Lecture Notes

# Chapter 13: Experimental Research: Quasi and Single-Case Designs

## Learning Objectives

* 1. Explain the difference between strong experimental research designs and quasi-experimental research designs.
  2. Explain the limitations of quasi-experimental designs in making causal inferences compared to strong designs.
  3. Explain the characteristics of the nonequivalent comparison-group quasi-experimental design and how to search for rival hypotheses that might explain the obtained results.
  4. Explain the characteristics of the interrupted time-series designs.
  5. Explain how the regression-discontinuity design assesses the effect of a treatment.
  6. Explain how time-series and single-case research designs attempt to rule out confounding variables.
  7. Explain how a treatment effect is demonstrated in single-case research designs.
  8. Explain the limitations of each of the single-case research designs.
  9. Recognize and understand the methodological issues in single-case research designs.

## Chapter Summary

The experimental research designs discussed in this chapter are used when it is impossible to randomly assign participants to comparison groups (quasi-experimental designs) and when a researcher is faced with a situation where only one or two participants can participate in the research study (single-case designs). As the designs in the last chapter, quasi-experimental and single-case designs do have manipulation of the independent variable (otherwise they would not be “experimental research” designs).

## Annotated Chapter Outline

1. Introduction
   1. The previous chapter discussed weak and strong experimental designs.
   2. This chapter focuses on quasi-experimental research designs and single-subject research designs. Three types of quasi-experimental designs are discussed: nonequivalent comparison-group, interrupted time-series, and regression-discontinuity research designs. Three types of single-case experimental designs are presented: A-B-A and A-B-A-B designs, multiple-baseline designs, and changing-criterion designs. In addition, methodological considerations for single-case designs are presented.
   3. Quasi- and single-case research designs are used in situations when not all the demands of experimental research can be met. Random assignment and multiple groups are not always possible.
2. Quasi-Experimental Research Designs: do not provide for full control of potential confounding variables because random assignment cannot be implemented.
   1. **Quasi-Experimental Research Designs**: an experimental research design that does not provide for full control of potential confounding variables primarily because it does not randomly assign participants to comparison groups.
      1. Superior to weak designs but inferior to strong designs.
      2. Causal explanations can be made designs but only when data are collected that demonstrate that plausible rival explanations are unlikely
         1. Thee evidence will still not be as strong as with a strong research design.
      3. Table 13.1: Summary of threats to internal validity of quasi-experimental designs.
      4. Discussion question: compare and contrast quasi-experimental research designs to the weak and strong designs discussed in Chapter 12.
   2. **Nonequivalent Comparison-Group Design:** A design consisting of an experimental group and a nonequivalent untreated comparison group, both of which are administered pretest and posttest measures.
      1. The groups are “nonequivalent” because the design lacks random assignment (although there are some control techniques that can help make the groups similar such as matching and statistical control). Because of the lack of random assignment, there is no assurance that the groups are highly similar at the outset of the study.
      2. Because there is no random assignment to groups, confounding variables (rather than the independent variable) may explain any difference observed between the experimental and control groups.
      3. The most common threat to the internal validity of this type of design is differential selection. The problem is that the groups may be different on many variables that are also related to the dependent variable (e.g., age, gender, IQ, reading ability, attitude, etc.).
         1. Table 13.2 presents the biases that can potentially be present in nonequivalent comparison-group designs.
      4. It is a good idea to collect data that can be used to demonstrate that key confounding variables are not the cause of the obtained results. Hence, the researcher will need to think about potential rival explanations during the planning phase of the research study so that he or she can collect the necessary data to control for these factors.
      5. The influence of many confounding variables can be eliminated by using the various control techniques, especially statistical control (measuring the confounding variables at the pretest and control for them using statistical procedures after the study has been completed) and matching (selecting people to be in the groups so that the members in the different groups are similar on the matching variables).
      6. Only when you can rule out the effects of confounding variables you can confidently attribute the observed group difference at the posttest to the independent variable.
      7. May not give results from these studies the same credibility as the results from stronger designs.
      8. See Figure 13.1.
      9. Discussion Question: Discuss the many biases that may impact nonequivalent comparison-group study’s results.
   3. **Interrupted Time-Series Design:** A design in which a treatment condition is assessed by comparing the pattern of pretest responses with the pattern of posttest responses obtained from a single group of participants. (See Figure 13.4.)
      1. Participants are pretested a number of times and then posttested a number of times during or after exposure to the treatment condition.
      2. The pretesting phase is called the baseline that refers to the observation of a behavior prior to the presentation of any treatment designed to alter the behavior of interest.
