ANALYSIS and INTERPRETATION
of FOREST FERTILIZER
EXPERIMENTS

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For Anne Felicity

and

In memory of my father

Leslie Charles Woollons

whose selfless actions in earlier years

made this Thesis possible

ABSTRACT

Sir Ronald Fisher's contributions to the design and analysis of field experiments have had a profound influence on the quality of forest fertilizer trials. Nevertheless, forestry experiments have unique features which are not always amenable to adoption of the routine analytical methods that are discussed and illustrated in many statistical texts. The aim of this thesis is to explore the nature and statistical standards appropriate for the analysis of forest fertilizer trials. The general linear model (GLM), embracing the techniques of analysis of variance, covariance, and regression analysis, represents a powerful tool with which to analyse forest nutrition experiments. An examination of pertinent literature reveals, however, that substantial numbers of forestry researchers have difficulty in applying GLM methodology to forest trial data. Indeed, there is clear evidence that some scientists are unaware of the utility of covariance as a means of removing the confounding effect of differences in initial plot growing stock; other researchers fail to extract all the data inherent in forest trial data, while a few advocate abandoning GLM methodology altogether, claiming it to be an insensitive tool for analysing forest fertilizer experiments. Re-analysis here shows how inappropriate some of these published results and claims are.

A system of analysis is presented which is considered a reliable and sensitive procedure for examining later-age forest fertilizer trials. For non-factorial layouts, the method employs applications of regression analysis, allowing responses in each treatment to be represented by a unique intercept and regression slope. Tests of hypotheses are introduced to determine the need for disparate slopes and independent intercepts; alike parameters are pooled to achieve a minimum available residual error. Secondary covariates such as stand competition or plot fertility values are added to models whenever pertinent, to decrease experimental errors further, and to aid interpretation of trial results. Factorial experiments are analysed very similarly, but utilizing factorial linear models and (multiple) covariance.

Appropriate manipulation of response variables and covariates is integral to the recommended system. The use of yield or growth as a response variable is demonstrated to give essentially equivalent results. Adoption of average yield per tree often achieves a more decisive analysis than with per hectare variables. Use of weighted least-squares can aid the interpretation of trial results when some plots have been partially damaged.

The presented system is tested by examining data from eight later age (some long term) fertilizer trials established by N.Z.Forest Products Limited. Analysis and derived results completely vindicate the value of the proposed methodology; in particular, use of two covariates increases precision in some analyses by up to 76%. The analyses irrefutably confirm the potential of nitrogen fertilizer to boost yields in thinned *Pinus radiata* stands belonging to the Company. These responses are demonstrated to be associated frequently with significant, but small and transient changes in stand form-factor.

Examination of the basal-area responses in each of the eight experiments highlights the fact that point estimation of fertilizer gain is inadequate for management planning and forecasting; yield tables of nutrient response are required, expressed as quantity of wood that can be realised in the future. The desired form can sometimes be achieved by modelling trial data to obtain a growth and yield model, then applying suitable realisation factors. Such a simulator has therefore been derived for the N.Z.Forest Products data, recognising variable inputs of fertilization, initial basal-area, initial stocking, and stand competition; experimental fertilizer response is estimated to be about 110 m^3/ha by age 30 years.

The suggested methodology has several implications for fertilizer trial experimental design as well as analysis; thus, it is now recommended that trials should be installed deliberately with a range of initial plot growing stock and to contain treatments which are distinctly different in their composition. Also, because multiple covariance is demonstrated to be a powerful technique to obtain good precision and additional information in later-age forest fertilizer trials, care must be taken to collect prior initial information about the trial material.

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COMPUTATIONAL NOTE

The analyses of variance presented in this thesis are calculated mostly by the GLM procedure, one of the analytical subroutines provided by the statistical package, SAS.

The sums of squares presented for any analysis are, unless stated to the contrary, extracted from the so-called 'Type III' analysis of variance table, Goodnight (1978), which adjust any effect for all other effects present. It should be noted that:

1. except for strictly balanced models without covariates, the total model sum of squares will not normally equate to the component model (Type III) total sum of squares;

2. for similar reasons, if a second covariate is added to a covariate model, the total component sum of squares will not necessarily equate to the original component model and residual sum of squares, but any reduction in the residual sum of squares is comparable.

1. INTRODUCTION

1.1 BACKGROUND

1.1.1 Prologue

"When a biologist believes there is information in an observation, it is up to the statistician to get it out". Sir Ronald Fisher's forthright remark to C.I.Bliss in 1934 is well known, but how well has Fisher's exhortation been obeyed by biometricians responsible for analysing forest fertilizer experiments? Such trials are commonplace in forest research, gathering data hard-earned in terms of resources and time. This thesis attempts to suggest some ways of "getting it out".

1.1.2 A historical overview

Foresters have been interested in fertilizers as a means of increasing volume production and correcting nutrient deficiencies in forests and plantations for at least 140 years [Anon (1844), Benzian (1951)]. Field experiments, as an adjunct to nutrition research, also date back to the nineteenth century [Ihrig (1870), Hallbauer (1891)]. Between 1900 and 1930, a considerable amount of fertilizer research was undertaken in Europe and North America, although investigations were preoccupied with applications of organic manures such as slag or lime, and aimed at applications in nurseries or at establishment [Herbert (1926), Guillebaud (1934), Albert (1936), Delevoy (1946) and Møller (1954)].

Early forest experimental practice was similar to agricultural field research. Although replicated experiments were certainly not unknown, and factorial systems sometimes utilised, see Yates (1935, 1964 and 1965), most trials were unreplicated, or no formal method had been devised for assigning standard errors to results [Yates (1975), Wright (1976)]. For example, Holmsgaard (1958) described some early European thinning experiments dating back to 1906, in which considerable thought had gone into the choice of treatments, including crown thinning, thinning from above and below, and unthinned controls, but no strict replication was employed. Similarly, Oleksyn and Giertych (1984) discussed a provenance trial, rediscovered by the authors in Poland using the plans of Kardiani, drawn up in 1912. Sixteen strains of *Pinus sylvestris* were laid out in 16 rectangular plots, and average treatment responses could be calculated from data, measured seventy years later.

Sir Ronald Fisher's monumental contribution to experimental design and analysis began at Rothamsted in 1919, and by 1925 he had revolutionised the entire subject [Yates (1964), Pearson and Kendall (1970)]. By adopting the principles of randomisation, replication and local control as basic requirements of good experimentation, the quality of trials was soon dramatically improved; for the first time a valid estimate of residual error was available, and the analysis of variance made formal testing of treatment effects feasible.

Forest scientists were not slow to adopt Fisher's methods. Donald (1956) implied that a 3 x 3 latin square was established in South Africa in 1921, but this is historically impossible and examination of the article suggests it was in fact 1930. Jeffers (1982) claimed that the very first latin square was installed by Steven in 1926 at Bagley, Oxford (a 6 x 6 square, investigating times of sowing of Sitka spruce), Fisher having produced the design too late to be included in the agricultural trials at Rothamsted. Steven (1928) later discussed the experiment and gave full details of the analysis, which indicate that the sowing dates were between March and June 1925. An early forest field experiment using the new methodology was a 5 x 5 latin square design involving species of larch, spruce, and pine, planted in 1929 at different altitudes at Bettgelert in Wales [Box (1978)]. Griffith and Ram (1947) discussed the analysis of a 5 x 5 latin square, examining the effect of fertilizers on height growth of Rosewood (*Dalbergia latifolia*) seedlings, in 1931.

By 1932 the British Forestry Commission had installed 31 latin squares and 98 randomised complete block designs, mostly short term experiments [Wright (1976)]. An exception is the well-known Bowmont thinning trial, established in 1930, a 4 x 4 latin square investigating grades of thinning in a stand of Norway spruce (*Picea abies*). Although the experimental layout has been subsequently criticised because of too small buffers and distances between plots, the experiment is a landmark for basic improvement in forestry experimental design and long-term measurement [MacDonald (1931), Hummel (1947), MacKenzie (1962), Hamilton (1976)]. The first long term factorial agricultural experiment was probably installed at East Malling in 1931, but it may have just lost by a few months to a 5 x 2 factorial designed with the help of Fisher for the Tea Research Institute in Ceylon [Pearce (1983)]. Stoate and Lane-Poole (1933) give full analytical details of a forest cultivation and fertilizer experiment, a 2^2 x 5 factorial in three blocks, established in three year old *Pinus pinaster* at Gnangara, Western Australia.

Day and Austin (1939) reported considerable detail of a giant 9^3 lattice nursery experiment, cited by Cochran and Cox (1966) on p.423, studying 729 *Pinus ponderosa* strains. Replicated nine times, this trial incorporated more plots than any other published forestry experiment. The trial was evidently laid out on the advice of Fisher, whilst his colleague F.Yates was currently exploring the analytical theory of cubic lattice designs [Yates (1939)]. In fact, 696 *Pinus ponderosa* types were utilised, many of which were repeated collections from the same trees, and the remainder comprised 33 *Pinus jefferyi* (Dr.K.Eldridge, *pers.comm.*).

Post-war, and as forestry research intensified, thousands of field experiments have been reported in the major journals, research station reports, or in numerous unpublished formats; for example Forestry Abstracts (1980-84) cited 56 references which allude to at least 260 fertilizer trials in established stands alone. The vast majority of such reports give no or very sparse details of statistical design and analysis, or alternatively the objectives of the experiments are concerned with biological mechanisms associated with fertilization rather than establishing statistical differences in yields. Some references report results from numerous trials; for example Lipas and Levata (1980) summarised responses in Scots pine (*Pinus sylvestris*) produced by applications of urea at different times of the year, where the data were gleaned from 44 experiments.

While most forest experimenters have adopted simple designs for trials, (for example, completely randomised or randomised complete blocks containing a few treatments), there are exceptions. Beard (1954, 1956) presented an account of a comprehensive uniformity trial in Black Wattle (*Acacia mollissima*), together with details of a series of 5 x 3 x 3 factorials partially confounded in three blocks, examining responses of young Black Wattle to phosphorus, lime and potassium on 13 sites. Schonau (1982) gave intermediate results of a *Eucalyptus grandis* thinning experiment where a 3^4 factorial in blocks of 9 was utilised. In Australia, H.D.Waring studied responses of *Pinus radiata* and *Pinus elliottii* using several complex experimental designs, including a set of six 3 x 3 latin squares, three 4 x 4 quasi-latin squares, two 1/4 replications of a 2^8 fractional factorial, and a $4^2 \times 2^2$ factorial in blocks of 16 [see Waring (1955, 1962, 1969 and 1980), Snowdon and Waring (1985), and Woollons and Snowdon (1981)]. Cellier and Stephens (1980) reported results from two South Australian establishment fertilizer trials, which used a 1/128 replicate of a 2^{13} fractional factorial as the experimental plan.

Occasionally, forest researchers have abandoned Fisher's methodology, and reverted to systematic designs, but using considerably more sophisticated layouts than before. Pudden proposed a systematic system for thinning experiments, Anon (1960), and despite objections by Scott (1962), this led Slyke (1965) to construct a better but similar system. Nelder (1962) suggested fan-shaped spacing experiments and several have been incorporated in forestry situations [Tennent (1973, 1976)]. An ingenious spacing design was suggested by Chuang-Sheng and Morse (1975), which overcame the problem of systematic layouts, at the lesser expense of utilising non-uniform plot sizes. No example is known of a forestry application, however.

Over the years there is evidence that the standard of experimental design has sometimes been insufficient to cope with the high variability inherent in forest trials. Luckhoff (1949) reported a pruning experiment with *Pinus patula* in which it became clear the 4 x 4 latin square had insufficient degrees of freedom to cope with fertility irregularities.

Schonau (1977) reviewed nutrient responses reported in trials associated with flooded gum (*Eucalyptus grandis*) and urged South African researchers to use more rigorous experimental designs as well as utilise more replication of treatments. Herbert (1983) confirmed these findings, and suggested *Eucalyptus grandis* nutrition research had been set back years by earlier researchers adopting inappropriate experimental designs and poor treatment selections. Weetman and Fournier (1984) rued the insufficient replication employed in standard Canadian interprovincial experiments studying the effect of nitrogen on jack pine (*Pinus banksiana*) which made it difficult to detect reliable treatment effects.

1.2 ANALYSIS OF FOREST FERTILIZER TRIALS

The plethora of forest fertilizer experiments reported in the literature indicate that researchers routinely utilise the well established statistical methods of experimental analysis, described in texts such as Ostle (1963), or Cochran and Cox (1966). Whether or not these expositions supply sufficient information to confirm that they have been well analysed is, however, open to speculation. Forest fertilizer experiments have unique features, not found with nutrition trials in general; they are different because they are:

> comparatively large, sometimes occupying several hectares;
>
> commonly designed to be maintained over decades;
>
> required to be measured over decades;
>
> frequently unbalanced with respect to quanta of initial growing stock in experimental units.

Moreover, fertilizer responses in forest stands are often complex in terms of:

> active elements;
>
> size and duration of response;
>
> interactions with silvicultural variables such as stand density, thinning and pruning;
>
> interactions with environmental variables beyond the control of the experimenter.

All these considerations suggest that analysis of forest fertilizer experiments is a specialised topic, which is not adequately covered in generalised statistical texts. References which do address the subject to some extent include those of Jeffers (1959), Freese (1967), and Pearce (1976, 1983). However, the first two authors treat the subject at a very elementary level, while the latter is essentially concerned with horticultural crops. In essence, a comprehensive and specific study of analysis of forest nutrition experiments has not previously been reported in the literature. This study, consequently, is aimed at filling this important gap.

1.3 OBJECTIVES

This thesis attempts therefore, to:

1. summarise available least-squares methods of statistical analysis, suited to the examination of forest fertilizer experiments, particularly trials established at later ages;
2. investigate through published literature the degree of success achieved previously in analysing examples of such experiments;
3. devise an appropriate methodology for analysing mid-rotation forest fertilizer experiments, and demonstrate the utility of the system by re-examining several field trials, details of which have already been published;
4. test the recommended methodology through analysing a series of existing fertilizer experiments available to the author, and interpreting the results of these trials;
5. integrate forest management informational needs, analytical tools, and forest fertilizer experimental data;
6. review the proposed methodology and informational needs as they affect forest fertilizer experimental design.

2. STATISTICAL METHODS

2.1 ANALYSIS OF VARIANCE

2.1.1 An overview

The predominant method of analysing fertilizer experiments is by the analysis of variance, a system first proposed by Fisher (1918, 1925) and subsequently discussed or developed further by many authors including Scheffé (1959), Ostle (1963), Kendall and Stuart (1966), Snedecor and Cochran (1967), and Mendenhall (1968). More recently, a few authors have claimed that analysis of variance is inappropriate for the examination of forest fertilizer trials (for example, the assertions of Gerig *et al.* (1978) are examined and challenged in Chapter 3 of this thesis). Nevertheless, analysis of variance techniques are still almost universally accepted to be the correct statistical tools for examination of forest fertilizer experiments. The basic methodology is therefore summarised here.

Suppose that we have n observations or measurements denoted as n random observations $Y_1, Y_2, \ldots., Y_n$ comprising linear combinations of p unknown quantities, $\beta_1, \beta_2, \ldots., \beta_p$ plus errors $e_1, e_2, \ldots., e_n$. Then

$$Y_i = x_{1i}\beta_1 + x_{2i}\beta_2 + \ldots. x_{pi}\beta_p + e_i \qquad (2.1)$$

$$(i = 1, 2, \ldots., n)$$

where the (x_{ji}) are known constant coefficients.

A minimal assumption about the random variables (e_i) is that their expected values are zero.

$$E(e_i) = 0 \qquad i = 1, 2, \ldots., n$$

and that (2.2)

$$E(e_i \times e_j) = \sigma^2 \delta_{ij}$$

where σ^2 is an unknown constant and δ_{ij} is 0 if $i \neq j$ and 1 if $i = j$.

The analysis of variance is a body of statistical methods for analysing measurements assumed to be of structure (2.1) where the coefficients (x_{ji}) are usually integers 0 or 1, which refer respectively to the absence or presence of the effects (β_j). If the (x_{ji}) are instead continuous variables, such as X = initial basal area and ln(X) = soil Nitrogen, and are called independent or concomitant variables, the observations (Y_i) are then said to represent a dependent variable Y in a regression analysis (see Section 2.2). If there are some (x_{ji}) of both kinds we have an analysis of covariance (see section 2.3). All three techniques are thus closely related and differ simply on the coding methodology of the x_{ji}.

The unknown effects (β_j) may be either unknown constants called parameters, or unobservable random variables subject to further assumptions about their distribution involving other unknown parameters.

2.1.2 Matrix Terminology

The set of equations (2.1) can be represented simply in matrix terms as

$$\underset{\sim}{y} = X \underset{\sim}{b} + \underset{\sim}{e} \tag{2.3}$$

as shown by Scheffé (1959), p.7, and $E(\underset{\sim}{e}) = 0$, with $E(\underset{\sim}{e}'\underset{\sim}{e}) = \sigma^2 I$, where 0 is the (n x 1) zero matrix and I is the (n x n) identity matrix. The matrix X in (2.3) is commonly referred to as the design matrix.

2.1.3 Least-squares Estimates and Normal equations

Suppose that b_1 b_2 ...., b_p denote quantities which we might consider using as estimates of $\beta_1, \beta_2, \ldots., \beta_p$. The (β_j) are fixed unknown constants, whereas the $b_1, b_2, \ldots, b_p$ will be sample estimates.

For any $\underset{\sim}{b} = (b_1, b_2, \ldots, b_p)'$ we form

$$S(\underset{\sim}{y}, \underset{\sim}{b}) = (\underset{\sim}{y} - X'\underset{\sim}{b})'(\underset{\sim}{y} - X'\underset{\sim}{b}) \qquad (2.4)$$

A set of functions of y

$$\hat{\beta}_1 = \hat{\beta}_1(\underset{\sim}{y}), \hat{\beta}_2 = \hat{\beta}_2(y) \ldots, \hat{\beta}_p = \hat{\beta}_p(\underset{\sim}{y})$$

where the values $b_j = \hat{\beta}_j$ $(j = 1, \ldots, p)$ minimise (2.4) is the set of <u>least squares (LS) estimates</u> of the (β_j). Any set of LS estimates satisfies the conditions

$$\partial S(\underset{\sim}{y}, \underset{\sim}{b})/\partial b_v = 0 \qquad (v = 1, \ldots, p)$$

utilisation of which leads to the <u>normal equations</u>

$$X'X\underset{\sim}{b} = X'\underset{\sim}{y} \qquad (2.5)$$

Provided that the rank of $X = p$, then (2.5) has a unique solution given by

$$\underset{\sim}{\hat{\beta}} = (X'X)^{-1}X'\underset{\sim}{y} \qquad (2.6)$$

and the variance covariance matrix of

$$V(\underset{\sim}{\hat{\beta}}) = \sigma^2(X'X)^{-1} \qquad (2.7)$$

2.1.4 Side conditions on parameters and estimates

If the rank $X < p$, as is usually the case with analysis of variance (ANOVA), then (2.6) is not determinate. However, by imposing <u>Linear constraints</u> on the p parameters, as detailed by Scheffé (1959) pp.15-16, then these processes lead to a matrix of full rank, which can be uniquely solved for $\hat{\underset{\sim}{\beta}}$ as given in (2.6).

2.1.5 Partitioning of Linear Model: Error Mean Square

For an experiment with p treatments, each replicated n_j times, an appropriate model is

$$Y_{ij} = \beta_0 X_0 \quad + \beta_1 X_1 \quad + \quad \dots \quad \beta_p X_p \quad + \quad e_{ij}$$

or in matrix terms

$$\underset{\sim}{y} = X \underset{\sim}{b} + \underset{\sim}{e} \tag{2.8}$$

Then, the sum of squares (SS) of the observations may be written as the sum of two non-negative components

$$\underset{\sim}{y}'\underset{\sim}{y} = (X\hat{\underset{\sim}{\beta}})'(X\hat{\underset{\sim}{\beta}}) + (\underset{\sim}{y}-X\hat{\underset{\sim}{\beta}})'(\underset{\sim}{y}-X\hat{\underset{\sim}{\beta}}) \tag{2.9}$$

of which the first term in (2.9) is the reduction in the SS due to any fitted model, and the second is the sum of squared residuals about the least-squares model.

Rearrangement of (2.9) gives the residual sum of squares to be

$$\underset{\sim}{y}'\underset{\sim}{y} \quad - \quad \hat{\underset{\sim}{\beta}}'X'\underset{\sim}{y} \tag{2.10}$$

and $s^2 = (\underset{\sim}{y}'\underset{\sim}{y} - \hat{\underset{\sim}{\beta}}'X'\underset{\sim}{y})/N-p$ (2.11)

where $N = \sum_{j=1}^{p} n_j$

This gives an unbiased estimate of σ^2 the variance of $\underset{\sim}{e}$ in (2.8) and is called the error mean square. All this leads to the well-known ANOVA table:

Source	d.f.	SS	MS
Due to model	p	$\underset{\sim}{b}'X'\underset{\sim}{y}$	$(MS)_{model}$
Residual	N-p	$\underset{\sim}{y}'\underset{\sim}{y}-b'X'\underset{\sim}{y}$	s^2
Total	N	$y'y$	

The ratio $F = MS_{model}/s^2$ is distributed as Snedecor's-F with p and (N-p) degrees of freedom under the null hypothesis which can thus be used as a test of, H_0: $\underset{\sim}{\beta} = 0$

The ANOVA above can be considerably augmented by splitting the model component into factors and interactions, according to the underlying experimental design and corresponding linear model as set out in Scheffé (1959). Similarly, blocked experimental designs, Cochran & Cox (1966), are easily handled by analysis of variance methodology. The design matrix, X, in (2.3) can be readily augmented to represent the blocking structure by further use of dummy variables, and, identical to the treatment effects, these are also constrained by considering a reduced model, or similarly equating the blocking parameters.

Analysis of variance is illustrated by the following example.

2.1.6 Analysis of variance: an example

In 1974 N.Z.Forest Products Limited (NZFP), an integrated wood growing and processing Company in the Central North Island of New Zealand, installed a trial at time of re-establishment after the first crop of *Pinus radiata* had been harvested. The experiment investigated the effects of nitrogen (N), phosphorus (P), magnesium (Mg), and boron (B), applied singly, and in combination, on the growth of radiata pine seedlings. Forms and rates of applied nutrients were:

N - urea, 60 g/tree
P - superphosphate, 360 g/tree
Mg - epsom salts, 15 g/tree
B - borax, 2.2 g/tree

The experimental design was a 2^4 factorial in blocks of 4, replicated six times, thus imposing partial confounding, Cochran and Cox (1966), p.235, of:

1st order interactions, 1/6 information lost
2nd order interactions, 1/2 information lost

The experimental units were 25 m^2 quadrats of 25 graded 1/0 seedlings planted at 1 x 1 m spacings. The plots were not buffered. The experimental area was cleared of debris and slash in June 1974, with care being taken not to disturb the top soil. A hareproof fence was erected and weed control was maintained by herbicides and hand releasing. Subsequent mortality was extremely light (22 of 2400 seedlings).

The trial can be analysed easily by analysis of variance procedures, after adjusting the partially confounded treatments effects. The sums of squares for these interactions are calculated for each replication and summated, but excluding values which are confounded with blocks. The response variables were collar diameter and seedling height in 1975. Assuming a factorial model for the main effects of nitrogen (N), phosphorus (P), magnesium (Mg), and boron (B), together with their interactions, gives an ANOVA:

Source	d.f.	SS (diameter)	p>F	SS (height)	p>F
N	1	173.59	0.0001	3055.5	0.0001
P	1	27.16	0.0010	589.1	0.0010
NP	1'	0.83		6.4	
Mg	1'	0.02		2.7	
NMg	1'	2.52		11.7	
PMg	1'	5.61	0.0360	125.8	0.0490
NPMg	1"	0.28		19.1	
B	1	0.38		2.4	
NB	1'	1.70		15.0	
PB	1'	0.39		2.8	
NPB	1"	1.54		59.4	
MgB	1'	1.68		15.9	
NMgB	1"	0.18		1.4	
PMgB	1"	1.04		31.4	
Reps	5	20.76		1075.4	
Blocks (within reps)	18	66.15		1116.1	
Error	58	95.76		1768.0	
	95				

(The degrees of freedom denoted 1', 1" represent partially confounded effects)

For both response variables, there are highly significant responses to the main effects of nitrogen and phosphorus but no indication of any real interaction. The apparently significant PMg interaction is attributed to a random outcome of investigating 15 effects, and deliberately ignored in the analysis and interpretation of results. Estimated main effect responses are (1975)

	N	P
Collar diameter	2.7 mm	1.1 mm
Height	11.3 cm	5.0 cm

Thus, the analysis of variance procedure has provided a salient and formal method of examining the imposed treatments in the experiment, and has given, for a stated null hypothesis, the probabilities that each effect is at least as extreme as those observed. However, in concluding that any treatment is real, the analysis of variance assumes that the size of a response is totally due to the application of fertilizer, and not to any other effect, for example, initial size. Since the experimental trees were graded before planting, the assumption of no extraneous variables contributing to response is acceptable for these data, but for many other types of fertilizer trials this cannot be taken for granted, and modifications are required to the assumed model and the ANOVA calculations.

2.2 REGRESSION ANALYSIS

The problem of initial differences in experimental plots can frequently be resolved by regression analysis techniques.

The basic results of straight-line regression are very well documented, for example in Williams (1959), Kendall and Stuart (1961), Daniel and Wood (1971), Draper and Smith (1981). For a linear model

$$E(Y) = \alpha + \beta X \tag{2.12}$$

the least-squares estimates of α and β in a sample of n paired observations X and Y through solving the normal equations are

$$\begin{aligned} \hat{\beta} &= (\Sigma XY - [\Sigma X][\Sigma Y]/n)/(\Sigma X^2-[\Sigma X]^2/n) \\ &= \Sigma xy/\Sigma x^2 \\ \hat{\alpha} &= \bar{Y} - \beta\bar{X} \end{aligned} \tag{2.13}$$

2.2.1 Groups of Data

Regression analysis has extremely useful applications in forest experiments for analysing several samples or groups of data (for example, sets of replicated treatments in a field trial). Given such data, the question arises, can one regression line be utilised for all the data? A more specific hypothesis occurs when k groups can be depicted by the linear model, $Y = \alpha_i + \beta_i X$: then, the question arises, does $\beta_1 = \beta_2 = \ldots.,$ β_k, or are the sample slopes estimates of the same true slope?

A general approach to this question is set out below.

Consider k linear models

$$\begin{array}{llll} Y = \alpha_1 + \beta_1 X_{1j} + e_{1j} & & j = 1, 2, \ldots, n_1 \\ Y = \alpha_2 + \beta_2 X_{2j} + e_{2j} & & j = 1, 2, \ldots, n_2 \\ . \quad . \quad . \quad . & & . \quad . \quad . \quad . \\ . \quad . \quad . \quad . & & . \quad . \quad . \quad . \\ Y = \alpha_k + \beta_k X_{kj} + e_{kj} & & j = 1, 2, \ldots, n_k \end{array} \tag{2.14}$$

with $\sum_{i=1}^{b} n_i = N \quad n_i > 2$ for all i

The set of linear models in (2.14) can be tabulated in the form:

Group	d.f.	Σx^2	Σxy	Σy^2	RSS
1	(n_1-1)	A_1	B_1	C_1	$C_1-B_1{}^2/A_1$
2	(n_2-1)	A_2	B_2	C_2	$C_2-B_2{}^2/A_2$
.	.	.	.	.	.
.	.	.	.	.	.
k	(n_k-1)	A_k	B_k	C_k	$C_k-B_k{}^2/A_k$

$$A_W=\Sigma A_i \qquad B_W=\Sigma B_i \qquad C_W=\Sigma C_i$$

where $A_i = \Sigma x_i{}^2$ $B_i = \Sigma x_i y_i$ $C_i = \Sigma y_i{}^2$

then, the hypothesis, $H_0: \beta_1 = \beta_2 = \ldots., \beta_k$, can be tested by

$$(S_2/[k-1])/(S_1/[N-2k]) \sim F_{(k-1),\ (N-2k)} \tag{2.15}$$

where, in (2.15)

$$S_1 = \sum_{i=1}^{k} C_i - (B_i{}^2/A_i)$$

and

$$S_2 = C_W - B_W{}^2/A_W - S_1$$

Given that the above hypothesis is accepted, it is logical to test if the intercepts in (2.14) are equivalent. A hypothesis, $H_0: \alpha_1 = \alpha_2 = \ldots, \alpha_k$ can be tested by

$$(S_t - [S_1 + S_2])/(k-1)/([S_1 + S_2)/(N-k-1]) \sim F_{(k-1),(N-k-1)} \tag{2.16}$$

[Warren (1974) provides an efficient formulation to execute the tests above] where S_t = total residual sum of squares.

An alternative, and more flexible test of hypothesis (2.16) can be developed in matrix notation. Suppose after applying test (2.15), an experiment of k groups (treatments) can be represented by

$$E(Y) = \sum_{i=1}^{k} \alpha_i + \sum_{j=1}^{m} \beta_j X \qquad (2.17)$$

where m in equation (2.17) represents the number of statistically independent regression coefficients. A test may be desired of:

$$H_o:\ E(Y_A/X_p) = E(Y_B/X_p) \qquad (2.18)$$

where Y_A, Y_B = average values of any two groups,

A and B, evaluated at the point $X = X_p$

Model (2.17) can be represented by $y = X\underset{\sim}{b}$ where the design matrix X contains k columns representing the intercepts k, and m columns representing the m slopes. Without loss of generality, let A and B refer to the first and second groups. Now let

$$\begin{aligned} X'_A &= (1\ 0\ \ldots.\ 0\ X_p\ \ldots.\ 0) \\ X'_B &= (0\ 1\ \ldots.\ 0\ 0\ X_p\ \ldots.\ 0) \end{aligned} \qquad (2.19)$$

Then the variance of $\overline{Y}_A - \overline{Y}_B$ is given by

$$\sigma^2(X_A'\ [X'X]^{-1}X_A + X'_B[X'X]^{-1}X_B) \qquad (2.20)$$

and a test of (2.18) is given by

$$(X'_A b - X'_B b)/\surd(\sigma^2[X'_A\{X'X\}^{-1}X_A + X'_B\{X'X\}^{-1}X_B]) \qquad (2.21)$$

where (2.21) is distributed as Students' t, with (N-k-1) degrees of freedom.

The case for the model $Y = \alpha_A + \alpha_B + \beta_A X + \beta_B X$ is developed and discussed in Chapter 4.

These regression methods are illustrated by the following example.

2.2.2 Regression Analysis: an example

Mead *et al.* (1984) discussed results from a *Pinus radiata* experiment laid out in Eyrewell State Forest, near Christchurch, New Zealand. While concerned mainly with aspects of dry matter production, the authors were able to deduce a strong response to nitrogen fertilizer, with an estimated 4.5 m^2/ha additional basal area achieved in both thinned and unthinned regimes, over four years. The data are reworked here, using regression methodology.[1]

The experiment consisted of four treatments, thinned (810 stems/ha) and unthinned (1530 stems/ha), fertilized with 400 kg N/ha as ammonium nitrate, in 1977, or not fertilized. The four treatments were replicated three times, in plots of 0.205 ha, with inner measurement plots of 0.0375 ha. Breast height diameters were available for 1977 and 1981, for all experimental trees.

Using basal area/ha as a response variable, assume a model

$$E(Y) = \sum_{i=1}^{4} (\alpha_i + \beta_i X) \tag{2.22}$$

where Y = response variable, defined above

X = basal area/ha, 1977

α_i, β_i = intercepts and regression coefficients associated with the four treatments

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[1] The writer is grateful to the senior author, Dr.D.Mead, for making the data available

A test of $H_0 : \beta_1 = \beta_2 = \beta_3 = \beta_4$ can be extracted from the ANOVA:

Source	d.f.	SS (Type I)	p > F
Single slope, intercept	1	240.96	0.0001
Disparate slopes	3	1.39	
Individual intercepts	3	73.34	0.0080
Error	4	1.77	
	11		

The hypothesis of no difference in slopes is accepted, thus reducing (2.22) to

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \beta X \tag{2.23}$$

Two hypothesis tests are now required: (1) the effect of fertilization for the <u>thinned</u> regime is zero; (2) the effect of fertilization for the <u>unthinned</u> regime is zero. In applying these tests, evaluation is carried out at the respective average initial basal area values, for <u>each</u> regime. The hypotheses above can be tested by

$$H_0 : E(Y_{ft}/\overline{X}_t) = E(Y_{nt}/\overline{X}_t)$$

and (2.24)

$$H_0 : E(Y_{fu}/\overline{X}_u) = E(Y_{nu}/\overline{X}_u)$$

where in (2.24), the subscripts

f, n = fertilized, not fertilized
t, u = thinned, unthinned

and $\overline{X}_t$, $\overline{X}_u$ = average initial basal area/ha of each regime are calculated by:

$$\begin{aligned} & (15.20 - 10.53)/\sqrt{(0.4514)\ (0.6724)} \\ = \quad & 4.67/0.5509 = 8.48 \sim t_{(7)}\ (p < 0.0005) \end{aligned}$$

and $$\tag{2.25}$$

$$\begin{aligned} & (14.09 - 8.86)/\sqrt{(0.4514)\ (0.6804)} \\ = \quad & 5.23/0.5542 = 9.44 \sim t_{(7)}\ (p < 0.0005) \end{aligned}$$

where the above calculations are evaluated by test statistic (2.21).

These highly significant results give estimated responses of

4.67 ± 0.59 m^2/ha (thinned)
5.23 ± 0.60 m^2/ha (unthinned)

The responses essentially reflect the differences in intercept values for fertilized and non-fertilized treatments separately for each thinning regime. However, the use of regression analysis has removed a serious confounding effect in the data. Unlike the example given in Section (2.1), the experimental plots could not be assumed to have equivalent initial basal area at the time of fertilizer application, but this effect has been overcome by utilising a regression coefficient in model (2.23), and evaluating responses at selected values of X. These points are fully developed in Chapter 4.

2.3 ANALYSIS OF COVARIANCE

Analysis of covariance combines features of regression analysis and analysis of variance. Where there is a need to draw inferences about group or treatment means for a response variable Y that is measured on each unit, an analysis of covariance makes use of information about Y that is contained in another variable X that is also measured on each unit. This is done in two basic ways:

(a) variation of Y that is associated with X is removed from the error variance, resulting in more precise estimates and more powerful tests;

(b) group means of the Y variable are adjusted to correspond to a common value of X, thus producing an "equitable" comparison of the groups.

Theoretical expositions of covariance have been given by Scheffé (1959), and Kendall and Stuart (1966). Introductory texts include those of Cox (1958), Li (1964), while Biometrics, Vol.13.1 (1957) devotes the entire issue to aspects of the subject. As mentioned in Section (2.1), the intimacy between covariance, analysis of variance, and particularly regression analysis, is considerable. With covariance, the design matrix X is typically constructed of systematic dummy variables and random independent variables, and is concerned with minimising sums of squares of deviations by least-squares criteria. The linear model (2.1) can be slightly augmented to

$$Y_i = x_{1i}\beta_1 + x_{2i}\beta_2 + \dots x_{pi}\ \beta_p + \theta X_{1(i)} + e_i \qquad (2.26)$$

where θ = regression coefficient

X_1 = a concomitant variable.

It will be noted above: (1) the relationship between the response variable and the covariate is <u>linear</u>. (2) <u>One</u> common slope is utilized, independent of levels of any factor.

2.3.1 Adjusted means

The new parameter, θ, is estimated in the usual manner, and adjusted treatment means are calculated by

$$\bar{Y}_{adj} = \bar{Y}_i - \theta(\bar{X}_i - \bar{\bar{X}}) \qquad (2.27)$$

where $\bar{Y}_{adj}$ = adjusted treatment means

θ = estimated regression coefficient

$\bar{X}_i$ = ith treatment covariate mean

$\bar{Y}_i$ = ith treatment mean

$\bar{X}$ = grand mean of all treatments.

2.3.2 Multiple covariance

Multiple covariance represents a natural extension of covariance procedures, where several auxiliary variables are added to the linear model (2.1), viz:

$$Y_i = x_{1i}\beta_1 + x_{2i}\beta_2 + \ldots x_{pi}\beta_p + \theta_1 X_{1(i)} + \theta_2 X_{2(i)} + \ldots \theta_q X_{q(i)} + e_i \qquad (2.28)$$

where X_1, X_2, ... X_q represent q auxiliary variables or covariates, and θ_1, θ_2 ... θ_q are regression coefficients associated with each auxiliary variable. Each covariate value is coded into the design matrix (2.3), and each θ_i is estimated by inversion of the X'X matrix, (2.5), identical to single covariance methodology.

Application of multiple covariance assumes the existence of suitable covariates, which have had an effect on the response variable, Y, and, unless removed, may inflate the residual error or confound treatment responses. Initial growing stock is a very common single covariate with forest fertilizer trials, but sometimes additional covariates like stocking, Woollons (1980), soil nitrogen, foliar nitrogen, or plot coordinates, denoting position in a trial can be utilised as well [Woollons and Whyte (1988)]. To be effective, all covariates need to be related to the response variable and to its deviations from the overall average, but relatively independent of each of the other covariates. The use of stocking and initial basal area, cited above, was unusual in that stand density and growing stock were not closely related in that instance. Examples of multiple covariance application are given in Chapters 3 and 6. Covariance methodology is illustrated by the following example.

2.3.3 Analysis of covariance: an example

A fertilizer trial in 16 year old thinned *Pinus radiata* was established in 1978, in Buccleuth forest, Tumut, N.S.W., Australia (W.J.B.Crane, in-house data). The experimental design was a 3 x 2 x 2 factorial arrangement, replicated three times, in blocks of six treatments. Such a configuration imposed slight confounding of effects, with one first order interaction having 1/9 information lost, and 4/9 information lost for the second order interaction [see Cochran and Cox (1966), p.240].

The factors were:

Nitrogen	nil, 100 kg N/ha, 400 kg N/ha as ammonium nitrate
Phosphorus	nil, 100 kg P/ha, as dicalcium phosphate
Sulphur	nil, 1 tonne gypsum/ha

The experimental plots were 0.11 ha with an inner 0.0494 ha measurement plot. Diameter at breast height was measured in 1978 and 1980 on all trees in each plot, and basal area/plot, 1980 was taken to be the response variable, Y. The following linear model was adopted

$$E(Y) = b_0 + b_1N + b_2P + b_3NP + b_4S + b_5NS + b_6PS + b_7R + \beta X \qquad (2.29)$$

where N, P, S = effects of nitrogen, phosphorus, and sulphur respectively

X = basal area/plot, 1978

β = a regression coefficient

b_i = estimated parameters

R = effect of blocks

Analysis of covariance of the data then gave:

Source	d.f.	SS $(\times 10^3)$	p > F
N_{lin}	1	6.06	0.0015
N_{quad}	1	0.31	
P	1	0.20	
NP	2	2.23	0.1068
S	1	0.15	
NS	2	0.42	
PS	1'	0.02	
Blocks	5	5.59	0.0538
Basal area, 1978	1	290.64	0.0001
Error	20	9.04	
	35		

where 1' denotes a partially confounded effect.

Initial basal area/ha emerges as a powerful covariate and the effect of nitrogen is highly significant; further analysis shows the linear component to be significant, $(p < 0.0015)$. There is a suggestion of a possible NP interaction, and the effect of the blocking is marginally significant (5%).

Adjusted, and unadjusted means of the nitrogen levels are as follows: (m^2/ha, 1980)

N level	Adjusted	Unadjusted	Average initial basal area/ha (1978)
n_0	23.05	23.74	18.9
n_1	23.51	22.85	17.9
n_2	23.71	23.67	18.5

These figures highlight the value of adjusted means. The covariance analysis isolates a highly significant linear response to the nitrogen levels, yet the straight (unadjusted) treatment averages suggest a quadratic response. This abnormality is removed by the adjusted means, suggesting that the chance randomisation process allocated some plots of somewhat smaller initial size to the n_1 level treatments, as shown above.

2.3.4 Examination of residuals

Graphical plottings of ($e_i = Y_i - \hat{Y}_i$), are a well-known technique in regression analysis to detect outliers, lack-of-fit, and bias in estimation [Draper and Smith (1981) pp. 141-183].

The graphical plotting of residuals is practised far less with analysis of variance or covariance, yet the technique is equally useful with the latter applications. Draper and Smith (1981) p.453 remark '...

> residuals in the analysis of variance play the same role as residuals in regression models and must be examined for the information they contain on the possible inadequacy of the model under consideration. (There seems to be a tacit assumption in most variance analysis the model is correct.)' Elsewhere they comment '... the question "What model are you considering?" is often met with "I am not considering one - I am using analysis of (co)variance"'.

It probably remains true that residual plots have to be interpreted with a certain amount of caution in forest trial data, because frequently the number of experimental units is small (say less than 20), and such graphs may be a random display rather than verifying or disputing a particular (co)variance model. For larger experiments, the practice is recommended.

Principal ways of plotting residuals, e_i, useful in forest experimentation are:

1. about a standardised mean;
2. against the fitted (predicted) values, $\hat{Y}$;
3. against covariates (if present).

The first of these should follow an approximate normal distribution, but if there is appreciable skewness, transformation of the data may be appropriate. The graph of (2) should exhibit a broad horizontal band; departure from this pattern reveals the presence of heterogeneity or lack-of-fit. Outliers (data lying three or four deviations from the mean of the residuals), if present, will also be shown. Dropping of outliers in regression analysis from non-experimental data, is common practice, but is open to dispute: whether or not it is justified with designed experiments is even more debatable. It is conceivable that plots can inadvertently be established on an atypical soil-type with unusual results, but generally dropping of data is not recommended although badly damaged sets of trees in an experimental unit represent an obvious exception.

Graph (3) should ideally be similar to (2): systematic patterns may indicate an inexact relationship between a covariate and the response

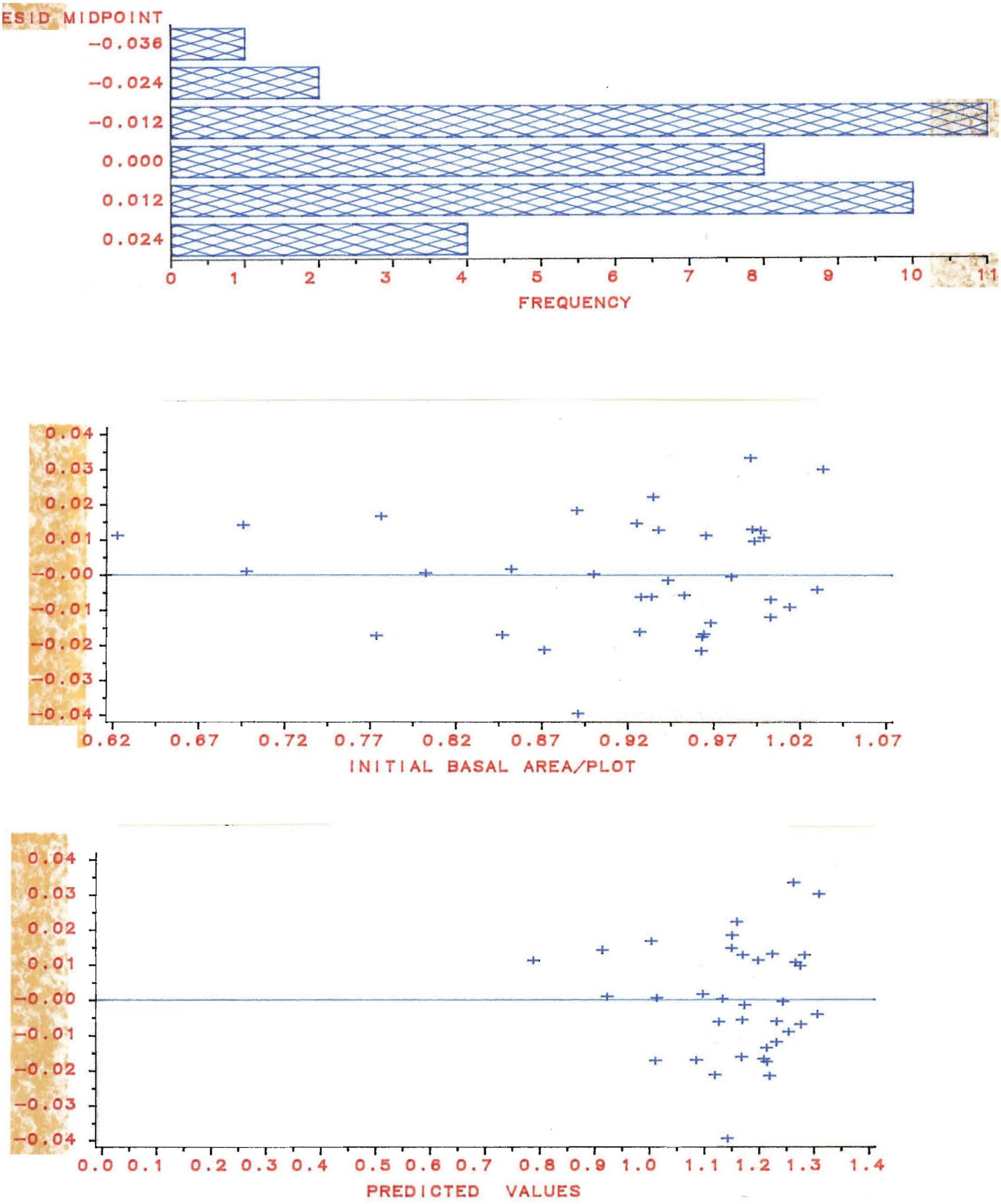

Figure (2.1) Plotting residuals from the data of Crane and Woollons, Buccleuth Forest, N.S.W., Australia.

variable. Figure (2.1) gives the respective plottings for Crane's experiment, analysed above, which show no real sign of serious distortion or systematic patterns.

2.4 STATISTICAL METHODS: SUMMARY

1. Analysis of variance, embracing also analysis of covariance and regression analysis, provides a powerful and flexible methodology for examination of forest fertilizer field trials. The three methods are closely related, and differ essentially only in the structure of the design matrix. The systems are, nevertheless, distinct.

2. Analysis of variance uses models of less than full rank, and requires linear constraints to give least-squares estimates. The underlying model typically involves mean, treatment, block and error effects, but assumes that the response variable is unconfounded by differences in initial conditions or size.

3. Regression analysis uses models usually of full rank, in which the least-squares estimates can be obtained by direct inversion of the X'X matrix. The models are concerned with functional relationships between Y, the response variable, and a set of predictor variables, X_i. Regression analysis can also be utilised to study groups of data, and to test hypotheses concerned with group slopes, intercepts, and/or predicted values, at nominated values of the predictor variable(s).

4. Covariance combines characteristics of both analysis of variance and regression analysis. The design matrix is not of full rank, and the assumed models are similar to those used with analysis of variance, <u>except</u> that they are augmented by an auxiliary

> variable, X, related to the response variable, Y, and referred to as a covariate. The latter can be used to remove initial differences in growing stock characteristics of the experimental units, frequently resulting in a more sensitive estimate of residual error. Adjusted treatment means can be calculated, to cater for such differences. Multiple covariance utilises several covariates, related to the response variable, and its deviations. Covariance assumes a single linear relationship between any covariate and a response variable, independent of factor levels.

The methods summarised above are routinely applied by researchers to the numerous fertilizer experiments established in many forests. Attention is now addressed firstly to the quality and secondly, the relevance of analyses presented in some published literature.

3. STATISTICAL ANALYSIS IN FORESTRY PUBLICATIONS

3.1 INTRODUCTION

Perusal of recent forestry literature provides considerable evidence that, despite a proliferation of statistical texts and availability of statistical computer packages, many researchers have adopted inexact or inappropriate methods for analysing fertilizer experiments. It is not always possible to gauge the consequences of such misapplications, however, due to the dearth of analytical detail presented in many papers.

Results from fertilizer trials appear in many formats - published papers, conference papers, posters, and internal documents. Frequently, statistical details of analysis carried out on trial data are tersely reported, or not at all. There are some mitigating reasons for this; confines of spaces and editorial whims sometimes curb the original intentions of authors, or the writers are essentially non-mathematicians who see little reason to include statistical methods.

Unfortunately, substantial numbers of doubtful statistical procedures occur in unpublished papers, so cannot be alluded to here. But many examples do appear in the published literature and a few are discussed below to illustrate several types of inappropriate analysis.

3.2 EXAMPLES OF FAULTY STATISTICAL ANALYSIS

3.2.1 Erroneous calculation of analysis of variance

Donald (1976) claimed to show that neither site variation nor fertilizer rates affects the relationship between tree height and breast height diameter in forest fertilizer trials; a conclusion refuted by Snowdon (1981) who demonstrated an incorrect analysis of variance procedure had been employed. Donald reached his conclusion by examining five *Pinus* fertilizer experiments, in which he calculated the error mean squares in a way that failed to isolate treatment and blocking effects; that is, these effects were pooled in the error term, thus grossly overestimating the residual error to an extent that three F-values obtained were significantly less than unity, symptomatic of an invalid linear model [Ostle (1963)].

3.2.2 Incorrect use of available degrees of freedom

Auchmoody (1985) developed some interesting methodology for estimating contributory effects of fertilizer response, due to (1) nutrient activity and (2) increased tree size. In demonstrating his system, the author alluded to a young black cherry (*Prunus serotina*) field trial, where the experimental design was five pairs of plots, (blocked for stand density, species, competition, age, and site quality), of which one of each pair was dressed with N, P and K fertilizers. Clearly, there are 10 degrees of freedom available for analysis, but the author extracted 100 trees from the experiment and used regression analysis of basal area at various times after fertilization against initial diameter. The derived equations have R^2 values of between 0.30 and 0.64, but irrespective of that, his approach overlooks that individual trees do not represent a valid independent dataset, thus creating a spuriously high number of residual degrees of freedom. In essence, the system contravenes the assumption of statistical independence of errors, which contravention can lead to major discrepancies in analysis as explained by Eisenhart (1947).

3.2.3 Failure to analyse experiments as to original design

McIntosh (1982) discussed results from a seven-year-old lodgepole pine (*Pinus contorta*) fertilizer experiment, in which forms and rates of nitrogen were investigated in a blocked split-plot design. There is little evidence that the trial was in fact analysed as designed; for example, the blocking outcome is not mentioned, and no analysis of variance with estimates of main and sub-plot error is given.

Griffin *et al.* (1984) investigated the effects of irrigation and fertilizers on cone production of a *Pinus radiata* seed orchard. Five treatments were originally arranged in a latin-square, but the authors decided to ignore the design and one treatment to "simplify the analysis and discussion". In effect, 20% of the available information was ignored, for lack of applying elementary statistical procedures to the data.

Woods (1976) described an establishment 4 x 3 factorial experiment in five randomised blocks, examining four levels of nitrogen and three levels of peat. The analysis involving main effects and interactions, probably leading to separation into linear and quadratic effects, should have been perfectly straightforward. Yet the presented calculations involve Duncan's multiple range test [Duncan (1955)], with contrasts of treatments relative to the control, and between all other treatments. The authors' approach represents a consequential loss of sensitivity, since the internal replication inherent in each main effect is not utilised.

3.2.4 Incorrect analysis for groups of experiments

Groups of experiments require specialised analysis [Yates and Cochran (1938), Cochran and Cox (1966)]. Adams *et al.* (1978) reported results from four factorial fertilizer experiments on Sitka spruce (*Picea sitchensis*). The trials on 14-year-old trees were located on four unique soil types, with appreciable differences in initial growing stock. However, the authors seemingly ignored the guidelines for analysing groups of trials, and no allusion is made as to how the site data were pooled into one (presented) table of analysis; moreover, initial differences in plot growing stock were not eliminated by covariance.

3.2.5 Failure to utilise, or ignorance of, covariance

Covariance was introduced by R.A.Fisher in 1932, illustrating the methodology from a uniformity trial with tea bushes [Fisher (1932), Cochran (1980)]. It is well known as a standard procedure for increasing precision in analysis, by removing the effects of initial differences in size on treatment yield or growth, as summarised here in Chapter 2. Hummel (1947) for example, acknowledges the work of D.J.Finney in using covariance with the Bowmont thinning trial data between 1930-1945 to provide realistic interpretations of results. Yet, there is a lot of evidence that many forest scientists are still unaware of covariance techniques, or fail to utilise them efficiently.

Lee and Barclay (1985) gave details of two 4 x 2 x 2 factorials, involving four levels of N, two thinning levels, and two fertilizer application timings, replicated in two blocks, laid out in 25 and 55-year-old Douglas-fir (*Pseudotsuga menziesii*), from which responses to fertilizer and thinning were reported. Initial basal area in the thinned (younger) plots varied between 18.2 to 23.1 m^2/ha, and in the unthinned from 20.6 to 40.1 m^2/ha. Plot densities went up to 8441 stems/ha, and complicated patterns of mortality emerged for both experiments, some apparently associated with nutrient application. Amid all this variation and dissimilar conditions within and among treatments, the authors state each age was subjected to a single analysis of variance, ignoring the large variation in initial size and the differential mortality. Nor did the authors consider the implications of including thinning as an experimental factor in one analysis [see Woollons (1985)].

