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**The Use of Monetary Incentives
in Census Bureau Longitudinal Surveys**

Kathleen P. Creighton¹
Karen E. King²
Elizabeth A. Martin³

Demographic Surveys Division¹

Demographic Statistical Methods Division²

Director's Office³

U.S. Census Bureau
Washington, D.C. 20233

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The Use of Monetary Incentives in Census Bureau Longitudinal Surveys¹

Kathleen P. Creighton, Karen E. King, and Elizabeth A. Martin
U.S. Census Bureau

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Introduction

The U.S. Census Bureau recently started using monetary incentives on an experimental basis in two of its demographic longitudinal surveys, the Survey of Income and Program Participation (SIPP) and the Survey of Program Dynamics (SPD). As in other longitudinal surveys, both SIPP and SPD have seen a steady increase in nonresponse over the life of the sample. Households selected for the SIPP 1996 panel, which concluded in March of this year, were in sample for a total of four years with lengthy interviews at 4-month intervals. With each wave of the 1996 SIPP Panel, cumulative household nonresponse increased and reached the highest level ever - nearly 34 percent at the end of 12 waves. We believe it would have been even higher if we had not used incentives in several waves of the panel.

Households selected for the SPD were originally interviewed in the last waves of the 1992 and 1993 SIPP panels and were recontacted for interviews in 1997 (known as the SPD "Bridge"), 1998, 1999, 2000, and will continue with 2001 and 2002 rounds of interviewing. The original SIPP households that were selected to continue into the SPD had reached a sample attrition rate of 50 percent at the conclusion of the 1998 interview cycle with 4 more contacts planned over the following 4 years. With appeals to Congress from several outside data users, we implemented an incentive program in SPD beginning in 1999. In addition, we tested in 1999 and implemented in 2000 a program to recontact and attempt to interview households that had previously dropped out of sample in the 1997 interview cycle. This paper reviews the results from these experiments and programs and discusses our plans for the use of monetary incentives in the SIPP and SPD next year.

¹This paper reports the results of research and analysis undertaken by Census Bureau staff. It has undergone a Census Bureau review more limited in scope than that given to official Census Bureau publications. This report is released to inform interested parties of ongoing research and to encourage discussion of work in progress. Thanks to Denise Lewis and Nancy Bates for their comments.

I. The Survey of Income and Program Participation (SIPP)

A. Design and Methodology

The SIPP provides national estimates of sources, amounts, and determinants of income for households, families, and persons. The principal goal of the SIPP is to provide information to federal policy makers to assist in evaluation and reform of welfare programs, taxes, and entitlement programs. In order to achieve these goals, the SIPP provides both cross-sectional and longitudinal estimates (such as transition probabilities and spell durations). SIPP panel members are interviewed at four month intervals for three to four years. SIPP panels are divided into four rotation groups of approximately equal size. One rotation group is interviewed each month. One round of interviewing of the entire panel is called a wave. This arrangement smooths out interviewing workloads and reduces bias in transition estimates.

In the initial interview, all persons living at sample addresses are listed as household members. Persons who are 15 years of age and older are interviewed. These original sample persons are the units of observation for SIPP and are followed for the life of the panel (unless they die, move abroad, or move into an institution or military barracks). Persons who move into households with original sample persons after wave 1 are also interviewed as long as they continue to reside with an original sample person.

Details of SIPP panels, such as sample size and panel length, vary among panels. More substantial changes are made after each Decennial Census when we update the sample frame and select new primary sampling units (PSUs), which are counties or groups of counties that comprise the first level of sampling done for household surveys. The 1990 redesign of the SIPP sample took effect with the 1996 panel. We reduced cluster sizes and oversampled for low income households using a stratification approach proposed by Waksberg (1973). Two within-PSU strata were formed, one with a high concentration of poverty and one with a low concentration. Computer-assisted interviewing using laptop computers was also introduced with the 1996 panel, a significant event for the field staff as well as headquarters staff who received and processed the data.

B. Motivation for Using Incentives

In a recent review of the literature, Singer et al. (1999) conclude that the well-documented positive effects of incentives in mail surveys also hold in surveys conducted by interviewers in person or by telephone. The positive (but modest) effects of incentives appear to hold for fresh respondents, panel respondents, and nonrespondents. Their review concludes that cash is a more effective inducement to respond than a gift, even holding constant the value of the gift, and that prepaid incentives are more effective than promised or contingent incentives. However, a promised incentive is better than no incentive.

In the 1996 SIPP panel, the effects of three types of incentives on response rates were evaluated experimentally: unconditional incentives given at the initial contact, “booster” incentives given at

a later wave, and incentives targeted to households that failed to respond in a prior wave. The first, Wave 1 experiment was conducted to test whether use of incentives could improve SIPP response rates. Subsequent experiments were motivated by unusually high sample attrition in the 1996 panel. In the first interview of the 1996 panel, wave 1, the Census Bureau obtained 36,700 interviews or 92 percent of eligible households. Based on prior experience, a 30 percent noninterview rate had been projected by the end of the 4-year panel. However, even with the use of incentives for half the sample in wave 1, the household noninterview rate was over 26 percent by the end of wave 6, much higher than in wave 6 of prior panels. If it continued, sample attrition at this level would compromise the longitudinal uses of the data. Several incentive experiments were embedded in waves 7 through 12 to stem further sample loss and to arrive at the most effective method. One experiment was independent of the initial wave 1 experiment, permitting estimation of their separate effects. By the end of the panel, the total cumulative nonresponse rate had stabilized at 33.6 percent.² The design and results of the incentive experiments are discussed below.

C. SIPP 1996 Panel Incentive Experiments: Description and Results

1. Wave 1: Differential Effects of \$0, \$10, \$20

The first experiment in the 1996 Panel was the least restrictive of the four tests in the number of sample cases receiving an incentive. SIPP PSU's were sorted by size and divided into three incentive groups (\$0, \$10, \$20) using systematic sampling. Incentives were distributed to sample addresses in rotation groups 2, 3, and 4 of Wave 1. Sample addresses in rotation group 1 were also assigned to treatment groups, but did not actually receive any incentives. Thus, rotation 1 cases served as a kind of benchmark against which to compare response rates for rotation 2-4 cases which were in the same PSUs and received incentives. For safety reasons, Field Division did not want field representatives to carry cash. Instead paper vouchers for \$10 and \$20 were distributed by SIPP interviewers at the door immediately after verifying the address. Interviewers gave vouchers to noninterviewed as well as interviewed households. The use of unconditional incentives was based on research that suggests that unconditional incentives are more effective than conditional ones. Recipients were instructed to fill in their name, check the address, and return the voucher to the Census Bureau in the postage paid pre-addressed envelope. After receiving the voucher, the Census Bureau mailed a check to the recipient within two to three weeks. Note the delay associated with using a voucher as the incentive. Table 1 shows the cumulative nonresponse rates by wave by incentive amount and poverty stratum.

