Article

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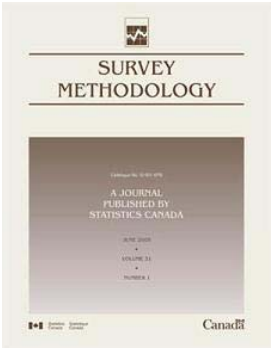

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Respondent incentives in a multi-mode panel survey: Cumulative effects on nonresponse and bias

Annette Jäckle and Peter Lynn [1]

Abstract

Respondent incentives are increasingly used as a measure of combating falling response rates and resulting risks of nonresponse bias. Nonresponse in panel surveys is particularly problematic, since even low wave-on-wave nonresponse rates can lead to substantial cumulative losses; if nonresponse is differential, this may lead to increasing bias across waves. Although the effects of incentives have been studied extensively in cross-sectional contexts, little is known about cumulative effects across waves of a panel. We provide new evidence about the effects of continued incentive payments on attrition, bias and item nonresponse, using data from a large scale, multi-wave, mixed mode incentive experiment on a UK government panel survey of young people. In this study, incentives significantly reduced attrition, far outweighing negative effects on item response rates in terms of the amount of information collected by the survey per issued case. Incentives had proportionate effects on retention rates across a range of respondent characteristics and as a result did not reduce attrition bias in terms of those characteristics. The effects of incentives on retention rates were larger for unconditional than conditional incentives and larger in postal than telephone mode. Across waves, the effects on attrition decreased somewhat, although the effects on item nonresponse and the lack of effect on bias remained constant. The effects of incentives at later waves appeared to be independent of incentive treatments and mode of data collection at earlier waves.

Key Words: Attrition; Item nonresponse; Mail survey; CATI; Unconditional incentive; Conditional incentive.

1. Introduction

Surveys around the world are facing declining response rates and, with this, increasing risks of nonresponse bias if nonrespondents' characteristics systematically differ from respondents' characteristics. For panel surveys this is particularly problematic, since even low nonresponse rates at each wave can lead to large cumulative losses. If nonresponse is differential, bias could increase with the duration of the panel. In order to boost participation rates, survey organisations increasingly offer respondent incentives. This paper provides new evidence on the cumulative effects of incentives on attrition, attrition bias and item nonresponse, using data from a large scale, multi-wave, mixed mode incentive experiment on a UK government panel survey of young people.

The effects of incentives have been studied in many settings: monetary incentives increase response more than gifts or lotteries (Church 1993; Singer, Hoewyk, Gebler, Raghunathan and McGonagle 1999); unconditional incentives (*i.e.*, those incentives that are given at the time of the survey request) increase response more than conditional incentives (those that are promised in return for participation) (Church 1993; Goyder 1994; Hopkins and Gullickson 1992; Singer *et al.* 1999); response rates increase with the value of the incentive (Armstrong 1975; Church 1993; Fox, Crask and Kim 1988; Hopkins and Gullickson 1992; Rodgers 2002; Yu and Cooper 1983); incentives have larger effects in studies with low response rates and larger effects in postal than interviewer administered surveys (Singer *et al.* 1999). Most evidence of differences between modes in the effect of incentives, however, stems from comparisons of separate studies and fails to control for differences in other measures affecting response. As a result, differences in the effects of incentives are not necessarily genuine mode effects. The study by Ryu, Couper and Marans (2006) is an exception. The authors compared the effects of monetary incentives and gifts in a mixed mode postal and face-to-face survey. Their study did not, however, include a no-incentive condition and so did not allow an evaluation of the magnitude of incentive effects across modes. We compared the effects of incentives in a mixed postal and computer-assisted telephone interviewing (CATI) survey and, in postal mode, also examined the effects of conditional and unconditional incentives.

Research on the effects of incentives has focused on response rates and little is known about the effects on bias, the ultimate reason for concern about low response. Incentive studies are mostly limited to studying effects on bias in sample composition and some studies have found that incentives disproportionately increase participation of respondents typically under-represented, for example those with low education (Singer, Van Hoewyk and Maher 2000), poor (James 1997), black or poor (Mack, Huggins, Keathley and Sundukchi 1998), of black or Indian minority ethnic groups, living in larger households or households with dependent children, aged 0-20, or single (Stratford, Simmonds and Nicolaas 2003). Biases in sample composition are

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however not necessarily correlated with biases in important survey estimates, especially since typically only a handful of compositional characteristics are studied. Ultimately, studies of nonresponse bias are limited by the lack of information about nonrespondents, a limitation which can be overcome to some extent by panel studies, where information about nonrespondents is available from waves prior to the dropout. We estimate the extent of bias due to attrition in terms of sample composition and survey variables. We then assess the effectiveness of incentives at reducing bias, exploiting the information on attriters available from the panel.

Additionally, little is known about the effects of incentives over waves of a panel survey, whether the same treatment is administered repeatedly or whether the treatment changes between waves. In a review of the use of incentives in longitudinal studies, Laurie and Lynn (in press) concluded that, given the cost implications of changing incentive conditions, there was surprisingly little evidence about the longer term effects of such changes to guide survey practitioners. Quoting an internal memorandum of the US Census Bureau, Ward, Boggess, Selvavel and McMahon (2001) wrote (see page 2) that a "review of the well-known longitudinal studies (Downs 1999) found that all non-Census Bureau studies used a monetary incentive during each wave, but there had been no scientific tests to determine the effectiveness of the incentives". If attrition leads to dropout of the least co-operative, the sample might increasingly be composed of committed respondents who are less responsive to incentives, because they are sufficiently motivated to participate even without the incentive (Laurie and Lynn in press). In this case, incentives may have decreasing marginal effects on response rates over the life of the panel. By the same token, incentives may have increasing effects on attrition bias, if they have disproportionate effect on sample members who would otherwise be more likely to drop out. Although some incentive studies have been carried out in the context of panels, they mostly only covered one wave, or examined the effect of changes in incentive treatment from one wave to the next. Martin, Abreu and Winters (2001) and Ward *et al.* (2001), for example, studied the effects of incentives on conversion rates of previous wave nonrespondents; Rodgers (2002) and Laurie (2007) examined the effects of changes in incentive values in a panel. The only studies which examined the effects of incentives over more than two waves appear to be those by James (1997), Mack, Huggins, Keathley and Sundukchi (1998) and Laurie and Lynn (in press), who reported that the positive effect of an incentive paid early in a panel persisted for several waves even without repeated incentive payment. These studies, however, only examined the effect of an incentive paid in a single wave and did not examine the cumulative effects of incentives offered over successive waves. We examine the cumulative effects of continued incentive payments across three waves spanning a time frame of three years, as well as the effects of changes from telephone to postal mode and from conditional to unconditional incentive treatment.

