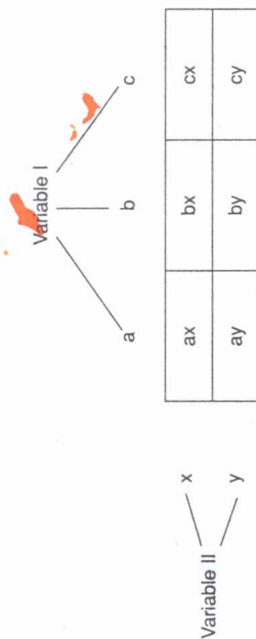


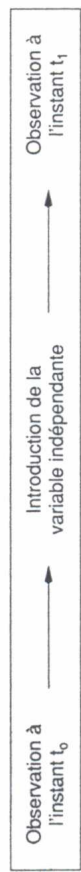
crédibilité de la source émettant les messages et des messages eux-mêmes, ces deux variables (ayant, respectivement, m et n modalités) étant plus ou moins corrélées selon les circonstances. Le *plan factoriel* qui sera alors utilisé prendra la forme d'un tableau comprenant $m \times n$ cases. Si $m = 3$ (a, b et c) et $n = 2$ (x et y), on aura ainsi six « croisements » possibles :



Cette exigence d'exhaustivité a pour contrepartie un coût très élevé. Il est des situations cependant où il semble opportun de négliger des informations jugées peu pertinentes par rapport à la problématique initiale, la « simplification » à laquelle on aboutit pouvant être représentée par un « plan en carré latin » (seule l'interaction des facteurs pris deux à deux est retenue). Soient trois variables : I, II et III ayant chacune quatre caractéristiques : d, e, f et g. On dénombre, par ce procédé, seize configurations (et non plus $4 \times 4 \times 4 = 64$) :

1. Id Ild Illd
2. Ie Ild Ille
3. If Ild Ilif
4. Ig Ild Ilig
5. Id Ile Ille
6. Ie Ile Ilif
7. If Ile Ilig
8. Ig Ile Illd
9. Id If Ilif
10. Ie If Ilig
11. If If Illd
12. Ig If Ille
13. Id Ig Ilig
14. Ie Ig Illd
15. If Ig Ille
16. Ig Ig Ilif

Lorsque l'on se place dans une perspective diachronique, l'une des techniques les plus couramment employées est celle de l'observation systématique :



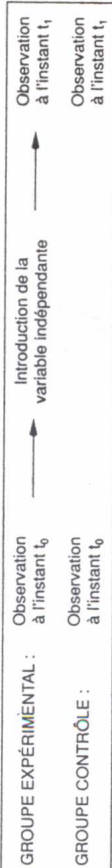
De nombreuses expériences relèvent de ce schéma. Ainsi, le professeur qui veut tester une méthode d'enseignement inédite sera-t-il amené à comparer les performances de ses élèves avant et après cette innovation. L'écart entre les deux évaluations le renseignera sur l'efficacité de la pédagogie mise en œuvre. Malheureusement, un tel protocole ne permet pas de tirer de conclusions définitives puisqu'il introduit plusieurs biais. Entre les deux estimations, des événements se sont produits, qui ont pu interférer avec les résultats obtenus. Outre l'usure de l'instrument, doivent être mentionnés la réactivité de la

mesure (certaines opérations, lorsqu'elles sont répétées à intervalles réguliers, induisent un apprentissage chez les sujets), les effets du temps sur les individus (processus de maturation) et la régression statistique (instabilité aléatoire propre à la population).

