RECENT EXPERIMENTS IN STATISTICAL SAMPLING IN THE INDIAN STATISTICAL INSTITUTE[1]

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INTRODUCTION

I was naturally gratified to receive in March 1946 an invitation to present a paper before the Royal Statistical Society during my visit to London. I had collected some material for this purpose and had written certain portions of the paper when I was suddenly obliged to leave India at the end of April to attend a session of the United Nations Statistical Commission in New York. Owing to unforeseen difficulties and pressure of work in connection with the Royal Society and Empire Scientific Conferences in London in June and July, I had no time to finish the paper. It was not even possible to circulate the tables which had to be presented before the meeting in a mimeographed form. I greatly appreciate the kindness shown to me in relaxing the rule about submitting the written paper in advance, and secondly in arranging the present meeting during the closed session. The present paper contains the observations made at the meeting as well as certain portions of my original notes. I acknowledge with thanks the help I have received from my young colleague, Prasad Banerjee, in putting this paper in its final shape.

Restricted scope of the paper. This paper has been deliberately named as "Recent Experiments in Statistical Sampling in the Indian Statistical Institute" to make it clear that I had no desire to speak on sample surveys in general, or to make any attempt to give an account of the many interesting and significant developments in statistical sampling which have taken place in recent years outside India. A large number of papers have been published on the subject in statistical and scientific journals, and Dr. F. Yates, in the paper recently presented before the Royal Statistical Society, has given a comprehensive review. It is not necessary for me to touch the ground covered by others. The aim of the present paper is to draw attention to certain experiments in statistical sampling conducted during the period 1937–45 by the Indian Statistical Institute, of which I am the Honorary Secretary.

The Indian Statistical Institute. It will be convenient if I give at this stage a brief account of the Indian Statistical Institute itself, as this would explain the set-up in which the work was done. The Institute was inaugurated on December 17th, 1931, and was formally registered on April 28th, 1932, as a non-profit-making scientific society. Work began on a small scale, and the total expenditure in the first year (1932-33) was about £40. Special courses in statistics were started in the second year, which later developed into regular post-graduate training classes. Research work was also begun, at first on a part-time

[1] This paper originally appeared in the *Journal of the Royal Statistical Society*, volume 109, part 4, 1946 and is being reprinted with the permission of the Royal Statistical Society—Editor.

basis, but later with the help of a whole-time nuclear staff. A scheme for conducting examinations and awarding certificates of proficiency for computers and statisticians had been prepared in 1935-36 which was brought into effect from 1938. In 1941 the Institute helped in establishing a post-graduate department of statistics in the Calcutta University offering whole-time courses for the degrees in Statistics of M.A. and M.Sc.; and the actual teaching work was done in the Institute itself by a practically joint Institute and University staff. *Ad hoc* enquiries on a small scale were being undertaken from 1935, but project work began to develop on a large scale from 1937 with the initiation of a scheme for improving the forecast of the area under jute in Bengal. The project side developed rapidly during the war, as the Institute had to undertake various statistical enquiries and surveys on behalf of Government Departments. In 1945-46 the total volume of employment was about 750 man-years, divided about equally between the statistical and field branches, and the total expenditure was of the order of £100,000. Excepting for a comparatively small research grant of about £4,000, practically the whole of the income was derived in the form of contract grants from Government Departments for specific enquiries and projects. It is interesting to observe that the machinery was practically the same which had developed independently in the U.S.A., where large schemes of statistical investigations relating to the war effort were being done in different Universities and scientific institutions with the help of contract grants from the Government.

Conditions of work. Several points deserve to be emphasized. Each project or enquiry had definite practical objectives which were laid down by the client (usually a Government Department); the permissible margin of error of the results was broadly indicated; the enquiry had to be conducted in accordance with a definite time schedule so as to enable interim or final reports to be submitted by particular dates; the whole work had to be done within the limits of an all-inclusive contract grant, so that any excess of expenditure would mean a financial loss to the Institute. Finally, owing to difficulties created by the war situation in North-east India, the human agency and the organizational side of the project had to be very carefully planned, and usually the field survey had to be carried out under great difficulties. The research that had to be undertaken was naturally of an applied type. Nevertheless fascinating theoretical problems were continually arising, some of which were and are being tackled on fundamental lines. In preparing the design of each sample survey the three most important considerations thus were time, cost, and the human agency—all, of course, in relation to the most important factor of the permissible margin of error which was usually stipulated in advance.

Statistical engineering. In the present paper I have very much in mind those problems of organization which arise when a sample survey has to be carried out on a very large scale. The difference in quantity is so great that it brings about practically a change in quality. The manufacture on a commercial scale of a chemical gives rise to problems of an entirely different type from those which have to be solved in small-scale working in a laboratory, and the difference is so great that we give expression to it by calling large-scale production a matter of chemical engineering rather than of pure chemistry. In the same way large-scale sample surveys may be appropriately called statistical engineering. A good deal of the present paper is in fact concerned with statistical engineering rather than the pure theory of sampling.

General observations

The sample enquiries discussed in the present paper fall into two broad types. A great deal of work has been done on sample surveys on an extensive scale covering whole provinces (50,000 to 140,000 square miles in extent in the same season), in which the object has been to obtain reliable estimates of the acreage, rate of yield per acre, and total production of important food and fibre crops like rice, wheat, jute, etc.; and of economic or demographic factors relating to indebtedness, unemployment, destitution, paddy land, plough cattle, birth rate, death rate, etc., of rural families. For such work the technique used has always been the area or grid method, in which comparatively small sampling units (of area or size ranging from about two acres to one square mile) were located at random, and information was collected for each sample-unit by direct physical inspection or investigation. Estimates for districts (about 2,500 square miles or so in area on an average) or the province as a whole were then obtained directly by multiplying observed averages by appropriate multiplying factors (equal to the ratio of the total geographical area of the region to the total area covered in the sample surveys).

In a second type of work the enquiry was more localized, and related to cost and level of living, housing, consumption of food, clothes, etc.; preferences for particular types of commodities; reactions to radio programmes; public opinion on various subjects, etc.

Terminology and classification of sample surveys

Before proceeding further it will be convenient to explain the terminology and classification of sample surveys. In the area method the smallest physical element which can be separately surveyed is called a *quad*. Typical examples are a single plant for determining the yield of crop; a single family or household (having food from the same kitchen) for ascertaining the cost of living; or a single individual in anthropometric or blood-group surveys. The whole region or field to be surveyed may be then considered to consist of a very large number of such ultimate physical elements or *quads*.

Quad and configurational sampling. Sampling may then proceed in two different ways. The ultimate physical element or quad may be adopted as the sampling unit, or a group of such elements taken together may be used as the unit for sampling; these two types are called "quad" and "configurational" (or "grid" or "cluster") sampling. The grid, in the area method, consists of a suitable number of adjoining quads or ultimate physical units. For example, in crop surveys in Bengal it was found that a suitable sampling unit was a grid of square shape and size 2.25 acre. Each grid thus consists of a compact group of adjoining quads.

It must be noted, however, that the sample unit may also consist of quads separated from one another, but in a fixed spatial configuration—for example, 4 quads at the corner of a square of a given size. It is convenient therefore to distinguish between the "grid" and the "cluster," and restrict the former to a compact group of adjoining individual units, and the latter to other types of configurational sampling where the quads are not adjoining but are arranged in some particular pattern in one or two dimensions. Every n-th individual unit in a linear series would also in this sense be an example of cluster or configurational sampling. From the present point of view all sampling methods may then be characterized as being of either (1) of *quad*, (2) of *grid*, or (3) of *cluster* type.

Zonal and non-zonal. The important point to be noted is that all the sample-units have to be located at random. But this can be done in two ways—namely, distributing the sample units over the region under survey as a whole, or by first dividing the whole region into a suitable number of sub-divisions or zones, allocating a suitable number of sample units to each zone, and then locating such sample units, grids, or clusters at random within each zone separately. This gives rise to two types—namely, (1) without zoning or non-zonal, and (2) zonal sampling.

Multi-stage. So far methods have been considered in which the sample units are located at random in one single stage over each zone or over the whole area under survey. In many enquiries this method cannot be used because of its high cost. For example, in estimating the rate of yield per acre of crops it is usually necessary to proceed by stages. A number of zones are first selected at random; within each selected zone a number of villages are next selected at random; within each village so selected a number of fields are then selected at random; and within each field so selected one or more sample cuts are finally located at random. In this method the act of randomization is performed, not in one single stage, but in successive stages. The coverage of the process of randomization takes place repeatedly over smaller and smaller regions. This type of sampling is called multi-stage.

Replicated networks of sample units. Information for each zone (or for the entire field under survey) may be collected in one single network of sample units; or the information may be collected separately for two or more networks of sample units. Each such network would give an independent estimate, and differences between such estimates supply immediately measures of the over-all or effective margin of error. A characteristic feature of the Statistical Institute's work has been the use of such independent replicated networks of samples.

It would be noticed that quad or configurational, zonal or non-zonal, and replicated sampling can be used with either the uni-stage or the multi-stage methods. Thus quad sampling may be used at one or more stages, and grid or cluster at another stage. In the same way, the sampling method may be non-zonal at one or more stages and zonal at others. There may be one single network of samples at one or more stages or more than one network at other stages. The present system of classification of sampling methods is thus flexible and comprehensive, and makes it possible to describe any particular design of sample survey in a precise and non-ambiguous manner. It is suggested that the present terminology (with such modifications that may be considered desirable) should be adopted as a standard system for the classification of sample surveys.

LARGE-SCALE SURVEYS

When the work is done on an extensive scale covering a large geographical area it is almost inevitable that the sample units must be located at a considerable distance from one another. The investigators have therefore to spend a good deal of time in travelling from one sample unit to another. The total time (or the total cost) of field operations thus consists of two broad portions—namely, the actual time or cost required for enumeration or investigation of the sample units, and the time required for the journeys between different sample units. In such surveys it would be obviously uneconomical to collect information for a single quad in each locality. It is therefore usually arranged to use a grid or a cluster—that is, a group of quads for investigation in each locality visited by the investigator.

Cost function. Large-scale sample surveys are necessarily expensive, and considerations of cost are therefore of great importance.

As already noted, in surveys covering large geographical areas the cost of field operations depends on the time required for actual enumeration or investigation and the time required for journeys from one sample unit to another. Collecting information for grids of a large size would naturally take more time. If grids of a large size are used, then the total number which can be surveyed must be necessarily small. Grids of a large size would be thus more widely scattered, so that the time required for journeys would be larger for each journey on an average, but there would be fewer journeys to perform. Working with grids of a small size it would be possible to use a large number so that the average distance apart would be less. The time for journeys on an average would be less, but a large number of journeys would have to be undertaken. The total cost of operations thus depends on the size of the grids and their total number. Working within a fixed budget this means that once the size of the grids is settled, their total number must also be determined by the budget limit.

In enquiries which are geographically more localized, but which are conducted on a sufficiently large scale, the cost of operation depends on the way the field survey is organized—that is, on the nature of the human agency employed for the work, the methods adopted for collecting the primary data, arrangements for checking and supervision, etc. In every large-scale enquiry the cost is, therefore, determined by the particular plan or design of survey proposed to be adopted.

Variance function. Cost, however, is not the only consideration. The precision of the final results is also important. The variance of grids or sample units decreases as the size (*i.e.*, area) of the grid or sample unit is increased. In crop-survey work it became clear quite early that the decrease in variance with an increase in the size of the grid was appreciably slower than the normal rate of decrease (namely, inversely as the size of the sample unit). What particular size of grid would be most economical would therefore depend on how the variance changes with the size of the grid. This necessitates a study of the variance function which gives the relation between the variance and the size of the grid. More generally, each way of distributing the sample units—that is, each particular design of sampling—involves its own appropriate sampling error, so that corresponding to each design there exists its specific variance function.

Optimum solutions. It is now possible in terms of the cost and variance functions to state the conditions for an optimum or most economical design for sampling. The aim may be stated in either of the following two alternative forms: (*a*) to determine the size and distribution or density of grids or sample units in such a way that the variance (or margin of error) of the final estimate is a minimum when the total cost of the sample survey is fixed; or, alternatively, (*b*) to determine the size and distribution of sample units so as to reduce the variance (or margin of error) of the final estimate to any desired level at a minimum cost. It has been shown elsewhere how concrete solutions (which are the same for either of the alternative forms) can be obtained with the help of an empirical knowledge of the variance and cost functions.

Two points are worth noting. The above approach is indispensable when the scale of operations is sufficiently large, and individual grids or sampling units are so widely separated that an appreciable amount of time and cost is incurred in journeys between grids or sample units, or when a large number of investigators are employed for the field survey so that the cost depends materially on the way in which the field staff is organized. Secondly, such large-scale sample surveys usually give rise to many problems involving the human agency which have engaged the special attention of the Indian Statistical Institute for a long time, and to which more detailed references have been made in later sections.

EXPLORATORY (OR SEQUENTIAL) DEVELOPMENT

The planning of a sample survey thus has several aspects—namely, (1) zoning and/or stratification; (2) size of the grid or sample unit; (3) arrangements for replication; (4) density or distribution of grids in different zones or strata; (5) preparation of forms and schedules; (6) structure and organization of the field staff; (7) arrangements for the statistical processing of the material, etc. It has been explained above how the efficiency of the design of the sample survey can be maximized from joint considerations of variance and cost functions. To use such methods it is necessary to have previous information about those two functions. Other relevant information relating to the region or universe to be surveyed is also of great value in preparing the design. In fact, the greater the amount and accuracy of such information the greater is the possibility of reducing the cost of operations.

When absolutely no information is available, and yet the survey has to be completed at one single operation, there is no other alternative but to use unrestricted random sampling without zoning, or with such zoning or stratification as may be considered convenient from a purely organizational point of view.

Fortunately, some information is usually available. Also, especially in large-scale work, the survey has often to be continued from year to year or at suitable intervals. It is therefore usually possible to adopt the exploratory method, in which a sample survey is first carried out on a very small scale with the primary object of collecting basic information required for preparing an efficient design for later surveys. Sometimes such preliminary surveys have to be carried out more than once, and the scale of operations is gradually increased until finally the whole area or universe is fully covered. For example, in the Bengal crop survey a pilot survey was first carried out in 1937 covering only 124 square miles at a total cost of about £1,100. In 1938, a second survey was organized, in the light of the experience gained in the previous season, on a larger scale covering about 400 square miles at a cost of £2,500 ; next year the total area covered was nearly 2,600 square miles at a cost of £6,000. In 1940 the area covered was nearly 20,600 square miles at a cost of £8,100; and finally, in 1941 the whole jute tract of Bengal, measuring about 60,000 square miles, was surveyed at a cost of £10,100.

In the exploratory stage the object is to gather as quickly as possible some rough idea of the variance function, cost of operations, type and nature of human agency likely to be suitable for the field work or fluctuations from one region to another (*i.e.*, the distribution over space) of the elements or statistical variates proposed to be estimated in the survey. An intensive study is often essential in the exploratory stage. The cost per square mile (or per unit) is therefore necessarily high, but as basic information begins to be gathered the design of the survey steadily improves, and the cost per unit rapidly decreases in subsequent surveys.

In the Bihar crop survey the exploratory work was done in only two districts covering about 8,000 square miles from February to April 1944. As conditions were found in many ways similar to those in Bengal, it was possible to organize a full-scale provincial crop survey in Bihar covering about 70,000 square miles in the next crop season extending from July to September 1944. This survey was, however, admittedly weak in certain respects. In the next survey in the main winter rice season from October to December 1944 the Bihar crop survey was, however, fully organized. This shows that where previous experience is available it is possible to expedite the exploratory phase.

In extensive surveys it is usually necessary to employ a large number of staff in both field and statistical branches. For example, in the Bengal crop survey the field staff consists of about 350 workers, who actually go round from village to village, with a number of inspectors and supervisors at the top. The total statistical staff in the Institute for computational and technical work consists at present of about 375 workers inclusive of all grades in 1945-46. Giving necessary training to the staff is an important part of the work, and takes a good deal of time. The exploratory approach is often convenient from this point of view.

In socio-economic or demographic enquiries also it has been found extremely helpful to conduct a small-scale preliminary enquiry, and then make final arrangements in the light of the experience gained thereby. A pilot survey enables forms and schedules, staff organization and other details being thoroughly worked out in advance, and often saves its cost many times over.

In large-scale surveys which are repeated from season to season or from year to year the exploratory development of technique, of course, continues all the time. As pointed out by A. Wald, the underlying logic is the same as in sequential tests. Advantage is taken of all available information in preparing the design of the survey. As information accumulates, the efficiency of the survey continues to improve from one survey to another.

Multiple surveys. A special form of the exploratory approach also deserves mention. Consider an actual example. In 1941 it was desired to develop a method for forecasting the yield of cinchona bark in blocks of standing plants each covering from 20 to 40 acres. In the first survey a number of physical measurements, such as girth, height, number of shoots, surface area of the plant, etc., were made on a number of standing plants picked up at random. These plants were then uprooted and the yield of bark was determined for each plant separately. Coefficients of correlation were then determined between the yield of bark and the different physical measurements. Three measurements—namely, height of the plant, girth at a height of 6 inches above ground, and the number of shoots—were selected for final use. A number of plants in the particular block under survey can be then picked up at random and these three physical characters measured for each plant, from which the average values of these three characters for the block as a whole can be determined. The plants are not, however, uprooted, and no direct measurements of bark yield are made during the second survey. (The number of plants in the block can be determined, if necessary, by a subsidiary survey on a grid basis.) Using regression equations based on the coefficients of correlation determined in the first survey, it is then possible to estimate the total yield of bark for the block as a whole.

Such methods have been used, for example, in forestry work in the U.S.A., and have been sometimes called "double sampling." Various other possibilities are of course open; recourse may be had to triple or quadruple or n-ple samples, so that this type of work may be suitably called the method of multiple sampling (which is, of course, to be carefully distinguished from multi-stage sampling). In one sense it is really an extension of the method of concomitant variations. The statistical relation between two (or more) variates is first determined by a preliminary sample survey or a series of such surveys. Estimates are then prepared for one of the variates by a second or subsequent survey, and estimates for the other correlated variates are then calculated with the help of the previously determined regression relations. A simple example occurs in crop-cutting experiments. In the Indian Statistical Institute the weight of green plants of jute or the weight of paddy immediately after harvesting are recorded on an extensive scale. In only a small fraction of cases (of the order of 10 per cent), the jute plant is steeped in water, retted and the dry fibre extracted and its weight determined directly, or the paddy is dried, husked and the weight of rice measured separately. These auxiliary measurements serve to supply the regression relation between the weight of green plants of jute or the weight of paddy immediately after harvesting and the yield of dry fibre of jute or of husked rice, respectively, which can then be used to estimate the corresponding final yields from the more extensive weights taken immediately after harvesting. Such a procedure simplifies the field work enormously without any appreciable loss of precision in the final results.

Such methods, in which the estimates made in later surveys are based on correlations determined in earlier surveys, may perhaps be called "covariate sampling."

PLANNING OF SAMPLE SURVEYS

Coming back to the planning of sample surveys, the ultimate aim from the statistical point of view is, of course, to reduce the over-all or effective margin of error to the desired extent. It has been explained that in large-scale work this involves considerations of time, cost, and the human agency. These three factors are intimately connected, and are merely different facets of the same thing—namely, the plan or design of the survey. Any change in human agency, in general, would involve changes in the time programme, the total cost, and also (a point to be remembered) in the effective margin of error. In fact we learnt quite early the overwhelming importance of the human factor in the organization of statistical sampling on a large scale.

The over-all or effective margin of error does not consist simply of sampling variations, but also of errors of ascertainment arising from the fallibility of the human agency W. Edwards Deming has emphasized this point on many occasions, and has given a comprehensive review in his paper on "Errors in Surveys" (*American Sociological Review*, Vol. IX, No. 4, August 1944, pp. 359–369). He gives a list of thirteen factors affecting the ultimate usefulness of a survey. I shall adopt a simpler classification for present purposes and omit differences arising from variability in response or changes in the universe, or the population or the field under survey. These sources of error are of particular importance in socio-economic or public opinion surveys and offer fruitful subjects for research. A good deal of work has been already done and is still going on in this field in the U.S.A. and U.K. In the Indian Statistical Institute we have also made a few studies, but on a comparatively small scale, as the great bulk of the Institute work has been concerned primarily with crop surveys, in which these problems are not of great importance.

CLASSIFICATION OF ERRORS

Very early it became clear that mistakes committed in the compilation of primary data and, to a less extent, at different stages of processing the material, were equally or even more important than errors of sampling. Continuous attention has had to be given, therefore, to this aspect of the problem. Faults in planning (including defects in the design and technique of sampling, the preparation of questionnaires and field schedules), as aslo mistakes in the final analysis or interpretation, belong to the purely scientific and technical level of the work, and have been kept out of the present discussion. The remaining sources of inaccuracy have been usually considered in the Institute under the three broad heads described below.

Sampling variations, which have to be considered by familiar methods of theoretical statistics. The large sample theory (based on the normal distribution) has been naturally used most extensively. Observed frequency distributions are, however, sometimes definitely non-normal in character, and special forms have been or are being investigated in certain cases. The logarithmic (for example, in crop yields) or square-root transformations (in certain economic and demographic studies) or truncated distributions (for example, in studying the correlation between weight of crop immediately after harvesting and weight of grain after drying) have been found useful. The subject, however, deserves and requires extensive investigations.

Recording mistakes have been usually considered under two sub-heads: mistakes which occur either through bias or personal equation of the observers or gross negligence or even deliberate dishonesty on their part; and mistakes which arise at the stage of compilation, classification, tabulation, computation, and final analysis and presentation of the material. Both types have been investigated in considerable detail in the Statistical Institute, and a great deal of attention is still being given to developing methods for controlling such recording mistakes.

Physical fluctuations. It has also been found that for every element (or quantity) sought to be estimated by the sampling method there exists a normal margin of physical uncertainty which is a characteristic feature of the element under survey. For example, it is believed that wheat increases in weight by as much as 4 or 5 per cent by the absorption of moisture during the rainy season in a dry province like Sind. Traders, in fact, move the grain by railways at such times that the gain accruing from the increase of weight practically pays for the cost of storage and freight. In this case the exact meaning of the term "total production of wheat" has to be carefully specified. But even when this has been done, a residual margin of physical fluctuations of the order of probably 1 or 2 per cent would still remain. It is clear that no useful purpose would be served by trying to reduce the sampling error to a value below the margin of such physical fluctuations.

CONTROLS AT THE STAGE OF STATISTICAL PROCESSING

Output and mistakes occurring at different stages of copying, compilation, or numerical computations have been studied in considerable detail. For this purpose, computational work has been broken down into a large number of jobs or items with standard specifications such as copying three-figure tables, adding four-figure quantities, squaring three-figure entries, preparing frequency tables with not more than ten classes, etc. For each item, standard rates of output have been worked out, as also permissible limits of rates of mistakes. For each item standard rates of payment have also been set up. A daily job account is kept of the item, time spent, and output for each computer separately, and these are punched on Hollerith cards and tabulated for the valuation of the total work done by each computer in each month.

Individual differences are also studied, which enables inefficient workers to be weeded out on an objective basis, and work to be distributed among different workers in a suitable way. (Some workers were found to have such high rates of mistakes that their net valuation was negative, showing that a premium would have to be paid to the Institute for employing them.) The advantage of the above system is two-fold. It supplies a scientific method for controlling mistakes at different stages of the processing of the primary material. It also supplies a sound basis for cost accounting, which is a characteristic feature of work in the Institute.

In the valuation of computational work it is necessary, of course, to take into consideration not merely the total output, but also the number of mistakes committed. The method of making deduction on account of mistakes is simple in principle. Each mistake involves a certain amount of expenditure in correcting it; and, in theory, the deduction made should be equated to this additional expenditure. In practice the matter is, however, complicated, but the subject is being studied, and attempts are being made to set up standard procedures.

