

Randomization Analysis of Experimental Designs under Non Standard Conditions

by

David Dry Morris

Dissertation submitted to the Faculty of the
Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy
in
Statistics

APPROVED:

Klaus Hinkelmann, Chairman

Marvin M. Lentner

Raymond H. Myers

Marion R. Reynolds, Jr.

Eric P. Smith

March, 1987

Blacksburg, Virginia

18/11/87 7:57

Randomization Analysis of Experimental Designs under Non Standard Conditions

by

David Dry Morris

Klaus Hinkelmann, Chairman

Statistics

(ABSTRACT)

Often the basic assumptions of the ANOVA for an experimental design are not met or the statistical model is incorrectly specified. Randomization of treatments to experimental units is expected to protect against such shortcomings. This paper uses randomization theory to examine the impact on the expectations of mean squares, treatment means, and treatment differences for two model mis-specifications: Systematic response shifts and correlated experimental units.

Systematic response shifts are presented in the context of the randomized complete block design (RCBD). In particular fixed shifts are added to the responses of experimental units in the initial and final positions of each block. The fixed shifts are called border shifts. It is shown that the RCBD is an unbiased design under randomization theory when border shifts are present. Treatment means are biased but treatment differences are unbiased. However the estimate of error is biased upwards and the power of the F test is reduced.

Alternative designs to the RCBD under border shifts are the Latin square, semi-Latin square, and two-column designs. Randomization analysis demonstrates that the Latin square is an unbiased design with an unbiased estimate of error and of treatment differences. The semi-Latin square has each of the t treatments occurring only once per row and column, but t is a multiple of the number of rows or columns. Thus each row-column combination contains more than one experimental unit. The semi-Latin square is a biased design with a biased estimate of error even when no border shifts are present. Row-column interaction is responsible for the bias. Border shifts do not contaminate the expected mean squares or treatment differences, and thus the semi-Latin square is a viable alternative when the border shift overwhelms the row-column interaction. The two columns of the two-column design correspond to the border and interior experimental units respectively. Results similar to that for the semi-Latin square are obtained. Simulation studies for

the RCBD and its alternatives indicate that the power of the F test is reduced for the RCBD when border shifts are present. When no row-column interaction is present, the semi-Latin square and two-column designs provide good alternatives to the RCBD.

Similar results are found for the split plot design when border shifts occur in the sub plots. A main effects plan is presented for situations when the number of whole plot units equals the number of sub plot units per whole plot.

The analysis of designs in which the experimental units occur in a sequence and exhibit correlation is considered next. The Williams Type II(a) design is examined in conjunction with the usual ANOVA and with the method of first differencing. Expected mean squares, treatment means, and treatment differences are obtained under randomization theory for each analysis. When only adjacent experimental units have non negligible correlation, the Type II(a) design provides an unbiased error estimate for the usual ANOVA. However the expectation of the treatment mean square is biased downwards for a positive correlation. First differencing results in a biased test and a biased error estimate. The test is approximately unbiased if the correlation between units is close to a half.

Acknowledgements

I would like to thank Dr. Hinkelmann for his encouragement, excellent suggestions, and timely help. The generous time and thought given by Drs. Marvin Lentner, Ray Myers, Marion Reynolds, Jr., and Eric Smith are also appreciated. I also thank Dr. I. J. Good for listening and helping with the designs for correlated data. Among those who cannot witness the fruition of their faith are my parents, _____ who made this long journey immeasurably easier. I also acknowledge the counsel and encouragement given by Dr. Kenneth Koonce and the late Dr. Prentiss Schilling. Last but not least, thanks to _____ for tactfully ignoring the crumpled paper balls and eraser crumbs.

This research was largely funded by the Merck Predoctoral Fellowship, a gift from Merck & Co., Rahway, New Jersey. It was not only the gift of money, but also the gift of the freedom to work without interruption, that earned my gratitude.

Table of Contents

I. Introduction	1
1.1 A Proper Context	1
1.2 Research Scope	2
1.2.1 Examples	2
1.2.2 Current Approaches	4
1.2.3 Another Perspective	5
1.3 Summary	6
II. Background Material	7
2.1 ANOVA and Design	7
2.1.1 Assumptions	8
2.1.2 Experimental Design	8
2.1.3 Violation of ANOVA Assumptions	9
2.1.3.1 Non-normality	10
2.1.3.2 Gross Errors	10
2.1.3.3 Error Violations	11
2.1.3.4 Non-additivity	11

2.1.4	Randomization	12
2.1.4.1	The Debates	12
2.1.4.2	A Difficulty	13
2.1.5	Design Criteria	13
2.1.6	Summary	14
2.2	Outlier Literature	15
2.2.1	Background	15
2.2.2	Deletion Diagnostics	17
2.2.3	Alternative to Residuals	18
2.2.4	Robust Designs	19
2.2.5	Border Shifts	19
2.3	Introduction to Randomization Theory	20
III.	Border Shifts	26
3.1	Effect on the RCBD	26
3.2	The Split-plot Design	32
3.2.1	Without border shifts	32
3.2.2	Effect of border shift on the split plot design	35
3.2.3	Summary	39
3.3	Alternatives to the RCBD	41
3.3.1	Latin square design	41
3.3.2	Semi-Latin square	46
3.3.3	Two-column Design	55
3.3.4	Summary of RCBD Alternatives	61
3.4	Split Plot Alternatives	63
3.5	Simulation Studies	68
3.5.1	Normal Theory	69
3.5.2	Randomization Theory	77

3.6 Chapter Summary	78
IV. Correlated Experimental Units	82
4.1 History	83
4.2 Competing Models	84
4.2.1 Papadakis	85
4.2.2 Least Squares Smoothing	88
4.2.3 First Differencing	90
4.3 Designs for Correlated Observations	92
4.4 Randomization Analysis	93
V. Results for Correlated Experimental Units	100
5.1 Type II(a) Design with the Usual Analysis	101
5.1.1 Five Treatments	101
5.1.2 Seven Treatments	110
5.1.3 Summary	114
5.2 First Differencing	114
5.2.1 Five Treatments	115
5.2.2 Seven Treatments	123
5.2.3 Summary	125
5.3 The Addition of Border Shifts	125
5.3.1 Randomized Complete Block Design	126
5.3.2 Type II(a) Designs	129
5.3.3 First Differencing	134
5.4 Chapter Summary	139
VI. Summary and Future Research	140
6.1 Summary of Border Shifts	141

6.2 Summary of Correlated Observations	143
6.3 Further Research	143
Bibliography	145
A. Derivations from Chapter Three	154
A.1 RCBD with Border Shifts	154
A.1.1 Expectation of squared shift term	154
A.1.2 Cross-product with residual terms	155
A.2 Derivation of Covariance of Two Treatment Totals	156
A.2.1 Expanded Expression of the Covariance	156
A.2.2 Terms (A.2.2) and (A.2.3)	157
A.2.3 Terms A.2.4 and A.2.5	158
A.2.4 Term A.2.6	158
A.2.5 Remaining terms	159
A.3 Subplot Expectations for Split Plot with Border Shifts	160
A.3.1 Expectation of subplot treatment sum of squares	160
A.3.1.1 Term A.3.3	162
A.3.1.2 Term A.3.4	162
A.3.2 Expectation of whole plot by subplot interaction sum of squares	163
A.3.2.1 Term A.3.11	164
A.3.2.2 Term A.3.12	165
A.3.2.3 Term A.3.13	165
A.3.2.4 Term A.3.14	166
A.3.3 Expectation of the total sum of squares	167
A.3.3.1 Terms A.3.19 and A.3.20	168
A.3.3.2 Term A.3.21	168
A.3.3.3 Term A.3.22	169

A.4	The Latin square with Border Effects	170
A.4.1	Expectation of Treatment Sum of Squares	170
A.4.1.1	Terms A.4.2 and A.4.3	171
A.4.1.2	Term A.4.4	171
A.4.2	Expectation of Total Sum of Squares	172
A.4.3	Variance of a Treatment Total	173
A.4.4	Covariance of Two Treatment Totals	174
A.5	Semi-Latin Square with Shift	174
A.5.1	Expectation of Treatment Sum of Squares	174
A.5.2	Expectation of the Total Sum of Squares	175
A.6	Two Column Design without Border Shifts	177
A.6.1	Expectation for Treatment Sum of Squares	177
A.6.2	Terms A.6.2 and A.6.3	178
A.6.3	Expectation of Total Sum of Squares	179
A.7	Two Column Design with Border Shift	180
A.7.1	Expectation of Treatment Sum of Squares	180
A.7.2	Expectation of Total Sum of Squares	181
A.8	Split Plot with Subplots in a Latin Square	182
A.8.1	Expectation of Subplot Treatment Sum of Squares	183
A.8.2	Expectation of Replication by Subplot Treatment Sum of Squares	184
B.	SIMULATION PROGRAMS	186
B.1	Simulation to Compare Power and MSE when $T = 4$	186
B.2	Simulation to Compare Power and MSE when $T = 6$	192
B.3	Simulation to Obtain the Empirical 95th Percentile when $T = 6$	202
B.4	Simulation to Compare Power and MSE when $T = 8$	213
B.5	Simulation to Obtain the Empirical 95th Percentile when $T = 8$	225

List of Illustrations

Figure 1. Semi-Latin square with $a = 2$ and $b = 3$	47
Figure 2. Semi-Latin square with $a = 2$, $b = 3$, and altered for border shift.	51
Figure 3. Two-column design with $b = 3$ and $t = 6$	54
Figure 4. Split plot design with sub plots arranged in a Latin square.	64

List of Tables

Table 1. ANOVA for Usual RCBD	24
Table 2. ANOVA for RCBD with Border Shifts	30
Table 3. ANOVA for Usual Split Plot	36
Table 4. ANOVA for Split Plot with Border Shifts	40
Table 5. ANOVA for Latin Square with Border Shifts	45
Table 6. ANOVA for the Semi-Latin Square	49
Table 7. ANOVA for the Semi-Latin Square with Border Shifts	53
Table 8. ANOVA for the Two-Column Design	59
Table 9. ANOVA for the Two-Column Design with Border Shifts	62
Table 10. ANOVA for Split Plot with Subplots in a Latin Square	67
Table 11. Simulation Results on Power and MSE for Four Treatments, Case I	71
Table 12. Simulation Results on Power and MSE for Four Treatments, Case II	72
Table 13. Simulation Results on Power and MSE for Six Treatments, Case I	73
Table 14. Simulation Results on Power and MSE for Six Treatments, Case II	74
Table 15. Simulation Results on Power and MSE for Eight Treatments, Case I	75
Table 16. Simulation Results on Power and MSE for Eight Treatments, Case II	76
Table 17. Empirical 95th Percentile of the Randomization F for $T = 6$	79
Table 18. Empirical 95th Percentile of the Randomization F for $T = 8$	80
Table 19. ANOVA for RCBD with Correlated Errors	99
Table 20. ANOVA for Type II(a), $T = 5$, with Correlated Errors	111
Table 21. ANOVA for Type II(a), $T = 7$, Correlated Errors	113

Table 22. ANOVA for Type II(a), T = 5, with First Differencing	122
Table 23. ANOVA for Type II(a), T = 7, with First Differencing	124
Table 24. ANOVA for RCBD with Correlation and Border Shifts	130
Table 25. ANOVA for Type II(a) with Correlation and Border Shifts	135
Table 26. ANOVA for Type II(a), T = 5, First Differencing, Both Violations	137
Table 27. ANOVA for Type II(a), T = 7, First Differencing, Both Violations	138

Chapter I

I. Introduction

1.1 A Proper Context

Walker Percy's (1975) apt summary of science is that science "cannot utter a single word about an individual molecule, thing, or creature in so far as it is an individual but only so far as it is like other individuals." Although this statement follows a discussion of the scientific method, the idea of the tension between how things are like and unlike leads to the study of statistics. In fact, many statisticians regard the practice of statistics and the procedure of the scientific method as closely knit ideas.

Box (1976) draws several parallels between these two areas. His main points of similitude are iteration between theory and practice, flexibility, parsimony, selective worry, and the role of mathematics in science. Speaking of the role of mathematics in science, Box says of the statistician that with "assumptions, known to be false, he can often derive results which match, to a useful approximation, those found in the real world." Our task is to find parsimonious models that closely approximate the data and yet not fear to discard or alter the models as additional data dictate;

hence, we arrive at the need for flexibility and iteration in experimentation. We shall attempt to follow these guidelines as we examine the performance of some relatively standard procedures when confronted with non-standard yet realistic conditions.

1.2 Research Scope

The statistical tool of interest is the analysis of variance procedure which, as we shall see later in Section 2.1.2, directly leads to experimental design. In one sense the concern here is with under-specification of the statistical model for the analysis of variance. The presence of anomalous data values, or outliers, in a region of a design may well be the result of an under-specified model; i.e., a significant factor has been left out of the model. The assumption that correlated experimental units are uncorrelated does not involve excluded factors, but it does mean that the correlation matrix of the variables is under-specified. The purpose of this research is to examine the above problems from a randomization viewpoint.

1.2.1 Examples

Two non-standard experimental conditions are thus available for inquiry, but are they realistic conditions? Do researchers actually encounter systematic outliers and correlated experimental units? Examples of research conditions leading to each under-specification come easily to mind. First consider the case of systematic outliers.

Suppose we are testing the efficacy of a new drug on cattle, and that the cattle are kept in r barns, each of which has a single row of t pens. A complete set of t treatments is independently assigned at random to the pens in each barn. If there is some concern that pens on each end of the barns may cause a shift in response, we have an environmental factor that should be included in

the statistical model. This experiment is a randomized complete block design (RCBD) with a shift in the borders. Note that the shift is not influenced by the treatment applied to the pen.

Inter-cropping experiments present an example in which the effect is not due to environmental factors but due to a positional factor. Suppose that we have an RCBD with r blocks and t treatments, treatments being various fertilizers. If a dual crop of corn on the hills and a legume in the furrows is planted, a problem arises at the end of each block. The difficulty is that either corn or the legume will end the block because the usual alternation scheme is truncated. This could result in response shifts at the borders of the design.

Our other problem, the failure of experimental units to be uncorrelated, has long been a concern of agronomic researchers using the analysis of variance procedure. Agricultural field experiments are typically arranged in sets of contiguous plots. The dependence of yields on soil fertility trends combined with the close proximity of plots makes some correlation of responses nearly inescapable.

The problem of correlated experimental units is certainly not restricted to agricultural research. Consider for example a biologist who is doing research on catfish. The scientist puts twenty fish into a holding tank and withdraws one fish every two days until ten fish are removed. One of five treatments is applied to the sampled fish with the restriction that each treatment is applied to two different fish. Thus the entire experiment, consisting of five treatments with two replications each, takes approximately twenty days to conduct. It seems reasonable to assume that the extended stay in a small holding tank will result in correlations among the sampled fish.

The preceding examples illustrate that the theoretical problems of systematic shifts and correlations do exist in the actual practice of statistics. As a matter of fact, some strategies for dealing with the problems already exist; however, current techniques certainly do not cover all situations. We will also see that some of the new procedures have not been evaluated from a randomization framework.

1.2.2 Current Approaches

Strategies for dealing with border shifts tend to avoid the borders or to use blocking schemes. In the inter-cropping study, buffer rows or plots could be planted and then not harvested. Any border problem would then be confined to the buffer areas. The cattle experiment presents a bit more difficulty, but some type of incomplete block design could be implemented which would not use the end pens. Two treatments could be dropped although that is unlikely to be a viable option. An alternative for either case would be a Latin square design in which the border shift would be included in a column effect.

Unfortunately all of these schemes could be relatively expensive in terms of experimental units (E.U.'s) or blocks. The incomplete block strategy for the cattle example could easily require more barns than are available. When land is scarce the buffer plots might require too much area. A Latin square requires t^2 E.U.'s which might result in excessive costs. The blocking scheme seems reasonable, but are there blocking schemes in two directions that require fewer E.U.'s than the Latin square?

As for correlation, the situation can be approached in several ways. The attitude used by many is to ignore the correlation and hope that randomization will provide adequate protection. Alternatively, analysis of covariance based on control plots or a variable measured on each experimental unit is often used when trends are present. The currently developing methods of nearest neighbours are becoming popular analyses when trends are present. Another parameterization of the problem involves generalized least squares, necessitating the estimation of the correlations. Other than blindly trust randomization, the researcher is left to choose between nearest neighbour methods which depend heavily on specific models and generalized least squares which involves the estimation of many parameters. The lack of adequate randomization comparisons among these techniques makes the choice even harder for the scientist.

1.2.3 Another Perspective

Assuming there is existing knowledge that correlation or border shifts are likely to occur, the prudent researcher would want to have a design that best copes with the likely problem. A difference in design implies a difference in randomization, and thus our emphasis is on the relative performance of alternative randomization schemes when either border shifts or correlation are present.

In particular there will be an investigation of the effects of border shifts on the randomized complete block and split plot designs. Are the estimates of treatment means biased, and if so, by how much? Are the designs biased in Fisher's sense? Once we ascertain the extent of damage, we will turn to alternative blocking strategies. A class of designs that block in two directions but use fewer E.U.'s than the Latin square will be examined for design bias, mean square error bias, and bias in estimates of treatment means and differences. Comparisons will be made to the designs blocked in only one direction. Simulation studies will be used for a final comparison of the new designs with the original RCBD.

Similarly for the correlated case, the effect on the RCBD will first be investigated, concentrating on design bias as well as bias in the estimation of error and treatment estimates. The only alternative randomization to be examined is the Williams Type II(a) design. However it will be examined in conjunction with the usual analysis of variance and with the method of first differencing. Comparisons between the two analyses and with the original RCBD will be made on the basis of design bias and bias in estimation.

Randomization theory will be used to evaluate the necessary expectations and to provide a valid basis for comparisons. Although a normal theory framework is often presented as the definitive approach, there are several reasons why a randomization framework is preferred here. Probably the most important reason is that many consider the randomization model to be more parsimonious than the normal model. An intuitive reason is that it is natural to compare different randomization schemes by the yardstick of randomization theory. Finally, no one has looked at either border shifts or the alternatives touted for use with correlated data from a randomization

perspective. Kempton (1984a) lists the development of a randomization theory in nearest neighbour studies as an area needing further work.

1.3 Summary

We may now define our goals in terms of the preceding developments. Our concern is with two types of assumptions for the analysis of variance and these types are systematic response shifts in the borders of a design and correlation between adjacent experimental units. First, what are the effects of these violations on selected design analyses? Second, how may we detect and/or correct for these violations? In terms of the interplay of the scientific method, detection serves to point up the faults of a previous design while randomization techniques can aid the design of future experiments. We are concerned primarily with the ability of restricted randomizations, with blocking defined as a type of restricted randomization, to protect our analyses from the effects of correlated E.U.'s and systematic outliers.

Chapter II provides the background material necessary for the development of these topics. A review of the concepts of the ANOVA and experimental design will be followed by a review of the outlier literature and randomization theory. Chapter III contains the investigation of the effects of border shifts on the split plot and RCBD, followed by some alternative designs. The literature on nearest neighbours is reviewed in Chapter IV, and Chapter V presents the results pertaining to correlated data. Due to their tedious nature, many derivations and all simulation programs have been relegated to the appendices.

Chapter II

II. Background Material

2.1 ANOVA and Design

The analysis of variance is generally described in introductory texts as a device used to compare three or more population means, and this is the context in which we will examine it. Suppose that we have a group of $n = rt$ objects (experimental units or E.U.'s) and t treatments, and that we assign each treatment to r E.U.'s completely at random. An average response among all E.U.'s and an average response for each treatment group of E.U.'s are calculated. The ANOVA procedure compares the variation of E.U.'s about their treatment mean to the variation of the treatment means about the overall mean. The intra-group variability is usually called "within", or error, variance and the inter-group variability called the "between", or treatment, variability. When population means differ substantially, we expect the between variability to exceed the within variability.

Development of the ANOVA is generally attributed to R. A. Fisher (1918) who is probably most responsible for popularizing it. However Scheffe' (1956) cites Airy (1861) and Chauvenet

(1863) as having developed similar divisions of total variability into distinct components. The first instance of a complete analysis of variance table seems to be due to Fisher and MacKensie (1923), although the data were analyzed using an incorrect model. Tippett (1931) added the familiar column for expected mean squares and the modern ANOVA table was essentially complete. Since this time a number of competing models, parameterizations, analyses, and philosophies have arisen with the customary arguments and misunderstandings. Reviews of these debates may be found in Eisenhart (1947), Kempthorne (1955), Scheffe' (1956), Plackett (1960), and elsewhere.

2.1.1 Assumptions

Within the normal theory framework, the comparison of variability estimates (mean squares) is done with the F-ratio formed by taking (mean square treatment) / (mean square error). Simplifying assumptions generally made for this test are additivity between treatments and E.U.'s and independent random observations following normal distributions with common variance. The treatment-unit additivity assumption means that all E.U.'s respond to a particular treatment application in the same way, while the distributional assumptions provide a straight-forward analysis. As stated earlier, these assumptions are probably false in almost all cases although the procedure has great utility. Questions regarding detection and correction of serious departures from the assumptions will concern us.

2.1.2 Experimental Design

Constant interplay between experimentation and analysis of the resulting data brings us to the relationship between the ANOVA and experimental design. Asking who begat whom in this case is similar to being trapped in a chicken-and-the-egg argument. Even though the design does dictate the type of analysis, a thorough analysis may also suggest a future design. The design principles

of blocking, replication, and randomization are responses to commonly encountered problems in experimental research. Blocking was developed in order to remove systematic environmental variability that is unrelated to the treatments but often exists among the E.U.'s. When forming complete blocks, sets of t homogeneous E.U.'s are formed and all t treatments are assigned at random in each set set of E.U.'s. Since randomization is done separately in each block, blocking can be viewed as a special type of restricted randomization. The randomized complete block design (RCBD) blocks in one direction, the Latin square blocks in two directions, and the Graeco-Latin square blocks in three directions. While blocking is used to remove environmental variability, replication provides proper estimates of variability and increased replication provides more precise parameter estimates. Randomization is an integral, sometimes controversial, tenet of modern design. Some think of randomization as insurance against unsuspected factors in the experiment while others view it as a basis for the validity of the ANOVA test. Since this work will be deeply involved with randomization, a full discussion will be postponed until Section 2.1.4.

Thus the ANOVA with its assumptions and experimental design with its framework of blocking, replication, and randomization fit nicely into the framework of the scientific method. Hypotheses are formed, experiments conducted by the principles of experimental design in conjunction with present knowledge, the data analyzed using the ANOVA, and a new cycle begun on the basis of these findings. At each step in this iterative process we should examine the accuracy of our assumptions and make adjustments in the design and/or analysis as needed. Our emphasis will be on this process and the ability of special randomization schemes to aid us in planning experiments.

2.1.3 Violation of ANOVA Assumptions

Before looking at specific cases, a review of the effect of common assumption violations and the usual remedies is in order. In the spirit of Box's selective worry, Cochran (1947) states his main worries about the ANOVA assumptions:

In general, the factors that are liable to cause the most severe disturbances are extreme skewness, the presence of gross errors, anomalous behavior of certain treatments or parts of the experiment, marked departures from the additive relationship, and changes in the error variance, either related to the mean or to certain treatments or parts of the experiment.

We will follow Cochran's lead in discussing the most common violations, namely non-normality, gross errors, heterogeneity of variance, correlation, and non-additivity.

2.1.3.1 Non-normality

Non-normality, says Cochran, results in "no serious error ... in the significance levels of the F-test or of the two-tailed t-tests." Unfortunately there is some loss of power and there is a tendency to find too many significant results. The concern with skewness arises from difficulties with one-sided t-tests since the distribution is not symmetric. There are non-parametric analogues to some analysis of variance procedures, but these are also dependent on symmetric distributions. An option which makes no assumptions as to symmetry is the randomization test as developed by Kempthorne (1952). We will look at this test in detail in Section 2.3.

2.1.3.2 Gross Errors

The effects of gross errors, outliers in modern terminology, are relatively easy to guess in advance. Treatments are poorly estimated and the standard errors over-estimated. Except for obviously wrong data values which almost anyone would discard, questions of concern are: When is a data point an outlier and what to do about it if it is. Another interesting question is how to design such that the impact of a single errant point or a group of points in a specific region can be minimized. A thorough examination of these questions will be left until Section 2.2.

2.1.3.3 Error Violations

Heterogeneity of errors and correlation of errors will cause a loss in efficiency and a substantial bias in the standard errors. The standard suggestions seem to be a partitioning of the error estimate for the former case and some type of transformation in the latter case. Some writers suggest that randomization will control the correlation, and we will examine that advice in Chapter V.

2.1.3.4 Non-additivity

Non-additivity can be of two types. Cochran's concern is whether environmental effects (blocks) and treatment effects are additive; i.e., do treatment differences remain constant over different levels of an environmental factor? He finds that non-additivity in this sense tends to produce heterogeneity of the error variance and recommends transformation to some scale where effects are additive. Another type of additivity concerns treatment effects and individual experimental units. Suppose we apply treatment A to all the E.U.'s. If the treatment causes the same response on all E.U.'s, we have unit-treatment additivity. This has been examined in detail by Wilk and Kempthorne (1957), Addelman (1970), and others. My feelings on the subject were summed up by White (1975) who says,

... it does not seem very practical to avoid the (E.U./treatment) additivity assumption, because making general inferences from an experiment ultimately requires the assumption, if only for the purpose of extending its results to other E.U.'s.

Thus most of the assumptions seem to be dealt with as being inevitable or examined after the experiment is done. Pre-experiment concerns in this area seem to concentrate on blocking or faith in our next topic, randomization.

2.1.4 Randomization

Early in his work on the analysis of variance and experimental design, Fisher began to stress the random assignment of treatments to experimental units. A sectional heading in Fisher (1935), “Randomisation; the Physical Basis of the Validity of the Test”, illustrates Fisher’s view on the importance of randomization. Within seventeen years, a randomization theory was codified into the convenient notation developed by Kempthorne (1952). This theory differed from the normal distribution theory in several important respects. First of all, plot values were considered to be fixed quantities rather than random variables, and secondly the source of any distributional properties in a design were determined entirely by the randomization scheme used in the conduct of the experiment. These and other differences freed the ANOVA from many of the restrictive assumptions discussed previously and opened the path to much fruitful research. We shall develop this theory in detail in Section 2.3.

2.1.4.1 *The Debates*

Random assignment of treatments to plots was a novel idea at the time and sparked a debate which continues today. An immediate argument broke out with the proponents of systematic designs, including a rebuttal to Fisher by Gosset (1938). Yates (1939) provides a nice summary of both the advantages and disadvantages of a systematic design. According to Yates, the chief advantages are that it gives more accurate results and that it is easier to execute. The disadvantages are that there is no assurance that the error estimate is unbiased, there are many ways to estimate error, all treatment comparisons don’t have the same error, and biased treatment means are possible.

Pearson (1937) points out that Fisher’s randomization test with means can be less powerful than using midpoints when the data come from a rectangular distribution. This illustrates the lack of a unique test statistic in a randomization framework. Another difficulty pointed out by Folks

(1984) arises from defining the reference set of repetitions from which a design is drawn; like the test statistic, the reference set is not unique. Harville (1975) stresses a Bayesian objection that the designer should rely on past experience rather than the vagaries of randomization. However no one can have complete knowledge of and control over all the possible influences on an experiment, and randomization would seem to lessen the chances of a ruined experiment due to these unknown effects. As Folks (1984) summarizes,

Still deep within me, I have the feeling that the interpretation is clearer, the conclusions are stronger and the analysis has greater validity if treatments have actually been assigned at random.

2.1.4.2 A Difficulty

Despite the on-going debate, randomization is stressed in most experimental design courses, texts, and consultations. Recent advances in computer speed have prompted some statisticians to forsake normal theory ANOVA and to use only randomization tests (see Edgington (1980)). These tests compute the usual F-ratio for each of the possible assignments of treatments to the E.U.'s and calculate the exact probability of obtaining a larger F-ratio under the randomization procedure. Aside from ultimate constraints on the size of experiments that can be analyzed in this manner, there is an unfortunate tendency to assume that any randomization procedure/test statistic combination is acceptable. It is rather risky to do randomization tests indiscriminately without examining expectations over the complete randomization set. As a matter of fact, this will be one of our major concerns as we examine the efficacy of certain restricted randomizations in combatting systematic outliers and correlated experimental units.

2.1.5 Design Criteria

As we suggest alternative randomization schemes and hence designs, there is the question of how to compare them. There are a multitude of design criteria from which to choose, including

various optimality measures and unbiasedness. As far as optimality criteria are concerned, we will be dealing with orthogonal designs which generally are optimal under many of the criteria. Thus we will restrict our attention to the question of unbiasedness. Preece, Bailey, and Patterson (1978) list three criteria for the adequacy of a randomization scheme. They are

Weak criterion attributed to Fisher (1925) and Yates (1933): The expectation of mean square error and the expectation of mean square treatment are the same when there are no treatment differences.

Strong criterion attributed to Grundy and Healy (1950): The mean square based on any set of treatment comparisons must have the same expectation as the error mean square when there are no treatment differences.

Generalized Grundy and Healy criterion attributed to Nelder (1965a 1965b): All normalized response contrasts within each block have the same variance.

We will be concerned only with the weak criterion. If a design meets this criterion, it will be called unbiased.

Obviously a single criterion cannot handle all possible comparisons. For instance, a design may be unbiased even though the estimate of experimental error is biased. We may also want to compare biased designs with each other. Therefore designs will be compared on the basis of the amount of the bias in the estimate of experimental error as well as on the basis of unbiasedness in Fisher's sense.

2.1.6 Summary

At this juncture we have reviewed the assumptions of the ANOVA and have examined the relationship between the ANOVA and experimental design. The purpose of design is to reduce

experimental error in the ANOVA and to insure that valid inferences may be made to the appropriate population. Randomization is seen as an integral part of design as it provides a physical basis for the validity of the inferences. A quick survey of some design criteria has provided a basis for comparing alternative designs. It is now time to examine in detail the previous work done in the area of outliers.

2.2 Outlier Literature

2.2.1 Background

Although the notion of outliers has been traced by some as far back as Bernoulli, there is not much consensus on exactly when one has an outlier much less what to do about it. Beckman and Cook (1983) provide an excellent discussion which will be summarized in part here. They classify outliers as either discordant or contaminant depending respectively on whether the observation is merely surprising or discrepant to the investigator, or whether the observation is not a realization from the target population. A contaminant observation might not be noticed by the investigator hence not be discordant. Conversely a discordant observation might be from the target population and not be contaminant. It is certainly possible that an observation could be both.

In a more mechanistic formulation, an aberrant data point can be modelled in one of two ways. The response can be construed as a result of the true model plus some fixed shift, or as a result of the true model plus an extra source of variability. The former approach is useful when a shift is due to recording errors or to an unmodelled environmental factor. Heavy-tailed distributions, measurement errors, and mixtures of distributions are more amenable to the latter formulation. A choice is ultimately dependent on the underlying process generating the outliers.

Perhaps the origins of outliers are legion, but they can be placed into three broad categories. An unusual observation might be due to the natural variability of the data. Heavy-tailed distributions are prone to yielding extreme values when sampled. Being unaware of the nature of the underlying distribution, the researcher can wrongly classify correct values from the tails as outliers. Another broad category, labelled local model weakness, concentrates on factors that might affect the one suspect point. Factors such as recording errors are not an indication the model is wrong, but reflect non-systematic perturbations at single data locations. The last category is called global model weakness and is the source of the response shift we will investigate. Outliers are caused in this instance by a basic flaw in the statistical model; i.e., pertinent factors have been left out of the model and should be included. If we see this type of outlier in an experiment, we can plan for it in future experiments.

Research into outliers has concentrated on identification and accommodation. The identification approach stresses identifying the outlier as a step either to rejection of the point, revision of the model, or further experimentation. Accommodation attempts to reduce the possible impact of an outlier through model modification and alternate methods of analysis. Even though accommodation tends to require more information about the underlying process, greater immunity to outliers is oftentimes worth the effort. On the other hand, a thorough analysis of outliers can yield important insights into the adequacy of a tentative model.

Regardless of the approach, outliers will eventually appear in every researcher's data and necessitate some response. Suggested responses are deletion of the point, never delete a point, and assigning weights to the data points. Deletion requires a decision rule of some sort and these rules are the source of much conflict. Keeping the point does not require any reflection, and may sometimes be as foolish as always deleting the point. Weighting schemes fall into four camps: L estimation, M estimation, Bayesian techniques, and hybrid techniques. In L estimation the order statistics are weighted whereas M estimation bases weights on the residuals. A possible hybrid approach useful in a design setting would be to delete the point and then estimate it using the appropriate linear model.

2.2.2 Deletion Diagnostics

The idea of deletion and recomputation leads to the broad area of deletion diagnostics. It is a natural approach in the analysis of variance and appears quite early. For instance, Cochran (1947) suggests deleting suspect observations and recomputing the sum of squares for error. The difference in estimates was used to develop a test for deleting the point. Although methods such as this could be used on single points, the burden of examining every data point in every data set was staggering even with computers. It took the Sherman-Morrison-Woodbury (S-M-W) theorem in Rao (1973) to permit the wholesale examination of observations by deletion diagnostics.

The S-M-W theorem presents a method for generating all the statistics for an analysis without the observation by using only the quantities already generated during the analysis with the observation. Freeing deletion procedures from severe computational constraints permitted the creation of a multitude of diagnostics. These diagnostic statistics re-examine residuals, mean square error, estimates of coefficients, predicted points, prediction variances, test statistics, and so on. The amount of new information can overwhelm and possibly mislead.

Problems arise when this cornucopia of information is used by the unwary in wholesale hypothesis testing. If we have n observations, and perform n tests at $\alpha = .05$ for a significant change in the estimate of mean square error, our overall error rate is quite high. The actual rate is unclear since the tests are not independent, but we can expect some spuriously significant results to appear. An option is to use a significance level of $\frac{\alpha}{n}$, but then we would rarely find significant differences. Perhaps a more reasonable approach is to adopt a less rigorous interpretation of the new statistics and look upon them as only diagnostic aids, not a series of hypothesis tests.

It seems obvious that the fertile soil of deletion diagnostics is well tilled. Although the work has been done in a regression setting, the extension to discrete designs seems straight-forward. The S-M-W theorem is stated in terms of a matrix of full rank whereas the ANOVA is typically expressed in a matrix form of less than full rank. However the ANOVA analysis can be easily reparameterized into a full rank form and the deletion diagnostics computed.

2.2.3 Alternative to Residuals

Noting that residuals are not reliable indicators of outliers, Bradu and Hawkins (1982) have taken another tack in their approach. They assume $Y_{ij} \sim N(\mu_{ij}, \sigma^2)$ where $\mu_{ij} = \mu + \alpha_i + \beta_j + \delta_{ij}$ and δ_{ij} is the deviation from the usual two-way additive model $\mu + \alpha_i + \beta_j$. The key to this method is a proper tetrad

$$T_{ij,eg} = Y_{ij} - Y_{ej} - Y_{ig} + Y_{eg}, \quad i \neq e, j \neq g.$$

Note that the usual residual can be obtained by averaging all tetrads, proper and improper, to get $Y_{ij} - Y_{.j} - Y_{.i} + Y_{..}$. The diagnostic procedure may be described as a three step process.

1. Calculate $Q_2(i,j)$, the median of all proper tetrads of cell (i,j) .
2. Rank cells by decreasing order of $|Q_2(i,j)|$.
3. Apply confirmatory diagnostics such as half normal plots.

Deviations from a straight line on the half normal plots indicate possible outlying cells.

The $Q_2(i,j)$ statistics are highly robust with a breakdown point of fifty per cent. This means that if at least half of the tetrads do not include an outlier, then $Q_2(i,j)$ will give a reasonable estimate of δ_{ij} . Bradu and Hawkins also note that a "total separation", meaning the median tetrads of all non-outlier cells are smaller in absolute value than the median tetrads of all outlier cells, is achievable for sufficiently large outliers. Unfortunately, their method is oriented toward two-way tables and does not seem to be easily extended to multi-way tables.

2.2.4 Robust Designs

An alternative to worrying about outliers after the experiment is completed is to worry about outliers while planning the experiment. Some work has been done to construct designs relatively immune to outliers. The approach of Box and Draper (1975) is to look at designs which minimize the impact of outliers on the predicted points obtained by least squares. They conclude that the diagonal elements of $X(X'X)^{-1}X'$ should be as uniform as possible, a condition met by the orthogonal designs we will consider.

Herzberg and Andrews (1976) are more concerned with designs that are relatively insensitive to removal of the outlying data points. This is in contrast to Box and Draper's implicit assumption that all points are to be retained. Designs are compared on the probability of a breakdown, the inability of the design to estimate all the unknown parameters of the chosen model when points are removed.

Draper and Herzberg (1979) look at minimizing the bias resulting from the presence of outliers. The minimum integrated mean square error is used as the criterion by which to gauge a design's robustness to outliers. Since this is done in a response surface setting, they derive designs which are robust to model bias and the presence of outliers.

2.2.5 Border Shifts

All three of the robust design approaches mentioned are primarily concerned with the occasional stray value and hence local model weakness. We wish to examine a type of global model weakness evidenced by systematic shifts in response in certain parts of a design. These shifts need not be due to environmental factors such as fertility differences although they can be. In Cochran's terms, they are "anomalous behavior of ... parts of the experiment" which we wish to include in the statistical model. The immediate thought is that blocking schemes should cure the problem, and

they will if we have enough experimental material. Are there alternatives to the usual blocking schemes when the E.U.'s are scarce, expensive, or both? We will examine this question in Chapter III.

2.3 Introduction to Randomization Theory

There remains the choice of which analytic framework to use in posing the theoretical questions. We could use either a normal theory or a randomization theory approach. Since our concerns are with using alternative randomization schemes to offset border effects and correlated E.U.'s, the more reasonable course is randomization theory. This theory is more closely tied to the actual conduct of the experiment and requires fewer model assumptions. Despite the controversies mentioned earlier, many still regard the normal theory tests as mere approximations to the appropriate randomization tests. The notation and development of the randomization test will be introduced by using the RCBD as an example.

As formalized by Kempthorne (1952), randomization theory derives the probability structure of a design from the physical act of randomly assigning treatments to experimental units. If we consider all possible realizations of the randomization scheme for a particular RCBD, we have a finite population of conceptual outcomes. For an RCBD with t treatments and r blocks, the number of equally likely arrangements of treatments on experimental units is $(t!)^r$. Kempthorne provides the mathematical notation necessary to take expectations over the finite population of treatment assignments.

As an introduction to this notation, consider an RCBD with t treatments and r blocks. First note that the replacement of a subscript with a dot indicates the average over that subscript; i.e.,

$Y_{ij.} = t^{-1} \sum_{k=1}^t Y_{ijk}$. A conceptual response for an RCBD is written as

$$Y_{ijk} = Y_{...} + (Y_{i..} - Y_{...}) + (Y_{ijk} - Y_{ij.}) + \\ (Y_{ijk} - Y_{ij.} - Y_{..k} + Y_{...}) + (Y_{ij.} - Y_{i..})$$

where

Y_{ijk} is the conceptual yield when treatment k ($= 1, \dots, t$) is applied to plot j ($= 1, \dots, t$) in block i ($= 1, \dots, r$),

$Y_{...}$ is the average over all conceptual yields,

$Y_{i..} - Y_{...}$ is the difference between a block average and the overall average,

$Y_{ijk} - Y_{ij.}$ is the difference between a conceptual response of plot (i, j) to treatment k and the average conceptual response of plot (i, j) over all treatments,

$Y_{ijk} - Y_{ij.} - Y_{..k} + Y_{...}$ is assumed to be zero, implying plot-treatment additivity, and

$Y_{ij.} - Y_{i..}$ is the difference between the average conceptual yield at plot (i, j) and the average for that block.