      3. A treatment effect is demonstrated only if the pattern of posttreatment responses differs from the pattern of pretreatment responses. That is, the treatment effect is demonstrated by a discontinuity in the pattern of pretreatment and posttreatment responses. (See Figure 13.6.)
         1. For example, an effect is demonstrated when there is a change in the level and/or slope of the posttreatment responses as compared to the pretreatment responses.
         2. Visual inspection is used but in addition, statistical tests must be used to determine whether the change in response patterns is statistically significant.
      4. Many confounding variables are ruled out in the interrupted time-series design because they are present in both the pretreatment and posttreatment responses (i.e., the pretreatment and posttreatment responses will not differ on most confounding variables).
      5. However, the main potentially confounding variable that cannot be ruled out is a history effect. The history threat is a plausible rival explanation if some event other than the treatment co-occurs with the onset of the treatment.
      6. Discussion Question: Identify the critical components of the interrupted time-series design.
   4. **Regression-Discontinuity Design:** A design that assesses the effect of a treatment condition by looking for a discontinuity in regression lines between individuals who score lower and higher than some predetermined cutoff score. (See Figures 13.7 and 13.9.)
      1. This is actually quite a strong design, and methodologists have, for a number of years, been trying to get researchers to use this design more frequently.
      2. One uses statistical techniques to control for differences on the assignment variable and then checks to see whether the groups differ significantly.
      3. If you cannot assign the participants to the treatment condition based on their assignment variable scores, you will not be able to use this design. On the other hand, if you can do this, then this is an excellent design.
      4. Requirements for the regression-discontinuity design (Table 13.3)
         1. Assignment to comparison groups must be based only on the cutoff score.
         2. The assignment variable must be at least an ordinal variable and is best if it is a continuous variable. It cannot be a nominal variable such as sex, ethnicity, religious preference, or status as a drug user or nonuser.
         3. The cutoff score ideally should be located at the mean of the distribution of scores. The closer the cutoff score is to the extremes, the lower the statistical power of the design.
         4. Assignment to comparison groups must be under the control of the experimenter to avoid a selection bias. This requirement rules out most retrospective uses of the design.
         5. The relationship between the assignment and outcome variables (whether it is linear, curvilinear, etc.) must be known to avoid a biased assessment of the treatment effect.
         6. All participants must be from the same population. With respect to the regression-discontinuity design, this means that it must have been possible for all participants to receive the treatment condition. Therefore, the design would not be appropriate if, for example, the experimental participants are selected from one school, and control participants are selected from another school.
      5. Any threat to the validity of this design would have to cause a sudden discontinuity in the regression line coinciding with the cutoff.
         1. Differential history effect is most common in these designs.
         2. Differential effects are also possible but unlikely because the researcher can statistically control for this in making group assignments.
      6. Discussion Question: Identify strengths and weaknesses of the regression-discontinuity design.
3. Single-Case Experimental Designs: use a single participant in the experimental design to investigate the efficacy of an experimental treatment
   1. **Single-case experimental designs:** Design that uses a single participant to investigate the effect of an experimental treatment condition.
      1. All are some sort of time-series design because there is repeated measurement of the dependent variable before and after the treatment.
         1. This allows for ruling out extraneous variables such as history and maturation.
      2. Cases can be more than one individual. For example, the single case could be a single class.
   2. A-B-A and A-B-A-B Designs
      1. **A-B-A design:** a single-case experimental design in which the response to the experimental treatment condition is compared to baseline responses taken before and after administering the treatment condition. (See Figure 13.10.)
         1. A phase is baseline (no experimental treatment)
         2. B phase is experimental treatment condition.
         3. The effect of the experimental treatment is demonstrated if the pattern of the pre- and posttreatment responses (the first A phase and the B phase) differs and the pattern of responses reverts back to the original pretreatment level when the pretreatment conditions are reinstated (the second A or return to baseline phase).
         4. Including the second A phase controls for the potential rival hypothesis of history that is a problem in a basic time-series design (i.e., in an A-B design).
         5. Basically, you are looking for the “fingerprint” of a stable baseline (during the first A phase), then a clear jump or change in level or slope (during the B phase), and then a clear reversal or return to the stable baseline (during the second A phase).
         6. For example, if you hope for low values on your dependent measure (e.g., talking out behavior), you would hope to see a high-low-high pattern.
         7. Conversely, if you hope for high values on your dependent measure (e.g., attending to what the teacher says), you would hope to see a low-high-low pattern.
