EXPERIMENTS IN CULTIVATORS' FIELDS

By V. G. Panse and P. V. Sukhatme
I.C.A.R., New Delhi and F.A.O., Rome

1. Introduction

In two previous papers entitled "Crop Surveys in India" (1948, 1951) we have described the role of sampling technique for estimating statistics of acreage and yields. Though basic, these statistics are by no means adequate to serve the purpose of policy makers in establishing national agricultural plans for increasing production. To be able to do the latter, one needs to know the responses under actual farming conditions to the different improvement measures such as fertilizers, irrigation, improved varieties and so on. In the absence of information on responses under actual farming conditions the results obtained at experimental stations provide the only guide to work out the plans. It is to be remembered however that the number of experimental stations in a country is usually small and further the fertility of the soil and the level of management at experimental farms are superior to those in cultivators' fields. Any generalization of the conclusions obtained at experimental stations for application over the whole country is therefore attended with risk. Under the circumstances it is important to determine the responses to different improvement measures under the actual farming conditions by experimenting on cultivators' farms selected in accordance with the principles of random sampling. Since the method combines the use of survey and experimental techniques it is known as the survey method of experimentation. The planning and conduct of such experiments, however, presuppose the availability of certain minimum facilities and technical skill which are not available on the cultivators' fields. It is the object of this paper to discuss the difficulties involved in experimentation of this type and present their solutions.

2. Practical Difficulties

The idea of locating a group of experiments in a representative sample of cultivators' fields has not been put into practice on an extensive scale except in a few countries. On account of the apparent difficulties and the heavy cost involved in organizing an experimental programme of this type, the approach has been generally regarded as impracticable. These difficulties in general are the limited experimental facilities in the countryside and the apathetic, if not antagonistic, attitude towards experimentation on the part of the cultivator apart, of course, from the relative inaccessibility of many of the fields selected for the experimental program. An average cultivator is a poor man working on a small, usually unfenced, area of land and is preoccupied with his daily routine. He can therefore hardly be expected to divert any of his limited resources to experimental work which might disturb the normal operations on his field or in which there is a risk of incurring losses. A correct psychological approach to win his confidence and gain his co-operation thus becomes the first step before initiating a successful experimental programme of this type. For this purpose it is necessary to ensure that the design of the experiment is simple enough to be conducted within the limited resources available on a cultivator's field and such that it can easily be fitted into the normal routine of his work. Secondly utmost care is called for in choosing the treatments for experimentation so that he may not incur loss through the granting of facilities for trying these treatments on his field. As will be shown in the succeeding sections, it is possible to design very simple experiments under the cultivator's conditions which can fit into his time-table of operations on the field and yet collectively supply the information required. Similarly, with well-chosen agronomic treatments such as manures and fertilizers under conditions of an assured moisture supply, or with promising varieties of crops, the probability of loss can be reduced to the minimum. The manures, fertilizers or seed required for the experimental treatments would be supplied free. Payment would be made to the cultivator for any labour assistance which he may provide for purposes of the experiment. In any event it should not be difficult to guarantee the cultivator against loss as measured by the yield obtained with the control treatment which in these experiments would be the cultivator's normal practice. It will thus be seen that the practical difficulties referred to above are in no way insuperable and can and must be overcome in the interests of the need for obtaining experimental results under a truly representative set of farming conditions. Experience has, in fact, shown that where experimental programmes on these lines have been taken up, the cultivators have soon come forward to pay the cost of experimentation.