²The sample loss rate is calculated to include a growth factor to account for outmovers from noninterviewed households. At the completion of the 1996 panel, the sample loss rate was 36 percent.

Table 1. Cumulative Household Nonresponse Rates for All Waves of 1996 SIPP Panel by Wave 1 Incentive Amount for Total, Poverty, and Nonpoverty Stratum Households (1992 & 1993 Panel shown for comparison; sample growth not included)

Wave 1 Incentive Group			Cumulative Nonresponse Rate (%) by Wave											
			1	2	3	4	5	6	7	8	9	10	11	12
1992			9.29	14.43	15.94	17.46	19.58	20.67	21.93	23.54	24.97	25.09	N/A	N/A
1993			8.9	14.02	15.85	17.69	19.57	21.37	23.33	24.4	25.72	N/A	N/A	N/A
1996--All households 1/			8.6	14.42	17.67	20.54	24.03	26.57	28.94	30.17	31.48	32.52	33.51	33.78
Rot. 2-4 only 2/	\$0	10,328	9.18	15.14	18.33	21.46	24.70	27.64	29.85	30.91	32.37	33.68	34.10	34.34
	\$10	9,686	9.26	14.32	18.08	21.18	24.24	26.77	29.03	30.08	31.23	32.04	33.14	33.38
	\$20	10,038	7.72	12.72	15.77	18.85	22.44	24.78	27.12	28.34	29.70	30.74	31.80	31.80
Poverty stratum 3/			8.03	14.46	17.63	20.36	24.06	26.83	29.13	30.48	32.00	33.16	34.09	34.71
Rot. 2-4 only 2/	\$0	3,185	9.30	16.06	19.19	22.36	25.53	28.98	31.43	32.26	33.84	35.84	36.51	36.96
	\$10	2,602	8.12	13.78	17.65	20.74	24.26	27.10	28.99	30.85	32.21	33.32	33.89	34.70
	\$20	2,898	5.91	11.40	14.39	16.91	21.06	23.00	25.56	27.05	28.75	29.40	30.82	30.90
Non poverty 4/			9.00	14.40	17.68	20.62	24.03	26.46	28.84	30.01	31.25	32.23	33.24	33.37
Rot. 2-4 only 2/	\$0	7,143	9.14	14.88	18.10	21.22	24.48	27.27	29.42	30.54	31.97	33.09	33.44	33.63
	\$10	7,084	9.51	14.44	18.17	21.27	24.24	26.70	29.04	29.91	31.01	31.76	32.98	33.09
	\$20	7,140	8.61	13.05	16.12	19.33	22.78	25.22	27.51	28.66	29.94	31.07	32.04	32.02

1/ Household total rates include all rotations

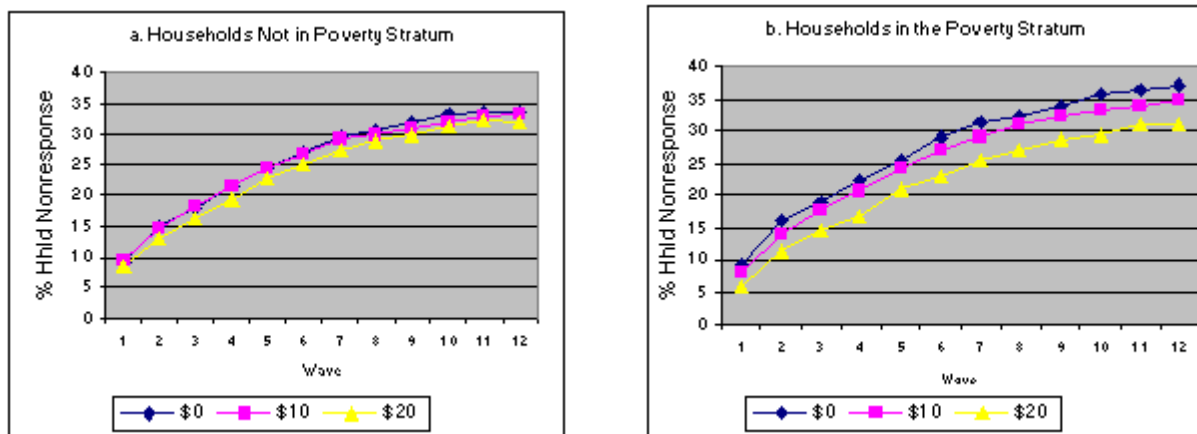
2/ Incentive group total rates include rotations 2-4 only, since rotation 1 was not part of the incentive test

3/ Poverty stratum contains a high concentration of low income ($\leq 150\%$ of poverty threshold) households

4/ Nonpoverty stratum contains a high concentration of high income ($>150\%$ of poverty threshold)

Compared to no incentive, a \$20 incentive significantly ($p < .10$) reduced household nonresponse rates at Wave 1 and in subsequent waves as well (James, 1997; Mack et al., 1998). This result holds for the 1996 panel as a whole, and separately for the poverty and non-poverty strata (see fig. 1).

Figure 1. Cumulative Household Nonresponse, Rotations 2-4: 1996 SIPP Panel



The response rate differences among incentive treatment groups appear greater for households in the poverty stratum, but the significance of the interaction effect has not been tested so definite conclusions cannot be drawn. (In addition, response rates in wave 7 and later waves reflect the effects of a booster incentive given in poor households.)

The \$10 incentive group did not achieve significantly higher rates of response initially or in subsequent waves compared to no incentive, although in a few waves there are significant differences.

The beneficial effect of the \$20 incentive extended to item nonresponse rates as well. Mack et al. (1998) found that the rate of imputation of the amount of gross wages was significantly lower in the \$20 (but not the \$10) treatment group.

A question of particular urgency for SIPP was whether incentives might help reduce or eliminate the higher rates of attrition of poverty households (Waite, et al., 1997). It makes sense that incentives might be more effective in poor households, since the need and value of the incentive are greater in relation to income. The differences in fig. 1b appear larger than those in 1a. However, the analyses of the wave 1 experiment are not conclusive on this point. Both poverty and nonpoverty stratum households who received \$20 responded at higher rates than those who received no money; the difference between strata in the size of the effect (i.e., the interaction effect) was not tested. Among black households, the \$20 treatment group responded at the same rates as the \$0 group (see Mack et al. 1998), suggesting no significant incentive effect for them.

