DOCUMENT RESUME

ED 053 170 TM 000 689

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TITLE	Analysis of Covariance for Nonrandomized Data.
INSTITUTION	Educational Testing Service, Princeton, N.J.
REPORT NO	RB-71-27
PUB DATE	May 71
NOTE	20p.
EDRS PRICE	EDRS Price MF-$0.65 HC-$3.29
DESCRIPTORS	*Analysis of Covariance, *Data Collection, *Educational Research, Hypothesis Testing, Mathematical Applications, *Mathematics, Multiple Regression Analysis, *Psychological Studies, Sampling, Statistical Analysis

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Educational Testing Service
Princeton, New Jersey
May 1971

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Considerable attention has been given lately to analysis of covariance, particularly in educational research where several factors of factorial design are fixed and some factors (or none) are randomized. The journal, Biometrics, dedicated the September 1957 issue to the problem of analysis of covariance. Recently, Lord (1967) presented the problems of this type of analysis as a paradox, and Campbell and Erlebacher (1970) presented some of the follies in use of the procedure. It is the intention of this paper to review the purpose, underlying assumptions, and calculation procedures and to arrive at a means of handling analysis of covariance when the classical assumptions are not met.

Definition of the Problem

As expressed by Cochran (1957, p. 262), a covariate "is a measurement taken on each experimental unit before treatments are applied, which is thought to predict to some degree the final response...on that unit." In experimentation, a covariate is usually a measurement taken of some factor which influences the experiment but cannot be controlled.

The analysis of covariance is a statistical technique for eliminating the effect of the covariate on the response variate(s) being studied. The mathematics of the analysis of covariance is somewhat complex and requires using the figures shown in Table 1. In the table, S denotes Sum, H, Hypothesis, E, errors, T, totals, Y, the response variate, X, the covariate, and an * denotes an "adjusted" sum of squares. β denotes the various regression coefficients.

Table 1

Various Sums of Squares Used in Analysis of Covariance

Source	Variate	Cross-product	Covariate	Regression Coefficient
Hypothesis	$S_H\ (y^2)$	$S_H\ (xy)$	$S_H\ (x^2)$	β_H
Errors	$S_E\ (y^2)$	$S_E\ (xy)$	$S_E\ (x^2)$	β_E
Total	$S_T\ (y^2)$	$S_T\ (xy)$	$S_T\ (x^2)$	β_T

As is customary $S_T = S_E + S_H$.

The calculations usually proceed as follows:

First calculate the adjusted sum of squares for totals

$$S_T^*\ (y^2) = S_T\ (y^2) - S_T^2\ (xy)/S_T\ (x^2)$$

designating

$$\beta_T = S_T\ (xy)/S_T\ (x^2) \quad .$$

Then calculate the adjusted sum of squares for errors

$$S_E^*\ (y^2) = S_E\ (y^2) - S_E^2\ (xy)/S_E\ (x^2)$$

designating

$$\beta_E = S_E\ (xy)/S_E\ (x^2) \quad .$$

Then find

$$S_H^* (y^2) = S_T^* (y^2) - S_E^* (y^2) \quad .$$

$$\beta_H = S_H (xy)/S_H (x^2)$$

is not calculated ordinarily.

The appropriate statistical test is then formed,

$$F = \text{const.} \ \ S_H^* (y^2)/S_E^* (y^2),$$

the probability of F found and a decision made.

This method is espoused by Cochran (1957), Scheffé (1959), Rao (1965), and most others and will be called here the classical method. The method was apparently devised by Wishart and Sanders (1935). It was first reported by Fisher (1932).

Mathematics

In order to examine the mathematics of covariance analysis, we shall use the method of fitting constants as devised by Fisher (1932) and consolidated by Yates (1933) and Wilks (1938).