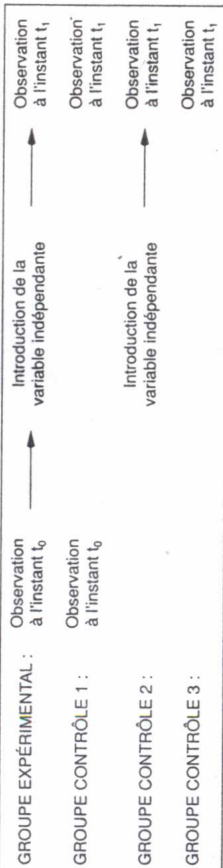
Dans la variante synchronique, on effectue les mêmes mesures sur un groupe contrôle non soumis au dispositif mis en place :



Là aussi, des variables parasites tenant à la nature et aux caractéristiques du phénomène étudié peuvent intervenir. Afin de remédier à ces inconvénients, I. Fisher eut l'idée de combiner les deux approches précédentes. D'où le schéma suivant :



Il arrive néanmoins que, très souvent, un entretien préalable sensibilise les personnes à un problème donné et fausse ainsi les informations recueillies. Pour surmonter cette difficulté, deux nouveaux groupes (n'ayant subi aucun prétest) sont introduits, conformément au « plan de Solomon » :



S'il est impossible de gagner sur les deux tableaux du concret et de la rigueur, cela ne veut pas dire que l'expérimentation soit vaine, mais qu'il faut bien situer ses limites et se garder de toute généralisation hâtive : ce qui est vrai d'un groupe restreint ne l'est pas nécessairement d'une communauté plus vaste. D'autant que la personnalité de l'observateur n'est pas sans influencer sur le déroulement des opérations. On sait ainsi que la mesure de la pression artérielle chez des malades hospitalisés donne des résultats différents selon qu'elle est effectuée par de jeunes internes ou par des chefs de service. De même, les attitudes et les jugements des enseignants peuvent être décisifs dans

tions, and control group comparisons. Seldom, however, and perhaps never, are we capable of eliminating all potential confounding variable effects in a given experiment. Rather, we attempt to eliminate the most obvious first, and through additional replication—extension studies attempt to verify that other possible confounding variables are not having an influential effect on the reported independent-dependent variable relation.

The assessment of cause and effect, or causality, in the study of independent-dependent variables relations requires several basic conditions. First, a temporal relation must be established between the independent and dependent variable, where the causal independent variable *must* precede the dependent variable. That is, if the antecedent is a cause of a consequence, the antecedent must precede the consequence. Second, to establish a causal relationship, the researcher must control for extraneous events that might confound the proposed independent-dependent relation. Finally, the scientist must demonstrate concomitant variation or demonstrate that variables *x* and *y* covary together. The more fully a researcher can meet these three basic conditions the more assurance one can have in realizing a cause-and-effect relationship.

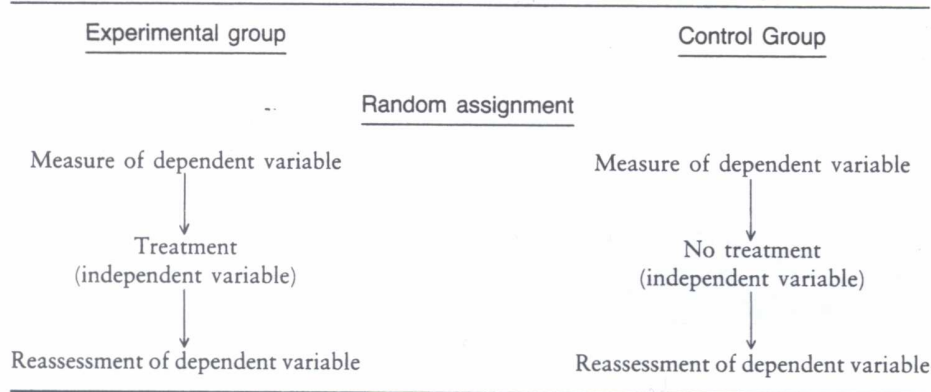
In summary, to document a cause-and-effect relationship, potential confounding or extraneous variable influences must be eliminated. These confounding variable influences are best removed through experimental control. We shall now turn to three of the most common research design techniques used in controlling and eliminating possible extraneous variable influences.

