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The route to take-up: raising incentives or lowering barriers?

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INSTITUTE FOR SOCIAL
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THE ROUTE TO TAKE-UP: RAISING INCENTIVES OR LOWERING BARRIERS?

Welfare support to poor individuals in the United Kingdom is mostly delivered through means-tested benefits, requiring a preliminary assessment of claimants' financial resources. In principle this form of delivery represents the most efficient way of reducing poverty, as public resources are spent only for those in need. However, a sizeable proportion of individuals entitled to receive welfare benefits do not claim them. This phenomenon, known as non take-up, endangers the effectiveness of means-tested programs and has therefore caught the attention of policy makers and social science researchers.

Seemingly an irrational behaviour, non take-up has been explained by economists as the result of a rational decision in which an entitled individual compares the financial gain from claiming (the amount of benefit receivable) with the costs inherent in claiming (the hassle of acquiring the relevant information and going through the claiming process and social stigma related to welfare dependence) and concludes that the expected benefit does not compensate adequately for the costs.

This work investigates policy interventions designed to raise the take-up of welfare benefits, considering two possible routes. One is to increase the financial incentive to claim – the amount of benefit receivable. The other possibility is to lower claim costs, for example through simplified application procedures and benefit rules, and information campaigns. This route implies potentially different consequences for the public welfare budget and the social acceptability of benefit dependence.

The analysis exploits a policy reform to the income support scheme available to British pensioners, the introduction of Pension Credit in 2003. The reform increased the benefit amount for a subgroup of eligible pensioners and was accompanied by a publicity campaign aimed at lowering information costs and stigma. The application process was also changed into a more claimant-friendly form. Because only a subgroup of pensioners exposed to the 'lowering barriers' policies was also affected by the increase in the benefit level, it is possible to use the difference in the way pensioners were 'treated' by the reform to identify the distinct role of the 'raising incentive' and 'lowering barriers' policies. Both can be evaluated comparing the take-up behaviour of eligible pensioners observed before and after the reform came into force.

Results support the idea that raising the benefit level has been an effective way of fostering new claims. In contrast, no evidence of an increase in take-up due to the 'lowering barriers' policies *per se* has been found, even though these policies seem to have partially enhanced the effect of financial incentives when concurrently applied.

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Abstract

This paper investigates which measures are effective in promoting the take-up of welfare support, considering both financial incentives and policies intended to lower claiming costs. The analysis uses an exogenous policy change, the introduction of the Pension Credit in the UK, that increased the entitlement amount for a subgroup of eligible pensioners, and was accompanied by an advertisement campaign, reformed 'light touch' administration and a more claimant-friendly application procedure. The behavioural response is identified using parametric and non parametric analysis of repeated cross sections of FRS data. Results consistently show the efficacy of increased financial incentives, while no effect is found for the 'lowering barriers' policies when unaccompanied by concurrent raised monetary rewards.

Keywords: policy evaluation, welfare targeting, take-up, matching estimators, pensioner poverty

JEL codes: C14, C25, H53, H55, I38

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1 Introduction

Non take-up, that is a situation where individuals entitled to receive a government benefit do not claim it, is generally regarded as a failure of welfare targeting, as it compromises the effectiveness of means tested¹ programs, which otherwise represent the most cost-efficient way of delivering welfare support. Economic research on non take-up was developed mainly in the US and the UK (see Currie 2004 for a recent review), where a substantial component of the social security system relies on means testing; but is also growing in other OECD countries (Hernanz et al. 2004). Studies have focussed on measuring take-up rates across different social programs and population subgroups; on investigating which personal or program characteristics are related to claiming; and on trying to identify which factors ('barriers') are responsible for non take-up behaviour. However, despite general agreement about the undesirability of non take-up, little attention has been dedicated to identifying the type of policy measures that would most effectively succeed in triggering claims.

Since Moffitt's (1983) seminal model of non-take-up, economists have represented take-up as utility maximising choice, where individuals compare the disutility inherent in claiming - due to stigma, transaction costs or cost of acquiring information - with the expected utility of the financial gain that take-up would entail. Economic theory therefore predicts that higher benefit entitlements will increase take-up.² The role of the expected benefit amount as a trigger of take-up is the most consistent empirical finding so far. Several studies have obtained this result exploiting the cross-sectional variation of entitlement levels across the eligible population. A few have gone further towards causality, by using exogenous changes in programs' parameters to identify the take-up elasticity to entitlement amounts (Anderson et al. 1997; Blank et al. 1991; Dahan et al. 2007; Zantomio et al. 2008). However, using financial incentives as take-up triggers might entail disadvantages such as increased fiscal

¹While non take-up is not exclusively confined to means-tested benefits, it is with this type of programs that it proves to be most serious.

²If barriers are fixed, independent of the benefit amount. This seems to be the case according to the empirical evidence found so far.

burden, political acceptance concerns and the worsening of the poverty trap inherent in means-tested programs.

Lowering the barriers to claiming could be considered as another route to take-up. Claimant-friendly application procedures, simplified benefit rules, advertising campaigns and automatic enrolment in programs are a few examples of measures that have been suggested (Hernanz et al. 2004) as potentially promising. Knowledge about the importance of different types of barrier - stigma, transaction costs, or lack of information- would indicate which type of policy initiatives should be pursued. However, evidence on this is mixed and inconclusive. Barriers to claiming are mostly unobservable in available data and identification of each of them using individual characteristics as proxies is problematic, as these are potentially correlated with several types of barriers.

Some studies have stressed the role played by information costs. Perceived ineligibility for example was found to be a major determinant of non take-up (Coe 1979, Daponte et al.1999). Dahan et al. (2006) found that longer exposure to the incentive to look for information significantly increased take-up probability in the context of a non means tested water price benefit in Israel. The low take-up probability found for residents of small cities (Bramley et al.2000), and the high one for individuals already receiving welfare (Blundell et al.1988; Dorsett et al. 1991; Zedlewski and Brauner 1993) might also be considered as indirect evidence of the role played by information. However, they could also be due to difference in administrative costs, potentially higher in rural areas (Edmonds 2002, Warlick 1982) and lower for recipients already accustomed to benefit administration (Blundell et al.1988). The potential relevance of administration costs (Koning et al., 1997; Blank et al. 1996) is reinforced also by the finding that uncertainty about the outcome of a claim discourages take-up (Halpern et al. 1986). Instead, the evidence of lower take-up by more socially active individuals and pensioners -regarded as more adverse to ‘government handouts’’ (Kayser et al. 2000; Andrade 2002)- has been related to the presence of stigma-type of barriers.

Still, very little is known about individuals' responsiveness to policies aimed at lowering barriers to claiming. This study follows a quasi-experimental approach to investigate how individuals' claiming behaviour responds to increases in financial rewards and to policies intended to reduce 'barriers' e.g. the non monetary costs of claiming. The exogenous policy change used is the introduction of the Pension Credit, which modified the income support scheme available to British Pensioners in 2003. The reformed more generous entitlement rules were accompanied by a publicity campaign aimed at lowering information costs and stigma, a reformed 'light touch' claims administration and a friendlier application procedure.

Since the available panel data do not offer adequate sample size or the necessary detailed income and assets information, the analysis instead uses two cross sections of the Family Resources Survey. The first provides a sample of pensioners interviewed up to six months before the reform and the second a sample of pensioners interviewed after the reform came into force. Potential pre-benefit income endogeneity is addressed by restricting the sample to older pensioners, for whom employment rates are negligible. While the random design of the survey sampling and the exogenous nature of the Pension Credit reform rule out the chance of endogenous selection into the pre- or post-reform samples, bias might arise from different patterns of non response and of covariates distribution in the two samples. Following a 'selection on observables' approach, the average reform effect on the take-up behaviour of pensioners is identified using a range of parametric and non parametric evaluation techniques, including parametric regression, matching on variables and propensity score matching. Because only a subgroup of previously eligible pensioners exposed to 'lowering barriers' policies was *de facto* affected by the more generous benefit rules through an increase in financial entitlement, it is possible to exploit the differences in the way pensioners were 'treated' by the reform to identify the distinct role played by the 'raising incentive' and 'lowering barriers' initiatives and in particular the responsiveness to the second type of measures.

Any successful way of fostering benefit claiming is of interest to governments, as raised take-up improves the poverty alleviation efficacy of targeted programs; but also the way take-up increase is achieved matters. Firstly, the adoption of ‘lowering barriers’, rather than ‘raising incentives’ policies, might bear different implications for fiscal balances. Secondly, ‘lowering barriers’ measures aimed at delivering the agreed welfare expenditure, rather than increasing its level (as raising financial incentives would imply), might also gather more social and political consensus, even if the same amount of public resources were dedicated to either type of measure. Moreover, from a microeconomic point of view, ‘lowering barriers’ policies would not add to the poverty trap as ‘raising incentives’ could do. However, there are doubts about the effectiveness of the ‘lowering barriers’ route. If the government is interested in improving target effectiveness by investing in either or both type of measures, the optimal share of public resources to be allocated to each measure depends crucially on their relative effectiveness in generating new claims. A balanced mixture of measures is more likely to be optimal the higher their interacted effectiveness, relative to the effectiveness of each of them implemented separately. Knowledge about individuals’ responsiveness to each type of measure and their interaction therefore emerges as the key to identifying which policy initiatives should be pursued to improve target effectiveness: this constitutes the objective of the following empirical analysis. Besides contributing to the international take-up literature by providing some evidence on the way individuals’ respond to ‘lowering barriers’ policies and therefore the viability of such route, this study also offers the first rigorous evaluation of the impact of Pension Credit on the take-up of welfare support by British pensioners.

The rest of the paper is set out as follows. The Pension Credit reform is detailed in Section 2, covering both modified entitlement rules and the further measures intended to lower claiming barriers. Section 3 presents the evaluation setting and identification strategy. The data description and the implementation of the econometric analysis are carried out in Section 4. Section 5 presents the results and the final section concludes.

2 The Pension Credit reform

2.1 Background: pensioners take-up in the UK

Despite a remarkable improvement in pensioners' relative economic position in the course of the last century and several recent government measures, approximately one in five British pensioners is currently estimated to live in relative poverty (DWP 2008; Eurostat 2006). For several decades, public pension provision in the UK has been delivered through the contributory Basic State Pension and a means tested income top-up for those with incomplete contribution histories. As in other OECD countries, the role of means testing in public provision for old age has become more prominent: the Basic State Pension has decreased in real terms, while Income Support increased in real terms. However, a low pensioner take-up rate, fluctuating around 70 % of eligible pensioners in the years prior to PC introduction (DWP 2004, DWP 2005, DWP 2006), has compromised the effectiveness of the British means-tested program for the elderly, and has been the object of considerable policy and academic interest (Costigan et al. 1999; McConaghy et al. 2003; Talbot et al 2005; Bunt et al. 2006; Pudney et al. 2006; Hernandez et al. 2007; Zantomio et al. 2008). While complete take-up of income support would not in itself suffice to eradicate pensioner poverty (Brewer et al. 2007), it could significantly reduce its depth. Increasing take-up could also represent a cost-effective way of reducing pensioner poverty, if compared to more expensive options such as the introduction of a universal or/and more generous Basic State Pension (Brewer et al. 2007).

