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AN ANALYSIS OF INTERVIEW FREQUENCY
AND REFERENCE PERIOD IN RURAL
CONSUMPTION EXPENDITURE SURVEYS:
A CASE STUDY FROM SIERRA LEONE

by

Sarah Gibbons Lynch

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ABSTRACT

AN ANALYSIS OF INTERVIEW FREQUENCY AND REFERENCE PERIOD IN RURAL CONSUMPTION EXPENDITURE SURVEYS: A CASE STUDY FROM SIERRA LEONE

by

Sarah Gibbons Lynch

Interview frequency and length of reference period are two facets of survey design crucial to the collection of reliable and cost-efficient consumption expenditure data. The influence of these two factors on consumption expenditure estimates was analyzed using parametric and non-parametric techniques. A comprehensive rural consumption expenditure survey conducted in Sierra Leone in 1974-1975 served as data base for the study.

This study analyzes differences in household expenditure estimates based on data collected using 1) one versus two interviews per month; 2) each of the four individual days of recall contained in one interview; and 3) the first versus the sum of the second and third day of recall.

Results of this analysis provided evidence that expenditure estimates based on one interview per month were statistically, but not substantively, different from two interviews per month. Expenditure estimates from the first day of recall were statistically different from and consistently higher than those from the other three days of recall. Moreover, expenditure estimates from the first interview were higher than those from the second interview in a month. Problems of memory decay, respondent fatigue, and telescoping of expenditures were cited as explanations for the results.

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1. INTRODUCTION

1.1 Consumption Expenditure Survey Methodology in Low Income Countries

Knowledge of consumption patterns derived from rural household expenditure surveys is an important input into policy analysis and economic planning in many low income countries. Besides providing useful information on the general state of health and nutrition in rural areas, household budget surveys can help identify the trends in consumption expenditure patterns of different income groups, and the distribution of food within and among different groups. These surveys can also help identify potential consumption-based linkages with local small-scale industries. Finally, information from such surveys can also be used to estimate elasticities of demand for goods and services - knowledge crucial in both short- and long-run economic planning.

In many low income nations the paucity of reliable information on rural consumer behavior represents a serious constraint on development planning. Lacking country specific consumer data, many of these nations have been forced to use general income elasticities of demand provided by the FAO in order to project consumer demand for some types of commodities. The lack of information also impedes the efforts of international agencies to develop and implement strategies designed to reach the rural poor.

While the need for information on consumption patterns is clear, there is no consensus on the optimum survey methodology to obtain it. The numerous consumption expenditure surveys that have been conducted in developing countries reflect a wide range of objectives and methods. Examples

of some of these studies are included in Massell and Heyer (1967). Ikhtiar Ul Mulk (1966), Jamei (1966), Houyouk (1973) and King (1977).

There are several reasons for the lack of consensus on methodology. First, more is generally known about the interpretation of results than about the methodology used to obtain those results. Often methodological mistakes are buried, barring others from learning from them. Also, the purpose of the survey is seldom to investigate methodological issues; thus improvements in survey design are not field tested and evaluated systematically. This is understandable, though not desirable, given the high costs that would result from complicated replications of different survey techniques under similar conditions.

In the profession's uncertainty over what is essential in the collection of comprehensive rural consumption expenditure data in low income countries, there has been a tendency to implement the frequent visit survey methodology. This survey methodology is based on an interview schedule that calls for repeated visits to participating households during a month and extending over a relevant period, such as one crop season or calendar year. The advantage of the frequent visit methodology over other survey types is that less reliance is placed on a respondent's ability to remember events. With frequent interviewing, events are recorded as they occur. It is hypothesized that this improves the quality of the data by reducing measurement error. Given the heterogeneity of populations in rural areas of low income countries, it is often believed that this methodology is essential in order to generate accurate expenditure estimates for different regions, income groups and seasons.

But this methodological approach is generally costly and time-consuming. Its comprehensive nature generates higher costs in every phase of the data collection process. A larger staff of enumerators are necessary. It generally requires significant administrative capacity to supervise the implementation of the survey and the interpretation of

results. Usually, the sheer physical quantity of data collected cannot be absorbed and analyzed by local processing facilities and personnel. Often the sophistication of the data obtained goes far beyond what Collinson (1979) describes as the "bread and butter" needs of the host government.

There is an important trade-off to be considered between the reduction of measurement error resulting from the intensive interview schedule and the increased costs of obtaining that improvement in accuracy. Improvements in accuracy can always be achieved, but at a diminishing rate. At some point the added gains associated with an increase in accuracy are exceeded by the cost of obtaining them. This happens either because resources are limited or because the increase in accuracy is not necessary, given the objectives of the study.

The need for knowledge of rural consumption patterns for planning purposes and the lack of available resources and capital in many low income countries make it essential that the most cost-efficient survey methodology be adopted. Efforts must be made to develop a methodology which can quickly generate, with some minimum criterion of reliability, the kind of "bread and butter" information needed by governments. It should also be compatible with the nation's human and physical capacity to collect, process and absorb information, if it is to have an impact on the developmental process. It is important, therefore, that survey methodologies be developed which strike a balance between theory, necessity and cost.

1.2 Focus of the Study

This paper analyzes the effects of two factors in survey design that affect the cost of collecting, processing and using information, as well as its reliability. The first is interview frequency, or the number of times during a month a household is visited. The frequent visit methodology assumes that a more intensive interview schedule improves the reliability of the expenditure estimates by reducing the measurement error in the sample. A more intensive interview frequency,

however, requires a greater commitment of resources which are generally in scarce supply.

The second is the reference period used in an interview, or the length of time over which a respondent is requested to report purchases during one interview. The period of recall can range anywhere from twenty-four hours to a month, three months, six months, or a year. The reference period is extremely important because it influences both the measurement and sampling error in the survey. A central issue in determining its length is the ability of a respondent to remember purchases over time. It is presumed that memory decays over time and, therefore, a direct relationship exists between the length of the reference period and the degree of measurement error.

An empirical assessment of the impact of these factors is made using data collected in a comprehensive frequent visit micro-level study conducted in rural Sierra Leone in 1974-1975. Parametric and non-parametric tests are used to examine the differences between mean expenditure estimates derived from one interview per month and two interviews per month. This is done on a monthly and annual basis for both a very disaggregated list of commodities and a consolidated list of commodity groups.

A four day reference period is used for an interview in the Sierra Leone study. In order to determine if the problem of memory decay was more evident in a particular day of recall, an assessment is made of the differences in the mean expenditure estimates derived from each of the four different days of recall obtained in one interview.

Since the purpose of this paper is to explore methodological issues, an effort has been made to describe in detail the steps taken in conducting the analysis. Wherever appropriate, tables giving the statistical results are included to allow readers to assess the data for themselves.

1.3 Outline of Remaining Chapters

In Chapter 2, the issues involved in determining interview frequency and reference period are discussed in greater depth. The concepts of measurement error and sample error are described and their relationship to interview frequency and reference period is explored.

Chapter 3 describes the methodology used in the micro-level survey conducted in Sierra Leone, one component of which was the consumption expenditure study which provides the data base for this paper. Detailed information is given on sample selection, the household interview schedule, and the length of reference period. Also included is a description of the data preparation carried out for this analysis. Particular attention is given to describing the three categories of interview frequency used in this analysis.

The procedures and results of non-parametric tests performed on 257 disaggregated commodity groups using monthly expenditure estimates are presented in Chapter 4. This analysis compares three different data sets representing expenditure estimates based on one and two interviews per month.

This is followed in Chapter 5 by a description of the procedures and results obtained when using the correlated t-test to determine whether the differences between annual commodity expenditure estimates based on two interviews are significantly different from those based on one interview per month. For this analysis, 16 commodity groups representing food items, beverages and some frequently purchased items are used.

The four days of recall obtained during one interview are examined individually in Chapter 6. An analysis of the differences in expenditure estimates generated by the four different days of recall is made using Hotelling's T^2 test. A comparison is made of first and second interview expenditure estimates derived from particular days of recall.

Finally, Chapter 7 provides a summary of the research findings and the conclusions of this analysis.

2. THE DESIGN OF HOUSEHOLD BUDGET SURVEYS

2.1 Factors in Survey Design

Numerous methodological factors involved in survey design contribute to the cost per unit of information and data turn-around time: sample size, sample selection procedure, collection technique (e.g., interview, questionnaire, group interview), and the duration of the survey. Critical to the choices made concerning these factors are the objectives of the intended research. The survey design implemented should generate the type of information and level of accuracy needed to test the desired hypotheses. An attempt should be made, therefore, to minimize the relevant threats to validity which vary depending on the objectives of the study, while keeping data collection costs as low as possible.

While many of the factors mentioned above represent important and sometimes controversial issues in survey design, they are beyond the scope of this paper. It is recognized, however, that there is a great deal of interdependence between the decisions made with respect to interview frequency and reference period and other variables involved in survey design. The trade-offs between these variables should be given serious consideration in designing a survey methodology.

Central to the issues of interview frequency and reference period are the concepts of sample and measurement error. The validity of the inferences drawn from the data depends to a great extent on the degree to which these two types of errors exist in the data. Boruch (1972) defines measurement or response error as the difference between the recorded response to the inquiry and a potentially measurable, true condition associated with that inquiry. Sources of measurement error

in survey questionnaires are identified as faulty recall, a deliberate or accidental distortion of responses, structural weakness or ambiguity in the item, lapses in the quality of data reporting, and errors in processing and maintaining the data. Moser and Kalton (1972) also identify interviewer bias as a source of measurement error.

Another source of measurement error arises when the panel method is used in survey design. This method, incorporated into the design of the Sierra Leone study, specifies the collection of data from the same sample on more than one occasion. Moser and Kalton (1972) identify sample mortality and conditioning as two of the specific problems associated with this method. The former occurs when, over the course of the survey, participants drop out, move or die. Sample mortality does not necessarily result in biased results if the exit of participants is random. Problems could arise, however, if the participants' discontinued participation could be correlated with particular characteristics such as income, education, ethnic group and/or religion.

The other problem associated with the panel method, also discussed by Neter and Waksberg (1964), is conditioning. There is a risk that repeated visits to particular households will in some way become untypical. If this happens, the panel or sample of households may become, as Moser and Kalton (1972) point out, "...untypical--not in composition but in its characteristics--of the population it was selected to represent." This may affect the accuracy of the expenditure records obtained from these households. Repeated visits can sensitize the participants, making them more aware of their expenditures, thereby improving the expenditure records. Alternatively, repeated visits to households can result in respondent fatigue that can cause a decrease in the accuracy of expenditure records.

Measurement error is a critical factor in data reliability. Its presence can introduce significant bias in expenditure estimates. This is especially serious if the bias introduced is large and in an unknown direction. The problem

is made more difficult because there is no method for statistically measuring the extent or direction of the bias from the data themselves.

The other factor influencing sample reliability is the class of errors described as sample errors. As described by Moser and Kalton (1972), sample errors lead to fluctuations of the sample or population estimates around their true or expected values. The standard error is the measure of this fluctuation. Sample size and the variability in the population are two factors which influence the degree of sample error present. The smaller the sample size and/or the greater the variance in population characteristics, the greater the standard error. Intuitively, this implies that a wide variation in population characteristics makes the estimation of the population mean from one sample less reliable. The size of the standard error also influences the ability to use certain types of statistical tests. A large standard error widens the confidence intervals within which the population's expected value is found. Conversely, a smaller standard error tightens these boundaries, improving the reliability of statistical tests.

2.2 Factors in Determining Interview Frequency

A trade-off between the two types of errors is inherent in the choice of frequency of interview. A large sample size results in a smaller standard error. A large sample size and/or an intensive interview schedule results, in general, in a smaller standard error. The costs of collecting data from a large sample or from repeated visits to households can be quite high, however. The implementation of such surveys necessitates a large staff of enumerators and also requires significant administrative and supervisory capacity. Also required is the facility to handle and process the extensive amount of data being collected. If these capabilities are not available, significant measurement error can be

introduced into the data. A balance must be struck between sample error and bias. One consideration important in the assessment of this trade-off is the extent of variation in household expenditures due to income, household size, and cultural or regional preferences.

Rey (1976) suggests that another important concern in the determination of interview frequencies is that they cover the span of time during which consumption expenditures follow a certain pattern. They should include at least one buying cycle for each interval into which the year is divided. Knowledge of the population characteristics and production and marketing cycles will give the first indication of what the necessary frequency pattern might be. It is essential that the influence of marketing cycles on household expenditures not be overlooked, given the dominance of periodic markets in many low income countries. Also, seasons will have great impact on expenditure patterns in many low income countries where the majority of the population is involved in subsistence agricultural production. It is essential, therefore, that the influence of seasons be accounted for in inter-month interview scheduling.

