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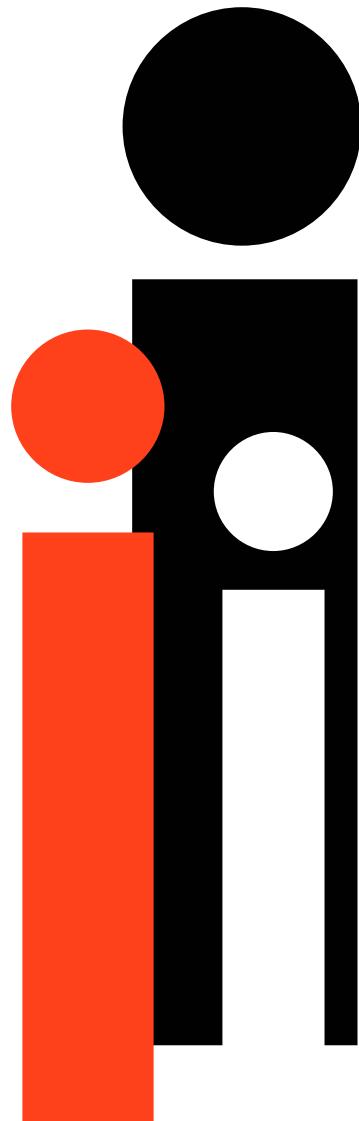
The Use of Respondent Incentives on Longitudinal Surveys

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Non-Technical Summary

People selected to take part in social surveys are often paid a small amount of money or offered a small gift of some kind. The reasons for providing this payment or gift may be to encourage people to co-operate with the survey, or to thank them for taking part. These payments or gifts are typically referred to as “incentives”, suggesting perhaps that the emphasis is on encouragement.

Researchers additionally hope that providing an incentive will not merely increase the proportion of selected people who agree to take part in the survey, but will increase participation amongst particular groups who may otherwise be less likely to take part. This is referred to as reducing “non-response bias”. In other words, it is helping to make the sample more representative.

This paper is concerned with the use of incentives on longitudinal surveys, i.e. surveys where the intention is to return to interview the same people on several occasions. We review current practice with respect to the use of incentives on longitudinal surveys and we summarise what is known about the effects that incentives have on co-operation and on non-response bias for such surveys.

The Use of Respondent Incentives on Longitudinal Surveys

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Abstract

We review current practice concerning the use of respondent incentives on longitudinal surveys and we review experimental evidence concerning the effects of incentives on longitudinal surveys, particularly on cumulative response rates and on sample composition. To provide context, we also briefly review the research literature regarding the effects of incentives on cross-sectional surveys and discuss the extent to which findings from such studies are likely to carry over to longitudinal surveys. We identify some aspects of longitudinal surveys that may be unique in terms of how incentives operate.

Keywords: Panel attrition, survey costs, survey nonresponse

JEL Codes: C81, C83

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1 INTRODUCTION

Incentives in the form of a gift or money are often given to survey respondents in the hope that this might increase response rates and possibly also reduce non-response bias. Incentives can also provide a way of thanking respondents for taking part and showing appreciation for the time the respondent has given to the survey. Survey organisations are keen to make interviewers feel confident as they approach sample members for an interview and having something which interviewers can give to respondents, even if a small token of some kind, helps with this through a process of reciprocity and social interaction between the interviewer and respondent. The aim in using the incentive is to encourage respondents to see their participation as being important and, as a result, increase response propensities and enhance the quality of the data collected.

Many studies have been carried out into the effects of respondent incentives, though most are based on cross-sectional surveys (see section 2 below). These studies show that both the form of the incentive, gift or money, and the way in which it is delivered to the respondent has a measurable impact on response rates. A monetary incentive sent to the respondent in advance of the interview has the greatest effect on increasing response, regardless of the amount of money involved. This type of unconditional incentive is thought to operate through a process of social reciprocity: the respondent perceives that they have received something unconditionally on trust and so reciprocate in kind by taking part in the research. Published studies present a mixed picture regarding the extent to which the increase in response rate may or may not be associated with a reduction in non-response bias. Additionally, some of the literature suggests an improvement in data quality from respondents who are given an incentive, though again some studies conclude the opposite. It is generally felt that incentives are more appropriate the greater the burden to respondents of taking part. Longitudinal surveys certainly constitute high burden surveys, but there is little guidance on how and when incentives should be employed on longitudinal surveys.

This paper reviews the use made of incentives on longitudinal surveys, describing common practices and the rationale for these practices. We attempt to identify the features of longitudinal surveys that are unique and the features that they share with cross-sectional surveys in terms of motivations and opportunities for the use of incentives and possible effects of incentives. In section 2 we review briefly what is known about the effect of incentives on cross-sectional surveys. Section 3 then sets out issues in the use of incentives

that are specific to longitudinal surveys. Section 4 summarises current practice on longitudinal surveys in different countries and with differing designs and section 5 reports experimental evidence on the effect of changing the way in which incentives are used midway in a longitudinal survey. This evidence includes the findings from three experimental studies in the UK carried out on the British Election Panel Survey (BEPS), the British Household Panel Survey (BHPS) and the England and Wales Youth Cohort Study (EWYCS). Each experiment addressed a different type of change in incentive administration.

The BEPS experiment involved introducing an incentive for the first time at wave 6. Three experimental groups were used at both waves 6 and 7, consisting of a zero incentive and two different values of unconditional incentive. The BHPS experiment was carried out at wave 14. BHPS respondents had always received a gift token as an incentive and since wave 6 this had been offered unconditionally in advance of the interview to the majority of respondents. The wave 14 experiment was designed to assess the effect on response of increasing the level of the incentive offered from £7 to £10 for established panel members, many of whom have co-operated with the survey for thirteen years. The EWYCS experiment concerned the introduction of incentives at wave 2 of cohort 10 in the context of a mixed mode design, with the nature of the incentive changing for some groups at wave 3, but repeated unchanged for other groups.

2 RESPONDENT INCENTIVES ON CROSS-SECTIONAL SURVEYS

A number of messages emerge from existing research into the effects of providing respondent incentives. Findings regarding effects on response rate are generally consistent, while studies addressing effects on non-response bias or on data quality are less numerous and provide somewhat more mixed messages. Overall, the evidence shows that incentives are effective in increasing response rates (Singer and Kulka, 2000) but the effect of incentives varies depending on the mode of data collection, the type of incentive used, and the delivery method used (Singer, 2002). Some surveys use a monetary incentive, others give respondents a small gift, and some offer entry into a lottery or prize draw. The way in which the incentive is delivered to the respondent also varies. Some are paid or given unconditionally in advance while others are promised contingent on response

2.1 Effects of Incentives on Response Rates on Mail Surveys

Church (1993) found that for mail surveys monetary payments are more effective than gifts, pre-paid unconditional monetary incentives have a greater effect on increasing response rates than those contingent on response, and that response rates increase as the monetary amount paid increases. Church found an average increase in response rates to mail surveys of 19.1 percentage points where a pre-paid monetary incentive was used compared to 4.5 percentage points where the incentive was conditional on response. In addition, the average increase where a gift was sent unconditionally in advance was 7.9 percentage points. James and Bolstein (1992) found similar results on a mail survey where response rates increased as the incentive amount increased. Sending the payment in advance was also more effective as amounts of \$1 or \$5 in cash or a \$5, \$10, \$20, or \$40 cheque sent unconditionally in advance had a greater effect on response than an offer of \$50 once the questionnaire was returned. Couper et al (2005) also found that a cash incentive in a mail survey yielded higher response than a gift in kind. The evidence on the use of lotteries for mail surveys is mixed with some studies reporting a positive effect on response rates (McCool, 1991; Balakrishnan et al 1992; Kim, Lee and Whang, 1996) and others finding no effect (Wariner et al, 1996).

