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Impact of Monetary Incentives and Mailing Procedures

An Experiment in a Federally Sponsored Telephone Survey

Methodology Report

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1. Introduction

Achieving high response rates in sample surveys has become increasingly more difficult in recent years. Atrostic, Bates, Burt, and Silberstein (2001) show that at the end of the twentieth century, the rates of nonresponse were increasing for in-person household surveys conducted by the U.S. Census Bureau. The issue is of even greater concern in random digit dial (RDD) telephone surveys. Curtin, Presser, and Singer (2000) and Steeh et al. (2001) examine changes in response rates in RDD surveys, but the Survey of Consumer Attitudes (SCA) is the only national RDD survey they review. No response rates for RDD surveys conducted after 1999 are given in these articles. However, steep declines in RDD survey response rates after 1999 have been reported by Curtin, Presser, and Singer (2005).

Groves and Couper (1998) describe a host of factors that may affect response rates in household surveys. For RDD surveys, the most important of these factors are those related to the study characteristics (e.g., burden, salience, sponsorship, and content), the general survey environment (e.g., call screening technology, level of telemarketing and other types of calls to the household, and the economy), and the survey methodology (e.g., use of advance letters, refusal conversion attempts, and monetary incentives). Curtin, Presser, and Singer (2000) present evidence that the level of effort used to gain survey participation in the SCA increased over approximately 18 years. More specifically, they show that the mean number of call attempts to complete an interview more than doubled and that there was an increase in refusal conversions, resulting in an increase of about 25 percent in interview time to complete a survey.[1] By 1996, monetary incentives were introduced experimentally in the SCA and by 1999, incentives were used for all possible respondents in an effort to keep response rates from falling to unacceptable levels.

The National Household Education Surveys Program

The National Household Education Surveys Program (NHES) is an RDD survey developed by the National Center for Education Statistics (NCES) in the Institute of Education Sciences, U.S. Department of Education. It is designed to collect information on important educational issues through telephone surveys of households in the United States. NHES has enabled NCES to gather data on a wide range of issues, such as early childhood care and education, children's readiness for school, parent perceptions of school safety and discipline, before- and after-school activities of school-age children, participation in adult and continuing education, parent involvement in education, school choice, homeschooling, and civic involvement. NHES uses computer-assisted telephone interviews (CATI) and has been conducted by Westat in 1991, 1993, 1995, 1996, 1999, 2001, and 2003.

NHES provides data on the populations of special interest to NCES and education researchers as defined by age and/or grade in school for each child survey and for persons ages 16 and older who are not enrolled in grade 12 or below for each adult survey. It targets these populations using specific screening and sampling procedures. Specific age or grade ranges for a given survey are determined by the survey topic and the research questions formulated for the specific survey administration.

NHES provides national cross-sectional estimates for the 50 states and the District of Columbia. The NHES design also yields estimates for subgroups of interest for each survey, as defined by age or grade for children, educational participation status for adults, and Black and Hispanic origin for all populations of interest. In addition to providing cross-sectional estimates, NHES is also designed to provide estimates of change over time in key statistics. The survey instruments are designed to address

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[1] Curtin, Presser, and Singer (2000) cite an increase from 2.1 hours in 1981 to 2.7 in 1996; these 2 years were used for this comparison because the average interview lengths for these two administrations were comparable.

the selected issues in sufficient detail so that analyses can be performed to help explain the phenomena of interest.

This report describes an experiment conducted in NHES:2003 that was designed to examine the effect of different mailings and monetary incentives. The goal of the experiment was to determine if monetary incentives should be used in future surveys, and to identify the effectiveness of various levels of incentives in gaining increased initial cooperation, refusal conversion, and overall unit response rates. The 2003 administration was conducted from January 2 through April 13, 2003. Data were collected using a two stage sampling procedure, where households were selected and then screened for eligible subjects for the topical surveys. Then, within each household, eligible subjects were sampled and topical interviews were conducted about them. In the NHES:2003 Screener, household members were enumerated and demographic and educational information that determined eligibility for the two topical surveys was collected. The NHES:2003 topical surveys were the Parent and Family Involvement in Education Survey (PFI) and the Adult Education for Work-Related Reasons Survey (AEWR). The populations of interest in the surveys were children enrolled in kindergarten through 12th grade for PFI and persons age 16 or older who were not enrolled in grade 12 or below, not institutionalized, and not on active duty in the U.S. armed forces for the adult education survey. The Screener was brief and took an average of 3.5 minutes to complete, while the PFI interview took an average of 24.1 minutes to complete and the adult education interview took an average of 16.8 minutes to complete (data not shown).

The monetary incentive experiment (detailed later) focused primarily on the initial contact, or Screener, because gaining participation at this initial stage is the most difficult component of RDD surveys (Groves and Couper 1996; Maynard and Schaeffer 1997). Table 1 shows the number of completed screener interviews and the response rates for NHES data collections from 1991 to 2003. A graphical depiction of the NHES Screener response rates between 1991 and 2003 is given in Figure 1. The response rates are weighted by the base weights of the sampled telephone numbers to account for differential selection probabilities used in the surveys.

The Screener unit response rates in the first two administrations (1991 and 1993) were more than 80 percent, but then fell in 1995 and 1996. Brick and Collins (1997a) show that much of this decrease is due to changes that increased the length and content of the screening interview. Brick and Collins (1997b) also report on an experiment that confirmed these findings.[2] In 1999, the length and content of the Screener were revised to be more consistent with the earlier surveys and the response rates rose to 74 percent. The Screener response rates for 2001 and 2003 declined despite the fact that the length and content of the Screener were very similar to 1999.

This decrease in Screener response rates occurred despite increases in the levels of effort used in NHES over the years. These increases were in the number of call attempts, the number of refusal conversions, and the use of mailings. The levels of effort for the earlier surveys (1991, 1993, 1995, and 1996) are summarized in Brick and Collins (1997a), Nolin et al. (2000) for the 1999 survey, Hagedorn et al. (2003) for the 2001 survey, and Hagedorn et al. (forthcoming) for the 2003 survey. These reports discuss methods to encourage participation in NHES such as: use of letters, messages left on answering machines, refusal conversion attempts, additional call attempts for different types of outcomes, special interviewer training, and monetary incentives.

In NHES:2001, addresses that could be used for mailing purposes (referred to as mailable addresses) were obtained for the sampled telephone numbers from either of two commercial firms (referred to as a mailable address). Addresses were obtained for about 48 percent of sampled telephone numbers (about 85 percent of known residential telephone numbers in the sample). A first class letter

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[2] The Brick and Collins (1997b) study included a comparison of response rates for a brief Screener containing a "screen-out" question with an expanded Screener that included full enumeration.

explaining the purpose of the survey and encouraging participation in the study was mailed to a random sample of one-half of the households with mailable addresses. The weighted unit response rate for numbers with addresses to which letters were mailed was 75 percent and for those numbers with addresses to which letters were not sent the rate was 70 percent. (By comparison, the unit response rate for numbers without addresses was 55 percent.)

Table 1. Weighted unit response rates and percentage distribution of reasons for unit nonresponse for the NHES Screener: Selected years, 1991–2003

Year of survey	Number of completed screeners	Overall unit response rate (percent)	Reasons for unit nonresponse (percentage distribution)		
			Refusals	Maximum calls[1]	Other nonresponse[2]
1991	60,322	81.0	84	7	9
1993	63,844	82.1	68	15	18[3]
1995	45,465	73.3	84	9	7
1996	55,838	69.9	83	10	7
1999	55,929	74.1	76	17	7
2001	48,385	67.5	74	18	8
2003	32,049	61.7	76	16	8

[1] "Maximum call" cases are those that received at least eight call attempts during which contact was made with a person yet the Screener was not completed.

[2] "Other nonresponse" includes language problems, a portion of residential no-answer and answering machine calls, and other forms of nonresponse.

[3] The NHES:1993 percent of "other nonresponse" cases is higher than that in other surveys. The lower rate of refusals in NHES:1993 and the generally higher response rate are indicative of the fact that less refielding of other nonresponse cases was needed prior to ending data collection with an acceptable Screener response rate.

NOTE: To avoid any differences in rates that might be attributable to the calculation method, all unit response rates given here were calculated using the business office method. The official rates for 2001 and 2003 use the survival method (see appendix A for details on the methods for computing response rates). The screening procedure (i.e., number of household members enumerated) in each data collection differed according to the sample requirements of the topical surveys conducted in the specific year. Percents may not sum to 100 because of rounding.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, Selected years, 1991–2003.

Figure 1.   Weighted unit response rates for the NHES Screener: Selected years, 1991–2003
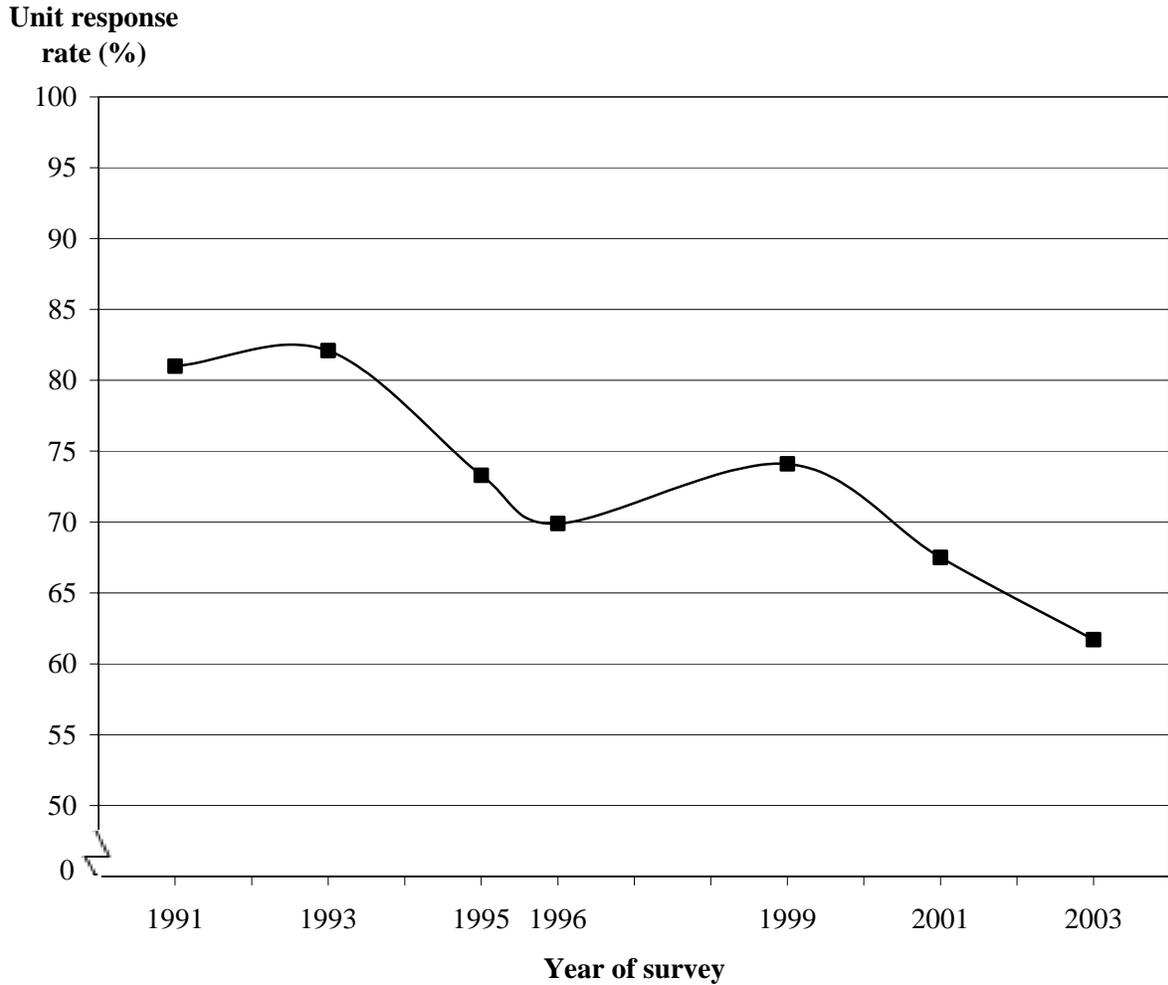
**Unit response
  rate (%)**

Year of survey

NOTE: To avoid any differences in rates that might be attributable to the calculation method, all unit response rates given here were calculated using the business office method. The official rates for 2001 and 2003 use the survival method (see appendix A for details on the methods for computing response rates). The screening procedure (i.e., number of household members enumerated) in each data collection differed according to the sample requirements of the topical surveys conducted in the specific year.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, Selected years, 1991–2003.

The results of the advance mailing in NHES:2001 provided further evidence that sending an advance letter increased Screener response rates in the survey. Previously, Brick and Collins (1997b) reported this result from an experiment conducted in a pretest for NHES:1996. This finding is not consistent with experiments in some other RDD surveys (e.g., Singer, Van Hoewyk, and Maher, 2000) where an advance letter did not increase response rates. The NHES advance letter is sent in an official U.S. Department of Education envelope, which may increase the likelihood that respondents open the mail and give greater legitimacy to the survey, as noted in Dillman (2000). This issue will be addressed later in the analysis of the incentive experiment findings.