3. Need for Using the Principle of Random Sampling in Selecting Sites

It is well known that the method of random sampling can give a representative sample of fields and that it is only from experiments on such fields that results of general applicability and of measurable precision can be derived. Notwithstanding these advantages, non-random methods of selection based upon the personal judgment of experimenters are not infrequently used for selecting representative sites. It should however be emphasized that the advantages of deliberate selection are more apparent than real. Experience has shown that the use of non-random methods, even in the hands of experts, cannot be relied upon to give a sample representative of the population and consequently estimates of response obtained from experiments in such fields are liable to be seriously biased. Even if quotas are set up to represent the different categories like soil types, the ultimate selection of actual fields within each category is influenced by the personal judgment of the experimentor and the result is therefore likely to be biased. It is of course true that such methods are convenient to use in practice. Their cost is also low relative to that of the method of random sampling. However unlike random sampling, these methods lack the means for judging the precision of the response obtained. Since an important primary object of experimentation on cultivators' fields is to estimate the average response to a given agricultural improvement measure over the tract and to test the consistency of this response in different parts of the tract, the method of random sampling should be used in locating fields for experiments.

It is contended that the selection of experimental sites should not be made from all the fields in the tract but rather from say, a given soil type or a climatic or agriculturally homogeneous zone. If a sufficiently detailed soil map is available and if the experimental treatments are such as warrant their trial only on a specified soil type, there is nothing to prevent using the method of random sampling in selecting fields out of the given soil type. Where, however, detailed sampling frames like a soil map are not available, it is convenient to select from the totality of fields in the tract and then assign the selected fields for experimentation by the desired types or zones or regroup the results in any desired manner. The principle of random sampling is just as valid whether one selects sites from a population of fields within a given geographic region or from any given soil climatic or agricultural zone.

Another possible objection to the use of the method of random sampling for the selection of experimental sites is that a particular experiment may be located on a site where a manurial treatment may be ineffective because of the operation of some limiting factor such as salinity. It must be remembered, however, that such factors form part of the conditions which affect the average yield of the tract and it is therefore necessary that the experimental treatments should be tested under all conditions obtaining in the tract. There is however nothing to prevent the experimenter from defining the population of fields in advance in a way considered most suitable for trying out the given experimental treatments. Thus in experimenting with manurial treatments one might confine the selection to fields receiving irrigation. Or again, in experimenting with a new promising variety of a crop one might well have to experiment only in those areas where the growing season is long, if the variety to be tried is a late maturing one. Whatever be the population, whether it is the totality of fields from the tract or fields belonging to any given type within it, the experimental sites to represent the population should be selected using the principle of random sampling.

One objection of any substance to the use of the principle of random sampling arises from the limitations of communication. Thus, fields may be inaccessible during the rainy season, making transport of manure, fertilizer, seed, etc., difficult. Deviations from the principle of random sampling under such conditions may in extreme cases be inevitable, but even here the principle of random sampling can be approximated by sub-sampling randomly a small predetermined number of fields out of the initially selected fields in the sample whose omission appears unavoidable and making a determined effort to experiment thereon. Provided the omissions in the sub-sample are few, they will not seriously affect the validity of the results.

4. The Design of Experiment

Apart from the choice of treatments which we have seen must be few in number and promising in their results, the design of an experiment on cultivators' fields must be extremely simple and possess demonstration value if we are to win the co-operation of the cultivator in any experimental program of this type. The simplest of experimental designs is the randomized blocks design. But even a randomized blocks design with its replications involving numerous small plots lying side by side in the field cannot fulfil the requirement mentioned in section 2, namely, that of enabling the cultivator to carry out his normal field operations undisturbed. A design which might appeal to a cultivator would be the one in which replication is eliminated altogether, *e.g.*, we may divide his field into as many portions as there are treatments, apply the treatment over the whole of each of these portions and harvest plots of given dimensions at harvest time in the presence of the experimenter. Thus, with an experiment with five treatments, a field would be divided into five approximately equal portions; in one portion the crop would be grown according to the cultivator's normal practice and this would be the control treatment for purposes of experiment. In the other four portions of the field the experimental dressings would be superimposed on the cultivator's normal practice, namely, the control. If the field is too large a suitable section thereof, restricted to an upper limit of, say, one acre, would be selected for experimentation on these lines. In brief, the idea underlying this design would be that the whole field would be cultivated, seeded, etc., by the cultivator in his usual way, but four suitable portions, neither of which would exceed a given area, would have experimental treatments superimposed on the normal. The procedure however presents a practical difficulty. This arises from the fact that the field staff are required to measure the areas of the different portions in order to determine the precise quantities of the treatments to be applied to each portion. It should not however be difficult to train the field staff in the measurement of the areas of the portions into which a field may be divided for purposes of experimentation. The alternative solution is to standardize the size of the portions, say 1/10 acre each, and arrange them in one compact block in the midst of the field of the cultivator. This arrangement could have the advantage of being economical in that a smaller amount of manure, fertilizer, seed, etc., would be required for use as experimental treatments. But the greater disadvantage is that the cultivator would no longer be able to carry out his normal field operations undisturbed. The arrangement might therefore result in a loss of experiments. Where however the cultivator is co-operative and prepared to put up with some inconvenience to his normal operations entailed by this arrangement, this design has been tried with success; but in either case, the procedure is open to obvious objections on statistical grounds.