2.2 Effects of Incentives on Response Rates on Interviewer-Administered Surveys

Singer (1999) analyses the results of 39 experiments with incentives on face to face and telephone surveys, concluding qualitatively similar effects on response rates to those found by Church for mail surveys. However, Singer found that the percentage increase in response rates on interviewer conducted surveys was somewhat smaller than those found on mail surveys. On average, each dollar of incentive paid produced about one third of a percentage point difference in response between incentive and non-incentive groups. Money was more effective than a gift and a prepaid incentive resulted in a significantly higher response rate than a conditional incentive. Singer concluded that for surveys conducted by interviewers there is no evidence that the effect of incentives differs between telephone and face-to-face interviews. A method using (unconditional) promissory notes given by the interviewer to the respondent at the point of interview has also been found to increase response rates. Lynn et al (1998) found that the promissory note increased response by 7.3%, from 56.0% for the non-incentive group to 63.3% for the promissory note group.

Some studies report an interaction between the burden of the interview and response rates for incentive and non-incentive groups (Singer, 1999). As a survey becomes more burdensome, the difference in response rates increases between those paid an incentive and those with no incentive payment. Similar effects were found on an experiment where respondents were offered a monetary incentive for completing a time use diary, typically a high burden type of survey (Lynn and Sturgis, 1997). The leverage-saliency theory proposed by Groves et al (2000) might be expected to predict that incentives will have greater leverage where the salience of the research for respondents is low. This appears to be supported by the evidence. For a survey with low response rates and where the saliency of the research to the respondent is low, the effect of the incentive is likely to be greater than for a survey with high response rates and high saliency (Baumgartner and Rathbun, 1997; Groves et al, 2000; Shettle and Mooney, 1999). There is also some evidence that incentives work primarily by reducing refusals and have little effect on non-contact rates (Shettle and Mooney, 1999; Singer et al, 2000).

2.3 Effects of Incentives on Sample Composition and Bias

It is of interest to researchers to know whether incentives are effective in encouraging groups who are typically under-represented in surveys to respond (such as those on low incomes, ethnic minority groups and those with low levels of education), or whether the additional respondents are similar to the ones who would respond anyway, in which case incentives have no beneficial effect on non-response bias. Couper et al (2006) examined the effect of incentives on sample composition and response distributions and found that a cash incentive was more likely to increase response than a gift in kind among those with low education levels, single people and those who were not in paid employment. Singer et al (2000) found that on a RDD survey a \$5 incentive was more likely to increase response among those with low education levels. An earlier review by Singer et al (1999) had found that in experiments on interviewer-administered surveys, three studies had indicated that incentives may be successful in boosting response particularly amongst low-response groups, while five studies indicated no significant differences between demographic groups in the effects of incentives and one study showed mixed results. A recent study on the German Family Panel pilot (Brüderl et al 2008) suggests that incentives may reduce non-response bias over three waves.

2.4 Effects of Incentives on Data Quality

Couper et al (2006) found that the higher response rates produced by the cash and non-cash incentive did not translate into lower data quality and there was no evidence of differential measurement errors in the responses for each group. Concerns about reducing data quality through the use of incentives have also been addressed by Singer et al (1999) who concluded that incentives do not appear to adversely affect data quality as measured by the levels of item non-response or the effort expended in the interview measured by the number of words given to verbatim items. On a survey of social attitudes, Tzamourani and Lynn (2000) found that incentives made no difference to the level of item non-response. Willimack et al (1995) too found no significant association between the use of a prepaid non-monetary incentive and data quality. Other studies have found that item non-response is reduced when incentives of any kind are used (Singer et al, 2000). James and Bolstein (1990) found that respondent effort increased with the value of the incentive in a mail survey.

2.5 Summary: Effects of Incentives

The cross-sectional evidence shows that incentives are effective in increasing response rates, though the extent varies by survey mode and the type of incentive strategy used. It also seems that incentives can improve sample composition, by disproportionately boosting response amongst groups with a relatively low baseline response propensity. There is little evidence of incentives having a significant impact on data quality.

3 RESPONDENT INCENTIVES ON LONGITUDINAL SURVEYS

Incentives are particularly likely to be used on longitudinal surveys, due to the inherently burdensome nature of the survey design and the particular value to the researcher of encouraging repeated co-operation. The burden is primarily related to the request for regular participation over a period of time, but is accentuated when the survey involves certain other features which are common in longitudinal surveys. These include long or complex questionnaires, sensitive subject matter, interviews with more than one member of a household, and additional measurements such as anthropometric measures or psychological or cognitive tests. Incentives are therefore used to show respondents that the survey organisation recognises the level of burden which is being imposed and wants to thank

respondents for their participation. An issue is that payment of any kind may raise the expectations of respondents who will, in future surveys, expect some payment or incentive (Singer et al, 1998; Singer et al 1999). Certainly, once the decision has been taken to use incentives in a longitudinal survey it may be difficult to withdraw them without having an adverse effect on response rates, something which is discussed further in the sections which follow. The initial decision about the use of incentives for a particular longitudinal survey has long term consequences for the survey both financially in terms of cost and the expectations of respondents.

Longitudinal surveys differ from cross-sectional surveys in terms of the use of incentives in two ways. First, the *administration* of incentives (type, value, method, etc) to each sample member can be different at different waves. This leads to a multitude of potential incentive regimes, each consisting of a combination of treatments over waves. Second, the *effects* of incentives may be more complex and may need to be evaluated differently. For example, effects on response rates or on non-response bias may be temporary, constant, delayed or cumulative.

A longitudinal survey may choose to change the *value* of incentives offered between waves. The value may be increased, for example to reflect increases in the cost of living or to recognise the increasing value to the researcher of continuing participation, or it may be decreased, perhaps because the survey budget is reduced. Similarly, a survey might choose to vary the *form* the incentive takes, switching from monetary to a gift or lottery or *vice versa*. There is little or no evidence on the relative effectiveness of possible combinations over waves.

Another area where there is limited knowledge is on the effect of *introducing* an incentive for the first time on a longitudinal survey which has previously not used them. While we might expect that the highest levels of attrition will occur in the early waves of a survey, the introduction of an incentive after one or more waves have been carried out may have a positive effect on cementing the loyalty of sample members for later waves of the survey (Laurie, 2005). Conversely, introducing an incentive may have little effect as the attrition already suffered on the survey may have left a sample which is essentially fairly co-operative so responds in a limited way to an incentive. The effects of *ceasing* to provide an incentive for the first time where respondents have previously received them, so have an expectation of continued receipt, are largely unknown even though some studies suggest that these may not be significant (Singer, 1999; Lengacher et al 1995). It is likely that the expectations of

respondents would play a key role, but there is limited evidence of how these expectations play out in terms of respondent behaviour in a longitudinal survey.

The longitudinal design, where detailed information is known about respondents' previous response history and characteristics, should be an ideal vehicle for the *tailoring* of incentive strategies. Indicators which may be associated with later attrition, such as the level of item non-response at a previous wave (Lynn et al, 2005; Burton et al, 1999; Schrapler, 2003) and interviewer assessments of respondent co-operation, may provide an opportunity to target resources at respondents who have a higher risk of dropping out of the survey. In order to target incentives efficiently, information on the most effective alternatives is required. For example, differing incentive strategies may produce differing responses from sub-groups within the population and changes in response behaviour as a result of using an incentive may vary for different sub-groups over several waves of data collection.

There may also be a role for preference questions which allow the respondent to choose the form their incentive takes, choosing for example between a cash payment, a gift, or making a donation to charity. Lengacher et al (1995) found that charitable giving tends to increase subsequent wave response rates amongst those who are already co-operative respondents but not amongst those who were initially reluctant to take part at the previous wave and were persuaded to do so after a first refusal. They concluded that charitable giving can be viewed as a proxy for altruistic activities more generally, including participation in social surveys. In contrast, Tzamourani (2000) found that offering to make a payment to charity had no beneficial effect on response rates. Providing respondents with a choice of type of incentive may allow the respondent to tailor their incentive to fit their own preferences in terms of altruism or individual benefit but the likely effects on response rates and bias are unknown.

