{cases}1 & \text { if } y_{i t}^{*}>0 \\ 0 & \text { if } y_{i t}^{*} \leq 0\end{cases}
$$

and

$$
y_{i, t-1}= \begin{cases}1 & \text { if } y_{i, t-1}^{*}>0  \tag{5.6}\\ 0 & \text { if } y_{i, t-1}^{*} \leq 0\end{cases}
$$

with the same assumptions as above for the random effects probit model. Thus, a simple test for state dependence would appear to be a test of the hypothesis of state dependence against the alternative of no state dependence (based on say, a likelihoodratio test) such that

$$
\begin{array}{ll}
H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t}\right) \neq \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}\right) & \text { state dependence } \\
H_{A}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t}\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}\right) & \text { no state dependence }
\end{array}
$$

where, for the moment, we have ignored the individual-specific effects. However, by ignoring these effects, the above test for state dependence can lead to deceptive conclusions. The hypothesis of state dependence will be accepted in the presence of heterogeneity even when there is actually no true state dependence. To see this,
consider the joint probability of $y_{i t}$ in the absence of heterogeneity $\left(\mu_{i}=0\right)$. Assuming the $u_{i t}$ are independently identically distributed we have

$$
\begin{equation*}
\operatorname{Pr}\left(\mathbf{y}_{1}, \ldots, \mathbf{y}_{N}\right)=\prod_{i=1}^{N} \prod_{i=1}^{T} F\left(\beta^{\prime} \mathbf{x}_{i t}\right)^{y_{i t}}\left[1-F\left(\beta^{\prime} \mathbf{x}_{i t}\right)\right]^{1-y_{i t}} \tag{5.7}
\end{equation*}
$$

where $\mathbf{y}_{i}=\left(y_{i 1}, \ldots, y_{i T}\right)^{\prime}$. Alternatively, in the presence of heterogeneity $\left(\mu_{i} \neq 0\right)$ we have

$$
\begin{equation*}
\operatorname{Pr}\left(\mathbf{y}_{1}, \ldots, \mathbf{y}_{N}\right)=\prod_{i=1}^{N} \int \prod_{t=1}^{T} F\left(\beta^{\prime} \mathbf{x}_{i t}+\mu_{i}\right)^{y_{t}}\left[1-F\left(\beta^{\prime} \mathbf{x}_{i t}+\mu_{i}\right)\right]^{1-y_{i t}} d H(\mu) \tag{5.8}
\end{equation*}
$$

where $H($.$) is a univariate distribution function describing the \mu_{i}$ 's. Now equation (5.7) implies that

$$
\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, y_{i, t-1} ; \mu_{i}=0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i}=0\right)
$$

whereas equation (5.8) implies

$$
\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, y_{i, t-1} ; \mu_{i} \neq 0\right) \neq \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right) .
$$

Hence, by ignoring heterogeneity, a test based simply on the above hypotheses and accepting $H_{0}$ does not necessarily imply state dependence, since acceptance might imply heterogeneity (or serial correlation of the $u_{i t}$ for that matter). A better test procedure accounts for the presence of heterogeneity, and an intuitively appealing test would be based on the hypotheses

$$
\begin{aligned}
& H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i} \neq 0\right) \neq \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right) \\
& H_{A}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)
\end{aligned}
$$

for state dependence and no state dependence respectively. This test suffices in the case that the disturbance terms $u_{i t}$ (conditional on $\mu_{i}$ ) are serially uncorrelated. If however, they are serially correlated, then accepting $H_{0}$ in favour of no state dependence might be due to the lagged $y_{i, t-1}$ containing some information about the $u_{i t}$. Thus, in the case of serial correlation, this test would not meet our requirements either.

In order to overcome these difficulties, Anderson and Hsiao (1982) suggest a simple test procedure to distinguish true state dependence from its spurious counterpart (heterogeneity and/or serial correlation). Their test procedure is based on analysing the dynamic response to changes in $\mathbf{x}_{i t}$ in the model described by (5.5) and (5.6). In the case of no state dependence (when $\gamma=0$ ) a change in the explanatory
variables, denoted $\Delta \mathbf{x}$, has an immediate effect on the response variable, $y_{i t}$. On the other hand, if state dependence does actually exist (when $\gamma \neq 0$ ) then $\Delta \mathbf{x}$ has a distributed-lag response. To make this difference clearer consider the following: suppose $\mathbf{x}_{i t}$ is increased at time $t$ and returned to its previous level. Then in the case where $\gamma=0, \operatorname{Pr}\left(y_{i, t+1}\right)$ will be unaffected since, by assumption, the distribution of $u_{i t}$ is unaffected. However, if $\gamma \neq 0$, increasing $\mathbf{x}_{i t}$ at time $t$ will have an effect that persists, i.e. $\operatorname{Pr}\left(y_{i t}\right)$ as well as $\operatorname{Pr}\left(y_{i, t+1}\right)$ will be affected. In comparison, when the $u_{i t}$ are serially correlated, any change to the $u_{i t}$ will persist into the next period since its lag structure is autoregressive. Therefore, a test for state dependence that is not sensitive to functional form includes lagged explenatory variables such that the hypothesis of state dependence is

$$
\begin{equation*}
H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, \mathbf{x}_{i, t-1}, \mathbf{x}_{i, t-2}, \ldots ; \mu_{i} \neq 0\right) \neq \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right) \tag{5.9}
\end{equation*}
$$

against the alternative of no state dependence

$$
\begin{equation*}
H_{A}: \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, \mathbf{x}_{i, t-1}, \mathbf{x}_{i, t-2}, \ldots ; \mu_{i} \neq 0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right) . \tag{5.10}
\end{equation*}
$$

In order to implement these tests, suppose that $\log L_{A}$ is the $\log$-likelihood from maximising (5.8) whereas $\log L_{0}$ is the $\log$-likelihood from maximising (5.8) including any lagged variables. The likelihood- ratio test statistic is thus

$$
\begin{equation*}
L R=-2\left(\log L_{A}-\log L_{0}\right) \stackrel{a}{\sim} \chi_{r}^{2} \tag{5.11}
\end{equation*}
$$

where $r$ is the number of restrictions imposed (i.e. the number of lagged variables).

### 5.3.2 Accounting for Future Events in the Decision to Take-Up

The second dynamic issue we address in this chapter relates to future employment events and their impact on current decision-making. With the aid of panel data we are able to estimate econometric models of take-up in which each individual takes into account their future employment status and corresponding wage. These models are based on the random effects probit model discussed above and draw on the economic models of take-up developed in Chapter 3. The economic models of Chapter 3 considered the way future events enter current decision-making with respect to takeup. Future events enter in either of two ways: in the simplest case, each individual knows with certainty what their employment status and corresponding wage in the next time period will be; in the second model, we allowed for uncertainty about the
future employment status and wage. For either of these model we showed that for individuals who work (or who have a greater probability of working) in the next time period, the probability to take-up at present is diminished. In this section we test this hypothesis by constructing suitable econometric models.

As before, the decision to take-up at time $t$ is based on whether the net utility from a claim exceeds zero. Net utility is regarded as a latent variable so that what we observe is only whether an individual will take-up or not, given by

$$
y_{i t}= \begin{cases}1 & \text { if } y_{i t}^{*}>0  \tag{5.12}\\ 0 & \text { if } y_{i t}^{*} \leq 0\end{cases}
$$

where the latent variable is described by

$$
y_{i t}^{*}=\beta^{\prime} \mathbf{x}_{i t}+\varepsilon_{i t} \quad i=1, \ldots, N \quad t=1, \ldots, T
$$

as in section 5.3.1 above. Under the assumption that individuals know with certainty what their employment status and wage will be at time $t+1$, the model is described by (5.12) and the latent variable by

$$
\begin{equation*}
y_{i t}^{*}=\beta^{\prime} \mathbf{x}_{i t}+\theta e m p_{i, t+1}+\delta\left(e m p_{i, t+1} \times w_{i, t+1}\right)+\varepsilon_{i t} \tag{5.13}
\end{equation*}
$$

where

$$
e m p_{i, t+1}= \begin{cases}1 & \text { if employed at } t+1 \\ 0 & \text { if unemployed at } t+1\end{cases}
$$

$w_{i, t+1}$ is the net weekly wage at time $t+1$, and $\theta$ and $\delta$ are unknown parameters to be estimated. We also assume error components $\varepsilon_{i t}=\mu_{i}+u_{i t}$ with assumptions as above.

However, if the individual claimant is uncertain about whether she will actually be employed at $t+1$, and what her wage rate at $t+1$ will be, then she must form some wage expectation based on the information currently available at time $t$. Assume that $I_{i t}$ describes the information set available to the individual at time $t$. Both the probability an individual attaches to being employed at $t+1$, and the wage rate at $t+1$ are then formed conditional on this information set. Let the former be $\hat{p}_{i, t+1}=\operatorname{Pr}\left(e m p_{i, t+1} \mid I_{i t}\right)$ and the latter $\hat{w}_{i, t+1}=E\left(w_{i, t+1} \mid I_{i t}\right)$. Then the expected wage of employment is the product $\hat{p}_{i, t+1} \hat{1}_{i, t+1}$, and the take-up model is now given by (5.12) with latent variable

$$
\begin{equation*}
y_{i t}^{*}=\beta^{\prime} \mathbf{x}_{i t}+\theta^{*} \hat{p}_{i, t+1}+\delta^{*}\left(\hat{p}_{i, t+1} \times \hat{w}_{i, t+1}\right)+\varepsilon_{i t} \tag{5.14}
\end{equation*}
$$

where all previous assumptions apply and $\theta^{*}$ and $\delta^{*}$ are again unknown parameters to be estimated.

Implementing the model described by (5.12) and (5.13) is straightforward. For each time period $t$ we include as regressors $e m p_{i, t+1}$ and its interaction with $w_{i, t+1}$. However, (5.14) differs from (5.13) in that we must compute for each individual at each time period an estimate of (i) the probability of labour force participation and (ii) expected wages. Our strategy here is to estimate a model of earnings (which accounts for sample selection) for each cross-section of the BHPS. ${ }^{20}$ This model is then used to predict a wage for each individual in our entitled sample using their specific characteristics. ${ }^{21}$ By making use of a sample selection model we inherently address two issues of interest: firstly, we account for the fact that, within our sample, a wage rate is observed only when an individual participates in the labour force. A model based only on the sample of individuals who participate in the labour force will yield misleading conclusions about earnings of the entire sample. Simple OLS estimates based on the entire sample without accounting for the selection mechanism are inconsistent and inefficient. ${ }^{22}$ Secondly, our selection model allows us to compute for each individual an estimate of their probability of participating in the labour force, that is the probability of finding employment in the next time period.

The selection model we adopt is based on a simple two-stage method first proposed by Heckman (1979). Suppose that labour force participation is determined by whether the wage paid to an individual exceeds her reservation wage, given by a latent variable

$$
\begin{equation*}
r_{i}^{*}=\alpha^{\prime} \mathbf{z}_{i}+\eta_{i} \tag{5.15}
\end{equation*}
$$

and

$$
r_{i}= \begin{cases}1 & \text { if } r_{i}^{*}>r_{i}^{w}  \tag{5.16}\\ 0 & \text { if } r_{i}^{*} \leq r_{i}^{w}\end{cases}
$$

where $r_{i}^{w}$ is the $i$-th individual's reservation wage. A wage is observed only for individuals who participate ( $r_{i}=1$ ) and wages in turn are determined by

[^111]\[

$$
\begin{equation*}
w_{i}=\lambda^{\prime} \mathbf{v}_{i}+\xi_{i} \tag{5.17}
\end{equation*}
$$

\]

where $\mathbf{z}_{i}$ is a vector of characteristics which determine participation, $\mathbf{v}_{i}$ a vector of characteristics which determine wages, $\alpha$ and $\lambda$ the corresponding parameter vectors to be estimated, and $\eta_{i}$ and $\xi_{i}$ are bivariate normal with correlation $\rho$, denoted $\eta_{i}, \xi_{i} \sim N\left(0,0, \sigma_{\eta}^{2}, \sigma_{\xi}^{2}, \rho\right)$. Now note that in the sample of participants we have

$$
\begin{align*}
E\left(w_{i} \mid r_{i}=1\right) & =E\left(w_{i} \mid \eta_{i}>-\alpha^{\prime} \mathbf{z}_{i}\right) \\
& =\lambda^{\prime} \mathbf{v}_{i}+E\left(\xi_{i} \mid \eta_{i}>-\alpha^{\prime} \mathbf{z}_{i}\right) \\
& =\lambda^{\prime} \mathbf{v}_{i}+\rho \sigma_{\xi} \sigma_{\eta}\left\{\frac{\phi\left(-\alpha^{\prime} \mathbf{z}_{i}\right)}{1-\Phi\left(-\alpha^{\prime} \mathbf{z}_{i}\right)}\right\} \\
& =\lambda^{\prime} \mathbf{v}_{i}+\rho \sigma_{\xi} \sigma_{\eta}\left\{\frac{\phi\left(\alpha^{\prime} \mathbf{z}_{i}\right)}{\Phi\left(\alpha^{\prime} \mathbf{z}_{i}\right)}\right\} \tag{5.18}
\end{align*}
$$

from properties of the truncated bivariate normal distribution, where $\phi($.$) and \Phi($.$) are$ the normal pdf and cdf respectively. The ratio $\phi(.) / \Phi($.$) gives the inverse Mill's$ ratio. Note also that it is not possible to determine $\sigma_{\eta}$ separately so that we take $\sigma_{\eta}=1$. Hence the model to be estimated is

$$
\begin{equation*}
\left(w_{i} \mid r_{i}=1\right)=\lambda^{\prime} \mathbf{v}_{i}+\Xi \times I M R_{i}+\tau_{i} \tag{5.19}
\end{equation*}
$$

where $\Xi=\rho \sigma_{\xi}, I M R_{i}$ is the inverse Mill's ratio for the $i$-th individual, and $\tau_{i}$ is some i.i.d. error term. From (5.19) we can see that OLS estimates based on the sample of participants will yield consistent estimates of $\lambda$ and $\Xi$ only if $\rho=0$; moreover, these OLS estimates are inefficient since the error term $\tau_{i}$ is heteroskedastic:

$$
\begin{aligned}
& E\left(\tau_{i} \mid r_{i}=1\right)=\rho \sigma_{\xi} \times I M R_{i} \\
& \operatorname{Var}\left(\tau_{i} \mid r_{i}=1\right)=\sigma_{\xi}^{2}-\sigma_{\xi}^{2} \rho^{2} \times I M R_{i} \times\left(I M R_{i}+\alpha^{\prime} \mathbf{z}_{i}\right)
\end{aligned}
$$

A consistent and efficient estimator of the selection model of earnings in (5.19) can be obtained by maximum likelihood estimation (see Dhrymes (1986)). The appropriate log-likelihood function is given by

$$
\log L=\log \Phi\left(-\alpha^{\prime} \mathbf{z}_{i}\right)
$$

when $r_{i}=0$, and for the selected sample ( $r_{i}=1$ ) by

[^112]$$
\log L=-\frac{1}{2} \log 2 \pi \sigma_{\xi}^{2}-\frac{1}{2}\left\{\frac{\left(w_{i}-\lambda^{\prime} \mathbf{v}_{i}\right)}{\sigma_{\xi}}\right\}^{2}+\log \Phi\left\{\alpha^{\prime} \mathbf{z}_{i}+\frac{\rho\left(w_{i}-\lambda^{\prime} \mathbf{v}_{i}\right)}{\sigma_{\xi} \sqrt{\left(1-\rho^{2}\right)}}\right\} .
$$

However, Heckman's much simpler two-stage method will yield consistent estimates as follows. First, we estimate a probit participation model of (5.15) and (5.16) to give an estimate of the parameter vector $\alpha$. Thereafter we compute the inverse Mill's ratio for each observation as defined above. In the second step, the model of (5.19) is estimated by least squares to give estimates of $\lambda$ and $\Xi$. These estimates are consistent but their corresponding standard errors are heteroskedastic as shown above. A corrected asymptotic covariance matrix is given by

$$
\operatorname{Cov}(\beta, \theta)=\sigma_{\xi}^{2}\left(\mathbf{V}^{\prime} \mathbf{V}\right)^{-1}\left[\mathbf{V}^{\prime}\left(\mathbf{I}-\rho^{2} \Delta\right) \mathbf{V}+\rho^{2}\left(\mathbf{V}^{\prime} \Delta \mathbf{Z}\right) \Sigma\left(\mathbf{Z}^{\prime} \Delta \mathbf{V}\right)\right]\left(\mathbf{V}^{\prime} \mathbf{V}\right)^{-1}(5.20)
$$

where $\mathbf{V}$ is a matrix concatenation of the $\mathbf{v}_{i}$ vectors and the $I M R_{i}$ 's, $\mathbf{Z}$ is a matrix of the $\mathbf{z}_{i}$ 's, $\Delta$ is a diagonal matrix with elements given by $I M R_{i} \times\left(I M R_{i}+\alpha^{\prime} \mathbf{z}_{i}\right)$, and $\mathbf{I}$ is the identity matrix of same dimension as $V^{23}$

Finally, once estimates of $\alpha, \lambda$ and $\Xi$ are obtained we can predict the probability of labour force participation, $\hat{p}_{i, t+1}=\operatorname{Pr}\left(e m p_{i, t+1} \mid I_{i t}\right)$, from equation (5.15), and the expected wage rate, $\hat{w}_{i, t+1}=E\left(w_{i, t+1} \mid I_{i t}\right)$, from equation (5.19). These predicted values are then used as explanatory variables in estimating the model of (5.12) and (5.14) above.

### 5.4 Application and Results

This section describes the results obtained from the various panel data models of the take-up decision discussed above. Our choice of explanatory variables is motivated by the same reasons as outlined in the previous chapter (see Chapter 4 for a more detailed discussion). To reiterate, we anticipate the probability of IS take-up to be increasing in the IS entitlement but decreasing with rising levels of non-benefit income. ${ }^{24}$ In addition we include a variety of socio-economic dummy variables in order to capture differences in take-up behaviour and to proxy the stigma and hassle of

[^113]claiming. The variables are described in Appendix 5A and basic descriptive statistics are given in Table 5.12

Like all our previous analyses we concentrate only on non-pensioners aged 18 and above. The data used is drawn from the BHPS waves A to D (years 1991-1994) and our panel consists of 1,965 observations on 1,196 individuals. The average takeup rate for this sample is 78.4 percent. Our sample includes all individuals who experience at least one spell of being entitled to IS throughout the four waves in which we follow them. Since the majority of individuals in our sample experience only one spell of entitlement, the average time period over which we follow them is relatively short. ${ }^{25}$ We thus have an unbalanced panel in which the $T$-dimension is not the same for all $N$.

The models presented in this chapter differ from those in the preceding chapter in that they account for unobserved heterogeneity amongst cross-sectional units (with the exception of the standard pooled probit estimator). Furthermore, our unique panel data on take-up also allow some dynamic structure to be imposed on the take-up decision. Previous empirical studies of benefit take-up have ignored both these issues in spite of recognition of the importance of dynamics (see, for example, Walker (1994)). We extend our basic panel data model by testing for simple dynamics in the take-up decision. As discussed in section 5.3 above, we test for (i) state dependence, and (ii) the impact of future events on current decision-making.

In Table 5.13 we present results from the basic panel data model of take-up. In order to interpret the parameter estimates we also calculate marginal effects evaluated at the means of the explanatory variables. The first column gives the results based on the standard probit model (with no random effects) which does not account for heterogeneity. Estimates are based on the model of equation (5.7) and henceforth we denote this model by $\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i}=0\right)$. Recall that this model yields consistent (but inefficient estimates) even when the error terms are correlated due to the random effects. The second column gives the results based on the random effects probit model which does account for heterogeneity but assumes the $\mu_{i}$ to be

[^114]independent of the $\mathbf{x}_{i t}$. The estimates are derived from the model of equation (5.8) using the method of Butler and Moffitt discussed above. Hereafter this model is denoted $\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)$. An estimate of the individual-specific random effects is presented as an estimate of the proportion of the total error variance explained by this individual-specific effect, given by $\hat{\rho}=\hat{\sigma}_{\mu}^{2} /\left(\hat{\sigma}_{\mu}^{2}+\hat{\sigma}_{u}^{2}\right)$.

The results based on the both models are alike in the sense that the parameter estimates are all in the same direction and the models suggest similar variables to be statistically significant. As expected, the probability of take-up is increased with rising IS levels whereas it is decreased with rising non-benefit income (as was also the case for the logit models presented in Chapter 4). However, the results for the two models presented in Table 5.13 do differ in size. For every one percent increase in the IS entitlement level the probability of take-up increases by about 0.2 percent according to the standard pooled probit model, and by about 0.3 percent according to the random effects probit model. The effects of increasing non-benefit income are greater: an increase of one percent in non-benefit income decreases the take-up probability by roughly 0.5 percent (standard pooled probit model) and by just over 0.7 percent (random effects probit model) respectively. In addition, increasing numbers of children, being a lone parent, the head of household, renting local authority council accommodation, being unemployed, and being sick are all found to have a significant effect of increasing the probability to take-up in both models considered. However, when comparing the models in Table 5.13 we note that heterogeneity is of importance and significant in modelling take-up. More than half of the estimated total error variance can be explained by the individual-specific effect. In line with other studies (see Hsiao (1986)) we find that by not accounting for heterogeneity, the parameter estimates based on a simple pooled probit analysis are virtually all less in absolute value than those based on the random effects probit estimator. Previously we noted that random effects probit gives rise to consistent estimates as does simple pooled probit (in the case where the error terms are correlated due to random effects). In our case though we find that the parameter estimates in Table 5.13 differ in size. This is perhaps somewhat surprising but could be indicative of other factors (e.g. wrong choice of model, a misspecified model or perhaps omitted variables) which may bias the resulting parameter estimates.

### 5.4.1 Testing for State Dependence

The sample used in testing for state dependence excludes all individuals with only a single entitlement spell. Furthermore, in testing for state dependence we condition all observations on the first wave. That is, we exclude individuals entitled at wave $A$ only since we do not have the relevant information to include a lagged dependent variable for these individuals. (This enables us to perform likelihood ratio tests based on the same number of observations.) As a result our sample size is reduced to only 767 observations.

Note that we are, in general, unable to trace the take-up status of our sample to the beginning of the process. In other words, we do not deal with the problem of initial conditions in our dynamic models. One possibility of examining initial conditions in our state dependence models would be to follow individuals at the very beginning of their (potential) IS eligibility (e.g. school leavers). However, as noted previously, our sample sizes for such sub-samples would be very small indeed and therefore of limited use. ${ }^{26}$

The results based on the various state dependence models are shown in Table 5.14. The first two columns give the results based on the models estimated above (with no state dependence), except for the fact that we now use a smaller sample of observations. Column three gives estimates based on the state dependence model which ignores heterogeneity (state dependence probit I). This model is denoted $\operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i}=0\right)$ hereafter. The fourth column gives results based on the state dependence model with heterogeneity (state dependence probit II), indicated by $\operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)$ hereafter. The final column gives estimates based on the state dependence model which accounts for lagged $\mathbf{x}_{i t}{ }^{\prime} s$ as well as heterogeneity (state dependence probit III), referred to as $\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, \mathbf{x}_{i, t-1}, \mathbf{x}_{i, t-2}, \ldots ; \mu_{i} \neq 0\right)$. Note also that both state dependent models which account for heterogeneity are based on the estimation technique described above, which assumes the $\mu_{i}$ to be independent of the $\mathbf{x}_{i t}$.

Comparing the state dependence models with the models which do not account for state dependence we note that in the case with no heterogeneity (column 1 vs.

[^115]column 3) the parameter estimates are, on the whole, quite similar. The effect of state dependence however, albeit being significant at the 10 percent level, is very small. In terms of the predicted probability of take-up, whether an individual has taken-up their entitlement in the past affects the current probability by (at most) about 0.1 to 0.2 of a percentage point. When heterogeneity is accounted for, parameter estimates based on the state dependence model differ by a considerably greater amount as compared to the non-state dependent model (column 2 vs. columns 4 and 5). However, the effect of state dependence is once again very small indeed. Moreover both state dependence models which account for heterogeneity suggest that it explains only a minor proportion of the total error variance ( 4.3 percent), this result being statistically insignificant as well. Consequently it is not surprising to find that in comparing the state dependence models with and without heterogeneity respectively (column 3 vs. column 4) the parameter estimates remain the same.

In order to test the hypothesis of state dependence we consider the various test procedures described in section 5.3.1 above. The simplest of these was based on testing the hypothesis

$$
H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i}=0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i}=0\right)
$$

for no state dependence. The likelihood-ratio test statistic is found to be $\operatorname{LR}(1)=3.6$ as compared to $\chi_{1}^{2}=3.84$ ( 5 percent rejection level) so that, based on this test, we cannot reject the hypothesis of no state dependence in favour of state dependence. However, as noted above, this test ignores the presence of heterogeneity. A test which incorporates heterogeneity is based on the hypothesis

$$
H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid y_{i, t-1}, \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)
$$

for no state dependence. In this case the likelihood-ratio test statistic is found to be $L R(1)=12.4$ so that, based on this test, we are able reject the hypothesis of no state dependence in favour of state dependence. This test procedure can also be misleading, so that the last test procedure we consider accounts not only for heterogeneity but also for serial correlation. In section 5.3 .1 a better test was suggested, based on testing the hypothesis

$$
H_{0}: \operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t}, \mathbf{x}_{i, t-1}, \mathbf{x}_{i, t-2}, \ldots ; \mu_{i} \neq 0\right)=\operatorname{Pr}\left(y_{i t}=1 \mid \mathbf{x}_{i t} ; \mu_{i} \neq 0\right)
$$

for no state dependence. The likelihood-ratio test statistic yields $\operatorname{LR}(48)=39.4$ whereas $\chi_{48}^{2}=64.89$ ( 5 percent rejection level). Therefore, on this basis, we once again accept the hypothesis of no state dependence. Hence, when we control for both heterogeneity and serial correlation we do not find any evidence in favour of state dependence in the decision to take-up IS. Even if there is some evidence for state dependence (as suggested by the test accounting for heterogeneity only) the effect of any previous take-up experience on the current decision to take-up is minimal.

### 5.4.2 Testing for Future Events

The samples used to test the effect of future events on current IS take-up depend on which of the models discussed in section 5.3.2 are being implemented. In the case where we assume no future uncertainty the model of (5.12) and (5.13) applies and our sample consists of all entitled individuals in waves A to $C$ only. We exclude wave $D$ individuals since we do not have the relevant information to include future events. The resulting sample consists of 1,491 observations. When uncertainty is introduced the model of (5.12) and (5.14) applies. This model requires the estimation of a separate earnings function for each cross-section of the BHPS. In turn, the estimated earnings functions are then used to predict the relevant future employment indicators. Hence, there is no need to drop any observations and thus we use the full panel covering waves A to D .

Clearly, estimating the latter model with future events is more involved due to the earnings function evaluation. In order to implement the estimation of wages we draw data from the four waves of the BHPS. The data are adjusted so as to resemble the samples used in computing IS entitlements (see Chapter 2). In essence this means we consider only adult non-pensioners aged 18-60 for women and 18-65 for men, who are not self-employed or in full-time education (with the exception of lone parents) at the time of being interviewed.

The model estimated is described by equations (5.15) to (5.17). Our choice of explanatory variables is motivated by the standard literature on earnings and human capital as surveyed by, for example, Willis (1986). He shows that an individual's age (acting as a proxy for experience) and its squared value are key determinants of earnings. In addition, we include educational dummy variables, health status and
regional indicators. For the participation decision we also add the number of dependent children (we expect these to have a greater effect on women's participation than on men's), marital status, and age-educational interaction terms. We perform separate analyses for men and women and the results are presented in Table 5.16a for men and Table 5.16b for women (descriptive statistics for the samples are given in Table 5.15). For both men and women our models explain about 30-35 percent of the variation in log wages. Recent work on estimating earnings functions using the BHPS use explanatory variables much the same as our choice with similar results (see for example Harkness (1996) and Thomas et al. (1996)).

The estimates in Tables 5.16a and 5.16 b are used to compute the estimated future probability of labour force participation, $\hat{p}_{i, t+1}$, and the expected future wage, $\hat{w}_{i, t+1}$, for each of the individuals in our panel data set (as outlined in section 5.3.2 above). To reiterate, the important point here is that we assume each individual to form some expectation of $\hat{p}_{i, t+1}$ and $\hat{w}_{i, t+1}$ conditional on the information available at time $t$. We take as the information set the explanatory variables used in Tables $5.16 \mathrm{a} \& \mathrm{~b}$.