In the first place there is no replication. This objection can however be met by repeating the experiment on another field. In other words, fields rather than compact blocks within fields would constitute the replication for the experiment. The second objection is that the procedure does not allow an effective use to be made of the principle of local control in eliminating fertility variation from treatment comparisons. In an experimental program involving a number of experiments spread over a large tract, this is an unimportant factor, for the main object of the experimental programme is to estimate the average response of the various treatments for the tract as a whole and not for any specified field. The accuracy of a single experiment thus plays only a secondary role in the whole scheme. That the experimental plots for harvest are not contiguous or of a shape considered advantageous in a field experiment at a research station is thus altogether unimportant owing to the small contribution of these factors to the variation of response compared to the contribution of variation from other sources, namely, the variation between experiments. Even then, the fact that all treatments are grouped together in the same field provides a degree of local control. In regard to randomization it is important that the treatments should be allotted at random to the different portions or plots within a field. Further, the randomization should be carried out independently on different fields. It is sometimes considered that one random arrangement of treatments is satisfactory for all fields. This is a mistaken notion. Looked at from this angle it can be seen that the procedure of spreading the experiment over several fields dividing each into as many portions as there are treatments to be tried subject to certain upper limits for the field and for portions thereof, allocating the treatments to these portions in a random order and harvesting and weighing the produce from plots of the requisite dimensions marked in a random position in each portion at harvest time, not only would enable the cultivator to proceed with his normal operations undisturbed but at the same time would satisfy the basic principles of randomization, replication and local control of an experimental design.

The design described above pre-supposes that the fields are large and divisible into portions of say 1/10 acre each. For certain crops and in certain regions however fields are likely to be small. Thus in terraced regions, paddy fields are long, narrow and small. Under these conditions a field itself might be the unit of treatment, 4 or 5 adjacent fields constituting the different portions of the experiment. A cluster of adjacent fields forms in this case the block. The arrangement has one distinct advantage over the arrangement of dividing a single field into different portions, in that it is no longer necessary to put up bunds between different portions of a field. On the other hand, the principle of local control is now less effective.

5. The Number of Experimental Fields and its Distribution between and within Places

Fields for experiments will ordinarily be selected in two stages of sampling—places (usually villages) in the first stage and fields within the selected places in the second stage. The cost of repeating an experiment in one more field in the same place will obviously be smaller than that of locating it in a field in another randomly selected place. Likewise, the variation of the treatment response within a place will ordinarily be smaller than that between places. On the first of these two considerations, the total cost of experimentation in a given number of fields would decrease as the number of fields per place is ncreased. The second consideration would pull in the opposite direction, the variance of the treatment response increasing with the number of fields in a place at the cost of the number of places. The aim of an experimenter should clearly be to so determine the number of experimental fields and its distribution between and within places that the treatment response over the tract is estimated with the minimum variance for a given budget, or alternatively, with the desired precision at a minimum cost. It is therefore necessary as a first step, to know the relationships (1) between the total cost of experimentation and the number of places and fields per place and (2) between the variance of the estimated response and the number of places and fields per place.

