Faculty Working Papers

A COMPARISON OF ALTERNATIVE PROCEDURES FOR COLLECTING CONSUMER EXPENDITURE DATA FOR FREQUENTLY PURCHASED PRODUCTS

Seymour Sudman and Robert Ferber

#87

College of Commerce and Business Administration
University of Illinois at Urbana-Champaign

FACULTY WORKING PAPERS

College of Commerce and Business Administration

University of Illinois at Urbana-Champaign

February 6, 1973

A COMPARISON OF ALTERNATIVE PROCEDURES FOR COLLECTING CONSUMER EXPENDITURE DATA FOR FREQUENTLY PURCHASED PRODUCTS

Seymour Sudman and Robert Ferber

#87

A COMPARISON OF ALTERNATIVE PROCEDURES FOR COLLECTING CONSUMER EXPENDITURE DATA FOR FREQUENTLY PURCHASED PRODUCTS

Seymour Sudman and Robert Ferber

Survey Research Laboratory
University of Illinois
Urbana-Champaign, Illinois

December, 1972

ACKNOWLEDGMENTS

We acknowledge with gratitude the financial and intellectual support from the staff of the U.S. Bureau of the Census. Especially helpful were Barbara Bailar, Thomas Jabine, and Benjamin Tepping. Barbara Bailar was Census Project Officer for this study and coordinated Census — Survey Research Laboratory joint activities. The collection and interpretation of the data were the responsibility of SRL, as are any remaining errors or limitations in this report.

The field work for this project was supervised by Michael Cox and Teresa de Jesus of the Chicago Office of SRL. Ronald Czaja supervised the sample selection, while Alan Langowski was responsible for data reduction. Data processing was supervised by Nancy Morrison and Michael Burmester, and this report was edited by Mary Spaeth.

1. Introduction

It is now generally recognized that the use of recall methods for collecting expenditure data for frequently purchased products is inadequate (Flueck, Waksberg, and Kaitz, 1971; Pearl, 1968; Sudman, 1964; Sudman and Ferber, 1971). The single best alternative to recall procedures is the use of diaries, and this is the method adopted by the U.S. Bureau of the Census for the national Consumer Expenditure Survey being conducted in 1972-73.

Diary methods are not perfect, however. A major problem that remains to be investigated is the danger of sample biases due to the unwillingness or inability of some households to keep written records, a problem especially severe among less well educated households, particularly in the inner city. The present pilot study was undertaken by the Survey Research Laboratory (SRL) of the University of Illinois with the cooperation of the Bureau of the Census to test the diary approach against some alternatives. The chief alternative considered was the use of daily telephone calls to the household, a method suggested by Flueck, Waksberg, and Kaitz (1971) and by the growing success of SRL and other organizations with telephone interviewing. Another alternative tested was the respondent's free choice of weekly diaries, daily telephone calls, or the use of a tape recorder.

The relative effectiveness of these alternative methods was compared on the basis of three criteria:

1. The level of household cooperation.
2. The level of household reporting by expenditure categories.
3. The level of reported expenditures by individual household members for food and drink away from home.

In addition to the method of data collection, two other factors were tested to ascertain their effect on cooperation and level of reporting:

1. Compensation of households versus no compensation.--Half the households were scheduled to receive $5.00 for keeping a diary or for cooperating in one of the other collection methods for two weeks; the other households received no compensation.

2. Auspices of the survey.--In general, the U.S. Bureau of the Census achieves a higher rate of cooperation on its continuing surveys than do private survey research organizations. For this study, we attempted to learn whether auspices would have any effect on cooperation or on reporting of consumer expenditures. Half the households received an advance letter signed by the Director of the Bureau of the Census while the other half received a letter signed by the Director of the Survey Research Laboratory (see Appendix B). While the interviewer was always identified as being from SRL, in half the cases the respondent was told the data were being collected for the Census Bureau and in the other half that the data were being collected for SRL.

Sampling Design

The study was conducted in the Chicago Standard Metropolitan Statistical Area during the period of April-August, 1972. The sample households selected were a subsample of the master sample for Illinois selected and listed by

the Survey Research Laboratory. This sample, which is used for a variety of purposes, is a multistage area probability sample of the state. Census tracts and blocks or segments within tracts have been selected with probabilities proportionate to size. The selected blocks have been listed, and the sampling rates within each block are computed and households selected as required for each new study. In this study, only the listings for the Chicago SMSA were used, with differential sampling rates in the central city and suburban areas.

Households were randomly assigned to the method of data collection, compensation or no compensation, and auspices of the study. Of the households that were given a choice of keeping a diary, receiving daily phone calls, or using a tape recorder, none chose the tape recorder and only a few selected the daily phone calls, while most preferred to use the diary.

