

This PDF is a selection from an out-of-print volume from the National Bureau of Economic Research

Volume Title: The Flow-of Funds Approach to Social Accounting: Appraisals, Analysis, and Applications

Volume Author/Editor: Conference on Research in Income and Wealth

Volume Publisher: Princeton University Press

Volume ISBN: 0-870-14182-1

Volume URL: <http://www.nber.org/books/unkn62-1>

Publication Date: 1962

Chapter Title: Consumer Surveys as a Source of Information for Social Accounting: The Problems

Chapter Author: Arthur L. Broida

Chapter URL: <http://www.nber.org/chapters/c2042>

Chapter pages in book: (p. 335 - 381)

Consumer Surveys as a Source of Information for Social Accounting: The Problems

ARTHUR L. BROIDA

BOARD OF GOVERNORS OF THE FEDERAL RESERVE SYSTEM

SINCE there are various kinds of consumer surveys as well as social accounts it may be useful at the outset to indicate the scope of this discussion. This paper is concerned with household surveys as a source of data for the flow-of-funds and national income accounts, as they are presently constructed and as they might be modified and extended to better serve the needs of economic analysis. Broadly speaking, the subject matter of such surveys would be drawn from the two areas of consumer assets and debt and income and expenditure.

The first part of the paper summarizes the present and potential contribution to the accounts of information reported by consumers, and the reasons why the potential has not been fully realized. The burden of this part is that the social accountants rely primarily on business and government sources for their consumer sector estimates, partly because of the paucity of data available from consumers. The main potential contribution of consumer surveys, however, is not in providing an alternative source for present estimates, but in providing a unique route to useful breakdowns of present figures. What inhibits the development of surveys for this purpose is that consumers are a high-cost source of information of a quality that is apparently often poor. But the evidence on quality consists mainly of comparisons of dollar aggregates estimated from consumer surveys with those derived from business sources and is thus not necessarily relevant to the real issue: whether consumer surveys are an efficient means for developing distributions of the aggregates.

The remaining three parts of the paper are concerned with sources and patterns of error in dollar figures obtained in consumer surveys and with their implications for various formulations that fall under the general heading of distributions. Part II catalogues some of the main error sources. For simplicity, the discussion in this part is confined to the effects on survey means and, hence, estimates of

NOTE: The views expressed are the author's, and do not necessarily reflect those of the Board of Governors or of other members of the Board's staff.

aggregates; and for perspective, the impact of each error source on consumer surveys is contrasted with the impact of the same type of error on inquiries of business.

Part III posits a number of simple, hypothetical error patterns that might result from particular sources, and examines the implications of each for various useful kinds of distributions. The message of this part is that the problem is extremely complex, and that sweeping generalizations about purposes for which surveys are efficient are apt to be suspect, if not unwarranted. Part IV presents some findings suggestive of the error patterns in a survey of new-car buyers conducted by the Federal Reserve in the summer of 1956.

As indicated by its title, this paper is concerned with problems. In his related paper Robert Ferber considers the research needed to increase the efficiency of consumer surveys as a source of data for social accounting, and the prospects for success. The concentration here on problems should not be taken to imply the view that data must be perfect to be useful, or the belief that analytical questions can be resolved by pointing to imperfections in figures.

I. Present and Potential Contributions of Consumer Surveys to the Social Accounts

It is a fact of life to social accountants that the great bulk of the data available for their use originates in reports of businesses and governments, with relatively little forthcoming from consumers. In developing consumer sector estimates they rely mainly on the circumstance that transactions generally involve two parties, and base their figures on the reports of those with whom consumers deal. This procedure has consequences for the forms in which consumer sector data can be shown, particularly that of enforcing a high level of aggregation in the consumer accounts.

Some figures originating in consumer reports are used in flow of funds and national income. Perhaps the outstanding instance is in connection with size distributions of income, which are based on annual figures from the Census Bureau's Current Population Survey, the Federal Reserve-Michigan Survey of Consumer Finances, and that mandatory survey conducted by the Internal Revenue Service on Form 1040. Other instances tend to be more peripheral. Some represent cases where there is no alternative source—for example, where both parties to a transaction are consumers. A brief review of the main data sources now employed will indicate the contributions made by consumer reports.

SOURCES OF DATA FOR CONSUMER SECTOR ESTIMATES

Among the various forms of personal income, wage and salary payments, which account for about two-thirds of the aggregate, are estimated in the main from private employer reports of disbursements under the federal social security program and from reports of government at various levels. Dividend income is estimated from data on dividend payments and receipts in corporate income tax returns; and transfer payments, largely from reports of governments. Interest income, on the other hand, is estimated in a process that involves building up totals for monetary interest paid by all private sectors, and includes some use of consumer sources. For example, consumer mortgage interest payments are benchmarked on the 1950 Survey of Residential Housing. Consumer sources are also employed in connection with estimates of personal net rental income from nonfarm property, which utilize data from individual income tax returns; and of imputed rent from owner-occupied dwellings, the estimation of which involves figures from the Census of Housing, the 1950 Bureau of Labor Statistics Survey of Consumer Expenditures, and the Survey of Consumer Finances. Farmers are relied on for data on farm income (via the Census of Agriculture and current reporting programs of the Department of Agriculture) and businessmen for data on income from unincorporated enterprises (via income tax returns).

Consumer expenditure figures are benchmarked in large part on business census and survey data, via the commodity-flow technique, with current estimates developed mainly from retail sales data. Consumer reports play some role in estimating expenditures; thus, estimates of space rent are benchmarked on data from censuses of population and housing, and the allocation of expenditures on automobiles between consumers and businesses employs data from surveys of the Bureau of Public Roads.

Consumer reports provide benchmarks for certain capital account items. For example, estimates of residential mortgages owned by consumers are based on the 1950 Survey of Residential Financing. Housing census data are also useful in the allocation of residential construction and of mortgage debt between owner-occupied and tenant-occupied dwellings. The data underlying estimates of residential construction (building permits) and of mortgage debt outstanding (lender records) provide no basis for this allocation. Estimates of additions and improvements to residential real estate are benchmarked on the Bureau of Labor Statistics 1950 Survey of Consumer Expenditures and the Survey of Consumer Finances of that year;

SOURCES OF INFORMATION

and current estimates make some use of data from later Consumer Finances Surveys.

In the main, however, consumer assets and debts are estimated from reports of other parties on their liabilities and assets. Consumer holdings and transactions in securities are derived as residuals, by deducting holdings of banks, insurance companies, and other corporations and financial institutions from totals for the whole economy. Demand deposits, as reported by banks, are distributed among the consumer and other sectors largely on the basis of information provided by banks in the Federal Reserve Demand Deposit Ownership Survey. Consumer holdings of savings deposits and shares are estimated from reports of banks, savings and loan associations, and credit unions. Because of various legal limitations on ownership, the bulk of the liabilities of these institutions is taken to represent consumer assets. Almost the entire amount of assets reported as acquired by life insurance companies and pension funds is taken to represent consumer saving in these forms.

The estimates of short- and intermediate-term debt of consumers are based largely on figures reported by lenders. An exception is debt to doctors, dentists, and hospitals; the Survey of Consumer Finances provides annual benchmarks for such medical debt in the monthly consumer credit statistics, and by this route the Survey figures enter the accounts. Some of the other types of debt, for which lender reports are used, can be readily identified with the consumer sector because of the nature of the lending activity—for example, loans made under the small loan laws and installment credit extended to purchase consumer durable goods. In other cases, such as commercial bank loans to “individuals,” it is necessary to estimate the proportions involving consumers as distinct from businesses.

POTENTIAL CONTRIBUTIONS OF CONSUMER SURVEYS

If consumer censuses and surveys of the types noted above were taken more frequently or were broadened in scope, the social accountants undoubtedly would find the added material useful in preparing their estimates. Such surveys could help resolve some problems encountered in developing estimates at present levels of aggregation, including cases where there is difficulty in distributing figures among consumer and other sectors. But most of the business and government data presently used is available to the social accountants without collection cost, and for this as well as other reasons, it would not be particularly efficient to expand consumer survey programs for this purpose alone.

The main potential contribution of consumer surveys to the social accounts lies elsewhere—in providing the basis for breaking down

many of the monolithic aggregates now shown in various significant ways. A number of useful kinds of detail could be developed for the consumer sector that are not feasible so long as nonconsumers are the main data source. These may be grouped into two broad categories: first, additional size distributions, which involve distributions of consumer units (however defined—families, spending units, saving units) according to the size of specified dollar variables; and, second, refinements in sectoring, which involve distributions of dollars among specified subgroups of the population.

The value for analysis of having size distributions that are consistent both conceptually and statistically with the aggregate figures shown in the accounts is suggested by the usefulness of such figures for income, the one variable for which they are now available. Similar distributions would be desirable for other variables—saving, financial asset holdings, mortgage debt, and other debt, to name a few.

The prerequisite for size distributions is that the various elements composing the variable must be available concurrently for the individual unit, so that they can be summed to a unit total. To develop a size distribution of family income, for example, it is necessary to know how much income was received by individual families from various sources. While each income inflow may represent an outflow by a nonconsumer, and hence be reportable by him, the integration of inflows to an individual family can be performed only with the assistance of the family itself. Thus, aggregate consumer income can be estimated from nonconsumer sources, but income size distributions must be based on consumer reports. A similar situation holds with respect to size distributions for other variables, from broad constructions with many elements, such as saving, to narrowly defined items, such as demand deposit holdings. On the latter, bank reports can provide the basis for a size distribution of accounts but not of consumer units by size of their demand deposit holdings, since individual units may hold several such accounts.

With respect to sectoring, consumer surveys are essential for certain desirable refinements in present procedures, and would facilitate others. Perhaps most important, they would provide the means for subsectoring the household accounts along various significant economic and demographic lines, possibly with a number of alternative breakdowns shown concurrently. The broadest variety of purposes would be served by subsectors defined in terms of income of the consumer unit. Expenditures data (in greater or less detail) by income level would permit Engel-curve analyses, and amounts of income by income level would provide the basis for Lorenz-curve

studies. Analogous constructions would be useful for data on saving and on assets and debt.

Breakdowns of the household accounts by variables other than income would also have important if perhaps more limited applications. Among the possible principles of classification are those by level of saving; by the size and composition of the consumer unit and its housing status; and by various characteristics of the head of the unit, such as age and occupation.

For all of these, the prerequisite is the same as for size distributions—an integration of diverse elements at the level of the consumer unit—and hence, consumer surveys. The fact that surveys are a unique route to such detail is not only the source of their main potential contribution to social accounting, but is also the main justification for such surveys as have been taken. It is perhaps unnecessary to dwell on the advantages of data in this form. An outstanding characteristic of the economy is diversity in situation and experience, and information on the nature and extent of diversity is fundamental to an understanding of economic developments. Aggregate figures are an oversimplification in that they net out all diversity; they can mystify the sophisticated and mislead others. Separate figures for groups of consumer units in different circumstances, as a regular feature of the accounts, would illuminate and explain the sector aggregates and make the accounts more useful in almost any application. They would make the accounts serviceable for the many policy questions that turn on incidence among groups rather than on totals for all consumers. And they would make the accounts more flexible by permitting analysis of individual groups that are of interest, and by facilitating special combinations of groups appropriate to particular purposes.

One much-debated problem in sectoring concerns the treatment of entrepreneurial units—those with an interest in an unincorporated business. Data problems are particularly great in this area, and a good deal of the debate is concerned with the feasibility of particular treatments. Some of the debate involves questions of fact: To what extent do entrepreneurs make distinctions in their accounting between personal and business affairs? To what extent are their business decisions affected by their personal financial situation—whether or not the accounts are separate—and vice versa?

Consumer surveys could make a contribution to these problems, although by themselves they probably would be inadequate to resolve them. Personal interviews with the people concerned clearly would increase the feasibility of segregating entrepreneurial from other units. They would facilitate various intragroup breakdowns that

might be wanted, such as a classification of units as between those with a partnership or sole proprietorship interest, and a classification of transactions of individual units as between business and personal, to the extent that the units themselves make such distinctions. Personal interviews could also help to provide answers to the questions of how entrepreneurs do in fact act.