Finally, it appears that there was variability in the administration of the Wave 1 incentives experiment. Huggins and James (n.d.) find that there were large and significant differences among incentive treatment groups in some regional offices and no effects at all in others. Their results suggest the experiment may not have been implemented in the same way in all offices. This may have reduced its effectiveness; clearer and larger incentive differences would likely have resulted from an experiment that was consistently implemented.

2. Wave 7 : Differential effects of \$0 and \$20 "booster" incentive for poverty households

In Wave 7 of the 1996 Panel, a \$20 booster incentive was given to households that met both of the following criteria:

- ◆ received either a \$10 or \$20 incentive in Wave 1; and
- ◆ had household income less than or equal to 150 percent of poverty threshold in Wave 1.

All spawned (split) household as well as all their sample neighbors were also eligible for the same treatment. This test extends the Wave 1 test by targeting low income or near poverty households that were eligible for the original Wave 1 incentive. In addition, some non-poor households that had received an incentive in Wave 1 experimentally received either \$0 or \$20 in wave 7. The paper vouchers of the Wave 1 test were replaced by plastic debit cards which could be used in any ATM

machine when the appropriate PIN number was given.

Table 2 compares the cumulative nonresponse for poverty households who received an incentive in Wave 1 (and wave 7) to the cumulative nonresponse for all other households.

Table 2. Nonresponse Rates for 1996 SIPP Panel Low Income Households Receiving a \$20 Incentive in Wave 7, by Wave 1 Incentive Group vs. All Other Households

Incentive Treatment Group		Nonresponse Rates (%)					
		Wave 7	Wave 8	Wave 9	Wave 10	Wave 11	Wave 12
Low income (as of Wave 1)		22.81	24.95	26.43	27.72	29.03	29.66
Rot. 2-4 only	\$0 in wave 1 \$0 in wave 7	24.58	26.07	27.60	29.42	30.19	31.06
	\$10 in wave 1 \$20 in wave 7	22.15	24.27	25.84	27.13	28.59	28.84
	\$20 in wave 1 \$20 in wave 7	21.29	22.94	24.52	25.76	27.07	27.38
Not low income (as of Wave 1)		22.38	23.49	24.92	26.00	27.04	27.18

Sundukchi's (1999) comparison of response rates for the \$10 and \$20 groups (which received incentives in both waves 1 and 7) with the \$0 group (which received no incentives) suggests that the combined effect was beneficial, resulting in a slightly lower Type A³ nonresponse rate (by 1.23 percentage points). Because the wave 1 and wave 7 experiments are confounded, it is not possible to separately estimate the effects of the wave 1 and wave 7 incentives.⁴ There is no evidence that the incentive had different effects for poor and non-poor households.

3. Wave 8 & 9: Differential Effects of \$0, \$20, \$40 Incentives on Obtaining Interviews from Previous Nonrespondents

SIPP drops cases from the sample after two successive noninterviews. In order to reduce sample loss, an experiment was conducted to target incentives to first-time nonrespondents to try to keep them in sample. In Wave 8, households that had been Type A noninterviews for the first time in Wave 7 were randomly assigned to an incentive group and given either \$0, \$20, or \$40. The same procedure was done in Wave 9 for Wave 8 Type A's. About 2,700 Type A noninterviews were

³Type A includes noninterviews due to refusals, not at home, temporary absence, or language barrier.

⁴Sundukchi (1999) reports but does not evaluate response rate differences of slightly less than 2 percentage points between the \$20 and \$0 wave 7 treatment groups in the experimental portion of the sample.

eligible for the test over the two waves. For noninterviewed cases, SIPP's usual procedure is to send a special letter to try to convince nonrespondents to participate in the upcoming round of interviews. On average, about 33 percent of noninterviews are successfully converted to interviews in the next wave. In the experiment, letters to nonrespondents were sent by priority mail and for households in the incentive groups, they contained a debit card and PIN. As in previous experiments, the incentive was not conditional on an interview.

Table 3. Conversion Rates for Wave 7, 8, and 9 Households by Treatment Group by Type of Noninterview by Poverty Stratum

Wave	Noninterview Followup/Incentive Treatment	Type A Noninterviews (Occupied hhs)				Stratum	
		Unweighted N	Total	Refusal	No one home, temp absent	Pov	Non pov
7	Traditional (usual letter, regular mail, no incentive)	~1,500	41.0	29.8	58.1	N/A	N/A
8	Experimental (priority mail), all groups	2,737	50.5	41.4	64.2	54.6	49.5
	& \$0 group (priority mail, regular letter)	889	45.9	36.1	61.1	47.1	45.7
9	\$20 group (priority mail, incentive letter, \$20 debit card)	927	51.2	41.1	66.1	61.1	48.8
	\$40 group (priority mail, incentive letter, \$40 debit card)	921	54.1	46.8	65.2	54.9	54.0

Source: Martin, Abreu, and Winters (2000).

Both incentive amounts significantly improved the conversion rate for Type A noninterviews (to 51.2 and 54.1 percent for \$20 and \$40, respectively), compared to 45.9 percent for no incentive. There was no significant overall difference in response rates between the two incentive amounts.

Although the comparison is nonexperimental, the results also suggest that sending the advance letter by priority mail, without an incentive, may have improved response. The conversion rate for the control group was 45.9 percent, which is significantly higher than the conversion rate of 41.0 percent in wave 7. Conversion rates for Waves 3-6 are 30.8, 35.1, 29.7, and 38.6 percent, respectively.

The results presented in Table 3 seem to suggest that incentives were more effective for prior wave refusals than for other noninterviews. Compared to the control group, \$40 resulted in a 10.7 percentage point increase in the conversion rate for refusals, compared to a 4.1 percentage point increase for noncontact cases. However, this difference is not statistically significant, so we conclude that the effect of the incentives did not differ for refusals and other noninterviews.

The effect of incentives interacted with poverty stratum. The \$20 incentive was more effective relative to \$0 and \$40 in the poverty stratum than in the nonpoverty stratum. (Note that the highest conversion rate in the poverty stratum was obtained in response to \$20, not \$40, although the

difference between 61.1 and 54.9 percent is not statistically significant.) The differential effectiveness of \$20 may not be a direct effect of poverty, per se, however. Additional analysis suggests the effect may be due to the indirect effects of marital status and race, which are correlated with poverty stratum and which interact with incentive amounts in their effects on conversion rates. When incentives do not attract sample households uniformly, their use may influence the demographic composition of the interviewed sample. Indeed, there are significant differences in the characteristics of each of the three treatment groups: in the \$20 group, 23.2 percent of the interviewed households are in the high poverty stratum, compared to 19 and 20 percent in the \$0 and \$40 groups, respectively (Martin, Abreu, and Winters, 2000).