Denoting $\mu = Y_{...}$, $b_i = Y_{i..} - Y_{...}$, $t_k = Y_{..k} - Y_{...}$, and $e_{ij} = Y_{ij.} - Y_{i..}$ and applying the additivity assumption, we have

$$Y_{ijk} = \mu + b_i + t_k + e_{ij}.$$

The Y_{ijk} are conceptual yields in that they represent the hypothetical response of plot (i, j) to treatment k should that assignment be made. Design random variables are the mechanism by which the fixed conceptual yields are connected to the observed yields, denoted by y_{ik} . For our purposes consider the design random variable δ_{ij}^k which is unity when treatment k is applied to plot (i, j) and zero otherwise. The probability that $\delta_{ij}^k = 1$ or 0 is determined by the randomization

scheme of the design. For an RCBD, $P\{\delta_{ij}^t = 1\} = t^{-1}$ and $P\{\delta_{ij}^t = 0\} = 1 - t^{-1}$ for all i, j , and k . Other useful probabilities are

$$P\{\delta_{ij}^t = 1 \text{ and } \delta_{ij}^{k'} = 1\} = 0, \quad (k \neq k')$$

$$P\{\delta_{ij}^t = 1 \text{ and } \delta_{i'j'}^t = 1\} = 0, \quad (j \neq j')$$

$$P\{\delta_{ij}^t = 1 \text{ and } \delta_{i'j'}^t = 1\} = t^{-2} \quad (i \neq i').$$

These indicate respectively that two treatments cannot be on the same plot, a treatment occurs only once per block, and treatments are randomized independently in each block. The relationship of conceptual yield to observed yield is

$$y_{ik} = \sum_{j=1}^t \delta_{ij}^k Y_{ijk} = \mu + b_i + t_k + \sum_{j=1}^t \delta_{ij}^k e_{ij}. \quad (2.1)$$

Once estimates are written in terms of the y_{ik} , expectations may be taken. Note that randomness enters via the design random variables only.

Using (2.1), we observe that a treatment sum $y_{.k} = T_k$ is $r\mu + rt_k + \sum_{i=1}^r \sum_{j=1}^t \delta_{ij}^k e_{ij}$. The expectation of T_k is thus

$$E(T_k) = r\mu + rt_k$$

since $E(\delta_{ij}^k) = t^{-1}$ for all i, j, k , and $\sum_{j=1}^t e_{ij} = 0$ for all i . Writing the variance of T_k as $E[(T_k - E(T_k))^2]$ we obtain $VAR(T_k) = E[(\sum_{i=1}^r \sum_{j=1}^t \delta_{ij}^k e_{ij})^2]$ which Kempthorne shows to be $t^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2$. Similarly it is shown that $COV(T_k, T_{k'}) = -[t(t-1)]^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2$.

The division of the total sum of squares into portions due to blocks, treatments, and error uses the identity

$$\sum_{i,j}^{r,t} (y_{ik} - y_{..})^2 = t \sum_{i=1}^r (y_{i.} - y_{..})^2 + r \sum_{k=1}^t (y_{.k} - y_{..})^2 + \sum_{i,j}^{r,t} (y_{ik} - y_{i.} - y_{.k} + y_{..})^2. \quad (2.2)$$

Noting that $\sum_{k=1}^t \delta_{ij}^k = 1$ and $\sum_{k=1}^t t_k = 0$, we have $y_{i.} = \mu + b_i$ and $y_{..} = \mu$ since $\sum_{i=1}^r b_i = 0$. Substitution into the expression for block sum of squares yields

$$t \sum_{i=1}^r (y_{i.} - y_{..})^2 = t \sum_{i=1}^r b_i^2.$$

The treatment sum of squares are obtained by rewriting the second term of (2.2) as

$$r^{-1} \sum_{k=1}^t T_k^2 - rty_{..}^2.$$

This quantity has expectation $r^{-1} \sum_{i,j}^{r,t} e_{ij}^2 + r \sum_{k=1}^t t_k^2$. Similarly the total sum of squares may be written as

$$\sum_{i,k}^{r,t} y_{ik}^2 - rty_{..}^2$$

which has expectation

$$\sum_{i,j} e_{ij}^2 + r \sum_{k=1}^t t_k^2 + t \sum_{i=1}^r b_i^2.$$

The expectation for error sum of squares is obtained by subtraction and is $r^{-1}(r-1) \sum_{ij} e_{ij}^2$. Thus we have the analysis of variance (Table 1).

The analysis of variance table suggests using the ratio

$$\frac{\text{Mean Square (treatments)}}{\text{Mean Square (error)}}$$

as a test statistic. Under H_0 : $t_k = 0$ for all k , the statistic will be near one, but when the null hypothesis is false the statistic will be substantially larger than one. The test is conducted by enumerating all possible values of the test statistic under permutation of treatments to plots; setting aside the largest $\alpha(100)$ per cent of the values as a rejection region; and observing whether the realized test statistic falls in the rejection region.

Table 1. ANOVA for Usual RCBD

Source	DF	Expected Mean Square
Block	$r-1$	$t(r-1)^{-1} \sum_{i=1}^r b_i^2$
Treatment	$t-1$	$[r(t-1)]^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2 + r(t-1)^{-1} \sum_{k=1}^t t_k^2$
Error	$(r-1)(t-1)$	$[r(t-1)]^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2$

Use of the above ratio is reminiscent of the usual infinite model test when using the normal distribution. In the infinite case one would assume that the e_{ij} (constants in the finite model) follow identical and independent normal distributions with mean zero and variance σ^2 . The ratio of mean squares then follows a central F distribution under the null hypothesis. Since the finite and infinite models present similar test statistics, what is their relationship?

Proponents of randomization maintain that the test under randomization theory is more applicable since fewer assumptions are necessary and since the probability structure more closely follows the actual conduct of the experiment. Unfortunately, the computations involved in a randomization test may be considerable; for example, an RCBD with t treatments and r blocks has $(t!)^r$ possible plans for each of which a test statistic must be calculated. Thus the normal theory test is often presented as an approximation to the randomization test. It has been demonstrated by Kempthorne, et al (1961) that the approximation is quite good in reasonably large experiments. Now that we have developed the necessary concepts and notation, we are ready to look at the impact of border shifts on the randomized complete block and split plot designs.

Chapter III

III. Border Shifts

3.1 Effect on the RCBD

A border shift will be modelled as a constant shift c_{ij} where $i = 1, 2, \dots, r$ and $j = 1$ or t . That is if treatment k is applied to the end plots of any block, we have conceptual yields $Y_{ijk} = \mu + b_i + t_k + e_{ij} + c_{ij}$ leading to observed yields

$$y_{ik} = \sum_{i,j} \delta_{ij}^k Y_{ijk} = \mu + b_i + t_k + \sum_{j=1}^t \delta_{ij}^k [e_{ij} + c_{ij} I_{\{1,t\}}(j)] . \quad (3.1)$$

where $I_{\{1,t\}}(j) = 1$ if $j = 1, t$ and zero otherwise. If we wish c_{ij} to be random, the expectations derived in this section may be regarded as conditional on the set of observed c_{ij} .

We now use (3.1) in all the expressions used in the ordinary RCBD analysis. Thus a treatment total is

$$T_k = r\mu + rt_k + \sum_{i,j} \delta_{ij}^k [e_{ij} + c_{ij} I_{\{1,t\}}(j)]$$

with

$$E[T_k] = r\mu + rt_k + t^{-1} \sum_{i=1}^r [c_{i1} + c_{it}].$$

We may substitute these expressions into $E[(T_k - E(T_k))^2]$ to obtain the variance of a treatment total. Upon substituting and squaring, we have

$$VAR(T_k) = E[\sum_{i,j} \delta_{ij}^k e_{ij}]^2 + \quad (3.2a)$$

$$E[\sum_{i,j} \delta_{ij}^k c_{ij} I_{\{1,t\}}(j)]^2 + \quad (3.2b)$$

$$E[t^{-2} \sum_{i=1}^r (c_{i1} + c_{it})]^2 + \quad (3.2c)$$

$$E[2\sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i,j} \delta_{ij}^k c_{ij} I_{\{1,t\}}(j)] + \quad (3.2d)$$

$$E[2t^{-1} \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r (c_{i1} + c_{it})] + \quad (3.2e)$$

$$E[2t^{-1} \sum_{i,j} \delta_{ij}^k c_{ij} I_{\{1,t\}}(j) \sum_{i=1}^r (c_{i1} + c_{it})]. \quad (3.2f)$$

Term (3.2a) has expectation $t^{-1} \sum_{i,j} e_{ij}^2$ as shown in chapter two. Noting that $E(\delta_{ij}^k) = t^{-1}$, we see that

$$E[2t^{-1} \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r (c_{i1} + c_{it})] = 2t^{-2} \sum_{i,j} e_{ij} \sum_{i=1}^r (c_{i1} + c_{it}) = 0$$

and that

$$E[2t^{-1} \sum_{i,j} \delta_{ij}^k c_{ij} I_{\{1,t\}}(j) \sum_{i=1}^r (c_{i1} + c_{it})] = 2t^{-2} [\sum_{i=1}^r (c_{i1} + c_{it})]^2.$$

The derivations of the expectations for (3.2b , 3.2d) are in Appendix A.1. Combining these individual expectations, we find that

$$VAR(T_k) = t^{-1} \left\{ \sum_{i,j} e_{ij}^2 + \sum_{i=1}^r [(c_{i1}^2 + c_{ii}^2) - t^{-1}(c_{i1} + c_{ii})^2] + 2 \sum_{i=1}^r [c_{i1} e_{i1} + c_{ii} e_{ii}] \right\}. \quad (3.3)$$

Thus the variance is a function of experimental error, variability among the border shifts, and a cross-product of the two. Similarly we find

$$COV(T_k, T_{k'}) = - [t(t-1)]^{-1} VAR(T_k).$$

The derivation of the covariance is contained in Appendix A.2.

Comparing these results to those for the basic RCBD, we see that the shift variability (as expressed by the second term of (3.3)) and the cross-product terms have been added to the original formula. The overall change will probably be upward due to the shift variance, but the impact of the cross-product term clouds the issue somewhat. Of course the variance of a treatment difference is

$$VAR(T_k - T_{k'}) = VAR(T_k) + VAR(T_{k'}) - 2COV(T_k, T_{k'})$$

and hence is polluted with the border shift. As we have stated, the net result is probably a higher variance. The actual difference in treatment means, however, is unbiased as we see by finding $E(T_k - T_{k'}) = r(t_k - t_{k'})$. We expect that an unbiased difference plus higher variability will result in lower power for t-tests of the hypothesis that two treatment means are the same.

Turning our attention to the expectations of the sums of squares, we again use equation (2.2) to subdivide the total sum of squares. We will make use of the sums

$$Y_{.i} = t\mu + tb_i + (c_{i1} + c_{ii}) \quad (3.3a)$$

and

$$Y_{..} = rt\mu + \sum_{i=1}^r (c_{i1} + c_{ii}). \quad (3.3b)$$

The expectation of the correction factor, $CF = (rt)^{-1}Y_{..}^2$, is the value itself since δ_{ij}^k does not appear in (3.3b); thus, we have

$$E(CF) = rt\mu^2 + (rt)^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{it}) \right]^2 + 2\mu \sum_{i=1}^r (c_{i1} + c_{it}). \quad (3.4)$$

We now write the sum of squares for blocks as $t^{-1} \sum_{i=1}^r Y_i^2 - CF$ and take its expectation. Looking back at (3.3a) we see that Y_i has no random component and its expectation is merely its value, leading directly to

$$E[SS_{block}] = t \sum_{i=1}^r b_i^2 + t^{-1} \left[\sum_{i=1}^r (c_{i1}^2 + c_{it}^2) - r^{-1} \left(\sum_{i=1}^r (c_{i1} + c_{it}) \right)^2 \right] + 2 \sum_{i=1}^r b_i (c_{i1} + c_{it}). \quad (3.5)$$

Once again we have the original expectation plus variance and cross-product terms due to the border shifts.

The treatment sum of squares is relatively easy since the $VAR(T_k)$ has already been obtained. Once the expectation of the first term of $r^{-1} \sum_{k=1}^t Y_k^2 - CF$ is expanded, we find all its random components are contained in $VAR(T_k)$. We take the difference in expectations to obtain $E(SS_{trt})$. Dividing this quantity by $(t-1)$ degrees of freedom, we have the expected treatment mean square as shown in Table 2. The pattern of contamination seen previously continues for the treatment sum of squares.

We will take a roundabout route to finding the error sum of squares. Since the sums of squares are additive, we may take the expectation of the total sum of squares and find the expectation of the error sum of squares by subtracting the block and treatment expectations from the total. This is considerably less tedious than obtaining the expectation directly. Thus we have

$$\begin{aligned} E[SS_{total}] &= t \sum_{i=1}^r b_i^2 + r \sum_{k=1}^t t_k^2 + \sum_{i,j} e_{ij}^2 + \\ &\sum_{i=1}^r (c_{i1}^2 + c_{it}^2) - (rt)^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{it}) \right]^2 + \\ &2 \sum_{i=1}^r b_i (c_{i1} + c_{it}) + 2 \sum_{i=1}^r (e_{i1} c_{i1} + e_{it} c_{it}). \end{aligned}$$

Table 2. ANOVA for RCBD with Border Shifts

Source	DF	Expected Mean Square
Block	$r-1$	$t(r-1)^{-1} \sum_{i=1}^r b_i^2 + \phi_1(c_{ij})$
Treatment	$t-1$	$[r(t-1)]^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2 + \phi_2(c_{ij}) + r(t-1)^{-1} \sum_{k=1}^t t_k^2$
Error	$(r-1)(t-1)$	$[r(t-1)]^{-1} \sum_{i=1}^r \sum_{j=1}^t e_{ij}^2 + \phi_2(c_{ij})$

where

$$\phi_1(c_{ij}) = [t(r-1)]^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{it})^2 - r^{-1} \left(\sum_{i=1}^r (c_{i1} + c_{it}) \right)^2 \right] + 2(r-1)^{-1} \sum_{i=1}^r b_i (c_{i1} + c_{it})$$

$$\phi_2(c_{ij}) = [r(t-1)]^{-1} \sum_{i=1}^r [(c_{i1}^2 + c_{it}^2) - t^{-1} (c_{i1} + c_{it})^2] + 2[r(t-1)]^{-1} \sum_{i=1}^r (e_{i1} c_{i1} + e_{it} c_{it})$$

By subtracting the expected block and treatment sums of squares from the expected total sum of squares, we get the expected error sum of squares. The expected sum of squares is divided by the appropriate degrees of freedom to obtain the expected error mean square in Table 2.

The usual estimator for experimental error is the mean square error, and it is biased by the shift variance and the cross-product term. This bias is probably upward in general, a conclusion to be bolstered by simulation results in Section 3.5. On the positive side we observe that when the null hypothesis is true, the error and treatment mean squares are identical. This means that the RCBD is unbiased in Fisher's sense even when border shifts are present. Of course we must bear in mind that the increased size of the error mean square may reduce the power of the F-test. This will also be borne out by the simulation study.

It is worthwhile to examine some special cases of this general result. A reasonable, simplifying assumption is that $c_{ij} = c$ for all i and j ; i.e., the shift is the same for both ends of every block. In this situation the expectation for the block mean square reduces to $t \sum_{i=1}^t b_i^2$ which is identical to the block expectation when no border effects are present. This simplification leads to

$$E[SS_{trt}] = [r(t-1)]^{-1} \sum_{i,j} e_{ij}^2 + \varphi(c) + r(t-1)^{-1} \sum_{k=1}^t t_k^2$$

and

$$E[SS_{error}] = [r(t-1)]^{-1} \sum_{i,j} e_{ij}^2 + \varphi(c)$$

where

$$\varphi(c) = 2c^2(t-2)[t(t-1)]^{-1} + 2c[r(t-1)]^{-1} \sum_{i=1}^t (e_{i1} + e_{it}) .$$

Another assumption, that the shifts c_{ij} are independent random variables with mean zero and common variance σ_c^2 , results in the loss of the cross products. Then we have

$$E(MS_{trt}) = [r(t-1)]^{-1} \sum_{i,j} e_{ij}^2 + 2\sigma_c^2(t-2)[t(t-1)]^{-1} + r(t-1)^{-1} \sum_{k=1}^t t_k^2$$

and

$$E(MS_{error}) = [r(t - 1)]^{-1} \sum_{i,j} e_{ij}^2 + 2\sigma_c^2(t - 2) [t(t - 1)]^{-1}.$$

Under the assumption that the shifts are independent random variables as described above, it is obvious that the estimate of experimental error will be biased upwards.

Comparing equation (3.3) to the expected mean square for error, we see that

$$VAR(T_k) = rt(t - 1)^{-1} E(MS_{error}).$$

Our work indicates that this variance is biased upwards in general. This is especially evident under the further assumptions that the c_{ij} are independent and identically distributed with mean zero and variance σ_c^2 . The increased variability will cause a loss in power of the t-test to detect treatment differences. Although the RCBD does preserve its unbiasedness in the F-ratio, it suffers a loss in power and a generally upward bias in the estimation of experimental error. Both of these conclusions will be supported by simulation studies in Section 3.5.

3.2 *The Split-plot Design*

3.2.1 Without border shifts

Another popular design which we wish to examine is the split plot design. Frequently used for factorial experiments, the split plot gets its name from the splitting or subdividing of experimental units, called whole plots, into smaller plots (the split or sub plots). One or more factors will be assigned at random to the whole plots, and after splitting the whole plots, the remaining factors are assigned at random to the subplots. A typical agronomic example might be a lime and

manganese study on a variety of soybeans. Since lime is difficult to apply, each rate of lime is applied to separate large fields at random. Each field is then subdivided into s plots, where s is the number of manganese rates to be applied, to which the various rates of manganese are assigned at random. Each large field thus acts as an experimental unit and as a block for the manganese levels.

In the above example, the use of a split plot seems to arise from experimental convenience and not statistical considerations. This is often the case, but Steel and Torrie (1980) list other reasons for its use. Our example corresponds to the situation in which the factor lime requires a larger amount of experimental material per experimental unit than does the subplot factor. Additionally, the design may be used whenever another factor needs to be added to the experiment. An important case from a design viewpoint is that many times comparisons among one factor's levels need to be made with greater precision than another factor's.

The splitting of the E.U.'s, regardless of the reason, is accompanied by a randomization scheme different from that of the RCBD. Two independent randomizations are done in the split plot experiment: One at the whole plot level and one for subplots within each whole plot. The randomization analysis reflects this double randomization and provides us with separate error terms for the whole plot and subplot F-tests. From an intuitive standpoint, we expect the whole plot variability to be larger than the subplot variability since experimental material usually grows less homogeneous as its size increases.

Kempthorne (1952) extends his treatment of the RCBD to the split plot experiment. For the split plot we suppose the plots have yields x_{iuv} with i ($= 1, \dots, r$) representing replications, u ($i = 1, \dots, t$) whole plots, and v ($= 1, \dots, s$) subplots. The conceptual yield of plot (iuv) under treatment combination (jk) is denoted as $x_{iuvjk} = x_{iuv} + t_{jk}$ where $j = 1, \dots, t$ and $k = 1, \dots, s$. The conceptual yields may alternatively be decomposed as

$$x_{iuvjk} = x_{\dots} + (x_{i\dots} - x_{\dots}) + (x_{\dots jk} - x_{\dots}) + (x_{i,jk} - x_{i\dots} - x_{\dots jk} + x_{\dots}) + (x_{iuvjk} - x_{i,jk})$$

which reduces to

$$x_{iuvjk} = (x_{...} + t_{..}) + (x_{i..} - x_{...}) + (t_{jk} - t_{..}) + (x_{iuv} - x_{i..}) \quad (3.6a)$$

since by unit-treatment additivity

$$x_{i,jk} - x_{i....} - x_{...jk} + x_{.....} = 0 .$$

Now note that

$$(x_{iuv} - x_{i..}) = (x_{iu.} - x_{i..}) + (x_{iuv} - x_{iu.})$$

and that

$$t_{jk} = t_{..} + (t_{j.} - t_{..}) + (t_{.k} - t_{..}) + (t_{jk} - t_{j.} - t_{.k} + t_{..}) .$$

Substitute the two above expressions into (3.6a) to obtain

$$\begin{aligned} x_{iuvjk} = & (x_{...} + t_{..}) + (x_{i..} - x_{...}) + (t_{j.} - t_{..}) + (x_{iu.} - x_{i..}) + \\ & (t_{.k} - t_{..}) + (t_{jk} - t_{j.} - t_{.k} + t_{..}) + (x_{iuv} - x_{iu.}) . \end{aligned}$$

A suitable relabelling of terms leads to an observed yield

$$y_{ijk} = \mu + r_i + t_j + \eta_{ij} + s_k + (ts)_{jk} + e_{ijk} \quad (3.6b)$$

where

$\mu = x_{...} + t_{..}$ is the overall mean;

$r_i = x_{i..} - x_{...}$ is the difference between a replicate average and the overall average;

$t_j = t_{j.} - t_{..}$ is the difference between a whole plot treatment mean and the overall treatment mean;

$\eta_{ij} = \sum_{u=1}^t \delta_{iu}^j (x_{iu.} - x_{i..})$ where δ_{iu}^j is unity when treatment j is applied to whole plot unit u in replicate i and zero otherwise;

$s_k = t_{.k} - t_{..}$ is the difference between a subplot treatment mean and the overall treatment mean;

$(ts)_{jk} = t_{.jk} - t_{.j.} - t_{.k.} + t_{..}$ represents the interaction of whole and subplot treatments; and

$e_{ijk} = \sum_{u,v} \delta_{iuv}^{jk} (x_{iuv} - x_{i..})$ where δ_{iuv}^{jk} is unity if treatment combination (jk) is applied to plot (uv) in replicate i and zero otherwise.

Points worth mentioning are that treatment-plot additivity is assumed for this model and that there are two random variables corresponding to the independent randomizations of whole and subplot treatments.

The ANOVA table (see Table 3) for the split plot is obtained in a manner similar to that illustrated in Section 2.3 for the RCBD. We see from the table that the whole plot treatments require a different error term, sometimes called error (a), from the subplot treatment and interaction terms. A similar result can be obtained using normal theory which indicates that error (a) should be larger than residual error. Although one would expect this in general, the whole plot error term is occasionally smaller than the subplot error in practice.

3.2.2 Effect of border shift on the split plot design

As with the RCBD, we wish to examine the impact of a border shift on the usual statistical analysis. We will examine the basic split plot experiment with t whole plots and s subplots per whole plot. The model of the observed yields as shown in (3.6b) will be augmented by a border shift term, and the expectations with respect to the randomization calculated.

Table 3. ANOVA for Usual Split Plot

Source	DF	Expected Mean Square
Replication	$r-1$	$st(r-1)^{-1} \sum_{i=1}^r r_i^2$
Treatment W	$t-1$	$\sigma_w^2 + rs(t-1)^{-1} \sum_{j=1}^t t_j^2$
Rep * W	$(r-1)(t-1)$	σ_w^2
Treatment S	$s-1$	$\sigma_s^2 + rt(s-1)^{-1} \sum_{k=1}^s s_k^2$
W * S	$(t-1)(s-1)$	$\sigma_s^2 + r[(s-1)(t-1)]^{-1} \sum_{i,k} (ts)_{ik}^2$
Residual	$(r-1)t(s-1)$	σ_e^2

where

$$\sigma_w^2 = s[r(t-1)]^{-1} \sum_{i,u} (x_{iu.} - x_{i..})^2$$

$$\sigma_s^2 = [rt(s-1)]^{-1} \sum_{i,u,v} (x_{iuv} - x_{i..})^2$$

Suppose that a shift, denoted by c_{iv} , occurs in the first and last subplots of each whole plot. The observed yield may then be written as

$$y_{ijk} = \mu + r_i + t_j + \eta_{ij} + s_k + (ts)_{jk} + e_{ijk} + \gamma_{ijk}$$

where all the terms except γ_{ijk} are as defined for (3.6b) and $\gamma_{ijk} = \sum_{u,v} \delta_{uv}^k c_{iv} I_{(1,t)}(v)$. The γ_{ijk} adds shift c_{i1} to any observed response in the first subplot of each whole plot in replicate i and shift c_{is} to any on the last subplot.

In order to calculate the appropriate sums of squares for the analysis, we need the following totals:

$$Y_{...} = rst\mu + t \sum_{i=1}^r (c_{i1} + c_{is}),$$

$$Y_{i..} = st\mu + str_i + t(c_{i1} + c_{is}),$$

$$Y_{.j.} = rs\mu + rst_j + \sum_{i=1}^r \eta_{ij} + \sum_{l,k} e_{ljk} + \sum_{i=1}^r (c_{i1} + c_{is}),$$

$$Y_{ij.} = s\mu + sr_i + st_j + s\eta_{ij} + \sum_{k=1}^s e_{ijk} + (c_{i1} + c_{is}),$$

$$Y_{..k} = rt\mu + rts_k + \sum_{l,j} e_{ljk} + \sum_{l,j} \gamma_{ljk}, \quad \text{and}$$

$$Y_{.jk} = r\mu + rt_j + \sum_{l=1}^r \eta_{lj} + rs_k + r(ts)_{jk} + \sum_{l=1}^r e_{ljk} + \sum_{l=1}^r \gamma_{ljk}.$$

The correction factor, $CF = (rst)^{-1}Y_{...}^2$, which is not a random variable has the simple expectation

$$E[CF] = rst\mu^2 + t(rs)^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{is}) \right]^2 + 2t\mu \sum_{i=1}^r (c_{i1} + c_{is}). \quad (3.7)$$

Similarly we note that

$$E[SS_{rep}] = (rs)^{-1} \sum_{l=1}^r Y_{l..}^2 - CF$$

does not involve a random variable and simplifies to

$$E[SS_{rep}] = st \sum_{l=1}^r r_l^2 + \varphi(c_{ij}) \quad (3.8)$$

where

$$\varphi(c_{ij}) = ts^{-1} \left\{ \sum_{l=1}^r (c_{l1} + c_{ls})^2 - r^{-1} \left[\sum_{l=1}^r (c_{l1} + c_{ls}) \right]^2 \right\} + 2t \sum_{l=1}^r r_l (c_{l1} + c_{ls}).$$

The pattern seen for the RCBD is continuing; i.e., a term associated with variability among the border shifts and a cross-product term are added to the unshifted expectation.

All of the remaining sums of squares contain random components and have expectations that are less easy to obtain. First consider the whole plot treatments which have

$$SS_w = (rs)^{-1} \sum_{j=1}^t Y_{.j}^2 - CF.$$

The expectation of the first term of the above expression will be that from the unshifted case plus $t(rs)^{-1} \left[\sum_{l=1}^r (c_{l1} + c_{ls}) \right]^2$ and cross-products involving $\sum_{l=1}^r (c_{l1} + c_{ls})$. Since we know that $\sum_{j=1}^t t_j = \sum_{l=1}^r E(\eta_{lj}) = \sum_{l,k} E(e_{ljk}) = 0$, the only cross-product term will be $2t \mu \sum_{l=1}^r (c_{l1} + c_{ls})$. Combining expectations and subtracting the correction factor leaves us with

$$E(SS_w) = rs \sum_{j=1}^t t_j^2 + sr^{-1} \sum_{l,\mu} (x_{l\mu} - x_{l..})^2.$$

This is the same expectation as when no shift is present so we conclude that a shift in the subplots does not affect the whole plot mean square, which is not surprising.

The expectation for the sum of squares of the error (a) term is

$$E[SS_a] = E[s^{-1} \sum_{i,j} Y_{ij}^2 - (st)^{-1} \sum_{i=1}^r Y_{i..}^2 - (rs)^{-1} \sum_{j=1}^t Y_{.j.}^2 + CF].$$

Noting that the last three terms have already been obtained, we need only find the expectation of $s^{-1} \sum_{i,j} Y_{ij}^2$. The same logic used for the whole plot sum of squares is used to obtain

$$E[s^{-1} \sum_{i,j} Y_{ij}^2] = rts \mu^2 + ts \sum_{i=1}^r r_i^2 + rs \sum_{j=1}^t t_j^2 + s \sum_{i,u} (x_{iu.} - x_{i..})^2 + ts^{-1} (c_{i1} + c_{is})^2.$$

Taking the appropriate linear combination of expectations, we get

$$E(SS_a) = s(r-1)r^{-1} \sum_{i,u} (x_{iu.} - x_{i..})^2.$$

Thus the usual F-ratio for testing whole plot treatments is unbiased, and both mean squares are individually unbiased by the border shifts.

Derivations of expectations for the subplot sums of squares become quite tedious since the random variable γ_{ijk} is now included in the totals used in the sums of squares. Hence the derivations are relegated to Appendix A.3 and the completed expectations are displayed in Table 4. We see that the subplot F-ratios are unbiased and thus the design as a whole is unbiased. However, the individual mean squares are biased by the variability among the shifts and the appropriate cross-product terms. It is reasonable to suspect that although the tests are unbiased, there can be a considerable loss of power for sizeable shifts. The estimate of experimental error experiences considerable bias under a large shift, and this estimate is often important to the researcher.

3.2.3 Summary

Generalizing from our examination of the RCBD and split plot experiment, we see that a shift variability term is added to most sums of squares, the exception being the whole plot term of a split plot. The shift variability term will bias the variability estimates upward, especially for experimental

Table 4. ANOVA for Split Plot with Border Shifts

Source	DF	Expected Mean Square
Replication	r-1	$st(r-1)^{-1} \sum_{i=1}^t r_i^2 + \phi_1(c_{iv})$
Treatment W	t-1	$\sigma_w^2 + rs(t-1)^{-1} \sum_{j=1}^t t_j^2$
Rep * W	(r-1)(t-1)	σ_w^2
Treatment S	s-1	$\sigma_s^2 + \phi_2(c_{iv}) + rt(s-1)^{-1} \sum_{k=1}^s s_k^2$
W * S	(t-1)(s-1)	$\sigma_s^2 + \phi_2(c_{iv}) + r[(s-1)(t-1)]^{-1} \sum_{i,k} (ts)_{ik}^2$
Residual	(r-1)t(s-1)	$\sigma_s^2 + \phi_2(c_{iv})$

where

$$\sigma_w^2 = s[r(t-1)]^{-1} \sum_{i,u} (x_{iu} - x_{i..})^2$$

$$\sigma_s^2 = [rt(s-1)]^{-1} \sum_{i,u,v} (x_{iuv} - x_{iu.})^2$$

$$\phi_1(c_{iv}) = t[s(r-1)]^{-1} \left\{ \sum_{i=1}^t (c_{i1} + c_{is})^2 - r^{-1} \left(\sum_{i=1}^t (c_{i1} + c_{is}) \right)^2 \right\} + st(r-1)^{-1} \sum_{i=1}^t r_i (c_{i1} + c_{is})$$

$$\phi_2(c_{iv}) = [r(s-1)]^{-1} \sum_{i=1}^t [(c_{i1}^2 + c_{is}^2) - s^{-1} (c_{i1} + c_{is})^2] + 2[rt(s-1)]^{-1} \sum_{i,u} (e_{iu1} c_{i1} + e_{iut} c_{is})$$

error, and probably cause a loss in power. An additional cross-product term is also added, but its influence is not certain. Fortunately the usual F-ratios are still unbiased although their power may be reduced. The effect on alpha levels of the randomization test will be checked by simulation for the RCBD in Section 3.5. Thus if a good estimate of experimental error and increased power are important to a researcher, alternative designs are required when border shifts are anticipated.

3.3 Alternatives to the RCBD

The first alternative to come to mind when there are systematic environmental effects orthogonal to blocks is the Latin square. We will demonstrate that the Latin square design copes best with border shifts when sufficient experimental material is available. Two other bi-directional blocking designs that require fewer experimental units than the Latin square will also be explored. These will be shown to be useful intermediate designs when the RCBD is not appropriate and the Latin square is not feasible.

3.3.1 Latin square design

The Latin square is the classic response to systematic environmental effects in two directions. If we have t treatments, a Latin square has t rows and t columns. Treatments are randomized such that no treatment occurs twice in the same row or column. Kempthorne (1952) fully develops the randomization analysis of the Latin square, and Wilk and Kempthorne (1952) extend the analysis to non-additivity of treatments and experimental units. Our concern is naturally the impact of border shifts on the analysis.

Following Kempthorne while adding the border shifts, we see that a response apart from the shift may be expressed as

$$y_{ijk} = x_{ij} + \tau_k$$

where x_{ij} represents the conceptual yield of plot (ij) independent of treatments and τ_k is the effect of treatment k. These terms may be further decomposed such that we have conceptual responses

$$y_{ijk} = \mu + \rho_i + \gamma_j + t_k + e_{ij} + c_j I_{\{1,t\}}(j)$$

where $i,j,k = 1,2, \dots, t$ and

$\mu = x_{..} + \tau_{.}$ is the overall mean;

$\rho_i = x_{i.} - x_{..}$ is the difference between a row average and the overall average;

$\gamma_j = x_{.j} - x_{..}$ is the difference between a column average and the overall average;

$t_k = \tau_k - \tau_{.}$ is the difference between a treatment average and the overall treatment average;

$e_{ij} = x_{ij} - x_{i.} - x_{.j} + x_{..}$ is the residual error under the assumption of no row-column interaction; and

c_j is the border shift in columns 1 and t since $I_{\{1,t\}}(j)$ is unity only for $j = 1,t$.

Since c_j is confounded with columns, we expect to find that the column sum of squares will remove the border shift from the treatment and residual sums of squares.

Response totals for rows, columns, and treatments will be needed to compute the sums of squares. The observed totals are

$$Y_{...} = t^2 \mu + t(c_{t1} + c_{tt}),$$

$$Y_{i..} = t \mu + t \rho_i + (c_{t1} + c_{tt}),$$

$$Y_{.j} = t\mu + t\gamma_j + tc_j I_{\{1,t\}}(j), \quad \text{and}$$

$$Y_{..k} = t\mu + tt_k + (c_{i1} + c_{it}) + \sum_{i,j} \delta_{ij}^k e_{ij}.$$

The term δ_{ij}^k is one if the random choice of a Latin square results in treatment k being applied to plot (ij) and is zero otherwise. Before finding the expectations for the sums of squares, we note that the expectation of a treatment mean is $\mu + t_k + t^{-1}(c_{i1} + c_{it})$. Thus the treatment means are biased, but any difference between two treatment means will be unbiased.

Since $Y_{..}$, $Y_{i..}$, and $Y_{.j}$ do not contain any δ_{ij}^k , the row and column sums of squares have no random component. Hence we just substitute the appropriate sums into the usual sums of squares formulae and simplify, obtaining

$$E[CF] = t^2\mu^2 + (c_1 + c_t)^2 + 2t\mu(c_1 + c_t), \quad (3.8)$$

$$E[SS_{row}] = t \sum_{i=1}^t \rho_i^2, \quad \text{and} \quad (3.9)$$

$$E[SS_{col}] = t \sum_{j=1}^t \gamma_j^2 + t\{(c_1^2 + c_t^2) - t^{-1}(c_1 + c_t)^2\} + 2t(\gamma_1 c_1 + \gamma_t c_t). \quad (3.10)$$

The row sum of squares is not affected by the shifts, but column sum of squares contains the ubiquitous terms for border variability and cross-products.

The treatment and total sums of squares are a bit more difficult since they contain random components whose expectations are needed. The derivations of these expectations are sketched in Appendix A.4 while we merely state that

$$E[SS_{trt}] = t \sum_{k=1}^t t_k^2 + (t-1)^{-1} \sum_{i,j} e_{ij}^2 \quad (3.11)$$

and that

$$E[SS_{\text{total}}] = t \sum_{i=1}^t \rho_i^2 + t \sum_{j=1}^t \gamma_j^2 + t \sum_{k=1}^t t_k^2 + \sum_{i,j} e_{ij}^2 + \varphi(c_j) \quad (3.12)$$

where

$$\varphi(c_j) = [t(c_1^2 + c_t^2) - (c_1 + c_t)^2] + 2t(\gamma_1 c_1 + \gamma_t c_t) .$$

Because we have an orthogonal design, the sums of squares are additive and we may obtain the error sum of squares by subtracting the sum of (3.8), (3.9), (3.10), (3.11) from (3.12). This has expectation

$$E[SS_{\text{error}}] = \frac{(t-2)}{(t-1)} \sum_{i,j} e_{ij}^2 .$$

The expected mean squares are displayed in Table 5.

Again reserving the derivations for Appendix A.4, we find that

$$VAR(Y_{..k}) = (t-1)^{-1} \sum_{i,j} e_{ij}^2 ,$$

$$COV(Y_{..k}, Y_{..k'}) = -(t-1)^{-2} \sum_{i,j} e_{ij}^2 ,$$

$$VAR(Y_{..k} - Y_{..k'}) = 2[t(t-1)^2]^{-1} \sum_{i,j} e_{ij}^2 .$$

Thus the variance of a treatment mean and the variance of the difference of two treatment means are both estimated with simple functions of the error mean square. These estimated variances are also unbiased by the border shifts.

The Latin square is now seen to be an unbiased design as was the RCBD but with the error and treatment mean squares also being unbiased. Since the error mean square is not biased, the t- and F-tests should not suffer any loss of power due to border shifts. Treatment contrasts are unbiased even though individual treatment means are biased. The Latin square seems to be the ideal design when we have border shifts that are constant across the rows of the design and when we have

Table 5. ANOVA for Latin Square with Border Shifts

Source	DF	Expected Mean Square
Row	t-1	$t(t-1)^{-1} \sum_{i=1}^t \rho_i^2$
Column	t-1	$t(t-1)^{-1} \sum_{j=1}^t \gamma_j^2 + \varphi(c_j)$
Treatment	t-1	$(t-1)^{-2} \sum_{i,j} e_{ij}^2 + t(t-1)^{-1} \sum_{k=1}^t t_k^2$
Residual	(t-1)(t-2)	$(t-1)^{-2} \sum_{i,j} e_{ij}^2$

where

$$\varphi(c_j) = t[(c_1^2 + c_t^2) - t^{-1}(c_1 + c_t)^2] + 2t(\gamma_1 c_1 + \gamma_t c_t)$$

enough experimental material. Unfortunately we often do not have the t^2 experimental units necessary for the Latin square. We now wish to examine two designs that block in a manner similar to the Latin square but require fewer experimental units. Although they will not have as many nice properties as the Latin square, they will provide a compromise between the RCBD and the Latin square.

3.3.2 Semi-Latin square

The semi-Latin square, also called the modified Latin square, is a generalization of the usual Latin square. Experimental units are arranged into b rows and b columns where each of the $t = ab$ treatments occurs once in each row and column. This means that each row-column combination consists of a experimental units. An example of the design when $a = 2$ and $b = 3$ is shown in Figure 1. Although shown by Yates (1935) to be a biased design, Preece and Freeman (1983) report its continued use. This use is understandable since the design requires fewer E.U.'s than the Latin square and still blocks in two directions.

Rojas and White (1957) analyze the semi-Latin square from a randomization viewpoint. They characterize a conceptual response by the linear model

$$y_{ijkl} = x_{ijk} + \tau_l$$

where x_{ijk} is the " contribution due to the experimental unit independent of treatment and τ_l is a contribution due to the treatment. " A further decomposition of terms leads to the linear model

$$y_{ijkl} = \mu + r_i + s_j + (rs)_{ij} + e_{ijk} + t_l \quad (3.13)$$

where

$\mu = x_{...} + \tau$, is the overall average;

		Columns					
		1		2		3	
Rows	1	A	B	C	D	E	F
	2	E	F	A	B	C	D
	3	C	D	E	F	A	B

Note: Capital letters denote treatment levels.

Figure 1. Semi-Latin square with $a = 2$ and $b = 3$.

$r_i = x_{i.} - x_{...}$ is the difference between the average for row i ($= 1, 2, \dots, b$) and the overall average;

$s_j = x_{.j} - x_{...}$ is the difference between the average for column j ($= 1, 2, \dots, b$) and the overall average;

$(rs)_{ij} = x_{ij.} - x_{i.} - x_{.j} + x_{...}$ represents the row-column interaction;

$e_{ijk} = x_{ijk} - x_{ij.}$ is the difference between the experimental unit k ($= 1, 2, \dots, a$) and the average of row-column combination (ij) ; and

$t_l = \tau_l - \tau$ is the effect of treatment l ($= 1, 2, \dots, t = ab$).

Using this formulation, they obtain the analysis of variance in Table 6. No row by column interaction mean square is computed since it is confounded with treatments.