Another factor to be considered in determining interview frequency is the availability of administrative capacity and trained personnel to participate in the study. Poorly trained and/or supervised enumerators can introduce significant bias in the data colleciton process, which could threaten the validity of the results. An increase in interview frequency per household also puts a greater strain on respondents. This could possibly generate fatigue on the part of respondents and the potential for decreasing reliability in response. Non-response on the part of participating households, due to absenteeism, requires callbacks that can be costly both in terms of travel expenses and enumerator's time. Supervision of data collection and processing procedures in multi-visit surveys can also be demanding of scarce administrative capacity.

2.3 Factors in Determining the Length of Interview Reference Period

Directly related to the intermonth interview schedule is the length of the reference period chosen. Such choices reflect trade-offs between accuracy and cost and sample and measurement error similar to those involved in determining the interview frequency. A longer reference period per interview reduces the cost per unit of information by permitting the collection of more data points during the one interview at little extra cost. Alternatively, information could be obtained in separate interviews, but the costs would be significantly higher. Yet a long reference period increases the possibility of response error due to memory decay which threatens the reliability of the data. Thus, in this case there is a trade-off between decreasing the cost of data collection by lengthening the reference period and reducing the reliability of the data by introducing significant measurement error. The reference period chosen also influences the size of the standard error. A longer reference period decreases the sampling error in that more data points are collected which capture more of the variation in a population's expenditures, thereby reducing the standard error. However, as mentioned previously, memory decay which increases over time can introduce a potentially significant bias in expenditure estimates. A decision must be made, then, as to the point at which the benefits brought about by the reduction in standard error are swamped by the increase in measurement error due to memory loss.

Moser and Kalton (1972) identify two primary factors which influence a respondent's ability to remember expenditures. The first is the length of time since the event took place. There is a greater probability of forgetting a purchase as the length of time for which it must be remembered increases. The importance of the purchase to the respondent is the second factor which influences the ability to recall. The less significant the item, the easier it is to forget.

To avoid this type of bias some studies have used reference periods of different lengths, depending on the type of purchase (Hussain, 1966; King, 1977). A shorter reference period is used for items with a shorter recall, i.e., those items frequently purchased and less significant to the respondent. A longer reference period is used to collect information on those items which are purchased less frequently but are major or more significant purchases.

Two major issues in determining the length of the reference period are identified in the literature (Neter, 1965; Moser and Kalton, 1972; Prais and Houthakker, 1971). One concern is what is referred to by Prais and Houthakker (1971) as recall loss. This has been described in the preceding paragraphs and refers to the respondent's failure to report an activity because of memory failure. Neter notes that the probability of this occurring increases as time passes and is a more important influence on the ability to recall frequent and less significant purchases.

The second issue is the end period or telescoping effect. This describes the tendency to include expenditures incurred just before the beginning of the inquiry. The telescoping effect is believed to have greater influence on the reporting of exceptional expenditures such as those made on major durables (Prais and Houthakker, 1971). There is also some evidence to suggest that there is a greater general telescoping effect for shorter reference periods. This has been suggested as a potential explanation for the relatively higher expenditure levels associated with short recall periods commonly found in survey results (Moser and Kalton, 1972).

Another factor which can influence the magnitude of the telescoping effect is whether the recall period is bounded or unbounded. Unbounded recall occurs when respondents are asked to report expenditures made since a given date but where no control is exercised over the possibility that expenditures from the previous period are repeated. Bounded recall techniques attempt to reduce the telescoping effect through

repetition of past purchases, to prevent duplication in subsequent interviews (Moser and Kalton, 1972).

Empirical tests have been conducted to analyze the influence of telescoping using bounded and unbounded recall periods. Neter and Waksberg (1964) found in their study that expenditure estimates derived from a one-month unbounded recall period were significantly higher than the expenditure estimates obtained from a bounded one-month recall period.

The issues discussed in the preceding sections must be considered when determining the interview frequency and reference period used in a particular study. The accuracy of the data and the cost per unit of information are heavily influenced by these decisions. Unfortunately, very little is known about the magnitude of the trade-offs involved in choosing among the alternative frequency and recall patterns. While theory and common sense suggest that these factors have significant influence on reducing measurement errors, there is little existing empirical evidence to indicate either how much or at what cost the improved accuracy is obtained.

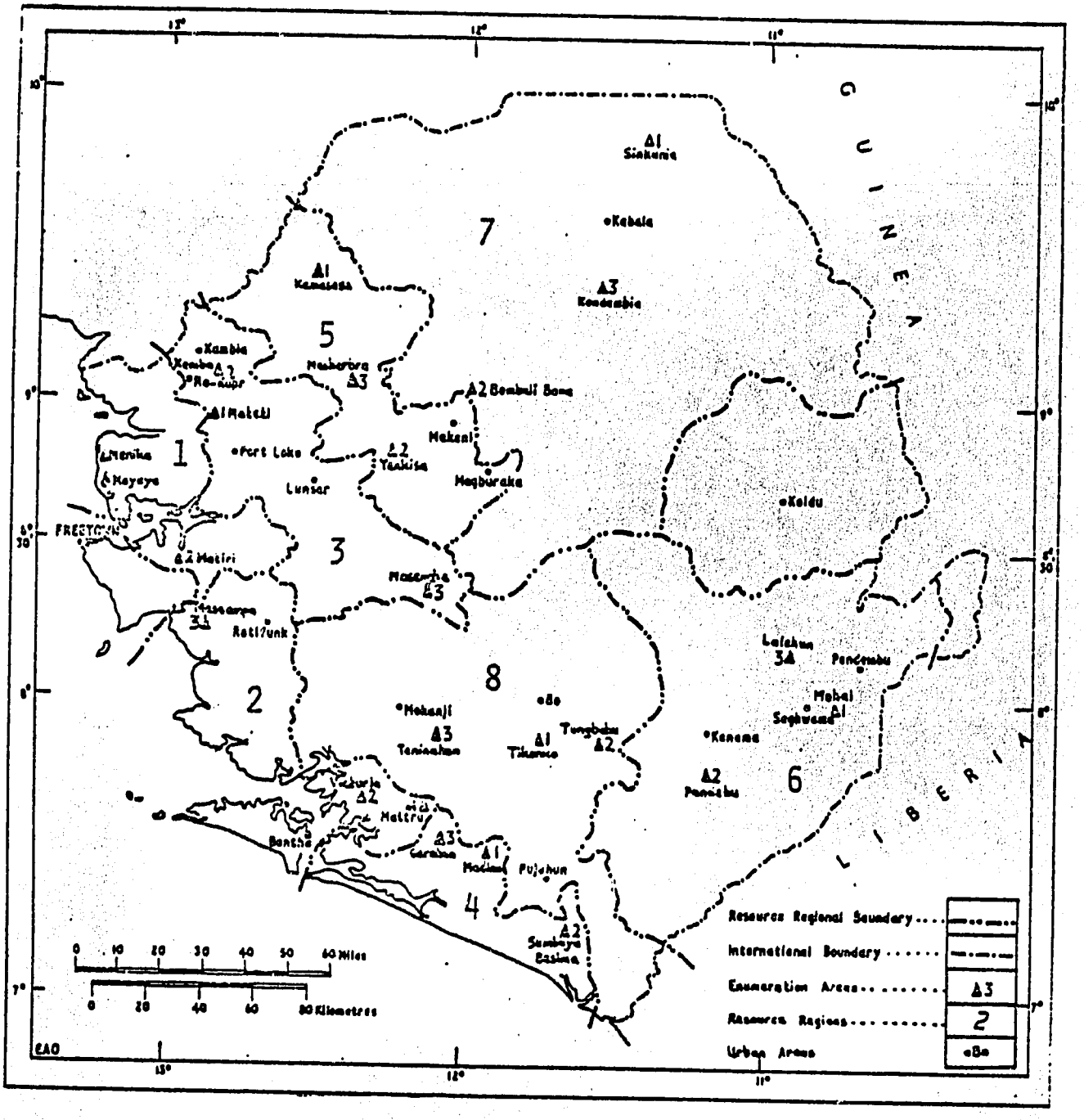
3. SURVEY METHODOLOGY USED IN SIERRA LEONE RURAL CONSUMPTION EXPENDITURE SURVEY

3.1 Sample Selection

The data used in this analysis were collected in a comprehensive rural household budget survey conducted in Sierra Leone from March 1974 through May 1975. A frequent visit or cost route survey methodology was used to collect 14 months of cross-sectional data covering a wide spectrum of rural activities. The integrated survey was designed primarily to collect micro-level information on farm production and non-farm activities for an entire crop year. A secondary objective of the survey was to collect data on migration and consumption expenditures. The following description of the Sierra Leone study relies heavily on the information provided in Spencer, et al. (1976); Spencer and Byerlee (1977); King (1977); and Rural Employment Research Project (1974).

In the Sierra Leone survey the enumeration areas as well as the participating households were selected through a stratified sampling procedure. Sierra Leone was divided, using available secondary data, into eight resource regions reflecting different physical and climatic factors. Each of the eight resource regions shown in Figure 3.1 was subdivided into enumeration areas of approximately ten square miles each. Roughly 130 farm families located in one to ten villages were contained in each enumeration area.

Since the purpose of the survey was to obtain information on rural households, enumeration areas falling into or containing urban areas were excluded. In this study urban areas were defined as localities with greater than 2,000 people and where more than 50 percent of the labor force was engaged in non-farm activities. Information already



Note: (1) Scarcies, (2) Southern Coast, (3) Northern Plains, (4) Riverain Grasslands, (5) Bolilland, (6) Moa Basin, (7) Northern Plateau, and (8) Southern Plains.

FIGURE 3.1

SIERRA LEONE RURAL RESOURCE REGIONS

available on the occupational distribution in Sierra Leone and the 1963 population census was used to determine which of the enumeration areas were to be eliminated because they were characteristically urban by this definition.

Within each of the eight resource regions three non-urban enumeration areas were chosen at random. In this way a total of 24 enumeration areas were identified for inclusion in the sample. Though the same number of enumeration areas was selected from each resource region, there was great variation in the percentage of rural households sampled in each region.

Enumerators visited each of the households in the three enumeration areas selected to participate in the study. Information gathered in this way was used to construct the sample frame. Recorded for each household were the name and sex of the household head, the type of crops grown, and any non-farm occupations of household members. A stratified sample of 20 farm households and 4 non-farm households was then chosen at random from the sample frame. Given the intensive interview schedule, it was decided that 24 households per enumeration area was the maximum number of households that could be handled by one enumerator.

In the original survey design, approximately 500 households were to be interviewed to obtain micro-level farm data. During the course of survey implementation and data processing, however, certain households had to be dropped from the survey for reasons such as death of the household head, movement from the village, or severe problems of missing data, for example. As a result, the final number of households analyzed was about 20 percent lower than originally planned.

Households included in the farm production study were interviewed by a resident enumerator twice weekly over the fourteen-month survey period. Using a four-day reference period at each interview session, daily data on labor inputs and outputs for farm and non-farm activities and enterprises were obtained. Other types of farm production data were

gathered by means of seven other questionnaires which used varying interview schedules and reference periods.

Approximately one-half of the 500 households in the farm production survey were chosen at random to participate in the consumption expenditure survey administered during the same period. Only part of the original sample was included in the expenditures survey, in order not to overburden and fatigue respondents and/or enumerators. From each enumeration area one-half, or 12, of the originally included households were chosen. The sample households were divided for convenience into three groups, each containing four households. One household in each group corresponded to each week in the month. Thus, the first household in each group was to be interviewed in the first week of each month, the second household in each group in the second week, and so on through the month.

3.2 Description of Questionnaires and Interview Schedule

Households chosen to participate in the consumption expenditure survey were administered two questionnaires. Different reference periods were used on the two questionnaires in order to reduce the errors in response due to memory decay and telescoping.

