
THE RELIABILITY
OF CONSUMER REPORTS
OF FINANCIAL ASSETS
AND DEBTS



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THE RELIABILITY OF CONSUMER REPORTS
OF FINANCIAL ASSETS AND DEBTS

Studies in Consumer Savings

- No. 1 Collecting Financial Data by Consumer Panel Techniques. *Robert Ferber*
- No. 2 An Investigation of Response Error. *John B. Lansing, Gerald P. Ginsburg, and Kaisa Braaten*
- No. 3 The Composition of Consumer Savings Portfolios. *Henry J. Claycamp*
- No. 4 Survey Reliability and Interviewer Competence. *Mathew Hauck and Stanley Steinkamp*
- No. 5 Financial Stock-Flow Relationships Among Central Illinois Farmers. *Jerry Curnutt and Robert Ferber*

Studies in Consumer Savings, No. 6

CONSUMER SAVINGS PROJECT
INTER-UNIVERSITY COMMITTEE FOR
RESEARCH ON CONSUMER BEHAVIOR

THE RELIABILITY OF CONSUMER REPORTS OF FINANCIAL ASSETS AND DEBTS

Robert Ferber

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PREFACE

This monograph is designed to summarize and synthesize the results of a series of studies conducted to investigate the accuracy of savings statistics as obtained by consumer survey techniques. It is based on research conducted by the Consumer Savings Project of the Inter-University Committee for Research on Consumer Behavior, a project financed by the Ford Foundation with supplementary assistance from the United States Department of Agriculture, the United States Department of Labor, and later from the National Science Foundation. The objectives of this project were threefold, as follows:

(1) To determine the reliability with which quantitative information on various kinds of assets and debts and on the size and composition of saving and dissaving can be collected from consumers by survey techniques.

(2) To develop procedures for obtaining these data on a current and continuing basis with a known degree of reliability.

(3) To begin to collect consumer financial data of a kind not hitherto available and of strategic importance in studying the decisions consumers make about their assets and debts, spending and saving.

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The first of these objectives is the concern of the present monograph, which reviews the findings of the various subprojects relating to the reliability with which different assets and debts were reported in the different surveys. In addition, an attempt is made to incorporate the relatively small amount of work done previously on this subject, to provide in a single source a relatively comprehensive review of present knowledge of the reliability of consumer reports of financial assets and debts. The principal exception is the 1963 reliability study carried out in conjunction with that year's Federal Reserve Survey of Family Financial Characteristics, which is covered only briefly here because the results of that study are not complete at the time of this writing.

This monograph is divided into eight chapters. A general discussion of the reliability problem as applied to consumer financial surveys is provided in Chapter I, followed in Chapter II by a review of aggregative comparisons of data collected by such surveys. Findings relating to the

reliability of individual reports of assets and debts, based largely on the studies of the Consumer Savings Project, are presented in Chapters III-VI. The reliability of reports of debt is discussed in Chapter III, that of time deposits in Chapter IV, and that of life insurance, demand deposits, and farm assets in Chapter V. The effects of a panel operation on the reliability of such reports are investigated in Chapter VI, based on two such operations which were conducted as an extension of studies covered in earlier chapters.

Means of improving the reliability of consumer survey data by straightforward survey techniques are evaluated in Chapter VII. The results of various experiments designed to test the efficiency of such approaches are also presented in that chapter. The final chapter contains a summary discussion of the significance of the results and of the possible directions for future work on survey reliability in the light of these findings.

This monograph does not purport to present the complete details and results of the Consumer Savings Project, not even in the methodological area. This is hardly necessary since five monographs on the project, all part of the series on Studies in Consumer Savings published by the Bureau of Economic and Business Research of the University of Illinois, have preceded this volume. Also, one other study is still in progress.

The members of the Inter-University Committee for Research on Consumer Behavior are:

Lincoln Clark, New York University, Secretary-Treasurer

Robert Ferber, University of Illinois

George Katona, University of Michigan

Theodore Newcomb, University of Michigan

Howard Raiffa, Harvard University

James Tobin, Yale University

Guy Orcutt, University of Wisconsin, Chairman

Raymond Goldsmith was a member of the committee until he left in June, 1963, for an OECD assignment in Paris.

The monographs in this series are research reports. The Inter-University Committee, as sponsor of this research, makes every effort to ensure both the quality of the reports and their orientation toward meeting a real need. Nevertheless, the findings reported in this way summarize conclusions arrived at by project staff and do not necessarily represent the individual or collective views of the members of the Inter-University Committee.

GUY ORCUTT, *Chairman*
Inter-University Committee
for Research on Consumer Behavior

ACKNOWLEDGMENTS

Three types of assistance were required in the course of this work. First, the Inter-University Committee under the leadership of Guy Orcutt was of inestimable value in planning the Consumer Savings Project, in obtaining the necessary funds, and in advising on the design and analysis of the different survey operations. In addition to Guy Orcutt, Lincoln Clark, George Katona, Raymond Goldsmith, Theodore Newcomb, Howard Raiffa, and James Tobin willingly devoted their time in numerous meetings of one to two days each to reviewing project plans and discussing methodology. Meetings of this type were most rewarding and, from a personal point of view, a highly valuable fringe benefit.

For the financing of the work, special appreciation is due to the Ford Foundation and to Lloyd Reynolds, the director of their Division of Economic Development and Administration, who showed a keen understanding of the nature of the problem and advised on the initial plans for the project. With his support the Ford Foundation provided substantial initial funds to enable the project to be launched. As is frequently the case, however, expansion of the project in various respects was later desirable. This expansion was made possible by supplementary grants from the United States Department of Labor, the United States Department of Agriculture, and the National Science Foundation. For these grants, and for helpful advice generally, thanks are due to Jack Alterman of the Department of Labor, Nathan Koffsky and Fred L. Garlock of the USDA, and Howard Hines of the NSF.

Third, but not least, appreciation is due to the people who did the work. Mathew Hauck did an outstanding job of directing the all-important field operations during the entire course of the project, and a difficult job it was in view of the subject matter and the continual changes necessitated by the numerous experiments. Stanley Steinkamp, and later Harold Guthrie, helped plan the various studies (together with Mathew

Hauck) and carried the lion's share of the processing and analysis of the data. The project was also fortunate in having a highly capable staff of research assistants over the years, particularly Jeffrey Green, Anne Grossman, Carol Hamilton, Anne Harper, Keith Johnson, and Robert Piskie. In addition, Virginia Speers and William Curtis performed the painstaking job of reading and checking proofs and offered many useful suggestions for improvement of the manuscript. To all of them, my sincere thanks for a job well done.

The author would like to thank many of the above people, particularly George Katona, for helpful comments in the writing of this monograph. He would also like to note that parts of Chapters IV and V were published originally in the March, 1965, and March, 1966, issues of the *Journal of the American Statistical Association*.

It must be admitted, however, that the basic direction of the project and the contents of this monograph are my own responsibility, and it is therefore to me that criticism should be directed.

ROBERT FERBER

Urbana, Illinois
April 12, 1966

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I. THE RELIABILITY PROBLEM IN CONSUMER SURVEYS

The principal focus of this chapter is on the nature and measurement of survey errors in consumer financial studies. As a basis for such a discussion, however, it would seem desirable to define relevant terms and to enumerate some of the principal conceptual problems involved in the measurement of savings. Accordingly, the first part of this chapter will be concerned with the general question of the measurement of consumer savings and saving, and the second part will be devoted to problems arising in the measurement process, specifically that of data reliability.

Measurement Problems

Savings and Saving

In any study of savings, or of saving, a clear distinction has to be made between these two terms. By *savings* is meant the stock of assets and debts at a particular moment in time. *Saving* refers to the accumulation of assets and debts over a period of time. In other words, *saving* is a flow concept, representing the change in the stock of assets and debts between two points in time.¹

Both saving and savings can be positive or negative. Saving is negative if expenditures plus debt exceed income plus other receipts during the given period; in such a case, there is *dissaving*. In a similar fashion, savings are negative if the stock of debts exceeds the stock of assets at the given moment.

Alternative Definitions of Savings

Definitions of savings, and of saving, vary in scope and complexity. No single definition is adequate for all purposes. Data corresponding to the more general definitions tend both to be more difficult to obtain and to involve more frequent and more doubtful assumptions.

¹ To be sure, savings could also represent the plural of saving. However, for the sake of clarity this use of "savings" will be avoided, invoking — if necessary — poetic license to do so.

First, and simplest, is a definition that equates savings with liquid assets. Liquid assets include currency, checking accounts, United States savings bonds, and savings accounts in their various forms (including certificates of deposit and share and investment accounts in commercial and mutual banks, savings or building and loan associations, postal savings, and credit unions). All of these assets have the following two properties: they either consist of cash or are immediately convertible into cash, and they are in fixed dollars. The latter means that the number of dollars obtainable from an account equals the sum of the net deposits plus interest, if any. The amount does not fluctuate with changes in prices or market or business conditions.

A more inclusive definition of savings is in terms of financial, or intangible, assets. The alternate term, "intangible," is the more descriptive because it includes, in effect, assets with no tangible value of their own. Included are not only liquid assets but all other financial assets as well — marketable government and other bonds, common and preferred stocks (such as insurance company stocks), annuities, mortgages and debt (on both the debit and the credit sides), and pensions.

More inclusive still is to incorporate houses, cars, and other durable goods, the latter valued generally at market (resale) prices. There are two main reasons for including such expenditures in a savings definition. One is that to many people these purchases are not really expenditures. Especially when prices are rising appreciably, or are expected to do so, the purchase of durable goods may represent a substitute form of savings.² If prices rise sufficiently, ownership of durables can indeed serve to maintain one's capital or even to bring about substantial capital appreciation, as was the case during and shortly after World War II.

A second reason for treating durables as a form of savings is that such goods almost always possess resale value, which for houses and cars may well represent the major part of a consumer's capital. That such a tendency actually exists on the part of consumers to treat investments and purchase of durables as substitute forms of savings is supported by studies which show that the sum of the two bears a more stable relationship to income than does each component separately.³ In addition, one could cite socio-psychological studies which indicate that ownership of many durables possess status and prestige as symbolic of wealth as ownership of

² Irwin Friend and Robert Jones, "The Concept of Saving," in *Proceedings of the Conference on Consumption and Saving*, Vol. 2 (Philadelphia: University of Pennsylvania Press, 1960), pp. 336-59.

³ E. S. Maynes, "The Relationship Between Tangible Investment and Consumer Saving," *Review of Economics and Statistics*, Vol. 41, No. 3 (August, 1959), pp. 278-93.

financial assets.⁴ Analytically, too, such a definition has proved useful, leading to provocative theories of consumer behavior.⁵

Conceptually, however, a slight extension of the previous definition of savings would seem desirable because of the fact that certain so-called nondurables, such as suits and overcoats, also possess resale value and may have a longer life span than many items classified as durables. Such an extension leads to the most inclusive definition of savings, which may be termed the net-worth concept. According to this definition, savings would include the resale value of all tangible goods in addition to financial holdings, deducting debt and other financial obligations. In other words, this definition covers the material value of one's entire possessions.

Although the simplest of all theoretically, this definition is rarely, if ever, used in empirical work because of the difficult valuation problems that arise. Perplexing as it is to value items for which relatively organized markets exist (such as houses and cars), valuing items such as clothing and household furnishings, which come in innumerable sizes, styles, and shapes, becomes a virtually hopeless task. In the present volume, no attention is given to the valuation of consumer goods, durable or otherwise, because the main focus is on the collection of financial assets data.

Estimating Saving

By virtue of the two different ways of defining saving, a survey procedure designed to estimate saving — whether by consumer survey or by institutional canvass — can also follow either of two approaches, the specification of which has major ramifications for numerous aspects of the survey procedure.

The first approach, the more direct of the two, entails asking the respondent (or the institution) to report directly the amount of saving(s) during a period. Thus, by this approach the change in one's checking account(s) during the first quarter of the year would be ascertained in a consumer survey by asking the sample member a question such as: "How much change was there in the balance in your checking account(s) between January 1 and April 1?" This approach, which provides a direct estimate of saving, we shall call the *change approach*.

The second approach is to estimate saving as the computed difference between holdings of particular assets or debts reported at the beginning (H_0) and at the end (H_1) of a period.

⁴ Thus, in many neighborhoods, especially suburbs, ownership of a high-priced car was for a long time a far more important symbol of success and affluence than ownership of savings accounts or stocks — which could hardly be parked outside one's house for neighbors to envy.

⁵ For example, Milton Friedman, *A Theory of the Consumption Function* (Princeton: Princeton University Press, 1956).

By this approach, the *holdings approach*, each estimate requires answers to two questions:

H_0 : "What was the balance in your checking account(s) on January 1?"

H_1 : "What was the balance in your checking account(s) on April 1?"

These two questions could be asked on a single interview (on or after April 1), or each could be asked in a separate interview, the first on or shortly after January 1 and the second on or shortly after April 1. To obtain this information in the course of a single interview is clearly most economical. Unfortunately, this economy comes only at a price, since such factors as memory errors and lack of information tend to reduce the reliability of the data furnished for the earlier point in time. Evidence to this effect will be provided in later chapters.

Apart from this question, the selection of one of these two general approaches in a particular case is a difficult matter. From a purely survey point of view, the change approach is the easier. It requires asking only one question — a question which is likely to be less sensitive than one on holdings; and it shifts the burden of computation from the survey staff to the respondent. Further thought, however, indicates that these advantages in favor of the change approach are by no means clear-cut, and they may not even be advantages. Thus, the question put to the respondent is not really one but is in effect three, with the very rare exception occurring when the respondent has actually computed his saving for the period in question. The questions are:

- (1) "Look up (or recall) the balance in your checkbook on January 1."
- (2) "Look up (or recall) the balance in your checkbook on April 1."
- (3) "Compute the difference and tell me what it is."

The first of these questions implicitly places strain on the respondent's willingness to cooperate because he is being asked to recall a figure at least as old as the period under study — in this case three months. The first two questions taken together pose another problem, one particularly pertinent to reliability: has the respondent selected comparable balances? Thus, if the balance at one date includes a mortgage payment for that month and the balance for the other date selected is exclusive of the mortgage payment for that month, the reported saving will be biased. Of course, a similar danger arises with the holdings approach except that, as will be noted later, the danger is not so great, apparently because the explicit nature of the question helps both the interviewer and the respondent to focus on the matter of comparability.

The third of these questions also contains a hidden danger, namely,

the assumption that the respondent can subtract correctly.⁶ The conditions under which these subtractions take place are not usually conducive to a high degree of accuracy, being made mentally and generally very quickly. Errors in arithmetic are therefore not unlikely.

From these various points of view, the holdings approach would appear to offer an advantage. In addition, the holdings approach clearly provides some extra highly pertinent data, namely, savings in these particular holdings, thereby enabling *saving* to be related to *savings*. It is precisely this, however, which tends to be the principal disadvantage of the holdings approach. The request for figures on holdings is one of the most personal questions that can be asked. If there is any doubt in the respondent's mind regarding the value or authenticity of the study or of the confidentiality of the data, the result is likely to be a refusal or possibly the giving of erroneous information.

For the sake of completeness, a third method of estimating savings should be mentioned, although it is not generally reliable with consumer surveys. This method, which we may call the income-expenditure approach, seeks to estimate saving by ascertaining total income and deducting from it expenditures for current consumption, and in some cases expenditures for durable goods as well. The method is used by the United States Department of Commerce in estimating aggregate personal savings. In that particular case, estimates of aggregate income and of aggregate consumption expenditures are obtained, with aggregate saving derived as the residual.

When applied to consumer surveys, the method is subject to considerable error because saving generally constitutes such a small proportion of total income. As a result, any error in the estimates of income or of expenditures tends to be magnified many times in the residual estimate of saving. The Department of Commerce estimate of saving is subject to much the same difficulty, except that it might be argued that for a single aggregate estimate the possibility of large errors is low, due in part to the availability of crosschecks in the national income accounts.

Institutions Versus Consumers as the Source of Data

With the exception of currency, all forms of assets and debts involve some organization or individual other than the consumer unit, generally an institution or a government. Since saving in the form of currency is negligible from year to year, two alternative sources of information on

⁶ It also involves the assumption that positive and negative changes can be distinguished from each other, an error that seems to be made infrequently but is of substantial proportions when it does occur.

consumer saving would appear to exist — the consumer himself or the appropriate financial institution.⁷

In theory, either source could supply the data needed to estimate aggregate consumer savings, or saving; at least, the data are there. Similarly, data could be obtained from either source on the size distribution of individual holdings, although not of all holdings of a particular asset or debt. Thus, a random selection of savings accounts of individuals in banks and savings institutions would yield an unbiased estimate of the distribution of individual accounts as ascertained from a consumer survey, abstracting from survey errors. However, such a random selection would not yield an unbiased estimate of the size distribution of *total* savings of consumer units in the form of savings accounts.

For the latter purpose, as well as for any data involving the relationship between ownerships of different types of savings (or saving), the consumer survey is the only source. A consumer survey is also required if saving is to be related to consumer characteristics, attitudes, or plans, except for those few instances in which these characteristics have been recorded by the institution.⁸ In addition, there is one aggregate for which institutional data are clearly inadequate and which is only obtainable by consumer survey, namely, saving in farm and nonfarm unincorporated business.

On the other hand, certain data, pertaining primarily to institutional characteristics of consumer savings, can only be obtained from institutions. These would include such data as the breakdown of demand deposits among individuals, nonprofit organizations, businesses, and so forth, and the distribution of ownership of mortgages among consumers and other holders. Also included would be data which, although theoretically available from consumers, are not likely to be reported with much reliability, such as accumulated saving in the form of life insurance and pension plans.

For many purposes, therefore, these two data-collection sources are essentially complementary. In actual practice, however, situations frequently arise in which the data requirements could be met by going to either source. In addition, questions have been raised regarding the reliability of financial data obtained in consumer surveys (the subject of the present work). As a result, considerable controversy exists regarding the pros and cons of approaching each source.

⁷ For the sake of simplicity, we shall assume for the time being that consumer assets and debts are held only by institutions, including governments, and not by other consumers. With the possible exception of personal loans, this assumption is no doubt true.

⁸ Even then a consumer survey may be required, since such data are generally of marginal value to the institution's operations and little effort is made to collect them accurately or to keep them up-to-date.

Two basic arguments are advanced in favor of contacting institutions rather than consumers for financial data: they are fewer, and they are accustomed to supplying financial information. For any single type of savings, it is clearly more efficient to sample the records of a relatively few institutions than to contact hundreds or thousands of consumers for comparable information. In such a case, contacting institutions is likely to be quicker as well as much cheaper. Furthermore, institutions are likely to be more receptive to such requests—since they prepare and furnish data on their other operations frequently—and are also likely to provide such data more accurately because of the records they keep. Hence, to the extent that data can be obtained from institutions, this source should be preferred.

Yet these arguments do not detract from the indispensability of consumer interviews. Interest in the distribution of single accounts or more generally in the distribution of single holdings of a single asset—without reference to consumer characteristics, ownership of holdings of the same asset, or holdings of different assets—represents a small minority of consumer financial studies. Even in this minority of situations, certain holdings (such as equity in an unincorporated business) can only be obtained by consumer surveys. When attention is broadened to include several assets and debts or all holdings of a single asset (such as a consumer unit's total investment in government savings bonds), consumer surveys are generally the only answer. Although they are obtainable theoretically from institutions also, the multiplicity of institutions and of the record-keeping practices of different financial intermediaries, combined with the absence of attempts to keep names and addresses up-to-date, makes the collating problem insoluble for all practical purposes.

Consumer surveys would also seem superior if interest is in obtaining only univariate size distributions of different assets and debts separately, e.g., a size distribution of checking accounts *and* a size distribution of savings accounts *and* a size distribution of government savings bonds, and so forth. This information could be obtained by sampling the records of institutions in a corresponding number of different financial areas. However, since information on all holdings could be also obtained in a single visit to a consumer unit, abstracting from the data reliability problem, the consumer survey is likely to be more economical in such a case and perhaps quicker, depending on the number of holdings involved.

The two basic problems associated with the use of consumer surveys are data reliability and cost. The data reliability problem stems from a number of factors which lead some people either not to provide any information or to provide inaccurate information. Since these factors will be considered in later chapters, it need only be said here that their net

result is to produce estimates of financial holdings biased not only in the aggregates but in the distribution of these holdings as well. These biases are substantial enough to make questionable the advisability of devoting extensive resources and analytical talent to the collection of such data, if the biases are not detected and corrected.

Cost also is a major problem, partly because of the difficulty of contacting all sample members and partly because of the inherently high expenses of the field interviews. The confidential nature of the data requested precludes anything but personal contact, at least on an initial interview. The interview itself requires a personable and high-caliber individual as well as one with considerable skill in asking questions and recording the replies. The result costwise is that field charges alone can average anywhere between \$15 and \$25 per interview, and the overall cost of a nationwide survey of about 3,000 consumers can aggregate between \$100,000 and \$200,000.

Despite these limitations, the fact remains that consumer surveys are indispensable. With increasing attention being devoted to problems of consumer behavior, and particularly to consumer financial behavior, such surveys are likely to be required even more in the future. The basic problems therefore become how to use such surveys most effectively and how to overcome their limitations. In the past, attention has focused primarily on the improvement of survey procedures — better interviewers, more intensive training sessions, simpler questionnaires, special attempts to interview initial refusals and noncontacts, and so on. Improvement along these lines will no doubt be helpful and may produce significantly more reliable data. At the same time, a broader perspective may well be worth considering. Such a perspective was provided by Irwin Friend's proposal in the late 1950's designed to take advantage of some of the best features of contacting both sources.⁹ The basis of this proposal is sampling institutional records for data on consumer holdings and then contacting the consumers sampled for such classifying information as age, occupation, family size, and so forth. To quote Friend:

The general plan to be followed for each institutional body of data to be covered is, first, to select a sample of institutions and, second, to select a sample of accounts within each institution, transcribing names and addresses of the owners and dollar amounts of the accounts at the beginning and end of the year. Though a number of variations in procedure are possible and should be tested, the final step would probably involve contacting the individuals directly to obtain occupa-

⁹ Irwin Friend, "Institutional Data as a Source of New Information for Use in Social Accounting Systems," paper delivered at the Conference on Research in Income and Wealth, November 6-7, 1959, and printed in *The Flow-of-Funds Approach to Social Accounting, Studies in Income and Wealth*, Vol. 26, National Bureau of Economic Research (Princeton: Princeton University Press, 1962). Friend had been working on this idea for some years before this paper was given.

tion (including occupation of the head of the family if different from the individual), income class, and other significant family characteristics. Such contacts can be effected by mail, with telephone and interview follow-ups if necessary.¹⁰

In that paper, Friend describes one such case study in which stockholder characteristics were ascertained by a mail questionnaire, yielding almost a two-thirds response without any follow-up.

This method has the advantage of yielding highly accurate financial statistics, which are then related to selected consumer characteristics. It is also likely to be far more economical than a personal interview survey. The method is effective in allocating *single* holdings of assets and debts among consumer units. The emphasis has to be on "single," however, because it is not feasible to match different assets via institutional records to obtain a consumer unit's total portfolio, or even to match different holdings of the same asset to derive a consumer unit's total holdings of that asset.

Apart from this, the method possesses other disadvantages, some not apparent offhand. For one thing, it does not do away with a consumer survey, and hence it leaves survey problems and biases still to be considered. In this respect, the following assertion is open to question:

... certain basic family characteristics such as occupation can be obtained quite reliably from consumer surveys and even income, which is more difficult to measure, has a much smaller margin of error attached to it than data on saving.¹¹

Such errors obtained in the few studies that were designed for this purpose all pertain to personal interviews. To acquire such data by mail, or by telephone, is likely to involve much larger errors even in replies about consumer characteristics, because these are the techniques that are most conducive to misunderstanding, replies by wrong respondents, distrust of auspices, and so on. The nature and magnitude of these errors have yet to be investigated.

Furthermore, it is not clear that information about a characteristic such as occupation can be secured much more reliably in a consumer interview than data on saving, even by personal interview, because of inherent biases and prejudices concerning the subject. To take one example, very few "janitors" are reported on consumer surveys — they are all "superintendents of operations," or something similar. The little validation work that has been done on this subject, as yet unpublished, points to a strong upward occupational bias, even in the United States Census data. That this bias is likely to be still larger on a mail questionnaire is a strong probability.

Finally, it might be noted that only a limited amount of information

¹⁰ *Ibid.*, pp. 5-6.

¹¹ *Ibid.*, p. 7.

can be requested on a small questionnaire and still produce high response rates. If much detail or probing is desired, the data will probably have to be obtained by personal interview. Even when relatively little information is required from consumer units and response rates are high, a reliable survey will undoubtedly entail personal interview follow-ups with a subsample of the mail nonrespondents.

These limitations notwithstanding, the method advocated by Friend has served to broaden the horizons of possible solutions to the problems of collecting consumer saving data. It fulfills a real need by furnishing a much more reliable method than formerly existed for ascertaining characteristics of holders of particular assets or debts — provided that possible biases are recognized and appropriate precautionary steps are taken.

Survey Errors

Sampling Versus Nonsampling Errors

Estimates derived from surveys are hardly likely to coincide with the true values in the population, for two reasons. One is that the sampling process produces some deviation between the sample estimate, y say, and the true value, x , simply because the sample is only a part (and generally a very small part) of the population. These so-called *sampling errors* can be measured if a probability sampling procedure is used. With these sampling designs, the probability of securing estimates differing by given amounts from the true, unknown population value can be computed, given certain basic information such as the sampling technique, sample size, and population variances (usually estimates of these variances).

The derivation of sampling error formulas has become essentially an exercise in the mathematics of probability, although in some cases the mathematical problems can be intricate. To be sure, in many instances a nonprobability design is employed in a survey, a design for which the probabilities of selection are not known. In those instances, the sampling error cannot be estimated (although the usual sample precision formulas are often applied anyway). The fact remains, however, that the sampling error problem has been reduced virtually to an exact science, and a considerable literature is available on optimum selection of sample designs for particular purposes. Practicable theories exist for measuring and controlling sampling errors,¹² so that in a particular survey the precision can be set at any desired figure, given adequate resources.

The second reason why survey estimates deviate from the true population figure is the myriad of other types of errors that can occur in such an

¹² See, for example, M. H. Hansen, W. N. Hurwitz, and W. G. Madow, *Sample Survey Methods and Theory*, Vols. 1 and 2 (New York: Wiley, 1953) or W. E. Deming, *Sample Design for Business Research* (New York: Wiley, 1960).

operation — most of which have nothing to do with the sampling procedure used — or even do occur whether or not the survey is based on a sample. These errors include such varied forms as recording of mistakes by interviewers, misinterpretation of replies, coding mistakes, errors in tabulation, and errors in computation. There are so many varieties that the most appropriate designation for them is errors other than sampling errors, or *nonsampling errors*.

Perhaps the one general characteristic of these errors is that from an estimation point of view they all may serve to bias the survey estimates. In the absence of nonsampling errors, surveys can be designed to yield unbiased estimates of the true population values, meaning that as the size of the sample approaches that of the population the survey estimate converges asymptotically to the true value. When nonsampling errors enter the picture, however, this is no longer the case, for these errors or biases generally have little to do with sample size. As we shall see shortly, the magnitude of some of these biases tends, if anything, to increase with sample size.

Since these biases are usually not measurable and are often not even suspected to exist, the sampling property of unbiased estimates can become artificial. In general, the larger is the relative importance of nonsampling errors, the less meaningful becomes the sampling property of unbiasedness. This is not to deny that this property has practical value, for control of sampling errors is desirable in any event.¹³

Effect of Bias on Survey Estimates

Biases affect survey data in two ways: they distort the population estimate obtained from the survey data, and they distort the reliability of the estimate. In the first instance, the expected value of the sample-based estimate will be biased, even if the estimate otherwise would be unbiased. In other words, if x is the true value of the statistic in the

¹³ A distinction should be made between biased sampling estimates and biases or nonsampling errors. In the former case, reference is to a sampling procedure, that is at times more efficient, of using an estimation procedure which yields a biased estimate in order to achieve certain other properties, such as reduced sampling variance relative to that of the unbiased estimating procedure. Such is the case, for example, with ratio estimates, in which instances the bias is incorporated as part of the sampling procedure; its magnitude is known and hence can be eliminated in the final analysis. As was noted previously, biases due to nonsampling considerations are not often known or controllable, and they are rarely introduced deliberately — certainly not to improve efficiency of estimation. Since this monograph is concerned exclusively with nonsampling errors, possible confusion arising from the use of "bias" in two different senses is not likely to arise. Henceforth, "bias" will refer to the nonsampling errors in a survey operation. (See, for example, Hansen, Hurwitz, and Madow, *op. cit.*, Vol. 1, Chapters IV and VIII.)

population, the sample estimate of x , say y , will deviate from the true value, on the average, by a certain non-zero quantity, e .

Various error models can be set up to explore the effects of bias. For the present purposes, it is sufficient to base such a model on the following simple relationship:

$$(1.1) \quad y_i = x_i + e_i,$$

where y_i is the observed (or reported) value for the i th sample member, x_i is the true value of this observation,¹⁴ and e_i represents the discrepancy in the report, defined as $y_i - x_i$.

Correspondingly, we may define the means of these variables, which are related to each other in a fashion similar to those of the individual observations, namely:

$$(1.2) \quad \bar{y} = \bar{x} + \bar{e}.$$

If there are no errors in the individual observations, e_i and \bar{e} are zero; \bar{e} may also be zero even if errors are present in the individual observations but tend to offset each other. Alternatively, \bar{e} may be small relative to \bar{x} , in which case bias arising from the use of \bar{y} as an estimate of \bar{x} may go unnoticed. However, if \bar{e} is large relative to \bar{x} the bias may be noticed even in the absence of validation information, as has been the case with many past saving surveys.

Biases may affect the reliability of an estimate as well as the estimate itself. Bias serves to increase the standard error of estimate of a particular statistic, with the result that confidence interval estimates turn out to be faulty. As is shown in various sampling texts,¹⁵ if bias is present in the mean ($\bar{e} \neq 0$), the true variance, or mean square error, of the mean is measured in terms of the variance of the observations, y_i , about the true mean, \bar{x} , i.e.:

$$(1.3) \quad \text{Mean square error (MSE)} = \sigma_{\bar{y}}^2 + \bar{e}^2,$$

where $\sigma_{\bar{y}}^2$ is the variance of the mean of the observed values, y_i , and \bar{e} is the bias in the estimate of \bar{y} .

Clearly, whenever bias is present the variance of the sample estimate is increased, and the more so as the square of the bias rises relative to the

¹⁴ In many surveys, particularly those dealing with opinions or attitudes, "true value" may be a nebulous concept. In the present case, we may assume that we are dealing with factual information (such as face value of life insurance) and that the "true value" refers to the actual amount held by that sample member. Admittedly, even in such a situation questions may arise regarding "true value," as in the case of equity in a pension fund, but this is beyond the scope of the present discussion.

¹⁵ For example, W. G. Cochran, *Sampling Techniques* (New York: Wiley, 1963), pp. 12-16.

variance of the observations. Thus, if \bar{e} is a certain multiple of $\sigma_{\bar{y}}$, say k , the above equation reduces to:

$$(1.4) \quad MSE = (1 + k^2) \sigma_{\bar{y}}^2.$$

Some idea of the extent of this bias can be obtained by considering the manner in which different values of k influence the probability that the usual symmetrical 95 percent confidence interval will include the true mean. The exact probability in the absence of bias, using both tails of the normal distribution, is .95. The following tabulation gives values of this probability for different values of k .¹⁶

Value of k	Probability
0.....	.950
.25.....	.941
.5.....	.921
.75.....	.883
1.0.....	.830
1.5.....	.677
2.0.....	.484
3.0.....	.149
4.0.....	.021
5.0.....	.001

For relatively small biases—even those in which the bias is 50 percent of the variance of the true value—little distortion takes place in the standard error estimate and in the confidence interval. However, as the bias approaches, and then surpasses, the standard error of \bar{y} , the bias increases at an increasing rate. When the bias term is more than twice the size of the standard error of \bar{y} , the probability that the confidence interval contains the true mean is just about half the presumed value; and if the bias exceeds the standard error of \bar{y} by more than 4.0, the probability is virtually zero.

Equation (1.3) also may be used to demonstrate that even in the absence of bias the estimated variance of the mean may differ from the true variance as long as errors are present in the individual observations. The reason for this is that the variance term on the right-hand side of Equation (1.3) is based on the observed values, y_i , not on the true values, x_i . As a result, using the definition in Equation (1.1), it is a simple matter to show that $\sigma_{\bar{y}}^2$ represents a combination of sampling variances of x_i and of e_i , as follows:

$$(1.5) \quad \sigma_{\bar{y}}^2 = \sigma_x^2 + \sigma_e^2 + 2r\sigma_x\sigma_e,$$

where r is the coefficient of correlation between x_i and e_i .

¹⁶ Adapted from Cochran, *ibid.* The probability is:

$$\frac{1}{\sigma \sqrt{2\pi}} \int_{-1.96 - e/\sigma}^{1.96 + e/\sigma} e^{-\frac{t^2}{2\sigma^2}} dt.$$

Equation (1.5) reflects the errors in the individual observations. Hence, to arrive at the total effect of nonsampling errors on the reliability of survey results, we may substitute Equation (1.5) into Equation (1.3) to obtain:

$$(1.6) \quad MSE = \sigma_{\bar{x}}^2 + \sigma_{\bar{e}}^2 + 2r\sigma_{\bar{x}}\sigma_{\bar{e}} + \bar{e}^2.$$

The effect on the sampling variance of nonsampling errors is measured by the last three terms on the right-hand side. The last term measures the increase in the variance due to bias, whereas the preceding two terms measure the increase in the variance due to errors in the individual observations, including intercorrelation between these errors and the true values.

Equation (1.6) serves as a framework for the analysis of the effect of nonsampling errors on the reliability of survey results. As has been demonstrated, this effect is manifested in two ways: through increase in the sampling variance of the observations, because this variance is based on y_i rather than x_i , and through distortion of the variance if bias is present, because the sampling variance is estimated around \bar{y} , not the unbiased value \bar{x} . To be sure, the former effect is incorporated in the estimate of the sampling variance and therefore does not bias this estimate. However, to the extent that this component can be eliminated, the reliability of the estimate of the sampling variance is increased.

The effect of bias is measured by the ratio, $\bar{e}/\sigma_{\bar{y}}$, or k (Equation 1.4). As k increases, the apparent sampling variance, $\sigma_{\bar{y}}^2$, differs increasingly from the true sampling variance, or mean square error. With validation data, these quantities can be computed and compared with each other.

In a similar fashion, the effect of errors in individual observations can be computed by comparing $\sigma_{\bar{y}}^2$ with $\sigma_{\bar{x}}^2$, or:

$$(1.7) \quad \begin{array}{l} \text{Relative increase in sampling variance due} \\ \text{to errors in individual observations} \end{array} = \frac{\sigma_{\bar{e}}^2 + 2r\sigma_{\bar{x}}\sigma_{\bar{e}}}{\sigma_{\bar{x}}^2}.$$

The validation data enable us not only to compute this ratio but also to allocate the increase among the two principal causes: errors and intercorrelation between errors and true values.

Nonsampling Errors in Savings Surveys

Different types of surveys are subject to nonsampling errors in varying degrees, depending on certain salient characteristics of the surveys and of the populations sampled. Consumer financial surveys possess distinctive characteristics which contribute to the likelihood of more frequent occurrences of certain types of nonsampling errors than are found on other surveys. A review of the main such characteristics and their implications for the nonsampling-error problem would seem to be in order. For this purpose, we distinguish between "random" and "nonrandom" non-

sampling errors. Random nonsampling errors are defined as those which are scattered symmetrically about the true values, so that even though single deviations may be large, on an overall basis the deviations tend to nullify each other as the sample size increases. In other words, the expected value of the observations converges stochastically to the true value. On the other hand, nonrandom sampling errors tend to bias the observed value away from the true value.

For analytical work — establishing relationships between variables — both types of errors may be of importance, the primary determinant being their size and dispersion rather than their randomness or lack of randomness. However, for enumerative purposes — estimating averages or totals — it is essentially the nonrandom errors that matter, for with increasing sample size the effect of the random nonsampling errors will tend to cancel out.

The distinction between random and nonrandom nonsampling errors is not always clear-cut and may vary with what is being measured. Thus, response errors may be random for estimation of debt but nonrandom for estimation of stock holdings. In a particular case, however, the distinction generally can be made, sometimes on an *a priori* basis and at other times on the basis of past experience.

Personal Nature of Interview

In a savings interview of the type considered here, the respondent is questioned among other things about either his saving during a specific period (the last month, the last quarter, or the last year being the most common periods) or about his savings at a particular moment of time, often as of the date of the interview. In either case, the questions are of highly personal nature, delving into matters which the respondent may have told nobody (not even his psychiatrist . . .) and, accordingly, raising stresses and strains not ordinarily present in survey interviews.

In addition, the presence of an interviewer, although not usually from the same neighborhood but invariably from the same city or county, is likely to place the respondent under various social pressures, the nature of these pressures depending on his personality. Thus, if the respondent is a conformist, he will hesitate to give information showing lack of conformity with the group to which he feels a kinship, such as admitting having more liabilities than assets while living in a very prosperous neighborhood. If he is on relief, he may not care to divulge ownership of a sizeable savings account for fear of losing his relief status if this fact should become known to the proper authorities. Alternatively, he may not care to reveal sizeable holdings of stocks and bonds while living in a poor neighborhood, because of not having reported the income from these holdings on his tax returns, or for other reasons.

Whatever the nature of these pressures, all of them contribute to increased incidence of response errors. This is particularly true in view of the ease of making such errors. The answers can hardly be checked, and if a respondent is not sure of an answer, or does not want to give the correct answer, it is a simple matter for him to give some other answer. Alternatively, he may refuse altogether, which is another form of response error (unless the entire interview is refused, in which case it becomes a form of noncontact).

All of these errors are likely to be serious, since not only can they be substantial but they are without any doubt nonrandom. The combination of social pressures and personal stress brought about by the questions on savings or saving can lead to substantial overestimates and to substantial underestimates, with the latter tending to be considerably larger. As we shall see, the most important response error, or nonsampling error of any type on a consumer savings survey, is a report of no holdings when an asset is owned. The personal nature of the interview also increases the frequency of noncontact errors, and it also contributes to their being primarily nonrandom. Evidence will be presented later to show that people who refuse to be interviewed or are not contacted tend to differ from respondents in certain significant respects.

Request for Figures

The request for exact figures, generally unavoidable, is another cause of nonsampling error, again a contribution to response error. Whether the request is for a balance or for a change during a given period, the exact figure is hardly at the respondent's fingertips, and there is a strong inclination to cite a figure from memory rather than to start searching for records. Clearly, the inclination is even stronger if the respondent is not in a fully cooperative mood.

Quoting from memory figures that in many cases relate to matters with which the respondent may not have had recent contact no doubt serves to contribute to the incidence of response errors, both directly and through possible interaction with the preceding factor. Thus, if a respondent is somewhat dubious about mentioning the complete extent of his life insurance holdings, this tendency may manifest itself through a memory quirk leading him to report, in all sincerity, a lower amount of holdings than is actually the case.

Complicated Subject Matter

Still another factor contributing to response error in savings surveys is the complexity of the subject matter. Even when people have savings in a particular form, they may not know too well what it is. Savings and loan associations tend to be confused with mutual savings banks, and both

with commercial banks; investment accounts with savings share accounts; common stock with preferred stock; term insurance with ordinary life insurance; and so on. At times neither the respondent nor the interviewer may be too familiar with a particular type of asset, a situation which undoubtedly increases the incidence of errors brought about by faulty recording or misinterpretation.

Insecurity resulting from respondent awareness of his own limitations in dealing with questions on this subject may also contribute to refusals and noncontacts. This is particularly likely if the respondent knows in advance that fairly detailed questions will be asked about his financial position or about his knowledge of different assets and debts. (This is one reason why introductory letters are best made brief and not too explicit.) Furthermore, because of the complex, and confidential, nature of the subject, the interview must be conducted with a family member who is acquainted with its finances, not just with any member that happens to be at home. This, of itself, makes a refusal or noncontact more likely, for if that particular family member is unavailable for an interview, there is no alternative.

To a lesser extent, complexity of subject matter contributes to the incidence of data-processing errors, particularly in transcribing hurriedly-written dollar amounts. As a rule, however, such errors are likely to be random and can be reduced to negligible proportions with provisions for check-coding and data verification.

Length of Interview

Few interviews on one's savings habits and practices take less than half an hour; some take almost two hours. Although these are by no means the longest duration of personal interviews,¹⁷ toward the end of such interviews — particularly after the first hour — some respondents tend to become restless and try to finish the interview as quickly as possible. Under such circumstances, response errors can be more frequent, especially errors on dollar amounts and other factual data about which the respondent may not then want to devote much thought.

On lengthy interviews, errors also become more likely from the interviewer's side, for he too may be fatigued, less alert, and more prone to misinterpret, to record answers incorrectly, and even to abbreviate questions to the point of confusing the respondent.

Highly Skewed Population

Economic and financial data distributions are generally highly skewed

¹⁷ The consumer expenditure interviews of the United States Bureau of Labor Statistics may extend over eight hours, although not usually continuously; and other interviews on consumer expenditures requiring anywhere between two and four hours are not uncommon.

to the right, and this is especially true of savings and saving. As shown by Lampman,¹⁸ in 1953 the great majority of American consumer units held relatively little wealth, and the situation has changed little since that time. At one end of the scale, 31 percent, holding less than \$1,000 of assets per consumer unit, accounted for 1 percent of the nation's total financial wealth. This is no doubt at least as true of saving during a given period. The proportion of consumer units adding, say, more than \$100,000 to their savings accounts or to their stocks during a given period is undoubtedly extremely low, yet of substantial importance to the estimation of aggregate saving. As a result, unless the sample contains many thousands of consumer units — and most surveys of consumer savings rarely cover more than 3,000 consumer units — errors relating to the sampling distribution are not unlikely, and they are clearly nonrandom.

Such errors are much less likely for estimation of attributes, e.g., the proportion of consumer units increasing their assets by more than a certain amount, rather than for dollar amounts. It is for this reason that the former type of estimation is generally preferred when a choice is possible. Even in such cases, however, errors relating to the sampling distribution can be expected, since a difference of one or two sample members in an extreme class can amount to many thousands of consumer units when aggregated.

Estimation of Unknown Information

Complete coverage of saving, even if only financial saving, leads to requests for data about which a respondent can hardly be sure of being correct. Saving in the form of pensions is a case in point. Theoretically, once the concept of saving in this form is defined, the necessary information could be obtained either from the respondent's records or by going to the institutional source. In practice, however, such records are rarely available and it is not usually feasible to obtain the data from the institution. The result is that peripheral data are generally sought from the respondent, and the data then serve as a basis for estimating saving in the form of pensions.

These estimates are subject to errors of valuation, stemming from two sources. First, the peripheral information supplied by the respondent may not be accurate, in which case even a fully accurate estimation procedure will lead to some error. Second, the estimation procedure will very likely not be perfect. Although it may be stochastic, so that the average of many individual estimates will tend to coincide with the true average, errors in individual cases may well be substantial. If the estimation pro-

¹⁸ Robert Lampman, "Changes in the Share of Wealth Held by Top Wealth Holders," *Review of Economics and Statistics*, Vol. 41, No. 4 (November, 1959), pp. 379-92.

cedure has been well designed, these errors will tend to be random. The first type of error is less likely to be random, however, to the extent to which personal data are requested, such as contributions to a pension plan. The exact nature of these errors is as yet unknown.

If saving in the form of durable or other tangible goods is included, the scope for errors of valuation increases enormously, for reasons already discussed. Hopefully, such errors are random, although their magnitude is likely to be substantially greater than errors of valuation of financial assets.

Overall View

This brief review of distinctive characteristics of consumer saving surveys is sufficient to bring out clearly the fact that the danger of response and noncontact errors is greatly increased in such surveys. The increased incidence and seriousness of these nonsampling field errors is essentially the direct effect of the complexity and distinctive nature of these interviews. Partly as a result, other types of nonsampling errors, such as data-processing errors, tend to increase also, although undoubtedly nowhere near to the extent that errors in the field increase. Although exact estimates of the relative magnitudes of these different types of nonsampling errors have never been made, there is little doubt that at least in consumer financial surveys these field errors are by far the more important, and that adequate means of coping with them would contribute more than any other single factor toward raising the reliability of such surveys.

Panel Effects

Although panels offer unique advantages for the study of consumer saving behavior, they also serve to introduce additional ramifications to the nonsampling error problem. We shall discuss these sources with specific reference to the type of panel used in the course of the Consumer Savings Project. In these panels the same family is interviewed approximately every three months for at least one year. In each interview data are sought regarding saving in the intervening period or the stock of savings at the time of the interview. Additional questions are also asked, differing from one interview to another. These questions may be designed partly to acquire supplementary information — income, saving attitudes, savings practices, and so forth — and partly to improve rapport by reducing the apparent emphasis on securing dollar figures.

The additional sources of nonsampling errors stem primarily from the repetitive interviewing of the same people, intensified in a consumer savings panel by the fact that the same basic questions are repeated from one interview to another. On the other hand, this repetitive interviewing

procedure may also contribute to *reducing* certain nonsampling errors, as will be noted shortly. First, however, let us consider the possible deleterious effects of a panel operation. These effects relate either to possible loss of representativeness of the panel over time or to conditioning effects due to respondent participation.

Representativeness

Whether or not a panel provides a perfect miniature of the population at the start of the operation, chances are that it will become less representative as time goes on, for two reasons. First, a panel is a static miniature: only to a limited extent, if at all, will it reflect changes in population characteristics over time. Migration, new family formation, and family dissolution are among the demographic factors that cannot be replicated by a static panel.

To be sure, attempts can be made to introduce such autonomous factors through a policy of panel rotation — periodic replacement of old panel members with new ones, with appropriate stratification to allow for changing population composition. Such a policy, however, reduces the period over which data are available for individual families. This policy is followed by various commercial consumer panels, in which data are sought for individual families over long periods of time. It has not been used, as far as is known, in consumer saving panels, which are generally of brief enough duration that changes in population composition assume negligible importance.

The second reason for possible lack of representativeness is far more serious for a savings panel, and probably for other continuous panels as well, namely, panel mortality and nonresponse. Since a panel is essentially an extension of a one-time survey, the refusal and noncontact rates on the first interview, or wave, of a panel operation are certainly no less than those encountered in a corresponding one-time survey interview — and may be considerably larger if sample members are informed of the panel nature of the operation before the interview is carried out. On the second wave, nonresponse may be less than on the first wave, but it still tends to be sizeable, perhaps because only then do some respondents become fully cognizant of the meaning of the panel operation. Nonresponse on later waves usually declines sharply, although rarely to zero, because by that time the panel consists essentially of a “hard core” of cooperative respondents, at least so far as willingness to grant an interview is concerned.¹⁹

¹⁹ Nonresponse rates in panel operations of the Consumer Savings Project will be presented in Chapter VI. For a discussion of nonresponse rates in a different type of panel, see Robert Ferber, “Observations on a Consumer Panel Operation,” *Journal of Marketing*, Vol. 17, No. 3 (January, 1953), pp. 246-59.

Those dropping out of the panel do so for a number of reasons — sickness or death, moving away, dislike of the interviewer, boredom with the study, resentment of questions, and so on. Taken as a group, however, there is always doubt as to whether these people are similar in the characteristics under study to those who remain in the panel, particularly in the case of a savings panel. From a survey viewpoint these dropouts consist of two groups: the noncontacts and the refusals. Noncontacts are generally people who are not at home. In addition, in a savings survey they are likely to include very high-income, high-asset sample members — people who exert tremendous influence on the dollar volume of savings transactions and who, partly for this reason, are difficult to contact. In a savings, or any economic, panel, dropouts are also likely to include relatively high proportions of independent businessmen. Members of both of the latter groups may be willing to give one or two interviews at the start. However, partly because of pressures of time and partly perhaps because they may be more reluctant to discuss their financial position, they would seem more likely than other people to drop out of a panel operation (as will be corroborated in Chapter VI).

Similar inferences can be drawn about refusals in a savings panel. Refusals, more likely than noncontacts, will include people who do not care to discuss their finances, having perhaps taken some action in the recent past, or possibly planning to take one, that influences them to discontinue further participation. They are also very likely to be people for whom time is at a premium, and hence they are probably in high-income brackets or have their own business.

Progressive dropping out of people of this type is undoubtedly the principal cause of lack of representativeness in a savings panel, a type of nonsampling error which is clearly nonrandom and can increase with time. Item nonresponse may be a contributory factor: there is little doubt that respondents refusing to answer individual questions probably differ from other respondents, but it is not clear if this atypicalness increases with time.

Conditioning Effects

Does participation in a panel operation cause respondents to act or respond differently from what they normally would? If so, another form of nonsampling error has to be dealt with.

The basis for expecting an error of this form is that presumably panel members tend to become more conscious of their participation in the panel as time goes on, which manifests itself in altered attitudes or behavior. The resulting information supplied by these people is then atypical of the population group which they originally represented, and

thus the nonsampling error is more likely than not to be nonrandom as well.

Conditioning effects of this type are considered most likely in attitudinal studies, particularly studies relating to personal habits or to questions of social import. Thus, a panel operation on budgetary practices and durable goods purchases may induce participants to become more conscious of the desirability of budgeting, with the result that some of them begin to budget purchases largely as a consequence of their membership in the panel.

The extent to which conditioning effects may exist in a particular case depends on an evaluation of many facets of the operation and will require much experimentation before a conclusive judgment can be made. Anything likely to affect the respondent's replies has to be considered — the content of the questionnaire, the frequency of interviews, the approach taken by the interviewer (if one is used), the contents of any advance or introductory letter, information supplied to the respondent relating to the survey, and so forth. If the presence of conditioning effects is suspected, about the only way of proving it is to introduce new members into the panel periodically, ask the new and the old members the same questions, and then compare the results. In a large continuing panel operation this is not difficult to do, particularly since it is also a means of coping with the problem of representativeness, as was noted earlier.²⁰

Whether conditioning effects are likely to be present in a consumer savings panel of the type under discussion here is not clear. Since the focus of the operation is on saving and on saving habits, the attention of panel members is centered on this subject much more than would otherwise be the case. Certainly they are made more aware of saving alternatives and reasons for saving. But does this influence them to save more (or less) or to invest their savings otherwise than they ordinarily would have? Saving habits would seem too deeply ingrained to be affected appreciably by one interview on the subject every three or four months for a year. To be sure, there is always the possibility that the interview will remind a respondent of an action he had been planning to take and hence lead him to it, e.g., to convert an ordinary life policy to an endowment policy. Data presented in Chapter VI suggest that to the extent that such actions did take place they were not of major importance, considering the panel as a whole. The fact remains, however, that conditioning effects are an ever-present danger and that rotating panel membership is to be highly recommended in any continuing panel operation.

²⁰ For an example of this procedure, see Joseph Waksberg and R. P. Pearl, "The Current Population Survey: A Case History in Panel Operations," *Proceedings of the Social Statistics Section of the American Statistical Association* (Washington: American Statistical Association, 1964), pp. 217-29.

Restriction on the length of panel membership can be a positive force in maintaining panel member rapport and, as was noted earlier, serves to mitigate both principal sources of nonsampling error.

Beneficial Effects

Although not undertaken primarily for this purpose, a panel operation does offer interesting possibilities for the reduction of nonsampling errors. The continued contact with the panel members offers excellent opportunities for reconciling answers that are inconsistent, unclear, or possibly based on misinterpretation of the questions. To be sure, something along the same line can be done in a one-time survey by means of callbacks and supplementary letters, but it is hardly likely to be done to anywhere nearly so great an extent or with the degree of economy with which it can be done in a panel study — and generally it is not done at all. For studies in which extensive data collection is involved and errors of interpretation or transcription are not unlikely, this checking process can be very useful.

Improved rapport leading to the supplying of more complete and more accurate data is another major advantage of a panel operation with regard to reducing nonsampling errors. This is a particular advantage in a savings panel, in which the personal nature of the questions leads some panel members to fear ulterior motives, such as secret participation by the Internal Revenue Service. In such instances, only after two or three interviews is full cooperation likely to be obtained and complete data made available. At the same time, there are instances in which the reverse occurs, when rapport deteriorates possibly to a point at which the panel member drops out.

Apart from rapport, a major reason for improvement lies in the increased accuracy with which data are recalled on later interviews. This was particularly noticeable in a durable goods consumer purchase panel conducted some time ago, in which the average number of purchases reported increased consistently with length of panel membership.²¹ Membership on the panel, combined with advance notice of an interviewer visit, apparently motivated the panel members to recall more completely durable goods purchases made since the preceding interview.

This factor is undoubtedly operative with a consumer savings panel, even though panel members are continually asked to consult records rather than to cite figures from memory. Knowing this fact in advance tends to make records either more readily available or to give the respondent a chance to look up the figures in his records. Here again, the effect could be in the reverse direction, but this appears to be more the exception than the rule.

²¹ Ferber, *op. cit.*

From an overall point of view, perhaps the primary beneficial effect of a panel operation in dealing with nonsampling errors—in this case, response and noncontact errors—is to bring the problem more sharply into focus. With refusals and noncontacts, information is invariably available from prior waves to serve as a basis for a sound judgment of their effects. Similarly, the wealth of previously obtained information on panel members makes it much easier to detect response errors and even to pinpoint their causes. Despite deterioration of rapport with some panel members, the overall result turns out to be more accurate data both on a current and on an *ex post* basis.

General Comments

The foregoing review suggest that the data reliability problem is far more serious in a consumer savings survey than it is in most other types of surveys and even in most other economic surveys. The problem is one essentially of coping with nonsampling errors, particularly with response and noncontact errors. The latter types of error can be tremendously more frequent in savings surveys, producing serious biases and distortions in the survey data. For documentation of these tendencies, we turn to a consideration of the available studies.

II. AGGREGATIVE COMPARISONS

This chapter presents the few comparisons made of consumer survey derived aggregates for components of savings or saving with corresponding aggregates obtained from other sources. Since the accuracy of the latter aggregates is generally not known, the results of such comparisons have to be interpreted with considerable caution. In particular, they cannot indicate the precise degree of error in the survey data, and only in the most indirect fashion can they point to possible causes or reasons for error. They only serve to indicate the presence of error to the extent that the aggregates disagree and it is not likely to be clear which aggregate is in error by how much. Nevertheless, as we shall see, useful inferences are possible.

Detailed attempts to aggregate survey data on savings or on saving have as yet been made only in the United Kingdom and in the United States. Accordingly, this chapter focuses on such studies conducted thus far in these two countries.

United Kingdom Savings Surveys

Savings surveys were carried out in the United Kingdom during 1952-55. A primary objective of these surveys was to obtain data on asset holdings and on saving in various forms between the time of one survey and that of the next. It is therefore of particular interest to examine how well the survey estimates of these aggregates compare with those from other sources. Such a comparison is shown for total liquid assets and for the two primary categories of English liquid assets in Table 1. The definition of liquid assets in the United Kingdom is essentially the same as the definition used in this volume, including individuals' holdings of checking accounts, savings accounts in the various types of savings institutions, and British government bonds; holdings of currency, however, are excluded.

Three sets of survey estimates are shown in Table 1 — estimates based on the survey data alone, estimates based on weighting the survey responses by income level with independent estimates of the British income distribution computed from tax data, and estimates obtained by weighting

TABLE 1. COMPARISON OF SURVEY ESTIMATES OF AGGREGATE LIQUID ASSET HOLDINGS IN UNITED KINGDOM WITH INDEPENDENT ESTIMATES, 1952-55

Category	Date	Independent estimate (millions of pounds)	Survey estimate as a percentage of independent estimate		
			Un-adjusted	Weighted by income	Weighted by occupation
"National Savings" ^a	April, 1952	5,413	62	63	66
	April, 1953	5,316	56	60	60
	April, 1954	5,292	59	59	59
	June, 1955	5,408	64	65	67
Other liquid assets ^b	April, 1952	3,368	61	68	68
	April, 1953	3,577	47	54	53
	April, 1954	3,810	77	80	78
	June, 1955	4,037	67	68	73
Total liquid assets	April, 1952	8,781	61	65	67
	April, 1953	8,893	52	57	57
	April, 1954	9,102	66	68	67
	June, 1955	9,445	65	66	70

^a Defined as the sum of deposits in Post Office Savings Banks and Trustee Savings Banks, savings certificates, and British defense bonds.

^b Includes deposits in other savings banks, demand deposits, and shares and deposits in building societies and in cooperative societies.

Source: Adapted from M. J. Erritt and J. L. Nicholson, "The 1955 Savings Survey," *Bulletin of the Oxford University Institute of Statistics*, Vol. 20, No. 2 (1958), p. 127.

the survey responses by independent estimates of the British occupational distribution. The independent estimates of aggregate liquid assets are from institutional sources and, because of their nature, would seem to be reasonably accurate. However, it is not clear how closely they approximate individuals' holdings of liquid assets because the estimates include unknown amounts of such nonpersonal holdings as trust funds, for which there was apparently no reasonable basis for estimating the amount. On the other hand, the estimates also exclude loan stock in cooperative societies, which many respondents apparently included among their liquid assets and which could not be segregated.²²

Unless these nonindividual holdings are substantial, it is clear from Table 1 that the survey estimates greatly understate the independent estimates. In the case of "National Savings" (deposits in Post Office Savings Banks and Trustee Savings Banks, savings certificates, and British defense bonds), the survey accounts for barely more than 60 percent of the institutional aggregates. For other liquid assets (deposits in other savings

²² M. J. Erritt and J. L. Nicholson, "The 1955 Savings Survey," *Bulletin of the Oxford University Institute of Statistics*, Vol. 20, No. 2 (May, 1958), pp. 113-52.

banks, demand deposits, and shares and deposits in building societies and in cooperative societies), the survey estimates vary from 47 percent to 77 percent of the independent estimate. As a result, the survey estimates of total liquid assets are seen to understate the independent aggregate by roughly half to one-third.

Weighting by income or by occupation serves to increase the survey coverage somewhat more than does the use of unadjusted data. However, the degree of understatement is substantial and "it seems clear that there is considerable under-reporting of assets, even though we do not know the amount of nonpersonal holdings included in the independent estimates."²³

The extent to which the survey estimates of aggregate saving compare with independent estimates is shown in Table 2. As was done in Table 1, three sets of survey percentages are shown — unadjusted estimates, estimates obtained by weighting by independent estimates of the income distribution, and estimates obtained by weighting by independent estimates of the occupational distribution.

As is evident from this table, the accuracy of the survey estimates seems to vary substantially with the type of saving. The survey estimates of contractual saving (life insurance premiums, pension contributions, and repayment of mortgage debt) correspond very well on the whole with the independently estimated aggregates. However, the survey estimates of aggregate saving in the two forms of liquid assets and in the form of "miscellaneous" saving (primarily the saving of unincorporated business) invariably understate the independently estimated amount of saving or dissaving. The amount of saving or dissaving in "National Savings" is substantially understated. In the three years when saving in this form appears to have been negative, the actual amount of dissaving was overstated by anywhere from 150 to 350 percent, and in the one year when saving was positive, the survey figures indicated dissaving.

The picture is even worse for saving in the form of other liquid assets. The survey estimates of this form of saving are uniformly negative, whereas the independent estimates point to positive saving during all four periods studied. Survey estimates of "miscellaneous" saving also bear little relationship to the independent estimates. As often as not, the direction of saving indicated by the survey conflicts with the direction indicated by the independent estimates. In this instance, however, the validity of the comparison is in serious doubt, partly because the correspondence of the survey and independent concepts of business saving is not clear, and partly because the independent estimates were obtained as residuals, which are invariably subjected to appreciable margins of error.

It is interesting to note that with a few exceptions the survey estimates

²³ *Ibid.*, p. 129.

TABLE 2. COMPARISON OF SURVEY ESTIMATES OF AGGREGATE CONSUMER SAVING IN UNITED KINGDOM WITH INDEPENDENT ESTIMATES, 1952-55

Type of saving	Period	Independent estimate (millions of pounds)	Survey estimate as a percentage of independent estimate		
			Un-adjusted	Weighted by income	Weighted by occupation
Contractual ^a	1951-52	433	100	105	105
	1952-53	453	90	100	97
	1953-54	494	101	98	93
	1954-55	530	95	93	98
Change in "National Savings" ^b	1951-52	-75	352	352	333
	1952-53	-97	148	139	152
	1953-54	-24	300	254	262
	1954-55	116	-62	-121	-73
Change in other liquid assets ^b	1951-52	104	-162	-171	-194
	1952-53	209	-103	-113	-102
	1953-54	233	-82	-104	-79
	1954-55	224	-32	-20	-50
Miscellaneous saving ^c	1951-52	-314	-15	-37 ^d	-13
	1952-53	-335	-7	-13	-18
	1953-54	-403	6	-10	15
	1954-55	-617	16	15	18
Total saving	1951-52	148	32	90 ^d	32
	1952-53	230	52	64	71
	1953-54	300	96	94	77
	1954-55	260	92	98	105

^a Includes life insurance premiums, pension contributions, and repayment of mortgage debt.

^b Defined in footnotes to Table 1.

^c Primarily unincorporated business saving, but includes also net purchase of land, real estate, securities and annuities, property improvement, gifts and loans, and reduction of outstanding debt.

^d Excludes adjustment of business saving for underestimate of business expenses in 1952 survey because of incomplete coverage of the relevant questions in that year.

Source: Adapted from M. J. Erritt and J. L. Nicholson, "The 1955 Savings Survey," *Bulletin of the Oxford University Institute of Statistics*, Vol. 20, No. 2 (1958), pp. 129-40.

obtained by weighting by income or by occupation are no better than the unadjusted estimates. Also pertinent is the fact that experimentation with alternative means of estimating saving did not yield any better estimates than those shown in Table 2. The survey estimates in that table were based on the change in holdings as reported in the last survey in the given period.²⁴ Since respondents were also asked for holdings in each of these four surveys, saving could also be estimated as the change in holdings reported on two different dates, or, by hindsight, as the change in the previous year's holdings of respondents interviewed the year before. Only

²⁴ For example, the estimate of 1951-52 saving was based on respondent reports of saving in the survey conducted in April, 1952.

the first of these alternatives appears to have been tested, and it produced results no better than those shown in Table 2. From data in the original study,²⁵ it is hardly likely that the other two alternatives would have proved superior, because few substantial differences were apparent between current holdings reported one year and current holdings reported the following year. Clearly, failure to obtain complete and accurate data was a serious shortcoming of these surveys.

Savings Surveys in the United States

At the present time, three survey operations seek data on the saving of American consumers on a nationwide basis. Two are conducted only occasionally, whereas the other, although conducted annually, does not cover all types of assets and liabilities. The former are the expenditure-income studies of the United States Bureau of Labor Statistics and the survey of family financial characteristics of the Federal Reserve Board. Although conducted primarily to compile data to serve as weights for the cost of living index, the BLS surveys in recent years have also been seeking estimates of family saving by acquiring data on individual assets and liabilities. However, since these surveys are conducted only every 10 to 15 years, available data on consumer saving from this source are as yet very sparse. The Federal Reserve survey has been conducted in 1963 and 1964 and covers all assets and debts.

The third survey, the Survey of Consumer Finances, is conducted every year by the Survey Research Center, but only savings in selected assets and liabilities are covered each time. In addition, the coverage of different assets and liabilities has varied from year to year.

The two earlier surveys have been utilized at various times as a basis for estimating aggregate consumer saving in different forms; the Federal Reserve survey data are only beginning to be used for this purpose and no results are as yet available. The following pages review the results of these surveys.

Expenditure-Income Studies

The first attempt to collect complete data on saving and debt by means of a nationwide consumer survey appears to have been made as part of the 1935-36 Consumer Purchases Study. The interviews on this survey sought not only a complete breakdown of income and expenditures, but also data on changes in each of the assets and liabilities held by the family. However, because of the scope of the study and its emphasis on income and expenditures, the questions on saving were accorded secondary

²⁵ Erritt and Nicholson, *op. cit.*, p. 126.

importance, and indications are that they may have been treated in this manner during the course of the field work. In addition, the coverage of the sample was limited primarily to native-born husband-wife families. Furthermore, the representation, and apparently the responses, at the upper-income levels was so poor that these data were not considered reliable enough to serve as a basis for aggregation. Hence, saving aggregates were obtained by combining the survey responses for families earning under \$10,000 a year with estimates for (1) families in these income levels who had been excluded from the survey and for (2) families earning over \$10,000 a year in 1935-36.²⁶ The resulting estimates, therefore, represent only in part — and for many categories of saving, only a very small part — the responses obtained in the 1935-36 Consumer Purchases Study.

Perhaps for this reason, the survey aggregates for 1935-36 do not as a rule depart too far from the national income figures, as is shown in Table 3. Although comparisons between survey aggregates and external data are necessarily very rough, the comparison for 1935-36 is even more so because of the many assumptions that had to be made regarding the saving of that part of the population not covered in the survey. These various sets of assumptions led to eight different estimates for each aggregate.²⁷ Since the relative superiority of these different estimates is an unknown question, two accuracy percentages for each form of saving are shown in Table 3, covering the entire range of the survey estimates. This range is in many cases fairly large, and as a result definitive judgments on the accuracy of these estimates are not easy to make.

It is also difficult to ascertain the extent to which particular deviations may be due to the poor quality of the survey data and the extent to which they may be due to faulty assumptions regarding the saving of that part of the population not covered by the survey. Indeed, comparison of the percentages in Table 3 for 1935-36 with the accuracy percentages for 1941 would seem to indicate that the estimation procedures for the excluded parts of the population may have been primarily responsible for the relatively good comparisons obtained by the 1935-36 survey aggregates.

For this reason, the 1941 estimates probably provide a better indication of the quality of the savings data collected in these prewar surveys. These aggregates, which are based entirely on the nationwide study of family spending and saving in 1941-42, indicate that on the whole saving in the form of assets was underestimated but that dissaving (accumulation

²⁶ Dorothy S. Brady, "Family Saving, 1888-1950" in R. W. Goldsmith, D. S. Brady, and H. Mendershausen, *A Study of Saving in the United States*, Vol. 3 (Princeton: Princeton University Press, 1956), p. 158-78.

²⁷ *Ibid.*, Table H-13. There are two alternative estimates of the saving pattern of excluded relief and farm families and four alternative estimates of the saving and saving distribution of those earning over \$10,000.

TABLE 3. COMPARISON OF 1935-36 AND 1941 ESTIMATES OF AGGREGATE SAVING AND DISSAVING WITH NATIONAL INCOME ESTIMATES

Item	Survey estimate as a percentage of national income figure	
	1935-36 ^a	1941
Currency on hand.....	66 to 150	27
Bank accounts.....	87 to 110	-50 ^b
Checking accounts.....	134 to 191	315 ^c
Savings accounts.....	59 to 72	34
Investment in business.....	115 to 135	102
Nonfarm.....	-84 to 94	99
Farm business and real estate.....	-218 to -369 ^d	105
Investment in nonfarm real estate.....	33 to 79	88
Securities.....	-57 to -104 ^d	43
United States Government.....	n.a.	42
Other.....	n.a.	38 ^c
Insurance premiums paid.....	103 to 122	78
Insurance premiums surrendered.....	n.a.	15
Insurance policies settled.....	n.a.	-14 ^b
Mortgage loans lent.....	n.a.	-61 ^b
Social Security contributions.....	n.a.	78
Old age survivors contributions.....	n.a.	78
Unemployment insurance.....	n.a.	75
Total of matched asset items.....	135 to 159	61
Mortgage debt.....	116 to 91 ^c	92
Nonfarm debt to banks and insurance companies.....	114 to 176	495
Nonfarm charge accounts.....	111 to 162	130
Nonfarm installment balances.....	86 to 98	305
Other nonfarm debt.....	440 to 606	448
Total of liability items.....	85 to 128	168

^a 1935-36 comparisons based on two alternative estimates regarding saving of farm families not covered in the 1935-36 Consumer Purchases Study. The assumptions underlying the estimates were that the saving of these families (which included families on relief as well as nonrelief families which had neither a husband-wife present or were nonwhite) was, in turn, zero, and the same as that of eligible families at the same income level.

^b Survey estimate negative, national income estimate positive.

^c Both estimates negative.

^d Survey estimate positive, national income estimate negative.

Source: Adapted from Tables H-19 and H-20 of Dorothy S. Brady, "Family Saving, 1886-1950," in R. W. Goldsmith, D. S. Brady, and H. Mendershausen, *A Study of Saving in the United States* (Princeton: Princeton University Press, 1956), pp. 174-78.

of debt) was overestimated. Particularly striking is the very poor coverage of saving in the forms of currency, bank accounts, securities, and mortgage loans lent. In the latter case, the survey aggregate indicated dissaving, whereas the national income aggregate indicated positive saving. In the case of currency and savings accounts, only about one-third of the total saving in these forms could be accounted for by the survey.

On the other hand, dissaving in the form of personal loans, installment credit, and other nonfarm debt was substantially overestimated. The deviations are so large, being of the order of 300 percent to 500 percent, as to

TABLE 4. COMPARISON OF 1950 BLS-WHARTON ESTIMATES
OF AGGREGATE URBAN SAVING WITH SEC AGGREGATES
FOR TOTAL UNITED STATES SAVING
(BILLIONS OF DOLLARS)

Item	BLS-Wharton estimate, 1950 urban United States	Adjusted SEC aggregate, total United States
Cash and deposits.....	-3.6	3.9
Securities.....	.3	.4 to .8
Net business investment (before inventory valuation adjustment).....	2.2	0.0
Private insurance contributions.....	3.7	3.5
Life insurance premiums.....	4.2	5.7
Change in equity in nonfarm property.....	7.8	10.5
Improvement of nonfarm property.....	1.8	2.2
Nonfarm mortgages.....	-4.6	-7.2
Other consumer debt.....	-3.3	-4.4
Total saving, SEC concept ^a	5.9	11.3 to 11.7

^a Sums of individual figures will not add to total saving because some items were not included in the SEC concept and others are not shown because comparable data were not available from both sources.

Source: Adapted from Irwin Friend and Stanley Schor, *Who Saves?* published as a supplement to the *Review of Economics and Statistics*, Vol. 41, No. 2 (May, 1959), Table 4.

indicate that either the respondents misunderstood the questions or that the survey was not obtaining information comparable with the national income data.²⁸

The savings estimates obtained from the 1950 Survey of Consumer Expenditures appear to have been more accurate in many respects, although comparison is difficult because the coverage of that survey was restricted to urban areas, whereas the corresponding data on aggregate saving refer to both rural and urban areas. As a result, the survey estimates of saving in 1950, shown in Table 4, would be expected to fall considerably below the corresponding figures for the total nation for most assets. If the urban-rural income distribution were any indication of the urban-rural saving distribution, the 1950 survey should have accounted for about 73 percent of individuals' saving in that year.²⁹ However, since the distribution of wealth is generally more concentrated than the distribution of income, this percentage is undoubtedly a lower limit to the

²⁸ It has been suggested that much of this discrepancy may have resulted from misplaced income in the reporting process (*ibid.*, p. 178). Examination of the original questionnaire form, however, raises doubt regarding the validity of this explanation.

²⁹ This percentage is given in the monograph by Irwin Friend and Stanley Schor, *Who Saves?* published as a supplement to the *Review of Economics and Statistics*, Vol. 41, No. 2 (May, 1959), Table 4.

actual proportion of the nation's saving accounted for by the urban areas in 1950. It is also clear that saving in certain types of assets, such as in securities, is almost entirely accounted for by the urban population.

The comparisons in Table 4 indicate that for some types of saving the 1950 survey data appear to be highly reliable. This is the case for saving in the form of life insurance, change in equity in nonfarm property, improvement of nonfarm property, and nonmortgage debt. For other forms of saving not much can be said about the accuracy of the survey data, either because the concepts underlying the particular form of saving may not be the same in both cases (as is true of net business investment) or because considerable uncertainty surrounds the accuracy of the external aggregate estimate (as in the case of securities).³⁰

In estimating the amount of saving in the form of cash and bank deposits, the 1950 survey estimates differ completely from the apparent actual figures. Not only is the discrepancy huge, being of the order of \$7.5 billion, but the direction of change is completely missed (the survey indicating substantial dissaving, whereas other sources point to appreciable saving in these forms during 1950). Here, as was the case with so many previous surveys, the data appear to have been clearly inadequate. Since only one question was asked on all of these forms of saving — cash on hand; checking accounts; savings accounts in commercial and savings banks; and savings and other accounts in savings and loan associations, in building and loan associations, in credit unions, and in postal savings — it is not possible to determine the extent, if any, to which this discrepancy varied with these different types of saving. Whatever the situation in this regard, the principal cause may well have been the estimation of saving as the difference between current holdings and holdings as of a year earlier, accentuated by placing these questions toward the end of an interview that lasted anywhere from four to eight hours. If the respondent had much reluctance initially at divulging information on his assets, this reluctance undoubtedly would have been increased by the time these questions were asked and would have manifested itself primarily in underreporting of current holdings rather than underreporting of past holdings.

As a result of this understatement of saving in the form of cash and deposits, the survey estimate of total urban saving in 1950 comes to little more than half of the SEC estimate of the saving of all individuals in that

³⁰ The SEC estimate in the form of securities depends heavily on whether or not brokerage commissions paid on purchases and sales of outstanding issues are excluded from reported net purchases of securities. As noted by Friend and Schor (*ibid.*, pp. 85-86), the survey questionnaires do not indicate what course was followed. Friend and Schor chose to exclude these commissions from the SEC estimate of saving; if the figure had been included, the SEC estimate would have been doubled, as shown in Table 4.

year. Adjusted for the exclusion of the rural areas, the survey estimate rises to \$8.1 billion as compared with the SEC figure of \$11.3 billion. No appreciable improvement in the survey estimate is obtained if various regressions between saving and income are used to obtain an aggregate figure.³¹

Survey of Consumer Finances

Contrary to the procedure in the consumer expenditure studies, information on asset holdings has constituted a central portion of the annual interviews of the Survey of Consumer Finances. The general approach has been to ask first a number of questions on asset holdings, or on saving, as well as on income of the spending unit. Questions on debt were asked at times as part of the questions on assets and at other times to a large extent in connection with the questions on recent purchases of durable goods.

The coverage of the assets has varied appreciably since these annual surveys began. Nevertheless, almost all the annual surveys have asked for information on liquid assets, that is, on holdings in checking accounts, savings accounts, and in United States government bonds. In addition, questions have been asked occasionally on holdings of corporate stock and, on the debt side, on the amount outstanding by type of debt. As a result of these questions, and of the availability of roughly corresponding aggregates from the SEC estimates of individuals' savings as well as from the more recent Federal Reserve flow-of-funds data, a fair amount of information is available for evaluating the reliability of these figures.

Comparison of selected asset and debt holdings from the Survey of Consumer Finances of 1958 with corresponding estimates from the Federal Reserve flow-of-funds system is shown in Table 5. As is evident from this table, the pattern of correspondence between the survey estimates and the external estimates is fairly similar to that of the BLS consumer expenditure surveys and to that of the British surveys discussed earlier. Holdings of liquid assets, excluding cash, are understated substantially, particularly holdings of government bonds and of savings accounts. In each of these cases, less than half of the estimated amount in the hands of individuals was accounted for by the survey. The correspondence is somewhat better in the case of checking accounts, but even for this typically less-sensitive asset, the survey aggregate is about 30 percent below the estimated national total.

Although the flow-of-funds estimates of these aggregates are undoubtedly subject to errors, these errors are hardly likely to explain such sub-

³¹ Friend and Schor, *ibid.*, pp. 22-23.

TABLE 5. COMPARISON OF 1958 SURVEY OF CONSUMER FINANCES
ESTIMATES OF HOLDINGS OF SELECTED ASSETS AND DEBTS
WITH FLOW-OF-FUNDS ESTIMATES FOR END OF 1957,
TOTAL UNITED STATES

Category	Flow-of-funds estimate (billions of dollars)	SCF estimate as a percentage of flow-of-funds estimate
United States Government bonds.....	65	42
Checking accounts.....	32	69
Savings accounts.....	129	46
Liquid assets.....	226	48
Corporate stock.....	295 ^a	26 ^a
Nonfarm mortgages.....	101	97
Nonmortgage installment debt.....	34	56
Nonmortgage noninstallment debt.....	15	33 ^b
Total comparable debt.....	149	81 ^b

^a Early 1957 estimates.

^b This does not allow for the fact that charge accounts are excluded from the SCF coverage but are included in the flow-of-funds estimates.

Source: *Federal Reserve Bulletin*, Vol. 44, No. 9 (September, 1958), pp. 1047-50.

stantial differences.³² A major source of error appears to lie in the inadequate reporting of holdings by the survey respondents. Thus, according to a survey of demand deposit ownership conducted among financial institutions by the Federal Home Loan Bank Board, about 45 million checking accounts were owned by individuals and farm operators in early 1958, whereas the estimate of this number obtained from the Survey of Consumer Finances is 33 million.³³ On the basis of another survey conducted by the same agency among financial institutions, individuals and farm operators were estimated to own 22 million savings and loan share accounts at the end of 1957; the estimate obtained from the Survey of Consumer Finances for early 1958 is 8 million.³⁴ Thus there is little doubt that, as in the other surveys, many holdings of these assets are not reported, and probably other holdings are understated.

The substantial understatement of holdings of corporate stock may well be due to an even stronger manifestation of the same phenomenon. In addition, a large part of this discrepancy is undoubtedly due to the much greater degree of concentration of these holdings than of liquid assets.

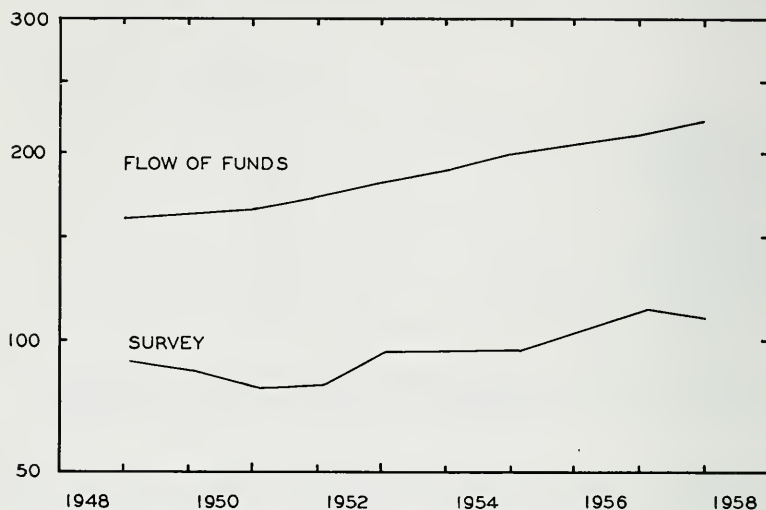
³² For a discussion of the possible sources of error in the flow-of-funds estimates of these aggregates, see the Technical Appendix to "The Financial Position of Consumers" in the *Federal Reserve Bulletin*, Vol. 44, No. 9 (September, 1958), pp. 1047-51.