2.2 The change in entitlement rules: raising monetary incentives

The introduction of Pension Credit(PC) has been one of the major policy changes aimed at reducing pensioner poverty in the last decade. The PC reformed the pre-existing means tested income support scheme available to people aged 60 or older, known as Minimum

Income Guarantee (MIG) at the time. MIG and PC policy parameters applying at the time of the transition to PC are summarized in Table 1.

Table 1: Minimum Income Guarantee and Pension Credit rates, 2003/04 £per week,

		single pensioner		pensioner couple	
		MIG	PC	MIG	PC
<i>Needs:</i>					
Basic Allowance	MIG and GC	102.10	102.10	155.80	155.80
	Severe Disability premium	42.90	42.90	85.80	85.80
	Carer premium	25.10	25.10	50.20	50.20
	Saving Credit threshold	-	77.35	-	123.80
	Maximum Saving Credit	-	14.79	-	19.20
	Capital limits	6,000-12,000	6,000+	6,000-12,000	6,000+
	Capital means ratio	1 every 250	1 every 500	1 every 250	1 every 500

Source: CPAG(2003/04)

The MIG worked by topping up income to a ‘Basic Allowance’ level, uprated yearly by the government, varying according to pensioners partnership status and awarding additional ‘premium’ amounts for disability and care needs (‘Severe Disability’ and ‘Carer’ premiums). The unit of assessment, both for the means test and the definition of needs, was the ‘pensioner unit’, comprising the individual pensioner, if single, or both members of a couple, if either of them satisfied the age condition. The means test took into account all income from private and public sources (excluding a few disability benefits and other means-tested benefits helping with housing costs and local council tax), with the exception of capital income. This was imputed as £1 of weekly income for every £250 of capital above £6,000. Eligibility was lost if capital exceeded an upper limit of £12,000. The dashed line in Figure 1 represents the amount of entitlement for increasing levels of ‘means’ under MIG rules. The means test involved a 100% taper rate, so that each £1 of owned means would be lost as entitlement from the ‘Basic allowance’ level.

The PC was introduced in October 2003 as a replacement for the MIG. A few changes affected the means formula: the upper capital limit to eligibility was removed and the capital-

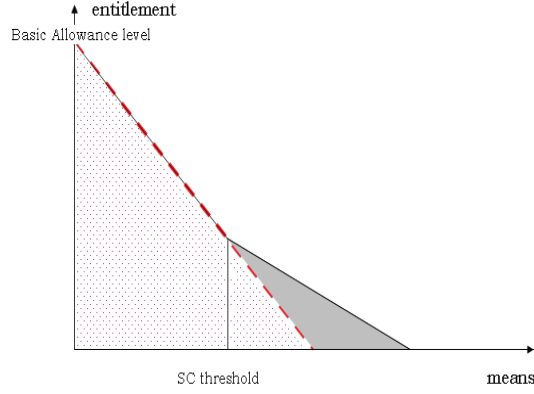


Figure 1: Entitlement structure under MIG and PC

to-means conversion rate was made more generous, changing to £1 of means imputed for every £500 of capital held above £6,000. The reformed entitlement was composed of a Guarantee Credit (GC) element, basically replicating the previous MIG (in terms of ‘Basic Allowance’ and ‘premium’ amounts, and the 100% taper rate, represented by the same dashed line in Figure 1) and a new Saving Credit (SC) component of entitlement (represented by the dark grey area in Figure 1) for people aged 65 or more whose means exceeded the ‘Saving Credit threshold’, corresponding to the level of the contributory Basic State Pension. The SC was intended to improve incentives to save for retirement and so a reduced 40% taper rate was applied to means above the SC threshold; however its amount could not exceed a defined ‘Maximum Saving Credit’.

After the introduction of the PC the ‘poorest’ pensioners continued to be entitled to the same amount as under the MIG (now called GC) and did not experience any increase in entitlement. However, those with accumulated means above the ‘SC threshold’, or with capital exceeding £6,000 were ‘rewarded’ by a reform induced entitlement increase because of the new SC³ component of entitlement and the new capital conversion rate respectively.

³Its introduction brought into the program pensioners that otherwise would be ineligible to the Guarantee Credit, and previous MIG, because of too high means. However, this group of pensioners is not of interest

2.3 Concurrent measures: lowering barriers

Besides tackling pensioner poverty and improving incentives to save for retirement, the government declared aims in introducing the PC included increasing the take-up amongst pensioners through lowering claiming costs. To this end, several policy initiatives were undertaken. On the benefit administration side, a new Pension Services was created, involving ‘local partners’ such as voluntary organisations, physically closer to claimants and ready to visit them at home if required. To ease the application process, a freephone national telephone line was instituted. While under MIG the application process was completed through written correspondence or face to face interviews in local offices, the telephone line allowed the administration to post completed application forms to applicants, ready for them to sign. The means test was made less intrusive: private transfers from families and friends did not need to be reported any more; and income or other personal circumstances would only need to be re-checked every five years, rather than claimants having to communicate any change to avoid being fined.

Between April 2003 and April 2004, a targeted mailing campaign took place, whereby 6.8 million pensioners not yet receiving MIG were sent a letter informing them of the PC, and the chance that they might be entitled to it⁴. However, the letter did not work as a form of automatic enrolment; pensioners were not told about how much their entitlement would have been and would still need to complete the application process to know that. A media marketing campaign ‘Pick it up: it’s yours’, involving both television and press, was launched in September 2003, with the purpose of informing but also influencing pensioners’ attitudes towards what was often perceived as a ‘government handout’.

An omnibus survey conducted between April and May 2004 (Age Concern 2004) showed widespread awareness about the existence of PC among pensioners, as 89% of those inter-

for the present work, as they did not face a pre-reform take-up decision. Moreover, newly entitled have been found to take time to adjust to new policy rules (Hernandez et al. 2007).

⁴Pensioner already receiving the MIG were instead sent a letter informing them about their transfer to the PC with effect from October 2003.

viewed reported having heard about it, and that the most common source of information was direct mailing, followed by media and the Pension Service⁵. Another omnibus survey (Talbot et al. 2005) conducted in November 2004 found that 81% of the interviewed pensioners had heard about PC, but two thirds of them reported having done nothing as a consequence and 35% reported that ‘nothing could be done’ to convince them to claim.

The government claimed that the PC campaign was effective in increasing take-up, as ‘for every £1 spent for it, £55 more benefits were paid out’ (CAG 2006). However additional benefits paid out do not in themselves imply that the campaign was effective in increasing take-up, as more people became eligible as a consequence of the PC introduction or might have claimed it anyway. The following analysis will make a more rigorous analysis of individuals’ response to the campaign.

3 Evaluating the take-up response

3.1 Evaluation setting

Consider a population of pensioner units indexed by i , and two time periods $t = 0$, before the PC introduction, and $t = 1$, after the PC introduction. Denote by e_i^r the benefit amount pensioner unit i is entitled to under the benefit rules applying in period $t = r$, so that $e_i^{r=0}$ indicates the entitlement under the pre-reform rules, and $e_i^{r=1}$ the entitlement under the post-reform rules. Also, denote with b^l the level of barriers to claim faced under the application environment in place at time $t = l$, so that b^0 and b^1 would capture social attitude, information and application costs before and after the PC reform respectively.

The take-up outcome can only be meaningfully defined for pensioner units eligible for income support; therefore the analysis is based on the subset of the pensioner population defined as

$$P = \{i | e_i^0 > 0\}$$

⁵Previously friends and family had been reported as the most common way of hearing about MIG.

Note that because entitlement rules were relaxed in a more generous direction with the introduction of PC, any pensioner unit entitled under MIG rules would also be entitled under PC rules. The outcome of interest is defined as $T_{t,i}^{r,l}$, a binary indicator for take-up of pensioner unit i in period t under entitlement rules r and barriers level l , where $T_{t,i}^{r,l} = 1$ indicates take-up and $T_{t,i}^{r,l} = 0$ indicates non take-up. In each period a sample of pensioner units is observed; the binary indicator $S_{t,i}$ takes the value 1 if i is observed in t , and 0 otherwise⁶. Therefore in each period t , if $S_{t,i} = 1$, $T_{t,i}^{r=t,l=t}$, $e_i^{r=t}$, $e_i^{r=1-t}$ and a set of other characteristics $X_{t,i}$ are observed.

3.2 The impact of the overall PC reform

The PC introduction changed both e_i^0 to e_i^1 and b^0 to b^1 . To identify the causal impact of the overall reform on take-up behaviour, one would ideally want to observe both of the $T_{t,i}^{1,1}$ and $T_{t,i}^{0,0}$ potential outcomes and base the identification of the behavioural response on

$$\Delta_t = E(T_t^{1,1} - T_t^{0,0}) \quad t = 0, 1$$

However only $T_{0,i}^{1,1}$ in period $t = 0$ and $T_{1,i}^{0,0}$ in period $t = 1$ are observable. This type of setting is representative of the ‘evaluation problem’ where causal inference requires the comparison of two potential outcome but only one of them is observed (Cochran and Rubin, 1973; Roy, 1951). The issue then becomes finding a ‘counterfactual’ that can best represent the unobserved outcome, e.g. the take-up behaviour of pensioner units observed after the PC reform, had they not been exposed to it. Following an approach previously adopted (Zantomio, Pudney and Hacock 2008) the appropriate counterfactual for i observed in t will be ‘built’ drawing from the sample of pensioners observed in $1 - t$.

Identification of Δ_t requires the following assumptions:

Assumption 1 $S_t \perp\!\!\!\perp T_t^{r,l} \mid X_t = x, \quad \forall r, l \quad t = 0, 1$

⁶ $S_{t,i} = 0$ covers pensioner units not sampled by the survey, survey non response, item non response preventing entitlement assessment and cases excluded because of the sample restrictions explained in section 4.1.

Assumption 2 $0 < Pr(S_{1-t} = 1 | X_{1-t} = x) < 1, \quad \forall x \text{ observed in } t = 0, 1$

Assumption 3 $E(T_t^{r,l} | X_t = x)$ is independent of $t \quad \forall r, l \quad t = 0, 1$

Assumption 1 requires that, conditionally on a given value x of observable characteristics X , any potential outcome is independent of i being observed in period t or not. In other words, we assume that there are no unobserved confounding characteristic according to which pensioner units would be non randomly assigned to the pre-reform or post-reform observed sample. Assumption 2 concerns the joint distribution of observable covariates X and S_t and requires ‘overlap’ in the cross-sectional distribution of X_t across $t = 1$ and $t = 0$. Assumption 3 requires no confounding macro-level factors, other than those already captured by X_t , e_i^r and b^l .

If Assumption 1 holds, then

$$E(T_1^{0,0} | X_1 = x) = E(T_0^{0,0} | X_0 = x)$$

$$E(T_0^{1,1} | X_0 = x) = E(T_1^{1,1} | X_1 = x)$$

and therefore, conditioning on observables, $\Delta_t(x)$ can be re-written as

$$\Delta_1(x) = E(T_1^{1,1} - T_1^{0,0} | X_1 = x) = E(T_1^{1,1} - T_0^{0,0} | X_1 = x, X_0 = x)$$

$$\Delta_0(x) = E(T_0^{1,1} - T_0^{0,0} | X_0 = x) = E(T_1^{1,1} - T_0^{0,0} | X_1 = x, X_0 = x)$$

Under assumption 2, these expectations can be estimated for all values of $x \in S^t$ and integrated over. Under further Assumption 3 the overall effect of the PC reform is identified. This will reflect the take-up response to any policy measure undertaken as part of the PC reform., eg. the combined effect of lowered barriers and improved incentives. Different ways of implementing conditioning on x and consequent estimators for Δ_t are detailed in section 3.4.

