THE MISUNDERSTOOD SPLIT PLOT

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April, 1974

BU-505-M in the Mimeo Series of the Biometrics Unit

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Abstract

As exemplified in statistical textbooks and other publications, there is often a misunderstanding of the relationship of statistical design and the role of confounding on the resulting statistical analyses and statistical inferences. Since the split plot design and its multitude of variations is a commonly occurring experiment design, it was selected as a basis to discuss appropriate statistical procedures and analyses. In order to have a common ground for discussion a number of items, including the textbook split plot and split block experiment designs, are defined. Standard textbook analysis of variance (ANOVA) procedures are described. Alternate ANOVA's, alternate experiment designs for whole plots, and alternate experiment designs for split plots are then discussed. Analogies, differences, and variations of the standard split plot and split block experiment designs are discussed. The dependence of split plot and whole plot analyses of variance is considered. In the final section of the paper eight rules to follow in analyzing data from complexly designed investigations are presented, and three algorithms are given for keying-out the degrees of freedom in an ANOVA, for computing sums of squares in an ANOVA, and for determining appropriate error variances.

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* In the Mimeo Series of the Biometrics Unit, Cornell University. Partially supported by Public Health Research Grant 5-R01-GM-05900, National Institutes of Health.

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1. Introduction

During the course of statistical consulting, of serving as a book reviews editor, in talking with statisticians, in reading the statistical literature, and in participating in panels and discussions on statistical applications, it has become quite apparent that many statisticians do not understand the effect of confounding and of statistical design on statistical analyses and the subsequent statistical inferences. The main source of confusion appears to stem from whether a valid error mean square must be ascertained or whether it should be defined. The more mathematically-oriented individuals tend to define the error variance (and linear model) whereas the more experimentally-minded individuals tend to ascertain which experimental components give rise to a valid estimate of error variance. The tendency to define an error variance is predominant in statistical pedagogy.

The misunderstanding of the role of confounding on the subsequent statistical procedures is readily apparent in discussions concerning the split plot and split block designs. The purpose of this paper is to first define a number of quantities in order to be certain that the reader is using the same terminology as the author; then a discussion of the classical or textbook version of the split plot and split-split plot designs is given. Since a single form of the analysis of variance (ANOVA) is universally presented, alternate ANOVA's are discussed in the third section. Also, since only one experiment design is usually presented for whole plot treatments and for split plot treatments, alternate experiment designs are presented in the fourth and fifth sections. The analogies and differences between split plot and split block designs are discussed in section six. In section seven, eight variations of standard split plot and split block designs are presented. The dependence of split plot analyses on whole plot analyses is discussed in section eight, while rules and algorithms for obtaining ANOVA's from complex experiments are given in the last section. Error variances for the

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various contrasts are indicated in each of the above cases.

Before proceeding further a number of definitions are required. First, a treatment is a single entity of interest to an experimenter. The selection of the set of treatments to be studied or compared in a comparative experiment is the treatment design. The arrangement of the treatments in the experiment is denoted as the experiment design. The smallest unit to which one treatment is applied is called an experimental unit. An observational unit is the smallest unit on which an observation is made; often the observational unit and the experimental unit represent the same unit, but in repeated measurements situations and in cases wherein the experimental unit is composed of several separate entities, the observational unit is smaller than the experimental unit. Confusion between these units can, and has, lead to difficulties in statistical analysis of data.

In split plot designs at least two different experimental units are utilized. Also, in this design, a two-factor factorial is usually involved. Let one of the factors (or set of factors) be denoted by a with p different levels, $a_1,a_2,a_3,\cdots,a_p$ and let the second factor (or set of factors) be denoted by b with q different levels, $b_1,b_2,b_3,\cdots,b_q$. Denote the whole plot treatments to be the a_i and the split plot treatments to be the b_j, $j=1,2,\cdots,q$. A whole plot treatment experimental unit is the smallest unit to which one whole plot treatment, a_i, is applied. Each whole plot is subdivided into split plot treatment experimental units; this unit is the smallest unit to which one split plot treatment, b_j, is applied. A whole plot treatment has sometimes been called a one-way whole plot treatment, and the split plot treatments have sometimes been denoted as sub-plot treatments. If the split plot treatment is further subdivided and if additional treatments, say c_h, $h=1,2,\cdots,k$, are applied to the subunits of the split plot treatment experimental unit, the c_h are denoted as the split-split (or sub-sub) plot treatments and the smallest unit to which a c_h is applied is defined to be the split-split plot treatment experimental unit. There can be additional splits to extend the definitions and concepts (see Federer [1955], page 294, e.g.). A valid estimate of the error variance for a treatment contrast has been defined by Fisher [1966] as one which contains all sources of variation inherent in the variation among treatment effects except that portion of the variance due specifically to the treatments themselves.

A literature coverage is not envisioned here. If such is desired, the reader is referred to Federer and Balaam [1972], classification E12, for literature on this subject. A discussion of these designs may be found in Cochran and Cox [1957] chapter 7, Federer [1955] chapter X and sections XII-3 to XII-5, XIII-4, XVI-5, and XVI-6, and Kempthorne [1952] chapter 19 and section 24.5. Furthermore, specific literature citations of misuse will not be given as the author wishes to stress the positive aspects of appropriate statistical analyses for experimental data. Nothing is to be gained from pointing out published examples of misuse.

2. The Textbook Split Plot and Split-Split Plot Designs

The almost universally illustrated example of a split plot design appearing in textbooks is the type illustrated by Yates [1937] wherein the whole plots are arranged in a randomized complete block design and the split plot treatments are randomly allotted to the experimental units within each whole plot. Example 2.1 is an illustration of such a layout for r=4 complete blocks, p=3 whole plot treatments, and q=4 split plot treatments.

Example 2.1.

Block I

b_1	b_2	b_3
b_0	b_0	b_1
b_2	b_3	b_0
b_3	b_1	b_2
a_1	a_0	a_2

Block II

b_1	b_3	b_0
b_3	b_1	b_1
b_2	b_2	b_3
b_0	b_0	b_2
a_1	a_2	a_0

Block III

b_3	b_0	b_1
b_2	b_3	b_0
b_0	b_2	b_3
b_1	b_1	b_2
a_2	a_1	a_0

Block IV

b_2	b_1	b_0
b_0	b_3	b_3
b_1	b_2	b_1
b_3	b_0	b_2
a_0	a_1	a_2

There are r=4 randomizations for the a_i treatments and rp=4(3)=12 randomizations for the b_j treatments.

