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The Theoretical Analysis of Experimental Research

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Abstract: *Among the various research methods, the experiment is particularly suitable for cause and effect relationships. Through observation one finds many things that occur together, but observation alone cannot determine whether one thing is the cause of another. All the alternative explanations must be ruled out. A classic example is the relationship between cigarette smoking and lung cancer. Doubts about the contribution of cigarette to lung cancer were removed when experiment on animals showed that the substances in cigarette were carcinogenic (cancer causing). Other environmental factors such as air pollution may also affect the relationship. The experiment enables each of these factors to be tested in a systematic way. An artificial situation is created in which events that generally go together may be pulled apart (Sommer & Sommer 1980).*
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I. Introduction

Experiments are ways of assessing causal relationships, by randomly allocating "subjects" to two groups and then comparing one (the "control group") in which no changes are made, with the other (the "test group") who are subjected to some manipulation or stimulus. Anyone who has done secondary school science will be familiar with laboratory experiments. Physical substances re -subjected to some kind of stimulus (chemicals are heated or mixed with other chemicals; electrical currents are applied to wires; plants are given varying munt of water or light etc). A change is predicted and (usually) found. The change being interpreted a caused by the stimulus (Payne & Payne 2004).

In his book, Zan and the art of motorcycle maintenance, Rosert Pirsig (1974) describe the purpose of the experimental method as making sure that Nature has not misled you into believing you know something you actually don't know.

II. Experimental Design

Experimental Research is the most tightly controlled of the various kinds of research we have. It involves systematic manipulation of experimental conditions in which extraneous influence are controlled or eliminated. Experimental is different from descriptive research, the position is taken that the effects of one variable upon another can be investigated by isolation upon another can be investigated by isolation and study of those variables. Usually these effects are nt stated in definite cause-and-effect terms because whether causes can ever be identified in problematic. A study by Rosenthal and Fode (1963) will serve to eliminate these points. In this study, the experimenters influence on the outcome of an experiment was investigated using a group of psychology students as experiments in a study of maze learning with laboratory rates as subjects. Half of the experimenters were told that their rate were brad to be "maize-bright", and the remaining experiments were told that their rate were brad from "maize-dull" strain. According to Rosenthal and Fode, animals believed to be brighter by the students experimenters performed better from the first day of the study. (Mason & Bramble, 1989).

In addition, at the end of the experimental period, students experimenters were told that they had brighter rate tended to describe their rate in more favourable terms. Rosenthal and Fode concluded that the belief's of the students-experiments were associated with the performance of the rats. They were not saying that the experimenters belief's Directly caused the rats' behaviour; a number of intermediate events could have occured to cause the alleged "maize-bright" rats to perform better (mason & bramble 1989).

In experimental designs, subjects are randomly assigned to the control and experimental groups, and the independent variable can be manipulated. Such designs allow for all operations that make causal inferences possible: comparison, manipulation and control. To illustrate the application of an experimental design in a social setting, let us examine a study designed to assess the effect of teachers' expectations on intellectual growth. The study, conducted by Rosenthal and Jacobson (1968), is based on the idea that one person's expectations for anothers behaviour can often serve as a self-fulfilling prophecy. The authors hypothized that the school children whom their teachers believed were brighter would actually become brighter because of their leaders beliefs. A public elementry in which this hypothesis was tested. All the children participated in the experiment were pretested with a standard nonverbal test of intelligence. The test was represented to the teachers one that predicted intellectual blooming. Following the pre-test, each of the participants teachers was given the names expected to show intellectual growth. The predictions were allegedly made on the basis of pre-test; actually, however, the names of the blooms were randomly chosen. The experimental group thus consisted of the expected bloomers, and all other children were in the control group. The expectation of the teachers was defined as the independent variable; the intellectual growth of the pupils was the dependent variable (Nachmias and Nachmias 1976).

All the children in the two groups were retested with the same intellectual test after one year, and gains in intelligence were computed. A significant difference between the pre-test and the post test was found only among the children of the experimental group. This finding led the investigators to conclude that the expectation of teachers is causally relate to intellectual growth. The classic experimental design was approximately in this study because the experimental and control groups were similar to each other both were protested and protest, and the experimental stimulus (positive teacher expectation) was withheld from the control group (Nachmias and Nachmias, 1976).

