# Experimental Designs to Evaluate Crop Response on Adjacent Soil Mapping Units

L. A. Nelson* and S. W. Buol

## ABSTRACT

**Soil mapping units are scaled representatives of naturally occurring soil properties and serve as reference units for technology transfer. To accurately evaluate the interactions between a given management practice and a range of soil properties represented by a soil mapping unit, tailoring of the experimental design is required. This paper identifies the design principles involved in setting up field experiments to compare the responses to different management treatments of crops grown on areas within different soil mapping units, and suggests experimental designs for this purpose. Representation of the entire soil mapping unit in the area studied is emphasized. Repetition of experiments across locations, years, or both is recommended if the inferences are to apply to more than one environment and if a test of the main effect of the area represented by the soil mapping unit is desired. Procedures for designing a series of experiments and analyzing the resulting data are discussed.**

SOIL SURVEYS in the USA are conducted by the National Cooperative Soil Survey program. Leadership for this program is provided by the Soil Conservation Service and the results are published as county or soil-survey-area reports. Each soil-survey report contains a soil map that has areas delineated and identified as soil mapping units. Each soil mapping unit encompasses a range of soil properties, and identification includes the taxonomic name(s) of the predominant soil(s) in that unit. This taxonomic identification is usually a soil series name.

A great deal of confusion can result when the same name is used for the soil mapping unit and the soil series (Cline, 1977). This confusion should be avoided by the proper use of terms. A soil series is a concept that is defined by a rigid range of soil pedon properties. The soil mapping unit, however, being a reduced-scale map representation of nature, does not conform to the same range of soil properties. A soil mapping unit includes pedons with the properties of other series identified in the map-unit description but not in the map-unit name. Soil mapping units often are named as types and/or phases of a series; thus, their properties are more specific than the entire range of properties that define the taxonomic soil series. The soil series exists only as a definition. No experimental site can possibly contain the entire range of soil properties defined by a series name. Therefore, no field experiment can be conducted on a soil series. Field experimentation can only be conducted on representative occurrences of soils, with some naturally occurring spatial variability. In the absence of detailed characterization of the soils within the experimental site that could establish the actual range of soil properties present, the mapping-unit name is the most informative and practical identification of the soil properties at an experimental site.

### *Soil Mapping Units*

Soil mapping units depict segments of the natural continuum of soil properties in the landscape and seldom conform to field or property-ownership boundaries. For this reason, it is important to know if they should be managed in a similar manner or if separate management practices need to be used on each soil mapping unit (or certain groups of them) occurring in the same field. Management practices, in a broad sense, might include cultural practices, crop varieties, soil fertility amendments, and pest-control practices.

The task, then, becomes to design experiments within a field (or fields) to compare the different soil mapping units with respect to crop management response. The feature of this experimental situation that presents challenges for the experimental design specialist is the systematic occurrence of soil mapping units, stripped across the field. This stripping precludes true replication of the soil-mapping-unit factor, which is a requirement for the estimation of a valid experimental error for the soil-mapping-unit effect. It is necessary to evaluate the crop response to management over the entire range of soil properties of each mapping unit within a field so that the results may have generality. The need for generality also implies carrying out the experiment in more than one location (or year) because one location may not contain the complete range of properties of the soil mapping unit or the climatic conditions within which it occurs. Also, the location may have an interaction with the environment over time, and it is useful to assess this effect when studying treatment-response patterns.

### *Soil Variability*

Variability within soil mapping units is assumed but poses no particular problem: it occurs in many experimental situations, and is also encountered by the farmer operating on the experimental unit. We are benefitted by the stabilizing effect of averaging—plots have a range of soil properties and the yield and certain other responses are averaged over a number of plants per plot.

Scientific validity requires that randomization, replication, and local control be incorporated into any design adapted to this particular application. Also, we wish to comply with the general experimental design principle of choosing the simplest design that will do the job, which often results in a convenient layout for field operations.

## EXPERIMENTAL DESIGNS

We will describe three designs, all of which involve a factorial arrangement of treatment × soil mapping unit. In the analysis of data from these designs, our

---

L.A. Nelson, Dep. of Statistics, North Carolina State Univ., Raleigh, NC 27695-8203; S.W. Buol, Dep. of Soil Science, North Carolina State Univ., Raleigh, NC 27695-7619. Paper no. 12061 of the Journal Series of the North Carolina Agric. Res. Serv., Raleigh, NC 27695-7643. Received 13 Mar. 1989. *Corresponding author.

Published in Soil Sci. Soc. Am. J. 54:841-849 (1990).

interest lies in estimating the main effects of soil mapping unit and treatment, the interaction between these factors, and the experimental error(s). Our emphasis should be on estimating relationships rather than on simply performing hypothesis tests. Hypothesis tests provide a guide about how estimates should be made.

Each of the three designs described is a variant of a split-plot design. One unique feature of split-plot designs is that, in the analysis of variance, there is more than one experimental error term, i.e., one for each plot size. For an ordinary split-plot design, there are two errors—error(a) for the main plot factor and error(b) for the subplot factor. For a design that has the plots for one factor stripped horizontally across the vertical strips for another factor, there are three error terms because there are three plot sizes—a vertical-strip plot, a horizontal-strip plot, and an intersection plot for the interaction between the two factors.

The three designs described here may be used for a variety of different soils investigations. Treatments could consist of levels of fertility, management, or tillage factors. One of the designs that involves treatment strips across the field lends itself to treatments that require large equipment and consequently long, large plots (e.g., tillage treatments). This design might be used in erosion/productivity research for comparing crop response to treatments on areas of the field having different erosion classes. In this case, one would apply to the erosion classes the principles developed here for the soil mapping units.

## A General-Purpose Design

First, we will describe a general-purpose design that is appropriate for most field situations in which treatments are to be applied to more than one soil mapping unit. It is especially useful in cases where the soil mapping units have a patchy distribution. A series of randomized-block experiments are laid out within each of the soil mapping units in the field. Experimental design, number of replications, plot size, and all other conditions are controlled as much as is feasible from one soil mapping unit to the next so that a combined analysis of variance will be possible. Each soil mapping unit will have one or more separate randomized-block experiments located in such a way that the experiments and the replications represent the different soil-property conditions within the mapping unit. A field diagram of the layout of an experiment according to Design 1 is shown in Fig. 1.

