- Homogenous selection: select samples who are homogeneous.
- Building variables into the design: putting the extraneous variables into the study as independent variables and calculate the analysis of variance to determine its effect.
- Statistical control: using analysis of covariance (ANCOVA) to control for the effect of an extraneous variable known to be correlated with the dependent variable by statistically adjusting the mean scores for any initial differences between the groups on the pretest.
- Using subjects as their own controls: Assigning the same subjects to all experimental conditions and then obtaining measurements of the subjects.
- Controlling situational differences
  - Holding situational variables constant: all conditions in the groups are exactly alike except for the exposure to the independent variable. (e.g., wine testing)
  - Randomizing the treatment situations
  - Manipulating the situations systematically (practice/fatigue effect: control the order of treatment conditions through counterbalancing – first half with A->B order then the second with half B->A order)

Experimental Designs

Two-Group Experimental Designs

The simplest true experimental designs are two group designs involving one treatment group and one control group, and are ideally suited for testing the effects of a single independent variable that can be manipulated as a treatment. The two basic two-group designs are the pretest-posttest control group design and the posttest-only control group design, while variations may include covariance designs. These designs are often depicted using a standardized design notation, where R represents random assignment of subjects to groups, X represents the treatment administered to the treatment group, and O represents pretest or posttest observations of the dependent variable (with different subscripts to distinguish between pretest and posttest observations of treatment and control groups).

Pretest-posttest control group design. In this design, subjects are randomly assigned to treatment and control groups, subjected to an initial (pretest) measurement of the dependent variables of interest, the treatment group is administered a treatment (representing the independent variable of interest), and the dependent variables measured again (posttest). The notation of this design is shown in Figure 10.1.

R	O_1	X	O_2	(Treatment group)
R	O_3		O_4	(Control group)

Figure 10.1. Pretest-posttest control group design

The effect E of the experimental treatment in the pretest posttest design is measured as the difference in the posttest and pretest scores between the treatment and control groups:

E = (O2 – O1) – (O4 – O3)

Statistical analysis of this design involves a simple analysis of variance (ANOVA) between the treatment and control groups. The pretest posttest design handles several threats to internal validity, such as maturation, testing, and regression, since these threats can be expected to influence both treatment and control groups in a similar (random) manner. The selection threat is controlled via random assignment. However, additional threats to internal validity may exist. For instance, mortality can be a problem if there are differential dropout rates between the two groups, and the pretest measurement may bias the posttest measurement (especially if the pretest introduces unusual topics or content).

Posttest-only control group design. This design is a simpler version of the pretest-posttest design where pretest measurements are omitted. The design notation is shown in Figure 10.2.

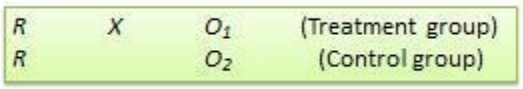

Figure 10.2. Posttest only control group design.

The treatment effect is measured simply as the difference in the posttest scores between the two groups:

E = (O1 – O2)

The appropriate statistical analysis of this design is also a two- group analysis of variance (ANOVA). The simplicity of this design makes it more attractive than the pretest-posttest design in terms of internal validity. This design controls for maturation, testing, regression, selection, and pretest-posttest interaction, though the mortality threat may continue to exist.

Covariance designs. Sometimes, measures of dependent variables may be influenced by extraneous variables called covariates. Covariates are those variables that are not of central interest to an experimental study, but should nevertheless be controlled in an experimental design in order to eliminate their potential effect on the dependent variable and therefore allow for a more accurate detection of the effects of the independent variables of interest. The experimental designs discussed earlier did not control for such covariates. A covariance design (also called a concomitant variable design) is a special type of pretest posttest control group design where the pretest measure is essentially a measurement of the covariates of interest rather than that of the dependent variables. The design notation is shown in Figure 10.3, where C represents the covariates:

R	C	X	O_1	(Treatment group)
R	C		O_2	(Control group)

Figure 10.3. Covariance design

Because the pretest measure is not a measurement of the dependent variable, but rather a covariate, the treatment effect is measured as the difference in the posttest scores between the treatment and control groups as:

E = (O – O2)

Due to the presence of covariates, the right statistical analysis of this design is a two-group analysis of covariance (ANCOVA). This design has all the advantages of post-test only design, but with internal validity due to the controlling of covariates. Covariance designs can also be extended to pretest-posttest control group design.

Factorial Designs. Two-group designs are inadequate if your research requires manipulation of two or more independent variables (treatments). In such cases, you would need four or higher-group designs. Such designs, quite popular in experimental research, are commonly called factorial designs. Each independent variable in this design is called a factor , and each sub-division of a factor is called a level . Factorial designs enable the researcher to examine not only the individual effect of each treatment on the dependent variables (called main effects), but also their joint effect (called interaction effects).