Sub-samples in statistical analysis. In large-scale computational work it has been sometimes found convenient and useful to carry out the statistical analysis in the form of a number of sub-samples. The sample units are allotted consecutive serial numbers in the order in which these are located on maps or picked up for being included in the survey. It is possible therefore to divide the sample data into a number of independent random sets by choosing sample units with odd or even serial numbers, or with numbers ending in particular digits. The actual computational work and analysis are then done separately for each such randomized sub-set of the material. This has three distinct advantages. Firstly, it often enables dimensional results to be obtained very quickly, which is sometimes of great help to administrators. Secondly, a comparison of the results of different sub-sets often gives clues for the detection of gross mistakes in procedure or in calculations. Finally, results for different sub-sets supply a very good idea of the over-all or effective margin of error.

Controls in mechanical tabulation. It may be mentioned here that during the last two years a great deal of the Institute work has been mechanized. The Institute has at present a full equipment of Hollerith eighty-column sorter, tabulator, and multiplier units with a battery of about fifty hand-operated punching and verifying machines, and a summary reproducing punch. In the Hollerith system each card is punched, and then passed a second time through a verifying machine operated by a different worker. With about fifty punching and verifying workers, the human factor becomes quite important. In the beginning there was some difficulty in getting the verifying work done with proper care. A simple but effective statistical control has been now introduced which is probably worth mentioning. A concrete example will make the method clear. Consider the procedure for checking the accuracy of punching of say 10,000 cards. A suitable number (say 100 or 200 or 500) of punched cards are withdrawn from the set at random. A dummy set of an equal number of test cards are punched deliberately with mistakes in assigned columns; and this second set is substituted for the first set at the appropriate places. The whole set of 10,000 cards is then verified. It is clear that, if the work is done properly, then the set of (100, 200 or 500 as the case may be) test cards must be rejected at the stage of verification in the exact order of their serial number. Besides the test cards it is possible and likely that other cards also would be rejected; but the test cards must come out in any case. This supplies a good foolproof check at the stage of verification.

Efficiency at the stage of processing. Generally speaking, the basic problem may be stated in the following way. A given volume of computational work (in the most general sense, including scrutiny of primary material, sorting, tabulation, statistical analysis, checking, etc.) is required to be done. The problem is to organize the processing work in such a way that it can be completed with maximum accuracy in minimum time and at minimum cost. The subject is important and fascinating, and attempts are being made to study it as far as possible along with the regular work of the Indian Statistical Institute. Valuable information has been collected and indications for future investigations have been secured, but it has not yet been possible to tackle the problem on fundamental lines for lack of resources.

Illustrative examples

I shall now try to give some idea of the kind of information which it has been possible to collect by the method of statistical sampling in recent years. It is quite impossible, of course, to give a detailed account of the numerous enquiries undertaken by the Indian Statistical Institute during the last five or six years. I have therefore made a selection of typical examples which are likely to be of interest outside India. The use of replicated networks of samples (in which information for each network is collected independently by different sets of investigators) has been a characteristic feature of the work in the Institute. It includes as a special case the same sample being surveyed twice or more often by independent investigators. Such methods have been used very successfully in furnishing estimates of the over-all margin of error, and I have intentionally laid stress on the use of replication in choosing my examples.

Crop surveys

This is the field in which the largest volume of work has been done in the Indian Statistical Institute. An account has been given elsewhere (*Philosophical Transactions*, B 584) of the general development of work which took place from 1937 to 1941, in which year a sample survey of the area under jute was carried out throughout the province of Bengal. The survey of jute acreage was repeated in 1942. Although by that time, owing to Japan's entry into the war, the food situation in Bengal had already become difficult, I failed completely to persuade the Government to extend the sample survey to cover the paddy crop in Bengal. The Bengal famine occurred in 1943. Since that year we have had the opportunity of carrying out a sample survey of both jute and rice crops throughout the province.

Bihar Crop Survey. As already mentioned, a pilot survey had been started in only two districts, comprising about 8,000 square miles, in the *rabi* (winter wheat) season in February 1944. The scheme was rapidly expanded to cover the whole province, and a full-scale survey of *aghani* (winter rice) of 1944-45 season was carried out only a few months later.

TABLE 1. BIHAR CROP SURVEY: PROGRESSIVE REPORTS ON WINTER RICE, 1944-45
ESTIMATED AREA (IN HUNDRED THOUSAND ACRES) UNDER DIFFERENT CROPS

serial no.	field work upto	grids surveyed		sampling fraction	area in hundred thousand acres under			
		number	percentage	one in	rice	pulses	sugarcane	potatoes
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	October 17th	2,588	4.0	4175	133.35	29.55	—	—
2	November 2nd	14,361	22.3	752	135.37	30.65	3.72	—
3	„ 11th	19,813	30.8	545	140.70	31.28	3.74	—
4	„ 18th	25,934	40.4	417	141.71	30.31	3.94	—
5	„ 26th	32,312	50.3	334	140.89	29.77	3.87	—
6	December 4th	38,818	60.4	278	139.46	29.78	4.01	—
7	„ 12th	45,006	70.0	240	138.72	30.40	4.03	—
8	„ 21st	51,502	80.1	210	140.05	29.63	4.06	—
9	January 12th, 1945	64,260	100.0	168	136.50	29.03	3.97	1.12
	(total area covered = 432.23) proportion =				31.58%	6.72%	0.92%	0.26%

The above table shows the actual progressive estimates which were submitted to the Government from time to time. It will be noticed that the first estimates were based on only 2,588 grids (or about 4 per cent of the total number ultimately surveyed), which represented a sampling fraction of one in 4,175, and yet the estimates of the area sown with rice (133.35) and with pulses (29.55) were of right dimensional order and compare quite favourably with the final figures, 136.50 and 29.03 (in 100,000 acres), respectively. After receiving the first estimates of rice and pulses, the Government desired to have similar figures for the area under sugarcane, which were submitted regularly from the second progressive report. At the last stage, at the request of the Government, an estimate of 1.12 (in 100,000 acres) was supplied for the area under potatoes.

The total geographical area covered in the survey was 432.23 (in 100,000 acres), so that the proportion of land was 31.58 per cent under rice, 6.72 per cent under pulses, 0.92 per cent under sugarcane, and only about 0.26 per cent, or one quarter or 1 per cent, under potatoes. This would supply a good idea of the wide range of the survey. It, however, scarcely needs mentioning that the percentage (but not the absolute) error would be naturally quite high for potatoes or sugarcane which have comparatively small acreages.

Another point is worth explaining. Sowings of rice continue for a good length of time, possibly till the middle of November, depending on actual conditions of rainfall in different areas. In Bihar (as well as in Bengal) the rice crop is mainly rain-fed, and usually a certain proportion of marginal land is put under cultivation in the hope of a favourable distribution of rainfall. When the rainfall is not exceptionally favourable (as often happens) it is inevitable that some of the lands which in the earlier part or middle of the season are under a particular crop will go out of cultivation owing to lack of rain at the end of the season. The total area under rice therefore increases from the beginning to the middle of the season, and then usually decreases. For this reason the earlier estimates would often agree fairly well with estimates based on material collected at the end of the season.

Bengal Crop Survey. A brief description of the Bengal Crop Survey would be useful at this stage. The province of Bengal has been divided into about 1,100 "cells", each approximately 64 square miles in area. In doing this the boundaries of *mauzas* (broadly corresponding to villages, and roughly about one square mile in area) have been kept intact, so that each *mauza* (or village) lies entirely within a single cell. This has been done in order to facilitate the building up of estimates for higher administrative or revenue units such as *thana* (or police circle of 150 square miles on an average), sub-division (about 850 square miles) or districts (about 2,500 square miles), and finally the province as a whole (70,000 square miles).

Flexibility of the design. The design of the sample survey has naturally to take into consideration the intensity of cultivation or the importance of particular economic variates desired to be investigated in different regions. For example, in crop surveys it is legitimate to omit areas known to be under forests. In the case of particular crops like jute it is permissible and necessary to allot a comparatively small number of sample units to regions or cells where jute is known to be grown only in small quantities (when it is desired to obtain an accurate estimate of the total production of the crop for the province as a whole). The arrangement by which the whole province is divided into a large number of cells gives great flexibility in designing, as it permits unimportant cells to be left out of the survey altogether or to be allotted a comparatively small number of grids. The first stage in the design of the survey is thus to settle the size (area) and the number of grids to be allotted to the different regions or cells. For surveys covering a number of crops or a number of different elements it is often convenient to adopt a uniform density, or the same number of grids for each cell included within the survey. But this is a special case; in general, the number in each cell can be varied as desired.

In Bengal the total number of sample units allotted to each cell (of 64 square miles) is at present of the order of 100 for crop surveys. The position of each grid is located at random and marked on maps. In this way each *mauza* gets none or one or more random points. These random points are numbered serially as they are marked on the map, and a grid (of 2.25 acres for crop surveys) is located at each random point. All fields falling within the grid are subsequently surveyed by the field staff.

Two sub-samples. Within each cell (in which the act of randomization is carried out separately) the sample units, as they are located at random, are given consecutive serial numbers, so that the set of sample units or grids with even serial numbers and the set of sample units or grids with odd serial numbers form two independent but interpenetrating and random networks of sample units, each of which covers the same area. These two sets are allotted to different parties of field investigators, so that information for each set is collected independently. These two sets of data thus supply two independent estimates for each cell as a whole.

Duplicate grids. A further control is used in crop surveys as a routine measure at present. A certain number of grids or sample units are allotted to both networks so that information for such "duplicated" sample units is collected by both sets of field investigators. In the Bengal Crop Survey in 1946, for example, out of fifty-four grids allotted to each network of samples, fourteen are "duplicated" or common to both sets. A detailed comparison of the two sets of field records for these fourteen grids in each cell shows how far the field survey has been done with care. The field investigators know that certain grids are duplicated, but have no knowledge as to which particular grids have to be enumerated twice. The field programme is arranged in such a way that the two different parties of investigators are separated by at least two or three days' journey, so that there is no chance of the investigators copying the records from one another. This method has proved extremely useful in supplying a good control at the point of collection of the primary material.

It is worth mentioning briefly another control used in connection with duplicated (or sometimes replicated) grids or sample units which are enumerated twice (or more often) by different parties of investigators. In the crop survey the investigators are required to note the average height of the plant in each field. As the duplicated (or replicated) surveys are done at different times—sometimes with an interval of even two or three weeks—this supplies a good check. For example, cases have occurred in which the height of plants were recorded as being lower at the time of the later survey than the height recorded during an earlier survey. Such discrepancies can arise only through mistakes in identifying the fields, or through gross negligence or dishonesty on the part of the investigators. Such controls also serve the useful purpose of supplying an objective basis for the rejection of unreliable primary material.

Agreement between half-samples. Table 2 shows the two independent (replicated) estimates of crop acreages in Bengal during the three years 1943, 1944 and 1945. The design was that of two interpenetrating random networks of samples. The actual design of the survey was different in each year, but it is not possible to enter into these details. The two independent samples (*A* and *B*) are usually called half-samples as both together make up the complete sample. These might also have been called "double" samples; but this would have probably raised misapprehensions about cost in the minds of administrators.

TABLE 2. BENGAL CROP SURVEY: HALF-SAMPLE COMPARISONS, 1943-45

year	number of grids surveyed			area under crops in thousand acres			difference (*A* − *B*)		standard error of difference	Fisher's *t*
	sample (*A*)	sample (*B*)	combined	sample (*A*)	sample (*B*)	combined	actual	percentage		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					jute crop					
1943	29,676	29,676	59,352	2,759	2,757	2,758	2	0.1	100	0.02
1944	30,487	30,037	60,524	2,150	2,056	2,106	94	4.45	50	1.87
1945	53,504	52,623	106,127	2,512	2,528	2,520	— 16	0.8	*	—
					aus (monsoon) rice					
1943	29,676	29,676	59,352	6,807	6,923	6,865	—116	—1.7	446	0.26
1944	30,457	30,037	60,524	7,815	7,942	7,873	—127	—1.6	90	1.41
1945	53,504	52,623	106,127	6,966	6,805	6,864	161	2.3	*	—
					aman (winter) rice					
1943	31,216	31,215	62,431	23,840	24,044	23,942	—204	—0.8	703	0.29
1944	50,501	50,107	100,608	22,491	21,903	22,201	588	2.7	550	1.07
1945	46,148	45,971	92,119	20,970	21,202	21,087	—232	—1.1	103	2.26†

* Error calculations for 1945 were not completed at the time of writing.

† Significant at 5 per cent level.

The two crop estimates in thousand acres based on the two independent samples (A and B) are shown in cols. 5 and 6 respectively, and the pooled estimate in col. 7. The difference between the two estimates $A-B$ in thousand acres is shown in col. 8 and the same difference expressed as percentage of the combined value given in col. 7 is shown in col. 9. The standard error of the difference calculated from the samples is given in col. 10, and Fisher's t in col. 11. This table shows the kind of results which are actually obtained in practice. I should like to note, however, that the field survey in 1943 had to be organized at very short notice and under great difficulties (owing to the incidence of the Bengal famine), and supervision was probably not quite adequate; the close agreement between the two half-samples may be, therefore, to some extent spurious.

Discrepancies in crop enumeration. Since 1937, when work was started by the sampling method on a very small scale, the relative accuracy of the results obtained by a so-called complete enumeration and by the sample survey respectively have been the subject of acute controversy in India. From the very beginning attempts were therefore made to investigate the reliability or otherwise of crop enumeration done by ordinary field investigators in actual practice. The method adopted has been to carry out independently twice (or more often) complete enumerations of crops on fields in the same village or region by entirely different sets of investigators. If crop enumeration can be carried out with complete accuracy, then the two sets of records should be in perfect agreement. Certain discrepancies are, however, introduced by the fact that the two surveys have necessarily to be carried out one after the other in order that there is no chance of one party of investigators copying from the records prepared by the other party. If the first survey is carried out before sowings are completed, it is possible that certain fields which were uncultivated at the time of the first survey might have been brought under cultivation by the time the second survey is made. Or if the second survey is made rather late in the season, it is possible that some of the fields having standing crops at the time of the first survey might go out of cultivation at the time of the second survey for lack of rainfall or other causes. Such discrepancies can, however, be usually distinguished from a knowledge of crop conditions in relation to the two dates of survey.

A great deal of material of this nature has been compiled, year by year, since 1937. Even a cursory examination of the material leaves no doubt that very large discrepancies occur at the stage of crop enumeration. Some of the mistakes are genuine, in the sense that the investigators actually fail to distinguish between different crops, or make wrong estimates of the proportion of the field under different crops. Mistakes due to gross negligence, however, also occur; and sometimes, of course, entries are made by pure guesswork by unreliable investigators who want to avoid the trouble of going round to the fields. It must be remembered that conditions in which the crop enumeration has to be done in Bengal are particularly difficult. In summer the temperature would often go up to 110°F. or more; during the monsoon many of the roads are submerged; throughout the crop survey a large number of investigators suffer from malaria and other diseases every year. This makes it all the more necessary to have adequate controls at the point of collection of the material.

I am giving below a few tables to indicate the kind of material collected and nature of comparisons made in the Indian Statistical Institute on an extensive scale from year to year.

TABLE 3. ILLUSTRATIVE SUMMARY COMPARISONS OF DUPLICATE CROP RECORDS PREPARED BY DIFFERENT INVESTIGATORS (*A* AND *B*)

province and zone under survey	years of survey	name of crop	estimated acreage		discrepancies (*A* − *B*) between the two records				percentage to *A*	
					actual		sum			
			(*A*)	(*B*)	positive	negative	algebraic	absolute	algebraic	absolute
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bengal 2 *thanas*	1937	jute	355	385	103	133	—30	236	—8.5	66.5
Bengal 8 *thanas*	1938	jute	3,584	3,646	466	528	—62	994	—1.7	27.7
Bengal 6 *thanas*	1944	wheat	286	298	100	112	—12	212	—4.3	74.6
Bihar 6 *thanas*	1944	gram	730	636	244	150	94	394	12.9	53.9
Bihar 36 *thanas*	1944	*bhadoi* paddy	6,715	6,481	1,658	1,424	234	3,082	3.5	45.9

The above gives a comparison of duplicated crop records prepared independently by two different sets of investigators called *A* and *B*. Cols. 4 and 5 give the two independent figures of total acreage under the same crop in the same geographical area. A detailed comparison of the two sets of records was carried out for each field separately. For example, a particular field may be shown as being under jute by the first party of investigators (*A*), but not under jute by the second party of investigators (*B*). In such cases the discrepancies $A-B$ may be considered to be positive. When a field is shown under a particular crop by party *B*, but not by party *A*, the discrepancy $A-B$ would be then naturally considered to be negative. If such discrepancies are added up for each individual plot, one would get the total positive or negative discrepancies. These are shown in cols. 6 and 7, and the point to be emphasized is that such absolute discrepancies are actually very large, and in the above table range from 27.7 per cent to 74.6 per cent. So far as detailed crop records are concerned, it is quite clear, therefore, that the so-called complete enumeration cannot supply reliable results under conditions actually existing at present in Bengal or Bihar.

Many of the mistakes are, however, unbiased and tend to cancel out. The total algebraic errors are obtained by simply adding the positive and negative discrepancies, and are shown in col. 8. For example, in the first line of Table 3 the figure—30 in col. 8 is obtained by adding the positive and negative discrepancies in cols. 6 and 7, and is of course also simply the difference between the two figures given in cols. 4 and 5 respectively. In comparison with the absolute discrepancies shown in col. 9 it will be seen that algebraic discrepancies are proportionately much smaller, owing to the cancelling out of positive and negative mistakes. In other words, results of complete enumeration of crops as carried out at present in Bengal or Bihar have exactly the same qualitative kind of statistical reliability as results based on sample surveys, with the important difference that in the so-called complete enumeration (as usually conducted without any superposed sampling controls) there is no possibility of estimating the margin of error. This is a conclusion of the greatest practical significance.

Two illustrative examples of a detailed comparison, plot by plot, of duplicated crop records are given below. Table 4 refers to material collected in *mauza* (village) Kazikandi in P.S. Goalundo (an administrative unit in Bengal in 1943). In this village 332 fields were surveyed by investigator *A* on August 14th, 1943. He noted the name of the crop (or mixture of crops) growing on each individual field. A second complete enumeration was made by investigator *B* about a fortnight later, on September 2nd, 1943, who also noted independently the name of the crop or mixture of crops growing on each individual field. Two records were thus available for each field, and can be shown in the form of a two-way table as in Table 4.

TABLE 4. BENGAL CROP SURVEY, 1943: COMPARISON OF DUPLICATED COMPLETE ENUMERATION IN P.S. GOALUNDO, *MAUZA* KAZIKANDI

		B-survey September 2nd, 1943				
		jute	*aman* rice	jute–*aman*	no crop	total
A-survey August 14th, 1943	jute	4	15	4	3	26
	aus rice	4	12	1	4	21
	jute–*aus*	17	66	2	9	94
	jute–*aus*–*aman*	—	2	—	—	2
	aus–*aman*	1	—	—	—	1
	no crop	37	45	4	102	188
	total	63	140	11	118	332

If the crop enumerations made by the two investigators, (*A* and *B*) were in complete agreement, then all fields shown as being under jute by investigator *A* would also be shown under jute by investigator *B*, and vice versa, so that entries would occur only in the diagonal cells. A glance at Table 4 would show that the actual position was entirely different. For example, according to investigator *B* there were altogether 63 fields under jute on September 2nd. According to investigator *A*, who had surveyed the same identical field a fortnight earlier, apparently only four were growing jute, four *aus* (monsoon) rice, seventeen had a mixture of jute and *aus* rice, one field had mixture of *aus* (monsoon) and *aman* (winter) rice while no fewer than thirty-seven fields were without any crops at all. Discrepancies are equally striking for other crops. In fact out of 332 fields surveyed, the two investigators were in agreement only in regard to four plots, which both recorded to have been under jute, and also in regard to 102 fields, which were shown by both as having no crops at all. In other words, in only 106 out of 332 entries was there agreement between the two independent crop enumerations. This is clearly unsatisfactory, and shows that either one or both the investigators had failed to do their work with reasonable care.

TABLE 5. BENGAL CROP SURVEY, 1943: COMPARISON OF DUPLICATED COMPLETE ENUMERATION IN P.S. CHUADANGA, *MAUZA* MANIRAMPUR

		B-survey August 10th, 1943			
		aus	jute–*aus*	none	total
A-survey June 28th, 1943	jute	1	—	1	2
	aus	123	2	8	133
	jute–*aus*	—	3	—	3
	none	7	—	189	196
	total	131	5	198	334

TABLE 6. BIHAR CROP SURVEY, *BHADOI*, 1944 45: COMPARISON OF TWO INDEPENDENT ESTIMATES OF *P* (PROPORTION OF LAND UNDER *BHADOI* RICE IN EACH INDIVIDUAL PLOT) BY TWO PARTIES OF INVESTIGATORS

(District, Hazaribagh; Thana, Giridih; No. of villages, 86)

second survey (anna estimates of proportion of land under rice during 2nd survey between Aug. 6th and Oct. 12th) \ first survey (anna estimates of proportion of land under rice during first survey between August 3rd and September 29th, 1944)	zero	1-anna	2-anna	3-anna	4-anna	5-anna	6-anna	7-anna	8-anna	9-anna	10-anna	11-anna	12-anna	13-anna	14-anna	15-anna	16-anna	total
zero	23,988	72	116	29	116	26	51	14	108	10	33	13	68	15	31	24	4,307	29,021
1-anna	87	6	8	3	2	—	1	—	3	—	—	1	1	—	—	3	33	148
2-anna	115	6	12	3	4	—	1	2	9	2	—	—	2	1	—	2	79	238
3-anna	32	6	3	—	2	—	—	—	3	—	5	—	1	—	—	—	24	76
4-anna	129	6	9	2	16	1	7	1	9	—	4	1	2	—	2	—	102	291
5-anna	14	1	1	1	—	—	—	—	1	—	1	—	1	—	—	—	17	37
6-anna	·59	—	3	1	6	—	3	1	5	1	—	3	2	2	—	—	56	142
7-anna	23	—	—	—	3	—	—	—	1	—	1	—	—	—	—	—	19	47
8-anna	130	4	3	1	7	—	5	—	13	1	7	1	4	—	5	1	157	339
9-anna	9	—	1	—	2	—	2	—	1	—	1	—	—	—	—	—	13	29
10-anna	44	—	3	3	3	—	2	—	3	1	8	2	3	1	2	—	77	152
11-anna	18	—	—	—	2	2	1	—	—	—	—	—	—	—	—	—	13	36
12-anna	63	—	—	1	5	—	3	1	6	—	1	—	4	1	2	—	109	196
13-anna	7	1	—	—	—	—	—	1	1	—	1	—	—	—	1	—	26	38
14-anna	43	—	2	—	—	—	—	—	5	—	2	—	4	—	1	3	112	172
15-anna	21	—	—	—	3	—	1	—	2	—	1	—	3	3	4	1	121	160
16-anna	3,721	27	25	11	65	10	43	10	113	18	70	16	82	34	109	82	20,635	25,071
total	28,503	129	186	55	236	39	120	30	283	33	135	37	177	57	157	116	25,900	56,193

A more satisfactory example is shown in Table 5 for another pair of investigators working in Bengal in the same year. In this case out of 131 fields recorded as having *aus* (monsoon) rice by investigator *B* on August 10th, 1943, no fewer than 123 had been shown as having rice also by investigator *A* during the earlier survey on June 28th, 1943. Out of five fields shown as having both jute and *aus* (monsoon) rice by investigator *B*, three were shown so in the earlier survey, while two fields were shown as having only *aus* rice by investigator *A*. This is quite possible, as *aus* rice is sown earlier, and these two fields might have been sown with jute after the first survey was over. As regards uncultivated fields, the two investigators had agreed in the case of 189 out of 196 fields. There was agreement in 315 (or 94.3 per cent) out of 334 fields. This may be contrasted with the agreement in the case of only 106 (31.9 per cent) out of 332 fields in the previous table. Such differences show the wide variation in the quality of field work done by different investigators. The importance of providing statistical controls like duplicated grids can be therefore easily appreciated.

