STATISTICAL APPROACHES TO STUDIES INVOLVING PERENNIAL CROPS

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Two characteristics of perennials which distinguish them from annuals are that they live longer and tend to be larger. It turns out that much of what needs to be said about experimentation with perennials follows from these 2 trivial observations and their consequences.

The fact that perennials live longer than annuals has many implications which must be faced in experimental design and later analysis. First, there is more time for all sorts of mishaps and calamities to occur, meaning that unbalanced data are to be expected. Second, the experimental objectives may shift over time; indeed, the experiment may outlast the experimenter, and with new personnel may come new interests. Third, we may want to provide for the possibility that, when the current experiment ends, the same plants might be carried over into another experiment. And fourth, we are likely to have data from several seasons on the same plants.

That perennials are likely to be larger than annuals also has important implications. Single-plant and several-plant plots will be common; the individual plant's data (versus the aggregate response from many plants in the plot) will be of greater interest, and plant-to-plant variability will be a more important source of variability with perennials.

This paper focuses on issues which are especially pertinent in experimentation with perennials. The emphasis is on field experimentation with large perennials (e.g., tree crops), since the differences between perennials and annuals are most clear-cut and important in these applications. Of course, many of the basic principles of experimental design discussed by other contributors to this symposium, although not repeated in this paper, apply to studies on perennials as well as annuals.

Although standard literature citations appear in the following discussion, I must emphasize the contributions of S. C. Pearce to this entire area; his work has shaped much of my own thinking. Reference (7) is especially useful, but unfortunately not readily available through libraries in this country.

Experimental designs for controlling environmental variation

A central issue in experimental design is the desire to reduce error variance by partitioning out extraneous variability, i.e., by accounting for known or suspected sources of variability other than treatment effects. By doing so we hope to increase the power of tests of significance, sharpening our ability to recognize treatment differences where they exist, and to improve our estimates of treatment means and of the differences between treatment means, reducing standard errors and shortening confidence intervals.

Traditionally, in designing field experiments, we have sought to account for environmental variability by incorporating some form of blocking into the experimental design. Such designs should still be considered when dealing with perennials, but they probably will be less effective than with annuals; with perennials, plots are likely to contain only one or a few plants, so plot-to-plot variability is less due to environment, more associated with plant-to-plant variability. However, we would expect the effects of environmental differences to be magnified over time, so the value of blocking in experiments on perennials should be greater for long-term experiments.

In selecting an experimental design, one should prefer a design which will be workable when the anticipated losses occur and unbalanced data result, and which offers some degree of flexibility when experimental objectives shift or we wish to carry the plants of this experiment into some subsequent one. Fairly simple designs are preferable. Complex designs (e.g., confounded or incomplete block designs) require a utopian view of field experimentation; adopting one invites trouble! The reduction in error variance one *hopes* to realize is not worth the risk that part of the anticipated data will be missing. (We refer to data which are missing through mishap or misfortune, not as a treatment effect.) Unbalanced data can make a simple design complex; a complex design may become a nightmare, and the problems will be interpretational as well as computational. Besides, since with perennials much of the variability is due to the plants themselves, not the environment, the gains in precision will probably be less than imagined. Furthermore, complex designs offer little flexibility for modification or conversion. The following are some brief comments on fairly simple designs which have potential for field experiments with perennials.

Completely random design. The completely random design (or 1-way classification) is worth considering for small experiments. Error degrees of freedom are maximized and, since the experiment can be contained within a relatively small area, blocking is usually less important than for experiments spread over large areas. Unbalanced data cause minimal disturbance.

Randomized complete block design. This is the most commonly-used design with perennials as with annuals. As Rigney (12) observed, ". . . intimate contact with several hundred analyses involving almost every conceivable type of design leaves one with an almost reverent attitude toward a simple randomized block design. . . . considerable thought should be given to the problem before using a layout which cannot be alternatively analyzed as a randomized block." With the randomized complete block design, blocks and treatments are orthogonal, and one will still have a randomized complete block design if entire blocks or treatments are lost or deleted, as might occur if a block is flooded badly or a treatment fails utterly. Missing plots are fairly easy to handle.