In general, the use of differential incentives where some respondents receive more than others is avoided by most longitudinal surveys, or at least restricted to situations in which different sample members have different response tasks. The reasons for this are several. The first issue is an ethical one of parity and fairness. Surveys requiring ethical approval will be discouraged from using differential incentives and fieldwork agencies and interviewers may not be willing to implement these (see Lessof, in press). In addition, little is known about the longer term effects of tailoring strategies. In particular, a design which offers higher incentives to non-cooperative respondents is effectively rewarding them for failing to respond. As such, the payments may be seen as inequitable by co-operative respondents,

who, if they become aware of these payments, will see them as being unfair and may refuse to co-operate in future surveys (Groves et al 1997; Singer et al 1999).

A potential problem in implementing differential incentives arises on surveys, such as household panel surveys, in which multiple household members take part. It would be difficult for example, to offer one household member a higher amount than another. Indeed, the design of most household panels leads to the sample including relatives in different households, so even with a consistent incentive treatment within households, sample members receiving different incentives may discuss what they have received. The final problem is raising respondent expectations for the future. If a respondent receives a higher amount at one year of the survey it is reasonable that they will expect this amount the following year and if they do not receive it, may drop out of the survey. There is some evidence that paying higher incentives as part of a refusal conversion programme does not deliver higher participation rates at the following year of the survey (Lengacher et al, 1995). These strategies may therefore have a beneficial effect which is only temporary and may create a longer term problem for the survey organisation which must choose either to continue to pay at the higher level or risk losing the respondent.

Despite this reticence to using differential incentives, many surveys do have some mechanisms in place for varying what respondents receive. This is generally done by providing additional payments for specific circumstances such as particular types of outcome or respondent behaviour, or through the provision of small gifts or fees in kind where the interviewer has some discretion in how best to handle particular cases (Martin et al, 2001). One off payments or 'end game' payment strategies to increase response from the least co-operative sample members have also been used (Juster and Suzman, 1995). However, little is known about how successful these are in delivering long term commitment to the study.

Additional payments for what the survey organisation sees as extra burden are also fairly common. Where additional modules are included for specific types of respondents or the interview length is longer than usual, survey organisations may pay more in recognition of this – even though there is no clear evidence of a relationship between interview length or perceptions of burden on subsequent wave response (Hill and Willis, 2001; Martin et al, 2001). Survey length and complexity has been identified by some studies as being a complaint made by reluctant respondents while a very short questionnaire may reduce response rates on a panel survey (Lynn et al, 2005). Nonetheless, longitudinal survey practitioners are typically concerned that a longer or more burdensome interview may have a greater effect on response at the following rather than current year of the survey. In this

sense, higher incentive payments for the current interview are used as something of an insurance policy against higher attrition at the following contact, even though the effects on subsequent response rates are not clear.

4 CURRENT PRACTICE ON LONGITUDINAL SURVEYS

Longitudinal surveys vary in their use of incentives for respondents but the use of incentives is generally part of a wider package of measures to encourage participation. The measures typically involve the use of letters and short reports of findings designed to inform and motivate respondents, procedures for tracing respondents who move address and methods for respondents to let the survey organisation know their new address, providing flexibility and adapting to the respondent's constraints regarding when and where interviews takes place, mixed mode data collection approaches, and individual personal contacts in response to a bereavement, the birth of a child or a birthday. In the absence of experimental evidence, it is difficult to disentangle the effect of incentives from these other procedures, some of which may have significant impacts on response rates. Nonetheless, it is instructive to consider how longitudinal surveys are currently using incentives and how this process has changed over time for many long running surveys.

Some major longitudinal surveys such as the Canadian Survey of Labour and Income Dynamics (SLID), the Swiss Household Panel (SHP) and the British Birth Cohort Studies (NCDS, BCS70, Millennium Cohort) have never used a respondent incentive. Surveys which do not use incentives instead rely on appeals to respondents' sense of altruism. The SHP additionally uses collective motivational measures (media communication). In the 2007 wave the SHP gave a small gift to each person who had participated in each of the previous four waves of the survey – the first time that any incentive or reward had been offered on the survey (Zimmerman et al, 2003).

The form that incentives take varies across longitudinal surveys as do the amounts paid. Surveys may make a financial payment in cash, cheque or money order or by issuing a debit card with PIN number to respondents. Alternatively, they may use a cash equivalent such as a store or gift voucher, a lottery ticket or provide a small gift of some kind. Table 1 summarises current practice in the use of incentives on various longitudinal surveys.

Many surveys change the incentive treatment over time in an attempt to maximise longitudinal response rates. For long running panel surveys that opt to use financial incentives from the start, it is almost inevitable that the incentive amount will have to

increase over time in order to remain meaningful to respondents as the cost of living increases. Increases in the incentive to long serving sample members may also demonstrate the respondent's continued importance to the survey organisation and even relatively small increases may have some symbolic value in signifying this to respondents.

4.1 Panel Study of Income Dynamics (PSID)

The Panel Study of Income Dynamics (PSID) began in 1968 and initially paid respondents \$5 per interview, increasing this amount in fairly small incremental steps over the following years. By 1973 it had been raised to \$7.50, in 1981 it increased to \$10, in 1987 to \$12.50, in 1990 to \$15 and in 1995 to \$20. From 1997 the PSID has been a biennial survey rather than annual and when the interview increased substantially in length in 1999 (partly due to the introduction of an extensive child development supplement), the incentive was doubled to \$40 per interview and in 2005 had reached \$60 per interview. Despite the fact that the incentive is not a payment for time but a token of appreciation, these increases are recognition that in order for the incentive to be effective it must have some value to respondents relative to the current cost of living and also reflect the level of burden being imposed in terms of interview length. Current PSID practice is to pay an incentive of around \$1 per interview minute. A cheque is sent to respondents within a week of the interview being completed. In some cases a money order is sent instead, if the respondent is unable to cash a cheque. Additionally, small 'finders fees' are paid to family members who provide a new address for sample members who have moved. Also, respondents are paid \$10 for returning an address confirmation card which is mailed to them each year with details of the next wave of the survey.

Table 1 Summary of Incentive Use on Various Longitudinal Surveys (2005/2006)

Survey	Form of incentive	Amount or gift value	Conditionality and timing	Individual level	Household level bonus	Incentive varies for different groups/ households
Panel Study of Income Dynamics	Monetary (cheque or money order)	\$60	Mailed 1 week after interview	Yes	No	No
Survey of Income Program Participation	Monetary (cheque or cash via debit card with PIN)	\$40	Currently experimenting with unconditional in advance vs at discretion of interviewer/ Targeted at previous wave refusals only	Yes	No	Yes, depends on previous non-response
National Longitudinal Survey of Youth	Monetary (cash, cheque or money order) Plus gift in-kind e.g. meal/ pizza. Additional \$20 gift card in metropolitan areas.	\$40 base rate; \$60 or \$80 if respondent calls in to do interview. Gift in-kind up to \$20	Face to face interviews cash at point of interview Telephone interviews sent cheque post interview	Yes	No	Yes, depends on mode of data collection, whether respondent calls in or not, amount given in previous rounds, previous response history, and whether living in a metropolitan area
US Health and Retirement Survey	Monetary (cheque)	\$40 \$10 extra in 2006 for longer interview	Unconditional in advance	Yes	No	No
The National Longitudinal Survey of Children and Youth	Gift (age specific) e.g. 0-3 giraffe growth chart/ 4-5 colouring book or stickers/6+ book lights or key chains	\$2	Given to child at interview	Yes	No	Yes, given to child only, no incentive for parents
German Socio-Economic Panel	Lottery ticket Plus gifts	Euro 1.50 (lottery ticket) Euro 5-7 (gift)	Lottery ticket mailed post interview. Gift given at interview.	Yes	No	Yes (mothers receive an extra lottery ticket for child questionnaire/ new 16 year olds receive additional gifts)
Household Income and Labour Dynamics in Australia	Monetary (cheque)	\$25 per individual \$25 per fully co-operating household	Post interview (can take six weeks to process)	Yes	Yes	Yes, household bonus depends on full response
English Longitudinal Study of Ageing	Monetary (gift voucher)	£10	Mailed post interview	Yes	No	No
British Household Panel Survey	Monetary (gift voucher) Plus gift (diary/pens)	£10 (voucher) £1 (gift)	Unconditional in advance Gift given at interview	Yes	No	No

4.2 National Longitudinal Surveys of Youth (NLSY)

A similar progression of the incentive amount is observed on the National Longitudinal Survey of Youth (NLSY) in the US, which has used financial incentives for both the 1979 and more recent 1997 samples. On the 1979 sample the incentive payment increased from \$10 to \$20 by 1996. In 1998, as a reaction to response rate concerns, households were offered a bonus of \$100 to \$150 depending on the number of household members, which could be up to seven people. The survey team found that response rate continued to fall but judged that there would have been greater losses without this significant bonus. However, the subsequent round of interviewing suffered when the same incentive was not offered again, demonstrating the difficulties of raising respondent expectations in a longitudinal survey.