I may now give a quantitative example. In the sample survey of crops the field staff is required to estimate the proportion of land in each field which is under a particular crop. These estimates are made in terms of the Indian coin anna, sixteen of which make up a rupee. In other words, crop estimates are made in units of 6¼ per cent.

Table 6 shows a comparison of results of such estimates of the proportion of land sown with rice on the same group of 56,193 fields which were surveyed independently by two sets of investigators. The comparison between the two sets of records has been made with increasing latitude of discrepancy. For example, if the two entries (estimates of the proportion of field under rice) relating to the same field (made by the two different parties of investigators) agree within 1 anna, (or 6¼ percent), then the latitude of comparison is 1 anna, or 6¼ per cent. In the same way, if the two entries agree within 4 annas, then the latitude of comparison is 25 per cent; and if they agree within 8 annas, the latitude is 50 per cent. Finally for a 16-anna (or 100 percent) latitude of comparison two entries relating to the same field would be considered to be in agreement if both the parties record the field to be under rice irrespective of the quantitative proportion.

TABLE 7. BIHAR CROP SURVEY, 1944: *BHADOI* (MONSOON) RICE: COMPARISON OF COMPLETE ENUMERATION BY TWO SETS OF INVESTIGATORS

latitude of comparison	number of plots surveyed			total number of plots compared	percentage of plots surveyed		
	in agreement		not in agreement		in agreement		not in agreement
	no crop	with rice			no crop	with rice	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 anna (6¼%)	23,988	20,699	11,506	56,193	42.69	36.84	20.47
4 anna (25%)	23,988	21,269	10,936	"	"	37.85	19.46
8 anna (50%)	23,988	21,742	10,463	"	"	38.69	18.62
16 anna (100%)	23,988	22,657	9,548	"	"	40.32	16.99

A glance at the summary Table 7 would show that if a 6¼ per cent latitude of comparison is permitted, then records relating to 11,506 (or 20.47 per cent) of 56,193 fields were discrepant. Increasing the latitude of comparison to 25 per cent, the number of discrepant fields is only slightly reduced to 10,939, or 19.46 per cent. Allowing a much higher margin of comparison of 50 per cent, the number of discrepant fields is again only very slightly reduced to 10,463, or 18.62 percent. Finally, allowing the maximum possible latitude of comparison—that is, considering merely whether the field is recorded to have rice or not—it is seen that no fewer than 9,548 or 16.99 per cent of fields still show discrepant entries. The latitude of comparison thus makes very little differences, which shows that mistakes usually occur in the identification of the fields rather than in making quantitative estimates of "*p*" (the proportion of land under a crop).

Comparison of "duplicate" grids. It has been already mentioned that in the same survey of crops the present practice is to have a certain proportion of grids enumerated in duplicate by two independent sets of investigators. This supplies a valuable check on the quality of the field survey. The field investigators make an entry for each grid of the proportion (called "*p*") of the total land included within each grid (namely, 2.25 acres in the Bengal Crop Survey of 1945-46) which is estimated to be under *aman* (winter) rice. Such. estimates were prepared twice by independent parties of field staff so that for each grid there are two values of "*p*", one estimated by party *A* and the other by party *B*. The two sets of records can therefore be shown in the form of a two-way table as in Table 8.

TABLE 8. BENGAL CROP SURVEY, 1945–46: COMPARISON OF TWO INDEPENDENT ESTIMATES OF *P* (PROPORTION OF GRID UNDER *AMAN* RICE) BY TWO PARTIES OF INVESTIGATORS

		half-sample (*B*)												
	p (in%)	0	1–10	11–20	21–30	31–40	41–50	51–60	61–70	71–80	81–90	91–99	100	total
half-sample (*A*)	0	1,159	84	50	33	30	17	18	15	27	24	28	62	1,547
	1–10	74	150	39	22	13	9	11	11	4	14	3	13	363
	11–20	31	35	88	14	14	12	10	11	9	3	5	16	248
	21–30	26	20	22	65	18	18	16	6	5	7	6	8	217
	31–40	15	10	16	25	97	42	15	15	13	16	11	11	286
	41–50	17	11	7	14	31	85	32	14	17	12	14	9	263
	51–60	17	6	9	8	18	29	71	30	24	11	16	30	269
	61–70	22	6	7	6	13	10	27	78	37	31	22	25	284
	71–80	24	7	7	8	11	23	17	45	91	45	32	45	355
	81–90	28	7	9	11	9	9	25	15	45	129	52	57	396
	91–99	29	4	7	6	13	11	12	17	29	48	264	143	583
	100	68	13	11	14	13	23	23	31	54	64	152	927	1,393
	total	1,510	353	272	226	280	288	277	288	355	404	605	1,346	6,204

Frequency constants: mean $p(A)$ = 52.0 percent, mean $p(B)$ = 51.9 per cent.

s.d. of A = 40.9 per cent, s.d. of B = 41.2 per cent, coefficient of correlation = 0.739.

TABLE 8a. SUMMARY TABLE OF PROPORTION OF AGREEMENT WITH DIFFERENT LATITUDES OF COMPARISON

	number of grids	accumulated totals	accumulated percentage
complete agreement	3,204	3,204	51.6
1–10% margin	1,069	4,273	68.9
11–20% "	511	4,784	77.1
21–30% "	367	5,151	83.0
31–40% "	225	5,376	86.7
41–50% "	185	5,561	89.6
51–60% "	142	5,703	91.9
61–70% "	96	5,799	93.5
71–80% "	106	5,905	95.2
81–90% "	86	5,991	96.6
91–99% "	83	6,074	97.9
100% "	130	6,204	100

If the two sets of crop enumeration made by the two different parties (A and B) were in complete agreement, then the entries would occur only in the diagonal cells and the coefficient of correlation between the two sets of records would be $+1$. This of course cannot happen in practice, as different investigators would have different "personal equations" of observation and estimation. In the table shown above it would be noticed that in 3,204 (or 51.6 per cent) out of 6,204 grids, the two sets of records are in complete agreement. If agreement is defined to include a margin of variation upto 10 per cent on either side then 4,273 (or 68.9 per cent) of all grids are in agreement. In the same way about 83 per cent of the grids would be in agreement if a latitude of comparison upto 30 per cent is permitted. This is not unsatisfactory.

The agreement between two sets of records can also be expressed in the form of a coefficient of correlation of $+0.739$. Owing to the cancellation of positive and negative errors the two mean values $p(A) = 52.0$ per cent and $p(B) = 51.9$ per cent are in entirely satisfactory agreement. Duplicated readings thus show two things—namely, (*a*) detailed agreement, plot by plot or grid by grid, can never be attained in practice even when the field work is done with reasonable care; and yet, (*b*) the agreement between mean values may be quite satisfactory so that reliable sample estimates can be made with confidence.

Bias in the estimation of crop yields

It may be mentioned that in crop-cutting work the principle of random sampling was explicitly recognized for the first time in the classical experiments of J. (now Sir John) Hubback of the I.C.S. in India between 1923 and 1925. He used sampling units of a very small size, 1/3,200 acre, in the form of a triangular cut obtained by a special implement devised by him. He was the first person to point out that owing to high correlation between yields in adjoining parts of the same field there was not much gain in precision by using sample units of a large size. He further emphasized the convenience and economy of using small cuts, of which a much larger number can be collected in the same time by investigators moving from one place to another. He gave a general account of his work in a paper on "Sampling for Rice Yield in Bihar and Orissa" published by the Government of India in

1927 as Bulletin No. 166 of the Imperial Agricultural Institute, Pusa. C. D. (now Sir Chintaman) Deshmukh, of the I.C.S., used Hubback's method a little later between 1928 and 1931 in his crop-cutting work on rice conducted together with settlement operations in two districts in the Central Provinces. About the same time, P. S. Rau, I.C.S., used the same method in two other districts of the same province. I succeeded recently in recovering the original material collected by C. D. Deshmukh, but all papers relating to P. S. Rau's work appear to have been destroyed. As Hubback's paper has been out of print for a long time, it has been reprinted in *Sankhyā* (the Indian Journal of Statistics), Vol. 7, Part 3, pp. 281–94, and in the same number I have given a brief account of Deshmukh's work. In 1938-39 H. P. V. Townend (also of the I.C.S.) used cuts of a comparatively small sampling unit (27.04 sq. ft.) in the form of a square, harvested with the help of a rigid frame, in his work on rice in certain districts in Bengal. It is interesting to observe that four administrators belonging to the I.C.S. used cuts of a small size evidently because they were guided by reasons of convenience and economy.

It is worth mentioning that R. A. Fisher's crop-cutting work on wheat at Rothamsted was influenced by that of Hubback, as stated by Fisher in a memorandum on crop estimating surveys submitted by him to the Imperial Council of Agricultural Research in India on March 2nd, 1945. Subsequently, Yates and others used sample cuts of a small size.

When we first started work on the estimation of crop yields in 1939, we naturally thought of using cuts of a small size. At the same time I thought it advisable to investigate whether the size of the sample unit had any influence on the results. It was necessary at this stage to distinguish between two different possible effects. If the size of the sample unit is appreciable in comparison with the size of the field, then there will be obviously a chance of over-sampling the central area of the field. I have discussed such over-sampling elsewhere (in the paper "On Large-Scale Sample Surveys," *Phil. Trans.* Vol. 231 (B), No. 584, Appendix 6, p. 404). There is a second possibility of bias arising from bordering plants being included within the sample cut. It was this second effect which I had in mind in undertaking an experimental study of the problem.

In our very first series of crop-cutting experiments on jute in Bengal in 1939 we collected some material with sampling units of five sizes ranging between 5.5 sq. ft. and 66 sq. ft. There was some evidence of over-estimation with small cuts, but the material was meagre. In 1940 we used five different sizes of sample cut. A square of size 16 ft. by 16 ft. was located at random in each field (which also was selected at random) and the crop was harvested in the form of a number of separate sub-cuts supplying yield rates for sizes 1 ft.×1 ft., 3 ft.×3 ft., 12 ft.×4 ft., 12 ft.×12 ft., and 16 ft.×16 ft. Cuts of different sizes were intentionally obtained from the same spot so as to secure effective local control for purposes of comparison. Information was collected for many items, such as number of plants per acre, the weight of green plants immediately after harvesting, and also the final yield of dry fibre per acre (after the plants were retted, dried, and the fibre extracted). I am giving in Table 9 figures for one particular item—namely, the yield of jute expressed as the weight of green plants (immediately after harvesting) in pounds per acre collected from four different districts in Bengal in 1940.

Yield rates based on 1 ft.×1 ft. (1 sq. ft.) sample cuts were clearly over-estimates. Such extremely small sizes were therefore omitted in subsequent years. The next larger

TABLE 9. BENGAL CROP SURVEY, 1940–41: YIELD OF JUTE (GREEN PLANT) IN POUNDS PER ACRE

size of cut (sq. ft.)		Dacca (n = 80)	Mymensingh (n = 80)	Rangpur (n = 94)	Tipperah (n = 66)	combined (n = 320)
(1)		(2)	(3)	(4)	(5)	(6)
1	(1 × 1)	36,603 ± 2,288	24,893 ± 2,535	23,453 ± 1,860	24,292 ± 2,370	27,271 ± 1,160
9	(3 × 3)	20,466 ± 1,317	15,462 ± 872	15,800 ± 749	18,598 ± 1,260	17,462 ± 535
48	(12 × 4)	10,188 ± 576	20,145 ± 774	16,293 ± 650	19,997 ± 848	16,080 ± 411
144	(12 × 12)	9,784 ± 494	19,305 ± 905	18,038 ± 617	20,309 ± 856	16,763 ± 428
256	(16 × 16)	9,332 ± 593	18,836 ± 774	17,552 ± 617	20,433 ± 790	16,828 ± 395

size 3 ft. × 3 ft. (9 sq. ft.) also appeared to give over-estimates, but this size was retained for purposes of comparison. Since 1940 similar material has been collected year by year, for a number of crops like jute, wheat, *aus* (monsoon), and *aman* (winter) rice in different provinces of India. An account of the first series of experiments on jute in 1940 was given in a report submitted to the Indian Central Jute Committee, which had financed the work. The report was printed by this Committee for official use, but was not released to the public owing to war-time restrictions. Other reports on crop-cutting work were submitted by us in subsequent years, but the Government restricted these also for official use. Although a good deal of material has been accumulated, it has not yet been possible to publish a comprehensive account.[1] I am giving a few typical results in Table 10.

TABLE 10. CROP SURVEY: PERCENTAGE YIELD RATES BASED ON SAMPLE CUTS OF DIFFERENT SIZES

size of cut (sq. ft.)	Bengal jute, '40 320	Bengal jute, '41 185	U.P. wheat, '41 178	U.P. wheat, '42 346	Bengal rice, 1943–44 40	average index (unweighted)
(1)	(2)	(3)	(4)	(5)	(6)	(7)
9	103.8	116.1	121.4	118.1	113.3	114.7
18	—	—	111.6	109.4	—	110.5
25, 27	—	100.7	—	109.0	—	105.0
36	—	—	100.1	99.0	112.1	103.7
48, 49	95.5	95.3	—	—	—	95.4
54	—	—	—	96.0	—	96.0
64	—	105.9	—	—	—	—
81	—	—	—	93.6	—	—
135	—	—	99.9	—	—	—
144	99.6	96.4	—	—	101.2	—
225	—	*100.0*	97.4	—	—	—
256	*100.0*	—	—	—	—	—
324	—	—	—	*100.0*	—	—
576	—	—	*100.0*	—	*100.0*	—

In this table the results have been expressed as percentages of the yield rate based on sample cuts of the largest size used in different series of experiments. On the whole, the above table indicates that the bias decreases as the size of the cut is increased from 9 sq. ft. to probably something of the order of 40 or 50 sq. ft. and becomes negligible with cuts of larger size.

The results given in Table 10 all refer to sample cuts which were located in the field with pegs and ropes. Experiments were also made with rigid and semi-rigid frames made of wood or of wood and iron. I have no time here to discuss these results, beyond stating that there was evidence of over-estimation with small cuts in such cases also.

1 A few observations have been made in *Sankhyā*, Vol. 7, part 3, April 1946, pp 269-80.

I may mention at this stage that in the paper "On Large-Scale Sample Surveys" written at the end of 1942, I referred to the over-estimation arising from the use of sample cuts of small sizes, and explicitly stated (*Phil. Trans.*, Vol. 231 (B), No. 584, p. 409):

> "It was found that there was persistent over-estimation in working with units of very small size. In the case of field survey the obvious explanation is that the investigator has a tendency to include rather than to exclude plants or land which stand near the boundary line or perimeter of the grid. This boundary effect naturally becomes less and less important as the size of the grid is increased. In crop-cutting work on jute it was found, for example, that mean values for all the characters studied (such as number of plants per acre, weight of green plants, weight of dry fibre) were much higher for sample units of small size, so that it was not at all safe to work with cuts of a size less than say 25 sq. ft."

TABLE 11. BENGAL CROP SURVEY, 1945–46: YIELD IN POUNDS PER ACRE AND INDEX-NUMBER OF YIELD OF *AMAN* RICE (NOT IN HUSK) FOR CIRCULAR CUTS OF DIFFERENT SIZES

serial no.	name of district	no. of cuts	yield in lbs. per acre			index-number of yield with yield based on largest size of cuts as 100	
			12.57 sq. ft.	50.27 sq. ft.	100.88 sq. ft.	12.57 sq. ft.	50.27 sq. ft.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	Bakarganj	180	961.1	880.5	891.2	107.8	98.8
2	Bankura	109	750.4	686.3	723.3	103.8	94.9
3	Birbhum	87	947.1	864.0	883.8	107.2	97.8
4	Bogra	76	728.2	639.4	637.7	114.2	100.3
5	Burdwan	122	801.5	724.9	723.3	110.8	100.2
6	Chittagong	25	1,156.1	935.6	924.9	125.0	101.1
7	Dacca	113	922.4	734.0	758.7	121.6	96.7
8	Dinajpur	218	933.1	819.6	822.0	113.5	99.7
9	Faridpur	36	1,036.0	655.8	597.4	173.4	109.8
10	Hoogly	45	706.8	613.0	616.3	114.7	99.5
11	Howrah	38	604.8	420.5	401.6	150.6	104.7
12	Jalpaiguri	60	655.8	578.5	579.3	113.2	99.9
13	Jessore	86	723.3	613.8	621.3	116.4	98.8
14	Khulna	91	832.7	741.4	750.4	111.0	98.8
15	Malda	73	799.8	738.9	749.6	106.7	98.6
16	Midnapur	249	767.7	703.5	700.2	109.6	100.5
17	Murshidabad	55	870.6	785.8	777.6	112.0	101.1
18	Mymensingh	263	766.9	671.4	679.7	112.8	98.8
19	Nadia	38	680.5	598.2	613.8	110.9	97.4
20	Noakhali	58	789.9	701.1	669.0	118.1	104.8
21	Pabna	55	512.6	431.2	429.5	119.3	100.4
22	Rajshahi	83	805.6	687.1	665.7	121.0	103.2
23	Rangpur	204	794.1	715.9	715.9	110.9	100.0
24	Tipperah	92	704.4	598.2	610.6	115.4	98.0
25	24-Parganas	113	706.0	602.3	618.0	114.2	97.5
	total	2,569	812.2	704.4	706.8	—	—
	percentage	—	114.9	99.7 weighted	100	117.3 unweighted	100.1

At that time the idea I had in mind was that the unconscious pulling in of bordering plants was probably effective so long as the linear dimensions of the sample cuts were smaller than the human span.[1] In 1944 Jitendra Mohan Sen Gupta of the Statistical Laboratory suggested the use of circular-shaped cuts to be harvested with the help of an arm rotating over a pivot with a light stylus attached at the end of the rotating arm to catch the plants. This proved very convenient in practice, and various improvements were made. The latest model is now standard implement in our work. It is the usual practice at the present time to take three (or four) concentric circular cuts on the same field. This is done by drawing out the rotating (radius) arm to three (or four) fixed lengths one after another, and harvesting the appropriate cuts. In this way yield rates are obtained for three or four different sizes.

Illustrative results of certain experiments are given in Table 11. Two things are clear from the above table. Circular cuts of 12.57 sq. ft. are still over-estimating when the work is done on an extensive scale by a large field staff scattered over the whole province of Bengal, comprising about 60,000 or 70,000 square miles. Secondly, it is equally clear that there is practically no bias when the size of the cut is increased to 50.27 sq. ft. which is in good agreement with previous observations.

Along with extensive experiments carried out by the ordinary field staff, arrangements were also made to repeat the work at a few selected centres by experienced investigators working under the direct supervision of trained statisticians. Illustrative results are given below in Table 12, which shows the yield in lbs. per acre of *aman* (winter) rice at three centres in Bengal in 1945-46. The numerical values show that the systematic over-estimation with cuts of 2 ft. radius is no longer appreciable. This is confirmed by Student's paired "t" values of which are given at the bottom of the table.

TABLE 12. BENGAL CROP SURVEY, *AMAN* (WINTER) RICE, 1945–46: YIELD IN POUNDS PER ACRE FOR DIFFERENT SIZES OF CUTS

symbol	size of sample cut in sq. feet	mean yield in lbs. per acre with s.e.		
		Katwa ($n = 124$)	Gouripur ($n = 64$)	Sainthia ($n = 48$)
x_1	$\pi(2')^2 = 12.57$	1,261 ± 33	726 ± 25	1,593 ± 46
x_2	$\pi(4')^2 = 50.27$	1,283 ± 26	747 ± 20	1,518 ± 31
x_3	$\pi(5\frac{2}{3}')^2 = 100.88$	1,304 ± 26	761 ± 19	1,523 ± 28
x_4	$\pi(8')^2 = 201.06$	1,340 ± 29	764 ± 18	1,495 ± 24
		Values of Student's paired "t"		
$x_1 - x_2$	—	1.53	1.33	2.18
$x_2 - x_3$	—	2.55 *	1.31	0.43
$x_3 - x_4$	—	3.07 †	0.63	2.47 *

* Significant at 5 per cent level. † Significant at 1 per cent level.

[1] Dr. P. V. Sukhatme in a letter to *Nature* of May 11th, 1946, has published some material showing the over estimation of crop yields with cuts of a small size. He writes: "The reason for over-estimation appears to be the human tendency to include border plants inside the plot. This factor becomes serious when the perimeter of the plot is large in proportion to its area." This is exactly what I had suggested in the paper written in 1942. Dr. Sukhatme has not referred to my paper, or is not aware of the observations made by me much earlier. I have recently sent a letter to *Nature* on this subject.

We thus find that when the work is done under adequate statistical supervision, the over-estimation with cuts of a small size becomes practically negligible. In view of this finding, I believe that unconscious pulling in of bordering plants may not be the only, or even the most important factor in this matter. In fact, I am now inclined to adopt a line of explanation offered by F. Yates in his paper on bias in sampling in the *Annals of Eugenics* in 1936. Discussing the observed bias in crop yields harvested from within hoops (of area 10 sq. ft.) supposed to have been thrown at random on fields in the United Provinces, Yates suggested:

> "The bulk of the bias, however, is probably due to the tendency, conscious or unconscious, to cast the hoop on the good parts of the crop."

It is likely that there are patches of greater fertility distributed either in a random manner or in a mildly patterned fashion over each field. In locating the sample cut, the investigator may unconsciously tend to favour such more fertile patches by shifting about the "random point" to some extent. From the fact that the bias appears to become negligible beyond 40 or 50 sq. ft., it would seem, without entering into detailed investigations, that the average size of such patches would be probably fairly small and of the order of only a few square feet. Unconscious pulling in of bordering plants may also be a contributing factor.

Another point is perhaps just worth mentioning. In case there is any bias in favour of locating the sample cuts on or near more fertile patches, then one would expect over-estimation with the smaller cuts to be followed by compensating under-estimation in cuts of intermediate size. Some of the figures of Table 10 may perhaps be indicative of this, but more precise and detailed experimentation is necessary to settle this point.

A certain amount of loss of grain in the actual process of harvesting, threshing, drying, weighing, etc., is inevitable which will necessarily introduce a component of "error". The proportional error arising from this factor may be independent of or may increase or decrease with the size of the cut. In case the loss of grain is proportionally smaller with small cuts (owing to the greater care with which the different processes can be carried out), then there might be an apparent over-estimation with small cuts, which, however, would really indicate an under-estimation with large cuts.

It is pertinent to observe in the present connection the danger of under-estimation in using cuts of a comparatively large size which are demarcated by pegs and ropes on the field. It is doubtful whether ordinary investigators can make accurate measurements of distance on the field. Any sagging of the ropes is bound to reduce the real size of the cut, and hence lead to under-estimation. There are serious difficulties in deciding the allowance to be made for the boundary ridges or demarcations between adjoining fields, a point which is of particular importance in a province like Bengal, where the average size of the field is very small and less than half an acre, so that the perimeter is large in comparison with the area of the field. In fact, it is difficult to define the "whole field" in an unambiguous manner; and until this is done the concept of total production must also remain to some extent vague. This subject clearly requires much further study.

