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Abstract

Refusal conversion is one of the fieldwork strategies commonly used to minimise non-response in surveys. However, there is relatively little evidence about the effectiveness of this strategy, particularly in face-to-face longitudinal surveys. Moreover, much of the existing evidence is based on observational studies. This paper evaluates the effectiveness of fieldwork strategies to covert refusals using evidence from a randomised experiment implemented on a large-scale longitudinal study in the UK. We show that intensive re-issuing is an effective way of increasing the proportion of refusals converted to a productive interview and hence increasing the sample size and reducing the refusal rate. We also show that refusal conversion may have led to a reduction in non-response bias in the survey estimates for several key variables.

Key words

Acknowledgements

1. Introduction

Most surveys typically devote considerable resources to maximising response rates because non-response is the main source of non-observational error in sample surveys (Groves, 1989). In the context of declining response rates over recent years (de Leeuw and de Heer, 2002), survey organisations have had to make increased efforts to maintain response rates (Stoop, 2005) and there has been a growth in methodological research into the effectiveness of fieldwork strategies to minimise non-response.

However, there is evidence that higher response rates do not necessarily imply lower levels of non-response bias, which is whether or not non-respondents are systematically different from respondents on observed variables of interest. In their meta-analysis, Groves and Peytcheva (2008) conclude that the extent to which higher response rates are associated with less bias depends on the degree of correlation between the predictors of survey participation and the substantive survey variables and that the level of bias can differ between surveys with similar response rates and between variables of interest within the same surveys.

Refusal conversion is one of the fieldwork strategies commonly used to minimise non-response and involves re-approaching sample members who refuse the initial request for an interview with the aim of persuading them to take part. This paper evaluates the effectiveness of refusal conversion within an ongoing longitudinal study by assessing its impact on sample size (and hence precision), response rate and non-response bias.

As we shall see, the majority of the literature about the effectiveness of this approach relates to cross-sectional telephone surveys. There is relatively little evidence about refusal conversion in face-to-face surveys and even less in longitudinal surveys. In addition, although there are some examples of experimental approaches to the evaluation of the effectiveness of refusal conversion techniques on telephone surveys, the majority of the evidence in this area is from observational studies. The contribution of this paper comes from the evidence it presents from a methodological experiment designed to convert refusals in a face-to-face longitudinal survey. The experiment consisted of two interventions in a crossed design, i.e. four groups with randomised assignment to groups. The first intervention involved sending a leaflet to refusals aimed at persuading them to take part and the second intervention involved attempting face-to-face conversion for all refusals.

The next section reviews the literature in relation to fieldwork strategies to minimise non-response with a focus on the evidence in relation to refusal conversion and non-response bias. Section 3 discusses the design of the experiment to convert refusals and Section 4 provides details of the implementation of the experiment and the study, the UK Millennium Cohort Study, on which it was carried out. Section 5 provides results from the experiment and Section 6 concludes and reflects on the implications of the findings for fieldwork strategies in longitudinal studies.

2. Strategies for minimising non-response

There is large body of literature relating to fieldwork strategies for minimising non-contact for both face-to-face (e.g. Campanelli et al., 1997; Lynn and Clarke, 2002: Lynn et al., 2002) and telephone surveys (e.g. Weeks et al., 1980, 1987). However, this section focuses on the evidence in relation to minimising refusals. On most cross-sectional and longitudinal surveys, refusals are the largest source of non-response and are the focus of this paper.

The decision about whether to co-operate or refuse to a survey is made by the sampled person and is a product of the interaction between them and the survey, which is usually mediated through an interviewer. Groves and Couper (1998) outline four main influences on the decision to co-operate for cross-sectional surveys: the social context or environment, characteristics of the sampled person, survey design factors and the characteristics of the interviewer. Lepkowski and Couper (2002) point out that the decision to participate at the second or later wave of a longitudinal study will be influenced by the respondents' experience at prior waves and any between-wave contact with the survey.

In relation to survey design factors, there is considerable evidence that survey mode and the topic of the survey are associated with differential refusal rates, e.g. face-to-face surveys tend to get lower refusal rates than telephone surveys and health surveys tend to get lower refusal rates than economic surveys. There is also evidence about the effectiveness of different fieldwork strategies for minimising non-response due to failure to co-operate. For example, monetary and non-monetary incentives can help minimise refusal rates on both cross-sectional (Singer et al., 1999; Singer, 2002; van den Brakel, 2006) and longitudinal surveys (Laurie and Lynn, 2009) and using advance letters or leaflets can also reduce refusal rates (de Leeuw et al. 2007). In relation to interviewer characteristics, there is evidence that the interviewer's attitude, behaviour and experience can have a considerable impact on refusal rates (Hox and de Leeuw, 2002; Campanelli, 1997). On longitudinal studies, interviewer continuity is often associated with lower refusal rates at subsequent waves (Campanelli and O'Muircheartaigh, 1999).

The focus of this paper is on the effectiveness of refusal conversion as a strategy for increasing response rates and reducing non-response bias. Refusal conversion is when a sampled person who has initially refused to take part is re-approached and attempts are made to try to get them to reconsider their decision not to take part, i.e. to 'convert' them from a refusal to a successful interview. Often, but not necessarily, they will be re-approached by a different interviewer. The rationale for this is that a sample member's co-operation decision is influenced by their interaction with the interviewer and sending a different interviewer will lead to a different interaction and, hopefully, a positive decision about participating. In addition, more experienced interviewers are often used for refusal conversion as they tend to have lower refusal rates. It is relatively common practice for survey agencies to attempt refusal conversion during the data collection phase of a survey. Some of these attempts at persuading sample members to take part are clearly successful as converted

refusals constitute a significant minority of completed interviews in many surveys. Lynn et al. (2002) report that, on six UK face-to-face surveys conducted between 1995 and 1998, converted refusals accounted for between 1.2 per cent and 8 per cent of all completed interviews. In addition, there is some evidence that, for repeated cross-sectional surveys, the proportion of completed interviews, which are converted refusals, has been increasing over time. Curtin et al. (2000) report that on the Survey of Consumer Attitudes, a long-running repeated cross-sectional telephone survey in the US, the proportion of interviews from refusal conversions doubled from 7.4 per cent in 1979 to 14.6 per cent in 1996.

Much of the literature attempts to evaluate the impact of refusal conversion on non-response bias. In order to assess the level of non-response bias, it is necessary to have information about both non-respondents and respondents, e.g. from sample frame data or linked administrative data. However, most studies do not have much information of this kind and instead estimate the impact of refusal conversion on bias reduction by comparing the distributions of survey variables with and without converted refusals and comparing the distributions of those originally interviewed with those interviewed as a result of a refusal conversion. The argument is that if the exclusion of converted refusals makes little or no difference to the distribution of the survey variables, refusal conversion has had little or no impact on non-response bias.