The problem with consumer surveys as a medium for such interviews is that unless the sample is very large, the number of entrepreneurs included is likely to be too small for firm answers to many questions. A survey directed specifically at entrepreneurs—taken, for example, by sampling places of business rather than households—would be a more efficient method of obtaining information on entrepreneurs per se. The consumer survey would nevertheless be invaluable for placing the entrepreneurs in the context of the full population and, in the absence of special entrepreneurial surveys, could provide useful insights on a small sample basis.

THE LIMITATIONS OF CONSUMER SURVEYS

The immediate explanation of the failure to use consumer reports more extensively in the social accounts, as noted earlier, is that the social accountants have not had much choice in the matter. The paucity of surveys is not the whole explanation, however, for the social accountants have not found ways of using relevant information that has been collected regularly.

Perhaps the outstanding example is the failure to use data from the Survey of Consumer Finances in the flow-of-funds accounts. The specific content of this Survey has varied over the past fourteen years, but it usually has included information on liquid assets held by consumers and on personal and mortgage debt owed—both highly relevant to flow of funds—as well as on income, expenditures for houses, cars and other durables, demographic variables, and various other subjects. In 1947–51, when an attempt was made to estimate saving on a spending unit basis, these data were supplemented by information on changes in balance sheet items and on certain specific transactions relevant to saving estimates, such as real estate transfers, life insurance premiums and benefits, and gifts and inheritances.

These Survey estimates have been almost totally neglected in the flow-of-funds accounts despite the fact that, by and large, they are in usable conceptual form, and despite the fact that the two groups of analysts concerned are located in the same corridor of the Federal Reserve building. Among the reasons is a judgment that the quality of the Survey figures is not adequate for the purposes of the accounts. This judgment is based mainly on the differences between estimates

from the Survey and from independent sources at the one level at which the two bodies of figures are commensurable: the aggregate level.

Similar aggregative comparisons have been made at various times and places for many variables from different consumer surveys.¹ The results have varied, and so have the interpretations; for often only rough adjustments could be made for conceptual differences and for deficiencies in the independent figures. The comparisons involve the kinds of complex and uncertain judgments that characterize so many methodological issues in social accounting.

On the whole it seems clear, however, that aggregates estimated from consumer surveys drastically understate certain asset items, such as demand deposits, savings deposits, and corporate and government securities; and certain debt items, such as installment debt, single-payment bank loans, and loans from brokers and security dealers. The degree of understatement for the different asset and debt items ranges from one-quarter to one-half or more. With respect to income, there apparently are understatements of interest and dividends received; and with respect to expenditures, of outlays on liquor and tobacco. However, other forms of income and expenditure, as well as the volume of mortgage debt, characteristically are not understated in consumer surveys, or at least not to the same extent.

While some survey estimates of total consumer saving have checked out reasonably well with independent estimates, the agreement conceals offsetting discrepancies in components. For example, the results of the Surveys of Consumer Finances taken early in the years 1947-51 implied a net liquidation of liquid assets (demand deposits, savings accounts and shares, and United States savings bonds) over each year. The aggregate liquidation indicated for the four-year period amounted to more than \$20 billion. Independent

¹ See, for example: (a) Irwin Friend and Vito Natrella, *Individuals' Saving*, New York, pp. 56-83; (b) Irwin Friend and Stanley Schor, "Who Saves?," *Review of Economics and Statistics*, May 1959, Part 2; (c) Raymond W. Goldsmith, Dorothy S. Brady, and Horst Mendershausen, *A Study of Saving in the United States*, Princeton, 1956, Vol. III, pp. 158-179; (d) Reports of Federal Reserve Consultant Committees on Economic Statistics, Hearings before the Subcommittee on Economic Statistics of the Joint Committee on the Economic Report, 84th Cong., 1st sess., 1955, pp. 275-293; (e) "The Financial Position of Consumers," *Federal Reserve Bulletin*, September 1958, pp. 1041-1051; (f) "Consumer Indebtedness," *Federal Reserve Bulletin*, July 1956, pp. 696-701; (g) "The 1955 Savings Survey," *Bulletin of the Oxford University Institute of Statistics*, May 1958, pp. 119-129; (h) Helen H. Lamale, *Methodology of the Survey of Consumer Expenditures in 1950*, Philadelphia, 1959, pp. 113-136; (i) Franco Modigliani and Albert K. Ando, "The 'Permanent Income' and the 'Life Cycle' Hypothesis of Saving Behavior: Comparison and Tests," *Proceedings of the Conference on Consumption and Saving*, Philadelphia, 1960, Vol. II, pp. 49-174.

data, although not entirely comparable with Survey figures, suggested that on balance consumers were accumulating such assets in this period. On the other hand, Survey estimates of saving through unincorporated businesses were substantially in excess of independent estimates.

IMPLICATIONS OF ERRORS IN AGGREGATES

Insofar as evidence for gross understatement in survey aggregates exists (and is accepted), it raises substantial questions about the serviceability of consumer reports for their main application—illuminating differences among consumer units. If a survey aggregate, or more particularly, a survey mean, is seriously in error, the set of individually reported figures on which it is based fails in some respects to represent the population from which it is drawn. To develop other statistics from the same set of unrepresentative figures obviously entails risks.

But it does not necessarily follow from gross understatement of a particular mean that the set of underlying figures is not useful for other purposes. The key question is how the error in the mean arises, and what "patterns" it follows. It is possible, given certain patterns of error, for a set of reports to yield a poor estimate of the mean and an excellent estimate of a particular type of distribution. By the same token a reliable mean does not necessarily imply accurate distributions; and in any case the *possibility* that a set of reports may yield serviceable distributions should not be confused with the *conclusion* that this is the case.

The rest of this paper is concerned with these questions. The various sources of error in surveys are discussed in part II, primarily in relation to means and aggregates. Error patterns, and their implications for various types of distributions, are considered subsequently.

II. Sources of Error in Consumer Surveys

Sources of error in surveys can be classified in various ways. The classification that will be used here is as follows: (1) factors relating to the sampling distribution, including questions of skewness as well as variance; (2) factors associated with noncontacts, one type of nonresponse; and (3) factors affecting the ability and willingness of respondents to cooperate, which are associated with other types of nonresponse as well as with response errors.

These various sources of error affect business inquiries as well as consumer surveys with differences of degree rather than kind. For

SOURCES OF INFORMATION

the sake of perspective, it is useful to consider the impact of each error source on dollar aggregates estimated from consumer surveys relative to the effect on business surveys yielding equivalent estimates. The business inquiries that will be discussed in this connection are hypothetical surveys of those businesses directly involved in each type of consumer transaction; they do not necessarily correspond to the actual data sources of the social accounts, which depend in large part on the happenstance of present data collection programs. The subject of costs will intrude repeatedly, for the impact of most problems encountered in surveys can be reduced by additional outlay.

The question considered, then, will be the relative efficiency of consumer and business sources for the various aggregates now defined in the consumer sector accounts. If aggregates were the only objective sought, undoubtedly businesses would usually be the more efficient source, particularly in view of the large body of data from them already available to the social accountants. The discussion is cast in terms of aggregates not because this needs to be demonstrated anew, but to help isolate and evaluate the problems of consumer surveys, preparatory to discussing their implications for the kinds of distributional data which such surveys alone can yield.

One other qualification seems in order, relating to the difficulties of generalization. The list of detailed variables subsumed under the broad headings of assets, debt, income, and expenditure is very long. Since particular problems impinge quite differently on different variables, an overriding source of difficulty for one variable may be of negligible importance for another. Moreover, the alternative business sources are themselves highly varied, and the importance of individual problems differs among kinds of businesses as well as by subject matter. At best one can only suggest how the effect of particular factors might vary from case to case, and offer qualified generalizations. Some of the variations by subject matter are noted in connection with each of the factors reviewed below. Variations by type of business are discussed briefly in a following section.

FACTORS RELATING TO THE SAMPLING DISTRIBUTION

Consumers and businesses are linked to each other by dollar flows in both directions, and by asset-debt, or "fund," relations. To obtain information on the aggregate dollar amount involved in any flow or fund relation, either consumers or the businesses linked to consumers in that relation may be queried.²

² Provided various possible problems are waived, such as that businesses may not be able to determine accurately which of their flow or fund relations involve consumers as distinct from other groups; that there may be differences in the timing of registration of

In effect, underlying the consumer sector aggregate for each variable are two alternative distributions, differing in the units into which the aggregate is decomposed: the distribution of consumers by the size of the variable for each; and the distribution of those businesses concerned with the particular flow or fund relation, also by the size of the variable for each. Either of these distributions might be sampled.

The relative efficiency of the two approaches is affected, among other ways, by the shapes of the two distributions. The greater the population variance, the greater the variance of the sampling distribution for samples of specified size; or, to put it another way, the larger the sample required to achieve any specified level of sampling error. The greater the degree of skewness in the population distribution, the more likely it is that with samples of moderate size the sampling distribution itself will be skewed. A skewed sampling distribution is one in which, for a majority of repetitive samples of specified size, the sample mean will fall to one side of the population mean.

Characteristically, the *number* of businesses in the relevant business population will be smaller than the *number* of units in the consumer population, with the average business accounting for a larger proportion of the aggregate than the average consumer unit. This is not necessarily indicative of the relative sample sizes required for given sampling errors, unless the business population is quite small, or unless a few identifiable businesses are known to account for a very large proportion of the aggregate. In the extreme case there may be so few businesses involved that a complete census, or something close to it, may be feasible. Thus, with respect to holdings of government savings bonds, there is only one "business"—the Treasury Department—to be canvassed. There are only fifty issuers of state bonds, a relatively small population of stock brokers, and larger but still moderately sized populations of commercial banks and savings banks, sales finance companies, and life insurance companies. In such cases smaller samples would ordinarily suffice if the inquiries were directed to businesses rather than consumers.

Skewness in the population distribution is often a serious problem in consumer surveys. For many variables the consumer distributions are extremely skewed; they are characterized by high frequencies in the zero class and long tails to the right, with the relatively small number of cases out in the tail accounting for an important part of

individual flows at the two ends; that some of the relations of consumers may be with other consumers, rather than with businesses; and that some variables, such as inventories of tangible assets, may not involve any relations with businesses. The implications of the first two of these problems are discussed at a later point.

the aggregate. For example, only about 10 per cent of consumer units hold corporate stock, and a small proportion of stockholders account for the bulk of the dollar amount. Dividend income is correspondingly concentrated. Marketable bonds of all types—federal, state and local, and corporate—are probably held by fewer than 2 per cent of consumer units. Liquid assets and mortgage debt are less concentrated—about one-quarter of all units hold savings bonds, one-half have checking accounts and savings accounts, and one-third have mortgage debt—but even for such variables a substantial proportion of the aggregate is accounted for by a minority of those concerned.

The problem in sampling highly skewed distributions is that randomly drawn samples of moderate size are likely to include none of the rare units who individually account for large amounts. As a result, the sample mean would usually be understated. (In the course of many repetitive samples such rare “tail” units would be caught occasionally, and when this occurred the sample mean would probably be much too high, offsetting the larger number of cases in which it was too low.) Skewness alone may account for a substantial part of the apparent understatement of consumer survey means for many variables.

While business populations also may often be highly skewed, the problem usually can be dealt with more efficiently than in the case of consumers. The main method of dealing with skewness is to sample certain population groups at a higher rate than others; specifically, to “oversample” those groups considered most likely to contain units in the tail of the distribution.³ The efficiency of this procedure turns on the ability of the sampler to specify in advance the groups in which “tail” units are most likely to be found. In business surveys the sampler often can draw on published balance sheets, returns in business censuses and other surveys, and similar data. In consumer surveys he must usually rely on less efficient sources: block rent statistics, inspections of the exteriors of homes, or the expensive “double-sampling” procedure—interviewing a large sample on a few questions and using the replies to draw an appropriate subsample for the main questioning.

FACTORS ASSOCIATED WITH NONCONTACTS

Noncontacts include various types of failures to communicate with intended respondents. In mail surveys letters may be misdirected, lost, or unopened. In personal interview surveys the premises may be

³ When different population groups are sampled at different rates, the returns are weighted inversely to the sampling rates to avoid bias in the results.

dark, or the intended respondent not accessible for other reasons. Like other types of nonresponse, noncontacts can damage the representativeness of a sample and lead to erroneous results. And, as with most other sources of error, the extent of the damage can be reduced by additional outlay, such as by making repeated call-backs in a personal interview survey.

