Estimating the impact of a policy reform on welfare participation: The 2001 extension to the Minimum Income Guarantee for UK pensioners

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ABSTRACT

In 2001 the Minimum Income Guarantee for UK pensioners was reformed, changing the structure and level of benefits. We evaluate the behavioural response to this reform, using nonparametric analysis of data on some pensioners interviewed before and others after the reform, using matching on simulated pre- and post-reform entitlements and other characteristics. We consider the effect of measurement error in simulated entitlements and argue that nonparametric and conventional parametric approaches are complementary, since they bound the true reform effect in simple cases. The take-up response is found to be significant and positive, with evidence of larger impacts from the nonparametric analysis.

NON-TECHNICAL SUMMARY

Evidence suggests that a substantial portion of individuals entitled to receive welfare benefits do not claim them, thus compromising the effectiveness of government programmes designed to reduce poverty. Take-up is particularly low for means-tested benefits requiring an evaluation of income and assets of the claimant. Existing qualitative research on welfare participation emphasises claim costs arising from the difficulty and hassle of making a claim and other intangible costs such as distaste for welfare participation and social stigma associated with dependence on benefits. The phenomenon seems to be particularly severe for British pensioners. Official estimates report that, although approximately 2 million pensioners were living in low income households in 2000-01, between a third and a quarter of them did not claim the Minimum Income Guarantee payments to which they were entitled. It has been suggested that pensioners experience more difficulties than others in acquiring information and pursuing a claim.

Most economic analyses of take-up behaviour have considered claiming as a rational choice based on a comparison of the expected benefits from welfare participation with the tangible and intangible costs of applying, so that the individual chooses to search for information and make a claim if the expected benefit adequately compensates for the costs. Typical research studies use individual-level survey data on income and asset holdings to simulate benefit entitlements and, for those believed to have positive entitlements, a statistical model is estimated for the probability of take-up of the entitlement. A well established result in the literature has been the positive impact of the benefit entitlement level on the claiming decision. In other words, sufficiently large levels of entitlements work as an incentive for more people to overcome the claim barriers.

This standard approach, involving modelling of the takeup probability, has some drawbacks: particularly the risk of misspecifying the statistical model and of measurement error in simulated entitlements, arising from the unreliable nature of survey responses on income and assets. The main aim of this paper is to test directly whether there is a response of takeup behaviour to incentives, using an approach that is less vulnerable to specification and measurement error. We examine a 2001 policy reform, which substantially increased the Minimum Income Guarantee entitlements levels and relaxed eligibility criteria. We try to identify the effect of this increase in entitlement on the take-up behaviour of older British pensioners by comparing the benefit receipt of otherwise similar pensioners from the pre- and post-reform periods.

We find that the take-up of the Minimum Income Guarantee was significantly increased by the 2001 reform for those with the largest potential gains from claiming. These results support the idea that higher entitlement levels do provide an effective incentive for welfare participation and they are reassuringly close to results previously obtained using the conventional statistical modelling approach.

1 Introduction

The evidence concerning people who do not claim welfare benefits to which they are entitled has long animated the economic policy debate on the design of income maintenance programmes. A better understanding of non-take-up and the implied effect of policy design on take-up rates would contribute to the development of more effective policies to reduce poverty, to improvements in the simulation of policy reforms, and in forecasting the public expenditure associated with these policies. The issue mainly concerns means tested benefits, which require an evaluation of the income and assets of potential claimants. Existing qualitative research (Costigan *et. al.*, 1999) suggests that welfare participation involves some claim costs arising from the actual difficulty and hassle of making a claim and other intangible costs such as the social stigma associated with dependence on welfare benefits. Pensioner take-up behaviour is particularly uncertain, since this vulnerable group may face more difficulty than others in acquiring information and pursuing a claim.

There is a large non-economics literature on the take-up issue (see Kerr, 1982; Hirsch and Rank, 1999; Kayser and Frick, 2001; Castranova *et. al.*, 2001) exploring various aspects of behaviour. Most economic analyses of take-up behaviour have considered claiming as a utility maximizing choice (see Moffitt, 1983; Blundell, Fry, Walker, 1988; Duclos, 1995; Anderson and Meyer, 1997; Bollinger, 1997; Pudney *et. al.*, 2007; Hernandez *et. al.*, 2007). The individual compares expected benefits from claiming with the inherent costs of applying, and chooses to claim only if the expected benefit adequately compensates the costs. The typical econometric approach consists in simulating benefit entitlements and, for those believed to have positive entitlements, modelling parametrically the probability of benefit receipt. This standard approach has some drawbacks, including the risk of misspecification of the underlying parametric model and bias caused by error in simulating entitlements.

In this paper, as an alternative approach, we use a policy change to identify the impact of variation in entitlement on take-up behaviour, following a non- parametric approach which avoids the necessity of specifying a functional form. This reform involved the Minimum Income Guarantee (MIG), which is the main means-tested income support scheme available to pensioners in Britain. It generated a substantial real increase in the MIG level, an increase in the allowable level of assets which claimants can have before losing entitlement, and a modification of the system of age additions. We consider a set of pensioners interviewed in the Family Resources Survey before the changes were introduced and another set of pensioners interviewed after the reform came into force.

We use a nonparametric matching approach ("matching in variables" rather than "propensity score matching"). For each group, we simulate the pair of MIG entitlements under the pre- and post-reform systems. Members of the two sample groups are matched according to their entitlement pairs and other characteristics, allowing us to identify the behavioural response to the reform. The nonparametric approach reduces the scope for misspecification and we also argue that, in the presence of measurement error, methods based on nonparametric matching and parametric modelling are likely to be biased in opposite directions, giving bounds on the true reform effect.

The paper is organized as follows: Section 2 explains the MIG system and the 2001 Reform and describes trend in MIG claims over the relevant period. Section 3 describes the data we use, the measures taken to minimise the impact of measurement error and the method of simulating of entitlemnts. Section 4 sets out the matching methodology, section 5 gives the results of the analysis. Section 6 presents an alternative ex-ante evaluation method and section 7 makes a comparison with the results of the parametric approach. Section 8 concludes.

2 The 2001 Minimum Income Guarantee reform

Income Support is a means-tested, non-taxable and non-contributory welfare programme designed for people on low income. Since 1999, when particular rates were established for people aged 60 and over, it has been named the Minimum Income Guarantee (MIG) when claimed by people over 60. In April 2001 the MIG scheme was reformed to increase its generosity and simplify its structure. The unit of assessment for the MIG is the pensioner unit: a single pensioner or a couple where at least one is a pensioner. People are considered to be a couple if married or if living together as if married. For eligibility, the claimant must be 60 or over, not working more than 16 hours a week and not living with a partner working more than 24 hours a week. The scheme works by topping up income to a guaranteed level, which depends on personal circumstances. The awarded amount is then the difference between needs, as reflected by the guaranteed level, and assessable income, calculated from the claimant's incomes and capital, according to predetermined rules.

The guaranteed level is a basic allowance, different for singles and couples, plus housing costs and any premium awarded in consideration of particular circumstances like disability and (in the pre-reform scheme) age. Before April 2001, there was a system of age-additions to the MIG: a "pensioner premium" for single people aged 60-74 and for couples with at least one aged 60 or over and both under 75; a higher "enhanced pensioner premium" for single people aged 75-79 and for couples with at least one aged 75-79 and both under 80; a "higher pensioner premium" for single people and people living in a couple when aged over 80 (or if aged 60-79, if receiving a disability benefit such as Attendance Allowance, Disability Living Allowance, Severe Disablement Allowance, the long term rate of Incapacity Benefit or if registered as blind).

Table 1 shows rates of MIG allowances and premiums, in £ per week, before and after the April 2001 reform. To calculate assessed income, both income and financial assets have to be considered. In the pre-reform system, eligibility is lost when assets exceed £8,000.[1] If assets are below £8,000, MIG can be claimed but a notional "tariff income" of £1 for every £250 of assets between £3,000 and £8,000 is added to net earnings, pensions and some state benefits to calculate assessed income. Actual returns from capital and some benefits, including Housing Benefit, Council Tax Benefit, Attendance Allowance and the mobility and care components of Disability Living Allowance are not taken into account. Some other elements of income are also disregarded (see CPAG, 2000 for full details). Finally, if the difference between the applicable amount and the assessed income is positive, its amount represents the MIG payment to which the pensioner unit is entitled. However, payment of the MIG is not automatic and entitlements must be claimed by filling in and submitting a detailed claim form. In April 2001, a more generous scheme was introduced, involving a significant real increase in

[1] For this purpose, assets include cash, bank and building society accounts, National Savings accounts and certificates, premium bonds, stocks and shares, property (other than the main residence). The surrender value of life assurance policies, the arrears of some benefits (Attendance Allowance, Disability Allowance or Income Support for 52 weeks since first received) and personal possessions (if not bought to decrease the amount of savings) are excluded.

the benefit level, the elimination of the age additions and an increase in the allowable level of assets, with the eligibility thresholds raised to £6,000 and £12,000 (see CPAG, 2001, for full details). As a consequence of this reform, more people were entitled to, and likely to claim, the MIG.

