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Alternatives in Analyzing the Solomon Four Group Design

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Abstract

This paper dealt with an alternative approach of a Solomon four group design. Earlier writings of Solomon and others have indicated that there should be a more sophisticated approach to the statistical analysis of this research design. The suggested approach presented in this paper allows one to take advantage of pre-test scores when they exist, thereby reducing the error term and making the analysis more powerful.

Introduction

Solomon (1949) first introduced the Four Group Design, citing the paradoxical situation presented by the experimental group-control group comparison strategy in use at that time; i.e., that comparisons of posttest scores on an experimental group having taken a pretest with one control group which has taken the pretest and a second control group which has not had the pretest actually may reduce the treatment effects as they were being measured. Solomon noted that "more sophisticated statistical procedures, such as an adaptation of the analysis of covariance...in particular the mathematical nature of...the interaction term, needs to be investigated" (p. 146). Thus he suggested what has come to be known as the Solomon Four Group Design, diagrammed below:

$$
\begin{array}{cccc}
R & O_1 & X & O_2 \\
R & O_3 & & O_4 \\
R & & X & O_5 \\
R & & & O_6
\end{array}
$$

Campbell and Stanley (1963) cite this design as the first consideration of external validity factors, and that "both the main effects of testing and the interaction of testing and X are determinable" (p. 25). This very powerful design has become frequently used, and often referenced. It would appear that there has tended to be more written and discussed on the design than on the statistical analysis utilized to answer the questions that can be reflected by this design.

Purpose

The purpose here is to demonstrate alternative strategies to analyzing the four group design that can add to the questions researchers may wish to investigate. For example, when (only) a two way analysis of variance is used to analyze Solomon type data there is much information available that is not being statistically addressed.

Alternative approaches are herein shown that utilize more of the information and may be able to reflect questions not considered previously. The analyses presented are based upon a conceptual work completed earlier by these authors (Newman, Benz & Williams, 1980). Solomon's 1949 statement is perhaps even more relevant today; i.e., that the

> Control group design seems to have awaited the development of statistical concepts which allow for the characterization of group performances in terms of measures of central tendency; and, psychologists seem to have been slow to combine statistical sophistication with experimental design. (p. 137)

Perhaps a more "statistically sophisticated" (in Solomon's terms) analysis can be suggested that adds to both the utility and the effectiveness of this research design.

Newman et al. (1980) earlier considered a repeated measures design while conducting t-tests among subjects, some of whom who had been pretested and some of whom who were not pretested. That research demonstrated an increase in power using what was termed the "independent-dependent simultaneous t-test." While this presentation is not concerned with t-tests, conceptually there is a similarity with the Solomon Four Group Design strategies, including writing models that reflect the research question using more of the available information than has typically been done. Williams and Newman (1982) earlier considered the Solomon Four Group Design to be a three-way analysis of variance with two empty cells.

It is useful to address the data as both a two way analysis of variance (experimental/control and pretested/ not pretested) and also as a psuedo-analysis of covariance, albeit the covariate is missing for two of the groups. The data in Table 1 is used in both analyses.

TABLE 1

Data for Analyzing Solomon Type Data for Two Way Analysis of Covariance and a Psuedo-Analysis of Covariance

Pre	Y	X_1	X_2	X_3	X_4	X_5	X_6
5	15	1	0	0	0	1	1
7	12	1	0	0	0	1	1
5	10	1	0	0	0	1	1
12	17	1	0	0	0	1	1
6	11	1	0	0	0	1	1
5	8	0	1	0	0	0	1
4	7	0	1	0	0	0	1
4	8	0	1	0	0	0	1
6	6	0	1	0	0	0	1
6	6	0	1	0	0	0	1
0	11	0	0	1	0	1	0
0	8	0	0	1	0	1	0
0	10	0	0	1	0	1	0
0	9	0	0	1	0	1	0
0	12	0	0	1	0	1	0
0	9	0	0	0	1	0	0
0	8	0	0	0	1	0	0
0	6	0	0	0	1	0	0
0	3	0	0	0	1	0	0
0	4	0	0	0	1	0	0

Where

Pre = the pretest score if present; 0 if no pretest score;

Y = the posttest score;

X_1 = 1 if a member of the experimental group that is pretested, 0 otherwise;

X_2 = 1 if a member of the control group that is pretested, 0 otherwise;

X_3 = 1 if a member of the experimental group that is pretested, 0 otherwise;

X_4 = 1 if a member of the control group that is not pretested, 0 otherwise;

X_5 = 1 if a member of either experimental group, 0 otherwise; and

X_6 = 1 if a member of either pretested group, 0 otherwise.

