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Expenditure Surveys: A Case Study  
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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. The data base used in this study was a comprehensive rural consumption expenditure survey conducted in Sierra Leone in 1974-1975.

The objectives of this study were: 1) to analyze the differences in household expenditure estimates based on data collected during two interviews per month with data collected during one interview per month; 2) to compare expenditure estimates derived from each of the four individual days of recall contained in one interview; and 3) to determine if the expenditure estimates based on a first or sum of the second and third day of recall differed depending on whether the data were drawn from the first or second interview.

Results of this analysis provided some, though not conclusive, evidence that expenditure estimates based on one interview per month were not statistically 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. Expenditure estimates of days of recall from the first interview were higher than those from the second interview in a month. Problems of memory decay, respondent fatigue, and some telescoping of expenditures were cited as explanations for the results.

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## CHAPTER 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. From these surveys it may be possible to estimate elasticities of demand for goods and services, information which is 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 thwarts 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 found 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. Firstly, 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 the investigation of methodological issues, and thus improvements in survey design are not field tested and evaluated systematically. This is understandable, though not desirable, given the high costs that complicated replications of different survey techniques under similar conditions would entail.

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 includes 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 when compared with 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.

The problem with this methodological approach is that it is generally costly and time-consuming. Its comprehensive nature generates higher costs in every phase of the data collection process. 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 marginal utility of an increase in accuracy is exceeded by the marginal cost of obtaining it. This happens either because resources are limited or because the increase in accuracy is not necessary given the objectives of the study.

The imperative 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 the kind of "bread and butter" information needed by governments with some minimum criterion of reliability. 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

The purpose of this paper is to explore two issues relevant to the cost of collecting, processing and using information as well as its reliability. First is interview frequency, that is 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. The reference period is the length of time over which a respondent is requested to report purchases during one interview. This period can range anywhere from a twenty-four hour recall to a month, three-month, six-month, or year recall. 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 was made of these two issues using data collected in a 1974-75 comprehensive frequent visit micro-level study conducted in rural Sierra Leone. Parametric and non-parametric tests were used to examine the differences in mean expenditure estimates derived from one interview per month versus two interviews per month. This was done on a monthly and annual basis for both a very disaggregated list of commodities and a consolidated list of commodity groups.

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

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 this 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 of this paper 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 contained in this chapter 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 approach compared three different data sets representing expenditure estimates based on one and two interviews per month.



This will be 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 were created representing food items, beverages and some frequently purchased items.

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 tested using Hotelling's  $T^2$  test. In this chapter 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.

## CHAPTER 2

### THE RESEARCH PROBLEM

#### 2.1 Factors in Survey Design

There are numerous methodological factors involved in survey design which contribute to the cost per unit of information and data turn-around time. Some of the factors relevant to survey design include sample size, sample selection procedure, collection technique (e.g., interview, questionnaire, group interview), and duration of survey. Critical to the choices made 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. The attempt should be made, therefore, to minimize the relevant threats to validity, which vary depending on the objectives of the study.

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. The panel method, which was incorporated into the design of the Sierra Leone study, specifies the collection of data from the same sample on more than one occasion. Two specific problems arise when using this method which Moser and Kalton (1972) identify as sample mortality and conditioning. 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, however, could arise 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. With repeated visits to particular households there is a risk that they 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. The 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 which 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 type of error influencing sample reliability is the class of errors described as sample errors. As Moser and Kalton (1972) describe it, 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. Two factors influencing the degree of sample error present are sample size and the variability in the population. The smaller the sample size and/or the greater the variance in population characteristics the greater the standard error. What this intuitively implies is 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.

There are several factors involved in determining the interview frequency and reference period. The trade-off between increased accuracy and

increased cost has been discussed earlier. The possibility of obtaining more accurate data always exists. However, at some point diminishing returns to accuracy set in. Thus, it is important to be able to judge when the marginal cost of improved accuracy is greater than its marginal utility for the particular objectives of the survey. Determining this point helps to determine the survey's tolerance to measurement and sample error.

