DOCUMENT RESUME

ED 128 720 CG 010 829

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TITLE Strong Designs for Behavior Analysis.
PUB DATE [73]
NOTE 19p.; Paper presented at the Annual Meeting of the American Educational Research Association (San Francisco, California, April 19-23, 1976)

EDRS PRICE MF-$0.83 HC-$1.67 Plus Postage.
DESCRIPTORS *Behavioral Science Research; Behavior Change; Evaluation; Experiments; *Laboratory Techniques; Methods; *Research Design; *Research Methodology; Validity
IDENTIFIERS *Behavior Analysis

ABSTRACT

This paper deals with two problems confronting the applied researcher using a behavior analysis approach. The first is concerned with the need for independence of behaviors, subjects and settings. The effectiveness with which the researcher can solve this type of problem depends on the researcher's emphasis on the examination of procedures, and not experimental design. Similarly, the external validity problems involved in reactive experimental arrangements and multiple treatment interference are experimental procedure problems rather than design problems. Confronting those problems in this manner enables the applied researcher to add to the bank of empirical knowledge on human behavior. An extensive appendix which reviews the sources of invalidity proposed by Campbell and Stanley suggests that many are inappropriate to behavior analysis designs. Seven designs are presented graphically. (NG)

Strong Designs for Behavior Analysis

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The use of multiple baseline designs in evaluating behavior analysis research in applied settings has been criticized for a number of reasons. This paper represents an attempt to examine two of the major criticisms and pose a clarification. The first criticism has been called the need for independence of behaviors, subjects, and settings. The second concerns the problem of external validity. It is the contention of the authors that these two problems will continue to disturb applied researchers until they are aware of their relation to basic research and until they see the correspondence between experimental design and experimental procedures.

THE BEHAVIOR INDEPENDENCE PROBLEM

The behavior independence problem has been clearly stated by Kazdin (1973) as follows:

> A major area of concern in using this (multiple Baseline) design is that one must be reasonably assured beforehand that the target behaviors used are not interdependent or interrelated with each other. In such a situation, implementing a contingency for the performance of one behavior may be expected to alter the behavior(s) for which continued baseline data are collected. (p. 519, emphasis ours)

The problem is said to be applicable not only to behaviors but to individuals (one individual being observed may influence another observed individual) and situations (behavior changed in one situation may influence that behavior in some other place.) It is further contended that because of the possible changes in the other observed behaviors, situations, and/or subjects, the multiple baseline designs lose the power to demonstrate the effects of the contingencies. This is said to result in an experimental design problem.

Kazdin's only recommendation was a return to control group research methods on occasions where correlated behaviors, etc. were a problem. But as Sidman (1960, 1973) has pointed out this is helpful only at the most primative level of analysis when behavioral control techniques are not available.

The solution to the problem can be found in two perspectives involving the applied researchers view of where his or her research fits into the complex of empirical knowledge regarding human behavior. The first perspective involves the distinction between experimental design and experimental procedures, and the second the age old dichotomy between basic and applied research.

Designs and Procedures Perspective

The major point in the distinction between designs and procedures is that if one views the "design" as the problem confounding interpretation of results, then one fails to examine procedure.

The multiple baseline design is a time-series design, and the researcher has considered it empirically important to represent behavior change in more than two dependent variables in order to demonstrate a cause and effect relationship, and/or follow the change in behaviors as related to the change in independent variables. The same rationale can be applied to any of the other time series designs.

The power of the behavior analysis approach is in its continuous reeording of behavior over time and the specification of behavioral control techniques (procedures). It is the application of control techniques over time that allows for the demonstration of a functional relationship between behavior and the manipulated variables. Thus, procedures are considered the first aspects of research, the portion of the research that specifies the variables, both independent and dependent, that will be examined and the nature of the relationship they will have to each other during different experimental phases. The result of taking this procedural perspective is to reduce design to a method of representing behavior over time as related to procedural modifications. The lack of this perspective results in a loss of problem solving behavior on the part of the investigator. The researcher's forgotten strategy is the examination of behavior control procedures.