We are assuming here that two separate experiments have been run within each soil mapping unit, one to sample the edge of the unit and one to sample the middle. There are precision and mechanical-convenience advantages to grouping the replications into two separate experiments rather than having six scattered replications within the mapping unit. The latter layout, however, might be more representative of the entire soil mapping unit. The combined analysis of variance for this example is given in Table 1. For the testing patterns shown in this table and in Tables 2 and 3 it is assumed that soil mapping unit (SMU) and treatment (T) are fixed effects but that experiments (Expt.) and replications (Rep.) are random effects.

The expected values of the mean squares shown in Table 1 for this set of model assumptions provide a guide as to how the tests of significance should be carried out. The test for the main effect of SMU is not valid in the combined analysis over the mapping units because the soil mapping units per se have not been randomly allocated and replicated, but the T effect and T × SMU effect tests are valid and are tested with (Expt. in SMU) × T. Treatments may be compared within SMU, but the comparison of SMU within T is not valid.

In the analysis of variance, (Expt. in SMU) × T may be pooled with error to form an error having 45 degrees of freedom if there is no evidence of an (Expt. in SMU) × T interaction. A preliminary test of the type suggested by Bancroft (1964) for the significance of (Expt. in SMU) × T may be used to decide if pooling is justified. A test of (Expt. in SMU) × T is made at the 0.25 significance level and, if (Expt. in SMU)

Table 1. Analysis of variance for data from a location in which treatments have been assigned to plots according to Design 1 (experiment and replicate are random effects; soil mapping unit and treatment are fixed effects).

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Soil mapping unit (SMU) | 2 | SMU | $\sigma^2 + t\sigma^2_{R(E(S))} + rt\sigma^2_{E(S)} + ert\theta^2_S$ |
| Experiment (E) in SMU | 3 | E(SMU) | $\sigma^2 + t\sigma^2_{R(E(S))} + rt\sigma^2_{E(S)}$ |
| Replications (R) in E in SMU | 12 | R[E(SMU)] | $\sigma^2 + t\sigma^2_{R(E(S))}$ |
| Treatment (T) | 3 | T | $\sigma^2 + r\sigma^2_{ET(S)} + ers\theta^2_T$ |
| T × SMU | 6 | T × SMU | $\sigma^2 + r\sigma^2_{ET(S)} + er\theta^2_{TS}$ |
| (E in SMU) × T | 9 | E × T(SMU) | $\sigma^2 + r\sigma^2_{ET(S)}$ |
| Residual | 36 | RESIDUAL | $\sigma^2$ |
| Total (corrected for mean) | 71 | | |
| Levels of: | | | |
| Soil mapping unit | $s = 3$ | | |
| Experiment | $e = 2$ | | |
| Replication | $r = 3$ | | |
| Treatment | $t = 4$ | | |

† MS = mean square. Testing pattern for the above assumptions:
F (T) = (MS for T)/[MS for E × T (SMU)]
F (T × SMU) = (MS for T × SMU)/[(MS for E × T(SMU)]

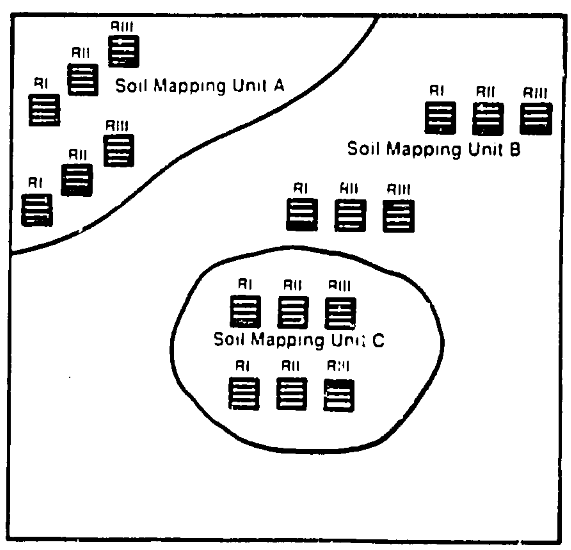


Fig. 1. Field diagram showing layout of an experiment according to Design 1. There are three soil mapping units, two experiments per soil mapping unit, three replications per experiment, and four treatments.

× T is significant, one would not pool. Otherwise, pooling would be justified.

For another set of model assumptions regarding fixed and random effects, one would write out the expected values of the mean squares under that assumed model and revise the testing pattern accordingly. Schultz (1955) and Steel and Torrie (1980) offer practical guidelines for writing out expected values of mean squares.

## Designs That Involve Strips Across Soil Mapping Units

At sites where there are complicated patterns of soil variation, Design 1 should be used. In cases where the soil-mapping-unit variation pattern is less complicated, there are two possible split-plot configurations that could be used. Mechanically, it is often convenient to lay out strips across the soil mapping units

Table 2. Analysis of variance of data for Design 2 assuming complete balance (strip is a random effect; soil mapping unit and treatment are fixed effects)

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Strip (R) | 5 | R | $\sigma^2 + t\sigma^2_{RS} + st\sigma^2_R$ |
| Soil Mapping Unit (SMU) | 2 | SMU | $\sigma^2 + t\sigma^2_{RS} + rt\theta^2_S$ |
| Error(a) | 10 | R × SMU | $\sigma^2 + t\sigma^2_{RS}$ |
| Treatment (T) | 3 | T | $\sigma^2 + rs\theta^2_T$ |
| T × SMU | 6 | T × SMU | $\sigma^2 + r\theta^2_{TS}$ |
| Error(b) | 45 | T × R × SMU | $\sigma^2$ |
| Total (corrected for mean) | 71 | | |
| Levels of: | | | |
| Strips | $r = 6$ | | |
| Soil mapping unit | $s = 3$ | | |
| Treatment | $t = 4$ | | |

† MS = mean square. Testing pattern for the above assumptions:
F (T) = (MS for T)/(MS for T × R × SMU)
F (T × SMU) = (MS for T × SMU)/(MS for T × R × SMU)