Field Procedures

Every selected household received an advance letter telling it about the survey. Depending on the auspices, this letter was either on Census Bureau or SRL stationery and signed by either the Director of the Census Bureau or the Director of SRL. All households received an identical initial interview asking about usual shopping behavior.

After the initial interview, the respondents were asked to provide expenditure information for the next two weeks by the method to which they had been assigned. No switching was permitted. Thus, a respondent who refused to keep a diary but might have been willing to respond by telephone was treated as a refusal.

Diaries were picked up at the end of each week, while phone calls were made daily. At the end of the two-week period, a final interview was conducted with all available household members other than the respondent to obtain information on food and drink purchased away from home and clothing purchases.

Organization of Results

The main results of this study are discussed in the four sections that follow. The next section considers the effect of the alternative procedures for data collection on cooperation. Section 3 discusses the effects of the alternative procedures on the level of household reporting by expenditure categories, while Section 4 relates to individual reports of expenditures, especially for food and drink away from home. Section 5 deals with the costs of the various alternative methods. Section 6 discusses some of the operational procedures used in the study, and a final section briefly summarizes the results and suggests some possible implications as well as possible additional research.

2. Effect of Alternative Procedures on Cooperation

Initial Cooperation

Initially, 409 households were selected, 288 in the city of Chicago and 121 in suburban areas. As is evident from Table 1, which gives the results of the initial contacts, the overall cooperation rate was only 60 percent, with high refusal and noncontact rates. This was due to lack of concentrated follow-up activity on refusals and noncontacts to the initial interview. We decided to allocate the limited resources available almost exclusively to obtaining cooperation on the consumer expenditure portion of the study, although had we anticipated that the response rates would be so low, more effort might have been allocated to increasing response. While this relatively low cooperation should be kept in mind, we have no reason to suspect that it has any major impact on the comparisons of the alternative data-collection methods.

As one might suspect, cooperation was slightly higher in the suburbs than in the central city. Overall, Census Bureau auspices resulted in a slightly higher initial cooperation rate than did Survey Research Laboratory auspices. There appears to be an interesting interaction in these results, however. While there was only a one percentage point difference for cooperation between Chicago and suburbs under SRL auspices, there was almost a 10 point difference between Chicago and suburbs under Census Bureau auspices. Thus, while government auspices produced higher cooperation in both Chicago and suburbs, they were more effective in the suburbs.

TABLE 1

COOPERATION OF SAMPLE ON INITIAL INTERVIEW

Result	Total		Chicago		Suburbs	
	N	Percent	N	Percent	N	Percent
Total sample:						
Total selected	422		301		121	
Vacant or nondwelling	44		38		6	
Total occupied	378	100.0	263	100.0	115	100.0
Initial interview completed	227	60.1	154	58.6	73	63.4
Refusal	78	20.6	54	20.5	24	20.9
Noncontact	57	15.1	40	15.2	17	14.8
Other (language, illness, etc.)	16	4.2	15	5.7	1	0.9
Census Bureau auspices:						
Total occupied	197	100.0	141	100.0	56	100.0
Initial interview completed	124	62.9	85	60.3	39	69.6
Refusal	35	17.8	26	18.4	9	16.1
Noncontact	29	14.7	21	14.9	8	14.3
Other	9	4.6	9	6.4	0	0
SRL auspices:						
Total occupied	181	100.0	122	100.0	59	100.0
Initial interview completed	103	56.9	69	56.5	34	57.6
Refusal	43	23.7	28	23.0	15	25.4
Noncontact	28	15.5	19	15.6	9	15.3
Other	7	3.9	6	4.9	1	1.7

There were no differences in initial cooperation due to the interviewers' knowledge of the diary form and the compensation that respondents were to receive. In an earlier study, interviewers' knowledge of whether respondents were to receive compensation did influence initial cooperation (Sudman and Ferber, 1971). However, in that study only diaries were tested while here phone and choice alternatives were available. This more complex design may have reduced or eliminated interviewer concern about compensation. Whatever the reason, the earlier finding is not replicated in this study.

Panel Cooperation

Of the three variables studied, compensation appears to be the most important, as may be seen in Table 2. About 85 percent of the interviewed

TABLE 2

PANEL COOPERATION BY ALTERNATIVE PROCEDURES

Procedure	Percentage Level of Cooperation				N
	Full	Partial	Refusal	Total	
Method:					
Diary	71.3	4.6	24.1	100.0	87
Telephone	75.4	7.2	17.4	100.0	69
Choice	73.3	5.6	21.1	100.0	71
Compensation:					
Yes	79.3	5.4	15.3	100.0	111
No	67.2	6.0	26.8	100.0	116
Auspices:					
Census Bureau	72.6	4.0	23.4	100.0	124
SRL	73.8	7.8	18.4	100.0	103

households who were compensated participated in the panel for all or part of the two-week period while only 73 percent participated if they were not compensated.