In order to establish and maintain the procedural perspective, it is necessary to differentiate between micro and macro research procedures. Micro procedures involve the quantitative, qualitative, temporal, and/or special manipulation of independent variables within an experimental phase. For example, one may vary reinforcement to shape a particular psychomotor skill. The behavioral control technique used is differential reinforcement which involves a temporal manipulation of the independent variable that is reciprocally dependent on the change in the behavior under investigation.

Macro procedures involve the elimination or presentation of behavioral control techniques. For example, after the above psychomotor skill was established and stable, the investigator may be interested in seeing how it extinquishes. He does this by the elimination of one set of control techniques and the establishment of another. This is typically called an experimental phase change, but the important point to remember is that it is a procedural manipulation.

The distinciton between micro and macro procedures is not free from some possible ambiguity. But most of that ambiguity can be eliminated by a review of a researcher's questions of interest as related to the procedures used. With or without ambiguity the distinction puts the emphasis were it belongs: on procedures.

Keeping the above design - procedure perspective in mind, it is possible to clarify the interdependence problem as posed by Kazdin. If the researchers are interested in eliminating the interdependence of subjects, they can do so simply by incorporating procedures that would at least physically separate the subjects. But if they are interested in the interactive process and wished to determine the controlling variables, they would incorporate different procedures. The difference between the two pieces of research is not a difference in experimental design, but a difference in experimental procedure which is governed by the research question asked.

An example involving interrelated behaviors and settings could be one where the researcher is interested in the process of generalization of a skill, like the learning of a problem solving strategy. But another researcher could be for some reason interested in keeping skill and situational transfer to a minimum. Both researchers will develop procedures to solve and/or examine their specific questions but for each the multiple baseline design would be appropriate. It would be appropriate for the first because he or she would follow the course of change for the multiple behaviors in multiple settings. For the second it would allow for the assessment of the procedures that were designed to reduce interrelatedness. In either case a failure (however it is defined) would lead to a change in procedure not design.

Essentially, what one is doing when one complains about correlated or interrelated behaviors, subjects and settings, is blaming the behaviors of interest for not doing what the researcher wanted them to do. Now, if behavior is lawful, it seems strange that the researcher should blame the experimental design. In most areas of science, the researcher would follow the course of events and see what occurs. (i.e., to hold or/change procedures depending on how the data change the researcher's experimental questions). Here the problem becomes one of investigating the controlling variables by the manipulation of variables thought to be in control of the observed changes in behavior. The encounter with such a phenomenon could lead to possible serendipitous discoveries and Skinner's (1974, p. 195) advice becomes meaningful; drop everything and follow the data.

Basic and Applied Perspective

Skinner's advice brings us to the relationship between basic and applied research, a relationship we feel is in need of a change in perspective if the applied researcher is to deal effectively with problems like those posed by Kazdin. The change in perspective deals with what the applied researcher has to add to the complex of human knowledge. Everyone agrees that it is the applied researcher's job to deal with socially relevant problems with knowledge gained from basic research. With only a quick glance one can see a multitude of social problems which involve human behavior but another glance will tell you that not a great deal of basic knowledge about human behavior is known. The correlated behavior problem is a clear example. Here the applied researcher is dealing with a complex phenomenon that is not understood in terms of its controlling variables.

At present, attempts are being made to deal in an applied way with phenomenon that need to be treated in terms of their controlling variables. Such related questions can be considered primarily basic research questions only because the applied researcher cannot drop everything. But what is it that the applied researcher can contribute and how does applied research relate to basic research?

The answer to the what and the how are closely related. As Skinner (1972) has pointed out the practical implications of applied research has lead the applied research to deal with variables the basic researcher would have avoided until a later date when more related knowledge would be available. For example, the improvement in the life of retardates and psychotics has made it necessary to deal with more variables than convenience dictates and is feasible in the laboratory. The result is that discoveries are made that laboratory practices would have made impossible, but which will at a future date facilitate laboratory practices in dealing with more complex phenomenon. Thus, the relationship is reciprocal. The applied researcher uses basic research findings to help develop procedures that are applied in more complex situations which can yield discoveries that can be used later by the basic researcher in his scientific endeavors.

