Chapter 3

A Comparison of Discrete and Continuous Models in Agricultural Production Analysis

Types of Experimental Procedures

Historical Review

In a recent review article (1), this writer traced the development of multifactor experimental procedures. A brief résumé of this development seems desirable at this time: In the first multifactor experiments, a single factor was varied at a time. For example, with five factors, one might plan $5\ell$ experiments, in which each of the factors in turn was used at $\ell$ levels while the other four factors were held at some starting level. Fisher (14) and Yates (25) encouraged the use of complete factorials and developed a large number of special designs involving them. In a complete factorial, all combinations of the factor levels are used, e.g., $\ell^5$ for the above experiment. These designs were developed for experiments in which the experimental error could not be neglected. In order to estimate the magnitude of this error in each experiment, the experiment had to be repeated several times, e.g., $r$. These factorial designs were formed largely for useful field experiments in which sequential experimentation would be less than the laboratory experiments, and the factors were often of the discrete type, e.g., varieties or rations.

Because of the large number of factor combinations required in many field experiments, it was felt that some form of incomplete block design was needed to reduce the experimental error. This resulted in the so-called confounded designs, e.g., with $2^k$, $3^k$, $3 \times 2^k$, $3^k \times 2$, $4^k$ designs. These are described by Yates (26). More complicated factorial designs have been constructed by Nair (21, 22), Bose (4), Finney (13), and Li (20), among others.

When physical scientists and engineers became interested in multifactor experiments, they found that complete and confounded factorials required too many experimental units, especially since the experimental errors were often much lower than in field experiments. One method of reducing the number of experimental units was to use higher order interaction effects to estimate the error and hence avoid repetition of the design. Fisher (14) and Cornish (9) described the analysis of the singly replicated unconfounded factorial design and used the higher order...
interactions for this purpose. Jeffreys (17) and Kempthorne (18) have advance justifications for this approach. Then Finney (11, 12), Plackett and Burman (23), Kempthorne (18), Rao (24), and Davies and Hay (10) developed the fractional replication designs, based on using parts of the confounded designs. Yates (25) and Hotelling (16) had already mentioned the use of such designs.

Some General Considerations of Factorial Experiments

The results of multifactor experiments are usually summarized in various two- and more-way tables of means and an analysis of variance. For example, let us assume there are two factors (A and B), one with $p$ and the other with $q$ groups, each of the $pq$ classes having $r$ samples. Some characteristic, such as yield, is measured for each of the $pqr$ samples. The results are summarized in a $(p \times q)$ table of class means ($\bar{Y}_{ij}$) with the corresponding $(p + q)$ border means ($\bar{A}_i$ and $\bar{B}_j$).

\[
\begin{array}{cccc}
1 & 2 & \ldots & q \\
1 & \bar{Y}_{i1} & \bar{Y}_{i2} & \ldots & \bar{Y}_{iq} & \bar{A}_1 \\
2 & \bar{Y}_{i21} & \bar{Y}_{i22} & \ldots & \bar{Y}_{i2q} & \bar{A}_2 \\
\vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\
p & \bar{Y}_{ip1} & \bar{Y}_{ip2} & \ldots & \bar{Y}_{ipq} & \bar{A}_p \\
\bar{B}_1 & \bar{B}_2 & \ldots & \bar{B}_q & \bar{Y}
\end{array}
\]

For example, the border means for A represent averages over all B-groups. There are two circumstances under one or both of which these A-means are of importance:

1. Differences between B-groups are the same for all A-groups, i.e., there is no AB interaction.
2. The experimenter desires to make inferences regarding A only when averaged over these particular B-groups.

If item 1 is true, one can set up the following model to represent the yield for a given sample:

\[(1) \quad Y = (\text{mean}) + (A \text{ effect}) + (B \text{ effect}) + (\text{error}).\]

The A and B effects are estimated by computing the deviations of group means from the general mean, e.g.,

\[A_1 \text{ effect} = \bar{A}_1 - \bar{Y}.\]

The errors are assumed to be normally and independently distributed.
with zero means and same variances, $\sigma^2$. In this case, analysis of variance is:

<table>
<thead>
<tr>
<th>Source of Variation</th>
<th>d. f.</th>
<th>S. S.</th>
<th>M. S.</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>p-1</td>
<td>SSA</td>
<td>MSA</td>
</tr>
<tr>
<td>B</td>
<td>q-1</td>
<td>SSB</td>
<td>MSB</td>
</tr>
<tr>
<td>Residual</td>
<td>(p-1)(q-1)</td>
<td>SSI</td>
<td>MSI</td>
</tr>
<tr>
<td>Error</td>
<td>(r-1) pq</td>
<td>SSW</td>
<td>$s^2$</td>
</tr>
</tbody>
</table>

In the above analysis of variance, $SSA = qr \sum A_i^2 - pqr \bar{Y}^2$ and $SSB = pr \sum B^j - pqr \bar{Y}^2$. The residual sum of squares measures the failure of the A and B effects to be additive, i.e., presence of AB interaction. It is computed as:

$$r \sum \bar{Y}_{ij}^2 - pqr \bar{Y}^2 = SSA - SSB.$$

The error variance, $\sigma^2$, is estimated from the variability within classes. The mean squares are all computed by dividing the sums of squares by the corresponding degrees of freedom. One can test for the existence of interaction by use of $F = MSI/s^2$. Presumably, if this is significant, inferences about A effects must be confined to averages over these q B-groups. Otherwise one should consider the general model:

$$(2) \quad Y = (\text{class mean}) + (\text{error}).$$

Then each of the $pq$ classes is considered separately and the simple analysis of variance is:

<table>
<thead>
<tr>
<th>Source of Variation</th>
<th>d. f.</th>
<th>S. S.</th>
<th>M. S.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatments</td>
<td>pq-1</td>
<td>SST</td>
<td>MST</td>
</tr>
<tr>
<td>Error</td>
<td>(r-1) pq</td>
<td>SSW</td>
<td>$s^2$</td>
</tr>
</tbody>
</table>

$$SST = r \sum \bar{Y}^2 - pqr \bar{Y}^2$$

The same procedures can be followed for more than two classification variables. In this it is advisable to look at the individual contributions to the interaction: AB, AC, BC ... ABC ... . In many cases it is even possible to subdivide SSA, for example, into pertinent single degree of freedom contracts; hence, SS(AB) can also be subdivided. This subdivision of SSI is useful in detecting particular aspects of nonadditivity which may be concealed in blanket tests of MSI/s^2. For more exact discussion of these problems, see Chapter 20 of Anderson and Bancroft (2).
Extension of Factorial Experimentation to Continuous Variables

In the past, even though the factors could be varied continuously, most analyses of experimental data have followed the same procedures as for discrete classifications. For example, if one had an experiment to study the effect of nitrogen (n) and potash (k) on the yield of corn, one might consider a simple $2 \times 2$ experiment with four treatment combinations: low n and k (00); low n and high k (02); high n and low k (20); and high n and high k (22). Suppose each treatment were randomly assigned to $r$ plots. The usual summary procedure would be to form the four-treatment totals and means in $2 \times 2$ tables. The totals are indicated as follows:

<table>
<thead>
<tr>
<th>Nitrogen</th>
<th>Potash</th>
</tr>
</thead>
<tbody>
<tr>
<td>low</td>
<td>(00) N₀</td>
</tr>
<tr>
<td>high</td>
<td>(02) N₂</td>
</tr>
<tr>
<td></td>
<td>(20) K₀</td>
</tr>
<tr>
<td></td>
<td>(22) K₂</td>
</tr>
</tbody>
</table>

The border totals are indicated by capital letters, with G for the grand total.