In the meantime I may perhaps mention that the over-estimation of cuts of a very small size can itself be used as an excellent control in the following way. As already mentioned, yield rates are being obtained by the Statistical Institute for three (or four) concentric circular cuts of different sizes on the same plot. If the work is done honestly by ordinary investigators, then yield rates based on sample cuts of the smallest size would show over-estimation in comparison with results based on sample cuts of larger size. Results based on the two (or three) larger cuts should, however, be in good agreement. At the same time, the variance of the yield per acre should decrease with increasing sizes of sample cuts. Besides being extremely convenient and economical, concentric circular cuts thus enable a two-fold control (one over mean rates of yield and the other over variances) to be exercised at the point of collection of the primary material.

Bengal labour enquiry: Jagaddal 1941, 1942 and 1945

As already mentioned, in socio-economic surveys special efforts have been made to study the personal equation or bias of the investigating staff. Here also the use of replicated sub-samples proved to be of great value. I shall give illustrative examples from certain enquiries carried out in 1941, 1942 and 1945 in an industrial area at Jagaddal, about 20 miles to the north of Calcutta. The Government of Bengal had asked us to survey family budgets, housing, and other economic conditions of factory workers in this area.

Design of the survey. The general approach can be best studied from the design of the enquiry of 1941. In order to ensure proper randomization, the addresses of working-class employees were obtained from the factories located in this area, on the basis of which a preliminary survey was made of the geographical distribution of working-class families at Jagaddal, which is a municipal area. The number of families residing in different groups of buildings was noted on large-scale maps. A sample check was also carried out, but no attempt was made to attain high precision in the preliminary survey, as this was not considered to be of great importance.

On the basis of the preliminary survey the area was divided into five blocks (which were of unequal size and irregular in shape and), which were demarcated on certain general considerations. For example, blocks nos. 1, 2 and 3 were located in the immediate vicinity of factories; block no. 4 was within the town, but was situated at a greater distance from the factories themselves; finally, block no. 5 covered the area outside municipal limits. For present purposes the first three blocks were most important; block no. 4 of moderate importance, while block no. 5 was included more or less as a control and for comparative purposes. The buildings from which the families were to be surveyed were picked out purely at random on maps.

The sample units within each block were divided into 5 equal sub-samples, each of which was independently random and covered the whole of the block. These five sub-samples thus constituted five independent networks of sample units within each block. Each such sub-sample was assigned to a different investigator. In this way information for each block was collected by five investigators, all of whom in consequence worked in all the blocks. The number of families surveyed in 1941 and 1942 by the different investigators in different blocks are shown in Table 13.

Possibilities of analysis of variance. The interesting point is that the design of the survey makes it possible to carry out a Fisherian analysis of variance. For example, the 1941 and 1942 designs resemble in appearance Fisherian Latin squares. These are, however, not true Latin squares, but only analogues, as the sub-samples are not geographically distinct. In fact there is no intrinsic difference between the Latin square and the

TABLE 13. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941-42: NUMBER OF FAMILIES SURVEYED BY BLOCKS AND INVESTIGATORS

blocks	investigators 1	2	3	4	5	total
(1)	(2)	(3)	(4)	(5)	(6)	(7)
			1941 survey			
1	33	33	33	31	27	157
2	36	36	31	31	34	168
3	37	36	45	34	29	181
4	20	18	20	23	18	99
5	7	9	8	6	7	37
total	133	132	137	125	115	642
			1942 survey			
1	41	37	40	39	38	195
2	41	36	46	39	35	197
3	38	42	39	41	37	197
4	19	24	19	24	21	107
5	9	9	8	8	10	44
total	148	148	152	151	141	740

randomized block, so that an analysis of variance can be carried out for any two or more or all the blocks at the same time.

The unequal numbers in the different block-investigator cells introduce a difficulty. There are three possibilities. If the first three blocks are selected the numbers are not very different, so that a simple analysis of variance on orthodox lines would give approximate results. A second possibility (for the same three blocks) is to equalize the number in each cell by rejecting an appropriate number of schedules at random from each cell. Finally, the method of fitting constants can of course be used, but would involve a great deal of computational labour.

The results of an analysis of variance (for the first three blocks) for the family budget enquiry at Jagaddal in 1942 are given for a few selected items in Table 14. The total number of household schedules was 589 with 588 degrees of freedom, which can be split up into two degrees of freedom for comparisons between the three blocks, four degrees of freedom for comparisons between five investigators, and eight degrees of freedom for the interaction making up altogether fourteen degrees of freedom for comparisons between sub-samples.

In the standard Fisherian analysis of variance it is usual to use the interaction as error to test the significance of difference between blocks, etc. In the present case a direct estimate of the error can be made from the variations "within block-investigators cells" (that is, within each portion of the sample for which the material is collected by the same investigator in the same geographical area) based on 574 degrees of freedom.

It is also possible to carry out a similar analysis after equalizing the frequencies in each cell by rejecting an appropriate number of schedules at random. This was done, and the results are given in Table 14 for convenience of comparison. The total number of schedules used for this purpose was 525, so that the total variance was based on 524 degrees of freedom. Deducting fourteen degrees of freedom for comparisons among the sub-samples, this left 510 degrees of freedom for estimating the variance within sub-samples.

The ratios of variance with unequal as well as equalized frequencies in each cell are shown in the lower half of Table 14. The results are broadly similar, but equalization of frequencies makes the comparison more stringent, as a number of ratios are significant here, but not in the analysis with unequal frequencies.

TABLE 14. BENGAL LABOUR ENQUIRY—JAGADDAL, 1942: ANALYSIS OF VARIANCE

(3 blocks × 5 investigators)

sources of variation	d.f.	age in years	expenditure in rupees per month per capita: total	expenditure in rupees per month per capita: food	expenditure in rupees per month per capita: cereals	consumption of cereals in lbs. per head per month
(1)	(2)	(3)	(4)	(5)	(6)	(7)
values of variance						
(*a*) actual (unequal) number of families per cell (n = 589)						
between blocks	2	76.1	606.5	23.4	0.08	82.5
between investigators	4	211.6	220.4	19.5	1.3	61.8
blocks × investigators	8	134.1	114.1	11.2	1.9	212.4
between sub-samples	14	147.9	214.8	15.3	1.5	151.1
within sub-samples	574	112.7	172.9	10.2	0.6	97.3
total	588	113.5	173.9	10.3	0.6	98.6
mean value	-	38.84	19.16	7.80	3.05	35.59
standard deviation	—	9.71	11.41	3.15	0.73	8.64
coefficient of variation	—	25.0	59.5	40.4	23.9	23.8
(*b*) equalized number of families per cell (n = 525)						
between blocks	2	62.13	805.52	36.5	0.07	79.6
between investigators	4	304.84	275.78	22.3	1.25	114.7
blocks × investigators	8	78.47	129.38	9.6	1.47	152.8
between sub-samples	14	140.81	267.80	16.8	1.21	132.0
within sub-samples	510	127.74	168.12	9.9	0.49	100.3
total	524	128.09	170.78	10.1	0.51	100.8
mean value	—	39.31	20.86	8.15	3.09	35.96
standard deviation	—	11.32	13.07	3.18	0.71	10.04
coefficient of variation	—	28.7	62.6	39.0	22.9	27.9
ratios of variance						
(*a*) actual (unequal) number of families per cell (n = 589)						
between blocks	2	0.68	3.51*	2.29	0.13	0.85
between investigators	4	1.88	1.27	1.91	2.17	0.63
blocks × investigators	8	1.19	0.66	1.10	3.17†	2.18
between sub-samples	14	1.31	1.24	1.50	2.50†	1.55
within sub-samples	574	—	—	—	—	—
total	588	—	—	—	—	—
(*b*) equalized number of families per cell (n = 525)						
between blocks	2	0.49	4.79†	3.69*	0.14	0.80
between investigators	4	2.39*	1.64	2.26	2.55*	1.15
blocks × investigators	8	0.61	0.77	0.98	3.00†	1.53
between sub-samples	14	1.10	1.59	1.70	2.47†	1.32
within sub-samples	510	—	—	—	—	—
total	524	—	—	—	—	—

* Significant at 5 per cent level. † Significant at 1 per cent level.

Several points are worth noting. Firstly, it will be noticed that, except in the case of monthly expenditure on cereals, the interaction (blocks × investigators) variances are not significant, showing that the Fisherian assumption of the interaction variance being about the same as the "true" within-cell variance is broadly confirmed.

The significant value itself was further analysed, block by block and investigator by investigator, by Janardan Poti and K. C. Cheriyan; and it was found that the abnormally high values were due to one single investigator in one particular block. This shows the possibilities of deeper analysis offered by replicated inter-penetrating samples.

Investigator differences were insignificant in two cases, and significant at 5 per cent level in two cases. Personal equations had not been completely eliminated but their influence was not large.

Nagpur Labour Enquiry 1942–43. I am giving a second example from a labour enquiry carried out in Nagpur in 1942–43 by M. P. Shrivastava, at that time a worker of the Institute, under the technical guidance of the Indian Statistical Institute. In this case the design was arranged in the form of a randomized block of five zones and four investigators with practically the same number of family schedules—namely, fifty in each block-investigator cell. The analysis of variance is shown in Table 15.

TABLE 15. NAGPUR FAMILY BUDGET ENQUIRY, 1943: ANALYSIS OF VARIANCE
(5 zones × 4 investigators)

sources of variation	d.f.	total income	monthly expenditure		
			total	food	cereals
(1)	(2)	(3)	(4)	(5)	(6)
		(a) values of variance			
between zones	4	4,439.58	3,707.91	708.41	206.83
between investigators	3	85.43	597.08	77.09	3.70
zones × investigators	12	382.54	397.28	177.75	49.80
between sub-samples	19	1,189.74	1,127.07	237.58	75.61
within sub-samples	977	401.57	384.71	84.73	24.99
total	996	424.67	398.87	88.33	25.95
mean values (in rupees)	—	36.09	34.96	20.09	11.41
standard deviations	—	18.60	16.12	8.53	4.96
coefficients of variation	—	51.5	46.1	42.5	43.5
		(b) ratios of variance			
between zones	4	11.06†	9.64†	8.36†	8.28†
between investigators	3	0.21	1.55	0.91	0.15
zones × investigators	12	0.95	1.03	2.10*	2.00*
between sub-samples	19	2.96†	2.93†	2.80†	3.02†
within sub-samples	977	—	—	—	—
total	996	—	—	—	—

* Significant at 5 per cent level. † Significant at 1 per cent level.

It will be noticed that in this case the sub-samples are all significantly differentiated. Block differences are also highly significant in each case.

It may be mentioned here that in the Nagpur enquiry the blocks had been deliberately demarcated on lines which could almost certainly be expected to show large differences in income and conditions of living. For example, one block consisted almost exclusively of employees of the Bengal Nagpur Railway Workshop, who certainly earned higher wages and had generally a higher standard of living. The other blocks also were differentiated in many ways.

It is, however, most satisfactory to find that investigator's differences were negligible in every case, showing that personal equations had been completely eliminated. It is only by an analysis of the present kind that one can be certain on this point.

Margin of error of cost of living index: Similar studies are also possible in regard to the margin of error of the index-number of cost of living. For example, the C.L. index-number can be calculated separately for each block-investigator cell. Adopting 1941 consumption pattern as base and 1942 and 1945 prices, the results are given in Table 16. In this table the number of 1941 family schedules on which each index-number is based is given within brackets. From the values of the index in different cells it is possible to calculate the standard error of the marginal index-numbers separately for each block or for each investigator; and finally, in the same way, the standard error of the over-all index number. These are shown in Table 16.

TABLE 16. BENGAL LABOUR ENQUIRY: JAGADDAL COST-OF-LIVING INDEX 1942 AND 1945, WITH 1941 AS BASE BY BLOCKS AND INVESTIGATORS (WITH SIZE OF SAMPLE WITHIN BRACKETS)

blocks	investigators					total	
	1	2	3	4	5	size of sample in '41	mean index with s.e.
(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			1942 cost-of-living index				
1	120 (33)	122 (33)	122 (33)	124 (31)	124 (27)	157	122 ± 0.67
2	122 (36)	120 (36)	121 (31)	124 (31)	118 (34)	168	121 ± 1.88
3	121 (37)	123 (36)	121 (45)	121 (34)	121 (29)	181	121 ± 0.36
4	119 (20)	122 (18)	120 (20)	123 (23)	121 (18)	99	121 ± 0.65
5	121 (7)	124 (9)	124 (8)	124 (6)	124 (7)	37	123 ± 0.53
n (1941)	(133)	(132)	(137)	(125)	(115)	642	—
mean index	121 ± 0.46	122 ± 0.57	121 ± 0.41	123 ± 0.41	121 ± 1.04	—	122 ± 0.33
			1945 cost-of-living index				
1	270 (33)	280 (33)	275 (33)	271 (31)	279 (27)	157	276 ± 2.0
2	263 (36)	265 (36)	284 (31)	269 (31)	267 (34)	168	269 ± 3.6
3	264 (37)	274 (36)	278 (45)	272 (34)	269 (29)	181	272 ± 2.4
4	263 (20)	274 (18)	271 (20)	297 (23)	263 (18)	99	275 ± 6.2
5	260 (7)	279 (9)	296 (8)	274 (6)	284 (7)	37	279 ± 6.2
n (1941)	(133)	(132)	(137)	(125)	(115)	642	—
mean index	265 ± 1.38	273 ± 2.56	279 ± 2.69	276 ± 4.57	271 ± 2.87	—	273 ± 1.64

It will be noticed that the 1942 mean index is 122 with a standard error of about 0.4 (which is only about 0.3 per cent of the mean value). The standard errors of marginal index-numbers are quite small and usually less than 1.

The 1945 C.L. index has a standard error of 1.6 or about 0.5 per cent of the mean index-number 273. Standard errors in 1945 were, however, much higher in comparison with standard errors in 1942. This is partly accounted for by the fact that whereas price relatives had varied very little among themselves (roughly about 10 per cent of their mean values) in 1942, price relatives had varied among themselves about 80 per cent of their mean values in 1945.

Analysis of variance has been also carried out both for five blocks × five investigators and for three blocks × five investigators (for the three blocks in which the number of schedules are nearly equal). The results are shown in Table 17.

TABLE 17. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945: ANALYSIS OF VARIANCE: COST-OF-LIVING INDEX FOR 1942 AND 1945, WITH 1941 AS BASE

source of variation	5 blocks × 5 investigators			3 blocks × 5 investigators			expected ratio	
	d.f.	variance	ratio	d.f.	variance	ratio	5%	1%
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				1942 with 1941 as base				
between blocks	4	5.25	2.40	2	2.60	0.80	4.46	8.65
between investigators	4	4.50	2.12	4	2.06	0.63	3.84	7.01
residual	16	2.12	—	8	3.27	—	—	—
total	24	3.04	—	14	2.83	—	—	—
				1945 with 1941 as base				
between blocks	4	59.75	0.81	2	37.80	1.54	4.46	8.65
between investigators	4	194.00	2.64	4	69.00	2.81	3.84	7.01
residual	16	73.44	—	8	24.55	—	—	—
total	24	91.25	—	14	39.14	—	—	—

It is seen that block and investigator differences were both insignificant for both 1942 and 1945. This is satisfactory and gives confidence in using the present index-numbers.

It is of course also possible to study the cost-of-living index for different groups of items separately in each block-investigator cell. Illustrative results are given in Table 18.

There are many other possibilities of breakdowns. For example, the cost-of-living index can be studied by groups of items for different over-all size of households. Relative figures are given in Table 19, from which it would be noticed that the change in the cost-of-living index-number did not depend appreciably on the size of the household.

Similar detailed figures for the change in the cost of living in 1942 and 1945 for different families in different expenditure levels are given in Table 20. Here also the cost of living appears to have changed more or less in the same way in all expenditure levels. It must be remembered, however, that the enquiry was restricted to working-class families in which the consumption pattern was fairly homogeneous.

TABLE 18. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945 : COST-OF-LIVING INDEX BY BLOCK × INVESTIGATOR

block no.	investi-gator no.	food 1942	food 1945	clothing 1942	clothing 1945	fuel and light 1942	fuel and light 1945	miscellaneous 1942	miscellaneous 1945	total 1942	total 1945
I	1	120	276	159	310	126	258	114	275	120	270
	2	121	279	159	304	129	285	117	293	122	280
	3	122	277	160	310	127	277	119	285	122	275
	4	125	280	156	301	130	289	120	271	124	271
	5	120	280	160	314	125	275	125	287	124	279
	total	122	280	159	308	127	276	119	283	122	275
II	1	123	273	159	306	127	280	114	253	122	263
	2	121	270	158	304	126	289	114	262	120	265
	3	122	279	155	297	125	267	117	303	121	284
	4	125	272	156	305	127	283	120	271	124	269
	5	121	270	156	304	126	281	113	273	118	267
	total	122	273	157	304	126	280	116	273	121	269
III	1	122	275	156	297	129	293	114	249	121	264
	2	127	277	161	304	124	281	114	279	123	274
	3	121	280	160	306	125	288	117	289	121	278
	4	121	281	153	298	125	283	115	266	121	272
	5	122	269	159	305	125	280	115	278	121	269
	total	123	277	158	302	126	286	115	272	121	272
IV	1	119	272	153	277	126	280	114	256	119	263
	2	124	274	158	289	126	285	115	292	122	274
	3	121	274	157	242	131	303	114	290	120	271
	4	122	306	161	313	124	273	120	311	123	297
	5	124	275	154	302	123	274	112	253	121	263
	total	122	281	157	293	126	283	115	283	121	275
V	1	120	266	159	304	128	259	118	281	121	260
	2	124	288	158	303	127	266	119	292	124	279
	3	119	273	162	312	125	266	123	340	124	296
	4	123	283	158	315	125	240	120	280	124	274
	5	125	280	163	309	131	276	114	315	124	284
	total	122	277	160	308	127	264	119	308	123	279
I–V	1	121	274	157	303	127	278	114	258	121	265
	2	123	276	159	304	126	284	115	280	122	273
	3	121	278	159	296	126	282	118	293	121	279
	4	124	283	157	305	127	282	119	277	123	276
	5	123	276	158	307	126	278	117	278	121	271
	total	122	277	158	303	126	280	117	276	122	273

N.B.—No change in "rent and tax."

TABLE 19. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945: COST-OF-LIVING INDEX BY SIZE OF FAMILY

size of family in 1941	no. of families in 1941	food 1942	food 1945	clothing 1942	clothing 1945	fuel and light 1942	fuel and light 1945	miscellaneous 1942	miscellaneous 1945	total 1942	total 1945
1	266	122	274	160	310	124	274	117	272	121	270
2	94	124	275	156	298	127	286	114	285	123	273
3	69	123	280	157	299	127	287	114	275	122	272
4	70	122	286	156	297	129	288	117	281	122	277
5	56	121	285	154	296	127	284	118	287	122	279
6	34	122	275	157	297	129	282	118	285	122	271
7	19	124	280	155	295	131	284	120	286	125	275
8	13	122	280	157	302	132	294	126	284	124	274
9	12	123	285	154	294	131	273	120	292	124	278
10	1	125	294	160	324	119	228	119	272	123	270
11	3	124	288	159	300	127	304	124	266	127	279
13	3	125	279	157	296	138	316	128	286	128	278
14	1	124	284	161	306	139	323	119	346	126	236
total	641	122	277	158	305	126	280	117	276	122	273

N.B.—No change in "rent and tax."

TABLE 20. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945: COST-OF-LIVING INDEX BY EXPENDITURE LEVELS IN RUPEES PER MONTH PER FAMILY

expenditure levels in 1941	no. of families in 1941	food 1942	food 1945	clothing 1942	clothing 1945	fuel and light 1942	fuel and light 1945	miscellaneous 1942	miscellaneous 1945	total 1942	total 1945
0– 9	22	123	276	156	308	126	285	121	301	123	282
10–19	189	122	275	158	306	126	201	114	281	121	268
20–29	199	122	275	158	306	125	278	115	273	121	270
30–39	107	123	277	157	301	126	279	117	268	122	269
40–49	56	122	291	157	301	128	285	118	279	122	281
50–59	30	122	281	161	305	129	286	123	276	124	273
60 & above	38	124	276	161	304	129	284	123	277	125	272
total	641	122	277	158	305	126	280	117	276	122	273

N.B.—No change in "rent and tax."

Comparison of consumption in 1941 and 1945. As already mentioned, family budget enquiries were repeated in the same area in 1941 and also in 1942 and again in 1945. This enables a direct comparison being made of levels of consumption in different years. Somewhat similar material also happens to be available for Calcutta middle-class families in 1939 and in 1945. As prices and consumption patterns had not changed very much in Bengal between 1939 and 1941, the above two series can also be used to compare (at least approximately) the changes in patterns of consumption in working-class and middle-class families.

Relevant data are given in Table 21. The name of the item is given in col. 1, and the unit of measurement in col. 2. The over-all *per capita* consumptions in 1945 and 1941

TABLE 21. COMPARISON OF CONSUMPTION IN DIFFERENT YEARS FOR WORKING AND MIDDLE-CLASS FAMILIES IN BENGAL

item	unit per head per year	Jagaddal working-class: consumption *per capita* per year 1945	1941	index-number, 1945 : 1941 con-sumption	prices	Calcutta middle-classes: consumption *per capita* per year 1945	1939	index-number, 1945 : 1939 con-sumption	prices
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
rice and products	lbs.	214.0	321.8	66.5	273	221.4	224.7	98.5	308
wheat and products	,,	133.3	113.6	117.4	187	85.6	74.1	115.5	258
all cereals	,,	345.6	435.3	79.4	—	306.9	298.7	102.8	—
pulses	,,	68.3	70.0	97.6	366	34.5	32.1	107.6	317
potatoes	,,	50.2	64.8	77.5	533	54.9	83.1	66.0	377
milk and products	,,	18.3	43.2	42.3	300	77.8	174.5	44.6	333
butter and ghee	,,	0.66	3.9	16.8	210	1.6	16.5	10.0	333
oil	,,	20.5	15.2	134.7	304	19.1	23.0	83.0	235
salt	,,	12.0	15.9	75.6	190	11.8	17.3	68.3	300
sugar and gur	,,	26.4	15.0	176.0	185	27.9	48.6	57.5	172
fish	,,	12.1	13.1	92.2	290	24.5	50.2	48.8	589
meat	,,	8.0	12.0	66.6	343	7.7	11.5	67.0	360
eggs	no.	—	—	—	—	11.8	42.4	28.0	562
coal	lbs.	297.1	279.0	106.5	377	—	—	—	—
fire wood	,,	302.0	405.7	74.4	330	—	—	—	—
cloth (dhuti)	yds.	6.9	9.9	69.7	283	—	—	—	—
clothing (lungi)	,,	0.6	0.5	120.0	416	—	—	—	—
number of families	—	755	641	—	—	610	1,151	—	—
,, persons	—	2,313	1,866	—	—	4,335	10,539	—	—
,, persons per family	—	3.06	2.91	—	—	7.11	9.16	—	—

for working-class families are given in cols. 3 and 4 respectively. The consumption in 1945 expressed as a percentage of the corresponding consumption in 1941 is given in col. 5; and the price ratios in col. 6. Similar data for *per capita* consumptions in 1945 and 1939 in middle-class families are given in cols. 7 and 8. The ratio of consumptions is given in col. 9, and price ratios in col. 10.

I should mention here that food began to be rationed in 1943. The ration of rice was somewhat restricted owing to shorter supply, and the price control was arranged in such a way as to make "wheat" and "wheat products" comparatively cheaper. Sugar was strictly rationed, and there was a partial rationing of oil.

The consumption of all cereals had dropped very appreciably to about 79 per cent in working-class families, although there was an increase in the consumption of wheat, owing no doubt to more favourable prices. In middle-class families the cereal consumption (which had been originally much lower in comparison with working-class families) had remained more or less steady, but there was a higher consumption of wheat. The consumption of sugar had increased a great deal (to 176 per cent) among working-class families, but had decreased appreciably in middle-class families, due almost certainly to rationing. The consumption of oil had increased somewhat in working-class families, which was probably due to partial rationing and as an offset against the practical discontinuance of the use of higher-quality fats like butter or ghee (clarified butter). In practically all other items the consumption had decreased very seriously in 1945 in both working-class and middle-class families at Jagaddal and in Calcutta.