We see from Table 6 that the design is indeed biased, and further that experimental error is biased by a function of the row-column interaction. Rojas and White demonstrate that when $\frac{\sigma_r^2}{\sigma_e^2}$ is fixed, the relative bias ($\text{BIAS} / \text{EMS}_{\text{error}}$) approaches zero as either a or b become large. If column effects are small, the bias in the design, both for the F-test and the experimental error estimate, is a severe price to pay for marginal reductions in experimental error. However the design is appealing when border shifts are larger than the incurred bias.

The design is also useful if the row-column interaction can be considered negligible. Unfortunately it is easier to cite examples where this is not true than to specify when it definitely is true. The absence of row-column interaction depends on the independence of the two blocking factors, a relationship that may sometimes be questioned beforehand. Consider an industrial experiment in which columns are the six days Monday through Saturday and rows are the three shifts per day. If there are six treatments, a semi-Latin square could be imposed; however, an assumption that shift differences remain constant over days might not be valid. Of course the Latin square would be subject to the same problem in this situation.

Table 6. ANOVA for the Semi-Latin Square (1957)

Source	DF	Expected Mean Square
Row	b-1	$ab(b-1)^{-1} \sum_{i=1}^b r_i^2$
Column	b-1	$ab(b-1)^{-1} \sum_{j=1}^b s_j^2$
Treatment	t-1	$[b(t-1)]^{-1} \sum_{ijk} e_{ijk}^2 + a[(b-1)(t-1)]^{-1} \sum_{ij} (rs)_{ij}^2 + b(t-1)^{-1} \sum_{i=1}^t t_i^2$
Residual	(t-2)(b-1)	$[b(t-2)]^{-1} \sum_{ijk} e_{ijk}^2 + a(b-2)[(t-2)(b-1)]^{-1} \sum_{ij} (rs)_{ij}^2$

Large experiments may also call the assumption of negligible interactions into question. The larger the amount of experimental material, the greater is the chance of long term trends and non-additive design regions to appear. Since the semi-Latin square uses fewer E.U.'s than the Latin square, it is possibly less prone to this problem for a given number of treatments. A final concern arises if the row and/or column factors include levels at the extremes of their possible values. Intuitively, one would expect that additivity might break down when factor levels occur near the endpoints of their feasible ranges. Although linear relationships may be reasonable approximations for moderate ranges of the factor levels, the approximations may fail over the full spectrum of values. Thus a decision as to whether the interaction may be negligible rests on the experience a researcher brings to the experiment and on the size of the experiment.

We will now examine this design when border shifts are present. A simplifying assumption is necessary to make this design work for the border shift case. We must assume that the shift is the same for both borders of the design; if the assumption is untenable, we must use a Latin square design. Thus suppose we have a common shift in the two borders and have one column of the design containing the E.U.'s in the borders, as shown in Figure 2. The basic idea of blocking is to have homogeneous E.U.'s and not particularly that the E.U.'s be adjacent. We will then have a column corresponding to the border E.U.'s. The border column will contain each treatment an equal number of times; and under the usual case when $a=2$ and $b = t/2$, each treatment occurs exactly once in the border column. Typically the remaining columns would be formed as in Figure 2 with the restriction that each treatment occurs exactly once in each row and each column.

Using the same definitions as in model (3.13), a conceptual response is

$$y_{ijkl} = \mu + r_i + s_j + (rs)_{ij} + e_{ijk} + t_l + c I_{\{1\}}(j)$$

where c is the border shift and $I_{\{1\}}(j)$ is unity if $j=1$ and is zero otherwise. The shift is arbitrarily placed in column one with no loss of generality. The observed totals needed for the analysis are

$$Y_{....} = ab^2\mu + abc,$$

$$Y_{i...} = ab\mu + ac + abr_i,$$

		Columns					
		1	2		3		1
1	A	C D		E F		B	
Rows 2	E	A B		C D		F	
3	C	E F		A B		D	

Note: Capital letters denote treatment levels.

Figure 2. Semi-Latin square with $a = 2$, $b = 3$ and altered for border shift.

$$Y_{.j..} = ab\mu + abc I_{\{1\}}(j) + abs_j, \text{ and}$$

$$Y_{...l} = b\mu + \sum_{ijk} \delta_{ijk}^l ((rs)_{ij} + e_{ijk}) + bt_l + c,$$

where δ_{ijk}^l is unity if treatment l occurs on plot (ijk) and is zero otherwise. We note that a treatment total is biased by the shift but a treatment contrast is not.

The correction factor, row, and column sums of squares contain no random variables and their expectations are found through simple algebra. Derivation of the treatment and error sums of squares are found in Appendix A.5 and the results shown in Table 7. We see in Table 7 that the border shift is confined to the column mean square. Referring back to the RCBD with a border shift, we remember that it is an unbiased design with a biased estimate of experimental error. The Latin square is unbiased in both senses whereas the semi-Latin square is a biased design with a biased estimate of error. Under what conditions will the bias incurred by the semi-Latin square be less than the border shift bias in the RCBD? When this happens, we will want to use the semi-Latin square.

We will restrict our attention to the bias in estimating experimental error. Ignoring the cross-product term in the expected mean square error for the RCBD, we find the bias in estimating σ_r^2 to be approximately $2c^2(t-2)[t(t-1)]^{-1}$. The bias shown by Rojas and White for $a=2$ experimental units per row-column combination is $(b-2)(b-1)^{-1}\sigma_r^2$. Thus we want to switch to the semi-Latin square when

$$\frac{2c^2(t-2)}{t(t-1)} \geq \frac{(b-2)\sigma_r^2}{(b-1)}.$$

Under the assumption that we have two borders and $a=2$, we also must have $b = \frac{t}{2}$ rows. Substituting into the above expression and simplifying, we obtain

$$\frac{c^2}{\sigma_r^2} \geq \frac{t(t-1)(t-4)}{2(t-2)^2} \geq \frac{t-4}{2}.$$

Table 7. ANOVA for the Semi-Latin Square Design with Border Shifts

Source	DF	Expected Mean Square
Row	b-1	$ab(b-1)^{-1} \sum_{i=1}^b r_i^2$
Column	b-1	$ab(b-1)^{-1} \sum_{j=1}^b s_j^2 + \varphi(c)$
Treatment	t-1	$[b(t-1)]^{-1} \sum_{i,j,k} e_{ijk}^2 + a[(b-1)(t-1)]^{-1} \sum_{ij} (rs)_{ij}^2 + b(t-1)^{-1} \sum_{i=1}^t t_i^2$
Residual	(t-2)(b-1)	$[b(t-2)]^{-1} \sum_{i,j,k} e_{ijk}^2 + a(b-2)[(t-2)(b-1)]^{-1} \sum_{ij} (rs)_{ij}^2$

where

$$\varphi(c) = ac^2 + 2ab(b-1)^{-1}cs_1$$

		Columns				
		1	2			1
1	A	B	C	E	F	D
Rows 2	B	A	C	D	F	E
3	C	A	B	D	E	F

Note: Capital letters denote treatment levels.

Figure 3. Two-column design with $b = 3$ and $t = 6$

As the number of treatments increases, the border shift must be quite large in relation to the row-column interaction. This is understandable as the relative impact of the shifts decreases as the size of the experiment increases.

The relative efficacy of the semi-Latin square design will be examined again in Section 3.5. At that point, we will compare the RCBD with the semi-Latin square and our next alternative, the two-column design, on the basis of mean square error and power.

3.3.3 Two-column Design

An assumption for the semi-Latin square is that the experimental units should be grouped into b columns which correspond to b levels of some environmental source of variability. Suppose however that the only source of variability orthogonal to rows is the border shift. The semi-Latin square then consists of some unnecessary columns which decrease the error degrees of freedom. If we have a situation for which the E.U.'s within a block are homogeneous except for the end plots, a possible design is what we shall call a two-column design.

The two-column design consists of b rows but only two columns. One column consists of all the end plots and must have a multiple of t E.U.'s, so that each treatment occurs equally often in the border. The remaining column contains all the remaining units. As in the Latin and semi-Latin squares, each treatment occurs only once per row. Figure 3 illustrates that for $b = t/2$ each treatment occurs once in the border column and $(b-1)$ times in the interior column. Of course we can expect problems similar to those for the semi-Latin square, but we have saved a few degrees of freedom for the error mean square.

As for the statistical model, it is quite similar to that for the semi-Latin square except for the variable numbers of observations per column. We denote the various sample sizes as:

n_{ij} is the number of observations in row i ($= 1, 2, \dots, b$) and column j ($= 1, 2$);

$$n_i = n_{i1} + n_{i2} = t ;$$

$$n_j = 2b \text{ for } j = 1 \text{ and } b(t-2) \text{ for } j = 2; \text{ and}$$

$$n_{..} = bt \text{ where } t \text{ is the number of treatments.}$$

A conceptual response is written as

$$y_{ijkl} = x_{ijk} + \tau_l$$

where x_{ijk} is the contribution of the experimental unit independent of treatments and τ_l is the treatment effect. Further decomposition leads to the model

$$y_{ijkl} = \mu + r_i + s_j + (rs)_{ij} + e_{ijk} + t_l \quad (3.14)$$

where

$\mu = x_{...} + \tau$ is the overall mean;

$r_i = x_{i..} - x_{...}$ is the difference between the i 'th row average and the overall average;

$s_j = x_{.j.} - x_{...}$ is the difference between the j 'th column average and the overall average;

$(rs)_{ij} = x_{ij.} - x_{i..} - x_{.j.} + x_{...}$ represents the interaction between rows and columns;

$e_{ijk} = x_{ijk} - x_{ij.}$ is the difference between an individual plot value and the average of the (i,j) row-column combination; and

$t_l = \tau_l - \tau$ is the effect of the treatment l ($l = 1, 2, \dots, t$).

Denoting sums by capital letters and averages by small letters, it is seen that

$$\begin{aligned}\sum_{i=1}^b r_i &= \sum_{i=1}^b (x_{i..} - x_{...}) = \sum_{i=1}^b (t^{-1}X_{i..} - (bt)^{-1}X_{...}) \\ &= t^{-1}X_{...} - t^{-1}X_{...} = 0\end{aligned}$$

and that

$$\begin{aligned}\sum_{j=1}^2 n_{.j} s_j &= \sum_{j=1}^2 n_{.j} (x_{.j.} - x_{...}) = \sum_{j=1}^2 n_{.j} x_{.j.} - x_{...} \sum_{j=1}^2 n_{.j} \\ &= (X_{.1.} + X_{.2.}) - (bt)^{-1} (bt)X_{...} = X_{...} - X_{...} = 0.\end{aligned}$$

It is obvious that $\sum_{i=1}^t t_i = \sum_{i=1}^t (\tau_i - \tau) = 0$. Looking at the interaction terms, we have

$$\begin{aligned}\sum_{i=1}^b n_{ij} (rs)_{ij} &= \sum_{i=1}^b n_{ij} (x_{ij.} - x_{i..} - x_{.j.} + x_{...}) \\ &= \sum_{i=1}^b (X_{ij.} - t^{-1}n_{ij}X_{i..} - (n_{.j})^{-1}n_{ij}X_{.j.} + (n_{..})^{-1}n_{ij}X_{...}) \\ &= X_{.j.} - t^{-1} \sum_{i=1}^b n_{ij}X_{i..} - X_{.j.} + (n_{..})^{-1}n_{.j}X_{...} \\ &= -t^{-1} \sum_{i=1}^b n_{ij}X_{i..} + (n_{..})^{-1}n_{.j}X_{...}.\end{aligned}$$

This expression is

$$-2t^{-1}X_{...} + 2t^{-1}X_{...} = 0, \quad j = 1$$

and

$$-\frac{t-2}{t}X_{...} + \frac{t-2}{t}X_{...} = 0, \quad j = 2.$$

Turning to the sum over the subscript j , we have

$$\begin{aligned}
\sum_{j=1}^2 n_{yj} (rs)_{yj} &= \sum_{j=1}^2 n_{yj} (x_{yj} - x_{i..} - x_{.j} + x_{...}) \\
&= \sum_{j=1}^2 (X_{yj} - t^{-1} n_{yj} X_{i..} - (n_{.j})^{-1} n_{yj} X_{.j} + (n_{..})^{-1} n_{yj} X_{...}) \\
&= X_{i..} - X_{i..} - \sum_{j=1}^2 (n_{.j})^{-1} n_{yj} X_{.j} + (n_{..})^{-1} n_{ij} X_{...} \\
&= - \left(\frac{n_{11}}{n_{.1}} X_{.1} + \frac{n_{12}}{n_{.2}} X_{.2} \right) + b^{-1} X_{...} \\
&= -b^{-1} X_{...} + b^{-1} X_{...} = 0 .
\end{aligned}$$

Thus the terms of the model obey the following relationships:

$$\begin{aligned}
\sum_{i=1}^b n_{iy} (rs)_{iy} &= \sum_{j=1}^2 n_{iy} (rs)_{iy} = 0 \\
\sum_{i=1}^b r_i &= 0 , \quad \sum_{j=1}^2 n_{.j} s_j = 0 , \quad \sum_{i=1}^t t_i = 0 .
\end{aligned}$$

We now have enough machinery to calculate the row and column sums of squares.

Once a suitable plan is chosen at random, the computation of sums of squares involves the calculation of several observed response totals, these being

$$Y_{...} = bt\mu ,$$

$$Y_{i..} = t\mu + tr_i ,$$

$$Y_{.j..} = n_{.j}\mu + n_{.j}s_j , \quad \text{and}$$

$$Y_{...l} = b\mu + \sum_{ijk} \delta_{ijk}^l (rs)_{ijk} + \sum_{ijk} \delta_{ijk}^l e_{ijk} + bt_l ,$$

Table 8. ANOVA for the Two-Column Design

Source	DF	Expected Mean Square
Row	b-1	$t(b-1)^{-1} \sum_{i=1}^b \rho_i^2$
Column	1	$\sum_{j=1}^2 n_{.j} \gamma_j^2$
Treatment	t-1	$[b(t-1)]^{-1} \sum_{i,j,k} e_{ijk}^2 + t[b(t-1)(t-2)]^{-1} \sum_{ij} n_{ij}(rs)_{ij}^2 + b(t-1)^{-1} \sum_{i=1}^t t_i^2$
Residual	b(t-1)-t	$(b-1)[b(b(t-1)-t)]^{-1} \sum_{i,j,k} e_{ijk}^2 + \psi(b,t) \sum_{i,j} n_{ij}(rs)_{ij}^2$

where

$$\psi(b,t) = [b(t-2) - t][b(t-2)(b(t-1) - t)]^{-1}$$

where δ'_{ijk} is unity if treatment l is on plot (ijk) and is zero otherwise. The row and column sums of squares are functions of the first three totals and hence have no random components. It is then easily determined that

$$E[SS_{row}] = t \sum_{i=1}^b r_i^2$$

and

$$E[SS_{col}] = \sum_{j=1}^2 n_{.j} s_j^2 .$$

The sums of squares corresponding to treatments and error require considerably more effort, and their derivations are found in Appendix A.6. Since the structure of the two-column design is similar to that of the semi-Latin square, we find that the ANOVA in Table 8 resembles that for the semi-Latin square.

Thus the general guidelines for using the semi-Latin square also apply to the two-column design. We may expect that the potential of a row-column interaction is greater for the two-column design due to the large amount of experimental material in the interior column. If we do truly have homogeneous E.U.'s except for the end plots, then we should be able to accomplish the same result as the semi-Latin square with fewer degrees of freedom lost to columns.

We look next at the effect of an equal shift in the end plots of a two-column design. The statistical model is

$$y_{ijkl} = \mu + r_i + s_j + (rs)_{ij} + e_{ijk} + t_l + c I_{\{1\}}(j)$$

where all terms also in (3.14) retain their original meaning, and $I_{\{1\}}(j)$ is unity when $j = 1$ and is zero otherwise. The shift c is regarded as a constant for the analysis. The response totals are now

$$Y_{\dots} = bt\mu + 2bc ,$$

$$Y_{l\dots} = t\mu + tr_l + 2c ,$$

$$Y_{.j..} = n_{.j}\mu + n_{.j}s_j + 2bc I_{\{1\}}(j) , \text{ and}$$

$$Y_{...l} = b\mu + \sum_{ljk} \delta_{ijk}^l (rs)_{ij} + \sum_{ljk} \delta_{ijk}^l e_{ijk} + bt_l + c .$$

The treatment mean now has a bias of $t^{-1}c$, but any treatment contrast will be unbiased.

Once again it is easy to show that

$$E[SS_{row}] = t \sum_{i=1}^b r_i^2$$

and

$$E[SS_{col}] = \sum_{j=1}^2 n_{.j} s_j^2 + 4bcs_1 + 2(b-1)c^2 .$$

The treatment and error sums of squares are not appreciably more difficult to obtain than in the original analysis, but the derivation is contained in Appendix A.7. The expected mean squares for the ANOVA are displayed in Table 9. We see from Table 9 that the border shift effects are restricted to the column mean square as expected. As in the case of the semi-Latin square, we want to use the design only when the border shift bias is larger than the bias incurred by the design. The similarity of expected mean squares in the two designs indicates that similar conclusions apply.

3.3.4 Summary of RCBD Alternatives

We have seen that the ideal alternative to the RCBD when border shifts are present is the Latin square. The Latin square is an unbiased design with an unbiased estimate of experimental error as long as the border shifts are constant among rows. If the Latin square is not practical and the end plots have a common shift, a compromise design is the semi-Latin square which is a biased design with a biased estimate of experimental error. The general guideline is to use the semi-Latin

Table 9. ANOVA for the Two-Column Design with Border Shifts

Source	DF	Expected Mean Square
Row	b-1	$t(b-1)^{-1} \sum_{i=1}^b \rho_i^2$
Column	1	$\sum_{j=1}^2 n_j \gamma_j^2 + \varphi(c)$
Treatment	t-1	$[b(t-1)]^{-1} \sum_{ijk} e_{ijk}^2 + t[b(t-1)(t-2)]^{-1} \sum_{ij} n_{ij} (rs)_{ij}^2 + b(t-1)^{-1} \sum_{i=1}^t t_i^2$
Residual	b(t-1)-t	$(b-1)[b(b(t-1)-t)]^{-1} \sum_{ijk} e_{ijk}^2 + \psi(b,t) \sum_{i,j} n_{ij} (rs)_{ij}^2$

where

$$\varphi(c) = 2(b-1)c^2 + 4bc\gamma_1$$

$$\psi(b,t) = [b(t-2) - t][b(t-2)(b(t-1) - t)]^{-1}$$

square when the border shift is expected to be large in relation to the row-column interaction. This seems to be more likely for small designs with few treatments. If we have few treatments and otherwise homogeneous E.U.'s within the blocks, a third alternative is the two-column design. This design is quite similar to the semi-Latin square in its characteristics, but can reserve more degrees of freedom for error. This is a nice property when dealing with small designs.

3.4 Split Plot Alternatives

In Section 3.2.2, the split plot design was examined for robustness to border shifts in the subplots. The analysis revealed that the whole plot test was unaffected by the shifts although the subplot tests were affected. The F-ratios were unbiased, but the estimate of experimental error was biased. Following the line of logic established for the RCBD, a blocking scheme among the subplots seems reasonable as a means of combatting the shift effects. The lack of treatment replication among the subplots dictate that the blocking among subplots must be done across the whole plots.

We shall consider only the case in which each replication of the split plot experiment has s whole plots and s subplots per whole plot. Each whole plot is considered to be a row, or block. The s subplots of each whole plot will constitute the individual E.U.'s in s columns which run orthogonal to the whole plots (i.e., across the whole plots). Thus we can assign subplot treatments as in a Latin square with rows being whole plots and columns being position of the subplots within each whole plot. See Figure 4 for an example.

As in the development of formulae (3.6a, 3.6b), the conceptual response of plot (iuj) is represented as

$$y_{ijkl} = x_{iuj} + \tau_{kl}$$

		Sub Plots			
		1	2	3	4
Whole Plots	1	A	B	C	D
	2	D	A	B	C
	3	C	D	A	B
	4	B	C	D	A

Note: Capital letters denote sub plot treatment levels.

Figure 4. Split plot design with sub plots arranged in a Latin square.

where x_{iuj} is the response of E. U. (iuj) apart from treatments and τ_{kl} is the treatment effect. A decomposition of x_{iuj} and τ_{kl} slightly different from 3.6a, 3.6b plus the addition of a border shift results in the linear model

$$y_{iujkl} = \mu + r_i + t_k + \eta_{ik} + b_{ij} + s_l + (ts)_{kl} + e_{iuj} + c_{ij} I_{\{1,s\}}(j)$$

where

$\mu = x_{...} + \tau_{..}$ is the overall average;

$r_i = x_{i..} - x_{...}$ is the difference between the average of replication i ($i = 1, \dots, r$) and the overall mean;

$t_k = \tau_k - \tau_{..}$ is the difference between the average of whole plot treatment k ($= 1, \dots, s$) and the overall treatment average;

$\eta_{ik} = \sum_{u=1}^s \delta_{iu}^k (x_{iu.} - x_{i..})$ where $\delta_{iu}^k = 1$ if treatment k is applied to whole plot u and is zero otherwise;

$b_{ij} = x_{i.j} - x_{i..}$ is the difference between the j 'th column average in the i 'th replication and the i 'th replication average;

$s_l = \tau_{.l} - \tau_{..}$ is the difference between the average of subplot treatment l ($= 1, \dots, s$) and the overall treatment average;

$(ts)_{kl} = \tau_{kl} - \tau_k - \tau_{.l} + \tau_{..}$ represents the interaction between whole plot and subplot treatment factors.

$e_{iuj} = x_{iuj} - x_{i.j} - x_{iu.} + x_{i..}$ represents the interaction between rows and columns for each replication; and

$c_{ij} I_{\{1,s\}}(j) = c_{i1}$ when $j = 1$, c_{is} when $j = s$, and zero otherwise.

This design is closely related to the Latin square, with the primary distinction being that the row effects are now randomly assigned when treatments are assigned to whole plots.

Analysis of this design will require use of the observed totals

$$Y_{\dots} = rs^2\mu + s \sum_{i=1}^r (c_{i1} + c_{is}) ,$$

$$Y_{i\dots} = sr\mu + s^2r_i + s(c_{i1} + c_{is}) ,$$

$$Y_{ij..} = s\mu + sr_i + sb_{ij} + sc_{ij} I_{\{1,s\}}(j) ,$$

$$Y_{..k.} = rs\mu + rst_k + s \sum_{i=1}^r \eta_{ik} ,$$

$$Y_{\dots l} = rs\mu + rss_l + \sum_{i,k,u,j} \delta_{iuj}^{kl} e_{iuj} + \sum_{i=1}^r (c_{i1} + c_{is}) ,$$

$$Y_{i.k.} = s\mu + sr_i + st_k + \eta_{ik} + (c_{i1} + c_{is}) + \sum_{i u j} \delta_{iuj}^{kl} e_{iuj} ,$$

$$Y_{..kl} = r\mu + rt_k + \sum_{i=1}^r \eta_{ik} + rs_l + r(ts)_{kl} + \sum_{i u j} \delta_{iuj}^{kl} (b_{ij} + e_{iuj} + c_{ij} I_{\{1,s\}}(j)) , \text{ and}$$

$$Y_{l..l} = s\mu + sr_i + s(s_l) + (c_{i1} + c_{is}) + \sum_{k u j} \delta_{iuj}^{kl} e_{iuj} ,$$

where δ_{iuj}^{kl} is unity if treatment combination (kl) is applied to plot (iuj) and is zero otherwise. We see that the whole plot treatments remain unbiased by the shifts, and that the subplot treatments remain biased. The derivations of the sums of squares expectations are similar to those of the usual split plot and are relegated to Appendix A.8.

The resulting expected mean squares for the design, as shown in Table 10, do not include the usual residual mean square. This is a result of the confounding of the column and treatment interaction effects. However, proper F-ratios for testing main effects may be obtained from the

Table 10. ANOVA for Split Plot with Subplots in a Latin Square

Source	DF	Expected Mean Square
Replication	$r-1$	$s^2(r-1)^{-1} \sum_{i=1}^r r_i^2 + \varphi(c_{iw})$
Treatment W	$s-1$	$\sigma_w^2 + rs(s-1)^{-1} \sum_{k=1}^s t_k^2$
Rep * W	$(r-1)(s-1)$	σ_w^2
Treatment S	$s-1$	$\sigma_s^2 + rs(s-1)^{-1} \sum_{i=1}^r s_i^2$
Rep * S	$(r-1)(s-1)$	σ_s^2

where

$$\varphi(c_{iw}) = (r-1)^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{is})^2 - r^{-1} \left(\sum_{i=1}^r (c_{i1} + c_{is}) \right)^2 \right] + 2s(r-1)^{-1} \sum_{i=1}^r r_i (c_{i1} + c_{is})$$

$$\sigma_w^2 = s[r(s-1)]^{-1} \sum_{i,u} (x_{iu.} - x_{i..})^2$$

$$\sigma_s^2 = [r(s-1)^2]^{-1} \sum_{i,u,v} (x_{iuv} - x_{iu.})^2$$

mean squares in the table. The whole plot treatments are tested as with the usual split plot analysis, but the subplot treatments use the mean square for the replicate by subplot treatment interaction as the denominator of the F -ratio. There will be some loss of degrees of freedom to offset the elimination of the border effect. A matter of some concern is that the whole plot by subplot treatment interaction is lost and must remain untested. The trouble is that the interaction is confounded with columns. If this interaction is of small concern, this design is useful as a main effects plan when border shifts are present.

What is to be done if s whole plots are not available in each replicate? Possible alternatives are the semi-Latin square and two-column designs presented earlier. Although the analyses will not be derived here, the work should be relatively straight-forward but tedious. Results similar to those for the RCBD are likely, and the design will probably be restricted to testing main effects only.

3.5 Simulation Studies

At this point some questions regarding the previous theoretical work remain. Does the normal theory test closely approximate the randomization test for the semi-Latin square and two-column designs? If the responses are generated from a normal distribution, just how large is the contribution of border shifts to estimates of the experimental error? Answers to these questions will provide insight into the choice of designs, and computer simulation is a natural approach to the problem.

The simulation study concerns itself strictly with the RCBD and its alternatives. Two main sections comprise the study with one section following a normal theory methodology and the other following randomization theory. The normal theory section looks at the impact of border shifts on the estimation of experimental error and the power of F -tests in the randomized complete block, semi-Latin square, and two-column designs. In contrast the randomization part checks to see if

the critical F-value for the normal test is approximately equal to the critical F for the randomization test.

Programs for both simulation studies are Statistical Analysis System (SAS) programs using the matrix procedure. The random number generators are those provided by SAS and the subsequent analyses of the results were conducted with SAS. For each combination of border shift and treatment effect examined, two simulation runs were made for the normal theory test; three runs were made for the randomization tests. Each simulation run consisted of 5000 randomizations of treatments to experimental units. Copies of the programs and attendant documentation are found in Appendix B.

3.5.1 Normal Theory

The data for this part of the simulation are generated from the normal distribution with mean zero and variance one. Border shifts of 0,1,2, and 3 standard deviations and block effects are added to the basic distribution. For treatment effects, consider a $t \times 1$ vector τ containing the treatment effects to be randomly assigned to units. The effects are assigned such that $\lambda = \tau' \tau = 0, 1, 4, \text{ or } 9$. Each nonzero λ is examined in four runs (two runs otherwise) of 5000 iterations each. Two runs have τ defined with the initial entry as $\sqrt{\lambda}$ and all other entries zero. The other two runs have τ defined with the first two entries as $\sqrt{\frac{\lambda}{2}}$ and all other entries zero. These two situations will be referred to as Case I and Case II respectively. For each combination of block and treatment effects considered, a replicate or run consists of 5000 iterations for which a new set of random variates is generated at each iteration. Thus we look at 5000 different data sets for each replication. The RCBD, semi-Latin square, and two-column design are randomized onto the data at each iteration, analyzed, and the results tabulated.

Due to computer time and costs, it is prohibitive to examine all 16 factorial points. Therefore we use pseudo-factors to take a one-half fraction of the factorial. Letting factor A represent shift

effects and B represent treatment effects, pseudo-factors A_1, A_2, B_1, B_2 are formed in the manner below.

A	B	A_1	A_2	B_1	B_2
0	0	0	0	0	0
0	1	0	0	0	1
0	2	0	0	1	0
0	3	0	0	1	1
1	0	0	1	0	0
1	1	0	1	0	1
1	2	0	1	1	0
1	3	0	1	1	1
2	0	1	0	0	0
2	1	1	0	0	1
2	2	1	0	1	0
2	3	1	0	1	1
3	0	1	1	0	0
3	1	1	1	0	1
3	2	1	1	1	0
3	3	1	1	1	1

A half fraction is taken by using only those treatment combinations such that $A_1 + A_2 + B_1 + B_2 = 0$, modulo 2. We now have eight combinations of border and treatment effects superimposed on data which are analyzed with three designs. Each of the eight combinations are run in replications of 5000 iterations. Considering this as one experiment done with t treatments, we repeat the experiment for $t = 4, 6$, and 8. The experiments will be analyzed separately for each value of t . A minor discrepancy is that the semi-Latin and two-column designs are equivalent for $t = 4$, and as a result, only two designs are analyzed for that experiment.

Table 11. Simulation Results on Power and MSE for Four Treatments, Case I

Shift	Treatment	Power		MSE	
	Effect	RCBD	2-COL	RCBD	2-COL
0	0	5.330	5.310	1.004	1.010
0	3	36.180	24.010	1.000	1.004
1	1	7.400	7.470	1.347	1.003
1	2	15.740	14.020	1.334	1.011
2	1	7.250	7.740	2.324	0.985
2	2	11.130	13.140	2.341	1.004
3	0	7.990	5.090	3.952	1.008
3	3	13.540	24.510	3.951	0.995

Notes:

2-COL is the two-column design.

$\tau = (\sqrt{\lambda}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Table 12. Simulation Results on Power and MSE for Four Treatments, Case II

Shift	Treatment	Power		MSE	
	Effect	RCBD	2-COL	RCBD	2-COL
0	3	25.450	18.050	1.005	1.002
1	1	7.120	6.460	1.329	1.004
1	2	11.960	10.510	1.337	1.000
2	1	6.780	6.320	2.348	1.018
2	2	9.700	10.860	2.329	1.008
3	3	12.290	18.190	3.970	0.994

Notes:

2-COL is the two-column design.

$\tau = (\sqrt{\frac{\lambda}{2}}, \sqrt{\frac{\lambda}{2}}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Table 13. Simulation Results on Power and MSE for Six Treatments, Case I

Shift	Treatment		Power			MSE	
	Effect	RCBD	SLS	2-COL	RCBD	SLS	2-COL
0	0	5.160	5.220	5.190	1.008	1.006	1.003
0	3	81.140	76.220	79.370	0.992	0.992	0.993
1	1	11.460	12.180	12.070	1.267	0.996	1.008
1	2	33.720	38.290	40.840	1.270	1.000	1.003
2	1	8.560	11.830	12.410	2.063	0.994	0.996
2	2	21.360	39.590	41.050	2.049	0.997	1.003
3	0	5.320	4.850	5.360	3.405	0.998	1.001
3	3	27.090	75.860	79.720	3.386	1.004	1.004

Notes:

2-COL is the two-column design.

$\tau = (\sqrt{\lambda}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Table 14. Simulation Results on Power and MSE for Six Treatments, Case II

Shift	Treatment		Power			MSE	
	Effect	RCBD	SLS	2-COL	RCBD	SLS	2-COL
0	3	71.100	65.560	68.420	0.999	1.004	0.999
1	1	9.970	10.930	11.400	1.263	1.001	1.004
1	2	28.010	32.190	33.780	1.264	0.998	0.998
2	1	8.270	10.280	11.290	2.068	1.004	1.002
2	2	17.870	31.930	33.480	2.054	0.995	0.996
3	3	21.800	65.470	68.450	3.405	1.002	1.002

Notes:

2-COL is the two-column design.

$\tau = (\sqrt{\frac{\lambda}{2}}, \sqrt{\frac{\lambda}{2}}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Table 15. Simulation Results on Power and MSE for Eight Treatments, Case I

Shift	Treatment		Power			MSE	
	Effect	RCBD	SLS	2-COL	RCBD	SLS	2-COL
0	0	5.190	5.590	5.220	1.004	0.999	1.003
0	3	96.060	95.280	95.630	1.003	1.003	1.007
1	1	15.350	16.200	17.020	1.209	1.000	0.997
1	2	52.580	59.800	61.740	1.219	1.002	1.002
2	1	10.750	15.980	16.250	1.849	1.001	1.002
2	2	34.490	60.520	62.040	1.850	0.996	1.002
3	0	5.030	4.850	5.170	2.933	0.998	0.996
3	3	48.550	94.900	95.870	2.936	0.998	1.003

2-COL is the two-column design.

$\tau = (\sqrt{\lambda}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Table 16. Simulation Results on Power and MSE for Eight Treatments, Case II

Shift	Treatment		Power			MSE	
	Effect	RCBD	SLS	2-COL	RCBD	SLS	2-COL
0	3	92.290	91.090	92.000	1.002	1.002	0.999
1	1	12.100	14.000	14.190	1.221	1.000	1.002
1	2	45.570	53.170	53.880	1.217	0.996	1.000
2	1	9.740	14.260	14.810	1.861	1.002	1.000
2	2	29.390	52.010	53.880	1.860	0.999	1.001
3	3	40.920	90.760	91.950	2.942	0.999	1.000

2-COL is the two-column design.

$\tau = (\sqrt{\frac{\lambda}{2}}, \sqrt{\frac{\lambda}{2}}, 0, \dots, 0)'$ is the vector of treatment effects.

The values are percentages based on 5000 observations.

Averages for the power of the F-test at $\alpha = .05$ and for the estimate of experimental error are shown in Tables 11-16 for $t = 4, 6,$ and 8 respectively. Noting that only minor differences separate Cases I and II, we observe that the mean square error for the RCBD rises in direct proportion to the size of the shift, whereas the mean square error for the other designs merely fluctuates about the true experimental error. When no shift is present, the RCBD has somewhat higher power although the advantage seems to decrease as we look at larger designs. The semi-Latin square and two-column designs present consistently higher power levels for the larger border shifts while appearing to hold the alpha level at $.05$. The two-column and semi-Latin square designs are largely indistinguishable in practical terms.

Even though it seems clear that the RCBD does not perform as well as the other designs under border shifts, we should be cautious in generalizing. This simulation study did not add row-column interactions to the data, and as we recall, these interactions are the source of bias in the semi-Latin square and two-column designs. Perhaps further simulations using uniformity trials should be done in the future to see if our conclusions hold under actual experimental conditions.

3.5.2 Randomization Theory

In this part of the simulation study, we are concerned with the accuracy of the normal approximation to the randomization test. Our particular concern is the correspondence between the $\alpha = .05$ critical point from the F distribution and the same five percent critical point from the distribution of randomization F-ratios. Rather than attempt the herculean task of completely enumerating all possible assignments of treatments to plots and obtaining the exact critical F-ratio under randomization, we will sample the finite population of treatment assignments and estimate the critical F-ratio of the randomization set of each design.

The simulation design is similar to that of the normal theory study, but there are some important differences. In the normal theory study, each of the two replications looked at 5000 different data sets generated from the normal distribution. This time we will run three replications

which will each sample 5000 treatment assignments with replacement, but only one data set will be generated from the normal distribution for each replication. Thus for each shift value we will be sampling the finite population of conceptual responses for three sets of data. No treatment effects will be added in this study so that we may estimate the critical F value at $\alpha = .05$. The same border shift values and designs as before will be used; however, only six and eight treatment designs will be studied.

The average critical F values for the designs and the appropriate normal theory value are shown in Tables 17 and 18 for $t=6$ and 8 respectively. For $t=6$, an ANOVA run on the data did not reveal any significant differences among shifts. Thus it would appear that border shifts have no effect on the critical values of any of the designs. We note however that the small sample size in this study coupled with substantial variability may be hiding actual differences among the randomization F values. When we have eight treatments, shift differences do appear and they indicate that the critical value tends to get larger as the border shift increases. This may indicate that large border shifts in bigger experiments tend to skew the distribution of randomization F-ratios.

Results of this simulation study are interesting and indicate that further research is useful at some later date. If the randomization F does increase with sizeable border shifts, the normal theory F will tend to provide a relatively poor approximation under these circumstances. This poor approximation will occur regardless of the design utilized.

3.6 Chapter Summary

We have seen that an awareness of possible border shifts is desirable as we iterate between planning and analyzing experiments. The myriad of detection techniques for response shifts are necessary to warn us of border problems in a current experiment. With this warning we use alternative designs for the future experiment. The loss of power and the biased mean square error in the RCBD, for example, is ameliorated to varying extents by the Latin square, semi-Latin square,

Table 17. Empirical 95th Percentile of the Randomization F for T = 6

Design	F	Shift			
		0	1	2	3
RCBD	3.33	3.070	2.984	3.357	3.707
SLS	3.69	4.121	3.075	3.328	3.072
2-COL	3.48	3.304	2.950	3.611	3.282

where

F stands for the 95th percentile of the F distribution with the appropriate degrees of freedom

SLS is the semi-Latin square

2-COL is the two-column design.

Table 18. Empirical 95th Percentile of the Randomization F for T = 8

Design	F	Shift			
		0	1	2	3
RCBD	2.49	2.264	2.384	2.398	2.473
SLS	2.58	2.066	2.448	2.504	2.778
2-COL	2.51	2.191	2.462	2.319	2.598

where

F stands for the 95th percentile of the F distribution with the appropriate degrees of freedom

SLS is the semi-Latin square

2-COL is the two-column design.

and two-column designs. The Latin square is the ideal choice although lack of experimental material may necessitate use of the other alternatives. We have seen that the split plot design responds to shifts in ways similar to the RCBD and that similar design alternatives are useful.

The problem of border shifts and response shifts in general can be viewed as the discrete version of more gradual changes. Suppose that instead of having a sudden response shift in some area of our design, there is a gradual shift or trend as we move into an area of the design. This trend problem is possibly as damaging to the analysis and is much less obvious than a sudden shift in response. We will see that a trend may often be modelled as correlated experimental units, the topic of the next chapter.

Chapter IV

IV. Correlated Experimental Units

Concern with gradual response shifts begins early in the statistical literature. Although the references to be cited are mostly concerned with fertility trends in field experiments, the problem certainly is present in other disciplines. Suppose for example that t treatments are to be applied r times each in an experiment concerning a chemical process. If the treatments are applied in a sequence over a narrow time span, it might be reasonable to assume that the adjacent observations are correlated or that some time trend might exist in the data.

As noted before with the border shifts, there are two approaches to this problem. Either a design can be constructed to deal with the possibility of a trend in the data, or alternative analyses can be developed to account for the trend after the experiment is conducted. Both viewpoints are valid and serve to complement each other in the balancing act between planning and analyzing experiments. Our concern will be the properties of a specific alternative analysis when a design for correlated data is implemented; i.e., our concern is the overlap of the two viewpoints.

4.1 History

Any history scholar will probably point out that the past informs us of the present and future. Therefore before beginning any theoretical work on the subject of correlated observations, a peek at the evolution of thought on the subject is advisable. Trends in agronomic data were objects of scrutiny quite early in agricultural statistics. The typical concerns were fertility trends that increased experimental variability and could make the analyses suspect. Wiancko (1914) suggests the use of check (control) plots to adjust the responses of the treated plots, but his worry is that the check variety could be resistant to trends affecting the test varieties.

Wiancko's misgivings about check plots and the increase in experimental size that they precipitate were not fatal blows to the concept. McClelland (1926) discusses different methods of using control plots and lists the following assumptions necessary for using control plots:

1. The yields of both the control and the test plots are correct;
2. The changes in the plots are gradual;
3. Seasonal conditions affect the control and test plots equally; and
4. Correlation between check and test plots must exist.

Yates (1936) formalizes the use of systematic check plots in an analysis of covariance, and demonstrates that it is less efficient than using a lattice design.

Despite Yates' conclusions which were supported theoretically by Atiqullah and Cox (1962) for incomplete block designs, the use of control plots receives continued interest. For example, Lin (1983) presents a method of including systematic check plots in the subplots of a split plot design. The use of check plots is generally restricted today to large variety selection programs where replication is restricted or nonexistent.