From 2002, the NLSY79 employed an alternative strategy where the base rate for an interview rose to \$40 but they attempted to get the more co-operative 'easy' cases at low cost by asking the respondent to call in to do the interview by telephone. Respondents are paid up to \$80 if they call in themselves to do the interview, resulting in reduced costs for these cases. Respondents who are called by the survey organisation receive a maximum of \$40. Telephone respondents receive their incentive as either cash or a cheque following the interview unless they specifically request a money order (which can be cashed at most grocery stores, the post office, or a local bank). Where the interview is conducted face-to-face the interviewer gives cash to the respondent at the end of the interview. Gifts in-kind and gift cards or food up to \$20 max for a household are also offered in some circumstances. For example the interviewer may take a pizza and soft drinks to a family where they are expecting to interview several people at the same visit.

The NLSY97 approach is similar to that of the earlier cohort. In rounds 5 and 6, the rate for all respondents was raised to \$20. In round 6, as on the NLSY79 sample, the option to complete the interview by telephone was also offered and resulted in an increase in the overall response rates, an increase which the survey team attribute to mode flexibility rather than the incentive level. In previous rounds of the NLSY97, telephone interviews comprised 5 – 7% of interviews completed compared to 15% at round 6 of the survey. In rounds 7, 8 and 9 (fielded in 2005) the respondent fee was maintained at \$20 per interview, but past round refusals or non-interviews received an extra \$5 per missed round. The justification for this was greater burden as the questionnaire collects event data back to the time of the previous interview. Initial analyses suggest that this is most effective with respondents who have been

out of the survey for the shortest amount of time. In round 9, an additional \$20 gift card was offered to respondents in major metropolitan areas where the cost of living is higher. Offering an alternative site, rather than the home, for the interview combined with the offer of a free meal is also a strategy that the NLSY have found to be effective. This is done by purchasing a light meal and meeting at a coffee house, chain restaurant, or fast food service and doing the interview over a lunch hour. While the "fee" is in-kind, respondents have been found to be quite receptive to this approach.

4.3 British Household Panel Survey (BHPS)

The British Household Panel Survey (BHPS) conducts an annual face-to-face interview and provides an incentive in the form of a store gift voucher rather than cash or cheque. From 1991 (wave 1) to 1995 this was £5 per interview and was raised to £7 per interview from 1996 (wave 6). In 1994, children aged 11-15 were interviewed for the first time and received £3 for this interview. In 1996 this was raised to £4 per youth interview. In 2004 (wave 14) the BHPS conducted a split-sample experiment on increasing the incentive to £10 (£5 for the youth interview). The results of this experiment are reported in section 5.2 below. From 2005 all respondents have received £10 per interview (£5 for the youth interview). The BHPS differs from other studies in that it sends the incentive in advance to respondents who were interviewed at the previous wave. Interviewers have spare vouchers for any new household members or respondents who were not interviewed at the previous wave and hand these to them on conclusion of the interview. The BHPS also offers small gifts to respondents in addition to the gift voucher incentive. In the past these have included pens and diaries embossed with the survey logo and the diary has now become an annual feature which respondents have come to expect to receive. The cost of the diary is around £1 (GBP) per respondent and it is given to respondents by the interviewer when they visit. As with the NLSY, interviewers have some leeway to offer small additional gifts such as a bunch of flowers if they know there has been a bereavement, a box of chocolates for a birthday or a small toy for a new child and so on. Interviewers also have the option to arrange to meet the respondent in a location other than the home if required.

4.4 The Health and Retirement Survey (HRS) and the English Longitudinal Study of Ageing (ELSA)

The US Health and Retirement Survey (HRS) which conducts a bi-annual survey with a sample of the over 50s, used an ‘end game’ strategy to boost response at their first wave in 1992 (Juster and Suzman, 1995). Reluctant respondents were offered a large financial bonus of \$100 for participation and asked for an immediate yes or no decision on whether to take part, a strategy which increased the initial HRS response rate by around 4 percentage points. From 1992 to 2002 the standard amount respondents received on the HRS was \$20, sent unconditionally in advance. In 2004 this increased to \$40. Each sample member in a household receives this by cheque in the advance mailing prior to the interview. In 2006, a sub-sample was asked to do an expanded face-to-face interview and for this each respondent was given \$50, reflecting what is seen as the additional burden of the interview length.

Another major survey in the UK, the English Longitudinal Study of Ageing (ELSA) began in 2002 and conducts a bi-annual interview. It has a similar design and aims to the HRS and gives a store gift voucher of £10 per individual interviewed. Unlike the HRS and the BHPS, these are given to the respondent by the interviewer at the point of interview rather than being sent out in advance of the interview.

4.5 The Survey of Income and Program Participation (SIPP)

The Survey of Income and Program Participation (SIPP), run by the US Bureau of the Census since 1984 is an example where the relationship between level of burden and paying an incentive does not apply. The SIPP is relatively burdensome for respondents as it conducts interviews at four month intervals with each panel recruited for up to twelve interviews over a 32 month period. In response to concerns about attrition, the 2001 panel was reduced to 9 waves over 3 years (Weinberg, 2002). SIPP has never used financial incentives for all sample members, even though it is a survey where experimentation with gifts and financial incentives have been carried out through its history. The results of these experiments are summarised in section 5 below. The most recent experiment was conducted on the 2001 panel and in contrast to other surveys where the norm is to give all sample members the same or similar levels of incentive, SIPP tested a discretionary payment against an unconditional incentive sent in advance to sample members who had been non-respondents at the previous

wave. Interviewers could offer an incentive of \$40, if they thought it would be effective, to up to one-tenth of their sample in each one year cycle.

4.6 The German Socio-Economic Panel Survey (SOEP)

The German Socio-Economic Panel Survey (SOEP), which has involved an annual interview with sample members since 1984, provides an incentive in the form of a ticket for the German national lottery rather than cash. Since the early 1990s various small gifts such as pens, bags, an umbrella and so on have been given to respondents in addition to the lottery ticket. The cost of the lottery ticket for SOEP is 1.50 Euros and the gifts cost an additional 5 to 7 Euros per respondent. As the main carer of young children (usually the mother) is asked an additional questionnaire about their children they are given an additional lottery ticket in recognition of the additional burden. The ticket is mailed to each individual respondent after the interview, a mailing which is combined with a follow up to collect details of any new addresses. The gift is given to respondents by the interviewer when they call. While no experimental data are available on the effect of these incentives, interviewers on the survey report that having something to give on the doorstep makes it harder for respondents to refuse and increases participation through reciprocity. The SOEP has also used additional incentives to encourage response amongst their youth cohorts entering the main panel and being interviewed for the first time, as recruiting these young panel members is critical for the longer term health of the survey.