The results from the various models with future events are given in Table 5.17. The first two columns present results based on the model with no uncertainty (future events probit I and II) whereas the remaining two columns show the results based on the model with future expectations (expected future events probit I and II). The models are estimated with and without heterogeneity. When no uncertainty is assumed we find that being employed within the next time period has a negative effect on the current probability to take-up. This is in line with the economic models developed in Chapter 3. However, when heterogeneity is accounted for this finding is not statistically significant. Somewhat surprising, actual earnings in the next time period have a positive effect on current take-up. The parameter estimate, however, is only very small in magnitude and insignificant for both models considered. A likelihood ratio test statistic of comparing the model with and without future events yields $L R(2)=3.8$ in the case with no heterogeneity and $L R(2)=3.6$ in the case with heterogeneity. Based on these tests there is thus no evidence of future events having a
significant impact on current take-up behaviour ( $\chi_{2}^{2}=5.99$ at the 5 percent rejection level).

The estimates based on the future events model with expected employment probabilities and expected wages suggest similar findings. The greater is the probability of finding employment in the next time period, the less likely an individual is to take-up in the current time period. In contrast, the expected future wage rate again has a positive effect on current take-up. Nevertheless, estimates for both these findings are very small in magnitude and insignificant. Not surprisingly, likelihood ratio tests $(L R(2)=3.52$ for the model with no heterogeneity, and $L R(2)=4.16$ for the model with heterogeneity) also reject the models with future events. Therefore, based on our models in this section, we do not find any evidence for the effect of future employment events on the current decision to take-up IS or not.

### 5.5 Concluding Remarks

In the first part of this chapter we presented a simple descriptive analysis of time path changes entitled non-recipients undergo. The main findings are summarised in section 5.2.3 above and to reiterate, our evidence provides an interesting new perspective on the problem of non-take-up of IS in Britain. Our evidence suggests that what hitherto has been regarded as a major draw-back of the means-tested benefit system might, in fact, be less of a problem than previously thought.

In the second part of this chapter we have presented a variety of panel data models of IS take-up. We returned to the issue of the determinants of benefit take-up and have presented what - to our knowledge - are the first estimates of their kind for British data. The key result which emerges from this part of the chapter is that increasing levels of non-benefit income have a negative effect on the take-up probability whereas rising IS entitlement levels have a positive effect. In both the standard pooled probit model and the random effects probit model the marginal effect for non-benefit income is greater than the marginal effect for the IS entitlement level. Moreover, when we account for random effects the marginal effects on these two variables are about 50 percent larger in magnitude as compared to the marginal effects obtained in the standard pooled probit model. Nevertheless, in both models the marginal effect for non-benefit income is approximately 2.5 times the size of the
marginal effect for the IS entitlement (all other variables remaining unchanged and when evaluated at the means of the explanatory variables). Hence, according to these models, a rise in non-benefit income is much more likely to reduce the probability to take-up than a rise in IS increases this probability. However, under current IS legislation the entitlement level is reduced by any non-benefit income at a rate of almost one-to-one. This aspect has not been considered in modelling the take-up of IS above.

In the preceding Chapter 4, where we considered a smaller cross-sectional data set and estimated a logit model, similar findings emerged. There too we found that increasing levels of non-benefit income have a negative effect on the take-up probability whereas rising IS entitlement levels have a positive effect. However, the marginal effects estimated for the logit model are considerably smaller when compared to the marginal effects in this chapter. For example, the standard pooled probit model estimated in this chapter suggests a marginal effect for the IS entitlement level which is about six times greater than the marginal effect on the same variable as estimated in Chapter 4. To some extent this may be explained by the fact that the marginal effects are evaluated at the means. Earlier we noted that the take-up probability is particularly sensitive to changes in IS entitlement at the lower end of the IS scale (as shown, for example, in Figures 4.1 to 4.3 in Chapter 4). The data set used in this chapter has a mean IS entitlement of $£ 39.49$ as compared to $£ 52.13$ in Chapter 4.

Further findings from our panel data models suggest that individual heterogeneity is shown to play an important role in modelling IS take-up amongst non-pensioners in Britain. Thus, it appears that there are substantial differences between the various sub-groups in our sample. The majority of our sample are very likely to take-up all the time (lone parents, the unemployed and, to some degree, the sick) whereas a somewhat smaller proportion of our sample do not non-take-up their entitlement (those with part-time jobs and/or other non-benefit income).

Finally, we performed tests of various dynamic models which suggested only very limited evidence for, firstly, state dependence in take-up and, secondly, for the effect of anticipated future events. Contrary to intuition, having been a claimant or non-claimant of IS at some previous point in time has, if any effect, only a very small
effect indeed. Similarly, expecting a job and thus additional income from earnings, does appear to influence the current decision to take-up but our data do not suggest this finding to be significant. Nevertheless, we must bear in mind that our models are based on short panels and the choice of model itself might be somewhat restrictive or possibly unsuitable for our purposes.

In terms of further work, a natural extension would be the inclusion of future waves of the BHPS as they emerge. This would increase the $T$ dimension and consequently the number of observations of our panel data set. ${ }^{27}$ From a descriptive perspective, evidence based on monthly or even quarterly data would provide a much clearer picture of the dynamics of take-up. In particular, the changes which entitled non-recipients undergo are likely to become more apparent if, rather than observing them at annual intervals, we would be able to observe individuals more frequently.

From a modelling perspective, random effects probit models which do not rely on all the assumptions specified above (independence of the random effects and the explanatory variables and/or equicorrelation) would yield results that provide an interesting comparison. Finally, two further issues that could be addressed in future work are, firstly, a similar analysis for the take-up decisions of pensioners (this would require the availability of more reliable data), and secondly, modelling the joint decision of labour supply and take-up behaviour (around 6 to 7 percent of entitled IS recipients work part-time).

[^116]
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## APPENDICES

A Microeconometric Analysis of the Take-Up of Income Support in Britain

## APPENDIX 2A

## TABLES FOR CHAPTER 2

Variable Definition
age $=$
gross wage $=$
private pension $=$
rent receipt $=$
durables $=1$
owner $=1$
rented $=1$
no. of rooms $=$
no. of persons $=$ widow $=1$
age of individual
total gross wage in $£$ /week
income from private pension schemes in $£ / w k$
income from rented property in $£ / w k$
if household owns video recorder, freezer and washing machine
if accommodation is owned outright
if accommodation is rented (private sector or local authority)
total number of rooms in accommodation
total number of persons in household
if individual is widow(er)

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Table 2.1 Household/Individual Response Rates for the BHPS Waves A - D

|  | Wave $\boldsymbol{A}$ | Wave $\boldsymbol{B}$ | Wave $C$ |
| :--- | :---: | :---: | :---: |
| No. of households | 5,511 | 5,227 | 5,232 |
| No. of individuals $\dagger$ | 10,264 | 9,845 | 9,600 |
| of which: |  |  | 5,127 |
| Full Interview | 9,912 | 9,459 | 9,024 |
| Proxy Interview | 352 | - | 324 |
| Telephone Interview | - | 252 | 9,060 |
| $\dagger$ Adults aged l6 and above. |  |  | 112 |

Table 2.2 Missing Value Conventions Within the BHPS

| Value | -1 | -2 | -3 | -7 | -8 | -9 |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: |
| Reason | Don't know | Refused | Invalid/ | Proxy | Not | Missing |
|  |  |  | Uncodeable |  | Applicable |  |

Table 2.3 Applicable Amounts for Income Support: 1991/92 to 1994/95

|  | April 91-April 92 <br> (Wave A) | April 92-April 93 <br> (Wave B) | April 93-April 94 (Wave C) | April 94-April 95 (Wave D) |
| :---: | :---: | :---: | :---: | :---: |
| Personal Allowances |  |  |  |  |
| Single (age 18-24) | 31.15 | 33.60 | 34.80 | 36.15 |
| Single (age $\geq 25$ ) | 39.65 | 42.45 | 44.00 | 45.70 |
| Single Parent | 39.65 | 42.45 | 44.00 | 45.70 |
| Couples | 62.25 | 66.60 | 69.00 | 71.70 |
| Children (age < II) | 13.35 (13.60) | 14.55 | 15.05 | 15.65 |
| Children (age 11-15) | 19.75 (20.00) | 21.40 | 22.15 | 23.00 |
| Children (age 16-17) | 23.65 (23.90) | 25.55 | 26.45 | 27.50 |
| Children (age 18) | 31.15 (31.40) | 33.60 | 34.80 | 36.15 |
| Premiums |  |  |  |  |
| Family | 7.95 (8.70) | 9.30 | 9.65 | 10.05 |
| Lone Parent | 4.45 | 4.75 | 4.90 | 5.10 |
| Single Pensioner: |  |  |  |  |
| Standard Rate | 13.75 | 14.70 (16.70) | 17.30 | 18.25 |
| Enhanced Rate | 15.55 | 16.65 (18.65) | 19.30 | 20.35 |
| Higher Rate | 18.45 | 20.75 (22.75) | 23.55 | 24.70 |
| Pensioner Couple: |  |  |  |  |
| Standard Rate | 20.90 | 22.35 (25.35) | 26.25 | 27.55 |
| Enhanced Rate | 23.35 | 25.00 (28.00) | 29.00 | 30.40 |
| Higher Rate | 26.20 | 29.55 (32.55) | 33.70 | 35.30 |
| Disability: |  |  |  |  |
| Single | 16.65 | 17.80 | 18.45 | 19.45 |
| Couple | 23.90 | 25.55 | 26.45 | 27.80 |
| Severe Disability: 33.70 |  |  |  |  |
| Single | 31.25 | 32.55 | 33.70 | 34.30 |
| Couple (one qualifies) | 31.25 | 32.55 | 33.70 | 34.30 |
| Couple (both qualify) | 62.50 | 65.10 | 67.40 | 68.60 |
| Disabled Child | 16.65 | 17.80 | 18.45 | 19.45 |
| Carers | 10.80 | 11.55 | 11.95 | 12.40 |

[^117]Table 2.4 Descriptive Statistics: FES Samples

| Variable | Mean | Std. Dev. | Minimum | Maximum |
| :---: | :---: | :---: | :---: | :---: |
|  | FES 1991 |  |  |  |
| age | 48.17 | 19.15 | 18 | 99 |
| gross wage | 184.67 | 234.29 | 0 | 4999.8 |
| private pension | 37.25 | 88.50 | 0 | 2320.3 |
| rent receipt | 0.92 | 10.49 | 0 | 538.44 |
| durables | 0.32 | 0.47 | 0 | 1 |
| owner | 0.25 | 0.43 | 0 | 1 |
| rented | 0.30 | 0.46 | 0 | 1 |
| no. rooms | 5.21 | 1.42 | 1 | 15 |
| no. persons | 2.67 | 1.37 | 1 | 10 |
| widow | 0.14 | 0.35 | 0 | 1 |
|  | FES 1992 |  |  |  |
| age | 47.71 | 18.81 | 18 | 99 |
| gross wage | 187.23 | 251.97 | 0 | 3522.7 |
| private pension | 42.46 | 92.33 | 0 | 1129.6 |
| rent receipt | 1.11 | 15.46 | 0 | 839.68 |
| durables | 0.33 | 0.47 | 0 | 1 |
| owner | 0.25 | 0.43 | 0 | 1 |
| rented | 0.31 | 0.46 | 0 | ${ }_{1}^{15}$ |
| no. rooms | 5.22 | 1.40 | 1 | 15 |
| no. persons | 2.69 | 1.37 | 0 | 10 |
| widow | 0.13 | 0.34 | 0 | 1 |
|  | FES 93 |  |  |  |
| age | 47.71 | 18.89 | 18 | 98 |
| gross wage | 189.68 | 262.25 | 0 | 4643.1 |
| private pension | 51.77 | 146.87 | 0 | 3703.9 |
| rent receipt | 1.07 | 17.95 | 0 | 3.0 |
| durables | 0.36 | 0.48 | 0 | 1 |
| owner | 0.25 | 0.43 | 0 | 1 |
| rented | 0.31 | 0.46 1.55 | 0 | 20 |
| no. rooms | 5.41 | 1.55 | 1 | 20 10 |
| no. persons | 2.70 0.13 | 1.37 0.33 | 1 | 1 |
| widow | 0.13 | 0.33 | 0 |  |
|  | FES 94 |  |  |  |
|  | 47.96 | 19.01 | 18 |  |
| gross wage | 191.52 | 264.11 | 0 | 4895.2 2869.6 |
| private pension | 197.46 | 266.88 | 0 | 2869.6 1112.8 |
| rent receipt | 1.33 | 19.95 0.48 | 0 | 1 1 |
| durables | 0.38 | 0.48 0.43 | 0 | 1 |
| owner | 0.26 | 0.43 0.46 | 0 | 1 |
| rented | 0.31 | 0.46 1.44 | 1 | 15 |
| no. rooms | 5.24 | 1.44 1.37 | 1 | 10 |
| no. persons | 2.71 0.13 | 1.37 0.34 | 0 | 1 |
| widow | 0.13 | 0.34 |  |  |

Note: Sample sizes are FES 91 $=8.264$, FES $92=8.653$. FES $93=8.096$. and FES $94=8.294$.

Table 2.5 Tobit Estimates for Savings Model

|  | FES 91 | FES 92 | FES 93 | FES 94 |
| :---: | :---: | :---: | :---: | :---: |
| constant | -0.762 | -0.859 | -0.948 | -0.905 |
|  | (0.037) | (0.040) | (0.042) | (0.044) |
| age | 0.054 | 0.077 | 0.085 | 0.080 |
|  | (0.004) | (0.005) | (0.005) | (0.005) |
| gross wage | 0.019 | 0.033 | 0.029 | 0.027 |
|  | (0.003) | (0.003) | (0.003) | (0.003) |
| private pension | 0.069 | 0.072 | 0.027 | 0.050 |
|  | (0.006) | (0.006) | (0.004) | (0.005) |
| rent receipt | 0.079 | 0.049 | 0.371 | 0.066 |
|  | (0.051) | (0.035) | (0.032) | (0.038) |
| durables | 0.011 | 0.036 | 0.051 | 0.048 |
|  | (0.014) | (0.015) | (0.015) |  |
| owner | 0.107 | 0.159 | 0.149 | 0.162 |
|  | (0.017) | (0.018) | (0.019) | (0.020) |
| rented | -0.018 | -0.105 | -0.135 | -0.098 |
|  | (0.017) | (0.019) | (0.020) | (0.018) |
| no. rooms | 0.446 | 0.430 | 0.556 | 0.487 |
|  | (0.050) | (0.055) | (0.050) | (0.052) |
| no. persons | -0.250 | -0.594 | -0.625 | -0.655 |
|  | (0.058) | (0.065) | (0.068) | (0.062) |
| widow | -0.698 | -0.097 | -0.141 | -0.101 |
|  | (0.020) | (0.022) | (0.023) | (0.022) |
| sigma | 0.455 | 0.481 | 0.478 | 0.465 |
|  | (0.006) | (0.006) | (0.007) | (0.007) |
| $\log L$ | -4,260.3 | -4,253.8 | -3,843.1 | -4,102.4 |
| no. obs. | 8,261 | 8,641 | 8,093 | 8,294 |

Note: 1. gross wage, private pension and rent receipt all scaled by 100 and measured in $£ / \mathrm{wk}$.
2. Asymptotic standard errors in parentheses.

Table 2.6 Heteroskedastic Tobit Estimates for Savings Model

|  | FES 91 |  | FES 92 |  | FES 93 |  | FES 94 |  |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | $\beta$ | Sigma | $\beta$ | Sigma | $\beta$ | Sigma | $\beta$ | Sigma |
| constant | -0.231 | - | -0.193 | - | -0.388 | - | -0.266 | - |
|  | $(0.023)$ |  | $(0.018)$ |  | $(0.023)$ |  | $(0.022)$ |  |
| age | 0.040 | -0.082 | 0.011 | -0.119 | 0.034 | -0.080 | 0.030 | -0.110 |
|  | $(0.003)$ | $(0.003)$ | $(0.002)$ | $(0.002)$ | $(0.003)$ | $(0.004)$ | $(0.003)$ | $(0.005)$ |
| gross | 0.010 | 0.005 | 0.010 | 0.008 | 0.014 | 0.009 | 0.012 | 0.008 |
| wage | $(0.002)$ | $(0.001)$ | $(0.002)$ | $(0.001)$ | $(0.002)$ | $(0.001)$ | $(0.002)$ | $(0.001)$ |
| private | 0.007 | 0.023 | 0.016 | 0.061 | 0.009 | 0.088 | 0.011 | 0.072 |
| pension | $(0.009)$ | $(0.005)$ | $(0.005)$ | $(0.004)$ | $(0.004)$ | $(0.006)$ | $(0.005)$ | $(0.005)$ |
| rent | 0.158 | 0.278 | 0.161 | 1.625 | 0.143 | 0.990 | 0.149 | 0.896 |
| receipt | $(0.060)$ | $(0.088)$ | $(0.167)$ | $(0.033)$ | $(0.116)$ | $(0.048)$ | $(0.102)$ | $(0.038)$ |
| durables | 0.058 | -0.355 | 0.039 | -0.165 | 0.038 | -0.420 | 0.041 | -0.399 |
|  | $(0.009)$ | $(0.008)$ | $(0.006)$ | $(0.010)$ | $(0.008)$ | $(0.008)$ | $(0.008)$ | $(0.009)$ |
| owner | -0.560 | 0.834 | 0.065 | 0.280 | 0.026 | 0.460 | 0.030 | 0.556 |
|  | $(0.016)$ | $(0.010)$ | $(0.015)$ | $(0.011)$ | $(0.017)$ | $(0.013)$ | $(0.018)$ | $(0.012)$ |
| rented | -0.165 | 0.794 | 0.059 | -0.667 | 0.046 | -0.642 | 0.039 | -0.693 |
|  | $(0.014)$ | $(0.010)$ | $(0.009)$ | $(0.013)$ | $(0.010)$ | $(0.018)$ | $(0.010)$ | $(0.020)$ |
| no. rooms | -0.200 | 2.197 | -0.1662 | 1.885 | -0.050 | 1.402 | -0.126 | 1.534 |
|  | $(0.038)$ | $(0.023)$ | $(0.028)$ | $(0.032)$ | $(0.033)$ | $(0.030)$ | $(0.029)$ | $(0.031)$ |
| no. | 0.005 | -0.887 | 0.142 | -2.309 | 0.142 | -2.650 | 0.138 | -1.982 |
| persons | $(0.038)$ | $(0.024)$ | $(0.017)$ | $(0.040)$ | $(0.031)$ | $(0.047)$ | $(0.030)$ | $(0.048)$ |
| widow | -0.061 | 0.226 | 0.007 | -0.349 | 0.007 | -0.381 | 0.006 | -0.450 |
|  | $(0.017)$ | $(0.012)$ | $(0.012)$ | $(0.013)$ | $(0.011)$ | $(0.016)$ | $(0.011)$ | $(0.020)$ |
| log L |  |  |  |  |  | $-3,983.0$ |  | $-3,246.8$ |
| no. obs. |  | 8,261 |  |  | 8,641 |  | 8,093 | 8,294 |

[^118]Table 2.7 Predicted Savings for the FES: Descriptive Statistics

|  | Savers $\dagger$ | Mean | Median | Std. Dev. | Minimum | Maximum |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | FES 91 |  |  |  |  |  |
| Actual | 35.3\% | 14.63 | 2.15 | 45.99 | 0.01 | 769.20 |
| $E\left(y^{*}\right)$ | 0.5\% | 14.43 | 6.70 | 23.48 | 0.01 | 124.19 |
| $E\left(y \mid y^{*}>0\right)$ | 49.0\% | 53.92 | 28.09 | 50.39 | 0.01 | 1,282.19 |
| $E(y)$ | 49.0\% | 38.14 | 16.35 | 25.33 | 0.01 | +682.19 |
|  | FES 92 |  |  |  |  |  |
| Actual | 36.9\% | 14.81 | 2.99 | 46.00 | 0.01 | 882.56 |
| $E\left(y^{*}\right)$ | 0.9\% | 12.88 | 5.53 | 24.41 | 0.01 | 202.60 |
| $E\left(y \mid y^{*}>0\right)$ | 43.4\% | 44.82 | 20.12 | 40.77 | 0.01 | 1,682.13 |
| $E(y)$ | 43.0\% | 26.87 | 19.32 | 20.06 | 0.01 | 886.49 |
|  | FES 93 |  |  |  |  |  |
| Actual | 33.9\% | 14.99 | 2.79 | 46.90 | 0.01 | 1,035.59 |
| $E\left(y^{*}\right)$ | 0.9\% | 10.89 | 5.01 | 23.54 | 0.01 | 1,772.45 |
| $E\left(y \mid y^{*}>0\right)$ | 29.4\% | 45.95 | 22.19 | 38.49 | 0.01 | 1,177.74 |
| $E(y)$ | 28.9\% | 20.36 | 18.16 | 20.22 | 0.01 | 619.17 |
|  | FES 94 |  |  |  |  |  |
| Actual | 36.1\% | 15.23 | 3.12 | 48.06 | 0.01 | 1,169.45 |
| $E\left(y^{*}\right)$ | 1.6\% | 12.99 | 6.73 | 24.03 | 0.01 | 443.21 |
| $E\left(y \mid y^{*}>0\right)$ | 48.9\% | 44.88 | 28.56 | 43.77 | 0.01 | 1,476.54 |
| $E(y)$ | 48.4\% | 31.07 | 22.81 | 22.15 | 0.01 | 1,004.66 |

[^119]Table 2.8 Predicted Income from Savings for the BHPS: By Bands

| Percentages |  |  |  |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Wave A |  | Wave B |  | Wave C |  | Wave D |  |
|  | Actual | Predicted | Actual | Predicted | Actual | Predicted | Actual | Predicted |
| Nothing | 43.8 | 34.8 | 44.4 | 31.4 | 40.8 | 39.2 | 42.3 | 38.8 |
| < 1100 | 25.7 | 54.4 | 28.6 | 58.6 | 31.9 | 50.7 | 32.4 | 50.0 |
| f100-1000 | 22.5 | 10.8 | 19.7 | 10.0 | 20.8 | 10.1 | 19.4 | 11.2 |
| > $£ 1000$ | 8.0 | 0.0 | 7.3 | 0.0 | 6.5 | 0.0 | 5.8 | 0.0 |
| Sample Size | 4,944 |  | 4,903 |  | 4,660 |  | 4,703 |  |

[^120]Table 2.9 Average Mortgage Rates

| Sampling Month/Year | Average Mortgage Rate (\%) |  |
| :---: | :---: | :---: |
|  | Wave A |  |
| September 91 | 11.91 |  |
| October 91 | 11.82 |  |
| November 91 | 11.47 |  |
| December 91 | 11.41 |  |
|  | Wave B |  |
| September 92 | 10.61 |  |
| October 92 | 10.60 |  |
| November 92 | 10.52 |  |
| December 92 | 10.11 |  |
|  |  |  |
| September 93 |  | 8.00 |
| October 93 | 7.99 |  |
| November 93 | 7.98 |  |
| December 93 | 7.94 |  |
|  |  |  |
|  |  | 7.57 |
| September 94 | 7.85 |  |
| October 94 |  | 7.83 |
| November 94 |  |  |
| December 94 |  |  |

[^121]Table 2.10 Proportion of Mortgage Holders and Mortgage Types in the BHPS
Percentages

|  | Wave $A$ | Wave B | Wave C | Wave D |
| :--- | :---: | :---: | :---: | :---: |
| Sample Size | 4,944 | 4,903 | 4,660 | 4,703 |
| Mortgage Holders <br> of which: | 41.3 | 43.3 | 44.2 | 44.2 |
| Repayment | 27.4 | 26.6 |  |  |
| Endowment | 68.7 | 68.5 | 24.6 | 24.1 |
| Part Rep./End. | 2.2 | 2.4 | 71.2 | 70.0 |
| Other | 1.6 | 2.0 | 2.2 | 2.6 |

Table 2.11 Sample Sizes at Various Stages of the IS Algorithm

|  | Wave $\boldsymbol{A}$ | Wave $\boldsymbol{B}$ | Wave $\boldsymbol{C}$ | Wave $\boldsymbol{D}$ |
| :--- | :---: | :---: | :---: | :---: |
| Stage I Sample | 4,944 | 4,903 | 4,660 | 4,703 |
| Eligible Sample | 1,614 | 1,834 | 1,301 | 1,315 |
| Entitled Sample | 908 | 934 | 763 | 785 |

Note: Stage 1 Sample is the sample drawn from the BHPS after it has passed the first main stage (data cleaning and manipulation) of the model. The Stage 1 Samples also apply to the savings imputation and mortgage interest calculation. Eligible Sample and Entitled Sample are defined in the main text.

Table 2.12 Entitlement and Recipiency for Individuals Eligible for IS

|  | Positive Calculated Entitlement $\dagger$ |  | Negative Calculated Entitlement $\ddagger$ |  |
| :---: | :---: | :---: | :---: | :---: |
|  | Receiving | Not Receiving | Receiving | Not Receiving |
|  | Wave A |  |  |  |
| All Non-Pensioners of which: | 393 | 124 | 35 | 271 |
| Singles | 173 | 77 | 11 | 124 |
| Lone Parents | 129 | 15 | 8 | 14 |
| Childless Couples | 29 | 15 | 9 | 86 |
| Couples with Kids | 62 | 17 | 7 | 47 |
| All Pensioners of which: | 112 | 279 | 24 | 719 |
| Singles | 108 | 274 | 18 | 407 |
| Couples | 4 | 5 | 6 | 312 |
|  | Wave B |  |  |  |
| All Non-Pensioners of which: | 400 | 126 | 29 | 236 |
| Singles | 188 | 75 | 18 | 106 |
| Lone Parents | 127 | 15 | 4 | 9 |
| Childless Couples | 24 | 19 | 3 | 67 |
| Couples with Kids | 61 | 17 | 4 | 54 |
| All Pensioners | 117 | 291 | 22 | 613 |
| of which: |  |  |  |  |
| Singles | 113 | 289 | 11 |  |
| Couples | 4 | 2 | 11 | 272 |
|  | Wave C |  |  |  |
| All Non-Pensioners | 386 | 98 | 32 | 161 |
| of which: |  |  |  | 81 |
| Singles | 171 | 58 | 18 2 | 81 8 |
| Lone Parents | 129 | 14 | 2 5 | 8 44 |
| Childless Couples | 24 | 14 | 5 | 28 |
| Couples with Kids | 62 | 12 | 7 | 28 |
| All Pensioners | 108 | 171 | 11 | 334 |
| of which: |  |  |  | 190 |
| Singles | 102 | 170 1 | 8 | 144 |
| Couples | 6 | 1 |  | 144 |
|  | Wave D |  |  |  |
| All Non-Pensioners of which: | 380 | 114 | 38 | 137 |
|  |  | 67 | 14 | 59 |
| Singles | 182 | 10 | 5 | 6 |
| Lone Parents Childless Couples | 111 25 | 19 | 7 | 44 |
| Childless Couples Couples with Kids | 25 62 | 18 | 12 | 28 |
| All Pensioners of which: Singles | 117 | 174 | 11 | 344 |
|  |  |  | 5 | 202 |
|  | 114 | 172 2 | 6 | 142 |
| Couples | 3 |  |  |  |

$\dagger$ IS entitlement $\geq £ 0.10 . \quad \ddagger$ IS entitlement $<£ 0.10$.
Note: All results are based on the sample of individuals eligible for IS as computed by our our microsimulation program.

Table 2.13 Take-Up of IS: By Family Type

|  | Entitled Recipients | Entitled NonRecipients | Non-Entitled Recipients | Total Number of Recipients | Take-Up $\dagger$ |
| :---: | :---: | :---: | :---: | :---: | :---: |
|  | Wave A |  |  |  |  |
| All Non-Pensioners of which: | 393 | 124 | 58 | 451 | 78.4 \% |
| Singles | 173 | 77 | 25 | 198 | 72.0 \% |
| Lone Parents | 129 | 15 | 11 | 140 | 90.3 \% |
| Childless Couples | 29 | 15 | 14 | 43 | 74.1 \% |
| Couples + Children | 62 | 17 | 8 | 70 | 80.5 \% |
| All Pensioners | 112 | 279 | 25 | 137 | 32.9 \% |
|  | Wave B |  |  |  |  |
| All Non-Pensioners of which: | 400 | 126 | 53 | 453 | 78.2 \% |
| Singles | 188 | 75 | 24 | 212 | 73.9 \% |
| Lone Parents | 127 | 15 | 6 | 133 | 89.6\% |
| Childless Couples | 24 | 19 | 16 | 40 | 67.8\% |
| Couples + Children | 61 | 17 | 7 | 68 | 78.8 \% |
| All Pensioners | 117 | 291 | 25 | 142 | 32.8 \% |
|  | Wave C |  |  |  |  |
| All Non-Pensioners | 386 | 98 | 59 | 445 | 82.0 \% |
| of which: |  | 58 | 29 | 200 | 77.5 \% |
| Singles <br> Lone Parents | 171 129 | 58 14 | 7 | 136 | 90.7\% |
| Lone Parents | $\begin{array}{r}124 \\ \hline\end{array}$ | 14 | 9 | 33 76 | $70.2 \%$ $86.4 \%$ |
| Couples + Children | 62 | 12 | 14 | 76 | 86.4 \% |
| All Pensioners | 108 | 171 | 30 | 138 | 44.5 \% |
|  | Wave D |  |  |  |  |
| All Non-Pensioners | 380 | 114 | 61 | 441 | 79.5 \% |
|  |  |  |  | 201 | 75.0 \% |
| Singles <br> Lone Parents | 182 111 | 67 10 | 12 | 123 | 92.5 \% |
| Lone Parents | 25 | 19 | 16 | 41 | 68.3\% |
| Couples + Children | 62 | 18 | 14 | 76 | 80.9\% |
| All Pensioners | 117 | 174 | 25 | 142 | 44.9 \% |

$\dagger$ Take-Up Rate $=$ Total No. of Recipients $\div$ (Total No. of Recipients + Entitled Non-Recipients).