The Basic Experimental-Control Group Designs

For years Campbell and Stanley (1963; Cook & Campbell, 1979) have been cited as the classic review of experimental research design. Since little improvement on their excellent treatise has been made, we shall draw heavily on their work in outlining the three major experimental research designs. But prior to examining each individual design, we shall begin by outlining the major conceptual similarity between the three research designs. Table 7.1 provides this outline.

As mentioned earlier in CHAPTERS 1, 3 and 4, the independent and dependent variables are the workhorses of hypothesis testing. The independent variable is the variable that is directly manipulated and controlled by the researcher. The dependent variable is the measure of behavior. In using the experimental design, a formal hypothesis is constructed with some form of an “if-then” statement. The “if” refers to the independent variable and the “then” to the dependent variable. Therefore, the hypothesis is a formal statement indicating that “if” a certain independent variable is introduced to a group of people, “then” it is expected to have a certain effect or outcome (change in the dependent variable).

To test the if-then relation, a true experimental design must, at a minimum, have at least one *experimental* group and one *control* group. Simply put, the experimental group receives a specific treatment, which is controlled by the experimenter through manipulation of the independent variable. In contrast, the control group receives no such treatment and is allowed to behave in their own typical way, or is given an experience, devoid of immediate influential effects. Therefore, as can be seen in Table 7.1, the experimental process is a straightforward one. A sample of people is identified. Half are randomly placed in the experimental group. Individuals in both

Table 7.1. Basic Components of the Experimental-Control Group Design

groups are assessed on the dependent variable at the beginning of the study. The experimental group is given a treatment (i.e., the independent variable is introduced), while the control group is not directly manipulated. This process allows a comparison between a group that has a special treatment experience with one that has not. And finally, individuals in both groups are reassessed on the same dependent variable. Thus, the experimental laboratory process is complete. The matter of determining whether the experimental treatment has an “if-then” effect is a straightforward process. Through the use of statistical analyses, the experimenter then attempts to show that the scores on the average for the experimental group are different (either higher or lower, depending on the hypothesis) than those of the control group.

In summary, through the use of experimental and control groups, hypotheses are tested to determine if the independent variable has an effect on the dependent variable measure. Control groups allow us to determine if the independent variable had a true experimental effect, or if some extraneous variable might be confounding the proposed if-then relation. In our examination of three specific experimental designs we shall deal more with the issue of using control groups to eliminate confounding factors.

The Pretest–Posttest Control Group Design. This design is the most frequently employed experimental design in social science research. The power of this design is that it eliminates numerous extraneous confounding variables through its control group comparison. The design matches that outlined in Table 7.2.

To briefly describe the table we begin with two randomly determined groups. True randomization eliminates differences between the groups on the assessment of the dependent measure at Time 1 and thus assures equivalence between the two groups. However, the pretest at Time 1 allows a check for equivalence. The researcher can compare the average scores for the two groups to verify that they are approximately the same. Next, the time that elapses between Times 1 and 2 is held constant for both groups. However, the experimental group receives some special treatment prior to the posttest. Dependent variable measures for the two groups are then compared at Time 2.

But how does this design eliminate potential confounding variable effects?

Table 7.2. Basic Elements of the Pretest–Posttest Control Group Design

	Time 1		Time 2
Experimental group (R ^a)	Dependent variable(s)	Treatment (independent variable)	Dependent variable(s)
Control group (R)	Dependent variable(s)		Dependent variable(s)

^a R = randomization

nesso
effetto
della
univaria-
zione
della v.
dipenden-
te (PRETEST)
e dell'in-
tervallo
di tem-
po inter-
corso su
ENTRATI-
BI I GRUP-
PI (in
media)

Given the same amount of time elapses for both groups between the first and second test of the dependent variable, maturation and historical effects are ruled out. If such effects are influencing the behavior of individuals in each of the two groups, they should be doing so equally since the same amount of time has elapsed for both groups. But how about testing effects? Could the pretest be accounting for the changes from Time 1 to Time 2? While pretest effects might be influencing behavior, once again because both groups are having pretest experiences, the potential influence should be the same (on the average) for both groups. Further, the problem of subject selection is eliminated through random assignment to the two experimental groups, so that on the average each group is actually equivalent to the other. Therefore, the pretest–posttest control group design eliminates a number of potential confounding extraneous variables, including maturation, history, testing (instrumentation), and subject selection.