***** TABLE 1 HERE *****

Implementation of the reform was preceded, in May-November 2000, by a national publicity campaign designed to raise awareness of the MIG. As part of this campaign, 2.4 million pensioners were contacted by post, but the campaign was not specifically linked to the reform. Attempts to evaluate this campaign concluded that "most low-income pensioners still have very little knowledge about the benefits that are available [...] the most important source of information on benefits for pensioners was friends and relatives, rather than official sources" (CAG, 2002). The finding of a weak effect of this publicity campaign is supported by the observation that previous attempts to raise awareness have had little impact on the trend in take-up rates. However, suppose the campaign did have a positive effect on take-up, as illustrated in Figure 1. Assume that the response to the increase in awareness occurs within 5 months (as is likely, since claim processing times are generally much shorter) and that the raised level of awareness persists throughout the post-reform period. Then the comparison between the pre-reform (April 2000-March 2001) and post-reform (April 2001-March 2002) periods is only distorted to the extent that, for the one pre-publicity month (April 2000), there was a lower take-up propensity than for the rest of the pre-reform period. This will generate a small upward bias in the apparent reform effect. However, this bias would be offset to some degree if there is some decay in awareness after the end of the publicity campaign.We return to this issue later in section 6.

***** FIGURE 1 HERE *****

A further issue is the possibility of an anticipation effect, which produces a downward bias in estimates of the impact of a pre-announced policy reform. The 2001 MIG reform was mixed in this sense. One of its components, the change in capital limits, was announced a year in advance in the April 2000 budget. This had had two effects: removing the ineligibility of some people who were previously over the £8,000 asset ceiling; and increasing slightly the entitlement levels for people with assets between £3,000 and £8,000. Our analysis focuses on people who were eligible both before and after the reform, so pre-announcement will not affect our results as far as the former group is concerned. Moreover, the large number of MIG-entitled pensioners below the £3,000 limit were not affected at all by the change in asset limit nor, therefore, its pre-announcement. The other components of the reform - a large real increase in benefit levels and a changed structure of age additions - were only announced in the Chancellor's October 2000 statement, six months before the change took effect. In our view, anticipation effects were unlikely to have been significant for the group covered by our analysis.

Published estimates (DWP, 2004) of the numbers of pensioners receiving MIG, together with the numbers of entitled non recipients and take-up rates are presented in table 2. The evident fall in the take-up rate after the April 2001 reform cannot be directly interpreted in terms of incentive effects, since the reform not only increased the entitlements of people who were already entitled pre-reform, it also brought into the MIG system for the first time many people whose new entitlement levels were small. The effects of the reform at these intensive and extensive margins are likely to have acted in opposite directions in terms of their impact on the overall take-up rate[2]. The growth in MIG caseload after April 2001, together with the fall in the estimated take-up rate, clearly demonstrates the importance of the extension of entitlement, which is absent from the IS programme.

***** TABLE 2 HERE *****

[2]Figures plotting the trend in the number of recipients of the MIG programme in comparison to that of the similar Income Support programme applicable to non-pensionersare reported in Pudney et al.(2006).

3 The Data

3.1 The Family Resources Survey

The Family Resources Survey (FRS) is a repeated cross section study covering private households in Great Britain. It is carried out on behalf of the Department for Work and Pensions with the aim of providing information to monitor social security programmes and related public expenditure. It provides detailed information at the personal level on income from different sources, tax payments and refunds, national contributions, benefit receipts, assets, savings and investments. The survey thus allows, for each benefit unit, the assessment of entitlement to MIG in the year considered, providing at the same time information about the take-up of the benefit and the actual amount received.

Between April 2000 and March 2001, 23,790 private households were interviewed, corresponding to 28,093 benefit units and 55,801 individuals. Between April 2001 and March 2002, 25,320 households, corresponding to 30,037 benefit units and 59,499 individuals, successfully completed the interview. From the whole samples, only pensioner benefit units consisting of single people aged over 60 or couples with at least one partner aged over 60 are relevant for further analysis. We make further sample exclusions by deleting: single people aged less than 5 years above the state pension age; couples with either partner aged less than 5 years above the state pension age; and benefit units with labour market income or living in households containing multiple benefit units or repaying a mortgage or receiving allowances from an absent spouse. The purpose of these exclusions is to remove benefit units with the option of deferring drawing their state pensions and those for whome measurement error is most likely. We also exclude respondents providing insufficient information for the evaluation of their entitlement to MIG. The samples are then subjected to an error detection and correction procedure with internal coherence checks, to reduce further the scope for measurement errors. These cleaning procedures are described in detail by Hancock and Barker (2005).

3.2 Simulation of the MIG entitlements

Simulation of the MIG entitlement requires calculation of the financial assets held by the benefit unit: if this is above the upper capital limit, the benefit unit is automatically ineligible to MIG and is omitted from the analysis. Otherwise, a 'tariff income' is calculated from the amount of capital above the lower capital limit. The income guarantee level is then identified according to age, disability status and whether the unit is single or living as a couple. Assessed income is then computed as the sum of income from all assessable sources and the tariff income from capital. Finally the difference between assessed income and the guarantee level is computed. If positive, it represents the MIG entitlement for the pensioner unit. If negative, the pensioner unit is ineligible to MIG and the unit is excluded from the analysis. For further details, see CPAG(2000, 2001).

To identify the behavioural response to the 2001 MIG reform, we use a matching procedure which compares observed take-up for people interviewed in different years, but who would have faced a similar pair of pre- and post-reform entitlements. This requires us to evaluate two entitlements for each pensioner unit: actual entitlement in their year of interview and the entitlement they would have had, if assessed under the MIG system of the 'other' year. The simulation is made under constant prices to remove the effect of automatic indexation of benefit rates. It is important to note that the simulation of the MIG entitlement is not compromised by simultaneous entitlements to other benefits since the MIG entitlement can be calculated independently. We only include in the analysis pensioner units with simulated entitlements above £1 per week under both systems.

The final sample used in the statistical analysis consists of 845 benefit units (80.9% singles, 18.1% couples) observed in 2000/2001 and 756 (83.6% singles, 16.4% couples) observed in 2001/2002. In both years the vast majority of single pensioners are women (85.6% in 2000/2001 and 84.5% in 2001/2002).

3.3 FRS evidence on new applications for the MIG

The FRS provides some direct evidence on the generation of new MIG claims. Interviewees were asked whether they were awaiting the outcome of an application for the MIG[3]. It should be emphasised that the sample numbers involved here are very small indeed, but there is a raised level of pending applications for pensioners around the time of the reform in April 2001. No such peak is evident for new IS applications. It should be noted that this post-reform peak in the number of applications does not necessarily reflect only an increase in the take-up of the benefit, since the reform extended the coverage of the MIG programme as well as making it more generous for those already entitled. Nevertheless, it is evidence of a response to the reform. We now attempt to separate the pure take-up response by analysing in more detail the set of pensioners who were entitled under both versions of the MIG system.

4 Statistical analysis of the reform

To identify the effect of the 2001 MIG reform on the take-up behaviour of eligible individuals requires the comparison of MIG-entitled pensioners observed in the 2000/1 FRS with a comparison group from the 2001/2 FRS. This comparison is not straightforward because, for any observed pensioner, we have only a single observation of take-up behaviour under a single benefit regime. Thus, for pensioners observed before the reform, their take-up behaviour under the new regime is unobserved (and conversely for those observed after the reform). This is essentially the same problem of an unobserved counterfactual that occurs in the standard Roy-Rubin approach to the evaluation problem (Roy, 1951; Cochran and Rubin, 1973).

