SIMPLE DESIGNS FOR ESTATE EXPERIMENTS

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The content and limitations of various types of simple experimental designs are explained. The conditions necessary for a design from which valid inferences could be drawn are discussed. The importance and use of pre-treatment data for improving the precision of an experiment are demonstrated.

"Shortly after detergents in powder form for domestic use appeared on the British market" writes *Finney, "my wife remarked to a friend that she found a particular brand very good, for washing clothes. 'I would never use that', said her friend, in a horrified tone, 'why, its a chemical ! ' Despite increasing realization that many of the problems of biological science are intrinsically statistical, why, it's statistical !' probably remains the unspoken reason for neglecting to employ techniques that could aid research." Prof. Finney is here pointing out that the fact that a detergent is fundamentally a chemical need not prevent one from successfully using it to wash clothes. Similarly, any biological experiment, by its very nature, is statistical. But this fact need not stand in the way of laying down simple experiments to help solve one's problems.

This article is written to enable planters to understand the uses and limitations of the various types of designs they are called upon to lay down on their estates. The ability to draw valid inferences and the relevance of the experiment to the problem it is expected to solve, depend very largely on the type of design used.

There are four essential ingredients in a valid design ;

1 – The set of treatments, *eg*, shade *vs* no-shade on the main plots and say, four levels of nitrogen on the sub-plots ;

2 – The specification of the units to which the treatments are to be applied—As far as estate trials go, the ultimate experimental unit is a *plot ie*, a small area of land with known dimensions. But the word 'plot' has now acquired a technical meaning and it can mean a single tea-bush, an evaluation by a tea-taster, the mean number of shot-hole galleries in a sample of twigs or even a single leaf on a growing plant.

3 – The method of allocating the treatments to the experimental units. This method is randomization. In some cases however, randomization may be subjected to certain restrictions. It has been shown that anyone who adopts an arrangement which he thinks is effectively random, runs a grave risk of biasing the results. In practice, it would suffice if lots were drawn to find out which treatment is to be applied to a particular plot.

4 – The type of records to be taken—We are usually interested in yield but sometimes we may be interested in the number of dead branches or the death of whole bushes.

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Simple designs

The object of all experimental designs is not only to minimize the uncontrollable variations of soil and environment but also to estimate them. The problem of such estimation is a purely statistical one and need not concern us here.

Paired Plots

If it is possible to find two plots about 1/50th acre each identical in every respect including yield, then the efficacy of two treatments can be compared. But such plots have not been discovered anywhere in the world. So, we choose two plots adjacent to one another and hope that they are of equal fertility and yield. What happens if our hopes are belied ? To meet this contingency we select ten to fifteen pairs of plots. We then allocate the two treatments to each plot within a pair at random. This may be done by tossing a coin. Each treatment is then likely to receive its fair share of good, bad and indifferent plots. The term *paired* here means that each plot of a pair must be *physically contiguous* (Cox 1958). The pairs themselves need not be in one compact area. The principle here is that plots adjacent to one another are likely to be less variable in soil and other external conditions than plots further apart.

It will be noted that 20 to 30 plots are needed for this design. The great limitation of this design is that only two treatments can be compared.

Randomized Block

This is the most widely used of all experimental designs. The name itself is a shortened form of "Treatments randomized within blocks". The blocks themselves need not be located at random. They are generally arranged neatly in one compact area. A block of an area of tea considered to be uniform is divided into as many plots as there are treatments to be tested. The treatments are then allocated at random to the plots. Suppose there are ten treatments to be compared, then at least, three such blocks are necessary, *ie*, there must be thirty plots in all. The word '*block*' in this design has identically the same meaning as the word *replicate* used in an earlier article (Kanapathipillai 1965) because the full set of treatments goes into each block.