Blocks can be at different locations to broaden the inferences which can be drawn from the data, as suggested by Monroe and Mason (4). One can also form blocks of a convenient size to serve as work units in the carrying out of the experiment, rather than solely to account for environmental variation; an entire block then can be sprayed on the same day, harvested on the same day, and so forth. This important use of blocking is frequently overlooked.

It is often possible to superimpose new treatments on old treat-

Table 1. Example of a design obtained by superimposing new treatments over old treatments which had been arranged in a randomized complete block design.

		Block 1	2	3	4
Old treatment	A	Aa	Ab	Ac	Ad
	B	Bd	Ba	Bb	Bc
	C	Cc	Cd	Ca	Cb
	D	Db	Dc	Dd	Da

Capital letters specify old treatments; small letters specify new treatments.

Source of variation	Degrees of freedom
Total	15
Blocks	3
Old treatments	3
New treatments	3
Residual	6

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ments which were arranged according to a randomized complete block design — provided the original design is sufficiently intact. The details of how this might be done depend on the numbers of blocks and old treatments from the randomized complete block design, and on the number of new treatments. Table 1 presents a tiny example to illustrate one simple possibility. The old treatments would, of course, have been randomized within each block. In Table 1, the new treatments are superimposed on the old with the restrictions of a Latin square, i.e., each new treatment is assigned once to each old treatment ("row" of the Latin square) and once in each block ("column" of the Latin square). The sources of variation and degrees of freedom are as in a standard Latin square analysis, where old treatments and blocks are the familiar "rows" and "columns," respectively. Use of the residual as "error" assumes that interactions are negligible. The example shown is an orthogonal design; orthogonality would be preserved if this example were enlarged to include 8, 12, 16, or any number of blocks which was a multiple of four, the number of new treatments. If Block 4 were deleted in the example given, the result would be a Youden square with new treatments orthogonal to blocks and in a balanced incomplete block design with respect to old treatments.

As is well known, if the number of treatments is large, the block size of a randomized complete block design may become too large to account effectively for environmental variation; the environmental variability *within* a block becomes too great and the value of blocking is reduced.

Latin square design. The Latin square is often said to be of limited usefulness because the numbers of rows, columns, treatments, and replications of each treatment must be equal, and small Latin squares provide too few degrees of freedom for error, while large ones may give more replication than desired and have rows and columns which become too long to account well for environmental variability.

However, when the number of treatments is small, one might consider using several Latin squares and analyzing them together, thereby increasing the error degrees of freedom and overcoming the objection noted above. If the Latin squares are "tied," i.e., placed side-by-side so that they share common rows (or columns), then even more degrees of freedom can be obtained for error.

Latin squares can suffer some loss of data without too much resultant difficulty. Also, new treatments can readily be superimposed on old treatments, provided there are equal numbers of new and old treatments, and interactions are negligible. The assignment is handled analogously to what was done in the example of the randomized complete block design (Table 1). Now each new treatment must appear once in each row, once in each column, and once with each old treatment. The resulting design is known as a Graeco-Latin square. Table 2 illustrates the conversion of a 5 × 5 Latin square to a Graeco-Latin square. Not all Latin squares permit this conversion, so, in planning an experiment, one might for insurance choose a Latin square which one knew could be converted later if desired; no 6 × 6 Graeco-Latin square exists (2), so selecting a 6 × 6 Latin square initially would preclude later conversion.

Split-plot design. Split-plot designs, which by definition involve a factorial set of treatments, are most commonly adopted for convenience in applying or containing 1 type of treatment (or factor) in relatively large areas as, e.g., when working with sprays or infections. The 1 factor is applied to the large areas (main plots) which are then subdivided into smaller units (subplots) to accommodate the levels of the other factor; the second factor is thus run in blocks which are the main plots. A split-plot design is also recommended for cases where differential precision is desired; by sacrificing precision on the main plot factor, one can gain precision on the subplot factor and on the interaction of factors.