If one were unwilling to make any assumptions about the comparability of the four treatments, he would look only at the four-cell mean (cell totals divided by $r$) and use model 2 and the accompanying analysis.

If the experimenter feels that the effect of increased n or k is the same regardless of the level of the other element, he would use an adaptation of model 1 as follows:

$Y = (\text{mean}) \pm (n \text{ effect}) \pm (k \text{ effect}) \pm (\text{error}),$

where the + sign refers to high level plots and the - to low level plots. For example, the average or expected yield for a plot receiving high n and low k is:

$(\text{mean}) + (n \text{ effect}) - (k \text{ effect}).$

The n effect, for example, represents the expected increase in yield due to high n over the average of high and low n, and is estimated by

$$\frac{N₂ - N₀}{2r} - \frac{N₀}{2r}.$$

The analysis of variance is the same as for model 1. The residual can be used to test the adequacy of the additive model 1', i.e., test for the existence of an (NK) interaction. If this residual is significant, the

$1^0$ is used for the low level and 2 is used for the high level, so that 1 may be introduced as a middle level.
effect of increased $n$ is not the same for low and for high $k$ (and vice versa). Hence it is necessary to interpret each cell mean separately, i.e., use model 2.

Continuing this aping of the models for discrete factors, the following general model has been constructed for the $2 \times 2$ experiment:

\[(3) \quad Y = (\text{mean}) \pm (n \text{ effect}) \pm (k \text{ effect}) \pm (nk \text{ interaction effect}) + (\text{error}),\]

where the interaction effect receives a plus sign for the $(0,0)$ and $(2,2)$ plots and a minus sign for the $(0,2)$ and $(2,0)$ plots. For example, the expected yield for a plot receiving high $n$ and low $k$ is:

\[(\text{mean}) + (n \text{ effect}) - (k \text{ effect}) - (nk \text{ interaction effect}).\]

The interaction effect is estimated by:

\[\frac{(00) - (02) - (20) + (22)}{4r}.\]

If the response surface can be approximated by a simple mathematical function, it seems more logical to estimate the parameters of this function instead of main effects and interactions. In the present example, consider the following continuous model:

\[(4) \quad Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_{12} X_1 X_2 + (\text{error}).\]

$X_1$ and $X_2$ represent the respective levels of nitrogen and potash as deviations from the mean level in the experiment ($X = -1$ for low and $X = +1$ for high level); $\beta_0$ is the expected yield for $n$ and $k$ midway between the amounts applied in the experiment ($X_1 = X_2 = 0$); $\beta_1$ and $\beta_2$ are linear effects of added $n$ and $k$; $\beta_{12}$ is the interaction parameter. Using model 4, the cell totals (of $r$ plots each) have these expectations:

<table>
<thead>
<tr>
<th>Source of Variation</th>
<th>Sum of Squares</th>
<th>Mean Square</th>
</tr>
</thead>
<tbody>
<tr>
<td>N</td>
<td>$(N_2 - N_0)^2$</td>
<td>$\frac{(N_2 - N_0)^2}{4r}$</td>
</tr>
<tr>
<td>K</td>
<td>$(K_2 - K_0)^2$</td>
<td>$\frac{(K_2 - K_0)^2}{4r}$</td>
</tr>
<tr>
<td>Residual</td>
<td>$\frac{[(00)-(02)-(20)+(22)]^2}{4r}$</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>low $k$ ($X_2=-1$)</th>
<th>high $k$ ($X_2=1$)</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>low $n$ ($X_1=-1$)</td>
<td>$r(\beta_0 - \beta_1 - \beta_2 + \beta_{12})$</td>
<td>$r(\beta_0 - \beta_1 + \beta_2 - \beta_{12})$</td>
<td>$2r(\beta_0 - \beta_1)$</td>
</tr>
<tr>
<td>high $n$ ($X_1=1$)</td>
<td>$r(\beta_0 + \beta_1 - \beta_2 - \beta_{12})$</td>
<td>$r(\beta_0 + \beta_1 + \beta_2 + \beta_{12})$</td>
<td>$2r(\beta_0 + \beta_1)$</td>
</tr>
<tr>
<td>Total</td>
<td>$2r(\beta_0 - \beta_2)$</td>
<td>$2r(\beta_0 + \beta_2)$</td>
<td>$4r \beta_0$</td>
</tr>
</tbody>
</table>
The estimators of the $\beta$'s in equation 4 are:

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Estimator</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_1$</td>
<td>$b_1 = \frac{N_rN_n}{4r}$</td>
</tr>
<tr>
<td>$\beta_2$</td>
<td>$b_2 = \frac{K_rK_n}{4r}$</td>
</tr>
<tr>
<td>$\beta_{12}$</td>
<td>$b_{12} = \frac{(00)-(02)-(20)+(22)}{4r}$</td>
</tr>
<tr>
<td>$\beta_0$</td>
<td>$b_0 = G/4r = \bar{Y}$.</td>
</tr>
</tbody>
</table>

The variance of each estimator is $\sigma^2/4r$. Note that these estimators are the same as for the effects of model 3. $\beta_1$, for example, measures the average difference in yield per unit change in $n$ for these two $k$ treatments, i.e., the change in $Y$ for a unit change in $X_1$, neglecting interaction. Also the analysis of variance produces the same three orthogonal sums of squares for treatments, using either models 3 or 4.

Hence it appears that models 3 and 4 are identical. However, there is a very important difference. Model 3 makes no assumption regarding the shape of the response surface, but model 4 implies a definite continuity of response; hence, one would feel free to use the results of model 4 to interpolate between the actual levels used in the experiment. If he did use model 3 for this purpose, he would actually be assuming the continuous model 4. One is often tempted to extrapolate the results beyond the levels used in the experiment; such extrapolation assumes the same response surface holds beyond the experimental levels. In other words, one uses model 2 or 1 if he does not wish to assume a quadratic response surface, but uses model 4 if experience or theory indicates such a surface would be satisfactory.

If the design is spread out so that the low and high levels differ by $2d$ units (instead of 2), $b_1$ will have a denominator of $4rd$ and $b_{12}$ a denominator of $4rd^2$. Hence the variance of $b_1$ is reduced by a factor of $d^2$ and $b_{12}$ by a factor of $d^4$. The only reason for not using extremely divergent levels is that the response surface may have a different shape at extremely large or small fertilizer applications.