## 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 result, in general, in a smaller standard error. However, the costs of collecting data from a large sample or from repeated visits to households can be quite high. The implementation of surveys reflecting these two types of design generally requires significant administrative and supervisory capacity. It is also necessary to have the facility to handle and process the extensive amount of data being collected. If these capabilities are not available, significant measurement error could be introduced into the data. A balance must be struck between sample error and bias. Considerations important in assessing this relationship are the extent of variations in household expenditures due to income, household size, and cultural or regional preferences.

Rey (1976) suggests that an important concern in determining 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, in many low income countries where the majority of the population is involved in subsistence agricultural production, seasons will have great impact on expenditure patterns. It is essential, therefore, that the influence of seasons be accounted for in intramonth interview scheduling.

Other factors involved in determining interview frequency are 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 collection 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 which 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. Choices concerning the length of reference period 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. This is because a long interview reference period

permits the collection of more data points during the one interview at little extra cost. Alternatively, this information could be obtained in separate interviews but the costs would be significantly higher. At the same time, however, 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 marginal cost by lengthening the reference period and reducing the marginal utility 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 thus be made 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 the longer the period for which it must be remembered. The importance of the purchase to the respondent is the second factor which influences how well the expenditure is remembered. 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 was used for items with a shorter recall, i.e., those items

frequently purchased and less significant to the respondent. A longer reference period was 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. In a study done by Neter and Waksberg (1964) it was found 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 paragraphs 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 these factors have significant influence on reducing measurement errors, there is little existing empirical evidence to tell us either how much or at what cost the improved accuracy is obtained.

## CHAPTER 3

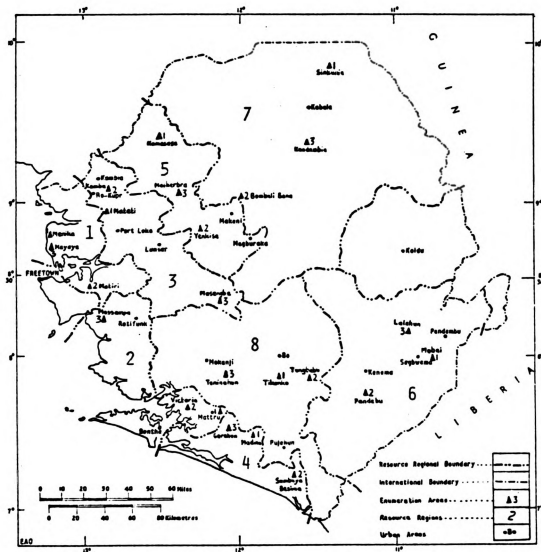
### SURVEY METHODOLOGY USED IN SIERRA LEONE RURAL CONSUMPTION EXPENDITURE SURVEY

#### 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 the collection of 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 using a stratified sampling procedure. Based on available secondary data Sierra Leone was divided 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. Each of these areas was approximately ten square miles. Roughly 130 farm families located in one to ten villages were contained in each enumeration area.

As the purpose of the survey was to obtain information on rural households, enumeration areas falling into or containing urban areas were



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

FIGURE 3.1  
SIERRA LEONE RURAL RESOURCE REGIONS

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 available on the occupational distribution in Sierra Leone and the 1963 population census were used to determine which of the enumeration areas were to be eliminated based on this criterion.

Within each of the eight resource regions three non-urban enumeration areas were chosen at random. This generated a total of 24 enumeration areas to be included 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 here generated the sample frame. Recorded for each household was the name, 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. However, during the course of survey implementation and processing certain households had to be dropped from the survey. Reasons for this included deaths within selected households, movement from village, and households with severe problems of missing data. 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 twice weekly over the fourteen-month survey period by a resident enumerator. 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 through the use of seven other questionnaires which used varying interview schedules and reference periods.

Approximately one-half of the 500 households included 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. For convenience the sample households were divided into three groups, each containing four households. Each household in each group corresponded to a 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 error in response due to memory decay.

