DOCUMENT RESUME

ED 096 361 TM 003 977

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TITLE A Design Selection Procedure.
PUB DATE [Apr 74]
NOTE 13p.; Paper presented at the Annual Meeting of the American Educational Research Association (59th, Chicago, Illinois, April 1974)

EDRS PRICE MF-$0.75 HC-$1.50 PLUS POSTAGE
DESCRIPTORS *Analysis of Covariance; *Analysis of Variance; Hypothesis Testing; *Research Design; *Sampling; Statistical Analysis

ABSTRACT

The problem of blocking on a status variable was investigated. The one-way fixed-effects analysis of variance, analysis of covariance, and generalized randomized block designs each treat the blocking problem in a different way. In order to compare these designs, it is necessary to restrict attention to experimental situations in which observations are collected within a fixed-effects generalized randomized block framework. An analytical technique was developed to aid a researcher in choosing one of these three designs. The technique is based on the criterion of maintaining power against particular treatment main effect non-nullities, at certain specified levels. (Author)

A DESIGN SELECTION PROCEDURE

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A.E.R.A., April, 1974, Chicago

Most of us regard experimental design as an integral part of research. Prior to initiating an experimental manipulation we consider various alternatives with respect to the potential utilization of factors believed to be systematically related to the dependent variable. In particular, we conceive a sampling plan that reflects our research objectives and that allows efficient data analysis.

When designing experiments pertaining to the comparison of specified treatments, a researcher in the behavioral sciences is often faced with the problem of choosing one of several experimental designs. If the presence of a non-treatment nuisance factor is anticipated, the simplest designs under consideration are likely to be the completely randomized, the generalized randomized block, and the analysis of covariance designs. Each of these designs requires a fundamentally different way of dealing with a potential nuisance factor.

Consider a comparative study in which an investigator wants to determine the effect of each of three different instructional methods on achievement. Before performing the xperiment he may consider the use of the one way fixed effects analysis of variance model and question the wisdom of controlling a potentially troublesome secondary factor, such as I.Q. Lack of control may result in substantial loss of power of the statistical test involving treatment differences. On the other hand, implementation of controls may require additional subjects and greater expense. This paper describes an analytical method for making a choice among the one way fixed-effects, the generalized randomized block, and the analysis of covariance designs.

The sampling plan adopted for the study consists of sampling subjects from fixed I.Q. levels (or blocks) and randomly assigning them to treatment conditions within blocks. In this manner the one-way and the analysis of covariance models may be conceptualized as special cases of the generalized randomized block model and the resulting design comparison is facilitated.

Table 1 depicts the three options under consideration. Option 1 represents a decision to ignore the I.Q. factor. The corresponding model consists of the grand mean parameter μ, the treatment effect parameter α_i, and the discrepancy, ε_{ij}, between the observed and predicted values of the criterion score.

Option 2 represents a decision to incorporate I.Q. as a covariate. In addition to μ and α_i, the model contains a criterion component $\beta(X_{ij} - \bar{X})$ where X_{ij} is value of the covariate for the j^{th} subject in the i^{th} treatment and β is the slope of the regression of the criterion score Y_{ij} on the covariate score X_{ij}. The model also contains the discrepency, ε'_{ij}, between the observed and predicted values of the criterion score. Note that the result of removing the component $\beta(X_{ij} - \bar{X})$ from the one way model error term, ε_{ij}, is ε'_{ij}.

Option 3 represents a decision to incorporate I.Q. as a blocking factor. In addition to μ and α_i, the model contains the I.Q. effect parameter β_j, the treatment by I.Q. interaction effect parameter γ_{ij}, and the discrepency, ε_{ijk}, between the observed and predicted criterion scores.

Under what circumstances might an investigator be led to prefer one of the three designs to the remaining two? First, the anticipated presence of substantial treatment by block interaction effects, γ_{ij}, and/or substantial non-linear block effects, $\{\beta_j - \beta(X_{ij} - \bar{X})\}$, clearly suggest the use of the generalized randomized block design. Secondly, the anticipated absence of treatment by block interaction effects and non-linear block effects, in conjunction with the anticipated presence of substantial linear block effects, $\beta(X_{ij} - \bar{X})$, suggest the use of the analysis of covariance design. Finally, the anticipated absence of both treatment by block interaction effects and block main effects suggest the use of the one way design.