Table 2.14 Take-Up of IS for Non-Pensioners: By Tenure Type

|  | Entitled Recipients | Entitled NonRecipients | Non-Entitled Recipients | Total Number of Recipients | Take-Up $\dagger$ |
| :---: | :---: | :---: | :---: | :---: | :---: |
|  | Wave A |  |  |  |  |
| Owned Outright | 42 | 20 | 15 | 57 | 74.0 \% |
| Mortgaged | 84 | 63 | 21 | 105 | 62.5\% |
| Local Authority | 192 | 30 | 6 | 198 | 86.8 \% |
| Housing Assoc. | 25 | 5 | 1 | 26 | 83.9 \% |
| Rented | 49 | 6 | 9 | 58 | 90.6 \% |
| Missing | 1 | 0 | 6 | 7 | - |
| Total | 393 | 124 | 58 | 451 | 78.4 \% |
|  | Wave B |  |  |  |  |
| Owned Outright | 28 | 20 | 10 | 38 | 65.5 \% |
| Mortgaged | 96 | 60 | 15 | 111 | 64.9 \% |
| Local Authority | 197 | 32 | 6 | 203 | 86.4 \% |
| Housing Assoc. | 22 | 6 | 5 | 27 | 81.8\% |
| Rented | 57 | 8 | 4 | 61 | 88.4 \% |
| Missing | 0 | 0 | 13 | 13 | - |
| Total | 400 | 126 | 53 | 453 | 78.2 \% |
|  | Wave C |  |  |  |  |
| Owned Outright | 38 | 17 | 9 | 47 | 73.4 \% |
| Mortgaged | 93 | 41 | 32 | 125 | $75.3 \%$ $88.4 \%$ |
| Local Authority | 180 | 24 | 3 | 183 35 | 88.4 \% |
| Housing Assoc. | 28 | 4 | 7 | 35 55 | $89.7 \%$ $83.3 \%$ |
| Rented | 47 | 11 | 8 | 55 0 | 83.3 \% |
| Missing | 0 | 1 | 0 | 0 | - |
| Total | 386 | 98 | 59 | 445 | 82.0 \% |
|  | Wave D |  |  |  |  |
|  | 23 | 26 | 12 | 35 105 | 57.4 \% |
| Mortgaged | 95 | 46 | 10 | 105 190 | 69.5\% |
| Local Authority | 189 | 29 | 1 | 190 | 96.8\% |
| Housing Assoc. | 26 | 3 | 6 16 | 32 62 | 88.6 \% |
| Rented | 46 | 8 | 16 | 17 | $88.6 \%$ - |
| Missing | 1 | 2 | 16 | 17 | $79.5 \%$ |
| Total | 380 | 114 | 61 | 441 | 79.5 \% |

$\dagger$ Take-Up Rate $=$ Total No. of Recipients $\div$ (Total No. of Recipients + Entitled Non-Recipients).

Table 2.15 Take-Up of IS for Non-Pensioners: By Relationship to Head of HH

|  | Entitled Recipients | Entitled NonRecipients | Non-Entitled Recipients | Total Number of Recipients | Take-Up $\dagger$ |
| :---: | :---: | :---: | :---: | :---: | :---: |
|  | Wave A |  |  |  |  |
| Head of HH | 285 | 77 | 35 | 320 | 80.6 \% |
| Natural Child | 86 | 37 | 16 | 102 | 73.4 \% |
| Other Relative | 6 | 6 | 3 | 9 | 60.0 \% |
| Non-Relative | 16 | 4 | 4 | 20 | 83.3 \% |
| Missing | 0 | 0 | 0 | 0 | - |
| Total | 393 | 124 | 58 | 451 | 78.4 \% |
|  | Wave B |  |  |  |  |
| Head of HH | 275 | 85 | 38 | 313 | 78.6 \% |
| Natural Child | 92 | 34 | 8 | 100 | 74.6 \% |
| Other Relative | 14 | 3 | 3 | 17 | $85.0 \%$ |
| Non-Relative | 19 | 4 | 4 | 23 | 85.2 \% |
| Missing | 0 | 0 | 0 | 0 | - |
| Total | 400 | 126 | 53 | 453 | 78.2 \% |
|  | Wave C |  |  |  |  |
| Head of HH | 269 | 64 | 48 | 317 | 83.2 \% |
| Natural Child | 96 | 29 | 4 | 100 | $77.5 \%$ |
| Other Relative | 10 | 2 | 4 | 14 | $87.5 \%$ |
| Non-Relative | 11 | 3 | 3 | 14 | 82.4 \% |
| Missing | 0 | 0 | 0 | 0 | - |
| Total | 386 | 98 | 59 | 445 | 82.0 \% |
|  | Wave D |  |  |  |  |
| Head of HH | 268 | 74 | 51 | 319 | 81.2 \% |
| Natural Child | 88 | 34 | 5 | 93 | 73.2 \% |
| Other Relative | 8 | 3 | 2 | 10 | 76.9 \% |
| Non-Relative | 16 | 3 | 3 | 19 | 86.4 \% |
| Missing | 0 | 0 | 0 | 0 | - |
| Total | 380 | 114 | 61 | 441 | 79.5 \% |

$\dagger$ Take-Up Rate $=$ Total No. of Recipients $\div$ (Total No. of Recipients + Entitled Non-Recipients).

Table 2.16 Take-Up of IS for Non-Pensioners: By Current Job Status

|  | Entitled Recipients | Entitled NonRecipients | Non-Entitled Recipients | Total Number <br> of Recipients | Take-Up $\dagger$ |
| :---: | :---: | :---: | :---: | :---: | :---: |
|  | Wave A |  |  |  |  |
| Employed | 25 | 25 | 11 | 36 | 59.0 \% |
| Unemployed | 207 | 55 | 27 | 234 | 81.0 \% |
| Early Retired | 6 | 7 | 3 | 9 | 56.3 \% |
| Family Care | 114 | 21 | 2 | 116 | 84.7 \% |
| Sick/Disabled | 25 | 10 | 4 | 29 | 74.4 \% |
| Other | 11 | 6 | 5 | 16 | 72.7 \% |
| Missing | 5 | 0 | 6 | 11 | - |
| Total | 393 | 124 | 58 | 451 | 78.4 \% |
|  | Wave B |  |  |  |  |
| Employed | 20 | 11 | 4 | 24 | 68.6 \% |
| Unemployed | 206 | 48 | 30 | 236 | 83.1 \% |
| Early Retired | 6 | 13 | 1 | 7 | 35.0 \% |
| Family Care | 128 | 21 | 1 | 129 | 92.1 \% |
| Sick/Disabled | 29 | 23 | 3 | 32 | 58.2 \% |
| Other | 9 | 10 | 6 | 15 | 60.0 \% |
| Missing | 2 | 0 | 13 | 10 |  |
| Total | 400 | 126 | 53 | 453 | 78.2 \% |
|  | Wave C |  |  |  |  |
| Employed | 26 | 16 | 10 | 36 | 69.2 \% |
| Unemployed | 202 | 25 | 29 | 231 8 | 90.2\% |
| Early Retired | 4 | 10 | 4 | 8 124 | 44.4 \% 89.2 \% |
| Family Care | 118 | 15 | 6 | 124 28 | 54.9\% |
| Sick/Disabled | 26 | 23 | 2 | 18 | $54.9 \%$ $66.7 \%$ |
| Other | 10 | 9 | 8 | 18 0 | $66.7 \%$ - |
| Missing | 0 | 0 | 0 | 0 | - |
| Total | 386 | 98 | 59 | 445 | 82.0 \% |
|  | Wave D |  |  |  |  |
|  | 24 | 14 | 8 | 32 | 69.6 \% |
| Employed <br> Unemployed | 195 | 42 | 26 | 221 | 84.0 \% |
| Early Retired | 6 | 11 | 1 | 5 | 31.3 \% |
| Family Care | 101 | 17 | 5 | 106 | 86.2 \% |
| Sick/Disabled | 37 | 18 | 4 | 41 | 69.5 \% |
| Other | 17 | 12 | 5 | 22 | 64.7 \% |
| Missing | 0 | 0 | 12 | 14 |  |
| Total | 380 | 114 | 61 | 441 | 79.5 \% |

Total $\quad$ Take-Up Rate $=$ Total No. of Recipients $\div$ (Total No. of Recipients + Entitled Non-Recipients).

Table 2.17 Comparing IS Take-Up Estimates
Percentages

|  | 1991 | 1992 | 1993 | 1994 |
| :--- | :---: | :---: | :---: | :---: |
| TU I | 78.4 | 78.2 | 82.0 | 79.5 |
| TU 2 | 76.0 | 76.0 | 79.8 | 76.9 |
| TU 3 | 87.2 | 86.1 | 91.9 | 89.3 |
| DSS | $83-89$ | $84-93$ | $87-96 \dagger$ | $87-96 \dagger$ |


|  | Pensioners |  |  |  |
| :--- | :--- | :--- | :--- | :--- |
| TU I | 32.9 | 32.8 | 44.5 | 44.9 |
| TU 2 | 28.6 | 28.7 | 38.7 | 40.2 |
| TU 3 | 35.0 | 34.8 | 49.5 | 48.8 |
| DSS | $67-87$ | $65-77$ | $65-73 \dagger$ | $65-73 \dagger$ |

Source: TU 1, TU 2, and TU 3 are based on our microsimulation model using BHPS data (see main text for their definition). For DSS estimates see DSS (1994b, 1995b and 1995c).
$\dagger$ Combined estimates for 1993/94.

## APPENDIX 2B

## FIGURES FOR CHAPTER 2

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Figure 2.1 Flow Chart for the IS Algorithm


Note The final output from each entitlement module consists of a data set in which is Entitlement $\geq £ 0.10$.

Figure 2.2 Stage 1-Data Extraction and Manipulation


Figure 2.3 IS Entitlement for All Eligible Non-Pensioners - Wave A


Note: 1. Plot of 796 observations ( 27 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.4 IS Entitlement for All Eligible Pensioners - Wave A


Note: 1. Plot of 764 observations ( 27 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.5 IS Entitlement for Eligible Non-Pensioners - Wave B


Note: 1. Plot of 768 observations ( 23 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.6 IS Entitlement for Eligible Pensioners - Wave B


Note: 1. Plot of 1,009 observations ( 34 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.7 IS Entitlement for Eligible Non-Pensioners - Wave C


Note: 1. Plot of 662 observations ( 15 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.8 IS Entitlement for Eligible Non-Pensioners - Wave C


Note: 1. Plot of 611 observations ( 13 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.9 IS Entitlements for Eligible Non-Pensioners - Wave D


Note: 1. Plot of 658 observations ( 11 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.10 IS Entitlements for Eligible Pensioners - Wave D


Note: 1. Plot of 633 observations ( 13 outliers deleted).
2. Solid line gives IS entitled sample cut-off ( $£ 0.10$ ).

Figure 2.11 Non-Pensioner IS Take-Up By Family Type


Figure 2.12 Non-Pensioner IS Take-Up By Tenure Type


Figure 2.13 Non-Pensioner IS Take-Up By Relationship to HH


Figure 2.14 Non-Pensioner IS Take-Up By Current Job Status


Figure 2.15 Comparing IS Take-Up Estimates: Non-Pensioners


Note: See main text for definition of TU $1, \mathrm{TU} 2$ and TU 3.

Figure 2.16 Comparing IS Take-Up Estimates: Pensioners


Note: See main text for definition of TU $1, \mathrm{TU} 2$ and TU 3.

## APPENDIX 3

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3.2 Identifying Sets of Benefit Entitlement

Table 3.1 Applications of Kerr's Threshold Model

| Model | Structure | Key determinant |
| :--- | :--- | :--- |
| Kerr (1982) | Six thresholds in successive | Beliefs and feelings about |
| Pensioners | order. | claiming. |
| Ritchie \& Matthews (1982) | Key factors separating claimants | Perceptions of need and |
| Rent Allowances | from non-claimants. | eligibility. |
| Corden (1983 \& 1987) | As for Kerr but addition of | Perceptions of eligibility. |
| Family Income Supplement | administrative stages in <br> claiming process. |  |
| Graham (1984) | As for Kerr but addition of | Basic knowledge and perceived |
| Family Income Supplement | 'instinctive barrier' (i.e. general | eligibility. |
|  | notions of welfare perceptions). |  |
| Millar \& Cooke (1984) | Model consists of only 2 main | Basic knowledge and perceived |
| One-Parent Benefit | thresholds - basic knowledge \& | eligibility. |
|  | perceived eligibility. |  |

## Figure 3.1 The Claiming Process



Figure 3.2 Identifying Sets of Benefit Entitlement

Not drawn to scale


Note: $\quad$ Shaded Set $=$ Individuals Reporting Benefit Receipt $(b>0)$
Unshaded Set $=$ Individuals with Positive Computed Benefit Entitlement $\left(b_{A}>0\right)$
Sample Space $=$ Data Set (BHPS)
$\mathbf{E R}=$ Eligible Recipients
NER $=$ Non-Eligible Recipients
ENR $=$ Eligible Non-Recipients

## APPENDIX 4A

## TABLES FOR CHAPTER 4

| Variable Definition |  |
| :--- | :--- |
| log $I S=$ | log of IS entitlement level per week |
| income $=$ | all other income per week (scaled by s0 for logit regressions) |
| age $=$ | age of claimant (in decades for logit regressions) |
| lonepar =1 | if claimant is lone parent (DSS definition) |
| no. kids $=$ | number of dependent children in household (DSS definition) |
| female $=1$ | if claimant is female |
| couple $=1$ | if household consists of married couple (DSS definition) |
| head $=1$ | if claimant is head of household |
| degree $=1$ | if claimant holds a first degree or higher qualification |
| owner $=1$ | if house is owned outright |
| tenant $=1$ | if local authority council tenant |
| $U=1$ | if claimant is unemployed |
| weeks $U=$ | duration of unemployment measured in number of weeks |
| sick $=1$ | if claimant is sick or disabled (DSS definition) |
| subjectl =1 | if claimant is worse off financially than a year ago |
| subject $2=1$ | if claimant expects to be better off financially next year |

$\log I S=\quad \log$ of IS entitlement level per week
income $=\quad$ all other income per week (scaled by 50 for logit regressions)
age $=\quad$ age of claimant (in decades for logit regressions)
lonepar $=1 \quad$ if claimant is lone parent (DSS definition)
no. kids $=\quad$ number of dependent children in household (DSS definition)
female $=1 \quad$ if claimant is female
couple $=1 \quad$ if household consists of married couple (DSS definition)
head $=1 \quad$ if claimant is head of household
degree $=1 \quad$ if claimant holds a first degree or higher qualification
owner $=1 \quad$ if house is owned outright
tenant $=1 \quad$ if local authority council tenant
$U=1 \quad$ if claimant is unemployed
weeks $U=\quad$ duration of unemployment measured in number of weeks
sick $=1 \quad$ if claimant is sick or disabled (DSS definition)
subject $1=1 \quad$ if claimant is worse off financially than a year ago
subject $2=1 \quad$ if claimant expects to be better off financially next year

Note: subject 1 and subject 2 are subjective measures of financial well-being.

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Approximation Estimators - Comparing Calibration Models Comparison of Predicted Logit Take-Up Probabilities from Naive Model and Corrected Models

Table 4.1 Summary of Main Take-Up Studies Using UK/British Data ${ }^{1}$

| Variable | $\begin{gathered} \hline \hline S B+v e ~ e f f e c t \\ \text { (out of } 6 \text { ) } \end{gathered}$ | $\begin{gathered} \hline \text { SB -ve effect } \\ \text { (out of 6) } \end{gathered}$ | $\begin{gathered} \hline \text { HB +ve effect } \\ \text { (out of } 5 \text { ) } \end{gathered}$ | $\begin{gathered} \hline \overline{H B} \text {-ve effect } \\ \text { (out of } 5 \text { ) } \end{gathered}$ |
| :---: | :---: | :---: | :---: | :---: |
| Entitlement | 4 | 0 | 5 | 0 |
| Age | 1 | 0 | 1 | 0 |
| Private tenant | 1 | 2 | 0 | 5 |
| Owner-occupier | 0 | 3 | 0 | 1 |
| Single parent | 1 | 0 | 0 | 0 |
| Add. children | 1 | 0 | 3 | 0 |
| Add. adults | 0 | 1 | 1 | 1 |
| Head of house | 1 | 1 | 0 | 0 |
| Income | 1 | 2 | 0 | 4 |
| Away from work | 3 | 0 | 1 | 0 |
| Short-term U | 0 | 1 | 0 | 1 |
| Long-term $U$ | 2 | 0 | 0 | 0 |
| Education | 0 | 0 | 0 | 1 |
| Part-time work | 0 | 2 | 0 | 0 |

Note: Table gives the number of studies (out of the total number indicated at the top of each column) which found a corresponding coefficient statistically significant at the conventional 5 or 10 percent level.

[^122]Table 4.2 Descriptive Statistics for Main Data Set

|  | Mean | Std. Dev. | Minimum | Maximum |
| :--- | :---: | :---: | :---: | :---: |
| $y$ | 0.74 | 0.44 | 0 | 1 |
| IS $\dagger$ | 52.13 | 38.23 | 0.10 | 454.53 |
| income $\dagger$ | 14.71 | 27.05 | 0 | 185.00 |
| age | 33.47 | 13.56 | 18 | 65 |
| no. kids | 0.71 | 1.11 | 0 | 1 |
| female | 0.49 | 0.50 | 0 | 1 |
| couple | 0.23 | 0.42 | 0 | 1 |
| lonepar | 0.23 | 0.42 | 0 | 1 |
| head | 0.59 | 0.49 | 0 | 1 |
| degree | 0.06 | 0.25 | 0 | 1 |
| owner | 0.12 | 0.32 | 0 | 1 |
| tenant | 0.43 | 0.50 | 0 | 1 |
| U | 0.53 | 0.50 | 0 | 1 |
| weeks $U$ | 15.11 | 0.56 | 0 | 19 |
| sick | 0.04 | 0.50 | 0 | 14 |
| subject 1 | 0.53 | 0.16 |  | 0 |

Note: Sample size $=1,196$ observations. Data are a pooled sample drawn from the BHPS waves A to D for nonpensioners aged 18 and over only.
$\dagger$ Unscaled variables in $£$ per week.

Table 4.3 Naive Logit Estimates from Full Model Specification

|  | $\beta$ | s.e. | $P$-value | Marginal Effect |
| :---: | :---: | :---: | :---: | :---: |
| intercept | -1.395 | 0.513 | 0.007 | -0.146 |
| $\log I S$ | 0.303 | 0.092 | 0.001 | 0.032 |
| income | -0.987 | 0.193 | <0.001 | -0.103 |
| age | -0.156 | 0.076 | 0.040 | -0.016 |
| no. kids | 0.158 | 0.117 | 0.178 | 0.017 |
| female | 0.118 | 0.188 | 0.529 | 0.001 |
| couple | 0.655 | 0.287 | 0.023 | 0.069 |
| lonepar | 1.536 | 0.317 | $<0.001$ | 0.161 |
| head | 0.232 | 0.204 | 0.256 | 0.024 |
| degree | 0.642 | 0.364 | 0.078 | 0.067 |
| owner | -0.056 | 0.244 | 0.818 | -0.006 |
| tenant | 1.207 | 0.202 | $<0.001$ | 0.126 |
| $U$ | 0.536 | 0.232 | 0.021 | 0.056 |
| weeks $U$ | 0.037 | 0.006 | $<0.001$ | 0.004 |
| sick | 1.268 | 0.415 | 0.002 | 0.133 |
| subject 1 | 0.596 | 0.165 | $<0.001$ | 0.062 |
| subject 2 | -0.170 | 0.220 | 0.442 | -0.018 |
| $\log L$ | -508.05 |  |  |  |
| $\chi^{2}$ | 1,016.1 |  | 0.999 |  |
| sample size | 1,199 |  |  |  |

Notes: 1. Full model specification refers to logit model with all explanatory variables (see main text).
2. Log IS is measured in $£ / \mathrm{wk}$, income in $£ / \mathrm{wk} \div 50$, and age in decades.
3. Marginal effects are evaluated at the means of the explanatory variables.

Table 4.4 Descriptive Statistics for Main Data and Validation Data and OLS Estimates from Validation Data

|  | Main Data Set |  | Validation Data Set |  | OLS Estimates |  |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Mean | Std. Dev. | Mean | Std. Dev. | $\beta$ | s.e. |
| intercept | - | - | - | - | 1.677 | 0.423 |
| IS | 52.13 | 38.23 | 54.44 | 31.73 | 0.474 | 0.211 |
| $I^{2}$ | - | - | - | - | -0.004 | 0.028 |
| income | 14.71 | 27.05 | 10.54 | 20.26 | $2.88 \mathrm{e}-4$ | 0.002 |
| income ${ }^{2}$ | - | - | - | - | $-6.69 \mathrm{e}-6$ | $1.91 \mathrm{e}-5$ |
| age | 33.47 | 13.56 | 32.07 | 12.29 | 0.043 | 0.007 |
| age $^{2}$ | - | - | - | - | $-4.79 \mathrm{e}-4$ | $9.38 \mathrm{e}-5$ |
| couple | 0.24 | 0.42 | 0.24 | 0.43 | 0.582 | 0.048 |
| lonepar | 0.23 | 0.42 | 0.32 | 0.47 | 0.377 | 0.041 |
| tenant | 0.43 | 0.50 | 0.51 | 0.50 | 0.032 | 0.031 |
| $U$ | 0.53 | 0.50 | 0.52 | 0.50 | -0.069 | 0.048 |
| weeks $U$ | 15.11 | 20.56 | 16.90 | 21.61 | $1.73 \mathrm{e}-3$ | $9.40 \mathrm{e}-4$ |
| sick | 0.04 | 0.19 | 0.04 | 0.20 | -0.116 | 0.074 |
| subjectl | 0.53 | 0.50 | 0.55 | 0.50 | -0.050 | 0.028 |
|  |  |  |  |  |  |  |
| $\bar{R}^{2}$ | - | - | - | - | 0.455 |  |
| sample |  | 1,199 |  | 739 |  | 739 |
| size |  |  |  |  |  |  |

Table 4.5 Reduced Model Specification Logit Estimates from Naive Model and Approximation Estimators

|  | Naive |  | Zero-Order Approx. $\dagger$ |  | Second-Order Approx. $\dagger$ |  |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: |
|  | $\beta$ | s.e. | $\beta$ | s.e. $\ddagger$ | $\beta$ | s.e. $\ddagger$ |
| intercept | -1.212 | 0.471 | $-3.137(+)$ | 0.962 | $-3.330(+)$ | 0.684 |
| log IS* | 0.301 | 0.088 | $0.884(+)$ | 0.253 | $0.942(+)$ | 0.179 |
| income | -0.919 | 0.184 | $-1.160(+)$ | 0.178 | $-1.195(+)$ | 0.125 |
| age | -0.161 | 0.063 | $-0.231(+)$ | 0.064 | $-0.241(+)$ | 0.046 |
| couple | 0.815 | 0.255 | $0.802(-)$ | 0.272 | $0.806(-)$ | 0.195 |
| lonepar | 1.898 | 0.253 | $1.689(-)$ | 0.283 | $1.689(-)$ | 0.200 |
| tenant | 1.267 | 0.185 | $1.115(-)$ | 0.174 | $1.132(-)$ | 0.123 |
| U | 0.583 | 0.217 | $0.736(+)$ | 0.217 | $0.761(+)$ | 0.157 |
| weeks $U$ | 0.035 | 0.006 | $0.031(-)$ | 0.006 | $0.031(-)$ | 0.004 |
| sick | 1.228 | 0.411 | $1.340(+)$ | 0.436 | $1.367(+)$ | 0.308 |
| subjectl | 0.571 | 0.160 | $0.605(+)$ | 0.163 | $0.604(+)$ | 0.116 |
| log $L$ |  | -512.58 |  | -512.03 |  | -511.81 |

[^123]Table 4.6 Approximation Estimators - Comparing Calibration Models

|  | Naive | Approx + Calibration <br> Model I $\dagger$ | Approx + Calibration <br> Model II $\ddagger$ |
| :--- | :---: | :---: | :---: |
|  | $\beta$ | $\beta$ | $\beta$ |
| intercept | -1.212 | -3.137 | -5.873 |
| log IS | 0.301 | 0.884 | 1.256 |
| agcome | -0.919 | -1.160 | -0.938 |
| couple | -0.161 | -0.231 | -0.292 |
| lonepar | 0.815 | 0.802 | 0.801 |
| tenant | 1.898 | 1.689 | 1.378 |
| U | 1.267 | 1.115 | 1.114 |
| weeks $U$ | 0.583 | 0.736 | 0.887 |
| sick | 0.035 | 0.031 | 0.031 |
| subject | 1.228 | 1.340 | 1.432 |
| log $L$ | 0.571 | 0.605 | 0.620 |

$\dagger$ Zero-Order Approximation Estimates based on IS predictions from the calibration model

$$
\begin{aligned}
\log I S_{\kappa}= & \beta_{0}+\beta_{1} \log I S_{c}+\beta_{2} \log I S_{c}^{2}+\beta_{3} \text { income }+\beta_{4} \text { income }^{2} \\
& +\beta_{3} \text { age }+\beta_{6} \text { age }{ }^{2}+\beta_{1} \text { lonepar }+\beta_{1} \text { tenant }+\beta_{,} \text {couple } \\
& +\beta_{10} U+\beta_{11} \text { weeks } U+\beta_{12} \text { sick }+\beta_{10} \text { subject } 1+\varepsilon
\end{aligned}
$$

where $I S_{k}$ is the reported IS entitlement in $£ / \mathrm{wk}, I S_{c}$ is the computed IS entitlement in $£ / \mathrm{wk}$ and $\varepsilon$ is an iid error term. Model gives computed $\overline{R^{2}}=0.455$.
$\ddagger$ Zero-Order Approximation Estimates based on IS predictions from the calibration model

$$
\begin{aligned}
\log I S_{n}= & \beta_{0}+\beta_{1} \log I S_{c}+\beta_{\mathrm{s}} \text { income }+\beta_{,} \text {age }+\beta_{1} \text { lonepar }+\beta_{s} \text { tenant } \\
& +\beta_{6} \text { couple }+\beta_{9} U+\beta_{\imath} \text { weeks } U+\beta_{9} \text { sick }+\beta_{10} \text { subject }+\varepsilon
\end{aligned}
$$

where $I S_{R}$ is the reported IS entitlement in $£ / \mathrm{wk}, I S_{c}$ is the computed IS entitlement in $£ / \mathrm{wk}$ and $\varepsilon$ is an iid error term. Model gives computed $\bar{R}^{2}=0.426$.