One of the various ways of accomplishing a two way analysis of variance is to use four linear models:

$$Y = b_0 + b_1X_1 + b_2X_2 + b_3X_3 + e_1; \qquad [1]$$

$$Y = b_0 + b_5X_5 + e_2; \qquad [2]$$

$$Y = b_0 + b_6X_6 + e_3; \text{ and} \qquad [3]$$

$$Y = b_0 + b_5X_5 + b_6X_6 + e_4, \qquad [4]$$

where the b_i^* are regression coefficients and are unique to each equation.

Focusing on the sums of squares, SS_1 = 150.00; SS_2 = 125.00; SS_3 = 20.00; and SS_4 = 145.00. Also SS_T = 224.00 and SS_W = 74.00. The interaction sum of squares is given by $SS_1 - SS_4$ = 150.00 - 145.00 = 5.00. These results can easily be incorporated into a summary table; see Table 2.

TABLE 2

Summary Table for the Two Way Analysis of Variance of Posttest Data in a Solomon Design

Source of Variation	df	SS	MS	F
Experimental-Control	1	125.00	125.00	27.03
Pretested-Not Pretested	1	20.00	20.00	4.32
Interaction	1	5.00	5.00	1.08
Within	16	74.00	4.625	

The thrust of the Solomon design is focused on testing the second and third listed sources of variation, whether or not a group was pretested and the interaction. Some might claim that the interaction effect may even be the more important test in a Solomon design. It is worthwhile to focus on the hypothesis tested as the interaction: $\bar{Y}_1 - \bar{Y}_2 = \bar{Y}_3 - \bar{Y}_4$. A reparameterization of equation 1 (a full model) is given:

$$Y = b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + e_1, \qquad [5]$$

then the hypothesis, in terms of the regression coefficients in equation 5 is:

$$b_1 - b_2 = b_3 - b_4 \text{ or } b_1 = b_3 + b_2 - b_4.$$

Imposing this restriction on equation 5 yields:

$$Y = (b_3 + b_2 - b_4)X_1 + b_2X_2 + b_3X_3 + b_4X_4 + e_5;$$

$$Y = b_2(X_2 + X_1) + b_3(X_3 + X_1) + b_4(X_4 - X_1) + e_5.$$

Letting $V_1 = X_2 + X_1$, $V_2 = X_3 + X_1$ and reparameterizing by letting $b_4 = 0$, $Y = b_0 + b_2V_1 + b_3V_2 + e_5$. [6]

The use of equation 6 yields SS_6 = 145.00, so that the interaction sum of squares would be $SS_1 - SS_6 = 150.00 - 145.00 = 5.00$, yielding the same sum of squares as previously found for interaction.

Considering a Psuedo-Analysis of Covariance

One approach to simultaneously using all the data is to use the pretest as a covariate for those individuals when a pretest is available. The linear model can be given as

$$Y = b_0 + b_1X_1 + b_2X_2 + b_3X_3 + b_pPre + e_6. \qquad [7]$$

What are the outcomes of using this psuedo-analysis of covariance? The pretest-posttest effect is partially nested in the covariate. If interest is centered on the adjusted means, adjusting for covariate differences for the groups that are pretested, but having the non-pretested group left alone, the adjusted means are identical to the adjusted means were the non-pretested groups completely eliminated from the analysis; in either case, the within regression coefficient is .55264. In making these covariate adjustments, care must be taken to avoid mechanically assuming that those who have not been pretested have a pretest score of zero and adjust accordingly (some computer programs in fact might do this). Any multiple comparison of interest can be done in the presence of the covariate for those pretested. If the interaction hypothesis is of interest, $\bar{Y}_1 - \bar{Y}_2 = \bar{Y}_3 - \bar{Y}_4$ which as before, translates to $b_1 = b_3 + b_2 - b_4$. A reparameterized full model is given in equation 8:

$$Y = b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_pPre + e_6. \qquad [8]$$

When the restriction $b_1 = b_3 + b_2 - b_4$ is imposed,

$$Y = (b_3 + b_2 - b_4)X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_pPre + e_7, \text{ or}$$

$$Y = b_2(X_2 + X_1) + b_3(X_3 + X_1) + b_4(X_4 - X_1) + b_pPre + e_7.$$

Letting $V_1 = X_2 + X_1$ and $V_2 = X_3 + X_1$ and reparameterizing by letting $b_4 = 0$ (all as before)