The split-plot design may also evolve during the course of an experiment. The plots of an ongoing experiment, say, a randomized complete block design, can sometimes be split to accommodate an additional factor, provided the nature of the plots and the factors involved permit this splitting. This may be useful when, during the course of an experiment, interest develops in a factor not originally incorporated into the design, or in the interactions of this new factor with the original factor or factors.

More complex designs. If a more complex experimental design must be used, it is most likely to succeed in a short experiment using established plants. The brevity of the experiment minimizes the likelihood of mishap which could unbalance the data; the use of established plants reduces the risk of plant loss and, again, unbalanced data, and means that environmental effects have operated longer, enhancing the prospects that fancy blocking will be worthwhile.

Use of covariates

Experiments with perennials are usually long, large, and expensive. The plots themselves occupy a great deal of space, and then even more land may be required for guard rows or alleys, external guards, and so forth. When comparing varieties or pruning methods, it may be enough to allow sufficient space between plants to eliminate competition effects and to permit movement of equipment; with sprayings, infections, or soil treatments, additional guards between plots are often necessary to contain treatment effects.

We need to find ways to reduce standard errors and improve the power of tests of hypotheses other than through increased replication within a standard experimental design. The analysis of covariance is often applicable and effective; the use of covariates is often worthwhile in addition to, not necessarily instead of, a traditional experimental design.

When the covariate is measured on the experimental plants themselves, covariance is sometimes referred to as "calibration." Calibration has been used successfully on a wide variety of perennials. Loosely speaking, calibration compares the performance under treatment with what might have been expected in the absence of treatment. It works best on single-plant plots, as one would expect. For "crop" as the dependent variable of interest, useful covariates or calibrators

Table 2. Example of obtaining a Graeco-Latin square by superimposing new treatments over old treatments which had been arranged in a Latin square.

		Columns				
		1	2	3	4	5
Rows	1	Aa	Bb	Cc	Dd	Ee
	2	Ed	Ae	Ba	Cb	Dc
	3	Db	Ec	Ad	Be	Ca
	4	Ce	Da	Eb	Ac	Bd
	5	Bc	Cd	De	Ea	Ab

Capital letters specify old treatments; small letters specify new treatments.

Source of variation	Degrees of freedom
Total	24
Rows	4
Columns	4
Old treatments	4
New treatments	4
Residual	8

Table 3. Some applications of calibration (covariance) where yield or crop is the dependent variable.

Crop	Preferred covariates	Reference
Apples	Previous 2-year crop	(5)
Apples	Trunk circumference or cross-sectional area	(8)
Apples	Trunk measurements after periods of good growth, poor crop; previous crop after periods of high yield	(9)
Apples	Trunk cross-sectional area	(17)
Apples	Trunk circumference	(19)
Apples	Previous crop	(20)
Apricots	Trunk circumference	(11)
Blueberries	Number of shoots	(13)
Cocoa	Previous crop	(18)
Cocoa	Previous 2-year crop	(3)
Coffee	Stem diameter	(1)
Grapes	Visual ratings of vines	(13)
Oranges	Previous crop	(6)
Pears	Previous crop and trunk circumference	(9)
Pecans	Previous crop	(14)

have been: crop in the previous year or years, trunk diameter or circumference, number of shoots (for blueberries), and visual ratings of vines (for grapes); little is known about the usefulness of calibration for dependent variables other than crop (or yield). Table 3 summarizes some of the literature on application of calibration.

The use of calibration often reduces error variance by 25–50% (5, 8, 9). Calibration is likely to be most successful when the covariates are measurements on established plants which have not been under different treatments recently. However, calibration can partially account for residual effects of past treatments, as well as for genetic or environmental differences. Calibration cannot be used when the treatments are inherent from the start (e.g., varieties) or are initiated early (e.g., graftings or pruning methods).

Analysis of covariance can be useful in ways other than calibration — for example, employing covariates which characterize plot differences (soil type, available nutrients). One can also attempt to account for environmental effects by covariance adjustment based on measurements from neighboring plots (10); for each plot in the experiment, calculate the difference between the response in that plot and its treatment mean, and then use as the covariate for each plot the average of these differences from some number of surrounding plots. Clearly, the neighboring plots must be nearby, suggesting that this approach is most likely to be useful in the absence of guards, unless the guards themselves provide the covariate.