An alternative approach to adjusting for fertility trends by using check plots was first advanced by Papadakis (1937). The basic idea is to adjust the yield of each plot by using the information from adjacent plots. Bartlett (1938) comments on the method, but the Papadakis analysis was generally ignored until Atkinson (1969) showed its similarity to maximum likelihood estimators. Interest in the area, now called nearest neighbours models, picked up considerably after Bartlett (1978) presented a thorough review of the then current state of the field. There are now a bevy of competing models and analytic techniques.

4.2 *Competing Models*

The diversity of approaches to the basic problem of fertility trends can be traced to several sources. One possible cause is that Papadakis did not assume an explicit probability model in forming his adjusted estimates, and he thus left the area open to competing probability models. A more likely reason is the diversity of applications to which the idea of nearest neighbours has been extended. Besag and Kempton (1986) cite four distinct applications of the use of neighbouring plots. The traditional check plot methodology and the Papadakis analysis constitute the first two applications which are generally concerned with fertility trends. Interplot competition between different varieties or plants necessitates a different model from fertility trends. Besag and Kempton recommend "an autoregressive formulation of a competition model, where plot values are directly related, usually negatively related, to the values of neighbouring plots." The final application is that in which plot values are affected by the actual treatment applied to neighbouring plots.

Another source of model diversity comes from Ord (1975) who postulates an autoregressive model of the form

$$Y_i = \alpha + \rho \sum_{j \in J(i)} w_{ij} Y_j + \varepsilon_i$$

where

ρ is a parameter of the correlation matrix which reflects the dependence on the i 'th location on the other locations;

$J(i)$ is the set of locations whose variates interact with Y_i ;

w_{ij} is a set of nonnegative weights; and

$\varepsilon_i \sim N(0, \sigma^2)$ in general.

The choice of weights w_{ij} , restricted only so that $\sum_j w_{ij} = 1$, permits great flexibility in model formulation. Aside from simple weighting schemes based on square lattices or time series principles, a researcher can create weights from information on distance between locations, length of common boundaries, direction, item size, or whatever seems pertinent.

From the plentitude of viable models, we shall discuss only three. These three methods will serve to illustrate the basic concepts of nearest neighbour analyses. We shall examine the original Papadakis analysis, least squares smoothing, and the method of first differencing.

4.2.1 Papadakis

The original analysis proposed by Papadakis is a non-iterative procedure for plots in a one-dimensional sequence. Following the notation of Atkinson (1969), an experiment consists of $N = mt$ experimental units where t is the number of treatments, each of which occur m times. The yield of the i 'th plot is first corrected by the mean of the other plots receiving the same treatment; i.e., the corrected yield y_i^c of plot i receiving treatment s is

$$y_i^c = y_i - m^{-1} \sum_{\{j\}=s} y_j .$$

The $[j] = s$ notation represents the set of plots j receiving treatment s . A concomitant variable,

$$x_i = 2^{-1}(y_{i-1}^c + y_{i+1}^c) ,$$

is then calculated from the adjacent corrected plot values.

Analysis of covariance on the corrected yields with x_i as covariate will provide us with estimates of the treatment effects. Denoting the effect of treatment s as τ_s , the Papadakis estimator is

$$\hat{\tau}_s = m^{-1} \left[\sum_{[j]=s} y_j - \hat{\beta} 2^{-1} \sum_{[j]=s} (y_{j\pm 1} - m^{-1} \sum_{[l\pm 1]=s} y_l) \right]$$

where

$$\hat{\beta} = \left(\sum_{i=1}^N x_i y_i^c \right) + \left(\sum_{i=1}^N x_i^2 \right) .$$

Atkinson proceeds to simplify this expression upon the assumption that a Williams Type II(a) design is used and derives expectations and variances of the estimates.

Comparing the Papadakis method to the maximum likelihood estimator for a first-order autoregressive model, Atkinson finds that "the Papadakis estimator is a first approximation to the maximum likelihood estimator, regardless of the design." The Papadakis estimator is found to be "readily calculable and yields estimators of greater precision than those obtained by averaging over the relevant treatments" for designs in which the maximum likelihood estimator is difficult to obtain.

Draper and Faraggi (1985) provide a matrix formulation of the Papadakis estimator and demonstrate that it is appropriate for at least one covariance assumption. Using the same experiment as described by Atkinson, they assume the model to be

$$y_i = \tau_s + \varepsilon_i$$

where

τ_s is the effect of treatment s ($= 1, 2, \dots, t$) and

ε_i are the correlated errors with $VAR(\varepsilon_i) = V\sigma^2$.

Let X be the $n \times t$ design matrix for the experiment and N be the $n \times n$ neighbour matrix whose i 'th row contains 1 in positions j for which plot j is adjacent to plot i and zeros elsewhere.

The Papadakis estimator is then

$$\hat{\tau}_s = m^{-1}[X'Y - \hat{\phi}X'N(I - m^{-1}XX')Y]$$

where

$$\hat{\phi} = \frac{Y'(I - m^{-1}XX')N(I - m^{-1}XX')}{Y'(I - m^{-1}XX')N^2(I - m^{-1}XX')}$$

Draper and Faraggi demonstrate that this estimator is appropriate for $V^{-1} = I - \gamma N$ where $\gamma = \rho(1 + \rho^2)^{-1}$ and ρ is a parameter of the correlation structure. They conclude that the maximum likelihood estimator is preferred in this situation.

The correspondence of the Papadakis estimator to the maximum likelihood estimator is noteworthy, especially since Ripley (1978) shows the equivalence of the maximum likelihood estimator to generalized least squares by using the model

$$Y = X\bar{\mu} + \varepsilon$$

where

Y is the $n \times 1$ vector of observations;

X is the $n \times t$ design matrix;

$\bar{\mu}$ is a vector of treatment means (no overall mean); and

ε is random error with mean zero and variance structure $\sigma^2 B^{-1}$ where σ^2 is the variability associated with an individual unit.

The variance matrix B is defined to be $I - \beta N$ where N is the neighbour matrix and β is the covariance between neighbouring units. Thus the maximum likelihood estimate is

$$\tau^{ML} = (X'BX)^{-1}X'BY$$

with variance matrix $(X'BX)^{-1}$. It is shown that the Papadakis estimator is a first approximation to the maximum likelihood estimator.

Since the Papadakis estimator can be considered a first approximation to the maximum likelihood estimator, it is perhaps inevitable that an iterative Papadakis estimator has appeared. Extensions to two-dimensional situations have also been done, and Papadakis has considered making his estimator robust to abnormal plot values. However we will confine our attention to the one-dimensional case which is a first approximation to maximum likelihood.

4.2.2 Least Squares Smoothing

An alternative to the models used by Papadakis and to the autoregressive models comes from Green, Jennison, and Seheult (1985). They use a "smooth trend plus independent error" model under which plot values are additively decomposed as

$$Y = X\tau + F + \varepsilon$$

where

Y is an $n \times 1$ vector of responses;

X is an $n \times t$ design matrix;

τ is an $t \times 1$ vector of treatment effects;

F is an $n \times 1$ vector representing trend effects; and

ε is an $n \times 1$ vector of residual errors.

Although the method has been extended to the two-dimensional situation, we will be concerned only with the one-dimensional.

A differencing matrix Δ is chosen such that $\Delta F \cong 0$ for every plot i . If F is locally nearly linear, then for every plot i which is not an end plot, we have constants 1, -2, 1 in respectively the $(i-1)$, i , $(i+1)$ columns of Δ . The observed values of ΔF indicate how far from linear the trend effects are, and hence Green, et al. fit τ, F, ε by minimizing

$$\lambda(\Delta F)' \Delta F + \varepsilon' \varepsilon$$

where λ is a tuning constant used to vary the smoothness in the fitted F . In general, a larger λ will result in a smoother fitted F . There is a side condition that $1' X \tau = 0$ where $1 = (1, 1, \dots, 1)'$ is an $n \times 1$ vector.

The fitted values of τ and F may also be viewed as solutions to the simultaneous equations

$$(I + \lambda \Delta' \Delta) F = Y - X \tau$$

$$X' X \tau = X' (Y - F)$$

$$1' X \tau = 0 .$$

Alternative Δ matrices, such as first-differences, may be used in this formulation. A first differencing matrix would have entries 1, -1 for columns $i, (i+1)$ for each row of Δ and would be invariant to the addition of a separate constant to each column.

Green, et al. note that the least squares smoothing treatment estimate may also be obtained by a generalized least squares analysis on ΔY , assuming $E(\Delta Y) = \Delta X \tau$ and $VAR(\Delta Y) = \sigma^2(\lambda^{-1} I + \Delta \Delta')$. This is true for a random F , independent of ε , with elements of ΔF independent with mean zero and variance $\sigma^2 \lambda^{-1}$. Whether seen as generalized least squares or least squares smoothing, the rub is in determining a value for λ . The authors note "that the var-

iances of treatment estimates decrease with increasing λ but on 'average' bias increases. " A subjective, graphics oriented approach is suggested by the authors to determine λ .

4.2.3 First Differencing

A third approach to trend elimination is quite similar to least squares smoothing and is called the first differencing model by Besag and Kempton (1986). They assume that the plot response can be modelled by a combination of treatment effects and a stochastic fertility process $F = (F_1, \dots, F_n)'$. Thus they write

$$Y = \mu 1 + X\tau + F$$

where

Y is an $n \times 1$ vector of responses;

μ is an overall mean and $1 = (1, \dots, 1)'$ is an $n \times 1$ vector;

X is the $n \times (t-1)$ design matrix;

τ is the $(t-1) \times 1$ vector of treatment effects; and

F is a vector of the stochastic fertility variables.

In this formulation there is no separate term for pure error, making it analogous to a conceptual yield. Besag and Kempton also extend this model to the case with independent pure errors. With appropriate assumptions and decomposition of F we could have the least squares smoothing model discussed previously.

Besag and Kempton assume that the first differences $F_i - F_{i+1}$, $i = 1, \dots, (n - 1)$, to be uncorrelated random variables with mean zero and variance κ . Using the first differencing matrix Δ as defined for least squares smoothing, we have

$$U_{n-1} = \Delta Y = \Delta\mu 1 + \Delta X\tau + \Delta F$$

where $\Delta\mu 1 = 0$ and ΔF is assumed to have $E(\Delta F) = 0$ and $VAR(\Delta F) = \kappa I_{n-1}$. Therefore we have

$$E(U) = \Delta X\tau = D\tau \text{ and}$$

$$VAR(U) = \kappa I_{n-1}.$$

The usual least squares estimate is now obtained using the adjusted plot values. This estimate is

$$\tau_0^a = (D'D)^{-1}D'U$$

with variance-covariance matrix $\kappa(D'D)^{-1}$.

The authors note that " apart from possible end-plot assumptions, τ_0^a is equivalent to the intra-NN (nearest neighbour) estimate of Wilkinson et al (1983) and to the iterated Papadakis (1970) estimate with $b = 1$." Since the first differencing approach seems to agree with several other approaches, it will be a target of our scrutiny. We have also seen that several models can be re-expressed as special cases of generalized least squares.

It seems reasonable to think of gradual trends in the data as a manifestation of correlated experimental units. Specifically, we wish to examine correlation between adjacent experimental units only. Thus far we have looked only at analyses meant to recover information after the experiment has been conducted and correlation discovered. Once again a valid alternative is to design in advance to account for suspected correlation. Some work has been done in this area and merits discussion.

4.3 Designs for Correlated Observations

Williams (1952) provides a class of designs to be used for serially correlated observations. The observations are assumed to be in a long sequence or line and "the errors are correlated in a stationary linear autoregressive process (without trend)." Three types of designs are considered. The Type I designs are systematic and hence will not be considered here. Two examples given by Williams are

$$I(a) \quad (1,2,3, \dots, t)(1,2,3, \dots, t) \dots (1,2,3, \dots, t)$$

and

$$I(b) \quad (1,2,3, \dots, t)(t,t-1,t-2, \dots, 1)(1,2,3, \dots, t) \dots$$

where t is the number of treatments. Designs with which we will be concerned are the Type II designs, specifically the Type II(a). Type II(a) and II(b) designs require respectively

II(a) "that each treatment shall occur equally often (say c times) adjacent to every other treatment," and

II(b) "that each treatment shall occur equally often adjacent to every treatment including itself."

The final group of Williams designs, designated Type III, require that each treatment occurs equally often adjacent to every other treatment and each treatment occurs equally often adjacent but one to every other treatment. A problem with the Type II and III designs is that no algebraic means of construction has been offered, nor have they been completely enumerated. This will present problems in the next chapter when expectations under randomization are needed.

Other designs and specific criteria have been developed since Williams' paper. Finney and Outhwaite (1956) slightly modify Williams' designs such that each ordered pair of treatments occurs c times. Extensions to two-dimensional designs are considered by Freeman (1979) and Martin (1982). A design criterion for the construction of optimal neighbour designs is advanced by Williams (1985).

4.4 Randomization Analysis

The object of this investigation is to examine the effect of correlated observations on the expected mean squares, treatment estimates, and treatment variances by using randomization theory. The model will focus on the variance-covariance matrix which reflects correlation between adjacent experimental units. As a way of introduction to the problem, we first consider the RCBD with independent blocks. Kempthorne (1952) considers the responses of the experimental units to be fixed quantities with all distributional properties provided by the randomization process. The additional assumption of distributional properties for the experimental units indicates that an alternate analytical technique might be advisable. Aastveit (1983) provides an excellent methodology.

Using Aastveit's approach, the model for an RCBD is

$$Y_{i(j)} = \mu + \beta_i + \tau_j + e_{ij} \quad (4.1)$$

where

$Y_{i(j)}$ is the observed response for the unit receiving treatment j ($= 1, 2, \dots, t$) in block i ($= 1, 2, \dots, b$);

μ is the overall mean;

β_i is the effect of the i 'th block; and

τ_j is the effect of the j 'th treatment;

e_{ij} has mean zero and variance-covariance matrix V .

Model (4.1) may be written in matrix notation as

$$Y = X\gamma + e$$

where

Y is an $n \times 1$ vector of responses ;

$X = (X_1 : X_2)$ where X_1 is the design matrix for the β and X_2 corresponds to the τ ;

$\gamma = (\mu, \beta_1, \dots, \beta_b, \tau_1, \dots, \tau_t)'$ is the vector of block and treatment effects; and

$e = (e_{11}, e_{12}, \dots, e_{bt})'$ is the vector of residuals.

The Y_{ikj} are considered to be in the original order of the plots which are assumed to be in a sequence. This must be the case in order to construct the proper variance-covariance structure of the observations. Of course the treatments are randomly assigned to the plots and we do not know in advance what specific values the entries of X will have. This is the point at which a randomization matrix is used to connect the Y_{ikj} which are in the natural order to a new set of random variables Z_{ij} which have a specified arrangement of the design matrix. This randomization matrix is denoted by $P_{n,m} = \{\delta_{ij}^k\}$ where

$\delta_{ij}^k = 1$ if treatment j is placed in plot k of block i , and

$\delta_{ij}^k = 0$ otherwise.

The matrix P is a block diagonal matrix whose b blocks consist of $t \times t$ submatrices denoted by P_j . For example, consider $t = 4$ with the randomization in block 1 given by

$$P_1 = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \end{bmatrix}$$

Note that $P'_1 P_1 = P_1 P'_1 = I$ and hence that $P'P = PP' = I$. We can think of P as an orthogonal random mapping from the Y_{ij} in the natural sequence to the Z_{ij} in the specified order; conversely, P' randomly reorders the design matrix for the Z_{ij} as it maps to the natural sequence of the $Y_{(j)}$. Thus the P matrix is used to define

$$Z = PY = PX\gamma + Pe$$

which we write as

$$Z = X^\delta \gamma + e^\delta.$$

We can write as well that $Y = P'Z$ and that $X = P'X^\delta$.

Aastveit partitions the X^δ matrix into 2 parts, $X^\delta = (X_1^\delta : X_2^\delta)$, corresponding to block and treatment effects. We define the two parts as

$$X_1^\delta = \begin{bmatrix} 1 & 1 & 0 & \dots & 0 \\ : & : & : & \dots & : \\ 1 & 1 & 0 & \dots & 0 \\ 1 & 0 & 1 & \dots & 0 \\ : & : & : & \dots & : \\ 1 & 0 & 1 & \dots & 0 \\ : & : & : & \dots & : \\ : & : & : & \dots & : \\ 1 & 0 & 0 & \dots & 1 \\ : & : & : & \dots & : \\ 1 & 0 & 0 & \dots & 1 \\ 1 & -1 & -1 & \dots & -1 \\ : & : & : & \dots & : \\ 1 & -1 & -1 & \dots & -1 \end{bmatrix}$$

and

$$X_2^\delta = \begin{bmatrix} D \\ : \\ : \\ D \end{bmatrix}$$

where

$$D = \begin{bmatrix} 1 & 0 & \dots & 0 \\ 0 & 1 & \dots & 0 \\ \vdots & \vdots & \dots & \vdots \\ \vdots & \vdots & \dots & \vdots \\ 0 & 0 & \dots & 1 \\ -1 & -1 & \dots & -1 \end{bmatrix} .$$

Thus the randomization matrix P connects the known matrices of X^δ to the unknown matrices of X. Expectations of the usual analysis of variance, conditional on P, may be taken with the X^δ , and then expectations taken with respect to the randomization P.

Suppose that the Y_{ij} have variance-covariance structure

$$V = \begin{bmatrix} \sigma_0 & \sigma_1 & 0 & \dots & 0 & 0 \\ \sigma_1 & \sigma_0 & \sigma_1 & \dots & 0 & 0 \\ \vdots & \vdots & \vdots & \dots & \vdots & \vdots \\ \vdots & \vdots & \vdots & \dots & \vdots & \vdots \\ 0 & 0 & 0 & \dots & \sigma_0 & \sigma_1 \\ 0 & 0 & 0 & \dots & \sigma_1 & \sigma_0 \end{bmatrix} . \quad (4.2)$$

Then we have

$$E(Z|P) = X^\delta \gamma = PX\gamma$$

and

$$VAR(Z|P) = VAR(PY) = PVP' .$$

The least squares estimate of γ is

$$\hat{\gamma} = (X'X)^{-1}X'Y$$

which has

$$E(\hat{\gamma}|P) = (X^{\delta'} X^{\delta})^{-1} X^{\delta'} P P' X^{\delta} \gamma = \gamma$$

and

$$VAR(\hat{\gamma}|P) = (X^{\delta'} X^{\delta})^{-1} X^{\delta'} P V P' X^{\delta} (X^{\delta'} X^{\delta})^{-1} .$$

The relevant sums of squares are

$$SS_{trt} = Y' X_2 (X_2' X_2)^{-1} X_2' Y$$

and

$$SS_{error} = Y' [I - X(X'X)^{-1} X'] Y$$

with

$$E[SS_{trt} | P] = tr [X_2^{\delta'} (X_2^{\delta} X_2^{\delta})^{-1} X_2^{\delta'} P V P'] + \gamma_2' (X_2^{\delta'} X_2^{\delta}) \gamma_2$$

and

$$E[SS_{error} | P] = tr [V] - tr [(X^{\delta'} X^{\delta})^{-1} X^{\delta'} P V P' X^{\delta}] .$$

Aastveit has derived the unconditional expectations of these sums of squares. The expected mean squares are shown in Table 19. We now have the analytic tools to derive the expectations under randomization, a design for correlated observations, and several available analyses for correlated data.

Table 19. ANOVA for RCBD with Correlated Errors

Source	DF	Expected Mean Square
Treatment	$t-1$	$[1 - 2t^{-1}\rho] \sigma_0 + (t - 1)^{-1} \gamma'_2 (X_2^0 X_2^0) \gamma_2$
Error	$(t-1)(t-1)$	$[1 - 2t^{-1}\rho] \sigma_0$

Chapter V

V. Results for Correlated Experimental Units

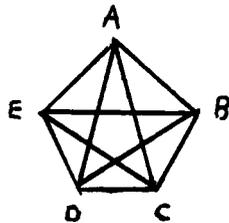
Our intent is to model a trend in the data with the variance-covariance structure of (4.2). Aastveit has shown its effect on the RCBD, but we intend to examine the efficacy of the Type II(a) design. The data arising from the Type II(a) design will be analyzed with the usual ANOVA as well as the method of first differencing, and the expectations under randomization computed.

The final investigation will examine a combination of gradual trends and border shifts. Border shifts will be added to Aastveit's treatment of the RCBD. We will also look at border shifts for the Type II(a) design under both the usual ANOVA and first differencing. Thus we will have examined response shifts as abrupt changes in a specific design region, the effect of correlated responses, and a combination of the two.

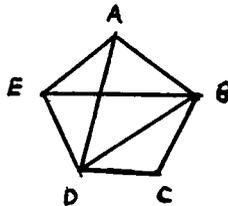
5.1 Type II(a) Design with the Usual Analysis

5.1.1 Five Treatments

First consider the Type II(a) design for $t = 5$ treatments and $c = 1$. The design may be characterized by the complete graph shown below.

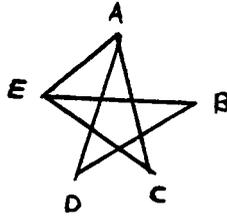


The nodes of the graph correspond to the five treatments and the connecting lines can be considered as paths between the treatments. As treatments are randomly placed in the experimental sequence, the lines between adjacently placed treatments are removed from the graph. For example, suppose that the first three treatments are A,C,E; the new graph follows.



Typically the treatments are chosen for the Type II(a) design in "blocks" such that each treatment occurs exactly once per block; in graph theory these blocks are called Euler circuits. A complete graph with an odd number of nodes has $e = (t - 1) \div 2$ Euler circuits. When the e Euler circuits (or blocks) are constructed, all the connecting lines have been removed and a Hamilton circuit has been constructed. Thus the Type II(a) design is a Hamilton circuit composed of $e = (t - 1) \div 2$ Euler circuits.

Suppose for $t = 5$ that the first Euler circuit is ABCDE, leaving the following graph.



Graph theory indicates that this circuit is isomorphic, essentially equal to, the other 5! Euler circuits that could be obtained by interchanging the treatment labels. Thus for each valid design completed from this particular graph there are 5! other valid designs, and this set of designs will constitute all possible $t = 5, c = 1$ Type II(a) designs. The six ways to get another Euler circuit from the above graph are shown below.

ABCDE ACEBD

ABCDE ADBEC

ABCDE BDACE

ABCDE BDAEC

ABCDE CADBE

ABCDE CAEBD

Thus there are $6(5!)$ possible Type II(a) designs for $t = 5$ and $c = 1$.

Assume that the experimental units lie in a sequence and have a variance-covariance structure of the type in (4.2). The linear model of the observations is written

$$Y = X\tau + e \quad (5.1)$$

where

Y is the $n \times 1$ vector of observations in their observed sequence;

X is the design matrix which is different for each distinct randomization;

$\tau = (\mu, \tau_1, \dots, \tau_{t-1})'$ which reflects the imposition of the restriction that $\sum_{j=1}^t \bar{\tau}_j = 0$ upon
 $\bar{\tau} = (\mu, \bar{\tau}_1, \dots, \bar{\tau}_t)'$. and

e is the residual error with $E(e) = 0$ and $\text{VAR}(e) = V$ where V is defined by (4.2).

We now define

$$Z = PY = X^\delta \tau + e^\delta .$$

where P is the randomization matrix as defined by the Type II(a) design and

$$X^\delta = \begin{bmatrix} 1 & 1 & 0 & \dots & 0 \\ : & : & : & \dots & : \\ 1 & 1 & 0 & \dots & 0 \\ 1 & 0 & 1 & \dots & 0 \\ : & : & : & \dots & : \\ 1 & 0 & 1 & \dots & 0 \\ : & : & : & \dots & : \\ : & : & : & \dots & : \\ 1 & 0 & 0 & \dots & 1 \\ : & : & : & \dots & : \\ 1 & 0 & 0 & \dots & 1 \\ 1 & -1 & -1 & \dots & -1 \\ : & : & : & \dots & : \\ 1 & -1 & -1 & \dots & -1 \end{bmatrix} .$$

The new vector Z has expectation $X^b\tau$ and variance PVP' .

The usual analysis of variance will have

$$SS_{error} = Y'[I - X(X'X)^{-1}X']Y$$

and

$$SS_{trt} = Y'[X_2(X_2'X_2)^{-1}X_2']Y$$

where X_2 consists of all the X matrix except the first column. Expectations conditional on the randomization are known from linear model theory to be

$$E[SS_{error} | P] = tr\{V\} - tr\{(X'X)^{-1}X'V'X\}$$

which upon substituting $X = P'X^\delta$ is

$$E[SS_{error} | P] = tr\{V\} - tr\{(X^{\delta'}X^\delta)^{-1}X^{\delta'}PVP'X^\delta\} \quad (5.2)$$

and

$$E[SS_{trt} | P] = tr\{X_2^\delta(X_2^{\delta'}X_2^\delta)^{-1}X_2^{\delta'}PVP'\} + \tau_2'(X_2^{\delta'}X_2^\delta)\tau_2 \quad (5.3)$$

where $\tau_2 = (\tau_1, \dots, \tau_{t-1})'$.

Unconditional expectations rely on the expectation under randomization of PVP' . For a single replication of a design for $t = 5$ and $c = 1$, the randomization matrix P consists of elements δ'_{jk} with

$\delta'_{jk} = 1$ if application k ($= 1, 2$) of treatment j ($= 1, \dots, t$) falls on unit l ($= 1, \dots, 10$) and

$\delta'_{jk} = 0$ otherwise.

Multiple independently randomized replications could be accommodated by an appropriate extension of P. Reflecting that a sequence of Euler circuits is used in constructing the design, there is a restriction that

$$\delta_{jk}^l = 0 \text{ if } k = 1 \text{ and } l > 5 \quad (5.4a)$$

and

$$\delta_{jk}^l = 0 \text{ if } k = 2 \text{ and } l < 6 . \quad (5.4b)$$

Some simple properties of the δ_{jk}^l which meet this restriction are

$$P\{\delta_{jk}^l = 1\} = t^{-1}$$

$$P\{\delta_{jk}^l = 1, \delta_{j'k}^l = 1\} = 0$$

$$P\{\delta_{jk}^l = 1, \delta_{j'k}^{l'} = 1\} = 0 \text{ and}$$

$$P\{\delta_{jk}^l = 1, \delta_{j'k}^{l'} = 1\} = [t(t-1)]^{-1} \text{ for } j \neq j', l \neq l' .$$

It is now possible to examine E(PVP'). First consider

$$PV = \begin{bmatrix} \delta_{11}^1 & \delta_{11}^2 & \dots & \delta_{11}^{10} \\ \delta_{12}^1 & \delta_{12}^2 & \dots & \delta_{12}^{10} \\ \vdots & \vdots & \vdots & \vdots \\ \delta_{51}^1 & \delta_{51}^2 & \dots & \delta_{51}^{10} \\ \delta_{52}^1 & \delta_{52}^2 & \dots & \delta_{52}^{10} \end{bmatrix} \begin{bmatrix} \sigma_0 & \sigma_1 & \dots & 0 & 0 \\ \sigma_1 & \sigma_0 & \dots & 0 & 0 \\ 0 & \sigma_1 & \dots & 0 & 0 \\ \vdots & \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & \dots & \sigma_0 & \sigma_1 \\ 0 & 0 & \dots & \sigma_1 & \sigma_0 \end{bmatrix}$$

whose (jk)'th row, denoted by $(PV)_{jk}$, is

$$(PV)'_{jk} = \begin{bmatrix} \delta_{jk}^1 \sigma_0 + \delta_{jk}^2 \sigma_1 \\ \delta_{jk}^1 \sigma_1 + \delta_{jk}^2 \sigma_0 + \delta_{jk}^3 \sigma_1 \\ \delta_{jk}^2 \sigma_1 + \delta_{jk}^3 \sigma_0 + \delta_{jk}^4 \sigma_1 \\ \vdots \\ \vdots \\ \delta_{jk}^8 \sigma_1 + \delta_{jk}^9 \sigma_0 + \delta_{jk}^{10} \sigma_1 \\ \delta_{jk}^9 \sigma_1 + \delta_{jk}^{10} \sigma_0 \end{bmatrix} .$$

We now use

$$P' = \begin{bmatrix} \delta_{11}^1 & \delta_{12}^1 & \dots & \delta_{51}^1 & \delta_{52}^1 \\ \delta_{11}^2 & \delta_{12}^2 & \dots & \delta_{51}^2 & \delta_{52}^2 \\ \vdots & \vdots & \vdots & \vdots & \vdots \\ \delta_{11}^{10} & \delta_{12}^{10} & \dots & \delta_{51}^{10} & \delta_{52}^{10} \end{bmatrix}$$

to look at elements of PVP'. A diagonal element of PVP' is the (jk)'th row of PV multiplied by the (jk)'th column of P'. Thus a diagonal element is

$$\delta_{jk}^1 (\delta_{jk}^1 \sigma_0 + \delta_{jk}^2 \sigma_1) + \sum_{l=2}^9 \delta_{jk}^l (\delta_{jk}^{l-1} \sigma_1 + \delta_{jk}^l \sigma_0 + \delta_{jk}^{l+1} \sigma_1) + \delta_{jk}^{10} (\delta_{jk}^9 \sigma_1 + \delta_{jk}^{10} \sigma_0) . \quad (5.5)$$

Because treatment j cannot occur on adjacent units, (5.5) simplifies to $\sum_{l=1}^{10} (\delta_{jk}^l)^2 \sigma_0$ which has expectation $t(t)^{-1} \sigma_0 = \sigma_0$. Off-diagonal elements of PVP' involve $(PV)_{jk}$ and the (j'k')th column of P'; at least one of $k \neq k'$ and $j \neq j'$ must be true. Thus an off-diagonal element of PVP' is

$$\delta_{jk}^1 (\delta_{j'k'}^1 \sigma_0 + \delta_{j'k'}^2 \sigma_1) + \sum_{l=2}^9 \delta_{jk}^l (\delta_{j'k'}^{l-1} \sigma_1 + \delta_{j'k'}^l \sigma_0 + \delta_{j'k'}^{l+1} \sigma_1) + \delta_{jk}^{10} (\delta_{j'k'}^9 \sigma_1 + \delta_{j'k'}^{10} \sigma_0) .$$

Entries of PVP' involving the same treatment have the form

$$\delta_{jk}^1(\delta_{jk}^1 \cdot \sigma_0 + \delta_{jk}^2 \cdot \sigma_1) + \sum_{l=2}^9 \delta_{jk}^l (\delta_{jk}^{l-1} \sigma_1 + \delta_{jk}^l \cdot \sigma_0 + \delta_{jk}^{l+1} \sigma_1) + \delta_{jk}^{10}(\delta_{jk}^9 \cdot \sigma_1 + \delta_{jk}^{10} \cdot \sigma_0) .$$

Since treatment j cannot occur in adjacent units, the expectation of the above expression is zero.

The off-diagonals involving different treatments will also have two types of entries. Entries will have different treatments from either the same or different blocks where k indexes blocks as per (5.4a,b). These blocks are not used in the analysis but merely serve as a convenient way to generate the design. First consider two different treatments in the same block. The entry in PVP' is

$$\delta_{jk}^1(\delta_{j'k}^1 \sigma_0 + \delta_{j'k}^2 \sigma_1) + \sum_{l=2}^9 \delta_{jk}^l (\delta_{j'k}^{l-1} \sigma_1 + \delta_{j'k}^l \sigma_0 + \delta_{j'k}^{l+1} \sigma_1) + \delta_{jk}^{10}(\delta_{j'k}^9 \sigma_1 + \delta_{j'k}^{10} \sigma_0) . \quad (5.6)$$

We know that $E(\delta_{jk}^l \delta_{j'k}^l) = 0$. For convenience and with no loss of generality, assume that $k = 1$ such that $\delta_{j'k}^l = 0$ for $l > 5$. Thus (5.6) is now

$$\sigma_1 [\delta_{j1}^1 \delta_{j'1}^2 + \sum_{l=2}^4 \delta_{j1}^l (\delta_{j'1}^{l-1} + \delta_{j'1}^{l+1}) + \delta_{j1}^5 \delta_{j'1}^4]$$

where $E(\delta_{j1}^l \delta_{j'1}^l) = [t(t-1)]^{-1}$ for $l \neq l'$ and $l < 6$. There are $2(t-1)$ non-zero terms in the expression, leaving us with the expectation of (5.6) being $2t^{-1}\sigma_1$.

Remaining entries of PVP' consist of different treatments in separate blocks. The form of an entry is

$$\delta_{jk}^1(\delta_{j'k}^1 \cdot \sigma_0 + \delta_{j'k}^2 \cdot \sigma_1) + \sum_{l=2}^9 \delta_{jk}^l (\delta_{j'k}^{l-1} \sigma_1 + \delta_{j'k}^l \cdot \sigma_0 + \delta_{j'k}^{l+1} \sigma_1) + \delta_{jk}^{10}(\delta_{j'k}^9 \cdot \sigma_1 + \delta_{j'k}^{10} \cdot \sigma_0) \quad (5.7)$$

Recall that if $k = 1$ and $l > 5$ then $\delta_{j'k}^l = 0$, and that if $k' = 2$ and $l < 6$ then $\delta_{j'k}^l = 0$. Suppose $k = 1$ and $k' = 2$; then (5.7) reduces to $\delta_{j1}^5 \delta_{j'2}^5 \sigma_1$. This expression represents the probability that treatment j is on unit 5 and treatment j' is on unit 6. This is equivalent to

$$\sum_{i=1}^5 P(j \text{ is on unit } 5, j' \text{ is on unit } 6, j' \text{ is on unit } i)$$

$$\begin{aligned}
&= \sum_{i=1}^5 P(j' \text{ is on unit 6} \mid j \text{ is on unit 5, } j' \text{ is on unit } i) P(j \text{ is on unit 5, } j' \text{ is on unit } i) \\
&= \sum_{i=1}^5 P(j' \text{ is on unit 6} \mid j \text{ is on unit 5, } j' \text{ is on unit } i) P(j \text{ is on unit 5} \mid j' \text{ is on unit } i) P(j' \\
&\text{ is on unit } i) \\
&= [t(t-1)]^{-1} \sum_{i=1}^5 P(j' \text{ is on unit 6} \mid j \text{ is on unit 5, } j' \text{ is on unit } i).
\end{aligned}$$

We find the final probability by observing the six designs at the beginning of the section. The expectation of (5.7) is seen to be $\sigma_1 [t(t-1)]^{-1}$.

Rows and columns of PVP' are indexed by (j,k) in the sequence (11,12,21,22, ..., 51,52). The expectation of PVP' may then be characterized by entry as

$$\sigma_0 \text{ for } j = j', k = k'$$

$$0 \text{ for } j = j', k \neq k'$$

$$2t^{-1}\sigma_1 \text{ for } j \neq j', k = k'$$

$$[t(t-1)]^{-1}\sigma_1 \text{ for } j \neq j', k \neq k'.$$

Unconditional expectations for the sums of squares may now be taken.

Looking first at the sum of squares for error in (5.2), we see immediately that $\text{tr}(V) = 2t\sigma_0$.

The second term of the expression involves some extra algebra. We know that

$$X^{\delta'} X^{\delta} = \begin{bmatrix} 2t & 0 & 0 & 0 & 0 \\ 0 & 2b & b & b & b \\ 0 & b & 2b & b & b \\ 0 & b & b & 2b & b \\ 0 & b & b & b & 2b \end{bmatrix}$$

The inverse of the (1,1) element is $(2t)^{-1}$ and the remaining $(t-1) \times (t-1)$ matrix is a patterned matrix of form $(\alpha_1 I + \beta_1 E)$ where $\alpha_1 = \beta_1 = b$ and E is a matrix of all ones. It is well known that $(\alpha_1 I + \beta_1 E)^{-1} = (\alpha_2 I + \beta_2 E)$ where $\alpha_2 = \alpha_1^{-1}$, $\beta_2 = -\beta_1[\alpha_1(\alpha_1 + r\beta_1)]^{-1}$, and r is the rank of the matrix. The inverse is then

$$(X^{\delta'} X^{\delta})^{-1} = (bt)^{-1} \begin{bmatrix} 1 & 0 & 0 & 0 & 0 \\ 0 & (t-1) & -1 & -1 & -1 \\ 0 & -1 & (t-1) & -1 & -1 \\ 0 & -1 & -1 & (t-1) & -1 \\ 0 & -1 & -1 & -1 & (t-1) \end{bmatrix}$$

Multiplying as required by (5.2), we have

$$\text{tr}\{(X^{\delta'} X^{\delta})^{-1} X^{\delta'} E [PVP'] X^{\delta}\} = bt[(bt)^{-1} t \sigma_0] = t \sigma_0$$

and hence

$$E[SS_{error}] = t(b-1)\sigma_0.$$

The expectation of the treatment sum of squares shown in (5.3) requires the inverse of $(X_2^{\delta'} X_2^{\delta})$. This is also a patterned matrix of form $(btI + bE)$ which has inverse $(bt)^{-1}(tI - E)$. We again multiply the matrices to find

$$\text{tr}\{X_2^{\delta'} (X_2^{\delta'} X_2^{\delta})^{-1} X_2^{\delta'} E(PVP')\} = bt\{(t-1)(bt)^{-1}[\sigma_0 - \sigma_1 t^{-1}(2 + (t-1)^{-1})]\}$$

and thus

$$E[SS_{tr}] = (t-1)\sigma_0 - \left[\frac{(2t-1)}{t}\right]\sigma_1 + \tau_2'(X_2^{\delta'} X_2^{\delta})\tau_2.$$

This shows that the treatment sum of squares is biased by the correlation between adjacent units.

We finish the five treatment design with a look at the variance-covariance matrix of the treatment estimates. The expression for this matrix is

$$VAR(\hat{\tau} | P) = (X^{\delta'} X^{\delta})^{-1} X^{\delta'} PVP' X^{\delta} (X^{\delta'} X^{\delta})^{-1}$$

from which we get

$$VAR(\hat{\mu}) = (bt)^{-1} [\sigma_0 + \sigma_1 t^{-1} (2t - 1)]$$

$$COV(\hat{\mu}, \hat{\tau}_j) = 0 \text{ for } j = 1, 2, \dots, (t - 1)$$

$$VAR(\hat{\tau}_j) = (bt)^{-1} [\sigma_0(t - 1) - \sigma_1 t^{-1} (2t - 1)]$$

$$COV(\hat{\tau}_j, \hat{\tau}_{j'}) = (bt)^{-1} [-\sigma_0 + \sigma_1(2t - 1)(t(t - 1))^{-1}] \text{ for } j \neq j'.$$

The treatment variances and covariances are also affected by the correlation.

The expected mean squares are shown in Table 20, and we immediately note that the Type II(a) design is biased. That is, the expectation of the treatment mean square does not equal that of the error mean square under the null hypothesis. Although the estimate of experimental error is unbiased, the treatment mean square will be biased downwards for a positive correlation. This will cause a loss of power for the F-test.

5.1.2 Seven Treatments

The five treatment design is the simplest non-trivial design and serves to illustrate the block abuttal terms. We next look at the seven treatment design in order to generalize the pattern of abuttal terms for all Type II(a) designs. The primary concern will be with this pattern in E(PVP').

When there are seven treatments, three blocks are required for the Type II(a) design with $c = 1$. The analysis is quite similar to that for five treatments and two blocks. Since the matrix representations, except for E(PVP'), are easily extended to the general case, we will limit our attention to E(PVP'). The rows and columns of PVP' are still indexed by (jk), but the sequence is now

Table 20. ANOVA for Type II(a), T = 5, with Correlated Errors

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0 \left[1 - \frac{2(bt-1)}{bt(bt-1)} \rho \right] + (t-1)^{-1} \gamma'_2 (X_2^T X_2^T) \gamma_2$
Error	(r-1)(t-1)	σ_0

(11,12,13, ... ,71,72,73). The entries of PVP' still have the same form as (5.5 - 5.7) with the obvious change that $l = 1, 2, \dots, 21$ now. Aside from the size of PVP', the main difference is in the expectation of (5.7) which is the expectation of

$$\delta_{jk}^1(\delta_{j'k'}^1\sigma_0 + \delta_{j'k'}^2\sigma_1) + \sum_{l=2}^{20} \delta_{jk}^l(\delta_{j'k'}^{l-1}\sigma_1 + \delta_{j'k'}^l\sigma_0 + \delta_{j'k'}^{l+1}\sigma_1) + \delta_{jk}^{21}(\delta_{j'k'}^{20}\sigma_1 + \delta_{j'k'}^{21}\sigma_0). \quad (5.8)$$

Recall that for five treatments there was only one non-zero term in this expression, and this term arose at the abuttal of two blocks. Now there are abuttal terms where blocks one and two meet and also where the second and third blocks meet. The expectations of these abuttal terms are still $\sigma_1[t(t-1)]^{-1}$ for each term. All other entries have the same expression for their expectation as before.