Finally, there is conflicting evidence in the literature about the effects of incentives on data quality. Although concern is frequently voiced that incentives may lead to lower data quality, by marginally increasing the motivation of respondents who would otherwise have dropped out of the study and are not sufficiently able or motivated to respond diligently, existing studies have either found that incentives lead to improved respondent effort and less item nonresponse (James and Bolstein 1990; Mack *et al.* 1998; Singer *et al.* 2000), or have found no relationship (Berk, Mathiowetz, Ward and White 1987; Davern, Rockwood, Sherrod and Campbell 2003; Goyder 1994; Shettle and Mooney 1999; Singer *et al.* 1999; Teisl, Roe and Vayda 2005; Tzamourani and Lynn 1999; Willimack, Schuman, Pennell and Lepkowski 1995). Item nonresponse is potentially critical, because analysts typically only use cases with complete data. This leads to losses in efficiency due to reductions in sample sizes and, similar to unit nonresponse, can lead to biased estimates and invalid inference if item nonrespondents are not a random subset of the sample (Mason, Lesser and Traugott 2002). Problems of item nonresponse increase for multivariate analysis, if the patterns of missingness vary across items, and for analysis of change, which in addition depends on complete information at different points in time. Since incentives may affect both unit and item nonresponse, it is then not clear what their net effect may be on repeated measures derived from a panel study. We examined the effect of incentives on item nonresponse rates and calculated their net effect on attrition and item response.

2. Hypotheses tested

The outcomes measured for this analysis were the attrition rate, item nonresponse rate and attrition bias. Attrition was an absorbing state, since the survey did not reissue nonrespondents at later waves. Item nonresponse was measured as the number of non-filtered items missing, either due to refusals or 'don't know' answers. (Non-filtered items are those which apply to all sample members: items for which eligibility is determined by the response to an earlier question are excluded from our measure of item nonresponse.) Attrition bias was measured in terms of sociodemographic characteristics and wave 1 survey measures. These three outcome measures were used to test the following:

*H*1: *Effects of incentives on attrition, item nonresponse and attrition bias.*
In previous studies incentives have generally increased response rates, be it because norms of social exchange oblige the respondent to return a "favour" (norm of reciprocity, Gouldner 1960) or because the incentive substitutes for a lack of motivation to participate for other reasons, such as civic duty or topic interest (leverage-salience theory, Groves, Singer and Corning 2000). Incentives may in addition motivate respondents to provide better quality responses, reducing item nonresponse. At the same time incentives may change the sample composition to include more respondents who are not diligent about answering the survey questions, and as a result increase item nonresponse. Finally, incentives may have differential effects on attrition across sample members. Those with a high propensity to participate in the survey without the incentive may be less likely to be affected by incentives, while those more likely to drop out of the survey may be more susceptible. As a result, incentives may reduce attrition bias.
*Null hypothesis H*1: *Incentives have no effect on attrition, item nonresponse or attrition bias.*

*H*2: *Effects of incentives across waves*.
The effect of incentives in increasing unit and item response rates may weaken across waves, if attrition leads to dropout of the least motivated sample members and the remaining members are sufficiently motivated to participate for other reasons and hence less susceptible to incentives (Laurie and Lynn in press). However, the extent to which incentives reduce nonresponse bias could increase over waves, if incentives disproportionately retain those in the sample who are most likely to otherwise drop out.
*Null hypothesis H*2: *The effects of incentives do not change across waves.*

*H*3: *Effects of unconditional and conditional incentives in a panel context*.
Previous studies, carried out on cross-sectional surveys, suggest that unconditional incentives have larger effects on unit nonresponse, possibly because the prepayment signals that the survey organisation trusts the sample member will participate, reinforcing the norm of reciprocity. Whether the different incentive conditions have different effects on item nonresponse is not clear.
*Null hypothesis H*3: *Unconditional and conditional incentives have similar effects in a panel context.*

*H*4: *Effects of incentives in postal and telephone mode*.
Comparisons of previous studies suggest that incentives have a larger effect in postal mode, possibly because in telephone mode the interviewer already functions as an external motivator to increase both unit and item response (Singer *et al.* 1999) and the scope for additional improvements is smaller. The same may not necessarily be true in a panel context where the effect of mode on response may be mediated by the respondent's experience of previous waves.
*Null hypothesis H*4: *Incentives have similar effects in postal and telephone mode.*

*H*5: *Effects of changes over waves in mode or incentive treatment*.
Compared to sample members allocated to the same mode and treatment across waves, those who were allocated to different treatments or different modes may differ in their experiences of previous survey waves and their expectations about future waves. As a result, the effect of incentives may not only be conditional on mode at the current wave, but may be influenced by the incentive treatment and mode in previous waves.
*Null hypothesis H*5: *Changes in mode or incentive treatment over waves do not have lasting effects.*

*H*6: *Effects of incentives across ability levels*.
Sample members with low education levels are typically more likely to drop out of surveys. If incentives reduce attrition bias, they should therefore disproportionately reduce attrition among lower achievers. Low ability respondents may at the same time be more likely to provide incomplete responses, if they find the task of completing the postal questionnaire more difficult. Therefore, incentives *may* increase mean levels of item nonresponse.
*Null hypothesis H*6: *Incentives have similar effects across ability levels.*

3. Study design

The Youth Cohort Study of England and Wales (YCS) investigates transitions from compulsory education to further or higher education or the labour market and typically samples cohorts of 16 to 17 year-olds every two years, who are surveyed on several occasions at annual intervals. The incentives experiment was embedded in waves 2, 3 and 4 of cohort 10. The survey is managed and funded by the Department for Children, Schools and Families, who jointly designed the incentive experiment

with the National Centre for Social Research, the survey contractors for waves 2 and 3 of YCS cohort 10.

3.1 The survey

The population studied in the YCS cohort 10 consisted of pupils in England and Wales who had reached minimum school leaving age of 16 in the 1998/1999 school year (Russell and Phelps 2001), that is, a one year age cohort of pupils born between 1-9-1982 and 31-8-1983. A 10% random sample was drawn from the registers of schools (excluding special schools and schools with fewer than 20 pupils of that age) in 1999, by asking schools to provide the names and addresses of pupils born on the 5th, 15th and 25th of every month. From the resulting file of 31,424 names and addresses a systematic random sample of 25,000 pupils was drawn. The first wave of the survey took place a year later in spring 2000, the second at the end of 2000, the third in spring 2002 and the fourth in spring 2003. Nonrespondents were not issued in subsequent waves and, as a result, attrition was monotonic.