The experimental study is designed to minimize alternative explanations for the obtained results. That is, if we do an experiment in which we compare two teaching methods, unless all factors, which might contribute to a difference between the two methods, are controlled, the experiment is not very useful. Experiments studies involve comparing conditions under various stages of the treatment. In a simple experiment a collection of subjects might be divided into two groups, one to undergo a treatment condition, the other to receive a neutral treatment (or control) the two groups are controlled after the treatment is applied using a criterion measure: if there is a difference in the measure between the groups, it may be attributed to treatment effects only if the two groups work similar on the measure before the treatment conditions were applied. (Mason & Bramble, 1989).

Because of the nature of the real-life situations in which behavioural scientists and educational researchers collect data, many studies have to be conducted under less than controlled conditions. Researchers often find that they cannot select their subjects or assign treatment as they would like and may therefore design what they call quasi-experimental studies which are similar to experimental control is not a realistic probability or would be too costly (Mason and Bramble, 1989).

III. Quasi-Experimental Design

Cook and campbell (1979) coined the term "quasi-experiments" to apply to this research investigation that lacked one or more of the characteristics that denote a true experience (e.g randomisation, manipulation of an independent variable, presence of a control group). Thus a quasi-experiment is akin to an experiment, except for the fact that the actual causal dynamics underlying the research question remain somewhat obscure. A quasi-experiment is a research design resembling an experiment, but it lacks one or more features heightening experimental control. Quasi experiments enable investigators to approximate but not delicate causal effects (Dunn 2001).

The term quasi experiment is given to studies in which experimental procedures are applied yet the study does not meet the full requirement control, namely, random allocation of subjects to conditions (coolican 1990).

Quasi experimentcj field differ in several important respects from true experiments. First of all, although true experiments may often introduce treatment conditions that are unfamiliar to participants, quasi experiments often can (and should) make more subtle modifications of interventions not as disruptive (unfamiliar) to the participants. However, such deceptive of large numbers of people who are not aware that they are part of an experiment. In more generic terms, the researcher using quasi experimental methods can control the "when" and "to whom" of measurement and occasionally the "when" of experimental exposures unlike true experiments, however, there usually is little control over the "when" of exposure (randomised exposure). (Campbell and Stanley, 1963).

Occasionally the field of experiment may have full experimental control, as was true in the already mentioned (Darley and Batson 1973) study of good Samaritanism among seminarians. The best research designs should always be used, where feasible. Thus, if a naturalistic field of study can be done with true experimental controls, the researcher will exercise more control over interpretations of findings since he or she can reject more plausible rival hypothesis (Smith, 1981).

The experimental design as the strongest model of proof is the reference for all other designs. However, it cannot be employed as a research plan in situations where manipulation and random allocation of respondents is impossible. There limitations very often derive from the nature of relationships studied by social scientists. Rosenberg (1968) has made a distinction between two kinds of relationships. The first is stimulus respond relationship, characterized by an independent variable that is external, specific and well defined, with a dependent variable being a particular response to it. For instance, relationship between reward and satisfaction, or between reward and satisfaction, or between advertisement and consumption patterns, are the stimulus response kind. The second type that is predominant in social science research is between a property (usually some background characteristics) and dispositions such as attitudes, values and orientations. Examples are the relationships between race and prejudice. Whereas stimulus response relationships are well suited for experimental investigation, property - disposition relationships are not. (nachmias & Nachmias 1976).

Quasi-experiments are used to study those situations that do not lend themselves to experimentation. The most common reason the quasi experiment employee is due to the inability to randomly assign participants to some treatment condition or group corresponding to a level of independent variable. Random assignment is not possible, for example, when a research wants to study the behaviour of some intact group of persons who have experienced something unique. An investigator cannot randomly assign people to live along the San Andreas Fault in California to assess their reactions to earthquakes for example, instead investigators must be content with drawing conclusions about the experiences of those who choose to live there, even if that experience is "contaminated" by or confounded with many other influential variables (Pennebaker & Haber, 1993).