The most basic factorial design is a 2 x 2 factorial design, which consists of two treatments, each with two levels (such as high/low or present/absent). For instance, let's say that you want to compare the learning outcomes of two different types of instructional techniques (in-class and online instruction), and you also want to examine whether these effects vary with the time of instruction (1.5 or 3 hours per week). In this case, you have two factors: instructional type and instructional time; each with two levels (in-class and online for instructional type, and 1.5 and 3 hours/week for instructional time), as shown in Figure 8.1. If you wish to add a third level of instructional time (say 6 hours/week), then the second factor will consist of three levels and you will have a 2 x 3 factorial design. On the other hand, if you wish to add a third factor such as group work (present versus absent), you will have a 2 x 2 x 2 factorial design. In this notation, each number represents a factor, and the value of each factor represents the number of levels in that factor.

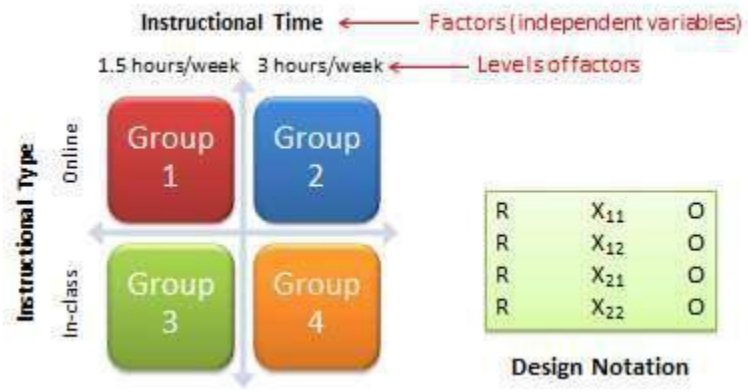

Figure 10.4. 2 x 2 factorial design

Factorial designs can also be depicted using a design notation, such as that shown on the right panel of Figure 10.4. R represents random assignment of subjects to treatment groups, X represents the treatment groups themselves (the subscripts of X represents the level of each factor), and O represent observations of the dependent variable. Notice that the 2 x 2 factorial design will have four treatment groups, corresponding to the four combinations of the two levels of each factor. Correspondingly, the 2 x 3 design will have six treatment groups, and the 2 x 2 x 2 design will have eight treatment groups. As a rule of thumb, each cell in a factorial design should have a minimum sample size of 20 (this estimate is derived from Cohen's power calculations based on medium effect sizes). So a 2 x 2 x 2 factorial design requires a minimum total sample size of 160 subjects, with at least 20 subjects in each cell. As you can see, the cost of data collection can increase substantially with more levels or factors in your factorial design. Sometimes, due to resource constraints, some cells in such factorial designs may not receive any treatment at all, which are called incomplete factorial designs . Such incomplete designs hurt our ability to draw inferences about the incomplete factors.

In a factorial design, a main effect is said to exist if the dependent variable shows a significant difference between multiple levels of one factor, at all levels of other factors. No change in the dependent variable across factor levels is the null case (baseline), from which main effects are evaluated. In the above example, you may see a main effect of instructional type, instructional time, or both on learning outcomes. An interaction effect exists when the effect of differences in one factor depends upon the level of a second factor. In our example, if the effect of instructional type on learning outcomes is greater for 3 hours/week of instructional time than for 1.5 hours/week, then we can say that there is an interaction effect between instructional type and instructional time on learning outcomes. Note that the presence of interaction effects dominate and make main effects irrelevant, and it is not meaningful to interpret main effects if interaction effects are significant.

Hybrid Experimental Designs

Hybrid designs are those that are formed by combining features of more established designs. Three such hybrid designs are randomized bocks design, Solomon four-group design, and switched replications design.

Randomized block design. This is a variation of the posttest-only or pretest-posttest control group design where the subject population can be grouped into relatively homogeneous subgroups (called blocks) within which the experiment is replicated. For instance, if you want to replicate the same posttest-only design among university students and full -time working professionals (two homogeneous blocks), subjects in both blocks are randomly split between treatment group (receiving the same treatment) or control group (see Figure 10.5). The purpose of this design is to reduce the "noise" or variance in data that may be attributable to differences between the blocks so that the actual effect of interest can be detected more accurately.

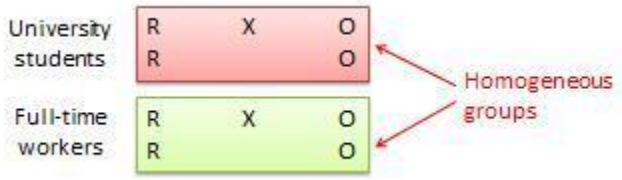

Figure 10.5. Randomized blocks design.