We use the following notation. The set of observable characteristics of the pensioner unit in year t is X_t, where $t = 0, 1$ denotes the 2000/1 and 2001/2 fiscal years. B_t^r denotes the unit's (simulated) MIG entitlement that would result if benefit regime r is in force ($r = 0$ or 1) and their characteristics are X_t. The binary variable T_t^r indicates the corresponding take-up behaviour, where $T_t^r = 1$ indicates take-up and $T_t^r = 0$ indicates non-take–up. A binary variable R_t indicates whether the unit would be a respondent ($R_t = 1$) or non-respondent ($R_t = 0$), if approached for interviewing in the FRS of year t. Then, in the FRS sample in year t, we observe $\{X_t, B_t^0, B_t^1, T_t^t\}$ if $R_t = 1$

[3]Figures showing the number of respondents who were waiting, on a monthly basis over the period January 2000-December 2001 (and for comparison, the corresponding figure for non-pensioner IS applicants) are reported in Pudney et al.(2006).

and nothing otherwise. The potential take-up behaviour that would occur under the "other" year's MIG rules (T_0^1 and T_1^0) are never observed.

It only makes sense to assess the reform-induced change in take-up behaviour for those who have a positive entitlement under both the pre- and post-reform MIG rules. Given this, there are two natural definitions of the average impact of the reform on take-up:

$$\Delta_0 = E\left(T_0^1 - T_0^0 | B_0^0 > 0, B_0^1 > 0\right) \quad (1)$$

$$\Delta_1 = E\left(T_1^1 - T_1^0 | B_1^0 > 0, B_1^1 > 0\right) \quad (2)$$

These differ only in the choice of base year distribution of X used to construct entitlements.

4.1 Analysis without matching

The difference in the crude take-up rate between MIG-entitled respondents in the FRS 2000/1 and the analogous group in the FRS 2001/2 is a consistent estimate of the following population parameter:

$$\Delta = \frac{E\left(R_1 T_1^1 | B_1^0 > 0, B_1^1 > 0\right)}{E\left(R_1 | B_1^0 > 0, B_1^1 > 0\right)} - \frac{E\left(R_0 T_0^0 | B_0^0 > 0, B_0^1 > 0\right)}{E\left(R_0 | B_0^0 > 0, B_0^1 > 0\right)} \quad (3)$$

Our sample estimate of the crude difference (3) is:

$$\widehat{\Delta} = \frac{1}{n_1} \sum_{i \in S_1} T_{1i} - \frac{1}{n_0} \sum_{i \in S_0} T_{0i} \quad (4)$$

where S_0 and S_1 are the sets of respondents in the 2000/1 and 2001/2 FRS pensioners samples, whose pre- and post-reform simulated entitlements are both positive; n_0 and n_1 are the sample sizes. Table 3 below summarises the results, together with average entitlements and take-up rates in the two years.[4] These unmatched differences suggest a general increase in the post-reform take-up rate, although a decrease is found for the oldest group. Table 3 shows no clear pattern of take up behaviour for increasing levels of the

[4]Comparing these figures with published DWP estimates for all pensioners, the takeup rates in the subset of pensioners included in our analysis are lower than the DWP figures, which are in the range 68-76% for 2000/1 and 63-72% for 2001/2.

post reform change in mean entitlement. Some groups show a striking increase in the take-up rate despite a low increase in the average entitlement, whilst others display a significant increase in post reform entitlement with no accompanying increase in the post reform take-up rate.

***** TABLE 3 HERE *****

The implicit assumption underlying analysis of empirical take-up rates is that survey nonresponse is ignorable in the following sense.

Assumption 1 $R_t \perp T_t^t | B_t^0 > 0, B_t^1 > 0, \quad t = 0, 1$

where $\perp$ denotes statistical independence. Under this assumption, (3) simplifies to:

$$\Delta = E\left(T_1^1 | B_1^0 > 0, B_1^1 > 0\right) - E\left(T_0^0 | B_0^0 > 0, B_0^1 > 0\right) \tag{5}$$

In general, this is not equal to Δ_0 or Δ_1. Instead, Δ can be written in either of the following forms:

$$\Delta = \Delta_1 + \left[E\left(T_1^0 | B_1^0 > 0, B_1^1 > 0\right) - E\left(T_0^0 | B_0^0 > 0, B_0^1 > 0\right)\right] \tag{6}$$

$$\Delta = \Delta_0 + \left[E\left(T_1^1 | B_1^0 > 0, B_1^1 > 0\right) - E\left(T_0^1 | B_0^0 > 0, B_0^1 > 0\right)\right] \tag{7}$$

The bias term in square brackets in (6) or (7) summarises the impact on the take-up rate, under the old or new benefit regime respectively, of the change in the distribution of X, B^0, B^1 that occurred between years 0 and 1.

There are two obvious shortcomings of an estimator based on (3). Firstly, the assumption of unconditionally ignorable nonresponse for the benefit-entitled population is unduly strong. It is well known, for example, that response rates in household surveys tend to vary with economic circumstances of the household (Lynn *et. al.*, 2005). Secondly, the unmatched comparison of respondents from different survey years introduces an additional confounding term that reflects changes in the distribution of pensioner characteristics over time. Both of these may lead to avoidable bias.

4.2 Analysis of matched samples

Define $W_t = (X_t, B_t^0, B_t^1)$ to be the set of observable influences on take-up behaviour. Conditional on a particular value for W, the change in take-up rates between periods 0 and 1 is:

$$\Delta^*(w) = \frac{E\left(R_1 T_1^1 | W_1 = w\right)}{E\left(R_1 | W_1 = w\right)} - \frac{E\left(R_0 T_0^0 | W_0 = w\right)}{E\left(R_0 | W_0 = w\right)} \tag{8}$$

where $w \in S$ and S here is the subset of the support of W in which $B^0 > 0$ and $B^1 > 0$ are satisfied. Now weaken assumption A1 to require only ignorability of non-response conditional on W:

Assumption 1* $\quad R_t \perp T_t^t | W_t = w$, for $t = 0, 1$ and all $w \in S$

Then assumption 1* implies $\Delta^*(w) = E\left(T_1^1 | W_1 = w\right) - E\left(T_0^0 | W_0 = w\right)$.

Make the further assumption that, for any given set of personal characteristics (X) and benefit rules (B^0, B^1), the mean take-up rate is unchanging over time:

Assumption 2 $\quad E\left(T_t^r | W_t = w\right)$ is independent of t for all $w \in S$ and for each benefit regime $r = 0, 1$

Assumption 2 rules out confounding macro-level changes besides those already reflected in W_t. This assumption might be questionable if the reform happened to coincide with other unobservable or unquantifiable changes, for example in the application procedure or in social attitudes. In such cases, the result will be an estimate of the combined change in take-up caused by the reform itself and the other contemporaneously varying factors.

Under assumptions 1* and 2, the conditional change in the take-up rate (8) is expressible in either of the following two forms:

$$\Delta^*(w) = E\left(T_1^1 - T_1^0 | W_1 = w\right) \tag{9}$$

$$\Delta^*(w) = E\left(T_0^1 - T_0^0 | W_0 = w\right) \tag{10}$$

This in turn implies that (1) and (2) can be written:

$$\Delta_t = \int_S \Delta^*(w) dF_t(w | w \in S) \quad , \qquad t = 0, 1 \tag{11}$$

where F_0 and F_1 are the cross-section distributions of W in periods 0 and 1.

Since the vector W_t contains continuous variables, it is not generally possible to implement the conditioning in (9)-(10) exactly in the estimation process. To overcome this problem, we use a matching approach, which pairs together individual respondents in the pre- and post-reform samples. This is similar in spirit to propensity score matching (Rosenbaum and Rubin, 1983), but we match on the vector W rather than a propensity score. From the viewpoint of the evaluation literature, the unusual feature of this application is that there is no possibility of bias stemming from the allocation of individuals to pre-reform and post-reform samples, since this is essentially random as a consequence of the FRS design. However nonresponse is a potential confounding factor whose impact is reduced by matching.

We use a nearest-neighbour matching algorithm, based on observables covering: a set of discrete demographic characteristics (sex, age group, marital status, disability status) and the MIG entitlements B_t^0 and B_t^1. The first step of the algorithm is stratification, which acts as a first adjustment for confounding variables. The year 0 and year 1 samples (analogous to control and treatment cases respectively) are divided into nine mutually exclusive subclasses, indexed by k, according to their demographic characteristics. The stratification partitions the sets of respondents with positive entitlements, S_0 and S_1 so that

$$S_t = \bigcup_{k=1}^{9} S_{tk} \ , \qquad t = 0, 1$$

Take year 0 as the baseline.[5] For each individual i within stratum S_{0k}, choose an appropriate match from the same stratum in year 1 (S_{1k}). The criterion for matching is distance minimization, so the matched individual $\widetilde{j}(i) \in S_{1k}$, satisfies

$$D(i, \widetilde{j}(i)) \leq D(i, j) \quad \forall j \in S_{1k} \tag{12}$$

where $D(i, j)$ is a distance function based on a comparison of $P_{0i} = (B_{0i}^0, B_{0i}^0)$ for case i in the year 0 sample with $P_{1j} = (B_{1j}^0, B_{1j}^0)$ for case j in the year 1 sample. We use the Mahalanobis distance measure (Rubin, 1980; Abadie *et. al.*, 2001):

$$D(i, j) = (P_{0i} - P_{1j})' V^{-1} (P_{0i} - P_{1j}) \tag{13}$$

[5]The estimation problem is symmetric, so we can repeat this with year 1 as baseline.

where V is the pooled within-sample covariance matrix of P_{0i} and P_{1j} based on the subsamples of treated and non treated individuals. Matching is performed with replacement, to ensure the closest possible match.