3.3 Which role for different policy instruments?

In addition to the evaluation of the overall reform effect, a second research question involves disentangling the role played by the different types of policy instruments involved. To this end remember that while the post reform claiming environment was experienced by all eligible pensioner units, only a subgroup were ‘treated’ with raised monetary reward from claiming, thanks to the reformed entitlement rules. More formally, the set P can be partitioned into

$$P^1 = \{i \in P | e_i^0 = e_i^1, b^0 > b^1\}$$

$$P^2 = \{i \in P | e_i^0 < e_i^1, b^0 > b^1\}$$

Because of the different treatments they received, P^1 and P^2 require distinct approaches. Consider P^1 first and define $\Delta_t^{P^1}$ as $\Delta_{t,i \in P^1}$. Because $e_i^0 = e_i^1$ it follows that

$$T_{0,i \in P^1}^{0,0} = T_{0,i \in P^1}^{1,0}$$

$$T_{1,i \in P^1}^{1,1} = T_{1,i \in P^1}^{0,1}$$

Therefore, conditioning on observables, $\Delta_t^{P^1}(x)$ can be re written as

$$\begin{aligned} \Delta_1^{P^1}(x) &= E(T_1^{1,1} - T_1^{0,0} | X_1 = x) = E(T_1^{1,1} - T_0^{0,0} | X_1 = x, X_0 = x) = \\ &= E(T_1^{1,1} - T_1^{1,0} | X_1 = x, X_0 = x) \end{aligned}$$

or

$$\begin{aligned} \Delta_0^{P^1}(x) &= E(T_0^{1,1} - T_0^{0,0} | X_0 = x) = E(T_1^{1,1} - T_0^{0,0} | X_1 = x, X_0 = x) = \\ &= E(T_1^{0,1} - T_1^{1,0} | X_1 = x, X_0 = x) \end{aligned}$$

This allows us to evaluate the impact of the reformed claiming environment on the take-up of those not exposed to any additional financial incentive, i.e. the effect of lowered barriers per se.

Define now $\Delta_t^{P^2}$ as $\Delta_{t,i \in P^2}$. Disentangling the separate role played by lowered barriers, raised incentives and their interaction in the case of P^2 is more problematic, because they acted as concurrent treatments. It is however possible to get some further understanding about their effectiveness. One way to gauge the role played by financial incentives could be the non parametric comparison of $\Delta_t^{P^2}$ for increasing intervals of entitlement. However, the most interesting research questions remains that of evaluating whether the concurrent lowering barrier policies played any role, possibly interacting with the benefit increase. Define the following estimand

$$\Lambda_t = E(T_t^{1,1} - T_t^{0,1}) - E(T_t^{1,0} - T_t^{0,0})$$

where the expectation of the change in take-up due to raised financial incentives, under the pre-reform claiming environment, is subtracted from the expected take-up response to the same increase in financial incentives but under the reformed claiming environment. $\Lambda_t^{P^2}$ therefore reflects any difference in the behavioural response to the raised financial incentives activated by the lowering barrier policies, or the behavioural response that would not have happened without the reformed claiming environment. Identification of $\Lambda_t^{P^2}$ however requires a further assumption:

Assumption 4 $E(T_t^{0,0}|X_t = x, e^0 = \tilde{e}) = E(T_t^{1,0}|X_t = x, e^1 = \tilde{e}),$

$$E(T_t^{1,1}|X_t = x, e^0 = \hat{e}) = E(T_t^{1,1}|X_t = x, e^1 = \hat{e}), \quad \forall x, \forall e^0, e^1, t = 0, 1$$

In other words, it is assumed that, under unchanged claiming environment, the way in which individuals respond to different entitlement levels - the decision rule, for any given level of entitlement, inherent in their utility function- is not altered by the change in entitlement rules alone. This type of assumption has been adopted in evaluation settings (Ichimura et al. 2000; Todd et al. 2006) where ex-ante variation in policy instruments is used to predict behavioural responses to exogenous manipulations of those instruments. Following a similar approach, cross sectional variation in the entitlement levels for pensioner units observed under MIG rules and the pre-reform claiming environment can be exploited to predict what

the behavioural response to entitlement change only would have been, had the claiming environment stayed the same. Under Assumption 4, and conditioning on x and e , Λ_t can be written as

$$\begin{aligned}\Lambda_t(x, e) = & E(T_1^{1,1}|X_1 = x, e^1 = \bar{e}) - E(T_1^{0,1}|X_1 = x, e^1 = \tilde{e}) - \\ & - [E(T_0^{1,0}|X_0 = x, e^0 = \bar{e}) - E(T_0^{0,0}|X_0 = x, e^0 = \tilde{e})]\end{aligned}$$

where \bar{e} is the level that a pensioner unit entitled to \tilde{e} under MIG benefit rules would be entitled to under PC benefit rules. If these expectations can be estimated for all values of x and e and integrated over, corresponding estimators of Λ_0^{P2} and Λ_1^{P2} can be calculated.

3.4 Methods

There are different ways in which conditioning on observables can be performed, and estimators derived. One possibility is to use regression methods to estimate the conditional probability of relevant take-up outcomes, and then average their difference over the distribution of observed covariates. The take-up probability for those simulated to be entitled can be parametrically modeled: adopting the probit specification, the conditional probability of take-up for pensioner unit i under entitlement rules r and claiming environment b can be written as

$$\Phi(z_i^r \beta_b)$$

where z_i denotes the covariates, including the amount of entitlement under rules r , and a set of socio-economic characteristics, Φ the standard normal distribution function, and the subscript b on the coefficients vector β_b reflects the pre or post reform claiming environment under which the estimation sample is observed. Prediction of the conditional probabilities $\Phi(z^1 \hat{\beta}_1)$ and $\Phi(z^0 \hat{\beta}_0)$ allows us to estimate Δ_t by either of

$$\hat{\Delta}_1^{rp} = \frac{1}{N_1} \sum_i \left(\Phi(z^1 \hat{\beta}_1) - \Phi(z^0 \hat{\beta}_0) \right)$$

and

$$\hat{\Delta}_0^{rp} = \frac{1}{N_0} \sum_i \left(\Phi(z^1 \hat{\beta}_1) - \Phi(z^0 \hat{\beta}_0) \right)$$

where N_1 and N_0 denote the post-reform and pre-reform sample numbers respectively. Alternatively, the predicted conditional probability of the observed outcome can be replaced by its realization, with the advantage of reducing the scope for misspecification bias:

$$\hat{\Delta}_1^{ra} = \frac{1}{N_1} \sum_i \left(T_1^{1,1} - \Phi(z^0 \hat{\beta}_0) \right)$$

and

$$\hat{\Delta}_0^{ra} = \frac{1}{N_0} \sum_i \left(\Phi(z^1 \hat{\beta}_1) - T_0^{0,0} \right)$$

Analogously, Λ_t can be estimated by either

$$\hat{\Lambda}_1^{rp} = \frac{1}{N_1} \sum_i \left((\Phi(z^1 \hat{\beta}_1) - \Phi(z^0 \hat{\beta}_1)) - (\Phi(z^1 \hat{\beta}_0) - \Phi(z^0 \hat{\beta}_0)) \right)$$

and

$$\hat{\Lambda}_0^{rp} = \frac{1}{N_0} \sum_i \left((\Phi(z^1 \hat{\beta}_1) - \Phi(z^0 \hat{\beta}_1)) - (\Phi(z^1 \hat{\beta}_0) - \Phi(z^0 \hat{\beta}_0)) \right)$$

or, using the actual realization of observed outcomes,

$$\hat{\Lambda}_1^{ra} = \frac{1}{N_1} \sum_i \left((T_1^{1,1} - \Phi(z^0 \hat{\beta}_1)) - (\Phi(z^1 \hat{\beta}_0) - \Phi(z^0 \hat{\beta}_0)) \right)$$

and

$$\hat{\Lambda}_0^{ra} = \frac{1}{N_0} \sum_i \left((\Phi(z^1 \hat{\beta}_1 - \Phi(z^0 \hat{\beta}_1)) - (\Phi(z^1 \hat{\beta}_0) - T_0^{0,0})) \right)$$

Despite the reduced scope for misspecification bias of Δ^{ra} over Δ^{rp} , the regression based approach anyway relies on the correct specification of the conditional probability function. However another possibility is to follow the non-parametric route and use matching estimators. In this case, for any i observed in t , the outcome of pensioner unit j observed in $(1-t)$ is used as counterfactual outcome, where j is selected to be the ‘nearest’ to i according to a distance function $M(i, j)$ of conditioning covariates x .

Matching estimators for Δ_t can be derived as

$$\hat{\Delta}_1^m = \frac{1}{N_1} \sum_i \left(T_{1,i}^{1,1} - T_{0,j^*(i)}^{0,0} \right)$$

where $i \in N_1$, $j^*(i) \in N_0$ and $M(i, j^*(i)) \leq M(i, j) \forall j \in N_0$; or as

$$\hat{\Delta}_0^m = \frac{1}{N_0} \sum_j \left(T_{1,i^*(j)}^{1,1} - T_{0,j}^{0,0} \right)$$

where $i^*(j) \in N_1$, $j \in N_0$ and $M(i^*(j), j) \leq M(i, j) \forall i \in N_1$, when covariates observed in the pre-reform sample are assumed as baseline.

Estimation of Λ_t can also be based on the comparison of pairs of pensioners units matched across the pre-reform and post-reform samples, as in

$$\hat{\Lambda}_1^{rpm} = \frac{1}{N_1} \sum_i \left((T_{1,i}^{1,1} - \Phi(z_i^0 \hat{\beta}_1)) - (\Phi(z_{j^*(i)}^1 \hat{\beta}_0) - T_{0,j^*(i)}^{0,0}) \right)$$

where $i \in N_1$, $j^*(i) \in N_0$ and $M(i, j^*(i)) \leq M(i, j) \forall j \in N_0$; or

$$\hat{\Lambda}_0^{rpm} = \frac{1}{N_0} \sum_j \left((T_{1,i^*(j)}^{1,1} - \Phi(z_{i^*(j)}^0 \hat{\beta}_1)) - (\Phi(z_j^1 \hat{\beta}_0) - T_{0,j}^{0,0}) \right)$$

where $i^*(j) \in N_1$, $j \in N_0$ and $M(i^*(j), j) \leq M(i, j) \forall i \in N_1$ ^{7 8}.

Any of the described estimators can be computed restricting observations in each sample to their P^1 and P^2 partitions respectively. More detail on the implementation of the above described methods is provided in section 4.3.

4 Implementation

4.1 The Family Resources Survey data

The Family Resources Survey (FRS) was launched by the British Department for Work and Pensions in 1992 as large-scale cross-sectional population survey collecting detailed information about different income sources, assets held and benefit receipt, as well as standard

⁷Again, actual realization of observed outcomes $T_1^{1,1}$ and $T_0^{0,0}$ can be replaced by their parametric predictions $\Phi(z^1 \hat{\beta}_1)$ and $\Phi(z^0 \hat{\beta}_0)$ and corresponding estimators $\hat{\Lambda}_0^{ram}$ and $\hat{\Lambda}_1^{ram}$ derived.