There are several examples in the literature from cross-sectional telephone surveys in the US which employ this approach and find that although refusal conversion (and other extended field efforts to maximise response) can have a positive impact on response rates, there is little or no evidence that this is beneficial in terms of reducing non-response bias (Carlson and Strouse, 2005; Curtin et al., 2000; Keeter et al., 2000; Retzer et al., 2004). The strong implication of these papers is that the additional resources devoted to response maximisation on these surveys may not be justified, as they appear to bring little or no benefits in terms of bias reduction.

A similar approach to assessing the impact of refusal conversion (and other extended interviewer efforts) on non-response bias was taken on six face-to-face cross-sectional surveys in the UK (Lynn and Clarke, 2002; Lynn et al., 2002). They found that refusal conversion did appear to bring some benefit in terms of bias reduction for survey estimates relating to financial variables as significant differences were found between those initially interviewed and converted refusals. However, this conclusion was not replicated for health or attitude variables, as there were no systematic significant differences between reluctant and non-reluctant respondents in relation to these variables.

Longitudinal surveys are typically in a better position to assess the impact of refusal conversion on non-response bias than cross-sectional surveys, as information about most if not all sample members is available from prior waves. However, the context of refusal conversion is very different for an ongoing longitudinal study compared with a cross-sectional survey. The main difference is that refusal conversion can take place both within and across waves of data collection, i.e. refusals can be re-approached at subsequent waves as well as (or instead of) during the current wave of data collection. It is therefore important for those running longitudinal surveys to be

able to take informed decisions about when it is most cost-effective to devote resources to refusal conversion. Longitudinal survey managers take decisions, wave by wave, about which sample members will be re-approached for interview and most longitudinal surveys do not automatically exclude respondents who have refused at a prior wave from future surveys. This is because it is well-established that refusal to participate at a particular wave of a longitudinal survey, conditional on participation at the baseline wave, does not necessarily mean that the sample member will not take part in future waves.

Longitudinal surveys can also attempt refusal conversion within a particular wave of data collection. However, the balance of risks and rewards in relation to this kind of refusal conversion is different in longitudinal surveys compared with cross-sectional surveys. In cross-sectional surveys the overriding aim is to secure the one-off participation of the sample member. As there is no intention to go back to the sample member after the interview, the impact of refusal conversion attempts on future participation is not a consideration. In longitudinal surveys, the objective of securing participation needs to be repeated at each wave of data collection and there will be another opportunity to convert refusals at a future wave so, while maximising response at a particular wave is important, this short-term aim needs to be balanced against the desire not to jeopardise participation in future surveys. Similarly, the impact of refusal conversion on non-response bias in longitudinal surveys cannot be fully realised or assessed until future waves of data collection have taken place. Burton et al. (2006) evaluate the long-term effectiveness of within-wave refusal conversion procedures on a long-running household panel survey in the UK. They use pooled data from waves 4-13 of the British Household Panel Survey (BHPS) and report that within-wave refusal conversion was attempted for around 36 per cent of refusals and 37 per cent were converted to a face-to-face, proxy or telephone interview, giving an unconditional conversion rate of 13 per cent. They also show that, in their study, the majority of converted refusals went on to participate in future waves, i.e. within-wave refusal conversion did not just have a short-term effect and did not appear to jeopardise future participation.

As discussed above, longitudinal surveys are generally better equipped than cross-sectional surveys to assess the benefits of refusal conversion in terms of bias reduction. This means that longitudinal surveys are able to extend and refine the approach taken by previous cross-sectional research on bias reduction. As well as comparing the characteristics of converted refusals and those who were initially interviewed, it is also possible to include comparisons between unconverted refusals and both converted refusals and initially interviewed sample members. This approach is likely to give a better estimate of the impact of refusal conversion on bias reduction. Burton et al. (2006), taking this approach, provide evidence to show that refusal conversion on the BHPS improved the representation in the sample of certain groups (such as the geographically mobile, self employed and local authority renters) and, to the extent that these variables are correlated with other variables of interest, may be expected to reduce non-response bias. Other than this BHPS study, there appears to be no published evidence about the effectiveness of refusal conversion attempts in longitudinal studies.

A major limitation of almost all of the literature assessing the impact of refusal conversion on bias reduction is that it is based on observation of fieldwork procedures rather than randomised experimental interventions. There are only a few studies which experimentally evaluate the effectiveness of different refusal conversion techniques and these have been on telephone surveys (Basson and Chronister, 2006; Keeter et al. 2000). This is problematic because estimates of the effectiveness of refusal conversion attempts, including their impact on non-response bias, will be influenced by selection effects, i.e. the sample members for whom refusal conversion was attempted may themselves have been systematically different from sample members for whom refusal conversion was not attempted and therefore conclusions based on a non-random sub-sample of refusals may not be generalisable to all refusals. For example, a finding that converted refusals have similar characteristics to sample members who are interviewed initially may be due to the fact that refusals with similar characteristics to initially interviewed sample members are more likely to be allocated to refusal conversion than those with different characteristics. The randomised experiment described in the following section was specifically designed to address this limitation in the existing methodological research evidence.

3. A randomised experiment to convert refusals in an ongoing birth cohort study

The design of an experiment to convert refusals in a longitudinal study needs to be driven by hypotheses about the factors which affect an individual's decision to participate in subsequent waves of a longitudinal study, conditional on participation in the baseline survey. In addition, any intervention which involves experimenting with fieldwork strategies in an ongoing longitudinal study needs to be sensitive to the potential impact of this experiment on the long-term integrity of the study.

The experiment described in this paper was developed for and carried out on the fourth wave of the Millennium Cohort Study (MCS). The MCS, which is following over 19,000 UK children born in 2000/1, is one of four birth cohort studies in the UK. The sample, which was recruited through child benefit1 records, is disproportionately stratified and clustered. The data collection for the study takes place in the home and involves face-to-face interviews with multiple informants in each family. There have been four waves of the study so far at 9 months (2001-2), age 3 (2003-4), age 5 (2006) and age 7 (2008). The achieved sample size at wave one was 18,552 families, representing a baseline response rate of 72% (Plewis, 2007). The achieved sample declined to 15,590 families at age 3, 15,246 at age 5 and 13,857 at age 7 (Ketende, 2010). The data collection for the study is competitively tendered and sub-contracted to a reputable and suitably experienced fieldwork agency. The interviewers who work on the study receive special training and the data collection is carried out to high quality standards.

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[1] Child Benefit is a universal benefit paid to parents.

In the fourth wave of the study, as in previous waves, interviewers were responsible for sending an advance letter and leaflet to study families and then making contact with them by phone or in person to set up an appointment for the interview. Although contact procedures have been shown to be related to refusal rates, it was decided that experimenting with these standard contact procedures had the potential to jeopardise the integrity of the study and, as a result, this was not considered. Similarly, incentives are not used in MCS. Giving some families incentives as part of an experiment might have meant that they would have been reluctant to take part in future waves without an incentive. For this reason, experimentation with incentives was not considered.

More generally, an important aim of this methodological research was to provide strong experimental evidence on which to make recommendations for fieldwork strategies in future waves of MCS and other longitudinal studies. For this reason, it was important that the fieldwork interventions tested had broad application and were potentially generalisable.

