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THE CONTROL OF ENVIRONMENTAL VARIATION WITHIN THE EXPERIMENTAL AREA

By S. C. PEARCE

Institute of Mathematics and Statistics, University of Kent, Canterbury, CT2 7NF, UK

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SUMMARY

In any experiment the land will not be completely uniform and it is helpful to have ways of allowing for the variation. Some sources, for example altitude, will be obvious; some will be permanent but less obvious, such as depth of topsoil; and some may depend upon season and so be unpredictable. Various methods of local control have been suggested. Where the experimenter has good knowledge of the land it can be divided into blocks, each as far as possible uniform within itself. Then comparisons are made within the blocks rather than within the area as a whole. Where such knowledge does not exist, it is sometimes reasonable to make an assumption about the fertility pattern and make use of that, as in a row-and-column design like a Latin square. There is also the possibility of judging the fertility pattern from the data themselves and assessing the performance of a plot by reference to that of its neighbours.

The approach will be to generate bodies of data on the computer to form realizations of diverse fertility patterns and to use all methods on all realizations, noting success and failure.

When the variation forms a trend, blocks succeed only if they are aligned along fertility contours; the other methods do not depend upon orientation. Row-and-column designs can fail badly if the rows and columns interact. Some random variation is inevitable and it makes all methods less effective, especially nearest-neighbour methods, which can fail also when there are discontinuities. Random patches of different soil types are very difficult to deal with and any method might fail.

INTRODUCTION

The problem

In any field experiment variable fertility within the area can cause difficulty. Some sources are obvious, like altitude or proximity to water, but others are not so obvious, for example variable depth of topsoil or a strip of compaction left by a diverted path. Other sources are sporadic and unpredictable, like storm damage or an area of impeded drainage that becomes apparent only in a wet season.

Most methods of local control have been developed at long-established research institutes where experiments are conducted on land known to be even, but much research has to be conducted on land that has only recently been cleared and has little recorded history. Further, even if uniform land is available, some experiments to be meaningful have to be conducted on land of the sort used by local farmers.

Any method of controlling variability can fail. Elaborate ones that require many degrees of freedom can do especial harm. Let S with e degrees of freedom be the sum of squares for error when no attempt is made to control variability, becoming $\acute{S}$ with $(e-f)$ if control is attempted. By ill fortune, S' could equal S thus increasing all standard errors by a factor equal to the square root of $e/(e-f)$. The larger the value of f, the greater the harm that can arise from injudicious use, so it is a mistake to choose a complicated method in the belief that it must be very effective. It may prove to be so but, if it fails, it may do especial harm.

The methods to be studied

The oldest method of local control is the use of blocks, i.e. dividing the experimental area into smaller parts, each of which is as far as possible uniform within itself. This method is designated here as BL. The commonest design is randomized complete blocks, in which each block contains a complete replicate of the treatments, but there are other possibilities, such as balanced incomplete blocks. Whatever the design, it is important that the treatments in each block should be allocated at random to the plots. A good knowledge of the site is essential for the method to be used effectively.

Another established method, designated here as RC, is to use rows and columns, as in a Latin square. In effect this gives two blocking systems at right angles. Usually, each row and column contains a complete replicate of the treatments, but other possibilities exist, e.g. a Youden square. Again, randomization is essential. The design exemplifies spatial methods in which detailed knowledge of the area is replaced by an assumption about the likely fertility pattern – in this instance that the fertility of a plot is the sum of two components, one derived from the rows and one from the columns.

The third method is based on the work of Papadakis (1937), who saw soil variation in terms of patches rather than trends and proposed the adjustment of each plot by the performance of its neighbours. If they are doing well its own performance should be partly discounted but if they are doing badly it is credited accordingly. His was the first of the so-called nearest-neighbour methods. Though old, it is less well known because of the extended computations it requires – at one time a serious obstacle to it use. The approach has been studied recently in various forms (Pearce, 1998). Two of these, P2i and P3i, are used here, though renamed respectively N1 and N2. In N1 the data are adjusted by a covariate intended to estimate the fertility of each plot from the performance of its neighbours assuming a quadratic fertility surface locally. Note, corner plots do not have enough neighbours to determine a quadratic surface so a plane has to suffice. Outside plots with five neighbours present no difficulty. In N2 a second covariate is added in which plots are accounted neighbours only if they share a boundary. The intention is to derive methods of local control in which a feature in one part of the field does not affect adjustments elsewhere.