A primary focus of the current experiment, the effectiveness of mailings to households that initially refuse to participate, was also examined in the 2001 NHES survey. For the 2001 survey, refusal conversion letters (i.e., letters intended to persuade respondents to change their minds and complete the survey) were sent via FedEx or Priority Mail to 40 percent of the eligible telephone numbers for which addresses were available. Of those that received refusal conversion letters, 55 percent completed the Screener, compared to 38 percent of those that were not sent a letter. This difference must be interpreted with caution, however, as the receipt of a refusal conversion letter is confounded with the availability of address information. In the 2003 NHES experiment discussed in this report, a more systematic approach was taken to examining the effectiveness of sending a letter in increasing the probability of refusal conversion.

In the next section, some of the literature on the use of incentives in surveys is reviewed, concentrating on RDD surveys to the extent possible; the design of the experiment in NHES:2003 is then described. Next the findings of the experiment are presented, including information on the effects of varying incentive and mailing conditions on response rates, costs, and quality. The conclusion provides some discussion of the implications of these findings for future RDD surveys, and NHES in particular.

2. Incentives in Surveys

Incentives have been used as a way of improving response rates in surveys for decades; yet the theory supporting the use of incentives is still not fully established. One theory proposed to explain the effectiveness of incentives is social exchange (Dillman, 2000), which suggests that giving an incentive to a household is considered by the household as an act that must be reciprocated. Responding to the survey is the reciprocal act. Monetary exchange theory is also sometimes invoked to explain responses when incentives are used. This theory suggests the household may be more likely to respond to the survey because the incentive is payment for this response. Groves, Singer, and Corning (2000) suggested a more complex model in which incentives may be used to compensate for other survey characteristics that may diminish likelihood of responding, such as long length (i.e., high respondent burden), content that lacks relevance to a respondent, or sponsorship by an organization that may be unknown to a respondent. Singer (2002) summarized these theories and reviewed the intended and unintended consequences of using incentives in surveys. Some of this literature is reviewed briefly below, focusing primarily on topics relevant to incentives in telephone surveys.

A meta-analysis by Church (1993) shows that the payment of incentives in mail surveys is one method that consistently results in higher response rates. A similar finding was reported in a study by Trussell and Lavrakas (2004). They illustrated that for households that did not have a prior agreement to complete a survey, incremental increases in cash incentives were associated with increased response. Shettle and Mooney (1999) reported on an experiment in using monetary incentives that further supports the cost-effectiveness of incentives in mail surveys that are conducted under the aegis of the U.S. Federal Government. The studies of incentives nearly always find that prepaid incentives (those offered without

requiring the recipient to respond) are more effective than promised incentives that are contingent on response. Berk et al. (1987) found that for an in-person survey prepaid incentives were effective, but promised incentives were not. Church (1993) reported a similar finding for mail surveys. Singer et al. (1999) and Gelman, Stevens, and Chan (2003) use different meta-analysis models for telephone surveys and do not agree that promised incentives are ineffective, but do agree that prepaid incentives are more effective than promised ones.

The evidence on the utility of incentives in RDD surveys is more limited and more recent. One of the reasons for the delay in the use of incentives in telephone surveys is that it is more difficult to send prepaid incentives because addresses cannot be obtained for some sampled telephone numbers. Recently, the proportion of residential telephone numbers that can be linked to addresses using inexpensive methods has increased. This development has made sending advance mailings and prepaid incentives more attractive for RDD surveys. Both Singer et al. (1999) and Gelman, Stevens, and Chan (2003) have found that monetary incentives increase response rates in RDD surveys.

Another way of using incentives is to provide an incentive only to those units that refuse to complete the interview in the initial attempt. However, the published literature on incentives for refusal conversion is limited. Martin, Abreu, and Winters (2001) found that response rates increased when monetary incentives were used to convert refusers in the Survey of Income and Program Participation, a Census Bureau survey. Groves et al. (1999) studied the effect of the differential use of incentives (i.e., only for refusal conversion) and found that most respondents already believed incentives were commonly used by survey organizations. Knowledge of the fact that other respondents were given incentives or different levels of incentives did not have a statistically significant effect on the participation of sampled persons for future surveys. Less is known about the effectiveness of incentives at the refusal conversion stage for RDD surveys. In one published report addressing this issue, Singer, van Hoewyk, and Maher (2000) found that prepaying incentives was effective in increasing survey participation among those who had initially refused to participate.

In addition to the initial contact with an adult in a sampled household, called the Screener interview in NHES, obtaining the cooperation of persons sampled in the Screener for extended interviews is very important. However, no reports on the effectiveness of incentives for improving extended interview response rates exist. Generally, those sampled for extended interviews for NHES topical surveys have been willing to participate at relatively high levels, especially when a parent is responding about a sampled child. Response rates for surveys of adults about their adult education experiences have traditionally been lower. For example, in NHES:2001, the weighted unit response rate for the surveys of parents about their children was 87 percent while the corresponding adult education survey unit response rate was 77 percent (these are rates computed for sampled persons in households that completed the Screener). As a result, there was much interest in the potential of incentives to increase the response rates for the AEWR-NHES:2003 and a test of incentives for this survey was included in the experiment.

3. Screener Experiment

The overall objective of the incentive experiment in 2003 was to determine if there were economical alternatives for improving unit response rates in NHES. In light of the results of studies comparing the effectiveness of prepaid and promised incentives (see section 2), only prepaid incentives were used in this experiment. The experiment focused primarily on the Screener because this is where most nonresponse occurs, but an additional experiment for the adult education survey was also included. The experiment at the Screener level examined the effect of different treatments in the initial contact, at the first refusal conversion stage, and at the second refusal conversion stage. The adult education survey experiment (discussed in the next section) was only at the first refusal conversion stage.

Since advance first class letters had been shown to be effective in improving response rates in previous administrations of the NHES, a first class letter in a U.S. Department of Education envelope and on official stationery was sent to every sampled telephone number for which an address could be identified. Alternatives such as sending the advance letter by Priority Mail were considered to be too costly.

The treatments for the Screener that were subject to manipulation were

- the amount of money included with the advance letter;

- the type of mail used for converting those that refused the initial contact; and,

- the amount of money included with the refusal mailing.

In addition, mailing an official NHES brochure has been considered a possible method of improving response rates. To test this possibility, one of the experimental groups that received no monetary incentive was sent a brochure in the initial mailing.

Two types of mailings were used in this experiment to examine their effects on refusal conversion; these were first class mail sent in U. S. Department of Education letterhead envelopes (with a mailing cost of $0.37 per letter), and Priority Mail sent in U. S. Postal Service Priority Mail envelopes (with a mailing cost of $3.85 per letter). The experimental groups were developed to permit the examination of the following effects on response rates:

- the effect of varied incentives in the advance letter ($0, $2, and $5);

- the effect of varied incentives in the refusal conversion letter ($0, $2, and $5); and

- the effect of Priority Mail versus first class mail for the refusal conversion letter.

Table 2 shows the 10 experimental conditions included in the Screener experiment. The sample sizes for each of the conditions and for the nonmailable[3] cases (which were excluded from the experiment) are also given in the table. Addresses were identified for 59,365 of the telephone numbers in the experiment. Because the experimental treatments all involve mailings, telephone numbers for which an address could not be found were not included in the experiment. The sample also included 50,435 telephone numbers for which addresses could not be identified. The percentage of cases for the experiment is affected by differential sampling of cases with addresses and differential sampling of telephone numbers in areas with a high concentration of minorities. Hagedorn et al. (forthcoming) provide the details on the sample design for NHES:2003. The fact that all numbers cannot be treated will depress the effectiveness of any strategy that involves mailing letters, but the effect is not as great as might be expected. In NHES:2003, 90 percent of all numbers that could be determined to be residential numbers had addresses (85 percent when the numbers are appropriately weighted by the inverse of the probability of selection). For most of this report, the focus is on the cases with addresses that were included in the experiment. Where relevant, cases without addresses are included in tabulations.

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[3] Nonmailable cases are the cases for which an address could not be obtained. These cases were not sent an advance letter or incentive. This subgroup was not treated as a control group for this experiment because there is a considerable literature, including results from previous NHES surveys, that shows that this subgroup has lower response rates in RDD surveys than mailable cases.

Table 2.   Sample sizes for Screener experimental conditions: 2003

Group	Advance letter		Refusal conversion letter plus brochures		Sample size[1]
	Mailing method	Incentive	Mailing method	Incentive	
Total	†	†	†	†	109,800
Total mailable cases	†	†	†	†	59,365
1	First class/brochure	$0	First class	$0	5,765
2	First class	$0	First class	$2	5,700
3	First class	$0	First class	$5	5,700
4	First class	$0	Priority	$0	6,850
5	First class	$0	Priority	$2	5,700
6	First class	$2	First class	$0	6,850
7	First class	$2	Priority	$0	5,700
8	First class	$2	Priority	$2	5,700
9	First class	$5	First class	$0	5,700
10	First class	$2	First class	$2	5,700
Total nonmailable cases	†	†	†	†	50,435

† Not applicable.

[1] For mailable cases, the sample size is the number of telephone numbers for which an address match was obtained, randomly allocated to the experimental conditions. For nonmailable cases, the sample size is the number of telephone numbers.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Group 1 is essentially a control group, but every case in this group contained a treatment that was not included in any other group. Along with the advance letter, the group 1 cases were sent a short color brochure describing NHES to determine the possible effectiveness of a brochure for increasing response rates. The brochure was not included in any other advance mailings. Thus, the experimental design allows for a test of the effect of the brochure in the $0 advance incentive condition on whether potential participants will ever refuse to respond to the Screener survey. A brochure was included with an informational letter if a respondent specifically asked to be mailed more information on the study before agreeing to be interviewed. Refusal conversion mailings included brochures.

The sample sizes in the table are the number of telephone numbers with addresses assigned to each group. Group 4 (no incentive in either the advance letter or the refusal conversion letter, and a refusal conversion letter sent by Priority Mail) is similar to the condition that was used in NHES:2001, although in 2001, FedEx was used rather than Priority Mail.[4] Larger sample sizes were assigned to groups 4 and 6 because they were identified as the treatments that might increase response rates and be acceptable approaches in a federal government survey in that advance incentives have some advantages over refusal conversion incentives.

Once sample sizes for the Screener experimental groups were determined and the cases were randomly assigned to the groups, advance letters were mailed with or without incentives according to the

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[4] In NHES:2001, FedEx was used for refusal letters, with the exception of rural route and Post Office box addresses, to which FedEx cannot be sent. For these addresses, Priority Mail was used.

plan. If the household refused to participate, refusal conversion letters were sent with the appropriate incentives. If a refusal was hostile (profane or abusive), no further efforts were made to contact the household. In addition, a small number of cases that directly contacted the U.S. Department of Education to refuse received no further contact.

For analyses of the effects of the treatments on refusal conversion, the sample sizes in each group are considerably smaller, because only the subset of the telephone numbers with addresses for which Screener refusal conversion is necessary are included. Of the 59,365 telephone numbers in the experiment, only 20,259 cases required and received Screener refusal conversion letters (see table 10 for the total number of cases in each incentive group that ever refused).

If the household refused a second time, they were subject to a second refusal conversion call. To boost response rates, a sample of 75 percent of the households that had not received a Priority Mail letter previously (groups 1, 2, 3, 6, 9, and 10) was sent a Priority Mail letter prior to the second refusal conversion call. Only 75 percent were sent by Priority Mail so that the effectiveness of the special mailing could be evaluated. The sampling to determine which numbers were assigned to the special mailing was done randomly within the experimental groups. The findings section of this report focuses on the initial and first refusal conversion rates to avoid the effect of the Priority Mailing at second refusal. The results of the Priority Mail at the second refusal conversion are then analyzed separately.

An examination of the treatments applied to each experimental group (shown in table 2) reveals that a full factorial design was not used for this experiment. One reason for this is that, while testing each level of advance incentive ($0, $2, and $5), each level of refusal conversion incentive ($0, $2, and $5), and each mailing method for first refusal letters (first class and Priority Mail), the sample sizes in NHES:2003 were not sufficient to support a full factorial design. A second reason is that some treatments in a full factorial design (such as $5 advance incentive, $5 refusal conversion incentive, and Priority Mail for the refusal mailing) were deemed cost-prohibitive for NHES; therefore, even if such a treatment was found to be effective, there is little chance that it would ever be used in NHES. A third reason is that by balancing the experimental sample with respect to prior treatments or conditioning on the prior treatments during analysis, it is possible to minimize confounding effects. Exhibit 1 summarizes the key experimental comparisons that can be made with respect to the Screener treatments used in the experiment.

Table 3 shows the final disposition code for all the telephone numbers included in the experiment. In addition, the numbers without addresses are included for comparison. This table gives an overall assessment of the effects of the various treatment groups. There are essentially five dispositions: an adult in the household completed the Screener interview, the number was determined to be nonresidential, all the calls to the household resulted in either a ring no-answer or an answering machine, the household refused to participate, or the household did not respond for some other reason (such as a language barrier or it was not available during the field period). The percentages in the table are weighted by the household base weight to take account of the differential sampling of minority areas and telephone numbers with addresses. All other analyses in this paper also use the household base weights.