Use of quasi experimental design necessitates demonstrating the between group differences are due to some treatment, a naturally occurring even (e.g an earthquake), or something about the research participants themselves. Many times an adequate control or comparison group is not even available. For example, should you compare the responses of earthquake survivors with those of people who do not live in earthquake zones, or is it better to compare them with the survivors of other natural (e.g floods, fires) or man-made (e.g ship wrecks, plane crashes) disasters? Is it even necessary to have a control group? Questioni like these- and researchers must duly consider the concerns they raise - before any firm conclusion about the results of a quasi experiment can be drawn (Dunn 2001).

The distinguishing characteristic of a quasi experiment, also known as a natural experiment is that the condition for a true experiment (random assignment) is not met. These are studies in which it has not been possible or feasible to randomly assign subjects to treatment groups or conditions. The "treatment" may be a natural event, a new law or program or something else outside the researcher's control (Sommer and Sommer 1980).

Pre-Experimental Designs

The One Short Case Study

In this design a single group is studied subsequent to some treatment or agent assumed to cause change. The design is represented as X O. As campbell and Stanley (1963) correctly point out, this design is essentially worthless. Comparisons are possible only in so far as the researcher's experience allows knowledge of what the observed scores would have been without exposure to the treatment. This type of comparison is of dubious value, however (Mason and Bramble 1989).

The one-shot case study involves an observation of a single group at one point in time, usually subsequent to some events that allegedly produced change. For example, the study might be an observation me a community after an urban renewal programme, a political system after elections, or a school after it has been exposed to an innovative teaching method, this design is an observation only of what exists at the time of study; as such, it has no control over extrinstic and intristic factors. In addition, it ends not allow for manipulation of the independent variable or for before- after or control-group-experimental group comparison. Furthermore, since case studies analyse single unsampled systems, they are weak on generalization as well. Studies that employ the one shot case study design have no check on internal validity and thus are of little use in testing causal relations. Indeed, this design has been denoted by som methodists as pre experimental. Selltiz et al (1959) designate it as a "stimulating insight" maintaining that the intensive case-study approach is particularly useful in unformulated areas, where it might suggest hypothesis for further research (nachmias and Nachmias1986).

Pilot Testing

Before actually running an experiment, it is essential to run a pilot study in order to be sure that the equipment is working and that directions are closely understood. Pick a number of persons to use as pilot subjects. Do not use anyone you intend to use in the actual experiment because the pilot test experience might affect their later responses. Think of the pilot study as a last minute check on the details. Make it as similar to the anticipated experiment as possible. If major problems are encountered, correct them and conduct another pilot test before running the actual experiment. The importance of the pilot test cannot be over emphasized. A preliminary run may save considerable time and effort later (Sommer and sommer 1980).

Pretesting

In the simplest experimental design, subjects are measures in terms of a dependent variable (pretested) (Babbie 1986).

Sometimes it is possible to check on the equivalence of groups by having the subjects perform a pretest. The pretest must be identical to or highly a correlated with the dependent measure. The procedure is to assign the subject to conditions randomly, administer the pretest, introduce the independent variable, and measure performance on the dependent variable. This allows the investigator to determine whether the groups were equivalent prior to the introduction of the independent variable. In the rare case in which random assignment does not produce equivalent groups, it would be impossible to make them equivalent prior to the introduction of the independent variable, or to correct for the differences in the pretest scores when analyzing the scores of subject on the dependent measure (Wood, 1974).

The Posttest-Only Control Group

While the classic experimental design is normally utilized in most true experiment some researchers forgo the pretesting of control and the experimental groups. Even though the posttest-only control group design for similarity of the experimental and control groups through randomization, it cannot give evidence-supporting equality of each group prior to introduction of the experimental treatment. Ironically, while social and behavioral scientists have found it psychologically troublesome to wean themselves of pre experimental treatment of group "equality" most physical and biological experimentations forgoes pretesting (Smith 1981).