Margin of error of index-number of earnings and consumption. As information had been collected about total earnings of different families in 1941 and 1945, it is possible to calculate the average increase in earnings. The geographical blocks had been kept the same in both years. It is possible therefore to calculate average earnings for each block separately in 1941 and 1945, and hence to calculate the ratio of earnings for each individual block. Similar material is also available for a number of other items, like monthly expenditure, consumption of cereals, pulses, vegetables, vegetable oil, meat and fish, dairy products, and clothing. Index-numbers for 1945 with 1941 as base can be therefore calculated for all these items for each of the five geographical blocks at Jagaddal. These index-numbers are shown in Table 22. Similar figures were available for index-numbers for 1942 with 1941 as base, but these have not been printed here.

Table 22 supplies some idea of variations in the index-numbers from block to block. It is possible to form an unweighted average index for the whole area, and also to calculate the unweighted standard error from the figures for the different blocks. These unweighted average index-numbers for 1942 on 1941 are shown in col. 3, and for 1945 on 1941 in col. 4 of Table 24. Information regarding the size of the samples in 1941 and in 1945 in the different blocks is given together with their harmonic means at the top of each column in Table 22. Using such harmonic means as weights, a series of weighted averages with standard errors were calculated, and are shown in col. 5 of Table 24.

Mean values of monthly earnings, expenditure, and consumption of various commodities for working-class families in Jagaddal in 1941, 1942 and 1945 are shown in Table 23. In this table figures have been given both on a "per family" and "*per capita*"

TABLE 22. BENGAL LABOUR ENQUIRY—JAGADDAL, 1945 AND 1941 : INDEX-NUMBERS FOR 1945 WITH 1941 AS BASE (= 100)

	(a) per family basis					(b) *per capita* basis				
	block 1	block 2	block 3	block 4	block 5	block 1	block 2	block 3	block 4	block 5
n (1941)	157	168	181	99	37	516	484	434	268	164
n (1945)	195	212	192	104	51	580	601	467	374	284
items harmonic mean	174	188	186	101	43	546	536	450	312	208
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
cost of living	275	269	272	275	279	275	269	272	275	279
earnings	173	195	207	228	221	191	199	204	172	176
expenditure	165	179	194	212	206	182	183	191	160	164
cereals	78	80	80	107	81	86	82	79	81	65
pulses	89	115	93	126	98	98	118	91	95	78
vegetables	90	98	103	113	92	99	100	101	85	73
vegetable oil	94	149	176	166	136	104	153	173	111	108
meat and fish	71	78	104	95	72	79	80	102	72	57
species and salt	75	74	71	88	109	83	76	70	67	87
dairy products	30	48	46	47	32	34	50	46	36	26
clothing	73	73	83	87	92	81	75	81	66	73

TABLE 23. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945 : AVERAGE EARNING AND EXPENDITURE AND QUANTITY CONSUMED PER MONTH

serial no.	item	unit of measurement per month	1941	1942	1945
(1)	(2)	(3)	(4)	(5)	(6)
		average size of family	2.91	2.79	3.06
		(a) per family basis			
1.	earning	rupees	26.93 ± 0.92	36.38 ± 1.22	55.40 ± 5.16
2.	expenditure	,,	30.11 ± 1.16	35.57 ± 1.15	57.61 ± 5.38
3.	cereals	pounds	103.41 ± 5.70	98.08 ± 5.53	87.49 ± 7.63
4.	pulses	,,	15.47 ± 0.70	12.61 ± 0.56	15.93 ± 0.82
5.	vegetables	,,	32.32 ± 2.20	31.41 ± 1.52	31.56 ± 2.55
6.	vegetable oil	,,	3.56 ± 0.19	3.48 ± 0.19	5.10 ± 0.47
7.	meat and fish	,,	6.09 ± 0.35	5.90 ± 0.33	5.00 ± 0.33
8.	species and salt	,,	5.74 ± 0.33	5.74 ± 0.29	4.96 ± 0.97
9.	dairy products	,,	13.31 ± 1.69	12.08 ± 1.46	5.10 ± 0.88
10.	clothing	yds. per year	60.49 ± 2.11	55.42 ± 2.52	49.77 ± 5.78
		(b) *per capita* basis			
1.	earning	rupees	9.01 ± 0.42	13.32 ± 0.53	16.54 ± 1.09
2.	expenditure	,,	9.93 ± 0.35	12.08 ± 0.55	17.20 ± 1.15
3.	cereals	pounds	33.37 ± 0.74	31.76 ± 0.53	26.08 ± 1.48
4.	pulses	,,	5.16 ± 0.25	4.20 ± 0.16	4.98 ± 0.64
5.	vegetables	,,	10.45 ± 0.45	10.55 ± 0.56	9.46 ± 0.62
6.	vegetable oil	,,	1.15 ± 0.04	1.13 ± 0.02	1.52 ± 0.21
7.	meat and fish	,,	1.97 ± 0.02	2.04 ± 0.10	1.52 ± 0.14
8.	species and salt	,,	1.85 ± 0.06	1.89 ± 0.06	1.40 ± 0.06
9.	dairy products	,,	4.05 ± 0.23	3.79 ± 0.33	1.50 ± 0.06
10.	clothing	yds. per year	19.92 ± 0.64	18.33 ± 0.58	14.70 ± 0.87

TABLE 24. BENGAL LABOUR ENQUIRY—JAGADDAL, 1941, 1942 AND 1945: INDEX-NUMBERS OF EARNINGS, EXPENDITURE AND CONSUMPTION

serial no.	item	1942 : 1941	1945 : 1941		
		unweighted average of ratios	unweighted average of ratios	weighted average of ratios	ratios of average
(1)	(2)	(3)	(4)	(5)	(6)
	cost-of-living index	122 ± 0.33	273 ± 1.64	272.8 ± 1.3	—
	(a) index-number on "per family" basis				
1.	earnings	137 ± 4.3	205 ± 9.8	199.1 ± 8.3	205.8 ± 20.50
2.	expenditure	121 ± 5.1	191 ± 8.6	186.0 ± 7.3	191.3 ± 19.35
3.	cereals	98 ± 6.1	85 ± 5.5	83.5 ± 4.4	84.6 ± 8.73
4.	pulses	86 ± 5.4	104 ± 7.0	103.1 ± 6.2	103.0 ± 7.09
5.	vegetables	105 ± 6.9	99 ± 4.1	99.2 ± 3.4	97.6 ± 10.33
6.	vegetable oil	101 ± 5.5	144 ± 4.3	144.1 ± 14.0	143.3 ± 15.28
7.	meat and fish	106 ± 7.0	84 ± 6.6	85.3 ± 6.1	82.1 ± 7.20
8.	species and salt	105 ± 7.0	83 ± 7.0	77.7 ± 4.3	86.4 ± 17.61
9.	dairy products	100 ± 9.3	41 ± 4.0	41.8 ± 3.5	38.3 ± 8.37
10.	clothing	92 ± 5.8	82 ± 3.8	78.9 ± 2.8	82.3 ± 9.98
	(b) index-number on "*per capita*" basis				
1.	earnings	141 ± 7.2	188 ± 6.3	191.5 ± 5.1	183.6 ± 14.83
2.	expenditure	122 ± 5.7	176 ± 6.0	179.1 ± 4.8	173.2 ± 13.09
3.	cereals	97 ± 3.0	79 ± 3.6	80.5 ± 3.4	78.2 ± 4.75
4.	pulses	85 ± 4.3	96 ± 6.5	99.2 ± 5.6	96.5 ± 13.22
5.	vegetables	106 ± 7.5	92 ± 5.5	94.9 ± 4.1	90.5 ± 7.15
6.	vegetable oil	98 ± 3.6	130 ± 14.0	133.4 ± 5.8	132.2 ± 18.52
7.	meat and fish	109 ± 8.1	78 ± 7.3	80.5 ± 6.5	77.2 ± 7.31
8.	species and salt	105 ± 5.0	77 ± 3.8	76.3 ± 3.0	75.7 ± 4.14
9.	dairy products	100 ± 9.0	38 ± 4.3	40.3 ± 4.2	37.0 ± 2.58
10.	clothing	93 ± 3.4	75 ± 2.8	76.4 ± 2.2	73.8 ± 4.95

basis, and the standard errors have been calculated from the block figures in 1945 and 1942. From this material, index-numbers can be prepared for 1945 with 1941 as base by dividing the mean values in 1945 by the corresponding mean values in 1941. One measure of the standard error of the index-number can also be obtained by compounding the standard error of the two mean values. These index-numbers are shown in col. 6 of Table 24, in which the C.L. index-number has been also given at the top for convenience of reference.

Index-numbers for 1945, calculated in three different ways, are given in cols. 4, 5 and 6 of Table 24. It will be noticed that it does not matter very much which particular method of calculation is followed, as the three index-numbers are in broad agreement.

It is of course possible to make a deeper analysis by taking into consideration the sampling errors within each block, which however, would entail a great deal of computational labour. My purpose here is to draw attention to the possibility of making rough but quick comparisons by using the mean values for replicated sub-samples.

I have no desire to discuss the economic implications, but may just note that, with a cost-of-living index of 273 and an index-number of earnings of 205, the index-number of real wages was 75 in 1945. The index-numbers of consumption shown in col. 5 of Table 21 or cols. 5 or 6 of Table 24 are in broad agreement with this value. Consumption of essential articles had naturally remained more steady, while consumption of other commodities had decreased more appreciably. I am not competent to discuss the economic aspects of the subject. My object is to point out that even approximate values of the margin of error are likely to be useful in obtaining a more critical appreciation of the economic situation.

I may mention here that in working-class family enquiries at Jagaddal arrangements had been made to keep a certain number of families common in the enquiries in different years. This enables a direct comparison of the change in the consumption pattern being made on a family or household basis. In fact the form in which the material has been collected makes it possible to undertake a critical study of such enquiries. Owing to lack of resources, we have not been able to take up this work seriously, but Ambika Ghosh and H. K. Chaturvedi are doing what they can.

TABLE 25. BENGAL SAMPLE SURVEY (1936–42): ESTIMATED CONSUMPTION OF FOOD ITEMS IN POUNDS PER HEAD PER YEAR BY EXPENDITURE LEVELS

food items	levels of family expenditure in rupees per month								all levels
	0–10	10–20	20–30	30–50	50–100	100–200	200–300	300+	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
rice	310.22	371.93	370.28	360.41	342.31	278.95	257.55	262.49	341.48
rice products	4.11	5.76	10.70	9.87	10.70	10.70	9.87	5.76	8.23
wheat products	1.64	7.41	13.16	17.28	26.23	44.43	59.24	75.70	19.75
all cereals	315.97	385.10	394.14	387.56	379.34	334.08	326.66	343.95	369.46
pulses	15.63	27.98	27.03	36.20	32.91	31.27	34.56	36.20	32.91
salt	9.87	13.27	13.99	14.07	15.63	15.63	17.28	18.10	15.30
potatoes	12.34	26.33	32.09	37.03	48.55	63.36	79.81	108.61	48.55
vegetables	22.22	18.45	39.50	44.43	65.00	94.21	109.44	139.88	64.26
oil	5.76	9.87	11.52	12.34	15.63	19.75	21.39	25.51	14.81
sugar and gur	3.29	9.05	12.34	14.42	23.86	54.31	36.20	41.96	24.31
fish	6.58	8.23	11.52	14.82	23.86	41.96	49.37	57.60	25.51
meat	1.64	4.11	5.76	5.76	8.23	9.05	11.52	17.28	9.05
eggs	0.13	0.16	0.25	0.70	2.01	3.28	4.84	6.04	2.74
milk and milk products	5.76	10.70	23.86	49.37	83.93	145.77	167.86	236.98	84.75
butter and ghee	0.53	0.82	1.64	2.47	5.76	10.70	13.16	23.86	6.58
no. of persons per family	3.67	4.52	4.66	5.68	7.19	9.12	11.52	15.36	5.29
total no. of families	3,212	4,142	2,885	2,510	1,519	649	268	224	15,409
total no. of persons	11,788	18,712	13,443	14,247	10,919	5,916	3,088	3,441	81,554

Per capita *consumption of food items by expenditure levels.* I may give another illustration of the use of the sample survey in investigating consumption patterns. A number of enquiries were undertaken in both rural and urban areas in Bengal between 1936 and 1942. Although economic conditions and prices had changed during this period, the actual magnitude of such changes was small in comparison with that which occurred under war conditions. It has therefore been considered permissible to prepare a composite table showing the *per capita* consumption of various food items at different expenditure levels (in rupees per month per family) based on the above enquiries. Relevant data are given in Table 25. The same table has been expressed in the form of index-numbers in Table 26 in which the over-all average consumption has been adopted as 100.

TABLE 26. ESTIMATED INDEX-NUMBERS OF PER CAPITA CONSUMPTION OF VARIOUS FOOD ITEMS BY EXPENDITURE LEVEL

food items	units of measurement	monthly expenditure in rupees							
		0–10	10–20	20–30	30–50	50–100	100–200	200–300	300 and above
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. rice	maunds	90.8	108.9	108.4	105.5	100.2	81.7	75.4	76.9
2. rice products	„	50.0	70.0	130.0	120.0	130.0	130.0	120.0	70.0
3. atta flour, etc.	„	8.3	37.5	66.7	87.5	133.3	225.0	300.0	383.3
4. all cereals	„	85.5	104.2	106.7	104.9	102.7	90.4	88.4	93.1
5. pulses	seers	47.5	85.0	112.5	110.0	100.0	95.0	105.0	110.0
6. salt	„	64.5	86.7	91.4	91.9	102.1	102.1	112.9	118.3
7. potatoes	„	25.4	54.2	66.1	76.3	100.0	130.5	164.4	223.7
8. vegetables	„	34.6	28.7	61.5	69.1	101.1	146.0	170.3	217.7
9. oil	„	38.9	66.7	77.8	83.3	105.6	133.3	144.4	172.2
19. sugar and gur	„	13.5	37.2	50.8	59.3	98.1	223.3	148.9	172.6
11. fish	„	25.8	32.3	45.2	58.1	93.6	164.5	193.6	225.8
12. meat	„	18.2	45.4	63.6	63.6	90.9	100.0	127.3	190.9
13. eggs	number	4.9	5.7	13.1	25.5	73.2	119.8	177.0	220.6
14. milk and milk products	seers	6.8	12.6	28.2	58.2	99.0	172.0	198.1	279.6
15. butter and ghee	„	8.1	12.5	25.0	37.5	87.5	162.5	200.0	362.5
index no. of persons per family	—	69.4	85.4	88.1	107.4	135.9	172.4	217.8	290.4

The consumption of all cereals is naturally fairly steady, but shows a slight falling off above the expenditure level of Rs. 100 per month. Rice products and wheat are increasingly preferred with rising expenditure level in Bengal. Index-numbers for pulses and salt are also, on the whole fairly steady. The consumption of all other items increased steadily with rising expenditure level. The disparity in consumption is very high in sugar, fish and meat, and still higher in the consumption of eggs, milk and milk products, and butter and ghee (clarified butter). Another point is worth noting—namely, the steady increase in the size of the household with rising economic status.

Radio programme preference survey

I shall now give an example of what is usually called "listener research" in the United Kingdom. This work was undertaken at the request of the Government of India. The object was to ascertain public reactions to the broadcast of war programmes from the All-India Radio in the earlier part of 1941. It was apprehended, however, that any direct enquiry would meet with considerable psychological resistance. In this situation the Indian Statistical Institute undertook to organize a comprehensive public preference survey with a broad coverage, including reactions to war broadcasts.

As regards the design of the survey, it may be briefly mentioned that the sample was picked out of households possessing radio licences, of which a list was supplied by the Government. The households were selected on the basis of pure space randomization, and arranged in the form of several interpenetrating replicates. Information for each replicate or sub-sample was collected by an independent set of investigators. The field work was carried out in April-May 1941, and a comprehensive report was submitted to the Government in July 1941, but it was not published owing to wartime restrictions.

Four tables are reproduced below to give a general idea of the approach. Table 27 shows the frequency (in the form of percentages) of listening "often" to different items of the radio programme broken down by age, educational, and occupational groups. The size of the sample in each group is shown at the head of each column.

The nature of the fluctuations in different age-groups is of considerable interest, and is on the whole in keeping with what may be called commonsense expectations. For example, "war news" had practically the highest preference in almost every group, which is just what might have been expected. "News talk" and "foreign news" were also, on the whole, generally popular, as these were also mostly concerned with the war.

In the music and entertainment section it was found that "modern Bengali" and "(Rabindranath) Tagore songs," as well as "plays" and "instrumental music," were, on the whole, highly popular, with "devotional music" holding quite a good place; "classical music" was, however, distinctly less popular. There were also interesting age-variations in this group. Modern Bengali and Tagore songs, plays, and instrumental music were far more popular among the younger people, and showed a definite and considerable decrease in popularity with increasing age. Devotional music, on the other hand, showed a sharp contrast and increasing popularity with age.

There are many other interesting points which, however, need not detain us. It is the sampling errors to which I should like to draw attention. The values for three independent but interpenetrating samples or replicates are shown in Table 28. The variance (calculated from three replicates—that is, on two degrees of freedom) for the different items is shown in col. 6. The "within binomial variance" for the whole material is given in col. 7; and the ratio of variances in col. 8. The population is not Gauss-Laplacian, and strictly speaking Fisher's F cannot be used to test the significance. The use of F may, however, give approximate results, and significance has been shown in the usual manner.

The important point to note is that, except in three cases, errors calculated from the replicated samples are greater than the theoretical binomial errors. This shows the need for caution.

I am giving below a table to indicate the practical use of such investigations. In this table the index-numbers of frequency of listening are shown separately for men and women in cols. 2.1 and 2.2 respectively. Index-numbers of demand (as expressed by the desire of persons interviewed to have more time given to particular items) are shown in cols. 3.1 and 3.2. Pooled index-numbers of preference were also calculated by taking the average of the two previous index-numbers, and are shown in cols. 4.1 and 4.2. Index-numbers of broadcast time actually allotted to various items at the time of the survey are shown in col. 5. Dividing the pooled index of preference in cols. 4.1 and 4.2 by the index of broadcast time we get index-numbers of disparity given in cols. 6.1 and 6.2 for men and women separately.

These index-numbers of disparity indicate to what items more time should be given. The disparity index cannot, of course, be treated as a quantitative measure of the time likely to be required for satisfaction. For example, the relative demand for talks is very high,

TABLE 27. CALCUTTA RADIO PROGRAMME PREFERENCES—APRIL-MAY, 1941 : FREQUENCY OF LISTENING "OFTEN"

item of radio programme	all groups	by age-groups (in years)					by educational groups			by occupational groups					
		14–18	19–25	26–35	36–55	55–	non-matrics	under-graduates	graduates	student	petty trade	service	business	profession	landlords
	$n = 803$	$n = 24$	$n = 170$	$n = 278$	$n = 298$	$n = 33$	$n = 156$	$n = 354$	$n = 293$	$n = 149$	$n = 11$	$n = 300$	$n = 184$	$n = 95$	$n = 64$
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
WAR															
1. war news	54.7	54.2	52.9	60.1	49.9	60.6	50.3	58.5	52.2	54.0	54.5	54.0	54.9	54.7	57.8
2. news talk	50.8	45.8	52.4	52.2	47.9	57.6	46.5	54.2	48.8	46.0	54.5	49.0	52.2	58.9	54.7
3. foreign news	43.5	41.7	44.1	46.4	39.3	54.5	35.1	46.6	45.1	46.7	27.3	41.3	41.8	49.5	43.8
MUSIC															
4. modern Bengali	46.0	70.8	59.9	47.1	37.9	18.2	45.9	49.4	42.0	58.7	54.5	41.0	44.6	40.0	51.6
5. Tagore	40.2	66.6	49.4	39.6	35.6	21.2	30.6	44.4	40.3	49.3	27.3	38.3	37.0	42.0	35.9
6. plays	42.5	54.2	51.2	37.4	41.9	36.4	51.6	51.1	33.1	48.7	45.5	42.0	45.7	38.9	53.1
7. instrumental	42.0	50.0	51.2	43.5	36.2	27.3	41.4	46.6	36.5	46.7	54.5	39.7	40.2	41.1	45.3
8. devotional	35.1	25.0	30.0	32.4	39.3	51.5	43.3	36.4	29.0	30.0	27.3	35.3	35.3	36.8	43.8
9. classical	23.3	15.7	29.9	24.8	19.8	12.1	27.4	23.2	21.2	26.6	27.3	22.0	23.4	18.9	26.6
TALKS															
10. humorous	29.8	29.2	28.8	32.4	28.2	27.3	29.3	31.6	27.6	26.7	36.4	30.7	28.3	26.3	40.6
11. scientific	26.2	37.5	27.1	28.1	22.5	30.3	20.4	24.9	30.4	28.0	9.1	27.9	21.7	27.4	26.6
12. literary	18.7	20.8	21.2	15.8	18.5	30.3	8.9	25.7	21.5	24.7	9.1	22.7	14.7	21.1	23.4
13. educational	24.9	33.3	26.5	25.2	22.5	30.3	17.8	30.2	24.6	37.3	27.3	26.3	16.8	23.2	25.0
14. music lessons	20.7	33.3	37.6	15.1	15.8	15.2	21.7	24.9	15.0	34.7	36.4	18.3	17.4	11.6	18.8
MISCELLANEOUS															
15. children's	16.1	37.5	21.8	13.3	13.4	18.2	16.6	18.9	12.3	26.0	9.1	11.7	15.8	13.7	18.8
16. women's	14.7	20.8	17.6	12.2	13.8	24.2	19.1	16.7	9.9	22.7	9.1	10.0	14.1	12.6	23.4
17. folk music	16.9	25.0	17.0	16.9	16.1	18.2	15.3	18.9	15.4	14.7	0	17.0	20.6	14.7	17.2
18. rural	12.0	12.5	18.2	11.2	8.1	21.2	10.8	12.4	11.9	16.0	9.1	11.3	12.5	7.4	10.9

but it is possible that a comparatively small increase would enable the satisfaction point to be reached. This merely means that the index of disparity has to be re-estimated as conditions change.

Finally, in Table 30 I give the reactions to war broadcasts. It is quite clear that early in 1941 enemy broadcasts were found more interesting as well as more convincing as compared to All-India Radio propaganda. The margin of error of the results (calculated from replicated samples) was reasonably small. In any case, the possibility of estimating the margin of error (by using independent networks of samples) is clearly a great gain.

