DOCUMENT RESUME

ED 292 835 TM 011 213

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TITLE	The Importance of A Priori Contrasts in Analysis of Variance Research.
PUB DATE	11 Nov 87
NOTE	22p.; Paper presented at the Annual Meeting of the Mid-South Educational Research Association (Mobile, AL, November 11, 1987).
PUB TYPE	Speeches/Conference Papers (150) -- Reports - Evaluative/Feasibility (142)
EDRS PRICE	MF01/PC01 Plus Postage.
DESCRIPTORS	*Analysis of Variance; *Research Methodology; *Statistical Analysis; Statistical Significance; Statistical Studies
IDENTIFIERS	*Orthogonal Comparison; *Planned Comparisons

ABSTRACT

The use of planned, or "a priori," and unplanned, or "post hoc," comparisons to isolate differences among means in analysis of variance research is discussed. Planned comparisons typically involve weighting data by sets of "contrasts." Planned comparison offer more power against Type II errors. In addition, they force the researcher to be more thoughtful in conducting research. It is suggested that these two reasons make planned comparisons superior to unplanned comparisons. It is further suggested that orthogonal tests are more useful than non-orthogonal tests. Planned comparisons can be used even when omnibus tests are not statistically significant. (SLD)

power.rno 11/9/87

ED292835

THE IMPORTANCE OF A PRIORI CONTRASTS
IN ANALYSIS OF VARIANCE RESEARCH

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ABSTRACT

The literature regarding the use of multiple comparisons in analysis of variance is reviewed. Two reasons why planned comparisons are generally superior to the use of unplanned or post hoc tests are presented. It is suggested that orthogonal tests are generally more useful than non-orthogonal tests. It is argued that planned comparisons can be used even when omnibus tests are not statistically significant, or in place of such tests. Use of planned comparisons tends to result in more thoughtful research with greater power against Type II error.

Empirical studies of research practice (Edgington, 1974; Goodw'n & Goodwin, 1985; Willson, 1980) indicate that the analysis of variance (ANOVA) methods presented by Fisher (1925) several generations ago remain popular with social scientists, notwithstanding withering criticisms of some of these applications (Cohen, 1968; Thompson, 1986). Most users of ANOVA-type methods (ANOVA, ANCOVA, MANOVA, MANCOVA--hereafter labelled OVA methods) are aware that "A researcher cannot stop his analysis after getting a significant $\underline{F}$; he must locate the cause of the significant $\underline{F}$" (Huck, Cormier & Bounds, 1974). Gravetter and Wallnau (1985, p. 423) concur that "Reject Ho indicates that at least one difference exists among the treatments. With $\underline{k}$ [means] = 3 or more, the problem is to find where the differences are." Moore (1983, p. 299) suggests that:

> If we have statistical significance when we have only two groups, and thus only two means, we can visually inspect the data to determine which group performed better than the other. But when we have three or more groups, we need to investigate specific mean comparisons.

Many researchers employ unplanned (also called a posteriori or post hoc) multiple comparison tests (e.g., Scheffe, Tukey, or Duncan) to isolate means that are significantly different within OVA ways (also called factors) having more than two levels. As Glass and Hopkins (1984, p. 368) note,

> MC procedures are a relatively recent addition to the statistical arsenal; most MC techniques were developed during the 1950's, although their use in

behavioral research was rare prior to the 1960's. Textbook authors tend to discuss unplanned comparison or contrast procedures in a somewhat prejorative terms. For example, Kirk (1984, p. 360) speaks of the use of unplanned comparisons as "ferreting out significant differences among means, or, as it is often called, data snooping." The following quotations are additional representatives of this genre of views:

> Techniques that have been developed for data snooping following an over-all [significant omnibus] F test... are referred to as a posteriori or post hoc tests. (Kirk, 1968, p. 73)

> The post hoc method is suited for trying out hunches gained during the data analysis. (Hays, 1981, p. 439)

> Post hoc comparisons, on the other hand, enable the researcher to engage in so-called data snooping by performing any or all of the conceivable comparisons between means. (Pedhazur, 1982, p. 305)

> Prior to running the experiment, the investigator in our example had no well-developed rationale for focusing on a particular comparison between means. His was a "fishing expedition"... Such comparisons are known as post hoc comparisons, because interest in them is developed "after the fact"--it is stimulated by the results obtained, not by any

prior rationale. (Minium & Clarke, 1982, p. 321)