Wave 1 was a postal survey with telephone follow-up of nonrespondents after 4 mailings (initial questionnaire mailing and three reminders). Based on reported examination results, wave 1 respondents were classified as either 'higher achievers' (if they had obtained 5 General Certificate of Secondary Education examination passes at grades A* to C) or 'lower achievers' otherwise. This led to around one-third of wave 1 respondents being classified as lower achievers. At wave 2 roughly one third of issued sample members were randomly selected for additional questions on particular topics and assigned to computer assisted telephone interviewing (CATI). In addition to the core questionnaire, telephone respondents were administered a module on decisions about entering higher education (for higher achievers) or on educational and employment aspirations (for lower achievers). The remaining sample members were administered the core questionnaires by post. At wave 3 all lower achievers received the core mail questionnaire, although the telephone module continued to be carried for a third of higher achievers. At wave 4 all respondents were assigned to the core postal survey. Figure 1 illustrates the allocation to modes and incentives.

The core questionnaire remained mainly unchanged for the three experimental waves. Telephone respondents were asked the core questions before the additional modules. The core questionnaire was the same as the postal questionnaire, although some items were adapted for administration over the telephone. The average telephone interview took around 20 minutes. (The questionnaires and technical reports are available via the UK Data Archive in the appendices of the YCS User Guide at http://www.data-archive.ac.uk/findingdata/snDescription.asp?sn=4571&key=YCS.)

3.2 The incentives experiment

Facing growing concerns over declining response rates, an experiment was introduced in the second wave of cohort 10, to study the effect of incentive payments on response rates and nonresponse bias. A proportion of wave 1 respondents on both the postal and telephone surveys were sent a GBP5 voucher (approx. USD10 or EUR7), while the control groups received no such incentive. Additionally, in the postal survey the incentives were either unconditional (the incentive was sent with the initial mailing) or conditional (the voucher was promised in the original mailing, but only sent on receipt of a completed questionnaire). At waves 3 and 4, all incentives were paid unconditionally.

Wave 1	Postal core questionnaire. Reported exam results used to classify respondents for wave 2 allocation.									
	Higher Achievers					Lower Achievers				
Wave 2	T_x	T_u	P_x	P_u	P_c	T_x	T_u	P_x	P_u	P_c
Wave 3	T_x	T_u	P_x	P_u	--	--	--	P_x	P_u	--
Wave 4	--	--	P_x	P_u	--	--	--	P_x	P_u	--
Questionnaire	Core + Higher education		Core			Core + Education and employment		Core		

Notes: T = telephone, P = postal, x = control, u = unconditional incentive, c = conditional incentive. Arrows indicate changes in incentive treatment or mode allocation between waves.

Figure 1 Experimental design

3.3 Allocation of respondents to modes and incentive treatments

At wave 2, wave 1 respondents were randomly assigned to either telephone or postal mode. The allocation of incentive treatments was however done at the school level (randomised cluster assignment by mode). Each school represented in the sample was allocated to one telephone treatment (control or incentive) and independently allocated to one, potentially different, postal treatment (control, unconditional incentive or conditional incentive), so that all sample members from the same school approached in the same mode received the same incentive treatment.

The 4,712 wave 1 lower achiever respondents were stratified by identification number within school within Government Office Region and alternately allocated to telephone and postal treatments. We have excluded from the analysis 627 cases for which there was no valid telephone number on file, as those amongst this group who had been allocated to telephone mode were approached by post. Consequently, analysis of lower achievers is restricted to 2,097 approached by telephone and 1,988 approached by post.

A similar procedure was carried out for higher achievers, except that a larger proportion was allocated to postal treatment. There were 8,909 wave 1 higher achiever respondents of which 751 had no valid telephone number and are excluded from the analysis. After these exclusions there are 2,922 higher achievers allocated to telephone mode and 5,236 allocated to postal mode.

For the allocation of schools to incentive treatment groups, the schools containing telephone sample members (i.e. all schools apart from a few of the very smallest schools with fewer than five pupils in the sample) were stratified according to the ratio of lower to higher achievers in the sample and randomly assigned to incentive treatments within strata. (The proportion of schools assigned to incentives was 1/2 if the ratio of lower to higher achievers in the sample was ≥ 2; 1/3 for $1/2 \leq$ ratio < 2 and 1/4 for all remaining schools.) The procedure was repeated for the allocation of schools in the postal treatment groups, where those selected for incentive treatment were randomly split into a conditional and an unconditional treatment group. (The proportions allocated to incentives were 2/3 if the ratio was ≥ 2; 1/3 for $0 \leq$ ratio $< 1/2$ and 1/6 for all other schools.) All estimates of significance presented in this text account for the clustered sampling design of the incentive experiment.

Table 1 shows the issued sample sizes at each wave for the different treatment and mode combinations, excluding cases of known ineligibility who had either moved abroad or died ($n = 13$ at wave 2; $n = 3$ at wave 3). Ineligible cases at wave 4 are not identified in the data, but the number is likely to be small. The analysis also excludes wave 1 respondents for whom no telephone number was known at the time of the allocation to modes for wave 2, as described above, and 117 higher achievers assigned to telephone mode at wave 2, who responded by post and were subsequently allocated to postal mode.

Table 1 also documents the observed wave-on-wave and cumulative response rates (AAPOR RR1). The rates are shown by achievement level and sequential mode/incentive combination. Wave-on-wave response rates for the higher achiever sample allocated to telephone control at wave 2 and moved to postal control at wave 4 (Col 1) were, for example, 76.82%, 69.13% and 72.21%. The issued numbers of cases declined from 2,075 to 1,101 across the three waves, because nonrespondents were not issued in subsequent waves.