The relative magnititudes of the afore menticned effects may be combined to form several highly useful indices. The first is a blocking index, m, (equation 1) reflecting the magnititude of block main effect and/or treatment by block interaction effect variance relative to error variance.

$$m = \frac{\sigma_B^2 + \sigma_{BT}^2}{\sigma_B^2 + \sigma_{BT}^2 + \sigma_\varepsilon^2} \tag{1}$$

σ_B^2 is the finite population variance due to block effects, σ_{BT}^2 is the finite population variance due to treatment by block interaction effects, and σ_ε^2 is the error variance in the generalized randomized block model. The blocking index, m, assumes values between 0 and 1. Clearly, when m is close to 1, the use of the one way design is inappropriate.

The second index, k, is a covariance index (equation 2) reflecting the magnititude of the linear block effect variance relative to the block within treatment effect variance.

$$k = \frac{\sigma_{B_L}^2}{\sigma_B^2 + \sigma_{BT}^2} \tag{2}$$

$\sigma_{B_L}^2$ is the finite population variance due to linear block effects. The covariance index, k, also assumes values between 0 and 1. When k is close to 1 and m is sufficiently different from 0, the use of the analysis of covariance design is appropriate.

Next, let us turn our attention to the test statistic, $F = \frac{SS_T}{df_T} \Big/ \frac{SS_{Error}}{df_{Error}}$, (equation 3) that is used in testing the hypothesis that the treatment effects are null. The denominator consists of a different sum of squares term depending upon the choice of model.

Under the generalized randomized block model, the test statistic is distributed in general as a noncentral F random variable (equation 4).

$$F = F_{\{df_T,\ df_{Error},\ \delta_T^2\}} \tag{4}$$

where δ_T^2 is the treatment effect noncentrality parameter.

Under the analysis of covariance model, F is distributed as a doubly noncentral F random variable (equation 5).

$$F = F_{\{df_T,\ df_{Error},\ \delta_T^2,\ \delta_\varepsilon^2\}} \tag{5}$$

where δ_ε^2 is the noncentrality parameter associated with the non-linear block and treatment by block interaction effects. Note that δ_ε^2 may be expressed as a function of m and k, the blocking and covariance indices, respectively.

Under the one way model, F is distributed as a doubly noncentral F random variable (equation 6).

$$F = F_{\{df_T,\ df_{Error},\ \delta_T^2,\ \delta_\varepsilon^2\}} \tag{6}$$

where δ_ε^2 is the noncentrality parameter associated with the block main effects and treatment by block interaction effects. Note that δ_ε^2 may be expressed as a function of m, the blocking index.

The power function of the test of treatment differences (Figure 1) is substantially dependent upon the choice of model by virtue of the noncentrality parameter associated with the block and treatment by block effects. Therefore, to facilitate comparisons among the three designs it is stipulated that certain power requirements be met, regardless of the model chosen. Figure 1 illustrates that an amoun of power P_1 is desired against a treatment effect non-nullity of magnititude Δ_1 and an amount of power P_2 is desired against Δ_2, where Δ_1 is the upper bound on the set of non-nullities that the investigator has decided are trivial and where Δ_2 is the lower bound on the set of non-nullities that he has decided are important.

A computer program is used to calculate the value of the type I error rate, α, and the sample size, N, corresponding to the power function in figure 1. Different values of α and N are obtained depending upon the choice of model.

Consider the data in Table 2. An investigator is testing a hypothesis about a linear trend among the treatments (say) and wishes to include a blocking variable consisting of 8 levels. The solutions for α and N required by the generalized randomized block model are .0604 and 36.39 respectively. The solutions required by the one way model are .0630 and 24.00 respectively.

The one way model appears to be more economical if no block within treatment variability is present, that is to say if $m=0$. However, to the extent that m is nonzero, the desired power function is depressed when the one way model is used. An obvious remedy is to increase the number of subjects beyond 24 in order to meet the specified power criteria. However, to increase the number beyond 36 is pointless because the generalized randomized block model, whose power function is independent of m, may be used. In fact, it seems that if one expects an m value in excess of 8% one ought to use the generalized block model. The reason is simply that too many subjects are required by the one way model in order to meet the specified power criteria.

Let m^* represent the largest m value that may be tolerated by a researcher using a one way model. A value less than m^* is acceptable since the power criteria are satisfied but a value greater than m^* is unacceptable since the power criteria are not satisfied. The tabled values of m^* for a number of sample size values range from 6% to 8%. The implication is clear, very little block within treatment variability is required before the researcher is led to incorporate the blocking factor in the design.

All of the data collected thus far strongly supports an investigator who incorporates a blocking factor into the design whenever block main effects or treatment by block interaction effects are expected to exceed minimal levels. Table 4 represents the most extreme case with respect to m^* magnititudes encountered to date.