The results are sharpened if one separates Chicago and suburban respondents (Table 3). There it may be seen that neither compensation nor the diary or telephone method made any difference in the suburbs. In Chicago, however, 85 percent of the households who were compensated participated while only 68 percent participated if they were not compensated. Also, in Chicago 82 percent of the households contacted by phone gave some information, compared to 73 percent who kept diaries. One might have

TABLE 3

PANEL COOPERATION IN CHICAGO AND SUBURBS BY ALTERNATIVE PROCEDURES

Procedure	Percent Cooperating Fully or Partially	
	Chicago	Suburbs
Method:		
Diary	72.6 (62)	84.0 (25)
Telephone	81.6 (49)	85.0 (20)
Choice	74.4 (43)	85.7 (28)
Compensation:		
Yes	84.9 (73)	84.2 (38)
No	67.9 (81)	85.7 (35)
Auspices:		
Census Bureau	73.7 (95)	89.7 (39)
SRL	82.6 (69)	79.4 (34)

NOTE: In this and subsequent tables, figures in parentheses are the base numbers for the accompanying percentages.

expected that those given a choice would have had the highest cooperation, but that was not the case; cooperation in this group was about the same as that for diary keepers, since in fact most of these households chose to keep diaries.

The standard errors of the differences in Table 2 are about 5 percentage points and in Table 3 about 7 percentage points for Chicago and 10 percentage points for the suburbs. Thus, while the differences between compensation and no compensation are statistically significant, particularly if combined with earlier results, the other differences are not clearly significant.

The results for auspices seem suggestive and deserve additional exploration, even if not statistically significant. In the suburbs, 90 percent of the households contacted under Census Bureau auspices cooperated in the panel, compared to 79 percent contacted under Survey Research Laboratory auspices. In the city, however, this pattern is reversed; 83 percent of the households cooperated under SRL auspices while 74 percent cooperated under Census Bureau auspices.

In Table 4, the data for the city of Chicago are split by whether compensation was received and by method and auspices. The results are shown separately since no interactions were detected in these data; for each method and auspices, the difference in cooperation between those who did and did not receive compensation was about the same. Thus, almost nine-tenths of the households in Chicago cooperated fully or partially in the panel if they received compensation and were contacted by phone, while only about two-thirds cooperated if asked to keep a diary with no compensation.

TABLE 4

PANEL COOPERATION IN CITY OF CHICAGO BY METHOD, AUSPICES, AND COMPENSATION

Procedure	Percent Cooperating Fully or Partially	
	Compensation	No Compensation
Method:		
Diary	80.8 (26)	66.7 (36)
Telephone	88.5 (26)	73.9 (23)
Choice	85.7 (21)	63.6 (22)
Auspices:		
Census Bureau	80.6 (36)	63.3 (49)
SRL	89.2 (37)	75.0 (32)

It would appear that telephones do offer the opportunity to increase cooperation in the inner city, particularly when combined with compensation, if the data received over the phone are comparable to those obtained from diaries. This is discussed in the next section.

3. Effect of Alternative Procedures on Household Reporting

Phone versus Diary

One might hope that there would be no differences between daily recall and diary methods, but this was not the case. There were substantial differences in the number of purchases reported and less important differences in reported expenditures. These differences were, however, in a different direction from those found with longer recall periods--daily recall was lower than diary reporting.

As seen in Table 5, the number of purchases reported by phone averaged 75 percent of that reported in diaries.[1] Only in the "all other food" category was the number reported by phone higher than the number reported by diary, suggesting that interviewers and coders had greater difficulty in classifying purchases over the phone.

Besides reporting error, one other possible explanation for the differences observed in Table 5 is that some or all of them might be due to differing purchase patterns by the phone and the diary households. Although the experimental design controlled for differing purchase patterns by randomly assigning families to phone and diary treatments, the differential cooperation and the selection of either diaries or phones by those households given this choice could confound these results. However,

---

[1]The three recording methods presented to respondents--diary, phone, and choice of either--are combined into diary versus phone for the purpose of this and later analyses because only 10 respondents selected the telephone of those given a choice.

sampling bias factors cannot account for all or even a majority of the observed differences.