It can be seen that the analysis of variance of plot yields of a group of experiments at n places with m fields (replications) per place takes the form shown in Table I.

TABLE I

Analysis of variance of plot yields from group of experiments

Source	D.F.	M.S.	Estimate
Treatments ..	$t-1$	..	
Places ..	$n-1$	..	
Fields within places, *i.e.*, blocks ..	$n(m-1)$	..	
Places × Treatments ..	$(n-1)(t-1)$	$s^2{}_b$	$\sigma_e^2 + m\sigma_m^2$
Blocks × Treatments ..	$n(m-1)(t-1)$	$s^2{}_{ic}$	σ_e^2

Further the error variance per plot of a treatment response will consist of two parts: (1) the error variance per plot at a place σ_e^2 and (2) the variance due to interaction of response with places σ_m^2, the variance of the average treatment response estimated from nm experiments spread over n places with m replications each, being given by

$$V = 2\,\frac{(m\sigma_m^2 + \sigma_e^2)}{nm} \qquad (1)$$

This expression determines the relationship between the precision of the estimated response and the number of places and replications per place. Likewise, the total cost of experimentation can also be considered as made up of two components: (1) the cost of setting up an experimental place or centre, and (2) the cost of operations in the conduct of experiments. The cost of setting up an experimental place includes the cost of salary of the experimenter for the days of his visit to the place and the cost of his journey including transport of equipment. The total cost of experimentation can therefore be expressed as

$$C = c_1 n + c_2 nm \qquad (2)$$

c_1 representing the cost of setting up a place and c_2 the cost of conducting the experiment in a single field.

The problem of determining the number of experimental places n and the number of replications per place m is thus the problem of minimising the total cost for a prescribed value of the variance, V_0, with which the response to a given treatment is sought to be estimated. It can be shown that these values known as the optimum values of n and m, are given by

$$\hat{m} = \sqrt{\frac{c_1}{c_2}} \cdot \frac{\sigma_e}{\sigma_m} \qquad (3)$$

and hence

$$\hat{n} = 2\,\frac{\left(\sigma_m^2 + \dfrac{\sigma_e^2}{m}\right)}{V_0}$$

Experimental surveys on a pilot scale should provide the data to evaluate n and m. The value of the ratio σ_ϵ/σ_m will depend on the relative magnitude of the true variance of response to treatment between places and between fields within places. As far as the other component $\sqrt{c_1/c_2}$ of formula (3) is concerned, c_1 will, of course, be larger than c_2, but the exact value of the ratio would depend upon the local conditions, the rates of pay, the cost of labour and the number of treatment plots. To illustrate the application of the formulæ where district agricultural staff is conducting the experiments each within his jurisdiction and is paid on a monthly basis, his salary and travelling allowance, we might take $c_1/c_2 = 4$. What little data on experimental surveys are available indicate that the ratio of true variances between places to that within places is as 1 to 2. The optimum value of the number of replications (fields) per place can thus be taken to be 3. Substituting $m = 3$ in formula (3) we should arrive at the number of places to be selected for experimentation. Should treatment effects vary more widely from place to place, σ_m would be comparable or even larger than σ_e and the experimental plan might take the form of a larger number of places with fewer replications per place, a field always accommodating one replication and no more, unless conditions warrant otherwise. The minimum replication in that case could be one per place but this would deprive the experimenter of the opportunity to assess the variance within places for use in planning further investigations. Two replications per place would, therefore, seem advisible as the minimum.