³³ *Ibid.*, p. 1049.

³⁴ The actual discrepancy is somewhat less than is indicated to the extent that dead and forgotten accounts exist but cannot be picked up on a consumer survey.

FIGURE 1. ESTIMATED CONSUMER HOLDINGS OF SELECTED LIQUID ASSETS FROM SURVEY OF CONSUMER FINANCES AND FROM FLOW-OF-FUNDS, 1949-58^a

BILLIONS OF DOLLARS



^a Flow-of-funds estimates are for end of years 1948-57 inclusive; survey figures are for early in years, 1949-58, inclusive.

Source: *Federal Reserve Bulletin*, Vol. 44, No. 9 (September, 1958), p. 1047.

As has been noted by the Federal Reserve in an evaluation of this discrepancy,

The chance inclusion of a single large holding can have a tremendous impact on the estimated aggregate on consumer holdings. For example, one respondent in the 1953 survey reported owning about \$1 million in corporate stock. His case alone raised the estimated aggregate from \$37 billion to \$53 billion.

No doubt, part of the difference can be explained by this factor and other parts by the fact that the flow-of-funds estimates include holdings of personal trusts and of stock not publicly traded. The fact remains, however, that the survey estimates have consistently understated external estimates of consumer holdings of corporate stock as well as of liquid assets. If anything, the discrepancy rose between 1949 and 1958, as is shown in Figure 1. Over this period, the survey estimates have been roughly half the size of the flow-of-funds estimates and, with the base increasing over time, the dollar amount of discrepancy has tended to rise.

In the case of consumer debt, the survey estimates correspond very well in some instances with the flow-of-funds data. Thus, the survey estimate of nonfarm mortgage debt is very close to the flow-of-funds estimate based

on lender reports, as has apparently been the case in previous years also.³⁵ On the other hand, the survey estimate of nonmortgage debt is substantially lower than the flow-of-funds estimate. Both installment and noninstallment debt appear to be understated substantially, and this seems to have been the situation in previous years as well.³⁶ As with liquid assets, the evidence points to a widespread underreporting of debt as the principal cause of this discrepancy.

Turning to estimates of saving, the Survey of Consumer Finances presents a somewhat mixed picture. Total consumer saving as estimated from the Survey of Consumer Finances — usually as the change between holdings reported currently and holdings reported as of the previous year — agrees well with the aggregate obtained from other sources. However, this is because the substantial understatement of saving in the form of liquid assets is counterbalanced by a substantial overstatement in the estimate of saving in the form of nonliquid assets. Thus, the picture for the years 1947-50 is as follows:³⁷

	SCF	SEC
	(billions of dollars)	
Saving in liquid assets	-22.5	7.9
Saving in nonliquid assets	71.3	39.4
Total saving	48.8	47.3

A more detailed comparison of these two sets of data is shown for 1950 in Table 6. Here again, perhaps the most striking feature is the correspondence of the two aggregate estimates of total saving, despite tremendous differences in the estimates of saving in particular forms. Most striking of all is the difference of almost \$11 billion between the two estimates of aggregate saving in the form of cash and deposits, a difference almost as large as estimated total saving that year. Nevertheless, this discrepancy is offset by substantial discrepancies in the opposite direction in the estimates of saving in securities, in nonmortgage debt, and particularly in net business.³⁸ It is apparently because of such discrepancies that the Survey of Consumer Finances has found little use in the preparation of

³⁵ Technical Appendix to "The Financial Position of Consumers," *op. cit.*, pp. 1050-51.

³⁶ *Federal Reserve Bulletin*, Vol. 42, No. 7 (July, 1956), pp. 696-701.

³⁷ *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 Congress, 1st Session, July 19-26 and October 4 and 5, 1955, p. 282.

³⁸ It is interesting to note that these discrepancies would have been increased even further had the estimates been obtained by computing regressions of saving on income or by using external estimates of the consumer income distribution; see Friend and Schor, *op. cit.*, pp. 28-30.

TABLE 6. COMPARISON OF 1950 SURVEY OF CONSUMER FINANCES
ESTIMATES OF AGGREGATE SAVING WITH CORRESPONDING SEC
ESTIMATES OF TOTAL UNITED STATES SAVING
(BILLIONS OF DOLLARS)

Item	Survey of Consumer Finances estimate	Adjusted SEC estimate
Cash and deposits.....	-6.9	3.9
All securities.....	3.1 ^a	.4 ^a
United States government securities.....	1.1	-.6
Net business investment (before inventory valuation adjustment).....	5.1	0
Life insurance premiums.....	5.5	5.7
Consumer nonmortgage debt.....	-2.0	-4.2
Total saving, SEC concept ^b	12.6	11.3

^a Urban United States, estimate for all securities from p. 27 of this publication.

^b Sums of individual figures will not add to total saving because some items are not included in the SEC concept and others are not shown because comparable data were not available from both sources.

Source: Adapted from Irwin Friend and Stanley Schor, *Who Saves?* published as a supplement to the *Review of Economics and Statistics*, Vol. 41, No. 2 (May, 1959), Table 4.

national income estimates or in the flow-of-funds accounts,³⁹ although they have been used widely in studies of consumer behavior.

Overall Comments

In virtually none of the studies mentioned in this chapter are aggregate estimates of financial data available from external sources with known (small) margins of error and on a basis directly comparable with the survey data. In most instances, these external estimates come either from institutional sources or from national income studies, which are themselves based primarily on institutional reports. As a result, only rough adjustment can be made for conceptual differences in the two sources, and unknown portions of the observed discrepancies are attributable either to the adjustments that are made or to adjustments that should have been made but for which no basis existed for doing so. Despite these differences, the discrepancies in many cases are so substantial and consistent from one survey to another that there is little doubt regarding the inadequacy of the past survey data in various respects.

For one thing, survey aggregates tend to understate the apparent true figures in estimating most financial magnitudes. The underestimates appear to be relatively low for total saving, generally of the order of 10 to

³⁹ Arthur L. Broida, "Consumer Surveys as a Source of Information for Social Accounting—The Problems," paper delivered at the *Conference on Research on Income and Wealth*, 1959, and published in *Studies in Income and Wealth*, Vol. 26, National Bureau of Economic Research (Princeton: Princeton University Press, 1962).

20 percent. Estimates of total asset holdings appear to be understated much more, although the extent of understatement varies tremendously with the particular asset. This understatement appears to be largest for the income-earning assets and for income derived therefrom, particularly for the more liquid assets. Thus, in the case of income, the largest discrepancies percentagewise are encountered in reports of interest and dividend receipts, whereas with asset holdings the largest discrepancies appear in reports of holdings of savings accounts, government bonds, and corporate stock, with saving in these forms also understated substantially.

Although liquid assets and the income earned therefrom are understated, many nonliquid assets and saving in these forms appear to be overstated, for reasons not clear from the aggregative comparisons. Thus, saving in the form of investment in own business, in nonfarm real estate, in farm real estate, and in improvements on property tend, if anything, to exceed the corresponding estimates of these magnitudes from the national income data. Since national income estimates of these magnitudes are subject to considerable margins of error, the principal fault may not lie with the survey data. Certainly, at least part of the discrepancy seems to be attributable to the different ways in which investment in own business has been defined. Nevertheless, it would be surprising if there were not also errors in the survey data, particularly since the poorest cooperation is generally obtained from the entrepreneurial class.

Although failure to report and errors in data that are reported appear to permeate information from all economic groups, these errors appear to be more pronounced among surveys of the wealthier class. At the same time, the Federal Reserve estimates suggest that holdings of the more liquid assets are in many cases not being reported, and these instances are much too frequent to be accounted for only by the upper-income groups.

Although the aggregative comparisons cannot provide conclusive evidence on the causes of survey biases or on the relative effectiveness of different survey approaches, the data presented in this chapter do provide a number of important inferences regarding the value of different survey techniques. Since these inferences are to be tested more thoroughly in later chapters, it is of particular interest at this point to list them briefly.

One point on which the aggregative comparisons provide reasonably clear evidence is on the highly skewed distribution of assets and of savings. As a result, it appears that more reliable data on consumer financial behavior are obtainable if heavy oversampling of the upper-income groups can be incorporated into a survey design, as is the case with the Survey of Consumer Finances. Second, it is clear that relatively poor cooperation is obtained from the high-income groups, which tends to reduce the *effective* representation from these income levels at the same time that the

reports which are obtained possess a greater likelihood of containing substantial errors. This, then, is yet another basis for disproportionately high representation from these groups. For similar reasons, heavy concentration is needed on the entrepreneurial sector; in this case, not only is the problem of poor cooperation encountered, but both the meaning of the questions and the underlying concepts are easily misinterpreted.

A basis for optimism exists in the growing recognition of these problems, which led in more recent years to studies designed specifically to explore means of obtaining financial data from the very wealthy⁴⁰ and to much higher disproportionate representation in savings surveys from the upper-income groups, especially so in the recent surveys of consumer finances and in the Federal Reserve surveys. Such techniques promise to improve considerably the coverage of financial assets and also serve to throw more light on the reliability of the older studies.

The aggregate comparisons also suggest that in many surveys the wrong member of the consumer unit may be interviewed. The respondent on most of the past surveys has been the housewife, who in many cases is not well acquainted with the family's finances. This bias tends to be reinforced by the broad coverage of the questions, which have often related to all members of the family or consumer unit. Even if the respondent is familiar with his family's finances, he may not know how various family members are handling their savings, or how much they have, particularly other working members.

Placing the primary emphasis on subjects other than family finances is another danger in savings surveys. In such cases, the interviews tend to be so long and involved that by the time the subject of finances is reached both the interviewer and the respondent are fatigued, with the result that both are anxious to run through the questions rather than to concentrate on accuracy or completeness of information.

A crucial area on which the aggregative comparisons leave many questions unanswered is whether better estimates of saving are obtained by asking the respondent directly, or by asking him for current and past holdings, or by conducting two interviews and asking for holdings at each time. One thing is clear, namely, that estimating saving as the difference between income and expenditures tends to produce large errors. This is supported by most aggregative comparisons in which saving has been esti-

⁴⁰ For example, see George Katona and J. B. Lansing, "The Wealth of the Wealthy," *Review of Economics and Statistics*, Vol. 46, No. 1 (February, 1964), pp. 1-13. In this survey of families with incomes of \$20,000 or more, the top 10 percent of high-income people were found to own half of the total assets of the entire sample.

mated as a residual, and it was also demonstrated very well in an analytical study by Modigliani and Ando.⁴¹

In closing this chapter, it should be noted that even with their limitations consumer savings surveys have made many contributions to economic knowledge. The major such contributions made through the availability of such data have been listed by George Katona as follows:⁴²

(1) The paramount importance of "contractual saving" for the majority of individual families has been demonstrated, and it has been established that contractual saving is more stable and depends on other factors than those which determine noncontractual saving.

(2) Differences in saving habits by age and life-cycle stages have been indicated.

(3) It has been shown that during the postwar years most of dissaving was not associated with low incomes or with adversity but was connected with the desire to acquire "good things of life"; thus dissaving was a sign of prosperity and contributed to it.

(4) Owners and purchasers of United States government savings bonds, life insurance policies, and new and used cars have been characterized in some detail, along with borrowers.

(5) Information has been collected on the distribution of the burden of both installment and mortgage debt among different types of families and on the relation of that burden to income.

(6) Progress has been made in clarifying the influence of past income changes, as well as of liquid asset holdings on amounts saved by consumers.

(7) The high degree of concentration of amounts saved among relatively few families has been indicated.

Clearly, therefore, consumer survey data on savings can be very valuable, even with their limitations, and would be much more valuable if these limitations could be ameliorated. Especially in comparing changes in distributions or other statistics over time, the survey data can be very useful because the bias in the data in this respect appears to be fairly constant.

⁴¹ See Part I of Franco Modigliani and A. K. Ando, "The 'Permanent Income' and the 'Life Cycle' Hypotheses of Saving Behavior: Comparison and Tests," *Proceedings of the Conference on Consumption and Saving*, Vol. 2 (Philadelphia: University of Pennsylvania Press, 1960), pp. 51-73.

⁴² George Katona, "Federal Research Board Committee Reports on Consumer Expectations and Savings Statistics," *Review of Economics and Statistics*, Vol. 39, No. 1 (February, 1957), pp. 44-45.

III. VALIDATION STUDIES: DEBT

This is the first of several chapters devoted to the reviewing and summarizing of validation studies, mostly conducted as part of the Consumer Savings Project. These are studies in which external information serves as a basis for comparing the replies of the individual respondents on one or more aspects of their financial behavior. Such studies offer immense potential for the development of survey techniques because they are the only means of pinpointing errors due to response and to nonresponse. Moreover, they serve as a basis for evolving and testing hypotheses on the causes of these errors. Unfortunately, these studies are also perhaps the most difficult to make. Not only do many problems have to be resolved before the necessary external cooperation can be obtained, but other difficulties arise in the conduct of these studies and in resolving problems of comparability that arise in the analysis of the data.

The present chapter is concerned with an evaluation of those studies in which one or more forms of debt have been validated. Since this is the first chapter on validation, we begin with a general discussion of the manner in which these studies have been carried out and of the types of difficulties encountered. Then, a brief overview is provided of the various empirical validation studies that have been conducted, relating to assets as well as to debt. The third part of the chapter focuses on debt validation studies, with a final section evaluating the results.

Characteristics of a Validation Study

Offhand, the conduct of a validation study would seem to be fairly straightforward, once the necessary arrangements for securing the external data are made.⁴³ Actually, however, numerous problems arise in the course of such a study, many of which could destroy much of the value of the operation. A brief review of the principal such problems is therefore

⁴³ The process of securing the necessary cooperation of external sources need not concern us here. Suffice it to note that particularly in the case of financial data any arrangements for validation have to contain a variety of safeguards to protect the cooperating institutions and to preserve anonymity as much as possible.

indicated, so that the validation studies described in later sections can be more properly assessed.

First it is important, from an analytical point of view, to distinguish between two types of validation. One type, which we may call "ex ante," or "primary validation," consists of using one or more institutional sources as a basis for sample selection. The sample consists of holders of the asset at that source, usually selected on a probability basis, assuring representation of all types of holders and permitting unbiased estimates of the nature and extent of omissions and reporting errors (at least for that source).

Second, we may have "ex post," or "secondary validation," by securing permission to validate certain information after the interviews have been completed. The latter approach is the less desirable of the two, because only those items can be validated that are reported in the interview. As a result, it provides virtually no basis for estimation of the extent of complete nonreporting which, as will be shown later, is often the principal source of survey errors. Nevertheless, this approach does provide considerable information on the nature and frequency of particular types of error. It can also prove highly useful for supplementary purposes. Thus, to date it has not proved feasible to cross-validate holdings on an ex ante basis, because of the widespread dispersion of asset ownership and the difficulty of making the necessary arrangements.⁴⁴ More feasible is a combination of ex ante validation on one holding with ex post validation on others, which can provide valuable data concerning the interrelationship of reporting errors.

An important feature of a validation study is that the sample is selected and the necessary data are not usually obtained by members of the survey organization, but by members of the cooperating institution. Often the names selected by the institution are combined with names from more general sources, such as city directories or telephone books. This is a useful experimental feature, but at the same time it serves to introduce an additional source of confusion when it comes to matching information.

The fact that virtually all of this preliminary work is carried out by the clerical staff of the cooperating institution — people who generally have no survey background — means that biases due to sample selection and errors due to recording mistakes are more likely to occur than would otherwise be the case. This is particularly true since this work is usually assigned to the institution's staff in addition to their regular duties, and they are generally under pressure to complete the work quickly. The fact

⁴⁴ For a discussion of the implications of matching errors, see John Neter, E. S. Maynes, and R. Ramanathan, "The Effect of Mismatching on the Measurement of Response Errors," *Proceedings of the Social Statistics Section of the American Statistical Association* (Washington: American Statistical Association, 1964), pp. 1-8.

that this information is supplied on a one-time basis and that rechecks are not always possible does not make the situation any easier.⁴⁵

The field work on such studies also tends to be more difficult than is usually the case. The principal reason is that the interview generally has to be conducted not only with a specific family at a specific address, but with a specific member of that family. In some cases, insufficient information exists for the member to be specified, a problem that becomes all the more difficult because the sources of the names and the real purposes of the study cannot be divulged to the interviewers. The result is considerable leeway for mistaken identification.

No less difficult than the preceding stages is comparison of the information supplied by the respondent with that available from the external source. At this stage, all the possibilities for faulty matching or identification come together. Essentially, there would seem to be three principal sources of error. One source is in identifying the respondent. The interview might have been conducted with a different family of the same name (something not at all unlikely when interviewing in a large apartment building and dealing with fairly common names) or the respondent who should have been interviewed was not available for one reason or another. For example, a son who moves out of his parents' home may still have many of his assets and debts recorded at the previous address.

A second source of error lies in faulty identification of the holding being validated. This is particularly true of assets and debts of which more than one are held by many consumer units. For example, many families have more than one savings account, more than one debt, more than one life insurance policy, or more than one bond or share of stock. Since the external source consists invariably of a particular institution or group of institutions, the matching has to be carried out by individual holdings. Since these holdings often are not fully described, the matching problem may become partly a matter of guesswork. Thus, only one of two debts on a car may be known from an external source. If the respondent reports two debts, and if the name of the institution is not obtained, there may be little basis for deciding which debt corresponds to the external report. Even if the name of the institution is reported by the respondent, there is so much similarity of names in the financial world that reporting of an inaccurate name is always possible.

Third, there is the matching of holdings in time. If the external report is obtained just prior to the payment of an additional installment on a debt and the respondent reports the amount of the debt after this payment, differences will result solely for this reason. The problem is partic-

⁴⁵ Institutions will rarely allow outsiders to work with their records, not to mention the fact that after a relatively short time many records are put in dead storage and are virtually inaccessible.

ularly perplexing in the case of holdings where there is a large "float," as in the case of checking accounts. The balance reported by the respondent and the balance obtained from the external source may differ substantially, yet both may be fully accurate.

Many of these sources of error can be remedied if the validation study is carried out as part of a panel operation. Even then, however, problems remain and, as will be noted later, discrepancies can arise for many different reasons. The central point that emerges from this discussion, therefore, is that discrepancies do not automatically indicate response error. Indeed, as we shall soon see, many discrepancies uncovered in these studies are not attributable to inaccurate reporting at all, but rather serve to highlight the unique characteristics of the particular source from which the data are obtained.

Background Information on Empirical Studies

General information on the nature and objectives of past financial validation studies is presented in Table 7. For each study the table shows the sponsoring organization, the date of the field interviews, the scope and nature of the sample and of the population studied, and the items validated. In the latter case, holdings validated on an *ex ante* basis are distinguished from holdings validated on an *ex post* basis. In most cases, only one item of information was validated. As is evident from the table, to date four organizations have been responsible for the financial validation studies. Eight studies were undertaken as part of the Consumer Savings Project, of which four were subcontracted to the Survey Research Center; the other four were conducted from the project headquarters at the University of Illinois.

In addition to providing general background information on each of these studies, the table also brings out various salient features. One interesting characteristic is that all of these studies are relatively recent, none having been undertaken before 1954 and most of them having been conducted during the later 1950's and early 1960's. Second, most of these studies have been of limited scope. Few of the samples exceeded 300. This was partly due to limitation of financial resources and partly because of the feeling that the most information per dollar in this relatively unexplored area could be obtained by a sequential series of small, pilot studies rather than by two or three large studies. Also, a number of small studies enabled a variety of survey techniques to be evaluated, some by experimental design and others by observation. The sequential nature of the operation made possible the use of results from previous studies in selecting promising new approaches for testing and in the construction of appropriate testing procedures.

TABLE 7. LIST OF CONSUMER SURVEY FINANCIAL VALIDATION STUDIES

Organization	Study number	Date	Approximate sample size	Sample design	Coverage	Item validated ^a					General remarks	
						Debt (7)	Checking accounts (8)	Savings accounts (9)	Life insurance (10)	Other assets (11)		Income (12)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Federal Reserve Board.....	F1	1956	1,900	Stratified geographic	New car buyers in the United States in 1954-55	P	By-product of larger study
Federal Reserve Board.....	F2	1960	275	Judgment	Families in an urban area earning over \$20,000	P	P	
Netherlands Post Office.....	N	1958	3,300	Stratified by size of account	Holders of only one account, over 19 yrs. old, in four urban areas	P	
Survey Research Center.....	S1	1954	130	Stratified by size of account and account activity	Account owners in an urban area	P	
Survey Research Center (CSP)...	S2a	1957	25	Judgment	New car buyers in three Michigan counties	P	Short questionnaire focused on car purchase Follow-up of Study S2a
Survey Research Center (CSP)...	S2b	1957	90	Unrestricted	New car buyers in one urban area	P	
Survey Research Center (CSP)...	S2c	1957	35	Judgment	Reported new car buyers in 1956 Survey of Consumer Finances	P	Covered only those for whom state title offices reported liens. Reinterview study
Survey Research Center (CSP)...	S3	1958-59	170	Unrestricted	Large account holders in an urban area	P	
Survey Research Center (CSP)...	S4	1959	140	Unrestricted	Borrowers in two urban areas	P	Mail follow-up on attitude toward survey Panel study
Survey Research Center (CSP)...	S5	1959	150	Unrestricted	Large account holders in an urban area	P	
Consumer Savings Project.....	P1	1958-59	200	Stratified by economic area	Families in one urban area	S	S	Panel study
	P2	1959-60	300	Stratified by size of account	Account holders in one urban area	S	S	P	S	Panel study
	P3	1960-61	350	Stratified by size of account and lending agency	Debtors and account holders in a farm area	P	P	P	S	S	...	Farm panel study
	P4	1963	1,500	Stratified by size of account or of stock holding	Account holders and stockholders in one part of the nation	P	...	P	...	Same techniques as 1963 FRB study

^a P denotes primary sample-selection source; S denotes secondary sample-selection source.

Sources: F1: Arthur L. Broida, "Consumer Surveys as a Source of Information for Social Accounting — The Problems," paper delivered at the Conference on Research in Income and Wealth, 1959, and published in *Studies in Income and Wealth*, Vol. 26, NEBR (Princeton: Princeton University Press, 1962); F2: unpublished; N: W. Horn, *A Survey on the Reliability of Responses to an Interview Survey* (Netherlands Post Office Savings Bank, 1960); S1 to S5: John B. Lansing, Gerald P. Ginsburg, and Kaisa Braaten, *An Investigation of Response Error*, Studies in Consumer Savings, No. 2 (Urbana: University of Illinois Bureau of Economic and Business Research, 1961); and P1 to P3: unpublished.

A third characteristic is that increasing focus has been placed on securing heavy representation from the high-income levels or from large holders of the asset or debt being validated. This is not surprising in view of the findings of the aggregation studies covered in the previous chapter that these groups might account for most of the errors. The validation studies have not contradicted this finding, although the situation does not seem to be as clear-cut as was originally suspected. The fact remains that even if these groups do not account for a more-than-proportionate share of the errors, the size of their financial holdings warrants special emphasis on them.

Fourth, most of the studies have been conducted in urban areas. This has been partly because of cost considerations, partly because certain assets are concentrated heavily in urban areas, and partly because survey errors appear to be more frequent in such areas.

Fifth, most of the studies have been one-time surveys. In other words, the objective has been to obtain relevant financial information in a single interview, with no attempt to obtain information on the same assets and debts at some later time. The latter approach, which when extended fully becomes a continuous panel operation, has been undertaken only in some of the studies of the Consumer Savings Project. In one of the other studies, however, sample members were interviewed roughly nine months apart and asked for current holdings each time.

Sixth, most of the validation studies focused either on debt or on savings accounts. The reason for the former was primarily a matter of expediency. The necessary arrangements were relatively easy, and there is generally not much difficulty in inducing people to talk about their debts. Hence, this asset fits in well at the exploratory stage. The three pilot tests undertaken as part of Study S2 are a good example of this type of approach.

On the other hand, validation of savings accounts appears desirable for much the opposite reason. As will be shown later, it is one of the more, if not the most, sensitive assets. People generally are more reluctant to discuss their savings accounts than any other aspect of financial (or other) behavior. As a result, it provides an acid test of the reliability of a particular set of survey techniques. In addition, unlike similarly sensitive assets such as common stock, it is widely held, so that estimates can be obtained of the reliability of survey techniques on all different types of people.

Seventh, cross-validation — the validation of more than one type of holding per sample member — is comparatively rare. Although feasible theoretically, in practice the necessary collation of institutional records is often impossible to arrange. Different types of institutions tend to keep their records in different ways. This, combined with restricted access to

records and restricted time provided by institutional staff, makes the matching of records for validation purposes very difficult. In the only instance where more than one asset served as an original validation source, Study F1, all the data came from the same set of records.

An alternative procedure, followed in Study P3, was to use different types of records as original validation sources, each accounting for a separate portion of the sample. After the interviews were carried out, group-validation was attempted both among these assets and debts and among certain others indicated in the table.

Although not recorded in the table, it is pertinent to note that all of these studies, with the exception of S2c and N, covered in the interview situation not only the items being validated but also a number of other assets and debts. In the cases of the Federal Reserve and the Consumer Savings Project studies, the entire range of assets and debts was covered in most of the interviews.⁴⁶

More detailed information on the interviews and on the content of the individual studies will be presented in later sections. Certain details, however, can not be revealed in view of the confidential arrangements under which these studies were conducted. For this reason, no information is given, other than that shown in Table 7, on the location of the studies, the sources of external information, or the manner in which the validation data were obtained.

Debt

The remainder, and largest part, of this chapter is devoted to a review of the principal findings of known studies of debt validation. Primarily the highlights are presented for those studies¹ that have already been published, with references to the original sources for the supporting evidence. More detail is given on studies reported here for the first time, namely, three studies of the Consumer Savings Project.

National Survey of New Car Debt

The first known study validating debt was carried out in 1956 by the Federal Reserve Board. As part of a comprehensive investigation of consumer installment credit in 1956, the Federal Reserve Board undertook a national survey of means of financing new car purchases.⁴⁷ In connection with this survey, information on new car purchases and on the conditions under which loans were granted was sought from new car purchasers as well as from lenders. As a result, it proved feasible to match certain data from both sources.

⁴⁶ See, for example, the questionnaire in Appendix A.

⁴⁷ The results of this study were published in *Consumer Installment Credit, Part IV, Financing New Car Purchases* (Washington: Board of Governors of the Federal Reserve System, 1957).

The original sample consisted of a random selection of 550 new car registrants in each of the 24 months in 1954-55 from 112 stratified counties in 41 states. About 1,000 registrations were in the names of businesses and governments and were therefore eliminated. Of the remaining registrations, lender interviews were sought for all new car purchases which involved credit, and buyer interviews were sought for a randomly selected half of the roughly 12,000 registrations.

The validation was restricted to those registrations for which a buyer interview was sought, for which conclusive information was obtained from the lender that the new car purchase involved credit, and for which there seemed little doubt of the accuracy of the data obtained from the lender. Altogether, this amounted to about 6,500 registrations.

The actual interviews were conducted during June and July of 1956. The questionnaire focused heavily on new car purchases, on the reasons and circumstances under which the purchases were made, and on the use of credit and the conditions under which credit was obtained. On the average the interviews lasted 45 minutes.

Considerable pressure was placed on the buyers to cooperate, and they were sent an advance letter mentioning that "the study was being made at the request of the President and interested committees of Congress." Nevertheless, the overall response rate on the buyer interviews was only 72 percent of the 6,444 attempted. Most of the nonresponse rate (15 percent of the 28 percent) was due to the long time lag between the date of the new car registration and the date of the interview, by which time many of the buyers had moved out of the area and could not be located. Noncontacts of people living in the area were responsible for roughly half of the remaining nonresponse, with the other half resulting from refusals.⁴⁸

Matching lender reports were obtained from 90 percent of the 6,389 credit purchases for which such information was sought. Roughly one-third of the nonresponse was due to refusals and the other two-thirds to the inability to locate the necessary records, which in many cases had been discarded because the accounts were already closed. As a result, roughly 3,000 lender interviews were available which involved the use of credit for which a buyer interview was also attempted.

These interviews are the basis for the validation analysis, a summary of which is shown in Table 8. The four items of information for which data are presented in this table served as the basis for the analysis of response and nonresponse errors. These are the new car price, the total amount borrowed, the amount of the principal loan (which would be the same as the total loan if there were only one loan), and the monthly payment. The approach taken in this table is similar to that followed in the

⁴⁸ *Ibid.*, p. 122.

TABLE 8. RESPONSE AND NONRESPONSE ERRORS IN NATIONAL SURVEY OF NEW CAR BUYERS (STUDY F1), 1956

Item	New car price	Total loan	Principal loan	Monthly payments
1. Sample size.....	1,655	1,918	1,836	1,814
2. Average "actual" value of validated item ^a	\$2,886	\$2,038	\$1,735	\$76
Nonrespondents: sample averages as percentage of "actual" value				
3. Noneligibles.....	101	107	106	108
4. Noncontacts.....	106	102	102	105
5. Refusals.....	108	105	105	107
6. Respondents: sample average as percentage of "actual" value.....	99	98	98	97
7. Nonreporters: percentage of respondents not mentioning item.....	1	7	.. ^b	.. ^b
Reporters: response errors				
8. Average percent of error ^c ...	1.3	4.0	4.9	0
9. Percentage overstating by 10 percent or more.....	10.1	6.2	11.5	6.0
10. Percentage within 10 percent.....	76.4	74.7	64.9	88.4
11. Percentage understating by 10 percent or more.....	13.5	19.1	23.6	5.6
Allocation of error by percent				
12. To nonrespondents.....	28.2	28.1	28.5	29.3
13. To nonreporting of respondents.....	26.8	78.0	48.7	141.7
14. To inaccurate reporting of respondents.....	45.0	-6.1	22.8	-71.0

^a The "actual" values in all instances are estimated from the lender reports.

^b Not available, but reported to be between 1 and 7 percent.

^c Lender report-respondent report

Lender report

Source: Arthur L. Broida, "Consumer Surveys as a Source of Information for Social Accounting—The Problems," paper delivered at the Conference on Research on Income and Wealth, 1959, and published in *Studies in Income and Wealth*, Vol. 26, NEBR (Princeton: Princeton University Press, 1962), Part IV.

original study: the lender figure was considered to be the correct figure for analysis unless there was clear evidence to the contrary. In effect, therefore, these figures are accepted as the "actual" figures, and deviations of the respondent reports were computed using these figures as the base.

Such deviations, converted for each segment of the sample as a ratio, are shown in lines 3-6 of Table 8. As is evident from these data, neither the errors due to nonresponse (lines 3, 4, and 5) nor the response errors (line 6) are particularly large—at least, they are not comparable with the error magnitudes encountered in the previous chapter. Nevertheless, certain patterns are apparent. Thus, there is a clear tendency for the figures for the noncontacts and the refusals to exceed the overall sample

average. These differences, which are statistically significant at the .05 probability level, are most pronounced for the refusals and for the non-eligibles. (In the latter case, however, it is puzzling to note how much smaller the deviation of the new car price is than the deviations of the other three items.)

On the other hand, the reports of the respondents tend to understate slightly, although consistently, the overall sample averages for all four items. Part of the reason for this understatement lies in the failure of some respondents to report the purchase or the debt at all. The extent of such nonreporting, shown in line 7 of Table 8, varies from 7 percent of the respondents in the case of the total loan to barely 1 percent for the purchase of the new car itself.

Data on the other component of response error — errors in those debts that were reported — are provided in line 8 of the table. The error figures in this line are restricted to those instances in which information on the particular item was obtained on a comparable basis from both the buyer and the lender. As the data in this line show, the average monthly payment coincided exactly (at least to the nearest dollar) with the figure reported by the lender. Furthermore, no appreciable bias in the distribution of errors is perceptible, since roughly 6 percent of the respondents understated the actual monthly payment by 10 percent or more and the same proportion overstated the figure by this margin.

On the other three items, however, the response errors are in the direction of understatement. The response error on the purchase price of the new car is the lowest of the three, coming within 1.3 percent of the figure reported by the lender. The tendency toward understatement is brought out by the positive sign of this error as well as by the fact that the proportion of respondents erring by 10 percent or more on the purchase price is considerably higher than in the case of the monthly payment. Nevertheless, more than three-fourths of the respondents were within 10 percent of the lender's figure.

Appreciably higher errors were obtained on the amounts of the loans, and in both instances pronounced tendencies toward understatement are apparent. Thus, the amount of the total loan was understated on the average by 4 percent, and the principal loan was understated by almost 5 percent. In addition, almost one-fifth of the respondents understated the total loan by 10 percent or more and the same was true of almost one-fourth of the respondents for the principal loan. In contrast, overstatements were less than half as frequent.

As Broida points out,⁴⁹ the much higher accuracy of a report of the

⁴⁹ Broida, *op. cit.*, p. 66.

monthly payment is not surprising, considering the fact that this figure is brought to the attention of the debtor every month. On the other hand, the price of the new car is more or less a matter of past history, the purchase having taken place anytime between 6 months and 30 months prior to the interview. The same is true of the amounts of the loans. The still larger errors in those cases could be due to various reasons. One is that some people might have hesitated to report the full extent of their borrowing. Others might have by this time more or less unconsciously rationalized the amount of debt to a lower level. In still other cases, lack of comparability between the reports of the lender and those of the buyer might have been responsible (although it is not clear why this should lead to a persistent tendency toward understatement).

The last three lines of the table (lines 12-14) provide estimates of the relative importance of these different types of errors. These estimates are necessarily rough, partly because all the required data were not available and partly because of the various ways in which such estimates can be made. Nevertheless, these estimates furnish striking indications of the relative importance of the different types of errors, which are not likely to be altered significantly by changes in the basic assumptions. Furthermore, this general approach toward allocating errors enables useful comparisons to be made between different studies.

For the present purposes, we are concerned with segregating the error in the sample estimate of the mean value of each of the four validated financial items among three causes: error due to inability to interview all sample members, error due to failure of those interviewed to report the validated item, and error due to inaccuracies in the amounts given by those respondents who did report the validated item. Let us designate by N the total sample; N_s , the number of nonrespondents; N_o , the respondents not reporting the item; and N_1 , the respondents who did report the item. A_i denotes the corresponding "true" average values of the validated item for segment i , and X_i is the computed average for that segment based on the interview data. Then, the actual average value of that item for the entire sample is:

$$(3.1) \quad A = \frac{N_s A_s + N_o A_o + N_1 A_1}{N},$$

and the corresponding interview-derived estimate of A is:

$$(3.2) \quad \text{Estimate of } A = X = \frac{N_s X_s + N_o X_o + N_1 X_1}{N}.$$

In practice, no figures are reported for X_o , which is identically zero. Also, the value of the item under study is not known for the nonrespondents and is therefore estimated, implicitly or otherwise, from the informa-

tion supplied by the respondents, usually as the mean value of the latter.⁵⁰ Hence, we take as the estimate of A_s the value of X_r , the mean value reported by the respondents ($= N_1 X_1 / (N_0 + N_1)$). From a computational point of view, therefore, the estimate of A becomes:

$$(3.3) \quad \text{Estimate of } A = X = \frac{N_s X_r + N_1 X_1}{N}.$$

The error (E) in this estimate is, from the above:

$$(3.4) \quad E = A - X = [N_s(A_s - X_r) + N_0 A_0 + N_1(A_1 - X_1)]/N.$$

The right-hand side of this equation partitions the total error into the three desired components, and the percentage distributions of these components are shown in lines 12-14 of Table 8.⁵¹ The results indicate that with the exception of new car price, the principal source of error in this study was nonreporting by respondents, even though such nonreporting was low. In the case of new car price, nonreporting was virtually absent, which explains the relatively low error attributed to that item for this cause.

Errors due to faulty estimates of the mean value of the validated item for the nonrespondents accounted for roughly 30 percent of the total error, in all cases tending to reduce the sample estimate, as was noted previously. The effect of inaccurate reports of respondents varied substantially from one item to another. For the total loan and for monthly payments, inaccurate reports partially offset errors for the other two causes, particularly in the case of the monthly payments. On the other hand, for new car price and for principal loan, inaccurate reports served to add to the total error.

All things considered, the results of this study did seem to indicate that, except for cases in which nonreporting was virtually nil, it represented the largest part of the field errors. The fact remains, however, that this survey was carried out under special circumstances and was restricted to an easily discussed aspect of financial behavior.

Let us now consider the extent to which the biases shown in Table 8 distort the *reliability* of the estimate of the mean rather than the estimate itself. To do so, we make use of Equation (1.3) from Chapter I to compute the true variance of the mean as the sum of the variance of the mean

⁵⁰ A somewhat more refined approach is to estimate the value for each nonrespondent as the mean value of respondents having the same set of prior-determined relevant characteristics. Such a modification could be included in the present framework, but is unnecessary for the present purposes.

⁵¹ This technique may be used to partition the total error into any number of relevant components, provided that the necessary data are available, including allowance for incorrect estimates of the population distribution (in this case, the N_i 's). It will be used for such purposes in later chapters.

of the observed values and the square of the bias. The bias is estimated as the difference between the average value obtained from the lender reports (line 2 of Table 8) and the average figure computed from the data supplied by the respondents.

The results of these computations are shown in the following tabulation together with estimates of the "bias multiplier," $k(E/\sigma_{\bar{y}})$:

	<i>Bias</i>	<i>k</i>	<i>Standard error of mean</i>	
			<i>Apparent</i>	<i>True value</i>
New car price	\$ 78	4.5	\$17.28	\$ 79.89
Total loan	128	8.1	15.83	128.97
Principal loan	128	9.6	13.38	128.70
Monthly payments	2	3.0	.66	2.10

Clearly, the bias term, although small, is still many times larger than the estimated variance of the mean. Indeed, the large size of the sample in this case acts to magnify the importance of the bias, serving to reduce sampling errors to almost minute proportions while having virtually no effect on the bias itself. As a result, the *apparent* standard error of the mean for the buyer reports, reproduced from the original source,⁵² actually understates tremendously the true standard error of the mean. Indeed, in this survey the principal effect of the bias is to distort completely estimates of reliability while producing little bias in the estimates themselves.

We may next move one step further and ask: what proportion of the apparent standard error of the mean is attributable to response error? In other words, what would be the true standard error of the mean in the absence of response error? Although data are lacking to make exact calculations, on the basis of Equation (1.7) on page 14, there is little doubt that in this case the difference is virtually nil. The biases noted in Table 8 are so small that the increase in sampling variance due to response error is also likely to be very small.⁵³ Hence, the principal effect of response

⁵² Broida, *op. cit.*, Table 3, p. 372.

⁵³ This is based on the following reasoning. As noted previously, the true values and the response errors tend to be positively correlated. Using the same terminology as in Chapter I (pp. 11 and 12), assume as a rough approximation that:

$$e_1 = g x_i, \text{ where } 0 < g < 1, \text{ and}$$

$$\sigma_e^2 = g^2 \sigma_x^2.$$

Substituting in Equation (1.5) on page 13:

$$\sigma_{\bar{y}}^2 = \sigma_{\bar{x}}^2 (1 + g^2 + 2g r_{xe}).$$

To judge from Table 8, a liberal estimate of g (for present purposes) is .05. Similarly, r is hardly likely to exceed .5, based on similar correlations presented in this and later chapters. Substituting:

$$\sigma_{\bar{y}}^2 = \sigma_{\bar{x}}^2 (1 + .0525).$$

Thus the increase in the estimated standard error due to response error is not likely to exceed

$$\sqrt{.0525} \sigma_{\bar{x}}, \text{ or 2.3 percent.}$$

error on the reliability of the estimates is to distort completely the standard error estimate, and any confidence intervals based on it, rather than to inflate the estimate.

Further light on the nature of the nonsampling errors has been obtained by chi-square tests carried out in turn to ascertain the extent to which different variables were associated with error in the amount of total loan and in the amount of principal loan; these variables reflected respondent characteristics, interviewer characteristics, and characteristics of the interview, and the dependent variable in all of these tests was the percentage error in the amount of the loan.⁵⁴ Percentages rather than absolute numbers were used because of a pronounced tendency for the absolute error to increase with the amount of the loan.

In all of these tests the percentage errors were subdivided in three categories: underreported by more than 1 percent, accurate within 1 percent, and overreported by more than 1 percent. In the light of later studies, this distinction at 1 percent seems arbitrarily fine, and it may have served to obscure characteristics of those making really substantial errors. The fact remains, however, that a high degree of accuracy was obtained in this study, which renders this 1 percent boundary line a much more sensitive discriminant of error than it otherwise would have been.

The tests revealed a number of characteristics to be related significantly (at the .05 probability level) to one or the other of these variables. For total loan, the following variables were significant:

Race. Negroes were accurate more frequently than whites. It is not clear, however, whether or not this might be due to income and occupation differentials rather than to race as such.

Consultation of records. Many more of those consulting records were accurate within 1 percent than of those who did not consult records.

Occupation of head. Those in laboring occupations were the most accurate and the self-employed were the least accurate.

Type of lender. Borrowers from sales finance companies were more frequently accurate than borrowers from banks or trust companies. Here, too, the difference may reflect more basic factors, such as the differing extent of patronage of these institutions by various economic groups.

Education of interviewer. Oddly enough, interviewers who had attended but not completed college received a higher proportion of accurate responses than interviewers with either more or less education.

⁵⁴ Rikumo Ito, "A Multivariate Analysis of Response Errors," unpublished Ph.D. dissertation, University of Illinois, 1962; and "An Analysis of Response Errors: A Case Study," *Journal of Business*, Vol. 36, No. 4 (October, 1963), pp. 440-47.

Age of interviewer. Those in the 36-45 age group produced the highest frequency of accurate responses.

Only three variables proved significantly associated with error in the report of the principal loan:

Consultation of records. The same marked relationship was apparent as with total loan.

Region. Those in the Northeast and in the North Central areas tended to be accurate more frequently.

Date of interview. A lower frequency of accurate reports was produced on later interviews. This is not surprising because later interviews are generally either with people who could not be contacted previously or with those who have for one reason or another put off being interviewed and are likely to be less cooperative.

Particularly noteworthy is the fact that virtually all of the statistically significant relationships were characterized by a single attribute: the differences occurred entirely among the percentages of respondents that were accurate and that underreported. Virtually no differences were observed in the percentage of respondents overreporting either principal loan or total loan. This would suggest that in this particular study both the intentional as well as the unconscious tendencies to misreport were entirely in the direction of understating the true figure. Since, as was noted previously, the frequency of underreporting was substantially higher than the frequency of overreporting, the result was an understatement of the aggregate, albeit by a small percentage.

Variables not significantly related to the frequency of error in either case included liquid assets, income of the respondent, interviewer attendance at a training session, whether the interviewer was a supervisor or not, and the number of months prior to the interview that the car had been purchased.

Only one variable, consultation of records, was associated significantly with the frequency of error in both loan amounts. However, both occupation of head and age of interviewer were almost significant at the .05 level with regard to principal loan, and region was almost significant with regard to total loan.

Exploratory Studies of New Car Debt

With funds supplied by the Consumer Savings Project, the Survey Research Center of the University of Michigan carried out during 1957 three exploratory studies on the nature of response errors on new car debt. All of these studies were of limited scope, each designed to test particular hypotheses. The first one, Study S2a, consisted of 25 interviews in three Michigan counties during January-February, 1957, with people

who had incurred debt in buying a new car in 1956. The questionnaire was short, focusing on new cars and on new car debt, and lasted on the average about 20 minutes.⁵⁵ The one interviewer employed was aware of the purposes of the study and was informed of the respondent's debt situation before the interview.

The second Study (S2b) followed up on the previous one with interviews of a probability sample of 105 owners of new car debt in Chicago in mid-1957. In addition to seeking further information on the nature of response error, a split-run design was used to test two hypotheses: (1) that response error would depend on whether or not the interviewer knew the amount of the loan and (2) that response error would depend on the length of the questionnaire.

To test these hypotheses, the sample addresses were divided by random methods into four equal segments, and each interviewer was given an equal number of interviews in each segment. These segments were:

<i>Group</i>	<i>Interviewer's knowledge</i>	<i>Length of questionnaire</i>
1	Knew amount of loan	Long questionnaire
2	Knew amount of loan	Short questionnaire
3	Did not know amount	Long questionnaire
4	Did not know amount	Short questionnaire

Before interviewing sample members in Groups 1 and 2, interviewers were told the amount of the loan. They were not given such information in interviewing sample members in Groups 3 and 4 (although they doubtless were aware that these people did possess car debt).

The questionnaire used on Groups 2 and 4 was considerably shorter than that used on the other two groups, requiring about half an hour on the average as compared with almost one hour for the longer form. Like the shorter questionnaire, the longer form focused on new cars and car debt but sought information on other debts as well.

The third of these three studies (S2c) was undertaken concurrently with the others, to obtain additional data on response errors on car debt. It consisted of searching through the completed questionnaires of the 1956 Survey of Consumer Finances for those respondents who had reported a new car purchase. For such names, an attempt was made to obtain from the title office in the appropriate state information on whether or not there was a lien on the purchase and, if so, the amount.

Offhand, results obtained from such a procedure would seem of limited applicability because respondents who had not reported such a purchase could not be included, as was also true of respondents residing in states in which the title offices were unable or unwilling to supply the necessary information. In this sense, errors obtained from such a check might be

⁵⁵ Lansing, Ginsburg, and Braaten, *op. cit.*, p. 21.

construed as lower limits to the true amount of response error in the data. From a different perspective, however, such a check could serve to throw greater light on the magnitude of response errors because of the more realistic circumstances under which the interviews were carried out. Thus, the coverage of the questionnaire in the 1956 Survey of Consumer Finances was very broad, covering all debts in addition to new car debt and also covering a wide range of assets and various other aspects of consumer saving behavior. The interviewers clearly had no forewarning of such a check, because it was not even contemplated at the time that the 1956 survey was conducted. Unfortunately, however, this check produced only 33 comparisons, limited to a few states.

A summary of the extent and nature of response errors obtained in these three studies is presented in Table 9. In some respects, the results differed considerably from one study to another, but nevertheless certain basic characteristics emerge. Thus, the average debt of the nonreporters appears to be generally considerably below the average debt of those who did report. Despite this fact, however, the average reported debt for all the respondents, which would presumably be taken as the estimate of the population average debt, understates the average actual debt of all respondents in all three studies (line 10), substantially the same as in the check of the 1956 Survey of Consumer Finances. The principal reason for these understatements is respondent nonreporting which, as shown in line 11, varied from less than 4 percent to over 40 percent of the total interviews in these three studies.

In a similar fashion, the average response error (for respondents who reported debt) varied from virtually nothing in Study S2a to almost 10 percent in Study S2c. In the latter case, a substantial tendency toward underreporting is evident from the fact that more than one-third of the sample members understated the actual debt by 10 percent or more; this error served to contribute to the large overall net error of Study S2c. The same tendency toward understatement is apparent in the other two studies, even though the average error in Study S2b points toward overstatement (due primarily to two unusually high overestimates of the actual debt).

The allocations of error in the last four lines of the table are not comparable between studies because the nonrespondent problem was not considered in Studies S2a and S2c.⁵⁶ Nevertheless, these results support the

⁵⁶ It was not considered fully in Study S2b either, because no data were compiled on the debt of those sample members who refused or were not contacted. The nonrespondents covered in Table 9 are only "item nonrespondents," people who were interviewed but did not answer (or were not asked) the question on car debt. The allocation of error for Study S2b is, therefore, as for the other studies, also incomplete. Note, however, that omission of the noninterviews does not necessarily imply that the error due to nonrespondents is underestimated. The

TABLE 9. RESPONSE ERRORS IN SURVEYS OF CAR DEBT, 1957,
AND CASH LOANS, 1959

Characteristic	Survey Research Center studies			
	S2a	S2b	S2c	S4 ^a
1. Method of sample selection.....	Judgment	Probability	Judgment	Probability
2. Sample size, initial.....	25	105	33	133 ^b
3. Noncontacts.....	... ^c	12	... ^c	27
4. Refusals.....	... ^c	2	... ^c	12
5. Respondents.....	25	91	33	94
6. Average actual debt of all respondents	\$2,075	\$1,126	\$1,516	\$23.12
7. Average actual debt of respondent nonreporters.....	1,642	553	1,428	23.24
8. Average actual debt of respondent reporters.....	2,090	1,526	1,544	\$22.95
9. Average reported debt of all respondents (including nonreporters)...	2,005	1,057	1,059	9.53
10. Overall net error: 1-(line 9)/(line 6)	3.5	6.2	30.1	59.0
11. Nonreporters: percentage of respondents not mentioning debt...	4.2	40.7 ^d	24.2	60.6
Reporters: response errors				
12. Average percentage of error.....	.2	-7.7	9.5	-2.1
13. Percentage overstating by 10 percent or more.....	4.2	14.8 ^e	0	10.8
14. Percentage within 10 percent.....	87.5	66.7 ^e	64.0	81.1
15. Percentage understating by 10 percent or more.....	8.3	18.5 ^e	36.0	8.1
Allocation of error by percent				
16. To nonrespondents.....	... ^c	-83.9	... ^c	... ^c
17. To nonreporting of respondents....	94.8	562.3	75.8	103.7
18. To inaccurate reporting of respondents.....	5.2	-378.4	24.2	-3.7

^a Dollar figures are weekly payments rather than amount of loan; latter was not validated.

^b Excluding 5 respondents no longer in debt at time of the interview.

^c Not applicable or data not available.

^d Includes 6 instances in which debt identification is not clear.

^e Restricted to 54 loans for which loan reported appeared to be the same as that validated.

Source: J. B. Lansing, G. P. Ginsburg, and Kaisa Braaten, *An Investigation of Response Error*, Studies in Consumer Savings, No. 2 (Urbana: University of Illinois Bureau of Economic and Business Research, 1961), Chs. III and VI and unpublished data sheets.

principal finding obtained from the Federal Reserve data that the primary cause of error was nonreporting by respondents. This is by far the principal cause of the underestimates yielded by all three studies. Particularly interesting in this respect is Study S2b, where nonreporting would have increased the error far more substantially than was actually the case, had it not been for the offsetting overstatement in the debts that were reported by the respondents and in the estimate of the average debt of the nonrespondents.

magnitude of this error depends not only on the number of such sample members, but also on the difference between their average debt and that of the respondents. In this case, the latter was likely to be much smaller than that obtained only from the item nonresponses.

The two experiments undertaken in Study S2b led to inconclusive results. Interviews for which advance knowledge was given to the interviewers did produce information somewhat more accurate than that obtained in other cases, but the results were not statistically significant. Similarly, there was some evidence that the short questionnaire was more successful than the long questionnaire, but again the results were not significant. By hindsight, the results of both experiments may well have been confounded by interviewer perception of the purposes of the study.

A more important finding of Study S2b was the substantial differences obtained in the reporting of primary loans versus secondary loans. Thus, primary loans appear to have been reported in at least 70 percent of the interviews where they were encountered, whereas the same was true of less than half of the secondary loans. Considerable confusion seems to have been encountered in obtaining data on secondary loans because they do not appear to have been distinguished very clearly in respondents' minds.

Turning to the effect of the bias in these three studies on the reliability of the estimates, the following tabulation shows that only in Study S2c is the true standard error of the mean substantially larger than the standard error computed from the interview data.

	<i>S2a</i>	<i>S2b</i>	<i>S2c</i>
$k = E/\sigma_{\bar{y}} \dots \dots \dots$.43	.63	3.2
$r \dots \dots \dots$.11	.002	.24
Apparent standard error of mean ($\sigma_{\bar{y}}$) . . .	\$163	\$110	\$145
True standard error of mean (\sqrt{MSE}) . . .	\$154	\$119	\$486
Error-free standard error ($\sigma_{\bar{x}}$)	\$137	\$ 97	\$117

In Study S2c, the bias reached substantial proportions due to the high incidence of nonreporting. As a result, the apparent standard error of the mean understates the true figure by almost 70 percent. In addition, the data show that in the absence of response error the standard error of the mean would have been reduced appreciably. In part this is due to the positive intercorrelation between the errors and the true values, although the main effect is the relatively large size of the response errors, particularly in Study S2c.

Cash Loans

As part of the Consumer Savings Project, the Survey Research Center undertook in 1959 a study of response errors in reports of cash loans. In contrast to new car debt, cash loans are traditionally one of the most difficult to ask about in personal interviews. With the experience gained in the preceding studies on new car debt, as well as in two prior studies of savings accounts (covered in Chapter IV), it was felt desirable to explore this more difficult area of consumer debt.

The study was carried out by selecting a probability sample of known borrowers in two urban areas. Contrary to the procedure in Study S2b, interviewers were not told of the real purpose of the study and, to allay suspicion still further, some names and addresses were selected from sources other than the available list of cash borrowers. In further contrast to the previous debt studies, the questionnaire was a general one, covering a variety of topics, similar to that used in the annual Survey of Consumer Finances. Among the topics included were attitudes on economic events, housing arrangements, management of children's education, saving practices and attitudes, and financial information covering liquid assets, mortgage payments, and installment purchases as well as cash loans. Psychological questions were asked to probe two dimensions of personality — social conformity and "personal effectiveness" — felt to be related to reporting of the loan, as did indeed turn out to be the case.

From an experimental point of view, the sample was divided into three random groups. With members of Group A, interviewers were instructed to ask for the respondent by name. With Group B, interviewers were given the name of the person to contact but were requested not to use the name during the interview. With Group C, interviewers were also requested not to make use of the name of the respondent. In addition, they were to utilize a "sealed-envelope" technique, by which the respondent was to be offered the option of filling out the financial data himself on a separate sheet, sealing it in an envelope addressed to the Survey Research Center, and having the interviewer drop the envelope into the mail. The interviewer was requested in these interviews to show the respondent an IBM card and stress the anonymity of the data and the importance of the study in throwing light on the manner in which people handle their financial affairs.

Excluding names and addresses selected to camouflage the purpose of the study, the sample consisted of 138 households. As shown in Table 9, interviews were conducted at 99, or 73 percent, of these households;⁵⁷ nevertheless, the base for the study of response error was 94 completed interviews, because 5 of the respondents had paid off their debt between the time the sample was selected and the date of the interview.

As is evident from Table 9, the overall error was far larger than on any of the preceding pilot studies. The average debt reported by the re-

⁵⁷ The large proportion of households not interviewed was due primarily to the desire of the researchers to obtain only 100 completed interviews for purposes of analysis. Although it might have been more desirable to have covered as much of the sample as possible (for there is always the possibility that the accuracy of those interviewed toward the end of the study may be different from that of sample members interviewed earlier), after 100 interviews had been achieved, no further interviews were attempted. Lansing, Ginsburg, and Braaten, *op. cit.*, Chapter VI.

spondents understates the actual average by 59 percent (line 10). Virtually the entire error resulted from the fact that in this case the respondents who did not report the known loan (over 60 percent) outnumbered those who did.⁵⁸ More than 80 percent of the nonreporters did report other cash loans, thus supporting the assertion of the researchers that the degree of cooperation was much better than would be inferred just from glancing at the figure for the percentage of nonreporters. However, these other loans could not be identified as the loan under study.⁵⁹

Loans that were reported coincided well, on the average, with the true figure (line 12), although it should be borne in mind that accuracy of report served as a basis for identification of the loan, so that these two factors may not be independent. There was also no tendency for bias in these errors to be in any particular direction.

In such a case, with error due to nonrespondents not being considered, the allocation of error in lines 16-18 leads to the foregone conclusion that just about all the error is the result of respondent nonreporting. Although no definitive reason can be given for this high incidence of nonreporting, aside from the general sensitivity of consumers to providing information on this type of debt, breakdown of the nonreporting by various characteristics helps to throw some light on the question. A pronounced and statistically significant tendency toward reporting of the known loan was apparent as income fell, as education rose, and as age of head declined; no relationship was found for the other two socio-economic characteristics studied, namely, occupation of head and sex of respondent.

The reason for higher frequency of reporting among younger people and among those with more education is not difficult to understand, in view of the general tendency of these groups to be more cooperative on surveys of this type. The better reporting among the lower-income levels is less understandable. However, the investigators offer a plausible reason for this phenomenon, namely, that cash borrowing is the sort of debt to which more stigma is likely to be attached at higher-income levels. "For a person with a high or moderately high income to admit that he had to borrow *cash* to meet his expenses would be incongruent with his perceived status position."⁶⁰ This may well explain why only 11 percent of

⁵⁸ This abstracts from possible errors due to faulty estimates of cash loans of nonrespondents, actual data for which were not available.

⁵⁹ A basic difficulty in this study as well as in the prior validation studies was that the respondent was not asked to indicate the name of the institution, the date when the debt was taken out, or in whose name the debt was registered. As a result, identification had to be made on the basis of various bits of circumstantial evidence. In this study, four such items were used: the monthly payment, the number of payments, the purpose of the debt, and the type of lending agency. In view of this lack of direct identification, mismatching of debts was much more likely.

⁶⁰ Lansing, Ginsburg, and Braaten, *op. cit.*, p. 103. Italics in original.

the families with incomes of \$10,000 and over reported the known loan as compared with 58 percent of those with incomes under \$5,000.

Several personality characteristics and attitudes were also related to debt reporting. People who scored high on a series of questions on the acceptability of using credit were much more likely to report the loan than others. Furthermore, people with a middle tendency toward social conformity were more likely to report the debt than people with either high or low conformity scores. As the investigators suggest, "It may be that people with low conformity scores are not susceptible to the influence of the interviewer and do not respond to her request for information. At the other end of the scale, people with high conformity scores may be unwilling to admit to such doubtful behavior as borrowing cash from a small loan company."⁶¹

Moreover, people with low scores on personal effectiveness (essentially, ability to make up one's mind and to plan ahead) were significantly more likely to report the loan than people with high scores. An explanation of this finding is that, "People with high scores on effectiveness are able to resist the pressure from the interviewer to report their financial situation, but those with low scores simply do as the interviewer requests because they lack the self-confidence to do otherwise."⁶²

Reporting of debt was also much higher with the sealed-envelope technique than with the method used for either of the other two experimental groups. Where the sealed-envelope technique was used, only 3 percent of 30 individuals reported no loans, as contrasted to 16 percent of the 64 individuals in Groups A and B.

Finally, in view of the substantial bias in the estimate of the average debt, it is not surprising to find that the apparent standard error of the mean computed from the survey data understates by a tremendous margin the true standard error. The relevant figures are:

k	9.7
r30
Apparent standard error of mean ($\sigma_{\bar{y}}$).....	\$ 1.20
True standard error of mean (\sqrt{MSE}).....	\$11.60
Error-free standard error ($\sigma_{\bar{x}}$).....	\$.60

In this particular case, everything acts against accurate estimation of the standard error. The sample is relatively large and the actual amounts borrowed are clustered within a fairly narrow range, so that the standard error of the average amount borrowed (\$23.12) is very low — barely \$.60. At the same time, the interview data missed the true mean by more than 50 percent, with the result that \bar{e} is very large, and hence k rises to 9.7.

⁶¹ *Ibid.*, p. 110.

⁶² *Ibid.*, p. 114.

In addition, the intercorrelation is positive, .30. The overall result is that both the estimates and the standard errors of the estimates bear little relation to the true values. Furthermore, in the absence of response error, the standard error would have been half the apparent value and almost 95 percent below the true value for these observations.

Farm Debt

In 1960 a farm panel was launched as part of the Consumer Savings Project, with financial assistance from the United States Department of Agriculture. The initial sample consisted of 377 names and addresses of farm owners or operators in a well-to-do area plus 66 additional names and addresses for pretest purposes. Names and addresses were selected by probability methods primarily from lists of farmers in debt or of farmers with demand or time deposits. The interviewers, however, were only told that statistical probability methods were used in sample selection.

Aside from methodological aspects, the study sought to throw light on the asset and debt position of farmers and on how farmers handled their finances. The questionnaires used on this study were therefore very broad, covering all assets and debts in addition to farm income, saving practices and attitudes, and a variety of attitudinal information (see Appendix A for copies of some of these questionnaires). Information was asked about individual assets and debts, including names of institutions, dates, and members owning, so that identification problems could be kept to a minimum.⁶³

Two controlled experiments were carried out on the first wave of interviews.⁶⁴ One experiment involved using alternative advance letters. One was a straightforward appeal based on the need for data on how farmers handle their finances. The other letter took the same general approach but in addition sought to involve the sample member further by seeking his suggestions and aid in improving the data-collection procedure. To this end, sample members sent this letter were given a form to fill out after the interview in which they recorded their impressions of various parts of the interview and were asked for suggestions on improving the conduct of these interviews.

The second experiment involved the use of two different questionnaire forms. With one form, a "holdings form," used on half the sample, re-

⁶³ Such information is virtually a necessity anyway in a panel study, if track is to be kept of changes in individual holdings over time. From a substantive point of view, these data are also of considerable value in studying the influence of location of financial services on patronage.

⁶⁴ Other experiments, carried out on later waves, will be discussed in Chapter VI.

spondents were asked for a detailed list of their holdings of assets and debts; the other half of the sample was interviewed with a "change form" and asked for detailed *changes* in assets and debts since the previous July 1 (the interviews were in the fall) with only a rough indication of total holdings of different assets and debts. This experiment was designed to throw further light on the possibility raised in earlier project panel operations that people might be more willing to divulge their full financial position after having been given a chance to convince themselves of the authenticity of the study and of the trustworthiness of the interviewer. Furthermore, since the ultimate aim of many such studies is to estimate saving, this experiment serves as a basis for comparing the accuracy of reported change with that of computed change.

To avoid confounding of the two experiments, the sample was divided into four equal random segments by systematic selection. Each segment was accorded a different treatment, as follows:

- (1) Holdings form, straightforward appeal.
- (2) Holdings form, appeal for advice.
- (3) Change form, straightforward appeal.
- (4) Change form, appeal for advice.

Including the pretest sample, interviews were attempted with 409 farmers; the other 34 names and addresses were not eligible for various reasons — deceased, moved, no longer farming, and so forth. The results of these contacts were:

	<i>Number in pretest sample</i>	<i>Number in regular sample</i>
Respondents.....	57	308
Nonrespondents.....	9	35
Refusals.....	8	33
Noncontact.....	1	2
Total.....	66	343

In other words, roughly 99 percent of the sample members were contacted, and of those contacted, about 90 percent were interviewed, so that the overall interview rate was 87 percent for the pretest and 90 percent for the regular sample. This was a much better performance than was obtained on any of the other savings studies, panel or otherwise.⁶⁵

Debt information served as a source of sample selection for 272 of the 409 eligible sample members. Of this number, 133 were to be interviewed

⁶⁵ It might be noted that the basis for analysis in this study, as in the other project panels, was the "savings unit." In this study, a savings unit consisted of the farm owner, or operator, his wife, all children under 16, and other family members in the same dwelling earning less than \$600 per year or having savings of less than \$600 in their own name.

TABLE 10. RESPONSE AND NONRESPONSE ERRORS IN REPORTS OF FARM DEBT ON WAVE 1 OF STUDY P3

Category (1)	Number of sav- ings units (2)	Number of debts (3)	Average size of individual debt		Percentage deviation, reported from actual (6)
			Actual (4)	Reported (5)	
1. Total sample ^a	133	150	\$6,897	\$6,751	2.1
2. Respondents.....	120	137	7,052	6,751	4.3
3. Nonreporters.....	22	26	3,885
4. Reporters.....	98	111	7,794	8,333	6.9
Nonrespondents					
5. Noncontacts.....
6. Refusals.....	13	13	5,267
Allocation of error					
7. To nonrespondents...	-88.1%
8. To nonreporting of respondents.....	461.5
9. To inaccurate re- porting of respondents.....	-273.4

^a Restricted to those interviewed with a holdings form and for whom debt information was a primary validation source.

with a holdings form and 139 with a change form. The results of these attempts were as follows:

	<i>Holdings form</i>	<i>Change form</i>
Respondents.....	120	132
Nonrespondents.....	13	7
Refusals.....	13	6
Noncontacts and other.....	0	1
Total.....	133	139

Because of the different data validated, meaningful comparisons of the accuracies of report obtained on these two forms are not possible (at least not on the basis of this one wave of interviews). The results of the validation are therefore presented separately for each form.

Holdings Form

A general summary of the accuracy and completeness of report of debt on the holdings form is shown in Table 10. As is evident from this table, the reported average debt understates the actual figure by about 2 percent. This understatement appears to be the result of three different types of errors, which do not fully offset each other, namely:

(1) Almost one-fifth of the respondent debts were not reported in the interview. These debts, however, were generally small ones.

(2) On the other hand, the debts of the nonrespondents — in this case all the nonrespondents were refusals — averaged well below the average

TABLE 11. DISTRIBUTION OF ERRORS IN DEBT REPORTS, WAVE 1, HOLDINGS FORM, STUDY P3

<i>R</i> report relative to <i>I</i> report (1)	Percent of total		Average debt as reported by	
	Number of debts (2)	Dollar amount ^a (3)	<i>R</i> (4)	<i>I</i> (5)
Overstated by 25 percent or more....	10.8	7.4	\$13,980	\$6,322
Overstated by 11 to 24 percent.....	3.6	3.5	5,375	4,650
Overstated by 10 percent or less.....	8.1	5.2	5,105	4,916
No discrepancy.....	14.4	17.1	9,168	9,168
Understated by 10 percent or less.....	31.5	30.9	7,475	7,558
Understated by 11 to 24 percent.....	19.8	7.9	8,529	9,380
Understated by 25 percent or more...	11.7	28.0	4,565	7,863
Total.....	99.9	100.0	\$8,333	\$7,794
Base.....	111	\$856,653		

^a Based on *I* report.

debt of the respondents (line 2 versus line 6), with a resulting tendency for such debts to be overstated if estimated from data supplied by the respondents.

(3) A further reinforcing tendency is evident in the fact that the average debt of the respondent reporters slightly exceeded the actual figure (line 4).

The results of these offsetting tendencies are brought together in the error allocation in lines 7-9 of Table 10. This allocation shows that the entire underestimate is due to respondent nonreporting and that the extent of the underestimate would have been much larger had it not been for overestimates both on the part of the respondent reporters and in the imputed debt estimates of the nonreporters.⁶⁶

The understatement of the averages is also reflected in the distribution of errors, shown in Table 11. Oddly enough, large errors appear to occur with more or less equal frequency — about 11 percent of the debts were overstated by 25 percent or more and an almost equal proportion were understated by this percentage. However, striking differences appear when the frequency of small errors of different magnitudes are examined. Al-

⁶⁶ It may seem somewhat peculiar that the inaccurate reporting of the respondents is responsible for an overestimate of 273 percent when the average reported debt of this group was less than 7 percent above the actual figure. The explanation lies in the large size of this group. Representing nearly three-fourths of all the known debts of these sample members, any error, even a relatively small one, affects the *aggregate* substantially when summed over this number of sample members. Also, with the overall bias so low, relative deviations tend to be magnified.

TABLE 12. PERCENTAGE DISTRIBUTION OF FARM DEBT, BY SIZE, WAVE 1, HOLDINGS FORM, STUDY P3

Size of debt (1)	Balances reported by both <i>R</i> and <i>I</i>		Debts of non- reporters (4)	All validated debts of <i>R</i> (5)	Debts of nonre- spondents ^a (6)
	Report of <i>I</i> (2)	Report of <i>R</i> (3)			
Under \$1,000.....	2.7	3.6	19.2	5.8
\$1,000 to \$2,499.....	14.4	17.1	34.6	18.3	38.4
\$2,500 to \$4,999.....	27.0	22.5	19.2	25.5	23.0
\$5,000 to \$7,499.....	19.8	24.3	11.5	18.3	15.4
\$7,500 to \$9,999.....	10.8	9.0	7.7	10.2	7.7
\$10,000 to \$14,999.....	13.5	9.9	3.9	11.7	15.4
\$15,000 to \$24,999.....	7.2	8.1	3.9	6.6
\$25,000 and over.....	4.5	5.4	3.6
Total.....	99.9	99.9	100.0	100.0	99.9
Base number.....	111	111	26	137	13

^a In this case, all nonrespondents were refusals.

most one-third of the debts were *understated* by less than 10 percent, whereas less than one-tenth were *overstated* by less than 10 percent. Similarly, almost one-fifth of the debts were *understated* by 10 to 25 percent, whereas debts *overstated* by 10 to 25 percent represented less than 4 percent of the total.

Much the same phenomenon exists when comparisons are made in terms of dollar amounts rather than numbers of debts. In addition, debts understated by 25 percent or more now assume greatly increased importance, which one would expect by definition.

Table 11 also brings out the fact that roughly half of the debts reported were accurate within 10 percent and almost four-fifths were accurate within 25 percent. In terms of dollar amounts, however, only two-thirds of the total were accurate within 25 percent, with more than one-fourth having been understated by more than this percentage.

Columns 4 and 5 of this table suggest that the understated debts tend to be higher than those which are overstated. For example, the 35 debts understated by 10 percent or more averaged \$8,817, whereas the 16 debts that were overstated by this proportion averaged \$5,904.

The results of these various errors are reflected in the alternative debt distributions presented in Table 12. The debt distribution obtained from the interviews, column 3, appears to approximate reasonably closely the actual distribution both of those debts (column 2) and of all known debts, including those of the nonreporters (column 5). Nevertheless, systematic

differences do appear. Thus, the respondent debt distribution tends to overstate the frequency of large debts and, relative to all known debts of the respondents, tends to understate the frequency of small debts. As is apparent from column 4, this is due to the heavy concentration of debts of the nonreporters at the lower end of the distribution. Column 6 suggests that this understatement of the frequency of small debts and overstatement of the frequency of large debts would have been even more pronounced had the debts of the nonrespondents been added; these were small debts more frequently than were respondent debts.

Evaluation of the effect on the standard error of the mean of these errors indicates, somewhat surprisingly, that the true standard error in this case is overestimated by the sample observations, as follows:

k42
r26
Apparent standard error of mean ($\sigma_{\bar{y}}$).....	\$722
True standard error of mean (\sqrt{MSE}).....	\$783
Error-free standard error ($\sigma_{\bar{x}}$).....	\$571

The reason for this appears to be the wide range of variation both in actual debts and in reported debts which, combined with the relatively small sample, serves to produce relatively high measures of sampling variation. Thus, $\sigma_{\bar{x}}$ is \$571, whereas $\sigma_{\bar{y}}$ is \$722. Since the degree of understatement is rather moderate (Table 10), the value of k , the bias indicator, is not high. Nevertheless, in the absence of response error the standard error would have been reduced roughly 20 percent relative to the sample estimate. In contrast to previous studies, the primary reason for this is the positive intercorrelation between the actual values and the response errors; this term accounts for two-thirds of the reduction.

Change Form

The debt information on the change form seems to be less reliable than on the holdings form although, paradoxically, the change form appears to have been more successful in obtaining interviews. Thus, of those contacted to be interviewed with a change form, little more than 4 percent refused (6 of 138), as shown in column 2 of Table 13. In contrast, 13 of 133 sample members contacted with the holdings form refused to be interviewed (see Table 10, p. 66), a refusal rate of almost 10 percent. The difference is only statistically significant at the .08 probability level but, combined with information to be presented later, suggests that the change form does produce a higher rate of response.

Offhand, such an inference may seem strange, since the respondent presumably does not know in advance what type of questions will be asked. The explanation lies in the interchange between the interviewer and the

TABLE 13. RESPONSE AND NONRESPONSE ERRORS IN REPORTS OF CHANGE IN FARM DEBT ON INITIAL INTERVIEW, STUDY P3

Category (1)	Number of savings units (2)	Number of debts (3)	Average change per debt		Percentage of deviation, reported from actual (6)
			Actual (4)	Reported (5)	
Total sample ^a	139	147	\$ -62	\$84	135.5
Respondents	132	140	-31	88	283.9
Nonreporters	55	57	-78
Reporters	77	83	-1	148	14,800.0
Nonrespondents	7	7	667
Noncontacts	1	1	0
Refusals	6	6	778

^a Restricted to those interviewed with a change form and for whom debt information was a primary validation source.

respondent before the interview begins. With reluctant respondents, the interviewer often has to divulge much more information about the interview than is otherwise necessary. If this discussion kept on long enough the interviewer did indicate, according to reports, the type of questions to be asked by that approach. It was clearly much easier to use the change form as an inducement to grant an interview than to do this with the holdings form, since in the former case the interviewer could, and did, stress the fact that only changes had to be reported and not aggregate holdings.⁶⁷

The results of this interchange apparently served, oddly enough, to increase the reliability of the data obtained on the holdings form. The reason appears to have been the greater tendency of the less cooperative sample members to drop out when approached with a holdings form than was their tendency when approached with a change form. Thus, almost 42 percent of the respondents on the change form failed to report one or more debts (Table 13), whereas the corresponding percentage on the holdings form was only 18 percent. In addition, on the change form the number of debts reported per respondent averaged 1.04, whereas on the holdings form the number of debts reported per respondent averaged 1.14. Since the two subsamples are presumably random, this would suggest that more respondents report debt on the holdings form than on the change form and, in addition, that more complete information is obtained even for the reporters on the holdings form.

Columns 4-6 of Table 13 also suggest that the quality of the debt infor-

⁶⁷ In addition, on a few interviews a refusal was encountered after the interview was begun. These interviews, which were treated as refusals, were nearly always encountered with the holdings approach.

TABLE 14. DISTRIBUTION OF ERRORS IN DEBT REPORTS, WAVE 1,
CHANGE FORM, STUDY P3

<i>R</i> report of change relative to <i>I</i> report ^a	Increases in debt		Decreases in debt		No change: percent- age of debts
	Percent- age of debts	Percentage of dollar change	Percent- age of debts	Percentage of dollar change	
Overstated by 25 percent or more.....	33.3	65.1	25.9	18.4	5.3
Overstated by 11 to 24 percent	11.1	40.9
Overstated by 10 percent or less.....	14.8	14.3
No discrepancy.....	7.4	6.1	94.7
Understated by 10 percent or less.....
Understated by 11 to 24 percent	11.1	6.8	3.7	7.3
Understated by 25 percent or more.....	55.6	28.0	37.1	13.0
Total.....	100.0	99.9	100.0	100.0	100.0
Base number.....	18	\$27,026	27	\$27,078	38

^a Overstatements and understatements refer to absolute changes.

mation reported on the change form may not be high. The interview reports indicate that the debt of the respondents *increased* during the three to four months between July 1 and the date of the interview by about \$88, on the average. Actually, these debts *decreased* by \$31 on the average and by \$62 for the entire subsample. As a result, the direction of change was missed completely. Contrary to the situation with the holdings form, the error appears to have been equally characteristic of reporters; indeed, it was the largest of all: they reported an average debt increase of \$148, whereas the amount of debt actually declined by \$1.

Reasons for these errors become evident in Table 14. This table shows the following:

(1) Paradoxically, of debts that increased in amount, more than half were understated by 25 percent or more, although one-third were overstated by 25 percent or more. The latter group accounted for almost two-thirds of the dollar increase in these debts.

(2) The absolute change of most debts that decreased in amount was overstated. This was true both of more than half of the number of such debts and of almost three-fourths of the dollar change. The decrease of more than one-third of these debts was understated by 25 percent or more, although representing only 13 percent of the total dollar amount.

(3) Debts which had not changed in amount were as a rule correctly

TABLE 15. PERCENTAGE DISTRIBUTION OF CHANGE IN FARM DEBT,
BY SIZE, WAVE 1, STUDY P3

Amount of change (1)	Changes reported by both <i>R</i> and <i>I</i>		Changes not re- ported by <i>R</i> (4)	All validated changes (5)
	Report of <i>I</i> (2)	Report of <i>R</i> (3)		
—\$1,500 and under.....	6.1	8.5	10.7	6.5
—\$1,000 to —\$1,499.....	6.1	7.3	...	3.6
—\$500 to —\$999.....	4.9	1.2	14.3	8.6
—\$100 to —\$499.....	12.2	6.1	8.9	10.8
—\$1 to —\$99.....	3.7	0	1.8	3.6
No change.....	46.3	59.8	44.6	45.3
\$1 to \$99.....	1.2	0	...	0.7
\$100 to \$499.....	3.7	3.7	5.4	4.3
\$500 to \$999.....	8.5	4.9	8.9	9.4
\$1,000 to \$1,499.....	3.7	2.4	3.6	3.6
\$1,500 and over.....	3.6	6.1	1.8	2.8
Total.....	100.0	100.0	100.0	99.8
Base number.....	83	83	56	139

reported. For only two of 38 such debts were errors made, both of which were overstatements.

The reason for this substantial overstatement in the aggregate change in debt now becomes clear. According to the institutional data (*I*), the aggregate decrease in debt was only slightly larger than the aggregate increase. However, debts that increased were substantially overstated, whereas debts that decreased were, on the average, only slightly overstated (in absolute amount). The figures are as follows, with *R* denoting respondent reports:

	<i>Increase</i>	<i>Decrease</i>
<i>I</i>	\$27,026	\$27,078
<i>R</i>	41,670	29,379

Largely as a result of these overstatements, the net reported increase in debt was \$12,291 instead of the actual figure of —\$52.

The effect of these errors on the distribution of debts by amount of change is shown in Table 15. Perhaps most surprising about this table is that despite the large error in the aggregate, the reported distribution of change (column 3) is not too different from the actual distribution, either for respondents alone or for all validated changes. Thus, the *R* distribution indicates that about 17 percent of the debts increased in amount,

whereas the corresponding true figure was 19.5 percent. According to the *R* distribution, 23 percent of the debts decreased by \$100 or more, whereas the true figure was 29 percent.

The most pronounced errors were in the tendencies of the reporters to overstate the frequency of no change, as well as the frequency of change of more than \$1,000 at both ends of the distribution. Since changes in debt not reported appear to have involved more frequently amounts of less than \$1,000 (column 4), the distribution of reported change tends to overstate even further the relative frequency of large changes.