Let us use as an example a design with two groups and a covariate. We construct two pseudovariables (dummy parameters or design variables), one which is 1 for all observations and serves to extract the grand mean (constant term) from the observations, and one which is +1 for all observations in the first group and -1 for all observations in the second group and serves to produce the calculations for the t test.[1] The covariate will be x and

[1]Wilks would have used three pseudovariables, the constant term, and two variables for group membership. The two group membership variables were set up as 1 for the instant group and 0 for all other groups.

the variate y . Each observation vector then consists of

$$(1, x, \pm 1, y) \ .$$

The sum of the cross-products matrix will then be[2]

$$A = \begin{bmatrix} N & \Sigma x & n_1 - n_2 & \Sigma y \\ & \Sigma x^2 & \Sigma x_1 - \Sigma x_2 & \Sigma xy \\ & & n_1 + n_2 & \Sigma y_1 - \Sigma y_2 \\ (\text{symmetric}) & & & \Sigma y^2 \end{bmatrix} .$$

We will assume orthogonality of design to make the discussion easier; this requires $n_1 - n_2 = 0$. Constant fitting is usually done by Gaussian reduction (sweep out). Constants are usually fitted in the order in which they appear in the observation vector only to simplify notation.

To fit the grand mean to the second row of A (sweep out μ), form the regression coefficient $\Sigma x/n$ and make the following calculations

$$\Sigma x^2 - (\Sigma x/n)\ (\Sigma x)\ ;\ (\Sigma x_1 - \Sigma x_2) - (\Sigma x/n)\ (n_1 - n_2)\ ,\ \text{and}$$

$$\Sigma xy - (\Sigma x/n)\ \Sigma y \ .$$

The reduction process of any entry a_{ij} of A for any constant k is $a_{ij} - (a_{ki}/a_{kk})\ (a_{jk})$. It is obvious that the regression coefficient

[2]Notation conventions: Subscripts denote sample 1 or sample 2, and lack of subscripts denotes all observations. For example $N = n_1 + n_2$ and Σxy_1 denotes the sum of cross-products over group 1.

for variable k on variable i is a_{ki}/a_{kk} . For later use we will show the sweep of μ from all of A giving

$$B = \begin{bmatrix} N & 0 & 0 & 0 \\ 0 & \Sigma x^2 - (\Sigma x)^2/N & \Sigma x_1 - \Sigma x_2 & \Sigma xy - \Sigma x\Sigma y/N \\ 0 & & n_1 + n_2 & \Sigma y_1 - \Sigma y_2 \\ 0 & (\text{symmetric}) & & \Sigma y^2 - (\Sigma y)^2/N \end{bmatrix} .$$

After sweeping the second row and column of B we arrive at a matrix C which has relevant entries

$$C_{33} = (n_1 + n_2) - (\Sigma x_1 - \Sigma x_2)^2/(\Sigma x^2 - (\Sigma x)^2/N)$$

$$C_{34} = (\Sigma y_1 - \Sigma y_2) - [(\Sigma x_1 - \Sigma x_2)/(\Sigma x^2 - (\Sigma x)^2/N)][\Sigma xy - \Sigma x\Sigma y/N]$$

$$C_{44} = (\Sigma y^2 - (\Sigma y)^2/N) - (\Sigma xy - \Sigma x\Sigma y/N)^2/(\Sigma x^2 - (\Sigma x)^2/N) .$$

With sufficient labor, it can be seen that

$$SS_T^* = C_{44}$$

$$SS_H^* = C_{34}^2/C_{33}$$

and $\beta_T = b_{24}/b_{22}$.

After sweeping row 3 out of C, what remains in the (4,4) position is $S_E^*(y^2)$.

The constant β_E does not appear in this calculation sequence but can be calculated as follows.

Sweep row 3 of B out of rows 2 and 4 giving a matrix D with elements

$$d_{22} = (\Sigma x^2 - (\Sigma x)^2/N) - (\Sigma x_1 - \Sigma x_2)^2/(n_1 + n_2)$$

$$d_{24} = (\Sigma xy - \Sigma x\Sigma y/N) - [(\Sigma x_1 - \Sigma x_2)/N][\Sigma y_1 - \Sigma y_2] \quad .$$

