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RESTRICTION ERRORS FOR LINEAR MODELS (AN AID TO DEVELOP MODELS FOR DESIGNED EXPERIMENTS)

VIRGIL L. ANDERSON
Department of Statistics, Purdue University, Lafayette, Indiana, 47906 U. S. A.

SUMMARY

A random error component is introduced into linear models for analyzing data from designed experiments. This error component is called a 'restriction error' because it is used whenever a restriction on randomization occurs in the design.

Discussion of the usefulness of this restriction error in randomized complete block designs, nested factorials, and split plot designs is given. Accompanying each discussion is an example of an experiment on prosthetic cardiac valves.

Further, the restriction error concept is used to look at the controversy regarding a pooled split plot error.

INTRODUCTION

There are many possible models for the analysis of data from any designed experiment. It is almost certain that any linear model used is only an approximation to the appropriate nonlinear one. Given that the nonlinear model is not known, we believe the first problem in developing a good linear model is to write down the most general linear model to represent all the possible sources of variation. The next problem is to decide which terms to discard in this general model. This decision should be based upon theory and/or experience.

Specifically what makes the development of linear models in this paper different from the development in other papers and books is that we introduce a random component called a 'restriction error' into our model corresponding to every restriction on randomization introduced in the design. This forces the general linear model to be different for different designs but allows experimenters to delete terms for a specific experiment if they believe these effects are zero.

The restriction error is not estimable from the data but it is placed in the model (appropriately indexed) and is allowed to appear in the corresponding analysis of variance as a source of variation. There are no degrees of freedom (D.F.) and no sum of squares (SS) for the error; however, since it appears in the theoretical linear model, the variance component for it does appear in the expected mean squares (EMS). This variance component in the EMS forces the experimenter to recognize it and account for it in the F-tests. The real power of restriction error is that it forces the experimenter not only to recognize the restriction on randomization he has imposed on

his design (usually to save time and/or money) but also to see its effect on the overall results of the experiment. Almost always this effect is to decrease the number of degrees of freedom for the appropriate error.

It is interesting to note that the restriction error does not disturb the algorithm presented by Bennett and Franklin [1954] and more recently by Hicks [1964] to derive the expected mean squares given the linear model. The EMS are still obtained in the same straightforward manner and are correct when restriction errors are in the model.

The first design discussed in this paper that has a restriction on randomization is the randomized complete block design (RCBD). The second is the nested factorial and the final one is the split plot design. In the linear model presented for the analysis of the data from each of these designs, the restriction error has a decided effect on the F-tests for the important main effects and interactions. For all designs, an example using prosthetic cardiac valves in the experiments is given.

SIMPLE DESIGNS

Suppose that b levels of one fixed set of treatments and t levels of another fixed set are run in all possible bt combinations on bt experimental units completely at random. The design of such an experiment is called a completely randomized design (CRD). The linear model for the analysis of the data from such an experiment, if the interaction of the sets of treatments is zero, is usually expressed as:

$$y_{ij} = \mu + B_i + T_j + \epsilon_{(ij)} \qquad \begin{matrix} i = 1, 2, \cdots, b \\ j = 1, 2, \cdots, t \end{matrix} \tag{1}$$

where

y_{ij} = the response from the experimental unit treated with the ith level of treatment B and the jth level of treatment T,

μ = overall mean,

B_i = the effect of the ith level of (fixed) treatment B,

T_j = the effect of the jth level of (fixed) treatment T,

$\epsilon_{(ij)}$ = the (random) within error resulting from repeating the ith level of treatment B with the jth level of treatment T on different experimental units. In this case the (ij)th combination is not repeated but the interaction is assumed zero. Thus an estimate of the error mean square may come from the interaction source and all analysis of variance assumptions hold so that the $\epsilon_{(ij)}$ are NID$(0, \sigma_\epsilon^2)$, i.e. normally and independently distributed with mean zero and variance σ_ϵ^2. The corresponding analysis of variance (ANOVA) is given in Table 1.

To be able to write model (1) it seems appropriate that one must assume there are no restrictions on randomizations in the design of the experiment. This may be called a two-way factorial completely randomized design.