Table 1 Conditional and cumulative response rates

Wave	Response Rate %	Higher Achievers					Lower Achievers				
		$T_xT_xP_x$	$T_uT_uP_u$	$P_xP_xP_x$	$P_uP_uP_u$	$P_cP_uP_u$	$T_xP_xP_x$	$T_uP_uP_u$	$P_xP_xP_x$	$P_uP_uP_u$	$P_cP_uP_u$
2	Conditional	76.82	80.91	78.23	86.45	82.32	65.21	70.41	64.93	75.00	71.35
	(Issued *n*)	(2,075)	(728)	(3,262)	(1,004)	(967)	(1,282)	(811)	(807)	(608)	(569)
3	Conditional	69.13	73.17	73.07	81.91	81.36	59.09	70.93	63.36	71.93	70.20
	(Issued *n*)	(1,594)	(589)	(2,551)	(868)	(794)	(836)	(571)	(524)	(456)	(406)
	Cumulative	53.11	59.20	57.16	70.82	66.94	38.53	49.94	41.14	53.95	50.09
4	Conditional	72.21	85.61	76.11	85.65	86.82	63.16	74.26	65.36	75.30	81.34
	(Issued *n*)	(1,101)	(431)	(1,863)	(711)	(645)	(494)	(404)	(332)	(328)	(284)
	Cumulative	38.31	50.69	43.48	60.66	58.03	24.34	36.99	26.89	40.63	40.60

Notes: AAPOR Response Rate 1. Treatment groups are identified by T = telephone, P = postal, x = control, u = unconditional incentive, c = conditional incentive. $T_x T_x P_x$ for example, refers to the sample allocated to telephone control at waves 2 and 3 and to postal control at wave 4. Conditional response rates are conditional on response at the previous wave. The base is the number of issued cases, which excludes previous wave nonrespondents and ineligible cases. Cumulative response rates are the percentage of wave 1 respondents remaining in the respondent sample. The base is the wave 2 number of issued cases, excluding three higher achievers ineligible at wave 3 (1 $P_x P_x P_x$ and 2 $P_c P_u P_u$).

4. Outcome measures and methods

The analysis is based on the sample of wave 1 respondents, since allocation to experimental treatments used information collected in the first wave and the corresponding characteristics of wave 1 nonrespondents are unknown. Our focus is therefore on attrition, conditional upon wave 1 response. This is the aspect of non-response that is particular to panel surveys, though of course it must be recognised that the characteristics of attrition are conditional on the characteristics of wave 1 response. The response rate at wave 1 (AAPOR RR1) was 54.80%, excluding 5 cases of known ineligibility (Russell and Phelps 2001). This section describes the outcome measures and methods used to evaluate the hypotheses about the effects of incentives.

4.1 Attrition

To test the effect of incentives on attrition, we estimated the probability of attrition as a function of the experimental design variables (*tel*ephone mode, *unc*onditional incentives, *cond*itional incentives, *l*ower *a*chievers) and their interactions. For each of the three experimental waves ($t = 2, 3, 4$), we estimated a separate probit model of the probability of attrition, in each case using the wave 1 respondent sample as the base:

$$\begin{aligned}\Pr(attrition_{it}) = F(&\beta_{0t} + \beta_{1t} tel_i + \beta_{2t} unc_i + \beta_{3t} cond_i \\ &+ \beta_{4t} la_i + \beta_{5t} tel_i * unc_i + \beta_{6t} la_i * tel_i \\ &+ \beta_{7t} la_i * unc_i + \beta_{8t} la_i * cond_i \\ &+ \beta_{9t} la_i * tel_i * unc_i + \varepsilon_i) \end{aligned} \quad (1)$$

where F is the probit link function. The estimated coefficients and standard errors from this model were then used to calculate predicted probabilities of attrition under different treatment conditions and to test for differences due to incentives.

4.2 Item nonresponse

To test the effect of incentives on item nonresponse, we estimated count models of the number of items missing, using all non-filtered items from the core questionnaires in waves 2 ($n = 44$), 3 ($n = 48$) and 4 ($n = 46$), where 'don't know' was counted as a missing value. We used the same specification of the predictors as for model (1) to estimate separate negative binomial regression models for each of the three experimental waves, conditional on response to the given wave. (Overdispersion meant that Poisson models did not fit the data: the P-value of the Likelihood Ratio test of equal mean and variance was 0.0000 for all three waves.) The estimated coefficients and standard errors from these models were used to calculate predicted item nonresponse under different treatment conditions and to test for differences due to incentives.

4.3 Attrition bias

To test the effect of incentives on attrition bias, we estimated the probability of attrition using model (1) but including wave 1 respondent characteristics and their interactions with the experimental design variables as predictors. We estimated separate probit models for attrition at each of the experimental waves ($t = 2, 3, 4$) and for each characteristic, again using the wave 1 respondent sample as the base:

$$\begin{aligned}\Pr(attrition_{it}) = F(&\beta_{0t} + \beta_{1t} tel_i + \beta_{2t} unc_i + \beta_{3t} cond_i \\ &+ \beta_{4t} la_i + \beta_{5t} tel_i * unc_i + \beta_{6t} la_i * tel_i \\ &+ \beta_{7t} la_i * unc_i + \beta_{8t} la_i * cond_i \\ &+ \beta_{9t} la_i * tel_i * unc_i + \beta_{10} w1char \\ &+ \beta_{11} w1char_i * tel_i + ... \\ &+ \beta_{19} w1char_i * la_i * tel_i * unc_i + \varepsilon_i) \end{aligned} \quad (2)$$

where β_{11} to β_{19} are the coefficients for the interactions of the characteristic with the design variables. The coefficient for the respondent characteristic, β_{10}, provides information about the direction, magnitude and, in combination with its standard error, the significance of attrition bias for the postal, no incentive, higher achiever reference group. The interaction of the characteristic and the incentive indicators provide information about the change in attrition bias due to incentives. The significance of all interactions presented in this text was calculated following recommendations for nonlinear models by Norton, Wang and Ai (2004) using the command 'predictnl' in Stata version 9.

The characteristics tested were gender, school type, exam results, current activity (full-time education, employment, not in education, employment or training ("neet")), experience of unemployment, studying for vocational or academic qualifications, household composition (living with parent, partner, neither) and a set of attitudinal questions about employment and training. The wording of all questions is documented in Table 6. The characteristics chosen were those for which respondents and non-respondents could be expected to differ, based on previous studies of nonresponse in the YCS and other surveys and on nonresponse theories (Groves and Couper 1998; Lynn, Purdon, Hedges and McAleese 1994).

4.4 Reported results

Since coefficients from non-linear models cannot be interpreted substantively (Long 1997), we report predicted values based on the model estimates, rather than coefficients. Unless stated otherwise, the results are for the higher achiever group. To convey a sense of the magnitude of differences in outcomes across treatments, we report transformations of the predicted values, comparing each treatment with the comparison group, the higher achiever postal control.

5. Attrition, item nonresponse and attrition bias in the control groups

As a background to the evaluation of the effects of incentives, this section documents the extent of attrition, item nonresponse and attrition bias in the control groups, highlighting differences across waves, achievement levels and modes. Throughout the discussion the higher achiever postal no-incentive group is the reference category, with which all other treatments are compared.