Other methods exist. The paper of Wilkinson *et al.* (1983) was read and discussed at a meeting of the Royal Statistical Society. In their reply to the discussion the authors proposed a nearest-neighbour method that appeared to meet all objections. Later, Gleeson and Cullis (1987) developed it by assuming that the yields from a row of plots would form an autoregressive series. That made it a spatial method despite its origins. The idea was extended to two dimensions by Cullis and Gleeson (1992). Their paper gives a number of examples in which their method was more effective than rows and columns in reducing standard errors. Other good results were reported by Kempton *et al.* (1994). These successes were mostly with cereal crops on fields of research institutes, i.e. in conditions where only trends were to be expected and random error would be at a minimum. In rough conditions it might be unwise to assume any such relationship.

Similar considerations apply to most spatial methods. At one extreme are experiments on even land with plants in competition with one another so as to exploit the area fully. Here spatial methods often do very well. At the other extreme are experiments on uneven land with varying depth of topsoil, perhaps even with rocks breaking the surface. In such conditions it would be unwise to make any assumptions at all about fertility patterns. The present study is directed to the conduct of experiments in rough conditions, possibly with appreciable random error.

STATISTICAL METHOD

The statistical approach

The approach was by simulation. Bodies of data were generated on the computer to exemplify features that might be found in the land. For each pattern 3600 realizations were generated and analysed by all four methods, BL, RC, N1 and N2.

This approach had the advantage of providing a test of bias. This is important because there is a widespread suspicion that the nearest-neighbour methods lead to bias in the estimation of treatment differences. The regression coefficient of the calculated treatment means on the values used in generating the data should equal 1. In what followed for both N1 and N2, mean values of the regression coefficients based upon 3600 realizations were always found to be 1.00. Any randomization gives advantage to one treatment or another by allocating it to better plots, but the mean values showed that there was no bias.

Generation of data

Methods were tested on an array of sixty-four square plots in eight columns of eight rows with sixteen treatments, which were equally replicated and assigned at random to the entire area. They were numbered consecutively and the numbers, after being scaled to a mean of 100.0 and a variance of 12.0 over the plots, were used as treatment parameters. Where blocks were required, they were formed along columns. On account of the randomization being over the whole area, the design for BL and RC was usually non-orthogonal. That is to say, each block contained a random selection of treatments, not a complete replicate and similarly with rows and columns. In order to calculate the error with BL use was made of an already published algorithm (Pearce, 1987). Since the rows and columns were mutually orthogonal, a simple extension would cope with RC.

Fertility patterns were formed by the addition of components, each exhibiting a desired feature. Perhaps the need is for a steady trend in a given direction, or a discontinuity, or an interaction. The values required would then be scaled to give a desired variance and added plot by plot to the data. The variances of the components would sum to 12.0, the same as that given by the treatments. (If components are not mutually orthogonal the total will not be exact but it will be near enough.) The intention is to generate data in which the significance level of the treatment differences will be about 0.05. Of itself the requirement is of no importance but it keeps the data within the limits of what might be found in practice.

RESULTS

Generalities

The following tables set out two kinds of result. First there are the percentage reductions in standard error brought about by use of the four methods. The degrees of freedom for error and treatments are such that the worst results possible for Tables 1 to 5 are: BL, −8.2; RC, −18.8; N1, −2.2; and N2, −4.4.

The tables show also the values of the regression coefficients used by N1 and N2 to adjust data to standard values of the covariates. For N1 there is only one, b. The covariate is intended to estimate the fertility so b should be about 1, but it is more important that it should be nearly constant for all patterns. If some sources of variation give one value and some another, the statistical analysis will estimate an intermediate value that will suit neither. For N2 there are two regression coefficients, b_1 for the main covariate and b_2 for the subsidiary. Again, constancy over a range of patterns is desirable. Note that these regression coefficients are not those already used to study bias.

Trends

The first study dealt with a very simple fertility pattern, i.e. a steady trend not necessarily parallel to either rows or columns. Taking an origin at the mid-point of the area with axes parallel to the sides, co-ordinates x and y were assigned to the mid-point of each plot. Then each plot was assigned the value $x\cos\theta + y\sin\theta$, where θ is an angle in the range 0° to 90°. A change in θ rotates the pattern relative to the rows and columns,–being the angle between the fertility contours and the columns (blocks).