Not only is there a possibility for the applied researcher to add to the knowledge of the field but in behavior analysis research, it is facilitated by the fact that the basic research model is the same as the applied treatment model; the basic research manipulation is the applied treatment manipulation, and the basic evaluation procedure is the applied evaluation procedure. The differences in the two are primarily in the details of the questions to be asked and the related specificity of the experimental procedures. This is a well-known fact, but one that appears to be over looked by researchers. If it were not overlooked, we would not have a concern for avoiding phenomena like correlated behaviors in applied behavior analysis research.

THE EXTERNAL VALIDITY PROBLEM

Designs and Procedure Perspective

The second complaint, also mentioned by Kazdin (1973), is the problem of external validity and the restrictions it places on the ability to generalize to other behaviors, subjects, and settings. Two possible confoundings are said to restrict the ability to generalize the findings. The first is multiple treatment interference. This refers to the possibility that a conclusion derived from a later treatment is dependent upon previous phases because the effects of each are not erasable. The second, called the reactive effects of experimental arrangements, refers to the fact that the particular experimental arrangements may preclude generalization to treatment effects across time, situations, and subjects. For example, the use of observers in the classroom and how their presence may change the behavior of the student irrespective of the treatment.

Here again, a reactive arrangement is not an experimental design problem but an experimental procedure problem. The concern is not for altering the multiple base-line design or what ever time-series design is used, but for modification of the experimental procedures. The process of observing by using observers could be eliminated not by changing the experimental design but by using different instrumentation procedures or by collecting data that shows that the observers are not affecting events. Following the former procedure often requires waiting for technological advances. Using the latter procedure may add confirmation to previous findings regarding the use of observers.

The multiple treatment interference concern is perhaps the kind of problem where the applied researcher can offer the greatest contribution to basic knowledge. This is because the phenomenon is a historical problem for which time-series designs are perfectly suited. The applied researcher's aim, for example, would be to set up a complex educational program and take "detailed" readings of the evolution of various phases and behaviors. The researcher would evaluate and adjust the various components of the phases until the terminal phase yielded a range of outcomes that meets social needs, so in the end the domain of inputs would be related to a range of outputs. The functional relation may not be spelled out but the major components will have been specified so the basic researcher will not have to grope in the dark. The practical applications in real life have given a general description of change over time, a general set of potentially relevant variables, and perhaps some measurement instruments.

The outcome of eliminating the above and other possible confoundings leads to clear procedural interpretations which represent facts about the world. If these facts can be related to other facts, then functional relations can be established for subjects, variables, processes and/or the methodologies involved. Thus,for the behavior analysis researcher, the problem is not one of external validity but one of the clarity of procedure. If no confoundings can be detected, the research results either add to or restrict the generality of one or more functional relationships.

Basic and Applied Perspective

This brings us to two points that should fully clarify the relation between basic and applied research. The first is to say that if one looks to the other sciences with a technology it becomes evident that there is no basic-applied dichotomy. At all levels from the most theoretical to the most technological there is a trading of findings. Perhaps the clearest example of this is in the physical sciences where the instruments developed by technology allow for the confirmation of some abstract theory. In the behavioral sciences the trade off has even greater potential because technological techniques applicable to designing environments could far outstrip behavior theory.

The second and most important point is the choice applied behavior analysis researchers must make with regards to their future activities. Basically there are two alternatives. The first, is the continued demonstration of the applicability of techniques and processes discovered in basic research to human activities. The emphasis has been on short-term research with a rigorous demonstration of cause and effect. The second alternative is to deal with clearly stated social problems in which research procedures are used to evaluate the outcomes in the sense of adjusting the variables so that the desired outcomes are met. Here, the research becomes longitudinal because social concerns can only be effectively dealt with in-the-long-run. The authors advocate the second alternative because it is prevention oriented. This is not to contend that some of this type of work is not going on. An example would be the Engleman and Becker system for teaching math, reading and language. It effectively solves a social problem and prevents, where ever it is used, many of the problems that previously occurred in these settings.