4. Wave 10, 11 & 12: Continuation of \$20 and \$40 Incentives on Obtaining Interviews from Previous Nonrespondents

After the use of targeted incentives in Waves 8 and 9 proved successful, we continued the practice of offering incentives to first time Type A noninterviews in the remaining waves of the 1996 panel. The only change was to drop the control group for the refusals, so they became eligible for a debit card of either \$20 or \$40. The experiment continued for the no one home or temporarily absent Type As, who received \$0, \$20 and \$40. At this time no results are available. For these waves, offering incentives to convert refusals became less of a test and more of a standard practice.

D. SIPP Incentives: Was it Worth the Cost?

It is difficult to provide a good cost/benefit analysis because there are so many factors that affect data collection costs that we are unable to control for. SIPP is particularly difficult because of its topical modules and overlapping panel design in earlier years. Our cost and accounting system and the nature of field work make it difficult to associate costs with particular cases to fully evaluate the cost benefits that may be associated with particular households that are eligible for incentives.

Some data from Wave 1, shown in Table 4, suggest that incentives may reduce time and money spent in reaching respondents. These results are based on reports from 78 percent of all Field Representatives.

Table 4. Average Miles and Number of Personal Visits per case: Wave 1 SIPP 1996

Incentive Group	Miles per case	Personal visits per case
\$0	42.78	3.6
\$10	43.07	3.4
\$20	40.61	3.46

Source: James (1997)

Over Waves 7-12 (when interviewing is mostly by telephone), the cost per completed interview declined, suggesting that interviews required less time to complete, perhaps because fewer attempts

were required to get an interview. No data are readily available for miles and visits per case or for attempted telephone contacts in later waves.

Looking at the SIPP 1996 panel globally, we spent about \$415,000 in incentives for respondents over the life of this 4-year panel, less than 1 percent of the total data collection costs for the panel. If we had not offered incentives to nonresponding households in Wave 8 and beyond, our best estimate is that the final cumulative nonresponse rate after 12 waves would have been 36 percent, about 2.25 percentage points higher than the actual rate.⁵ This would have meant the loss of an additional 880 units that we would not have had interviews for at the completion of the panel (longitudinal loss).

II. The Survey of Program Dynamics (SPD)

A. Design and Methodology

The SPD is the vehicle for assessing changes in behavior and economic well-being resulting from the 1996 welfare reforms. It is the only survey that provides both baseline and longitudinal data on individual and family outcomes. The data gathered for the 10-year period (1992-2002) will aid in assessing short- to medium-term consequences of the welfare legislation.

Congress specifically directed the Census Bureau to continue to collect data on the 1992 and 1993 panels of the SIPP in Title 42, United States Code, Section 614 (Public Law 104-193, Section 414, signed August 22, 1996). The use of the 1992 and 1993 SIPP panels provides the baseline for pre-welfare reform data; however, it also presents several challenges for the collection of quality statistical data.

Households selected for the SPD had already provided 9 or 10 waves of detailed data over a three-year period and on average contributed 10 hours of their time in the interview process. At the last wave of SIPP interviews, respondents were thanked for their time and told that there would be no more interviews. Then one or two years later, these respondents were contacted and asked to continue in the SPD for 7 more years.

B. Motivation for Using Incentives in SPD

The first round of SPD interviewing was completed in 1997 with the 1992 and 1993 SIPP Panel households that were still participating at the end of each panel. Thus, SIPP sample households which had dropped out prior to the end of the panel were not recontacted in the 1997 SPD survey. This implies that SPD "inherited" a 26.6 percent sample loss rate from the original SIPP panels.

Noninterviews from the 1997 Bridge survey were not recontacted in 1998 or 1999. By the end of the 1999 interviewing cycle, the SPD sample loss rate (sample loss includes a sample growth

⁵ This was derived by applying the 1993 panel (when no incentives were used) increases in nonresponse by wave to the 1996 panel nonresponse after Wave 6.

adjustment from the 1992/93 SIPP panels) was about 50 percent of the original 1992/93 sample from the SIPP after the subsampling was accounted for. As shown by prior research on SIPP, sample loss is not uniform but is concentrated in lower income households. Since households in poverty comprise the key target population in the study of welfare reform, the Census Bureau received pleas from policy makers and congressional staff to address this sample bias by using monetary incentives on a wider basis. Up until this point, the use of incentives in Census Bureau surveys had been done experimentally on a smaller scale. As the table below shows, the sample loss rate stabilized and declined to 44 percent of the original SIPP sample at the conclusion of 2000 interviewing as a result of the various incentive experiments described in C below.

Table 5. Sample Loss- An Average of the 1992 and 1993 SIPP Panels and the SPD

Interview Cycle	Eligible HHs	Interviewed HHs	Avg Sample Loss (%)
SIPP 92/93 Wave 1	43,394	39,446	8.8%
SIPP 92/93 Wave 2	44,225	37,936	14.4%
SIPP 92/93 Wave 3	45,043	37,882	16.3%
SIPP 92/93 Wave 4	45,468	37,477	18.1%
SIPP 92/93 Wave 5	45,985	36,985	20.3%
SIPP 92/93 Wave 6	46,437	36,676	21.9%
SIPP 92/93 Wave 7	46,704	36,133	23.6%
SIPP 92/93 Wave 8	47,030	35,761	25.1%
SIPP 92/93 Wave 9	47,273	35,291	26.6%
1997 SPD ("bridge")	48,633*	30,125	41.3%
1998 SPD	32,800**	16,395	50.0%
1999 SPD	33,200	16,659	49.8%
2000 SPD - core	33,600	16,845	49.9%
core + 1997 sample recontacts	33,600	18,716	44.3%

* Only those households interviewed in the last (9th) wave of the 1992 or 1993 panels were sent to the Field for the SPD Bridge; eligible households included outmovers from original sample households.

