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Experiments with Methods to Reduce Attrition in Longitudinal Surveys

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Non-technical summary

This paper provides experimental tests of alternative methods to encourage response in surveys that interview the same persons or households repeatedly over time. Using the British Household Panel Survey for 2007 we carried out an experiment aimed at 1) testing different methods for getting people to respond although they move house (between-wave contact experiment) and 2) testing the effectiveness of tailoring the content of between-wave respondent report mailings in order to stimulate interest, loyalty and cooperation (tailored material experiment).

In particular, in the *between-wave contact experiment* three alternative strategies were compared:

1. asking all sample members to confirm their address details and providing a freepost Address Confirmation card
2. asking only those whose details have changed to inform us of their new address and providing a freepost Change of Address card
3. asking only those whose details have changed to inform us of the details and */not/* providing a reply card

Moreover, within each group, alternative strategies for increasing response were tested. Within group 1 (address confirmation card, AC) we created four different incentive schemes differing both in the definition of the conditions for getting gift vouchers (unconditional versus conditional upon the return of the card) and in the amount of the vouchers (£5 versus £2). Within group 2 (change of address COA) we compared two levels of incentives (£5 versus £2), while no incentives were offered for group 3.

We found that the COA card is the most effective strategy in a) collecting information on change of address b) reducing the number of cases left untraced at the following wave c) lowering the number of attempts interviewers have to make to contact respondents. Finally, we found that the amount of the incentive plays a limited role in reducing the

number of those who do not respond, thus suggesting that it is probably more important to focus on the incentive strategy used rather than on the associated amount.

In the *tailored material experiment*, we tested the opportunity of tailoring the respondent reports to specific categories that are less likely to respond. In particular, we focused on young and busy people and we sent them a new improved report, explicitly designed both in content and appearance to appeal to our chosen categories of respondents. Our results show that tailoring the respondent reports could be a successful strategy for increasing the interest in the survey of people who are generally less likely to respond. In particular, tailored reports seem to be effective in a) increasing the number of face-to-face interviews among young people; b) increasing the response rate for busy people when telephone interviews are added. However, the effect on the total number of those who respond is constrained by the limited size of the group under analysis.

Experiments with Methods to Reduce Attrition in Longitudinal Surveys

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Abstract

We conduct two large-scale randomised experiments on the British Household Panel Survey (BHPS) to study methods of reducing panel attrition. The first experiment compares different strategies for tracing and maintaining contact with sample members. We find that change-of-address cards conditional on moving are most effective both in increasing response rates and in reducing fieldwork costs. The second experiment substitutes the standard between-wave Respondent Report with reports tailored for specific categories of respondents. We find that tailored reports have a positive though small effect on the number of young people completing a face-to-face interview and increase cooperation for busy people when telephone interviews are included.

Keywords: attrition, non-response, tailoring, respondent incentives, longitudinal surveys, experiments
JEL codes: C81, C83, C90

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1. Introduction and background

Panel surveys are becoming more and more popular as a way of studying society and its dynamic changes. However, the validity of estimates derived from panel surveys crucially depends on the extent to which the sample selected at wave one remains representative over time. In other words, how successful the study is in maintaining people within the sample and minimising potential bias arising from attrition. Although much has been done to study non response rates, attrition and the characteristics associated with them (see, for example, Groves (2006), Watson and Wooden (2009), Fitzgerald et al. (1998), and Uhrig (2008)), the availability of experimental evidence on methods to maximise response rates and minimise attrition is quite scarce. This paper aims to help fill this gap by providing evidence from two large scale experiments assessing different methods to reduce attrition in longitudinal surveys.

There are several reasons why it is important to study efficient ways of reducing panel attrition. First is the need to maintain the sample size for analysis, including relatively small subgroups of the population. Second, panel attrition can be a source of non response bias, given that those dropping out of the panel may have different characteristics from the stayers, potentially leading to biased parameter estimates and misleading estimates of change measures. Third, understanding the forces driving non response in a panel survey has a crucial role in terms of cost reduction. The more quickly and easily survey respondents can be contacted and interviewed, the lower the fieldwork effort and costs. In particular, panel surveys have additional costs in tracing people who move address, with any strategies which minimise the effort and cost of tracing making the survey process more efficient. Finally, panel surveys contain detailed longitudinal information on sample members which can be used to set up tailored strategies to reduce non response and to tackle non response bias by focusing on specific groups with low response propensities (Couper and Ofstedal, 2009; Laurie and Lynn, 2009; Groves and Couper, 1998; Groves et al. 2000). While there is much discussion of tailoring approaches to respondents in the literature, there is little experimental evidence on this in a longitudinal context.

Lepkowski and Couper (2002) point out that the process of sample attrition is not the result of a single mechanism, but rather of a 3-phase process, each phase of which could potentially be studied on its own. The phases consist of location, contact and cooperation.

These relate closely to the two main causes of panel attrition identified by Laurie et al (1999). The first cause is due to the geographical mobility of sample members and arises when movers, in spite of all the efforts made by interviewers in the field and the survey organisation, can not be traced and are lost to the sample (for an analysis of attrition among those with unstable migration histories see Fitzgerald et al (1998) and Zabel (1998)). This corresponds to the location and contact phases of the process. The second cause is "panel fatigue", which refers to the fact that, after some years or waves of cooperation, respondents can feel bored, over-burdened, less interested in the study if they do not see its salience to their own circumstances, or feel they have contributed sufficiently over several years and hence become less willing to respond. Panel fatigue affects the cooperation phase.

This paper reports the results from an experimental study comparing different strategies for reducing attrition due to (a) geographical mobility and (b) panel fatigue through a loss of interest or perceived salience of the study. In doing so, we cover the whole process of panel attrition as described by Lepkowski and Couper, 2002. In particular, we shed some light on the efficacy and efficiency of alternative tracing methods specifically targeted to movers, in other words, our prime concern is whether the between-wave intervention is associated with higher levels of response, reduced numbers of finally untraced cases and a reduction in fieldwork effort and costs as measured by the number of calls needed to locate a sample member. In addition, we test the effectiveness of a method to enhance interest in the survey among targeted groups of respondents.

As a secondary finding, we also add new evidence to the literature on the role of incentives in the determination of response to between-wave contact exercises (for recent reviews of the effects of incentives on survey response rates see Laurie and Lynn, 2009 and Petrolia and Bhattacharjee 2009).Finally, we give some suggestions on how to reach sample members who are less likely to respond through offering an alternative mode of responding, in this case telephone interviews.

2. Previous literature and our contribution

An influential contribution pointing out the importance of effective tracing methods targeted at mobile sample members is Couper and Ofstedal (2009) which suggests a model for understanding the first phase of the Lepkowski and Couper (2001) model of panel attrition. Couper and Ofstedal argue that the probability of locating and tracing mobile sample members is the result of the likelihood of moving (not manipulable by data collectors) and the likelihood of location given a move, which, instead, can be manipulated. Posed in this way, the model contains an implicit suggestion for those interested in survey methods to study the impact of alternative manipulable factors on the probability of locating movers, and consequently on the non-response rate.