This paper has attempted to deal with two problems confronting the applied researcher using a behavior analysis approach. The first was the correlated behaviors problem. The effectiveness with which the researcher can solve this type of problem, it was argued, depends on the researcher's emphasis on the examination of procedures and not experimental design. Next, it was argued that to maximize success in dealing with these problems one must also see the possible contribution the researcher can add to the complex of empirical knowledge on human behavior. This latter point required a particular perspective related to the basic-applied research distinction.

APPENDIX

The following section is a review of behavior analysis designs and possible sources of invalidity proposed by Campbell and Stanley. We should emphasize that the sources of invalidity as applied to behavior analysis designs in many cases is inappropriate. As reviewed in the previous section the criticism of the designs is unwarranted because the real problem lies with the development of appropriate procedures.

Behavior Analysis Designs

First, let us briefly review the 12 factors that Campbell and Stanley cite as possible sources of invalidity. The first eight refer to internal validity, and the last four refer to external validity. These factors are as follows:

1. History, the specific events occurring between the first and second measurement. These events usually occur in the environment.

2. Maturation, the processes within the subject operating as a function of the passing of time. These processes are biological (as opposed to environmental).

3. Testing, the effects of taking a test upon the scores of a second testing.

4. Instrumentation, any changes due to observer effects or changes in the calibration of the measuring instrument.

5. Statistical regression, operates where groups have been selected on the basis of extreme scores.

6. Selection, any biases resulting from the differential selection of subjects for comparison groups.

7. Mortality, any differential loss of subjects from comparison groups.

8. Selection--maturation, interaction, etc., any confounding effects with the experimental variable.

9. Interaction effect of testing and treatment, where a pretest might increase or decrease the subject's sensitivity or responsiveness to the experimental variable.

10. Interaction effect of selection and treatment, where selection may interact with the treatment to produce a confounding variable.

11. Reactive arrangements, deals with generalizability in non-experimental settings. It also deals with the presence or absence of observers in experimental settings.

12. Multiple-treatment interference, what are the effects of prior treatments?

These 12 factors will be used to analyze the basic designs that are employed in behavior analysis studies.

There are a number of research designs that are used by experimenters to analyze behavior in a naturalistic setting. They are (1) A-B reversal, (2) A-B-A-B reversal, and (3) Multiple Baselines. Let us now look at each of these designs with regard to threats to internal and external validity.

1. A-B Reversal Design (Figure 1)

In this design, a behavior is recorded for an operant level (baseline or A) and then some contingency or change is instated (B). The way in which the behavior changes from condition A to condition B for an individual subject determines the strength of the causal inference made by the researcher.

The strength of this inference is open to possible attacks to its internal validity. The first confounding factor is subject history, because other change-producing events in the environment may have occurred in addition to the experimenter's manipulations. A second possible confounding could be due to maturation or change-producing biological events. A third confounding may be due to an interaction of maturation and selection. The other Campbell and Stanley sources of threats to internal validity are either controlled for or are not applicable. Testing is controlled for through the use of multiple baselines. Behavior is recorded continuously as opposed to a pretest, post-test recording. Instrumentation is controlled for also. Instrumentation is not a design problem <u>per se</u> but a <u>procedural</u> problem for experimental analysis designs, and is usually taken care of through the use of individual subjects. The factor of selection is irrelevant because of the use of an individual subject and not groups. There is no comparison group, and therefore, there is no need to worry about whether or not the experimental groups are the same. Our concern is only with the individual subject. Finally, mortality is not applicable because of the emphasis on the individual subject. If that subject drops out, there is no experiment to be run!