The C-1 questionnaire was used to record daily expenditure on food, beverages, tobacco, and other commonly purchased items. It was administered twice a month, each time using a four-day reference period. The interviews were to occur within three days of one another so as to collect expenditure information for seven contiguous days. Thus, in the course of two interviews given during seven succeeding calendar days, one week of consumption expenditure data was collected. Figure 3.2 gives an example of an interview schedule for a given household. The numbers 4, 3, 2, and 1 refer to the day of recall for which the information was collected. If the first questionnaire was administered on the 15th of the month, then

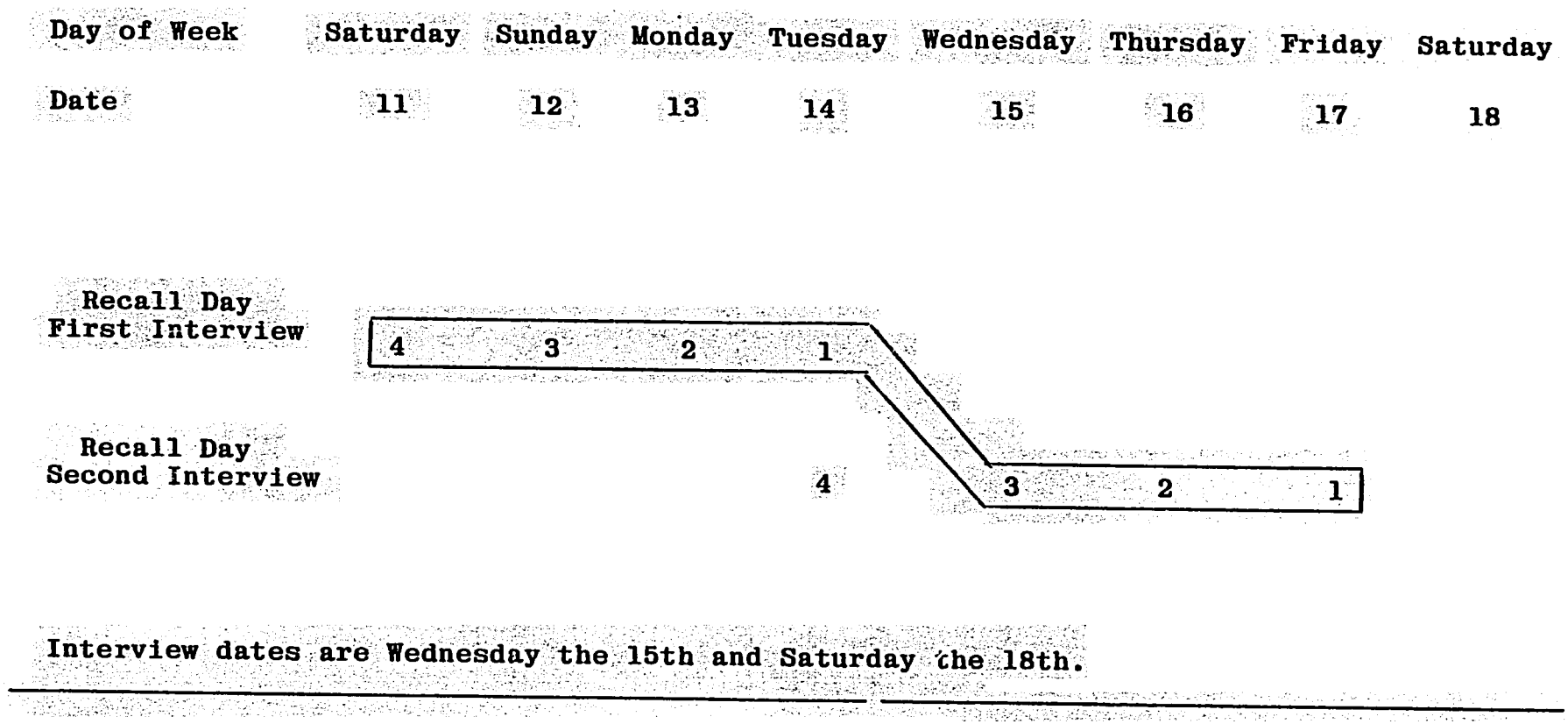


FIGURE 3.2

EXAMPLE INTERVIEW SCHEDULE

expenditures reported on Tuesday the 14th represent a one-day reference period, expenditures reported for Monday the 13th reflect recall over two days, Sunday the 12th over three days, etc. The second interview took place three days later--in this example on Saturday the 18th. The same reference period was used. Three different interview day combinations were used, Monday-Thursday, Tuesday-Friday, and Wednesday-Saturday, to insure that each day of the week except Sunday had an equal chance to represent a first, second, third and fourth day of recall.

As Figure 3.2 indicates, an overlap day exists between the first and second interview. The fourth day of recall in the second interview was coded differently in the processing of the data and generally ignored. The only reason for its collection was for consistency.

Theoretically, this data collection procedure lends itself very well to purposes of this analysis. Seven days of information for each month, collected on the C-1 questionnaire during two interviews, should be available for each household included in the survey. Thus, within each two-interview set of information on a particular household there is an identifiable subset of data on expenditures obtained in just one interview. The information from the one interview subset would have a recall pattern of 4-3-2-1. Having the data organized in this way permits the calculation of commodity expenditures estimates, based on the more intensive two-interview-per-month data set, to be compared with expenditure estimates obtained from the one-interview subset. The fact that the households included in each sample are identical reduces the possibility that factors other than the experimental variable of interview frequency are responsible for any observed variation in expenditure estimates between the two sets.

The C-2 questionnaire asked respondents to report purchases made on durable and less frequently purchased goods. This questionnaire was administered once a month,

theoretically at the end of the month, and had a reference period of one month. Checks were made in the data processing to ensure that purchases reported on one form were not also included on the other.*

Both questionnaires allowed respondents to report purchases on a highly disaggregated set of commodities (see Appendix A). Very specific information was requested on each purchase. The type and/or brand, if known, of each item was recorded. The total expenditure on each item was recorded in Leonian cents. Special codes were used to reflect the specific unit measurement of the item and the quantity of units purchased. Detailed information was collected on where the item was purchased, e.g., in the village market, a store, from a trader. Names were obtained where possible. The last category of information collected on each expenditure was the origin of the item, or where it was produced. Respondents could choose between four general categories: 1) rural areas (population less than 2,000); 2) large urban areas (population greater than 100,000); 3) small urban areas (population greater than 2,000 but less than 100,000); and 4) imported.

On the C-1 or short reference questionnaire this information was recorded for each purchase made during the four-day reference period. The C-2 questionnaire recorded all this information for major purchases made during an entire month.

3.3 Description of Interview Categories

Several problems with the data were encountered during the analysis. While each household was to have been

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*In this part of the analysis only information on expenditures obtained from the C-1 or short reference period questionnaire is being included. The C-2 or long reference questionnaire administered once a month would not be relevant in an intermonth comparison of different interview or recall patterns.

interviewed twice to obtain seven days of information per month, this was not always the case. Households were often over- or under-interviewed. As a result, complete monthly data for some households were not available. The problem was not that no expenditures were made, which was considered a valid expression of an expenditure pattern, but rather that for some reason a household was not interviewed during a given month and, therefore, had zero days of information. At the other extreme, some households had information for more than seven days per month.

Presumably, numerous reasons exist for the wide variation in the amount of monthly data collected for each household. A household might have an inconsistent interview pattern because the family moved during the survey period, experienced a death, and/or was absent at the time of interview. Alternatively, enumerators could miss the first, second, or even both interviews in a particular month for any number of reasons; or incomplete information could be collected during an interview. Over-interviewing a particular household could reflect an attempt to compensate for other missed households. Finally, some of the missing data might be explained by coding and processing errors.

In order to conduct the analysis in this study, it was necessary to identify for each household those months for which at least seven days of information were recorded. A household could have more than seven days of information in a given month, but only seven were used for purposes of analysis. Further, for a seven-day set of information to be included in the sample, the following had to hold: 1) the seven days had to represent two interviews; 2) the days had to be seven consecutive calendar days; 3) the sequence of the recall pattern had to be 4-3-2-1-3-2-1 or, though rarely observed, 3-2-1-4-3-2-1.

After identifying and making a separate computer tape consisting of only those months for which a household had

seven days of information, there remained a number of household month observations for which there were four days or more of information but less than seven. If, in this residual data, information existed for a particular household on four consecutive calendar days with a recall pattern of 4-3-2-1, for a month for which a seven-day record did not exist, then the data were included on a tape containing four-day or, in this paper's terminology, the one-interview independent sets. If, for a particular month, a household had both a seven-day set and a four-day independent set, priority was always given to including the seven-day set. If the data collection process overlapped two months, the overlap data set was assigned arbitrarily--the guiding principle being to include as many seven-day sets as possible. Details of this procedure are given in Appendix B. Table 3.1 shows the number of household observations contained in each month. Estimates for the two-interview set and one-interview independent set are given separately.

In order to make the seven-day and four-day expenditures representative of the same period of time, they were expanded to reflect one month's purchases. This was accomplished by mulitplying each estimated expenditure by the number of days in the month divided by the number of days of information. In doing this the assumption is made that the expenditure pattern for several days is representative of that for an entire month. The details of this procedure can be found in Appendix C.

Month and Year	Two Interviews Per Month	One Interview Per Month	Total
May 1974	88	32	120
June 1974	118	33	151
July 1974	142	32	174
August 1974	167	30	197
September 1974	152	44	196
October 1974	136	57	193
November 1974	160	42	202
December 1974	156	38	194
January 1975	146	45	191
February 1975	120	36	156
March 1975	159	37	196
April 1975	149	33	182

4. NON-PARAMETRIC ANALYSIS OF THE INFLUENCE OF INTERVIEW FREQUENCY ON EXPENDITURE ESTIMATES

4.1 Non-Parametric Tests and Their Application to This Analysis

Several approaches were used to examine the influence of interview frequency on expenditure estimates. In order to compare the data in its most disaggregated form, non-parametric tests were used. This statistical procedure allowed the comparison of each of the original 257 commodities listed in the C-1 questionnaire on a monthly basis.

Using this highly disaggregated list of commodities, parametric tests could not be used because of their restrictive assumption that the population sample has a normal distribution. The assumption of normalcy is clearly not the case when dealing with expenditure data where purchases of zero represent a large proportion of the observations for a particular commodity. The zero observations cannot be eliminated, since they are a reflection of non-purchase rather than non-response. The former is a valid expression of a household's demand and should not be automatically excluded from the sample.

In light of the inability to assume a normal distribution in monthly commodity estimates, non-parametric tests, which do not depend on assumptions concerning the form of the underlying distribution, were used. Non-parametric methods allow statistical tests in which no hypotheses are made about specific values of parameters. These methods are useful in many situations where ordinal data are being examined. In this analysis the non-parametric sign test was employed. This test is based on the signs generated by the differences between pairs of observations. It uses plus or minus signs as data rather than quantitative measures. Thus it does not take into

consideration magnitudes of the differences between the paired observations. The non-parametric sign test is particularly useful when dealing with two samples that are not independent.

To conduct the sign test, mean monthly expenditure estimates and variances were calculated for each of the 257 commodities and services (see Appendix A for listing of these) using data obtained from the two-interview set, the one-interview subset, and the one-interview independent set. The differences between the means of these three samples were calculated using paired data. The number of times that the difference was greater than or less than zero was counted. Similarly, a ratio of variances was constructed for each pair. The number of times the ratio was greater than or less than one was counted.*

Assuming for the moment that the three samples were drawn randomly from the same population, it would be expected that their estimated mean expenditures would be equal. In comparing any pair of monthly expenditure estimates there would presumably be a 50-50 chance that one sample's expenditure estimate would be larger than the other sample's estimate. Thus, the probability on any comparison of means between two samples is $p = .5$ that one would be larger than the other and vice versa.

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*A non-parametric comparison took place only in those cases where the two-interview set contained some positive observation for a particular commodity. This restriction was implemented because of the number of zero observations. In any given month there were a number of commodities which were not purchased by any household. In this case, expenditure estimates based on either interview frequency would have means and variances of zero. These were, therefore, not calculated. Given the way these data were prepared for analysis, if the mean derived from two interviews per month equaled zero, then by definition the means of the one-interview subset equaled zero. Basing the decision rule on the value of the two-interview set seemed to be the most efficient way of handling this problem.

If the sample size is large, the binomial probability distribution approaches the normal distribution, permitting the computation of test statistics with which to test a particular hypothesis.

The hypothesis tested here was that no difference exists in the probability distribution of the means and variances when comparing the two-interview set with the one-interview subset, the two-interview set with the one-interview independent set, and the one-interview subset with the one-interview independent set. Put in another way, the hypothesis tested was that the probability of one sample's commodity mean and variance being larger than the other sample's equaled p = .5.

4.2 Comparison of the Two-Interview Set with the One-Interview Subset

The first comparison called for is between the two-interview set and the one-interview subset. As shown in Table 4.1, the means from the two-interview set were larger in 509 instances while the opposite was true in 617 cases. In computing the standardized binomial variable a Z value of -3.22 was obtained. This statistic has a two-tailed significance level of .0014. Thus, at the .05 level of significance the hypothesis of no difference between the means cannot be accepted on the basis of these sets of data.