Some previous work has proposed and discussed tracing methods (Cohen et al, 1996; Freedman el al, 1980; Ribisl et al., 1996; Scott, 2004; Couper and Ofstedal, 2009), but there is little experimental evidence available to assess the effectiveness of alternative between-wave contact strategies. The sole exception appears to be McGonagle et al (2009), a study using the Panel Study of Income Dynamics (PSID) that was carried out at the same time as our experiment on the UK British Household Panel Survey (BHPS) sample. Methods for keeping track of sample members between waves are particularly important in the case of the PSID since, starting from 1997, the survey has shifted to interviews once every two years rather than annually, with the longer gap between contacts increasing the likelihood of non response due to non contact (Couper and Ofstedal, 2009; Duncan and Kalton, 1987)

McGonagle et al (2009) divided the 8829 families eligible for the 2009 interview into 24 treatment groups constructed on the basis of four conditions regarding: incentives for updating the address (unconditional versus conditional $10 incentive), mailing design (traditional black and white design versus contemporary colour design), receiving or not receiving a study newsletter and timing of the mailing (June, October, or both). The authors find that both incentives and the study newsletter have little effect in inducing response to the mailing, the traditional mail design seems to be more effective than the updated one, and that people receiving two mailings are more likely to respond to the mailing request than those receiving just one mailing.

The preliminary conclusion of McGonagle et al is that monetary incentives are of little help in the process of locating mobile sample members, while alternative forms of effort for contacting the respondents (namely multiple mailings) seem to be a better option. However, the PSID started in 1968, so sample members are likely to be a selected sub sample of the original sample and are likely to be characterised by a higher-than-average willingness to cooperate with the survey, a loyalty which could weaken the effect of incentives on response behaviour. This concern is confirmed by the finding that when in October 2008 the US economy experienced a downturn, the incentives did seem to have an effect, showing that when the sample becomes more sensitive to monetary transfers, we should be more careful in concluding that incentives are not a worthwhile strategy. Moreover, it is impossible to say whether the success of the traditional mailing design is due to the design itself or whether it is due to respondents reacting to a familiar format they have known and identified with for a long time.

Our study makes a number of unique contributions. Notably, we compare experimentally three fundamentally different ways of asking sample members to supply address updates. Additionally, we test the role of conditional versus unconditional incentives and the effect of different amounts of monetary incentives in encouraging people to confirm or update their address details. Collectively, these experiments provide evidence on the strategy which has always been used on the BHPS to encourage respondents to tell us their new address details when they move, in comparison with a number of alternative strategies. Section 4 below describes the experiments in detail.

Another contribution this paper makes to the literature on attrition is on the use of longitudinal information to set up strategies explicitly tailored for respondents with a lower propensity to respond. The role of tailored designs in enhancing response and stimulating cooperation has been extensively discussed in the case of single-purpose surveys targeted to specific groups of people (e.g. surveys of visitors of national parks or surveys to university faculties, Dillman et al, 2009), but is has not been applied to different demographic or other sub-groups within a general household survey nor in the context of a panel design.

When testing the impact of sending a newsletter to sample members to keep them interested in the survey, McGonagle et al (2009) did not exploit the longitudinal information contained in the panel or try to tailor the newsletter to the characteristics of

specific groups of respondents. Indeed, since their introduction in the context of interviewers' doorstep behaviour (Groves at al 1992, Groves and Couper 1998) tailoring methods have not spread to many other aspects of the survey process affecting response. The idea of tailoring the content and design of mailing materials remains quite innovative and no experimental evidence is available on its effect on response propensity.

This paper offers some evidence on this. We test the effect on response rates of sending a tailored versus standard format Respondent Report to our experimental units using individual respondent characteristics to define the sub-groups receiving the tailored reports. This strategy makes use of the information we have from previous waves to add new evidence to the literature on topic salience and response rate/bias (see Groves et al, 2000; 2006). If tailored reports of this type are effective in keeping specific groups with a higher non-response propensity in the study, they have the potential not only to reduce non-response but also to limit the scope for non response bias.

It should also be noted that our experiment provides evidence in a slightly different context to that of McGonagle et al (2009). The BHPS is younger than PSID, having started in 1991, so respondents are less "seasoned", on average. Also, as noted above, the PSID has biennial waves (since 1997), while the BHPS is annual. This may increase the importance of the double mailing on the PSID as a tool to keep the respondents interested in the survey and in touch during the gap year and will of course also have implications for the proportion of sample members who change address between survey waves.

3. The BHPS

The BHPS is a national panel survey of Great Britain which started in 1991 with an original sample of 5,500 households and 10,300 individuals. Additional samples of 1,500 households in each of Wales and Scotland were added in 1999 and 2,000 households in Northern Ireland were added in 2001. The sample includes every member of the selected households regardless of age, but sample members are only asked for a full individual interview from age 16 upwards with a self-completion youth interview for children aged 11 to 15. As children reach the age of 16 they become eligible for a full individual interview. Interviews are carried out annually with all eligible members of the household. Sample members who move are followed to their new address and the members of their new household become eligible for an interview. The interview mode is face-to-face (CAPI) but telephone interviews are carried out within the refusal conversion process as a last resort to avoid complete unit non response (see Burton and al. 2006). The telephone interview is shorter than the face to face interview, containing the core longitudinal items from the main individual questionnaire.

The BHPS uses various strategies to reduce non response, including (see also Laurie et al, 1999):

- At each interview, providing respondents with a freepost change of address card pre-printed with their address details as we currently hold them.
- Sending a £5 gift voucher incentive to any person returning a change of address card with their new address details.
- An annual between fieldwork mailing of a short respondent report of research findings with a confirmation of address card and a reminder that if they inform us of a new address, they will receive a £5 gift voucher.
- Unconditional gift voucher incentive (£10 per person) sent with the advance letter prior to the interview (since wave 6).

4. Experimental design

We carried out two experiments simultaneously on the sample of all wave 17 BHPS respondents, using a randomised interpenetrated design. The *between-wave contact experiment* is inspired by Couper and Ofstedal (2009). The aim is to understand and evaluate alternative strategies for reducing attrition due to a failure to locate sample members. The experiment involves seven treatment groups, described in section 4.1 below. The *tailored materials experiment* aimed to test the tailoring of content for the between-wave respondent report mailing in order to stimulate interest, loyalty and co-operation. It consisted of two treatment groups, as described in section 4.2. Thus, the full interpenetrated design involved fourteen treatment groups. Our experiments aimed to test the validity of the existent BHPS procedures against a set of alternative ones which could be implemented instead of those currently in place.

The experimental units mailed were either a single individual or, where individuals are in a couple, the couple itself. A single mailing was sent to couples, while respondents who were not in couples received an individual mailing. This was done in order to avoid large treatment units, while containing the spill-overs between individuals sample members within the same family. Separately allocating individuals and couples to treatments, rather than whole households, also maximises the scope for tailoring in the *tailored materials experiment* and, arguably, increases the chances of obtaining new address information in cases where a subset of the household has changed address. For the incentive treatments, units were also clustered within households so that all members of the same household received the same incentive treatment.

The experiments were carried out in the period between waves 17 and 18 of the BHPS. The mailing was sent out in June 2008 with replies received up until the start of the wave 18 fieldwork which took place between September 2008 and February 2009. The sample was all BHPS respondents from wave 17 (2007), about 12,500 persons in total. Survey response rates following the experiments are computed on the BHPS data for wave 18.

4.1. Reducing attrition due to geographical mobility: the between-wave contact experiment

The between-wave contact experiment has a complex design which allows us to test multiple hypotheses. The overall aim is to identify strategies which provide the most effective means of updating address information, as this information should help to minimise the proportion of the sample that can not be located at the following wave (wave 18) and to minimise the cost and effort required to trace sample members at that wave. We therefore compare strategies in terms of the proportion of updated addresses obtained, the costs and effort of locating sample members, and the proportion ultimately not located at the subsequent wave.

Three alternative strategies were compared:

1. asking all sample members to confirm their address details and providing a freepost Address Confirmation card
2. asking only those whose details have changed to inform us of their new address and providing a freepost Change of Address card
3. asking only those whose details have changed to inform us of the details and */not/* providing a reply card.