TABLE 28. CALCUTTA RADIO PROGRAMME PREFERENCES—APRIL–MAY, 1941: PERCENTAGES OF PERSONS "LISTENING OFTEN" TO DIFFERENT ITEMS OF PROGRAMME

Calcutta radio programme item early 1941	sample (replicates) 1 $n = 287$	2 $n = 268$	3 $n = 248$	pooled values with s.e. based on replicates $n = 803$	variance between samples d.f. = 2	variance within samples d.f. = 800	ratios of variance
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WAR							
war news	55.8	50.7	57.7	54.7 ± 2.52	3,340	2,479	1.35
news talk	54.0	42.9	55.6	50.8 ± 3.99	12,728	2,477	5.14†
foreign news	45.3	40.3	44.8	43.5 ± 1.59	2,032	2,461	0.83
MUSIC							
modern Bengali	48.8	39.6	49.6	46.0 ± 3.38	8,284	2,472	3.35*
Tagore	39.4	35.1	46.8	40.4 ± 3.42	8,978	2,391	3.75*
plays	43.6	41.4	42.3	42.4 ± 0.67	319	2,452	0.13
instrumental	42.2	40.7	43.1	42.0 ± 0.70	402	2,444	0.16
devotional	31.0	28.8	46.8	35.5 ± 5.67	24,734	2,225	11.12†
classical	26.1	20.1	23.4	23.2 ± 1.74	2,506	1,794	1.40
TALKS							
humorous	28.9	28.0	32.7	29.9 ± 1.44	1,567	2,094	0.75
scientific	27.9	21.3	29.4	26.2 ± 2.49	4,958	1,926	2.57
literary	24.4	16.0	14.9	18.4 ± 3.00	7,364	1,506	4.89†
educational	23.7	20.9	30.6	25.1 ± 2.88	6,450	1,861	3.47*
music lessons	18.1	19.8	24.6	20.8 ± 1.95	2,953	1,639	1.80
MISCELLANEOUS							
children's programme	14.3	14.9	19.4	16.2 ± 1.61	1,970	1,349	1.46
women's	12.9	9.3	22.6	14.9 ± 3.97	12,036	1,228	9.80†
folk music	14.3	16.0	21.0	17.1 ± 2.01	3,130	1,404	2.23
rural programme	13.2	10.1	12.5	11.9 ± 0.94	748	1,055	0.71

* Significant at 5 per cent level. † Significant at 1 per cent level.

TABLE 29. RADIO PROGRAMME PREFERENCE—CALCUTTA, APRIL–MAY, 1941: INDEX-NUMBERS OF PREFERENCE, BROADCAST TIME AND DISPARITY BY SEXES

name of programme item	index-numbers of								
	listening		demand		preference (pooled)		broad-cast time	disparity	
	men	women	men	women	men	women		men	women
(1)	(2.1)	(2.2)	(3.1)	(3.2)	(4.1)	(4.2)	(5)	(6.1)	(6.2)
WAR	299	186	148	47	224	116	154	145	75
news talk	155	92	116	27	136	60	24	567	250
MUSIC									
modern Bengali	140	148	207	210	174	179	262	66	68
Tagore	123	128	184	236	154	182	47	328	387
plays	130	188	172	236	151	212	125	121	170
instrumental	128	114	175	176	151	144	134	113	107
devotional	107	114	142	155	124	134	219	57	61
classical	71	87	47	54	59	90	208	28	43
TALKS									
humorous	91	94	104	108	98	101	8	1,225	1,262
scientific	80	27	102	34	91	30	4	2,275	750
literary	57	47	74	27	66	37	3	2,200	1,233
educational	76	67	90	68	83	68	20	415	340
music lesson	63	101	28	94	46	98	65	71	151
MISCELLANEOUS									
children's	49	74	45	33	47	54	174	27	31
women's	45	136	38	203	42	170	73	58	233
folk music	57	40	20	7	38	24	49	78	49
rural	37	54	9	13	23	20	131	18	15

TABLE 30. WAR BROADCAST REACTIONS: CALCUTTA SAMPLE, APRIL–MAY, 1941

source of broadcast	percentage of persons finding the broadcast								
	interesting			convincing			effective propaganda		
	n	mean	s.e.	*n*	mean	s.e.	*n*	mean	s.e.
(1)	(2)	(3)		(4)	(5)		(6)	(7)	
enemy	139	36.1	2.02	133	34.5	2.18	156	40.5	2.60
allied (mostly AIR)	56	14.5	1.11	41	10.6	1.10	57	14.8	1.30
neutral (incl. USSR)	34	8.8	0.74	50	13.0	1.15	19	4.9	0.62
no opinion	156	40.5	1.74	161	41.8	2.77	153	39.7	2.72
total	385	99.9%		385	99.9%		385	99.9%	

N.B.—Standard errors based on eighteen sub-samples.

CALCUTTA PUBLIC PREFERENCE SURVEY, 1941

I shall give another example of a different type, something like what is called public preference or "Gallup" polls in the west. The survey was carried out in certain sections of Calcutta middle-class families along with the broadcast reaction survey. The schedule covered a large number of items including preferences for classical and modern literature in both English and Bengali, different kinds of games, tobacco, *pan* (betel leaves) etc., and a number of social and political questions. A few tables are given here for purposes of illustration.

TABLE 31. CALCUTTA PUBLIC PREFERENCE SURVEY, 1941: POSSESSION OF HOROSCOPES

groups	number of persons: total	having horoscopes: observed	having horoscopes: expected	difference between observed and expected	χ^2
(1)	(2)	(3)	(4)	(5)	(6)
total sample	1,470	919	$p = 0.625$		
		(a) by age group			
below 18 years	38	23	23.8	− 0.8	0.07
19–25 ,,	259	145	161.9	−16.9	4.70
26–35 ,,	486	297	303.8	− 6.8	0.41
36–55 ,,	601	405	375.6	29.4	6.14
above 55 ,,	86	49	53.8	− 4.8	1.14
	$P(\chi^2) = 0.015$, d.f. $= 4$, $\chi^2 = 12.46$				
		(b) by educational groups			
non-matrics	380	195	273.5	−42.5	20.28
undergraduates	656	437	410.0	27.0	4.74
graduates	434	287	271.3	15.7	2.42
	$P(\chi^2) < 0.001$, d.f. $= 2$, $\chi^2 = 27.44$				
		(c) by occupational groups			
students	189	107	118.1	−11.1	2.78
petty trade	101	43	63.1	−20.1	17.07
service	522	329	326.3	2.7	0.06
business	390	245	243.8	1.2	0.16
learned profession	177	125	110.6	14.4	5.00
landlords	91	70	56.9	13.1	8.04
	$P(\chi^2) < 0.001$, d.f. $= 5$, $\chi^2 = 33.11$				
		(d) by expenditure levels (per month per family)			
below Rs. 40	105	45	65.6	−20.6	17.25
Rs. 41–Rs. 100	370	210	231.2	−21.2	5.18
Rs. 101–Rs. 200	386	249	241.2	7.8	0.67
Rs. 201–Rs. 400	316	216	197.5	18.5	4.62
above Rs. 400	241	173	150.6	22.4	8.88
	$P(\chi^2) < 0.001$, d.f. $= 4$, $\chi^2 = 36.60$				
		(e) by communities			
Hindus	1,389	907	868.1	38.9	4.64
Muslims	81	12	50.6	−38.6	78.98
	$P(\chi^2) < 0.001$, d.f. $= 1$, $\chi^2 = 83.62$				

Table 31 gives the percentages of persons having horoscopes, broken down by age, educational and occupational groups, expenditure levels, and communities. The total number of persons covered in the survey is shown in col. 2, and the number possessing horoscopes in col. 3. The over-all proportion of persons having horoscopes is 62.5 per cent The expected number of persons having horoscopes is shown in col. 4, and the difference between the observed and expected number in col. 5. The corresponding value of χ^2 is given in col. 6.

Variations by age-groups are not important, but non-matrics have comparatively fewer horoscopes (possibly because non-matrics on the whole belong to lower expenditure levels). The occupational distribution shows that the learned profession or landlords proportionately have more horoscopes. The distribution by expenditure levels clearly brings out that richer people on the whole have a proportionately larger number of horoscopes. Finally, from the distribution by communities it is clear that Hindus are much more interested in horoscopes than Muslims.

TABLE 32. CALCUTTA PUBLIC PREFERENCE SURVEY, 1941: REMARRIAGE OF WIDOWS AND WIDOWERS

groups	total number of persons giving opinion about remarriage of		percentage opinion in favour of							
			unconditional support		conditional support		unconditional opposition		indifferent	
	widows	widowers	widows	widowers	widows	widowers	widows	widowers	widows	widowers
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
				(a) by educational groups (males only)						
non-matrics	367	348	19.07	22.42	52.32	58.05	21.15	13.78	7.36	5.75
undergraduates	658	634	17.02	16.56	62.31	67.67	14.44	9.78	6.23	5.99
graduates	436	410	26.15	24.15	62.16	65.60	6.19	4.88	5.50	5.37
total	1,461	1,392	20.26	20.26	59.75	64.65	13.69	9.34	6.30	5.57
				(b) by sex (all cards)						
men	1,461	1,392	20.26	20.26	59.75	64.66	13.69	9.34	6.30	5.74
women	37	37	5.41	10.81	48.65	48.65	40.54	35.14	5.40	5.40
total	1,498	1,429	19.89	20.01	59.48	64.24	14.35	10.01	6.28	5.74
				(c) by community (all cards)						
Hindus	1,408	1,341	17.47	17.52	60.87	65.92	15.13	10.59	6.53	5.97
Muslims	90	88	57.78	57.95	37.78	38.64	2.22	1.14	2.22	2.27
total	1,498	1,429	19.89	20.01	59.48	64.24	14.35	10.01	6.28	5.74

Table 32 shows the distribution of opinion about the re-marriage of widows and widowers by educational groups, communities, and sex. Figures for unconditional opposition to the re-marriage of widows and widowers shown respectively in cols. 8 and 9 are of considerable interest. It is clear that there is much greater opposition to the re-marriage of widows. Opposition definitely decreased with increasing educational status, and women are more opposed to re-marriages than men. Finally, unconditional opposition to re-marriage is practically restricted to Hindus.

Table 33 shows public opinion on inter-group marriages, broken down by educational groups. From col. 6 it is seen that unconditional opposition decreases in every case with increasing educational status. On the whole, there still exists considerable opposition to inter-marriage between sub-castes (28.2 per cent) which is greater against inter-marriage between different castes (36.41 per cent). It is interesting to observe that the opposition is greater (49.14 per cent) against marriages within the same *gotra* (that is, within traditionally the same patrilineal family or clan) than against marriages between provinces (44.61 per cent). Opposition against marriage between communities—*i.e.*, Hindus and Muslims (60.64 per cent)—is as strong as opposition to marriages between different nationalities (58.66 per cent). It is interesting to note, however, that on the whole 12 or 13 per cent of the persons surveyed and about 20 or 22 per cent of graduates are unconditionally in favour of inter-communal or international marriages.

TABLE 33. CALCUTTA PUBLIC PREFERENCE SURVEY: INTER-GROUP MARRIAGE BY EDUCATIONAL GROUPS

type of inter-group marriage	educational groups	total number	percentage of			
			uncon-ditional support	con-ditional support	uncon-ditional opposition	indifferent
(1)	(2)	(3)	(4)	(5)	(6)	(7)
(*a*) between sub-castes	non-matrics	367	19.35	27.79	38.42	14.44
	undergraduates	658	31.00	27.66	30.55	10.79
	graduates	436	44.50	30.96	10.06	8.48
	total	1,461	32.10	28.68	28.20	11.02
(*b*) between castes	non-matrics	367	17.17	22.88	45.78	14.17
	undergraduates	658	23.56	25.84	40.42	10.18
	graduates	436	38.53	30.05	22.48	8.94
	total	1,461	26.42	26.35	36.41	10.82
(*c*) within same *gotra*	non-matrics	348	12.64	15.23	54.89	17.24
	undergraduates	635	16.70	20.94	51.18	11.18
	graduates	409	26.40	21.03	41.08	11.49
	total	1,392	18.53	19.54	49.14	12.79
(*d*) between provinces	non-matrics	348	10.92	22.99	53.16	12.93
	undergraduates	635	15.91	27.87	47.87	8.35
	graduates	409	26.65	30.32	32.27	10.76
	total	1,392	17.82	27.37	44.61	10.20
(*e*) between communities (Hindus and Muslims)	non-matrics	367	8.17	10.90	67.03	13.90
	undergraduates	658	9.57	14.13	66.42	9.88
	graduates	436	22.25	19.50	46.55	11.70
	total	1,461	13.00	14.92	60.64	11.44
(*f*) between nationalities	non-matrics	367	6.27	9.53	67.85	16.35
	undergraduates	658	10.03	17.17	62.16	10.64
	graduates	436	19.50	21.79	45.64	13.07
	total	1,461	11.91	16.63	58.66	12.80

Finally, Table 34 shows public opinion about religious instructions in colleges, broken down by educational groups. From col. 6 it is seen that 59.34 per cent are in favour of such instruction and 18.83 per cent not in favour, while nearly the same proportion appears to be indifferent. From col. 7 it is clear that opinion in favour of religious instruction in colleges definitely decreases with increasing educational status and that nearly one-third of graduates are not in favour of such instruction.

TABLE 34. CALCUTTA PUBLIC PREFERENCE SURVEY, 1941: RELIGIOUS INSTRUCTION IN COLLEGES BY EDUCATIONAL GROUPS

educational group	number of persons				percentage of persons			
	in favour	not in favour	indif-ferent	total	in favour	not in favour	indif-ferent	total
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
non-matrics	288	22	87	397	72.54	5.54	21.92	100.00
undergraduates	422	116	125	663	63.65	17.50	18.85	100.00
graduates	179	144	115	438	40.87	32.88	26.25	100.00
total	889	282	327	1,498	59.34	18.83	21.83	100.00

MODEL SAMPLING EXPERIMENTS

I must refer to another special feature of the Institute—namely, the extensive use of model sampling experiments in applied work. For example, in the Bengal crop survey, information was collected about the crop grown on each individual field for large tracts, and these were recorded on maps. It was then possible to carry out, in the Statistical Laboratory, a large variety of model sampling experiments using grids of different sizes and densities. Such studies proved most helpful in developing the actual sampling technique.

In the case of space distributions, it has been shown elsewhere that the patch number supplies a useful concept. Consider a map of, say, an agricultural area with fields which are assumed, for the sake of simplicity, to be of the same size. Suppose each field which is shown with paddy is coloured yellow, and fields without paddy are left blank or white. The map of any such region would be then broken up into a number of yellow and white patches, depending on the actual distribution over space of fields under paddy. If alternate fields in both directions are sown with paddy, then the pattern will be something like that of a chessboard; and in this case the total number of patches will be equal to the total number of fields. On the other hand, if half the whole area is sown with paddy in one compact block and the other half is left without paddy, then the patch number will be simply two. In any actual situation the patch number will lie between these two limits. If the fields to be sown with paddy (or to be marked yellow) are selected purely at random, then the patch number would show a characteristic distribution.

It is obvious that the design of the sample survey can be made more efficient if something is known about the distribution of the patches. For example, if the patch-pattern is of the chessboard type, then it will be quite sufficient to explore (in the ideal limit) only two adjoining cells to secure a complete picture. If the patch number is only two, then the best plan would be to try to settle the boundary between the two heaps. In any case, when there is any appreciable tendency towards patterning, it is often possible to have recourse to configurational (which is sometimes called "systematic") sampling.

For two colours (*i.e.*, for a binomial distribution) in one dimension the theoretical solution is known. For more than two colours in one dimension, or for two or more colours in two dimensions, complete theoretical solutions have not yet been given. Raj Chandra Bose is carrying on mathematical researches on the sampling distribution of the patch number in the case of a binomial field in two dimensions, and has obtained a number of useful results.

Investigations are also proceeding on purely experimental lines under the leadership of Jitendra Mohan Sen Gupta. A number of different sets of fields each of size 100×100 —*i.e.*, consisting of 10,000 cells arranged in the form of a square—were constructed and filled with numbers from 0 to 9 at random (or, to be more strict, what are believed to be arranged at random) in two dimensions. Some were based on well-known tables of random numbers. A large number were produced mechanically with the Hollerith equipment by the British Tabulating Machine Co., Ltd. By assigning different digits to different colours, it is possible to prepare random distributions of patches on these fields and to study the patch number experimentally. Work is being done on a large scale, and has already yielded results which, it is believed, would supply useful information for the guidance of practical work, as well as valuable clues for mathematical researches.

I shall give one concrete example. Consider an $n \times n$ field. Let p be the probability for a cell to be black, and q to be white $(p + q = 1)$. In counting black patches contact is recognized only along sides, but in counting white patches contact is permitted both along sides and at corners. Let B be the number of black patches and W the number of white patches (as defined above), but embedded within black patches. R. C. Bose has given the following results :

$$\text{Expectation } E(B - W) = p + 2(n-1)pq + (n-1)^2(pq^2 - p^3q)$$

$$\text{Variance } V(B - W) = n^2pq - 4(n-1)p^2q - (6n^2 - 10n + 4)p^2q^2 - (6n^2 - 40n + 50)p^3q^2 + $$
$$+ (14n^2 - 56n + 54)p^3q^3 + (32n^2 - 104n + 84)p^4q^3 - (9n^2 - 30n + 25)p^4q^4.$$

The results of one series of model sampling experiments are given in the following two Tables, 35 and 36. The observed average number of (black minus embedded white) patches and the expected number calculated by the above formula are shown in cols. 2 and 3 of Table 35 for various values of p. The difference between the observed and expected number divided by the standard error of the difference (based on the theoretical value of the variance as calculated from the formula given above) is shown in col. 4. Although the distribution may not be strictly normal in the present case, it is likely that the figures in col. 4 may behave approximately as normal deviates. It will be noticed that out of thirty-six comparisons no fewer than ten are significant at 1 per cent level and three at the 5 per cent level.

TABLE 35. MODEL SAMPLING EXPERIMENTS: COMPARISON OF EXPECTED AND OBSERVED AVERAGE NUMBER OF (BLACK MINUS EMBEDDED WHITE) PATCHES IN A BINOMIAL FIELD

p	average number: observed	average number: expected	normal deviate	average number: observed	average number: expected	normal deviate
(1)	(2)	(3)	(4)	(5)	(6)	(7)
	10 cells × 10 cells (n = 1000)			20 cells × 20 cells (n = 250)		
0.1	7.99	8.21	−3.07†	31.54	32.44	−0.35
0.2	12.81	12.93	−1.56	49.78	50.18	−1.34
0.3	14.44	14.46	−0.32	54.83	54.52	0.95
0.4	13.30	13.21	0.32	48.34	47.67	1.76
0.5	10.28	10.06	2.17*	33.62	32.56	2.51*
0.6	5.29	5.70	−4.09†	11.97	13.19	−2.93†
0.7	0.96	1.25	−3.38†	− 7.37	− 5.72	−4.70†
0.8	−2.06	−2.02	−0.63	−18.34	−18.53	0.74
0.9	−2.89	−2.66	−4.85†	−18.34	−18.75	2.03*
	50 cells × 50 cells (n = 200)			100 cells × 100 cells (n = 50)		
0.1	199.93	201.24	−1.67	797.48	802.98	−1.76
0.2	305.87	307.84	−2.40*	1,215.70	1,233.68	−4.46†
0.3	328.44	328.45	—	1,294.34	1,297.93	0.98
0.4	278.42	277.47	0.87	1,090.06	1,082.91	1.64
0.5	177.40	175.06	1.93	681.74	662.56	3.93†
0.6	41.03	47.17	−5.17†	122.82	142.21	−4.05†
0.7	− 77.50	− 74.52	−3.00†	−352.26	−348.78	0.87
0.8	−151.82	−152.55	1.03	−654.86	−657.71	1.01
0.9	−143.17	−143.70	0.92	605.84	−607.50	0.71

* Significant at 5 per cent level. † Significant at 1 per cent level.

A similar comparison of observed and expected values of the standard deviation is shown in Table 36. Using large sample theory, the differences between observed and expected values are found to be significant in three out of thirty-six cases at the 5 per cent, and three out of thirty-six cases at the 1 per cent level of significance.

TABLE 36. COMPARISON OF OBSERVED AND EXPECTED VALUES OF STANDARD DEVIATIONS OF THE NUMBER OF (BLACK MINUS EMBEDDED WHITE) PATCHES IN A BINOMIAL FIELD

p	10 cells × 10 cells (n = 1,000)			20 cells × 20 cells (n = 250)			50 cells × 50 cells (n = 200)			100 cells × 100 cells (n = 50)		
	s.d. observed	s.d. expected	normal deviate	s.d. observed	s.d. expected	normal deviate	s.d. observed	s.d. expected	normal deviate	s.d. observed	s.d. expected	normal deviate
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
0.1	2.24	2.27	−0.60	4.31	4.47	−0.80	11.17	11.08	0.16	21.85	22.09	−0.11
0.2	2.42	2.40	0.40	5.20	4.70	2.38*	12.90	11.62	2.19*	25.71	23.18	1.09
0.3	2.62	2.55	1.17	5.75	5.13	2.70	15.20	12.89	3.61†	30.26	25.84	1.71
0.4	2.99	2.91	1.14	6.05	6.01	0.15	15.67	15.34	0.43	32.16	30.89	0.41
0.5	3.16	3.20	−0.57	6.35	6.68	−1.10	17.45	17.12	0.38	41.72	34.52	2.09*
0.6	3.19	3.17	0.29	6.38	6.58	−0.69	16.76	16.81	−0.06	29.71	33.85	−1.22
0.7	2.66	2.72	−1.00	5.55	5.55	0.00	13.57	14.03	−0.66	25.16	28.16	−1.06
0.8	1.82	1.99	−4.25†	3.92	4.01	−0.50	10.05	10.00	0.10	17.85	19.98	−1.06
0.9	1.70	1.50	6.67†	3.36	3.19	1.21	8.49	8.23	0.63	17.01	16.64	0.22

* Significant at 5 per cent level. † Significant at 1 per cent level.

It is clear that expected values of the patch number or of the variance are not confirmed in a rigorous manner. The general agreement between observed and expected values indicates, however, that the theoretical results are probably not wrong. The failure is, therefore, most probably due to some or all the fields used for experimental sampling not being of a sufficiently random character. This often happens in practice. In fact, as pointed out in the paper on large-scale sample surveys (in the *Phil. Trans.*), the concept of degrees of randomness is particularly useful in situations similar to the present one. I may conclude this section by stating that we have a big programme of work for model sampling experiments, but progress is slow for lack of resources.

Sample survey of the economic background of the Bengal famine

A sample survey was undertaken in 1944-45 to collect information relating to the after-effects and the economic background of the Bengal famine of 1943. A first report by Ramkrishna Mukherjee, Ambika Ghosh and myself has been published in *Sankhyā* (the Indian Journal of Statistics), Vol. 7, Part 4, and it is not necessary to enter into details, but a few typical results may be of interest in the present connection. The enquiry covered 15,769 families selected at random from 386 villages, which themselves were selected at random from 41 (out of 86 rural) sub-divisions (administrative units) covering about 60,000 sq. miles in Bengal. The design was zonal, with stratification of sub-divisions by intensity of incidence of famine conditions; and randomization of villages was completed separately within each sub-division.

From the sample survey it appeared that the land position was precarious even before the famine, with one-third of all rural families having no paddy land, while two-fifths had less than 2 acres, so that about three-fourths of all rural families had either no paddy land or owned less than 2 acres. With an average production of 820 lbs. of rice per acre, an average consumption of about 320 lbs. per head per year and an average family size of 5.4 persons, the subsistence level would be about 2 acres of paddy land per family on an average. The actual over-all average for the province was, however, found by the sample survey to be about 1.8 acres of paddy land per rural family, which was below the subsistence level. It is not surprising therefore that, averaged over a number of years before the war, there was a small but net import of a little over 1 per cent of total production of rice and other cereals into the province.

The sample survey showed that the classification of sub-divisions by amount of paddy land owned per family before the famine was roughly parallel to the degree of incidence of famine conditions, indicating that sub-divisions in which there were more families with paddy land below subsistence level were more vulnerable to the famine.

It was found that about 1.6 millions of families (about one-fourth of the number who had owned paddy land before the famine) had either sold in full or in part, or had mortgaged their paddy land during the famine period. About a quarter of a million of families were obliged to sell all their paddy land, and were thus reduced to the rank of landless labour.

The net loss of plough-cattle was about a million, or 13 per cent, during the famine period. Sales of cattle largely exceeded purchases, showing that transfers had taken place not merely from one rural family to another, but that large purchases to the extent of about 600,000 head of cattle had been made by outsiders (possibly by contractors for the supply of meat for army consumption).