Using this new $E(\text{PVP}')$, we use the same algebra as for five treatments and find that

$$E[SS_{error}] = t(b-1)\sigma_0.$$

The treatment sum of squares may now be generalized for the Type II(a) design with t treatments. If we have b blocks, there will be $2(b-1)$ entries in $E(\text{PVP}')$ with abuttal terms since there will be two entries from each of the $(b-1)$ abuttals. Combining the terms and doing the necessary algebra, it follows that

$$E[SS_{trt}] = (t-1)\sigma_0 - \left[\frac{2(bt-1)}{bt}\right]\sigma_1 + \tau_2'(X_2^{\delta'}X_2^{\delta})\tau_2.$$

This agrees with the five treatment expectation when $b=2$. The expected mean squares are shown in Table 21. The variance-covariance matrix of the treatment estimates has entries

$$\text{VAR}(\hat{\mu}) = t[b\sigma_0 + 2t^{-1}\sigma_1(bt-1)],$$

$$\text{COV}(\hat{\mu}, \hat{\tau}_j) = 0 \text{ for } j = 1, 2, \dots, (t-1),$$

$$\text{VAR}(\hat{\tau}_j) = 2(t-1)^2[b\sigma_0 - 2t^{-1}\sigma_1], \text{ and}$$

Table 21. ANOVA for Type II(a), T = 7, Correlated Errors

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0 \left[1 - \frac{2(bt-1)}{bt(t-1)} \rho \right] + (t-1)^{-1} \gamma_2' (X_2^T X_2^T)^{-1} \gamma_2$
Error	t(b-1)	σ_0

$$COV(\hat{\tau}_j, \hat{\tau}_j) = - [bt\sigma_0 + 2t^{-1}(t-2)\sigma_1] .$$

5.1.3 Summary

The analysis of variance for the Type II(a) design indicates that the design is biased in the presence of correlation among adjacent experimental units. The error mean square is an unbiased estimator of experimental error, but the treatment sum of squares is biased by the correlation between units. The bias will be downward for a positive correlation which is the usual situation; i.e., power will be reduced for a positive correlation. The amount of bias in the mean square is the correlation multiplied by

$$\left(\frac{2}{t-1}\right)\left(\frac{bt-1}{bt}\right) \leq \frac{2}{t-1}$$

which goes to zero as the number of treatments increase. We conclude from this that the Type II(a) design may be suitable when there are many treatments and low correlation.

5.2 First Differencing

The method of first differencing, as discussed previously, will now be applied to the model developed for the Type II(a) design. Model (5.1), variance-covariance matrix (4.2), and Type II(a) randomization will again form the basis of the development. Suppose then that the experimental units are in a one-dimensional sequence and follow model (5.1). The first differencing method uses a differencing matrix Δ , defined such that row i ($= 1, \dots, n$) has value zero in all columns but i , $(i+1)$ which have value 1, -1 respectively. A problem arises when the final row of the matrix is

reached and there is no unit $(i + 1)$. This is rectified by assuming that for five treatments there are eleven units with unit eleven receiving the same treatment as unit one; for seven treatments, there would be a unit twenty-two.

5.2.1 Five Treatments

There are eleven units with unit eleven having the same treatment as unit one. The differencing matrix is applied to model (5.1), resulting in

$$U_n = \Delta Y = \Delta X\tau + \Delta e = X_{\Delta}\tau + e_{\Delta} \quad (5.9)$$

where the intercept term has been removed from the parameter vector and its corresponding column removed from X . Recalling that $\text{VAR}(Y) = V$, we have

$$E(U) = \Delta X\tau$$

and

$$\text{VAR}(U) = \Delta V\Delta' = V_{\Delta}$$

where

$$V_{\Delta} = \begin{bmatrix} a & b & c & .. & 0 & 0 & 0 \\ b & a & b & .. & 0 & 0 & 0 \\ c & b & a & .. & 0 & 0 & 0 \\ 0 & c & b & .. & 0 & 0 & 0 \\ : & : & : & .. & : & : & : \\ : & : & : & .. & : & : & : \\ 0 & 0 & 0 & .. & b & c & 0 \\ 0 & 0 & 0 & .. & a & b & c \\ 0 & 0 & 0 & .. & b & a & b \\ 0 & 0 & 0 & .. & c & b & a \end{bmatrix} \quad (5.10)$$

with

$$a = 2(\sigma_0 - \sigma_1) ,$$

$$b = 2\sigma_1 = \sigma_0 , \text{ and}$$

$$c = -\sigma_1 .$$

The design matrix X has been transformed by the differencing matrix and now consists of differences of treatments. This will necessitate a new definition of the randomization matrix P.

Once again the randomization matrix P' is used to take a known arrangement of treatments and to assign them randomly to the actual experimental units in their natural sequence. In this case however, treatment differences instead of treatments are being randomly assigned. The model for the known arrangement of treatment differences is written

$$Z = PU ,$$

which upon substituting the model for U, is

$$Z = PX_{\Delta}\tau + Pe_{\Delta} = X^{\delta}_{\tau} + e^{\delta}.$$

The rows of X^{δ} are obtained from the rows of X^{δ}_0 , consisting of all pairwise differences of rows in X such that the differences of treatments are always of the form

$$(\text{Treatment } i) - (\text{Treatment } j), i < j.$$

For example when $t = 5$, we have

$$X^{\delta}_0 = \begin{bmatrix} 1 & -1 & 0 & 0 & 0 \\ 1 & 0 & -1 & 0 & 0 \\ 1 & 0 & 0 & -1 & 0 \\ 1 & 0 & 0 & 0 & -1 \\ 0 & 1 & -1 & 0 & 0 \\ 0 & 1 & 0 & -1 & 0 \\ 0 & 1 & 0 & 0 & -1 \\ 0 & 0 & 1 & -1 & 0 \\ 0 & 0 & 1 & 0 & -1 \\ 0 & 0 & 0 & 1 & -1 \end{bmatrix}$$

which consists of all pairwise differences such that $i < j$. Of course in the actual randomization, a treatment difference may be the negative of that recorded in X^{δ}_0 . Unfortunately X^{δ}_0 is not of full rank, a condition corrected by subtracting the final column from the first $(t-1)$ columns. The result is

$$X^{\delta} = \begin{bmatrix} 1 & -1 & 0 & 0 \\ 1 & 0 & -1 & 0 \\ 1 & 0 & 0 & -1 \\ 2 & 1 & 1 & 1 \\ 0 & 1 & -1 & 0 \\ 0 & 1 & 0 & -1 \\ 1 & 2 & 1 & 1 \\ 0 & 0 & 1 & -1 \\ 1 & 1 & 2 & 1 \\ 1 & 1 & 1 & 2 \end{bmatrix} .$$

Keeping these difficulties in mind, we define the randomization matrix P such that the entry in row (i,j) (= (1,1), .. ,(1,t),(2,3), .. , (2,t), ... ,(t-1,t)) and column l (= 1, ... ,bt) is δ'_{lj} . Each entry takes on values in the following manner

-1 if treatment difference (j-i) appears in difference unit l ;

1 if treatment difference (i-j) appears in difference unit l ; and

0 otherwise.

The matrix P is still orthogonal, but $E(\delta'_{lj}) = 0$ now. Some other properties of the design random variables are

$$P \{ \delta'_{lj} = 1 \} = [t(t-1)]^{-1} ,$$

$$P \{ \delta'_{lj} = -1 \} = [t(t-1)]^{-1} ,$$

$$P \{ \delta'_{lj} = 0 \} = 1 - 2[t(t-1)]^{-1} , \text{ and}$$

$$E[(\delta'_{ij})^2] = 2[t(t-1)]^{-1}.$$

The joint distribution of the design random variables can be quite difficult to obtain, especially if they reflect units that are in different blocks of the design. Since there is no known algebraic way to represent the design so as to obtain the probabilities analytically, they were obtained empirically from a complete list of designs as needed. Obviously this will prevent the generalization of these results to the case of the general design with t treatments.

We may now look at

$$SS_{error} = U' [I - P'X^\delta(X^\delta'X^\delta)^{-1}X^\delta'P] U$$

and

$$SS_{trt} = U'P'X^\delta(X^\delta'X^\delta)^{-1}X^\delta'PU$$

with

$$E[SS_{error} | P] = tr[V_\Delta - P'X^\delta(X^\delta'X^\delta)^{-1}X^\delta'PV_\Delta] \quad (5.11)$$

and

$$E[SS_{trt} | P] = tr[P'X^\delta(X^\delta'X^\delta)^{-1}X^\delta'PV_\Delta] + \tau'_2(X^\delta'X^\delta)\tau_2. \quad (5.12)$$

where $\tau_2 = (\tau_1, \dots, \tau_{t-1})'$. Once again we must determine $E(PV_\Delta P')$ in order to compute the unconditional expectations. A similar process to that in section 5.1.1 shows that a diagonal entry of $PV_\Delta P'$ is

$$\begin{aligned} & \delta_{ij}^1(\delta_{ij}^1 a + \delta_{ij}^2 b + \delta_{ij}^3 c) + \delta_{ij}^2[(\delta_{ij}^1 + \delta_{ij}^3)b + \delta_{ij}^2 a + \delta_{ij}^4 c] + \\ & \sum_{l=3}^8 \delta_{ij}^l[(\delta_{ij}^{l-1} + \delta_{ij}^{l+1})b + \delta_{ij}^l a + (\delta_{ij}^{l-2} + \delta_{ij}^{l+2})c] + \\ & \delta_{ij}^9[(\delta_{ij}^8 + \delta_{ij}^{10})b + \delta_{ij}^9 a + \delta_{ij}^7 c] + \delta_{ij}^{10}(\delta_{ij}^9 b + \delta_{ij}^{10} a + \delta_{ij}^8 c) \end{aligned}$$

where a, b, c are as defined for (5.10). Since each treatment difference occurs only once, this expression reduces to

$$E \left\{ \sum_{l=1}^{bt} (\delta_{ij}^l)^2 [2(\sigma_0 - \sigma_1)] \right\} = \frac{4bt(\sigma_0 - \sigma_1)}{t(t-1)} = \frac{4b(\sigma_0 - \sigma_1)}{t-1} .$$

This is $2(\sigma_0 - \sigma_1)$ for $t=5$.

The off-diagonal entries can be separated into four groups of which one group has zero expectation and the other groups have identical expectations apart from the sign. Expectation zero arises when no subscripts in the products match; i.e., the entry is

$$\begin{aligned} & \delta_{mn}^1 (\delta_{ij}^1 a + \delta_{ij}^2 b + \delta_{ij}^3 c) + \delta_{mn}^2 [(\delta_{ij}^1 + \delta_{ij}^3) b + \delta_{ij}^2 a + \delta_{ij}^4 c] + \\ & \sum_{l=3}^8 \delta_{mn}^l [(\delta_{ij}^{l-1} + \delta_{ij}^{l+1}) b + \delta_{ij}^l a + (\delta_{ij}^{l-2} + \delta_{ij}^{l+2}) c] + \\ & \delta_{mn}^9 [(\delta_{ij}^8 + \delta_{ij}^{10}) b + \delta_{ij}^9 a + \delta_{ij}^7 c] + \delta_{mn}^{10} (\delta_{ij}^9 b + \delta_{ij}^{10} a + \delta_{ij}^8 c) . \end{aligned}$$

All products involving $\delta_{mn}^l \delta_{ij}^l$ will be zero since only one difference occurs per difference unit. Any remaining term of the form $\delta_{mn}^l \delta_{ij}^{l+1}$ or $\delta_{mn}^l \delta_{ij}^{l-1}$ must be zero since four distinct treatments cannot fit on three original units. Remaining terms $\delta_{mn}^l \delta_{ij}^{l+2}$ and $\delta_{mn}^l \delta_{ij}^{l-2}$ are equally likely to be positive or negative and hence have expectation zero.

All the remaining entries have products involving one common subscript whose position determines whether the expectation is positive or negative. These relationships are summarized as

$$E [\delta_{ij}^l \delta_{ij}^{l+1}] = E [\delta_{ij}^l \delta_{ij}^{l-1}] = - E [\delta_{ij}^l \delta_{ij}^{l+2}] = - [t(t-1)(t-2)]^{-1} .$$

A major difficulty arises when expectations regarding terms involving difference units l and $(l+2)$ are needed. As long as both l and $(l+2)$ involve only original units from the same block, the expectation is zero; each treatment occurs only once per block. However when l and $(l+2)$ involve original units that come from different blocks, the expectations are non-zero, and the derivation of probabilities is non-trivial. The probabilities used here are obtained empirically by enumerating all

the five- and seven-treatment designs. The empirically derived expectations for entries with one common subscript are functions of

$$\frac{6\sigma_0 - 11\sigma_1}{20} . \quad (5.13)$$

Thus we have the expectation for all entries of $PV_{\Delta}P'$. Rows and columns of the matrix are indexed by (i,j) in the manner ((1,1), ... , (1,t),(2,3), ... , (2,t), ... , (t-1,t)). The diagonals have value $2(\sigma_0 - \sigma_1)$, entries with no common subscript are zero, and all other entries are (5.13) which is positive if the matching subscripts are in the same position, negative otherwise.

Now it is possible to obtain the unconditional expectations of (5.11) and (5.12). Note that $(X^{\delta}X^{\delta}) = tI + tE$ which has inverse $t^{-2}(tI - E)$. This implies that

$$E[SS_{error}] = 2bt(\sigma_0 - \sigma_1) - t^{-1}[8t(\sigma_0 - \sigma_1) + 12t(6\sigma_0 - 11\sigma_1)(.05)]$$

or equivalently

$$E[SS_{error}] = \left[\frac{42 - 27\rho}{5} \right] \sigma_0 \quad \text{where } \rho = \frac{\sigma_1}{\sigma_0} .$$

From (5.12) it is seen that

$$E[SS_{trt}] = \left[\frac{58 - 73\rho}{5} \right] \sigma_0 + \tau'_2(X^{\delta'}X^{\delta})\tau_2 .$$

Expected mean squares are found in Table 22.

The variance-covariance matrix for $\hat{\tau}$ is

$$VAR(\hat{\tau}) = E[(X^{\delta'}X^{\delta})^{-1}X^{\delta'}PV_{\Delta}P'X^{\delta}(X^{\delta'}X^{\delta})^{-1}] .$$

The diagonal elements of this matrix are $4(2d + 3a)$ and the off-diagonals are $-(2d + 3a)$ where $d = (\sigma_0 - \sigma_1)$ and $a = (6\sigma_0 - 11\sigma_1) \div 20$.

The analysis results in a biased estimate of error and a biased test for treatments. If $6\sigma_0 - 11\sigma_1$ is zero, then all the off-diagonals of $E[PV_{\Delta}P']$ vanish and we would have an unbiased

Table 22. ANOVA for Type II(a), T = 5, with First Differencing

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0[2.90 - 3.65\rho] + (t - 1)^{-1}\gamma'(X^*X^*)\gamma$
Error	t(b-1)	$\sigma_0[1.68 - 1.08\rho]$

estimate of error. This is true for $\rho = 6 \div 11 = .5454\dots$. Looking back at V_{Δ} , the correlation between adjacent difference units is $2\sigma_1 - \sigma_0$ which is zero for $\rho = 0.50$. Thus this design and analysis should work well when the correlation between adjacent units is approximately a half.

5.2.2 Seven Treatments

Extension of first differencing to seven treatments is straight-forward with the exception of determining the expectations of $\delta'_{ij} \delta'_{ij}$. In particular this is a problem when one subscript matches and $l, (l + 2)$ involve units across block boundaries. A complete enumeration of the seven treatment designs provided empirical probabilities for these quantities.

The design matrix X^{δ} now is of dimension 21×6 , consisting of the 21 possible treatment differences, and although the size of P increases, the definition of its entries is similar to that for $t = 5$. Diagonal entries still have expectation $\frac{4b(\sigma_0 - \sigma_1)}{t - 1}$, entries with no common subscript have expectation zero, and the equivalent expression to (5.13) for entries with one common subscript is

$$\frac{40\sigma_0 - (5218 \div 68)\sigma_1}{210} .$$

There are now three blocks and two block boundaries to worry about, but otherwise the entries of $PV_{\Delta}P'$ have the same form as before.

The sums of squares are found to have

$$E[SS_{error}] = \sigma_0 \left[\frac{170}{7} - \left(\frac{9062}{476} \right) \rho \right]$$

and

$$E[SS_{trt}] = \sigma_0 \left[\frac{44}{7} - \left(\frac{494}{476} \right) \rho \right] + \tau'_2 (X^{\delta'} X^{\delta}) \tau_2 .$$

The expected mean squares given are in Table 23.

Table 23. ANOVA for Type II(a), T = 7, with First Differencing

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0[1.05 - 0.17\rho] + (t - 1)^{-1}\gamma'(X^0 X^0)\gamma$
Error	t(b-1)	$\sigma_0[1.73 - 1.36\rho]$

The variance-covariance matrix of $\hat{\tau}$ has diagonal entries $6ct^{-2}$ and off-diagonal $-ct^{-2}$ where

$$c = 2(\sigma_0 - \sigma_1) + 5\left[\frac{40\sigma_0 - (5218 + 68)\sigma_1}{210}\right].$$

This time the off-diagonal elements of $E[PV_{\Delta}P']$ will be zero if $40\sigma_0 - (5218 + 68)\sigma_1$ is zero. The solution to this equation is $\rho = .5212$. Thus again this combination of a Type II(a) design and first differencing works best when the correlation is approximately 0.5.

5.2.3 Summary

A combination of a Type II(a) design and first differencing does not provide either an unbiased design or an unbiased estimate of error in general. It would appear that the design provides an approximately unbiased test and error estimate when the correlation between adjacent units is about 0.5. A statement about the general case with t treatments is not possible until some method of obtaining the joint distribution of the design random variables for all t is discovered. Otherwise an empirical determination must be made for each t , a daunting task considering the large number of possible designs for $t > 7$.

5.3 *The Addition of Border Shifts*

Thus far in Chapter V we have looked only at correlated observations which might have resulted from trends in the data and some techniques recommended to cope with this situation. As a further complicating factor, now suppose that border shifts as well as correlated observations are present. What effect will this have on the expected mean squares for the designs and analyses previously considered in this chapter? The next three sections will deal in turn with the RCBD as

discussed in Section 4.4, the Type II(a) design from Section 5.1, and the first differencing procedure from Section 5.2. The expected mean squares will be obtained for the ANOVA in each case. We first look at the RCBD.

5.3.1 Randomized Complete Block Design

From Section 4.4 model (4.1) for the RCBD and its matrix equivalent is now written as

$$Y = X\gamma + e + c$$

where $c = (c_{11}, 0, \dots, 0, c_{t1}, \dots, c_{1b}, 0, \dots, 0, c_{tb})'$. The variance-covariance structure is again (4.2) and P is as described by Aastveit (1983). Multiplication by the randomization matrix P results in

$$Z = PY = PX\gamma + Pe + Pc$$

which is rewritten as

$$Z = X^\delta \gamma + e^\delta + c^\delta .$$

We now look at the expectation of the sums of squares and the variance of the treatment estimate vector.

Beginning with error, we know that

$$E[SS_{error} | P] = tr\{V\} - tr\{X^\delta (X^{\delta'} X^\delta)^{-1} X^{\delta'} PVP\} + (X\gamma + c)'(I - X(X'X)^{-1}X')(X\gamma + c) .$$

Since the first two terms were obtained by Aastveit, we look only at

$$(X\gamma + c)'(I - X(X'X)^{-1}X')(X\gamma + c) = c'c - c'P'X^\delta (X^{\delta'} X^\delta)^{-1} X^{\delta'} Pc .$$

The first term is $\sum_{j=1}^b (c_{1j}^2 + c_{tj}^2)$ and the second term is a quadratic form, $Q'AQ$, where $Q = Pc$. From linear model theory we know that $E[Q'AQ] = tr(AV_Q) + E(Q)'AE(Q)$ where $V_Q = E[(Q - E(Q))(Q - E(Q))']$. Note that

$$E(Q) = \begin{bmatrix} t^{-1}(c_{11} + c_{t1}) \\ \vdots \\ t^{-1}(c_{11} + c_{t1}) \\ \vdots \\ t^{-1}(c_{1b} + c_{tb}) \\ \vdots \\ t^{-1}(c_{1b} + c_{tb}) \end{bmatrix}$$

is a $bt \times 1$ vector, as is

$$Q - E(Q) = \begin{bmatrix} c_{11}(\delta_{11}^1 - t^{-1}) + c_{t1}(\delta_{t1}^1 - t^{-1}) \\ \vdots \\ c_{11}(\delta_{11}^1 - t^{-1}) + c_{t1}(\delta_{t1}^1 - t^{-1}) \\ \vdots \\ c_{1b}(\delta_{11}^b - t^{-1}) + c_{tb}(\delta_{t1}^b - t^{-1}) \\ \vdots \\ c_{1b}(\delta_{11}^b - t^{-1}) + c_{tb}(\delta_{t1}^b - t^{-1}) \end{bmatrix}$$

Thus $V_Q = E[(Q - E(Q))(Q - E(Q))']$ is a block diagonal matrix

$$V_Q = \begin{bmatrix} V_1 & 0 & \dots & 0 \\ 0 & V_2 & \dots & 0 \\ \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & \dots & V_b \end{bmatrix}$$

where V_j is a $t \times t$ matrix with diagonal elements

$$t^{-1}(c_{1j}^2 + c_{tj}^2) - t^{-2}(c_{1j} + c_{tj})^2$$

and off-diagonal elements

$$[t(t-1)]^{-1}2c_{1j}c_{tj} - t^{-2}(c_{1j} + c_{tj})^2 .$$

Thus we have

$$\text{tr}\{X^\delta(X^\delta'X^\delta)^{-1}X^\delta'V_Q\} = b^{-1}\sum_{j=1}^b [(c_{1j}^2 + c_{tj}^2) - t^{-1}(c_{1j} + c_{tj})^2]$$

and

$$E(Q)'X^\delta(X^\delta'X^\delta)^{-1}X^\delta'E(Q) = t^{-1}\sum_{j=1}^t (c_{1j} + c_{tj})^2 .$$

Combining terms, we have

$$E[SS_{error}] = (b-1)(t-1)[1 - 2t^{-1}\rho]\sigma_0 + \varphi(c_{ij})$$

where

$$\varphi(c_{ij}) = b^{-1}(b-1)\sum_{j=1}^b [(c_{1j}^2 + c_{tj}^2) - t^{-1}(c_{1j} + c_{tj})^2] .$$

The treatment sum of squares has conditional expectation

$$E[SS_{\tau\tau} | P] = tr\{X_2^\delta(X_2^\delta X_2^\delta)^{-1} X_2^\delta PVP'\} + (X_2\gamma_2 + c)' X_2(X_2 X_2)^{-1} X_2'(X_2\gamma_2 + c)$$

for which the additional terms are

$$2c'P'X_2^\delta\gamma_2 + c'P'X_2^\delta(X_2^\delta X_2^\delta)^{-1} X_2^\delta Pc .$$

Restrictions on the columns of X_2 force the first term from above to be zero. Using the same quadratic form as for the error sum of squares, we see that

$$c'P'X_2^\delta(X_2^\delta X_2^\delta)^{-1} X_2^\delta Pc = b^{-1} \sum_{j=1}^b [(c_{1j}^2 + c_{2j}^2) - t^{-1}(c_{1j} + c_{2j})^2] \quad (5.15)$$

and that

$$E[SS_{\tau\tau}] = [1 - 2t^{-1}\rho]\sigma_0 + \varphi(c_{ij}) + \gamma_2'(X_2^\delta X_2^\delta)\gamma_2$$

where $\varphi(c_{ij})$ is defined by (5.15).

The expected mean squares are shown in Table 24. We see that the border shift further biases the estimate of experimental error, but does not bias the F-ratio. As with the original RCBD, we can expect a loss of power in the detection of treatment differences.

5.3.2 Type II(a) Designs

Model (5.1) from Section 5.1 will have shifts added to the first and last units of the design.

The new model is

$$Y = X\tau + e + c$$

where $c = (c_1, 0, \dots, 0, c_b)'$. This is transformed by the randomization matrix P to

$$Z = PX\tau + Pe + Pc = X^\delta\tau + e^\delta + c^\delta .$$

Table 24. ANOVA for RCBD with Correlation and Border Shifts

Source	DF	Expected Mean Square
Treatment	t-1	$[1 - 2t^{-1}\rho]\sigma_0 + \varphi(c_{ij}) + (t - 1)^{-1}\gamma_2'(X_2^R X_2^R)\gamma_2$
Error	(t-1)(t-1)	$[1 - 2t^{-1}\rho]\sigma_0 + \varphi(c_{ij})$

where $\varphi(c_{ij}) = [b^2(t - 1)]^{-1} \sum_{j=1}^b [(c_{ij}^2 + c_{ij}^2) - t^{-1}(c_{i.} + c_{.j})^2]$.

The observations Y have variance-covariance structure (4.2) and $\text{VAR}(Z) = PVP'$.

Once again we have

$$E[SS_{error} | P] = \text{tr}\{V\} - \text{tr}\{X^\delta (X^{\delta'} X^\delta)^{-1} X^{\delta'} PVP'\} + E(Y)'(I - X(X'X)^{-1}X')E(Y)$$

for which we know the unconditional expectation of the first two terms. The final term is

$$E(Y)'(I - X(X'X)^{-1}X')E(Y) = (X\tau + c)'[I - X(X'X)^{-1}X'](X\tau + c)$$

which reduces to

$$c'c - c'P'X^\delta (X^{\delta'} X^\delta)^{-1} X^{\delta'} Pc .$$

Note that $c'c = (c_1^2 + c_{10}^2)$ and that the second term has quadratic form $Q'Q'$ where $Q = Pc$ is a $bt \times 1$ vector. The expectation of Q is

$$E(Q) = 5^{-1} \begin{bmatrix} c_1 \\ c_{10} \\ c_1 \\ : \\ : \\ c_{10} \\ c_1 \\ c_{10} \end{bmatrix} \quad \text{or} \quad 7^{-1} \begin{bmatrix} c_1 \\ 0 \\ c_{21} \\ : \\ : \\ c_1 \\ 0 \\ c_{21} \end{bmatrix}$$

for $t=5$ or $t=7$ respectively. Random vector Q has variance $V_Q = E[(Q - E(Q))(Q - E(Q))']$ where

$$Q - E(Q) = \begin{bmatrix} c_1(\delta_{11}^1 - 5^{-1}) \\ c_{10}(\delta_{12}^{10} - 5^{-1}) \\ : \\ : \\ : \\ c_1(\delta_{51}^1 - 5^{-1}) \\ c_{10}(\delta_{52}^{10} - 5^{-1}) \end{bmatrix} \quad \text{or} \quad \begin{bmatrix} c_1(\delta_{11}^1 - 7^{-1}) \\ 0 \\ c_{21}(\delta_{13}^{21} - 7^{-1}) \\ : \\ : \\ c_1(\delta_{71}^1 - 7^{-1}) \\ 0 \\ c_{21}(\delta_{73}^{21} - 7^{-1}) \end{bmatrix}$$

for $t = 5$ or $t = 7$ respectively.

The variance matrices for Q have the general form

$$V_Q = \begin{bmatrix} A & B & .. & B & B \\ B & A & .. & B & B \\ : & : & : & : & : \\ B & B & .. & A & B \\ B & B & .. & B & A \end{bmatrix} .$$

For $t = 5$

$$A = 5^{-2} \begin{bmatrix} c_1^2(5-1) & -c_1 c_{10} \\ -c_1 c_{10} & c_{10}^2(5-1) \end{bmatrix}$$

and

$$B = 5^{-2} \begin{bmatrix} -c_1^2 & c_1 c_{10}(5-1)^{-1} \\ c_1 c_{10}(5-1)^{-1} & -c_{10}^2 \end{bmatrix} .$$

For $t = 7$

$$A = 7^{-2} \begin{bmatrix} c_1^2(7-1) & 0 & -c_1 c_{21} \\ 0 & 0 & 0 \\ -c_1 c_{21} & 0 & -c_{21}^2(7-1) \end{bmatrix}$$

and

$$B = 7^{-2} \begin{bmatrix} -c_1^2 & 0 & c_1 c_{21}(7-1)^{-1} \\ 0 & 0 & 0 \\ c_1 c_{21}(7-1)^{-1} & 0 & -c_{21}^2 \end{bmatrix}.$$

Letting $A = X^{\delta}(X^{\delta} X^{\delta})^{-1} X^{\delta}$,

$$E[Q'AQ] = \text{tr}\{X^{\delta}(X^{\delta} X^{\delta})^{-1} X^{\delta} V_Q\} + E(Q)' X^{\delta}(X^{\delta} X^{\delta})^{-1} X^{\delta} E(Q)$$

which is

$$E[Q'AQ] = b^{-1}[(c_1^2 + c_{bt}^2) - t^{-1}(c_1 + c_{bt})^2] + (bt)^{-1}(c_1 + c_{bt})^2.$$

Combining with the previous work in Section 5.1, we have

$$E[SS_{error}] = t(b-1)\sigma_0 + b^{-1}(b-1)(c_1^2 + c_{bt}^2).$$

Thus the estimate of experimental error will be biased upwards by the shifts.

Turning next to treatments, we have

$$E[SS_{trt} | P] = \text{tr}\{X_2^{\delta}(X_2^{\delta} X_2^{\delta})^{-1} X_2^{\delta} PVP'\} + \tau_2'(X_2^{\delta} X_2^{\delta})\tau_2 + Q' X_2^{\delta}(X_2^{\delta} X_2^{\delta})^{-1} X_2^{\delta} Q$$

for which we know the unconditional expectation of the first term. The unconditional expectation of the last term is obtained in the same way as for the error sum of squares, except that A is now $X_2^{\delta}(X_2^{\delta} X_2^{\delta})^{-1} X_2^{\delta}$. We find that

$$E[Q'AQ] = b^{-1}[(c_1^2 + c_{bt}^2) - t^{-1}(c_1 + c_{bt})^2] \quad (5.16)$$

and that

$$E[SS_{tr}] = (t-1)\left[1 - \left(\frac{2(bt-1)}{bt(t-1)}\right)\rho\right]\sigma_0 + \tau'_2(X_2^{\delta'}X_2^\delta)\tau_2 + \varphi(c_{ij})$$

where $\varphi(c_{ij})$ is defined by (5.16). The expected mean squares are found in Table 25. For the Type II(a) design, the table indicates that border shifts increase the bias in the F-ratio.

5.3.3 First Differencing

Model (5.9) from Section 5.2 will be augmented such that

$$U_n = X_\Delta \tau + e_\Delta + \Delta c$$

where $c = (c_1, 0, \dots, 0, c_{bt+1})'$ and we denote Δc as $c_\Delta = (c_1, 0, \dots, 0, -c_{bt+1})'$ which is an $bt \times 1$ vector. Other than the additional shift vector, everything in the model has its original meaning. The randomization matrix P maps the observed difference units to

$$Z = PU = X^\delta \tau + e^\delta + c^\delta$$

where $X^\delta = PX_\Delta$, $e^\delta = Pe_\Delta$, and $c^\delta = Pc_\Delta$.

The error sum of squares has

$$E[SS_{error}] = \text{tr}(V_\Delta) - \text{tr}\{X^\delta(X^{\delta'}X^\delta)^{-1}X^{\delta'}PV_\Delta P'\} + E(U)'[I - P'X^\delta(X^{\delta'}X^\delta)^{-1}X^\delta P]E(U)$$

where we already know the unconditional expectation of the initial term from Section 5.2. The second term is

$$E(U)'[I - P'X^\delta(X^{\delta'}X^\delta)^{-1}X^\delta P]E(U) = E[c'_\Delta c_\Delta - c'_\Delta P'X^\delta(X^{\delta'}X^\delta)^{-1}X^\delta Pc_\Delta]$$

Note that $c'_\Delta c_\Delta = (c_1^2 + c_{bt+1}^2)$ and that we again have a quadratic form in $Q'AQ$ where $Q = Pc_\Delta$. The expectation of Q is the null vector. Thus the variance-covariance matrix of Q is merely

Table 25. ANOVA for Type II(a) with Correlation and Border Shifts

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0 \left[1 - \frac{2(bt-1)}{bt(t-1)} \rho \right] + \varphi(c_{ij}) + (t-1)^{-1} \gamma'_2 (X_2' X_2^0) \gamma_2$
Error	t(b-1)	$\sigma_0 + (bt)^{-1} (c_1^2 + c_{bt}^2)$

where $\varphi(c_{ij}) = [b(t-1)]^{-1} [(c_1^2 + c_{bt}^2) - t^{-1} (c_1 + c_{bt})^2]$.

$$V_Q = E[(Q - E(Q))(Q - E(Q))'] = E[QQ'] .$$

If we index the rows and columns of V_Q by (i,j) in the sequence $((1,2), \dots, (1,t), (2,3), \dots, (2,t), \dots, (t-1,t))$, the entries $(i,j) \times (i',j')$ of V_Q may be characterized as follows

$$2[t(t-1)]^{-1}(c_1^2 + c_{bt+1}^2) \text{ for } i = i', j = j' ;$$

$$2[t(t-1)]^{-1}c_1 c_{bt+1} \text{ for } (i = i', j \neq j') \mid (i \neq i', j = j') ;$$

$$-2[t(t-1)]^{-1}c_1 c_{bt+1} \text{ for } (i \neq i', j' = i) ; \text{ and}$$

$$0 \text{ for } i \neq i', j \neq j' .$$

It is now possible to obtain unconditional expectations such as

$$E[Q'AQ] = t^{-1}[2(c_1^2 + c_{bt+1}^2) + 2(t-2)c_1 c_{bt+1}] .$$

Combining terms we have

$$E[SS_{error}] = \left[-\frac{170}{7} - \frac{9062}{476}\rho\right]\sigma_0 + \frac{t-2}{t}(c_1 - c_{bt+1})^2$$

for $t=7$. We see that if $c_1 = c_{bt+1}$, then the estimate of experimental error suffers no additional bias due to border shifts.

We also find that

$$E[SS_{trt} | P] = tr\{X^\delta(X^{\delta'}X^\delta)^{-1}X^{\delta'}PV_\Delta P'\} + \tau'_2(X^{\delta'}X^\delta)\tau_2 + c'_\Delta P'X^\delta(X^{\delta'}X^\delta)^{-1}X^{\delta'}Pc_\Delta$$

where the first two terms have been examined previously. The unconditional expectation of the third expression was obtained for the error sum of squares and is

$$E[c'_\Delta P'X^\delta(X^{\delta'}X^\delta)^{-1}X^{\delta'}Pc_\Delta] = 2t^{-1}(c_1 - c_{bt+1})^2 + 2c_1 c_{bt+1} .$$

Table 26. ANOVA for Type II(a), T = 5, First Differencing, Both Violations Shifts Are Present

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0[2.90 - 3.65\rho] + \varphi_2(c_i) + (t - 1)^{-1}\gamma'(X^b X^b)\gamma$
Error	t(b-1)	$\sigma_0[1.68 - 1.08\rho] + \varphi_1(c_i)$

where

$$\varphi_1(c_i) = \frac{t-2}{t^2(b-1)}(c_1 - c_{bt+1})^2$$

$$\varphi_2(c_i) = \frac{2}{t(t-1)}(c_1 - c_{bt+1})^2 + 2(t-1)^{-1}c_1 c_{bt+1}.$$

Table 27. ANOVA for Type II(a), T = 7, First Differencing, Both Violations

Source	DF	Expected Mean Square
Treatment	t-1	$\sigma_0[1.05 - 0.17\rho] + \varphi_2(c_i) + (t - 1)^{-1}\gamma'(X^y X^y)\gamma$
Error	t(b-1)	$\sigma_0[1.73 - 1.36\rho] + \varphi_1(c_i)$

where

$$\varphi_1(c_i) = \frac{t-2}{t^2(b-1)}(c_1 - c_{bt+1})^2$$

$$\varphi_2(c_i) = \frac{2}{t(t-1)}(c_1 - c_{bt+1})^2 + 2(t-1)^{-1}c_1 c_{bt+1}.$$

If the two shifts are equal, the term is merely $2c^2$. The expected mean squares are found in Tables 26 and 27 for five and seven treatments respectively. Comparing mean squares, we see that the F-ratio is further biased by the border shifts. The estimate of experimental error will be unbiased for equal border shifts, but the treatment mean square will remain biased upwards in this situation by $2(t-1)^{-1}c^2$. As discussed in Section 5.2, a statement about the general experiment with t treatments must await a general determination of the joint distribution of the design random variables.

5.4 Chapter Summary

Following the analytic lead of Aastveit, it has been demonstrated that the Type II(a) design is a biased design when adjacent observations are correlated. The estimate of experimental error is unbiased, but the treatment mean square is biased by the correlation of the units. For a positive correlation, a downward bias will result in a loss of power for the testing of treatment differences. This bias decreases as the number of treatments increases.

The method of first differencing does not help the situation in general. A first differencing analysis in conjunction with a Type II(a) design provides a biased test and a biased estimate of error. If the correlation between units is approximately a half, the method will provide both an approximately unbiased test and an approximately unbiased error estimate.

Border shifts bias the F-ratios for both analytic procedures. The usual analysis using the Type II(a) design has a biased estimate of error as well. If the border shifts are identical, the first differencing procedure will provide an estimate of error without extra bias; however, the treatment mean square is biased upwards.

Chapter VI

VI. Summary and Future Research

Violations of the analysis of variance assumptions have been a concern of statisticians ever since the analysis of variance was conceived. In many respects the growth of experimental design has been driven by the need to insure the validity of experiments in the face of assumption violations. The interplay between statistical analysis and design corresponds to the interactive nature of the scientific method. That is, the scientist designs an experiment so as to test hypotheses with a minimum of environmental variability, and then once the experiment is finished, the scientist re-designs on the basis of the new knowledge.

Our concern has been with systematic response shifts in the borders of the randomized complete block and the split plot designs as well as the problem of correlated experimental units. Both of these problems constitute a violation of the assumptions of the analysis or a mis-specification of the statistical model. The statistical literature is replete with alternative analyses to detect the occasional response shift and to adjust for the presence of correlation. However not much has been done to design from the beginning with these problems in mind. The emphasis in this research has been the examination of alternative designs which can provide reasonable results in the presence of border shifts and/or correlation.

Randomization theory was the analytic tool used to compare designs. This approach was chosen for several reasons. First, randomization theory requires a minimum number of assumptions about the nature of the data. Second, the nature of each design is more fully revealed by exploring the effects of its unique randomization scheme on the expectations of estimates. In some sense, the concern here is with the ability of alternative randomization schemes to cope with assumption violations. As a final point, the near universal use of randomization in the agricultural sciences demands that its impact be explored.

6.1 Summary of Border Shifts

The effects of border shifts on the randomized complete block design (RCBD) and the split plot design generally were a decrease in power and bias in estimation. Turning first to the RCBD, the shift adds an extra variance term and a cross-product term to all the mean squares of the analysis of variance. Although the F-test for treatments remains unbiased in the sense that the expected mean squares for treatments and error are identical when the null hypothesis is true, the estimate of experimental error is biased upwards. The estimate of treatment contrasts is unbiased. Combining unbiased treatment contrasts with an upwardly biased error estimate results in a loss of power.

Similar results appear in a split plot experiment with border shifts in the first and last subplots of each whole plot. The whole plot treatment and whole plot error mean squares are unbiased, and the whole plot F-test is unbiased. This conclusion holds as long as the border shifts are equivalent for each whole plot in a replicate. Despite the lack of bias in the whole plots, the subplot treatment and interaction mean squares, as well as residual error mean squares are biased. Once again the F-tests are unbiased but a loss in power is expected. This is unfortunate since better precision is generally expected for the subplot comparisons.