Determinants of Error

From the foregoing it is abundantly clear that reporting of debt was affected by type of questionnaire form. Indeed, both of the experiments appear to have had some effect on debt reporting, although only the joint effect of the two is statistically significant with regard to frequency of debt reported. The relevant data are as follows:

<i>Letter</i>	<i>Form</i>	<i>Percentage of savings units reporting debt</i>	<i>Base (number of savings units)</i>
Regular	Holdings	73.8	84
	Change	76.0	96
	Total	75.0	180
Evaluation	Holdings	88.6	88
	Change	77.9	95
	Total	83.1	183
Total	Holdings	81.4	172
	Change	77.0	191
	Total	79.1	363

More frequent reports of debt — and, hence, presumably more complete data — apparently occurred on the holdings form than on the change form, and this was also true when the evaluation letter (asking for advice and suggestions) was used in place of the regular letter; the latter difference — between 83.1 percent and 75.0 percent — is barely not statistically significant at the .05 probability level. The highest frequency of debt reports by far, almost 90 percent, was achieved with the combined use of the holdings form and the evaluation letter. This percentage is statistically above (at the .05 significance level) the percentages for each of the other three combinations. Evidently, the interaction of these two factors yields results superior to either one alone.

Results of tests of the effects on reporting of farm debt of a number of respondent interviewer and interview characteristics are summarized

TABLE 16. CHI-SQUARE TESTS OF EFFECT OF DIFFERENT INTERVIEW CHARACTERISTICS ON REPORTING OF FARM DEBT, STUDY P3

Characteristic	Value of chi-square	Degrees of freedom	Significance level
Age of farm operator.....	5.020	8	.76
Education of farm operator.....	4.044	8	.85
Income level, 1959.....	18.706	12	.10
Value of total assets.....	15.356	12	.22
Size of farm.....	14.986	14	.45
Renter versus owner.....	.933	2	.6
Size of debt.....	32.347	14	.004
Interviewer ratings of:			
Accuracy of financial data.....	9.593	6	.15
Completeness of financial data.....	3.891	6	.69
Accuracy of debt report ^a	8.292	4	.15
Completeness of debt report.....	.910	2	.63
Use of records on checking accounts....	15.347	4	.01
Interviewer rating when hired.....	7.411	6	.24
Interviewer dominance score on EPPS ^b ..	20.782	12	.06
Interviewer deference score on EPPS....	18.256	10	.05

^a Chi-square is 6.012 with 2d.f. ($P = .05$) if test is carried out between reporters and non-reporters.

^b *Edwards Personal Preference Schedule*, discussed in Mathew Hauck and Stanley Steinkamp, *Survey Reliability and Interviewer Competence* (Urbana: University of Illinois Bureau of Economic and Business Research, 1964), pp. 39-44.

in Table 16. The chi-squares in this table were obtained by classifying the validated debts into three categories: those which were not reported, those which were reported within 25 percent of the actual figure, and those which were reported but deviated by more than 25 percent from the actual figure.⁶⁸ This classification differs from the simple reporter-nonreporter distinction used previously but, with one exception to be mentioned shortly, yields the same results in either case.

Although most of the characteristics in this table are not significant at the .05 probability level, four are significant or nearly so, and several others would seem to merit further consideration. Of the characteristics which are significant, one refers to a feature of the debt itself, namely, its size; one refers to a measure of respondent cooperativeness — use of records on checking accounts; and two, rather surprisingly, refer to personality characteristics of the interviewer, as based on scores obtained from the *Edwards Personal Preference Schedule* (EPPS) test.

In each case, the significance of the relationship is supported by its plausible nature. Thus, those with large debts were less likely to report their existence than those with small debts. People who used records in giving checking account balances were more likely to report the debt ac-

⁶⁸ There were no instances in which the debt was reported but the balance was refused.

curately, and they were even more likely still to report the existence of the debt.⁶⁹

Interviewers with low scores on dominance were much more likely than those with high scores to turn in questionnaires with no debt reported, as was also true of interviewers with high scores on deference. To some extent, one of these characteristics is the complement of the other, but there is some indication that taken together they provide more information than each one separately.

Table 16 indicates further that the interviewer may be able to gauge the accuracy of a report, and that accuracy of report may be a more sensitive indicator from the interviewer standpoint than completeness of report. In particular, a chi-square test of the interviewer's estimate of accuracy of report of the debt related to reporting versus nonreporting was significant at the .05 probability level.

It is interesting to note that none of the socioeconomic characteristics tested was statistically significant, not even age which had been significant in many previous studies. There is some indication, however, that those at the very lowest income levels were likely to be more inaccurate and also more likely not to report the debt than those at higher income levels.

Two Secondary Validation Studies

After the sample had been selected in Studies P1 and P2, a limited amount of debt validation by obtaining debt information from a central credit source in each area proved feasible. Such secondary validation cannot possess the reliability of primary validation for various reasons, two of them general and others specific to the particular sources used. Perhaps most important of the general reasons is the fact that no debt information is obtained for the nonrespondents, with some exceptions to be noted later. Second, a nonreported holding is less likely to be picked up by the credit source than a reported one; it is clearly impossible for any one institutional source to cover all forms of debt, including debt to individuals and to neighborhood merchants.

The specific reasons relate to the validation source used, which was the same in both of these studies. Since the source operated as a clearing-house for member institutions, its data depended on the scope and cooperativeness of these institutions and on the care taken in supplying the data. In one of these studies (P1), many of the institutions in the area,

⁶⁹ The reason for testing use of records on checking accounts rather than on some other asset was that virtually all the respondents had checking accounts so that classification by this asset yielded information for most of the sample members. Use of records on debt was out of the question because if a respondent had no debt or did not report it, records yielded nothing.

including most of the nonretail-debt-handling institutions, were not part of this clearinghouse operation. In addition, some of the member institutions in both areas exhibited erratic reporting practices, tending to withhold debt information on "good customers." Moreover, the procedure used by these central sources to update debt information was to ask for such information from previously reported creditors *only*. Presumably, the central file would contain for each individual a complete list of his creditors insofar as prospective creditors asked for previous debt information before extending credit, and this request was recorded in the individual's file for later follow-up. However, if a prospective creditor did not ask for such information, perhaps not feeling any need for debt information on the prospective borrower, the central clearing source would not be aware of the new creditor. Although creditors were supposed to report all new loans or credit, circumstantial evidence indicates they did not always do so.

A final limitation of these data is the difficulty of matching debts and debt balances, with regard both to identity and to time of a given balance. The latter is invariably reported by the creditor as of the date of the request. Since the requests had to be sent out some time after the interviews had been completed — generally several weeks — it was often not easy to extrapolate a debt back to the time of the interview, even though the terms of the debt were generally stated. Also, with retail debt a matching problem frequently arose, since the one balance figure could represent several debts as well as debts by more than one family member. The existence of the latter could only be discerned from the interview data.

For these reasons, the results of the secondary validation studies are subject to errors not present in the other studies and are not strictly comparable with the other studies. The table forms and presentation of results in this section reflect this fact and differ from those of the previous sections in various ways, as will be noted shortly.

The two secondary validation studies were carried out under similar conditions, and both purport to cover all forms of nonmortgage debt. However, since there are important differences in the manner in which each was conducted, as well as in coverage of debt by each of the two validation sources, it seems preferable to present the results of these studies separately, making comparisons whenever they are appropriate.

Study P1

This, the first panel validation study, was concerned with secondary validation of nonmortgage debt as well as of life insurance. In addition, the operation was designed to test the effectiveness of various interview approaches toward soliciting financial data. To this end, three controlled

experiments were incorporated into the first of the five waves of interviews in this operation. These experiments were:

(1) Use of a relatively brief, vague advance letter on the purposes of the study versus a longer, more detailed advance letter. (For copies, see Appendix A.)

(2) Dollar figures requested on the first wave versus no dollar figures requested until the second wave. In the latter case, only general information on asset and debt holdings was sought on the first wave, giving the respondent and interviewer a chance to get to know each other before taking up dollar amounts.

(3) Use of a structured questionnaire with the interviewer required to follow strictly the order and wording of the questions versus an unstructured approach whereby the interviewer asked questions from memory in whatever order seemed most suitable, jotted notes on a pad during the interview, and wrote a detailed report immediately afterward. The purpose of this latter, depth-type interview situation was to ascertain whether or not it left the respondent (and the interviewer) more at ease and hence presumably more likely to discuss finances frankly.

As a result of these experiments, four different questioning approaches were used on the first wave of interviews; copies of the two structured questionnaires are shown in Appendix A. To avoid the confounding of one experiment with another, the sample was divided by systematic selection into eight random segments, each segment receiving a different combination of these experimental approaches in a manner analogous to that described for Study P3 (p. 65). In all instances, the questionnaire covered the entire spectrum of consumer assets and debts, including annuities, pensions, brokerage accounts, and ownership of a business. Where dollar figures were requested, the subject was always holdings, not the change since a particular past period.

The sample for Study P1 was selected by area probability methods from the population of a large urban area, using 1950 census tract data. Disproportionate representation was given to the high-income areas — census tracts in which 25 percent or more of the consumer units reported incomes in excess of \$10,000 in 1949. As a result, over 40 percent of the sample reported incomes above \$10,000 in 1958; a rough estimate of the actual proportion of such consumer units in the area at that time is 10 percent.

The initial sample for this study comprised 308 names and addresses, selected with a view toward securing at least 200 completed interviews on the first wave. The eligible sample turned out to be 296, because of attrition due primarily to deaths and migration out of the area. Of this number, 210 were interviewed, 61 refused, and 25 could not be contacted.

In other words, the response rate (interviews/contacts) was 78 percent; the contact rate (contacts/sample) was 92 percent; and the overall interview rate was 71 percent.

The results of the debt validation are summarized in Table 17. The principal finding evident from this table is the rather startling one that in no instance could a debt reported by *R* be identified as also having been reported by *I*. In other words, the debts obtained from these two sources were mutually exclusive, all the information obtained from one source serving to supplement rather than to verify information obtained from the other source.

There is reason to believe that this state of affairs was a peculiarity of two conditions under which the debt information was obtained from the two sources. One condition was the unclear manner in which this information was sought in the interview. The opening question on debt advised the respondent, among other things, not to include "bills payable in 30 days." The intent of this phrase was to exclude debt of less than 30-days' duration. However, because of the manner in which this request was worded, respondents were encouraged to omit any debt which they thought might be payable *within* 30 days, irrespective of the length of time which they might already have had it. By hindsight, a considerable number of retail debts may fall into this category, since people often expect to pay off such debts within the next 30 days. Second, the institutional source appears to have had poor cooperation from nonretail creditors. As a result, debt obtained from this source was almost exclusively retail debt.

These circumstances are reflected in Table 17 in a much larger number, but much lower average size, of debts obtained from *I* than from *R*. This situation is reflected even more strikingly by segregating retail debt from nonretail debt. Doing so, we find that for Questionnaire A, on which dollar amounts were requested, the respondent reports represented about 90 percent of both the number and the amount of nonretail debt obtained from both sources. On the other hand, virtually all (95 percent) of the retail debt was reported by *I*. Moreover, nonretail debt averaged more than 15 times higher than retail debt. As a result, even though the number of nonretail debts obtained from both sources for the respondents was not much more than half the number of retail debts, nonretail debts represented more than 85 percent of the total known debts of these sample members. It is this difference in the frequency of debt reported in the average size of debt that largely accounts for the mixed picture shown in Table 17.

Results obtained from the use of Questionnaire Form B, in which no dollar amounts were requested, correspond closely with those from Ques-

tionnaire A. There are some differences in the corresponding average debts obtained by the two forms, based on the *I* reports, but these differences appear to be largely a manifestation of the small sample sizes. In the one instance in which the sample sizes are appreciable — the reports of retail debt by *I* only — the two averages correspond closely. Indeed, there appears to have been little difference in the reporting of this debt whether or not requests were made for dollar amounts. The number of debts obtained on the two forms was much the same in either case, both for retail debt and for nonretail debt.

Because of the relatively small sample sizes, these data do not provide much evidence as to whether or not the debts of the refusals and of the noncontacts are any different in size or any more or less frequent than those of the respondents. With regard to size, the variability in debts reported for refusals and noncontacts is too high to permit any clear inferences. With regard to frequency, however, there is evidence that the refusals and noncontacts might have been more often in debt than the respondents. Debts were reported by *I* for 35 percent of the latter, but for 57 percent of 23 noncontacts and for 59 percent of 49 refusals for which debt reports were obtained. To be sure, most of these debts were retail debts, and it is not clear whether the same phenomenon also exists for other types of debt.

Analysis of the frequency of debt report by the other two experimental variables indicates that no appreciable, nor statistically significant, difference is obtained whether a brief, vague advance letter is used or a longer, more detailed letter. However, the structured questionnaire did produce a noticeably higher frequency of debt reports than the unstructured questionnaire — 31 percent versus 19 percent, respectively. This difference is almost statistically significant at the .05 probability level ($p = .065$). More important, it coincides with the interviewers' reports of having difficulty in the unstructured approach of remembering all the items of information to be covered.

Study P2

The second of the panel validation operations, this study was designed primarily to validate time deposits, as will be discussed in Chapter IV. At the same time, it proved feasible to use the same procedure followed in Study P1 to attempt secondary validation of nonmortgage debt. As with Study P1, five waves of interviews were scheduled with the panel members in this operation, the interval between interviews being roughly three to four months. Once more, certain controlled experiments were incorporated into the study, four on the first wave, as follows:

(1) Use of a straightforward appeal in the advance letter based on the need for information on how people handle their finances versus the same letter supplemented with an attempt to involve the panel member further by seeking his suggestions and aid on the conduct of the interview. (Copies of the advance letters are shown in Appendix A.)

(2) Use of questionnaire forms in the interview versus use of only reminder cards pasted into the interviewer's notebook. This was a follow-up of the third experiment of the first wave on Study P1 (p. 77), designed to cope with interviewer difficulties in remembering all of the details to be obtained for each holding.

(3) Request for dollar holdings as of the date of the interview versus request only for changes that had taken place in each holding during the preceding three months.

(4) Request for information on all assets and debts versus request for information on only specific assets and debts. The purpose of this experiment was to test the possibility that more complete information would be obtained if the interviewer did not have to cover the entire range of assets and debts.

As a result of these experiments, four different questionnaire forms were used on the first wave of interviews. With the exception of modifications necessitated by the fourth of these experiments, the questionnaire covered the entire spectrum of consumer assets and debts. Contrary to the procedure in Study P1, sample members were asked in all instances for dollar figures—in roughly half the interviews for dollar holdings and in the remainder for dollar changes. As on other panel studies, an attempt was made to avoid confounding of experiments; this was done by a systematic subdivision of the sample into random segments, in this case 16, with the members of each segment receiving a different combination of experimental approaches.

The initial sample size for this operation was set at 300 completed interviews. Actually, 316 interviews were obtained from a total eligible sample of 411. Nonrespondents numbered 95, of which 82 were refusals and 13 were noncontacts. Overall, the field performance appears to have been slightly better than in the previous panel operation (Study P1), with a contact rate of 97 percent, a response rate of 79 percent, and an overall interview rate of 77 percent.

Table 18 shows the results of the debt validation for the holdings form. As were the previous tables relating to Study P1, this table was prepared on an individual debt basis rather than on a savings unit basis. The debts recorded in this table correspond to information sought for 441 sample members, of whom 320 were interviewed, 82 refused, and 39 were not

TABLE 18. COMPARISON OF DEBT REPORTS FROM TWO SOURCES IN INITIAL INTERVIEWS,
HOLDINGS FORM, STUDY P2

Category (1)	Source of debt report (2)	Number of debts		Aggregate debt		Average debt	
		Number (3)	Percent (4)	Dollar amount (5)	Percent of total (6)	Dollar amount (7)	Percent of combined average (8)
1 Respondents.....	<i>I</i> and <i>R</i> comparable	21	18
2. <i>I</i> report	<i>I</i> report	5,355	14	255	80
3. <i>R</i> report	<i>R</i> report	5,524	15	263	82
4. <i>I</i> only	<i>I</i> only	55	47	7,653	20	139	43
5. <i>R</i> only	<i>R</i> only	41	35	24,474	65	597	187
6. All <i>I</i> reports	All <i>I</i> reports	76	65	13,008	35	171	53
7. All <i>R</i> reports	All <i>R</i> reports	62	53	29,998	80	484	151
8. <i>I</i> and <i>R</i> combined ^a	<i>I</i> and <i>R</i> combined ^a	117	100	37,482	99	320	100
9. Refusals.....	<i>I</i> report	18	...	7,695	..	428	...
10. Noncontacts.....	<i>I</i> report	7	...	2,160	..	309	...

^a Sum of lines 1, 4, and 5 for columns 3 and 4; sum of lines 2, 4, and 5 for columns 5 and 6; and average (weighted by cell size given in column 3) of lines 2, 4, and 5 for columns 7 and 8.

contacted. Debts were reported for 315 interviews and 93 nonrespondents, as follows:

	<i>Holdings form</i>	<i>Change form</i>
Debts reported by both <i>R</i> and <i>I</i> , although not necessarily the same debt(s) in both cases.....	20	12
Debts reported by <i>R</i> only.....	12	15
Debts reported by <i>I</i> only ⁷⁰	45	35
No debts reported from either source ⁷¹ ..	124	145

The overall results shown in Table 18 are somewhat mixed; neither source shows up too well. Less than one-fourth of the known debts of the respondents, representing about 15 percent of the aggregate dollar amount owed, was reported by both sources. Slightly more than half of the number of debts, although constituting only one-fifth of the aggregate dollar amount, was reported only by *I*; whereas about one-third of the number, representing almost two-thirds of the total dollar amount, was obtained only from the *R* reports.

Hence, of the total known respondent debts, three-fourths of the number, although only one-third of the dollar total, was accounted for by *I*; and a little over half the number, but almost four-fifths of the dollar amount, was supplied by *R*.⁷² To the extent that additional debts were not reported by either source, all these percentages would be too high.

Columns 7 and 8 of the table highlight the tendency for the average debt reported by *R* to be more than twice the size of the average debt reported by *I*. Consequently, when the reports from the two sources are combined to form a "best estimate" of the average-sized debt, the *I* reports provide a substantial underestimate and the *R* reports give a substantial overestimate, with the latter coming a little closer to the "best estimate."

The debts of the nonrespondents appear to average higher than the debts of the respondents, based on comparison of the corresponding *I* reports (line 6 versus lines 9 and 10). However, the margin of error in these estimates is undoubtedly much higher than for those of the respondents, partly because of the smaller sample sizes and partly because the

⁷⁰ Includes 18 nonrespondents on the holdings form and 8 nonrespondents on the change form.

⁷¹ Includes 37 nonrespondents on the holdings form and 30 nonrespondents on the change form.

⁷² These figures assume that the *I* report was the more accurate when data were available from both sources. This was an arbitrary assumption necessitated by the absence of any firm basis for making a decision. The *I* reports, coming from the creditor, should have been more complete but they also tended to be out-of-date. The *R* reports, although frequently based on records, nevertheless were not always fully complete.

TABLE 19. ALTERNATIVE ESTIMATES OF PERCENTAGE DISTRIBUTION OF INDIVIDUAL DEBTS, BY SIZE, STUDY P2

Size of debt (1)	Reported by <i>R</i> and <i>I</i>		All <i>I</i> reports (4)	All <i>R</i> reports (5)	All reports combined (6)
	<i>R</i> (2)	<i>I</i> (3)			
Under \$100.....	38	43	66	28	51
\$100 to \$499.....	43	33	24	37	27
\$500 to \$999.....	14	24	8	24	15
\$1,000 and over.....	5	..	3	11	7
Total.....	100	100	101	100	100
Base (number of debts).....	21	21	76	62	117

lack of interview data made identification of all adult members of the savings unit more difficult and the overlooking of debt more likely.

Based on the 21 debts for which a comparison can be made between the two sources, little overall tendency is apparent for respondents to understate or overstate those debts that are reported. The average of these debts as reported by *R* differs by only 3 percent from the average for the same debts based on *I*. Nevertheless, for more than one-third of these debts, the two reports differed from each other by at least 25 percent, as is shown by the following tabulation:

<i>R</i> report relative to <i>I</i> report	Number of debts	Average debt as reported by	
		<i>R</i>	<i>I</i>
Within 25 percent.....	13	\$274	\$271
Higher by 25 percent or more.....	5	339	192
Lower by 25 percent or more.....	3	89	291

The offsetting of large positive discrepancies by large negative discrepancies is largely responsible for the close correspondence between the aggregates. The same tendencies operate to bring the distributions into reasonably close alignment (considering the small sample sizes) for debt information available from both sources, as shown in Columns 2 and 3 of Table 19. Because of the more frequent oversight of large debts by *I* but the more frequent omission of many small debts by *R*, the *I* data would seem to supply a somewhat closer estimate of the distribution of debts by size, whereas the *R* reports would seem to be a considerably better source for the distribution of cumulated debt by size bracket. In the latter case, the *I* reports would be omitting more than three-fourths of the known debt of \$1,000 or more.

Perhaps the principal reason for the mediocre performance of both *R* and *I* in covering debts is evident from Table 20. The layout of this table is similar to that of Table 18 except that separate tabulations are now provided for retail debt and for nonretail debt, the latter comprising in this case exclusively debt to banks, other savings institutions, and finance companies. The pertinence of this distinction is brought out by the following inferences drawn from this table:

(1) Retail debts are generally small and frequent, whereas nonretail debts (in this case exclusively debts with banks and finance companies) are much fewer but also more sizable. Particularly striking in this connection is the fact that the total dollar amount of the 39 nonretail debts listed in the table aggregate is more than triple that of the 102 retail debts listed.

(2) The *R* reports tend to be less comprehensive in the retail area than in the nonretail area, whereas the reverse is true for the *I* reports.

(3) For both types of debt, substantial differences are apparent in the average dollar amounts obtained from each source. For both retail and nonretail debt, the average of the *R* reports is considerably above the average of the *I* reports — more so for retail debt than for nonretail debt.

(4) The foregoing results serve to highlight the general tendency, which exists apparently in both types of debt, for the average debt reported by *R* to be considerably higher than the average debt reported by *I*. Lines 4 and 5 in both parts of the table bring out the fact that debts picked up by *I* only, although much more numerous than the debts reported by *R* only (at least in the case of retail debt), are generally very small ones. Thus, the average retail debt picked up by *I* only was less than one-fourth the size of the average retail debt reported by *R* only. Indications are that those debts reported by *I* only are relatively unimportant and may have been omitted, for the most part, accidentally.⁷³

(5) For debts available from both sources, lack of correspondence is evident between the two aggregates. The *R* report is considerably above the *I* report for nonretail debt and below the *I* report for retail debt. (In the former case, however, the sample size is very low, with virtually all the discrepancy being accounted for by one debt.) Reports on roughly half of the retail debts deviated by more than 25 percent from each other. Of these large discrepancies, the *R* report is higher in roughly half of the

⁷³ There is the further possibility that many of these debts were not really debts of more than 30 days and therefore should not have been included. Respondents were asked not to include debts that were being paid off within 30 days or less, whereas the *I* reports included all debts of any length. The nature of the *I* reports made it difficult to determine which debts were indeed of more than 30 days; in cases of doubt, the debts were included.

TABLE 20. COMPARISON OF DEBT REPORTS FROM TWO SOURCES
IN INITIAL INTERVIEWS, BY TYPE OF DEBT, STUDY P2

Category (1)	Source of debt report (2)	Number of debts		Aggregate debt		Average debt	
		Number (3)	Percent (4)	Dollar amount (5)	Percent of total (6)	Dollar amount (7)	Percent of "combined" (8)
A. Nonretail debt							
1. Respondents.....	<i>I</i> and <i>R</i> comparable	6	16	3,604
2.	<i>I</i> report	4,189	12	601	77
3.	<i>R</i> report	5,367	14	698	89
4.	<i>I</i> only	11	30	20,010	19	488	62
5.	<i>R</i> only	20	54	8,971	69	1,000	128
6.	All <i>I</i> reports	17	46	24,199	31	528	67
7.	All <i>R</i> reports	26	70	28,981	83	931	119
8.	<i>I</i> and <i>R</i> combined ^a	37	100	1,550	100	783	100
9. Refusals.....	<i>I</i> report	1	...	1,550	...	1,550	...
10. Noncontacts.....	<i>I</i> report	1	...	1,382	...	1,382	...
B. Retail debt							
1. Respondents.....	<i>I</i> and <i>R</i> comparable	15	19	1,751
2.	<i>I</i> report	1,336	21	117	110
3.	<i>R</i> report	2,286	16	89	84
4.	<i>I</i> only	44	55	4,465	27	52	49
5.	<i>R</i> only	21	26	4,037	53	213	201
6.	All <i>I</i> reports	59	74	5,801	47	68	64
7.	All <i>R</i> reports	36	45	8,502	69	161	152
8.	<i>I</i> and <i>R</i> combined ^a	80	100	980	100	106	100
9. Refusals.....	<i>I</i> report	16	61	...
10. Noncontacts.....	<i>I</i> report	6	...	778	...	130	...

^a Sum of lines 1, 4, and 5 for columns 3 and 4; sum of lines 2, 4, and 5 for columns 5 and 6; and average (weighted by cell size given in column 3) of lines 2, 4, and 5 for columns 7 and 8.

cases and lower in the other half. Since the latter discrepancies are much larger, the *I* reports yield higher debt estimates:

<i>R</i> report relative to <i>I</i> report	Number of debts	Average debt as reported by	
		<i>R</i>	<i>I</i>
Within 25 percent.....	7	\$ 72	\$ 70
Higher by 25 percent or more.....	4	123	79
Lower by 25 percent or more.....	3	89	291

In contrast to previous comparisons, it does not necessarily follow that the higher figures are the more accurate ones.

The result of the lack of correspondence between the debt figures obtained from the two sources is evidenced not only in the differences in average debt, but also in differing distributions of debt by size. The failure of *R* to report many small debts combined with the failure of *I* to pick up many large debts is reflected, for both types of debt, in more elongated distributions for *R* than for *I*. The differences are particularly striking in the case of nonretail debt, where 41 percent of the debt reports obtained from *I* are above \$500 compared with 73 percent of the debts reported by *R*. To a lesser extent the same phenomenon is present in the retail debt distributions.

Analysis of the frequency of debt report by the four experimental variables (p. 81) indicates that neither use nor nonuse of questionnaire forms in the interview nor covering all assets and debts or only some assets and debts made any perceptible difference. However, considerably higher frequencies of debt reports were obtained when the holdings form was used rather than the change form and when the suggestion-and-evaluation advance letter was used instead of a straightforward advance letter. Thus, 21 percent of the savings units interviewed with a holdings form reported one or more debts as compared with 16 percent of those interviewed with a change form; and 21 percent of those interviewed after receiving an evaluation letter reported debt compared with 17 percent of the respondents receiving a regular advance letter. Furthermore, 53 percent of all known debts — that is, debts reported by both *R* and *I* — of those interviewed with a holdings form were reported in the interview as compared with only 45 percent of known debts of those interviewed with a change form. The margin of superiority on this basis of the evaluation letter over the regular letter is even larger — 55 percent over 45 percent, respectively.

Only this last difference is statistically significant (at the .05 significance level). Yet, the differences are consistent enough to leave little doubt regarding their existence. Even more significant is the fact that the combination of the two approaches yields better results than either ap-

proach alone — differences that are consistent on either basis of analysis, savings units, or debts. In either case, the most frequent debt reports were obtained when the evaluation letter was used with a holdings form, and the least frequent debt reports when a regular letter was used with a change form.

Unfortunately, the superiority of the holdings form and of the evaluation letter is obtained only at the cost of apparent reduced response. After an advance regular letter was sent, completed interviews were obtained with 92 percent of those contacted; the response rate when the evaluation letter was used amounted to only 68 percent. Similarly, completed interviews were obtained with 83 percent of those who were contacted and who were to be interviewed with a change form, whereas the corresponding response rate with the holdings form was 72 percent. Both differences are statistically significant.

Apparently, therefore, the evaluation letter and the more comprehensive holdings form act to discourage people who might otherwise cooperate in granting an interview. The people thereby discouraged would seem to be primarily less reliable respondents, with the result that the information supplied by those who do cooperate is, on the whole, likely to be more comprehensive.

Comparative Evaluation

The eight debt-validation studies reviewed in this chapter differ in numerous respects, relating not only to the types of debt being validated but also to the circumstances under which the survey was conducted, the conditions of the interview, and the types of data that were sought. As a result, despite the limited scope of each, taken together these surveys provide much information on the accuracy with which debt is reported in consumer surveys.

Some Generalizations

Perhaps the most significant generalization to be drawn from these studies is how the reliability of debt information varies with many different characteristics, among which are the types of debt being validated, the format of the questionnaire, the types of data requested, the conditions of the interview, the abilities of the interviewer, the personality of the respondent, and the attitude of the respondent toward the study.

At the same time, certain general characteristics of errors in the report of debt appear to underlie all of these studies. One is the tendency for the error in the sample-derived estimate of average, or of aggregate, debt to be in the direction of underestimation. This is true even in the case of the Federal Reserve Survey of car-buyer debt (Study F1) in which the aggregate error was at times less than 1 percent. It is also true of the study of

cash loans, one of the most sensitive debts, in which the error was far more substantial. Indeed, the variability in degree of underestimation among the different studies tends to support the view that the more sensitive is the particular debt being covered, the larger is the degree of underestimation.

A second general phenomenon, which serves largely to explain the one just postulated, is that the principal source of error in estimates of means or of totals is nonreporting of debt by respondents. This factor accounted for the largest share of the error of underestimation in every case for which allocation of error by cause was feasible. The relative importance of this factor increased in magnitude with increases in the extent of underestimation, leaving little doubt that this factor was primarily responsible for instances of substantial underestimation of debt.

A third general phenomenon is the tendency for response errors in debt that was reported to be relatively small. As a general rule, these response errors tended toward overstatement rather than understatement, with the result that they served to offset partly the substantial underestimation due to respondent nonreporting (especially in Studies S2b and P3).

Still another general phenomenon is the tendency for errors in estimates of distributions of debt to vary with the type of debt. When comparisons were possible, primarily in the car-buyer study (F1) and in the farm study (P3), it was found that the debt distribution obtained from the respondent reports approximated fairly closely the debt distribution of the entire sample. Certain errors of a consistent nature were apparent, deriving partly from the nature of the response error patterns, but the errors were not substantial.

These consistent errors were essentially of two types — a tendency to underestimate the frequency of small debts and a tendency to overestimate the frequency of large debts. Study P2, covering all consumer nonmortgage debt (but only by *ex post* validation) suggests that the same phenomenon may be characteristic of all debt, particularly because of the frequent, apparently accidental, oversight of small debts.

A more tenuous generalization is that those who refused or could not be contacted were more in debt than those who were interviewed. Support for this phenomenon is obtained from three of the urban studies (F1, P1, and P2) but is not supported by the study of farm debt (P3).

A final broad inference from these results is that the effect of bias on the reliability of estimates may at times be more substantial than its effect on the estimates themselves. Indeed, a relatively small bias in particular estimates may be deceptive, serving to hide a substantial bias in confidence intervals obtained with the sample data. Such was the case with the Federal Reserve car-buyer study in which, despite very small biases in

estimates of the population averages, the usual two-sigma confidence intervals based on the sample data had virtually zero probability of including the true population averages, for three of the four characteristics tested.

As a general working proposition, it appears that if a substantial bias is present in sample estimates, confidence intervals based on the sample data are very likely to underestimate substantially the true interval. However, relatively low biases in sample estimates do not necessarily indicate correspondingly low biases in estimates of the standard errors of these estimates. Whether or not the latter is true depends on the variance in the population and, to a large extent, on sample size. In this respect, the effect of the latter tends to run counter to the usual precepts of sampling theory: the larger is the sample size, the more influential are the non-sampling errors and the more likely are the confidence intervals of the estimates to be understated substantially.

Distinctive Factors

The studies provide considerable support for the view that the accuracy of debt reported is affected by a host of different factors relating in part to the conditions of the interview, in part to the respondent, and in part to the interviewer himself. One of the most impressive findings is that the type of approach and the type of questionnaire used may affect strongly the reliability of the data. Thus, strong evidence is provided for the superiority of a sealed-envelope technique (Study S4), for the greater reliability of holdings data compared with data on change in debt, and for the desirability of using an approach whereby the respondent participates in the evaluation of the interview instead of playing a purely passive role (Studies P2 and P3). It may seem somewhat paradoxical that the latter two benefits are obtained only with the aid of appreciably lower response. However, it is this lower response that brings about the higher reliability, because the stricter demands of the requests for holdings and for evaluation of the interview serve to weed out respondents who apparently would not have been inclined to provide accurate information anyway.

There is also evidence to support the view that a relatively short questionnaire focusing more directly on the particular debt under study is likely to produce more accurate data, as is also true of giving the interviewer advance knowledge of the holdings of the respondent. However, neither of these conditions may be feasible in an actual study.

The foregoing findings should not be taken as suggestive that all variations in interview approach may influence the accuracy of the data supplied. Thus, in the present studies, no noticeable differences in accuracy were obtained whether the interview covered all assets and debts

or only selected assets and debts. Similarly, no differences were obtained in the frequency of debts reported whether dollar figures were or were not requested.

Accuracy of information is also affected by the characteristics of the respondent. Although insufficient data were collected to permit any conclusive inferences, the personality of the respondent and his attitude toward the study may be more relevant in this connection than his socioeconomic characteristics.

Study P3 suggests that the interviewer may not only influence the accuracy of report but under certain conditions may be able to gauge this accuracy. Interviewers who are more dominant appear more likely to obtain accurate and complete information, although partly at the expense of a lower rate of response. This is another manifestation of the "weeding out" phenomenon noted in the comparison of the holdings form versus the change form.

The Validation Process

The studies summarized in this chapter serve to illustrate the difficulties encountered in validating survey data. The results suggest that in certain situations the institutional reports, although covering presumably the very same holdings, may be more incomplete than the consumer interview data. Indeed, in these circumstances the primary use of data from the external source would be not for validation per se but for supplementing and adding to the data supplied in the interview. So far as validation in the case of debt is concerned, a central institutional source — even one that presumably has the complete support of the retailers and lending institutions in the area — may not provide the comprehensive coverage needed for validation.

IV. VALIDATION STUDIES: TIME DEPOSITS

Time deposits and debts are in many ways at the opposite ends of the spectrum. Debts are widely discussed, and their ownership is not generally considered to be a confidential matter. Ownership of time deposits is not considered confidential either, but the full extent and amount of such ownership is to many people probably the most confidential aspect of their financial (and in many cases, personal) life.

For this reason, a proper validation study of time deposits is, on the one hand, more difficult to conduct but, on the other hand, it may yield far more useful information than validating almost any other type of financial holding. Indeed, such a study subjects survey techniques to the acid test. If any data are likely to be in error, time deposits are the first possibility. Hence, to the extent that errors in the reporting of time deposits can be pinpointed and remedial measures taken, data of greatly improved reliability could be expected for all types of financial holdings.

Despite the difficulty in conducting such studies, several have been carried out in the past few years — most of them in this country and one in the Netherlands. We shall first review the studies conducted in the United States and later the study carried out in the Netherlands. In the last part of the chapter an attempt will be made to bring together and compare the results of these studies.

The Initial Study (Study S1)⁷⁴

As a partial check on the Survey of Consumer Finances, the Survey Research Center undertook in the summer of 1954 a pilot study to measure the reliability of individual reports of time deposits and of changes in these accounts. A sample of accounts was selected from the files of a savings institution, the sample being stratified by size of account, by account activity, and by local area; the purpose of the last-mentioned was to

⁷⁴ This section is based on an unpublished memorandum by James Morgan, who directed the original study. This and the other Survey Research Center studies described in this chapter are discussed in considerably more detail in Lansing, Ginsburg, and Braaten, *op. cit.*

TABLE 21. PERCENTAGE DISTRIBUTION OF NUMBER AND AGGREGATE AMOUNTS IN VALIDATED ACCOUNTS, BY OUTCOME OF INTERVIEW ATTEMPT

Outcome of interview attempt	Percentage of	
	Amounts	Accounts
Nonsample addresses		
Trusts for minors.....	1.7	10.0
Estates of deceased.....	3.6	.5
Armed services address.....	3.5	.5
Address out of sample area.....	2.8	4.6
P. O. box or inadequate address.....	3.1	1.1
Other.....	1.7	2.7
Subtotal.....	16.4	19.4
Sample addresses: nonrespondents		
Refusal.....	16.9	4.2
Noncontact.....	22.8	13.9
Address not used.....	7.1	2.5
Subtotal.....	46.8	20.6
Sample addresses: interviewed.....	36.8	60.0
Total.....	100.0	100.0

facilitate interviewing. After this stratification had been carried out and ineligible addresses had been removed, the sample consisted of 133 names and addresses.

The interviews with the sample members were relatively short. Questions dealt with how the family was doing financially, the occupation and income status of the family, and its debts and savings in various forms. In connection with saving accounts, sample members were asked about the number of accounts, the type of account (single or joint), and, if more than one account, whether or not they were all in the same institution. However, no attempt was made to ascertain names of institutions or, if more than one account, the amount in each institution. The sample member was also asked for some judgment on the accuracy of the dollar figures reported (although there was apparently no pressure to consult records), and the interview closed with a few standard questions on demographic characteristics.

Interviews were obtained with 95, or 71 percent, of the sample households. Refusals were encountered at 17 percent of the households, and at 11 percent of the households nobody could be contacted.

Comparison of the respondents with the refusals, noncontacts, and other sample categories, shown in Table 21, reveals sharp differences in

TABLE 22. PERCENTAGE DISTRIBUTION OF ACCOUNTS,
BY REPORTED AND ACTUAL SIZE

Account balances (1)	Actual (2)	Reported	
		Excluding nonreporters (3)	Including nonreporters (4)
\$0.....	23.4
\$1 to \$199.....	40.2	30.5	23.4
\$200 to \$499.....	14.3	11.8	9.1
\$500 to \$999.....	9.1	13.5	10.4
\$1,000 to \$1,999.....	5.2	17.0	13.0
\$2,000 to \$4,999.....	10.4	17.0	13.0
\$5,000 to \$9,999.....	13.0	6.8	5.2
\$10,000 to \$24,999.....	5.2	1.7	1.3
\$25,000 and over.....	2.6	1.7	1.3
Total.....	100.0	100.0	100.1
Base number.....	77	59	77

average holdings among these categories of people. Accounts of those interviewed were considerably smaller than the average — this group accounted for 60 percent of the number of accounts in the total sample (including those of persons at ineligible addresses) but for only 37 percent of the aggregate balance in these accounts. In contrast, accounts of refusals and of noncontacts were considerably larger than the average. This was even true, interestingly enough, of accounts of sample members at eligible addresses that were not used. Inadvertently, apparently, a selection process appears to have been at work in the classification of such addresses.

The account balances of those at ineligible addresses were, in the aggregate, close to the sample average. However, considerable variation is evident by subcategories, with trust account balances for minors being much lower than the average, whereas accounts of estates, of armed services personnel, and of those with inadequate addresses had much higher balances than the average.

To judge by these differences, therefore, bias was already present in the results by virtue of inability to contact all sample members. Table 22 shows that appreciable response error was also present in the study. A clear tendency is evident for the number of small accounts (including zero accounts) to be overstated and for the number of large accounts to be substantially understated. Thus, 16 of the accounts of the respondents contained \$5,000 or more, but only six such accounts were reported. The discrepancy decreases somewhat if nonreporters are excluded from the distribution, as is done in column 4 of Table 22. Nevertheless, the same

TABLE 23. PERCENTAGE DISTRIBUTION OF ACCOUNTS,
BY REPORTED AND ACTUAL DOLLAR AMOUNT OF CHANGE

Amount of change	Actual	Reported
Up \$1,000 or more.....	8.8	2.9
Up \$500 to \$999.....	10.3	7.4
Up \$200 to \$499.....	5.9	5.9
Up \$1 to \$199.....	28.0	8.8
No change.....	14.7	39.7
Down \$1 to \$199.....	7.4	2.9
Down \$200 to \$499.....	10.3	14.7
Down \$500 to \$999.....	2.9	2.9
Down \$1,000 or more.....	11.7	14.7
Total.....	100.0	99.9
Base number.....	68	68

tendency of understatement of the frequency of large accounts remains, not to mention the fact that an actual study would ordinarily contain no basis for identifying nonreporters.

Comparison of the characteristics of those having a large response error (\$200 or more) with those who were more accurate indicated that the following groups were much more likely to report accounts with large errors: those who owned their own business, people over 45 years of age, those who had large accounts (\$5,000 or more), and those with active accounts (one or more withdrawals in the previous five years). To some extent, these factors undoubtedly interact with each other — for example, older people are more likely to have large accounts — so that these findings alone do not necessarily establish the significance of these variables.

Since balance data were requested not only as of the date of the interview but also as of one year earlier, estimates of change in the accounts could be compared with the actual change for those 68 respondents who reported this information. The resulting saving distribution, shown in Table 23, also appears to depart substantially from the actual distribution. In particular, the frequency of positive saving is greatly understated, especially the frequency of large positive saving. Thus, 10 percent of the accounts were reported up by \$500 or more, whereas the actual percentage was over 19 percent. The frequency of "no change" was overstated substantially — 40 percent of the respondents made this assertion — although it was true for only 15 percent of the accounts. Inferences with regard to aggregate change based on the respondent's reports would have been highly unreliable. The sample data suggest that, on the average, saving in these accounts decreased, whereas it actually increased.

TABLE 24. RESPONSE AND NONRESPONSE ERRORS
IN REPORTS OF TIME DEPOSITS, STUDY S1

Category (1)	Sample size (number of accounts) (2)	Average size of account ^a	
		Actual (3)	Reported (4)
Total sample.....	133	n.a.	\$1,464
Respondents.....	95	\$2,922 ^b	1,547 ^b
Nonreporters.....	18	2,692	0
Balance refused, or not obtainable.....	17	n.a.	n.a.
Balance reported.....	60	2,991	2,389
Nonrespondents.....	38	n.a.
Noncontacts.....	15	n.a.
Refusals.....	23	n.a.

^a Estimated from frequency distributions in memorandum by James Morgan.^b Excludes accounts for which balance was refused.

n.a. Not available.

A study of the characteristics of respondents associated with inaccurate reports of change yields some of the same results as in the case of inaccurate reports of balances. Inaccurate reports were more likely to be given by those owning a business, by those over 45 years of age, and by those with large accounts. On the other hand, active accounts did not seem to be inaccurate more frequently than inactive accounts, and there was some tendency for persons at higher-income levels to be accurate more often than others.

The results of these various biases in the reports of balances are brought together in Table 24. As is evident from this table, the average (or aggregate) reported account understates the true figure by about 45 percent. Much of this is the result of nonreporting of accounts, but, in addition, this tendency is aggravated by the understatement of accounts that were reported. Unfortunately, data on the holdings of the nonrespondents are not available, so no allocation of error is possible in this study, other than the fact that 90 percent of the response error is due to nonreporting.

R reinterview Validation (Study S3)

Background

This was the first of several time-deposit validation studies carried out for the Consumer Savings Project by the Survey Research Center under a subcontract arrangement. It was designed to investigate the nature of the response and nonresponse errors in time deposits obtained from consumer interviews and the manner in which these errors varied.

In the latter connection, two experiments were built into this study. One experiment involved the use of a structured approach with half of the sample and an unstructured approach with the other half. In the

former case, interviewers were required to make use of a highly structured questionnaire (shown in Appendix B of Lansing, *et al.*), and to ask individual questions in the same order as they appeared on the questionnaire. Interviews with the other half of the sample were conducted by means of an unstructured approach, with the interviewer having only a four-page form on which to record dollar figures and other specified information. With this approach, the interviewer was left free to ask whatever questions seemed most suitable and to seek the required information by whatever sequence appeared most appropriate.

The second experiment consisted of reinterviewing the same respondents approximately six months after the initial interview. In this reinterview, respondents were asked not only for their current balance but also for the balance as of the date of the first interview. If there was any discrepancy in the originally reported balance and the balance reported for that date in the second interview, the respondent was asked to reconcile the difference. In this way, it was hoped that the experiment would throw light on the reliability of financial data obtained for an earlier period, as well as on the value of a reconciliation procedure.

For this study a probability sample of account holders was selected in a metropolitan area which, after removing ineligible names and addresses, numbered 168. The sample was restricted to individual or joint accounts of \$1,000 or more owned by one or two adults living in the metropolitan area.

The questionnaire was similar to that employed in the Survey of Consumer Finances, the sample members being requested to cooperate in a study of money management and financial practices. Dollar amounts were requested for all assets and debts, although no attempt was made to obtain names of institutions. The unstructured interview covered much the same material, except that the order of the questions was at the interviewer's discretion, and questions on other subjects could be asked in addition to those for which information was required.

Interviewers used in the study had had considerable experience with the Survey of Consumer Finances. Since they were accustomed primarily only to the structured approach, they were given additional training in the conduct of an unstructured interview.

The remainder of this section presents the principal findings obtained from the initial interviews. The results of the reinterviews belong with the discussion of the effects of a panel operation and will therefore appear in Chapter VI.

Response and Nonresponse Errors

The response rates obtained in this study are shown in column 2 of

TABLE 25. AVERAGE SIZE OF ACCOUNT, BY OUTCOME OF INTERVIEW ATTEMPT, STUDY S3

Category (1)	Number of accounts (2)	Average size of account		Percentage of deviation, reported from actual ^a (5)
		Actual (3)	Reported (4)	
Total sample	168	\$3,579	\$2,105	41.1
Respondents	109	3,809	2,105	44.7
Nonreporters	28	4,212	0	100.0
Balance refused	20	4,768
Balance given	61	3,310	2,832	14.4
Nonrespondents	59	3,122
Refusals	34	3,155
Other	25	3,076

^a $\frac{\text{Actual} - \text{reported}}{\text{Actual}}$

Table 25. Interviews were carried out with 65 percent of the 168 sample members; 20 percent refused and 15 percent either could not be contacted or were not approachable for some other reason.

Column 3 shows that the account balances of the nonrespondents averaged considerably below those of the respondents, which was contrary to the findings of Study S1. However, the accounts of the nonrespondents averaged considerably above the *reported* average size of accounts of the respondents. As a result, the reported accounts of the respondents tend to understate the actual accounts of the nonrespondents, even though the true situation is the reverse. (In this respect, it is interesting to note the little difference between the average size of the accounts of the refusals and that of the accounts of the noncontacts.)

As was the case in the previous study, Table 25 indicates the presence of substantial response, as well as nonresponse, errors. Of the 109 respondents, 26 percent denied having an account, and an additional 18 percent reported an account but refused to give the balance. Furthermore, of the 55 percent who did report an account and the balance, the average understated the true figure by 14 percent.

If we combine the understatements of those who did report a balance with the failure of many respondents to report an account, the average reported size of account is seen to understate the actual figure by roughly 45 percent.⁷⁵ Since the balances of the nonrespondents were less than

⁷⁵ This estimate is obtained on the assumption that the average size of account of those refusing to state a balance is equivalent to the average size account of those who did report a balance. This is a somewhat extreme assumption since, at least by hindsight, there is a strong basis for believing that those who refuse dollar figures generally have more than those who report dollar figures.

those of the respondents, the overall understatement is somewhat less, namely, about 40 percent.

The error in these estimates can be allocated among four causes: error due to faulty estimates of the balances of the nonrespondents, error due to the nonreporters, error due to inaccurate estimates of balances refused, and response error.⁷⁶ In view of the consistent understatement, it is not surprising to find that in this case all the components of error are positive, and thus each of these four factors serves to magnify the extent of error in the overall estimate. The allocation is as follows:

Error in estimated balances of the nonrespondents.....	23.1%
Error due to the nonreporters.....	48.8
Error in account estimates of those refusing balances.....	16.0
Response error.....	12.1
	<hr/> 100.0%

Hence, failure to report the existence of accounts is again the principal cause of error, in this instance accounting for roughly half of the total. Contrary to the findings of previous studies, error in the estimated balances of the nonrespondents is also fairly substantial, being compounded of a sizeable understatement of accounts by the reporters and a relatively large number of nonrespondents.

The substantial understatement in the average balance also serves to produce a sizeable underestimate of the true standard error of the mean. The pertinent data are shown at the top of the following page.

⁷⁶ The error allocation is based on the following segmentation, similar to that used previously. Denote relative proportions of sample members in particular categories by P , average actual balance by A , and average reported balance by X , with subscripts representing the following: s , nonrespondents; o , respondents, account not reported; 1 , respondents reporting account but refusing balance; 2 , respondents reporting account and balance. The actual average balance per sample member is, then:

$$A = P_s A_s + P_o A_o + P_1 A_1 + P_2 A_2.$$

The estimated average balance is:

$$X = P_s X_s + P_1 X_2 + P_2 X_2,$$

where

$$X_s = \frac{P_1 + P_2}{P_o + P_1 + P_2} X_2,$$

on the assumption that average balances are attributed to nonrespondents on the basis that, (a) the proportion of nonrespondents owning accounts is the same as the (reported) proportion of respondents owning accounts, and (b) the average balances of nonrespondents account owners is the same as that reported by P_o .

The error (E) is, then:

$$E = P_s (A_s - X_s) + P_o A_o + P_1 (A_1 - X_2) + P_2 (A_2 - X_2),$$

which serves as the basis for the error allocation.

$k = E/\sigma_{\bar{y}}$	6.7
r	-.59
Apparent standard error of the mean ($\sigma_{\bar{y}}$)	\$ 253
True standard error of the mean (\sqrt{MSE})	\$1,727
Error-free standard error ($\sigma_{\bar{x}}$)	\$274

The reason for the very high value of the true standard error is the dominance of the expression by the bias in the estimate of the mean, \$1,705. The negative correlation between the reporting errors and the true values, -.59, is itself the result of the high incidence of nonreporting, which serves to assign all the nonreporters opposite values of X and E . This same phenomenon is responsible for causing the standard error of the mean in the absence of response error to exceed the apparent standard error: the latter is spuriously low because of the many zero-reported balances.

The substantial understatement in account balances also results in the size distribution of accounts as obtained from the interview data greatly overstating the frequency of small accounts and understating the frequency of large accounts, as shown in Table 26. If nonreporters are included, about 45 percent of the accounts were reported to contain less than \$1,000, whereas the true figure for this group was less than 4 percent.⁷⁷ Even if nonreporters are excluded, about 18 percent of the respondents still alleged the balance in their account to be less than \$1,000.

At the other end of the scale, about 28 percent of the respondents had over \$5,000 in their accounts, but accounts of this size were reported by a little over 12 percent (including those who refused dollar figures), and by 18 percent of respondents who reported a dollar balance.

Table 26 also brings out rather strikingly the much higher frequency of large accounts both among nonreporters and among those who refused balances than among those who gave dollar figures. Thus, whereas 18 percent of the latter had accounts of more than \$5,000, this was true of 36 percent of the nonreporters and of 45 percent of those who refused balances.

Further investigation revealed a number of relationships between response and nonresponse errors and other variables. In particular, people with smaller accounts were more likely to report the account and to report it accurately. A similar tendency, although less strong, was apparent for income level, accuracy being higher among those with lower incomes. In addition — not surprisingly in view of the preceding results — greater accuracy was associated with increased education and with white-collar rather than blue-collar occupational status. On the other hand, there was

⁷⁷ People with accounts of less than \$1,000 at the time of sample selection were excluded. However, the validation was carried out as of the date of the interview, so that it was possible for some accounts to be actually less than \$1,000.

TABLE 26. PERCENTAGE DISTRIBUTION OF ACCOUNTS OF RESPONDENTS, BY SIZE AND TYPE OF RESPONSE, STUDY S3

Size of account (1)	Actual				Reported	
	Total (2)	Non-reporters (3)	Balance refused (4)	Balance reported (5)	Excluding non-reporters (6)	Including non-reporters (7)
\$0.....	31.5
\$1 to \$499.....	13.1	9.0
\$500 to \$999.....	3.7	10.0	3.3	4.9	3.4
\$1,000 to \$1,499.....	13.8	7.1	21.3	24.6	16.8
\$1,500 to \$1,999.....	18.3	14.3	15.0	21.3	11.5	7.9
\$2,000 to \$2,999.....	14.7	14.3	5.0	18.0	9.8	6.7
\$3,000 to \$3,999.....	14.7	14.3	20.0	13.1	18.0	12.4
\$4,000 to \$4,999.....	7.3	14.3	5.0	4.9
\$5,000 to \$6,999.....	11.9	14.3	20.0	8.2	8.2	5.6
\$7,000 to \$8,999.....	7.3	14.3	10.0	3.3
\$9,000 or more.....	8.3	7.1	15.0	6.6	9.8	6.7
Total.....	100.0	100.0	100.0	100.0	99.9	100.0
Base number.....	109	28	20	61	61	89
Average balance...	\$3,809	\$4,212	\$4,768	\$3,310	\$2,832	\$1,941

little evidence that age of the respondent or sex was related to accuracy of report.

Respondents who reported keeping records were also more likely to report accurate data than those who reported not keeping records (although no pressure was put on the respondents to consult these records or a passbook in giving dollar figures). People who used their accounts frequently were less likely to make errors, particularly in regard to overstating the true figure.

The structured approach was more successful than the unstructured one. The response rate in the former case was considerably higher (77 percent versus 54 percent) and response errors were less numerous. Nevertheless, the significance of this finding remains doubtful because, in retrospect, the interviewers appear to have been unsure of themselves in using the unstructured approach and do not seem to have received sufficient training to have enabled them to handle the many different problems that arose in the use of this approach.⁷⁸

A Third Validation Study (Study S5)

Background

Study S5 was carried out by the Survey Research Center about a year

⁷⁸ Lansing, Ginsburg, and Braaten, *op. cit.*, pp. 61-62.

after Study S3 and concurrently with the study of cash borrowers (Study S4). Its purpose was to explore further the factors contributing to response error in reports of time deposits and to see if certain additional methods might not help reduce this error.

The sample was selected from account holders in another metropolitan area and was limited to accounts between \$500 and \$15,000 in the names of adults. As in the study of cash loans, the sample included a number of names and addresses selected from the area telephone directory, partly to safeguard further the anonymity of the names and addresses coming from the institutional sources and partly to make the survey more realistic from an interviewing standpoint. After the completed questionnaires were turned in, those for people selected from the telephone directory were excluded from the analysis.

Two experimental approaches were tested in this study. One approach involved interviewing the head of the household half the time and the wife of the head the rest of the time. The second approach consisted of offering a \$10 payment on half of the interviews but not on the other half. As before, these experiments were carried out by means of a randomized factorial design, in this case dividing the sample into four equal random segments and using a different combination of approaches with each segment.

The questionnaire was a general one relating, as in Study S3, to different aspects of financial management and money-handling practices. A structured approach was used in all interviews. Financial data were to be supplied on forms which the respondent filled in and sealed himself — the “sealed-envelope” technique used in Study S4.

An additional feature was the use of a follow-up mail questionnaire to obtain data on respondent reactions to the interview. In this questionnaire, questions were asked to probe the attitude of the respondent to the interviewer, to the study and its sponsor (the Survey Research Center), and to the interview itself. The questions asked, and their relationship to these categories, were as follows:⁷⁹

- (1) *Forces arising out of the interpersonal relation between the respondent and the interviewer.*
 - (a) General attitude toward the interview as a whole.
Question: How would you feel about being interviewed again?
 - (b) Attitude toward the interviewer.
Question: How did you like the interviewer as a person?
 - (c) Attitudes toward the content of the interview.
Questions: How interesting did you find the interview? How well did the interviewer succeed in making clear to you what the study was about?

⁷⁹ *Ibid.*, pp. 154-55.

- (2) *Forces arising out of the relation of the respondent to the project and its sponsor and to the University.*

Questions: How do you feel about whether surveys like this one are a good idea? During the interview did you have any doubts as to whether you should be giving the information?

- (3) *Forces arising out of other attitudes of the respondent.*

Questions: Were there any questions which you thought were too personal or prying? Do you think people will give us accurate information about their finances or not?

Estimates of Holdings

After eliminating ineligible names and addresses, the sample consisted of 153 family units. Of this number, interviews were completed with only 58 percent, due primarily to refusal to grant an interview by one out of every three of those contacted. By comparison, the refusal rate was 23 percent in Study S3, 17 percent in Study S1, and less than 9 percent in the cash-loan study, Study S4.

Two reasons are advanced to explain the higher refusal rate of Study S5. One was the lesser degree of direct supervision. At the time of the study, the Survey Research Center did not have any field supervisors in that area (contrary to the situation in the areas where preceding studies had been conducted), so that these interviewers were left more to their own resources. Second, considerable interviewer dissatisfaction was reported with the experiment involving an offer of cash to the respondent. Although this should not of itself affect the likelihood of obtaining an interview, substantial differences appear nevertheless in the response rates of sample members who were offered payment as compared with those who were not offered payment. Interviewers in the former situation managed to interview two-thirds of the sample members, or three-fourths of those contacted; whereas among sample members who were offered a cash payment, interviews were carried out with only 60 percent of those contacted. As was the case in the experiment with the unstructured approach, the fact that the interviewer was going to be doing something which was either disagreeable or for which the reason was not clear apparently influenced his approach to the respondent and, accordingly, the likelihood of obtaining the interview.

From a validation point of view, Table 27 indicates that the high rate of nonresponse was doubly unfortunate because, as in previous studies, the holdings of the nonrespondents appear to have differed appreciably from those of the respondents. In this study, the accounts of the nonrespondents averaged higher than those of the respondents, so that any underestimation of holdings by the respondents would be magnified when used as a basis to estimate the holdings of the nonrespondents.

TABLE 27. AVERAGE SIZE OF ACCOUNT, BY OUTCOME OF INTERVIEW ATTEMPT, STUDY S5

Category (1)	Number of accounts (2)	Average size of account		Percentage of deviation, reported from actual (5)
		Actual (3)	Reported (4)	
Total sample.....	153	\$2,572	\$1,742	32.3
Respondents.....	89	2,394	1,742	27.2
Nonreporters.....	22	2,168	0	100.0
Balance refused.....	13	3,273
Balance given.....	54	2,274	2,314	-1.8
Nonrespondents.....	64	2,819
Refusals.....	44	2,704
Other.....	20	3,072

Table 27 shows substantial understatement of holdings by the respondents, primarily because of the failure of one-fourth of those interviewed to report the validated account. In addition, those who refused to report a balance, representing 15 percent of those interviewed, had substantially higher holdings, on the average, than did those who reported balances. Hence, even though response errors among those who reported balances were negligible, the estimated average reported account size understates the actual figure by 27 percent. Since the holdings of the nonrespondents averaged higher than those of the respondents, the estimated average account balance for the entire sample understates the true figure by even more, 32 percent.

The allocation of this error by the four principal sources of error is as follows:

Error in estimated balances of the nonrespondents.....	54.3%
Error due to nonreporters.....	37.6
Error in account estimates of those refusing balances.....	9.8
Response error.....	-1.7
	<u>100.0%</u>

In contrast to most of the previous validation studies, the principal source of error in this case is faulty estimates of the account balances of the nonrespondents — stemming in part from the large proportion of the sample in this category and in part from the substantial difference between their average holdings and those reported by the respondents.⁸⁰ Failure to report accounts is the other principal source of error.

⁸⁰ Note that this error depends to a large extent on the difference between the *actual* holdings of the *nonrespondents* and the *reported* holdings of the *respondents*. The latter in turn is affected substantially by nonreporting. The result is that even if those who report balances are fully accurate, and even if the holdings of the respondents correspond with those of the nonrespondents, error in this component will appear to the extent that accounts are not reported.

TABLE 28. PERCENTAGE DISTRIBUTION OF ACTUAL AND REPORTED ACCOUNTS, BY SIZE, STUDY S5

Size of account (1)	Actual (2)	Reported ^a	
		Excluding nonreporters (3)	Including nonreporters (4)
\$0.....	29.0
\$1 to \$499.....	13.5	16.7	11.8
\$500 to \$999.....	22.5	22.2	15.8
\$1,000 to \$1,999.....	20.2	14.8	10.5
\$2,000 to \$2,999.....	16.8	16.7	11.8
\$3,000 to \$4,999.....	12.4	16.7	11.8
\$5,000 or more.....	14.6	12.9	9.2
Total.....	100.0	100.0	99.9
Base number.....	89	54	76

^a Excludes refusals of balances (13).

The low degree of response error is reflected in a somewhat better comparison of the reported distribution of accounts by size with the actual distribution, as is evident from Table 28. Nevertheless, the relatively high frequency of nonreporting leads to systematic understatement of the frequency of large accounts and to some overstatement of the frequency of small accounts. This is particularly true of the reported distribution as compared with the distribution of all validated accounts by size because of the greater relative proportion of large accounts among the nonrespondents.

The standard error of the mean is again greatly understated, as is apparent from the following data:

$k = E/\sigma_{\bar{y}}$	2.8
r	-.56
Apparent standard error of the mean ($\sigma_{\bar{y}}$).....	\$229
True standard error of the mean (\sqrt{MSE}).....	\$698
Error-free standard error ($\sigma_{\bar{x}}$).....	\$253

In addition, the standard error in the absence of response error is again somewhat higher than the apparent standard error, and for the same reason as before — the high frequency of nonreporters.

Investigation of the factors associated with response error supports previous findings that greater reliability (more frequent reporting of the account as well as more accurate reporting) was associated with higher income, higher education, white-collar rather than blue-collar occupational status, and possibly younger age group. No appreciable differences in reliability were found for sex. A number of variables other than the

demographic ones were also associated with reliability of report. These included whether or not records of family expenditures were kept, whether or not the respondent reported dollar figures (for those who did so) in round numbers, and whether or not the family norms were in the direction of free discussion of financial subjects.⁸¹ In addition, a significant correlation was obtained between interviewers' ratings of respondent accuracy and both frequency of report of the validated account and the response error when a dollar figure was given. On the other hand, no relationship was found between reliability of report and the size of the account, the offer of a cash payment, or the date for which the balance was reported. Virtually the same frequency of nonreporting, as well as of inaccurate reporting, was obtained when balances were requested eighteen months prior to the interview as when balances were requested six months prior to the interview.

Estimates of Change

Since balances were requested from the respondents as of both January 1, 1958, and January 1, 1959, the saving of the respondents in these accounts could be estimated by taking the difference between the two figures. This proved feasible for 48 respondents who reported balance figures on both dates. To judge by columns 2 and 3 of Table 29, the distribution of these 48 respondents by amount of change corresponded reasonably well with that of all respondents; the only appreciable difference is a much higher proportion of the latter who experienced no change in the account balances.

Comparison of the actual and reported change distributions for the 48 respondents for whom such a comparison could be made indicates that with one exception the two distributions do not differ appreciably from each other (columns 3 and 4 of Table 29). The exception lies in the much higher reported frequency of no change: about one-fifth of the respondents made such an allegation although this was actually true for none of them.⁸² Large changes, particularly large increases, were also somewhat understated in this study, as in the other validation studies. Of the nine changes involving \$500 or more, seven were reported in the same interval, but the other two were understated substantially.

⁸¹ The latter was ascertained by asking the respondent how old he was before he knew the family income and how old he thought children should be before being told about family income and savings. The replies to both questions exhibited a strong inverse correlation between reliability of report and age.

⁸² In two of these instances, the balance increased less than \$100, which might be regarded as a negligible difference. In six other instances, the balance increased between \$100 and \$500; one balance increased between \$500 and \$1,000; and the remaining balance decreased between \$500 and \$1,000.

TABLE 29. PERCENTAGE DISTRIBUTION OF ACCOUNTS, BY REPORTED AND ACTUAL AMOUNT OF CHANGE, CALENDAR 1958, STUDY S5

Amount of change (1)	Actual		Reported (4)
	All respondents (2)	Balances reported on both dates (3)	
Up \$2,000 or more.....	4.6	4.2	6.2
Up \$1,000 to \$1,999.....	10.5	10.4	8.3
Up \$500 to \$999.....	10.5	8.3	14.6
Up \$100 to \$499.....	29.1	41.7	20.9
Up \$1 to \$99.....	18.6	16.7	12.5
No change.....	10.5	0	20.9
Down \$1 to \$99.....	3.5	6.2	4.2
Down \$100 to \$499.....	7.0	8.3	6.2
Down \$500 to \$999.....	5.8	4.2	6.2
Total.....	100.0	100.0	100.0
Base number.....	86	48	48

The Mail Questionnaire

The mail questionnaire described on page 102 was sent to all of the respondents within two weeks after the interview. The response was very good, with 80 percent returning the questionnaire; this was the highest response rate achieved to date in a number of such mail follow-up studies of the Survey Research Center.

Analysis of the returns indicated that on all eight questions a favorable response was associated with a higher likelihood of reporting the account, as is shown in Table 30. In addition, reporting the existence of an account but refusing the balance was much more frequent among those who were influenced unfavorably by the interview than among other respondents. Contrary to expectations, however, among those who did report dollar balances, frequency of accurate reporting was no higher among those favorably influenced by the interview than among those unfavorably influenced.

Two Panel Studies

Background

Time deposits were validated in two panel studies of the Consumer Saving Project. Since the methods used in each study were similar, the results will be presented jointly.

Time deposits served as a primary validation source for the urban panel study (P2), all the sample members having been selected by random

TABLE 30. DISTRIBUTION OF MAIL RESPONDENTS, BY REPORTING OF ACCOUNT AND ANSWERS TO MAIL QUESTIONNAIRES

Question	Response ^a	Percent- age of total	Number of nonre- porters	Number refus- ing balance	Number report- ing dollar figure
Clarity of interviewer explanation	Favorable (60)	100	25	10	65
	Unfavorable (11)	100	27	18	55
Interest in interview . . .	Favorable (49)	99	26	6	67
	Unfavorable (22)	101	23	23	55
Attitude toward possible reinterview	Favorable (55)	101	26	7	68
	Unfavorable (16)	100	25	25	50
Attraction of interviewer as a person	Favorable (59)	100	27	10	63
	Unfavorable (12)	99	16	17	56
Value of such surveys . . .	Favorable (63)	99	25	8	66
	Unfavorable (8)	101	25	38	38
Were questions too personal?	Favorable (43)	100	28	5	67
	Unfavorable (28)	99	21	21	57
Doubts about giving information	Favorable (30)	100	27	3	70
	Unfavorable (41)	100	25	17	58
Likelihood of other people giving ac- curate financial information	Favorable (34)	101	24	9	68
	Unfavorable (37)	101	27	14	60

^a Figures in parentheses are number of people giving particular response.

Source: J. B. Lansing, G. P. Ginsburg, and Kaisa Braaten, *An Investigation of Response Error, Studies in Consumer Savings*, No. 2 (Urbana: University of Illinois Bureau of Economic and Business Research, 1961), pp. 159-61.

probability methods from the files of savings institutions. Since the design of this study was described in the preceding chapter, it is not necessary to repeat the details. However, it is pertinent to note that the questionnaires used were general in scope, covering many assets and debts in addition to time deposits. Also, in contrast to the procedure in previous studies covered in this chapter, questions were asked about names of institutions in all instances, so that the problem of account identification, although not eliminated, was simplified considerably.

Stratification of account size was used in selecting the sample members. At the time of sample selection, equal numbers of sample members had accounts in each of the following categories: under \$100; \$100 to \$1,000; \$1,000 to \$5,000; and \$5,000 or more. As was the case in the previous studies, accounts were limited to those held by individuals only.

Because of the split-run experiment involving asking half of the panel

members for holdings and half for change, account balances were not obtained for half of the sample on the first wave. Hence, the analysis that follows deals separately with those who were asked about holdings and those who were asked about change, as was done in the case of debt. Furthermore, since the focus of this chapter is on single-interview studies, the emphasis is on the reports of balances, the only reports which could be validated on the first wave.

The other panel validation study (P3) was conducted in a farm area. Savings accounts served as a primary validation source for 74 of the 409 eligible sample members (farmers) in the study. As was noted in the initial description of this study (pages 64-75), some of these respondents were to be interviewed with a holdings form and others with a change form. In addition, some were sent an advance letter with a straight-forward appeal for cooperation, whereas the others were sent an advance letter asking for the sample member's advice and suggestions on the data-collection procedure. In contrast to Study P2, it was not feasible to stratify accounts by size in the sample selection process.

Response and Nonresponse Errors⁸³

The extent to which accounts were reported and the correspondence between reported and actual balances, by type of form used and by study, are shown in Table 31. Overall, nonreporting of accounts in the urban study was substantial, accounting for roughly 40 percent of all of the validated accounts. Nonreporting was higher on the change form than on the holdings form, 47 percent versus 35 percent, respectively, which conforms to previous findings. Moreover, column 4 of Table 31 indicates that the average size of these nonreported accounts exceeded by a substantial margin the average size of the accounts which were reported and for which balances were given.

The comparisons obtained from the holdings form with regard to the reported and actual sizes of accounts are particularly interesting. Especially pertinent are the following:

(1) The average nonrespondent account is considerably larger than the average respondent account (column 4), primarily because of the large account balances of those who refused to give figures.

(2) Accounts for which balances were given tend to be considerably smaller than accounts which were reported but for which the balance was refused.

⁸³ The presentation of results is based on the author's article, "The Reliability of Consumer Surveys of Financial Holdings: Time Deposits," *Journal of the American Statistical Association*, Vol. 60, No. 309 (March, 1965), pp. 148-63.

TABLE 31. AVERAGE SIZE OF ACCOUNT, BY OUTCOME OF INTERVIEW ATTEMPT AND FORM ASSIGNED

Category (1)	Number of accounts ^a (2)	Number of accounts ^b (3)	Average size of account ^b		Percentage of deviation, reported from actual ^{b,c} (6)
			Actual (4)	Reported (5)	
Study P2: Urban					
Total sample.....	196	207	\$3,066	\$1,476	51.9
Respondents.....	158	151	2,663	1,476	44.6
Nonreporters.....	75	53	2,985	100.0
Figure refused.....	4	7	3,102
Figure given.....	79	91	2,469	2,477	-0.3
Nonrespondents.....	38	56	4,153
Noncontacts.....	8	9	3,062 ^d
Refusals.....	30	47	4,361 ^e
Study P3: Farm					
Total sample.....	35	39 ^f	1,175	390	66.8
Respondents.....	29	32	938	390	58.4
Nonreporters.....	14	7	750	100.0
Figure refused.....	0	2	6,588	100.0
Figure given.....	15	23	504	543	-7.7
Nonrespondents.....	6	7	2,259	100.0
Refusals.....	4	7	2,259	100.0
Noncontacts.....	2

^a Change form.^b Holdings form.^c Actual-reported.^d Actual.^e One account closed.^f Two accounts closed.^g Excludes two validated accounts on holdings form closed before the date of interview.

(3) The average size of reported accounts is less than one-half the actual size of all accounts. In other words, an (unweighted) estimate of aggregate time-deposit holdings for this sample would have understated the actual total by roughly 52 percent.

(4) This deviation represents the accumulated effects of nonreporting, of large accounts for which balances were refused, and of still larger accounts of nonrespondents.

(5) Interestingly enough, the average size of accounts for which balances were reported corresponds almost exactly with the true figure.

The second part of Table 31, which summarizes the extent to which farmers' accounts were reported, indicates that much the same results were obtained in this study. Nonreporting was less frequent than in Study P2, occurring on roughly one-third of these interviews. The discrepancy between the incidence of nonreporting on the change form and its in-

cidence on the holdings form is even larger than before: 48 percent versus 22 percent, respectively. In this case, the difference is large enough to be statistically significant at the .05 probability level.

Here, as was the case with other validation studies, a marked tendency existed for people not to report holdings when asked for change if they had no or little change to report. The feeling of many respondents in such cases appeared to be that since the main interest was apparently in ascertaining change, there was no reason to "bother" the interviewer with additional holdings which might not matter. In addition, the highly personal nature of the request for holdings tended to antagonize so-called "marginal respondents" into refusing to cooperate altogether. The same type of people, however, when asked for change seemed likely to remain in the sample but with a much higher tendency to omit relevant information.

Data obtained from the holdings form indicate that, as on Study P2, the average balance in these accounts was underestimated substantially. Although they were relatively few, the nonreporters still contributed to this underestimate. In addition, the balances of those who refused this information average out to a considerably higher figure than the average obtained from the respondents: \$6,588 versus \$543.⁸⁴ A slight offsetting tendency is apparent in the overestimate of the balances that were reported. The overall result is that the sample average was understated by about 67 percent.

Numerical estimates of the relative importance of these different sources of error in the estimate of the average-sized balance reveals that virtually all the error on Study P2 is attributable almost equally to inaccurate estimates of the balances of the nonrespondents and to nonreporting. The error allocation is as follows:

	<i>Study P2</i>	<i>Study P3</i>
Error in estimated balances of the nonrespondents . . .	48.1%	43.8%
Error due to nonreporters	50.7	17.6
Inaccurate estimates of those refusing balances	1.4	41.5
Response error	— .2	— 2.9
	<hr/> 100.0%	<hr/> 100.0%

On the farm study (P3) however, nonreporting is seen to constitute a relatively small source of error, partly because nonreporting was somewhat less frequent than on other studies. Also, differences between the balances given by the respondents and the actual balances of the nonreporters, as well as between the respondent reports and the balances of the nonrespondents, were relatively larger on the farm study. Although

⁸⁴ It should be noted that there were only two refusals in this category, so it is not clear how typical such a discrepancy may be.

TABLE 32. PERCENTAGE DISTRIBUTION OF VALIDATED ACCOUNTS, BY SIZE AND TYPE OF RESPONSE, HOLDINGS FORM

Size of account (1)	Actual size				Reported size (6)
	Total (2)	Non-re- spondents (3)	Non-re- porters (4)	Balance reported (5)	
Study P2: Urban					
\$0.....	1.5	5.4	36.8 ^a
\$1 to \$99.....	5.5	3.6	9.4	4.4	2.1
\$100 to \$499.....	21.0	17.9	17.0	25.3	15.3
\$500 to \$999.....	11.5	12.5	13.2	9.9	6.9
\$1,000 to \$1,499.....	10.5	7.1	5.7	15.4	11.1
\$1,500 to \$1,999.....	6.5	3.6	7.5	7.7	2.8
\$2,000 to \$2,999.....	9.5	7.1	15.1	7.7	6.9
\$3,000 to \$4,999.....	12.0	12.5	9.4	13.2	7.6
\$5,000 to \$9,999.....	14.0	19.6	13.2	11.0	5.6
\$10,000 or more.....	8.0	10.7	9.4	5.5	4.9
Total.....	100.0	100.0	99.9	100.1	100.0
Base number.....	200	56	53	91	144
Study P3: Farm					
\$0.....	23.3 ^a
\$1 to \$99.....	48.6	28.6	57.1	52.2	23.3
\$100 to \$499.....	10.8	14.3	13.0	26.7
\$500 to \$999.....	10.8	17.4	10.0
\$1,000 to \$1,999.....	10.8	28.6	8.7	6.7
\$2,000 or more.....	18.9	57.2	14.3	8.7	10.0
Total.....	99.9	100.1	100.0	100.0	100.0
Base number.....	37	7	7	23	30

^a Includes nonreported accounts.

the nonrespondents, and particularly the respondents who refused balances, represented relatively small parts of the total sample, the differences in the dollar figures were so large that these two factors accounted for the principal sources of error in this study. In contrast, response errors were very small.

Further information on the correspondence between the actual and reported balances for the urban study (P2) is provided in the top panel of Table 32, which presents size distributions of these accounts by different categories. As is evident from this table, particularly because of non-reporting, the size distribution of reported accounts, shown in column 6, greatly overestimates the frequency of zero accounts. Otherwise, fairly close correspondence would exist between the distribution of reported

accounts and the distribution of actual balances (column 2). This table also serves to bring out the heavy preponderance of large accounts among the nonrespondents (column 3). Over 30 percent of these accounts exceeded \$5,000, which was true of only one out of every five other accounts. The table also shows that the nonreported accounts were concentrated at both ends of the distribution. Over one-fourth contained less than \$500 each, whereas a nearly equal proportion contained more than \$5,000 each.

The effects of these various errors on the distribution of reported accounts by size in the farm study is illustrated in the lower part of the table. On an overall basis, the sample distribution is seen to overstate the frequency of very small accounts and to understate the frequency of large accounts (column 2 versus column 6). Both large accounts and very small accounts are seen to be more frequent among nonreporters than among reporters. As a result, the frequency of large accounts is understated and the frequency of small accounts is overstated.

Effect on Reliability

The following results are obtained in estimating the reliability of the standard error of the mean:

	<i>Study P2</i>	<i>Study P3</i>
<i>k</i>	5.1	4.5
<i>r</i>	-.62	-.92
Apparent standard error of the mean ($\sigma_{\bar{y}}$).....	\$ 211	\$113
True standard error of the mean (\sqrt{MSE}).....	\$1,107	\$590
Error-free standard error (σ_x).....	\$ 256	\$289

It is clear that the errors in these studies introduced substantial distortions in the estimates. Without having access to the validation data, the standard error of the mean on the urban study would have been estimated at \$211, whereas the true standard error of the mean is more than five times that figure. Reflecting this fact is a value for *k* of 5.1. As a result, the usual symmetrical 95 percent confidence interval has a probability of containing the true parameter, not of .95, but of .001. In other words, in this case the 95 percent confidence interval had virtually *no* likelihood of containing the true value.

The intercorrelation between the true balances and the errors in the respondent reports is markedly negative. The reason for this is the high frequency of nonreporting, which serves to couple x_i as the true value with $-x_i$ as the amount of error. Finally, these calculations indicate that had these nonsampling errors not existed, the standard error of the mean would have been roughly one-fourth of the true standard error or, as it turns out, of roughly the same order of magnitude as the apparent standard error of the mean.