If the continuous model 4 is used, it seems unreasonable to include a quadratic term involving $X_1X_2$ without also including terms involving $X_1^2$ and $X_2^2$. The shape of a response surface such as model 4 is rather grotesque. In other words one would be more likely to consider the following general quadratic model:

$$Y = \beta_0 + \beta_1X_1 + \beta_2X_2 + \beta_{12}X_1X_2 + \beta_{11}X_1^2 + \beta_{22}X_2^2 + \text{(error)}.$$  

It should be clear that the difference between low and high levels is a two-unit change, e.g., if low is 50 pounds per plot and high is 100 pounds per plot, a unit change is 25 pounds and $\beta_1$ and $\beta_2$ measure the linear effects of 25-pound increases.

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2It should be clear that the difference between low and high levels is a two-unit change, e.g., if low is 50 pounds per plot and high is 100 pounds per plot, a unit change is 25 pounds and $\beta_1$ and $\beta_2$ measure the linear effects of 25-pound increases.
If model 5 is the true continuous model, the expectations of the cell totals are:

<table>
<thead>
<tr>
<th></th>
<th>low (k (X_k=-1))</th>
<th>high (k (X_k=1))</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>low (n(X_i=-1))</td>
<td>(r(\beta_0 - \beta_1 + \beta_{11}))</td>
<td>(r(\beta_0 + \beta_1 + \beta_{11} - \beta_2))</td>
<td>(2r(\beta_0 - \beta_1 + \beta_{11}))</td>
</tr>
<tr>
<td>high (n(X_i=1))</td>
<td>(r(\beta_0 + \beta_1 + \beta_{11}))</td>
<td>(r(\beta_0 + \beta_1 + \beta_{11} + \beta_2))</td>
<td>(2r(\beta_0 + \beta_1 + \beta_{11}))</td>
</tr>
<tr>
<td>Total</td>
<td>(2r(\beta_0 + \beta_1 + \beta_{11}))</td>
<td>(2r(\beta_0 + \beta_1 + \beta_{11}))</td>
<td>(4r(\beta_0 + \beta_1 + \beta_{11}))</td>
</tr>
</tbody>
</table>

It turns out that the estimates of \(\beta_1\), \(\beta_2\), and \(\beta_{12}\) are the same as for model 4 and are not mixed up with the quadratic terms \((\beta_{11}\) and \(\beta_{22}\), i.e., they are unbiased estimates. However, there is no method of estimating \(\beta_0\), \(\beta_{11}\), or \(\beta_{22}\), since their sum is estimated by \(\bar{Y}\). Hence this analysis indicates that it is safe if only statements are made regarding the treatments used in the experiment and no attempt is made to predict the results for other fertilizer levels.

Of course the solution to the above dilemma is to add other levels of \(n\) and \(k\). The traditional design to estimate quadratic effects is the 3 x 3 complete factorial with the three levels of \(n\) and \(k\) equally spaced.\(^3\) Assuming the middle values of \(n\) and \(k\) are the averages of the low and high levels used in the 2 x 2 experiment, i.e., if the low and high applications were 50 and 100 pounds per plot, the middle application would be 75 pounds per plot. In the factorial setup, the levels are designated as 0, 1, and 2 with \(X = -1, 0, 1\), respectively. Henceforth, factor combination will be designated by the levels used, e.g., \((-1, -1)\). Assuming \(r\) plots per cell and using model 5, the expectations for the \((-1, -1)\), \((-1, 1)\), \((1, -1)\) and \((1, 1)\) totals would be as before. The expectations for the other five class totals and the border totals would be:

\[
\begin{align*}
(-1, 0) & \quad r(\beta_0 - \beta_1 + \beta_{11}) \\
(0, -1) & \quad r(\beta_0 - \beta_2 + \beta_{22}) \\
(0, 0) & \quad r \beta_0 \\
(0, 1) & \quad r(\beta_0 + \beta_2 + \beta_{22}) \\
(1, 0) & \quad r(\beta_0 + \beta_1 + \beta_{11})
\end{align*}
\]

\[
\begin{align*}
N_0 & \quad 3r(\beta_0 - \beta_1 + \beta_{11}) + 2r \beta_{22} \\
N_1 & \quad 3r \beta_0 + 2r \beta_{22} \\
N_2 & \quad 3r(\beta_0 + \beta_1 + \beta_{11}) + 2r \beta_{22} \\
K_0 & \quad 3r(\beta_0 - \beta_2 + \beta_{22}) + 2r \beta_{11} \\
K_1 & \quad 3r \beta_0 + 2r \beta_{11} \\
K_2 & \quad 3r(\beta_0 + \beta_2 + \beta_{22}) + 2r \beta_{11}
\end{align*}
\]

\[
G = 9r \beta_0 + 6r (\beta_{11} + \beta_{22})
\]

\(^3\)Equal spacing enables one to analyze linear and quadratic components in a simple manner, but it is not an essential, or even the most efficient, method of spacing.
The following estimators and variances are obtained:

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Estimator</th>
<th>Variance of Estimator</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_1$</td>
<td>$b_1 = (N_2-N_0)/6r$</td>
<td>$\sigma^2/6r$</td>
</tr>
<tr>
<td>$\beta_2$</td>
<td>$b_2 = (K_2-K_0)/6r$</td>
<td>$\sigma^2/6r$</td>
</tr>
<tr>
<td>$\beta_{12}$</td>
<td>$b_{12} = [(1,-1) - (1,-1) - (1,1) + (1,1)]/4r$</td>
<td>$\sigma^2/4r$</td>
</tr>
<tr>
<td>$\beta_{11}$</td>
<td>$b_{11} = (N_2-2N_1+N_0)/6r$</td>
<td>$\sigma^2/2r$</td>
</tr>
<tr>
<td>$\beta_{22}$</td>
<td>$b_{22} = (K_2-2K_1+K_0)/6r$</td>
<td>$\sigma^2/2r$</td>
</tr>
<tr>
<td>$\beta_0$</td>
<td>$b_0 = [5(N_1+K_1)-(N_0+N_2+K_0+K_2)]/18r$</td>
<td>$5 \sigma^2/9r$</td>
</tr>
</tbody>
</table>

Note that $b_1$, $b_2$, and $b_{12}$ are the same as before; also, if the levels are $(-d,0,d)$, the variances for the linear coefficients are again reduced by a factor of $d^2$ and for the quadratic and interaction coefficients by a factor of $d^4$.