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

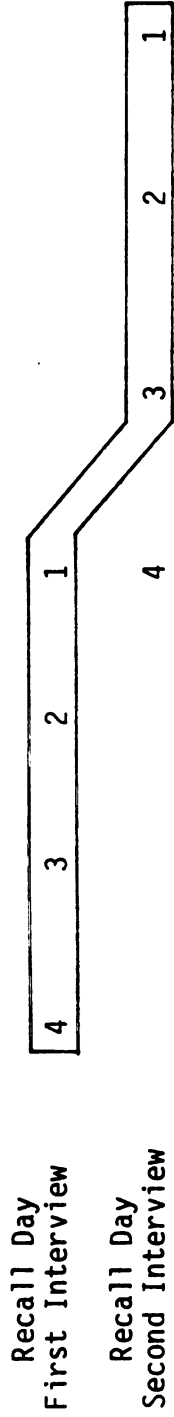
interviews were to occur within three days of one another so as to collect expenditure information for seven contiguous days. Thus, in the course of two interviews given during seven succeeding calendar days one week of consumption expenditure data was collected. Figure 3.2 gives an example of an interview 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 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. In the Sierra Leone study three different interview day combinations were used, Monday-Thursday, Tuesday-Friday, and Wednesday-Saturday. This insured 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, there is an overlap day 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 the 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

---

Day of Week	Saturday	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Date	11	12	13	14	15	16	17	18



Interview dates are Wednesday the 15th and Saturday the 18th.

---

FIGURE 3.2  
EXAMPLE INTERVIEW SCHEDULE

commodity expenditure 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. Where possible names were obtained. 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

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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.



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 purchases made during an entire month.

### 3.3 Description of Interview Categories

Problems with the data were encountered in attempting to test our research hypotheses. While each household was to have been interviewed twice obtaining 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 missing. This is not the same thing as reporting that no expenditures were made. The latter was considered a valid expression of an expenditure pattern. What is being referred to here is that for some reason a household was not interviewed during a given month and, therefore, has zero days of information. At the other extreme some households had information for more than seven days per month.

Presumably there are numerous reasons 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, deaths, and/or absenteeism 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. 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 test the research hypotheses we had to identify for each household those months for which there were at least seven days of information 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 our 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 which consisted of only those months for which a household had seven days of information, there remained a quantity of household month observations for which there were four days or more of information but less than seven. If in this residual data there existed information 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 it was 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 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 with 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.

TABLE 3.1  
NUMBER OF HOUSEHOLD-MONTH OBSERVATIONS

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

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

## CHAPTER 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 have a normal distribution. The assumption of normalcy is clearly not the case when dealing with expenditures data where purchases of zero represent a large proportion of the observations for a particular commodity. The zero observations cannot be eliminated as they are not a reflection of non-response, but rather non-purchase. The latter 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 provide 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 qualitative 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,

---

\* 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 our decision rule on the value of the two-interview set seemed to be the most efficient way of handling this problem.

the probability on any comparison of means between two samples is  $p = .5$  that one would be larger than the other and vice versa. If the sample size is large, the binomial probability distribution approaches the normal distribution, permitting the computation of test statistics with which to test the research hypothesis.

The research hypothesis tested here was that there was no difference 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 testing of the research hypothesis compared the two-interview set with 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 research 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

TABLE 4.1

RESULTS OF NON-PARAMETRIC TESTS 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  $j^{\text{th}}$  commodity (1,...,257)  
                     in the  $k^{\text{th}}$  month (1,...,14) based on two interviews per  
                     month.

$\bar{X}_{Sjk}$  = mean monthly expenditure on the  $j^{\text{th}}$  commodity (1,...,257)  
                     in the  $k^{\text{th}}$  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 standardized normal  
 distribution  $\approx N(0,1)$ .

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


---

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.