The experimental intervention developed and implemented on MCS4 was designed to test the hypothesis that devoting extra field resources to converting refusals would be an effective way to increase the overall co-operation rate and that this would bring analytical benefits in the form of an increased sample size and hence more precise survey estimates and (possibly) less attrition bias. The aim of the intervention was to increase the proportion of refusals who are converted to productive interviews.

In order to achieve this aim, the experiment consisted of two interventions. The first involved providing additional information in the form of a leaflet, which encouraged the sample member to reconsider their decision not to take part by addressing common reasons for refusal and emphasising the importance of the study. The leaflet included a Freephone number, which sample members who changed their mind upon receipt of the leaflet could phone to request an interview. The rationale for this intervention was guided by evidence that adopting a 'tailored' approach to securing co-operation, i.e. adapting arguments for taking part in a survey to the individual sample members concerns, is an effective way of minimising refusal rates (Campanelli et al., 1997). The leaflet was tailored to previously reported reasons for refusal on the study. The control condition for this intervention was not sending a leaflet.

The second intervention, designed to increase the proportion of sample members for whom refusal conversion was attempted, involved the intensive re-issuing of refusals, i.e. re-issuing all refusals rather than a sub-set. As discussed above, one of the limitations of previous work in this area is that the selection of refusals into the treatment of refusal conversion is non-random. In this experiment, it was decided that all refusals should be re-issued, i.e. the discretion of field staff over whether or not to re-issue was, at least in principle, removed. The control condition for this intervention is the standard re-issuing strategy of the fieldwork organisation, i.e. field staff making case-by-case decisions and re-issuing a sub-set of refusals. The fieldwork agency was the same at waves 3 and 4. At wave 3, the standard re-issuing procedures

resulted in just 4 per cent of initial refusals being re-issued and 34 per cent of these being converted to a successful interview.

The design of these interventions was guided by the over-arching hypothesis that the failure to co-operate in later waves of a longitudinal survey, conditional on having been willing to co-operate initially, will be largely driven by the sample member's circumstances when they are approached for an interview at a subsequent wave and their judgement about the relative costs and benefits of taking part again. It may also be influenced by their interaction with the interviewer, who may or may not be the same person who has interviewed them previously. For this reason, it may be expected that a further attempt to persuade sample members to take part, at a later date and by a different interviewer, or a leaflet providing sample members with tailored information aiming at persuading them to change their mind about participating would lead to a refusal conversion.

As the experiment involved two interventions, each with two levels, a 22 factorial design was used. This general approach has been widely used in survey research to test question wording in the guise of 'split-ballots' (Presser et al., 2004), but not, to our knowledge, to test the efficacy of interventions to increase cooperation in longitudinal studies. The strength of the factorial design is that it makes it possible to separate in the analysis the effects on refusal conversion of the different components of the intervention, and to test whether a combination of these components is more effective than each component on its own. For example, it was hypothesised that the leaflet in combination with intensive re-issuing would lead to an improved conversion rate compared with intensive re-issuing alone as respondents who had received and read the leaflet would be more receptive to an interviewer and easier to persuade. It was expected that the success or failure of the conversion attempt would rely less on the interviewer's skills of persuasion if a leaflet broadly tailored to the sample members concerns was sent ahead.

The crossed experimental design and the intended treatment for each of the experimental groups is summarised in Table 1 below.

Table 1: Experimental groups and interventions

Group A: Intensive reissue and leaflet I1: Leaflet sent to all refusals I2: All refusals reissued (to a different interviewer)	**Group C: Intensive re-issue, no leaflet** C1: No leaflet sent to refusals I2: All refusals reissued (to a different interviewer)
Group B: Standard reissue and leaflet I1: Leaflet sent to all refusals C2: Standard proportion of refusals reissued (to a different interviewer)	**Group D: Standard re-issue, no leaflet** C1: No leaflet sent to refusals C2: Standard proportion of refusals reissued (to a different interviewer)

Based on the expectation that each of the experimental groups would contain roughly 450 cases, it was estimated that the statistical power of a 5 per cent increase in conversions, from a low overall base of 5 per cent, is over 0.80.

3.1 Designing the leaflet

As discussed above, the first intervention involved developing a leaflet addressing common reasons for refusal. Many surveys routinely record reasons for refusal. One of the benefits of carrying out this experiment in a longitudinal study such as MCS was that the design of the leaflet could be tailored to the study by using information about reasons for refusal at the previous wave. This information was reported by interviewers, i.e. it is proxy information rather than information collected directly from sample members who refused. This kind of tailoring would not have been possible in a cross-sectional survey. The most common reasons for refusal reported at MCS3 are shown in Table 2 below. Interviewers were

Table 2: Reasons for refusal at MCS3 [1]

Rank	Reason	%
1	"Too busy"	28.4
2	"Doesn't want to bother"	19.0
3	"Stressful family situation"	12.4
4	"Survey too long"	8.9
5	"Looking after children"	7.1
6	"Questions too personal"	4.1
7	"Don't see personal benefit"	4.0
8	"Survey not important"	3.3
9	"Survey waste of time"	2.8
10	"Other family members opposes participation"	2.4

[1] Unweighted percentages based on 1786 families. Percentages do not sum to 100 as more than one reason per family could be recorded.

Table 2 shows that many of the reasons reported by the interviewers for refusal at MCS3 were circumstantial e.g. 'too busy', 'stressful family situation' and 'looking after children'. This evidence lends supports to the hypothesis that refusals at the second or later waves of longitudinal studies are strongly influenced by the sample members' circumstances at the time of the interview request.

The leaflet aimed to address these reasons for refusal in the form of frequently asked questions and answers. The leaflet was piloted during the dress rehearsal for MCS4 and revised following feedback from interviewers. A copy of the leaflet is provided in Appendix A.

4. Implementing the experiment

4.1 Randomisation

The random assignment of cases to treatment and control groups took place prior to the start of the fieldwork for the fourth wave of the study. The decision to carry out this assignment in advance was taken primarily for operational reasons. During fieldwork refusals were to be re-issued on an ongoing basis by field staff and it was felt that prior allocation to treatment and control group would help to ensure that the experiment was implemented accurately and efficiently.

The MCS study contains 19,244 families. Although not all of these families were expected to be issued to the field for the fourth wave, all of these families were randomly assigned to an experimental group prior to the start of fieldwork. Prior to the randomisation, the file containing the 19,244 families was stratified by a selection of variables, which were expected to be related to participation in order to ensure that these variables were equally distributed in each of the experimental groups. Implicit stratification (the cases ordered by the value of the selected variables) rather the explicit stratification (the cases split into groups) was used. After the cases were ordered by the stratification variables, a random start was generated and cases were allocated sequentially to each of the four experimental groups until all cases were assigned.