The analysis of variance is as follows ($\ell$ stands for linear and $q$ for quadratic component):

<table>
<thead>
<tr>
<th>Effect</th>
<th>d.f.</th>
<th>M. S.</th>
</tr>
</thead>
<tbody>
<tr>
<td>$N\ell$</td>
<td>1</td>
<td>$(N_2-N_0)^2/6r$</td>
</tr>
<tr>
<td>$K\ell$</td>
<td>1</td>
<td>$(K_2-K_0)^2/6r$</td>
</tr>
<tr>
<td>$N\ell K\ell$</td>
<td>1</td>
<td>$[(-1,-1)-(-1,1)-(1,-1)+(1,1)]^2/4r$</td>
</tr>
<tr>
<td>$Nq$</td>
<td>1</td>
<td>$(N_2-2N_1+N_0)^2/18r$</td>
</tr>
<tr>
<td>$Kq$</td>
<td>1</td>
<td>$(K_2-2K_1+K_0)^2/18r$</td>
</tr>
<tr>
<td>Residual</td>
<td>3</td>
<td>$[SST - SS(N\ell + K\ell + \ldots + Kq)]/3$</td>
</tr>
<tr>
<td>Error</td>
<td>$9(r-1)$</td>
<td>$s^2 = SSW/9(r-1)$</td>
</tr>
</tbody>
</table>

The residual mean square can be used to test for the adequacy of the model. If the $3 \times 3$ complete factorial is used, it turns out that these three degrees of freedom can be subdivided into three orthogonal components, which measure $N\ell/Kq$, $NqK\ell$, and $Nq Kq$ interaction effects $[\beta_{122}X_1^2X_2 + \beta_{112}X_1X_2 + \beta_{122}X_1^2X_2$ is added to model 5].

Once again a factorial model similar to model 3 can be constructed with the same linear and quadratic effects as in model 5. However, there seems little reason for estimating such effects unless one is willing to assume a quadratic response surface. If he does not wish to assume a quadratic response surface, he has two possible factorial models:

1. Model 2 with nine treatments
2. Model 1' with two effects for each factor: above and below the middle application or referred to either the high or low application. The analysis based on model 1' would include a sum of squares attributable to the interactions, giving a test of the adequacy of the model. These remarks hold for any number of factors and levels per factor. If there is a mixture of classification variables (e.g., varieties) and continuous variables, a combined factorial and continuous model can be set up and analyzed in a manner analogous to covariance. This would assume that the parameters for the continuous variables were the same for each discrete classification; a test of this hypothesis can also be constructed.

The Use of Blocking Methods to Reduce Experimental Error

The use of blocking methods in the previous discussion has not been considered because they only complicate the presentation without altering any of the conclusions. However, one must consider the blocking procedure if there is confounding. Unfortunately, the procedures used in constructing such designs have been based on confounding certain parts of the higher order interactions which are not related to higher degree components. For example, the so-called I and J parts of the NK interaction in a 3 x 3 experiment do not pertain to any one of the four degree components, \( N_f N_q, N_f K_q, N_q K_f, \) or \( N_q K_q \). One would prefer a design which minimized the confounding on \( N_f K_q \).

A bulletin now in press by Binet, Leslie, Weiner, and Anderson (3) presents the confounding patterns in terms of degree components. This bulletin should be of use in three ways:

1. It presents short-cut methods of analyzing these confounded experiments when degree components are of interest.
2. Several new confounded designs are presented.
3. It presents the confounding patterns for various designs, so the reader can select the design which will be best for his problem.

To illustrate the procedures, suppose the nine treatments in the 3 x 3 experiment were put in 3 blocks of 3 plots each. One such arrangement would be (the treatments refer to levels, and \( B_i \) are block totals):

<table>
<thead>
<tr>
<th>Block</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1,-1)</td>
<td>(-1,-1)</td>
<td>(0,-1)</td>
</tr>
<tr>
<td></td>
<td>(-1,0)</td>
<td>(0,0)</td>
<td>(1,0)</td>
</tr>
<tr>
<td></td>
<td>(0,1)</td>
<td>(1,1)</td>
<td>(-1,1)</td>
</tr>
<tr>
<td>( B_1 )</td>
<td>( B_2 )</td>
<td>( B_3 )</td>
<td></td>
</tr>
</tbody>
</table>

*Cf. Anderson and Bancroft (2), Section 20.5.*
If \( b'_j \) represents the mean of the j-th block, then two block contrasts are formed:

\[
2C_1 = b'_1 - b'_2 \quad \text{and} \quad 6c_2 = b'_2 - 2b'_2 + b'_1.
\]

The least squares equations for the two block and four NK effects are:

<table>
<thead>
<tr>
<th>( C_1 )</th>
<th>( N_{qKq} )</th>
<th>( N_{qKq} )</th>
<th>( C_2 )</th>
<th>( N_{qKq} )</th>
<th>( N_{qKq} )</th>
<th>( \text{Yield Sum} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>6</td>
<td>-6</td>
<td>6</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>( B_3 - B_1 )</td>
</tr>
<tr>
<td>-6</td>
<td>12</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>( (N_{qKq}) )</td>
</tr>
<tr>
<td>6</td>
<td>0</td>
<td>12</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>( (N_{qKq}) )</td>
</tr>
<tr>
<td>0</td>
<td>0</td>
<td>0</td>
<td>18</td>
<td>-6</td>
<td>-18</td>
<td>( B_1 - 2B_2 + B_3 )</td>
</tr>
<tr>
<td>0</td>
<td>0</td>
<td>0</td>
<td>-6</td>
<td>4</td>
<td>0</td>
<td>( (N_{qKq}) )</td>
</tr>
<tr>
<td>0</td>
<td>0</td>
<td>0</td>
<td>-18</td>
<td>0</td>
<td>36</td>
<td>( (N_{qKq}) )</td>
</tr>
</tbody>
</table>

The yield sum for \( N_{qKq} \), for example, is:

\[
[(1,1) - 2(1,0) + (1,-1)] - [(-1,1)-2(-1,0) + (-1,-1)].
\]

The usual procedure in analyzing these results would be to assume the block contrasts and \( N_{qKq} \) were the only real effects. This leaves only one contrast for testing the model, since there are only four degrees of freedom in the above six equations. The method of analysis proposed in the bulletin is the abbreviated Doolittle method, which is also discussed in detail by Anderson and Bancroft (2). Obviously there is no estimate of error from this experiment. If such an estimate is needed, another replicate should be used, preferably one which has a different confounding pattern, as indicated in the bulletin.

For experiments with many factors, it is often possible to estimate the pertinent contrasts by use of fractional designs.

Special Designs To Estimate Parameters of Response Surfaces

The material by Binet et al. (3) furnishes a method of using existing confounded factorial designs to estimate the important degree components. However, for most experiments in which the experimenter has evidence that a smooth response surface is suitable, he should consider designs especially constructed to estimate the parameters of this surface and not to estimate class means for a classification model. Box (5) developed some general design principles for estimating the parameters of planar surfaces.