Table 4 gives the mean number of calls required to finalize the experimental Screener cases. This table gives a measure of the workload required on the part of the interviewer in order to finalize a Screener. On average, it required seven calls to finalize a Screener. This mean includes calls made to telephone numbers that were not resolved as residential or nonresidential. The table also gives the mean numbers of call attempts by final disposition code of the Screener. There is significant variation in the mean number of call attempts to finalize a Screener, ranging from three calls for nonresidential numbers to 22 calls for never-answered numbers. For nonmailable cases, an average of six calls were needed to

finalize a Screener, but nearly 60 percent of these numbers were identified as nonworking or nonresidential before any call attempts were made by the interviewers.

Exhibit 1.   Summary of major study comparisons

Experimental process	Characteristic(s)	Aspect(s) compared	Groups compared
Overall	Unit response rate	No monetary incentive vs. any monetary incentive	Groups 1, 4 vs. groups 2, 3, 5, 6, 7, 8, 9, 10
Advance mailing	Ever refused rate	Brochure vs. no brochure	Group 1 vs. groups 2, 3, 4, 5
Advance mailing	Ever refused rate	$0 advance incentive vs. $2 advance incentive vs. $5 advance incentive	Groups 1, 2, 3, 4, 5 vs. groups 6, 7, 8, 10 vs. group 9
Advance mailing	Percent completed for cases that never refused	$0 advance incentive vs. $2 advance incentive vs. $5 advance incentive	Groups 1, 2, 3, 4, 5 vs. groups 6, 7, 8, 10 vs. group 9
First refusal mailing	First refusal conversion rate	$0 refusal conversion incentive vs. $2 refusal conversion incentive[1]	Groups 1, 4, 6, 7 vs. groups 2, 5, 8, 10
First refusal mailing	Unit response rate after first refusal conversion,[2] first refusal conversion rate	Priority Mail vs. first class for first refusal mailing, for groups with the same advance and refusal conversion monetary incentives	Group 1 vs. group 4; group 2 vs. group 5; group 6 vs. group 7; group 8 vs. group 10
Second refusal mailing	Percent completed after second refusal conversion	Priority Mail vs. no mailing for second refusal conversion	Group 1 Priority Mail vs. Group 1 no mailing, and similar comparisons for groups 2, 3, 6, 9, 10

[1] The balance of the design allows for first refusal conversion comparisons without considering the advanced incentive, since half of the four groups with an initial incentive of $0 had a $2 conversion incentive and half of the four groups with an initial incentive of $2 had a $2 conversion incentive, and the sample sizes in each group were approximately equal.

[2] Screeners that were converted on second refusal conversion attempts are counted as nonrespondents in this comparison.

A primary goal of the Screener experiment was to determine if the different incentive and mailing conditions would have variable effects on response rates. Therefore, much of the analysis focuses on whether interviews were completed and related estimates. One of the statistics used is the unit response rate. The definition of the unit response rate used is explained in detail in appendix A, but is briefly described here. The overall unit response rate is the percentage of interviews completed taking all survey stages into account. For the Screener interview, there is only one survey stage. To compute the Screener response rate, it is assumed that 19.7 percent of the sampled numbers classified as never answered were residential.

Table 3. Weighted percentage distributions of final disposition codes for numbers sampled for the Screener, by incentive group: 2003

Incentive group	Sample size[1]	Final disposition				
		Percent complete	Percent nonresidential	Percent never answered	Percent refused	Percent other nonresponse[2]
Total	109,800	—	—	—	—	—
Total mailable cases............	59,365	53.3 (0.20)	21.2 (0.19)	2.0 (0.04)	17.9 (0.17)	5.6 (0.10)
1 – ($0 brochure/1st $0)..	5,765	50.5 (0.65)	21.3 (0.55)	2.2 (0.14)	20.4 (0.51)	5.7 (0.36)
2 – ($0/1st $2).................	5,700	53.0 (0.63)	21.2 (0.51)	2.0 (0.14)	17.8 (0.53)	5.9 (0.34)
3 – ($0/1st $5).................	5,700	54.7 (0.62)	21.3 (0.57)	2.0 (0.13)	16.5 (0.49)	5.5 (0.24)
4 – ($0/Priority $0)..........	6,850	50.0 (0.64)	21.4 (0.54)	2.1 (0.13)	20.7 (0.54)	5.8 (0.30)
5 – ($0/Priority $2)	5,700	52.7 (0.68)	21.0 (0.59)	2.0 (0.13)	18.3 (0.54)	6.0 (0.28)
6 – ($2/1st $0).................	6,850	53.2 (0.54)	21.6 (0.50)	2.0 (0.10)	17.6 (0.46)	5.6 (0.29)
7 – ($2/Priority $0)..........	5,700	54.8 (0.78)	20.5 (0.68)	1.9 (0.12)	17.3 (0.51)	5.5 (0.31)
8 – ($2/Priority $2)..........	5,700	54.2 (0.75)	21.6 (0.61)	1.8 (0.11)	17.1 (0.59)	5.3 (0.29)
9 – ($5/1st $0).................	5,700	54.8 (0.60)	21.3 (0.52)	1.9 (0.15)	16.6 (0.57)	5.3 (0.31)
10 – ($2/1st $2)..............	5,700	55.2 (0.58)	21.0 (0.61)	2.1 (0.12)	16.6 (0.47)	5.1 (0.31)
Total nonmailable cases......	50,435	6.0 (0.12)	87.8 (0.15)	1.7 (0.03)	3.5 (0.09)	1.0 (0.04)

— Not reported.

[1] For mailable cases, the sample size is the number of telephone numbers for which an address match was obtained, randomly allocated to the experimental conditions. For nonmailable cases, the sample size is the total number of telephone numbers; this total includes 30,725 telephone numbers that were identified as business or nonworking numbers through pre-field procedures and were never dialed.

[2] *Other nonresponse* includes cases that finalized due to language problems, the inability to make contact with the respondent (maximum call status), or the respondent being unavailable during the field period.

NOTE: Standard error shown in parenthesis. Percents may not sum to 100 because of rounding. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The Priority Mail letters were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. Final dispositions include the results of the second refusal conversion attempts.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 4. **Mean number of Screener call attempts for mailable cases and for nonmailable cases, by final disposition: 2003**

Final Screener disposition	Sample size[1]	Mean screening call attempts	
Total	79,075		—
Total mailable cases..........................	59,365	7.0	(0.04)
Completed....................................	29,196	4.7	(0.03)
Nonresidential...............................	12,259	3.4	(0.04)
Never answered..............................	3,385	22.3	(0.10)
Refused	10,102	12.9	(0.09)
Other nonresponse[2]	4,423	18.7	(0.18)
Total nonmailable cases...................	19,710	5.9	(0.05)
Completed....................................	2,853	5.5	(0.09)
Nonresidential...............................	9,968	3.2	(0.03)
Never answered..............................	4,730	16.8	(0.07)
Refused	1,659	13.8	(0.18)
Other nonresponse[2]	500	19.9	(0.41)

— Not reported.

[1] For mailable cases, the sample size is the number of telephone numbers for which an address match was obtained, randomly allocated to the experimental conditions. For nonmailable cases, the sample size of 19,710 is the total number of telephone numbers that were ever dialed; this total excludes 30,725 telephone numbers that were identified as business or nonworking numbers through pre-field procedures and were never dialed. These pre-field procedures only identify a subset of business and nonworking numbers.

[2] *Other nonresponse* includes cases that finalized due to language problems, the inability to make contact with the respondent (maximum call status), or the respondent being unavailable during the field period.

NOTE: Detail may not sum to totals because of rounding.

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

For some analyses the unit response rates are not the most relevant measures. A reason noted earlier for focusing on other statistics, in addition to response rates, is that the response rates include the effect of the Priority Mailing at second refusal conversion. This mailing was differential and not part of the main Screener incentive experiment. It only affected groups without any previous Priority Mailing (groups 1, 2, 3, 6, 9, and 10). A second reason that other statistics may be more pertinent is that experimental groups (e.g., all groups sent a $2 incentive in the advance mailing) are sometimes combined to increase the precision of the estimates of the effect of the treatment. Because the combined groups have different treatments at different stages of refusal conversion, the response rates for the groups confound the treatment effects.

Instead of response rates, either "ever refusal" rates or the percentage of cases that complete the interview at a specific stage of the process are often provided. Ever refusal rates are used primarily to evaluate the effect of a treatment in the first stage, prior to any refusal conversion. Ever refusal rates give the (weighted) number of cases in the group that refused the Screener divided by the total weighted number of cases that either completed or refused the Screener. The nonresponse cases that were never classified as refusals (such as those with language problems or that are not available in the field period) are not included in the computations of this rate. The ever refusal rate is the complement of the rate that

AAPOR (2004) calls the cooperation rate. The term "ever refusal rate" is preferred because cooperation and response rates are often confused.

In addition to measures related to improving response rates, estimates of the number of call attempts required to finalize the case or to complete a specific phase of the interview are produced. For example, table 4 provides the mean numbers of call attempts by the final disposition of the Screener. In other tables, statistics such as the mean numbers of call attempts needed to move from an initial refusal to a second refusal by experimental group are given. These numbers are used as an indication of whether there are obvious cost issues associated with an experimental group beyond the direct costs of the mailings and incentives.

The results from the Screener experiment are given below, and the adult education survey experiment is discussed in the next section. All of the analyses were done with the appropriate survey weights using WesVar software (Westat 2000) to calculate jackknife variance estimates. Tests of hypotheses were done using two-tailed *t*-tests with a 5 percent type I error rate. Bonferroni adjustments were used to compensate for multiple comparisons to ensure that the overall significance level associated with all of the comparisons remained at or below the specified level of 5 percent. Given the problem of falling response rates, relatively small increases in response rates associated with a particular experimental condition are considered noteworthy in this paper. Differences that are not statistically significant and statistically significant differences of less than one percentage point are not discussed.

Table 5 gives the Screener response rates and ever refusal rates for each of the 10 treatment groups. Since nonresidential telephone numbers are not used in computing response rates, the 30,725 nonresidential numbers identified through prefield procedures, the 12,259 nonresidential mailable numbers, and the 9,968 nonresidential nonmailable numbers (see table 4) are excluded, and the estimates in the table are based on the remaining 47,106 mailable and 9,742 nonmailable numbers. The corresponding rates for the cases without addresses are also presented for comparison purposes. The response rates in this table and the percentage of sampled cases that were completed given in table 3 highlight the efficiency considerations that led to oversampling telephone numbers with addresses in the survey. Figure 2 contains a graphical display of the Screener response rates and ever refusal rates by incentive group.

Advance Mailing

The first topic considered is the effect of including the brochure in the advance mailing. This is the starting point because if the brochure does improve cooperation, then it would not be useful to combine group 1 with other groups that have no monetary incentive in the advance mailing. If including the brochure were effective, the ever refusal rate for group 1 should be lower than the ever refusal rates for the other groups with no money in the advance letter (groups 2, 3, 4, and 5). The results in table 6 indicate that the inclusion of the brochure in the advance letter does not increase cooperation at the initial stage of the process. In fact, the initial cooperation rate for the brochure group (group 1) was lower than the rate for the other groups with no advance incentive combined.

Table 5 illustrates that the ever refusal rates show the type of pattern expected, with rates for those with no incentive higher than those with $2 or $5 in the advance letter. The size of the differences by incentive amount is discussed in connection with the next table (table 6). Table 5 also gives the response rates for the 10 experimental groups. Keeping in mind the caveat given earlier about the potential effects of the second refusal conversion Priority Mailing (sent to subsamples of groups 1, 2, 3, 6, 9, and 10), these rates are examined. The unit response rates for the two groups with no incentive (groups 1 and 4) are generally lower than the rates for the other groups. Ordering the groups from those sent no monetary

incentive to those sent either $4 or $5 (groups 8, 9, and 10), it can be seen that the higher incentives do result in higher response rates (table 5), but with diminishing returns. Figure 3 illustrates this trend. The effect of the refusal conversion mailings is discussed later.

Table 5.   Screener response rates and ever refusal rates, by incentive group: 2003

Incentive group	Sample size[1]	Response rate, percent		Ever refused rate, percent	
Total	56,848	—		—	
Total mailable cases	47,106	—		—	
1 - ($0 brochure/1st $0)........	4,574	64.1	(0.67)	48.8	(0.76)
2 - ($0 /1st $2)......................	4,524	67.3	(0.71)	47.1	(0.84)
3 - ($0 /1st $5)......................	4,518	69.5	(0.64)	45.4	(0.74)
4 - ($0 /Priority $0)	5,422	63.7	(0.69)	46.6	(0.69)
5 - ($0 /Priority $2)	4,543	66.7	(0.71)	45.2	(0.83)
6 - ($2 /1st $0)......................	5,424	67.9	(0.67)	40.9	(0.67)
7 - ($2 /Priority $0)	4,558	68.9	(0.72)	42.1	(0.69)
8 - ($2 /Priority $2)	4,498	69.1	(0.76)	42.4	(0.81)
9 - ($5 /1st $0)......................	4,505	69.7	(0.70)	38.9	(0.76)
10 - ($2 /1st $2)....................	4,540	69.9	(0.56)	42.1	(0.70)
Total nonmailable cases	9,742	49.3	(0.59)	55.9	(0.64)

— Not reported.