Suppose, next, there are b blocks in an experiment, and t fixed treat-

TABLE 1

ANOVA FOR THE DATA FROM THE CRD USING MODEL (1)

Source	D. F.	EMS (Expected mean square)
Treatments (B_i)	$b - 1$	$\sigma_\epsilon^2 + t\Phi_2(B)$
Treatments (T_j)	$t - 1$	$\sigma_\epsilon^2 + b\Phi_1(T)$
Within error ($\epsilon_{(ij)}$) (assuming interaction BT is zero)	$(b - 1)(t - 1)$	σ_ϵ^2

EMS: Theoretical values derived from model (1).
Φ Stands for fixed component (all levels of these treatments are in the experiment).
All sums of squares (SS) can be calculated from data.
The symbols appearing in 'source' are used to help the reader utilize the algorithm to derive the EMS.

ments are completely randomized onto t experimental units in each of the b blocks such that there is a different randomization in each block. The model for analyzing the data from such an experiment has been given by most authors[1] and implied by many others to be the same as model (1). This design is certainly not completely randomized because there is a different randomization of the treatments in each block, not just one randomization over the whole experiment as would be demanded for a CRD. In general, statisticians agree that this is a randomized complete block design (RCBD); however, they do not always agree on the model to analyze the data. Wilk [1955], Addelman [1969], and others have suggested using a model for a generalized randomized block design which includes the usual CRD and RCBD. This paper stresses restriction errors and in this section deals with a model that accounts for errors between blocks in contrast to the usual one that does not, namely model (1).

Since there is a different randomization of treatments within each block, it seems there should be some recognition in the model that the error to test the hypothesis of equality among the treatment means should come from within the blocks. On the other hand, if there is interest in testing the hypothesis that block means are equal, it seems that there must be an error outside the blocks. The former test (for treatment means) would be satisfied using the error designated in model (1), but the latter test (for block means) does not seem to be satisfied using the same error.

One linear model that seems to express all the sources of variation mentioned above with the assumption of no interaction of blocks by treatments and also provide intuitively correct tests is:

$$y_{ij} = \mu + B_i + \delta_{(i)} + T_j + \epsilon_{(ij)} \qquad \begin{matrix} i = 1, 2, \cdots, b \\ j = 1, 2, \cdots, t \end{matrix} \qquad (2)$$

[1] Since this procedure is so common, Peng ([1967] p. 93) provides merely a recent example. Pointing this out is not meant to be disparaging to him.

where

y_{ij} = the response from the experimental unit given the jth treatment in block i,

μ = overall mean,

B_i = effect of the ith block. In this case B_i may be random or fixed, but we will assume it is fixed for the ANOVA,

$\delta_{(i)}$ = the ith block or restriction error, NID$(0, \sigma_\delta^2)$, and completely confounded with B_i. Here $\delta_{(i)}$ (σ_δ^2 is not estimable from this experiment), will have zero D.F. and no sum of squares, but will be recognized in the analysis of variance expected mean squares and called *restriction error* because of the restriction on randomization of the treatments onto the ith block's experimental units,

T_j = the effect of the jth treatment (fixed),

$\varepsilon_{(ij)}$ = error within the combination of the ith block and the jth treatment combination. It is assumed that $\varepsilon_{(ij)}$ is NID$(0, \delta_\epsilon^2)$. This term is estimated in the analysis of variance from the interaction source assuming the interaction of the ith block with the jth treatment is zero in this experiment.

The analysis of variance for this model, including the EMS which can be derived from the algorithm given by Bennett and Franklin [1954], may be written as in Table 2. The line drawn between the restriction error and treatments indicates that no source below this line should be tested with any source above it and vice versa. One can understand this concept by looking at the EMS. This will be a general procedure for all analyses in this paper.

We do not wish to digress but it should be understood that we use 'sometimes pooling' procedures (Bozivich *et al.* [1956]), and if the mean squares may be pooled (usually with the probability of type I error (α) taken as 0.25) the rule of crossing over the line may be violated for certain tests of significance.