Turner (1982) described a long running *Pinus radiata* fertilizer experiment, involving five rates of superphosphate applied to plots nested in four randomised blocks. One block was located on a differing soil type; for unstated reasons, data from this block were rejected and eliminated from the analysis. The fertilizers were applied at ages four to five, and a thinning was executed at age 16, yet no mention was made of testing for initial differences in plot growing stock nor any allowance for differential thinning yields; furthermore the analysis presented was limited to L.S.D. tests.

Gagnon (1965) compared the growth of 30 red pine (*Pinus resinosa*) trees, fertilized with and without magnesium and potassium. The trees were put into matching pairs (by initial size and crown competition). The pairing was naturally approximate, and two pairs were in fact significantly different. Nevertheless, he applied a paired t-test to the data and was able to detect a significant height response five years after treatment. While he rightly recognised that initial differences could seriously confound the true response, it is clear that the more relevant technique, covariance, was not considered.

Olson *et al.* (1979) pointed out that fertilizer responses in western hemlock (*Tsuga heterophylla*) are often confounded by the magnitude of initial tree size, and described a procedure to remedy this by extrapolating pre-treatment diameter growth (obtained by increment cores), and analysing the difference between predicted and measured diameters. While not invalid, the methods are time-consuming and approximate and, throughout the article, the topic of covariance is not mentioned.

The above examples demonstrate that forest scientists are failing to adopt the most appropriate statistical methods with which to analyse forest fertilizer experiments, and seemingly prepared to discard data which otherwise might enhance precision in analysis, or are totally unaware of basic statistical procedures. These oversights, moreover, are not being picked up by referees. Three published experiments are now re-analysed in some detail to reinforce these general criticisms.

3.3 REANALYSIS OF THREE PUBLISHED EXPERIMENTS

3.3.1 The experiment of Hunter *et al.* (1986)

Data from Hunter *et al.* (1986) were forwarded for re-examination.[1] The trial was a pruning x fertilizer x thinning study, laid out in five-year-old *Pinus radiata* at Woodhill, North Auckland. Three replications of each treatment were employed in randomised blocks.

Hunter's original analysis essentially involved two analyses of covariance, one each for thinned and unthinned, and regarding pruning and fertilization as a 2^2 factorial. Using basal area per hectare, 1983, as a response variable, and basal area per hectare, 1981, as a covariate, gives:

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[1] The writer is grateful to the senior author for kindly supplying the data

ANOVA

Source	d.f.	SS (thinned)	p > F	SS (unthinned)	p > F
Fertilization	1	17.57	0.0003	23.72	0.0006
Pruning	1	6.41	0.0052	0.94	
Pruning x fertilization	1	0.04		1.83	
1981 b.a./ha (covariate)	1	5.60	0.0018	95.19	0.0001
Blocks	2	1.20		1.11	
Error	5	1.424		3.509	
	11				

Thus, for both regimes the effect of fertilizer is strongly significant, as is the covariate. Pruning is significant, but only for the thinned regime. The blocking is ineffective, and there is no pruning x fertilizer interaction for either regime.

Estimated (adjusted) responses are:

(m^2/ha)	Thinned	Unthinned
Control	14.6	22.7
Fertilized	16.9	24.7
Pruned	12.9	21.3
Pruned and Fertilized	15.5	25.0

95% confidence intervals for the fertilizer responses are (m^2/ha)

Thinned	2.3 ± 1.5
Unthinned	2.0 ± 1.6

These calculations give a plausible analysis of the trial, but it can be questioned whether the procedures are recovering all the available information inherent in the experiment. Specifically:

1. Only five degrees of freedom are available for error estimation, leading inevitably to insensitive hypothesis testing, and to too wide confidence intervals;
2. No information is available on possible differences in the respective regression coefficients associated with the thinning regimes.

An alternative analysis is to regard the experiment in terms of a regression model

$$E(Y) = \sum_{i=1}^{8} \alpha_i + \sum_{j=1}^{2} \beta_j X + \sum_{k=1}^{3} \gamma_k R \qquad (3.1)$$

where

Y = the response variable (m^2/ha, 1983)

X = the covariate (m^2/ha, 1981)

α_i = intercepts representing the 8 fertilizer and pruning treatments

β_j = regression coefficients for the two thinning regimes

γ_k = parameters representing the three blocks, R.

An analysis of variance for model (3.1) is:

Source	d.f.	SS	$p > F$
Treatments	7	49.27	0.0003
Slopes	2	122.26	0.0001
Blocks	2	1.62	
Error	12	5.1298	
	23		

Thus, the effects of the treatments and regression slopes are strongly significant, but the blocks are non-significant and are dropped from the model.

An hypothesis, $H_o: \beta_t = \beta_u$, where the subscripts t, u, refer to thinned and unthinned regimes, is tested by

$$(1.4146 - 1.8812)/\surd(0.4274[0.282 + 0.032]) = 1.28 \qquad (3.2)$$

where the numerator in (3.2) represents the difference of the two estimated regression coefficients, and the denominator the pooled standard error of the regression coefficients obtained from the error mean square, and the Gaussian multipliers from the $(X'X)^{-1}$ matrix, (2.7). Test (3.2) is distributed as Student's t, but is not significant at the 5% level. Nevertheless, it is decided to retain the separate regression coefficients[1] and test for treatment differences <u>within</u> each thinning class. If the subscripts, p, denote pruning, and f, denote fertilization, then for <u>unthinned</u>

$$H_o: \; E(Y_{pf}/\bar{X}_u) \; = \; E(Y_f/\bar{X}_u)$$

and for <u>thinned</u>

$$H_o: \; E(Y_t/\bar{X}_t) \; = \; E(Y_{pf}/\bar{X}_t)$$

are both accepted, evaluated by the test statistic (2.21) and $\bar{X}_t$, $\bar{X}_u$ refer to the mean initial basal area/ha of each treatment. Pooling the equivalent treatments gives the following analysis of variance:

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[1] See section (4.2.2) for a rationale of this decision

ANOVA

Source	d.f.	SS	p > F
Treatments	6	47.64	0.0001
Slopes	2	147.79	0.0001
Error	16	8.389	
	24 (uncorrected)		

Testing of the hypotheses

$$H_o: E(Y_t/\overline{X}_t) = E(Y_{ft}/\overline{X}_t)$$

$$H_o: E(Y_t/\overline{X}_t) = E(Y_{tp}/\overline{X}_t)$$

however, are both rejected at the 0.1 level, and hypotheses

$$H_o: E(Y_u/\overline{X}_u) = E(Y_{fu}/\overline{X}_u)$$

$$H_o: E(Y_u/\overline{X}_u) = E(Y_{pu}/\overline{X}_u)$$

are rejected at the 1% and 5% level, respectively.

Estimated yields from this analysis, are: (m^2/ha).

	Thinned	Unthinned
Control	15.0	22.7
Fertilized	16.9	24.8
Pruned	12.9	21.3
Fertilized, pruned	15.0	24.8

and 95% confidence limites for the fertilizer responses are: (m^2/ha).

Thinned	1.9 ± 1.14
Unthinned	2.1 ± 1.10

The final model is illustrated in Figure (3.1).

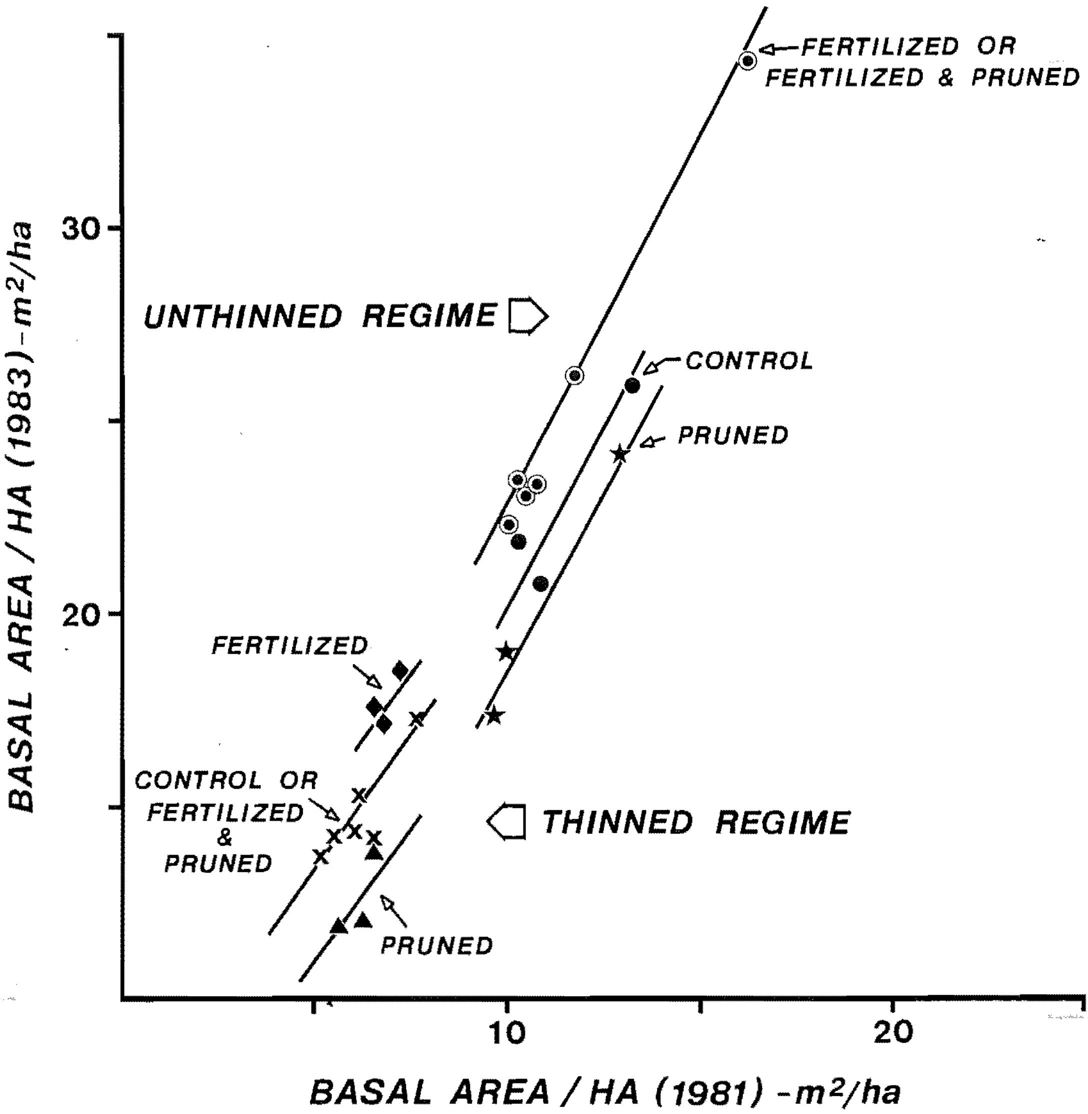

Figure 3.1 Final model derived from the data of Hunter *et al.* (1986).

The two methods of analysis have naturally given very similar estimated responses to the treatments. Nevertheless, there are strong grounds for preferring the combined analysis as opposed to splitting the data, because:

(a) The error term has more degrees of freedom, giving a more robust error mean square and lower t-value for confidence interval determination;

(b) the regression slopes can be tested for significance, that is, a test of H_0: $\beta_t = \beta_u$ is available.

Thus, the precision of the combined analysis is superior to the split calculations, essentially as a consequence of more degrees of freedom available for the experimental error. The estimated mean square reflects a pooled figure for two regimes, so although it is a little higher than the value estimated for the thinned regime, it is associated with 16 (compared with five) degrees of freedom. Subsequent tests then become more statistically sensitive. This is further reflected in the size of the respective confidence intervals calculated for the fertilizer responses by both methods, which are reduced in the combined analysis. Consequently, pruning becomes significant in the unthinned regime, whereas it was deemed non-significant in the earlier analysis.

By retaining the full data-set, a direct comparison of the thinned and unthinned regimes is feasible. The regression coefficient for thinned is less than unthinned, albeit not significantly so. This latter result can be explained by the lack of dispersion of the thinned data, with consequent high variance.

3.3.2 The Experiment of Salonius *et al.* (1982)

Salonius *et al.* (1982) presented the results of a 3^2 NP factorial black spruce (*Picea mariana*) experiment established in 1968 in central New Brunswick, Canada. Treatments were replicated twice, except the controls, which had four replications. Nitrogen, applied as urea, was broadcast at 0, 168, 336 N kg/ha, and Phosphorus, applied as triple superphosphate at 0, 112 and 224 P kg/ha. The stand was approximately 60 years old at the time of treatment. Experimental plots were 0.02 ha, with an inner 0.01 ha measurement area.

Published data included that given in Table 3.1 (reproduced from Salonius, Table 1).

Table (3.1): Data of Salonius *et al.* (1982)

Treatment	Plot	Δd	Δd	ΔG	ΔG
		(mm)		(m^2/ha)	
		1957-67	1967-77	1957-67	1967-77
n_2p_0	1	17.44	10.18	6.52	4.34
n_1p_2	2	16.14	11.66	7.97	6.54
n_0p_2	3	17.86	11.12	8.57	5.89
n_1p_0	4	20.13	11.03	8.34	5.16
n_1p_2	5	21.51	12.30	10.76	6.99
n_2p_2	6	17.98	9.80	6.12	3.76
n_2p_1	7	15.20	9.84	5.44	3.84
n_1p_0	8	16.52	11.16	5.47	3.99
n_0p_0	9	17.44	9.04	8.73	5.09
n_0p_1	10	20.60	10.61	9.82	6.00
n_0p_0	11	22.13	10.92	6.64	3.78
n_2p_1	12	20.27	10.90	7.46	4.57
n_0p_1	13	20.86	11.77	8.56	5.49
n_2p_0	14	19.26	13.00	7.10	5.45
n_1p_1	15	20.35	12.10	6.34	4.51
n_0p_0	16	18.26	9.69	8.06	4.82
n_2p_2	17	16.53	9.36	6.77	4.24
n_0p_2	18	14.82	8.10	6.90	4.21
n_0p_0	19	15.94	7.73	6.71	3.57
n_1p_1	20	13.25	9.52	6.07	4.81

Salonius evidently experienced considerable difficulty in coping with the differences in basal area increment, after observing that the variation in growth was caused largely by differences in plot growth before application of fertilizer. Thus the writers observed:

> *This confounds the measurement of response to treatment (s) when post-treatment growth between plots is compared because growth rates appear to be more influenced by pre-treatment growth rates than by the fertilizer treatment. The built in assumption of conventional methods (1) is that growth rates before treatment, stocking and population structure (individual diameter distribution) are similar on treated and control plots and any differences in growth rates after treatment result from fertilizer addition.*

Salonius then proceeded to express post-treatment basal area increment as a percentage of pre-treatment growth and rank the treatments in factorial order, from which it was deduced treatments n_1p_1 and n_1p_2 were the most effective. Elsewhere, an analysis of variance of percentage basal area increment was reported to have significant treatment effects, (without presenting any analysis of variance table) but percentage mean diameter did not produce statistical significance or of a magnitude "*probably not one which would encourage foresters to invest in fertilizers for the management of such old and unresponsive stands which are in definite decline*".

From the above description, it is clear that Salonius is either unaware of covariance methods to cope with differences in initial quantities of the experimental units, or else dismissed their potential capabilities.

The published data do not include plot stocking, so basal area/tree cannot be used as a possible response variable. However, utilising post-treatment basal area/ha increment (Y) and assuming a linear model

---

[1] (presumably analysis of variance)

$$E(Y) = b_o + b_1N + b_2N^2 + b_3P + b_4P^2 + b_5NP + b_6N^2P + b_7NP^2 + b_8N^2P^2 + \beta X \tag{3.3}$$

where

X = pre-treatment basal area/ha increment

N, P = the effects of Nitrogen and Phosphorus, separated into linear and quadratic components [see, for example, Ostle (1963), Mendenhall (1968)]

b_i = coefficients associated with treatments

β = a regression coefficient

ANOVA of (3.3) then gives: (using orthogonal polynomials given below, and considering a reduced model to calculate each treatment sum of squares)

Source	d.f.	SS	p > F
N_1	1	0.0124	
N_q	1	1.3053	0.0152
P_1	1	0.3119	
P_q	1	0.0099	
N_1P_1	1	1.0499	0.0254
N_qP_q	1	0.0357	
N_1P_q	1	0.1766	
N_qP_1	1	0.5982	0.0758
Covariate	1	3.5418	0.0007
Error	10	1.5247	
	19		

Two components (N^2 and NP) are significant at least at the 2.5% level, while the covariate pre-fertilization basal area increment is very strongly significant. Growth (adjusted for the covariate) of the nine treatment combinations is illustrated in Figure (3.2). The response surface is quite complex, with higher dosages of Nitrogen in combination with Phosphorus giving meagre responses, but lighter rates with

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Orthogonal polynomials used:				
	-9	1	11	(linear)
	3	-8	4	(quadratic)

Phosphorus strongly enhancing growth. The optimum combination is 168 kg N, 224 kg P/ha which gives an estimated 1.58 m^2/ha additional basal area increment, in the 10 years (37%), relative to the control. This response is significant at the 0.005% level.

The inability of Salonius to analyse the experiment adequately has had devastating consequences - several treatment combinations are clearly responsive and the boost in growth rate is quite substantial. Note that it is possible that the analysis may be even more favourable if the effect of stocking had been included as an additional covariate, or used to calculate basal area increment/tree as a response variable and an alternative covariate.

3.3.3 The Experiment of Gerig *et al.* (1978)

Gerig *et al.* (1978) have also given analytical details of a 3^2 NP factorial experiment, but in loblolly pine (*Pinus taeda*) established in 1966 at Andrews, South Carolina, U.S.A. There were four randomised blocks utilised, each of which was augmented by an additional treatment, an NP combination plus trace elements. Nitrogen was applied in the form of ammonium nitrate at 0, 56 and 112 kg N/ha, and Phosphorus as triple superphosphate at 0, 28 and 56 kg P/ha. The layout was installed in a thinned (approximately 494 stems/ha) 19-year-old stand, in plots 0.10 ha, with inner 0.04 ha measurement plots. Diameter at breast height was measured before treatment in late 1966 for all trees, and annually for each of the five years thereafter. Tree mortality was low and unassociated with fertilizer, so dead trees were removed from the dataset.

Gerig performed MANOVA and ANOVA analysis on the measurements over the

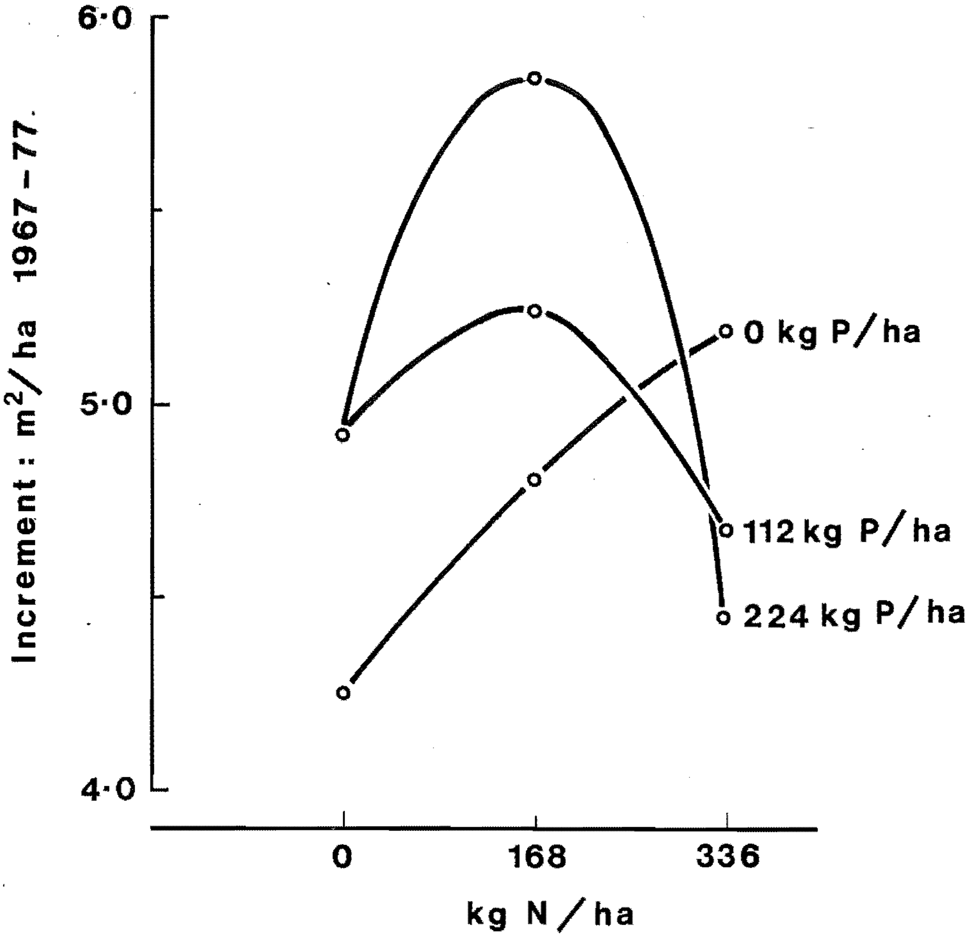

Figure (3.2) Growth responses from the experiment of Salonius *et al.* (1982).

five years. The blocking was found to be ineffective and removed from subsequent analysis. Initial basal area per hectare showed itself to be a strong covariate, but treatment responses were generally transient. Small nitrogen effects were detected in 1967 and 1968, but no significant phosphorus or nitrogen-phosphorus interactions emerged. According to Gerig "These analyses did not reveal expected phosphorus effects nor nitrogen-phosphorus interaction. We felt the analyses used were simply not sensitive enough to detect such effects".

Gerig then abandoned least-squares methodology and examined the data by an exploratory technique developed by Tukey (1977), the so-called two-way median or resistant-fit analysis, and, using this procedure, the authors were led to deduce that by 1972, combinations of nitrogen and phosphorus had stimulated growth. They refrained, however, from giving any estimate of response, and were unable to give any measure of precision. Several contradictions and obscurities appear in their procedures. Having dismissed least-squares as a sensitive tool, they utilised covariance to adjust the response variables for initial differences in size. Two levels of phosphorus were combined because "there is little or no difference between them", presumably decided by subjective judgement. An obscure footnote was given in conjunction with the median-fit table indicating that residual effects larger than 0.5 could be considered 'real', citing Tukey (1977), but without any elaboration.

The data are re-examined here[1] using least-squares methods. The average increase in basal area (m^2/ha) for the ten treatments, 1966-1972 are:

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[1] The author is grateful to the fourth author, Dr. Carol Wells, for making the data available

		Nitrogen		
		n_0	n_1	n_2
	p_0	7.0	7.1	6.4
Phosphorus	p_1	6.6	7.5	8.0
	p_2	6.9	7.4	7.3

n_2p_2 + trace elements = 7.7

These figures suggest that nitrogen x phosphorus combinations may have superior growth to N and P alone dressings, but highlights that any responses are very small in magnitude. Plot stockings vary between 346 and 618 live stems/ha, so the response variable (Y) was defined as

$$Y = \text{basal area/tree, 1972}$$

The rates of N and P in the additional treatment are identical to the n_2p_2 combination and preliminary covariance analysis (assuming ten treatments in a randomised block design), shows no sign of disparate growth, so it is pooled with the n_2p_2 treatment. (Gerig, for unstated reasons, ignored these data in the published analysis).

Using a model

$$E(Y) = b_0 + b_1N + b_2P + b_3NP + b_4R + \beta X \qquad (3.4)$$

where

Y = response variable (previously defined)

X = a covariate, basal area/tree, 1966

β = a regression coefficient

N, P = effects of nitrogen and phosphorus

b_i = coefficients

R = effect of blocks

gives an ANOVA:

Source	d.f.	SS ($\times 10^6$)	$p > F$
Covariate	1	527.90	0.0001
Blocks	3	24.65	0.0444
N	2	6.71	
P	2	6.15	
NP	4	4.89	
Error	27	72.13	
	39		

The effect of blocking is significant at the 5% level, and the covariate is very highly significant (0.1%). (Gerig utilised the logarithm of basal area/ha as the response variable and covariate, to '<u>stabilise the error variance</u>'. A plot of Y on X shows no sign of heterogeneity, so their method may suffer because responses are expressed as geometric means, which when re-transformed, will give biased estimates.) No treatment effects are significantly responsive, however. Additional available data for the experiment include estimates of each plot soil-nitrogen levels, before fertilization. This variable, if included as a second covariate, gives the following ANOVA (subdivided into linear and quadratic effects).

Source	d.f.	SS $(X10^6)$	$p > F$
Initial basal area/tree	1	550.97	0.0001
Initial soil nitrogen	1	23.27	0.0016
Blocks	3	15.25	0.0660
N	2		
N_l	1	0.69	
N_q	1	0.34	
P	2		
P_l	1	5.70	0.0933
P_q	1	5.76	0.0917
NP	4		
N_lP_l	1	0.50	
N_lP_q	1	0.76	
N_qP_l	1	2.94	
N_qP_q	1	1.44	
Error	26	48.85	
	39		

The inclusion of initial soil nitrogen (significance approaching the 0.1% level), reduces the error mean square by 30%, and the effect of phosphorus, in terms of its linear and qudaratic components, becomes weakly significant (10%). The blocking just fails to be significant at 5%, some previous variation now being explained by the second co-variate. The adjusted yields of the nine treatments (m^2/ha, 1972) are:

	Nitrogen		
	n_0	n_1	n_2
p_0	28.7	28.8	28.5
p_1	28.9	29.6	29.6
p_2	29.2	29.0	29.3

These results emphasize the very small responses present in the experiment, but nevertheless a contrast between p_0 and p_1 with p_2 is significant at the 2.5% level, with estimated response 0.57 m^2/ha/5 years or 2%, with a 95% confidence interval given by $\pm$ 0.50 m^2ha for five years. The likelihood of a linear phosphorus response reflects the difference between the p_0 and p_2 levels, and the quadratic effect the superiority of the p_1 level relative to p_0 and p_2. There is a suggestion of combinations n_1p_1 and n_2p_1 being optimal, but the absence of nitrogen x phosphorus interaction being significant, at least at the 10% level, makes this conclusion speculative.

This analytical approach has shown, therefore, that utilising multiple covariance procedures has provided some evidence that phosphorus has enhanced growth, up to five years after application. Use of least-squares methodology has:

1. allowed utilisation of all the available experimental units;
2. efficiently accounted for initial differences in plot growing stock;
3. demonstrated the existence of fertility gradients inherent in the blocks;
4. isolated significant differences in initial soil N levels which have affected consequent growth, and have not been removed by the blocking structure;

5. allowed the error mean square to be reduced by 30%, sufficient to suggest a clear phosphorus effect;
6. given a formal method of testing for treatment effects, and allowed a confidence interval to be constructed to demonstrate the precision of response.

Tukey's median-fit analysis has allowed Gerig to specify some nitrogen x phosphorus combinations as active elements, but whether it represents a superior technique to least-squares is extremely doubtful, and their conclusions can be challenged. Claims by Gerig that least-squares analysis is insufficiently sensitive to detect fertilizer responses in forest experiments are apparently unfounded, at least for these data; ANOVA methodology has secured considerably more information than their preferred technique.

3.4 SUMMARY OF CITED EXAMPLES

The preceding citations and re-analyses of data represent a sample of published papers where there is clear evidence that researchers have had difficulty in applying basic statistical techniques to forest fertilizer experiments. The examples cover papers from Australia, Canada, England, United States of America, South Africa, and New Zealand, all from reputable scientific journals. The evidence is therefore sufficient to conclude that unsatisfactory or non-optimal statistical methodology is not an artefact of one organisation.

In preparing this critique of forest fertilizer analysis, nearly 90% of researched articles could not be questioned, in that the presented analysis were so tersely reported that objective judgement was impossible to formulate, a conclusion supported by Warren (1986). Indeed the standard of statistical analysis could even be much worse than examples presented here would suggest.

It should not be construed, however, that all analysis of fertilizer experiments are seen to be inadequate. For example, Miller and Cooper (1973) were able to extract considerable information from *a* Corsican pine (*Pinus nigra*) experiment involving five rates of nitrogen in three randomised blocks. Intensive foliar sampling and stem sampling from ring-widths examined under a microscope were adopted, and the data were thoroughly examined by clearly explained and appropriate statistical techniques. Whyte and Mead (1976) devoted considerable effort to secure an optimal covariate to adjust for initial differences in plot volume, when analysing a NP factorial experiment in mature *Pinus radiata*, and suggested ways of minimising the size of confidence intervals by various sampling strategies. Miller and Tarrant (1983) examined the long-term response of Douglas-fir (*Pseudotsuga menziesii*) to applications of ammonium nitrate, taking care to adjust initial plot differences by covariance, and utilised orthogonal polynominals to highlight response trends. Snowdon and Waring (1985) efficiently analysed a $4^2 \times 2^2$ factorial establishment fertilizer trial where linear and quadratic components of response were used to describe early responses and the procedures were clearly explained.

Obviously not all forest researchers need have the technical skills necessary to extract small differences in treatments from an ecosystem as complex and variable as is encountered in forest stands, but there is a pressing need for them to recognise the existence of statistical systems which have been well known to statisticians for over 60 years. Analyses such as those presented here can be utilised to extract considerable amounts of information from forest fertilizer experiments.

4. ANALYSIS OF FERTILIZER EXPERIMENTS IN ESTABLISHED STANDS

4.1 A FERTILIZER TRIAL WITH TWO TREATMENTS

4.1.1 Introduction

In the previous chapter it was conclusively established that forest scientists frequently have difficulty in analysing later-age fertilizer trials adequately. The purpose of this chapter is to explore these problems in some depth, and to propose methodology that is able to extract the best precision and information from pole-crop fertilizer experiments.

Consider an experiment in which fertilizer was applied to a number of recently thinned plots, while other thinned plots were left unfertilized. Inevitably, the amount of growing stock at the time of application of fertilizer would have varied from experimental plot to plot, even if specific effort was made to achieve some degree of uniformity. Subsequent growth will be strongly influenced, consequently, by quanta of initial growing stock, (a critical point apparently overlooked by Lee and Barclay (1985), Turner (1982) or mishandled by Salonius *et al.* (1982), and Gagnon (1965), as described in Chapter 3). Moreover, the net basal area/ha difference equation proposed by Clutter (1963) is

$$\ln(G_2) = \ln(G_1)\ (T_1/T_2) + \alpha(1-[T_1/T_2]) \qquad (4.1)$$

where G_2, G_1 = net basal area/ha at ages T_2 and T_1 respectively

α = an estimated parameter, denoting an upper limit to yield

The above equation (4.1) and variants thereof [Clutter *et al.* (1983), Clutter and Jones (1980), Pienaar *et al.* (1985), Woollons and Hayward (1985)] have been successfully utilised to predict future basal area/ha in several growth models of different species and the dynamics of (4.1) can be taken to reflect usual stand (plot) development. Inspection shows that, apart from extraordinary mortality over the period, if $G'_1 > G_1$ at time T_1, then $G'_2 > G_2$ for all $T_i > T_1$, thus vindicating the above proposition. Thus, to isolate the effect of any additional growth caused by nutrient applications, the effect of initial growing stock must be accounted for.

Figure 4.1(a) shows graphically the kind of result one can obtain for fertilized and control units, when six replications of each are measured in some way at time of fertilization and subsequently in terms of an appropriate response variable. At first inspection, it is intuitive that the fertilizer has boosted growth, but the situation is complicated in that the control plots are generally larger initially. Nevertheless, the growth of each treatment appears disparate. If the fertilizer response is estimated by the difference of average treatment values of the response variable alone, it is certain to be underestimated, and no information is available on the likely true responses in the fertilized plots which were initially smaller than average.

These kinds of raw data can be satisfactorily analysed by least-squares methods as follows. Let

$$E(Y) = \alpha_f + \alpha_c + \beta_f X + \beta_c X \quad \text{and} \quad V(\underset{\sim}{Y}) = I\sigma^2 \tag{4.2}$$

where α_f and α_c are intercepts associated with dummy variables for fertilized and control treatments with slopes β_f and β_c, Y is the response variable (growing stock at some later age), X is a concomitant variable (quantum of growing stock at fertilization), and the operator E depicts statistical expectation. More generally, if fertilized and control treatments are replicated n times, the design matrix applicable for model (4.2) is

$$X = \begin{vmatrix} 1 & 0 & X_{1f} & 0 \\ 1 & 0 & X_{2f} & 0 \\ . & . & . & . \\ . & . & . & . \\ 1 & 0 & X_{nf} & 0 \\ 0 & 1 & 0 & X_{1c} \\ 0 & 1 & 0 & X_{2c} \\ . & . & . & . \\ . & . & . & . \\ 0 & 1 & 9 & X_{nc} \end{vmatrix} \qquad (4.3)$$

Transposing (4.3) and multiplying gives

$$X'X = \begin{vmatrix} n & 0 & \Sigma X_f & 0 \\ 0 & n & 0 & \Sigma X_c \\ \Sigma X_f & 0 & \Sigma X_f^2 & 0 \\ 0 & \Sigma X_c & 0 & \Sigma X_c^2 \end{vmatrix} \qquad (4.4)$$

(4.4) is of full rank, and with alternative zero elements can be inverted as two 2 x 2 matrices, leading to the estimates of α and β, and their respective variances. The appropriate workings and formulae are given in Appendix 2.

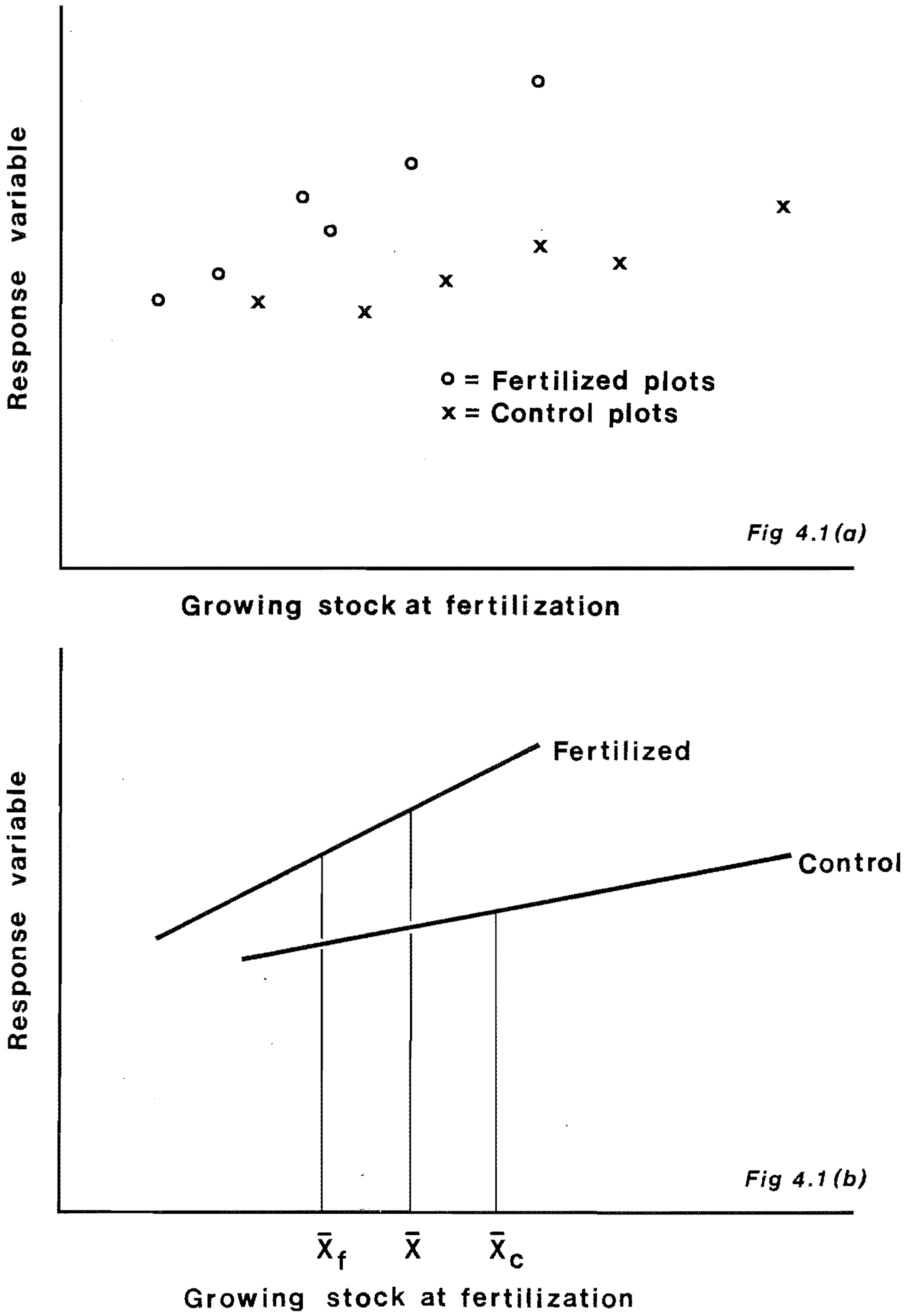


Figure 4.1(a) and 4.1(b) Illustration of fertilizer response in a simulated experiment of two treatments.

4.1.2 Tests of Hypotheses for two treatments

In estimating the four parameters in (4.2), it is natural to establish whether two regression slopes are justified. This problem can be resolved by

$H_o : \beta_f = \beta_c$, which can be tested by

$$(\hat{\beta}_f - \hat{\beta}_c)/\surd(\sigma^2[1/\Sigma x^2_f + 1/\Sigma x^2_c]) \sim t_{(2n-4)} \qquad 4.5)$$

where σ^2 = residual mean square

Σx_i^2 = corrected sum of squares for fertilized or control treatments

t = students-t distribution, with (2n-4) degrees of freedom

A logical continuation of the analysis is to investigate whether the observed differences in predicted values are statistically real. It is reasonable to assume that a fertilizer response exists only when fertilized plots grow faster than unfertilized ones containing equivalent amounts of growing stock. Following the methods of Chapter (2.2)

Let X_o be a selected value of X, and if

$X'_f = (1\ 0\ X_o\ 0)$ and $X'_c = (0\ 1\ 0\ X_o)$, and in addition $\underset{\sim}{b} = \begin{vmatrix} \alpha_f \\ \alpha_c \\ \beta_f \\ \beta_c \end{vmatrix}$

then the predicted mean estimates for $X = X_o$ are

$$\hat{Y}'_f = X'_f\underset{\sim}{b} \quad \text{and} \quad \hat{Y}_c = X'_c\underset{\sim}{b} \qquad (4.6)$$

So a predicted difference $Y_f - Y_c$ is given by

$X'_f\underset{\sim}{b} - X'_c\underset{\sim}{b}$ with variance

$$\sigma^2(X'_f[X'X]^{-1}X_f + X'_c[X'X]^{-1}X_c) \qquad (4.7)$$

Thus, a test of the predicted difference between fertilized and control regression lines (depicted in Fig. 4.1 b) is given by the hypothesis

$$H_o: E(Y_f/X_o) = E(Y_c/X_o) \tag{4.8}$$

and is tested by the statistic

$$(X'_f b - X'_c b)/\surd(\sigma^2[X'_f\{X'X\}^{-1}X_f + X'_c\{X'X\}^{-1}X_c])$$

for t, with (2n-4) degrees of freedom (4.9)

identical to the general case given in Chapter (2.2).

The test (4.9) is a function of X_o, the chosen value of X, and usually this is taken at $\overline{\overline{X}}$, the grand mean of the two treatments (see Fig. 4.1 b). In the case where $\beta_f = \beta_c$, that is, where a common regression coefficient is utilised, then hypothesis (4.8) and test (4.9) are equivalent to the familiar covariance test for adjusted means, Snedecor and Cochran (1967), p.423, with variance

$$\sigma^2(2/n + [\overline{X}_f - \overline{X}_c]^2/\Sigma x^2) \tag{4.10}$$

When both β_f and β_c are required, (4.9) gives an efficient test for examining regions of response for differing values of X, initial growing stock. For example, it may transpire that the fertilizer response can be declared significant only beyond a minimum value of X, indicating that suppressed trees with low initial growing stock have not responded to nutrient application.

The calculations of (4.2) through to (4.9) for the simulated data depicted in Figure 4.1(a) are documented fully in Appendix 2.

4.2 RECOMMENDED PROCEDURES FOR ANALYSING FERTILIZER EXPERIMENTS IN ESTABLISHED STANDS

The system presented for the simple case of two treatments, can be extended to a general and systematic procedure for analysing types of fertilizer experiments where a covariate representing initial size is required.

Three broad classes of fertilizer trials can be usefully recognised:

1. Experiments involving t fertilizer treatments, not in factorial arrangement;
2. Experiments involving t fertilizer treatments, not in factorial arrangement, applied to r thinning regimes or species;
3. Experiments involving p^n treatments comprising n factors at p levels in factorial combination.

These three classes obviously do not constitute an exhaustive list, but serve to categorise most fertilizer experiments met in practice.

4.2.1 Trials involving t treatments

To analyse an experiment with t fertilizer treatments, the following sequential steps are suggested:

(a) Secure a plotting of the response variable against the covariate by treatments, then consider the model

$$E(Y) = \alpha + \beta f(X) \qquad (4.11)$$

where α and β are respectively intercept and regression coefficients for the pooled treatments, and f(X) some function of X. A graph like Y on X, depicted in Fig. 4.1(a), will usually, but by no means always, show the relationship to be linear; that is, $f(X) = X$. If not, some transformation, possibly $f(X) = \ln(X)$, where ln = logarithm to base e, is required, to linearize the relationship. Figure (4.2) shows part of the data from an unpublished thinning x fertilizer x pruning trial, from Hunter (in-house data). Models with and without a logarithmic transformation give error mean squares of 0.306 and 0.402, a 24% difference.

(b) Having secured a covariate appropriate to the circumstances, such that Y and f(X) have a linear relation, test

$$H_0 : \beta = 0$$

Rejection of H_0 implies the covariate f(X) is justified, which in practice is almost always the case. (For a majority of experiments the significance of β is self-evident from a graph.)

(c) Modify (4.11) to

$$E(Y) = \sum_{i=1}^{t} \alpha_i + \sum_{i=1}^{t} \beta_i f(X) \qquad (4.12)$$

and test $H_0 : \beta_1 = \beta_2 = \ldots, \beta_t$. Rejection of H_0 implies disparate slopes are present. Acceptance infers a common slope. Usually the latter hypothesis will be true, or sometimes the t treatments can be represented by, say, two slopes. Repeated testing of H_0, with treatments possessing similar slopes and progressively pooling into subgroups, should resolve the position. It is advantageous to isolate the number of (true) regression slopes needed in (4.12), so as to find an appropriate and parsimonious structure for the underlying relationships.

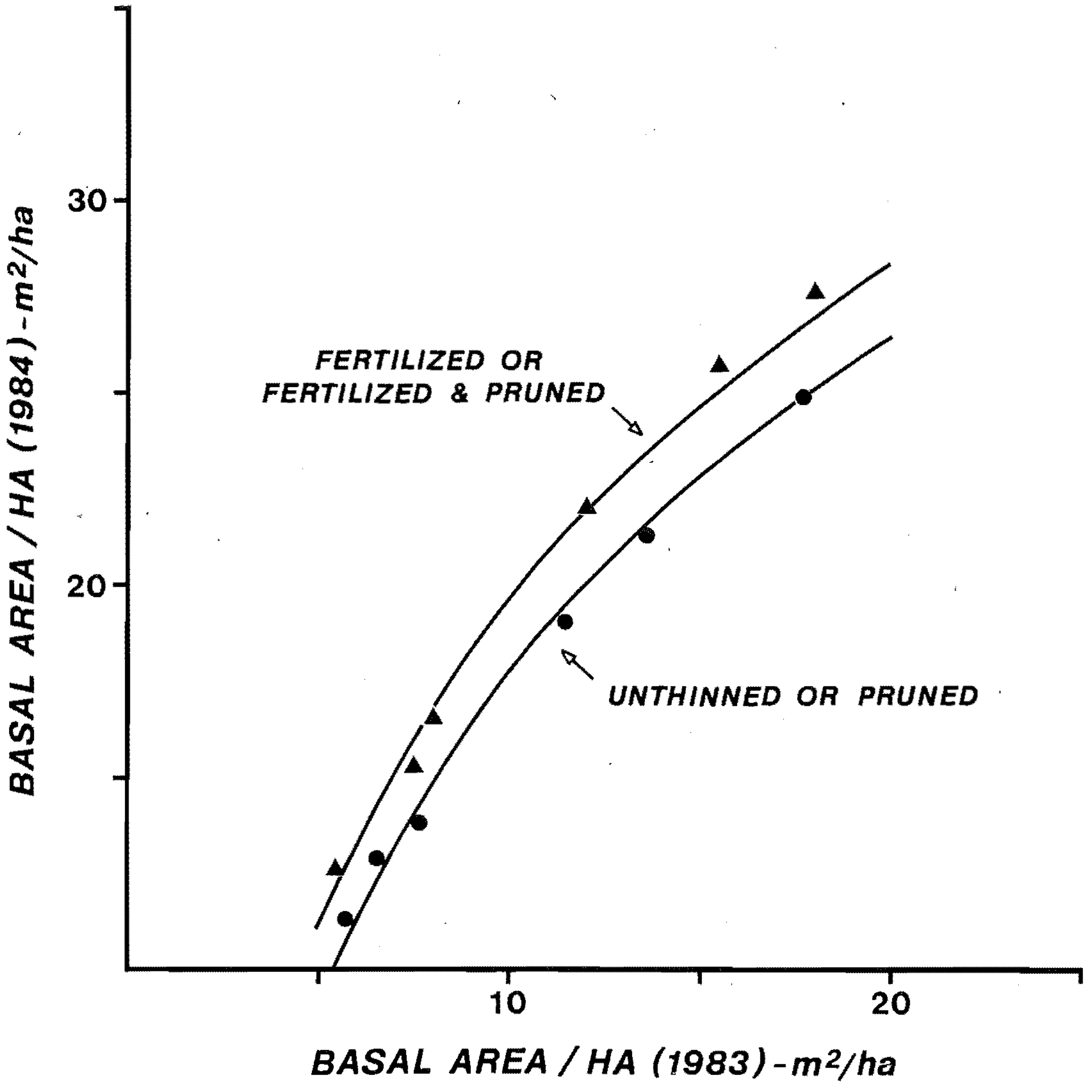


Figure 4.2 In-house trial data of I.Hunter. Illustration of a curvilinear relationship between response variable and covariate.

(d) Assuming s ($\leqslant$ t) statistically independent slopes are present, reduce (4.12) to become

$$E(Y) = \sum_{i=1}^{t} \alpha_i + \sum_{i=1}^{s} \beta_i f(X) \qquad (4.13)$$

and, if relevant, augment (4.13) to include any available additional covariates, and again test for functional form, significance and disparity of treatments. (It is advisable to defer the inclusion of secondary covariates until the s independent regression coefficients have been determined; the residual mean square in model (4.12) is frequently quite large before determination of the slopes, to an extent that additional covariates could be erroneously declared non-significant.)

(e) Test H_0: $E(Y_i/\bar{\bar{X}}) = E(Y_j/\bar{\bar{X}})$ i,j($\leqslant$ t), using the test statistic (2.21), for all levels of α_i represented by a specific β_i. Rejection of H_0 implies the i th treatment response is significantly different to the j th treatment, but acceptance infers the i th and j th treatments have alike response and the respective intercepts can be pooled. Usually the t treatments will fall into a small number of groupings, and this will again increase the degrees of freedom available to estimate the residual mean square.

(f) Calculate and plot residuals against predicted and covariate values, as well as overall [Draper and Smith (1981)], p.142. (For small experiments, this step is not necessary.)

(g) Assuming q ($\leqslant$ t) statistically independent intercepts exist, and (f) above reveals no irregularities, calculate adjusted yields at

$\bar{X}$, using the model

$$E(Y) = \sum_{i=1}^{q} \alpha_i + \sum_{i=1}^{s} \beta_i f(X) \qquad (4.14)$$

where $\bar{X}$ is the overall initial mean yield of the q responsive treatments.

The above steps are quite closely linked to standard covariance analysis, but they ensure the analyst does not overlook:

(i) curvilinearity;
(ii) disparate regression slopes;
(iii) a possible parsimonious model structure;
(iv) lack of fit;

thus extracting the maximum amount of information from any experiment, and optimising its precision.

4.2.2 Fertilizer x thinning trials

This class of experiment is encountered very frequently, [for example Wagle and Beasley (1968), Weetman (1975), Weetman *et al.* (1980), Groot *et al.* (1984), Binkley and Reid (1984), Mead *et al.* (1984), Barclay and Brix (1985), Hunter *et al.* (1985), Lee and Barclay (1985), and Woollons (1980, 1985)]. Comparable variants include fertilizer x species experiments (H.D.Waring, analysed in Chapter 6), or further interactions such as pruning, [Hunter *et al.* (1986)], examined in Chapter 3. Such trials have proved quite difficult to analyse efficiently, and have been the subject of special study, [see, for example, Woollons (1985), presented in full in Appendix I].

The general sequence of tests given in (4.2.1) still holds but modifications are required for calculating treatment yields, and testing for responses among intercepts. The crucial difference present in this case is that the overall initial mean ($\bar{X}$)is no longer an appropriate datum about which to judge treatment effects. By experimenting in distinct thinning regimes (commonly unthinned and thinned to an appreciably low stocking), the trial data often reflect these states by displaying trends similar to Figure 4.1(a) but show pairs (in the case of two treatments) of responses in each thinning class. Fig. 4.3(a) shows hypothetical data for a thinned and unthinned regime, to which fertilizer is applied or not (replications shown are arbitrary in this context).

For a fertilizer x thinning trial involving t treatments applied to each of r thinning regimes, a general regression model is

$$E(Y) = \sum_{i=1}^{t} \sum_{j=1}^{r} (\alpha_{ij} + \beta_{ij}f[X]) \tag{4.15}$$

Recommended analytical procedures are:

(a) As before, the appropriate functional form of f(X) needs to be determined. The isolation of the significantly independent β_{ij} requires some care. Initial tests are best directed at the hypothesis

$$H_0 : \beta_{1(j)} = \beta_{2(j)} = \ldots, \beta_{t(j)} \tag{4.16}$$

thus determining if disparate growth is present <u>within</u> any thinning class. Such an outcome is not common, but certainly not impossible.

(b) Assuming that the null hypothesis (4.16) is accepted for all *r* thinning regimes, the model (4.15) is reduced to

$$E(Y) = \sum_{i=1}^{t} \sum_{j=1}^{r} (\alpha_{ij}) + \sum_{j=1}^{r} \beta_j f(X) \tag{4.17}$$

The test of the hypothesis

$$H_0 : \beta_1 = \beta_2 = \ldots, \beta_r \tag{4.18}$$

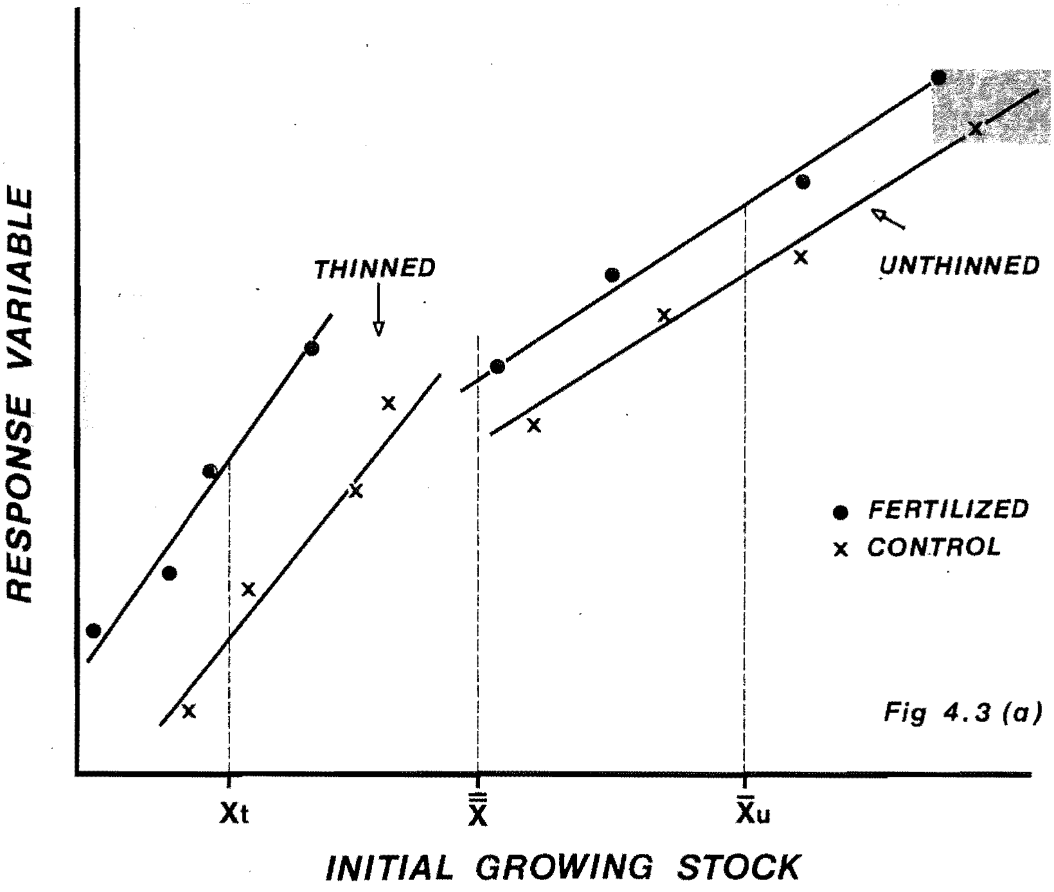
is simple statistically, but frequently requires some judgement on the part of the analyst. A problem occurs when thinning regimes consist of strictly imposed, fixed residual basal areas and stockings. The dispersion of thinned data tends to be narrow relative to unthinned treatments, and by the structure of (4.5), the precision of thinned regression coefficients can become considerably less than unthinned, so (4.18) is often accepted, even if the actual estimates are considerably different. An example is the data of Mead *et al.* (1984), illustrated in Figure 4.3(b) where the thinned plots can be seen to be very tightly grouped.