TABLE 5

AVERAGE NUMBER OF DAILY PURCHASES AND AVERAGE DAILY EXPENDITURES BY TYPE OF PRODUCT FOR DIARY AND TELEPHONE, WEEKS 1 AND 2

Type of Product	Number of Purchases			Daily Expenditures		
	Diary	Phone	Ratio of Phone to Diary	Diary	Phone	Ratio of Phone to Diary
Dairy and bakery	1.17	.81	.69	$ .90	$1.29	1.43
Meat, fish, poultry	.79	.50	.63	1.42	1.04	.73
Fruits and vegetables	.91	.64	.70	.54	.37	.69
Beverages	.49	.35	.71	.63	.60	.95
All other food	1.24	1.31	1.06	1.14	1.09	.96
Total food	4.60	3.61	.78	4.63	4.39	.95
Meals and snacks	.79	.49	.62	1.54	1.10	.71
Clothing, linens	.29	.25	.86	2.38	2.22	.93
All other purchases	2.69	1.93	.72	18.83	14.66	.78
Total purchases	8.37	6.28	.75	27.38	22.37	.82
Number of cases:						
Week 1	107	67		107	67	
Week 2	100	62		100	62	

As an extreme case, assume for the moment that households in the city of Chicago who were willing to cooperate by phone but not by diary had a purchase rate only half that of households who kept a diary. The results of Table 3 for the city indicate that approximately 10 percent more households were willing to cooperate by telephoning than by diary. Combining

these figures, the maximum difference due to sample bias would be about 5 percent. Even if one assumed a purchase rate for telephone households only one-third that of diary households, the maximum difference would be only 7 percent. Since the actual differences in the purchase rates are likely to be even smaller than in our assumptions, sample biases undoubtedly account for less than 5 percent of the differences.

The differences are also too large to be attributed to sampling errors, as may be seen by observing the sampling error estimates given in Appendix A.

The differences in reporting may be due to interviewer as well as respondent omissions on the phone interview. Since no effort was made to control interviewer assignments in this experiment, no measure of interviewer effects is possible.

The differences in average daily dollar expenditures were much more variable by product class type than the differences in the number of purchases. For dairy and bakery products, expenditures reported on the phone were much higher than in the diaries, although for all purchases reported, phone expenditures were lower than diary expenditures. The differences in durable goods and services would not be very important, since diaries or phone recall would not usually be used on purchases other than food and meals because of low incidences of purchasing in a short period of time.

How does one explain the different results for number and dollar value of expenditures? One explanation is that respondents remembered the total amount of money spent for food during the day, but forgot some of the details. If this is borne out in additional experiments, it would

suggest that phone interviews could be used to improve estimates of total daily expenditures, but would not be as useful for the details.

One question of continuing concern in any panel is the possibility of conditioning effects. In this experiment, one would be able to observe these effects if there had been major differences in the results of Week 1 versus Week 2. Table 6 indicates that there were no significant

TABLE 6

AVERAGE NUMBER OF DAILY PURCHASES AND AVERAGE DAILY EXPENDITURES BY TYPE OF PRODUCT FOR DIARY AND TELEPHONE BY WEEK 1 VERSUS WEEK 2

Type of Product	Number of Purchases				Daily Expenditures			
	Diary		Phone		Diary		Phone	
	Week 1	Week 2	Week 1	Week 2	Week 1	Week 2	Week 1	Week 2
Dairy and bakery	1.21	1.10	.82	.81	$ .87	$ .92	$ .66	$2.04
Meat, fish, poultry	.81	.75	.48	.54	1.41	1.45	1.04	1.11
Fruits and vegetables	.97	.88	.67	.64	.60	.51	.41	.36
Beverages	.48	.50	.40	.31	.62	.66	.79	.43
All other food	1.12	1.41	1.21	1.51	.74	1.62	1.23	1.03
Total food	4.59	4.64	3.58	3.81	4.24	5.16	4.13	4.97
Meals and snacks	.82	.79	.46	.54	1.40	1.70	1.06	1.17
Clothing, linens	.29	.30	.24	.27	2.08	2.80	1.96	2.68
All other purchases	2.84	2.56	1.80	2.08	20.92	17.34	13.55	15.28
Total purchases	8.54	8.29	6.08	6.70	28.64	27.00	20.70	24.10
Number of cases	107	100	67	62				

conditioning effects for diary keepers. Both on the number of purchases and average daily expenditures on food items and meals, none of the differences was larger than one would expect due to sampling variability. This supports

the findings of the earlier study (Sudman and Ferber, 1971), which also indicated no conditioning effects.

For the phone interviews, however, there is some indication of an increase in the number of purchases and average daily expenditures in the second week as compared to the first, although because of the large sampling variability, these results are only suggestive. Even in the second week, the number of food items that phone respondents reported having purchased is only 80 percent of that reported in diaries.

Compensation Effects

One would not expect that compensation should influence the accuracy of reporting once cooperation is obtained, but in our earlier study we found that noncompensated households reported less completely than compensated households. The same results are not evident in this experiment. Controlling for city-suburban location and diary-phone methods, no significant differences are found in the number of items reported in total, for food products only, or by individual product class types.