TABLE 33. TESTS OF ASSOCIATION BETWEEN SELECTED CHARACTERISTICS AND NONREPORTING, STUDY P2

Characteristic	Chi-square	Degrees of freedom	Significance level
Respondent characteristics			
Education	12.86	3	Less than .01
Occupation	11.82	4	.02
Age	8.42	4	.08
Size of savings unit	8.67	4	.07
Income of savings unit	21.08	5	Less than .01
Size of account	4.87	5	.44
	Value of <i>t</i>		
Cooperativeness: objective measures			
Use of records	3.30		Less than .01
Refusal of information	2.30		.02
Cooperativeness: interviewer ratings			
Accuracy of information	4.10		Less than .01
Completeness in reporting	3.91		Less than .01
Questionnaire approach			
Holdings versus change	2.43		.01
Regular letter versus evaluation letter	1.28		.20
Forms in interview versus no forms	1.32		.19
All assets versus some assets23		.82

On the farm study, the standard error of the mean computed in the usual way is also much too low, again because of the very high negative correlation between the true balances and the errors, so that response errors serve to reduce sharply the apparent sampling variance.⁸⁵ As before, the true standard error of the mean turns out to be more than five times the size of the apparent standard error. Because of this phenomenon and the large bias in the estimate of the mean, a high value of *k* is obtained again, namely, 4.5. In this case, the usual symmetrical .95 confidence interval, based on the apparent standard error, has a probability of containing the true population mean of roughly .005.

Determinants of Response Error

Four broad types of variables were tested for association with the frequency of nonreporting: characteristics of the savings unit, objective measures of respondent cooperativeness, subjective rating of respondent cooperativeness (based on interviewer reports), and the questionnaire approach. The results of these tests for the urban study, shown in Table 33, indicate that most of the variables in each category were related sig-

⁸⁵ This high correlation was due principally to five accounts with balances between \$1,000 and \$7,000, three of which were not reported and two of which were understated by about 90 percent. If these accounts were eliminated, the correlation would be virtually zero.

nificantly to nonreporting. With the exception of the size of the account, all of the respondent characteristics were related significantly to the frequency of nonreporting, or were almost so.⁸⁶ Furthermore, in all instances the relationship is in accord with what might be expected. Thus, the frequency of nonreporting is higher among those with less education, those in a laboring occupation or retired, older people, those with lower incomes, and those who are in larger-sized savings units.

Highly significant relationships are also apparent between nonreporting and the various measures of cooperativeness. As would be expected, frequency of reporting increases with use of records and with a lower incidence of refusals. Particularly noteworthy is the fact that the interviewers appear to be able to detect nonreporting (although not aware that they are doing so) much more frequently than would be expected as a result of chance. The results therefore suggest that interviewer ratings might be of assistance in dealing with this problem. In addition, Table 33 brings out once more the superiority of the holdings form over the change form.

As was noted previously, a substantially higher incidence of reporting is obtained with the holdings form than with the change form, the difference being statistically significant. With the other experiment, however, virtually no effect is apparent, the regular letter yielding, if anything, a slightly lower frequency of nonreporting than the evaluation letter.

Table 34 presents the results of significance tests of the extent to which reporting of an account on the farm study was associated with the same four general categories of variables tested in Study P2. As is evident from this table, in Study P3 many fewer characteristics are related significantly to reporting of the account than in Study P2, perhaps because of the smaller sample sizes involved or perhaps because of the different nature of the sample, focusing, as it does, on a farm area.

Of the respondent characteristics, only income, farm tenancy, and operator status are statistically significant, with tenants reporting the account more frequently than owners, and operators reporting the account more frequently than nonoperators. Although not significant, there is some indication that education, age, and size of savings unit may have some effect, the nature of the relationships being the same as before.

Of the measures of cooperativeness, only use of records is statistically significant, with those using records reporting accounts far more frequently than those who did not. It is interesting to note that a significant relationship is obtained for interviewer ratings of completeness in reporting, but not for interviewer ratings of the accuracy of information. Clearly, these

⁸⁶ Strictly speaking, size of account is not a characteristic of the respondent, although it is classified here under this heading for convenience.

TABLE 34. TESTS OF ASSOCIATION BETWEEN SELECTED CHARACTERISTICS AND NONREPORTING, STUDY P3

Characteristic	Chi-square	Degrees of freedom	Significance level
Respondent characteristics			
Education	3.56	2	.18
Age	4.77	3	.19
Size of savings unit	4.27	2	.12
Income of savings unit	5.95	2	.05
Owner versus tenant	8.54	2	.02
Operator versus nonoperator	13.87 ^a	..	Less than .01
Cooperativeness: objective measures			
Use of records	2.13 ^a	..	.03
Refusal of information	1.15 ^a	..	.25
Cooperativeness: interviewer ratings			
Accuracy of information66	2	.72
Completeness in reporting	7.33 ^a	..	Less than .01
Questionnaire approach			
Holdings versus change	2.22 ^a	..	.03
Regular versus evaluation letter50 ^a	..	.62

^a Value of *t* rather than chi-square.

two questions register different things in the minds of interviewers—in this instance, the dimension of completeness was the more pertinent.

The Netherlands Reliability Survey⁸⁷

Background

In 1958, the Economic-Statistical Department of the Post Office Savings Bank of the Netherlands undertook an experimental survey to investigate (1) the reliability with which individuals in the Netherlands were reporting savings information in personal interviews and (2) the factors that might be related to the reliability of this information. Previous comparison of external information with data from savings surveys conducted in 1950, 1953, and 1955 by the Post Office Savings Bank had raised doubt concerning the reliability of the interview data.

The 1958 survey was undertaken using the accounts in the Post Office Savings Bank as the validation source. The frame consisted of account holders in the Post Office Savings Bank living in four urban areas of the country and having a balance of more than 10 guilders as of January 1, 1958.⁸⁸ To avoid ambiguities in response, the survey was limited to account holders with only one account. It was further restricted to people who were 20 years or more of age and who had opened their own account.

⁸⁷ The material in this selection is based on W. Horn, "Reliability Survey: A Survey on the Reliability of Response to an Interview Survey," *Het PTT-bedrijf*, Vol. 10, No. 3 (October, 1960), pp. 105-56.

⁸⁸ A guilder as of that date was equivalent to 26 cents.

A stratified sample was selected from this population, with stratification proportionate to the percentage of total balances in each of three size classes of accounts: 10-1,000 guilders; 1,000-2,500 guilders; and 2,500 guilders or more. As a result, there was heavy overrepresentation of the larger accounts, with more than one-third of the sample members having accounts over 2,500 guilders, although less than 5 percent of the population had accounts of that size.

The questionnaire covered a variety of topics relating to savings practices and attitudes. Questions were asked about the general economic situation, current income, income expectations, and opinions about the need and means of saving, as well as questions about holdings in savings institutions (including the Post Office Savings Bank), checking accounts, ownership of a business, and a variety of economic and demographic characteristics. Respondents were not told that the survey was being conducted by the Post Office Savings Bank.

Three experiments were incorporated into this survey design, as follows:

(1) The relative efficiency of male versus female interviewers. For this purpose, the field staff consisted of 32 male interviewers and 32 female interviewers.

(2) The effectiveness of a combination of oral and written interviewer instructions versus the use of only written instructions.

(3) The mailing of advance letters to the respondent versus not sending any advance notification.

As was done in the various studies in the United States, these experiments were combined with each other in a factorial design with interviewer-interviewing approaches allocated systematically among the sample members.

Operating Results

Interviews were obtained with nearly 80 percent of the sample members. About 10 percent of the sample refused to be interviewed, and another 12 percent could not be contacted for one reason or another. Most of the latter (about 85 percent of the 12 percent) were not at home or were too ill to be contacted; the rest represented people who had moved or died.

Analysis of the nonrespondents on the basis of institutional information indicated that the noncontact rate was much higher among the youngest (20-34) and the oldest (65 and over) age groups than among others. The high rate in the former case was due primarily to people either not being at home or having moved, whereas the high noncontact

rate in the older age groups was due to the much higher incidence of sickness and death.

No relationship was found between the noncontact rate and the size of the account. Such a relationship was found, however, between the refusals — the refusal rate tended to rise as the size of the account increased — and the reporting of balances. Thus, only 7 percent of the sample members in the lowest account group refused to state a balance, whereas this rate rose to almost 25 percent of those with accounts of 2,500 guilders or more.

Two other factors were found to influence the refusal rate. One factor was sex, with relatively fewer refusals received from men than from women. In addition, a strong upward tendency was found for the rate of refusal to increase with age. The refusal rate varied from 5.6 percent of those aged 20-34 to 14.5 percent of those who were 65 years of age or more.

Although almost 80 percent of the sample members were interviewed, complete information on the savings account was supplied by little more than half of the sample. About 5 percent of the sample members denied having an account and an additional 19 percent, or almost one-fourth of those interviewed, mentioned the existence of the account but refused to give the current balance. The tendency not to report an account was much higher for the smallest accounts than for any others. This phenomenon suggests that, to some extent, nonreporting may have been accidental, reflecting poor memory. Support for this inference was found in the fact that nonreporting in relation to the total number of accounts rose substantially as the interval between the date of the interview and the date of the last transaction increased.⁸⁹

Nonreporting was also related significantly to age and to education, but not to sex. Older people were more inclined not to report an account than those in other age groups, as were people with only an elementary school education. There was some tendency for low-income people not to report more frequently than others, but this may reflect primarily interacting effects of education and (low) size of account.

Interestingly enough, some relationship was found between nonreporting and interviewer characteristics, as well as between nonreporting and respondent-interviewer interactions. Thus, older interviewers were found to receive significantly more denials of account ownership than younger interviewers. In addition, nonreporting was significantly lower in instances involving female interviewers and male respondents than with any other combination.

⁸⁹ Actually the argument is not complete, because Horn fails to demonstrate whether or not the size of the account was inversely related to the interval between the last transaction and the date of the interview.

An age interaction effect was also detected, the frequency of nonreporting tending to rise as the age of the interviewer relative to the age of the respondent decreased.

Errors in Balance Reports

A general summary of the errors in estimates of the sizes of the accounts is presented in Table 35, for the total sample as well as for four different size-of-account categories. The data in the "total" column are weighted by the reciprocals of the sampling fractions used for the different size-of-account categories, to present an unbiased estimate of the population totals.

On balance, the data suggest (lines 1-6) that the average account of the nonrespondents was somewhat larger than that of the respondents and that those who refused to give balances had accounts much larger than the average. At the same time, nonreporters, as well as those who reported both an account and the balance in the account, had considerably smaller accounts than the average.

The data further suggest that these patterns are not always true for all size-of-account categories. Thus, in the lower account categories nonrespondents tended to have smaller accounts than respondents, and it was only for accounts of 2,500 guilders or more that nonrespondents' accounts exceeded those of respondents. In a similar manner, only among the largest accounts was the average for those who gave a balance less than for other sample members. Also, the primary reason for the very low average size of accounts for the nonreporters was a large number of extremely low accounts; at the upper levels, the average account of this group exceeded the average for the sample as a whole. Only for those who refused to give balances did the average account exceed the overall average for all size-of-account categories.

To some extent these results are not too surprising, since undoubtedly various motives interacted with the tendency to report accurately about current sizes of accounts. Thus, with very small accounts, memory error was more likely to play a major role. As the size of account increased, however, interaction between this factor and any suspicion or fear that might have existed on the part of the respondent would have been manifested either in the form of nonreporting or in refusal to give the balance.

The effect of these different errors on estimates of the average balance is shown in lines 7-10 of Table 35. As is evident from line 9, the response error is relatively small, an overstatement of 5 percent. However, this overstatement reflects the cancellation of substantial opposing errors, particularly an overstatement of 71 percent in the estimate of the average account in the lowest category. Fortunately, from this respect the largest

TABLE 35. ACCURACY OF ACCOUNT BALANCES AND ALLOCATION OF ERROR, BY SIZE OF ACCOUNT, STUDY N

Item	Category	Size of account (in guilders) ^a				Total (weighted)
		10 to 100	100 to 1,000	1,000 to 2,500	2,500 or more	
1. Average actual balance	Nonrespondents	90	535	1,568	3,791	749
2.	Respondents	127	508	1,672	3,612	727
3.	Nonreporters	16	412	1,528	3,888	406
4.	Balance not given	139	623	1,814	3,913	1,202
5.	Balance given	150	491	1,636	3,431	669
6.	Total	119	513	1,649	3,654	731
7. Average reported balance	Respondents: balance given	257	536	1,575	3,092	704
8.	All respondents ^b	215	500	1,476	2,894	631
9. Percentage of error: (A-X)/(A)	Respondents: balance given (lines 5 versus 7)	-71	-9	4	10	-5
10.	All respondents versus total sample (lines 6 versus 8)	-81	2	10	21	14
11. Allocation of error (percent)	To nonrespondents	-28.8	52.4	12.0	27.2	19.6
12.	To respondents	-71.2	47.6	88.0	72.8	80.4
13.	Nonreporters	2.1	172.8	42.8	25.4	78.0
14.	Inaccurate estimate for "balance not given"	-9.0	92.6	26.2	26.3	49.9
15.	Errors in balance given	-64.3	-217.8	19.0	21.1	-47.5

^a Figures in body of table refer to account balances reported as of date of the interview, which extended from September 10, 1958, to November 15, 1958.

^b Including nonreporters (with zero balances) and balance refusals (with same size average balance as "respondents: balance given").
Source: W. Horn, "Reliability Survey: A Survey on the Reliability of Response to an Interview Survey," *Het PIT-bedrijf*, Vol. 10, No. 3 (October, 1960), pp. 138-39.

accounts were understated by 10 percent which, because of the size of these accounts, served to almost offset the appreciable overstatement of the smallest accounts.

The best estimates of the average size holdings of the total sample from the interview data are shown in line 8. The overall average is seen to be understated by 14 percent (line 10), again representing the result of cancellation of much larger opposing errors. The average size of the smallest accounts is overstated by 81 percent, which was more than offset by a 21 percent understatement of the largest accounts and by an understatement of 10 percent of accounts in the next largest category.

The error in these estimates is allocated among four different causes in the last six lines of the table, as was done in the previous studies. Overall, the results of this error allocation are seen to correspond fairly closely with those obtained earlier. Nonreporting is the principal cause of error, leading, with errors in the estimates of refused balances, to understatement of the true figure. Response errors are in the direction of overestimation and hence help to offset to some extent the other causes of error. However, strikingly different error patterns are observed by size-of-account categories. This is especially true of the smallest accounts, where response errors represent most of the error in the average size of the account, whereas errors due to nonreporting are negligible. In this particular case, the average size of the accounts not reported was so small that even the relatively high frequency of such accounts exerted little effect on the overall estimate.

The error in the estimate of the average size of accounts in the 100-1,000 guilder range is particularly interesting. Although the overall error is almost negligible — an underestimate of 2 percent — it is seen to be the result of the cancellation of relatively large error tendencies in opposing directions. Error due to nonreporting was almost twice the size of the overall error, whereas the error in the estimate of balances refused almost equalled the overall error. At the same time, the response error in this case was both relatively substantial and in the opposite direction to the other errors, so the resulting cancellation of errors produced a close overall estimate.

Determinants of Errors in Balances

A number of factors were found to influence the reliability of the report of account balances. Among these were the following:

(1) *Willingness to consult bankbooks.* If a respondent did not consult his bankbook in giving the balance figure or did not refuse to answer, he was told: "We should like to know exact amounts. Are you willing to look (at your account) and tell us exactly how much you have?" Those

who had consulted their passbooks previously, or did so in response to this question, gave considerably more accurate information than those who did not consult bankbooks. In the former case, the overall error was almost negligible, compared with underestimates of the order of 20 percent for those who gave a figure but refused to consult their bankbook.

(2) *Size of account.* Small accounts tended to be overstated and large accounts to be understated. When the balance reports were arranged in descending order and combined into groups of 100, a linear relationship was obtained between the error and the size of the balance, with a correlation coefficient for the 17 observations of $-.96$.

(3) *Income.* A much looser relationship was apparent between error and level of income, with some tendency for those at the upper-income levels to *overstate* the actual size of the account and those at lower-income levels to *understate* the size of the account. In addition, income was found to affect the relationship between error and size of account, the zero point for error tending to rise with income level. A multiple correlation coefficient of $.73$ resulted when error was regressed on income level and on size of the account, after combining the observations into groups of 10 by size of account within income level.

(4) *Age.* Account balances reported by older people were more likely to be in error than those reported by younger people. As Horn notes, this phenomenon might be explained for the most part by the larger accounts of these people and by their greater unwillingness to consult bankbooks.

(5) *Use of an introductory letter.* Recipients of an introductory letter were more likely to consult bankbooks than those not sent such a letter (65 percent versus 56 percent, respectively).

(6) *Interval between date of interview and date for which balance was requested.* In the interview, balances were requested for two dates — the date of the interview (some time during September-October, 1958) and January 1, 1958. Errors in the balance given for the earlier date were much more frequent than errors for the later date. Oddly enough, the biggest differential occurred among those who consulted bankbooks, 30 percent of whom erred about the balance on the interview date and 53 percent of whom gave the incorrect balance on January 1, 1958. (However, apparently most of these errors were relatively small — between 2 and 100 guilders.)

Estimates of Saving

Since the questionnaire sought balance data for two different dates, estimates of respondents' saving between these dates could be computed. Comparison of these computed estimates with the actual figures indicated

substantial underestimation of actual saving, the figures being as follows (in guilders):⁹⁰

<i>Average balance on</i>	<i>Actual</i>	<i>Computed</i>	<i>Difference</i>
Interview date.....	731	630	101
January 1, 1958.....	645	617	28
Saving.....	86	13	73

Since the degree of understatement was much larger for the balance on the interview date than for the balance as of January 1, 1958, the computed average saving was very much lower than the actual figure.

Comparison of actual saving with computed saving for individual respondents indicated that the absolute amount of saving was understated at both ends of the distribution: the greater the amount of saving, or of dissaving, the larger the extent of understatement. This relationship was measured by means of a regression line of average error on average actual saving, combining the observations into groups of 25 by amount of saving. The relationship was negative, with a correlation coefficient of .94.

Comparative Evaluation

Overall, these time-deposit validation studies point to the existence of substantial errors in such data when obtained by consumer surveys. Thus, we find that:

- (1) Sample averages tend to understate substantially the true population figures.
- (2) As a general rule, the principal cause of these underestimates is the nonreporting of respondents. A second major factor appears to be underestimates of the holdings of the nonrespondents.
- (3) Response errors are generally small. As was true in the case of debt, there is some tendency toward overstatement, but this tendency is slight.
- (4) A finding that runs counter to the corresponding one for debt is the overstatement of the frequency of small savings accounts. There is also some tendency for the frequency of large savings accounts to be understated, particularly if nonreporters are included in the size distributions. This tendency toward the so-called average-man effect — by which it is meant that people tend to bias their replies in accordance with their visualization of the actions of the average man — has been found to exist also in a number of other instances.⁹¹

⁹⁰ Horn, *op. cit.*, p. 141.

⁹¹ See E. S. Maynes, "The Anatomy of Response Errors: Sample Survey Data on Saving," *Journal of Marketing Research*, Vol. 2, No. 4 (November, 1965), pp. 378-87.

(5) The tenuous generalization in the case of debt that nonrespondents hold more than respondents do receives strong support in the case of savings accounts. In some of these studies, the average balances of the nonrespondents were several times as large as those of the respondents.

(6) The bias in the estimates of the means leads to substantial errors in the estimates of confidence intervals. The confidence intervals obtained from the sample observations can understate tremendously the true size of the confidence interval, stemming from the dominance of the square of the bias in the expression for the true variance of the mean. A strong negative correlation may also exist between the true value and the response error, due to frequent nonreporting, and this may reduce the apparent standard error of the mean to a spuriously low level.

As was true in the case of debt, accuracy of report was affected substantially by a number of different factors. These were found to include characteristics of the savings unit, objective measures of respondent cooperativeness, interviewer ratings of respondent cooperativeness, and the questionnaire approach. Again, strong evidence is provided for the greater reliability of a holdings form compared with that of a change form and for using an approach whereby the respondent participates in the evaluation of the interview. As before, both alternatives yield lower response, but nevertheless there is no question regarding the superiority of the resulting data.

The relationships between accuracy of reporting and respondent characteristics were essentially the same as in the case of debt, with one exception: in most of these studies, reporting of holdings was not associated especially closely with the level of income, and where such an association was present, higher accuracy was generally obtained from the upper-income levels. Particularly interesting is the strong association in some studies between the reporting of holdings and interviewer ratings of respondent cooperativeness. These results suggest that information supplied by properly trained interviewers may be of considerable aid in detecting response errors.

V. VALIDATION STUDIES: OTHER ASSETS

This chapter completes our coverage of one-time validation studies, presenting validation results relating to life insurance, demand deposits, and farm assets. Some of these are primary validation studies; others are secondary validations.

Life Insurance

Ownership of life insurance policies was checked on the three panel studies of the Consumer Savings Project — P1, P2, and P3. However, the results do not possess the reliability of many of the previously discussed analyses because in all instances this asset was a secondary validation source. Holdings could be checked only for companies with which insurance was reported, and not for all of these. Therefore, the full extent of nonreporting of life insurance holdings could not be ascertained.

In contrast to the other asset validations, life insurance in each panel operation was validated only on a one-time basis on a wave of interviews later than the first. This was done in part because of the difficulty of validating these data, which combined with the relatively static nature of insurance holdings, at least in the short run, did not make more than one check per year feasible. Partly too, experimentation with different questionnaire approaches meant that information on the holdings of all panel members could not be available before the second or third wave, depending on the particular experiment. Because of these limitations, the validity of the results is greatly limited, particularly with regard to nonreporting. Their principal value lies in highlighting the nature and scope of response errors.

Data Collection

In Study P1, it will be recalled (p. 77), one questionnaire variant was not to request dollar amounts until the second wave of half those interviewed with a structured form and half those interviewed with an unstructured form. Hence, information on the life insurance ownership of all the panel members in Study P1 was not available until Wave 2. In addition, by the time the necessary validation arrangements had been made, the

Wave 3 interviews had already been conducted, so this Wave 3 information served as the basis for the validation. Also in Studies P2 and P3, full details about life insurance ownership of the sample members was not obtained until Wave 3, again because of the experimental aspects. Hence, for these studies as well, the Wave 3 information served as the basis for validation.

In Study P1, 672 life insurance policies reported by 173 savings units were submitted for validation on face value and on premium payment; term insurance was excluded—it is not a form of saving. The comparable figures for Studies P2 and P3 are 634 policies of 200 savings units and 757 policies of 278 savings units, respectively. Since the results obtained on both bases were essentially the same, results are presented only in terms of face value; the principal difference was a tendency for fewer “don’t knows” or “no answers” to be received on face value than on premium payment.

The type of validation information obtained is shown by the following tabulation:

	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
Policy verified and face value supplied . . .	376	259	267
Policy verified but no face value given . . .	7	2	5
Policy could not be located	269	322	51
No report received	<u>20</u>	<u>51</u>	<u>434</u>
Total	672	634	757

In other words, fully useable validation information was returned on little more than half (55 percent) of the policies submitted in Study P1, about 40 percent in Study P2, and about 35 percent in Study P3. By far the main reason for this low rate of return was inability to locate policies or names of policyholders, despite the fact that information was supplied on the name of the respondent, address, age, type of policy, and length of time held. Among the contributing factors were the following:

(1) Given names of wives and children were not generally obtained when policies were in their name. Particularly awkward was the situation when (usually unknown to the interviewer) a policy was in a wife’s maiden name.

(2) Policies whose holders had common names were difficult to validate, since current addresses did not often correspond with addresses at the time the policy was taken out.

(3) Most companies did not have a master file of all policyholders, but had instead a number of files for different types of insurance. For some types of insurance, such as debt insurance, the only file of policyholders might be in local agency offices. For industrial insurance, records were kept by policy number, so that names could not readily be located.

(4) Considerable confusion was apparent among respondents on name of company and type of policy, the former clearly compounded by the widespread similarity of company names in the industry. Except where records were used, wrong names of companies seem to have been a common occurrence. In this connection, it is pertinent to note that none of the policies reported by 41 of 173 savings units in Study P1 could be located by the company named by the respondent.

(5) Some companies refused to supply the requested information, although such instances were relatively few.

Study of respondent reports on the policies that could not be validated suggests that, on the average, they were considerably smaller than policies that were validated: the average face value of these policies, compared with the average face value of the validated policies, was as follows:

	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
Not validated.....	\$3,123	\$1,866	\$2,427
Validated	8,127	3,636	2,915

Many of these nonvalidated policies appear to have been industrial or group policies, and they were also much more likely to have been taken out on a member of the savings unit other than the main wage earner. To judge by the fact that records were used about as often with these policies as with others, plus the fact that cooperativeness was about the same, the only basis for expecting a different pattern of response error for these policies than for those validated would be some relationship between accuracy of response and size of policy. This question will be considered shortly.

The smaller size of the nonvalidated policies also may explain the greater frequency of such policies in the later studies. The income status of the members of Studies P2 and P3 was generally considerably lower than that of the members of Study P1, and this was reflected in the much higher frequency of small policies. Thus, the average face value of policies that could not be located by the institution (and for which dollar figures were reported by the respondent) in Study P2 was \$1,866, in contrast to an average face value of \$3,123 for the corresponding policies in Study P1. Furthermore, 40 percent of the policies that could not be traced in Study P2 had a (reported) face value of less than \$1,000, compared with 12 percent of those that were traced. Examination of the interview data and discussions with company representatives tended to confirm the suspicion that a much higher proportion of policies in Study P2 than in Study P1 were either group policies or industrial policies.⁹²

⁹² In addition, inability to induce a principal company in Study P3 to supply validation information accounts for 126 of the 434 policies for which no report was received. No comparable situation was encountered on the two other studies.

Magnitude of Error

A general picture of the response errors on the policies that were validated is provided in Table 36, together with figures on the extent to which additional nonreported policies were picked up in the validation. This pickup was made possible because information was sought on all policies owned by the respondent with the particular company, not just on validated policies. For reasons noted earlier, this pickup serves as a lower limit estimate of the extent of nonreporting of life insurance policies.

As is evident from Table 36, this pickup varied from 57 policies not mentioned in the interviews on Study P1 to only 15 policies on Study P3. These represent about 15 percent of all validated policies on Study P1 (17 percent of the corresponding aggregate face value) but barely 1 percent on Study P3, a difference that may have been due to more accurate reporting on the latter study and/or to the larger proportion of policies not validated.

The policies not reported on Study P2 tended to be considerably smaller, on the average, than policies that were reported, with the result that the aggregate face value of nonreported policies is an even smaller proportion of the total, less than 8 percent. This more favorable experience than that obtained with Study P1 is undoubtedly due in part to the smaller average size of policies encountered in the later study since, as will be noted shortly, nonreporting rose appreciably with the size of the policy. In addition, there is the further possibility that the interviewing staff in Study P2 was better than that in Study P1, since selection procedures and training undoubtedly benefited from the earlier study, and this was probably even more true with Study P3. Indicative, perhaps, of this tendency, and of interest in its own right, is that, of the policies that were reported (and validated), figures were refused on 37 in Study P1, 7 in P2, and only 5 in P3.

Where face value was reported and comparisons could be made, exact correspondence occurred on almost two-thirds of the policies on P1, three-fourths of the policies on P2, and nearly nine-tenths of those on P3. On the other hand, one-fifth of the reports in Study P1 deviated more than 50 percent from the true figure. However, on the average the largest errors were not associated with the largest policies. Those with fully correct reported face value were much smaller than the others, but the largest policies clearly were more likely to be characterized by moderate errors in face value than by large errors, something to which we will return shortly.

Response errors generally averaged lower in Studies P2 and P3 than in Study P1 and also occurred less frequently. However, large errors were

relatively more substantial when they did occur in the later studies, particularly errors of more than 50 percent. On balance, such errors produced overestimates of 233 percent in Study P3 and of 90 percent in Study P2, as compared with overestimates of 50 percent in Study P1.

It is also interesting to note that the response errors tended to be primarily in the direction of overstatement, although progressively less so on the later studies. From an aggregative point of view, these overstatements help offset, to some extent, the face value of policies not reported, as is clear from the following comparison for Study P1 (estimating the "reported" figure for face value refusals on the basis of reported face value policies for which this figure was given) :

	<i>Reported</i>	<i>Actual</i>	<i>Percentage of discrepancy</i>
Face value of policies reported	\$1,423,348	\$1,254,342	-13.5
Policies reported, face value refused	226,033	300,562	24.8
Face value of policies not reported	582,121	100.0
Total	\$1,649,381	\$2,147,025	23.2

In other words, an aggregative estimate of face value based on these two sets of data would have resulted in an underestimate of 23 percent, mainly due to nonreporting of insurance. To be sure, the average face values of policies not reported and of face value refusals are substantially higher than the average face value of the policies that were reported. For this reason, we find that the estimated *average* face value of validated life insurance policies understates the true figure by only 6 percent.⁹³

In the case of Study P2, the interview and the institutional aggregates correspond very closely, as shown by the following data :

	<i>Reported</i>	<i>Actual</i>	<i>Percentage of discrepancy</i>
Face value of policies reported	\$ 985,134	\$916,210	-7.5
Policies reported, face value refused	27,363	8,480	-222.1
Face value of policies not reported	75,017	100.0
Total	\$1,012,497	\$999,707	-1.3

The 1 percent overstatement of the aggregate is seen to reflect the (offsetting) overstatement both of face value of policies reported and of face values imputed when this figure was refused, offset against the relatively small face value of policies not reported.

For Study P3, the correspondence is even closer, and for the same reasons as before :

⁹³ The difference between this figure and the 23 percent understatement of the aggregate is primarily due to the fact that the base number of policies for the average reported size of policy is so much less than for the average actual size (233 versus 326).

	<i>Reported</i>	<i>Actual</i>	<i>Percentage of discrepancy</i>
Face value of policies reported.....	\$778,395	\$744,977	-4.5
Policies reported, face value refused.....	14,575	9,500	-53.4
Face value of policies not reported.....	44,200	100.0
Total.....	<u>\$792,970</u>	<u>\$798,677</u>	0.7

Allocating the sources of error among the three possible causes (non-response bias is unknown) yields the following percentage distributions for the three studies:

	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
Error due to nonreporters.....	119.4	-591.8	774.5
Inaccurate estimates of those refusing balances.....	15.3	149.0	-88.9
Response error.....	<u>-34.7</u>	<u>542.8</u>	<u>-585.6</u>
Total.....	100.0	100.0	100.0

In view of the earlier material, it is not surprising to find nonreporting to be the principal cause of understatement, and face value refusals and response errors to be offsetting causes of overstatement. The large size of the percentage errors in the two later studies derives from the small relative error in the sample estimate of the aggregate. Entering into the denominator, this figure serves to inflate sharply the error ratios.

The effect of these various types of errors on the size distribution of policies is shown in Table 37. For Study P1, this table indicates that a much higher proportion of policies that were not reported — although not of policies for which the face value was refused — tended to be of large size. Thus, 5 percent of policies that were reported had a face value of more than \$22,500, compared with 20 percent of policies that were not reported and 3 percent of policies reported but for which the face value was refused.

In the case of face value of policies that were reported, errors in the distributions appear to be relatively slight. There is some tendency for policies that were overstated to be more frequently above \$22,500 in face value and for policies that were understated to be more frequently in the lower range, under \$2,500 in face value. However, neither difference is particularly substantial, with the result that the reported and actual distributions by face value of those policies that were reported coincide closely (columns 6 versus 9). Even more significant is the close correspondence between the distribution of reported face values (column 2) and that of all policies (column 9), the latter including policies not reported. Clearly, in this distributional sense, response (including non-reporting) errors are of little consequence.

For Studies P2 and P3, Table 37 shows that, contrary to the finding

TABLE 37. SIZE DISTRIBUTION OF VALIDATED POLICIES, BY ACCURACY OF FACE VALUE, STUDIES P1 TO P3

Face value (1)	Re- spondent report (2)	Higher by more than 10 percent (3)	Within 10 percent (4)	Lower by more than 10 percent (5)	Total policies; face value given (6)	Face value refused (7)	Policy not reported (8)	True (total) dis- tribution (9)
Study P1								
Under \$1,000.....	2%	...	1%	4%	1%	...	2%	1%
\$1,000 to \$2,499.....	43	31%	44	49	44	38%	37	42
\$2,500 to \$7,499.....	32	54	33	20	33	41	29	33
\$7,500 to \$12,499.....	13	4	16	18	15	19	10	15
\$12,500 to \$22,499.....	6	4	4	4	5	4	2	4
\$22,500 and over.....	5	8	2	4	3	3	20	5
Total.....	101%	101%	100%	99%	101%	101%	100%	100%
Base (Number of policies).....	240	26	163	45	234	37	56	327
Study P2								
Under \$1,000.....	8%	5%	8%	7%	8%	29%	19%	10%
\$1,000 to \$2,499.....	55	60	55	55	55	57	59	56
\$2,500 to \$7,499.....	25	20	25	29	25	14	16	24
\$7,500 to \$12,499.....	8	10	8	7	8	...	6	8
\$12,500 to \$22,499.....	2	5	1	3	2	1
\$22,500 and over.....	2	...	3	...	2	2
Total.....	100%	100%	100%	101%	100%	100%	100%	101%
Base (Number of policies).....	252	20	201	31	252	7	32	291
Study P3								
Under \$1,000.....	3.0%8%7%	...	6.7%	1.0%
\$1,000 to \$2,499.....	58.8	53.3%	67.2	63.2%	61.8	60.0%	60.0	61.7
\$2,500 to \$7,499.....	28.8	40.0	27.9	36.8	29.2	40.0	26.7	29.3
\$7,500 to \$12,499.....	8.2	6.7	7.7	...	7.1	6.6
\$12,500 and over.....	1.1	...	1.3	...	1.1	...	6.7	1.4
Total.....	99.9%	100.0%	99.9%	100.0%	99.9%	100.0%	100.0%	100.0%
Base (Number of policies).....	267	15	233	19	267	5	15	287

for Study P1, the policies that were not reported, or for which the face value was refused, tended to be very much smaller than those that were reported. Thus, roughly one-fourth of the P2 policies in the former group had a face value of less than \$1,000, which was true for only 10 percent of the policies that were reported.

Despite these differences, the size distribution of policies based on the respondent reports on these two studies (column 2) is seen to correspond very closely with the true size distribution of all policies (column 9), including those that were not reported. The only difference of any magnitude — and this tends to be small — is a tendency to understate the proportion of policies having a face value of less than \$1,000.

The moderate nature of these errors in the frequency distribution comparisons is unfortunately not fully reflected in the estimates of the true standard error of the mean. Carried out in the manner discussed in previous chapters, the computations produce the following results:

	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
<i>k</i>	1.20	.55	.70
<i>r</i>	-.02	.07	-.08
Apparent standard error of the mean ($\sigma_{\bar{y}}$)	\$601	\$503	\$180
True standard error of the mean (\sqrt{MSE})	\$942	\$573	\$219
Response-error-free standard error of the mean ($\sigma_{\bar{x}}$).....	\$487	\$476	\$172

In Study P1, the high value of the bias relative to the apparent standard error serves to bring about an underestimate of the true standard error of more than one-third. Furthermore, if the response errors could have been eliminated, the true standard error would have been reduced to almost half its present size. In Study P2, the relatively small errors in the aggregates nevertheless are sufficient to produce a 14 percent discrepancy between apparent reliability and true reliability. The discrepancy is due only in part to the bias, since the low (positive) correlation between the response errors and the true values accounts for roughly one-third of the discrepancy.

In Study P3, the apparent standard error of the average face value, \$180, represents an understatement of the true standard error of these data of 18 percent. Furthermore, although the correlation between the response errors and the true values is very low, elimination of the response errors would have reduced the true standard error by 21 percent, as a result of the moderately high variance of these errors.

The values of *k* in these studies suggest that the bias in the usual manner of constructing the 95 percent confidence interval is of moderate proportions. This bias varies from a relatively low value for Study P2, for which the usual 95 percent confidence interval has a probability of

containing the true parameter of .91, to Study P1, where this probability falls to .76. Thus, bias is present, but to nowhere near the extent observed for other assets and debts.

Determinants of Response Error

Two series of tests were carried out to measure the extent of association of different respondent and interview characteristics with response error. One series of tests sought to determine which factors were associated significantly with a tendency not to report particular policies. A dichotomy was employed for this purpose, namely, "policies reported" and "policies not reported."⁹⁴

The second series of tests sought to measure the characteristics associated with different degrees of response error. Only policies for which the face value was reported by both the respondent and the institution were included in this test. The test itself was carried out on a savings-unit basis, combining all such policies for each respondent and basing the response error figure on the percentage difference between the actual cumulated face value of these policies and the reported face value.

The results obtained with the first series of tests designed to measure factors influencing nonreporting, are shown in Table 38.⁹⁵ The characteristics tested are classified under four general headings, pertaining to characteristics of the savings unit, cooperativeness in the interview (subjective as well as objective ratings), the circumstances of the interview, and the experimental variables.

As is evident from this table, various characteristics under each of these four headings are found to be related significantly to the reporting of policies. Of the savings unit characteristics tested in Study P1, significant factors are seen to be education, age, and size of policy. Perhaps even more interesting is the fact that the nature of these relationships is not always what might be expected. Thus, the least-accurate groups of respondents were those with a high school education and those who were either relatively young (under 35) or much older (over 65). Furthermore, policies not reported tended to fall most frequently either in the largest size category or in the smallest size category. These results (as well as further tabulations) appear to suggest that two diverse groups are more guilty than others of nonreporting — those who are relatively young and have fairly small policies and those who are much older, have a high school education, and own relatively large policies.

⁹⁴ If a respondent reported some policies but not others, he was classified as a nonreporter.

⁹⁵ No attempt was made to explore factors associated with nonreporting in Study P3 because of the very small number of policies (15) in this category.

TABLE 38. ASSOCIATION BETWEEN SELECTED CHARACTERISTICS AND REPORTING OF INSURANCE, STUDIES P1 AND P2

Characteristic	Study P1			Study P2		
	Chi-square	Degrees of freedom	Level of significance	Chi-square	Degrees of freedom	Level of significance
Savings unit characteristics						
Education.....	8.57	3	.03	2.55	3	.47
Age.....	19.14	4	Less than .01	6.41	3	.09
Size of savings unit....	2.85	5	.73			
Occupation.....	3.10	3	.44	4.52	4	.35
Income.....	2.52	4	.64	4.77	4	.31
Home ownership.....	1.44 ^a	..	.15	1.34 ^a	..	.18
Value of home (if owned).....	3.32	3	.35
Nationality.....	1.19	3	.76
Marital status.....	1.11 ^a	..	.27
Size of policy.....	8.91	3	.03	1.78 ^a	..	.08
Size of savings unit....	2.04	2	.37
Cooperativeness						
Accuracy of information	.09 ^a	..	.94	.75	2	.70
Completeness of information.....	2.29 ^a	..	.02	1.22 ^a	..	.22
Use of records.....	1.24 ^a	..	.21	.61 ^a	..	.54
Remained in panel after Wave 1.....	1.03 ^a	..	.30
Interest in study.....06 ^a	..	.95
Items refused.....23 ^a	..	.82
Circumstances of interview						
Location of interview (home versus other)...	3.02 ^a	..	Less than .01	1.13 ^a	..	.26
Length of interview....	17.28	3	Less than .01	.47	2	.79
Number of contacts before interview.....	23.88	4	Less than .01	3.20	2	.20
Status of respondent in savings unit.....	1.65	2	.45
Experimental variables						
Type of advance letter..	1.36 ^a	..	.17	.09 ^a	..	.93
Structured versus unstructured approach..	5.48 ^a	..	Less than .01			
Dollar amounts versus no dollars.....	.42 ^a	..	.67
Assets first versus debts first.....	.48 ^a	.63
Holdings first versus change first.....	2.60 ^a	..	Less than .01
Forms in interview versus no forms.....	1.23 ^a	..	.22
All assets versus some assets.....	1.12	..	.27

^a The *t* test rather than the chi-square test was used in this case.

Except for size of policy, it is interesting to note that none of the usual indicators of affluence appears to be related to nonreporting. This includes occupation, income, and value of home. Noteworthy is the fact that three of the four characteristics having to do with circumstance of the interview were related significantly to the reporting of these policies. As one might expect, the shorter interviews tended to produce a higher frequency of nonreporting, and interviews made after several contacts exhibited a similar characteristic. Somewhat unexpectedly, however, nonreporting was much more extensive among interviews conducted in the respondent's home than among interviews conducted elsewhere, usually in the respondent's office.⁹⁶

Only one of the four experimental variables appears to differentiate significantly between reporting and nonreporting of policies. A much lower incidence of nonreporting was experienced by interviewers using the unstructured approach. This coincides with the reports of the interviewers, many of whom asserted that they, as well as the respondents, were more at ease in the unstructured situation.⁹⁷

Table 38 also indicates that many fewer characteristics were significantly related (at the .05 level) to nonreporting of life insurance in Study P2 than in Study P1. Indeed, none of the cooperativeness or savings unit characteristic variables in Study P2 are significant, not even size of policy. The one variable to be almost significant, age, is in this position because virtually no nonreporting was noted among those 65 years of age or more, quite the reverse of the situation in Study P1.

Among the experimental variables and those reflecting the circumstances of the interview, a similar situation is apparent with only one variable — type of form used — being statistically significant. Furthermore, the direction of the relationship contradicts all previous findings: nonreporting is much more frequent with the holdings form than with the change form. The secondary nature of the present validation renders this inference particularly suspect; if *complete* nonreporting of holdings were more frequent on the change form than on the holdings form, as is true of other assets and debts, the secondary validation process would ascribe spuriously high accuracy to the change form.

The extent of association between some of the respondent and interview characteristics and the magnitude of response error is summarized in

⁹⁶ Of course, when the interview is conducted in the office, one's wife is not present. . . .

⁹⁷ In evaluating these results, the secondary nature of this validation should be stressed. For this reason, these tests do not reflect the full extent of nonreporting, and it is not clear whether or not undetected nonreporting might vary in different ways from the ways in which the nonreporting that was detected was found to vary.

Table 39. Not all the characteristics appearing in Table 38 were used for this test, because inspection of that table, supplemented with cursory examination, indicated virtually no chance of obtaining significant results with such characteristics as nationality and status of respondent in the savings unit. Three categories of accuracy were used in this test: "overstated by 10 percent or more," "within 10 percent," and "understated by 10 percent or more."

Table 39 shows seven variables to have some value in Study P1 in discriminating between accurate and inaccurate reports at or near the .05 probability level. Three of these variables are savings unit characteristics, and the other four relate to cooperativeness or to circumstances of the interview; none of the experimental variables is significant.

The nature of these relationships indicates that accurate reports were more frequently given by persons in the upper-income levels, those with higher education, and those not self-employed. Reports from those in low-income levels or the self-employed were much more likely to understate the true face value than reports from other groups. In addition, accurate reports occurred much more frequently when records were consulted, when the interview took place at the respondent's office, when the interview was relatively long (two hours or more), and when the respondent was rated as being fairly complete in his report. Here again, the data highlight the somewhat paradoxical finding that the interviewers were much more likely to gauge accuracy correctly when asked to judge the *completeness* of the information than when asked to judge the *accuracy* of the information.

Even fewer variables are statistically significant on Studies P2 and P3, but those that are significant appear to be meaningful. Thus, the significance and near-significance of education, age, and income in Study P2 are due to the much greater tendency toward understatement of those with less education, in laboring occupations, and in a middle-income level.

Use of records is significant in Study P2 because nonusers were much more likely than users to understate face value. On the other hand, the near-significance of accuracy of information is the result of the much higher tendency of respondents rated "poor" in this respect to overstate face value.⁹⁸

In Study P3, only four variables were found to be related significantly to accuracy of reporting if the .05 probability level is used, and the number rises to five if the level of significance is relaxed to .10. Of these

⁹⁸ It is interesting to note that in this particular case accuracy is almost significant whereas completeness is not. However, it should be noted that the "completeness" rating applies to savings accounts and not to life insurance; such a rating was not obtained for the latter.

variables, three relate to savings unit characteristics: smaller savings units as well as those headed by the less-well-educated tended to understate face value more frequently, whereas upper-income savings units tended to overstate face value more frequently. In the case of education and income, these findings coincide with those of the earlier studies.

The one variable significantly related to accuracy of report among the

TABLE 39. ASSOCIATION BETWEEN SELECTED CHARACTERISTICS AND ACCURACY OF VALIDATED POLICIES, STUDIES P1 TO P3

Characteristic	Chi-square	Degrees of freedom	Level of significance
Study P1			
Savings unit characteristics			
Education.....	11.77	4	.02
Age.....	3.10	4	.54
Income.....	13.64	4	Less than .01
Size of savings unit.....	4.60	4	.34
Size of policy.....	.99	4	.91
Occupation.....	8.38	4	.08
Cooperativeness			
Accuracy of information.....	1.82	4	.76
Completeness of information.....	10.95	4	.03
Use of records.....	10.74	2	Less than .01
Circumstances of interview			
Location of interview (home versus other).....	4.07 ^a	..	Less than .01
Length of interview.....	17.20	4	Less than .01
Number of contacts.....	4.19	6	.65
Experimental variables			
Type of advance letter.....	2.36	2	.31
Structured versus unstructured.....	2.35	2	.31
Dollar amounts versus no dollars.....	3.10	2	.21
Study P2			
Savings unit characteristics			
Education.....	5.05	2	.08
Age.....	6.49	4	.17
Occupation.....	7.38	4	.12
Income.....	14.38	4	Less than .01
Size of savings unit.....	1.48	4	.83
Size of policy.....	0 ^a	2	1.00
Home ownership.....	.61	..	.54
Cooperativeness			
Accuracy of information.....	7.44	4	.12
Completeness of information (savings accounts).....	.42	2	.81
Interest in study.....	.35	2	.84
Use of records.....	16.51	2	Less than .01
Items refused.....	4.89 ^a	..	Less than .01
Circumstances of interview			
Location of interview.....	.45 ^a	..	.65
Length of interview.....	2.85	2	.25
Number of contacts.....	1.12	2	.58

TABLE 39. (concluded)

Characteristic	Chi-square	Degrees of freedom	Level of significance
Experimental variables			
Type of advance letter.....	.95	2	.63
Holdings first versus change first.....	2.18	2	.35
Forms in interview versus no forms.....	3.64	2	.17
All assets versus some assets.....	1.70	2	.44
Study P3			
Savings unit characteristics			
Education.....	6.51	2	.04
Age.....	2.24	2	.33
Income.....	12.54	2	Less than .01
Size of savings unit.....	4.89	2	.09
Size of policy.....	.43	2	.81
Farm ownership.....	.74	2	.69
Cooperativeness			
Accuracy of reporting ^b	1.37	2	.50
Completeness of savings account reporting.....	1.01 ^a	..	.31
Use of records.....	1.18	2	.56
Items refused.....	4.13 ^a	..	Less than .01
Circumstances of interview			
Location of interview.....	4.13 ^a	..	Less than .01
Length of interview.....	.32 ^a	..	.75
Number of contacts.....	1.22 ^a	..	.22
Experimental variables			
Type of advance letter.....	1.27	2	.53
Type of form used first.....	2	...

^a The *t* test rather than the chi-square test was used in this case.

^b On checking accounts and life insurance combined.

measures of cooperativeness — items refused — reflects a somewhat peculiar type of relationship. Although highly significant, the data suggest that people who refuse one or more items of information are *more* likely to be accurate than those who do not refuse any information. To be sure, such a relationship is conceivable, so far as the former group is composed of people who either give accurate information or prefer to give no information at all. However, this result is at variance with the direction of the relationship for the same variable noted in Study P2, although it must be remembered that the composition of the sample in Study P3 is very different from that in Study P2.

Among variables reflecting the circumstances of the interview, location is the only one to be statistically significant. As was found in some of the previous analyses, these results support the possibility that much more accurate data are obtained when the respondent is interviewed elsewhere than at home.

Demand Deposits⁹⁹

Validation of demand deposits presents two difficulties not encountered heretofore. First, because of their equivalence to cash and because they are held primarily for transactions and liquidity purposes, demand deposits tend to be highly volatile. For many, if not most, consumers the fluctuation in the monthly balance over the period of, say, a month may exceed substantially the average balance in the account. Second, at any particular moment the balance in a given account according to institutional records may or may not coincide with the balance in that account according to the consumer's records: the higher is the velocity in the account, the less likely are the two balances to coincide. This is the well-known "float problem," brought about by the interval that elapses between the time a check is written and the time that it is presented for redemption at the issuing institution.

Whether or not the bias due to check-float is serious, its possible presence serves to confuse interpretation of such validation studies, for segregation of errors due to these phenomena from nonsampling errors of the operation is hardly feasible. Nevertheless, as far as is known, no validation studies of demand deposits have been published, and it was therefore considered desirable to make some preliminary efforts in this direction.

Two of the operations of the Consumer Savings Project incorporated validation of demand deposits. Study P2 included secondary validation of these holdings, and such holdings served as a primary validation source for part of the data obtained in Study P3.

The Data

Study P2

Toward the end of the field work on this study, an attempt was made to validate checking account balances reported in two cooperating institutions. No attempt was made to pick up unreported accounts of panel members not mentioning ownership of such accounts.¹⁰⁰ Hence, this analysis relates solely to response error in balances that were reported.

One institution was able to carry out this check only partially, with the result that 125 reported balances were validated. Lack of institutional reports and absence of complete information reduces this number to 65, as shown by the following breakdown:

⁹⁹ This section is based largely on the author's article, "The Reliability of Consumer Savings of Financial Holdings: Demand Deposits," *Journal of the American Statistical Association*, Vol. 61, No. 313 (March, 1966), pp. 91-103.

¹⁰⁰ This was not feasible. The possibility that little additional information would have been obtained is suggested by the ability of the institutions to locate only one additional checking account for the panel members whose balances were validated.

	<i>Number</i>
Balance not reported by institution	48
Balance not reported by respondent	10
Report not comparable	2
Report comparable, balance obtained from both sources	65

Comparison of the accounts not validated by the institutions with those that were suggests that the former group had larger accounts, with balances averaging \$1,893 compared with \$992 for the validated accounts (based on the respondent reports). More significant, however, may be the fact that the nonvalidated accounts were more concentrated at both ends of the (size) distribution. About 20 percent of the nonvalidated accounts had balances of less than \$100 compared with 14 percent of those that were validated; at the other extreme, the relative frequencies of accounts with balances of \$2,000 or more were 17 percent and 11 percent, respectively. Hence, for some unknown reason, the validated balances in this instance are more likely to represent accounts with balances closer to the average than accounts with very large or very small balances.

Study P3

Demand deposit balances served as a primary validation source for part (roughly one-fifth) of the sample members in this farm panel operation; the other primary validation sources, it will be recalled, were debt and savings accounts. As was true with other primary validations in panel operations, it was feasible to validate these balances throughout the course of the operation (the results of the temporal comparisons will be presented in Chapter VI).

Response and Nonresponse Errors

The analysis in the following sections will focus on four aspects of response and nonresponse errors in these reports of demand deposits—their effect on estimates of the average size of the validated accounts, their effect on frequency distributions by size, their effect on estimates of the reliability of the mean, and the extent to which the observed errors in the data are attributable to check-float. Results will be presented for both surveys so far as this is feasible. Principal emphasis will be placed on Study P3, however, because it contains the most complete information on the accuracy of reports of this asset.

In the next two sections, discrepancies between the reported figures and those obtained from the institutions will be treated as nonsampling errors, with the institutional figures given as “actual” and the respondent figures as the “reported” data. In practice, these discrepancies may not reflect the true extent of nonsampling errors insofar as the data contain matching errors and are affected by check-float. Matching errors are

highly unlikely as a result of the panel nature of both operations and the feasibility of checking and rechecking to ensure matches; as was noted previously, two reports were discarded for this reason. The possibility of check-float, however, cannot be ruled out, and an attempt will be made to measure this phenomenon.

Effect on Estimated Size of Balance

Unfortunately, data on nonreporting are available only from Study P3. These data are presented in the following tabulation, subdivided by form used:

	<i>Holdings form</i>	<i>Change form</i>
Respondents.....	33	36
Reporters (including refusals of balance)....	30	33
Nonreporters.....	3	3
Noncontacts.....	0	0
Refusals to be interviewed.....	6	2
Total.....	39	38

In terms of the number of accounts, nonreporting is seen to amount to roughly 10 percent of the validated accounts of those interviewed. This percentage is substantially below figures which varied from 25 percent to 40 percent nonreporting for such assets as time deposits and personal debt recorded in previous studies. Moreover, nonreporting was just as low in this case when respondents were asked about changes as it was when they were asked about holdings; this is contrary to the situation observed for other assets, for which there was a tendency for nonreporting to be much higher because of the inclination of respondents not to report holdings with little or no change. It is interesting to note, however, that refusals are more frequent (though not significantly so at the .05 percent probability level) when the more stringent holdings form is used, a finding in line with past experience.

A broader picture of the errors in the balances reported by both samples is shown in Table 40. As was noted previously, the data for Study P3, based on primary validation techniques, cover all forms of these errors, whereas the data for the urban study, obtained by secondary validation, relate only to discrepancies in balances reported. On the farm study, the reported figure for the total sample is obtained by assigning the same average balance as reported by respondents who gave balances to those respondents who refused to cite any balances or to grant an interview; there were no noncontacts. Other allocation techniques could conceivably have yielded a higher balance, although the present sample is too small for this purpose. Use on other assets of such techniques as allocating average balances after classifying the sample into particular strata suggests that they would have had relatively little effect.

TABLE 40. AVERAGE SIZE OF ACCOUNT, BY OUTCOME OF INTERVIEW ATTEMPT

Category	Number of accounts	Average size of account		Percentage of deviation, reported from actual ^a
		Actual	Reported	
Study P3				
Total sample.....	24	\$1,780	\$1,350	24.2
Respondents.....	22	1,611	1,350	16.2
Nonreporters.....	2	266	0	100.0
Figure refused.....	1	2,030
Figure given.....	19	1,731	1,484	14.3
Nonrespondents ^b	2	3,642
Study P2				
Respondents.....	75	\$1,195	\$ 992	16.8
Figure refused.....	10	2,280
Figure given.....	65	1,029	992	3.6

^a Actual-reported

Actual

^b Both refusals; there were no noncontacts.

On the more comprehensive farm study, the average balance appears to have been understated by roughly 25 percent. This discrepancy represents partly the tendency for the average actual balances of the nonrespondents to exceed considerably the actual average balance of the respondents and partly the underestimation by respondents of their holdings. In the latter instance, it is interesting to note that in both samples the extent of underestimation of balances by respondents is roughly one-sixth. Moreover, both samples also indicate that balances for which the figure is refused tend to exceed the actual balances of people who report these figures, substantially so in the urban study (in the farm study there is only one such refusal). The discrepancy resulting from imputing a balance to those refusals on the basis of the balances that were reported tends to be increased because of the tendency of the reporters to underestimate their own balances, although this underestimation tends to be relatively small.

Numerical estimates of the relative importance of these different sources of error result in the following error allocation:

Discrepancies in estimated balances of nonrespondents.....	44.2%
Error due to nonreporters.....	5.1
Inaccurate estimates of those refusing balance.....	5.3
Discrepancies in the balances reported by the respondents.....	45.4
Total.....	100.0%

TABLE 41. DISTRIBUTION OF BALANCES IN VALIDATED ACCOUNTS, BY SIZE

Study P3								
Actual size	Reported size						Not reported	Total
	Under \$100	\$100 to \$299	\$300 to \$999	\$1,000 to \$2,499	\$2,500 and over			
Under \$100.....	..	1		1	2
\$100 to \$299.....	..	4	4
\$300 to \$999.....	..	1	3	2	..		1	7
\$1,000 to \$2,499...	2		2	4
\$2,500 and over...	1	5		1	7
Total.....	0	6	5	3	5		5	24

Study P2 (reporters only)								
Actual size	Reported size						Figure refused	Total
	Under \$100	\$100 to \$299	\$300 to \$999	\$1,000 to \$1,499	\$1,500 to \$2,499	\$2,500 and over		
Under \$100.....	7	2	1	10
\$100 to \$299.....	2	11	3	1	17
\$300 to \$999.....	1	..	16	3	20
\$1,000 to \$1,499...	2	5	3	..	1	11
\$1,500 to \$2,499...	2	5	1	2	10
\$2,500 and over...	1	3	3	7
Total.....	10	13	22	7	9	4	10	75

These results support previous statements that the primary sources of error in the estimate of the mean were the disproportionately large balances of the nonrespondents and the tendency of respondents to understate balances when reported. This understatement, although relatively small, is magnified in the present case by the proportionately large number of sample members in that category.

Effect on Frequency Distributions by Size

The effect of these various discrepancies on frequency distributions by size suggests some tendency for the frequency of both very large and very small accounts to be understated (Table 41), for much the same reasons mentioned earlier. The differences are particularly pronounced at the upper end when the distribution of reported balances is compared with the distribution of balances for the total sample, including refusals, nonreporters, and nonrespondents.

TABLE 42. RESPONSE AND NONRESPONSE ERRORS IN REPORTS
OF CHANGE IN DEMAND DEPOSIT BALANCES ON INITIAL
INTERVIEW, CHANGE FORM, STUDY P3

Category	Number of accounts	Average change		Percentage of discrepancy ^a
		Actual	Reported	
Total sample.....	19	\$481	\$ -149	131.0
Respondents.....	19	481	-149	131.0
Nonreporters.....	1	4,013	100.0
Reporters.....	18	285	-158	155.4
Nonrespondents.....	0

^a $\frac{\text{Actual-reported}}{\text{Actual}}$

The accuracy of the reports of change in demand deposit balances, for which data are available from the other subsample on the farm study, is substantially below that of the balances themselves. This is brought out in Table 42, which shows data for change comparable with the data on balances in Table 40. The data in Table 42 are less comprehensive than those in the former case because, unfortunately from an analytical point of view, there were no nonrespondents in the subsample. Nevertheless, the results are clearly not favorable. There was only one nonreporter among the 19 respondents, but the change in his balance was the largest of the group — an increase of over \$4,000. The others reported an average decrease in their balances of \$158 in the preceding three-month period, whereas their average balances actually increased by nearly \$300. As a result, not only was the amount of change substantially understated, but the direction of change was missed altogether.

Further investigation reveals that the poor correspondence of the average changes for the respondents is the result of a relatively large number of substantial errors (Table 43). Although the sample sizes are small, the discrepancies are striking. First, a clear tendency is apparent for respondents to report no change when a change in the balance did indeed occur. Second, the frequency of large decreases — \$500 or more — tends to be overstated. Third, the frequency of large increases in the balances tends to be understated substantially. Of eight deposit balances that had increased substantially over the preceding quarter, only three were reported as increases — one other was not reported, two were reported as “no change,” and two were reported as having decreased by \$500 or more.

That large discrepancies occurred with much greater frequency in reports of change than in reports of dollar balances is apparent from the following comparison of the frequency of discrepancies of different size:

<i>Discrepancy</i>	<i>On changes</i>	<i>On balances</i>
Within 25 percent.....	2	8
26-50 percent.....	2	4
51-100 percent.....	8	4
Over 100 percent.....	6	3
	<u>18</u>	<u>19</u>

In other words, only about one-tenth of the change reports came within 25 percent of the institutional figure, whereas the same was true of almost half of the balance reports. More than three-fourths of the change reports differed from the institutional figure by 50 percent or more, and one-third differed by 100 percent or more. In contrast, the corresponding percentages for reports of balances are 37 percent and 16 percent, respectively.

TABLE 43. DISTRIBUTION BY SIZE OF ACTUAL AND REPORTED CHANGES IN DEPOSIT BALANCES, CHANGE FORM, STUDY P3

Actual change	Reported change							Not re-ported	Total
	Down			No change	Up				
	\$500 or more	\$100 to \$499	Less than \$100		Less than \$100	\$100 to \$499	\$500 or more		
Down \$500 or more.....	1	1
Down \$100 to \$499.....	1	2	..	1	4
Down less than \$100.....	1	1	..	1	3
No change....	0
Up less than \$100.....	1	1
Up \$100 to \$499.....	..	1	..	1	2
Up \$500 or more.....	2	2	..	2	1	1	8
Total.....	5	4	0	6	0	2	1	1	19

Effect on Reliability

Using the same procedure as was used in Chapter IV to measure the effect of these nonsampling errors on estimates of the reliability of the mean, the following estimates are obtained for Study P3:

k78
r	-.67
Apparent standard error of the mean ($\sigma_{\bar{y}}$).....	\$317
True standard error of the mean (\sqrt{MSE}).....	\$403
Response-error-free standard error of the mean ($\sigma_{\bar{x}}$).....	\$413

Thus, without knowledge of the validation information, the standard error of the mean balance for this sample would be estimated at \$317, whereas the true standard error is appreciably more, namely, \$403. This true standard error turns out to be much the same as what would be obtained if no nonsampling errors were present, a correspondence brought about because of the tendency for the increase in the variance due to the errors to be offset by the negative correlation between the actual values and the errors. (This correlation remains strongly negative even if non-reporters are eliminated, since the balances of the nonreporters were very small; see Table 40.)

The value of k , .78, means that the 95 percent confidence interval applied to the sample observations actually had a probability of containing the true parameter, not of .93, but of .87.

Influence of Check-Float

Ideally, the effect of check-float would be investigated by obtaining information on the balance of the account in the institutional records and the balance in the checkbook of the respondent showing transactions not yet recorded by the institution. Any difference between these two balances would then represent check-float, whereas differences between the respondent report and his checkbook balance would represent response error. However, securing such data was out of the question and a more indirect, less-exact procedure was devised.

The basis for the procedure is the division of the sample into two groups. Group I consists of those respondents for whom any observed discrepancies are clearly attributable solely to check-float — respondents who consulted records and gave a balance to the nearest dollar or nearest cent and for whom no doubt existed regarding the accuracy of the reported figures. Group II consists of those respondents who showed reluctance to cooperate and who, in most instances, rounded off figures to the nearest \$100 or to the nearest \$1,000. For this group some response error is highly probable, and there is little doubt that such errors will be compounded with the float problem. Included among this group are people who may not have been fully cooperative but nevertheless appeared to be making some attempt to provide accurate data. Because of the difficulty of classifying these people by presence of response error, they are included in this second category.

The approach is to estimate the magnitude of the float on the basis of the Group I data. The resulting estimate is then compared with the discrepancy from Group II, and any overall discrepancy exceeding that for the former group is assumed to represent response error. To be sure, there is little evidence that the two groups are necessarily comparable with each other, a basic assumption underlying this approach; but as will be shown shortly, some allowance can be made for differences.

Algebraically, the method is one of segmentation of variances. If e_i represents the response error of an individual in the i th group, f_i the discrepancy due to float, and d_i the total discrepancy, we have:¹⁰¹

$$(5.1) \quad d_i = e_i + f_i.$$

If we assume absence of correlation between float and response error, it follows that:

$$(5.2) \quad \sigma_{d_i}^2 = \sigma_{e_i}^2 + \sigma_{f_i}^2.$$

If we further assume that:

$$\begin{aligned} e &= \text{zero for all members of Group I,} \\ e &\text{ differs from zero for some members of Group II, and} \\ \sigma_{f_1}^2 &= \sigma_{f_2}^2, \end{aligned}$$

then we can segregate the influence of float and that of response error on the basis that $\sigma_{f_1}^2 = \sigma_{d_1}^2$, and $\sigma_{e_2}^2 = \sigma_{d_2}^2 - \sigma_{f_1}^2$.

However, suppose that the two groups are not comparable because the checking account transactions of one group differ from those of the other. For present purposes, the most pertinent such difference is likely to be in the average balance or in the variability of the average balance, since either might affect the magnitude of check-float.¹⁰³ As a first approximation, one supported by the data, let us assume that the size of float is related to the average balance, e.g.:

$$(5.3) \quad f_i = \alpha + \beta x_i,$$

where x_i is the average balance.

It then follows that for two groups with different average balances,

$$(5.4) \quad \sigma_{f_2}^2 = \sigma_{f_1}^2 \frac{\sigma_{x_2}^2}{\sigma_{x_1}^2},$$

and:

$$\sigma_{e_2}^2 = \sigma_{d_2}^2 - \sigma_{f_2}^2.$$

Applying this method to the data leads to the following results:

¹⁰¹ Strictly speaking, a subscript should be added to denote each individual in the group, but this is unnecessary for present purposes.

¹⁰² The data suggest that large accounts (over \$500) tend to be more accurate than small accounts, in terms of percentage error, but this relationship may itself be due to float rather than to response error. If the assumption were not valid (2.2) would be replaced by:

$$(5.2a) \quad \sigma_{d_i}^2 = \sigma_{e_i}^2 + \sigma_{f_i}^2 + r_{e_i f_i} \sigma_{e_i} \sigma_{f_i}$$

In that case, the relative influence of float could only be estimated given the correlation between float and response error, or a range of estimates could be derived for alternative values of r .

¹⁰³ Pertinent also would be the velocity of transactions, but no data were collected on this subject.

	Study P2		Study P3	
	Group I	Group II	Group I	Group II
d_i	\$ 78	\$ -18	\$ 217	\$ 256
σ_d^2	36,805	1,175,136	92,789	1,161,312
x_i	873	1,162	1,060	1,970
σ_x^2	724,118	4,899,640	1,399,439	4,378,333
N	31	34	5	14

Since both the average balance and the variance of the balance are substantially greater for the possibly inaccurate group (II) than for the clearly accurate group in each case, Equations (5.4) are applied to yield

for Study P2: $\sigma_{f_2}^2 = 249,036$, $\sigma_{e_2}^2 = 926,100$, and
for Study P3: $\sigma_{f_2}^2 = 290,303$, $\sigma_{e_2}^2 = 871,009$.

For Study P2, this leads us to infer that 21 percent of the (variance in the) discrepancy in checking account balances for Group II is attributable to check-float and 79 percent to response error. Combining the two groups, we would conclude that 24 percent of the total variance is due to check-float and 76 percent to response error.¹⁰⁴ Thus, it appears in this case that response error was of much greater importance than check-float in accounting for the observed discrepancies in checking balances.¹⁰⁵

For the farm study, the data suggest that 25 percent of the (variance in the) discrepancy in checking account balances is accounted for by check-float and 75 percent by response error. Combining the two sets of data yields an estimate of 73 percent as the proportion of total variance in the observed discrepancies attributable to response error and 27 percent to check-float.¹⁰⁶ These estimates come remarkably close to the 76-24 division in the urban study.¹⁰⁷ Hence, they serve to reinforce the idea that response errors may be a much more important explanation of discrepancies in demand deposit balances than check-float.

Farm Assets

Nature of Validation

With the aid of information obtained from cooperating lending

¹⁰⁴ Allocation of response error to the total variance is based on the product of the proportion of Group II variance due to response error (79 percent) and the contribution of the Group II observations to the total sum of squares, the percentage being obtained by dividing this product by the total sum of squares.

¹⁰⁵ To judge from the Group I data, check-float appears to have been of minor importance from an aggregative point of view, being less than 10 percent of the average balance. Even individual discrepancies were not too large, the average *absolute* discrepancy representing 13 percent of the average balance.

¹⁰⁶ The combination method used is the same as that described in Footnote 104.

¹⁰⁷ In this case, however, \bar{d}/\bar{x} for Group I is much larger than before, namely, about 20 percent. Since all discrepancies in this group were positive, this percentage also represents the average magnitude of individual discrepancies.

agencies, a variety of farm assets of the panel members of Study P3 were validated. These validations were carried out at the end of the operation, focusing on information obtained on Wave 5 interviews.

From a conceptual point of view, this procedure was tantamount to primary validation, even though the checks were made on an *ex post* basis. The reason for this was that with one exception (stock in a lending agency) all assets were physical and the coverage related to total coverage of each type of asset. For the latter reason, nonreporting was virtually nonexistent, particularly since the assets served in many instances as collateral for the loans. Indeed, one would expect to encounter, if anything, overreporting rather than underreporting in such a situation. At the same time, these data do not possess the other virtues of primary validation. Thus, they cannot be construed as representing a random selection of farmers owning each of the assets being validated, since the validation pertained only to patrons of a particular type of lending institution. In addition, the institutional data themselves were based to a large extent on information supplied by the individual farmer, although this information was supplemented by checks by field representatives of the agency.

From a practical point of view, certain difficulties arose in this validation not encountered in any previous analyses. The principal such difficulty derived from the fact that the data at the agency were not continuous; they related to selected times at which the assets of the farmer happened to be brought up to date, and only rarely did these dates coincide with the dates of the interviews. Hence, some interpolative procedure was required to estimate the farmer's holdings of an asset on a particular date. For the more volatile assets, such as crops and certain types of livestock, such interpolation became highly tenuous if the date of the agency's records and the date of the interview differed substantially; and in some instances the most prudent course appeared to be not to make the attempt.

A second practical difficulty was that the files of the agency were at times incomplete. Primary attention appeared to be given to those assets serving as collateral for loans or to those assets constituting the major part of the farmer's equity. Third, the data serving as the basis for these validations were considerably fewer than the number of panel members borrowing from these institutions, partly because not all of these assets were owned by every farmer and partly because the records of one of the lending agencies (specializing in long-term loans) were not suitable for this type of analysis.

Fourth, the determination of the true figure was in many cases a matter of judgment. With dollar amounts this is self-evident, for value of livestock or of machinery (and, to a lesser extent, of crops) can not be determined absolutely until the asset is sold. Even when numbers are

TABLE 44. CORRESPONDENCE OF VALIDATED FARM ASSETS, STUDY P3

Asset	Measure	Number of observations	Average percentage of discrepancy ^a	
			Arithmetic	Absolute
Acreage (owners).....	Number of acres	34	-8	20
Acreage (renters).....	Number of acres	70	-2	14
Automobiles and trucks...	Value	78	-20	42
Other machinery.....	Value	76	-9	33
Hogs.....	Value	46	-2	65
Cows and steers.....	Value	57	-10	44
Other livestock.....	Value	13	1	44
Crops.....	Value	76	-8	134
Stock in lending agency...	Value	76	12	71

^a $\frac{\text{Institution-respondent}}{\text{Institution}}$

involved — as in the case of livestock or crops — differences of opinion may arise, since exact counts are not often made. For this reason the analysis is couched primarily in terms of “correspondence” of information rather than “accuracy,” with the base as the institutional figure, which may be expected to be the least biased.

Magnitude of Error

The nine categories of farm assets validated and the average errors obtained in each category are shown in Table 44. For comparative purposes, absolute averages of the individual percentage errors are shown as well as arithmetic averages. These absolute averages indicate that substantial discrepancies occurred between respondent reports and the institutional data. However, the average discrepancies vary sharply by asset, ranging from 14 percent for the farm acreage of renters to 134 percent for the value of farm crops. Even if the average error on the value of crops is attributed largely to differences in coverage,¹⁰⁸ it is clear that appreciable individual differences exist between the two sets of data (as shown in further detail in Table 45, p. 154).

It is also clear that the two estimates of physical quantity exhibit a discrepancy considerably less than that in the estimates of value. This dif-

¹⁰⁸ This figure may indeed reflect lack of comparability between the interview data and the institutional data more than anything else. As reported in the institutional records, value of farm crops was meant to exclude stored crops, whereas in the interviews, information on stored crops was sought as well as information on other crops. Although an attempt was made to eliminate stored crops from the interview data, it was not always clear whether this attempt was fully successful. Moreover, in some instances there was a strong basis for believing that stored crops were included in the institutional data (themselves obtained from the farmers), but this could not be checked.

ference could be attributed to the particular assets covered, but it more likely reflects the fact that a value estimate in the present instance necessitates both an estimate of physical quantity and an estimate of value per unit; and these two different estimates do not seem to cancel each other.

Also apparent from Table 44 is the fact that these large absolute errors do tend largely to cancel each other in the aggregate, so that the arithmetic averages are not much different from zero in most instances. Indeed, all but one of these averages are within 12 percent of the institutional averages. However, it is somewhat surprising to note that seven of these nine averages tend to be higher for the interview data than for the institutional data. Since many of these assets served as collateral for the loans from the institution, the debtor might have been expected to attempt to bias upward his valuation of his holdings. Clearly, however, no such tendency is apparent. If anything, the data suggest that the farmers were more comprehensive in reporting holdings in the interviews (in part undoubtedly due to the more detailed nature of the interview). This tendency receives further support from the following tabulation, which shows the number of times farmers failed to report particular holdings to the institutions and the number of similar situations in the interviews:

	<i>Not reported to institution</i>	<i>Not reported in interview</i>
Acreage (owners).....	4	0
Acreage (renters).....	1	0
Autos and trucks.....	1	0
Other machinery.....	3	0
Hogs.....	4	3
Cows and steers.....	2	3
Other livestock.....	33	1
Crops.....	3	8
Stock in lending agency.....	1	21

With the exception of crops and stock in the lending agency, omissions appear to be considerably more frequent in the data reported to the institution. The more frequent omission of crops in the interview than in reports to the institution may well reflect the lack of comparability which was mentioned in the previous footnote. The frequent omission of stock in the lending agency could be due to memory. In order to borrow from these agencies, farmers were required to purchase stock in them at the time of receipt of their initial loan. Such stock constituted a nominal sum, did not have to be sold at any particular time, and therefore could be easily overlooked.

Distributions of the validated farm assets by degree of correspondence between the two sources are shown in Table 45, together with data on the true average value (or acreage) in each category. These distributions

highlight the considerable dispersion of the discrepancies for all assets but farm acreage. Discrepancies of 50 percent or more exist in roughly 40 percent of the valuations of hogs, of crops, and of stock in the lending agency. At the same time, these distributions also suggest that, with the exception of stock in the lending agency, the frequency of errors is distributed more or less symmetrically. Only for stock in the lending agency are there considerably more large errors on one side (understatements) than on the other.

Nevertheless, for several assets the data suggest that the holdings of those erring in the direction of understatement tend to be considerably higher than the corresponding holdings for the overstaters. For example, the average value of the hogs of farmers overstating this holding by 50 percent or more was \$810, whereas the average value of hogs of farmers understating this holding by 50 percent or more was \$3,773. Much the same phenomenon is apparent for acreage of both owners and renters and for value of automobiles and trucks, for other machinery, for cows and steers, and for crops. In other words, as is true with financial assets, there is a tendency for the more affluent to understate and the less affluent to overstate.

The effects of these errors as well as of nonreporting on the size distributions of these holdings is shown in Table 46. In view of the fact that nonreporting was more frequent in the institutional records for most assets, there is even less reason than before for assuming that the size distribution as obtained from the institution provides the more accurate estimates. Nevertheless, it is of interest to observe the extent to which the two size distributions correspond. As is evident from this table, such correspondence is close for all assets for which nonreporting in one source or the other was relatively small. This is true for five of the nine assets covered. In addition, for a sixth asset, hogs, nonreporting occurred with more or less equal frequency in both sources and here, too, close correspondence is apparent.

Substantial differences exist among the size distributions for the other three assets: for other livestock because of extensive nonreporting to the institutions; for stock in the lending agency and for crops because of relatively higher nonreporting in the interviews.

It is interesting to note, however, that if the "zero" category were eliminated from these distributions, no appreciable differences would be apparent for any of the nine assets.

Reliability of Error Estimates

Computations of the extent to which the apparent standard error of the mean coincides with the true standard error for each of the nine farm

assets yields mixed results. It is evident from Table 47 that the estimates virtually coincide for some assets (for acreage and for two of the livestock categories) but understate substantially the true standard error for such assets as automobiles and trucks, hogs, crops, and stock in the lending agency. For the latter group of assets, the true confidence interval differs

TABLE 45. DISTRIBUTION OF VALIDATED FARM ASSETS,
BY CORRESPONDENCE OF RESPONSES, STUDY P3

Degree of correspondence	Acreage (owners)		Acreage (renters)	
	Percent- age of ob- servations	Actual average acres	Percent- age of ob- servations	Actual average acres
Over by 50 percent or more.....	3	80	6	321
Over by 25 to 49 percent.....	1	225
Over by 10 to 24 percent.....	6	325
Within 10 percent.....	85	153	71	287
Under by 10 to 24 percent.....	4	375
Under by 25 to 49 percent.....	3	146	9	383
Under by 50 percent or more.....	9	233	3	1,062
Total.....	100	158	100	326
Degree of correspondence	Automobiles and trucks		Other machinery	
	Percent- age of ob- servations	Actual average value	Percent- age of ob- servations	Actual average value
Over by 50 percent or more.....	21	\$1,533	12	\$ 6,966
Over by 25 to 49 percent.....	6	1,956	13	7,639
Over by 10 to 24 percent.....	13	2,185	13	9,615
Within 10 percent.....	32	2,613	25	10,101
Under by 10 to 24 percent.....	9	2,179	20	11,846
Under by 25 to 49 percent.....	12	2,173	12	9,502
Under by 50 percent or more.....	8	3,016	5	10,511
Total.....	101	\$2,403	100	\$ 9,624
Degree of correspondence	Hogs		Cows and steers	
	Percent- age of ob- servations	Actual average value	Percent- age of ob- servations	Actual average value
Over by 50 percent or more.....	20	\$ 810	14	\$3,642
Over by 24 to 49 percent.....	13	1,414	14	7,466
Over by 10 to 24 percent.....	37	1,333	14	5,261
Within 10 percent.....	17	3,299	25	3,968
Under by 10 to 24 percent.....	11	2,811	12	6,177
Under by 25 to 49 percent.....	13	1,249	7	6,925
Under by 50 percent or more.....	20	3,773	14	6,774
Total.....	101	\$2,210	100	\$5,467

TABLE 45. (concluded)

Degree of correspondence	Other livestock		Value of stock in lending agency	
	Percent- age of ob- servations	Actual average value	Percent- age of ob- servations	Actual average value
Over by 50 percent or more.....	15	\$ 210	9	\$ 471
Over by 25 to 49 percent.....	23	152	1	830
Over by 10 to 24 percent.....	5	490
Within 10 percent.....	8	36	22	544
Under by 10 to 24 percent.....	8	312	12	1,131
Under by 25 to 49 percent.....	39	367	15	835
Under by 50 percent or more.....	8	50	36	685
Total.....	101	\$ 239	100	\$ 676

Degree of correspondence	Crops	
	Percent- age of ob- servations	Actual average value
Over by 50 percent or more.....	18	\$1,667
Over by 24 to 49 percent.....	7	2,395
Over by 10 to 24 percent.....	8	3,098
Within 10 percent.....	22	3,717
Under by 10 to 24 percent.....	8	3,219
Under by 25 to 49 percent.....	15	3,751
Under by 50 percent or more.....	22	4,904
Total.....	100	\$3,435

substantially from the .95 confidence interval that would be expected by using the range of the mean plus and minus 1.96 apparent standard errors.