We also explore several modifications of this algorithm. One is to avoid stratification. Another is to exclude the possibility of very poor matches, using a caliper to reject matches which breach the following requirement:

$$D\left(i, \widetilde{j}(i)\right) \leq \epsilon \tag{14}$$

where ϵ is a pre-set critical value. Individuals i for whom there is no match satisfying (14) are dropped from the comparison. This has the effect of reducing the range of pensioner types over whom the impact of reform can be estimated. By improving match quality, it also reduces the bias caused by imbalances in the covariate distributions, at the cost of an increase in variance.

The estimator of the change in take-up for a particular stratum k is computed as

$$\widehat{\Delta}_k^* = \frac{1}{n_{kt}} \sum_{i \in M_{kt}} \left[T^1_{1\widetilde{j}(i)} - T^0_{0i}\right] \tag{15}$$

and M_{kt} is the set of n_{kt} individuals in stratum k in year t, for whom a match can be found. These can be combined into an overall estimator of the reform-induced change as follows:

$$\widehat{\Delta}_t^* = \sum_{k=1}^{9} \psi_{kt} \widehat{\Delta}_k^* \tag{16}$$

where ψ_{kt} is the relative size of stratum k in the baseline year t.

5 Implementation and Results

5.1 Matching estimates

The two covariates used for matching are the simulated MIG entitlements under the pre- and post-reform systems. When stratification is used, the strata are based on age group, sex, marital status and disability. Table 4 reports results for matching with different degrees of stratification. With no stratification, the estimated impact of the reform is an increase of around 9 percentage points, from a baseline average take-up probability of 0.6-0.65. This is statistically significant, both for matching the 2000/1 to 2001/2 pensioners and for the symmetric matching of the 2001/2 sample to 2000/1. A similar estimate is obtained using a range of caliper options. Again, the estimated reform-induced change in take-up is large: above 8 % and significant in all cases. For calipers of 0.05, 0.025 and 0.01 respectively, the proportion of discarded matches rises from 4.3% to 6.8% and 10.5% when 2000/1 characteristic are used (and from 2.8% to 4.5% and 7.7% when matching with 2001/2 characteristics is performed). There is a consequent increase in the standard error and the average number of times each 'control' is used, although this remains below 2.5. With stratification the average reform effects, $\widehat{\Delta}_0^*$ and $\widehat{\Delta}_1^*$, remain positive but decline in magnitude and progressively lose significance as the degree of stratification is increased.

***** TABLE 4 HERE *****

To give more detail on the role of entitlement as an influence on take-up behaviour, we can also perform the analysis separately for groups defined in relation to the size of the reform-induced increase in entitlement. Table 5 gives results with varying degrees of stratification, which suggest that higher MIG entitlement does indeed have an incentive effect on benefit take-up. The estimated impact of reform substantially increases when the reform-induced change in entitlement increases above £10 per week. For those gaining over £15, the estimated reform effect is estimated to be around 30 percentage points when little or no stratification is used, falling to 14-24 percentage points with 9 strata, from a baseline take-up rate of 24-40%. When finer demographic stratification is used, statistical significance becomes less clear. However, the estimated reform effect remains significant for those gaining the largest amounts and we continue to observe a pattern of response rising with potential gain.

***** TABLE 5 HERE *****

To evaluate the success of our matching strategy, Table 6 examines the balance in the mean values of the covariates in the matched samples. The mean values of the covariates for treated units can be compared both for the full control sample and the matched control sample. The difference between covariates means after matching appears negligible and the reported reduction in bias due to differences in sample characteristics for the comparison groups suggest that the matching procedure is a good one.

***** TABLE 6 HERE *****

The issue of common support is not straightforward in this case since matching is not implemented using a scalar variable like the propensity score. Instead, we evaluate matching performance in Figure 2, by plotting the percentage of matches whose Mahalanobis distance stays below the threshold τ as this increases. The pattern is sharply increasing, reaching 90% when τ is still below 0.05. This motivates our choice of caliper values in the range 0.01-0.05.

***** FIGURE 2 HERE *****

6 Ex-ante evaluation

The results of the previous section come from an orthodox *ex post* approach to policy evaluation. It might be argued that the effect we have found is representative of the short-term reform impact on the take-up behaviour, while people may take some time in adjusting to such policy changes (see Hernandez *et. al.*, 2007, for evidence of such delays). There is also a possibility that the previous results might contain an upward bias due to the 2000 publicity campaign. To overcome both of these potential shortcomings, we derive an alternative estimator of the reform effect in the context of an ex-ante evaluation (Todd and Wolpin 2005, Ichimura and Taber, 2000) using pre-reform variation in the data as an instrument for policy reform induced changes.

The implicit assumption underlying *ex ante* evaluation is that the individual's welfare participation decision rule is not altered as a consequence of the reform and therefore:

Assumption 3 $E(T_0^1|B_0^1 = b, X_0 = x) = E(T_0^0|B_0^0 = b, X_0 = x)$, for all $(b, x) \in S_0$

Together with Assumption 1*, this justifies an estimator of the form:

$$\widehat{\Delta}_0^{ex-ante} = \frac{1}{n^*} \sum_{i \in M^*} \left[T^0_{0\widetilde{j}(i)} - T^0_{0i} \right] \tag{17}$$

where $\widetilde{j}(i)$ is the identity of the sample individual who is most closely matched to individual i in terms of personal characteristics X_0 and in terms of the simulated entitlements (B^0_{0i}, B^1_{0j}); n^* is the number of such pairs in the matched set M^*.

Results obtained from this estimator, using the same nearest-neighbour algorithm based on Mahalanobis distance minimization and with different caliper and stratification options, are reported in table 7. We do not apply the method for the 9-strata comparison because the matching condition of assumption 3 could never be satisfied within each stratum in that case. The effect of the reform on eligible pensioners is positive and significant for all matching specifications and is robust to the imposition of higher matching quality through smaller calipers. The estimated average impact is much higher than in table 4, suggesting that, after pensioners have completed the adjustment to the policy change, their take-up response is higher than we observe *ex post* over a short horizon. Moreover, since $\widehat{\Delta}_0^{ex-ante}$ uses only pre-reform observations, sample members will be affected by the 2000 publicity campaign randomly according to their interview date, causing no systematic bias from the campaign effect.

***** TABLE 7 HERE *****

7 A comparison with the parametric approach

7.1 The probit estimator

The preceding results can be compared with those obtained from a standard parametric analysis. After estimating a probit model of take-up behaviour, we can predict the take-up probability for each pensioner unit in the non-observed year. The predicted change in the take-up rate between the pre-reform and the post-reform systems is then calculated, together with a confidence interval for the comparison. The probit model is written:

$$\Pr(T_i = 1|x_i) = \Phi(x_i\beta) \tag{18}$$

where x_i denotes the covariates, including variables reflecting benefit entitlement. The specification and parameter estimates for this probit model are given in appendix 2. They are representative of the results to be found in most of the applied literature on take-up.

The reform changes x_i from x_i^0 to x_i^1 and the take-up rate from $E\Phi(x_i^0\beta)$ to $E\Phi(x_i^1\beta)$ where expectation is taken with respect to the distributions of x_i^0 and x_i^1 among the entitled population. When i is sampled in 2000/1 two estimators (the first using the actual take up and the second using the predicted one for the observed period) of this change are defined as

$$\hat{\Delta}_0^a = \frac{1}{n_0}\sum_{i\in S_0}\left[\Phi(x_i^1\hat{\beta}_A) - T_{0i}^0\right] \tag{19}$$

$$\hat{\Delta}_0^p = \frac{1}{n_0}\sum_{i\in S_0}\left[\Phi(x_i^1\hat{\beta}_A) - \Phi(x_i^0\hat{\beta}_B)\right] \tag{20}$$

where T_{0i}^0 is the observed take-up in the pre reform period, S_0 is the set of observations in this sample with positive entitlement under both regimes and n_0 is the number of such cases. Analogous formulae apply to the 2001/2 sample. In (19)-(20), $\hat{\beta}_A$ and $\hat{\beta}_B$ are coefficient estimates. There are several possibilities: $\hat{\beta}_A$ might be estimated from the 2001/2 sample and $\hat{\beta}_B$ from the 2000/1 sample; another alternative is to use a single estimate from the pooled sample for both.