Solomon four-group design. In this design, the sample is divided into two treatment groups and two control groups. One treatment group and one control group receive the pretest, and the other two groups do not. This design represents a combination of posttest-only and pretest-posttest control group design, and is intended to test for the potential biasing effect of pretest measurement on posttest measures that tends to occur in pretest-posttest designs but not in posttest only designs. The design notation is shown in Figure 10.6.

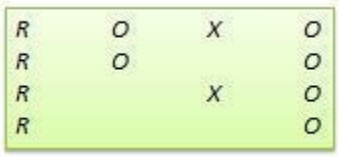

Figure 10.6. Solomon four-group design

Quasi-Experimental Designs

Quasi-experimental designs are almost identical to true experimental designs, but lacking one key ingredient: random assignment. For instance, one entire class section or one organization is used as the treatment group, while another section of the same class or a different organization in the same industry is used as the control group. This lack of random assignment potentially results in groups that are non-equivalent, such as one group possessing greater mastery of a certain content than the other group, say by virtue of having a better teacher in a previous semester, which introduces the possibility

of selection bias . Quasi-experimental designs are therefore inferior to true experimental designs in interval validity due to the presence of a variety of selection related threats such as selection-maturation threat (the treatment and control groups maturing at different rates), selection-history threat (the treatment and control groups being differentially impact by extraneous or historical events), selection-regression threat (the treatment and control groups regressing toward the mean between pretest and posttest at different rates), selection-instrumentation threat (the treatment and control groups responding differently to the measurement), selection-testing (the treatment and control groups responding differently to the pretest), and selection-mortality (the treatment and control groups demonstrating differential dropout rates). Given these selection threats, it is generally preferable to avoid quasi-experimental designs to the greatest extent possible.

Many true experimental designs can be converted to quasi-experimental designs by omitting random assignment. For instance, the quasi-equivalent version of pretest-posttest control group design is called nonequivalent groups design (NEGD), as shown in Figure 10.8, with random assignment R replaced by non-equivalent (non-random) assignment N . Likewise, the quasi -experimental version of switched replication design is called non-equivalent switched replication design (see Figure 10.9).

Figure 10.8. NEGD design.

Figure 10.9. Non-equivalent switched replication design.

In addition, there are quite a few unique non -equivalent designs without corresponding true experimental design cousins. Some of the more useful of these designs are discussed next.

Proxy pretest design. This design, shown in Figure 10.11, looks very similar to the standard NEGD (pretest-posttest) design, with one critical difference: the pretest score is collected after the treatment is administered. A typical application of this design is when a researcher is brought in to test the efficacy of a program (e.g., an educational program) after the program has already started and pretest data is not available. Under such circumstances, the best option for the researcher is often to use a different prerecorded measure, such as students' grade point average before the start of the program, as a proxy for pretest data. A variation of the proxy pretest design is to use subjects' posttest recollection of pretest data, which may be subject to recall bias, but nevertheless may provide a measure of perceived gain or change in the dependent variable.

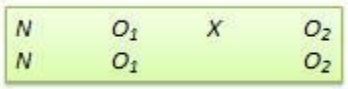

Figure 10.11. Proxy pretest design.

Nonequivalent dependent variable (NEDV) design. This is a single-group pre-post quasi-experimental design with two outcome measures, where one measure is theoretically expected to be influenced by the treatment and the other measure is not. For instance, if you are designing a new calculus curriculum for high school students, this curriculum is likely to influence students' posttest calculus scores but not algebra scores. However, the posttest algebra scores may still vary due to extraneous factors such as history or maturation. Hence, the pre-post algebra scores can be used as a control measure, while that of pre-post calculus can be treated as the treatment measure. The design notation, shown in Figure 10.13, indicates the single group by a single N , followed by pretest O1 and posttest O2 for calculus and algebra for the same group of students. This design is weak in internal validity, but its advantage lies in not having to use a separate control group.

Figure 10.13. NEDV design.

In summary, this chapter introduced key concepts in the experimental design research method and introduced a variety of true experimental and quasi-experimental designs. Although these designs vary widely in internal validity, designs with less internal validity should not be overlooked and may sometimes be useful under specific circumstances and empirical contingencies.

LICENSES AND ATTRIBUTIONS

CC LICENSED CONTENT, SHARED PREVIOUSLY

- Social Science Research: Principles, Methods, and Practices. **Authored by**: Anol Bhattacherjee. **Provided by**: University of South Florida. **Located at**: http://scholarcommons.usf.edu/oa_textbooks/3/. **License**: *CC BY-NC-SA: Attribution-NonCommercial-ShareAlike*