Box and Wilson (8) proposed a new design for estimating quadratic surfaces which gives more information on the quadratic effects and less on the high-degree effects. Their composite design would push the \((0,1), (0,-1), (1,0), \) and \((-1,0)\) points \( \alpha \) units from the center of the design as indicated in figure 3.1.
If $\alpha = 2$, the expectations of the totals for the four altered cells are:

(-2,0) $r(\beta_0 - 2\beta_1 + 4\beta_{11})$

(0,-2) $r(\beta_0 - 2\beta_2 + 4\beta_{22})$

(0,2) $r(\beta_0 + 2\beta_2 + 4\beta_{22})$

(2,0) $r(\beta_0 + 2\beta_1 + 4\beta_{11})$

In this case one cannot analyze the results as for a 3 x 3 table, because it is an incomplete 5 x 5 factorial experiment. Here one must use the general least-squares approach. The matrix for the normal equations is:

<table>
<thead>
<tr>
<th>Coefficients of Estimators</th>
<th>Right hand side</th>
</tr>
</thead>
<tbody>
<tr>
<td>Equation</td>
<td>$b_0$</td>
</tr>
<tr>
<td>$b_0$</td>
<td>9r</td>
</tr>
<tr>
<td>$b_1$</td>
<td>0</td>
</tr>
<tr>
<td>$b_2$</td>
<td>0</td>
</tr>
<tr>
<td>$b_{12}$</td>
<td>0</td>
</tr>
<tr>
<td>$b_{11}$</td>
<td>12r</td>
</tr>
<tr>
<td>$b_{22}$</td>
<td>12r</td>
</tr>
</tbody>
</table>
In the preceding, for example, \( g_1 = (1,1) + 2(2,0) + (1,-1) - (-1,1)-2(-2,0)\cdot(-1,-1) \), where \((1,1), \text{etc.}, \) stand for class totals. The solutions and variances of the estimators are:

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Estimator</th>
<th>Composite 3 x 3</th>
<th>3 x 3 (Adjusted)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \beta_1 )</td>
<td>( g_1/12r )</td>
<td>1/12</td>
<td>1/12</td>
</tr>
<tr>
<td>( \beta_2 )</td>
<td>( g_2/12r )</td>
<td>1/12</td>
<td>1/12</td>
</tr>
<tr>
<td>( \beta_{12} )</td>
<td>( g_{12}/4r )</td>
<td>1/4</td>
<td>1/16</td>
</tr>
<tr>
<td>( \beta_{11} )</td>
<td>( (30g_{11} + 18g_{22} - 64G)/384r )</td>
<td>5/64</td>
<td>1/8</td>
</tr>
<tr>
<td>( \beta_{22} )</td>
<td>( (30g_{22} + 18g_{11} - 64G)/384r )</td>
<td>5/64</td>
<td>1/8</td>
</tr>
<tr>
<td>( \beta_0 )</td>
<td>( (10G - 3g_{11} - 3g_{22})/18r )</td>
<td>5/9</td>
<td>5/9</td>
</tr>
</tbody>
</table>

One gets the impression that there is a tremendous reduction in variances of estimators by use of the composite design instead of the 3 x 3 factorial. However, most of this gain is the natural result of using a wider range of \( X \)'s; the incompleteness of the factorial in the composite design is not responsible for all the gain. This was indicated for the 2 x 2 experiment. One could adjust the coordinates of the 3 x 3 design so that the spread is the same as for the composite design. The variance of the coordinates for the latter (with \( \alpha = 2 \)) is \( [2(4) + 4(1) + 3(0)]/9 = 4/3 \). Let the new coordinates for the 3 x 3 design be \((-d,0,d)\), so that the variance of these coordinates is \( 2d^2/3 = 4/3 \); or \( d = \sqrt{2} \). Hence, the variances of linear terms are reduced by \( 1/2 \) and of quadratic terms by \( 1/4 \). Therefore, the composite design has improved the quadratic estimators at the expense of the interaction one. Box and Wilson (8) show that this is desirable in estimating the optimal factor combination.

Another criterion of the relative efficiency of two different designs in estimating the parameters of a response surface would be the amount of information used to estimate the high degree coefficients, which are assumed to be unimportant.

Box and Hunter (7) have advanced another principle of a good surface-fitting design; it should be rotatable; i.e., the accuracy of the estimates of the parameters should not depend on the orientation of the design with respect to the true surface itself. They have constructed several incomplete factorial designs which meet this requirement.

Mason discusses in Chapter 5 some recent experiments in which the composite designs have been used.

These are obtained by inverting the left-hand matrix. The abbreviated Doolittle or square-root method is usually used, although special pattern matrices can be used.
Sequential Experimentation

Much of the impetus for the Box-Wilson paper (8) came from a need to develop sequential procedures for determining optimal factor combinations. Various procedures have been summarized in Anderson's review article (1). Since then, Box (6) has published an extensive discussion of the entire problem. Although the use of these sequential methods may be somewhat limited in fertilizer experiments because of the length of time needed to obtain results, it probably would be desirable to develop a more systematic procedure of utilizing past experience in designing future experiments.

Better methods are needed to pool data from a series of experiments. Researchers should be encouraged to spend more time on these problems.

Some Special Comparisons of Discrete and Continuous Models

Comparison of Discrete Model 2 and Quadratic Model 5
Using 3 x 3 Design

1. The quadratic model is correct. In this case the estimated average yield for plots receiving \( X_1 \) units of \( N \) and \( X_2 \) units of \( K \) (measured from the mean level) is:

\[
\hat{Y} = b_0 + b_1X_1 + b_2X_2 + b_{12}X_1X_2 + b_{11}X_1^2 = b_{22}X_2^2.
\]

In order to obtain the sampling variance of \( \hat{Y} \), it requires the variances of the estimators given previously and the covariances. All of these could be obtained by inverting the matrix of sums of squares and products of the regression variables in the normal equations. This matrix is as follows:

\[
\begin{array}{cccccc}
\text{b}_0 & \text{b}_1 & \text{b}_2 & \text{b}_{12} & \text{b}_{11} & \text{b}_{22} \\
\text{b}_0 & 9r & 0 & 0 & 0 & 6r & 6r \\
\text{b}_1 & 0 & 6r & 0 & 0 & 0 & 0 \\
\text{b}_2 & 0 & 0 & 6r & 0 & 0 & 0 \\
\text{b}_{12} & 0 & 0 & 0 & 4r & 0 & 0 \\
\text{b}_{11} & 6r & 0 & 0 & 0 & 6r & 4r \\
\text{b}_{22} & 6r & 0 & 0 & 0 & 4r & 6r \\
\end{array}
\]

Since \( b_0, b_{11}, \) and \( b_{12} \) are the only correlated variables, consider them separately in a 3 x 3 matrix \( A \), which when multiplied by its inverse \( C \) is the identity matrix.

\[
A = \begin{bmatrix}
9r & 6r & 6r \\
6r & 6r & 4r \\
6r & 4r & 6r
\end{bmatrix}, \quad C = \begin{bmatrix}
C_1 & C_2 & C_2 \\
C_2 & C_3 & C_4 \\
C_2 & C_4 & C_3
\end{bmatrix} = \begin{bmatrix}
1 & 0 & 0 \\
0 & 1 & 0 \\
0 & 0 & 1
\end{bmatrix}
\]
There are only four different elements of C. These can be determined quite simply as follows:

\[
\begin{align*}
9r C_1 + 12r C_2 &= 1 \\
6r C_1 + 10r C_2 &= 0 \\
6r C_2 + 6r C_3 + 4r C_4 &= 1 \\
9r C_2 + 6r C_3 + 6r C_4 &= 0
\end{align*}
\]