- 7.1 Let us turn, briefly, to an illustration of this design. Elaine Wagener (1976) has attempted to determine if the method of reading instruction could alter the self-concept of black children. Her hypothesis was that a program in black literature would enhance the self-concept of elementary schoolage children. As part of a larger experimental design, this educational researcher randomly placed Black-American children into an experimental literature program and a control group. Pretest and posttest scores were obtained using two measures of self concept. At the close of the program each group was compared on self-concept gains. For one of the two measures, there was a significant gain in self-concept attainment for the experimental group but not the control group. Therefore, the reader can conclude from this study that if black children experience a program on cultural heritage, wherein they read Black-American literature, they are likely to show gains in their personal worth, as measured by a self-concept scale. Given the design strengths, one can further eliminate the effects of history, maturation, testing, and selection of subjects as possible rival hypotheses (confounding variable) effects. Thus, such a design has strong internal validity as discussed in CHAPTER 4.

The Solomon Four-Group Design. An improvement on the pretest–posttest control group design is advanced in the Solomon four-group design technique. Specifically, through the addition of two more control groups, an increase in external validity is gained. This design is shown in Table 7.3.

The design includes the strength of the experimental and control groups of the pretest–posttest control group design, with the added utility of two additional groups. A second experimental group is included where the subjects who were

Table 7.3. Basic Elements of the Solomon Four-Group Design

	Time 1		Time 2
Experimental group 1 (R ^a)	Dependent variable(s)	→ Treatment →	Dependent variable(s)
Control group 1 (R)	Dependent variable(s)	→	Dependent variable(s)
Experimental group 2 (R)	NO PRETEST	Treatment →	Dependent variable(s)
Control group 2 (R)			Dependent variable(s)

^a R = randomization

randomly assigned to that experience are not given a pretest but do experience the experimental treatment and the Time 2 dependent variable measurement. Further, a second control group is included that is not given the pretest or the experimental treatment, but is measured during the final posttest period.

In addition to controlling for the possible confounding effects of maturation, history, testing, and subject selection, the additional two research groups help the researcher to eliminate the extraneous variable effect of testing and the possible interaction of testing with the treatment. In that the second experimental group did not receive a pretest, the measurement of the dependent variable at Time 2 is unaffected by any possible pretesting effects. Therefore, a comparison of the Time 2 measure between Experimental Groups 1 and 2 allows the researcher to assess for possible test–retest effects that might confound the independent–dependent variable relation. Further, the same comparison could be made between the two control groups to assess for possible test–retest effects.

An illustration of the utility of the Solomon four-group design is highlighted in a report by Harvey Parker (1974). Parker was specifically interested in the application of “contingency management” in classroom settings on elementary schoolage children’s self-concepts. Through a review of the research literature this investigator documented that effective use of classroom behavior modification techniques can lead to positive changes in children’s classroom behavior. However, he argued that such techniques should likewise have positive effects on children’s self-concepts, but such an assumption had not been adequately demonstrated by experimental research. Therefore, this investigator used a Solomon four-group design to test, over an 8-week period, whether contingency management by elementary school teachers can have a predicted positive effect on self-concept development of disruptive children. In the experimental groups, rewards were given for (1) raising a hand to peak, (2) staying in one’s seat, and (3) completing classroom work. Also, children who were unable to control themselves were sent to a “time-out booth” where they were isolated from their peers for short periods of time. The control group children were, in comparison, housed in classrooms of a traditional nature where contingency management was not utilized. On several of the self-concept posttest measures it was found that children who experienced the contingency management classroom experience showed more positive self-concepts than those children from the control groups. Thus, a comparison of the dependent variable measures from the Experi-

7.2

mental Groups 1 and 2 were appreciably higher than those from Control Groups 1 and 2. The investigator rightfully concludes that the application of behavior modification techniques with children can have a positive effect on children's self-concept development.