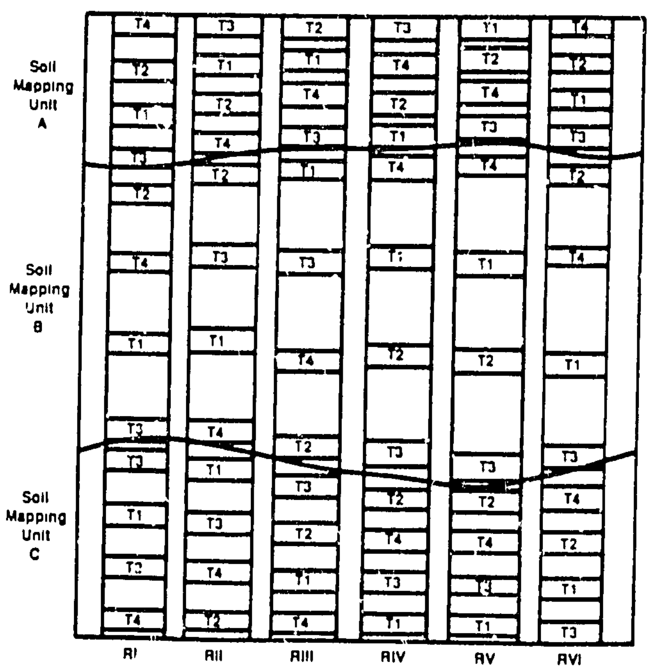


Fig. 2. Field diagram showing layout of Design 2. There are six strips (replications), laid out across three soil mapping units, and four treatments.

either for replications or for treatments in experiments of this type. The following discussion will center on the ways in which the strips should be laid out and how the treatments should be applied within (or to) them.

Design 2 is a stripped split-plot design in which the strips are the replications and the whole-plot factor is the soil mapping unit, levels of which are systematically stripped across the replications. Essentially, this is the same design as that described by Cochran and Cox (1957, p. 305–306), which they called a split-plot design with systematic arrangement of the treatments applied to the whole units. Plots are marked off within a soil mapping unit within each strip and subplot treatments are randomly assigned. A diagram of this arrangement is shown in Fig. 2.

In order to represent the complete range of each soil mapping unit within the location being studied, it is important that the strips cover the range of characteristics that occur within each of the soil mapping units at that location. This is not always the strategy that will result in the best precision on the two main effects and the interaction, but the need for full representation of the range within each of the soil mapping units offsets the need for a high degree of precision. In cases where the precision is limiting, it may be improved by increasing the number of replications, by improving experimental technique, or by using the optimal plot size or shape. The strips may be placed systematically across the field. The systematic placement will be easy to implement and will also provide a representative sample of the soil mapping units involved.

One variant of Design 2 is to provide eight strips and then randomize treatments according to two latin squares per soil mapping unit, giving a total of six latin squares. This would give more precision but would be slightly more complicated and result in less flexibility.

The analysis of variance of data for Design 2, assuming complete balance of six strips, three soil mapping units, and four treatments, is shown in Table 2.

Some highlights of the analysis (assuming that the treatments are randomly allocated to the subplots) are as follows:

1. The whole-plot analysis is made invalid by the systematic arrangement of the whole-plot factor (SMU) within the replications (strips.)
2. The tests of significance of T and the interaction between T and SMU are valid and are made using error(b).
3. The systematic arrangement also affects the interaction comparisons, making the comparisons of two SMU at the same level of T invalid. However, the comparisons of two T within the same SMU (as discussed in no. 4 below) are valid.
4. If interaction is significant, we will want to compare treatments within each soil mapping unit. If the treatments are quantitative levels of the same factor, perhaps fitting a separate curve for each soil mapping unit using polynomial regression will be in order. An example of a set of such curves showing response to fertilizer rates for three soil mapping units at a particular experi-

mental location is shown in Fig. 3. Soil mapping unit A has a completely different response pattern, so one may wish to handle it separately from a management point of view. If the T are qualitative, the appropriate comparisons among T within each SMU may be made using contrasts.

In Design 3, the treatments are placed in sets of parallel contiguous strips across the soil mapping unit (with as many strips in a set as there are treatments). The treatments are randomly assigned to strips within a set, which is a randomized complete block. For a

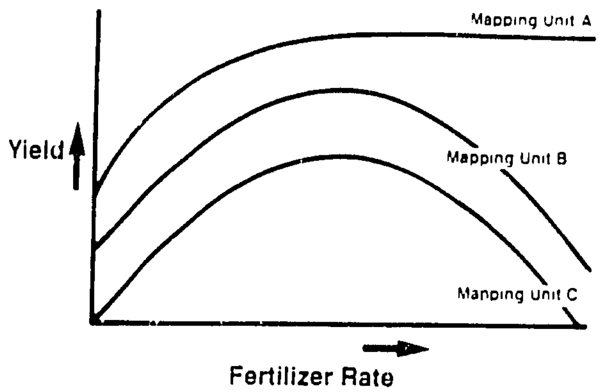


Fig. 3. An example of a set of curves showing yield response to fertilizer for three soil mapping units within a field.

given treatment strip, there is a possibility of treating the entire strip and then harvesting only the subplot portions of the strip for response data. Or, if the soil mapping units are wider, one may wish to treat only the parts of the strip that correspond with the subplots and then measure response data from these treated areas. Both the soil boundary area and the central part of the mapping unit should be represented approximately in proportion to their relative areas when locating the subplots within a soil mapping unit within a strip.

The name for this design is a split-block design with the treatments and soils being stripped across each other. See Federer (1955) for a description of the split-block design. A diagram of this arrangement is shown in Fig. 4. The analysis of variance of data for Design 3, assuming complete balance, six sets (replications), three soils, and four treatments, is shown in Table 3.

This design is easy to carry out mechanically and would lend itself to situations where long, large plots are necessary (e.g., tillage experiments).

Some important considerations concerning the analysis (assuming that treatments are randomly allocated to strips within a set) are as follows:

1. Because the soil mapping units are stripped across the sets (replications), there is not a valid test of the main effect of SMU.
2. Each main effect requires its own error term in the analysis of variance, and the interaction of