We can only speculate why compensation affected reporting on the earlier experiment but not on this one. The earlier experiment tested periods up to four weeks, and the greatest differences between compensated and noncompensated households were found in the third and fourth weeks. Thus, shortening the record-keeping period for the present experiment reduced the effects that compensation had on the level of diary recording. Still, for the first two weeks combined, there was about a 10 percent difference in reported level of expenditures between compensated and noncompensated households on the earlier experiment, as compared to no

difference this time. Aside from sampling error, there may have been some interviewer effects since the previous study stressed gifts as a major variable, with three different gifts being tested, while this time the major variable was use of telephones.

Auspices

One would not expect auspices to affect the level of reporting of cooperating families, and it does not. There are no differences in the number of expenditures reported between families recruited under Census Bureau auspices and those recruited under Survey Research Laboratory auspices. On dollar expenditures for food, SRL auspices resulted in higher reported levels than Census auspices, but these results are likely to be due to sampling variability and differential cooperation.

The same differences are not observed for total purchases or for number of purchases. The possibility that the results may be due to different household characteristics of the respondents using each of the alternative methods is considered as part of the regression analysis to be discussed next.

Multiple Regression Analysis

In order to investigate the extent to which the differences, or lack of differences, observed that were ascribed to the experimental variables might in fact be a manifestation of differences in the socioeconomic characteristics of the households, a stepwise multiple regression analysis was undertaken to estimate the relative importance of these different factors in influencing the reported number of purchases and the average

daily expenditures. Four dependent variables were used in these regressions, all with the household as the unit of observation. These variables were:

1. Number of daily purchases of food products.
2. Number of daily purchases of all products and services.
3. Daily dollar expenditures for food products.
4. Daily dollar expenditures for all products and services.

The independent variables included initially were household size, household income, education of household head, and the following dummy variables: ownership of home, residence in suburb, phone kept instead of diary, compensation paid, and SRL auspices.

The results, in Table 7, support those in Tables 5 and 6. For both number of food purchases and total purchases, only three factors are significant in the regression--household size, household income, and whether the household kept a diary or reported by phone. These three variables explain a fairly sizable portion of the variance in the dependent variables. For food purchases, the variance explained by the regression (R^2) is .29. For all purchases, the explained variance is .35.

For dollar expenditures on food, whether the respondent kept a diary or reported by phone does not enter significantly into the regression, but it is significant at the .05 level on expenditures for all products. The sign of the coefficient is negative, supporting the prior comments on the larger average purchases reported by diary.

A surprise variable that enters into the regression is auspices. For food expenditures, SRL auspices yields a positive beta coefficient, while for total expenditures, the beta is negative, which may suggest that both

results are due to sampling variability. Note also that auspices was not a significant variable in number of purchases.

TABLE 7

MULTIPLE REGRESSION ANALYSIS OF FACTORS AFFECTING RECORDING

Dependent Variable	Independent Variable	Beta	Standard Error	F
Number of food purchases	Household size	.304	.114	18.87
	Household income	.299	.030	18.35
	Reported by phone	-.184	.428	7.48
Number of total purchases	Household income	.365	.041	29.71
	Household size	.297	.155	19.71
	Reported by phone	.201	.580	9.79
Dollar food expenditure	Household size	.309	.221	18.41
	Household income	.284	.065	12.66
	SRL auspices	.125	.800	3.36
	Suburban location	.128	.944	2.78
	Home ownership	.121	.950	2.20
Total dollar expenditure	Household income	.599	.190	74.17
	Suburban location	.127	2.734	3.69
	Household size	.111	.670	2.92
	SRL auspices	-.100	2.448	2.56
	Reported by phone	-.088	2.515	2.00
	Compensation paid	.084	2.444	1.81

For food expenditures, the explained variance is .28 for the regression with four demographic variables--household size, income, suburban location, and home ownership--and with SRL auspices. However, R^2 is already .26 when only household size and income are used, so the other three variables increase the explained variance only from .26 to .28.

For total expenditures, the regression model with three demographic variables (household size, income, and suburban location) and the three

test variables (SRL auspices, reporting by phone, and compensation) explains 40 percent of the variance. However, 38 percent of the variance is explained by the demographic variables and only the remaining 2 percent by the other variables. One would have to conclude that even if the effects of the test variables are barely statistically significant, they are still of little practical importance.

4. Food and Drink Away from Home

Several earlier diary studies have indicated a serious understatement of food and drink away from home by other household members. For example, in our previous experiment reported expenditures were about half that estimated from sales tax records (Sudman and Ferber, 1971). In this experiment, at the conclusion of the two weeks of reporting by either diary or phone, the interviewer asked to speak to all other household members who were fourteen years of age or older about their expenditures for meals and snacks that day.