If one were laying out an orchard, perhaps for future unspecified use, a checkerboard of control and experimental plots might merit consideration (N. Scott Urquhart, personal communication). Analysis of covariance would follow naturally. The checkerboard orchard might be especially appropriate when the nature of the treatments (e.g., varieties, graftings) precluded use of calibration.

One is not limited to the use of a single covariate or calibrator. Similarly, covariates can be transformed to improve linearity. Although there are exceptions, as a rule one should avoid covariates which themselves are affected by the treatments.

Data collected over years on the same plants

With perennials, the typical data are from a single experiment conducted over years, and are repeated measurements on the same plants. Several approaches to the data analysis seem possible. One can simply sum over years for each plant, then analyze the total response (total crop, total growth). If the total response is the variable of interest, then this approach may be adequate, but the ability to test for interaction with years is sacrificed.

The total time of the experiment can be divided into time periods for analysis, in which case "period" becomes a factor in the treatment set, providing a final split of the submost plots. The dependent variable would be, say, the yield within each period, and the analysis would be as a split-plot design. The periods used should be chosen with some thought. For example, many perennials are biennial croppers; for them, defining 2-year (or at least even-year) periods might be appropriate, whereas 1-year periods would probably lead to heterogeneous errors. Summing over more than one year generally has a beneficial smoothing effect, since most crop yields fluctuate from year to year.

Other approaches to the data analysis also have been tried or suggested. Stevens (16) has successfully analyzed total crop and then differences between periods. Finally, Steel (15) has proposed a multivariate analysis, taking each period's data as a separate variate; I am unaware of any application where this approach has proven effective, but I doubt that it has been attempted often, since I have detected little enthusiasm among horticulturists for multivariate analyses.

Summary

Since perennials, by definition, live longer than annuals, and since they are likely to be larger, experiments with them face special problems. The experiments often run for years, so the plants are at risk longer, increasing the likelihood of mishap and thus unbalanced data. Whimsical (some would say malevolent) "Mother Nature" has greater opportunity to flood plots and blow down trees. A careless worker has more chance to mangle a plant with a tractor, or apply a treatment to the wrong plot. The experimental design must be able to tolerate the inevitable missing data.

Experimental objectives may change over time, requiring modification of the original study plan. And, at the conclusion of one experiment, we may wish to carry over the same plants into a new trial. We therefore favor experimental designs which are flexible. Simple experimental designs meet our requirements best; complex designs are inflexible and are seriously undermined by missing data in both computation and interpretation.

Experiments with large perennials will probably use single-plant or several-plant plots. Proportionally, plot-to-plot variability will be less due to environmental effects and more due to plant-to-plant variability than with annuals. Although use of traditional simple experimental designs seems appropriate, reducing error variance solely by accounting for positional effects through blocking probably will be less successful with perennials than with annuals.

Attempting to reduce error variance by merely increasing replication within a standard design may not be satisfactory. The analysis of covariance has proven to be a very effective alternative in many cases, often reducing the error variance by 25–50%.

The data will probably include several observations collected over time on the same plant. The statistical analysis must take this into account, and several possible methods are suggested.

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The purpose of an experiment is to answer questions. The truth of this seems so obvious, that it would not be worth emphasizing were it not for the fact that the results of many experiments are interpreted and presented with little or no reference to the questions that were asked in the first place. In other cases, it appears that the wrong questions were asked.

Interpretation of dose and response

Let us look at an example. An experiment was performed in which there were 7 treatments consisting of 7 levels of a material applied to the plants. The simple, straightforward, and useful question that might have been asked by the experimenter is: What is the relation between the amount of material applied and the plant response? Then there is a much more cumbersome and less useful question: Of the 21 possible pairs of treatments, which ones are significantly different from each other?