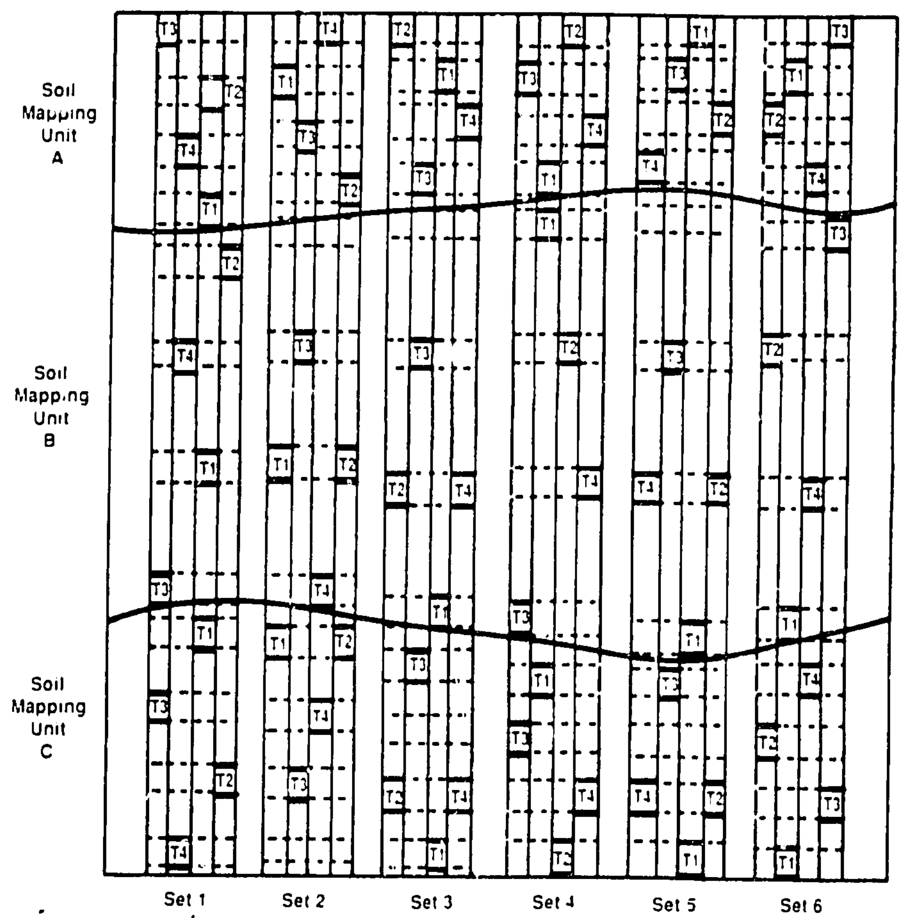


Fig. 4. Field diagram showing layout of Design 3. There are six sets (replications), laid out across three soil mapping units, and four treatments stripped lengthwise across soil mapping units within each replication.

the two factors requires a third error term.
3. There is more precision on the interaction of T and SMU than on either of the main effects.
4. The precision on T would be much less in this configuration than in Design 2.
5. The interaction simple-effect comparisons are restricted to comparisons among T within SMU. It is not possible to compare SMU within T.

### Layout Specifications

Each field will present a unique set of conditions to which experimental layout must be adapted. It is assumed for this discussion that soil-property-distribution data are not available for the experimental site. The following are some general principles that will serve as guides in the layout of experiments using the designs described here.

*Number of Replications*

An estimate is needed of the coefficients of variation calculated from each of the error terms in the analysis of variance. These may be estimated from actual analyses of data from previous experiments in similar areas or from "educated guesses" where data are not available. Tables 2.1 on p. 20 and 21 of Cochran and Cox (1957) can be used to estimate the number of replications required for the stated precision of each main effect and interaction, once the desired size of detectable differences and, in the case of significance tests, the $\alpha$ level are determined. If interaction is not expected to be large and important, the number of replications required for a given amount of precision for main-effect mean estimates is smaller than that required for an equivalent amount of precision for interaction mean estimates due to the hidden replication arising from the levels of the other factor(s). Another consideration in determining the number of replications is the standard rule in experimental design that each estimated experimental-error term should have a minimum of 10 degrees of freedom.

As many replications of any of these designs should be run as is feasible with the amount of time and effort available. They are not likely to be highly precise experiments.

*Location of Experiments in Design 1*

It is not possible to be very specific in this discussion about where to locate the experiments in Design 1. The stripped designs lend themselves to more objectivity, e.g., in location of strips and experimental units within strips. If a limited number of experimental sites are available per mapping unit, it is preferable to use judgement as to their location rather than to develop an elaborate scheme to select the experimental sites completely at random. Judgement selection implies choosing areas that best represent the bulk of the soil mapping unit. If considerable variation exists within a mapping unit, it is advisable to place individual experiments on these separate areas. The edge of the mapping unit as well as the middle area should be represented. Mechanical convenience should be taken into consideration when organizing and orienting the experiments or replications within the field. Precision of an individual experiment should be maximized. This implies that replications should be homogeneous within but they may differ from one another.

**Table 3.** Analysis of variance of data for Design 3 assuming complete balance (set is a random effect; treatment and soil mapping unit are fixed effects).

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Set (R) | 5 | R | $\sigma^2_{RST} + s\sigma^2_{RT} + st\sigma^2_{R}$ |
| Treatment (T) | 3 | T | $\sigma^2_{RST} + s\sigma^2_{RT} + rs\theta^2_{T}$ |
| Error(a) | 15 | R × T | $\sigma^2_{RST} + s\sigma^2_{RT}$ |
| Soil mapping unit (SMU) | 2 | SMU | $\sigma^2_{RST} + t\sigma^2_{RS} + rt\theta^2_{S}$ |
| Error(b) | 10 | R × SMU | $\sigma^2_{RST} + t\sigma^2_{RS}$ |
| T × SMU | 6 | T × SMU | $\sigma^2_{RST} + r\theta^2_{TS}$ |
| Error(c) | 30 | R × T × SMU | $\sigma^2_{RST}$ |
| Total (corrected for mean) | 71 | | |
| Levels of: | | | |
| Replication | $r = 6$ | | |
| Treatment | $t = 4$ | | |
| Soil Mapping Unit | $s = 3$ | | |

† MS = mean square. Testing pattern for above assumptions:
F (T) = (MS for T)/(MS for R × T)
F (T × SMU) = (MS for T × SMU)/(MS for R × T × SMU)

*Location of Strips in Stripped Experiments*

After the number of strips has been determined according to the number of replications (Design 2) or the number of treatments and sets (Design 3), the decision of where to place the strips (or sets of strips) should be made. The variation in soil properties within a mapping unit at a particular site is usually not known prior to the experiment. In the absence of such information, it is best to place strips (Design 2) or sets of strips (Design 3) so that their distance from one another is equal. In this way, the entire soil mapping unit is representatively sampled. The distance of the first and last plots from the two side edges of the field should be equal to the space between strips (Design 2) or sets of strips (Design 3). For example, suppose that the width of a field is 167.6 m and eight 4.57-m-wide strips are to be laid out according to Design 2. The combined width of the eight strips is 36.6 m. Therefore 167.6 − 36.6 = 131 m is to be divided among seven interstrip areas and two edge areas. The width of the interstrip areas, then, is 131/9 = 14.56 m. As another example, suppose that the width of a field is 457.2 m and that six sets of four strips are to be laid out according to Design 3. Each plot has a width of 5.49 m. The combined width of all of the strips is then 24 × 5.49 = 131.8 m. Therefore, 457.2 − 131.8 = 325.4 m is to be divided among the five interstrip areas and two edge areas. The width of each of these areas is 325.4/7 = 46.5 m.