Without too much labor it can be shown that

$$\beta_E = d_{24}/d_{22} \quad .$$

Statistical Estimation of Regression Coefficients

The usual design restriction placed on the covariate is that it be "unaffected by treatment." This statement has led to several practices. One is that of taking covariate measurements before experimental treatments are applied to the groups. When this is done it is expected that the subjects will be assigned at random to treatment groups. This will leave Σx_1 approximately equal to Σx_2 so that $\Sigma x_1 - \Sigma x_2 = \varepsilon$ and $\varepsilon \approx 0$ except for random fluctuations. When subjects are assigned at random to treatments it is safe to assume that covariate values are distributed at random to treatments also. This leads Cochran (1957, p. 264) to state that "it is important to verify that treatments have had no effect on x [i.e., the covariate]. This is obviously true when the x's were measured before treatments were applied." A caution is taken by the observation that Cochran expects subjects to be assigned at random to treatments. When subjects are not randomly assigned it is obvious that covariate values are not randomly assigned either and that covariate values may be affected by factors which influence the assignment of subjects to treatments.

Another practice is that of choosing groups which are pairwise matched on the covariate and assigning the members of the pairs to the two groups at <u>random</u>. This will give $\Sigma x_1 - \Sigma x_2 = \varepsilon = 0$ exactly.

The effect of having covariates "unaffected by treatment" can be seen quite readily when we examine the arithmetic of the classical solution. The adjusted estimates for treatment effect[3] are the regression coefficients C_{34}/C_{33} used for sweeping row 3 out of row 4 of C. Writing this using $\Sigma x_1 - \Sigma x_2 = \varepsilon$ gives us

$$\frac{C_{34}}{C_{33}} = \frac{(\Sigma y_1 - \Sigma y_2) - \varepsilon\cdot(\Sigma xy - \Sigma x\Sigma y/N)/(\Sigma x^2 - (\Sigma x)^2/N)}{(n_1 + n_2) - \varepsilon^2/(\Sigma x^2 - (\Sigma x)^2/N)} .$$

We can see that as $\varepsilon \to 0$ the <u>adjusted</u> estimate of treatment effect approaches the <u>unadjusted</u> estimate of treatment effect. That is, the effect of the covariate on the treatment means and treatment sum of squares becomes negligible as the difference between covariate means becomes small.

The error sum of squares can be shown to be

$$\Sigma y^2 - \frac{\Sigma y^2}{N} - \frac{(\Sigma xy - \Sigma x\Sigma y/N)^2}{\Sigma x^2 - (\Sigma x)^2/N}$$

$$- \frac{[(\Sigma y_1 - \Sigma y_2) - \varepsilon(\Sigma xy - \Sigma x\Sigma y/N)/(\Sigma x^2 - (\Sigma x)^2 - (\Sigma x)^2/N)]^2}{(n_1 + n_2) - \varepsilon^2/(\Sigma x^2 - (\Sigma x)^2/N)}$$

or, using ε throughout,

---

[3]These are not the ones usually reported, but they are the ones which are used in the analysis.

$$\Sigma y^2 - \frac{(\Sigma y)^2}{N} - \left[\left(\Sigma xy_1 - \frac{\Sigma x_1 \Sigma y_1}{n_1}\right) + \left(\Sigma xy_2 - \frac{\Sigma x_2 \Sigma y_2}{n_2}\right) - \frac{\varepsilon(\Sigma y_1 - \Sigma y_2)}{N}\right]^2$$

$$- \frac{[(\Sigma y_1 - \Sigma y_2) - \varepsilon(\Sigma xy - \Sigma x \Sigma y/N)/(\Sigma x^2 - (\Sigma x)^2/N)]^2}{N - \varepsilon^2/(\Sigma x^2 - (\Sigma x)^2/N)} .$$

As $\varepsilon \to 0$ this expression approaches

$$\Sigma y^2 - (\Sigma y)^2/N - (\Sigma y_1 - \Sigma y_2)^2/N - (\Sigma xy_1 - \Sigma x_1 \Sigma y_1/n_1 + \Sigma xy_2 - \Sigma x_2 \Sigma y_2/n_2)^2$$

which is the adjusted error term. Combining this information about the adjusted error term with the above observation that the treatment sum of squares is unaffected by a covariate with no difference between group means, we come up with the familiar observation, "Covariance adjustment reduces the sum of squares for treatment very little while reducing the sum of squares for error considerably" (see Snedecor, 1956, p. 399). This effect is also reflected in the common statement: covariance analysis is used to reduce errors.[4]

