AGRICULTURAL ECONOMICS RESEARCH

A Journal of Economic and Statistical Research in the
Bureau of Agricultural Economics and Cooperating Agencies

Volume II JULY 1950 Number 3

Sampling Methods in Marketing Research

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Such keen interest was created by the papers presented by this author before the annual meeting of The Association of Southern Agricultural Workers at Biloxi, Mississippi, last February, and before a meeting later that month which was sponsored by the Committee on Experimental Design in the United States Department of Agriculture, that the editors asked for the content of them to give to the readers of this magazine.

"ALTHOUGH THE USE of sampling necessarily introduces certain inaccuracies, owing to sampling errors, the results obtained by sampling are frequently more accurate than those obtained in a complete census or survey. The random sampling errors are always assessable. The other errors to which a survey is subject, such as incompleteness of returns and inaccuracy of information, are liable to be very much more serious in a complete census than in a sample census. Furthermore, the use of sampling greatly facilitates the imposition of additional more detailed checks. Indeed, a complete census can only be properly tested for accuracy by some form of sampling check." This is Frank Yates in his recent excellent book on sampling.[1]

This fact that a sample can be more accurate than a census, under certain conditions, is becoming widely accepted. The explanation is simple. With the exception of rather unusual cases, surveys and censuses are subject to many errors which have little, if anything, to do with the way a sample is selected. The challenging problem is often how to get accurate and useful information from respondents, or how to keep errors due to causes other than sampling at a minimum—not how to design an efficient and adequate sample. There is no implication here that designing a sample is no longer a problem. But the emphasis is on the need for a better sense of proportion as to the probable magnitude of various components of error in survey results. We are all concerned about the costs of surveys, but too often the concern is to get as much data as possible with the funds without sufficient regard for quality, overlooking the simple truth that work of higher quality even with a smaller sample might provide more accurate results. Emphasis on holding down the costs may also lead to use of inefficient sampling designs. As sample size is only one of many factors that influence accuracy, we should attempt to maximize the accuracy for a given amount of money and not to minimize the cost per schedule in order to get as much data as possible. Results of research on sampling methods, over the last 10 years, are helpful in the attempt to achieve maximum accuracy per dollar spent.

A common deficiency of surveys is inadequate clarification and development of the objectives, the hypothesis to be tested, the needs for the data, the definition of the data, the tabulation

[1] YATES, F. SAMPLING METHODS FOR CENSUSES AND SURVEYS. Hafner Publishing Company, New York; Charles Griffin & Company, Ltd., London, 1949. 298 pp.

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plans, and the content of the resulting report. That is, the whole plan of the survey is left in terms which are too nebulous. The end result is a report that lacks unambiguous answers to specific questions or that overlooks certain phases of the problem which should have been stated during the planning of the survey. This is true particularly when working in new areas of study because an adequate formulation of the problem may depend upon advance informal investigation or case studies, followed by the pre-testing of a questionnaire which, incidentally, should be a part of every survey. Adequate advance planning should help to avoid the situation in which a research worker discovers, when involved in the analysis phase of the project, that certain much needed tabulations cannot be made because either the sample or the questionnaire was not properly designed.

Only part of the benefit derived from the application of modern statistical methods is attributable to the statistical design. Much benefit results from better planning, since good planning is essential to successful application of modern statistical methods. Some surveys, however, are centered around the sampling plan *per se* which is expected to carry nearly the whole burden of providing useful and accurate results. In the interest of trying to use good sampling methods, a sampling plan is sometimes chosen prematurely and around it the study is developed. This is putting the cart before the horse. Logically, the objectives should be clarified first. Then the sampling design should be developed as a tool for better accomplishment of these objectives. Actually, the expressed objectives are commonly limited by various factors including feasibility of alternative sampling plans, but the emphasis should be on adapting a sampling design to the objectives, and not vice versa.

The preceding remarks may seem trite to many research workers but they represent ideas which the writer feels are too often overlooked, ignored, or not understood. Attention is now turned to selected recent experiences with the application of modern sampling methods including controlled experiments in stores.