Establishing contact with businesses is apt to be much less difficult than with consumers. Businesses usually observe working hours, open their mail, expect visitors, and maintain facilities for receiving them. Consumers, on the other hand, are frequently away from home for the month or the week or the evening; they are often engaged in activities which prevent their considering the request of an interviewer; and they may have defenses of privacy in such forms as apartment house receptionists. For such reasons, a higher rate of noncontact—or a higher cost of achieving any specified rate of contact—is likely to be encountered in approaching consumers than businesses.

FACTORS AFFECTING WILLINGNESS AND ABILITY TO COOPERATE

Establishing a line of communication with intended respondents is only a first step; the success of any survey depends on both the willingness and the ability of the individuals reached to answer questions, and, preferably, to answer them accurately. The extent to which nonresponse and erroneous response are avoided turns on various factors: the respondents' attitude toward the inquiry, their understanding of the questions asked, their knowledgeability with respect to the subject matter, the circumstances under which communication occurs, and so forth. Again, the size of the survey budget is involved, since it determines the quality of technical skill that can be brought to bear and the extent to which special efforts can be made to minimize particular problems.

Businesses, by and large, are likely to rank ahead of consumers in both ability and willingness to cooperate, although the margin may be greater with respect to ability than willingness. Before discussing the underlying reasons it may be noted that the greater ease of surveying businesses frequently results in lower costs, insofar as the survey can be made by mail rather than personal interview.

With few exceptions, personal interviews are a *sine qua non* for obtaining detailed financial information from consumers at large. The personal interview provides an opportunity to win cooperation and facilitate communication which, in view of some of the problems to be discussed, cannot ordinarily be forgone without devastating effect on rate and quality of response. The much higher cost of

personal interviews, together with the larger samples often needed in the consumer approach, can lead to tremendous differences in costs.

Attitude

What happens after a line of communication is established—after the letter is opened and read, or the interviewer's introductory speech has been heard—depends in good part on the attitude of the respondent. Questionnaires make demands on people, and people react in various ways.

Businesses traditionally have been vocal in resenting questionnaires, but over recent decades they have become increasingly accustomed to supplying information, particularly to the government. Most businesses are subject to some mandatory reporting programs, and are included in sample surveys from time to time. Most usually have little doubt about the authenticity of legitimate inquiries and few worries about the danger of disclosure to their disadvantage. Thus, while individual businesses may refuse to reply to individual questionnaires, the majority generally are predisposed to go along, or at least to hear the arguments for doing so.

Consumers, too, have been subjected to an increasing volume of interrogation in recent years by market research organizations, political pollsters, foundation and university researchers, and the government. Americans are probably as ready as the nationals of any country to answer questions. But their receptivity to questioning in general may not always extend to economic surveys. To many people their personal financial affairs are highly private, to be disclosed only when there is good reason for doing so. Ordinarily, they have experienced questioning on financial subjects only in connection with acts or objectives of their own. And a great many are suspicious—often with good reason—of persuasive strangers who knock at their doors.

Consider what happens: A person who describes himself as an interviewer appears and offers a brief explanation of his visit. Usually he would have been preceded by a letter from the organization conducting the survey offering similar explanations and providing assurance of confidentiality. Once inside, the interviewer asks (along with other questions) how much money the respondent has in the bank.

The interviewer's explanations plus any advance letters add up to relatively slight preparation for a demand as unusual as this. What convincing proof has the respondent that the inquiry is legitimate? Why should he risk the disclosure of his bank balance to a stranger? What is perhaps most surprising is how frequently people do answer the question.

The extent to which questions may evoke suspicion and fear varies with the subject matter. Holdings of financial assets, including savings and checking accounts and securities, are probably more sensitive than other economic subjects. Income apparently is a less sensitive subject, and questions on debt also seem to be less troublesome—except perhaps among that diminishing group who associate debt with improvidence. Little problem is posed by questions on ownership of nonfinancial assets, such as homes and durables, and on most categories of purchases, with the exceptions of liquor and tobacco.

Negative reactions to questions considered overly personal take several forms. They may result in outright refusal to grant or continue with the interview. Among the more courteous and the less self-assured, they may result in semicooperation, in grudging response to those questions that seem “safe,” and nonresponse or fallacious response to more delicate questions. In any event, they damage the usefulness of the results.

Other attitudes toward the interview may also affect the quality of responses. Some people do not take the proceeding seriously; they make hasty or ill-considered responses, or answer haphazardly. Some may want to impress, overstating or understating particular figures depending on their own standards or their guess as to the interviewer's. Some may want to avoid disclosing the truth to their wives or other persons who happen to be present. Such difficulties can be reduced by properly calculated effort, but this, of course, implies higher costs.

Businesses, on the whole, are likely to be more matter-of-fact than consumers about a statistical inquiry. Ordinarily, they would have less motive for deliberate misstatement or suppression of figures, except perhaps where they mistakenly associate the questions with some form of government investigation or regulation.

Understanding of Questions

The problem of the respondent's understanding of the questions—a prerequisite to a useful reply—plagues all survey work. In a survey involving a large number of respondents, where standardized questions are used, it is important that the sequence and wording of the questions be shaped to the frame of reference and vocabulary of the great bulk of respondents. This is frequently easier to do in the case of businesses; familiarity with the subject matter and understanding of relevant technical distinctions is apt to vary much less among business employees than among consumers.

One can refer to “checking accounts” instead of “demand deposits” and increase the number of consumers who will comprehend; one

can ask, "How much do you have in your account as of today?" instead of "What is your present balance?" and do likewise. But inevitably some people will fail to distinguish between checking and savings accounts. Some will not hear the question right, or will assume the interviewer really has a different question in mind, and report a minimum balance, or a customary balance, or the balance they anticipate after they deposit the pay check in their pocket. Many people are unable to distinguish between savings accounts in banks and shares in savings and loan associations, and will misreport their particular holdings. The problems of getting accurate distinctions among various forms of consumer credit, particularly that of segregating charge accounts from installment debt, have always been serious, and with the recent flowering of variously labeled new forms of consumer financing have become more so. With respect to some of the less common types of financial assets, those who own them can be expected to be more familiar with the appropriate technical vocabulary than the bulk of the population, but the ignorance of nonholders may pose a problem; not understanding the question, they may incorrectly report ownership.

Problems of communication can be reduced by pretesting questionnaires extensively, and by employing only the most skilled interviewers and training them thoroughly. Such efforts, of course, raise costs; and interviewers can help untangle misunderstandings only to the extent that they are aware of them.

Knowledgeability

The knowledgeability of respondents has two levels: what they "know" with the particular effort they are willing to make in responding to an inquiry, and what they can find out with a reasonable additional effort. The range, which may be sizable, depends on how strongly the respondent is motivated to cooperate. The matter of written records—their existence, quality, and accessibility—may often be the ultimate determinant of knowledgeability, for responses from memory will generally be subject to larger margins of error than quotations from records.

Businesses have greater need for records than consumers, and greater incentives to keep them current and accurate; in responding to a statistical inquiry they usually would consult records as a matter of course. Consumers not only have less need for records, but experience indicates that it is difficult to get them to consult what records they have. All too often the respondent remains sofa-bound, answering questions from memory, despite the interviewer's best efforts. These efforts ordinarily must be gentle; pressure can

easily antagonize the respondent and destroy whatever rapport has been established.

But even when consumers are willing to consult records, the gain may be less than desired. For example, the respondent's checkbook may not reflect recent checks written, or deposits made, by his wife—or indeed, himself. And an exact quotation from an accurate record may still result in a sizable error, if the family has a second account which the respondent forgets. Such oversights can occur easily in connection with most variables; the respondent can forget some elements of his family's income, assets, debt, or expenditure, and may not even be aware of some that accrue to other members of the family.

The fact that businesses usually consult records does not imply that their responses always correspond with the information sought. There can be considerable variation in the form and content of records of different companies in one line of business, and the information that can be developed from them may deviate substantially from what the investigator would like to know. As only one example, the variety of accounting periods employed often makes for great difficulty. Similarly, there are differences among businesses in the extent to which they will bother to work up information that is not immediately available. Again, the difference between businesses and consumers is one of degree, with the problems characteristically more intense for consumers.

But knowledgeability has other aspects, and the advantages in these connections can be markedly on the side of consumers. One concerns the timing of registration of flows; and another, the problem of distinguishing flow or fund relations involving consumers from those involving others.

The timing problem exists because flows often are in transit for some period. The outflow from the paying group may differ from the inflow of the receiving group in the same period because of fluctuations in the amount in transit; and the records of the two parties may differ on a particular date by the amount in transit. Thus, since aggregates are "dated," the two parties to the relation are not completely substitutable as sources of information. The preferred reporting group is the one that can supply data consistent with the definitions adopted in the accounts, and this may well be consumers rather than businesses.

Insofar as businesses deal with nonconsumers as well as consumers, the usefulness of their reports for developing consumer sector data turns on the possibility of distinguishing the transactions that are with consumers. Businesses that deal with both consumers

and others can be of greater or less help in this regard, depending on how much they know about their transactors. In any case, consumer surveys often would provide a much firmer basis for estimation.

Demand deposit estimates may be used to illustrate. Banks participating in the Demand Deposit Ownership Survey (which provides the main basis for sectoring deposits in flow of funds) are asked to classify each reported deposit by ownership category. In order to apply the instructions given them for classification, they need certain information about the account owner and his use of the account in question. Often the name of the depositor would be enough, but sometimes not. Some unincorporated businessmen, for example, may segregate their various accounts as between personal and business in practice, but not in the identification they attach to the accounts. Farmers cannot be identified by name, and often not even from the additional information that they engage in farming; appropriate classification may turn on primary activity. Nonprofit-organization accounts may be listed in the name of an officer rather than that of the organization, and so forth.

In many cases where the name is insufficient for appropriate classification the bank would have the necessary additional information in its files. But, like many consumers, banks may fail to use all the information available to them, and may classify by name alone.

It is clear that a personal interview with the account owner would provide a more reliable basis for identifying consumer accounts. There would be no need to depend on the happenstance of a bank's knowledge; the personal interview questionnaire could be designed to elicit the relevant information, and appropriate classifications could be made for each reported account in the process of editing the questionnaires. The potential contribution of consumer surveys to accurate sectoring is even greater for certain other variables, particularly those where consumer sector figures are now estimated as residuals or by other involved procedures.

Circumstances of the Interview

Although this list of factors affecting the quality of survey information is by no means exhaustive, only one more of importance in personal interviews will be noted: the fact that the circumstances of the interview may not be conducive to accurate communication. In a consumer survey the interviewer enters into a conversation with the respondent in his home, under whatever circumstances may prevail at the moment. The interviewer's job is to explain the purpose of the inquiry, allay any doubts the respondent may express or imply by his actions, ask the questions—and in the process, untangle any

misunderstandings that arise—and record the responses. This is a large order, not easy to do even when the interview is conducted in quiet surroundings, without interruption or distraction.

Not all consumer interviews are of this sort, however. Some go on over the glare of television set, on which the respondent may keep one eye during the whole conversation. During others there may be curious relatives, neighbors, children, and animals in and out of the room, and perhaps in and out of the conversation. Telephones ring; babies cry; children quarrel; everything happens that happens in homes. And they all have effects on quality of responses. The surroundings in which communication occurs in business surveys may often be far from ideal, but business offices in general might tend to provide a more “businesslike” atmosphere for personal interviews than homes; and mail inquiries afford greater flexibility in picking a convenient time for responding.

SOME VARIATIONS BY TYPE OF BUSINESS

Any broad comparisons such as the foregoing between consumers and businesses are necessarily oversimplified. With respect to most of the factors discussed, a great variety of circumstances exists among different groups of businesses.

Some of this variation may be summarized in terms of a continuum: Governments and large corporations would be at the top, as the group usually posing fewest problems; other corporations, on a decreasing scale of size, would fall in the middle; and small corporations and unincorporated businesses would be at the bottom. The relative efficiency of consumers and businesses as a source of data on some variable would depend in part on how high up on the continuum lies the particular set of businesses concerned.

For businesses at the bottom—say small, unincorporated retail stores or service establishments—the intensity of various problems may approach that of consumer surveys. The noncontact rate may be high for small establishments from which the owner is often absent, and for one-man operations based on the owner’s home. A mail inquiry may be ruled out because of problems of cooperation and communication. The records kept may be primitive, and the amount of interruption and distraction during the interview may equal or exceed that experienced in many consumer interviews.