It can be seen from Table 2 that the correct error for blocks is restriction error with zero D.F. not the within error with $(b-1)(t-1)$ D.F. indicated

TABLE 2

ANOVA FOR DATA FROM RCBD USING MODEL (2)

Source	D. F.	EMS
Blocks (B_i)	$b-1$	$\sigma_\epsilon^2 + t\sigma_\delta^2 + t\Phi_2(B)$
Restriction error ($\delta_{(i)}$)	0	$\sigma_\epsilon^2 + t\sigma_\delta^2$
Treatments (T_j)	$t-1$	$\sigma_\epsilon^2 + b\Phi_1(T)$
Within error ($\epsilon_{(ij)}$)	$(b-1)(t-1)$	σ_ϵ^2

Note: The restriction error is represented in the theoretical model but cannot be estimated from the data in this experiment. All of this is recognized in Table 2 by writing down the source (restriction error), showing zero (0) D.F. to depict the lack of data and expressing the theory in the EMS; also, there is no sum of squares or mean square. Of course, the other three sources have sums of squares that can be computed from the data.

before. Since there are zero D.F. and no sum of squares for the restriction error, there is no test for blocks in this experiment. Many authors[2] have indicated that there should be no test for blocks for various reasons, but none has been specific to indicate it from the model as we attempt to do here. It should be pointed out that many times the experimenter is interested only in whether or not blocks have been effective in reducing the estimated error to test treatments. Under these circumstances the investigator may test that the combination of block effects and the block error (our restriction error) is zero. That is, the hypothesis could be stated from Table 2 as $H_0 : \sigma_\delta^2 + \Phi_{(2)}(B) = 0$. It is obvious, then, that the within error is appropriate for the test and it would make no difference in the test whether model (1) or model (2) is considered.

The real danger in using model (1) in analyzing data from RCBD occurs when the experimenter is interested in the block means. To be more explicit, frequently experimenters want to use treatments to represent the blocks in a RCBD and restrict randomization of another set of treatments inside the ones used as blocks. Recognition of this restriction on randomization demands a model other than model (1). Our suggestion is that model (2) will provide the basis for a more nearly correct analysis of the data from such a design.

An example to demonstrate this danger occurred in a medical-engineering problem (Beeson [1965]). An engineer constructed a mechanical apparatus to simulate the circulatory system of human beings. A storage tank was used to control the pressure of the liquid, simulating the blood, in the system. A pulse pump squeezed flexible rubber tubing in the system to create the pulsing action needed to simulate the heart action. The motor on the pump could vary the pulse rate between 0 and 220 beats per minute. In the experiment 6 rates, between 60 and 160 beats per minute, were used and 4 prosthetic cardiac valves were inserted individually in the mechanism. The experimenter assumed (perhaps erroneously) that each valve type was adequately represented by only one valve used in the experiment.

The purpose of the experiment was to select the best valve type out of the four for all pulse rates and/or best valve type for particular pulse rates. To a statistician this means that the main effect of valve types and the interaction of valve types by pulse rates should be examined carefully.

One of the variables to be measured and analyzed was maximum flow gradient (mmHg.) and it was found to have reasonably good statistical properties for analysis.

The experimenter thought it was too time-consuming to run a completely randomized design (CRD) of all 24 treatment combinations of 4 valve types and 6 pulse rates with 2 observations per treatment combination. The CRD would require that the mechanism holding the valve type would have to be dismantled and reassembled 47 times. Hence he could draw, at random, one valve type out of the 4, seat it and randomly run the 12 pulse rates (6 actual pulse rates each repeated once). Next he could select one of the

[2] An example is Ostle [1963] p. 368.

remaining 3 valve types at random and with a new random order, run the 12 pulse rates as before. He could continue this procedure for the other 2 valve types.

This experimenter recognized that he would have basically a RCBD (really more than one observation per cell). Since valve types act as blocks and each valve type occurs for only one run, there is no repeat of the valve type's performance by repeating in the machine or replication. Intuitively then the error should come from between blocks but all of these D.F. (3) are used for valve types.

If (as commonly occurs) model (1) is taken as the basis, the following linear model would be used:

$$y_{ijk} = \mu + V_i + P_j + VP_{ij} + \epsilon_{(ij)k} \qquad \begin{matrix} i = 1, 2, 3, 4 \\ j = 1, 2, \cdots, 6, \quad k = 1, 2, \end{matrix} \tag{3}$$

where

y_{ijk} = kth maximum flow gradient (mmHg.) for the ith valve type and jth pulse rate,

μ = overall mean,

V_i = effect of the ith valve type (fixed),

P_j = effect of the jth pulse rate (fixed),

VP_{ij} = effect of the interaction of the ith valve type with the jth pulse rate,

$\epsilon_{(ij)k}$ = error of the kth observation within the ith valve type and jth pulse rate, NID$(0, \sigma_\epsilon^2)$, (random).