In the analysis presented here it has been assumed that σ_e is constant and independent of the number of treatments. Actually the value of σ_e will vary with the number of treatments. In extending the results to planning of experiments with different numbers of treatments, therefore, care would be necessary to use the appropriate values for σ_e and σ_m in the formulæ for the number and distribution of replications between and within places. Any difference resulting from a change in the value of σ_e would however be ordinarily small and the result obtained on one set of treatments can be taken to serve as a rough guide to the number of replications and its distribution in order to plan similar experimental programme with the maximum statistical efficiency.

All through the discussion in this and the previous sections we have assumed that the tract would first be divided into homogeneous agricultural zones. Even within the agricultural zones the experiments may have to be confined to given soil and climatic types so that the cropping system is more or less uniform over the zone. Again, it is not sufficient to study the interaction of response with regions. It is equally important to study the interaction of response with seasons. In fact, no results can be recommended for adoption in practice unless the experimental programme in cultivators fields is continued in the same region for three or more years. This of course does not imply that the same fields and plots should continue to be experimented with year after year. On the contrary, fields for experiments should be chosen afresh each year. If it is desired to estimate residual effects this could be done by taking observations on a pre-selected fraction of the total number of fields of the previous year.

6. Choice of Treatments

The choice of treatments is governed by three considerations: (1) they should be promising in results as judged by past research at experimental stations, (2) they should be small in number so as to avoid laying out more than 5 or 6 plots per field, and (3) they should form a self-contained set in the sense that easily intelligible comparisons of direct practical value can be made from the set of treatments adopted. A very common objective in manurial trials is the comparison of levels of nitrogen and phosphate, singly and in combination. Now if we take three levels of each of these components, no application of manure being one of the three, it would require 9 plots to lay out a factorial experiment with these two factors. Such a trial cannot possibly be carried out in cultivators' fields on any extensive scale. The treatments to be tried may therefore be divided into sets as follows:—

Group A: Set (i) $0, n_1, n_2$

(ii) $0, p_1, p_1n_1, p_1n_2$

(iii) $0, p_2, p_2n_1, p_2n_2$

Group B: Set (i) $0, p_1, p_2$

(ii) $0, n_1, n_1p_1, n_1p_2$

(iii) $0, n_2, n_2p_1, n_2p_2$

where n and p stand for nitrogen and phosphate and the numerical suffixes attached to the symbols indicate the level of nitrogen and phosphate at which the fertilizer is to be applied.

It will be seen that each group enables the experimenter to get information on the interaction NP and response to N or P as compared to the cultivator's normal practice, response to P being confounded in group A and response to N in group B. Depending on whether the experimenter is primarily interested in comparisons of N or P, the sets of treatments given in the first or the second group may be tried at each place, one-third the total number of experiments being devoted to each set of treatments in either case. If, however, information is required on the response of both N and P both groups of treatments may be adopted, each set being allotted to one-sixth the total number of experiments. An obvious advantage of the suggested arrangement is that it does not call for trial of more than four treatments at a time while permitting all comparisons of interest to be made.

Keeping down the number of treatments to be tried in an experiment to 3 or 4 is of particular importance when the field work is to be managed by relatively unskilled staff and when the co-operation of the cultivator has to be enlisted for the first time in organising an experimental programme in his fields. When, however, trained and skilled staff is available for this type of work and there is adequate provision of supervision and the cultivator is appreciative of the value of such experiments more ambitious experimental programmes can be undertaken. Even then it is usually necessary to confine the number of treatments to be tried to 5 or 6 and it calls for the exercise of considerable skill in the choice of treatments so as to provide comparisons of interest with this number of treatments. As an example we shall give illustrative sets of treatments from a programme of fertilizer research in progress in India, sponsored jointly by the Government of India and the U.S. Technical Co-operation Mission. The objective is to compare different types and levels of nitrogenous and phosphatic fertilizers. Three nitrogenous and four phosphatic fertilizers, namely, n (ammonium sulphate), n' (ammonium nitrate), n'' (urea), p (superphosphate), p' (nitrophos), p'' (ammonium phosphate) and p''' (bone meal) are included for trial. The suggested sets are as follows:—

I. *Comparison of levels and types of nitrogen*

(*a*) On soils not expected to respond to phosphatic manuring, the following sets of five treatments are to be tried, each in one-third of the total number of experiments in this category:

(i) $0, n_1, n_2, n'_1, n'_2$

(ii) $0, n_1, n_2, n''_1, n''_2$

(iii) $0, n_1', n'_2, n''_1, n''_2$

Nitrogen at 20 and 40 lb. per acre.