5.1 Attrition

The predicted cumulative attrition rate among higher achievers allocated to the postal control group, increased from 21.77% in wave 2 to 56.53% in wave 4 (Table 2, Col 1). For *lower achievers* (Col 2), attrition rates in the postal control group were 61% higher at wave 2, but this difference decreased across waves to 29% at wave 4. The difference by achievement level was nonetheless significant in all three waves (P-value of $\beta_4 = 0.0000$ for $t = 2, 3, 4$). In *telephone mode* (Col 3), attrition rates in the control group were not significantly different at wave 2, but 9% higher at wave 3 (P-value of $\beta_5 = 0.0034$ for $t = 3$). This is contrary to findings from other studies, where nonresponse is generally lower in telephone mode due to the role of the interviewer in persuading respondents to take part in the survey. One possible reason for finding the opposite in this study is that for both the postal and CATI treatment groups, further attempts to obtain responses from initial non-respondents were made by telephone, so that only the postal group had a multi-mode treatment. Secondly, the burden of the wave 2 survey (measured by the interview length) was higher for the telephone respondents due to the additional modules, possibly leading to higher nonresponse at wave 3 than among the postal sample. The predicted cumulative response rates, which were the base for the calculation of percentage differences across treatment groups, are documented in the first three columns of Table 5.

5.2 Item nonresponse

The predicted number of missing items in the higher achiever postal control group was 2.89 at wave 2, falling to 1.75 at wave 4 (Table 3, Col 1). For *lower achievers* (Col. 2), the expected count for the control group was 21% higher at wave 2, with the gap increasing to 45% at wave 4. The differences by achievement level were significant in all three waves ($P \leq 0.0001$ for β_4, $t = 2, 3, 4$). For *telephone mode* (Col 3), the predicted count was 4% lower at wave 2 and 12% lower at wave 3 ($P = 0.0000$ for β_5, $t = 2, 3$), compared with postal mode. The predicted item non-response counts, used as the base for the calculations presented in Table 3, are documented in columns 4 to 6 of Table 5.

5.3 Attrition bias

Nonresponse in the higher achiever postal control group was differential for all of the domains tested (Table 4). The respondent samples significantly over-represented those living with their parents, in full-time education or studying for academic qualifications. Predicted attrition rates for those in full-time education in the higher achiever postal control group, for example, were 14% lower than for those not in full-time education at wave 2, with the difference increasing to 17% by wave 4 ($P = 0.0000$ for β_{10}, $t = 2, 3, 4$). At the same time, the respondent samples under-represented males, those in secondary modern schools, with low or no exam results, who thought employers did not give young people the right training and that making plans for the future was a waste of time, those in full-time employment, those who had experienced unemployment and those who were studying for vocational qualifications. Bias was particularly strong with respect to qualifications. Those without any or with very low exam qualifications were around 50% more likely to have attrited from the sample by waves 3 and 4, compared to sample members with better qualifications. Similarly, those in full-time employment were 17% more likely than those not in employment (most of whom were still in education) to drop out at wave 2, with the difference increasing to 22% by wave 4.

Including background information used by the YCS for weighting (gender, school type, exam results and region) in the models did not affect the bias for any of the characteristics (in each wave and for each item, the P-value > 0.05 from Wald tests of the equality of β_{10} estimated with and without background characteristics; not reported), except for bias with respect to qualifications, which was somewhat reduced when the background information was included.

The extent of attrition bias was mostly stable across waves, except for a few characteristics. In the higher achiever postal control sample, the under-representation of males significantly increased from waves 2 to 4 (P-value

from a Wald test of the equality of β_{10} across the two waves = 0.0295; not reported). For some of the other characteristics, the bias significantly decreased across waves. Nonresponse bias associated with attending a modern school fell between waves 3 and 4 and bias associated with not having any qualifications fell between waves 2 and 3 and again between waves 3 and 4.

For lower achievers there were few differences in the extent of attrition bias (not reported). Bias by gender, that is the difference in predicted nonresponse rates between males and females, was 12% less than for higher achievers at wave 4 (*P*-value of the interaction between achievement level and gender was 0.0425 for $t = 4$), and bias by full-time employment was 4% less at wave 2 (*P*-value = 0.0269 for $t = 2$); bias according to attitudes on training provided by employers was 9% higher at wave 2 (*P*-value = 0.0056); bias according to whether studying for academic or vocational qualifications was higher at wave 2 (22% and 13%), 6% lower and 1% higher at wave 3, and lower at wave 4 (81% and 92%).

Table 2 Effect of incentives on attrition rates

	Control groups			Incentives		Incentives by ability		Incentives by mode and ability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Wave	P_x^{ha}	$\frac{P_x^{la} - P_x^{ha}}{P_x^{ha}}$	$\frac{T_x^{ha} - P_x^{ha}}{P_x^{ha}}$	$\frac{P_u^{ha} - P_x^{ha}}{P_x^{ha}}$	$\frac{(P_c^{ha} - P_x^{ha})/P_x^{ha}}{(4)^{ha}}$	$\frac{(4)^{la}}{(4)^{ha}}$	$\frac{(P_c^{la} - P_x^{la})/P_x^{la}}{(P_c^{ha} - P_x^{ha})/P_x^{ha}}$	$\frac{(T_u^{ha} - T_x^{ha})/T_x^{ha}}{(4)^{ha}}$	$\frac{(T_u^{la} - T_x^{la})/T_x^{la}}{(T_u^{ha} - T_x^{ha})/T_x^{ha}}$
2	21.77	0.6112	0.0650	-0.3777	0.4966	0.7602	0.9763	0.4669	0.8471
(*P*-Value)		(0.0000)	(0.2268)	(0.0000)	(0.0142)	(0.5085)	(0.4332)	(0.0556)	(0.6810)
3	42.86	0.3734	0.0941	-0.3191	0.7066	0.6820	0.6743	0.4074	1.4275
(*P*-Value)		(0.0000)	(0.0034)	(0.0000)	(0.0592)	(0.7834)	(0.8287)	(0.0057)	(0.0861)
4	56.53	0.2933	-	-0.3040	0.8402	0.6179	0.7340	0.6597	0.8338
(*P*-Value)		(0.0000)	-	(0.0000)	(0.2244)	(0.2535)	(0.8177)	(0.0911)	(0.9265)

Notes: *P* = postal, *T* = telephone, *x* = control, *u* = unconditional incentive, *c* = conditional incentive, *ha* = higher achievers, *la* = lower achievers. Column (1) shows the predicted attrition rate for the postal control higher achiever sample. The remaining columns show proportionate change in predicted rates. *P*-values of columns 2-4 represent standard errors of the main effects in the probit model; column 5 represents *P*-values from a Wald test of the equality of the coefficients for conditional and unconditional incentives; columns 7-9 represent *P*-values for the relevant interactions calculated using 'predictnl' in Stata version 9, according to Norton *et al.* (2004).