[1] The sample size is the number of sampled telephone numbers excluding those numbers that were classified as nonresidential.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. Nonresidential telephone numbers are excluded from the table. The sample sizes are unweighted. The rates are calculated after weighting cases in the sample for differential probabilities of selection; therefore, rates cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Figure 2.   Screener response rates and ever refusal rates, by incentive group: 2003

Incentive group

NOTE: The incentive group descriptions give the advance mailing condition before the / mark and the initial refusal condition after the / mark. After the / mark, 1st means the initial refusal materials were sent by first class mail. After the / mark, Priority means the initial refusal materials were sent by Priority Mail. All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions specifying first class mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 6 combines groups with a similar advance mailing treatment. group 1 is included with the other groups with no incentive. The ever refusal rates are the most important rates in this table since the response rates combine groups with different subsequent treatments. The ever refusal rates for the three groups differ, with the highest rates associated with no incentive and the lowest rates for the $5 incentive group.[1] One way of thinking about the relationship is that adding the $2 incentive reduces the ever refusal rate by 2.4 percentage points for each dollar relative to no incentive, while adding $5 reduces the refusal rate by 1.5 percentage points for each dollar relative to no incentive. Thus, while the larger incentive is more effective in the experiment, the relative effectiveness decreases when more money is sent, at least for the dollar values considered here.

Table 6.   Screener response rates and ever refusal rates, by advance letter incentive: 2003

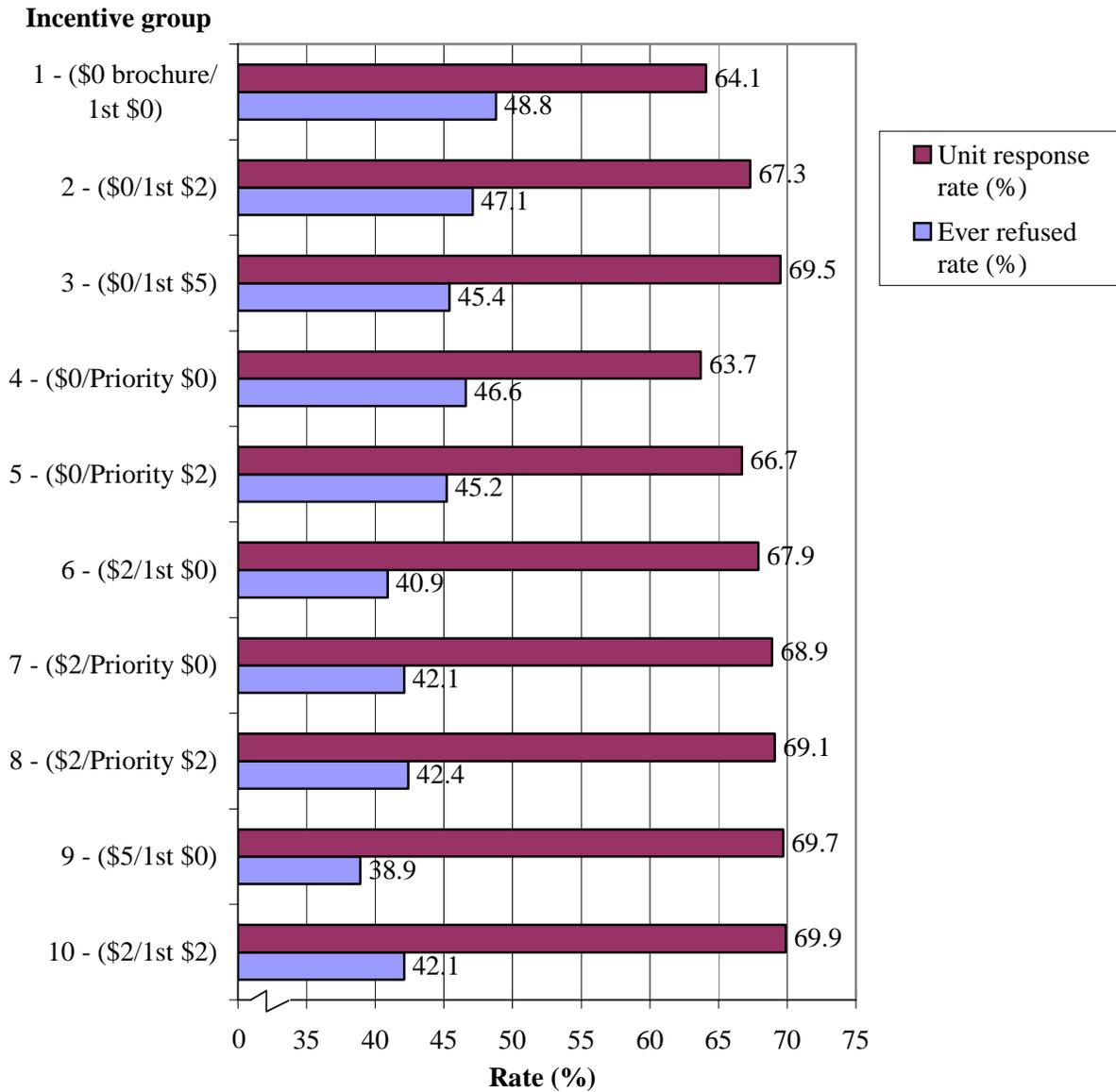| Advance letter incentive	Sample size[1]	Response rate, percent	Ever refused rate, percent
$0	23,581	66.1 (0.31)	46.6 (0.30)
$2	19,020	68.9 (0.36)	41.8 (0.37)
$5	4,505	69.7 (0.70)	38.9 (0.76)

[1] The sample size is the number of sampled telephone numbers excluding those numbers that were classified as nonresidential.
NOTE: Standard error shown in parenthesis. The sample sizes are unweighted. The rates are calculated after weighting cases in the sample for differential probabilities of selection; therefore, rates cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Refusal Conversion

In the previous section, advance mailing conditions were evaluated. Here, treatments involving experimental conditions for Screener refusal conversion are considered. These treatments (refusal conversion mailings and possible further monetary incentives) were applied only if a household refused to complete the Screener. Before discussing the effects of the refusal conversion treatments, it is important to remember that most households that completed the Screener never refused (table 7). To help put refusal conversion strategies into perspective, analysis is first conducted to show how cases that never refused behaved by the kind of incentive they received in the advance mailing (table 8). Next, cases that ever refused are studied (table 9). It should be kept in mind that cases that ever refused include those that refused more than once.

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[1] The ever refusal rate higher for the brochure group than 2 of the other 4 groups that received no initial monetary incentives. However, all groups that received some initial monetary incentive (6-10) had lower ever refusal rates than all groups that received no initial monetary incentives (1-5). For this reason, groups that received no initial monetary incentives were combined to facilitate comparisons.

Figure 3.   Screener response rates by total incentive amount and incentive group: 2003

Incentive group

Incentive group	Unit response rate (%)
$0 (Group 1)	64.1
$0 (Group 4)	63.7
$2 (Group 2)	67.3
$2 (Group 5)	66.7
$2 (Group 6)	67.9
$2 (Group 7)	68.9
$4 (Group 8)	69.1
$4 (Group 10)	69.9
$5 (Group 3)	69.5
$5 (Group 9)	69.7

Unit response rate (%)

NOTE: All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 7. Percentage distribution of Screener completes for mailable cases that completed the Screener, by refusal conversion stage: 2003

Stage	Sample size[1]	Percentage distribution
Total	29,196	100.0 (†)
Never refused	20,501	70.3 (0.28)
First refusal conversion	6,740	23.1 (0.27)
Second refusal conversion	1,955	6.7 (0.14)

† Not applicable.
[1] The sample size is the number of mailable telephone numbers with completed Screeners.
NOTE: Nonresidential telephone numbers are excluded from the table. Percents may not sum to 100 because of rounding. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.
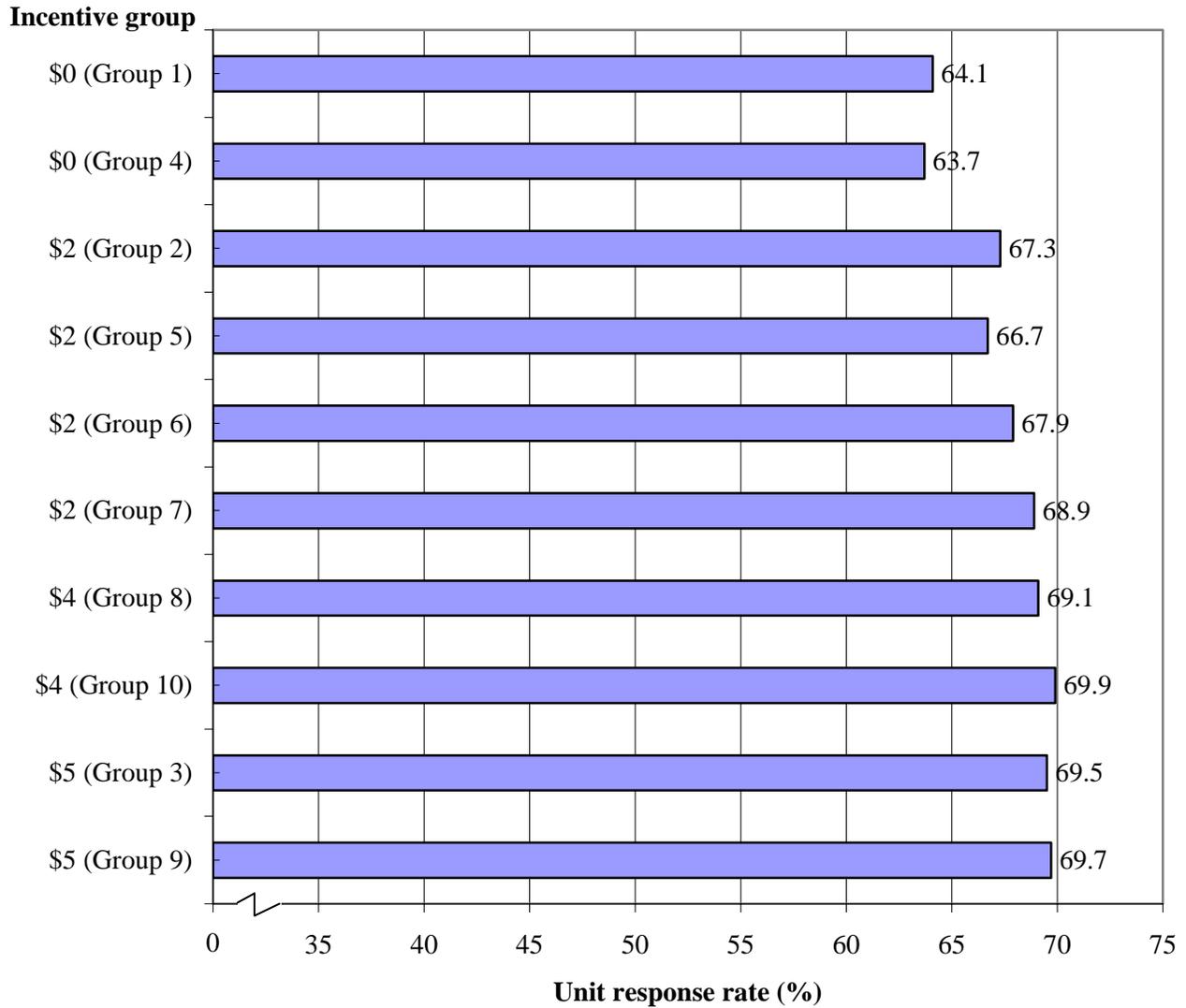
Table 8 shows that, among cases that never refused to complete the Screener, the percent that completed the Screener was lower for cases that received no incentive in the advance mailing than it was for cases that did. The cases that did not complete the Screener could be nonresidential cases that never answered the phone, or other nonresponse.[2]

Table 8. Percentage of cases with completed Screeners that never refused, by advance letter incentive: 2003

Advance letter incentive	Sample size[1]	Percent completed	Mean total calls[2]	Mean calls to complete[2]
$0	18,927	57.0 (0.37)	5.5 (0.06)	3.8 (0.05)
$2	16,158	60.2 (0.41)	5.2 (0.05)	3.7 (0.04)
$5	3,955	61.7 (0.73)	4.9 (0.12)	3.6 (0.11)

[1] Sample size is the number of telephone numbers at which a refusal was never received, minus numbers identified as nonresidential prior to calling (i.e., through prefield activities).
[2] The *total calls* estimates include calls to all telephone numbers in the subgroup, while the *calls to complete* estimates include calls to only the telephone numbers for which completed Screeners were obtained.
NOTE: Standard error shown in parenthesis. The $0 advance letter incentive category combines incentive groups 1, 2, 3, 4, and 5. The $2 advance letter incentive category combines incentive groups 6, 7, 8, and 10. The $5 advance letter incentive category is incentive group 9. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

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[2] There were 16,731 nonmailable cases that never refused and 15.6 percent of these completed the Screener (s.e. = 0.32 percent). The mean numbers of calls for these cases were 4.6 for all cases and 4.3 for completed cases (with standard errors of 0.04 and 0.11, respectively).

Table 9 includes all cases that ever refused to complete the Screener; thus, it excludes all the cases shown in table 8. The percentage completed (those cases that eventually completed a Screener) is an overall measure of the joint effectiveness of the first and second refusal conversion attempts. Several of the groups with larger completion rates in the table are those that had Priority Mailings that were sent to 75 percent of the second refusals (groups 1, 2, 3, 6, 9, and 10 [data not shown]). Because of the confounding of the two refusal conversion treatments, these results are analyzed separately.