Can it be that this second question is really the one the experimenter had in mind when the experiment was planned? At any rate, that is the question that was answered when the results were reported like this:

Treatment Level	Plant Response
0	222 a
1	202 ab
2	205 ab
3	186 bc
4	164 cd
5	156 cd
6	147 d

Suppose the simpler question of the relation between dosage and response had been asked and answered. It would have been reported that there was a highly significant linear relation that accounted for over 96% of the variation in response. This could have been illustrated graphically, as in Fig. 1.

Partitioning of treatment sum of squares in Example 1, Variable 1

Source of variation	df	SS	Percent of total
Treatments	6	23805	
Linear	1	22885	96%
Residual	5	920	4%

The question of which treatment responses are significantly different from each other is now irrelevant, and it need not, in fact, should not, have been asked. Once a significant linear trend is established, all treatment levels within the range of those used in the experiment are significantly different from one another in their effects. The best estimates of the treatment effects are the points on the regression line.

There are, of course, other kinds of relations between dosage and response besides simple linear ones, such as various curvilinear relations.

One of the variables measured in this same experiment showed a response to treatments that was obviously not linear. (Incidentally, variables should never be referred to as "parameters".) The data were presented as follows:

Treatment level	Response
0	9 d
1	12 cd
2	15 bc
3	22 a
4	19 ab
5	17 b
6	11 cd

Interpreting these results by examination of the letters is even more confusing than in the previous example. However, partitioning the treatment effects into individual degrees of freedom shows that over 85% of the variability among responses is accounted for by the linear and quadratic components. The data can therefore be simply summarized by a simple second-degree curve (Fig. 2).

Partitioning of treatment sum of squares in Example 1, Variable 2

Source of variation	df	SS	Percent of total
Treatments	6	650.0	
Linear	1	71.5	11%
Quadratic	1	482.0	74%
Cubic	1	40.8	6%
Residual	3	55.7	9%

Whenever the treatments consist of a series of dosage levels, an effort should be made to find some meaningful relation between dosage and response, rather than resorting to a confusing and almost meaningless multiple comparison procedure.

Factorials

Let us look at another example. This is a 2 × 2 × 2 factorial. In other words, there are 3 factors, each applied in 2 different ways with all 8 possible combinations applied. This differs from the previous example in that no trends are involved. The logical questions to ask are: What is the effect of each factor? Are there interactions, or in other words, does the response to one factor depend on the level of another factor?

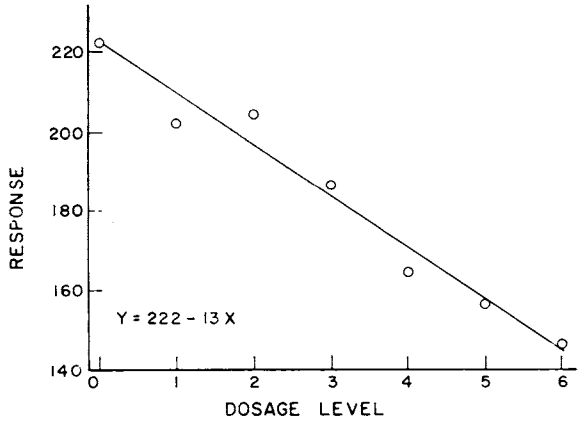

Fig. 1. Linear effect of 7 dosage rates on plant response.

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It wasn't these simple questions that were answered in the presentation of the data. Instead, the main question that was answered was: Which of the 28 possible pairs of treatment responses differed from each other significantly? Here is the way the data were presented:

Treatment combination	Response
ABC	10.4 b
ABc	8.5 b
AbC	15.2 ab
Abc	13.0 ab
aBC	21.4 a
aBc	18.6 ab
abC	22.6 a
abc	22.7 a

These letters don't tell us much, but if we partition the treatment sum of squares into main effects and interactions, we find that factor A had a highly significant effect, accounting for over 83% of the total treatment variation. Factor B accounted for another 12%, and there was no evidence of any effect of factor C or any of the interactions.