Table 1 shows that method BL was very effective when blocks lay along the fertility contours but was harmful when they lay across them. The other methods were all effective regardless of orientation. These results can be extended, however. They will be found for any set of trends, whether steady or not, provided those for the x- and y-directions act independently. By way of illustration, Table 1 was recalculated with the column effect x replaced by x^3 and with a row effect

Table 1. Results for steady trends. Fertility contours are inclined by an angle θ to the columns, which form the blocks.

	Reduction in standard error (%)				Regressions		
θ	BL	RC	N1	N2	b	b_1	b_2
0°	99.7	99.1	99.2	99.4	1.000	1.002	−0.002
15°	72.5	99.1	99.2	99.4	1.000	1.003	−0.002
30°	47.0	99.1	99.2	99.4	1.000	1.002	−0.002
45°	24.9	99.1	99.2	99.4	1.000	1.002	−0.002
60°	8.0	99.1	99.2	99.4	1.000	1.002	−0.002
75°	−2.4	99.1	99.2	99.4	1.000	1.003	−0.002
90°	−6.0	99.1	99.2	99.4	1.000	1.003	−0.003

Table 2. Results with an interaction. The first line is copied from Table 1 for θ = 45°. The others show the effect of increasing, p, the proportion of variance due to an interaction.

	Reduction in standard error (%)				Regressions		
p	BL	RC	N1	N2	b	b_1	b_2
0.00	24.9	99.1	99.2	99.4	1.000	1.002	−0.002
0.05	23.2	74.6	98.4	98.5	1.002	1.045	−0.047
0.10	21.3	64.1	98.0	98.1	1.003	1.079	−0.082
0.20	17.9	49.3	97.3	97.6	1.005	1.134	−0.139
0.40	11.2	28.2	96.4	97.1	1.010	1.208	−0.217
0.60	5.2	12.2	95.8	96.9	1.014	1.261	−0.272
0.80	−0.6	−1.3	95.5	96.9	1.019	1.298	−0.310
1.00	−6.0	−13.2	95.2	97.1	1.024	1.331	−0.346

derived from a cosine curve between the limits 0° and 180°. The new figures differed little from those in Table 1. All reductions in standard error were repeated within limits of ± 0.1. For the regression coefficients, all values of b were 1.000, while all those for b_1 and b_2 lay within ± 0.001 of their former values.

Interactions

So far the fertility pattern has been formed from independent components of x and y, but if they interact, the regularity of Table 1 is disturbed. That can occur in several ways. If, for example, the experiment is on a slope, the rows might represent altitude and the columns exposure to wind. If those two features interact, so will rows and columns. Another possible reason is a ridge or depression running diagonally across the area.

Table 2 sets out the results when a component derived from xy was included and contributed a proportion, p, of the variance to be controlled, the rest being as for θ = 45° in Table 1.

For BL the introduction of an interaction led to reduced effectiveness. For RC the effect was very marked, but N1 and N2 were less affected. For small values of p the regression coefficients were little changed, so the effect of the interaction could be controlled with the trends.

Table 3. Results with discontinuities. The discontinuity follows a diagonal across the area. In the first line it represents a former field boundary and in the second a forgotten path. In the third, both effects exist and are of equal importance.

	Reduction in standard error (%)				Regressions		
Cause	BL	RC	N1	N2	b	b_1	b_2
Boundary	16.2	43.4	73.3	72.8	0.983	0.354	0.690
Path	−5.8	−12.9	1.4	21.2	0.160	−0.999	2.297
Both	4.4	10.4	20.1	45.0	0.623	−0.883	2.055

Discontinuities

Discontinuities are not always apparent but if they exist they can do a lot of harm. Ideally experiments are carried out on land that has been farmed in one piece for a long time, but there could be former field boundaries. Also, forgotten paths and roads may have left strips of compacted soil, which can last a long time. Archaeologists trace ancient paths by taking aerial photographs of growing crops, and Roman roads can be located by noting strips of sparse growth in modern forests.

Table 3 sets out some examples. In the first line it is supposed that a forgotten field boundary once ran across the experiment from corner to corner. In the second line the diagonal marks, instead, the centre line of a former path, the width of which equals the length of the side of a plot. In the last line it is supposed that both effects have been present in the past and are now contributing equal variances.

The former field boundary has not caused much difficulty, the best control being achieved by N1 with an unremarkable value of b. Method N2 gave much the same level of control but with atypical regression coefficients. The former path, on the other hand, gave rise to a pattern that no method could control. The least ineffective was N2 but with regression coefficients that were even more atypical. The last pattern was more amenable but no method can be recommended as effective.