Table 4.7 Logit Estimates from Naive Model and SIMEX Procedure $\dagger$

|  | Extrapolant Function |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Naive | Linear | Quadratic | Cubic | Quartic | Non-Linear |
| intercept | -1.212 | -1.440 ( + | -1.865 (+) | -2.183 (+) | $-2.470(+)$ | -2.836(+) |
|  | (0.471) | (0.500) | (0.589) | (0.662) | (0.724) | (0.889) |
| $\log I S$ | 0.301 | 0.355 (+) | 0.454 (+) | $0.528(+)$ | $0.594{ }^{(+)}$ | $0.676{ }^{(+)}$ |
|  | (0.088) | (0.097) | (0.121) | (0.140) | (0.155) | (0.211) |
| income | -0.919 | -0.867 (-) | -0.774 (-) | -0.707 (-) | -0.647 (-) | -0.583 (-) |
|  | (0.184) | (0.188) | (0.197) | (0.206) | (0.213) | (0.262) |
| age | -0.161 | -0.154 (-) | -0.140 (-) | -0.130 (-) | -0.120 (-) | -0.108 (-) |
|  | (0.063) | $(0.063)$ | $(0.064)$ | (0.065) | $(0.066)$ | (0.091) |
| couple | 0.815 | 0.754 (-) | 0.644 (-) | 0.565 (-) | 0.493 (-) | 0.481 (-) |
|  | (0.255) | (0.259) | (0.270) | (0.280) | (0.288) | (0.299) |
| lonepar | 1.898 | 1.880 (-) | $1.851(-)$ | $1.832(-)$ | 1.817 (-) | 1.803 (-) |
|  | (0.253) |  | (0.255) | (0.256) | (0.257) | (0.302) |
| tenant | 1.267 | 1.293 (+) | $1.338(+)$ | 1.371 (+) | $1.400{ }^{(+)}$ | $1.429(+)$ |
|  | (0.185) | (0.186) | (0.191) | (0.194) | (0.197) | (0.226) |
| $U$ | 0.583 | 0.581 (-) | 0.576 (-) | $0.573(-)$ | 0.568 (-) | 0.564 (-) |
|  | (0.217) | (0.218) | (0.218) | (0.219) | (0.220) | (0.281) |
| weeks $U$ | 0.035 | 0.035 () | 0.035 () | $0.036(+)$ | 0.036 (+) | $0.038(+)$ |
|  | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| sick | 1.228 | 1.239 (+) | $1.261(+)$ | 1.279 (+) | 1.297 (+) | 1.329 (+) |
|  | (0.411) | (0.413) | (0.416) | (0.420) | (0.422) | (0.547) |
| subject1 | 0.571 | 0.570 (-) | $0.568(-)$ | $0.567(-)$ | 0.570 (-) | 0.566 (-) |
|  | (0.160) | (0.161) | (0.161) | (0.162) | (0.162) | (0.236) |

$\dagger$ SIMEX estimates (see main text) based on above extrapolant functions using grid range [0,2].
Note: 1. Number of observations $=1,199$.
2. Log IS measured in $£ / \mathrm{wk}$, income in $£ / \mathrm{wk} \div 50$, and age in decades,
3. SIMEX corrected standard errors in parentheses (based on above extrapolant functions using grid range [ 0,2 ]).
4. The signs in parentheses indicate the direction of the absolute change relative to the naive estimate.

Table 4.8 Comparison of SIMEX Extrapolant Functions and Grid Ranges

|  | Quadratic Extrapolant |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Grid Range 0-2 |  | Grid Range 0-3 |  | Grid Range 0-4 |  |
|  | $\beta$ | 95\% CI | $\beta$ | 95\% CI | $\beta$ | 95\% CI |
| intercept | -1.865 | -1.952 to -1.779 | -1.726 | -1.811 to - 1.641 | -1.617 | -1.707 to -1.527 |
| $\log I S$ | 0.454 | 0.434 to 0.474 | 0.422 | 0.402 to 0.442 | 0.396 | 0.375 to 0.417 |
| income | $-0.774$ | -0.793 to -0.756 | -0.804 | -0.822 to-0.786 | -0.828 | -0.847 to -0.808 |
| age | -0.140 | -0.143 to -0.137 | -0.145 | -0.147 to -0.142 | -0.148 | -0.151 to -0.145 |
| couple | 0.644 | 0.623 to 0.666 | 0.680 | 0.658 to 0.701 | 0.707 | 0.685 to 0.730 |
| lonepar | 1.851 | 1.846 to 1.856 | 1.860 | 1.855 to 1.865 | 1.867 | 1.861 to 1.873 |
| tenant | 1.338 | 1.330 ¢ 1.347 | 1.324 | 1.315 to 1.333 | 1.312 | 1.303 to 1.322 |
| $U$ | 0.576 | 0.575 to 0.578 | 0.578 | 0.577 to 0.579 | 0.579 | 0.578 to 0.580 |
| weeks $U$ | 0.035 | 0.035 to 0.035 | 0.035 | 0.035 to 0.035 | 0.035 | 0.035 to 0.035 |
| sick | 1.261 | 1.256 to 1.266 | 1.253 | 1.249 to 1.258 | 1.248 | 1.243 to 1.252 |
| subject 1 | 0.568 | 0.568 to 0.568 | 0.568 | 0.568 to 0.569 | 0.569 | 0.569 to 0.569 |

Non-Linear Extrapolant

|  | Grid Range 0-2 |  | Grid Range 0-3 |  | Grid Range 0-4 |  |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | $\beta$ | $95 \% ~ C I$ | $\beta$ | $95 \% ~ C I$ | $\beta$ | $95 \% ~ C I$ |
| intercept | -2.836 | -2.929 to.- .743 | -2.765 | -2.822 to -2.707 | -2.73 | -2.770 to -2.685 |
| log IS | 0.676 | 0.655 to 0.697 | 0.660 | 0.647 to 0.673 | 0.652 | 0.642 to 0.661 |
| income | -0.583 | -0.601 to -0.566 | -0.597 | -0.608 to -0.586 | -0.604 | -0.619 to 0.596 |
| age | -0.108 | -0.111 to -0.105 | -0.111 | -0.113 to -0.109 | -0.112 | -0.113 to -0.110 |
| couple | 0.925 | 0.870 to 0.979 | 0.954 | 0.909 to 1.000 | 0.976 | 0.936 to 1.017 |
| lonepar | 1.803 | 1.800 to 1.805 | 1.804 | 1.803 to 1.806 | 1.805 | 1.804 to 1.806 |
| tenant | 1.429 | 1.420 to 1.438 | 1.422 | 1.417 to 1.427 | 1.418 | 1.414 to 1.422 |
| U | 0.588 | 0.586 to 0.590 | 0.589 | 0.587 to 0.591 | 0.590 | 0.588 to 0.591 |
| weeks $U$ | 0.038 | 0.037 to 0.038 | 0.037 | 0.037 to 0.038 | 0.037 | 0.037 to 0.037 |
| sick | 1.329 | 1.320 to 1.337 | 1.324 | 1.320 to 1.329 | 1.323 | 1.320 to 1.326 |
| subject 1 | 0.566 | 0.565 to 0.567 | 0.565 | 0.564 to 0.566 | 0.565 | 0.564 to 0.566 |

Note: 1. Number of observations $=1,199$.
2. Log IS measured in $£ / \mathrm{wk}$, income in $£ / \mathrm{wk} \div 50$, and age in decades.

Table 4.9 Comparison of Approximation Estimates and Selected SIMEX Estimates

|  | Naive | Zero-Order <br> Approx. | Second-Order <br> Approx. | SIMEX NL2 | SIMEX NL4 |
| :--- | :---: | :---: | :---: | :---: | :---: |
|  |  | -1.212 | $-3.137(+)$ | $-3.330(+)$ | $-2.836(+)$ |
| intercept | 0.301 | $0.884(+)$ | $0.942(+)$ | $0.676(+)$ | $-2.73(+)$ |
| log IS | -0.919 | $-1.160(+)$ | $-1.195(+)$ | $-0.583(-)$ | $-0.604(-)$ |
| income | -0.161 | $-0.231(+)$ | $-0.241(+)$ | $-0.108(-)$ | $-0.112(-)$ |
| age | 0.815 | $0.802(-)$ | $0.806(-)$ | $0.481(-)$ | $0.486(-)$ |
| couple | 1.898 | $1.689(-)$ | $1.689(-)$ | $1.803(-)$ | $1.805(-)$ |
| lonepar | 1.267 | $1.115(-)$ | $1.132(-)$ | $1.429(+)$ | $1.418(+)$ |
| tenant | 0.583 | $0.736(+)$ | $0.761(+)$ | $0.564(-)$ | $0.567(-)$ |
| U | 0.035 | $0.031(-)$ | $0.031(-)$ | $0.038(+)$ | $0.037(+)$ |
| weeks $U$ | 1.228 | $1.340(+)$ | $1.367(+)$ | $1.329(+)$ | $1.323(+)$ |
| sick | 0.571 | $0.605(+)$ | $0.604(+)$ | $0.566(-)$ | $0.565(-)$ |
| subject 1 |  |  |  |  |  |

Notes: 1. SIMEX NL2 $=$ SIMEX estimates obtained from the non-linear extrapolant, grid-range $[0,2]$.
2. SIMEX NL4 = SIMEX estimates obtained from the non-linear extrapolant, grid-range [0,4].
3. The signs in parentheses indicate the direction of the absolute change relative to the naive estimate.

Table 4.10 Comparison of Predicted Logit Take-Up Probabilities from Naive Model and Corrected Models

|  |  | IS Entitlement |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: |
|  |  | f10 | £20 | $£ 50$ | £100 |
| Lone <br> Parent | Naive | 0.703 | 0.745 | 0.794 | 0.826 |
|  | Zero Order | 0.462 | 0.613 | 0.781 | 0.868 |
|  | Second Order | 0.439 | 0.601 | 0.781 | 0.873 |
|  | SIMEX Q2 | 0.642 | 0.711 | 0.788 | 0.836 |
|  | SIMEX Q4 | 0.666 | 0.724 | 0.790 | 0.832 |
|  | SIMEX NL2 | 0.544 | 0.656 | 0.780 | 0.850 |
|  | SIMEX NL4 | 0.555 | 0.662 | 0.781 | 0.848 |
| Couple | Naive | 0.750 | 0.787 | 0.830 | 0.857 |
|  | Zero Order | 0.581 | 0.719 | 0.852 | 0.914 |
|  | Second Order | 0.562 | 0.712 | 0.854 | 0.918 |
|  | SIMEX Q2 | 0.676 | 0.741 | 0.812 | 0.856 |
|  | SIMEX Q4 | 0.705 | 0.759 | 0.819 | 0.856 |
|  | SIMEX NL2 | 0.681 | 0.773 | 0.864 | 0.910 |
|  | SIMEX NL4 | 0.698 | 0.784 | 0.868 | 0.912 |
| Single | Naive | 0.676 | 0.720 | 0.772 | 0.807 |
|  | Zero Order | 0.519 | 0.666 | 0.818 | 0.892 |
|  | Second Order | 0.503 | 0.660 | 0.822 | 0.899 |
|  | SIMEX Q2 | 0.622 | 0.693 | 0.774 | 0.824 |
|  | SIMEX Q4 | 0.643 | 0.703 | 0.773 | 0.818 |
|  | SIMEX NL2 | 0.546 | 0.658 | 0.781 | 0.851 |
|  | SIMEX NL 4 | 0.554 | 0.661 | 0.780 | 0.848 |
| Notes. | $\begin{aligned} & 2=\text { SIMEX estir } \\ & L 2=\text { SIMEX esti } \\ & 4=\text { SIMEX estir } \\ & L 4=\text { SIMEX esti } \end{aligned}$ | ained fro tained fro ained fro tained fro |  | d-range <br> rid-range <br> d-range <br> rid-range |  |

## APPENDIX 4B

## FIGURES FOR CHAPTER 4

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Figure 4.1 Female Lone Parent with 2 Children-Approximation Estimates
——Naive $\quad--$ Zero-Order $\quad$.... Second-Order


Figure 4.2 Married Couple with 1 Child - Approximation Estimates


Figure 4.3 Single Male - Approximation Estimates


Figure 4.4 Scatter Plot of Computed IS against Reported IS


Figure 4.5 Scatter Plot of Predicted IS against Reported IS


Note: Number of observations in Figures 4.4 and $4.5=739$.

Figure 4.6 SIMEX Extrapolation Step Plots, Grid Range [0-2]


Figure 4.6 - continued




Figure 4.7 Female Lone Parent with 2 Children-SIMEX Estimates

| - Naive | -- -Linear | $\cdots$ Quadratic |
| :--- | :--- | :--- |
| $-\cdots$ Cubic | $-\cdots$ Quartic | - |



Figure 4.8 Married Couple with 1 Child-SIMEX Estimates


Figure 4.9 Single Male - SIMEX Estimates


Figure 4.10 Extrapolation Functions for Log IS Parameter


Figure 4.11 Extrapolation Functions for Lone Parent Parameter


Note: Squares give computed SIMEX values. Extrapolant lines are then fitted where: linear = solid, quadratic = dashed $\&$ dotted, cubic $=$ dotted, quartic $=$ dashed, non-linear $=$ solid.

Extrapolant Functions for Figure 4.10: Parameter - Log IS

| Extrapolant | Estimated Extrapolant Function | $\bar{R}^{2}$ |
| :--- | :---: | :---: |
| Linear | $0.281-0.074 \lambda$ | 0.952 |
| Quadratic | $0.297-0.130 \lambda+0.028 \lambda^{2}$ | 0.997 |
| Cubic | $0.300-0.155 \lambda+0.061 \lambda^{2}-0.011 \lambda^{3}$ | 0.999 |
| Quartic | $0.301-0.166 \lambda+0.089 \lambda^{2}-0.033 \lambda^{3}+0.005 \lambda^{4}$ | 1.000 |
| Non-Linear | $0.008+[0.520 /(1.779+\lambda)]$ | - |

Extrapolant Functions for Figure 4.11: Parameter - Lone Parents

| Extrapolant | Estimated Extrapolant Function | $\bar{R}^{2}$ |
| :--- | :---: | :---: |
| Linear | $1.904+0.024 \lambda$ | 0.960 |
| Quadratic | $1.899+0.040 \lambda-0.008 \lambda^{2}$ | 0.998 |
| Cubic | $1.898+0.047 \lambda-0.017 \lambda^{2}+0.003 \lambda^{3}$ | 0.999 |
| Quartic | $1.898+0.049 \lambda-0.023 \lambda^{2}+0.008 \lambda^{3}-0.001 \lambda^{4}$ | 1.000 |
| Non-Linear | $1.998-[0.204 /(2.046+\lambda)]$ | - |

Note: Tables give the estimated extrapolant functions used to predict the SIMEX estimate at $\lambda=-1$.

Figure 4.12 Comparison of Grid Ranges for SIMEX Extrapolation:

## Log IS Parameter





Note: Squares give computed SIMEX values. Extrapolant lines are then fitted where: quadratic = solid, quartic $=$ dotted, non-linear $=$ dashed.

Figure 4.13 Comparison of Grid Ranges for SIMEX Extrapolation:

## Lone Parent Parameter



Note: Squares give computed SIMEX values. Extrapolant lines are then fitted where: quadratic $=$ solid. quartic $=$ dotted, non-linear $=$ dashed.

Extrapolant Functions for Figure 4.12: Parameter - Log IS

| Extrapolant | Estimated Extrapolant Function | $\bar{R}^{2}$ |
| :--- | :---: | :---: |
|  | Grid Range $0-2$ |  |
| Quadratic | $0.297-0.130 \lambda+0.028 \lambda^{2}$ | 0.997 |
| Quartic | $0.301-0.166 \lambda+0.089 \lambda^{2}-0.033 \lambda^{3}+0.005 \lambda^{4}$ | 1.000 |
| Non-Linear | $0.008+[0.520 /(1.779+\lambda)]$ | - |
|  | Grid Range 0-3 |  |
| Quadratic | $0.292-0.111 \lambda+0.018 \lambda^{2}$ | 0.994 |
| Quartic | $0.301-0.159 \lambda+0.070 \lambda^{2}-0.019 \lambda^{3}+0.002 \lambda^{4}$ | 0.999 |
| Non-Linear | $0.004+[0.540 /(1.824+\lambda)]$ | - |
|  | Grid Range 0-4 |  |
| Quadratic | $0.287-0.097 \lambda+0.013 \lambda^{2}$ | 0.990 |
| Quartic | $0.300-0.151 \lambda+0.058 \lambda^{2}-0.013 \lambda^{3}+0.001 \lambda^{4}$ | 0.999 |
| Non-Linear | $0.002+[0.551 /(1.848+\lambda)]$ | - |

Extrapolant Functions for Figure 4.13: Parameter - Lone Parents

| Extrapolant | Estimated Extrapolant Function | $\bar{R}^{2}$ |
| :--- | :---: | :---: |
|  | Grid Range 0-2 |  |
| Quadratic | $1.899+0.040 \lambda-0.008 \lambda^{2}$ | 0.998 |
| Quartic | $1.898+0.049 \lambda-0.023 \lambda^{2}+0.008 \lambda^{3}-0.001 \lambda^{4}$ | 1.000 |
| Non-Linear | $1.998-[0.204 /(2.046+\lambda)]$ | - |
|  | Grid Range 0-3 |  |
| Quadratic | $1.900+0.035 \lambda-0.005 \lambda^{2}$ | 0.996 |
| Quartic | $1.898+0.047 \lambda-0.019 \lambda^{2}+0.005 \lambda^{3}-0.0005 \lambda^{4}$ | 1.000 |
| Non-Linear | $1.999-[0.209 /(2.074+\lambda)]$ | - |
| Quadratic | Grid Range $0-4$ | 0.993 |
| Quartic | $1.898+0.046 \lambda-0.016 \lambda^{2}+0.003 \lambda^{3}-0.0003 \lambda^{4}$ | 0.999 |
| Non-Linear | $1.999-[0.211 /(2.087+\lambda)]$ | - |

[^124]Figure 4.14 SIMEX Estimates for Single Male - Quadratic Extrapolant



Figure 4.15 SIMEX Estimates for Single Male - Non-Linear Extrapolant

| - Naive $\quad--$ Grid $0-2 \cdots$ | Grid $0.3 \cdots$ |
| :--- | :--- |



Figure 4.16 Female Lone Parent with 2 Children-Comparison of Estimates
——_Naive - - Approximation $\cdot$.... SIMEX


Figure 4.17 Married Couple with 1 Child - Comparison of Estimates


Figure 4.18 Single Male - Comparison of Estimates


Note: Approximation $=$ Second-order estimator; SIMEX $=$ Non-linear extrapolant, grid range [0,2].

## APPENDIX 4C

## THE ITERATIVELY REWEIGHTED LEAST SQUARES ALGORITHM

The iteratively reweighted least squares algorithm (IRLS) can be used to obtain maximum likelihood estimates (MLEs) in generalized linear models such as the logit model (see McCullagh and Nelder (1983)). We make use of this algorithm to obtain the MLEs of both the zero-order approximation estimator and the second-order approximation estimator discussed in the main text.

Suppose $x$ is some covariate (scalar or vector) of interest and $\beta$ is some corresponding unknown parameter to be estimated. The algorithm updates the parameter value at each iteration according to the equation

$$
\begin{equation*}
\beta_{t+1}=\beta_{t}+\left(x^{\prime} w x\right)^{-1} x^{\prime} w q \tag{A}
\end{equation*}
$$

where $w$ is a weighting function defined as

$$
w=\operatorname{diag}\left\{\frac{(d F(z) / d z)^{2}}{F(z)[1-F(z)]}\right\}
$$

and

$$
q=[y-F(z)] \frac{d z}{d F(z)}
$$

Furthermore $F(z)$ is the distribution function of interest and $z$ are the arguments of that function.

Consider first the zero-order logistic approximation for which the distribution function is the logistic function

$$
F(z)=\frac{e^{z}}{1+e^{z}} \quad \text { and } \quad z=\beta^{\prime} x
$$

It follows that

$$
\frac{d F(z)}{d z}=\frac{e^{z}}{\left(1+e^{z}\right)^{2}}
$$

and thus

$$
w=\left[\frac{e^{z}}{\left(1+e^{z}\right)^{2}}\right]^{2} /\left[\frac{e^{z}}{\left(1+e^{z}\right)^{2}}\right]=\frac{e^{z}}{\left(1+e^{z}\right)^{2}}=\frac{d F(z)}{d z}
$$

and

$$
q=\frac{y\left(1+e^{z}\right)^{2}}{e^{z}}-\left(1+e^{z}\right) .
$$

Hence the algorithm simplifies to

$$
\begin{equation*}
\boldsymbol{\beta}_{t+1}=\boldsymbol{\beta}_{t}+\left[x^{\prime}\left(\frac{e^{z}}{\left(1+e^{z}\right)^{2}}\right) x\right]^{-1} x^{\prime}[y-F(z)] . \tag{B}
\end{equation*}
$$

For the second-order approximation estimator we use the simplification of Chesher (1991a) such that

$$
\begin{equation*}
G(z)=\exp \left\{z+\beta^{2} \frac{\hat{\sigma}_{v}^{2}\left(1-e^{z}\right)}{2\left(1+e^{z}\right)^{2}}\right\} / 1+\exp \left\{z+\beta^{2} \frac{\hat{\sigma}_{v}^{2}\left(1-e^{z}\right)}{2\left(1+e^{z}\right)^{2}}\right\} . \tag{C}
\end{equation*}
$$

Then we have the first derivative

$$
\frac{d G(z)}{d z}=\frac{\Omega \exp (\Gamma)}{[1+\exp (\Gamma)]^{2}}
$$

where

$$
\begin{aligned}
& \Gamma=z+\frac{\psi\left(1-e^{z}\right)}{2\left(1+e^{z}\right)^{2}} \\
& \Omega=\frac{d}{d z} \exp (\Gamma)=1+\frac{\psi\left(e^{z}-e^{2 z}-2\right)}{2\left(1+e^{z}\right)^{3}} \text { and } \psi=\beta^{2} \hat{\sigma}_{v}^{2} .
\end{aligned}
$$

Since $G(z)$ can now be written as

$$
G(z)=\frac{\exp (\Gamma)}{1+\exp (\Gamma)}
$$

we have

$$
G(z)[1-G(z)]=\frac{\exp (\Gamma)}{[1+\exp (\Gamma)]^{2}} .
$$

Thus the weighting function is given by

$$
w=\frac{\Omega^{2} \exp (\Gamma)}{[1+\exp (\Gamma)]^{2}}
$$

and

$$
q=\frac{y[1+\exp (\Gamma)]^{2}-\exp (\Gamma)[1+\exp (\Gamma)]}{\Omega \exp (\Gamma)}
$$

Finally, the algorithm for the second-order approximation estimator is then

$$
\begin{equation*}
\beta_{t+1}=\beta_{t}+\left[x^{\prime}\left(\frac{\Omega^{2} \exp (\Gamma)}{[1+\exp (\Gamma)]^{2}}\right) x\right]^{-1} x^{\prime} \Omega[y-G(z)] \tag{D}
\end{equation*}
$$

Both forms of the algorithm were programmed in the Interactive Matrix Language (IML) of the SAS package. For the second-order approximation estimator the zero-order approximation estimates were used as starting values. Careful choice of starting values for the second-order approximation is recommended in order to aid convergence (the zero order corrected logit estimates are a natural choice).

## APPENDIX 5A

## TABLES FOR CHAPTER 5

## Variable Definition

| $\log I S=$ | log of computed IS entitlement in $£$ /week |
| :---: | :---: |
| income $=$ | all other income in f /week (scaled by 50 for probit regressions) |
| age $=$ | age of individual (in decades for probit regressions) |
| lonepar $=1$ | if individual is lone parent (DSS definition) |
| no. kids $=$ | number of dependent children in household (DSS definition) |
| female $=1$ | if individual is female |
| couple $=1$ | if household consists of married couple (DSS definition) |
| head $=1$ | if individual is head of household |
| degree $=1$ | if individual holds a first degree or higher qualification |
| owner $=1$ | if accommodation is owned outright |
| tenant $=1$ | if local authority council tenant |
| $U=I$ | if individual is unemployed |
| weeks $U=$ | duration of unemployment measured in number of weeks |
| sick $=1$ | if individual is sick or disabled (DSS definition) |
| subject $1=1$ | if individual is worse off financially than a year ago |
| subject $2=1$ | if individual expects to be better off financially next year |

The following additional variables are used in Tables 5.16a and 5.16b:

| log wage $=$ | log of after tax wage in f/week |
| :--- | :--- |
| married $=1$ | if individual is married |
| degree $=1$ | individual's highest academic qualification $=$ first or higher degree |
| hnd $=1$ | if individual's highest academic qualification = hnd or hnc |
| a-level $=1$ | if individual's highest academic qualification = a-levels |
| o-level $=1$ | if individual's highest academic qualification =o-levels |
| london $=1$ | if individual lives in Greater London |
| north $=1$ | ifindividual lives in north of England |
| $I M R=$ | inverse Mill's ratio |

5.1 Panel Data Composition
5.2 Spell Runs for Entitled at Wave A
5.3a Cross-Tabulations of Take-Up Status: Following Wave A
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5.4a Following Wave A ENRs: From Employment Status Into Employment Status
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5.5a Following Wave A ENRs: By Employment Status Into Take-Up Status
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5.6a Following Wave A ENRs: From Current Financial Situation Into Current Financial Situation
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5.8a Following Wave A ENRs: From Past Financial Situation Into Past Financial Situation
5.8b Following Wave B ENRs: From Past Financial Situation Into Past Financial Situation
5.8c Following Wave C ENRs: From Past Financial Situation Into Past Financial Situation
5.9a Following Wave A ENRs: From Past Financial Situation Into Take-Up Status
5.9b Following Wave B ENRs: From Past Financial Situation Into Take-Up Status
5.9c Following Wave C ENRs: From Past Financial Situation Into Take-Up Status
5.10a Following Wave A ENRs: From Future Financial Situation Into Future Financial Situation
5.10b Following Wave B ENRs: From Future Financial Situation Into Future Financial Situation
5.10c Following Wave C ENRs: From Future Financial Situation Into Future Financial Situation
5.11a Following Wave A ENRs: From Future Financial Situation Into Take-Up Status
5.11b Following Wave B ENRs: From Future Financial Situation Into Take-Up Status
5.11 c Following Wave C ENRs: From Future Financial Situation Into Take-Up Status
5.12 Descriptive Statistics for Panel Data
5.13 Probit Estimates of Take-Up Using Panel Data
5.14 Testing for State Dependence in Take-Up
5.15 Descriptive Statistics
5.16a Selection Model of Earnings - Men
5.16b Selection Model of Earnings - Women
5.17 Testing for Future Events in Take-Up

Note: ENRs $=$ Entitled Non-Recipients

Table 5.1 Panel Data Composition

|  |  | Wave |  |  |  |
| :--- | :---: | :---: | :---: | :---: | :---: |
|  | A | B | C | D | Panel |
| Sample Size I | 517 | 524 | 484 | 494 | - |
| Sample Size 2 | 510 | 509 | 475 | 478 | 1,201 |
| Take-Up Rate (\%) | 78.0 | 76.3 | 80.6 | 77.8 | 78.3 |

Note: Sample size 1 gives the sample of entitled individuals as generated by our microsimulation model of Chapter 2. We delete a relatively small number of large outlying values for the IS entitlement to give the samples of sample size 2. The take-up rates are computed for the samples of sample size 2 and are thus somewhat misleading (see Chapter 2 for details).