With businesses higher on the continuum, special problems may be sometimes encountered which reduce their efficiency as sources of data. Reporting problems may be met within some of the largest corporations because of the geographical spread of their activities—

and records—and the diversity of their operations. Personnel and record-keeping practices of some local governments may be inadequate for reliable reporting. Some industries and some firms are characteristically reluctant to cooperate in surveys. For such reasons, the differences between consumer and business surveys may often be less than implied by the preceding discussion.

III. Error Patterns and Their Implications for Distributions

It is not hard to see why the means of dollar variables derived from consumer surveys are more often too low than too high. Of the various factors affecting accuracy discussed in part II, the majority would usually lead to understatements.

Thus, skewness in the population distribution may lead to underrepresentation in the sample of the units that lie out in the tail. Since for most economic variables the tail extends to the right, the sample means would tend to be too low rather than too high. Non-contacts may have the same consequence, for frequently it is the high-income people who are least accessible to interviewers—they vacation and travel more than others, and they can afford defenses of privacy. High-income people tend to be in the upper parts of most dollar distributions, including those of assets, most types of expenditure, and many categories of debt.

Various problems relating to the behavior of respondents in the interview also may tend to produce understatements. The extent to which people are reluctant to disclose financial assets or income or are embarrassed with respect to debt may be associated with the dollar amounts involved; and refusals to respond, or deliberate misstatements, may be more frequent for large amounts than small. And respondents are more likely to forget some items of income, assets, or debt than to remember some they do not have.

Of course, not all factors lead to understatement. Some types of low-income people, including the young, and single individuals of all ages, may not be reached in proper numbers because they spend much time away from home or because their dwelling units, located in odd and unexpected places, are overlooked. The boastful may exaggerate. Respondents who attempt to make honest estimates of dollar figures may err in either direction, as may those who misunderstand the question. Education is associated with income, and through income with other variables, and the better educated may be generally less suspicious and fearful, easier to communicate with, and more knowledgeable. This would produce a tendency toward a higher rate of useful reply in the upper than the lower parts of the distributions.

On the other hand, the education factor may sometimes work in the other direction, for even in a voluntary survey some poorly educated people may cooperate because of fear of the consequences of refusal.

It is clear that observed errors in means, including understatements as well as the less frequent overstatements, cannot ordinarily be expected to have simple explanations. The observed error is the net of all factors affecting accuracy, and an error of a given size and direction can come about through many different combinations of sources. But the implications that a given error in the mean has for the accuracy of various kinds of distributions depend entirely on the particular sources of error and how they are related to the basis of distribution. This is the problem of the "pattern" of error.

It may be useful to postulate a number of simple error patterns and examine the implications of each for different kinds of distributions. As noted in part I, the latter might be broadly grouped into size distributions, which involves counting consumer units for whom the dollar variable falls in specified ranges, and finer sectoring of the aggregate, which involves summing dollars for units falling in specified subgroups of the population.

For the sake of generality, a few other types of statistics will be included in the discussion. The derivation of "quantile points"—the median, and deciles, quintiles, and percentiles—is analogous to the development of size distributions in that it also involves counting units: a size array for the variable is counted down to determine the dollar values separating groups of equal number. And both size distributions and quantile points may frequently be wanted for population subgroups, for purposes of comparison.

In finer sectoring of a dollar aggregate, subgroups might be distinguished in terms either of the same variable as is summed (e.g. the Lorenz curve) or of a different variable. The term "Lorenz curve" will be used specifically to refer to a distribution of dollars of some variable according to quantile rank on the same variable (leading to such statements as "the X per cent with lowest incomes receive Y per cent of aggregate income"). The term "dollar shares" will be used to refer to distributions of dollars of some variable according to classes of another variable (leading to such statements as "those with incomes under X dollars, or those in the age range A to B, hold Y per cent of aggregate assets"). Finally, the mean itself, which is analogous to these statistics in the sense that it also involves summing dollars, will be considered. This list of potentially useful formulations of data is by no means complete, but it is sufficient for illustrating the differential impact of different patterns of error.

SOURCES OF INFORMATION

THE CASE OF THE "MISSING" CASES

Consider some dollar variable, such as financial asset holdings of consumers, and assume that as a result of a sample survey a set of figures is in hand for a number of consumer units. The hypothetical questionnaire covered other subjects also, so with each report of financial assets is associated information on income and other dollar variables, and on age, occupation, and other attributes.

Such a sample might often underrepresent holders of large amounts of financial assets for various reasons discussed above: skewness in the population distribution, the lesser accessibility of large holders, a higher rate of refusal to reply to questions on financial assets in cases of large holdings, and so forth. All of these factors, of course, are matters of degree, but we might postulate circumstances in which only a small fraction of intended respondents is affected. Assume that a sample of 5,000 reports is in hand which, if it had been expanded by five "missing" reports of amounts equal to the highest of those actually received, would represent the parent population with fidelity.

These circumstances ordinarily would have a negligible effect on the results of most counting operations. If very fine classes were used in the size distribution of financial asset holdings the relative frequency in the top class might be perceptibly affected, and so might be the value of the ninety-ninth percentile, but not much else. There might be a somewhat greater effect on comparative size distributions and quantiles for, say, different income groups, since it is likely that most of the missing large-asset holders would fall in the highest income class. But the results would rarely lead to mistaken inferences with the small fraction of reports postulated as missing, or with the fractions often likely to be missing in practice for the reasons specified.

Results of all dollar summing operations, however, would be affected, the degree depending on how large the five missing reports were in the particular case. The mean would be too low and the Lorenz curve would be insufficiently concave because of the omission of a large number of dollars in the highest class. All dollar-share calculations would be subject to error, depending on whether the missing reports tended to be concentrated in certain of the subgroups distinguished.

PATTERNS OF RESPONSE ERROR

Now assume that a sample of reports on financial assets is in hand in which neither large holders nor any other group is underrepresented, i.e. the sample would be faithful to the parent population if each respondent supplied accurate information. However, each response is subject to error; the amount reported is equal to the algebraic

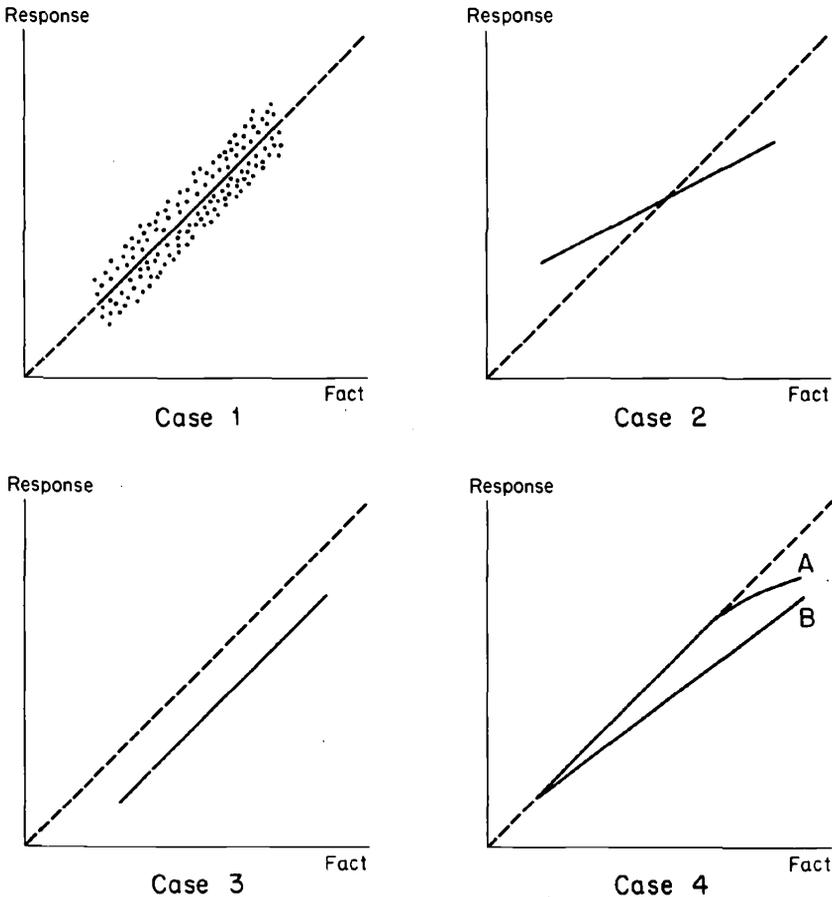
CONSUMER SURVEYS AS A SOURCE

sum of the unknown "true" amount, or "fact," and an error term. Different patterns of response error might then be distinguished according to whether (1) the mean error differs from zero, and (2) the error is correlated with the "true" amount. Four, more or less plausible, patterns will be discussed.

Case 1: Mean Error Zero, Error not Correlated with Fact

The first panel of Chart 1 shows a scatter diagram relating responses to facts, in which the responses are assumed to reflect this pattern of

CHART 1
Hypothetical Error Patterns



Note: Regression line in Case 1 and dashed lines in other cases have 45° slope.

error. As noted in the chart, the hypothetical least-squares regression line drawn through the scatter has a slope of 45° .

An error of this type can be described as random. It might be specified somewhat more exactly as follows: When any "fact" in the population is reported, associated with it is a response error drawn randomly from an error distribution which is symmetrical about a zero mean. The chart illustrates the case in which the error distribution is symmetrical in absolute terms; an alternative hypothesis would involve symmetry in percentage terms. Algebraically, if y is the response, x the fact, and e the error, a response subject to random absolute error is defined as $y = x + e$, and one subject to random percentage error as $y = x + ex$, where e is statistically independent of x and distributed symmetrically about zero.

Neither random absolute nor random percentage errors would bias the mean; the expected value of the mean of responses in a sample would be the mean of facts in the population. However, with a random absolute error, the variance of the population distribution of responses would equal the sum of the variances of the distributions of fact and error. With a random percentage error the variance would also be increased, although not in a simple additive fashion. In either case, the shape of the population distribution of responses would necessarily be different from that of facts, since the two have the same mean but different variances. Consequently, the expected size distribution in a sample of responses would differ from that in a sample of facts. In other words, even if the error is random, the size distribution of a sample of responses is a biased estimate of the size distribution of facts.

The nature and magnitude of the bias depend on whether the errors are random in the absolute or in the percentage sense; on the shape of the population distribution; and on the variance of the error distribution relative to that of the distribution of facts. For example, if there are random absolute errors and the population of facts is distributed symmetrically, the distribution of the population of responses (and the expected sample distribution) would be extended at both tails and lowered in the center—to a degree dependent on the size of error variance. If the population of facts is skewed, a random absolute error would lead to a lesser degree of skewness in the population of responses.⁴ On the other hand, if there are random

⁴ As customarily measured, skewness depends on the relationship between the third and second moments about the mean—specifically, on the ratio of the square of the third moment to the cube of the second moment. The third moment of the distribution of e , given symmetry in the absolute sense, is zero, and given independence of e and x , e makes no contribution to the third moment of the distribution of $e + x$. It does increase the second moment, however, thus reducing the ratio.

percentage errors and the distribution of facts is symmetrical, the distribution of responses would be skewed to the right.⁵ If the population of fact is itself skewed to the right, a random percentage error would accentuate the skewness, and conversely with left-skewed populations.⁶

Random errors may bias other statistics also. A random absolute error would not bias the median of a symmetrical distribution, but as a consequence of the increased variance of the distribution, it would lead to overstatements of the higher quantile points and understatements of the lower. A random percentage error would lead to understatement of the median, whatever the shape of the distribution, as well as to bias in other quantile points. Similarly, as a result of increased variance, a random absolute error would result in a Lorenz curve that overstated the degree of concavity. A random percentage error would have the same tendency, to a greater degree if the mean error is large, perhaps to a lesser degree if it is small.

Dollar-share calculations would not be biased by either type of random error, assuming the variable used for classification into subgroups is itself free of error, because the subgroup means, like the over-all mean, would be unbiased. However, inferences drawn from comparisons of size distributions and quantile points among subgroups would be biased by both types of random error, to the extent that any differences in distributions of fact among subgroups led to a differential effect of the errors.