$$Y = b_0 + b_2V_1 + b_3V_2 + b_pPre + e_7. \qquad [9]$$

The hypothesis for overall experimental-control differences is given by $\bar{Y}_1 + \bar{Y}_3 = \bar{Y}_2 + \bar{Y}_4$; in terms of the regression coefficients, $b_1 + b_3 = b_2 + b_4$ or $b_1 = b_2 + b_4 - b_3$. Imposing this restriction on equation 8 yields $Y = (b_2 + b_1 - b_3)X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_pPre + e_8$, or $Y = b_2(X_2 + X_1) + b_3(X_3 - X_1) + b_4(X_4 + X_1) + b_pPre + e_8$. Letting $V_1 = X_2 + X_1$ and $V_3 = X_3 - X_1$, and reparameterizing by letting $b_4 = 0$,

$$Y = b_0 + b_2V_1 + b_3V_3 + b_pPre + e_8. \qquad [10]$$

To address the pretested-not pretested effect, the restriction, $b_1 + b_2 = b_3 + b_4$, or $b_1 = b_3 + b_4 - b_2$, corresponding to the hypothesis $\bar{Y}_1 + \bar{Y}_2 = \bar{Y}_3 + \bar{Y}_4$, can be placed on equation 8, yielding $Y = (b_3 + b_4 - b_2)X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_pPre + e_9$, and $Y = b_2(X_2 - X_1) + b_3(X_3 + X_1) + b_4(X_4 + X_1) + b_pPre + e_9$; letting $V_4 = X_2 - X_1$, $V_2 = X_3 + X_1$ and reparameterizing by letting $b_4 = 0$,

$$Y = b_0 + b_2V_4 + b_3V_2 + b_pPre + e_9. \qquad [11]$$

It should be pointed out that, though this test can be accomplished for the data at hand, a more useful test of this hypothesis could be completed if an independent covariate or covariates are available; if the pretest is used as a covariate, the pretesting effect is partially nested in the pretest scores used as a covariate. A model for the covariate can also be given:

$$Y = b_0 + b_P Pre + e_{10} \qquad [12]$$

A summary table for this pseudo-analysis of covariance can be formed; see Table 3. In Table 3, $SS_W = 62.39$ from the use of the full model (equation 7); $SS_7 = 161.61$. For the interaction, $SS_{INTERACTION} = SS_7 - SS_9$ (which yields 161.61 - 160.72, or $SS_{INTERACTION} = .89$. For the experimental control difference, $SS_{EXP/CONTROL} = SS_7 - SS_{10}$; $SS_{EXP/CONTROL} = 161.61 - 74.21 = 87.40$. The pretesting effect is given by $SS_7 = SS_{11} = 161.61 - 160.10 = 1.51$. The sum of squares for the covariate is given by $SS_{12} = 54.04$. These results are shown in Table 3.

TABLE 3

Summary Table for the Psuedo-Analysis of Covariance with a Solomon Design

Source of Variation	df	SS	MS	F
Covariate	1	54.04		
Pretest-No Pretest	1	1.51	1.51	.36
Experimental-Control	1	87.40	87.40	21.01
Interaction	1	.89	.89	.22
Within	15	62.39	4.16	

It should be clear that the summary table for this psuedo-analysis of covariance is not additive. Finally the adjusted means for the pretested groups can be found:

$\bar{Y}_1(adj) = \bar{Y}_1 - b_W(\bar{X}_1 - \bar{X}_T)$ or

$Y_1(adj) = 13 - .55264(7 - 6)$ or 12.45; for

$\bar{Y}_2(adj) = 7 - .55264(5 - 6)$ or 7.55.

Discussion and Conclusions

An essential issue for the Solomon Four Group Design is in regard to the experimenter's expectations in choosing the design. Is the design chosen as a panacea to rid the analysis of unwanted alternative interpretations, i.e., doesn't this design come with certain "warranties?" If so, choosing this design (or any other) is just another misstep in searching for the "holy grail." Alternative interpretations of literally any data analysis would seem not only to be a constant, but also a welcome constant, particularly to those who subscribe to Popper's view (as cited in Griffin, 1988) of scientists who actively seek evidence to refute their pet theories. Our own recommendation regarding data analysis (including the Solomon Four Group Design) is to first formulate the research process so that the precise questions of interest can be answered. Then state hypotheses and linear models that precisely address those questions. Beyond this, also recognize that a myriad of other issues can distort interpretations. In addition to the issues addressed in Campbell and Stanley (1963) and Cook and Campbell (1979), other concerns that may have different readings by other diligent investigators have to be considered, including issues regarding the criterion (or criteria)--do they in fact measure what they are claimed to measure? Do those who disagree with the use of a particular measure of a given construct as a measure of that construct have any validity in their arguments? Similar issues regarding experimental groups or definitions of the independent variables also come into play. In a more relativistic vein than is our practice, there probably are no final solutions; data and their interpretations would seem always to be subject to reanalysis and reinterpretation.

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