Table 5.2 Spell Runs for Entitled at Wave A

| Spell Runs | Frequency | Percent |
| :---: | :---: | :---: |
| Entitled at $A$ and $B$ | 218 of which: |  |
| 11 | 184 | 80.7 |
| 10 | 14 | 6.1 |
| 01 | 10 | 4.4 |
| 00 | 20 | 8.8 |
| Entitled at $A, B$ and $C$ | 116 of which: |  |
| 111 | 92 | 79.3 |
| 110 | 3 | 2.6 |
| 101 | 9 | 7.8 |
| 100 | 0 | 0.0 |
| 011 | 6 | 5.2 |
| 010 | 1 | 0.9 |
| 001 | 0 | 0.0 |
| 000 | 5 | 4.3 |
| Entitled at A, B, C and D | 71 of which: |  |
| 1111 | 55 |  |
| 1110 | 3 | 4.2 |
| 1101 | 2 | 2.8 |
| 1011 | 3 | 4.2 |
| 0111 | 3 | 4.2 |
| 1100 | 0 | 0.0 |
| 1010 | 2 | 2.8 0.0 |
| 1001 | 0 | 0.0 |
| 0011 | 0 | 0.0 |
| 0101 | 0 | 0.0 |
| 1000 | 0 | 0.0 |
| 0100 | 0 | 0.0 |
| 0010 | 0 | 0.0 |
| 0001 | 0 | 0.0 4.2 |
| 0000 | 3 | 4.2 |

[^125]Table 5.3a Cross-Tabulations of Take-Up Status: Following Wave A

| Wave A | Wave B |  |  | Wave C |  |  | Wave D |  |  | Total |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | ENR | ER | NE | ENR | ER | NE |  |
| ENR | 20 | 11 | 84 | 8 | 11 | 96 | 4 | 10 | 101 | 115 |
| Row \% | 17.39 | 9.57 | 73.04 | 6.96 | 9.57 | 83.84 | 3.48 | 8.70 | 87.83 |  |
| Col. \% | 16.53 | 2.84 | 12.14 | 8.51 | 2.89 | 13.22 | 3.70 | 2.70 | 13.97 |  |
| ER | 14 | 183 | 198 | 6 | 128 | 261 | 11 | 109 | 275 | 395 |
| Row \% | 3.54 | 46.33 | 50.13 | 1.52 | 32.41 | 66.08 | 2.78 | 27.59 | 69.62 |  |
| Col. \% | 11.57 | 47.16 | 28.61 | 6.38 | 33.60 | 35.95 | 10.19 | 29.46 | 38.04 |  |
| NE | 87 | 194 | 410 | 80 | 242 | 369 | 93 | 251 | 347 | 691 |
| Row \% | 12.59 | 28.08 | 59.33 | 11.58 | 35.02 | 53.40 | 13.46 | 36.32 | 50.22 |  |
| Col. \% | 71.90 | 50.00 | 59.25 | 85.11 | 63.52 | 50.83 | 86.11 | 67.84 | 47.99 |  |
| Total | 121 | 388 | 692 | 94 | 381 | 726 | 108 | 370 | 723 | 1,201 |

Notes: $\quad$ ENR $=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.

| Chi-square | $\mathrm{A} \rightarrow \mathrm{B}$ | $82.4(P=0.001)$ | $\mathrm{A} \rightarrow \mathrm{C}$ | $70.7(P=0.001)$ | $\mathrm{A} \rightarrow \mathrm{D}$ |
| :--- | :--- | :--- | :--- | :--- | :--- |
| LR Chi-square | $\mathrm{A} \rightarrow \mathrm{B}$ | $90.8(P=0.001)$ | $\mathrm{A} \rightarrow \mathrm{C}$ | $84.9(P=0.001)$ | $\mathrm{A} \rightarrow \mathrm{D}$ |

Table 5.3b Cross-Tabulations of Take-Up Status: Following Wave B

| Wave B | Wave C |  |  | Wave D |  |  | Total |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | ENR | ER | NE |  |
| ENR | 17 | 21 | 83 | 13 | 16 | 92 | 121 |
| Row\% | 14.05 | 17.36 | 68.60 | 10.74 | 13.22 | 76.03 |  |
| Col. \% | 18.09 | 5.51 | 16.37 | 12.04 | 4.32 | 18.25 |  |
| ER | 14 | 172 | 202 | 11 | 123 | 254 | 388 |
| Row \% | 3.61 | 44.33 | 52.06 | 2.84 | 31.70 | 65.46 |  |
| Col. \% | 14.89 | 45.14 | 39.84 | 10.19 | 33.24 | 50.40 |  |
| NE | 63 | 188 | 222 | 84 | 231 | 158 | 473 |
| Row \% | 13.32 | 39.75 | 46.93 | 17.76 | 48.84 | 33.40 |  |
| Col. \% | 67.02 | 49.34 | 43.79 | 77.78 | 62.43 | 31.35 |  |
| Total | 94 | 381 | 507 | 108 | 370 | 504 | 982 |

Notes: $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.

| Chi-square | $\mathrm{B} \rightarrow \mathrm{C}$ | $50.2(P=0.001)$ | $\mathrm{B} \rightarrow \mathrm{D}$ | $140.8(P=0.001)$ |
| :--- | :--- | :--- | :--- | :--- |
| LR Chi-square | $\mathrm{B} \rightarrow \mathrm{C}$ | $56.7(P=0.001)$ | $\mathrm{B} \rightarrow \mathrm{D}$ | $154.1(P=0.001)$ |

Table 5.3c Cross-Tabulations of Take-Up Status: Following Wave C

| Wave C | Wave D |  |  | Total |
| :--- | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE |  |
| ENR | 29 | 12 | 53 | 94 |
| Row \% | 30.85 | 12.77 | 56.38 |  |
| Col. \% | 26.85 | 3.24 | 20.78 |  |
| ER | 11 | 168 | 202 | 381 |
| Row\% | 2.89 | 44.09 | 53.02 |  |
| Col. \% | 10.19 | 45.41 | 79.22 |  |
| NE | 68 | 190 | 0 | 258 |
| Row\% | 26.36 | 73.64 | 0.0 |  |
| Col. \% | 62.96 | 51.35 | 0.0 |  |
| Total | 108 | 370 | 255 | 733 |
| Notes: ENR = entitled non-recipients, ER = entitled recipients, NE $=$ not entitled. |  |  |  |  |

Notes: $\quad \mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.

| Chi-square | $\mathrm{C} \rightarrow \mathrm{D}$ | $272.3(P=0.001)$ |
| :--- | :--- | :--- |
| LR Chi-square | $\mathrm{C} \rightarrow \mathrm{D}$ | $372.6(P=0.001)$ |

Table 5.4a Following Wave A Entitled Non-Recipients: From Employment Status Into Employment Status

| Wave A | Wave B |  |  |  |  | Wave C |  |  |  |  | Wave D |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | E | U | FC | Other | Total | E | U | FC | Other | Total | E | U | FC | Other | Total |
| E | 13 | 1 | 1 | 7 | 22 | 15 | 1 | 0 | 7 | 23 | 11 | 0 | 2 | 8 | 21 |
| Row \% | 59.09 | 4.55 | 4.55 | 31.82 |  | 65.22 | 4.35 | 0.00 | 30.43 |  | 52.38 | 0.00 | 9.52 | 38.10 |  |
| Col. \% | 36.11 | 5.26 | 7.69 | 21.88 |  | 35.71 | 6.67 | 0.00 | 21.21 |  | 28.21 | 0.00 | 18.18 | 22.22 |  |
| U | 18 | 16 | 0 | 7 | 41 | 22 | 13 | 0 | 7 | 42 | 22 | 9 | 0 | 7 | 38 |
| Row \% | 43.90 | 39.02 | 0.00 | 17.07 |  | 52.38 | 30.95 | 0.00 | 16.67 |  | 57.89 | 23.68 | 0.00 | 18.42 |  |
| Col. \% | 50.00 | 84.21 | 0.00 | 21.88 |  | 52.38 | 86.67 | 0.00 | 21.21 |  | 56.41 | 100.00 | 0.00 | 19.44 |  |
| FC | 2 | 1 | 11 | 2 | 16 | 3 | 0 | 10 | 4 | 17 | 3 | 0 | 9 | 4 | 16 |
| Row \% | 12.50 | 6.25 | 68.75 | 12.50 |  | 17.65 | 0.00 | 58.82 | 23.53 |  | 18.75 | 0.00 | 56.25 | 25.00 |  |
| Col. \% | 5.56 | 5.26 | 84.62 | 6.25 |  | 7.14 | 0.00 | 90.91 | 12.12 |  | 7.69 | 0.00 | 81.82 | 11.11 |  |
| Other | 3 | 1 | 1 | 16 | 21 | 2 | 1 | 1 | 15 | 19 | 3 | 0 | 0 | 17 | 20 |
| Row \% | 14.29 | 4.76 | 4.76 | 76.19 |  | 10.53 | 5.26 | 5.26 | 78.95 |  | 15.00 | 0.00 | 0.00 | 85.00 |  |
| Col. \% | 8.33 | 5.26 | 7.69 | 50.00 |  | 4.76 | 6.67 | 9.09 | 45.45 |  | 7.69 | 0.00 | 0.00 | 47.22 |  |
| Total | 36 | 19 | 13 | 32 | 100 | 42 | 15 | 11 | 33 | 101 | 39 | 9 | 11 | 36 | 95 |
| Notes: $1 . \mathrm{E}=$ in paid employment, $\mathrm{U}=$ unemployed, $\mathrm{FC}=$ family care, Other = early retired, full-time student/school, long-term sick/disabled, and government training. <br> 2. Number of observations at wave $\mathrm{A}=115$. <br> 3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 . |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |

Table 5.4b Following Wave B Entitled Non-Recipients: From Employment Status Into Employment Status

| Wave B ENRs | Wave C |  |  |  |  | Wave D |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | E | U | FC | Other | Total | E | U | FC | Other | Total |
| E | 8 | 2 | 0 | 0 | 10 | 8 | 2 | 0 | 0 | 10 |
| Row \% | 80.00 | 20.00 | 0.00 | 0.00 |  | 80.00 | 20.00 | 0.00 | 0.00 |  |
| Col. \% | 30.77 | 9.52 | 0.00 | 0.00 |  | 23.53 | 13.33 | 0.00 | 0.00 |  |
| U | 14 | 17 | 0 | 5 | 36 | 19 | 11 | 0 | 5 | 35 |
| Row \% | 38.89 | 47.22 | 0.00 | 13.89 |  | 54.29 | 31.43 | 0.00 | 14.29 |  |
| Col. \% | 53.85 | 80.95 | 0.00 | 12.50 |  | 55.88 | 73.33 | 0.00 | 13.16 |  |
| FC | 1 | 2 | 13 | 4 | 20 | 5 | 1 | 7 | 5 | 18 |
| Row \% | 5.00 | 10.00 | 65.00 | 20.00 |  | 27.78 | 5.56 | 38.89 | 27.78 |  |
| Col. \% | 3.85 | 9.52 | 86.67 | 10.00 |  | 14.71 | 6.67 | 70.00 | 13.16 |  |
| Other | 3 | 0 | 2 | 31 | 36 | 2 | 1 | 3 | 28 | 34 |
| Row \% | 8.33 | 0.00 | 5.56 | 86.11 |  | 5.88 | 2.94 | 8.82 | 82.35 |  |
| Col. \% | 11.54 | 0.00 | 13.33 | 77.50 |  | 5.88 | 6.67 | 30.00 | 73.68 |  |
| Total | 26 | 21 | 15 | 40 | 102 | 34 | 15 | 10 | 38 | 97 |
| Notes: 1. $\mathrm{E}=$ in paid employment, $\mathrm{U}=$ unemployed, $\mathrm{FC}=$ family care, Other $=$ early retired, full-time student/school, long-term sick/disabled, and government training. <br> 2. Number of observations at wave $B=121$. <br> 3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 . |  |  |  |  |  |  |  |  |  |  |

Table 5.4c Following Wave C Entitled Non-Recipients: From Employment Status Into Employment Status

| Wave C | Wave $D$ |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | E | U | FC | Other | Total |
| E | 8 | 4 | 0 | 3 | 15 |
| Row \% | 53.33 | 26.67 | 0.00 | 20.00 |  |
| Col. \% | 34.78 | 20.00 | 0.00 | 9.09 |  |
| U | 7 | 11 | 1 | 2 | 21 |
| Row \% | 33.33 | 52.38 | 4.76 | 9.52 |  |
| Col. \% | 30.43 | 55.00 | 10.00 | 6.06 |  |
| FC | 1 | 3 | 8 | 1 | 13 |
| Row \% | 7.69 | 23.08 | 61.54 | 7.69 |  |
| Col. \% | 4.35 | 15.00 | 80.00 | 3.03 |  |
| Other | 7 | 2 | 1 | 27 | 37 |
| Row \% | 18.92 | 5.41 | 2.70 | 72.97 |  |
| Col. \% | 30.43 | 10.00 | 10.00 | 81.82 |  |
| Total | 23 | 20 | 10 | 33 | 86 |
| Notes: 1. E = in paid employment, U $=$ unemployed, FC $=$ family care, Ot |  |  |  |  |  |

Notes: $\quad 1 . \mathrm{E}=$ in paid employment, $\mathrm{U}=$ unemployed, $\mathrm{FC}=$ family care,$~ O t h e r=$ early retired, full-time student/school, long-term sick/disabled, and government training. 2. Number of observations at wave $\mathrm{C}=94$.
3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.5a Following Wave A Entitled Non-Recipients: By Employment Status Into Take-Up Status

| Wave A | Wave B |  |  |  | Wave C |  |  |  | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | ENR | ER | NE | Total | ENR | ER | NE | Total | ENR | ER | NE | Total |
| E | 2 | 2 | 19 | 23 | 3 | 0 | 20 | 23 | 2 | 1 | 20 | 23 |
| Row \% | 8.70 | 8.70 | 82.61 |  | 1304 | 0.00 | 86.96 |  | 8.70 | 4.35 | 86.96 |  |
| Col. \% | 10.00 | 18.18 | 22.62 |  | 37.50 | 0.00 | 20.83 |  | 50.00 | 10.00 | 19.80 |  |
| U | 5 | 5 | 41 | 51 | 1 | 7 | 43 | 51 | 0 | 6 | 45 | 51 |
| Row \% | 9.80 | 9.80 | 80.39 |  | 1.96 | 13.73 | 84.31 |  | 0.00 | 11.76 | 88.24 |  |
| Col. \% | 25.00 | 45.45 | 48.81 |  | 12.50 | 63.64 | 44.79 |  | 0.00 | 60.00 | 44.55 |  |
| FC | 6 | 3 | 11 | 20 | 2 | 2 | 16 | 20 | 1 | 2 | 17 | 20 |
| Row \% | 30.00 | 15.00 | 55.00 |  | 10.00 | 10.00 | 80.00 |  | 5.00 | 10.00 | 85.00 |  |
| Col. \% | 30.00 | 27.27 | 13.10 |  | 25.00 | 18.18 | 16.67 |  | 25.00 | 20.00 | 16.83 |  |
| Other | 7 | 1 | 13 | 21 | 2 | 2 | 17 | 21 | 1 | 1 | 19 | 21 |
| Row \% | 33.33 | 4.76 | 61.90 |  | 9.52 | 9.52 | 80.95 |  | 4.76 | 4.76 | 90.48 |  |
| Col. \% | 35.00 | 9.09 | 15.48 |  | 25.00 | 18.18 | 17.71 |  | 25.00 | 10.00 | 18.81 |  |
| Total | 20 | 11 | 84 | 115 | 8 | 11 | 96 | 115 | 4 | 10 | 101 | 115 |
| Notes: 1. $\mathrm{E}=$ in paid employment, $\mathrm{U}=$ unemployed, $\mathrm{FC}=$ family care, Other = early retired, full-time student/school, long-term sick/disabled, and government training. <br> 2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled. <br> 3. Number of observations at wave $A=115$. |  |  |  |  |  |  |  |  |  |  |  |  |

Table 5.5b Following Wave B Entitled Non-Recipients: By Employment Status Into Take-Up Status


Table 5.5c Following Wave C Entitled Non-Recipients: By Employment Status Into Take-Up Status

| Wave C ENRs | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total |
| E | 6 | 0 | 9 | 15 |
| Row \% | 40.00 | 0.00 | 60.00 |  |
| Col. \% | 20.69 | 0.00 | 16.98 |  |
| U | 5 | 4 | 16 | 25 |
| Row \% | 20.00 | 16.00 | 64.00 |  |
| Col. \% | 17.24 | 33.33 | 30.19 |  |
| FC | 4 | 5 | 7 | 16 |
| Row \% | 25.00 | 31.25 | 43.75 |  |
| Col. \% | 13.79 | 41.67 | 13.21 |  |
| Other | 14 | 3 | 21 | 38 |
| Row \% | 36.84 | 7.89 | 55.26 |  |
| Col. \% | 48.28 | 25.00 | 39.62 |  |
| Total | 29 | 12 | 53 | 94 |
| Notes: | 1. $\mathrm{E}=$ in paid employment, $\mathrm{U}=$ unemployed, $\mathrm{FC}=$ family care, Other = early retired, full-time student/school, long-term sick/disabled, and government training. <br> 2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled. <br> 3. Number of observations at wave $\mathrm{C}=94$. |  |  |  |

Table 5.6a Following Wave A Entitled Non-Recipients: From Current Financial Situation Into Current Financial Situation

| Wave A | Wave B |  |  |  |  |  |  | Wave C |  |  |  |  |  |  | Wave D |  |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | +2 | +1 | 0 | -1 | -2 | Tot. | Proxy | +2 | +1 | 0 | -1 | -2 | Tot. | Proxy | +2 | +1 | 0 | -1 | -2 | Tot. |
| +2 | 2 | 1 | 4 | 7 | 0 | 0 | 14 | 1 | 4 | 5 | 3 | 1 | 0 | 14 | 0 | 6 | 3 | 4 | 0 | 1 | 14 |
| Row \% | 14.29 | 7.14 | 28.57 | 50.00 | 0.00 | 0.00 |  | 7.14 | 28.57 | 35.71 | 21.43 | 7.14 | 0.00 |  | 0.00 | 42.86 | 21.43 | 28.57 | 0.00 | 7.14 |  |
| Col. \% | 50.00 | 9.09 | 19.05 | 15.91 | 0.00 | 0.00 |  | 14.29 | 26.67 | 15.15 | 12.00 | 7.69 | 0.00 |  | 0.00 | 30.00 | 18.75 | 11.76 | 0.00 | 20.00 |  |
| +1 | 0 | 6 | 9 | 4 | 3 | 0 | 22 | 3 | 5 | 9 | 2 | 3 | 0 | 22 | 3 | 6 | 6 | 4 | 0 | 0 | 19 |
| Row \% | 0.00 | 27.27 | 40.91 | 18.18 | 13.64 | 0.00 |  | 13.64 | 22.73 | 40.91 | 9.09 | 13.64 | 0.00 |  | 15.79 | 31.58 | 31.58 | 21.05 | 0.00 | 0.00 |  |
| Col. \% | 0.00 | 60.00 | 42.86 | 9.76 | 21.43 | 0.00 |  | 42.86 | 33.33 | 27.27 | 8.00 | 23.08 | 0.00 |  | 100.00 | 30.00 | 37.50 | 11.76 | 0.00 | 0.00 |  |
| 0 | 2 | 1 | 6 | 14 | 5 | 2 | 30 | 1 | 6 | 7 | 12 | 3 | 0 | 29 | 0 | 4 | 6 | 12 | 5 | 0 | 27 |
| Row \% | 6.67 | 3.33 | 20.00 | 46.67 | 16.67 | 6.67 |  | 3.45 | 20.69 | 24.14 | 41.38 | 10.34 | 0.00 |  | 0.00 | 14.81 | 22.22 | 44.44 | 18.52 | 0.00 |  |
| Col. \% | 50.00 | 10.00 | 28.57 | 34.15 | 35.71 | 25.00 |  | 14.29 | 40.00 | 21.21 | 48.00 | 23.08 | 0.00 |  | 0.00 | 20.00 | 37.50 | 35.29 | 50.00 | 0.00 |  |
| -1 | 0 | 2 | 1 | 9 | 2 | 4 | 18 | 1 | 0 | 7 | 4 | 4 | 2 | 18 | 0 | 2 | 1 | 8 | 4 | 1 | 16 |
| Row \% | 0.00 | 11.11 | 5.56 | 50.00 | 11.11 | 22.22 |  | 5.56 | 0.00 | 38.89 | 22.22 | 22.22 | 11.11 |  | 0.00 | 12.50 | 6.25 | 50.00 | 25.00 | 6.25 |  |
| Col. \% | 0.00 | 20.00 | 4.76 | 21.95 | 14.29 | 50.00 |  | 14.29 | 0.00 | 21.21 | 16.00 | 30.77 | 50.00 |  | 0.00 | 10.00 | 6.25 | 23.53 | 40.00 | 20.00 |  |
| -2 | 0 | 0 | 0 | 7 | 4 | 2 | 13 | 1 | 0 | 4 | 3 | 2 | 2 | 12 | 0 | 1 | 0 | 6 | 1 | 3 | 11 |
| Row \% | 0.00 | 0.00 | 0.00 | 53.85 | 30.77 | 15.38 |  | 8.33 | 0.00 | 33.33 | 25.00 | 16.67 | 16.67 |  | 0.00 | 9.09 | 0.00 | 54.55 | 9.09 | 27.27 |  |
| Col. \% | 0.00 | 0.00 | 0.00 | 17.07 | 28.57 | 25.00 |  | 14.29 | 0.00 | 12.12 | 12.00 | 15.38 | 50.00 |  | 0.00 | 5.00 | 0.00 | 17.65 | 10.00 | 60.00 |  |
| Total | 4 | 10 | 20 | 41 | 14 | 8 | 97 | 7 | 15 | 32 | 24 | 13 | 4 | 95 | 3 | 19 | 16 | 34 | 10 | 5 | 87 |

Notes: 1. Subjective categories are: $+2=$ living comfortably, $+1=$ doing allight, $0=$ just about getting by, $-1=$ finding it quite difficult, $-2=$ finding it very difficult.
2. Number of observations at wave $\mathrm{A}=115$.
3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.6b Following Wave B Entitled Non-Recipients: From Current Financial Situation Into Current Financial Situation


Table 5.6c Following Wave C Entitled Non-Recipients; From Current Financial Situation Into Current Financial Situation

| Wave B | Wave C |  |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | +2 | +1 | 0 | -1 | -2 | Tot. |
| +2 | 0 | 4 | 2 | 1 | 0 | 0 | 7 |
| Row \% | 0.00 | 57.14 | 28.57 | 14.29 | 0.00 | 0.00 |  |
| Col. \% | 0.00 | 40.00 | 14.29 | 3.03 | 0.00 | 0.00 |  |
| +1 | 0 | 3 | 4 | 2 | 1 | 0 | 10 |
| Row \% | 0.00 | 30.00 | 40.00 | 20.00 | 10.00 | 0.00 |  |
| Col. \% | 0.00 | 30.00 | 28.57 | 6.06 | 5.88 | 0.00 |  |
| 0 | 2 | 2 | 5 | 20 | 7 | 4 | 40 |
| Row \% | 5.00 | 5.00 | 12.50 | 50.00 | 17.50 | 10.00 |  |
| Col. \% | 100.00 | 20.00 | 35.71 | 60.61 | 41.18 | 50.00 |  |
| -1 | 0 | 1 | 1 | 7 | 6 | 1 | 16 |
| Row \% | 0.00 | 6.25 | 6.25 | 43.75 | 37.50 | 6.25 |  |
| Col. \% | 0.00 | 10.00 | 7.14 | 21.21 | 35.29 | 12.50 |  |
| -2 | 0 | 0 | 2 | 3 | 3 | 3 | 11 |
| Row \% | 0.00 | 0.00 | 18.18 | 27.27 | 27.27 | 27.27 |  |
| Col. \% | 0.00 | 0.00 | 14.29 | 9.09 | 17.65 | 37.50 |  |
| Total | 2 | 10 | 14 | 33 | 17 | 8 | 84 |
| Notes: | 1. Subjective categories are: +2 = living comfortably, +1= doin |  |  |  |  |  |  |

Notes: 1. Subjective categories are: $+2=$ living comfortably, $+1=$ doing allight, $0=$ just about getting by, $-1=$ finding it quite difficult, $-2=$ finding it very difficult. 2. Number of observations at wave $C=94$.
3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.7a Following Wave A Entitled Non-Recipients: From Current Financial Situation Into Take-Up Status

| Wave A | Wave B |  |  |  | Wave C |  |  |  | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | ENR | ER | NE | Total | ENR | ER | NE | Total | ENR | ER | NE | Total |
| +2 | 2 | 0 | 13 | 15 | 0 | 0 | 15 | 15 | 0 | 1 | 14 | 15 |
| Row \% | 13.33 | 0.00 | 86.67 |  | 0.00 | 0.00 | 100.00 |  | 0.00 | 6.67 | 93.33 |  |
| Col. \% | 10.00 | 0.00 | 15.48 |  | 0.00 | 0.00 | 15.63 |  | 0.00 | 10.00 | 13.86 |  |
| +1 | 7 | 4 | 14 | 25 | 2 | 4 | 19 | 25 | 1 | 3 | 21 | 25 |
| Row \% | 28.00 | 16.00 | 56.00 |  | 8.00 | 16.00 | 76.00 |  | 4.00 | 12.00 | 84.00 |  |
| Col. \% | 35.00 | 36.36 | 16.67 |  | 25.00 | 36.36 | 19.79 |  | 25.00 | 30.00 | 20.79 |  |
| 0 | 8 | 2 | 24 | 34 | 4 | 2 | 28 | 34 | 3 | 2 | 29 | 34 |
| Row \% | 23.53 | 5.88 | 70.59 |  | 11.76 | 5.88 | 82.35 |  | 8.82 | 5.88 | 85.29 |  |
| Col. \% | 40.00 | 18.18 | 28.57 |  | 50.00 | 18.18 | 29.17 |  | 75.00 | 20.00 | 28.71 |  |
| -1 | 1 | 2 | 18 | 21 | 1 | 3 | 17 | 21 | 0 | 1 | 20 | 21 |
| Row \% | 4.76 | 9.52 | 85.71 |  | 4.76 | 14.29 | 80.95 |  | 0.00 | 4.76 | 95.24 |  |
| Col. \% | 5.00 | 18.18 | 21.43 |  | 12.50 | 27.27 | 17.71 |  | 0.00 | 10.00 | 19.80 |  |
| -2 | 2 | 3 | 12 | 17 | $\because 1$ | 2 | 14 | 17 | 0 | 3 | 14 | 17 |
| Row \% | 11.76 | 17.65 | 70.56 |  | 5.88 | 11.76 | 82.35 |  | 0.00 | 17.65 | 82.35 |  |
| Col. \% | 10.00 | 27.27 | 14.29 |  | 12.50 | 18.18 | 14.58 |  | 0.00 | 30.00 | 13.86 |  |
| Total | 20 | 11 | 81 | 112 | 8 | 11 | 93 | 112 | 4 | 10 | 98 | 112 |
| Notes: $\quad$. Subjective categories are: $+2=$ living comfortably, $+1=$ doing allright, $0=$ just about getting by, $-1=$ finding it quite difficult, $-2=$ finding it very difficult. <br> 2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled. <br> 3. Number of observations at wave $A=115$. <br> 4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 . |  |  |  |  |  |  |  |  |  |  |  |  |

Table 5.7b Following Wave B Entitled Non-Recipients: From Current Financial Situation Into Take-Up Status

| Wave B | Wave C |  |  | Wave $\boldsymbol{D}$ |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total | ENR | ER | NE | Total |
| +2 | 6 | 1 | 7 | 14 | 2 | 0 | 12 | 14 |
| Row \% | 42.86 | 7.14 | 50.00 |  | 14.29 | 0.00 | 85.71 |  |
| Col. \% | 35.29 | 4.76 | 8.43 |  | 15.38 | 0.00 | 13.04 |  |
| +1 | 2 | 1 | 23 | 26 | 0 | 5 | 21 | 26 |
| Row \% | 7.69 | 3.85 | 88.46 |  | 0.00 | 19.23 | 80.77 |  |
| Col. \% | 11.76 | 4.76 | 27.71 |  | 0.00 | 31.25 | 22.83 |  |
| 0 | 7 | 12 | 24 | 43 | 8 | 5 | 30 | 43 |
| Row \% | 16.28 | 27.91 | 55.81 |  | 18.60 | 11.63 | 69.77 |  |
| Col. \% | 41.18 | 57.14 | 28.92 |  | 61.54 | 31.25 | 32.61 |  |
| -1 | 0 | 4 | 17 | 21 | 2 | 1 | 18 | 21 |
| Row \% | 0.00 | 19.05 | 80.95 |  | 9.52 | 4.76 | 85.71 |  |
| Col. \% | 0.00 | 19.05 | 20.48 |  | 15.38 | 6.25 | 19.57 |  |
| -2 | 2 | 3 | 12 | 17 | 1 | 5 | 11 | 17 |
| Row \% | 11.76 | 17.65 | 70.59 |  | 5.88 | 29.41 | 64.71 |  |
| Col. \% | 11.76 | 14.29 | 14.46 |  | 7.69 | 31.25 | 11.96 |  |
| Total | 17 | 21 | 83 | 121 | 13 | 16 | 92 | 121 |

Notes: 1. Subjective categories are: $+2=$ living comfortably, $+1=$ doing allight, $0=$ just about getting by, $-1=$ finding it quite difficult, $-2=$ finding it very difficult. 2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.
3. Number of observations at wave $B=121$

Table 5.7c Following Wave C Entitled Non-Recipients: From Current Financial Situation Into Take-Up Status

| Wave C ENRs | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total |
| +2 | 3 | 1 | 4 | 8 |
| Row\% | 37.50 | 12.50 | 50.00 |  |
| Col. \% | 10.34 | 8.33 | 7.55 |  |
| +1 | 3 | 2 | 7 | 12 |
| Row \% | 25.00 | 16.67 | 58.33 |  |
| Col. \% | 10.34 | 16.67 | 13.21 |  |
| 0 | 17 | 2 | 24 | 43 |
| Row \% | 39.53 | 4.65 | 55.81 |  |
| Col. \% | 58.62 | 16.67 | 45.28 |  |
| -1 | 4 | 4 | 10 | 18 |
| Row \% | 22.22 | 22.22 | 55.56 |  |
| Col. \% | 13.79 | 33.33 | 18.87 |  |
| -2 | 2 | 3 | 7 | 12 |
| Row \% | 16.67 | 25.00 | 58.33 |  |
| Col. \% | 6.90 | 25.00 | 13.21 |  |
| Total | 29 | 12 | 52 | 93 |
| Notes: 1. Subjective categories are: $+2=$ living comfortably, $+1=$ doing allright, $0=$ just about getting by, $-1=$ finding it quite difficult, $-2=$ finding it very difficult. <br> 2. $E N R=$ entitled non-recipients, $E R=$ entitled recipients, $\mathrm{NE}=$ not entitled. <br> 3. Number of observations at wave $\mathrm{C}=94$. <br> 4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 . |  |  |  |  |