Hence the matrix of variances and covariances of the b's is:

\[
\begin{bmatrix}
\frac{5}{9} & 0 & 0 & 0 & -\frac{1}{3} & -\frac{1}{3} \\
0 & \frac{1}{6} & 0 & 0 & 0 & 0 \\
0 & 0 & \frac{1}{6} & 0 & 0 & 0 \\
0 & 0 & 0 & \frac{1}{4} & 0 & 0 \\
-\frac{1}{3} & 0 & 0 & 0 & \frac{1}{2} & 0 \\
-\frac{1}{3} & 0 & 0 & 0 & 0 & \frac{1}{2}
\end{bmatrix}
\]

The variance of \( \hat{Y} \) is:

\[
\hat{\sigma}^2(\hat{Y}) = \frac{\sigma^2}{r} \left[ \frac{5}{9} + \frac{1}{6} (X_1^2 + X_2^2) + \frac{1}{4} (X_1^2 X_2^2) + \frac{1}{2} (X_1^4 + X_2^4) - \frac{2}{3} (X_1^2 + X_2^2) \right]
\]

\[
= \frac{\sigma^2}{r} \left[ \frac{5}{9} + \frac{1}{2} (X_1^4 + X_2^4 - X_1^2 - X_2^2) + \frac{1}{4} X_1^2 X_2^2 \right].
\]

If the discrete model is used, every mean will have a sampling variance of \( \sigma^2/r \). For even the most divergent points \( (\pm 1, \pm 1) \),

\[
\hat{\sigma}^2(\hat{Y}) = 29 \sigma^2/36r
\]

which is less than \( \sigma^2/r \). Hence, if the quadratic model is correct, even the yields at the experimental points are estimated more accurately from the regression model instead of the simple average yield at that point. Of course \( \hat{Y} \) is even more accurate for the other five points.

The results might be even more favorable if another design were used.

2. The quadratic model is biased. Suppose the true model is model 5 plus \( \beta X_1^2 \). In this case \( \hat{Y} \) is too small by \( \beta \) when \( X_1 = 1 \) and too large by \( \beta \) when \( X_1 = -1 \). Some mean differences would be biased by \( 2\beta \), others by \( \beta \), and others not at all. However, the estimates using
model 2 would be unbiased. The problem of whether to use the biased estimates depends on a comparison of the suspected magnitude of the bias and the variances mentioned above. This problem may be even more serious if the form of the response equation is radically different from the quadratic, e.g., if it is exponential or logistic.

Returning to the bias of $\beta X^2$, it should be mentioned that at least one of the other $\beta$'s will also be biased if this term is not considered in the estimation procedure (when $\beta \neq 0$); for example:

$$E(b_1) = \beta_1 + \beta.$$

$\beta$ is called an *alias* of $\beta_1$. Box and Wilson (8) consider possible aliases in evaluating various designs. It is possible to construct designs so that possible aliases will not have much effect on the estimates. This may be one of the chief reasons why agricultural experimenters have not considered continuous models. Hildreth (15) has considered an estimation procedure which is built on model 2, but uses certain inequality restrictions on the production function. The estimation procedure used by Hildreth is discussed in Chapter 4.

**Pseudo-Interactions in Some Factorial Experiments**

The tendency to follow the mechanical procedure of analyzing factorial experiments in terms of main effects and interactions can result in serious loss of information, often of a misleading nature. As an example, consider an experiment involving two levels of nitrogen (coded $n = -1$ and 1) and two different cover crops to be plowed under. Suppose $C_1$ supplies no nitrogen to the soil, whereas $C_2$ supplies 2 units of $n$ (coded $n = -1$ and 1). In addition, the two crops supply other unspecified nutrients. Assume that the yield is a quadratic function of $n$ plus some additive amount due to the unspecified nutrients in the soil and furnished by the two crops: $\beta_0 - \gamma$ for $C_1$ and $\beta_0 + \gamma$ for $C_2$ ($\gamma$ may be positive or negative). Hence the model is:

$$Y = \beta_0 + \beta_1 n + \beta_{11} n^2 \pm \gamma + \text{(error)}.$$

where $\gamma$ is added for $C_2$ plots and subtracted for $C_1$ plots. The expected class and border total yields are:

<table>
<thead>
<tr>
<th></th>
<th>Crop 1</th>
<th>Crop 2</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>$n = -1$</td>
<td>$r(\beta_0 - 2\beta_1 + 4\beta_{11} - \gamma)$</td>
<td>$r(\beta_0 + \gamma)$</td>
<td>$2r(\beta_0 - \beta_1 + 2\beta_{11})$</td>
</tr>
<tr>
<td>$n = 1$</td>
<td>$r(\beta_0 - \beta_1 + 2\beta_{11} - \gamma)$</td>
<td>$r(\beta_0 + 2\beta_1 + 4\beta_{11} + \gamma)$</td>
<td>$2r(\beta_0 + \beta_1 + 2\beta_{11})$</td>
</tr>
</tbody>
</table>

$$2r(\beta_0 - \beta_1 + 2\beta_{11} - \gamma)$$

$$2r(\beta_0 + \beta_1 + 2\beta_{11} + \gamma)$$

$$4r(\beta_0 + 2\beta_{11})$$

*The center of the system is now one unit more than the average of the two nitrogen levels.*
The estimators and their variances are:

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Estimator</th>
<th>Variance</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_1$</td>
<td>$(N_i - N_{-1})/4r$</td>
<td>$\sigma^2/4r$</td>
</tr>
<tr>
<td>$\beta_{11}$</td>
<td>[(-1,1)-(-1,2)-(1,1)+(1,2)]/8r</td>
<td>$\sigma^2/16r$</td>
</tr>
<tr>
<td>$\gamma$</td>
<td>[(-1,2)-(1,1)]/2r</td>
<td>$\sigma^2/2r$</td>
</tr>
<tr>
<td>$\beta_0$</td>
<td>[(-1,2)+(1,1)]/2r</td>
<td>$\sigma^2/2r$</td>
</tr>
</tbody>
</table>

Compare these results with those obtained by use of traditional factorial methods.

<table>
<thead>
<tr>
<th>Effect</th>
<th>Yield</th>
<th>E(Yield)</th>
<th>E(MS)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nitrogen</td>
<td>$N_i - N_{-1}$</td>
<td>$4r\beta_1$</td>
<td>$4r\beta_1^2 + \sigma^2$</td>
</tr>
<tr>
<td>Crop</td>
<td>$C_2 - C_1$</td>
<td>$4r(\gamma + \beta_1)$</td>
<td>$4r(\gamma + \beta_1)^2 + \sigma^2$</td>
</tr>
<tr>
<td>$N \times C$</td>
<td>(-1,1)-(-1,2)-(1,1)+(1,2)</td>
<td>$8r\beta_{11}$</td>
<td>$16r\beta_{11}^2 + \sigma^2$</td>
</tr>
</tbody>
</table>

An $N \times C$ interaction is indicated if there is a quadratic effect of nitrogen; also the crop effect will be mixed up with the linear effect of nitrogen (this is satisfactory if one only wants to test for differences in yields and not to determine basic causes of such differences). But a major criticism is a failure to provide a method of estimating the quadratic effect of nitrogen. The $N \times C$ interaction effect is the least squares estimate of $\beta_{11}$, but this fact is concealed in a routine factorial analysis of variance.