(*b*) On soils expected to respond to phosphate, the following sets of six treatments are to be tried each in one-third of the number of experiments of this type:

(i) $0, p, n_1p, n_2p, n'_1p, n'_2p$

(ii) $0, p, n_1p, n_2p, n''_1p, n''_2p$

(iii) $0, p, n'_1p, n'_2p, n''_1p, n''_2p$

P_2O_5 at 20 lb. per acre.

II. *Comparison of type of nitrogen and effect of phosphate*

$0, n, np, n'p, n''p$

Nitrogen and P_2O_5 each at 20 lb. per acre.

III. *Comparison of types of phosphate and effects of nitrogen (for unirrigated areas)*

$0, n, np, np', np'', np'''$

On non-acid soils, the np''' treatment would be omitted.

On all plots except the untreated plot, nitrogen is to be made up to a total of 20 lb. per acre by the addition of sulphate of ammonia.

IV. *Comparison of types and levels of phosphates and effect of nitrogen (irrigated areas)*

The following sets of six treatments, each on one-sixth of the number of experiments of this type is to be tried:

(i) 0, n, np_1, np_2, np'_1, np'_2

(ii) 0, n, np_1, np_2, np''_1, np''_2

(iii) 0, n, np'_1, np'_2, np''_1, np''_2

(iv) 0, n, np_1, np_2, np'''_1, np'''_2

(v) 0, n, np'_1, np'_2, np'''_1, np'''_2

(vi) 0, n, np''_1, np''_2, np'''_1, np'''_2

P_2O_5 at 20 and 40 lb. per acre.

On all plots except the untreated plot, nitrogen should be made up to a total of 40 lb. by the addition of sulphate of ammonia. Sets (iv), (v) and (vi) are to be tried only on acid soils.

The grouping illustrated by these sets has been made possible by the judicious use of the information derived from past research, namely, that crops respond to nitrogen in some easily available form everywhere in the country whereas response to phosphate is limited to certain areas, while response to potash is generally absent. The main objective of these experiments is consequently the estimation of response to different forms and levels of nitrogenous fertilizers. Further in phosphate-responsive areas the experiments provide for information on the response to nitrogen when supplemented with phosphate. Different forms and levels of phosphate are therefore proposed for trial in conjunction with nitrogen. Treatments with forms and levels of phosphate alone are not included as the economic interest centres on the basis of past research on the effect of phosphate in the presence of nitrogen rather than supplied by itself. Finally, it should be noted that the sets of treatments given above are not the only sets possible for realising the objectives and alternative sets can be thought of. The general principle to bear in mind in planning an experimental program of this type is that the objectives should be defined clearly so that these can be realised by the experimental treatments proposed within the practical limitations imposed by the experimental design.

7. Auxiliary Observations

An experimental program on the above lines would provide information on the mean responses for the tract and their interaction with the different subdivisions of the tract. If interaction is absent, the results would usually give adequate information for making recommendations applicable to the entire tract. If interaction is discovered, it would be necessary to make specific recommendations for different parts of the tract. Such recommendations would be strengthened by investigating causes responsible for the variations of response. This can be done by collecting suitable ancillary data on the soil and other environmental factors of the experimental fields for correlation with the experimental results. Thus, observations on the topography and soil characters with particular reference to depth of surface soil, depth to which roots are observed to penetrate, colour, texture, permeability, drainage of substratum and sub-soil water table would be of value. A laboratory analysis of soil samples taken from the experimental field would provide useful data. Details about the conduct of the experiment such as the date and method of sowing, seed rate, variety, whether the crop was irrigated and if so the amount of irrigation given, etc., should of course be recorded. Data on rainfall are important particularly for experiments on rainfed crops. Observations on the growth of crops and damage by diseases and pests should be also taken.