> Post hoc comparisons often take the form of an intensive "milking" of a set of results--e.g., the comparison of all possible pairs of treatment means. (Keppel, 1982, p. 150)

> Post hoc comparisons are made in accordance with the serendipity principle--that is, after conducting your experiment you may find something interesting that you were not initially looking for. (McGuigan, 1983, p. 151)

Planned (also called a priori) comparisons provide an alternative to the OVA user who is interested in isolating differences among means. As Keppel (1982, p. 164) notes in his excellent treatment, decisions about which unplanned or planned comparisons to employ in OVA research are complex and not always well understood by researchers:

> The fact that there is little agreement among commentators writing in statistical books and articles concerning specific courses of action to be followed with multiple comparisons simply means that the issues _are_ complex, and that no single solution can be offered to meet adequately the varied needs of researchers. Consequently, you should view the situation... with a realization that you _must_ work the problem out for yourself.

The purpose of the present paper is to acquaint the reader with

some of these complex issues, and to argue that planned comparisons should be employed more frequently in OVA research.

Rationale Underlying Unplanned Comparisons

Most contemporary researchers recognize that

> *t*-tests performed on all possible pairs of means involved in the *F*-test... [to] reveal where significant differences between means lie... is quite unacceptable methodology. The *t*-test was not designed for this use and is invalid when so applied... In spite of the patent invalidity of *t*-testing following a significant *F*-ratio in the analysis of variance, or multiple *t*-testing in lieu of the analysis of variance, this method has often been and continues to be used. (Glass & Stanley, 1970, p. 382)

However, not all researchers understand the basis for these conclusions. The rationale involves the control of experimentwise Type I error rate, and thus requires an understanding of the nature of experimentwise error rate.

When a researcher conducts a study in which only one hypothesis is tested, the Type I error probability is the nominal alpha level selected by the researcher, i.e., often the 0.05 level of statistical significance. The probability of making a Type I error when testing a given hypothesis is called the testwise error rate. Experimentwise error rate refers to the cumulative probability that a Type I error was made somewhere in the full set of hypothesis tests conducted in the study overall.

In the case of a study in which only one hypothesis is tested, the testwise error rate exactly equals the experimentwise error rate.

However, when several hypotheses (e.g., two main effect and one interaction effect) are tested within a single study, the experimentwise error rate may not equal the nominal testwise alpha level used to test each of the separate hypotheses. Witte (1985, p. 236) provides an analogy that may clarify why this is so:

> When a fair coin is tossed only once, the probability of heads equals 0.50--just as when a single _t_ test is to be conducted at the 0.05 level of significance, the probability of a type I error equals 0.05. When a fair coin is tossed three times, however, heads can appear not only on the first toss but also on the second or third toss, and hence the probability of heads on _at least one_ of the three tosses exceeds 0.50. By the same token, when a type I error can be committed not only on the first test but also on the second or third test, and hence the probability of committing a type I error on _at least one_ of the three tests exceeds 0.05. In fact, the cumulative probability of at least one type I error can be as large as 0.15 for this series of three _t_ tests.

In fact, as Thompson (in press) explains, the experimentwise error ate would range somewhere between the nonimal testwise

alpha level and (1 - (1 - testwise alpha) raised to the power of the number of hypotheses tested. For example, if nine hypotheses were each tested at the 0.05 level in a single study, the experimentwise error rate would range between 0.05 and 0.37.

Experimentwise error rate is at a maximum when the hypotheses tested within an experiment are orthogonal or uncorrelated. For example, the tests of all omnibus hypotheses in a factorial multi-way ANOVA with equal numbers of subjects in each cell are all uncorrelated. This is why the sums of squares (SOS) for each effect plus the error SOS add up to exactly equal the SOS total. Thus, in a 3x4 ANOVA in which both main effect and the one two-interaction omnibus hypotheses are tested at the 0.05 level, the experimentwise error rate would be about 0.14.