As seen in Table 8, and as might have been expected, there was a

TABLE 8

ADDITIONAL REPORTS OF EXPENDITURES FOR MEALS AND SNACKS AWAY FROM HOME BY OTHER HOUSEHOLD MEMBERS FOR DIARY AND PHONE

Meals and Snacks	Diary	Phone
Number of purchases:		
Initial number of daily purchases	.79	.49
Additional number of purchases reported	.83	.69
Percentage increase (2/1)	105%	141%
Daily expenditures:		
Initial average daily dollar expenditures	$1.54	$1.10
Additional dollar expenditures reported	$2.01	$ .98
Percentage increase (5/4)	131%	89%

substantial increase in reported expenditures. By diary, the additional number of purchases was more than the initial daily purchases reported, and the dollar amounts were about 1.25 times more than the initial amounts. Thus, the revised estimate is near to what might be expected from sales tax records.

For phone methods, the relative increases from the final interview are of the same magnitude but below the diary levels. Where the daily reporting of expenditures was done by phone, the final interview was also by phone, while for families keeping the diary, the final interview was face-to-face with other household members. It would appear that diaries and face-to-face interviews are more effective than phone methods in involving other household members and increasing the completeness of recording.

As part of the final interview, other household members were also asked about their personal clothing purchases since the start of the month in which the interview took place. For both diaries and phones, there was an increase of about one-third in reported expenditures. It is uncertain, however, whether this reflects improvement in the data or an overstatement due to recall telescoping by other household members. In the earlier study (Sudman and Ferber, 1971), clothing purchases reported by diary agreed closely with sales tax figures.

5. Cost of Interviewing

The costs of alternative methods are important factors in the ultimate design of large-scale data-gathering operations. In this experiment, although interviewers were used in both the phone and the diary methods, an effort was made to separate times and expenses between the two procedures and to estimate costs separately for the city of Chicago and for the suburbs. These results are given in Table 9. The cost

TABLE 9

TIME AND COSTS OF DIARY AND PHONE METHODS
IN CHICAGO AND SUBURBS PER INTERVIEW

Time and Costs	Chicago		Suburbs	
	Diary	Phone	Diary	Phone
Average interviewer time (hrs.)	2.77	5.54	4.90	5.54
Cost @$2.50/hr.	$6.92	$13.85	$12.25	$13.85
Travel cost @10¢/mile and parking or public transportation	5.49	1.65	12.48	5.24
Phone charges	--	1.25	--	4.92
Total cost	12.41	16.75	24.73	24.01
Ratio of phone to diary		1.35		.97
Phone installation charge $19.00 x 15% of households		2.85		
Total cost including installation		$19.60		
Ratio of phone to diary		1.58		

figures are on the basis of $2.50 per hour of interviewer time, 10¢ per mile for travel using private car, and the phone rates in the Chicago area. The results should be treated cautiously because, nationally, there will be some variation in these figures. Also, there was some arbitrariness in time and expense allocation when the interviewer was doing several things on the same trip, such as picking up a diary and conducting an initial interview. Nevertheless, the data should be sufficiently accurate for planning purposes.

In the suburbs, there are no differences in the total costs of the two methods, with the increased time on the phone and the phone charges being almost balanced by the increased travel time and expenses for the diary method. Since phone methods do not increase cooperation in the suburbs and result in less accurate reporting, they could not be recommended for those areas.

In Chicago, phone methods are about 35 percent more expensive although phone charges are less. This is because travel time and expenses are lower in the city for the diary method. Nevertheless, there would be no major impact on costs if a limited number of city interviews were done by phone.

This comparison does not include the costs for telephone installation in those homes without phones. This was done in six households at an average cost of about $19, of which $12 was for installation and $7 for the first month's service charge. If phones were to be installed in every seventh or eighth house, this would add an additional phone charge of $2.50-$3.00 per phone interview and raise the total phone charge in Chicago to about $20.00, or 60 percent more than the diary method.

6. Field and Processing Procedures

In this section, we discuss briefly the operational procedures used in this study. This discussion may be of some use to others who might wish to duplicate this study or conduct a larger consumer expenditure study. The specific methods also influence the cost data presented above.

Time Schedule

Interviewer recruitment	January 21 - April 1, 1972
Design of forms for printing	
a. Final draft of product diaries	March 10 - March 24
b. Final draft of final interview	March 10 - March 24
c. Journal diary for telephone	March 10 - March 24
d. Revised brochure "Where Do Your Dollars Go"	March 10 - March 24
Pretest	
Training	April 3
Data collection	April 3 - April 11
Training and data collection--Wave I	
Mailing for home study	April 20
Printing	April 10 - April 24
Advance letters	
Prepared	March 20 - April 17
Sent	April 24
Training	
Group 1	April 26-27
Group 2	April 28-29
Group 3	May 1
Field work	April 29 - July 14
Wave II	
Advance letters sent	June 19
Field work	June 24 - August 22

Forms Used

Briefly described here are the forms used in this experiment. Appendix B contains copies of these forms.