⁸A purely non parametric version of $\hat{\Lambda}_t$ would involve 3 concurrent matching algorithms and result in poor conditioning quality given available sample numbers.

household and personal characteristics. The fieldwork period mirrors the UK fiscal policy year, spanning from April to the March of the following calendar year. Because of its large sample size (approximately 65 thousand individuals) and topics covered, it represents the reference dataset for government official take-up statistics and has been used in several previous analysis of take-up behaviour in the United Kingdom (Hancock et al. 2005; Hernandez et al. 2007; Pudney et al. 2006; Zantomio et al. 2008). A preliminary step in the empirical analysis of claiming behaviour is the simulation of benefit entitlements, based on individuals means, necessary to determine who belongs to the sample of eligible pensioners facing the take-up decision. The FRS level of detail in recording income and assets information makes it particularly suited to the task⁹.

The present empirical analysis is based on the 2002/03 and 2004/05 surveys, sampling 8,599 and 8,625 pensioner units respectively. Figure 2 depicts the timing of each sample fieldwork and relevant policy initiatives. The exclusion of the FRS 2003/04 sample leaves out pensioner units interviewed while the mailing and media campaign took place and allows for the transition to PC to be completed before the fieldwork for the post-reform sample began. The exclusion of the six months prior to the reform addresses potential problems related to anticipated adjustments to expected policy changes.

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

Before the simulation of entitlements, samples were restricted according to five different criteria ensuring a reasonable degree of accuracy in the entitlement simulation. Restrictions excluded pensioner units living with further individuals in the household; where any member was aged below 5 years from pension minimum age; reporting a pending mortgage, earnings or maintenance received from an absent spouse. The inclusion of any such type would have involved an increased scope for measurement error, known to seriously undermine non take-up analysis (Duclos 1995; Hernandez and Pudney, 2007). Moreover the age and no-earnings

⁹Available UK panel data such as ELSA and BHPS suffer from significantly lower sample sizes; and moreover in the BHPS case financial assets information is not available for the years of interest.

conditions address the potential endogeneity arising from the retirement timing decision. Such restrictions reduced samples sizes by approximately one third. Further cases had to be discarded when providing insufficient income or capital information. A series of procedures were applied to detect and resolve inconsistencies in reported benefit receipts, in the spirit of Hancock and Barker (2005)¹⁰. The resulting samples included 1,205 pensioner units eligible to MIG in FRS 2002/03; and 863 pensioner units eligible to PC in FRS 2004/05 who would have also been entitled to income support under MIG rules. From a comparison with the overall population of UK pensioner in each corresponding year, as resulting from the FRS (Table A1 in Appendix), as one might expect eligible pensioners in the two samples emerge as a disadvantaged group, in terms of financial resources, council tax band -reflecting the value of the accommodation-, tenure type and educational attainment. They also appear less likely to be in a partnership, and more likely to be single women.

Table 2a reports the average take-up rates and entitlement levels in the pre-reform and post-reform samples (first and second column) and their respective partitions between those treated by ‘lowering barriers’ policies alone (P^1 , in the third and fourth columns) and those concurrently treated by the raised financial incentives (P^2 , in the fifth and sixth columns).

***** TABLE 2a ABOUT HERE *****

Overall the post reform sample exhibits a higher average take-up, accompanied by higher average entitlement level. Comparing the P^1 and P^2 partitions of both samples, pensioners in the P^1 partition exhibit a remarkably higher average take-up than in P^2 , paired with a higher average pre and post reform entitlement. In fact, as seen in Figure 1, the P^1 partition includes the ‘poorer’ pensioners, with higher entitlement levels resulting from their lower means, and therefore having most to gain from claiming at any point in time. For pensioners in P^2 , the average entitlement appears remarkably higher in the post reform sample than in the pre-reform one, reflecting their financial gain attained through the reformed entitlement rules.

¹⁰This applied mostly to cases confusing receipt of income support, retirement pension and disability benefits when answering survey questions

When subgroups of P^2 defined according to the size of reform induced increase in entitlement are considered, a consistent pattern of higher average take-up for higher entitlement levels emerges.

***** TABLE 2b ABOUT HERE *****

Table 2b produces the same figures separately for couples, single men and single women: the average take-up in P^2 (those who did experience a reform induced entitlement increase) is consistently higher in the post-reform sample than in the pre-reform one. Single women generally exhibit a higher take-up rate than couples and single men; however, the difference from couples' average take-up rate is consistently lower when the post-reform samples are considered.

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

Table 3 presents the sample mean and standard deviation of conditioning covariates, separately for each treatment partition P^1 and P^2 of the pre- and post-reform samples. Covariates include partnership status, gender, house tenure, receipt of disability benefits, years in employment, terminal education age, ethnicity, income and, for the post-reform sample only, a dummy for being entitled to the SC component. The proportion of single women, non white, low pre-benefit income and short employment histories is higher in the the P^1 partitions of each sample, confirming the relatively disadvantaged economic position of those who did not have their entitlement increased by the reform. In both the pre- and the post-reform samples the average entitlement increases when PC, rather than MIG rules are applied, with a slightly higher increase registered under the post-reform sample covariates. As expected, the vast majority of P^2 pensioners result entitled to the SC component under PC rules ¹¹. Figures in the columns reporting the ratio of the pre- and post- sample mean difference to

¹¹The remaining minority experienced an increase in the GC component of PC entitlement due to more generous capital rules, as explained in section 2.2

the standard deviation provide no evidence of severe imbalances in covariates distribution between the two samples.

4.2 Parametric modeling of take-up behaviour

The chosen specification for the probit index function includes a spline function of entitlement with linear restrictions on coefficients bounding the function to be piecewise continuous. This leads to the estimation of a take-up probability with entitlement elasticity varying for different ranges of entitlement values.

To allow for potential structural instabilities entailed by the reformed claiming environment, maximum likelihood estimation can be performed separately for the pre-reform and post-reform samples. Denoting entitlement by e , the remaining covariates by x and the corresponding coefficients γ and η , take-up probability can be written as

$$\begin{aligned} \Phi(\alpha_0 + e\gamma_0 + x'\eta_0) & \quad \text{if } e < k_1 \\ \Phi(\alpha_1 + e\gamma_1 + x'\eta_1) & \quad \text{if } k_1 \leq e < k_2 \\ \Phi(\alpha_2 + e\gamma_2 + x'\eta_2) & \quad \text{if } e \geq k_2 \end{aligned}$$

where knots k_1 and k_2 are set at £10 per week and £20 per week. Maximum likelihood estimated marginal effects are produced in Table 4. The estimates proved not to be sensitive to alternative specifications of the knot values.

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

Results appear in line with previous findings (Blundell et al. 1988; Hancock et al. 2005; Pudney et al. 2006; Hernandez et al. 2007; Zantomio et al. 2008). The take-up probability significantly increases with entitlement level; the reaction to higher entitlement levels is particularly prominent for the £10-£20 per week range for both samples¹². As previously

¹²Figure A1 plots the obtained predicted probability of take up estimated on each sample, for a representative single man aged 75 years old, homeowner, educated above 14, white, who worked 33 year and has a net income of £100 per week (continuous line) and for a representative single woman, aged 89 years old, disabled, educated below 14, white, who worked 20 years and has a net income of £45 per week.

found, the take-up probability is consistently lower for pensioners on higher pre-benefit incomes and for more educated and non-white pensioners. Other work has related similar findings to higher stigma perceived by more educated people and application difficulties for members of ethnic minorities (Warlick 1982; Currie 2004; Hernanz 2004). When estimation is performed on the pre-reform sample, home owners appear significantly less likely to claim than the baseline category of private renters; however the coefficient loses significance for the post-reform sample, while social tenants exhibit a significantly higher claiming probability, possibly related to their better access to relevant information or higher exposure to the ‘lowering barriers’ policies. More years in employment and entitlement to the saving credit component are also positively correlated with take-probability for the post reform sample. Despite the unchanged sign of marginal effects between the pre- and post-reform sample estimates, the fact that some of them are significant only for estimation on the post-reform sample hints at some kind of structural change having taken place after the reform, and the hypothesis of structural stability across survey years is rejected. This might reflect an underlying change in take-up behaviour induced by the PC introduction, for example through the reformed claiming environment experienced by pensioners observed in the post-reform sample.

4.3 Matching algorithms

While the random design of the FRS sampling and the exogenous nature of the Pension Credit introduction rule out the chance of endogenous selection into the pre- or post-reform samples, bias might arise both from different patterns of non response in the two years and the chance of demographic and socioeconomic trends affecting the underlying population. Matching reduces the scope for such confounding factors. A variety of matching estimators can be used, depending on the distance function to be minimized. One possibility is to match on the conditional probability of being allocated to the post-reform, rather than the pre-reform sample, usually referred to as ‘propensity score’ in the context of the evaluation

literature (Rosenbaum and Rubin, 1983). While random allocation of pensioners to the pre- or post-reform sample would imply a 0.5 constant probability, any difference in non response or population characteristics would result in a deviation from this value. In fact, the parametric estimates of the propensity score, reported in Appendix Table A2, reveal a significantly higher probability of single, more educated, non-white, non disabled and on higher income to be sampled in the post-reform year, reflecting an expected trend in the underlying population characteristics, evident from Table 3. Nearest neighbour matching on the propensity score is performed with replacement for both samples baseline distribution of covariates. Estimated propensity score distributions by sample, represented in Appendix Figure A2, confirm the soundness of the common support Assumption 2.

A purely non parametric alternative is offered by ‘matching on variables’ algorithms. In this case the choice of the nearest neighbour is aimed at minimizing the difference in pre- and post- reform entitlements e^0 and e^1 between each matched pair of pensioners. Distance is measured using the Mahalanobis metric (Rubin, 1980), accounting for the within sample-covariance of e^0 and e^1 . As a further adjustment for confounding variables, matching can be preceded by stratification, whereby each sample is partitioned into mutually exclusive ‘strata’, defined according to demographic characteristics. Both stratification by partnership status and gender, and a finer one considering also age and years in employment ¹³ are implemented. While realizing a closer conditioning on covariates, stratification reduces statistical precision, depending on the size of available samples. Another option is the impositions of a caliper, representing the maximum allowed distance between matched pairs, so that pairs whose distance exceed the caliper would be discarded. While ensuring that matching quality is not compromised by imbalances in the covariates distributions, this option also increases standard errors.

The quality of conditioning achieved through matching can be gauged from the distribution of covariates in the unmatched and matched samples (Table A3 in Appendix).

¹³only for single females, samples numbers would not allow the same for couples or single male pensioners.

While propensity score matching allows more observables to be conditioned on, matching on variables gives a closer match on pre and post reform entitlement levels, and on further characteristics when preceded by stratification. The imposition of a caliper gives more precise conditioning, although at the cost of a reduced support for the calculation of the reform effect.