In practice, a strip of different fertility across the area would be quickly noticed if everywhere else was even. It could be lost in other variation, however. If it is detected, it may be possible to devise a pseudovariate in which each plot is assigned the proportion of its area to lie on the path. An ordinary analysis of covariance can then be calculated, adjusting all data to a value of zero in the pseudovariate.

Patches

Patches can arise in many ways, e.g. from an area of impeded drainage or the site of a shed where noxious substances were used. Large trees that blow over can have lasting effects. Sometimes governments allocate areas for research purposes and everything, including former dwellings, roads and pounds for animals, is cleared all without previous detailed surveying.

Table 4. Results with patches. Each pattern consists of n circular patches with centres randomly located within the experimental area. The patches are of various sizes. $A = \pi s^2$, where s is the length of side of a plot.

		Reduction in standard error (%)				Regressions		
Plot size	n	BL	RC	N1	N2	b	b_1	b_2
$2A$	8	7.7	18.3	57.3	56.9	0.951	0.644	0.434
	16	7.7	18.4	57.5	57.0	0.952	0.638	0.442
$4A$	4	10.5	25.2	62.5	62.3	0.953	0.612	0.442
	8	10.7	25.8	62.5	62.2	0.955	0.604	0.465
	16	10.7	25.9	62.6	62.2	0.955	0.594	0.465
$8A$	4	13.1	32.7	65.6	65.3	0.960	0.600	0.444
	8	13.0	33.4	66.0	65.7	0.963	0.617	0.424
	16	13.5	33.7	65.9	65.5	0.962	0.610	0.432

If patches are small they provide a further source of random variation. If they are large they give rise to discontinuities such as those just considered, but there are those of intermediate size that need special attention. To examine them, fertility patterns were formed from circular patches, each uniform within itself, at random locations within the experimental area. They were given positive and negative signs alternately. The results were troublesome to assess because the algorithm repeatedly failed, sometimes because the patches formed regular patterns and sometimes because two of different sign would nearly extinguish one another. It was difficult, therefore, to achieve an uninterrupted series of 3600 realizations, but there were some successes, which are presented in Table 4.

At first glance the conclusions are clear. The effectiveness of all methods depends chiefly on the size of the patches and not their number. Also, the most effective methods are those using nearest neighbours. The value for b is within the usual range.

In fact, the situation is more complex. The table shows mean results from many realizations, but conclusions are different if minima and maxima are used instead. To take the case of eight patches of size 4A as an example, the extreme values for percentage reduction of standard errors are:

	BL	RC	N1	N2
Minimum	−7.9	−15.1	−0.6	8.0
Maximum	52.7	67.5	86.6	86.6

The range of values is such that any method may have reasonable success, and any method may fail.

These results arose from patches that were unforeseen. If, however, they could be identified and their boundaries noted, it might be possible to generate a pseudovariate and adjust in the way suggested for discontinuities.

Random variation

Results so far have ignored random variation, but it is unavoidable. It derives

Table 5. The effects of random error. As in Table 2, the first line is a copy of that for θ = 45° in Table 1. The others show the effect of an increasing proportion, p, of variance arising from random error.

	Reduction in standard error (%)				Regressions		
p	BL	RC	N1	N2	b	b_1	b_2
0.00	24.9	99.1	99.2	99.4	1.000	1.002	−0.002
0.05	23.5	77.6	73.4	75.3	0.972	−0.129	1.189
0.10	22.1	68.3	62.7	66.0	0.942	−0.186	1.243
0.20	19.5	55.2	46.6	52.3	0.865	−0.243	1.287
0.40	14.2	36.2	24.5	32.8	0.661	−0.245	1.241
0.60	8.9	22.0	11.5	18.1	0.437	−0.216	1.109
0.80	4.5	10.4	4.6	7.2	0.232	−0.119	0.714
1.00	0.1	0.3	1.5	1.0	0.053	0.114	−0.366

from several sources, one of which is technical error. No measurements are perfect, least of all those that derive from sampling or eye estimates. Crop weights depend upon correct delineation of the area to be harvested and records like height of plants require judgement because height may vary over a plot. The application of treatments also needs care. Concentrations of chemicals should be exact and fertilizers applied strictly to the designated area.

Another source of random error derives from failure of the design. With BL, for example, there is an assumption of uniform fertility within each block – its almost certain failure is allowed for by randomization. Where blocks have been chosen to allow for perceived differences, these could well interact with the treatments. Randomization again adds the variance to random error.