1. Dear Friend Letter. This letter was mailed with a brochure, "Where Do Your Dollars Go," to each sample address before an interviewer's initial visit. The letter explained the purpose of the survey and informed the occupants that an interviewer would call. Interviewers had extra copies to leave with respondents who had not seen it, but did not offer the letter unless requested.
2. Initial Interview Questionnaire. This questionnaire was used for interviewing the main shopper for all household units that entered the survey during the data-collection period. The interview was designed to gather information on size of household, demographic information, usual place of shopping, and type of store in which purchases were made.
3. Consumer Expenditure Diary. The diary used was a product diary identical to the diary used by the U.S. Bureau of the Census in the 1972 Consumer Expenditure Survey.
4. Final Interview Questionnaire. The final interview was to be made with each household member fourteen years of age or older other than the main shopper. It was administered at the end of the two-week recording period. The questionnaire was designed to gather information frequently missed by household members in consumer recording. Such items are food and drink eaten away from home and clothing items.

5. Telephone Questionnaire. This questionnaire was used to gather information by daily telephone interviewing. Because of the experimental nature of the phone survey, interviewers were encouraged to develop individual probing techniques for eliciting expenditure information. The structure in this phone questionnaire served simply as a guideline.

Staff Utilization

Two supervisors were utilized for this study. Michael Cox and Teresa de Jesus supervised the work of the interviewers, which entailed assigning work, maintaining weekly records of field progress, communicating weekly with interviewers, answering questions concerning the study, and editing completed diaries for accuracy and completeness.

Interviewer Selection and Training

It was decided that 37 interviewers be selected and asked to come to the training sessions. It was assumed that 25-30 would remain with the study and work full time if possible through both waves.

Interviewers were chosen on three criteria: (1) interviewing experience evaluation, (2) location, and (3) interest and availability. The following breakdown of interviewers by area was determined from the sample provided (based on 30 interviewers):

	Chicago	Suburbs
North	10	3
Northwest	4	2
West	9	2
South	2	2
Southwest	5	1
Total	20	10

Of 37 potential interviewers invited to the training sessions, 28 attended.

Verification

A total of 39 interviews, or approximately 20 percent of each interviewer's completed work, was verified by telephone from the SRL office in Chicago. The quality of all interviewers' work for this study was found to be good with all cases verified where contact could be made. Validation questions included demographic information from the initial interview. No expenditure questions were validated since respondent memory errors would have prevented meaningful comparisons. Five cases could not be verified because the respondents did not have telephones, while in one case the respondent had moved to another state.

Problems

There were three major problems with the study. First, as already mentioned, because of the limited resources available, there was no follow-up of refusals to initial interviews, so that the cooperation rate was lower than usual. Second, another field problem arose because of delays in developing forms and obtaining approval from OMB. The field period was delayed and extended into the summer, which in turn lengthened the field period since some households were away from home on vacations.

Finally, there were some problems with the punching and editing of the expenditure data. An examination of the data frequency distributions indicated about 75 cases where the dollar amount punched for the daily expenditures of a specific product was suspiciously large. Most of these proved to be keypunch errors. Substantial time and effort would

have been saved if these errors had been caught in the cleaning program before being put on the final data tape. An easy way to do that would have been to set upper bounds on the magnitude of dollar expenditure by product type and automatically check all cases above that level.

7. Summary and Implications

In this section, we summarize the main results of this experiment with regard to the variables that were tested. Implications of the results are discussed and an assessment is provided of the possible effect of limitations on these results.

Compensation

The results in Chicago, as well as those of earlier studies, indicate that compensation did help to improve cooperation on record keeping--85 percent of compensated households cooperated, compared to 68 percent of noncompensated households. Compensation seems to have no important effect in suburban areas, but it is difficult to see how one could use differential compensation systems in cities and suburbs.

One other positive effect of compensation suggested by the multiple regression results is that the compensated households reported higher average expenditures than other households. Although more expenditures do not necessarily carry any presumption of higher accuracy, past experience suggests that this is indeed likely to be true.

For these reasons, the results of this study would clearly suggest that at least in terms of the information obtained, the offer of compensation may have highly favorable effects.

Auspices

Census Bureau auspices resulted in higher initial cooperation both

in the city and the suburbs. Panel cooperation was higher in the suburbs under Census Bureau auspices but higher in the city under Survey Research Laboratory auspices. However, the final cooperation rates were about the same regardless of auspices.

There was some evidence of suspicion of the government's reasons for doing the survey in the city, but not in the suburbs. In terms of response, however, combining initial and panel cooperation rates, Census Bureau and SRL auspices produced identical results in the city while Census auspices were somewhat better in the suburbs.

Telephone Methods

The results of the experiment indicate that telephone procedures may be a useful supplement to improve cooperation in central cities, where households are unwilling to keep diaries. In suburban areas, there would seem to be no need for telephone supplementary procedures, and cost and recording accuracy problems would argue against their use in these areas. In the city, the telephone would not solve all cooperation problems, but would be likely to raise the cooperation level to that in the suburbs, or just slightly lower.