Unplanned comparisons incorporate a correction (Games, 1971a, 1971b) that minimizes the inflation of experimentwise error rate as a function of conducting more hypothesis tests in a single study, especially given that omnibus hypotheses have already been tested. As Horvath (1985, p. 223) notes, "Performing a multitude of comparisons between the treatments raises the spectre of an increased overall probability of a Type I error. Post _F_-test procedures must include some accomodation for this danger." As Kirk (1984, p. 360) explains,

> The principal advantage of this multiple comparison procedure over Student's _t_ is that the probability of erroneously rejecting one or more null hypotheses doesn't increase as a function of the number of hypotheses tested. Regardless of the number of tests performed among _p_ means, this

probability remains equal to or less than alpha for the collection of tests.

Snodgrass, Levy-Berger and Haydon (1985, p. 386) note that:

> The post hoc tests for such multiple comparisons all adjust, to one degree or another, for the increase in the probability of a Type I error as the number of comparisons in increased. They differ in the degree to which the probability of a Type I error is reduced.

The authors discuss which tests are more conservative in this adjustment and which are more liberal.

Planned Comparison Procedures

Planned comparisons are the alternative to unplanned comparisons for researchers who wish to isolate differences between sets of specific means. Pedhazur (1982, chapter 9) a Loftus and Loftus (1982, chapter 15) provide valuable explanations of these methods. Various types of planned comparisons can be used, including both orthogonal and non-orthogonal planned comparisons. Planned comparisons typically involve weighting data by sets of "contrasts" such as those presented by Thompson (1985a) or the contrasts presented in Table 1. Other types of contrasts, those which test for trends in means, are provided by Fisher and Yates (1957, pp. 90-100) and by Hicks (1973) for various research designs.

INSERT TABLE 1 ABOUT HERE.

Contrasts are typically developed to sum to zero, as do all

five contrasts presented in Table 1. Contrasts are uncorrelated or orthonogonal (and the hypotheses they represent likewise) when the contrasts each sum to zero and when the cross-products of each pair of contrasts all sum to zero also. Thus, the contrasts presented in Table 1 are all uncorrelated. Planned contrasts are employed in a regression analysis in the manner illustrated by Thompson (1985a) and as explained by Pedhazur (1982). The required computer cards for this case are presented in Appendix A.

The number of orthogonal planned comparisons always equals the number of degrees of freedom for a given effect. As Hays (1981, p. 425) notes,

> Each and every degree of freedom associated with treatments in any fixed-effects analysis of variance corresponds to some possible comparison of means. The number of degrees of freedom for the mean square between is the number of possible <u>independent</u> [i.e., orthogonal] comparisons to be made on the means.

Some researchers do not believe that planned comparisons should necessarily be orthogonal. For example, Winer (1971, p. 175) argues that, "In practice the comparisons that are contructed are those having some meaning in terms of the experimental variables; whether these comparisons are orthogonal or not makes little or no difference."

However, most researchers believe that orthogonal planned comparisons have special appeal. Kachigan (1986, p. 309) notes that:

> The importance that we place on a set of orthogonal comparisons is that both of these [individual test and experimentwise] significance levels is known to us... On the other hand, when we deal with sets of unplanned non-orthogonal comparisons, these probabilities are not generally available to us, because of the unplanned nature of the comparisons, and because of the non-independence among them.

Keppel (1982, p. 147) suggests that:

> The value of orthogonal comparisons lies in the independence of inferences, which, of course, is a desirable quality to achieve. That is, orthogonal comparisons are such that any decision concerning the null hypothesis representing one comparison is uninfluenced by the decision concerning the null hypothesis representing any other orthogonal comparison. The potential difficulty with nonorthogonal comparisons, then, is interpreting the different outcomes. If we reject the null hypotheses for two nonorthogonal comparisons, which comparison represents the "true" reason for the observed differences?

Two Reasons Why Planned Comparisons are Superior

There are two reasons why researchers generally prefer the use of planned comparisons to the use of unplanned comparisons. First, as noted by numerous researchers, planned comparisons

offer more power against making Type II errors:

> procedures recommended for a priori orthogonal comparisons are more powerful than procedures recommended for a priori nonorthogonal and a posteriori comparisons. That is, the former procedures are more likely to detect real differences among means. (Kirk, 1968, p. 95)

> The probability of test's detecting that... [the contrast's effect] is not zero [i.e., is statistically significant] is greater with a planned than with an unplanned comparison on the same sample means. Thus, for any particular comparison, the test is more powerful when planned than when post hoc. (Hays, 1981, p. 438)