Within group 1 (address confirmation card, AC), four alternative incentive treatments were compared, consisting of a cross-classification of levels (£5 versus £2) and conditionality (unconditional versus conditional upon the return of the card). Within group 2 (change of address card, COA), two alternative levels of incentive were compared, but both were offered only conditional upon return of the card (as in this case the incentive was aimed only at those who had moved, and we did not know in advance which cases these were). No incentives were offered within group 3.

The sample was assigned randomly to treatments, with one quarter of the sample assigned to group 3 (no card) and one eighth to each of the other six treatments. Sample units were systematically assigned to treatments, after stratifying the sample by region at wave 17 and then by interviewer area. Two people belonging to the same unit receive the same treatment by construction. Table 1 summarises the treatments and sample sizes for each treatment group.

Table 1. Treatment groups for the between-wave contact experiment

Group number	Type of card	Type of incentive	amount	Sample size of the group (in mailing units)
1	Address confirmation card (AC)	Unconditional	£5	1124
2	Address confirmation card (AC)	Unconditional	£2	1111
3	Address confirmation card (AC)	Conditional on return	£5	1125
4	Address confirmation card (AC)	Conditional on return	£2	1104
5	Change-of-address card (COA)	Conditional on return	£5	1104
6	Change-of-address card (COA)	Conditional on return	£2	1096
7	No card	No incentive	none	2213

4.2. Reducing attrition due to refusals: the tailored materials experiment

The tailored materials experiment focuses on mechanisms to improve interest, loyalty and cooperation among sample members and it relies on differences in the design and format of short respondent reports mailed to each survey participant. In particular, the experiment tries to assess whether the use of reports which contain information directly relevant to the respondent's circumstances can increase the level of cooperation for groups characterized by lower response rates, namely young people aged under 25 and busy people.

It has been frequently observed in panel surveys that younger people exhibit lower response rates and higher attrition rates (Behr et al., 2005; Lillard and Panis, 1998; Stoop, 2005; Uhrig, 2008; Watson and Wooden, 2009). Also, young people have been in the BHPS responding sample for a relatively short period of time. Young people aged under 25 have been eligible for a full individual interview for between 1 and 9 waves, while most other sample members have been eligible for longer, many for 18 waves. They may

therefore have a lesser sense of loyalty or commitment to the survey. Busy people have similarly been observed to exhibit lower response propensities (Abraham et al, 2006; Groves and Couper, 1998; Lynn and Clarke, 2002; Watson and Wooden, 2009). As well as exhibiting lower response rates, both groups can be expected to be distinctive in terms of important survey measures (and therefore bring the potential to introduce non-response bias) and share common characteristics that should make tailoring of the respondent report possible.

In addition to the two tailored reports, a standard report was used, as described in the next paragraph. This was in the standard BHPS format and contained the general content sample members are used to receiving. The tailored reports differed from the standard one in both their content and in their appearance. Compared to the standard reports, the tailored reports were smaller, more colourful, easier to read and generally much more direct as written text was kept to a minimum and partially substituted by pictures. The content of the reports was tailored in a number of aspects. Not only were the findings and topics from the previous wave selected to be of particular salience for each subgroup, but the figures, colours, photographs, layout and even the font were carefully chosen to appeal to the treatment group. All three versions of the reports are shown in Appendix A. The tailored reports were designed to be more direct and appealing than the standard report, but there are no reasons why a report for one group should be considered more effective than the report for the other group in capturing the attention of the target population. Following the terminology introduced by Groves and al (2000), we could say that the two tailored reports are equally salient, but they can have different leverage when different sub samples of respondents are considered.

We randomly allocated sample units to one of two treatments. In one treatment (control group), all received the standard report. In the other, young or busy people received the relevant tailored report while others received the standard report. The random allocation was carried out within each of the seven treatment groups described in section 4.1, thus resulting in fourteen treatment groups overall. As with the between-wave contact experiment, one report only was sent to both members of a couple. If either member of the couple belonged to one of the target groups and the couple was allocated to the tailored treatment, they received the tailored report for that group.

Within the tailored treatment, reports were assigned hierarchically, meaning that busy people in treatment group 2 received the busy report if they were not “young”, otherwise they received the young report. Those in the treatment group who are neither young nor busy received the standard report. “Young” was defined as aged 25 or younger at the scheduled time of the start of wave 18 fieldwork. “Busy” was defined as working more than 40 hours per week, or commuting for more than 10 hours per week in addition to working full time, or being self-employed.

5. Results and discussion

5.1. Returning the address cards

We first focus on whether different experimental treatments are associated with different propensities to return the Address Confirmation (AC) or Change of Address (COA) cards.

Table 2a. Percentage of cards returned, by treatment group

		Freq.	tot	%
AC, unconditional	£5 incentive	454	1124	40.4
	£2 incentive	440	1,111	39.6
	pooled	**894**	**2,235**	**40.0**
AC, conditional on return	£5 incentive	394	1,125	35.0
	£2 incentive	351	1,104	31.8
	pooled	**745**	**2,229**	**33.4**
COA, conditional on return	£5 incentive	144	1,104	13.0
	£2 incentive	167	1,096	15.2
	pooled	**311**	**2,200**	**14.1**
no card or incentive	**pooled**	**149**	**2,213**	**6.7**

Table 2b. Returning behaviour: significance of pair wise mean comparison tests between pairs of treatments

		AC, unconditional			AC, conditional on returning			COA, conditional on return			no incentive
	incentive	£5	£2	**pooled**	£5	£2	**pooled**	£5	£2	**pooled**	**pooled**
AC, unconditional	£5		ns		**	***		***	***		***
	£2	ns			**	***		***	***		***
	pooled						***			***	***
AC, conditional on return	£5	**	**			**		***	***		***
	£2	***	***		**			***	***		***
	pooled			***						***	***
COA, conditional on return	£5	***	***		***	***			*		***
	£2	***	***		***	***		*			***
	pooled			***			***				***
no incentive	**pooled**	***	***	***	***	***	***	***	***	***	

Table 2a shows the share of people returning the card for each treatment group and for pooled groups where the two levels of incentive are combined. We see that the way in which the incentive is delivered is associated with whether the card is returned. In particular, the unconditional incentive seems to perform better than the conditional one, regardless of the amount of the incentive and a higher proportion of AC than COA cards are returned. Table 2b shows that all differences between unconditional and conditional incentives and most of the differences between treatment groups are significant at the 1% level.

There seem to be two channels through which incentives work (Singer, 2002): the first one is loyalty, when the respondent who gets the unconditional incentive feels an obligation to cooperate with the request irrespective of any additional benefit the cooperative behaviour might provide. The second channel can be an economic exchange argument according to which the respondent considers the pros and cons of responding to the request and puts into practice the action associated with the biggest difference between the former and the latter. The net effect is ex ante unknown, given that if loyalty prevails, unconditional incentives look more appealing, while conditional incentives are more suitable when a maximising approach is expected. The data for the card returns seem to point towards a prevalence of loyalty over maximising behaviour, something which is consistent with the early PSID findings and with other research on incentives (Church 1993; Singer et al, 1999).

The experimental design makes it possible to identify separately the effect of the type of incentive i.e. unconditional versus conditional and the effect of the amount of the incentive offered (unlike some studies which confound these two elements, e.g. Petrolia and Bhattacharjee (2009)). In the case of AC cards there is a positive effect of the amount of the incentive on the probability of return, which, however is significant only in the case of conditional incentives (at 5% level of significance). In the case of the COA cards there was a higher share of returns among those receiving the lower monetary incentive, the opposite of what might be expected. However, for this group, the probability of returning the COA card is the joint probability of moving and returning the card given a move. Even if monetary incentives do affect the latter, they do not play any role in determining the former. Therefore, the result we find could be due to a random difference within subgroups in the share of the movers, which could have hidden the real effect of the incentive amount.