Sampling Retail Grocery Stores

The first illustration involves the application of objective methods of sampling for the purpose of making a study of the problems and possibilities of developing a retail market news service.[2] Briefly, plans for the study called for weekly estimates of the prices of about 125 food items and estimates of the volume of about 40 of these items. The 125 items included canned, frozen, and fresh fruits and vegetables, meats, and dairy and poultry products. As the sample was built mainly with the end in mind of estimating prices and volume of specified items for the city of Baltimore, including suburban areas, the first example is represented by a brief description of the techniques used to obtain a sample of stores for city-wide statistics, in contrast to the next example which relates to experiments in retail stores.

For purposes of sampling, the universe was divided into three parts: Independent stores, chain stores, and stalls in the city markets. This discussion is confined to prices and to the methods of obtaining the sample of independent stores used for weekly reports.

Two possible bases for sampling were weighed: (1) Lists and (2) area sampling through the use of Sanborn or street maps. There were two sources of lists: A newspaper and the Office of the Retail Sales Tax Collector. These lists gave very little information about the characteristics of individual stores. Those on the list from the newspaper were classified into three volume classes and by type of ownership, but according to field checks the list was very inaccurate and the volume classification was very poor. The list from the tax collector's office was no doubt more accurate in the sense of giving correct addresses, but from it grocery stores could not be separated from certain other types of stores, no information on size of store was given, and as the stores on the list could not be easily classified geographically there was no basis for stratification.

It was decided that a preliminary survey of about 400 to 500 stores was advisable using a short questionnaire to obtain information on the characteristics of each store, including size in terms of total dollar volume of business, the type of store, type of ownership, commodities handled, and volume of business done on a credit or delivery basis. Such a sample would provide a good basis for selecting a smaller subsample to be used for

[2] A project under the Research and Marketing Act of 1946 conducted by the Production and Marketing Administration.

weekly reports. If the sample had been wanted for a one-time survey, probably a single-phase sampling plan would have been used instead of the double-phase sampling plan here described. For this quick preliminary survey, the Sanborn maps provided the best basis for sampling.

On the Sanborn maps (which are available for most cities that have more than about 10,000 inhabitants) the buildings within each city block are indicated, together with limited information as to use, structure, and size. It is possible to identify the blocks having retail outlets, but not the type of outlet. In areas that have coverage by Sanborn maps, the Bureau of the Census has, as part of the Master Sample project, a listing of all blocks within which retail outlets are indicated. These lists were used for the territory within Baltimore proper. Blocks containing retail outlets were arrayed in geographical order and every sixth block was selected, which provided a sample of 609 blocks. These blocks were visited and a short questionnaire was completed for each independently owned grocery store, including meat markets, fruit and vegetable stands, and delicatessens, but excluding national chains and stalls in city markets. To check on accuracy of the Sanborn maps, a sample was selected comprising 52 blocks on which no retail stores were indicated. On investigation no stores were found on any of these blocks, so we were satisfied that in this case the Sanborn maps gave an excellent basis for sampling.

In the outlying districts of Baltimore, the Sanborn map coverage was inadequate. This left two alternatives: To select a sample of all blocks from a street map or to select a sample from the newspaper list. It was decided to use the newspaper list because the outlying areas have many dead-end streets and other features which cause difficulty in defining blocks on the map that can be located in the field. This is particularly true of maps that are not up-to-date. Moreover, as the blocks on which retail outlets were located could not be distinguished in the office many sample blocks would be visited on which there were no stores.

A total of 470 independent stores were visited in the preliminary survey. After much discussion on designing the subsample of the 470 stores, it was decided to group them into 36 strata as follows: The stores were first classified into six groups by type: (1) Self service stores, (2) service stores carrying a complete line of commodities (that is, all major commodity groups such as meats, produce, and canned goods), (3) service stores with an incomplete line of commodities, (4) delicatessens, (5) fruit and vegetable specialty stores, and (6) meat markets. The most numerous type—service stores carrying complete lines—was subdivided into three groups on the basis of the percentage of the total dollar volume of business which was done on a credit or delivery basis or on both, as these factors affect price. This gave a total of eight groups which were further stratified as to total dollar volume of business, giving the 36 strata. These strata were made of approximately equal size in terms of total dollar volume of business. Strata comprised of the largest stores contained only three stores each, compared with one stratum which consisted of 39 small stores. One store was selected at random (equal probability) from each stratum. Hence a store's chance of being in the sample was approximately in proportion to its size.

