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An Introduction to Design of experiments following R A Fisher

Jyotsna Chandel
Department of Mathematics
D.S. College, Aligarh, India
jyotsnaraghuvanshi5@gmail.com

Poonam Bajpai
Department of Mathematics
M.B.P.Govt.P.G.College, Lucknow, India
pvineet76@yahoo.com

***Abstract*—We know that Researchers doing research in social fields, collect the data for their purposes and for giving some interpretation, they need to apply some statistical methods in which Design of experiments is mostly applied. In view of that we are giving a brief introduction to Design of Experiment following R.A. Fisher who proposed the Methodology of this field.**

Keywords*— *design of experiment; randomisation; statistical control; factorial experiment;

I. Introduction

A methodology for design experiments was proposed by Ronald A. Fisher, in his innovative books: "The Arrangement of Field Experiments" (1926) and *The Design of Experiments* (1935). Much of his pioneering work dealt with agricultural applications of statistical methods. As a mundane example, he described how to test the hypothesis that a certain lady could distinguish by flavor alone whether the milk or the tea was first placed in the cup (AKA the "Lady tasting tea" experiment). These methods have been broadly adapted in the physical and social sciences, and are still used in agricultural engineering. The concepts presented here differ from the design and analysis of computer experiments [1,2].

A. Comparision

In some fields of study it is not possible to have independent measurements to a traceable standard. Comparisons between treatments are much more valuable and are usually preferable. Often one compares against a scientific control or traditional treatment that acts as baseline.

B. Randomisation

Random assignment is the process of assigning individuals at random to groups or to different groups in an experiment. The random assignment of individuals to groups (or conditions within a group) distinguishes a rigorous, "true" experiment from an observational study or "quasi-experiment". There is an extensive body of mathematical theory that explores the consequences of making the allocation of units to treatments by means of some random mechanism such as tables of random numbers, or the use of randomization devices such as playing cards or dice. Assigning units to treatments at random tends to mitigate confounding, which makes effects due to factors other than the treatment to appear to result from the treatment. The risks associated with random allocation (such as having a serious imbalance in a key characteristic between a treatment group and a control group) are calculable and hence can be managed down to an acceptable level by using enough experimental units. The results of an experiment can be generalized reliably from the experimental units to a larger population of units only if the experimental units are a random sample from the larger population; the probable error of such an extrapolation depends on the sample size, among other things. Random does *not* mean haphazard, and great care must be taken that appropriate random methods are used.

C. Measurements

Measurements are usually subject to variation and uncertainty. Measurements are repeated and full experiments are replicated to help identify the sources of variation, to better estimate the true effects of treatments, to further strengthen the experiment's reliability and validity, and to add to the existing knowledge of the topic. However, certain conditions must be met before the replication of the experiment is commenced: the original research question has been published in a peer-reviewed journal or widely cited, the researcher is independent of the original experiment, the researcher must first try to replicate the original findings using the original data, and the write-up should state that the study conducted is a replication study that tried to follow the original study as strictly as possible.

II. IMPORTANT FACTORS WHEN SETTING UP AN EXPERIMENTAL DESIGN

An experimental design or randomized clinical trial requires careful consideration of several factors before actually doing the experiment. An experimental design is the laying out of a detailed experimental plan in advance of doing the experiment. Some of the following topics have already been discussed in the principles of experimental design section:

1. How many factors does the design have? And are the levels of these factors fixed or random?

2. Are control conditions needed, and what should they be?

3. Manipulation checks; did the manipulation really work?

4. What are the background variables?

5. What is the sample size? How many units must be collected for the experiment to be general sable and have enough power?

6. What is the relevance of interactions between factors?

7. What is the influence of delayed effects of substantive factors on outcomes?

8. How do response shifts affect self-report measures?

9. How feasible is repeated administration of the same measurement instruments to the same units at different occasions, with post-test and follow-up tests?