Partitioning of treatment sum of squares in Example 2

Source of variation	df	SS	Percent of total
Treatments	7	877	
Factor A	1	730	83%
Factor B	1	107	12%
Factor C	1	23	3%
A × B	1	8	2%
A × C	1	1	
B × C	1	3	
A × B × C	1	5	

The important facts to present are therefore the main effects of factor A and B:

Factor	Average effect
A	11.8
a	21.3
B	14.7
b	18.4

Actually, the means of the main effects were presented in a table, but 4 out of the 6 means were incorrect! Here are the means presented alongside the individual treatment means:

Individual treatment means		Main effects: Calculated	Main effects: Published
ABC 10.4	A	11.8	11.8
ABc 8.5	a	21.3	21.3
AbC 15.2			
Abc 13.0	B	14.7	17.8
aBC 21.4	b	18.4	15.3
aBc 18.6			
abC 22.6	C	17.4	20.5
abc 22.7	c	15.7	12.6

Unfortunately, mistakes of this kind are not uncommon in the pages of our journals. What is worse, they are seldom corrected in subsequent issues. Published papers are the permanent records of scientists' work, and every effort should be made to avoid presenting erroneous results. Tables and graphs should agree, and statements in the text should be borne out by the presented data.

It is all too easy to blame mistakes on a secretary, a statistical clerk, or the computer, but the ultimate responsibility for accuracy belongs to the authors. Some experimenters act as though operating a calculating machine is beneath them, and statistical analysis is a menial task

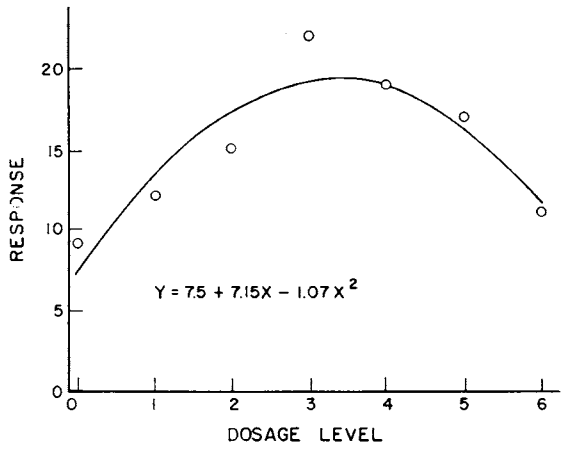

Fig. 2. Quadratic response of 1 variable to dosage level.

that should be relegated entirely to secretaries or clerks. Their work often reflects this attitude.

The next example combines both factorial and trend analysis, and illustrates how interactions can be interpreted and presented. It is a 2 × 2 × 4 factorial, with factor C consisting of 4 exposure times. Obvious questions would be: What are the effects of factors A and B? Is there a relation between response and exposure time? Are there any significant interactions among the 3 factors?

The question that was actually answered in the presentation of the data was: Within each level of factor C, which of the six pairs of treatments differed from each other significantly? Here are the results:

Level of A	Level of B	Level of C: 0	1	2	3
1	1	74 c	92 c	108 b	134 b
1	2	48 b	108 d	156 c	292 d
2	1	12 a	18 a	76 a	92 a
2	2	18 a	58 b	90 ab	162 c

A partitioning of the 15 degrees of freedom for treatments tells us much more about the effects of the various factors and their interactions.

Partitioning of treatment sum of squares in Example 3

Source of variation	df	SS	Percent of total
Treatments	15	216,083	
Factor A	1	44,287	20.5%
Factor B	1	19,927	9.2%
A × B	1	817	0.4%
C linear	1	113,274	52.4%
C quadratic	1	2,977	1.4%
C cubic	1	163	0.1%
A × C linear	1	1,717	0.8%
B × C linear	1	21,094	9.8%
Residual	7	11,827	5.5%

There were only 4 comparisons that were significant, and these accounted for 92% of the variation among treatments. These were: the main effect of factor A, the main effect of factor B, the linear trend of factor C, and the interaction between factor B and the linear trend of factor C. These results can be summarized graphically (Fig. 3 and 4).

This experiment illustrates another important point. The overall F value for the treatment sum of squares based on 15 degrees of freedom is meaningless. This is because it is the average of 4 highly significant single degrees of freedom and 11 non-significant ones. The idea that one should proceed no further with an analysis, once a non-significant F-value for treatments is found, has led many experimenters to over-

look important information in the interpretation of their data.