Table 5.8a Following Wave A Entitled Non-Recipients: From Past Financial Situation Into Past Financial Situation

| Wave A | Wave B |  |  |  |  | Wave C |  |  |  |  | Wave D |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | $+$ | - | 0 | Total | Proxy | + | - | 0 | Total | Proxy | + | - | 0 | Total |
| + | 3 | 5 | 7 | 5 | 20 | 0 | 11 | 5 | 5 | 21 | 0 | 7 | 7 | 5 | 19 |
| Row \% | 15.00 | 25.00 | 35.00 | 25.00 |  | 0.00 | 52.38 | 23.81 | 23.81 |  | 0.00 | 36.84 | 36.84 | 26.32 |  |
| Col. \% | 75.00 | 26.32 | 17.95 | 13.89 |  | 0.00 | 36.67 | 17.86 | 15.63 |  | 0.00 | 30.43 | 21.21 | 17.24 |  |
| - | 0 | 8 | 20 | 16 | 44 | 2 | 13 | 14 | 14 | 42 | 0 | 11 | 15 | 12 | 38 |
| Row \% | (1).00) | 18.18 | 45.45 | 36.36 |  | 4.76 | 30.95 | 33.33 | 30.95 |  | 0.00 | 28.95 | 39.47 | 31.58 |  |
| (\%) \% | 0.00 | 42.11 | 51.28 | 44.44 |  | 28.57 | 43.33 | 50.00 | 40.63 |  | 0.00 | 47.83 | 45.45 | 41.38 |  |
| 0 | 1 | 6 | 12 | 15 | 34 | 5 | 6 | 9 | 13 | 33 | 3 | 5 | 11 | 12 | 31 |
| Row \% | 2.94 | 17.65 | 35.29 | 44.12 |  | 15.15 | 18.18 | 27.27 | 39.39 |  | 9.68 | 16.13 | 35.48 | 38.71 |  |
| Col. \% | 25.00 | 31.58 | 30.77 | 41.67 |  | 71.43 | 20.00 | 32.14 | 40.63 |  | 100.00 | 21.74 | 33.33 | 41.38 |  |
| Total | 4 | 19 | 39 | 36 | 98 | 7 | 30 | 28 | 32 | 97 | 3 | 23 | 33 | 29 | 88 |

Notes: 1. Subjective categories for financial situation compared to last year: $+=$ better off, $-=$ worse off, $0=$ about the same.
2. Number of observations at wave $A=115$.
3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.8b Following Wave B Entitled Non-Recipients: From Past Financial Situation Into Past Financial Situation

| Wave B ENRs | Wave C |  |  |  |  | Wave D |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Proxy | $+$ | - | 0 | Total | Proxy | + | - | 0 | Total |
| + | 0 | 4 | 4 | 4 | 12 | 0 | 3 | 6 | 3 | 12 |
| Row \% | 0.00 | 33.33 | 33.33 | 33.33 |  | 0.00 | 25.00 | 50.00 | 25.00 |  |
| Col. \% | 0.00 | 16.00 | 11.43 | 12.50 |  | 0.00 | 15.00 | 19.35 | 8.11 |  |
| - | 3 | 12 | 23 | 8 | 46 | 4 | 10 | 17 | 12 | 43 |
| Row \% | 6.52 | 26.09 | 50.00 | 17.39 |  | 9.09 | 22.73 | 38.64 | 27.27 |  |
| Col. \% | 50.00 | 48.00 | 65.71 | 25.00 |  | 100.00 | 50.00 | 54.84 | 32.43 |  |
| 0 | 3 | 9 | 8 | 20 | 40 | 0 | 7 | 8 | 22 | 37 |
| Row \% | 7.50 | 22.50 | 20.00 | 50.00 |  | 0.00 | 18.92 | 21.62 | 59.64 |  |
| Col. \% | 50.00 | 36.00 | 22.86 | 62.50 |  | 0.00 | 35.00 | 25.81 | 59.46 |  |
| Total | 6 | 25 | 35 | 32 | 98 | 4 | 20 | 31 | 37 | 92 |
| Notes: | 1. Subjective categories for financial situation <br> 2. Number of observations at wave $\mathrm{B}=121$. <br> 3. Total number of observations decline from |  |  |  | pared to <br> to wave | year: + = <br> to missi | ter off, espons | orse of <br> ence ro | about | percent |

Table 5.8c Following Wave C Entitled Non-Recipients: From Past Financial Situation Into Past Financial Situation


Table 5.9a Following Wave A Entitled Non-Recipients: From Past Financial Situation Into Take-Up Status

| Wave A ENRs | Wave B |  |  |  | Wave C |  |  |  | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total | ENR | ER | NE | Total | ENR | ER | NE | Total |
| + | 3 | 2 | 19 | 24 | 1 | 3 | 20 | 24 | 0 | 1 | 23 | 24 |
| Row \% | 12.50 | 8.33 | 79.17 |  | 4.17 | 12.50 | 83.33 |  | 0.00 | 4.17 | 95.83 |  |
| Col. \% | 15.00 | 18.18 | 22.62 |  | 12.50 | 27.27 | 20.83 |  | 0.00 | 10.00 | 22.77 |  |
| - | 8 | 6 | 35 | 49 | 1 | 6 | 42 | 49 | 1 | 5 | 43 | 49 |
| Row \% | 16.33 | 12.24 | 71.43 |  | 2.04 | 12.24 | 85.71 |  | 2.04 | 10.20 | 87.76 |  |
| Col. \% | 40.00 | 54.55 | 41.67 |  | 12.50 | 54.55 | 43.75 |  | 25.00 | 50.00 | 42.57 |  |
| 0 | 9 | 3 | 28 | 40 | 6 | 2 | 32 | 40 | 3 | 4 | 33 | 40 |
| Row \% | 22.50 | 7.50 | 70.00 |  | 15.00 | 5.00 | 80.00 |  | 7.50 | 10.00 | 82.50 |  |
| Col. \% | 45.00 | 27.27 | 33.33 |  | 75.00 | 18.18 | 33.33 |  | 75.00 | 40.00 | 32.67 |  |
| Total | 20 | 11 | 82 | 113 | 8 | 11 | 94 | 113 | 4 | 10 | 99 | 113 |

Notes: 1. Subjective categories for financial situation compared to last year: $+=$ better off, $-=$ worse off, $0=$ about the same.
2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.
3. Number of observations at wave $A=115$.
4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.9b Following Wave B Entitled Non-Recipients: From Past Financial Situation Into Take-Up Status

| Wave B <br> ENR | Wave C |  |  |  | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total | ENR | ER | NE | Total |
| + | 4 | 1 | 9 | 14 | 0 | 3 | 11 | 14 |
| Row \% | 28.57 | 7.14 | 64.29 |  | 0.00 | 21.43 | 78.57 |  |
| Col. \% | 23.53 | 4.76 | 10.84 |  | 0.00 | 18.75 | 11.96 |  |
| - | 4 | 6 | 48 | 58 | 3 | 7 | 48 | 58 |
| Row \% | 6.90 | 10.34 | 82.76 |  | 5.17 | 12.07 | 82.76 |  |
| Col. \% | 23.53 | 28.57 | 57.83 |  | 23.08 | 43.75 | 52.17 |  |
| 0 | 9 | 14 | 26 | 49 | 10 | 6 | 33 | 49 |
| Row\% | 18.37 | 28.57 | 53.06 |  | 20.41 | 12.24 | 67.35 |  |
| Col. \% | 52.94 | 66.67 | 31.33 |  | 76.92 | 37.50 | 35.87 |  |
| Total | 17 | 21 | 83 | 121 | 13 | 16 | 92 | 121 |

Notes: 1. Subjective categories for financial situation compared to last year: $+=$ better off, $-=$ worse off, $0=$ about the same.
2. $E N R=$ entitled non-recipients, $E R=$ entitled recipients, $N E=$ not entitled
3. Number of observations at wave $B=121$
4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.9c Following Wave C Entitled Non-Recipients: From Past Financial Situation Into Take-Up Status

| Wave C <br> ENRs | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: |
|  | ENR | ER | NE | Total |
| + | 6 | 1 | 9 | 16 |
| Row \% | 37.50 | 6.25 | 56.25 |  |
| Col. \% | 20.69 | 8.33 | 16.98 |  |
| - | 14 | 5 | 23 | 42 |
| Row \% | 33.33 | 11.90 | 54.76 |  |
| Col. \% | 48.28 | 41.67 | 43.40 |  |
| 0 | 9 | 6 | 20 | 35 |
| Row \% | 25.71 | 17.14 | 57.14 |  |
| Col. \% | 31.03 | 50.00 | 37.74 |  |
| Total | 29 | 12 | 52 | 93 |
| Notes: 1. Subjective categories for financial situation compared to last year: + = |  |  |  |  |

[^126]Table 5.10a Following Wave A Entitled Non-Recipients: From Future Financial Situation Into Future Financial Situation

| Wave | Wave B |  |  |  |  |  | Wave C |  |  |  |  |  | Wave D |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | Don't know | + | - | 0 | Total | Proxy | Don't know | + | - | 0 | Total | Proxy | Don't know | + | - | 0 | Total |
| $+$ | 1 | 2 | 19 | 4 | 8 | 34 | 2 | 3 | 19 | 5 | 5 | 34 | 1 | 3 | 11 | 7 | 7 | 29 |
| Row \% | 2.94 | 5.88 | 55.88 | 11.76 | 23.53 |  | 5.88 | 8.82 | 55.88 | 14.71 | 14.71 |  | 3.45 | 10.34 | 37.93 | 24.14 | 24.14 |  |
| Col. \% | 25.00 | 18.18 | 70.37 | 33.33 | 18.18 |  | 28.57 | 23.08 | 59.38 | 38.46 | 15.63 |  | 33.33 | 30.00 | 40.74 | 63.64 | 18.92 |  |
| - | 1 | 1 | 4 | 3 | 12 | 21 | 0 | 4 | 5 | 5 | 7 | 21 | 0 | 2 | 7 | 2 | 10 | 21 |
| Row \% | 4.55 | 4.55 | 18.18 | 13.64 | 59.09 |  | 0.00 | 19.05 | 23.81 | 23.81 | 33.33 |  | 0.00 | 9.52 | 33.33 | 9.52 | 47.62 |  |
| Col. \% | 25.00 | 9.09 | 14.81 | 25.00 | 29.55 |  | 0.00 | 30.77 | 15.63 | 38.46 | 21.88 |  | 0.00 | 20.00 | 25.93 | 18.18 | 27.03 |  |
| 0 | 2 | 6 | 2 | 5 | 21 | 36 | 4 | 3 | 6 | 3 | 17 | 33 | 1 | 3 | 8 | 2 | 18 | 32 |
| Row \% | 5.56 | 16.67 | 5.56 | 13.89 | 58.33 |  | 12.12 | 9.09 | 18.18 | 9.09 | 51.52 |  | 3.13 | 9.38 | 25.00 | 6.25 | 56.25 |  |
| $\mathrm{Col} . \%$ | 50.00 | 54.55 | 7.41 | 41.67 | 47.73 |  | 57.14 | 23.08 | 18.75 | 23.08 | 53.13 |  | 33.33 | 30.00 | 29.63 | 18.18 | 48.65 |  |
| Total | 4 | 9 | 25 | 12 | 41 | 91 | 6 | 10 | 30 | 13 | 29 | 88 | 2 | 8 | 26 | 11 | 35 | 82 |
| Notes: | 1. Subjective categories for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same. <br> 2. Number of observations at wave $A=115$. <br> 3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 . |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |

Table 5.10b Following Wave B Entitled Non-Recipients: From Future Financial Situation Into Future Financial Situation

| Wave | Wave C |  |  |  |  |  | Wave D |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | $\begin{aligned} & \text { Don't } \\ & \text { know } \end{aligned}$ | + | - | 0 | Total | Proxy | Don't know | + | - | 0 | Total |
| + | 1 | 2 | 12 | 2 | 4 | 21 | 2 | 2 | 11 | 1 | 3 | 19 |
| Row \% | 4.76 | 9.52 | 57.14 | 9.52 | 19.05 |  | 10.53 | 10.53 | 57.89 | 5.26 | 15.79 |  |
| Col. \% | 16.67 | 15.38 | 42.86 | 15.38 | 10.53 |  | 50.00 | 25.00 | 40.74 | 8.33 | 7.14 |  |
| - | 1 | 3 | 3 | 8 | 8 | 23 | 2 | 0 | 6 | 8 | 7 | 23 |
| Row \% | 4.35 | 13.04 | 13.04 | 34.78 | 34.78 |  | 8.70 | 0.00 | 26.09 | 34.78 | 30.43 |  |
| Col. \% | 16.67 | 23.08 | 10.71 | 61.54 | 21.05 |  | 50.00 | 0.00 | 22.22 | 66.67 | 16.67 |  |
| 0 | 2 | 2 | 10 | 3 | 25 | 42 | 0 | 1 | 9 | 3 | 27 | 40 |
| Row \% | 4.76 | 4.76 | 23.81 | 7.14 | 59.52 |  | 0.00 | 2.50 | 22.50 | 7.50 | 67.50 |  |
| Col. \% | 33.33 | 15.38 | 35.71 | 23.08 | 65.79 |  | 0.00 | 12.50 | 33.33 | 25.00 | 64.29 |  |
| Total | 4 | 7 | 25 | 13 | 37 | 86 | 4 | 3 | 26 | 12 | 37 | 82 |
| Notes: | 1. Subjective categorics for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same. <br> 2. Number of observations at wave $\mathrm{B}=121$. <br> 3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column pe |  |  |  |  |  |  |  |  |  |  |  |

Table 5.10c Following Wave C Entitled Non-Recipients: From Future Financial Situation Into Future Financial Situation

| Wave | Wave D |  |  |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | Proxy | Don't know | + | - | 0 | Total |
| + | 1 | 2 | 19 | 4 | 9 | 35 |
| Row \% | 2.86 | 5.71 | 54.29 | 11.43 | 25.71 |  |
| Col. \% | 50.00 | 25.00 | 70.37 | 30.77 | 26.47 |  |
| - | 0 | 1 | 1 | 6 | 6 | 14 |
| Row \% | 0.00 | 7.14 | 7.14 | 42.86 | 42.86 |  |
| Col. \% | 0.00 | 12.50 | 3.70 | 46.15 | 17.65 |  |
| 0 | 1 | 4 | 6 | 3 | 16 | 30 |
| Row \% | 3.33 | 13.33 | 20.00 | 10.00 | 53.33 |  |
| Col. \% | 50.00 | 50.00 | 22.22 | 23.08 | 47.06 |  |
| Total | 2 | 7 | 26 | 13 | 31 | 79 |
| Notes: | 1. Subjective categories for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same. <br> 2. Number of observations at wave $\mathrm{C}=94$. |  |  |  |  |  |

Table 5.11a Following Wave A Entitled Non-Recipients: From Future Financial Situation Into Take-Up Status

| Wave A |  | Wave B |  |  |  | Wave |  |  |  | Wave |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | ENR | ER | NE | Total | ENR | ER | NE | Total | ENR | ER | NE | Total |
| + | 4 | 3 | 33 | 40 | 1 | 6 | 33 | 40 | 0 | 4 | 36 | 40 |
| Row \% | 10.00 | 7.50 | 82.50 |  | 2.50 | 15.00 | 82.50 |  | 0.00 | 10.00 | 90.00 |  |
| Col. \% | 20.00 | 27.27 | 39.29 |  | 12.50 | 54.55 | 34.38 |  | 0.00 | 40.00 | 35.64 |  |
| - | 4 | 4 | 16 | 24 | 0 | 3 | 21 | 24 | 1 | 2 | 21 | 24 |
| Row \% | 16.67 | 16.67 | 66.67 |  | 0.00 | 12.50 | 87.50 |  | 4.17 | 8.33 | 87.50 |  |
| Col. \% | 20.00 | 36.36 | 19.05 |  | 0.00 | 27.27 | 21.8 |  | 25.00 | 20.00 | 20.79 |  |
| 0 | 12 | 3 | 24 | 39 | 6 | 1 | 32 | 39 | 3 | 2 | 34 | 39 |
| Row \% | 30.77 | 7.69 | 61.54 |  | 15.38 | 2.56 | 82.05 |  | 7.69 | 5.13 | 87.18 |  |
| Col. \% | 60.00 | 27.27 | 28.57 |  | 75.00 | 9.09 | 33.33 |  | 75.00 | 20.00 | 33.66 |  |
| Total | 20 | 10 | 73 | 103 | 7 | 10 | 86 | 103 | 4 | 8 | 91 | 103 |
| Notes: | 1. Subjective categories for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same. <br> 2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled. <br> 3. Number of observations at wave $A=115$. <br> 4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to |  |  |  |  |  |  |  |  |  |  |  |

Table 5.11b Following Wave B Entitled Non-Recipients: From Future Financial Situation Into Take-Up Status

| Wave B | Wave C |  |  |  | Wave D |  |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
| ENRs | ENR | ER | NE | Total | ENR | ER | NE | Total |
| + | 2 | 4 | 21 | 27 | 0 | 6 | 21 | 27 |
| Row \% | 7.41 | 14.81 | 77.78 |  | 0.00 | 22.22 | 77.78 |  |
| Col. \% | 11.76 | 19.05 | 25.30 |  | 0.00 | 37.50 | 22.83 |  |
| - | 4 | 4 | 18 | 26 | 2 | 3 | 21 | 26 |
| Row \% | 15.38 | 15.38 | 69.23 |  | 7.69 | 11.54 | 80.77 |  |
| Col. \% | 23.53 | 19.05 | 21.69 |  | 15.38 | 18.75 | 22.83 |  |
| 0 | 8 | 9 | 37 | 54 | 9 | 6 | 39 | 54 |
| Row \% | 14.81 | 16.67 | 68.52 |  | 16.67 | 11.11 | 72.22 |  |
| $\mathrm{Col} . \%$ | 47.06 | 42.86 | 44.58 |  | 69.23 | 37.50 | 42.39 |  |
| Total | 14 | 17 | 76 | 107 | 11 | 15 | 81 | 107 |

Notes: 1. Subjective categories for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same.
2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.
3. Number of observations at wave $B=121$.
4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.11c Following Wave C Entitled Non-Recipients: From Future Financial Situation Into Take-Up Status

| Wave $A$ | Wave $\boldsymbol{B}$ |  |  |  |
| :---: | :---: | :---: | :---: | :---: |
| ENRs | ENR | ER | NE | Total |
|  |  | 7 | 4 | 28 |
| Row \% | 17.95 | 10.26 | 71.79 | 39 |
| Col. \% | 24.14 | 33.33 | 52.83 |  |
|  |  |  |  |  |
| - | 5 | 2 | 9 | 16 |
| Row \% | 31.25 | 12.50 | 56.25 |  |
| Col. \% | 17.24 | 16.67 | 16.98 |  |
| 0 | 17 | 4 | 11 | 32 |
| Row \% | 53.13 | 12.50 | 34.38 |  |
| Col. \% | 58.62 | 33.33 | 20.75 |  |
| Total | 29 | 10 | 48 | 87 |

Notes: 1. Subjective categories for expected financial situation next year: $+=$ better off, $-=$ worse off, $0=$ about the same.
2. $\mathrm{ENR}=$ entitled non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled
3. Number of observations at wave $\mathrm{C}=94$.
4. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

Table 5.12 Descriptive Statistics for Panel Data

|  | Mean | Std. Dev. | Minimum | Maximum |
| :--- | :---: | :---: | :---: | :---: |
| y | 0.784 | 0.412 | 0 | 1 |
| log $I S+$ | 3.676 | 0.986 | -2.303 | 6.119 |
| $I S \dagger$ | 39.49 | 2.68 | 0.10 | 454.41 |
| income $\dagger$ | 16.21 | 26.42 | 0 | 185.00 |
| age | 34.41 | 13.26 | 18 | 65 |
| lonepar | 0.279 | 0.449 | 0 | 1 |
| no. kids | 0.801 | 1.161 | 0 | 1 |
| female | 0.535 | 0.499 | 0 | 1 |
| couple | 0.228 | 0.420 | 0 | 1 |
| head | 0.650 | 0.477 | 0 | 1 |
| degree | 0.048 | 0.215 | 0 | 1 |
| owner | 0.101 | 0.301 | 0 | 1 |
| tenant | 0.491 | 0.500 | 0 | 1 |
| U | 0.481 | 0.500 | 0 | 1 |
| weeks $U$ | 16.93 | 22.19 | 0 | 1 |
| sick | 0.025 | 0.158 | 0 | 1 |
| subject 1 | 0.475 | 0.370 | 0 | 169 |
| subject 2 |  |  | 0 | 0 |

$\dagger$ Measured in $£ / \mathrm{wk}$.
Note: Number of observations in sample $=1,965$.

Table 5.13 Probit Estimates of Take-Up Using Panel Data


Table 5.14 Testing for State Dependence in Take-Up

|  | Standard Pooled Probit | Random Effects Probit | State Dependence Probit I $\dagger$ | State Dependence Probit II $\dagger$ | State Dependence Probit III $\dagger$ |
| :---: | :---: | :---: | :---: | :---: | :---: |
| intercept | $\begin{gathered} -0.260 \\ (0.418) \end{gathered}$ | $\begin{gathered} -0.059 \\ (0.649) \end{gathered}$ | $\begin{gathered} -0.128 \\ (0.478) \end{gathered}$ | $\begin{gathered} -0.128 \\ (0.443) \end{gathered}$ | $\begin{gathered} \hline-0.214 \\ (2.677) \end{gathered}$ |
| lagged y | - | - | $\begin{gathered} 0.0003^{* *} \\ (1.5 \mathrm{e}-4) \end{gathered}$ | $\begin{gathered} 0.0003^{* *} \\ (1.8 \mathrm{e}-4) \end{gathered}$ | - |
| $\log I S$ | $\begin{aligned} & 0.335^{*} \\ & (0.074) \end{aligned}$ | $\begin{aligned} & 0.461^{*} \\ & (0.125) \end{aligned}$ | $\begin{aligned} & 0.342^{*} \\ & (0.075) \end{aligned}$ | $\begin{aligned} & 0.342^{*} \\ & (0.084) \end{aligned}$ | $\begin{aligned} & 0.339^{*} \\ & (0.116) \end{aligned}$ |
| income | $\begin{aligned} & -0.710^{*} \\ & (0.139) \end{aligned}$ | $\begin{aligned} & -1.092^{*} \\ & (0.197) \end{aligned}$ | $\begin{gathered} -0.691^{*} \\ (0.140) \end{gathered}$ | $\begin{gathered} -0.691^{*} \\ (0.136) \end{gathered}$ | $\begin{gathered} -0.771^{*} \\ (0.215) \end{gathered}$ |
| age | $\begin{gathered} -0.100 \\ (0.062) \end{gathered}$ | $\begin{gathered} -0.164 \\ (0.103) \end{gathered}$ | $\begin{gathered} -0.106^{* *} \\ (0.062) \end{gathered}$ | $\begin{gathered} -0.106^{* *} \\ (0.059) \end{gathered}$ | $\begin{gathered} -0.257 \\ (0.289) \end{gathered}$ |
| lonepar | $\begin{aligned} & 0.532^{*} \\ & (0.236) \end{aligned}$ | $\begin{aligned} & 0.641^{* *} \\ & (0.370) \end{aligned}$ | $\begin{aligned} & 0.521^{*} \\ & (0.239) \end{aligned}$ | $\begin{aligned} & 0.521^{*} \\ & (0.233) \end{aligned}$ | $\begin{gathered} 0.208 \\ (0.496) \end{gathered}$ |
| no. kids | $\begin{gathered} 0.084 \\ (0.081) \end{gathered}$ | $\begin{gathered} 0.125 \\ (0.132) \end{gathered}$ | $\begin{gathered} 0.085 \\ (0.082) \end{gathered}$ | $\begin{gathered} 0.085 \\ (0.084) \end{gathered}$ | $\begin{gathered} -0.006 \\ (0.250) \end{gathered}$ |
| female | $\begin{gathered} 0.135 \\ (0.166) \end{gathered}$ | $\begin{gathered} 0.147 \\ (0.283) \end{gathered}$ | $\begin{gathered} 0.112 \\ (0.166) \end{gathered}$ | $\begin{gathered} 0.112 \\ (0.166) \end{gathered}$ | $\begin{gathered} 0.270 \\ (0.704) \end{gathered}$ |
| couple | $\begin{gathered} -0.031 \\ (0.231) \end{gathered}$ | $\begin{gathered} -0.129 \\ (0.432) \end{gathered}$ | $\begin{gathered} -0.030 \\ (0.233) \end{gathered}$ | $\begin{gathered} -0.030 \\ (0.254) \end{gathered}$ | $\begin{gathered} -0.249 \\ (0.579) \end{gathered}$ |
| head | $\begin{gathered} 0.101 \\ (0.173) \end{gathered}$ | $\begin{gathered} 0.214 \\ (0.288) \end{gathered}$ | $\begin{gathered} 0.100 \\ (0.173) \end{gathered}$ | $\begin{gathered} 0.100 \\ (0.168) \end{gathered}$ | $\begin{gathered} -0.231 \\ (0.398) \end{gathered}$ |
| degree | $\begin{gathered} 0.209 \\ (0.499) \end{gathered}$ | $\begin{gathered} 0.076 \\ (0.885) \end{gathered}$ | $\begin{gathered} 0.262 \\ (0.503) \end{gathered}$ | $\begin{gathered} 0.262 \\ (0.735) \end{gathered}$ | $\begin{gathered} 0.810 \\ (1.254) \end{gathered}$ |
| owner | $\begin{gathered} 0.016 \\ (0.239) \end{gathered}$ | $\begin{gathered} 0.014 \\ (0.387) \end{gathered}$ | $\begin{gathered} 0.039 \\ (0.241) \end{gathered}$ | $\begin{gathered} 0.039 \\ (0.245) \end{gathered}$ | $\begin{gathered} 0.069 \\ (0.571) \end{gathered}$ |
| tenant | $\begin{aligned} & 0.654^{*} \\ & (0.157) \end{aligned}$ | $\begin{aligned} & 0.944^{*} \\ & (0.321) \end{aligned}$ | $\begin{aligned} & 0.631^{*} \\ & (0.159) \end{aligned}$ | $\begin{aligned} & 0.631^{*} \\ & (0.187) \end{aligned}$ | $\begin{aligned} & 0.798^{* *} \\ & (0.472) \end{aligned}$ |
| $U$ | $\begin{aligned} & 0.674^{*} \\ & (0.183) \end{aligned}$ | $\begin{aligned} & 0.881^{*} \\ & (0.343) \end{aligned}$ | $\begin{aligned} & 0.663^{*} \\ & (0.183) \end{aligned}$ | $\begin{aligned} & 0.663^{*} \\ & (0.246) \end{aligned}$ | $\begin{gathered} 0.711^{* *} \\ (0.413) \end{gathered}$ |
| weeks $U$ | $\begin{gathered} -0.0005 \\ (0.0015) \end{gathered}$ | $\begin{gathered} -0.0008 \\ (0.0060) \end{gathered}$ | $\begin{gathered} -0.0005 \\ (0.0015) \end{gathered}$ | $\begin{gathered} -0.0005 \\ (0.0045) \end{gathered}$ | $\begin{gathered} -0.0007 \\ (0.0076) \end{gathered}$ |
| sick | $\begin{gathered} -0.930^{* *} \\ (0.534) \end{gathered}$ | $\begin{gathered} -1.290 \\ (0.938) \end{gathered}$ | $\begin{gathered} -0.890^{* *} \\ (0.533) \end{gathered}$ | $\begin{gathered} -0.890^{* *} \\ (0.611) \end{gathered}$ | $\begin{gathered} -0.680 \\ (0.721) \end{gathered}$ |
| subject I | $\begin{aligned} & -0.255^{* *} \\ & \cdot(0.142) \end{aligned}$ | $\begin{gathered} -0.332 \\ (0.236) \end{gathered}$ | $\begin{gathered} -0.242^{* *} \\ (0.143) \end{gathered}$ | $\begin{gathered} -0.242^{* *} \\ (0.166) \end{gathered}$ | (0.238) |
| subject 2 | $\begin{gathered} -0.027 \\ (0.180) \end{gathered}$ | $\begin{gathered} -0.737 \\ (0.312) \end{gathered}$ | $\begin{aligned} & -0.045 \\ & (0.182) \end{aligned}$ | $\begin{gathered} -0.045 \\ (0.212) \end{gathered}$ | (0.321) <br> Yes |
| lagged $\mathbf{x}$ 's | - | - | - |  |  |
| $\rho$ | - | $\begin{aligned} & 0.569^{*} \\ & (0.219) \end{aligned}$ | - | $\begin{gathered} 0.043 \\ (0.256) \end{gathered}$ | $\begin{gathered} 0.043 \\ (0.454) \end{gathered}$ |
| no. obs. | 767 | 767 | 767 -2236 | 767 -222.4 | 767 -196.5 |
| $\log L$ | -225.4 | -216.2 | -223.6 |  | 10 percent |

*indicates statistical significance at the 5 percent level. ** indicates statistical significance at the 10 percent level. $\dagger$ see main text for definitions.
Notes: See Table 5.13.