**The 1998 sample was reduced by subsampling units that were not in the target population for measuring welfare reform effects, i.e., households with incomes greater than 150% of the poverty threshold. Only Hhs interviewed in the Bridge and selected during the subsampling were contacted for interviews in the 1998 SPD. The number of eligible households is derived by assuming uniform sample loss among the subsampled groups

C. SPD Incentive Experiments: Description and Results

1. 1997 SPD: Differential Effects of \$0 and \$20 Incentive in Poverty Households

An incentive experiment was embedded in the first round of SPD interviewing in 1997 to determine the effectiveness of a monetary incentive in achieving cooperation in a follow-up longitudinal survey which had already completed a 3-year interview cycle in 1994 and 1995. A subset of sample clusters containing low income households in the SPD designated sample was selected to receive a \$20 voucher. This experimental group consisted of 10,683 households that were at or below 150 percent of their poverty threshold and their sample neighbors. The control group consisted of 3,343 households who were at or below 150 percent of their poverty threshold and their sample neighbors. All other households were not eligible.

Based on the results of this incentive test, providing a \$20 incentive to households did have a positive, but not significant, effect on response rates overall, as well as for demographic subgroups (Galvin, 1998). Table 6 shows nonresponse rates overall, and by income and race.

Table 6. 1997 SPD Nonresponse by Incentive Group by Household Characteristics

Incentive Group	Total	Low Income	Mid/High Income	Black
\$0	81.47 %	76.13 %	85.35 %	79.43 %
\$20	83.35 %	77.47 %	87.23 %	82.45 %

2. SPD 1999 Incentive Usage

No incentives were used in the 1998 SPD data collection, which undoubtedly contributed to the nearly nine percentage point increase in the overall sample loss rate (from 41.3 percent in 1997 up to 50 percent in 1998). Following endorsements by the user community and the OMB, we suspended experiments in favor of offering \$40 to all Type A noninterview households from the 1998 SPD. This was very similar to the test done in the SIPP Wave 8 and Wave 9 experiment but without a control group. As in the SIPP 1996 procedure, the Type A noninterviews received the plastic debit card incentive in the advance letter, by priority mail, prior to 1999 SPD interviewing cycle. Each receiving household was allowed to cash the incentive regardless of the interview outcome. Indications are that we received a 94 percent response rate in households where the debit cards were cashed. In other words, very few nonrespondents ended up cashing the debit cards they had received. In addition, households that were reluctant to continue the survey in 1999 were given a \$40 debit card as part of the Type A conversion procedures. As shown in Table 5, the sample loss rate held steady at about 50 percent in 1999.

3. SPD Exploratory Attrition Study

In 1999, interviews were attempted with the SPD “core” sample that met the following conditions:

- ▶ were respondents in the last interview waves of the SIPP 1992 and 1993 Panels
- ▶ were respondents in the SPD 1997 Bridge;
- ▶ survived the subsampling done prior to 1998.

Some data users have stated that the SPD data will not be viable because we have not followed every sample person, including nonrespondents, from the SIPP Panel 1992 and 1993. As an initial step toward addressing this issue, an exploratory attrition study (EAS) was conducted. The objectives were:

- ▶ to determine the success rate and cost of tracking down the SIPP Panel 1992 and 1993 sample people who became confirmed noninterviews 5 or 6 years earlier;
- ▶ to determine the differential interview rate for previous SIPP Type A (which are mostly refusals) and Type D noninterviews (people who moved and could not be located). Incentive amounts of \$0, \$50, and \$100 were tested to determine their effects on response rates.

Since the primary interest of the SPD is in low income households, the study included households with total household incomes below 200 percent of the poverty threshold at the time they attrited from the 1992 or 1993 SIPP Panels. A sample of 120 low income households for each of the three (\$0, \$50, and \$100) incentive groups was selected. The households that spawned (or split) from the selected households were also included in the study and were assigned the same incentive amount as the originating sample household. Samples were chosen to represent SIPP refusals and movers who could not be located.

The target response rate was 50 percent. This was based on the experience of the Panel Study of Income Dynamics (PSID) at the University of Michigan, which reportedly obtained a response rate of 50 percent using an incentive of \$50 to bring attritors back into their sample.

Although the samples are small, the results in Table 7 suggest that incentives are a promising strategy for bringing long-ago attritors back into sample.

Table 7. Response Rates from Exploratory Attrition Study

Incentive group	Type of Nonrespondent in SIPP '92 or '93 Panel		
	All eligible cases (N=373)	Type A (refusals) (N=172)	Type D (movers) (N=201)
Total	36.7	41.9	32.34
\$0	28.9	29.4	28.6
\$50	36.7	40.0	33.8
\$100	43.9	53.0	34.3

Source: Galvin et al. (2000)

Although the target of 50 percent was not met, a substantial fraction (37 percent) of the sample was interviewed and returned to the SPD panel. The response rates among the incentive groups differed fairly dramatically, from 29, 37, and 44 percent for the \$0, \$50, and \$100 groups, respectively. However, with the small sample sizes, only the difference between the \$0 and \$100 groups was statistically significant.

One problem that has plagued the SPD as well as other longitudinal surveys is locating cases after losing track of them for several years. About 38 percent of the sample cases for the study were not located at all. On the other hand, 62 percent were located, including 80 percent of the cases which attrited from SIPP as Type As, and 46 percent of those which attrited because they moved and could not be located. These results are encouraging, and suggest that our tracking abilities have improved.

Once they were found, 60 percent of the located and eligible sample households cooperated. Once found, the previous Type D sample households were significantly more likely to rejoin the survey and respond than previous Type A sample households (70 vs. 53 percent response rates). The incentives improved cooperation, which was 51, 60, and 66 percent in the \$0, \$50, and \$100 groups, respectively. Again, only the difference between the \$0 and the \$100 groups is statistically significant.

The average cost per case (sample household) was approximately \$1000, which is considerably higher than the cost per case of the production survey. This reduced efficiency is due to distributing small workloads across the 12 regional offices.

4. 2000 SPD Incentive Usage

In SPD 2000, a \$40 debit card was again distributed as a "maintenance" incentive to households which received or were eligible for an incentive in 1999 and to potential refusals. We also recontacted 3,600 out of 6,700 households that had been noninterviews in the 1997 SPD Bridge, and which had not been contacted in 1998 or 1999. An incentive of \$100 was offered to these households to encourage their participation in the survey. The incentive was given whether an interview was obtained or not. Support for this effort was widespread among data users and additional funds were procured to cover the increase in field costs. Although we had not reached the target of 50 percent response rate in the EAS, our experience in that research led us to expect that, with some additional resources, we could reach the goal of 50 percent response in the recontacted households and maintain response rates in the continuing sample. Our goal in revisiting these eligible households was to raise the overall interview rate to between 55-57 percent. As table 5 shows, we achieved our goal in 2000 and raised the overall interview rate to 55.7 percent (in other words, sample loss declined to 44.3 percent).