Unfortunately, the data obtained by telephone, even using daily recall, do not appear to be as complete and accurate as data from diaries. There was some evidence of under-reporting of purchases by phone households, although not apparently for the total amount spent on food. Phone respondents reported only about three-fourths as many purchases as diary keepers. They also reported a larger number of purchases that ended up in the "all other food" category, indicating potential problems at the data-reduction stage.

Several strategies would be possible, given these results, but the choice of the best one would require additional testing. One could use phone data as they are and ignore the possible errors, although this would not seem too wise. Alternatively, one could use the dollar expenditure data by product class and estimate by regression procedures the number of purchases or other details required. Another method, if phone interviews were done from a central location in a city, would be to have them edited quickly after the interviews (not by the interviewer, but by a trained editor) so that possible errors or gaps in the data could be checked on the next day's interviews.

Phone data do seem to improve as the respondents, and perhaps the interviewers, get more experienced, which might suggest that the data for the first few days be only for practice. On the other hand, since phone interviews cost more than diary methods, a somewhat shorter time period might be considered for supplementary interviews, say a week or ten days if the first three days were practice.

The installation of telephones, where necessary, does increase cooperation and is administratively easy to handle. There is no reason not to continue this procedure for telephone supplementary samples. One way to reduce costs would be to omit any additional compensation for these households, since the phone installation charge is itself worth about four times the cost of the compensation.

All in all, although phone methods do have problems, the results are sufficiently promising to warrant additional testing, especially in cities, where diary cooperation is a serious problem.

Limitations

Any assessment of the significance of these results must take into account the small scale, the limited scope, and the restricted nature of this pilot study. In particular, the study was carried out with a relatively small sample size due to restrictions on available funds and was conducted in a single urban area over a period of a few weeks. For these and other reasons, the scope of the experimental variables was highly limited and the results should not be extrapolated to other situations. Thus, only one alternative was considered in the test of auspices, and the fact that there was no difference in the results does not necessarily imply that a similar finding would be obtained if another type of auspices was tested, possibly in a different area. In a roughly analogous fashion, there is no assurance that the same results with regard to compensation or the use of telephones would be obtained in other areas.

The small sample size is also a major limitation since it is not clear to what extent observed differences currently ascribed to sampling variations might really exist in a large-scale survey. With coefficients of variation up to 10 percent for even the larger categories, and considering the small sample sizes used in this study, differences in the population would have to be substantial before they could be labeled as such.

A final limitation is the disappointingly low rate of initial cooperation. Only about 60 percent of those originally contacted agreed to cooperate. Since not all of those did so for a full two weeks, the effective rate of cooperation is only 47 percent for the entire sample. While this low rate of cooperation was the result of a conscious decision to maximize sample size for analysis of reporting, the cooperation obtained was lower

than expected. Biases are likely to be present even when the rate of cooperation is high, and such dangers must be considered even more carefully when the rate of cooperation is low. As an example, it is not clear if the group that did cooperate represented households for which an offer of compensation really made a difference and if cooperation of the other members of this group would have been much poorer had they been induced to take part in the study.

More broadly, the extent to which the nonrespondents would have reacted to experimental variables in reporting purchases in a manner similar to the respondents is an open question. All that can be said is that we have no reason to believe that such differences exist, but clearly this is a question that can only be answered by further testing. In particular, any additional tests should place major emphases on maximizing cooperation and on later comparisons of the quality of the data obtained from the "readily cooperative" respondents and the "reluctantly cooperative" respondents.

Despite the limitations of this pilot study, the results do allow us to hypothesize the following:

1. An offer of compensation will improve both cooperation and the quality of data obtained.
2. Differences in auspices, such as between the Bureau of the Census and a part of a major state university, will have no effect.
3. The use of the telephone on a selective basis can be very helpful in improving the response rate and in obtaining better quality data.

It is hoped that further studies can be undertaken that will incorporate more comprehensive tests of these hypotheses.

REFERENCES

Flueck, John A.; Waksberg, Joseph; and Kaitz, Hyman B. "An Overview of Consumer Expenditure Survey Methodology." Proceedings of the American Statistical Association, Business and Economic Statistics Section (1971), 238-46.

Pearl, Robert. "Methodology of Consumer Expenditure Surveys." Working Paper No. 27. Washington, D.C.: U.S. Bureau of the Census, 1968.

Sudman, Seymour. "On the Accuracy of Recording of Consumer Panels: I and II." Journal of Marketing Research, 1 (May, 1964), 14-20; (August, 1964), 69-83.

_____, and Ferber, Robert. "Experiments in Obtaining Consumer Expenditures by Diary Methods." Journal of the American Statistical Association, 66 (December, 1971), 725-35.