The possibility needs also to be considered that the variable used for classification into subgroups is affected by random error. For example, assume it is desired to compare the shares of assets held by those with incomes of \$6,000 and \$7,000, respectively. If the reports on income are subject to random error, the expected true incomes of those who report incomes of each of these amounts would differ from

⁵ Using the measure of skewness mentioned in the preceding note, the numerator, which is zero for a symmetrically distributed x , is positive for $y = x + ex$ (with x and e independent), indicating positive skewness for the distribution of y . An intuitive demonstration might be given as follows: By assumption, all values are subject to percentage errors in a specified range, with equal probability of percentage errors of specified size in either direction. Any given percentage error upward in high figures (i.e. those above the mean) would move the figures farther to the right, on an absolute scale, than equivalent percentage errors downward in low figures would move the figures to the left. Conversely, downward percentage errors in high figures would move them farther to the left than equivalent upward percentage errors in low figures would move them to the right. The net effect would be to extend the tail more to the right than to the left, and, as noted later in the text, to lower the median.

⁶ These results would not necessarily hold for distributions that include negative as well as positive values—e.g. saving. If, by chance, the population of fact is distributed symmetrically about zero, the effect of a random percentage error on values above the zero mean would be mirrored in the effect on those below.

the respective amounts.⁷ The groups for which assets are compared would thus be different from those intended. The particular nature of the bias produced would depend on the shapes of the two distributions of fact as well as the two distributions of error. In general, however, a random error in a classification variable would lead to understatement of differences among subgroups.⁸

Case 2: Mean Error Zero, Error Correlated with Fact

The second panel of the chart shows a regression line relating responses to facts for the case where low amounts are overstated and high amounts understated. (No underlying scatter diagram is shown because all observations are assumed to fall on the regression line; for simplicity, the possibility of an additional random error is excluded.) This error pattern might be approached, for example, if respondents tended to report their customary holdings of financial assets rather than their actual holdings at the time for which they were requested. Or it might arise from a predilection among respondents to appear "average"; if both large and small holders were embarrassed by their differences from the norm they might tend to minimize them in reporting.

With these tendencies the mean might well be accurate, but any confidence consequently placed in the quality of distributions would be mistaken. The variance of the population of responses would be less than that of facts; sample size distributions would tend to be too peaked, and quantile points, too closely bunched toward the middle. The Lorenz curve would be insufficiently concave.⁹ Differences

⁷ With a random error distinguishing the population of responses from that of facts, the average reported income of all those with a true income of say, \$6,000, would be expected to be \$6,000; i.e. the regression of responses on facts has a zero intercept and a unitary slope. However, as long as there is any error (i.e. any scatter about the regression of responses on facts) the slope of the regression of facts on responses would be less than unity; the expected value of true income of those reporting incomes below the mean would be higher than the reported value, and for those reporting incomes above the mean, lower.

⁸ Some of the implications of absolute and percentage random errors were discussed at an earlier meeting of this Conference by Hyman B. Kaitz [see *An Appraisal of the 1950 Census Income Data*, Studies in Income and Wealth, Princeton (for NBER), 1958, Volume 23, pp. 277-281]. There is an extensive literature on the bias in regression lines when both the dependent and independent variables are subject to random error. For example, see Albert Madansky, "The Fitting of Straight Lines When Both Variables are Subject to Error," *Journal of the American Statistical Association*, March 1959, and the bibliography given there.

⁹ With the simple error pattern assumed, the ranking of cases would be unaffected, but those nearer the low end of the distribution would account for relatively larger proportions of the total of responses than of facts, and those nearer the high end, smaller proportions. With the more realistic assumption of an additional random error, the net effect on the Lorenz curve could be in either direction; for, as noted earlier, a random error leads to overstatement of the degree of concavity.

among subgroups as indicated by comparisons of size distributions or by dollar-share calculations would all tend to be understated.

Case 3: Mean Error not Zero, Error not Correlated with Fact

The regression line in the third panel describes a case in which there is a constant understatement at all levels of the true amount. This pattern might be approached if in the effort to account for all elements of a variable, respondents tended to err on the side of omission and mainly reported those elements with which they were most actively concerned. In reporting financial assets, for example, they might tend to overlook second accounts, bonds and other savings of children, assets earmarked for a purpose and considered as in effect spent, and so forth. Any constancy in this tendency is probably more likely to be in percentage than absolute terms.

With this error pattern, all size distributions would be pressed to the left; all quantile points would be too low; and the mean would be understated. The Lorenz curve, however, would not be affected if the understatement was constant in percentage terms. (A constant absolute understatement would increase the concavity of the Lorenz curve.) And, again assuming a constant percentage error, the relative positions of subgroups would be correctly reflected in comparative size distributions and dollar-share calculations.

Case 4: Mean Error not Zero, Error Correlated with Fact

This pattern might arise if there was a tendency toward understatement that grew with the size of the true amount. For example, any inclination to understate assets because of the fear of risk in disclosure might well affect only those with larger holdings, and to a degree related to the size of the holding; the poor man has nothing to lose.

Two regression lines are shown in the fourth panel, illustrating two plausible variants of this pattern. In the variant labeled A, the tendency toward understatement does not appear until very high levels of the true amount are reached; in that labeled B, the tendency affects the whole distribution, with the percentage understatement rising as the true amount increases.

The effect of this error pattern depends in part on whether it takes the A or the B form. In the A variant, the errors might have little effect on size distributions and on any but the highest quantile points; in the B, the former would be pressed to the left, and the latter understated. In both variants, however, the mean would be too low, the Lorenz curve would be insufficiently concave, and dollar-share calculations would tend to understate differences among groups.

This brief discussion suggests the complexity of the issues involved in the question of whether surveys may yield adequate "distributions" in cases where the means they yield are understated—and, indeed, in cases where the means are accurate. Of the various simple error patterns postulated, some would lead to understatement of means (and hence aggregates), and some would not. In both groups, however, the implications for various other types of statistics are highly diverse. Thus, over-all size distributions and quantile points (except the median in some instances) are biased by all of the patterns discussed except that of a few "missing" large amounts and that of progressive understatement of large amounts beginning high in the distribution (variant A of Case 4). Lorenz curves are biased by all of the patterns except constant percentage understatement. Dollar-share calculations are not biased by random errors in the tabulated (as distinct from the classification) variable nor by constant percentage understatement, but are biased by the other patterns. Comparisons of size distributions for subgroups are not biased by a few missing large amounts, constant percentage understatement, and progressive understatement beginning at a high level, but are biased by the other patterns.

In practice, no one simple pattern could be expected to hold; the concurrent operation of various factors would usually lead to some weighted average of different patterns, probably including some not discussed. And additional complexities are introduced when variables are considered not separately or in simple joint relations as above, but in terms of the more complicated relations frequently involved in analytical problems.

The use of survey results to study changes over time is a case in point. Data for different times might be obtained in a single interview (e.g. for present and "year-ago" figures), in successive interviews with the same respondent (the panel study), or in successive interviews with different respondents selected by consistent methods (e.g. several Surveys of Consumer Finances). The analysis might involve repeating any of the counting or summing operations on data for the different times and contrasting the results; or it might involve deriving measures of change for individual cases and performing counting or summing operations on these. If the sources of error tend to impinge on the data for the different times in the same fashion, temporal comparisons may yield valid conclusions even when single cross sections are subject to bias. On the other hand, there are a host of factors that could lead to different error patterns for the different time periods and, thus, invalidate temporal studies even where the cross sections taken separately have valid applications.

Another case in point is provided by analyses that involve variables constructed from others. Thus, saving or net worth figures may be

derived for each respondent by combining a number of separately reported figures and subjecting them to various counting or summing operations. Since each of the component figures might have its own complicated error pattern, the question of the error pattern likely in the saving or net worth figure, and its implications for various types of distributions, may be extremely complex.

IV. Some Evidence on Error Patterns in One Survey

What error patterns are likely to predominate in practice? A few generalizations seem warranted from a consideration of the error sources themselves. Thus, for highly skewed distributions it is a safe presumption that a sample of moderate size will underrepresent the tail units unless effective methods are employed to assure against it. There is also likely to be a random error component.

Beyond this, little can be safely assumed. Evidence on the nature of "truth" is usually required to draw firm conclusions about patterns of error in particular cases, and such evidence is hard to come by. In research on consumer errors the "truth" may be sought in reports of financial institutions or other businesses with whom responding consumers deal. But where the consumer and business reports differ, there is always the possibility that the explanation lies either in some noncomparability or in an error in the business report rather than the consumer response. Moreover, when conclusions can be reached about errors in a consumer survey, they are likely to be specific to that survey. Any important variation in population covered, subject matter, or procedures followed can have consequences for error patterns, and there is almost no limit to the possible variations along these lines.

Some suggestions as to the error patterns in a survey taken in 1956 by the Federal Reserve can be obtained as a result of the plan of the study. The survey was concerned with new-car purchases in 1954-55; and for various reasons, personal interviews were sought not only with the car buyers, but, in all cases of credit purchases, also with the bank, sales finance company, or other lender advancing credit to the car buyers in the sample.¹⁰ Because lender reports are available for cases where buyers in the sample were not interviewed for

¹⁰ This survey was part of a broad study of consumer credit and was made in cooperation with National Analysts, Inc., of Philadelphia. The results were published in *Financing New Car Purchases*, Consumer Instalment Credit, Part IV, Board of Governors of the Federal Reserve System, Washington, 1957. Thanks are due to National Analysts and to Arnold J. King, President, for assistance in preparing materials for the analysis reported here. Miss Barbara E. Schriver of the Board's staff wrote some of the computer programs used for tabulating, oversaw the input, translated the output, and in general proved herself invaluable in processing the data. A brief description of the procedures used is given in the appendix to the present paper.

some reason, it is possible to illuminate the effects of nonresponse on the survey of buyers. And because the information sought from both buyer and lender was in part equivalent, it is possible to analyze the differences in figures reported by buyers and lenders for light on buyer response errors.

The rest of this paper describes some results of a study along these lines.¹¹ The analysis is concerned with four dollar variables on which equivalent information was sought from both buyer and lender: the price paid for the new car, the principal amount borrowed (i.e. the difference between the price and the downpayment), the total amount of the loan (i.e. the principal plus any added finance and insurance charges), and the amount of individual monthly payments scheduled in the loan contract. The effects of complete nonresponse to the interview and of nonresponse to individual items are considered first, followed by a discussion of patterns of response error.

Before turning to the results, some special characteristics of the car buyers survey working for and against reliability might be noted. On the negative side is that the nonresponse rate—28 per cent—was unusually high because of the exigencies of the sampling procedure. Addresses taken from new-car registrations filed in the period from January 1954 to December 1955 were given to interviewers in the summer of 1956, and it proved impossible to locate about 15 per cent of the buyers listed. For about 10 per cent of the list it was established that the buyer had moved; for the remaining 5 per cent no trace of the buyer, and often not even of the address listed, could be found. This problem would not affect surveys employing dwelling unit samples, where present residents of specified units are designated for interview. Also on the negative side is the fact that buyers interviewed were asked to report details of a transaction that occurred anywhere from six to thirty months earlier—a longer recall period than ordinarily required.

Other factors worked in favor of the quality of results. Skewness in the distributions of the variables covered is quite moderate by any standard, and is far less than in many other variables. The motivation given buyers to cooperate may have been unusually strong—the advance letter said that the study was being made “at the request of the President and interested committees of Congress.” Many people apparently take pleasure in talking about their recent new-car purchases; results of a postcard inquiry sent to a subsample of respondents after their interviews indicated that the majority enjoyed the proceeding. The information requested on price, loan amounts,

¹¹ Robert Ferber presents some additional findings of this study in his related paper prepared for this session of the Conference.

and monthly payments involved separate items for which there was no possibility of forgetting "components," as there is with respect to, say, income or assets. (Any "second loans"—i.e. to help make up the downpayment—were asked about in connection with a discussion of sources of cash paid the dealer but were not added to the primary loan.) And finally, the extended and detailed discussion in the interview of one transaction probably resulted in greater accuracy than would be obtained in an inquiry covering a broader variety of subjects.

Another special aspect of the survey is that it was confined to a particular, and not necessarily typical, population group. New-car buyers tend to have higher incomes than the average, and may differ in other characteristics related to their ability and willingness to cooperate in a survey.

EFFECTS OF COMPLETE NONRESPONSE

Nonresponse among buyers apparently acted to produce some downward bias for all four variables. This is indicated for car price by the comparisons in the first three columns of Table 1, part A, and for the other variables in parts B, C, D.