The sample and subsample were so designed primarily for two reasons: (1) For purposes of estimating total volume of sales of the various commodities, the large stores should have a much greater chance of being in the sample. (2) The specifications of data indicated a preference for estimates of a city average price weighted by sales volume, rather than an unweighted price. A simple arithmetic average price from a sample is automatically properly weighted under the following hypothetical situation, taking eggs as an example. If the volume of eggs sold is proportional to total volume of business and the chance of a store being in the sample is proportional to its total volume of business, then a simple unweighted average of the egg prices from the sample stores is automatically an average price weighted by total volume of eggs sold. Exact proportionality from store to store between the sales of a particular commodity and total sales obviously does not exist, but it was hoped that a simple average price from a sample as described above would be close enough to the desired weighted average price so that weighting of the sample prices would not be necessary.

This idea of aiming toward a sample which would automatically give properly weighted prices led to modifications in the sample design which have not yet been pointed out. Prices of meats in a meat market that handles only meats and which has a business of $2,000 per week should have more weight in an average meat price than do prices of meat in a general grocery store that has a total general business of $2,000 per week. As a result, an exception to having the strata of equal size was made in the case of meat markets, which were one of the eight type-of-store groups. Strata comprised of meat markets were made only about one-third as large as the strata comprised of stores that carry all commodity groups, because it was estimated that meats constitute about one-third of the business of a complete-line store. Thus, a meat market with a business of $2,000 per week had about three times the chance of being in the sample as a complete-line grocery store with a business of $2,000 per week. This type of modification should give a slight improvement in the sample used for estimating both prices of meat and volume of sales. A similar modification in the sample design was made for stores that specialize in fresh produce and for stores that carry an incomplete line of commodities. Such arbitrary decisions affect the statistical efficiency of the sample, but each store had a known probability of being selected, so the sample is a probability sample.

For this sample of 36 independent stores the sampling errors, at the 95-percent probability level applying to differences in price from week to week for five selected commodities, were estimated as follows:

Commodity:	*Sampling error* [1] *(cents)*
Eggs, large grade A	0.6
New potatoes	.6
Ground beef	1.5
Canned green beans	.5
Canned peaches, 2½	.8

[1] Sampling errors pertain to estimated differences between weeks for the same stores for a 5-week period beginning June 6, 1948.

Prices for independent stores are combined with prices for chain stores and stalls in public markets. The combined average prices will have lower sampling errors than these just indicated.

Controlled Experiments in Retail Stores

Next, a type of statistical design is indicated which might be useful in a case in which the appropriate approach to a problem might be through a controlled experiment in retail stores. Designs of this type are being used for analogous situations in biological fields.

Let us begin with a case which involves a simpler statistical design than is found with experiments in retail stores. Consider a problem of testing three different kinds of tubes for coring bags of grease wool, the purpose being to learn the best kind for sampling bags of wool when the yield is to be estimated—that is, the ratio of scoured wool to grease wool.

Here are two possible experimental designs, each involving six bags of wool and the taking of six cores from each bag. (1) Divide the six bags at random into three groups of two. In the first group take six cores from each bag with the first tube; in the second group take six cores from each bag with the second tube; and so forth. This design is inefficient in comparing the coring devices because the observed differences among tubes include variability attributable to differences among bags. (2) Take two cores from each bag with each instrument. This design gives a more accurate comparison between the tubes because variations among bags do not influence the observed differences among the tubes.

The problem just presented is over-simplified, but without going into the statistical analysis it gives an illustration of how, through the use of an appropriate design, more accurate information can be obtained with about the same effort, or of how an inappropriate design might fail to provide the information sought.

Turning now to experiments in retail stores, the analogy is that the stores correspond to the bags of wool and the treatments (which might be differences in display, packaging, grading or prices, etc.) correspond to the coring tubes. But from the standpoint of statistical methodology there is at least one major difference: In the case of the wool, we were not particularly concerned with the time element. Then, too, there was an assumption that the taking of two cores from a bag with one coring tube did not appreciably change the characteristics of the contents of the bag for the second tube.

With retail stores, the time element commonly enters the picture in two important ways. (1) Certain trends may take place through time regardless of whether or not a treatment is applied. (2) There may be certain carry-over effects; that is, if treatment B, for example, follows treatment

A in the same store, the results observed for treatment B might be affected by the fact that treatment A preceded it.