As might be expected in the analysis of variances 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 in Table 4.2, the variances of the two-interview set were smaller than the variances of the one-interview independent set in 407 cases; the opposite was true in 721 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

---

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

$\sigma^2_{Sjk}$  = variance of monthly expenditure estimate for the  $j^{\text{th}}$  commodity (1,...,257) in the  $k^{\text{th}}$  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 there is a tendency for the one-interview subset expenditure estimates 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 research hypothesis that the two means were equal was tested. The hypothesis tested and the derivation of these figures is shown in Table 4.3.

TABLE 4.3  
COMPARISON OF TOTAL MEAN EXPENDITURES

$$\begin{aligned} \text{Ho: } \bar{X}_{TE} &= \bar{X}_{SE} \\ \text{Ha: } \bar{X}_{TE} &\neq \bar{X}_{SE} \end{aligned}$$

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:

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

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

$$\begin{aligned} j &= \text{commodity } (1, \dots, 257) \\ k &= \text{month } (1, \dots, 14) \end{aligned}$$

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

The total mean expenditure estimate for the two-interview data set for fourteen months of information is 25.095. The total mean expenditure estimate for the one-interview subset is 26.920. Using the correlated 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 research 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 7 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 research 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.

TABLE 4.4  
COMPARISON OF THE TWO-INTERVIEW SET WITH  
THE ONE-INTERVIEW INDEPENDENT SET

---

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

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

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

$n$  = 1398

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

$$\bar{X}_{Tjk} - \bar{X}_{Sjk} > 0 \quad \text{in 973 cases and}$$

$$\bar{X}_{Tjk} - \bar{X}_{Sjk} < 0 \quad \text{in 425 cases.}$$

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

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


---

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 14.655.

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

---

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

$\sigma^2_{Ijk}$  = variance of monthly expenditure estimate for the  $j^{\text{th}}$  commodity (1,...,257) in the  $k^{\text{th}}$  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.

TABLE 4.6

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

---

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

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

$\bar{X}_{Ijk}$  = mean expenditure for the  $j^{\text{th}}$  commodity (1,...,257) for the  $k^{\text{th}}$  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

---

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

$\sigma^2_{Ijk}$  = variance of expenditure estimates for the  $j^{\text{th}}$  commodity (1,...,257) in the  $k^{\text{th}}$  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 research 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 data suggested 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 inaccessibility 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, their 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. What can cause serious problems in the reliability of the data is if this difference in receptivity is not random but based on specific population characteristics such as income, education, type of employment, or ethnic group. In survey design this is known as the problem of self-selection.

This same type of difference in population characteristics mentioned above 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.

This 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 more difficult to travel to. 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.



## CHAPTER 5

### ANALYSIS OF AGGREGATED COMMODITY GROUPS

#### 5.1 Data Preparation

The analysis using non-parametric tests compared mean monthly expenditure estimates of different interview frequency for a highly disaggregated set of commodities. However, for many research and planning purposes 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,

TABLE 5.1  
AGGREGATED COMMODITY GROUPS

1. Rice	9. Sugar
2. Grains	10. Fresh Fish
3. Cassava and Other Root Crops	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

contains all the possible food items 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 making comparisons between expenditure estimates based on one and two interviews per month, the C-2 questionnaire was not relevant.

The research hypothesis to be tested was that the annual mean expenditure estimates based on two interviews per month were equal to those based on the one-interview subset. The alternative hypothesis was that the means were 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 they have it. The cumulative frequency is also given. What this table shows is that 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 a household possesses valid data 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 months 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 were equal. This hypothesis was tested for each of the 16 commodity groups using the correlated t-test. The alternative hypothesis was that the means were 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 difference between the means was determined to be

TABLE 5.3  
RESULTS OF COMPARISON OF MEAN ANNUAL ESTIMATES

Ho: $\bar{X}_{TAj} = \bar{X}_{SAj}$		$\bar{X}_{TA}$ = two interview mean annual commodity expenditure estimate				
Ha: $\bar{X}_{TAj} \neq \bar{X}_{SAj}$		$\bar{X}_{SA}$ = one-interview subset mean annual commodity expenditure estimate				
n = 104		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

significant. Thus, the research hypothesis 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 doing 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 occurrence 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 to .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

<u>Probability Range</u>	<u>Frequency</u>	<u>Expected Frequency Under Ho</u>
.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
	<u>16</u>	<u>16.0</u>

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 oils, two 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 estimates 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 research hypothesis that

the means of the two sets were equal could not 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

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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 annual commodity estimates for 16 commodity groups, the expenditure estimates based on the one-interview subset were on average 5 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 4 percent higher than that of the two-interview subset.