Table 3 Effect of incentives on item nonresponse (counts)

	Control groups			Incentives		Incentives by ability		Incentives by mode and ability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Wave	P_x^{ha}	$\frac{P_x^{la} - P_x^{ha}}{P_x^{ha}}$	$\frac{T_x^{ha} - P_x^{ha}}{P_x^{ha}}$	$\frac{P_u^{ha} - P_x^{ha}}{P_x^{ha}}$	$\frac{(P_c^{ha} - P_x^{ha})/P_x^{ha}}{(4)^{ha}}$	$\frac{(4)^{la}}{(4)^{ha}}$	$\frac{(P_c^{la} - P_x^{la})/P_x^{la}}{(P_c^{ha} - P_x^{ha})/P_x^{ha}}$	$\frac{(T_u^{ha} - T_x^{ha})/T_x^{ha}}{(4)^{ha}}$	$\frac{(T_u^{la} - T_x^{la})/T_x^{la}}{(T_u^{ha} - T_x^{ha})/T_x^{ha}}$
2	2.89	0.2068	-0.9579	0.1008	1.3849	2.4825	0.6927	0.1820	-0.4094
(*P*-Value)		(0.0005)	(0.0000)	(0.0173)	(0.4790)	(0.1308)	(0.6472)	(0.6251)	(0.9202)
3	2.54	0.3879	-0.8828	0.1660	1.5599	1.6788	1.2445	-0.9526	-0.1378
(*P*-Value)		(0.0001)	(0.0000)	(0.0049)	(0.2372)	(0.4339)	(0.6796)	(0.0442)	(0.3890)
4	1.75	0.4533	-	0.0085	17.5491	16.8405	0.4621	13.8706	2.3073
(*P*-Value)		(0.0013)	-	(0.9262)	(0.2133)	(0.4724)	(0.6481)	(0.5049)	(0.4530)

Notes: *P* = postal, *T* = telephone, *x* = control, *u* = unconditional incentive, *c* = conditional incentive, *ha* = higher achievers, *la* = lower achievers. Column (1) shows the predicted number of missing items of 44 non-branched items at wave 2, 48 at wave 3 and 46 at wave 4. The remaining columns show proportionate change in predicted item nonresponse counts. *P*-values of columns 2-4 represent standard errors of the exponentiated coefficients from the count model; column 5 represents *P*-values from a Wald test of the equality of the exponentiated coefficients for conditional and unconditional incentives; columns 7-9 represent *P*-values for the relevant interactions calculated using'predictnl' in Stata version 9, according to Norton *et al.*(2004).

Table 4 Attrition bias (higher achiever postal control group)

	Wave 2	*P*-Value	Wave 3	*P*-Value	Wave 4	*P*-Value
Male	0.0807	(0.0000)	0.1330	(0.0000)	0.1474	(0.0000)
School type						
Comprehensive 16	0.0196	(0.2645)	0.0102	(0.6178)	0.0259	(0.2060)
Comprehensive 18	-0.0197	(0.1966)	-0.0138	(0.4444)	-0.0200	(0.2650)
Selective	-0.0188	(0.3661)	-0.0547	(0.0407)	-0.0213	(0.4577)
Modern	0.2310	(0.0001)	0.2423	(0.0004)	0.1597	(0.0261)
Independent	-0.0142	(0.4639)	0.0147	(0.5245)	-0.0068	(0.7756)
Exam results						
5+ grades A-C	-0.0977	(0.1778)	-0.0866	(0.3060)	-0.1795	(0.0320)
1-4 grades A-C	0.0831	(0.2857)	0.0721	(0.4298)	0.1696	(0.0606)
5+ grades D-G	0.0324	(0.8769)	0.0715	(0.7739)	0.1849	(0.4536)
1-4 grades D-G	-0.2177	(0.0000)	0.5714	(0.0000)	0.4347	(0.0000)
None	0.7826	(0.0000)	0.5716	(0.0000)	0.4348	(0.0000)
Attitudes						
Employers don't give training	0.0842	(0.0000)	0.0882	(0.0000)	0.0798	(0.0001)
Training more important than pay	0.0108	(0.4808)	-0.0070	(0.6979)	-0.0062	(0.7370)
Plans for future are a waste of time	0.0656	(0.0959)	0.1457	(0.0015)	0.1371	(0.0030)
Information about opportunities	0.0034	(0.8431)	-0.0204	(0.3266)	-0.0236	(0.2549)
Enough support planning future	0.0063	(0.6771)	0.0043	(0.8233)	-0.0105	(0.5848)
Current activity						
In full-time education	-0.1371	(0.0000)	-0.1462	(0.0000)	-0.1728	(0.0000)
In full-time employment	0.1661	(0.0003)	0.1983	(0.0001)	0.2201	(0.0000)
Neither in employment, education or training	0.0898	(0.1387)	0.1036	(0.1495)	0.1098	(0.1184)
ILO unemployed	0.0112	(0.6272)	0.0573	(0.0421)	0.0475	(0.0879)
Unemployed during past 12 months	0.0246	(0.4216)	0.0731	(0.0523)	0.0891	(0.0146)
Studying for academic qualifications	-0.1173	(0.0000)	-0.1351	(0.0000)	-0.1341	(0.0000)
Studying for vocational qualifications	0.0677	(0.0001)	0.0882	(0.0000)	0.0721	(0.0003)
Living arrangements						
Living with parent	-0.1348	(0.0111)	-0.1916	(0.0027)	-0.1033	(0.0986)
Living with partner	0.0904	(0.4457)	-0.0441	(0.7475)	-0.1042	(0.4525)

Notes: Predicted differences in attrition rates based on $\hat{\beta}_{10,t}$, *i.e.*, prediction for each category compared to all residual categories. Each table entry is from a different model as explained in the text. *P*-values based upon estimated standard errors of the coefficient for the characteristic in the probit model.

Attrition bias in telephone mode was no different from postal mode, except for differential nonresponse by gender: the bias was 7% less at wave 2, 2% less at wave 3 and 1% more at wave 4 (*P*-value of the interaction between telephone mode and gender was ≤ 0.002 for $t = 2, 3, 4$).

6. Evaluation of hypotheses

The evidence discussed here is summarised in Table 2 (effects of incentives on attrition), Table 3 (effects on item nonresponse), Table 4 (effects on attrition bias) and Table 5 (net effect on unit and item nonresponse).