Another cause of random variation arises when plants have been brought over from the nursery without careful grading.

In Table 5, p is the proportion of the variance due to random causes. The rest, as in Table 4, derives from Table 1 with θ = 45°. It emerges that a value as low as 0.05 has an appreciable effect. It is marked with BL, more so with RC and even more so with N1 and N2, because the regression coefficients are altered. The large reductions in standard error found in Table 1 do not often occur in practice and random components of error could well provide the explanation.

There is a further point. The table shows mean values over all realizations, but minima tell a different story. If p equals 0.10 the lowest values found for the reduction in standard error were: BL, 6.6; RC, 59.6; N1, 22.0; and N2, 52.2.

That is to say, although N1 was successful in most instances it was liable to fail. Methods RC and N2 were more consistent.

Blocks and nearest-neighbour methods used in conjunction

So far, randomization has taken place over the whole area, but there are advantages in using blocks quite apart from their usefulness in controlling local variation. They help in the conduct of the experiment by providing boundaries where work may stop if it has to be carried out over several days or divided

Table 6. The effect of introducing blocks into some of the patterns considered in previous tables.

	Reduction in standard error (%)					Regressions		
Plot size	BL	BL + N1	BL = N2	N1	N2	b	b_1	b_2
A	100.0	100.0	100.0	99.5	99.6	−0.018	−0.018	−0.000
B	24.4	99.7	99.8	99.5	99.6	1.000	1.002	−0.002
C	−6.9	99.7	99.8	99.6	99.7	1.000	1.002	−0.002
D	20.7	98.2	98.4	98.3	98.4	1.005	1.145	−0.152
E	−6.8	−7.8	29.1	−0.6	33.3	0.083	−1.028	2.360
F	21.7	53.4	64.0	55.1	64.3	0.800	−0.437	1.490
X	6.2	44.2	44.4	53.5	54.8	0.898	0.640	0.436
Y	9.5	69.0	69.0	59.0	60.4	1.011	0.845	0.245
Z	13.8	82.5	82.5	62.6	63.9	1.015	0.871	0.187

The lines refer to patterns already examined, as follows:
A Table 1, $\theta = 0°$; B Table 1, $\theta = 45°$; C Table 1, $\theta = 90°$;
D Table 2, $p = 0.1$; E Table 3, path; F Table 5, $p = 0.1$;
X Table 4, 2*A*,8; Y Table 4, 4*A*,8 Z Table 4, 8*A*,8
After the letter there are three columns that show reductions instandard errors when the design is in randomized complete blocks.
The next two columns show corresponding reductions without blocks.
The last three columns give the regression coefficients with blocks.

between teams. Any differences introduced will be associated with blocks and eliminated in the analysis of data. Table 6 sets out some results when columns were used as blocks, both with and without nearest-neighbour adjustments. All patterns have been taken from those already studied.

This calls for certain changes in method. For each pattern, two sets of 3600 realizations were needed: one in which randomization took place over the whole area, as in the rest of the paper; and another in which the set of treatments was randomized within each block separately, as would usually be expected in a block experiment. As a consequence, there now have to be eight replicates of eight treatments, rather than the four replicates of 16 as in the rest of the paper.

Table 6 shows what happens with some of the patterns already considered when blocks are introduced. Line A deals with an idealized case. The blocks correspond exactly to the pattern and are therefore completely effective, leaving only rounding errors. The addition of N1 and N2 can do no more and leads to meaningless regression coefficients. Line B, on the other hand, is an example of the blocks being only partly successful, but N1 and N2 have made up for its deficiencies. Line D is similar and Line C makes the point more emphatically.

In Line E the addition of N1 made little difference. Method N2 was more successful alone than in combination, but with extraordinary regression coefficients that would preclude its use with other sources of variation.

Lines X, Y and Z give special attention to the problem of patches. If an experiment is designed in blocks and fails, the addition of a nearest-neighbour adjustment might well improve matters. In Lines Y and Z, N1 and N2 do better when used in conjunction with blocks, but that was not so in Line X.

CONCLUSIONS AND RECOMMENDATIONS

Occasions for local control

The designer of an experiment is not obliged to allow for variation within the area and sometimes it may be better not to try. In a small area, there may be little variation to control and only a few degrees of freedom for error, none of which can be spared. For example, four replicates of three treatments leaves nine degrees of freedom once the effect of treatments is removed, and that is minimal. To squander three of them by introducing four blocks could be quite wrong. If some control is needed, it might be good enough to use two blocks, each with two replicates randomized within it. That leaves eight degrees of freedom for error, which is much better than six.