D. SPD Incentives: Was it Worth the Cost?

Table 8 shows the cost per case and interview yield for four cycles of the SPD administered thus far.

Table 8. SPD Costs and Interview Yield

	Incentives Use	Cost per interviewed case (including incentive costs)	% cost change from prior year	% change in number of households interviewed
1997 SPD	\$20 to about 3/4 of poor households, randomly selected	\$216	N/A	N/A
1998 SPD	None	\$317	+ 46.7%	- 45.6%
1999 SPD	\$40 to '98 nonrespondents; \$40 for nonresponse conversion	\$299	- 5.6%	+ 1.6%
2000 SPD (core + 1997)	\$40 "maintenance" incentive to previous incentive recipients; \$40 for refusal conversion; \$100 to recontacted '97 nonrespondents	\$332	+ 11.0%	+ 11.1%

Notes: The 1997 "bridge" survey consisted of a much larger sample (economies of scale) and a much shorter interview (fewer minutes per case)

The figures are not exactly comparable (see footnote), and reflect the effects of a large sample cut in the 1998 SPD. (Interestingly, in that year the cost per case increased by almost the same fraction as the reduction in sample.) They do illustrate that average cost per case declined in 1999 with the use of incentives, compared to 1998 when no incentives were used. And, although the cost per case rose in 2000 when we used \$100 incentives to bring back prior noninterviews from 1997, there was a similar rise in the number of interviews. In addition, the costs for locating and interviewing prior nonrespondents turned out to be lower than expected. The use of unconditional incentives in some ways proved more cost-effective than might have been expected, since relatively few nonrespondents ended up cashing them.

III. Respondent and Interviewer Reactions to Incentives

A. Respondent Reactions

In discussions about the possible use of incentives in the SIPP, there was some concern that incentives in a government survey might be viewed as inappropriate by respondents. To investigate these concerns, an open-ended debriefing question was added at the end of the SIPP instrument to identify possible negative reactions to the incentives targeted to nonrespondents in Waves 8-12. Respondents (including breakoffs) were to be asked, "How did you feel about receiving an incentive to participate in this survey?" Unfortunately, an error in the automated instrument resulted in the data not being captured in waves 8-11. Reactions of wave 12 incentive recipients were largely positive. Of the 365 designated incentive recipients who began the wave 12 interview and for whom an entry to the question was made,

- 32 percent registered positive reactions;
- 4 percent were negative;
- 20 percent were neutral, didn't remember or didn't know about the incentive, or had other

sorts of reactions or comments;

- 6 percent said they hadn't received an incentive; and for
- 38 percent no reaction was recorded (in most cases the question apparently wasn't asked due to breakoff or other reasons).

Thus, positive reactions outnumbered negative ones by 8 to 1, although it should be remembered that really negative individuals probably never began the interview, and a large fraction of households had dropped out of the survey before wave 12. Most positive reactions were surprise and pleasure at receiving the money; a few respondents indicated the incentive made the survey more credible ("wife got ATM card and felt beholden to us...she now believes we are 'for real'"). The incentive created a sense of guilt or obligation to which some respondents reacted positively ("made me feel guilty enough to do it but not enough to answer the financial questions") and others negatively ("threw it away, did not want to be obligated"). The common perception that accepting an incentive created an obligation to participate may explain why so few nonrespondents cashed the debit cards.

Some respondents felt the money was insufficient or owed ("she thinks for the length of time it should be a lot more") but provided some compensation for the time they had spent. Several indicated the money was not important, either because they had other reasons for participating ("if this is helping the poverty rate that is more important") or because it didn't overcome their objections to the survey ("I cut it up, I do not want anything from you, just be left alone").

The perception that it was inappropriate for the government to give incentives appeared to be quite rare, although not inconsequential. Only one respondent made a comment to this effect ("he thinks we are just wasting money that should be used for defense and social security"). However, an irate respondent lodged a complaint about misuse of government funds, resulting in an investigation by the inspector general. Ironically, the complaint was about the use of priority mail, rather than the use of incentives.

B. Interviewer Reactions

One of the unknowns about the use of incentives is how they affect interviewer expectations and behavior. As Singer, Van Hoewyk, and Maher (2000) note, an incentive may boost cooperation due to its effect on the interviewer, or its effect on the respondent. Even if a respondent is indifferent, interviewers may approach a household more confidently when an incentive has been sent in advance, resulting in greater success (cf. Groves and Couper, 1998, on interviewer confidence as a factor contributing to higher response rates). In our experiments, interviewers knew which households had been sent incentives, so their expectations might have influenced the results obtained in the SPD and the SIPP. However, Singer, Van Hoewyk, and Maher (2000) shows that telephone survey response rates improve even when interviewers are blind to an incentive treatment, suggesting that the immediate positive effect of incentives is due to their influence on respondents rather than interviewers.

Another question is how incentives influence interviewers over the longer run. Singer (2000) cautions that "If incentives come to be seen by interviewers as a normal tool for persuading

respondents, the absence of incentives from some studies may negatively impact their performance.”

There is some indication in the SIPP wave 1 experiment of possible interviewer effects, although the evidence is not clearcut. Households in all four rotations were assigned to experimental treatment groups, but only households in rotations 2-4 actually received incentives. The intent was to use rotation 1 cases as a benchmark for comparison with rotations 2-4. However, there were large and unexpected differences in response rates among the rotation 1 incentive treatment groups -- even though none of the rotation 1 households actually received incentives! The differences dwarf and reverse the direction of the treatment differences for rotations 2-4, which did receive incentives⁶ (see e.g., Mack et al., Tables 4 or 7). This puzzling result is found for poor households and for black households, and may result from sample differences among treatment groups, or from interviewer effects. Possibly, interviewers in the incentive groups came to rely on the incentives to keep their overall nonresponse rates low, devoting less effort to their rotation 1 (nonincentive) households in the later waves. This might explain why response rates apparently deteriorated in rotation 1 households in the incentive conditions.

Although we cannot fully account for the puzzling rotation 1 results with any reasonable explanation, they do alert us to be on the lookout for possibly adverse effects of incentives on interviewers’ expectations and performance. As Singer (2000) cautions, there is a slippery slope with incentives use, perhaps greased by interviewer expectations. Administrative staff in field organizations may come to rely upon incentives to compensate for weak field efforts. This apparently was an unintended result of an incentives experiment conducted by Statistics Canada (Tremblay, 2000). Unbeknownst to the researchers, field administrative staff reassigned weak interviewers to areas which were designated to receive a valued incentive, thus obscuring the effects of incentives. We do not have evidence of this sort of effect in the Census Bureau’s experiments.