The economic deterioration was measured by the number of families transferred from occupations at a higher economic level to occupations at a lower level. Assessed by such methods, it was found that about 700,000 of rural families had suffered a lowering of economic status during the famine. It was also found that even at the beginning of 1943 (before the advent of famine conditions) there had been already an increase of 150,000 of destitute persons. There was a further increase of about 330,000 destitutes during the famine itself, so that nearly half a million persons were rendered destitute under war and famine conditions in Bengal.

Even in the pre-famine period (January 1939 to January 1943) about 6.84 per cent of rural families had suffered economic deterioration against 3.32 per cent who had improved their economic level, while the position of 1.07 per cent was not clear. Economic deterioration had thus set in definitely in the pre-famine period. Rates of change became more rapid during the famine period. Improvement in economic status during the famine period was relatively twice as great as that in the pre-famine period; but this was offset by a three times greater rate of economic deterioration and twelve times greater of destitution. The famine period was thus one of greatly accelerated economic changes. Improvement of economic conditions was quicker, but was restricted to a comparatively small number of families. Deterioration and destitution were even more accelerated, and were shared by a large number of families. The poorer sections of the community, especially landless labour, fishermen, and village craftsmen, were most seriously affected, and many were rendered destitute; the middle group, who had land of their own and other assets, were naturally less vulnerable; and a comparatively small number of families in the upper stratum had remained immune, and sometimes even became prosperous.

The above summary gives an idea of the kind of information which it was possible to secure by the sample survey. The margin of error of the sample estimates in the cases investigated varied roughly between 3 and 5 per cent. Comparison with other available statistics also showed differences of the same order. For example, extrapolating from the cattle census figures of 1930 and 1940, the calculated number of plough-cattle in rural Bengal in the beginning of 1943 was about 8.3 millions. The sample estimate was about 7.9 millions, giving a difference of about 5 percent. The sample estimate of 18.6 millions of acres of paddy land was also not inconsistent with other available estimates. On the whole, the above enquiry showed the possibility of using the sample survey in a quick and efficient manner to obtain information about economic conditions in rural areas with a margin of error sufficient for many practical purposes.

A MULTI-PURPOSE SURVEY IN BENGAL, 1946–47

I may briefly refer to an extensive survey (which is actually in progress in Bengal at the present time, in 1946–47) as an example of a multiple survey. The size of the grid (sample unit) in this case is just 1 sq. mile, and the information is being collected in the form of two independent but interpenetrating networks of sample units. In the first stage of the survey all households falling within each grid were surveyed with a rather short schedule covering a number of basic items, such as sex and age composition of the family, caste and community, occupation, agricultural land, number of cattle, total indebtedness, etc. Information has been already collected for 84,370 families from 475 grids scattered over about 60,000 sq. miles of rural Bengal. The information is being tabulated on Hollerith machines, and certain preliminary estimates have been made on the basis of about half the number of families surveyed.

The total rural population based on this portion of the material is found to be 50.16 millions, roughly as in March or April 1946, with a calculated standard error of 5.26 millions. The corresponding census population in rural Bengal in March 1941 was 47.185 millions. Owing to uncertainties in census counts it is difficult to calculate reliable rates of growth of population, especially in Bengal. It is also difficult to assess the exact effects of the Bengal famine of 1943 on the growth of population. We may, however, make some rough calculations. The total population (including rural and urban areas) of Bengal was about 46.7 millions in 1921, 50.1 millions in 1931, and 60.3 millions in 1941. Very rough estimates by linear extrapolation can be made on the above basis. Adopting the rate of growth during 1931–41, the extrapolated rural population as in March 1946 would be about 51.9 millions. There are, however, reasons to believe that the census count in 1931 was abnormally low on account of the non-co-operation movement. Adopting the average rate of growth during the twenty-year period 1921-41, the extrapolated figure for the rural population at the end of 1944 would be about 50.4 millions. The observed sample estimate of 50.1 millions is thus of the right dimensional order; and is fairly satisfactory with a sampling fraction of about 1 : 250.

The enquiry just completed is, however, only the first fold or layer of the survey. Arrangements are now being made to select about 20 per cent of the families included in the first survey for a more detailed study of rural indebtedness and agricultural labour. If everything proceeds smoothly it should be possible to amplify the results for the second survey for making estimates for the sample covered in the first survey, and hence for the rural population of Bengal as a whole. After the second survey is over, if funds permit, it is proposed to reduce the number of families much further and carry out a detailed enquiry into family budget and other socio-economic conditions. At each successive survey the families would be selected at random, but with appropriate zoning and/or stratification, so that it should be possible to amplify the results of one survey to obtain estimates for the families covered in an earlier survey, and hence, finally, for all rural families in the province.

OTHER WORK IN STATISTICAL SAMPLING

A good deal of other work on statistical sampling has been and is being done in the Indian Statistical Institute. In most cases these have definite practical ends in view. The important point which I should, however, like to emphasize is that such practical studies have continually given rise to theoretical problems of fundamental interest. The subject of crop surveys itself raised many questions of theoretical interest, to some of which I have already referred. I have also mentioned that probability problems relating to space distributions are being tackled on fruitful lines with the help of topological and combinatorial concepts by Raj Chandra Bose. The close integration of applied work and theoretical researches has been such a valuable feature of the work of the Institute that it seems worth while giving a few more examples.

Analysis of anthropometric measurements. The statistical analysis of anthropometric measurements—work which may be properly considered to be purely of an applied nature—had led, about 20 years ago, to interesting theoretical developments in the formulation of the generalized distance (D^2-statistic). It was the starting point of a good deal of mathematical research in the theory of sampling from multivariate correlated population by Raj Chandra Bose and Samarendra Nath Roy, whose work in this subject is already well known.

We had occasion recently to take up an anthropometric study of an extensive series of physical measurements covering 3,000 individuals belonging to twenty-two different castes and tribes in the United Provinces of India. All the measurements were taken by one individual observer, Dr. D. N. Majumdar of the Lucknow University, and were therefore free from differences of personal equation. This made the material particularly suitable for comparative purposes. The use of the D^2-statistic has, I believe, yielded information of great interest and significance. As the paper is in the press, and will be shortly available, I need not enter into details.

I may mention one result which is of some methodological interest. From general considerations I reached the conclusion that, when correlations between variates are taken properly into consideration (as in the D^2-statistic), the use of indices (such as the cephalic or the nasal index) do not supply any additional information. This has been fully confirmed by C. R. Rao by actual numerical calculations. In fact, the present study has given rise to a number of interesting theoretical developments on which Rao has done some very useful work.

More recently, in 1945–46, Dr. D. N. Majumdar, working under the auspices of the Institute, has collected a large volume of material relating to physical measurements and blood-group tests of about 4,000 individuals belonging to various communities, castes, and tribes of Bengal. The analysis of this material should yield valuable results. A survey of blood pressure which was concluded some time ago is perhaps worth mentioning in the present connection.

Design of experiments. At one time a great deal of work relating to the design of agricultural experiments has been done in the Institute. This had led to significant theoretical researches in which the concepts of the Galois field and finite geometry were used with great success by R. C. Bose and, under his leadership, by workers like K. Raghavan Nair, K. Kishen, C. R. Rao, Harikinkar Nandi and others.

Circulation of rupees and rupee notes. At the request of the Reserve Bank of India, experimental surveys were made and methods were devised for estimating the circulation of silver rupees of various dates by sample counts at a number of receiving centres, like banks or railway stations. A method was also devised, based on sample counts of rupee notes received back in the Reserve Bank, to estimate the average life of such notes. It may be mentioned, incidentally, that this project gave rise to an interesting problem of measuring the magnitude of the difference between samples drawn from multinomial populations which was tackled on fundamental lines on the theoretical side by Anil Kumar Bhattacharyya, with promising results. A brief note was issued in the Report of the Reserve Bank of India on Currency and Finance for the year 1940–41, pp. 49–54.

Sampling for yield of cinchona bark. I have already mentioned the project for estimating the yield of cinchona bark as an example of multiple sampling. It is interesting to note that the final choice of physical measurements was made in this case from considerations of cost. It was found, for example, that although the measurements of the surface area of the standing plant gave the highest correlation with the yield of bark (0.865 in one case against the next highest 0.690 for a different character), the use of three simple physical measurements like the "standing vertical height of the plant," the "girth at height of 6 inches above ground level," and the "number of stems of the plant at ground level," gave a multiple correlation of 0.848. Theoretically the use of surface area would be more efficient, inasmuch as observations on eighty-nine plants would give the same information as the measurement of the three other characters on 100 plants. The time and trouble to measure the surface area for eighty-nine plants would be, however, far greater than that for measuring the other three characters on 100 plants. This is a simple example of the use of cost considerations in settling the sampling programme.

This particular project also has given rise to many interesting problems of the estimation of appropriate errors in the case of multiple sampling of various kinds. Mrs. Chameli Bose has obtained a number of useful results and is working on the subject.

Population enquiries. After the Indian Census of 1941, the Government of India had decided, as a matter of economy, to cut down most of the standard tables; even age or occupational tables were not prepared. Fortunately, Mr. M. W. M. Yeatts, as Census Commissioner, had issued instructions for the preservation of a 2 per cent sample consisting of every fiftieth individual census slip. In certain areas, mostly in Indian States, full tabulation had been carried out. A comparison of results based on the complete count and on the 2 per cent Y-sample in such areas showed that it should be possible to reconstruct most of the tables with sufficient accuracy for practical purposes. About 7 millions of the 2 per cent slips have been brought over to the Statistical Laboratory in Calcutta from all over India, and the work of transferring the information to Hollerith cards has already started. In only one province—namely, Bihar—the original census slips had also been preserved; and *ad hoc* sampling studies on the basis of this material are also proceeding.

The above projects were sanctioned on the recommendations of the Population Data Committee which was appointed by the Government of India in May 1944, and which submitted its report in June 1945. A comprehensive programme relating to population census and demographic statistics generally was prepared by this Committee, and it was definitely recommended that a continuing sample census of the Indian population should be carried out from year to year.

Statistics of road development. Mention may also be made of the use of sampling methods for traffic count on roads and connected socio-economic variates in the traffic catchment. The object in this project is to lay down a scientific foundation for preparing programmes of road development. K. B. Madhava and Satyabrata Sen are at present actively engaged in this work.

Postal traffic and revenue. An enquiry is proceeding at present to explore the possibilities of using the sampling method for estimating in advance the volume of traffic and revenue in different postal sectors. Exploratory surveys under the guidance of Satyabrata Sen have already yielded encouraging results.

INTEGRATED PROGRAMME OF WORK

The need of handling computational work on a very large scale has been salutary in teaching us the importance of efficient organization of the human agency. J. M. Sen Gupta and N. T. Mathew and others have done valuable work in this direction. An account of this work, especially from the point of view of cost accounting, would, I think, be of considerable interest. Unfortunately, we have not yet had sufficient time or money to undertake this in a systematic manner.

Along with computational work may be mentioned the preparation of statistical tables of various kinds (which often require the calculation of numerical values of Bessel and other mathematical functions), which has been proceeding for many years, and offers scope for training in a very useful branch of statistical work. The names of J. M. Sen Gupta, Purnendu Kumar Bose, and Raja Rao may be mentioned.

The contact between statisticians and field workers has also been fruitful in every way. The credit for the successful organization of the field side is almost entirely due to the organizing leadership and the ability to appreciate statistical needs on the part of Nihar Chandra Chakravarti and workers in the Field Branch, like Dhirendra Mohan Ganguly, Pronay Kumar Chatterjee and others. Much valuable work on the field has also been done by statisticians like J. M. Sen Gupta, Birendranath Ghosh, N. T. Mathew, Purnendu Kumar Bose, Harikinkar Nandi, and others. I may perhaps also mention that during the last few years the usual practice was for all theoretical workers, including R. C. Bose and S. N. Roy, to live in camp on the field for a few weeks in connection with the crop-survey work.

Training in statistics has also been an important part of Institute activities from the very beginning. One-year post-M.Sc. courses, as also occasional special courses in particular subjects, are being arranged for a long time. The Institute workers have also been intimately associated with the two-year course leading to the M.A. or M.Sc. degree and the recently introduced two-year course leading to the Honours B.Sc. degree in Statistics in the Calcutta University.

In the light of the experience gained during the last fifteen years I am convinced that the close integration of applied work, theoretical research, and training offers the soundest line of advance in statistical work, especially on the professional side. It offers scope for developing the spirit of team work among persons with widely varying interest which alone can make it possible to tackle many problems on a scale large enough to ensure success. I have been fortunate in having secured a large group of able and enthusiastic workers, each of whom in his own line has much greater knowledge and ability than myself. Whatever success the Indian Statistical Institute has achieved is in fact essentially due to the spirit of co-operative effort.

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DISCUSSION ON PROFESSOR MAHALANOBIS'S PAPER

DR. F. YATES : I have much pleasure in proposing the vote of thanks. Professor Mahalanobis has given us a most interesting description of the work for which he has been responsible in India. His address has provided a vivid picture of the Institute which he directs, and at the same time has shown what enormous strides have been made there, and how extremely able his direction has been.

There is not time to discuss all the many interesting points that Professor Mahalanobis has raised, but I should first like to take the opportunity of expressing my admiration for the way in which he has handled the analysis of the social survey and broadcast survey data, the discussion of which formed the last part of his address. Those who have had experience of the analysis of data of this type will know that while it is comparatively easy to present tables giving over-all percentages, etc., the critical analysis of such material does present very great difficulties. Professor Mahalanobis is certainly to be congratulated on the penetrating methods of analysis he has developed in connection with these surveys. There is no doubt that the statistical analysis is the most difficult part of social survey work of this kind, and it is much to be hoped that further efforts will be made to develop the methodology.

Professor Mahalanobis did not spend much time discussing his sampling methods. There are several points of mutual interest that I should like to have the opportunity of discussing with him personally, but which I will not trouble to bring up now. I fully agree with Professor Mahalanobis that the determination of the sampling procedure is often the easiest part of a survey, but I would stress that this ease is in fact based on experience, which has to be acquired by a thorough study of the variability of the material that is being sampled.

Professor Mahalanobis finds the sampling problems easy because his Institute has built up a body of experience in sampling of the particular types of material with which it is concerned. This contains a moral for us all. We cannot devise good statistical methods merely by sitting in our studies and theorizing. Good statistical methods almost invariably result from contact between the mathematical statistician and the workers who are responsible for collecting the data, and who are interested not in the statistical methods themselves, but in the conclusions that emerge from their work. Consequently, the statistician must be in intimate contact with the actual numerical material. He must not think solely in terms of algebraic symbols. Professor Fisher once made what I consider a most revealing remark : "Most of the statistics which I have learnt, I have learnt on the computing machine."

Professor Mahalanobis has been concerned with the problem of securing numerical accuracy in the computations. In this connection I might mention the experience I had in the geodetic survey of West Africa. In that part of the world it was necessary to carry out much of the main framework of the survey by means of traverses instead of triangulation. The difficulty with traverses is that they involve a vast amount of computation which is not self-checking. In triangulation, once the base line is determined, the remainder of the computations are self-checking—it is impossible to make an error of any magnitude in a computation which will not reveal itself later in glaring discrepancies.

We had a very great struggle to get the numerical work of our traverses correct. We eventually evolved a most rigorous system of computation. All computations were done in the field in duplicate, the duplicates being compared only at certain points. A third computation was then done at headquarters using the "setting-out" taping, which was carried out in metres instead of feet, and bearings 45° different from the true bearings, so as to give different sines and cosines. This last computation was carried out so as to eliminate errors of the type that are likely to be made independently by different computers—*e.g.*, writing an odd taping length of 449.321 ft. as 499.321 ft., when practically all the taping lengths are just under 500 ft.

I was interested, after this system had been evolved, in finding that the U.S. Coast and Geodetic Survey laid it down in their standard instructions that triangulation was to be used in preference to traverses, if the cost was not more than 100 per cent greater, simply because of the danger of computing errors. (It is now recognized that with modern invar tapes traverses are quite as accurate, possbily more accurate, than triangulation.)

Professor Mahalanobis stressed the importance of providing the results of sampling surveys on the required dates. We had an interesting example of this—which illustrates the flexibility of sampling methods—during the war, when we were making an estimate of the amount of standing timber in the country. The first estimates were required within about five months of the decision to undertake the survey. Initially the survey had been planned to be undertaken in two 5 per cent samples, the sampling units being 6-inch Ordnance Survey Quarter Sheets (3 miles × 2 miles). Owing to the almost inevitable delays in organization, collection of staff, etc., it was clear by the time we were ready to start work, that the first 5 per cent sample would not be completed and analysed by the given date. I therefore insisted that the first 5 per cent sample should be divided into two parts. This was done by the simple process of drawing a diagonal line across each sampled 6-inch Ordnance Survey Quarter Sheet. There was, in fact, considerable opposition to this, as it was thought that the results of a 2½ per cent sample would not be sufficiently accurate for the required purpose. Fortunately I knew, from previous experience, the variability of the material, and was able to promise that the results would be of the required accuracy, and that moreover when they were obtained, estimates of the actual standard errors could be furnished so as to provide concrete evidence of this accuracy. I am not sure whether these standard errors really carried conviction, but what certainly did convince those who were initially in doubt was the close agreement between the first 2½ per cent sample and the second 2½ per cent sample, and between these and the second 5 per cent sample.

This illustrates an important point in survey work. In general, over-all estimates for the whole area are first required. It is on these estimates that the broad administrative decisions are based. Subsequently more detailed estimates for different parts of the area may well be required, in order to implement the administrative decisions. By carrying out a sampling survey in stages, as was adopted in the Forestry Survey, and as is also, I think, Professor Mahalanobis's practice, the administrators can be given the necessary information on which to base their decisions, while additional data for more detailed and accurate estimates are being collected.

Professor Mahalanobis mentioned the large biases that he has encountered when taking small areas for estimating crop yields. To a certain extent, we have had the same trouble in this country, but only to a limited degree. I suspect that many of these troubles will gradually fade away, as a permanent organization of trained people is built up. In our survey work in Africa, for example, much of the field work was undertaken by native surveyors who were trained in a Survey School which had been specially set up for the purpose. We had very little trouble with these surveyors. The very rigorous methods of booking observations which are always followed in survey practice were a great help. All observations must be booked direct into properly printed field note-books which have numbered pages. The surveyor is completely at liberty to reject any observations with which he is not satisfied, but a permanent record of such observations is always available, so that it is possible to find out what the surveyor has really done. Sometimes they did very funny things. One surveyor—quite a conscientious man—had been taken to task because his field work was not reaching the required standard of accuracy. Subsequent to this he turned in a series of traverses in which the closing errors in his bearings (on astronomical azimuths) were far smaller than they should have been. At first it was thought that the results had been deliberately faked. Subsequent cross-questioning revealed, however, that having made a preliminary computation of his closing error and finding it larger than he thought it should be, he had gone back the next day and re-observed some of the angles, rejecting the old or the new observation of each angle according to which improved the closure. The methods of booking enabled him to prove that this was what had been done, and also enabled the matter to be set right without any re-observation.

DR. C. OSWALD GEORGE, in seconding the vote of thanks: Never have I so much enjoyed listening to an address of this character, not only because of its intrinsic interest, but also because of the particularly charming manner of its delivery.

The first point which strikes one is perhaps the difference between conditions in India and in this country. On the other hand, there are important similarities. Of the various headings Professor Mahalanobis has written on the blackboard, we in this country also have to consider problems of time and sampling errors, and —for there is a body here known as the Treasury—the cost of statistical enquiries. Calculating and recording errors are also not unknown here. Had I been speaking a fortnight ago, I might have spoken more confidently on the much higher standards of accuracy in this country, but I fear Professor Mahalanobis may have seen the unfortunate calculations of mean yields in a recent issue of what we had previously regarded as an almost infallible newspaper. So I will pass over that point.

What he calls errors from physical fluctuation, I assume, arise with crops such as jute, where one reporter may find two acres under the crop, while another reporter, visiting the same farm a fortnight later, may find no jute acreage whatever. Under the existing system in this country, where returns relate to a specified day, the same problem does not arise, but some of his sampling problems interest us. He tells of the various sampling methods he adopted, "unitary and zonal, configurational and unrestricted," but unfortunately he has not given us sufficient detail to permit any useful comment on their theoretical or practical merits. He frequently mentioned large-scale sampling theory, but if greater use had been made of stratified sampling, small sample theory would possibly have come more into the picture.

I was particularly interested by his detailed treatment of errors, for I think the question of errors (other than sampling errors) is of more importance than is sometimes believed. How many can lay their hands on their hearts and say that in their published work, errors—if not quite of the type most troublesome in India, nevertheless of serious import—have never reached the printers? But it is noteworthy that Professor Mahalanobis does not mention errors due to the machines: perhaps in the Indian climate machines are infallible!

Another point that struck me was the speaker's attitude towards a system of complete enumeration and his declaration, when comparing it, detrimentally, with sample surveys, that complete enumeration is entirely lacking in any kind of control and precludes any valid estimate of the margin of error of the final results. Without further explanation it is not easy to see why the underlying weaknesses are inherent in the one and entirely absent from the other, or why a system of duplicated surveys cannot be used in conjunction with complete enumeration. And whatever may be desirable in India, it is not likely that in this country complete enumeration will be entirely dispensed with, but rather that it will be used as a basis upon which sampling surveys may be developed.

Professor Mahalanobis has laid particular stress on the results of the Bihar survey shown in Table 1, and it is to this that I should like to confine my final remarks. He says that his system of sampling was very successful, in that in the case of rice and pulses a very small sample of about 0.025 per cent gave results within about 2 per cent of the final result, obtained from the total sample. We are not told what was the result of the small sample in the case of sugarcane. But what strikes one most about Table 1 is perhaps the variations between the various estimates, particularly that between estimates 8 and 9, which suggest something more than sampling errors. Estimate 9 for rice gave 13,650,000 acres, a fall of 355,000 acres compared with estimate 8. It is noticeable that this is a larger difference than that between estimate 1 (based on a sample of one in 4,175) and estimate 9, although it resulted from a large increase in the sample (from 80.7 to 100.0 per cent, or 19.3 per cent of the whole sample). If my arithmetic is not wrong, it seems that the total acreage in or about January, if it had been estimated from this 19.3 per cent of the total sample, would be only about 12.2 million acres.

What is the explanation of such large discrepancies? We have not been given sufficient detail to decide this point. An apparent explanation would be that there are what Professor Mahalanobis referred to as physical fluctuations, which would presumably mean that the acreage under rice changes substantially from month to month, or even from day to day. But this can hardly be the explanation, for if it were, any similarity between the first estimate in October and the last in January could hardly be attributed to the merits of the sampling methods employed. The question would also arise of what was being estimated: was it the acreage under the crop in October, or in January, or the total cropped acreage for the season; and how are any of these to be estimated from a sample drawn over a lengthy period? As for the remaining crops, estimate 8 for pulses is 2,963,000 acres, as against 2,903,000 for estimate 9, and the sugarcane estimates show similar intriguing differences.

I have no doubt that there is some very simple explanation of all these divergencies, and that this would have been made quite clear if circumstances had permitted Professor Mahalanobis to submit his paper in the customary form for printing and distribution to Fellows at this meeting.

Time will permit only one further very brief point. Professor Mahalanobis said that at the last stage, at the request of the Government, an estimate of 312,700 acres was also given for the area under potatoes, only about 0.26 per cent of the total geographical area covered by the sample survey, and he added that this supplies a good idea of the wide range of the survey. It would therefore be interesting to have details of this estimate and to know the sampling errors involved, how they were calculated, and how they compare with those of the other estimates.

Dr. George concluded by expressing the deep pleasure it gave him to second the vote of thanks for such a valuable paper from so distinguished a visitor.

The vote of thanks was then put to the meeting, and carried unanimously.