The standard textbook analysis of variance presented has the following form almost without exception:

Source of variation	Degrees of freedom		F-test
	General	Example 2.1	
Total	rpq	48	
Correction for mean	1	1	
Whole plot analysis			
Complete blocks = R	r-1	3	
Among whole plot treatments = A	p-1	2	
Error (a) = R × A	(r-1)(p-1)	6	
Split plot analysis			
Among split plot treatments = B	q-1	3	
A × B	(p-1)(q-1)	6	
R × B : A = error (b)*	p(r-1)(q-1)	27	

* See following page footnote.

Occasionally, the A × B interaction mean square is used as the denominator for an F-test involving the B mean square. This is for the situation wherein the a_i represent a random sample from a population of a_i and inferences are being made to the entire population. For the fixed effects case, some discussion is presented concerning the error variances for comparing two levels of b_j for one a_i level, two a_i levels for one b_j level, and two b_j levels from different a_i levels (see Cochran and Cox [1957], section 7.16, and Federer [1955], example X-1). We shall be considering the treatments as fixed effects and the blocks as random effects throughout the ensuing discussion.

Another type of textbook example is the one wherein ℓ locations (or laboratories) are involved and a designed experiment, usually a randomized complete block design, is conducted at each location. Here the whole plots are locations (or laboratories), and there is only <u>one</u> replicate of whole plots. For this case the analysis of variance (ANOVA) for r blocks of a randomized complete block design with t treatments is given as:

Source of variation	Degrees of freedom	F-test
Total	$r\ell v$	
Correction for mean	1	
Locations (laboratories) = L	ℓ-1	⤴
Blocks within locations = R : L	ℓ(r-1)	⤵
Treatments = T	(t-1)	⤴ ⤴
Treatments by locations = T × L	(ℓ-1)(t-1)	⤵⤴ or ⤵⤴
Treatments by blocks within locations=R×T:L	ℓ(r-1)(t-1)	⤵ ⤵

The above design is not usually included in discussions of split plot design analyses, but is sometimes treated under repetitions of a design. Also, note that the R : L* sum of squares is not partitioned into an R with (r-1) degrees of freedom and an R × L sum of squares with (r-1)(ℓ-1) degrees of freedom. This would

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* The notation R : L is used to indicate that block contrasts are nested within locations. The symbol to the left of the colon is nested within those to the right of the colon.

not be correct since the numbering of replications (blocks) at each location (laboratory) is purely arbitrary, that is replication 1 at location 1 has nothing in common with replication 1 at location 2. Any replication at a location could have been designated as replication 1. Hence, it is meaningless to partition the R : L sum of squares into an R and an R × L sum of squares. This is <u>not</u> a three-factor factorial but is a two-factor factorial (blocks and treatments) nested within a third factor, locations.

If the split plot experimental unit is further subdivided and if an additional set of treatments, say $c_1, c_2, \cdots, c_k$ are applied to the subdivisions of the split plot treatments, then with proper randomization, a split-split plot experiment design results. The c_h are called the split-split plot treatments. Such a design is illustrated in example 2.2. Obviously the splitting can be continued as long as it is feasible and desirable.

<u>Example 2.2</u>. For the split plot design in example 2.1, suppose that it was possible to further subdivide the split plot experimental unit into two split-split plot experimental units for the two split-split plot treatments c_0 and c_1 as follows:

Block I			Block II			Block III			Block IV		
b_1 c_1/c_0	b_2 c_1/c_0	b_3 c_0/c_1	b_1 c_1/c_0	b_3 c_0/c_1	b_0 c_1/c_0	b_3 c_1/c_0	b_0 c_1/c_0	b_1 c_0/c_1	b_2 c_1/c_0	b_1 c_1/c_0	b_0 c_0/c_1
b_0 c_0/c_1	b_0 c_1/c_0	b_1 c_1/c_0	b_3 c_1/c_0	b_1 c_0/c_1	b_1 c_0/c_1	b_2 c_1/c_0	b_3 c_0/c_1	b_0 c_1/c_0	b_0 c_1/c_0	b_3 c_0/c_1	b_3 c_1/c_0
b_2 c_0/c_1	b_3 c_0/c_1	b_0 c_1/c_0	b_2 c_0/c_1	b_2 c_0/c_1	b_3 c_0/c_1	b_0 c_0/c_1	b_2 c_1/c_0	b_3 c_0/c_1	b_1 c_1/c_0	b_2 c_0/c_1	b_1 c_0/c_1
b_3 c_1/c_0	b_1 c_1/c_0	b_2 c_0/c_1	b_0 c_0/c_1	b_0 c_0/c_1	b_2 c_1/c_0	b_1 c_1/c_0	b_1 c_0/c_1	b_2 c_1/c_0	b_3 c_1/c_0	b_0 c_1/c_0	b_2 c_0/c_1
a_1	a_0	a_2	a_1	a_2	a_0	a_2	a_1	a_0	a_0	a_1	a_2

There are r=4 randomizations for the a_i, rp=12 randomizations on the b_j, and rpq=48 randomizations on the c_h treatments. One a_i treatment is applied to a set of eight split-split plot experimental units and one b_j treatment is applied to two split-split plot experimental units. The textbook ANOVA for the above experiment design is:

Source of variation	Degrees of freedom: General	Degrees of freedom: Example	F-test
Total	rpqk	96	
Correction for mean	1	1	
<u>Whole plot analysis</u>			
Complete blocks = R	r-1	3	
Among a_i = A	p-1	2	↰
R × A = error (a)	(r-1)(p-1)	6	↲
<u>Split plot analysis</u>			
Among b_j = B	q-1	3	↰
A × B	(p-1)(q-1)	6	↰
R × B : A = error (b)	p(r-1)(q-1)	27	↲
<u>Split-split plot analysis</u>			
Among c_h = C	k-1	1	↰
A × C	(p-1)(k-1)	2	↰
B × C	(q-1)(k-1)	3	↰
A × B × C	(p-1)(q-1)(k-1)	6	↰
R × C : A and B = error (c)	pq(r-1)(k-1)	36	↲

For situations wherein the pq treatments in a split plot design do not form a factorial arrangement, the first set of analyses in the next section may be appropriate. Some discussion of this situation is given by Federer [1955], section XIII-4, and literature citations on the general topic are presented.