*Location of Plots within Strips—Design 2*

After the choice of the number of strips and their location has been made, a diagram of the field should be drawn to scale showing the location of the soil mapping units and the strips crossing them. The length of each strip as it traverses each soil mapping unit is then measured with a ruler. The number of subplots within a strip in a soil mapping unit is multiplied by the length of the subplot and this gives the total length of the strip within that soil mapping unit that will be occupied by the treatment subplots. Enough positions are marked to accommodate the treatment subplots with equal distances between them. The two end positions should be at the two extremities of the soil mapping unit within the strip in order to assure that the probability of an edge being sampled will be in proportion to the relative areas of edge and central portions of the mapping unit. Treatments are then randomly assigned to the subplot positions using a random-number table or a random-number generator on a computer.

As an example, suppose that we are setting up an experiment according to Design 2. The length of the field is 457.1 m. A length of 243.8 m of the first strip on the left occurs in SMU A, 121.9 m occurs in SMU B, and 91.4 m occurs in SMU C. There are six T subplots, each of which will be randomly assigned to one of six subplots, each 9.14 m long. The combined length of subplots in each SMU in the strip is 9.14 × 6 = 54.8 m. The intersubplot distance in SMU A = (243.8 − 54.8)/5 = 189.0/5 = 37.8 m. The intersubplot distance in SMU B = (121.9 − 54.8)/5 = 13.4 m. The intersubplot distance in SMU C = (91.4 − 54.8)/5 = 7.3 m. A separate randomization is then applied to each mapping unit of the strip corresponding to SMU A, B, and C.

The above process will need to be applied to each strip individually, because the relative lengths of mapping-unit segments will vary by strip. When all of the interplot segments have been calculated and the randomization carried out, the field diagram may be finalized to show location of plots and the randomization scheme. When the experiment is laid out in the field, the distances along each strip can be paced to locate the plot boundaries.

It is important to use the procedures described above to formally lay out the plots on the strips and assign the treatments to the subplots completely at random with an objective randomization process in order that no biases occur in the estimation of SMU, T, or interaction effects.

*Location of Plots within Strips—Design 3*

After location of the sets of strips for Design 3, a diagram of the field should be drawn to scale showing the location of the soil mapping units and the sets of strips crossing them. The randomization of treatments to strips within each set is then carried out using a random-number table or random-number generator. In most cases, the treatment will be applied to the entire strip, although only a portion of the strip will be harvested for experimental purposes. The length of each strip as it traverses each soil mapping unit is then measured with a ruler. Four potential positions will be marked off within each soil mapping unit. The two end positions will be at the two extremities of the soil mapping unit within the strip. All distances between positions will be equal. For each soil mapping unit within each strip, the position at which the plot response for that treatment will be taken is then randomly chosen.

For example, suppose that the length of the strip for T 1 on SMU A in the first set of strips is 121.9 m. The plots are 9.14 m in length. Thus, 4 × 9.14 = 36.6 m will be reserved for possible experimental purposes. The difference, 121.9 − 36.6 m will be divided equally among three interposition increments, i.e., (121.9 − 36.6)/3 = 85.3/3 = 28.4 m. The plot positions are drawn to scale within the strip on the diagram and one of the four positions is then selected at random for measurement of the plot response. This process will be repeated for each treatment in each set on each soil mapping unit. Again, one would want to finalize the field diagram to show the location of sets of strips, the randomization scheme, and the location of plots. When the experiment is laid out in the field, the dis-

**Table 4. Combined analysis of variance for an experiment conducted at four locations according to Design 1 with the levels of factors and model assumptions as shown in Table 1 at each location. Location is a random effect.**

| Source | df | MS† | Expected value of MS |
|---|---|---|---|
| Location (L) | 3 | L | $\sigma^2 + t\sigma^2_{R(E(LS))} + rt\sigma^2_{E(LS)} + erst\sigma^2_L$ |
| Soil Mapping Unit (SMU) | 2 | SMU | $\sigma^2 + t\sigma^2_{R(E(LS))} + rt\sigma^2_{E(LS)} + ert\sigma^2_{LS} + elrt\theta^2_S$ |
| L × SMU | 6 | L × SMU | $\sigma^2 + t\sigma^2_{R(E(LS))} + rt\sigma^2_{E(LS)} + ert\sigma^2_{LS}$ |
| Experiment (E) in L × SMU | 12 | E(L × SMU) | $\sigma^2 + t\sigma^2_{R(E(LS))} + rt\sigma^2_{E(LS)}$ |
| Replications (R) in E in L × SMU | 48 | R[E(L × SMU)] | $\sigma^2 + t\sigma^2_{R(E(LS))}$ |
| Treatment (T) | 3 | T | $\sigma^2 + r\sigma^2_{TE(LS)} + ers\sigma^2_{TL} + erls\theta^2_T$ |
| T × L | 9 | T × L | $\sigma^2 + r\sigma^2_{TE(LS)} + ers\sigma^2_{TL}$ |
| T × SMU | 6 | T × SMU | $\sigma^2 + r\sigma^2_{TE(LS)} + er\sigma^2_{TLS} + elr\theta^2_{TS}$ |
| T × L × SMU | 18 | T × L × SMU | $\sigma^2 + r\sigma^2_{TE(LS)} + er\sigma^2_{TLS}$ |
| T × E in L × SMU | 36 | T × E(L × SMU) | $\sigma^2 + r\sigma^2_{TE(LS)}$ |
| Residual | 144 | RESIDUAL | $\sigma^2$ |
| Total (corrected for mean) | 287 | | |
| Levels of: | | | |
| Location | $l = 4$ | | |
| Soil mapping unit | $s = 3$ | | |
| Experiment | $e = 2$ | | |
| Replication | $r = 3$ | | |
| Treatment | $t = 4$ | | |

† MS = mean square. Testing pattern for above assumptions:
F (SMU) = (MS for SMU)/(MS for L × SMU)
F (L × SMU) = (MS for L × SMU)/[MS for E(L × SMU)]
F (T) = (MS for T)/(MS for T × L)
F (T × L) = (MS for T × L)/[MS for T × E(L × SMU)]
F (T × SMU) = (MS for T × SMU)/(MS for T × L × SMU)
F (T × L × SMU) = (MS for T × L × SMU)/[MS for T × E(L × SMU)]
F [T × E(L × SMU)] = [MS for T × E(L × SMU)]/(MS for RESIDUAL)

6

tances along each strip can be paced to locate the plot boundaries.