Finally, we directly solicited interviewers’ reactions to the use of incentives through various debriefing sessions and field reports. Several of the field staff’s more important reactions and suggestions are as follows:

- ▶ If we use prepaid unconditional incentives in households which have not been visited for a long time, these should not be mailed out as they were for the SPD 2000 noninterview cases that were brought back from the 1997 sample. There are no guarantees that the correct household receives them, because of all the movers.

⁶For example, wave 6 nonresponse rates among households that were in poverty (as of wave 1) were as follows:

	Treatment Group Assignment		
	\$0	\$10	\$20
Rotation 1 (no incentives)	16.39%	24.88%	25.27%
Rotation 2-4 (received incentives)	23.50%	19.64%	20.63%

Source: Table 4, Mack et al. (1998). Rotation 1 vs. 2-4 differences, and \$0 vs. \$10 and \$0 vs. \$20 differences, are statistically significant.

- ▶ The Regional Offices would like to be able to hand out debit cards at the door rather than mail them with the advance letter. The incentive should be introduced in the advance letter, but handed out by the interviewer. This gives the interviewer an extra tool to obtain interviews.
- ▶ Once you provide a large incentive and gain cooperation, we should continue to offer some kind of incentive in future contacts.
- ▶ \$40 is the minimum denomination we should offer for SPD households.
- ▶ While \$40 may be appropriate in SPD and later waves of SIPP, \$20 may be appropriate in early waves of SIPP.
- ▶ Monetary incentives should be conditioned on obtaining an interview and offered at the discretion of the FR or the SFR who is in the best position to make that determination.
- ▶ Small monetary or nonmonetary incentives should also be available in the FR's "tool kit" as a reward for cooperative households as well.
- ▶ If debit cards are mailed out, the PIN number should not be included. Many felt we were throwing money away, particularly at the initial contact in controlled experiments. (However, as noted above, it turned out that relatively few nonrespondents cashed in an incentive, even though they could have done so.)

Feedback and discussions with field and survey operations staff also suggested a number of operational issues and problems that need to be addressed in the future:

- ▶ Access to debit cards should be strictly limited and controlled. There were a few isolated instances of cards being cashed by staff rather than respondents. A good control system and alert managers caught these in the SPD 2000, perhaps because the prior year few controls were in place. But these controls must be constantly monitored.
- ▶ Although debit cards are more vulnerable to misuse, paper vouchers are clumsy and even more difficult to manage.
- ▶ Incentives can backfire, as suggested by the case of the individual who complained about misuse of government funds in a SIPP experiment. This may be an argument for having the interviewers decide whether or not to offer an incentive: they know their respondents much better than headquarters does.
- ▶ Proper evaluation of the effectiveness of incentives requires careful planning and instrument design for the capture and processing of the data. The evaluation should be concurrent with data collection so adjustments can be made as needed.

- ▶ When budgets are cut, incentives will be the first item cut.
- ▶ Control the release of debit cards to the ROs. Too many, too soon invites their misuse and complicates the control process.
- ▶ In the reinterview process, include a question on receipt of debit card in households that are eligible for an incentive.
- ▶ Controlled experiments are difficult to administer uniformly across 12 regional offices and may actually have unintended consequences.

IV. Conclusions and Issues for Further Investigation

The SIPP and SPD experiments taken as a whole demonstrate that incentives resulted in beneficial but modest improvements in response. Larger effects were seen for incentives targeted to nonrespondents in the prior wave than for initial incentives given to everyone. An initial incentive of \$20 was more effective than \$10, but \$40 targeted to nonrespondents from a prior wave was not more effective than \$20. Incentives appear effective for both poverty and nonpoverty households; the design of the experiments makes it difficult to conclude whether the effects differ between these two groups. (In the experiment that targeted incentives to nonrespondents, \$20 was more effective relative to \$0 or \$40 in the poverty stratum, compared to its effects in the nonpoverty stratum.) Priority mail alone may help reduce attrition of prior wave nonrespondents. Several issues remain in need of further investigation, including:

- ▶ *The effect of incentives on nonresponse due to no contact.* The experiment that targeted incentives to nonrespondents found lower attrition rates both for prior wave refusals and other noninterviews (mostly no contact), but the effect for the latter was not statistically significant (nor was it significantly different from the effect for refusals). Thus, we do not yet have conclusive evidence to determine whether targeted incentives are effective in reducing nonresponse due to no contact.
- ▶ *Effects of frequency and amount of incentive.* It is not clear whether, and how much, the booster incentive in Wave 7 of the SIPP reduced attrition, above the effects of the wave 1 incentive. In a longitudinal survey, careful thought and further research are needed to address the issue of how often, and in what amounts, incentives should be provided in successive waves of interviewing. This should be addressed in an experimental study.
- ▶ *Effects of incentives on data quality.* An incentive resulted in lower rates of missing data for wage amounts in the SIPP Wave 1 experiment. A \$20 incentive targeted to nonrespondents was differentially effective in the poverty stratum, resulting in a significantly greater representation of poverty stratum households in \$20 treatment group compared to either the control group or the \$40 group. Additional research is needed on the possible effects of

incentives on completeness and quality of data, on response distributions, and on sample composition.