Model (3) implies there is only one error term, namely *within* valve types, but this will not give us a correct test for valve types because we need a *between* block error for this test.

The corresponding analysis of variance is given in Table 3, which shows clearly that if model (3) and the corresponding analysis were used to investigate valve types, the *within* error mean square would be the denominator in the F-test. This is a contradiction and another model must be found for such an experiment. We believe that model (2) offers a basis for a more realistic model, namely:

$$y_{ijk} = \mu + V_i + \delta_{(i)} + P_j + VP_{ij} + \epsilon_{(ij)k}, \tag{4}$$

TABLE 3

ANOVA USING MODEL (3)

Source	D. F.	EMS
Valve types (V_i)	3	$\sigma_\epsilon^2 + 12\,\Phi_3(V)$
Pulse rates (P_j)	5	$\sigma_\epsilon^2 + 8\,\Phi_2(P)$
Interaction (V_{ij})	15	$\sigma_\epsilon^2 + 2\Phi_1(VP)$
Within error ($\epsilon_{(ij)k}$)	24	σ_ϵ^2

where

$\delta_{(i)}$ = restriction error caused by the ith valve type being used continuously while all pulse rates are run, NID$(0, \sigma_\delta^2)$, (random).

The corresponding analysis of variance for this experiment is set out in Table 4. It is seen that the mean square for the restriction error (which is really a *between* block error and intuitively correct) would be the denominator to test valve types. However, there are zero D.F. and no sum of squares for the restriction error. Consequently there is no mean square and there is no test for one of the most important factors, valve types, when this design is used. Hence if the experimenter uses model (4) in his preliminary outline of the analysis from this proposed design, he will know he should change the design of his experiment before he has run any part of it.

COMPLICATED DESIGNS

A more correct design for the experiment described in the previous section is to repeat the tests on the valve types at random and not to repeat the pulse rates. In this case a randomly chosen valve type is inserted and the 6 pulse rates are run at random. Next, the valve type is removed and another randomly drawn valve type is inserted again. The 6 pulse rates are randomly run on this new valve type. This is continued until all 4 valve types have each occurred, at random, twice. These occurrences are similar to blocks in the RCBD with the exception that it is impossible for the same block to occur for valve type 1 as for valve type 2, and so on. For this reason the occurrences are really nested inside valve types. In addition, pulse rates appear with all combinations of valve types. Both of these concepts lead some authors to call this design a nested factorial. An example is in section 11.4 of Hicks [1964]. As in the RCBD the restriction on randomization (in this case pulse rates in the occurrences of the valve types) is not explained in the model. Ordinarily the model for this experiment would be:

$$y_{ijk} = \mu + V_i + O_{(i)j} + P_k + VP_{ik} + OP_{(i)jk} + \epsilon_{(ijk)} \qquad \begin{matrix} i = 1, 2, 3, 4, \quad j = 1, 2, \\ k = 1, 2, \cdots, 6, \end{matrix} \tag{5}$$

TABLE 4
ANOVA USING MODEL (4)

Source	D. F.	EMS
Valve types (V_i)	3	$\sigma_\epsilon^2 + 12\,\sigma_\delta^2 + 12\,\Phi_3(V)$
Restriction error ($\delta_{(i)}$)	0	$\sigma_\epsilon^2 + 12\,\sigma_\delta^2$
Pulse rates (P_j)	5	$\sigma_\epsilon^2 + 8\,\Phi_2(P)$
Interaction (VP_{ij})	15	$\sigma_\epsilon^2 + 2\,\Phi_1(VP)$
Within error ($\epsilon_{(ij)k}$)	24	σ_ϵ^2

where

y_{ijk} = maximum flow gradient (mmHg.) obtained from the jth occurrence of the ith valve type with the kth pulse rate,
μ = overall mean,
V_i = effect of the ith valve type (fixed),
$O_{(i)j}$ = effect of the jth occurrence (random) in the ith valve type NID$(0, \sigma_0^2)$,
P_k = effect of the kth pulse rate (fixed),
VP_{ik} = effect of the interaction of the ith valve type with the kth pulse rate,
$OP_{(i)jk}$ = effect of the interaction of the jth occurrence in the ith valve type by the kth pulse rate, NID$(0, \sigma_{op}^2)$,
$\epsilon_{(ijk)}$ = within error, NID$(0, \sigma_\epsilon^2)$, in this case since there is only one observation within the jth occurrence of the ith valve type and kth pulse rate, there are zero D.F.