8. The Analysis of Data

No new principles are involved in the analysis and interpretation of data from experiments on cultivators' fields except that we should add that even if the treatments differ in different sets of experiments, results for any particular comparison of interest can be consolidated from all experiments in which this comparison is possible. In this section we shall consider an example to illustrate the analysis of data obtained from such experiments.

The example refers to the data obtained from experiments carried out as part of a wider programme initiated in Tanjore District of Madras State for testing under cultivators' conditions treatments found promising at experimental stations. The data considered here relate to the value to the paddy crop of ammonium sulphate alone and in combination with superphosphate. The district was divided into four agriculturally homogeneous zones and in each zone a certain number of villages were selected randomly. In each village one field was selected randomly and this field was divided into three approximately equal parts. To one part was allotted ammonium sulphate (25 lb. nitrogen per acre), to the second ammonium sulphate (25 lb. nitrogen per acre) in combination with superphosphate (40 lb. P_2O_5 per acre) while the third was left untreated to serve as control representing the cultivator's own practice. For recording the yield of each treatment a plot measuring 7·26 cents was located in a random position and its produce harvested. In all there were thirty-eight villages in which the experiment was carried out. Table II gives the yield of paddy for each of the three treatments in each village and for each zone.

Comparing the zonal mean values, interesting differences are observed. In the first place the general yield level is distinctly higher in the fourth zone than in the other three. Secondly, the first three zones have reacted differently to the treatment than the fourth, ammonium sulphate producing a distinctly larger response in the first three compared to the fourth, and phosphate (applied in the presence of ammonium sulphate) produced a response only in the fourth as compared to the first three. This is brought out clearly in the analyses of variance for the individual zones given in Table III. It will be seen that there are no replications within villages but such replication is not essential since for our present purpose the various villages within each zone constitute the replications and the groups of replications in the four zones can be regarded as four replicated trials. Splitting the two degrees of freedom into one for control *versus* fertilizer (T_1) and the other for ammonium sulphate alone *versus* ammonium sulphate *plus* superphosphate (T_2), we see that the first has a significant mean square in the first three zones and not in the fourth, while the second has a significant mean square only in the fourth.

In considering the possibility of carrying out a pooled analysis of variance, we notice that the error mean squares in individual zones (Table III) vary appreciably; but applying Bartlett's test we get

$$\chi^2 = 4\cdot567 \text{ with 3 d.f.}$$

This is a non-significant value and there is no justification for assuming the errors to be heterogeneous. We can, therefore, carry out a simple analysis of variance of the entire data given in Table II. This analysis is shown in Table IV. In view of the indication that the zones differ in their average yield and in the relative response to nitrogen

TABLE II

Experiments on cultivators' fields in Tanjore District of Madras State

Yield of dry paddy in lb. from plots of 7·26 cents area

	No. of village	Control	Am. Sulphate	Am. Sulph. + Superphos	Zonal Mean
Zone 1 ..	1	90	116	171	
	2	146	171	157	
	3	189	196	186	
	4	222	287	222	
	5	168	166	185	
	6	131	134	142	
	7	185	201	263	
	8	131	118	160	
	9	58	89	98	
	Mean ..	146·7	164·2	176·0	162·3
Zone 2 ..	10	170	227	183	
	11	218	244	196	
	12	114	154	186	
	13	138	114	126	
	14	104	150	126	
	15	180	215	225	
	16	162	197	214	
	17	152	193	213	
	18	136	153	159	
	19	191	204	210	
	Mean ..	156·5	185·1	183·8	175·1
Zone 3 ..	20	137	155	170	
	21	288	293	296	
	22	167	171	183	
	23	122	129	138	
	24	101	136	144	
	25	218	240	273	
	26	204	208	205	
	27	144	167	104	
	28	187	217	258	
	29	91	128	136	
	30	142	169	189	
	Mean ..	163·7	183·0	190·5	179·1
Zone 4 ..	31	181	198	210	
	32	202	204	206	
	33	249	263	277	
	34	216	230	268	
	35	186	154	171	
	36	203	216	230	
	37	188	176	200	
	38	194	203	196	
	Mean ..	202·4	205·5	219·8	209·2