3. Alternate ANOVA for Textbook Split Plot Design

The analysis of variance involving locations suggests an alternate ANOVA for the textbook split plot design. That is, one may consider the a_i whole plot treatments as locations and consider the randomized complete block design for the b_j treatments within **each** whole plot as follows:

Source of variation	d.f.	a_1 ss	a_2 ss	a_3 ss	...	a_p ss	Sum
Total	rq	T_1	T_2	T_3	...	T_p	ΣT_i
Correction for mean	1	C_1	C_2	C_3	...	C_p	ΣC_i
Blocks	(r-1)	R_1	R_2	R_3	...	R_p	ΣR_i
B treatments	(q-1)	B_1	B_2	B_3	...	B_p	ΣB_i
R × B (blocks × treatments)	(r-1)(q-1)	E_1	E_2	E_3	...	E_p	ΣE_i

Combining these single ANOVA's, we obtain the textbook split plot sums of squares as follows:

Source of variation	d.f.	Sum of squares
Total	rpq	ΣT_i
Correction for mean	1	compute = CT
Whole plot analyses		
Blocks within a_i = R : A	p(r-1)	ΣR_i
Blocks = R	r-1	compute = R
Blocks × A = error (a)	(r-1)(p-1)	ΣR_i - R
A	(p-1)	ΣC_i - CT
Split plot analyses		
B within A = B : A	p(q-1)	ΣB_i
B	q-1	compute = B
A × B	(p-1)(q-1)	ΣB_i - B
R × B : A = error (b)	p(r-1)(q-1)	ΣE_i

The above form of an analysis of variance for split plot experiments is particularly useful for computing single degree of freedom contrasts for the B treatment sums of squares. For example, suppose that the sum of squares for B is partitioned into k sets of contrasts such that each set is associated with n_g degrees of freedom such that $\sum_{g=1}^{k} n_g = q-1$ and with a sum of squares designated as B_{ig}. Then, the $\sum_{i=1}^{p} B_{ig}$ sum of squares is associated with pn_g degrees of freedom; the $B_{\cdot g}$ contrast over all levels of a is subtracted from $\sum_{i=1}^{p} B_{ig}$ to obtain the interaction sum of squares with $(p-1)n_g$ degrees of freedom.

The above form is also useful for partitioning the E_i sum of squares into various parts. One such partitioning would be to compute Tukey's one degree of freedom for nonadditivity sum of squares for each whole plot, say N_i, and to sum these over all whole plots, $\sum_{i=1}^{p} N_i$, to obtain a sum of squares with p degrees of freedom. If $N_i = (\sum_h e_{ih} \hat{e}_{ih})^2/\sum_h \hat{e}^2_{ih}$, then $(\sum\sum_{ih} e_{ih} \hat{e}_{ih})^2/\sum\sum_{ih} \hat{e}^2_{ih} = N$ would be the nonadditivity sum of squares over all whole plots a_i and $\Sigma N_i - N$ would be the corresponding interaction sum of squares with p-1 degrees of freedom. Similarly other single degree of freedom or a set of degree of freedom contrasts could be obtained and the residuals could be summed for possible use as an error variance.

Although the above partitioning and combining may appear obvious after once being pointed out, one wonders why textbook writers do not do this. On the other hand, some authors feel inclined to partition the error (b) sum of squares into R × B and R × A × B sums of squares. Although this computation can always be performed, it may be meaningless and incorrect. These sums of squares are confounded as may be shown from the second ANOVA described in section 2. Note that the replication numbering in each a_i is purely arbitrary and hence the R × B sum of squares is confounded with the R × A × B sum of squares resulting in an R × B : A sum of squares for the error (b) sum of squares.

The question arises as to when it might be meaningful to compute an R × B and an R × A × B sum of squares for a split plot experiment. If the blocks are a random sample from a population of blocks and if the whole plots are a random sample from whole plots, then it would not be meaningful to partition. However, if the blocks represent large overriding effects (e.g. locations or laboratories) which interact with levels of the factor B, then the R × B interaction should be partitioned out of the error (b) sum of squares. Hence, it can be seen that the population and sampling structure are the important items to be considered in the analysis of experimental data. The statistical analyst should not be misguided by the numbering system or by an "apparent" analogy to a three-factor factorial. Over-emphasis on the computing aspects of sums of squares has led students and practitioners of statistical methodology into the trap described above.

4. Alternate Experiment Designs for the Whole Plots

The whole plot treatments may be arranged in any appropriate experiment design necessary to control the heterogeneity in the experiment. For example, a completely randomized design, a latin square design, a Youden design, an incomplete block design, a lattice square design, or any one of a number of experiment designs may be utilized. Example 4.1 illustrates the partitioning of the degrees of freedom in the ANOVA for k^2 whole plots arranged in a balanced lattice square design with the split plot treatments being randomly allocated to the split plot experimental units within each whole plot.

Example 4.1. The following is the ANOVA for $k^2=p$ whole plot treatments arranged in a balanced lattice square design with $k+1=r$ complete blocks. The q split plot treatments are randomly allocated to the experimental units within each whole plot.

Source of variation	Degrees of freedom	
	k	k=3
Total	$q(k^3+k^2)$	36q
Correction for the mean	1	1
Whole plot analysis		
Blocks = R	k	3
A (ignoring rows and columns)	k^2-1	8
Rows (eliminating treatments; ignoring columns)	k^2-1	8
Columns (eliminating treatments and rows)	k^2-1	8
Intrarow-column error	$(k^2-1)(k-2)$	8
Split plot analysis		
B	q-1	q-1
A × B	$(k^2-1)(q-1)$	8(q-1)
R × B : A	$k^3(q-1)$	27(q-1)

Note that the split plot analysis is unchanged by the experiment design utilized for whole plots, and that this analysis is simply a partitioning of the within whole plot variation which is orthogonal to the among whole plot variation.

5. Alternate Experiment Designs for the Split Plots

Any appropriate experiment design may be utilized for the split plot treatments. Two types of design need to be considered. First consider experiment designs for the split plot treatments <u>within each whole plot</u> as described in example 5.1.