Since there are *a priori* biological reasons for expecting different growth rates with contrasting stand densities, especially with large differences in stocking and over time, the following strategy is suggested.

(i) Test (4.18), and isolate any significantly different β_i.

(ii) If (4.18) is accepted, re-examine the hypothesis using 1-tailed tests, and reject H_0 at relatively low levels of significance, say 0.2 and smaller.

(iii) If (4.18) is still accepted, or if a hypothesis $H_0 : \beta_i = 0$ is accepted, then accept a common regression coefficient for all regimes.



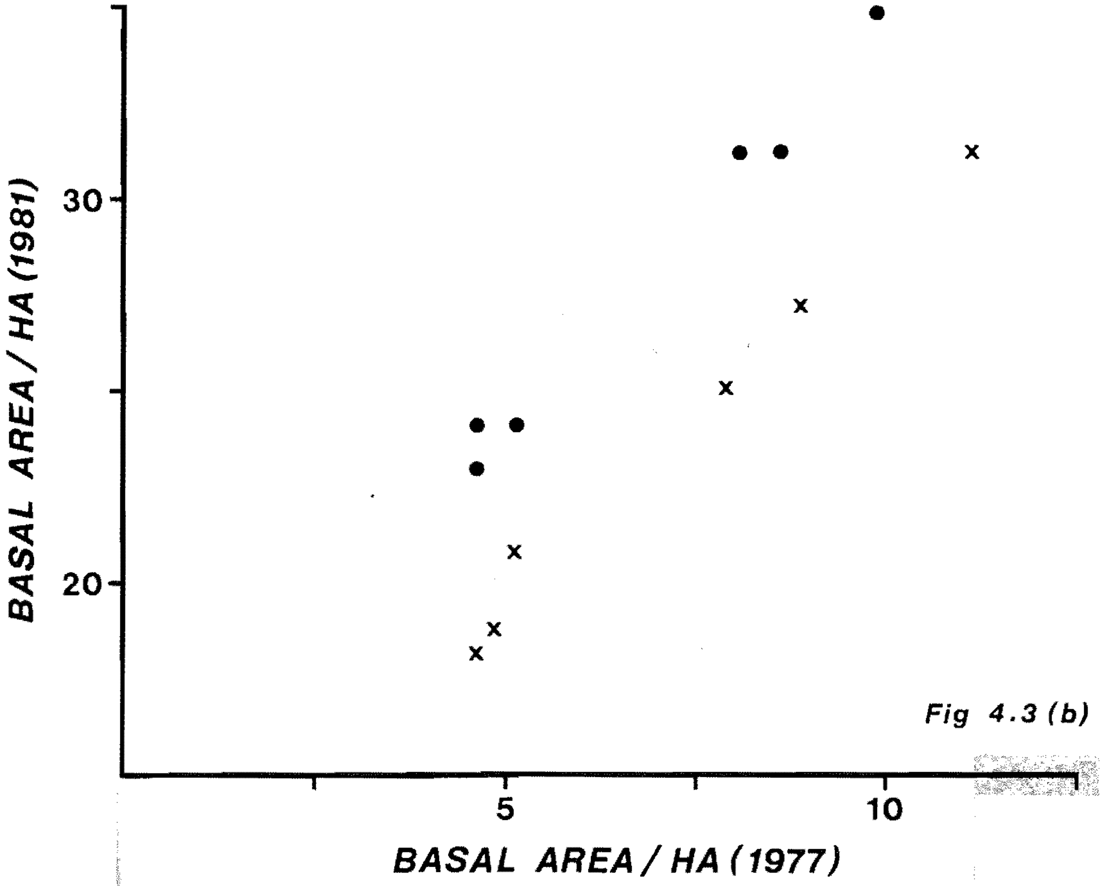


Figure 4.3(a) and 4.3(b) Illustration of thinning x fertilizer experiments, depicting conceptual responses and data of Mead *et al.* (1984).

These procedures have been illustrated in the experiments of Mead *et al.* (1984) and Hunter *et al.* (1986), examined in Chapters 2 and 3 respectively. For the Mead experiment, the test of hypothesis (4.18) is given by

$$(1.8180 - 2.0553)/\surd(0.4514[2.577 + 0.1348]) = -0.214$$

$$\text{for t with 7 degrees of freedom} \qquad (4.19)$$

So, judged even by a one-tailed test, (4.19) fails to be significant at the 20% level, and the thinned regression coefficient is barely significantly different from zero. Accordingly, hypothesis (4.18) is accepted, and a common regression slope is used for both regimes.

The Hunter data, however, give a test

$$(1.4146 - 1.8812)/\surd(0.4274\ [0.282 + 0.032]) = -1.28$$

$$\text{for t with 16 degrees of freedom} \qquad (4.20)$$

which evaluated as a one-tailed test, is significant at the 11% level, and the thinned regression coefficient is significantly different from zero at the 0.01% level, so hypothesis (4.18) can be rejected.

(c) Examine differences in predicted treatment values <u>within</u> regimes by the hypothesis

$$H_o\colon E(Y_{i(j)}/\bar{X}_j) = E(Y_{k(j)}/\bar{X}_j) \qquad (4.21)$$

where i, k represent fertilizer levels within the j th thinning regime, and the conditional $\bar{X}_j$ signifies the test should be evaluated at the initial mean of the j th thinning class, and not the grand mean $\bar{\bar{X}}$, (see Figure 4.3(a)). If (4.21) is accepted for any i and k, the treatments in question should be pooled.

Testing of predicted values among regimes can be hazardous. It should not be attempted with regimes having regression coefficients that were previously declared different, since such action in effect nullifies the claim of independent slopes. For two regimes with identical slopes, a comparison between predicted values will inevitably result in a coarse test, since the "distance" between the regimes, will inflate the denominator in (2.21).

Thus, some judgement is required in deciding whether to pool alike treatments among regimes. By pooling, additional replication is obtained, but predicted responses will be identical for all regimes. Thus, a decision to pool or not is probably best decided by the objectives of the trial. If the emphasis is on establishment of treatment effects, such practice is desirable. However, if the best estimate of treatment response is required, alike intercepts among regimes should not be combined.

4.2.3 Summary of procedures for analysis of fertilizer x thinning trials

A procedure for effective analysis of this class of experiment is unavoidably extensive and requires some judgement by the analyst. Recommended procedures can be summarised as follows:

(a) Establish the appropriate form of f(X) in (4.15) to establish linear relationships between Y and X;

(b) Isolate significantly different slopes within regimes;

(c) Test for independent slopes among regimes, and be prepared to consider 1-tailed tests and low levels of significance when so testing;

(d) Test for significance differences among predicted treatment values within thinning regimes, evaluating differences at the initial mean of each regime;

(e) Consider pooling treatments among regimes if the objectives of the experiment are to isolate treatment effects, rather than establish most accurate estimates of response.

4.2.4 Fertilizer trials arranged in factorial combinations

Many mid-rotation fertilizer experiments utilise factorial arrangements of treatments, [for example, Woollons and Will (1975), Whyte and Mead (1976), Gerig *et al.* (1978), Miller and Webster (1979), Salonius *et al.* (1982)]. The properties and advantages of factorial systems are well documented, [for example Yates (1937), Ostle (1963), Mendenhall (1968)]; whereas non-factorial layouts are profitably analysed by regression linear-models, factorial layouts are generally well evaluated by factorial models, embracing the concepts of main-effects and interactions.

Nevertheless, many of the procedures suggested earlier can still be invoked.

(a) If a factorial experiment is represented by

$$E(Y) = \sum_{i=1}^{m} \alpha_i + \sum_{j=1}^{m} \beta_i X \tag{4.22}$$

where

$m = p^n$, denoting the n factors involved in the design, and p the number of associated levels, then, as before, an appropriate relation between Y and X should be derived first, and the independent β_j ascertained through testing of the hypothesis, $H_o: \beta_i = \beta_2 = \ldots, \beta_m$.

(b) When $p \geqslant 2$, all main effects and interactions can be broken down into orthogonal components, and (4.22) can be expressed as a response surface [Ostle (1963), Mendenhall (1968)]. The subdivision of factorial forest fertilizer experiments into a response surface model has been reported or suggested often in forestry research, [for example, Clutter (1968), Donald and Glen (1974), Farnum (1979), Woollons and Snowdon (1981), Snowdon and Waring (1985)]. However, its usefulness is debatable, [Yang (1983)], and it is perhaps significant that Farnum (1979) is able to cite only one actual example where the approach was successful, for grapevines in this case. Experience with several species suggests that responses behave in a complex manner, and can fail to give a discerning model [Snowdon and Waring (1985)]. The experiments of Salonius and Gerig add credibility to this observation. Nevertheless, the splitting of factors into components is quite valid, and can establish the significance of a factor when the overall effect is deemed non-responsive [Yates (1937)].

(c) As with non-factorial systems, the analyst can sometimes enhance the precision of a trial, by pooling non-responsive interactions into the experimental error, with a consequent gain in residual degrees of freedom. Binns (1976) reported

> it worth remarking that in all our experience of forest nutrition trials in Britain we have not yet found a second or higher order interaction of any practical significance in forest nutrient research.

For example, in a 2^4 factorial, in 2 randomised blocks, the standard ANOVA would be:

Source	d.f.
A, B, C, D	1 (each)
AB, AC, AD, BC, BD, CD	1 (each)
ABC, ABD, ACD, BCD	1 (each)
Blocks	1
Covariate	1
Error and third order interaction	19
	31

If preliminary analysis showed responsive nutrients were strictly limited to say, A, B, with a small AB, the remaining interactions may well be pooled with the error term, thereby adding nine more degrees of freedom with which to estimate the residual mean square.

However, it would be wrong to imply this strategy always achieves a marked increase in precision. A fairly common result, especially with large factorial systems, is for one or two interactions to show weak significance, when there is no logical reason to regard the effect(s) as real. The analysis of variance example in Chapter 2, for example, shows a PMg effect significant at the 5% level, yet it is clear the trial responses are dominated by very large main effects of N and P alone. But if such spurious interactions are pooled with the error term, the sum of squares may be inflated relatively more than the increase in degrees of freedom, so that the resultant mean square is no smaller than before. F-tests and confidence intervals, however, tend to be a little more sensitive, on account of more degrees of freedom. On balance, therefore, pooling of non-responsive interactions is recommended.

For example, NZFP established a 2^4 factorial experiment in five-year-old *Pinus radiata* regeneration, (see Trial No.6, Chapter 7 for more detail) investigating the effects of nitrogen, phosphorus, magnesium, and potash, replicated twice, in 4 randomised blocks with NPMgK totally confounded. The ANOVA below gives:

(i) the full model;

(ii) model, with non-responsive interactions pooled.

ANOVA

Source	d.f.	SS(full) x10^3	$p > F$	SS(pooled) x10^3	$p > F$
N	1	58.94	0.0051	60.80	0.0025
P	1	1.94		2.64	
Mg	1	8.18		5.29	
K	1	7.53		6.06	
Other effects	10	56.31			
Covariate	1	599.23	0.0001	805.39	0.0001
Blocks (reps)	3	181.47	0.0006	174.05	0.0001
Error (full)	13	67.74			
Error (pooled)	(23)			119.99	
	31				

Analysed as the full model, the main effect of N is highly significant (0.1%) but no other effect or interaction is significant at the 5% level.

The two estimates of σ^2, the residual mean square,

5.2108 from the full model and 5.2169 from the pooled one,

give essentially equivalent values. One interaction (MgK) is significant at the 9.1% level, however, in the pooled analysis; although there is no reason to interpret the result as real, it accounts for 41% of the pooled residual sum of squares and inflates the error estimate. One recourse may be to retain such spurious results in the treatment model effects, but this could be seen as a contrived error estimate.

On balance, however, pooling of non-effective interactions is recommended. Even if the error mean square is essentially similar to the full model, some gain is obtained by increased degrees of freedom, giving somewhat more sensitive F-tests, and narrower confidence intervals.

4.2.5 Factorial Experiments: summary

Factorial fertilizer experiments are generally well analysed by standard (multiple) covariance techniques, and, using factorial models, as demonstrated with the data of Salonius and Gerig, in Chapter 3. The analyst should check, however, if:

(i) disparate slopes are present;
(ii) splitting into orthogonal components gives enhanced significance;
(iii) pooling non-effective interactions gives better precision.

4.3 PROCEDURE FOR ANALYSING FERTILIZER EXPERIMENTS: DISCUSSION and SUMMARY

The previous sections contain a suggested methodology for examining data from later-age forest fertilizer experiments which is not readily available in forestry texts and other relevant sources on experimental analysis. It is designed specifically to cater for the several problems that arise when analysing such trials. The procedures are considered to be applicable generally to the large majority of nutrition field trial experiments; conversely, given the broad nature of the topic, it is inevitable that a few analytical steps are too tersely described, or some recommendations reflect a school of preference rather than universal acceptance. Few statistical procedures are without some detraction, and the above methodology will be no exception. This Chapter ends with a presentation of some wider consequences of adopting the methodology.

A major thrust of the procedures is to secure a minimum (available) residual error. Some biometricians may not agree with this emphasis, preferring to obtain the most appropriate model strictly within the imposed treatments and experimental structure. The approach adopted here is justified for the following reasons.

1. Even when statistical significance between treatments is demonstrated, confidence intervals in forest experiments can be disconcertingly high. Whyte and Mead (1976) derived limits that differed from the mean by ± 39%, and these were estimated from models that recognised pre-treatment growth and had the advantage of measurements obtained by intensive stem-analysis. Standard covariance analysis gave limits of ± 98%. The trial of Hunter *et al.* (1986) reanalysed in (3.3.1) has confidence limits of

± 56%, despite individual treatments being highly significant. It is strongly contended that an analyst has a duty to adopt techniques which help to narrow these intervals. A forest manager cannot be expected to pay much heed to results which perhaps inform him that although fertilizer X is highly significant, its effect is likely to range around an interval approaching its estimated point value.

2. The significance of regression coefficients and (multiple) covariates are likely to rise by virtue of a smaller residual error, thereby supplying more information from a trial. It makes little sense to spend several years, perhaps decades, collecting data from an experiment, then overlooking or ignoring several results for want of applying a sensitive analysis.

The analyst usually cannot derive the benefits of multiple hypotheses testing without incurring some likelihood of erroneous decision-making. The proposed series of analytical steps are sequential in structure, in that each test is conditional on the previous step, and hence the probability of Type I or II errors [Ostle (1963), p.108] become progessively higher, analogous to stepwise regression [Draper and Smith (1981), p.311]. Such events cannot be ignored, but they are considered (1) well worth the risk of taking, particularly since the ramifications of an erroneous decision are not high, and (2) occurrences are in fact not likely because:

(a) The number of treatments (t) actually employed in forest fertilizer experiments is not high, usually 2-5, thus the actual number of sequential tests is not inordinate;

(b) In a large majority of cases, the number of disparate slopes or distinct intercepts claimed in an experiment is low.

> Commonly, a single regression slope suffices for all treatments, or the treatments obviously subdivide into two or perhaps three groupings, or occasionally only one; that is, no responses are evident.

In this thesis, eighteen forest fertilizer trials are examined in some detail. Four of these exhibit significant disparity with respect to regression coefficients, two of which are due to the presence of differing thinning regimes. Moreover, no trial has more than three unique intercepts within a specific class, even in cases where up to sixteen are conceptually possible. A rationale for determining appropriate rejection or acceptance levels in hypothesis testing is difficult to formulate, but is not seen to be required [recommendations for utilising multiple regression coefficients in thinning x fertilizer trials developed in section (4.2.2) represent one exception]. This stance is justified by observing how few sequences of hypotheses in fact incur levels of significance around the 5% level, the majority clearly showing real effects or not, and frequently any marginal decisions can be augmented by *a priori* evidence. For example, Table (4.1) lists the hypotheses assayed when examining the data of Hunter *et al.* (1986), presented in section (3.3.1). The table summarises the hypotheses and their respective probabilities of erroneous rejection.

Table (4.1) Hypotheses associated with data of Hunter *et al.* (1986)

Hypothesis	$p > F$
Treatments in full model are equivalent	0.0003
Slopes are equivalent to zero	0.0001
Effects of blocks are equivalent	0.1859
Regression coefficient for unthinned regime is equal to or less than thinned	0.1100
Pruned and fertilized, and fertilized treatments in unthinned, are equivalent	< 0.50
Pruned and fertilized, and control treatments in thinned, are equivalent	< 0.50
Treatments in reduced model are equivalent	0.0001
Thinned and fertilized, equivalent to thinned	< 0.001
Thinned and pruned, equivalent to thinned	< 0.001
Fertilized and unthinned, are equivalent	< 0.01
Pruned and unthinned, are equivalent	< 0.0261

Thus, of eleven hypotheses undertaken, only the test for disparity of regression coefficients among regimes is questionable, but, as discussed in (4.2.2), there are grounds for expecting a weaker test in this instance. However, as there are silvicultural reasons why one could expect a difference in growth rate.

The recommended procedures are listed in a sequence that an analyst could follow to obtain a final model and estimates of response (if present). Clearly, there are instances when specific steps might be waived, or preferably tested at a later stage. For example, some people may prefer to commence with model (4.12) rather than (4.11); there are conceivable data which exhibit highly disparate slopes, in which case a model $E(Y) = \alpha + \beta X$ will not be appropriate. This model is recommended to aid detection of linearity between Y and X, and/or to isolate the rare case where the quantum of initial growing stock covariate is ineffectual [see Appendix 6.1.1) for an example]; such a result renders most of the subsequent steps redundant.

While a few aspects of the suggested methodology are open to more than one opinion, adoption of the procedures will go a long way to rectifying many analyses presented in the literature, or provide a sensitive analytical framework for examining forest fertilizer experiments. In this Chapter, the basic system has been described; the next task is to draw attention to several related topics which are an integral part of the methodology, but which have not yet been fully developed or justified.

5. RESPONSE VARIABLES AND COVARIATES

5.1 RAMIFICATIONS OF SUGGESTED METHODOLOGY

5.1.1 Introduction

In the previous chapter, methodology was presented to analyse types of mid-rotation fertilizer experiments commonly conducted by forest scientists. The efficiency and success of the suggested procedures are related to the specific adopted response variables and covariates. The purpose of this Chapter is to review the nature and choice of these variables, and to discuss their influence on the proposed systems of analysis.

5.1.2 Fertilizer trial measurements

The generally used method of measuring stem growth in fertilizer trials of pole-crop and older is through tape or caliper measurements at diameter breast height. Plot response, however, is subsequently derived and expressed as basal area/unit area or /tree. Sometimes tree heights are also determined, usually obtained by clinometer or extended poles, and plot volume then estimated, normally by applying a standard 2-dimensional volume equation [Weetman (1975), Miller and Tarrant (1983)].

The subject of measurement in forest trials is a specialised topic which is largely outside the theme of this study. Occasionally, researchers have adopted more intensive mensuration procedures. Allusion has been made to Miller and Cooper (1973) who assessed a nitrogen rates experiment in Corsican pine. Five trees in each plot were felled three years after fertilizer application, and subjected to stem analysis. Radiographs of discs cut from each one-tenth height up the stem were used to obtain measurements of bark thickness and the width of each of the outermost six rings along four radii. Measurements were made down to 0.01 mm using a travelling microscope, rings as narrow as 0.05 mm being encountered on smaller trees. The subsequent data gave estimates of tree and plot volume at various ages, and allowed analysis of changes in stem form. Woollons and Will (1975) reported a series of *Pinus radiata* mid-rotation fertilizer experiments, where Barr and Stroud dendrometers were utilised to measure upper stem diameters, providing estimates of over-bark tree volume, and subsequent plot growth.

These methods, although costly and sometimes laborious, in fact represent a distinct improvement over basal area and height as response variables as demonstrated by Whyte and Mead (1976), particularly since many fertilizer responses are partially reflected in changes in bole shape [Snowdon *et al.* (1981)].

Nevertheless, basal area remains the major response variable utilised for the large majority of later age fertilizer experiments.

5.1.3 Response variable units

In this thesis, emphasis is placed on unit area response variables, because:

1. a substantial majority of forest fertilizer trials use plots of fixed area as experimental units, and randomisation of treatments is assayed accordingly, necessitating that the response variable be expressed as some function of unit area;

2. management is concerned primarily with unit area production and growth;

3. the large majority of current growth models are stand-level simulators, [Munro (1974), Clutter *et al.* (1983)] which require unit area statistics as input;

4. forest estate models [such as RMS85, Allison (1985)], and IFS, [Garcia (1981)], utilise unit area yield tables for calculations and prediction.

5.1.4 Response variable: yield or growth

Researchers are occasionally unsure whether a response variable should be expressed in terms of yield (Y) at a given time, or as growth (Y-X), where X is the initial value of yield at the commencement of the experiment, and frequently used as a covariate in the analysis.

Thus, given a model

$$E(Y) = \alpha + \beta X \tag{5.1}$$

then if growth is estimated by

$$(Y-X) = \alpha_T + \beta_T X \tag{5.2}$$

and substituting (5.2) for Y in (5.1) and denoting α and β as α_T and β_T, then it can easily be proved (see Appendix 3), that

$$\hat{\beta}_T = \hat{\beta} - 1 \quad \text{and} \quad \hat{\alpha}_T = \hat{\alpha} \tag{5.3}$$

That is, $\hat{\beta}_T$ is equivalent to $\hat{\beta}$ less unity, while the intercept, $\hat{\alpha}_T$, is equivalent to α, and is unchanged by the transformation.

Secondly, the residual sum of squares associated with (5.1), RSS, is estimated by

$$RSS = y'y - \underset{\sim}{\beta}X'y \qquad 5.4)$$

or

$$\Sigma Y^2 - \hat{\alpha}\Sigma Y - \hat{\beta}\Sigma XY \qquad (5.5)$$

Given the transformation (Y-X) for Y then it can also be proved (see Appendix 3) that the corresponding residual sum of squares associated with (5.3) is also equivalent to (5.5), showing that the residual sum of squares is invariant to the above transformation.

These proofs demonstrate that for virtually all purposes, the two response variables (yield and growth) lead to exactly the same results. In both cases, treatment differences and estimates of experimental error are identical. Furthermore, since the $(X'X)^{-1}$ matrix is unchanged by either variable, standard errors associated with each corresponding regression coefficient and intercept(s) are also identical. Essentially therefore, the analyst may choose either yield or growth as a response variable, although the former has the minor advantage of allowing an explicit calculation of percentage yield gain, which for management purposes can be an important statistic.

There is, however, a need to interpret carefully tests of hypotheses concerning the regression coefficient, in relation to the response variable utilised. With yield (Y), the slope (β) measures the change in yield for unit change in X, initial yield. Over short time spans, say 1-3 years, Y and X are intimately related by definition through $Y = X + \Delta X$, but with $\Delta X << X$, and hence β is almost always estimated to be greater than unity. Consequently, a standard test of H_0: $\beta = 0$ is usually rejected, but the significance obtained is partly a ramification of definition, rather than any biological importance. Use of growth instead will reduce the slope by unity, and the hypothesis H_0: $\beta = 0$ now examines whether growth is independent of initial growing stock. In this context, growth might be considered a more meaningful response variable, although the same test is available by the use of yield, but testing H_0: $\beta = 1$. In practice, therefore, the distinction is mathematically unimportant, since either variable gives identical tests of significance of treatment effects, and equivalent differences in adjusted means with the appropriate analysis.

Some care also needs to be exercised with the interpretation of correlation statistics. A common practice with linear models is to display or include an estimate of the square of the multiple correlation coefficient (R^2). The practice is disputable, but seemingly enduring [see Warren (1971)], perhaps perpetuated by the automatic calculation of the statistic in many computer package outputs. If yield is used as a response variable, over short time spans the R^2-value will be high, caused partly by the imposed relation between Y and X. R^2 is closely related to the absolute value of the regression coefficient [Draper and Smith (1981), p.45] so use of yield will increase its value, relative to that obtained by growth. In this thesis R^2-values are very rarely quoted; if used, they are employed solely to demonstrate relative increases in explained variation over equivalent time periods through adopting alternative models. Situations could arise when the analyst wishes to quantify the goodness-of-fit of a specific model, and here R^2 may be an adjunct; in these cases the response variable is preferably expressed as growth.

The above discussion highlights the outcome of a fairly common practice of using growth as a response variable, but without the inclusion of a covariate, representing initial yield [see for example, Lee and Barclay (1985)]. Such procedures are equivalent to using yield as the response variable in conjunction with a covariate, but where the regression coefficient is *a priori* assumed to be unity; alternatively, it can be regarded as assuming a model where growth is independent of initial growing stock. Such assumptions are frequently coarse and restrictive, and, above all, completely avoidable by the adoption of the methods suggested in Chapter 4.

5.1.5 Mortality

In later-age fertilizer experiments, death or damage of some experimental trees is inevitable. Mortality may be caused among other things, by competition, physical environmental damage, thinning damage, pathogenic effects, or by the action of the applied fertilizer. Analysis of data must therefore address the practical problem of how to treat dead or damaged trees. A first requirement is to ascertain whether the mortality is a treatment (nutrient applied) effect or caused by extraneous factors. This can be easily accomplished by a familiar χ^2 - contingency table analysis [Siegel (1956), p.104], although a correction for continuity may be used to sharpen the approximation, [see for example, Ostle (1963), p.131], when only two treatments are involved.

If not significant (which is the usual outcome) it is useful to remove all damaged/dead trees from the dataset, and calculate initial and subsequent yields in terms of surviving trees only. It might be argued that this practice represents a manipulation of the data, but conversely it is illogical to carry out an analysis with a removable component of variation inherent in the data, particularly as the objective of many experiments is to screen or isolate responsive nutrients, rather than estimate yields. In any event, such a practice will not seriously bias the latter, since responses are usually reported as differences in yield or growth. In the rare case where responses are affected by nutrients applied, yields should be calculated in gross terms.

5.1.6 Transformation of response variable and covariate

A response function often usefully employed in analysis of fertilizer trials is basal area per tree, calculated for each experimental plot as

$$\pi/4 \times \sum_{i=1}^{n} d_i^2/n \tag{5.7}$$

where d_i = measured diameter at breast height

n = number of stems in any plot.

Frequently, the actual number of stems in each plot, after removal of dead trees, will vary a little. Alternatively, if the trial is first thinned to a nominated basal area, or if laid out to include unthinned conditions, the number of trees will likely vary through the experimental units. In these cases, response variable (5.7) will tend to disperse around a mean initial value for each treatment. This dispersion is useful, since the precision of the regression coefficient(s) will be increased by the larger range of initial plot values, than would be the case if, say, plot basal area, or basal area per hectare was utilised as a response variable and covariate instead [Woollons (1985)].

For example, Mead (1974) and Whyte and Mead (1976) reported results from a mature *Pinus radiata* 4 x 2 NP factorial experiment at Braeburn, Nelson. The ANOVA below gives equivalent analysis of the data, using basal area/ha and basal area/tree (both 1973) as the response variable, and corresponding variables (1968) as the covariate. Six replications were available.

ANOVA

Source	d.f.	SS (/ha)	p > F	SS (/tree)x10^3	p > F
N	3	19.02	0.0012	0.21339	0.0002
N_1	1	18.41	0.0001	0.20435	0.0001
N_q	1	0.08		0.00462	
P	1	1.43	0.2331	0.02487	0.0897
NP	3	4.42		0.2538	
Covariate	1	3638.66	0.0001	70.76	0.0001
Error	39	38.26		0.32032	
	47				

Estimated (adjusted) yields are: (m^2/ha)

(a) Through modelling basal area/ha

	n_0	n_1	n_2	n_3	
p_0	61.0	61.4	61.9	62.4	
P_1	61.6	61.1	62.6	63.0	$s_{\bar{y}} = \pm 0.404$

(b) Through modelling basal area/tree

	n_0	n_1	n_2	n_3	
p_0	61.0	61.4	61.9	62.3	
P_1	61.5	61.1	62.4	63.1	$s_{\bar{y}} = \pm 0.366$

The two analyses have logically given very similar estimated yields, but there is some evidence of a more sensitive analysis, achieved by utilising basal area/tree; the effect of phosphorus is now weakly significant, as was anticipated [Dr.D.Mead, *pers.comm.*]. Inspection of the data suggests that the relationship between basal area/ha 1973 and 1968 is slightly curvilinear, whereas this has been removed by the use of basal area/tree which has a strictly linear relationship. In addition, the dispersion of these data has been enhanced; plot stocking varied between 185 and 580 stems/ha, and hence the range of mean tree size is widened.

5.1.7 Response variable transformation; published literature

The considerations and results above conflict with the conclusions of Cellier and Correll (1984) who stated

> The question of whether to express results as an average per tree or on a per unit area basis is a decision for the experimenter rather than the biometrician. If there is strong competition between trees and the remaining trees have made full use of the available space, it is appropriate to express the data on a per area basis. If the trees are not competing, it would be better to consider the data on a per tree basis.

These comments are, it is claimed here, not well founded, and indicate that the Australian authors are unaware of the complications inherent in mid-rotation forest experimental analysis. Their conclusions would also seem to contradict claims by the senior author, Cellier (1979), that spatial distance between trees could be dismissed as an experimental variable. If that is so, it is difficult to see how their guidelines, stated above, can be acted on by the analyst. If their comments pertain to stocking, their recommendations are still not substantiated. Basal area/tree is shown here to be a most useful response variable in experiments where plot density varies, a result reinforced by the analysis of Woollons (1985), where basal area/tree was likewise found to ameliorate the precision of a *Pinus radiata* thinning x fertilizer trial.

5.1.8 Covariates

For a substantial majority of fertilizer experiments, primary measurement is concurrent with treatment applications, so the major covariate available is usually some function of initial growing stock. Whyte and Mead (1976), however, demonstrated that, for their data, a superior covariate was growth for the five years before fertilizer application, and certainly if the information is available, pre-treatment growth should be tested as an alternative covariate. The experiment of Salonius *et al.* (1982), reanalysed in Chapter 3, gained appreciable precision by utilising pre-treatment basal area/ha increment as a covariate, though unfortunately data are not available to make a comparison of the effect of using pre-treatment yield instead. Related approaches are also given by Salonius and Mahendrappa (1983) and Ballard and Majid (1985).

It would be incorrect to deduce, however, that pre-treatment growth always provides a superior covariate. For the thinning x fertilizer experiment analysed by Woollons (1985), data were available allowing growth to be used as the concomitant variable, but in this instance no additional gain in precision was achieved relative to the use of initial growing stock/tree.

5.1.9 Use of covariates with various response variables

The choice of concomitant variable with different traits (for example, basal area, volume, form-factor), is best resolved by adopting the corresponding predictor; for example, if basal area/tree is the chosen response variable, initial basal area/tree will nearly always be found to be a discerning covariate. Accordingly, this writer disagrees again with the conclusions of Cellier (1979) and Correll (1984) who stated

> a related covariate may be almost as effective and much easier to measure, for example, if bole volume is the measure of interest, bole cross-sectional area is just as efficient a covariate as bole volume and more readily measured.

While any expression of initial size will probably give some increase in precision, [for example, eye-ball estimates are probably preferable to no use at all of covariates, Yates (1971), p.163], the authors' inference that "more readily measured variables are just as efficient" is contested. Woollons and Will (1975) gave details of an NZFP thinning x fertilizer experiment (described in detail in Chapter 7 and Appendix 5) where a composite fertilizer was applied or not, to thinned *Pinus radiata* in residual stockings of 620 and 370 stems/ha, in 1967. Four replications of each treatment were available, laid out in a completely randomised design. Tree volume estimates were available for all experimental trees, obtained by Barr and Stroud dendrometers [Pearce and Woollons (1973), Woollons and Will (1975)].

To demonstrate the relative efficiency of the available covariates, the analysis below gives results from the experiment, using volume/tree (1971) as a response variable, and

(a) volume/tree, 1967

(b) basal area/tree, 1967

as covariates. Invoking the procedures suggested in Chapter 4, both pairs of variables have a linear relationship, and the dataset can be best represented by the model

$$E(Y) = \sum_{i=1}^{2} \alpha_i + \beta X \tag{5.8}$$

That is, a common regression slope is appropriate for all treatments, and two intercepts are justified, one each for fertilized and control, for both residual stockings. The respective ANOVA's are:

Source		d.f.	SS	p > F
(a)	Initial volume/tree	1	0.20227	0.0001
	Intercepts	1	0.01938	0.0001
	Error	13	0.00721	
		15		
(b)	Initial basal area/tree	1	0.17882	0.0001
	Intercepts	1	0.03611	0.0018
	Error	13	0.03065	
		15		

Thus while initial basal area/tree is clearly a highly significant (0.1%) covariate, the significance of the intercepts drops from the 0.1 to the 0.18% level, and the residual error is appreciably coarser (76%),which would also be reflected in the width of a confidence interval constructed for the estimated response.

5.2 RESPONSE VARIABLES AND COVARIATES: SUMMARY

The units, functional form, and expression of response variables and covariates are most important considerations in harnessing the capabilities of the analytical procedures described in Chapter 4 to the fullest extent. In particular:

1. while basal area per hectare is the customary measure of plot yield, appreciable gains in precision and information may be obtainable by adopting intensive measurement of stem volume;
2. unit area response variables are preferred because of randomisation considerations, and because growth simulators, and forest estate models commonly employed today, use stand level inputs;
3. response variables can be expressed in terms of yield or growth, unless R^2-statistics are desired, in which case growth variables should be used;
4. random mortality should be removed from datasets, and plot yields expressed in terms of live trees;
5. consideration should be given to expressing response variables and covariates as yield/tree, rather than yield/unit area;
6. if available, pre-treatment growth should be tested as a covariate;
7. different traits of response should be analysed by their corresponding covariate, rather than relying on basal area in all instances.

6. FURTHER ASPECTS OF ANALYSIS OF FERTILIZER EXPERIMENTS

6.1 ASSUMPTIONS IN ANALYSIS OF VARIANCE

6.1.1 Introduction

In the previous Chapter, the importance of response variables and covariates was discussed, together with the contribution they play in gaining the best out of the procedures proposed in Chapter 4. To complete the review, some secondary topics which also influence the degree of success the methodology can achieve, are examined.

6.1.2 Underlying assumptions

In using analysis of variance procedures, the analyst makes certain assumptions about the observations or data, which, unless satisfied, can make the results inexact. Specifically for linear models, it is assumed that the errors will be:

1. normally distributed;
2. independent, and have a constant variance.

Eisenhart (1947) explored ramifications when these assumptions are violated. In general, departures from normality, provided that they are not severe, present few problems. Tests of hypothesis for significance tend to be a little high, possibly resulting in too many significant results in F- and t-tests [Snedecor and Cochran (1967), p.325].

Independence of errors is a crucial assumption, however, and dependent data may induce major disturbances, with F- and t-tests becoming far too significant. Fortunately with designed experiments, provided that a valid method of randomising treatments within any experimental plan is adhered to [see, for example, Cochran and Cox (1966)], the problem is almost always avoided.

Heterogeneity in forest experiments can occur for a number of reasons. It can be a consequence of treatments; for example, control plots may exhibit disproportionate variation relative to fertilized treatments, when the stand is seriously devoid of nutrients. Thinning may induce a period of thinning shock, relative to unthinned plots. Alternatively, physical damage to plots may induce heterogeneity; wind, hail, lightning, fungal or insect attack can destroy a portion of an experimental unit, and the remaining trees may grow in an impaired manner. While small amounts of heterogeneity do not seriously disturb the validity of analysis of variance, more substantial quantities can lead to F-tests giving too many significant results. Standard methods to combat heterogeneity are well documented, for example Ostle (1963), Snedecor and Cochran (1967), mostly involving transformations of variables using powers or logarithms.

6.2 WEIGHTED LEAST-SQUARES ESTIMATION

An alternative method of dealing with heterogeneity is to use weighted least-squares estimation. In this instance, each datum is assigned a weight or factor. Loosely, the more homogeneous data are afforded more importance or "weight" in the calculation of the least-squares estimates and error variance. In proposing models such as (4.12), (4.15) and (4.22), estimation of the parameters involved is achieved by solution of

$$\hat{\underset{\sim}{\beta}} = (X'X)^{-1}X'\underset{\sim}{y} \tag{6.1}$$

With weighted least-squares (6.1) is augmented by

$$\hat{\underset{\sim}{\beta}} = (X'V^{-1}X)^{-1}X'V^{-1}\underset{\sim}{y} \tag{6.2}$$

where V in (6.2) is a diagonal matrix, with the weights associated with each datum, occupying the leading diagonal. The practice is mainly feasible when there is a reliable method of assessing the weights, as in the case where a known area or portion of an experimental plot is lost or damaged.

5.2.1 Weighted least-squares estimation: an example

The above methods and the procedures of Chapter 4 are illustrated by analysis of an experiment, discussed by Waring (1968), investigating effects of cultivation, competition, and aluminium foil mulch on growth of *Pinus elliottii* at Jervis Bay, A.C.T., Australia. In 1966 the trial was redesigned to investigate the effects of thinning and fertilizer in a completely randomised 4 x 2 factorial layout.[1] Four residual thinning densities: 2960, 2220, 1480 and 740 stems/ha were interacted with fertilizer (present or not), in the form of N-P-K (John Innes base fertilizer: 5-9-11) applied in 1966 and 1967 with a total of 1 kg/tree, in a slit. The experimental trees were six years old at treatment arranged in square (18 m x 18 m) plots, with an inner measurement plot (15 m x 11 m). The original spacing was 2960 stems/ha, and thinning was essentially done from below. Nine replications of each treatment were originally available, but a problem arose when the experiment was partially flooded in 1966 (see plate 6.1), which had the effect of killing some trees in 11 plots, while seven other plots were completely destroyed. Rather than exclude the partially damaged plots from the analysis, a weight was assigned to each plot, calculated proportionately to the net area not affected by the flooding; thus damaged units had weights ranging from 0.25 to 0.95, undamaged plots a weight of one.

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[1] The writer is grateful to Messrs.H.D.Waring and P.Snowdon for supplying the data

The period of growth investigated was from 1966 to 1968; diameter breast height measurements were available for all experimental trees for both years. Mortality, exclusive of flooded plots, was very light, and unassociated with treatments, so basal area was calculated in terms of live trees, 1986.

Assuming a model

$$E(Y) = \sum_{i=1}^{8} (\alpha_i + \beta_i f[X]) \qquad (6.3)$$

where Y = basal area/tree, 1968

X = basal area/tree, 1966

α_i, β_i = intercepts and slopes associated with the eight treatments

then a graph of Y on X clearly shows the relationship to be linear, but to display signs of disparate slopes (see Fig. 6.1); thus, f(X) in (6.3) is defined as X. The resultant ANOVA, utilising weighted least-squares gives:

ANOVA

Source	d.f.	Type 1 SS	p > F
Single slope, intercept	1	18380.8	0.0001
Individual intercepts	7	6148.5	0.0001
Disparate slopes	7	522.5	0.0042
Error	49	1002.8	
	64		

An equivalent ANOVA, but without weights, gives a residual sum of squares of 1239.7. The covariate, X, is extremely significant, and there is

Plate (6.1) Jervis Bay Experiment, one of the plots at the time of flooding.

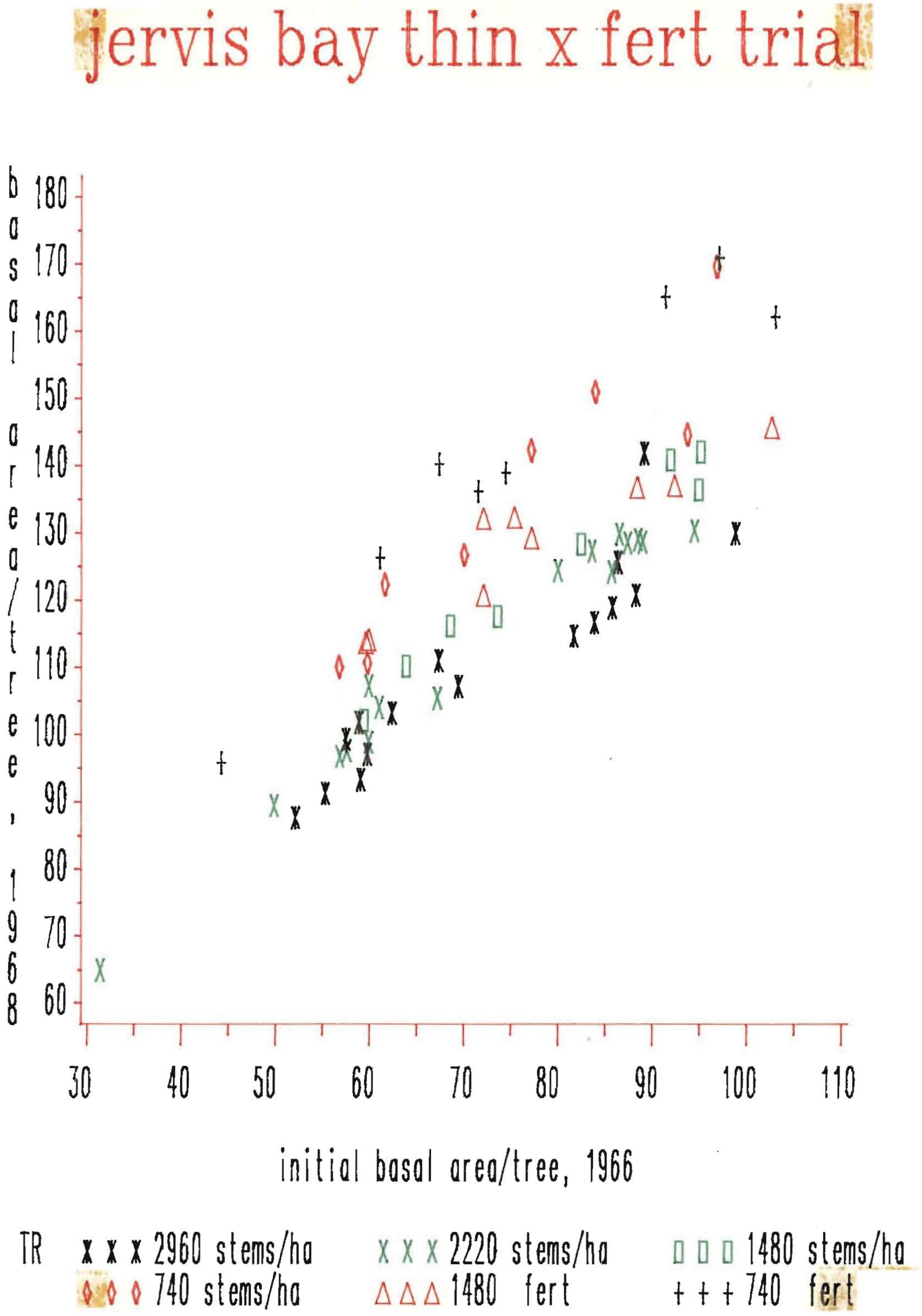

Figure 6.1 Jervis Bay thinning x fertilizer trial. Relationship between the response variable and the covariate.

strong evidence of differing slopes among treatments. Following the methodology developed in Chapter 4, the slopes of fertilized and control treatments for each thinning class are compared, and found to be equivalent, reducing (6.3) to

$$E(Y) = \sum_{i=1}^{8} \alpha_i + \sum_{j=1}^{4} \beta_j X \tag{6.4}$$

where j represents the four stocking classes. The estimated slopes for the 2960, 2220, 1480 stems/ha classes are virtually identical, and can be pooled, reducing (6.4) to

$$E(Y) = \sum_{i=1}^{8} \alpha_i + \beta_{rest} X + \beta_{740} X \tag{6.5}$$

but a test of $H_o: \beta_{740} = \beta_{rest}$ is given by

$$(1.2480 - 0.9365)/\surd(22.49\ [0.000\ 093 + 0.000\ 25]) = 3.57$$

for t with 55 degrees of freedom

which is significant at the 0.1% level, signifying that the 740 stems/ha regime has a distinct slope, which differs from that for the three other regimes. The intercepts in (6.5) are logically tested by commencing from the highest density, and utilising the test statistic (2.21); substituting the appropriate initial mean basal area/tree for each regime reduced (6.5) to a model

$$E(Y) = \begin{array}{l} 2960 \\ 2220 \\ 1480\ (\text{fert}) \\ 1480\ (\text{control}) \\ \ \ 740\ (\text{fert}) \\ \ \ 740\ (\text{control}) \end{array} + \beta_{rest} X + \beta_{740} X \tag{6.6}$$

where the fertilized and control intercepts for 1480 and 740 stems/ha are significantly different, at least at the 5% level. Figure (6.2) shows the residual plottings, and confirms model (6.6) to be satisfactory. The estimated (adjusted) yields are given in Table (6.1).

Table 6.1 (m^2/ha): Results from Waring's thinning x fertilizer experiment

Treatment		1966	1968	ΔG	% ΔG
2960		21.9	33.1	11.2	51
2220		16.2	25.4	9.2	57
1480	control	11.9	18.8	6.9	58
1480	fertilized	11.9	19.6	7.7	65
740	control	5.7	10.3	4.6	81
740	fertilized	5.7	10.8	5.1	89

6.3 DEGREES OF FREEDOM FOR ERROR VARIANCE

When comparisons are required between treatments laid out in plots, the correct error variance is between plots, after experimental structure (blocking) and treatment effects have been removed, where a plot is defined as the smallest unit that receives a specific treatment, and can be randomised. A mistake in forest experimental analysis frequently seen in unpublished papers, is to substitute the between tree variance, and calculate the available degrees of freedom in terms of trees resident in a trial. Auchmoody (1985) appears to have used such a practice, while Warren (1986) deduced Barclay and Brix (1985) may well have used the same erroneous procedure.

Clearly, the practice is invalid. Sets of trees within experimental plots are correlated (albeit to an unknown extent), the randomisation process carried out at trial installation having been addressed to the

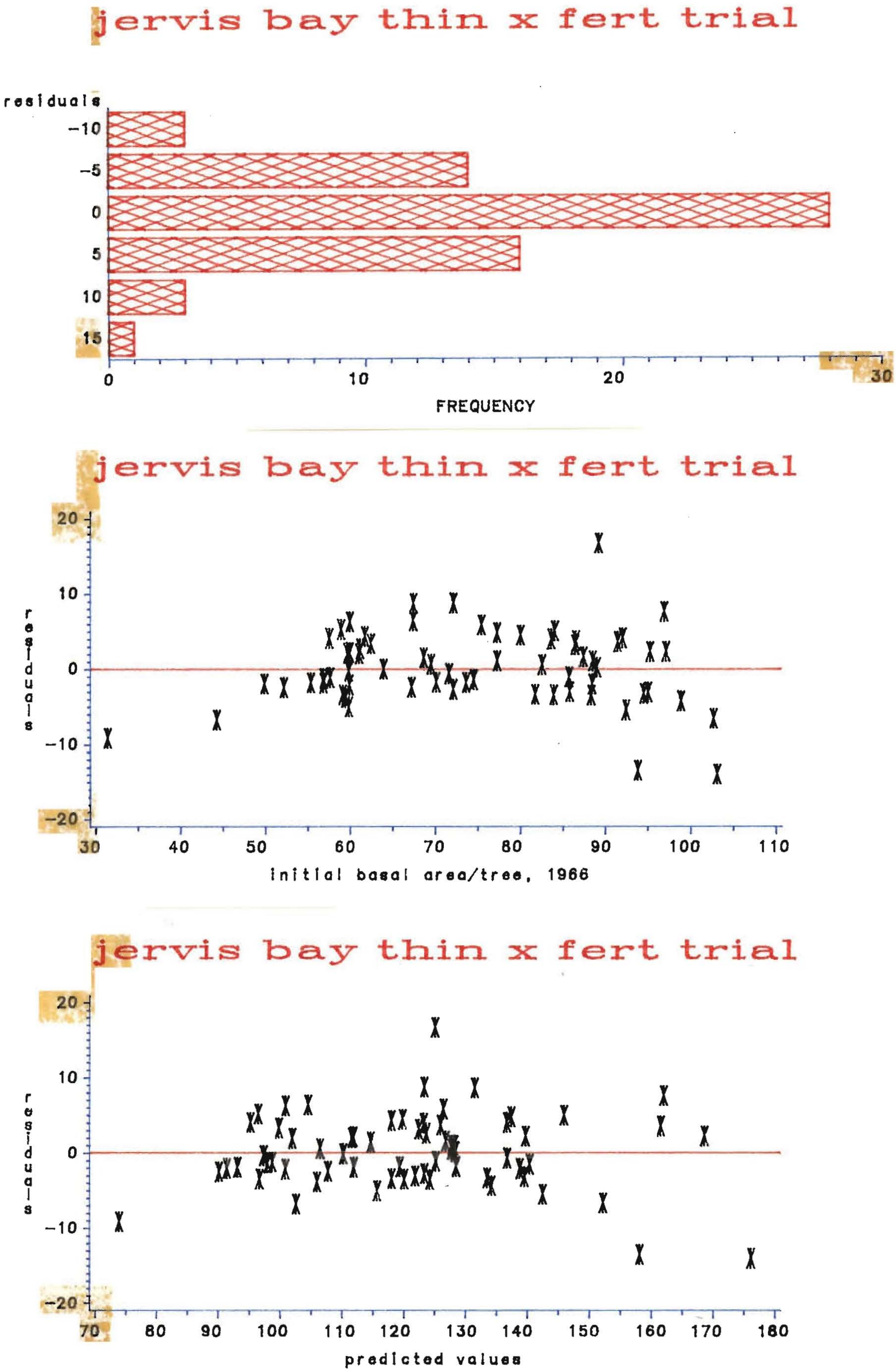

Figure (6.2) Residual plottings from Waring's thinning x fertilizer trial.

plot alone. While such a process possibly leads to unbiased estimates of treatment effects, any tests of hypotheses must be compromised, because:

1. the crucial assumption of independence of errors cannot be assumed;
2. the degrees of freedom associated with the error are too high.

The situation may arise in forest experimentation because trees within units are individually measured, and the analyst comes to regard the trees as basic units. The situation is analogous to cluster sampling [see Cochran (1963)], where a group of units are measured, but the cluster represents a datum.

6.4 IMPLICATIONS FOR BLOCKED EXPERIMENTS: SUGGESTED METHODOLOGY

The system of analysis developed in Chapter 4 is readily extended to structured experiments with any form of blocking. Kendall and Buckland (1960) define a block as

> the name given, mainly in experimental design, to a group of items (or experimental units) under treatment or observation ... the variation in the experimental observations is usually divided (by variance analysis) into effects due to differences between blocks and effects due to variation within blocks. The general purpose of dividing all the material in an experiment into blocks is to isolate sources of heterogeneity.

Linear models (4.11),(4.15) and (4.22) can be readily augmented to include any specific blocking structure, but subsequent analysis may proceed as shown. The only ramification is that generally the models are not of full rank, so some form of constraints is required to be imposed on the model parameters (see Chapter 2.1).

Unfortunately, experience with forest trials shows that blocking is quite frequently ineffective [Whyte and Mead (1976), Woollons (1980, 1985)]. Also the Bowmont spruce latin-square did not benefit from the structured rows and columns [Whyte (1965)]. Specifically, intra-block variation exceeds the inter-block component in many trials, so the benefit of a blocked design often does not eventuate. There are exceptions; moderate blocking efficiency was achieved in the experiment of Gerig (see Chapter 3), the NZFP trial mentioned in Chapter 4 has a relative efficiency of 184%, while Cromer and Pawsey (1957), using a randomised complete block design, achieved a relative efficiency of 257% in a South Australian spacing trial. In this case, the blocks were arranged over three discrete site classes.

The analytical ramifications of ineffective blocking do not impair the methodology developed in Chapter 4. Consideration might be given to pooling the variation associated with unsuccessful blocking with the residual error. Some care, however, is required, since a blocked design implies constrained randomisation, and it should not be undertaken when there is an *a priori* basis for imposing blocking, or when the design is highly structured. Some incomplete block designs not arranged in replications, cannot be validly unblocked, and can end up giving lesser precision, [Yates (1940)], but they are very rarely met in practice.