Table 9.   Percentage of cases that ever refused to complete the Screener and final disposition, by incentive group: 2003

Incentive group	Sample size[1]	Final disposition	
		Percent completed	Percent refused
Total	23,230	—	—
Total mailable cases	20,259	—	—
1 - ($0 brochure/1st $0)	2,166	42.9 (1.17)	50.6 (1.05)
2 - ($0 /1st $2)	2,088	47.2 (1.14)	45.6 (1.08)
3 - ($0 /1st $5)	2,023	49.1 (1.16)	43.8 (1.15)
4 - ($0 /Priority $0)	2,486	39.5 (1.17)	53.7 (1.16)
5 - ($0 /Priority $2)	2,007	44.2 (1.16)	48.8 (1.18)
6 - ($2 /1st $0)	2,188	40.0 (1.07)	52.0 (1.08)
7 - ($2 /Priority $0)	1,850	44.3 (1.25)	49.7 (1.25)
8 - ($2 /Priority $2)	1,839	44.8 (1.28)	49.3 (1.38)
9 - ($5 /1st $0)	1,734	40.8 (1.32)	51.7 (1.25)
10 - ($2 /1st $2)	1,878	45.1 (1.08)	47.3 (1.23)
Total nonmailable cases	2,971	28.2 (0.89)	55.9 (1.02)

— Not reported.
[1] Sample size is the number of telephone numbers at which a refusal was received.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. Details do not sum to 100 percent because of the exclusion of the "Other nonresponse" category. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 10 gives the mean number of calls needed to reach the final disposition status for Screener refusal conversion cases by the experimental groups. The values in the table show little variation by experimental group in the mean total calls, the mean calls to obtain a completed Screener, and the mean calls to obtain a final refusal. Table 11 gives more details on the mean number of call attempts, dividing the total call attempts into those used to obtain the first refusal and those used to finalize the initial refusal case as either a completed Screener or final refusal. Even with the added detail, the mean numbers of calls generally did not vary across experimental groups.

Table 10.   Mean number of calls to final status for Screeners that ever refused, by incentive group: 2003

Incentive group	Sample size[1]	Mean total calls		Mean calls to complete		Mean calls to refusal	
1 - ($0 brochure /1st $0).........	2,166	10.1	(0.17)	7.4	(0.18)	12.5	(0.24)
2 - ($0 /1st $2).......................	2,088	10.0	(0.20)	7.1	(0.20)	13.0	(0.31)
3 - ($0 /1st $5)......................	2,023	9.8	(0.18)	6.8	(0.17)	13.2	(0.26)
4 - ($0 /Priority $0)	2,486	10.1	(0.15)	7.0	(0.16)	12.5	(0.23)
5 - ($0 /Priority $2)	2,007	10.3	(0.20)	7.2	(0.18)	13.1	(0.32)
6 - ($2 /1st $0)......................	2,188	10.0	(0.18)	7.0	(0.19)	12.7	(0.24)
7 - ($2 /Priority $0)	1,850	10.0	(0.19)	7.1	(0.18)	12.6	(0.29)
8 - ($2 /Priority $2)	1,839	10.1	(0.21)	6.8	(0.18)	13.3	(0.33)
9 - ($5 /1st $0)......................	1,734	10.5	(0.18)	7.0	(0.22)	13.4	(0.28)
10 - ($2 /1st $2).....................	1,878	10.1	(0.19)	7.0	(0.20)	13.1	(0.30)

[1] Sample size is the number of telephone numbers at which a refusal was received.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 11.   Mean number of calls for initial refusal Screener cases, by outcome of call and incentive group: 2003

Incentive group	Sample size[1]	Mean number of calls		
		To initial refusal	From refusal to complete	From first to second refusal
1 - ($0 brochure/1st $0)........	2,166	3.9 (0.10)	4.1 (0.12)	3.3 (0.10)
2 - ($0 /1st $2).....................	2,088	3.9 (0.11)	3.9 (0.14)	3.7 (0.13)
3 - ($0 /1st $5).....................	2,023	3.8 (0.10)	3.7 (0.12)	3.5 (0.11)
4 - ($0 /Priority $0)	2,486	3.8 (0.08)	3.8 (0.11)	3.5 (0.10)
5 - ($0 /Priority $2)	2,007	4.0 (0.12)	4.0 (0.13)	3.6 (0.12)
6 - ($2 /1st $0).....................	2,188	3.7 (0.10)	4.0 (0.12)	3.6 (0.12)
7 - ($2 /Priority $0)	1,850	3.9 (0.10)	3.9 (0.14)	3.7 (0.13)
8 - ($2 /Priority $2)	1,839	4.0 (0.12)	3.6 (0.13)	3.6 (0.12)
9 - ($5 /1st $0).....................	1,734	3.9 (0.10)	3.9 (0.16)	3.7 (0.14)
10 - ($2 /1st $2)...................	1,878	4.0 (0.12)	3.7 (0.12)	3.6 (0.12)

[1] Sample size is the number of telephone numbers at which a refusal was received.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 12 shows the percentage of cases that refused that were completed after the first refusal efforts (called the first refusal conversion rate). These rates vary among the experimental groups from 29 percent to 39 percent. If the use of Priority Mail at this stage is ignored, the 10 incentive groups can be classified into two groups that can be compared by the amount of incentive given in the conversion letter ($0 or $2). The mean first conversion rate for those in the $0 group (groups 1, 4, 6, 7) is 31.8 percent (s.e. = 0.58 percent), and the mean for those in the $2 group (Groups 2, 5, 8, 10) is 35.7 percent (s.e. = 0.54 percent). The groups with $2 conversion incentives have significantly higher first conversion rates than the groups with $0 conversion incentives (Groups 1, 4, 6, 7, and 9). While this analysis does not explicitly account for the incentive amount in the initial mailing, the balance of the design supports the conclusion that these results are the effect of the refusal conversion incentive amount.[3] "Balance" refers to the fact that, excluding incentive groups 3 and 9, half of the four groups with an initial incentive of $0 had a $2 conversion incentive and half of the four groups with an initial incentive of $2 had a $2 conversion incentive.

Table 12.   Screener response rates after first conversion attempt, by incentive group: 2003

Incentive group	Response rate after first refusal conversion[1]			First refusal conversion rate		
	Sample size[2]		Percent	Sample size[3]		Percent
Total	56,848		—	23,230		—
Total mailable cases	47,106		—	20,259		—
1 - ($0 brochure/1st $0)	4,574	57.9	(0.78)	2,166	30.7	(1.20)
2 - ($0 /1st $2)	4,524	61.9	(0.71)	2,088	36.3	(1.05)
3 - ($0 /1st $5)	4,518	64.6	(0.76)	2,023	38.8	(1.23)
4 - ($0 /Priority $0)	5,422	59.9	(0.70)	2,486	31.8	(1.11)
5 - ($0 /Priority $2)	4,543	62.4	(0.68)	2,007	35.1	(1.06)
6 - ($2 /1st $0)	5,424	63.4	(0.69)	2,188	29.5	(1.06)
7 - ($2 /Priority $0)	4,558	65.0	(0.78)	1,850	35.1	(1.26)
8 - ($2 /Priority $2)	4,498	65.6	(0.73)	1,839	36.7	(1.10)
9 - ($5 /1st $0)	4,505	65.3	(0.74)	1,734	29.9	(1.30)
10 - ($2 /1st $2)	4,540	65.4	(0.64)	1,878	34.9	(1.10)
Total nonmailable cases	9,742	45.6	(0.63)	2,971	21.1	(0.85)

[1] Screeners that were converted on second refusal conversion attempts are counted as nonrespondents in this table.

[2] For mailable cases, the sample size is the number of telephone numbers assigned to each experimental condition, minus numbers identified as nonresidential. For nonmailable cases, the sample size is the total number of telephone numbers, minus numbers identified as nonresidential.

[3] Sample size is the number of telephone numbers at which a refusal was received.

NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. The sample sizes are unweighted. The rates are calculated after weighting cases in the sample for differential probabilities of selection; therefore, rates cannot be calculated directly from the sample sizes presented. SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

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[3] A regression analysis, discussed in appendix B, also supports this conclusion.

Table 12 also shows the response rates computed after the first refusal conversion. (If a Screener was completed in the second refusal conversion attempt, it is classified as a refusal for this table in order to assess the results of the initial refusal treatment.) This table provides support for the combined effects of the advance letter mailing treatments and the first refusal conversion treatments, that is, without the effect of the second refusal conversion treatment. An examination of the response rates by group given in table 12 reveals many of the same patterns discussed earlier in conjunction with table 6. The two groups that did not include any payment generally have the lowest response rates.[4] The response rates for groups 3, 7, 8, 9, and 10 are all close to 65 percent. Of these five groups, only group 7 involved sending the household less than $4 and group 7 had a Priority Mailing at refusal conversion. These findings are consistent with previous research in that they show these monetary incentives improve response rates and that higher levels of payment ($4 and $5 are still very modest payments) are typically more effective than $2.

Priority Mail

Refusing households were sent a refusal conversion letter using Priority Mail after they initially refused the screener in several of the groups with the hope that Priority Mail would distinguish the letter from other types of mail and convey the importance attached to responding to the survey. If a household refused twice and was in a group that had no previous Priority Mailing (groups 1, 2, 3, 6, 9, and 10), then 75 percent of the households in the group were randomly sampled and these households were sent a third mailing using Priority Mail. The remaining 25 percent (like the households in the other groups) were not sent any mailing. A discussion of the effectiveness of the initial refusal Priority Mailing is provided below, followed by an analysis of the second refusal conversion Priority Mailing in NHES:2003.

The response rates in table 12 can be used to examine the effectiveness of Priority Mail in the first refusal conversion attempt. For example, groups 2 and 5 have the same advance mailing treatment ($0 in a first class mail) and the same monetary incentive ($2) for the refusal conversion attempt. They differ only in that the refusal conversion letter for group 2 was sent by first class while for group 5 it was sent by Priority Mail. The four valid comparisons are: group 1 versus group 4, group 2 versus group 5, group 6 versus group 7, and group 8 versus group 10. No differences in unit response rates for these contrasts were detected. Thus, the experiment shows that using Priority Mail in the initial refusal conversion letter does not improve response rates in NHES, holding the other conditions constant.

Priority Mail was used for the second refusal conversion in order to examine whether its use improved the conversion rate. At the second refusal conversion stage, letters were sent by Priority Mail to a 75 percent random sample of cases in the groups that had not previously been sent Priority Mail. Those that had received a letter by Priority Mail during initial refusal conversion were not sent another letter at this stage. Table 13 shows the percentage of the second refusal conversions that were completed for each of the six groups by whether the case was in the 75 percent sample sent a Priority Mail letter. Among the six groups, the maximum difference in the completion percentage for households sent the Priority Mail letter and those not sent any letter was 15 percentage points. When the treated groups are aggregated into one category, the difference in the completion percentage between those sent a letter by Priority Mail and those not sent any mail in the second refusal conversion is statistically significant.

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[4] There was no detectable difference in the rate for group 4 compared to groups 2 and 5 after adjusting for multiple comparisons.

Table 13.   Percentage of cases that completed Screeners after second refusal conversion, by Priority Mail and incentive group: 2003

Incentive group	No Priority Mail			Priority Mail		
	Sample size[1]		Percent	Sample size[1]		Percent
1 - ($0 brochure/1st $0)............	286	10.5	(1.77)	915	25.2	(1.31)
2 - ($0 /1st $2)..........................	247	18.9	(2.87)	772	23.1	(1.75)
3 - ($0 /1st $5)..........................	241	16.8	(2.71)	694	23.7	(1.54)
6 - ($2 /1st $0)..........................	303	13.3	(1.97)	877	20.8	(1.51)
9 - ($5 /1st $0)..........................	214	15.6	(2.88)	710	21.1	(1.55)
10 - ($2 /1st $2)........................	215	16.9	(2.78)	714	21.4	(1.74)
Total nonmailable cases...........	1,695	12.4	(0.83)	†		†

† Not applicable.
[1] The sample size is the number of sampled telephone numbers that refused two times.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Thus, sending a priority letter after a second Screener refusal conversion does improve response rates in NHES, but the difference is not uniformly large and depends on previous treatments. Also, notice that the difference is between sending a Priority Mail letter and sending no mail, where in all the other situations considered the differences were between two different types of mailings. The data do not indicate whether sending Priority Mail at this refusal conversion stage would be more effective than sending a first class letter.[5] Since only about 7 percent of the completes for those with an address match are obtained in the second refusal conversion attempt, the evidence suggests that using Priority Mail at the second refusal conversion if there are no previous Priority Mailings seems likely to improve response rates, but not by very much. The primary reason the improvement in response rates is expected to be small is that the subgroup of cases who refuse twice and are mailable is small; additionally, the differences in second refusal conversion rates among all of the experimental groups, though statistically significant in some cases, are small. Because no first class letters were sent at the second refusal stage, it is not possible to ascertain whether a first class mailing would have affected the conversion rates in the same way.