10. What about using a proxy pretest?

11. Are there lurking variables?

12. Should the client/patient, researcher or even the analyst of the data be blind to conditions?

13. What is the feasibility of subsequent application of different conditions to the same units?

14. How many of each controls and noise factors should be taken into account?

III. STATISTICAL CONTROL

It is best that a process be in reasonable statistical control prior to conducting designed experiments. When this is not possible, proper blocking, replication, and randomization allow for the careful conduct of designed experiments. To control for nuisance variables, researchers institute control checks as additional measures. Investigators should ensure that uncontrolled influences (e.g., source credibility perception) do not skew the findings of the study. A manipulation check is one example of a control check. Manipulation checks allow investigators to isolate the chief variables to strengthen support that these variables are operating as planned.

One of the most important requirements of experimental research designs is the necessity of eliminating the effects of spurious, intervening, and antecedent variables. In the most basic model, cause (X) leads to effect (Y). But there could be a third variable (Z) that influences (Y), and X might not be the true cause at all. Z is said to be a spurious variable and must be controlled for. The same is true for intervening variables (a variable in between the supposed cause (X) and the effect (Y)), and anteceding variables (a variable prior to the supposed cause (X) that is the true cause). When a third variable is involved and has not been controlled for, the relation is said to be a zero order relationship.

In most practical applications of experimental research designs there are several causes (X1, X2, X3). In most designs, only one of these causes is manipulated at a time.

A. Blocking

Blocking is the arrangement of experimental units into groups (blocks/lots) consisting of units that are similar to one another. Blocking reduces known but irrelevant sources of variation between units and thus allows greater precision in the estimation of the source of variation under study.

B. Factorial Experiments

Use of factorial experiments instead of the one-factor-at-a time method. These are efficient at evaluating the effects and possible interactions of several factors (independent variables). Analysis of Experiment Design is built on the foundation of the analysis of variance, a collection of models that partition the observed variance into component.

C. Example

In this example weights of eight objects are measured using a pan balance and set of standard weights. Each weighing measures the weight difference between objects in the left pan vs. any objects in the right pan by adding calibrated weights to the lighter pan until the balance is in equilibrium. Each measurement has a random error.

TABLE.I

	Left pan	**Right pan**
1st weighing:	1 2 3 4 5 6 7 8	(empty)
2nd weighing:	1 2 3 8	4 5 6 7
3th weighing:	1 4 5 8	2 3 6 7
4th weighing:	1 6 7 8	2 3 4 5
5th weighing:	2 4 6 8	1 3 5 7
6th weighing:	2 5 7 8	1 3 4 6
7th weighing:	3 4 7 8	1 2 5 6
8th weighing:	3 5 6 8	1 2 4 7

Fig.1

. The average error is zero; the standard deviations of the probability distribution of the errors is the same number σ on different weighings; and errors on different weighings are independent. Denote the true weights by θ_1……….θ_8.

We consider two different experiments:

1.Weigh each object in one pan, with the other pan empty. Let Xi be the measured weight of the ith object, for i = 1, ..., 8.

2.Do the eight weighings according to the following schedule and let Yi be the measured difference for i = 1, ..., 8Then the estimated value of the weight θ_1 is

$$\widehat{\theta_1} = \frac{Y_1 + Y_2 + Y_3 + Y_4 - Y_5 - Y_6 - Y_7 - Y_8}{8}$$

Similar estimates can be found for the weights of the other items. For example

$$\widehat{\theta_2} = \frac{Y_1 + Y_2 - Y_3 - Y_4 + Y_5 + Y_6 - Y_7 - Y_8}{8}$$

The question of design of experiments is: which experiment is better?

The variance of the estimate X1 of θ1 is σ2 if we use the first experiment. But if we use the second experiment, the variance of the estimate given above is σ2/8. Thus the second experiment gives us 8 times as much precision for the estimate of a single item, and estimates all items simultaneously, with the same precision.

What the second experiment achieves with eight would require 64weighings if the items are weighed separately. However, note that the estimates for the items obtained in the second experiment have errors that correlate with each other. Many problems of the design of experiments involve combinatorial designs, as in this example and others.

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