In this connection Applebaum and Spears [3] have stated: "Where two or more variables are to be tested simultaneously, a different group of test stores will be required to investigate each variable. In addition, a group of control stores will also be necessary. For example, if the regular price of an item is 10¢ and the problem is to determine what sales results will be achieved at 9¢ and at 2/19¢, then three groups of experimental stores are required to carry out the tests simultaneously—a group for each price, 10¢, 9¢, 2/19¢. The element of time cannot be controlled in any other way."

Actually, there may be other methods of handling the time factor and carry-over effects that will prove to be useful—methods which are more efficient statistically, and at the same time give additional information.

Applebaum and Spears went on to say: "Of course in some experiments the element of time may be a very minor factor in the results, and each variable can be tested by successive steps. Also, in some experiments it is possible to test different variables by criss-crossing the tests between different groups of stores. A word of caution is in order here. The authors' experience indicates that to safeguard the validity of results it may be worth spending the extra money required to set up an additional group of test stores." It appears that the statistical advantages of applying all of the treatments in each store were recognized but that such a design was considered unsafe because of time trends or carry-over effects.

Analogous situations are found in biological fields. A good example is a dairy-cattle feeding experiment, reported by Cochran, Autrey, and Cannon, [4] to ascertain the differences in yield of milk when three rations were used. In an experiment in which each cow receives only one ration, the yield of milk observed for any given ration will depend upon both the producing ability of the animals (which is highly variable among cows) and the ration itself. Variation in producing ability can be partly controlled by an appropriate grouping of the cows, but variability among cows is still an influential contributor to the experimental error. If variability among cows is eliminated from the experimental error by feeding the three rations, successively, to each cow during a single lactation period, it is obvious that carry-over effects and characteristic changes in milk production during the lactation period should be taken into account, which is what the design used by Cochran and his associates did.

Let us examine this design to see what it would be like when used for retail stores—even though the design and the accompanying analysis cannot be fully discussed here. Consider three different treatments: A, B, and C. These three treatments might be three different types of display, three different prices on a particular grade of oranges, or three methods of packaging. Or the treatment might be complex, as, for example, three different price patterns involving several varieties and grades of apples. The effect of the treatments is measured in terms of volume of sales. For the experiment the number of stores should be a multiple of six: Let us assume it is 12. Six groups of stores would be set up so the members of each group would be as much alike as possible. Here is the lay-out for applying the three treatments to the six groups:

Period	Group of stores					
	1	2	3	4	5	6
1	A	B	C	A	B	C
2	B	C	A	C	A	B
3	C	A	B	B	C	A
4	A	B	C	A	B	C

There are six orders in which the treatments are applied which is the minimum number of orders if, during any period, each treatment is to follow every other treatment. (As there might be carry-over effects in the first period from whatever the situation was in each store before the experiment started, perhaps the treatments applied in the first period should be repeated during a fourth period as indicated in the above layout; in that event, the data for the first period might not be used in the analysis.) From the above

[3] Applebaum, William, and Spears, Richard F. Controlled experimentation in marketing research. Jour. Marketing. 14 (4): 505–517. Jan. 1950.

[4] Cochran, W. G., Autrey, K. M., and Cannon, C. Y. A double change-over design for dairy cattle feeding experiments. Jour. Dairy Science. 25: 937–951. 1941.

design, estimates of the direct effects of the treatments and the carry-over effects are estimated by the method of least squares. The mathematical solution involves setting up an equation for each period within each group of stores, assuming that the carry-over effects from the first period do not extend into the third period. For example, the equation representing the volume sold during periods 2, 3, and 4 for the first group would be:

$$V_2 = m + p_2 + b + a' + e_{21}$$
$$V_3 = m + p_3 + c + b' + e_{31}$$
$$V_4 = m + p_4 + a + c' + e_{41}$$

where m = average volume sold per store, the p's represent the average effects of the three periods, b, c, and a represent the direct effects of the treatments, a', b', c' represent the carry-over effects, and the e's are the experimental errors.

Actual experience with a few such designs is needed if several questions on technique are to be answered. Is it necessary to repeat the treatments applied in the first period? If the stores in the experiment vary widely in size, should something be done about heterogeneity of variance? Are the effects more nearly multiplicative than additive, suggesting that logarithms of the volumes be used in the analysis? How should the stores be selected?