Table 5 represents m^* values corresponding to various sample size and covariance index values, derived from the initial data in table 2. For example, an N value of 36.39 and a k value of .67 yields an m^* value of 20%. A researcher expecting block within treatment variability leading to an m^* value in excess of 20% would choose the generalized randomized block design. On the other hand, expecting a value less than 20% leads to the choice of the analysis of covariance design. Table 6 also represents m^* values and is derived from initial data in table 4.

In summary, the strong recommendation based on this research is "When in doubt, block; it is almost always to your advantage".

TABLES

TO ACCOMPANY

A DESIGN SELECTION PROCEDURE

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Table 1

Design Decisions

Researcher's objective is to test a hypothesis concerning treatment differences. His design question centers on the manner in which to handle a nuisance factor. (The latter is a factor related to the dependent variable and one in which the researcher has no interest.)

Option 1

Ignore the nuisance factor

Example:

T_1	T_2	T_3

1 factor design consisting of 3 treatment conditions

Model $\quad Y_{ij} = \mu + \alpha_i + \varepsilon_{ij}$

Option 2

Incorporate the nuisance factor into the design as a covariate

Model $\quad Y_{ij} = \mu + \alpha_i + \beta(X_{ij} - \bar{X}) + \varepsilon'_{ij}$

Note: $\varepsilon'_{ij} = \varepsilon_{ij} - \beta(X_{ij} - \bar{X})$

Option 3

Incorporate the nuisance factor into the design as a blocking factor

Example:

	T_1	T_2	T_3
B_1			
B_2			
B_3			
B_4			

3 levels of treatment factor
4 levels of nuisance factor

Model $\quad Y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_{ij} + \varepsilon_{ijk}$

TABLE 2

Design: 4 treatment conditions
8 levels of blocking variable

Power function specification:

$\Delta_1 = .25$ and $P_1 = .1$, and

$\Delta_2 = 1.50$ and $P_2 = .9$

Sample size and α values:

GRB design $N = 36.39$ and $\alpha = .0604$ and

OW design $N = 24.00$ and $\alpha = .0630$.

N	m^* Values
36.39	.08
33.91	.08
31.44	.07
28.96	.06
26.48	.06

TABLE 3

Design: 3 treatment conditions
5 levels of blocking variables

Power function specifications:

$\Delta_1 = .25$ and $P_1 = .1$, and

$\Delta_2 = 1.20$ and $P_2 = .9$.

Sample size and α values:

GRB design $N = 47.47$ and $\alpha = .0469$, and

OW design $N = 46.53$ and $\alpha = .0472$

N	m^* Values
47.47	.01
47.28	.01
47.09	.01
46.90	<.01
46.71	<.01

EQUATIONS

1. Blocking index

$$= \frac{\sigma_B^2 + \sigma_{BT}^2}{\sigma_B^2 + \sigma_{BT}^2 + \sigma_\varepsilon^2}$$

σ_B^2 finite population variance due to block effects

σ_{BT}^2 finite population variance due to treatment by block interaction effects

σ_ε^2 error variance in the generalized randomized block (GRB) model

2. Covariance index

$$k = \frac{\sigma_{B_L}^2}{\sigma_B^2 + \sigma_{BT}^2}$$

$\sigma_{B_L}^2$ finite population variance due to linear block effects

3.

$$F = \frac{SS_T / df_T}{SS_{Error} / df_{Error}}$$

4. $F = F\{ df_T,\ df_{Error}\ \delta_T^2 \}$

δ_T^2 treatment effect noncentrality parameter

5. $F = F\{ df_T,\ df_{Error},\ \delta_T^2,\ \delta_{\varepsilon'}^2 \}$

$\delta_{\varepsilon'}^2$ non-linear block + treatment by block interaction effect noncentrality parameter