In summary, the Solomon four-group design is an improvement over the pretest-posttest control group design in that it can further eliminate possible testing or test x (by) treatment confounding effects. Also, it offers further credibility to the "if-then" relation through multiple experimental and control testing. Therefore, one would expect similar results between experimental groups, while differing from control groups—whose average scores would be expected to be similar. However, in all due honesty, this design is the least commonly used in social science research due to the extensiveness (and expense) of the technique.

The Posttest-Only Control Group Design. Campbell and Stanley (1963) have noted that while scientists are thoroughly entrenched with the security of using pretest measures (which can be compared to assure group equivalence at Time 1), the use of randomization in the placement of research subjects into the different experimental groups makes such a practice unnecessary. Randomization does serve the same purpose of pretesting for equivalence in eliminating initial biases between research groups. Further, on commonsense grounds, there are many cases in which pretesting is either unfeasible or too costly. Therefore, where true randomization is possible, the posttest-only control group design is an adequate substitute for the two research designs discussed previously. This design is shown in Table 7.4.

Subjects are randomly placed into 1 of 2 research groups based on a total random chance criterion. One group is given a treatment; the other is not. Both groups, at the same point in time, are given the assessment for the dependent variable.

7.3

For example, Margaret H. Thomas and Ronald S. Drabman (1978) were interested in the influence of television violence on children's expectations of aggressive behavior. Children were either placed into an experimental group where they watched a violent television series for 15 minutes or a control group where they observed a nonaggressive neutral "nature" series. Following these experiences, children from each of the two groups were assessed on their expectations of normative behavior. Children who saw the aggressive film were more likely than children who saw the nonaggressive nature series to view aggression as a normal, typical behavior in several hypothetical situations.

In summary, the posttest-only control group design, while not receiving as wide a recognition as it warrants, is an appropriate experimental design when complete randomization is possible. In particular, it is appropriate where pretesting is

Table 7.4. Basic Elements of the Posttest-Only Control Group Design

Experimental group (R^e)	Treatment	Dependent variable
Control group (R^c)		Dependent variable

^a R = randomization

unfeasible, and additionally it eliminates any possible contamination of pretest effects. (See Tips for Consumers 7.1.)

TIPS FOR CONSUMERS 7.1

The basic experimental research design has two fundamental groups. These two groups include:

1. An experimental group that is given a pretest and a posttest;
2. A control group that is given a pre- and posttest.

While several variations of this basic design are used, the two basic groups are an absolute necessity. However, placement into each group must be random to be maximally effective. Has the researcher drawn a sample of a known population? Were the subjects randomly placed into the two basic groups? Is there evidence that the experimental group made greater change from pre- to posttest assessments than did the control group? The basic notion of causality in an experimental design requires that (1) evidence of change be found from the pre- to posttest for the experimental group, and (2) substantial differences be observed in the posttest behavior between the experimental and control groups.

Between Versus Within Research Design Question

subject for changes in behavior over treatment conditions. For example, a between-subject design might be used in the study of the effects of dieting techniques. A group of volunteers might be randomly placed into either an exercise-oriented program or one using a special medical treatment. Average weight loss might be compared between the two dieting techniques. Conversely, a within-subject design might be used when a specific type of individual is being studied. Let us assume that we believe heavy males differ from females in their underlying reasons for obesity and that dieting may be better than exercising for one sex than the other. Thus, both obese males and females are introduced to both dieting techniques. The effectiveness of each technique is compared for each subject. Usually such a design requires a counterbalancing strategy. Half of each sex would experience the exercise program first, followed by the dieting procedure. The other half would experience the techniques in the reverse order. In this manner, the counterbalancing would allow the researcher to assess whether the order in which