### Multilocation and Multiyear Aspects

It is possible to adequately sample the range in soil characteristics within a location. But a location with one or two delineations may not have the full range of soil characteristics that exists within the soil mapping unit throughout the county or soil-survey area. If generalizations about the treatments are to be made over an area larger than a single location, the experiment should be conducted at more than one location or delineation of the same soil mapping unit. A side benefit to this is that it provides a valid test of the SMU main effect. The design and all cultural factors should be standardized for the entire series of locations, although any one of the three designs discussed here could be used.

Sample locations should be selected to represent the range in properties that occurs within the mapping units so results can be extrapolated to a larger area. If resources permit the sampling of only a limited number of locations, judgement selection of locations should be practiced. If resources permit a larger number of locations, randomized selection of locations would be appropriate.

In most cases, location may be considered a random rather than a fixed effect. For our purposes, the assumptions made about the other effects are the same as in the single-location analysis.

An analysis of variance for an experiment conducted at four locations (L) according to Design 1 with the levels of factors shown in Table 1 at each location is shown in Table 4. In addition to the tests done for a single location, the interaction of SMU and T with L is also of interest. Testing patterns that are inferred from the expected values of the mean squares are shown at the bottom of Table 4. The test of SMU is a conservative one—the degrees of freedom for the error mean square for L × SMU are only six. Tests of T and of the various interaction involving T are more powerful.

A combined analysis of variance for an experiment conducted at four locations according to Design 2 with the levels of factors shown in Table 2 at each location is shown in Table 5. Again, the test for SMU lacks power, but those involving T are more powerful.

A combined analysis of variance for an experiment conducted at four locations according to Design 3 with the levels of factors shown in Table 3 at each location is shown in Table 6. For this combined analysis, there are three pooled error terms, which result fro a pooling each of the three errors in Table 3 across locations. These are used for testing the interactions of T, SMU.

Table 5. Combined analysis of variance for an experiment conducted at four locations according to Design 2 with the levels of factors and model assumptions as shown in Table 2 at each location. Location is a random effect.

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Location (L) | 3 | L | $\sigma^2 + t\sigma^2_\delta + st\sigma^2_{R(L)} + rst\sigma^2_L$ |
| Replications (R) in L | 20 | R(L) | $\sigma^2 + t\sigma^2_\delta + st\sigma^2_{R(L)}$ |
| Soil Mapping Unit (SMU) | 2 | SMU | $\sigma^2 + t\sigma^2_\delta + rt\sigma^2_{LS} + lrt\theta^2_S$ |
| L × SMU | 6 | L × SMU | $\sigma^2 + r\sigma^2_\delta + rt\sigma^2_{LS}$ |
| Error(a) | 40 | R × SMU(L) | $\sigma^2 + t\sigma^2_\delta$ |
| Treatment (T) | 3 | T | $\sigma^2 + rs\sigma^2_{TL} + lrs\theta^2_T$ |
| T × L | 9 | T × L | $\sigma^2 + rst\sigma^2_{TL}$ |
| T × SMU | 6 | T × SMU | $\sigma^2 + r\sigma^2_{TLS} + rl\theta^2_{TS}$ |
| T × L × SMU | 18 | T × L × SMU | $\sigma^2 + r\sigma^2_{TLS}$ |
| Error(b) | 180 | ERRORb | $\sigma^2$ |
| Total (corrected for mean) | 287 | | |
| Levels of: | | | |
| Location | $l = 4$ | | |
| Replication | $r = 6$ | | |
| Soil Mapping Unit | $s = 3$ | | |
| Treatment | $t = 4$ | | |

† Testing pattern for above assumptions:
F (SMU) = (MS for SMU)/(MS for L × SMU)
F (L × SMU) = (MS for L × SMU)/[MS for R × SMU(L)]
F (T) = (MS for T)/(MS for T × L)
F (T × L) = (MS for T × L)/(MS for ERRORb)
F (T × SMU) = (MS for T × SMU)/(MS for T × L × SMU)
F (T × L × SMU) = (MS for T × L × SMU)/(MS for ERRORb)

Table 6. Combined analysis of variance for an experiment conducted at four locations according to Design 3 with the levels of factors and model assumptions as shown in Table 3 at each location. Location is a random effect.

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Location (L) | 3 | L | $\sigma^2_{RST(L)} + s\sigma^2_{RT(L)} + ts\sigma^2_{R(L)} + rst\sigma^2_L$ |
| Replications (R) in L | 20 | R(L) | $\sigma^2_{RST(L)} + ts\sigma^2_{R(L)}$ |
| Treatment (T) | 3 | T | $\sigma^2_{RST(L)} + s\sigma^2_{RT(L)} + rs\sigma^2_{LT} + rls\theta^2_T$ |
| L × T | 9 | L × T | $\sigma^2_{RST(L)} + s\sigma^2_{RT(L)} + rs\sigma^2_{LT}$ |
| R × T(L) | 60 | R × T(L) | $\sigma^2_{RST(L)} + s\sigma^2_{RT(L)}$ |
| Soil Mapping Units (SMU) | 2 | SMU | $\sigma^2_{RST(L)} + t\sigma^2_{RS(L)} + rt\sigma^2_{SL} + rlt\theta^2_S$ |
| SMU × L | 6 | SMU × L | $\sigma^2_{RST(L)} + t\sigma^2_{RS(L)} + rt\sigma^2_{SL}$ |
| SMU × R(L) | 120 | SMU × R(L) | $\sigma^2_{RST(L)} + t\sigma^2_{RS(L)}$ |
| SMU × T | 6 | SMU × T | $\sigma^2_{RST(L)} + r\sigma^2_{SLT} + rl\theta^2_{ST}$ |
| SMU × L × T | 18 | SMU × L × T | $\sigma^2_{RST(L)} + r\sigma^2_{SLT}$ |
| SMU × T × R(L) | 40 | SMU × T × R(L) | $\sigma^2_{RST(L)}$ |
| Total (corrected for mean) | 287 | | |
| Levels of: | | | |
| Location | $l = 4$ | | |
| Replication | $r = 6$ | | |
| Treatment | $t = 4$ | | |
| Soil Mapping Unit | $s = 3$ | | |

† MS = mean square. Testing pattern for above assumptions:
F (T) = (MS for T)/(MS for L × T)
F (L × T) = (MS for L × T)/[MS for R × T(L)]
F (SMU) = (MS for SMU)/(MS for SMU × L)
F (SMU × L) = (MS for SMU × L)/[MS for SMU × R(L)]
F (SMU × T) = (MS for SMU × T)/(MS for SMU × L × T)
F (SMU × L × T) = (MS for SMU × L × T)/[MS for SMU × T × R(L)]

and T × SMU, respectively, with L. In turn, T is tested by the mean square of T × L, SMU is tested by the mean square of SMU × L, and SMU × T is tested by the mean square of SMU × L × T.