Research findings up to this point have provided some, though rather weak, statistical evidence which refutes the research 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.

## CHAPTER 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 expenditures 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 using 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 were examined. Only the data contained in the one-interview subset were used in this analysis. This data set consisted of household expenditures gathered on four consecutive days during one interview in a month. Each household included in the sample had a reference period which included a 1st, 2nd, 3rd, and 4th day of recall. The 16 commodity groups described in Chapter 5 were used for this analysis. Mean expenditure totals for each of the days of recall for each of the 16 commodity groups were computed. All 14 months of data were used.

The purpose of this analysis was to observe the estimates of mean expenditures generated by the different days of recall to detect significant differences in their behavior. Assuming 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 were 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 research hypothesis to be tested first was that the mean expenditure estimates of the four days of recall were equal. The alternative hypothesis was that not all the expenditure estimates of the four days of

recall were equal. Hotelling's  $T^2$  statistic was 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 there is a statistically significant difference between expenditure estimates obtained from four succeeding days of recall.

Unfortunately, these statistics do not reveal any information about the relationship between the individual days of recall. For our purposes more specific information was needed on the behavior characteristics of different days of recall. To obtain this a simple comparison of expenditure means was made between each of the days of recall. A count was 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 was 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 was that the three-day average expenditure means equaled those generated by the first day of recall. To make this a stronger test a one-tailed alternative hypothesis was used which stated that the expenditure means of the first day of recall were greater than those of the second, third and fourth days of recall combined.

TABLE 6.1  
TEST STATISTICS FOR EQUALITY OF MEANS OF THE FOUR INDIVIDUAL  
DAYS OF RECALL FROM THE FIRST INTERVIEW

Ho: $\bar{X}_{Aj} = \bar{X}_{Bj} = \bar{X}_{Cj} = \bar{X}_{Dj}$	$\bar{X}_A$ = mean expenditure based on the first day of recall from the first interview
Ha: not all the $\bar{X}$ 's are equal	$\bar{X}_B$ = mean expenditure based on the second day of recall from the first interview
n = 1784	$\bar{X}_C$ = 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)
	</

\*NS = not significant at the .05 level

S = significant at the .05 level

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

<u>Frequency</u>		<u>Frequency</u>		<u>Frequency</u>		<u>Frequency</u>	
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 was rejected in 8 out of 16 cases. This analysis provided strong statistical evidence that the mean expenditure estimate derived from the first day of recall was significantly different from the average of the other three days at the .05 level of significance.

The results also indicated that the observed difference was generally in one direction. In 15 out of 16 cases the mean expenditures of the first day of recall were higher than those based on the average of the second, third and fourth days of recall. On average the former tended to be roughly 121 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 was between 260 and 550 percent. When these three outlying observations were excluded, the average difference fell to 58 percent.

TABLE 6.3  
COMPARISON OF FIRST DAY OF RECALL WITH THE AVERAGE OF THE  
SECOND, THIRD AND FOURTH DAY OF RECALL

$H_0: \bar{X}_{A_j} = \bar{X}_{T_j}$ $H_a: \bar{X}_{A_j} > \bar{X}_{T_j}$ $n = 1787$					
$\bar{X}_A = \text{expenditure mean based on the first day of recall}$ $\bar{X}_T = \text{expenditure mean based on the second, third, and fourth day of recall}$ $j = \text{commodity (1,...,16)}$					
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 Projects	.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

To ascertain further whether or not it was 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 included. Thus, the research hypothesis tested was that the mean expenditure estimates based on the second, third and fourth day of recall were equal.