Table 5.15 Descriptive Statistics

$\dagger$ measured in $£ / \mathrm{wk}$

Table 5.16a Selection Model of Earnings - Men

|  | Wave A |  | Wave B |  | Wave C |  | Wave D |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Participation <br> Probit | Eamings 2SLS | Participation Probit | Earnings 2SLS | Participation Probit | Eamings 2SLS | Participation <br> Probit | Eamings <br> 2SLS |
| intercept | $\begin{aligned} & -2.892 \\ & (0.392) \end{aligned}$ | $\begin{gathered} \hline 3.746 \\ (0.278) \end{gathered}$ | $\begin{aligned} & -1.719 \\ & (0.377) \end{aligned}$ | $\begin{gathered} \hline 3.582 \\ (0.264) \end{gathered}$ | $\begin{aligned} & \hline-1.686 \\ & (0.386) \end{aligned}$ | $\begin{gathered} \hline 3.574 \\ (0.277) \end{gathered}$ | $\begin{aligned} & -2.031 \\ & (0.379) \end{aligned}$ | $\begin{aligned} & 3.515 \\ & (0.324) \end{aligned}$ |
| age | $\begin{gathered} 0.174 \\ (0.019) \end{gathered}$ | $\begin{gathered} 0.100 \\ (0.013) \end{gathered}$ | $\begin{gathered} 0.123 \\ (0.019) \end{gathered}$ | $\begin{gathered} 0.113 \\ (0.012) \end{gathered}$ | $\begin{gathered} 0.127 \\ (0.019) \end{gathered}$ | $\begin{gathered} 0.115 \\ (0.013) \end{gathered}$ | $\begin{gathered} 0.156 \\ (0.019) \end{gathered}$ | $\begin{gathered} 0.114 \\ (0.016) \end{gathered}$ |
| $a g e^{2} / 100$ | $\begin{aligned} & -0.217 \\ & (0.022) \end{aligned}$ | $\begin{gathered} -0.114 \\ (0.016) \end{gathered}$ | $\begin{aligned} & -0.170 \\ & (0.022) \end{aligned}$ | $\begin{aligned} & -0.132 \\ & (0.015) \end{aligned}$ | $\begin{aligned} & -0.184 \\ & (0.022) \end{aligned}$ | $\begin{gathered} -0.132 \\ (0.017) \end{gathered}$ | $\begin{aligned} & -0.223 \\ & (0.023) \end{aligned}$ | $\begin{aligned} & -0.129 \\ & (0.021) \end{aligned}$ |
| no. kids | $\begin{gathered} -0.186 \\ (0.044) \end{gathered}$ | - | $\begin{aligned} & -0.185 \\ & (0.045) \end{aligned}$ | - | $\begin{aligned} & -0.212 \\ & (0.045) \end{aligned}$ | - | $\begin{gathered} -0.239 \\ (0.046) \end{gathered}$ | - |
| married | $\begin{gathered} 0.842 \\ (0.088) \end{gathered}$ | - | $\begin{gathered} 0.873 \\ (0.090) \end{gathered}$ | - | $\begin{gathered} 0.939 \\ (0.093) \end{gathered}$ | - | $\begin{gathered} 0.880 \\ (0.094) \end{gathered}$ | - |
| sick | $\begin{aligned} & -1.260 \\ & (0.153) \end{aligned}$ | $\begin{aligned} & -0.579 \\ & (0.145) \end{aligned}$ | $\begin{aligned} & -1.362 \\ & (0.145) \end{aligned}$ | $\begin{aligned} & -0.544 \\ & (0.153) \end{aligned}$ | $\begin{aligned} & -1.178 \\ & (0.140) \end{aligned}$ | $\begin{aligned} & -0.623 \\ & (0.133) \end{aligned}$ | $\begin{aligned} & -1.102 \\ & (0.131) \end{aligned}$ | $\begin{aligned} & -1.133 \\ & (0.147) \end{aligned}$ |
| degree | $\begin{gathered} 0.979 \\ (0.427) \end{gathered}$ | $\begin{gathered} 0.376 \\ (0.069) \end{gathered}$ | $\begin{gathered} 0.036 \\ (0.380) \end{gathered}$ | $\begin{gathered} 0.468 \\ (0.064) \end{gathered}$ | $\begin{gathered} 0.030 \\ (0.365) \end{gathered}$ | $\begin{gathered} 0.389 \\ (0.067) \end{gathered}$ | $\begin{gathered} 0.467 \\ (0.350) \end{gathered}$ | $\begin{gathered} 0.350 \\ (0.070) \end{gathered}$ |
| hnd | $\begin{gathered} 2.049 \\ (0.445) \end{gathered}$ | $\begin{gathered} 0.301 \\ (0.079) \end{gathered}$ | $\begin{gathered} 1.969 \\ (0.486) \end{gathered}$ | $\begin{gathered} 0.273 \\ (0.080) \end{gathered}$ | $\begin{gathered} 1.510 \\ (0.457) \end{gathered}$ | $\begin{gathered} 0.139 \\ (0.088) \end{gathered}$ | $\begin{gathered} 1.284 \\ (0.441) \end{gathered}$ | $\begin{gathered} 0.327 \\ (0.094) \end{gathered}$ |
| a-level | $\begin{gathered} 1.092 \\ (0.282) \end{gathered}$ | $\begin{gathered} 0.202 \\ (0.053) \end{gathered}$ | $\begin{gathered} 0.792 \\ (0.281) \end{gathered}$ | $\begin{gathered} 0.202 \\ (0.055) \end{gathered}$ | $\begin{gathered} 0.944 \\ (0.288) \end{gathered}$ | $\begin{gathered} 0.111 \\ (0.062) \end{gathered}$ | $\begin{gathered} 0.727 \\ (0.274) \end{gathered}$ | $\begin{gathered} 0.116 \\ (0.065) \end{gathered}$ |
| o-level | $\begin{gathered} 1.266 \\ (0.254) \end{gathered}$ | $\begin{gathered} 0.137 \\ (0.052) \end{gathered}$ | $\begin{gathered} 0.896 \\ (0.250) \end{gathered}$ | $\begin{gathered} 0.140 \\ (0.052) \end{gathered}$ | $\begin{gathered} 0.712 \\ (0.252) \end{gathered}$ | $\begin{gathered} 0.102 \\ (0.055) \end{gathered}$ | $\begin{gathered} 0.821 \\ (0.247) \end{gathered}$ | 0.110 $(0.058)$ |
| degxage | $\begin{aligned} & -0.006 \\ & (0.011) \end{aligned}$ | - | $\begin{gathered} 0.011 \\ (0.010) \end{gathered}$ | - | $\begin{gathered} 0.013 \\ (0.010) \end{gathered}$ | - | $\begin{aligned} & -0.002 \\ & (0.009) \end{aligned}$ | - |
| hndxage | $\begin{aligned} & -0.043 \\ & (0.011) \end{aligned}$ | - | $\begin{aligned} & -0.039 \\ & (0.011) \end{aligned}$ | - | $\begin{aligned} & -0.021 \\ & (0.011) \end{aligned}$ | - | $\begin{aligned} & -0.022 \\ & (0.010) \end{aligned}$ | - |
| a-levxage | $\begin{aligned} & -0.021 \\ & (0.007) \end{aligned}$ | - | $\begin{aligned} & -0.012 \\ & (0.007) \end{aligned}$ | - | $\begin{aligned} & -0.012 \\ & (0.007) \end{aligned}$ | - | $(0.007)$ | - - |
| o-levxage | $\begin{aligned} & -0.021 \\ & (0.006) \end{aligned}$ | - | $\begin{aligned} & -0.013 \\ & (0.006) \end{aligned}$ | - | $\begin{aligned} & -0.008 \\ & (0.006) \end{aligned}$ | - | (0.006) | 0.146 |
| london | $\begin{aligned} & -0.230 \\ & (0.103) \end{aligned}$ | $\begin{gathered} 0.218 \\ (0.060) \end{gathered}$ | $\begin{aligned} & -0.142 \\ & (0.102) \end{aligned}$ | $\begin{gathered} 0.138 \\ (0.060) \end{gathered}$ | $\begin{aligned} & -0.147 \\ & (0.104) \end{aligned}$ | $(0.063)$ | -0.230 $(0.104)$ | 0.146 $(0.072)$ 0.066 |
| north | $\begin{aligned} & -0.179 \\ & (0.067) \end{aligned}$ | $\begin{gathered} 0.041 \\ (0.037) \end{gathered}$ | $\begin{aligned} & -0.130 \\ & (0.067) \end{aligned}$ | $\begin{aligned} & -0.040 \\ & (0.038) \end{aligned}$ | $\begin{aligned} & -0.243 \\ & (0.068) \end{aligned}$ | $(0.041)$ | (0.069) | (0.044) |
| IMR | - | $\begin{aligned} & -0.731 \\ & (0.120) \end{aligned}$ | ) | $\begin{aligned} & -0.700 \\ & (0.123) \end{aligned}$ | ) | $\begin{aligned} & -0.720 \\ & (0.125) \end{aligned}$ | ) | (0.149) |
| $\log L$ | -1,074.3 | - | -1,072.3 | - | -1,029.1 | 0346 | -1,028.8 | 0.331 |
| $\bar{R}^{2}$ | - | 0.286 | - | 0.3 | - 2,117 | 1,534 | 2,127 | 1,549 |
| no. obs. | 2,320 | 1,751 | 2,214 | 1,015 | 2,17 |  |  |  |

Note: Response variable is log of net wage in $£ / \mathrm{wk}$.

Table 5.16b Selection Model of Earnings - Women

|  | Wave A |  | Wave B |  | Wave C |  | Wave D |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Participation <br> Probit | Earnings 2SLS | Participation Probit | Eamings 2SLS | Participation Probit | Eamings $2 S L S$ | Participation <br> Probit | Eamings 2SLS |
| intercept | $\begin{aligned} & -2.061 \\ & (0.573) \end{aligned}$ | $\begin{gathered} 5.615 \\ (0.750) \end{gathered}$ | $\begin{aligned} & -1.302 \\ & (0.526) \end{aligned}$ | $\begin{gathered} 5.405 \\ (0.589) \end{gathered}$ | $\begin{aligned} & -1.450 \\ & (0.520) \end{aligned}$ | $\begin{gathered} 4.588 \\ (0.558) \end{gathered}$ | $\begin{aligned} & -0.935 \\ & (0.497) \end{aligned}$ | $\begin{gathered} 3.812 \\ (0.516) \end{gathered}$ |
| age | $\begin{gathered} 0.143 \\ (0.029) \end{gathered}$ | $\begin{gathered} 0.034 \\ (0.036) \end{gathered}$ |  | $\begin{gathered} 0.036 \\ (0.030) \end{gathered}$ | $\begin{gathered} 0.125 \\ (0.028) \end{gathered}$ | $\begin{gathered} 0.075 \\ (0.029) \end{gathered}$ |  | 0.122 (0.027) |
| $a g e^{2 / 100}$ | $\begin{aligned} & -0.192 \\ & (0.037) \end{aligned}$ | $\begin{aligned} & -0.045 \\ & (0.048) \end{aligned}$ | $\begin{aligned} & -0.159 \\ & (0.035) \end{aligned}$ | $\begin{gathered} -0.033 \\ (0.041) \end{gathered}$ | $\begin{aligned} & -0.186 \\ & (0.035) \end{aligned}$ | $\begin{aligned} & -0.084 \\ & (0.039) \end{aligned}$ | $\begin{aligned} & -0.151 \\ & (0.035) \end{aligned}$ | $\begin{gathered} -0.149 \\ (0.036) \end{gathered}$ |
| no. kids | $\begin{aligned} & -0.388 \\ & (0.050) \end{aligned}$ | — | $\begin{aligned} & -0.391 \\ & (0.046) \end{aligned}$ | - | $-0.447$ <br> (0.050) |  | $\begin{aligned} & -0.407 \\ & (0.048) \end{aligned}$ |  |
| married | $\begin{gathered} 0.779 \\ (0.110) \end{gathered}$ | - | $\begin{gathered} 0.847 \\ (0.101) \end{gathered}$ | - | $\begin{gathered} 0.914 \\ (0.107) \end{gathered}$ | - | $\begin{gathered} 0.673 \\ (0.102) \end{gathered}$ | - |
| sick | $\begin{aligned} & -2.222 \\ & (0.358) \end{aligned}$ | $\begin{gathered} 2.826 \\ (0.884) \end{gathered}$ | $\begin{aligned} & -1.256 \\ & (0.151) \end{aligned}$ | $\begin{gathered} 0.187 \\ (0.278) \end{gathered}$ | $\begin{aligned} & -1.283 \\ & (0.154) \end{aligned}$ | $\begin{gathered} 0.217 \\ (0.257) \end{gathered}$ | $\begin{aligned} & -1.136 \\ & (0.151) \end{aligned}$ | $\begin{aligned} & -0.002 \\ & (0.238) \end{aligned}$ |
| degree | $\begin{aligned} & -0.607 \\ & (0.627) \end{aligned}$ | $\begin{gathered} 0.140 \\ (0.231) \end{gathered}$ | $\begin{gathered} 0.090 \\ (0.537) \end{gathered}$ | 0.278 $(0.183)$ | $\begin{aligned} & -0.349 \\ & (0.531) \end{aligned}$ | 0.279 $(0.167)$ | $\begin{aligned} & -1.298 \\ & (0.597) \end{aligned}$ | 0.045 $(0.161)$ |
| hnd | $\begin{gathered} 0.365 \\ (0.655) \end{gathered}$ | $\begin{gathered} 0.164 \\ (0.278) \end{gathered}$ | $\begin{gathered} 2.001 \\ (0.657) \end{gathered}$ | $\begin{aligned} & -0.069 \\ & (0.230) \end{aligned}$ |  | $\begin{gathered} 0.046 \\ (0.210) \end{gathered}$ | 1.054 $(0.629)$ | 0.009 $(0.209)$ |
| a-level | $\begin{gathered} 0.209 \\ (0.425) \end{gathered}$ | $\begin{aligned} & -0.143 \\ & (0.203) \end{aligned}$ | $\begin{gathered} 0.566 \\ (0.379) \end{gathered}$ | -0.108 $(0.166)$ |  | -0.104 $(0.158)$ | 0.515 $(0.366)$ | -0.055 $(0.147)$ |
| o-level | $\begin{gathered} 0.384 \\ (0.355) \end{gathered}$ | $\begin{aligned} & -0.221 \\ & (0.169) \end{aligned}$ | $\begin{gathered} 0.077 \\ (0.312) \end{gathered}$ | $\begin{aligned} & -0.145 \\ & (0.130) \end{aligned}$ |  | -0.066 $(0.121)$ | -0.171 $(0.297)$ | -0.088 $(0.112)$ |
| degxage | $\begin{gathered} 0.037 \\ (0.019) \end{gathered}$ | -- | $\begin{gathered} 0.009 \\ (0.015) \end{gathered}$ | - | 0.027 $(0.016)$ | - | $\begin{gathered} 0.062 \\ (0.019) \end{gathered}$ | - |
| hndxage | $\begin{gathered} 0.002 \\ (0.017) \end{gathered}$ | - | $\begin{aligned} & -0.035 \\ & (0.016) \end{aligned}$ | -- | $\begin{aligned} & -0.001 \\ & (0.015) \end{aligned}$ | - | $(0.016)$ |  |
| a-levxage | $\begin{gathered} 0.010 \\ (0.012) \end{gathered}$ | - | $\begin{aligned} & -0.003 \\ & (0.011) \end{aligned}$ | - | $\begin{aligned} & -0.001 \\ & (0.011) \end{aligned}$ | - | (0.011) |  |
| o-levxage | $\begin{gathered} 0.002 \\ (0.009) \end{gathered}$ | - |  | - | $\begin{aligned} & 0.008 \\ & (0.008) \end{aligned}$ | - | (0.008) $-0.691$ | 0.275 |
| london | $\begin{aligned} & -0.307 \\ & (0.125) \end{aligned}$ | $\begin{gathered} 0.546 \\ (0.181) \end{gathered}$ | $-0.209$ <br> (0.116) | $\begin{gathered} 0.411 \\ (0.148) \end{gathered}$ | $(0.116)$ | (0.137) | $(0.119)$ -0.166 | $(0.127)$ 0.057 |
| north | $\begin{aligned} & -0.206 \\ & (0.085) \end{aligned}$ | $0.102$ <br> (0.120) | $\begin{aligned} & -0.092 \\ & (0.080) \end{aligned}$ | $\begin{aligned} & -0.040 \\ & (0.099) \end{aligned}$ | $\begin{array}{ll}  & -0.034 \\ ) & (0.083) \end{array}$ | (0.093) | (0.081) | $(0.090)$ -1.542 |
| $I M R$ | - | $\begin{aligned} & -1.969 \\ & (0.292) \end{aligned}$ | ) | $\begin{aligned} & -1.720 \\ & (0.222) \end{aligned}$ |  | $(0.209)$ | ) -7520 | (0.225) |
| $\log L$ | -679.4 | - | -763.9 | - | -728.5 | 0.389 | 9 | 0.357 |
| $\bar{R}^{2}$ | - | 0.357 | - |  |  | 898 | 1,383 | 920 |
| no. obs. | 1,232 | 786 | 1,380 | 868 |  |  |  |  |

Note: Response variable is log of net wage in $£ / \mathrm{wk}$.

Table 5.17 Testing for Future Events in Take-Up


## APPENDIX 5B

## FIGURES FOR CHAPTER 5

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5.1 Composition of Entitled Samples - By Employment Status
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Figure 5.1 Composition of Entitled Samples - By Employment Status


## Source: Author's computations based on BHPS.

Note: 1. Category 'Other' includes retired, full-time student/school, long-term sick/disabled, government training scheme and others not covered by any of these categories.
2. Sample sizes for (i) entitled recipients are: wave $A=395$, wave $B=388$, wave $C=381$, wave $D=380$; (ii) entitled non-recipients are: wave $A=115$, wave $B=121$, wave $C=94$, wave $\mathrm{D}=98$.

Figure 5.2 Composition of Entitled Samples - By Current Financial Situation


## Source: Author's computations based on BHPS.

Note: $\quad$. Subjective answers to question about current financial situation ranked in the following order: 1. living comfortably, 2 . doing all right, 3 . just about getting by, 4 . finding it quite difficult, and 5 . finding it very difficult 2. Sample sizes for (i) entitled recipients are: wave $A=395$, wave $B=388$, wave $C=381$, wave $D=380$; (ii) entitled non-recipients are: wave $A=115$, wave $B=121$, wave $C=94$, wave $D=98$.

Figure 5.3 Composition of Entitled Samples - By Change in Financial Situation Compared to One Year Ago


Source: Author's computations based on BHPS.
Note: 1. Subjective answers to question about current financial situation as compared to financial situation of one year ago. 2. Sample sizes for (i) entitled recipients are: wave $A=395$, wave $B=388$, wave $C=381$, wave $D=380$; (ii) entitled non-recipients are: wave $\mathrm{A}=115$, wave $\mathrm{B}=121$, wave $\mathrm{C}=94$, wave $\mathrm{D}=98$.

Figure 5.4 Composition of Entitled Samples - By Expectation of Change in Financial Situation for the Year Ahead


## Source: Author's computations based on BHPS.

Note:

1. Subjective answers to question about expected future financial situation next year as compared to current financial situation.
2. Sample sizes for (i) entitled recipients are: wave $A=395$, wave $B=388$, wave $C=381$, wave $D=380$; (ii) entitled non-recipients are: wave $\mathrm{A}=115$, wave $\mathrm{B}=121$, wave $\mathrm{C}=94$, wave $\mathrm{D}=98$.

[^0]:    ${ }^{1}$ After 1988, FC replaced Family Income Supplement (FIS) and HB replaced a two-tier HB system consisting of Standard HB and Certificated HB. As for the IS scheme, the post-1988 benefits were meant to be simpler and more effective schemes than their predecessors.

[^1]:    ${ }^{2}$ For discussions see, for example, Esping-Andersen (1993), Atkinson (1992) and Gordon (1988).
    ${ }^{3}$ Expenditure on health and personal social services comes second at about 16 percent of total government expenditure.
    ${ }^{4}$ That is of the ratio of the number of persons aged $60 / 65$ and above to those of working age. On future dependency ratios see OECD (1988) and Falkingham (1989).
    ${ }^{5}$ See, for example, Atkinson (1994) and Dilnot et al. (1994).
    ${ }^{6}$ Dilnot and Webb (1989) discuss the Social Security Act 1986 and the changes brought about by it in greater detail: An excellent review of the pre-1986 social security system is provided by Atkinson et. al. (1986).

[^2]:    ${ }^{7}$ These were (i) greater targeting, (ii) consistency with government economic policy, and (iii) less complexity. See Dilnot and Webb (1989) for details.
    ${ }^{8}$ This shift of benefits to specific population groups is generally known as targeting of benefits. In practice, benefit targeting often takes the form of means-testing (i.e. targeting benefits to those groups in greatest financial need of them). In this thesis we concentrate primarily on targeting by meanstesting.

[^3]:    ${ }^{9}$ For an excellent discussion on targeting see Atkinson (1993) and on means-testing versus universal provision of benefits see Besley (1990).
    ${ }^{10}$ For an interesting comparison of means-testing the unemployed in Britain, France and Germany see Evans (1996). He finds that the extent and growth in means-testing has been considerably greater in Britain than in either France or Germany over the last 15 years.

[^4]:    ${ }^{11}$ Currently, there are two other means-tested benefits: Council Tax Benefit (recently Community Charge Benefit) and Disability Working Allowance. However, expenditure on these benefits, particularly the latter, are comparatively smaller.
    ${ }^{12}$ Lister (1989) notes that the shift towards greater means-testing can be traced to the 1950s where 'benefit selectivity' was high on the political agenda. By the early 1970s this enthusiasm subsided somewhat with the discovery of low take-up and the poverty trap. However, means-testing as a form of selectivity in paying out benefit is by no means a phenomenon of the twentieth century. Its origins can be traced to at least the time of the Poor Law Reforms of 1834 (see Atkinson (1987), Barr (1987), and Hill (1990)).

[^5]:    ${ }^{13}$ We begin in 1983 since it is the first year of the inception of the Housing Benefit Scheme. Prior to this HB consisted of two schemes: one for those on SB and one for those not on SB.

[^6]:    ${ }^{14}$ See Atkinson (1994) for a critical discussion.

[^7]:    ${ }^{15}$ A benefit unit is a single individual or a head of household. Estimates are rounded and are based on a benefit unit working 30 hours or more a week, and exclude the self-employed and pensioners.

[^8]:    ${ }^{16}$ The data apply to England and Wales only.
    ${ }^{17}$ A more detailed discussion of the attitudes and characteristics of lone parents in 1990s Britain can be found in Lewis (1995).
    ${ }^{18}$ Child Support is the maintenance payment for dependent children (since 1993 delivered via the Child Support Agency on behalf of absent parents who are able to support their children). Bingley et al. (1994) find that the number of lone parents who receive Child Support and IS dramatically declined between 1979-1992. This is because the IS entitlement calculation imposes a 100 percent tax on Child Support payments.

[^9]:    ${ }^{19}$ For a detailed desciption of the BHPS see the Taylor (1996). A brief description of the BHPS and its contents is also given in the latest edition of the CSO's Social Trends (1996).
    ${ }^{20}$ These issues are discussed in Taylor (1996). See also Buck et al. (1994) on apllications using BHPS data.

[^10]:    ${ }^{21}$ Grossing-up a data set is discussed further in Chapter 2.
    ${ }^{22}$ The official statistics give recipient numbers only on a quarterly basis and we thus adopt the same convention here.

[^11]:    ${ }^{1}$ Strictly speaking an entitlement is only granted if it exceeds $£ 0.10$.
    ${ }^{2}$ The first edition of CPAG's National Welfare Benefits Handbook in the early 1970s was 72 pages long whereas the 1994 edition, for example, runs to 570 pages.
    ${ }^{3}$ Later on we draw attention to the fact that it is generally not possible to accurately assess savings from the information provided by the BHPS.

[^12]:    ${ }^{4}$ The computer program itself was written entirely in the $S A S$ programming language.

[^13]:    ${ }^{5}$ A household is defined as a single individual living by themselves or a group of individuals who share accommodation or a meal a day, and for whom this household is their sole (or main) residence. Institutions were excluded from the survey.

[^14]:    ${ }^{6}$ As will be seen later, we will in fact use the FES to supplement the BHPS.

[^15]:    ${ }^{7}$ These themes are addressed in detail in Chapter 5 of this thesis.
    ${ }^{8}$ By the time wave B was released more detailed information on children was provided in separate record types. This information though is still relatively limited in comparison to the information on adults.
    ${ }^{9}$ The Income Record in particular needs to be adjusted in order to be merged with the other data sets. Up to 33 different income sources (the number of weeks they cover and whether the individual is currently in

[^16]:    receipt of them) are all recorded in grids that appear as a single variable in the data set. Thus for any individual in receipt of more than one income from this record, the merging process will produce multiple observations, since one observation is produced for each income received. In order to overcome this problem additional variables need to be created for this record before merging occurs.
    ${ }^{10}$ Note that the DSS definition of couples encompasses not only married couples but also cohabiting couples. However, as recognized in the introduction, the decision as to whether two persons cohabiting are regarded as a couple or not is often at the discretion of the DSS. The BHPS questionnaire attempts to unveil true 'couple status' as accurately a possible and it is this information which we draw upon. Also note that within a BHPS defined household there may be more than one couple.
    ${ }^{11}$ It is important to combine couples at this stage since the next stage could possibly delete some members of a couple, thereby producing couples for which only one member can be identified. This might happen, for example, when proxy individuals or any other sub-groups are deleted (in certain cases the reference person provided information about his or her partner as a proxy).

[^17]:    ${ }^{12}$ Several relevant variables give quantities per month and these are converted into weekly figures by dividing by 4.33 which is the standard BHPS convention.
    ${ }^{13}$ Atkinson and Micklewright compared grossed-up income figures from the FES for the years 1970-77 with those from national accounts (Blue Book). They found that self-employment incomes as reported in the FES were only $55.2 \%$ of that from the national accounts. In comparison, wages and salaries for the non-self-employed in the FES (again grossed-up) were $96.9 \%$ of those reported in the national accounts.

[^18]:    14 The DSS performs an annual survey, each May, of one in every one-hundred IS recipients. The resulting data set, the IS Annual Enquiry, provides a detailed insight to the composition and characteristics of current IS recipients.
    ${ }^{15}$ Attendance Allowance is a non-means-tested benefit for the severely disabled.

[^19]:    ${ }^{16}$ This is unlikely to strongly bias the resulting calculations as there are, once again, likely to be only a very small number of cases where this occurs.

[^20]:    ${ }^{17}$ From wave $C$ onwards band (iii) was extended into two bands ranging from $£ 100-£ 500$ and $£ 501-£ 1000$ respectively. However, this provides little additional information for our purposes.

[^21]:    ${ }^{18}$ If we are unable to assume that such misclassifications occur at random then, of course, the nature of our resulting eligible sample will change. For example, consider the case where a certain group of individuals are particularly prone to misreport their weekly hours worked. If they persistently under-report their hours worked we will include a larger number of them in the eligible sample than would be the case if they accurately reported their hours worked. However, only if we then also computed a positive IS entitlement for them would they remain in the entitled sample. Hence the final entitled sample may be unaffected by the original misreporting.

[^22]:    ${ }^{19}$ Rent is covered by the means-tested benefit Housing Benefit.

[^23]:    ${ }^{20}$ The disregards were mostly unchanged between 1991/92 and 1994/95.

[^24]:    ${ }^{21}$ Note that in the case where $h r s \geq 24 / 16$ a positive benefit entitlement can still be established, subject to

[^25]:    the above special rules.

[^26]:    ${ }^{22}$ The latter of these is collected privately by National Opinion Polls and therefore is not necessarily available to the research community at large.

[^27]:    ${ }^{23}$ See CSO Financial Statistics, particularly Tables 13.12, 13.9 and 13.10.

[^28]:    ${ }^{24}$ See Poterba (1994) for more examples.

[^29]:    25 Tobit models with various specifications were estimated, often including a greater number of explanatory variables than in the reported model. However, for most of these (e.g. age squared, dummies for marital status and region) the estimated coefficients were very small and highly insignificant.
    ${ }^{26}$ The most valuable durable good owned by many households in Britain is property. Ideally we would want a measure of the value of any property owned but since this information is contained in the BHPS but not in the FES we make use of a very rough proxy giving the number of rooms in the main accommodation for the household. This is certainly misleading in many cases but is of interest in itself, since a large number of rooms might be an indicator of relatively high income and thereby a greater propensity to save.