6.4.1 Uncontrolled fertility gradients

An alternative problem arises when significant fertility gradients do arise in forest experiments, but the trial design is unblocked or the direction of the gradient is not accounted for by the blocking. Yates (1965) strongly supports the idea of eliminating such gradients by a linear regression on plot position, which can very easily be achieved through use of a covariate, provided "when it is resorted to, it is clearly stated". Pearce (1983) elaborated on Yates' proposals, pointing out such trends are rarely linear, and squared or root transformations could be appropriate. Federer and Schlottfeldt (1954), and Outhwaite and Rutherford (1955) give examples for tobacco seeds, where polynominals up to the sixth degree were attempted. Pearce (1983) also pointed out trends in two directions are quite feasible and suggested regressing the data obtained from an experiment upon five covariates, namely x, y, the coordinates (of plot position) in two directions, and the functions x^2, y^2 and xy. The basic fertility pattern is thus assumed to be paraboloidic; if a little too exact to be practically true, large patches of good/bad fertility could be approximately adjusted for, but several such features would not be modelled well. For the method to be effective, a large number of residual degrees of freedom would be desirable, since up to five would be lost if the full model was utilised.

An allied method is to adjust treatment yields by neighbouring plots, following the suggestion of Papadakis (1937, 1940). For reasons of language and assessibility, the comments of Pearce are followed

> First he (Papadakis) took treatment means, and worked out the extent to which the performance of each plot deviated from the appropriate mean. Then for each plot a concomitant variable is worked out from the deviations of the neighbours. That quantity can be regarded as a measure of the inherent fertility of the plot. Finally, the data are analysed using a covariance adjustment on the concomitant variable.

While the Papadakis approach has been used with forest trials, for example, Correll and Anderson (1983), there are numerical difficulties and objections to the method. The covariance coefficient has to be solved iteratively until the estimate stabilises, [Pearce (1983), pp.51-54], and there are problems with dealing with edge and corner plots. Still other methods of dealing with systematic growth trends have been described by Wright (1978) and Swindel and Squillace (1980).

6.4.2 Example of a trial with an unblocked fertility gradient

Pearce's suggestion fordealing with fertility gradients is applied to an establishment-age trial. In 1959, H.D.Waring[1] laid out a species x fertilizer experiment (Waring, in-house data), at Jervis Bay, A.C.T., Australia. Three conifers, *Pinus radiata*, *Pinus elliottii* and *Pinus pinaster* were interacted with 11 fertilizer applications, involving rates and combinations of P, N, S, K, Ca, and several trace elements (see Appendix 4). The trial was completely randomised, containing 88 experimental units, each with 64 trees planted at 2.3 m x 2.3 m, with an inner 16 tree measurement plot. Four replications of each *Pinus elliottii* treatment were established, but two only for *Pinus radiata* and *Pinus pinaster*. Mensurational data included seedling heights a few months following planting (1959), and plot basal area (1966). Mortality was negligible, except in four plots which were severely damaged and removed from the dataset.

Analysis is not straightforward: examination of the initial seedling heights shows differences within and among the treatments, thus partially confounding the effect of species. Average initial heights and associated standard deviations are: (m)

Pinus radiata	0.34 ± 0.060
Pinus elliottii	0.22 ± 0.042
Pinus pinaster	0.28 ± 0.024

In addition preliminary inspection of the treatment yields shows high variation, with no obvious responses within species, and it is clear that the experiment is under-replicated to detect differences (if any) among

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[1] The author is grateful to Messrs.H.D.Waring and P.Snowdon for forwarding the data

several of the minor differences in imposed treatments.

These deficiencies are confirmed by using the model

$$E(Y) = \sum_{i=1}^{33} \alpha_i + \sum_{j=1}^{3} \beta_j X \tag{6.7}$$

where Y = basal area/plot, 1966

X = initial plot height, 1959

β_j = regression coefficients associated with the tree species

α_i = intercepts associated with the 11 x 3 treatments

giving an ANOVA:

Source	d.f.	SS	$p \geq F$
Slopes	3	0.0266	0.0001
Intercepts	32	0.0547	0.0338
Error	48	0.0460	
	83		

and subsequent testing confirms that while $H_0:\beta_{pin} = 0$, $H_0:\beta_{rad} = \beta_{ell} = 0$ is rejected ($p < 0.002$), but examination of the intercepts shows that significance is a consequence of *Pinus radiata* species yield, and not to any differential growth within species.

Accordingly, treatments 1-5 (control plus rates of P) and 6-11 (NP combinations) are pooled, so that utilising the model

$$E(Y) = \sum_{i=1}^{6} \alpha_i + \beta_{re} X \tag{6.8}$$

where β_{re} = a pooled regression coefficient for *Pinus elliottii* and *Pinus radiata* then further testing indicates that there are no significant nutrient responses for *Pinus radiata* and *Pinus elliottii*, but NP combinations have significantly ($p < 0.0035$) increased *Pinus pinaster* yield, relative to P and control treatments, reducing model (6.8) to

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \beta_{re}X \qquad (6.9)$$

with an ANOVA:

Source	d.f.	SS	p > F
Initial height	1	0.0561	0.0001
Intercepts	3	0.0338	0.0001
Error	79	0.0933	
	83		

Model (6.9) is highly significant, but exhibits considerable variation, with $R^2 = 0.599$. Accordingly, each datum is assigned x, y co-ordinate pairs based on plot position, and (6.9) is augmented to

$$E(Y) + \sum_{i=1}^{4} \alpha_i + \beta_{re}X + \beta_1 x + \beta_2 x^2 + \beta_3 y + \beta_4 y^2 + \beta_5 xy \qquad (6.10)$$

which gives an ANOVA:

Source	d.f.	SS	p > F
Slope	1	0.0264	0.0001
Intercepts	3	0.0132	0.0034
x	1	0.0108	0.0008
y	1	0.0023	0.1154
xy	1	0.0011	0.2793
x^2	1	0.0162	0.0001
y^2	1	0.0019	0.1468
Error	74	0.0656	
	83		

Subsequently, xy, then y^2 are omitted as covariates, giving an ANOVA:

Source	d.f.	SS	$p > F$
Slope	1	0.0272	0.0001
Intercepts	3	0.0135	0.0031
x	1	0.0178	0.0001
y	1	0.0057	0.0139
x^2	1	0.0150	0.0001
Error	76	0.0682	
	83		

All effects are now significant, at least at the 2% level, $R^2 = 0.707$, and the residual mean square, relative to model (6.9) is reduced by 25%. Residual plottings confirm the above model is satisfactory (see Fig. 6.3) and estimated basal area yields (m^2/ha), adjusted for plot position, (but not initial heights), are:

Pinus radiata	13.8
Pinus elliottii	10.7
Pinus pinaster	5.6
Pinus pinaster (fert)	8.8

Figure (6.4) illustrates the final model adjusted for plot position, while Figure (6.5) demonstrates the paraboloidic gradient (five plots are offset from the remainder of the experiment).

6.5 ALLIED TOPICS: SUMMARY

In this Chapter, several topics have been discussed which relate to or augment the methodology developed in Chapter 4. These can be summarised as follows:

1. In any analysis of variance the suggested procedures are affected by the statistical assumptions of normality, homogeneity, and independence of errors. The last is the most critical assumption, but is overcome by valid randomisation of treatments;

2. Weighted least-squares can be successfully incorporated into the system, and may be a useful adjunct when a fertilizer trial has several partially damaged plots;

3. The methodology is dependent on the determination of a valid estimate of experimental error. The degrees of freedom associated with the residual error usually depend on the number of plots, not trees within plots;

4. Blocked experiments can be analysed by the suggested system, after adopting appropriate linear constraints. If the blocking is not efficient, the trial might be analysed assuming a completely randomised design; provided the amount of blocking is not excessive or a structured design was not chosen for *a priori* reasons:

5. Uncontrolled fertility gradients in large fertilizer experiments can sometimes be partially removed by utilising covariates representing plot position.

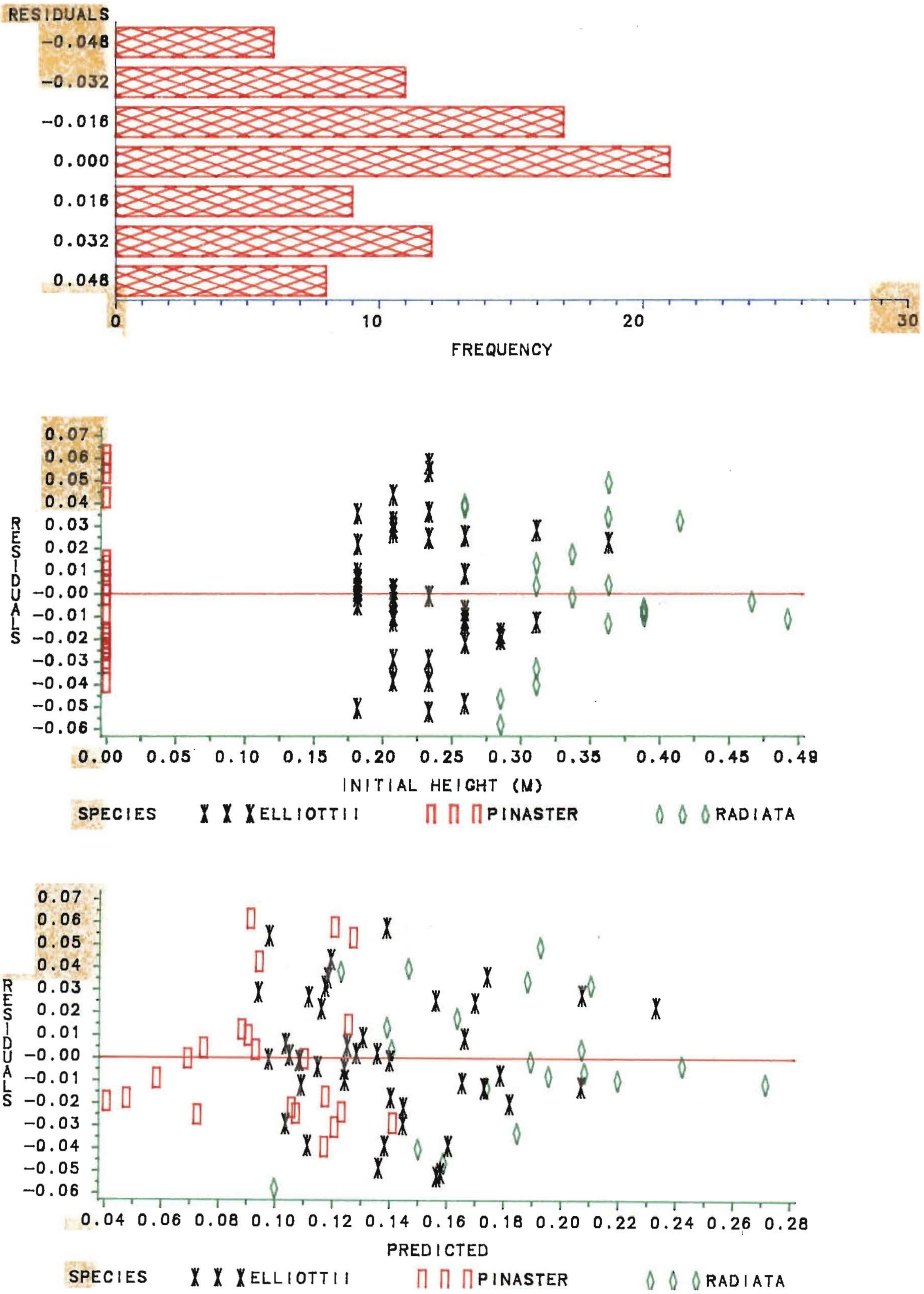


Figure (6.3) Residual plottings of model developed for species x fertilizer experiment, Jervis Bay, Compt.4.

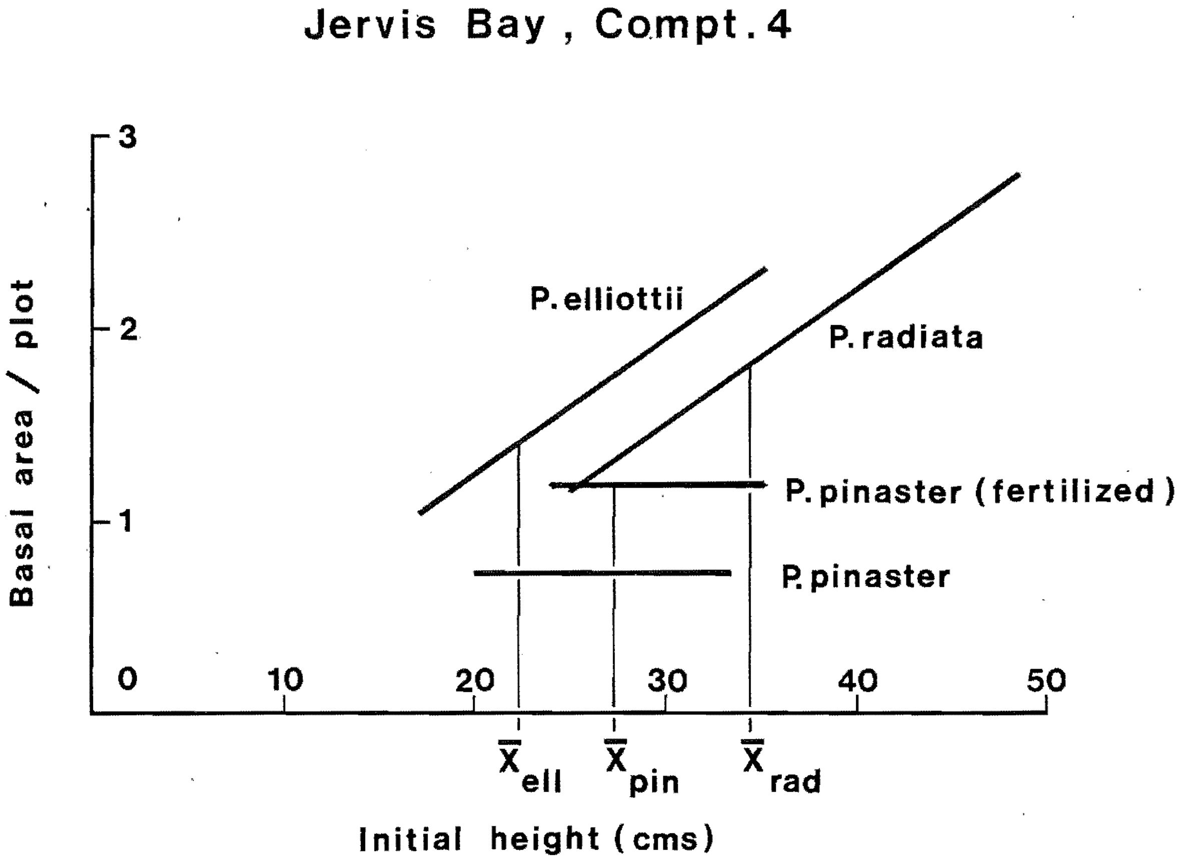


Figure (6.4) Model for species x fertilizer experiment, Jervis Bay, Compt.4.

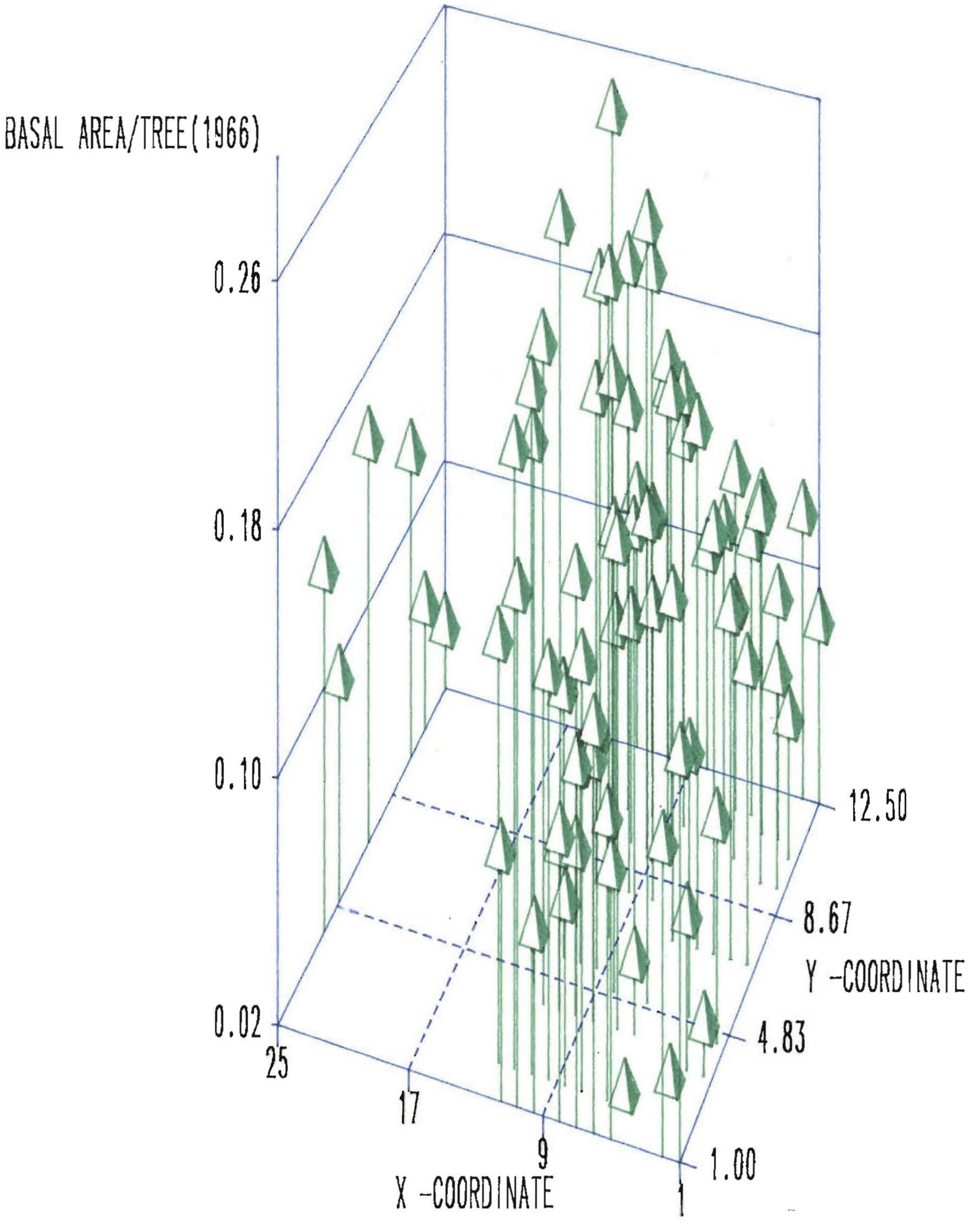


Figure (6.5) Diagrammatic illustration of the fertility gradient in Jervis Bay fertilizer x species experiment.

7. EMPIRICAL TESTING OF PROPOSED METHODOLOGY

7.1 Introduction

In Chapter 4, methodology was proposed to analyse the classes of fertilizer experiments which researchers commonly lay-out in established stands to study nutrition responses. This proposed system has already been demonstrated by re-analysis of several trials, but the aim of this Chapter is to apply the specific methodology in detail by using it with a major set of experiments, and to highlight the benefits that accrue through following the recommended procedures.

The methodology developed through Chapters 4 to 6 places considerable importance on defining a correct model structure for a given forest fertilizer experiment. In some situations, emphasis is also placed on the desirability of securing a small residual error. For example, judicious pooling of ineffectual blocking or non-responsive interactions [ascertained by the methods described in section (4.2)] can give a smaller experimental error, than would be obtained by rigorous adherence to a designed structure, and/or sole use of *a priori* contrasts. It was conceded in section (4.3) some analysts can object to this practice on statistical grounds as being risky or speculative, with increased chances of erroneous decisions from hypothesis testing. This criticism is fully accepted, but a case for occasional use of *a posteriori* pooling of alike effects is nevertheless supported, and justified further, because:

1. forest fertilizer experiments are nowadays very seldom conducted in isolation; results of an experiment can be built into subsequent trials to be tested again;

2. many conclusions can be substantiated by supplementary knowledge (for example, Gerig's experiment, presented in section [3.3.3])

suggests small responses to P and possibly to NP, a fully expected result based on *a priori* information;

3. when evaluating the consequences of incorrect decisions in hypothesis testing, the worst outcome is where no differences in treatments (applied) are erroneously declared. Failure to detect a fertilizer effect actually present deprives a forest manager of a potential source of wood, whereas the incorrect declaration of a response should quickly be recognised in further trials.

Between 1967 and 1975 NZFP established eight experiments studying the effects of fertilizers in thinned stands of *Pinus radiata* in areas of its forests around Tokoroa. Early results from five of these trials were reported by Woollons and Will (1975); additional information is now available in some of these five, while three others are hitherto unpublished. Altogether these experimental data collectively constitute a validation set to test the efficiency of the proposed system of analysis.

7.1.1 N.Z.Forest Products Limited Experiments: Mensurational Procedures

The NZFP experiments were characterised by a high level of mensurational effort; in particular the volumes of experimental trees were usually measured annually with a Barr and Stroud dendrometer. Since the method is common to all trials, it can be summarised right at the start. The Barr and Stroud dendrometer is an optical device for measuring overbark diameters at any point on the bole, over the range 3.8 to 500 cm, together with the distance along the stem between any two diameter measurements. The theory and manual operation of the instrument are reported by Jeffers (1956), Grosenbaugh (1963), and Sandrasegaran (1969). Pearce and Woollons (1973) detail the field drill utilized to measure the experimental trees in these experiments; tests suggest the adopted methodology gives a consistent estimate of overbark volume to within ± 4%.

The early trials were laid out in thinned stands of *Pinus radiata* regeneration. Field data collected included the bearings and distances between all trees in each of the experimental plots. In preparing the data for analysis, a measure of average plot competition was able to be developed, based on the Hegyi tree competition formula

$$CI_i = \sum_{j=1}^{n} (d_j/d_i)/Dist_{ij} \tag{7.1}$$

where, in (7.1)

CI_i = competition index for subject tree i

d_j/d_i = diameter breast height of a competitor j expressed as a proportion of the diameter breast height of a subject tree i

$Dist_{ij}$ = distance between tree i and competitor j

n = number of competitors, around a subject tree

Formula (7.1) is given by Hegyi (1974); Daniels (1976) found it to be strongly correlated with loblolly pine (*Pinus taeda*) diameter and height growth. The subject of competition and tree growth is very large; attention to it here is only brief and concerned strictly with the one specific application. In developing a useful index of competition the aim was simply to provide a possible additional covariate reflecting inter-tree competition within plots to decrease experimental error. The number of competitors, n, was originally regarded by Hegyi as all trees within a 3.05 metre (10 feet) radius of the subject tree, but he emphasised that the definition was arbitrary. Daniels (1976) defined a competitor as any tree that would be counted by an angle gauge factor of 10 centred on the subject tree. For this study, n was regarded as the number of competitors within a 5 metre radius of a subject tree. Two indices were calculated, one using diameter at breast height and a second with tree volume. Plot measures of competition were defined as

$$PCI_p = \sum_{i=1}^{N} (\sum_{j=1}^{n} [X_j/X_i]/Dist_{ij})/N \tag{7.2}$$

where, in (7.2)

X = selected trait (diameter or volume)

N = number of trees originally in any plot, before treatment

PCI_p = plot competition index for any experimental unit

Generally, stem volume o.b., diameter breast height, and tree height measurements were available, as detailed in Appendix 5.

The eight experiments represent a trial series studying responses to fertilizers on the Central North Island pumice soils, in thinned stands of *Pinus radiata* regeneration and newland plantings. Full details of each trial's objectives, design, layout, and applied fertilizers are given in Appendix 5. A brief summary of these is given in Table (7.1) which lists, for each experiment:

(a) a reference number
(b) a colloquial (road) name
(c) the year of establishment
(d) a summary of basic design and treatments imposed
(e) number of replications
(f) type of fertilizer applied
(g) last year of measurement
(h) whether plot competition data are available
(i) net residual stocking

with respect to (i), response variables (and associated covariates) were calculated in terms of trees alive at the time of final measurement; in no instance in any trials, was mortality found to be associated with fertilization.

7.2 TESTING OF STATISTICAL METHODOLOGY

7.2.1 Relationship between response variable and initial growing stock

(In the ensuing sections, reference should be made to Table 7.1, and Appendix 5, for details of trial treatments)

The total dataset, embracing the traits of volume, basal area and height, has given rise to nearly 100 analyses of response variables at various ages, modelled as functions of the relevant treatment and blocking structure, together with a covariate representing initial quanta of growing stock; [in only one case is the latter statistically non-significant]; the use of covariance, therefore, has contributed substantially to a lesser residual error and to less biased treatment responses, in the form of adjusted treatment means. In general, the significance and precision of the initial size covariate, is enhanced by (a) the number and dispersion of initial data, and (b) a shorter time span between the response variable and the commencement of an experiment. For example, from Trial No.5, an analysis of basal area/tree response, one year following fertilization gives an ANOVA:

TABLE 7.1

SUMMARY OF N.Z.FOREST PRODUCTS LIMITED EXPERIMENTS

(see Appendix 5 for complete details)

Trial	Colloquial Name	Year Est.	Basic Design and Layout	No. Reps	Fertilizer Applied	Final Year Meas.	Plot Competition Index	Net Residual Stocking
1.	Johnstone	1967	Completely randomised; two residual thinnings interacted with two levels of fertilizer	4	Composite	1986	Yes	571, 357
2.	Wainui	1969	Randomised complete block; 2^3 factorial, involving N, P, Mg/K[1]	4	As per factorial	1972	Yes	478
3.	Rata	1970	Randomised complete block, 0, 25, 50, 75 and 100% rates of fertilizer, as used in 1	4	Composite	1976	Yes	463
4.	Owen	1971	Completely randomised. 0, 100, 200 and 300% rates of fertilizer	4	Composite	1974	No	432
5.	Plateau	1973	Randomised blocks; partial confounding. 2^4 factorial involving N, P, Mg, and K	2	As per factorial	1974	No	592
6.	Hioho	1973	As per Trial 5	2	As per factorial	1975	No	1178
7.	Urea	1974	Randomised complete block. 4 treatments of N	4	N, as urea	1980	No	437
8.	Pepper	1975	Completely randomised. 4 treatments of N	5	N, as urea	1986	Yes	398

1. The symbol Mg/K denotes that both Mg and K were combined as a single factor, and as such, their individual effects are totally confounded

Source	d.f.	SS(x10^6)	p > F
Initial basal area/tree	1	801.20	0.0001
N	1	2.71	0.0005
P	1	0.03	
Mg	1	0.38	
K	1	0.61	0.0668
NK	1	0.03	
NP	1	0.14	
Error + pooled interactions	24	4.00	
	31		

The regression coefficient is extremely significant ($p \leqslant 0.0001$) with an estimated coefficient 1.0229 ± 0.01476. An ANOVA (without utilisation of the covariate) gives a residual mean square of 32.208, compared to 0.166, a 200-fold increase.

In contrast, an analysis of Trial No.1 for basal area/tree response, 1967-1976 (preliminary analysis confirmed that one pooled slope is sufficient, and two intercepts are justified, fertilized and non-fertilized) gave the ANOVA;

Source	d.f.	SS(x10^6)	p > F
Initial basal area/tree	1	592.71	0.0006
Plot competition	1	499.78	0.0012
Treatments	1	463.48	0.0015
Error	12	335.24	
	15		

While the initial growing stock covariate is still strongly significant ($p < 0.0006$) the estimated coefficient is 1.891 ± 0.4105 and the reduction in the residual mean square by utilising the coefficient is two-fold. In all applications the utilisation of an initial quantum covariate is most beneficial, but its efficiency is relatively less with increasing interval to final measurement of the response variable, and few available degrees of freedom.

7.2.2 Functional form of initial growing stock covariate and response variable

For most applications, the relationship between the response variable and initial growing stock is found to be linear, but there are occasional exceptions. For example, in an analysis of Trial No.7, basal area response, 1976, some curvilinearity is present between basal area/tree, 1976, and initial basal area/tree, 1974. Analyses are required to ascertain an appropriate transformation to linearise the relationship; the following ANOVA gives three analyses: (1) using basal area/tree, 1974 as a covariate, (2) the latter transformed by a logarithm, and (3) the latter transformed by a square-root.

Source	d.f.	SS(1) x 10^6	p > F	SS(2) x 10^6	p > F	SS(3) x 10^6	p > F
Covariate (1)	1)	448.24	0.0001				
Covariate (2)	1) 1			450.81	0.0001		
Covariate (3)	1)					449.70	0.0001
Blocks	3	11.90	0.14	10.60	0.11	11.24	0.12
Treatments	3	6.66	0.32	9.87	0.13	8.16	0.20
Control v Fert.	1	5.89	0.09	8.00	0.04	6.91	0.06
Tr. C v Tr. D	1	0.69	0.53	1.82	0.24	1.16	0.37
Error	8	12.80		10.23		11.34	
	15						

Basal area/tree is a highly significant (0.1%) covariate, but use of a logarithmic transformation decreases the error mean square by 20%, and achieves a better linearity than the square-root transform. (The gain in sensitivity is sufficient to raise the significance of the control versus fertilized treatments contrast from 9 to 4%). The original and transformed data are illustrated in Figure (7.1).

7.2.3 Test for disparate slopes, for trials with t treatments

For a majority of experiments, a single regression coefficient suffices to model the relationship between a response variable, and the covariate rperesenting initial quanta of growing stock, but there are exceptions to this. For example, in Trial No. 8, an examination of basal area yield between 1975 and 1980 gives an initial model

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \sum_{j=1}^{4} \beta_j X \qquad (7.3)$$

where, in (7.3)

Y = basal area/tree, 1980
X = basal area/tree, 1975.

A graph of Y on X clearly shows the relationship to be linear, but with signs of disparate slopes. The resultant ANOVA gives:

Source	d.f.	SS($\times 10^4$)	p > F
Single slope, intercept	1	2181.56	0.0001
Individual intercepts	3	66.07	0.0141
Disparate slopes	3	34.43	0.0820
Error	11	43.30	
	18		

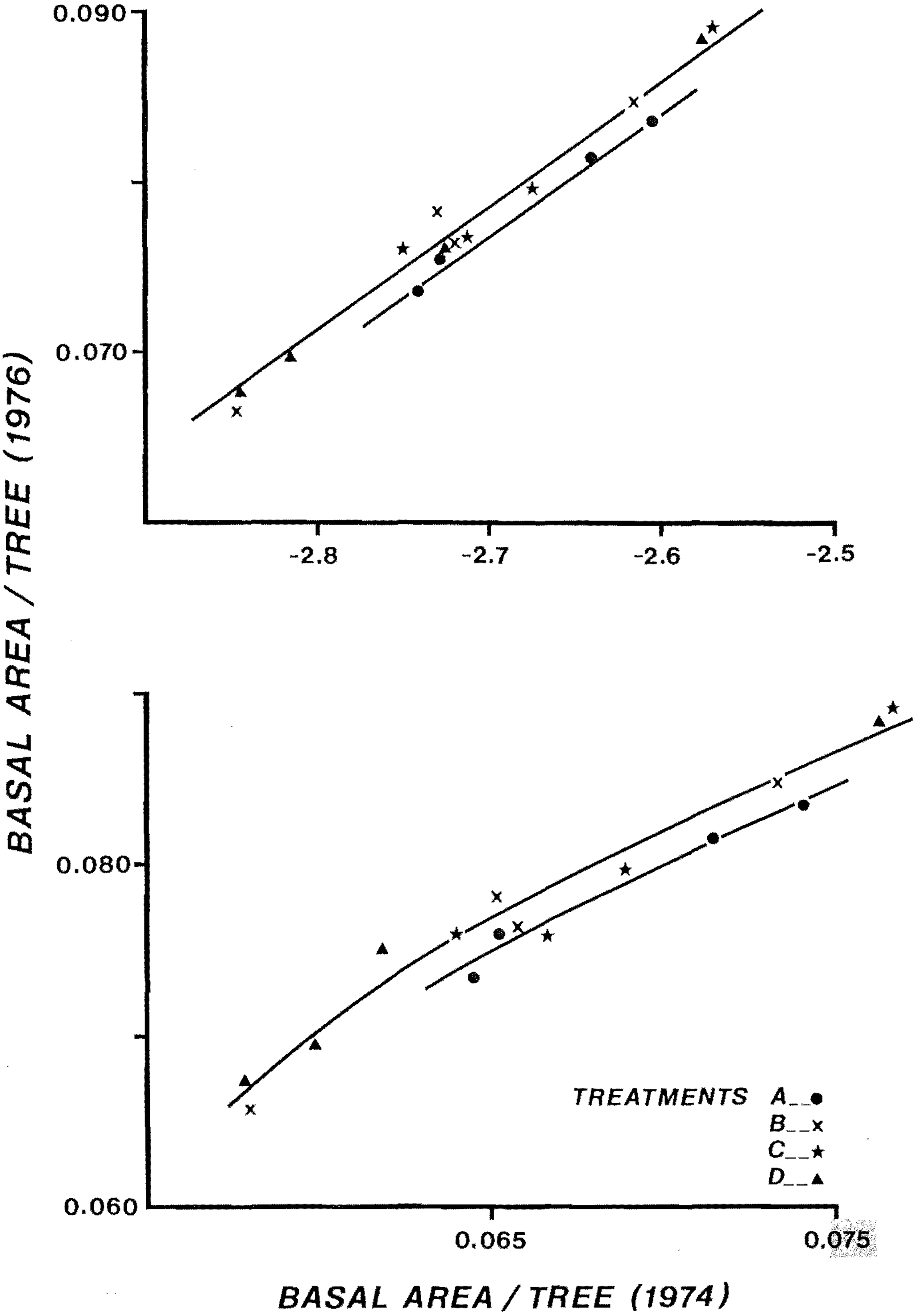


Figure (7.1) Data for Trial No. 7, illustrating the curvilinear relationship between the response variable and covariate, and its linearisation by a logarithm transformation

(For the above data, one plot was ruined by wind-throw). The effects of an overall slope and treatment intercepts are strongly significant, but there is some evidence of significant differences among the regression slopes (10%). The four estimated coefficients are:

$$\hat{\beta}_A = 1.1471 \qquad \hat{\beta}_B = 1.6218$$
$$\hat{\beta}_C = 1.5398 \qquad \hat{\beta}_D = 1.1388$$

where the subscripts A to D define the treatments given for Trial 8 in Appendix 5.

Tests of the hypotheses

$$H_o: \beta_A = \beta_D \quad \text{and} \quad \beta_B = \beta_C$$

are both accepted, $p < 0.50$

The groupings A + D, B + C logically subdivide into treatments dosed at later ages, and those with a single application, or not at all. Model (7.3) then reduces to

$$E(Y) = \sum_{i=1}^{4} \alpha_i \quad \sum_{j=1}^{2} \beta_j X \tag{7.4}$$

With an ANOVA:

Source	d.f.	SS(x10^6)	p > F
Slopes	2	1902.64	0.0001
Individual intercepts	3	378.61	0.0001
Error	13	44.11	
	18		

A test of H_o: $\beta_{A+D} = \beta_{B+C}$ is given by

$$(1.6008 - 1.1423)/\surd(3.39 \times 10^{-6}[1580 + 4652]) = 3.14$$

for t with 13 degrees of freedom

which is significant at the 1% level. Thus model (7.4) has a residual mean square of 3.3931 whereas a model with a single pooled slope has a residual mean square of 5.5521, representing a loss on precision of 39%.

A second example is available from Trial No. 4 analysing volume response between 1971-1973. Utilising volume/tree, 1973, as the response variable, and volume/tree, 1971 as a covariate and assuming a model

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \sum_{j=1}^{4} \beta_j X \tag{7.5}$$

gives an ANOVA

Source	d.f.	SS(x10^4)	p > F
Single slope, intercept	1	703.37	0.0001
Individual intercepts	3	45.88	0.0073
Disparate slopes	3	16.18	0.0921
Error	8	14.47	
	15		

A test for disparate slopes is weakly significant; the estimated values are:

$\hat{\beta}_A = 0.7956$ $\hat{\beta}_B = 0.9255$

$\hat{\beta}_C = 1.2792$ $\hat{\beta}_D = 1.3012$

where the subscripts A to D define the treatments given for Trial No.4 in Appendix 5.

Tests of the hypotheses:

$$H_o: \beta_A = \beta_B \quad \text{and} \quad H_o: \beta_C = \beta_D$$

are both accepted ($p < 0.50$)

The two groupings subdivide into high or low rates of fertilizer application, giving a comprehensible reason for observing disparate growth.

If one employs pooled slopes β_{A+B}, β_{C+D}, giving the model

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \sum_{j=1}^{2} \beta_j X \tag{7.6}$$

the ANOVA from model (7.6) is:

Source	d.f.	SS($\times 10^4$)	p > F
Slopes	2	506.52	0.0001
Treatments	3	15.39	0.0604
Error	10	14.98	
	15		

The hypothesis H_o: $\beta_{A+B} = \beta_{C+D}$ is tested by

$$(1.2871 - 0.8984)/\surd(1.4976 \times 10^{-4}[58.85 + 45.14]) = 3.11$$

for t with 10 degrees of freedom.

which is rejected at the 1.5% level. Thus, the recognition of two regression coefficients for appropriate treatments, as opposed to a pooled slope, decreases the error mean square by 28%.

7.2.4 Use of additional covariates

While covariates representing initial quanta of growing stock almost always give some increase in precision and less biased estimates of treatment response, further gains may be achieved by utilising additional covariates as shown in Chapter 3, with the re-analysis of the data of Gerig *et al.* (1978). Further examples can be demonstrated for Trial No. 1 of the NZFP data, examining basal area response between 1970 to 1976.

Preliminary analysis of the experiment shows that the models

$$E(Y_k) = \sum_{i=1}^{2} \alpha_i + \beta X \qquad (7.7)$$

where in (7.7)

Y_k = basal area/tree, 1970, 1971, ..., 1976

X = basal area/tree, 1967

β = a common regression coefficient

α_i = two intercepts, representing fertilised and control treatments, pooled for both stockings

give satisfactory fits over each of the seven years. However, when plot competition is added as a second covariate, appreciable decreases in residual mean squares are achieved. Table (7.2) summarises the analysis for 1970 to 1976, giving:

(a) the significance of the two covariates, and the difference between the two intercepts, evaluated at $\bar{\bar{X}}$, the grand mean;

(b) the two estimates of the error mean square, with and without the inclusion of the competition covariate;

(c) the estimated coefficient, denoted γ, associated with the competition index.

Table (7.2): Results from analysis of Trial No.1

(a) 1 = initial basal area/tree, 2 = PCI, 3 = intercepts

	(p > F)						
	1970	1971	1972	1973	1974	1975	1976
1	0.0001	0.0001	0.0001	0.0001	0.0001	0.0002	0.0006
2	0.0012	0.0006	0.0011	0.0008	0.0012	0.0012	0.0012
3	0.0001	0.0002	0.0002	0.0003	0.0008	0.0013	0.0015

(b) 1 = M.S., with second covariate included

2 = M.S., with initial growing stock covariate, alone

Error mean square

1	3.76	5.48	8.81	11.41	16.23	21.79	27.94
2	8.57	19.74	20.43	28.13	37.23	50.18	64.23
% decrease in error mean square	56	72	57	59	56	56	56

(c) Estimated regression coefficient, for PCI

$\hat{\delta}_i$	-0.0131	-0.0175	-0.0203	-0.0248	-0.0273	-0.0318	-0.0360

From Table (7.2) it is clear that the second covariate, plot competition index, has substantially reduced the residual error (up to 72%) with consequent better sensitivity in hypothesis testing, and narrower confidence intervals pertaining to treatment response. The regression coefficients ($\hat{\delta}_i$) are logically negative, and allow an interpretation that, with successive years, the influence of the competition variable becomes progressively stronger. Figures (7.2) and (7.3) give plot maps of two experimental units from Trial No.1, depicting those with the highest and lowest competition index, respectively. (It should not be inferred that stocking would operate in a similar manner to plot competition. While correlated to PCI, substitution of plot density as an alternative second covariate in model (7.7) does not reveal any significant effect.)

It may be seen that the annual measurements of basal area available for Trial No. 1 allow an alternative form of analysis. Each plot may be modelled by a yield or difference equation, then the estimated parameters examined by an ANOVA; an example is given by Woollons (1985) [see Appendix 1]. The data for Trial No. 1 is utilised in an allied way, in Chapter 9 of this thesis.

Further analysis for Trial No. 8, in (7.2.3) confirms that the efficiency of plot competition as a secondary covariate is not an artifact of one experiment. Augmenting model (7.4) to

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \sum_{j=1}^{2} \beta_j X + \sum_{k=1}^{2} \gamma_1 Ƶ \qquad (7.8)$$

where in (7.8), $Ƶ$ = plot competition index, and the subscripts k denote coefficients associated with treatments A+D, and B+C, gives an ANOVA

Source	d.f.	SS($\times 10^6$)	$p > F$
Slopes	2	1312.62	0.0001
Among intercepts	4	127.10	0.0001
Plot competition	2	30.61	0.0015
Error	11		
	19 (uncorrected)		

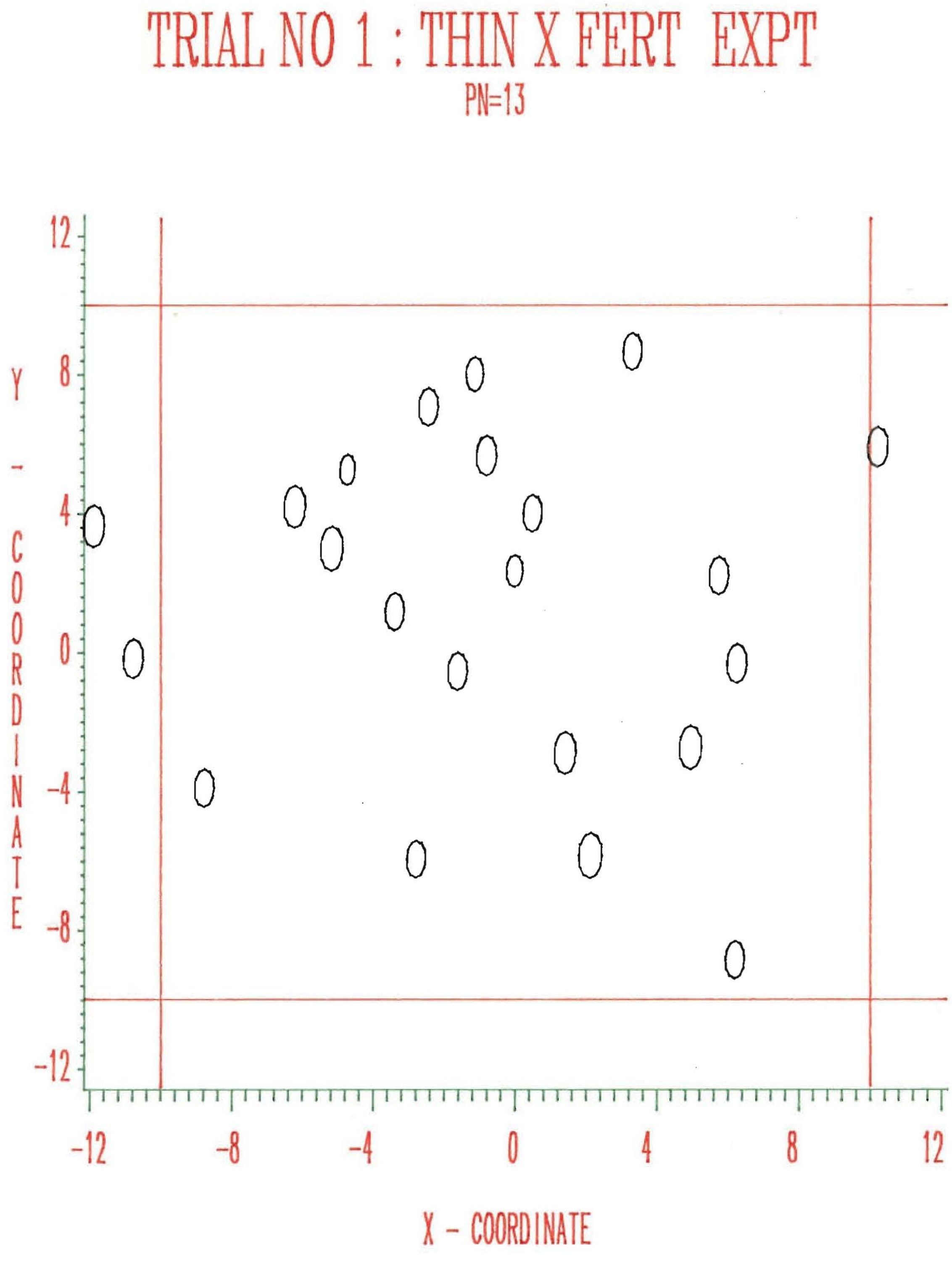


Figure (7.2) Plot 13, Trial No.1. Example of high competition (PCI = 1.052).

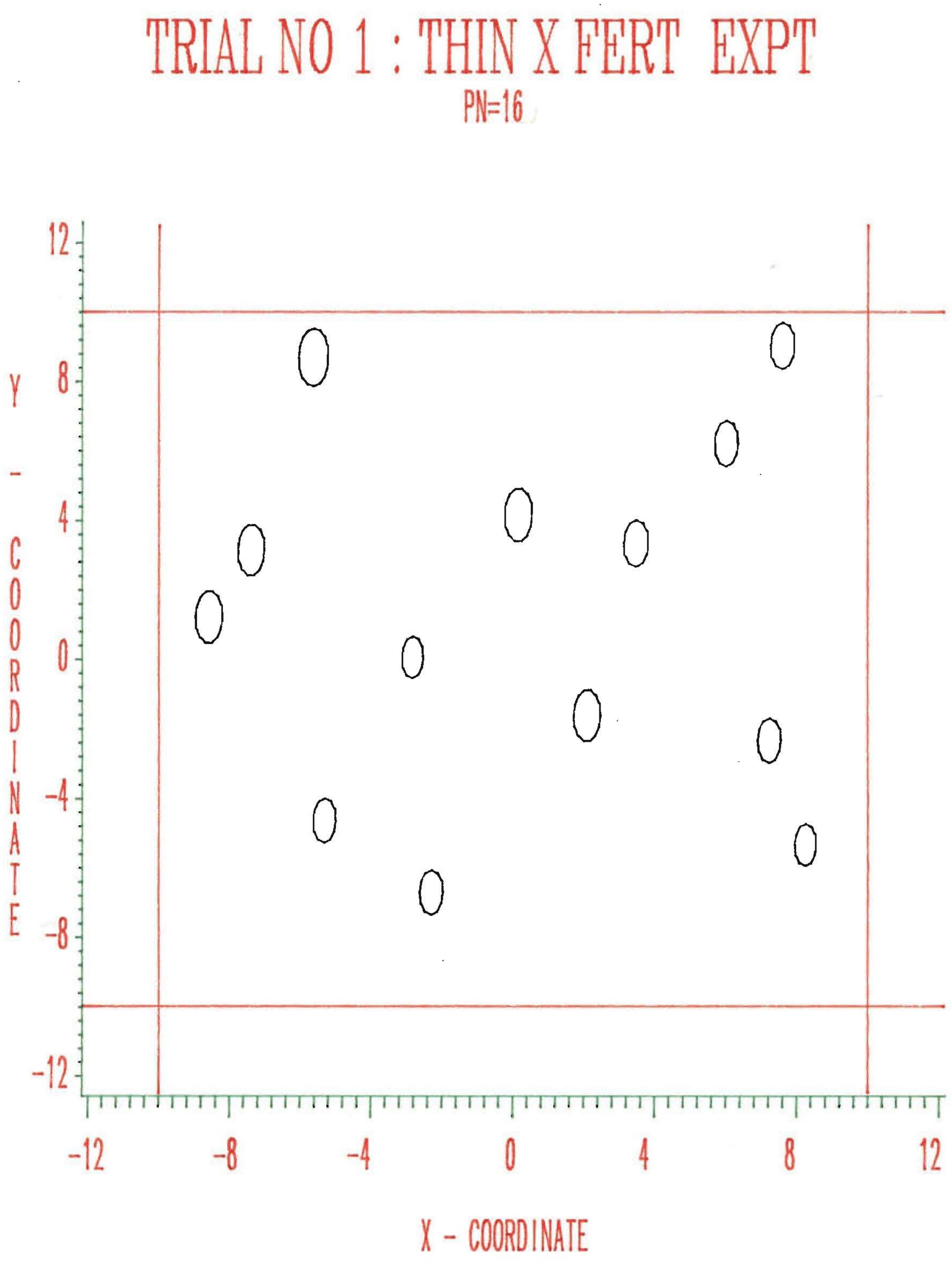


Figure (7.3) Plot 16, Trial No.1. Example of low competition (PCI = 0.272).

Comparison to model (7.4) and the associated ANOVA, shows that the residual mean square has been further reduced by 64%, thus again substantially contributing to more sensitive tests of hypotheses, and narrower confidence intervals. Substitution of stocking for PCI in (7.8) gives no equivalent gain.

7.2.5 Adjusted treatments responses

The process of adjusting treatment yields or responses by covariance is well covered in statistical texts, but its value, particularly with multiple covariance models can be demonstrated by further analysis of Trial No.1, basal area responses from 1970 to 1976, which were given in Section (7.2.4). Table (7.3) gives estimated basal area/ha response, 1970-1976, for the two residual stockings estimated by (a) treatment means, (b) treatment means, adjusted for initial differences in quanta of growing stock, (c) as for (b), but further adjusted for initial differences in plot competition index.

Table (7.3): Responses to fertilizer for Trial No.1

	Unadjusted		Adjusted for initial size		Adjusted for initial size and competition	
	571	357	571	357	571	357
	stems/ha		stems/ha		stems/ha	
1970	3.48	2.17	2.68	1.67	3.04	1.91
1971	4.41	2.76	3.64	2.28	3.97	2.48
1972	5.04	3.15	4.20	2.62	4.59	2.87
1973	5.43	3.40	4.46	2.79	4.93	3.08
1974	5.69	3.56	4.62	2.89	5.15	3.22
1975	6.17	3.86	5.02	3.13	5.63	3.52
1976	6.72	4.20	5.50	3.44	6.19	3.87

By chance, fertilized treatments were initially allocated to plots of somewhat larger growing stock; hence, when adjusted by the regression coefficient, estimated responses are significantly reduced by the adjustment process. On the other hand, the fertilized plots have generally lower competition indices; the regression coefficient associated with PCI is negative, so the effect of the second adjustment is to increase the estimated responses, relative to the first adjustment. The absolute values of both regression coefficients increase with time, so the disparity among the three estimates becomes more marked with successive years. It can be seen that the act of measuring plot competition and incorporating it in analysis, apart from securing better precision, has also given appreciably larger estimates of response, than would be the case if a single covariate had been utilised.

7.2.6 Analysis of fertilizer x thinning experiments

The recommended procedures for examination of fertilizer x thinning experiments given in Chapter 4 are applied to Trial No.1; assuming a model

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \sum_{j=1}^{4} \beta_j X + \gamma Z \qquad (7.10)$$

where Y, X = basal area/tree, 1968 and 1967 respectively

α, β = intercepts and regression coefficients associated with the four treatments

Z = plot competition index

gives an ANOVA:

Source	d.f.	Type I SS($\times 10^6$)	$p > F$
Common slope	1	600.93	0.0001
Intercepts	3	9.31	0.0099
Among slopes	3	0.53	
Plot competition	1	0.04	
Error	7	2.56	
	15		

A single pooled regression coefficient is strongly significant, and the addition of treatment intercepts are also significant (1%), but the further addition of unique slopes for each treatment is non-significant, as is the second covariate, plot competition. Accordingly, model (7.10) is reduced to

$$E(Y) = \sum_{i=1}^{4} \alpha_i + \beta X \tag{7.11}$$

giving an ANOVA:

Source	d.f.	SS(x10)	p > F
Slope	1	190.40	0.0001
Intercepts	3	9.31	0.0013
Error	11	3.13	
	15		

Tests of the hypotheses

$$\begin{aligned} &H_0: E(Y_{571(f)}/\overline{X}_{571}) = E(Y_{571(c)}/\overline{X}_{571}) \\ \text{and} \\ &H_0: E(Y_{357(f)}/\overline{X}_{357}) = E(Y_{357(c)}/\overline{X}_{357}) \end{aligned} \tag{7.12}$$

where in (7.12)

f, c = denote fertilized or control treatments

571,357 = denote the two residual stockings of the experiment

are tested by formula (2.21) and are rejected at the 2 and 1% level, respectively.

The final model is illustrated in Figure (7.4) together with the original data.