Results indicate that if power and unbiased estimates of experimental error are important, the RCBD and split plot do not perform well when border shifts are present; and of course power and error estimates are always important. What are some alternatives? The obvious tactic with border shifts is to try alternative blocking schemes that will block out the shifts. The Latin square, semi-Latin square, and two-column designs were examined as alternatives to the RCBD. A Latin square type arrangement was tried as an alternative to the split plot.

The Latin square is the ideal alternative to the RCBD when border shifts are present. Error is estimated unbiasedly and the F-test for treatments is unbiased even if the shifts are different in the two borders. Unfortunately the Latin square requires t^2 experimental units which may be unavailable. If it is possible to assume that the shift is identical in the two borders, there are two alternative designs which require fewer experimental units.

One alternative is the semi-Latin square which also has each treatment occurring only once per row and column. However for this design, the number of treatments is a multiple of the number of rows and hence each row-column combination has more than one experimental unit. For our purposes the border will constitute one column of the design. This is a biased design, but for a large border effect, it provides an improved error estimate and higher power.

The two-column design has only two columns of course. One column contains the border effect where each treatment occurs once. An interior column contains each treatment an equal number of times. This also a biased design, and like the semi-Latin square, it can provide a better error estimate and higher power when large border effects are present. Since some degrees of freedom are saved for error with this design, there is a marginal increase in power over the semi-Latin square.

An alternative to the split plot is a Latin square type arrangement. The whole plot treatments are assigned as usual to the whole plots, but the subplot treatments are randomized as in a Latin square. Rows of the Latin square are the whole plots and columns are subplot positions within the whole plots. This provides an unbiased design for main effects, but the whole plot by subplot treatment interaction is confounded with columns. Extension of the semi-Latin and two-column designs to this case should be straight-forward and should encounter similar difficulties as before.

6.2 Summary of Correlated Observations

Border shifts can be viewed as an abrupt manifestation of gradual response shifts or trends. Trends can be modelled as correlation, the final topic discussed here. The impact of adjacent correlated experimental units was examined from a design randomization perspective. Since the effect on the RCBD had been previously explored, the efficacy of the Type II(a) design was investigated. The Type II(a) design randomizes such that each treatment occurs next to every other treatment, except itself, exactly c times. We took c to be one. Two types of analyses were used in conjunction with the Type II(a) design, the usual analysis of variance and first differencing.

The usual analysis of variance used with the Type II(a) design provides an unbiased estimate of error, but the treatment mean square is biased. For a positive correlation the bias is downward, leading to lower power for the F-test. The impact of this bias decreases with an increased number of treatments. First differencing, on the other hand, results in a biased test and a biased estimate of error. The bias will be minimal when correlation between adjacent units is approximately a half.

Border shifts were added to the correlated data case, and it was seen that the shifts further bias both analyses used with the Type II(a) design. Should the shifts be identical in the two borders, the estimate of error with first differencing is not biased any further by the shifts; however, the treatment mean square is biased upwards.

6.3 Further Research

It has been demonstrated here that border shifts and correlation do adversely affect many of the designs and analyses which are normally used. This is not surprising since shifts and correlation reflect model and assumption violations. Other than the Latin square for border shifts, the alternatives examined here are not encouraging. What can and should be done at this point?

In terms of border effects, additional work needs to be done with the semi-Latin square and two-column designs. The simulation work done here did not add row-column interactions to the random variates. This caused the semi-Latin and two-column designs to look better than they actually are. Solutions would be to run additional simulations with the interaction, or possibly better, to run simulations on uniformity trial data.

Only the usual analysis of variance procedures were used on the border shifts, but alternative analyses suggest themselves. Randomization analyses using medians or other robust measures might provide better results than the usual analysis. Use of the median would be quite resistant to the shifts. Another possibility is the use of rank statistics rather than the actual data values.

A possibility in terms of alternate designs might be the use of incomplete block structures. Perhaps information on some treatments could be sacrificed in order to obtain some treatment comparisons and estimates free of an outlier effect. This might be a possibility for forming the columns of a bi-directional blocking scheme.

Turning to the correlated experimental units, it has been demonstrated that the Type II(a) design is a biased design under the usual analysis of variance. A look at the Type III designs might be useful for a couple of reasons. One reason is that the Type III design might result in lower bias in the treatment mean square. The other possibility is that the Type III design should be able to cope with a more complicated error structure than that examined here.

On a more theoretical level, the problem of counting and describing the geometry of the Type II(a) design is generally unexplored. Graph theory provides a concise explanation of the Type II(a) design, but some basic probabilities for the design random variables seem to be unsolved for the general case.

A final, broad area of future research is a systematic examination of the remaining nearest neighbour analyses. The correlation model appears to be reasonable although the formulation provided here is probably too simplistic. Lack of a randomization theory, generally cited as a problem in the field of nearest neighbours, can be partially addressed by this general approach.

Chapter VII

Bibliography

- Aastveit, A. H. (1983). "On the effect of correlation between plots in randomized block experiments", *Biom. J.*, 25, 129-153.
- Addelman, S. (1970). "Variability of treatments and experimental units in the design and analysis of experiments", *J. Amer. Statist. Assoc.*, 65, 1095-1108.
- Airy, G. B. (1861). *On the Algebraical and Numerical Theory of Errors of Observations and the Combination of Observations*, Cambridge, London: Macmillan and Co., Ltd.
- Atiqullah, M. and Cox, D. R. (1962). "The use of control observations as an alternative to incomplete block designs", *Biometrika*, 49, 464-471.
- Atkinson, A. C. (1969). "The use of residuals as a concomitant variable", *Biometrika*, 56, 33-41.
- Bartlett, M. S. (1938). "The approximate recovery of information from replicated field experiments with large blocks", *J. Agric. Sci.*, 28, 418-427.
- Bartlett, M. S. (1968). "A further note on nearest neighbour models", *J. R. Statist. Soc. A*, 131, 579-580.
- Bartlett, M. S. (1971). "Physical nearest-neighbour models and non-linear time series", *J. Appl. Prob.*, 8, 222-232.
- Bartlett, M. S. (1971). "Two-dimensional nearest-neighbour systems and their ecological applications", *Statistical Ecology*, Vol. 1, 179-194, Pennsylvania State University Press.
- Bartlett, M. S. (1975). *The Statistical Analysis of Spatial Pattern*, Andover: Chapman and Hall.
- Bartlett, M. S. (1978). "Nearest neighbour models in the analysis of field experiments", *J. R. Statist. Soc. B*, 40, 147-174.
- Bartlett, M. S. (1981). "A further note on the use of neighbouring plot values in the analysis of field experiments", *J. R. Statist. Soc. B*, 43, 100-102.
- Bartlett, M. S. (1984). "Recent investigations involving stochastic population models", *Adv. Appl. Prob.*, 16, 449-470.

- Bartlett, M. S. and Besag, J. E. (1969). "Correlation properties of some nearest-neighbour models", *Bull. Statist. Inst.*, 43, Book 2, 191-193. Beckman, R.J. and Cook, R.D. (1983). "Outlierss" *Technometrics*, 25, 119-149.
- Bellhouse, D. R. (1984). "Optimal randomization for experiments in which autocorrelation is present", *Biometrika*, 71, 155-160.
- Bellhouse, D. R. (1986). "Randomization in the analysis of covariance", *Biometrika*, 73, 207-211.
- Berenblut, I. I. and Webb, G. I. (1974). "Experimental design in the presence of autocorrelated errors", *Biometrika*, 61, 427-437.
- Besag, J. E. (1972). "Nearest-neighbour systems and the auto-logistic model for binary data", *J. R. Statist. Soc. B*, 34, 75-83.
- Besag, J. E. (1974). "Spatial interaction and the statistical analysis of lattice systems (with Discussion)", *J. R. Statist. Soc. B*, 36, 192-236.
- Besag, J. E. (1977). "Error in variables estimation for Gaussian lattice schemes", *J. R. Statist. Soc. B*, 39, 73-78.
- Besag, J. E. (1978). "Contribution to Discussion of Bartlett (1978)", *J. R. Statist. Soc. B*, 40, 165-166.
- Besag, J. E. (1983). "Contribution to Discussion of Wilkinson et al. (1983)", *J. R. Statist. Soc. B*, 45, 180-183.
- Besag, J. E. (1984). "A method for analysis of field experiments based on first differences, including a GENSTAT macro for its implementation (RAK)", *Proc. Biometric Society Workshop*, 9-14.
- Besag, J. E. (1984). "Randomisation assessment of five nearest neighbour methods", *Proc. Biometric Society Workshop*, 44-46.
- Besag, J. E. and Kempton, R. A. (1984). "Spatial methods in the analysis of agricultural field trials", *Proc. XIIIth International Biometric Conference*, 80-88.
- Besag, J. E. and Kempton, R. A. (1986). "Statistical analysis of field experiments using neighbouring plots", *Biometrics*, 42, 231-251.
- Box, G. E. P. (1976). "Science and statistics", *J. Amer. Statist. Assoc.*, 71, 791-799.
- Box, G. E. P. and Draper, N. R. (1982). "Robust designs", *Biometrika*, 62, 347-352.
- Bradu, D. and Hawkins, D. M. (1982). "Location of multiple outliers in two-way tables, using tetrads", *Technometrics*, 24, 103-107.
- Brook, D. (1964). "On the distinction between the conditional probability and the joint probability approaches in the specification of nearest-neighbor systems", *Biometrika*, 51, 481-483.
- Butcher, J. C. (1956). "Treatment variances for experimental designs with serially correlated observations", *Biometrika*, 43, 208-212.
- Cannell, M. G. R., Njuguna, C. K., Ford, E. D., Smith, R. and Ross-Parker, H. M. (1977). "Variation in yield among competing individuals within mixed genotype stands of tea: A selection problem", *Journal of Applied Ecology*, 14, 969-985.

- Chauvenet, W. (1863). *A Manual of Spherical and Practical Astronomy*, 2 vols., Philadelphia: J. B. Lippincott Company.
- Chin, K. M., Wolfe, M. S. and Minchin, P. N. (1984). "Host-mediated interactions between pathogen genotypes", *Plant Pathology*, 33, 161-171.
- Cliff, A. D. and Ord, J. K. (1981). *Spatial Processes*, London: Pion.
- Cochran, W. G. (1947). "Some consequences when the assumptions of the analysis of variance are not satisfied", *Biometrics*, 3, 22-38.
- Cox, D. R. (1982). "Randomization and concomitant variables in the design of experiments", *Statistics and Probability: Essays in Honor of C. R. Rao* (G. Kallianpur, P. R. Krishnaiah, and J. K. Ghosh, eds.), North Holland.
- Craven, P. and Wahba, G. (1979). "Smoothing noisy data with spline functions", *Numer. Math.*, 31, 377-403.
- Draper, N. R. and Faraggi, D. (1985). "Role of Papadakis estimator in one- or two-dimensional field trials", *Biometrika*, 72, 223-226.
- Draper, N. R. and Guttman, I. (1980). "Incorporating overlap effects from neighbouring units into response surface models", *Appl. Statist.*, 29, 128-134.
- Draper, N. R. and Herzberg, A. M. (1979). "Designs to guard against outliers in the presence or absence of model bias", *Canadian J. of Statist.*, 7, 127-135.
- Dyke, G. V. (1980). "Covariance and field experiments", *Bull. Appl. Stat.*, 7, 10-21.
- Dyke, G. V. and Jenkyn, J. F. (1984). "Interplot interference and spatial analysis", *Proc Biometric Society Workshop*, 31-33.
- Dyke, G. V. and Shelley, C. F. (1976). "Serial designs balanced for effects of neighbours on both sides", *J. Agric. Sci.*, Cambridge, 87, 303-305.
- Edgington, E. S. (1980). *Randomization Tests*, New York: Marcel Dekker.
- Eisenhart, C. (1947). "The assumptions underlying the analysis of variance", *Biometrics*, 3, 1-21.
- Federer, W. T. and Schlottfeldt, C. S. (1954). "The use of covariance to control gradients in experiments", *Biometrics*, 10, 282-290.
- Finney, D. J. (1962). "An unusual salvage operation", *Biometrics*, 18, 247-250.
- Finney, D. J. and Outhwaite, A. D. (1956). "Serially balanced sequences in bioassay", *Proc. of the Royal Society B*, 145, 493-507.
- Fisher, R. A. (1918). "The causes of human variability", *Eugen. Rev.*, 10, 213-220.
- Fisher, R. A. (1925). *Statistical Methods for Research Workers*, Edinburgh: Oliver and Boyd.
- Fisher, R. A. and MacKenzie, W. A. (1923). "Studies in crop variation II. The manurial response of different potato varieties", *J. of Agric Sci.*, 13, 311-320.
- Folks, J. L. (1984). "Use of randomization in experimental research", *Experimental Design, Statistical Models, and Genetic Statistics*, K. Hinkelmann, ed., New York: Marcel Dekker.

- Freeman, G. H. (1979). "Some two-dimensional designs balanced for nearest neighbours", *J. R. Statist. Soc. B*, 41, 88-95.
- Gill, P. S. and Shukla, G. K. (1985). "Efficiency of nearest neighbour balanced block designs for correlated observations", *Biometrika*, 72, 539-544.
- Gosset, W. S. (1938). "Comparison between balanced and random arrangements of field plots", *Biometrika*, 29, 363-378.
- Gomez, K. A. (1972). "Border effects in rice experimental plots.II.varietal competition", *Experimental Agriculture*, 8, 295-298.
- Gomez, K. A. and Gomez, A. A. (1984). *Statistical Procedures for Agricultural Research*, 2nd edition. New York: Wiley.
- Green, P. J. (1984). "Comparison of neighbour methods using cross-validation", *Proc. Biometric Society Workshop*, 47-50.
- Green, P. J., Jennison, C. and Seheult, A. H. (1983). "Contribution to Discussion of Wilkinson et al. (1983)", *J. R. Statist. Soc. B*, 45, 193-194.
- Green, P. J., Jennison, C. and Seheult, A. H. (1985). "Analysis of field experiments by least squares smoothing", *J. R. Statist. Soc. B*, 47, 299-315.
- Grundy, P. M. and Healy, M. J. R. (1950). "Restricted randomization and quasi-Latin squares", *J. R. Statist. Soc. B*, 12, 286-291.
- Hald, A. (1948). *The Decomposition of a Series of Observations*, Copenhagen: G. E. C. Gads Forlag.
- Harville, D. A. (1975). "Experimental randomization: Who needs it?" *Amer. Statist.*, 29, 27-31.
- Herzberg, A. M. (1982). "The design of experiments for correlated error structures: Layout and robustness", *The Canadian Journal of Statistics*, 10, 133-138.
- Herzberg, A. M. and Andrews, D. F. (1976). "Some considerations in the optimal design of experiments in non-optimal situations", *J. R. Statist. Soc. B*, 38, 284-289.
- Huber, P. J. (1978). *Robustness in Statistics* (R. L. Launer and G. N. Wilkinson, eds.), New York: Academic Press.
- Jenkyn, J. F., Bainbridge, A., Dyke, G. V. and Todd, A. D. (1979). "An investigation into inter-plot interactions in experiments with mildew on barley, using balanced designs", *Ann. of Appl Biol.*, 92, 11-28.
- Jennison, C. and Seheult, A. H. (1984). "Two-dimensional least squares smoothing analysis of field experiments", *Proc. Biometric Society Workshop*, 25-28.
- Jensen, N. F. and Federer, W. T. (1964). "Adjacent row competition in wheat", *Crop Science*, 4, 641-645.
- John, J. A. and Williams, E. R. (1982). "Conjectures for optimal block designs", *J. R. Statist. Soc. B*, 44, 221-225.

- Kemphorne, O., Zyskind, G., Addelman, S., Throckmorton, T. N., White, R. F. (1961). "Analysis of variance procedures", Report submitted to Aeronautical Research Laboratory, Contract No. AF 33 (616) - 5599, Project No. 7071, Task No. 70418.
- Kemphorne, O. (1952). *The Design and Analysis of Experiments*, New York: John Wiley and Sons.
- Kempton, R. A. (1982). "Adjustment for competition between varieties in plant breeding trials", *J. Agric. Sci.*, 98, 599-611.
- Kempton, R. A. (1984a). "Comparison of nearest neighbour and classical methods of analysis", *Proc. Biometric Society Workshop*, 51-52.
- Kempton, R. A. (1984b). "The design and analysis of unreplicated field trials", *Vortrage fur Pflanzenzuchtung*, 7, 219-242.
- Kempton, R. A. (1984c). "Nearest neighbour analysis and interplot competition", *Proc. Biometric Society Workshop*, 34-35.
- Kempton, R. A., Gregory, R. S., Hughes, W. G. and Stoehr, P. J. (1986). "The effect of interplot competition on yield assessment in triticale trials", *Euphytica*, 35, 257-265.
- Kempton, R. A. and Howes, C. W. (1981). "The use of neighbouring plot values in the analysis of variety trials", *Appl. Statist.*, 30, 59-70.
- Kempton, R. A. and Lockwood, G. (1984). "Interplot competition in variety trials of field beans", *J. Agric. Sci.*, 103, 293-302.
- Kiefer, J. and Wynn, H. P. (1981). "Optimum balanced block and Latin square designs for correlated observations", *Annals of Statistics*, 9, 737-757.
- Kiefer, J. and Wynn, H. P. (1984). "Optimum and minimax exact treatment designs for one-dimensional autoregressive error processes", *Annals of Statistics*, 12, 431-450.
- Koopmans, T. (1942). "Serial correlation and quadratic forms in normal variables", *Ann. Math. Statist.*, 13, 14-33.
- LeClerg, E. L., Leonard, W. H. and Clark, A. G. (1962). *Field Plot Technique*, Minneapolis: Burgess.
- Lin, C. S. and Poushinsky, G. (1983). "A modified augmented design for an early stage of plant selection involving a large number of test lines without replication", *Biometrics*, 39, 553-561.
- Lin, C. S., Poushinsky, G. and Jui, P. Y. (1983). "Simulation study of three adjustment methods for the modified augmented design and comparison with the balanced lattice square design", *J. of Agric. Sci.*, 100, 527-534.
- Martin, R. J. (1981). "A note on the adequacy of spatial models to some agricultural uniformity data", *Biometrika*, 68, 336-338.
- Martin, R. J. (1982). "Some aspects of experimental design and analysis when errors are correlated", *Biometrika*, 69, 597-612.
- Martin, R. J. (1984a). "Papadakis' development of his method", *Proc. Biometric Society Workshop*, 3-4.

- Martin, R. J. (1984b). "Some theoretical results on the design and analysis of experiments with second-order stationary spatially correlated errors", *Proc. Biometric Society Workshop*, 23-24.
- McClelland, C. K. (1926). "New methods with check plots", *J. Amer. Soc. of Agronomy*, 18, 566-575.
- Mead, R. (1967). "A mathematical model for the estimation of interplant competition", *Biometrics*, 23, 189-205.
- Mead, R. (1968). "Measurement of competition between individual plants in a population", *J. Ecol.*, 56, 35-45.
- Mead, R. (1971). "Models for interplant competition in irregularly distributed populations", *Statistical Ecology*, Vol. 2, 13-32, Pennsylvania State University Press.
- Nelder, J. A. (1983). "Contribution to Discussion of Wilkinson et al. (1983)", *J. R. Statist. Soc. B*, 45, 200-202.
- Nelder, J. A. (1965a). "The analysis of randomized experiments with orthogonal block structure. I. Block structure and the null analysis of variance", *Proc. Roy. Soc. A*, 283, 147-162.
- Nelder, J. A. (1965b). "The analysis of randomized experiments with orthogonal block structure. II. Treatment structure and the general analysis of variance", *Proc. Roy. Soc. A*, 283, 163-178.
- Neyman, J. (1929). "The theoretical basis of different methods of tasting cereals, Part II. The Method of parabolic curves.", *Pedigree Seed Cultures, No. 4*, Warsaw: K. Buszczyński and Sons.
- Ord, J. K. (1975). "Estimation methods for models of spatial interaction", *J. Amer. Statist. Assoc.*, 70, 120-126.
- Outhwaite, A. D. and Rutherford, A. (1955). "Covariance analysis as an alternative to stratification in the control of gradients", *Biometrics*, 11, 431-440.
- Papadakis, J. S. (1937). "Methode statistique pour des experiences sur champ", *Bull. Inst. Amel. Plantes a Salonique*, No. 23.
- Papadakis, J. S. (1940). "Comparaison de diferentes metodos d'experimentation phytotechnique", *Revista Argentina de Agronomia*, 7, 297-362.
- Papadakis, J. S. (1954). *Ecologia de los cultivos 2*, Buenos Aires.
- Papadakis, J. S. (1970). *Agricultural Research*, Buenos Aires.
- Papadakis, J. S. (1984). "Advances in the analysis of field experiments", *Proc. Acad. Athens*, 59, 326-342.
- Patterson, H. D. (1983). "Contribution to Discussion of Wilkinson et al. (1983)", *J. R. Statist. Soc. B*, 45, 151-211.
- Patterson, H. D. and Hunter, E. A. (1983). "The efficiency of incomplete block designs in national list and recommended list cereal variety trials", *J. Agric. Sci.*, 101, 427-433.
- Patterson, H. D. and Silvey, V. (1980). "Statutory and recommended list trials of crop varieties in the United Kingdom (with Discussion)", *J. R. Statist. Soc. A*, 143, 219-252.

- Patterson, H. D. and Thompson, R. (1971). "Recovery of inter-block information when block sizes are unequal", *Biometrika*, 58, 545-554.
- Patterson, H. D., Williams, E. R. and Hunter, E. A. (1978). "Block designs for variety trials", *J. Agric. Sci.*, 90, 395-400.
- Pearce, S. C. (1957). "Experimenting with organisms as blocks", *Biometrika*, 44, 141-149.
- Pearce, S. C. (1978). "The control of environmental variation in some West Indian maize experiments", *Trop. Agric. (Trinidad)*, 55, 97-106.
- Pearce, S. C. (1980). "Randomized blocks and some alternatives: A study in tropical conditions", *Trop. Agric. (Trinidad)*, 57, 1-10.
- Pearce, S. C. (1983). "Contribution to Discussion of Wilkinson et al. (1983)", *J. R. Statist. Soc. B*, 45, 183.
- Pearce, S. C. and Moore, C. S. (1976). "Reduction of experimental error in perennial crops, using adjustment by neighbouring plots", *Exp. Agric.*, 12, 267-272.
- Pearson, E. S. (1937). "Some aspects of the problem of randomization", *Biometrika*, 29, 53-64.
- Percy, W. (1975). *The Message in the Bottle*, New York: Farrar, Straus, and Giroux.
- Preece, D. A., Bailey, R. A., and Patterson, H. D. (1978). "A randomization problem in forming designs with superimposed treatments", *Austral. J. Statist.*, 20, 111-125.
- Preece, D. A. and Freeman, G. H. (1983). "Semi-Latin squares and related designs", *J. R. Statist. Soc. B*, 45, 267-277.
- Plackett, R. L. (1960). "Models in the analysis of variance", *J. R. Statist. Soc. B*, 22, 195-217.
- Quenouille, M. H. (1949). "On a method of trend elimination", *Biometrika*, 36, 75-91.
- Rao, C. R. (1973). *Linear Statistical Inference and Its Applications*. 2nd edition, p. 33. New York: John Wiley.
- Rice, J. (1981). "An approach to peak area estimation", *J. Res. Nat. Bur. Standards*, 87, 53-65.
- Richey, F. D. (1924). "Adjusting yields to their regression on a moving average as a means of correcting for soil heterogeneity", *J. Agric. Research*, 27, 79-90.
- Ripley, B. D. (1978). "Contribution to Discussion of Bartlett (1978)", *J. R. Statist. Soc. B*, 40, 162-163.
- Ripley, B. D. (1981). *Spatial Statistics*, New York: Wiley.
- Rojas, B. and White, R. F. (1957). "The modified Latin square", *J. R. Statist. Soc. B*, 19, 305-317.
- SAS Users' Guide: Statistics 1982 Edition*, A. A. Ray, editor, Cary, N. C.: SAS Institute Inc.
- Scheffe, H. (1956). "Alternative models for the analysis of variance", *Annals of Math. Statistics*, 27, 251-271.
- Schwarzbach, E. (1985). "A new approach in the evaluation of field trials", *Proc. Eucarpia Cereals Section, Vortrage fur Pflanzenzuchtung*, 8.

- Steel, R. G. D. and Torrie, J. H. (1980). *Principles and Procedures of Statistics. A Biometrical Approach*, 2nd edition, New York: McGraw-Hill, Inc.
- Tiao, G. C. and Ali, M. M. (1971). "Analysis of correlated random effects: Linear model with two random components", *Biometrika*, 58, 37-51.
- Tippett, L. H. C. (1931). *The Methods of Statistics*, London: Williams and Norgate, Ltd.
- Thompson, R. (1984). "The use of multiple copies of data in forming and interpreting analysis of variance", *Experimental Design, Statistical Methods, and Genetics Statistics* (K. Hinkelmann, ed.), New York: Marcel Dekker.
- Thompson, R. (1984). "Multiple copies, the GENSTAT analysis of variance algorithm and neighbour analyses", *Proc. Biometric Society Workshop*, 20-22.
- Wahba, G. (1977). "A survey of some smoothing problems, and the method of generalized cross-validation for solving them", *Applications of Statistics* (P. R. Krishnaiah, ed.), North Holland.
- White, R. F. (1975). "Randomization and the analysis of variance", *Biometrics*, 31, 555-572.
- Whittle, P. (1954). "On stationary processes in the plane", *Biometrika*, 41, 434-449.
- Wiancko, A. T. (1914). "Use and management of check plots in soil fertility investigations", *J. Amer. Soc. of Agronomy*, 6, 122-124.
- Wiancko, A. T., Army, A. C. and Salmon, S. C. (1921). "Report of committee on standardization of field experiments", *J. Amer. Soc. of Agronomy*, 13, 368-374.
- Wilk, M. B. and Kempthorne, O. (1955). "Fixed, mixed, and random models", *J. Amer. Statist. Assoc.*, 50, 1144-1167.
- Wilk, M. B. and Kempthorne, O. (1957). "Nonadditivities in a Latin square design", *J. Amer. Statist. Assoc.*, 52, 218-236.
- Wilkinson, G. N. (1984). "Nearest neighbour methodology for design and analysis of field experiments", *Proc. XIIth Inter. Biom. Conf. Tokoyo*.
- Wilkinson, G. N. (1984). "Some points about NN analysis of field experiments", *Proc. Biometric Society Workshop*, 15-19.
- Wilkinson, G. N., Eckert, S. R., Hancock, T. W. and Mayo, O. (1983). "Nearest-neighbour (NN) analysis of field experiments (with Discussion)", *J. R. Statist. Soc. B*, 45, 151-211.
- Williams, E. R. (1975). "Efficiency-balanced designs", *Biometrika*, 62, 686-689.
- Williams, E. R. (1977). "Iterative analysis of generalized lattice designs", *Austral. J. Statist.*, 19, 39-42.
- Williams, E. R. (1985). "A criterion for the construction of optimal neighbour designs", *J. R. Statist. Soc. B*, 47, 489-497.
- Williams, E. R. (1986a). "A neighbour model for field experiments", *Biometrika*, 73 (in press).
- Williams, E. R. (1986b). "Neighbour analysis of uniformity data", *Austral. J. Statist.*, 28, 182-191.

- Williams, R. M. (1952). "Experimental designs for serially correlated observations", *Biometrika*, 39, 151-167.
- Yates, F. (1933). "The formation of Latin squares for use in field experiments", *Empire J. Exp. Agric.*, 1, 235-244.
- Yates, F. (1935). "Complex experiments", *J. R. Statist. Soc. Suppl.*, 2, 181-247.
- Yates, F. (1936). "A new method of arranging variety trials involving a large number of varieties", *J. Agric. Sci.*, 26, 424-455.
- Yates, F. (1939). "The comparative advantages of systematic and randomized arrangements in the design of agricultural and biological experiments", *Biometrika*, 30, 440-466.

Appendix A

A. Derivations from Chapter Three

A.1 RCBD with Border Shifts

A.1.1 Expectation of squared shift term

$$E \{ [\sum_{i,j} \delta_{ij}^k c_{ij} I_{(1,t)}(j)]^2 \} = E \{ [\sum_{i=1}^t (\delta_{ii}^k c_{i1} + \delta_{ii}^k c_{it})]^2 \}$$

Note that

$$E [(\delta_{ij}^k)^2] = t^{-1} , \tag{A.1.1}$$

$$E [(\delta_{ij}^k \delta_{i'j'}^k)] = 0 , \text{ and} \tag{A.1.2}$$

$$E [(\delta_{ij}^k \delta_{i'j'}^k)] = t^{-2} \text{ for } i \neq i' . \tag{A.1.3}$$

Consider a $(2r) \times (2r)$ matrix whose entries are $c_{ij} c_{i'j'} \delta_{ij}^k \delta_{i'j'}^k$, which has expectation $t^{-2} c_{ij} c_{i'j'}$ for $i \neq i'$. If this were the case for all entries, the sum of all the entries would be

$$t^{-2} \left(\sum_{i=1}^r (c_{i1} + c_{ir}) \right)^2. \quad (A.1.4)$$

This not the case, so subtract the terms with expectation zero in (A.1.2) to get

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{ir}) \right]^2 - t^{-2} \sum_{i=1}^r 2c_{i1} c_{ir}. \quad (A.1.5)$$

Since the diagonal terms of the matrix are $t^{-1} c_{ij}^2$ and the corresponding terms of (A.1.4) are $t^{-2} c_{ij}^2$, we correct by subtracting

$$t^{-2} c_{ij}^2 - t^{-1} c_{ij}^2 = -c_{ij}^2 (t-1) t^{-2}$$

from (A.1.5) to obtain

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{ir}) \right]^2 - t^{-2} \sum_{i=1}^r 2c_{i1} c_{ir} - (t-1) t^{-2} \sum_{i=1}^r (c_{i1}^2 + c_{ir}^2)$$

which simplifies to

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{ir}) \right]^2 + t^{-1} \sum_{i=1}^r [(c_{i1}^2 + c_{ir}^2) - t^{-1} (c_{i1} + c_{ir})^2].$$

A.1.2 Cross-product with residual terms

$$\begin{aligned} & E \left\{ 2 \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i',j'} \delta_{i'j'}^k c_{ij} I_{(1,t)}(j) \right\} \\ &= E \left\{ 2 \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r [\delta_{i1}^k c_{i1} + \delta_{ir}^k c_{ir}] \right\} \end{aligned}$$

$$= E \left\{ 2 \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r \delta_{i1}^k c_{i1} + 2 \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r \delta_{it}^k c_{it} \right\} .$$

The two terms of the above expression will have identical expectations as far as the δ_{ij}^k are concerned. Thus consider only $E \left\{ 2 \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r \delta_{i1}^k c_{i1} \right\}$. Since the same treatment cannot occur twice in the same block, all terms except $2E \left\{ \sum_{i=1}^r (\delta_{i1})^2 e_{i1} c_{i1} \right\}$ vanish. From (A.1.1) we see that these terms have expectation $2t^{-1} \sum_{i=1}^r e_{i1} c_{i1}$. Combining the two halves of the above expression, we have

$$2t^{-1} \sum_{i=1}^r [e_{i1} c_{i1} + e_{it} c_{it}] .$$

A.2 Derivation of Covariance of Two Treatment Totals

A.2.1 Expanded Expression of the Covariance

$$E \{ [T_k - E(T_k)] [T_{k'} - E(T_{k'})] \}$$

$$= E \left\{ \left[\sum_{i,j} \delta_{ij}^k e_{ij} + \sum_{i,j} \delta_{ij}^k c_{ij} I_{(1,0)}(j) - t^{-1} \sum_{i=1}^r (c_{i1} + c_{it}) \right] \left[\sum_{i,j} \delta_{ij}^{k'} e_{ij} + \sum_{i,j} \delta_{ij}^{k'} c_{ij} I_{(1,0)}(j) - t^{-1} \sum_{i=1}^r (c_{i1} + c_{it}) \right] \right\}$$

which equals

$$E \left[\sum_{i,j} \delta_{ij}^k e_{ij}^k \sum_{i,j} \delta_{ij}^{k'} c_{ij} \right] + \tag{A.2.1}$$

$$E \left[\sum_{i,j} \delta_{ij}^k e_{ij}^k \sum_{i=1}^r (\delta_{i1}^{k'} c_{i1} + \delta_{it}^{k'} c_{it}) \right] + \tag{A.2.2}$$

$$E[\sum_{i,j} \delta_{ij}^{k'} e_{ij} \sum_{l=1}^r (\delta_{il}^k c_{il} + \delta_{il}^k c_{il})] - \quad (A.2.3)$$

$$E[t^{-1} \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{l=1}^r (c_{il} + c_{il})] - \quad (A.2.4)$$

$$E[t^{-1} \sum_{i,j} \delta_{ij}^{k'} e_{ij} \sum_{l=1}^r (c_{il} + c_{il})] + \quad (A.2.5)$$

$$E[\sum_{i=1}^r (\delta_{i1}^k c_{i1} + \delta_{it}^k c_{it}) \sum_{i=1}^r (\delta_{i1}^{k'} c_{i1} + \delta_{it}^{k'} c_{it})] - \quad (A.2.6)$$

$$E[t^{-1} \sum_{i=1}^r (\delta_{i1}^k c_{i1} + \delta_{it}^k c_{it}) \sum_{i=1}^r (c_{i1} + c_{it})] - \quad (A.2.7)$$

$$E[t^{-1} \sum_{i=1}^r (\delta_{i1}^{k'} c_{i1} + \delta_{it}^{k'} c_{it}) \sum_{i=1}^r (c_{i1} + c_{it})] - \quad (A.2.8)$$

$$E[t^{-2} (\sum_{i=1}^r (c_{i1} + c_{it}))^2] . \quad (A.2.9)$$

Term (A.2.1) is shown by Kempthorne (1952) to be $-[t(t-1)]^{-1} \sum_{i,j} e_{ij}^2$.

A.2.2 Terms (A.2.2) and (A.2.3)

These terms will have identical expectations and we will use the same simplification used in Appendix A.1. We will also need the following expectations from Kempthorne:

$$E[\delta_{ij}^k \delta_{ij}^{k'}] = 0 ,$$

$$E[\delta_{ij}^k \delta_{i'j'}^{k'}] = [t(t-1)]^{-1} \text{ for } j \neq j' \text{ } k \neq k' , \text{ and}$$

$$E[\delta_{ij}^k \delta_{i'j'}^{k'}] = t^{-2} \text{ for } i \neq i' .$$

Thus when $i \neq i'$, the sum will be

$$[t(t-1)]^{-1} \sum_{j>1} e_{ij} c_{i1} = - [t(t-1)]^{-1} e_{i1} c_{i1} ;$$

otherwise the sum will be $t^{-1} c_{i1} \sum_{j=1}^t e_{ij} = 0$ Thus we have

$$E \left[\sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r \delta_{i1}^{k'} c_{i1} \right] = - [t(t-1)]^{-1} \sum_{i=1}^r e_{i1} c_{i1} .$$

Combining (A.2.2) and (A.2.3) we have

$$- 2[t(t-1)]^{-1} \sum_{i=1}^r (c_{i1} e_{i1} + c_{it} e_{it}) . \quad (A.2.10)$$

A.2.3 Terms A.2.4 and A.2.5

We observe that these two terms have identical expectations. Since $E(\delta_{ij}^k) = E(\delta_{ij}^{k'}) = t^{-1}$, we have

$$E \left[2t^{-1} \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i=1}^r (c_{i1} + c_{it}) \right] = 2t^{-2} \sum_{ij} e_{ij} \sum_{i=1}^r (c_{i1} + c_{it}) = 0 .$$

A.2.4 Term A.2.6

Consider the $(2r) \times (2r)$ matrix with entries $\delta_{ij}^k \delta_{i'j'}^{k'}, c_{ij} c_{i'j'}$, ($j=1,t$) representing the product (A.2.6).

If all entries had expectation $t^{-2} c_{ij} c_{i'j'}$, we would have the sum

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{it}) \right]^2 . \quad (A.2.11)$$

Since two treatments cannot occur on the same plot, we know the diagonals of the matrix must be zero. Thus we subtract $t^{-2}c_{ij}^2$ from (A.2.11) to obtain

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{it})^2 \right] - t^{-2} \sum_{i=1}^r (c_{i1}^2 + c_{it}^2) . \quad (\text{A.2.12})$$

The off-diagonals have expectation $[t(t-1)]^{-1}c_{ij}c_{ij}$, rather than $t^{-2}c_{ij}c_{ij}$. Thus we subtract the difference

$$[t^{-2} - \{t(t-1)\}^{-1}] \sum_{i=1}^r 2c_{ij}c_{it} = -t^{-2}(t-1)^{-1} \sum_{i=1}^r 2c_{i1}c_{it}$$

from (A.2.12). The result is

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{it})^2 \right] - t^{-2}(t-1)^{-1} \sum_{i=1}^r [(c_{i1}^2 + c_{it}^2)(t-1) - 2c_{i1}c_{it}]$$

which equals

$$t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{it})^2 \right] - [t(t-1)]^{-1} \left[\sum_{i=1}^r (c_{i1}^2 + c_{it}^2) - t^{-1} \sum_{i=1}^r (c_{i1} + c_{it})^2 \right]$$

A.2.5 Remaining terms

- Terms (A.2.7, A.2.8) have identical expectations of $t^{-2} \left[\sum_{i=1}^r (c_{i1} + c_{it})^2 \right]$.
- Term (A.2.9) is a constant with respect to randomization.

Combining the expectations derived for terms (A.2.1 - A.2.9), we have the covariance of two treatment totals to be

$$- [(t-1)]^{-1} \left[\sum_{i,j} e_{ij}^2 + 2 \sum_{i=1}^r (e_{i1}c_{i1} + e_{it}c_{it}) + \sum_{i=1}^r [(c_{i1}^2 + c_{it}^2) - t^{-1}(c_{i1} + c_{it})^2] \right] .$$

A.3 Subplot Expectations for Split Plot with Border Shifts

A.3.1 Expectation of subplot treatment sum of squares

$$E[SS_s] = E[(rt)^{-1} \sum_{k=1}^s Y_{\dots k}^2 - CF]$$

We consider only the first term above which we write as

$$(rt)^{-1} E \left[\sum_{i=1}^s (rt\mu + rts_k + \sum_{i,j} e_{ij} + \sum_{i,j} \gamma_{ijk})^2 \right].$$

Squaring and summing this, we have

$$rts\mu^2 + rt \sum_{k=1}^s s_k^2 +$$

$$(rt)^{-1} \sum_{k=1}^s E[(\sum_{i,j} e_{ijk})^2] + \tag{A.3.1}$$

$$(rt)^{-1} \sum_{k=1}^s E[(\sum_{i,j} \gamma_{ijk})^2] + \tag{A.3.2}$$

$$2\mu \sum_{k=1}^s E[\sum_{i,j} \gamma_{ijk}] + \tag{A.3.3}$$

$$2(rt)^{-1} \sum_{k=1}^s E[\sum_{i,j} e_{ijk} \sum_{i,j} \gamma_{ijk}]. \tag{A.3.4}$$

We know from Kempthorne (1952) that (A.3.1) is $(rt)^{-1} \sum_{iuv} (x_{iuv} - x_{iu.})^2$.