4.7 The Household Income and Labour Dynamics in Australia (HILDA) survey

The Household Income and Labour Dynamics in Australia (HILDA) survey which began in 2001 has taken a slightly different approach in that they gave a financial incentive to the household rather than to individual respondents for the first four waves of the survey (2001 – 2004). Where all eligible household members were interviewed a cheque for Aus\$50 was sent out following the interview and for a partial household this was \$20. If in the follow-up fieldwork a partially co-operating household was converted into a fully co-operating household, a further \$30 was sent to the household. With this method of delivering the incentive it was assumed that the person named on the cheque would be responsible for giving each person their share but the extent to which this happened is unknown. From 2005, HILDA respondents have received \$25 per individual with a bonus of \$25 to the household

reference person (identified by the interviewer as the main person for communication about the survey) for a fully responding household. The rationale for this change was to encourage response at the individual level as well as gaining complete household co-operation. The previous incentive structure was also felt to be somewhat unfair to larger households who received the same amount as households with fewer members. In addition, there was some anecdotal information that the cheque sent to one person in the household was not always shared between household members but kept by one person. The new incentive structure aimed to ensure that no household was worse off than in previous years while removing some of the problems with the former system. On average it takes about six weeks following the interview for respondents to receive their cheque. As with other surveys, HILDA are not allowed to offer differential levels of incentives even though in practice the whole household completion bonus does this in return for full co-operation.

4.8 The National Longitudinal Survey of Children and Youth (NLSCY)

The National Longitudinal Survey of Children and Youth (NLSCY), a long-term child development study of Canadian children conducted by Statistics Canada and Social Development Canada (SDC) began in 1994. The sample consists of several longitudinal age cohorts selected between birth and 11 years of age, some of whom will be followed until they are 25 years old. The NLSCY is conducted face-to-face and gives small gifts depending on the age of the child(ren) in the sample. The 0-3 years group are given a giraffe growth chart, 4-5 year olds get stickers or colouring books, and older children are given small items such as key chains or book lights. The cost of the items is approximately two dollars per child and anecdotal comments from interviewers suggest that the children and parents generally appreciate the gifts.

4.9 Summary: Current Practice on Longitudinal Surveys

This review of current practice shows that longitudinal survey research teams are continually revising and rethinking their incentive structure in terms of the type of incentive offered, the value of the incentive and the delivery of the incentive. In many cases, these decisions are made on the basis of their own experience in the field, comments from interviewers and on the advice of other survey practitioners rather than being based on experimental evidence. While some surveys have conducted formal experiments to test the effects of changing

incentive structures, there is surprisingly little evidence about the longitudinal effects of these changes. This may partly be explained by longitudinal survey organisations being reluctant to carry out large scale experiments on their samples that may risk harming future response in some way. However, given the cost implications of increasing incentives or changing incentive structures and the potential benefits of tailoring, it is somewhat surprising more information is not available to guide survey practitioners in this area. In the following section we review the results of some experiments that have been carried out on longitudinal surveys and report results from three recent UK studies where incentives have been changed during the survey.

5 EXPERIMENTAL EVIDENCE ON LONGITUDINAL SURVEYS

The evidence on the use of incentives on longitudinal surveys suggests that incentives can be effective in reducing attrition over multiple waves of a survey, and that making changes through introducing an incentive, offering higher amounts and targeting of various kinds does affect response, though these effects depend on the survey context. In this section we first summarise the findings of some experiments on longitudinal surveys reported in the literature and then look in more detail at three recent experiments in the UK.

Effects of incentives on cumulative response: A question of interest is how incentives affect response not just at the wave of administration, but cumulatively over multiple waves. An experiment carried out on the SIPP in 1996 found that the effect of a \$20 pre-paid incentive in lowering non-response rates in the initial interview compared with both \$10 pre-payment and no incentive cases (James, 1997) was upheld over the first three waves of the survey. However, the difference in response between the \$10 and no incentive cases was not significant, suggesting that the amount paid does have an independent effect on response over and above whether or not the incentive is unconditional. Mack et al (1998) extended James' analysis to look at the effect of the incentive on response over six waves of the SIPP and also looked at the effect by the poverty status of the household, race and education level. They found that the \$20 incentive reduced household, individual and item non-response (gross wages) in the initial wave 1 interview and that household response rates remained higher across all six waves for the \$20 incentive group and the higher incentive was particularly effective for poor and Black households. The \$20 incentive also significantly increased response rates in low education households at all of waves 2 to 6 and in high education

households in waves 2 to 5. The \$10 incentive did not reduce cumulative non-response over the six waves.

The positive effect on subsequent wave response does not seem to be limited to monetary incentives paid at an earlier wave. The Swiss Household Panel Survey carried out an experiment where entry into a lottery was offered in conjunction with completion of a biographical questionnaire sent by mail at wave 2 of the survey (Scherpenzeel et al 2002). Response rates increased amongst the group offered the lottery incentive at wave 2 and this positive effect persisted over the following three waves of the survey. This suggests that the incentive effect was enduring rather than just delaying response for one or two waves.

Effects of differential incentives on response at subsequent waves: Practice on some surveys is to offer reluctant sample members increasingly higher incentives to secure co-operation. This is sometimes referred to as the “end game.” A particular concern on longitudinal surveys is the impact that such a strategy might have on response at subsequent waves.

The effect of the end game strategy during refusal conversion on the first wave of the Health and Retirement Study (1992) on later wave response has been examined by Lengacher et al (1995). Part of the concern at wave 2 of the survey was that respondents who had been paid up to \$100 as part of the end game strategy at wave 1 would have significantly lower response at wave 2 when offered the standard \$20 incentive due to an expectation effect. Lengacher et al looked at three groups in the sample 1) those interviewed at the previous round with no persuasion, 2) those who were reassigned to a different interviewer or sent a persuasion letter after an initial refusal at the previous round, and 3) those who were part of the non-response study and went through refusal conversion last time and were eventually interviewed. They found that those in groups 2 and 3 were significantly less likely to be interviewed again at wave 2 compared to the compliant group 1 respondents. However, there were no significant differences between the response rates of those in group 2 compared to those in group 3. This suggests that the large payment of \$100 at the first wave had no effect on increasing or decreasing later response relative to others who initially refused and were persuaded to take part by other means, nor did the large incentive at wave 1 induce an expectation that large incentives would be offered in later waves of the panel. Despite this, they did find an interaction effect between the level of enjoyment of the wave 1 interview and whether paid a large incentive. While for the sample as a whole, enjoyment of the first interview was associated with increased response propensity at wave 2, amongst those who

were paid the large incentive, those who enjoyed the interview were less likely to take part at wave 2. The large incentive appeared to cancel out the enjoyment effect, with the memory of the incentive being dominant in respondents' minds when asked to take part at wave 2.

The issue of whether unequal payments for reluctant respondents affects the co-operation of those who were not persuaded by offers of a larger incentive was examined in an experiment on the Detroit Area Study by Singer et al (1999). They tested the effect of disclosure of unequal incentives on later response. While respondents in the disclosure group perceived these unequal payments as being unfair, there was no significant difference in response rates to the survey one year later between the group who were told about the unequal payments and those who were not told. Singer et al concluded that the factors that motivated participation in the survey were not associated with whether or not the unequal payments had been disclosed to them. Nonetheless, they conclude that this area deserves further enquiry - given that maintaining the goodwill of survey respondents is paramount - as there may be unintended consequences of perceptions of inequity.

Targeting incentives based on response at previous waves: Longitudinal surveys offer considerable opportunities for targeting incentives based on the observed response behaviour of sample members at previous waves. Martin et al (2001) reported the results of an experiment on SIPP that targeted pre-paid incentives at non-responding households from a previous wave. They found that both a \$20 and \$40 incentive significantly improved conversion rates of people who had refused at an earlier wave compared to those who were offered no incentive.

The NLSY79 conducted an experiment in 2002 targeted on relatively co-operative 'easy' cases that were identified using contact data from previous waves. The aim was to establish whether it was possible to reduce data collection costs for these 'easy' respondents by asking them to call in to do the interview by telephone. The standard incentive was \$40 but sample members were offered a higher amount if they chose to phone in to do the interview. This amount was randomly varied between \$60 and \$80 to test whether the amount had an effect on response. Response rate was higher when the opportunity to phone in was offered and the cost per interview was lower. However, the \$20 difference in the incentive offered for calling in did not have any significant effect on response. Additionally, the most reluctant respondents were offered \$80 and the opportunity to call in, but this had no effect on response rate (Kymn Kochanek, personal communication).