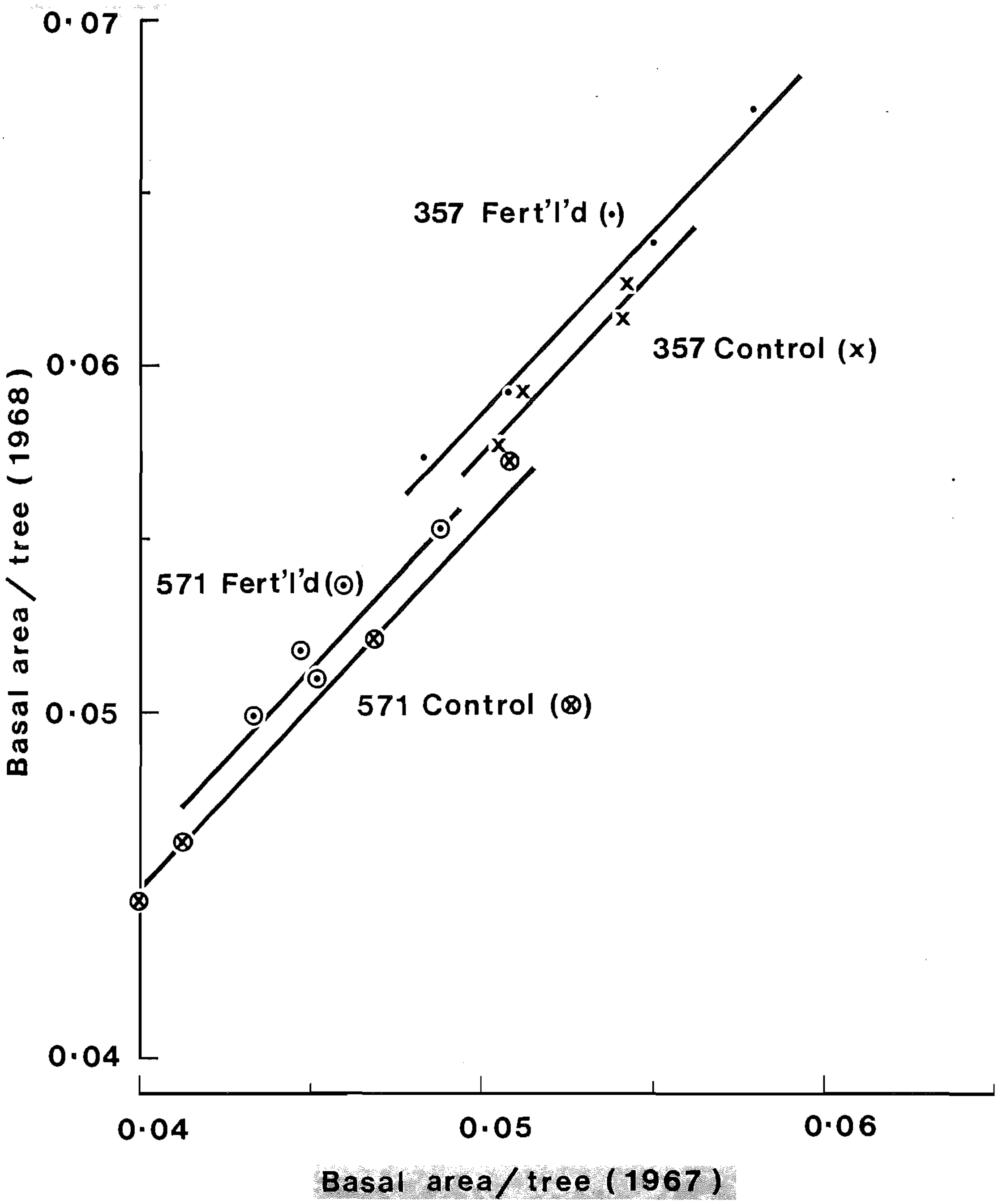


Figure 7.4 Model and data for Trial No.1, basal area growth, 1967-1968.

The regression model (7.11) essentially reveals four levels of basal area growth for 1967-1968, and suggests a small interaction between thinning levels and fertilizer application, by virtue of the responses not being strictly proportional to stocking. The efficiency of the analysis can be judged by comparing it to a factorial model and analysis, an approach favoured for example, by Barclay and Brix (1985), but rejected by Woollons (1985), for the general examination of fertilizer x thinning experiments. Assuming a model

$$E(Y) = \mu + T_i + F_j + (TF)_{ij} + \beta X \tag{7.13}$$

where Y = basal area/tree <u>or</u> basal area/plot, 1968
X = basal area/tree <u>or</u> basal area/plot, 1967
T_i = effect of thinning
F_j = effect of fertilization
$(TF)_{ij}$ = an interaction term
β = a regression coefficient

then, analysis of variance of model (7.13), for (1) basal area/tree and (2) basal area/plot, as a response variable and covariate, gives:

ANOVA (1)

Source	d.f.	SS(x10^6)	p > F
Fertilization	1	3.53	0.0029
Thinning	1	5.91	0.0004
Fertilizer x thinning	1	0.26	0.3252
Initial basal area/tree	1	190.86	0.0001
Error	11	2.67	
	15		

The effects of thinning, fertilization, and the covariate are strongly significant (< 1%), and the residual mean square is 15% more precise than that derived for model (7.11). However, when estimates of response are evaluated for each regime, the contrasts are significant at the 3 and 7% level (c.f. 2 and 1%).

ANOVA (2)

Source	d.f.	SS(x10^6)	p > F
Fertilization	1	819.76	0.0147
Thinning	1	62.83	0.4406
Fertilizer x thinning	1	9.86	0.3672
Initial basal area/plot	1	116 974.00	0.0001
Error	11	98.13	
	15		

Use of basal area/plot as a response variable, however, produces a completely illogical analysis. Thinning is declared non significant, a consequence of the variable being heavily correlated with the covariate. Fertilization is significant only at the 2% level, and contrasts for fertilizer response within regimes are significant at the 4% (357 stems/ha) or non-significant (571 stems/ha).

There are analytical grounds, therefore, for (1) preferring basal area/tree as a response variable, and (2) utilising a regression model such as (7.11) and evaluating responses at each regime initial mean, for adequate examination of a fertilizer x thinning experiment.

7.2.7 Analysis of factorial experiments

Examination of volume growth, 1973-1975, in Trial No.6 gives an example of a factorial model analysis. Using volume/tree (Y) as the response variable and adopting a model

$$
\begin{aligned}
E(Y) \;=\; & \mu + N_i + P_j + (NP)_{ij} + Mg_k + (NMg)_{ik} + (PMg)_{jk} \\
& + (NPMg)_{ijk} + K_l + (NK)_{il} + (PK)_{jk} + (NPK)_{ijl} \\
& + (MgK)_{kl} + (NMgK)_{ikl} + (PMgK)_{jkl} + R(B)_{mn} + \beta X \quad (7.14)
\end{aligned}
$$

where N_i, P_j, Kg_k, K_l = effects of nitrogen, phosphorus and potash

$R(B)$ = effects of blocks within replications

β = a regression coefficient

X = volume/tree, 1973

gives an ANOVA:

Source	d.f.	SS($\times 10^4$)	$p > F$
N	1	589.40	0.0051
P	1	19.43	
NP	1	49.54	
Mg	1	81.84	
NMg	1	88.87	
PMg	1	9.32	
NPMg	1	164.67	0.0988
K	1	75.32	
NK	1	3.54	
PK	1	9.68	
NPK	1	69.64	
MgK	1	83.06	
NmgK	1	70.81	
PMgK	1	14.29	
Blocks	3	1814.68	0.0001
Initial volume/tree	1	5992.34	0.0001
Error	13	677.40	
	31		

The main effect of nitrogen is highly significant, (0.5%), the covariate is very strongly significant (0.1%) and the effect of the blocking is strongly significant, providing a rare example in New Zealand of a blocked design giving increased precision in a forest fertilizer experiment; the relative efficiency of the blocking [Ostle (1963), p.375] is calculated to be 1.61. The estimated response to nitrogen is 4.73 m^3/ha and a 95% confidence interval is given by $\pm$ 3.03 m^3/ha.

A second analysis, achieved by pooling all interactions with the error, gives an ANOVA:

Source	d.f.	SS($\times 10^4$)	$p > F$
N	1	608.01	0.0025
P	1	26.39	
Mg	1	52.87	
K	1	60.60	
Blocks within Reps	3	1740.51	0.0001
Initial volume/tree	1	8053.88	0.0001
Error	23	1219.94	
	31		

Thus, while the residual mean square is slightly inflated (53.04 compared to 52.11) by pooling the interactions, the effect of nitrogen is now significant at the 0.25% level, and a 95% confidence interval is given by $\pm$ 2.93 m^3/ha. As discussed in Chapter 4, the pooling of non-significant interactions can give limited success, because one or two combinations commonly exhibit weak, but illogical significance. For these data, the second order interaction NPMg is shown to be significant at the 10% level. There is absolutely no reason to regard this effect as real, but the chance outcome results in nullifying somewhat the decision to pool non-responsive treatments with the error estimate; nevertheless the strategy is still recommended, even if the enhancement is often minor provided that the reasons for so doing are logical and that the dangers are recognised.

7.2.8 Use of yield and growth as response variables

It was explicitly demonstrated in Chapter 5 and Appendix 3 that examination of forest fertilizer experiments can be undertaken by using either yield or growth as a response variable together with a covariate representing initial quanta of growing stock. Both give identical estimates of residual error, significance of treatment effects, and estimates of response; two minor differences occur with growth as the response variable, (1) the regression coefficient is reduced by unity; and (2) the multiple correlation coefficient squared, R^2, is reduced in value, but either response variable is equally valid in practice.

As argued in Chapter 5, use of growth as a response variable, but without an auxiliary variable is occasionally a good approximation, but can easily result in coarse significance testing, and biased estimates of response. For example, Table (7.4) gives results from Trials Nos. 1 and 7, analysing (1) volume response 1967-1971, and (2) volume response, 1974-1977, respectively. The four analyses of variance and related results given for each experiment, represent the outcome of analysing:

(a) volume/tree growth, with a covariate representing initial average volume/tree;

(b) volume/tree Yield, with a covariate representing initial average volume/tree;

(c) as for (a), but with no covariate;

(d) as for (b), but with no covariate.

Table (7.4): Results from Trials Nos. 1 and 7

Trial No.1

(a) growth(with covariate) (b) yield (with covariate)

Source	d.f.	SS	p > F	SS	p > F
Treatments	1	0.018 96	0.0004	0.018 96	0.0004
Covariate	1	0.063 67	0.0001	0.196 91	0.0001
Error	13	0.010 76		0.010 76	
	15				

$\hat{\beta} = 1.318 \pm 0.1503$ $\hat{\beta} = 2.318 \pm 0.1503$

$R^2 = 0.901$ $R^2 = 0.957$

Adj.	Treatment	Response	Adj.	Treatment	Response
1	0.4182		1	0.8576	
2	0.4880	0.0698	2	0.9274	0.0698

(c) growth (no covariate) (d) yield (no covariate)

Source	d.f.	SS	p > F	SS	p > F
Treatments	1	0.033 30	0.0253	0.04 62	0.0993
Error	14	0.074 44		0.20 77	
	15				

$R^2 = 0.309$ $R^2 = 0.182$

Treatment		Response	Treatment		Response
1	0.4075		1	0.8386	
2	0.4988	0.0913	2	0.9463	0.1077

Trial No.7

(a) growth (with covariate) (b) yield (with covariate)

Source	d.f.	SS	p > F	SS	p > F
Treatments	3	0.010 14	0.0136	0.010 14	0.0136
Covariate	1	0.001 35	0.1537	0.086 34	0.0001
Error	10	0.005 69		0.005 69	
	15				

$\hat{\beta} = 0.1432 \pm 0.0928$ $\hat{\beta} = 1.1432 \pm 0.0928$

$R^2 = 0.677$ $R^2 = 0.950$

Adj.	Treatment	Response	Adj.	Treatment	Response
1	0.2843		1	0.9899	
2	0.2853	0.0010	2	0.9909	0.0010
3	0.3293	0.0440	3	1.0349	0.0440
4	0.3447	0.0604	4	1.0503	0.0604

(c) growth (no covariate) (d) yield (no covariate)

Source	d.f.	SS	p > F	SS	p > F
Treatments	3	0.010 60	0.0147	0.021 15	0.4987
Error	11	0.07 05		0.092 04	
	14				

$R^2 = 0.602$ $R^2 = 0.187$

Treatment		Response	Treatment		Response
1	0.2863		1	1.0059	
2	0.2835	- 0.0028	2	0.9765	- 0.0296
3	0.3258	0.0395	3	1.0074	0.0015
4	0.3490	0.0627	4	1.0849	0.0790

For both experiments, the use of either yield or growth as the response variable, in conjunction with an initial quantum covariate, gives identical (1) tests of hypotheses concerning differences in treatments, (2) residual mean squares, and (3) estimated adjusted responses. With growth, the regression coefficient is reduced by unity, but as the associated standard error is the same in either case, this reduces the significance of the hypothesis, $H_0:\beta=0$. Utilisation of yield as a response variable but with no covariate, for both experiments gives appreciably weaker tests of significance; for Trial No.7 the hypothesis of treatment differences is completely non-significant, whereas an equivalent analysis including a covariate achieves significance at the 1.4% level.

Analyses using growth as a response variable without an initial quantum covariate can be inconsistent and should be avoided. As argued in Chapter 5, such an approach is equivalent to utilising yield as a response variable with a covariate *a priori* defined as unity; alternatively it can be regarded as a model which assumes that growth is independent of initial growing stock, a premise which may or may not be valid for a specific fertilizer experiment dataset. For Trial No.1, the assumption is virtually refuted, with the significance of treatment effects dropping from the 0.4% to the 3% level, and the estimated response differing by 24%; the latter discrepancy is equivalent to reporting the volume/ha response as 52.1 m^3/ha, rather than 39.9 m^3/ha. Conversely, in Trial No.7, use of unadjusted growth in an analysis of variance has given results and precision close to those achieved by including a covariate; for these data, the assumption of growth being independent of initial growing stock appears tenable. Clearly, a strategy of utilising growth as a response variable but not undertaking covariance, is risky, and may well lead to unreliable conclusions.

7.3 EMPIRICAL TESTING OF METHODOLOGY: SUMMARY

Examination of some of the data from the N.Z.Forest Products Limited experimental series has given abundant evidence that the suggested procedures for analysing later-age forest fertilizer experiments, detailed in Chapters 4 and 5, frequently give (1) enhanced precision (2) less biased estimates of treatment responses, and (3) fuller interpretation of results. The recommended methodology refines and focusses on elements of established regression and covariance analysis, but offers specific rules for extracting maximum sensitivity in hypothesis testing, so that a better understanding of the behaviour of data available for any nutrition experiment can be achieved.

A review of the previous validation process highlights that multiple covariance provides the researcher with a potent technique for radically reducing experimental error and producing true estimates of treatment responses. For example, in Section (7.2.4) utilisation of two covariates reduces residual error by up to 72%, and the dual adjustment of treatment averages gives estimates of basal area response up to 13% [Table (7.3), Section (7.2.5)] different to those derived by adjusting for initial growing stock alone. The three estimates in Table (7.3) represent substantial differences in nutrient response, and suggest many other fertilizer experiments, may contain seriously biased estimates of nutrient effects, for want of securing appropriate covariates and applying the most pertinent statistical analysis.

Recognition of curvilinearity or disparity in regression coefficients relating response variables and covariates, produces smaller gains in precision, but gives considerable assistance in interpreting trial data and in summarising the information they provide. Isolation of independent or non-linear slopes infers non-proportional growth with increasing initial quanta of growing stock, which may be an artefact of nutrient response being related to plot (and tree) dominance. Disparate regression coefficients are silviculturally logical in thinning x fertilizer experiments, where larger differences in plot stockings can be expected to induce distinct growth rates.

The complete NZFP trial datasets were analysed following the recommended procedures, and a summary of analyses obtained is given in Appendix 6. They vindicate the proposed analytical methodology and the results so obtained have allowed a provisional prescription for operational top-dressing of Company stands to be developed, which form the basis of the next Chapter.

8. AN OPERATIONAL PRESCRIPTION FOR FERTILIZATION OF N.Z.FOREST PRODUCTS LIMITED's KINLEITH FOREST

8.1 OVERVIEW

In making a decision as to whether to fertilize forest holdings, a manager should consider (a) the urgency of procuring additional wood, (b) the economics of top-dressing, (c) the quality and precision of results, and (d) the amount of logistic information available, specifying how to realise maximum fertilizer responses (if obtainable) in various regimes of a particular forest. The first two considerations are well beyond the scope of this study, while the third point is seen as fully analysing each trial following the methodology recommended here.

The need for a manager to be presented with a rationale for top-dressing is paramount, however, and should dictate to a researcher where experimental effort needs to be addressed. Before top-dressing can reasonably commence in a forest, information required would include:

(a) active elements, producing responses;
(b) rates of application;
(c) responsive ages of stands;
(d) interactions with stocking and thinning;
(e) interactions with pruning;
(f) timing of fertilizer applications;
(g) interactions with existing pathogens;
(h) yield prediction considerations (for example changes in tree form, possibly nullifying the efficacy of existing volume equations);
(i) the size and duration of response;
(j) soil and site variation within the forest.

Results from the NZFP experimental series, summarised in Appendix 6, allow a provisional top-dressing prescription to be deduced for Kinleith forests. The ability of some of the company's *Pinus radiata* stands to respond to fertilizer is demonstrated as concluded previously by Woollons and Will (1975), and subsequently confirmed by Mead and Gadgil (1978) and Hunter *et al.* (1985), embracing results from further experiments on similar pumice soils in the Kaingaroa forest region.

8.2 TOP-DRESSING RATIONALE FOR NZFP FORESTS

8.2.1 Active Elements

The consistent responsive element is nitrogen; N is present (alone or as part of a mixed fertilizer) in all eight trials, and a significant response is detectable in all cases. This general observation can be further refined from the results of the factorials, Trials Nos. 2, 5, and 6, where the main effect of nitrogen was significant in all three cases. Trial No.2 showed also an undoubted response to magnesium/potassium, and Trial No.5 gives an indication of a potassium effect, but these latter responses are generally transient, or are quantitatively smaller than nitrogen. Phosphorus completely failed to improve growth throughout the experimental series. These conclusions are not incompatible with those of Hunter *et al.* (1985), who likewise identified nitrogen to be the principal responsive element in Kaingaroa pumice soils, but recognised also limited areas where there could be a response to a mixed fertilizer rather than nitrogen alone. In terms of operational viability, nitrogen remains the only nutrient worth considering in Kinleith forest.

8.2.2 Rates of application

Rates of application are less conclusively isolated; (for comparison, the composite fertilizer applied at Trials Nos. 1, 3 and 4 are considered in relation to their nitrogen concentration alone). It is clear a minimum of 500 kg urea/ha is required to sustain response. In Trials Nos. 3, 7 and 8, a single application of 250 kg urea/ha, gave no or a small response confined largely to one year. Conversely Trial No.1, and other treatments in Trials Nos. 7 and 8, did allow growth to be sustained either through 500 kg urea/ha applied in one dosage, or in two successive applications of 250 kg urea/ha. Higher rates were also attempted in Trials Nos. 4 and 8, but their longer-term effect is similar to the 500 kg urea/ha dressings. Hunter *et al.* (1985) concluded that the shape of the response curve to nitrogen is indecisive, and suggested an optimum response at 200 N/ha (430 kg urea/ha), based on pooled *Pinus radiata* data throughout New Zealand.

8.2.3 Responsive ages

The evidence suggests that fertilizer responses are obtainable from applications at any age between 4 and 20 years. The substantial majority of company experiments were dosed at ages 13-14, but Trial No.6 was treated at age 5, and produced a singificant response. Mead and Gadgil (1978) and Hunter *et al.* (1985) cite comparable trials fertilized at ages 4, 6, 7, 9, 11, 15, 16 and 20, all of which produced some response.

8.2.4 Thinning requirements

There is clear evidence that thinning status controls response; thinning (planned removal of trees) is a prerequisite for fertilizer gain in Kinleith stands. Woollons and Will (1975) reported that trials in unthinned regeneration of 1100, 740 and 440 stems/ha dosed with up to 750 kg urea/ha failed to give any significant response. Madgwick (1977) concluded that thinning is a major perturbation in the nutrient cycle within a stand, and that residual trees should be responsive to fertilizers, while Hunter *et al.* (1985) confirm that while very young unthinned stands closing canopy may respond to nitrogen, older trees require a recent thinning in order to respond.

8.2.5 Interactions with pruning

Interactions with pruning have not been studied in the series of trials. However, Hunter *et al.* (1985) hypothesised that pruning has a similar effect to thinning in young plantations with respect to fertilizer response. Reanalysis of the experiment of Hunter *et al.* (1986) presented in Chapter 3, supports this proposition, albeit that the results are strictly applicable only to North Auckland sites. Considered in relation to NZFP's current silviculture, which largely avoids early and heavy blanket pruning, risks of negative interactions in pruned stands are small.

8.2.6 Timing of fertilizer applications

Timing of fertilizer rates relative to thinning is an important consideration. There is some evidence that fertilizer response is related to growth rate; Figure (8.1) depicts basal area responses regressed against non-fertilized current annual increments for one and two years after thinning, for the eight experiments. The analysis is provisional; site and climatic factors are obvious confounding variables, but the relationship suggests that potential response is related to increment. If it is accepted that recently thinned stands experience a period of relative dormancy, as suggested for example by Evans (1982) and and Harrington and Reukema (1983), then fertilization immediately after thinning may well not be optimal. Trial No.4 was treated with up to 750 kg urea/ha in the first year following thinning and completely failed to respond. Trials Nos. 7 and 8 exhibited small, non-significant responses in the first year, but Trial No.2, not fertilized until 16 months following thinning, produced a large gain (0.6 m^2/ha).

Similarly, Trial No.1 produced 0.4 and 0.6 m^2/ha in the first year, and 0.7 and 1.4 m^2/ha in the second. While these results are partially confounded by first year residual nutrient effects, the trend is quite marked.

A result from Trial No.8 suggests that third year responses are quite feasible where a dosage of 500 kg urea/ha, added to an earlier (1975) dressing of 250 kg urea/ha, consequently gave an estimated 2.5 m^2/ha over 1975-1986. Combining these results with the conclusions concerning rates of application summarised above, then it follows that consecutive dosages of 250 kg urea/ha, applied two and three years after thinning, or a single application of 500 kg urea/ha two years after thinning, are best applications (derived to date) for maximum response in the company's stands. This combination was used in Trial No.4 (ignoring non-responsive first applications), and gave a highly significant response in the following year. It remains true, however, that sustained responses to thinning have been achieved through earlier dosages, so recommendations cannot be definitive.

There is clearly a case for further experimentation with rates of 500 kg urea/ha, either in split dressings or at one time, applied over the first three years following thinning. This could be achieved by six treatments, as shown in Table (8.1).

Table (8.1): Recommended fertilizer experiment to provide further information on rates and timings of fertilizer

	Year following thinning		
Treatment	1	2	3
1	no fertilizer		
2	500	-	-
3	-	500	-
4	-	-	500
5	250	250	-
6	-	250	250

(the above numbers denote kg urea/ha)

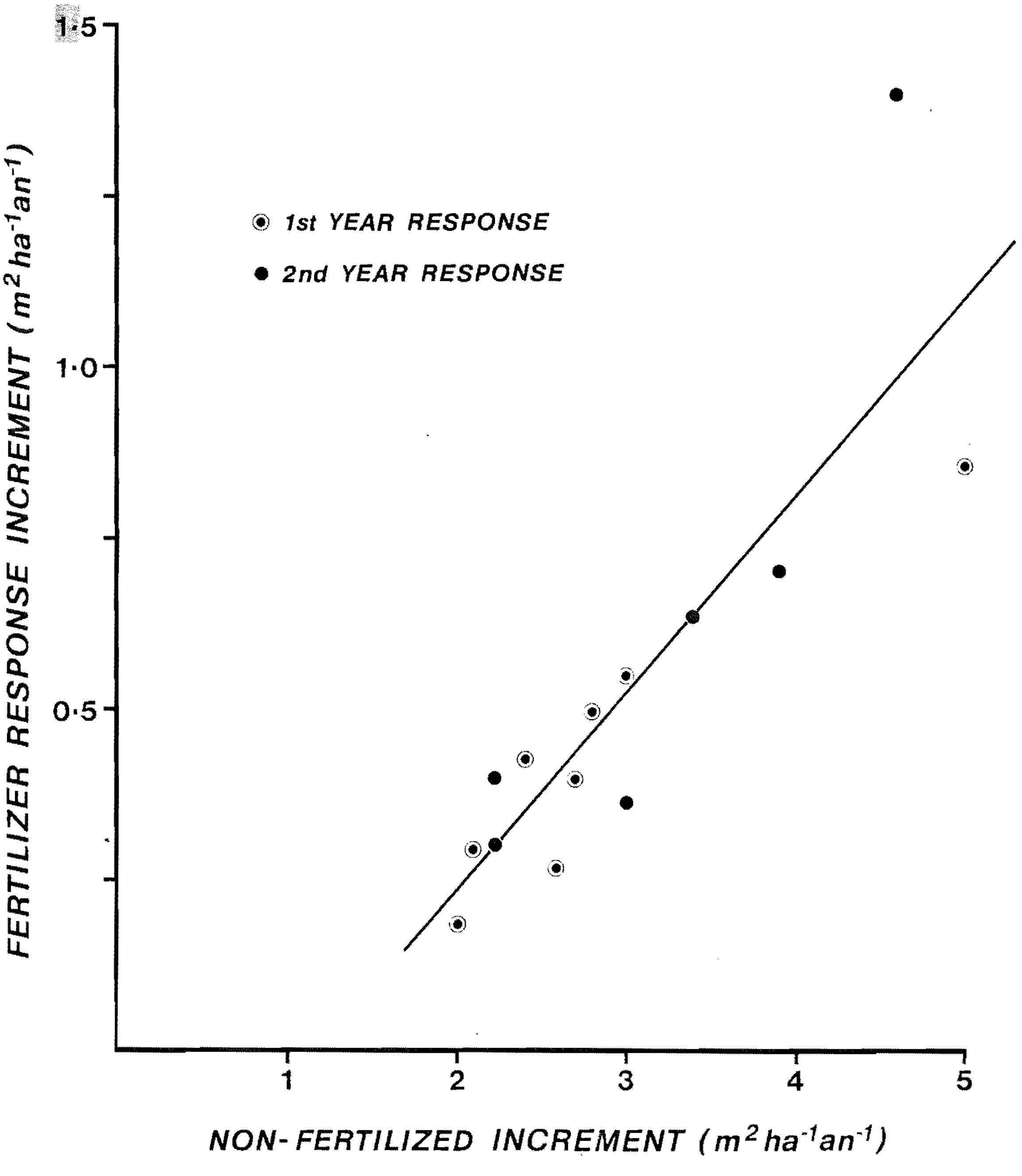


Figure (8.1) Relationship between basal area response, and unfertilized increment, in NZFP trials

If it transpires that these specific applications are overshadowed by considerations of site and climate, and give equivalent responses, then a flexible prescription for timing of applications can be tolerated, which operationally would be advantageous.

8.2.7 Interactions with *Dothistroma pini*

Experiments Nos. 1 to 6 were generally free of the fungal pathogen *Dothistroma pini*, Gilmour (1967 [a]), but Nos. 7 and 8 were infected, causing severe suppression of between 60 and 100 stems/ha, and probably creating additional variation in these trials. Figure (8.2) plots diameter breast height, 1986 and 1975, for all (live) experimental trees in Trial No.8, distinguishing between affected and healthy stems. Fertilizer response lost through infection is impossible to estimate exactly with available data, but if assumed to be proportional to stem mortality and suppression, it represents a loss approaching 20%. As such, heavily infected stands should not be fertilized, at least not without accompanying spray programmes. To provide more data the *Dothistroma pini* experiment described by Woollons and Hayward (1984) was recently modified to examine the effect of fertilizer on trees receiving annual sprays of copper fungicide or not.

8.2.8 Changes in stand form-factor

A detailed study of changes in stand form-factor is largely beyond this study, except to note the absolute size and duration of such changes. Fertilizer altered stand form-factor when a total of 500 kg urea/ha was applied; the degree of form-change and its occurrence are summarised in Table (8.2).

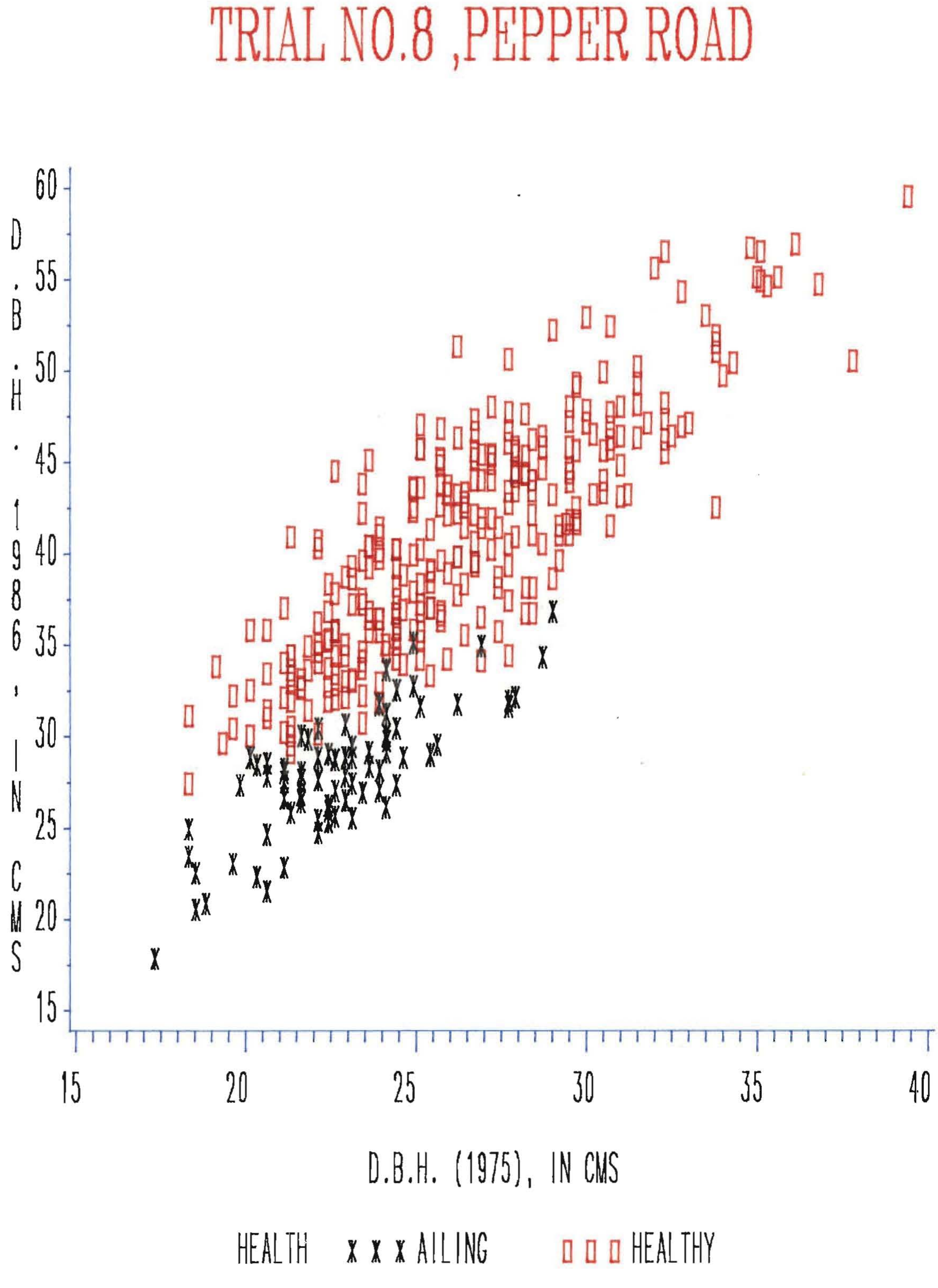


Figure (8.2) Relationship between (d.b.h.) 1975 and 1986, for healthy and suppressed trees, in Trial No.8.

Table (8.2) Form factor changes in the Trial series.

Trial No.	Maximum change in form-factor *	Duration (in years) of significant difference
1	0.008	4
2	0.008	2
3	-	-
4	0.009	3
5	-	-
6	-	-
7	-	-
8	0.014	2

* caused by, or assumed to be, through nitrogen application(s)

Thus, changes in stand form-factor induced by fertilizer application range between 0.008 to 0.014, (representing approximately changes of 2-3%), but they last (within the confines of available data) no more than four years in terms of statistical significance. Where differences become non significant, the average differences remain, but do not become greater. Estimates of the changes in form-factor, as they affect volume/ha, are developed in Chapter 9.

8.2.9 Size and duration of response to fertilizer

Results from Trial No.1 indicate that fertilizer gain is related to initial growing stock; for example, 357 stems/ha (18.5 m^2/ha) gave 3.9 m^2/ha over nine years, while 571 stems/ha (25.2 m^2/ha) gave 6.2 m^2/ha. Extension of this principle to other experiments is hazardous, however, because response is also a resultant of site, climate, fertilizer rates, and presence of pathogens. For example, Trial No.7 was established at 494 stems/ha, but a proportion of generally smaller stems became infected with *Dothistroma pini*, essentially reducing the merchantable stocking to 432 stems/ha. Despite this, the net initial basal area was 29.2 m^2/ha, but response after six years is estimated at only 0.9 m^2/ha. Mead and Gadgil (1978) discuss an 11-year-old Kaingaroa experiment where stockings of 620 and 370 stems/ha gave volume responses of 3.6 and 4.8 m^3/ha/annum, so response and initial growing stock are not consistently related.

Table (8.3) summarises estimated basal area responses achieved in the eight trials with respect to total nitrogen applied, ignoring the presence of other nutrients and excluding responses to lesser treatments. The last column denotes the elapsed years over which the responses are measured.

Table 8.3 Estimated responses in basal area/ha achieved in N.Z.Forest Products Limited trial series

Trial No.	Initial (net) stocking (stems/ha)	Total urea applied (kg/ha)	Response (m^2/ha)	90% confidence interval	Duration of response (years)
1	571	500	6.2	± 2.7	9
1	357	500	5.6	± 5.2	19
2	478	500	0.7	± 0.5	2
3	463	250	1.5	± 1.3	6
4	432	500 *	1.9	± 0.6	3
5	592	250	0.3	± 0.2	1
6	1192 **	250	0.9	± 0.3	1
7	437	500	0.9+	-	6
8	398	500	3.8	± 1.5	11

* = 1971 applications ignored; ** = age 5; + = non-significant

8.3 AN OPERATIONAL PRESCRIPTION: SUMMARY

From the above discussion, an operational prescription can be formulated for fertilization of NZFP Kinleith forests.

1. Limit top-dressing to thinned stands not appreciably infected with *Dothistroma pini*, or, if the stand is considered a high-hazard area, first schedule the area for fungicide spraying.

2. Fertilizer to be applied as urea, at a total rate of 500 kg urea/ha, either applied in two split dressings one and two years after thinning, or in one dosage one year after thinning.

3. Fertilizer can be applied to any thinned stand within the age-range 5 to 20 years.

4. Fertilizer can be applied to pruned stands, provided that NZFP pruning prescriptions are followed.

It will be noted from Table (8.3) that several estimates of basal area response are given, achieved over 1-19 years. From section (8.2.9) it cannot be assumed necessarily that all the responses will equate to 6.2 m^2/ha, obtained from Trial No.1. Moreover, even if this were so, a working figure of 6.2 m^2/ha for long-term fertilizer gain *per se*, is inadequate and of limited value for management planning. These operational problems are examined in detail, in the next Chapter.

9. TRANSLATION OF EXPERIMENTAL RESULTS INTO MANAGEMENT REQUIREMENTS

9.1 INTRODUCTION

A series of experiments such as that discussed in Chapters 7 and 8, and detailed in Appendices 5 and 6, may allow researchers to draw useful conclusions regarding growth responses obtainable by fertilization in various stands, and perhaps to arrive at an interim prescription for operational top-dressing. Clearly the information gleaned from the NZFP set of experiments could not have been acquired in one or two trials, and even with eight, the total nature of an optimal response is not known exactly. While the evidence is comprehensive enough to form a provisional top-dressing rationale, sufficient to commence operational fertilization, both manager and researcher are left with several gaps in their technical knowledge.

The process of estimating the duration and magnitude of fertilizer response in several experiments introduces an additional component of variation; responses will vary considerably from site to site and from year to year, as discussed by Whyte *et al.* (1978), Woollons (1980) and Woollons and Snowdon (1981). This phenomenon is well recognised in agriculture as shown in Yates and Cochran (1938), and Cochran and Cox (1966), but probably the forest researcher has to contend with even more confounding efforts. For example, over the ten years that a scientist observes the effect of a fertilizer slowly compound in an experiment, stands in a forest may become infected with a needle cast fungus, or plagued by insects, or affected by droughts. Alternatively, thinning regimes (represented in the trial) may be abandoned or modified. Seed sources may be upgraded rendering the original stock an outmoded variety.

Several statistical procedures have been developed for the analysis of groups of experiments [Yates and Cochran (1938), Cochran and Cox (1966), John and Quenouille (1977)], but most of these techniques assume a chain of experiments where the treatments and type of design are held largely constant. This is rarely achieved in forest research, with time and resources imposing considerable constraints. In these circumstances, the scientist must rely on: (1) thorough analysis of each trial component and evaluation of the series by summary and deduction - as assayed for the NZFP data, (2) coordination in the experimental series, ensuring the successful treatments of the first trial are included in the second, and so on, thus making sure the worthwhile responsive factors are represented throughout.

The form of the results in Chapter 8 are largely inadequate for planning on and choosing fertilization as an operational option. Consider the responses cited in Table (8.3); nine point (in time) estimates of fertilizer gain (to various rates) are given in terms of one responsive variable, basal area. Moreover, each estimate varies appreciably with respect to its statistical significance and width of confidence-interval. The question immediately arises - what response should be taken as the norm? If the largest (measured) estimate is taken, this virtually assumes the others will ultimately achieve the same order of response, which is clearly speculative. What is required is a means of summarising and integrating all the information into one system, comprising a number of predictive formulae which are flexible enough to cater for <u>varying</u> initial conditions.

Even then, such a system could be limited in value. If output was expressed in basal area/ha, a forest planner would have considerable difficulty in translating results into a workable format. Forest yields are required to be expressed most commonly as <u>volume</u> of realisable wood. Thus, not only do the basal area estimates need to be converted into volumetric units, but suitable conversions need to be applied, recognising that operational fertilizer gain will be appreciably less than experimental responses. For example, allowances are required for:

(a) breakage in felling;

(b) inefficient harvesting;

(c) net forest area deductions for unstocked or unthinned sub-areas within stands;

(d) losses of fertilizer during or after application;

all of which will lead to lower actual production gains than were obtained through experimentation. Derivation of these loss-factors is not pursued here in detail; each component represents a comprehensive, on-going study or analysis. However, an attempt is made to equate the responses derived for the NZFP data into a single predictive system.

9.1.1 Forecasting crop yields

In recent years, forest estate modelling has become a vital tool in New Zealand forest management. Two simulation systems in general use for long term planning are RMS80/RMS85, Allison *et al.* (1979), Allison (1980,1985), and IFS, Garcia (1981). These models can allow an evaluation of fertilization as it affects crop production and future wood supply, and to explore its longer term response on forest capacity and outturn. Input to these systems is essentially through yield tables; fertilizer response is required to be expressed as realisable volume for a given age, but also as a continuum from immaturity to clearfell ages and beyond. For the responses of Table (8.3) to be translated into a yield-table format therefore, a fertilizer growth and yield model is required to generate relevant yield tables as input into the RMS80/85 or IFS systems.

9.2 A FERTILIZER GROWTH AND YIELD MODEL

9.2.1 Introduction

The NZFP fertilizer trial data are just sufficient to allow the construction of a growth and yield simulator to summarise established responses and generate yield tables, thus giving estimates of fertilizer yields at clearfell ages. Available data for modelling are unavoidably limited; for example, only Trial No.1 has data at ages 29-30, while Trial No.8 extends only to age 24. The relative paucity of information implies that any yield system should be kept simple in its objective and modelling capacity. It has been decided, therefore, to:

(a) model only stand (per hectare) statistics and not attempt stand-table (diameter distribution) projections;

(b) restrict simulation of fertilization to one rate and application; "sustained response" or "not". This also reflects that the trial series does not isolate specific levels of nitrogen for optimum response beyond minimum dosages.

However, it was desirable to include plot competition in the modelling dataset. The modified Hegyi index increased precision of several analyses in the trial series appreciably, but no attempt was then made to quantify its effect on growth. Inclusion of plot competition reduces the modelling dataset to that available from Trials Nos. 1, 2, 3, and 8. Care was taken to exclude non-representative fertilized data, specifically:

(a) Treatment E, from Trial No.3, as only 250 kg/urea were applied;

(b) Treatments Mg/K and NMg/K, or combinations thereof from Trial No.2, in that they represent atypical responses;

(c) Treatment C, from Trial No.8, since a response did not emerge until the third year after thinning, through a total of 750 kg urea/ha.

9.2.2 Model rationale and construction

A detailed discussion of forest growth model strategy is beyond the scope of this dissertation. It suffices here to say that the principles of Clutter (1963), and Clutter *et al.* (1983) were adopted, in which emphasis is placed on consistency, invariance, and compatibility for sound derivation of growth and yield. Consider therefore, the yield equation

$$\ln(Y) = \alpha + \beta/T \qquad (9.1)$$

where, in (9.1)

Y = a response variable (volume, height, or basal area)
T = stand age
α, β = estimated parameters

then, (9.1) may be differentiated with respect to T, to give the growth equation

$$dY/dT = Y/T\ (\alpha - \ln[Y]) \qquad (9.2)$$

or

$$dY/(Y[\alpha - \ln\{Y\}]) = dT/T \qquad (9.3)$$

The simultaneous integration of (9.3) with respect to Y and T with limits of integration (Y_2, Y_1) and (T_2, T_1) produces the corresponding difference equation

$$\ln(Y_2) = \ln(Y_1)\ (T_1/T_2) + \alpha(1-[T_1/T_2]) \tag{9.4}$$

which allows the projection of Y_1, growing stock at age T_1, to an estimated Y_2 units at age T_2, under the assumption of the parent yield equation (9.1). Such a system has several advantages [Clutter *et al.* (1983)].

1. Growth at any age is given directly by (9.2) and has the invaluable property that aggregation of growth over a period will equate exactly to the corresponding yield estimate;

2. When $T_1 = T_2$, in (9.4), then $Y_1 = Y_2$ (any other result is inconsistent);

3. When $T \to \infty$ in (9.1), then $Y \to \alpha$; thus there is an upper (asymptotic) limit to yield:

4. A projection from T_1 to T_3 is identical to a projection from T_1 to T_2, and then T_2 to T_3. This invariance property achieves consistency in estimation, and avoids iterative calculations in computer simulation.

   This system was adopted by Woollons and Hayward (1985) in a revision of the NZFP growth and yield model, and forms the basis of the fertilizer system developed here [see Appendix 7].

9.2.3 Modelling net basal area/ha

Basal area is a key equation in any growth model framework, [Clutter

(1963)], and largely governs the accuracy of any specific simulator. The difference equation given by Clutter *et al.* (1983)

$$\ln(G_2) = (T_1/T_2)^{\beta} \ln(G_1) + \alpha(1-[T_1/T_2]^{\beta}) + \gamma S\ (1-[T_1/T_2]^{\beta}) \qquad (9.5)$$

where in (9.5)

G_2, G_1 = net basal area/ha at ages T_2 and T_1

S = site index

α, β, γ = estimated parameters

was modified and extended to

$$\ln(G_2/N_2) = (T_1/T_2)^{\beta} \ln(G_1/N_1) + \alpha(1-[T_1/T_2]^{\beta}) + \gamma F(1-[T_1/T_2]^{\beta}) + \delta C(1-[T_1/T_2]^{\beta}) + \varepsilon \qquad (9.6)$$

where in (9.6)

(G_2/N_2), (G_1/N_1) = net basal area/tree at ages T_2, T_1

F = effect of fertilizer

(0 = control, 1 = fertilizer)

C = stand competition

α, β, γ, δ = estimated parameters

ε = an error term, assumed $\varepsilon \sim NID(o,\sigma^2)$

Model (9.6) has to be estimated by non-linear least squares, [Bard (1974)]; the SAS non-linear procedure (NLIN) was used to estimate the parameters in (9.6) which gives an ANOVA:

Source	d.f.	SS	MS
Regression	4	2020.61	505.15
Residual	323	0.22	
	327		

Estimated parameters, and 95% asymptotic confidence intervals are:

$\hat{\beta}$ = 0.950 245 8 (0.84 to 1.05)

$\hat{\alpha}$ = -0.649 802 5 (-0.86 to -0.44)

$\hat{\gamma}$ = 0.205 685 6 (0.14 to 0.27)

$\hat{\delta}$ = -0.992 960 0 (-1.14 to -0.84)

No formal tests of significance for the parameters were attempted, since the residual mean-square is certain to be underestimated by because of the age-correlated data [see West (1981)]. We can observe, however, that all parameter estimates cannot be considered to be zero, as judged by the asymptotic confidence intervals, and that the signs of the four coefficients behave logically; the negative asymptotic coefficient (α) reflects that the response variable is less than zero. Figures (9.1) to (9.4) depict graphically trends in the residuals of the data against predicted values, age, basal area, and competition, respectively. Apart from detecting some sign of over prediction at age 13 with age, the scattergrams are generally shown to be random, thus supporting the robustness of (9.6).

9.2.4 Stand top-height

No height response to fertilizer was detected in any trial, so the top-height function

$$E[\ln(H)] = 4.1931 - 13.8217/T \qquad (9.7)$$

FERTILIZER GROWTH MODEL

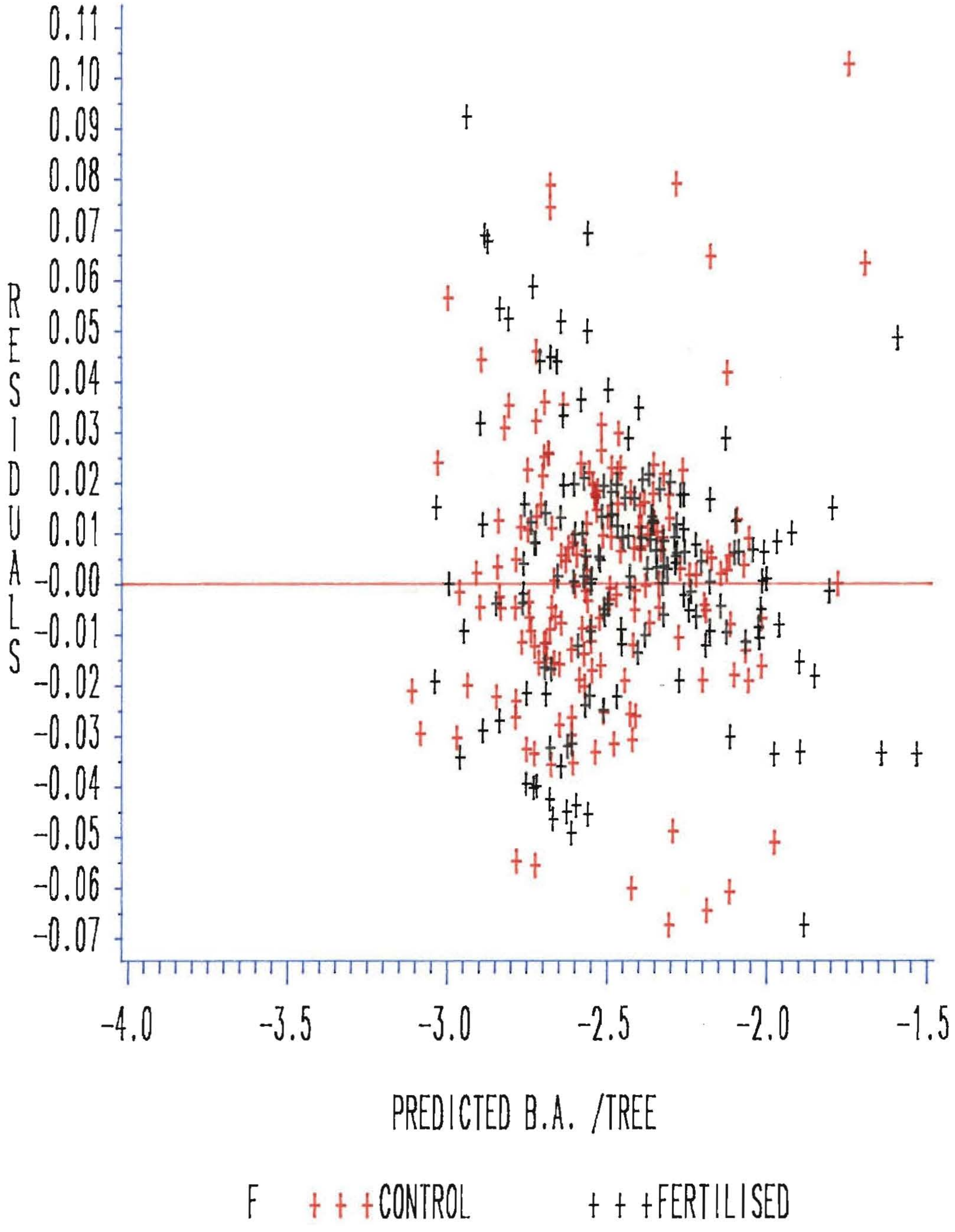


Figure (9.1) Residual and Predicted values of basal area function for fertilizer growth model.

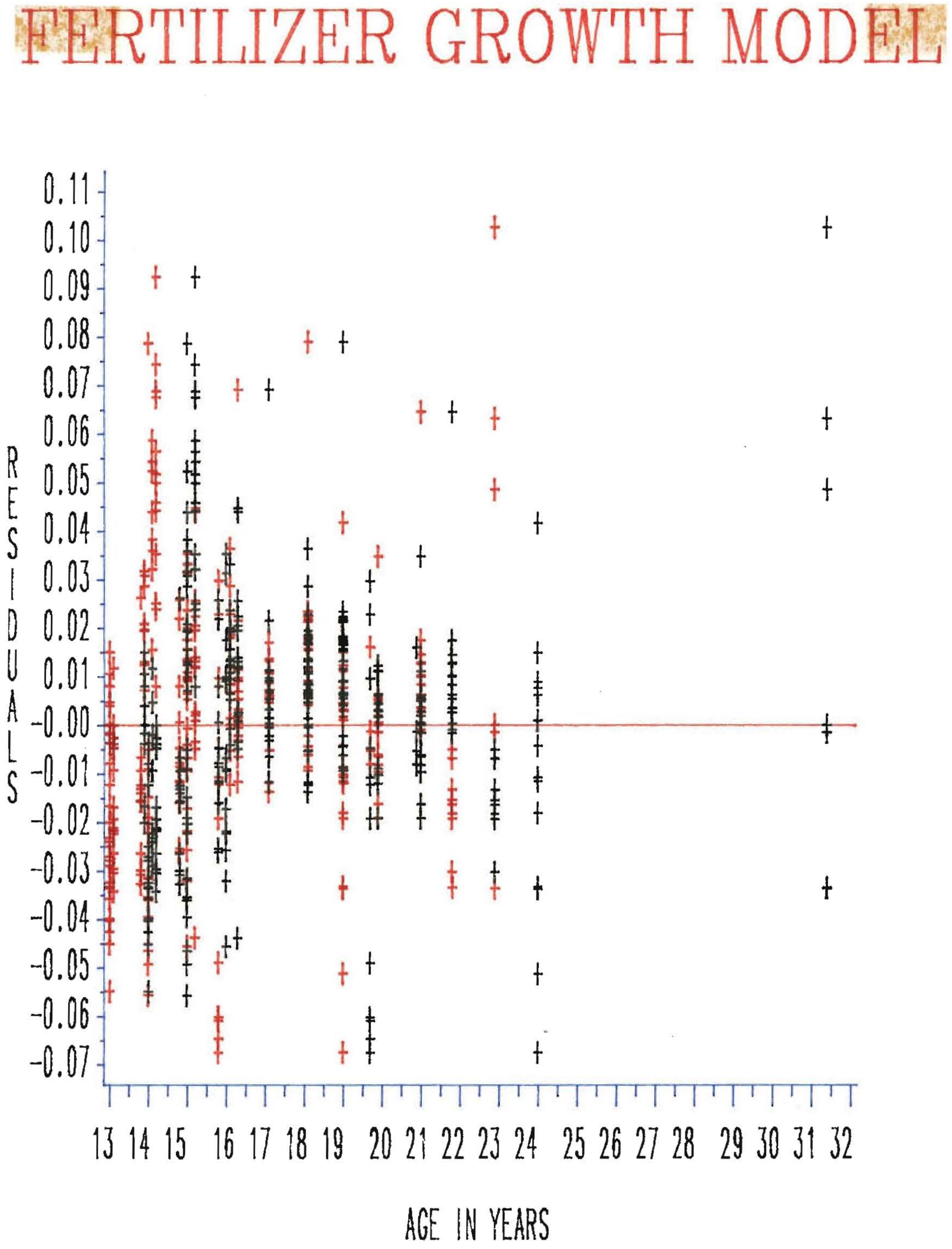


Figure (9.2) Residual and age values for basal area function.