These estimates of Δ_0 and Δ_1 have two sources of error: sampling error in the sample averages; and parameter estimation error. Standard errors for each estimator can be derived taking account of both, as shown in appendix. Due to its use of a sample average of outcomes rather than the average predicted probability, the estimate $\hat{\Delta}_t^a$ will have lower precision than $\hat{\Delta}_t^p$; however, it will be affected differently by any misspecification bias that exists.

Estimated probit coefficients, estimated separately for singles and couple, are given in Appendix Tables A1-A3. Chow-type parameter stability tests give no evidence of misspecification in these models.

7.2 The impact of measurement error

Measurement error is an important issue for models of benefit take-up. Simulated entitlements may differ from actual or perceived entitlements because of measurement error in the income and asset levels reported in household surveys, or because of mistaken perceptions of potential claimants or errors in the administration of the MIG system by programme administrators. The resulting simulation errors may be quite large, despite the effort we have devoted to data cleaning.

Consider first the parametric model. The vector x contains a variable equal to the (log) simulated MIG entitlement under the system in force at the time of sampling. If this is subject to measurement error, there is a consequent large-sample bias in the estimated probit coefficients, leading to bias in the simulated reform effect. In the classical case of additive zero-mean random measurement error, one would expect to see an attenuation bias, with underestimation of the coefficient of the entitlement variable and thus the reform effect. However, the probit model is nonlinear and it is possible for the bias to be positive in certain circumstances (see Stefanski and Carroll, 1985). The matching estimator is also complicated, so the comparison between parametric and non-parametric approaches is not straightforward. To avoid these complications, we now consider a simple case in which it is possible to derive definite results.

Make the following simplifying assumptions: (i) survey nonresponse is random and independent of all other variables; (ii) a linear probability model of take-up is valid, with $E(T_t^r|W_t) = \alpha^r + \beta^r B_t^r$,[6] where B_t^r is now redefined as the log of entitlement under year t conditions and the rules of system r; (iii) B_t^r is observed with error as $B_t^{r+} = B_t^r + \varepsilon_t^r$, where the ε_t^r are zero-mean

[6]Note that assumption 2 rules out any dependence of α^r and β^r on t.

serially-independent measurement errors[7], independent of all other variables; (iv) measurement errors and log entitlements are Gaussian:

$$
\begin{aligned}
\varepsilon_t &= \begin{bmatrix} \varepsilon_t^0 \\ \varepsilon_t^1 \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_\varepsilon^2 & \rho\sigma_\varepsilon^2 \\ \rho\sigma_\varepsilon^2 & \sigma_\varepsilon^2 \end{bmatrix}\right) \\
W_t &= \begin{bmatrix} B_t^0 \\ B_t^1 \end{bmatrix} \sim N\left(\begin{bmatrix} \mu_t^0 \\ \mu_t^1 \end{bmatrix}, \begin{bmatrix} \omega_{00} & \omega_{01} \\ \omega_{01} & \omega_{11} \end{bmatrix}\right)
\end{aligned} \tag{21}
$$

where ρ is the correlation between the errors made in simulating entitlements under the two benefit systems at any time.

Under linearity, the true average reform effect with respect to period 1 characteristics is:

$$\Delta_1 = \left(\alpha^1 - \alpha^0\right) + \left(\beta^1\mu_1^1 - \beta^0\mu_{01}^0\right) \tag{22}$$

The analysis that follows relates to Δ_1 and an analogous argument applies to Δ_0. Define $W_t^+ = (B_t^{0+}, B_t^{1+})^T$ and consider $\Delta^*(w)$ given by (8). Under our assumptions, its observable counterpart is:

$$\Delta^+(w) = E\left(T_1^1|W_1^+ = w\right) - E\left(T_0^0|W_0^+ = w\right) \tag{23}$$

The corresponding estimate of Δ_1 is the integral of (23) over w with respect to the convolution density of W_1^+, $f_1^+(w) = \int g(w - W_1)f_1^1(W_1)dW_1$, where $g(.)$ is the bivariate density of ε_1 and $f_1(W_1)$ is the pdf of W_1. This can be written:

$$\Delta_1^+ = E\left(T_1^1 - T_1^0\right) + E\left(T_1^0\right) - \int E\left(T_0^0|W_0^+ = w\right) f_1^+(w)dw \tag{24}$$

Define $h_t(W_t|W_t^+ = w) = g(w - W_t)f(W_t)/\int g(w - W_t)f_t(W_t)dW_t$ as the density of true entitlement conditional on observed entitlement. The last

[7]Assumption (iv) means that measurement error does not affect the classification of people into the entitled and non-entitled groups. If selection into (simulated) entitlement is affected by measurement error, both parametric and nonparametric approaches are greatly complicated (see Pudney, 2001; Hernandez and Pudney, 2006).

term in (24) is:

$$
\begin{aligned}
&\int E\left(T_0^0|W_0^+=w\right) f_1^+(w)dw \\
&\qquad = \int\int E\left(T_0^0|W_0,W_0^+=w\right) h_0(W_0|W_0^+=w)dW_0 f_1^+(w)dw \\
&\qquad = \int\left[\int E\left(T_0^0|W_0\right) h_0(W_0|W_0^+=w)dW_0\right] f_1^+(w)dw \\
&\qquad = \int\left[\int E\left(T_1^0|W_1=u\right) h_0(W_0=u|W_0^+=w)du\right] f_1^+(w)dw
\end{aligned} \tag{25}
$$

where we have used the independence of ε_0 and T_0^0 and the assumption that $E\left(T_0^0|W_0=u\right)=E\left(T_1^0|W_1=u\right)$. Under the linearity assumption:

$$
\Delta_1^+ = \Delta_1 + \beta^0\left(\mu_1^0 - \int E(B_0^0|W_0^+=w)\, f_1^+(w)dw\right) \tag{26}
$$

Note that we have used the fact that $E\left(T_1^0|W_1=u\right)=\alpha^0+\beta^0 u^0$, where $u=(u^0,u^1)$ and that $\int u^0 h_0(W_0=u|W_0^+=w)du$ is the definition of $E(B_0^0|W_0^+=w)$. Under normality, $B_0^0|W_0^+=w$ has mean $\mu_0^0+[w-\mu_0]b$, where $\mu_0=(\mu_0^0,\mu_0^1)$ and b is the vector:

$$
\begin{bmatrix} b^0 \\ b^1 \end{bmatrix} = \begin{bmatrix} \omega_{00}+\sigma_\varepsilon^2 & \omega_{01}+\rho\sigma_\varepsilon^2 \\ \omega_{01}+\rho\sigma_\varepsilon^2 & \omega_{11}+\sigma_\varepsilon^2 \end{bmatrix}^{-1} \begin{bmatrix} \omega_{00} \\ \omega_{01} \end{bmatrix} \tag{27}
$$

Thus:

$$
\begin{aligned}
\Delta_1^+ &= \Delta_1 + \beta^0\left(\mu_1^0 - \int\left(\mu_0^0 + b[w-\mu_0]\right) f_1^+(w)dw\right) \\
&= \Delta_1 + \beta^0(1-b^0)\left(\mu_1^0-\mu_0^0\right) - \beta^0 b^1(\mu_1^1-\mu_0^1)
\end{aligned} \tag{28}
$$

Note that, as σ_ε^2 increases from 0 to ∞, $b=(b^0,b^1)$ goes (not necessarily monotonically) from $(1,0)$ to $(0,0)$ and that $b^1 \lesseqgtr 0$ as $\rho \gtreqless \omega_{01}/\omega_{00}$. Thus, if the correlation between the measurement errors in B_t^0 and B_t^1 is approximately equal to the correlation between B_t^0 and B_t^1 (adjusted for any reform-induced change in variance), the last bias component is approximately zero. It will be reasonable in most circumstances to expect that $0<b^0<1$ and

$b^1 \approx 0$ and consequently the matching method to have a bias of the same sign as $(\mu_1^0 - \mu_0^0)$. Since each of the pairs (μ_1^0, μ_0^0) and (μ_1^1, μ_0^1) refers to mean entitlement under the same benefit system in consecutive years, the bias in (28) will normally be moderate in size.

