

THE AIM OF FIELD EXPERIMENTS, II.

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In the last number of this journal some of the difficulties which face the field experimenter were enumerated and explained. The elementary principles upon which an adequately designed experiment must be based were set forward, and special mention was paid to the question of the comparability of plots. In this connection, soil heterogeneity was emphasized as being the main stumbling block in the way of its achievement. Having so far dealt with the general aspects, it remains to consider experimental design in rather more detail.

When all the precautions outlined in the previous article have been observed, there still remains sufficient local soil heterogeneity and crop individuality to produce errors of sometimes considerable size. No amount of ingenuity can eliminate these errors entirely, but they can be greatly reduced, and, what is of even greater importance,



PLATE VI.

ROSELLINIA INFECTION EXPERIMENT.

they can be estimated with precision. It is in the nature of an article of faith with an experimenter that he must know the size of the errors under which he is compelled to work. Experience has shown that it is never safe to assume that differences between treatments are significant, until evidence can be tabled to show that they are in excess of those which could quite easily be produced by chance. Chance in nine cases out of ten takes the form of soil heterogeneity and, in tea, bush to bush variation.

Discussion as to how the estimate of error is arrived at is beyond the scope of this article, but this much should be said,—an error is a statistical value which is readily calculable from any well designed experiment. It shows the limits of variation which any given value might have assumed if, for instance, the same experiment had been conducted independently under similar conditions. Since the conception of experimental error is one which will be frequently brought to the notice of readers of the *Tea Quarterly* it may be as well to familiarise them with the convention that, to establish the significance of a value (e.g., the difference in yield of one treatment from another), the value must exceed at least twice the "standard error" and three times the so-called "probable error."

There are four methods by which the error of an experiment may be reduced, viz:—

- (1) By having several plots of each treatment at the same time.
- (2) By repeating the experiment over a period of years.
- (3) By employing the best size and shape of plot.
- (4) By employing the best arrangement of the plots amongst themselves.

It is usual to take advantage of all these methods, for the one reacts upon the others, as will be shown.

It is in the first place essential to have more than one plot of each treatment, because not only are the errors usually inconveniently high with single plots, but without replication it is impossible to determine the error. The assessment of an error depends upon having a number of values to compare, which are experimentally (though not actually) alike. The more plots one has, the smaller will be the error, but the error does not diminish in proportion as the number of plots increases. In order to halve the error, the number of plots has theoretically to be quadrupled, and for other reductions increased in like proportion. There comes a time therefore when any further reduction in error is only attained at the cost of a cumbersome number of plots covering an unwieldy area. One has always to keep these practical considerations in mind.

The repetition of a trial over a number of years makes for greater accuracy, and furthermore is not rendered less necessary by having

a number of replications in one year. Climatic effects are so variable that a treatment may do well in one year and hardly make any difference in another. A term of years is consequently necessary to get a fair verdict.

The size of plot is of great importance. Plots may be so small that the least trace of soil variation will make one plot distinct in productivity from another, and with small plots differences in individual bush vigour will exert a preponderating effect. To go to the other extreme is equally bad, for it is more usual to find relatively uniform soil conditions in moderate-sized tracts than over large areas. Here again labour conditions are a consideration. Given a piece of land of a certain size, a larger number of smaller plots will always assure better results than a smaller number of larger ones, down to a certain limit of number and size. It will be seen that what is required is to investigate these limits of number and size of plots with respect to the magnitude of error they produce. When this is done the available land can then be used to the best advantage. This problem of limits is being investigated at Nwara Eliya.

It is not infrequently objected that small plots are of no use to the practical man. The idea behind this statement is obviously that, although a small plot may give a certain response, a similar one could not be expected from a large area. This objection, if it is valid, is undoubtedly a serious one and merits consideration.

In the first place the declaration of this view is merely another way of expressing the very natural fear of soil heterogeneity. In an experiment covering a few acres, it would in all probability happen that any one treatment had only a fraction of an acre devoted to it. The burden of the complaint is that this fraction is not representative in production of a practical-sized area, such as the two or three acres covered by the experiment itself. The practical man feels safer if he can speak in terms of acres rather than in fractions of them.

If the principles which have been stressed in these articles are adhered to, then complete assurance can be given to the practical planter that his fears are groundless. For herein lies the value and absolute necessity of knowing the experimental error. That error is a measure of the representativeness of the small plots. If small in size it tells him not only that the half dozen plots are representative of their combined area, but of the whole area of land over which they are scattered in the course of the design of the experiment.

Even if it were considered that an experiment of two or three acres will not give yields comparable with that obtained over larger areas, the validity of the trial still stands unassailed. It is the differences between comparably situated treatments which matter, and not the particular level of production attained by any one of them. The latter is bound to vary from locality to locality, but even so, the funda-

mental importance of a given treatment can be established. This is all an experiment can give, and is all it is fair to demand of it.

Of the four methods for reduction of plot errors, there remains the arrangement of the plots. Little can be said here, except that the effect is considerable and that the experimenter should be alive to it. In addition, experiments should always be designed so that the treatments dovetail into one another. For example if two kinds of nitrogen are being tested in varying quantities, the variations should correspond in each case. Consider three quantities. Any question regarding the kind of nitrogen can be determined from all the varying quantities considered together, from three plots of each instead of one. Any question of quantity can be settled using both kinds together, and so on, with consequent reduction in error.

This survey of aims and possibilities would not be complete without referring to the advantages and disadvantages which result from a previous knowledge of the productivity of the experimental area. It is sometimes customary to use a knowledge of the yield from the plots before treatment to compensate the results of subsequent treatment on what is known as the "percentage increase" system. If plot A untreated gave a 10% better crop than B untreated in the same year, it is held that next year, when B is treated and A left as control, a ten per cent. increase can be made to B in order to bring it to its true comparable level of production. Unfortunately, though A will in all probability maintain its superiority, it is certain that the 10% level will not be rigidly observed. By making the assumption of stability one introduces a new and variable error, one which cannot possibly be calculated. Such a procedure thus violates the canons of sound method and defeats its own ends.

There are nevertheless great advantages in knowing the previous history. Such knowledge enables one to choose as regular a tract of land as possible, discarding uneven areas. In so far as it throws light on fertility slopes, it is a great help in determining the arrangement of plots. Then, in any experiment where good plots remain consistently good from year to year, a shorter period of years is required before a fair answer to the problem tackled can be obtained.

If, in conclusion, the impression has been given that experiments are complicated things and that the introduction of statistical conceptions only tends to bewilder the non-technical reader, two points should be borne in mind. Complications are not introduced for complications' sake, but only because research has shown that the old simple experiments lack certainty. From the experimenter's point of view the old methods would save him much time and trouble if only he could rely on them. In the second place, the attitude that field experiments should be simple to study, and all plain sailing, is at

variance with that generally adopted in respect of other branches of research,—mycological, entomological, and chemical. In these cases the methods of arriving at conclusions are recognised as being specialised and technical, and the intricacies of culture preparation and analysis are not viewed with disfavour. The same view should be taken of field experiments, for, though the methods may be elaborate, if the experimenter says plainly, as he should, what the assured results are, the true aim of field experiments will have been attained.