Table 6.4 provides the statistical results of this second test of the original research hypothesis. The research hypothesis was 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 occurring within one interview. The hypothesis that the larger one-day recall means resulted from 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



TABLE 6.4  
COMPARISON OF MEAN EXPENDITURES FROM THE SECOND,  
THIRD AND FOURTH DAY OF RECALL

Ho: $\bar{X}_{Bj} = \bar{X}_{Cj} = \bar{X}_{Dj}$		$\bar{X}_B$ = mean expenditure of second day of recall			
Ha: not all the $\bar{X}$ 's are equal		$\bar{X}_C$ = mean expenditure of third day of recall			
n = 1787		$\bar{X}_D$ = mean expenditure of fourth day of recall			
Commodity		T <sup>2</sup>	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

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.

#### 6.4 Differences in Expenditure Estimates Between the First and Second Interview

The results of the tests performed in Chapters 4 and 5 provided evidence that the one-interview subset generated higher, though not necessarily statistically different, expenditure estimates than the two-interview set. Because these two sets also represented 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. Based on the evidence provided by the parametric and non-parametric tests, it seemed evident that expenditure records from the first interview were 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 occurrence.

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. In order that the sample of households used had 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 facilitated the generation of 16 annual commodity expenditure estimates. Once these were obtained the correlated t-test was used to test the research hypothesis that the means from the paired sets were the same.

This hypothesis was first tested 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 as the first day of recall is believed to represent the most accurate recall. Memory of expenditures is freshest in a one-day recall.

The test indicates that there was not a significant difference for any of the 16 commodity groups at the .05 level of significance. There was, however, as Table 6.5 reveals, a tendency for the expenditure estimates from the first interview to be larger than those of the second interview. In 9 cases out of 16 the first interview estimates were larger than those of the second interview. On average they were 57 percent larger. If the Other Grains commodity category was excluded because of the extreme difference between the two estimates the first interview estimate still would be 13 percent higher.

TABLE 6.5  
RESULTS OF COMPARISON OF ANNUAL EXPENDITURE ESTIMATES FROM THE FIRST AND SECOND INTERVIEW  
BASED ON THE FIRST DAY OF RECALL

Ho: $\bar{X}_{Aj} = \bar{X}_{Bj}$		$\bar{X}_A$ = first interview one day recall mean annual expenditure estimate			
Ha: $\bar{X}_{Aj} \neq \bar{X}_{Bj}$		$\bar{X}_B$ = second interview one day recall mean annual expenditure estimate			
n = 104		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



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 were larger than those based on the second interview in 14 out of 16 cases, and in percentage terms they were approximately 31 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 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 were larger than those of the second interview.

While these results indicate that the expenditure estimates of the first interview were 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 explanation may lie in 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

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: $\bar{X}_{Cj} = \bar{X}_{Dj}$		$\bar{X}_{Cj}$ = first interview, average of the second and third day of recall mean annual expenditure estimate			
Ha: $\bar{X}_{Cj} \neq \bar{X}_{Dj}$		$\bar{X}_{Dj}$ = second interview, average of the second and third day of recall mean annual expenditure estimate			
n = 104		j = commodity (1,...,16)			
Commodity	$\bar{X}_C$ (Leofes)	$\bar{X}_D$ (Leofes)	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

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), for the telescoping of purchases to 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 take 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 is that they are not mutually exclusive. It is theoretically possible to observe both effects occurring in the data at the same time. As their existences both lead to the same results, that is 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 done by Neter and Waksberg (1964) analyzing expenditure records from bounded and unbounded recall periods, evidence of both telescoping and conditioning were discovered. In their study the authors compared reports of alteration and repair expenditures by households 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 telescoping effect increased with the size of the alteration or repair job. This is consistent with the evidence on telescoping which indicates that it 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. They found this to be 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 another study done by Turner (1961), itemized records of expenditures were kept by households for 14 days. Expenditure records from the first week were then compared with expenditure records from the second week. An analysis of the two sets of expenditure estimates 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. 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 were 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.