The distortion would have been considerably larger were it not for some substantial negative correlations between the true values and the errors. These correlations serve to offset partially the increased sampling variance caused by discrepancies between the institutional and the individual reports.¹⁰⁹

Column 6 of Table 47 shows that in many instances appreciable reductions in the standard error of the mean would have taken place had there been no discrepancy between the two sets of data. This is true of all of the assets but acreage and other livestock. It is interesting to note, however, that in the latter case, the standard error of the mean in the absence of error would have been considerably larger than when response

¹⁰⁹ It should be stressed that these correlations appear to be due primarily to two or three extreme observations in each case and do not seem to typify all the observations. If these extreme observations were removed, the correlations would become virtually zero.

error was present, because of the high negative correlation between the true values and the response errors.

Comparative Evaluation

The nature of the validation results obtained in this chapter vary substantially with the type of asset, influenced no doubt by the type of validation. Thus, the low frequencies of nonreporting on life insurance may be due in part to the secondary nature of the validations. The same may be said of the low nonreporting of demand deposit accounts in Study

TABLE 46. PERCENTAGE DISTRIBUTION OF VALIDATED FARM ASSETS, BY SIZE AND BY SOURCE, STUDY P3

Item	Inter- view	Institu- tion	Item	Inter- view	Institu- tion
Farm acreage (owners)			Farm acreage (renters)		
0.....	..	11	0.....	..	1
Under 50.....	18	11	Under 150.....	9	7
50 to 74.....	13	13	150 to 249.....	25	28
75 to 99.....	21	24	250 to 349.....	34	35
100 to 199.....	26	18	350 to 449.....	13	10
200 to 299.....	8	11	450 to 549.....	13	10
300 to 399.....	5	8	550 and over.....	7	9
400 and over.....	8	5			
Total.....	99	101	Total.....	101	100
Value of automobiles and trucks			Value of other machinery		
\$0.....	..	1	\$0.....	..	4
Under \$1,000.....	13	15	Under \$1,000.....	1	3
\$1,000 to \$1,499.....	19	18	\$1,000 to \$2,499.....	5	3
\$1,500 to \$1,999.....	13	18	\$2,500 to \$7,499.....	35	30
\$2,000 to \$2,499.....	10	15	\$7,500 to \$12,499....	35	38
\$2,500 to \$2,999.....	14	9	\$12,500 to \$22,499...	19	19
\$3,000 to \$3,999.....	13	11	\$22,500 and over....	4	4
\$4,000 to \$4,999.....	9	4			
\$5,000 and over.....	10	9			
Total.....	101	100	Total.....	99	101
Value of hogs			Value of cattle		
\$0.....	6	8	\$0.....	3	3
Under \$500.....	22	14	Under \$1,000.....	10	7
\$500 to \$999.....	14	20	\$1,000 to \$2,499....	14	17
\$1,000 to \$1,499.....	20	18	\$2,500 to \$4,999....	31	29
\$1,500 to \$2,499.....	16	18	\$5,000 to \$7,499....	22	22
\$2,500 to \$3,499.....	20	14	\$7,500 to \$12,499....	14	15
\$3,500 to \$4,499.....	2	8	\$12,500 and over....	7	7
Total.....	100	100	Total.....	101	100

TABLE 46. (concluded)

Item	Inter- view	Institu- tion	Item	Inter- view	Institu- tion
Value of other livestock			Value of crops		
\$0.....	2	72	\$0.....	10	4
Under \$50.....	28	2	Under \$1,000.....	15	19
\$50 to \$99.....	13	7	\$1,000 to \$1,499.....	15	11
\$100 to \$299.....	37	11	\$1,500 to \$4,999.....	38	38
\$300 to \$999.....	15	7	\$5,000 to \$9,999.....	17	25
\$1,000 and over.....	4	2	\$10,000 and over.....	5	3
Total.....	99	101	Total.....	100	100
Value of stock in lending agency					
\$0.....	27	1			
Under \$150.....	9	9			
\$150 to \$249.....	8	9			
\$250 to \$349.....	7	13			
\$350 to \$599.....	17	26			
\$600 to \$899.....	18	22			
\$900 to \$1,499.....	9	10			
\$1,500 and over.....	5	9			
Total.....	100	99			

P2, except that nonreporting of demand deposits was even lower in Study P3, for which this asset served as a primary validation source.

With regard to response error, the significance of the results is more evident. On all three types of assets, large individual errors are frequent. Yet in the aggregate a clear tendency is apparent for these errors to produce overstatements of the face value of insurance and understatements of demand deposit balances, and to more or less cancel out in evaluating farm assets. In the case of life insurance, these overstatements are beneficial, serving to offset the effect of nonreporting so that the sample-based aggregates virtually coincide with the true aggregates. However, the understatements of reported demand deposit balances reinforce the effect of nonreporting in underestimation of the aggregate.

Here again some tendency appears for small amounts to be overstated and for large amounts to be understated. In terms of size distributions, however, this tendency—as well as one toward nonreporting—is of little consequence, the distributions compiled from the interview data corresponding closely with the true distributions. The principal effect of these errors shows up in the consistent underestimates of the measures of sampling variations, these measures and the associated confidence intervals being understated as much as 40 percent.

TABLE 47. RELIABILITY OF ESTIMATES OF SAMPLING VARIABILITY
IN ESTIMATES OF THE MEANS OF HOLDINGS OF DIFFERENT
FARM ASSETS, STUDY P3

Asset (1)	$k = e/\sigma_{\bar{y}}$ (2)	r (3)	Standard error of the mean			True probability of .95 confidence interval (7)
			Ap- parent (4)	True (5)	Error- free (6)	
Acreage (owners).....	-.09	-.22	26	26	25	.95
Acreage (renters).....	-.49	-.64	18	20	22	.92
Automobiles and trucks....	1.32	-.29	182	301	137	.74
Other machinery.....	.38	-.23	741	793	637	.93
Hogs.....	-1.84	-.79	246	516	401	.55
Cattle.....	-.10	.01	939	944	758	.95
Other livestock.....	-.39	-.71	53	57	71	.93
Crops.....	-.99	-.55	367	517	369	.83
Stock in lending agency...	-1.97	-.42	93	206	89	.49

Investigation of determinants of response error, feasible only for life insurance, suggests that — as with other holdings — high incomes were not associated with nonreporting or with high response error. Indeed, in the latter case, high income was more consistently associated with low response errors than with large response errors. These results also support earlier findings that nonreporting rises with age; that accuracy of reporting increases with use of records, with level of education, and with conduct of the interview at one's office instead of in the home; and that the use of a change form produces less reliable data than the use of a holdings form (the latter based on the analysis of errors in demand deposit balances).

All things considered, the accuracy of the reporting of the three types of assets studied in this chapter seems to be considerably superior to that of the reporting of time deposits and, in many respects, to that of debt as well. Indeed, for various farm assets the interview data would seem to be more reliable than the institutional data. Particularly noteworthy is the fact that reports of demand deposit balances are substantially more accurate than those of time deposit balances, although most of the discrepancies between reported and actual demand deposit balances appear to be the result of response errors rather than of check-float.

VI. PANEL EFFECTS

Three major potential sources of bias are recognized in the literature on consumer panels: changes in population composition, panel mortality, and panel conditioning effects. It is because of changes in population composition that panel data may become atypical of the population even though the original sample may have been perfectly representative at the time it was selected. Most panels are set up entirely at a particular moment in time; as a result, changes in population composition are reflected only to a limited extent in changes in the characteristics of the panel (ignoring, for the time being, the effect of panel mortality).

Theoretically, such factors could be taken into account in a continuing panel operation by instituting a system of panel-member rotation, and such adjustments are included in the large national panel operations of the United States Bureau of the Census and the Market Research Corporation of America.

A second source of error is panel mortality. Because of the continuous nature of a panel operation, nonresponse tends to be much higher than on the corresponding single-interview survey. Furthermore, this nonresponse is cumulative, tending to increase consistently, although at a decreasing rate, as time goes on.¹¹⁰

Actually, studies made so far indicate that bias due to panel mortality may be of much lesser consequence than has been feared. Thus, appreciable mortality was obtained in a financial panel operated by the Survey Research Center, in which panel members were interviewed five times at six-month intervals. Yet little demographic bias resulted from panel dropouts, even though disinterested people, renters, and low-income people were more likely to drop out.¹¹¹ In an earlier purchase survey in which

¹¹⁰ For example, see H. E. Allison, C. J. Zwick, and A. Brinser, "Recruiting and Maintaining a Consumer Panel," *Journal of Marketing*, Vol. 22, No. 4 (April, 1958), pp. 377-90; Robert Ferber, "Observations on a Consumer Panel Operation," *Journal of Marketing*, Vol. 17, No. 3 (January, 1953), pp. 246-59; M. G. Sobol, "Panel Mortality and Panel Bias," *Journal of the American Statistical Association*, Vol. 54, No. 285 (March, 1959), pp. 52-68; Industrial Surveys Company, *Problems of Establishing a Consumer Panel in the New York Metropolitan Area* (Washington: United States Government Printing Office, 1952).

¹¹¹ M. G. Sobol, *op. cit.*

panel members were interviewed every month for one year, no bias at all in demographic characteristics was observed.¹¹² Similarly, in a pilot operation preceding the Consumer Savings Project, no evidence of bias due to panel mortality was observed, either in the usual socio-economic characteristics of the refusals or in their asset holdings.¹¹³

To be sure, in none of these instances were validation data available. Therefore, the analysis of the effects of panel mortality could be carried out only on such indirect, observed variables as respondent characteristics, and not on the principal variables under investigation.

The third major source of panel bias, panel conditioning effects, is probably the most pervasive of all. Its source derives from the effects on the panel members of growing awareness over time of membership in the panel.¹¹⁴ Thus, one result of this growing self-consciousness could be a tendency by the panel member to modify his replies on attitudinal questions to bring them more into line with the majority view. Another result could be modification of replies on later interviews to rationalize answers given on earlier interviews. Still another result could emanate from loss of interest in the study, leading to the reporting of less accurate or less comprehensive data.

At the same time, panel conditioning effects may have favorable results as well. Thus, frequent interviews have led to improvement in memory, thereby producing more complete data on past behavior, such as on consumer purchases.¹¹⁵ In addition, the continuing contact between the interviewer and the panel member may reassure the latter of the authenticity and reliability of the study, or may prod his memory and lead him to provide more complete and/or more accurate information.

To date, evidence on panel conditioning bias is mixed. Little conditioning bias has been observed on attitudinal questions relating to business or economics.¹¹⁶ On the other hand, a fair amount of conditioning bias was observed in a food purchase panel, this bias serving to increase purchases with time.¹¹⁷ Panel conditioning effects were also responsible for a marked improvement in the accuracy in reporting durable goods pur-

¹¹² Ferber, "Observations on a Consumer Panel Operation," *op. cit.*

¹¹³ Robert Ferber, *Collecting Financial Data by Consumer Panel Techniques*, Studies in Consumer Savings, No. 1 (Urbana, Illinois: University of Illinois Bureau of Economic and Business Research, 1959), Chapter III.

¹¹⁴ A summary of early studies on the effect of repeated interviews on sample results can be found in F. L. Ruch, "Effects of Repeated Interviewing on the Respondent's Answers," *Journal of Consulting Psychology*, Vol. 5, No. 4 (July-August, 1941), pp. 179-82.

¹¹⁵ Ferber, "Observations on a Consumer Panel Operation," *op. cit.*

¹¹⁶ C. H. Sandage, "Do Research Panels Wear Out?" *Journal of Marketing*, Vol. 20, No. 4 (April, 1956), pp. 397-401; M. G. Sobol, *ibid.*

¹¹⁷ G. G. Quackenbush and J. D. Shaffer, *Collecting Food Purchase Data by Consumer Panel* (East Lansing, Michigan: Michigan State University, 1960).

chases in a monthly panel study.¹¹⁸ In addition, the pilot panel study for the Consumer Savings Project produced considerable evidence of conditioning effects, due primarily to consistent improvements in rapport through most of the panel operation.¹¹⁹ This improvement in rapport was reflected in a sharp decline in refusals of dollar amounts, in an increase in the number of panel members supplying financial data from records, and in pickup of a fair amount of financial assets not reported on earlier interviews although owned at that time. Some panel members did become less cooperative over time, but this number was small relative to those whose cooperativeness increased.

From the foregoing, it is clear that panel conditioning effects may affect sample data for better or for worse and may do so in numerous ways. In the present state of knowledge, it is difficult to predict the result of such effects in a particular study.

In what follows, our primary concern will be with panel mortality and with panel conditioning effects. Because of the absence of exact data and the short-run nature of these panel studies, the investigation of bias due to changes in population composition is hardly feasible. Moreover, the latter source of bias is likely to be of little consequence compared with the two other sources.

The general approach is to examine first some overall effects of the panel aspects on sample composition and on respondent cooperativeness in each of the panel studies of the Consumer Savings Project. This includes not only the panel studies, P1 to P3, but also the reinterview study, S3.¹²⁰ Because of the reinterview aspect, the latter study may be considered as roughly equivalent to a two-wave panel study, with interviews nine months apart. In each of these studies, the data permit us to focus on the influence of three aspects of the panel operation: changes in the composition of the panel, variations in the apparent cooperativeness of the panel members, and the extent of pickup of panel member holdings not reported previously.

Second, we may use the validation data to investigate the nature of response and nonresponse error in each of these studies, as will be done in the final part of this chapter. Such an analysis can be carried out only for those holdings for which primary validation data are available, which

¹¹⁸ Ferber, "Observations on a Consumer Panel Operation," *op. cit.*

¹¹⁹ Ferber, *Collecting Financial Data by Consumer Panel Techniques*, *op. cit.*

¹²⁰ One other panel operation of the project, that conducted in Chicago in 1956-57, is not included here because the circumstances under which it was conducted do not make the data comparable with those of the other operations. Members of that sample were interviewed initially as part of the larger one-interview study, and only later were they approached again to participate in panel operations. The full study is summarized in Ferber, *ibid.*

is usually not more than one asset or debt in each case. Nevertheless, these data throw considerable light on the dynamic aspects of such errors, especially since this is the first time such an analysis is feasible.

Changes in Panel Composition

The response experience on all four panel operations is summarized in Table 48. In all instances appreciable mortality is evident, as shown by the last column of figures in this table. Roughly half of the panel members were lost by the end of the first three operations. Study P3 was much more successful, with nearly three-fourths of the panel members still participating at the end of the fifth wave. As has been noted previously, in terms of response this operation was highly successful — much more so than any of the others.

The data in Table 48 indicate that in each of these four studies refusal, rather than failure to contact sample members, was the primary reason for sample mortality. This was even true of Study S3, although the attempt to avoid use of names led to a much higher rate of noncontacts than that found in the other studies. For these four studies noncontacts and dropouts because of ineligibility were virtually negligible.

The large majority of refusals occurred on the first three waves of interviews. By that time, 70 percent to 80 percent of the refusals encountered over all five waves had already been obtained. As is evident from the following tabulation of refusal rates over time for each of the longer panel operations, nearly half of the refusals were obtained in the first wave and another fairly large proportion in the next two waves, with the refusal rate declining sharply thereafter.

<i>Wave</i>	<i>Cumulative refusal rate</i>		
	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
1.....	20%	20%	10%
2.....	27	31	12
3.....	42	36	16
4.....	47	47	18
5.....	52	49	20

The refusal rates in the first three waves of Study P2 would have been much higher had it not been for retention until the fourth wave of sample members who were not giving financial information.

To what extent, if any, were those who dropped out of the panel operations different from those who remained? In view of the varying quantities of information available, it seems desirable to attempt to answer this question by comparing first those who dropped out in the first wave with those who remained in the first wave, and second, those who dropped out of later waves with those who remained in the panel to the end.

TABLE 48. RESPONSE AND NONRESPONSE DATA ON FOUR PANEL STUDIES

Characteristic	Wave					All waves
	1	2	3	4	5	
Study S3						
Initial sample size	168	109				168
Interviewed	109	80				80
Refused	34	15				49
Noncontact, still eligible	25	4				29
Noncontact, no longer eligible
Contact rate ^a (percent)	85	73				83
Response rate ^b (percent)	76	84				62
Mortality rate ^c (percent)	35	17				46
Study P1						
Initial sample size	296	251	244	178	155	296
Interviewed	210	187	178	154	131	131
Refused	60	20	44	16	13	137
Noncontact, still eligible	25	44	8	6	11	11
Noncontact, no longer eligible	1	..	14	2	..	17
Contact rate ^a (percent)	92	94	91	96	93	91
Response rate ^b (percent)	77	79	75	91	91	49
Mortality rate ^c (percent)	22	9	21	13	15	56
Study P2						
Initial sample size	411	304	263	266 ^d	219	411
Interviewed	316	255	233	210	205	205
Refused	82	45	20	47 ^a	7	198
Noncontact, still eligible	13	3	10	9	3	3
Noncontact, no longer eligible	1	4	5
Contact rate ^a (percent)	97	97	96	97	97	98
Response rate ^b (percent)	79	85	92	82	97	51
Mortality rate ^c (percent)	23	16	11	21	7	50
Study P3						
Initial sample size	417	180 ^f	350	160 ^{d,f}	314	417
Interviewed	365	169	321	153	301	301
Refused	41	8	19	7	5	95
Noncontact, still eligible	8	1	2	..	5	5
Noncontact, no longer eligible	3	2	8	..	3	16
Contact rate ^a (percent)	98	99	99	100	98	95
Response rate ^b (percent)	90	96	94	96	98	76
Mortality rate ^c (percent)	12	5	6	4	4	28

^a Contacts

Initial sample size

^b Interviewed

Interviewed plus refused

^c Noncontacts plus refused

Initial sample size

^d Includes 25 in Study P2 and 5 in Study P3 dropped from panel because of continued refusal to provide financial data.^e Only half of panel interviewed on this wave.^f Roughly half of panel interviewed by mail, with personal interview follow-up of nonrespondents.

Comparisons of the first type suggest that high response rates on the initial interview were more likely to be obtained from young people, from people of American nationality, and from farmers having large farms. On the other hand, refusals on the initial interview were somewhat more frequent among renters than among owners, among older people than among younger people, among small-sized families, and among those of other than American nationality. Such inferences have to be treated with considerable caution, however, because relatively little classifying information could be obtained on many of the refusals, particularly on such characteristics as age of the main wage earner and farm tenure.

Much the same characteristics serve to differentiate those who are likely to remain in the panel from those who are not. In particular, those remaining in the panel throughout the operation were more likely to be owners than renters, have homes valued between \$10,000 and \$25,000, be in the lower age groups, have medium-sized families, be of American nationality, and have somewhat smaller balances in their savings accounts. In most instances the differences were not large, but the fact that they occurred in all three panel operations would seem to impart greater significance to them than to other differences.

Examination of the relative frequencies with which people of different characteristics remain in the panel, starting with those interviewed on the first wave (and for whom much additional data are available), serves to reinforce the earlier findings. Thus, a higher proportion of younger people than of older people remained in the panel to the end. In addition, those with more education were more likely to remain in the panel operation (although this was not true in Study P1); the retired and self-employed were more likely to drop out than other occupational groups; and there was very little tendency for the dropout rate to vary with income levels, although the dropout rate did tend to fall as the value of the financial assets of the savings unit rose. To a large extent, these latter findings may represent interaction with age, since older people tend to possess more financial assets and are, of course, more likely to be retired.

Thus, these data suggest that refusals and dropouts were not uniform by socio-economic characteristics, but were somewhat higher among older age groups, among those with less education, among those with greater value of assets, and among the self-employed. These results are consistent with the earlier panel study (mentioned in footnote 113, p. 160) which also showed that refusals and dropouts were more frequent among those with little education, among the self-employed, and among people who were older, although not in the oldest age category.

Cooperativeness

Cooperativeness has many different facets. In this analysis, the cooperativeness of the panel members over time is studied in three respects: the pattern of cooperativeness over time and the nature of the changes that occur, the extent to which the degree of cooperativeness varies by different population groups, and the reasons for changes in cooperativeness over time. These questions are studied with the aid of two different sets of data — subjective information obtained about the interview situation and objective data on the same subject. The subjective information comes from interviewer reports on each interview recorded on Interviewer Report Forms (some of which are shown in Appendix A), primarily in the form of rating of respondent attitudes, such as overall cooperativeness, accuracy of data supplied, and completeness of data supplied, as well as changes in these attitudes from one wave to another. The objective information relates also to respondent attitudes but is more factual in nature, such as reports on respondent use of records and refusal to answer questions.

To a large extent, these two sets of data supplement each other, throwing light on different aspects of respondent cooperativeness. This is especially so in view of the experimental nature of these operations, one result of which was asking different questions on respondent cooperativeness at different stages of the panel operation.

Patterns Over Time

Interviewer ratings of various aspects of respondent cooperation in Study P1 are presented in Table 49. Four of these ratings are subjective and one — use of records — is objective. All five of these ratings suggest that cooperation increased over time, although the suggestion is stronger for some characteristics than for others. On cooperativeness and on completeness, a clear tendency is present for the percentage rated “excellent” to rise over time and for the proportion rated “poor” or “very poor” to decline. On accuracy, the proportion rated “excellent” rises, but so does the proportion rated “poor” or “very poor.” Much the same is true on evidence of withholding data: relatively fewer sample members are labelled as sure withholders over time, but relatively fewer are also rated as clearly not withholding anything. The objective measure of use of records indicates that a much lower proportion of the sample members did not use any records as time went on, although the proportion always using records did not increase.

To measure the relative sensitivity of these measures to changes in

TABLE 49. MEASURES OF RESPONDENT COOPERATION
OVER TIME, STUDY P1

Characteristic	Wave	Base	Percentage of total respondents rated			
			Ex- cellent	Good	Fair	Poor or very poor
Degree of cooperativeness.	1	199	52	29	13	7
	2	182	62	26	10	3
	3	171	64	24	8	3
	4	71	63	23	6	9
	5	137	68	20	9	4
Degree of accuracy.	1	195	36	28	18	7
	2	181	37	25	20	6
	3	170	40	29	14	7
	4	71	38	28	13	6
	5	137	43	31	13	12
Degree of completeness.	1	194	37	32	16	14
	2	181	45	23	18	15
	3	170	45	30	12	12
	4	71	44	27	8	21
	5	137	50	28	15	7
Use of records.			Always	Occasionally	Never	
	1	101	29	27	45	
	2	181	29	27	44	
	3	173	20	36	43	
	4	71	11	51	38	
Evidence of withholding data. . . .	5	137	28	41	31	
			None	Some	Clear evidence	
	1	201	70	9	21	
	2	183	69	9	22	
	3	172	61	17	22	
	4	71	55	25	20	
	5	137	64	22	14	

respondent cooperation over time, the following so-called Index of Sensitivity (S) will be used:

$$\frac{\sum_i |P_{it} - P_{i,t-1}|}{n} \cdot \frac{n}{200} = \frac{\sum |P_{it} - P_{i,t-1}|}{200}$$

where

P_{it} is the proportion of sample members at time t rated in class i ,
 $P_{i,t-1}$ is the proportion of sample members at time $t-1$ rated in class i , and
 n is the number of ratings, i.e., $i = 1, \dots, n$.

TABLE 50. RATINGS OF CHANGE IN RESPONDENT COOPERATION
SINCE LAST INTERVIEW, STUDY P1

Characteristic	Waves	Base	Percentage rated		
			Better	Same	Worse
Degree of cooperativeness	2 vs. 1	88	30	68	2
	3 vs. 2	150	19	73	8
	4 vs. 3	64	9	83	8
	5 vs. 4	100	12	80	8
Degree of accuracy	2 vs. 1	89	30	66	3
	3 vs. 2	150	20	69	9
	4 vs. 3	63	13	71	16
	5 vs. 4	100	12	77	11
Degree of completeness	2 vs. 1	89	27	71	2
	3 vs. 2	150	22	70	8
	4 vs. 3	63	11	75	14
	5 vs. 4	100	15	74	11

The value of the first term on the left can vary between $200/n$ and zero. Hence, dividing by $200/n$ yields a standardized ratio that varies between zero and one. The higher is the value of S , the greater is the overall change in cooperation by that characteristic. However, the direction of the change has to be inferred from the data.

Applying this measure to the data in Table 49 for the first and last waves yields the following values of S :

Degree of cooperativeness15
Degree of accuracy10
Degree of completeness125
Use of records145
Evidence of withholding data13

These results suggest that ratings of cooperativeness and use of records have noticeably more of what might be termed discriminatory power than the other ratings. Moreover, both of these ratings indicate that the cooperativeness of sample members increased over time. This inference receives further support from the data in Table 50, based on interviewer ratings of change in respondent attitudes from one wave to another for three of the same characteristics covered in Table 49. Particularly on the earlier waves, the proportion of sample members rated as improving is seen to be substantially higher than the proportion becoming worse, for each of these three characteristics. Over time the proportion of the sample improving by any of these three ratings declines while the proportion becoming worse rises. In part this trend is a statistical phenomenon, for with increasing proportions of sample members being rated "excellent"

TABLE 51. DROPOUT RATES BY MEASURES OF RESPONDENT COOPERATION, STUDY P1

Characteristic	Wave	Percentage with given rating on previous wave dropping out on current wave				
		Excellent	Good	Fair	Poor	Very poor
Degree of cooperativeness,	2	10	21	24	33 ^a	50 ^a
	3	5	15	39	67 ^b	0 ^b
	4	5	20	36	0 ^b	75 ^b
	5	16	6	0 ^b	0 ^b	75 ^b
Degree of accuracy	2	11	7	23	37	67 ^a
	3	3	4	24	20	33 ^a
	4	3	12	5	28	67
	5	9	20	11	45	100 ^b
Degree of completeness	2	8	13	25	33	44
	3	2	7	34	24	33 ^b
	4	4	16	10	17	67 ^a
	5	13	11	14	54	100 ^b
Use of records		Always	Occasionally		Never	
	2	14	7		29	
	3	2	8		21	
	4	3	5		25	
	5	12	11		44	
Evidence of withholding data		None	Some		Clear evidence	
	2	10	21		38	
	3	7	19		25	
	4	5	10		26	
	5	13	22		57	

^a Based on 5 to 9 observations.^b Based on fewer than 5 observations.

on an absolute basis (Table 49), the proportion that could be rated better in a temporal sense is bounded more at the upper limit than at the lower limit. In part, too, the increasing proportion rated "worse" does suggest less cooperation on the part of some of the sample members.

That these ratings do reflect the cooperation tendencies of the panel members is brought out by the data in Table 51, showing what proportion of those given a particular rating on one wave dropped out on the next wave. For all five characteristics and for all waves a strong tendency is apparent for the dropout rate to rise as the rating becomes worse. If anything, this tendency seems to become more pronounced over time, with the spread between dropout rates at the extremes rising from Wave 2 to Wave 5 for each of the five characteristics shown, although the sample sizes for many of the poorer ratings are very small.

Much the same results were obtained with Study P2 as with Study P1. Cooperation improved generally over time for all the characteristics on which ratings were obtained. Since ratings were not obtained in most instances on so continuous a basis as on Study P1, it is difficult to compare the sensitivity of these various measures. However, some comparisons are possible, and in those cases the following values of S were obtained for Study P2:

<i>Waves</i>	<i>Characteristic</i>	<i>S</i>
2 to 5.....	Interest in study	.11
	Attitude toward giving information	.04
	Use of records	.15
1 to 4.....	Accuracy on checking accounts	.08
	Accuracy on savings accounts	.08
	Accuracy on life insurance	.24
	Accuracy on corporate stock	.18
	Completeness on checking accounts	.065
	Completeness on savings accounts	.12
	Completeness on life insurance	.12
	Completeness on corporate stock	.02

As before, use of records turns out to have relatively high sensitivity, the highest of the three ratings with which it can be compared. However, even greater sensitivity is obtained with the accuracy ratings on life insurance and on corporate stock. In the former case in particular, substantial improvement in both accuracy and completeness of the data is reported. Why this should be more characteristic of life insurance than of any of the other three assets is not clear. Possibly to the extent that sample members became more cooperative, they may have been more likely to report fully on a relatively insensitive asset such as life insurance — or it may have seemed that way to the interviewer. If so, however, the value of S for the checking account ratings should not be much less than for those of life insurance, but this is clearly not true.

The ratings of change in cooperation on Studies P2 and P3 exhibit much the same pattern as those on Study P1. The proportions improving exceeded the proportions becoming less cooperative on every wave, although the difference between the two sets of proportions declined over time. At the same time, the ratings became more stable over time, much more so than in Study P1. By the last wave, roughly 85 percent of the sample members on either rating in Study P2 did not shift in cooperativeness.

The meaningfulness of the ratings is again supported by comparisons of the proportions of sample members with given ratings on one wave dropping out on the next wave. These comparisons show that, by any characteristic rated, a much higher proportion of those rated low drop

TABLE 52. REFUSAL RATE FOR SELECTED HOLDINGS,
BY WAVE AND BY STUDY

(PERCENTAGE OF KNOWN OWNING SAVINGS UNITS REFUSING AMOUNTS)

Wave	Savings accounts	Stocks and non-United States government bonds	Life insurance	Ten assets and debts ^a	Constant sample: stocks and non-United States government bonds
Study P1					
1.....	27.8	38.2	20.8	25.7	29.4
2.....	25.4	32.2	23.5	23.5	30.0
3.....	20.2	13.8	5.6	13.5	9.9
5.....	8.9	4.1	3.8	7.9	4.5
Study P2					
1.....	13.2	13.3	15.0	10.6	4.5
2.....	17.1	13.3	6.5	10.6	5.1
3.....	14.3	14.9	9.1	10.1 ^b
5.....	6.6	1.1	1.6	2.0	0
Study P3					
1.....	14.1	2.2	1.3	4.0	5.0 ^c
2.....	14.3	3.8	0.0	3.1	5.0 ^c
3.....	8.8	8.2	3.1	4.8 ^b
5.....	7.0	5.2	.7	2.5	4.0 ^c

^a Includes checking accounts, savings accounts, government bonds, corporate stock and non-United States government bonds, life insurance, brokerage accounts, annuities, mortgages, and other non-installment debt.

^b Savings units are counted only if dollar holdings (not change) were requested. Wave 4 is omitted because all savings units were asked only for change on that wave; the same is true of the constant sample on Wave 3 of Studies P2 and P3.

^c Savings accounts, rather than stocks and nongovernment bonds.

out on the next wave than of those rated high. As before, the difference between dropout rates at high and low ratings tends, if anything, to increase over time.

A final, and fairly objective, measure of the pattern of respondent cooperation over time is the trend in the refusal rate for selected holdings. This rate is defined as the proportion of sample members interviewed on a particular wave who refuse to give dollar amounts of their holdings when asked.¹²¹ It is not an overall measure of respondent cooperation because the base necessarily excludes nonreporters and because respondents could be apparently cooperative but supply misleading information. It is essentially a supplementary measure of cooperativeness.

These refusal rates are shown in Table 52 for all three studies for

¹²¹ Except for corporate stock and bonds, in which cases the refusal refers to the name and number of the holding.

selected individual assets and for a group of 10 holdings, as listed at the bottom of that table. According to these data, refusal rates declined sharply in virtually all instances, for both individual holdings and for the group of 10 holdings. Moreover, similar declines are apparent for stocks and nongovernment bonds in Studies P1 and P2 and for savings accounts in Study P3, as shown in the last column of Table 52. In this sense, therefore, cooperativeness appears to have risen sharply during all three panel operations, and to a large extent because of improved rapport with respondents remaining in the panel.

Cooperation by Respondent Characteristics

Data on the extent to which degree of cooperation varied with a number of respondent characteristics are summarized in Table 53 for the three panel operations. A plus sign indicates a noticeable positive relationship between higher values of the respondent characteristic and reliability of the data by the particular rating involved, a negative sign indicates a reverse relationship, and a zero shows absence of any noticeable relationship.¹²² For those respondent characteristics that do not follow any numerical order, that category of the characteristic for which the interviewer rating is highest is shown in the tables. To facilitate comparability among ratings, the same directional system of notation has been used for the ratings of uncooperativeness, such as "evidence of withholding of data." In other words, in those instances a positive sign indicates a positive association with what might be termed "data reliability," meaning a negative association with the frequency of the particular rating.

For Study P1, Table 53 shows that data reliability tends to increase with size of the savings unit, with education, with the amount in savings accounts, and with the "blue collariness" of the occupation. Reliability tends to decline with age and possibly with income and does not appear to be associated with value of home and with gross value of (financial) assets. However, the data relating to Study P2 do not exhibit the same patterns. In that study, reliability appears to be associated positively not only with education and with blue-collar occupations, the same as in Study P1, but also with American nationality, which was not the case in the previous study. Gross value of assets exhibits a negative relationship with data reliability, but no such relationship was shown in Study P1.

The data for the farm panel present perhaps the least-consistent

¹²² A "noticeable relationship" is admittedly somewhat arbitrary. In practice, the criteria for establishing such a relationship were either a monotonic trend in the proportions with extreme ratings or a sharp drop (10 percentage points or more) from one end of a respondent characteristic to the other. Since the basic data are presented in Appendix B, the reader is free to modify these definitions as he wishes.

TABLE 53. RELATIONSHIP BETWEEN RESPONDENT CHARACTERISTICS AND COOPERATION RATING, STUDIES P1 TO P3

Study P1					
Respondent characteristic	Cooperativeness	Accuracy	Completeness	Use of records	Withholding data ^a
Value of home.....	+	0	-	0	0
Age of main wage earner....	-	-	-	-	-
Size of savings unit.....	+	0	+	0	+
Nationality.....	Other	Other	0	U.S.	0
Occupation.....	Labor	Labor	Labor	Labor	Labor
Education.....	+	+	0	0	+
Income.....	+	-	+	-	-
Gross value of assets.....	+	0	0	+	-
Balance in savings accounts..	+	+	+	+	+

Study P2			
Respondent characteristic	Use of records	Willingness to give financial information	Accuracy of savings accounts
Value of home.....	0	0	+
Age of main wage earner....	+	0	-
Size of savings unit.....	-	0	+
Nationality.....	U.S.	U.S.	U.S.
Occupation.....	Labor	Self-employed	Labor, professional
Education.....	+	+	+
Income.....	0	+	-
Gross value of assets.....	0	-	-
Balance in savings accounts..	-	0	0

Study P3					
Respondent characteristic	Checking account balances		Data withheld ^b	Lack of knowledge ^b	Use of records
	Accuracy	Completeness			
Condition of home.....	-	-	+	+	+
Age of farmer.....	0	+	-	-	-
Size of savings unit.....	-	-	0	-	0
Nationality.....	U.S.	U.S.	Other	0	0
Tenure.....	Owner	Owner	Tenant	Tenant	Tenant
Education.....	-	0	+	+	+
Income.....	0	0	-	-	-
Value of assets.....	0	0	-	0	0
Size of farm.....	0	-	+	+	+
Amount of farm debt.....	-	-	+	+	0
Balance in savings accounts..	0	0	-	-	0

^a Relationship is with improved reliability of data. For example, a negative sign for "age of wage earner" means that as age increased, withholding of data was suspected more frequently, i.e., the data seemed less reliable.

^b Relationship is with improved reliability of data. For example, a plus sign under "data withheld" for "condition of home" means that as the condition of the home improved, instances of data withheld became relatively less frequent, i.e., the data appeared to be more reliable.

picture of all. In only one instance are as many as four of the ratings consistent with each other. Judging by the instances in which three or more ratings coincide, there is evidence of a positive association between data reliability and condition of home, education, size of farm, and tenant status. Tendency to cooperate appears to decline with age of the farmer, income, and size of the savings unit; but no relationship appears to exist with nationality, value of assets, and amount in savings accounts. However, these broad statements have to be hedged by the fact that Table 53 in many ways exhibits two different sets of relationships, one set for the ratings of checking account balances and the other set for the other three more general ratings. This is brought out when we compare the two sets of ratings by characteristic, using plus, minus, and zero signs only when all the ratings in a set agree with each other. The result is:

	<i>Checking account ratings</i>	<i>Other ratings</i>
Condition of home.....	—	+
Age of farmer.....	—
Size of savings unit.....	—
Nationality.....	United States
Tenure.....	Owner	Tenant
Education.....	+
Income.....	0	—
Value of assets.....	0
Size of farm.....	+
Amount of farm debt.....	—
Balance in savings accounts.....	0

On this basis, checking account ratings are seen to be negatively correlated with condition of home, with size of savings unit, and with the amount of farm debt; to be positively correlated with American nationality and with ownership of the farm; and to show no relationship to income, value of assets, and balance in savings accounts. On the other hand, the more general ratings indicate positive correlation with condition of home, with education, with tenant status, and with size of farm; and negative correlation with age of farmer and with income. Clearly, in three of these instances the two sets of ratings conflict with each other — in condition of home, in tenure status, and in income. In addition, Table 53 suggests that three more conflicts may exist, namely, in size of savings unit, in amount of farm debt, and in balance in savings accounts.

Altogether, the two sets of ratings appear to disagree much more often than they agree. However, they may well be measuring different things, the checking account ratings focusing on a specific asset and the other ratings providing a more general indication of the reliability of the data. If so, this suggests that overall ratings may camouflage a heterogeneous

pattern of data reliability in the interview and that a respondent may provide reliable data on one set of questions but not on another. General ratings may still be useful since, as has been shown in the previous section, such ratings do serve to reflect respondents' propensity to drop out of a panel operation. However, for analyzing the reliability of data on individual holdings it would seem desirable to secure ratings for each holding separately.

From a more general point of view, the data in this section suggest that respondent cooperation does tend to vary with particular characteristics. Thus, taken together, these studies show that respondent cooperation tends to increase with education and with laboring occupations and to decline with age and possibly with income. Perhaps equally significant is the fact that there is relatively little evidence to suggest that respondent cooperation is influenced by the gross value of assets or by the size of savings account balances, particularly since Study P1 had basically a higher income sample than Study P2.

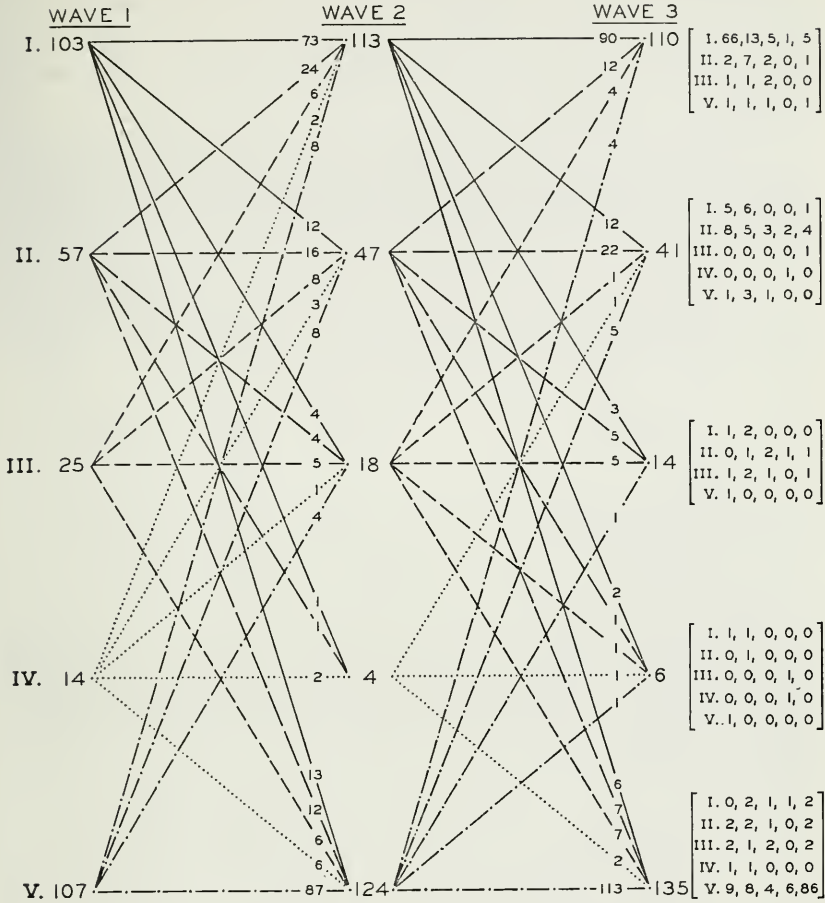
Reasons for Change in Cooperativeness

Did the improvement in panel cooperativeness over time result primarily from a change in the sentiments of continuing panel members or was it a manifestation of differential panel mortality? Circumstantial evidence from data presented in preceding sections suggests the latter explanation. Dropouts were more frequent among older people, among those with less education, among the retired and the self-employed, and among farm owners. At the same time, Table 53 shows that, by and large, these were the groups to receive poor cooperativeness ratings initially. Hence, if these groups contain more dropouts than others, it seems plausible to infer that higher frequency of panel mortality among the less cooperative is the principal reason for the improvement in cooperativeness over time.

Such an inference cannot be conclusive, however, since the possibility remains that many of the dropouts, even in these particular groups, were not less cooperative than other panel members. In other words, comparing marginal distributions at different points in time, as was done earlier in this chapter, cannot provide a definitive answer to this question. Such an answer can only be obtained by studying joint distributions over time. Distributions of this type for a number of cooperativeness ratings have been obtained by preparing "tree diagrams" in which the cooperativeness rating of each panel member was traced from one wave to another, as shown in Figure 2 for the first three waves of Study P1.

The numerals in this diagram show how many respondents received different ratings on each wave and how these ratings changed from one

FIGURE 2. COOPERATIVENESS: WAVES 1, 2, AND 3, STUDY P1



wave to another. For example, 103 people were rated "excellent" in terms of cooperativeness on Wave 1. On Wave 2, 113 received the same rating, this number consisting of 73 rated "excellent" on Wave 1, 24 rated "good" previously, 6 rated "fair" previously, 2 rated "poor" or "very poor," and 8 nonrespondents. On Wave 3, 110 people received a rating of "excellent," the numerals on the lines between Wave 2 and Wave 3 showing how many of these people had the same or different ratings on Wave 2. The figures in the brackets following the Wave 3 figures show the Wave 1 origin of each group for each rating on Wave 3. For example, the first row of the matrix next to 110 on Wave 3 indicates that of those receiving this rating on that wave, 66 were rated "excellent" on Wave 1, 13 were rated "good"

on Wave 1, 5 were rated "fair" on that wave, 1 was rated "poor" or "very poor," and 5 were nonrespondents on Wave 1. The total is 90, the number of the 110 to receive a rating of excellent on Wave 2. The other rows of this matrix and the the other four matrixes are interpreted in a similar manner.

Additional waves are not shown because of the tremendously complicated nature of these diagrams. However, these three waves alone are sufficient to provide clear answers to the central question of this section, as is evident from the following inferences that may be drawn from Figure 2:

(1) Cooperativeness among panel members rose from Wave 1 to Wave 2 and changed very little on the following wave. The relative marginal distributions, excluding the nonrespondents, are as follows:

	<i>Wave 1</i>	<i>Wave 2</i>	<i>Wave 3</i>
Excellent.....	52%	62%	64%
Good.....	28	26	24
Fair.....	13	10	8
Poor or very poor.....	7	2	4
Total.....	100%	100%	100%
Base number.....	199	182	171

(2) Dropouts on Wave 2 were primarily those who were less cooperative on Wave 1, and the same phenomenon carried over, to a lesser extent, on Wave 3. The following are the percentages of sample members given a particular cooperativeness rating on Wave 1 who dropped out on Wave 2 or on Wave 3:

	<i>Wave 2 dropouts</i>	<i>Wave 3 dropouts</i>
Excellent.....	13%	5%
Good.....	21	11
Fair.....	24	16
Poor or very poor.....	43	7

Of those with particular ratings on Wave 2, the following proportions dropped out on Wave 3:

Excellent.....	5%
Good.....	15
Fair.....	26
Poor or very poor.....	25

(3) Cooperativeness among those interviewed on all three waves also rose substantially from Wave 1 to Wave 2 and retrogressed slightly on the next wave, as is evident from the following marginal distributions for this "constant" sample:

	<i>Wave 1</i>	<i>Wave 2</i>	<i>Wave 3</i>
Excellent.....	58%	69%	68%
Good.....	27	23	21
Fair.....	10	6	8
Poor or very poor.....	5	1	3
Total.....	<u>100%</u>	<u>99%</u>	<u>100%</u>
Base number.....	146	146	146

(4) Cooperativeness among those later dropping out was much lower than among panel members interviewed on all three waves, as shown by the distribution of these ratings on Wave 1 of those dropping out on Waves 2 or 3:

Excellent.....	34%
Good.....	34
Fair.....	19
Poor or very poor.....	13
Total.....	<u>100%</u>
Base number.....	53

The conclusion is clearly that the increase in cooperativeness over time is a product of two factors—the greater panel mortality among those less cooperative initially and the increase in rapport among those remaining in the panel. Perhaps most surprising is the fact that the increase in cooperativeness among those that remained in the panel appears to have been at least as large as that brought about by differential panel mortality.

Other comparisons based on these three diagrams suggest, however, that in most instances the differential panel mortality is primarily responsible for the increase in cooperativeness. Comparisons for terminal waves of each panel, based on respondent distributions for both total and constant samples, reveal that overall cooperativeness improved in every instance. In no case, however, was the improvement in cooperativeness of the constant sample so pronounced as that of the total sample.

Supporting data on this general point are provided in Table 54, which shows the percentage of panel members with particular ratings on Wave 1 that had dropped out by the terminal wave. In all instances, the drop-out rate correlates inversely with degree of cooperativeness, the increase in dropout rates with lowered cooperativeness being particularly substantial for Studies P1 and P2. The principal cause of improved cooperativeness, therefore, appears to have been differential panel mortality.

Pickup

The frequency of pickup in the three panel operations is shown in Table 55 for each of 10 types of holdings. As is evident from this table,

TABLE 54. DROPOUTS AS PROPORTION OF TOTAL PANEL MEMBERS, BY COOPERATIVENESS RATINGS ON EARLIER WAVES

Study	Characteristic	Waves compared	Percentage rated			
			Excellent	Good	Fair	Poor or very poor
P1	Accuracy of report.	1 to 5	30	27	46	59
	Completeness of report. . .	1 to 5	25	34	41	64
P2			Fully	Fairly	Not much	Very little
	Accuracy of checking accounts.	1 to 4	42	52	67	92
	Accuracy of savings accounts.	1 to 4	39	60	47	77
	Completeness of checking accounts.	1 to 4	44	58	82	88
	Completeness of savings accounts.	1 to 4	43	66	15	60
	Interest in study.	1 to 4	3	19	33	61
			Always	Sometimes		Never
P3	Use of records.	1 to 4	17	19		53
	Use of records.	1 to 5	8	14		26
			Excellent	Good	Fair	Poor or very poor
	Willingness to give financial information. . .	2 to 5	4	8	22	25

pickup varied considerably, not only by holding, but also by the particular panel operation. Thus, pickup of checking accounts on Study P3 appears to have been negligible, whereas pickup of stocks and bonds on Study P2 amounted to as much as 36 percent of the total, in terms of the number of holdings. Moreover, although pickup of the total value of checking accounts was very low on Study P3, it had reached a figure of almost 18 percent of these accounts on Study P1.

Nevertheless, certain general patterns appear to exist in these data. Thus, on an overall basis, pickup seems to have been lower for Study P3 than for either of the other two studies. This may have been due to the possibly higher interviewer efficiency obtained on this last study or to the greater cooperativeness of farmers as sample respondents.

Also evident is the fact that pickup was much higher in value terms than in terms of the number of holdings for Study P1, whereas the op-

TABLE 55. PICKUP OF FINANCIAL HOLDINGS ON LATER WAVES, BY STUDY
(PERCENTAGE OF TOTAL HOLDINGS PICKED UP AFTER WAVE 1)

Holding	Number of holdings			Value of holdings		
	Study P1	Study P2	Study P3	Study P1	Study P2	Study P3
Checking accounts.....	7.5	8.6	1.7	18.0	7.1	.3
Savings accounts.....	7.0	11.2	7.0	35.1	6.7	10.7
United States government savings bonds.....	19.3	23.1	5.8 ^a	5.8	17.6	14.1 ^a
Stocks and other bonds.....	8.7	36.2	11.7	23.8	17.3	4.5
Life insurance.....	3.8	16.7	6.8	7.4	15.1	5.7
Pension plan equity.....	0	23.1	... ^b	5.8	.7	... ^b
Loans to others.....	14.3	29.4	5.0	46.3	16.9	7.4
Real estate.....	2.6	11.3	10.6 ^c	6.7	11.4	.5 ^c
Mortgages.....	4.9	12.7	7.0 ^d	4.1	14.3	.6 ^d
Personal loans.....	30.6	22.2	5.3 ^e	43.1	8.8	2.3 ^e

^a Includes other bonds as well.^b Not asked.^c Nonfarm real estate only.^d Secured debt.^e Unsecured debt.

Note: If mention was made on a previous wave (a) of a holding without giving its value or (b) of the total value of a holding without giving more details, e.g., number of United States savings bonds, it was considered a pickup on the later wave when (a) value or (b) number was reported.

posite was the case for Study P2. This phenomenon is highlighted in Table 56, which compares the average size of holdings picked up with the average size of holdings reported on the initial interview, by study and by holding. In the case of Study P1, the average size of the pickup is seen to exceed considerably the average size of the initial holding. As a rule, however, the reverse is true for Study P2, and for Study P3 to a lesser extent. The explanation for this somewhat contradictory result may lie in the timing of these operations and in the much higher representation of the upper-income levels in Study P1. By the time of Study P2, interviewing procedures had been developed more fully and results from Study P1 could be utilized as a basis for special interviewer training in areas in which mistakes or omissions appeared to be frequent. In addition, because of the much higher income level of many of the sample members in Study P1, more time was required to convince them of the authenticity of the study and of the need for full cooperation. As a result, a number of wealthier sample members did not begin to provide complete information on their holdings until the second or third wave. By contrast, in the later studies much more stress was placed on complete reporting in the very first interview, so that holdings not reported until later were more likely to represent inadvertent omissions rather than deliberate withholding.

Although considerable variation occurred in the pickup of different holdings from one study to another, there is some tendency for pickup to

TABLE 56. AVERAGE DOLLAR SIZE OF PICKUP HOLDING AND OF ORIGINALLY REPORTED HOLDING, STUDIES P1 TO P3

Holding	Study P1		Study P2		Study P3	
	Initial	Pickup	Initial	Pickup	Initial	Pickup
Checking accounts.	\$ 1,563	\$ 4,265	\$ 1,352	\$ 1,082	\$ 1,021	\$ 176
Savings accounts.	1,161	8,283	2,353	1,297	1,061	1,687
United States government bonds.	304	78	139	99	456 ^a	1,231 ^a
Stocks and other bonds. . . .	4,848	15,896	2,968	1,097	1,246	447
Life insurance.	8,474	17,278	4,251	3,780	2,958	2,437
Pension plan equity.	10,769	28,800	4,320	100 ^b ^b
Loans to others.	2,753	14,233	2,873	1,405	1,970	3,000
Real estate.	31,229	73,267	16,549	16,691	10,201 ^c	440 ^c
Mortgages.	12,437	10,313	7,677	8,815	8,109 ^d	678 ^d
Personal loans.	1,611	2,763	3,503	1,186	1,469 ^e	626 ^e

^a Includes other bonds as well.^b Not asked.^c Nonfarm real estate only.^d Secured debt.^e Unsecured debt.

be highest for loans to others, for personal loans, and for stocks and bonds other than United States Government savings bonds. Even for other holdings, pickup tends to represent anywhere from 10 to 30 percent of the total number or value of holdings. Only in the case of Study P3 did pickup amount to less than 10 percent for most holdings.

Further investigation reveals that pickup is a characteristic of less than half of the sample members. In this respect, the two urban studies provide similar results, as is apparent from the following tabulations:

Number of pickups	Percentage of savings units	
	Study P1	Study P2
0.	55%	55%
1.	20	15
2.	8	11
3.	8	5
4.	2	3
5 or more.	6	11
Total.	99%	100%
Base (number of savings units).	226	312

In each study, pickup was characteristic of 45 percent of the panel members. For most of the savings units in which pickup occurred, two or more additional holdings were obtained. In some instances, however, five or more additional holdings were obtained, a phenomenon much more frequent in Study P2 than in Study P1.

Various cross-tabulations suggest that pickup of one holding tends to

be correlated with pickup of other holdings. For example, the following tabulation illustrates the extent to which pickup of savings accounts was related to pickup of life insurance in Study P1.

<i>Life insurance</i>	<i>Savings accounts</i>		
	<i>Yes</i>	<i>No</i>	<i>Total</i>
Yes.....	14	21	35
No.....	18	173	191
Total.....	32	194	226

Response Accuracy Over Time

Having examined some of the overall effects of a consumer financial panel operation on the sample, we now turn to the effect of the panel on response and nonresponse errors in the reporting of financial data. Such effects can only be measured for holdings serving as the basis for primary validation in the panel studies, which restricts the analysis to debt on Study P3, time deposits on Studies P2 and P3, and demand deposits on Study P3.

Farm Debt

The data requested from the panel members of Study P3 were affected, it will be recalled, by two experimental manipulations. First, half of the panel was interviewed every three months and the other half was interviewed every six months. Second, half of the three-month panel members and half of the six-month panel members were requested to report holdings on the first two waves, changes on the next two, and holdings on the last wave; the other half was asked for change on the first two waves, holdings on the third, change on the fourth, and holdings again on the last wave. For future reference, it is useful to represent the factorial design diagrammatically and to label each group, as follows:

<i>Wave</i>	<i>Group</i>			
	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>
1.....	Holdings	Holdings	Change	Change
2.....	Holdings		Change	
3.....	Change	Change	Holdings	Holdings
4.....	Change		Change	
5.....	Holdings	Holdings	Holdings	Holdings

As a result of this design, the following questions relating to panel effects on reporting can be investigated, based on comparisons of the experimental groups:

- (1) How do response and nonresponse errors in reports of (debt) holdings vary over time?

(2) Is a similar pattern evident among errors in reporting changes in debt?

(3) Are response and nonresponse errors fewer and smaller in reports of holdings in interviewing panel members every three months or every six months? What about reports of changes?

(4) Do panel members tend to report more accurately if holdings are requested in the very first interview or if a "softer" approach is used and only change figures are sought at the outset?

(5) Do reports of change improve after panel members have been asked for holdings?

(6) Are changes computed from holdings data reported at two points of time more accurate than changes reported directly?

These questions serve as the framework for the analysis that follows, being taken up first with regard to number of holdings and then with regard to dollar amounts.

Holdings Reported

The extent to which validated debts were reported on the various waves is shown in Table 57, for each of the four experimental groups separately. The data relate to individual debts rather than to individual savings units. This is because a few savings units had more than one validated debt, which makes it more convenient to carry out the analysis on the latter basis.¹²³

The table shows that the number of validated debts in each of the four subsamples declined markedly throughout the course of the panel operation. To some extent, this was because a certain number of debts were paid off.¹²⁴ Deaths and moves out of the area accounted for an additional part of this decline. Most important, however, was the tendency for people to drop out of the panel on later waves, which was most pronounced on the second and third waves.

Other facets of the panel operation appear to have acted in the direction of improving the reliability of debt-reporting. First, nonresponse fell off sharply on later waves, so that more of the total validated debt was represented by the respondents. Second, the frequency of nonreporting of debt by respondents declined throughout the operation, particularly on the change forms (Groups C and D) on the second and third waves.

¹²³ Cursory examination suggests that the results would not have been altered in either event.

¹²⁴ At the same time, other debts were being incurred and reported. However, these debts do not properly belong in this table and have therefore been excluded, with the exception of "new" debts with the primary validating institutions. These debts are included because such debts could be construed as continuations of previous debts, and they were so treated by some institutions.

TABLE 57. DISTRIBUTION OF VALIDATED DEBTS, BY SAMPLE RESPONSE STATUS, STUDY P3

Category	Wave					After five waves
	A. Holdings, three months					
	1	2	3	4	5	
Total sample.....	64	64	65 ^a	65	65	65
Respondents.....	57	52	49	48	48	48
Refused amount; not asked.....	1
Nonreporters.....	10	5	6	5	4	4
Reporters.....	47	39	32	30	31	31
Debts paid off.....	..	8	10	13	13	13
Nonrespondents.....	7	5	5	1	..	17
Noncontacts.....	..	1
Refusals.....	7	4	5	1	..	17
Prior dropouts.....	..	7	11	16	17	..
C. Changes, three months						
Total sample.....	69	69	70 ^a	70	70	70
Respondents.....	66	59	58	57	57	57
Refused amount; not asked.....	..	4
Nonreporters.....	22	8	6	5	2	2
Reporters.....	44	42	45	44	40	40
Debts paid off.....	..	5	7	8	15	15
Nonrespondents.....	3	7	2	1	..	13
Noncontacts.....	..	2	2	4
Refusals.....	3	5	..	1	..	9
Prior dropouts.....	..	3	10	12	13	..
D. Changes, six months						
Total sample.....	67	67	68 ^a	68	68	68
Respondents.....	63	63	58	56	56	56
Refused amount; not asked.....	..	1	2
Nonreporters.....	20	20	7	3	3	3
Reporters.....	42	42	46	46	46	46
Debts paid off.....	3	7	7	7
Nonrespondents.....	6	3	12	12
Noncontacts.....	1	1	3	1	4	4
Refusals.....	3	3	3	2	8	8
Prior dropouts.....	4	9
B. Holdings, six months						
Total sample.....	61	62 ^a	62	62	62	62
Respondents.....	55	49	46	46	46	46
Refused amount; not asked.....	..	1
Nonreporters.....	8	8	3	3	3	3
Reporters.....	47	39	39	39	39	39
Debts paid off.....	..	1	4	4	4	4
Nonrespondents.....	6	7	5	5	16	16
Noncontacts.....	..	2	3	3	3	3
Refusals.....	6	5	2	2	13	13
Prior dropouts.....	..	6	11	11

^a Increase in number of debts relative to previous wave denotes addition of one "new" debt at a validating institution.

TABLE 58. EFFECT OF PANEL OPERATION ON REPORTING OF VALIDATED DEBTS

Category	A. Holdings, three-months		B. Holdings, six-months	
	Wave 1	After five waves	Wave 1	After five waves
Reported ^a	73%	68%	77%	69%
Not reported.....	16	6	13	5
Nonrespondent.....	11	26	10	26
Total.....	100%	100%	100%	100%
Base.....	64	65	61	62

Category	C. Change, three-months		D. Change, six-months	
	Wave 1	After five waves	Wave 1	After five waves
Reported ^a	64%	79%	64%	78%
Not reported.....	32	3	30	4
Nonrespondent.....	4	18	6	18
Total.....	100%	100%	100%	100%
Base (total validated debts).....	69	70	67	68

^a Includes refusal of amount and debts paid off.

The overall conclusion from Table 57 would seem to be that the panel operation served to reduce the number of debts being covered, but at the same time it rendered more reliable those debts that were covered. This point is brought out more clearly in Table 58, which compares the distributions of validated debts on the first wave and on the last wave for each of the four experimental groups. To make the data more comparable, the figures have been converted into percentages, with the base being the total validated debts of that group at the particular time. As is evident from this table, the number of debts reported on the fifth wave was somewhat, although not very much, smaller than on the first wave for Groups A and B, from whom holdings were requested on the first two waves. By contrast, increases are apparent in the proportion and number of debts reported when changes were requested initially (Groups C and D).¹²⁵

¹²⁵ To some extent, this is due to the inclusion on Wave 5 of debts paid off as "reported" debts. However, the same phenomenon remains if paid off debts are excluded completely from Table 58. The distribution of total debts reported for these two groups then becomes:

	Wave 1	After five waves
Group C.....	64%	73%
Group D.....	64	75

TABLE 59. NONREPORTING, NONRESPONSE, AND DROPOUT RATES
FOR VALIDATED DEBTS, STUDY P3

Statistic	Subsample	Wave				
		1	2	3	4	5
Nonreporting rate ^a . . .	A. Holdings, 3 months	18%	10%	12%	10%	8%
	B. Holdings, 6 months	15	..	16	..	7
	C. Change, 3 months	33	14	10	9	4
	D. Change, 6 months	32	..	12	..	5
Nonresponse rate ^b . . .	A. Holdings, 3 months	11	8	8	2	0
	B. Holdings, 6 months	10	..	11	..	8
	C. Change, 3 months	4	10	3	1	0
	D. Change, 6 months	6	..	9	..	4
Dropout rate ^c	A. Holdings, 3 months	..	11	17	25	26
	B. Holdings, 6 months	10	..	18
	C. Change, 3 months	..	4	15	17	19
	D. Change, 6 months	6	..	13

^a Ratio of nonreported debts to validated debts of respondents.^b Ratio of nonrespondent validated debts to total sample validated debts.^c Ratio of number of debts of dropouts to total sample debts.

Table 58 also brings out the fact that the proportion of debts reported initially was higher among sample members asked for holdings than among those asked for change. The principal reason for this was the much higher proportion of debts not reported on the change form, a phenomenon associated with the tendency not to report holdings for which there was no change, noted in Chapter III. Somewhat surprisingly, however, by the fifth wave the proportion of debts reported was, if anything, higher among those who had been asked initially for changes than among those who had been asked initially for holdings. A principal explanation appears to lie in the higher dropout rate among those asked for holdings. Table 58 also suggests that nonreporting declined appreciably among all experimental groups as time went on, the decline being especially large for those groups where a change had been requested initially (and where, apparently as a result, nonreporting was much more substantial). This point is brought out more clearly in Table 59, which depicts the variations in the nonresponse rate and other rates over time.

In addition, the nonresponse rate was much higher among those asked for holdings of debt initially than among those asked for changes initially, a point also brought out in Table 59. As has been noted previously, a higher nonresponse rate appears to be characteristic of the holdings form, this form seemingly serving to "scare away" many borderline respondents.

Overall, these tables suggest that so far as the reporting of debt is concerned, the change approach is as effective as the holdings approach — after a few waves of interviews. Initially, however, the holdings ap-

proach shows a clear advantage with regard to the reporting of debt by respondents, but this is obtained at the expense of a higher nonresponse rate and of a higher dropout rate among people asked for holdings, as is clear from Table 59. With either approach, there is a marked decline in nonreporting over time as well as in the proportion of sample members not interviewed on a particular wave.¹²⁶

Dollar Amounts

Analysis of errors in reports of dollar amounts is more complicated than analysis of the number of holdings because of the experimental manipulations. On some waves, part of the sample was asked for dollar holdings and the rest of the sample for changes in debt. On other waves, the situation was reversed, as noted earlier. In addition, sample members asked for holdings on the third wave were also asked to report changes at the same time. Hence, a number of comparisons relating to the accuracy of dollar figures over time can be made, some relating to holdings and others relating to changes. The comparisons that are made relate to three different aspects of the panel operation: errors in debts reported or not reported by respondents, errors due to differences between the debts of the nonrespondents and the debts of the respondents, and errors due to possible biases because of dropouts.

Respondents. Errors in amounts based on figures furnished by respondents who reported validated debt are shown in Table 60. Perhaps the most striking characteristic of this table is the substantial differences in accuracy between the reports of holdings and the reports of change, differences which persist throughout the study. The average reported holdings of the debt of the sample members deviate less than 15 percent from the true figure (although the latter is consistently underestimated). By contrast, the average reported change in debt differs from the true figure by magnitudes ranging from 50 percent on up (with one exception). Moreover, in both instances no perceptible trend is evident toward greater accuracy over time. Thus, these results reinforce the findings of Chapter

¹²⁶ The reason for the decline in the nonresponse rate is that all of the nonrespondents on Wave 1, all but one of whom were refusals, had dropped out of the panel by the next wave.

Conversion of nonreporters to reporters was far more frequent among those asked initially for change than among those asked initially for holdings. However, the reason for the decline in nonreporting varied with the particular form that was used first. In the case of the holdings form, the decline was attributable primarily to some of these debts being paid off or to a sample member dropping out of the panel. In contrast, with the change form, the substantial initial nonreporting was reduced by more than two-thirds because many of these debts were picked up on later waves. The great majority of these pickups occurred on the following interview — Wave 2 for Group C, and Wave 3 for Group D.

TABLE 60. ERRORS OVER TIME IN VALIDATED DEBTS
OF RESPONDENT REPORTERS, STUDY P3
(PERCENTAGE OF DEVIATION IN AVERAGE REPORTED FROM
AVERAGE ACTUAL DEBT)

Wave	Holdings initially		Change initially	
	A. Three months	B. Six months	C. Three months	D. Six months
Holdings reports				
1.....	2.2	5.4
2.....	11.4
3.....	13.2	11.7
4.....
5.....	21.7	19.2	20.3	11.0
Change reports				
1.....	-109.2 ^a	20.0	976.6 ^b
2.....	-47.4
3.....	75.0	-547.0	53.6	10,666.7 ^c
4.....	135.2	222.6 ^d

^a Computed, rather than reported, change, based on difference between holdings reported on Wave 1 and on Wave 2.

^b Large percentage due to low base: average actual change, -\$33.27; average reported change, \$291.63.

^c Large percentage due to low base: average actual change, -\$5.07; average reported change, -\$545.87.

^d Large percentage due to low base: average actual change, -\$40.81; average reported change, \$50.05.

III regarding the much greater accuracy of reports of holdings than of reports of change.

It should be stressed, however, that this greater accuracy is in terms of *percentage* deviations, brought about by the very much higher base of the levels involved than of the changes. This point is illustrated by the errors in the reports of Group C on the third wave, a time at which both holdings and changes were requested from the same people. As is evident from Table 60, the average percentage of error in the reports of change was roughly four times that of the reports of holdings. Yet, when differences in scale are taken into account, the change reports appear in a much better light, as shown by the following data:

	Actual figure	Percentage of error	Average discrepancy
Holdings.....	\$6,860	13.2%	\$1,007
Change.....	143	53.6	77

In other words, the much smaller base from which changes are measured greatly magnifies any resulting discrepancies. Still, this is a phe-

nomenon that cannot be ignored, for percentage deviations are usually the most important measure of the accuracy of such estimates. In terms of these percentage deviations, the following additional inferences may be drawn from Table 60 regarding panel effects on the reporting of farm debt:

(1) Reports of change, but not of holdings, appear to be more accurate when obtained every three months than when obtained every six months. To be sure, the differences are not always substantial. In the case of change reports, however, pronounced differences are evident in all cases, and the differences are statistically significant on Wave 3. Such greater differences on change reports are not surprising, since one would expect the margin of possible error to rise in such cases, due to the increasing difficulty of selecting the base figure as the time span lengthens.

(2) Panel members from whom holdings are requested initially tend to be no more accurate in later waves than those from whom changes are requested initially.

(3) Reports of change are not much more accurate after people have been asked for holdings (compare Group A versus Group C and Group B versus Group D on Waves 3 and 4).

(4) In the one instance in which a three-month comparison was possible (Group A, Wave 2), average change in debt computed from holdings reports at different points in time was no more accurate than change reported directly. However, the true state of affairs may have been the opposite, because the computed changes were dominated by two extreme errors not present in the reported changes: in one instance the error was \$23,000, and in the other it was \$14,500. If these extreme observations are removed, the accuracy of reported change is as high as that on the holdings forms.¹²⁷

(5) The predominance of positive signs in errors of change reflects mixed tendencies. In three of these instances the actual change was positive, and in all cases it exceeded the reported change in absolute amount. In the other four cases average debt declined, with the absolute change greater than reported change in two instances but less than reported change in the other two cases. For the two negative errors, reported debt declined more than actual debt in both cases. All in all, reported absolute debt understated actual absolute debt almost half of the time — four times out of nine.

¹²⁷ The propriety of such an adjustment could be questioned on the grounds that in practice such extreme observations tend to arise anyway. However, the effect of one or two extreme observations on a large-scale study would clearly be far less than in the present case. Moreover, in these two instances some basis existed for questioning the comparability of the actual and the computed change figures.

When we expand the coverage of Table 60 to include respondent non-reporters as well as respondent reporters, essentially the same results are obtained. As before, no improvement in accuracy is evident over time; the tendency for holdings to be understated is more pronounced than before (as was to be expected), and three-month reports seem to be more accurate than six-month reports, especially on change. Also the data suggest that change reports may be more accurate after holdings data have been requested. This time, computed change unadjusted appears more accurate than reported change and, again, its accuracy approaches that of the holdings data (the error drops from 57 percent to 5.4 percent) if the two extreme errors are removed.

Roughly similar results are also obtained when the various comparisons are carried out in terms of average absolute discrepancies rather than average arithmetical discrepancies, so that errors of opposite sign do not cancel. Again, reports of three-month panel members are more accurate than those of six-month panel members only in the case of change, and there is no tendency for those asked for holdings initially to report changes later more accurately than those asked for change initially. The holdings reports are still substantially more accurate than the change reports and no perceptible trend toward greater accuracy over time is indicated.

Considering the fact that nonreporting declined substantially over time, particularly among those asked for change initially, it is somewhat surprising to find no improvement in accuracy of reports of the total respondents. The explanation seems to be twofold. First, no significant increase in accuracy of reporting occurred over time among the respondent reporters. From the start, those reporting holdings were about as accurate as might be expected, and the accuracy of the change reports failed to improve, at least in the aggregate. Second, the debts of the nonreporters averaged considerably below those of the reporters. Hence, the non-reporters were relatively much less important in terms of dollars than in terms of frequencies, as is also evident from Chapter III (Table 10). This was not always true of changes; except that in this case, the actual changes of the nonreporters bore little relationship to the average changes of the reporters, as is illustrated by the following data for Group C:

<i>Wave</i>	<i>Average changes of reporters</i>	<i>Actual changes of nonreporters</i>
1.....	\$ 148	\$-166
2.....	-460	- 81
3.....	143	-729
4.....	-41	110

Apparently the relatively few nonreporters, combined with the difficulty of reporting change, precluded any stability in this relationship, so

that at least for this purpose it made little difference whether nonreporters were picked up or not.

Nonrespondents. The foregoing provides the principal explanation for the failure of the debt reports to become more accurate over time, since Table 61 shows that errors due to differences between the debts of the nonrespondents and those of the respondents are of little consequence in an overall sense, both for holdings and for changes. This is not because no appreciable differences exist between these two sets of figures: columns 4 and 5 of the table suggest that the debts of the respondents may differ substantially from those of the nonrespondents, and that changes in debt were more often than not in different directions for the two groups. Rather, the primary reason is the relatively small number of nonrespondents, this group constituting not much more than one-tenth of the panel on all waves after the first, and in some cases containing no members at all. Hence, the effect of even substantial differences on sample estimates was greatly reduced, as is evident from columns 6 and 7 of this table.

Moreover, from the point of view of survey errors, the important thing is not the difference between nonrespondent and respondent *actual figures* but between the nonrespondent and respondent *reports*, for it is the latter data that comprise the sample estimate. Comparison of columns 3 to 5 of Table 61 suggests that as often as not the errors in the respondent reports help mitigate the errors in the overall survey estimate, because these errors serve to bring the reported figures closer to the nonrespondent holdings than are the actual debt figures for the respondents. This is true of 3 out of 7 such comparisons for the holdings reports and for 5 out of 9 comparisons for the change reports. It is to a large extent the instances in which this is not true that the nonrespondent effect is substantial, as for Group C on Wave 1 and for Group D on Wave 3.¹²⁸

Dropouts. Finally, we may ask if panel mortality, small as it was, exerted any cumulative effect in causing the panel to become less representative over time of the population from which it was selected, at least with regard to debt. For present purposes, this question reduces to investigating the extent to which the holdings, and changes, of the dropouts deviated from the corresponding statistics for the nonrespondents.

The relevant data are shown in Figure 3, comparing the average amount of debt of the respondents and that of the nonrespondents by wave. Separate lines are shown for the average debt of panel members dropping out at different times. The dollar amounts in this figure relate

¹²⁸ The other principal reason is a high frequency of nonrespondents combined with a substantial difference in the reported averages and nonrespondent holdings, a phenomenon most likely to occur on the first wave, as was the case for Groups A and B on Wave 1.

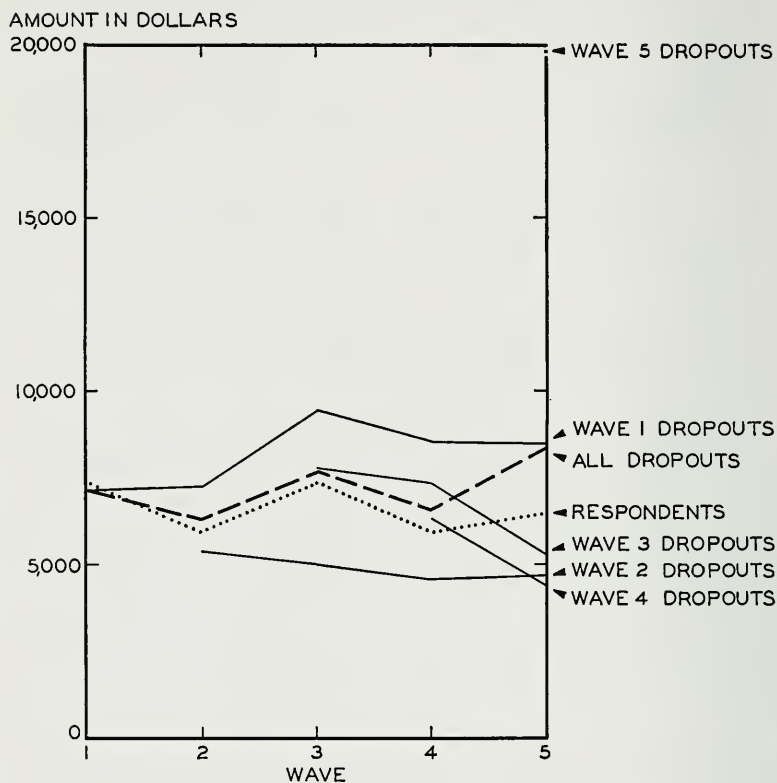
TABLE 61. COMPARISON OF DEBT OF RESPONDENTS AND NONRESPONDENTS,
BY GROUP AND BY WAVE, STUDY P3

Group (1)	Wave (2)	Average dollar amount for			Percentage of error in estimate for	
		Respondent reports (3)	Respondent actuals (4)	Nonre- spondents (5)	Respon- dents (6)	Total sample ^a (7)
Holdings reports						
A. Three months (holdings first).....	1	6,004	6,640	7,126	9.6	14.1
	2	4,693	5,792	9,231	19.0	26.8
	5	3,693	5,112 ^b	27.8	29.4
B. Six months (holdings first).....	1	7,849	9,407	3,098	16.6	10.7
	5	7,543	9,488	20,282	20.5	28.3
C. Three months (change first).....	3	5,341	6,347 ^b	15.9	13.0
	5	4,786	6,015 ^b	20.4	21.4
D. Six months (change first).....	3	6,889	7,803	4,131	24.9	21.3
	5	5,434	6,107	17,434	12.2	19.5
Change reports						
A. Three months (holdings first).....	3	148	697	1,186	78.7	80.0
	4	176	-415	1,242	142.4	146.2
B. Six months (holdings first).....	3	-376	-218	-345	-72.3	-60.6
C. Three months (change first).....	1	119	149	-1,222	-81.3	796.3
	2	-586	-555	-258	-5.5	-11.9
	3	60	25	-141.4	-149.6
	4	36	-28	231.0	233.4
D. Six months (change first).....	1	-33	292	1,072.4	1,134.0
	3	-5	-546	1,149	-331.4	6,846.3

^a Excluding prior dropouts.

^b None in category.

FIGURE 3. AVERAGE DEBT OF RESPONDENTS AND OF DROPOUTS, BY WAVE, STUDY P3



to the validated debts of all panel members (since the relatively small number of nonrespondents does not make it feasible to compare experimental groups) and for holdings rather than for changes in debt, because bias would seem most likely to occur in the former case.

To judge by Figure 3, differences between the holdings of the respondents and those of the nonrespondents were relatively minor for most of the study, although the differences tended to widen toward the end of the operation. For the first four waves, the average for one group is within 10 percent of the average debt of the other group. However, throughout the study some tendency is apparent for those dropping out to have more debt than those remaining, a tendency that becomes most pronounced on the last wave.

The comparison is also good in terms of change in debt. If change is estimated as computed change based on the difference between average

TABLE 62. ALLOCATION OF ERROR IN AVERAGE DEBT,
BY SAMPLE CATEGORIES, STUDY P3

Wave	Group	Per- cent- age of error	Allocation (percent)					Total
			Non- re- port- ing	Re- spon- dent re- ports	Amount refused or NA	Cur- rent non- re- spon- dents	Prior drop- outs	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1.	A. Three months	10.3	64.4	17.8	..	17.8	100.0
	B. Six months	10.7	106.8	43.1	..	-49.9	100.0
2.	A. Three months	23.0	28.8	34.9	..	25.3	11.0	100.0
3.	C. Three months	12.4	21.6	88.9	..	-20.2	9.7	100.0
	D. Six months	23.9	54.1	34.2	1.5	-8.9	19.1	100.0
5.	A. Three months	36.5	13.8	35.5	50.7	100.0
	B. Six months	22.3	5.1	61.2	..	48.3	-14.6	100.0
	C. Three months	16.4	1.2	108.5	-9.7	100.0
	D. Six months	19.1	5.3	39.2	..	44.6	10.9	100.0

holdings on two successive waves, the figures coincide very well until Wave 5. At that point the high average debt of the nonrespondents produces appreciable differences in the change in debt of this group relative to that of the respondents for the last two comparisons.

Similar results are obtained if the comparison is made in terms of reported change. In that case, however, the sample is reduced to only those interviewed with change forms. The number of nonrespondents is then very small, so that sampling variability is greatly increased.¹²⁹ Nevertheless, in all but one comparison (on Wave 5) the changes were in the same direction.

Allocation of error. The relative importance of various sources of error in reporting debt are evaluated in relationship to each other in Table 62, using the same basic allocation principle developed in previous chapters. To indicate the magnitude of the error involved, column 3 shows the relative error in the overall estimate of the average debt of the particular group at that time.

This table brings out the gradual shift in the principal cause of error from nonreporting on the first wave to errors in the reports of the respondents and to errors due to dropouts on the later waves. From the previous results, it is clear that this shift is attributable not so much to fluctuations in the average amounts involved, but rather to shifts in the proportion of

¹²⁹ This is especially true because half of the sample was not interviewed on Waves 2 and 4, which affects the variability of change estimates on all waves.

sample members in each group at a particular time. Thus, on the first wave nonreporting is at a peak (both absolutely and relatively), whereas nonrespondents are at a minimum and dropouts are nonexistent. On later waves, the frequency of nonreporting declines and the proportion of sample members who have dropped out rises gradually. While this shift is taking place, the relative influence of reporting error tends to increase, as some nonreporters shift into the reporter category.