The variables chosen for the stratification were the original sampling strata, issued batch for MCS4 and whether the family had refused or not at MCS3. The MCS sample is a stratified sample and the stratification was based on the country and the characteristics of the sampled wards. In Scotland, Wales and Northern Ireland, there were two strata in each country: disadvantaged and advantaged. In England, there were three strata: ethnic, disadvantaged and advantaged. More details of the MCS sample design, including the definition of sampling strata, can be found in Plewis (2007). This variable was chosen as it has been shown to be related to participation rates at all waves of MCS. The issued sample for MCS4 was split into two batches within each country with fieldwork for each batch taking place at different times of the year. This variable was chosen to account for the influence of seasonality on response rates and for practical reasons, i.e. to ensure that the experimental cases were evenly distributed throughout the fieldwork period. The other variable chosen was whether or not the family had taken part at the previous wave, i.e. MCS3 as it was expected that those who had refused then would be less likely to take part at MCS4.

The experiment was intended to cover all four countries in the UK. However, data collection in Northern Ireland was sub-contacted to a different fieldwork agency. It was recognised at the outset that this may lead to comparability issues between Great Britain and Northern Ireland as standard procedures for re-issuing were different in the different agencies and different fieldwork staff would be taking the decisions about re-issuing cases. However, it was important to include Northern Ireland in the experiment, especially as response rates there have tended to be lower

on MCS. Extensive efforts were made by the GB agency to ensure that the same procedures for the experiment were followed in Northern Ireland. However, despite this, the experiment was not implemented adequately in Northern Ireland. Over half of cases eligible for the leaflet intervention were not sent a leaflet and almost all cases, which were eligible to be re-issued, were excluded. For this reason, Northern Ireland has been excluded from our analysis and the remainder of the paper focuses on GB only.

In total, 15,350 families in Great Britain were issued for the fourth wave of the study. Appendix B shows the number of issued cases by experimental group along with descriptive statistics for the stratification variables and a selection of background variables. As expected, families are distributed roughly equally by experimental group and the distribution of these stratification and background variables is roughly the same for each experimental group.

4.2 Intended treatment and actual treatment

Although the intention of the experiment was to apply the treatment to all refusals, from the outset it was recognised that in practice some 'hard' refusals would need to be excluded from the experiment. For this reason, the actual treatment was likely to differ from intended treatment for some cases. Two sources of information about the nature of the refusal were used to determine whether or not it was a 'hard' refusal. The first was the survey outcome code allocated in the field, usually by the interviewer. In common with most surveys, MCS uses a pre-coded list of outcome codes during fieldwork to store and convey information about the eligibility, contact and participation status of sample members. There are several different refusal outcome codes in order to distinguish between different types of refusal. The second is textual information, usually from interviewer notes, about the nature and circumstances of the refusal.

In relation to the first intervention, i.e. the leaflet, all types of refusal outcome code were considered as eligible to be sent a leaflet and cases were only excluded as 'hard' refusals from this treatment if the field staff felt this was necessary based on a case-level review of the interviewer notes about the nature and circumstances of the refusal.

In relation to the second intervention, i.e. re-issuing, only certain types of refusal outcome code were considered for re-issue. Refusals given by the sample member directly to the office of the fieldwork agency (or to the office of study's principal investigator) rather than to the interviewer, refusals during tracing and refusals during the interview were considered 'hard refusals' and hence not considered for re-issue, in either the treatment or control groups. This follows the standard practice of the fieldwork agency. Office refusals are generally treated as 'hard' refusals as the fact that the sample member has proactively contacted the office, without waiting for the interviewer to make contact, usually indicates a firm decision not to take part. Refusals during tracing and refusals during interview are relatively uncommon refusal

outcome codes and are excluded as 'hard' refusals primarily because it is difficult to send an interviewer back in these circumstances. In addition to this automatic exclusion based on survey outcome code field staff had the discretion to exclude other cases as 'hard' refusals based on a case-level review of the interviewer notes about the nature and circumstances of the refusal.

An automated procedure was set-up to ensure that all refusals were reviewed upon receipt and strict guidelines were given to field staff in order to ensure that the experiment was implemented as designed and to minimise the number of cases that were excluded at the discretion of the field staff.

In addition to the exclusion of 'hard refusals', a small number of cases were excluded from the experiment for other reasons. This was usually because the fieldwork agency was unable to supply an interviewer to work on the re-issue in the time available or because the refusal was received too late in the fieldwork period to apply the treatment.

Overall, there were 1660 refusals. Table 3 below shows the number of refusals in each experimental group and summarises exclusions and actual treatment by experimental group.

The fact that only a sub-set of refusals was considered for re-issuing, but all refusals were eligible for the leaflet meant that, in the experimental groups in which the leaflet treatment was given, some families were sent a leaflet, but excluded from the re-issuing as 'hard refusals'. We treat these cases as 'hard refusal' exclusions from the experiment.

Table 3: Refusals, exclusions and actual treatment by experimental group

	Group A: Intensive re-issue, leaflet	Group B: Standard re-issue, leaflet	Group C: Intensive re-issue, no leaflet	Group D: Standard re-issue, no leaflet
Number of Refusals[1]	414 (24.9%)	437 (26.3%)	389 (23.4%)	420 (25.3%)
Excluded as 'hard' refusal'[2]	100 (23.5%)	61 (13.9%)	80 (20.5%)	67 (15.9%)
Outcome code[2]	55 (13.3%)	56 (12.8%)	52 (13.4%)	67 (15.9%)
Field staff decision[2]	45 (10.9%)	5 (1.1%)	28 (7.2%)	0 (0.0%)
Excluded for other reason	8	9	5	0
Eligible for treatment	306	367	304	353
Type of treatment				
Re-issue and leaflet	300	4	0	0
Re-issue only	6	0	304	3
Leaflet only	0	363	0	0
None	0	0	0	350

[1] Percentage of issued cases [2] Percentage of refusals

Table 3 shows that, as expected, refusals were roughly equally distributed by experimental group. The proportion of cases excluded as 'hard refusals' varied by experimental group. As explained above, a proportion of all refusals were excluded based solely on the outcome code they were assigned in the field, which reflected the nature of the refusal. As expected, the proportion of refusals excluded based on their outcome code was roughly equal (around 13 per cent) in most experimental groups. It was slightly higher (16 per cent) in the control group (D). This accounts for all of the exclusions in the control group. As no 'treatment' was being administered in this group, field staff did not need to make any case-level exclusions. In group B, in which only the leaflet treatment was given, almost all of the exclusions were automatic based on outcome code with a further five cases who were due to receive the leaflet excluded by field staff. The exclusion rate by field staff was higher in group C (7%) than in groups B (1%) and D (0%). This is expected as this group received the intensive re-issue treatment and, as a result, field staff had to review whether each case should be re-issued and take an active decision to exclude them. The exclusion rate was highest in group A (11%). Again, this is expected, as this group was eligible for both the intensive re-issue treatment and the leaflet treatment. Field staff had to review a higher proportion of refusals in this group compared with group C (as more refusals were eligible for leaflet than re-issue) and take an active decision to exclude them.

Overall, the exclusions reduced the number of cases eligible for treatment by 20%, from 1660 to 1330. As the non-random exclusion of cases by field staff reintroduces selection into actual treatment, it has the potential to undermine the validity of the conclusions based on the experimental intervention. However, partial implementation of this kind is a known and common problem in field experiments. The standard solution to this problem is to estimate treatment effects for all cases for which there was an intention to treat as well as for estimate the effects of treatment on the treated (Shadish and Cook, 2009). We follow this approach in the next section on results.