<u>Example 5.1.</u> Suppose that the p=3 whole plot treatments (a_1, a_2, a_3) are in a randomized complete blocks design composed of r=5 replicates. Furthermore, suppose that the q=4 split plot treatments (b_1, b_2, b_3, and b_4) are arranged in a 4-row (order) by 5-column Youden design. One such arrangement, where the order within each whole plot is taken into account is:

Block I			Block II			Block III			Block IV			Block V		
a_2	a_1	a_3	a_1	a_3	a_2	a_2	a_3	a_1	a_2	a_1	a_3	a_3	a_2	a_1
b_2	b_3	b_1	b_4	b_1	b_3	b_4	b_4	b_1	b_1	b_1	b_2	b_3	b_1	b_2
b_4	b_4	b_3	b_1	b_2	b_2	b_1	b_2	b_2	b_4	b_4	b_4	b_1	b_3	b_3
b_3	b_2	b_4	b_3	b_3	b_1	b_2	b_1	b_4	b_3	b_3	b_3	b_2	b_4	b_1
b_1	b_1	b_2	b_2	b_4	b_4	b_3	b_3	b_3	b_2	b_2	b_1	b_4	b_2	b_4

The ANOVA for each whole plot a_i is:

Source	df	a_1 ss	a_2 ss	a_3 ss	Sum of ss
Total	20	T_1	T_2	T_3	ΣT_i
Correction for mean	1	A_1	A_2	A_3	ΣA_i
Columns	4	C_1	C_2	C_3	ΣC_i
Orders (ignoring b_j)	3	O_1	O_2	O_3	ΣO_i
Treatments (eliminating orders)	3	B_1	B_2	B_3	ΣB_i
Residual	9	E_1	E_2	E_3	ΣE_i

Note that a 4-row (order) by 5-column Youden design was constructed for each a_i, and hence the above ANOVA's.

A combined ANOVA for the above is:

Source	df	ss
Total	60	ΣT_i
Correction for mean	1	compute = CT
<u>Whole plot analysis</u>		
Whole plot treatments = A	2	ΣA_i - CT
Columns: A	12	ΣC_i
Complete blocks = R	4	compute = C
R × A	8	ΣC_i - C
<u>Split plot analysis</u>		
Treatments (eliminating orders): A	9	ΣB_i
B	3	compute = B
A × B	6	ΣB_i - B
Orders (ignoring b_j): A	9	ΣO_i
Residual: A	27	ΣE_i

The second type of design for split plot treatments completes the design <u>within each complete block</u> for the whole plot treatments. Such a design is illustrated in example 5.2. The orthogonality aspects are lost in this type of design. Sometimes the statistical analysis becomes cumbersome. The solution of a set of simultaneous equations was avoided for the analysis given in example 5.2.

<u>Example 5.2</u>. The four whole plot treatments A, B, C, and D were laid out in a 4 × 4 latin square design. The four split plot treatments a, b, c, and d were laid out in a 4 × 4 latin square design within <u>each</u> column of the whole plot latin square design. The plan follows:

A cbda	C abcd	B dacb	D abdc
B bcad	A dabc	D bcad	C bdca
D adbc	B cdab	C cbda	A dcab
C dacb	D bcda	A adbc	B cabd

2^4 factorial notation $\left(e_r f_s g_u h_v\right)$

Aa = 0000	Ca = 0100
Ab = 0010	Cb = 0110
Ac = 0001	Cc = 0101
Ad = 0011	Cd = 0111
Ba = 1000	Da = 1100
Bb = 1010	Db = 1110
Bc = 1001	Dc = 1101
Bd = 1011	Dd = 1111

Note that for the 16 columns the following confounding scheme results:

1	2	3	4	5	6	7	8
$(EH)_1$	$(EH)_0$	$(EH)_1$	$(EH)_0$	$(FH)_1$	$(EH)_0$	$(FH)_0$	$(EH)_1$
$(EFG)_0$	$(EFG)_1$	$(EFG)_1$	$(EFG)_0$	$(EFG)_1$	$(EFG)_0$	$(EFG)_1$	$(EFG)_0$
$(FGH)_1$	$(FGH)_1$	$(FGH)_0$	$(FGH)_0$	$(EGH)_0$	$(FGH)_0$	$(EGH)_1$	$(FGH)_1$

9	10	11	12	13	14	15	16
$(EG)_0$	$(EG)_1$	$(EG)_1$	$(EG)_0$	$(EG)_1$	$(EH)_1$	$(EG)_0$	$(EH)_0$
$(EFH)_0$	$(EFH)_1$	$(EFH)_0$	$(EFH)_1$	$(FH)_1$	$(FG)_0$	$(FH)_0$	$(FG)_1$
$(FGH)_0$	$(FGH)_0$	$(FGH)_1$	$(FGH)_1$	$(EFGH)_0$	$(EFGH)_1$	$(EFGH)_0$	$(EFGH)_1$

The ANOVA is:

Source	df		ss
Total	64		
Correction for mean	1		
Rows	3		
Columns	3		
Whole plot treatments	3		Sum of following three ss
E = - A - C + B + D		1	
F = - A - B + C + D		1	
EF = A + D - B - C		1	
Error for whole plot treatments	6		
Split plot analysis			
Orders within columns (ignoring interactions)	12		
Split plot treatments	3		Sum of following three ss
G = b + d - a - c		1	From all 16 columns
H = c + d - a - b		1	" " " "
GH = a + d - b - c		1	" " " "
W.P. × S.P. (eliminating orders within columns)	9		Sum of following nine ss
EG' (5/8 information)		1	Compute from columns 1-8, 14, 16
EH' (1/2 ")		1	" " " 5, 7, 9-12, 13, 15
EGH' (7/8 ")		1	" " " 1-4, 6, 8-16
FG' (7/8 ")		1	" " " 1-13, 15
FH' (3/4 ")		1	" " " 1-4, 6, 8-12, 14, 16
FGH' (3/8 ")		1	" " " 5, 7, 13-16
EFG' (1/2 ")		1	" " " 9-16
EFH' (3/4 ")		1	" " " 1-8, 13-16
EFGH'(3/4 ")		1	" " " 1-12
Error for split plot treatments	24		Error for above 12 d.f.

By combining knowledge of analyses for factorial treatment designs and confounding concepts, it is possible to obtain the complete analysis of variance and solutions for parameters without solving a set of simultaneous equations.

Note that if the 4 x 4 latin squares had been within each plot treatment rather than within each column, there would have been no confounding of interaction components with orders within columns and full information would have been obtained on each of interaction contrasts instead of only partial information. Also, note that there is some interaction information in the orders within columns (ignoring interaction contrasts) sum of squares which could be recovered if desired. These contrasts would involve the comparison of the levels of effects in the columns in which the effect is confounded. Three degrees of freedom would remain for an error sum of squares for these contrasts. The two estimates of the effects could be combined in the usual manner for combining estimates with different variances. Also, one could recover interblock (intercolumn) information in the usual manner for a pseudo-factorial (see, e.g., Federer [1955] and Kempthorne [1952]). The contrast of a level of an effect for columns in which the effect is confounded with the same level in the columns in which the effect is unconfounded, would produce a sum of squares with nine degrees of freedom. These nine together with the three described above would produce a sum of squares with 12 degrees of freedom, which would be free of treatment effects. The resulting mean square could be used to obtain an estimate of the interblock variance $\sigma_\epsilon^2 + 4\sigma_\gamma^2$. The expectation of the above mean square would be $\sigma_\epsilon^2 + 3\sigma_\gamma^2$ and is obtained directly from the theory of pseudo-factorial analyses. Then, the usual estimate of treatment means with recovery of interblock information may be obtained.