This is a very simple illustration of the need for more basic models in discussing responses to treatments. Classification models may conceal basic response patterns. One might consider this problem when three instead of two levels of $n$ were used. In this case the factorial estimate of $\beta_{11}$ probably would be inefficient, because of neglect of the information from the $N \times C$ interaction.

Yates (26) presents a $2^3$ experiment with 4 replications, the factors being $N$, $K$, and $D$ (dung). Levels were none and some, the latter being 0.45 cwt. $N$ per acre, 1.12 cwt. $K$, and 8 tons of $D$ per acre. Assume that this amount of dung supplies the same as the "some" of $n$ and $k$, plus "some" other nutrients (called $d$). Code these data with -1 for none and +1 for some. Hence, the values of the variables for the various plots are:

7 A unit of nitrogen is 0.225 cwt., of potash is 0.56 cwt., and of dung is 4 tons; the center is at 0.45 N, 1.12 K and 4 D.
Assume a quadratic equation in \( n \) and \( k \), with \( d \) appearing linearly.

Hence:

\[
Y = \beta_0 + \beta_1 n + \beta_2 k + \beta_{11} n^2 + \beta_{22} k^2 + \beta_{12} nk + \beta_3 d + \text{(error)}.
\]

Because this experiment was not designed to estimate quadratic effects, it turns out that if a complete quadratic model was used with \( \beta_{33} d^2 \), \( \beta_{13} nd \), and \( \beta_{32} kd \) included, the following pairs of coefficients could not be separated: \( \beta_0 \) and \( \beta_{33} \); \( \beta_{11} \) and \( \beta_{13} \); and \( \beta_{22} \) and \( \beta_{23} \). In other words the constant and \( d^2 \), \( n^2 \), and \( nd \) and \( k^2 \) and \( kd \) are aliases. It is assumed here that \( d \) is essentially a residual variable, which is unlikely to have any effect and especially not a quadratic one; however, one cannot be sure which of two aliases is responsible for an effect.

The matrix for the least squares equations for model 6 is:

\[
\begin{array}{cccccccc}
\text{b}_0 & \text{b}_1 & \text{b}_2 & \text{b}_{11} & \text{b}_{22} & \text{b}_{12} & \text{b}_3 & \text{Yield Sum} \\
32 & 0 & 0 & 64 & 64 & 32 & 0 & 9,331 \\
64 & 32 & 0 & 0 & 0 & 32 & & 3,320 \\
64 & 0 & 0 & 0 & 32 & & & 5,258 \\
256 & 128 & 128 & 0 & & & 18,984 \\
256 & 128 & 0 & & & & 17,324 \\
128 & 0 & & & & & 8,928 \\
32 & & & & & & 2,987 \\
\end{array}
\]

The forward solution of the abbreviated Doolittle method is as follows:
The variance-covariance matrix and the estimates are:

\[
\begin{bmatrix}
16 & 0 & 0 & -4 & -4 & 4 & 0 \\
4 & 0 & 0 & 0 & 0 & 0 & -4 \\
4 & 0 & 0 & 0 & 0 & -4 & 4 \\
2 & 1 & -2 & 0 & \\
2 & -2 & 0 \\
4 & 0 \\
12
\end{bmatrix}
\]

\[
\begin{aligned}
b_0 & = 310.75 \\
b_1 & = 10.41** \\
b_2 & = 70.97** \\
b_{11} & = 0.88 \\
b_{22} & = -12.09** \\
b_{12} & = 3.28 \\
b_3 & = 11.97*
\end{aligned}
\]

Since the error variance in the experiment was 347.01 (with 21 degrees of freedom), \(\sigma^2/128\) is estimated by 2.71. This is multiplied by the diagonal terms to obtain the estimated variances for the estimates. All linear terms and the quadratic term for \(k\) are significant (\(b_3\) barely so at the 5% level) while \(b_{12}\) is about the same size as its standard error. The sum of squares can be compared with those of Yates as follows:
COMPARISON OF DISCRETE AND CONTINUOUS MODELS 57

<table>
<thead>
<tr>
<th>Effect</th>
<th>Yates</th>
<th>Here⁸</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nq</td>
<td>3,465.3</td>
<td>172,225.0</td>
</tr>
<tr>
<td>Kq</td>
<td>161,170.0</td>
<td>269,700.1</td>
</tr>
<tr>
<td>Nq and NqDq</td>
<td>810.0</td>
<td>810.0</td>
</tr>
<tr>
<td>Kq and KqDq</td>
<td>13,986.3</td>
<td>13,986.3</td>
</tr>
<tr>
<td>NqKq</td>
<td>344.5</td>
<td>344.5</td>
</tr>
<tr>
<td>Dq</td>
<td>278,817.8</td>
<td>1,528.0</td>
</tr>
<tr>
<td>NqKqDq</td>
<td>124.0</td>
<td>124.0</td>
</tr>
</tbody>
</table>

This is only an illustrative example, however, and should serve as an example of the procedure. There may be some questions concerning the use of the coded values. These are put in so that the estimators will be as nearly uncorrelated as possible; this enables one to better evaluate the usefulness of various predictors in the model. Box and Wilson (8) generally follow this procedure.

Problem of Adjustment for Available Nutrients With Continuous Models

One of the major needs in the determination of fertilizer response surfaces is a method of adjusting for nutrients available in the soil before the experiment is started. In a single experiment, it is usually assumed that the variation in basic levels is random, with the average level being taken account of by the constant term. If there are no essential differences between the basic levels in the plots for each of the treatments, the results of the experiment can be used to indicate treatment contrasts. However, if a continuous model such as the quadratic model 5 is used, the experimenter should be careful about extending the results to plots with different available nutrients.