6. $F = F\{ df_T,\ df_{Error},\ \delta_T^2,\ \delta_\varepsilon^2 \}$

δ_ε^2 block main + treat. by block interaction effect noncentrality parameter

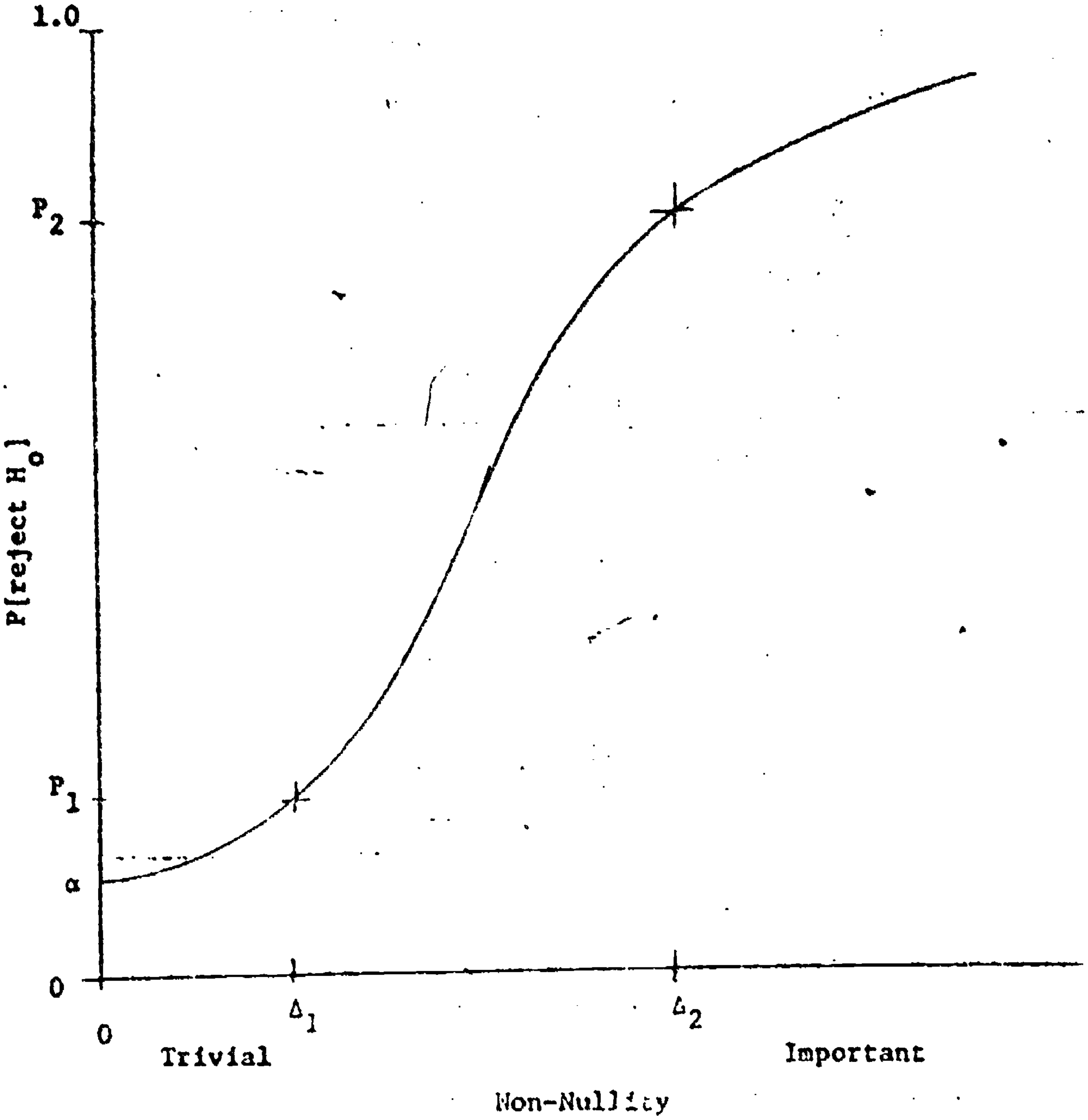
1.0
P_2
P[reject H_o]
P_1
α
0
0
Δ_1
Δ_2
Trivial
Important
Non-Nullity

Figure 1

TABLE 4

Design: 8 treatment conditions
4 levels of blocking variable

Power function specifications:

$\Delta_1 = .25$ and $P_1 = .1$, and
$\Delta_2 = 1.50$ and $P_2 = .9$

Sample size and α values:

GRB design $N = 35.43$ and $\alpha = .0582$

OW design $N = 20.39$ and $\alpha = .0629$

N	m^* Values
35.43	.19
32.43	.18
29.42	.17
26.41	.16
23.40	.15

TABLE 5
m^* Values

N	k=.33	k=.50	k=.67
36.39	.11	.14	.20
33.91	.11	.14	.19
31.44	.10	.13	.18
28.96	.09	.12	.17
26.48	.08	.11	.15

TABLE 6
m^* Values

N	k=.33	k=.50	k=.67
35.43	.24	.30	.39
32.47	.24	.30	.38
29.50	.23	.29	.38
26.53	.22	.27	.36
23.57	.21	.26	.35