> Post hoc tests protect us from making too many Type I errors by requiring a bigger difference before declaring it to be significant than do planned comparisons. But this protection tends to be too conservative for planned comparisons, thereby lowering the power of the test. (Minium & Clarke, 1982, p. 322)

> The tests of significance for a priori, or planned, comparisons are more powerful than those for post hoc comparisons. In other words, it is possible for a specific comparison to be not significant when tested by post hoc methods but

> significant when tested by a priori methods. (Pedhazur, 1982, pp. 304-305)

> Post hoc comparisons must always follow the finding of a significant overall *F*-value... There are no limits to the number of combinations that can be tested post hoc, but none of these procedures has the power of planned comparison tests for detecting statistical significance. (Sowell & Casey, 1982, p. 119)

> The test of planned subhypotheses is more powerful than the test of post hoc subhypotheses. For this reason, we should make planned comparisons whenever possible in planning the design of research within the ANOVA context. (Glasnapp & Poggio, 1985, p. 474)

Second, and perhaps even more importantly, <u>planned comparisons tend to force the researcher to be more thoughtful in conducting research</u>, since the number of planned comparisons that can be tested is limited by the number of degrees of freedom for an effect, as noted previously. As Snodgrass, Levy-Berger and Haydon (1985, p. 386) suggest, "The experimenter who carries out post hoc comparisons often has a rather diffuse hypothesis about what the effects of the manipulation should be." Keppel (1982, p. 165) notes that, "Planned comparisons are usually the motivating force behind an experiment. These comparisons are targeted from the start of the investigation and represent an interest in

particular combinations of conditions--not in the overall experiment." In summary, as Kerlinger (1986, p. 219) suggests, "While post hoc tests are important in actual research, especially for exploring one's data and for getting leads for future research, the method of planned comparisons is perhaps more important scientifically."

Use of Planned Comparisons in Lieu of Omnibus Tests

Some researchers suggest that at least some unplanned comparisons can be made even if an omnibus effect is not statistically significant. For example, Spence, Cotton, Underwood and Duncan (1983, p. 215) suggest that,

> The Tukey hsd [honestly significant difference test] usually is performed only if the *F* obtained in the analysis of variance is significant, but it theoretically permissible to perform whatever the significance of *F*.

Similarly, Hays (1981, p. 434) notes:

> This statement is not to be interpreted to mean that post hoc comparisons are somehow illegal or immoral if the original *F* test is not significant at the required alpha level... What one cannot do is to attach an unequivocal probability statement to such post hoc comparisons, unless the conditions underlying the method have been met.

However, the preponderant view regarding use of unplanned post hoc tests is expressed by Gravetter and Wallnau (1985, p. 423):

> These [a posteriori] tests attempt to control the

overall alpha level by making the adjustments for the number of different samples (potential comparisons) in the experiment. To justify a posteriori tests, the *F*-ratio from the overall ANOVA must be significant.

On the other hand, with respect to the use of planned comparisons, "Most statisticians agree that planned *t* tests between means are appropriate, even when the overall *F* is insignificant" (Clayton, 1984, p. 193). Snodgrass, Levy-Berger and Haydon (1985, p. 386) concur:

> For planned comparisons, it is not necessary for the overall ANOVA to be significant in order to carry them out... Post hoc comparisons, on the other hand, may not be carried out unless the overall ANOVA is significant.

Gravetter and Wallnau (1985, p. 423) agree that, "Planned comparsions can be made even when the overall *F*-ratio is not significant."

In fact, "It is not necessary to perform an over-all test of significance prior to carrying out planned orthogonal *t* tests" (Kirk, 1968, p. 73). As Hays (1981, p. 426) suggests,

> The *F* test gives evidence to let us judge if all of a set of *J* - 1 such orthogonal comparisons are simultaneously zero in the populations. For this reason, if planned orthogonal comparisons are tested separately, the overall *F* test is not carried out, and vice versa.

Swaminathan (in press) presents the same arguement with respect to the MANOVA case:

> The often advocated procedure of following up the rejection of the null hypothesis with a more powerful multiple comparison procedure should be discouraged. First, the overall rejection of the null hypothesis does not guarantee any meaningful contrast among the means will be significant, as our example showed. Second..., signficant contrasts may be found even when the null hypothesis would not have been rejected. Third, follow up multiple comparison procedures which are unrelated to the overall test result in an inflation of the experiment-wise error rate. If multiple comparisons are of primary interest, a suitable multiple comparison procedure can be used without first performing an overall test.