Table 3 also shows how actual treatment differed from intended treatment among those eligible for treatment. In group A, six cases were re-issued without being sent a leaflet. These families were eligible for the leaflet treatment and should have been sent a leaflet prior to re-issue. However, as they were received late in the fieldwork period, there was not enough time to send a leaflet ahead of the interviewer visit for the re-issue.

In groups which were not eligible for the intensive re-issuing treatment, a very low proportion of refusals were re-issued by the 'standard' procedure (<1% in groups B and D). However, in groups which were eligible for intensive re-issuing, a very high proportion of refusals were re-issued (74% of the refusals in group A and 78% in group C). This treatment clearly succeeded in increasing the proportion of refusals that were re-issued. However, it should also be noted that the proportion of cases re-issued in the 'standard re-issue procedure' groups was much lower than the proportion re-issued by the same fieldwork agency at the previous wave of the study (4%). This provides indicative evidence that one of the unintended side-effects of carrying out this experiment may have been that field staff re-issued fewer cases in these 'standard' groups than they otherwise would have in the absence of this

experiment. However, this interpretation is speculative and there are other possible reasons for this difference, e.g. an independent change in the 'standard' procedure of the fieldwork agency in relation to re-issuing or the impact of 'formalising' the procedure for this experiment.

4.3 Interviewer allocation

The fieldwork agency's standard practice is that, where possible, re-issues should be given to a different interviewer and they are generally given to a more experienced interviewer. The aim is to allocate an interviewer who, based on the evidence available and characteristics of the sample member, is judged by field staff to have a good chance at securing a successful conversion. In addition to experience, other interviewer characteristics such as age, gender and ethnicity may also be considered. In practice, there are constraints on the optimal allocation of interviewers to re-issues such as geographical proximity and interviewer availability. In the experiment reported in this paper, around nine in ten re-issued refusals were allocated to a different interviewer.

5. Results

5.1 What proportion of eligible refusals was converted to productive interviews in each of the experimental groups?

Table 4: Final survey outcome for all eligible refusals by experimental group

	Group A: Intensive re-issue, leaflet	Group B: Standard re-issue, leaflet	Group C: Intensive re-issue, no leaflet	Group D: Standard re-issue, no leaflet
Fully productive	54	3	44	2
Partially productive	17	0	24	0
Unproductive	235	364	236	351
Total	306	367	304	353
Productive (%)[1]	23.2	0.8	22.4	0.6
Productive (%)[2]	*17.1*	*0.7*	*17.9*	*0.5*

[1] Percentage based on all eligible refusals.
[2] Percentage based on all refusals.

Table 4 shows the final survey outcome for all eligible refusals by experimental group. Eligible refusals are defined, as explained in Table 3, as those refusals for which treatment was considered i.e. exclusions for any reason are not included. It shows that in the experimental groups in which the intensive re-issuing intervention was administered (A and C) almost a quarter (23% and 22% respectively) of refusals were successfully converted to a productive interview. This compares to less than one per cent in the experimental groups in which the standard re-issuing strategy

was followed. It is clear that intensive reissuing is an effective way to increase the proportion of refusals that are converted to productive interviews. The proportion of re-issued refusals, which were converted in these two experimental groups, was lower than at the previous wave of fieldwork (34%). This is, however, unsurprising as it is to be expected that if a higher proportion of refusals are re-issued, refusals will be on average harder to convert, so the proportion of refusals who are successfully converted will decrease.

Table 4 also shows that the leaflet had no effect on the proportion of refusals successfully converted. There is almost no difference in the proportions between the two groups with the standard re-issue procedure (groups B and D) or between the two groups with the intensive re-issue procedure (groups A and C). In group B, one of the families who were successfully converted was re-issued at the request of the family who changed their mind about participating upon receipt of the leaflet. If this leaflet had not been sent, this family would not have been interviewed. However, this was the only family who requested an interview on receipt of the leaflet. The expectation that the leaflet would have an additional effect in combination with intensive re-issuing was not borne out.

As discussed above, the exclusion rates differed between experimental groups owing to the impact of field staff decisions in the intensive re-issue groups (A and C). This was an expected consequence of the design of the experiment as, unlike in the standard re-issue groups (B and D), field staff were required to review all refusals. However, as the outcome of the experiment is observed for all refusals, including those who were excluded, it is possible to estimate treatment effects based on all refusals, i.e. the 'intention to treat' group. The bottom row of Table 4 shows the proportion of refusals converted to productive interviews based on all refusals. The estimated treatment effect for intensive re-issuing is reduced from 22 per cent and 23 per cent to 17 per cent and 18 per cent in groups A and C due to the increased number of cases in the denominator. However, the pattern of the results is the same as those based on eligible refusals, confirming the conclusions drawn earlier about the effectiveness of the interventions.

5.2 What impact did re-issuing have on the overall achieved sample size and refusal rate?

We see from Table 4 (by summing the first two rows) that refusal conversion has increased the achieved sample size in GB by 144 families. This is equivalent to a one per cent reduction in the refusal rate among the issued sample (from 11 per cent to 10 per cent).

5.3 Were converted refusals less likely than those initially interviewed to complete all of the survey elements?

Table 5: Outcome of different survey elements for families initially interviewed and converted refusals

	Initially interviewed	Converted refusals
Fully productive	10,929 (88.6%*)	103 (71.5%)
Partially productive	1,412 (11.4%*)	41 (28.5%)
Ratio of full to partially productive	7.7	2.5
Main interviewed in person	12,299 (99.7%*)	139 (96.5%)
Partner interviewed in person[1]	8,274 (84.9%*)	67 (63.3%)
Child cognitive assessments completed	12,166 (98.6%)	141 (97.9%)
Child physical measurements completed	12,199 (98.8%)	141 (97.9%)
Child self-completion completed	11,610 (94.1%)	135 (93.8%)
Base: Productive families	12,341	144
Base: Productive families with eligible partner	9,751	109

[1] Percentage of productive families in which there was an eligible partner
* indicates that percentage of initially interviewed is statistically significantly different (p < 0.05) from percentage of converted refusals. T-tests were used to test for statistical significance.

The MCS4 survey consisted of several different data collection elements: personal interviews with a main respondent (usually the cohort child's mother) and a partner respondent (usually the cohort child's father or step-father, if co-resident), cognitive assessments and physical measurements with the cohort child as well as, for the first time on the study, a self-completion questionnaire for the cohort child to complete themselves. If a family completed all of the data collection elements they are eligible for, they were classified as a fully productive family. If they completed some, but not all of the elements they are eligible for, they were classified as a partially productive family.

Table 5 shows that, despite the small sample size for converted refusals, several statistically significant differences in the proportion completing the different survey elements were found for these families when compared with families who were initially interviewed. Families who were interviewed initially were much more likely to have completed all of the survey elements they were eligible for than families who were converted refusals (87% compared with 72%). The ratio of fully productive to partially productive families was 2.4 for converted refusals compared with 7.7 for those who did not refuse initially.