The Solomon Four-Group Design

The pretest in an experimental setting has advantages as well as disadvantages. Although it provides an assessment of the time sequence as well as basis of comparison, it can have severe reactive effects (Nachmias & Nachmias 1976). This design combines the classical and post test - only designs to determine (and thus control for) any reactive effects of testing (pre test measurement or observation). This is the design used by McCall and others (1974) to pretest for reactive effects of observational accuracy tests. Because of the fact that the researcher needs twice as many participants and groups as in the other two types of experimental designs, this design is normally used only when reactive effects of pretesting are suspected (Smith 1981).

IV. Internal Validity Of Experimental Design

In judging the worth of a research design, validity must be our primary consideration. Internal validity is the basic requirement without which an experiment cannot be interpreted. It answers the question: "did in fact the experimental treatments make a difference in this specific instance?". In a study of a new-programmed text for mathematics instruction, the question of internal validity would be: "D the students in the study really know more about mathematics because of the programmmed text? (Campbell and Stanley 1963).

A study that tests a hypothesis and has internal validity is called a true experiment. A study that has no internal validity is called pre experimental and is not very useful. Another kind of experimental study, the quasi experiment is used when a true experimental design is not possible. In designing any experiment, the researchers must be aware of the factors that threaten its validity (Mason and Bramble 1989).

Threats to Internal Validity

Campbell and Stanley (1963) identified eight different types of influences that can threaten the internal validity of a research design. These influences if not properly controlled, can become confounded with (i.e entangled with and not separable from) the effects of the treatment variable. When these influences are not controlled, it is impossible to determine whether the treatment variable has produced the particular effect observed or whether that effect is due to something else. The right threats to internal validity are listed below (Mason and Bramble 1989).

History: This involves the influence the events that may occur in the time span between measurements. generally, the longer this time span, the greater the probability that something can happen in the subjects environment to affect the results (Mason and Bramble 1989).

History refers to all events occuring during the time of the study that might affect the subjects and provide a rival explanation for the change in the independent variable (Nachmias and Nachmias 1976).

Maturation

During an experiment, the subjects themselves can change. They must grow tired, hungry, bored, or, in the case of a longer experiment older (mason and Bramble 1989).

Maturation includes biological and psychological processes that produce changes in the subjects, with the passage of time. These changes could possibly influence the dependent variable. Maturation like history, is a serious threat to the validity of causal inferences (Nachmias and Nachmias 1976).

Testing

Taking a test before hand can cause a person to have a higher score the second time because of familiarity with the test. Thus, in a pretest - post test situation, the practice provided by the pretest can result in a higher score on the pretest (Mason & Bramble 1989).

The possible reactivity of measurement is a major problem in social science research. The process of testing may change the phenomena being measured. The effect of being pretested might sensitize the subjects and improve their scoring on the posttest (Nachmias and Nachmias 1976).

Instrumentation

When changes in the measurement or observation occur during the experiment, they can influence the experimental results. Such changes can occur when observers or scorers change or become tired or bored or when different scoring criteria are used because subjects are older (Mason and Bramble 1989).
Instrumentation designates changes in the measuring instruments between the pretests and the protests (Nachmias and Nachmias 1976).

Statistical Regression

There is a tendency for groups chosen on the basis of extreme scores to score closer to the average subsequently. For example, a group of composed subjects who scored extremely high on a test will probably average a lower score on a second administration of the test regardless of the treatment. (Mason and Bramble 1989).

Selection

When groups are to be compared to determine the effects of different treatments, but the groups are systematically different from the start, selection can become a problem (Mason and Bramble 1989).

Experimental Mortality

Experimental mortality can be a problem when subjects withdraw from the comparison groups at differential rates (Mason and Bramble 1989).

Experimental mortality refers to dropout problem that prevent the researcher from obtaining complete information on all cases. When subjects dropout selectively from the experimental or control group, the final sample on which complete information is available may be biased (Nachmias and Nachmias 1976).
Factors can operate together to influence experimental results. That is, threats to internal validity such as instrumentation, selection and experimental mortality could operate in combination to affect the results of the study (Mason and Bramble 1989).

External Validity of Experimental Design

Although internal validity is a crucial aspect of research and additional significant question concerns the generalization of the findings. Surely, most research is concerned not with Campbell and Stanley (1963) as the external validity of research designs and it refers to the ability to generalize the results (Nachmias and Nachmias 1976).