Tables 14 and 15 present more data on the dispositions and mean number of calls for Screener cases after second refusal conversion attempts, showing all the experimental groups. Table 14 shows the percentage of the second refusal cases that were completed and refused for each group (a small percentage fell into other disposition categories and are not shown). When considering the results in table 14, it is important to keep in mind that Priority Mail letters were sent after the second refusal only to households in those incentive groups that had not previously been sent a Priority Mail letter. Although some of the groups do have statistically significantly different percentages completed, none of the

[5] Since all households with mailable addresses had already been sent at least a first class letter, it was believed that after the second refusal, it would generally be necessary to do something more to capture their attention, and the use of Priority Mail was the "attention-getter" that was tested.

differences in the table would have a large effect on overall response rates. The mean number of calls given in table 15 are not statistically different and do not suggest any major differences in the level of effort for the second refusal Screener cases.

Table 14.   Percentage of Screeners that refused twice, by final disposition and incentive group: 2003

Incentive group	Sample size[1]	Percent completed		Percent refused	
1 - ($0 brochure/1st $0).............	1,245	21.2	(1.10)	74.7	(1.16)
2 - ($0 /1st $2)..........................	1,062	21.5	(1.41)	73.9	(1.59)
3 - ($0 /1st $5)..........................	982	21.4	(1.26)	75.3	(1.35)
4 - ($0 /Priority $0)	1,349	14.0	(1.08)	83.2	(1.18)
5 - ($0 /Priority $2)	1,019	18.2	(1.15)	79.6	(1.26)
6 - ($2 /1st $0)..........................	1,228	18.7	(1.22)	78.2	(1.30)
7 - ($2 /Priority $0)	977	17.1	(1.30)	80.3	(1.20)
8 - ($2 /Priority $2)	909	16.3	(1.42)	81.1	(1.59)
9 - ($5 /1st $0)..........................	959	19.3	(1.37)	76.3	(1.53)
10 - ($2 /1st $2)........................	970	19.8	(1.39)	76.3	(1.40)

[1] Sample size includes telephone numbers at which two refusals were received.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. groups 1, 2, 3, 6, 9, and 10 were sent a Priority Mail letter prior to second refusal conversion efforts. Details do not sum to 100 percent because of the exclusion of the "Other nonresponse" category. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 15.   Mean number of calls between refusals for second Screener refusal cases, by incentive group: 2003

		Mean number of calls		
Incentive group	Sample size[1]	From first to second refusal	From second refusal to complete	From second to final refusal
1 - ($0 brochure/1st $0)........	1,245	3.3 (0.10)	2.5 (0.10)	3.2 (0.08)
2 - ($0 /1st $2).....................	1,062	3.7 (0.13)	2.6 (0.12)	3.2 (0.10)
3 - ($0 /1st $5).....................	982	3.5 (0.11)	2.5 (0.14)	3.4 (0.11)
4 - ($0 /Priority $0)	1,349	3.5 (0.10)	2.5 (0.15)	3.2 (0.10)
5 - ($0 /Priority $2)	1,019	3.6 (0.12)	2.7 (0.16)	3.2 (0.10)
6 - ($2 /1st $0).....................	1,228	3.6 (0.12)	2.3 (0.11)	3.4 (0.08)
7 - ($2 /Priority $0)	977	3.7 (0.13)	2.4 (0.15)	3.2 (0.10)
8 - ($2 /Priority $2)	909	3.6 (0.12)	2.5 (0.15)	3.2 (0.11)
9 - ($5 /1st $0).....................	959	3.7 (0.14)	2.4 (0.15)	3.3 (0.09)
10 - ($2 /1st $2).....................	970	3.6 (0.12)	2.5 (0.14)	3.3 (0.11)

[1] Sample size includes telephone numbers at which two refusals were received.
NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Carryover Effect of Screener Treatments on Extended Interview Cooperation

The last issue to be considered in connection with the Screener experiment is whether the treatments at the Screener level influenced cooperation at the extended interview level for PFI or AEWR. Singer, Groves, and Corning (1999) discuss the hypothesis that once an incentive has been paid, it continues to have an effect on the respondents for other survey-related tasks such as completing the extended interview.

To examine the carryover effect the Screener incentive has on extended interview cooperation, table 16 shows the percentage of PFI and AEWR sampled persons that completed the extended interview for each of the Screener experimental groups and for cases without addresses. The percent complete for the PFI are more informative because the adult education survey percent completes are confounded with the experimental treatments applied in the adult education survey sample. The unit response rates for PFI vary somewhat by the experimental group, from a low of 80 percent for group 5 to a high of 89 percent for group 8. The adult education survey unit response rates are not measurably different; they are all within 5 percentage points of each other. When the groups are ranked by the percentage completed, the groups with higher extended unit response rates are predominately those groups in which the household received greater incentives, but the pattern is not consistent. See figure 4 for an illustration. For example, group 6 has the third lowest percentage completed for PFI and the third highest percentage completed for adult education survey. The relatively small differences and lack of a pattern relative to the payment amount across most of the groups does not lend much support to the hypothesis that incentives at the Screener will result in significantly higher unit response rates for the extended interview.

Table 16.   Percentage of completed PFI and AEWR interviews, by incentive group: 2003

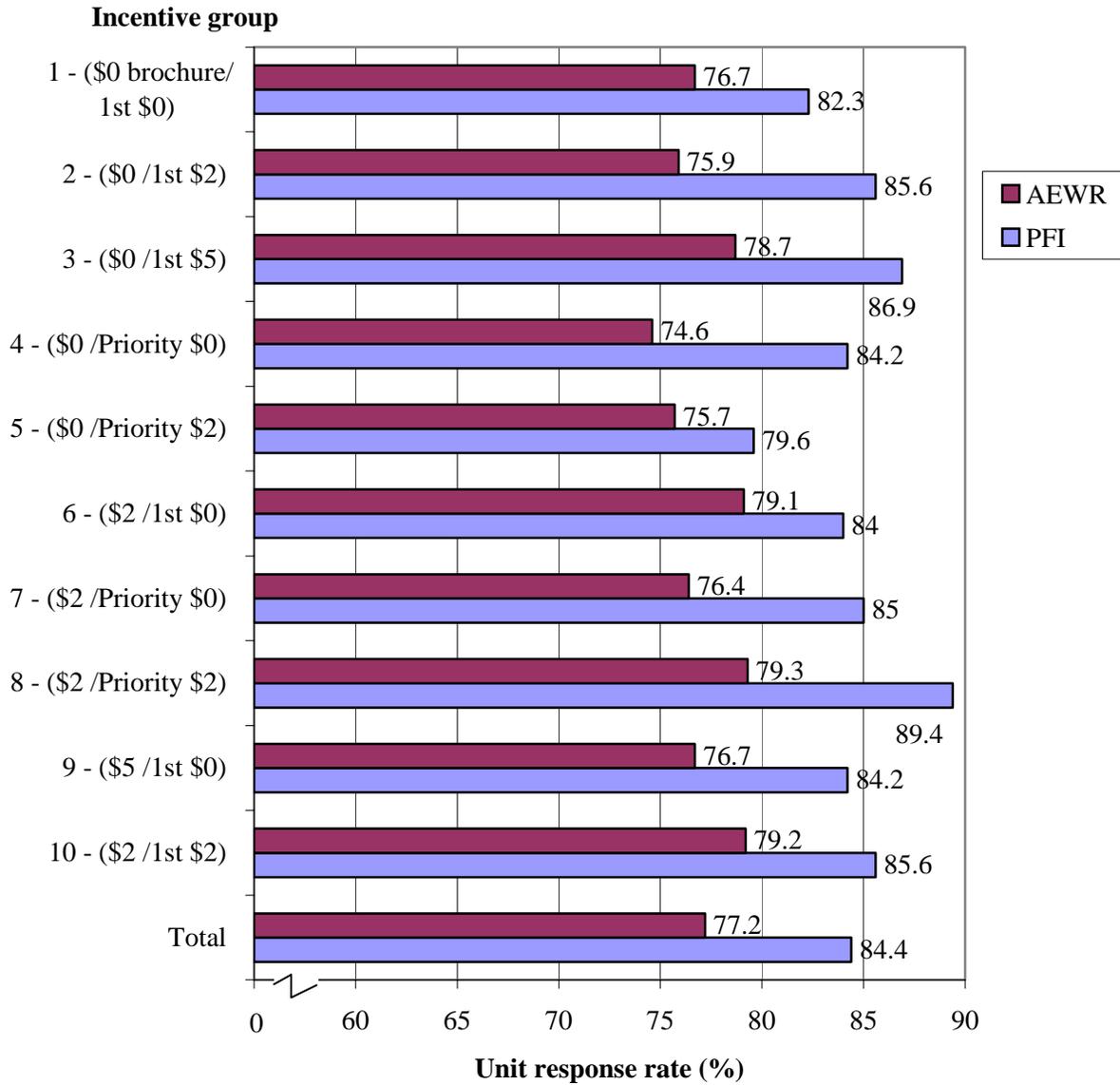| Incentive group	PFI		AEWR	
	Sample size[1]	Percent complete	Sample size[2]	Percent complete
Total	14,805	—	15,800	—
Total mailable cases	13,306	84.4 (0.44)	14,487	77.2 (0.47)
1 - ($0 brochure/1st $0).....	1,183	82.3 (1.58)	1,392	76.7 (1.50)
2 - ($0 /1st $2)...................	1,320	85.6 (1.28)	1,382	75.9 (1.39)
3 - ($0 /1st $5)...................	1,283	86.9 (1.23)	1,409	78.7 (1.43)
4 - ($0 /Priority $0)	1,501	84.2 (1.37)	1,547	74.6 (1.47)
5 - ($0 /Priority $2)	1,347	79.6 (1.38)	1,354	75.7 (1.64)
6 - ($2 /1st $0)...................	1,497	84.0 (1.37)	1,664	79.1 (1.25)
7 - ($2 /Priority $0)	1,350	85.0 (1.25)	1,413	76.4 (1.47)
8 - ($2 /Priority $2)	1,215	89.4 (1.24)	1,459	79.3 (1.45)
9 - ($5 /1st $0)...................	1,331	84.2 (1.49)	1,439	76.7 (1.60)
10 - ($2 /1st $2).................	1,279	85.6 (1.44)	1,428	79.2 (1.45)
Total nonmailable cases	1,499	78.0 (1.35)	1,313	70.4 (1.65)

— Not reported.

[1] Sample size is the number of children sampled for PFI, by Screener experimental group.

[2] Sample size is the number of adults sampled for AEWR, by Screener experimental group.

NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the advance mailing condition (before the / mark) and the initial refusal condition (after the / mark). All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. PFI is the Parent and Family Involvement in Education Survey of the National Household Education Surveys Program, 2003. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Parent and Family Involvement in Education Survey (PFI) and the Adult Education for Work-Related Reasons Survey (AEWR) of the National Household Education Surveys Program, 2003.

Figure 4.   Percentage of completed PFI and AEWR interviews, by incentive group: 2003

Incentive group

NOTE: The incentive group descriptions give the advance mailing condition before the / mark and the initial refusal condition after the / mark. After the / mark, 1st means the initial refusal materials were sent by first class mail. After the / mark, Priority means the initial refusal materials were sent by Priority Mail. All advance letters were sent by first class mail in a U.S. Department of Education business envelope, as were initial refusal letters in conditions specifying first class mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. Refusal letters included a colored NHES project brochure. PFI is the Parent and Family Involvement in Education Survey of the National Household Education Surveys Program, 2003. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Parent and Family Involvement in Education Survey (PFI) and the Adult Education for Work-Related Reasons Survey (AEWR) of the National Household Education Surveys Program, 2003.

4. Adult Education for Work-Related Reasons Survey Experiment

A second experiment was designed and implemented to examine the effect of incentives and mailing strategies in improving response rates for the adult education interview. Once the NHES Screener was completed, the interviewer immediately asked to speak with a person sampled for an extended interview. As noted earlier, this experiment covered only persons sampled for the adult education survey because response rates for this survey were expected to be considerably lower than for the PFI. The sampled adults were included in the experiment if they either refused or were classified as having reached maximum call status.[1] Like the Screener experiment, the adult education survey experiment was restricted to persons with addresses because the treatments required sending a letter to the household; for the adult education survey it was further restricted to those cases that did not respond (specifically refused or were maximum calls).

The treatment varied the type of letter mailed and the monetary incentive. Table 17 shows the number of adults assigned to each of the three different incentive groups by whether they were included because they refused or reached maximum call status. The sample sizes were developed assuming a baseline refusal conversion rate of 20 percent, based on results from the 2001 survey. This rate would yield groups large enough to detect differences in conversion rates of 5 percentage points. This would allow for examination of the effect of a $2 incentive and the effect of Priority Mail on refusal conversion and the completion of maximum call cases.

As shown in table 17, a total of 3,123 adults sampled for the adult education survey with addresses were included in the experiment, with 1,975 who were initial refusals and 1,148 who were maximum calls. (Overall, 16,004 adults were sampled for the adult education survey.) Both initial refusals and maximum calls were randomly assigned to one of the three groups. Cases with any other status were not included in the incentive experiment. The random assignment was not implemented until January 29, 2003, a few weeks after the start of the field period. As a result, 202 adult education survey cases were not assigned to an incentive group because they were called before January 29. These cases are excluded from all analysis. Table 5 also gives the estimated percent of persons in each of the incentive groups. The estimate is weighted by the base weight of the sampled adult that includes the household level weight and the inverse of the probability of selecting the adult within the household. This weight is used to produce the estimates for all the adult education survey experimental findings.