The inability to accept the null hypothesis based on this outcome suggests that the frequency of interview does influence expenditure estimates, at least in statistical terms. In practical terms, however, the numbers are not extremely dissimilar. They indicate that 5/11 of the time $\bar{X}_{Tjk} > \bar{X}_{Sjk}$ and that 6/11 of the time the opposite is true. This suggests that there is on average a tendency for expenditure estimates based on one interview to be larger than the expenditure estimates based on two interviews per month.

In the analysis of variance using the non-parametric sign test, the variances of the two interview expenditure estimates were smaller than those of the one-interview subset. As shown

TABLE 4.1

RESULTS OF NON-PARAMETRIC TEST COMPARING THE TWO-INTERVIEW SET WITH THE ONE-INTERVIEW SUBSET

Ho: $p = .5$ where p = probability that $(\bar{X}_{Tjk} > \bar{X}_{Sjk})$

Ha: $p \neq .5$

where: $\bar{X}_{Tjk}$ = mean monthly expenditure on the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on two interviews per month.

$\bar{X}_{Sjk}$ = mean monthly expenditure on the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on one interview per month which is a subset of $\bar{X}_{Tjk}$.

n = 1126

From the estimates for $\bar{X}_{Tjk}$ and $\bar{X}_{Sjk}$ the following were calculated:

$\bar{X}_{Tjk} - \bar{X}_{Sjk} > 0$ in 509 cases and

$\bar{X}_{Tjk} - \bar{X}_{Sjk} < 0$ in 617 cases.

These are standard binomial random variables with a standarized normal distribution ≈ N(0,1).

$$Z = \frac{509-.5(1126)}{\sqrt{1126(.5)(1-.5)}} = -3.22$$

in Table 4.2, the variances of the two-interview set were smaller than the variances of the one-interview independent set in 721 cases; the opposite was true in 407 cases. This occurs because in interviewing twice a month, expenditure variations are averaged out over a greater number of days. This results in a smaller variance.

TABLE 4.2

COMPARISON OF VARIANCE OF ESTIMATES FROM THE TWO-INTERVIEW SET AND THE ONE-INTERVIEW SUBSET

σ^2_{Tjk} = variance of monthly expenditure estimate for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on data collected in two interviews per month.

σ^2_{Sjk} = variance of monthly expenditure estimate for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on one interview per month which is a subset of the two-interview set.

n = 1128

In calculating the ratio of variances, it was observed that:

$\frac{\sigma^2_{Tjk}}{\sigma^2_{Sjk}} > 1$ in 407 cases, while $\frac{\sigma^2_{Tjk}}{\sigma^2_{Sjk}} < 1$ in 721 cases.

4.2.1 Comparison of Total Mean Expenditures for All Commodities

While these non-parametric tests indicate that the one-interview subset expenditure estimates tend to be greater than estimates based on two interviews, the figures do not tell what the magnitude of this difference is. To obtain some rough indication of this magnitude, all available mean monthly expenditure estimates were totaled using both the two-interview set and one-interview subset. The hypothesis that the

two total mean expenditure levels were equal was tested. The test results are shown in Table 4.3.

TABLE 4.3

COMPARISON OF TOTAL MEAN EXPENDITURES

Ho: $\bar{X}_{TE} = \bar{X}_{SE}$

Ha: $\bar{X}_{TE} \neq \bar{X}_{SE}$

where: $\bar{X}_{TE}$ = total mean expenditure for all commodities for all months based on two interviews per month.

$\bar{X}_{SE}$ = total mean expenditure for all commodities for all months based on the one-interview subset.

n = 1126

and where:

$$\bar{X}_{TE} = \sum_{j=1}^{257} \sum_{k=1}^{14} \bar{X}_{Tjk}/n = .25 \quad \text{(Leones)}$$

$$\bar{X}_{SE} = \sum_{j=1}^{257} \sum_{k=1}^{14} \bar{X}_{Sjk}/n = .27$$

j = commodity (1,...,257)
k = month (1,...,14)

$$t = \frac{.25-.27}{\sqrt{[\sigma^2_{TE} + \sigma^2_{SE} - 2(COV)]\frac{1}{n-1}}} = -3.135$$

The total mean expenditure estimate for the two-interview data set for fourteen months of information is .25. The total mean expenditure estimate for the one-interview subset is .27. Using the co-related T-test procedure to test the difference between the two means, the test statistic derived was -3.135. From a statistical point of view the difference between these two means is significant at the .05 level. Therefore, the hypothesis that the total mean expenditure estimate based on two interviews per month is equal to the mean expenditure estimate obtained from a one-interview subset cannot be accepted. These figures support the results obtained earlier that the expenditure estimates based on one interview have a tendency to be slightly larger than those based on two interviews per month.

Again, while these figures are different from a statistical point of view, they are in practical terms very similar. The one-interview subset estimate is only 8 percent larger than the expenditure estimate generated by the two-interview set. Depending on the purpose of the survey, and the level of accuracy needed, these differences could be viewed as very slight. If so, the additional cost of a second monthly interview might not be deemed necessary.

4.3 Comparison of the Two-Interview Set and the One-Interview Subset with the One-Interview Independent Set

The same hypothesis of no difference in the probability distribution of the means and variances of the paired data was tested by comparing the two-interview set and the one-interview subset with the one-interview independent set. The results present an interesting contrast to those obtained from the first tests. Mean monthly expenditure estimates based on the two-interview set are larger than those derived from the one-interview independent set in 973 cases. The reverse situation prevails in only 425 cases, as shown in Table 4.4.

TABLE 4.4

$H_o: p = .5$
$H_a: p \neq .5$

where p = probability that $(\bar{X}_{Tjk} > \bar{X}_{Ijk})$

where: $\bar{X}_{Tjk}$ = mean monthly expenditure on the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on two interviews per month.

$\bar{X}_{Ijk}$ = mean monthly expenditure on the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on the one-interview independent set.

n = 1398

From the estimates for $\bar{X}_{Tjk}$ and $\bar{X}_{Ijk}$ the following were calculated:

$\bar{X}_{Tjk} - \bar{X}_{Ijk} > 0$ in 973 cases and

$\bar{X}_{Tjk} - \bar{X}_{Ijk} < 0$ in 425 cases.

These are standard binomial random variables with a standardized normal distribution = N(0,1).

$$Z = \frac{973-.5(1398)}{\sqrt{1398(.5)(1-.5)}} = 14.656$$

These results are the reverse of those obtained in the previous test comparing the two-interview set with the one-interview subset. In that test the one-interview means tended on average to be larger than the two-interview means. In this test not only are the means of the two-interview set larger on average than the one-interview independent set, but the frequency with which one is larger than the other is much greater, as evidenced by the larger Z statistic of 14.656.

The variances of the two-interview set estimates also are consistently higher than those for the one-interview independent set, as shown in Table 4.5

TABLE 4.5

COMPARISON OF VARIANCES OF ESTIMATES FROM THE TWO-INTERVIEW SET AND THE ONE-INTERVIEW INDEPENDENT SET

σ^2_{Tjk} = variance of monthly expenditure estimate for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on the two-interview set.

σ^2_{Ijk} = variance of monthly expenditure estimate for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on the one-interview independent set.

n = 1398

In calculating the ratio of variances, it was observed that:

$\frac{\sigma^2_{Tjk}}{\sigma^2_{Ijk}} > 1$ in 998 cases, while $\frac{\sigma^2_{Tjk}}{\sigma^2_{Ijk}} < 1$ in 401 cases.

In comparing the one-interview subset with the one-interview independent set, similar results are obtained. As shown in Table 4.6, the mean expenditure estimates generated by the one-interview subset are higher than the one-interview independent set in 767 cases. The opposite occurs 429 times. This difference has a Z value of 9.774 using the normal approximation. The variances for the one-interview subset are higher than those of the four-day independent set by a margin of 794 to 403, as shown in Table 4.7.

TABLE 4.6

COMPARISON OF THE ONE-INTERVIEW SUBSET WITH THE ONE-INTERVIEW INDEPENDENT SET

Ho: $p = .5$
Ha: $p \neq .5$

where p = probability that $(\bar{X}_{Sjk} > \bar{X}_{Ijk})$

where: $\bar{X}_{Sjk}$ = mean expenditure for the j^{th} commodity (1,...,257) for the k^{th} month (1,...,14) based on the one-interview subset.

$\bar{X}_{Ijk}$ = mean expenditure for the j^{th} commodity (1,...,257) for the k^{th} month (1,...,14) based on the one-interview independent set.

$n = 1198$

From the estimates for $\bar{X}_{Sjk}$ and $\bar{X}_{Ijk}$ the following were calculated:

$\bar{X}_{Sjk} - \bar{X}_{Ijk} > 0$ in 767 cases and

$\bar{X}_{Sjk} - \bar{X}_{Ijk} < 0$ in 429 cases

$$Z = \frac{767-.5(1196)}{\sqrt{1196(.5)(1-.5)}} = 9.774$$

TABLE 4.7

COMPARISON OF VARIANCE OF ESTIMATES FROM THE ONE-INTERVIEW SUBSET AND THE ONE-INTERVIEW INDEPENDENT SET

σ^2_{Sjk} = variance of expenditure estimates for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on the one-interview subset.

σ^2_{Ijk} = variance of expenditure estimates for the j^{th} commodity (1,...,257) in the k^{th} month (1,...,14) based on the one-interview independent set.

n = 1197

In calculating the ratio of variances, it was observed that:

$\frac{\sigma^2_{Sjk}}{\sigma^2_{Ijk}} > 1$ in 794 cases, while $\frac{\sigma^2_{Sjk}}{\sigma^2_{Ijk}} < 1$ in 403 cases.

The results presented in Tables 4.4-4.7 present a potentially important contrast. In the first test of the hypothesis comparing the two-interview set with the one-interview subset the only difference between the two samples was frequency of interview. Since the one-interview subset was taken from the two-interview data set, the households contained in each sample were the same. This significantly reduced the possibility of other factors such as income, household size, and education having any influence on the results. Thus, to the extent possible the impact of interview frequency on expenditure estimates at the monthly level was isolated. The results suggest that the isolated effect of the difference in interview frequency was for one-interview mean expenditures to be on average somewhat larger than those based on two interviews per month. In contrast, when comparing the one-interview independent set with the two-interview set and its subset, the expenditure estimates of the former were smaller than those of the other two sets.

The unavailability of information on the characteristics of the households contained in the two sets prohibits a conclusive explanation of these observed differences. However, several hypotheses can be offered to explain these results. The first deals with an issue concerning the internal validity of the study. One could hypothesize that the households visited in the specified manner (two interviews in a month) went through a conditioning process such as that discussed briefly in Chapter 2. Because these households were visited consistently during the survey period, they became more sensitive to the survey process. Thus, they had a greater tendency to remember more accurately the purchases made during subsequent recall periods. Households visited inconsistently and not in the specified manner might report fewer expenditures because they had been interviewed infrequently and were not necessarily anticipating further interviews.

Another hypothesis with far more serious implications is that the two samples were not drawn randomly from the same population. This would imply that the two samples reflect different population characteristics. This might occur for two reasons. One deals with the respondent's willingness to participate or the sample's morbidity rate, while the other deals with an enumerator's interviewing techniques. In the former case a respondent's willingness or unwillingness to participate in a survey might be reflected in whether or not the household was interviewed in the correct manner. A household's receptiveness to the survey, its availability during interview sessions, and general interest in the survey could influence the number of times per month and year the household was visited by enumerators. If this difference in receptivity is not random but based on specific population characteristics such as income, education, type of employment, or ethnic group, it can cause serious problems in the reliability of the data. In survey design this is known as the problem of self-selection.

These same types of differences in population characteristics could also influence the number of times an enumerator visited a particular household. Enumerators could be less rigorous in their attempts to interview households of a particular ethnic group, income bracket, or level of education.

These factors could explain the results obtained when comparing the two-interview and one-interview subset with the one-interview independent set. The latter might reflect a greater proportion of households with a lower income, more removed from urban areas and thus less involved in a market economy and/or not as readily accessible. If this were the case, the lower means might reflect fewer purchases, a smaller variety in purchases and/or less total income spent on commodity purchases. This would also explain why the variance of the one-interview independent set is characteristically smaller than those of either the two-interview or one-interview subset.

If this hypothesis is valid, then a potentially significant distortion has been introduced into the data. Failure to obtain data from this genre of households could result in biased expenditure estimates and economic policies which might have undesired consequences.

Assuming for the moment that this hypothesis is true, the results reveal how essential well-trained enumerators and adequate field supervision are in the collection of reliable data. If the complexity of the survey design goes beyond the capacities of enumerators and administrators, then serious problems might arise.