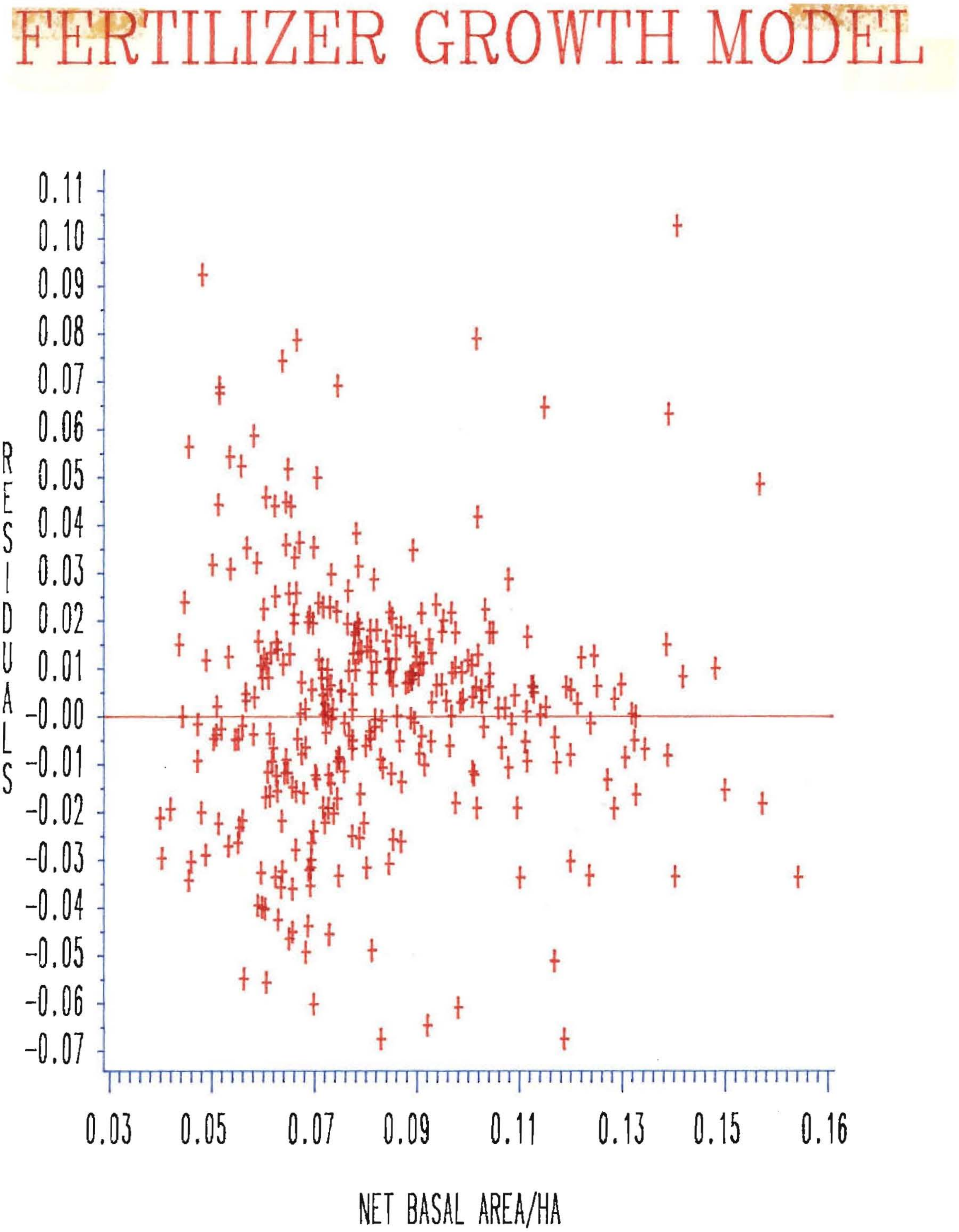


Figure (9.3) Residual and basal area values for basal area function.

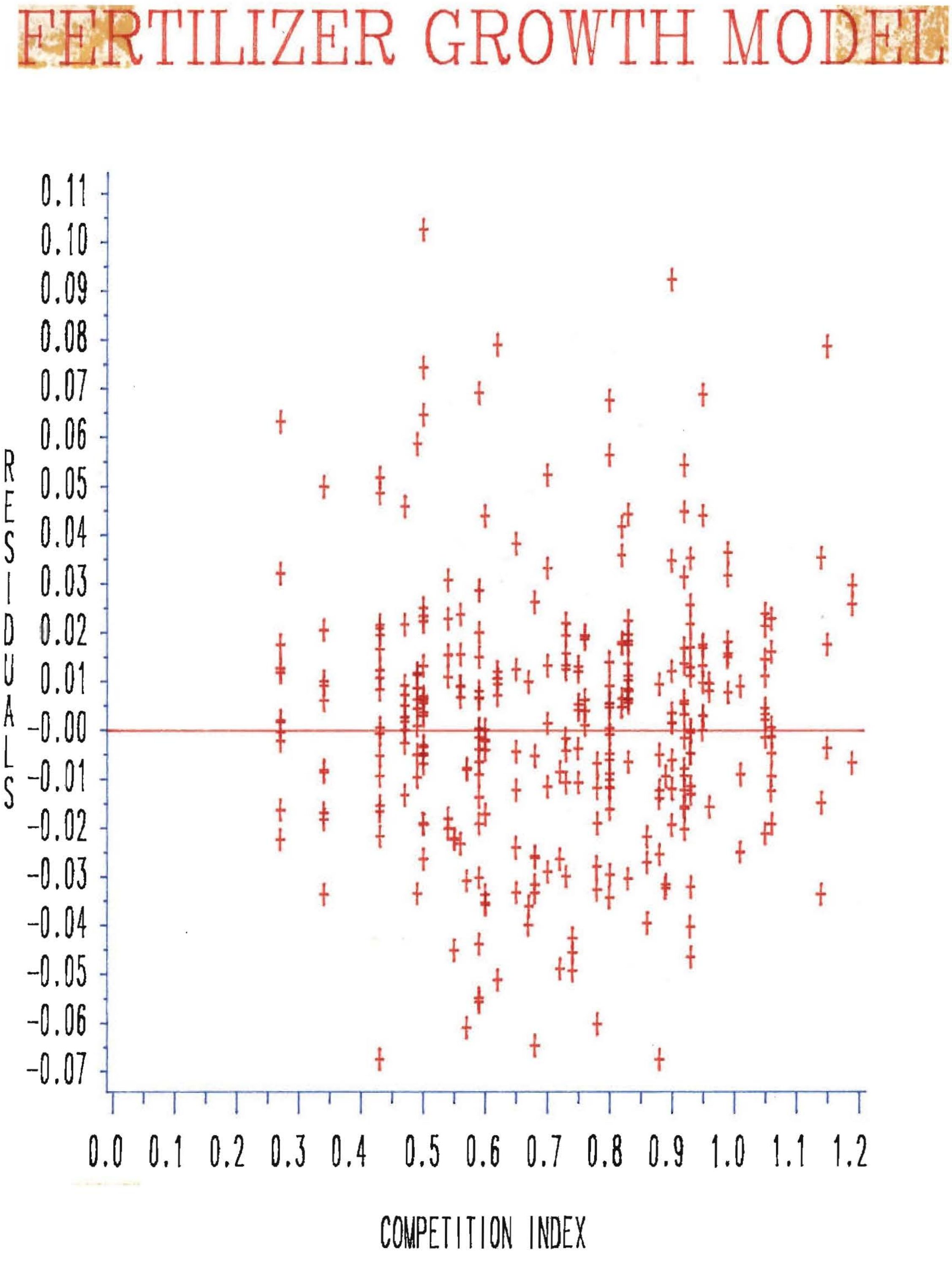


Figure (9.4) Residual and competition values for basal area function.

where in (9.7)

H = stand mean top-height

of Woollons and Hayward (1985) was adopted. (In fact data from the trial series were utilised in part construction of model (9.7) .)

9.2.5 Stand Mortality

Since model (9.6) uses net basal area/tree as the response variable, a mortality function is required to generate unit area statistics. No extraordinary mortality (due to fertilization) was detected in the trial series, so the function of Woollons and Hayward (1985)

$$E[N_2] = 200 + ([N_1 - 200]^{\beta} + \alpha[T_2^{\gamma} - T_1^{\gamma}])^{(1/\beta)} \tag{9.8}$$

where in (9.8)

N_2, N_1 = living stems at ages T_2 and T_1

α, β, γ = estimated parameters

was considered as a candidate equation. Analysis showed, however, that model (9.8) over-predicts death in relation to the observed mortality in the four experiments. Such discrepancy should not imply (9.8) is a suspect model, but simply the very small (thinned) subset of data incurs a different pattern of mortality. In passing, it is pertinent to repeat the comments of Glover and Hool (1979):

> Mortality (however) is one of the most difficult and least reliable aspects of evaluating tree growth. It is fair to state that mortality probably will never be perfectly predicted because of forest ecosystem complexity and uncertainty of future conditions.

The authors' comments pertain largely to unthinned stands where inter-tree competition produces a substantial component of mortality; for thinned stands, prediction is probably even more intractable. Competition *per se* is relatively unimportant, but death can also be caused by thinning extraction damage, and later by sporadic physical effects. For these data, mortality was generally very light (ignoring death through *Dothistroma pini*). Trial No.1 lost 20 and 50 stems/ha over 19 years in the original 370 and 620 stems/ha stockings. For the latter, nearly 30 occurred in the first year and could be attributed to extraction damage. Modelling of these trends was not pursued to any extent (the presence of many plots with zero mortality further inhibited analysis). A model

$$N_2 = N_1 - (T_2 - T_1) \qquad (9.9)$$

was adopted for thinned, fertilized stands which uniformly decreases live stems annually by unity. While undeniably simplistic, it summarises the overall mortality in the four experiments adequately.

9.2.6 Net volume/ha

Woollons and Hayward (1985) derived volume/ha implicitly, through the function

$$E[V/GH] = 0.323 \exp(3.217\,88/T^{1.578\,18}) \qquad (9.10)$$

where in (9.10)

V = volume/ha and G = basal area/ha

(N.B. minor printing errors in the numerical coefficients appear in Woollons and Hayward (1985); the second parameter is given as -3.2109, and the third as 1.5767.)

Model (9.10) could be adopted in the fertilizer system, but it does not recognise any of the changes in stand form factor through fertilization, that were summarised in Chapter (8.2.8). Stand form-factor was changed by fertilizer when a total of 500 kg urea/ha was applied (as modelled here), albeit the duration and size of significant changes fluctuated considerably; moreover in absolute terms, the amount of form modification was small. Nevertheless, and ignoring that significance between treatments was lost after 3 to 4 years, for exploratory purposes, model (9.10) was augmented to

$$E[V/GH] = 0.323 \exp (3.217\,88/T^{1.578\,18}) + 0.008 \qquad (9.11)$$

to provide estimates of fertilized volume/ha.

9.2.7 A fertilizer growth and yield system

Equations (9.6, 9.7, 9.9, 9.10 and 9.11) are sufficient to assemble a growth and yield model simulating fertilization of NZFP's *Pinus radiata* stands. To facilitate the use of competition index, a conversion to initial stocking was constructed. In theory, a wide range of competition values could be associated with a particular stand density, but the actual spatial distribution of trees would be strictly uniform or clustered in an inordinate manner. Competition and stocking are linearly related, so a regression was constructed between these variables (see Figure (9.5) and Appendix 8), and the least-squares fit defined as <u>normal competition</u>. Parallel lines ± 0.1 above and below the derived slope are defined as <u>clumped</u> and <u>open</u> competition, respectively. A computer programme (see Appendix 9) was written in FORTRAN to calculate the component equations and to output relevant yield information.

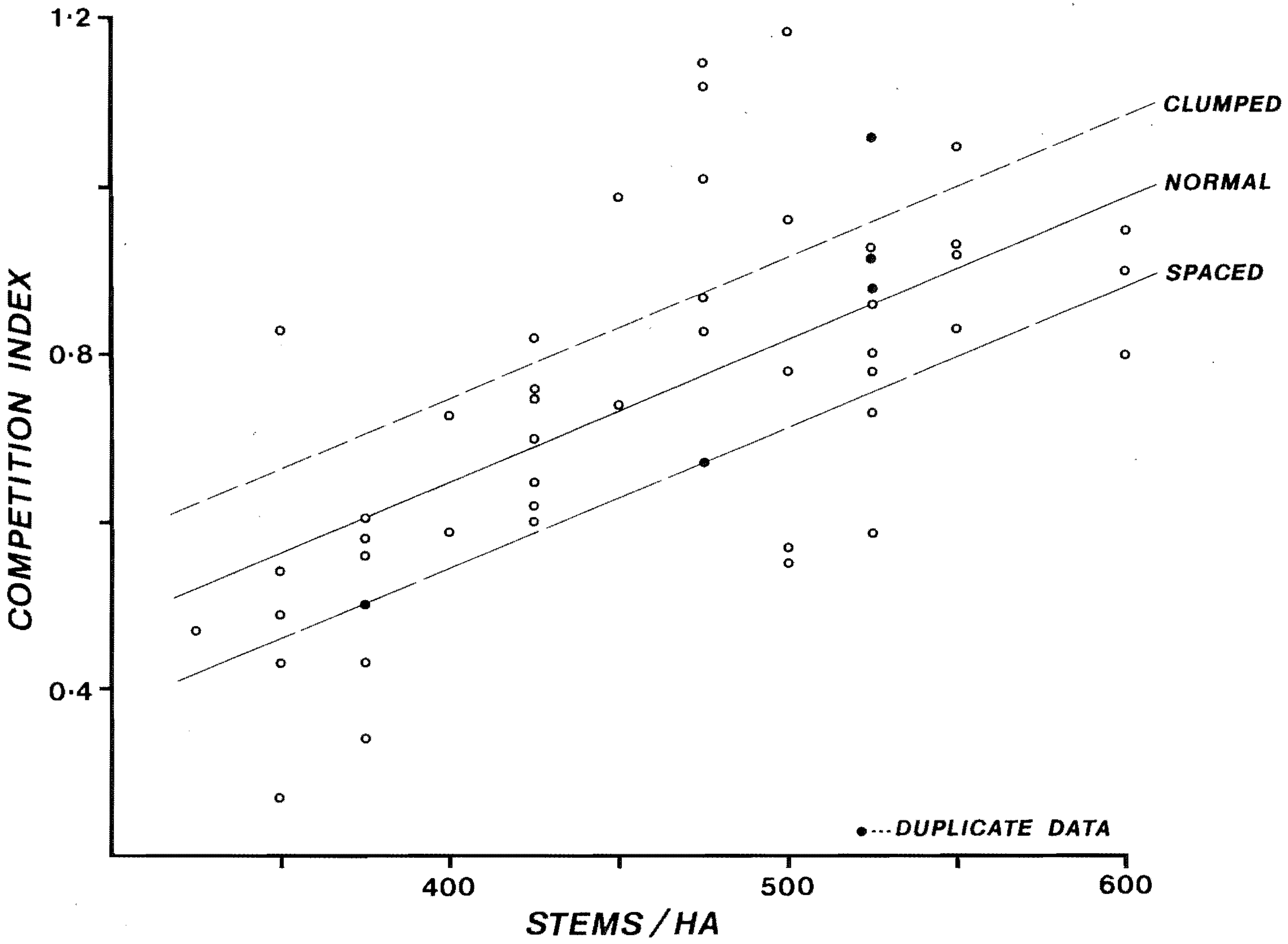


Figure (9.5) Relationship between stand competition index and stocking.

Input data required are:

1. starting age (NN);
2. initial basal area/ha (NN.N);
3. initial stems/ha (NNN)*;
4. clearfell age (MM);
5. fertilizer regime (0 = no, 1 = yes);
6. competition status (1 = open, 2 = normal, 3 = clumped);

and output is of form:

Age years	Basal area/ha (m^2/ha)	Stems/ha	Volume/ha (m^3/ha)	Top height (m)
T_1	G_1	N_1	V_1	H_1
T_2	G_2	N_2	V_2	H_2
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
T_f	G_f	N_f	V_f	H_f

where f = clearfall age

It is self-evident that the simulator must be regarded as exploratory, and used only within strict silvicultural bounds. There is no information available for validation, and available data at later ages are limited. However, the statistical evidence for the accuracy of the majority of component equations is quite strong, and provided that the simulator is used sensibly within reasonable limits of the series data, it should provide valid estimates of experimental

---

* This restriction was imposed in light of available data. Modelled stockings ranged no more than 450 ± 150 stems/ha. Moreover, unthinned regimes are known not to respond.

clearfell yield through fertilization. Suggested limits of usage are:

1. age of fertilization; 11-15;
2. maximum clearfell age; 30;
3. range of input stocking; 300-600 stems/ha;
4. range of input basal area; 15-30 m^2/ha.

(N.B. All simulations assume a thinning has been carried out before fertilization; fertilizer is equivalent to a total dosage of 500 kg urea/ha, applied 0.1 - 2.0 years following thinning.)

9.2.8 Some simulation output

A detailed examination of the system's predictions is (a) largely beyond the scope of this study, and (b) probably unwarranted in light of the limitations of the simulator. Six simulations are presented: age 11, 370 stems/ha and 18.0 m^2/ha of basal area, with clearfell at age 30. Runs assayed were (1) fertilized or not fertilized, (2) using clumped, normal, and open competition. Table (9.1) gives detailed output for normal competition, and summarises results for the other four regimes.

Thus, experimentally, fertilizer responses are estimated to lie between 107 and 121 m^3/ha, depending on the degree of competition which itself can affect yield by $\pm$ 42 m^3/ha. This latter estimate, however, is applicable only to natural regeneration where extreme conditions of spatial competition apply.

9.2.9 Form change, and its effect on predicted volume/ha

The above results include an estimate of the influence of changes in stand form factor on clearfell volume/ha, if the former effect is

Table (9.1) Simulator output from NZFP fertilizer growth model: normal competition

Age (Year)	Stems/ha	Height (m)	Basal area (control) m^2/ha	Basal area (fertilized) m^2/ha	Volume (control) m^3/ha	Volume (fertilized) m^3/ha
11	370	18.9	18.0	18.0	118	118
12	369	20.9	20.7	21.0	149	155
13	368	22.9	23.3	24.0	182	192
14	367	24.7	25.7	26.8	216	230
15	366	26.4	28.1	29.6	250	270
16	365	27.9	30.3	32.2	284	310
17	364	29.4	32.4	34.7	319	350
18	363	30.7	34.4	37.1	353	390
19	362	32.0	36.3	39.4	387	430
20	361	33.2	38.0	41.6	420	470
21	360	34.3	39.7	43.6	452	508
22	359	35.3	41.3	45.6	483	546
23	358	36.3	42.8	47.4	513	583
24	357	37.2	44.2	49.2	543	619
25	356	38.1	45.5	50.9	571	654
26	355	38.9	46.8	52.4	599	688
27	354	39.7	47.9	53.9	626	721
28	353	40.4	49.0	55.4	651	753
29	352	41.1	50.1	56.7	676	784
30	351	41.8	51.1	58.0	700	814

Similar models for clumped and open competition give estimates at age 30 of (m^3/ha):

	Spacing Clumped	Normal	Open
Fertilized	766	814	865
Non-fertilized	659	700	744
Difference	107	114	121

carried through until maturity. Without adjustment for form, the estimated fertilized yield at age 30 is 795 (cf. 814 m^3/ha). Thus, irrespective of the validity of the proposition that fertilizer-induced form-changes are maintained until clearfelling, Gordon & Graham (1986), their effect on predicted volume/ha (for this dataset) is negligible (2%).

9.3 OPERATIONAL REALISATIONS

Responses in the order of 110 m^3/ha, clearly represent gross over-estimates of additional yield that could be expected from operational top-dressing. As outlined in (9.1) yield tables such as those given in Table (9.1) require substantial adjustment before they could be utilized in a planning model such as RMS85. Adjustment factors for breakage, logging waste, and net area have been derived for NZFP croptypes, of the order 0.7 to 0.8 (N.Z.Forest Products Limited data). However, in relation to fertilization, net area would need to be further reduced to recognise unthinned sub-areas within otherwise thinned stands which would not respond to nutrient application. For illustrative purposes, a factor 0.65 will be assumed here to represent net (fertilizable) area and logging recovery.

Operational top-dressing would likely incur (a) unevenness of spread, (b) possible volatilisation of urea, and (c) some inaccuracy in aerial application, for example, ends of flight-runs not being fertilized. Realisations for these criteria are not easy to formulate: Mead (1977) suggested losses through volatilisation could reach 30% in warm, dry weather, but Kirkland (1977) reported that urea volatilisation was not generally seen as a major problem in New Zealand, with rain likely in many areas after short, dry spells. If an estimate of 0.7 is surmised to adjust for the deleterious results of top-dressing, then a reliable lower limit for fertilization of age 11 thinned *Pinus radiata* would be a realized yield of 50 m^3/ha. This figure is disputable, but probably confirms that experimental results and actual realisations are widely divergent.

In addition to supplying yield tables of fertilized regimes, use in planning models also requires the availability of croptype data, a statement of forest areas by years, suitable for operational top-dressing. Here, consideration should be given to relevant age-classes, topography of stands, and areas badly affected by *Dothistroma pini* where adequate responses to fertilizer cannot be expected.

9.4 FERTILIZER EXPERIMENTS AND MANAGEMENT REQUIREMENTS: SUMMARY

1. A single fertilizer experiment, however well planned and analysed, cannot provide sufficient information to formulate appropriate top-dressing prescriptions and give realistic estimates of clearfall response at clearfelling.

2. A series of fertilizer trials, necessarily established over time and on different sites, can largely overcome the restraints of (1) and may provide the necessary data to assemble an operational top-dressing strategy. Such series are difficult to analyse *in toto* by standard statistical techniques, and the researcher should use instead (a) deduction, (b) close examination of each experiment by the methods developed in Chapters 4, 5 and 6 and (c) if available, results of comparable trials by other researchers.

3. Several point estimates of fertilizer response do not necessarily provide good predictions of final yield. A growth and yield fertilizer model is required to integrate results, account for

different initial conditions, and merge various levels of significance between treatments.

4. Such a system should be simple in structure; available data will be probably sparse especially with longer-term results, and a sophisticated model cannot be supported. Mortality in thinned *Pinus radiata* stands is difficult to model well, but elementary equations suffice at this stage of development.

5. A valid fertilizer growth and yield model from NZFP data suggests responses in the order of 100 to 120 m^3/ha can be achieved by age 30. Stand competition can have a significant impact on yield, but current company holdings do not contain regeneration with exaggerated spatial distribution of trees.

6. For use in forest estate models, derived fertilizer yield tables must be corrected for (a) net areas, (b) logging realisations, and (c) operational fertilizer losses.

10. IMPLICATIONS FOR FOREST EXPERIMENTAL DESIGN

10.1 INTRODUCTION

The way experimental responses to fertilizer have to be adapted before being useful in estate and other planning models, together with the proposed methodology in Chapters 4 and 5, have an appreciable impact on the design of fertilizer trials.

As outlined in Chapter 1, the topic of experimental design was pioneered by Fisher (1925). The topic subsequently has been well covered in many texts; [for example Cox (1958), Cochran and Cox (1966), Snedecor and Cochran (1967), John and Quenouille (1977), and Petterson (1985)]. Forestry expositions include those of Jeffers (1959), Whyte and Woollons (1977), Whyte *et al.* (1978), Woollons (1985), and Andrew (1986); emphasis here is in relation to the recommended procedures of Chapters 4 and 5 and to the requirements for adequate growth-modelling, discussed in Chapter 9.

10.2 EXPERIMENTAL DESIGN

10.2.1 Replication

The recommended system of analysis, embracing the use of linear regression models, is enhanced with increasing replication. The more replication the experimenter has, the better his chance of securing significant differences between treatment (model) intercepts and discriminating among regression coefficient slopes. Proposals for specific numbers of replications for pole-crop forest fertilizer trials are dependent on:

1. the expected size of response(s);
2. the objectives of a trial;

3. the inherent variation in the species being studied and the ecosystem they reside in;
4. the planned duration of an experiment;
5. the actual experimental design and layout employed.

For example, only three replications of each treatment were employed in the experiment of Mead *et al.* (1984), re-examined in Chapter 2, yet very high significance was achieved; here the responses exceeded 23%. This may be contrasted to the data of Gerig *et al.* (1978), re-examined in Chapter 3; despite the presence of twelve replications associated with the levels of each factor, and utilisation of multiple covariance techniques to give a sensitive error mean-square, treatment effects are only weakly significant, largely caused by only 2-3% absolute differences in treatments.

It is possible to imagine situations where a researcher may decide to install a trial with low replication. *A priori*, forest crops may be known to respond appreciably to nitrogen fertilizer, where extensive trials to substantiate this may have used ammonium sulphate as the N source. An additional experiment, but substituting urea, might therefore be seen to require little replication. Alternatively, an expensive fertilizer such as ammonium nitrate could be utilised in another trial, under the premise that an extraordinary response would be required to justify its further use. Low replication would have to suffice.

Such practice is clearly hazardous, however, and in general the experimenter should strive to achieve the maximum possible replication in fertilizer trials. Over fifty years ago, Fisher and Wishart (1930) suggested 20 or more degrees of freedom were advisable for error computation in agricultural experiments. Woollons (1980) indicated that a minimum of

15 degrees of freedom was necessary to detect treatment responses in *Pinus radiata* on thrifty sites, a study based on NZFP trial data. These are general guidelines, which are always subject to modifications for reasons given above. Their adoption, nevertheless, would have considerably improved many fertilizer experiments in New Zealand. [See for example, Jacks *et al.* (1972), who cite many impaired nutrition trials.]

When experiments are installed in mature stands, or when responses are required to be evaluated over decades, the researcher must be prepared to install high numbers of replicates to cope with even more variation because mortality or damage to plots is likely to be severe as shown in the following examples. Hamilton (1976) reported that the Bowmont thinning experiment had become increasingly occupied by a substantial population of roosting pigeons, causing significant leader damage. Wagle and Beasley (1968) were forced to remove mistletoe-infected, porpucine-topped, or snow damaged trees, when installing a fertilizer experiment in 45-year-old *Pinus ponderosa*. Weetman *et al.* (1980) and Salonius *et al.* (1982) both faced mortality caused by spruce budworm (*Cloristoneura fumiferana*) on trials in stands of 60 and 65-year-old black spruce *Picea mariana*. Over a period of 22 years, Waring (1980) observed increasing mortality caused by severe moisture stress, in a *Pinus radiata* nutrient experiment near Canberra, Australia. Accordingly, the researcher should recognise that there is a high chance of damage occurring in forest trials, sometimes to the extent of losing experimental units. In the NZFP experimental series, both Trials Nos. 7 and 8 incurred the loss of one plot, while Trial No. 4 was virtually destroyed by gale-force winds. An experiment with low replication, therefore, is frequently vulnerable to accidental loss of precision.

Apart from physical damage, the NZFP series suggests additional variation can be encountered on some sites as an experiment approaches maturity. For example, Trial No. 1 showed a highly significant response between 1968-1976, yet by 1986 the difference in treatments was barely significant [see Appendix 6]. In hindsight, a decision to re-thin one half of the experiment in 1976 was ill-conceived, as some of the plots developed unsuspected variation between 1976 and 1986, apparently associated with neither mortality nor damage. Examination of these plots suggests that as experimental trees enter a maturation phase, decline in growth occurs at differing times with different sets of trees, impairing the effect of initial (before treatment) covariates. For example, for Trial No. 1 of the NZFP series, both initial basal area/tree and plot competition are highly effective covariates over the period 1967-1976, yet by 1986 the second covariate is completely ineffective, while the first is no longer significant at the 10% level [see Appendix A 6.1.1]. Similarly, Trial No. 8 displays high efficiency of the same covariates over 1975-1981, but they fall below the 5% level by 1986 [see Appendix A 6.8.1]. Reasons for this may be genetic, and/or the compounding influence of microsites within plots. Utilising the methods and tests of Davis (1956), (effectively a t-test for the difference of two means in reverse), and estimating the error variance from Trial No. 1, would suggest that between 12 and 24 replications of each treatment would be required to detect a significant ($p < 0.05$) response over 20 years, depending on the chosen Type II error rate, [Ostle (1963), pp.108-109]. Recent work [Woollons *et al.* (1988)] suggests that other sites do not necessarily exhibit variation in this way.

With regard to the analytical system developed in Chapter 4, it is imperative that a minimum of three replicates be available for any specific treatment, otherwise it is not possible to detect curvature with respect to growth and initial quanta of growing stock. When the experimental design comprises a p^n factorial arrangement of treatments, and $p \geqslant 3$, less replication is necessary, since the internal replication inherent in the various factors should provide the data to test for curvilinearity and disparate slopes. When $p = 2$, a test for parallelism (but not curvilinearity) is available, although the latter can usually be assessed by graphical inspection. For example, the 2^4 factorial analysed in Chapter 7 exhibited clear linear relationships and highly significant ($p > 0.005$) responses to N, with only two physical replications of each treatment. It is erroneous to include thinning as a factor in this respect, however, since each regime is likely to have a unique regression coefficient [see the experiments of Hunter *et al.* and Waring analysed in Chapters 3 and 6, and Woollons (1985)].

10.2.2 Establishment of experimental units

Throughout this study emphasis has been given to the point that later-age fertilizer experiments generally require a covariate to adjust for differences in initial amount or size of experimental material, which, unless accounted for, will seriously inflate residual error and partially confound treatment responses. Since this is known prior to the installation of an experiment, it is most advantageous to establish plots so that the precision of the covariate will be enhanced. This can be achieved if the range of initial plot growing stock (of experimental units) is extended as far as possible.

The problem of fertilizer x thinning experiments was alluded to in Chapter 4, when thinned regimes have low precision with respect to their regression (growing stock) slope, in essence a treatment effect. While this can be ameliorated by utilising basal area or volume/tree as the response variable and the use of covariates [see, for example, Waring's experiment in Chapter 6, and Woollons (1985)], neither technique will work when both initial stocking and growing stock are tightly controlled.

To rectify this, the experimenter should refrain from imposing strictly defined thinning regimes in later-age trials, and deliberately allow a small range of initial stockings and growing stock to be included. For example, it may be intended to examine the performance of *Pinus radiata* in stands both unthinned and thinned to 300 stems/ha, in the absence and presence of fertilizer. Unthinned experimental units will usually carry a wide range of growing stock, but plots representative of 300 stems/ha would be tightly controlled in that respect. Instead, if the latter regime is regarded as "heavily thinned", and units of between, say, 200-400 stems/ha are established, the problem should be considerably lessened. There is a good chance of securing additional information by the latter approach; alternatively, insistence on an exact stocking is commonly removed by subsequent mortality.

It might be argued that this recommendation could be taken further by abandoning the examination of "unthinned" and "thinned" as experimental factors altogether, and establish a continuum of initial stockings from say 100 stems/ha, progressively up to planting densities as high as 1400-1800 stems/ha. One-half of the units on a ranked (by initial stocking) basis would then be fertilized as symbolised in Figure (10.1 (a)).

Such a concept is considered unwise, however, because of the following:

1. The functional form of the response model may be complex. For example, very heavily thinned plots are liable to undergo a period of initially low increment, and perhaps exhibit a small response. Lighter thinned densities may record faster initial growth, and produce a substantial response to fertilizer, while unthinned regimes may not respond at all, as depicted in Figure (10.1 (b)).

Such results may require non-linear functions to fit the data well, for example, log-reciprocal or logistic models:

$$\begin{aligned} \ln(Y) &= \alpha + \beta/X \\ Y &= \alpha/\ln\,(1 + \beta e^{-\gamma X}) \end{aligned} \qquad (10.1)$$

with fertilized and control treatments requiring unique coefficients to depict responses. While feasible to assay by non-linear least-squares techniques, [Bard (1974)], the absence of true replication would debar the utilisation of goodness-of-fit testing [Draper and Smith (1981)];

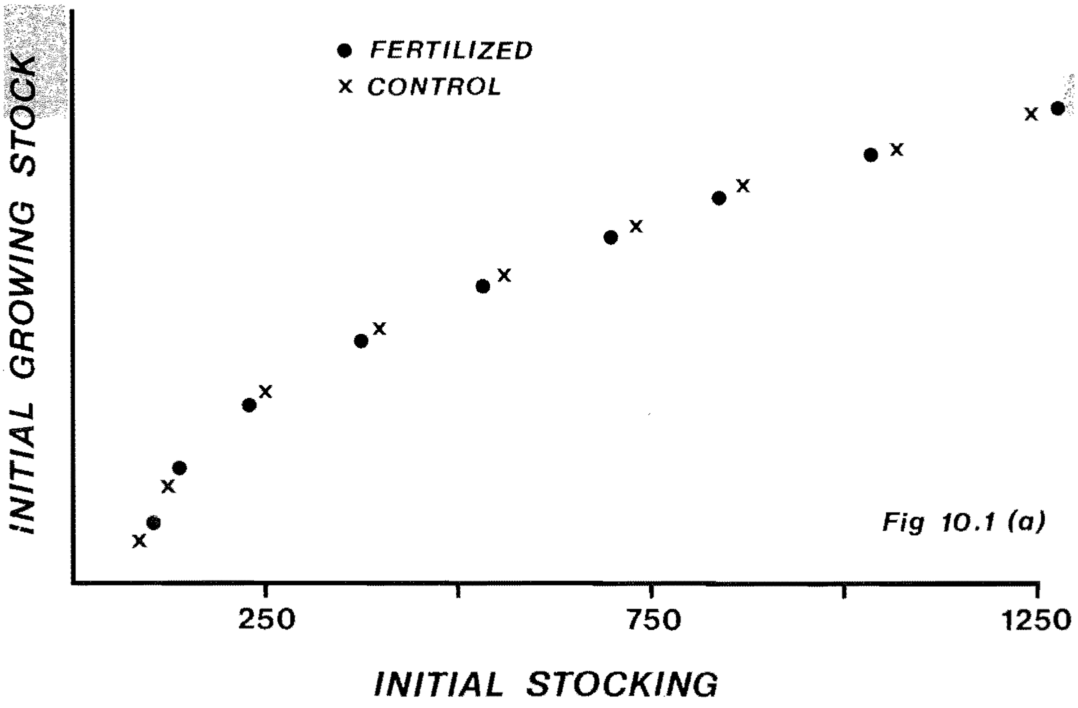
2. Analysis of (co)variance methodology would not be possible;

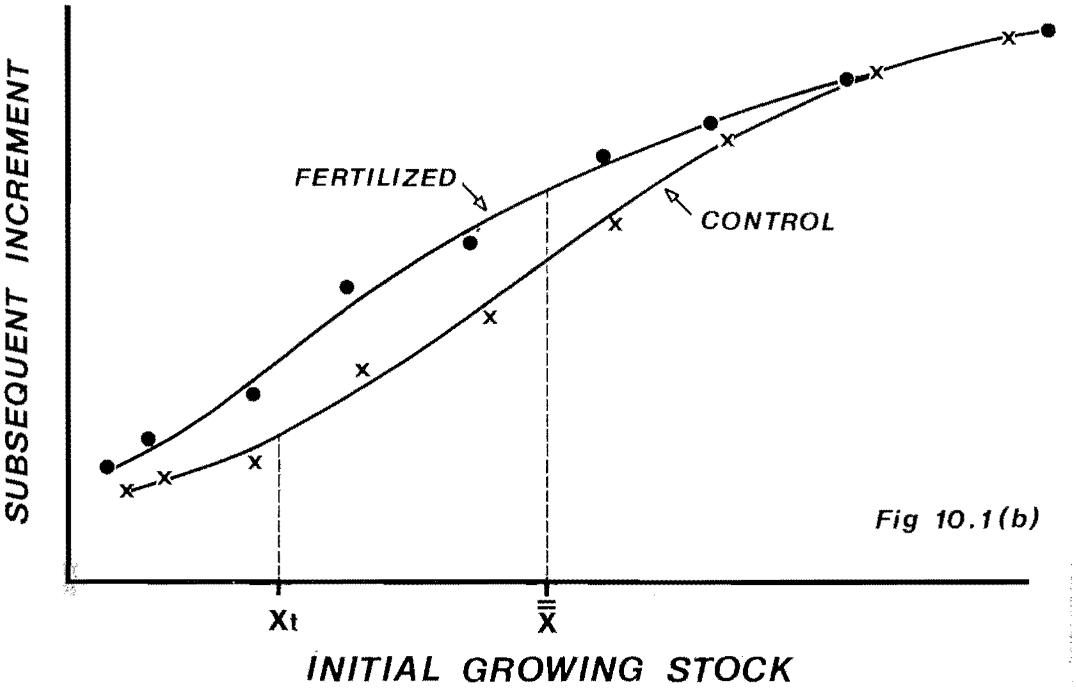
3. Standard errors and confidence intervals for responses, and treatments *per se*, could be coarse at extreme stockings. For example, the variance of a predicted thinning yield at the point X_t (see Figure 10.1 (b)) is

$$V(Y_t) = \sigma^2([1/n_t] + [X_t - \bar{\bar{X}}^2/\Sigma\, x^2) \qquad (10.2)$$

where n_t = number of control replicates.

The second term in (10.2) may well be a significant factor in absolute terms.





Figures (10.1(a) and 10.1(b))

Depiction of a hypothetical experiment involving unbounded thinning regimes, and uneven responses.

10.2.3 Choice of treatments in fertilizer experiments

An established experimental principle is that levels of treatments or factors should be chosen at relatively wide intervals so as to aid the formation of response surfaces or isolation of maximum/minimum zones of treatment responses [see Cox (1985) and Finney (1972), who formulated guidelines for choice of factor levels]. More generally, this strategy applies particularly to later-age forest fertilizer experimentation; irrespective of the quality of designs, mensurational effort, and analysis, a truism emerges from the results of trials reported in this study and elsewhere - forest trees can be complex and variable biological units, responses of which are frequently insensitive to minor differences in imposed treatments. Thus, when selecting treatment rates or contrasts, the experimenter should strive for disparity and avoid subtle or negligible differences.

For example, Trial No. 3 of the NZFP series was installed with rates of a composite fertilizer, including 0, 62, 125, 187, and 250 kg urea/ha. Subsequent analysis could isolate a response only to the highest level and, in hindsight, the chosen differences are far too restricted.

Waring's fertilizer x species experiment, examined in Chapter 6, consists of eleven fertilizer treatments [see Appendix 4], seven of which differ only in trivial absence or presence of trace elements or alternative forms of phosphorus. Analysis can only discern between two major groupings (NP and P with (1)); it is clear that the choice of treatments was not ideal, with no factorial structure and contrasts attempted between very similar nutrient combinations.

Hunter's fertilizer x thinning x pruning experiment, re-analysed in Chapter 3, shows that pruning to a height of 2 metres had no deleterious effect on response to fertilizer in unthinned conditions. The actual treatment imposed was a selective pruning where stem and branch form (as opposed to dominance) played a significant rôle in the selection process. The criterion of pruning is not strongly imposed, and the contrast may have been better defined by pruning all trees in relevant plots, regardless of size, and attempting higher lifts.

Waring's 4 x 2, fertilizer x thinning experiment, examined in Chapter 6, utilises stocking of 2960, 2200, 1480, and 740 stems/ha. At first sight, these densities represent divergent stand stockings, yet later analysis isolates only two distinct growth rates; possibly a better choice would have been approximately 3000, 1000, 500 and 200 stems/ha, thus covering the full range of feasible stockings. This latter recommendation should not be seen to contradict the earlier viewpoint that trials involving a continuum of densities should be avoided. Here, four average stocking figures are envisaged, with some replicate plots diverging by say ± 100 stems/ha to secure some precision on each regression coefficient, yet enabling the experiment to be analysed by a model

$$E(Y) = \sum_{i=1}^{8} \alpha_i + \sum_{j=1}^{8} \beta_j f[X] \qquad (10.3)$$

and the procedures of Chapter (4.2).

10.2.4 Additional covariates: importance of procuring comprehensive data at trial establishment

A significant finding in this study is how well the utilisation of multiple covariance techniques results in reducing error variance. For example, Trials Nos. 1, 2, 3 and 8 of the NZFP series, all gave enhanced analysis by incorporating competition as an additional covariate, while the analysis of data in Gerig *et al.* (1978) was considerably improved by introducing soil N level as an auxiliary variable. In addition, Waring's species x thinning data (Chapter 6) suggested the presence of an unblocked fertility gradient after utilising plot position co-ordinates as covariates.

While the latter is essentially an analytical procedure found when examining data, the first two examples are only feasible because the relevant data were obtained at the time of designing or installing the experiments. The experimenter should therefore be well aware that supplementary data, such as initial foliar or soil nutrient levels, or spatial distance, are frequently related to future plot growth, and can be used to improve considerably the precision of subsequent analysis. Moreover, joint use of quanta and nutrient covariates goes some way to explaining growth, and understanding the mechanisms involved.

The procurement of spatial information is relatively trivial, but attainment of reliable nutrient levels data is not without some cost although it can be reduced by efficient sampling. In the longer term, the expense is likely to be compensated by the additional information obtained, probably reducing the total number of trials required to study nutrient activity.

10.2.5 Experimental design and growth models

It was concluded in Chapter 9 that long term fertilizer gains are best estimated by a growth and yield simulator. It is generally unrealistic to recommend an independent series of experimental data to secure adequate modelling data; usually several years elapse before an operational rationale can be interpreted, but to establish more experiments with the treatments considered appropriate, is patently prohibitive in terms of time and cost. It follows, therefore, that an experimental series must not only serve to produce a top-dressing prescription, but also largely form the basis of a growth-modelling dataset. This has several implications for their experimental design:

1. Some long term data are imperative to produce reasonable estimates of asymptotic parameters resident in familiar modelling equations. For example, the Schumacher, Gompertz, and Chapman-Richards yield models respectively given by

$$\begin{aligned} \ln(Y) &= \alpha + \beta/T \\ Y &= \alpha\exp(-\beta e^{-\gamma T}) \\ Y &= \alpha(1-e^{-\beta T-To})^{\gamma} \end{aligned} \qquad (10.4)$$

   are well known sigmoid-shaped functions useful in forest growth models. Irrespective of the chosen estimation process, problems are likely to be encountered in establishing sound estimates of α (in 10.4), an upper asymptote to yield, unless some data are available to plot its likely limit. It has already been established that long-term fertilizer experiments can exhibit increasing variation with age; it is thus essential that the experimenter ensures that there are sufficient data to meet these criteria lest the modelling process become intractable. This would suggest that long-term experiments be installed with plots of sufficient size to absorb inevitable mortality, and have sufficient living trees to estimate net basal area/ha with some precision. Recently, Correll and Cellier (1987) have advocated small plots with 9-16 trees as an optimal size in forestry trials. Based on establishment experiments, this is considered here to be too small for trial installation of pole-crop age. Experimental units of, say, 15-25 trees are to be preferred, with the intention of removing the relatively larger between-plot variation by covariates in the analysis.

2. Factorial layouts would seem to be imperative in the early stages of an experimental series. If the active elements can be isolated quickly, these trials can be remeasured periodically until maturity, giving long-term data and non-active combinations serving as controls. Meantime, additional experiments can be installed, probably limited to only active nutrients (and controls), but investigating the criteria given in Chapter (8.1). In hindsight, the NZFP trial series, while laudable from a replication viewpoint, suffers a little from the use of mixed fertilizers in two of the early experiments. While it is highly likely nitrogen is the principal element, any doubt would have been avoided by utilisation of a factorial layout, as subsequently used in Trials Nos. 2, 5 and 6.

10.3 EXPERIMENTAL DESIGN: SUMMARY

1. Adequate replication is essential to utilise fully the suggested system of analysis described in this study. Longer term experiments are particularly susceptible to damage, but this can be largely overcome by utilising robust designs with sufficient replication.

2. Pole-crop fertilizer experiments can be considerably enhanced by installing plots with an adequate range of growing stock. This will improve the precision of regression coefficients, and provide more information.

3. Differences in chosen fertilizer levels should be broad, and not pre-occupied with minor differences in treatments.

4. Careful selection and measurement of auxiliary data at trial establishment will give a lower experimental error through multiple covariance analysis.

5. Data from experimental series will form the major component of data for fertilizer growth modelling. Long-term results are essential, best obtained from factorial experiments using relatively large plots and sufficient residual trees.

11. SUMMARY OF CONCLUSIONS

1. The general linear model incorporating analysis of variance, regression analysis, and analysis of covariance, a well established statistical technique for testing hypotheses about data from designed experiments, can be easily adapted for analysing forest fertilizer trials. Examination of the analysis of some published forest fertilizer experiments, however, shows that these methods have been frequently ignored by some forest scientists. Thus, nutrition experiments have been often superficially analysed, or else much less than the maximum amount of information available in a trial has been extracted.

2. A system of statistical analysis for examining data from pole-crop and older designed forest experiments, which allows results to be fully interpreted, is suggested. This system:

   (a) utilises a covariate to allow for initial differences in amount of growing stock which, if not rectified, can seriously confound treatment responses, and drastically reduce sensitivity or precision in hypothesis testing;

   (b) employs, if data are available, additional covariates to recognise initial differences in plot fertility or competitive status;

   (c) allows, when required, partially damaged or heterogeneous experimental plots to be accommodated in analysis, which otherwise can influence treatment responses, or give rise to imprecise hypothesis testing;

   (d) incorporates the most appropriate regression coefficients to cater for unique differences in growth and initial size of experimental material;

(e) gives the best available sensitivity or precision in hypothesis testing;

(f) suggests the use of basal area or volume/tree rather than growing stock/unit area as a response variable and covariate, because the former can give a more discerning analysis;

(g) demonstrates that either yield or growth may be used in such applications, since they give essentially equivalent analyses when a covariate as in (a) is included.

3. This system is advocated for the analysis of later-age fertilizer experiments. Re-examination of a number of published trials, and analysis of a series of NZFP experiments, support the utility of the suggested methodology. Particular reference is made in this regard to

   (a) reanalysis of data in Salonius *et al.* (1982), [Section 3.3.2];

   (b) reanalysis of data from Gerig *et al.* (1978) [Section 3.3.3];

   (c) utilisation of basal area/tree as a response variable in reanalysis of an experiment given by Mead (1974) [Section 5.1.6];

   (d) modification of the Hegyi competition index and its incorporation in analysis of NZFP trials as a second covariate which invokes gains in precision by up to 72% (use of multiple covariance, *per se*, produces very large gains in precision, and adjusted yields are shown to differ appreciably from unadjusted treatment averages).

4. The proposed analytical system allows a provisional top-dressing rationale to be deduced out of results from the NZFP trial series,

and elaborates or refines the findings of Woollons and Will (1975):

(a) experimentally, thinned stands of central North Island *Pinus radiata* can be boosted in basal area production by up to 6.2 ± 2.70 m^2/ha over nine years (90% confidence interval), through application of nitrogen fertilizer;

(b) a minimum of 500 kg urea/ha, best applied one year following thinning, is required to achieve a sustained response;

(c) presence of the fungal pathogen, *Dothistroma pini*, can lessen or impair fertilizer responses;

(d) fertilizer response can induce significant, but apparently transient changes in stand form-factor, between 0.006 to 0.014 in magnitude, equivalent only to a change of 2-3%.

5. The need for a series of experiments is crucial to understanding fully the nature and extent of fertilizer response; one experiment cannot provide all the relevant information because of necessary restrictions on treatment levels, while responses interact strongly with initial growing stock, time, and site.

6. To predict fertilizer responses at maturity, it is necessary to construct a growth and yield simulator principally from experimental data.

   A model, constructed from the NZFP trial data, recognising initial basal area, fertilizer application, and competition status, predicts fertilizer responses in the order of 100-120 m^3/ha for Company stands. Allowance for realisation factors and operational top-dressing, however, could reduce the estimates to 50-60 m^3/ha.

7. In order to exploit fully the recommended system of analysis, it is desirable to heed several criteria for experimental design. Specifically:

   (a) three levels of treatments are necessary to detect curvilinearity of response variables and initial quanta of growing stock, with all experimental designs, while more numerous replication represents an insurance against physical damage to experimental units and compounding variation sometimes encountered in long term experiments;

   (b) use of a covariate to represent quanta of initial growing stock is essential with later-age fertilizer experiments, precision in the use of which can be enhanced by deliberately installing plots with differing initial amounts of growing stock, although not to an extent that units do not reasonably represent a few pre-chosen silvicultural regimes;

   (c) trivial differences in fertilizer treatments or silvicultural regimes should be avoided;

   (d) the potential of multiple covariance to achieve sensitive analyses should be recognised by securing relevant auxiliary data at trial installation;

   (e) factorial layouts, using plots of sufficient size to withstand ensuing damage or mortality provide best data for modelling fertilizer responses, and measurements should be taken long-term, to estimate asymptotic parameters of growth model equations.

Applications of multiple covariance, in fertilizer trials where experimental units are deliberately installed with disparate quanta of growing stock, hold the key to successful forest fertilizer experiments.

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APPENDIX 1

Reprint of Woollons, R. C. (1985). Problems associated with analyses of long-term *Pinus* fertilizer x thinning experiments. Aust.For.Res. 15, 495-507.

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Problems associated with Analyses of Long-term *Pinus* Fertilizer × Thinning Experiments

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Problems associated with Analyses of Long-term *Pinus* Fertilizer × Thinning Experiments

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Abstract

A commonly encountered forest field experiment is one which involves a number of thinning regimes (including unthinned) to which a combination of fertilizer dressings are or are not applied. Statistical analyses of these experiments are not straightforward, because due allowance must be made for initial differences in growing stock, subsequent mortality and contrasting growth rates. Initial basal area per hectare and initial stocking are shown in this study to be unsatisfactory covariates for such analyses, and it is suggested that initial basal area per tree be used instead. Covariance analysis with one regression coefficient is demonstrated to be too restrictive, and an alternative regression model with separate coefficients for each treatment is recommended. It is argued that standard covariance-adjusted means can produce highly biased estimates of treatment responses. Better estimates are available by adjusting yields around each thinning class.

Occasionally, growth data over several years following fertilizer applications are available. Alternative methods of analysing these experiments involve fitting projection equations to the plot yields, and subsequently analysing the estimated coefficients by covariance or multivariate techniques.

[O.D.C. 242.5 : 237.4 : 174.7 *Pinus* spp.]

Introduction

Fertilizer × thinning field experiments are important to management and nutrition scientists in many forest holdings throughout Australasia. Evidence gathered over the last 30 years suggests strongly that later-age nutrition response is associated with stand density and site productivity (Woollons and Will 1975; Mead and Gadgil 1978; Waring 1981). On nutritionally poor sites, fertilizer response tends to occur irrespective of stocking, whereas on better sites thinning may be a pre-requisite for a response to fertilizer. Thus, in many forest areas there is a real need to establish the dependence of stand density to production gains by fertilizer. Because many responses interact strongly with site and time (Woollons 1980), it is prudent for the researcher to combine stand density and nutrient applications as treatments in a single experiment.

There is evidence in both published and unpublished literature that researchers frequently have difficulty in analysing this type of forest experiment. For example, both Wagle and Beasley (1968) and Hunter *et al.* (1985) refrained from examining all the data in a single analysis, and decided to split the treatments into matching thinning classes. While this approach is not invalid, it suffers because the experimental error is estimated from severely reduced degrees of freedom, with subsequent loss in precision when estimating treatment effects.

The objectives of this study were to examine analyses of fertilizer × thinning experiments and to develop a methodology for efficient analysis of such trials. The methods are illustrated here by applying them to data from a fertilizer × thinning experiment, and the results are discussed.

Statistical Considerations for Experiment

For simplicity, discussion is mainly limited to the case where one thinned and one unthinned regime are examined in combination with and without a fertilizer dosage. Extension to the case of multiple thinning and a combination of fertilizer treatments follows easily from the suggested procedures.

Consequences of Thinning

When a trial is established using 'thinned' and 'unthinned' as experimental factors, the following criteria apply. In a *thinned* environment:

(i) experimental plots contain a residual set of trees, after the removal of another set of trees;
(ii) if thinned to a chosen residual stocking, an equal number of trees will reside initially in all replications; if thinned to a predetermined basal area, the number of trees will differ slightly in each replication; in either case, subsequent mortality can be expected to be light;
(iii) the growth of the residual stems will probably increase relative to their previous (unthinned) growth, possibly after a period of thinning shock (Leech 1978).

In an *unthinned* environment, however:

(i) no trees are removed from any experimental plots;
(ii) the number and quantity of growing stock will vary substantially in each plot, particularly if the experiment is laid out in regeneration, and subsequent mortality can be expected to be significant, through competition;
(iii) if the trial is established after the time of maximum annual increment, the rate of growth of the trees in the plots will decline.

In addition, the creation of thinned plots necessarily produces appreciable differences between regimes in the amount of growing stock.

Let B_{1tk} and B_{2tk} denote the basal area per hectare of the kth thinned plot at age A_1 (trial establishment age) and A_2 (some future time). Similarly, let B_{1uk} and B_{2uk} denote the basal area per hectare of the kth unthinned plot at ages A_1 and A_2. If there is no extraordinary mortality in any unthinned plot, it then follows that:

$$B_{1uk} > B_{1tk} \qquad \text{for all } k \tag{1a}$$

and

$$B_{2uk} > B_{2tk} \qquad \text{for (virtually) all } k. \tag{1b}$$

Choice of Response Variable

The inequalities (1a, b) and the criteria outlined above create several problems in the analysis of fertilizer × thinning experiments. It is necessary to provide a system which ensures that estimated treatment responses are not confounded with initial differences in growing stock, distinct patterns of mortality or contrasting rates of growth.