         8. One limitation of the A-B-A design is that it ends with baseline condition or the withdrawal of the treatment condition so the participant does not receive the benefit of the treatment condition at the end of the experiment.
         9. Discussion Question: Explain how researches evaluate the effect of an experimental treatment by using an A-B-A single case research design.
      2. **A-B-A-B design:** an A-B-A design that is extended to include the reintroduction of the treatment condition.
         1. Allows for a second administration of the treatment variable so the benefit of the treatment can continue.
         2. A limitation of both the A-B-A and the A-B-A-B designs is that they are dependent on the pattern of responses reverting to baseline conditions when the experimental treatment condition is withdrawn. This may not occur if the experimental treatment is so powerful that its effect continues even when the treatment is withdrawn.
         3. If a reversal to baseline conditions does not occur, another design (such as the multiple-baseline design) must be used to demonstrate the effectiveness of the treatment condition.
         4. Discussion Question: Why is the treatment condition reinstated in the A-B-A-B design?
   3. **Multiple-Baseline Design:** A single-case experimental design in which the treatment condition is successively administered to different participants or the same participant in several settings after baseline behaviors have been recorded for different periods of time. (See Figure 13.13.)
      1. Focus on two or more behaviors in one individual, or the same behavior exhibited by two or more people, or on the same behavior exhibited by one individual in different settings.
      2. The multiple-baseline design requires that baseline behavior is collected on the several people, behaviors, or settings, and then the experimental treatment is successively administered to the people, behaviors, or settings.
      3. The experimental treatment effect is demonstrated if a change in response occurs when the treatment is administered to each person, behavior, or setting (i.e., when the fingerprint you are looking for is observed). See Figure 13.14.
      4. Rival hypotheses are unlikely to account for the changes in the behavior if the behavior change only occurs after the treatment effect is administered to each successive person, behavior, or setting.
      5. This design avoids the problem of failure to revert to baseline that can exist with the A-B-A and A-B-A-B designs.
      6. For the design to be effective in evaluating treatment effects, target behaviors or participants must not be interdependent.
      7. Discussion Question: Discuss the differences between A-B-A, A-B-A-B, and multiple-baseline designs.
   4. Changing-Criterion Design: A single-case experimental design in which a participant’s behavior is gradually altered by changing the criterion for success over successive treatment periods. See Figure 13.15.
      1. Useful for shaping behavior over a period of time or when looking for a step-by-step increase in accuracy, frequency, or amount of something.
      2. This design involves collecting baseline data on the target behavior and then administering the experimental treatment condition across a series of intervention phases where each intervention phase uses a different criterion of successful performance until the desired criterion is reached.
      3. The criterion used in each successive intervention phase should be large enough to detect a change in behavior but small enough so that it can be achieved. See Figure 13.16.
      4. Successful use requires attention to
         1. Length of baseline and treatment phases: Treatments should be of different lengths but if they are the same, the baseline should be longer than treatment phases. This prevents the impact of a history or maturational variable at the same time as the criterion change. Each treatment phase should allow for behavior change to new criterion level as well as stabilization at that new level.
         2. The change in the criterion should be large enough to detect a behavioral change but small enough to be achieved by the participant.
         3. The number of changes in treatment criterion should be between two and four changes.
      5. Discussion Question: Describe a situation in which a changing-criterion design would be useful. Describe a situation in which it would not be useful.
   5. Methodological Considerations in Using Single-Case Designs (See Table 13.4)
      1. Baseline--The behavior of the participant before the administration of the experimental treatment condition. Baseline serves as the benchmark for assessing change induced by the experimental treatment condition, and to serve this purpose, it must be stable. A stable baseline is characterized by (a) an absence of trend (no increase or decrease) over time and (b) little variability (e.g., 5% variation or less).
      2. Changing One Variable at a Time--Only one variable should be changed from one phase of the experiment to the next. This is necessary to isolate the effect produced by that variable.
      3. Length of Phases--Agreement does not exist regarding the length of phases. Some researchers state that the various phases should be of equal length, but others emphasize that each phase should be continued until stability has been achieved.
      4. Assessment of Treatment Effect--There are two approaches to assessing treatment effects:
         1. Visual inspection--Looking at the pattern of outcomes across the phases is sufficient to identify a treatment effect if the baseline and intervention levels do not overlap or if the trend of the data in the baseline phase is different from the pattern in the intervention phase.
         2. Statistical analysis--A statistical analysis such as a time-series analysis is necessary if there is a great deal of variability in the data. In general, the statistical analysis is not needed if there is little variability in the data and the baseline pattern is very stable. When these two conditions do not exist, however, a statistical analysis should be used.