5.2 Information on addresses

The analysis of the return behaviour alone does not provide much information on the effectiveness of the incentives in avoiding panel attrition due to unknown addresses following a move. The data allow us to examine whether different treatments are associated with different probabilities of receiving information on the address where sample members currently live.

Table 3a. Type of address information obtained, by treatment group

		address confirmed %	address changed /updated %	not known at address %	other reasons %	no information %	Total
AC, unconditional	£ 5 incentive	36.57	3.47	0.89	0.89	58.19	1124
	£ 2 incentive	36.18	2.7	1.44	1.35	58.33	1,111
	pooled	**36.38**	**3.09**	**1.16**	**1.12**	**58.26**	**2,235**
AC, conditional on return	£ 5 incentive	32.09	2.04	0.53	1.16	64.18	1,125
	£ 2 incentive	29.53	1.81	0.45	0.36	67.84	1,104
	pooled	**30.82**	**1.93**	**0.49**	**0.76**	**65.99**	**2,229**
COA, conditional on return	£ 5 incentive	10.42	2.45	1.36	0.45	85.33	1,104
	£ 2 incentive	12.59	2.1	0.27	0.64	84.4	1,096
	pooled	**11.5**	**2.27**	**0.82**	**0.55**	**84.86**	**2,200**
no incentive	**pooled**	**4.74**	**1.67**	**0.63**	**0.59**	**92.36**	**2,213**

Table 3b. Address changed: significance of pair wise mean comparison tests between treatment groups

	AC, unconditional	AC, conditional on return	COA, conditional on return	no incentive
AC, unconditional		***	**	***
AC, conditional on return	***		ns	ns
COA, conditional on return	**	ns		*
no incentive	***	ns	*	

Table 3a shows that both the AC unconditional and conditional groups perform better than the COA groups in increasing the share of those providing information on their address. However, the share of cards signalling a change of address from the COA groups (2.27%) is almost as high as the share obtained for the AC unconditional incentive groups (3.09%), providing some evidence in favour of the cheaper COA return conditional on move incentive scheme. The groups that performed least well in terms of gaining new address details were the AC conditional on return groups (1.93%) and the no incentive group (1.67%). Tables 3a and 3b show that unconditional incentives are significantly better than other schemes in getting information about address updates or changes, while COA cards with conditional incentives perform better than AC cards with conditional incentives and seem to lead to a significantly better outcome when compared to no incentives at all. When we focus on the behaviour of the people belonging to different treatment groups within the same type-of-incentive group, we notice that higher incentives are associated with more cooperative behaviour, but none of the within group differences were statistically significant (test results not shown). If we look at the nature of the information collected for those returning the card, we see that the share of cards reporting a change of address is fairly high in the case of the COA group. This suggests that the COA card with conditional incentive is quite effective in collecting information on those individuals who may be more likely to be lost to the panel at the following wave through a failure to trace them to a new address, even though the strategy induced a lower overall number of returns compared to the AC unconditional incentives. It is interesting to note that, conditional on returning a card, the highest propensity to report a change of address is found in the no incentive group. Fully 22% of the cards returned in this group provided a new address, compared to 15% of returned COA cards, 7% of returned AC cards with an unconditional incentive and 6% of returned AC cards with a conditional incentive. This suggests that without a prepaid card and an incentive, sample members are less inclined to report unchanged details.

5.3 Effects on tracing at the following wave

On the BHPS, tracing during fieldwork takes place in two ways. The interviewer first attempts to trace anyone they find has moved when they call at the issued address for a household. If the interviewer finds a new address they simply carry on and interview that person as usual unless the new address is out of their area in which case it is re-issued through the field office to another interviewer. If the initial interviewer fails to obtain a new address, the case is returned to the office where additional tracing attempts are made using other information held about the respondent. This includes for example, details of all current and past stable contact names, various alternative telephone numbers and contacting people the untraced respondent has lived with in prior waves. Tables 4a and 4b show the share of people who went into office tracing by treatment group, the share of those who were successfully traced and the share of those who were left untraced by the end of fieldwork.

Table 4a: Effectiveness of the tracing process, by treatment group

		went into tracing			traced		
		freq	tot	percent	freq	percent	percent of those in tracing
unconditional	£ 5 incentive	19	1,582	1.20	7	0.44	36.84
	£ 2 incentive	18	1,566	1.15	3	0.19	16.67
	pooled	**37**	**3,148**	**1.18**	**10**	**0.32**	**27.03**
conditional on return	£ 5 incentive	13	1,587	0.82	0	0.00	0.00
	£ 2 incentive	19	1,567	1.21	4	0.26	21.05
	pooled	**32**	**3,154**	**1.01**	**4**	**0.13**	**12.50**
conditional on moving	£ 5 incentive	18	1,573	1.14	6	0.38	33.33
	£ 2 incentive	22	1,550	1.42	1	0.06	4.55
	pooled	**40**	**3,123**	**1.28**	**7**	**0.22**	**17.50**
no incentive	**pooled**	**29**	**3,097**	**0.94**	**0**	**0.00**	**0.00**

Table 4b. traced respondents: significance of pair wise mean comparison tests between treatment groups

		traced									
		unconditional			conditional on returning			conditional on moving			
	incentive	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	**pooled**
unconditional	£ 5		ns		***	ns		ns	**		***
	£ 2	ns			**	ns		ns	ns		***
	pooled						*			ns	***
conditional on return	£ 5	***	**			**		***	ns		.
	£ 2	ns	ns		**			ns	*		***
	pooled			*						ns	**
conditional on moving and return	£ 5	ns	ns		***	ns			**		***
	£ 2	**	ns		ns	*		**			*
	pooled			ns			ns				***
no incentive	**pooled**	***	***	***	.	***	**	***	*	***	

		traced (conditional on being in tracing)									
		unconditional			conditional on returning			conditional on moving			
	incentive	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	**pooled**
unconditional	£ 5		*		***	ns		ns	***		***
	£ 2	*			*	ns		ns	ns		**
	pooled						*			ns	***
conditional on return	£ 5	***	*			**		***	ns		.
	£ 2	ns	ns		**			ns	*		***
	pooled			*						ns	**
conditional on moving and return	£ 5	ns	ns		***	ns			***		***
	£ 2	***	ns		ns	*		***			***
	pooled			ns			ns				***
no incentive	**pooled**	***	**	***	.	***	**	***	***	***	

On average it seems there is little difference across treatment groups in the share of those who required tracing. The only significant difference (at 10%) is between the COA group and the no incentive group. However, alternative incentive schemes do seem to matter when the actual outcomes of the tracing process are compared. In particular, the AC unconditional incentives seem to be the most efficient scheme both in successfully tracing respondents and in reducing the number of the untraced and this happens both when the shares are computed on the full sample and when they are derived by considering just those who went into tracing.

COA cards with a conditional incentive also seem to perform fairly well, in fact after performing mean comparison tests on the number of traced respondents, we fail to reject the hypothesis of equal mean for the AC unconditional group and the COA group. Finally, in the case of the no incentive group, none of the office attempts to trace the

respondents was successful. This evidence supports the use of monetary incentives in between-wave contact mailing exercises to reduce attrition due to geographical moves.

Our mean comparison tests provide in general significant results for the within-group comparisons. However, the results do not show any clear pattern.

5.4 Effects on final attrition due to moving

Although it is important to understand which phase of the location process is mainly affected by the different between-wave treatments, our main outcome of interest is the relation between the between-wave treatment and the share of those who could not be traced due to moving at the following wave.