The process of analysing a response variable in conjunction with other contributing variables can be achieved by the well-known analysis of covariance (e.g. Ostle 1963; Snedecor and Cochran 1967). However, its application here is not straightforward, and complications arise if initial basal area per hectare or initial stocking are chosen as covariates. Because of the thinning to a predetermined basal area or stocking, the dispersion of the response variable will be extremely restricted for thinned treatments, but comparatively spread for unthinned treatments. Fig. 1 illustrates the situation for a hypothetical experiment (for simplicity, fertilizer treatments are omitted). Clearly, any attempt to fit a regression line through the thinned data will be ill-defined, with the likelihood of introducing heterogeneous variances (Snedecor and Cochran 1967). Moreover it is evident that both initial basal area and stocking will be significantly correlated with the thinning treatments. When the treatment yields from such an experiment are adjusted by the covariance process, distorted estimates are likely to be produced, a consequence of the association between initial yield with thinning, and adjustment around the overall average initial yield ($\bar{X}$ in Fig. 1). Kempthorne (1952) and Cochran and Cox (1966) have discussed the implications of using treatment-affected covariates in covariance analyses.

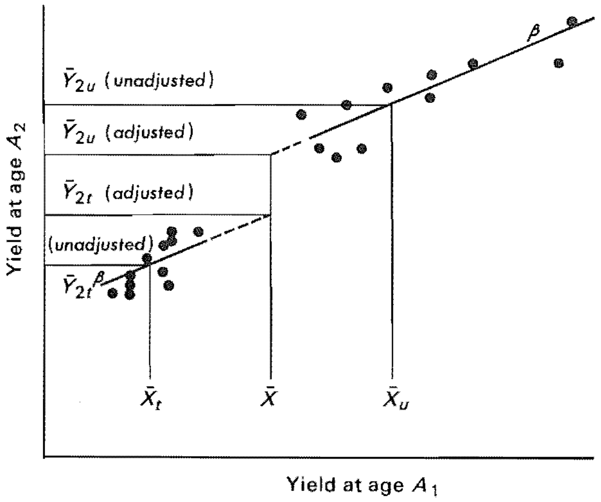

Fig. 1. Plot of data from a hypothetical experiment comparing yields from thinned (t) and unthinned (u) treatments at an initial age A_1 to a later age A_2.

The types of problems outlined above can often be avoided, however, by adopting an alternative response variable and covariate. For example, basal area and stocking may be combined to form the variable 'basal area per tree', that is,

$$Y_{2ik}=B_{2ik}/N_{2ik}, \qquad Y_{1ik}=B_{1ik}/N_{2ik}, \tag{2a, b}$$

where Y_{2ik} and Y_{1ik} are the basal area per tree at ages A_2 and A_1, N_{2ik} is the number of trees alive at A_2, and the suffix i replaces t, u in the inequalities (1). The use of these variables then can considerably improve the covariance analyses. Irrespective of whether thinning is determined by a residual stocking or basal area, the structure of equations (2) ensures a considerably better dispersion of data for the thinned treatments, thus giving a more precise regression line in the analysis. The data for the unthinned treatments are also extended, which has the effect of reducing the degree of correlation between the treatments and the concomitant variable.

The divisor N_{2ik} in equations (2) is appropriate if preliminary analysis substantiates that stem mortality is independent of fertilizer effects. If not, N_{2ik} should be replaced by N_{1ik}, the initial number of trees in each experimental plot at age A_1. In the absence of significant mortality, initial basal area can be calculated in terms of trees alive at age A_2.

A Representative Linear Model

Reconsider a forest field experiment which involves thinned and unthinned regimes combined with the absence or presence of fertilizer. Now let the statistical expectation E of a response variable Y (taken here as basal area per tree) be given by

$$E(Y)=\begin{bmatrix}\alpha_{ti}\\ \alpha_{ui}\end{bmatrix}+\begin{bmatrix}\beta_{ti}\\ \beta_{ui}\end{bmatrix}f(X)\,, \tag{3}$$

where α_{ti} and α_{ui} are the intercept values for the i levels of fertilizer applied to the thinned and unthinned regimes respectively, β_{ti} and β_{ui} are the regression coefficients associated with the change in the response variable Y for unit change in the independent variable X, and $f(X)$ is some function of the independent variable X (taken here to be initial basal area per tree). In addition, let $\bar{X}_t$ and $\bar{X}_u$ be the average initial thinned and unthinned basal area per tree.

The linear regression model (3) provides a powerful and flexible equation to analyse data from fertilizer × thinning trials. Statisticians will recognize (3) to be a small extension of covariance analysis, allowing the regression coefficient to vary with treatment, but its application in forestry experimentation seems to have been neglected. Differences in growth rate can be examined by testing the equivalence of β_{ti} and β_{ui}, while fertilizer responses in either regime are established by demonstrating significant differences between the intercepts α_{ti} or α_{ui} (Snedecor and Cochran 1967; Swindel 1970).

Since the relationship between Y and X is mostly found to be linear, the function $f(X)$ in (3) usually may be defined by X. If curvilinearity is present, it can often be removed by a logarithmic transformation of the independent or dependent variable. Estimates of fertilizer response are calculated by adjusting Y about the thinned *or* unthinned initial values $\bar{X}_t$ or $\bar{X}_u$ (see Fig. 1) by covariance (Ostle 1963). The overall initial mean $\bar{X}$ is inappropriate, because the standard errors of the estimates will be inflated by a factor

$$\nabla\sqrt{[(\bar{X}_{t,u}-\bar{X})/E_{XX}]}\,, \tag{4}$$

where ∇^2 is the model error mean square, $\bar{X}_{t,u}$ is the mean initial value of *either* the thinned *or* unthinned variable, and E_{XX} is the error sum of squares for X.

Experiments with Periodic Data

Occasionally, fertilizer × thinning trials are measured annually or biennially over several years, which allows for more detailed and alternative analyses. One approach in this situation is to model each experimental plot with a suitable projection equation (Clutter *et al.* 1983), of the form

$$B_2 = f(A_2, A_1, B_1)\,, \tag{5}$$

where B_1 and B_2 are the net basal area per hectare at age A_1 (current or initial age) and A_2 (some future age). Two examples of candidate equations are:

$$\ln B_2 = (A_1/A_2)^b \ln B_1 + a\{1-(A_1/A_2)^b\} \tag{6a}$$

or

$$\ln B_2 = a + (\ln B_1 - a)\exp\{-b(A_2 - A_1)\}\,, \tag{6b}$$

where a and b are estimated parameters and, in the usual way, ln denotes the natural logarithm and exp the exponential function. The models (6a) and (6b) can be derived by non-linear least squares. Many variants of (6) are available; for the method to be satisfactory, the selected equation should fit each plot well, as assessed by goodness-of-fit statistics and examination of the scatter of residuals (Draper and Smith 1981).

The estimated parameters a and b of the projection equation are now available for examination by the model (3), with $\hat{a}$ and $\hat{b}$ used as the response and initial variables respectively. Since the residual mean squares (σ^2) are available for each plot, any analysis can usefully incorporate these estimates as weighting variables (Freund and Littell 1981).

Methods of Analysis and Results

In this study, the statistical methods discussed above were applied to the data obtained from a fertilizer × thinning trial, established by H. D. Waring (in approximately 1944) at Belanglo State Forest, Moss Vale, New South Wales. The selected experiment is laid out in a stand of naturally regenerated *Pinus radiata* and consists of four treatments: (1) thinned; (2) thinned and fertilized; (3) unthinned; (4) unthinned and fertilized. Each treatment is replicated eight times, in four randomized blocks (two replications per block). Each experimental plot is 20 × 20 m, with a 10 m buffer surround. Thinning was to a nominated basal area of 16·5 $\text{m}^2\,\text{ha}^{-1}$ (72 $\text{ft}^2\,\text{ac}^{-1}$), carried out in 1967. A composite fertilizer was applied to designated plots in 1967 following the thinning. Annual diameter measurements were made between 1967 and 1981.

Covariance analyses were carried out on the data for 1967 and 1981 using four methods:

Method 1. Response variable, basal area per hectare for 1981; covariate, basal area per hectare for 1967.

Method 2. Response variable, basal area per hectare for 1981; covariate, stems per hectare for 1967.

Method 3. Response variable, basal area per tree for 1981; covariate, basal area per tree for 1967. Responses adjusted around each thinning regime.

Method 4. Response variable, basal area per tree for 1981; covariate, basal area per tree for 1967, with separate regression coefficients for thinned and unthinned. Responses adjusted around each thinning regime.

In addition, the periodic data of 1967–81 were modelled by the projection equation (6a) and the estimated parameters (for each plot) were analysed using equation (3).

A summary of the experimental data for 1967 and 1981 is given in Table 1. It will be noted that:

$$B_{1u} > B_{1t} \qquad \text{for all plots}$$

and

$$B_{2u} > B_{2t} \qquad \text{for 14 of 16 plots,}$$

Table 1. Experimental and statistical data for 1967 and 1981 in Waring's trial

The data given in (*a*) are the net basal area (m^2 ha^{-1}) and stocking (stems ha^{-1})

(*a*) *Experimental Data*

Plot	Fertilized				Unfertilized			
	basal area		stems		basal area		stems	
	1967	1981	1967	1981	1967	1981	1967	1981
				Unthinned regime				
1	27·5	56·2	1606	1458	24·2	46·9	1853	1656
2	30·7	59·3	2224	1853	27·9	48·3	1260	1112
3	35·3	67·1	2249	2100	32·5	57·3	1433	1310
4	27·9	54·7	1260	1137	24·1	44·6	964	933
5	28·6	60·7	1631	1557	29·3	56·6	1927	1853
6	29·4	56·0	1606	1384	26·1	45·4	1161	1038
7	24·9	47·0	890	741	31·5	56·2	1977	1828
8	31·5	54·0	1705	1433	30·5	54·8	1458	1408
Mean	29·5	56·9	1646	1458	28·3	51·2	1504	1392
				Thinned regime				
9	17·0	42·9	667	667	16·4	41·0	470	445
10	16·9	41·8	677	568	15·8	39·9	618	618
11	16·8	43·6	890	890	16·3	42·2	1063	1063
12	16·2	46·0	692	667	17·1	43·6	865	840
13	16·2	35·3	568	469	16·4	40·8	593	593
14	17·0	42·1	692	642	16·5	40·5	692	692
15	16·7	38·8	568	494	15·5	35·8	544	544
16	16·5	40·3	420	420	16·3	39·7	618	618
Mean	16·7	41·3	645	602	16·3	40·4	683	677

(*b*) *Correlation Coefficients*

Treatment	Initial basal area	Initial stocking	Initial basal area per tree
Thinning	0·944	0·835	0·468
Fertilizer	0·061	0·042	0·148

thus substantiating the inequalities (1). Table 1 also gives the correlation coefficients between initial basal area per hectare, initial stocking and initial basal area per tree, and the thinning–fertilizer treatments. All variables are independent of fertilization, but initial basal area and stocking are strongly correlated with thinning, whereas basal area per tree is appreciably less so.

Methods 1 and 2

A preliminary mortality analysis showed that stem death was independent of fertilization, and so initial basal area was calculated in terms of trees alive in 1981. The blocking of the experiment was completely ineffectual, and was removed from the analyses. Table 2 gives the results of the analyses of variance and the estimated responses during the period 1967–81 for the two methods.

Method 1 produces highly significant (1%) differences between treatments, and initial basal area per hectare is a strongly significant (0·1%) covariate. However, the estimated yields are distorted to a nonsensical degree by the high correlation between initial basal area and thinning. Tests for least significant difference

Table 2. Analyses of variance and estimated basal area yields for 1967–81 obtained by four methods of analysis

Significance: ***, significant at 0·1% level; **, 1%; *, 5%; ^{ns}, not significant at 5%

Analysis of variance				Estimated yields (m^2 ha^{-1})		
Source	d.f.	Mean square	Treatment	Thinned	Unthinned	Difference
Method 1						
Treatments	3	129·81**	Unfert.	49·9	42·3	−7·6
Initial basal			Fert.	51·3	46·5	−4·8
area ha^{-1}	1	431·96***	Fert.			
Residual	27	4·567	response	1·4^{ns}	4·2**	
Total	31	r^2=0·940				
Method 2						
Treatments	3	105·011*	Unfert.	44·8	47·4	+2·6
Initial			Fert.	46·1	51·6	+5·5
stocking ha^{-1}	1	278·763***	Fert.			
Residual	27	10·241	response	1·3^{ns}	4·2*	
Total	31	r^2=0·867				
Method 3						
Treatments	3	0·00137***	Unfert.	41·9	54·8	+12·9
Initial basal			Fert.	44·0	58·9	+14·9
area per tree	1	0·00378***	Fert.			
Residual	27	7·07×10^{-6}	response	2·1*	4·1*	
Total	31	r^2=0·981				
Method 4						
Treatments	3	0·000075**	Unfert.	42·0	54·8	+12·8
Initial basal			Fert.	44·0	58·9	+14·9
area per tree	2	0·00382***	Fert.			
Residual	26	5·86×10^{-6}	response	2·0*	4·1*	
Total	31	r^2=0·986				

substantiate a fertilizer response in unthinned conditions (1%), but no significant response in thinned.

Method 2 gives generally similar results, although the adjusted yields are somewhat less biased by the covariance process, a consequence of initial stocking being somewhat less correlated with thinning. However, the precision of method 2 is appreciably less than that of method 1, with the error mean square increased over twofold. Fig. 2 shows the model produced by method 2 fitted to the data; the lack of goodness-of-fit and the cluster-like nature of the thinned data are clearly shown.

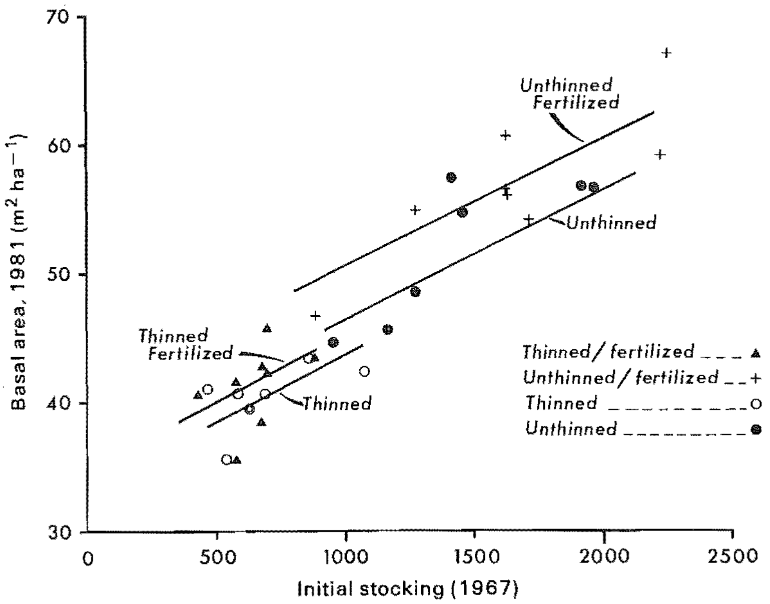

Fig. 2. Model fit to data produced by method 2, in which the initial stocking (stems ha^{-1}) is taken as the covariate.

Methods 3 and 4

Table 2 also gives the analyses of variance and the estimated responses during the period 1967–81 for methods 3 and 4.

Method 3 provides highly significant (0·1%) differences between treatments, and initial basal area per tree is a strongly significant (0·1%) covariate. Moreover, the estimated yields are undistorted by the covariance adjustments, and least significant difference tests substantiate a fertilizer response in both thinned and unthinned conditions (5%).

Method 4 refines the results of method 3. Separate regression coefficients, estimated for thinned and unthinned conditions, are significantly different from each other at the 2·5% level. The residual mean square is reduced by 17% (relative to method 3) through the use of separate regressions, and the fertilizer responses are significant at the 2·5% level.

Analyses of Periodic Data

Each experimental plot was modelled by the projection equation (6a), with net basal area per hectare taken as the predictor variable. Examination of residual scatter for each plot showed no sign of bias in prediction, and the minimum coefficient of determination was 0·988. All estimated parameters were significantly different from zero according to the asymptotic confidence intervals calculated by the non-linear procedure at the 5% level (Bard 1974).

The plot parameters a and b were incorporated into the model (3) using a weighted least squares analysis; each data pair was weighted inversely proportional to the corresponding residual variance, estimated by equation (6a). Examination of the relationship between b and a showed a curvilinear trend for unthinned plots, but strictly linear for the thinned set. Accordingly the data were split and analysed separately by thinning type. Differences between the intercept values for fertilized and non-fertilized plots were significant at the 1% level, for both regimes. Initial basal area per hectare was tested as an additional independent variable, but was not significant in both cases. The final equations were:

for unthinned plots,

$$\hat{a}=\begin{bmatrix} 5\cdot1601^{\mathrm{F}} \\ 4\cdot9633^{\mathrm{UF}} \end{bmatrix}-1\cdot091\ln\hat{b} \qquad (R^2=0\cdot811), \tag{7a}$$

where the indices F and UF denote fertilized and unfertilized values, and R^2 is the coefficient of determination;

for thinned plots,

$$\hat{a}=\begin{bmatrix} 5\cdot6399^{\mathrm{F}} \\ 5\cdot4949^{\mathrm{UF}} \end{bmatrix}-0\cdot623\hat{b} \qquad (R^2=0\cdot950). \tag{7b}$$

After adjustment by covariance for initial differences in b, the estimated parameters were substituted into equation (6a) giving:

for unthinned plots,

$$\ln B_2=(A_1/A_2)^{1\cdot873}\ln B_1+\begin{bmatrix} 4\cdot472^{\mathrm{F}} \\ 4\cdot327^{\mathrm{UF}} \end{bmatrix}\{1-(A_1/A_2)^{1\cdot873}\}; \tag{8a}$$

for thinned plots,

$$\ln B_2=(A_1/A_2)^{1\cdot316}\ln B_1+\begin{bmatrix} 4\cdot892^{\mathrm{F}} \\ 4\cdot672^{\mathrm{UF}} \end{bmatrix}\{1-(A_1/A_2)^{1\cdot316}\}. \tag{8b}$$

Equations (8a) and (8b) were projected from age 23 (1967) to age 36 (1981) using the average initial basal area per hectare for each regime. The differences in final growing stock gave estimated fertilizer responses of 3·3 m^2 ha^{-1} (thinned) and

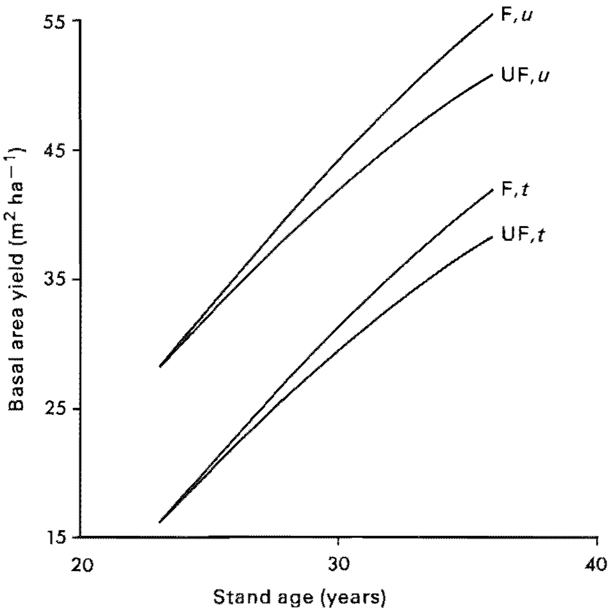

Fig. 3. Basal area yields of the four treatments from 1967 to 1981: F, fertilized; UF, unfertilized; *t*, thinned; *u*, unthinned.

$4{\cdot}7$ m^2 ha^{-1} (unthinned). The basal area yields of the four treatments are illustrated in Fig. 3.

These results were further substantiated by a multivariate canonical analysis (Mardia *et al.* 1979; Seal 1964) of *a* and *b* and initial basal area per hectare, for the four treatments. The first two canonical variates accounted for 99·69% of the between-groups variation, and the hypothesis of no overall treatment effect was rejected by Wilk's criterion at the 0·1% level. Fig. 4 depicts the four canonical means, together with approximate 10% confidence regions. The independence of the thinning regimes is clearly demonstrated, together with the fertilized treatments forming separate domains.

Discussion

From the preceding analyses, it is evident that considerable care should be taken with all analyses of fertilizer × thinning experiments. Such trials usually reflect a large input in time and expense, and an inappropriate analysis may fail to detect fertilizer response or may give badly biased estimates of treatment yields. It is beyond the scope of this study to examine the difficulties of designing these trials, except to emphasize the criteria which directly affect analyses.

The actual residual stockings or basal areas incorporated in an experiment will largely depend on local practice or preference. When experimental plots are established in a stand, the properties of equation (3) should not be overlooked; the variance of either β_i value is well known to be

$$\nabla^2/\Sigma(X_i-\bar{X})^2,$$

and this result has important practical implications. If a thinning regime is required

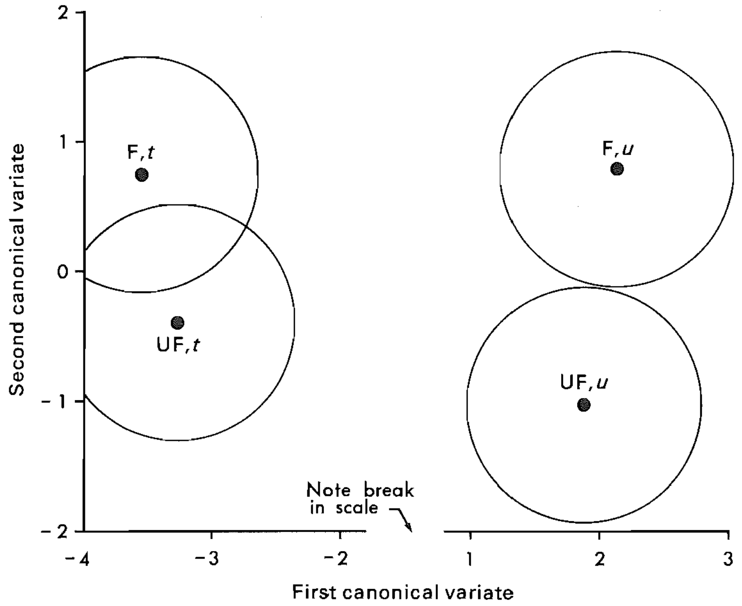

Fig. 4. Plots of canonical means for the four treatments: F, fertilized; UF, unfertilized; *t*, thinned; *u*, unthinned.

to a specified stocking, subsequent analysis is helped by the presence of varying basal area in each replicate, since this outcome will tend to partly minimize the variance of equation (3). On the other hand, if thinning is made to a nominated basal area, then different numbers of trees in the replicates, and the use of basal area per tree as the response variable, will achieve the same effect. Similarly, a range of initial basal areas in unthinned conditions is desirable. Variation in *Pinus radiata* stands is usually sufficient to achieve this system without undue difficulty. The researcher should certainly not be discouraged if an experimental layout appears to lack uniformity with respect to initial growing stock. This writer is aware of some unpublished fertilizer×thinning trials where extraordinary effort was put into balancing experimental plots into exactly equal initial units. Not only is this unnecessary (and usually futile because of subsequent mortality) but it also frequently creates plots with severely skewed diameter distributions, or sets of trees with extraordinary spatial distances. The four covariance analyses described above provide considerable evidence that initial basal area per hectare and stocking are inappropriate covariates, and the use of basal area per tree instead is much preferred.

The adjustment of treatment yields around each thinning class as opposed to the overall mean is an important process because it provides unbiased estimates of treatment yields. For the data analysed here the fertilizer responses can be estimated at the total mean, since the regression coefficients are equivalent for fertilized and unfertilized treatments within each thinning class. However, thinning effects are badly estimated, and so the expression of fertilizer responses as a proportion of unfertilized growth would be incorrect. In cases where fertilization causes a disproportionate growth rate, necessitating additional regression lines, the practice would give biased fertilizer responses. In either circumstance, failure to separate disparate treatments would lead to unnecessarily large standard errors of estimated responses.

The use of separate regression lines for thinned and unthinned treatments is a logical result from a silvicultural viewpoint and it allows additional information from the trial. In this experiment, the estimated yields were virtually equivalent to those derived from a single regression line; the differences in growth rates were not particularly large, and the degree of covariance adjustment required within each thinning group was small. However, in experiments involving heavy thinnings, appreciable differences in growth rate would be expected; alternatively, substantial differences in initial growing stock would require appreciable covariance adjustment. In either case the utilization of a single regression coefficient would produce inaccurate treatment responses.

The analysis of the periodic data with model (6a) confirms the covariance results, and effectively summarizes the treatment yields over 1967–81. The methods adopted here are far from exhaustive, and can be modified or amplified with other data sets. The multivariate analysis is included to guard against a possible deficiency of the periodic analysis; it can be argued that both the parameters *a* and *b* may be treatment effects and thus to use the latter as a concomitant variable may be injudicious. In this analysis, however, there is no evidence of a significant relationship between *a*, *b* and initial basal area (per hectare or per tree) or initial stocking.

Conclusions

The statistical assessment of the results from fertilizer × thinning experiments is not straightforward. For a satisfactory analysis, it is necessary to allow for potential differences in initial growing stock, growth rates and mortality. The use of basal area per tree as a response variable and covariate is recommended to help overcome these problems. Any covariance analysis should be augmented by allowing separate regression lines for different treatments, and adjusted yields should be calculated within thinning classes. If periodic data are available, an alternative analytic method is to model plot yields by projection equations and then analyse the estimated parameters by covariance techniques.

Acknowledgments

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APPENDIX 2

Estimators for α and β in model (4.1)

$$(x'X)^{-1} = \begin{vmatrix} \Sigma X_f^{2}/n\Sigma x_f^{2} & 0 & -\overline{X}_f/\Sigma x_f^{2} & 0 \\ 0 & \Sigma X_c^{2}/\Sigma x_c^{2} & 0 & -\overline{X}_c/\Sigma x_c^{2} \\ -\overline{X}_f/\Sigma x_f^{2} & 0 & 1/\Sigma x_f^{2} & 0 \\ 0 & -\overline{X}_c/\Sigma x_c^{2} & 0 & 1/\Sigma x_c^{2} \end{vmatrix} \qquad (1)$$

where $\Sigma x_i^{2} = \Sigma X_i^{2} - (\Sigma X_i)^{2}/n$ in (1)

As well, using $\hat{\underset{\sim}{\beta}} = (X'X)^{-1}X'y$, it follows

$$\hat{\alpha}_f = ([\Sigma X_f^{2}\Sigma Y_f]/n\Sigma x^{2}_f) - \overline{X}_f\Sigma[X_fY_f]/\Sigma x^{2}_f$$

$$\hat{\alpha}_c = ([\Sigma X_c^{2}\Sigma Y_c]/n\Sigma x_c^{2}) - \overline{X}_c\Sigma[X_cY_c]/\Sigma x^{2}_c$$

which is equivalent to

$$\hat{\alpha}_f = \overline{Y}_f - \hat{\beta}_f\overline{X}_f \text{ and } \hat{\alpha}_c = \overline{Y}_c - \beta_c\overline{X}_c \qquad (2)$$

and

$$\hat{\beta}_f = (\Sigma X_fY_f - [\Sigma X_f\Sigma Y_f/n])/(\Sigma X_f^{2} - [\Sigma X_f]^{2}/n)$$

$$\hat{\beta}_c = (\Sigma X_cY_c - [\Sigma X_c\Sigma Y_c/n])/(\Sigma X^{2}_c - [\Sigma X_c]^{2}/n) \qquad (3)$$

The algebraic form of (2) and (3) are therefore equivalent to the familiar formulae for the intercept and regression slope in straight-line regression, with the variance of $\hat{\beta}_f$ or $\hat{\beta}_c$ given by

$$V(\hat{\beta}_i) = \sigma^{2}/\Sigma x_i^{2}$$

and

$$V(\hat{\alpha}_i) = \sigma^{2}\Sigma X^{2}/n\Sigma x_i^{2}$$

APPENDIX 2.1

Calculations associated with the simulated data of Chapter 4.

Fertilized data		Control data	
Initial growing stock	Response Variable	Initial growing stock	Response Variable
3	27	6.5	27
5	28	10.0	26
9	31	12.5	28.5
8	34	15.5	31
11	36	18.0	30
15.5	41	23.5	33

$$X = \begin{vmatrix} 1 & 0 & 3 & 0 \\ 1 & 0 & 5 & 0 \\ 1 & 0 & 9 & 0 \\ 1 & 0 & 8 & 0 \\ 1 & 0 & 11 & 0 \\ 1 & 0 & 15.5 & 0 \\ 0 & 1 & 0 & 6.5 \\ 0 & 1 & 0 & 10 \\ 0 & 1 & 0 & 12.5 \\ 0 & 1 & 0 & 15.5 \\ 0 & 1 & 0 & 18 \\ 0 & 1 & 0 & 23.5 \end{vmatrix}$$

$$X'X = \begin{vmatrix} 6 & 0 & 51.5 & 0 \\ 0 & 6 & 0 & 86 \\ 51.5 & 0 & 540.25 & 0 \\ 0 & 86 & 0 & 1415 \end{vmatrix}$$

$$(X'X)^{-1} = \begin{vmatrix} 0.91\ 684\ 345 & 0 & -0.08\ 739\ 924 & 0 \\ 0 & 1.29\ 341\ 865 & 0 & -0.07\ 861\ 060 \\ -0.08\ 739\ 924 & 0 & 0.01\ 018\ 244 & 0 \\ 0 & -0.07\ 861\ 060 & 0 & 0.00\ 548\ 446 \end{vmatrix}$$

$$X'y = \begin{vmatrix} 197.0 \\ 175.5 \\ 1803.5 \\ 2587.75 \end{vmatrix}$$

$\hat{\underset{\sim}{\beta}} = (X'X)^{-1}X'y$, so

$\hat{\alpha}_f = \underline{22.9936}$ $\qquad$ $\hat{\alpha}_c = \underline{23.5704}$

$\hat{\beta}_f = \underline{1.1464}$ $\qquad$ $\hat{\beta}_c = \underline{0.3963}$

Residual SS $= y'y - \underset{\sim}{\beta}X'y$

$= 11\ 774.25 - 11\ 759.2333 = 15.0167$

therefore $s^2 = 15.0167/8 = 1.8771$ with 8 df

$H_o{:}\beta_f = \beta_c$ is tested by

$(1.1464 - 0.3963)/\surd(1.8771\ [0.010\ 182 + 0.005\ 484]) = 4.37 \sim t_{(8)}$

which is rejected at the 0.005% level.

The grand initial growing stock mean is 11.46 units, giving estimated yields at this point of

$\overline{Y}_f = \underline{36.1}$ and $\overline{Y}_c = \underline{28.1}$ units

with response = 8.0 units

H_o: $E(Y_f/\overline{X}) = E(Y_c/\overline{X})$

$(36.1 - 28.1)/\surd(1.8771\ [0.2506 + 0.2119]) = 8.58 \sim t_{(8)}$

and H_o is rejected at the 0.1% level.

APPENDIX 3

Derivation of regression coefficients and residual sum of squares, for yield and growth variables.

(a) Given a model

$$Y = \alpha + \beta X \tag{1}$$

then the least-squares estimates of α and β can be obtained by the formulae

$$\hat{\alpha} = \bar{Y} - \beta\bar{X} \tag{2}$$

and

$$\hat{\beta} = (\Sigma XY - [\Sigma X\Sigma Y]/n)/(\Sigma X^2 - [\Sigma X]^2/n) \tag{3}$$

$$= \Sigma xy/\Sigma x^2$$

Now growth can be estimated by $(Y - X)$, which substituted into (3), gives

$$\begin{aligned} \hat{\beta}_T &= (\Sigma X(Y-X) - (\Sigma X\Sigma[Y-X])/n)/(\Sigma X^2-[\Sigma X]^2/n) \\ &= (\Sigma XY - \Sigma X^2 - [\Sigma X\Sigma Y]/n + [\Sigma X]^2/n)/(\Sigma X^2 - [\Sigma X]^2/n) \end{aligned}$$

$= \hat{\beta} - 1$, where β_T is the regression coefficient associated with the growth response variable. Similarly, α_T, the corresponding intercept is given by

$$\begin{aligned} \hat{\alpha}_T &= (\bar{Y} - \bar{X}) - \hat{\beta}_T\,\bar{X} \\ &= (\bar{Y} - \bar{X}) - (\hat{\beta} - 1)\,\bar{X} \\ &= \bar{Y} - \hat{\beta}\bar{X} = \hat{\alpha} \end{aligned}$$

That is, substitution of growth for yield as a response variable, decreases the regression coefficient by unity, but the intercept remains invariant to the transformation.

(b) The residual sum of squares associated with (1), RSS, is given by

$$\Sigma Y^2 - \hat{\alpha}\Sigma Y - \hat{\beta}\Sigma XY \qquad (4)$$

Given the transformation (Y-X) for Y, then the corresponding residual sum of squares, RSS_T, is

$$\Sigma(Y - X)^2 - \hat{\alpha}\Sigma(Y-X) - (\hat{\beta}-1)\Sigma X\Sigma(Y-X) \qquad (5)$$

Expanding (5), and cancelling, we obtain

$$(\Sigma Y^2 - \hat{\alpha}\Sigma Y - \hat{\beta}\Sigma XY) \qquad (6)$$

$$+ \quad (\hat{\alpha}\Sigma X - \Sigma XY + \hat{\beta}\Sigma X^2) \qquad (7)$$

but $\Sigma Y = n\hat{\alpha} + \hat{\beta}\Sigma X$, so (7) is equal to

$$(\Sigma Y - \hat{\beta}\Sigma X)/n\Sigma X - \Sigma XY + \hat{\beta}\Sigma X^2$$

$$= (\Sigma X\Sigma Y)/n - \hat{\beta}(\Sigma X)^2/n - \Sigma XY + \hat{\beta}\Sigma X^2$$

$$= \hat{\beta}(\Sigma X^2 - [\Sigma X]^2/n) - \Sigma XY + (\Sigma X\Sigma Y)/n$$

$$= 0 \quad \text{(using (3))}$$

giving $RSS = RSS_T$, so the residual sum of squares is invariant to the transformation (Y-X).

APPENDIX 4

Treatments used in Waring's species x fertilizer experiment:

1. Unfertilized control;
2. 160 kg/ha rock phosphate;
3. 400 kg/ha rock phosphate;
4. 890 kg/ha rock phosphate;
5. 890 kg/ha rock phosphate, sulphur, and trace elements;
6. 890 kg/ha rock phosphate, 140 kg N/ha, in the form of urea;
7. 890 kg/ha rock phosphate, 140 kg N/ha, sulphur, potassium, and trace elements;
8. 890 kg/ha rock phosphate, 140 kg N/ha, and di-calcium phosphate;
9. 890 kg/ha rock phosphate, 140 kg N/ha, sulphur, potassium, di-calcium phosphate, and trace elements;
10. 890 kg/ha rock phosphate, 140 kg N/ha, sulphur, potassium, trace elements, and lime;
11. 890 kg/ha rock phosphate, 140 kg N/ha, sulphur, potassium, trace elements, di-calcium phosphate, and lime.

APPENDIX 5

Summary of N.Z.Forest Products Limited Fertilizer Experiments

Unless stated otherwise, all experimental plots in the trial series were of size 40 m x 40 m, with an inner 20 m x 20 m measurement plot. Fertilizer applications were applied in all cases, between August - late September, for any year.

TRIAL No.1: (Johnstone Road)

Established: 1967

Objective: Investigate whether 13-year old thinned *Pinus radiata* regeneration will respond to a composite fertilizer mix.

Experimental design: Completely randomised; 4 replications of two residual stockings, 620 and 370 stems/ha, fertilized, or not fertilized, giving 16 plots in total.

Fertilizers applied: The following fertilizers were applied in 1967, and reapplied in 1968, less trace elements. Quantities given, are per hectare:

250 kg urea (115 kg N)
125 kg blood and bone (8 kg N, 6 kg P)
625 kg superphosphate (62 kg P, 62 kg S, 125 kg Ca)
500 kg crushed dolomite (50 kg Mg, 110 kg Ca)
250 kg potash (130 kg K)

Trace elements

5.6 kg zinc oxide
5.6 kg manganese sulphate
11.2 kg copper sulphate
22.4 kg borax
175 g sodium molydate

In 1971, one replication of all treatments was dosed with:

250 kg urea
625 crushed dolomite
250 kg potash

Layout: Uniform site, altitude 450 m.

Redesign: In 1976, the 620 stems/ha plots were thinned from below to 306 stems/ha; no further fertilization was carried out.

Measurements available, for all trees

stem volume, o.b.	1967-1976
diameter breast height (tape)	1967-1976, 1985-1986
tree height (dendrometer)	1967-1976
spatial distances and bearings	

TRIAL No.2: (Wainui Road)

Established: 1969

Objective: Isolate active elements responsible for nutrient response, detected in Trial No.1.

Experimental design: Randomised complete block; 4 replications of a 2^3 factorial arrangement of treatments. Factors:

N - urea, blood and bone
P - superphosphate
Mg,K - dolomite and potash

with rates of application, per hectare, identical to those given for Trial No.1. Basal dressing of trace elements, applied to all plots, identical to those given for Trial No. 1.

Fertilizer applications: 1969, and reapplied in 1970, at identical rates, less the trace elements.

Layout: Established in 14-year-old *Pinus radiata* regeneration, in residual stockings between 420 and 543 stems/ha. Thinning of the plots took place 12 months before fertilization, in September 1969. Topography rolling to flat, altitude 335-380 m.

Measurements available, for all trees

stem volume, ob.)
diameter breast height (tape)) 1969-1971
tree height (dendrometer))
spatial distances and bearings

TRIAL No. 3 **(Rata Road)**

Established: 1970

Objective: Investigate rates of application of the composite fertilizer applied at Trial No.1.

Experimental design: Randomised complete block; 4 replications of 5 treatments,

Treatment A - control, no fertilizer
B - 25% of the mixed fertilizer, given for Trial No.1
C - 50% of the mixed fertilizer
D - 75% of the mixed fertilizer
E - 100% of the mixed fertilizer

Layout: Established in 13-year-old *Pinus radiata* regeneration, thinned to 494 stems/ha. Topography, uniformly flat, altitude 280 m. Fertilizer applied, 1970.

Measurements available, for all trees

stem volume, o.b.)
diameter breast height (tape)) 1970-1972, 1976
tree height (dendrometer))
spatial distances and bearings

TRIAL No. 4 (Owen Road)

Established: 1971

Objective: Investigate rates of application of nitrogen, with basal dressing of magnesium and potash.

Experimental design: Completely randomised; 4 replications of four treatments.

Treatment A - control, no fertilizer
B - 250 kg urea/ha
C - 500 kg urea/ha
D - 750 kg urea/ha

Treatments, B, C and D also received dressing of:

500 kg dolomite/ha
250 kg potash/ha

Fertilizers applied, 1971, and reapplied 1972, at identical rates, with urea only. No trace elements applied.

Layout: Established in 13-year-old *Pinus radiata* newland planting, thinned to 494 stems/ha. Topography steep, altitude 275-320 m, with a southerly aspect. Affected by wind-damage, 1973, reducing average stocking to 432 stems/ha.

Measurements available, for all trees

stem volume, o.b.	1971-1973
diameter breast height (tape)	1971-1974
tree height (dendrometer)	1971-1973

TRIAL No. 5: (Plateau Road)

Established: 1973

Objective: Isolate active elements

Experimental design: Randomised blocks, with confounding of treatments. 2 replications of a 2^4 factorial in 4 blocks, third order interaction totally confounded [Cochran and Cox (1966), p.234]. Factors:

N - 250 kg urea/ha
P - 625 kg superphosphate/ha
Mg - 500 kg dolomite/ha
K - 250 kg potash/ha

Layout: Established in 13-year-old *Pinus radiata* regeneration. Thinned to 600-620 stems/ha. Topography rolling to steep, various aspects. Altitude 335 to 400 m. Experiment ruined by aerial top-dressing, 1974.

Measurements available, on all trees

stem volume, o.b.	)	
diameter breast height (tape)	)	1973 - 1974
tree height (dendrometer)	)	

TRIAL No. 6: (Hioho Road)

Established: 1973

Objective: Isolate active elements

Experimental design: Randomised blocks, with confounding of treatments. 2 replications of a 2^4 factorial in 4 blocks, third order interaction totally confounded [Cochran and Cox (1966), p.234]. Factors:

N - 250 kg urea/ha
P - 625 kg superphosphate/ha
Mg - 500 kg dolomite/ha
K - 250 kg potash/ha

Layout and measurement: Established in 5-year-old *Pinus radiata*, resulting from regeneration and replanting. Plots 26.8 m x 26.8 m, with an inner 13.4 m x 13.4 m measurement plot. Stand underscrubbed from approximately 1500 stems/ha to 1180 stems/ha. Topography rolling to steep, altitude 400 to 450 m; various aspects. All experimental trees pruned to 2 m. Bole diameters of all experimental trees measured by calipers at 0.15, 1.4, 2.0, 3.0 m. Tree heights measured by aluminium extendable poles.

Measurements available, on all trees

stem volume, o.b.	1973-1975
diameter breast height (tape)	1973-1974 (data lost, 1975)
tree height (poles)	1973-1975

TRIAL No. 7: (Urea Road)

Established: 1974

Objective: Investigate rates and timings of application of nitrogen.

Experimental design: Randomised complete block; 4 replications of 4 treatments.

Treatment A - control, no fertilizer
B - 250 kg urea/ha, applied 1974
C - 500 kg urea/ha, applied 1974
D - 250 kg urea/ha, applied 1974, plus 250 kg urea/ha, applied 1975.

Layout: Established in 13-year-old *Pinus radiata* regeneration. Thinned to 494 stems/ha, but infection of *Dothistroma pini*, severely reduced increment of some suppressed trees, essentially reducing stockings to 437 stems/ha. Topography flat to rolling, altitude 300 to 340 m. One plot ruined by wind-throw, 1977.

Measurements available, on all trees

stem volume, o.b.	1974-1977, 1980
diameter breast height (tape)	
tree height (dendrometer)	

TRIAL No. 8: (Pepper Road)

Established: 1975

Objective: Investigate rates and timings of application of nitrogen.

Experimental design: Completely randomised; 5 replications of 4 treatments.

Treatment A - control, no fertilizer
B - 250 kg urea/ha, applied 1975, plus
250 kg urea/ha, applied 1976
C - 250 kg urea/ha, applied 1975, plus
500 kg urea/ha, applied 1977
D - 500 kg urea/ha, applied 1975

Layout: Established in 13-year-old *Pinus radiata*, resulting from regeneration. Topography rolling to steep, altitude 380 to 410 m; northerly aspect. Thinned to 494 stems/ha, but infection by *Dothistroma pini*, plus light physical damage, effectively reduced the average stocking to 398 stems/ha. One plot, ruined by wind-throw, 1980.

Measurements available, on all trees

stem volume, o.b.	1976-1978, 1980-1981
diameter breast height (tape)	1975-1978, 1980-1981, 1986
tree height (dendrometer)	1976-1978, 1980-1981
spatial distances and bearings	

APPENDIX 6

Summary of analysis from N.Z.Forest Products Limited Experiments

Notes

For all eight experiments, the data were analysed following the methodology of Chapters 4 and 5. In particular:

1. contingency - table analysis showed that in no case was mortality associated with fertilizer applications, and so data was prepared in terms of surviving trees at time of last available measurement.

2. response variables, and associated initial growing stock covariates, were calculated as trait/tree; specifically mean volume/tree, mean basal area/tree, mean top or average plot height, and mean stand form factor. Volumetric analyses are presented for Trials 4, 7 and 8 only; Woollons and Will (1975) essentially summarise the results for the other experiments.

3. a stand form-factor ($\bar{F}$) was defined and calculated from

$$\bar{F} = \frac{\text{volume/tree (overbark)}}{\text{(basal area/tree) (mean top height)}}$$

The following summary gives (for each Trial):

(a) the adopted model of any analysis;

(b) the associated ANOVA;

(c) estimated responses, through adjusted means.

N.B.: Subscripts denoting treatments are fully defined in Appendix 5.

A.(6.1) TRIAL No.1 (Johnstone Road)

A.(6.1.1) Basal area

Net stockings, 1976, are 571 and 357 stems/ha. Treatments are fertilized (f) or control (c).

1968: $E(Y) = \alpha_{571(c)} + \alpha_{571(f)} + \alpha_{357(c)} + \alpha_{357(f)} + \beta X$ (1)

Source	d.f.	SS(x10^6)	p > F
Covariate	1	190.40	0.0001
Intercepts	3	9.31	0.0013
Error	11	3.13	
	15		

Responses to fertilizer: 0.6 m^2/ha (571 stems/ha)
0.4 m^2/ha (357 stems/ha)

1969: Model, as for (1) (2)

Source	d.f.	SS(x10^6)	p > F
Covariate	1	249.31	0.0001
Intercepts	3	59.73	0.0068
Error	11	31.38	
	15		

Responses to fertilizer: 2.0 m^2/ha (571 stems/ha)
1.1 m^2/ha (357 stems/ha)

1970: $E(Y) = \alpha_{571 + 357(f)} + \alpha_{571 + 357(c)} + \beta_1 X + \beta_2 Z$ (3)

Z = plot competition

Source	d.f.	SS($\times10^6$)	p > F
Initial b.a.	1	291.66	0.0001
Plot comp.	1	66.67	0.0012
Intercepts	1	112.31	0.0001
Error	12	44.74	
	15		

Responses to fertilizer: 3.0 m^2/ha (571 stems/ha)

1.9 m^2/ha (357 stems/ha)

1976: Model, as for (3) (4)

Source	d.f.	SS($\times10^6$)	p > F
Initial b.a.	1	592.71	0.0006
Plot comp.	1	499.78	0.0012
Intercepts	1	473.48	0.0015
Error	12	335.24	
	15		

Responses to fertilizer: 6.2 m^2/ha (571 stems/ha)

3.9 m^2/ha (357 stems/ha)

1986: 571 stems/ha regime, thinned to 306 stems/ha 1976.

$$E(Y) = \alpha_{f(306 + 357)} + \alpha_{c(306 + 357)} + \beta X \qquad (5)$$

where X = basal area/tree, 1967, of residual trees
Y = basal area/tree, 1986

Source	d.f.	SS(c10^6)	p > F
Covariate	1	633.79	0.1759
Intercepts	1	978.13	0.0633
Error	13	336.50	
	15		

Responses to fertilizer: 4.8 m^2/ha (306 stems/ha)

5.6 m^2/ha (357 stems/ha)

Estimated component responses (1967-1986)

357 stems/ha

Response (1967-1976) = 3.9 m^2/ha

.˙. Response (1976-1986) = 1.7 m^2/ha

306 stems/ha (ex-571 stems/ha)

Thinning, 1976	14.4 m^2/ha	(fertilized)
	12.6 m^2/ha	(unfertilized)

.˙. fertilized wood removed = 1.8 m^2/ha

.˙. total response = 6.6 m^2/ha

A.(6.1.2) Height

No response detected in any year, for either mean top height, or mean plot height, at either stocking. Table (6.1.2) lists treatment mean height data through 1967-1976.

Table (6.1.2): Height development: Trial No.1

	571 stems/ha		357 stems/ha	
	Fertilized	Control	Fertilized	Control
	mean top height (m)			
1967	21.5	20.8	22.0	22.0
1968	23.2	22.6	23.6	23.9
1969	24.6	24.2	25.3	25.3
1970	26.0	25.7	26.7	26.8
1971	27.3	26.7	27.9	27.8
1972	28.4	28.3	29.3	29.3
1973	29.7	29.8	30.5	30.5
1974	31.0	31.6	31.9	32.2
1975	32.6	33.0	33.3	33.9
1976	33.8	34.1	34.3	34.9

mean plot height (m)

1967	20.7	20.0	20.7	20.5
1968	22.6	21.5	22.0	22.1
1969	23.8	23.1	23.7	23.5
1970	25.1	24.4	24.9	24.8
1971	26.4	25.4	26.0	26.0
1972	27.5	26.8	27.5	27.4
1973	28.8	28.0	28.8	28.7
1974	30.2	29.3	30.1	30.3
1975	31.6	30.7	31.4	31.8
1976	32.5	31.6	32.3	32.7

A.(6.1.3) Form-factor

1968-1976:

$$E(Y_k) = \alpha_f + \alpha_c + \beta_1 X_1 + \beta_2 X_2 \qquad (6)$$

where Y_k = stand form factor of year k

X_1 = stand form factor, 1967

X_2 = mean top height, 1967

Source	d.f.	SS_{68}	$p > F$	SS_{69}	$p > F$
Initial form factor	1	133.58	0.0063	367.75	0.0242
Initial top height	1	5.48	0.5162	196.96	0.0838
Intercepts	1	18.36	0.3152	299.85	0.0383
Error	12	146.71		608.17	
	15				

Source	d.f.	SS_{70}	$p > F$	SS_{71}	$p > F$
Initial form factor	1	340.87	0.0460	349.38	0.0068
Initial top height	1	576.30	0.0135	488.21	0.0023
Intercepts	1	424.17	0.0288	266.28	0.0146
Error	12	826.12		393.34	
	15				

Source	d.f.	SS_{72}	$p > F$	SS_{73}	$p > F$
Initial form factor	1	342.26	0.0394	130.19	0.1757
Initial top height	1	399.12	0.0281	450.82	0.0201
Intercepts	1	248.88	0.0722	104.04	0.2205
Error	12	768.55		754.52	
	15				

1974-1976: No significant difference in form-factor.

The development of stand-form factor, through 1968-1976 is shown in Figure (A.6.1).

A.(6.2) TRIAL No. 2 (Wainui Road)

A.(6.2.1) Basal area

1970:

$$E(Y) = b_0 + b_1N + b_2P + b_3NP + b_4Mg/K + b_5NMg/K + b_6PMg/K + b_7R + \beta_1X + \beta_2Z \quad (7)$$

R = effect of blocks
N = effect of nitrogen
P = effect of phosphorus
Mg/K = effects of magnesium or potassium
Z = stand competition index
X = initial basal area/tree

Source	d.f.	$SS(x10^6)$	$p > F$
N	1	4.02	0.0309
P	1	0.07	0.8991
NP	1	0.05	0.9311
Mg/K	1	3.07	0.0951
NMg/K	1	0.18	0.6305
PMg/K	1	0.04	0.9421
Initial b.a./tree	1	385.87	0.0001
Stand competition	1	2.77	0.0671
Blocks	3	4.28	0.1601
Error	20	14.92	
	31		

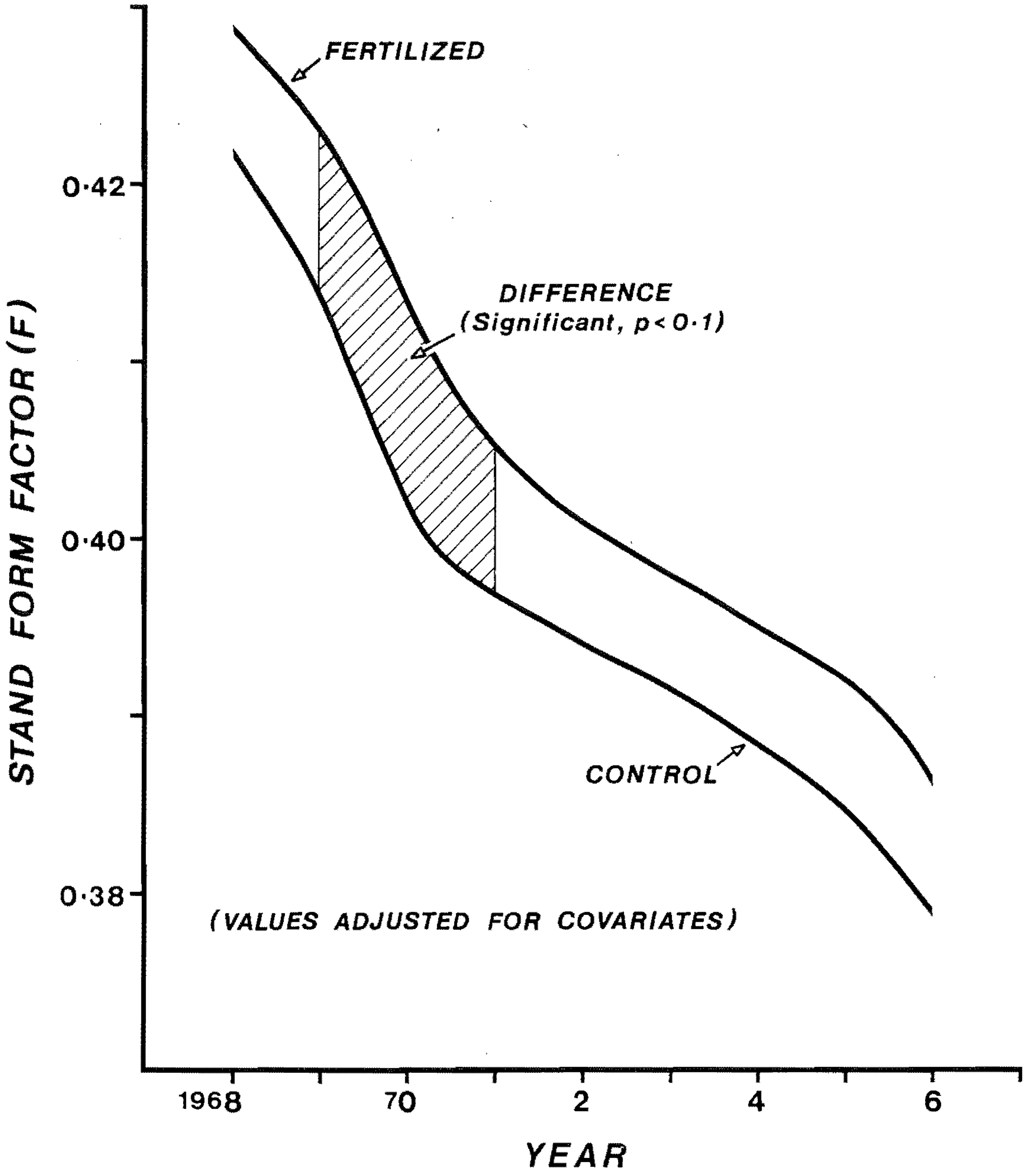

Figure (A.6.1) Trial No.1 (Johnstone Road). Development of stand form factor, through 1968-1976.

	n_0	n_1	(m^2/ha)
mg_0/k_0	34.3	34.6	
mg_1/k_1	34.6	34.9	

1971: Model: as for 1970

Source	d.f.	SS(x10)	p > F
N	1	19.59	0.0152
P	1	0.02	0.9352
NP	1	0.36	0.7242
Mg/K	1	8.06	0.0809
NMg/K	1	0.22	0.7827
PMg/K	1	0.03	0.9203
Initial b.a./tree	1	385.77	0.0001
Stand competition	1	7.62	0.0986
Blocks	3	20.69	0.0985
Error	20	55.59	
	31		

	n_0	n_1	(m^2/ha)
mg_0/k_0	36.3	37.0	
mg_1/k_1	36.8	37.6	

A.(6.2.2) Height

No response detected, in either year.