A study by Rodgers (2002), based on an experiment carried out in 2000 on the Health and Retirement Survey, addressed the issue of targeting incentives based both on the previous response behaviour of sample members and on previous wave interview data that was felt likely to be predictive of future response propensity. Rodgers concluded that the greatest cost-benefit ratio would have been achieved by offering a higher incentive to households in which there was non-response at the previous wave. A similar conclusion was reached by a study based on the SIPP (Martin et al, 2001). The study reported by Rodgers used incentives of \$20, \$30 and \$50 across four strata defined by 1) having poor health at the previous round, 2) proxied respondents at the previous round, 3) non-interviewed respondents at the previous round, and 4) all other eligible households. Rodgers found response rates to be consistently and significantly higher for those paid \$50 for all groups apart from strata 2 with the response rates for the \$30 payment being intermediate between the \$20 and \$50 response rates. This suggests there may be a positive association between the amount of the incentive paid and response rates. Though the incentive was particularly effective for stratum 3), Rodgers concludes that the HRS protocol of not automatically dropping non-respondents from the survey at the following wave is a more significant factor in maintaining response rates than the incentive level.

Effects of incentives on fieldwork costs: James (1997) found that either a \$10 or \$20 incentive reduced the number of calls that interviewers needed to make on SIPP. Similarly, Rodgers (2002) found evidence that the number of calls interviewers had to make to achieve an interview were reduced with a \$50 incentive, relative to \$20 or \$30, leading to some reduction in overall fieldwork costs. Similar effects have also been found on cross-sectional surveys (Lynn et al, 1998). Finally, as reported above, the NLSY79 2002 study found that providing incentives to respondents to phone in at a time convenient to them produced an overall reduction in field costs.

5.1 Previous Experiments on UK Longitudinal Surveys

In the UK, two sets of experiments with incentives on longitudinal surveys have been carried out prior to our own experiment, which is reported in section 5.2 below.

An experiment was carried out at waves 7 and 8 of the 1992-1997 British Election Panel Survey (BEPS) (Lynn et al 1997). No incentives had been used on the previous six waves (four of which had been face-to-face, one telephone and one postal), but at wave 7 a random

subset received an unconditional £5 incentive. Of those who received the incentive at wave 7, a random half received the same incentive again at wave 8 while the other half received no incentive. Despite the relative maturity of the panel at the stage that the experiment was carried out, the incentive had a positive effect on wave 7 response rate. The proportion of sample members who responded at *both* waves 7 and 8 was slightly higher amongst those given an incentive at wave 7 only than those given no incentives, but was considerably higher again amongst those given an incentive at both waves. The proportion of wave 7 responders who responded at wave 8 did not differ between those given no incentives at either wave and those given an incentive only at wave 7. These findings suggest that incentive effects on response rate may be largely independent between waves, with little or no carry-over effect. Interestingly, the BEPS incentives experiment was interleaved with a separate experiment at wave 7 in which some sample members were told explicitly, for the first time, that they were part of a panel and could expect at least two more contacts. Incentive effects were slightly stronger amongst sample members who were *not* told they were in a panel.

The England and Wales Youth Cohort Study is a series of panel surveys of young persons aged 16 to 23. Each survey in the series samples an age-cohort of 16-year olds who are then sent questionnaires on between three and five occasions over the following few years. On cohort 10, which used a combination of postal and telephone methods, an experiment with incentives was carried out. No incentives were provided at wave 1 (Spring 2000). Wave 2 (late 2000), which involved both CATI and postal samples, incorporated an experiment whereby within each sample a random subset were sent a £5 voucher while the remainder received no incentive. Furthermore, in the postal sample the incentive treatment group was subdivided into two: incentives were provided either unconditionally (the incentive was sent with the initial mailing) or conditionally (the voucher was promised in the original mailing, but only sent on receipt of a completed questionnaire). At wave 3 (Spring 2002), all incentives were paid unconditionally and all ‘lower achievers’ (identified from responses to questions about qualifications from earlier waves) were approached in postal mode. At wave 4 (Spring 2003), all respondents were sent postal questionnaires. Although the mode treatment and the use of conditional or unconditional incentives changed across the waves, the allocation of individuals to either an incentive or control treatment was fixed across the waves.

Analysis of the data from this experiment (Jäckle and Lynn 2008) showed that the positive effects of incentives on response propensity remained constant across waves: there was no evidence that incentives became less effective at increasing response across waves,

for example because the respondent sample became less sensitive to incentives as potentially less committed sample members dropped out. The difference in cumulative response rates between the incentive and no-incentive groups increased over waves. The positive effects of incentives on response propensity had little effect on sample composition, in terms of a range of characteristics. Incentives reduced non-response bias only in term of variables that could in any case be corrected by weighting (Jäckle and Lynn 2004). The effect of incentives on response rate was stronger in postal mode than telephone, but in both modes there was little evidence of impact on attrition bias. Neither changes in the incentive offered nor changes in the mode of survey administration, conditional on the incentive offered, appeared to influence response propensity. That is, mode/incentive treatment at wave $t-1$ had no effect on response propensity at wave t , conditional on mode/incentive treatment at wave t .

5.2 British Household Panel Survey Incentive Experiment

Respondents on the BHPS have always received an incentive in the form of a store gift voucher which is sent to previously co-operating respondents in advance of each wave of fieldwork. From 1996 (wave 6) to 2003 (wave 13) the value of the voucher was £7 for sample members eligible for the full adult interview (aged 16 or older) and £4 for those eligible for the shorter youth interview (aged 11 to 15). In 2004 (wave 14) a split-sample experiment was implemented to test the effect of increasing the value of the incentive to £10 for adults and £5 for youths - a relatively small increase in value. All persons in a random half of the sample households received the increased amounts, while those in the other half received the standard amounts. The experimental sample is a national general population sample of Great Britain consisting of just over 5,000 households¹. The design is described in more detail in Laurie (2007).

Laurie (2007) reports initial findings. The wave 14 individual response rate conditional upon full response at wave 13 was 96% amongst sample members receiving £10 compared to 93% amongst those receiving £7 ($P < 0.01$), perhaps a surprisingly large effect considering that the sample is one of established co-operative sample members and that the difference in value is small. We speculate that increasing an amount to which sample members have become accustomed may have a beneficial psychological effect independent of the value of the increase.

¹ The sample for the experiment included respondents in the original 1991 BHPS sample only, not the extension samples added in Scotland and Wales in 1999 and in Northern Ireland in 2001.

Effects on reluctant respondents: The increased incentive appears to have had a greater effect on response for those who were eligible but not interviewed at wave 13 than for those who were successfully interviewed at wave 13. This suggests that an increased incentive may be an effective strategy for respondents with a lower baseline response propensity. Amongst adults who had refused an interview at wave 13 in households where at least one person was interviewed (known as “within-household refusals” – many of whom had persistently refused for a number of waves), the percentage providing a full interview at wave 14 was 13% with the £10 incentive and 6% with the £7 incentive.

The BHPS carries out telephone interviews with some sample members who cannot be persuaded to provide the full face-to-face interview. These telephone interviews take place as part of the refusal conversion process. Also, proxy interviews are accepted in certain circumstances where it is not possible to interview the sample member personally. However, the survey instrument for telephone and proxy interviews is a reduced version of the full instrument, so providing a response in one of these two forms is sub-optimal and can be considered a form of partial response. It is therefore desirable to find ways not only of increasing the proportion of the sample who respond at all, but also of increasing the proportion who provide a full face-to-face interview rather than a telephone or proxy interview. Amongst sample members who had provided a telephone interview at wave 13, the within-household refusal rate at wave 14 was just 3% for the £10 group compared to 10% for the £7 group. The percentage of wave 13 telephone respondents who were converted to a full interview at wave 14 was 19% for the £10 group compared to 13% for the £7 group.