The main interview was completed in almost all productive families, but the proportion was slightly lower among converted refusals than those interviewed initially (97% compared with over 99%). The most striking difference between the two groups was in the proportion of partner respondents who were interviewed. In

productive families who were initially interviewed, almost 85 per cent of partners were interviewed compared with just 63 per cent among productive families who were converted refusals. Equally striking is the similarity between the two groups in the proportions of children completing the elements they were eligible for. Although the proportions are very slightly higher in the group who did not initially refuse, the differences are very small (around 1%) and not statistically significant. This shows that children in productive families who are converted refusals are no less likely to take part than those in productive families who were interviewed initially.

5.4 Were converted refusals more similar to those initially interviewed or unconverted refusals in relation to their prior response history?

Table 6: Prior response for families initially interviewed, converted refusals and unconverted refusals

	Initially interviewed	Converted refusals	Unconverted Refusals
Took part in wave 3	11,781 (95.5%*)	112 (77.8%*)	1015 (64.0%*)
Took part in all prior waves[1]	10,924 (88.5%*)	95 (66%*)	808 (50.9%*)
Re-issued refusal at wave 2	323 (2.6%*)	15 (10.4%)	221 (13.9%*)
Base: Productive families	12,341	144	1,586

[1] For the 18,552 families recruited to the study at wave one, all prior waves= three waves (1-3) and for the 692 families recruited at wave two, all prior waves= two waves (2 and 3).
* indicates a statistically significant (p < 0.05) difference in percentages between initially interviewed and converted refusals (in the initially interviewed column), between converted refusals and unconverted refusals (in the converted refusals column) and between unconverted refusals and initially interviewed (in the unconverted refusals column). T-tests were used to test for statistical significance.

Table 6 shows converted refusals were more similar to families who were initially interviewed in relation to their prior response history than unconverted refusals. Families who were interviewed initially were most likely to have taken part in the most recent wave (96%) and in all prior waves (89%) and the least likely to have been re-issued as a refusal at wave 2 (3%). Unconverted refusals were the least likely to have taken part in wave 3 (64%), in all prior waves (51%) and the most likely to have been reissued as a refusal at wave 2 (14%). Converted refusals occupied an intermediate position between those initially interviewed and unconverted refusals: 78 per cent had been interviewed in wave 3, 66 per cent in all prior waves and 10 per cent had been re-issued as a refusal at wave 2. In time it will be possible to analyse whether converted refusals are less likely than those initially interviewed (and more likely than unconverted refusals) to take part in future waves. As converted refusals have a less complete response history than those initially interviewed and a more complete response history than unconverted refusals, this may lead us to expect that they will have a less complete participation record than those initially interviewed and a more complete participation record than unconverted refusals in future waves.

Table 7: Refusal and conversion rates by prior response wave 3 characteristics

	Number of refusals	Refusal Rate	Conversion rate
	N	*%*	*%*
Total sample who responded at wave 3	1127	8.3	9.9
Main respondent NVQ Level			
NVQ level 1	94	9.0	10.6
NVQ level 2	352	9.5	10.5
NVQ level 3	155	7.7	7.7
NVQ level 4	213	5.9	11.3
NVQ level 5	62	6.0	9.7
Overseas qualifications only	39	9.2	5.1
None of these	209	11.8	9.6
p-value		*<0.001*	*0.870*
Main respondent in work/on leave	542	7.1	11.6
Main respondent not in work/on leave	582	9.7	8.4
p-value		*<0.001*	*0.073*
Cohort member's ethnic group			
White	880	7.9	10.3
Mixed	44	10.2	4.5
Indian	36	9.6	13.9
Pakistani	67	9.7	14.9
Bangladeshi	31	10.7	3.2
Black Caribbean	18	10.5	0.0
Black African	30	9.8	3.3
Other Ethnic group	21	8.6	9.5
p-value		*0.144*	*0.246*
Main respondent voted in last general election	537	6.7	8.8
Main respondent did not vote in last general election	562	10.3	11.2
p-value		*<0.001*	*0.175*
Main respondent's general health			
Excellent	194	7.2	9.3
Very good	371	7.5	10.8
Good	368	9.2	8.4
Fair	130	8.5	13.1
Poor	42	10.0	9.5
p-value		*<0.001*	*0.598*
Family Type			
Married natural parents	575	7.1	9.4
Cohabiting natural parents	189	9.4	13.8
Natural parents (other or unknown relationship)	13	31.7	15.4
Natural mother and step-father	59	11.1	10.2

	Number of refusals	Refusal Rate	Conversion rate
	N	*%*	*%*
Continued			
Lone natural mother	262	10.0	8.8
Other	29	10.3	3.4
p-value		*0.009*	*0.598*
Housing Tenure			
Own	570	6.6	9.8
Rent	491	10.9	10.0
Other	42	11.2	11.9
p-value		*<0.001*	*0.910*

Note: Chi-square tests were used to test the statistical significance of the relationship between each wave 3 characteristic and the refusal rate and, independently, the conversion rate. The results of these statistical tests for each wave 3 variable are given as p-values. P-values <0.05 indicate statistical significance at the 5% level.

Table 7 gives the number and proportion of families in the issued sample at wave 4 refusing originally, i.e. the refusal rate and the proportion of wave 4 refusals converted to a productive interview, i.e. the conversion rate, by a selection of wave 3 characteristics. The conversion rate shown is unconditional, i.e. based on all refusals rather than just those re-issued. The wave 3 characteristics chosen have been shown to be related to attrition and to key substantive variables on MCS and are comparable with Burton et al. (2006). Table 7 shows, as Plewis et al. (2008) did for wave two, that the main respondent's education level, employment status, voting behaviour, general health, as well as family type and housing tenure, were all significantly related to the refusal rate. Interestingly, Table 7 also shows that none of these variables were significantly related to the conversion rate. This indicates that refusal conversion was effective for many different types of sample members, regardless of their characteristics. This lends support to a fieldwork strategy of intensively re-issuing refusals rather than selecting cases for re-issuing based on characteristics, which may not be good predictors of their likelihood of conversion.