The table also documents the tendency — apparent also from Figure 3 — for the sample averages to understate consistently the true average debt. This is primarily due to the nonreporters and to consistent understatements by the reporters.

Time Deposits, Study P2

Because of differences in the experimental design and in the type of information obtained, it seems best to consider separately the panel effects on the reporting of savings accounts for the urban panel, Study P2, and for the farm panel, Study P3. The former will be considered first and then the latter.

It will be recalled (pp. 75-88) that four experiments were incorporated in the first wave of interviews in this study, relating to (1) the type of advance letter, (2) the use of questionnaire forms in the interview, (3) the type of data requested (holdings versus change), and (4) the extent of coverage of assets and debts. For the present purposes, only the distinction by type of data requested is of primary relevance, since the results on Wave 1 for the other three experiments (pp. 114-16) do not indicate that any of those variables had any significant effect on reporting of accounts. Since these experiments were of a one-time nature, panel effects due to these variables were hardly likely, as was verified by casual examination. Hence, for studying panel effects, the data-collection pattern over time divides the sample in two, as follows:

<i>Wave</i>	<i>Group A</i>	<i>Group B</i>
1.	Holdings	Change
2.	Holdings	Change
3.	Holdings ¹³⁰	Holdings
4.	Holdings and change	Holdings and change
5.	Holdings	Holdings

Holdings Reported

The substantial panel mortality on this study, first noted on p. 162, is brought out once again in Table 63. With reference to time deposits,

¹³⁰ On Wave 3, members of Group 3 were asked for holdings of demand and time deposits and of debt — assets and debts with fluctuating balances — and for change on other assets.

TABLE 63. DISTRIBUTION OF VALIDATED TIME DEPOSITS,
BY SAMPLE RESPONSE STATUS, STUDY P2

Response status	Wave					After five waves
	1	2	3	4 ^a	5	
A. Holdings first						
Total sample.....	240	243	245	246	246	246
Respondents.....	169	153	150	141	139	139
Refused amount, not asked....	8	9	7	21	8	8
Nonreporters.....	56	40	32	31	28	28
Reporters.....	105	104	111	91	103	103
Nonrespondents.....	71	31	11	17	6	107
Noncontacts ^b	22	9	8	3	4	26
Refusals.....	49	22	3	14	2	81
Prior dropouts ^c	59	84	86	101	...
B. Change first						
Total sample.....	222	224	227	227	227	227
Respondents.....	176	149	135	128	124	124
Refused amount, not answered	8	5	14	16	9	9
Nonreporters.....	71	47	28	21	15	15
Reporters.....	97	97	93	91	100	100
Nonrespondents.....	46	29	22	29	7	103
Noncontacts ^c	10	6	10	3	2	25
Refusals.....	34	23	12	26	5	78
Prior dropouts ^d	2	46	70	70	96	...

^a Roughly half of the sample was interviewed by mail on this wave.^b Includes accounts on 12 falsified questionnaires on Wave 1 and four falsified questionnaires on Wave 2.^c Includes two falsified questionnaires on Wave 1.^d Includes ineligible.

refusal to grant an interview was by far the most important cause of lost validated accounts, accounting after five waves for three-fourths of all such accounts and for one-third of all validated accounts. It is interesting to note that although more refusals were encountered initially on the holdings form than on the change form, the number of such refusals diminished much less rapidly for the latter group. This was particularly true of the third and fourth waves, when efforts were made for the first time to obtain holdings information from this group. As a result, by the end of the operation, the rate of refusal was higher among those asked initially for change than among those asked initially for holdings.

Among those who remained in the panel, much the same phenomena are apparent for time deposits as for debt on Study P3. Nonreporting of time deposits dropped through most of the study, particularly on the second and third waves. At the same time, the rate of nonreporting dropped more sharply among those asked initially for change — especially so on Wave 3 when balances were requested of the members of this group —

with the result that by the end of the field operation nonreporting for this group was lower than for those asked initially for holdings, as shown by the following percentages:

<i>Wave</i>	<i>Nonreporters as percentage of all respondents</i>		
	<i>Holdings first</i>	<i>Change first</i>	<i>Total sample</i>
1.....	23%	32%	27%
2.....	16	21	19
3.....	13	12	13
4.....	13	9	11
5.....	11	7	9

This phenomenon offset the loss of sample members during the course of the panel, so that the coverage of validated accounts actually increased despite the substantial mortality (almost 45 percent).

Also, similar to the results for debt on Study P3 are the rapid declines in the nonresponse rate (nonrespondent accounts/total sample accounts) and in the dropout rate (dropout accounts/total sample accounts on previous interview).¹³¹

A similar result is also obtained for the nature of the shift of panel members from one form of response to another, based on relating a sample member's status on Wave 1 to his status on Wave 5. The decline in nonreporters among those from whom changes were requested originally reflected primarily improved accuracy of reporting, whereas among those asked first for holdings the decline resulted principally from dropouts. In other words, the latter were basically less cooperative from the beginning and hence more likely to refuse at a later stage.

Dollar Amounts

Respondents. The pattern of errors in dollar amounts for time deposits is in many ways similar to that for debt, as is evident from a comparison of Table 64 with Table 60. The average error in balances reported by respondents is virtually negligible from the start and, not surprisingly, registers no improvement over time. However, the sample estimate of the average balance of all respondents is much more in error, understating the true figure on the first two waves by almost 40 percent. This understatement is compounded primarily by two factors: the much higher balances of nonreporters compared with reporters, and the roughly equally higher balances of those refusing to give figures (whose balances were then

¹³¹ The only exception, a rise in dropouts after the fourth wave, was the result of a decision not to interview further 25 savings units which had been consistently uncooperative all along and had provided virtually no financial data, although several had reported the validated account.

TABLE 64. PERCENTAGE OF ERRORS OVER TIME IN VALIDATED ACCOUNTS, STUDY P2

Wave	Respondent reporters		All respondents	
	A. Holdings first	B. Change first	A. Holdings first	B. Change first
Holdings reports				
1.....	2.3	39.5
2.....	-1.6	39.7
3.....	-6.9	8.2	27.1	19.8
4.....	-5.3	10.0	17.7	37.1
5.....	-11.5	-1.1	21.3	9.4
Change reports				
1.....	109.1	96.8
2.....	40,762.5 ^a	226.6 ^b
3.....
4.....	121.8	121.3	162.7	106.6

^a Large percentage error due to low base; actual average change was —\$.08, reported average change, \$32.53.

^b Large percentage error due to low base; actual average change was —\$17.30, reported average change, \$21.91.

estimated as equal to the average balance of the reporters). The following data illustrate the magnitude of these factors for Group A:

Wave	Average actual balance		
	Balance given	Balance refused	Nonreporters
1.....	\$2,446	\$3,102	\$2,949
2.....	1,944	3,106	3,078
3.....	1,938	4,162	3,141
4.....	2,062	1,650	2,439
5.....	1,985	2,747	2,920

Only on the fourth wave is no substantial underestimation bias present due to the nonreporters or to the balance refusals. In addition, as was noted in the preceding section, nonreporting was much more frequent on the first two waves than it was later, which served to depress the overall sample estimate still further. The decline in nonreporting after the second wave was primarily responsible for the pronounced improvement in the sample estimates of all respondent balances on later waves, for both groups. In this respect, the panel effect was beneficial.

For the change reports, the results on time deposits also parallel those on debt. The average errors are substantial — far above the errors in the reports of balances. Yet, as before, the dollar amounts involved are very

small, and in these terms the discrepancies are not large. Wave 4 is a case in point, as is shown by the following figures relating to all respondents:

<i>Group</i>	<i>Holdings</i>		<i>Change</i>	
	<i>Amount</i>	<i>Discrepancy (actual-reported)</i>	<i>Amount</i>	<i>Discrepancy (actual-reported)</i>
A.....	\$2,083	\$368	\$-7	\$-11
B.....	2,432	908	180	191

The relatively small average change figures may also supply much of the explanation for the failure of the actual and reported figures to move in the same direction. Using plus to represent an increase and minus to represent a decline, the picture for reporters — and essentially the same for all respondents — was as follows:

<i>Group</i>	<i>Wave</i>	<i>Actual change</i>	<i>Reported change</i>
A.....	4	—	+
B.....	1	+	—
	2	—	+
	4	+	—

In all four instances, the reported and actual directions of change did not coincide. However, in all instances the actual average changes were very small, the figures being \$-25, \$56, \$-0.08, and \$68, respectively. With sample sizes of less than 100 in each case, the observed differences are within the range of sampling variation.

Contrary to the situation with holdings, no tendency is apparent in Table 64 for the accuracy of the change reports to improve with time, nor do reports of change appear to be any more accurate after panel members had been reporting holdings for some time (compare the errors in the change reports for Groups A and B on Wave 4). On the other hand, for the two instances where accuracy of computed change (the difference between successive holdings reports) could be compared with the accuracy of reported change, on Wave 4 for Groups A and B, the former approach appears to be clearly superior, as is evident from the following data for reporters:

<i>Group</i>	<i>Computed change</i>			<i>Percentage of error in reported change</i>
	<i>Actual amount</i>	<i>Estimated amount</i>	<i>Percentage of error</i>	
A.....	\$ 134	\$ 0	100.0%	116.1%
B.....	-486	-483	— .6	121.3
Both (weighted).....	-166	-248	-49.4	121.7

Two different "actual" changes are involved in this comparison because some panel members on Wave 4 reported change but not holdings, and a few vice versa.

TABLE 65. COMPARISON OF TIME DEPOSITS OF RESPONDENTS AND NONRESPONDENTS, BY GROUP AND BY WAVE, STUDY P2

Group	Wave	Average dollar amount for			Percentage of error in estimate for	
		Respondent reports	Respondent actuals	Non-respondents	Re-spondents	Total sample ^a
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Holdings						
A.	1	2,391	2,446	3,924	2.3	47.1
	2	1,975	1,944	4,645	-1.6	48.8
	3	2,172	1,938	1,419	-6.9	24.8
	4	2,172	2,062	3,150	-5.3	22.7
	5	2,213	1,985	2,181	-11.5	21.3
B.	3	2,331	2,538	3,997	8.2	19.8
	4	1,848	2,052	3,836	10.0	37.3
	5	2,315	2,289	8,518	-1.1	9.4
Change						
A.	4	4	-7	313	121.8	87.6
B.	1	-5	56	-86	109.1	96.8
	2	33	.. ^b	-68	40762.5	226.6
	4	-15	68	386	121.3	106.6

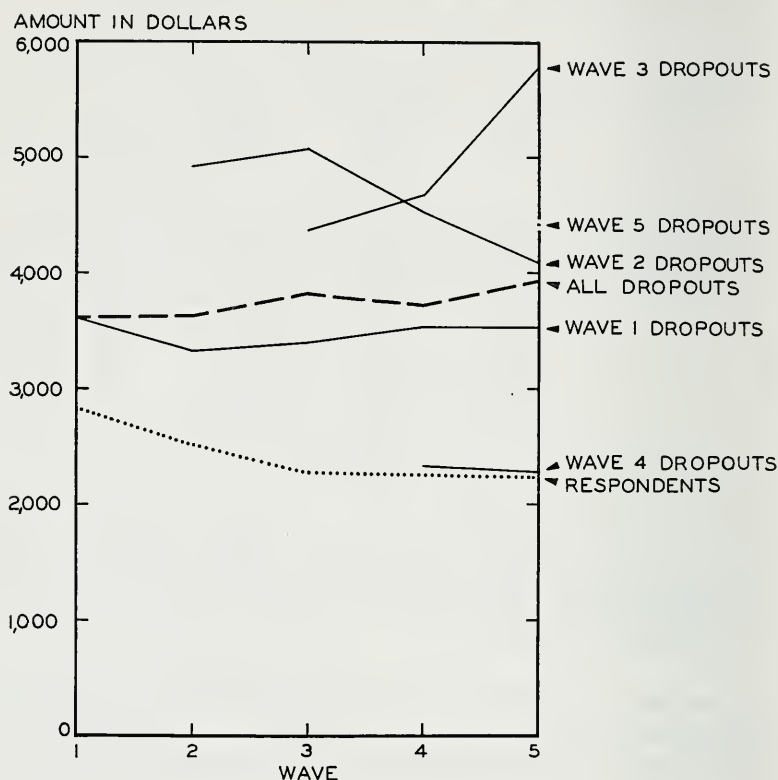
^a Excluding prior dropouts.^b Less than \$.50.

Nonrespondents. Differences are apparent between average holdings, and changes, of the nonrespondents and those of the respondents (Table 65). Balances of nonrespondents averaged considerably above those of the respondents and, perhaps because of the foregoing, changes in balances were much more pronounced. Since respondent reports tended to understate the true holdings, and changes, of respondents anyway (because of sizeable nonreporting), the result of this upward bias in the holdings of the nonrespondents is to increase the error in the sample estimate of the average balance of all panel members. On later waves this tendency was mitigated by the sharp decline in the number of nonrespondents.

The errors in the estimates of change are reduced as a result of adding the figures for the nonrespondents. However, this is the result of an arithmetical quirk stemming from the increase in the base for the calculation of the percentage errors as a result of combining the actual changes for the respondents with the much higher figures for the nonrespondents.

Dropouts. The average account sizes of the dropouts were consistently above those of the respondents (Figure 4). Thus, on Wave 1 the average balance in the validated accounts of the respondents was \$2,842, whereas

FIGURE 4. AVERAGE SIZE OF TIME DEPOSIT ACCOUNTS OF RESPONDENTS AND OF DROPOUTS, BY WAVE, STUDY P2



that of the dropouts was almost 30 percent larger, \$3,624. This margin widened gradually during later waves of the panel operation. As a result, by the last wave, the average balance of the respondents had declined to \$2,255, whereas that of the dropouts had risen to \$3,922.¹³² Although some discrepancies are evident in the wave-to-wave changes in the average balances of the two groups, the overall changes are similar: holdings of respondents in these accounts dropped by 20 percent, whereas those of nonrespondents rose by 8 percent.

As a result of these diverse movements, little correspondence is apparent between the saving of respondents and the saving of dropouts in these accounts. It might be noted that the pattern of changes in the holdings of the dropouts is influenced primarily by that of those dropping out

¹³² This decline does not necessarily suggest that the savings accounts of these panel members declined over time, for the comparison is restricted to the original validated accounts. Excluded are all nonvalidated accounts as well as new accounts.

TABLE 66. PERCENTAGE ALLOCATION OF ERROR IN AVERAGE TIME DEPOSIT BALANCES, BY SOURCE OF ERROR AND BY WAVE, STUDY P2

Allocation	A. Holdings first: Wave					B. Change first: Wave		
	1	2	3	4	5	3	4	5
Nonreporting.....	48.7	34.0	32.5	24.5	28.9	16.5	18.0	7.6
Errors in balances reported.....	1.7	-.7	-7.7	-3.1	-7.9	7.6	5.1	-.9
Errors in estimated balances of those reporting account but refusing balance.....	1.6	2.9	4.3	-3.6	1.5	.5	9.9	2.4
Errors in estimated balance of current nonrespondents.....	48.0	29.1	-1.1	9.4	.9	21.1	20.4	17.0
Errors in estimated balances of prior dropouts.....	34.7	72.0	72.8	76.6	54.3	46.6	73.9
Total.....	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Overall percentage of error.....	47.1	51.9	42.8	40.9	38.9	36.9	49.9	37.5

on the first wave. From a statistical point of view this is only to be expected, because the latter group represents more than half of all dropouts.

Allocation of error. As was found in the case of farm debt, nonreporting is seen to be the principal source of error at the beginning of the operation, accounting for nearly half of the total error on the first wave (Table 66). Over time, the relative importance of this source tends to diminish, principally because of the declining relative frequency of nonreporters. In contrast, errors in the estimated balances of nonrespondents and of dropouts increase in importance over time, as would be expected. By the last two waves, these sources account for 75 to 90 percent of the overall percentage of error in the average balance. This increase is attributable both to the rising proportion of the original panel in these categories and to the growing extent to which the reported balances understate those of the dropouts, as is supported by the following data:

Wave	Percentage of discrepancy between average balance of nonrespondents plus dropouts, and reported balance of all respondents	Panel nonrespondents and dropouts as percentage of original sample size
1.....	22%	25%
2.....	31	36
3.....	40	41
4.....	39	44
5.....	42	46

Compared with these large sources of error, errors in the balances of accounts reported by respondents are virtually negligible, as was true also of errors in the use of respondent reports to estimate balances of accounts for which figures were refused.

Time Deposits, Study P3

The validation of savings accounts on the farm panel followed the same experimental design used on farm debt, as described in pp. 181-94. It is perhaps needless to note, however, that different respondents were involved in the two validations.

Holdings Reported

The distribution of validated accounts over time by sample member status is shown in Table 67. As with the other validation analyses, panel mortality was fairly high, about 30 percent of the validated accounts being owned by panel members who had dropped out by the last wave of interviews. However, roughly half of these dropouts occurred on the first interview, before the sample members knew of the panel aspects, so that the panel feature accounts for a loss of only about 15 percent of the original sample.

As was true in previous instances, most of this loss is represented by refusals. Noncontacts were few, particularly since four of the nine noncontacts listed in the table represented people who had either moved out of the area or who had given up farming and were no longer eligible for interview. Comparing the different experimental groups, a much higher mortality is seen to occur among those asked first for holdings, as shown by the following tabulation:

<i>Wave</i>	<i>Refusals as a percentage of total sample</i>			
	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>
1.....	23%	19%	13%	10%
3.....	20	27	21	10
5.....	35	35	25	10

Since panel members originally interviewed with a change form were switched to a holdings form on Wave 3, the refusal rate for these groups (C and D) would be expected to rise at that time. This is seen to be true of Group C, but with Group D the refusal rate was not only initially very low but no additional refusals were encountered on later waves.

Table 67 also reaffirms the much higher incidence of nonreporting among those asked for changes than among those asked for holdings. This is brought out more clearly in the following tabulation of nonreporters as a percentage of total respondents, for each experimental group on Waves 1, 3, and 5.

TABLE 67. DISTRIBUTION OF VALIDATED TIME DEPOSIT ACCOUNTS,
BY SAMPLE MEMBER STATUS AND BY WAVE, STUDY P3

Category	Wave					Wave				
	A. Holdings, three months					B. Holdings, six months				
	1	2	3	4	5	1	3	5	After five waves	
Total sample.....	22	24	25	25	25	26	26	27	27	
Respondents.....	17	19	19	17	16	21	17	17	17	
Refused amount; not asked.....	2	2	1	..	1	
Nonreporters.....	5	4	5	4	2	5	4	2	2	
Reporters.....	10	13	13	13	13	16	13	15	15	
Nonrespondents.....	5	..	1	2	1	5	4 ^a	2	10	
Noncontacts.....	1	..	1	..	2 ^a	..	1	
Refusals.....	5	2	..	5	2	2	9	
Prior dropouts.....	..	5	5	6	8	..	5	8	..	
New accounts.....	..	2	1	1	..	
C. Changes, three months										
Total sample.....	23	23	24	24	24	29	30	30	30	
Respondents.....	19	18	18	17	17	26	26	23	23	
Refused amount; not asked.....	1	..	1	..	1	
Nonreporters.....	10	9	7	6	6	9	5	2	2	
Reporters.....	9	9	10	11	10	17	20	21	21	
Nonrespondents.....	4	1	1	1	..	3	1	3	7	
Noncontacts.....	1	1	3	4	
Refusals.....	3	1	1	1	6	3	3	
Prior dropouts.....	..	4	5	6	7	..	3	4	..	
New accounts.....	1	1	
D. Changes, six months										

^a One noncontact on Wave 3 was interviewed on Wave 5.

<i>Wave</i>	<i>Nonreporters as a percentage of respondents</i>			
	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>
1.....	29%	24%	53%	35%
3.....	24	13	39	19
5.....	13	12	35	9

Contrary to the findings of Study P2, the nonreporting rate, although declining for all groups over time, remains appreciably lower for the two groups (A and B) asked originally for holdings, except for that of Group D on Wave 5. It is somewhat surprising to note that the largest decline in nonreporting occurs between Waves 1 and 5 for the people interviewed every six months. The data in Table 67 also support earlier findings that nonreporters among members asked for holdings were most likely to drop out of the panel on a later wave, whereas nonreporters asked for change tended to stay in the sample, with an appreciable number reporting the account on a later interview.

Dollar Amounts

Respondents. Table 68 shows that the balances reported are reasonably accurate; only for Group A on the first wave is there any substantial error.¹³³ Nonreporting and the relatively large size of accounts for which the balance was refused leads to consistent understatement of the average balance of all respondents, but the accuracy of reports clearly improves over time, as it did in Study P2. The following tabulation, which combines Groups A and B to reduce sampling variation, shows that the actual balance in the accounts that were reported averages considerably below the balances in accounts not reported or in accounts for which the balance was refused:

<i>Groups</i>	<i>Wave</i>	<i>Average actual balance</i>		
		<i>Given</i>	<i>Refused</i>	<i>Not reported</i>
A and B.....	1	\$1,329	\$6,588	\$2,327
	5	1,629	6,515	3,545
C and D.....	3	1,657	5,032	1,123
	5	1,942	4,260	1,677

This tabulation also suggests that the error reduction over time in the balances of all respondents is due primarily to the decline in nonreporters rather than to any closer correspondence between the size of accounts reported and the size of accounts not reported.

As before, reports of change are considerably more in error percentage-wise than reports of holdings. (The strikingly high accuracy of the

¹³³ The large overstatement on Wave 5 is attributable almost entirely to one large discrepancy in which the institution reported a balance of roughly \$100 and the respondent reported a balance of \$12,000.

TABLE 68. PERCENTAGE OF ERRORS OVER TIME IN VALIDATED TIME ACCOUNTS, STUDY P3

Wave	Holdings initially		Change initially	
	A. Three months	B. Six months	C. Three months	D. Six months
Respondent reporters: holdings				
1.....	47.3	4.1
2.....	9.3
3.....	-6.6	-4.9
4.....
5.....	-88.0 ^a	13.9	-.2	-3.1
Respondent reporters: change				
1.....	-10.7	150.6 ^b
2.....	237.7
3.....	125.0	-70.4
4.....	95.3	104.7
All respondents: holdings				
1.....	65.2	57.8
2.....	28.0
3.....	29.2	15.0
4.....
5.....	-29.9 ^c	38.5	21.3	8.6
All respondents: change				
1.....	-.5	218.4 ^d
2.....	106.1
3.....	129.3	-70.3
4.....	94.4	104.7

^a If one extreme discrepancy, of almost -\$12,000, were removed, the error would be -9.5 percent.

^b Large percentage due to low base: average actual change, -\$10.09; average reported change, -\$25.29.

^c If one extreme discrepancy, of almost -\$12,000, were removed, the error would be 23.2 percent.

^d Large percentage due to low base: average actual change, \$13.97; average reported change, -\$16.54.

average change report on the first wave for Group C appears to be somewhat of a fluke, resulting from fairly close correspondence between the reported and actual changes of the reporters and from the minimization of the change in the accounts of the nonreporters because of the very small amounts of change involved.)

The errors in the average change in balances tends to exceed by far the errors in average balances themselves. Contrary to the findings of Study P2, many of the average changes obtained in this study were not small, some amounting to several hundred dollars. Nevertheless, as shown

TABLE 69. AVERAGE BALANCES AND CHANGES IN BALANCES IN TIME DEPOSITS OF RESPONDENTS AND OF NONRESPONDENTS, STUDY P3

Group (1)	Wave (2)	Average dollar amount for			Percentage of error in estimate for	
		Respondent reports (3)	Respondent actuals (4)	Non-respondents (5)	Respon- dents (6)	Total sample (7)
Holdings						
A and B.....	1	743	1,919	2,865	61.3	64.7
	5	1,963	1,629	5,195	−20.1	5.5
C and D.....	3	1,285	1,665	1,000	22.8	20.4
	5	1,617	1,947	8,555	16.9	37.1
Change						
A and B.....	3	−83	600	24	113.8	113.8
C and D.....	1	42	59	25	28.8	10.4

by the following tabulation for reporters, the direction of change was frequently incorrect:

<i>Group</i>	<i>Wave</i>	<i>Actual change</i>	<i>Reported change</i>
A.	3	\$1,668	\$-417
	4	118	6
B.	3	129	220
C.	1	227	251
	2	-46	64
	4	289	-14
D.	1	-10	-25

Once again, computed change makes a better showing than reported change, although only one comparison is possible. The result of this comparison, based on computed change for Group A between Wave 1 and Wave 2 versus reported change for the same period for Group C, is as follows:

Computed average change, Group A:

Actual.	\$1,062
Estimated.	\$1,716
Percentage of error.	61.6%

Reported average change, Group C:

Percentage of error.	237.7%
---------------------------	--------

Nonrespondents. Holdings of the nonrespondents were again higher than those of the respondents, to judge by Table 69. Changes in the holdings of the nonrespondents were appreciably less, in absolute terms,

than changes in the holdings of respondents, contrary to findings in Study P2. However, the base for many of these averages is very small — generally between three and five observations for the nonrespondents — and therefore not much weight can be placed on these comparisons.

Dropouts. In contrast to Study P2, Study P3 showed little correspondence between the holdings of the panel members and those of the dropouts over time. The holdings of the nonrespondents and of the respondents were roughly similar on Wave 1, but the pattern of change differed thereafter. Respondent average balances rose to Wave 3 and declined from Wave 3 to Wave 5, whereas dropout balances, dominated by those of Wave 1 nonrespondents, followed an opposite course. However, taking the period as a whole, the balances of both groups rose noticeably, although much more on the part of the balances of nonrespondents.

Allocation of error. Nonreporting was the principal initial source of error but declined in importance on later waves as the number of non-reporters declined and the number of dropouts increased. Errors in respondent reports of the balances provided another major source of error for Group A; however, this source of error was minor for the other three groups. Especially significant is the fact that, as in Study P2, the accuracy of the panel data once more is seen to increase over time.

Demand Deposits

The initial sample for validation of demand deposits in Study P3 was smaller than that for the other assets. Also, due to difficulties in obtaining balances on the initial wave, the sample size for analysis of errors in the dollar figures on Wave 1 was considerably smaller than the sample size serving as the basis for analysis for reporting and nonreporting of accounts. It might also be noted that after the first wave, when the greater accuracy of the reporting of these accounts became apparent, more emphasis was placed on asking for holdings. As a result, changes were sought on later waves only on Wave 4.

Holdings Reported

The time pattern of the distribution of 77 validated demand deposits, shown in Table 70, presents a more favorable picture than either time deposits or debt for the same study. Panel mortality on demand deposits was slightly lower than that for the other holdings. About 15 percent of those asked for holdings on the first wave dropped out, as was true of a much smaller proportion (5 percent) of those asked for changes. By the end of the operation, panel mortality amounted to roughly one-third of those asked initially for holdings, but it was only about half as much for those asked initially for changes.

TABLE 70. DISTRIBUTION OF VALIDATED DEMAND DEPOSIT ACCOUNTS,
BY SAMPLE MEMBER STATUS AND BY WAVE, STUDY P3

Category	Wave					After five waves
	A. Holdings, three months					
	1	2	3	4	5	
Total sample.....	18	18	18	18	18	18
Respondents.....	15	14	13	12	12	12
Refused amount; not asked.....
Nonreporters.....
Reporters.....	15	14	13	12	12	12
Nonrespondents.....	3	1	1	1	..	6
Noncontacts.....	1	1
Refusals.....	3	1	..	1	..	5
Prior dropouts.....	..	3	4	5	6	..
New accounts.....
Category	C. Changes, three months					D. Changes, six months
	A. Holdings, three months					
	14	13	14	14	14	
Total sample.....	14	13	14	14	14	14
Respondents.....	13	13	14	12	12	12
Refused amount; not asked.....	1	1	1	1	2	2
Nonreporters.....	1	1	1
Reporters.....	11	11	11	10	10	10
Nonrespondents.....	1	1	..	2
Noncontacts.....
Refusals.....	1	1	..	2
Prior dropouts.....	..	1	1	1	2	..
New accounts.....
Category	Wave					After five waves
	B. Holdings, six months					
	1	3	5	
Total sample.....	21	21	21	21	21	21
Respondents.....	18	16	16	16	16	16
Refused amount; not asked.....	1	1
Nonreporters.....	..	2	1
Reporters.....	15	14	16	16	16	16
Nonrespondents.....	3	2	5
Noncontacts.....	..	1	1
Refusals.....	3	1	4
Prior dropouts.....	..	3	3	5	5	..
New accounts.....
Category	Wave					After five waves
	D. Changes, six months					
	24	24	24	24	24	
Total sample.....	24	24	24	24	24	24
Respondents.....	23	21	21	19	19	19
Refused amount; not asked.....
Nonreporters.....	2	1	1	1	1	1
Reporters.....	21	20	20	18	18	18
Nonrespondents.....	1	2	2	2	2	5
Noncontacts.....	..	1	1	1	1	3
Refusals.....	1	1	1	2	2	2
Prior dropouts.....
New accounts.....

TABLE 71. PERCENTAGE OF ERRORS OVER TIME IN VALIDATED DEPOSIT ACCOUNTS, STUDY P3

Wave	Holdings initially		Change initially	
	A. Three months	B. Six months	C. Three months	D. Six months
Respondent reporters: holdings				
1.....	5.9	-65.6
2.....	4.7	16.8
3.....	14.4	-.9	-3.9	.3
4.....
5.....	24.4	35.0	16.1	22.3
Respondent reporters: change				
1.....	-25.2	74.7
2.....
3.....
4.....	131.8	49.0
All respondents: holdings				
1.....	7.1	-56.2
2.....	.5	52.0
3.....	14.4	-6.9	58.8	13.3
4.....
5.....	24.4	35.0	53.4	31.9
All respondents: change				
1.....	136.3	76.9
2.....
3.....
4.....	131.8	56.5

In addition, it is significant to note that, as before, most of the dropouts among those asked initially for holdings represented refusals, whereas nearly half of the dropouts from the other groups were noncontacts.

Nonreporting of demand deposits was appreciably less than that for the other holdings studied, even on the first wave and even for those asked initially for changes. Of this latter group, only three out of 36 accounts were not reported on the first wave, the number dwindling to one by the last wave. For those asked initially for holdings, two out of 33 accounts were not reported, but by the last wave all of the validated accounts of the respondents were being reported.

Dollar Amounts

Respondents. The magnitude of error in reports of checking accounts seems to be slightly less than for savings accounts, to judge from Table 71.

As with savings accounts and debt, reports of balances tend to be considerably more accurate than reports of change, and no trend toward greater accuracy in balances over time is apparent. Respondent reports of balances tend to be distributed almost equally between overstatements and understatements, although the average balance of all respondents is, as before, understated. Some tendency can be observed for accounts for which the balance is refused to be larger than those that are reported, as is also true of accounts not reported for Groups C and D.

Reports of change, although still less accurate than reports of holdings, exhibit smaller discrepancies than was true of savings accounts. Also, in the present instance, the direction of change is correct in three cases out of four, as shown by the following data:

<i>Groups</i>	<i>Wave</i>	<i>Actual change</i>	<i>Reported change</i>
A.....	4	\$ 412	\$ -131
C.....	1	-133	-167
C.....	4	791	403
D.....	1	493	125

Nevertheless, as in previous instances, computed change appears to be considerably more accurate than reported change, although direct comparisons are not possible. Errors for computed change are shown below:

	<i>Group A</i>		<i>Group C</i>
	<i>Wave 1 to</i> <i>Wave 2</i>	<i>Wave 2 to</i> <i>Wave 3</i>	<i>Wave 2 to</i> <i>Wave 3</i>
Actual change.....	\$1,151	\$-1,093	-362
Estimated change.....	1,283	-1,385	-90
Percentage of error.....	-11.5%	-26.7%	-75.2%

The one case in which computed change is substantially in error occurs when the actual change was relatively small. In the other two instances, the percentage of error in the computed change figures is below that of each of the four errors for reported change shown in Table 71 for other groups.

Nonrespondents. The relatively few nonrespondents, particularly on later waves, make it difficult to judge whether or not dollar figures for this group are appreciably different from those of the respondents. The data suggest, as in the case of time deposits, that the balances of the nonrespondents are higher than those of the respondents and that this difference tends to persist over time. However, in no case is the average for the nonrespondents based on more than six observations.

Dropouts. Very different comparisons between holdings of respondents and of dropouts are obtained, depending on whether the comparison

TABLE 72. ALLOCATION OF ERROR IN AVERAGE DEMAND DEPOSIT BALANCE, BY SOURCE OF ERROR AND BY WAVE, STUDY P3

Wave	Groups	Per-centage of error	Allocation (percent)					Total
			Non-reporting	Respondent reporters	Amount refused or not available	Current non-respondents	Prior drop-outs	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1.	A and B	-34.5	-.7	114.4	32.0	-17.0	99.9
2.	A and B	-13.4	...	32.5	29.0	103.5	100.0
	C and D	48.8	95.4	17.9	-8.1	-5.2	100.0
3.	A and B	12.6	.4	44.0	-15.8	-35.1	106.5	100.0
	C and D	39.2	70.2	-1.7	6.3	28.5	-3.3	100.0
5.	A and B	27.1	...	81.2	-5.9	24.8	100.1
	C and D	44.1	11.5	24.6	39.4	9.4	15.1	100.0

is based on those interviewed every three months or every six months (Waves 1, 3, and 5) or only on those interviewed every three months (for which Waves 2 and 4 are comparable). In the former case, the holdings of the dropouts are substantially above those of the respondents, but the two averages approach each other over time, virtually coinciding on the last wave. In the latter case, the respondent balances are much higher and no tendency toward a juncture is apparent (although only two points, each based on many fewer observations, are involved).

In either case, the direction of change in respondent balances coincides with that of the dropouts. Also, as was found on the other validation studies, the time pattern of the holdings of the dropouts is dominated by that of the nonrespondents on the first wave; the latter represented nearly half of the total dropouts but — somewhat surprisingly — less than one-fifth of the total dollar balances of the dropouts (as of Wave 5).

Allocation of error. Nonreporting as a source of error on the initial waves seems much less because of the relatively low incidence of this phenomenon (Table 72); it appears for Groups C and D only because the one nonreported account had a very high balance on Waves 1 and 3. The later influence of prior dropouts is also much less in evidence, as a result of the closer correspondence between respondent and dropout holdings over time. Nevertheless, the respondent averages tend to understate the overall sample average appreciably on later waves, due partly to understatement of balances reported and of estimated sizes of balances refused. Again, however, observations are few, and it is difficult to draw generalizations.

Overall Evaluation

The results of this chapter suggest that pronounced differences occur over time in the composition of the panel, in the cooperativeness of the panel members, and in the reliability of the financial data that are obtained. Essentially, those remaining in a panel operation are more likely to be younger families and are also more likely to be in the middle of the range in terms of value of home. There is some indication that continuing panel members possess somewhat less wealth than panel members who drop out, but such evidence is only circumstantial.

These studies do provide clear evidence that cooperativeness tends to increase over time, principally in the earlier stages of a panel operation. Moreover, respondent cooperation varies noticeably with the characteristics of the respondents, being higher among those with more education and those in laboring occupations and lower among older people and those in the higher income groups. However, there is little evidence to support the hypothesis that respondent cooperation is influenced by the gross value of assets or by the size of savings account balances.

The principal cause of improved cooperation over time is shown to be differential panel mortality. Those who are less cooperative on the first interview are more likely to drop out of the panel on later interviews. At the same time, rapport seems to increase among those remaining in the panel operation.

Pickup of holdings on later interviews varies substantially with the holding and with the study. However, in most instances pickup amounts to more than 10 percent of total holdings, whether computed on the basis of number or on the basis of dollar value. The fact that two or more pickups were obtained among 25 to 30 percent of the panel members in these studies supports the later findings that pickup for one holding tends to be correlated with pickup for other holdings. This suggests that once a single pickup is obtained it might well be worth probing for additional holdings not reported.

Although each of the validation analyses is based on a relatively small sample, taken together they enable a number of inferences to be drawn regarding the effect of a panel operation on the accuracy of reporting of financial holdings. One such inference is that despite appreciable panel mortality over time, improvement in the accuracy of the data reported by those remaining in the panel tends to more than offset bias due to panel mortality.

Second, there is some evidence that the holdings of nonrespondents exceed those of respondents. This bias tends to remain fairly constant over time.

Third, the principal causes of error in the reporting of financial data on the initial waves of a panel operation appear to be the relatively high incidence of nonreporters and the higher average holdings of nonrespondents, both acting to produce underestimates. These underestimates continue to be manifested in the later stages of the study as well, the dwindling number of nonreporters being offset by an increasing number of dropouts with relatively high holdings.

Fourth, the accuracy of reports of dollar holdings tends to be substantially higher than the accuracy of reported changes in holdings. However, on neither basis does overall accuracy improve noticeably over time.

Fifth, computed change, estimated as the difference between two successive reports of dollar balances, is substantially more accurate than change figures obtained directly. Where large changes are involved, the accuracy of computed change reports compares favorably with that of reported dollar balances.

Sixth, nonreporting when people are asked for change is appreciably greater than nonreporting when people are asked for dollar balances but, as has been noted in previous chapters, refusals tend to be much higher in the latter case. Nonreporting for both groups drops over time, but much more so for those asked initially for change. Interestingly enough, a tendency is evident for nonreporters of dollar balances to drop out of the panel at a later stage and for nonreporters of dollar changes to remain in the panel and report the holding on a later interview.

If one thing is clear from these results, it is that a consumer panel operation tends to improve the reliability with which financial data are reported. The principal source of field errors, nonreporting, declines sharply over time, partly because dropouts come largely from this group and partly because of pickup from those remaining in the panel. For purposes of population estimation, the dropouts (panel mortality) may become a major source of bias, although even in this case the favorable effects of the panel serve to reduce this bias, if anything, *below* that encountered on the first wave.

VII. SOURCES OF IMPROVEMENT

This chapter brings together and discusses the various experiments incorporated in project operations in attempts to improve the reliability of the survey data. These experiments are classified under five general headings, which also serve as the first five sections of this chapter. The first heading refers to the type of data sought, and the second encompasses the forms and instruments used in data collection. Excluded from this second heading, however, is the subject of interviewer selection, training, and supervision which, because of its importance, merits separate treatment under a heading of its own, the third part of the chapter. Other controls that might be imposed on the survey operation provide the subject matter for the fourth section, perhaps most important of which are means of motivating respondents to provide more accurate and complete information. The fifth section is devoted to different methods of evaluating the reliability with which data are reported.

The sixth part of this chapter is devoted to a general review of means of data improvement other than better survey methods. Although still relatively rare, approaches of this type would seem to be indispensable for surveys on a subject such as consumer saving. Reasons for this view, and two approaches developed in the course of the Consumer Savings Project, are outlined in this section, which is then followed by a chapter summary.

Data Sought

Either holdings or change data are invariably sought on financial surveys. The manner in which these types of data were sought on the different surveys of the Consumer Savings Project, and the results obtained, have already been discussed in preceding chapters. Hence, some supplementary and summary comments would seem sufficient at this point.

Holdings Information

Three different means of collecting holdings information have been investigated.

TABLE 73. REFUSAL RATES FOR SELECTED SENSITIVE ASSETS,
WAVE 1, STUDIES P1 TO P3

Asset	Study	Percentage of respondents refusing		
		Dollar amounts	Name of institution	Both
Savings accounts.....	P1	20.8	8.3	7.3
	P2	13.6	6.4	5.7
	P3	4.1	1.2	1.2
Life insurance.....	P1	15.6	19.8	12.5
	P2	14.3	7.1	5.0
	P3	1.8	0.0	0.0
Stocks and nongovernment bonds	P1	20.8	16.6	15.6
	P2	7.0	4.2	4.2
	P3	3.5	1.8	1.8
United States savings bonds.....	P1	24.0
	P2	19.7
	P3	7.0
Real estate (other than own home).....	P1	8.3
	P2	5.7
	P3	0.6

Names and Detailed Breakdowns

For validation data to be most useful, dollar balances must be obtained on each holding, and each must be identified by institution. Such names and individual listings may also be sought for reasons of substantive analysis, such as to investigate the concentration by institution of holdings of a particular asset or the geographic dispersion of consumer portfolios. For all of these reasons, detailed information on each holding was sought on each panel operation. These details included the name in which the asset was registered, the exact dollar balance, the name of the institution and, in some instances, the date of the last transaction. Of these details, the dollar balance and the name of the institution are the most personal and are hence likely to be highly sensitive.

Nevertheless, the panel experience suggests that the great majority of those who cooperated were willing to provide this information, although the request for these data did produce a somewhat higher refusal rate than might have been obtained otherwise. The first of these statements is supported by Table 73, which shows item refusal rates for the more sensitive assets on the first wave of three of the panel operations. Moreover, a certain proportion of those who refused these items of information on the first wave did give these data on later waves. Thus, for dollar balances on savings accounts, this was true of 30 percent, 53 percent, and 14 percent of the item refusals on Studies P1, P2, and P3, respectively. Virtually

the same percentages (and the same people) also later gave names of institutions. As a general rule, asking for the name of the institution caused no additional friction once the dollar balance had been reported.

That requests for these details produced a somewhat higher refusal rate than might otherwise have resulted can be inferred from experience with questionnaires on which such data were not sought and from an analysis of the reasons for refusals. As has been noted in previous chapters, total refusals can be segregated by those who refused to grant an interview and by those who granted an interview but refused to give any financial data. For the former group the request for details is clearly not relevant, because sample members were not told exactly what financial data would be sought until after the interview began.¹³⁴

Among the item refusals, the predominant reason appears to have been the reluctance to provide detailed information. This is borne out in part by reports of the interviewers and in part by the following two comparisons: The first relates to the change forms of Wave 1 of Studies P2 and P3, on which sample members were first asked for overall aggregate dollar balances and were later asked for changes in holdings for each asset individually. This comparison may be vitiated by the fact that change information tends to be less sensitive than dollar balances, but this lower sensitivity of the change data should be more than offset by the request for itemization and for names of institutions. The refusal rates on the overall aggregates for savings accounts are 3 percent and 2 percent on Studies P2 and P3, respectively, whereas the refusal rates on dollar figures (or names of institutions) item by item are 14 percent and 4 percent, respectively. The much greater emphasis placed on dealing with item refusals in the farm panel was undoubtedly at least partly responsible for the sharp decline from Study P2 to Study P3 in item refusals relative to refusals on overall aggregates.

Second, it is worth noting that item refusals as a proportion of total refusals on savings account dollar balances on Wave 1 of Studies P1, P2, and P3 were 33 percent, 41 percent, and 25 percent, respectively. If all of the item refusals were caused by the request for this information rather than for dollar aggregates, the total refusals would have been reduced somewhat. However, judging by the preceding comparison, a substantial proportion of the item refusals would have undoubtedly refused dollar aggregates.

Segmentation of the Portfolio

Another approach to improving data reliability is to cover fewer sub-

¹³⁴ The only exception was that interviewers were permitted to mention what types of data would be requested, although here, too, no reference was to be made to details nor were the respondents to be shown the questionnaire.

TABLE 74. DATA REPORTED BY NUMBER OF ASSETS COVERED
IN INTERVIEW, BY CHANGE VERSUS HOLDING FORM,
WAVES 1 AND 2, STUDY P2

Category	Holdings form		Change form	
	All	Some	All	Some
Percentage of respondents reporting				
Assets on both forms on Wave 1				
Savings accounts.....	100	100	78	86
Life insurance.....	82	94	6	11
Assets omitted on Wave 1 from "Some" forms ^a				
United States savings bonds.....	65	35	19	21
Marketable bonds and common and preferred stock.....	37	18	8	16
Number of holdings reported per respondent				
Savings accounts.....	1.9	2.3	1.6	1.8
Life insurance.....	3.5	3.3	2.0	1.3
United States savings bonds.....	24.9	13.4	3.8	4.3
Marketable bonds and common and preferred stock.....	4.1	2.4	2.0	1.8

^a Based on Wave 2 reports.

jects on a particular interview. This hypothesis was tested in Waves 1 and 2 of Study P2 by questioning half of the sample members about their total consumer portfolio, with the questioning focusing on dollar balances or change, depending on the particular approach. For the other half of the sample only certain assets were covered on the first wave, whereas the entire portfolio was covered on the second wave. The assets omitted on the first wave were United States savings bonds, marketable bonds, common and preferred stock, and own business. If the basic hypothesis is correct, assets covered on both forms should be reported more frequently when some assets are omitted, although the differential may be greater on the holdings form than on the change form because the tendency not to report particular assets if there was no change could camouflage any relationships in the latter instance. On the other hand, assets omitted on Wave 1 should be reported with much the same frequency on Wave 2 for that half of the sample as they would be for the other half of the sample questioned about the entire portfolio on Wave 1.¹³⁵

The data in Table 74 provide only partial support for this hypothesis. More frequent reports of holdings on the forms covering selected assets do result on all the change comparisons, although the number of holdings

¹³⁵ The reason for this is that respondents who had not been questioned about these assets on Wave 1 were questioned about not only these assets but the entire portfolio on Wave 2. By hindsight, it might have been more effective had the assets covered for this group been such that part of the portfolio was covered on Wave 1 and the other part on Wave 2, with no overlap in coverage.

per respondent differs little on either form. On the holdings form, however, only life insurance is reported more frequently with more limited asset coverage, whereas reports of the dollar values of two assets are far higher when asked for in conjunction with all other assets on the first wave than they are when questions about these holdings are postponed to the second wave. The number of holdings reported tends also to be much larger in the former case. Thus, these results would suggest that restriction of the scope of the questionnaire produces better reporting of the assets included initially, but at the expense (at least on the holdings form) of assets not covered until the second wave. Possibly this limitation might not exist if assets already covered in the first wave were excluded from the second wave questionnaire.

Overall Total Versus Detailed Breakdowns

Whether or not more complete data are obtained by asking for detailed breakdowns than by requesting overall totals was tested on Study P2 and on Study P3, each in a slightly different form. On the first wave of Study P2, all sample members were asked initially for overall assets and for total holdings in dollar brackets for each of 10 asset categories and for total asset holdings.¹³⁶ Then, the various experimental approaches, explained in previous chapters, were applied. Specifically, half of the sample members were asked for detailed holdings information and the other half were asked for detailed change information. As a result, comparisons can be made between the frequency of reporting and the asset size distributions for the two types of data on the holdings form, as well as between the statistics for the holdings form and the comparable aggregate estimates obtained from the change form.

If the financial data are more complete when detailed breakdowns are requested, as is often supposed, the frequency of reporting of particular assets from the detailed breakdowns should be higher than from the aggregate estimates. Also, one would expect the size distribution of the total holdings of each asset as obtained from the detailed breakdowns to be more skewed to the right than the size distribution as obtained from the aggregate estimates. In other words, omissions would be less likely on the detailed questions, so that total holdings would be larger in the former case than in the latter. Similarly, one would expect the size distribution of total holdings of a particular asset as obtained from the detailed breakdowns to be more right-skewed than the size distribution of total holdings obtained from the overall estimates on the change form, which in turn

¹³⁶ The asset categories were checking accounts, savings accounts, life insurance, United States savings bonds, marketable bonds, own business, common and preferred stock, trust funds and mortgages, loans lent, and own home and other real estate.

would not be expected to be significantly different from the comparable distribution obtained from the holdings form.

A similar test in Study P3 consisted of asking respondents on the change form initially for overall estimates of holdings, but not asking them for such overall estimates on the holdings form. As a result, the test is weaker than before, although the criteria are much the same. In other words, a lower rate of nonreporting and a more right-skewed distribution of holdings from the detailed information on the holdings form than from the overall estimates on the change form provide support for this hypothesis.

Comparison of ownership rates for the different assets on the two studies, by form and by approach, brings out virtually no differences in the frequency of reporting assets as a result of using the different approaches. Differences are slight and fail to exhibit any consistent pattern. Comparison of size distributions for each study for five types of holdings — checking accounts, savings accounts, life insurance, common and preferred stock, and total assets — did bring out some interesting points. Virtually no differences in the distributions of checking accounts and savings accounts are observed from the results of using the various approaches. However, larger holdings were reported more frequently on the detailed breakdowns in the case of life insurance and stock (Table 75). This tendency is most pronounced in the case of total assets, which includes the four categories mentioned previously, and savings bonds, marketable bonds, and own business — particularly on Study P2 in which nearly two-thirds reported total assets of \$25,000 or more on the detailed breakdowns as compared with one-half of those asked for overall estimates on the change form. Thus, by these criteria, seeking detailed breakdowns of financial holdings would seem to yield more complete information.

Saving Estimates

Saving could be estimated by asking directly for change in the stock of savings or by asking for the amount of savings at two different points in time. Chapter VI showed that in several instances in which comparison was possible, saving in the form of computed change was at least as accurate as estimates obtained directly, and it was often much more accurate. Indeed, in the latter case a question may be raised as to whether or not some estimates of saving should be sought at all in this manner because of the substantial errors that were observed.

Yet, the results of these and other studies suggest that estimates of saving in the form of computed change may also contain substantial errors. In particular, dollar balances obtained as of two different points in time during the same interview appear to yield highly inaccurate esti-

TABLE 75. SIZE DISTRIBUTION OF SELECTED ASSETS, BY TYPE OF QUESTION AND BY FORM, STUDIES P1 AND P3

Asset	Category	Study P1			Study P3	
		Change form, overall estimate	Holdings form		Change form, overall estimate	Holdings form, detailed breakdown
			Overall estimate	Detailed breakdown		
Checking accounts.....	Under \$1,000	68% 29	56% 40	57% 42	66% 32	63% 35
	\$1,000 to \$9,999	3	4	1	2	2
	\$10,000 and over					
Savings accounts.....	Total	100%	100%	100%	100%	100%
	Under \$1,000	28% 54	28% 54	27% 53	55% 38	62% 31
	\$1,000 to \$9,999	18	18	20	7	7
Life insurance.....	\$10,000 and over					
	Total	100%	100%	100%	100%	100%
	Under \$1,000	11% 50	11% 49	10% 47	9% 58	3% 57
Stock.....	\$1,000 to \$9,999	39	40	43	33	40
	\$10,000 and over					
	Total	100%	100%	100%	100%	100%
Total assets.....	Under \$1,000	32% 37	22% 47	26% 48	70% 26	69% 22
	\$1,000 to \$9,999	31	31	26	4	9
	\$10,000 and over					
Total assets.....	Total	100%	100%	100%	100%	100%
	Under \$1,000	2% 16	1% 18	0% 10	1% 4	0% 3
	\$1,000 to \$9,999	42	31	25	17	21
Total assets.....	\$10,000 to \$24,999	32	38	52	63	55
	\$25,000 to \$99,999	8	12	13	15	21
	\$100,000 and over					
Total assets.....	Total	100%	100%	100%	100%	100%

mates of saving. Such a result was obtained in the Netherlands reliability survey (Chapter IV) and also in Study S3. In other words, if only one interview is possible with a family, estimates of saving may be subject to substantial response error regardless of which method is used.

Saving and Savings Data Requested Simultaneously

On Wave 3 of Study P1 and Wave 4 of Study P3, respondents were requested to provide both dollar balances and changes in holdings occurring since the time of the last interview. Although this procedure was not tested on an experimental basis, the results provide no basis for questioning its feasibility.¹³⁷ No instances were reported of respondents refusing to provide both types of information, although some respondents noted that the change could be computed as the difference between the dollar balances reported on the current and preceding interviews. On the other hand, it should be noted from the preceding chapter that computed saving still was considerably more accurate than reported saving.

Data-Collection Methods

Structured Versus Unstructured Approach

Two experiments concerning the structure of the approach were carried out. As was noted in Chapter III, a highly structured questionnaire was used on a randomized part of the sample members in Study P1, and a completely unstructured approach was used on the remainder. However, this experiment was not fully controlled because different interviewers were used with each type of approach. In Study P2, half of the sample members were interviewed by means of a structured questionnaire form, and the other half were interviewed with the aid of reminder cards pasted into the interviewer's notebook but without any formal questionnaire. This experiment was under statistical control, with randomization of both interviewers and sample members.

The results of these two experiments are summarized in Table 76, based on the use of three criteria — the rate of refusal, the frequency with which ownership of particular assets was reported, and the frequency with which complete details were obtained from reported owners. As is evident from this table, similar differences resulted on both studies. In Study P1,

¹³⁷ The question layout used for this purpose is reproduced below:

<i>Present balance</i>	<i>Change since —</i>		
	<i>Increase</i>	<i>Same</i>	<i>Decrease</i>

This particular form was found to be much more effective in asking for change than the usual single-column arrangement because it differentiates clearly between increases and decreases.

TABLE 76. RESPONSE RATES AND ASSET OWNERSHIP REPORTED BY TYPE OF QUESTIONNAIRE, STUDIES P1 AND P2

Statistic	Study P1		Study P2	
	Struc- tured	Unstruc- tured	Ques- tionnaire	Reminder card only
Number of contacts.....	180	76	197	171
Response rate (percent).....	72	87	77	82
Percentage of respondents reporting ownership of				
Savings accounts.....	80	70	90	100
Life insurance.....	93	88	88	88
United States savings bonds.....	57	42	60	55
Stock and other marketable bonds	60	64	31	30
Real estate other than own home..	25	9	29	24
Percentage of validated savings accounts reported.....	65	58
Percentage of assets reported with full details				
Savings accounts.....	82	59	76	72
Life insurance.....	83	49	82	52
United States savings bonds.....	91	10	51	40
Stock and other marketable bonds	82	48	81	87
Real estate other than own home..	84	25	96	96

a much higher response rate was obtained with the unstructured approach. Yet by each of the other two criteria the structured questionnaire appears to be far superior. Not only is each of the assets listed reported with much higher frequency on the structured questionnaire than it is when the unstructured approach is used, but in each case a much larger proportion of the assets that were reported contain full details.¹³⁸ Thus, the completeness of reporting with the structured approach ranged between 80 and 90 percent, whereas with the unstructured approach the highest completeness rate was 59 percent and it fell as low as 10 percent for United States savings bonds. In Study P2 response was also higher when no formal questionnaire was used, but in the two other respects the structured approach seems to have been superior.

The fact that these two approaches were not randomized among interviewers in Study P1 (although they were randomized among sample members) is, if anything, another point in favor of the structured approach. The reason is that only interviewers who had had some experience with unstructured questionnaires and who were willing to undertake the necessary memory work were assigned to the unstructured approach. These

¹³⁸ By "full details" is meant definite responses to a list of items requested for each asset. Thus, for savings accounts, this list included name of the account, name of the institution, date of last transaction, and dollar balance as of the date of the interview.

interviewers were originally rated at least as able as the interviewers using the structured questionnaire.

At the same time, the pronounced differences between the two approaches suggest another explanation largely independent of interviewer quality. This explanation, advanced in previous chapters, is that the more rigorous structured questionnaire approach served to weed out people who were inclined to be uncooperative anyway, with the result that sample members who did grant interviews were basically more cooperative than those interviewed with the unstructured questionnaire. This explanation receives support from the experiment in Study P2, also shown in Table 76.

The "Warm-up" Hypothesis

It has sometimes been conjectured that more complete financial information would be obtained on consumer surveys if the request for such data were postponed to a later stage of the interview or, in the case of a panel operation, to a later interview. Both forms of this hypothesis were tested in Study P1 by the factorial method. As was explained in earlier chapters, three different split-run experiments were superimposed on each other on the first wave of this study — a structured questionnaire versus an unstructured questionnaire, requesting dollar balances and names of institutions for all holdings on the first wave versus seeking this information only on the second wave, and asking on the first wave for all details versus not asking for dollar balances or names of institutions.

The third experiment was carried out by splitting the two structured questionnaire subsamples into halves once more and asking half of this group first for data on asset holdings and then for data on debt, and following the reverse procedure with the other half. Presumably, respondents would be more reluctant to give information on assets than on debts. Hence, if the "warm-up" hypothesis held in Study P1, more assets should have been reported by the respondents when this information was sought last than when it was sought first.

On the whole, the hypothesis is supported by the data (Table 77). A higher proportion of respondents reported each of the five assets listed when debts were covered first than the proportion when assets were covered first. Some of the differences are substantial, and statistically significant, such as for life insurance and for stock and marketable bonds. In addition, the number of holdings reported per respondent when debts were covered first is much greater than the number when assets were covered first; this was particularly noticeable for savings bonds and for stock and marketable bonds.

Results pertaining to the panel application of the "warm-up" hypothesis do not provide much support for the desirability of a "warm-up"

TABLE 77. DATA REPORTED, BY ORDER OF COVERAGE OF ASSETS AND DEBTS ON STRUCTURED QUESTIONNAIRES, WAVE 1, STUDY P1

Statistic	Assets first	Debts first
Percentage of respondents reporting		
Savings accounts.....	78	82
Life insurance.....	88	98
United States savings bonds.....	53	62
Stock or marketable bonds.....	50	71
Real estate other than own home.....	24	26
Number of holdings reported per respondent		
Savings accounts.....	1.4	1.7
Life insurance.....	4.0	3.9
United States savings bonds.....	5.4	10.0
Stock or marketable bonds.....	1.9	2.9
Real estate other than own home.....	.4	.3

period before requesting sensitive information. The main evidence in support of this hypothesis is the higher frequency with which savings accounts and life insurance were reported on the structured questionnaire approach when dollar balances were not requested, and the lower refusal rate on the unstructured questionnaire approach when no dollar information was sought. However, only the first of these findings tends to support the hypothesis, because most refusals would have been encountered before dollar figures were requested. On the other hand, other assets were reported as frequently when one approach was used as when the other was used. Indeed, reporting of stock seems to have been, if anything, considerably higher when dollar figures were requested, and much the same was true of United States savings bonds.

A further comparison of the two approaches was made on the basis of the frequency with which holdings were reported and with which dollar amounts were given by the various subsamples after the first two waves of interviews on Study P1. By that time, figures and names of institutions had been requested of all of the sample members. Hence, if the "warm-up" period were really effective, the frequency with which holdings and dollar balance were reported should have been higher among those not asked initially for dollar figures.

The refusal rate was indeed lowest on the unstructured approach when no dollar amounts were requested, and the highest frequencies of holdings were obtained with this approach. However, complete details on holdings were more likely to be obtained with a structured questionnaire seeking dollar balances.

Varying the Interview Interval

The present studies provide two series of tests relating to length of interval between interviews. One series of tests, presented in Chapter VI,

related to the accuracy with which the panel members of Study P3 reported debt, checking accounts, and savings accounts when half of the members were interviewed on every wave and the other half were interviewed on every other wave. The findings, it will be recalled, were inconclusive, suggesting that more accurate reporting of debt was produced by the more frequent interviews than by the less frequent interviews. However, the same phenomenon was not in evidence for checking or savings accounts.

It may be argued that the discriminatory power of this first series of tests is not very great because the comparisons involved intervals fairly close to one another. To some extent this deficiency can be remedied by the second series of tests, summarized in Table 78. The comparisons which form the basis for this table relate to the estimation of saving in a particular form by summing the reports of savings accounts obtained from consecutive interviews with an estimate of saving obtained as the difference between holdings reported at the start of the period and holdings reported at the end of the period. Two such sets of comparisons are presented, one for savings accounts for Studies P2 and P3, and one for three different types of farm assets for Study P3. In the case of savings accounts, one set of estimates of saving was obtained as the difference between holdings reported on Wave 1 and holdings reported on Wave 5, using only those sample members in each study who reported holdings on both waves. The other set of estimates was derived as the cumulated sum of saving in the form of savings accounts, based on those sample members interviewed on every wave and reporting this information. For each comparison, corresponding saving estimates computed from the institutional data serve as the yardstick of accuracy.

The saving comparisons relating to farm assets are of a somewhat different nature. In this case, one comparison is based on replies obtained from respondents on Wave 5 of Study P3, conducted in January, 1962, in which estimates were requested of total saving in the form of land and buildings, livestock, and machinery for the entire year 1961. A second comparison was obtained by cumulating reports of saving of these assets provided by those members of the farm panel interviewed on every wave. Here, the comparison relates to the same panel members in both instances because no objective measure of accuracy is available.

To judge by Table 78, the results are still not conclusive. The more accurate estimate of saving in the form of savings accounts varies with the study, although the small sample sizes and the unstable nature of the averages in Study P3 lend greater credence to the Study P2 comparison. In that case, more accurate estimates are obtained by differencing the balances reported at the start and end of the panel operation. The results

TABLE 78. ESTIMATES OF SAVING IN SAVINGS ACCOUNTS AND IN FARM ASSETS OVER A ONE-YEAR PERIOD,
USING DATA FOR DIFFERENT INTERVALS, STUDIES P2 AND P3

Form of saving	Study	Coverage	Sample size	Average dollar saving per savings unit during year based on		
				Difference between Wave 1 and Wave 5 balances	Cumulated differences from wave to wave	Institutional balances on Wave 1 and on Wave 5
Savings accounts.....	P2	All accounts	62	\$ 208	\$ 346
		Validated accounts	44	328	492	\$ 251
	P3	All accounts	14	-751	-1,169
		Validated accounts	5	556	1,121	1,593
Land and buildings.....	P3		47	1,995	-22
Livestock.....	P3		61	497	-1,877
Machinery.....	P3		68	1,353	2,072

yielded by this method for saving in farm assets seem also more plausible than the negative saving obtained by the other method, since the period covered by that study was one of growing prosperity.

Mail Questionnaires Versus Personal Interviews

Substantial economy could be achieved if financial data were secured by mail. The practicability of this procedure, however, is very much an open question in two respects. First, the cost of a mail questionnaire remains to be established, that is, whether or not enough sample members will return the questionnaire to warrant use of the mail technique. Second, are data supplied by mail as reliable as data obtained in a personal interview? If the reliability of the mail questionnaire data is much lower, the technique may be of little value, regardless of the rate of return.

The use of a mail questionnaire was tested on three of the panel operations of the project. In each case the test was carried out on the fourth wave of interviews, so that the results cannot be used to judge the feasibility of mail questionnaires in a one-time operation. However, if the mail questionnaire is not feasible on a later wave of a panel study it would certainly not be feasible in a one-time operation.

In Study P1, a mail questionnaire was sent at the beginning of the fourth wave to all still-active panel members. Two types of mail questionnaires were used, being allocated to the panel members on a split-run basis. One questionnaire was relatively short, contained only two pages, and requested changes in holdings since the last interview. The second questionnaire was longer, covered four pages, and sought dollar balances on such holdings as checking accounts, savings accounts, and debt, and changes on such assets as common stock, life insurance, and real estate.

All together, 47 percent of the 178 regular mail questionnaires were returned.¹³⁹ Those not returning mail questionnaires were contacted by personal interview, and interviews were obtained with 82 percent. The response on the short form was somewhat higher, although not significantly so, than on the longer form — 51 percent versus 43 percent.

In Studies P2 and P3, mail questionnaires were sent to a randomized half of the cooperative and uncooperative panel members on Wave 4. In Study P2, the mail questionnaire contained four pages with virtually the same content as the long mail questionnaire in Study P1. The mail questionnaire in Study P3 contained three pages and focused on changes in holdings only.

¹³⁹ In addition, short mail questionnaires were sent to 17 "hard" refusals on the third wave and to five panel members who had indicated on earlier waves a preference for supplying data by mail. The returns from these two groups were one and two, respectively.

TABLE 79. COMPLETENESS OF REPORTING, BY MAIL RETURN VERSUS PERSONAL INTERVIEW, STUDIES P2 AND P3

Study	Holding	Percentage of known holders supplying all details		Percentage of dollar figures given to nearest cent	
		Mail	Personal interview	Mail	Personal interview
P2.....	Checking accounts	92	97	53	44
	Savings accounts	93	93	58	48
	Personal debt	82	94	44	27
P3.....	Checking accounts	94	100	47	36
	Savings accounts	94	100	27	15
	Personal debt	92	100	55	13
	Farm debt	88	100	40	18

The rates of return of the mail questionnaires on Study P2 were 41 percent among the cooperative panel members and 26 percent among the uncooperative ones. On Study P3, 62 percent of both the cooperative and the uncooperative panel members returned the mail questionnaire. On both studies, cooperative panel members not returning mail questionnaires were assigned to personal interviewers, as was done in Study P1. Such interviews were successfully carried out with 95 percent of such people on Study P2 and with all of those on Study P3. At the same time, it should be noted that 7 percent of the panel members sent mail questionnaires on Study P2 refused to cooperate any further, as did 6 percent of those on Study P3. Thus, mail returns did appear to be substantial, at least on a panel operation, and those who did not respond by mail were picked up successfully by personal interview.

To turn to the second principal question regarding the mail approach, full details on financial holdings were obtained somewhat more frequently on the personal interviews, as is apparent from Table 79. However, in terms of an indicator of accuracy of the data — reporting of dollar figures to the nearest cent — the mail returns were generally far superior. In other words, the personal interviews may be completed more frequently, but the accuracy of report may be higher on the mail returns.

Sequence of Questionnaire Forms

The apparent superiority of asking for holdings rather than for change in a one-time survey was pointed out in earlier sections. In the case of a panel operation, the question may still be raised as to whether or not the two alternative approaches led to results different from those for a one-time study. Some findings relating to this question can be deduced from Studies P2 and P3, in which half of the panel members were asked for

TABLE 80. EFFECT OF INTERVIEW SEQUENCE ON REPORTING OF VALIDATED HOLDING, STUDIES P2 AND P3

Holding	Study	Interview sequence	Percentage of respondents reporting validated asset		
			Wave 1	Wave 3	Wave 5
Savings accounts.....	P2	CCH	60	79	88
		HHC	67	79	80
	P3	CCH	58	73	80
		HHC	74	75	88
Debt.....	P3	CCH	62	88	94
		HHC	84	84	91

change on the first two interviews and for holdings on the third interview, whereas the reverse procedure was followed with the other half.

These findings indicate that over time it makes little difference which sequence is used. As was noted in Chapter VI, and is brought out more clearly in the following tabulation, in terms of response the use of the change form last may be more effective:

		<i>Cumulative refusal rate</i>	
		<i>CCH</i>	<i>HHC</i>
P2	After one wave.....	16%	22%
	After three waves.....	32	34
	After five waves.....	47	41
P3	After one wave.....	4	10
	After three waves.....	10	22
	After five waves.....	13	24

On the other hand, contrary to the findings pertaining to the first wave, reporting of holdings on later waves does not appear to be any better when respondents are first asked for change than for holdings. This point is brought out in Table 80, which shows that by the third interview the rate of reporting of the validated asset or debt is more or less the same regardless which approach has been used. At the same time, since reporting rates when respondents are first asked for holdings are not much lower at any time than reporting rates when respondents are first asked for change, the former approach would still seem preferable because of the more uniformly reliable data obtained.

Feedback

On all waves of the panel study after the first, data previously reported by the respondent were recorded on the questionnaire form for the next

wave. This procedure has the advantage of facilitating the conduct of the interview and requiring the respondent to recall less information.

Consideration was given to not supplying this information on particular waves or to withholding particular items of information, such as the identity of the family member having a particular holding. Such a withholding procedure might serve as a check of reliability of data provided earlier. Indeed, an even more complete check would be provided if the respondent were requested to report all of his holdings over again. However, this procedure was not followed because of strong resistance from the interviewers and because pilot tests indicated the possibility of considerable respondent irritation. Such a procedure would yield at best an indication not of the reliability of the data but only of their consistency, and this possibly at the cost of a substantial increase in panel mortality.

Use of Keysort Cards as Questionnaire Forms

In Study P1 all questionnaire forms were multilithed on white sheets of paper. In Studies P2 and P3, the questionnaires were printed on large Keysort cards ($9\frac{1}{2} \times 12\frac{1}{2}$), partly to permit certain key items of information to be coded on the edges of the questionnaire. In addition, this was done to indicate to the respondents how the data would be coded and to show them that individual identification was not used in preparing tabulations. Interviewers were acquainted with the coding procedures in the training sessions and were encouraged to discuss the coding with the respondents.

Although this approach was not incorporated into a controlled experiment, there seems little doubt regarding its effectiveness. The interviewers reported numerous instances of favorable reception of this questionnaire card and of respondents being left with a better impression of the scientific nature of the study. In some instances, the questionnaire card was cited as a prime factor in convincing the sample member to cooperate. No unfavorable reactions were reported.

Interviewer Control

Since interviewer procedures and their effects on the reliability of financial data are the subject of a separate monograph, reference can be made to that study¹⁴⁰ and relatively little data need be presented in this section. This is especially so because few controlled experiments could be carried out relating to interviewers, these experiments being largely observational. The results of these experiments are discussed under the

¹⁴⁰ Stanley Steinkamp and Mathew Hauck, *Survey Reliability and Interviewer Competence*, Studies in Consumer Savings, No. 4 (Urbana, Illinois: University of Illinois Bureau of Economic and Business Research, 1965).

usual three-way breakdown of interviewer control — selection, training, and supervision.

Selection

Sources of Interviewers

An interviewing staff had to be recruited anew for each panel. In the pilot operation in Chicago and in Study P1, interviewers were recruited through usual channels — newspaper ads, contacts through universities, and contacts with interview organizations. State employment offices were also used to recruit interviewers and were of considerable assistance both in attracting applicants and in facilitating the interviewing and weeding out of applicants.

The best source of all, however, was the local school districts, a source used heavily in Study P2 and almost exclusively in Study P3. Invariably, the local superintendent of schools was most helpful in mailing notices of the availability of these interviewing positions to his teaching staff. They, as well as the teacher associations, seemed particularly willing to cooperate in finding applicants, a task not difficult because of the large number of teachers always looking for part-time work.

At the same time, teacher-interviewers tended to make very acceptable and willing students for interviewer training. With their backgrounds, most of them had little difficulty mastering interviewing instructions as well as the meaning of financial terms. Moreover, respondents appeared to be more willing to provide confidential data to interviewers who were teaching than to interviewers who were in the business world. This inference receives support from the following tabulation, showing refusal rates on savings accounts in Wave 1 of Study P2 by the regular occupation of the interviewer.

<i>Regular occupation of interviewer</i>	<i>Interview refusal rate</i>	<i>Savings account balance item refusal rate</i>
Teacher	18%	1.6%
Business or industry	20	3.8
Other	27	9.1

It should be stressed, however, that sample members were not randomized among interviewers. The lower refusal rates of the teacher-interviewers might have been brought about by their higher education — and possibly their greater flexibility or greater intelligence — than by their regular occupation.

Use of Personality Test in Interviewer Selection

Applicants for interviewer positions on all three panel studies were asked to take a personality test in addition to filling out an application

form, going through a personal interview, and providing references. On Studies P1 and P2 this test was the *Inventory of Personal Attitudes*, and on Study P3 the *Edwards Personal Preference Schedule* was used. On the first two of these panel studies the results of these tests were used as a general guide, primarily to detect applicants with extreme personality traits. However, by the time of Study P3, evidence had been obtained that interviewers who scored high on "dominance" and low on "deference" appeared to be more successful than other interviewers in obtaining complete financial data.¹⁴¹ As a result, these personality characteristics were used as a primary basis for selecting interviewers for Study P3. Additional evidence accumulated on that study tended to reinforce the earlier findings, although such evidence can only be circumstantial in view of the non-experimental nature of the interviewer selection process.

Nevertheless, these results, supported by a multivariate analysis, suggest that personality tests may serve as a useful device for selecting interviewers. In particular, these tests apparently pinpoint certain characteristics of interviewers not commonly suspected. For example, there was some tendency for the completeness of information to be inversely correlated with the overall response rates obtained by individual interviewers.¹⁴²

Selection Procedure

Experience with the various survey operations led to the development of a two-stage procedure for selecting interviewers. The first stage involved the selection of so-called "interviewer candidates" from among the applicants for interviewer positions. These interviewer candidates were chosen by means of a scoring system based on the completeness with which the application form was filled out, on a checklist filled out by a project staff member after interviewing the applicant, on the scores of the personality tests, and on the quality of the reference letters. The scoring system was such that twice as much emphasis was given to the checklist and to the evaluation of the reference letters as to either of the two other criteria.¹⁴³

As a rule, the procedure for selecting interviewer candidates consisted of ordering applicants by their total score and selecting the requisite number with the highest scores. However, certain additional factors were taken into consideration. One such factor was the location of the applicant relative to the concentration of the sample members. A second factor was the applicant's interviewing experience, which, as a rule, was *not* con-

¹⁴¹ *Ibid.*, Chapter III.

¹⁴² *Ibid.*

¹⁴³ For the exact scoring method, see *ibid.*, p. 46.

sidered a point in his favor. This was particularly true if the applicant had done, say, primarily attitudinal interviewing of an unstructured type when interviewers were to be trained to follow a highly structured approach.

After interviewer candidates had been selected, they were put through a series of intensive training sessions. In the course of these sessions, discussed briefly in the following section,¹⁴⁴ candidates were observed while practice-interviewing and were given qualifying take-home examinations that covered financial terminology as well as interviewing procedures. In addition, candidates were assigned trial interviews with predesignated respondents. In one study, these were actual "dummy" interviews, in the sense that the respondents had already been approached by the project staff and had been coached on how to act and on what information to report. Such interviews proved to be highly valuable, since the interviewer report could be compared with the respondent report obtained after the interview. Candidates that survived this procedure were, with few exceptions, hired as project interviewers.

Although better procedures might have been devised, this procedure appears to have been fairly effective in selecting project interviewers. It was a fairly expensive procedure, since, as a rule, nine applicants had to be hired to produce one project interviewer.

Training

Training Sessions

Training sessions on technical studies have to be of two types. First, training is necessary at the outset to acquaint the interviewers with the objectives and reasons for the study as well as with how to conduct the interview and fill out the questionnaires. Second, if the study is a panel operation, additional training sessions are required to instruct the interviewers on methods and procedures in each following wave of interviews. Although no formal experiments were carried out with either type of training session, the experience with these sessions suggests preferred ways of handling them.

Since the interviewer candidates at the initial training session were largely unacquainted with the general subject of consumer finances, considerable emphasis on this subject was required. If the interviewers could be made to understand the value of consumer financial data and the uses to which they might be put, there was a much greater likelihood that they would remain with the study and do a conscientious job of collecting data. Hence, the latter part of an initial two-to-three-hour session was devoted

¹⁴⁴ For further details, see *ibid.*, Chapter IV.

to coverage of the subject matter of the study, especially to the necessity of obtaining complete and accurate financial data. Later meetings in the initial training sessions were devoted to a review of interviewing techniques, to means of dealing with the respondents, and to procedures for dealing with difficult situations. Only after this material had been covered did the sessions focus on the particular questionnaires to be used.

Experience with the initial pilot study in Chicago indicated a need for additional training sessions before each new wave of interviews. Hence, two-to-three-hour training sessions were held in advance of each wave of interviews on all of the later studies. These sessions proved useful, not only for instructing interviewers in the particular forms and procedures to be used, but also in correcting weak spots in interviewer practices and in reinforcing the interviewer's knowledge of the purpose and objectives of the study. In addition, in Study P3, interviewers on later waves were required to fill in a copy of the questionnaire on the basis of information supplied by the field director at the beginning of the training session, and these questionnaires were collected and checked for accuracy.

Value of Qualifying Examination

As was mentioned earlier, interviewer candidates were given a take-home qualifying examination as part of the initial training sessions. This examination covered interviewing methods and procedures, the purposes and objectives of the study, and the meaning of numerous financial terms. The examination was detailed enough that training manuals and other written materials had to be consulted in order to pass. In this way, it was felt that the interviewer candidates would become better acquainted with the details of the study. A passing grade was required before an interviewer candidate could be hired as an interviewer, although candidates with higher scores were not necessarily given preference over those with lower scores.

There is little doubt that these examinations made interviewers better acquainted with the study and with the interviewing procedures. The examination also brought out aspects of the study which had been misunderstood and which could be clarified before actual interviewing began. Moreover, the complexity and time required for the examination to be completed (at least two hours) served in each study to weed out several candidates who presumably were not sufficiently interested to expend the effort to finish the examination. For these reasons alone, an examination of this type would seem to be a useful training tool.

Whether or not such an examination is even more effective and helps to differentiate good from poor interviewers is, however, not clear. In terms of interviewer dropouts, the examination does not support this view, to judge by comparison between the average scores of those interviewers

who served on all five waves of the study and of those who dropped out before the study was over.

<i>Study</i>	<i>Average score</i>	
	<i>Dropouts</i>	<i>Others</i>
P2.....	128	129
P3.....	113	114

In terms of interviewer efficiency, the evidence is mixed, as is shown by correlations computed for Studies P2 and P3 between, first, the interviewer score on this examination and his response rate, and second, between the score and his pick-up of the validated holding. None of these correlations is statistically significant at the .05 level.

<i>Study</i>	<i>Correlation coefficient between examination score and</i>	
	<i>Response rate</i>	<i>Pickup rate</i>
P2.....	-.40	-.01
P3.....	.15	.05

In view of the relatively small sample sizes and lack of randomization, it is difficult to draw firm inferences from these data. However, at the least, these results suggest that such an examination is a useful part of the training sessions.

Supervision

Interviewer supervision has many facets, ranging from the method of compensation to the processing of the completed questionnaires. Those facets which received special attention are discussed in this section.

Compensation

On the initial pilot study and on Study P1, interviewers were paid on an hourly basis, portal to portal, with expenses for mileage, phone calls, and miscellaneous items. In an effort to attract and retain good interviewers, the rate offered was set above the going hourly rate at the time. In addition, as an incentive to remain with the study, the rate of compensation was raised five cents per hour on each following wave.

Nevertheless, various shortcomings became apparent with this method of compensation. The interviewers, who were largely professional people, seemed dissatisfied with an hourly wage. Perhaps for this reason, assignments were not often completed on schedule. Also, high variability in cost per interview was apparent from one interviewer to another, a variability which was larger among interviewers than among respondents for the same interviewer. The average cost per interview was also not low, and there was reason to suspect that some interviewers were unintentionally

inflating their expenses by visiting noncontacts at times when they did not really expect them to be home.

As a result, the method of compensation was altered on Study P2, and interviewers were paid by the completed interview instead of by the hour. Based on cost figures from the previous studies, \$10 was paid for each of the first 10 completed interviews, \$13 for all interviews over 10, and \$15 for interviews with panel members previously refusing another interviewer. Once established, these higher rates remained the same for all future waves, regardless of the interviewer, with the exception that refusal of an interview by a previously cooperative panel member moved him into the \$15 bracket if the refusal were converted. Supplementing these interview rates was an hourly rate of payment for attending training sessions and for study time and a mileage reimbursement for panel members living more than 10 miles from the interviewer's home.