With regard to external validity, the A-B reversal design is very weak. It is not generalizable beyond the individual subject in a specific setting. One final problem with this design deals with the question of what would have happened if the investigator had not introduced a treatment? In other words, the investigator would want to know if the subject's baseline behavior would have increased, decreased, or remained the same if a treatment had not been instated. The answer to this problem would control the threats of history, maturation, and selection by maturation interaction.

2. A-B-A-B Reversal Design (Figure II and III)

This design is a four-stage experimental design. It attempts to establish sufficient controls in order to make strong inferences. This is how the design works: (1) take a baseline--first A, (2) instate a contingency--first B, (3) reverse back to the baseline condition--second A, and (4) reverse back to the contingency--second B. This design will allow one to make statements about the sufficiency of the conditions to cause the change that is observed. Wolf and Risley (1971) state that if the behavior reverses back to something approximating the baseline level when the treatment is withdrawn, one can make a reasonalbe estimate about what the natural course of the uninterrupted baseline behavior would have been. In this design, the behavioral changes (expressed in terms of ongoing variability) are or are not shown to be a function of the repeated presentation (the B's) and removal (the A's) of the experimental condition.

With regard to internal validity, the A-B-A-B reversal design controls for the factors of history, maturation, and the interaction of selection by maturation. These controls are accomplished through the use of reversal. With the reversals, there is a measure across time which allows us to see what happens when the contingency is removed. Given appropriate procedures, the other threats to internal validity are controlled for or are not applicable (see A-B reversal design).

In reference to external validity, one cannot generalize beyond the subject in a specific situation. The reason for this is because of the lack of random sampling. One way around this problem is to employ a Cornfield-Tukey logic argument. If the experimenter adequately describes the subject, baseline, and situation, he may then generalize his findings to similar subjects and situations. One final problem with this design is the inability to examine irreversible behavior, be it functional (a true behavioral process) or practical (for some reason you just do not want to reverse the behavior). This design can also be elaborated if the treatment contains a number of components. In this way one can determine the amount of variability controlled by the individual components or combination of components (Figure III). How the components are introduced is determined by the questions the research wants to answer.

The multiple baseline designs were developed to overcome this last problem.

3. Multiple baselines are experimental designs in which data are collected: (a) across behaviors, (b) across individuals, or (c) across situations.

a. Multiple Baselines Across Behaviors (Figure IV)

In this design, two or more behaviors are observed for a single subject (multiple baselines). The treatment condition is then introduced for one of the behaviors but not for the second behavior. In this way, the second behavior acts as a control for the first behavior. The treatment condition is later introduced for the second behavior. In this fashion, the greater the number of baselines, the greater our confidence can be in asserting that the effect was in fact due to our treatment (internal validity) and not to some confounding variable. The chance of confounding the interpretations of this design is slight because of the differential and systematic introduction of experimental conditions. This design allows a cause and effect relationship to be made about behaviors. The experimenter, by using two or more behaviors in his design, can now make stronger inferential statements about behavior than he could in the A-B or A-B-A-B reversal designs.

Given proper prodedures, the internal validity of multiple baselines is very strong. (reviewed in first part of this paper)

The external validity of this design is stronger than the reversal designs because the experimenter can now generalize his findings to more than one behavior. His inferences are stronger because the experimental condition is applicable to more than one behavior.

b. Multiple Baseline Across Individuals (Figure V)

In this design, baseline data are collected for the same behavior of different subjects in the same situation. The treatment condition is introduced for one of the subjects; but it is not introduced for the other subjects, the purpose being that the behavior of a subject will not change until he is given the treatment. The treatment condition is later introduced to the other subjects one at a time. When one subject is given the treatment condition, the other subjects are very much like a control group in a group comparison study.