Identification of important sources of error

Where there is need to control local variation, the main concern must be with the larger effects. For example, if there were two independent sources of variation that separately contributed standard errors of 5 and 2, their combined effect is:

$$5.4 = \sqrt{(5^2 + 2^2)}.$$

What those major effects are is a matter for agronomic judgement, but some, such as altitude, are almost always important. Plots at the bottom of a slope usually have deeper topsoil, more moisture and less exposure to wind. Another source that can rarely be ignored is proximity to water, whether a stream or pool.

Priorities

It is clear from the results presented above that a method of local control that generally is effective will sometimes fail badly, while another that in general gives less good results, is more reliable. An outstanding example is use of row-and-column designs, where an unsuspected interaction can lead to disappointment. Also, on patchy land, N1 gives a good average performance, but it can fail for no reason that can be foreseen.

It is necessary, therefore, to consider how serious it would be if the experiment failed to give a clear result. In a series of out-station experiments to discover the area over which a recommendation can be made safely, one that gives inconclusive results would be unfortunate without necessarily being disastrous. If, on the other hand, it is a preliminary experiment to guide future research, it is important that the conclusions should yield a definite result.

The formation of blocks

It is best that local agronomists decide the blocks, which do not all have to be of the same shape, nor do they have to be contiguous. As far as possible, though, each should be uniform. It helps if each contains as many plots as there are treatments, then each can receive a complete replicate. If this can be achieved, an orthogonal design is possible – a situation with many advantages, among which is full efficiency of estimation for all contrasts. Sometimes, though, the nature of the site makes this impossible. For example, the area allocated may be a narrow strip down a slope and each block should, if possible, lie on a contour. If there are six treatments and the greatest number of plots that can reasonably be formed in a block is four, then non-orthogonality is required even though it implies a loss of efficiency of estimation of some of the treatment contrasts. If four replicates are required a statistician could suggest several possibilities with six blocks, each of four plots. There will have to be loss of information somewhere and the choice between possibilities is for the experimenter. If blocks take out the chief differences in the area, little will be gained by recovering inter-block information.

Use of rows and columns

Row-and-column designs are used less now than formerly, possibly because they have been found to be treacherous. They use so many degrees of freedom that the consequences of failure are serious.

Also, they can be inflexible. The site must be rectangular with no gaps apart from alleys between rows or between columns. On the other hand, they can deal with trends in all directions in a way that is not possible with blocks. Perhaps their chief use is not in the field but under glass, where the rows can allow for distance of plots from the glass and their positions relative to heating pipes. Columns would allow for position along the bench.

As with block designs, there are many non-orthogonal possibilities. If the treatments are disposed non-orthogonally with respect to both rows and columns, there will be two sources of inefficiency to be taken into account.

Nearest-neighbours methods

Two methods sometimes regarded as being of the nearest-neighbour class (Cullis and Gleeson, 1991; Gleeson and Cullis, 1994), are, for present purposes, better regarded as spatial and have already been considered. Otherwise there is not much experience with nearest-neighbour methods. At their best they can be very effective, though, as Ainsley *et al.* (1992) have pointed out, any interference between plots can lead to disturbing effects. For reasons already given, it is important that the regression coefficients should be the same for all kinds of fertility pattern. For method N1, b lies mostly in the range 0.95–1.00, which means that many sources of error can be controlled together, though not some discontinuities. Random error is less easily dealt with. Method N2 had some notable successes but unfortunately they were achieved by the use of atypical regression coefficients, so its use is very limited.

There has been a tendency to regard nearest-neighbour methods as a useful salvage tool if blocks have failed, and they can certainly be useful in that way. It is bad practice, however, to analyse the same body of data twice and to publish the more pleasing set of results. It might be sound to add N1 to BL as a matter of course whenever the blocks make up a rectangular array of plots.

Further, if blocks are needed primarily for purposes of administration, they can be formed with simple boundaries for the division of work, leaving a nearest-neighbour method to complete the control of variation.

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COMPUTER PROGRAM

A program, named *blnn*, has been written in Fortran 77 for experimenters to assess the value of method N1 in their own conditions. It can be used with existing data from experiments with a rectangular array of plots and equally replicated treatments, using any orthogonal block design. It can be downloaded from: http://www.ukc.ac.uk/ims/software/blnn/

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