The corresponding analysis of variance in Table 5 indicates that the test for valve types seems correct because the error mean square comes from between occurrences (comparable to blocks in the RCBD). The tests for pulse rate and the interaction of valve types by pulse rates appear intuitively accurate since the within occurrences provide the basis. However, if one wanted to test occurrences in valve types, the mean square for within pulse rates would be used as the error mean square if this model were correct. The same argument may be used here as for the RCBD to show that this should not be the error mean square for this test.

It seems, then, that the model for this nested factorial experiment would be more accurately portrayed as:

$$Y_{ijk} = \mu + V_i + O_{(i)j} + \delta_{(ij)} + P_k + VP_{ik} + OP_{(i)jk} + \epsilon_{(ijk)} \qquad i = 1, 2, 3, 4, \quad j = 1, 2, \quad k = 1, 2, \cdots, 6, \tag{6}$$

where

$\delta_{(ij)}$ = restriction error caused by the 6 pulse rates being run in the jth

TABLE 5

ANOVA FOR DATA FROM THE NESTED FACTORIAL USING MODEL (5)

Source	D. F.	EMS
Valve types (V_i)	3	$\sigma_\epsilon^2 + 6\,\sigma_0^2 + 12\,\Phi_3(V)$
Occurrence in valve types ($O_{(i)j}$)	4	$\sigma_\epsilon^2 + 6\,\sigma_0^2$
Pulse rates (P_k)	5	$\sigma_\epsilon^2 + \sigma_{op}^2 + 8\,\Phi_2(P)$
Interaction (VP_{ik})	15	$\sigma_\epsilon^2 + \sigma_{op}^2 + 2\,\Phi_1(VP)$
Interaction ($OP_{(i)jk}$)	20	$\sigma_\epsilon^2 + \sigma_{op}^2$
Within error ($\epsilon_{(ijk)}$)	0	σ_ϵ^2

TABLE 6

ANOVA USING MODEL (6)

Source	D. F.	EMS
Valve types (V_i)	3	$\sigma_\epsilon^2 + 6\sigma_\delta^2 + 6\,\sigma_0^2 + 12\,\Phi_3(V)$
Occurrences in valve types ($O_{(i)j}$)	4	$\sigma_\epsilon^2 + 6\sigma_\delta^2 + 6\sigma_0^2$
Restriction error ($\delta_{(ij)}$)	0	$\sigma_\epsilon^2 + 6\sigma_\delta^2$
Pulse rates (P_k)	5	$\sigma_\epsilon^2 + \sigma_{op}^2 + 8\Phi_2(P)$
Interaction (VP_{ik})	15	$\sigma_\epsilon^2 + \sigma_{op}^2 + 2\Phi_1(VP)$
Interaction ($OP_{(i)jk}$)	20	$\sigma_\epsilon^2 + \sigma_{op}^2$
Within error ($\epsilon_{(ijk)}$)	0	σ_ϵ^2

occurrence of the ith valve type. This has zero D.F. and no sum of squares. It is assumed that it is NID$(0, \sigma_\delta^2)$.

The corresponding analysis of variance is given in Table 6 from which it can be seen that all the previous tests that seemed correct are tested as they were before. In addition, there is no test for occurrences in valve types since there is zero D.F. for the appropriate source to test it, namely restriction error. This is consistent with the RCBD result.

The actual design of the experiment used by Beeson [1965] was a split plot. He set up 2 random blocks in which he forced all 4 valve types to appear once before any valve type occurred twice. Of course, he randomized the order of using the valve types in the machine and randomized the 6 pulse rates for each valve type.

Some authors[3] believe the analysis for such an experiment should be as in Table 7, where the arrows on the mean squares indicate that the main

TABLE 7

ANOVA OF USUAL SPLIT PLOT DESIGN

Source	D. F.	Mean Square (MS)
Blocks	1	
Valve types	3	V
Whole plot error	3	W
Pulse rates	5	P
Valve types × pulse rates	15	PV
Split plot error	20	S

effect of valve types would be tested by the whole plot error and that the main effects of pulse rates and the interaction of valve types by pulse rates would be tested using the split plot error. In order to calculate the whole

[3] Examples are Yates [1967] p. 785 and some of his references plus Federer [1955] p. 274.

plot error sum of squares one would use the blocks by valve types interaction sum of squares and to calculate the split plot error sum of squares one would combine the blocks by pulse rates and blocks by valve types by pulse rates sums of squares.