Let's look at one more example. This example consisted of 7 treatments. There was a "control" and 3 levels of a material in the ratio of 2:5:10 applied with and without an additive. It is not my assignment to criticize design, but good interpretation starts with good design. This was not a very good design. It was not a complete factorial. A treatment consisting of the additive alone would have made it so. Furthermore, there seems to be no logical justification for the particular series of treatment levels chosen. Generally, when studying the relation between treatment level and response, a series of rates in arithmetic progression is the most efficient.

Here is the way the results were presented:

Treatment	Response
0 level	81 a
2 level	77 ab
2 level + additive	74 ab
5 level	67 bc
5 level + additive	66 bc
10 level	50 d
10 level + additive	56 cd

Actually, in spite of the poor design, the experimenter was lucky and didn't know it. Partitioning the treatment effects shows that there was no evidence whatsoever of any effect of the additive, or any interaction between additive and level of material.

Partitioning of treatment sum of squares
Example 4, Initial Partitioning

Source of variation	df	SS	Percent of total
Treatments	6	3,216	
Control vs. others	1	915	28%
Linear among others	1	2,249	70%
Additives	1	0	0%
Remainder	3	52	2%

This being the case, we can disregard additives and consider the 7 treatments as consisting of 1 treatment at the zero level and 2 treatments each of 3 other levels. We can then carry out a regression analysis, and we find that linear regression accounted for over 98% of the variability among treatments. This result can be neatly summarized on a graph (Fig. 5).

Partitioning of treatment sum of squares
Example 4, Final Partitioning

Source of variation	df	SS	Percent of total
Treatments	6	3,216	
Linear	1	3,164	98%
Deviation From linear	5	52	2%

Partitioning of treatments

In each example I have given, I have mentioned the partitioning of treatment effects as though the technique for doing this were common knowledge. Unfortunately, this may not be the case. Over many years of participation in short statistical refresher courses and seminars for agricultural research workers, I have made an alarming observation. Nearly every participant knew how to calculate LSD, and in recent years, Duncan's multiple range test. Still, less than 10% knew how to partition a treatment sum of squares into meaningful comparisons. This is too bad, for the technique is so powerful and yet so simple.

I am glad to note that in the last couple of years there has been an increase in the use of this technique reflected in our journals. There may be several reasons why it is not more widely used. Although it is described in nearly all statistics texts, it is usually under some formidable name such as "orthogonal comparisons", "orthogonal linear forms", or "single degrees of freedom." Furthermore, the discussions often tend to be clothed in unnecessarily complex mathematical jargon and symbolism.

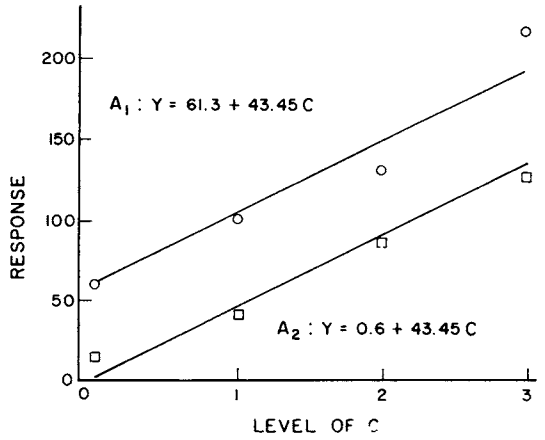

Fig. 3. Linear response to factor C at 2 levels of factor A, showing lack of interaction.

Let's face it, there is probably another reason why the technique is not more widely used. People in any profession tend to copy each other, and horticulturists are no exception. "Dr. John Doe used Duncan's multiple range test in presenting his results, so that's good enough for me."

I should hasten to add that there are situations where multiple comparison procedures, such as Duncan's multiple range test, are appropriate. Such would be the case when testing a random assortment of cultivars or chemicals. Even in these cases, the investigator should ask whether the treatments fall into groups, the comparison of which would provide important information. Cultivars, for example, might be classified into those which are resistant and those which are susceptible to a certain disease, and a comparison made between the 2 groups.