5. ANALYSIS OF AGGREGATED COMMODITY GROUPS

5.1 Data Preparation

The non-parametric analysis in the preceding chapter compared mean monthly expenditure estimates associated with different interview frequencies for a highly disaggregated set of commodities. For many research and planning purposes, however, annual commodity expenditure estimates are required. These estimates are essential in deriving elasticities of demand and in the formulation of economic policy.

In order to compare the annual expenditure estimates derived from the two-interview set and the one-interview subset, the original commodity list was aggregated into 16 groups. An attempt was made to aggregate individual commodities with sensitivity to the demand, origin and nutritional characteristics of that item. This particular aggregation, shown in Table 5.1, contains all the possible food items

TABLE 5.1

AGGREGATED COMMODITY GROUPS

1. Rice	9. Sugar
2. Grains	10. Fresh Fish
3. Cassava and Other Root Groups	11. Dried Fish
4. Vegetables, Leguminous Products and Fruit	12. Bakery Items
5. Groundnuts	13. Other Processed Foods
6. Palm and Other Oils	14. Alcoholic and Non-Alcoholic Beverages
7. Meat and Other Livestock	15. Tobacco and Kola Nuts
8. Salt and Other Condiments	16. Fuel and Light

listed on the original survey code along with all beverages, tobacco and kola nuts, and fuel and light. All other types of durables, home, and personal goods were excluded. For the most part these purchases are recorded on the C-2 questionnaire. As mentioned in Chapter 2, this questionnaire had a reference period of one month and was used to collect information on durables and other less frequently purchased goods. Since this analysis involved comparisons of expenditure estimates based on one and two interviews per month, the C-2 questionnaire was not relevant.

The hypothesis to be tested in this chapter is that annual mean expenditure estimates based on two interviews per month are equal to those based on the one-interview subset. The alternative hypothesis is that the means are not equal.

In estimating annual mean commodity expenditures based on this data, several issues were encountered. The first matter of concern was the households to be included in the sample. As discussed earlier in Chapter 2, very few households were interviewed for all 12 months. Table 5.2 shows how many households have data based on two interviews per month and for how many months data were available. The cumulative frequency is also given. Only three households included in the survey have 12 complete months of data. Eleven households have 11 months of data, making the cumulative frequency of households with greater than 11 months of data equal to 14. The least restrictive criterion, that a household have at least one month of data, generates a cumulative frequency of 247 households.

TABLE 5.2

TOTAL NUMBER OF TWO-INTERVIEW HOUSEHOLD-MONTH OBSERVATIONS

No. of Months for Which Household Has Data Based on Two Interviews	No. of Households in Two-Interview Sample with X Months of Data	Cumulative Frequency
12 months	3	3
11	11	14
10	24	38
9	30	68
8	36	104
7	42	146
6	26	172
5	25	197
4	26	223
3	15	238
2	6	244
1	3	247

The number of months for which valid household data is available is an important concern in this analysis because of the lack of independence between the two samples. It cannot be assumed that purchases made and recorded in the second interview are independent from the purchases made in the first interview. Nor, for that matter, are purchases made in January independent of expenditures made in December or February.

This lack of independence between samples can be corrected for through the use of the correlated t-test. Unlike the more common Student's t-test, the correlated t-test does not assume that the two samples share a common variance. Nor does the correlated t-test assume that the covariance between the two

samples is zero. In using the correlated t-test the variance of each sample is computed individually and then the covariance between the two samples is computed and subtracted out of the denominator. This removes any double-counting in the pooled variance arising from the non-independence of the samples.

Analyzing the difference in mean annual commodity expenditure estimates with the correlated t-test requires using households with 12 months of data. This is necessary in order to compute the individual variances of each sample from which the covariance between the two samples can be calculated.

As Table 5.2 indicates, few households have 12 months of data. In order to overcome this problem, monthly indices for the 16 commodity groups were computed using the procedure described in Appendix D. Separate monthly indices were calculated for both the two-interview set and the one-interview subset. Missing expenditure information was imputed for only those households that had eight months or more of data. Households with less than eight month were excluded from the sample. Taking households with eight or more months of data generated a sample of 104 households and held the maximum number of months to be imputed for any given household to only one third of the total.

5.2 Comparison of Mean Expenditure Estimates

These indexed data were then used to test the research hypothesis that the means of the two samples are equal. This hypothesis was tested for each of the 16 commodity groups using the correlated t-test. The alternative hypothesis was that the means are not equal.

Table 5.3 summarizes the results of this analysis. For 14 out of the 16 commodity groups the difference between the means proved insignificant at the .05 level. Rice and Palm and Other Oils were the two commodity groups where the

TABLE 5.3

RESULTS OF COMPARISON OF MEAN ANNUAL ESTIMATES

Ho: $\bar{X}_{TAj} = \bar{X}_{SAj}$

Ha: $\bar{X}_{TAj} \neq \bar{X}_{SAj}$

n = 104

$\bar{X}_{TA}$ = two-interview mean annual commodity expenditure estimate

$\bar{X}_{SA}$ = one-interview subset mean annual commodity expenditure estimate

j = commodity (1,...,16)

	Commodity	$\bar{X}_{TA}$ (Leones)	$\bar{X}_{SA}$ (Leones)	T-Value	Probability	Significance*
1.	Rice	58.96	69.99	-3.13	.002	S
2.	Other Grains	1.75	2.35	-1.01	.315	NS
3.	Cassava and Other Root Crops	3.18	3.72	- .98	.330	NS
4.	Vegetables, Beans and Fruit	3.11	3.62	-1.38	.170	NS
5.	Groundnuts	.57	.63	- .74	.462	NS
6.	Palm and Other Oils	36.55	29.60	3.94	.000	S
7.	Meat and Other Livestock Products	5.98	5.52	.61	.541	NS
8.	Salt and Other Condiments	10.59	11.08	-1.34	.184	NS
9.	Sugar	2.42	2.27	.79	.434	NS
10.	Fresh Fish	8.19	7.96	.60	.550	NS
11.	Dried Fish	34.47	36.73	-1.33	.186	NS
12.	Bakery Items	2.84	2.50	.97	.336	NS
13.	Other Processed Food	1.08	1.33	-1.67	.099	NS
14.	All Beverages	3.63	3.79	- .37	.713	NS
15.	Tobacco and Kola Nuts	13.52	13.55	- .07	.946	NS
16.	Fuel and Light	16.05	15.61	.73	.470	NS

*NS = not significant at the .05 level
S = significant at the .05 level

difference between the means was determined to be significant. Thus, the research hypothesis that the two means are equal cannot be rejected in the remaining 14 cases.

A closer look at the distribution of the two-tailed probability levels associated with each test of the hypothesis provides some additional insights. Table 5.4 compares the actual and expected frequency distributions of the test results. In this kind of statistical analysis, the possibility of committing a Type 1 error always exists--that is, rejecting the null hypothesis when it is in fact true. In this analysis the probability of a Type 1 error is .05. Given that the sample size is 16, one could anticipate the occurance of a Type 1 error approximately once in this analysis. As indicated by Table 5.4, in actuality this occurred twice. The table also indicates that about twice as many commodities have differences significant at the .060 and .200 level as would be expected on the basis of chance alone. Similarly, only one-third as many exhibited levels of significance above .600 as compared to the expected outcome under the null hypothesis.

TABLE 5.4

DISTRIBUTION IN PROBABILITY

Probability Range	Frequency	Expected Frequency Under Ho
.000 - .050	2	.8
.060 - .200	4	2.4
.210 - .400	3	3.2
.410 - .600	5	3.2
.610 - .800	1	3.2
.810 - 1.000	1	3.2
	16	16.0

Taken together these results provide some evidence that the annual mean expenditure estimates generated by the two different survey frequencies are not equal. The fact that the null hypothesis was not accepted in the cases of rice and palm and other--widely consumed items--was at first surprising. Intuitively, one might argue that the frequency of interview would have more of an impact on expenditure estimates of infrequently purchased goods rather than those items bought quite often. However, the standard error of expenditure estimates of infrequently purchased goods is oftentimes very large, making it impossible to reject the null hypothesis. Thus, while the differences in expenditure estimated of infrequently purchased goods based on an intensive and less intensive interview frequency may be larger in percentage terms than those of frequently purchased goods, they are less likely to be found significantly different in a statistical sense.

The reason for the rejection of the null hypothesis in the cases of rice and palm and other oils does not appear to be due to the introduction of a systematic bias caused by the less intensive interview schedule. The data up to this point have shown a tendency for the one-interview expenditure estimates to be larger than those based on two-interview estimates. In this test Rice and Palm and Other Oils gave conflicting results.

5.3 Comparison of Total Annual Expenditures

The inconclusive nature of the preceding test prompted a look at the total annual expenditures using the two sets of data. Expenditure estimates were summed over the 16 commodities for both the two-interview set and the one-interview subset. As summarized in Table 5.5, the results of the correlated t-test again indicated that the hypothesis, that the means of the two sets are equal, cannot be rejected. At the .05 level of significance, the difference between the means was not found to be significant.

TABLE 5.5

RESULTS OF COMPARISON OF TOTAL MEAN ANNUAL COMMODITY ESTIMATES

Ho: $\bar{X}_{TA} = \bar{X}_{SA}$ $\bar{X}_{TA}$ = Total annual expenditure for all commodities (1,...,16) based on two interviews per month.

Ha: $\bar{X}_{TA} \neq \bar{X}_{SA}$ $\bar{X}_{SA}$ = Total annual expenditure for all commodities (1,...,16) based on the one-interview subset.

n = 104

$\bar{X}_{TA}$ (Leones)	$\bar{X}_{SA}$ (Leones)	T-value	Probability	Significance
202.87	210.24	-1.36	.177	NS*

*NS = not significant at the .05 level

The results of the comparison of the two-interview set and the one-interview subset using both parametric and non-parametric tests have consistently indicated that the expenditure estimates based on the one-interview subset have a tendency to be larger than those based on the two-interview subset, though the differences were not always statistically significant at the .05 level of significance. The non-parametric tests used did not allow the magnitude of this difference to be examined. However, in computing annual household expenditure estimates, as has been done in this chapter, it is possible to compute a rough estimate of the percentage differences in expenditure estimates. In comparing the mean annual commodity estimates for 16 commodity groups, the expenditure estimates based on the one-interview subset were on average 5.3 percent higher than those based on two interviews per month. In the comparison of total mean annual commodity estimates (as described in Table 5.5) the one-interview subset expenditure estimate was 3.6 percent higher than that of the two-interview subset.

The results presented up to this point have provided some, though rather weak, statistical evidence which refutes the hypothesis that the expenditure estimates based on one interview per month are equal to those based on two. The results, however, have rather consistently shown that the differences between the two are small. The next chapter explains some sources of these differences and helps to explain why the one-interview subset means, which are based on expenditure records from the first interview, are larger than the means of the two-interview set, which are based on the expenditure records of the first and second interview.

6. ANALYSIS OF REFERENCE PERIOD

6.1 Introduction

The reference period used in a survey has a large influence on both the cost of the survey and the data's reliability. A reference period collecting several days worth of consumption expenditures per household interview reduces significantly the cost per unit of information. The greater the number of data points obtained during a survey, the lower the standard error. However, in dealing with consumption expenditure data there is the problem of measurement error typically caused by memory decay and by the telescoping effect. The magnitude of these effects on response increases over time. The optimum situation suggested in the literature is to use the reference period for which the sum of the sampling error and measurement error is lowest.

The problem with this formula is that measurement error is difficult if not impossible to measure. Therefore, the choice of reference period in survey design has tended to reflect the best guess of when the positive effects on accuracy caused by reducing the sample error are swamped by the negative effects caused by the increase in measurement bias. This, of course, will vary depending on the purpose of the survey and the degree of accuracy needed.

One of the critical factors in determining the appropriate reference period has been the perceived length of time over which a respondent can accurately remember expenditures. Also important are the marketing cycles of the sample population. These factors are often population and commodity specific. It is, therefore, not wise to generalize about the optimum length of recall for all populations and survey purposes.

6.2 Sample Description

In the first part of this chapter the characteristics of the daily expenditure records for four consecutive days of recall are examined. Only the data contained in the one-interview subset are used in this analysis. This data set consists of household expenditures gathered on four consecutive days during one interview in a month. Each household included in the sample has a reference period which includes a 1st, 2nd, 3rd, and 4th day of recall. The 16 commodity groups described in Chapter 5 are used for this analysis. Mean expenditure totals for each of the days of recall for each of the 16 commodity groups are computed. All 14 months of data are used.

The purpose of this analysis is to observe the estimates of mean expenditures generated by the different days of recall to detect significant differences in their levels. Assuming that the properties of independent random sampling hold, one would expect that the mean commodity expenditures of the four different days of recall would, on average, be equal. If expenditure estimates on a particular day of recall are consistently different from the mean expenditures of the other days of recall, this might indicate the introduction of a greater degree of measurement error.