Assuming that the change-over design is administratively feasible and that using it will meet the objectives of the study, its essential features, compared with setting up a separate group of stores for each treatment plus a control group, include the following.

1. Variation among stores is eliminated from comparisons of the effect of treatments. That is, the same number of stores will provide a more accurate comparison of the treatments. The question of the degree to which accuracy is improved can be estimated from an analysis of data obtained when change-over designs are used; it is unnecessary to use both designs simultaneously.

2. The carry-over effects can be estimated and, if they are important, the direct effects can be adjusted for carry-over effects. In some cases, the information provided on carry-over effects might be important. For example, if a store manager makes a special effort to reduce his stock of a particular commodity, what is the effect of this on his sales the following week?

3. It should not be necessary to have a control group of stores. If a comparison with some standard treatment or practice is desired, the standard treatment can be included in the lay-out as one of the treatments.

Sampling to Estimate Volume

As estimates of volume are frequently attempted with samples that are too small, it may be advisable to include a short discussion of sampling when volume is to be estimated. First, a simple contrast between sampling for prices and sampling for volume is given. Assume that a complete list of stores in some city is available, but that no information for the stores is given. Coefficients of variation, among all stores, pertaining to volume might be as large as 200 to 300 percent per store and higher; whereas the corresponding coefficients of variation for prices might be in the rough neighborhood of 20 to 30 percent. How much larger would a sample have to be if volume is to be estimated with a sampling error of 5 percent than it would have to be if the average price is to be estimated with a sampling error of 5 percent? Assuming a simple random sample, which is the best that could be done in the absence of any information about individual stores, the sample might have to be more than 100 times larger (unless corrections for finite populations come in). In other words, for a sample of a given size the relative sampling errors for estimates of volume could be as much as 10 or more times larger than for prices.

Compared with simple random sampling, available information on individual stores can be used in the sampling design or in the process of estimation to improve the accuracy of the results. In particular, if information is available on size of the store (or whatever the sampling unit is), the accuracy of estimates of volume can often be greatly improved by increasing the sampling rate with the size of the store. The improvement is attributable to both the stratification by size and the varying sampling rate.

There have been several cases in which records were sampled for the purpose of estimating the quantities and prices of selected commodities that move through various marketing channels. Examples are a wholesaler's sales record or a canner's record of shipments. The quantity of each sale is in the record and information with regard to each transaction is taken from the record, including the quantity of the sale. Instead of taking every *nth* transaction, a simple procedure can often be devised for going through such a file using heavier sampling rates on the larger transactions, which will result in a much more efficient sample for certain purposes. The gain in efficiency differs, of course, from one case to another, but in some cases, as compared with taking every *nth* transaction, a sample only one-fourth as large (or even smaller) might be as accurate if the sampling rates are properly increased as the size of the transaction increases. However, if the objectives call for estimates of the percentage of transactions that fall in various categories, to increase the sampling rate with the increase in the size of the sale would normally result in a loss of statistical efficiency, as compared with taking every *nth* transaction.

The following example will illustrate a few additional points about designing a sample whenever estimates of volume are the objectives. It is taken from a study in the North Central region which, as part of its objectives, included specifications for estimating the quantities of butter sold through various channels by butter manufacturers. For sampling purposes a list of the manufacturing plants and the pounds of butter produced by each, in 1948, were available. Using Minnesota for illustration, the number of plants and the total volume for each of four size groups are shown in table 1.

The sample was designed primarily for use in reaching the objective of making estimates of the volume of butter sold by manufacturers through various channels. For this purpose, how much heavier should the large plants be sampled than the small? If relevant data were available from previous studies, that question could be answered accurately. In the absence of such information, a good plan is to allocate the sample on the basis of volume. Some plants may be large enough to be included in the sample automatically; these are the plants which would come into the sample with certainty if one were selecting a sample of plants with probabilities proportional to their sizes.