This design is a logical extension of the paired-plot technique. Whereas earlier, only two treatments can be compared, now we can compare ten different treatments using almost the same number of plots *viz*, 30. The disadvantage of this design is that as the number of treatments increase, the size of the block increases with consequent increase in soil variation. Large soil variation tends to mask the effect of treatments. Randomized blocks, therefore, become unsuitable if more than say 10 to 12 treatments are to be studied.

Factorial Design

Any single treatment may be defined as a *factor*, *eg*, a clone is a factor, a particular level of fertilizer, say 100 lb N as S/A is a factor ; clean pruning is also a factor, and so on. The designs discussed so far (*viz* paired plots and randomized blocks) are mainly meant for the study of *single* factors. By contrast, a *factorial experiment* is one in which two or more factors are studied *simultaneously*, *ie* both singly, and in combination. If the performance of ten clones each at 3 levels of a particular fertilizer are studied, we call this a 10×3 factorial involving 30 plots for a single replicate. Similarly if four levels of shade each at four levels of sulphate of ammonia are studied, we say that the design is a 4×4 factorial. This is sometimes written as 4^2 factorial. It should be noted that the figure 2 stands for the *number* of factors and the figure 4 for the *levels* of each factor. Thus a 2^3 (read as two to the three) factorial would mean *three factors* each at *two levels* involving 8 treatment-combinations. Similarly, a 3^3 factorial (a well-known design for NPK trials) is a design to test three factors each at three levels involving 27 treatment—combinations and therefore requiring

27 plots for a single replicate. If all 27 treatments are laid out in a single block, the block size would be too large and the plots within the block are unlikely to be fairly uniform in regard to soil characteristics. They are, therefore, usually laid out in three blocks of 9 plots each. The actual treatments which go into each of the three blocks is a little tricky and should best be left to the Institute.

Split-plot

We have earlier referred to restricted randomization. There are some treatments which by their very nature, cannot be freely randomized. One such treatment is shade. It is obvious that shade cannot be randomly allocated in the sense in which, say, fertilizers can be. In such cases, a big block of shaded tea, say, 1/8th acre, is taken and divided into four plots. The big block of shade is called the *main plot* and the four plots into which it is divided are called the *sub-plots*. If four levels of fertilizer are tested, they are allocated to the four sub-plots under the shade main plot. The same four treatments are laid out on an adjacent big block which has no shade. This experiment would be called a *split-plot experiment* although split-block would have been a more appropriate name in this case. The difference between a factorial and a split-plot design would be that in the former each treatment would be on an equal footing whereas in the latter there would be a typical hierarchical set-up (Cochran & Cox 1950) ; the shade and no-shade tratment being in overall charge, so to speak, and fertilizer treatments being subordinate to them.

Pre-treatment records

It was stated earlier that in field experiments the block is presumed to be fairly uniform within itself. But this may not be the case. Very often, particularly in perennial crops, the variations of yield within a block can be quite large. The technique which takes account of this variation is called *covariance*. Although it sounds complex, the principle is quite simple. Suppose we were judging the performance of two javelin throwers A and B. For arguments sake, assume that B stood ten feet ahead of the other before delivering the throw. Obviously, in comparing their performances we will have to subtract ten feet from B's throw since he has had the initial advantage of that distance. In this case the comparison is quite easy since we know the initial advantage B had, and, what is more, the *base-line*. In the case of field plots, the pre-treatment records will tell us the initial yields of the plots before treatment but we now have no base-line. We have, therefore to set about finding one. It has to be taken on trust that such a line could be found and it is called the *regression line*. Some plots will have yields above this line, some below it and some even exactly on the line. For those plots whose yields are *above* the regression line, we subtract the initial advantage ; for those whose yields are below the regression line we add the necessary amount and for those on the line, we make no correction at all. In this way, the plots are brought to an equal footing before treatment-comparisons are made. It is a very efficient device for increasing the precision of treatment comparisons. Therefore, the importance of keeping accurate pre-treatment records will, it is hoped, now be appreciated.

References

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