DR. M. S. BARTLETT, after adding his personal thanks and his welcome to Professor Mahalanobis, said that there was one incidental point raised by the last speaker, Dr. George, on which he also was not very clear, and that was the precise meaning of "physical fluctuations" as distinct from "sampling variations."

When considering the paper generally, what he felt was perhaps not so much the novelty of the methods used, but the value of the integration or synthesis of methods used to meet a variety of demand. It was rare to find one department which used these methods for such varied activities as agricultural crops and the investigation of the cost of living and family budgets. Possibly in this country, especially on the social and economic side, those concerned with similar investigations would be studying further the methods which Professor Mahalanobis had described. In particular, there was a great deal to be said for this study of variability, a point which was well recognized in agricultural work, but which had been brought out that evening on the social and economic side. It not only enabled one to judge the accuracy of one's averages at the end, but it gave invaluable information on the material itself.

To take an example. Supposing they made an enquiry into the consumption of a particular food in connection with rationing. Such a study of variation, not only between the broadly classified groups of consumers, but within those classes, would not only give the accuracy of the mean level as shown by the sample, but it would give the variation between those classes. A knowledge of such variation was desirable in order to estimate the change in the mean level of consumption resulting from rationing and the amount of individual hardship which would be entailed if the people were rationed at definite mean levels.

There was one further theoretical point he wanted to mention. Although it was not covered by the paper that evening, it was mentioned in the paper in the *Philosophical Transactions* which had been referred to. He thought it had some relation too with the last Table which Professor Mahalanobis had produced, which he gathered had something to do with the question whether units were random and independent or not. In his paper in the *Philosophical Transactions* Professor Mahalanobis referred to what he called space correlation functions; this was theoretically an interesting concept related to the time correlation functions which occurred in time series. These had been encountered before; they had, for example, been used by Sir Geoffrey Taylor in the theory of turbulence. The property of stationarity in time series was the property of homogeneity in space correlation functions. Further questions that arose were: Were these functions isotropic?—that is, independent of the direction in which one was measuring the correlation—the answer in agriculture was probably "no"; and Did these functions ever show periodicity?

The sort of question which one asked in agricultural design work was usually answered on the basis of a uniformity trial, from which was worked out semi-empirically what was the optimum design. The same answer (for constant variability) must follow from a knowledge of the correlation function. Such an alternative approach did not of course affect the final analysis of any data which was accumulated on a basis of the design finally decided; that followed the standard analysis of variance along the lines that had been described.

The following contributions were received in writing after the meeting:

MAJOR M. D. W. ELPHINSTONE: Professor Greenwood mentioned his own admiration for Professor Mahalanobis's mathematical skill; I also worship from afar. But even more do I admire the courage which has enabled Professor Mahalanobis to build up his organization in the face of all the discouragements and difficulties which he has had to face.

I have during the war been responsible for certain administrative statistics in G.H.Q., India, and I can imagine some of the difficulties overcome.

In the first place, there is apathy—an apathy difficult to imagine even in our own constant struggle against wilful ignorance at home. Even those who care for good work are apt to be worn down by the continuous output of energy required to get any new idea accepted. The strain is greater in a hot climate, and appears to affect white and brown-skinned races alike.

Then there is the temptation of intellectual dishonesty. Professor Mahalanobis gave examples throughout his paper: the temptation of the field worker to "fudge" his figures, the political pressure resulting in an inaccurate census, the hint of the virtual suppression of an unpalatable report. If Professor Mahalanobis is not absolutely honest in his own work, then he must have practised on us this evening one of the subtlest deceptions of his career. Heat engenders fixed opinions and unreasoned prejudices, and I know how easy it is to hunt for figures to prove a theory rather than to hunt in the figures for the truth.

Then there is the administrative skill and thoroughness with which his investigations are planned. His account of the size of his organization must have made some of us jealous, but he is right to emphasize the factors of time and cost even in his Bengal surveys. Bullock-carts are a slow way of moving about; his field workers must spend much of their time travelling. Then he let fall a mention of work done near Karachi—only some 1,500 miles away. Only by the most careful economy can so much work be done in such conditions by so small a staff.

To give more point to my praise, I may perhaps add that having discarded one of my staff for "negative efficiency" I felt that my section was shaping well. We carried out a small survey on the output of work by coolies, and I designed most carefully all the working sheets and rules for checking. When the figures came up to me, it appeared that the discarded one's successor (specially picked as likely to do well in such work) had not troubled to see that decimal points were in the right place, being of the opinions that (1) I should not notice and (2) that even if I did it would not matter, as the mistakes would average out. Not all people are like that in any part of the world, but it is clear that Professor Mahalanobis has had enough of them to have had to devise techniques for protecting himself against them, and, moreover, has used by-products of these techniques to yield useful statistical information.

Anywhere in the world his achievements would be noteworthy, but as Professor Greenwood admires his work in Combinatory Analysis, so I, who have had some very small experience of the difficulties he has had to face, admire the skill with which administrative troubles unconceived in this country have been overcome.

Mr. D. M. Sen : The speaker has emphasized, and Dr. Yates has reiterated, the difficulties of dealing with recording mistakes in sample surveys in different countries. Dr. Yates has introduced into the discussion the much-abused concept of the "intellectual competence" of the field investigators. I have been associated, for some time now, with large-scale sample surveys in Great Britain. Recording mistakes are not a regional phenomenon, not even in their scale. They are common wherever large-scale surveys are undertaken. It would be totally wrong to try to correlate recording mistakes with "the state of intellectual competence," even if "intellectual competence" could be measured and calibrated, or averaged and graded, for countries as a whole.

To counter recording mistakes, various checks are necessary. I may perhaps be allowed to give some instances from my own firm, which is thought to be one of the largest sample survey organizations in the world. Out of the ten departments which share, between them, the whole work of these sample surveys, beginning from the collection of the data to their presentation to clients, two of the largest departments are charged with the functions of "checking field data" and "insepcting processed data." The Tabulating Department, moreover, applies several checks on its own work (punching etc.), and on Checking Department's work, which has supposedly already checked the data recorded by field investigators. I remember a somewhat irritated fieldman asking the question, "who checks Checking Department's work?" As a matter of fact, the Tabulating Department itself checks Checking Department's work to an extent, as I have mentioned above, but there are other departments too, which are doing exactly the same thing at various stages of processing the data. The Tabulating Department, for instance, uses the "unit" check on the Hollerith to ensure that purchases plus previous stocks equal sales plus present stocks on each card. The cards may have been checked rightly from the original information sheets which were vetted by Checking Department previously; yet it is felt necessary to employ the "unit test." Similarly for "unit costing." The fieldman records purchases in units and the total value. The cost per unit is computed, which serves as a check on the accuracy of fieldmen's records, since this unit purchase price is compared with the unit purchase price of other shops for the same goods or similar goods, and also compared against the "unit cost" price as recorded a month or so previously in the previous period's survey. This cost per unit is used by Tabulating Department on the Hollerith. The units are multiplied by the "unit cost" to arrive at the total purchase price. These totals for different shops are added up on the Hollerith, and their grand total for each item or brand from the Hollerith serves as a check against the sterling total, which the Checking Department has got on their comptometers for the purchases of the same item or brand.

It is therefore clear that within departments themselves there are various checks. There are also many checks between departments. Data are also checked against previous averages, which serves as a rough check, if not a precise one.

In the field itself, besides fieldmen, there are field supervisors, who in their turn are controlled by the field manager. The field supervisors' work consists of no actual work except constant supervision over the fieldmen's thoroughness of collecting data.

Over and above these checks in the field, and during the time when the data are passed through the Tabulator and processed for presentation in the form in which they are wanted by our clients, there is another department—"Inspection Department"—whose job it is to check the purchase, sales, stocks, price and other tables for internal correspondence and plausibility, having regard to known data about market conditions, etc. We often discover at the very last stages mistakes which should have been picked up weeks before. That reflects neither on the field investigators' "intellectual competence" nor on the "intellectual competence" of the checking and supervisory staff within the office itself. These mistakes, we find, are inevitable, and hence they are allowed for. Extra staff are employed to see that they do not go through. There are, of course, cases when deliberate dishonesty is noticeable. I recall an instance where the field investigator, who was supposed to have gone to certain shops and recorded their stocks of certain branded goods, never in fact went there. He filled in the field information sheets at his seaside hotel. In certain cases, where the fieldman detects inaccuracy in purchase data or his stock count, he is supposed to inform the office about it. We in the office know that a percentage of shop data will, over a period, be marked as inaccurate by the fieldmen, and if any one fieldman's audits are extraordinarily accurate, from that very fact alone, we sometimes suspect that the audits are not being conducted as thoroughly as they might have been. This particular fieldman went so far as to advise us from time to time that such and such shops should be thrown out of the sample for that period owing to inaccurate records, etc., and thus our confidence in his work was kept up. He was at last found out by certain discrepancies in his stock position which did not tally with our knowledge of the then existing zoning scheme for the distribution of certain products. There are thus instances of deliberate dishonesty which one can quote.

It is also very easy to question the "intellectual competence" of field investigators. In our work, for instance, present stocks are subtracted from past stocks by fieldmen themselves, so that if present stocks are higher than past stocks, the difference is shown up as a negative quantity. Obviously, this increase in stock, or this negative quantity, must be covered by an equivalent, or higher, purchase figure. It is physically impossible for stocks to increase without equivalent purchases, unless the previous stock-taking is erroneous. Now, one might say that all fieldmen should be intelligent enough to see that these minus quantities are covered by equivalent purchases. In fact, in a very large number of cases honest and very competent fieldmen fail from time to time to account for increase in stocks. Perhaps they would be called "physical fluctuations," but anyhow physical fluctuations are very difficult to explain in this context.

One word about the size of samples. The speaker has indicated that he often takes samples with a sampling fraction of 1 : 4,175, and his largest samples have been of the order of 1 : 168. The samples with which we deal in our particular field of Food and Drug trades, and which we know, from experience over a period of time, yield satisfactory results, are sometimes as large as 1 : 50 and even 1 : 25, but sometimes as small as 1 : 250. I should have thought the size of samples would be related to the variability of the data, among other factors, and consequently the sampling fraction is perhaps not in itself as significant as might be thought.

PROFESSOR MAHALANOBIS made a brief reply at the Meeting, and amplified it later, as follows :—

I am extremely gratified by the kind way in which my talk was received. Unfortunately, the paper could not be circulated nor presented before the meeting in a written form. This naturally left many gaps, to which references were made in the course of the discussion. Relevant material was in certain cases actually with me, but I had no time to discuss it at the meeting.

I greatly appreciate Dr. Yate's emphasis on the importance of close contact between mathematical statisticians and field workers and the need for intimate contact with numerical material. This is a point which I have always striven to emphasize in our Institute. My own view has always been that no one should be considered to have qualified as a statistician without having gone through an apprenticeship as a computer.

I am particularly interested in Dr. Yates's remarks about bias in estimating crop yields. I entirely agree with him in thinking that such troubles can probably be eliminated if a permanent organization of trained workers can be built up. Our problem has been to find out what can be done until such trained workers become available. I may also mention in passing that the size of the cut has been the subject of sharp controversy between the Indian Statistical Institute and the Imperial Council of Agricultural Research of the Government of India. I have been pressing for several years for a scheme to study and compare the differences in technique under the joint supervision of the I.S.I. and I.C.A.R., as this is the only way in which agreed conclusions can be reached. The proposal for joint investigations was strongly supported by Professor R. A. Fisher in his memorandum submitted to the I.C.A.R. in March 1945. Unfortunately, we have not yet succeeded in persuading the I.C.A.R. to agree to this proposal. I spoke to a number of statisticians in the United States a few weeks ago and I hope to take the matter up there again shortly.

Dr. Oswald George as well as Dr. Bartlett have asked what I meant by physical fluctuations. I have already referred to this point in the text. Conceptually, sampling variations may be considered to have arisen from the fact that only a small portion or a fraction (and not the whole) of the universe is being surveyed. Recording mistakes arise from the unconscious bias, or sometimes conscious and deliberate negligence or dishonesty, of the human agency. Even when these two sources of error are eliminated there still remains a certain margin of fluctuations which I have called physical. For example, it would not serve any useful purpose to try to determine the weight of a living human being correct to, say, a milligram. When the administrator demands an estimate for a certain entity which he calls "the acreage under rice in Bengal in a particular year" or "the per capita consumption of cereals, etc.," there is a certain margin of fluctuation which is residual in the dual sense of being something independent of sampling variations and recording mistakes and also of being of no importance for the practical purposes in view. Operationally, the different types of error are mixed up in practice; but it is possible to some extent to separate them by appropriate analysis. I may add that what I have in mind is something like the permissible margin of fluctuations in the quality control sense, where production may be considered to be under control even when the measurements do not strictly conform to statistical randomness. Determination of the limits of physical fluctuations (in relation, of course, to the purpose in view) is difficult and requires a purely experimental approach. We realize its importance, but cannot claim to have made much progress in the actual determination.

Dr. George referred to my use of the phrase "unitary and zonal, configurational," etc. I had given a preliminary account in the paper "On large-scale sample surveys" in *Philosophical Transactions*, and I had included a more up-to-date description of the terminology and notation of sample surveys in my original draft, which I had with me at the meeting, but to which I could not refer for lack of time. This has been included in the present paper.

Dr. George has pointed out the need for small sample theory. I agree, of course, that it might sometimes become necessary to use it. We found, however, that too fine a stratification was often uneconomical in the sense that the additional cost was not commensurate with the gain in precision. In fact, in large-scale surveys, we usually found large sample theory to be adequate for practical purposes. But I agree with Dr. George that in appropriate cases small sample theory would no doubt prove to be of great value. Incidentally, I may mention that one of the important lines of work in our Institute has been the development of the exact or small sample theory of distribution for multivariate analysis by my colleagues R. C. Bose and S. N. Roy.

I am glad to have Dr. George's support about the importance of errors other than sampling error. I also agree, that errors arising through faulty working of machines cannot be neglected. I did not refer to this, simply because there was nothing peculiar in the Indian experience to make it worth mentioning.

Dr. George has referred to my remarks on the defects of complete enumeration. Certain observations which I had no time to make at the meeting will be found in the text of the paper. I may add one or two more remarks. Complete statistics are often obtained almost as a by-product of routine administration—for example, the number of criminal cases sent up for trial. Apart from mistakes in compilation or similar clerical errors I have no doubt that such statistics would be substantially accurate. Consider another example, which is nearer to the subject of the present paper. In temporarily settled provinces of India, crop records are compiled by a permanent, and often hereditary, staff of village recorders (called *patwaries*) primarily for revenue purposes, as the assessment is fixed on the basis of crop production. In such provinces, crop-acreage figures are claimed to be sufficiently accurate for all practical pruposes. I have no personal experience of the quality of the work done by *patwaries*, but it may very well be true that crop records prepared by them are substantially accurate. This is quite different from what happens in a permanently settled province such as Bengal or Bihar. I had in mind the so-called complete enumeration made by an *ad hoc* staff whose number is large (of the order of fifteen or twenty thousand) and who are scattered widely over a province like Bengal. All available evidence points to the fact that work done under such conditions is unreliable. What I had wanted to emphasize was that the fact that complete paper records had been made ready was no guarantee that such records would be reliable. I may perhaps quote one concrete example.

In 1939 the Government of Bengal decided to prepare a complete record, plot by plot, of the land sown with jute. After these records were prepared the Government arranged to have certain portions checked by permanent Government officers. The primary records, when checked, were found so unreliable that the Bengal Government ordered all the records to be destroyed.

I agree with Dr. George who thinks that suitable systems of sample checks and controls can be used with complete enumeration with great advantage. Some of the checks described in this paper may be quite suitably taken over.

Dr. George has referred to the results of the Bihar crop estimates. He is quite right in pointing out that the area under any given crop is not determinate unless the date is specified. I have discussed this point elsewhere.[1] In a province like Bengal no machinery exists for obtaining the crop acreage on a specified date. The estimate obtained by a sample survey spread over a lengthy period must necessarily be somewhat indeterminate. In Bengal, by the alternative method of obtaining an estimate of complete enumeration, the result would be equally indeterminate, for the simple reason that the complete enumeration would have to be made by an *ad hoc* staff whose work would be spread over more or less the same period as that of a sample survey.

A little consideration would, however, show that although a specified date makes the crop acreage determinate it renders the estimate of total production necessarily somewhat vague. Whatever may be the crop acreage on a specified date it is likely that the harvested area would be somewhat different. As already mentioned, such differences are likely to be quite appreciable in India, especially in the case of rain-fed crops. In this situation we thought it advisable to develop a method for ascertaining the total production by a direct estimation of production per grid. This seems to be as far as one can go in existing conditions in Bengal.

I may mention here one particular procedure with which we have been experimenting for some time. At the time of harvesting, the yield per acre is determined by crop-cutting experiments in a number of grids. The total cropped area in each grid is estimated at the same time, and in this way the total yield for each such grid is ascertained. This enables a direct estimate to be made of the total production in each cell, zone or district, or in the province as a whole. All lands which go out of cultivation would not in this method contribute anything to the estimate.

In showing the progressive estimates for the *ad hoc* crop survey my object was to show the kind of data supplied to administrators. The sampling at each stage was not individually or independently representative of the whole "universe" as spread over space and time. Suitable modifications in the theory of estimation to cover such cases have not been worked out. Different progressive estimates necessarily referred to different regions of the province at different times, and were not strictly comparable. Differences were due to sampling variations and partly to actual physical differences in crop acreage arising from the different geographical coverages at different periods of the survey. My real point was that, in spite of such intrinsic differences, the figures supplied to administrators were probably quite useful for administrative purposes.

[1] Presidential Address to the Section of Mathematics and Statistics, *Indian Science Congress*, Baroda 1942, p. 25.

Dr. George has enquired about the proportional sampling errors. These, naturally, were large when the total acreage or the proportion of land under a particular crop was small. I have discussed this point in greater detail for the Bengal crop surveys, for which material was available for a number of years. The Bihar crop survey was, unfortunately, terminated after one year and sampling errors had not been fully investigated. I hope, however, to give an account of the effective margin of error in crop surveys in a subsequent paper.

I am glad Dr. Bartlett has referred to what I called space correlation functions, which I mentioned in the paper in *Phil. Trans.*, although I had no time to speak on this topic at the meeting. Since that paper was written certain other concepts have been developed; and, as already mentioned, some theoretical work has been done by R. C. Bose. The subject appears to be of considerable interest; but, unfortunately, owing to heavy pressure of other work, it has not yet been possible to tackle it on fundamental lines.

It may be useful, however, if I add a few brief remarks. Consider a field of, say, square or rectangular shape consisting of a very large number of rows and columns of elementary cells or quads. Along any single row (or column), the problem of space correlation would be one-dimensional, and identical with that of serial correlation in time series. The fact that space distributions are intrinsically two-dimensional, however, makes it necessary to have more generalized concepts. The approach at present adopted in the Statistical Laboratory is as follows.

Consider a single row or a single column consisting of a very large number of cells. (The boundary difficulty can be avoided by making the row or column endless by joining the two terminal cells.) Imagine any particular row or column divided into a number of segments, each segment itself still consisting of a very large number of individual cells or quads. The correlogram or serial correlation for different gaps can be then determined for each segment. The form of the correlogram or serial correlation for different gaps can then be determined for each segment. The form of the correlogram or the serial correlation function may then differ from segment to segment or may happen to be statistically the same for different segments. The serial correlation is usually called stationary if it happens to be statistically the same for different segments in whatever way the segments are formed (subject, of course, to the provision that each segment continues to have a sufficiently large number of quads to enable the serial correlation to be determined with the required precision).

Now consider the adjoining (or some other) row. The correlation function or serial correlation in this row may again be stationary, but may differ from the stationary serial correlation in the preceding row. Thus, although the serial correlation in each row is stationary, they may be all different. In a simplified case, the serial correlation along each row may be stationary and may be the same for all rows. Such a field may be called "uniform along rows."

Now consider the columns. The serial correlation along each or some of the rows differ from segment to segment. If they happen to be the same over all segments in any particular column, then the serial correlation is stationary over that column. In the same way, the serial correlations may be stationary for each column and finally may be the same for all columns, in which case the field would be "uniform along columns."

It is possible, however, to consider the serial correlation not merely along rows or columns but in different directions, making different angles with the direction of, say, rows. If the field is divided up into two-dimensional segments (each segment still consisting of a sufficiently large number of elements or quads) it is possible to determine the serial correlation in different directions. In this way it is possible to construct what may be called the surface of the serial or space correlation in such a way that a section of the surface in any particular direction supplies the corresponding curve of the serial correlation. If such surfaces of the correlation function happen to be the same for all segments, then the field may be called homogeneous. It might not, however, be isotropic. In fact, in each segment the field would be isotropic only if the correlation function is identical in all directions or, in other words, the surface of the correlation function is a surface of revolution.

The interesting fact then emerges that the surface may be isotropic for each segment, but the actual surface may be different in different segments, in which case the field would not be homogeneous, but would be isotropic. It is obvious that the field may be homogeneous, but not isotropic. Finally, when the field is both homogeneous and isotropic it may be called uniform.

Instead of working with the correlation function it is also possible to work with the variance function in the same way. As the two functions are mathematically connected the results based on them would be equivalent. For practical purposes the variance function would often be more convenient, as it usually enables the sampling errors to be calculated more directly.

I may add that the study of the surface of the correlation or variance function is of interest in connection with what I have called mapping problems, in which it is desired to ascertain the actual distribution of the variate under study over different elements or regions of the field under survey.

In the paper in *Phil. Trans.* I explained that fields may be of non-periodic, quasi-periodic or periodic types. I also explained the use of the patch number in characterizing the different types of fields. From the point of view of patch numbers, fields may be broadly considered as belonging to three types, namely, (*a*) fields in which the patch number is very small, corresponding to what I called "patterned" fields; (*b*) fields with patch numbers lying within an assigned range on either side of the model or expectation value, which I called fields of the random type, and finally; (*c*) fields in which the patch number was larger than the limit assigned for random fields, which I called the crystalline type and which would correspond to quasi-periodic and periodic types from the point of view of correlation function.

It is easy to see that previous knowledge of the type of the field can help in improving the efficiency of the sampling design, especially in the case of mapping surveys. For example, if the field is of a random type then a purely random sampling would be appropriate. If the field is however of quasi-periodic, or periodic, or what I have called of the crystalline type, then cluster or configurational sampling must prove quite useful. On the other hand, if the field is known to be of the patterned type then it should be possible to improve the efficiency by suitable zoning and stratification.

I am particularly grateful to Major Elphinstone for his kind remarks. I entirely agree with him in thinking that the apathy of administrators and the peculiar difficulties in which statistical work has to be carried out in India has to be experienced in order to be properly appreciated. The average administrator in India expects the scientific or statistical technician to supply evidence or proof in favour of what the administrator thinks to be right, rather than to give independent advice on objective grounds. Intellectual dishonesty, to which Major Elphinstone has referred, would in such circumstances be an actual advantage in securing promotion in official posts. This is why I have never favoured the idea of the Statistical Institute being run as a Government department or under predominating Government control. I think it is of the utmost importance that scientific organizations like the Statistical Institute should maintain an independent position, so as to be able to offer impartial technical service and advice on an objective basis. Mr. D. M. Sen has referred to the size of samples. I am afraid I had not made the position quite clear in the paper. It is not correct to say that we have used a sampling fraction of 1 : 4175. This was the fraction of the sample (in Table 1) on which we supplied a purely provisional first estimate. In the sample survey of crop acreage we are at present using a sampling fraction of the order of about 1 : 150 or 1 : 200. In estimating the yield of crops per acre the sampling fraction is necessarily far smaller and of the order of 1 : 6,000,000. On the other hand, in small-scale work one may go upto such large fractions as 1: 20. Mr. Sen is entirely right in thinking that the size of samples would depend on the variability of the material. Naturally, it also depends on the precision it is desired to attain in the final results, and on permissible budget limits.