To decide which plants to include automatically, the total volume of all plants is divided by the size of sample, which for purposes of this illustration is assumed to be 100. The quotient is about

TABLE 1.—*Allocation of sample butter plants in Minnesota by size of plant*

Size of plant (000)	All plants		Allocation of sample	
	Number	Volume (000,000) pounds	Proportional to number of plants	Proportional to volume
0 to 400	527	105	77	46
400 to 800	106	57	16	25
800 to 1,250	35	35	5	16
1,250+	13	26	2	[1] 13
Total	681	223	100	100

[1] Actually not quite 13 percent of the volume is in this group.

2.2 million pounds; that is, the average plant in a sample of 100 would correspond to about 2.2 million pounds. Hence, if a sample of 100 were allocated on the basis of volume, any plant with more than 2.2 million pounds should be in the sample automatically and one out of two plants, each with 1.1 million pounds, should be selected. In the absence of a better procedure, we could split the difference between 1.1 million pounds and 2.2 million pounds which is roughly three-fourths of the quotient—total volume divided by the sample size. A cut-off point of 1¼ million pounds was used since there were no plants whose production was in that neighborhood. The remainder of the range, 0 to 1¼ million pounds, was divided into three intervals of approximately equal size.

Two allocations of the sample are shown in table 1; one is proportional to number of plants and the other is proportional to volume. Without making a detailed technical analysis of this particular case, the best guess based upon general experience is that the sampling standard errors pertaining to volume will be about 30 to 40 percent less if the sample is in proportion to volume instead of in proportion to number of plants. For estimating the percentage of plants using various outlets (not volume sold through the outlets), statistical efficiency would usually be lost instead of gained by increasing the sampling rate with the increase in size of plant.

This discussion on sampling butter manufacturing plants has been from the viewpoint of estimating totals for all plants and not by groups of plants. Actually, the sample was not designed as described above, because of the nature of the tabulation plans and information on important factors in addition to size was available for use in stratification. One of those factors was the ability of a plant to switch between the manufacture of butter and of other milk products. Other things being equal, plants with a high degree of flexibility should be sampled at a heavier rate.

Sampling First Buyers of Various Agricultural Commodities

Frequently samples of first buyers of agricultural commodities are needed when no lists are in existence to be used as a basis for sampling. Other than taking steps to develop a list, two methods of obtaining a sample of first buyers are possible. The first is to begin with a sample of producers (growers) and get the names of persons or firms to whom they sell. This might be a practical approach when a sample of producers is being interviewed for reasons other than developing a sample of first buyers. It is clear that a large buyer has a much greater chance than a small buyer of being named by the sample producers and that unweighted averages of data in the buyer's sample are not unbiased statistically. Hence, before beginning the producer and buyer surveys, appropriate questions should be included on both schedules in order that statistically unbiased estimates can be made from the buyer's sample. The sampling theory for this method of sampling the first buyers needs thorough exploration, for the estimating procedures indicated below are results of only a preliminary examination of the problem.

As a specific illustration, consider the problem of estimation for such a sample of first buyers of eggs to obtain information on their marketing practices. Suppose that, for each producer in a random sample, the name of each buyer to whom he sold eggs in a given period and the quantities sold to each can be obtained. Then let y_1, y_2, . . . , y_n be the number of eggs sold to each where n is the number of different buyers to whom the sample producers sold eggs. That is, if three sample producers, for example, sold to buyer No. 1, y_1 would be the total number of eggs sold to buyer No. 1 by these three producers. In this discussion it is assumed that all buyers named by the producers are included in the sample of buyers. If not, it is necessary to modify accordingly the formula given below.

Let x_1, x_2, . . ., x_n be the total number of eggs bought by the n sample buyers from all producers (including producers not in the sample) as learned from the buyer's schedule. Note that shipped-in eggs or local eggs which might have been bought from some other dealer are not included. The formula given below provides statistically unbiased estimates when the x's be numbers such that, if a census of all producers were taken, y_i would equal x_i. That is, the expected value of $y_i = r\, x_i$ where r is the sampling rate applying to the sample of producers.