5 Results

Tables 5 and 6 report estimates of the Δ_1 and Δ_0 average treatment effects respectively, computed using the different methods outlined in sections 3 and 4. The first column of each table refers to the average reform effect on the set P of ‘treated’ pensioner units. The Pension Credit introduction is found to bear a positive and significant impact on take-up behaviour, consistent across the different conditioning methods adopted, and the choice of the baseline covariates year. The average increase is estimated around 8 percentage points from a baseline take-up probability of 0.58 under the parametric approach. The size of both $\hat{\Delta}_1$ and $\hat{\Delta}_0$ increases when matching on pre- and post- reform entitlements is used, with $\hat{\Delta}_1$ exceeding 20 percentage points; despite the lower statistical precision, reflected by the increased standard errors, the effect is still significant under the non parametric approach. The imposition of the 0.05 caliper, ensuring a better matching quality, brings to a slightly higher and still significant reform effect. When stratification by gender and partnership is used, the average impact remains positive and significant while under finer stratification by gender, partnership, age and years in employment, it loses significance under both choices of the baseline covariates year. A consistently positive and significant effect, intermediate in size between the purely parametric and purely non parametric cases is also obtained when matching is based on the parametric estimation of the propensity score.

***** TABLES 5 and 6 ABOUT HERE *****

The second column of Tables 5 and 6 presents the average reform effect on the take-up probability of the subgroup P^1 of pensioners who were exposed to the reformed claiming environment but did not face any increase in financial incentives. This group gathers the ‘poorest’ pensioners, who have most to gain from claiming in terms of entitlement and in fact exhibit a relatively higher pre-reform take-up probability of 0.75. While the comparison of the pre- and post-reform take-up sample averages would suggest a post reform claiming decrease, such crude difference could simply reflect confounding factors, such as differences in the distribution of covariates affecting the take-up decision in the two samples. Once covariates are controlled for, either through regression or matching, no significant impact of the PC reform is generally found, with the only exception of a slight decrease in take-up probability when the effect is measured by $\hat{\Delta}^{rp}$. ‘Lowering barriers’ policies, by themselves seem to have been either insufficient or ineffective in triggering take-up, for this subgroup.

When the subgroup P^2 (third column) of pensioners who were ‘treated’ by both the ‘lowered barriers’ and the reform induced entitlement increase is considered, a sizeable and significant positive reform impact on the take-up probability is consistently found across the different conditioning methods and baseline covariates years. The size of the average increase spans from about 10 percentage points, from a pre-reform take-up probability of 0.54, for the parametric version under 2002/03 covariates, to more than 35 percentage points when matching on pre and post reform entitlements under a 0.05 caliper is adopted for 2004/05 covariates. $\hat{\Delta}_1$ remains significant even when finer stratification is used.

The last three columns of Tables 5 and 6 show the average reform effect on subgroups of P^2 defined according to different ranges of reform induced increase in entitlement: below £5 per week, between £5 and £10 per week and above £10 per week. A positive gradient in the take-up response to higher increases in entitlement emerges systematically under different specifications of $\hat{\Delta}_1$ and $\hat{\Delta}_0$. While evidence of a reform-induced increase in take-up for pensioners experiencing a less than £5 weekly increase is mixed, a consistently higher and significant take-up response is apparent for the higher ranges of entitlement increase, with

the difference in the average response for those who experienced a less than £5 increase and a more than £10 increase statistically significant in most cases. Corresponding figures produced separately for couples, single men and single women, show that while both couples and single women exhibit a positive behavioral response to raised financial incentives, single men seem to face higher barriers to claim and to react only to sizeable additions to the monetary rewards.

Results so far have provided no evidence of a take-up response to the lowered claiming barriers for pensioners unaffected by the new entitlement rules, but have revealed a significant take-up increase for those who benefitted from increased financial incentives. Despite resulting ineffective or insufficient for the first group, lowered claiming barriers might still have played a role in amplifying the take-up response of pensioners in the second group. Pensioners in P^2 might for example share characteristics making them more receptive to such policies. Besides, it might be that ‘lowering barriers’ policies become effective only if concurrent with raised financial incentives.

Table 7 presents estimates of Λ_1 and Λ_0 , reflecting that part of the take-up response to more generous benefit rules that would not have happened under unchanged pre-reform claiming environment, or had the ‘lowering barriers’ policies not been implemented. As expected no effect is found for P^1 pensioners, strengthening the previous conclusion about the inefficacy of the ‘lowered barriers’ policies for this subgroup. However, a consistently positive and significant effect, around 5 – 6 percentage points, is found for P^2 , pointing at a concurrent contribution of the same policies in triggering the take-up response of the second subgroup.

***** TABLES 7 ABOUT HERE *****

When Λ_t is estimated for different ranges of reform induced entitlement increases, it becomes apparent that the ‘lowering barriers’ policies played a role for the subgroup of P^2

experiencing a moderate entitlement increase, while for those with less than £5 and more than £10 per week increases, $\hat{\Lambda}_t$ is significantly lower and possibly not statistically significant when matching is used. The obtained results seem to suggest that, while ineffective if not accompanied by a concurrent non trivial entitlement increase, ‘lowering barriers’ policies appear superfluous when accompanied by a substantial financial incentive increase of £10 per week or more.

6 Conclusions

This study has shed some light on whether individuals’ claiming behaviour responds to financial incentives and to measures intended to lower non monetary claiming costs. Identification of the behavioural response exploited an exogenous policy reform, the introduction of the Pension Credit in UK in 2003, involving the implementation of ‘lowering barriers’ policies and besides, for a subgroup of eligible pensioners, a real increase in the financial incentive to claim. Because of the limited sample size of available panel data, and the high information requirements necessary to the simulation of entitlements, repeated cross sections of a particularly rich British dataset, the Family Resources Survey, were used. Both parametric modelling and non parametric ‘selection on observables’ approaches to evaluation have been implemented, providing fairly robust results.

A first research question concerned the overall effectiveness of the Pension Credit introduction in increasing the take-up of pensioners who would have been entitled under both the pre- and post-reform systems. The average effect on the treated was found positive and significant across the different specifications implemented, supporting the idea that overall the reform was successful in fostering new claims.

A second research question involved disentangling the role played by the ‘raising incentives’ and lowering barriers’ policies. The ‘financial gain’ route, available to a subgroup of pensioners only, appears to have been effective, as a positive gradient of take-up response

to higher increases in financial incentives was found. This result is consistent with previous studies (Zantomio et al. 2008; Dahan et al. 2007) and confirms the idea that individuals' claiming behaviour responds to financial incentives.

'Lowering barriers' policies seem to have partially enhanced the effect of financial incentives for pensioners who were treated by both types of measure. However, evidence of a poorly enhancing effect for those who experienced a less than £5 per week or a more than £10 per week increase in entitlement seems to suggest that only the presence of concurrent financial incentives activate responsiveness to lowered barriers, while at the same time similar measures become 'superfluous' once the financial reward increase exceeds £10 per week.

No evidence of a response to 'lowering barriers' policies *per se* was found, as no take-up increase has emerged for pensioners who did not have their entitlement increased by the reformed PC rules, despite being treated by the 'lowering barriers' policies. While pensioners in this group exhibit relatively high take-up rates because of their relatively high entitlement levels in the first place, it is for 'highly resistant' non-claimants in this group that non take-up has the most severe consequences in terms of poverty gap. The 'lowering barriers' policy alone, as implemented under the PC reform, does not appear to have been effective in tackling the issue.

Take-up is key to the effectiveness of existing targeted welfare programs. Individuals' responsiveness to lowered barriers and raised financial incentives is crucial to the success of policy interventions aimed at increasing it. This paper provides an answer to questions concerning the relative and interacted efficacy of both routes. The finding that raised financial incentives increase the probability of take up has important policy implications as it suggests that there is scope for successful government intervention. The evidence that individuals react more to increases in financial incentives than to lowered non monetary claiming costs is suggestive as to which type of measures governments might want to concentrate resources on. Clearly, further research on the effectiveness of lowering barrier policies - implemented

in other contexts or other forms, or targeted at younger individuals - is required before any sound conclusion can be drawn.

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Table 1: Pre- and post-reform benefit rates
(pounds per week, 2003/04 prices)

	single pensioner		pensioner couple	
	MIG	PC	MIG	PC
<i>Needs:</i>				
Basic Allowance (MIG or GC)	102.10	102.10	155.80	155.80
Severe Disability premium	42.90	42.90	85.80	85.80
Carer premium	25.10	25.10	50.20	50.20
Saving Credit treshold	-	77.35	-	123.80
Maximum Saving Credit	-	14.79	-	19.20
Capital limits	6,000-12,000	6,000+	6,000-12,000	6,000+
Capital income ratio	1£ every 250£	1£ every 500£	1£ every 250£	1£ every 500£

Source: CPAG (2003/04)

Table 2a: Empirical entitlement levels and take-up rates, pre- and post-reform
(standard errors in parentheses)

	<i>sample</i>	<i>P</i>		<i>P¹</i>		<i>P²</i>	
		Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform
N_t		1,205	863	244	290	961	573
Take-up, $T_t^{r=t,l=t}$	mean	0.583	0.640	0.746	0.676	0.541	0.621
	(std. er.)	(0.014)	(0.016)	(0.028)	(0.028)	(0.016)	(0.020)
Entitlement, $e^{r=t}$	mean	28.842	38.945	55.396	53.465	22.100	31.597
	(std. er.)	(0.752)	(0.914)	(2.000)	(1.903)	(0.631)	(0.830)

Notes

Pre-reform sample statistics are based on the FRS 2002/03; post-reform sample statistics on the FRS 2004/05.

The set *P* includes all pensioner units who would have been eligible under pre- and post-reform rules. The partition *P¹* is restricted to pensioner units who did not have their entitlement increased by the reformed Pension Credit rules, while the partition *P²* covers those who experienced a real increase in entitlement as a consequence of the Pension Credit introduction.

N_t denotes sample number.

Source: Author's calculations based on the Family Resources Survey

Table 2b: Empirical entitlement levels and take-up rates, pre- and post-reform
(standard errors in parentheses)

	<i>sample</i>	<i>P</i>		<i>P</i> ¹		<i>P</i> ²	
		Pre-reform	Post-reform	Pre-reform	Post-reform	Pre-reform	Post-reform
<i>Couples</i>	N _t	229	168	30	38	199	130
Take-up, $T_t^{r=t,l=t}$	mean	0.48	0.63	0.57	0.61	0.47	0.63
	(std. er.)	(0.03)	(0.04)	(0.09)	(0.08)	(0.04)	(0.04)
Entitlement, $e^{r=t}$	mean	27.63	42.45	58.68	69.94	22.95	34.41
	(std. er.)	(1.99)	(2.54)	(6.59)	(6.82)	(1.85)	(2.16)
<i>Single male</i>	N _t	172	137	24	33	148	104
Take-up, $T_t^{r=t,l=t}$	mean	0.52	0.58	0.63	0.64	0.51	0.57
	(std. er.)	(0.04)	(0.04)	(0.10)	(0.09)	(0.04)	(0.05)
Entitlement, $e^{r=t}$	mean	26.43	34.32	66.62	47.47	19.91	30.15
	(std. er.)	(2.03)	(1.93)	(7.56)	(5.40)	(1.44)	(1.70)
<i>Single female</i>	N _t	804	558	190	219	614	339
Take-up, $T_t^{r=t,l=t}$	mean	0.58	0.66	0.79	0.69	0.57	0.63
	(std. er.)	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Entitlement, $e^{r=t}$	mean	28.84	39.02	53.46	51.51	22.35	30.96
	(std. er.)	(0.75)	(1.09)	(2.14)	(2.02)	(0.71)	(1.00)

Notes

Pre-reform sample statistics are based on the FRS 2002/03; post-reform sample statistics on the FRS 2004/05.