Effects on sample entrants: At each wave young people turning 16 become eligible for a full adult interview and are effectively recruited into the main panel, making them an important group for the long term health of the panel survey. Of new 16 year olds who had completed a youth interview at wave 13 and were eligible for a full adult interview at wave 14 for the first time, the higher incentive increased the response rate significantly, from 91% to 95%. It should be noted that for 16 year olds the £10 incentive represented an increase over the previous year’s incentive of £6, compared to an increase of £3 for those receiving the £7 incentive. This may suggest that for long running panels where children of original sample members are recruited into the sample at a given age, some form of ‘golden handshake’ to welcome and encourage them into the main panel at that point may be an effective strategy to ensure as many as possible are recruited into the sample over the longer term. Whether the

effect will hold over time for this group can only be assessed as future waves of data are collected.

Effects on household response: The rate of household response (meaning that at least the household interview was completed) for eligible households at wave 14 was higher amongst the £10 households (94.4%) compared to the £7 households (92.4%, $P < 0.01$). The majority of this difference is accounted for by an increase in the proportion of fully co-operating households (where all eligible household members provided a full interview) from 74.4% with the £7 incentive to 77.7% with the £10 incentive. The proportion of whole-household refusals reduced from 2.2% with the £7 incentive to 1.0% with the £10 incentive. There was also evidence that the higher incentive increased the chances of tracing and interviewing households which had moved address since the previous interview. Amongst non-mover households, the percentage of households co-operating fully increased from 78.2% with the £7 incentive to 79.7% with the £10 incentive, but amongst households where the whole household had moved, the response rate increased by fully ten percentage points, from 58.7% to 68.7%. The increased incentive appeared to improve both location and co-operation rates amongst movers: the household refusal rate was 4% lower amongst the £10 group of mover households than the £7 group and the proportion of untraced addresses was also reduced by half. As losing sample members through geographical mobility is a significant source of avoidable attrition over time, an incentive strategy which encourages mover households to remain in the survey could have a positive effect on longitudinal response rates, reduce the levels of differential attrition and lessen the potential for bias in the data.

Effects on individual response: At an individual level, previous wave response and response history across the life of the survey are significant predictors of current wave response. The increased incentive improved the response rate both for regular responders and for intermittent responders, but in different ways. Amongst sample members who had provided a full interview at all thirteen previous waves, 97.3% of those receiving £10 gave a full interview, compared to 95.6% of those receiving £7. This was achieved mainly through a reduction in the proportion providing telephone interviews, from 2.5% with £7 to 1.2% with £10. Amongst sample members who had been a non-respondent to at least one previous wave, the proportion giving a full interview did not differ between the two treatment groups (59.0% with £7; 59.3% with £10) but the proportions giving either telephone or proxy interviews were higher with the £10 incentive (proxy 3.1% with £7, 4.0% with £10; telephone 8.3% with £7, 14.1% with £10). This corresponded to a reduction with the

increased incentive in both the household refusal rate (from 12.4% to 9.3%) and the household non-contact rate (from 5.6% to 2.6%).

Amongst all persons known to be eligible for a full interview at wave 14, the effect of the higher incentive on survey outcomes is summarised in Table 2, both for the sample as a whole and for a number of important demographic subgroups. Overall, the increased incentive improved the wave 14 response rate from 77.6% to 81.3% ($P < 0.001$). The effect was significant, and similar in size, for both men and women. The effect was largest amongst the age group with the lowest response rate, namely 16-24 year-olds, for whom the response rate increased from 69.5% with £7 to 77.6% with £10. The result of this was that with the £10 incentive the response rate amongst this group was not much lower than that amongst other age groups. It may be that this group is less likely to be motivated to participate for altruistic reasons. However, the only other age group for which the £10 incentive increased the response rate significantly was the group with the highest response rate, namely 55-64 year-olds. The overall effect of the higher incentive on disproportionate response propensities by age is therefore unclear.

For marital status, the most marked increases in response rate with the higher incentive were for the separated and the never married, two groups who can be difficult to contact and interview – though the difference only reaches statistical significance for the latter due to the small sample size of the former. The response rate for the never married was 70.8% with the £7 incentive and 79.3% with the £10 incentive ($P < 0.001$). The effect of the incentive also varied by employment status, appearing most marked for the unemployed, another group who typically have lower response rates than others. The observed response rates amongst unemployed sample members were 70.7% in the £7 group and 80.3% in the £10 group, though this difference was not significant due to the modest sample sizes. Sample members whose main activity was looking after the home or family also demonstrated a particularly large effect on response of the higher incentive, from 80.9% to 87.7%. Differences in the effect of the incentive by level of education were limited, but the only significant effect was observed amongst the group with the lowest response rates, those with a lower level ‘other’ qualification. Amongst this group, response rate increased from 83.6% with £7 to 91.3% with £10.

Table 3 presents predicted coefficients from three logistic regression models where the dependent variable is an indicator of individual wave 14 response (including proxy and telephone interviews). Model 1 includes only the main effects of age and gender plus incentive treatment. The estimates indicate that the main effect of the higher value incentive

is positive and significant, increasing the odds of responding by a factor of 1.10. The significant associations of gender and age with response are also apparent.

In model 2, interaction terms are introduced in order to test whether the effect of the incentive varies across age groups or between the sexes. However, the results provide no evidence of any such differential effects between the groups. The predicted odds ratios (increased incentive vs. unchanged incentive) are highest for 16-24 year-olds (1.17) and for 55-64 year-olds (1.19), but neither of these estimates are significantly different from the average estimate of 1.10.

In model 3, indicators of economic activity status and *de facto* marital status are introduced along with their interactions with the incentive treatment. There is a suggestion that the effect of the incentive may be stronger for those whose main activity is looking after the family or home ($P = 0.07$) and those with an “other” economic status ($P = 0.06$), relative to those in employment. The effect may be weaker for students ($P=0.09$). With respect to marital status, there is a suggestion that the effect of the incentive may be greater amongst the small group who are separated from partners ($P = 0.11$) and those who have never been married ($P = 0.11$). A six-category indicator of level of education was also tested, but dropped from the model as none of the interaction terms even approached significance, providing no evidence that the effect of the incentive level varies by the level of education of the sample member.

In summary, the overall message seems to be that increasing the value of the incentive was effective at improving response rate across all the demographic groups tested, to broadly similar degrees. Where there were differences, the effect tended to be stronger amongst groups with relatively low response rates, notably 16-24 year-olds, but this was not a clear pattern. In so far as this is true, increasing the value of an incentive during the course of a panel survey could help to reduce non-response bias, though we have not assessed this directly here. It should be noted that the apparently smaller number of significant associations in the logistic regression models, compared to the descriptive analysis of Table 2, could partly be a result of the definition of the dependent variable. Table 2 shows that a reduction in the proportion of non-responding cases is often associated with a reduction in the number of telephone or proxy responses too, whereas the modelling treats telephone and proxy responses in the same way as full responses.

Table 2. BHPS Wave 14 Response Outcomes by Demographic Characteristics and Incentive Treatment

Row %	Unchanged incentive (£7 voucher)				Increased incentive (£10 voucher)			
	Full Interview	Proxy/ phone	No interview	N	Full Interview	Proxy/ phone	No interview	N
All ***	77.6	5.2	17.2	5053	81.3	5.2	13.6	4888
Gender								
** Male	73.4	6.0	20.6	2443	77.6	6.0	16.4	2345
** Female	81.5	4.6	14.0	2610	84.7	4.4	11.0	2543
Age								
*** 16-24	69.5	5.4	25.2	766	77.6	4.9	17.6	722
25-34	73.7	6.2	20.1	889	78.2	4.3	17.5	887
35-44	80.5	4.9	14.6	1022	82.6	5.9	11.5	950
45-54	80.5	5.0	14.5	780	81.7	5.6	12.7	803
* 55-64	81.4	5.1	13.6	708	85.9	5.6	8.5	647
65 and over	79.5	5.0	15.5	888	82.0	4.8	13.2	879
Marital status								
** Married	85.0	6.2	8.9	2482	87.6	5.9	6.5	2355
Cohabiting	81.5	4.8	13.7	664	83.9	4.7	11.5	620
Widowed	83.3	4.2	12.5	287	84.7	3.8	11.6	320
Divorced	84.6	4.0	11.5	253	87.2	5.6	7.3	234
Separated	80.5	2.6	16.9	77	91.1	3.6	5.4	56
*** Never married	70.8	5.2	24.0	945	79.3	4.1	16.5	944
Employment status								
*** Employee	81.5	5.8	12.8	2487	85.4	5.2	9.4	2480
* Self-employed	80.7	5.7	13.6	352	84.9	7.5	7.5	332
Unemployed	70.7	4.9	24.4	164	80.3	2.3	17.4	132
Retired	87.2	4.2	8.6	897	87.4	4.5	8.1	866
* Family care	80.9	5.9	13.3	324	87.7	5.1	7.2	277
Full-time student	76.3	5.4	18.4	261	78.7	5.1	16.2	253
Long term sick/disabled	78.4	7.4	14.2	176	80.4	7.0	12.7	158
* Other	69.6	5.4	25.0	56	87.0	6.5	6.5	46
Highest qualification								
Degree /higher degree	89.9	3.8	6.4	581	93.3	1.9	4.8	579
Teach/nurse/other higher	90.6	2.8	6.7	1228	92.0	2.3	5.7	1253
A level or equivalent	85.5	2.6	11.9	498	86.8	3.8	9.4	498
GCSE/O level	85.5	3.8	10.6	781	89.1	2.1	8.8	718
** Other	83.6	1.9	14.4	360	91.3	1.9	6.9	321
None	84.1	3.6	12.3	835	86.8	3.9	9.4	779