Table 7 presents a combined analysis of variance for an experiment conducted at four locations for three years (Y) according to Design 2, with levels of factors and model assumptions for each location-year (L-Y) the same as in Table 2. The unusual feature of this configuration is that, under the assumed model, synthetic error terms must be constructed for the tests for SMU, T, and T × SMU. These are linear combinations of three mean squares (MS). For example, for testing SMU, the error term is obtained by adding the MS of L × SMU to the MS of Y × SMU and then subtracting the MS of L × Y × SMU. An approximation from Satterthwaite (1946) is then used for finding the approximate degrees of freedom of the denominator of the *F*-ratio:

$$df_{denom} = \frac{(MS_1 + MS_2 - MS_3)^2}{(MS_1)^2/df_1 + (MS_2)^2/df_2 + (MS_3)^2/df_3}$$

To illustrate the use of this formula, suppose we wish to find the approximate degrees of freedom for the linear combination of variances that is used to test SMU in the analysis of variance given in Table 7. The degrees of freedom for the three mean squares are 6, 4, and 12, respectively. Substituting these into the above formula, the $df_{denom}$ is obtained as follows:

$$df_{denom} = [MS(L \times SMU) + MS(Y \times SMU) - MS(L \times Y \times SMU)]^2 / \{[MS(L \times SMU)]^2/6 + [MS(Y \times SMU)]^2/4 + [MS(L \times Y \times SMU)]^2/12\}$$

The results, of course, depend on the particular set of mean squares obtained for a specific set of data. Calculation of the denominators in the *F*-ratio for the *F*-tests of T and T × SMU would be done in a similar manner. Again, there is the possibility of pooling certain interaction terms in this combined analysis in a manner similar to that discussed for Design 1. Bancroft's (1964) preliminary test of significance would be useful in deciding whether or not pooling is justified.

In interpretation of any of the analyses of variance (Tables 1-7) it is useful to calculate means for those main effects and interactions that are important. Using a computing package, one would usually calculate means for all main effects and interactions as part of the analysis of variance process, and then use only those that pertain to the particular significance pattern for that data set. For example, if there is no interaction between SMU and T, the T main effect means should be used for making recommendations from the experiment because they are stable and have more generality than the interaction means. Significance of an interaction between these two factors, on the other hand, implies that the interaction means should be plotted, and possibly that soil mapping units should

**Table 7. Combined analysis of variance for an experiment conducted at four locations for 3 yr according to Design 2 with levels of factors and model assumptions for each location-year as shown in Table 2. Location and year are random effects.**

| Source | *df* | MS† | Expected value of MS |
|---|---|---|---|
| Location (L) | 3 | L | $\sigma^2 + t\sigma_a^2 + st\sigma^2_{R(LY)} + rst\sigma^2_{LY} + rsty\sigma^2_L$ |
| Year (Y) | 2 | Y | $\sigma^2 + t\sigma_a^2 + st\sigma^2_{R(LY)} + rst\sigma^2_{LY} + lrst\sigma^2_Y$ |
| L × Y | 6 | L × Y | $\sigma^2 + t\sigma_a^2 + st\sigma^2_{R(LY)} + rst\sigma^2_{LY}$ |
| Replications (R) in L × Y | 60 | R(L × Y) | $\sigma^2 + t\sigma_a^2 + st\sigma^2_{R(LY)}$ |
| Soil Mapping Unit (SMU) | 2 | SMU | $\sigma^2 + t\sigma_a^2 + rt\sigma^2_{LYS} + lrt\sigma^2_{YS} + ryt\sigma^2_{LS} + lrty\theta^2_S$ |
| L × SMU | 6 | L × SMU | $\sigma^2 + t\sigma_a^2 + rt\sigma^2_{LYS} + ryt\sigma^2_{LS}$ |
| Y × SMU | 4 | Y × SMU | $\sigma^2 + t\sigma_a^2 + rt\sigma^2_{LYS} + lrt\sigma^2_{YS}$ |
| L × Y × SMU | 12 | L × Y × SMU | $\sigma^2 + t\sigma_a^2 + rt\sigma^2_{LYS}$ |
| Error(a) | 120 | R × SMU (L × Y) | $\sigma^2 + t\sigma_a^2$ |
| Treatment (T) | 3 | T | $\sigma^2 + rs\sigma^2_{TLY} + rsy\sigma^2_{TL} + rls\sigma^2_{TY} + rlsy\theta^2_T$ |
| T × L | 9 | T × L | $\sigma^2 + rs\sigma^2_{TLY} + rsy\sigma^2_{TL}$ |
| T × Y | 6 | T × Y | $\sigma^2 + rs\sigma^2_{TLY} + rls\sigma^2_{TY}$ |
| T × L × Y | 18 | T × L × Y | $\sigma^2 + rs\sigma^2_{TLY}$ |
| T × SMU | 6 | T × SMU | $\sigma^2 + r\sigma^2_{TLYS} + rl\sigma^2_{TYS} + ry\sigma^2_{TLS} + ryl\theta^2_{TS}$ |
| T × L × SMU | 18 | T × L × SMU | $\sigma^2 + r\sigma^2_{TLYS} + ry\sigma^2_{TLS}$ |
| T × Y × SMU | 12 | T × Y × SMU | $\sigma^2 + r\sigma^2_{TLYS} + rl\sigma^2_{TYS}$ |
| T × L × Y × SMU | 36 | T × L × Y × SMU | $\sigma^2 + r\sigma^2_{TLYS}$ |
| Error(b) | 540 | ERRORb | $\sigma^2$ |
| Total (corrected for mean) | 863 | | |

Levels of:

| | |
|---|---|
| Location | $l = 4$ |
| Year | $y = 3$ |
| Replication | $r = 6$ |
| Treatment | $t = 4$ |
| Soil Mapping Unit | $s = 3$ |