The effect can also be seen by comparing β_E with β_T .

$$\beta_T = \frac{b_{24}}{b_{22}} = \frac{\Sigma xy - \Sigma x \Sigma y/N}{\Sigma x^2 - (\Sigma x)^2/N}$$

[4]This is another misstatement about analysis of covariance. The "reduction of error variance" is no reduction at all; it is the elimination of a portion of variance due to the covariate which has nothing to do with treatment variation. The "reduction" of the so-called "error sum of squares," then, is only another step in obtaining the error sum of squares: there is no "error sum of squares" until the covariate reduction has taken place.

$$\beta_E = \frac{d_{24}}{d_{22}} = \frac{\Sigma xy - \Sigma x\Sigma y/N - (\Sigma x_1 - \Sigma x_2)(\Sigma y_1 - \Sigma y_2)/N}{\Sigma x^2 - (\Sigma x)^2/N - (\Sigma x_1 - \Sigma x_2)^2/N}$$

$$= \frac{\Sigma xy - \Sigma x\Sigma y/N - \varepsilon(\Sigma y_1 - \Sigma y_2)/N}{\Sigma x^2 - (\Sigma x)^2/N - \varepsilon^2/N} \quad .$$

As $\varepsilon \to 0$, β_T converges to β_E . In short, since β_E estimates the population regression coefficient, β_T estimates it also, but only if ε is statistically null!

Difficulties with regression coefficients. Unfortunately, the above figures somewhat obscure what is really happening in the relationship between β_T and β_E . It is much easier to relate the two when β_E is written in terms of deviations from sample means. Let SCP_w denote the sum of cross-products within samples and SS_w be the pooled sum of squares within samples.

Using SCP_w to denote the sum of cross-products of variables within groups it can be shown that

$$\beta_E = \frac{SCP_w(x,y)}{SS_w(x)}$$

$$\beta_T = \frac{SCP_w(x,y) + (\Sigma x_1 - \Sigma x_2)(\Sigma y_1 - \Sigma y_2)/N}{SS_w(x) + (\Sigma x_1 - \Sigma x_2)^2/N}$$

$$= \frac{SCP_w + \varepsilon(\Sigma y_1 - \Sigma y_2)/N}{SCP_w + \varepsilon^2/N} \quad .$$

This shows more clearly that as $(\Sigma x_1 - \Sigma x_2) = \varepsilon$ gets small and vanishes, β_T approaches β_E . It also shows quite clearly that, since the numerator contains ε and the denominator contains ε^2 , β_T may be unpredictably different than β_E when ε is large.

These formulations show that the estimation of regression coefficients is affected by the differences of covariate means among the groups.

Difficulties with contrasts. An obscure and possibly crucial event happens to the entry C_{33} in classical covariance analysis when the difference between covariate means is not null. When row 2 of B is swept out of the matrix

$$C_{33} = n_1 + n_2 - (\Sigma x_1 - \Sigma x_2)^2/(\Sigma x^2 - (\Sigma x)^2/N)$$

$$= n_1 + n_2 - \varepsilon^2/(\Sigma x^2 - (\Sigma x)^2/N) \quad ,$$

which shows that C_{33} is altered by the covariance adjustment when the difference between covariate means is not zero.

This alteration is only a matter of scale on the dummy variable when only two groups are involved. When more than two groups are involved, the matter becomes serious.

For the three-group problem with deviation contrasts, group sizes n_1 , n_2 and n_3 , B_{33} is a submatrix,

$$\begin{pmatrix} n_1 + n_3 & n_3 \\ n_3 & n_2 + n_3 \end{pmatrix} .$$

After adjustment for the covariate, this submatrix becomes very complex algebraically. The (1,1) entry is

$$n_1 + n_3 - (\Sigma x_1 - \Sigma x_4)^2/N \quad .$$

The (1,2) entry is

$$n_3 - (\Sigma x_1 - \Sigma x_4)(\Sigma x_2 - \Sigma x_4)/N$$

and the (2,2) entry is even worse. If the differences among the covariate means are substantial, there is a substantial alteration of the contrasts which one set out to test. The result of this alteration is to produce a new set of contrasts among means which are unknown to the statistician: i.e., the mechanics of analysis produce a statistical test of unknown origin. When the differences among covariate means are large, the actual analysis may be very different from the one expected.