6. Split Plot-Split Block Analogies and Differences

In most split block experiment designs discussed in the literature, two-way whole plots are present in each block and a two-factor factorial represents the treatment design. If a randomized complete block design is constructed for the factor a, if one constructs a second set of whole plots for factor b such that any level of factor b contains all levels of factor a, and if there are r randomizations for levels of both factors a and b, a standard textbook example of a split block experiment design results. One such design is presented in example 6.1. Note that either factor could be a factorial treatment design. Also, note that r complete blocks are required for the standard split block design.

Example 6.1. Suppose that there are three levels of factor a, a_1, a_2, and a_3, and that factor b is a 2 X 2 factorial consisting of the four treatments $b_1 = c_0d_0$, $b_2 = c_1d_0$, $b_3 = c_0d_1$, and $b_4 = c_1d_1$; further suppose that r=3 complete blocks are utilized. The nonrandomized plan is:

Block I

	a_0	a_1	a_2
b_1			
b_2			
b_3			
b_4			

Block II

	a_0	a_1	a_2
b_1			
b_2			
b_3			
b_4			

Block III

	a_0	a_1	a_2
b_1			
b_2			
b_3			
b_4			

In order to obtain the split block experiment design randomly allot the three a_i levels to the "columns" in each block and then randomly allot the four $b_j = c_gd_h$ levels to the "rows" in each block. There are three randomizations for the a_i levels and three for the b_j levels. If the experimenter had desired to do so, the column orders within complete blocks could have been used to form a 3 X 3 latin square for the a_i levels; also, if variation due to row order needs to be controlled, a 4 X 3 Youden design could have been used. Such a nonrandomized design follows:

Block I

	a_0	a_1	a_2
b_0			
b_1			
b_2			
b_3			

Block II

	a_1	a_2	a_0
b_3			
b_0			
b_1			
b_2			

Block III

	a_2	a_0	a_1
b_2			
b_3			
b_0			
b_1			

This experiment design is not of the standard textbook type but it is a split block or two-way whole plot experiment design. One of the twelve 3 × 3 latin square arrangements is randomly selected for the a_i treatments and then the b_j are randomly allotted to the "rows" in block I, then to block II except that no b_j is allowed to appear twice in the same row, and finally to block III except that no b_j is allowed to appear more than once in a row.

The ANOVA for the first design above is:

Source	df	df (general)	F-test
Total	36	pqr	
Correction for mean	1	1	
Blocks = R	2	r-1	
Factor a = A	2	p-1	
A × R = error (a)	4	(p-1)(r-1)	
Factor b = B	3	q-1	
B × R = error (b)	6	(q-1)(r-1)	
A × B	6	(p-1)(q-1)	
A × B × R = error (ab)	12	(p-1)(q-1)(r-1)	

There are three error terms introduced because of the confounding scheme utilized. In most experimental situations it would not be appropriate to consider the A × R, the B × R, and the A × B × R mean squares to be estimates of the same error variance σ^2 . The reason for this is that the experimental unit size differs and hence can generally be expected to cause unequal variances. This fact is sometimes ignored by statisticians and experimenters alike.

The ANOVA for the second experiment treatment design given above follows:

Source	df	df (general)	F-test
Total	36	qp^2	
Correction for mean	1	1	
Blocks = R	2	p-1	
A	2	p-1	
Column order in blocks	2	p-1	
Error (a)	2	(p-1)(p-2)	
B (eliminating row order)	3	q-1	
Row order (ignoring b_j)	3	q-1	
Error (b)	3	(q-1)(p-2)	
A × B	6	(p-1)(q-1)	
A × B × R = error (ab)	12	$(p-1)^2(q-1)$	

In the above example, it may be noted that the particular analysis of variance required by the experiment design for factor A does not affect the ANOVA for factor B and for the A × B interaction. Also, it should be noted that different error terms are required for factor a, for factor b, and for the two-factor interaction. The number of randomizations on levels of a and on levels of b are the same as the number of complete blocks in the standard textbook split block design.

In actual experimentation involving biological organisms, a common mistake is to consider the split block design as a split plot design. For some experimental situations involving animals or humans, it is sometimes quite difficult to ascertain if there are whole plots and split plots or two-way whole plots. With plant experiments, the difference is usually apparent and the number of randomizations is easily determined. If the experimental units for the a_i and b_j differ in size and/or if the number of randomizations for the a_i is r and for the b_j is rp, then a split plot design is indicated. The experimental unit size can be different and the number of randomizations can differ but the design may not be a split plot design (see example 7.3). Considerable study of the method of experimentation may be involved before one can determine an appropriate ANOVA for a given experiment. Perhaps the most common mistake is to confuse the split block, split plot, and three-factor factorial designs in the resulting ANOVA for an experiment. Several examples in textbooks can be readily cited, when this is the case.

7. Variations and Deviations of Standard Split Plot and Split Block Designs

Eight different deviations or variations from standard textbook split plot and split block designs are discussed in examples 7.1 to 7.8. These examples have been encountered in practice.