6.3 Comparison of Mean Expenditure Estimates From Individual Days of Recall

The first hypothesis to be tested is that the mean expenditure estimates of the four days of recall are equal. The alternative hypothesis is that not all the expenditure estimates of the four days of recall are equal. Hotelling's T^2 statistic is used to test this hypothesis. The results are shown in Table 6.1. The null hypothesis is rejected in 5 out of 16 cases at the .05 level of significance. The probability of obtaining 5 rejections out of 16 by chance is very slim. Therefore, one would conclude that a statistically significant difference exists between expenditure estimates obtained from four succeeding days of recall.

TABLE 6.1

TEST STATISTICS FOR EQUALITY OF MEANS OF THE FOUR INDIVIDUAL DAYS OF RECALL FROM THE FIRST INTERVIEW

Ho: $\bar{X}_{Aj} = \bar{X}_{Bj} = \bar{X}_{Cj} = \bar{X}_{Dj}$

Ha: not all the X's are equal

n = 1784

$\bar{X}_A$ = mean expenditure based on the first day of recall from the first interview

$\bar{X}_B$ = mean expenditure based on the second day or recall from the first interview

$\bar{X}_C$ = mean expenditure based on the third day of recall from the first interview

$\bar{X}_D$ = mean expenditure based on the fourth day of recall from the first interview

j = commodity (1,...,16)

	Commodity	T^2	F Statistic	Probability	Significance*
1.	Rice	3.87	1.29	.277	NS
2.	Other Grains	3.56	1.18	.315	NS
3.	Cassava and Other Root Crops	2.58	.86	.462	NS
4.	Vegetables, Beans and Fruits	3.16	1.05	.369	NS
5.	Groundnuts	2.47	.82	.482	NS
6.	Palm and Other Oils	3.79	1.26	.286	NS
7.	Meat and Other Livestock Products	4.14	1.38	.248	NS
8.	Salt and Other Condiments	25.44	8.47	.000	S
9.	Sugar	6.74	2.25	.082	NS
10.	Fresh Fish	8.53	2.84	.037	S
11.	Dried Fish	25.36	8.44	.000	S
12.	Bakery Items	3.11	1.04	.376	NS
13.	Other Processed Foods	2.90	.97	.407	NS
14.	All Beverages	.52	.17	.914	NS
15.	Tobacco and Kola Nuts	25.92	8.63	.000	S
16.	Fuel and Light	20.77	6.92	.000	S

*NS = not significant at the .05 level
S = significant at the .05 level

Unfortunately, these statistics do not reveal any information about the relationship between the individual days of recall. To analyse this, more specific information is needed on the behavior characteristics of different days of recall. Therefore, a simple comparison of expenditure means is made between each of the days of recall. A count is made of the number of times one mean was greater or smaller than the other. The results of this comparison are given in Table 6.2. The results of this simple non-parametric test indicate that the expenditure means based on the first day of recall are higher in almost every case than those of the second, third and fourth days of recall.

Guided by the insights gained through the comparison of means just discussed, a stronger statistical test can be developed to examine more rigorously the relationship between the four days of recall. This is accomplished through a comparison of the average expenditures from recall days two through four with the first day of recall. Here the null hypothesis tested is that the three-day average expenditure means equal those generated by the first day of recall. To make this a stronger test, a one-tailed alternative hypothesis is used which states that the expenditure means of the first day of recall are greater than those of the second, third and fourth days of recall combined.

TABLE 6.2

COMPARISON OF MEAN EXPENDITURES OF EACH DAY OF RECALL

A = 1st day of recall mean expenditures
B = 2nd day of recall mean expenditures
C = 3rd day of recall mean expenditures
D = 4th day of recall mean expenditures

	Frequency		Frequency		Frequency		Frequency
A > B	15	B > A	1	C > A	1	D > A	0
A > C	15	B > C	6	C > B	10	D > B	7
A > D	16	B > D	9	C > D	10	D > C	6

As the results in Table 6.3 show, the null hypothesis is rejected in 8 out of 16 cases. This provides strong statistical evidence that the mean expenditure estimate derived from the first day of recall is significantly different from the average of the other three days at the .05 level of significance.

The results also indicate that the observed difference is generally in one direction. In 15 out of 16 cases the mean expenditure associated with the first day of recall is higher than that based on the average of the second, third and fourth days of recall. On average the former tend to be roughly 112 percent higher than the latter. This high figure is a bit deceptive, however. For three of the commodity groups--Other Grains, Meat and Other Livestock Products, and Sugar--the difference between the two estimates is between 260 and 550 percent. When these three outlying observations are excluded, the average difference falls to 50.7 percent.

To further ascertain whether or not it is the influence of the first day of recall which resulted in the rejection of the original null hypothesis, a second test was performed. Expenditure records from the first day of recall were not

TABLE 6.3

COMPARISON OF FIRST DAY OF RECALL WITH THE AVERAGE OF THE SECOND, THIRD AND FOURTH DAY OF RECALL

Ho: $\bar{X}_{Aj} = \bar{X}_{Tj}$

Ha: $\bar{X}_{Aj} > \bar{X}_{Tj}$

n = 1787

$\bar{X}_A$ = expenditure mean based on the first day of recall

$\bar{X}_T$ = expenditure mean based on the second, third, and fourth day of recall

j = commodity (1,...,16)

	Commodity	$\bar{X}_A$ (Leones)	$\bar{X}_T$ (Leones)	T-Value	Probability	Significance*
1.	Rice	.177	.173	.14	.446	NS
2.	Other Grains	.013	.002	1.24	.108	NS
3.	Cassava and Other Root Crops	.006	.011	- .98	.163	NS
4.	Vegetables, Beans and Fruit	.012	.008	1.30	.097	NS
5.	Groundnuts	.004	.002	1.55	.061	NS
6.	Palm and Other Oils	.120	.082	1.74	.041	S
7.	Meat and Other Livestock Products	.052	.011	1.77	.038	S
8.	Salt and Other Condiments	.043	.029	3.89	.000	S
9.	Sugar	.018	.005	1.91	.028	S
10.	Fresh Fish	.032	.022	2.08	.018	S
11.	Dried Fish	.129	.085	4.05	.000	S
12.	Bakery Items	.009	.006	1.14	.127	NS
13.	Other Processed Foods	.008	.003	1.36	.088	NS
14.	All Beverages	.010	.009	.41	.339	NS
15.	Tobacco and Kola Nuts	.046	.032	5.09	.000	S
16.	Fuel and Light	.057	.041	4.10	.000	S

*NS = not significant at the .05 level of significance
S = significant at the .05 level of significance

included, and the hypothesis that the mean expenditure estimates based on the second, third and fourth day of recall are equal was tested.

Table 6.4 provides the statistical results of this second test of the original hypothesis. The null hypothesis is not rejected in any of the 16 tests. These results give strong statistical support to the hypothesis that the first day of recall is significantly different from the following three days of recall. This test also provides some evidence that the expenditure means of the second, third and fourth day of recall are not significantly different from one another.

The observed tendency for the mean expenditures from the first day of recall to be larger than those of the following three days of recall is an interesting finding. It provides some indication of the degree of memory decay occuring within one interview period. The hypothesis that the larger one-day recall means are the result of less memory decay relative to the following three days is consistent with the existing knowledge on memory loss. It is assumed that over time memory declines. While the rate of memory decay may vary depending on the item, its importance, and the frequency of purchase, memory is nevertheless impaired by the passage of time.

These data suggest that, regardless of the recall period, the first day of recall yields a more accurate estimate of expenditures than do subsequent days. This is logical since one would expect that the likelihood of forgetting purchases increases over time. As noted previously, this is particularly true for frequently purchased goods. Neter and Waksberg (1964) cite a similar result found in a study on reports of milk purchases for each of the days in a seven-day reference period. In this study Metz noted a 74 percent drop in reports of milk purchased between the first day of recall and the seventh.

TABLE 6.4

COMPARISON OF MEAN EXPENDITURES FROM THE SECOND, THIRD AND FOURTH DAY OF RECALL

Ho: $\bar{X}_{Bj} = \bar{X}_{Cj} = \bar{X}_{Dj}$

Ha: not all the X's are equal

n = 1787

$\bar{X}_B$ = mean expenditure of second day of recall

$\bar{X}_C$ = mean expenditure of third day of recall

$\bar{X}_D$ = mean expenditure of fourth day of recall

	Commodity	T^2	F	Probability	Significance*
1.	Rice	3.16	1.58	.206	NS
2.	Other Grains	1.90	.94	.386	NS
3.	Cassava and Other Root Crops	2.31	1.16	.315	NS
4.	Vegetables, Beans and Fruit	1.46	.73	.483	NS
5.	Groundnuts	.02	.01	.989	NS
6.	Palm and Other Oils	.11	.05	.946	NS
7.	Meat and Other Livestock Products	.69	.34	.710	NS
8.	Salt and Other Condiments	5.39	2.69	.068	NS
9.	Sugar	2.99	1.49	.225	NS
10.	Fresh Fish	3.13	1.56	.210	NS
11.	Dried Fish	.37	.18	.933	NS
12.	Bakery Items	1.48	.74	.478	NS
13.	Other Processed Foods	.84	.42	.657	NS
14.	All Beverages	.13	.06	.938	NS
15.	Tobacco and Kola Nuts	.28	.14	.870	NS
16.	Fuel and Light	2.97	1.48	.228	NS

*NS = not significant at the .05 level of significance

6.4 Differences in Expenditure Estimates Between the First and Second Interview

The results of the tests performed in Chapter 4 and 5 provide evidence that the one-interview subset generated higher, though not nessarily statistically different, expenditure estimates that the two-interview set. Because these two sets also represent expenditure estimates from a first interview and the average from a first and a second interview, it was decided that an analysis of individual days of recall from both interviews would be useful. The evidence provided by the parametric and non-parametric tests indicates that expenditure records from the first interview are generally higher than those of the second interview. An analysis of the same type of recall day from the first and second interviews might yield some insights on the reasons for this occurence.

To examine this question, two comparisons were made. First, the expenditure records from the first day of recall from both the first and second interview were examined. The second comparison was between the sum of the expenditure estimates of the second and third day of recall from both the first and second interview. In both cases the test procedure was the same one used in comparing the two-interview subset as described in Chapter 5. Data from the individual days of recall being compared were raised to monthly estimates using the procedure described in Appendix C. So that all households in the sample would have 12 months of data, indices were created. The indices created for use with the one-interview subset (described more thoroughly in Chapter 5) were used in this analysis as the indices for the days of recall from the first interview. A new set of monthly commodity indices was created from mean expenditures estimated from the data obtained only from the second interview.

Using these indices to fill in missing data on households with eight months or more of data yielded a sample size of 104

households. This procedure permitted the generation of 16 annual commodity expenditure estimates. Once these were obtained the correlated t-test was used to test the hypothesis that the means from the paired sets are the same.

This hypothesis was tested first by comparing the annual expenditure estimates based on the first day of recall from the first interview, with those from the first day of recall from the second interview. This represents an important comparison, since the first day of recall is believed to represent the most accurate recall because memory of expenditures is freshest.

The test indicates that no significant difference exists for any of the 16 commodity groups at the .05 level of significance. As Table 6.5 reveals, however, the expenditure estimates from the first interview tend to be larger than those of the second interview. In 9 cases out of 16 the first interview estimates are larger than those of the second interview--57.3 percent, on average. If the Other Grains commodity category is excluded because of the extreme difference between the two estimates, the first interview estimate is still 13.8 percent higher.

The analysis of the mean annual expenditure estimates from the sum of the second and third days of recall from the first and second interview, yields similar results. As Table 6.6 shows, the research hypothesis that the two means are equal is accepted in only 13 out of 16 cases at the .05 level of significance. The research hypothesis is rejected in three cases--Rice, Dried Fish, and All Beverages. In this test the first interview means are larger than those based on the second interview in 14 out of 16 cases, and in percentage terms they are approximately 30.5 percent larger.