## CHAPTER 7

### CONCLUSION

#### 7.1 Summary of Research Findings

The purpose of this analysis was 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 obtained in this analysis have not conclusively supported, in a statistical sense, the research hypothesis that the mean expenditure estimates derived from data collected in one interview per month and two interviews per month were 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 revealed a tendency for the one-interview subset to generate monthly and annual expenditure estimates which were higher than those based on two interviews per month. On the average, the expenditure estimates of the former were found to be approximately 5 percent higher than those of the latter.

In analyzing the mean expenditure estimates generated by the four different days of recall, it was found that the means from the first day of

recall were 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 was found to be significant at the .05 level in 8 out of 16 cases. The expenditure estimates of the first day of recall were found to be 121 percent higher than those based on the sum of the second, third and fourth days of recall. This analysis provided 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 and those from each of the second and third days of recall, expenditure estimates based on the first interview were considerably larger than those of the latter. In percentage terms expenditure estimates from the first day of recall were approximately 57 percent larger and the expenditure estimates from the sum of the second and third day of recall were 31 percent larger. This difference was 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 remember accurately 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 our 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 over the possible influences of telescoping and conditioning. Attempts should be made to the extent possible to control for these effects. To reduce the amount of telescoping, comparisons can be made of expenditure reports of successive interviews checking 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, pre-testing 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. Over-extending 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 lacking adequate staff a simpler survey design might actually generate more accurate results.

Researchers involved 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	<u>Cereal Grains</u>	048	Plums
	001 Clean Parboiled Rice	049	<u>Other Crops</u>
	002 Rough Rice		050 Benniseed
	003 Husk Rice		051 Ginger
	004 Maize	052	<u>Fresh Fish</u>
	005 Sorghum or Guinea Corn		053 Fresh Bonga
	006 Millet		054 Fresh Skate
	007 Fundi		055 Fresh Spanish
008	<u>Root Crops</u>		056 Fresh Whiting
	009 Cassava		057 Fresh Catfish
	010 Yams		058 Fresh Snapper
	011 Cocoyams		059 Fresh Awefue
	012 Sweet Potatoes		060 Fresh Mackerel
	013 Chinese Yams		061 Fresh Lady
014	<u>Leguminous Products</u>		062 Fresh Mullet
	015 Groundnuts		063 Fresh Other Salt Water Fish
	016 Black-eyed Beans		064 Fresh Water Fish
	017 Green Beans	065	<u>Dried Fish</u>
	018 Broad Beans		066 Dried Bonga
	019 Pigeon Peas		067 Dried Skate
	020 Soya Beans		068 Dried Spanish
021	<u>Vegetables</u>		069 Dried Catfish
	022 Onions		070 Dried Snapper
	023 Okra		071 Dried Awefue
	024 Carrots		072 Dried Mackerel
	025 Cabbage		073 Dried Lady
	026 Egg Plants		074 Dried Mullet
	027 Greens (Plasas)		075 Dried Other Salt Water Fish
	028 Jackatoes		076 Dried Fresh Water Fish
	029 Pumpkins	077	<u>Frozen or Iced Fish</u>
	030 Tomatoes	078	<u>Tinned Fish</u>
	031 Watermelons	079	<u>Meat</u>
	032 Cucumbers		080 Fresh Beef
033	<u>Fruits</u>		081 Dried Beef
	034 Oranges		082 Pork
	035 Pineapples		083 Poultry
	036 Bananas		084 Goat
	037 Plantains		085 Sheep
	038 Mangoes		086 Bush Meat
	039 Coconuts	087	<u>Other Livestock Products</u>
	040 Paw Paws		088 Fresh Milk
	041 Grapefruit		089 Fullah Butter
	042 Tangerines (Lemons)		090 Eggs
	043 Sweet Limes	091	<u>Oils and Fats</u>
	044 Avocados		092 Palm Oil
	045 Lemons (Limes)		093 Nut Oil
	046 Guava		094 Groundnut Oil
	047 Bredfruit		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 Fees
- 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.

Basically the same 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 example 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:

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

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 B, 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 were 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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