Like the Screener, second refusal conversions were attempted for adults who refused the adult education survey a second time. In an effort to improve the survey unit response rate, the cases that had not been sent Priority Mail previously were sent Priority Mail with a refusal conversion letter. If the household had been sent Priority Mail at the Screener level, then it was excluded from the subgroup sent a Priority Mail at the second refusal conversion stage in the adult education survey. Thus, the experiment only evaluates the results of the first refusal conversion attempt. The goal of this special mailing was to increase the adult education survey response rate.

A total of 12,725 adult education interviews were completed in NHES:2003. In most of these interviews (11,554), the respondent never refused to participate. The remaining 1,171 adult education interviews were completed on either the first refusal conversion attempt (869) or the second refusal conversion attempt (302).

The adult education survey experiment involved only those cases that ever refused or those that had so many call attempts that they were classified into the maximum call disposition and then released for

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[1] The number of calls required to classify an interview as a maximum call is 9, but some cases do not get classified until at least 14 calls depending on the specific outcomes of the case.

more call attempts. Table 18 shows there was a total of 3,123 sampled persons included in the experiment, with 1,975 entering because of refusals and 1,148 because of the maximum call disposition.

Table 17.   Sample sizes and weighted percents for Adult Education for Work-Related Reasons experimental conditions: 2003

Incentive group	Sample size[1]			Weighted percent of total
	Total	Refusals	Maximum calls	
Total mailable cases....................................	3,123	1,975	1,148	100.0
A – First class letter, $0 incentive...........	1,078	703	375	34.2
B – First class letter, $2 incentive...........	1,045	642	403	32.4
C – Priority Mail letter, $0 incentive	1,000	630	370	33.4

[1] The sample size is the number of AEWR cases that received initial refusals or reached maximum call status.
NOTE: For the AEWR incentive experiment, group A is the control group and groups B and C are two experimental groups. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Table 18 gives the final disposition (complete, other nonresponse, and refusal) that accounts for nearly all sampled adults in the experiment. (A total of 17 adults who were out of scope are not in one of these three categories.) Using the percent completed as a measure of effectiveness, the table shows none of the three treatments resulted in a significant increase in completion rates, whether examining the differences overall or within the refusal and maximum call categories separately. Thus, contrary to the effects shown above for the Screener, even the $2 incentive in the refusal conversion did not result in a completion rate that was detectably higher. Table 19 shows that the mean number of call attempts is also not significantly affected by the treatment for either the refusals or the maximum call sampled adults.

As described earlier in this section, all sampled adults in households that had never been sent a Priority Mailing and refused the adult education survey a second time were sent a letter by Priority Mail. Adults who were not eligible for a Priority Mailing in the adult education survey were in households that previously refused at the Screener, otherwise they would have been eligible for the adult education survey special mailing. Adults who were eligible for a Priority Mailing in the adult education survey were a mix of households that had not refused the Screener and those that had refused the Screener but were in Screener experimental groups that did not have a Priority Mail treatment. Adults in households that did not refuse the Screener are probably more likely to complete the extended interview. The adults in the two groups are not comparable and were not randomly assigned to treatments. Consequently, examination of results on the use of Priority Mail in the second refusal conversion attempt of the adult education survey has limited value. However, analysis of outcomes for the two groups may suggest some additional experimentation.

Table 20 shows the results of the second refusal conversion attempt for the three groups by whether they were sent Priority Mail. Since treatment group C had all been sent Priority Mail in the adult education survey experiment, the outcome for all these are on one row of the table. The first two rows are the estimates for the adults who had been mailed only a first class letter in the first refusal conversion attempt. The second set of rows contains estimates for those who had been mailed a first class letter with $2 in the first refusal conversion attempt. The percentage complete for adults sent Priority Mail is at least

10 percentage points higher than it is for the adults who were not sent any letter at this stage. These results suggest it might be useful to test whether a first class letter at the second refusal conversion stage is useful. This outcome would be consistent with the findings from the Screener incentive experiment.

Table 18.   Percentage distribution of AEWR final dispositions, by type of nonresponse and incentive group: 2003

Incentive group	Sample size[1]	Percent complete		Percent other nonresponse[2]		Percent refusal	
Total	3,123	40.7	(1.03)	26.3	(0.95)	32.9	(0.94)
A – (1st, $0)	1,078	40.2	(1.94)	26.9	(1.73)	32.9	(1.73)
B – (1st, $2)	1,045	42.8	(1.87)	26.1	(1.58)	31.1	(1.48)
C – (Priority, $0)	1,000	39.3	(1.88)	26.0	(1.86)	34.8	(2.02)
Refusals..........................	1,975	38.2	(1.40)	9.9	(0.94)	51.9	(1.26)
A – (1st, $0)	703	38.4	(2.37)	10.6	(1.62)	51.0	(2.34)
B – (1st, $2)...............	642	39.2	(2.35)	9.3	(1.49)	51.5	(2.20)
C – (Priority, $0)	630	37.1	(2.48)	9.6	(1.36)	53.3	(2.64)
Maximum calls..............	1,148	45.0	(1.65)	54.2	(1.66)	0.8	(0.46)
A – (1st, $0)	375	43.4	(3.17)	56.0	(3.13)	0.6	(0.30)
B – (1st, $2)...............	403	48.3	(3.14)	51.5	(3.15)	0.2	(0.20)
C – (Priority, $0)	370	43.2	(3.44)	55.1	(3.43)	1.7	(1.19)

[1] Sample size includes persons sampled for AEWR who refused or reached maximum call status.
[2] *Other nonresponse* includes cases that finalized due to language problems, the inability to make contact with the respondent (maximum call status), the respondent being unavailable during the field period, or the telephone number being disconnected or reassigned before the AEWR interview could be completed.
NOTE: Standard error shown in parenthesis. Percents may not sum to 100 due to rounding. The parenthetical descriptions of the experimental conditions give the type of mail and the incentive. First class mail was sent in a U.S. Department of Education business envelope in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. All refusal letters included a colored NHES project brochure. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Adult Education for Work-Related Reasons Survey (AEWR) of the National Household Education Surveys Program, 2003.

Table 19.   Number of calls to final disposition for AEWR initial refusal and maximum call cases, by type of nonresponse and incentive group: 2003

Incentive group	Sample size[1]	Mean calls to first refusal		Mean calls to complete		Mean calls to other nonresponse[2]		Mean calls to second refusal	
Refusals............................	1,975	—		—		—		—	
A – (1st, $0)	703	9.8	(0.42)	6.2	(0.36)	10.6	(1.03)	4.1	(0.26)
B – (1st, $2)	642	10.3	(0.40)	5.4	(0.36)	11.0	(1.47)	4.5	(0.28)
C – (Priority, $0)...........	630	9.8	(0.37)	6.6	(0.39)	10.2	(1.27)	4.6	(0.31)
Maximum calls	1,148	†		—		—		†	
A – (1st, $0)	375	†		21.6	(0.78)	30.6	(0.92)	†	
B – (1st, $2)	403	†		22.1	(0.63)	30.9	(0.63)	†	
C – (Priority, $0)...........	370	†		21.6	(0.71)	30.9	(0.48)	†	

— Not reported.

† Not applicable.

[1] There were nine adults included in the experiment because of their maximum call disposition who eventually refused, but they are not included in this tabulation because of the small sample size.

[2] *Other nonresponse* includes cases that finalized due to language problems, the inability to make contact with the respondent (maximum call status), the respondent being unavailable during the field period, or the telephone number being disconnected or reassigned before the AEWR interview could be completed.

NOTE: Standard error shown in parenthesis. The parenthetical descriptions of the experimental conditions give the type of mail and the incentive for the initial refusal condition. First class mail was sent in a U.S. Department of Education business envelope in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. All refusal letters included a colored NHES project brochure. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Adult Education for Work-Related Reasons Survey (AEWR) of the National Household Education Surveys Program, 2003.

Table 20.   Number of AEWR second refusal cases, by final disposition, incentive group, and Priority Mail: 2003

Incentive group	Sample size[1]	Percent complete		Percent other nonresponse[2]		Percent refusal	
A – (1st, $0/no Priority)................	175	9.1	(3.08)	7.6	(2.13)	83.6	(3.48)
A – (1st, $0/Priority)....................	202	25.7	(3.58)	4.4	(1.66)	69.9	(3.78)
B – (1st, $2/no Priority)................	166	12.0	(2.46)	4.0	(1.40)	84.1	(2.72)
B – (1st, $2/Priority)....................	157	23.8	(3.44)	6.1	(2.35)	70.1	(3.94)
C – (Priority, $0/no Priority)	336	14.9	(2.38)	6.1	(1.53)	79.0	(2.65)

[1] The sample size is the number of persons sampled for AEWR who refused twice.

[2] *Other nonresponse* includes cases that finalized due to language problems, the inability to make contact with the respondent (maximum call status), the respondent being unavailable during the field period, or the telephone number being disconnected or reassigned before the AEWR interview could be completed.

NOTE: Standard error shown in parenthesis. Percents may not sum to 100 because of rounding. The parenthetical descriptions of the experimental conditions give the initial refusal mailing condition (before the / mark) and the second refusal condition (after the / mark). First class mail was sent in a U.S. Department of Education business envelope in conditions not specifying Priority Mail. The letters sent by Priority Mail were sent in the U.S. Postal Service Priority Mail envelopes. All refusal letters included a colored NHES project brochure. AEWR is the Adult Education for Work-Related Reasons Survey of the National Household Education Surveys Program, 2003. The sample sizes are unweighted. The percentages are calculated after weighting cases in the sample for differential probabilities of selection; therefore, percentages cannot be calculated directly from the sample sizes presented.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Adult Education for Work-Related Reasons Survey (AEWR) of the National Household Education Surveys Program, 2003.

5. Summary

It has become increasingly difficult to achieve high response rates in sample surveys in recent years, even as the level of effort in obtaining completed interviews has increased. Previous experience in NHES had demonstrated the effectiveness of an advance mailing (Hagedorn et al., 2003), contrary to the findings of Singer, Van Hoewyk, and Maher (2000). One possible reason the advance mailing is effective in NHES is that the U.S. Department of Education envelope and letterhead convey a level of legitimacy and salience that other forms of mailings may not. Singer, Van Hoewyk, and Maher (2000) used University of Michigan mailing envelopes and stationery.) Despite the effectiveness of the advance letter, however, Screener unit response rates have fallen over the years in NHES. These declining Screener unit response rates in NHES led to the design and execution of an experiment examining the effects of respondent incentives as a means to improve response.

The Screener incentive experiment implemented in the 2003 NHES administration included 10 conditions with varied combinations of mailing procedures (first class and Priority Mail) and respondent incentive amounts ($0, $2, and $5). The experimental conditions were used to study advance mailing strategies and refusal conversion strategies. An experiment was also conducted in the Adult Education Survey, one of two topical surveys conducted in NHES:2003. Three experimental conditions (first class letter advance mailing, $0 incentive; first class letter advance mailing, $2 incentive; and Priority Mail advance mailing, $0 incentive) were applied in adult education survey cases where the respondent initially refused or had not responded after a large number of calls.

The results of the Screener incentive experiment indicate that small cash incentives are effective in improving NHES Screener unit response. In NHES:2003, refusals occurred less often among those who received advance incentives of $2 or $5 than among those who received no incentive; the benefit of the incentive in the initial refusal rate was 5 to 7 percentage points, depending on the incentive amount. While the rate of refusal was lower for those who received an advance incentive of $5 compared with those who received $2, a diminishing effect per dollar of incentive was observed. Additionally, when an advance letter with no incentive and a refusal conversion letter with a monetary incentive are used (experimental groups 2 and 3), subsampling of refusals for conversion efforts is effective in gaining additional response.

Singer, Van Hoewyk, and Maher (2000) reported a 10 percentage point response rate benefit of an advance incentive. This effect is larger than that observed in the NHES:2003 experiment. One reason for the difference is that the 10 percentage point effect is only for the cases with addresses, while NHES gives the effect over the full sample. Another possible reason is the difference in the saliency and sponsorship of the surveys.

The NHES:2003 experiment also showed respondent incentives were effective in obtaining cooperation at the refusal conversion stage for the Screener. Experimental treatment groups that included payments were more likely to respond at the first refusal conversion stage compared with those who had not received payments. In addition, the advance incentive treatments and refusal incentive treatments combined to yield higher response rates after the first refusal conversion stage for those who had received payments of $4 (two payments of $2) and those who had received $5 (in either one of the payments). The use of Priority Mail did not significantly improve initial refusal conversion rates, contrary to expectations. This may reflect the efficacy of conducting first class mailings in U.S. Department of Education envelopes.

At the second refusal conversion stage for the Screener, a letter was sent by Priority Mail to households that had refused twice and had not previously been sent a Priority Mail letter. This strategy improved the rates of refusal conversion for these groups slightly, but the overall effect on the response

rate is marginal because only 7 percent of completed Screeners result from second refusal conversion efforts.