The degree to which the results of an experiment may be extended to other samples from the same population and to other populations is known as external validity. The method used to select a sample and the design of a study can have a major impact on the external validity of the study (Mason and Bramble 1989).

Choosing a representative sample from a population sampling is the act of drawing a sample from a population. Usually, the sample is considerably smaller than the population, though in the case of a relatively small population, the sample may be nearly the same size. An adequate sample must be large enough to provide fairly enough accurate estimates of the parameters of interest. It should also be representative of the population being studied and not of some atypical or bias, or part of it. A bias sample will lead to inappropriate conclusions about the population. It is surprisingly easy to find such samples. (Mason and Bramble 1989).

Probability methods such as random sampling would make generalization to larger and clearly defined population possible. In theory, the experimental and control groups should each constitute a probability sample of the population. The difference between them could them be generalised to the population. In practice, however drawing a probability sample for an experiment often involves problems such as high cost and high rate of refusal to cooperate (Nachmias and Nachmias 1976).

At this point, we should make a distinction between randomly assigning subjects to experimental groups and selecting a sample from a population for study. Randomly sampling involves selecting a group of subjects to be randomly assigned to treatment groups. In other words we are referring to two different processes. Ideally, first we select them, then we assign them (mason and Bramble 1989).

Random sampling concerns the selection of the entire group of participants who will participate in an experiment. When we conduct an experiment we want our conclusions to have relevance for more people than just those who took part in the experiment. One way we ensure this relevance is to have our participants constitute a representative sample of the entire population in which we are interested. By representative, we mean that the sample is similar in all major aspects, to the overall population from which it is taken. (Ray, 1997).

Random Assignment

After we have randomly selected our participants from the larger population we are studying, we can randomly assign them to our experimental and control group as a way of equating the groups prior to the experimental treatment. Randomly assigning participants has an important advantage over other procedures for equating groups; randomisation controls for both known and unknown potentially confounding variable (Ray 1997).

Simple Random Sampling

Simple random sampling is a way of selecting subjects in which every elements (or persons) in the population has an equal opportunity to be chosen. The major concern in determining the randomness of a sample is that every subject in the population has an equal opportunity of being selected (Mason and Bramble 1989).

Threats to External Validity

i. Reactive or interaction effect of testing. Treatment effects may not be generalizable when there is a pretest that sensitises the subject to the treatment that is, the findings regarding the treatment may only be generalized to those situations in which the pretest is administered.
ii. Interaction of selection and experimental variable. When subjects are selected who might be usually susceptible to the effects of the experimental variable, the results would not be generalizable to the larger group.
iii. Reactive Effects of Experimental Arrangements. This threat to external validity concerns the setting of the study. Findings observable in the laboratory may not be directly applicable to real-life settings.
iv. Multiple-treatment Interference. When multiple treatments are applied sequentially, subjects are likely to experience cumulative effects of subsequent treatments from those of prior ones (Campbell and stanley 1963).

V. Conclusion

Experimental research is the conventional method, which natural scientist employ in their investigations as against the survey method that they seldom employ.

An experiment involves the creation of an artificial situation in which events that generally go together are pulled apart. The participants in an experiment are called subjects. The elements or factors included in the study are termed variables. Experimental study is used to describe any study in which the investigator deliberately interferes with the situation by controlling what group of persons are exposed to certain conditions. Independent variables are those that are systematically altered by the experiments. Those items that are affected by the experimental treatment are the dependent variables. The experimental groups consists of those subjects exposed to the independent variable. Other subjects not exposed to the independent variable may be included in the study as a control group.

Whereas experiments are strong on control and weak on representation, quasi-experiments (especially surveys) are strong on representation but weak on control.

Experiments have several advantages. First and foremost, they enable valid causal inferences to be made by exerting a great deal of control. The second advantage is their control over the introduction of the independent variable, thus permitting the direction of causation to be determined. These advantages of experiments are the shortcomings of quasi-experiments. Lack of adequate control over rival explanations and difficulties in manipulating the independent variable prevent the researcher from drawing unambiguous inference.

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