*H*1: *Effects of incentives on attrition rate, attrition bias and item nonresponse.*

Incentives reduced attrition and increased item nonresponse but did not impact on attrition bias. Unconditional incentives reduced cumulative *attrition* in the postal higher achiever sample (Table 2, Col 4) by 38% (corresponding to an 8 percentage point difference) at wave 2, 32% at wave 3 and 30% at wave 4 (*P*-value of $\beta_2 = 0.0000$ for $t = 2, 3, 4$). At the same time, the incentive increased *item nonresponse* by 10% at wave 2 and 17% at wave 3 (*P*-value of $\beta_2 \leq 0.05$ for $t = 2, 3$), but had no effect at wave 4 (Table 3, Col 4). The difference across waves was however not significant (see *H*2).

Incentives had a proportionate effect on attrition across all respondent characteristics tested and therefore did not reduce *attrition bias*: the *P*-value of the interaction of unconditional incentives and respondent characteristics was > 0.05 for all characteristics and waves (not reported). The exception was the proportion of pupils in 'modern' schools who were under-represented in all three waves. (Modern schools were the smallest category, representing only 2.8% of the wave 1 respondent sample.) Unconditional incentives reduced this bias by 60%, 47% and 78% at waves 2, 3 and 4 respectively (*P*-values of the interaction of incentives and modern school ≤ 0.01 for $t = 2, 3, 4$).

Since incentives had a positive effect on unit response and a negative effect on item response, Table 5 documents the net effect on the amount of information collected in the survey. The benefits of incentives in terms of unit nonresponse clearly outweighed the cost in terms of item nonresponse. For each sample person issued at wave 2, the predicted unit and item response

rates for the postal higher achiever sample implied that by wave 4, 40% more valid items were collected with unconditional incentives compared to the control group. For lower achievers, 50% more information was collected with incentives. This is, however, a crude measure of the net effect of incentives, since in a multivariate analysis or for analyses of change, different patterns of missingness across items or across waves may lead to large numbers of cases being dropped by pairwise deletion.

*H*2: *Effects of incentives across waves.*
The effect on attrition decreased somewhat across waves, while the effects on item nonresponse and attrition bias were constant. Incentives reduced attrition by 38% at wave 2, 32% at wave 3 and 30% at wave 4 (Table 2, Col 4). The effects were similar at waves 2 and 3, but significantly different between waves 2 and 4 and between waves 3 and 4 (*P*-value from a Wald test of the equality of β_2 across waves was ≤ 0.05). Although the relative effect of incentives decreased, the absolute effect increased across waves (-17 percentage points at wave 4, compared to -8 and -14 at waves 2 and 3, see Table 5). The effect of incentives on *item nonresponse* was not significantly different across waves (*P*-value of equality of β_2 across waves was > 0.05), although the predicted numbers of missing items fell across waves. Similarly, the effects of incentives on *attrition bias* did not differ across waves.

*H*3: *Conditional compared to unconditional incentives.*
Unconditional incentives had a greater effect in reducing attrition than conditional incentives, but similar effects on item nonresponse and attrition bias. For higher achievers, the conditional incentives used at wave 2 were only half as effective at reducing attrition as unconditional incentives (Table 2, Col 5) and the difference between the two conditions was significant (*P*-value from a Wald test of the equality of β_2 and β_3 was 0.0142). At the same time, conditional incentives increased *item nonresponse* by 38% more than unconditional incentives (Table 3, Col 5), but the difference was not significant. Conditional incentives somewhat reduced *attrition bias* for a single characteristic: sample members in the control group studying for vocational qualifications at wave 1 were 6.8% more likely to drop out than those not studying for vocational qualifications. With conditional incentives the difference was 6.4% (*P*-value of the interaction of conditional incentives with this characteristic was ≤ 0.05 for $t = 2$).

*H*4: *Differential effects by mode.*
Incentives had more effect on attrition and item nonresponse in postal than telephone mode, but no effect on attrition bias in either mode. In telephone mode, unconditional incentives had less than half the effect on *attrition* they had in postal mode for the higher achiever group (Table 2, Col 8). The difference was significant at wave 3 (*P*-value of the interaction between telephone mode and unconditional incentives was 0.0057) but not at wave 2. At wave 3, incentives increased *item nonresponse* 5% less in telephone mode than in postal mode (*P*-value of the interaction was 0.0442), but the difference at wave 2 was not significant. The lack of effect of unconditional incentives on *attrition* was no different across the two modes.

*H*5: *Effects of changes in mode or incentive treatment.*
Changing the incentive condition or mode did not have lasting effects. Changing the treatment from conditional to unconditional incentives had no lasting effect on either *attrition* or *item nonresponse* ($P > 0.05$ from Wald tests of the equality of β_2 and β_3 for $t = 3, 4$) and the effects after the change in treatment were similar to those for the sample allocated to unconditional incentives from the start (Tables 2 and 3, Col 5). Changing the survey mode from telephone to postal did not have a lasting effect on *attrition* or *item nonresponse* either ($P =$ value of the interaction for telephone mode and unconditional incentives > 0.05 at $t = 4$) and the effects after the change in mode were no different from the effects for the sample allocated to postal unconditional incentives from the start (Tables 2 and 3, Col 8).

*H*6: *Differential effects by ability level.*
The effects of incentives were similar across achievement levels. Differences between achievement levels in the proportional effects of unconditional and conditional incentives on *attrition* and *item nonresponse*, were not significant (Cols 6 and 7 in Tables 2 and 3 report the *P*-values of the interactions of achievement level with each of the incentive treatments), since the absolute effects were comparable. Unconditional incentives, for example, reduced attrition at wave 2 by 8 percentage points among higher achievers and 10 percentage points among lower achievers. However, since the level of nonresponse in the control group was 61% higher for the lower achiever group, the similar absolute effect implied a smaller proportional effect of only 76% of the effect for higher achievers.
Similarly, the difference between modes was not differential by achievement (Tables 2 and 3, Col 9 report the *P*-values of the interaction between achievement level, unconditional incentives and telephone mode) and the lack of effect on attrition bias was no different for lower achievers (not reported).