6.5 Discussion of Results

The results from the comparison of the same recall days from the first and second interview help to explain the

TABLE 6.5

RESULTS OF COMPARISON OF ANNUAL EXPENDITURE ESTIMATES FROM THE FIRST AND SECOND INTERVIEW BASED ON THE FIRST DAY OF RECALL

Ho: $\bar{X}_{Aj} = \bar{X}_{Bj}$
Ha: $\bar{X}_{Aj} \neq \bar{X}_{Bj}$
n = 104

$\bar{X}_A$ = first interview one-day recall mean annual expenditure estimate
$\bar{X}_B$ = second interview one-day recall mean annual expenditure estimate
j = commodity (1,...,16)

	Commodity	$\bar{X}_A$ (Leones)	$\bar{X}_B$ (Leones)	T-Value	Probability	Significance*
1.	Rice	72.97	46.77	1.59	.115	NS
2.	Other Grains	7.05	.87	.99	.323	NS
3.	Cassava and Other Root Crops	2.48	1.51	1.25	.213	NS
4.	Vegetables, Beans and Fruit	5.66	4.15	.52	.605	NS
5.	Groundnuts	.85	.62	.64	.526	NS
6.	Palm and Other Oils	36.94	28.81	1.12	.267	NS
7.	Meat and Other Livestock Products	9.50	12.03	- .51	.613	NS
8.	Salt and Other Condiments	16.02	14.32	.72	.475	NS
9.	Sugar	2.97	4.07	-1.00	.321	NS
10.	Fresh Fish	9.64	10.03	- .21	.831	NS
11.	Dried Fish	45.16	47.70	- .50	.617	NS
12.	Bakery Items	2.70	3.05	- .33	.742	NS
13.	Other Processed Foods	1.27	.99	.41	.682	NS
14.	All Beverages	4.88	3.56	.87	.389	NS
15.	Tobacco and Kola Nuts	16.24	17.42	- .55	.583	NS
16.	Fuel and Light	17.27	21.28	-1.02	.311	NS

*NS = not significant at the .05 level of significance

TABLE 6.6

RESULTS OF COMPARISON OF ANNUAL EXPENDITURE ESTIMATES FROM THE FIRST AND SECOND INTERVIEW BASED ON THE AVERAGE OF THE SECOND AND THIRD DAYS OF RECALL

Ho: $\bar{X}_{Cj} = \bar{X}_{Dj}$

Ha: $\bar{X}_{Cj} \neq \bar{X}_{Dj}$

n = 104

$\bar{X}_{Cj}$ = first interview, average of the second and third day of recall mean annual expenditure estimate

$\bar{X}_{Dj}$ = second interview, average of the second and third day of recall mean annual expenditure estimate

j = commodity (1,...,16)

Commodity	$\bar{X}_C$ (Leones)	$\bar{X}_D$ (Leones)	T-Value	Probability	Significance*
1. Rice	72.40	43.28	2.50	.014	S
2. Other Grains	1.09	.74	.91	.366	NS
3. Cassava and Other Root Crops	5.49	3.09	.80	.424	NS
4. Vegetables, Beans and Fruit	2.87	1.68	1.79	.077	NS
5. Groundnuts	.52	.40	.55	.587	NS
6. Palm and Other Oils	27.79	24.16	.45	.657	NS
7. Meat and Other Livestock Products	5.12	4.48	.64	.521	NS
8. Salt and Other Condiments	9.49	7.74	1.54	.126	NS
9. Sugar	2.03	1.91	.27	.791	NS
10. Fresh Fish	7.38	7.71	- .35	.728	NS
11. Dried Fish	31.53	23.12	3.51	.001	S
12. Bakery Items	2.54	3.44	- .81	.418	NS
13. Other Processed Foods	1.18	.64	1.42	.159	NS
14. All Beverages	4.45	3.44	.55	.586	NS
15. Tobacco and Kola Nuts	12.38	10.24	2.17	.032	S
16. Fuel and Light	14.70	14.34	.23	.819	NS

*NS = not significant at the .05 level
S = significant at the .05 level

observed difference in the two-interview set and the one-interview subset. The latter represents the first interview. It has been found in this latest analysis that both the expenditure estimates from the first and the sum of the second and third day of recall from the first interview are larger than those of the second interview.

While these results indicate that the expenditure estimates of the first interview are consistently larger than the estimates derived from the second interview on a same day of recall basis, they do not explain the reason for these differences. There are several possible explanations of these results.

One may be the fact that the two-interview set is actually a combination of a first interview with unbounded recall and a second interview with bounded recall. The one-interview subset is, in contrast, based on a first interview with unbounded recall. In this view, the first interview administered to a household in a given month reflects the unbounded reference period. Approximately four weeks of expenditures have passed since the last interview. With an unbounded reference period there exists the possibility, as noted by Moser and Kalton (1972), that telescoping of purchases will occur. This would result in the inclusion of purchases made outside of the reference period under investigation. The one-interview subset consisted of this first interview. Though no attempt was made to control for possible repetition of purchases in the second interview, one could hypothesize that there would be less likelihood that the same magnitude of telescoping would occur. This would be due to the fact that the first interview was only three days prior to the second. That would give respondents more of a boundary on their memories. Some respondents might recall, without being reminded, the purchases they had reported three days prior. One could argue that this would reduce the amount of error arising from telescoping found in the expenditure estimates derived from the second

interview. On the average, this would be reflected in lower mean expenditure estimates from the second interview.

Another explanation for the observed differences between the first and second interview centers around the conditioning process discussed earlier in Chapter 2. This is a problem associated with repeated visits to survey participants. In the process of being interviewed repeatedly, the level of accuracy of reported expenditure decreases because of respondent fatigue. A certain manifestation of the conditioning process might take place between the first and second interview in a month. In the first interview administered in a month, respondents are relatively "fresh." They have not had to answer questions concerning consumption expenditures in three to four weeks. By the time the second interview takes place three days later, respondents have become fatigued by the process and are no longer willing to give the time and energy necessary to remember expenditures accurately. This results in lower records of expenditures reported during the second interview.

The problem with these two explanations if that they are not mutually exclusive. It is theoretically possible to observe both effects occurring in the data at the same time. As they both lead to the same results--higher expenditure estimates in the first interview than in the second—it is very difficult to isolate their effects from one another.

In a study by Neter and Waksberg (1964) that analyzed expenditure records from bounded and unbounded recall periods, evidence of both telescoping and conditioning were discovered. The authors compared reports of household alteration and repair expenditures derived from bounded and unbounded recall periods of lengths ranging from one month to six months. The cumulative evidence from their study indicated that unbounded recall periods were subject to a net forward telescoping of expenditures into the period covered by the interview. They also found evidence to suggest that the telescoping effect increased with the size of the alteration or

repair job. This is consistent with the evidence that telescoping is a phenomenon most closely associated with larger, more infrequent expenditures.

In the same study Neter and Waksberg found evidence of moderate conditioning losses occurring between first, second and third interviews; this was particularly true for smaller jobs. They estimated that participants interviewed a third time reported approximately 9 percent fewer jobs than they had in the second interview.

In a study done by Turner (1961), households kept itemized records of expenditures for 14 days. Expenditure records from the first week were then compared with expenditure records from the second week, and an analysis of the two sets showed that for various groups of commodities the average expenditures reported by households during the first week were significantly higher than the average of the expenditure estimates recorded during the second week. In this study Turner was able to group households according to certain group characteristics and found that the observed inter-week variation did not appear to be correlated with these characteristics. The design of Turner's survey did not permit him to separate out the influences of telescoping and conditioning. However, he did cite them both as possible explanations for the observed outcome of his study.

The design of the Sierra Leone study did not permit a closer examination of the separate effects of telescoping and conditioning. It is, therefore, difficult to determine which of these effects exerts a stronger influence on the expenditure records. The case could be made that because the data used in this analysis reflected primarily the frequently purchased and therefore less significant items, memory decay was a more serious problem than telescoping. If this is true, the Sierra Leone data would most likely be subject to the effects of conditioning or respondent fatigue, making the first-interview estimates more accurate than the two-interview estimates.

7. CONCLUSION

7.1 Summary of Research Findings

The purpose of this analysis is to provide empirical evidence on some of the trade-offs involved in determining the interview frequency and reference period to be used in the collection of consumption expenditure data. Both these issues have great impact on the cost and reliability of the data collected. An understanding of the influence of these variables is important in the development of a cost-efficient methodology for obtaining the needed "bread and butter" information so crucial to development planning.

The results in this analysis have not conclusively supported, in a statistical sense, the hypothesis that the mean expenditure estimates derived from data collected in one interview per month and two interviews per month are equal. In the analysis using non-parametric techniques, the null hypothesis could not be accepted at the .05 significance level. In looking at the data on an annual basis and in a more aggregated fashion, they reveal a tendency for the one-interview subset to generate monthly and annual expenditure estimates which are higher than those based on two interviews per month. On the average, the expenditure estimates of the former are approximately 5.3 percent higher than those of the latter.

In analyzing the mean expenditure estimates generated by the four different days of recall, the means from the first day of recall are consistently larger than those of the second, third and fourth day of recall. In analyzing the difference between the expenditure means of the first day of recall with those of the second, third and fourth day of recall combined, it is significant at the .05 level in 8 out of 16 cases. The

expenditure estimates of the first day of recall are 112 percent higher than those based on the sum of the second, third and fourth days of recall. This analysis provides some evidence on the degree of memory decay taking place between the days of recall in the Sierra Leone study.

Comparisons were also made between individual days of recall from the first and second interview. In comparing expenditure estimates from each of the first days of recall with those from each of the second and third days of recall, expenditure estimates based on the first interview are considerably larger than those of the latter. In percentage terms expenditure estimates from the first day of recall are approximately 57.3 percent larger and the expenditure estimates from the sum of the second and third day of recall are 30.5 percent larger. This difference is attributed to the presence of conditioning and/or telescoping. It was not possible, given the nature of the data, to isolate each of the effects to determine the extent of its influence.

7.2 Research Implications

Caution must be exercised in making inferences, based on this analysis, about the design of other consumption expenditure surveys in other countries. To some extent the results described in this analysis are location specific. Different groups of people may have a greater or lesser ability to accurately remember purchases made over a given period. Certain region-specific marketing cycles may necessitate certain types of survey designs. Knowledge of these differences would influence the choice of both the interview frequency and length of recall.

It is recognized that no one survey methodology is suitable for all purposes. The objectives of the study should determine to a great extent the scope of the data requirements and influence all phases of survey design, collection, tabulation and analysis. The amount and reliability of information

already in existence, the resources available, the budget, time, and labor available are also important variables. No methodology can substitute for in-depth knowledge of the system being examined. Some baseline information on the target population's characteristics, seasonal patterns, marketing cycles, and consumption habits is essential in the development of an adequate survey design.

While the results of this analysis do not generate absolute guidelines for survey design, they do provide some important empirical evidence and insights useful for field surveys. First, these results, on the whole, do suggest that an intensive survey methodology is unnecessary for purposes of collecting baseline statistical information on a population's expenditure levels and habits. In fact, the argument can be made that the frequent visit methodology jeopardizes expenditure results by increasing the likelihood of respondent fatigue.

Second, this study suggests that whatever the survey design, researchers need to be concerned with the possible influences of telescoping and conditioning. To the extent possible, attempts should be made to control for these effects. To reduce the amount of telescoping, comparisons can be made of expenditure reports of successive interviews to check for obvious repetition of expenditures. Also, enumerators can be instructed to attempt to associate each day of recall with an event unique to that day, such as the day of the thunderstorm, etc.

If frequent interviewing of households is deemed necessary, care must be taken to watch for signs of respondent fatigue. Kalton and Moser (1972) suggest a careful replacement of some proportion of household participants with new households. These replacement households must, of course, be carefully selected so as to reflect the same characteristics of the households being replaced.

Third, resources saved by interviewing less frequently could be applied to other areas of survey design. The large

sample error observed in the study could be reduced by increasing the sample size. This would tighten the confidence range around the parameters estimated from the data. Alternatively, some of the cost-saving could be used to fund pre-survey exploration. This might include some small pilot studies, pretesting of questionnaires, etc. Expenses saved by interviewing less frequently could also be used to develop a more intensive training program for enumerators and other survey personnel. The development of a thoroughly trained cadre of field researchers represents a substantial contribution to a nation's overall development process.

Fourth, survey designers have to be sensitive to the significant changes in the quality of memory from one day to the next. In this analysis the first day of recall was shown to differ significantly from the other three days of recall. In other survey situations the number of days before significant memory decay begins may be different. Small pilot surveys might be useful in determining the relevant period for a particular population.

A final insight provided by this study is the need to design a survey compatible with the resources and trained personnel available for the study. Overextending these resources can result in the introduction of significant distortions in the data. In order to maintain the integrity of the survey results it is essential that the participants be chosen and interviewed in the proper manner. Deviations from the design of the survey must be strictly controlled. This requires that the foot soldiers of all surveys, the enumerators, understand thoroughly the importance of all procedures and execute them faithfully. It also requires an adequate staff of field supervisors. If trained personnel are not available, it may be prudent not to attempt the implementation of the complex multi-visit methodology. When adequate staff is lacking, a simpler survey design might actually generate more accurate results.