The first column of this table shows size distributions, means, and medians of figures reported by lenders for responding and nonresponding buyers combined; and the second and third columns show similar data for the two groups separately.¹² A comparison of the distributions shown in the second and third columns indicates that nonrespondents tended to fall in the higher dollar classes somewhat more often than the buyers who were interviewed, and in the lower classes less often.

As a consequence of these differences in distributions, means and medians for nonrespondents were higher than for respondents. The largest relative difference occurred in connection with monthly payments, where the mean for nonrespondents, at \$81, is nearly 10 per cent above the mean of \$74 for interviewed buyers. The differences in means for the two measures of loan amount are 7 and 8 per cent, and for car price, 5 per cent. All these differences are statistically significant, as may be determined from the sampling errors of the individual means shown in the table.¹³

¹² Lender figures are used for both groups in the comparison, rather than buyer figures for respondents and lender figures for nonrespondents, to avoid introducing the effect of difference in source.

¹³ As noted in the table, the sampling errors shown do not allow for clustering in the sample design and, therefore, are undoubtedly somewhat understated. Since the clusters were counties, which are relatively large geographic areas, the understatement is less than in the case of many area samples which employ clusters as small as city blocks. The sample design for the car buyers survey is discussed briefly in the appendix, and more fully in the appendix to the original report.

SOURCES OF INFORMATION

TABLE 1
DISTRIBUTION OF NEW-CAR PURCHASES, EFFECT OF COMPLETE NONRESPONSE^a

Amount Reported by Lender	All Cases ^b	Buyer Inter- viewed	Buyer Not Interviewed				
			All Cases	Not at Home ^c	Refused	Moved	Other ^d
A. CAR PRICE							
Less than \$2,000	5.3%	5.6%	4.5%	4.2%	7.1%	3.6%	4.7%
\$2,000-2,249	11.1	11.4	10.3	7.8	7.1	12.0	12.6
2,250-2,499	17.5	18.1	16.0	13.3	18.9	14.4	21.3
2,500-2,749	15.7	16.9	12.7	14.5	11.8	13.8	8.7
2,750-2,999	12.1	12.5	11.1	12.0	10.2	11.1	11.0
3,000-3,499	22.5	21.6	24.7	19.9	18.9	28.1	27.6
3,500-3,999	9.4	8.8	10.7	16.9	7.9	11.1	4.7
4,000-4,999	4.7	3.6	7.3	8.4	11.0	5.1	7.9
5,000 or more	1.8	1.5	2.5	3.0	7.1	0.9	1.6
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Median	\$2,760	\$2,725	\$2,879	e	e	e	e
Mean	\$2,886	\$2,846	\$2,990	\$3,071	\$3,117	\$2,935	\$2,899
Number of cases	1,655	1,188	467	106	79	202	80
Sampling error of mean (1 sigma) ^f	\$17.67	\$19.76	\$36.93	\$76.91	\$112.85	\$50.19	\$84.83
B. MONTHLY PAYMENTS							
Less than \$50	9.0%	9.7%	7.1%	9.0%	11.3%	3.8%	8.4%
\$50-59	13.7	14.5	11.5	13.8	15.6	9.1	10.5
60-69	20.9	22.4	17.0	19.1	12.8	15.7	21.7
70-74	10.6	10.5	10.8	3.7	6.4	16.5	9.8
75-79	8.9	8.8	9.2	8.5	7.8	10.7	7.7
80-89	14.9	15.6	13.2	16.5	9.2	11.8	16.1
90-99	9.4	7.9	13.4	11.2	12.1	15.7	11.9
100-124	9.1	8.0	12.0	12.2	18.4	11.0	7.7
125 or more	3.5	2.5	6.0	5.9	6.4	5.8	6.3
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Median	\$73	\$70	\$76	e	e	e	e
Mean	\$76	\$74	\$81	\$80	\$81	\$83	\$79
Number of cases	1,814	1,303	511	117	87	219	88
Sampling error of mean (1 sigma) ^f	\$0.65	\$0.73	\$1.33	\$2.88	\$3.35	\$2.01	\$2.90

(continued)

TABLE 1 (concluded)

Amount Reported by Lender	All Cases ^b	Buyer Inter- viewed	Buyer Not Interviewed				
			All Cases	Not at Home ^c	Refused	Moved	Other ^d
C. TOTAL LOAN							
Less than \$1,000	6.0%	6.4%	5.0%	6.1%	7.5%	3.9%	4.0%
\$1,000-1,249	7.0	7.7	5.1	8.7	6.1	3.1	4.7
1,250-1,499	9.0	9.0	8.9	10.7	9.5	7.8	8.7
1,500-1,749	11.5	12.2	9.7	8.7	13.6	8.3	10.7
1,750-1,999	13.9	13.8	14.0	16.3	10.9	13.8	14.8
2,000-2,249	16.4	16.6	15.9	8.7	12.2	21.4	14.8
2,250-2,499	12.2	12.9	10.4	8.7	7.5	11.2	13.4
2,500-2,999	16.6	15.2	20.1	21.9	15.6	20.6	20.8
3,000 or more	7.5	6.1	10.8	10.2	17.0	9.9	8.1
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Median	\$2,040	\$2,010	\$2,124	e	e	e	e
Mean	\$2,038	\$1,994	\$2,149	\$2,080	\$2,143	\$2,189	\$2,144
Number of cases	1,918	1,379	539	124	91	232	92
Sampling error of mean (1 sigma) ^f	\$15.83	\$18.17	\$31.26	\$67.78	\$94.10	\$42.66	\$70.08
D. LOAN PRINCIPAL							
Less than \$1,000	7.7%	8.0%	6.9%	11.1%	9.7%	4.6%	4.8%
\$1,000-1,249	12.3	12.8	11.3	13.2	14.6	8.9	11.6
1,250-1,499	13.3	14.0	11.6	12.6	13.9	10.5	10.9
1,500-1,749	20.2	20.4	20.0	15.3	13.2	24.5	21.1
1,750-1,999	14.9	16.3	11.5	7.4	9.0	14.8	10.9
2,000-2,249	13.6	12.9	15.3	15.8	11.1	16.2	16.3
2,250-2,499	8.9	8.1	11.1	8.9	13.2	10.5	12.9
2,500-2,999	6.5	5.8	8.3	12.1	7.6	6.7	8.2
3,000 or more	2.5	1.8	4.1	3.7	7.6	3.2	3.4
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Median	\$1,700	\$1,689	\$1,755	e	e	e	e
Mean	\$1,735	\$1,700	\$1,820	\$1,773	\$1,823	\$1,834	\$1,845
Number of cases	1,836	1,312	524	120	88	225	91
Sampling error of mean (1 sigma) ^f	\$13.56	\$15.55	\$26.85	\$58.61	\$80.99	\$35.97	\$62.21

SOURCE: Data in this and all following tables based on special tabulations of returns in 1956 Federal Reserve survey of new-car purchases made in 1954-55. Details may not add to totals because of rounding.

^a Individual cases weighted to compensate for variations in sampling rate by months.

^b Includes new-car purchases involving credit for which lender was identified through lien record and supplied information. See appendix for explanation of coverage.

^c Buyer not found at home after four calls in built-up areas, three in open country.

^d No trace of buyer found at address given interviewer, buyer ill, deceased, unable to speak English, etc.

^e Not available.

^f Calculated from variance of cases included in column, without allowance for effects of clustering in sample design.

SOURCES OF INFORMATION

The net effect of nonresponse in a survey depends, of course, on the nonresponse rate as well as on the amount of difference between the nonresponding and responding groups. As noted earlier, nonresponse in the car buyers survey was at the high rate of 28 per cent.¹⁴ The net effect on means, as indicated by the figures in the first two columns, ranged from a 3 per cent understatement for monthly payments—\$74 rather than \$76—down to about a 1 per cent understatement for car price. In surveys marked by lower nonresponse rates the net effect on means would be less, as long as the means for nonrespondents did not differ more than in this case from those for respondents. The qualification, of course, is crucial; even a relatively low nonresponse rate could have a more disturbing effect on means than found here, given large enough differences between the two groups.

The remaining columns in the table show figures for nonrespondents classified by reason for lack of interview. The means for nonrespondents in each of the four categories distinguished are higher than those for interviewed buyers for all of the variables, although in some cases the differences are not significant at the 5 per cent level. As indicated in Table 1, part A, with respect to car price those refusing to cooperate differed most from respondents, followed by not-at-homes and then movers. For monthly payments and total loan, however, movers were the extreme group, with refusals intermediate, and not-at-homes showing less mean difference from respondents. The position of the refusing and the not-at-home groups is perhaps more interesting than that of movers, since this last category would not occur in any survey that involved a dwelling unit rather than a list sample.

These differences in means resulted from varying kinds of differences in the distributions for the several categories of nonresponse. For all four variables, those refusing were more often at the extremes of the distribution, and less often in the middle, than interviewed buyers. The differences were particularly marked at the high end. Thus, 18 per cent of the buyers refusing to be interviewed had bought cars priced at \$4,000 or more, as compared with only about 5 per cent of interviewed buyers; and about 25 per cent of those refusing had contracted for monthly payments of \$100 or more, as compared with only about 11 per cent of those responding. As reported in the original study, each of the variables analyzed is correlated with

¹⁴ Among lenders, the nonresponse was 10 per cent, of which one-third represented refusals, and two-thirds, inability to locate the relevant records. Some of the latter may have represented cases of incorrect identification of either the lender or the buyer. In other cases, the lenders reported that they regularly destroyed the detailed records of closed accounts.

income, and thus probably with education. It might be hypothesized that many refusals at the low end were related to suspicion or uncertainty about the purpose of the inquiry, and many at the high end to objections based on more knowledgeable grounds.

The patterns with respect to the other categories of nonresponse cannot be summarized so simply. For car price and monthly payments, not-at-homes, for example, fell in the upper part of the distributions more often than respondents. For loan principal, however, their pattern resembled that of refusals—higher frequencies at both extremes than among respondents—and for total loan, the pattern was mixed. Movers and the miscellaneous group generally tended to be in the upper parts of the distributions more frequently than those interviewed, although their frequencies in all cells above the middle were not uniformly higher than among respondents.

Weighting for Complete Nonresponse

In the original study, individual responses from both buyers and lenders were weighted (by card replication) to compensate for varying sampling rates by month of purchase.¹⁵ In an effort to deal with the problem of nonresponse, the weights were adjusted to allow for varying response rates among specified subgroups. The adjustment involved the assumption that the best estimate of all figures for a nonrespondent was the average of corresponding figures reported by respondents falling in same geographic region (of four distinguished), the same community-size group (of nine), and the same calendar quarter of purchase (of eight). Similar procedures have been used in a number of other surveys including the Survey of Consumer Finances, where a somewhat more elaborate technique is employed.

It is possible to test the efficacy of this procedure by the following method: Assume that the data of the first column of Table 1 represents the full population, but responses are available only for cases included in the second column. The data of the second column as well as the first are based on weighting that reflects varying sampling rates only. If the weights underlying the figures in the second column are adjusted for nonresponse, and the adjustment is a useful one, the figures should be brought closer to those for the full population as shown in the first column.

The results of such a test are given for the four variables in Table 2, which shows lender figures for responding buyers after weighting for nonresponse. A comparison of the figures in this table with those of

¹⁵ Sampling rates varied because the same number of purchases was drawn into the sample for each of the twenty-four months of the period covered, whereas aggregate purchases fluctuated widely for seasonal and other reasons.