Tables 5a summarises response outcomes at the household level for each treatment group. In particular, the proportion of untraced movers can be seen to be lowest with the COA incentive conditional on return and highest with the conditional AC incentive. Differences between the pooled groups in the proportion remaining untraced are generally statistically significant although the differences by the amount of the incentive are generally not significant, except for the conditional AC group, where the sign is unexpected (see also table 5.b)

Table 5a. Outcome at family level

		all interviewed %	some interviewed %	telephone interview %	untraced movers %	resident-non contact %	Refusals %	total
AC, unconditional	£5 incentive	77.84	13.34	5.93	0.19	0.39	2.32	1,552
	£2 incentive	76.52	14.68	5.94	0.2	0.52	2.15	1,533
	pooled	**77.18**	**14**	**5.93**	**0.19**	**0.45**	**2.24**	3,085
AC, conditional on return	£5 incentive	77.99	12.92	5.18	0.58	0.38	2.94	1,563
	£2 incentive	78.8	11.83	5.92	0.2	0.13	3.12	1,538
	pooled	**78.39**	**12.38**	**5.55**	**0.39**	**0.26**	**3.03**	3,101
COA, conditional on return	£5 incentive	78.44	13.51	6.31	0.06	0.39	1.29	1,554
	£2 incentive	75.98	14.84	6.39	0.07	0.6	2.13	1,503
	pooled	77.23	**14.16**	**6.35**	**0.07**	**0.49**	**1.7**	3,057
no incentive	**pooled**	**76.83**	**13.95**	**6.25**	**0.2**	**0.79**	**1.97**	3,039

Table 5b. Untraced movers: significance of pair wise mean comparison tests between pairs of treatments (any returning behaviour)

		AC, unconditional			AC, conditional on returning			COA, conditional on return			no incentive
	incentive	£5	£2	**pooled**	£5	£2	**pooled**	£5	£2	**pooled**	**pooled**
AC, unconditional	£5		ns		**	ns		ns	ns		ns
	£2	ns			**	ns		ns	ns		ns
	pooled						*			*	ns
AC, conditional on return	£5	**	**			**		***	***		**
	£2	ns	ns		**			ns	ns		ns
	pooled			*						***	*
COA, conditional on return	£5	ns	ns		***	ns			ns		ns
	£5	ns	ns		***	ns		ns			ns
	pooled			*			***				*
no incentive	**pooled**	ns	ns	ns	**	ns	*	ns	ns	*	

5.5 Effects on the number of calls and costs

Though the proportion of sample respondents who remain untraced at the end of the data collection is a key outcome, we are also interested in the costs of achieving a particular outcome.

The analysis of the return behaviour presented in section 5.1 above helps in estimating the expected monetary cost for each treated person in each experimental group (see Table 6).

Table 6. Expected monetary costs of each treatment

	amount of the incentive (£)	share of those getting the money, given the incentive scheme and the return behaviour	expected monetary cost per person (£)
AC, unconditional	5	1	5.00
	2	1	2.00
AC, conditional on return	5	0.35	1.75
	2	0.32	0.64
COA, conditional on return	5	0.13	0.65
	2	0.15	0.30
no incentives	0	0	0.00

Unconditional incentives are the most expensive, as each treated unit or respondent gets the money regardless of their resulting return behaviour. The £5 AC conditional incentives follow with a per capita expected cost of £1.75. The £2 AC conditional incentives at a per capita cost of £0.63 are similar in cost to the £5 COA incentives, at £0.65. Finally, the £2 COA incentives are the cheapest available option at a per capita cost of £0.30.

Obviously, direct monetary costs of the incentives are not the only relevant costs. The amount of fieldwork effort spent in trying to contact and trace sample members has a direct effect on survey costs. On the BHPS, the numbers of calls made by the interviewer when trying to contact respondents are recorded on the data. During office tracing, this work is done primarily by telephone and here we use an indicator of cost that is the number of phone calls made during the tracing process. The total number of calls made, in person by interviewers and by telephone during tracing are indicators of these elements of survey cost. The numbers of calls made by the interviewer in field are available for the

entire issued sample. The numbers of telephone calls made during tracing are available just for those who went into office tracing.

Experimental evidence from other surveys shows that monetary incentives used at the point of contacting sample members can reduce the number of calls interviewers have to make before making contact. (James, 1997; Lynn et al, 1998; Rodgers, 2002). The BHPS between-wave contact strategies used in these experiments differ in that they were specifically targeted at mobile sample members with the aim of improving the efficiency with which sample members could be located, rather than the efficiency of making contact conditional on successful location.

We decomposed the total number of interviewer visits into two parts: the "number of calls at issued address" and the "number of calls at a new address". The experimental treatments are likely to reduce the number of the latter as the survey organisation will, in most cases, be issuing the case to the current address (so there will be no additional "new address" identified during fieldwork). On the other hand, they could increase the number of visits by making it possible to contact more mobile sample members, who may require greater effort to contact.

Table 7a shows the average number of interviewer visits for each treatment group. We can see that COA card treatments are the most effective in reducing the number of calls to the issued address. Differences between the COA group and the other groups are all significant either at 5% or at 10% level of significance (see table 7b). This suggests that such a strategy is successful in avoiding calls to old addresses which no longer belong to sample members. Moreover, within-group mean comparison tests show that the amount of the incentive has a positive and significant impact in reducing the number of calls to the issued address only in the case of the AC unconditional incentive groups, but is ineffective in the other treatment groups.

Table 7a. Number of field visits to achieve contact, by treatment group

		(1) calls at issued address		(2) calls at new address		(3) total number of calls	
		group size	average number of calls	group size	average number of calls	group size	average number of calls
unconditional	£5 incentive	1581	1.73	90	1.31	1581	1.81
	£2 incentive	1560	1.90	93	2.12	1560	2.03
	pooled	**3141**	**1.82**	**183**	**1.72**	**3141**	**1.92**
conditional on return	£5 incentive	1586	1.86	55	1.09	1586	1.89
	£2 incentive	1566	1.79	81	1.38	1566	1.87
	pooled	**3152**	**1.83**	**136**	**1.26**	3152	**1.88**
conditional on moving and return	£5 incentive	1570	1.78	77	1.62	1570	1.86
	£2 incentive	1548	1.74	87	1.21	1548	1.81
	pooled	**3118**	**1.76**	**164**	**1.40**	**3118**	**1.83**
no incentives	1	1538	1.80	84	1.31	1538	1.87
	2	1540	1.84	59	1.07	1540	1.88
	pooled	**3078**	**1.82**	**143**	**1.21**	**3078**	**1.88**
total sample size		12489		626		12489	

Table 7b. Number of field visits to issued address: significance of pair-wise mean comparison tests between pairs of treatments

		unconditional			conditional on returning			conditional on moving			no incentives
	incentive	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	**pooled**
unconditional	£ 5		***		**	ns		ns	ns		**
	£ 2	***			ns	**		**	***		*
	pooled						ns			*	ns
conditional on return	£ 5	**	ns			ns		*	**		ns
	£ 2	ns	**		ns			ns	ns		ns
	pooled			ns						**	ns
conditional on moving and return	£ 5	ns	**		*	ns			ns		ns
	£ 2	ns	***		**			ns			**
	pooled			*			**				*
no incentives	**pooled**	**	*	ns	ns	ns	ns	ns	**	*	