A.(6.2.3) Form factor

$$E(Y) = b_0 + b_1N + b_2P + b_3NP + b_4Mg/K + b_5NMg/K + b_6PMg/K + b_7R + \beta X \qquad (8)$$

Y,X = stand form factor, in 1971 and 1970

Source	d.f.	SS(x10^5)	$p > F$
N	1	10.62	0.0276
P	1	3.76	0.1734
NP	1	0.15	0.9299
Mg/K	1	7.69	0.0569
NMg/K	1	4.17	0.1527
PMg/K	1	1.09	0.4562
Initial form factor	1	132.06	0.0001
Blocks	3	10.65	0.1663
Error	21	39.78	
	31		

	n_0	n_1
mg_0/k_0	0.381	0.385
mg_1/k_1	0.384	0.389

1971: Model as for (8), except X transformed to ln(X) for curvilinearity; and plot competition added as a second covariate.

Source	d.f.	SS(x10^5)	$p > F$
N	1	16.02	0.0488
P	1	0.83	0.6558
NP	1	10.72	0.1198
Mg/K	1	30.16	0.0130
NMg/K	1	0.46	0.9165
PMg/K	1	6.18	0.2316
Ln(initial form factor)	1	113.04	0.0001
Plot competition	1	18.72	0.0383
Blocks	3	16.92	0.2748
Error	20	81.18	
	31		

	n_0	n_1
mg_0/k_0	0.382	0.388
mg_1/k_1	0.389	0.390

A.(6.3) TRIAL No. 3 (Rata Road)

A.(6.3.1) Basal area

1971: $E(Y) = b_0 + b_1Tr. + b_2R + \beta_1X + \beta_2Z$ (9)

Tr = effect of treatments

Source	d.f.	SS($\times 10^6$)	p > F
Initial b.a./tree	1	287.86	0.0001
Stand competition	1	1.85	0.0227
Blocks	3	0.83	0.3993
Treatments			
Linear effect	1	2.03	0.0182
Deviations	3	1.26	-
Error	10	2.56	
	19		

A	B	C	D	E	(m^2/ha)
32.7	32.8	32.8	33.0	33.1	

1972: Model, as for 1971

Source	d.f.	SS($\times 10^6$)	p > F
Initial b.a./tree	1	319.82	0.0001
Stand competition	1	4.56	0.0990
Blocks	3	1.60	0.7647
Treatments			
Linear effect	1	5.23	0.0624
Deviations	3	2.96	0.1736
Error	10	13.79	
	19		

A	B	C	D	E	(m^2/ha)
34.9	35.0	35.1	35.3	35.8	

1976: Model, as for 1971

Source	d.f.	SS($\times 10^6$)	p > F
Initial b.a./tree	1	247.92	0.0001
Stand competition	1	63.66	0.0115
Blocks	3	17.52	0.4870
Treatments			
Linear effect	1	36.50	0.0416
Deviations	3	7.77	0.3064
Error	10	63.86	
	19		

A	B	C	D	E	(m^2/ha)
43.9	43.8	43.6	44.1	45.4	

A.(6.3.2) Height

No response detected, in any year.

A.(6.3.3) Form-factor

No response detected, in any year

A.(6.3.2) Height

No response detected, in any year.

A.(6.3.3) Form-factor

No response detected, in any year.

A.(6.4) TRIAL No. 4 (Owen Road)

A.(6.4.1) Basal area

1972: No response detected

1973: $E(Y) = b_0 + b_1 Tr. + \beta X$ (10)

Source	d.f.	SS($\times 10^6$)	p > F
Initial b.a./tree	1	378.50	0.0001
Treatments			
Linear effect	1	7.06	0.0428
Deviations	2	0.17	-
Error	11	14.80	
	15		

A	B	C	D
31.7	32.0	32.3	32.5

(m^2/ha)

1974: Model, as for 1973

Source	d.f.	SS($\times 10^6$)	p > F
Initial b.a./tree	1	371.87	0.0001
Treatments			
Linear effect	1	4.49	0.0104
Deviations	2	1.17	-
Error	11	52.13	
	15		

A	B	C	D
34.0	34.5	35.4	35.9

(m^2/ha)

The trial is characterised by abnormally low growth, in the first year following thinning, suggesting a period of thinning shock [Evans 1982), Harrington and Reukema (1983)]. Treatment basal area/ha increments for 1971-1974 are (m^2/ha).

Treatment	1971-1972	1972-1973	1973-1974
A	1.8	3.0	2.3
B	1.5	3.1	2.5
C	2.0	3.6	3.1
D	2.1	3.6	3.0

A.(6.4.2) Volume

1972: No detectable response

1973: $E(Y) = \alpha_{A + B} + \alpha_{C + D} + \beta_{A + B}X + \beta_{C + D}X$

Source	d.f.	SS(x10^4)	p > F
Slopes	2	592.03	0.0001
Intercepts	1	10.75	0.0248
Error	12	19.62	
	15		

$H_0: \beta_{A + B} = \beta_{C + D}$ is tested by

$(1.3123 - 0.9398)/(\sqrt{[1.63 \times 10^{-4}\{52.90 + 40.47\}]})$

$= 3.03 \sim t_{(12)}$ $(p > 0.02)$

A + B	C + D	Response	(m^3/ha)
348	363	15	

1974: No data available

A.(6.4.3) Height

No response detected, in any year.

A.(6.4.4) Form-factor

1972-1973: Using form-factor, 1971 as a covariate, no differences are apparent. If form-factor, 1972 is utilised instead, then:

1973 $$E(Y) = b_0 + b_1 Tr. + \beta_1 \ln(X) \qquad (12)$$

Source	d.f.	SS(x10)	p > F
Ln (form factor, 1972)	1	189.02	0.0001
Treatments			
Linear effect	1	15.98	0.0325
Deviations	2	0.51	-
Error	11	5.24	-
	15		

A	B	C	D
0.400	0.401	0.405	0.409

A.(6.5) TRIAL No. 5 (Plateau Road)

A.(6.5.1) Basal area

1974 $$\begin{aligned} E(Y) = {} & b_0 + b_1N + b_2P + b_3NP + b_4Mg + b_5NMg \\ & + b_6PMg + b_7K + b_8NK + B_9PK + b_{10}MgK \\ & + b_{11}\ \text{Reps} + b_{12}\ \text{Blocks (within Reps)} \\ & + \beta X \end{aligned} \qquad (13)$$

Source	d.f.	SS(x10^6)	P > F
N	1	2.66	0.0014
P	1	0.06	0.9326
NP	1	0.08	0.7907
Mg	1	0.26	0.2549
NMg	1	0.25	0.2607
PMg	1	0.68	0.8826
K	1	0.73	0.0685
NK	1	0.09	0.8324
PK	1	0.12	0.7273
MgK	1	0.18	0.5137
Reps	1	0.21	0.3042
Blocks (reps)	2	0.39	0.3716
Initial b.a./tree	1	495.42	0.0001
Error	17	4.04	
	31		

(m^2/ha)

	n_0	n_1
k_0	36.6	36.9
k_1	36.7	37.1

A.(6.5.2) Height

No detectable response

A.(6.5.3) Form-factor

No detectable response

A.(6.6) TRIAL No. 6 (Hioho Road)

A.(6.6.1) Basal area

1974:

$$E(Y) = b_0 + b_1N + b_2P + b_3NP + b_4Mg + b_5NMg + b_6PMg + b_7K + b_8NK + b_9PK + b_{10}MgK + b_{11}Reps + b_{12}\text{ Blocks (within Reps)} + \beta X \quad (14)$$

Source	d.f.	SS($x10^4$)	p > F
N	1	16.66	0.0003
P	1	0.02	0.8681
NP	1	0.16	0.6620
Mg	1	0.29	0.5650
NMg	1	1.12	0.2588
PMg	1	1.43	0.2043
K	1	0.23	0.6007
NK	1	0.08	0.7548
PK	1	0.53	0.4300
MgK	1	3.09	0.0896
Reps	1	14.97	0.0005
Blocks (reps)	2	16.47	0.0013
Initial b.a./tree	1	222.60	0.0001
Error	17	14.01	
	31		

n_0	n_1	(m^2/ha)
11.55	12.40	

1975 Basal area data lost

A.(6.6.2) Volume

1974 Model, as for (15)

Source	d.f.	SS($x10^3$)	p > F
N	1	18.88	0.0042
P	1	0.59	0.5709
NP	1	0.35	0.6615
Mg	1	0.41	0.6358
NMg	1	0.02	0.9118
PMg	1	3.69	0.1648
K	1	0.66	0.5465
NK	1	0.36	0.6547
PK	1	0.24	0.7137
MgK	1	0.72	0.5287
Reps	1	9.48	0.0326
Blocks within Reps.	2	2.84	0.4610
Initial volume/plot	1	599.41	0.0001
Error	17	29.78	
	31		

n_0	n_1	Response	(m^3/ha)
42.2	44.7	2.5	

1975 Model, as for (14)

Source	d.f.	SS(x10^3)	p > F
N	1	56.82	0.0068
P	1	2.94	0.4931
NP	1	5.37	0.3573
Mg	1	5.61	0.3468
NMg	1	5.94	0.2970
PMg	1	1.82	0.5886
K	1	5.71	0.3426
NK	1	0.58	0.7595
PK	1	0.58	0.7589
MgK	1	6.79	0.3021
Reps	1	24.72	0.0583
Blocks (reps)	2	137.14	0.0007
Initial volume/plot	1	690.93	0.0001
Error	17	101.99	
	31		

n_0	n_1	Response	(m^3/ha)
61.0	65.8	4.8	

A.(6.6.3) Height

No detectable response

A.(6.6.4) Form-factor

No detectable response

A.(6.7) TRIAL No. 7 (Urea Road)

A.(6.7.1) Basal area

1975 No response detected

1976 $E(Y) = \alpha_A + \alpha_B + \alpha_C + \alpha_D + \theta Bl + \beta \ln(X)$ (15)

Bl = effect of blocks

Source	d.f.	SS(x10^6)	p > F
Initial b.a./tree	1	450.81	0.0001
Blocks	3	10.60	0.1114
Treatment	3	9.87	0.1270
Control v. Fert.	1	8.00	0.0369
Tr. c v Tr. D	1	1.62	0.2689
Error	8	10.23	
	15		

A	B	C	D	(m^2/ha)
32.9	33.5	33.5	33.9	

1977-1981 No response detected

A.(6.7.2) Volume

1975, 1976 No response detected

1977 $E(Y) = \alpha_A + \alpha_B + \alpha_C + \alpha_D + \theta Bl + \beta X$ (16)

Source	d.f.	SS(x10^4)	p > F
Initial vol/tree	1	863.45	0.0001
Blocks	3	45.28	0.1874
Treatments	3	101.38	0.0136
Error	7	56.90	
	14 (one plot lost)		

A	B	B	D	(m^3/ha)
427	428	453	447	

A.(6.7.3) Height

No response detected, in any year.

A.(6.7.4) Form Factor

No response detected, in any year.

ERRATA

A.(6.8) TRIAL No. 8 (Pepper Road)

A.(6.8.1) Basal area

1978: $E(Y) = \alpha_A + \alpha_B + \alpha_C + \alpha_D + \gamma Z + \beta X$ (17)

Source	d.f.	SS(x10^6)	p > F
Initial b.a./tree	1	110.76	0.0001
Plot competition	1	6.72	0.0908
Intercepts	3	20.39	0.0504
Error	14	28.54	
	19		

A	B	C	D	(m^2/ha)
32.1	33.2	32.5	32.9	

1981:

$E(Y) = \alpha_A + \alpha_B + \alpha_C + \alpha_D + \beta_{B+C} X + \beta_{A+D} X + \gamma_{B+C} Z + \gamma_{A+D} Z$ (18)

Source	d.f.	SS(x10^5)	p > F
Slopes	2	137.35	0.0001
Plot competition	2	3.33	0.0027
Intercepts	3	5.74	0.0008
Error	11	1.73	
	18		

$H_0 : \beta_{B+C} = \beta_{A+D}$ is tested by

$(1.6260 - 1.1227)/\sqrt{(1.57 \times 10^{-6} [4652 + 2399])}$

$= 3.83 \sim t_{(11)} \quad (p < 0.01)$

$H_0: \gamma_{B + C} = \gamma_{A + D},$ is tested by

$(-0.018\ 38 + 0.003\ 30)/\sqrt{(1.57 \times 10^{-6}[6.50 + 10.70])}$

$= 2.89 \sim t_{(11)} \quad (p < 0.05)$

1986:

$$E(Y) = \alpha_A + \alpha_{B + C} + \alpha_D + \gamma Z + \beta_{B + C}X + \beta_{A + D}X \qquad (19)$$

Source	d.f.	SS(x10^5)	p > F
Slopes	2	206.90	0.0001
Plot competition	1	3.91	0.0540
Intercepts	2	8.44	0.0587
Error	13	10.31	
	18		

$H_0: \beta_{B + C} = \beta_{A + D},$ is tested by

$(1.9063 - 1.2260)/\sqrt{(7.93 \times 10^{-6}[4652 + 2399])}$

$= 2.83 \sim t_{(13)} \quad (p < 0.025)$

A	B	C	D	(m^2/ha)
51.6	55.4	54.1	53.5	

A.(6.8.2) Volume

No initial volumes (1975) were measured, so the covariate, initial (basal area x height)/tree was used instead.

1976: No response detected.

1977: $E(Y) = \alpha_{A + C} + \alpha_{B + D} + \gamma Z + \beta X \qquad (20)$

Source	d.f.	SS(x10^3)	p F
Covariate	1	112.59	0.0001
Plot competition	1	1.99	0.0333
Intercepts	1	1.84	0.0396
Error	16	5.89	
	19		

A + B	C + D	Response	(m^3/ha)
296	304	8	

1978: $$E(Y) = \alpha_A + \alpha_{B + C + D} + \gamma Z + \beta X \qquad (21)$$

Source	d.f.	SS(x10^3)	p > F
Covariate	1	103.38	0.0001
Plot competition	1	7.38	0.0020
Intercepts	1	12.99	0.0002
Error	16	7.90	
	19		

A	B + C + D	Response	(m^3/ha)
336	363	27	

1981: Model, as for (22)

Source	d.f.	SS(x10^3)	p > F
Covariate	1	222.36	0.0001
Plot competition	1	18.31	0.0023
Intercepts	1	29.71	0.0003
Error	15	20.37	
	18		

A	B + C + D	Response	(m^3/ha)
510	550	40	

A.(6.8.3. Height

No response detected in any year.

A.(6.8.4) Form-factor

Initial volumes are not available; no response was detected in 1976, so form factor 1976 was used as a covariate instead.

1977-1981: $E(Y) = \alpha_A + \alpha_B + \alpha_{C + D} + \beta X$ (22)

Source	d.f.	$SS_{77}(x10^4)$	p > F	$SS_{78}(x10^4)$	p > F
Form factor, 1976	1	18.35	0.0001	16.78	0.0004
Intercepts	2	0.25	0.7434	3.09	0.1896
Error	15	6.24		12.46	
	18				

1980-81:

Source	d.f.	$SS_{80}(x10^4)$	$p \geq F$	$SS_{81}(x10^4)$	$p \geq F$
Form factor, 1976	1	9.84	0.0023	7.91	0.0022
Intercepts	2	4.45	0.0786	5.54	0.0254
Error	15	11.02		8.76	
	18				

The development of stand form factor through 1977-1981 is illustrated in Figure (A.6.2)

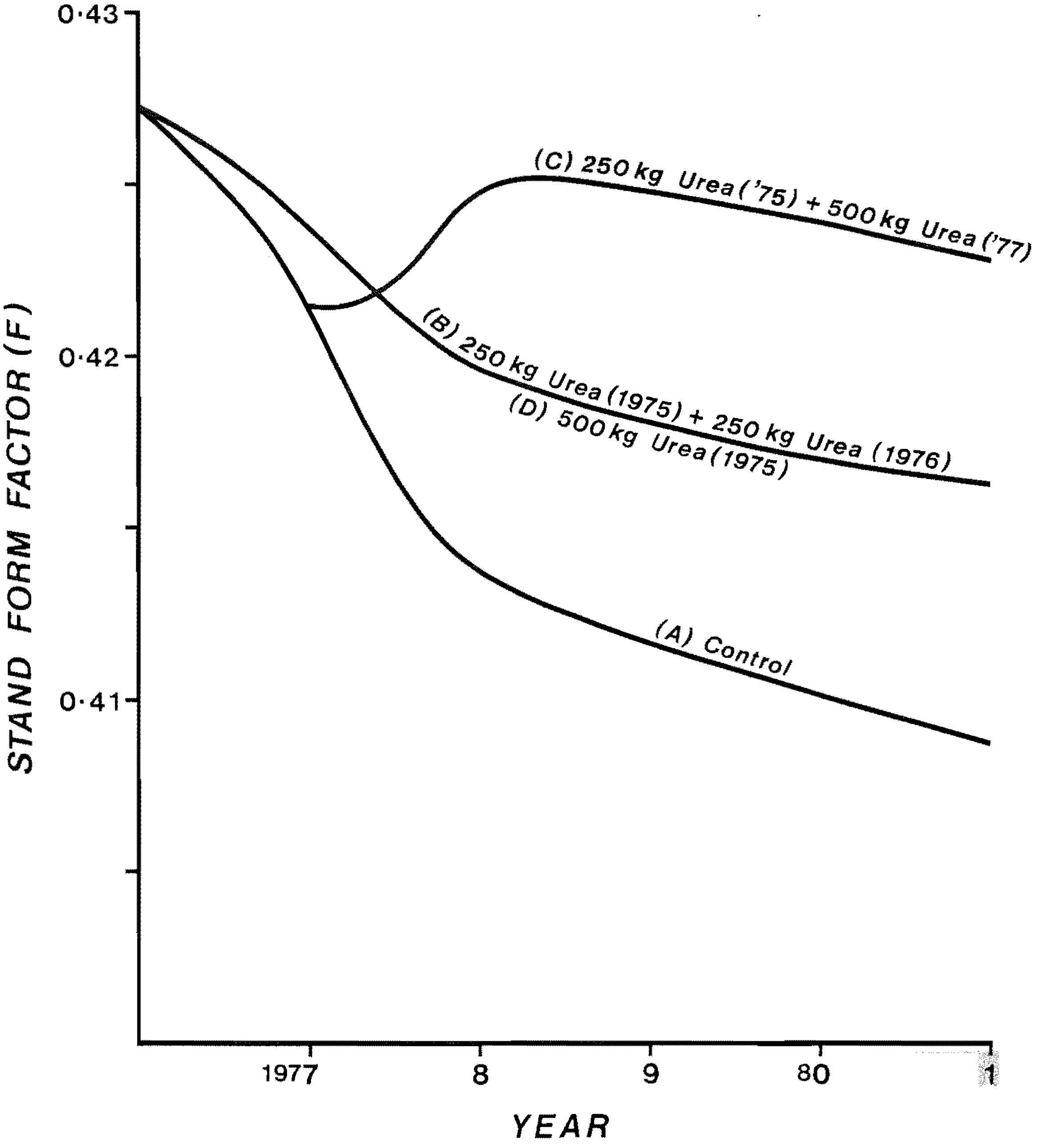

Figure (A.6.2) Changes in stand form-factor with treatment and time, at Trial No.8 (Pepper Road).

APPENDIX 7 Reprint of Woollons, R.C., and Hayward, W.J. (1985). Revision of a growth and yield model for radiata pine in New Zealand. For. Ecol. and Mangm 11, 191-202.

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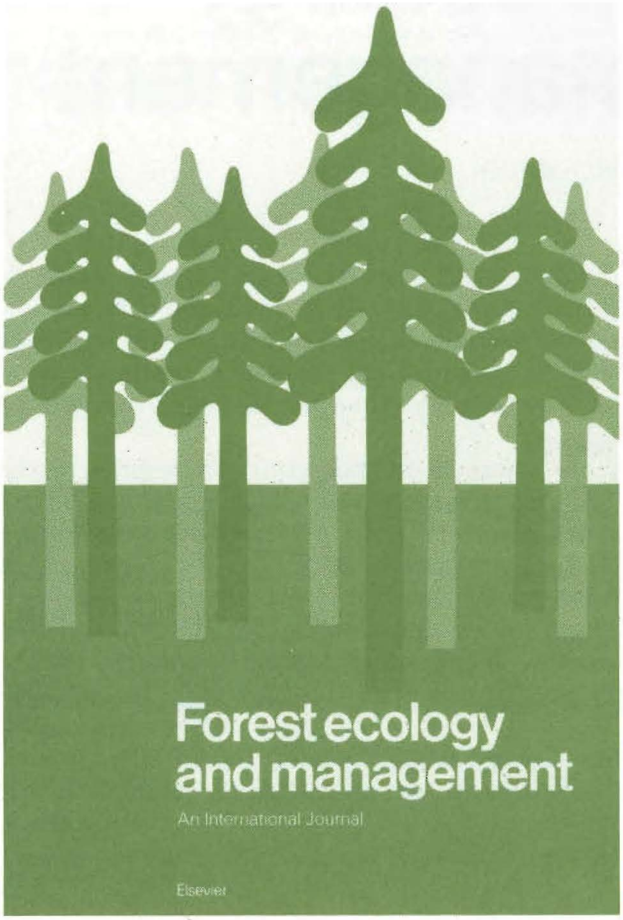

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ABSTRACT

Woollons, R.C. and Hayward, W.J., 1985. Revision of a growth and yield model for radiata pine in New Zealand. *For. Ecol. Manage.*, 11: 191—202.

This paper describes a major revision of a stand growth model for radiata pine belonging to N.Z. Forest Products Limited. Estimated growth and yields obtained from unthinned and specified thinned regimes are predicted from a stand level simulator. A fully compatible stock and stand table is also produced from single tree equations. The functions and methods used to calculate these statistics are described, and their rationale discussed.

INTRODUCTION

N.Z. Forest Products Limited currently owns 124 578 ha of *Pinus radiata* which comprise a major component of the pine plantations established on the central North Island plateau of New Zealand. These holdings are required to supply most of the wood to sustain the Company's diverse processing plant at Kinleith and outlying areas, currently running at about 2.4 million m^3/year.

This self-reliance requires the Company to maintain an intensive level of yield prediction and control. Earliest managers had the foresight to establish a series of permanent sample plots in the primary plantations. Excluding a period from 1937 to 1948, measurement of these plots has generally been sustained, while further plots have been regularly established in the subsequent second crop regeneration and recent newland plantings.

In 1969, the data base was sufficient to develop a computerised growth simulator as subsequently reported by Clutter and Allison (1974). At the time this model was quite novel, because

(a) it was not restricted to any predetermined regimes and so virtually any combination of thinning schedules could be simulated and the consequent yield estimated;

(b) appreciable use was made of non-linear regression models;

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(c) it had a considerable capacity of derive annual tree statistics, e.g., breast height diameter, stem height and volume.

Over the last decade this system has played a vital part in the Company's forest management, but in 1982 it was decided to update the model, with the objective of enhancing the prediction of second rotation crops. In this paper we give details of the revision, emphasising the formation of the component equations, and give a description of its working logic.

TABLE 1

Thinned and unthinned permanent sample plots included in revised growth model

Thinned

Thinning age (years)	Residual stems/ha						
	> 600	500	400	300	200	< 200	
< 6	1	1	1				
6	1		1				
8	4	2	4		6		
10	16		5	8	4		
12	12	4	4	5			
14	11	1	19	5	13		
16	6	2	11	4	8		
18	3		6	9	10		
20			1	8	4	1	
22			2		2		
> 22			4	3	2	4	
							(218)

Unthinned

Year of stand Establishment	Year of first measurement 19(. .)										
	56	64	66	68	70	72	74	76	78	80	
< 1952	6										
52						2					
54			2			3	1				
56			5			4					
58		2	9			4					
60											
62					7						
64					3	7					
66											
68							1				
70							12	13			
72								5	7		
74									7	4	
> 1974										5	
											(109)

DATA BASE

To construct the N.Z. Forest Products Limited 1983 growth simulator, 327 second crop sample plots altogether were available; 218 were thinned at some stage and the remainder were unthinned. Table 1 describes the coverage of data. The frequency of measurement was generally biennial or coincident with thinning. Plot area varies from 0.04 to 0.4 ha, measurement of diameter at breast height of all trees with a diameter tape, and a sample of heights with a clinometer were recorded on each occasion.

In addition, annual measurements were available for periods of from 2 to 9 years from untreated plots acquired from several thinned mid-rotation field experiments. The heights of all trees in plots of 0.04 ha were measured with Barr and Stroud dendrometers, and diameters at breast height were obtained by diameter tape. Relevant data were also obtained from more than 100 inventories carried out in the Company's forests at various ages. Plots of 0.05 or 0.1 ha were established 100 m apart, on surveyed lines 200 m wide. The diameters at breast height of all trees in the measurement plots were measured with diameter tape, and the heights of a subsample of trees were taken with a clinometer. Table 2 summarises the coverage of these data.

TABLE 2

Summary of inventory and experimental data incorporated in the revised growth model

Stand age (years)	Stocking (stems/ha) > 1400	1200—1399	1000—1199	500—999	300—499	200—299
10	19	21	20		1	
12				2	3	1
14				10	8	2
16				6	4	5
18					2	2
20					4	5
22					3	8
24						
						(126)

STATISTICAL METHODOLOGY

Alternative methods to ordinary least-squares regression for estimating growth and yield equation parameters have been commanding considerable attention recently. This is a consequence of permanent sample plot data being statistically correlated by virtue of having repeated measurements on the same set of trees. When such data are used in least-squares regression, residual errors and variances of regression coefficients are underestimated.

Alternative methodology has included maximum-likelihood estimates (Sullivan and Clutter, 1972), and generalised least squares (Ferguson and Leech, 1978; Davis and West, 1981). West et al. (1984) have reviewed this problem and described a number of attempts at solutions.

Alternatives to least-squares regression that may be appropriate in some circumstances were not considered necessary for the N.Z. Forest Products Limited growth model revision, because

(a) the model equations constructed were limited to relationships between dependent and predictor variables that were well established; for example, top height and age, or basal area/ha at maturity as a function of initial basal area/ha and age;

(b) only one function had more than two estimated parameters, and it was limited to three.

Accordingly, almost no hypothesis testing was required to establish the various functions in the model. Instead, comprehensive use was made of graphical plottings of residual and predicted or independent variables, searching for signs of bias in prediction by virtue of systematic patterns in the plots (Draper and Smith, 1981).

An exception to this methodology was in the formation of a top-height equation which included stand altitude as an independent variable; here a substantial amount of non-correlated inventory data were included in the modelling set.

Although some of the results may be slightly biased, the final set of model equations contained, in fact, only non-linear regression estimates which were all significantly different from zero according to the asymptotic confidence intervals at the 95% level (Bard, 1974), or ordinary least-squares estimates with associated t-values of at least 12. (Thus, even if the latter variance estimates are badly underestimated, the various parameters are still highly likely to be significantly different from zero.)

Consequently, given appropriate data of sufficient quantity, problems of correlated data can largely be side-stepped. We see a clear distinction, for example, between an equation

$$\ln(H) = \alpha_1 + \alpha_2/A \qquad \text{(A)}$$

and a set discussed by West (1981)

$$\delta d = b_1 + b_2\,(D - 10)^2$$

and

$$\left.\begin{aligned} b_1 &= p_{11} + p_{12}\,T^{-3} + p_{13}\,N \\ b_2 &= p_{21} + p_{22}\,e^{-T} + p_{23}\,N \end{aligned}\right. \qquad \text{(B)}$$

where, H = stand top height; T = stand age; δd = average annual diameter increment; D = tree diameter at the beginning of the increment period; N = stand stocking; and p_{ij} = regression coefficients.

Equation (A) is clearly a log reciprocal equation of top height against age, a relationship well known and functionally established for even-aged coniferous species. No significance tests for the regression coefficients are really required, and ordinary least-squares regression provides unbiased estimates of the coefficients α_1 and α_2 (Kendall and Stuart, 1961).

Conversely, the set of equations in (B) is considerably more complex, and we would agree with West (1981) that "it would be inappropriate to substitute (b_1 and b_2) back into (δd) and use ordinary least-squares regression, since this would underestimate the variance of the parameter estimates". Such a procedure would involve the estimation of six parameters, some involving interactions which are not obviously related to diameter increment. In this instance the use of OLS regression might incur a significant error.

THE REVISED GROWTH MODEL

In the following sections we describe the various functions constructed for the N.Z. Forest Products Limited revised growth model. The order of the presented equations approximate their successive use from the entry of input data to the output of predicted growth and yield data at clearfell.

Initiating a simulation

Use of the 1983 simulator requires the user to enter a 1- or 2-cm diameter breast height distribution (stand table) at any age between 5 and 35 years; the smaller class width is preferable at young ages. To this distribution the Weibull 3-parameter probability density function (Bailey and Dell, 1973)

$$f(x) = c/b\ (x - a)^{c-1} \exp(-\frac{x - a}{b})^{c} \tag{1}$$

is fitted. The location parameter, a, is estimated by equating it to the lower limit of the first non-zero diameter class in the input distribution. The scale and shape parameters, b and c, are obtained by using moment estimators (Bailey, 1972). In the earlier version of the model, this required the interative solution of

$$\frac{s}{\bar{x}} = \frac{\{\Gamma(1 + 2/\hat{c}) - \Gamma^2(1 + 1/\hat{c})\}^{0.5}}{\Gamma(1 + 1/\hat{c})} \tag{2}$$

where, s = the sample standard deviation; $\bar{x}$ = the sample mean; Γ = the gamma function; and s, $\bar{x}$ are obtained from the diameter distribution (Cohen, 1965). We avoid this numerical calculation in the 1983 revision by using an approximate analytical solution suggested by Garcia (1981).

The cumulative density function of (1) is well known to be

$$\mathrm{F}(x) = 1 - \exp(-\frac{x-a}{b})^{c} \tag{3}$$

from which any diameter class relative frequency can be readily derived.

Initial stand statistics

The initial stems/ha (N_1) and basal area/ha (G_1) are calculated directly from the stand table, and stand top height, defined as the average height of the largest 100 dbhob's/ha, is estimated by either

$$\ln(H) = 4.1931 - 13.8217/T \tag{4}$$

or

$$\ln(H) = 4.1055 - 13.777/T + 25.791/\mathrm{Alt} \tag{5}$$

where, H = stand top height in metres; T = stand age in years; and Alt = stand altitude in metres.

The inclusion of altitude is an occasional useful adjunct; top height can be reduced by approximately 25 cm for each 100 m increase in elevation. This variable may be regarded as a measure of site index, but its effect on top height is not particularly marked in practice and the residual mean square of (5) relative to (4) is only decreased by 13%. The forests which the model simulates lie in an altitude range of 700 m, but the upper part of this range (800 m) applies only to a very small proportion of the forest area (1200 ha). However, a traditional expression of site index is not required for the Company's holdings, an issue which simplifies modelling of height growth, and constructing a simulator overall. The log-reciprocal models (4) and (5) give excellent residual plottings, and are independent of stocking at any age.

Volume per hectare is calculated indirectly from the equation

$$F = V/GH = 0.323 \exp(-3.2109/T^{1.5767}) \tag{6}$$

where, V = volume/ha, and other symbols are as defined previously. This equation is noteworthy in that it estimates stand form factor (F) through time.

Initial tree statistics

Tree heights are allocated to the mid-point diameters of the initially specified stand table by the relative height equations

$$h/H = 0.7997 + 0.4759 \ln[\mathrm{F}(d) + 1] - 10^{-7} \times 49NT \quad \text{(unthinned)} \tag{7a}$$

or

$$h/H = 0.8853 + 0.2832 \ln[\mathrm{F}(d) + 1] - 10^{-7} \times 58NT \quad \text{(thinned)} \tag{7b}$$

where, h = total stem height in metres; N = stems/ha at age T; and $F(d)$ = Weibull cumulative distribution function value associated with the dbh value d. (The addition of 1 avoids an undefined logarithmic solution.)

Equation (7b) is developed from data in which

(a) a thinning has taken place a priori with at least 200 stems removed; and

(b) fewer than 600 stems/ha remain.

To illustrate the use of the relative height equations, suppose we have a diameter distribution of 1251 stems/ha at age 10 with estimated Weibull parameters of: $\hat{a}$ = 6.0; $\hat{b}$ = 12.23; $\hat{c}$ = 2.4207. Then, for a class mid-point of 7 cm, and using Eqn. (3), we have

$$F(7) = 1 - \exp(-\frac{(7-6)^{2.4207}}{12.23}) = 0.0023$$

so from Eqns. (4) and (7a)

$$h = 16.6 \times [0.7997 + 0.4769 \ln(1.0023) - (\frac{49 \times 1251}{1000\,000})] = 12.3 \text{ m}$$

Tree volumes are calculated from an appropriate two-dimensional volume equation for pumice North Island *Pinus radiata* (C. Goulding, personal communication, 1975).

Projection of stand statistics

Basal area/ha. Future net basal area/ha is estimated by either the equation

$$\ln(G_2) = \ln(G_1)\,(T_1/T_2)^{1.4728} + 4.3004\,[1 - (T_1/T_2)^{1.4728}] \quad \text{(unthinned)} \qquad (8a)$$

or

$$\ln(G_2) = \ln(G_1)\,(T_1/T_2)^{0.8234} + 5.06231\,[1 - (T_1/T_2)^{0.8234}] \quad \text{(thinned)} \qquad (8b)$$

where, G_1, G_2 represent the initial and the projected basal area, and T_1, T_2 the initial and projection ages. Net basal area refers to the alive basal area of trees after mortality, through competition and other physical reasons. Thinned is defined as per the criteria given for Eqn. (7b). These equations are key functions to the system and require a more detailed description. The functional form may be recognised as a non-linear variant of the projection equation utilised by Clutter (1963). It is derived by integrating the growth function

$$\frac{dG}{dT} = GT^{-1}\,[b_0 - b_2 \ln(G)] \qquad (9)$$

which in turn is obtained by differentiation of the yield equation

$$\ln(G) = b_0/b_2 + b_1/T^{b_2} \qquad (10)$$

The net basal area/ha yield, growth and projection equations are thus mathematically compatible. The advantages of this system have been fully detailed by Clutter (1963). Algebraically, we note from (8)

(a) when $T_1 = T_2$, $G_1 = G_2$;
(b) when $T_2 \rightarrow \infty$, $\ln(G_2) \rightarrow b_0/b_2$;
(c) a projection from T_1 to T_3 gives an identical result to projecting from T_1 to T_2, then T_2 to T_3 (where $T_1 < T_2 < T_3$).

With these properties it is possible to calculate directly an unthinned basal area estimate at clearfall age, without resorting to an iterative or annual solution. Similarly, if a single thinned simulation is requested, and the model is entered with a post-thinned stand table, then clearfell projections can also be estimated explicitly. Simulation of multiple thinnings require more protracted calculations, but direct projections between thinning ages are still available.

Stand mortality. Creation of an adequate model to predict stems/ha death was by no means straightforward, and in passing we would concur with the comments of Glover and Hool (1979): "It is fair to state that mortality probably will never be perfectly predicted because of forest ecosystem complexity and uncertainty of future conditions".

Our approach was to build a projection equation model that predicted N_2 as a function of N_1, T_1 and T_2, where, N_2 = stems/ha alive at age T_2; and N_1 = stems/ha alive at age T_1.

After considerable experimentation a function

$$N_2 = 200 + \{(N_1 - 200)^{-1.1162} + 10^{-9} \times 6.43\,(T_2^{3.4764} - T_1^{3.4764})\}^{0.8959} \qquad (11)$$

was developed, which adequately models the data. Equation (11) is a modification of that used by Clutter and Jones (1980) to examine mortality of *Pinus elliottii.*

From (11) we note:
when $T_1 = T_2$, $N_2 = N_1$, and
when $T_2 \rightarrow \infty$ $N \rightarrow 200$

and analogous to Eqn. (8), a direct projection from T_1 to T_3, is equivalent to projections from T_1 to T_2, then T_2 to T_3. Eqn. 11 is derived from the mortality rate equation

$$\frac{\mathrm{d}N}{\mathrm{d}T} = \frac{bc}{a}(N - 200)^{(1 - a)}T^{(c - 1)} \qquad (12)$$

with $b, c > 0$ and $a < 0$, which corresponds to the general mortality equation

$(N - 200)^{-a} = bT^{c}$

Projection of the initial diameter distribution

Each diameter class mid-point in the initial stand table is given a relative basal area (R_i) by

$$R_i = 0.00007854\, d_i^2/\bar{g}_1 \qquad (13)$$

where $\bar{g}_1 = G_1/N_1$. The expected mortality proportion (p_i) in any diameter class occurring in a projection period is given by

$$p_i = 1.0/(1.0 + R_i^{\,2.7367}\mathrm{e}^{\,0.4074R_i}) \qquad \text{(unthinned)} \tag{14a}$$

or

$$p_i = 1.0/(1.0 + R_i^{\,1.7872}\mathrm{e}^{\,1.0992R_i}) \qquad \text{(thinned)} \tag{14b}$$

It follows that the predicted mortality in number of stems in the ith diameter class is

$$m_i = n_{1i}/(1.0 + R_i^{a}\mathrm{e}^{bR_i}) \tag{15}$$

where, n_{1i} = number of stems/ha in the ith diameter class at age T_1; m_i = the predicted mortality in stems/ha for any diameter class during the projection period; and a, b = parameters defined in (14a) or (14b)

The proportion of total stem death (p_i) that occurs in the ith diameter class is

$$p_i = m_i/\Sigma m_i \tag{16}$$

where the summation implies a total over all non-zero diameter classes giving

$$n_{2i} = n_{1i} - p_i\,(N_1 - N_2) \tag{17}$$

where n_{2i} = the predicted number of trees in a diameter class at age T_2.

At this point, $\bar{g}_1$ and R_{1i} values are recalculated for surviving trees only (let $\bar{g}_1^*$ and R_{1i}^* be notation for these adjusted values). The appropriate computations are

$$\bar{g}_1^* = \Sigma n_{2i} g_{1i}/\Sigma n_{2i} \tag{18}$$

and

$$R_{1i}^* = g_{1i}/\bar{g}_{1i}^* \tag{19}$$

where g_{1i} is the corresponding basal area resultant from any diameter class.

Previously obtained values for G_2 and N_2 can be used to calculate the average basal area per tree at age T_2 as

$$\bar{g}_2 = G_2/N_2 \tag{20}$$

and projected values for the R_{2i} relative basal area figures can be obtained from

$$R_{2i} = (\bar{g}_1^*/\bar{g}_2)^{0.3494}\{R_{1i}^* + 0.00012/\bar{g}_1^*(T_2 - T_1)\} \qquad \text{(unthinned)} \tag{21a}$$

or

$$R_{2i} = (\bar{g}_1^*/\bar{g}_2)^{0.1473}\{R_{1i}^* + 0.00032/\bar{g}_1^*(T_2 - T_1)\} \qquad \text{(thinned)} \tag{21b}$$

It is advantageous to scale the R_{2i} values so that $\bar{R}_2 = 1$. This is achieved by computing

$$W = \Sigma n_{2i} R_{2i} / \Sigma n_{2i} \tag{22}$$

and then calculating the scaled relative basal areas (R'_{2i}) as

$$R'_{2i} = R_{2i}/W \tag{23}$$

The g_{2i} and d_{2i} values for that dbh class are readily calculated from $\bar{g}_2$ and R'_{2i} values. Since $\bar{R}'_2 = 1$, the weighted average of the g_{2i} figures will equal G_2/N_2.

The previously described Weibull estimation procedure is again used to fit a distribution to the projected diameters. By utilising moment estimators, the fitted distribution will have population moments 1 and 2 equal to the sample moments 1 and 2 this ensures the average basal area/tree equals G_2/N_2. Frequencies by 2-cm diameter classes can be calculated from (3).

Heights by diameter class (h_{2i}) can now be calculated using class midpoints and incorporating Eqn. 7 in either form. Volumes by diameter class (V_{2i}) are estimated from

$$V_{2i} = V_2(n_{2i}g_{2i}h_{2i})/\Sigma(n_{2i}g_{2i}h_{2i}) \tag{24}$$

where V_2 is obtained from Eqn. (6). The volume per tree in class i is calculated as V_{2i}/n_{2i}.

After the above sequence of computations a projected stock and stand table is available that is fully compatible with the projected stand statistics.

DISCUSSION

Much recent literature has been concerned with the construction of growth models. It has become common practice to classify a specific simulator as being one of three growth model groupings defined by Munro (1974):

(1) distance dependent tree-level models;
(2) distance independent tree-level models;
(3) stand-level models.

The 1974 N.Z. Forest Products Limited simulator contains characteristics of both stand- and tree-level models. For example, future volume/ha is estimated by the summation of projected stand table frequencies multiplied by tree volumes. Conversely, the stand table projection is centred on the estimation of gross basal area/ha increment and prediction of basal area/ha mortality which are direct stand-level processes.

The 1983 N.Z. Forest Products Limited growth model is also a dual simulator, but it includes a number of features which collectively represent a substantial improvement over the earlier model. A major refinement is the presence of explicit models for net basal area/ha thus avoiding the need of stand and basal area mortality equations to predict basal area and volume yield and growth. Stem and basal area death often exhibit considerable variation, and are frequently difficult to model with adequate precision (Clutter and Allison, 1974; Glover and Hool, 1979; West, 1981). In the 1974 model, mortality was required to be estimated as part of a cumbersome and implicit procedure for deriving volume/ha projections. In the current system, a model of basal area mortality is not needed at all while the equation for stem mortality is completely independent of any stand yield projections.

A second enhancement is the general use of projection equations in the simulator, which significantly improve computer efficiency when using the model. Predictions of growing stock at clearfell can be obtained in one age step for some regimes whereas the 1974 system is limited to annual iterative calculations. The structure of the projection equations ensures that predictions are invariant to the number of age steps requested in any simulation run.

Another improvement is the production of stock and stand tables at any age, which are independently derived by tree-level equations, while maintaining a strict compatibility with the stand statistics. The previous simulator also produces annual tree-level statistics, but does not produce projected tree frequencies by diameter classes.

SOME CONCLUDING REMARKS

In this paper we have attempted to describe the rebuilding of the N.Z. Forest Products Limited growth simulator, concentrating on the formation of the component equations and the rationale behind their construction. To keep the study within a reasonable size, several topics have been omitted: (a) the simulation of thinned regimes diameter distributions; (b) prediction of log sizes from the projected stand table; and (c) computerisation of the simulator.

We are also aware that nothing has been said concerning validation of the model; while it should not be inferred this has been neglected (indeed this is a current and continuing process with independent sample plot data) it is necessary to realize that the forest the model simulates is constantly monitored by (a) weighbridge yields; and (b) forest inventories, which provide additional independent estimates of stand yield which are being used to verify the simulator.

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Responsibility for statements and opinions expressed lie with the authors alone.

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APPENDIX 8

Regression analysis of stand competition index and stocking

Model $E(Y) = \alpha + \beta X$.

where Y = stand competition index

X = stems/ha

ANOVA

Source	d.f.	SS	MS	p > F
Model	1	1.1035	1.1035	0.0001
Error	55	1.3825	0.0251	
Lack of fit	10	0.4095	0.04095	0.0873
Pure error	45	0.9730	0.0216	

$$\hat{Y} = 0.001827X - 0.0937$$

The test for lack-of-fit is weakly significant (10%), suggesting a small degree of curvature, but the model is retained. Additional data from Waring (unpublished data) supports a linear relationship. Waring's data are also from regenerated *Pinus radiata*, but covers a wider range of stockings. Figure (A.8.1) illustrates the relationship between stand competition index and stocking for this data.

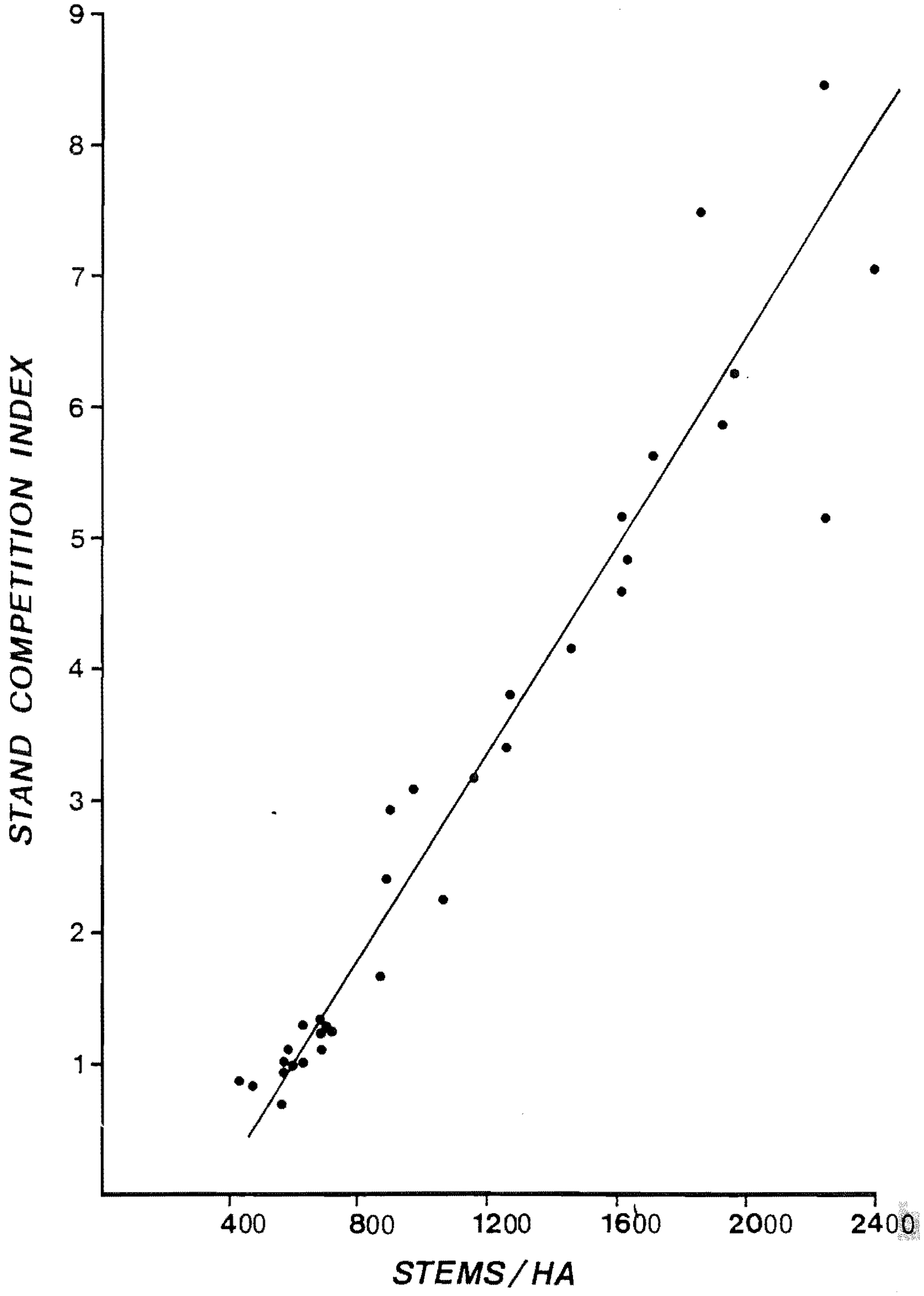

Figure (A.8.1) Relationship between stand competition index and stocking. Data from Waring (unpublished)

APPENDIX 9 Computer program for fertilizer growth and yield model.

N.Z.FOREST PRODUCTS LIMITED FERTILIZER GROWTH MODEL

```
     open (unit=6, file = 'output')
100  write(1,500)
500  format (' enter 1 to continue;  0 to exit')
     read (1,501)icon
501  format(ii)
     if(icon-1)110,502,502
502  write(1,10)
 10  format('enter initial age (nn)')
     read(1,15) iage
 15  format(i2)
     write(1,20)
 20  FORMAT('enter initial basal area/ha (nn.n)')
     read(1,25)ba
 25  format(f4.1)
     write(1,30)
 30  format ('enter initial sph (nnn)')
     read(1,35) sph
 35  format(f3.0)
     write(1,40)
 40  format('enter top height at this age (nn.n)(zero for default)')
     read(1,41) hite
 41  format(f4.1)
     ihte =hite
     write(1,42)
 42  format('enter the clearfall age(nn)')
     read(1,43)lage
 43  format(i1)
     write(1,44)
 44  format('enter fertilizer(1) or no fertilizer(0)')
     read(1,45)fert
     ifert=fert
 45  format(f1.0)
     write(1,46)
 46  format('enter degree competition regular(1),normal(2),clumped(3)'
     1)
     read(1,47)icomp
 47  format(i1)
     if(icomp-2)124,125,126
124  add=-0.10
     go to 127
125  add= 0.0
     go to 127
126  add= 0.10
127  comp = -0.02998369 + 0.001686842* sph + add
     fal=float(iage)
     write(6,48)
 48  format(9x,'age',2x,'toph',3x,'sph',3x,'ba',4x,'vol')
     if (ihte .eq. 0 ) tht= exp(4.1931 - 13.8217/fal )
     if (ihte .gt. 0 ) tht=hite
     vol = 0.32303*ba*tht*exp(3.21788/(fal**1.57818))
     write(6,60)iage, tht, sph,ba,vol
 60  format(10x,i2,2x,f4.1,2x,f4.0,2x,f4.1,2x,f6.1)
c
c    define constants for basal area function
c
     b1 = 0.9502458
     b2 =-0.6498025
```

```
      b3 = 0.2056856517
      b4 =-0.992960
      lage= lage- 1
      do 70 i = iage, lage
      rsph=        sph
      a1 = float(i)
      a2 = float(i + 1)
      ia2 = ifix(a2 )
      if( ihte .eq. 0 ) tht= exp(4.1931 - 13.8217/a2
      if( ihte .gt. 0 ) tht = hite*exp(13.8217*(1./fa1 - 1./a2))
      sph=rsph - (a2 - a1 )
      ba=ba/rsph
      ba= sph*exp(alog(ba)*(a1/a2)**b1 +b2(1-(a1/a2)**b1 )
    1 + b3*fert*(1-(a1/a2)**b1 ) + b4*comp*(1-(a1/a2)**b1))
      if(ifert .eq. 0) vol=0.32303*ba*tht*exp(3.21788/(a2**1.57818))
      if(ifert .gt. 0) vol=ba*tht*(0.32303*exp(3.21788/(a2**1.57818))
    1 + 0.008 )
      write(6,65) ia2,tht, sph,ba,vol,comp
   65 format(10x,i2,2x,f4.1,2x,f4.0,2x,f4.1,2x,f6.1,2x,f7.4)
   70 continue
      go to 100
  100 continue
      close(6)
      stop
      end
```