Example 7.1. A frequent deviation from a split plot design and one which frequently appears to escape the attention of the statistician and experimenter alike is the experiment wherein only one replicate of the standard split plot design is used. The following experiment was recently encountered where the numbers in the table refer to number of plants used for each treatment:

Plant types	Intensity of light		
	a_1	a_2	a_3
b_1 = tomato = T	8	8	8
b_2 = pigweed = P	8	8	8
b_3 = T + P	8	8	8

The plant types were grown in three different growth chambers with the light intensity in each growth chamber being one of the a_i levels. Furthermore, the eight plants of one plant type, b_2, were grown together in one greenhouse flat. Thus, for each a_i, one replicate of a randomized complete block for the b_j treatments was used. This latter situation is a frequently encountered experiment design where the correct ANOVA should be:

Source	df	df (general)	F-test
Total	24	kq	
Correction for mean	1	1	
Blocks = R	0	0	
Factor b = B	2	q-1	⌐
R × B	0	0	⌐
Plants: R and B	21	q(k-1)	

The last mean square above is often utilized as the error mean square for the b_j levels. There are many situations for which this is an erroneous procedure, one being when competition between items in each experimental unit occurs. In some instances, this mean square could be used to test for an R × B interaction. For the single replicate of a split plot design, the ANOVA for this example is:

Source	df	df (general)	F-test
Total	72	pqk	
Correction for mean	1	1	
Light intensities = A	2	p-1	
Repetitions of a_i = R	0	0	
R × A = error (a)	0	0	
Plant types = B	2	q-1	
A × B	4	(p-1)(q-1)	
R × B : A = error (b)	0	0	
Plants : B and A	63	pq(k-1)	

The experimenter had planned to use, and he probably did so despite advice to the contrary, the plants : B : A mean square as the error mean square in F-tests for the A, B, and A × B mean squares. This would be an inappropriate procedure for this experiment.

Example 7.2. A not uncommon deviation of the split plot design is to use a randomized complete block design for the a_i levels, then to lay out the b_j levels as a second way whole plot or split block but to use the same systematic layout in each block, and then to compute an ANOVA and F-tests as if the b_j were split plot treatments within the a_i whole plots. Even though more information was desired on the b_j levels, no randomization was performed because it was simpler experimentally to perform the experiment, because the experimenter forgot to randomize, or because he did not realize the need fcr randomization. One such example is given by Federer [1955], page 296, and will not be repeated here.

Example 7.3. Suppose that the following schematic plan was the basis for an experiment design involving p=3 levels of factor a and q=4 levels of factor b:

	Block I			Block II			Block III			Block IV		
	a_0	a_1	a_2	a_0	a_1	a_2	a_0	a_1	a_2	a_0	a_1	a_2
b_0												
b_1												
b_2												
b_3												

Four randomizations are used for levels of factor a but only two randomizations are used for levels of factor b . Thus the a_i levels are in a randomized complete block design with r=4 replicates and the b_j levels are in a randomized complete block design with r=2 replicates. If the b_j levels had been randomized within each block, a standard split block experiment design would have resulted. Note this fact in the following analysis. The ANOVA and F-tests for the above design are given below:

Source	df		F-tests
Total	48		
Correction for mean	1		
Blocks for A = RA	3		
Blocks for B = RB		1	
Block I vs. Block II = C		1	
Block III vs. Block IV = D		1	
A	2		↰
RA × A	6		⌡
B	3		↰
RB × B	3		⌡
B × C	3		not usable
B × D	3		not usable
A × B	6		↰
A × B × RA	18		⌡

The experimental unit size for the a_i is one-third of a block whereas the experimental unit size for the b_j is one-fourth of two blocks.

<u>Example 7.4</u>. Another variation of the split block and split plot designs is given on the following page. Four randomizations of treatments I, II, and III were used to form a randomized complete block design for the p_i levels. Likewise, four randomizations for stages 1, 2, and 3 were used to form a randomized complete block design for the s_j levels. Thus, the p_i and s_j levels are arranged in a standard split block design. Then, the t_g levels were designed as split plot treatments within the s_j whole plots and the levels of t_g were randomly allotted to the seven experimental units within <u>each</u> s_j whole plot. Therefore, the s_i and t_g levels are in a standard split plot design but the s_j and t_g levels are split block treatments to the p_i levels. The produce from each combination was subdivided into three parts -- grass, legume, and weeds. Consequently, the analysis of variance and the associated F-tests are:

Source of variation	df	F-test
Total	3×3×7×4×3=756	
Correction for mean	1	
Complete blocks = R	3	
Preconditioning = P	2	
R × P = error (p)	6	
Stages = S	2	
R × S = error (s)	6	
P × S	4	
R × P × S = error (ps)	12	
Treatments = T	6	
T × S	12	
R × T : S = error (t)	54	
T × P	12	
T × P × R = error (tp)	36	
T × P × S	24	
T × P × S × R = error (tps)	72	
Parts (grass, alfalfa, weeds) = C	2	
C × P	4	
C × S	4	
C × P × S	8	
C × T	12	
C × P × T	24	
C × S × T	24	
C × P × T × S	48	
C × R within all others = error (c)	63(2)(3)378	

Block II

66	67	72	73	78	79	84	85	90	91	96 I	97	102	103	108	109	114	115	120	121	126
65	68	71	74	77	80	83	86	89	92	95 II	98	101	104	107	110	113	116	119	122	125
64	69	70	75	76	81	82	87	88	93	94 III	99	100	105	106	111	112	117	118	123	124
7	6	3	5	2	1	4	1	4	6	2	7	3	5	5	6	3	1	7	2	4
Stage 3							Stage 1							Stage 2						

Block III

129	130	135	136	141	142	147	148	153	154	159 III	160	165	166	171	172	177	178	183	184	189
128	131	134	137	140	143	146	149	152	155	158 I	161	164	167	170	173	176	179	182	185	188
127	132	133	138	139	144	145	150	151	156	157 II	162	163	168	169	174	175	180	181	186	187
3	2	1	6	4	5	7	5	4	2	3	7	6	1	6	2	4	3	7	5	1
Stage 3							Stage 2							Stage 1						

Block I

3	4	9	10	15	16	21	22	27	28	33 III	34	39	40	45	46	51	52	57	58	63
2	5	8	11	14	17	20	23	26	29	32 II	35	38	41	44	47	50	53	56	59	62
1	6	7	12	13	18	19	24	25	30	31 I	36	37	42	43	48	49	54	55	60	61
3	6	2	5	1	4	7	6	3	7	5	4	2	1	1	2	6	4	7	3	5
Stage 1							Stage 2							Stage 3						

Block IV

192	193	198	199	204	205	210	211	216	217	222 II	223	228	229	234	235	240	241	246	247	252
191	194	197	200	203	206	209	212	215	218	221 III	224	227	230	233	236	239	242	245	248	251
190	195	196	201	202	207	208	213	214	219	220 I	225	226	231	232	237	238	243	244	249	250
7	2	3	5	1	4	6	7	6	3	2	4	1	5	1	4	5	6	3	2	7
Stage 1							Stage 3							Stage 2						

Preconditioning of soil

p_1 = I = 1/16 lb./A. 2-4-D
p_2 = II = 2/16 lb./A. 2-4-D
p_3 = III = None

Stage of growth

s_1 = stage 1
s_2 = stage 2
s_3 = stage 3

Treatment of young plants

1 = 2-4-D, 1/4 lb./A. = t_1
2 = 2-4-D, 1/2 lb./A. = t_2
3 = 2-4-D, 3/4 lb./A. = t_3
4 = MCP, 1/4 lb./A. = t_4
5 = MCP, 1/2 lb./A. = t_5
6 = MCP, 3/4 lb./A. = t_6
7 = Check = t_7

Crop: 1st year hay (Alfalfa)

There are seven different error mean squares in the above ANOVA. There is some validity for pooling the T × P × R and T × P × S × R mean squares for F-tests of the T × P and T × P × S mean squares. Even with this pooling six different error mean squares result.