Consider in contrast the parametric approach, based on a separate regression of T_t^t on B_t^{t+} for each year. These regressions are subject to the classical errors-in-variables bias and the parameters identified by these regressions are:

$$\widetilde{\beta}^t = P^t \beta^t \ ; \quad \widetilde{\alpha}^t = \alpha^t + \beta^t (1 - P^t) \mu_t^t \tag{29}$$

where $P^t = \omega_{tt} / (\omega_{tt} + \sigma_\varepsilon^2)$. Consequently, the reform effect identified by this regression approach is:

$$\begin{aligned} \widetilde{\Delta}_1 &= \widetilde{\alpha}^1 - \widetilde{\alpha}^0 + \widetilde{\beta}^1 \mu_1^1 - \widetilde{\beta}^0 \mu_1^0 \\ &= \Delta_1 - \beta^0 P^0 \left(\mu_1^0 - \mu_0^0 \right) \end{aligned} \tag{30}$$

and the regression approach gives a bias in the estimated reform effect opposite in sign to $(\mu_1^0 - \mu_0^0)$.

To the extent that this simple linear-Gaussian case is a good approximation to the nonlinear context of discrete choice, it suggests that the parametric and nonparametric approaches give estimates biased in opposite directions, which can be expected to give rather tight bounds on the true reform effect.

7.3 Parametric estimates

Table 8 shows the estimated average reform effects resulting from the probit analysis. Comparison with the nonparametric estimates in Table 5 reveals only small differences, at least when fine stratification is used in the matching procedure. There is some suggestion of a steeper take-up-entitlement gradient in the nonparametric results but this difference is modest in relation to the standard errors for the matching results.

***** TABLE 8 HERE *****

8 Conclusions

We have analysed the behavioural response of older pensioners to the 2001 reform of the Minimum Income Guarantee system by isolating the behavioural component of the change in take-up rates between the pre-reform and post-reform periods, using alternative approaches based on parametric modelling and nonparametric analysis. Although panel data are not available, it is shown that the behavioural element of the change in take-up rates can be identified by appropriate matching of survey respondents in the pre- and post-reform samples. This leads to a "matching on variables" approach, rather than propensity score matching. We also show that, to a simple linear-Gaussian approximation, the existence of measurement error causes the parametric and non-parametric approaches to act as bounds on the true average reform effects. Despite the potentially serious measurement error bias in the coefficients of the parametric take-up model, these bounds can be expected to be rather tight.

We implement this approach using data on older pensioner units from the UK Family Resources Survey, matching on demographic characteristics and the simulated values of pre- and post-reform MIG entitlements. The average effect of the reform, for those who would have been entitled under both pre- and post-reform systems, was found to be positive and significant for most of the implemented specifications. This finding supports the idea that the take-up of MIG was significantly increased by the reform and that the effect was particularly large for those with the largest potential gains from claiming.

The finding is confirmed by the results from an *ex-ante* matching analysis which uses only pre-reform data and matches the simulated post-reform entitlement to otherwise similar individual's pre-reform entitlements. The larger impacts detected by this ex ante analysis can be interpreted as evidence of a delay in adjustment of behaviour to the changed circumstances induced by the 2001 reform.

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Appendix 1: Standard errors for parametric predictions of take-up

Each estimate has two sources of error: the sampling error in the sample averages and the estimation error in $\hat{\beta}$. The standard error formula must take into account both of them. Considering as example $\hat{\Delta}_0^a$, its error can be written as

$$\begin{aligned}\hat{\Delta}_0^a - \Delta_0 &= \left[\frac{1}{n_0}\sum_{i\in S_0}\Phi(x_i^1\hat{\beta}_0) - \mu_1\right] - \left[\frac{1}{n_0}\sum_{i\in S_0}^{n_0} T_i^0 - \mu_0\right] \\ &= \frac{1}{n_0}\sum_{i\in S_0}\left[\left\{\Phi(x_i^1\hat{\beta}_0) - \Phi(x_i^1\beta)\right\} + \left\{\Phi(x_i^1\beta) - \mu_1\right\}\right] - \left[\frac{1}{n_0}\sum_{i\in S_0} T_i^0 - \mu_0\right]\end{aligned}$$

Making a tangent approximation about the point β

$$\begin{aligned}\hat{\Delta}_0^a - \Delta_0 &= \left[\frac{\partial\ \bar{\Phi}(x^1\hat{\beta}_0)}{\partial\hat{\beta}_0}\right]\left(\hat{\beta}_0 - \beta\right) + \left[\frac{1}{n_0}\sum_{i\in S_0}(\Phi(x_i^1\beta) - \mu_1)\right] \\ &\qquad - \left[\frac{1}{n_0}\sum_{i\in S_0} T_i^0 - \mu_0\right] + o_p(n^{-1/2}) \\ &= \left[\bar{\phi}(x^1\hat{\beta}_0)x^1\right]\left(\hat{\beta}_0 - \beta\right) + \left[\frac{1}{n_0}\sum_{i\in S_0}(\Phi(x_i^1\beta) - \mu_1)\right] \\ &\qquad - \left[\frac{1}{n_0}\sum_{i\in S_0} T_i^0 - \mu_0\right] + o_p(n^{-1/2})\end{aligned}$$

and by the usual espansion for maximum likelihood estimators

$$\sqrt{n_0}\left(\hat{\beta}_0 - \beta\right) = -\left(\frac{1}{n_0}H\right)^{-1}\frac{1}{n_0}\sum_{i\in S_0} s_i + o_p(1)$$

where s_i is the score vector for case i and H is the Hessian matrix of the log-likelihood, we get

$$\begin{aligned}
\sqrt{n_0}\left(\hat{\Delta}_0^a - \Delta_0\right) &= -\left[\bar{\phi}(x^1\hat{\beta}_0)x^1\right]\left(\frac{1}{n_0}H\right)^{-1}\frac{1}{\sqrt{n_0}}\sum_{i\in S_0} s_i \\
&\qquad + \left[\frac{1}{\sqrt{n_0}}\sum_{i=1}^{n_0}(\Phi(x_i^1\beta) - \mu_1)\right] - \frac{1}{\sqrt{n_0}}\sum_{i\in S_0} T_i^0 - \mu_0 + o_p(1) \\
&= \frac{1}{\sqrt{n_0}}\sum_{i\in S_0} e_i + o_p(1)
\end{aligned}$$

where e_i can be approximated as

$$\hat{e}_i = -\left[\bar{\phi}(x^1\hat{\beta}_0)x^1\right]\left(\frac{1}{\sqrt{n_0}}H\right)^{-1} s_i + \left[(\Phi(x_i^1\hat{\beta}_0) - \hat{\mu}_1)\right] - \left[T_i^0 - \hat{\mu}_0\right]$$

where the estimated take-up rates $\hat{\mu}_0$ and $\hat{\mu}_1$ are the sample means of $\Phi(x_i^1\hat{\beta}_0)$ and T_i^0 respectively. The approximate standard error can then be calculated as

$$se(\hat{\Delta}_0^a) = \sqrt{var(\hat{e})/n_0}$$

Similar asymptotic approximations can be used for $\widehat{\Delta}_0^p$, $\widehat{\Delta}_1^a$ and $\widehat{\Delta}_1^p$.