A Concrete Heuristic Example

Just as some researchers benefit from seeing heuristic demonstrations that all parametric significance testing procedures are subsumed by and can be conducted with canonical correlation analysis (Thompson, 1985b), it may be helpful to present a hypothetical analysis demonstrating the utility of planned orthogonal comparisons using the data presented in Table 1. Table 2 presents a conventional one-way ANOVA keyout associated with the Table 1 data. Even if the researcher conducted unplanned post hoc tests in the absence of a

statistically significant main effect, none of the unplanned tests would result in a statistically significant comparison. However, as noted in Table 3, a statistically significant ($p < 0.01$) result is isolated for the hypothesis that the mean attitude-toward-school score of the two school board members differs from the mean for the remaining 10 subjects.

INSERT TABLES 2 AND 3 ABOUT HERE.

Summary

The literature regarding the use of multiple comparisons in analysis of variance is reviewed. Two reasons why planned comparisons are generally superior to the use of unplanned or post hoc tests are presented. It is suggested that orthogonal tests are generally more useful than non-orthogonal tests. It is argued that planned comparisons can be used even when omnibus tests are not statistically significant, or in place of such tests. Use of planned comparisons tends to result in more thoughtful research with greater power against Type II error.

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Table 1
Hypothetical Data for Attitudes Toward School Study (n=12)

Group	LEVEL	ID	DV	Contrast C1	C2	C3	C4	C5
Students	1	1	10	0	0	0	0	-1
		2	20	0	0	0	0	-1
Teacher Aides	2	3	10	0	0	0	-1	-1
		4	20	0	0	0	-1	-1
Teachers	3	5	10	0	0	-1	-1	-1
		6	20	0	0	-1	-1	-1
Principals	4	7	10	0	-1	-1	-1	-1
		8	20	0	-1	-1	-1	-1
Superintendents	5	9	10	-1	-1	-1	-1	-1
		10	20	-1	-1	-1	-1	-1
Board Members	6	11	25	1	2	3	4	5
		12	35	1	2	3	4	5

Table 2
One-Way ANOVA Results

Source	SOS	df	Mean Square	F	p	Eta Square
Between	375.0000	5	75.0000	1.5000	.3155	.55556
Error	300.0000	6	50.0000			
Total	675.0000	11				

Table 3
Planned Comparison Results

Contrast Source	SOS	df	Mean Square	F	p	Eta Square
C1	.0000	1	.0000	0.0000		.00000
C2	.0000	1	.0000	0.0000		.00000
C3	.0000	1	.0000	0.0000		.00000
C4	.0000	1	.0000	0.0000		.00000
C5	375.0000	1	375.0000	12.5000	.0054	.55556
Error	300.0000	6	50.0000			
Total	675.0000	11				

APPENDIX A

Selected SPSS-X Control Cards

```
TITLE '*****OMNIBUS no  POSTHOC no  A PRIORI yes'
FILE HANDLE BT/NAME='APRIORI.DTA'
DATA LIST FILE=BT/LEV 1 DV 2-4
COMPUTE C1=0
COMPUTE C2=0
COMPUTE C3=0
COMPUTE C4=0
COMPUTE C5=0
IF (LEV EQ 6)C1=1
IF (LEV EQ 5)C1=-1
IF (LEV EQ 6)C2=2
IF (LEV EQ 4 OR LEV EQ 5)C2=-1
IF (LEV EQ 6)C3=3
IF (LEV GT 2 AND C3 EQ 0)C3=-1
IF (WAY EQ 6)C4=4
IF (WAY GT 1 AND C4 EQ 0)C4=-1
IF (WAY EQ 6)C5=5
IF (C5 EQ 0)C5=-1
REGRESSION VARIABLES=DV C1 TO C5/DESCRIPTIVES=ALL/
  CRITERIA=PIN(.95) POUT(.999) TOLERANCE(.00001)/DEPENDENT=DV/
  ENTER C5/ENTER C4/ENTER C3/ENTER C2/ENTER C1/
```