Term (A.3.2) can be expanded to

$$(rt)^{-1} \sum_{k=1}^s E \left[\sum_{lju} (\delta_{lu1}^{jk} c_{l1} + \delta_{lus}^{jk} c_{ls})^2 \right].$$

We will need the following expectations:

$$E[(\delta_{iuv}^{jk})^2 c_{iv}^2] = (ts)^{-1} c_{il}^2 \quad \text{for } l = 1, s,$$

$$E[\delta_{iuv}^{jk} \delta_{iuv}^{j'k} c_{iv} c_{iv'}] = 0 \quad \text{for } v \neq v',$$

$$E[\delta_{iuv}^{jk} \delta_{iuv}^{j'k} c_{iv} c_{iv'}] = 0 \quad \text{for } j \neq j',$$

$$E[\delta_{iuv}^{jk} \delta_{iu'v'}^{j'k} c_{iv} c_{iv'}] = [ts^2(t-1)]^{-1} c_{iv} c_{iv'} \quad \text{for } j \neq j', u \neq u',$$

$$E[\delta_{iuv}^{jk} \delta_{iu'v'}^{j'k} c_{iv} c_{iv'}] = (ts)^{-2} c_{iv} c_{iv'} \quad \text{for } i \neq i'.$$

Using these expectations we have $E[(\sum_{i,j} \gamma_{ijk})^2]$ as

$$(rt)^{-1} \sum_{k=1}^s \{ t^2 \sum_{l=1}^r [(ts)^{-1} c_{l1}^2 + (ts^2(t-1))^{-1} c_{l1} [(t-1)(c_{l1} + c_{ls})] + c_{l1} \sum_{i' \neq l} (c_{i'1} + c_{i's})(ts)^{-2}] \} +$$

$$(rt)^{-1} \sum_{k=1}^s \{ t^2 \sum_{l=1}^r [(ts)^{-1} c_{ls}^2 + (ts^2(t-1))^{-1} c_{ls} [(t-1)(c_{l1} + c_{ls})] + c_{ls} \sum_{i' \neq l} (c_{i'1} + c_{i's})(ts)^{-2}] \}$$

where t^2 is a result of summing over t treatments and t whole plots per replicate. Some tedious algebra will simplify this expression so that (A.3.2) is

$$(rs)^{-1} \sum_{l=1}^r [s(c_{l1}^2 + c_{ls}^2) + (t-1)(c_{l1} + c_{ls})^2 + t \sum_{i' \neq l} (c_{l1} + c_{ls}) (c_{i'1} + c_{i's})]$$

which further simplifies to

$$r^{-1} \left[\sum_{l=1}^r [(c_{l1}^2 + c_{ls}^2) - s^{-1}(c_{l1} + c_{ls})^2] \right] + t(rs)^{-1} \left[\sum_{l=1}^r (c_{l1} + c_{ls})^2 \right]. \quad (A.3.6)$$

A.3.1.1 Term A.3.3

We expand the expression to get

$$\begin{aligned} & 2\mu \sum_{k=1}^s E\left\{\sum_{i,j} (\delta_{iu1}^{jk} c_{i1} + \delta_{ius}^{jk} c_{is})\right\} \\ & = 2\mu (ts)^{-1} \sum_{i,j,k} (c_{i1} + c_{is}) \end{aligned}$$

which equals

$$2\mu \sum_{i=1}^r (c_{i1} + c_{is}) . \tag{A.3.7}$$

A.3.1.2 Term A.3.4

We expand the expression to obtain

$$2(rt)^{-1} \sum_{k=1}^s E\left\{\sum_{i,j} e_{ijk} \sum_{i,j} (\delta_{iu1}^{jk} c_{i1} + \delta_{ius}^{jk} c_{is})\right\} .$$

We may think of the terms of this sum as comprising a matrix whose rows are defined by e_{ijk} and whose columns are defined by $\delta_{iu}^k c_v$ where $v = 1, s$. Applying the expectations shown in the derivation of (A.3.2), we see that the sum must be

$$2(rt)^{-1} \sum_{k=1}^r \sum_{i,j,u} (ts)^{-1} [c_{i1} (x_{iu1} - x_{iu.}) + c_{is} (x_{ius} - x_{iu.})]$$

which equals

$$2(rt)^{-1} \sum_{i,u} (c_{i1} e_{iu1} + c_{is} e_{ius}) . \tag{A.3.8}$$

where $e_{tuv} = x_{tuv} - x_{tu}$. Combining (A.3.5 - A.3.8) and subtracting the correction factor, the expectation of the sum of squares for subplot treatments is

$$(rt)^{-1} \sum_{iuv} e_{tuv}^2 + \varphi(c_{iv}) + rt \sum_{k=1}^s s_k^2$$

where $\varphi(c_n)$ is

$$[r(s-1)]^{-1} \sum_{i=1}^r [(c_{i1}^2 + c_{is}^2) - s^{-1}(c_{i1} + c_{is})^2].$$

A.3.2 Expectation of whole plot by subplot interaction sum of squares

The sum of squares is the linear combination of sums below:

$$E[r^{-1} \sum_{j,k} Y_{.jk}^2 - (rs)^{-1} \sum_{j=1}^r Y_{.j}^2 - (rt)^{-1} \sum_{k=1}^s Y_{..k}^2 + CF].$$

All expectations, except for the first term, have been obtained previously so we will restrict our attention to that first term. Substituting the linear model, squaring, and summing yields

$$rst\mu^2 + rs \sum_{j=1}^r t_j^2 + rt \sum_{k=1}^s s_k^2 + r \sum_{j,k} (ts)_{jk}^2 +$$

$$r^{-1} \sum_{j,k} E[(\sum_{i=1}^r \eta_{ij})^2] + \tag{A.3.9}$$

$$r^{-1} \sum_{j,k} E[(\sum_{i=1}^r e_{ijk})^2] + \tag{A.3.10}$$

$$r^{-1} \sum_{j,k} E[(\sum_{i=1}^r \gamma_{ijk})^2] + \tag{A.3.11}$$

$$r^{-1} \sum_{j,k} 2r\mu E(\sum_{i=1}^r \gamma_{ijk}) + \tag{A.3.12}$$

$$2r^{-1} \sum_{j,k} E(\sum_{l=1}^r \eta_{lj}) (\sum_{l=1}^r \gamma_{ljk}) + \quad (A.3.13)$$

$$2r^{-1} \sum_{j,k} E(\sum_{l=1}^r e_{ljk}) (\sum_{l=1}^r \gamma_{lkj}) . \quad (A.3.14)$$

From Kempthorne (1952) we know that (A.3.9) is

$$rs^{-1} \sum_{l,u} (x_{iu.} - x_{i..})^2 \quad (A.3.15)$$

and that (A.3.10) is

$$r^{-1} \sum_{l,u,v} e_{luv}^2 . \quad (A.3.16)$$

A.3.2.1 Term A.3.11

We expand the expression to obtain

$$r^{-1} \sum_{j,k} E[(\sum_{l,u} (\delta_{lu1}^{jk} c_{l1} + \delta_{lus}^{jk} c_{ls}))^2]$$

whose square is

$$r^{-1} \{ts \sum_{l=1}^r t[(ts)^{-1} c_{l1}^2 + (ts^2)^{-1} c_{l1} \sum_{l' \neq l} (c_{l1} + c_{ls})]\} +$$

$$r^{-1} \{ts \sum_{l=1}^r t[(ts)^{-1} c_{ls}^2 + (ts^2)^{-1} c_{ls} \sum_{l' \neq l} (c_{l1} + c_{ls})]\} .$$

We combine the two halves to obtain

$$tr^{-1} \sum_{l=1}^r [(c_{l1}^2 + c_{ls}^2) + s^{-1} (\sum_{l=1}^r (c_{l1} + c_{ls}))^2 - s^{-1} \sum_{l=1}^r (c_{l1} + c_{ls})^2]$$

which is

$$t(rs)^{-1} \left[\sum_{i=1}^r (c_{i1} + c_{is}) \right]^2 + tr^{-1} \sum_{i=1}^r [(c_{i1}^2 + c_{is}^2) - s^{-1} \sum_{i=1}^r (c_{i1} + c_{is})^2] . \quad (A.3.17)$$

A.3.2.2 Term A.3.12

We expand to get

$$r^{-1} \sum_{j,k} 2r\mu E \left[\sum_{l,u} (\delta_{lu1}^{jk} c_{l1} + \delta_{lus}^{jk} c_{ls}) \right]$$

which equals

$$2t\mu \sum_{i=1}^r (c_{i1} + c_{is}) . \quad (A.3.18)$$

A.3.2.3 Term A.3.13

Observing that

$$E[\delta_{lu}^j \delta_{uv}^{jk}] = (ts)^{-1} ,$$

$$E[\delta_{lu}^j \delta_{uv}^{jk}] = 0 , \quad \text{and}$$

$$E[\delta_{lu}^j \delta_{uv}^{jk}] = (t^2s)^{-1}$$

we look at (A.3.13) in two parts. We first consider those products involving two different replicates and we have

$$2r^{-1} \sum_{l,u} (\delta_{lu1}^{jk} c_{l1} + \delta_{lus}^{jk} c_{ls}) \sum_{l' \neq l, u=1}^t \sum_{u'} \delta_{l'u}^j (x_{l'u} - x_{l'..})$$

which has expectation

$$2(rs t^2)^{-1} \sum_{l,u} [(c_{l1} + c_{ls}) \sum_{l' \neq l} (x_{l'u} - x_{l'..})] = 0 .$$

The remaining portion of (A.3.13) is

$$2r^{-1} E \sum_{l,u} (\delta_{lu1}^{jk} c_{l1} + \delta_{lus}^{jk} c_{ls}) \sum_{u'=1}^t \delta_{lu}^j (x_{lu'u} - x_{lu..}) ,$$

whose terms are non-zero only when $u = u'$. The sum is then

$$2(rs t)^{-1} [\sum_{l,u} (c_{l1} + c_{ls})(x_{lu.} - x_{lu..})] = 0 .$$

Thus (A.3.13) has zero expectation.

A.3.2.4 Term A.3.14

We follow the same logic for deriving (A.3.14) as in obtaining (A.3.8) from (A.3.4). It turns out that (A.3.14) is

$$2r^{-1} \sum_{l,u} (c_{l1} e_{lu1} + c_{ls} e_{lus}) .$$

Combining these expectations and those derived elsewhere, the expectation of the whole plot by subplot interaction sum of squares is

$$(rt)^{-1} \sum_{l,u} e_{luv}^2 + \varphi(c_{tv}) + r \sum_{j,k} (ts)_{jk}^2$$

where

$$\varphi(c_{tv}) = r^{-1} \sum_{l=1}^t [(c_{l1}^2 + c_{ls}^2) - s^{-1}(c_{l1} + c_{ls})^2] + 2(rt)^{-1} \sum_{l,u} (c_{l1} e_{lu1} + c_{ls} e_{lus})$$

A.3.3 Expectation of the total sum of squares

$$E[SS_0] = E\left[\sum_{ijk} Y_{ijk}^2 - CF\right]$$

We will restrict our attention to the first term. Substituting the linear model, we have

$$E\left[\sum_{ijk} (\mu + r_i + t_j + \eta_{ij} + s_k + (ts)_{jk} + e_{ijk} + \gamma_{ijk})^2\right].$$

Squaring and summing yields

$$rts \mu^2 + ts \sum_{i=1}^r r_i^2 + rs \sum_{j=1}^t t_j^2 + rt \sum_{k=1}^s s_k^2 +$$

$$\sum_{ijk} E(\eta_{ij}^2) + \sum_{ijk} E(e_{ijk}^2) +$$

$$\sum_{ijk} E(\gamma_{ijk}^2) + \tag{A.3.19}$$

$$2\mu \sum_{ijk} E(\gamma_{ijk}) + 2 \sum_{ijk} E(r_i \gamma_{ijk}) + \tag{A.3.20}$$

$$2 \sum_{ijk} E(\eta_{ij} \gamma_{ijk}) + \tag{A.3.21}$$

$$2 \sum_{ijk} E(e_{ijk} \gamma_{ijk}) . \tag{A.3.22}$$

We know from Kempthorne (1952) that

$$\sum_{ijk} E(\eta_{ij}^2) = \sum_{l,u} (x_{lu} - x_{l..})^2, \text{ and}$$

$$\sum_{ikk} E(e_{ijk}^2) = \sum_{iuv} e_{iuv}^2 .$$

A.3.3.1 Terms A.3.19 and A.3.20

Term (A.3.19) is expanded to get

$$\sum_{i,j,k} E[(\delta_{iu1}^{jk} c_{i1} + \delta_{ius}^{jk} c_{is})^2]$$

which is in turn

$$\sum_{i,j,k} \sum_{u=1}^t (ts)^{-1} (c_{i1}^2 + c_{is}^2) = t \sum_{i=1}^r (c_{i1}^2 + c_{is}^2)$$

since $E[\delta_{iu}^{jk} \delta_{iu'}^{jk}] = 0$. It is easily seen that (A.3.20) is

$$2t \mu \sum_{i=1}^r (c_{i1} + c_{is}) + 2t \sum_{i=1}^r r_i (c_{i1} + c_{is}) .$$

A.3.3.2 Term A.3.21

We rewrite (A.3.21) as

$$\sum_{i,j,k} E[\sum_{u=1}^t \delta_{iu}^j w_{iu} \sum_{u=1}^t (\delta_{iu1}^{jk} c_{i1} + \delta_{ius}^{jk} c_{is})] ,$$

considering only

$$\sum_{i,j,k} E[\sum_{u=1}^t \delta_{iu}^j w_{iu} \sum_{u=1}^t \delta_{iu1}^{jk} c_{i1}]$$

where $w_{iu} = x_{iu} - x_{i..}$. The only non-zero terms of this product are those terms from the same whole plot u . Summing over u , we have

$$(ts)^{-1} \sum_{i,j,k} \sum_{u=1}^t c_{i1} w_{iu} = 0 .$$

A.3.3.3 Term A.3.22

The final term of (A.3.22) is expanded to the form

$$\begin{aligned}
 & 2 \sum_{i,j,k} E \left[\sum_{u,v} \delta_{iuv}^{jk} e_{iuv} \sum_{u=1}^t (\delta_{iu1}^{jk} c_{i1} + \delta_{ius}^{jk} c_{is}) \right] \\
 &= 2 \sum_{i,j,k} (ts)^{-1} \sum_{u=1}^t (c_{i1} e_{iu1} + c_{is} e_{ius}) \\
 &= 2 \sum_{i,u} (c_{i1} e_{iu1} + c_{is} e_{ius}) .
 \end{aligned}$$

Combining the previously derived expectations, the expectation of the total sum of squares is

$$ts \sum_{l=1}^r r_l^2 + rs \sum_{j=1}^t t_j^2 + s \sum_{i,u} w_{iu}^2 + rs \sum_{k=1}^s s_k^2 + r \sum_{j,k} (ts)_{jk}^2 + \sum_{i,u,v} e_{iuv}^2 + \varphi(c_{iv})$$

where $\varphi(c_{iv})$ is

$$t \left[\sum_{i=1}^r (c_{i1}^2 + c_{is}^2) - (rs)^{-1} \left(\sum_{i=1}^r (c_{i1} + c_{is}) \right)^2 \right] .$$

The sum of squares for error and its expectation is computed from the difference between the total sum of squares and all other sources of variability previously computed. This difference, divided by degrees of freedom, is in Table 4 .

A.4 The Latin square with Border Effects

A.4.1 Expectation of Treatment Sum of Squares

$$E[SS_{tr}] = E\left[\sum_{k=1}^t Y_{..k}^2 - CF\right]$$

We already have the expectation of the correction factor so we confine our attention to the first term. Substitution of the linear model followed by squaring and summing leads to

$$t^2\mu + t \sum_{k=1}^t t_k^2 +$$

$$t^{-1} \sum_{k=1}^t E\left[\left(\sum_{i,j} \delta_{iu}^k e_{ij}\right)^2\right] + \quad (A.4.1)$$

$$t^{-1} \sum_{k=1}^t E\left[\left(\sum_{i,j} \delta_{ij}^k c_j I_{\{1,t\}}(j)\right)^2\right] + \quad (A.4.2)$$

$$t^{-1} \sum_{k=1}^t E\left[2t\mu \sum_{i,j} \delta_{ij}^k c_j I_{\{1,t\}}(j)\right] + \quad (A.4.3)$$

$$t^{-1} \sum_{k=1}^t E\left[2\sum_{i,j} \delta_{ij}^k e_{ij} \sum_{i,j} \delta_{ij}^k c_j I_{\{1,t\}}(j)\right]. \quad (A.4.43)$$

We have from Kempthorne (1952) that (A.4.1) is

$$(t-1)^{-1} \sum_{i,j} e_{ij}^2.$$

A.4.1.1 Terms A.4.2 and A.4.3

This expression may be written as

$$t^{-1} \sum_{k=1}^t E[(\sum_{i,j} (\delta_{i1}^k c_{i1} + \delta_{it}^k c_{it}))^2] . \quad (A.4.5)$$

We note that

$$E[(\delta_{ij}^k)^2] = t^{-1} ,$$

$$E[(\delta_{ij}^k \delta_{i'j'}^k)] = [t(t-1)]^{-1}$$

for $i \neq i'$ and $j \neq j'$ with all other expectations being zero.

Using these relationships we have that (A.4.5) is

$$\begin{aligned} & t^{-1} \sum_{k,t} [t^{-1}(c_1^2 + c_t^2) + (t-1)\{t(t-1)\}^{-1}(2c_1 c_t)] \\ & = (c_1^2 + c_t^2 + 2c_1 c_t) = (c_1 + c_t)^2 . \end{aligned}$$

Passing the expectation through the summation, we see that (A.4.3) has expectation $2t \mu(c_1 + c_t)$.

A.4.1.2 Term A.4.4

The cross-product (A.4.4) is first simplified as was (A.4.2) to obtain

$$t^{-1} \sum_{k=1}^t E[\sum_{i,j} \delta_{ij}^k e_{ij} \sum_{l=1}^t \delta_{il}^k c_{l1} + \sum_{i,j} \delta_{ij}^k e_{ij} \sum_{l=1}^t \delta_{il}^k c_{lt}] \quad (A.4.6)$$

Looking at only the first half of the expression and using the probability structure of the design, we find its expectation to be

$$c_1 \sum_{i=1}^t t^{-1} [e_{i1} - \sum_{i' \neq i} e_{i'1} (t-1)^{-1}]$$

which equals

$$(t-1)^{-1} c_1 \sum_{i=1}^t e_{i1} = 0$$

since $\sum_{i=1}^t e_{ij} = 0$. Combining the above derivations and subtracting the correction factor, we have

$$E[SS_{trt}] = (t-1)^{-1} \sum_{i,j} e_{ij}^2 + t \sum_{k=1}^t t_k^2.$$

A.4.2 Expectation of Total Sum of Squares

$$E[SS_0] = E[\sum_{i,j,k} Y_{ijk}^2 - CF]$$

We consider only the first term which is expanded to

$$E \sum_{i,j,k} [\mu + \rho_i + \gamma_k + \sum_{k=1}^t \delta_{ij}^k t_k + e_{ij} + c_j I_{\{1,t\}}(j)]^2.$$

Squaring and summing, we have

$$t\mu^2 + t \sum_{i=1}^t \rho_i^2 + t \sum_{j=1}^t \gamma_j^2 + \sum_{i,j} e_{ij}^2 + t(c_1^2 + c_t^2) +$$

$$2t\mu(c_1 + c_t) + 2t(\gamma_1 c_1 + \gamma_t c_t) +$$

$$\sum_{i,j} E[(\sum_{k=1}^t \delta_{ij}^k t_k)^2]. \quad (A.4.7)$$

The expectation for (A.4.7) is relatively simple since $E[\delta_{ij}^k \delta_{ij}^{k'}]$ is nonzero only for $k = k'$ in which case its expectation is t^{-1} . Thus (A.4.7) is $t \sum_{k=1}^t t_k^2$. Combining the expectations and subtracting the correction factor leaves us with

$$t \sum_{i=1}^t \rho_i^2 + t \sum_{j=1}^t \gamma_j^2 + t \sum_{k=1}^t t_k^2 + \sum_{i,j} e_{ij}^2 + \varphi(c_{ij})$$

where

$$\varphi(c_{ij}) = (c_1^2 + c_t^2) - (c_1 + c_t)^2 + 2t(\gamma_1 c_1 + \gamma_t c_t)$$

The expectation of the error sum of squares is obtained by difference.

A.4.3 Variance of a Treatment Total

$$VAR(Y_{..k}) = E[(Y_{..k} - E(Y_{..k}))^2]$$

$$= E[(\sum_{i,j} \delta_{ij}^k e_{ij} + (c_1 + c_t) - (c_1 + c_t))^2]$$

$$= E[(\sum_{i,j} \delta_{ij}^k e_{ij})^2]$$

$$= (t - 1)^{-1} \sum_{i,j} e_{ij}^2$$

as shown in Kempthorne (1952).

A.4.4 Covariance of Two Treatment Totals

$$COV(Y_{..k}, Y_{..k'}) = E[(Y_{..k} - E(Y_{..k}))(Y_{..k'} - E(Y_{..k'}))]$$

We see from the previous work on the treatment that this is just

$$E[(\sum_{i,j} \delta_{ij}^k e_{ij})(\sum_{i,j} \delta_{ij}^{k'} e_{ij})]$$

which is shown by Kempthorne to be

$$- (t - 1)^{-2} \sum_{i,j} e_{ij}^2 .$$

A.5 *Semi-Latin Square with Shift*

A.5.1 Expectation of Treatment Sum of Squares

$$E[SS_m] = E[b^{-1} \sum_{l=1}^t Y_{..l}^2 - CF]$$

We consider only the initial term which can be written

$$E[b^{-1} \sum_{l=1}^t (b\mu + bt_l + \sum_{i,j,k} \delta_{ikj}^l e_{ijk} + \sum_{i,j,k} \delta_{ijk}^l (rs)_{ij} + c)^2]$$

Square and sum this expression to obtain

$$ab\mu^2 + b \sum_{l=1}^t t_l^2$$

$$E[b^{-1} \sum_{l=1}^t (\sum_{ijk} \delta_{ijk}^l e_{ijk})^2] + \quad (A.5.1)$$

$$E[b^{-1} \sum_{l=1}^t (\sum_{ijk} \delta_{ijk}^l (rs)_{ij})^2] + \quad (A.5.2)$$

$$E[b^{-1} \sum_{l=1}^t c^2] + E[b^{-1} \sum_{l=1}^t 2b\mu c] + \quad (A.5.3)$$

$$E[b^{-1} \sum_{l=1}^t 2act^{-1} \sum_{i,j} (rs)_{ij}] . \quad (A.5.4)$$

Rojas and White (1957) show (A.5.1) and (A.5.2) to be $b^{-1} \sum_{ijk} e_{ijk}^2$ and $a(b-1)^{-1} \sum_{i,j} (rs)_{ij}^2$ respectively.

Simple algebra reduces (A.5.3) to $ac^2 + 2abc\mu$. Expression (A.5.4) is zero since $\sum_{i,j} (rs)_{ij} = 0$.

Combining terms and subtracting the correction factor yields

$$E[SS_{Tt}] = b \sum_{l=1}^t t_l^2 + b^{-1} \sum_{i,j} e_{ijk}^2 + a(b-1)^{-1} \sum_{i,j} (rs)_{ij}^2 .$$

A.5.2 Expectation of the Total Sum of Squares

$$E[SS_0] = E[\sum_{ijkl} Y_{ijkl}^2 - CF]$$

We consider only the initial term which may be written

$$E[\sum_{ikj} (\mu + r_i + s_j + (rs)_{ij} + e_{ijk} + c_j I_{\{1\}}(j) + \sum_{l=1}^t \delta_{ijk}^l t_l)^2] .$$

Square and sum this expression to obtain

$$ab^2\mu^2 + ab \sum_{i=1}^b r_i^2 + ab \sum_{j=1}^b s_j^2 + a \sum_{i,j} (rs)_{ij}^2 + \sum_{i,j,k} e_{ijk}^2 +$$

$$abc^2 + 2abcs_1 + 2abc\mu +$$

$$E \left[\sum_{i,j,k} \left(\sum_{l=1}^t \delta_{ijk}^l t_l \right)^2 \right] + \quad (A.5.5)$$

$$E \left[\sum_{i,j,k} 2c I_{(1)}(j) \sum_{l=1}^t \delta_{ikj}^l t_l \right]. \quad (A.5.6)$$

Noting that $E[\delta_{ijk}^l \delta_{i'jk}^{l'}]$ is nonzero only for $l = l'$, we find (A.5.5) to be $b \sum_{l=1}^t t_l^2$. Expression (A.5.6) has expectation

$$\sum_{i,j,k} 2c I_{(1)}(j) t^{-1} \sum_{l=1}^t t_l = 0.$$

Thus after combining terms and subtracting the correction factor, the expectation of the total sum of squares is

$$ab \sum_{i=1}^b r_i^2 + ab \sum_{j=1}^b s_j^2 + a \sum_{i,j} (rs)_{ij}^2 + \sum_{j,k} e_{ijk}^2 + b \sum_{l=1}^t t_l^2 + a(b-1)c^2 + 2abcs_1.$$

The expectation for the error sum of squares is obtained by difference.

A.6 Two Column Design without Border Shifts

A.6.1 Expectation for Treatment Sum of Squares

$$E[SS_{tr}] = E[b^{-1} \sum_{l=1}^t Y_{...l}^2 - CF]$$

We consider only the initial term which may be written as

$$E[b^{-1} \sum_{l=1}^t (b\mu + \sum_{i,j,k} \delta_{ijk}^l (rs)_{ij} + \sum_{i,j,k} \delta_{ijk}^l e_{ijk} + bt_l)^2] .$$

Square and sum to obtain

$$bt\mu^2 + b \sum_{l=1}^t t_l^2$$

$$E[b^{-1} \sum_{l=1}^t (\sum_{i,j,k} \delta_{ijk}^l (rs)_{ij})^2] + \tag{A.6.1}$$

$$E[b^{-1} \sum_{l=1}^t (\sum_{i,j,k} \delta_{ijk}^l e_{ijk})^2] + \tag{A.6.2}$$

$$E[2b^{-1} \sum_{l=1}^t (\sum_{i,j,k} \delta_{ijk}^l (rs)_{ij})(\sum_{i,j,k} \delta_{ijk}^l e_{ijk})] . \tag{A.6.3}$$

To obtain the expectations of A.6.1 - A.6.3, we need the probability structure of the δ_{ijk}^l . The required probabilities are:

$$P\{\delta_{ijk}^l = 1\} = t^{-1} ,$$

$$P\{\delta_{ijk}^l = 1, \delta_{ijk'}^l = 1\} = 0 \text{ for } (k \neq k') ,$$

$$P \{ \delta_{ijk}^l = 1, \delta_{i'j'k'}^l = 1 \} = 0 \text{ for } (j \neq j') ,$$

$$P \{ \delta_{i1k}^l = 1, \delta_{i'1k'}^l = 1 \} = 0 \text{ for } (i \neq i') ,$$

$$P \{ \delta_{i2k}^l = 1, \delta_{i'2k'}^l = 1 \} = (t-4)[t(t-2)]^{-1} \text{ for } (i \neq i') , \text{ and}$$

$$P \{ \delta_{ijk}^l = 1, \delta_{i'j'k'}^l = 1 \} = [t(t-2)]^{-1} \text{ for } (i \neq i') , (j \neq j') .$$

Turning first to (A.6.1), we find it has expectation

$$b^{-1} \sum_{l,i} \{ n_{i1}(rs)_{i1} [t^{-1}(rs)_{i1} + (t(t-2))^{-1} \sum_{l' \neq l} n_{i'2}(rs)_{l'2}] \} +$$

$$b^{-1} \sum_{l,i} \{ n_{i2}(rs)_{i2} [t^{-1}(rs)_{i2} + (t(t-2))^{-1} \sum_{l' \neq l} (n_{i'1}(rs)_{l'1} + (t-2)^{-1}(t-4)n_{i'2}(rs)_{l'2})] \} .$$

A healthy dose of intricate algebra leads us from the above expression to

$$t[b(t-2)]^{-1} \sum_{i,j} n_{ij}(rs)_{ij}^2 .$$

A.6.2 Terms A.6.2 and A.6.3

We use the fact that $\sum_{k=1}^{n_{ij}} e_{ijk} = 0$ to see that all non-zero cross-product expectations sum to zero for expression (A.6.2). Thus its expectation is $b^{-1} \sum_{i,j,k} e_{ijk}^2$.

The term (A.6.3) has expectation zero. For each (ij) combination in which $E[\delta_{ikj} \delta_{i'j'k'}] > 0$, the $(rs)_{ij}$ term is a constant. We then have a multiple of $(rs)_{ij} \sum_k e_{ijk}$ which is zero. When faced with $E[\delta_{ikj} \delta_{i'j'k'}]$, we have non-zero expectation only for $k = k'$. The $(rs)_{ij}$ is again a constant and we will have $\sum_{l,j} (rs)_{ij} \sum_k e_{ijk} = 0$.

Combining terms we get

$$E[SS_{rrt}] = b^{-1} \sum_{ijk} e_{ijk}^2 + b \sum_{l=1}^t t_l^2 + t[b(t-2)]^{-1} \sum_{i,j} n_{ij} (rs)_{ij}^2$$

A.6.3 Expectation of Total Sum of Squares

$$E[SS_0] = E[\sum_{ijk} Y_{ijk}^2 - CF]$$

We consider only the first term which can be written

$$E[\sum_{ijk} (\mu + r_i + s_j + (rs)_{ij} + e_{ijk} + \sum_{l=1}^t \delta_{ijk}^l t_l)^2]$$

Square and sum to obtain

$$bt\mu^2 + t \sum_{i=1}^b r_i^2 + \sum_{j=1}^2 n_{.j} s_j^2 + \sum_{i,j} n_{ij} (rs)_{ij}^2 + \sum_{ijk} e_{ijk}^2 + \sum_{ijk} E[(\sum_{l=1}^t \delta_{ijk}^l t_l)^2] \quad (A.6.4)$$

The $E[\delta_{ijk}^l \delta_{ijk}^{l'}] = t^{-1}$ for $l = l'$ and is zero otherwise; thus (A.6.4) is $b \sum_{l=1}^t t_l^2$.

Combining terms and subtracting the correction factor, we have

$$E[SS_0] = t \sum_{i=1}^b r_i^2 + \sum_{j=1}^2 n_{.j} s_j^2 + \sum_{i,j} n_{ij} (rs)_{ij}^2 + \sum_{ijk} e_{ijk}^2 + b \sum_{l=1}^t t_l^2$$

The expectation of the error sum of squares is obtained by subtraction.

A.7 Two Column Design with Border Shift

A.7.1 Expectation of Treatment Sum of Squares

$$E[SS_{tr}] = E[b^{-1} \sum_{l=1}^t Y_{...l}^2 - CF]$$

We will consider only the initial term which we write as

$$E[b^{-1} \sum_{l=1}^t (b\mu + \sum_{ijk} \delta_{ijk}^l (rs)_{ij} + \sum_{ijk} e_{ijk} + bt_l + c)^2] .$$

Square and sum to obtain

$$bt\mu^2 + b \sum_{l=1}^t t_l^2 + 2c^2 + 4bc\mu +$$

$$E[b^{-1} \sum_{l=1}^t (\sum_{ijk} \delta_{ijk}^l (rs)_{ij} + \sum_{ijk} \delta_{ijk}^l e_{ijk})^2] + \quad (A.7.1)$$

$$E[b^{-1} \sum_{l=1}^t 2c \sum_{ijk} \delta_{ijk}^l (rs)_{ij}] + \quad (A.7.2)$$

$$E[b^{-1} \sum_{l=1}^t 2c \sum_{ijk} \delta_{ijk}^l e_{ijk}] . \quad (A.7.3)$$

We know from Appendix that (A.7.1) has expectation

$$t[b(t-2)]^{-1} \sum_{i,j} n_{ij} (rs)_{ij}^2 + b^{-1} \sum_{ijk} e_{ijk}^2 .$$

Since $E[\delta'_{ijk}] = t^{-1}$ and $\sum_{i,j} (rs)_{ij} = \sum_{ijk} e_{ijk} = 0$, expressions (A.7.2) and (A.7.3) have expectation zero. Collecting terms and subtracting the correction factor gives us the expectation of treatment sum of squares as

$$b^{-1} \sum_{ijk} e_{ijk}^2 + \{b(t-2)\}^{-1} \sum_{i,j} n_{ij} (rs)_{ij}^2 + b \sum_{l=1}^t t_l^2 .$$

A.7.2 Expectation of Total Sum of Squares

$$E[SS_0] = E[\sum_{ijk} Y_{ijk}^2 - CF] .$$

We consider only the initial term which we write as

$$E[\sum_{ijk} (\mu + r_i + s_j + (rs)_{ij} + e_{ijk} + \sum_{l=1}^t \delta'_{ijk} t_l + c I_{\{1\}}(j))^2] .$$

Square and sum to obtain

$$bt\mu^2 + t \sum_{i=1}^b r_i^2 + \sum_{j=1}^2 n_{.j} s_j^2 + \sum_{i,j} n_{ij} (rs)_{ij}^2 + \sum_{ijk} e_{ijk}^2 +$$

$$2bc^2 + 4bc\mu + 4bcs_1 +$$

$$E[\sum_{ijk} (\sum_{l=1}^t \delta'_{ijk} t_l)^2] + \tag{A.7.4}$$

$$E[\sum_{ijk} c \sum_{l=1}^t \delta'_{ijk} t_l] . \tag{A.7.5}$$

We saw in Appendix 6 that (A.7.4) has expectation $b \sum_{l=1}^t t_l^2$. Since $E[\delta'_{ijk}] = t^{-1}$, expression (A.7.5) is

$$E\left[\sum_{ijk} ct^{-1} \sum_{l=1}^t t_{il}\right] = 0 .$$

Combining terms and subtracting the correction factor results in

$$E[SS_0] = t \sum_{i=1}^b r_i^2 + \sum_{j=1}^2 n_{.j} s_j^2 + \sum_{i,j} n_{ij} (rs)_{ij}^2 + \sum_{ijk} e_{ijk}^2 + \sum_{l=1}^t t_l^2 + \varphi(c)$$

where

$$\varphi(c) = 2(b-1)c^2 + 4bcs_1 .$$

The expectation for the error sum of squares is obtained by difference.

A.8 Split Plot with Subplots in a Latin Square

The new randomization scheme does not affect the application of whole plot treatments to the whole plot units. As a result, the whole plot expected mean squares are unchanged and we will look at only the subplot sums of squares, particularly the subplot treatment and the subplot treatment by replication interaction. The column and whole plot by subplot treatment interaction are confounded and will not be examined.

$$E[SS_s] = E\left[(rs)^{-1} \sum_{i=1}^r Y_{...i}^2 - CF\right]$$

The correction factor has expectation

$$rs^2 \mu^2 + r \left[\sum_{i=1}^r (c_{i1} + c_{is}) \right]^2 + 2s \mu \sum_{i=1}^r (c_{i1} + c_{is}) .$$

A.8.1 Expectation of Subplot Treatment Sum of Squares

Now we need the expectation of the initial term of the treatment sum of squares. We write this term as

$$E[(rs)^{-1} \sum_{l=1}^s (rs\mu + rs(s_l) + \sum_{i=1}^r (c_{i1} + c_{is}) + \sum_{ikuj} \delta_{iuj}^{kl} e_{iuj})^2] .$$

Square and sum to obtain

$$rs^2\mu^2 + rs \sum_{l=1}^s s_l^2 + r^{-1} \left[\sum_{l=1}^r (c_{l1} + c_{ls}) \right]^2 + 2s\mu \sum_{l=1}^r (c_{l1} + c_{ls}) +$$

$$E[(rs)^{-1} \sum_{l=1}^s (\sum_{ikuj} \delta_{iuj}^{kl} e_{iuj})^2] + \quad (A.8.1)$$

$$E[2(rs)^{-1} \sum_{l=1}^s (\sum_{i=1}^r (c_{i1} + c_{is})) (\sum_{ikuj} \delta_{iuj}^{kl} e_{iuj})] . \quad (A.8.2)$$

Expression (A.8.2) will have expectation zero since $E(\delta_{iuj}^{kl}) = s^{-2}$, leaving us with $s^{-2} \sum_{iuj} e_{iuj} = 0$.