The parts of the hay (grass, weeds, alfalfa) could have been designated as X_1, X_2, and X_3 and a multivariate ANOVA computed on the lines in the ANOVA above. Since the interaction terms with C are of importance to agronomists, the above univariate ANOVA is considered satisfactory. Also, before conducting a multivariate ANOVA on data of this nature the reader is referred to Finney [1956].

Example 7.5. As was pointed out in section 5, many experiment designs for the split plot treatments are possible. Some of these have been considered by Kempthorne [1952], Chapter 24, Federer [1955], sections XII-3 to XII-5, and Raktoe [1967]. For example, it would be possible to arrange the whole plots in a randomized complete block design (or some other) and then to arrange the split plot treatments in a lattice square or lattice rectangle design (see Yates [1940], Na Nagara [1957], Federer and Raktoe [1965, 1966], and the above references) within each whole plot treatment a_i . The conditions of the experiment and the need to control the heterogeneity determine the appropriate experiment design for any given situation.

Example 7.6. Another variation of the split plot design is the following example:

	Columns					
Rows	1	2	3	4	5	6
1	a_0 (b_1,b_2)	a_0 (b_1,b_3)	a_1 (b_3,b_4)	a_1 (b_2,b_4)	a_2 (b_1,b_4)	a_2 (b_2,b_3)
2	a_1 (b_1,b_4)	a_1 (b_2,b_3)	a_2 (b_1,b_2)	a_2 (b_1,b_3)	a_0 (b_2,b_4)	a_0 (b_3,b_4)
3	a_2 (b_3,b_4)	a_2 (b_2,b_4)	a_0 (b_1,b_4)	a_0 (b_2,b_3)	a_1 (b_1,b_3)	a_1 (b_1,b_2)

Within each a_i the b_j=4 treatments are arranged in a balanced incomplete block design in blocks of size k=2. The a_i treatments are orthogonal to rows and columns, the b_j treatments are orthogonal to the a_i treatments, the b_j treatments are orthogonal to rows but not to columns, and the A × B interaction is not orthogonal to columns. The ANOVA for each a_i is:

Source of variation	df a_0	df a_1	df a_2
Total	12	12	12
Correction for mean	1	1	1
Blocks (ignoring b_j treatments)	5	5	5
b_j treatments in a_i	3	3	3
Intrablock error	3	3	3

The combined ANOVA would have the following form:

Source of variation	df		F-test
Total	36		
Correction for mean : A	3		
Correction for overall mean		1	
A		2	
Blocks (ignoring b_j) : A	15		
Rows		2	
Columns (ignoring B and A × B)		5	
Remainder (ignoring B and A × B)		8	
B : A (eliminating columns)	9		
B (eliminating columns)		3	
A × B (eliminating columns)		6	
Intrablock error : A	9		
Blocks (eliminating b_j) : A	15		
Rows		2	
Columns (eliminating B and A × B)		5	
Remainder (eliminating B and A × B)		8	

In the above example, only a portion of the split plot treatments were included in each whole plot treatment experimental unit. This might be called an incomplete split block design as opposed to those considered to this point. These latter could be called complete split plot designs if a name is needed.

Example 7.7. The lattice square designs and the lattice rectangle designs mentioned in example 7.5 could in themselves be called a type of split block design where the rows and the columns represent the two-way whole plots. Even though this analogy could be made and note should be made of it, this is probably carrying the idea further than warranted and pedagogically desirable. If the k^2 treatments in the lattice square (or lattice rectangle) represent a $k \times k$ factorial, then the analogy is not so far-fetched.

Example 7.8. Suppose that one is interested in comparing six diets d_1, d_2, d_3, d_4, d_5, and d_6 and that five animals are to be used for each diet. Thirty animals are required and five are randomly allotted to each treatment. The animals are treated alike except for diet differences. Hence, the variation among animals within diets is considered to be an estimate of the error variance for comparing diets. Now suppose that the weights are obtained at seven different times. One form of analysis for these data is:

	df for weights at time t_i						
Source of variation	t_1	t_2	t_3	t_4	t_5	t_6	t_7
Total	30	30	30	30	30	30	30
Correction for mean	1	1	1	1	1	1	1
Diets	5	5	5	5	5	5	5
Animals within diets	24	24	24	24	24	24	24

Instead of actual weights one might consider weights adjusted for initial weights by covariance and/or might use weight gains in each time period in the above ANOVA's. Also, a similar ANOVA on total weight gain could be constructed.

In pooling the above ANOVA's, experimenters and statisticians sometimes consider the times to be split plot treatments, probably because the data are recorded in a manner for split plot treatments. This would be an erroneous procedure. Note that time periods are unreplicated and that there is only one arrangement of the time periods. Apparently the same kind of reasoning prevails here as for example 7.1. One particular pooled ANOVA for the above experiment would be:

Source of variation	df		F-test
Total	210		
Correction for mean	1		
Among t_j = T	6		
Among d_i : T = D : T	35		
Among d_i = D		5	
D × T		30	
Animals : D and T	7(24)		
Animals : D = A : D		24	
A : D × T		6(24)	

Alternatively, a multivariate ANOVA could have been constructed with the seven weights as multivariates. Other analyses for correlated measurements are possible.

In experiments involving time periods it is important to think clearly and rigorously about the entire experimental procedure. It is necessary to distinguish between <u>calendar time</u> (e.g. May 15) and <u>biological time</u> (number of days to first fruit set, first fruit ripe, etc.). The reader is referred to an experiment described by Snedecor and Cochran [1967], section 12.12, where cutting dates are replicated and to an experiment described by Federer [1955], section X-5.3, where the cutting dates are not replicated. At first glance these experiments appear to be similar but further study indicates the latter experiment is similar to the diets experiment above.

8. Dependence of Split Plot and Whole Plot ANOVA's

From the preceding discussion and examples, we can now state the following:

(i) If the q split plot treatments are randomly allotted to the q experimental units within each whole plot, the experiment design for whole plots does not affect the split plot analysis.