Table 7c. Total number of calls: significance of pair-wise mean comparison tests between pairs of treatments (any returning behaviour)

		unconditional			conditional on returning			conditional on moving			no incentives
	incentive	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	£ 5	£ 2	**pooled**	**pooled**
unconditional	£ 5		***		ns	ns		ns	ns		*
	£ 2	***			**	***		***	***		***
	pooled						ns			**	ns
conditional on return	£ 5	ns	**			ns		ns	*		ns
	£ 2	ns	***		ns			ns	ns		ns
	pooled			ns						ns	ns
conditional on moving and return	£ 5	ns	***		ns	ns			ns		ns
	£ 2	ns	***		*	ns		ns			*
	pooled			**			ns				ns
no incentives	**pooled**	*	***	ns	ns	ns	ns	ns	*	ns	

The relationship between treatment group and number of calls is almost inverted when visits made by the interviewer at a new address are considered. The results reported in column (2) of table 7a show that, for any treatment group with incentives, the number of calls made to a new address is greater than the number of calls made in the case of the no-incentives group. Rather than showing a failure in the role of the incentives used for the treatment groups, this result may indicate that incentives are effective in collecting between-wave information about sample members. They make it possible to spot new addresses and, perhaps, they are also successful in keeping in the sample individuals with a lower-than-average response propensity who require a greater effort in terms of numbers of calls.

Next we consider the net effect of the incentives on costs measured as total number of calls to achieve contact. Column (3) in table 7a. shows the results when the calls in column (1) and (2) are added together. We still conclude that the COA card with incentive conditional on return is the most cost effective method, since the average number of total calls for this group is significantly lower than the one computed for any other group. Moreover, despite being the most expensive method, the unconditional incentives do not seem to be more effective in reducing fieldwork effort than less costly alternatives. Finally, again, the amount of the incentive seems to play a role only in the case of AC card with unconditional incentives.

Beyond the calls made by interviewers in the field, we also need to consider telephone attempts made during office tracing as an additional cost. Table 8a shows the average number of phone calls made during office tracing for each treatment group. In general, the three treatment groups do not seem to differ in terms of calls dialled, while the no card, no incentive group is associated with a significantly lower average number of calls both when we consider just those who went through the tracing process in the office and when we consider the full sample. Mean comparison tests show that the only significant differences are those computed for the no card, no incentive group, while pair-wise differences between couples of alternative treatments are generally not significant (see table 8b).

Table 8a. Number of tracing calls from the office, by treatment group.

		(1)	(2)	(3)	(4)	(5)
		people contacted	group size	proportion of people contacted (%)	average number of calls for the contacted	average number of calls for the full sample
unconditional	£5 incentive	17	1582	1.075	3.412	0.037
	£2 incentive	5	1566	0.319	4.800	0.015
	pooled	**22**	**3148**	**0.699**	**3.727**	**0.026**
conditional on return	£5 incentive	6	1587	0.378	3.333	0.013
	£2 incentive	12	1567	0.766	3.333	0.026
	pooled	**18**	**3154**	**0.571**	**3.333**	**0.019**
conditional on moving	£5 incentive	15	1573	0.954	2.533	0.024
	£2 incentive	12	1550	0.774	4.583	0.035
	pooled	**27**	**3123**	**0.865**	**3.444**	**0.030**
no incentives	**pooled**	**14**	**3097**	**0.452**	**2.071**	**0.009**

Table 8b. Number of tracing calls from the office: significance of pair-wise mean comparison tests between pairs of treatments (any returning behaviour)

number of calls in the office (on the treated)

		unconditional			conditional on returning			conditional on moving			no incentives
	incentive	£5	£2	**pooled**	£5	£2	**pooled**	£5	£2	**pooled**	**pooled**
unconditional	£5		ns		ns	ns		ns	ns		ns
	£2	ns			ns	ns		ns	ns		**
	pooled						ns			ns	ns
conditional on return	£5	ns	ns			ns		ns	ns		ns
	£2	ns	ns		ns			ns	ns		*
	pooled			ns						ns	*
conditional on moving and return	£5	ns	ns		ns	ns			**		ns
	£2	ns	ns		ns	ns		**			***
	pooled			ns			ns				*
	pooled	ns	**	ns	ns	*	*	ns	***	*	

number of calls in the office (full sample)

		unconditional			conditional on returning			conditional on moving			no incentives
	incentive	£5	£2	**pooled**	£5	£2	**pooled**	£5	£2	**pooled**	**pooled**
unconditional	£5		ns		*	ns		ns	ns		***
	£2	ns			ns	ns		ns	ns		ns
	pooled						ns			ns	**
conditional on return	£5	*	ns			ns		ns	ns		ns
	£2	ns	ns		ns			ns	ns		**
	pooled			ns						ns	*
conditional on moving and return	£5	ns	ns		ns	ns			ns		**
	£2	ns	ns		ns	ns		ns			***
	pooled			ns			ns				***
	pooled	***	ns	**	ns	**	*	**	***	***	

There are three channels through which our between-wave contact strategies might affect the number of tracing calls in the office. First, locating mobile sample members could have as a consequence an increase in the average number of calls for those who went into office tracing, since mobile people are more difficult to contact and this requires an extra effort. The second mechanism goes in the opposite direction, given that more up-to-date information on the address where people moved to can make the process of tracing attempts more efficient, thus reducing the number of calls needed for those who went into office tracing. The net sign of the effect of the incentive on the number of calls for those who went into tracing in the office is ambiguous ex ante, but our results seem to suggest that the first two mechanisms prevail and the net effect is an increase in the number of calls for the treated units if compared to the untreated units (see column 4 of table 8a and

table 8b). However, there is a third channel through which incentives can affect the number of calls used for tracing in the office. In fact, incentives could help by collecting updated information on mobile sample members between waves who otherwise could have remained untraced. This, in principle, could make it possible to reach also those who previously would have remained untraced. This seems to be confirmed by the proportion of contacted people which differs across treatment groups (see column 3 in table 8a), with the COA group associated with the highest share of sample members who were traced in the office.

The joint effect of the three mechanisms mentioned above determines the average number of calls per case which is what we are interested in, since it best describes the effect on total costs of tracing phone calls (see column 5 in table 8a and table 8b). The results suggest that alternative incentives schemes are equally costly while the no incentive group is associated with a significantly lower number of calls.

In conclusion our cost-effectiveness analysis suggests that when total costs are taken into account, COA cards with incentives conditional on return are not only the least expensive among those considered, but they are also shown to be superior to other options in terms of effectiveness in tracing mobile sample members.

5.6 Effects of tailored materials on response rates.

Table 9 compares the response rate at wave 18 for the full sample and for the two target groups at whom the tailored materials were aimed. We consider two alternative definitions of response rate. Column (1) reports the results when considering responders as just those completing a full face-to-face interview. Column (2) reports the results obtained when we include also those completing a telephone interview (see section 3).

Table 9: Response rate, by tailored materials treatment and group

			(1) Just face-to-face interviews			(2) Face-to-face and telephone interviews		
	% in the sample		n	%	estimated impact of the overall response rate (%)	n	%	estimated impact of the overall response rate (%)
young	14.96	tailored	843	93.20		843	94.10	
		standard	856	91.60		856	94.20	
		diff		1.60	0.24		-0.10	-0.01
		two tails ttest		*			ns	
busy	19.68	tailored	1205	90.30		1205	97.50	
		standard	1157	90.10		1157	96.50	
		diff		0.20	0.04		1.00	0.20
		two tails ttest		ns			*	
full sample	100	tailored	5942	91.35		5942	96.82	
		standard	5857	91.12		5857	96.82	
		diff		0.23	0.23		-0.01	-0.01
		two tails ttest		ns			ns	

It is noticeable that, even though we are working on a mature sample of very loyal people – where the scope for response rate improvement might be thought to be limited - tailoring the respondent report does seem to have an effect. The share of full face-to-face interviews is always higher in the case of treated units. Nevertheless, the difference is significant (at the 10% level) only in the case of young people.