Harter [1961], Chew ([1958] p. 48), and others have doubts that one should always obtain the split plot error by pooling. In biological studies using the 'sometimes pooling' technique described by Bozivich *et al.* [1956], there seems to be good reason to pool 'almost always.' Some authors have used the sometimes pooling criterion when the split plot treatments are random.

We take the view that if the experimenter has worked in the field of investigation for some time and 'knows' the errors involved, he should pool and use the analysis given in Table 7 if this seems appropriate. If, however, he does not know whether or not blocks interact with the various treatments, we believe he should let the data guide him in this decision. We believe this thinking is consistent with our use of restriction error in the RCBD and that the following linear model is appropriate and allows for the sometimes pooling methods (a numerical example will be given later):

$$y_{ijk} = \mu + B_i + \delta_{(i)} + V_j + BV_{ij} + \omega_{(ij)} + P_k + BP_{ik} + VP_{jk} + BVP_{ijk} + \epsilon_{(ijk)} \qquad \begin{matrix} i = 1, 2, \quad j = 1, 2, 3, 4, \\ k = 1, 2, \cdots, 6, \end{matrix} \qquad (7)$$

where

y_{ijk} = maximum flow gradient (mmHg.) obtained from the ith block, jth valve type, and kth pulse rate,

μ = overall mean,

B_i = effect of the ith block (random), NID$(0, \sigma_B^2)$,

$\delta_{(i)}$ = first restriction error zero D.F., NID$(0, \sigma_\delta^2)$,

V_j = effect of jth valve type (fixed),

BV_{ij} = effect of the interaction of the ith block with the jth valve type, NID$(0, \sigma_{BV}^2)$,

$\omega_{(ij)}$ = second restriction error, zero D.F., NID$(0, \sigma_\omega^2)$ (cf. Kempthorne [1952] p. 375; the η_{ij} of equation (13) is similar to this $\omega_{(ij)}$),

P_k = effect of kth pulse rate

BP_{ik} = effect of the interaction of the ith block and the kth pulse rate, NID$(0, \sigma_{BP}^2)$,

VP_{jk} = effect of the interaction of the jth valve type with the kth pulse rate,

BVP_{ijk} = effect of the interaction of the ith block with the jth valve type with the kth pulse rate, NID$(0, \sigma_{BVP}^2)$,

$\epsilon_{(ijk)}$ = within error, zero D.F., NID$(0, \sigma_\epsilon^2)$.

The corresponding analysis of variance is given in Table 8, the tests in which are understandable from the EMS. We believe that the controversy on pooling or not pooling BP and BVP may be resolved by a sometimes pooling

TABLE 8
ANOVA FOR MODEL (7)

Source	D. F.	EMS
Blocks (B_i)	1	$\sigma_\epsilon^2 + 6\sigma_\omega^2 + 24\sigma_\delta^2 + 24\sigma_B^2$
First restriction error ($\delta_{(i)}$)	0	$\sigma_\epsilon^2 + 6\sigma_\omega^2 + 24\sigma_\delta^2$
Valve types (V_j)	3	$\sigma_\epsilon^2 + 6\sigma_\omega^2 + 6\sigma_{BV}^2 + 12\Phi_3(V)$
Interaction (BV_{ij})	3	$\sigma_\epsilon^2 + 6\sigma_\omega^2 + 6\sigma_{BV}^2$
Second restriction error ($\omega_{(ij)}$)	0	$\sigma_\epsilon^2 + 6\sigma_\omega^2$
Pulse rates (P_k)	5	$\sigma_\epsilon^2 + 4\sigma_{BP}^2 + 8\Phi_2(P)$
Interaction (BP_{ik})	5	$\sigma_\epsilon^2 + 4\sigma_{BP}^2$
Interaction (VP_{jk})	15	$\sigma_\epsilon^2 + \sigma_{BVP}^2 + 2\Phi_1(VP)$
Interaction (BVP_{ijk})	15	$\sigma_\epsilon^2 + \sigma_{BVP}^2$
Within error ($\epsilon_{(ijk)}$)	0	σ_ϵ^2

procedure for the given problem, unless the experimenter wished to specify his model before running his experiment.