(ii) If a complete block experiment design for split plot treatments is used within each whole plot treatment, the form of the split plot analysis is unaffected by the statistical design for whole plot treatments.

(iii) In a standard split block or two-way whole plot experiment design, the analysis of variance for one factor and for the two-factor interaction is unaffected by the experiment design utilized for the second factor.

These facts become apparent from a study of the results to date and from the partitioning of the sums of squares in the ANOVA. If the split plot treatments are nested within the factor a and the blocking, then the within whole plot sum of squares is independent of the among whole plot sum of squares and the partitioning in each part leaves the other unaffected. When nesting does not occur in one of the categories (see, e.g., example 5.2), then the split plot and whole plot analyses are not independent.

9. Rules and Algorithms for Obtaining an ANOVA for a Complex Experiment Design

Eight rules and three algorithms are presented for obtaining an appropriate partitioning of degrees of freedom in the ANOVA and appropriate error variances for interval estimation and F-tests.

In the course of statistical consulting it becomes apparent that the experimenter usually does not know what type of design he has nor what type of confounding of effects is present in the experiment. Perhaps the only type of consulting the author receives is for completely and partially confounded experiments and surveys, but in nearly every case there is no simple textbook answer. Each investigation and its related statistical design must be approached as a unique situation and not one that appears on page X of textbook Y. It may be like that one on page X but more often than not there is something in the experiment for which no close analogy to a textbook example can be made. This leads to Rule I.

Rule I: Make no assumptions about the form of the statistical design; always determine the exact experimental procedure, not the stated one.

Quite often the investigator states that his statistical design was D when in fact it was X . One should always have the consultee describe the investigation in minute detail, and then one may come to a conclusion as to the statistical design.

Once one thinks he knows the statistical design, it is then possible to key-out the degrees of freedom for the ANOVA. However in doing this Rules II, III, IV, and V have been found essential.

Rule II: Determine the experimental unit for levels of each category (factor, block, etc.); then determine any common experimental units for combinations of all possible pairs of categories, then for all possible triplets of categories, etc.

Rule III: Count the number of randomizations for each category (factor, block, etc.) in the experiment; then count randomizations for combinations of levels for all possible pairs of categories, then for all possible triplets, etc.

Rule IV: Determine which category levels are nested within another category level and determine which are cross classified.

Rule V: Ignore complexity of design in first key-out of degrees of freedom; relate key-out to nearest known design.

Application of rules I through V should enable one to key-out the degrees of freedom in an ANOVA as described in algorithm I. Note that these rules were applied to the examples throughout the paper.

Algorithm I: Keying-out degrees of freedom in the ANOVA

1. At every step perform simplest key-out of degrees of freedom that is possible.
2. First determine total degrees of freedom and partition into one for the correction term and the remainder for the sum of squares corrected for the mean.
3. Key-out degrees of freedom for category or categories offering the least difficulty.
4. Key-out degrees of freedom for ANOVA's for all possible pairs of categories, then all possible triplets, etc., excluding any pairs, triplets, etc. not needed.
5. Isolate all sets of degrees of freedom in the ANOVA for which the partitioning is not understood.
6. Defer the partitioning of sets of degrees of freedom that are not completely understood.
7. Approach the partitioning in step 6 from different directions in order to reduce steps 5 and 6 to the null set. Note that partitioning may be impossible until more information becomes available.

In using the algorithm always approach the key-out of degrees of freedom from the direction which is simplest and easiest to understand. Keep picking away at the remainder degrees of freedom until one reaches the desired stage, which could of course be single degree of freedom contrasts for the total degrees of freedom. When one knows the total number of observations N, one knows the total degrees of freedom which is N . Then one can always partition these N degrees of freedom into one for the correction for the mean and N-1 for the remainder. Then, if there are r blocks, one can always partition the N-1 into a set of r-1 and N-r degrees of freedom. This procedure is continued until step 7 in the algorithm is reached.

Investigators and statisticians often start computing sums of squares prior to using algorithm I. This practice can result in misspent effort and hence Rule VI.

Rule VI: Do NO computing of sums of squares until the correctness of the degree of freedom key-out in the ANOVA has been ascertained and the appropriate error variances have been designated.

Before computing any sums of squares, it is well to recognize the difficulty encountered in keying-out degrees of freedom in certain types of experiments. It is wise to consider the following two rules whenever human or animal experiments are involved.

Rule VII: With almost probability one, experiments and surveys involving humans and animals will have effects completely or partially confounded and one will need to follow Rules I through V in order to ascertain this.

Rule VIII: Be prepared to spend considerable time and effort unravelling the confounding schemes in any human or animal experiment as planned by the researcher (and perhaps even by a statistician).

When one is satisfied with the key-out of degrees of freedom for an investigation, then and only then should one consider computing totals, solutions for effects, and sums of squares. In connection with the last item algorithm II has been found useful.

Algorithm II: Computing sums of squares in the ANOVA

1. At every step compute the simplest ANOVA sums of squares, that is, sums of squares assuming nesting even though there was no nesting.
2. Compute sums of squares for degree of freedom key-outs in steps 2, 3, and 4 of algorithm I. For many investigations, this is a desk calculator job.
3. For partially confounded effects, it may be necessary to solve a set of normal equations prior to computing the sums of squares. (An exception is given in example 5.2.)
4. If steps 5 and 6 of algorithm I have not been reduced to the null set, nothing should be done about further partitioning of the sums of squares.

All too often computing specialists become imbued with a program or package for high speed computing and do not pay sufficient attention to simplifications. Example 5.2 is a case in point. The method of computing described indicates exactly what is being done whereas the use of a high speed nonorthogonal n-way classification program or a multiple regression program would not indicate the nature of quantities being computed. Likewise, rounding errors from high speed computer programs have always plagued this author, with complete nonsense resulting in several cases.

Once one computes the appropriate sums of squares in the ANOVA, then appropriate error variances need to be determined. Algorithm III is presented in this light.

Algorithm III: Determining appropriate error variances for F-tests

1. Factors with the same type of experimental unit may have the same error variance.

2. Factors with different experimental units almost always have different error variances.

3. In order to check the validity of an error variance, determine the appropriate error variance assuming other effects are absent from the experiment for single factors, for pairs of factors, etc.

4. Check to determine if partially confounded effects may be estimated from two sources and with two different error variances as in example 5.2.

5. Check your decisions with known situations.

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