Researchers working in low-income countries have an obligation to contribute to the development of improved field collection methodologies. Specifically, research methods should be developed that generate reliable data in the most cost-efficient manner. If properly developed, these procedures can contribute to the development of local capacities to generate, process and interpret information on consumer behavior. These are crucial inputs in the formation and evaluation of policy alternatives.

APPENDICES

APPENDIX A

DISAGGREGATED COMMODITY LIST

FOOD

000 Cereal Grains
- 001 Clean Parboiled Rice
- 002 Rough Rice
- 003 Husk Rice
- 004 Maize
- 005 Sorghum or Guinea Corn
- 006 Millet
- 007 Fundi

008 Root Crops
- 009 Cassava
- 010 Yams
- 011 Cocoyams
- 012 Sweet Potatoes
- 013 Chinese Yams

014 Leguminous Products
- 015 Groundnuts
- 016 Black-eyed Beans
- 017 Green Beans
- 018 Broad Beans
- 019 Pigeon Peas
- 020 Soya Beans

021 Vegetables
- 022 Onions
- 023 Okra
- 024 Carrots
- 025 Cabbage
- 026 Egg Plants
- 027 Greens (Plasas)
- 028 Jackatoes
- 029 Pumpkins
- 030 Tomatoes
- 031 Watermelons
- 032 Cucumbers

033 Fruits
- 034 Oranges
- 035 Pineapples
- 036 Bananas
- 037 Plantains
- 038 Mangoes
- 039 Coconuts
- 040 Paw Paws
- 041 Grapefruit
- 042 Tangerines (Lemons)
- 043 Sweet Limes
- 044 Avocados
- 045 Lemons (Limes)
- 046 Guava
- 047 Bredfruit
- 048 Plums

049 Other Crops
- 050 Benniseed
- 051 Ginger

052 Fresh Fish
- 053 Fresh Bonga
- 054 Fresh Skate
- 055 Fresh Spanish
- 056 Fresh Whiting
- 057 Fresh Catfish
- 058 Fresh Snapper
- 059 Fresh Awefue
- 060 Fresh Mackerel
- 061 Fresh Lady
- 062 Fresh Mullet
- 063 Fresh Other Salt Water Fish
- 064 Fresh Water Fish

065 Dried Fish
- 066 Dried Bonga
- 067 Dried Skate
- 068 Dried Spanish
- 069 Dried Catfish
- 070 Dried Snapper
- 071 Dried Awefue
- 072 Dried Mackerel
- 073 Dried Lady
- 074 Dried Mullet
- 075 Dried Other Salt Water Fish
- 076 Dried Fresh Water Fish

077 Frozen or Iced Fish

078 Tinned Fish

079 Meat
- 080 Fresh Beef
- 081 Dried Beef
- 082 Pork
- 083 Poultry
- 084 Goat
- 085 Sheep
- 086 Bush Meat

087 Other Livestock Products
- 088 Fresh Milk
- 089 Fullah Butter
- 090 Eggs

091 Oils and Fats
- 092 Palm Oil
- 093 Nut Oil
- 094 Groundnut Oil
- 095 Coconut Oil

096 Margarine
097 Cooking Oil
098 Processed Foods
099 Breads
100 Cakes
101 Fufu
102 Gari
103 Agidi
104 Rice Flour
105 Biscuits (NATCO)
106 Flour
107 Tinned Milk
108 Tomato Paste
109 Cassava Bread
110 Condiments
111 Salt
112 Sugar
113 Maggi Cubes
114 Peppers
115 Other Foods
116 Drinks
117 Soft Drinks
118 Bottled Soft Drinks
119 Ginger Beer (Local)
120 Alcoholic Drinks
121 Palm Wine
122 Omole
123 Bamboo Wine
124 Star and Heineken Beer
125 Liquors (Rum, etc.)
126 Coffee and Tea
127 Coffee
128 Tea
129 Tobacco
130 Snuff
131 Cigarettes
132 Tobacco
133 Kolanuts
134 Household Goods
135 Fuel and Light
136 Firewood
137 Charcoal
138 Panlamps
139 Kerosene
140 Candles
141 Matches
142 Lantern and Lantern Pants
143 Pots and Pans
144 Country Pots
145 Tin and Aluminum Pots
146 Enamel Pots and Ware
147 Wooden Spoons
148 Calabash
149 Eating Utensils
150 Plates
151 Buckets, etc.
152 Bucket
153 Drum
154 Baff Pan
155 Wood Furniture
156 Chairs
157 Beds
158 Mats
159 Other Furniture
160 Steel Beds
161 Hammocks
162 Construction Materials
163 Boards
164 Timber
165 Bricks
166 Nails
167 Paint
168 Locks
169 Roofing Iron
170 Cement
171 Other Household Items
172 Brooms
173 Radios
174 Batteries
175 Soap
176 Mosquito Nets
177 Personal Items
178 Cloth
179 Country Cloth
180 Gara Lappa
181 Cotton Lappa
182 Other Cloth
183 Clothing (Ready-Made)
184 Shirts
185 Shorts
186 Trousers
187 Gowns
188 Dresses
189 Underwear
190 Jongs
191 Caps
192 Shoes and Sandals
193 Rubber Sandals
194 Plastic Shoes
195 Cosmetics
196 Perfume
197 Vaseline
198 Jelly
199 Powder
200 Jewelry
201 Other Personal Items
202 Watch
203 Umbrella

204 Pipe
205 Suitcase
206 Services
207 Personal Services
208 Tailoring
209 Hair Grooming
210 Shoe Repair
211 Photography
212 Household Services
213 Thatching
214 Masonery
215 Buckling
216 Domestic Servant
217 House Rent
218 Transport
219 Fares
220 Lorry Fares
221 Taxi
222 Bus
223 Launch
224 Transport Equipment
225 Bike
226 Bike Repair
227 Ceremonial and Entertainment
228 Ceremonial
229 Initiation Fee
230 Funerals
231 Religious Festivals
232 Payments for Drummer, Dancer
233 Entertainment
234 Gambling
235 Medical
236 Medicines
237 Native
238 Imported
239 Medical Fees
240 Dispenser
241 Hospital
242 Native Doctor
243 Educational
244 School Fees
245 Books
246 Uniforms
247 Pens and Paper
248 Lodging
249 Arabic Fees
250 Savings
251 Osusu
252 Cooperative
253 Other Expenditures
254 Local Tax
255 Court Case
256 Purchase of Household Pets
257 Nothing

APPENDIX B

ASSIGNMENT OF INTERVIEWS WHICH OVERLAPPED TWO MONTHS

Due to the scheduling of interviews there were some instances where a household interview or two-interview set bridged two months. This occurred infrequently, but in order to maximize the size of the sample a special procedure was developed to assign the overlapping interviews to one of the two months involved. The decision rules to assign these overlapping months were chosen so as to maximize the number of household month observations and to maximize the number of seven-day, or two-interview, sets included in the sample.

A check was first made of the total data file to locate any of the overlapping interview sets. To be identified, an overlapping data set had to be either a valid seven-day set or valid four-day set as defined in Chapter 3. A check was then made of the other data available in the two months sharing the overlap interview. If the two months sharing an overlapping seven-day data set had no other data, the overlapping interview was assigned to the month which contained most of the interview days. If one of the months had either a valid seven- or four-day set, then the overlapping interview was assigned to the month with no data. If one or both months had valid four-day data sets, then the valid seven-day overlapping interview set replaced one of the four-day data sets. If both months had a valid seven-day data set, then the overlapping interview was ignored.

The same basic procedure was followed if the overlapping interview was a four-day data set. The only difference was that the four-day data set would never replace a seven-day data set.

APPENDIX C

PROCEDURE TO "PUFF UP" THE DATA

In order to compare the expenditure estimates based on two interviews per month and the one-interview subset, it was necessary to "puff" them up into a comparable form. Thus, expenditures were puffed up to represent monthly expenditure levels.

The basic procedure was to multiply the recorded expenditures for a particular commodity and month by the number of days in the month divided by the number of days of information present. Because there were several comparisons made of sets with different interview lengths, different ratios were constructed. For example, with the two-interview set, the denominator used in the "puffing up" procedure was 7, representing the number of days in a month for which there was information. The denominator in the one-interview subset was 4.

To give an axample of this procedure, assume that the month in question is August. To puff up the two-interview information into monthly data, the recorded expenditures for a particular commodity would be multiplied by the ratio:

$$\frac{\text{number of days in the month}}{\text{sum of observed expenditures}}$$ which in the example is $\frac{31}{7}$.

For the one-interview subset the only difference was in the denominator. Using the same example, it would be $\frac{31}{4}$.

APPENDIX D

INDEXING PROCEDURE

An indexing procedure was used to estimate monthly commodity expenditures for households with missing data. Two separate sets of indices were constructed, one reflecting the consumption patterns observed using two interviews per month, and the other reflecting those reported in one-interview per month. An individual index was constructed for each of the 16 commodities for each of the 12 months from May 1974 to April 1975. Data contained in the C-2 or long reference questionnaire and in the one-interview independent set were not included in the computation of the indices.

To calculate the indices, data from all 247 households included in the sample were used. However, the indices were used to estimate expenditures for missing months only in those cases where a particular household had eight months or more of data. No annual expenditure data were calculated for households with less than eight months of data.

In more detail, the procedure was as follows. After the data had been puffed up to represent monthly expenditures as described in Appendix C, mean monthly expenditures were calculated for each of the 16 commodities, for each month, and for both the two-interview and one-interview subset. The following formulae were used to calculate the monthly expenditure estimates:

$$\bar{X}_{Tjk} = \sum_{i=1}^{n} \bar{X}_{Tijk}/n$$

$$\bar{X}_{Sjk} = \sum_{i=1}^{n} \bar{X}_{Sijk}/n$$

T = expenditure record based on the two-interview set
S = expenditure record based on the one-interview subset
i = household with valid data set in given month
j = commodity (1,...,16)
k = month (1,...,12)

where: X_{Tijk} = expenditure by the i^{th} household on the j^{th} commodity group, in the k^{th} month based on the two-interview set.

X_{Sijk} = expenditure by the i^{th} household on the j^{th} commodity group, in the k^{th} month based on the one-interview subset.

N_j = the total number of households with valid data for the j^{th} month.

To obtain the denominators of the indices, the average monthly expenditures for each of the 16 commodity groups were summed over the 12 relevant months as shown in the following equations:

$$X^*_{Tj} = \sum_{k=1}^{12} \bar{X}_{Tjk}$$

$$X^*_{Sj} = \sum_{k=1}^{12} \bar{X}_{Sjk}$$

This generated average annual expenditures for both the two-interview set and one-interview subset for the j^{th} commodity group.

To obtain individual monthly indices for each of the j commodity groups for both the two-interview set and one-interview subset, the following calculation was performed:

$$I_{Tjk} = \frac{\bar{X}_{Tjk}}{X^*_{Tj}}$$

$$I_{Sjk} = \frac{\bar{X}_{Sjk}}{X^*_{Sj}}$$

where: I_{Tjk} = the index derived from the two-interview set for the j^{th} commodity and the k^{th} month.

I_{Sjk} = the index derived from the one-interview subset for the j^{th} commodity and the k^{th} month.

The sum of the monthly indices being equal to unity.

The adjusted total expenditure for an individual household, reflecting 12 months of data for a particular commodity group and interview frequency, was calculated next. The formulae used were:

$$EXP^{*}_{Tij} = [1/I_{Tj}]EXP_{Tij}$$

$$EXP^{*}_{Sij} = [1/I_{Sj}]EXP_{Sij}$$

where: EXP^{*}_{Tij} = the total adjusted annual expenditure by the i^{th} household for the j^{th} commodity group based on two interviews per month.

EXP^{*}_{Sij} = the total adjusted annual expenditure by the i^{th} household for the j^{th} commodity group based on the one-interview subset.

I_{Tj} = the sum of the indices for the j^{th} commodity group, for the months for which valid data are present for the i^{th} household based on the two-interview set.

I_{Sj} = the sum of the indices for the j^{th} commodity group for the months for which valid data are present for the i^{th} household based on the one-interview subset.

EXP_{Tij} = summation of expenditures on the j^{th} commodity for the months for which data are available for the i^{th} household based on two interviews per month.

EXP_{Sij} = summation of expenditures on the j^{th} commodity for the months for which data are available for the i^{th} household based on the one-interview subset.

Total annual expenditures for each household with eight months or more of information were estimated using this formula. This provided a sample size of 104 households with 12 months of information for each household.

The procedure just described was also used to calculate a set of monthly commodity indices based on data from the

second interview in a month. These indices were used in conjunction with data from the first day of recall and the second and third day of recall from the second interview in a month, to estimate annual expenditures.

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