The set P includes all pensioner units who would have been eligible under pre- and post-reform rules. The partition P¹ is restricted to pensioner units who did not have their entitlement increased by the reformed Pension Credit rules, while the partition P² covers those who experienced a real increase in entitlement as a consequence of the Pension Credit introduction.

N_t denotes sample number.

Source: Author's calculations based on the Family Resources Survey

Table 3: Sample statistics for conditioning covariates
(standard deviations in parentheses)

	<i>sample</i>	P			P ¹			P ²		
		Pre-reform	Post-reform	diff/sd	Pre-reform	Post-reform	diff/sd	Pre-reform	Post-reform	diff/sd
couple	<i>mean</i>	.190	.195	.01	.123	.131	.02	.207	.227	.05
	<i>(s.d.)</i>	(.392)	(.396)		(.329)	(.338)		(.405)	(.419)	
single female	<i>mean</i>	.684	.664	-.04	.795	.772	-.05	.656	.609	-.10
	<i>(s.d.)</i>	(.465)	(.473)		(.404)	(.420)		(.475)	(.488)	
age (head)	<i>mean</i>	77.74	77.35	-.06	74.668	76.41	.24	78.52	77.82	-.10
	<i>(s.d.)</i>	(7.12)	(7.05)		(7.080)	(7.369)		(6.91)	(6.84)	
home owner	<i>mean</i>	0.351	.386	.07	.262	.362	.21	.374	.398	.05
	<i>(s.d.)</i>	(0.477)	(.487)		(.441)	(.481)		(.484)	(.490)	
years in employment (head)	<i>mean</i>	31.90	31.25	-.04	27.13	25.81	-.07	33.11	34.00	.05
	<i>(s.d.)</i>	(17.52)	(17.59)		(17.29)	(17.93)		(17.38)	(16.77)	
social tenant	<i>mean</i>	.571	.523	-.10	.631	.528	-.21	.556	.520	-.07
	<i>(s.d.)</i>	(.495)	(.500)		(.483)	(.500)		(.497)	(.500)	
educated beyond 14 years old	<i>mean</i>	.341	.417	.15	.455	.493	.08	.312	.379	.14
	<i>(s.d.)</i>	(.474)	(.493)		(.499)	(.501)		(.464)	(.485)	
non white	<i>mean</i>	.152	.211	.14	.213	.276	.14	.136	.178	.11
	<i>(s.d.)</i>	(.359)	(.408)		(.410)	(.448)		(.343)	(.383)	
anyone disabled	<i>mean</i>	.157	.127	-.09	.152	.134	-.05	.158	.124	-.10
	<i>(s.d.)</i>	(0.364)	(.334)		(.359)	(.342)		(.365)	(.330)	
net income	<i>mean</i>	104.19	109.33	.11	68.21	78.23	.29	113.32	125.07	.27
	<i>(s.d.)</i>	(42.51)	(46.25)		(36.23)	(34.34)		(39.01)	(43.46)	
entitlement under MIG rules	<i>mean</i>	28.84	32.37	.12	55.39	53.46	-.06	22.10	21.70	-.02
	<i>(s.d.)</i>	(26.10)	(29.42)		(31.23)	(32.41)		(19.57)	(20.84)	
entitlement under PC rules	<i>mean</i>	34.45	38.94	.17	55.39	53.46	-.06	29.14	31.59	.12
	<i>(s.d.)</i>	(24.35)	(26.85)		(31.23)	(32.41)		(18.90)	(19.87)	
entitled to the Saving Credit component under PC	<i>mean</i>	.793	.643	-.31	-	-		.994	.969	-.14
	<i>(s.d.)</i>	(0.406)	(0.479)		-	-		(.079)	(.175)	

Notes

Pre-reform sample statistics are based on the FRS 2002/03; post-reform sample statistics on the FRS 2004/05.

The set P includes all pensioner units who would have been eligible under pre- and post-reform rules. The partition P¹ is restricted to pensioner units who did not have their entitlement increased by the reformed Pension Credit rules, while the partition P² covers those who experienced a real increase in entitlement as a consequence of the Pension Credit introduction.

Source: Author's calculations based on the Family Resources Survey

Table 4 : Parametric take-up probit estimates

Marginal effects(standard errors in brakets)		
	Pre-reform	Post-reform
Couple	0.072 [0.212]	0.100* [0.099]
Age - head	-0.051 [0.238]	0.028 [0.556]
Age squared - head	0.000 [0.287]	0.000 [0.438]
Female	0.049 [0.295]	0.098** [0.050]
Tenure: home owner	-0.201*** [0.001]	0.009 [0.887]
Tenure: social tenant	0.058 [0.320]	0.198*** [0.001]
Anyone educated beyond 14	-0.076** [0.031]	-0.072* [0.058]
Non white - head	-0.085* [0.052]	-0.178*** [0.000]
Years in employment - head	0.000 [0.976]	0.003*** [0.003]
Anyone registered as disabled	0.132*** [0.003]	0.054 [0.309]
Net income	-0.002*** [0.000]	-0.002*** [0.000]
Ln entitlement: (below £10 pw)	0.213*** [0.000]	- -
Lnentitlement: (£10-20 pw)	0.427*** [0.000]	0.862** [0.015]
Ln entitlement: (above £20pw)	-0.054 [0.140]	0.06 [0.129]
Whether entitled to the saving credit	- -	0.090* [0.070]
Number of Observations	1,204	863
LR $\chi^2(k)$	265.9	103.51
Prob > χ^2	0.000	0.000
Pseudo R ²	0.1626	0.0918

Notes

*** denotes coefficient statistically significant at the 1% level; ** denotes coefficient statistically significant at the 5% level; * denotes coefficient statistically significant at the 10% level

Source: Author's calculations based on the Family Resources Survey

Table 5: Estimates of impact Δ_1 under 2004/05 baseline covariates

			P	P ¹	P ²	P ² , by Reform-induced entitlement increase (£ per week)			
Conditioning Method						Less than 5	5-10	Above 10	
- Regression	<i>Actual, Δ_I^{ra}</i>	<i>ATT</i>	.086	-.025	.142	.065	.122	.188	
		<i>(se)</i>	<i>(.016)</i>	<i>(.025)</i>	<i>(.020)</i>	<i>(.036)</i>	<i>(.036)</i>	<i>(.030)</i>	
	<i>Predicted, Δ_I^{rp}</i>	<i>ATT</i>	.086	-.043	.151	.020	.113	.233	
		<i>(se)</i>	<i>(.006)</i>	<i>(.006)</i>	<i>(.007)</i>	<i>(.008)</i>	<i>(.009)</i>	<i>(.010)</i>	
- Matching	<i>no caliper</i>	<i>ATT</i>	.213	.083	.279	.015	.268	.374	
		<i>(se)</i>	<i>(.058)</i>	<i>(.101)</i>	<i>(.072)</i>	<i>(.073)</i>	<i>(.093)</i>	<i>(.092)</i>	
	<i>caliper 0.5</i>	<i>ATT</i>	.228	.088	.361	.023	.431	.414	
		<i>(se)</i>	<i>(.061)</i>	<i>(.102)</i>	<i>(.086)</i>	<i>(.093)</i>	<i>(.122)</i>	<i>(.103)</i>	
	<i>%dropped</i>		.07	.01	.21	.35	.33	.26	
	- Matching+ stratification	<i>3 strata</i>	<i>ATT</i>	.19	.01	.28	-.01	.21	.329
<i>(se)</i>			<i>(.075)</i>	<i>(.109)</i>	<i>(.095)</i>	<i>(.086)</i>	<i>(.135)</i>	<i>(.128)</i>	
<i>3 strata caliper 0.5</i>		<i>ATT</i>	.208	.029	.369	.122	.280	.356	
		<i>(se)</i>	<i>(.078)</i>	<i>(.111)</i>	<i>(.115)</i>	<i>(.105)</i>	<i>(.154)</i>	<i>(.145)</i>	
<i>%dropped</i>		10.66	3.79	24.26	26.87	22.88	34.27		
<i>10 strata</i>		<i>ATT</i>	.105	-.103	.211	.015	.170	.336	
		<i>(se)</i>	<i>(.117)</i>	<i>(.198)</i>	<i>(.137)</i>	<i>(.175)</i>	<i>(.204)</i>	<i>(.191)</i>	
<i>10 strata caliper 0.5</i>		<i>ATT</i>	.113	-.092	.263	-	-	-	
		<i>(se)</i>	<i>(.130)</i>	<i>(.205)</i>	<i>(.161)</i>	-	-	-	
<i>%dropped</i>		20.86	10.34	30.89	-	-	-		
- Propensity score		<i>ATT</i>		.093	-.059	.101	-.030	.103	.234
		<i>(se)</i>		<i>(.030)</i>	<i>(.061)</i>	<i>(.044)</i>	<i>(.075)</i>	<i>(.082)</i>	<i>(.067)</i>

Notes

The set P includes all pensioner units who would have been eligible under pre- and post-reform rules. The partition P¹ is restricted to pensioner units who did not have their entitlement increased by the reformed Pension Credit rules, while the partition P² covers those who experienced a real increase in entitlement as a consequence of the Pension Credit introduction.

Source: Author's calculations based on the Family Resources Survey

Table 6: Estimates of impact Δ_0 under 2002/03 baseline covariates

			P ² , by						
			P	P ¹	P ²	Reform-induced entitlement increase (£ per week)			
Conditioning Method						Less than 5	5-10	Above 10	
- Regression	<i>Actual, Δ₀^{ra}</i>	<i>ATT</i>	.077	-.022	.102	.001	.107	.233	
		<i>(se)</i>	<i>(.014)</i>	<i>(.028)</i>	<i>(.016)</i>	<i>(.023)</i>	<i>(.032)</i>	<i>(.028)</i>	
	<i>Predicted, Δ₀^{rp}</i>	<i>ATT</i>	.077	-.027	.103	.015	.129	.197	
		<i>(se)</i>	<i>(.004)</i>	<i>(.007)</i>	<i>(.005)</i>	<i>(.005)</i>	<i>(.006)</i>	<i>(.010)</i>	
- Matching	<i>no caliper</i>	<i>ATT</i>	.111	-.061	.153	.157	.193	.189	
		<i>(se)</i>	<i>(.058)</i>	<i>(.092)</i>	<i>(.063)</i>	<i>(.128)</i>	<i>(.064)</i>	<i>(.065)</i>	
	<i>caliper 0.5</i>	<i>ATT</i>	.126	-.064	.183	.139	.240	.238	
		<i>(se)</i>	<i>(.061)</i>	<i>(.093)</i>	<i>(.072)</i>	<i>(.075)</i>	<i>(.069)</i>	<i>(.076)</i>	
	<i>%dropped</i>		7.22	3.69	19.35	41.62	13.70	30.64	
- Matching+ stratification	<i>3 strata</i>	<i>ATT</i>	.08	-.05	.15	.12	.16	.246	
		<i>(se)</i>	<i>(.07)</i>	<i>(.11)</i>	<i>(.08)</i>	<i>(.13)</i>	<i>(.10)</i>	<i>(.10)</i>	
	<i>3 strata caliper 0.5</i>	<i>ATT</i>	.08	-.06	.16	.13	.20	.29	
		<i>(se)</i>	<i>(.08)</i>	<i>(.11)</i>	<i>(.08)</i>	<i>(.08)</i>	<i>(.10)</i>	<i>(.11)</i>	
	<i>%dropped</i>		7.22	4.92	24.45	45.69	20.37	33.33	
	<i>10 strata</i>	<i>ATT</i>	.041	-.016	.096	-.025	.104	.222	
		<i>(se)</i>	<i>(.106)</i>	<i>(.173)</i>	<i>(.123)</i>	<i>(.169)</i>	<i>(.189)</i>	<i>(.178)</i>	
	<i>10 strata caliper 0.5</i>	<i>ATT</i>	.046	-.033	.135	-	-	-	
		<i>(se)</i>	<i>(.118)</i>	<i>(.173)</i>	<i>(.135)</i>	-	-	-	
	<i>%dropped</i>		18.17	12.70	29.97	-	-	-	
- Propensity score	<i>ATT</i>		.083	-.029	.140	.028	.125	.239	
	<i>(se)</i>		<i>(.030)</i>	<i>(.067)</i>	<i>(.043)</i>	<i>(.080)</i>	<i>(.063)</i>	<i>(.065)</i>	