Table 1 Pre- and post-reform Minimum Income Guarantee rates

Pre-reform rates		
Allowances and Premiums	**£ per week-single**	**£ per week-couple**
Basic Allowance	52.20	81.95
Pensioner Premium	26.25	40.00
Enhanced Pensioner Premium	28.65	43.40
Higher Pensioner Premium	33.85	49.10
Capital limits	3,000 - 8,000	3,000 - 8,000
Post-reform rates (deflated values in brackets)		
Basic Allowance	53.05 (52.21)	83.25 (81.93)
Pensioner Premium	39.10 (38.48)	57.30 (56.39)
Enhanced Pensioner Premium	39.10 (38.48)	57.30 (56.39)
Higher Pensioner Premium	39.10 (38.48)	57.30 (56.39)
Capital limits	6,000 -12,000	6,000 - 12,000

Table 2 MIG recipients, entitled non recipients and caseload take-up rates

		Couple	**Single Male**	**Single Female**	**All**
Number of	1999/2000	240	240	900	1390
Recipients	2000/1	260	250	920	1430
(thousands)	2001/2	280	270	960	1520
Range of	1999/2000	90-170	60-170	220-460	390-770
Entitled non	2000/1	110-170	80-140	230-380	450-670
Recipients	2001/2	170-260	90-160	310-480	600-870
Caseload	1999/2000	59-72	59-79	66-80	64-78
Take-up	2000/1	60-69	65-76	70-80	68-76
Range	2001/2	52-62	64-75	67-75	63-72

(Ranges are 95% confidence interval to reflect sampling errors); source: DWP (2004)

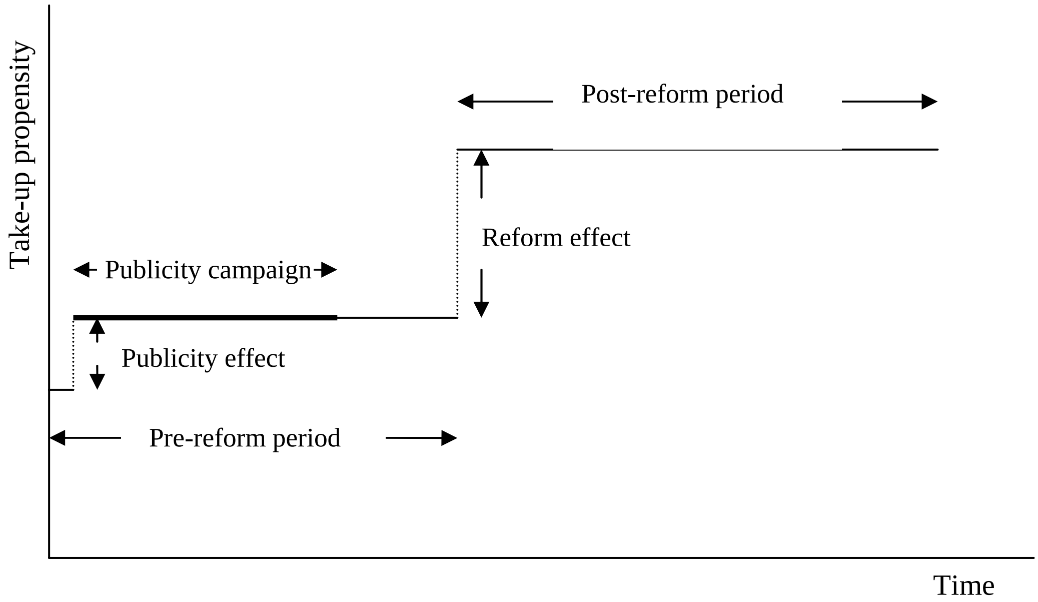

Figure 1 Schematic effect of the May-November 2000 publicity campaign and the April 2001 reform

Table 3 Empirical take-up rates, pre- and post-reform
(subsamples with simulated entitlements > £1 per week pre- and post-reform; standard errors in parentheses)

Population group	Pre-reform take-up rate (FRS 2000/1)	Post-reform take-up rate (FRS 2001/2)	Change in take-up rate $\hat{\Delta}$	Mean entitlement (£ per week) (FRS 2000/1)	Mean entitlement (£ per week) (FRS 2001/2)	Change in mean entitlement (£ per week)
Single disabled	.577	.651	.074	44.68	48.21	3.53
$n_{2000/1}$ = 189; $n_{2001/2}$ =189	(.495)	(.478)	(.688)	(23.92)	(25.54)	(33.82)
Couple, at least one disabled	.579	.618	.039	37.23	41.37	4.14
$n_{2000/1}$ =57; $n_{2001/2}$ =34	(.498)	(.493)	(.701)	(35.80)	(31.17)	(47.47)
Single aged below 70	.864	.868	0.004	16.47	32.31	15.84
$n_{2000/1}$ =66; $n_{2001/2}$ =38	(.346)	(.343)	(.487)	(14.21)	(22.74)	(26.81)
Single aged 70-74	.632	.835	0.203	16.14	29.42	13.28
$n_{2000/1}$ =106; $n_{2001/2}$ =97	(.484)	(.373)	(.612)	(17.12)	(20.73)	(26.89)
Single aged 75-79	.690	.731	0.041	14.58	28.60	14.02
$n_{2000/1}$ =116; $n_{2001/2}$ =119	(.465)	(.445)	(.644)	(15.04)	(22.36)	(26.95)
Single aged 80 or above	.637	.582	-0.055	18.56	18.87	0.31
$n_{2000/1}$ =215; $n_{2001/2}$ =189	(.482)	(.494)	(.691)	(18.35)	(13.84)	(22.98)
Couple at least one aged above 74	.491	.311	-0.181	19.58	31.23	11.65
$n_{2000/1}$ =57; $n_{2001/2}$ =45	(.504)	(.468)	(.688)	(29.67)	(28.47)	(41.12)
Couple both below 74, one below 68	.444	.476	0.032	48.69	52.07	3.38
$n_{2000/1}$ =18; $n_{2001/2}$ =21	(.511)	(.512)	(.723)	(44.49)	(40.91)	(60.44)
Couple both below 74, one above 68	.381	.708	0.327	35.62	47.71	12.09
$n_{2000/1}$ =21; $n_{2001/2}$ =24	(.498)	(.464)	(.681)	(54.01)	(34.340)	(64.03)
All groups	.624	.656	.032	25.78	33.35	7.57
$n_{2000/1}$ =845; $n_{2001/2}$ =756	(.485)	(.475)	(.679)	(26.50)	(25.91)	(37.06)

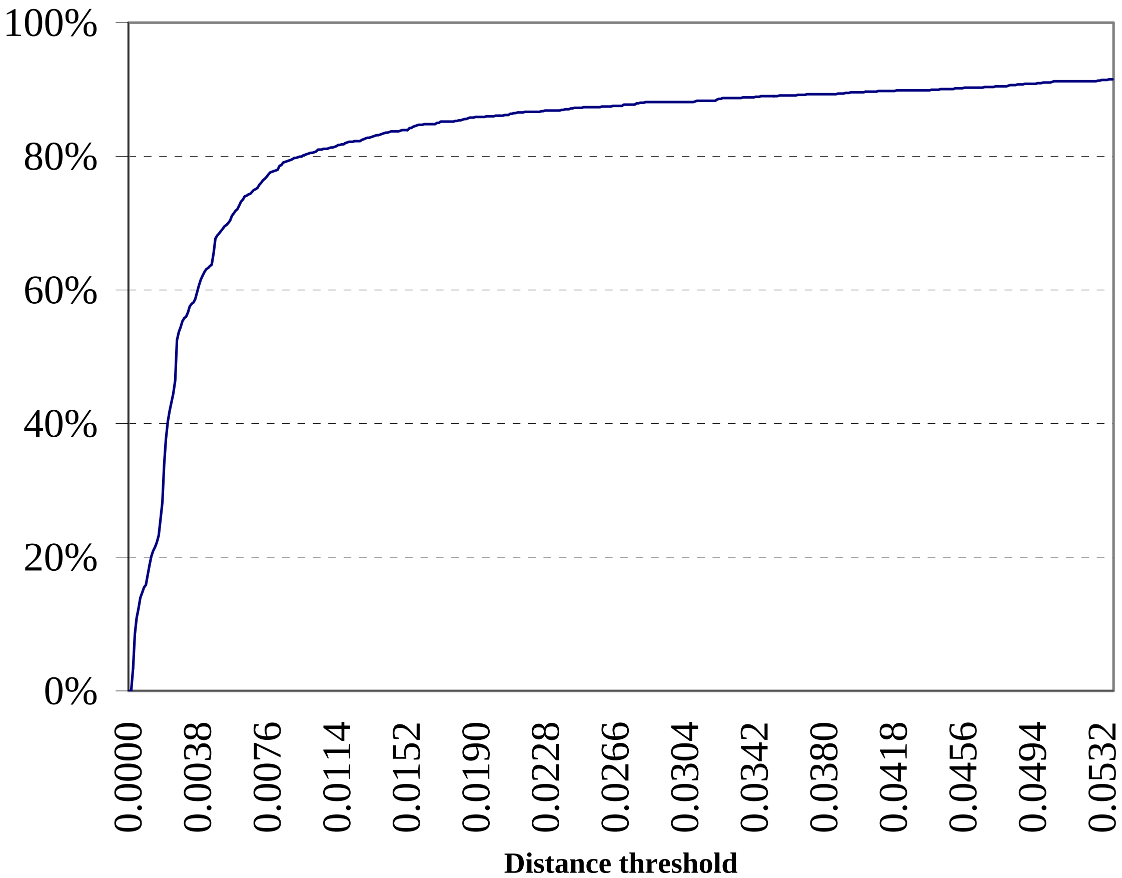

Figure 2 Percentage of cases matched with $D(i, j) <$ threshold

Table 4 Matching estimates with and without demographic stratification and caliper options
(standard errors in parentheses)