Threats to internal validity, again given proper procedures, may easily be avoided. With regard to external validity, this design extends the subject (or individual) generalizations beyond the N = 1 findings. Since we have more than one subject, we can generalize our findings to the other subjects as well. If a population of subjects was defined and a ranodm sample drawn, we could then generalize our findings to our population. The same holds for multiple baselines across behaviors and across situations. This design uses both intra- and inter-subject replication to give greater inferenctial strength.

c. Multiple Baselines Across Situations (Figure VI)

In this design, baseline data are collected for the same behavior of one subject in different situations. The treatment condition is introduced in one of the situations but not in the other situations. The treatment condition is later introduced in the other situations. The main interest here is to see how an individual behavior changes in different situations.

Given proper procedures, threats to internal validity and external validity are minimal. The more situations in which the experimental conditions are instated, the greater the situation generalization. With this design, the experimenter could define a population of situations and randomly sample from this population for use in the study. The inferences about the treatment condition could then be applied to the population.

d. Complex Multiple baseline across behaviors, subjects, and situations (Figure VII).

With this design we can achieve behavior, subject, and situation generality. Given consistent results this design is strongest in establishing external validity.

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Figure I: A-B Reveral Designs

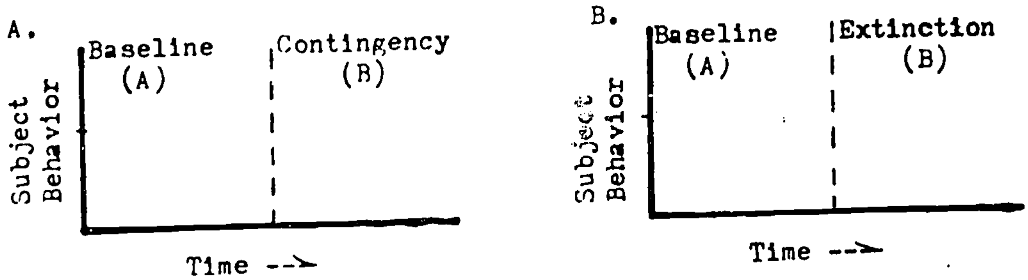

Figure II: A-B-A-B Reversal Designs

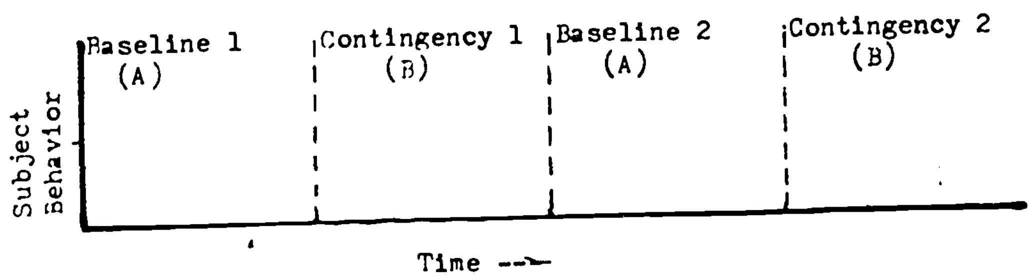

Figure III: A-B-A-B Reversal Design with Component Analysis

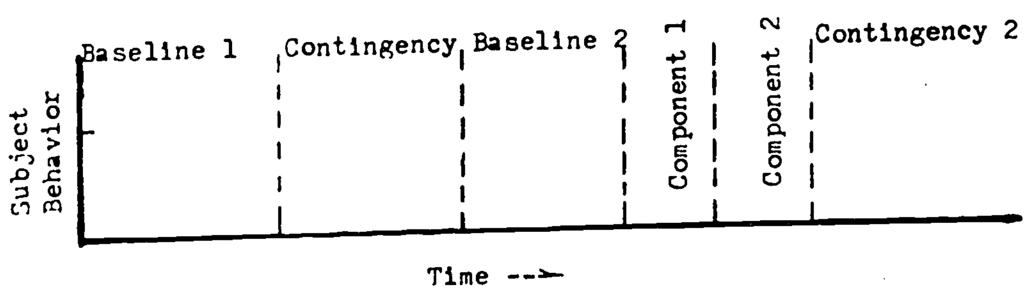

Figure IV: Multiple Baseline Design

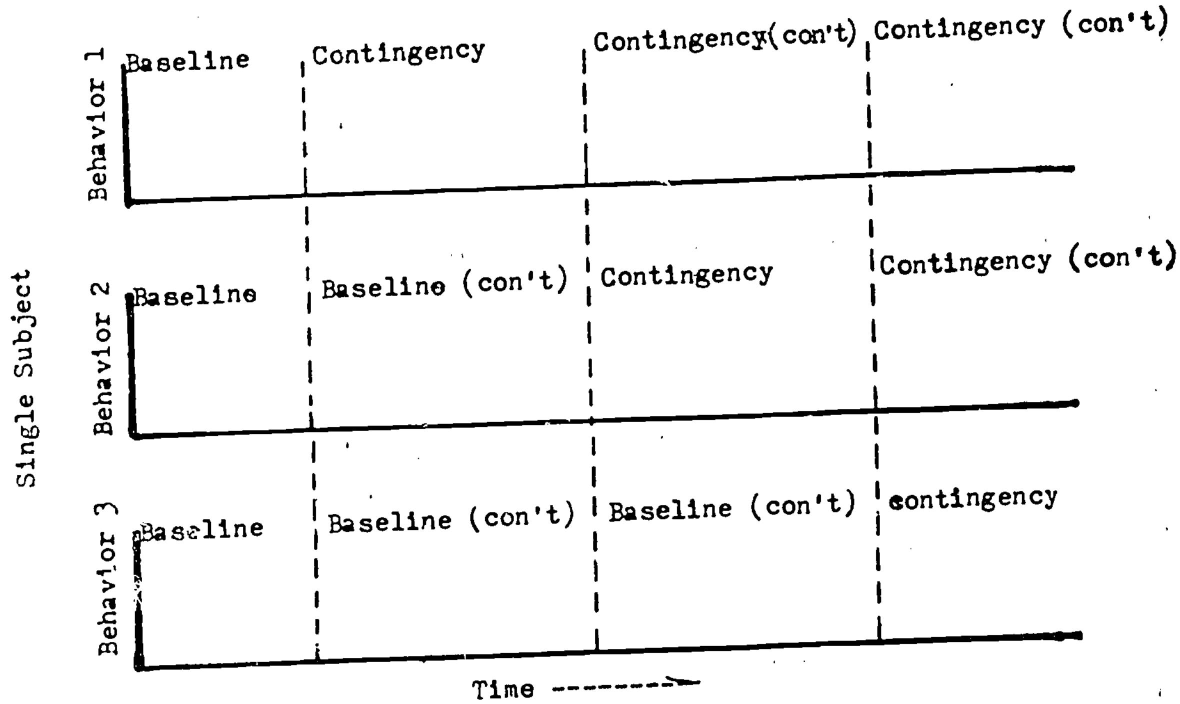

Figure V: Multiple Baseline Design for Subject Generality

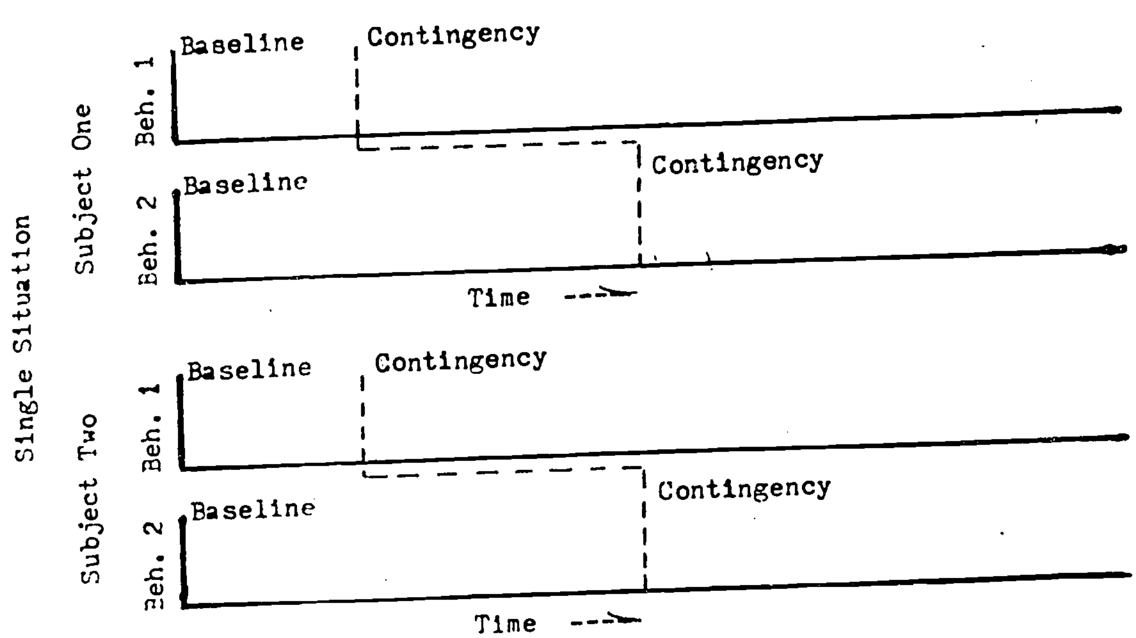

Figure VI: Multiple Baseline Design for Situation Generality

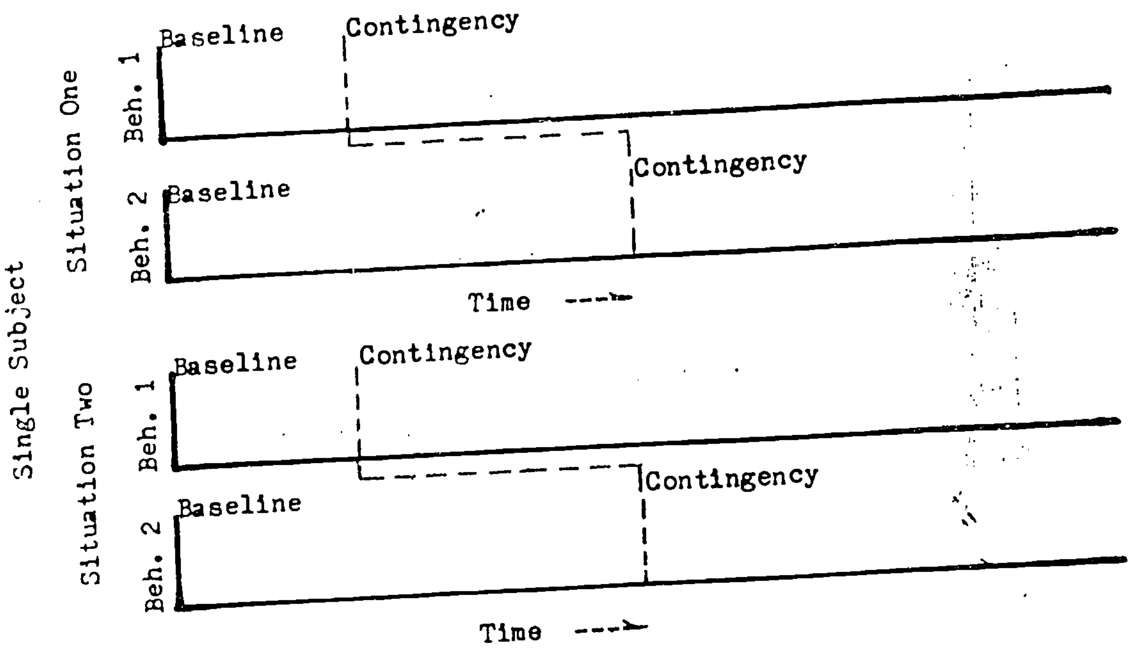

Figure VII: Multiple Baseline Design for Behavior, Subject, and Situation Generality