A similar method of compensation was employed on Study P3 with two modifications. First, the rate per interview was established at \$10 for the first 10 interviews on each wave and \$13 for all interviews over 10 — to give interviewers some incentive to obtain more than 10 interviews. In addition, interviewers were paid excess mileage on all assignments over 10 miles from their home regardless of the outcome — whether it was an interview, a refusal, or a noncontact. Such reimbursements seemed especially proper on that particular study because of its farm location and the large distances interviewers had to travel.

All things considered, payment by the completed interview appeared to be preferable to payment by the hour. Interviewer morale appeared to be higher, perhaps because the method of payment was more in agreement with the method by which these (professional) people were paid on their regular jobs. Moreover, surprisingly enough, compensation by the interview was, if anything, less expensive than compensation by the hour, as is shown by the following tabulation (these figures include compensation for meetings, mileage expenses, and trips by project staff to the field):

<i>Wave</i>	<i>Average cost per interview</i>			
	<i>Pilot study</i>	<i>Study P1</i>	<i>Study P2</i>	<i>Study P3</i>
1.....	\$18.87 ¹⁴⁵	\$23.11	\$16.87	\$14.79
2.....	18.46	20.61	13.62	13.15
3.....	18.23	15.42	13.23	14.35
4.....	18.19	14.41	12.58	12.49
5.....	19.02	10.03	13.68	13.92
Average.....	18.55	16.72	14.00	13.74

¹⁴⁵ This figure is relatively low because this panel operation had been preceded by a so-called "background interview" with the same panel members about six months earlier, so that much of the original contact work was out of the way by the first wave.

The cost figures shown in the pilot study are not directly comparable with the others because most of the interviewing on that study was sub-contracted. Nevertheless, it is clear that the average cost per interview on Studies P2 and P3, the latter with the extra mileage expenses, was still substantially less than the average cost per interview on Study P1. In addition, contrary to what was feared, there was no evidence that interviewers were trying to minimize the amount of time spent by rushing the respondents. On both of these studies, an interviewer did not receive any compensation if a questionnaire was turned in with many blanks.

Interviewer Rotation

A personal interview panel raises the danger that the interviewers and the respondents may become so well acquainted that the later interviews are regarded as mere formalities. This is especially true of those panels in which the main objective of the later interviews is to update previously obtained information.

One means of coping with this difficulty is to rotate the interviewers among the respondents on later waves. Such a procedure is not popular with interviewers, however, because it disrupts previously established relationships. Moreover, according to the arguments of the interviewers, such a procedure can increase the refusal rate because contact with a new interviewer may irritate the respondent and lead him to refuse to cooperate any further. Presumably, this would be especially so if the respondent had been asked to supply confidential information and if he were then asked to supply the same information to a new interviewer.

On the other hand, interviewer rotation may have advantages because of the very disruption of previously established relationships. Thus, a new interviewer might not take previously listed holdings so much for granted as the old interviewer and, in pressing for additional information, might uncover holdings hitherto not reported.

To test these ideas, a moderate amount of interview rotation was carried out on Wave 3 of both Studies P2 and P3. On Study P2, three respondents were taken away from each interviewer and three were assigned to him from other interviewers. In these assignments, an attempt was made to hold constant the type of form handled by each interviewer (change versus holdings), the approximate overall travel distance, and the rate per interview. Sample members who had refused to supply all financial data were not included, although this did not prevent sample members who had not reported validated holdings from being included. Subject to these restrictions, interviews were rotated on a random basis.

The rotation procedure on Study P3 was essentially the same, with three to five sample members rotated for each interviewer. As before, the

type of form was held constant, as was the travel distance assigned to each interviewer. In this study, however, an effort was made to rotate more panel members who had not supplied validated data, because the results of the rotation on Study P2 suggested that pickup of nonreported holdings was increased in this manner.

The results of these experiments do not provide much support for the desirability of interviewer rotation. The response rates obtained on the rotated and the nonrotated segments of the samples on each of the two studies, shown in the following tabulation, fail to bring out any superiority for the rotated interviews. If anything, a slightly higher proportion of the sample members contacted were interviewed on the nonrotated segment.

<i>Study</i>	<i>Response rate</i>	
	<i>Rotated</i>	<i>Nonrotated</i>
P2.....	94%	98%
P3.....	91	94

The results relating to the pickup rate are similar. As with the response rate, the proportion of previously nonreported validated holdings picked up on the third interview was slightly higher in each study among the nonrotated sample members than among the rotated sample members.

<i>Study</i>	<i>Pickup rate</i>	
	<i>Rotated</i>	<i>Nonrotated</i>
P2.....	16%	18%
P3.....	15	20

At the same time, these results do not preclude the possibility that the rotation procedure may have produced intangible benefits reflected in both segments of the sample. Thus, the fact that interviewers were informed at the beginning of the study of this rotation plan (but not for which interviews) may have influenced them to push harder on each interview for complete listing of holdings than might otherwise have been the case.

Quality Control

Virtually all of the standard techniques were used to maintain interviewer morale and to ensure that questionnaires were filled out in accordance with prior specifications. These included periodic group meetings (mentioned earlier), meetings with individual interviewers to clarify particular aspects of the study, mailing of so-called "interim reports" to both panel members and interviewers, and editing of questionnaires both in the field and at project headquarters. Field supervisors were hired for all of the studies but Study P3 in which, because of the proximity of the office to the sample area, the project field director acted as the field supervisor.

In that particular study all editing was done at the office but, again because of the same proximity, interviewers received edited questionnaires needing further clarification at least as quickly as on any previous studies.

Besides these methods, four additional procedures were used at various times to maintain and improve the quality of the information. First, interviewers were required to complete an Interviewer Report Form for every contact attempt, whether successful or not; one such form is shown in Appendix A. The specific content of this form varied with the particular wave and study, but in all instances an attempt was made to bring out on this form the circumstances and results of the contact as well as the interviewer's interpretation of the quality of the data. Interviewers were also asked to record on this form any peculiarities of the interview or circumstances which might reflect on the accuracy or completeness of the financial data or on the cooperativeness of the respondent. As was noted in Chapters III-V, these interviewer ratings were correlated to some extent with the reliability of the data, judging by the reports on the validated holdings. Evidence also exists to support the hypothesis that the value of these ratings tends to increase with the quality of the interviewer.¹⁴⁶ Aside from these indications, the Interviewer Report Form undoubtedly served to increase the emphasis on securing accurate and complete information.

A second technique was the use of telephone calls and return postcards to check on the interview and on the performance of the interviewer. Although interviewer cheating is not a major problem in a panel operation, such a technique may serve to detect cheating when it occurs, and it did serve to bring to light two such instances in Study P2. On the other hand, the use of postcards alone to detect cheating may not be very efficient, to judge by the one experiment carried out in Chicago. In this experiment, 10 people selected at random from the Chicago telephone directory were sent thank-you letters on University of Illinois stationery for granting an interview on their finances. Although these people were not actually interviewed, five returned the postcard indicating the length of the interview and their opinion of the interviewer!

The telephone calls and the return postcards were useful primarily in detecting interviewer qualities which tended to irritate respondents. Such defects were brought to the interviewer's attention without indicating which respondents were involved. Since the interviewers were told that these postcard checks and telephone calls were being made continuously, they were under pressure to correct these defects (and interviewer cheating was also forestalled).

The third technique was to impose deadline dates for individual interviewees rather than for an interviewer's complete assignment. Individual

¹⁴⁶ *Ibid.*, Chapter III.

questionnaire deadline dates were imposed on the last two waves of Study P3, and they were highly successful in ensuring that the entire wave of interviews would be completed on schedule. This technique also served to counteract the tendency of interviewers to leave their least-cooperative respondents for the very end. In practice, interviewers were given a deadline of returning the completed questionnaire within one week after the respondent received the advance letter. Less than 10 interviews were left outstanding with an interviewer at any particular time, so that he would not feel rushed in making these interviews.

The use of validation data for pinpointing poor interviewers served as a fourth means of quality control, although its full potential may not have been realized in these studies. With the aid of the validation data, interviewers who were picking up a much smaller proportion of the validated holdings than other interviewers could be singled out, and the circumstances of these interviews could be investigated. Such data can provide conclusive evidence of interviewer cheating, as was true of the two instances in Study P2 mentioned earlier. Even more important, such data indicate which interviewers may not be obtaining complete financial data and for which respondents. Given such knowledge, these interviewers may be trained more thoroughly on means of soliciting financial data or, if these attempts are unsuccessful, the interviews might be reassigned to interviewers more successful in obtaining these data. Apart from the planned rotation of interviewers, such attempts were not made in these studies because they would have interfered with the experimental design. Nevertheless, the use of validation data to control interview quality would seem to offer considerable promise.

Other Controls

A variety of other methods was used to improve data reliability both initially and during the later waves of the panel operations. These methods varied from the use of respondent incentives to attempting a more narrowly focused definition of the sampling unit.

Definition of the Sampling Unit

Lack of knowledge by the respondent(s) of the holdings of different family members may be a major source of response error, even when the "savings unit" definition is employed. There is little doubt that errors due to lack of knowledge would have been even more substantial if the more customary definition of sampling unit had been employed, such as the "consumer unit" or the "family." How much error was the result of lack of knowledge in such instances can only be conjectured.

To minimize errors due to lack of knowledge, a narrow definition of the sampling unit was formulated at the outset and was used on all of the panel studies. Illustrative of this definition is the one used in Study P3, namely, the farmer, "your wife, your children under 16 years of age, and other family members who earn less than \$600 per year and who have less than \$600 in their own name."

A definition of this type had at least two advantages. First, it served to focus coverage on the immediate family of the farmer, particularly on those still financially dependent on the head of the household. By restricting asset and debt coverage to this group, omissions due to lack of knowledge appear to have been minimized although not completely eliminated. Second, the definition is relatively simple and was readily understood by virtually all the respondents, to judge by the few questions of interpretation raised in the interviews.

On the other hand, such a definition does present the disadvantage of dividing the family or the household into several components, which may present problems of both economic and statistical analysis. Such problems are not unsurmountable, however, and could readily be handled with appropriate transformations of the data. For example, allowance could be made in the sampling frame for the fact that each household may contain more than one savings unit. In each household where a savings unit is interviewed, pertinent information could be obtained on the number and selected characteristics of other savings units in the same household. In this way, information obtained from a sample of savings units could be transformed into estimates pertaining to families or any other consumer unit.

Motivating the Respondent

Various attempts were made to involve the respondent more closely with the study. One approach was to use the advance letter to respondents for this purpose. On all three panel studies, P1 to P3, the covering letter to a split-run half of the sample on the first wave, in addition to the usual request for cooperation, asked the respondent to evaluate the interview procedures at the end of the interview and to offer suggestions for improvement. When the interview was over, these sample members were left a form to be returned by mail, on which to record their impressions of the interview and to make suggestions for changes.

The response rate on all three studies was only barely higher among those receiving this special letter than among the other sample members. However, over 61 percent of the forms requesting suggestions were returned and about one-half of the returned forms contained written comments or suggestions regarding the interview procedure. Moreover, this

technique may have increased the reliability of the data, as is evident from the following tabulation showing, according to the type of advance letter received, the percentage of interviewed respondents that reported the validated holding.

<i>Study</i>	<i>Regular letter</i>	<i>Special letter</i>
P2 (time deposits).....	59%	65%
P3 (debt).....	44	45
P3 (time deposits).....	8	12

Also, those originally receiving the evaluation letter were more likely to remain in the panel on later waves on both studies, although the differences are not statistically significant. Since use of this evaluation approach entails little extra cost, it would seem to be a worthwhile procedure.

Use of References

A characteristic incorporated in all of the advance letters on the three panel studies, P1 to P3, was the mention of names of prominent local people whom the sample members could contact to verify the authenticity of the study. As a rule, these people were well-known and highly regarded top administrators in local colleges and universities.

Since this technique was not part of a controlled experiment, its exact effect on response cannot be measured. Nevertheless, there is no questioning that the effect was positive, since no instance was reported of a sample member refusing to cooperate because of reference to these people in the advance letter. At the same time, the interviewers reported numerous instances where sample members cooperated only after contacting one of the people mentioned in the advance letter, and these instances were verified by the references themselves. In addition, other sample members indicated that the mention of one of these person's names in the advance letter influenced them to cooperate without actually calling the individual.

Gifts and Incentives

Various attempts were made to induce respondents to cooperate by offering them gifts and other incentives. On the first interview of the pilot panel operation, a choice of several gifts was offered to the respondent, at the discretion of the interviewer. According to reports received from the interviewers, the offer of a gift contributed noticeably to securing cooperation from about one-fifth of those interviewed.¹⁴⁷ In some instances interviewers also reported that the gifts influenced respondents to be more cooperative on subsequent interviews.

¹⁴⁷ Ferber, *Collecting Financial Data by Consumer Panel Techniques*, *op. cit.*, pp. 26-27.

At the beginning of the fourth wave of the same study, panel members were sent as a surprise gift a box of assorted cheeses and, somewhat later, a copy of a press release about the project. Due either to the cheese or to the press release, a clear improvement in cooperativeness was detected on that wave, as measured by interviewer ratings and by the frequency of data refusals.¹⁴⁸

On the other hand, offering cash to respondents was not at all successful in Study S5. In that study, half of the sample members were offered a \$10 payment by the interviewer. This offer appears to have boomeranged, since the response rate was actually lower among the paid group, with no appreciable reduction in response error.¹⁴⁹ Whether it was due to objections to a university offering money to the public or whether it was due to some other factor is not clear, but on future operations of the project no attempt was made to pay respondents.

On later studies, sample members were sent copies of interim reports and other project publications (excluding those relating to validation). On the basis of observation, the most effective of all appears to have been local newspaper releases. In various instances, photostats were prepared of these releases and sent to the sample members. Judging by interviewer reports, these newspaper stories were useful both in convincing sample members of the authenticity and value of the study and in maintaining their interest.

Attitudinal Questions

Attitudinal questions were used extensively in the initial studies of the project, primarily at the beginning of the questionnaire to "soften up" the respondent. This approach proved effective in many instances, but occasionally it was almost too effective.

Interviewers appeared to be spending so much time on the attitudinal questions that by the time requests were made for dollar balances and names of institutions, respondents were somewhat tired and were reluctant to keep on cooperating. As a result, two modifications were made in later questionnaires, in Studies P2 and P3. In Study P2, the attitudinal questions were interspersed among the requests for data, partly to cope with the aforementioned difficulty and partly to relieve the monotony of continual requests for data. To judge by interviewer reports, this technique was fairly successful, although no clear-cut evidence was obtained that its use produced information that would not otherwise have been obtained.

As a result, a second expedient was tried of reducing drastically the number of attitudinal questions. This procedure was followed in Study P3,

¹⁴⁸ *Ibid.*, pp. 27-30.

¹⁴⁹ Lansing, Ginsburg, and Braaten, *op. cit.*, Chapter VII.

on the first wave of which virtually no attitudinal information was requested. On the basis of observation, this procedure appears to have been successful, and very good cooperation was obtained on this wave (although, as was noted previously, many other factors were also involved). Later waves of Study P3, consequently, also sought very little attitudinal information. Such information would seem worth requesting only if desired for its own sake rather than to serve as a prelude for obtaining financial data.

Data Reliability

Regardless of which methods are used to collect financial data in a consumer survey, some means of evaluating the reliability of these data will be desired. The methods employed for this purpose in the present studies have been discussed in earlier chapters and are therefore covered here only briefly.

Interviewer Ratings

The use of interviewer ratings to gauge the reliability of the data has been covered to some extent in Chapter VI, in which a definite correlation was shown to exist between these ratings and the tendency of sample members to remain in the panel. These ratings were also shown to be related in varying degrees to both the reporting of the validated holding and to the accuracy of this report. The nature of the correlations was sometimes unexpected, however, as exemplified by the results shown in Chapter III where a higher correlation was obtained between the accuracy rating and nonreporting than that obtained between the completeness rating and nonreporting. There is some evidence that these ratings tended to be more reliable for interviewers who were most successful in picking up the validated debt. Presumably, the more effective interviewers were also more sensitive to the respondent's reaction to the request for financial data and were better able to gauge the true situation.

Evidence from other studies provides mixed support for the value of these ratings. Thus, in Study S2, validating car debt, interviewer judgments of the accuracy of response bore no relation to the true accuracy.¹⁵⁰ On the other hand, in Study S3, validating savings accounts, "The interviewers were most successful in telling whether the respondents failed to report the account completely."¹⁵¹

The value of interviewer ratings for detecting different forms of response errors is brought out in Table 81, which shows the proportions of members of different panels reporting validated holdings by different

¹⁵⁰ *Ibid.*, Chapter III.

¹⁵¹ *Ibid.*, p. 185. These results may have been affected by the possibility that some interviewers may have suspected that the particular sample contained only owners of savings accounts.

ratings of respondent cooperativeness. As is evident from this table, the frequency and accuracy of reporting of the validated holding for life insurance and savings accounts tend to increase sharply with the favorableness of the interviewer rating on several characteristics, although not so for debt. The accuracy and completeness ratings for the specific assets appear to be especially useful in this regard.

Use of Records

In some ways, information on the use of records by respondents is a more practicable indicator of data reliability than interviewer ratings, because of the greater objectivity of the former. The value of this indicator has been borne out in previous chapters where it was shown that for many of the validated holdings both completeness and accuracy of report were correlated positively with the use of records. Moreover, like interviewer ratings, use of records tends to reflect the propensity of sample members to remain in the panel (see p. 165).

Nevertheless, the full potentialities of a use-of-records variable remains to be explored. No attempt was made in these past studies to ascertain exactly what types of records were consulted nor to relate accuracy and completeness of report to these different types of records. Stratification of use of records in such ways may well improve the effectiveness of this indicator of data reliability.

Respondent Evaluation

A different and possibly novel approach is to request the respondent to provide an estimate of the accuracy of the data that he is supplying. This approach was tested in Studies P2 and P3, on which interviewers were instructed to ask for an estimate of the accuracy of dollar figures if records were not consulted. This request was made by showing the respondent a so-called "accuracy card," a copy of which is shown in the following diagram.

- a. Under \$500
- b. \$500–\$999
- c. \$1,000–\$4,999
- d. \$5,000–\$9,999
- e. \$10,000–\$14,999
- f. \$15,000–\$24,999
- g. \$25,000–\$49,999
- h. \$50,000–\$74,999
- i. \$75,000–\$99,999
- j. \$100,000 and over

Interviewers were instructed to use this card as follows: "Hand the card to the respondent and ask him to indicate which letter best describes how close the figure he gave is to the actual amount. Explain that we do not expect people to give exact figures from memory and that it is only natural to have some error when figures are given in this manner."¹⁵²

In terms of cooperativeness, this accuracy card was not difficult to administer, and few respondent refusals to use it were reported. For example, on Wave 2 of Study P2 when the accuracy card was first used, only one of 122 respondents asked to use this card in evaluating the accuracy of savings account balances refused to answer the question. The corresponding refusal rate for the face value of life insurance on that wave was 2 percent. On Wave 1 of Study P3, the refusal rate was zero for both savings accounts and life insurance. Considering the fact that the base for these percentages excludes the most cooperative sample members —

¹⁵² *Interviewer Guide for Farm Savings Panel*, p. 41.

TABLE 81. CROSS-TABULATION OF SELECTED INTERVIEWER RATINGS, BY ACCURACY AND COMPLETENESS OF REPORT OF VALIDATED HOLDING

Study	Characteristic rated	Rating	Percentage of respondents reporting holdings	Percentage of dollar figures accurate within 25 percent
Life insurance				
P1	Accuracy	Excellent	70	49
		Good	65	66
		Fair	58	50
		Poor or very poor	42	50
	Completeness	Excellent	70	49
		Good	64	61
		Fair	48	54
		Poor or very poor	50	62
P2	Accuracy on life insurance	Fully	55	88
		Fairly	60	62
		Not much, very little	25	33
	Completeness on life insurance	Fully	55	83
		Fairly	60	62
		Not much, very little	25	67
P3	Willingness to give financial information	Excellent	71	69
		Good	52	90
		Fair	60	78
		Poor or very poor	50	50

TABLE 81. (concluded)

Study	Characteristic rated	Rating	Percentage of respondents reporting holdings	Percentage of dollar figures accurate within 25 percent
Savings accounts				
P2.....	Accuracy on savings accounts	Fully	78	80
		Fairly	68	58
		Not much, very little	40	17
	Completeness on savings accounts	Fully	76	85
		Fairly	65	37
		Not much, very little	39	17
P3.....	Accuracy on savings accounts	Fully	34	78
		Fairly	23	33
		Not much, very little	24	0
	Completeness on savings accounts	Fully	30	69
		Fairly	24	0
		Not much, very little	33	0
Farm debt				
P3.....	Accuracy on debt	Fully	58	83
		Fairly	62	78
		Not much, very little	79	56
	Completeness on debt	Fully	61	94
		Fairly	61	58
		Not much, very little	70	80

those giving dollar balances from records—the feasibility of this accuracy-card approach would seem to be confirmed.¹⁵³

Given the feasibility of this approach, the basic question relates to its value. A partial answer to this question is provided in Table 82 in which the answers obtained from this accuracy card are cross-tabulated by accuracy of the data for savings accounts and life insurance for Studies P2 and P3 and for farm debt for Study P3.

As is evident from this table, the accuracy-card replies do provide information on the true accuracy of the data, although a noticeable tendency exists for the accuracy to be understated. Nevertheless, figures rated either exact or with a low margin of error are much more likely to possess high accuracy than those assigned larger margins of error. Figures re-

¹⁵³ This qualification need not be applied in the case of land and buildings, for which few farmers were in a position to consult records.

TABLE 82. COMPARISON OF ACCURACY CARD REPLIES AND TRUE ACCURACY, SELECTED ASSETS,
WAVES 2 AND 3, STUDIES P2 AND P3

Reply on accuracy card	True error, Study P2						True error, Study P3					
Savings account balance												
	Exact	Within \$10	Within \$50	Within \$100	Within \$500	Over \$500	Exact	Within \$10	Within \$50	Within \$100	Within \$500	Over \$500
Exact.....	1	2	1	1	..
Within \$10.....	2	7	7	3	4	4	1	1	1	..	1	..
Within \$50.....	..	2	6	3	3	3	1	3	2	1	1	..
Within \$100.....	2	3	2	7	1	..	1	1	1	..
Within \$500.....	5
Over \$500.....
Total.....	2	9	16	9	9	19	5	4	4	3	4	0 ^a
Life insurance face value												
	Exact	Within \$1,000	Over \$1,000	Exact	Within \$1,000	Over \$1,000						
Exact.....	12	4	3	58	2	8						
Within \$1,000.....	5	12	2	1						
Over \$1,000.....						
Total.....	17	4	3	70	4	9						
Farm debt, Study P3												
	Exact	Within \$100	Within \$500	Within \$1,000	Over \$1,000							
Exact.....	2	5	6	4	10							
Within \$100.....	1	9	10	5	14							
Within \$500.....	..	1	3	3	5							
Over \$500.....							
Total.....	3	15	19	12	29							

^a No errors of this magnitude among those using accuracy card.

ported with large margins of error are more subject to large error than figures reported with small margins of error, although instances are apparent of large error ranges being assigned to figures which are actually fully correct.¹⁵⁴ Clearly, if the tendency to overstate could be reduced, perhaps through a different form of question wording, the value of the accuracy card would be enhanced further. Even so, the relationships indicated in Table 82 would suggest further experimentation with this technique.

Personality Tests

Personality variables have been shown in previous chapters to be related to the accuracy with which data are reported. In addition, the analysis of the accuracy of the data by interviewer characteristics brings out certain personality traits which may be highly relevant to the reliability of the data.¹⁵⁵ Accordingly, an attempt was made to investigate the extent to which personality of the respondent might be related to data reliability. For this purpose, the *Edwards Personal Preference Schedule* was administered to respondents on Wave 5 of Study P2, the same test having been administered somewhat earlier to the interviewers in that study. Using these data, comparisons are shown in Table 83 between the accuracy and completeness of the validated savings accounts and the differences between the personality scores of the corresponding interviewers.

To judge by these data, a number of differences exist in the ability of interviewers with different personalities to obtain complete and accurate data on savings accounts. Most understandable is the greater tendency for the validated account to be reported, and for more accurate information to be obtained, when the interviewer is more dominant than the respondent. Order is another personality characteristic that appears to be more closely related to success in obtaining such information than interviewer skill. In this case, the more orderly is the respondent relative to the interviewer, the more likely is the validated account to be reported and to be reported accurately.

Interestingly enough, the data tend to bear out the danger of interviewers becoming too friendly with respondents, since less accurate and less complete information is obtained when the respondents have higher "succorance" scores, higher "nurturance" scores, or higher "affiliation" scores, any of which may indicate a tendency toward a "gabby" interview.

¹⁵⁴ Such instances are most frequent in the case of the face value of life insurance, where the amounts involved tend to be far larger than the error ranges used, so that the relative errors are still small. Moreover, as was noted in Chapter VI, the term "face value of life insurance" tends to be easily misunderstood.

¹⁵⁵ Steinkamp and Hauck, *ibid.*, Chapter III.

TABLE 83. COMPARISON OF AVERAGE OF CUMULATED RESPONDENT LESS INTERVIEWER SCORES ON EPPS AND ACCURACY AND COMPLETENESS OF VALIDATED ACCOUNTS, WAVE 5, STUDY P2

Characteristic	Validated account reported		Accuracy of reported account (of \$100 or more)	
	Yes	No	Within 10 percent	10 percent or more
Achievement.....	-1.1	-.1	-.9	-1.7
Deference.....	.3	-.4	.5	.3
Order.....	3.1	1.8	4.5	.6
Exhibition.....	-.8	-.4	1.1	.2
Autonomy.....	.6	-1.0	.3	1.1
Affiliation.....	.2	1.9	-.4	1.3
Intracception.....	-2.8	-2.1	-2.1	-4.2
Succorance.....	3.2	4.5	2.8	4.3
Dominance.....	-2.5	0	-3.2	-1.4
Abasement.....	3.3	2.6	3.7	2.0
Nurturance.....	1.2	.7	.8	1.5
Change.....	-3.3	-3.3	-3.2	-3.1
Endurance.....	1.2	.3	2.2	-.1
Heterosexuality.....	-3.2	-4.1	-4.8	-.4
Aggression.....	.4	.8	.8	-.9

Such characteristics as aggression, achievement, and autonomy do not appear to have much relation to the reliability of the data.¹⁵⁶

Asking Respondents for Validation Permission

By far the most useful means of evaluating the reliability of survey data is to obtain the corresponding true figures from external sources. Doing so by selecting the sample from the records of an external source is conceptually the best approach. Another way of doing so, however, is to attempt to obtain this information through the cooperation of the respondent by asking him for written permission to contact the institutions from which corresponding reports of his asset and debt holdings can be obtained.

This approach was tried on Wave 5 of Study P1 and on Wave 3 of Study P2, in both cases with regard to savings account balances. In each case, respondents were asked to sign a form requesting their savings institutions to provide by mail to the project headquarters the balance in their accounts as of the date of the interview. The basis for the request was the assertion that considerable uncertainty existed regarding the extent to which passbook balances coincided with records of the savings institution

¹⁵⁶ Most puzzling of all is the suggestion in Table 83 that accounts are reported with much greater accuracy when the interviewer achieves a higher score on "heterosexuality" than the respondent!

because of the possibility of unrecorded deposits, accumulated interest, transactions on the date of the interview, or balances representing holdings of more than just the passbook holder(s).

The results were mixed. On Study P1, 62 percent of 131 panel members approached with this request signed the form, whereas on Study P2 somewhat fewer panel members cooperated (47 percent). Those who refused to sign the requested form did include more nonreporters, but refusal to sign it did not necessarily imply refusal to provide financial data. This is brought out in the following tabulation relating to Study P2.

	<i>Asked to sign requested form</i>	
	<i>Signed</i>	<i>Did not sign</i>
Reported account.	75%	60%
Did not report account.	25	40

This approach may also provide useful information on an individual basis. Comparison of dollar balances of the respondents with corresponding balances obtained from the institutions revealed a fair number of discrepancies (Table 84). Since the respondent was fully aware of the validation procedure, a request for clarification could be (and was) made to the respondent or to the institution, so that the reliability of these reports could be improved substantially. The fact remains, however, that as with the accuracy-card approach, this procedure provides only a partial solution to the problem, since by definition it is not applicable if no holdings are reported.

Primary and Secondary Validation

From an analytical point of view, evaluation of respondent reports — whether by selecting the sample from the institutions or by secondary validation — is an ideal approach to data reliability. Since its feasibility has already been considered extensively, there is no need for further discussion. It should be stressed, however, that secondary validation possesses the disadvantage of being unable to detect complete nonreporting. It would be most useful if names of sample members could be circulated to all institutions of a given type in a particular area (as was apparently done in another financial study). This procedure would still permit population probability sampling, and at the same time it would greatly minimize the danger of overlooking nonreported holdings.

Reducing Residual Nonsampling Variance

Under the circumstances, if the overall efficiency of sample surveys is to be increased, the reduction of nonsampling errors merits primary consideration. To a certain extent, such reduction may be accomplished

TABLE 84. COMPARISON OF RESPONDENT REPORTS OF SAVINGS ACCOUNT BALANCES AND INSTITUTIONAL BALANCES,
BY RESPONDENT VERIFICATION METHOD, WAVE 5, STUDY P1, AND WAVE 3, STUDY P2

Respondent reported balance	Institutional balance									Total
	Under \$100	\$100 to \$499	\$500 to \$999	\$1,000 to \$1,499	\$1,500 to \$2,499	\$2,500 to \$4,999	\$5,000 to \$7,499	\$7,500 to \$9,999	\$10,000 or more	
Study P2										
Under \$100.....	11	11
\$100 to \$499.....	2	23	1	1	27
\$500 to \$999.....	..	2	9	..	1	12
\$1,000 to \$1,499.....	..	1	3	9	13
\$1,500 to \$2,499.....	1	1	9	11
\$2,500 to \$4,999.....	1	16	17
\$5,000 to \$7,499.....	1	..	2	8	1	1	13
\$7,500 to \$9,999.....	1	4	..	5
\$10,000 or more.....	2	2
Total.....	13	26	14	11	11	19	9	5	3	111
Study P1										
Under \$100.....	17	1	18
\$100 to \$499.....	..	30	8	30
\$500 to \$999.....	1	..	1	9
\$1,000 to \$1,499.....	..	1	..	4	6
\$1,500 to \$2,499.....	5	5
\$2,500 to \$4,999.....	1	2	5	8
\$5,000 to \$7,499.....	1	3	1	..	5
\$7,500 to \$9,999.....	4	..	4
\$10,000 or more.....
Total.....	17	32	9	5	8	5	4	5	..	85

through the improvement of survey procedures. Yet, such improvements can only go so far, and if the findings presented in this volume are valid, a substantial residual nonsampling variance is likely to remain in most survey operations. Essentially, the reason for this derives from the fact that the inducement to cooperate on any survey depends not only on the subject of the survey and the manner in which it is being conducted but also on the personality and background of the sample member as well as on the personal and physical circumstances in which the interview is attempted. These latter sets of variables may give rise to either response or nonresponse errors, as has been illustrated in previous chapters.

Moreover, response and nonresponse errors subject to survey control in theory may not always be so in practice. Thus, skepticism of the value of a study or doubts about the confidentiality of the data can presumably be resolved in each particular case by a proper survey approach. However, the nature of this "proper survey approach" will vary from one sample member to another, so the interviewer is hardly likely to select always "the" approach for dealing with each case. For example, if an interviewer attempts to induce an advocate of *laissez-faire* to cooperate because the data will be useful for government economic planning, he may be rebuffed, even though the same approach will be effective on other people.

As a result, nonsampling field errors may be said to be composed in practice of errors that could be and were avoided by proper survey methods, of errors related to the survey methods but which were not avoided, and of errors outside of survey control. Clearly, improved survey procedures help to limit the second type of error. At the same time, it seems equally clear that errors of the second type will occur and that errors of the third type can not be handled by the usual survey procedures. For such errors, something new must be added.

The nature of this new ingredient becomes evident when we consider that in the main such errors are not random but occur in certain systematic ways. These systematic ways reflect the factors mentioned previously that cause such errors—personality of the sample member, background, circumstances of the interview, and so forth. On the other hand, in practice it is hardly feasible to distinguish between the different types of errors, in part because of lack of data and in part because errors presumably within survey control also tend not to be random. These errors may also be related to a respondent's characteristics, such as respondent misunderstanding arising from interaction between the use of technical terms and low educational background, as well as to survey conditions.

Furthermore, since these errors are also systematic, to the extent that they exist there is a need for their detection and correction as much as there is for errors outside of survey control. In other words, the need is

for a means of identifying response and nonresponse errors as they occur and relating such errors to relevant characteristics of the sample member, of the interviewer, and of the interview situation. If such relationships can be derived and if they are reasonably stable, a basis then exists for adjusting survey data for the effects of such errors.

The derivation of such relationships within a general analytical framework points toward a second major direction which future research on nonsampling errors needs to follow. For if this variance cannot be reduced through better survey procedures, other procedures must be introduced to cope with the problem, and the most promising of such other procedures would seem to be the combination of external information with survey data through the use of multivariate analysis. Different ways by which this combination may be achieved are being explored. This is not the place to describe these methods in detail; indeed, much more work is needed before this can be done. It would seem appropriate here, however, to outline the methods that are being considered.

Alternative Approaches

Two alternative approaches are being investigated for transferring information on nonsampling errors from a validation sample to a population probability sample. Both of these approaches are predicated on the selection of two largely replicating samples from the population being studied: Sample 1 (S_1), representing a probability sample of consumer units from the general population, and Sample 2 (S_2), a probability selection of consumer patrons of cooperating financial institutions, the latter preferably also selected by probability methods. Identical questionnaires and interviewing methods are employed on both samples. As a result, the validation data applied to S_2 yield the errors in reports of the members of S_2 , as well as information not obtained from the refusals and the nonrespondents in S_2 . The problem then becomes one of making use of this information to correct the reports of the members of S_1 for both response and nonresponse errors.

One approach is what may be called the "error-determinant" model, or Model A. It is based on determining, with the aid of the S_2 data, the factors that account for response errors and nonresponse bias and making use of these results to detect and correct corresponding errors in S_1 . Since previous chapters have shown the most important form of response error to be nonreporting of holdings, the effectiveness of this model will rest primarily on its ability to pinpoint nonreporters in S_1 . Once nonreporters in S_1 are identified, nonowners of the given asset in S_1 can be obtained as a residual — as those sample members who reported not owning that asset and who were not identified as nonreporters.

The second approach, Model B, is an "owner-determinant" model. This model seeks to cope directly with the basic structural characteristic that differentiates S_1 from S_2 , namely, the fact that by definition all the members of S_2 own the particular asset, whereas this is not true of all of the members of S_1 . Consequently, the rationale underlying Model B is to apply analytical methods to the validation data, in conjunction with data for the general population, to obtain a means of discriminating between owners and nonowners in S_1 . With the aid of such methods, the members of S_1 are classified as owners or nonowners of the particular asset (or, alternatively, as each being an owner with a given probability). Non-reporters are then obtained as a residual — as those members of S_1 classified as owners by the analytical technique but who deny holding that asset.

Each model has advantages and disadvantages. The error-determinant model requires the derivation of a set of functions relating response errors to the characteristics of the respondent, interviewer, and interview situation. It also requires a means of distinguishing nonreporters in S_1 from true nonowners. On the other hand, the technique possesses the advantage of not requiring any data other than those collected in the interviews.

The owner-determinant model requires a technique for discriminating between owners and nonowners of the particular asset, a basic structural characteristic of the population. (It might be argued that if such a relationship could be found, much of the basic problem would no longer exist.) For obtaining such a relationship, the model requires external population data comparable with the survey data. Moreover, it requires an independent estimate of the aggregate number of owners of that asset in the population.

Estimation Procedures

These two models are being tested in various ways. Two variations of the error-determinant model have been formed. One variation consists of a set of five equations which attempts to identify and correct response errors classified into three categories: nonreporting, reporting of ownership but refusal of amount, and reporting of amount but with unknown error. A second form compresses this model into two equations, with one equation seeking to pinpoint those sample members making errors of more than a specified proportion and a second equation correcting for this error, given its existence.

Both of these variations of the error-determinant model are being tested in three ways. One way is by seeking standard discriminant or multiple regression relationships, classifying sample members as falling into one category or the other and then assigning amounts with the aid of other functions, as required. The second approach is to use the same

estimation methods as with the first approach, but in the classification of reporters-nonreporters and of errors-nonerrors to assign each sample member a probability of being a nonreporter (or reporting with error), and to use these probabilities as a basis for distributing amounts in the various size-of-holding classes. The third approach is to use probabilities rather than regression methods for pinpointing nonreporters and respondents reporting with error. This approach requires the construction of a tree diagram instead of a regression function, which has the advantage of maximum flexibility in forming categories in introducing interactions but which uses up data very quickly.

The owner-determinant model is being tested by the use of tree diagrams, since data are not available for deriving regression functions to segregate owners from nonowners.

The error-determinant model: Variant A. This variant is the five-equation model which attempts to detect response errors classified into three categories. The model can be summarized briefly, using the following notation:

D is a discrepancy of any kind between the true figure and reported figure,

E is an error in the amount reported by the respondent,

H is a true holder of the particular asset,

R' is a nonreporter (R , a reporter),

W is a respondent acknowledging ownership of an asset but refusing the amount,

Z is a set of explanatory variables relating to the characteristics of the respondent, interviewer, and interview situation,

l is a likelihood of a particular amount, and

s is a dollar amount.

With these definitions, the five-equation model is as follows:

Correction for nonreporters:

$$(1) \ l(R') = f_1(Z_i), \text{ and}$$

$$(2) \ s(R') = f_2(Z_i).$$

Amount withheld of acknowledged holding:

$$(3) \ s(W) = f_3(Z_i).$$

Correction for errors in reported amounts:

$$(4) \ l(E) = f_4(Z_i), \text{ and}$$

$$(5) \ s(E) = f_5(Z_i).$$

The parameters of these equations are to be estimated from the validation data, so that in all instances the equations contain conditional

dependent variables, namely, variables given that the respondent does own that asset. Therefore, when these equations are applied to an S_1 sample, Function (1) has to be adjusted by a separately determined probability that the nonreporter in S_1 really holds some of that asset and is not telling the truth when he reports zero holdings. In effect, for S_1 , Function (1) becomes

$$(1') l(R') = l(H) \cdot l(R'|H),$$

which assumes independence between the two relations.

Two approaches to estimating $l(H)$ are suggested. One approach is to seek an independent function, in the form of a tree diagram, expressing the relative frequency of ownership of the particular asset in the population for different combinations of relevant demographic and other variables.

The second approach rests on obtaining an independent estimate of the number of nonreported accounts in S_1 , using S_2 data. If p is the proportion of all family units in S_2 not reporting one or more validated accounts, the proportion of respondents in S_1 not reporting validated or nonvalidated accounts may be estimated as $p(1+k)$, where k is a fraction between zero and one reflecting the extent to which families other than those not reporting validated accounts may not have reported nonvalidated accounts. (Cross-validation data from prior panel operations should be of value in specifying reasonable limits for k .) The number of nonreporters in S_1 as a proportion of reporters is, then:

$$x = \frac{p(1+k)}{1-p(1+k)}.$$

Function (1) would be applied to S_1 , yielding estimated values of the dependent variable for each respondent. These estimates would be arrayed in descending order and the top Nx respondents designated as nonreporters, where N is the number of sample members in S_1 .

Variant B. This approach seeks to simplify the five-equation model into three equations. One equation represents the likelihood of any discrepancy whatsoever (including nonreporting), a second equation estimates the true amount (given that a discrepancy exists), and a third equation estimates amounts refused — this is Function (3) once more. The first two equations can be represented as follows:

$$(6) l(D) = f_6(Z_i), \text{ and}$$

$$(7) s(D) = f_7(Z_i).$$

The problem of correction for true nonownership in S_1 is the same as before, and could presumably be handled by the same methods.

Estimation of function parameters. Standard multiple regression methods would be used in all instances to estimate the parameters of the func-

tions containing a continuous dependent variable — Functions (2), (3), (5), and (7). The alternative procedures enter into the estimation of the likelihood functions. These alternatives are as follows:

The first is to estimate the parameters of these functions by the least-squares method, establish a cut-off point as

$$l_o = \frac{l(R) + l(R')}{2},$$

and classify respondent i as a nonreporter if l_i is less than l_o and as a reporter if l_i exceeds l_o . For the nonreporters so classified, use Function (2) to obtain an estimate of the amount not reported, which is then assigned to the sample member.

The second alternative is to estimate the parameters of these functions by the least-squares method. Assign to each observation a probability of being a nonreporter based on the following relation:

$$p(R' | l_1 < l < l_2) = \frac{\int_{l_1}^{l_2} p(l) v(l) dl}{\int_{l_1}^{l_2} v(l) dl},$$

where $p(l)$ is the probability of a respondent with the value l saying that he does not hold that asset (given that he does hold it) and $v(l)$ is the proportion of people with estimated values between the two limits, l_1 and l_2 . After probabilities of nonreporting are assigned, an estimated amount not reported is obtained from Function (2) for each respondent, as well as a standard deviation of this estimate. The resulting amount is allocated in a size-of-holding distribution using these probabilities on the basis of a lognormal distribution; this distribution would seem to approximate the true distribution of holdings better than a normal distribution. (Howard Raiffa has also suggested a means of using these data for regression analysis, although the exact details of the method have yet to be worked out.)

The third approach calls for the construction of tree diagrams rather than estimating the parameters of the functions by regression methods. Briefly, the approach involves selecting the most relevant variables felt to influence nonreporting, setting up meaningful categories of each variable, and combining the categories for different variables to obtain the maximum discrimination between cells for the relative frequency of non-reporters.

The owner-determinant model. This model seeks to discriminate first between owners and nonowners in S_1 and then obtain nonreporters as a residual. Because of the data problems associated with the model, its specification is best outlined by a series of steps, as follows:

(1) Estimate the total number of savings units owning the particular asset in the population.

(2) Using the S_2 data, obtain distributions of the number of owners of that asset in the population for whatever combinations of characteristics are deemed most relevant for segregating owners from nonowners.

(3) By relating these distributions to comparable census or other external data, obtain relative frequencies of ownership of that asset by population characteristics, in the form of a tree diagram.

(4) Use the resulting tree diagram to segregate owners from non-owners in S_1 . As with the error-determinant model, this classification could be made on a one-zero basis or by assigning each respondent a designated probability of ownership.

(5) Identify nonreporters in S_1 as those who have reported no holdings *and* are labeled as owners on the basis of the tree diagram.

(6) Estimate the amount not reported by using Function (2).

Undoubtedly other forms of model specification and estimation procedures will be developed with further work. The forms outlined here are not meant to be definitive but are intended instead to indicate the types of models that are being considered.

Summary Comments

The observations and experiments reported in this chapter provide evidence that improvements which could lead to considerable increases in data reliability are possible in current survey procedures. Judging by the state of the art today, the largest potentials for such improvement would seem to lie in interviewer selection and supervision and in devising means of evaluating the reliability with which data are collected. Little has been accomplished in the latter connection; in the former respect, much remains to be done despite the widespread attention interviewer selection and supervision have received in the past. In both these and other areas, the experiments described in this chapter—although exploratory in nature—indicate directions in which future gains may be substantial, gains which would be applicable far beyond the scope of consumer financial surveys.

Perhaps the largest source of improvement, however, lies in the integration of external data with the survey data. Some methods for doing this have been outlined, but work on such procedures is as yet in its initial stages.

In closing this chapter, the exploratory nature of the experiments carried out should be stressed. Their primary objective was not to obtain definitive results but to investigate the feasibility of different means of

improving survey procedures. Because of the limited scope of the operations, many of the ideas had to be tested on an *ad hoc* basis. Even when controlled experiments were conducted, their restricted scale led to results that are primarily suggestive. The full value of these alternative methods can only be established by large-scale tests.

VIII. SUMMARY AND CONCLUSIONS

This final chapter is divided into three parts. The first part presents a general overview of the principal findings in this volume. The significance of these findings to surveys other than those relating to consumer finances is discussed in the second part of the chapter. Suggested future approaches to the subject of nonsampling errors are discussed in the final part, which also presents recommendations for further research.

Summary of Results

At the outset, ways in which nonsampling errors can arise in consumer financial surveys were outlined, and a framework was provided of the effects that such errors might have on survey data. Among other things, it was stressed that in theory nonsampling errors may affect the reliability of survey data as well as the accuracy of such data, and the former effect was shown to exist even when sample estimates of population parameters were unbiased. It was also noted that there need be no correlation between the two types of errors.

The empirical studies presented in the later chapters indicate in striking fashion that nonsampling errors are not simply a matter of theory, but do in fact exist and are mainly responsible for the pronounced tendency of survey data to understate aggregates, as was noted in Chapter II. Not only was this bias present in the survey data, but in many instances the contribution of nonsampling errors to the total variance in the data was so large as to render meaningless confidence intervals computed by the usual statistical formulas. As was brought out in Chapters III and IV, the magnitude of this type of error tends, if anything, to increase with sample size, because such increases tend to reduce the sampling variance while having no effect on the nonsampling variance.

The magnitude and relative importance of these nonsampling errors were seen to vary substantially with the type of financial holding and with the type of data sought about that holding. In general, holdings considered more confidential than others tended to be subject to larger errors

than other holdings. Much larger biases and larger relative nonsampling variances were obtained for savings accounts and personal loans than for checking accounts and automobile debt. With the latter type of holdings, the principal effect of nonsampling errors appeared to be on the reliability of the estimates rather than on the accuracy of the estimates. Particularly interesting is the finding that roughly three-fourths of the variance due to discrepancies in checking account balances was due to nonsampling errors rather than to the frequently cited problem of check-float.

The analysis of the panel data in Chapter VI showed that nonsampling errors tend to persist throughout a survey operation, although their relative influence declines somewhat over time. Respondent rapport was shown to improve over time. Hence, although appreciable mortality was experienced on these panel operations, the improved accuracy of those who did remain in the panel more than offset the increased atypicalness of the panel. As a result, aggregate data obtained from the panel became more accurate over time, despite the increasing lack of representativeness.

How do nonsampling errors arise? A simple segmentation technique employed in these chapters showed that by far the principal cause of these errors was the failure of the sample member to acknowledge ownership of a particular asset or debt. Reinforcing this phenomenon was a tendency for nonrespondents and for those who refused to give dollar figures to have larger holdings than other sample members. In contrast, when holdings were reported and balances given, the figures were likely, if anything, to be overstated.

Various experiments seeking to cope with response and nonresponse errors through the improvement of survey procedures were considered in the preceding chapter, and the results of a number of tests designed to explore the practicability of such improvements were presented. As was brought out in that chapter, the main potential for improvement of survey techniques would seem to lie in better interviewer control and in seeking means of measuring the reliability with which data are reported. In the latter connection, the validation technique was once more seen to offer considerable promise. The same methodology can serve, on the one hand, as a basis for minimizing the occurrence of nonsampling errors and, on the other hand, as a basis for detecting and correcting such errors as they occur.

Significance for Other Types of Surveys

The applicability of the present findings to other types of surveys depends to a large measure on the similarity of the data sought in these surveys to consumer financial data. Indeed, the type of survey is not so important as the type of data being collected. In effect, two types of data may be distinguished — factual data and nonfactual data, the latter cate-

gory comprising principally attitudes and opinions. The applicability of the present findings to these two types of survey data may be considered with regard to each of the three principal areas in which the findings may be classified: the use of validation techniques, the measurement of non-sampling errors, and the improvement of survey procedures.

Use of Validation Techniques

For validation techniques to be applicable to survey data, two conditions have to be fulfilled. First, a true answer must exist in an operational sense. Second, a means must be available of ascertaining this true answer so that the respondent report can be compared with it.

These two conditions would seem to be fulfilled for most types of factual data, although at times ingenuity may be required to construct the answer from external sources of information. For many data on economic behavior, such as employment status, labor-force participation, and prices paid for goods or services, corroborative data can be obtained from the other party to the transaction, e.g., the employer or the merchant. This is also possible for expenditure data to the extent that people maintain charge accounts or their purchases are reported to state agencies (as in the case of a house or an automobile) or to manufacturers for warranty purposes. For other types of expenditures, different approaches may be required, ranging from observation of shopping behavior to the inclusion, in a panel operation, of internal consistency checks.

Much the same situation holds with regard to factual data of a non-economic nature. Thus, validation data have been used to check the reliability of consumer responses on illnesses and hospital visits.¹⁵⁷ Similar methods could be applied to personal characteristics such as age and education and to a wide variety of other behavioral phenomena ranging from magazine readership to job mobility. In general, validation methods would seem feasible with any type of data for which some record of a transaction is kept by a party other than the consumer. This other party will usually be a business firm or a governmental agency, although in some situations the other party may be another consumer.

The applicability of validation techniques to nonfactual data is much more difficult but may not be impossible. The primary problem in such a case is likely to be the formulation of an operational definition of a true answer. The basis for such a definition must clearly be some sort of objective information relating to the expressed opinion or attitude. Such information may be obtainable if the opinion or attitude is reflected in

¹⁵⁷ United States National Health Survey, *Comparison of Hospitalization in Three Survey Procedures*, Public Health Service Publication No. 584 (Washington: U. S. Department of Health, Education and Welfare, 1963).

past and current behavior. For example, if a business executive reports that his firm expects higher sales in the next three months, a partial check for this statement could be based on the firm's production plans. If a consumer indicates a preference for custom-made clothes, some idea of the meaningfulness of this preference can be obtained by ascertaining what sort of clothes have been purchased in the past.

In still other instances, when no form of factual check can be made, an approximation to a true answer may sometimes be obtained by means of consistency checks. Thus, if a public figure expresses sympathy for the labor movement, this sentiment can be compared with past statements that he might have made on the same subject. To be sure, consistency does not assure validity, but in many instances the two concepts are close enough to be considered interchangeable.

Although the applicability of validation techniques to nonfactual data may not be always possible, such techniques can be applied at times even to such data, if sufficient advance thought is given to the planning of the survey. The more likely is the subject under study to bring about non-sampling errors, the more thought should be given to this question.

Measurement of Nonsampling Errors

The practicality of the formulas for the sampling and nonsampling variances presented in Chapter I depends to a large extent on the availability of validation data. Yet, even when validation data are not available, estimates of nonsampling variance can often be obtained through proper sample design, as has been illustrated in past studies attempting to measure interviewer variability.¹⁵⁸ Techniques of the latter type would seem to be virtually essential for some types of nonfactual data.

Regardless of the methods used, the findings in this volume tend to underscore the need for measuring these nonsampling errors. These findings suggest that a low level of bias in an aggregative sense does not necessarily imply that nonsampling errors are of negligible importance. Rather, consideration must be given to the possible presence of nonsampling errors in individual observations and to the effect of these errors on the reliability of the sample data.

The present findings suggest that the influence of these nonsampling errors tends to increase with the sensitivity of the information being sought.

¹⁵⁸ For example, R. Franzen and R. J. Williams, "A Method for Measuring Error Due to Variances Among Interviewers," *Public Opinion Quarterly*, Vol. 20, No. 3 (Fall, 1956), pp. 587-92; W. F. F. Kemsley, "Interviewer Variability and a Budget Survey," *Applied Statistics*, Vol. 9, No. 2 (June, 1960), pp. 122-28; and Leslie Kish, "Studies of Interviewer Variance for Attitudinal Variables," *Journal of the American Statistical Association*, Vol. 57, No. 297 (March, 1962), pp. 92-115.

There is little reason to doubt that the same relationship will be found to exist for other types of data as well, including material relating to opinions and attitudes. However, sensitivity may not be the only variable related to the incidence of nonsampling errors. Other variables undoubtedly exist, relating to the adequacy of interviewer training, the nature of the population being studied, the organization of the questionnaire, and other aspects of the survey operation. The influence of these other variables may well vary with the type of information being sought.

Improvement of Survey Procedures

The need for improving survey procedures has been widely recognized, and many of the ideas explored in Chapter VII were taken from experiences with other types of survey data. For this reason, most findings in that chapter would be applicable directly to other types of surveys. This is particularly true of the findings relating to the data-collection method, interviewer training and compensation, and the measurement of data reliability. In the latter respect, the use of records and interviewer ratings of reliability of information would seem to offer considerable promise for improving the reliability of factual data collected on future surveys. To a more limited extent, interviewer ratings may also be of use in evaluating the reliability of nonfactual data, such as by throwing light on the degree of thought given to particular replies.

In certain other respects, the findings in Chapter VII will be of more restricted applicability. Thus, the apparent lack of influence of monetary gifts offered by universities does not mean that the same procedure may not be considerably more successful when data are collected by a commercial organization.¹⁵⁹ Asking for holdings or asking for change may be a crucial factor in determining the reliability with which financial data are recorded by consumers, but it may be of little consequence with regard to such data as prices or occupation.

The effects of a particular approach are bound to vary substantially with the nature of the survey, but the possible sources of nonsampling error are likely to be much the same in any survey. For this reason, any improvements that can be effected in one type of survey are bound to have at least limited applicability to other survey operations as well. At the very least, these results should serve as the basis for experimentation in other types of surveys.

¹⁵⁹ It should be noted, however, that a recent study of gifts and incentives offered commercially suggests that they may have little effect on the reliability of the data, within a wide range of offers. See, Seymour Sudman, "On the Accuracy of Recording of Consumer Panels," unpublished Ph.D. dissertation, University of Chicago, 1963.

All things considered, it is clear that improvement of survey procedures may not be sufficient to cope with the problem of nonsampling errors in many types of surveys; indeed, it may not even be the most effective way of doing so. Perhaps most promising of all would seem to be the second approach of seeking ways of integrating survey data with related information obtained from other sources. In this respect, the validation data are ideal, and some means of using them in conjunction with survey data were outlined in Chapter VII. The methods presented there are undoubtedly not the only ways that may be developed but, at least on the basis of preliminary results, they would seem to offer considerable promise for the detection and correction of nonsampling errors. This is especially true because the results indicate the methods to be most efficient in detecting nonreporters, the principal source of nonsampling errors in most cases.

Concluding Comments

If the findings of this project are any indication, increasing attention must be given to the detection and correction of nonsampling errors. Such attention will be needed particularly in the conduct of large-scale, well-designed probability samples, for as the efficiency of a sample design increases and the size of sampling variances decreases, the effect of nonsampling errors becomes progressively more important. Since nonsampling variances are virtually unaffected by sample size, we are faced with the paradoxical situation that the more efficient is the sample design, the more important are nonsampling errors likely to be and the more meaningless are confidence interval computations based on the usual error formulas.

To be sure, the relative importance of nonsampling errors will vary with the nature and subject matter of the particular survey. Yet, there is little reason to doubt that on many types of surveys, especially those on which numerical data are being collected, the effect of such errors will be substantial and may seriously distort estimates obtained by failing to allow for them.

Under the circumstances, much future work on survey methods needs to be directed toward means of coping with such nonsampling errors. In this respect, attention should be given, not only to the improvement of survey procedures, but also to the use of external information in conjunction with survey data, as has already been discussed. Indeed, research on behavioral science surveys has led to the proposal that virtually every such survey be accompanied by a validation study to serve as a basis for detection and correction of nonsampling errors.¹⁶⁰

¹⁶⁰ W. G. Madow, "On Some Aspects of Response Error Measurement," *Proceedings of the Social Statistics Section of the American Statistical Association* (Washington: American Statistical Association, 1965), pp. 182-92.

To many people, this proposal will seem extreme, but the fact remains that until such validation surveys are incorporated in survey designs there is no way of gauging the true extent of nonsampling errors. To be sure, such validation surveys will add considerably to the cost of a survey operation, but the result in terms of efficiency may well be worth this additional cost.

APPENDIXES

APPENDIX A

BRIEF ADVANCE LETTER, WAVE 1, STUDY P1

Dear Mr.

The purpose of this letter is to ask for your cooperation in a nationwide study on how people handle their finances. Possibly you have read or heard about the operations of this project in other cities. If not, the enclosed copy of an editorial from the New York Times will provide you with a general description.

The study is sponsored by our Committee and other nonprofit organizations. It has no connection with any business firms or government organizations, as Mr. _____, Chancellor of _____ University, can affirm.

A major aspect of this work is being carried out with a cross-section of families in the _____ area, through interviews with residents at addresses selected by statistical sampling from the population at large. Your name was obtained as the resident of one of the addresses we chose. Because you have been selected in this manner, we are most anxious to secure your cooperation.

Within a few days, Mr. _____ of our staff will call on you. The information you give will be kept in the strictest confidence. We hope that we may count on your support.

Sincerely yours,

Robert Ferber

Enc.

LONGER ADVANCE LETTER, WAVE 1, STUDY P1

Dear Mr.

The purpose of this letter is to ask for your cooperation in a nationwide study on how people handle their finances. Possibly you have read or heard about the operations of this project in other cities. If not, the enclosed copy of an editorial from the New York Times will provide you with a general description.

Essentially the aim of the project is to gain a better understanding of the manner in which changes in people's finances affect the ups and downs of the economy. The absence of such information constitutes one of the largest gaps in our present economic knowledge. If such information were available, it would constitute a major step forward in the control of business fluctuations.

To obtain the necessary information is a primary objective of this study, which is sponsored by our Committee and other nonprofit organizations. The study has no connection with any business firms or government organizations, as Mr. _____, Chancellor of _____ University, can affirm.

A major aspect of this work is being carried out with a cross-section of families in the _____ area, through interviews with residents at addresses selected by statistical sampling from the population at large. Your name was obtained as the resident of one of the addresses we chose. Because you have been selected in this manner, we are most anxious to secure your cooperation.

Within a few days, Mr. _____ of our staff will call on you. The information you give will be kept in the strictest confidence. We hope that we may count on your support.

Sincerely yours,

Robert Ferber

Enc.

ADVANCE LETTER WITH STRAIGHTFORWARD APPEAL
WAVE 1, STUDY P2

Dear Mr.

I am writing you on behalf of our Inter-University Committee to request your assistance in a nationwide study we are conducting on how people handle their finances. Possibly you have read newspaper accounts of this study.

This research study is sponsored by our Committee with the assistance of the Ford Foundation and other nonprofit organizations. It has no connection with any business firms or government organizations, as you may verify by contacting the _____ Better Business Bureau or the office of Dr. _____, President of _____ University.

The study is conducted by interviewing families in various parts of the country to obtain information on their attitudes toward different forms of saving and on their money-handling practices. The families interviewed are selected by probability methods from lists of the population in the area.

Your name happens to be one of those selected. Because you have been selected in this manner we are most anxious to secure your cooperation. Within a few days, Mrs. _____, who is assisting us in this study, will call on you to obtain some of the needed data. The information you give will be kept in the strictest confidence; published results of the study relate to groups of families, never to an individual family.

We hope that we may count on your support.

Sincerely,

Robert Ferber

ADVANCE LETTER SEEKING RESPONDENT SUGGESTIONS
WAVE 1, STUDY P2

Dear Mr.

I am writing you on behalf of our Inter-University Committee to request your assistance in a nationwide study we are conducting on how people handle their finances. Possibly you have read newspaper accounts of this study.

This research study is sponsored by our Committee with the assistance of the Ford Foundation and other nonprofit organizations. It has no connection with any business firms or government organizations, as you may verify by contacting the _____ Better Business Bureau or the office of Dr. _____, President of _____ University.

The study is conducted by interviewing families in various parts of the country to obtain information on their attitudes toward different forms of saving and on their money-handling practices. The families interviewed are selected by probability methods from lists of the population in the area.

Your name happens to be one of those selected. Because you have been selected in this manner we are most anxious to secure your cooperation. The nature of our request is rather unusual in that we seek not only information but also advice. A primary aim of this study is to evaluate and improve current methods of obtaining such data from people like yourself. Accordingly, we should like to have your help in advising us how well you feel that our questions succeed in securing the information. Also we should like to have your suggestions on what improvements might be considered in the questions and interviewing approach.

Within a few days, Mrs. _____, who is assisting us in this study, will call on you to obtain some of the needed data. The information you give will be kept in the strictest confidence; published results of the study relate to groups of families, never to an individual family.

We hope that we may count on your support.

Sincerely,

Robert Ferber

No. _____
Tract _____

INTERVIEWER REPORT FORM

Int. _____
Form _____

1. CONTACT REPORT

When Date Day Time	How Phone Pers.	Where		Family member(s) talked to	Results of Contact			Time Spent in Contact	Time Spent in Editing
		Home Off.	(explain)		Not	Too	Other		
_____	<input type="checkbox"/> _____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____
_____	<input type="checkbox"/> _____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____
_____	<input type="checkbox"/> _____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____
_____	<input type="checkbox"/> _____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____

If NON-CONTACT: Please explain circumstances:

- ☐ House vacant
- ☐ No such address
- ☐ Death or illness in family
- ☐ Person to be interviewed out of town
- ☐ Other (explain) _____

If REFUSAL: please give reason(s) in detail. _____

Page 2 -- IRF

2. CLASSIFICATION DATA (Fill in as complete as possible, even for refusals and non-contacts)

Type of dwelling: (Check one and fill in data below)

☐ House

☐ Apartment

Approx. value \$ _____

Approx. monthly rentals \$ _____

Condition of exterior _____

Condition of interior _____

Condition of interior _____

Approximate age of main wage earner _____

Occupation of main wage earner _____

Size of family _____

Nationality _____

Race _____

3. ATTITUDE OF PANEL MEMBER

- a. How would you describe the panel member's attitude in each of the following respects: (Check below)

	<u>Excel- lent</u>	<u>Good</u>	<u>Fair</u>	<u>Poor</u>	<u>Very poor</u>	<u>Interviewer comments</u>
Cooperativeness	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
Accuracy of information given (Exact figures for items requested)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
Completeness of information (All assets and liabilities)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____

- b. What was respondent's attitude toward the use of records in supplying the figures requested? _____

- c. Did you have the impression at any time that the respondent did not know the figures too well which he gave you?

Yes ☐ No ☐ Not sure ☐ Explain _____

Page 3 -- IRF

- d. Did you have the impression at any time that any figures were deliberately being "doctored"?

Yes ☐ No ☐ Not sure ☐ Explain _____

- e. Did you have the impression at any time that any figures were being withheld or suppressed?

Yes ☐ No ☐ Not sure ☐ Explain _____

4. GIFT INFORMATION

Was a gift offered to the respondent? Yes ☐ No ☐

↓

If Yes: What was the respondent's reaction to the gift? (Exact words of respondent) _____

If No: Was copy of report offered to respondent? Yes ☐ No ☐

↓

If Yes: What was respondent's reaction? (Exact words, if possible)

5. SUMMARY

Please summarize briefly the overall reaction of the respondent to the interview. Include any information or observations which might be of interest.

-2-

b. RECORDS CONSULTED?

	<u>Acquired or disposed of</u>		<u>Records consulted</u>		<u>Please explain why not.</u>
	<u>No</u>	<u>Yes</u>	<u>Yes</u>	<u>No</u>	
A. Life insurance	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
B. Annuities	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
C. Government bonds and notes	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
D. Other bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
E. Businesses	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
F. Stocks or mutual funds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
G. Investment clubs	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
H. Pension plans	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
I. Real estate	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
J. Mortgages on real estate owned	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	

	<u>Owned during period</u>		<u>Records consulted</u>		<u>Please explain why not.</u>
	<u>No</u>	<u>Yes</u>	<u>Yes</u>	<u>No</u>	
K. Debts	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
L. Checking account(s)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
M. Savings account(s)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
N. Loans and mortgages lent	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
O. Brokerage account(s)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
P. Personal trust(s)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	

Comments _____

-3-

3. INTERIM REPORT NO. 2

Did panel member mention receiving "Interim Report No. 2"?

No ☐Yes ☐

(Exact words of panel member) _____

_____4. MAIL QUESTIONNAIRE - Reason for not returning

Check the following which the panel member mentioned as being reasons why the mail questionnaire was not returned.

If at the end of the interview the panel member has not volunteered any reasons, probe and check the following list.

Do not suggest or mention possible reasons and do not let panel member see this list. We want their unbiased answer.☐ Did not have enough time to do so☐ Questionnaire was too long☐ Mislaidd the questionnaire☐ Could not understand the questions☐ Forgot about it☐ Questions were too personal☐ Didn't feel it was important☐ Other (please specify) __________

5. SUMMARY COMMENTS

Briefly summarize the respondents overall reaction to this interview. Please include any information which you feel may be helpful in understanding and analyzing the data of this panel member. _____

6. INTERVIEWER'S NOTES

This IRF will be returned to you on the next wave so use this space to record any information you think you may need. Following are suggestions.

Future contact arrangements

Who _____

When _____

Where _____

How _____

Any information which will make your interview on the next wave easier, touchy data areas or subjects, hobbies or other talking points, personal likes or dislikes, etc.

-2-

- b. How would you describe his, or her, willingness to use the accuracy card and to give an accuracy estimate on each of the following holdings?

	<u>Checking accounts</u>	<u>Savings accounts</u>	<u>Brokerage accounts</u>	<u>Loans lent</u>	<u>Real estate</u>	<u>Mort. owed</u>	<u>All types of debts</u>
Does not own . . .	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Has not bought or sold (acquired or disposed)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Used records; card not used. . .	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Accuracy card used willingly . .	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Card used re- luctantly (reason)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Refused to use card (reason). . .	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Reason _____

- c. Did you have the impression at any time that the respondent did not know any particular figure too well which he gave you or withheld or suppressed any?

☐ Yes ☐ No ☐ Not sure
↓ ↓

Explain in detail: _____

3. RESULTS OF THE BANK REQUEST

- ☐ Not used for this panel member
☐ Signed all requests willingly
☐ Signed all requests reluctantly
☐ Signed only some requests, not all (reason below)
☐ Refused to sign any requests (reason below)

Reason _____

4. SUMMARY COMMENTS

Briefly summarize the respondents overall reaction to the interview. Please include any information which you feel may be helpful in understanding and analyzing the data of this panel member. _____

5. INTERVIEWER'S NOTES

This IRF will be returned to you on the next wave so use this space to record any information you think you may need. Following are suggestions.

Future contact arrangements

Who _____

When _____

Where _____

How _____

Any information which will make your interview on the next wave easier, touchy data areas or subjects, hobbies or other talking points, personal likes or dislikes, etc.

-4-

6. OFFICE AND FIELD PROBLEMS AND COMMENTS

All Assets _____

Checking Accounts _____

Savings Accounts _____

Life Insurance _____

Annuities _____

Government Bonds _____

Non-Government Bonds _____

Own Business _____

Brokerage Accts _____

Stocks _____

Personal Trusts _____

Loans Lent _____

Real Estate _____

Mtgs. Owed _____

All types of debts _____

Budget Bureau No. 40 R3150
Approval Expires 12/31/61

INTERVIEWER REPORT FORM

University of Illinois in cooperation with the Agricultural
Research Service and The Agricultural Marketing
Service, U. S. Department of Agriculture

Int'ee _____
Int'er _____

1. CONTACT REPORT (Record all attempts)

When		Where		Approx. age	Results of contact				Time in interview
Date	Time	Home	Other (explain)		Person(s) talked to	Int.	R. not home	Ref. made	
_____	_____	<input type="checkbox"/>	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
_____	_____	<input type="checkbox"/>	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
_____	_____	<input type="checkbox"/>	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
_____	_____	<input type="checkbox"/>	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____
_____	_____	<input type="checkbox"/>	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____

IF NOT INTERVIEWED: Explain circumstances in detail _____

2. CLASSIFICATION DATA (Fill in as completely as possible, even for refusals and non-contacts)

Approx. size of farm _____ acres
Type of farm (cash grain, livestock, etc) _____
Livestock, if any, on farm ☒ _____
Condition of exterior of house _____
Size of family _____
Country of origin _____

-2-

3. ATTITUDE OF PANEL MEMBER

- a. In your opinion, how accurate and how complete is the information that was given for each of the following holdings.

	Checking accounts	Savings accts.	Life ins.	Gov. bonds	Own bus.	Stock	Debts
ACCURACY							
Fully so.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Fairly so.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Not much.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Very little.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
COMPLETENESS							
Fully so.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Fairly so.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Not much.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Very little.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Not applicable.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

- b. What was respondent's attitude toward the use of records in supplying the figures requested? _____

- c. Did you have the impression at any time that the respondent did not know the figures too well which he gave you? ☐ Yes ☐ No ☐ Not sure Explain _____

- d. Did you have the impression at any time that any figures were deliberately being "doctored"? ☐ Yes ☐ No ☐ Not sure Explain _____

- e. Did you have the impression at any time that any figures were being withheld or suppressed? ☐ Yes ☐ No ☐ Not sure Explain _____

- f. Did you leave an interview evaluation form with the panel member?

☐ Yes ☐ No ☐ Not applicable

↓

↓

What was the respondents reaction? _____

-3-

4. SUMMARY COMMENTS

Briefly summarize the respondents overall reaction to the interview. Please include any information which you feel may be helpful in understanding and analyzing the data of this panel member. _____

5. INTERVIEWER'S NOTES

This IRF will be returned to you on the next wave so use this space to record any information you think you may need. Following are suggestions.

Future contact arrangements

Who _____

When _____

Where _____

Any information which will make your interview on the next wave easier, touchy data areas or subjects, hobbies or other talking points, personal likes or dislikes, etc.

Inter _____

University of Illinois in cooperation with the Agricultural
Research Service and the Agricultural Marketing
Service, U. S. Department of Agriculture

Int'lee _____

I. THE FAMILY

1. Has anything unusual happened to you or your family since _____ ?
No ☐ Yes ☐ →
2. Have there been any additions or reductions to your family? No ☐ Yes ☐ → Correct below (If any new members in S. U., obtain personal characteristics listed below)

	1	2	3	4	5	6	7
3. Your immediate family then consists of... (Relation of each member to operator or to owner)	Owner <input type="checkbox"/> Operator <input type="checkbox"/>						
	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N
	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F	<input type="checkbox"/> M <input type="checkbox"/> F
4. Present during interview							
5. Age, sex							
6a. Empl. status (E, H, R, S, O)							
b. If Empl. (E): Type of work							
c. Hours worked last week							
d. Second job: Type of work							
e. Hours worked last week							
7. Social Security coverage							
8a. Family member in S. U. *	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N	<input type="checkbox"/> Y <input type="checkbox"/> N

b. As you may recall, for the purpose of our study, we want to include only you, your wife, your children under 16 years old, and other family members who earn less than \$600 per year and who have savings of less than \$600 in their own name. Which of these people (besides you and your wife) does this now cover?
(Check yes or no for each family member under 8a.)

* New members in S. U.: Na _____

Educ. _____

Marital Status _____

Occup. _____

9. I am going to mention some types of savings. Why do you think people in your position should have each type of savings? If you do not think people in your position should have that type of savings, please say so.

Type of savings	Interest or dividend income	Hedge against inflation	Quick profits	Safety	Res. for emerg.	Go on		Should not have	10. Ranking of these savings
						vac. buy goods	Build up capital		
Savings account in banks or savings & loan associations	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Life insurance	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Govt savings bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Marketable bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Common stock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Preferred stock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Investment in farm land and buildings	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Investment in farm equipment and live- stock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Annuity	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Checking account	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	

10. Here is a list of these various forms of savings (Card A). Could you rank them in the order of their importance to you. In other words, which one is most important, which is second most important, which is third most important and so on for all forms of savings. (Record in question 9 above)

II. THE FARM AND OTHER REAL ESTATE

	Current value of land and buildings	Acq. or disp. since (If yes) net amt	Source or use of funds
1. Farm land operated either personally or with hired labor. (None <u>N</u>)			
a. Owned wholly by you	_____ acres	\$ <u>N</u> →	_____ <u>S</u> <u>U</u>
b. Owned with someone outside savings unit	_____ acres	<u>N</u> → %	_____ <u>S</u> <u>U</u>
What proportion do you own? %			
c. Renting or operating on shares	_____ acres		
d. Total operated farm land	_____ acres		
2. Other farm land--which you do not operate personally or with hired labor. <u>N</u>			
a. Number of acres	_____		
b. Total value of land and buildings	\$ _____	\$ _____	
c. Own wholly or partly	<u>W</u> <u>P</u> → %	<u>W</u> <u>P</u> → %	
d. What percent do you own?	_____ %	_____ %	
e. (If change) Net amount	\$ _____ <u>N</u> <u>S</u> <u>U</u>	\$ _____ <u>N</u> <u>S</u> <u>U</u>	
f. Source or use of funds	_____ <u>S</u> <u>U</u>	_____ <u>S</u> <u>U</u>	
3. Real estate in town or city owned wholly or partly. <u>N</u>			
a. Type of real estate	_____		
b. Total value of real estate	\$ _____	\$ _____	
c. Own wholly or partly	<u>W</u> <u>P</u> → %	<u>W</u> <u>P</u> → %	
d. What percent do you own?	_____ %	_____ %	
e. Any acq. or disp. since _____?	\$ _____ <u>N</u> <u>S</u> <u>U</u>	\$ _____ <u>N</u> <u>S</u> <u>U</u>	
Net amount			
f. Source or use of funds	_____ <u>S</u> <u>U</u>	_____ <u>S</u> <u>U</u>	

6. Crops stored on or off the farm including sealed crops N

How large a quantity is this?	Quantity sealed?	What is total current value owned by S. U.?	Increased or decreased since Amt. Involved?	Source or use of funds
/	<u>N</u>	\$	<u>N</u> \$ <u>↑</u>	<u>S U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S U</u>

7. Cost of purchased supplies on hand

	Total cost of current supply owned by S. U.	Acq. since Cost involved?	(If acquired) Source of funds
Fertilizer, lime	\$ <u>N</u>	<u>N</u> \$ <u>↑</u>	
Seed	<u>N</u>	<u>N</u> <u>↑</u>	
Tractor fuel, gas	<u>N</u>	<u>N</u> <u>↑</u>	
Pchsed feed	<u>N</u>	<u>N</u> <u>↑</u>	
Fencing bldg mat'l's	<u>N</u>	<u>N</u> <u>↑</u>	
Other	<u>N</u>	<u>N</u> <u>↑</u>	

8. Crops now coring up (None N)

How many acres by S. U.?	Estimate of amount invested so far in these crops (Approx.)
	\$

9. We would like to review your debt situation and bring it up to date. How much do you or other members of the S. U. presently owe on the following? Comp. _____

Loans secured by mortgages, sales contracts or other liens on:	Add'l debts		Atr, how much do you owe?	Who do you owe this to?	Repayment method	Acq. (added to) or paid off (reduced) since _____?	Source or use of funds	Records	
	New	Old						Y	Card
The Farm	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	_____	_____	N\$ _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Other farm land that you own	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Town or city real estate that you own	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Livestock and poultry	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Autos, trucks, tractors mach. and equipment	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Growing crops	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Harvested crops on hand	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Appliances and other household goods	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Other property or business that you own	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Amounts borrowed on:	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Life insurance policies	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Stocks and bonds	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Unsecured debts	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Cash loans	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Open accounts owed to merchants and dealers	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>
Med. bills, unpaid taxes, etc.	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	N _____	A D →	<input type="checkbox"/>	<input type="checkbox"/>

Are any debts duplicated by being secured by the same asset? No ☐ Yes ☐ → (Explain) _____

III. FINANCIAL HOLDINGS

1. Checking Accounts ☒ Comp. _____

Acct is New <u>Old</u>	Mmbr(s)	Bank	Last balance at bank statement	Date of balance	Acc	Records Y <u>Card</u>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	\$ _____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>

2. Savings Accounts: Bank, Savings & Loan, Credit Union and Postal Savings ☒ Comp. _____

Acct is New <u>Old</u>	Mmbr(s)	Institution	Date of last dep or with? Dep <u>With</u>	Atry; present balance	Balance as of last interview	Source or use of funds	Acc	Records Y <u>Card</u>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	\$ _____	\$ _____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>

Stock is New <u>Old</u>	Mmbr(s)	Company	Type of stock		Number of shares	Present price per share	Any acquired or disposed since Amt. involved	Source or use of funds	Records	
			Com	Pfd					Other	Acc
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>		\$	N\$	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>			N	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>			N	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>			N	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>			N	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>			N	→	\$ U	<input type="checkbox"/> <input type="checkbox"/> N

a. Any members in investment clubs? ☒ ☐ → (Enter details above.)

b. Any members have brokerage acts? ☒ ☐ → Present cash balance \$ (Enter stocks above)

6. Personal Trusts			Comp.	Are you		Any acquired	
Describe you be receiving, anything payments since			how much will	receiving	When will	or disposed	
Trust is New <u>Old</u>	Mmbr(s)	trust	how often?	now?	start?	Amt. involved	Source or use of funds
<input type="checkbox"/>		\$	/	<input checked="" type="checkbox"/> <input type="checkbox"/> →		N\$	→
<input type="checkbox"/>			/	<input checked="" type="checkbox"/> <input type="checkbox"/> →		N	→

7. Business(es) Other Than This Farm			Comp.	Any owned		Any invested	
Type of business			net value	current	What pct	since	
Bus. is New <u>Old</u>	Mmbr(s)	Type of business	net value	is yours?	Amt. involved	Source or use of funds	Records
<input type="checkbox"/>		\$		<input checked="" type="checkbox"/> <input type="checkbox"/> →	N\$	\$ U	<input type="checkbox"/> <input type="checkbox"/> N
<input type="checkbox"/>				<input checked="" type="checkbox"/> <input type="checkbox"/> →	N	\$ U	<input type="checkbox"/> <input type="checkbox"/> N

8. Mortgages and Loans to Others			Comp.	Balance as	
Type of loan			how much	Repayment	of the last
Loan is New <u>Old</u>	Mmbr(s)	Recipient	Mtge Cont	is owed?	interview
<input type="checkbox"/>			<input type="checkbox"/>	\$	\$
<input type="checkbox"/>			<input type="checkbox"/>	/	\$ U
<input type="checkbox"/>			<input type="checkbox"/>	/	\$ U

IV. INCOME POSITION

	Comp.	Amount	Acc.	Records Yes <input type="checkbox"/> No <input type="checkbox"/>	Card <input type="checkbox"/> <input type="checkbox"/>
1. Taking into account current operating expenses, including allowances for depreciation, atyr, what was the net money income from <u>this farm</u> in 1960?		\$ <u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
2. Atyr, what income did you receive, after depreciation, from the rental or operation of <u>other farms</u> in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
3. What about net income after depreciation from other real estate or businesses in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
4. What about wages and salaries received from others before deductions in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
5. ... income from veterans' pensions and compensation, veterans' allotment, servicemen's family allotment in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
6. ... interest, dividends, trust funds in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
7. ... royalties on mineral leases in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
8. ... any inheritances or gifts in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
9. What gains or losses did you have in 1960 from sales of stock or property?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
10. ... any other income in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
11. Did <u> </u> Member <u> </u> From what source(s) (mention other members of SU) have any income in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
12. So your total SU income in 1960 was		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
13. How much do you estimate was the value of the wear and tear (depreciation) on equipment and machinery, and buildings on this farm in 1960?		<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>

14. How did your total family income in 1960 compare with the average income of your household over the preceding 2 or 3 years? (Card D)

Higher by 20% or more ☐
Higher by 10 - 19% ☐

Within 10% ☐ (Skip to 16)

Lower by 10 - 19% ☐
Lower by 20% or more ☐

15. Were there any unusual circumstances which served to bring this about? ☒ Y ☐ N → What _____

16. Do you believe your total income this year will be influenced by unusual circumstances? _____

☒ N ☒ Y → What? _____

Notes to Questionnaire:

I 6a. "E" meant employed, "H" meant housewife, "R" equaled retired, "S" stood for student, and "O" meant "Other." The interviewer was to explain what the "O" stood for.