	Impact with 2000/1 characteristics: $\hat{\Delta}_0^*$				Impact with 2001/2 characteristics: $\hat{\Delta}_1^*$			
caliper:	none	ϵ = 0.05	ϵ = 0.025	ϵ = 0.01	none	ϵ = 0.05	ϵ = 0.025	ϵ = 0.01
2 strata: singles, couples	.053	.048	.037	.040	.088	.086	.084	.095
	(.038)	(.047)	(.048)	(.048)	(.036)	(.044)	(.044)	(.045)
3 strata: singles by gender, couples	.046	.036	.031	.027	.088	.086	.086	.087
	(.039)	(.054)	(.055)	(.049)	(.037)	(.052)	(.052)	(.080)
9 strata: demographic groups	.035	.019	.021	.031	.086	.060	.067	.056
(marital status, age, disability)	(.035)	(.046)	(.046)	(.057)	(.036)	(.043)	(.045)	(.055)
No stratification	.092	.090	.085	.082	.094	.099	.091	.092
	(.040)	(.042)	(.042)	(.043)	(.037)	(.038)	(.038)	(.039)

Table 5 Matching estimates by increase in entitlement, with and without stratification (standard errors in parentheses)

Estimate of impact with 2000/1 characteristics: $\hat{\Delta}_0$							
Size of increase in entitlement	**Number of cases**	**Take-up rate 2000/01**	**Take-up rate 2001/02 (matched)**	**no stratification**	**2 strata**	**3 strata**	**9 strata**
< £10 per week	542	0.638	0.664	0.026 (0.051)	.007 (.065)	-.013 (.075)	.013 (.093)
£10-15 per week	223	0.668	0.771	0.103 (0.158)	.023 (.080)	.025 (.086)	.040 (.106)
>£15 per week	80	0.400	0.737	0.337 (0.161)	0.300 (0.145)	0.303 (0.147)	0.137 (0.106)
Estimate of impact with 2001/2 characteristics: $\hat{\Delta}_1$							
Size of increase in entitlement	**Number of cases**	**Take-up 2000/01 (matched)**	**Take-up 2001/02**	**no stratification**	**2 strata**	**3 strata**	**9 strata**
< £10 per week	484	0.566	0.620	0.054 (0.048)	.045 (.058)	.017 (.068)	.047 (.088)
£10-15 per week	192	0.417	0.771	0.354 (0.157)	.094 (.142)	.145 (.149)	.118 (.116)
>£15 per week	80	0.237	0.600	0.362 (0.127)	0.296 (0.097)	0.290 (0.099)	0.241 (0.101)

Table 6 Balance of covariates with pair matching, no caliper, no stratification

Matching with 2000/1 characteristics						
Variables	**Mean treated**	**Mean control**	**Mean matched control**	**Std % bias Before matching**	**Std % bias after matching**	**%Reduction in Absolute Bias**
Entitlement before reform	25.78	24.30	25.50	5.7	1.1	80.8
Entitlement after reform	34.74	33.35	34.57	5.3	0.7	87.4
Matching with 2001/2 characteristics						
Variables	**Mean treated**	**Mean control**	**Mean matched control**	**Std % bias before matching**	**Std % bias after matching**	**%Reduction in Absolute Bias**
Entitlement before reform	24.30	25.78	24.17	-5.7	0.5	90.9
Entitlement after reform	33.36	34.74	33.08	-5.3	1.1	79.9

Table 7 Matching estimates with and without demographic stratification and caliper options
(standard errors in parentheses)

Ex-ante impact with 2000/1 characteristics: $\hat{\Delta}_0^{ex-ante}$				
caliper:	none	$\epsilon = 0.05$	$\epsilon = 0.025$	$\epsilon = 0.01$
2 strata: singles, couples	15.74	13.16	14.28	13.61
	(8.65)	(8.56)	(7.44)	(6.17)
3 strata: singles by gender, couples	16.33	13.79	14.75	14.19
	(8.93)	(8.85)	(7.84)	(6.89)
No stratification	16.92	16.94	16.74	15.19
	(8.20)	(8.21)	(7.43)	(5.60)

Table 8 Predicted change in take-up rate (standard errors in parentheses)

Size of increase in entitlement	all	< £10 per week	£10-15 per week	>£15 per week
Separate probits for each year, whole sample, full covariates set				
$\hat{\Delta}_0^a$	.052	.015	.084	.212
	(.018)	(.022)	(.032)	(.066)
$\hat{\Delta}_0^p$	.052	.038	.072	.104
	(.002)	(.002)	(.004)	(.011)
n	839	540	221	78
$\hat{\Delta}_1^a$	.084	.048	.125	.138
	(.018)	(.024)	(.025)	(.061)
$\hat{\Delta}_1^p$	.084	.058	.140	.123
	(.004)	(.004)	(.015)	(.013)
n	753	408	267	78
Separate probits for each year, whole sample; same regressors as stratification variables in non parametric analysis				
$\hat{\Delta}_0^a$	.069	.036	.098	.216
	(.016)	(.020)	(.030)	(.056)
$\hat{\Delta}_0^p$	.069	.052	.096	.118
	(.002)	(.002)	(.005)	(.010)
n	842	542	122	78
$\hat{\Delta}_1^a$	.100	.066	.139	.142
	(.017)	(.024)	(.025)	(.058)
$\hat{\Delta}_1^p$	.100	.066	.135	.154
	(.004)	(.003)	(.007)	(.015)
n	753	408	267	78

APPENDIX 2: PARAMETRIC TAKE-UP ESTIMATES

Table A1 Take-up Probit estimates for single pensioners full covariates set

	Sampled in 2000/1		Sampled in 2001/2	
Regressor	**Coefficient**	**Standard error**	**Coefficient**	**Standard error**
Owner	-.444	.109	-.691	.118
Female	.081	.154	-.175	.171
Black	-.595	.448	-2.501	.644
Disabled	.064	.118	.255	.132
Years worked	-.001	.003	-.001	.003
Age	-.086	.158	-.086	.169
Age^2	.001	.001	.001	.001
Education	-.032	.036	-.001	.019
ln entitlement	.246	.055	.387	.091
Net income	-.009	.001	-.011	.001
Constant	5.255	6.232	5.003	6.721
n	688		632	
LR $\chi^2(10)$	80.78		124.54	
Pseudo R^2	.0905		.1585	
Log likelihood	-405.789		-330.648	
Test for parameter stability	$\chi^2(10)$=14.04			

Table A2 Take-up Probit estimates for pensioners couples full covariates set

	Sampled in 2000/2001		Sampled in 2001/2002	
Covariate	**Coefficient**	**Standard error**	**Coefficient**	**Standard error**
Owner	-.560	.230	-.941	.256
Disabled head	.256	.259	.561	.294
Disabled spouse	.238	.246	.516	.276
Years worked head	-.009	.009	.002	.010
Years worked spouse	.010	.008	-.010	.008
Head's education	.087	.074	.014	.025
Spouse's education	-.040	.055	.061	.085
ln entitlement	.356	.099	.406	.217
Net income	-.001	.002	-.002	.003
Constant	-1.399	1.337	-2.301	1.797
n	151		121	
LR $\chi^2(9)$	30.36		35.55	
Pseudo R^2	.1451		.2119	
Log likelihood	-89.455		-66.091	
Test for parameter stability	$\chi^2(9)$=13.18			

Table A3 Take-up probit estimates for pensioners
stratification variables in non parametric analysis used as covariates

	Sampled in 2000/2001		Sampled in 2001/2002	
Covariate	**Coefficient**	**Standard error**	**Coefficient**	**Standard error**
Single disabled	-.532	.139	-.207	.153
Couple, at least one disabled	-.312	.193	-.191	.247
Single aged below 70	.827	.216	.724	.278
Single aged 70-74	.065	.156	.607	.181
Single aged 75-79	.242	.153	.266	.157
Couple at least one aged over 74	-.380	.202	-.859	.227
Couple both below 74, one below 68	-.736	.338	-.558	.305
Couple both below 74, one over 68	-.473	.266	-.2597	.286
ln entitlement	.356	.051	.430	.085
Constant	-.555	.158	-.986	.255
n	842		753	
LR $\chi^2(10)$	82.38		81.20	
Pseudo R^2	0.0739		.0838	
Log likelihood	-515.975		-444.050	
Chow test for parameters stability	$\chi^2(10) = 16.5$			