*** P < 0.001; ** 0.001 < P < 0.01; * 0.01 < P < 0.05

Notes: The experiment is restricted to the original wave 1 BHPS sample and their descendants; the Scottish and Welsh boost samples and the Northern Ireland sample were excluded. The base is all sample members believed to be eligible for an interview at wave 14 and issued to field, regardless of response history at previous waves. "No interview" includes persons who were refusals or non-contacts within an otherwise co-operating household as well as those in non-responding households. The indicators of marital status, employment status and highest qualification are taken from the most recent interview data available, within the previous six years: for 84.6% of cases these indicators are from wave 13, 4.2% from wave 12, 1.8% from wave 11, 1.3% from wave 10, 0.8% from wave 9 and 0.5% from wave 8. 6.8% of cases had not completed an interview in the previous six years and are excluded from the analysis by these three variables.

Table 3. Logistic Regression Predicting Response at Wave 14

	Model 1		Model 2		Model 3	
	Odds ratio	S.E.	Odds ratio	S.E.	Odds ratio	S.E.
Higher value incentive (ref lower value)	1.099***	0.021	1.098*	0.053	1.061	0.054
Female (ref male)	1.586***	0.090	1.589	0.511	1.624	0.554
16-24 years	0.551***	0.051	0.336*	0.176	0.304*	0.189
25-34 years	0.649***	0.059	0.859	0.443	1.030	0.554
45-54 years	0.962	0.096	1.425	0.808	1.276	0.731
55-64 years	1.203	0.132	0.605	0.377	0.390	0.249
65 years and over (ref 35-44 years)	0.872	0.083	1.141	0.620	0.270+	0.208
Interaction HiVal*Female			1.000	0.038	0.993	0.040
Interaction HiVal * 16-24			1.062	0.066	1.080	0.080
Interaction HiVal * 25-34			0.967	0.059	0.947	0.060
Interaction HiVal * 45-54			0.954	0.064	0.968	0.065
Interaction HiVal * 55-64			1.087	0.081	1.118	0.086
Interaction HiVal * 65+			0.968	0.062	0.993	0.090
Self-employed					0.568	0.432
Unemployed					0.971	0.836
Retired					8.498**	7.025
Family care					0.362	0.301
Full-time student					5.635*	4.472
Long term sick/disabled					2.308	2.233
Other (ref employed)					0.039+	0.070
Cohabiting					1.425	0.797
Widowed					0.911	0.796
Divorced					0.591	0.548
Separated					0.067	0.118
Never married (ref married)					0.430+	0.213
Interaction HiVal * Self-emp					1.157	0.107
Interaction HiVal * Unemployed					0.994	0.103
Interaction HiVal * Retired					0.975	0.095
Interaction HiVal * Family care					1.205+	0.123
Interaction HiVal * Student					0.854+	0.080
Interaction HiVal * LT Sick					0.941	0.107
Interaction HiVal * OtherAct					1.537+	0.352
Interaction HiVal * Cohabiting					1.006	0.067
Interaction HiVal * Widowed					0.983	0.099
Interaction HiVal * Divorced					1.117	0.125
Interaction HiVal * Separated					1.436	0.325
Interaction HiVal * Never married					1.098	0.064

*** P < 0.001; ** 0.001 < P < 0.01; * 0.01 < P < 0.05; + 0.05 < P < 0.10

Notes: The base is all sample members believed to be eligible for an interview at wave 14 and issued to field, regardless of response history at previous waves. The dependent variable takes the value 1 if a full interview, telephone interview or proxy interview was achieved for the sample member at wave 14, 0 otherwise. Demographic variables are defined as in the note to Table 2, so the 675 cases with missing values are excluded from this analysis, leaving an analysis base of 9265 cases.

6 CONCLUSION

Several aspects of the use of respondent incentives are shared between cross-sectional and longitudinal surveys. In both contexts, an incentive sent unconditionally in advance of the interview appears to be most effective in increasing response rate, cash incentives are more effective than gifts in kind, a higher incentive amount tends to produce a higher response rate and there is some, but not consistent, evidence that incentives are more effective for those who are least likely to respond to the survey.

However, some aspects are distinct in the case of longitudinal surveys. Effects on long-term retention rates are perhaps more important than effects on wave-specific response rates and the evidence suggests that the effect of a repeated incentive can get more pronounced the more waves are involved. Retention rates are important in their own right as sample size cannot easily be manipulated for a long-term panel survey as it can for a cross-sectional survey. But arguably of more importance is attrition bias. Here, the effect of incentives is less clear. There is some evidence that incentives act disproportionately on sample members with low retention propensities, suggesting that they have potential to reduce bias. But some studies failed to show any effect at all of incentives on sample composition.

Longitudinal surveys often adjust their practices with regard to incentives over time, typically increasing the value of the incentive to keep it in line with the general cost of living and in some cases varying the type of incentives used. Our own study, reported in section 5.2 above, suggests that even a small increase in the value of an incentive on a mature panel can bring a significant improvement in response rates. This might suggest that regular small increases in the value of the incentive could be more effective than an occasional larger increase. Alternatively, the finding could be specific to the context in which the value of the incentive had remained unaltered for eight annual waves. Further research is needed on this point.

Even though most longitudinal surveys do not use differential incentive amounts for respondents based on past response behaviour or predicted response propensities, most have some circumstances under which a household or individual may receive more than the standard amount. Variation in incentive amounts within a sample is therefore already accepted, albeit perhaps implicitly only in order to reflect differences in the burden of participation. The great potential of longitudinal surveys to allow incentives to be tailored to the sample member's individual circumstances has not yet been realised. Tailoring could take many forms. The value of the incentive is just one dimension that could be varied between

sample members. Others might include the nature of the incentive (e.g. different kinds of vouchers), the way it is partitioned over time or waves (e.g. two small incentives, perhaps one with a between-wave mailing and one with the advance letter, *versus* a single larger incentive per wave), the timing of administration relative to data collection waves, and so on. Longitudinal surveys have thus far demonstrated very little willingness to experiment with targeted treatments, so evidence on effective strategies is thin.

Overall, it seems clear that the use of respondent incentives is an important element of the strategy to minimise attrition for many longitudinal surveys. The evidence suggests consistently that attrition rates would be higher in the absence of incentives. But we have limited knowledge of what the optimum strategies are for any given design of longitudinal survey and whether or how incentive strategies translate into improvements in the accuracy of estimation over the longer term. In particular, we still know relatively little about the effect of changing incentive amounts or delivery methods during a longitudinal survey, targeting particular groups based on demographic characteristics or previous response history, the use of differential incentive amounts for different cases or circumstances, and the longer term effect of incentives on attrition and bias. We urgently need to extend the research knowledge base if we are to be able to use survey budgets effectively and wisely when choosing respondent incentive strategies for longitudinal surveys.

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