The probability structure for the δ_{iuj}^{kl} is necessary to obtain the expectation in (A.8.1). This structure is that for a Latin square; i.e.,

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{iuj'} = 1 \} = 0 \text{ for } j \neq j' ,$$

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{i'u'}^{kl} = 1 \} = 0 \text{ for } u' \neq u ,$$

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{i'u'}^{k'l'} = 1 \} = s^{-2} \text{ for } i \neq i' ,$$

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{i'u'}^{k'l'} = 1 \} = 0 \text{ for } k \neq k' \text{ and } j \neq j' ,$$

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{i'u'}^{k'l'} = 1 \} = 0 \text{ for } k \neq k' \text{ and } u \neq u' ,$$

$$P \{ \delta_{iuj}^{kl} = 1, \delta_{iu'j'}^{k'l'} = 1 \} = [s(s-1)]^{-2} \text{ for } k \neq k', u \neq u', \text{ and } j \neq j' .$$

Thus terms of the form $E [\delta_{iuj}^{kl} e_{iuj} \sum_{u',j'} \delta_{iu'j'}^{k'l'} e_{iu'j'}] = 0$ and terms of the form

$$E [(\delta_{iuj}^{kl})^2 e_{iuj}^2] = s^{-2} e_{iuj}^2$$

have non-zero expectations within a whole plot. The remaining terms sum to

$$[r(s-1)]^{-1} \sum_{iuj} e_{iuj}^2 .$$

Combining terms and subtracting the correction factor yields

$$E[SS_S] = rs \sum_{l=1}^s s_l^2 + r(s-1) \sum_{iuj} e_{iuj}^2 .$$

A.8.2 Expectation of Replication by Subplot Treatment Sum of Squares

$$E [s^{-1} \sum_{l,l'} Y_{l,l'}^2 - s^{-2} \sum_{l=1}^r Y_{l,\dots}^2 - (rs)^{-1} \sum_{l=1}^s Y_{\dots,l}^2 + CF]$$

All the terms but the first are available elsewhere so we look only at this term. It may be written as

$$E [s^{-1} \sum_{l,l'} (s\mu + sr_l + s(s_l) + (c_{il} + c_{ls}) + \sum_{k,u,j} \delta_{iuj}^{kl} e_{iuj})^2] .$$

Square and sum to obtain

$$rs^2 \mu^2 + s^2 \sum_{l=1}^r r_l^2 + rs \sum_{l=1}^s s_l^2 +$$

$$\sum_{i=1}^r (c_{i1} + c_{is})^2 + 2s\mu \sum_{i=1}^r (c_{i1} + c_{is}) + 2s \sum_{l=1}^r r_l (c_{i1} + c_{is}) +$$

$$E [s^{-1} \sum_{i,l} (\sum_{k,u,j} \delta_{iuj}^{kl} e_{iuj})^2] . \quad (A.8.3)$$

The expectation of (A.8.3) involves the probabilities listed previously for this design. This expectation is nearly identical to that for (A.8.1) since the cross-product terms involving i and i' summed to zero before. Therefore we find (A.8.3) to be

$$(s-1)^{-1} \sum_{iuj} e_{iuj}^2 ,$$

and by combining terms, we have $E [s^{-1} \sum_{i=1}^s Y_{i..}^2]$ as

$$rs^2\mu^2 + s^2 \sum_{i=1}^r r_i^2 + rs \sum_{l=1}^s s_l^2 + (s-1)^{-1} \sum_{iuj} e_{iuj}^2 .$$

Subtracting the remaining terms of $E [SS_{rs}]$ gives us

$$E [SS_{rs}] = (r-1)[r(s-1)]^{-1} \sum_{iuj} e_{iuj}^2 .$$

Appendix B

B. SIMULATION PROGRAMS

B.1 Simulation to Compare Power and MSE when $T=4$

```
*      T=4  B=2 (2 TRTS/(ROW*COL) FOR SLS )      ;  
*                                                    ;  
*      DAVID D. MORRIS      MAY 1986      ;  
*                                                    ;
```

```
OPTIONS NOSOURCE;
```

```
DATA ONE;                                /* DATA FOR RCBD AND 2-COL*/
```

```
DO I = 1 TO 2;
```

```
DO J = 1 TO 4;
```

```
    IF J = 1 OR J = 2 THEN K = 1; ELSE K = 2;
    OUTPUT;
END;
END;
```

```
DATA COL2;
```

```
    INPUT START;
```

```
    CNT = _N_;
```

```
    CARDS;
```

```
1
```

```
2
```

```
3
```

```
4
```

```
3
```

```
4
```

```
1
```

```
2
```

```
PROC MATRIX;
```

```
    FETCH BASE DATA=COL2 (KEEP = CNT);
```

```
    FETCH TT DATA=ONE (KEEP = J);
```

```
    FETCH START DATA=COL2 (KEEP = START);
```

```
    FETCH BK DATA=ONE (KEEP = I);
```

```
    FETCH CL DATA=ONE (KEEP = K);
```

```
    CRIT = 9.28 19.16;
```

```
    BF1 = 0; BF2 = 1;
```

```

SHIFT = 3;                /* <= CHANGE SHIFT */
TF1 = 3;                 /* <= CHANGE TRT EFFECT */

N = 8;
B = 2;
T = 4;

STR = 1 / 2 ;

BULGE = J(N,1,0) + (BK = 2)#2 ;

BULGE2 = J(N,1,0) + (CL = 1)#((BK = 1)#2 + (BK = 2)#6) +
          (CL = 2)#((BK = 1)#4 + (BK = 2)#8);

BLK = DESIGN(BK);
COL2 = DESIGN(CL);
TTL = J(N,1,0);
BINV = I(B) #/ T;
TINV = I(T) #/ B;
C2INV = INV(COL2'*COL2);
QB = BLK*BINV*BLK';
QC = COL2*C2INV*COL2';
INT = J(N,1,1);
DE = (B + 1)*T;
FO = J(50,2,0);          /* <= CHANGE WITH BOUND */
MO = J(50,2,0);

```

```
DO O = 1 TO 50;
```

```
F = J(100,2,0);
```

```
ME = J(100,2,0);
```

```
DO Z = 1 TO 100;
```

```
R = J(DE,2,0);
```

```
DO M = 1 TO DE;          /* GENERATE RANDOM NOS */
```

```
R(M,1) = NORMAL(0);
```

```
R(M,2) = UNIFORM(0);
```

```
END;
```

```
Y0 = R(1:N,1) + (BK=2)#BF2 + (CL=1)#SHIFT;
```

```
*****;
```

```
*          TRY RCBD          ;
```

```
*****;
```

```
INDEX = RANK(BULGE + R(1:N,2));    /* PERMUTE TRTMNTS */
```

```
TR = TT;
```

```
TT(INDEX,) = TR;
```

```
TRT = DESIGN(TT);
```

Y = Y0 + (TT=1)#TF1;

CF = ((Y'*INT)#(Y'*INT)) #/ N;

SST = Y'*TRT*TINV*TRT'*Y - CF;

SSB = Y'*QB*Y - CF; /* COMPUTE F RATIO */

SSE = Y'*Y - CF - SST - SSB;

MST = SST #/ (T-1);

MSE = SSE #/ (N-B-T + 1);

F(Z,1) = MST #/ MSE;

ME(Z,1) = MSE;

```
*****  
*           TRY 2 COLUMN           ;  
*****;
```

TRT = J(N,1,0);

INDEX2 = RANK(R(5:12,2) + BULGE2); /* PERMUTE TRTS IN */

TMP1 = START; /* THE BLOCKS */

START(INDEX2,) = TMP1;

IN1 = RANK(R(5:6,2)); /* PERMUTE ROWS */

CHG = (STR - IN1)#(-4);

CHANGE = BLK*CHG ;

SORT = BASE + CHANGE;

TRT(SORT,) = START;

TRTS = DESIGN(TRT); /* ADD TRT EFFECTS */

Y = Y0 + (TRT=1)#TF1 ;

CF = ((Y'*INT)#(Y'*INT)) #/ N; /* COMPUTE SUMS OF SQS */

SST = Y'*TRTS*TINV*TRTS'*Y - CF;

SSB = Y'*QB*Y - CF;

SSC = Y'*QC*Y - CF;

SSE = Y'*Y - CF - SST - SSB - SSC;

MST = SST #/ (T-1); /* COMPUTE F RATIO */

MSE = SSE #/ (N-B-T);

F(Z,2) = MST #/ MSE;

ME(Z,2) = MSE ;

F(Z,) = F(Z,) > CRIT;

END;

FO(O,) = F(+,);

MO(O,) = ME(.,);

END;

MALPHA = FO(.,);

ALPHA2 = FO(##,);

```

MMSE = MO(.);
MSE2 = MO(##);
NOTE COLUMNN = > RCBD, COLUMN 2 = > 2COL, T=4, SHIFT=3, TE = 3;
PRINT FO MALPHA ALPHA2 MMSE MSE2;

```

B.2 Simulation to Compare Power and MSE when T=6

```

*      T=6  B=3 (2 TRTS/(ROW*COL) FOR SLS )      ;
*
*
*      DAVID D. MORRIS      MAY 1986      ;
*

```

```

OPTIONS NOSOURCE;

```

```

DATA ONE;                                /* DATA FOR RCBD AND 2-COL*/

```

```

  DO I = 1 TO 3;
    DO J = 1 TO 6;
      IF J = 1 OR J = 2 THEN K = 1; ELSE K = 2;
      OUTPUT;
    END;
  END;
END;

```

```

DATA COL2;

```

```

  INPUT START;

```

```

CARDS;
1
2
3
4
5
6
3
4
1
2
5
6
5
6
1
2
3
4
DATA PLANS;                                /* THE TWO SET OF SQUARES */
INPUT D1 D2;                                /* FROM ROJAS AND WHITE */
CNT = _N_;
CARDS;
1 1
2 4
3 2
4 6
5 3

```

6 5

5 3

6 6

1 1

2 5

3 2

4 4

3 2

4 5

5 3

6 4

1 1

2 6

```
DATA TWO (KEEP = C1-C3);           /* CREATE A MATRIX TO */
  ARRAY CC (K) C1-C3;             /* EXPAND COLUMNS LATER */
  DO I = 1 TO 3;
    DO J = 1 TO 6;
      IN = CEIL(J/2);
      DO K = 1 TO 3;
        CC=0;
        IF K = IN THEN CC = 1;
      END;
    OUTPUT;
  END;
END;
```

```
DATA THREE (KEEP = B1-B3);        /* CREATE A MATRIX TO */
  ARRAY BB (J) B1-B3;             /* EXPAND ROWS LATER */
```

```

DO I = 1 TO 18;
  IN = CEIL(I/6);
  DO J = 1 TO 3;
    BB=0;
    IF J = IN THEN BB = 1;
  END;
  OUTPUT;
END;

```

```

DATA FOUR (KEEP = C1-C9);
  ARRAY CC (K) C1-C9;
  DO I = 1 TO 9;
    DO J = 1 TO 2;
      DO K = 1 TO 9;
        CC = 0;
        IF K = I THEN CC = (J=1) - (J=2);
      END;
      OUTPUT;
    END;
  END;
END;

```

```

PROC MATRIX;

```

```

  FETCH D DATA=PLANS (DROP = CNT);    /* GET EXISTING WORK */
  FETCH BASE DATA=PLANS (KEEP = CNT);
  FETCH TB DATA=THREE;
  FETCH TC DATA=TWO;

```

FETCH TBC DATA=FOUR;
FETCH TT DATA=ONE (KEEP = J);
FETCH START DATA=COL2;
FETCH BK DATA=ONE (KEEP = I);
FETCH CL DATA=ONE (KEEP = K);

CRIT = 3.33 3.48 3.69;

BF1 = 0; BF2 = 1; BF3 = 2;

SHIFT = 3; /* <= CHANGE SHIFT */

TF1 = 3; /* <= CHANGE TRT EFFECT */

N = 18;

B = 3;

T = 6;

STR = 1 / 2 / 3;

BULGE = J(N,1,0) + (BK=2)#2 + (BK=3)#4;

BULGE2 = J(N,1,0) + (CL=1)#((BK=1)#2 + (BK=2)#6 + (BK=3)#10) +
(CL=2)#((BK=1)#4 + (BK=2)#8 + (BK=3)#12);

BLK = DESIGN(BK);

COL2 = DESIGN(CL);

TTL = J(N,1,0);

BINV = I(B) #/ T;

```

TINV = I(T) #/ B;
CINV = I(B) #/ T;
C2INV = INV(COL2'*COL2);
QB = BLK*BINV*BLK';
QC = COL2*C2INV*COL2';
INT = J(N,1,1);
DE = (B + 1)*T;
FO = J(50,3,0);           /* < = CHANGE WITH BOUND */
MO = J(50,3,0);

DO O = 1 TO 50;

    F = J(100,3,0);
    ME = J(100,3,0);

    DO Z = 1 TO 100;

        R = J(DE,2,0);

        DO M = 1 TO DE;           /* GENERATE RANDOM NOS */
            R(M,1) = NORMAL(0);
            R(M,2) = UNIFORM(0);
        END;

        Y0 = R(1:N,1) + (BK = 2)#BF2 + (BK = 3)#BF3 + (CL = 1)#SHIFT;

```

```

*****;
*          TRT RCBD                               ;
*****;

INDEX = RANK(BULGE + R(1:N,2));    /* PERMUTE TRTMNTS */
TR = TT;
TT(INDEX,) = TR;

TRT = DESIGN(TT);

Y = Y0 + (TT=1)#TF1;

CF = ((Y'*INT)#(Y'*INT)) #/ N;
SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*QB*Y - CF;                /* COMPUTE F RATIO */
SSE = Y'*Y - CF - SST - SSB;
MST = SST #/ (T-1);
MSE = SSE #/ (N-B-T+1);
F(Z,1) = MST #/ MSE;
ME(Z,1) = MSE;

```

```

*****;
*          TRY 2 COLUMN                           ;
*****;

```

```

TRT = J(N,1,0);

INDEX2 = RANK(R(7:24,2) + BULGE2);  /* PERMUTE TRTS IN */
TMP1 = START;                        /* THE BLOCKS */
START(INDEX2,) = TMP1;

IN1 = RANK(R(5:7,2));                /* PERMUTE ROWS */
CHG = (STR - IN1)#(-6);
CHANGE = BLK*CHG ;
SORT = BASE + CHANGE;
TRT(SORT,) = START;

TRTS = DESIGN(TRT);                  /* ADD TRT EFFECTS */
Y = Y0 + (TRT=1)#TF1 ;

CF = ((Y'*INT)#(Y'*INT)) #/ N;      /* COMPUTE SUMS OF SQS */
SST = Y'*TRTS*TINV*TRTS*Y - CF;
SSB = Y'*QB*Y - CF;
SSC = Y'*QC*Y - CF;
SSE = Y'*Y - CF - SST - SSB - SSC;

MST = SST #/ (T-1);                 /* COMPUTE F RATIO */
MSE = SSE #/ (N-B-T);
F(Z,2) = MST #/ MSE;
ME(Z,2) = MSE ;

```

```

*****;
*           TRY SLS           ;
*****;

```

```

U0 = UNIFORM(0);           /* PICK A PLAN AT RANDOM */
IF U0 LE 0.5 THEN TT1 = D(,1);
      ELSE TT1 = D(,2);

```

```

IN1 = RANK(R(1:B,2));      /* PERMUTE TRTS BY ROWS */
CHG = (STR - IN1)#(-6);
CHANGE = TB*CHG;
SORT = BASE + CHANGE;
TMP1 = TT1;
TTL(SORT,) = TMP1;

```

```

IN1 = RANK(R(22:24,2));    /* PERMUTE TRTS BY COLS */
CHG = (STR - IN1)#(-2);
CHANGE = TC*CHG;
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;
BIGC = TC*IN1;

```

```

IN2 = R(1:9,2) > 0.5;          /* PERMUTE TREATMENTS */
CHANGE = TBC*IN2;             /* WITHIN ROW-COL COMBO */
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;

```

```

Y = Y0 +
    (TTL=1)*TF1;

```

```

/* FORM DESIGN MATRICES */

```

```

COL = DESIGN(BIGC);
TRT = DESIGN(TTL);

```

```

CF = ((Y'*INT)#(Y'*INT))/N;    /* COMPUTE SUMS OF SQUARES */
SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*BLK*BINV*BLK'*Y - CF;
SSC = Y'*COL*CINV*COL'*Y - CF;
SSE = (Y'*Y - CF) - SST - SSC - SSB;

```

```

MST = SST #/ (T-1);           /* COMPUTE F RATIO */
MSE = SSE #/ (N-2*B - T + 2);
F(Z,3) = MST #/ MSE;

```

```
ME(Z,3) = MSE;
F(Z,) = F(Z,) > CRIT;
```

```
END;
```

```
FO(O,) = F(+,);
MO(O,) = ME(.,);
```

```
END;
```

```
MALPHA = FO(.,);
ALPHA2 = FO(##,);
MMSE = MO(.,);
MSE2 = MO(##,);
NOTE COL 1 = > RCBD, COL 2 = > 2COL, COL 3 = > SLS. T=6, SHIFT=3, TE=3;
PRINT FO MALPHA ALPHA2 MMSE MSE2;
```

B.3 Simulation to Obtain the Empirical 95th Percentile

when T = 6

```
*                               ;
*   T=6  B=3 (2 TRTS/(ROW*COL) FOR SLS)   ;
*                               ;
*   DAVID D. MORRIS      MAY 1986      ;
*                               ;
```

```
DATA ONE;                                /* DATA FOR RCBD AND 2-COL*/
  DO I = 1 TO 3;
    DO J = 1 TO 6;
      IF J = 1 OR J = 2 THEN K = 1; ELSE K = 2;
      OUTPUT;
    END;
  END;
```

```
DATA COL2;
  INPUT START;
  CARDS;
```

```
1
2
3
4
5
6
3
4
1
2
5
6
5
```

6

1

2

3

4

DATA PLANS;

/* THE TWO SET OF SQUARES */

INPUT D1 D2;

/* FROM ROJAS AND WHITE */

CNT = _N_;

CARDS;

1 1

2 4

3 2

4 6

5 3

6 5

5 3

6 6

1 1

2 5

3 2

4 4

3 2

4 5

5 3

6 4

1 1

2 6

DATA TWO (KEEP = C1-C3);

/* CREATE A MATRIX TO */

```

ARRAY CC (K) C1-C3;           /* EXPAND COLUMNS LATER */
DO I = 1 TO 3;
  DO J = 1 TO 6;
    IN = CEIL(J/2);
    DO K = 1 TO 3;
      CC=0;
      IF K = IN THEN CC = 1;
    END;
  OUTPUT;
END;
END;

```

```

DATA THREE (KEEP = B1-B3);    /* CREATE A MATRIX TO */
ARRAY BB (J) B1-B3;          /* EXPAND ROWS LATER */
DO I = 1 TO 18;
  IN = CEIL(I/6);
  DO J = 1 TO 3;
    BB=0;
    IF J = IN THEN BB = 1;
  END;
  OUTPUT;
END;

```

```

DATA FOUR (KEEP = C1-C9);
ARRAY CC (K) C1-C9;
DO I = 1 TO 9;
  DO J = 1 TO 2;
    DO K = 1 TO 9;

```

```

    CC = 0;
    IF K = I THEN CC = (J = 1) - (J = 2);
END;
OUTPUT;
END;
END;

```

PROC MATRIX;

```

    FETCH D DATA=PLANS (DROP = CNT);    /* GET EXISTING WORK */
    FETCH BASE DATA=PLANS (KEEP = CNT);
    FETCH TB DATA=THREE;
    FETCH TC DATA=TWO;
    FETCH TBC DATA=FOUR;
    FETCH TT DATA=ONE (KEEP = J);
    FETCH START DATA=COL2;
    FETCH BK DATA=ONE (KEEP = I);
    FETCH CL DATA=ONE (KEEP = K);

```

```

    SHIFT = 2;                /* < = SET SHIFT */
    TF1 = 0;                  /* < = SET TRT EFFECT */
    BF1 = 0; BF2 = 1; BF3 = 2;

```

```

    N = 18;
    B = 3;
    T = 6;

```

STR = 1 / 2 / 3;

BULGE = J(N,1,0) + (BK = 2)#2 + (BK = 3)#4;

BULGE2 = J(N,1,0) + (CL = 1)#((BK = 1)#2 + (BK = 2)#6 + (BK = 3)#10) +
(CL = 2)#((BK = 1)#4 + (BK = 2)#8 + (BK = 3)#12);

BLK = DESIGN(BK);

COL2 = DESIGN(CL);

TTL = J(N,1,0);

BINV = I(B) #/ T;

TINV = I(T) #/ B;

CINV = I(B) #/ T;

C2INV = INV(COL2*COL2);

QB = BLK*BINV*BLK';

QC = COL2*C2INV*COL2';

INT = J(N,1,1);

DE = (B + 1)*T;

FO = J(50,3,0); /* < = CHANGE WITH BOUND */

MO = J(50,3,0);

R = J(DE,2,0);

DO M = 1 TO DE; /* GENERATE RANDOM NOS */

R(M,1) = NORMAL(0);

END;

```
DO O = 1 TO 50;
```

```
F = J(100,3,0);
```

```
ME = J(100,3,0);
```

```
DO Z = 1 TO 100;
```

```
DO M = 1 TO DE;          /* GENERATE RANDOM NOS */
```

```
  R(M,2) = UNIFORM(0);
```

```
END;
```

```
Y0 = R(1:N,1) + (BK = 2)#BF2 + (BK = 3)#BF3 + (CL = 1)#SHIFT;
```

```
*****;
```

```
*          TRY RCBD          ;
```

```
*****;
```

```
INDEX = RANK(BULGE + R(1:N,2));  /* PERMUTE TRTMNTS */
```

```
TR = TT;
```

```
TT(INDEX,) = TR;
```

```
TRT = DESIGN(TT);
```

```
Y = Y0 + (TT = 1)#TF1;
```

```

CF = ((Y'*INT)#(Y'*INT)) #/ N;
SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*QB*Y - CF;          /* COMPUTE F RATIO */
SSE = Y'*Y - CF - SST - SSB;
MST = SST #/ (T-1);
MSE = SSE #/ (N-B-T + 1);
F(Z,1) = MST #/ MSE;
ME(Z,1) = MSE;

```

```

*****;
*           TRY 2 COLUMN           ;
*****;

```

```

TRT = J(N,1,0);

INDEX2 = RANK(R(7:24,2) + BULGE2);  /* PERMUTE TRTS IN */
TMP1 = START;                      /* THE BLOCKS */
START(INDEX2,) = TMP1;

IN1 = RANK(R(5:7,2));              /* PERMUTE ROWS */
CHG = (STR - IN1)#(-6);
CHANGE = BLK*CHG ;
SORT = BASE + CHANGE;
TRT(SORT,) = START;

```

```

TRTS = DESIGN(TRT);          /* ADD TRT EFFECTS */
Y = Y0 + (TRT = 1)#TF1 ;

CF = ((Y'*INT)#(Y'*INT)) #/ N;    /* COMPUTE SUMS OF SQS */
SST = Y'*TRTS*TINV*TRTS'*Y - CF;
SSB = Y'*QB*Y - CF;
SSC = Y'*QC*Y - CF;
SSE = Y'*Y - CF - SST - SSB - SSC;

MST = SST #/ (T-1);            /* COMPUTE F RATIO */
MSE = SSE #/ (N-B-T);
F(Z,2) = MST #/ MSE;
ME(Z,2) = MSE ;

```

```

*****;
*           TRY SLS           ;
*****;

```

```

U0 = UNIFORM(0);            /* PICK A PLAN AT RANDOM */
IF U0 LE 0.5 THEN TT1 = D(,1);
      ELSE TT1 = D(,2);

```

```

INI = RANK(R(1:B,2));          /* PERMUTE TRTS BY ROWS */
CHG = (STR - IN1)#(-6);
CHANGE = TB*CHG;
SORT = BASE + CHANGE;
TMP1 = TT1;
TTL(SORT,) = TMP1;

```

```

INI = RANK(R(22:24,2));       /* PERMUTE TRTS BY COLS */
CHG = (STR - IN1)#(-2);
CHANGE = TC*CHG;
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;
BIGC = TC*INI;

```

```

IN2 = R(1:9,2) > 0.5;        /* PERMUTE TREATMENTS */
CHANGE = TBC*IN2;           /* WITHIN ROW-COL COMBO */
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;

```

```

Y = Y0 +
    (TTL=1)*TF1 ;

```

```

                                /* FORM DESIGN MATRICES */
COL = DESIGN(BIGC);
TRT = DESIGN(TTL);

CF = ((Y'*INT)#(Y'*INT))#/N;    /* COMPUTE SUMS OF SQUARES */
SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*BLK*BINV*BLK'*Y - CF;
SSC = Y'*COL*CINV*COL'*Y - CF;
SSE = (Y'*Y - CF) - SST - SSC - SSB;

MST = SST #/ (T-1);            /* COMPUTE F RATIO    */
MSE = SSE #/ (N-2*B - T + 2);
F(Z,3) = MST #/ MSE;
ME(Z,3) = MSE;

END;

W1 = F(,1); I1 = RANK(F(,1)); F(I1,1) = W1;
W2 = F(,2); I2 = RANK(F(,2)); F(I2,2) = W2;
W3 = F(,3); I3 = RANK(F(,3)); F(I3,3) = W3;
FO(O,) = F(95,);
MO(O,) = ME(,);

END;

```

```

CRIT = FO(.,);
CRIT2 = FO(##,);
MMSE = MO(.,);
MSE2 = MO(##,);
NOTE COL1 = > RCBD, COL2 = > 2COL, COL3 = > SLS. T=6, SHIFT=2, TE = 0;
PRINT FO CRIT CRIT2 MMSE MSE2;

```

B.4 Simulation to Compare Power and MSE when T=8

```

*      T=8  B=4 (2 TRTS/(ROW*COL) FOR SLS)      ;
*
*
*      DAVID D. MORRIS      MAY 1986      ;
*
*

```

```

OPTIONS NOSOURCE;

```

```

DATA ONE;                                /* DATA FOR RCBD AND 2-COL*/
DO I = 1 TO 4;
DO J = 1 TO 8;
IF J = 1 OR J = 2 THEN K = 1; ELSE K = 2;
OUTPUT;
END;
END;

```

DATA COL2;

INPUT START;

CARDS;

1

2

3

4

5

6

7

8

3

4

1

2

5

6

7

8

5

6

1

2

3

4

7

8

7

8
1
2
3
4
5
6

DATA PLANS;

/* THE 10 SET OF SQUARES */

INPUT D1-D10;

/* FROM PREECE & FREEMAN */

CNT = _N_;

CARDS;

1111111111

2222222222

3333333333

4444544444

5555755555

6666466666

7777677777

8888888888

3333733333

4448848846

1111111161

2222226688

7777377727

8884682472

5555455515

6666564254

5555555555

6664686468
7777477717
8886868276
1313113381
2448244844
3131331123
4222722632
7777377777
8886464684
5555655525
6668782852
3131531113
4222828238
1313113341
2444246466

```
DATA TWO (KEEP = C1-C4);          /* CREATE A MATRIX TO */
  ARRAY CC (K) C1-C4;             /* EXPAND COLUMNS LATER */
  DO I = 1 TO 4;
    DO J = 1 TO 8;
      IN = CEIL(J/2);
      DO K = 1 TO 4;
        CC=0;
        IF K = IN THEN CC = 1;
      END;
    OUTPUT;
  END;
END;
```

```

DATA THREE (KEEP = B1-B4);          /* CREATE A MATRIX TO */
  ARRAY BB (J) B1-B4;              /* EXPAND ROWS  LATER */
  DO I = 1 TO 32;
    IN = CEIL(I/8);
    DO J = 1 TO 4;
      BB=0;
      IF J = IN THEN BB = 1;
    END;
  OUTPUT;
END;

```

```

DATA FOUR (KEEP = C1-C16);
  ARRAY CC (K) C1-C16;
  DO I = 1 TO 16;
    DO J = 1 TO 2;
      DO K = 1 TO 16;
        CC = 0;
        IF K = I THEN CC = (J=1) - (J=2);
      END;
    OUTPUT;
  END;
END;

```

```

PROC MATRIX;

```

```

  FETCH D DATA=PLANS (DROP = CNT); /* GET EXISTING WORK */

```

FETCH BASE DATA=PLANS (KEEP = CNT);

FETCH TB DATA=THREE;

FETCH TC DATA=TWO;

FETCH TBC DATA=FOUR;

FETCH TT DATA=ONE (KEEP = J);

FETCH START DATA=COL2;

FETCH BK DATA=ONE (KEEP = I);

FETCH CL DATA=ONE (KEEP = K);

CRIT = 2.49 2.51 2.58;

BF1 = 0; BF2 = 1; BF3 = 2; BF4 = 3;

SHIFT = 3; /* < = CHANGE SHIFT */

TF1 = 3; /* < = CHANGE TRT EFFECT */

N = 32;

B = 4;

T = 8;

STR = 1 / 2 / 3 / 4;

BULGE = J(N,1,0) + (BK=2)#2 + (BK=3)#4 + (BK=4)#6;

BULGE2 = J(N,1,0) +

(CL=1)#((BK=1)#2 + (BK=2)#6 + (BK=3)#10 + (BK=4)#14) +

(CL=2)#((BK=1)#4 + (BK=2)#8 + (BK=3)#12 + (BK=4)#16) ;

BLK = DESIGN(BK);

```

COL2 = DESIGN(CL);
TTL = J(N,1,0);
BINV = I(B) #/ T;
TINV = I(T) #/ B;
CINV = I(B) #/ T;
C2INV = INV(COL2*COL2);
QB = BLK*BINV*BLK';
QC = COL2*C2INV*COL2';
INT = J(N,1,1);
DE = (B + 1)*T;
FO = J(50,3,0);          /* <= CHANGE WITH BOUND */
MO = J(50,3,0);

DO O = 1 TO 50;

    F = J(100,3,0);
    ME = J(100,3,0);

    DO Z = 1 TO 100;

        R = J(DE,2,0);

        DO M = 1 TO DE;          /* GENERATE RANDOM NOS */
            R(M,1) = NORMAL(0);
            R(M,2) = UNIFORM(0);
        END;
    END;

```

```

Y0 = R(1:N,1) + (BK = 2)#BF2 + (BK = 3)#BF3 + (BK = 4)#BF4
      + (CL = 1)#SHIFT;

```

```

*****;

```

```

*          TRY RCBD          ;

```

```

*****;

```

```

INDEX = RANK(BULGE + R(1:N,2));    /* PERMUTE TRTMNTS */

```

```

TR = TT;

```

```

TT(INDEX,) = TR;

```

```

TRT = DESIGN(TT);                /* ADD TRT EFFECTS */

```

```

Y = Y0 + (TT = 1)#TF1;

```

```

CF = ((Y'*INT)#(Y'*INT)) #/ N;

```

```

SST = Y'*TRT*TINV*TRT'*Y - CF;

```

```

SSB = Y'*QB*Y - CF;              /* COMPUTE F RATIO */

```

```

SSE = Y'*Y - CF - SST - SSB;

```

```

MST = SST #/ (T-1);

```

```

MSE = SSE #/ (N-B-T + 1);

```

```

F(Z,1) = MST #/ MSE;

```

```

ME(Z,1) = MSE;

```

```

*****;
*           TRY 2 COLUMN           ;
*****;

TRT = J(N,1,0);

INDEX2 = RANK(R(7:38,2) + BULGE2);  /* PERMUTE TRTS IN */
TMP1 = START;                       /* THE BLOCKS */
START(INDEX2,) = TMP1;

IN1 = RANK(R(5:8,2));                /* PERMUTE ROWS */
CHG = (STR - IN1)#(-8);
CHANGE = BLK*CHG ;
SORT = BASE + CHANGE;
TRT(SORT,) = START;

TRTS = DESIGN(TRT);                 /* ADD TRT EFFECTS */
Y = Y0 + (TRT=1)#TF1 ;

CF = ((Y'*INT)#(Y'*INT)) #/ N;      /* COMPUTE SUMS OF SQS */
SST = Y'*TRTS*TINV*TRTS'*Y - CF;
SSB = Y'*QB*Y - CF;
SSC = Y'*QC*Y - CF;
SSE = Y'*Y - CF - SST - SSB - SSC;

```

```

MST = SST #/ (T-1);          /* COMPUTE F RATIO */
MSE = SSE #/ (N-B-T);
F(Z,2) = MST #/ MSE;
ME(Z,2) = MSE ;

```

```

*****;
*           TRY SLS           ;
*****;

```

```

P = 10;          /* <= THE NO. OF PLANS */

```

```

U0 = UNIFORM(0);      /* PICK A PLAN AT RANDOM */

```

```

Q = 1 #/ P;

```

```

U0 = (ROUND(U0,Q))#P;

```

```

U0 = ROUND(U0,1);

```

```

IF U0 = 0 THEN U0 = P;

```

```

TT1 = D(U0);

```

```

IN1 = RANK(R(1:B,2));    /* PERMUTE TRTS BY ROWS */

```

```

CHG = (STR - IN1)#(-8);

```

```

CHANGE = TB*CHG;

```

```

SORT = BASE + CHANGE;

```

```

TMP1 = TT1;

```

```

TTL(SORT,) = TMP1;

```

```

IN1 = RANK(R(37:40,2));      /* PERMUTE TRTS BY COLS */
CHG = (STR - IN1)#(-2);
CHANGE = TC*CHG;
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;
BIGC = TC*IN1;

IN2 = R(11:26,2) > 0.5;     /* PERMUTE TREATMENTS */
CHANGE = TBC*IN2;          /* WITHIN ROW-COL COMBO */
SORT = BASE + CHANGE;
TMP = TTL;
TTL(SORT,) = TMP;

Y = Y0 +
  (TTL=1)*TF1 ;

                                /* FORM DESIGN MATRICES */
COL = DESIGN(BIGC);
TRT = DESIGN(TTL);

CF = ((Y'*INT)#(Y'*INT))#/N; /* COMPUTE SUMS OF SQUARES */

```

```

SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*BLK*BINV*BLK'*Y - CF;
SSC = Y'*COL*CINV*COL'*Y - CF;
SSE = (Y'*Y - CF) - SST - SSC - SSB;

```

```

MST = SST #/ (T-1);          /* COMPUTE F RATIO    */
MSE = SSE #/ (N-2*B - T + 2);
F(Z,3) = MST #/ MSE;
ME(Z,3) = MSE;
F(Z,) = F(Z,) > CRIT;

```

```

END;

```

```

FO(O,) = F(+,);
MO(O,) = ME(.,);

```

```

END;

```

```

MALPHA = FO(.,);
ALPHA2 = FO(##.);
MMSE = MO(.,);
MSE2 = MO(##.);
NOTE COL 1 = > RCBD, COL 2 = > 2COL, COL 3 = > SLS, T=8, SHIFT=3, TE=3;
PRINT FO MALPHA ALPHA2 MMSE MSE2;

```

***B.5 Simulation to Obtain the Empirical 95th Percentile
when $T = 8$***

```
*                               ;  
*      T=8  B=4 (2 TRTS/(ROW*COL) FOR SLS )      ;  
*                               ;  
*      DAVID D. MORRIS      MAY 1986      ;  
*                               ;
```

```
DATA ONE;                               /* DATA FOR RCBD AND 2-COL*/
```

```
  DO I = 1 TO 4;  
    DO J = 1 TO 8;  
      IF J = 1 OR J = 2 THEN K = 1; ELSE K = 2;  
      OUTPUT;  
    END;  
  END;
```

```
DATA COL2;
```

```
  INPUT START;  
  CARDS;
```

```
1
```

```
2
```

3
4
5
6
7
8
3
4
1
2
5
6
7
8
5
6
1
2
3
4
7
8
7
8
1
2
3
4

5

6

DATA PLANS;

/* THE 10 SET OF SQUARES */

INPUT D1-D10;

/* FROM PREECE & FREEMAN */

CNT = _N_;

CARDS;

1111111111

2222222222

3333333333

4444544444

5555755555

6666466666

7777677777

8888888888

3333733333

4448848846

1111111161

222226688

7777377727

8884682472

5555455515

6666564254

5555555555

6664686468

7777477717

8886868276

1313113381

2448244844

3 1 3 1 3 3 1 1 2 3

4 2 2 2 7 2 2 6 3 2

7 7 7 7 3 7 7 7 7 7

8 8 8 6 4 6 4 6 8 4

5 5 5 5 6 5 5 5 2 5

6 6 6 8 7 8 2 8 5 2

3 1 3 1 5 3 1 1 1 3

4 2 2 2 8 2 8 2 3 8

1 3 1 3 1 1 3 3 4 1

2 4 4 4 2 4 6 4 6 6

DATA TWO (KEEP = C1-C4); /* CREATE A MATRIX TO */

 ARRAY CC (K) C1-C4; /* EXPAND COLUMNS LATER */

 DO I = 1 TO 4;

 DO J = 1 TO 8;

 IN = CEIL(J/2);

 DO K = 1 TO 4;

 CC=0;

 IF K = IN THEN CC = 1;

 END;

 OUTPUT;

 END;

END;

DATA THREE (KEEP = B1-B4); /* CREATE A MATRIX TO */

 ARRAY BB (J) B1-B4; /* EXPAND ROWS LATER */

 DO I = 1 TO 32;

 IN = CEIL(I/8);

 DO J = 1 TO 4;

```
    BB=0;
    IF J = IN THEN BB = 1;
END;
OUTPUT;
END;
```

```
DATA FOUR (KEEP = C1-C16);
ARRAY CC (K) C1-C16;
DO I = 1 TO 16;
    DO J = 1 TO 2;
        DO K = 1 TO 16;
            CC = 0;
            IF K = I THEN CC = (J = 1) - (J = 2);
        END;
    OUTPUT;
END;
END;
```

```
PROC MATRIX;
```

```
    FETCH D DATA = PLANS (DROP = CNT);    /* GET EXISTING WORK */
    FETCH BASE DATA = PLANS (KEEP = CNT);
    FETCH TB DATA = THREE;
    FETCH TC DATA = TWO;
    FETCH TBC DATA = FOUR;
    FETCH TT DATA = ONE (KEEP = J);
```

```
FETCH START DATA = COL2;  
FETCH BK DATA = ONE (KEEP = I);  
FETCH CL DATA = ONE (KEEP = K);
```

```
SHIFT = 3;                /* <= SET SHIFT      */  
TF1 = 0;                  /* <= SET TRT EFFECT  */  
BF1 = 0; BF2 = 1; BF3 = 2; BF4 = 3;
```

```
N = 32;                   /* <= SET PARAMETERS  */  
B = 4;  
T = 8;
```

```
STR = 1 / 2 / 3 / 4;
```

```
BULGE = J(N,1,0) + (BK=2)#2 + (BK=3)#4 + (BK=4)#6;
```

```
BULGE2 = J(N,1,0) +  
  (CL=1)#((BK=1)#2 + (BK=2)#6 + (BK=3)#10 + (BK=4)#14) +  
  (CL=2)#((BK=1)#4 + (BK=2)#8 + (BK=3)#12 + (BK=4)#16);
```

```
BLK = DESIGN(BK);  
COL2 = DESIGN(CL);  
TTL = J(N,1,0);  
BINV = I(B) #/ T;  
TINV = I(T) #/ B;  
CINV = I(B) #/ T;  
C2INV = INV(COL2'*COL2);
```

```

QB = BLK*BINV*BLK';
QC = COL2*C2INV*COL2';
INT = J(N,1,1);
DE = (B + 1)*T;
FO = J(50,3,0);          /* <= CHANGE WITH BOUND */
MO = J(50,3,0);

R = J(DE,2,0);
DO M = 1 TO DE;          /* GENERATE RANDOM NOS */
  R(M,1) = NORMAL(0);
END;

DO O = 1 TO 50;

F = J(100,3,0);
ME = J(100,3,0);

DO Z = 1 TO 100;

DO M = 1 TO DE;          /* GENERATE RANDOM NOS */
  R(M,2) = UNIFORM(0);
END;

Y0 = R(1:N,1) + (BK = 2)#BF2 + (BK = 3)#BF3 + (BK = 4)#BF4
      + (CL = 1)#SHIFT;

```

```

*****;
*          TRY RCBD                               ;
*****;

INDEX = RANK(BULGE + R(1:N,2));    /* PERMUTE TRTMNTS */
TR = TT;
TT(INDEX,) = TR;
TRT = DESIGN(TT);                 /* ADD TRT EFFECTS */

Y = Y0 + (TT=1)#TF1;

CF = ((Y'*INT)#(Y'*INT)) #/ N;
SST = Y'*TRT*TINV*TRT'*Y - CF;
SSB = Y'*QB*Y - CF;               /* COMPUTE F RATIO */
SSE = Y'*Y - CF - SST - SSB;
MST = SST #/ (T-1);
MSE = SSE #/ (N-B-T + 1);
F(Z,1) = MST #/ MSE;
ME(Z,1) = MSE;

*****;
*          TRY 2 COLUMN                           ;
*****;

```

```

TRT = J(N,1,0);

INDEX2 = RANK(R(7:38,2) + BULGE2);  /* PERMUTE TRTS IN */
TMP1 = START;                       /* THE BLOCKS */
START(INDEX2,) = TMP1;

INI = RANK(R(5:8,2));                /* PERMUTE ROWS */
CHG = (STR - INI)#(-8);
CHANGE = BLK*CHG ;
SORT = BASE + CHANGE;
TRT(SORT,) = START;

TRTS = DESIGN(TRT);                 /* ADD TRT EFFECTS */
Y = Y0 + (TRT = 1)#TF1 ;

CF = ((Y'*INT)#(Y'*INT)) #/ N;      /* COMPUTE SUMS OF SQS */
SST = Y'*TRTS*TINV*TRTS'*Y - CF;
SSB = Y'*QB*Y - CF;
SSC = Y'*QC*Y - CF;
SSE = Y'*Y - CF - SST - SSB - SSC;

MST = SST #/ (T-1);                 /* COMPUTE F RATIO */
MSE = SSE #/ (N-B-T);

```

F(Z,2) = MST #/ MSE;

ME(Z,2) = MSE ;

*****;

* TRY SLS ;

*****;

P = 10; /* <= THE NO. OF PLANS */

U0 = UNIFORM(0); /* PICK A PLAN AT RANDOM */

Q = 1 #/ P;

U0 = (ROUND(U0,Q))#P;

U0 = ROUND(U0,1);

IF U0 = 0 THEN U0 = P;

TT1 = D(,U0);

IN1 = RANK(R(1:B,2)); /* PERMUTE TRTS BY ROWS */

CHG = (STR - IN1)#(-8);

CHANGE = TB*CHG;

SORT = BASE + CHANGE;

TMP1 = TT1;

TTL(SORT,) = TMP1;

IN1 = RANK(R(37:40,2)); /* PERMUTE TRTS BY COLS */

CHG = (STR - IN1)#(-2);

CHANGE = TC*CHG;

SORT = BASE + CHANGE;

TMP = TTL;

TTL(SORT,) = TMP;

BIGC = TC*IN1;

IN2 = R(11:26,2) > 0.5; /* PERMUTE TREATMENTS */

CHANGE = TBC*IN2; /* WITHIN ROW-COL COMBO */

SORT = BASE + CHANGE;

TMP = TTL;

TTL(SORT,) = TMP;

Y = Y0 +

(TTL=1)*TF1 ;

/* FORM DESIGN MATRICES */

COL = DESIGN(BIGC);

TRT = DESIGN(TTL);

CF = ((Y'*INT)#(Y'*INT))/N; /* COMPUTE SUMS OF SQUARES */

SST = Y'*TRT*TINV*TRT'*Y - CF;

SSB = Y'*BLK*BINV*BLK'*Y - CF;

SSC = Y'*COL*CINV*COL'*Y - CF;

SSE = (Y'*Y - CF) - SST - SSC - SSB;

MST = SST #/ (T-1); /* COMPUTE F RATIO */

MSE = SSE #/ (N-2*B - T + 2);

F(Z,3) = MST #/ MSE;

ME(Z,3) = MSE;

END;

W1 = F(,1); I1 = RANK(F(,1)); F(I1,1) = W1;

W2 = F(,2); I2 = RANK(F(,2)); F(I2,2) = W2;

W3 = F(,3); I3 = RANK(F(,3)); F(I3,3) = W3;

FO(O,) = F(95,);

MO(O,) = ME(.,);

END;

CRIT = FO(.,);

CRIT2 = FO(##,);

MMSE = MO(.,);

MSE2 = MO(##,);

NOTE COL1 => RCBD, COL2 => 2COL, COL3 => SLS TE = 0 SHIFT = 3;

PRINT FO CRIT CRIT2 MMSE MSE2;

**The vita has been removed from
the scanned document**