On the basis of our findings in column (1), one could infer that the tailored reports have, on average, higher leverage in the case of young people, while any effort at tailoring is not effective in the case of busy people, who seem to be just slightly affected by the treatment. Such a conclusion, however, is based on the implicit assumption that busy people are flexible enough to be able to give a full face-to-face interview once they become more interested in the survey. In order to derive some information on whether the limited impact of the tailored report is due to time constraints or to limited leverage, we consider the second definition of response rate which includes telephone interviews. The idea is that, if busy people are time constrained and that does not make it possible for them to take part in a full face-to-face interview, well tailored materials could still make them willing to participate in the study by giving a telephone interview, including, perhaps, via mobile phone.

The results in column (2) show that the tailored report did not have any positive effect on the overall response rate for young people when telephone interviews are included. The implication for young people, then, is that the tailored report tends to increase the probability of a sample member giving a full face-to-face interview rather than just a telephone interview, but does not increase the overall probability of giving an interview of any kind. However, the findings for busy people suggest that tailored materials could improve the overall response rate for such people when there is a less time-consuming alternative to a face-to-face interview. When telephone interviews are included, response rate amongst busy people is significantly higher amongst the treated group. Thus, the tailored materials have a positive impact for both target groups, but in slightly different ways. For young people, the effect seems to be to encourage them to give a full face-to-face interview rather than just a telephone interview, whereas for busy people it seems to be to encourage them to give a telephone interview rather than no interview.

For the sample as a whole, the impact on overall response rate is modest (0.23%) and non-significant if considering face-to-face interviews and disappears completely when

also telephone interviews are added. The overall impact is of course constrained by the fact that in our experiment only 36.6% of sample units belonged to one of the target groups and, in any case, the treated unit belong to a very mature and co-operative panel of respondents. The impact of targeted materials could be greater if it proves possible to identify appropriate subgroups that account for a larger proportion of the total sample. The ability to do this will depend on the nature of the survey and of the study population.

6. Summary and conclusions

We have presented the results of two sets of experiments aimed at testing alternative methods to reduce panel attrition. The experiments address two main problems. The between-wave contact experiment evaluates the cost-effectiveness of alternative strategies aimed at encouraging sample members to tell us their current address details and hence to keep in the sample mobile sample members who might otherwise be lost. The tailored materials experiment exploits the longitudinal information we have about individuals to target particular groups of sample members in order to highlight the relevance of the study to their personal circumstances, increase the salience of the study to them, and limit panel fatigue. This was done through the design and content of the between-wave respondent report to make this more salient for two specific categories of lower propensity response groups, namely young people aged under 25 and busy people working or commuting long hours or self-employed.

The between-wave contact experiment identified that certain types of mailings are more effective than others in encouraging sample members to provide updated addresses and in improving the probability of tracing mobile sample members at the following wave. In particular, we found that, in spite of inducing a lower share of cards returned and of being one of the cheapest methods among those analysed, mailing a change-of-address card with an incentive conditional on return – the strategy that has been standard practice on the BHPS - is the most effective strategy both in collecting information on change of addresses and in actually reducing the number of cases left untraced at the following wave. This strategy was significantly more effective than mailing an address confirmation card to be returned by all sample members, regardless of the level or conditional nature of the incentive associated with the latter. In some respects, the fact that the COA card treatment seems to be more effective in terms of reducing the number of untraced movers is not surprising. If we have a current address for a sample household at the point the sample is issued to field, interviewers stand a better chance of making contact and by definition these cases will not go into tracing (unless a second move we have not been notified about has occurred). Sending back a card simply to confirm you have not moved may not appear particularly relevant to sample members who have no intention of moving address at the time they receive the address confirmation card. In contrast, returning a card which is specifically designed to provide change of address details from a recent move, may be more likely to be seen as relevant by sample members who have moved or

are intending to. Additionally, we found no evidence that the amount of the incentive (£2 or £5) made a difference to the final response rate at the following wave. This was the case for all three of the mailing strategies with which incentive levels were tested, suggesting that it is more important to focus on the strategy used rather than on the amount of the incentive.

In order to better assess the cost effectiveness of each mailing strategy, we also computed the effect of each strategy on the number of phone calls made during office tracing and the number of contact attempts made by interviewers in the field. The findings suggest that the COA card with conditional incentive results in a similar number of calls related to the tracing process in the office as the other strategies involved reply-paid cards and is associated with the lowest overall number of in-field contact attempts.

In sum, our analysis suggest that not only is the COA incentive scheme the most effective in reducing the number of people who remain untraced, but it is also efficient since it is the cheapest both in terms of direct monetary costs and in terms of indirect monetary costs due to phone calls.

The tailored materials experiment shows that tailoring respondent reports could be a successful strategy to keep specific categories of lower response propensity respondents interested in the survey. In particular, we found that, even in a mature sample, tailored materials significantly increased the share of full face-to-face interviews among young people, and increased the overall response rate amongst busy people when telephone interviews are included. It is interesting that the effect, while positive for both target groups, is of a different nature in each case, reflecting the ways in which different respondents are able to change their response behaviour as a consequence of the treatment, through for example answering by an alternative mode.

We believe that our findings have important practical implications for researchers designing and running panel surveys, as well as shedding some new light on aspects of the survey non-response process and suggesting promising avenues for further research. The finding that AC cards produced much higher rates of return than COA cards but had no greater impact on location rates at the subsequent wave suggests strongly that AC cards, which are considerably more expensive to administer than COA cards, are simply not an effective strategy and surely not an efficient one. The relative success of COA

cards – and to a lesser extent the treatment with no cards - at obtaining new addresses suggests that the emphasis of the message is important. The key point here is that new address information is of much greater value to the survey organisation than a simple confirmation of an existing address; this message may have been diluted in the AC treatment. The success of the tailored mailings at inducing response is perhaps surprising considering the mature and co-operative nature of the panel sample. This suggests that even greater gains from such strategies might be possible in other circumstances, such as at the early stages of a panel survey. Further work would certainly seem to be warranted in identifying the most promising subgroups for tailored materials and the most appropriate nature of the materials. One might also speculate as to whether there might be some crossover between the two experiments, in the sense that one might consider tailoring the approach used to collect updated address information, by targeting groups at heightened risk of changing address with more expensive or intensive methods (e.g. two mailings between each wave instead of just one, or the use of AC rather than COA cards), or by using different designs of letters and cards. There is plenty for researchers yet to learn about how best to maximise sample retention in longitudinal surveys.