SOURCES OF INFORMATION

TABLE 2
DISTRIBUTION OF INTERVIEWED BUYERS BY AMOUNTS REPORTED BY
LENDERS, RESULTS OF WEIGHTING FOR BUYER NONRESPONSE^a

A. CAR PRICE		B. MONTHLY PAYMENTS	
Less than \$2,000	4.8%	Less than \$50	8.7%
\$2,000-2,249	11.6	\$50-59	14.7
2,250-2,499	17.2	60-69	22.3
2,500-2,749	16.9	70-74	10.9
2,750-2,999	12.7	75-79	9.1
3,000-3,499	21.2	80-89	15.3
3,500-3,999	9.4	90-99	7.7
4,000-4,999	4.4	100-124	8.8
5,000 or more	1.8	125 or more	2.5
Total	100.0	Total	100.0
Median	\$2,740	Median	\$70
Mean	\$2,877	Mean	\$75
Number of cases	1,188	Number of cases	1,303
C. TOTAL LOAN		D. LOAN PRINCIPAL	
Less than \$1,000	5.9%	Less than \$1,000	7.7%
\$1,000-1,249	6.9	\$1,000-1,249	11.6
1,250-1,499	8.6	1,250-1,499	13.3
1,500-1,749	12.3	1,500-1,749	20.3
1,750-1,999	13.6	1,750-1,999	16.6
2,000-2,249	16.4	2,000-2,249	13.6
2,250-2,499	13.2	2,250-2,499	8.0
2,500-2,999	15.8	2,500-2,999	6.6
3,000 or more	7.3	3,000 or more	2.3
Total	100.0	Total	100.0
Median	\$2,040	Median	\$1,700
Mean	\$2,035	Mean	\$1,734
Number of cases	1,379	Number of cases	1,312

NOTE: Details may not add to totals because of rounding.

^a Data weighted to allow for varying nonresponse rates—individual cases weighted inversely to response rate in their group, defined in terms of quarter of purchase, region of country and size of community. Weights also compensate for varying sampling rate by months.

the first two columns of Table 1 indicates that the adjustment was highly useful for the means and medians, but that its value for the distributions is less clear. Practically all of the understatement resulting from nonresponse was removed in the means for the two measures of loan amount, and half or more in the cases of the two other variables. The medians were similarly improved, except in the case of monthly payments.

CONSUMER SURVEYS AS A SOURCE

With respect to distributions, the effect of weighting for non-response was to replace a pattern of understatement of frequencies in the higher dollar classes and overstatements of those in the lower classes with a pattern of overstatement of frequencies in the middle of the distribution and understatement at both extremes. This is a reasonable consequence of any procedure which assigns subgroup averages to missing cases.

It is not necessarily safe to infer from the substantial improvements in means and medians that nonresponse weighting would have a similar effect in all surveys. The efficacy of the adjustment depends on how closely the criteria used for identifying nonrespondents with particular groups of respondents (in this case, region, community size, and time of purchase) are related to the variables in question. As the substantive study found, car prices and loan terms were, in fact, correlated with the criteria used, particularly time of purchase and region. Such circumstances may not apply in all cases.

EFFECTS OF ITEM NONRESPONSE

An analysis similar to that for complete nonresponse was also made for item nonresponse—cases where buyers did not provide specific figures in an otherwise acceptable interview. Such omissions were found to have negligible effect on all of the variables studied, primarily because of their small number.

Item nonresponse rates ranged from less than 1 per cent for car price up to 7 per cent for total loan. The change in the means produced by adding data for item nonrespondents did not exceed 0.2 per cent in any case. It is likely that what item nonresponse occurred in each of the variables was the result of inability rather than unwillingness to report, since the respondents involved provided related figures.

PATTERNS OF RESPONSE ERROR

Where figures were obtained from both buyers and lenders, it is highly likely that the lender figures would be the more nearly accurate, for various reasons discussed in part II: Lenders could be expected to refer to records in the vast majority of cases; they would be more familiar than buyers with the subject matter of automobile financing and, therefore, less likely to misunderstand questions; they would be less likely to react to the inquiry with suspicion as to its purpose; they probably would be less often motivated to misstate facts for other reasons, and so forth.

As a consequence, differences between figures supplied by the lenders and buyers are suggestive with respect to errors in the latter.

SOURCES OF INFORMATION

TABLE 3

DISTRIBUTION OF NEW-CAR PURCHASES, INFORMATION REPORTED BY LENDERS AND BUYERS FOR IDENTICAL GROUP OF NEW-CAR PURCHASES^a

Amount	Lender Reports	Buyer Reports	Amount	Lender Reports	Buyer Reports
A. CAR PRICE			B. MONTHLY PAYMENTS		
Less than \$2,000	5.9%	8.1%	Less than \$50	10.1%	9.7%
\$2,000-2,249	11.2	11.4	\$50-59	16.4	15.8
2,250-2,499	17.4	16.1	60-69	21.4	22.0
2,500-2,749	17.3	16.1	70-74	10.4	9.8
2,750-2,999	13.3	13.6	75-79	8.8	8.8
3,000-3,499	20.1	20.3	80-89	14.2	14.5
3,500-3,999	9.8	9.6	90-99	7.8	7.7
4,000-4,999	3.6	3.2	100-124	8.3	8.5
5,000 or more	1.5	1.6	125 or more	2.7	3.2
Total	100.0	100.0	Total	100.0	100.0
Mean	\$2,844	\$2,808	Mean	\$74	\$74
Number of cases	1,606	1,606	Number of cases	1,757	1,757
Sampling error of mean (1 <i>sigma</i>) ^b	\$16.99	\$17.28	Sampling error of mean (1 <i>sigma</i>) ^b	\$0.64	\$0.66
C. TOTAL LOAN			D. LOAN PRINCIPAL		
Less than \$1,000	5.7%	7.1%	Less than \$1,000	8.0%	10.5%
\$1,000-1,249	8.2	9.6	\$1,000-1,249	12.8	15.9
1,250-1,499	8.9	10.4	1,250-1,499	14.7	15.8
1,500-1,749	13.0	13.7	1,500-1,749	20.6	19.9
1,750-1,999	14.8	14.7	1,750-1,999	16.4	13.6
2,000-2,249	16.4	16.2	2,000-2,249	12.7	12.2
2,250-2,499	12.0	10.4	2,250-2,499	7.5	5.8
2,500-2,999	15.1	12.8	2,500-2,999	5.5	4.5
3,000 or more	5.7	5.2	3,000 or more	1.8	1.9
Total	100.0	100.0	Total	100.0	100.0
Mean	\$1,985	\$1,906	Mean	\$1,690	\$1,607
Number of cases	1,794	1,794	Number of cases	1,785	1,785
Sampling error of mean (1 <i>sigma</i>) ^b	\$15.65	\$15.83	Sampling error of mean (1 <i>sigma</i>) ^b	\$13.21	\$13.38

NOTE: Details may not add to totals because of rounding.

^a Includes new-car purchases involving credit for which usable reports were obtained from both buyers and lenders.

^b Calculated from variance of cases included in column, without allowance for effects of clustering in sample design.

CONSUMER SURVEYS AS A SOURCE

They are no more than this, because the lender reports are also subject to error. As noted in the appendix, efforts of several types were made to limit the effect of errors in the lender reports on the analysis, but the procedures cannot be assumed to have been wholly successful.

Table 3 shows size distributions and means of figures reported by both lenders and buyers for identical groups of cases—those in which usable reports were obtained from both parties. If the lender figures are accepted as a standard of accuracy, it would appear that response errors in the buyer interviews acted to produce some downward bias in the means for three of the variables. Mean monthly payments were the same in both sets of reports; but for the two measures of loan amount, the buyer means were 4 and 5 per cent below those of lenders; and for car price, a little more than 1 per cent below. The differences in the last three cases are statistically significant, as can be determined from the sampling errors shown in the tables. Their magnitude is much less than the understatements apparently characterizing many financial variables in other surveys, probably as a

TABLE 4
DISTRIBUTION OF NEW-CAR PURCHASES, ABSOLUTE DIFFERENCES BETWEEN FIGURES REPORTED BY LENDERS AND BUYERS^a

Difference	Car Price	Total Loan	Loan Principal	Difference	Monthly Payments
Lender figure higher by			Lender figure higher by		
More than \$1	46.4%	43.9%	50.7%	More than \$1	10.4%
\$1,000 or more	0.8	1.8	1.8	\$75 or more	0.2
300-999	12.1	12.3	13.8	15-75	2.9
100-299	13.1	9.7	15.7	5-14	4.2
2-99	20.4	20.1	19.4	2-4	3.1
Difference \$1 or less	20.4	34.5	22.9	Difference \$1 or less	78.9
Lender figure lower by			Lender figure lower by		
More than \$1	33.3	21.6	26.4	More than \$1	10.7
\$2-99	14.8	11.9	11.2	\$2-4	3.2
100-299	9.6	5.9	9.3	5-14	4.1
300-999	8.2	3.1	5.5	15-75	3.2
1,000 or more	0.7	0.7	0.4	75 or more	0.2
Total	100.0	100.0	100.0	Total	100.0
Number of cases	1,606	1,794	1,785	Number of cases	1,757

NOTE: Details may not add to totals because of rounding.

^a Cases included are the same as in Table 3.

SOURCES OF INFORMATION

TABLE 5
DISTRIBUTION OF NEW-CAR PURCHASES, PERCENTAGE DIFFERENCE BETWEEN
FIGURES REPORTED BY LENDERS AND BUYERS^a

Difference	Car Price	Total Loan	Loan Principal	Monthly Payments
Lender figure higher by				
1% or more	40.4%	35.8%	47.5%	12.4%
40% or more	0.2	2.6	3.7	0.7
20-39.9	4.4	8.1	9.6	2.3
10-19.9	8.9	8.4	10.3	2.6
1-9.9	26.9	16.7	23.9	6.8
Difference less than 1%	32.6	48.0	28.5	65.5
Lender figure lower by				
1% or more	27.0	16.1	24.0	22.1
1-9.9%	16.9	9.9	12.5	16.1
10-19.9	6.2	2.6	6.0	1.9
20-39.9	3.3	2.4	4.0	2.4
40% or more	0.6	1.2	1.5	1.7
Total	100.0	100.0	100.0	100.0
Number of cases	1,606	1,794	1,785	1,757

NOTE: Details may not add to totals because of rounding.

^a Cases included are the same as in Table 3.

consequence of the balance of special factors in this survey affecting reliability.

With respect to the distributions, the two measures of loan amount show a rather consistent pattern: higher frequencies of buyer reports in the lower dollar classes, and higher frequencies of lender reports in the upper classes. A similar but less marked tendency is apparent in the distributions for car price. For monthly payments, no such pattern appears; and the differences in cell frequencies on the whole are smaller, not exceeding 0.6 of one percentage point in any cell, in contrast to differences of 2 and 3 points in some cells for the three other variables.

Differences in the distributions of lender and buyer reports represent the net effect of differences in the individual cases, which may have been offsetting to a greater or lesser extent. Tables 4 and 5 show the distributions of the individual differences themselves, the former in absolute dollar terms, and the latter in percentage

terms.¹⁶ Perhaps the most striking results relate to monthly payments: Nearly 80 per cent of the buyer reports on payments were within \$1 of the lender reports, and the cases of difference were distributed very nearly symmetrically about zero in absolute terms. This is in sharp contrast to car price, where the difference in means was only about 1 per cent. For car price, the two reports agreed within \$1 in only one-fifth of the cases, and within 1 per cent in only one-third. Moreover, the distribution of differences departs from symmetry in both absolute and percentage terms, with the lender's figure above the buyer's figure half again as often as it was below.¹⁷

Agreement within 1 per cent occurred in less than one-third of the reports of loan principal, and in about one-half of those of total loan. For these variables the lender's figure was above the buyer's figure about twice as often as it was below. Moreover, where buyer figures were lower, their average deviation from the lender figures was greater than in the cases where they were higher. With respect to total loan, for example, the mean difference (excluding \$1 differences) for cases where the buyer figure was lower was \$269, as compared with a mean difference of \$179 for cases where the buyer figure was higher. For loan principal, the corresponding figures were \$266 and \$197, respectively.

The most plausible explanation of the apparent excellence of buyer reports on monthly payments, in contrast to the results for car price and loan amounts, is that they had much better reason to be knowledgeable on the former subject. Buyers had necessarily considered the size of the monthly payment anywhere from six to thirty times in the recent past (depending on the date and maturity of their installment contracts), and the figure apparently was firmly implanted in the minds of most. On the other hand, their encounter with the car price and the loan amounts would have been mainly at the time of purchase, and might have been sketchy even then; it is commonly asserted that the buyer's key consideration in many car purchases is the size of the monthly payment, with much less attention paid to price, loan principal, or even maturity.

Various hypotheses might be offered to explain the fact that for

¹⁶ Because the absolute size of monthly payments was so much smaller than those of price and loan amounts, a given dollar difference in the reports on payments represents a much larger percentage difference than an equivalent dollar difference in the three other variables. This accounts for the somewhat different impression given by the two tables as to the relative rates of "no difference" (i.e. \$1 or less and less than 1 per cent) among the variables. It might also be noted that, in keypunching, any reported cents were dropped; so a "\$1 difference" might actually have amounted to only a few cents.