TABLE III

Analysis of variance for individual zones (lb./plot)

Source	Zone 1 D.F.	Zone 1 M.S.	Zone 2 D.F.	Zone 2 M.S.	Zone 3 D.F.	Zone 3 M.S.	Zone 4 D.F.	Zone 4 M.S.
Between villages ..	8	7382**	9	3449**	10	9225**	7	2597**
Control *vs.* fertilizer (T_1) .	1	3298*	1	5207*	1	3895*	1	560
Amm. Sulphate *vs.* Amm. sulph. plus super (T_2)	1	624	1	8	1	313	1	812
Error ..	16	510	18	367	20	303	14	151

TABLE IV

Analysis of variance pooled over all zones

Source	D.F.	M.S.
Between first three zones (Z_1) ..	2	2224
Av. of first three zones (Z_2) *vs.* fourth zone ..	1	25208*
Between villages within zones ..	34	5898**
Control *vs.* fertilizer (T_1) ..	1	11970**
Amm. Sulph. *vs.* Amm. Sulph. *plus* Super (T_2) ..	1	1107
Interaction (Z_1) × (T_1) ..	2	50
Interaction (Z_1) × (T_2) ..	2	214
Interaction (Z_2) × (T_1) ..	1	891
Interaction (Z_2) × (T_2) ..	1	222
Error ..	68	337

* significant at 5% level ** –do– at 1% level

and to a supplement of phosphate, the zones are divided into two groups, the first three together and the last, Z_1 indicating the differences within the first set and Z_2 the difference between this group and the last zone in the analysis of variance. The interactions of these groups with the two degrees of freedom for treatments, T_1 and T_2, are also shown separately.

The results are somewhat unexpected in that none of the interaction components is significant, although the mean square for the interaction Z_2T_1 is appreciably higher than the pooled error mean square. From the analysis of individual zones, we should have expected this mean square to be significant. The interaction Z_2T_2 should also have shown significance. Apparently we are here dealing with a borderline case in which the responses themselves are moderate and their interactions with the subdivisions of the tract are consequently not sufficiently marked to be demonstrated consistently. Nevertheless, we must not overlook the inferences derived from the analysis of results for the individual zones in planning future experimental work and in drawing conclusions for practical recommendations to be made to cultivators. This inference is that unlike in the first three zones a supplement of phosphate is necessary to the nitrogenous fertilizer in the fourth zone. This indication can be verified by further experimentation in the zone by adopting sets of treatments similar to those in group A mentioned in Section 6.

According to the present combined analysis of variance, the standard errors of the treatment responses can be based on the mean square pooled from the 68 degrees of freedom for interaction between zones and treatments from Table IV, in view of the lack of significance of any of the interaction components.

SUMMARY

The importance of fertilizer and other experiments under actual farming conditions by laying out these trials in a randomly selected sample of cultivators' fields is being increasingly realized and the present position of the problem in India is summarised in this paper. The practical difficulties of carrying out these trials have been discussed and possible solutions suggested. Suitable designs for these trials including appropriate sets of treatments have been discussed. The question of optimum number of experiments has been considered by taking into account the variation between and within places and the cost of experimentation and it has been pointed out that no gain is likely by having more than one or two trials at each place. The analysis of the results of an illustrative set of trials carried out on cultivators' fields in Tanjore District in Madras State has been presented.

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