5.5 What impact did re-issuing have on bias in the achieved sample?

Table 8: Wave 3 characteristics by outcome at wave 4

	Initially interviewed	*Converted refusals*	*Unconverted Refusals*
	%	*%*	*%*
Main respondent NVQ Level			
NVQ level 1	7.4	12.2	7.9
NVQ level 2	28.3	34.6	31.9
NVQ level 3	14.7	8.7	13.6
NVQ level 4	28.9	23.8	18.6*
NVQ level 5	7.9	6.1	5.5*
Overseas qualifications only	2.6*	0.8*	3.8

	Initially interviewed	Converted refusals	Unconverted Refusals
	%	%	%
None of these	10.2	13.8	18.6*
Main respondent in work/on leave	59.2	59.7*	47.9*
Cohort member's ethnic group			
White	86.9	86.5	82.7*
Mixed	3.1	1.4*	5.1*
Indian	1.9	2.2	2.4
Pakistani	3.1	6.6	3.3
Bangladeshi	1.0	2.0	1.6
Black Caribbean	1.0	0.0	1.2
Black African	1.6	0.4*	2.3
Other Ethnic group	1.4	0.9	1.5
Main respondent voted in last general election	60.8*	40.2	46.6*
Main respondent's general health			
Excellent	20.3*	13.4	17.4*
Very good	37.3	38.5	33.7*
Good	28.8	26.9	34.4*
Fair	10.8	17.9	11.2
Poor	2.8	3.3	3.3
Family Type			
Married natural parents	61.2*	46.9	49.2*
Cohabiting natural parents	14.8*	24.4*	15.4
Natural parents (other or unknown relationship)	0.2	0.9	1.2
Natural mother and step-father	3.8	5.2	6.3*
Lone natural mother	17.8	20.7	24.8*
Other	2.2	2.0	3.1
Housing Tenure			
Own	67.4*	49.2	50.0*
Rent	30.2*	45.1	46.3*
Other	2.5	5.7	3.7
Observations	11781	112	1015

* indicates a statistically significant (p < 0.05) difference in percentages between initially interviewed and converted refusals (in the initially interviewed column), between converted refusals and unconverted refusals (in the converted refusals column) and between unconverted refusals and initially interviewed (in the unconverted refusals column). Statistical significance was assessed by comparing the 95% confidence intervals for the estimates.

It was shown earlier in this section that refusal conversion led to an increase in sample size and a reduction in refusal rates on MCS4. However, as discussed earlier, a higher response rate does not necessarily lead to a reduction in bias in the achieved sample. In order to assess bias in any sample survey it is necessary to know the true population values from another source, e.g. administrative data.

However, in a longitudinal survey such as MCS it is possible to attempt to assess bias by comparing the distributions of variables measured at prior waves. Table 9 does this for a selection of variables measured at wave 3 by the different outcome groups at MCS4. Clearly, this is a relative assessment of bias, i.e. how much bias there is at wave 4 compared with wave 3 and does not account for any bias existing at wave 3.

Table 11 shows that the results are mixed and differ between variables. Because the sample size for converted refusals is small, there are relatively few statistically significant differences between those initially interviewed and the converted refusals, and between converted and unconverted refusals. However, if the distribution for converted refusals is similar to the distribution for those initially interviewed and the distribution for initially interviewed is significantly different from unconverted refusals, this indicates that there is bias in the survey estimates of this variable and that the refusal conversion attempts are unlikely to have led to a reduction in bias. Conversely, if the distribution of converted refusals is similar to unconverted refusals and the distribution for initially interviewed is significantly different from unconverted refusals, this indicates that there is bias in the survey estimates for this variable and that the conversion attempts are likely to have led to a reduction in bias.

In relation to employment status, the proportion of main respondents in work is almost identical in families who were initially interviewed and the converted refusals and there is a statistically significant difference in the proportion of main respondents in work between both those initially interviewed and converted refusals compared with unconverted refusals and converted refusals and (which have a much lower proportion in work), indicating that refusal conversion has not led to a reduction in bias in the estimates for this variable. Looking at family type and voting behaviour, the proportion of married natural parents, lone natural parents and main respondents who voted at the last election is similar in those families who were converted refusals and unconverted refusals and there is a statistically significant difference in these categories between those initially interviewed and the unconverted refusals, indicating that refusal conversion has led to a reduction in bias in the estimates for these variables. In relation to tenure, both unconverted refusals and converted refusals are significantly less likely to be living in owner-occupied accommodation and significantly more likely to be living in rented accommodation than those initially interviewed strongly indicating that refusal conversion has led to reduction in bias in the estimates for this variable.

Overall, these results are encouraging as in three out of the four variables which show clear and statistically significant patterns it appears that refusal conversion has led to a reduction in bias in the survey estimates.

In relation to main respondent's general health and education level and cohort member's ethnicity, there are some statistically significant differences between those initially interviewed and unconverted refusals, indicating possible bias in the estimates for these variables, but it is not possible to draw conclusions about the impact of re-issuing as there is no clear pattern.

Burton et al. (2006) also considered the impact of refusal conversion in relation to some similar variables: employment status, housing tenure, health and political preference. In relation to housing tenure, they also found that refusal conversion led to a reduction in bias. They found that refusal conversion led to a reduction in bias in relation to employment status, which we do not find. However, we use a binary indicator of whether the sample member is in work or not and they use a more detailed employment status variable which also distinguishes self-employed and retired. They draw the same conclusion we do in relation to health, i.e. no clear pattern. We find evidence of bias reduction in relation to voting behaviour and they do not find any evidence of this in relation to political preference, although again the variables used are not directly comparable. It should also be borne in mind that BHPS is a study of all households whereas MCS is a study of families with young children.

6. Conclusions

In this paper we have clearly shown that devoting additional field resources to converting refusals on the fourth wave of the MCS brought some benefits to the study in terms of an increased sample size and a lower refusal rate. Although for main and, in particular partners, the data obtained for converted refusals was less complete than for families who did not refuse initially, it was notable that the child data was no less complete. From a scientific perspective, this is an important finding as it means that crucially important data on key child development indicators – such as cognitive development and obesity – was obtained for almost 150 additional children as a result of refusal conversion. From a survey operational perspective, this is an encouraging finding as it indicates that the cohort children in families who refused initially are no less willing to take part in the study than children in families who did not refuse initially. This implies that the initial reluctance of the family to take part may be a reflection of their parents' (and in particular their fathers') unwillingness to participate rather than any reluctance among cohort members themselves. One implication of this is that, as the cohort children get older, efforts to persuade parents to let their children take part, even if they don't want to themselves, may prove fruitful. Despite the small sample size for converted refusals, there also appeared to be some evidence that re-issuing refusals may have led to a reduction in bias in the survey estimates on some key variables.

We have also made two important contributions to the methodological evidence on refusal conversion. Firstly, by providing evidence from an experiment involving random assignment to treatment and control groups, we have eliminated the effects of the non-random selection of sample members into refusal conversion treatments and addressed a limitation in the existing research in this area, which was largely based on observational studies. Secondly, we have also made a significant contribution to improving knowledge in an area of methodological research in which there is relatively little existing evidence, i.e. the effectiveness of refusal conversion strategies in longitudinal surveys.

However, the question remains whether the benefits of refusal conversion were worthwhile, i.e. is this a cost-effective intervention? It is, of course, difficult to place a monetary value on a successful interview. Nevertheless, it is important to attempt to make some assessment of the cost-effectiveness of converting refusals in order that informed decisions can be taken about how longitudinal surveys should most effectively use their limited resources. Data from the fieldwork agency show that the marginal cost of achieving an interview was over three times as high for converted refusals as it was for families who did not refuse initially. The additional marginal cost of the leaflet was trivial compared with the cost of re-issuing. However, given that the leaflet had no effect on conversion rates either independently or in combination with the re-issuing, it seems that the additional cost of the leaflet is not justified.