Table 5 Net effect of incentives on unit and item response

		Predicted cumulative RR (%)			Predicted mean # INR			# valid items per unit issued at w2: incentive/control		
		w2	w3	w4	w2	w3	w4	w2	w3	w4
Higher	P_x	78.23	57.14	43.47	2.89	2.54	1.75	-	-	-
Achievers	P_u	86.45	70.82	60.66	3.19	2.96	1.77	1.097	1.228	1.395
	P_c	82.32	66.80	57.91	3.30	3.20	2.01	1.042	1.152	1.324
	T_x	76.82	53.11	38.31	0.12	0.30	1.61	-	-	-
	T_u	80.91	59.20	50.69	0.12	0.25	1.80	1.053	1.116	1.317
Lower	P_x	64.93	41.14	26.89	3.49	3.52	2.54	-	-	-
Achievers	P_u	75.00	53.95	40.63	4.37	4.51	2.91	1.130	1.282	1.498
	P_c	71.35	50.09	40.60	3.83	4.66	2.72	1.090	1.186	1.504
	T_x	65.21	38.53	24.34	0.50	3.48	2.35	-	-	-
	T_u	70.41	49.94	36.99	0.49	3.56	2.99	1.080	1.294	1.498

Notes: RR = response rate, INR = item nonresponse, # = number. T = telephone, P = postal, x = control, u = unconditional incentive, c = conditional incentive. Calculation based on 44 non-branched items at wave 2, 48 at wave 3 and 46 at wave 4. The number of valid items is calculated as RR_4*(44-INR_2 + 48-INR_3 + 46-INR_4).

Table 6 Question wording of items included in analysis of nonresponse bias

Variable	Question wording
Year 11 exam results	"Please tell us: a) Which GCSE subjects you studied in Years 10 and 11, b) Which GCSE subjects you have taken an exam in, c) Your GCSE results (do not record any re-sit results obtained in Year 11)."
Attitudes:	"Here are some things which people have said. We would like to know what you think. Please put a cross in one box for each statement: Agree, Disagree, Don't know."
ATT: employers	Agree: "Most employers don't give young people the right king of training at work."
ATT: training/pay	Agree: "In looking for a job, I am more concerned to find one with training than one that pays the best."
ATT: plans	Agree: "I think that making plans for the future is a waste of time."
ATT: information	Agree: "I know how to find out about future work, training or education opportunities."
ATT: support	Agree: "I get enough support in planning my future."
Current activity:	"Please put a cross against one box to tell us your main activity at the moment: a) Out of work/unemployed, b) Modern Apprenticeship, National Traineeship, Youth Training or other government supported training, c) In a full-time job (over 30 hours a week), d) In a part-time job (if this is your *main* activity), e) In full-time education at school or college, f) Looking after home or family, g) Doing something else (please specify)."
In ft education	In full-time education.
In ft employment	In full-time employment.
NEET	Not in employment, education or training.
ILO unemployed	Unemployed and searching for job among economically active (YCS derived variable).
Unemployed	Unemployed in one or more months from April 1999 to March 2000: "We would also like to know what you have been doing over the past months. Please put a cross in one box for each month to show us what you were doing for all, or most of each month".
	Response options as for current activity, including 'On holiday'.
Studying (ac)	Yes: "At present, are you studying for GCSE, A/S or A-level qualifications?"
Studying (voc)	Yes: "At present, are you studying for any GNVQs (General National Vocational Qualifications)?" or "At present, are you studying for NVQ (National Vocational Qualification) or any other vocational or professional qualification including BTEC, City & Guilds or RSA qualifications?"
Household:	"Who lives in the same household as you? a) Father, b) Stepfather, c) Mother, d) Stepmother, e) Your own children, f) Brothers and sisters g) Other persons (please write in their relationship to you)."
Living with parent	Living with one or more of father, stepfather, mother or stepmother.
Living with partner	Living with boyfriend, girlfriend, husband, wife or partner.

7. Summary and discussion

This study has provided new evidence on the effects of continued incentive payments in a multi-mode panel study. We tested the effects of incentives on attrition, item nonresponse and attrition bias and whether these effects changed across waves. We also tested whether conditional and unconditional incentives had similar effects, whether incentive effects were differential across modes and ability levels, and whether changes in the incentive treatment or mode had lasting impact on the effect of incentives in subsequent waves.

The findings showed that unconditional incentives significantly reduced attrition and, although they also

increased item nonresponse, the net effect on the amount of information collected by the survey was positive. Incentives had proportionate effects across a range of respondent characteristics and as a result did not impact on attrition bias in terms of those characteristics. Item nonresponse increased more with unconditional than conditional incentives, and more in postal than in telephone mode. Attrition bias was not affected by either incentive treatment in either mode. Across waves, incentives had a somewhat decreasing effect on attrition, but similar effects on item nonresponse. The lack of effect on attrition bias was also a constant across waves. Changes in incentive treatment from conditional to unconditional, and in mode from telephone to postal, did not affect outcomes at later waves.

The findings imply that respondent incentives are an effective means of maintaining sample sizes of a panel and ensuring its value in terms of efficiency of estimation and feasibility of subgroup analyses. Among lower achievers, fully 50% more information was collected during the three experimental waves, in terms of the number of valid items per case issued at the start. Incentives were safe, in the sense that increased response rates did not inadvertently increase nonresponse bias in terms of observed characteristics.

Changes in incentive treatment did not have lasting effect; however, in this study the only change implemented was an improvement for the respondent, from conditional to unconditional incentives. Expectations formed on the basis of previous incentive treatments may well mean that changes have lasting effect, if the change reduces the value of the incentive in the eyes of the respondent (see, Singer, Van Hoewyk and Maher 1998).

Incentives had no effect on attrition bias. We could however not evaluate the effect on bias of nonresponse at wave 1. Ideally, we would assess both the magnitude of bias due to nonresponse at wave 1 and due to subsequent attrition, and the effects of incentives on both. It is possible that nonresponse at wave 1 is more detrimental in terms of bias than later attrition, especially in studies such as the present one with low initial response rates. In this case, the effect of incentives on bias at wave 1 may be more important than any effect on bias caused by attrition. In addition, the discussion of the effects of incentives on attrition bias has focused entirely on observed characteristics and although incentives did not have differential effects in terms of these, they may nonetheless have differential effects in terms of unobserved factors. If this were the case, the use of respondent incentives could introduce sample selection bias in multivariate estimates, if the unobservables determining the responsiveness to incentives are correlated with outcomes measured by the survey (Kennedy 2003). For example, if responsiveness to incentives depends on time preferences for money and this factor also determines the decision to leave further education and work instead, then models of the determinants of educational outcomes will lead to biased estimates.

Finally, there was little evidence that the respondent sample became less sensitive to incentives across waves as potentially less committed sample members dropped out. This finding is consistent with Laurie (2007), who reported that an increase in the value of an incentive in the British Household Panel Survey significantly increased response, even after 14 waves of the panel, with already high annual response rates of around 95% each year. Since previous studies have found that the effects of one-off incentives can carry over across waves (James 1997; Laurie and Lynn in press; Mack *et al.* 1998), a formal test of marginal effects of incentives would however require comparisons with a treatment group only offered an incentive at the first wave.

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