Insignificant digits

For my final comments on the presentation of data, I am indebted to Dr. M. T. Vittum of Cornell for his suggestions. They deal with false accuracy in publishing results. The U.S. Coast and Geodetic Survey plaque at the summit of Mount Whitney is an extreme example. This shows the altitude to be 14,496.811 ft.! I was chided by an engineering dean for ridiculing this claim, because, he said, we biologists just didn't understand how precisely the engineers could measure things. I

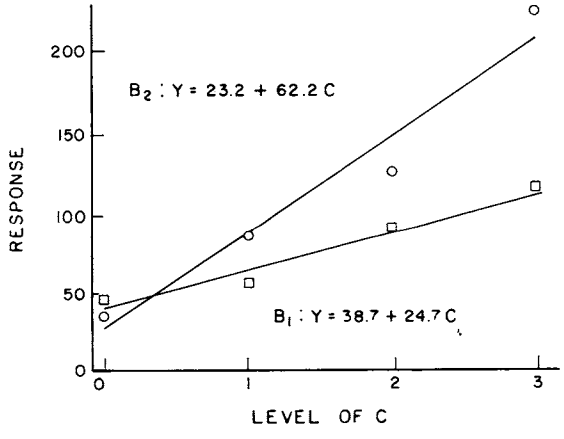

Fig. 4. Linear response to factor C at 2 levels of factor B, showing interaction.

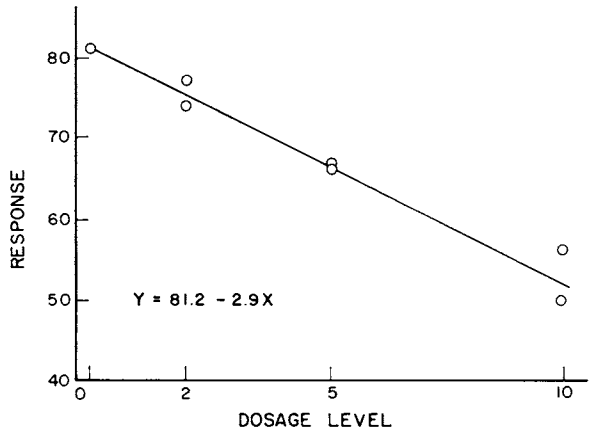

Fig. 5. Negative linear response to dosage level, disregarding non-significant treatment factor.

agreed. We biologists are not entirely blameless in this matter. While we do not generally go to the extremes in the above example, we do often pretend that our data are more accurate than they really are. In making measurements, more than 3 significant digits are almost never justified. Even in reporting means, only in cases where we are dealing with material with a very low coefficient of variability, or a very large number of replicates, are we justified in reporting 4 significant digits.

A common cause of reporting results with too many significant digits is the conversion of measurements from English to metric units. In questioning the results of one paper, I was told that part of the results had been given in pounds and part in kilograms, and if I would multiply the error mean square by 0.45359237^2 = 0.2057460381222169, everything would come out all right! In one paper, we were told that the rate of application of spray was 1402 liters/hectare, but the author is at least to be commended for telling us that this was 150 gallons/acre. In another paper, it was stated that the turf grass was cut to a length of 0.635 cm. Evidently a mower was used which was calibrated in quarter-inch increments.

Even statisticians, who should know better, sometimes exaggerate the accuracy of their statements. For a statistician to tell an experimenter that the probability of obtaining an F value greater than that which was observed is 0.068, is inexcusable. Yet this statement was made in a recent paper.

Suggestions for improvement

In conclusion, I would make the following suggestions:

1) In planning an experiment, decide definitely what questions you want to answer, and design the experiment to answer these questions.

2) In presenting the results, tell the reader what questions the experiment was designed to answer.

3) Interpret the results as answers to the questions you asked in the beginning.

4) Don't deceive yourself or the reader with exaggerated claims of accuracy.

5) Strive to avoid mistakes and inconsistencies in the final presentation. If you take the credit for the paper, the mistakes are yours too!

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