It is conceivable that serious alteration of contrasts could take place. That is, suppose the statistician originally specified the deviation contrasts among means as $\bar{Y}_1 - \bar{Y}.$, $\bar{Y}_2 - Y.$, $\bar{Y}_3 - \bar{Y}.$ (each group mean contrasted with the grand mean), it is not inconceivable that the differences between covariate means would alter these contrasts to something like $2\ \bar{Y}_1 - \bar{Y}.$, $\bar{Y}_2 - \bar{Y}.$ and $\bar{Y}_3 - 3/4\ \bar{Y}.$

These arguments lead to the conclusion that classical covariance analysis is not always applicable to all problems which are commonly perceived as analysis of covariance. In fact, Cochran (1957, p. 264) states: "The F - test of treatments against error for the x variate [i.e., covariate] is helpful when there is doubt whether treatments have had some effect on x." One can only conclude that if the covariate means are dispersed significantly over treatments, then classical analysis of covariance is inappropriate.

When subjects are not assigned at random to treatments. When subjects are not assigned to treatments at random, either over the entire list of subjects for the experiment or within blocks of subjects, the above arguments show the danger of using classical analysis of covariance to determine the results of treatment. The author would prefer to describe this kind of investigation as a survey and not as an experiment. The term "experiment," it seems, should be reserved for those investigations in which there is an opportunity for the experimenter to assign subjects to treatments randomly somewhere in the process of data collection. In educational investigations random assignment rarely occurs anywhere in the process. Teachers and students are almost never assigned to treatments at random and schools are always chosen from a list of those which will cooperate.

When random assignment of subjects to treatments is not performed, there is considerable likelihood that systematic differences between treatment groups have occurred. Any data collection for covariates then should certainly include F tests of covariate over the collection design to insure that covariates have not been influenced by treatments or choice of treatment groups. Cochran (1957, p. 264) suggests this even for randomized designs; Snedecor (1956, p. 397) implies as much when he notes that "the means (on the covariate)...differ little more than would be expected...."

What is one to do if it is apparent that the covariate means differ from treatment group to treatment group? The author has searched the literature of covariance analysis and no one seems to have come up with a reasonable answer. Zelen (1957, p. 310) reacts to the problem in this way.

> Still another use for covariance analysis as pointed out by Bartlett (1936) is to adjust treatment effects for systematic differences between experimental units in a non-random experiment. In this situation if the effects of treatment differences disappear after adjusting for initial differences, then one can conclude that the treatments do not differ. However, if after adjusting for initial differences among experimental units the treatment differences still remain, then the conclusion that the treatments actually differ is not necessarily a valid conclusion. Although an exact functional relationship between the dependent

> and independent variates may not be known, an approximate relationship may be sufficient to adjust for the effects of non-randomness. Thus, if differences among treatments do not disappear after making such an adjustment, then we may conclude (i) treatments actually differ or (ii) treatments do not differ, but the functional relationship is not known to a sufficient approximation to adjust adequately for the non-random nature of the experiment. The conclusion that treatments actually differ will only be valid if the explicit form of the adjustment makes use of a known functional relationship between the dependent and the independent variates.

The solution to the problem is stated in Zelen's last sentence. It can be restated as "If you can't find a good regression coefficient you don't have an analysis." Or: "If the analysis of covariance cannot provide a good regression coefficient, you can't use it." Or: "Find a good regression coefficient before you analyze."

When subjects are not assigned at random to treatments, the search for a regression coefficient can pose a serious problem. To generate an appropriate regression coefficient one should fulfill the following requirements

1. The regression coefficient should be independent of experimental data.

In classical analysis of covariance the independence is obtained mathematically by partitioning total variance. Tests of experimental hypotheses are generated from other independent partitions of the data. In some experimental situations it may be necessary to collect data exclusively for obtaining <u>an</u> appropriate regression coefficient.