¹⁷ As noted in the appendix, errors in the lender reports may have been more frequent for car price than for the three other variables.

car price the mean of buyer figures was quite close to that of the lender figures, but for the two loan measures considerably lower. Some respondents may have felt uncomfortable about the amount of indebtedness they had incurred, and consciously or unconsciously tended to minimize the loan figures, without any similar emotional involvement with respect to price. It is also probable that the particular form and sequence of questions put to the buyer had some bearing on the differences.

The results to this point relate to patterns of difference for all cases combined. Some evidence on patterns for different levels of the variables is provided in Table 6. In this table individual cases are sorted into dollar classes according to the level of the figure reported by the lender. For all cases in each class, means of the figures reported by both the lender and the buyer are shown as well as the absolute and percentage differences between these means.

The same general pattern is found for all four variables: The means of buyer figures in the lower classes tend to exceed the lender means; and in the higher classes, to fall below lender means. This occurs even with respect to monthly payments, where on an over-all basis differences over \$1 occurred in only about one-fifth of the cases, and what differences existed were distributed quite symmetrically. If the lender figures are accepted as accurate, the implication is that for monthly payments as well as for the other variables, buyer overstatements tend to be associated with low figures, and understatements with high figures.

This conclusion, however, must be tempered by consideration of the possibility of error in the lender figures. As noted in part III, a random error in the variable used for classification—the lender report in this case—would have this consequence. The extent to which the results reflect random lender errors as opposed to a particular pattern of buyer response error remains moot.

To sum up, in the new-car buyers survey, tendencies toward understatement are apparent in most of the variables analyzed, both as a result of complete nonresponse for various reasons and as a consequence of response errors. Nonresponse had the greatest impact on the figures for monthly payments, response errors the least impact on this variable and the most on the two measures of loan amount. The magnitude of the apparent errors on the whole was relatively small, probably because of the absence of various factors that tend to produce understatements in many cases. But it is interesting that even in the absence of these factors the direction of bias was downward.

For all of the variables there is at least a suggestion that low figures

TABLE 6
MEANS OF LENDER AND BUYER FIGURES BY SIZE GROUPS OF LENDER FIGURES

Amount Reported by Lender	Mean of Lender Figures	Mean of Buyer Figures	Differences between Means		Number of Cases
			Absolute	Percentage	
CAR PRICE					
Less than \$2,000	\$1,804	\$1,870	\$+66	+3.7%	94
\$2,000-2,249	2,140	2,190	+50	+2.3	180
2,250-2,499	2,372	2,367	-5	-0.2	279
2,500-2,749	2,625	2,597	-28	-1.1	278
2,750-2,999	2,872	2,834	-38	-1.3	213
3,000-3,499	3,234	3,164	-70	-2.2	323
3,500-3,999	3,716	3,589	-127	-3.4	157
4,000-4,999	4,330	4,158	-172	-4.0	58
5,000 or more	5,415	5,325	-90	-1.7	24
All cases	2,844	2,808	-36	-1.3	1,606
MONTHLY PAYMENTS					
Less than \$50	41	43	+2	+4.9	177
\$50-59	54	56	+2	+3.7	288
60-69	64	67	+3	+4.8	376
70-74	72	71	-1	-1.4	182
75-79	77	77	0	0	155
80-89	84	84	0	0	249
90-99	94	94	0	0	137
100-124	110	108	-2	-1.8	145
125 or more	159	146	-13	-8.2	48
All cases	74	74	0	0	1,757
TOTAL LOAN					
Less than \$1,000	712	749	+37	+5.2	102
\$1,000-1,249	1,117	1,143	+26	+2.3	148
1,250-1,499	1,384	1,353	-31	+2.2	160
1,500-1,749	1,636	1,587	-49	-3.0	233
1,750-1,999	1,870	1,805	-65	-3.5	266
2,000-2,249	2,119	2,043	-76	-3.6	295
2,250-2,499	2,370	2,227	-143	-6.0	216
2,500-2,999	2,715	2,565	-150	-5.5	271
3,000 or more	3,404	3,185	-219	-6.4	103
All cases	1,985	1,906	-79	-4.0	1,794
LOAN PRINCIPAL					
Less than \$1,000	725	774	+49	+6.8	143
\$1,000-1,249	1,098	1,081	-17	-1.5	229
1,250-1,499	1,372	1,355	-17	-1.2	263
1,500-1,749	1,617	1,547	-70	-4.3	367
1,750-1,999	1,862	1,754	-108	-5.8	292
2,000-2,249	2,104	1,994	-110	-5.2	227
2,250-2,499	2,360	2,182	-178	-7.5	133
2,500-2,999	2,662	2,411	-251	-9.4	99
3,000 or more	3,377	2,900	-477	-14.1	32
All cases	1,690	1,607	-83	-4.9	1,785

SOURCES OF INFORMATION

tended to be overstated and high figures understated, but the patterns of error differed in other respects. Even within their narrow scope, the results of the study suggest the variety of error patterns that can be associated with understatements of means.

Appendix

These notes include a brief discussion of certain relevant features of the survey of new-car buyers and of procedures followed in the subsequent study of response errors and the effect of nonresponse. A more detailed description of the original survey is provided in the appendix to the report.¹⁸

THE ORIGINAL SURVEY SAMPLE

For the original study, a basic sample of about 13,000 new-car registrants in 1954-55 was selected from the records of the R. L. Polk Co. The names and addresses of about 550 new-car registrants in each of the 24 months of the period covered were drawn by random methods from lists of all registrants in 112 counties, located in 41 states. The counties themselves had been selected as a stratified two- and three-stage probability sample.

The basic sample of new-car registrants was screened in several steps to exclude about 1,000 registrations in the names of businesses and governments. Of the remaining 12,000 registrations, one-half were designated for a buyer interview—those which had been given odd numbers in a serial enumeration of the list. Lender interviews were sought for all new-car purchases in the sample of 12,000 which involved credit. Thus, buyer interviews were solicited in many cases for which a lender report was not relevant (cash purchases) and lender reports were sought in many cases where no buyer interview was solicited (credit purchases given even numbers in the serial enumeration).

CASES INCLUDED IN THE ERROR STUDY

Broadly speaking, the analysis of the effect of buyer nonresponse was concerned with cases of credit purchase designated for buyer interview, for which a usable lender report was obtained. The analysis of response error was concerned with credit purchases for which usable reports were obtained from both buyer and lender. A number of cases meeting these conditions were excluded, however, for reasons discussed in this section and the one following.

One group was excluded from the nonresponse analysis because

¹⁸ *Financing New Car Purchases* (see note 10, above).

of the way in which the lenders concerned were reached. The majority (about 70 per cent) of lenders were identified by checking the new-car titles at state motor vehicle offices for recordings of lien. This method failed in the remaining cases for two reasons: 10 of the 41 states covered do not have title laws; and lenders in the other states often failed to record a lien on the title. To provide a route to the lender in such cases, all buyers interviewed who reported using credit were asked to give the name and address of their lender. This information was used wherever no record of lien was found, but ignored in other cases.¹⁹

The analysis of the effects of nonresponse, as noted in the text, involved the comparison of lender data for those credit buyers who responded with lender data for those who did not. This analysis was confined to data from lenders who were reached through lien records. Where no lien record existed a report could have been obtained from the lender only if the buyer was numbered among the respondents, with the result that all no-lien cases in the sample would have fallen in the buyer response group if they had been included. Since the no-lien cases tended to be concentrated in certain states and perhaps among certain types of lenders, it seemed possible that their inclusion in the response group would bias the comparisons.

Lenders reached by both routes were included in the analysis of response error, which, as noted, was concerned with the cases in which both lenders and buyers responded. The response error tabulations thus include some transactions not covered by the non-response tabulations (the no-lien cases) and exclude some transactions covered by the latter (the nonrespondents).

There are some differences in the number of cases included for the different variables in each set of tabulations, because lenders sometimes failed to report on a particular item in an otherwise acceptable return. The most common omission was with respect to car price. Also, transactions involving noninstallment loans, for which the monthly payment figure is irrelevant, were included in the data for car price and loan amounts.

EFFORTS TO LIMIT ERRORS IN LENDER REPORTS

The usefulness of information reported by lenders for investigating the errors in buyer reports is, of course, affected by the accuracy of the former. Several procedures were followed to limit the effects of lender errors on the analysis, including dropping certain cases.

¹⁹ In the half of the basic sample not designated for buyer interview, if no lien record was found, a brief check was made with the buyer (often by telephone) to learn the identity of any lender involved in his new-car purchase.

One source of difference in the figures reported by buyers and lenders was that the two parties sometimes reported on different transactions. In most such cases it was concluded that the lender report was incorrect; the buyer interview covered the individual's whole recent history of car ownership, and the discussion provided an excellent opportunity to insure that the price and loan figures obtained referred to the car that had been selected in the sample.

Lenders were given a great deal of information in specifying the transaction—the name and address of the buyer; the date of the new-car registration; and the make, model and serial number of the car. Nevertheless, where the buyer had financed two new cars in succession in 1954–55 with the same lender, the latter sometimes reported on the wrong (usually the more recent) loan. In other cases, lenders apparently confused customers with similar names, or simply drew the wrong card from the file. And some of the reporting lenders who had been reached through lien records were discovered to have not been concerned in the original financing of the car, but to have recorded a lien in connection with a subsequent loan for which the car was used as collateral.

The main device used to detect cases of different transactions was to compare the date of the new-car registration with that of the loan contract as reported by the lender. All cases in which these dates were not in the same or adjacent months were dropped from the error study. This criterion undoubtedly resulted in dropping some "good" cases, because car registrations often fall several months behind actual purchases in some states, particularly during peak purchase periods.

Other problem cases, including additional reports on wrong transactions, were removed as a result of an examination of two groups of questionnaires: the 150 cases involving the largest percentage differences in reported figures; and all cases (totaling 51) in which the punched cards prepared from the buyer interview carried a cash purchase code, and those coded from the lender report indicated a credit purchase. Of the former group, 29 were dropped from the error study because of evidence that the lender had reported on the wrong transaction (even though the dates were consistent) or because the lender report was internally inconsistent.

Of the second group, thirty were dropped because the examination indicated either that a cash code was correct (implying a coding error in connection with the lender report), or that there were some special circumstances which had been interpreted differently by editors working independently on buyer and lender reports. Most of the lender report coding errors involved thirty-day credit extensions

with no interest, which had been defined as cash purchases for purposes of the study, and had been coded properly in the buyer card. The cases of "special circumstances" included purchases by the car dealers themselves and their salesmen, and also cases where lender and borrower had complicated financial dealings in which a lien on the new car figured, but not in any simple way. Of the remaining twenty-one cases in this group, ten were necessarily omitted from the analysis because the lender, while not specifically denying the loan, was unwilling or unable to supply information. The credit code in these cases had been based on the existence of a lien.

In processing questionnaires for the original study, buyer and lender reports had been independently edited, with some of the figures entered by interviewers changed on the basis of consistency checks and other internal evidence of error. On the supposition that editing increased the accuracy of the lender data (e.g. by correcting transcription errors in single figures where several related figures were transcribed properly), the lender figures used in the analyses of both response error and the effect of nonresponse were those arrived at after editing. For the analysis of buyer response error, these lender figures were compared with buyer figures *before* editing, to abstract from the effect of the latter operation on the quality of the buyer data. To minimize the effect of keypunch errors, all data were newly keypunched for the error study, with 100 per cent verification.

It may be hoped that these various procedures reduced the impact of lender report errors on the conclusions. At least, some of the more egregious cases were removed in which differences between the two reports could not be equated with errors made in the buyer interview. It cannot be assumed, of course, that all such cases were detected, nor that all of the judgments involved in editing lender reports and excluding cases from the analysis were correct.

One final comment might be made with respect to the variables included. The information available to the lender on loan amounts and monthly payments originated in his own operations, while that on car price (as well as on cash downpayment and trade-in allowance) had been reported to him by the dealer or buyer. As a consequence, the lender reports are more likely to be accurate with respect to loan terms than car price. This is particularly the case because car prices in this period often had a fictional element (offset by a corresponding fiction in the trade-in allowance), and were not always taken seriously by those concerned with the transaction. As noted earlier, many lenders reported no knowledge of car price, but provided information on the loan.