Next, let z represent the universe total of a variable which is to be estimated. A statistically unbiased estimate z', of z is given by the following equation.

$$(1) \qquad z' = \frac{1}{r} \sum_{i=1}^{n} \frac{y_i}{x_i} z_i$$

where z_i, for example, is the total number of eggs shell-treated by the i^{th} sample buyer. The quantity $\frac{y_i}{x_i}$ is in effect a weight, so the estimating equation can be rewritten in the form:

$$(2) \qquad z' = \frac{1}{r} \sum_{i=1}^{n} w_i z_i \text{ where } w_i = \frac{y_i}{z_i}.$$

If an estimate of an average per buyer is desired, instead of an estimate of the universe total, we simply compute a weighted average using the w's as weights. That is, $\frac{\Sigma w_i z_i}{\Sigma w_i}$

The answers to some questions might be in the form of a proportion. That is, the question might ask for a percentage, p_i, which is equal to $100 \frac{z_i}{x_i}$ where x_i, as defined before, is the number of eggs bought by the i^{th} buyer from producers, and z_i, for example, is number of these eggs which are shell treated. To estimate the percentage of all eggs bought direct from producers that are shell treated by first buyers, the percentages, p_i, can be weighted by the y's. Thus $\frac{\Sigma y_i p_i}{\Sigma y_i}$. This weighting is applicable only when p_i is a fractional part or percentage of x_i.

To estimate, for example, the percentage of first buyers who happen to operate an egg route, the procedure is decided upon by referring to equation (2) and letting $z_i=1$ for buyers who answer Yes, and equal zero for buyers who answer No. The estimated percentage turns out to be the sum of the w's for buyers who answer Yes, divided by the sum of the w's for all buyers.

A second possible way of sampling first buyers in the absence of a list is the use of area sampling which was recently considered as one of the alternatives for sampling local buyers of cotton. It was reported that within a community all local buyers of cotton can be discovered by inquiring within the community. This suggested an adaptation of area sampling, the sampling units being parts of counties, these parts being probably as large as a minor civil division or larger. Briefly, the designing of the sample might proceed as follows, assuming a uniform sampling rate: First, identify any of the cities or places which are of sufficient importance to be included in the sample automatically. The interviewer would visit these places, develop a list of buyers and apply the overall sampling rate to the list. If, for the remainder of the universe, a sample of counties is to be selected, county statistics on cotton production and perhaps other information, including the knowledge of cotton experts, would be used as a basis for stratification of the counties.

Information available for defining sampling units within the sample counties (or over the whole area included in the universe, if a single stage sample is used) includes statistics by minor civil division, county highway maps, and population figures for the cities and villages. A sample of sampling units is then selected and the buyers who are located within the selected sampling units are in the sample. If space permitted, various modifications of this procedure could be illustrated, including the possibilities of varying the sampling rate with type or size of buyer.

For the sampling of local cotton buyers it seems advisable to travel through the counties and develop a complete list by local inquiry, including information on type and rough estimates of the quantity of cotton bought by each. This would provide a basis for sampling that is similar to that illustrated for the butter manufacturing plants which is important if the tabulation plans include estimates involving quantities of cotton. From a list so developed for all Delta counties in Mississippi, it was estimated that the 2 largest local buyers out of 161 on the list handled roughly 15 percent of the total cotton bought by them all. Buyers of less than 500 bales were not included on the list as they accounted for less than 2 or 3 percent of the cotton. If in a sample of 50, for example, these 2 buyers could be either in or out of the sample by chance, the effect on the sampling error for estimates involving quantities of cotton is clear.

If area sampling were to be used, it should be supplemented with complete coverage of a list of the largest buyers, if reliable estimates of volume were to be obtained. Under the circumstances, if estimates are required for areas as small as the Delta in Mississippi, it appears that effort spent on developing a complete list of the cotton buyers patronized by producers, with estimates of the quantities purchased by each is a good investment. If estimates were wanted for the South as a whole without geographic breakdown, a sample of counties could be selected and lists could be developed within the selected counties. However, all of the relatively few largest buyers probably should be included in the sample even if they were not located in a sample county.

As surveys differ with respect to objectives, to the break-down of data required in the analysis, to the universe covered, and so forth, two samples are seldom designed exactly alike. Frequently, a research worker asks the consulting statistician if his proposed sampling plan is sound without referring to the purpose of the survey in which it is to be used. The statistician who answers the question, without closely examining the objectives, the principal tabulations desired, and the possible alternatives, might be doing himself and the research worker a disservice. A statement made at the beginning of this paper seems worth repeating. The objectives of a survey may be limited by what is feasible in the way of alternative sampling plans, but the emphasis should be on adapting a sampling design to the objectives.

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