[^30]:    ${ }^{27}$ These findings are confirmed by scatter plots of actual against predicted savings (not shown here).

[^31]:    ${ }^{28}$ Questions about mortgage repayments asked at the time of interview relate to the previous month. Hence figures relating to the month prior to interview are applicable.

[^32]:    ${ }^{29}$ We adopt the convention for couples where, if a mortgage has been taken out, it is assumed that it is in the name of the head of household.

[^33]:    ${ }^{30}$ IS receipt might not always be reported correctly. If, say, an individual is actually aware of her nonentitlement to IS but nevertheless is in receipt of it she might misreport on purpose. We discuss this issue in greater detail later on.

[^34]:    ${ }^{31}$ The caseload take-up rate expresses take-up in terms of the number of individuals claiming and not claiming. The expenditure take-up rate considers the monetary amounts claimed and unclaimed. Here we concentrate on the former as a measure of take-up.
    ${ }^{32}$ By 'true' take-up is meant the take-up rate obtained if our data were subject to no errors whatsoever.

[^35]:    ${ }^{33}$ The dynamic evidence of take-up behaviour in the BHPS is considered in Chapter 5 of this thesis.

[^36]:    ${ }^{34}$ Grossing-up techniques for the FES are well documented (see Atkinson et al. (1989)). In essence, grossing-up a survey sample involves converting samples so as to be representative of the population as a whole. For example, for the FES which surveys about one in every 3,000 households in the UK the grossing-up factor is 3,000 . However, since certain groups of persons are often over- or under-represented in the survey each of these groups needs to be grossed-up by a different factor.

[^37]:    ${ }^{35}$ Supplementary Benefit was replaced by IS in the 1988 social security reforms. For details see Dilnot and Webb (1989).

[^38]:    ${ }^{36}$ The IS Annual Enquiry, although based on DSS records is also a sample of one in every one hundred IS recipients.

[^39]:    ${ }^{37}$ Unfortunately the latest figures by Institute for Fiscal Studies - who also produces take-up estimates extend to 1990 and are thus of no use for our comparisons.
    ${ }^{38}$ We have deleted from our a plots a number of observations for which the computed IS entitlement was very large (negative or positive). The small number of very large positive computed IS entitlements for non-pensioners are due to a high housing cost component (i.e. mortgage interest) in the IS computation. For pensioners virtually all outlying cases are in the negative IS entitlement since, quite clearly, there are considerably less cases with outstanding mortgages.

[^40]:    1 The terms 'take-up' and 'participation' are used interchangeably throughout this and all other chapters. The former is standard terminology in the UK literature whereas the latter is used mostly in the US literature.
    2 We will use the term 'individual' throughout thereby referring to a benefit unit that might consist of a single individual or a couple (with or without kids) since any one such unit can only have one claimant (see Chapter 2 for details).
    ${ }^{3}$ The distinction between eligibility and entitlement to Income Support - the main means-tested benefit in the UK - is described in detail in Chapter 2.
    ${ }^{4}$ We thus follow the general approach of Duclos (1992a, 1992b \& 1995) for his model of benefit take-up. Our approach differs from Duclos's in several ways though which will become apparent later on.

[^41]:    ${ }^{5}$ Namely the main means-tested benefits prior to the 1988 Reforms: Standard and Certificated Housing Benefit, Supplementary Benefit and Family Income Supplement.

[^42]:    ${ }^{6}$ In view of the findings from Table 3.1, this assumption appears to be highly restrictive since it is precisely these thresholds at which many potential claimants are prevented from claiming.

[^43]:    ${ }^{7}$ Family Credit is a means-tested benefit aimed at poor working families with children.

[^44]:    8 As in the previous chapter, the 'individual' refers to the main claimant of a family unit since only one claim can usually be lodged by any one benefit unit.
    ${ }^{9}$ In our models we assume the concepts of 'taking-up' and 'claiming' to be the same. With our modelling strategy a claim implies take-up so that here we exclude cases where an individual might claim and be awarded the benefit but may decide not to take-up.

[^45]:    ${ }^{10}$ The empirical work on take-up which builds on these simple models is fare more extensive, and is reviewed in Chapter 4.

[^46]:    ${ }^{11}$ It is this simple expression which underlies all of the models of take-up presented in the literature. We too will draw on this model when considering the decision to take-up.
    12 Note how this differs from the variable component of stigma. The variable component reduces the level of benefit in the utility function itself whereas a flat component that depends on $b$ subtracts a cost from the total utility of income and benefit.

[^47]:    13 The number of hours worked per week is part of the means test for the benefit (Aid to Families with

[^48]:    Dependent Children) considered by Moffitt.

[^49]:    14 The two types of FEDO considered are: main district offices and smaller branch offices. Unlike the smaller branch offices the main branches do not have a statistically significant effect on take-up rates. The explanation suggested for this is that the majority of social security recipients considered live in small rural areas of North Carolina and thus benefit more from the smaller branch offices than from the main district offices.
    ${ }^{15}$ The role of the government agency in modelling take-up is covered by Duclos (1995).

[^50]:    ${ }^{16}$ The rather complex steps in deciding on eligibility and on computing the entitlement level are outlined in Chapter 2.

[^51]:    ${ }^{17}$ There is of course the possibility that this error term is systematic. See Duclos (1992b) for a discussion.
    ${ }^{18}$ Taking general measures to decrease the costs of claiming to an individual could be more problematic since these costs are not directly observable by the government agency.

[^52]:    ${ }^{19}$ So we are ignoring stage 1 of Figure 1 and concentrate only on stages 2 and 3 .

[^53]:    ${ }^{20}$ The British Social Attitudes Survey is an opinion poll of a random sample of adults performed at regular intervals since 1983 by Social and Community Planning Research (SCPR). Sample sizes vary from year to

[^54]:    year. For the 1989 survey 2,930 persons were questioned whereas in 1994 the survey covered approximately 3,500 persons.

[^55]:    ${ }^{21}$ Besley and Coate refer to these two views of stigma as statistical discrimination and taxpayer resentment respectively.
    ${ }^{22}$ Since we assume that individuals who claim will receive the benefit, $C$ is equivalent to the total number of claimants.

[^56]:    ${ }^{23}$ We assume for simplicity that the actual benefit level, $b$, is fixed.

[^57]:    ${ }^{24}$ This equilibrium level of stigma always exists but is not necessarily unique (see Besley and Coate for details).

[^58]:    ${ }^{25}$ Note that the same argument holds for changes in the wage rate of the poor.

[^59]:    ${ }^{26}$ Unlike the equilibrium level of $s$ under the model of reputational externalities, this equation does in fact have a unique equilibrium.

[^60]:    ${ }^{27}$ In the empirical work of the subsequent chapters the net utility is interpreted as a latent variable which gives rise to an observed binary variable taking the value ' 1 ' only if it exceeds zero is satisfied and the value ' 0 ' otherwise. This is the familiar random utility model underlying the well-known probit and logit models in econometrics.
    ${ }^{28}$ Clearly, increases in either $h$ or $s$ will reduce the probability of take-up.

[^61]:    ${ }^{29}$ A not altogether different approach to state dependence is adopted by Heckman (1981) in a dynamic model of labour supply. However, Heckman does not account for any form of uncertainty in decisionmaking. Our approach draws on more recent work on choice theory under uncertainty with elation or

[^62]:    disappointment. The theory accounts for a degree of elation or disappointment as a consequence of what was expected from a decision and what the decision actually turned out as (see Bell (1985) and Loomes and Sugden (1986)).
    ${ }^{30}$ As before, we assume that if an entitled individual claims, she will be granted the benefit.
    ${ }^{31}$ This expression has the advantage of a simple discerning explanation. If, say an individual expected $£ 50$ but was granted only $£ 30$ she will feel some degree of disappointment. If however, she expected $£ 50$ but was granted $£ 75$ she would feel some degree of elation.

[^63]:    ${ }^{32}$ In either equation (3.25) or (3.26) we have not accounted for the fact that the probability $\pi$ at time $t$ might be formed conditional on $\pi$ at time $t-1$. A simple Bayesian updating mechanism would allow for such conditional probabilities.

[^64]:    ${ }^{33}$ We could relax the latter assumption by introducing uncertainty about the outcome of the future claim. However, this would unduly complicate the model without further insight to the problem at hand.
    ${ }^{34}$ For the sake of simplicity we have ignored discounting the future utility streams.

[^65]:    ${ }^{35}$ As before, introducing uncertainty about the future benefit claim does not enhance the findings of interest to us (namely, the effect of future employment on current claiming behaviour).

[^66]:    ${ }^{1}$ The scope for errors to enter the computation of the IS entitlement is discussed in Chapter 2.

[^67]:    ${ }^{2}$ See also McCullagh and Nelder (1989) and Cox and Snell (1989) for a more general discussion of binary choice models motivated by settings other than those encountered in economics.

[^68]:    ${ }^{3}$ By 'naive' we refer to the univariate logit/probit model which does not account for measurement error.
    ${ }^{4}$ SB was the forerunner to IS and was replaced by the latter after the April 1988 social security reforms. For a discussion of the 1988 reforms see Dilnot and Webb (1989).

[^69]:    ${ }^{5}$ Strictly speaking 'Standard' HB is considered which together with 'Certificated' HB was replaced by a single HB scheme after the 1988 social security reforms.
    ${ }^{6}$ FIS is the third main means-tested benefit aimed at working families with dependent children. Thus unlike HB and $\mathrm{SB} / \mathrm{IS}$ a key requirement is that at least one family member is employed at the time of claiming (currently the eligibility criterion is working for at least 16 hours a week). It too was replaced in the 1988 reforms by an allegedly simpler benefit, Family Credit (FC).

[^70]:    ${ }^{7}$ In analysing FIS/FC take-up, pooling data is often essential as for any one year of the FES the number of individuals receiving (and calculated as being entitled to) the benefit is very small (typically $<50$ ).
    ${ }^{8}$ On average they suggest a stigma effect of magnitude $£ 6$ for an average FC receipt of $£ 25$.

[^71]:    ${ }^{9}$ Throughout this chapter we solely use the UK meanings of social security and (means-tested) benefits. In the US, the former usually refers to state pensions whereas the latter is often referred to as 'welfare'.

[^72]:    ${ }^{10}$ A couple of recent US studies have considered the closely related issue of misclassification of the binary response variable in participation studies (see Hausman and Scott Morton (1994) and Poterba and Summers (1995)). However, these studies still do not explicitly address measurement error as an issue in itself.
    ${ }^{11}$ That is, caseload and expenditure take-up percentage rates as discussed and computed in Chapter 2.
    ${ }^{12}$ For the total sample (including non-pensioners, pensioners and the self-employed) his results suggest an average claiming cost of $£ 6.08$ for an average SB of $£ 35.49$. Overall, about one-fifth of SB payments are lost in some form of transaction costs.

[^73]:    ${ }^{13}$ Many of the early techniques and results relating to measurement error models are surveyed by Cochran (1968).

[^74]:    ${ }^{14}$ The general issues relating to measurement error in economic models are discussed by Morgenstern

[^75]:    (1963), Grilliches (1974 and 1986), Aigner et al. (1984) and Chesher (1990 and 1991b).
    ${ }^{15}$ We shall refer to $x$ as the 'true' covariate and to $z$ as the 'surrogate' covariate.

[^76]:    ${ }^{16}$ The instrumental variables technique is a common tool for consistent estimation in linear measurement error models (see Durbin (1954), Griliches and Mason (1972) or Maddala (1977)). The technique itself is well documented in Bowden and Turkington (1984) and Aldrich (1993) who provides a good account of

[^77]:    the origins and evolution of the technique. The choice of suitable instruments is discussed by White (1984).
    ${ }^{17}$ See, for example, Griliches and Ringstad (1970), Wolter and Fuller (1982), Amemiya (1985 and 1990), Iwata (1992), and Lee and Sepanski (1995).
    ${ }^{18}$ Assume all variates and covariates to be scalar unless specified otherwise.
    ${ }^{19}$ See, for example, Armstrong (1985), Stefanski (1985), Schafer (1987), Whittemore and Keller (1988), Stefanski (1989), Stefanski and Carroll (1990); interesting applications of these techniques can be found in Pierce et al. (1992) on atomic bomb survival data, and Carroll and Stefanski (1994) on the effects of blood pressure levels on coronary heart disease.
    ${ }^{20}$ In GLMs the additive structure of measurement error is not essential. Many of the results relate to an multiplicative measurement error structure as well (see Armstrong (1985)).

[^78]:    21 For similar procedures adopting small variance approximations see also Wolter and Fuller (1982), Stefanski (1985), Amemiya and Fuller (1988) and Stefanski and Carroll (1990).

[^79]:    ${ }^{22}$ See also Levine (1985) who considers the sensitivity of maximum likelihood to measurement error, i.e. $L(\theta, z)=\log f(y \mid \theta, z)$ where $\theta$ is some parameter to be estimated.
    23 The methods suggested do not necessarily produce unbiased parameter estimates of the model of interest. Instead, especially in large samples, they yield parameter estimates that are considerably less biased than the naive estimates.

[^80]:    ${ }^{24}$ The main advantage of the instrumental variables method is that there is no requirement for a validation or replication data set. In other words, there is no need to estimate the measurement error variance.
    ${ }^{25}$ Much of the earlier work on binary choice measurement error models originates from epidemiological questions raised in connection with the Framingham Heart Study (a large cohort study of the development

[^81]:    ${ }^{26}$ Certain income is subject to disregards so that strictly speaking the marginal tax rate of 100 percent does not always apply. However, marginal tax rates of 80 to 90 percent are common (see Dilnot and Webb (1989)).
    ${ }^{27}$ Some indication of this effect is provided in Chapter 2, where we compute a higher percentage take-up rate for couples with kids as compared to singles or childless couples.

[^82]:    ${ }^{28}$ By newly entitled we mean individuals entitled in wave B that have not been entitled in wave A , individuals entitled in wave $C$ that have not been entitled in wave $A$ or in wave $B$, and so on.
    ${ }^{29}$ As noted above, the latest take-up study using the FES by Fry and Stark (1993) does not conduct an analysis for pensioners either. They too find data on pensioners in the FES inaccurate and unreliable.
    ${ }^{30}$ As in the case of unemployment duration and its effect on the probability of take-up, a better understanding of these subjective measures is more likely to be obtained from the dynamic analysis in Chapter 5.

[^83]:    ${ }^{31}$ In Chapter 2, from our IS algorithm, we found 517 entitled individuals at wave A. Here we have deleted 7 outlying observations (with very large IS entitlements). Similarly, we have deleted outlying observations for the entitled samples at each of the other waves: 17 at wave $B, 9$ at wave $C$, and 16 at wave $D$.

[^84]:    ${ }^{32}$ The reference person for the logit regression is a single male, employed part-time, who rents from the private sector or has a mortgage, is not registered as being sick and does not hold a degree. He also replied negatively to the two subjective financial measures.

[^85]:    ${ }^{33}$ Extensions to the case where we have a vector of explanatory variables of which only a single or some covariates are measured with error and the others measured without error are straightforward. For the sake of simplicity we have chosen a scalar covariate not subject to measurement error. The same arguments apply if $x_{i}^{*}$ is a vector of covariates.
    ${ }^{34}$ Such data sets are often utilised in medical statistics where, for example, in a study of the factors determining heart disease accurate measures of the level of blood cholesterol are costly and difficult to obtain for a large sample. The observed covariate, blood cholesterol level, is thus subject to measurement error and, for a small sub-sample of the main data set, a more accurate measure of blood cholesterol level is made (for issues relating to the construction of validation data sets see Spiegelman and Gray (1991) and Lee and Sepanski (1995)).

[^86]:    ${ }^{35}$ Exceptions to this can be found in earnings studies using large US panel data sets such as the Panel Survey of Income Dynamics (see for example Bound and Krueger (1991) and Pischke (1995)).

[^87]:    ${ }^{36}$ See Carroll et al. (1995, pp.12-13) who strongly advise against using external validation data sets on these grounds. However, recently Imbens and Lancaster (1994) have suggested the use of official statistics in providing almost exact knowledge of true distributions of covariates in methods of moments estimation..

[^88]:    ${ }^{37}$ As was first suggested by Chesher (1991a), the density in (4.17) may well be a non-proper density function if the second order term induces a convexity. The transformation ensures that the density is proper, i.e. it lies in the interval $[0,1]$.
    ${ }^{38}$ This take-up rates differs from the ones in Chapter 2. Here we consider only all entitled individuals and amongst them, all who report IS receipt. We therefore (and unlike the figures in Chapter 2) exclude individuals who report receipt of IS but who are - according to our IS algorithm - not entitled to IS.

[^89]:    ${ }^{39}$ We also assume that none of the dummy variables is subject to misclassification.

[^90]:    ${ }^{40}$ Alternative functional forms used the same covariates in (i) linear form only, (ii) combinations of linear and quadratic terms, and (iii) combinations of linear, quadratic and cubic terms. Later on we compare the results obtained by using the linear specification and the above specification.
    ${ }^{41}$ We use a parametric bootstrapping scheme as follows: 500 data sets are formed by fixing the true covariates in the main data set and generating 500 Bernoulli random variables (i.e. the new response variables) conditional on these true covariates. Then, for each of the $m=1, \ldots, 500$ generated data sets, we estimate the unknown parameter vector $\beta_{m}$ by logit regression. Let the average of the $500 \hat{\beta}_{m}$ vectors be $\bar{\beta}$, and the estimated variance-covariance matrix is then

    $$
    \operatorname{Var}\left(\hat{\beta}_{m}\right)=\frac{1}{492} \sum_{m=1}^{5 \infty}\left(\hat{\beta}_{m}-\bar{\beta}\right)\left(\hat{\beta}_{m}-\bar{\beta}\right)^{\prime} .
    $$

    For further details on bootstrapping techniques see Efron and Tibshirani (1993).

[^91]:    ${ }^{42}$ The following cases are considered in all the examples given throughout this chapter: first, a female lone parent aged 32 with two dependent children and no other income (excluding income disregarded from IS). She is not currently taking-up IS and her computed entitlement is $£ 57.94$ per week. Secondly, a married couple with one dependent child where the head of household is male, aged 39 and unemployed for the past 13 weeks. His wife works part-time earning $£ 9.75$ per week (excluding any disregarded income). The head of household is entitled to $£ 86.50$ per week which is currently not being taken-up. Finally, we

[^92]:    ${ }^{43}$ Put another way, the take-up rate as computed for our sample is the true take-up rate of that sample.
    ${ }^{44}$ There is no reason to believe that data in the BHPS should be any more accurate than that of say the Family Expenditure Survey (FES) or the General Household Survey (GHS). Atkinson and Micklewright (1983) have shown that incomes are not always very reliable in the FES even though the FES is specifically aimed at collecting accurate income and expenditure data. See Chapter 2 for further discussion.
    ${ }^{45}$ We also checked residual plots for irregularities/heteroskedasticity for which no evidence was found.

[^93]:    ${ }^{46}$ The latter two papers also draw attention to the similarities between SIMEX and jackknife estimation.

[^94]:    ${ }^{47}$ Note that the original data set is contaminated as well since it contains the surrogate covariate, $z$.

[^95]:    ${ }^{48}$ Cook and Stefanski (1994) perform a Monte Carlo study of the SIMEX estimator with a relatively small number of simulations $(B=50)$. They also present results from an application with real data (taken from the Framingham Heart Study) with twice as many simulations.

[^96]:    ${ }^{49}$ For example, for $\lambda \in[0,2]$ we perform 250 simulation for the following values of $\lambda: 0.0,0.2,0.4,0.6$, $0.8,1.0,1.2,1.4,1.6,1.8$, and 2.0 .
    ${ }^{50}$ We have chosen 250 simulations after trying $50,100,200,250,350$, and 500 simulations respectively. The changes in the resulting average parameter estimates were negligible between 250,350 and 500 iterations.

[^97]:    ${ }^{51}$ Cook and Stefanski (1994) show that in simulation studies the best bias corrections are obtained from the non-linear extrapolant function. They use the linear, quadratic and non-linear extrapolant and the order of bias correction is non-linear $>$ quadratic $>$ linear.

[^98]:    ${ }^{52}$ Recall that the naive estimate for $\log I S$ is 0.301 .

[^99]:    ${ }^{1}$ As before, and for the same reasons (see Chapter 2), only adult non-pensioners are considered (i.e. men aged between 18 and 65 , and women aged between 18 and 60 ).
    ${ }^{2}$ For a discussion, see for example, Atkinson (1991 \& 1993).

[^100]:    ${ }^{3}$ See our discussion in Chapter 1 or Evans (1996).
    ${ }^{4}$ This example is based on the well known example provided by Ben-Porath (1973) with respect to female labour supply, as quoted by Hsiao (1986).

[^101]:    ${ }^{5}$ Recall that eligibility is a necessary but not sufficient condition for being entitled to IS. Determining eligibility is relatively simple as compared to determining entitlement. An eligible individual can have a negative IS entitlement and thus be not entitled to IS. For a more detailed discussion see Chapter 2.
    ${ }^{6}$ One of the problems with category (iii), i.e. fraudulent use of the IS system, is that respondents are perhaps somewhat less likely to reveal the fact that they are in receipt of IS when they know that they are not actually entitled to it.

[^102]:    ${ }^{7}$ In contrast, expenditure take-up rates are based on the total amounts of IS claimed an unclaimed. However, the simple caseload take-up rate is the most commonly encountered measure of take-up. Nevertheless, more recently computing take-up rates has become an issue of contention. For example, Fry and Stark (1993) have argued for what they believe to be a more accurate caseload take-up rate which excludes all NEs in receipt of IS, i.e. take-up $=E R \div(E R+E N R)$. Duclos (1992) and Harris (1994) have argued for take-up rates which include an indication for sampling and calculation errors in determining ENRs. For further discussion see Chapter 2.
    ${ }^{8}$ A precise comparison of estimates is strictly speaking not possible, since they are based on different data sources. The DSS uses administrative data to assess the number of ERs and the Family Expenditure Survey to estimate the number of ENRs up to and including 1992. The combined estimate for 1993/94 uses the Family Resources Survey to estimate the number of ENRs. In addition, since 1990, the DSS method of computing take-up rates attempts to take into account the scope for a variety of errors (e.g. mis-reporting, sampling errors etc.) so that, rather than producing a single point estimate of take-up, take-up intervals are produced instead. The DSS figures reported above give the lower bound of these intervals and as such, give the 'worst case' scenario. The actual intervals are: 84-93

[^103]:    percent in 1992, and 87-96 percent in1993/94. Finally, the DSS gross-up their estimates, something we do not do to our estimates.
    ${ }^{9}$ As described in our IS algorithm of Chapter 2, highly detailed information is required to establish an individual's entitlement to IS. Such data are only provided on an annual basis in the BHPS.

[^104]:    ${ }^{10}$ For details see Chapter 4.
    ${ }^{11}$ Consider for example the case of an individual who actually receives IS. Suppose also that our IS algorithm (correctly) computes a positive IS entitlement for this individual. Now suppose that, for some reason or another, this individuals does not report receipt of IS. Since our sample sizes are very small, several such misclassifications will have a large impact on the computed percentages.

[^105]:    ${ }^{12}$ See Chapter 3 for an economic exposition.

[^106]:    ${ }^{13}$ Note however that due to missing responses there are only 97 respondents remaining at wave B. It might of course be possible that non-respondents would have classed themselves into the two bottom categories.
    ${ }^{14}$ The concept of regression to the mean was first observed by Galton in the 19th century studying father-son height relationships. He observed that tall fathers tended to have tall sons, but even though these sons were taller than average, they were not as tall as their fathers. See Stigler (1986) for a detailed account of Galton's work, and Healy and Goldstein (1978) on regression to the mean.

[^107]:    ${ }^{15}$ When considering future financial expectations, we include not only movements into proxy status but also those who responded with 'don't know'. This enables us to retain slightly larger sample sizes than would otherwise be the case.

[^108]:    ${ }^{16}$ In panel data models we are usually faced with a situation in which $N$ is considerably greater than $T$ (as is the case in our application). Hence, we are particularly interested in an estimator's asymptotic properties for fixed $T$ and $N \rightarrow \infty$.
    ${ }^{17}$ In essence, the reason for this is that when the individual-specific effects are treated as random variables this produces correlations amongst the error terms. The multivariate logistic distribution (on which the logit model is based) is too restrictive in such a case since it limits all correlations to be 0.5 . The probit model is based on the multivariate normal distribution for which this is not the case.

[^109]:    ${ }^{18}$ Panel data models can include a time-specific effect which we assume here to be constant.

[^110]:    ${ }^{19}$ For a detailed discussion of the method see Butler and Moffitt (1982). A more elaborate exposition of Gaussian quadrature can be found in Stroud (1974).

[^111]:    ${ }^{20}$ The sample drawn is adjusted so as to be comparable to the BHPS sample from which all entitled individuals are drawn (see section 5.4 for details).
    ${ }^{21}$ These characteristics act as the information set available to the individual.

[^112]:    ${ }^{22}$ For surveys of the self-selection problem in econometrics see Dhrymes (1986), Heckman (1979, 1987 \& 1990), Manski (1989), and Greene (Ch. 22, 1993).

[^113]:    ${ }^{23}$ In addition, it is possible to obtain consistent estimates of $\sigma_{\xi}^{2}$ and $\rho$ (see Greene (1981)).
    ${ }^{24}$ As before, we use the logarithm of IS since we expect this increase to occur at a diminishing rate.

[^114]:    ${ }^{25}$ In Table 5.1 the panel data set consists of 1,201 observations. The same data set is used in the regression analysis. The difference in the number of observations arises as a result of 5 observations with missing explanatory variables which were not used in the regressions.

[^115]:    ${ }^{26}$ For further discussion of initial conditions in dynamic panel data models see Hsiao (1986).

[^116]:    ${ }^{27}$ Attrition, however, will still remain a problem.

[^117]:    Source: National Welfare Benefits Handbook (1991, 1992, 1993 \& 1994).
    Note: 1. All figures in $£ / \mathrm{wk}$.
    2. Figures in parentheses apply from October 1991 and October 1992 respectively.

[^118]:    Note: $\quad$ 1. $\beta$ gives the heteroskedastic tobit estimate and Sigma the estimated heteroskedastic term.
    2. gross wage, private pension and rent receipt all scaled by 100 and measured in $£ / w k$.
    3. Asymptotic standard errors in parentheses.

[^119]:    $\dagger$ Proportion of samples with savings income. All statistics are computed using these samples.
    Note: 1. All figures in $£ / \mathrm{wk}$
    2. Predictions are based on $E\left(y^{*}\right)=$ expected latent variable, $E\left(y \mid y^{*}>0\right)=$ conditional expectation, $E(y)$ $=$ unconditional expectation.

[^120]:    Note: Predictions are based on expected savings conditional on savings falling into the range $£ 3,000-£ 8,000$ (sec main text).

[^121]:    Source: CSO Financial Statistics (Table 13.12)
    Note: Average mortgage rates quoted refer to the month prior to the sampling month.

[^122]:    ${ }^{1}$ For SB the following six studies are considered: (i) Altman (1981), (ii) Fry and Stark (1989) on nonpensioners and (iii) on pensioners, (iv) Fry and Stark (1993) on men and (v) on women, and (vi) Duclos (1992). For HB the following five studies: (i) Blundell et al. (1988) on retired/unoccupied and (ii) employed/unemployed, (iii) Fry and Stark (1993) on men and (iv) on women, and finally Dorsett and Heady (1991) on HB only.

[^123]:    $\dagger$ See main text for definitions of zero-order approximation and second-order approximation estimators.
    $\ddagger$ Corrected bootstrap standard errors.

    * In the regressions involving the approximation estimators the predicted $\log$ IS is used (see main text).

    Notes: 1. Number of observations for all three regressions $=1,199$.
    2. Log IS measured in $£ / \mathrm{wk}$, income in $£ / \mathrm{wk} \div 50$, and age in decades.
    3. The signs in parentheses indicate the direction of the absolute change relative to the naive estimate.

[^124]:    Note: Tables give estimated extrapolant functions used to predict the SIMEX estimate at $\lambda=-1$.

[^125]:    Note: ' 0 ' indicates non-take-up and ' 1 ' indicates take-up of IS.

[^126]:    Notes: 1. Subjective categories for financial situation compared to last year: $+=$ better off, $-=$ worse off, $0=$ about the same.
    2. $\mathrm{ENR}=\mathrm{entitled}$ non-recipients, $\mathrm{ER}=$ entitled recipients, $\mathrm{NE}=$ not entitled.
    3. Number of observations at wave $\mathrm{C}=94$.
    3. Total number of observations decline from wave to wave due to missing responses. Hence row and/or column percentages do not neccessarily sum to 100 .