Although the marginal cost of an interview achieved through a refusal conversion appears high relative to the cost of an interview where conversion was not required, it should also be borne in mind that this cost was incurred for a very small proportion of the achieved sample and a small number of cases overall. So, in absolute terms, the additional total cost of the extra interviews achieved via refusal conversion is small compared with the fieldwork costs for the study as a whole. More generally, targeted interventions such as refusal conversion are likely to be more cost-effective than universal ones, such as incentives given to all respondents, as resources are not 'wasted' on sample members who would participate without this intervention.

In addition, on a longitudinal study such as MCS, the question of whether this is worthwhile cannot be answered definitively until future waves reveal whether these converted refusals continue to participate. Longitudinal studies are concerned about response over the long-term rather than at only one point in time and if these converted refusals continue to provide data, paying this premium for keeping them in the study at this wave might well be considered a price worth paying.

More generally, although refusal conversion has been shown to be an effective way of reducing the refusal rate, refusal remains a major source of non-response and a serious concern for studies like MCS. In the context of longitudinal surveys, there is clearly scope for further tailoring of fieldwork approaches and targeting of fieldwork resources using information from prior sweeps. For example, although the leaflet used in this experiment was not effective, this may have been because although it was tailored to reasons for refusal reported on the survey as a whole, it was not tailored to the reasons for refusal reported by individual families. The development of personalised approaches can be challenging (and potentially costly) on large-scale surveys but this is an area in which there may be scope for further innovation in survey practice.

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Appendix A: Leaflet used in the experiment

Your child is one of the children who have been chosen to represent their generation.

Child of the New Century is an extremely important research project that is providing unique understanding of what it is like to be a child growing up in the 21st century.

What do we learn from these studies?

Similar studies of previous generations have produced many significant findings. These findings have helped parents, professionals and the Government to understand health, education and social issues better. Studies like this one can help develop services that meet the needs of children and their families.

What have we found out so far?

Child of the New Century has already generated important new knowledge showing how their experiences differ from children of previous generations. For example, only one baby out of every 50 was born at home compared with half in 1946, a third in 1958 and one in eight in 1970.

As your child gets older we will build up a detailed picture of how their generation develops from a very young age, through their school years and into adulthood.

Why would we like you to take part again?

The study depends on you continuing to take part. It is really important that as many families as possible take part again and again so that the results give a true picture of the different experiences of this generation of children.

What if nothing has changed?

It is crucial that we hear about the things that haven't changed in your family's life as well as the changes. If we only interviewed families whose circumstances had changed or asked about things that were different, the results would be misleading.

Why do we ask so many questions?

This is because we are trying to produce as detailed a picture as possible of your child's and your family's life. We want to be able to look across all areas of your child's life to see how they relate to each other.

What if you don't want to answer one of the questions?

Even if you don't wish to answer all of the questions, we would still like you to take part. If you don't want to answer a particular question or discuss a certain topic, just tell the interviewer and they will move on to the next question or topic.

Can you choose when to be interviewed?

Of course! We know that families with children are very busy and that your time is precious. The interviewer will be happy to fit the interview around your family's schedule. If you wish, they can make more than one visit to your family at different times and on different days.

Can you choose which parts of the survey to complete?

Yes. If you don't want to do some parts of the survey or don't have time to complete it all, we would still like you to do some of the survey. Just tell the interviewer what you would like to do.

Can you request a different interviewer?

If you would like to request a different interviewer, please call the study Freephone number 0800 783 5890.

I really appreciate your help with this study and the time that you and your family devote to this important research. More information and further findings from the study can be found at www.childnc.net

I hope that you will be willing to take part again and that you enjoy doing so.
If you do wish to take part in the Age 7 Survey, please let us know by calling the study Freephone number: 0800 783 5890.
You can also call us if you have any questions about the study.

Heather Joshi

Professor Heather Joshi OBE
Study Director

NatCen
National Centre for Social Research

Appendix B: Stratification and background variables by experimental group for the issued sample in Great Britain for MCS3

	Group A: Intensive re-issue, leaflet	Group B: Standard re-issue, leaflet	Group C: Intensive re-issue, no leaflet	Group D: Standard re-issue, no leaflet
Experimental Group	3,824 (24.9%)	3,811 (24.8%)	3,840 (25.0%)	3,875 (25.1%)
Original Stratum				
England-advantaged	1085 (28.4%)	1083 (28.4%)	1088 (28.3%)	1095 (28.3%)
England-disadvantaged	1050 (27.5%)	1054 (27.7%)	1069 (27.8%)	1084 (28.0%)
England-ethnic	562 (14.7%)	557 (14.6%)	559 (14.6%)	567 (14.6%)
Wales-advantaged	187 (4.9%)	181 (4.7%)	179 (4.7%)	188 (4.9%)
Wales-disadvantaged	429 (11.2%)	425 (11.2%)	426 (11.1%)	420 (10.8%)
Scotland-advantaged	245 (6.4%)	245 (6.4%)	248 (6.5%)	254 (6.6%)
Scotland-disadvantaged	262 (6.9%)	260 (6.8%)	264 (6.9%)	262 (6.8%)
Northern Ireland-advantaged [1]	1 (0.0%)	2 (0.1%)	4 (0.1%)	2 (0.1%)
Northern Ireland-disadvantaged [1]	3 (0.1%	4 (0.1%)	3 (0.1%)	3 (0.1%)
Refusal at MCS3	162 (4.2%)	159 (4.2%)	164 (4.3%)	185 (4.8%)
MCS4 Survey Wave	162 (4.2%)	159 (4.2%)	164 (4.3%)	185 (4.8%)
England, W1	1343 (35.1%)	1337 (35.1%)	1349 (35.1%)	1376 (35.5%)
England, W2	1363 (35.6%)	1369 (35.9%)	1372 (35.7%)	1383 (35.7%)
Scotland, W1	123 (3.2%)	134 (3.5%)	129 (3.4%)	118 (3.3%)
Scotland, W2	384 (10%)	367 (9.6%)	383 (10.0%)	394 (10.2%)
Wales, W1	314 (8.2%)	305 (8.0%)	307 (8.0%)	305 (7.9%)
Wales, W2	294 (7.7%)	294 (7.7%)	297 (7.7%)	297 (7.7%)
Northern Ireland, W1 [2]	2 (0.1%)	5 (0.1%)	2 (0.1%)	1 (0.0%)
Northern Ireland, W2 [2]	1 (0.0%)	0 (0.0%)	1 (0.0%)	1 (0.0%)

	Group A: Intensive re-issue, leaflet	Group B: Standard re-issue, leaflet	Group C: Intensive re-issue, no leaflet	Group D: Standard re-issue, no leaflet
Lone natural mother at baseline	655 (17.2%)	650 (17.1%)	648 (16.9%)	672 (17.4%)
Cohort child from non-white ethnic group	748 (19.7%)	748 (19.8%)	735 (19.3%)	737 (19.1%)
Mother has no educational qualifications	564 (15.4%)	571 (15.6%)	594 (16.2%)	594 (16.1%)

[1] These are families who were sampled in Northern Ireland and subsequently moved to Great Britain

[2] These are families who were living in Northern Ireland at the time fieldwork waves were assigned but subsequently moved to Great Britain