Notes

The set P includes all pensioner units who would have been eligible under pre- and post-reform rules. The partition P¹ is restricted to pensioner units who did not have their entitlement increased by the reformed Pension Credit rules, while the partition P² covers those who experienced a real increase in entitlement as a consequence of the Pension Credit introduction.

Source: Author's calculations based on the Family Resources Survey

Table 7: Estimates of impact Λ_0 and Λ_1

		Λ_1							Λ_0						
					P2							P2			
		P	P1	P2	Reform induced entitlement increase (£ per week)				P	P1	P2	Reform induced entitlement increase (£ per week)			
					less 5	5-10	above10					less 5	5-10	above10	
Regression	actual	Λ^{ra}	.045	.018	.059	.058	.134	.027	ATT	.043	-.005	.055	.037	.151	-.004
		(se)	(.012)	(.022)	(.013)	(.036)	(.023)	(.016)	(se)	(.011)	(.024)	(.012)	(.019)	(.021)	(.022)
	predicted	Λ^{rp}	.045	.000	.068	.014	.125	.064	ATT	.043	.000	.054	.023	.129	.027
		(se)	(.012)	(.004)	(.018)	(.005)	(.028)	(.033)	(se)	(.012)	(.004)	(.016)	(.006)	(.037)	(.036)
Regression+Matching(b1,b2)															
	actual	Λ^{ram}	.039	.018	.050	.042	.108	.022	ATT	.05	-.005	.059	.009	.154	.040
		(se)	(.013)	(.025)	(.014)	(.039)	(.025)	(.017)	(se)	(.015)	(.025)	(.018)	(.021)	(.037)	(.039)
	predicted	Λ^{rpm}	.039	.000	.059	-.002	.099	.066	ATT	.047	.000	.059	-.005	.133	.076
		(se)	(.011)	(.003)	(.017)	(.004)	(.026)	(.031)	(se)	(.009)	(.003)	(.011)	(.002)	(.022)	(.030)

Notes

Λ_1 is derived under 2004/05 baseline covariates; Λ_0 is derived under 2002/03 baseline covariates

Source: Author's calculations based on the Family Resources Survey

FIGURE 1

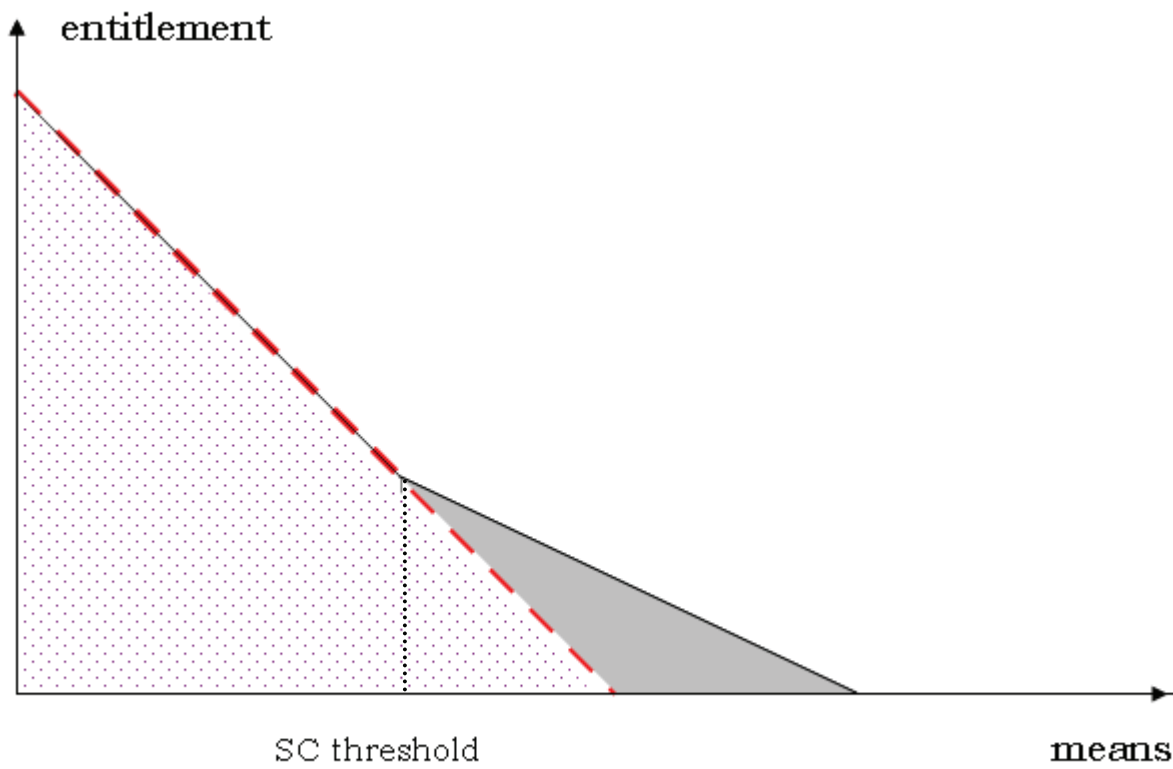
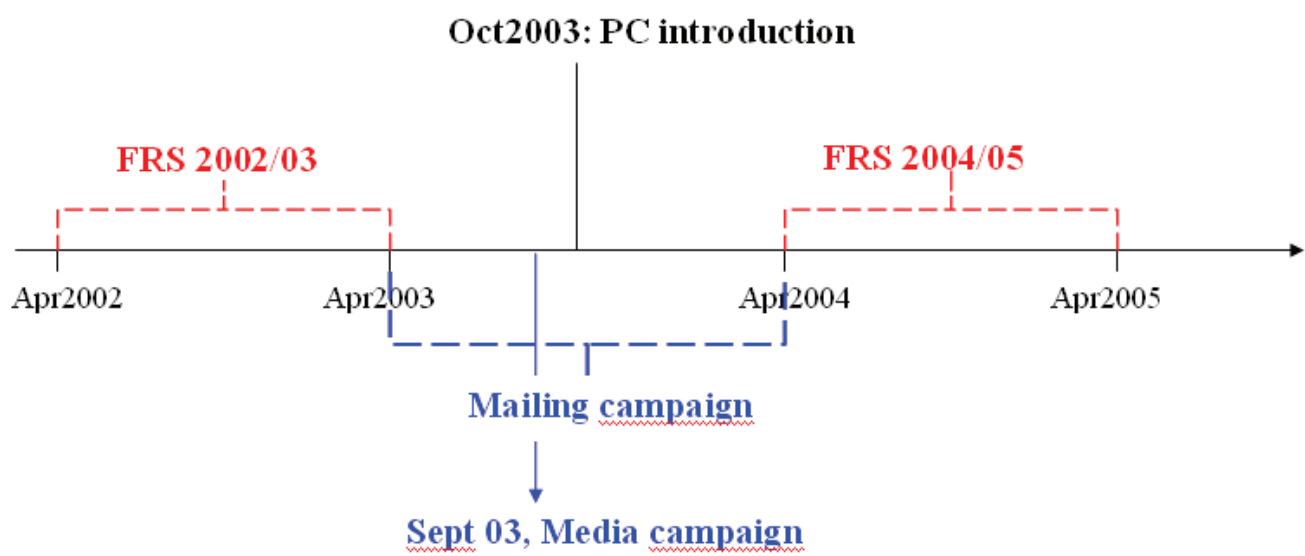


FIGURE 2



APPENDIX

TABLE A1: Sample descriptive statistics: UK pensioners and eligible pensioners

		pensioners sampled in FRS 2002/03		pensioners sampled in FRS 2004/05	
		eligible to MIG	Uk ¹	eligible to PC	Uk ¹
number of pensioner units		1,205	8,599	1,683	8,625
living in couple	<i>mean</i>	.324	.564	.332	.567
	<i>(se)</i>	(.012)	(.000)	(.015)	(.000)
single male	<i>mean</i>	.119	.105	.131	.107
	<i>(se)</i>	(.009)	(.000)	(.011)	(.000)
single female	<i>mean</i>	.558	.331	.537	.326
	<i>(se)</i>	(.013)	(.000)	(.016)	(.000)
age	<i>mean</i>	77.043	73.380	76.553	73.524
	<i>(se)</i>	(.191)	(.002)	(.224)	(.002)
any ed. qualification	<i>mean</i>	.098	.281	.054	.205
	<i>(se)</i>	(.008)	(.000)	(.007)	(.000)
non white	<i>mean</i>	.129	.056	.043	.036
	<i>(se)</i>	(.009)	(.000)	(.006)	(.000)
living with others in hh	<i>mean</i>	.000	.136	.000	.126
	<i>(se)</i>	(.000)	(.000)	(.000)	(.000)
economically active	<i>mean</i>	.010	.073	.006	.080
	<i>(se)</i>	(.003)	(.000)	(.002)	(.000)
retired in last year	<i>mean</i>	.003	.018	.010	.018
	<i>(se)</i>	(.002)	(.000)	(.003)	(.000)
receive maintenace from absent partner	<i>mean</i>	.000	.001	.000	.001
	<i>(se)</i>	(.000)	(.000)	(.000)	(.000)
pending mortgage	<i>mean</i>	.000	.074	.000	.069
	<i>(se)</i>	(.000)	(.000)	(.000)	(.000)
tenure: own	<i>mean</i>	.377	.728	.408	.758
	<i>(se)</i>	(.013)	(.000)	(.015)	(.000)
tenure: social renter	<i>mean</i>	.545	.228	.495	.197
	<i>(se)</i>	(.013)	(.000)	(.016)	(.000)
receving disability benefit	<i>mean</i>	.286	.160	.284	.169
	<i>(se)</i>	(.012)	(.000)	(.014)	(.000)
in council tax band a or b	<i>mean</i>	.610	.428	.725	.427
	<i>(se)</i>	(.013)	(.000)	(.014)	(.000)
equivament hh non benefit income	<i>mean</i>	3.137	28.182	3.683	30.731
	<i>(se)</i>	(.139)	(.029)	(.284)	(.030)
personal income	<i>mean</i>	140.913	193.437	152.779	215.235
	<i>(se)</i>	(1.589)	(.083)	(2.143)	(.061)
whether holds investments	<i>mean</i>	.044	.273	.037	.259
	<i>(se)</i>	(.005)	(.000)	(.006)	(.000)

¹ Uk estimates are weighted to correct for sample design and survey non response.