The effect of the Screener incentives on extended interview unit response rates was also examined. While, in general, PFI and AEWR unit response rates were higher when households had received a Screener incentive, the pattern was not consistent and the effects were not large. The findings do not support a hypothesis that the Screener incentives improve extended interview response in NHES. An examination of the effect of the incentive treatments on the numbers of calls required to finalize a Screener showed no effects that would have implications for the rest of the data collection.

The Adult Education for Work-Related Reasons Survey experiment showed that the use of a $2 incentive and the use of Priority Mail did not significantly increase the completion rates for initial refusal conversion or for cases with large numbers of contact attempts. In addition, no differences were detected in the numbers of calls required to finalize the cases. The use of Priority Mail was found to be effective in the second refusal conversion stage for the adult education survey, improving the unit response rate for cases that had refused twice by about 10 percentage points. The effect on the unit response rate for the entire adult education survey sample, however, was very small.

In addition to the findings presented in this report, additional questions for investigation were identified. A test of the relative refusal conversion rates realized with FedEx (rather than Priority Mail) versus first class would be useful. It may be that neither special mail procedure is superior to the first class mailing in a U.S. Department of Education envelope, but this has yet to be established for FedEx in a concurrent experiment. Another question as to mailing procedures is the efficacy of first class mail at the second refusal conversion stage. Dillman (2000) suggests that varying mail contacts with the household, rather than using the same approach in each contact, is more effective. It may be that a first class letter at the second refusal conversion stage would not be effective compared with Priority Mail or FedEx, but this has yet to be examined within the context of this survey program. Finally, researchers' experiences with gaining respondent cooperation are not static, and ongoing investigation into the efficacy of various methods over time is necessary.

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**Appendix A
Response Rate Calculation Methods**

Response Rate Calculation Methods

This appendix describes the calculation of unit response rates for the National Household Education Surveys Program (NHES:2003). A unit response rate is the ratio of the number of units with completed interviews (e.g., the units could be telephone numbers, households, or persons) to the number of units sampled and eligible for the interview. In some cases, these rates are easily defined and computed, while in other cases the numerator or denominator of the ratio must be estimated.

For reporting the results from NHES:2003, the overall unit response rate indicates the percentage of possible interviews that were completed taking all survey stages into account, while the unit response rate measures the percentage of interviews that were completed for a specific stage of the survey. For example, household members were identified for interviews in a two-stage process. Screener interviews were conducted to enumerate and sample household members, and then questionnaires were administered for the sampled members. If the responding household member failed to complete the first-stage Screener, no members could be sampled for other interviews. Under this design, the unit response rate for the second stage is the percentage of sampled persons who completed the PFI or AEWR interview. The overall unit response rate is the product of the first- and second-stage unit response rates.

Unit response rates can be either unweighted or weighted. The unweighted rate, computed using the raw number of cases, provides a useful description of the success of the operational aspects of the survey. The weighted rate, computed by summing the weights (usually the base weights, which are the reciprocals of the probability of selecting the units) for both the numerator and denominator, gives a better description of the success of the survey with respect to the sampled population since the weights allow for inference of the sample data (including response status) to the population. Both rates are usually similar unless the probabilities of selection and the response rates in the categories with different selection probabilities vary considerably. All of the unit response rates discussed in this report are weighted unless noted specifically in the text, since the main purpose of this report is to describe the results of the experiment with respect to the survey population.

In computing unit response rates for an RDD survey, the first concern is to classify each sampled telephone number according to its residential status. The three major categories of residential status are those identified as numbers for residential households, those identified as nonresidential numbers (primarily nonworking and business telephone numbers), and those numbers that, despite numerous attempts, could not be classified as either residential or nonresidential. Calculation of unit response rates is complex because of the possible ways residential status can be assigned to the latter set of numbers.

There are various approaches for computing RDD Screener unit response rates. Each of these approaches uses the same numerator, the number of households (weighted or unweighted, for the weighted and unweighted rates, respectively) that completed the Screener. Variability in the estimates arises because it is not possible to identify precisely the residential status for each telephone number. The difference among the rates is in the allocation of the numbers in the unknown residential status category that is used in the calculation of the denominator of the unit response rate.

The survival approach uses information about cases for which no answer was obtained in the estimation of their residency rate. Specifically, for NHES:2003, the listed status, interviewers' codings of answering machine call attempts, and the total number of call attempts are used in the estimation of the residency rate based on survival analysis methods. Estimates based on the survival method suggest that 19.7 percent of telephone numbers with unresolved residency status in NHES:2003 are residential. Therefore, the denominator of the unit response rate based on the survival method is all the telephone numbers that were known to be residences plus 19.7 percent of the numbers with an unresolved residential status. Because the survival method uses more information about the telephone numbers and

their call histories, it is a more accurate approach for estimating unit response rates in random digit dialing (RDD) surveys. See Brick, Montaquila, and Scheuren (2002) for further details about the survival method.

The business office method derives its name from the technique used to estimate the denominator of the rate. A random sample of 350 telephone numbers with unresolved residency status were selected in the NHES:1995 and the numbers were classified by local telephone companies. The telephone companies were asked to first classify the numbers as working or not working. The companies were asked to further identify working numbers as residential or business numbers. As a result of this process, it was estimated that 40.5 percent of the numbers were residential. This percentage is nearly identical to the result from a study conducted at the end of the NHES:1991. Therefore, the denominator of the unit response rate based on the business office method is all the telephone numbers that were known to be residences plus 40.5 percent of the numbers with an unresolved residential status. Some research suggests that the business office approach may be inaccurate due to the reporting practices of the phone companies (Shapiro et al. 1995).

Three other unit response rates may be computed by allocating different proportions of the numbers with unknown residency status into the residential category. The Council of American Survey Research Organizations (CASRO) rate is computed by allocating the numbers with unknown residential status in the same proportion observed in the numbers with known residential status, which, in the NHES:2003, was 40.4 percent. Evidence from the study of the business office method described earlier suggests that the residency rate for numbers with unknown residential status is lower than implied by the CASRO rate calculation. Therefore, the CASRO rate is not recommended for unit response rate calculations for NHES.

The conservative and liberal unit response rates define the lower and upper bounds of the unit response rate. The conservative unit response rate is computed assuming that all of the numbers with unknown residential status are actually residential numbers. The liberal rate is computed assuming that all of the numbers with unknown residential status are nonresidential. As noted earlier, the variability in the estimates arises because it is not possible to identify precisely the residential status for each telephone number.

Appendix B
Logistic Regression Analysis

Logistic Regression Analysis

To refine the analyses of the incentive experiment results, the initial cooperation and refusal conversion rates were examined using logistic regression, where the outcome for a sampled number was considered a "success" if the household completed the screener at that stage. After examining many variables unrelated to the experiments that were available for all telephone numbers,[1] two good predictors of initial cooperation were identified: region of the country, and metropolitan status. Table B-1 shows the final model for the initial cooperation rates. The model is parameterized so that the last level of each predictor is the reference cell and the parameter for that level is set equal to zero. In general, the significance tests for the effects of the predictor variables in the logistic regression model control for the effects of other variables in the model.

In table B-1, the estimated regression coefficients for the $0 and $2 incentive amounts indicate that, after controlling for other predictor variables in the model, households receiving these treatments have a lower probability of responding initially to the screener than those receiving the $5 incentive. The odds ratios show that compared to a household in the $5 incentive group, a household in the $2 incentive group is 0.87 times as likely to respond initially, and a household in the $0 incentive group is only 0.69 times as likely to respond initially. A contrast test shows that the effect of the $0 incentive is statistically different from the effect of the $2 incentive. These results are consistent with the results on initial cooperation rates given in table 6.

Table B-1. Logistic regression estimates for initial cooperation

Parameter	Estimate		p	Odds ratio
Intercept	0.56	(0.049)	0.000	†
Region (1)	–0.20	(0.020)	0.000	0.82
Metro status (1)	–0.13	(0.035)	0.001	0.88
Metro status (2)	–0.13	(0.031)	0.000	0.87
Metro status (3)	–0.12	(0.038)	0.003	0.89
Metro status (4)	–0.29	(0.050)	0.000	0.75
Initial incentive ($0)	–0.37	(0.034)	0.000	0.69
Initial incentive ($2)	–0.14	(0.034)	0.000	0.87

† Not applicable.
NOTE: Standard error shown in parenthesis. The sample size is 39,298 residential telephone numbers, excluding numbers that were never completed or refused, such as language problem cases. The modified likelihood ratio statistic proposed by Estrella (1998) as a goodness of fit measure for logistic regression models is .008. p is the p-value for the two-sided test that the parameter is equal to 0.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

Next, logistic regression was used to examine the first refusal conversion rates. In this analysis, predictor variables were included for the two first refusal conversion experimental treatments: whether

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[1] The following characteristics were examined: census region (combined Northeast and South/combined Midwest and West, with Midwest and West as the reference group), interview language (English/Spanish), presence of person under 21 in household, home ownership (own/rented/other arrangement), median home value in telephone exchange, median income in telephone exchange, Metropolitan Statistical Area (MSA) status [(1) in county in central city, (2) in county not in central city, (3) subcounty of MSA, (4) MSA in its own county, and (5) not MSA, minority stratum, percent of households with incomes of $75,000 to $100,000, percent of households with income above $100,000, percent Asian in exchange, percent Black in exchange, percent Hispanic in exchange, percent college graduates in exchange, and percent homeowners in exchange. Although all of the characteristics listed were considered, only those found to be significant were included in the final model.

Priority Mail was sent (no/yes) and the refusal incentive amount ($0/$2/$5). Other models were considered (e.g., including an interaction between Priority Mail and a level of the monetary incentive), but these are not discussed because they added little to the predictive power of the model. Table B-2 gives the estimated regression coefficients and odds ratios. There are three important results from this analysis. First, the estimated coefficient for the Priority Mail treatment is not significantly different from zero. Second, the monetary incentives at the refusal conversion stage do increase the conversion rates. The $2 incentive results in a higher conversion rate than no incentive, and $5 is more effective than $2. Third, the monetary incentives in the initial mailing do not have a statistically significant effect on the refusal conversion rates. In other words, after controlling for other variables in the model, the only treatment at the first refusal conversion stage that results in higher rates of completing the screener is the amount of money sent prior to the refusal conversion attempt. This analysis supports statements made earlier in the tabular analysis.

Table B-2.   Logistic regression estimates for first refusal conversion

Parameter	Estimate		p	Odds ratio
Intercept	−0.21	(0.102)	0.041	†
Region (1)	−0.08	(0.036)	0.032	0.92
Metro status (1)	−0.11	(0.044)	0.015	0.90
Metro status (2)	−0.01	(0.050)	0.850	0.99
Metro status (3)	−0.03	(0.038)	0.416	0.97
Metro status (4)	−0.11	(0.078)	0.149	0.89
Initial incentive ($0)	0.04	(0.069)	0.604	1.04
Initial incentive ($2)	0.06	(0.070)	0.411	1.06
First refusal Priority Mail (no)	−0.07	(0.035)	0.066	0.94
Refusal incentive ($0)	−0.37	(0.063)	0.000	0.69
Refusal incentive ($2)	−0.18	(0.063)	0.005	0.83

† Not applicable.
NOTE: Standard error shown in parenthesis. The sample size is 18,797 households that initially refused the screener and either completed or refused the conversion. The Estrella goodness of fit statistic is .005. p is the p-value for the two-sided test that the parameter is equal to 0.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.

The final logistic regression analysis examines the effectiveness of sending Priority Mail at the second refusal conversion stage. Since only second refusal households that had not been sent a Priority Mail in the first refusal conversion stage were treated, the sample for this analysis only includes groups 1, 2, 3, 6, 9, and 10. The predictor variable for using Priority Mail at the first conversion stage is dropped. Table B-3 gives the estimated regression coefficients and odds ratios that show the use of Priority Mail is effective at this stage. A household that was not sent a Priority Mail letter at this stage was only 0.6 times as likely to respond as a household that was sent a Priority Mail second refusal conversion letter. Interestingly, the incentives at the initial and first refusal conversion stages are not statistically significant, again suggesting that there is little or no carry-over effect from incentives at earlier stages.

Table B-3.   Logistic regression estimates for second refusal conversion

Parameter	Estimate		p	Odds ratio
Intercept	–0.85	(0.172)	0.000	†
Region(1)	–0.04	(0.067)	0.599	0.96
Metro status (1)	–0.53	(0.089)	0.000	0.59
Metro status (2)	–0.30	(0.115)	0.010	0.74
Metro status (3)	–0.33	(0.098)	0.001	0.72
Metro status (4)	–0.37	(0.172)	0.035	0.69
Initial incentive ($0)	0.10	(0.120)	0.421	1.10
Initial incentive ($2)	–0.06	(0.126)	0.640	0.94
Refusal incentive ($0)	–0.03	(0.102)	0.774	0.97
Refusal incentive ($2)	0.04	(0.107)	0.740	1.04
Second refusal Priority Mail (no)....	–0.51	(0.084)	0.000	0.60

† Not applicable.
NOTE: Standard error shown in parenthesis. The sample size is 5,951 households that refused the screener twice and had not been sent Priority Mail previously and completed or refused on the last attempt. The Estrella (1998) goodness of fit statistic is .014. p is the p-value for the two-sided test that the parameter is equal to 0.
SOURCE: U.S. Department of Education, National Center for Education Statistics, National Household Education Surveys Program, 2003.