- ▶ *Implementation of incentives by FR and region.* As discussed above, there were some very puzzling results suggesting that, in the SIPP incentive treatments, the rotation 1 households that did not obtain incentives obtained unusually low response rates. Were interviewers in incentive treatments relying upon the incentive cases to keep their response rates up to required levels, and expending less effort in the no-incentive rotation 1 cases? Did some regional offices assign their less experienced interviewers to the incentive treatments in an effort to boost overall response rates? We need to assess positive and negative effects of incentives on interviewer expectations and behavior, and to monitor the implementation of incentives programs in the regions.
- ▶ *Effects of prepaid vs. discretionary incentives.* Should incentives be prepaid, or should interviewers be permitted the discretion to choose who and under what circumstances an incentive is offered? The latter option is appealing in certain respects: It may be more cost effective, since interviewers may be able to judge when an incentive is likely to prove effective, and offer it only in those circumstances. (However, the relatively few people who received an incentive but refused an interview actually mailed in the voucher or cashed in the debit card; the result was a reduced cost.) On the other hand, it may be less effective to have an incentive offered in person at the door than in an advance letter to respondents. An advance letter may be a more effective way of notifying respondents, especially those who are hard to reach. Interviewers may not be good judges of when an incentive will prove effective. Regions and interviewers may come to rely on incentives, rather than strong interviewing and refusal conversion efforts. Variability in interviewers' (and regional offices') implementation of incentives may increase nonsampling sources of error and bias in the data.
- ▶ *Effects of different forms of monetary incentives.* In the experiments reported here, vouchers and debit cards were used; both were given unconditionally. However, a voucher is in a sense a promised rather than a prepaid incentive, because recipients had to mail back information in order to receive a check. Because of the form the incentive took, respondents may not have believed it was truly unconditional. Debit cards are closer to cash, yet the fact that so few nonrespondents cashed them suggests that they may not have been perceived as unconditional either. However, vouchers are closer to promised incentives and debit cards closer to prepaid incentives, and the evidence from the SIPP experiments suggests that the latter may have been more effective than the former. Debriefing questions asked of incentives recipients in reinterview may clarify their perceptions of the incentives. In any case, the literature has tended to treat promised incentives as equivalent to contingent incentives, but these are distinct.
- ▶ *Improvements in the capture, control, and tracking of incentive use.* The difficulties of implementing and controlling field experiments on the use of incentives has limited the

information that has come out of them, including reliable information about their costs and benefits.

V. Planning Future Incentives Programs in the SIPP and SPD

The incentive program for the 1996 SIPP panel evolved from a controlled experiment using paper vouchers in Wave 1 and not conditioned on response, to a remedial effort to retain reluctant respondents in sample through Waves 10, 11, and 12. The incentive program for the SPD underwent a similar evolution, although cumulative nonresponse was a much more significant problem and maintaining a core of respondents was more critical to the success of the program. Based on those experiences, we have modified and refined our plans for using incentives in the SIPP and SPD.

A. SIPP Incentive Plans

Our original plans for the SIPP 2000 panel were to continue with \$20 and \$40 prepaid debit cards in all households but to vary the frequency with which they are distributed over the life of the panel to determine which frequency and amount is most effective at minimizing nonresponse. This was based on the results of the 1996 panel experiments where it was clear that incentives improved response and we no longer needed a control group, but we hadn't determined whether we needed to continue the practice throughout the panel. This was also based on other studies that had found prepaid unconditional incentives to be most effective at reducing nonresponse.

A reduced FY00 budget for the SIPP 2000 panel precluded the use of any incentives at all. A further reduced FY01 budget and the disapproval of a budget initiative to improve poverty measurement required us to eliminate the 2000 panel altogether after 2 waves, and to choose between cutting the sample size for the 2001 panel or severely limiting the incentive program. We chose to reduce the incentive program since we felt that after the high attrition rates in 1996, we had to start the new panel with a large sample and an incentive plan in place from the start, one that would be flexible enough to expand or contract as the budget situation permits.

Because our Field staff wanted some discretion in deciding when to use an incentive to gain some cooperation, we will add a new treatment group to the 2001 panel incentive program. We will also continue to use the targeted incentive for previous nonresponders that was used in waves 10-12 of the 1996 panel. A control group which is never eligible for incentives is also planned, so we will be able to determine which type of incentive (discretionary/conditional versus targeted/unconditional) is more effective over the life of the panel. Table 9 summarizes the treatment groups and number of eligible housing units per wave.

Table 9. Planned Incentive Use in the 2001 Panel in SIPP

	Control Group: <i>Ineligible</i> for Incentive	Experimental Groups: <i>Eligible</i> for Incentives	
		Treatment 1-Discretionary: Limited number of \$40 debit cards to be used at the discretion of field staff, conditional on completion of interview 1/	Treatment 2-Targeted in advance: Limited number of \$40 debit cards mailed to nonrespondents from previous waves
Number of housing units	12,510	25,020	12,510
Implemented in:	Waves 1-9	Waves 1-9	Waves 3-9

1/ This treatment also empowers the 12 regional office field managers to determine who would actually offer the debit card to the respondent: the FR at the door when reluctance is first detected,; a supervisory FR when the case is assigned for refusal followup; or the Regional Office staff, who could mail the debit card and then assign the case for follow-up a few days later.

At this time, we estimate that we will only have sufficient funds to budget about 10 percent of the incentive-eligible workload per wave, but the program's design allows us to increase this percentage as the budget permits.

The appropriate number of debit cards will be distributed at 3-wave intervals to the ROs; at the end of the 12-month period, uncashed cards will be closed out, or deactivated. We will provide tools and guidelines for the ROs to use in managing the FR incentive budget and tracking the use of the cards. If the funding situation improves we may be able to increase the size of the incentive budget accordingly. In addition, we will assess whether we need to continue to condition the incentive on completing the interview, since the SPD experience (admittedly, a more burdensome survey) proved that 94-95 percent of households where incentives were cashed resulted in a completed interview.

B. SPD Incentive Plans

Our original plans for using incentives in the 1999 SPD were to mail every household a \$20 debit card prior to the FR's visit. This proposal was delivered to the OMB at the same time we had begun providing \$20 and \$40 incentives to new nonrespondents in Waves 10, 11, and 12 of SIPP. OMB requested that we reduce the scope of the 1999 incentive program by offering debit cards only to nonrespondents from 1998 and to new nonrespondents in 1999. They also requested that we test the feasibility of recontacting nonrespondents from 1997. Thus, FY99 funds had to be diverted to the Exploratory Attrition Study described above in II.C.3. Additional funding from another source enabled us to bring back prior nonrespondents in 2000.

For the 2001 SPD, we will continue offering a maintenance incentive of \$40 to households that received an incentive in 2000 and to those noninterview cases recontacted after the 1997 SPD Bridge. Pending receipt of additional funding, we also plan to reintroduce 6,000 noninterview cases

from the 1992 and 1993 Panels of SIPP using a \$100 incentive. The goal will be to improve the overall response rate to at least 62 percent.

Next year, SPD will also include an additional series of questions for each adolescent age 12-17 to complete using a self-administered questionnaire (SAQ). The FR must obtain permission from the parent in order to contact the adolescent. This was done in the 1998 survey and we only obtained a 58 percent response rate among the eligible adolescent population. We are offering a \$40 incentive to all of the households in the core SPD and the 1997 noninterviews that we brought back into the survey this year. This incentive will be conditioned on the completion of the SAQ for all adolescents in the household. Households brought back from the 1992 and 1993 SIPP who receive a \$100 incentive will not be eligible for this supplemental incentive.

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