2. The regression coefficient should be appropriate to the range of data covered by the experiment.

A large percentage of regressions are curvilinear over the extremes of the range of the data and if the experiment has a selected range within the extremes, the correlation may be linear having one value within the range of the experiment and other values in other ranges. A good example would be the relationships between reading ability and age. Before the age of six it is nearly zero, after age 30 it may again be zero, but between ages 6 and 18 the correlation may be linear and nearly .90.

3. The regression coefficient should be obtained from the same measures as those used in the experiment.

This requirement should be self-explanatory.

These requirements make several points quite clear about using analysis of covariance when the covariate is a pre-experiment measure of the criterion variable.

(a) Classical analysis of covariance is an analysis of gains scores if treatment groups are chosen at random or matched on the covariate. This is because the regression between variate and covariate can be estimated from the data at hand.

(b) Analysis of gains scores is an analysis of covariance with an external regression coefficient.

(c) Analysis of difference scores is an analysis of covariance (or gains) where the regression coefficient is assumed to be 1.0.

Finding the appropriate regression coefficient. There are numerous settings where analysis of covariance is an inappropriate technique. The typical educational survey or "experiment" done without random assignment is one of these. In these surveys there are techniques which can be used without completely destroying the value of analytic procedures and probability statements even though the techniques may not be rigorous.

In an "experiment" where the covariate is a pre-treatment measurement of the criterion variable and the treatment groups are at least representative of some whole population (which can be defined without referring to the criterion) it is possible to locate one very usable regression coefficient: the test-retest reliability coefficient from test norms,[5] when the norms have been obtained from the same population as used for the "experiment" and the time lapse between test and retest is similar to that used in the "experiment."

When test norms do not exist or test-retest reliabilities have not been determined and if treatment groups represent some whole population, the error regression coefficient is a reasonable substitute. Although it has been known that error regression coefficients are inappropriate almost since analysis of covariance was devised, the difference in probability of F statements is rather small as Cochran (1957, p. 275) shows.

There are circumstances where regression coefficients must be designed into the data collection. Consider a survey of a remedial program, say reading, where all pupils who have less than a particular reading score are subjected to a six-month remedial program. Reading scores are taken pre- and post-treatment and the question is asked "What happened?" Without a regression coefficient only descriptive statistics are available. The within-treatment group regression coefficient is inappropriate because it would wipe out gains if used. The nontreatment group regression coefficient is inappropriate because the range of scores has been truncated by removing the treatment group. Any regression coefficient developed from the whole group is

[5]The pretest posttest regression would be a reliability problem if no treatment had intervened.

inappropriate because part of the group has been treated. About the only thing to be done is to find a nearby school with the same kind of student body and run the same testing schedule for the sake of finding a regression coefficient.

A similar problem exists in evaluating headstart-type programs. Here, there may be no way of obtaining an appropriate regression coefficient for several reasons.

(1) The headstart program may exhaust the population (say ghetto dwellers) and no control group may exist. This makes it almost impossible to do any comparative study at all.

(2) The testing materials may not have been normed, thereby eliminating any comparison with a normative sample.

(3) Choosing a comparative group from another social stratum may be unsatisfactory because of systematic differences in pre-treatment scores and change in regression coefficient due to curvilinearity of regression in the extreme range of the testing materials.

(4) Choosing a comparable sample in a nearby area and running the same testing schedule to obtain a regression coefficient may be prohibitively expensive.

Conclusion

The article by Lord (1967) has stirred considerable investigation into this area of statistical analysis, so much so that the problem has become known as Lord's Paradox. Much has been written to explain the apparent paradox in analysis. It is this writer's opinion that there is no paradox; there is only a misapplication of classical analysis of covariance procedure.

The primary function of sampling design in analysis of variance is to obtain independent estimates of all the population parameters needed in a statistical problem from one complex data collection. The purpose of this investigation is to show that the traditional collection procedure is inadequate for estimating regression coefficients for the analysis of covariance and change when subjects are not assigned to treatments at random. Some suggestions have been made about compensating for this inadequacy.

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