If the effect of the available nutrients is to merely increase the actual levels of X, the results can be converted to a prediction equation in terms of the available plus added nutrients. In order to simplify the results, consider a quadratic prediction equation for an experiment involving only one nutrient,

\[
E(X) = \beta_0 + \beta_1X + \beta_{11}X^2,
\]

where X is the added amount of the nutrient. The actual amount (available plus added) of the nutrient in an experiment is designated as \( N = X + d \) (\( X = N - d \)). Then:

\[
E(N) = (\beta_0 - \beta_1d + \beta_{11}d^2) + (\beta_1 - 2\beta_{11}d)N + \beta_{11}N^2.
\]

Now try to apply the results of this experiment to a farm. The

⁸These are not adjusted sums of squares; i.e., \( Nq \) is not adjusted for \( Kq \) or \( Dq \); \( Kq \) is not adjusted for \( Dq \); and \( Nq \) and \( Kq \) not for \( NqKq \). Note the \( Nq = \text{Yates'} NqDq \) and the \( Kq = \text{Yates'} KqDq \), as indicated above.
predicted yield if $X$ is applied is $E(X)$. Suppose the value of $d$ for this farm is $d_0$ ($N = X + d_0$); then the expected yield when $X$ is added should be:

$$F(X) = (\beta_0 - \beta_1 d + \beta_{11} d^2) + (\beta_1 - 2\beta_{11} d) (X + d_0) + \beta_{11} (X + d_0)^2$$

$$= [(\beta_0 - \beta_1 (d - d_0) + \beta_{11} (d - d_0)^2) + [\beta_1 - 2\beta_{11} (d - d_0)] X + \beta_{11} X^2].$$

The bias in using $E(X)$ instead of $F(X)$ is:

$$E(X) - F(X) = (d - d_0) (\beta_1 + 2\beta_{11} X) - (d - d_0)^2 \beta_{11}.$$

One might suppose that even though the predicted yield is biased, at least the difference between the predicted yields for two different levels of farm application would be unbiased. Even this is not true. The bias in the predicted increase in yield for an application of $X_2$ instead of $X_1$ is $2\beta_{11} (X_2 - X_1) (d - d_0)$, which will be negative for $X_2 > X_1$ and $d > d_0$, since $B_{11}$ is expected to be negative; hence, one would tend to underestimate the effect of added nutrients if the available nutrients at the farm are less than at the experimental plots.

These problems become further aggravated when one attempts to combine the results of experiments at two locations with different values of $d$. Suppose $d = d_1$ for one location and $d = d_2$ for a second location, but the same rates of application are used in each experiment, e.g., $X = -1, 0, 1$. If a quadratic model is used, the experimental model $E(N)$ is:

$$E(N) = \beta_0^* + \beta_1^* N + \beta_{11}^* N^2,$$

where $\beta_0^*$, $\beta_1^*$ and $\beta_{11}^*$ can be found from model 8 above. The values of the $\beta^*$ are assumed to be the same for each experiment (neglecting other nutrients in this discussion); however, the values of $\beta_0$ and $\beta_1$ in model 7 are not the same. Let $\beta_1'$ and $\beta_1''$ represent the values of $\beta_1$ in experiments 1 and 2, respectively.

Then solving for the $\beta'$s in terms of the $\beta^*$s, yields:

$$\beta_0 = \beta_0^* + d_1 \beta_1^* + d_1^2 \beta_{11}^*$$

$$\beta_1 = \beta_1^* + 2d_1 \beta_{11}^*$$

$$\beta_1'' = \beta_1^* + 2d_2 \beta_{11}^*.$$

On the basis of the above results, the experimenter would make one of two incorrect decisions if he did not take account of the inequality of the available nutrients for the two experiments:

1. He would conclude that the true response pattern was different at the two localities and publish two prediction equations, each of which represents an inefficient use of the data in estimating the basic parameters. This may prevent the savings in extension work which overall recommendations entail. However, the biases mentioned above are less likely to be so important, because the experimenter realizes his prediction equation is different for different locations.

2. If the experimental error is large compared with $(d_2 - d_1)$, he might conclude that the differences in the estimates of the $\beta'$s was a
chance difference, and use average $\beta$'s for his prediction equation. This would produce the biased results mentioned above. However, the important point here is that the estimates of the parameters are quite inefficient because the large spread in $N$ over both experiments is neglected. There is uncertainty as to which incorrect procedure is worse, since this is a matter of weighing the extra costs of a wide variety of recommendations against the inefficiencies and biases of over-all recommendations.

To illustrate the fact that one can obtain more information regarding the response surface by combining the two experiments, suppose only two levels of $X$ ($X = -1$ and 1) are used for each experiment but the available coded levels are $d_1 = -1$ and $d_2 = 1$. If single estimates are made for each experiment, no estimator of $\beta_{11}$ will be available; hence, if $\beta_{11}$ is not zero, the separate estimators of the linear coefficient will be biased. However, in this case, the pooled estimator of $\beta_1$ will be unbiased because $d_1 + d_2 = 0$. Also, in this case, the objective is to compare the response surfaces in terms of the total nutrients ($X + d$). The number of plots for each level of $N$ and the estimators of $\beta_1^*$ and their expected values when $\beta_{11}^* \neq 0$:

<table>
<thead>
<tr>
<th>Experiment</th>
<th>$N$</th>
<th>$b_1^*$</th>
<th>$E(b_1^*)$</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>-2</td>
<td>$r$</td>
<td>$(N_0 - N_2)/2r$</td>
</tr>
<tr>
<td>2</td>
<td>0</td>
<td>$r$</td>
<td>$(N_2 - N_0)/2r$</td>
</tr>
</tbody>
</table>

In both experiments, $\sigma^2 (b_1^*) = \sigma^2 /2r$. The pooled estimate of $\beta_1^*$ is unbiased and has $\sigma^2 (b_1^*) = \sigma^2 /4r$.

If a combined analysis is made, $b_1^* = (N_2 - N_0 + N_{-2})/8r$, where $N_0$ is the sum of the yields of the $2r$ plots with $N = 0$; $\sigma^2 (b_1^*) = \sigma^2 /16r$. In this case $b_1^* = (N_2 - N_{-2})/4r$ with $\sigma^2 (b_1^*) = \sigma^2 /8r$; note that this variance is one-half the pooled variance. Even if the experimenter wants to assume different values of $\beta_{11}^*$ in each experiment because of unequal amounts of other nutrients, he obtains the same estimate of $\beta_{11}^*$ from the combined data, and the above pooled estimate of $\beta_1^*$.

If a more complicated model is considered, such as an exponential or logistic model, the experimenter will probably find that the inclusion of the available nutrients in the model is just as important. It may be that one of the reasons for obtaining such unrealistic production functions from combined data is the failure to adjust for the available nutrients. Also, this may account for the divergent shape of combined response surfaces when various mathematical forms are used. Someone might make studies similar to these for the more complicated production models.

If one can obtain more efficient and more nearly unbiased estimates by adjusting for available nutrients in several experiments in a combined analysis, why is this not done more often? In many cases, the answer may be lack of knowledge of how to make even the simple
combined analyses. However, the real answer may be generally more complimentary to experimenters:

1. Statisticians have not developed easy and efficient estimation procedures for the more complicated models.
2. Procedures for determining available nutrients are not too well developed.
3. It is often difficult to calibrate available and applied nutrients.
4. Even though only a few nutrients are added in the experiment, adjustments must be made for all available nutrients. This may result in a much more complicated analysis.
5. Research has not been well coordinated. As a result, computations may be complicated and total levels may not be spread out very much in the various experiments.
6. Adjustments for weather factors are also needed, especially when combining data from several years. Crop-weather and soil-weather relationships are even more poorly known than are crop-nutrient relationships.

Much of the computing difficulty will probably be relieved as more use of electronic computers is made. Hence, it should be recommended that coordination of efforts in the direction of setting up realistic models and measuring and calibrating available nutrients is needed.

References Cited

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References Cited