Source: Author's calculations based on the Family Resources Survey

TABLE A2: Propensity score Probit regression estimates

	Marginal effects [absolute value of z statistics]
pensioner couple	-.105*** [2.71]
female head	-.052 [1.59]
anyone educated beyond 14	.096*** [4.10]
non white head	.087*** [2.99]
years worked - head	-.001 [1.16]
anyone registered as disabled	-.096*** [3.00]
net income	.001*** [4.22]
Number of observations	2,068
LR $X^2(7)$	47.34
Prob > X^2	.0000
Pseudo R^2	.0168

Notes

* statistically significant at the 10% level ; ** statistically significant at the 5% level; *** statistically significant at the 1% level

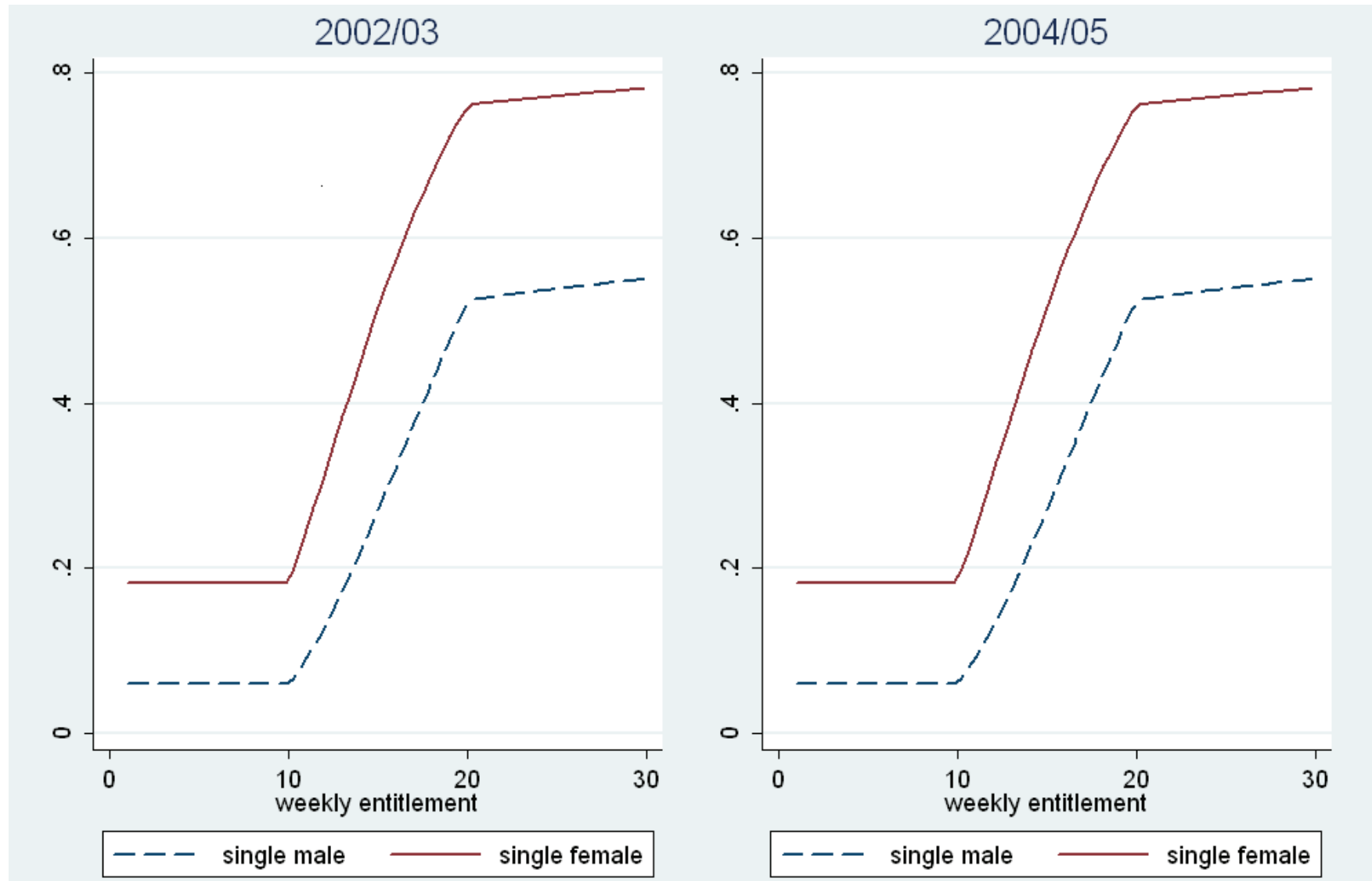
Source: Author's calculations based on the Family Resources Survey

TABLE A3: Covariates sample mean in unmatched and matched samples

		2002/03 as conterfactual to 2004/05				2004/05 as conterfactual to 2002/03			
		unmatched		matched		unmatched		matched	
Matching on e^0, e^1		<i>Treated</i> 2004/05	<i>Controls</i> 2002/03	<i>Treated</i> 2004/05	<i>Controls</i> 2002/03	<i>Treated</i> 2002/03	<i>Controls</i> 2004/05	<i>Treated</i> 2002/03	<i>Controls</i> 2004/05
no caliper	e^0	32.37	28.84	-	31.29	28.84	32.37	-	30.38
	e^1	38.94	34.45	-	37.80	34.45	38.94	-	35.99
0.05 caliper	e^0	32.37	28.84	30.59	29.71	28.84	32.37	26.93	28.54
	e^1	38.94	34.45	36.57	35.69	34.45	38.94	31.99	33.53
0.01 caliper	e^0	32.37	28.84	31.94	31.75	28.84	32.37	36.97	37.27
	e^1	38.94	34.45	37.18	36.99	34.45	38.94	40.28	40.54
3 strata, no caliper	e^0	32.37	28.84	-	31.08	28.84	32.37	-	31.37
	e^1	38.94	34.45	-	37.50	34.45	38.94	-	36.91
	Couple	.194	.190	-	.194	.190	.195	-	.190
10 strata, no caliper	e^0	32.37	28.84	-	30.51	28.84	32.37	-	31.29
	e^1	38.94	34.45	-	36.85	34.45	38.94	-	36.82
	Couple	.194	0.190	-	.194	.190	.195	-	.190
	Age head	77.35	77.741	-	76.92	77.74	77.35	-	77.95
Propensity score									
	e^0	32.37	28.84	-	29.23	28.84	32.37	-	32.39
	e^1	38.94	34.45	-	35.32	34.45	38.94	-	38.53
	Net income	109.33	104.19	-	109.14	104.19	109.33	-	107.13
	Couple	.195	.190	-	.198	.190	.195	-	.222
	Age (head)	77.35	77.74	-	77.56	77.74	77.35	-	77.70
	Female	.664	.684	-	.629	.684	.664	-	.682
	Educ.above 14	.417	.341	-	.417	.341	.417	-	.344
	Years worked	31.25	31.90	-	31.21	31.90	31.25	-	32.36
	Anyone disabled	.127	.157	-	.144	0.157	.127	-	.168

Source: Author's calculations based on the Family Resources Survey

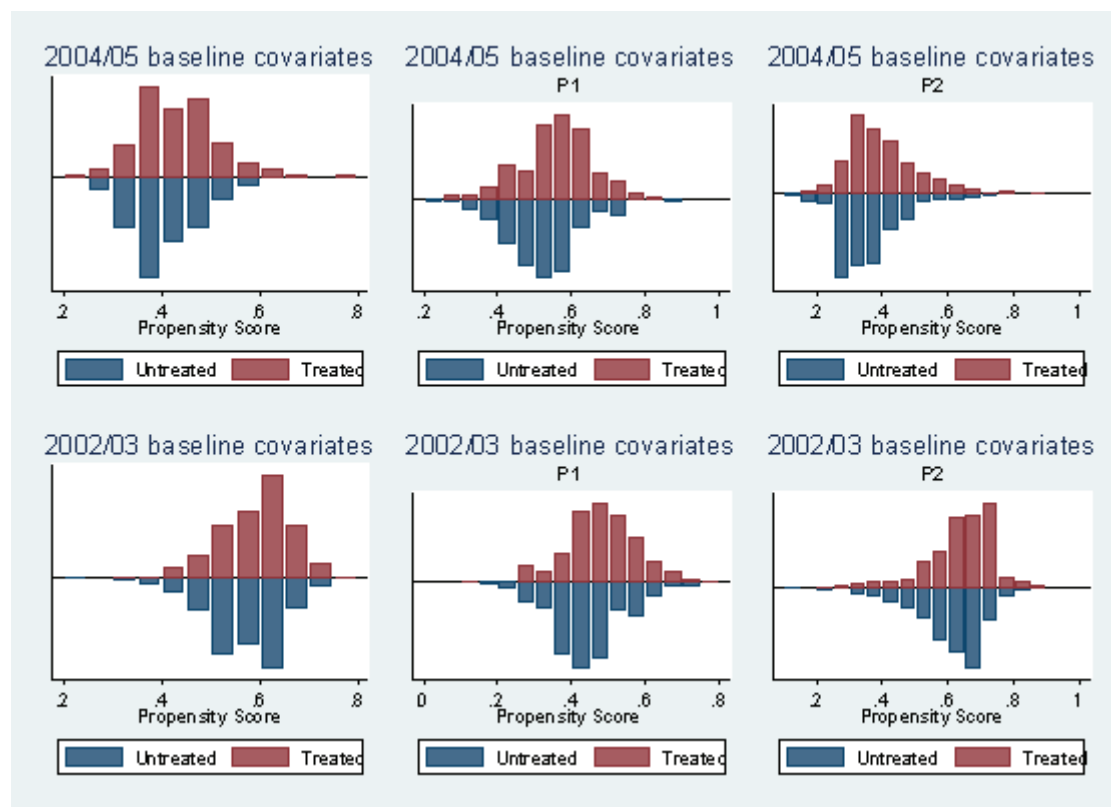
FIGURE A1: Predicted probability of take-up



Notes

Probability of take-up estimated on each sample, for a representative single man aged 75 years old, homeowner, educated above 14, white, who worked 33 year and has a net income of £100 per week (continuous line) and for a representative single woman, aged 89 years old, disabled, educated below 14, white, who worked 20 years and has a net income of £45 per week (dashed line).

FIGURE A2: Estimated propensity score distributions



Notes

In the top panel graphs 'treated' refers to pensioners sampled in 2004/05 FRS and 'controls' to pensioners sampled in 2002/03 FRS; in the bottom panel graphs 'treated' refers to pensioners sampled in 2002/03 FRS and 'controls' to pensioners sampled in 2004/05 FRS.