To give further explanation for our position in analyzing split-plot design data in this manner, let us compare this analysis step by step with the nested factorial, Table 6. The premise we use is that there is a possibility of a block by treatment (BV) interaction if a split plot design is used. This source has 3 D.F. in Table 8. There is no BV interaction, of course, if the nested factorial is used. Then the blocks and BV are not separable as in occurrences in valve types with 4 D.F. in Table 6.

If it is agreed that a block by treatment interaction may exist, and this seems quite logical when block means are of interest as in many engineering experiments, then there certainly may be a block by treatment interaction at another stage in the design. For example BP may exist, in which case we can see no reason for the possibility that BVP cannot exist separate from BP.

All this is not to say, in many agricultural experiments where blocks effects and block error are not separable or the experimenter does not want to separate them, that he should not run his split plot analysis as given in Table 7. In this case the experimenter has used a model that he 'knows' is correct when he began his experiment.

Our experience in various other fields is that experimenters do not have this information and may gain information by using the flexible model (7) and test this model using the analysis in Table 8. The use of the restriction errors at the various stages allows the investigator to think about various sources of variation that are covered up when only the analysis in Table 7 is carried out.

NUMERICAL EXAMPLE

Using the actual data from Beeson [1965], the analysis turned out to be as in Table 9. When the correct tests are made, only one source, pulse rates,

TABLE 9

ANOVA FOR DATA USING MODEL (7)

Source	D. F.	MS
Blocks (B_i)	1	
First restriction error ($\delta_{(i)}$)	0	17.52
Valve types (V_j)	3	420.41
Interaction (BV_{ij})	3	77.19
Second restriction error ($\omega_{(ij)}$)	0	
Pulse rates (P_k)	5	427.64*
Interaction (BP_{ik})	5	51.97
Interaction (VP_{jk})	15	133.79
Interaction (BVP_{ijk})	15	56.84
Within error ($\epsilon_{(ijk)}$)	0	

* Indicates significance at $\alpha = 0.05$.

is significant at the $\alpha = 0.05$ level. However, the mean squares of the interactions with blocks at the various stages are about the same size, indicating that the actual variance components with blocks are possibly zero (from model (7)). Using a certain criterion for pooling, the more liberal 'sometime poolers' may even obtain the analysis of Table 10. This shows that all effects

TABLE 10

ANOVA AFTER OBTAINING 'POOLED ERROR' FROM ALL SOURCES WITH BLOCKS FROM TABLE 9

Source	D. F.	MS
Valve types	3	420.41**
Pulse rates	5	427.64**
Valve types × pulse rates	15	133.79*
Pooled error	24	56.73

** Indicates significance at $\alpha = 0.01$.

and the interaction are significant; however, this last analysis is valid only if the errors are poolable and pooling is correct. In general, for a correct analysis, all restrictions on randomization must be considered before pooling procedures are undertaken.

CONCLUSIONS

(1) It is recommended that a component be added to analysis of variance models for each restriction on randomization to account for possible error caused by that restriction.

(2) These restrictions must be recognized prior to running the experiment,

so that preliminary models can indicate the problems that will be encountered in the analyses if such a designed experiment is run.

(3) Pooling procedures to increase the degrees of freedom for the appropriate errors may be used only after the data are analyzed according to the model that depicts all the restrictions on randomization that occurred in the actual experiment.

(4) There seems to be no reason for the restriction error not to be used in models for any designed experiment, for example in Latin squares or any complex incomplete block designs.

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'RESTRICTION ERRORS' POUR LES MODELES LINEAIRES (UNE AIDE POUR DEVELOPPER DES MODELES DANS LES EXPERIENCES PLANIFIEES)

RESUME

Une composante d'erreur aléatoire est introduite dans des modèles linéaires qui servent à analyser les données d'expériences planifiées. Ce taux d'erreur est appelé 'restriction error' car il est utilisé chaque fois qu'il y a restriction sur la randomisation.

L'utilité de cette notion est discutée dans les blocs complets randomisés, les expériences factorielles emboitées, et les split plot. Un exemple d'une expérience sur des valves cardiaques est donné.

Enfin, le concept sert à la discussion du problème de l'erreur dans les expériences en split plot.

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