I 9. The S. U. was the Savings Unit and was composed of those family members who fit the description following question 9.

II 9. "Atry" meant "According to your records." These words were to be used whenever the interviewer asked for amounts which would normally be found in some form of record.

IVC3

STUDY OF FARM FAMILY FINANCES
(Questionnaire for Wave 3)Budget Bureau No. 40R3150
Approval Expires 12/31/61

Inter _____

University of Illinois in cooperation with the Agricultural
Research Service and the Agricultural Marketing
Service, U. S. Department of Agriculture

Int'ee _____

I. THE FAMILY

1. Has anything unusual happened to you or your family since _____ ?
No ☐ Yes ☐ →
2. Have there been any additions or reductions to your family? No ☐ Yes ☐ → Correct below (If any new members in S. U., obtain personal characteristics listed below)

3. Your immediate family then consists of... (Relation of each member to operator or to owner)

4. Present during interview

5. Age, sex

- 6a. Empl. status (E, H, R, S, O)

- b. If Empl. (E): Type of work

- c. Hours worked last week

- d. Second job: Type of work

- e. Hours worked last week

7. Social Security coverage

- 8a. Family member in S. U. *

1	2	3	4	5	6	7
Owner <input type="checkbox"/>						
Operator <input type="checkbox"/>						
<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N
<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F	<input checked="" type="checkbox"/> M <input checked="" type="checkbox"/> F
_____	_____	_____	_____	_____	_____	_____
_____	_____	_____	_____	_____	_____	_____
_____	_____	_____	_____	_____	_____	_____
<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N
<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N	<input checked="" type="checkbox"/> Y <input checked="" type="checkbox"/> N

- b. As you may recall, for the purpose of our study, we want to include only you, your wife, your children under 16 years old, and other family members who earn less than \$600 per year and who have savings of less than \$600 in their own name. Which of these people besides you and your wife does this now cover?
(Check yes or no for each family member under 8a.)

* New members

in S. U.: No. _____

Educ. _____

Marital Status _____

Occup. _____

9. I am going to mention some types of savings. Why do you think people in your position should have each type of savings? If you do not think people in your position should have that type of savings, please say so.

Type of savings	Interest or dividend income	Hedge against inflation	Quick profits	Safety	Res. for emerg.	Go on vac. or buy goods	Build up capital	Other (explain)	Should not have	10. Ranking of these savings
Savings account										
in banks or sav- ings & loan asso- ciations	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Life insurance	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Govt savings bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Marketable bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Common stock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Preferred stock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Investment in farm land and buildings	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Investment in farm equipment and livestock	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Annuity	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	
Checking account	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	

10. Here is a list of these various forms of savings (Card A). Could you rank them in the order of their importance to you. In other words, which one is most important, which is second most important, which is third most important and so on for all forms of savings. (Record in question 9 above)

II. THE FARM AND OTHER REAL ESTATE

1. Farm land operated either personally or with hired labor. (None)

a. Owned wholly by you

b. Owned with someone outside savings unit

What proportion do you own?

c. Renting or operating on shares

d. Total operated farm land

2. Other farm land--which you do not operate personally or with hired labor.

- Number of acres
- Own wholly or partly
- What percent do you own?
- (If change) net amount
- Source or use of funds

<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div>
		%		
		\$		

<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div>
		%		
		\$		

<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">P</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">W</div>	+	<div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div> <div style="border: 1px solid black; padding: 2px; display: inline-block; margin-bottom: 5px;">N</div>
		%		
		\$		

3. Since , have you acquired or disposed of any buildings on your present farm property, nonfarm land or real estate? ☒ N

If yes Acq. Dis.	Type of real estate	Amount paid {received}	Any owned july? (If yes) What pct is yours?	Source or use of funds	
					\$
<input type="checkbox"/>			<input checked="" type="checkbox"/>		\$
<input type="checkbox"/>			<input checked="" type="checkbox"/>		\$
<input type="checkbox"/>			<input checked="" type="checkbox"/>		\$

b. Any others? (Yes, fill in above)

b. Any others? (Yes, fill in above)

6. Crops stored on or off the farm including sealed crops N.

How large a quantity is this?	Quantity sealed?	What is total current value owned by S. U.?	Increased or decreased since Amt. involved?	Source or use of funds
/	<u>N</u>	\$	<u>N</u> \$ <u>↑</u>	<u>S</u> <u>U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S</u> <u>U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S</u> <u>U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S</u> <u>U</u>
/	<u>N</u>		<u>N</u> <u>↑</u>	<u>S</u> <u>U</u>

7. Cost of purchased supplies on hand

Fertilizer, lime	Total cost of current supply owned by S. U.	Acq. since Cost involved?	(If acquired) Source of funds
Seed	\$ <u>N</u>	<u>N</u> \$ <u>↑</u>	
Tractor fuel, gas	<u>N</u>	<u>N</u> <u>↑</u>	
Pchsd feed	<u>N</u>	<u>N</u> <u>↑</u>	
Fencing, bldg mat'ls	<u>N</u>	<u>N</u> <u>↑</u>	
Other	<u>N</u>	<u>N</u> <u>↑</u>	

8. Crops now coming up (None N)

How many acres by S. U.?	Estimate of amount invested so far in these crops (Approx.)
	\$

9. We would like to review the changes in your debt situation and bring it up to date.

What is the net change in each of the following debts? Comp. ☐ No ☐ Yes (List below)
Have you incurred any new debts since _____?

Loans secured by mortgages, sales contracts, or other liens on:	Add'l debts		Who do (did) you owe this to?	Atrr, what is the net change since _____?		Source or use of funds		Records	
	New	Old		Net incr. \$	Net decr. \$	Acc	Y	Card	
This farm	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	\$ _____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Other farm land that you own	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Town or city real estate that you own	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Livestock and poultry	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Autos, trucks, tractors, mach. and equipment	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Growing crops	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Harvested crops on hand	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Appliances and other household goods	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Other property or business that you own	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Amounts borrowed on:									
Life insurance policies	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Stocks and bonds	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Unsecured debts	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Cash loans	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Open accounts owed to merchants and dealers	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Medical bills, unpaid taxes, etc.	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>
Are any debts duplicated by being secured by the same asset?	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	<input type="checkbox"/> No <input type="checkbox"/> Yes <input type="checkbox"/> (Explain) _____	_____	_____	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>

III. FINANCIAL HOLDINGS

1. Checking accounts ☒ Comp. _____

Acct is New <input type="checkbox"/> Old <input type="checkbox"/>	Mmbr(s) _____	Bank _____	Last balance, aty bank statement _____	Date of balance _____	Acc _____	Records Y _____ Card _____
<input type="checkbox"/>	_____	_____	\$ _____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>

2. Savings Accounts: Bank, Savings & Loan, Credit Union and Postal Savings ☒ Comp. _____

Acct is New <input type="checkbox"/> Old <input type="checkbox"/>	Mmbr(s) _____	Institution _____	Atry, change in balance since Increased Same Decreased	Where did the money come from? (What was done with the money?)	Acc _____	Records Y _____ Card _____
<input type="checkbox"/>	_____	_____	\$ _____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>

3. Life Insurance and Annuities (Any bought or cashed since _____ ?) ☒ Comp. _____

If yes, Bght Cshd	Mmbr _____	Company _____	Type of policy (or annuity) _____	Premium (Amt/Freq.) _____	Atry, face value (amt. rec'd) _____	If cashed, what was done with the money?	Acc _____	Records Y _____ Card _____
<input type="checkbox"/>	_____	_____	_____	\$ _____ / _____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____ / _____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____ / _____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>
<input type="checkbox"/>	_____	_____	_____	_____ / _____	_____	_____	_____	<input type="checkbox"/> <input checked="" type="checkbox"/>

4. Bonds: Federal, State, Local, Corporate, Foreign, Church, etc. (Any bought or cashed since ☒ ?) ☒ Comp. _____

If yes, Bgt Cshd	Mmbr	Who issued the bond? (if Fed'l) what series?	Price per bond	Number of bonds	Where did the money come from? (What was done with the money?)	Records	
						Y	Card
<input type="checkbox"/>	<input type="checkbox"/>		\$ _____			<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>					<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>					<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>					<input type="checkbox"/>	<input checked="" type="checkbox"/>

5. Common and Preferred Stock: Mutual Funds, PCATs, Elevators, Coop, Brokerage Accts, etc.
(Any acquired or disposed of since ☒ ?) ☒ Comp. _____

If yes, Acq Dis	Mmbr(s)	Company	Type of stock		Number of shares	Present price per share	Where did the money come from? (What was done with the money?)	Records	
			Com	Pfd Other				Y	Card
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>		\$ _____		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>			<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input checked="" type="checkbox"/>

a. Any members in investment clubs? ☒ ☒ → (Enter details above)

b. Any members have brokerage accounts? ☒ ☒ → Change in cash balance \$ ☒ ☒ Comp. _____

6. Personal Trusts (Any acquired or disposed of since ☒ ?) ☒ Comp. _____

If yes, Acq Dis	Mmbr(s)	Describe trust	Atr, how much will you be receiv- ing, how often?	When will payments start?	If disposed, what was done with the money?	Records	
						Y	Card
<input type="checkbox"/>			\$ _____	/		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>				/		<input type="checkbox"/>	<input checked="" type="checkbox"/>

7. Business(es) Other Than This Farm (Any invested in or sold since ?) ☒ Comp.

If yes, Inv Sold	Mmbr(s)	Type of business	Amount invested (received)	Where did the money come from? (What was done with the money?)	Any owned by? (If yes) What pct is yours?	Records	
						Y	Card
<input type="checkbox"/>	<input type="checkbox"/>		\$		<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>				<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input checked="" type="checkbox"/>

8. Mortgages and Loans to Others ☒ Comp.

Loan is New Old	Mmbr(s)	Recipient	Type of loan Mtg Cont Loan	since Increase \$ Decrease	?	Where did the money come from? (What will be done with the money?)	Records	
							Y	Card
<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	\$	<input type="checkbox"/>		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>	<input checked="" type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>		<input type="checkbox"/>	<input checked="" type="checkbox"/>

IV. INCOME POSITION

Comp.

	Amount	Acc.	Records	
			Y	Card
1. Taking into account current operating expenses, including allowances for depreciation, atyr, what was the net money income from this farm in 1960?	\$ <u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
2. Atyr, what income did you receive, after depreciation, from the rental or operation of other farms in 1960?	<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
3. What about net income after depreciation from other real estate or businesses in 1960?	<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
4. What about wages and salaries received from others before deductions in 1960?	<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>
5. ...income from veterans' pensions and compensation, veterans' school allotment, servicemen's family allotment in 1960?	<u> </u>	<u> </u>	<input type="checkbox"/>	<input checked="" type="checkbox"/>

6. ... interest, dividends, trust funds in 1960? _____ N _____
7. ... royalties on mineral leases in 1960? _____ N _____
8. ... any inheritances or gifts in 1960? _____ N _____
9. What gains or losses did you have in 1960 from sales of stock or property? _____ N _____
10. ... any other income in 1960? _____ N _____
- From which sources?
- | 11. Did _____ | Member _____ | From what source(s) _____ |
|--|--------------|---------------------------|
| (mention other mem- bers of SU) have any _____ → | _____ | _____ N _____ |
| income in 1960? _____ → | _____ | _____ N _____ |
12. So you total SU income in 1960 was. N _____
13. How much do you estimate was the value of the wear and tear (depreciation) on equipment and machinery, and buildings on this farm in 1960? _____ N _____
14. How did your total family income in 1960 compare with the average income of your household over the preceding 2 or 3 years? (Card D)
- | | | |
|--|--|---|
| Higher by 20% or more <input type="checkbox"/> | Within 10% <input type="checkbox"/> (Skip to 16) | Lower by 10 - 19% <input type="checkbox"/> |
| Higher by 10 - 19% <input type="checkbox"/> | | Lower by 20% or more <input type="checkbox"/> |
15. Were there any unusual circumstances which served to bring this about? N Y → What _____
16. Do you believe your total income this year will be influenced by unusual circumstances? N Y → What?

16. Do you believe your total income this year will be influenced by unusual circumstances?

☐ N ☐ Y ↑ What?

2.

4. a. Which of the statements on this card (E) best reflects your attitude on the uses to which savings should be put? (Check one)

	Best	Dis- agree
A. People should try never to draw on savings; if extra money is needed, they should borrow.....	<input type="checkbox"/>	<input type="checkbox"/>
B. People should draw on savings only in an emergency.....	<input type="checkbox"/>	<input type="checkbox"/>
C. People should draw on savings only in an emergency or to buy or build a house.....	<input type="checkbox"/>	<input type="checkbox"/>
D. People should draw on savings not only for the above purposes but also for any large expenditures, such as a car, appliances, take a trip, etc.....	<input type="checkbox"/>	<input type="checkbox"/>
E. People should draw on savings as needed when expenses exceed income.....	<input type="checkbox"/>	<input type="checkbox"/>

Comments: _____

- b. Now, which of these statements do you disagree with most? (Check one above.)

II DEBTS

- 5 .a. Following is a list of reasons why people have debts. Would you tell me if you or a member of your family presently have a debt for any of these reasons. Please include everything no matter how small. (Do not include bills payable in 30 days.)
Do you or any members of your family have any debts...

	Presently have		Check if
	Yes	No	records handled outside family
A. Mortgage(s) on own house.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
B. Mortgage(s) on other property.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
C. To repair or improve house (loan or mortgage).....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
D. To buy a car.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
E. To buy appliances, clothing, or other goods.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
F. Personal loan with car or other goods as collateral....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
G. To make a vacation trip.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
H. To pay medical, dental or other bills.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I. Personal loan on insurance, stock or bank accounts or on any other collateral.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
J. Refinance old loans.....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
K. Other loans (give purpose of loan).....	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

☐ No debts at present.

- b. (Ask, if they presently have debts.)

Are the records of any of the debts you have handled by someone outside your family? (Check above.)

3-

c. (Ask only if no debt at present.)

i. Have you or a member of your family ever borrowed any money?

☐ Yes ☐ No

Under what circumstances (Why? When?) _____

ii. Have you or a member of your family ever bought anything on installment?

☐ Yes ☐ No

Under what circumstances (Why? When?) _____

(Skip to

Page 5

6. We would like to have some details about each of the debts you mentioned. Before I start, let me assure you once again that the information we obtain on these interviews is kept strictly confidential. Your name and address do not even appear on this questionnaire--only a number. The code which connects your number with your name and address is kept under lock and key at the University of Illinois in Urbana.

Now, let us take your ...

(Be sure to cover every debt checked "yes" in Question 5a.)

II. ASSETS

5.

- 7 .a. Would you look over the statements on this card (A) and tell me which one, if any, best represents your attitude toward saving. (Check one)

	Best	Dia- agree
A. It is not particularly necessary to save because if times get bad, things will work out somehow.....	<input type="checkbox"/>	<input type="checkbox"/>
B. It is not particularly necessary to save because we are adequately covered by health and accident insurance and in various other ways.....	<input type="checkbox"/>	<input type="checkbox"/>
C. We should save primarily for old age, with perhaps a little in the bank for emergencies.....	<input type="checkbox"/>	<input type="checkbox"/>
D. We should save not only for old age but saving is also necessary for many other reasons.....	<input type="checkbox"/>	<input type="checkbox"/>
E. We give priority to saving under almost any circumstances.....	<input type="checkbox"/>	<input type="checkbox"/>

- b. Now, which of these statements do you disagree with most strongly? (Check one box in "disagree" column.)

Comments: _____

8. Would you look over these statements (Card B) on the different ways in which people handle their income and tell me which, if any of them, best describes your practices? (Circle one.)

- A. Buy whatever you need as long as you can get the credit.
 B. Try to keep your spending just within your income.
 C. Put aside whatever is left over at the end of the week (or month).
 D. Try to keep expenditures down so that some money is left over for saving at end of week (or month, or year).
 E. Have definite plan in advance for saving part of income, as through fixed savings plans, etc., and then spend remainder.
 F. Not only have definite plan in advance for saving part of income, but also try to have something left over from expenses for savings, too.

Comments: _____

If none, please explain _____

II ASSETS

6.

9.a. Following is a list of asset holdings. Would you please tell me which ones you or any member of your family have. Include everything, no matter how small. Do you or any members of your family have any ...

Presently have		Regarded as savings		Check if
Yes	No	Yes	No	records handled outside family
<input type="checkbox"/>	A. Checking accounts	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	B. Savings accounts--banks, savings and loan associations, credit unions, postal savings, or other savings accounts.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	C. Owner or part-owner of an unincor- porated business.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	D. Owner or part-owner of an incor- porated business and active in its management.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	E. Life insurance--ordinary life, en- dowment, term, children's educa- tion, group, Gov't, mortgage, cre- dit, savings, business life or other.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	F. Annuities	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	G. Pension plans	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	H. U. S. Treasury notes	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	I. Savings bonds--series E, series H, and other.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	J. Public authority bonds--state, mun- icipal, toll-road, etc.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	K. Corporate bonds or debentures.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	L. Other bonds--church, foreign, etc.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	M. Common or preferred stock, incl. mutual funds.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	N. Brokerage accounts	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	O. Own your home	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	P. Own land	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	Q. Own other real estate	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	R. Mortgages lent	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	S. Loans to relatives, friends or business associates.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	No holdings in any of the above forms. (Skip to 6d)			

b. Which of these holdings that you have do you regard as savings? (Record above)
Comments

c. Are the records of any of the holdings that you have handled by someone outside your family? (Check above.)

7.

d. Listed on this card (C) are some of the goals which people have in building up savings. Which if any of these represent your goals?

- ☐ To provide for our old age.
- ☐ To provide an inheritance for our children.
- ☐ To provide for our childrens' education.
- ☐ To pay off debts.
- ☐ To buy or build a house.
- ☐ To purchase some particular item, such as a car, furniture, appliance, etc.
- ☐ To make a trip at some future time, such as a vacation.
- ☐ For emergencies or a rainy day.

Comments: _____

Do you have any savings goals which are not listed on this card? _____

10.

We would like to have some details about each of the holdings you mentioned. Before I start, let me assure you once again that the information we obtain on these interviews is kept strictly confidential. Your name and address do not even appear on this questionnaire--only a number. The code which connects your number with your name and address is kept under lock and key at the University of Illinois in Urbana.

Now let us take your.....

(Make sure to cover every item checked at the top of pp. 5-10.)

A. CHECKING ACCOUNTS

Family member(s)	Joint account		Type of account (Check all that apply)		Name of bank	Balance in check book 5-31-58		Records consulted?		None kept
	Yes	No	Bus.	Hshold.		Yes	Ref.	Yes	Ref.	
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	\$ _____		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	\$ _____		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	\$ _____		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	\$ _____		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	\$ _____		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

B. SAVINGS ACCOUNTS

Family member(s)	Joint account		Where passbook kept	Institution			Name of institution	Interest		Balance in Bankbook passbook consulted?		
	Yes	No		S & L Bank Inv.	Credit Union	Postal savings		Other (specify)	Date last added	Rate	5-31-58	
											Yes	Ref.
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	%	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>

(C), (D) OWN BUSINESS

Family member	Organization (check one)		Type of business	Records		Net worth of entire business	Records consulted?		
	Prop.	Part. Incorp.		Type kept	Where kept		Yes	Ref.	None kept
	<input type="checkbox"/>	<input type="checkbox"/>				\$	<input type="checkbox"/>	<input type="checkbox"/>	
	<input type="checkbox"/>	<input type="checkbox"/>				\$	<input type="checkbox"/>	<input type="checkbox"/>	
	<input type="checkbox"/>	<input type="checkbox"/>				\$	<input type="checkbox"/>	<input type="checkbox"/>	

(E) LIFE INSURANCE

(Ordinary (straight, whole) life, limited pay life, endowment, retirement income insurance, family income insurance, term, industrial, group, govt, mortgage, credit, savings, etc.)

[illegible]

(F) ANNUITIES

Family member	Records		Receiving annuity payments now?		Total investment (total paid in)	Records consulted?	
	Type kept	Where kept	Company	Yes		No	Yes
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/> <input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/> <input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/> <input type="checkbox"/>

(G) PENSION PLANS

If you would quit the plan now,

Family member	Records Type kept	Where kept	Years in plan	Company or sponsor	Are contributions made by yourself, too?			Receive Employer's share?			No share, too?	Altogether No how much?	Records consulted?				
					No	Yes	Amount	Freq.	Yes	No			Yes	No	Yes	Ref.	None kept
					_____	_____	_____	_____	_____	_____			_____	_____	_____	_____	_____
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→ \$ _____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→ \$ _____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→ \$ _____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→ \$ _____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	

(H) U.S. TREASURY NOTES, (I) SAVINGS BONDS

Family member(s)	Jointly owned Yes No	Records		Denomination (maturity value)	Year purchased	No. of bonds	Notes		Records consulted? Yes Ref.
		Type kept	Where kept				Total invested		
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	\$	—	—	\$	—	<input type="checkbox"/> <input type="checkbox"/>

(J) PUBLIC AUTHORITY, (K) CORPORATE BONDS OR DEBENTURES (L) CHURCH,
FOREIGN AND OTHER BONDS

Family member(s)	Jointly owned Yes No	Records		Type of bond		Name of bond	No. of bonds	Present value per bond	Records consulted?	
		Type kept	Where kept	Corp.	Muni- cipal				Yes	Ref.
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	<input type="checkbox"/>	<input type="checkbox"/>	—	—	\$	<input type="checkbox"/>	<input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	<input type="checkbox"/>	<input type="checkbox"/>	—	—	\$	<input type="checkbox"/>	<input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	<input type="checkbox"/>	<input type="checkbox"/>	—	—	\$	<input type="checkbox"/>	<input type="checkbox"/>
—	<input type="checkbox"/> <input type="checkbox"/>	—	—	<input type="checkbox"/>	<input type="checkbox"/>	—	—	\$	<input type="checkbox"/>	<input type="checkbox"/>

(M) STOCKS AND MUTUAL FUNDS

Family member(s)	Jointly owned		Records		Company	Type of stock		No. of shares	Present price per share	Records consulted?	
	Yes	No	Type kept	Where kept		Com.	Pfd. Fund			Yes	Ref.
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>
	<input type="checkbox"/>	<input type="checkbox"/>				<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>

(N) BROKERAGE ACCOUNTS

Family member	Records		Balance in account	5-31-58	Name of broker	Records consulted?	
	Type kept	Where kept				Yes	Ref.
			\$			<input type="checkbox"/>	<input type="checkbox"/>
			\$			<input type="checkbox"/>	<input type="checkbox"/>
			\$			<input type="checkbox"/>	<input type="checkbox"/>
			\$			<input type="checkbox"/>	<input type="checkbox"/>

(O) OWN HOME, (P) OWN LAND, (Q) OTHER REAL ESTATE

Family member(s) owning	Records		Summer		Apt. Land bldg. Store (specify)	Net current saleable value	Receiving income from property		Records consulted?	
	Type kept	Where kept	Home	cottage			Yes	No	Yes	Ref.
_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

(R) MORTGAGES LENT, (S) LOANS OUTSTANDING

Family member making loan	Records		Person receiving loan	(Check one)		Amount owed 5-31-58	Records consulted?	
	Type kept	Where kept		Mort.	Land contract Loan		Yes	Ref.
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>

11 a. Following is a list of some goods. Would you tell me when if any, of these you own and how long you have had it? Also do you plan to purchase any of these goods (whether owned presently or not) between now and the end of the year?

Let's take the first one...

	Have it?		Plan-o-meter reading (Card D)	Probable cost	Install- ment	Financing		Set aside			
	No	Yes				How old?	Borrow the money	All out of the current income or savings	D. K.	Yes	No
A house	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A car	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A TV set	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A refrigerator	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A freezer	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A washing machine	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A dryer	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
An air conditioner	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
A stove or range	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
Other large appliance (specify) _____	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		
_____	<input type="checkbox"/>	<input type="checkbox"/>		\$	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		

b. Do you plan to make any of the following expenditures between now and the end of the year?

Plan-o-meter reading (Card D)	Probable cost	Install- ment	Financing			Set aside savings?		
			Borrow the money	All out of the current income or savings	D. K.	Yes	No	
Additions or improve- ments to house	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Major repairs to house	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Redecorate home or apt.	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Take a vacation	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Pay for own education	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Pay for child's education	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Other (specify) _____	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

IV INCOME

15.

12. a. Which family members presently have jobs from which they receive salaries, wages or commissions? Include not only full time work, but also part-time, temporary and summer jobs (baby sitting, newspaper routes, solicitation work, consulting, etc.). (If any family member had income from more than one job, use one column for each job and note whether it is that person's 1st, 2nd, 3rd, etc., job.).

Relation to main wage earner

Job number

Time employed:

Full time

Part-time

Temporary

Summer

Other (explain)

Type of work

MWE				
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

- b. Would you look over the sources of income on this Card (F) and tell me from which you have or expect to receive income this year, 1958. (Check appropriate boxes.)

- | | | | |
|---|--------------------------|--------------------------------------|--------------------------|
| 1. Net income from business | <input type="checkbox"/> | 11. Social security | <input type="checkbox"/> |
| 2. Interest or dividends from savings accounts. | <input type="checkbox"/> | 12. Welfare or relief payments | <input type="checkbox"/> |
| 3. Interest on Federal Government bonds | <input type="checkbox"/> | 13. Inheritances or bequests | <input type="checkbox"/> |
| 4. Interest on other bonds. | <input type="checkbox"/> | 14. Income tax refunds received | <input type="checkbox"/> |
| 5. Dividends on stocks. | <input type="checkbox"/> | 15. Insurance settlements | <input type="checkbox"/> |
| 6. Profit from rents collected | <input type="checkbox"/> | 16. Trust funds | <input type="checkbox"/> |
| 7. Bonuses | <input type="checkbox"/> | 17. Capital gains | <input type="checkbox"/> |
| 8. Retirement pay | <input type="checkbox"/> | 18. Royalties | <input type="checkbox"/> |
| 9. Unemployment or workmen's compensation. | <input type="checkbox"/> | 19. Alimony | <input type="checkbox"/> |
| 10. Old age pension | <input type="checkbox"/> | 20. Veterans pensions and allotments | <input type="checkbox"/> |
| | | 21. Annuities | <input type="checkbox"/> |
| | | 22. Gifts or prizes (value) | <input type="checkbox"/> |
| | | 23. Gambling winnings | <input type="checkbox"/> |
| | | 24. Other sources | <input type="checkbox"/> |

- c. Taking into consideration all income of wages and salaries plus income from these other sources, in what range do you expect your family's total income to fall for this year, 1958. (Card G)

- | | | | | |
|----------------------|--------------------------|----------------------|-----------|--------------------------|
| A. Less than \$1,000 | <input type="checkbox"/> | F. \$ 10,000 | \$ 14,999 | <input type="checkbox"/> |
| B. \$1,000 - \$2,999 | <input type="checkbox"/> | G. \$ 15,000 | \$ 24,999 | <input type="checkbox"/> |
| C. \$3,000 - \$4,999 | <input type="checkbox"/> | H. \$ 25,000 | \$ 49,999 | <input type="checkbox"/> |
| D. \$5,000 - \$7,499 | <input type="checkbox"/> | I. \$ 50,000 | \$ 99,999 | <input type="checkbox"/> |
| E. \$7,500 - \$9,999 | <input type="checkbox"/> | J. \$100,000 | \$249,999 | <input type="checkbox"/> |
| | | K. \$250,000 or more | | <input type="checkbox"/> |

- d. How do you expect your family's income this year to compare to last year?

Up _____%

No change ☐

Down _____%

V. TERMINATING INTERVIEW

16.

1. Asset and/or debt records held by other person.

(If any of the holdings listed in Question 6a or question 10a are handled by a person outside the family, i.e., if there is a check in the box marked "records handled outside family", ask the following question):

a. Who handled these holdings? Assets: _____

Debts: _____

b. We would like to get some details about these holdings. Could we have your permission to go to _____ (identity of other person) and obtain these details from him? Let me assure you once again that the information we obtain on these interviews is kept strictly confidential. Your name and address do not even appear on this questionnaire--only a number. The code which connects your number with your name and address is kept under lock and key at the University of Illinois in Urbana.

Willing ☐

Arrangements for contact. _____

Not willing ☐ Why? _____

. Thank respondent(s) for interview. Give assurances of confidentiality once more.

3. Offer gift, if respondent eligible.

(Should be offered only to respondents who meet both of the following requirements. (If information on either of these points has been refused, use your judgment as to whether or not to offer gift.)

1. Family head is not a college graduate.2. Total family income is not over \$7,500.)

4. Offer to send copies of survey report, if judged useful.

(Whether or not respondent is offered gift, he may be offered copies of releases and reports on this study as soon as they are completed.)

5. Sound out respondent(s) on future cooperation

(Respondents should be told that this is a continuing study and information will be gathered not more than 4 times a year for a period of not more than 2 years. It should also be stressed that future questionnaires will be shorter inasmuch as background information has already been furnished.)

a. Respondent(s) queried: Yes ☐ No ☐ → Why? _____

b. Method of future cooperation suggested.

☐ Mail☐ Personal interview at home☐ Personal interview at office☐ Other _____

6. Thank respondent(s) again.

STUDY OF FARM FAMILY FINANCES

Budget Bureau No. 40R3150
Approval Expires 12/31/61

Int'ee

IVHI

Inter-

I. THE FAMILY

- 1 . Relation of each family member in household to farm operator (Place next to those present)
- 2 . Any others away temporarily? Where? (Add to above list)
- 3 . Age and sex of each member
- 4 . Marital status (M, S, W, D)
- 5 . Schooling (years)
- 6a. Empl. status (E/n, H, R, S, O)
- b. Type of work, if employed
- c. Second job, if any
- d. (If farming) Pct of time on farm
- 7a. Social Security coverage?
- b. Other pension plans? What plan?
- 8a. Who pay(s) farm bills? (Check)
- b. Who keep(s) track of family savings and debts? (Check)
- 9a. Family member in S.U. ?

[illegible]

b. For the purpose of our study, we want to include only you, your wife, your children under 16 years old, and other family members who earn less than \$600 per year and who have savings of less than \$600 in their own name. Which of these people (besides you and your wife) does this cover? (Check yes or no box for each family member under 9a).

II. THE FARM AND OTHER REAL ESTATE

1. How much farm land are you operating altogether, either personally or with hired labor?

_____ acres

a. How much of this land is owned wholly by you?

_____ acres

b. How much of this land is owned partly by you?

_____ acres

What proportion do you own? _____%

c. How much of this land are you renting or operating on shares?

_____ acres

2. How much is (are) the land (and buildings) worth that you...

Value of land only

Value of land and bldgs

. . . own wholly?

\$

☐ No

\$

. . . own partly?

\$

☐ No

\$

. . . rent or operate on shares?

\$

☐ No

\$

3. Do you own wholly or partly any other farm land--land which you do not operate personally or with hired labor? ☐ Yes, wholly ☐ Yes, partly ☐ No

a. What is the value of this property?

\$ _____, \$ _____

b. (If owned partly) what percent do you own?

_____ % _____ %

4. Do you own wholly or partly any real estate in a town or city? ☐ Yes, wholly ☐ Yes, partly ☐ No

a. What type of real estate?

b. What is the value of this real estate?

\$ _____ \$ _____

c. (If owned partly) What percent do you own?

_____ % _____ %

6. What crops have you stored on or off the farm? N

Is it on or off the farm? On ☐ Off ☐

Any owned jtly? (If yes) What pct is yours? N

How large a quantity is this? /

What is its current selling value per...? \$

So, the total value of the stored crops...? \$

7. What is the cost of the following supplies on hand?

	Cost
Fertilizer, lime	<u>N</u>
Seed	<u>N</u>
Tractor fuel, gas	<u>N</u>
Pchased feed	<u>N</u>
Fencing, bldg mat'ls	<u>N</u>
Other	<u>N</u>

Any owned jtly? (If yes) What pct is yours?

8. What crops do you have coming up now?

N

How many acres?

Any owned jtly? (If yes) What pct is yours?

N

N

N

N

N

How much money have you invested so far in these crops?

\$

9. Now, we would like to know something about the debts that you have. Do you have any . . .

Int'ee _____

Category	A tyr, how much do you owe?	Who do you owe this to?	When and how are you paying this off?		Does this duplicate any other debt that you have mentioned?		Records		
			Amount incl.int.	Frequency	Y	N	Which?	Y	N
<u>Loans secured by mortgages, sales contracts, or other liens on:</u>									
This farm	<input checked="" type="checkbox"/>	\$ _____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Other farm land that you own	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Town or city real estate that you own	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Livestock and poultry	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Autos, trucks, tractors, machinery and equipment	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Growing crops	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Appliances and other household goods	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Other property or business that you own	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
<u>Amounts borrowed on:</u>									
Life insurance policies	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Stocks and bonds	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
<u>Unsecured debts</u>									
Cash loans	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Open accounts owed to merchants and dealers	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
Medical bills, unpaid taxes, etc.	<input checked="" type="checkbox"/>	_____			<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>

III. FINANCIAL HOLDINGS

1. Checking accounts

- a. How many checking accounts do you and other members of the savings unit have? N (number)
- b. How much variation has there been in the balance in each of these checking accounts since 7-1-60? In other words, what was the highest balance in each, what was the lowest balance, and what is the current balance? Let us begin with the first account.

In whose name(s) is this account?	At which bank? (Full name)	A t y r, since 7-1-60 what has been the . . .		(From checkbook) What is the present balance?	Now, let's see this acct is for. . .		Records					
		Highest bal.?	Lowest bal.?		Farm	Pers.	Both	Y	N	A		
_____	_____	\$ _____	_____	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

2. Savings accounts

- a. Turning to savings accounts--that is, accounts in banks, savings and loan associations, credit unions, postal savings--are there separate accounts for different family members, or do you have different accounts in various places under the same names, or what? Since I have several questions on each account, let's take each one in turn. First, . . .

In whose name(s) is this account?	Where is it? (Name of institution)	What is the purpose of this acct?	A t y r passbook, since 7-1-60 what has been the		What is the date of last dep. or with?	What is the pre-sent balance?	3 mos hence, do you expect this balance to be..		Records			
			Highest bal.	Lowest bal.			Up	Down	Y	N	A	
<u>N</u>	_____	_____	\$ _____	_____	_____	\$ _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
_____	_____	_____	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

- b. Do you or your (name other members) have savings accounts set aside for special purposes such as vacation, schooling, Christmas, home, etc. N V—(If yes, list all accounts above)

3a. Life Insurance and Annuities

When did you or your family last buy a new life insurance policy? Year N Did you buy this policy on your own initiative (), at the solicitation of an agent (), at the suggestion of a friend or relative (), or under some other circumstances? (Explain) _____

When did you last buy an annuity? Year N Did you buy this annuity on your own initiative (), at the solicitation of an agent (), at the suggestion of a friend or relative (), or under some other circumstances? (Explain) _____

May we get the following information about the policies (annuities) that you and other members of the immediate family now have (including paid-up policies)?

Name of insured or annuitant	With which company?	What type of policy (or annuity) is it?	When was it bought?	At yr what is its face value?	What premium is paid and how often?	Records	
						Y	N A
				\$	/	<input type="checkbox"/>	<input type="checkbox"/>
					/	<input type="checkbox"/>	<input type="checkbox"/>
					/	<input type="checkbox"/>	<input type="checkbox"/>
					/	<input type="checkbox"/>	<input type="checkbox"/>
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					/	<input type="checkbox"/>	<input type="checkbox"/>
					/	<input type="checkbox"/>	<input type="checkbox"/>
					/	<input type="checkbox"/>	<input type="checkbox"/>

b. Are you or any other members planning to buy or redeem any policies or annuities in the next 3 months, that is, until _____?

Which mmbr?	Buy or redeem?	What type of policy (or annuity)?	What is its probable face value?	(If redeem) How much will you receive?	Where is the money coming from? (What will be done with the money?)
<u>N</u>	<input type="checkbox"/>		\$		
	<input type="checkbox"/>				
	<input type="checkbox"/>				
	<input type="checkbox"/>				

5a. Common and preferred stock (incl. mutuals, equity in PCA's, cooperatives, etc.)

i. Have you or your _____ invested in mutual funds or common or preferred stocks since the war?

☐ Yes ☐ No (Skip to iii)

How have these purchases been made? (Check one or more) ☐ Through an investment club ☐ Through a bank
☐ Through a broker ☐ By buying securities privately ☐ Other (specify) _____

ii. What sources are consulted in deciding to buy or sell stocks?

☐ Broker ☐ Financial mag. ☐ Stock mkt service letter ☐ Friends or rel's ☐ Other _____

iii. At yr., can you give me a rundown of the common and preferred stocks presently owned by you or other members? For each, can you tell me. . .

In whose name is it?	Is that com. stock, pfd, or what?			How many shares are owned?		Is this stock on a listed exch? What is the pres. Records		
	Com	Pfd	Fund	check here	Yes	No	price / share?	Y N A
<input checked="" type="checkbox"/> _____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input checked="" type="checkbox"/>	\$ _____	<input type="checkbox"/> <input checked="" type="checkbox"/> <input type="checkbox"/>
_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>
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_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/>	<input type="checkbox"/>	_____	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>

iv. Are you or your _____ in an investment club? ☒ ☐ (Enter all details above. Under "price/share," enter equity in club.)

b. Are you or any other members of your family planning to buy or sell any stock in the next three months? ☒ ☐ (If Yes, enter all details under 4b)

6. Personal trusts

Are you or any members of your immediate family beneficiaries of a personal trust?

Which mmb? _____ <input checked="" type="checkbox"/> _____	What is the nature of this trust? _____	At yr., how much will you be receiving and how often? \$ _____ / _____	Are you receiving anything now? <input checked="" type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>	When will payments start? _____	Records		
					Y	N	A
					<input type="checkbox"/>	<input type="checkbox"/>	<input checked="" type="checkbox"/>

7. Business(es) other than this farm

a. Have you or your _____ invested in any business(es) other than this farm?

Which mmbr(s)?	What type of business or property is it?	Atyr what would you say is the current net value of the business?	If joint, what pct is owned?	Records Y N A
<u>N</u>	_____	\$ _____	<u>N</u>	<input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>
_____	_____	_____	<u>N</u>	<input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>

b. Are you or any members of your family planning to buy or sell any land, real estate or business in the next three months?

Which mmbr(s)?	Buy or sell? Buy Sell	What will be bought (sold)?	Approx. how much will it cost (bring)?	Where is the money coming from? (What will be done with the money?)
<u>N</u>	<input type="checkbox"/> <input type="checkbox"/>	_____	\$ _____	_____
_____	<input type="checkbox"/> <input type="checkbox"/>	_____	_____	_____

8. Mortgages and loans to others

a. Do you or your _____ have mortgages or other loans, representing money that you loaned to others?

Which mmbr(s)?	Who did you loan this money to?	What was the loan for?	Atyr how much is owed?	How will the money be repaid?	Records Y N A
<u>N</u>	_____	_____	\$ _____	/	<input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>
_____	_____	_____	_____	/	<input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>

b. Are you or any members of your immediate family planning to loan out any money in the next three months?

Which mmbr(s)?	Recipient	Type of loan Mtge. Cont. Loan	How much will be involved?	Where is the money coming from?
<u>N</u>	_____	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>	\$ _____	_____
_____	_____	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>	_____	_____

IV. INCOME POSITION

1. After taking into account current operating expenses including allowances for depreciation, how does the net money income expected from this farm compare with the net money income in 1959? First, . . .

	1959		Records		1960 Amount
	Amount		Y	N	
a. . . a t y r, what was the net money income from this farm in 1959? What do you expect it to be this year?	\$	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	\$
b. How does your expected income from other sources this year differ from that in 1959?					
i. At y r, what income did you receive, after depreciation, from the rental or operation of other farms in 1959? How much do you expect this year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
ii. What about net income after depreciation from other real estate or businesses in 1959? This year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
iii. What about wages and salaries received from others before deductions in 1959? This year? (Include bonuses and commissions)	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
iv. . . . income from veterans' pensions and compensation, veterans' school allotment, servicemen's family allotment in 1959? This year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
v. . . . interest, dividends, trust funds in 1959? This year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
vi. . . . royalties on mineral leases in 1959? This year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
vii. . . . Any inheritances or gifts in 1959? Any this year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
viii. What gains or losses did you have in 1959 from sales of stock or property? What do you expect from this source this year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
ix. . . . any other income in 1959? From which sources?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
x. . . . What about income from other sources this year?	<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

ci. Did _____ (mention family member) have any income in 1959? _____ From what source(s) _____ ☒ ☐ ☐ ☐

ii. What income do you expect from such sources this year? _____ ☒ ☐ ☐ ☐

d. So your total family income in 1959 was and in 1960 you expect it to be ☒ ☐ ☐ ☐

e. How much do you estimate was the value of the wear and tear (depreciation) on equipment and machinery, and buildings on this farm in 1959? _____ ☒ ☐ ☐ ☐ What do you expect it to be this year? ☒ ☐ ☐ ☐

2a. How did your total family income in 1959 compare with the average income of your household over the preceding 2 or 3 years? (Card C) ☐ Higher by 20% or more ☐ Within 10% ☐ (Skip to c) ☐ Lower by 10-19% ☐ Higher by 10-19% ☐ Lower by 20% or more ☐

b. Were there any unusual circumstances which served to bring this about? ☒ ☒ ☐ What? _____

c. Do you believe your total income this year will be influenced by unusual circumstances? ☒ ☒ ☐ What? _____

What?

c. Do you believe your total income this year will be influenced by unusual circumstances?

What?

STUDY OF FARM FAMILY FINANCES

University of Illinois in cooperation with the Agricultural
Research Service and the Agricultural Marketing
Service, U. S. Department of Agriculture

Budget Bureau No. 40 R3150

Approval Expires 12/31/61

Int'l use _____

IVCI

Inter _____

I. THE FAMILY

1. Relation of each family member in household to farm operator (Place* next to those present)
2. Any others away temporarily? Where? (Add to above list)
3. Age and sex of each member
4. Marital status (M, S, W, D)
5. Schooling (years)
- 6a. Empl. status (E/n, H, R, S, O)
- b. Type of work, if employed
- c. Second job, if any
- d. (If farming) Pct of time on farm
- 7a. Social Security coverage?
- b. Other pension plans? What plan?
- 8a. Who pay(s) farm bills? (Check)
- b. Who keep(s) track of family savings and debts? (Check)
- 9a. Family member in S. U. ?

1 Operator	2	3	4	5	6	7
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II. THE FARM AND OTHER REAL ESTATE

1. How much farm land are you operating altogether, either personally or with hired labor? _____ acres
- a. How much of this land is owned wholly by you? _____ acres
- b. How much of this land is owned partly by you? _____ acres

What proportion do you own? _____ %

- c. How much of this land are you renting or operating on shares? _____ acres

2. Do you own wholly or partly any other farm land--land which you do not operate personally or with hired labor? ☐ Yes, wholly ☐ Yes, partly ☐ No

- a. How many acres? _____ %
- b. (If owned partly) What percent do you own? _____ %

3. Since 7-1-60, have you acquired or disposed of any land or real estate, that is, land which you currently own (O), rent (R), operate on shares (S), or manage (M)? (If yes) For each piece, can you tell me if it was . . .

Acq.	Dis?	Is (was) it O, R, S, or M?	Type of property	Where did the money come from? (What was done with the proceeds?)	(If rented) What is the yearly rental?	What is the current value of the...	
						Land	Land and buildings?
a.	<input type="checkbox"/>	<input checked="" type="checkbox"/>	_____	_____	\$ _____	\$ _____	<input checked="" type="checkbox"/>
b.	<input type="checkbox"/>	_____	_____	_____	_____	_____	<input checked="" type="checkbox"/>
c.	<input type="checkbox"/>	_____	_____	_____	_____	_____	<input checked="" type="checkbox"/>

4. Since 7-1-60 how much livestock acquired or disposed of, or is number now same as then? (Card A)

☒ N

Any others? ☒ Y ☒ N

Any others? ☐ Y ☒ N

5. What about machines, including attachments? (Card B) Acquired or disposed of any since 7-1-60?

[illegible]

Any others? ☐ Y ☐ N

6. Since 7-1-60 increased or reduced the stored crops on or off the farm?	Net incr.	Net decr.	On or off the farm?		How large a quantity was this?	What was its total current value?	Any owned july? (If yes) What pct is yours?	Where did the money come from? (What was done with the proceeds?)
			On	Off				
<u>N</u>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	/	\$	<input type="checkbox"/> N	
	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	/		<input type="checkbox"/> N	
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7. Do you have any. . . .	Acq. since 7-1-60 or old ones paid off?		At yr, what is net change in debt since 7-1-60?	Does this duplicate any other debt that you have mentioned?		Records				
	N	Acq. S		Dis.	N	Y	Which	Y	N	A
<u>Loans secured by mortgages, sales contracts, or other liens on:</u>										
This farm	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Other farm land that you own	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Town or city real estate that you own	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Livestock and poultry	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Autos, trucks, tractors, machinery and equipment	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Growing crops	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Appliances and other household goods	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Other property or business that you own	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
<u>Amounts borrowed on:</u>										
Life insurance policies	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Stocks and bonds	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
<u>Unsecured debts</u>										
Cash loans	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Open accounts owed to merchants and dealers	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	
Medical bills, unpaid taxes, etc.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	

Int'ee _____

8. Could you give me an approximate idea of your net holdings of each of the following assets? As I read each asset, could you tell me which letter on this card corresponds to the bracket in which your holdings fall? Thus, what bracket would you say describes your net equity in this farm? (Repeat for each asset in turn using Card D.)

FARM (equity)	_____	LIFE INSURANCE-- ordinary life, endowment, term, group, GI, mortgage, credit, savings busi- ness life or other and ANNUITIES	_____
OTHER REAL ESTATE (equity)	_____	BUSINESS OWNED or part owned-- an unincorporated business or of an incorporated business and active in its management (equity in business)	_____
LIVESTOCK, STORED AND GROWING CROPS	_____	COMMON OR PREFERRED STOCK --mutual funds, investment clubs and BROKERAGE ACCOUNTS	_____
MACHINERY AND SUPPLIES	_____	TRUST FUNDS, MORTGAGES AND LOANS LENT--to friends, relatives or business associates	_____
CHECKING ACCOUNTS	_____		
SAVINGS ACCOUNTS--banks, S&L, credit union, postal savings	_____		

TOTAL ASSET HOLDINGS _____

III. FINANCIAL HOLDINGS

1. Checking accounts

a. How many checking accounts do you and other members of the S. U. have? N (number)

b. What change has there been in the balance in each of these checking accounts since 7-1-60?

In whose name(s) is this account?	At which bank? (Full name)	At yr how does the current balance differ from that on 7-1-60?	What was the reason for the change?	Now, let's see, this acct is for...		Records	
				Farm	Pers. Both	Y	N A
		\$ <u>U S D</u>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
		<u>U S D</u>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
		<u>U S D</u>		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

2. Savings accounts

a. Turning to savings accounts--that is, accounts in banks, savings and loan associations, credit unions, postal savings--are there separate accounts for different family members, or do you have different accounts in various places under the same names, or what? Since I have several questions on each account, let's take each one in turn. First, . . .

In whose name(s) is this account?	Where is it? (Name of institution)	What is the purpose of this acct?	What is the date of last change?	What was the reason for the change?	3 mos hence do you expect this balance to be...		Records	
					Up	Down	Y	N A
<u>N</u>		\$ <u>U S D</u>			<input type="checkbox"/>	\$ <input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
		<u>U S D</u>			<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
		<u>U S D</u>			<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>
		<u>U S D</u>			<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>

b. Have you or your (name other members) opened or closed any savings accounts since 7-1-60 for special purposes such as vacation, schooling, Christmas, home, etc.? N Y (If yes, list all accounts above.)

4a. Bonds: Federal, State, Local, Corporate, Foreign, Church, etc.

- i. When was the last time that you bought any bonds, that is, savings bonds, other govt. bonds, or state, local, corporate, or any other type of bonds? Year 1960 Did you buy this bond on your own initiative (), at the solicitation of a broker (), at the suggestion of a friend or relative (), or under some other circumstances? (Explain) _____

Why did you happen to buy this bond? _____

- iii. Since 7-1-60 have you or other members bought or cashed in any Fed'l bonds, state or local bonds, corporate or foreign bonds, church, or other bonds? ☒ Yes (If yes, enter details below)

Since 7-1-60 Bought Cashed	Who issued the bond? (If Fed'l) what series?	In what denomination?	How much was paid (rec'd) per bond?	How many did you buy (sell)?	Where did the money come from? (What was done with the money?)	Records	
						Y	N A
<input type="checkbox"/>		\$	\$			<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>						<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>						<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>						<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>						<input type="checkbox"/>	<input type="checkbox"/>

- b. Are you or any other members planning to buy or sell any bonds in the next three months?

Which mmbr?	Buy or sell?		Which company (or issuing authority)?	Bond or stock		Approx. how much will it cost (bring)?	Where is the money coming from? (What will be done with the money?)
	Buy	Sell		Bond	Stock		
<input checked="" type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>	\$	
	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		
	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		
	<input type="checkbox"/>	<input type="checkbox"/>		<input type="checkbox"/>	<input type="checkbox"/>		

5a. Common and preferred stock (incl. mutuals, equity in PCA's, cooperatives, etc.)

i. Have you or your _____ invested in mutual funds or common or preferred stocks since the war?

☐ Yes ☐ No

↑
How have these purchases been made? (Check one or more)

☐ Through an investment club
☐ Through a bank

☐ By buying securities privately
☐ Other (specify) _____

ii. What sources are consulted in deciding to buy or sell stocks?

☐ Broker ☐ Financial mag. ☐ Stock mkt service letter ☐ Friends or rel's ☐ Other _____

iii. Since 7-1-60 have you or other members of your family acquired or disposed of any common or preferred stocks? What about mutual funds, equity in PCA's, co-ops, etc? ☐ Y → (If yes, enter details below.)

Is that com.

Since 7-1-60 Acq./Dis.	In whose name is (was) it?	What is the name of the stock?	Com	Pfd	Fd	check	bought	(sold)	What price was pd (rec'd) per share?	Where did the money come from? (What was done with the money?)	Records		
											Y	N	A
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	\$ _____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→
<input type="checkbox"/>	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	_____	_____	_____	_____	<input type="checkbox"/>	<input type="checkbox"/>	→

iv. Are you or your _____ in an investment club? ☐ Y → (Enter all details above for transactions since 7-1-60.)

v. Do you or your _____ have any brokerage accts? ☐ Y → (Enter all details above for transactions since 7-1-60.)

b. Are you or any other members of your family planning to buy or sell any stock in the next three months?
☐ Y → (If yes, enter all details under 4b)

6. Personal trusts

Are you or any members of your immediate family beneficiaries of a personal trust? ☒ Y
 Any new trusts since 7-1-60 or disposals of old ones? ☒ Y→ (If yes, enter details below)

Since 7-1-60	Acq. Dis.	Which mmbtr?	What is the nature of this trust?	At y'r. how much will you (be) receiving and how often?	Are you receiving anything now?	When will payments start?	(If dis.) What was done with the money?	Records Y N A
<input type="checkbox"/>	<input type="checkbox"/>			\$ /	<input checked="" type="checkbox"/> <u>Y</u>	<input checked="" type="checkbox"/> <u>N</u> →		<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>

7. Business(es) other than this farm

a. Have you or your _____ sold or invested in any business(es) other than this farm since 7-1-60? ☒ Y→ (fill in below)

Since 7-1-60	Inv. Sold	Which mmbtr(s)?	What type of business or property is it?	How much was invested (rec'd)?	Where did the money come from? (What was done with the money?)	If joint, what pct is owned?	Records Y N A
<input type="checkbox"/>	<input type="checkbox"/>			\$		<input checked="" type="checkbox"/> <u>N</u>	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>A</u>
<input type="checkbox"/>	<input type="checkbox"/>					<input checked="" type="checkbox"/> <u>N</u>	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>A</u>

b. Are you or any members of your family planning to buy or sell any land, real estate or business in the next three months?

Which mmbtr(s)?	Buy or sell? Buy Sell	What will be bought (sold)?	Approx. how much will it cost (bring)?	Where is the money coming from? (What will be done with the money?)	Records Y N A
<input checked="" type="checkbox"/> <u>N</u>	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>S</u>		\$		
	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>S</u>				
	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>S</u>				

8. Mortgages and loans to others

a. Since 7-1-60, what changes have there been in mortgages or other loans, representing money loaned to others by you or your _____? ☒ Y→ (If yes fill in details below)

Which mmbtr(s)?	Who did you loan this money to?	What was the loan for?	What has been the change since 7-1-60?	Where is the money coming from? (What will be done with the money?)	Records Y N A
			\$	<input checked="" type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>A</u>	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>
				<input checked="" type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>A</u>	<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>

b. Are you or any members of your immediate family planning to loan out any money in the next three months?

Which mmbtr(s)?	Recipient	Type of loan Mgs. Cont. Loan	How much will be involved?	Where is the money coming from?
<input checked="" type="checkbox"/> <u>N</u>		<input type="checkbox"/> <u>Y</u> <input type="checkbox"/> <u>N</u> <input type="checkbox"/> <u>A</u>	\$	

IV. INCOME POSITION

1. After taking into account current operating expenses including allowances for depreciation, how does the net money income expected from this farm compare with the net money income in 1959? First, . . .

	1959		Records		1960 Amount
	Amount		Y	N A	
a. . . a year, what was the net money income from this farm in 1959? What do you expect it to be this year?	\$		<input checked="" type="checkbox"/>	<input type="checkbox"/>	\$
b. How do you expect income from other sources this year differ from that in 1959?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
i. At yr, what income did you receive, after depreciation, from the rental or operation of other farms in 1959? How much do you expect this year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
ii. What about net income after depreciation from other real estate or businesses in 1959? This year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
iii. What about wages and salaries received from others before deductions in 1959? This year? (Include bonuses and commissions)			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
iv. . . income from veterans' pensions and compensation, veterans' school allotment, servicemen's family allotment in 1959? This year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
v. . . interest, dividends, trust funds in 1959? This year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
vi. . . royalties on mineral leases in 1959? This year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
vii. . . Any inheritances or gifts in 1959? Any this year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
viii. What gains or losses did you have in 1959 from sales of stock or property? What do you expect from this source this year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
ix. . . any other income in 1959? From which sources?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	
x. . . What about income from other sources this year?			<input checked="" type="checkbox"/>	<input type="checkbox"/>	

ci. Did _____ (mention family member) have any income in 1959?

[illegible]

↑

↑

↑

□ □ ☒ ☒

↑

ii. What income do you expect from such sources this year?

[illegible]

d. So your total family income in 1959 was and in 1960 you expect it to be

[illegible]

e. How much do you estimate was the value of the wear and tear (depreciation) on equipment and machinery, and buildings on this farm in 1959?

How much do you estimate was spent during 1959 on equipment and machinery, and buildings on this farm in 1959?

What do you expect it to be this year?

2a. How did your total family income in 1959 compare with the average income of your household over the preceding 2 or 3 years? (Card C)

Higher by 20% or more ☐ ☐ ☐

Higher by 10-19%

Within 10% ☐ (Skip to c)

Lower by 10-19%

Lower by 20% or more ☐

b. Were there any unusual circumstances which served to bring this about?

What?

c. Do you believe your total income this year will be influenced by unusual circumstances?

What?

APPENDIX B

TABLE 1. DISTRIBUTION OF COOPERATIVENESS RATINGS, BY RESPONDENT
CHARACTERISTICS, STUDY P1, WAVE 1

Characteristic	Category	Base number	Percentage of total respondents rated			
			Excel- lent	Good	Fair	Poor or very poor
Value of home	Renter	46	39	43	17	0
	Under \$10,000	8	38	38	12	12
	\$10,000 to \$24,999	67	61	25	4	9
	\$25,000 to \$49,999	68	51	21	19	9
	\$50,000 or more	9	67	21	0	11
Age of main wage earner	Under 35	33	49	33	12	6
	36 to 49	54	54	30	13	4
	50 to 64	89	53	29	10	8
	65 or over	23	49	17	22	13
Size of savings unit	1	24	42	33	21	4
	2	60	45	30	13	12
	3	38	40	40	13	8
	4	41	66	17	12	5
	5 or more	36	67	25	5	3
Nationality	United States	186	52	27	13	8
	Other	13	54	46	0	0
Occupation	Professional- manager	70	57	26	11	6
	Self-employed	40	42	30	12	15
	Clerical-sales	40	50	39	10	2
	Laborer	19	68	21	10	0
	Retired	14	57	14	21	7
	Other	14	36	43	21	0
Education	Grade school	28	54	32	4	11
	High school	54	44	31	20	4
	College or more	113	57	24	11	8
Income of savings unit	Under \$5,000	30	40	50	10	0
	\$5,000 to \$9,999	69	57	28	12	4
	\$10,000 to \$24,999	63	57	29	17	8
	\$25,000 or more	24	62	17	8	12
Gross value of assets	Under \$10,000	19	42	47	10	0
	\$10,000 to \$49,999	61	57	31	10	2
	\$50,000 to \$99,999	36	61	31	6	3
	\$100,000 or more	47	66	19	11	4
Amount in savings accounts . .	None	27	44	37	7	11
	Under \$100	26	61	23	11	4
	\$100 to \$999	47	57	30	13	0
	\$1,000 to \$4,999	32	62	28	9	0
	\$5,000 or more	28	68	25	7	0

TABLE 2. DISTRIBUTION OF ACCURACY RATINGS, BY RESPONDENT
CHARACTERISTICS, STUDY P1, WAVE 1

Characteristic	Category	Base number	Percentage of total respondents rated			
			Excel- lent	Good	Fair	Poor or very poor
Value of home	Renter	45	33	29	24	13
	Under \$10,000	8	25	38	12	25
	\$10,000 to \$24,999	67	40	36	12	12
	\$25,000 to \$49,999	65	37	17	23	23
	\$50,000 or more	14	21	21	7	50
Age of main wage earner	Under 35	32	34	38	19	9
	36 to 49	54	37	26	19	19
	50 to 64	87	37	28	16	20
	65 or over	23	35	22	22	22
Size of savings unit	1	24	38	29	17	17
	2	58	34	22	17	26
	3	37	27	35	24	13
	4	40	48	20	18	15
	5 or more	34	35	41	12	12
Nationality	United States	182	36	29	18	18
	Other	13	39	46	15	0
Occupation	Professional- manager	70	37	27	23	13
	Self-employed	38	34	24	13	29
	Clerical-sales	38	32	42	10	16
	Laborer	19	53	32	16	0
	Retired	14	43	35	7	14
	Other	14	29	29	21	21
Education	Grade school	28	39	36	7	18
	High school	58	33	24	24	19
	College or more	106	39	27	17	17
Income of savings unit	Under \$5,000	30	47	30	17	7
	\$5,000 to \$9,999	67	37	33	22	8
	\$10,000 to \$24,999	62	42	26	14	18
	\$25,000 or more	23	26	35	13	26
Gross value of assets	Under \$10,000	20	45	35	15	5
	\$10,000 to \$49,999	60	37	40	18	5
	\$50,000 to \$99,999	36	44	28	17	11
	\$100,000 or more	44	50	23	18	9
Amount in savings accounts . . .	None	31	35	35	13	16
	Under \$100	25	36	44	12	8
	\$100 to \$999	46	43	33	13	11
	\$1,000 to \$4,999	31	45	16	26	13
	\$5,000 or more	28	54	25	21	0

TABLE 3. DISTRIBUTION OF COMPLETENESS RATINGS, BY RESPONDENT CHARACTERISTICS, STUDY P1, WAVE 1

Characteristic	Category	Base number	Percentage of total respondents rated			
			Excellent	Good	Fair	Poor or very poor
Value of home.	Renter	43	33	40	21	7
	Under \$10,000	8	13	50	12	25
	\$10,000 to \$24,999	67	46	34	10	9
	\$25,000 to \$49,999	66	35	23	21	21
	\$50,000 or more	9	33	33	0	33
Age of main wage earner.	Under 35	32	38	34	9	19
	36 to 49	41	29	39	17	15
	50 to 64	89	35	33	19	13
	65 or over	23	30	26	22	22
Size of savings unit.	1	23	35	43	13	9
	2	59	32	27	17	24
	3	37	24	35	30	11
	4	39	49	26	18	8
	5 or more	36	47	36	3	14
Nationality.	United States	201	34	28	15	13
	Other	11	27	55	18	9
Occupation.	Professional-manager	69	36	30	19	14
	Self-employed	38	32	32	13	24
	Clerical-sales	40	38	40	13	10
	Laborer	18	67	17	17	0
	Retired	14	36	29	14	21
	Other	13	23	46	31	0
Education.	Grade school	32	34	38	16	13
	High school	49	43	24	20	12
	College or more	109	37	33	14	17
Income of savings unit.	Under \$5,000	30	37	50	10	3
	\$5,000 to \$9,999	67	43	31	18	7
	\$10,000 to \$24,999	64	39	28	17	16
	\$25,000 or more	22	32	36	14	18
Gross value of assets.	Under \$10,000	20	35	40	25	0
	\$10,000 to \$49,999	59	46	41	12	2
	\$50,000 to \$99,999	36	44	31	14	11
	\$100,000 or more	45	44	33	13	9
Amount in savings accounts.	None	28	39	32	11	18
	Under \$100	25	36	40	12	12
	\$100 to \$999	46	39	39	20	2
	\$1,000 to \$4,999	31	48	26	19	6
	\$5,000 or more	28	46	36	14	4

TABLE 4. DISTRIBUTION OF USE-OF-RECORDS REPORTS, BY RESPONDENT CHARACTERISTICS, STUDY P1, WAVE 1

Characteristic	Category	Base number	Percentage of total reporting use		
			Always	Sometimes	Never
Value of home.....	Renter	49	27	18	55
	Under \$10,000	5	20	60	20
	\$10,000 to \$24,999	66	35	36	29
	\$25,000 to \$49,999	57	26	19	54
	\$50,000 or more	6	17	17	67
Age of main wage earner.....	Under 35	35	46	20	34
	36 to 49	50	34	26	40
	50 to 64	80	20	33	48
	65 or over	18	22	11	67
Size of savings unit. . .	1	24	17	25	58
	2	57	21	30	49
	3	35	17	34	49
	4	25	36	28	36
	5 or more	23	13	26	61
Nationality.....	United States	171	30	28	43
	Other	12	17	8	75
Occupation.....	Professional-manager	62	32	31	37
	Self-employed	39	18	8	74
	Clerical-sales	41	27	41	32
	Laborer	18	56	11	33
	Retired	9	22	22	56
	Other	14	21	36	43
Education.....	Grade school	30	33	30	37
	High school	48	23	31	46
	College or more	102	31	23	45
Income of savings unit	Under \$5,000	31	19	35	45
	\$5,000 to \$9,999	66	41	26	33
	\$10,000 to \$24,999	60	28	27	45
	\$25,000 or more	19	16	21	63
Gross value of assets. . .	Under \$10,000	21	29	19	52
	\$10,000 to \$49,999	62	39	34	27
	\$50,000 to \$99,999	31	19	39	42
	\$100,000 or more	43	37	21	42
Amount in savings accounts.....	None	30	37	13	50
	Under \$100	25	36	40	24
	\$100 to \$999	46	35	30	35
	\$1,000 to \$4,999	29	17	38	45
	\$5,000 or more	28	39	25	36

TABLE 5. DISTRIBUTION OF EVIDENCE OF WITHHOLDING DATA,
BY RESPONDENT CHARACTERISTICS, STUDY P1, WAVE 1

Characteristic	Category	Base number	Percentage of total withholding data		
			No	Not sure	Yes
Value of home.....	Renter	47	70	17	13
	Under \$10,000	8	50	25	25
	\$10,000 to \$24,999	68	82	1	16
	\$25,000 to \$49,999	68	60	10	29
	\$50,000 or more	9	67	11	22
Age of main wage earner...	Under 35	33	76	15	9
	36 to 49	55	78	5	16
	50 to 64	90	64	11	24
	65 or over	23	61	4	35
Size of savings unit.....	1	25	72	12	16
	2	60	62	12	27
	3	38	63	8	29
	4	42	79	10	12
	5 or more	36	78	6	17
Nationality.....	United States	188	69	10	21
	Other	13	77	0	23
Occupation.....	Professional- manager	71	77	6	17
	Self-employed	39	54	10	36
	Clerical-sales	41	76	10	15
	Laborer	19	89	11	0
	Retired	14	50	7	43
	Other	15	53	27	20
Education.....	Grade school	28	71	11	18
	High school	54	67	11	22
	College or more	114	72	7	21
Income of savings unit.....	Under \$5,000	31	77	19	3
	\$5,000 to \$9,999	70	81	7	11
	\$10,000 to \$24,999	63	70	6	24
	\$25,000 or more	24	58	17	25
Gross value of assets.....	Under \$10,000	20	75	25	0
	\$10,000 to \$49,999	62	90	3	6
	\$50,000 to \$99,999	36	67	11	22
	\$100,000 or more	48	75	10	15
Amount in savings accounts	None	28	68	11	21
	Under \$100	24	71	12	17
	\$100 to \$999	48	85	4	10
	\$1,000 to \$4,999	33	79	15	6
	\$5,000 or more	29	83	3	14

TABLE 6. DISTRIBUTION OF USE-OF-RECORDS RATINGS, BY RESPONDENT CHARACTERISTICS, STUDY P2, WAVE 1

Characteristic	Category	Base number	Percentage of total respondents rated	
			Always or sometimes	Never
Value of home	Renter	34	62	38
	Under \$10,000	83	47	53
	\$10,000 to \$24,999	141	62	38
	\$25,000 to \$49,999	31	52	48
	\$50,000 or more	5	40	60
Age of main wage earner	Under 35	22	59	41
	36 to 49	75	63	37
	50 to 64	108	57	43
	65 or over	87	48	52
Size of savings unit	1	57	47	52
	2	126	55	45
	3	58	57	43
	4	29	72	28
	5 or more	24	63	37
Nationality	United States	254	57	43
	Other	40	50	50
Occupation	Professional-manager	63	56	44
	Self-employed	30	63	37
	Clerical-sales	44	57	43
	Laborer	88	64	36
	Retired	63	44	56
	Other	5	40	60
Education	Grade school	81	52	48
	High school	107	55	45
	College or more	103	61	39
Income of savings unit	Under \$5,000	101	50	50
	\$5,000 to \$9,999	106	66	34
	\$10,000 to \$24,999	64	59	41
	\$25,000 or more	4	50	50
Gross value of assets	Under \$10,000	29	62	38
	\$10,000 to \$49,999	137	65	35
	\$50,000 to \$99,999	42	76	24
	\$100,000 or more	28	57	43
Amount in savings accounts . . .	Under \$100	19	63	37
	\$100 to \$999	70	51	49
	\$1,000 to \$4,999	114	61	39
	\$5,000 or more	91	52	48

TABLE 7. DISTRIBUTION OF WILLINGNESS-TO-GIVE-FINANCIAL-INFORMATION RATINGS, BY RESPONDENT CHARACTERISTICS, STUDY P2, WAVE 5

Characteristic	Category	Base number	Percentage of total respondents rated			
			Excel- lent	Good	Fair	Poor or very poor
Value of home	Renter	22	59	36	0	5
	Under \$10,000	53	47	34	17	2
	\$10,000 to \$24,999	105	43	38	13	6
	\$25,000 to \$49,999	20	45	45	10	0
	\$50,000 or more	5	80	0	0	20
Age of main wage earner	Under 35	17	29	59	6	6
	36 to 49	62	56	27	11	5
	50 to 64	75	47	39	12	3
	65 or over	50	42	38	14	6
Size of savings unit	1	28	46	29	18	7
	2	86	45	41	12	2
	3	43	51	33	7	9
	4	26	38	38	23	0
	5 or more	22	54	36	5	5
Nationality	United States	177	50	34	12	5
	Other	28	29	54	14	4
Occupation	Professional- manager	48	52	35	10	2
	Self-employed	21	62	33	5	0
	Clerical-sales	31	32	48	10	10
	Laborer	69	48	35	14	3
	Retired	32	44	34	16	6
	Other	4	25	25	25	25
Education	Grade school	51	45	37	12	6
	High school	75	39	37	19	5
	College or more	79	56	35	6	3
Income of savings unit	Under \$5,000	62	48	29	16	6
	\$5,000 to \$9,999	91	46	40	12	2
	\$10,000 to \$24,999	45	44	44	7	2
	\$25,000 or more	4	75	25	0	0
Gross value of assets	Under \$10,000	18	56	33	6	6
	\$10,000 to \$49,999	124	46	36	13	5
	\$50,000 to \$99,999	37	51	35	11	3
	\$100,000 or more	23	41	50	5	5
Amount in savings accounts	Under \$100	14	36	57	7	0
	\$100 to \$999	48	44	44	8	4
	\$1,000 to \$4,999	76	55	32	9	4
	\$5,000 or more	62	45	34	16	5

TABLE 8. DISTRIBUTION OF SAVINGS-ACCOUNT-ACCURACY RATINGS,
BY RESPONDENT CHARACTERISTICS, STUDY P2, WAVE 1

Characteristic	Category	Base number	Percentage of total respondents rated		
			Fully	Fairly	Not much or very little
Value of home.....	Renter	31	58	29	13
	Under \$10,000	73	37	42	21
	\$10,000 to \$24,999	134	53	37	10
	\$25,000 to \$49,999	30	47	33	20
	\$50,000 or more	4	50	50	0
Age of main wage earner.....	Under 35	22	59	23	18
	36 to 49	70	54	37	10
	50 to 64	100	52	37	11
	65 or over	79	37	42	21
Size of savings unit..	1	48	31	50	19
	2	118	50	35	15
	3	55	51	35	14
	4	28	61	32	7
	5 or more	24	54	38	8
Nationality.....	United States	117	50	36	14
	Other	15	43	43	14
Occupation.....	Professional- manager	61	56	38	7
	Self-employed	27	37	37	26
	Clerical-sales	43	47	37	16
	Laborer	79	56	30	14
	Retired	58	38	43	19
	Other	4	50	0	50
Education.....	Grade school	72	40	39	21
	High school	98	49	36	15
	College or more	99	55	36	9
Income of savings unit.....	Under \$5,000	90	41	41	18
	\$5,000 to \$9,999	101	55	37	8
	\$10,000 to \$24,999	62	53	34	13
	\$25,000 or more	4	25	50	25
Gross value of assets	Under \$10,000	27	59	26	15
	\$10,000 to \$49,999	128	58	34	8
	\$50,000 to \$99,999	39	54	41	5
	\$100,000 or more	28	50	39	11
Amount in savings accounts.....	Under \$100	27	48	48	4
	\$100 to \$999	61	48	36	16
	\$1,000 to \$4,999	94	54	37	9
	\$5,000 or more	78	46	40	14

TABLE 9. DISTRIBUTION OF LOW RATINGS ON ACCURACY
AND COMPLETENESS OF CHECKING ACCOUNT BALANCES,
BY RESPONDENT CHARACTERISTICS, STUDY P3, WAVE 1

Characteristic	Category	Base number		Percent rated "not much" or "very little" on	
		Accu- racy	Com- pleteness	Accu- racy	Com- pleteness
Condition of home..	Excellent	41	40	9	0
	Good to excellent	171	169	12	4
	Fair to good	89	87	15	6
	Poor to fair	43	44	14	14
Age of farmer.....	Under 35	51	50	8	2
	35 to 49	135	131	12	2
	50 to 64	115	114	17	11
	65 or over	48	50	10	4
Size of savings unit..	1	24	22	13	9
	2	106	106	16	7
	3	60	60	17	7
	4	58	56	16	0
	5 or more	99	99	6	4
Nationality.....	United States	317	314	14	6
	Other	12	11	8	0
Tenure status.....	Owner	230	229	14	6
	Operator only	119	116	10	4
Education.....	Grade school	118	120	18	8
	High school	199	193	10	4
	College or more	32	32	9	7
Income of savings unit.....	Under \$2,500	126	125	11	2
	\$2,500 to \$4,999	98	105	10	5
	\$5,000 to \$9,999	78	77	12	1
	\$10,000 to \$14,999	16	16	25	13
	\$15,000 or more	11	11	9	0
Value of assets.....	Under \$25,000	62	61	10	5
	\$25,000 to \$49,999	88	89	17	6
	\$50,000 to \$99,999	106	101	5	2
	\$100,000 or more	72	74	17	8
Size of farm.....	Under 100 acres	44	48	18	10
	100 to 199 acres	85	80	12	6
	200 to 299 acres	85	87	12	5
	300 acres or more	134	129	12	3
Amount of farm debt	Under \$1,000	72	69	14	12
	\$1,000 to \$4,999	93	95	18	5
	\$5,000 to \$9,999	71	70	8	3
	\$10,000 or more	109	107	9	3
Amount in savings accounts.....	None	217	215	12	4
	Under \$1,000	67	66	3	5
	\$1,000 to \$4,999	34	33	21	6
	\$5,000 or more	19	19	11	5

TABLE 10. DISTRIBUTION OF LOW RATINGS ON FIGURES WITHHELD
AND LACK OF KNOWLEDGE OF FIGURES, BY RESPONDENT
CHARACTERISTICS, STUDY P3, WAVE 1

Characteristic	Category	Base number	Percentage rated "yes" on	
			Data withheld	Lack of knowledge
Condition of home.	Excellent	42	5	2
	Good to excellent	175	9	18
	Fair to good	96	7	15
	Poor to fair	46	13	20
Age of farmer.	Under 35	51	2	10
	35 to 49	139	4	10
	50 to 64	122	13	18
	65 or over	52	13	31
Size of savings unit.	1	25	8	20
	2	113	13	20
	3	64	8	14
	4	59	5	8
	5 or more	101	5	15
Nationality.	United States	331	9	16
	Other	13	0	15
Tenure status.	Owner	240	11	19
	Tenant	124	3	9
Education.	Grade school	127	12	18
	High school	203	16	16
	College or more	34	6	6
Income of savings unit.	Under \$2,500	132	8	14
	\$2,500 to \$4,999	112	4	13
	\$5,000 to \$9,999	80	5	15
	\$10,000 to \$14,999	17	6	29
	\$15,000 or more	11	18	27
Value of assets.	Under \$25,000	68	4	12
	\$25,000 to \$49,999	91	8	18
	\$50,000 to \$99,999	108	5	14
	\$100,000 or more	75	12	19
Size of farm.	Under 100 acres	52	15	29
	100 to 199 acres	87	10	15
	200 to 299 acres	87	3	13
	300 acres or more	137	7	13
Amount of farm debt.	Under \$1,000	75	17	19
	\$1,000 to \$4,999	99	8	20
	\$5,000 to \$9,999	74	3	9
	\$10,000 or more	111	5	14
Amount in savings accounts	None	226	6	14
	Under \$1,000	71	4	14
	\$1,000 to \$4,999	34	12	26
	\$5,000 or more	20	15	20

TABLE 11. DISTRIBUTION OF USE-OF-RECORDS RATINGS, BY RESPONDENT CHARACTERISTICS, STUDY P3, WAVE 1

Characteristic	Category	Base number	Percentage of category rated		
			Always	Sometimes	Never
Condition of home	Excellent	42	31	48	21
	Good to excellent	173	16	56	28
	Fair to good	96	18	55	27
	Poor to fair	46	15	41	44
Age of farmer	Under 35	49	33	47	20
	35 to 49	139	15	60	25
	50 to 64	122	20	48	33
	65 or over	52	10	50	40
Size of savings unit	1	25	20	60	20
	2	113	15	48	37
	3	64	20	50	30
	4	59	20	47	32
	5 or more	99	18	61	21
Nationality	United States	329	18	52	30
	Other	13	8	77	15
Tenure status	Owner	239	16	51	33
	Tenant	123	22	56	22
Education	Grade school	126	14	49	37
	High school	202	19	55	25
	College or more	34	26	47	26
Income of savings unit	Under \$2,500	132	24	48	28
	\$2,500 to \$4,999	111	16	55	29
	\$5,000 to \$9,999	80	14	64	22
	\$10,000 to \$14,999	17	18	65	18
	\$15,000 or more	11	9	27	64
Value of assets	Under \$25,000	68	16	59	25
	\$25,000 to \$49,999	89	20	49	30
	\$50,000 to \$99,999	108	22	53	25
	\$100,000 or more	75	13	57	29
Size of farm	Under 100 acres	52	6	54	40
	100 to 199 acres	87	17	51	32
	200 to 299 acres	85	21	58	21
	300 acres or more	137	22	50	28
Amount of farm debt . .	Under \$1,000	75	20	49	31
	\$1,000 to \$4,999	99	15	55	30
	\$5,000 to \$9,999	73	29	49	22
	\$10,000 or more	110	14	56	30
Amount in savings accounts	None	225	15	56	30
	Under \$1,000	70	26	57	17
	\$1,000 to \$4,999	34	24	38	38
	\$5,000 or more	20	25	40	35



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