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Appendix. types of respondent reports

Figure 1. Standard respondent report: cover

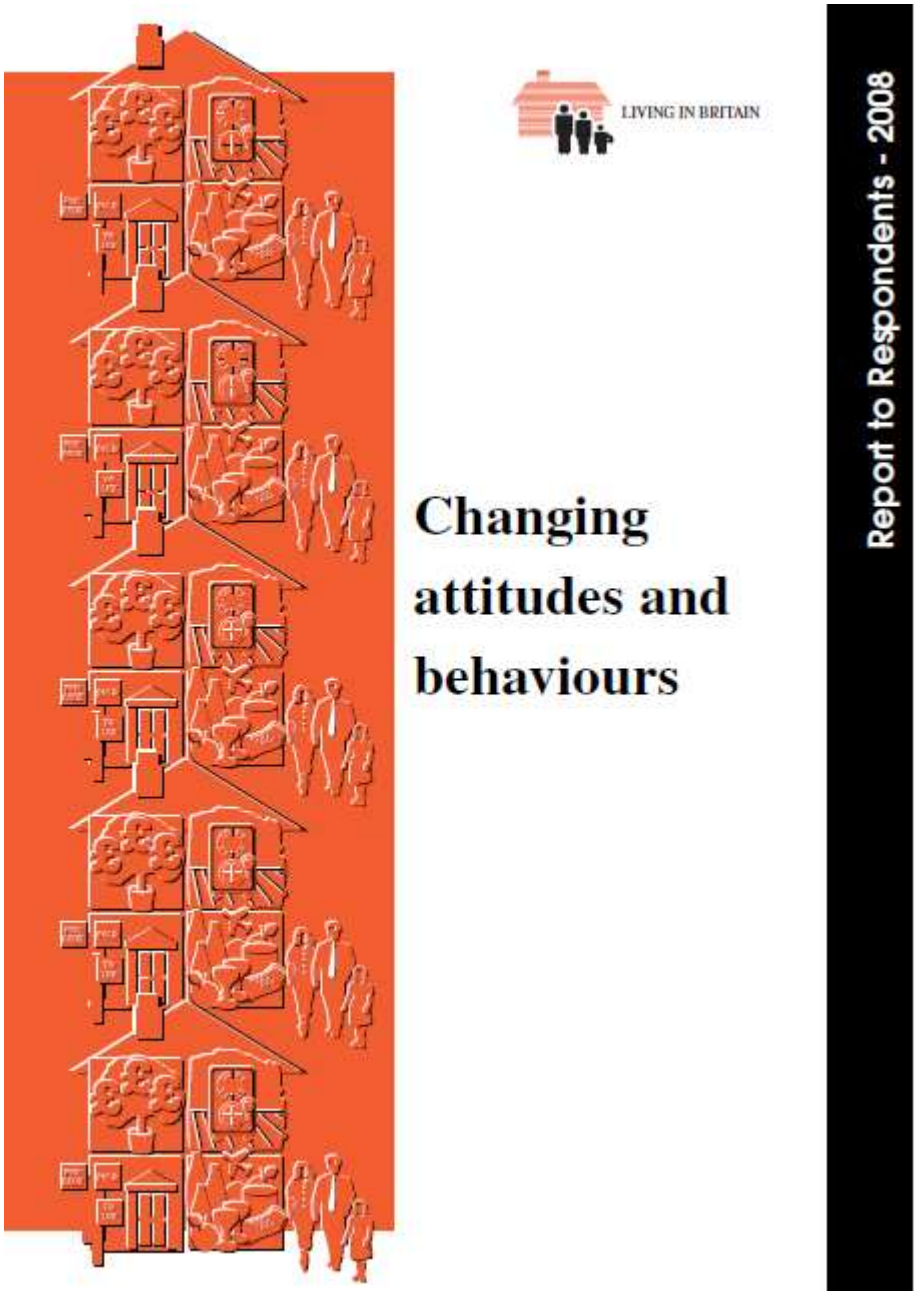
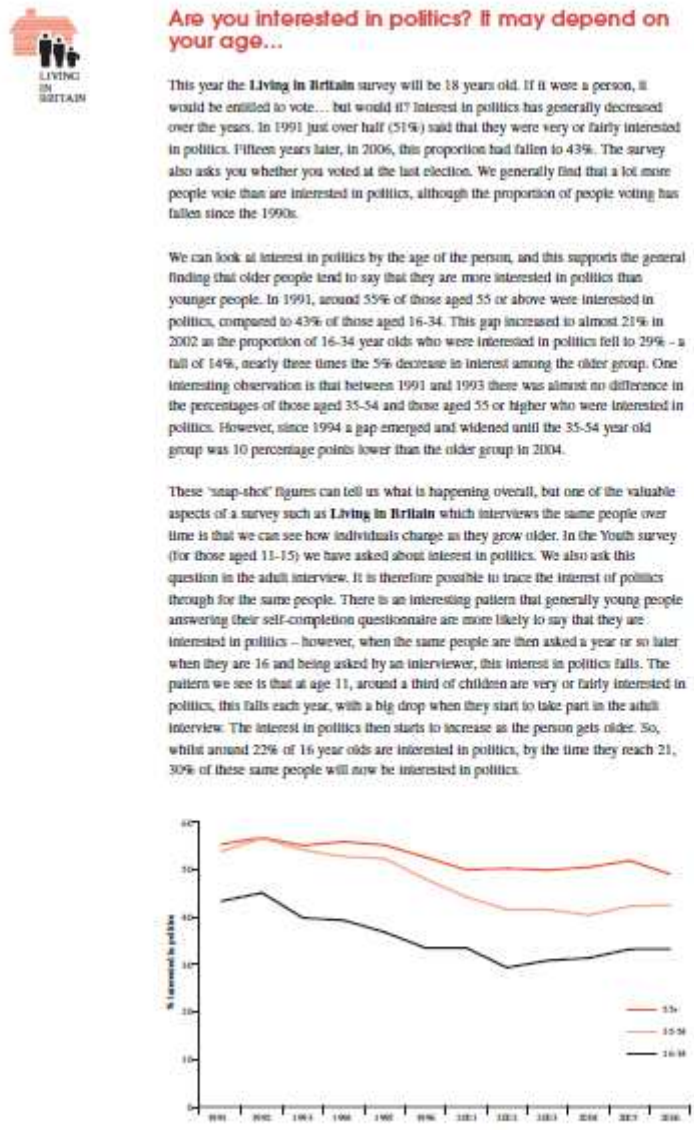


Figure 2. Standard respondent report: example inner page



LIVING IN BRITAIN

Are you interested in politics? It may depend on your age...

This year the **Living in Britain** survey will be 18 years old. If it were a person, it would be entitled to vote… but would it? Interest in politics has generally decreased over the years. In 1991 just over half (51%) said that they were very or fairly interested in politics. Fifteen years later, in 2006, this proportion had fallen to 43%. The survey also asks you whether you voted at the last election. We generally find that a lot more people vote than are interested in politics, although the proportion of people voting has fallen since the 1990s.

We can look at interest in politics by the age of the person, and this supports the general finding that older people tend to say that they are more interested in politics than younger people. In 1991, around 55% of those aged 55 or above were interested in politics, compared to 43% of those aged 16-34. This gap increased to almost 21% in 2002 as the proportion of 16-34 year olds who were interested in politics fell to 29% – a fall of 14%, nearly three times the 5% decrease in interest among the older group. One interesting observation is that between 1991 and 1993 there was almost no difference in the percentages of those aged 35-54 and those aged 55 or higher who were interested in politics. However, since 1994 a gap emerged and widened until the 35-54 year old group was 10 percentage points lower than the older group in 2004.

These 'snap-shot' figures can tell us what is happening overall, but one of the valuable aspects of a survey such as **Living in Britain** which interviews the same people over time is that we can see how individuals change as they grow older. In the Youth survey (for those aged 11-15) we have asked about interest in politics. We also ask this question in the adult interview. It is therefore possible to trace the interest of politics through for the same people. There is an interesting pattern that generally young people answering their self-completion questionnaire are more likely to say that they are interested in politics – however, when the same people are then asked a year or so later when they are 16 and being asked by an interviewer, this interest in politics falls. The pattern we see is that at age 11, around a third of children are very or fairly interested in politics, this falls each year, with a big drop when they start to take part in the adult interview. The interest in politics then starts to increase as the person gets older. So, whilst around 22% of 16 year olds are interested in politics, by the time they reach 21, 30% of these same people will now be interested in politics.

Figure 3. busy people respondent report: side1

Figure 3. young people respondent report: side 1