† MS = mean square. Testing pattern for above assumptions:
F (SMU) = (MS for SMU)/(MS for L × SMU + MS for Y × SMU − MS for L × Y × SMU)
F (L × SMU) = (MS for L × SMU)/(MS for L × Y × SMU)
F (Y × SMU) = (MS for Y × SMU)/(MS for L × Y × SMU)
F (L × Y × SMU) = (MS for L × Y × SMU)/[MS for R × SMU (L × Y)]
F (T) = (MS for T)/(MS for T × L + MS for T × Y − MS for T × L × Y)
F (T × L) = (MS for T × L)/(MS for T × L × Y)
F (T × Y) = (MS for T × Y)/(MS for T × L × Y)
F (T × L × Y) = (MS for T × L × Y)/(MS for ERRORb)
F (T × SMU) = (MS for T × SMU)/(MS for T × L × SMU + MS for T × Y × SMU − MS for T × L × Y × SMU)
F (T × L × SMU) = (MS for T × L × SMU)/(MS for T × L × Y × SMU)
F (T × Y × SMU) = (MS for T × Y × SMU)/(MS for T × L × Y × SMU)
F (T × L × Y × SMU) = (MS for T × L × Y × SMU)/(MS for ERRORb)

be grouped for averaging purposes into those that have similar response patterns.

## REFERENCES

Bancroft, T.A. 1964. Analysis and inference for incompletely specified models involving the preliminary tests of significance. Biometrics 20:427–442.
Cline, M.G. 1977. Historical highlights in soil genesis, morphology and classification. Soil Sci. Soc. Am. J. 41:250–254.
Cochran, W.G., and G.M. Cox. 1957. Experimental designs. 2nd ed. John Wiley & Sons. New York.
Federer, W.T. 1955. Experimental design, theory and application. Macmillan Publ. Co., New York.
Satterthwaite, F.E. 1946. An approximate distribution of estimates of variance components. Biometrics Bull. 2:110–114.
Schultz, E.F., Jr. 1955. Rules of thumb for determining expectations of mean squares in analysis of variance. Biometrics 11:123–135.
Steel, R.G.D., and J.H. Torrie. 1980. Principles and procedures of statistics. 2nd ed. John Wiley & Sons, New York.

# Corn Root Distribution and Yield Response to Subsoiling for Paleudults Having Different Aggregate Sizes

M. J. Vepraskas* and M. G. Wagger

## ABSTRACT

**Chisel plowing loosens dense root-restricting layers within 0.25 m of the soil surface, while subsoiling can be used to routinely loosen dense layers down to a depth of approximately 0.45 m. Selecting one tillage method over the other to maximize yield is problematic because the lower depth of a root-restricting layer is often difficult to determine precisely if the layer contains structural aggregates. We hypothesized that the ability of roots to penetrate dense soil layers is related to the size of aggregates in the dense layers. Therefore, this study compared root distributions for chisel plowed and subsoiled tillage treatments and related them to soil properties and corn (*Zea mays* L.) grain yield for soils having dense layers that varied in their aggregate size. Yields and root distributions were determined for the two tillage treatments on eight Typic and Arenic Paleudults. Each site had a dense, high-strength soil layer below the depth of chiseling. Subsoiling significantly ($P = 0.10$) increased yields over chisel plowing at five sites. At four of these sites, the proportion of roots between depths of 0.3 and 1.0 m in the chiseled treatment was low ($\leq 0.20$) due to the root-restricting layer extending below the depth of chiseling. Although all sites had pan-like layers, which were characterized on the basis of bulk-density and cone-index measurements, only layers whose ped meanweight diameter was $\leq 1.2$ mm resulted in root proportions being $\leq 0.20$ in the chiseled treatment. These virtually nonaggregated layers also tended to have low amounts of plant-available water ($< 0.12$ m$^3$ m$^{-3}$). To adequately characterize root restricting layers that reduced yield in these soils, some measure of subsurface aggregation or a correlated property such as plant-available water or texture had to be evaluated along with bulk density or cone index.**

DENSE SOIL LAYERS such as tillage pans are commonly found in the coarse-textured, well-drained Udults of the southeastern USA (Campbell et al., 1974; Cassel, 1981; NeSmith et al., 1987). These dense layers often restrict root development and reduce yields, particularly during dry years (Kamprath et al., 1979). Depth and thickness of the dense layers vary, but the most root-restrictive ones form from 0.2 to 0.4 m deep (Naderman, 1985; Vepraskas et al., 1986).

Chisel plowing is commonly used for corn (*Zea mays* L.) production in part of the Southeast to disrupt these pans when their depth is shallow (i.e., $\leq 0.25$ m) (Naderman, 1985). In-row subsoiling is used to a depth of approximately 0.45 m when restrictive layers are deeper. Determining the depth of layers that restrict root growth in coarse-textured, nonaggregated soils can be done using penetrometers (Bowen, 1981; Campbell et al., 1974; Taylor, 1974). Dense soil layers that are aggregated have not been studied extensively, and hence their effect on root growth and yield are generally not known.

Vepraskas and Wagger (1989) examined the relationship between corn root abundance and cone index (CI) for chiseled and subsoiled treatments in seven Paleudults having dense layers. Root abundance was measured as either a root-length density (cm root/cm$^3$ soil) for soil cores, or as a number of roots per unit area for observations made on soil pit walls. They found that the CI/root-abundance relationship varied with soil depth and clay percentage. Diagnostic CI values, defined as those CI values where subsoiling would reduce CI and increase root abundance, were estimated for soil depths below the depth of chiseling. The effects of subsoiling on root development in the entire profile and on yield were not addressed.

Subsoiling's effect on yield for a given soil depends both on the soil's physical properties and rainfall. In addition to having a root-restricting layer, the soils where the largest yield increases from subsoiling are found have sand or loamy sand Ap horizons that retain low amounts of plant-available water (Campbell et al., 1974; Vepraskas et al., 1987). Given these physical conditions, the size of the yield increase produced by subsoiling depends on the amount and timing of rainfall and irrigation. For example, Porro and Cassel (1986) compared corn yields for subsoiled and disked-only treatments on a Norfolk loamy sand (Typic Paleudult) with a tillage pan. They found that during a dry year the relative yield increase produced by subsoiling, compared with disking, could range from 15%

M.J. Vepraskas, Dep. of Soil Science, Box 7619, North Carolina State Univ., Raleigh, NC 27695-7619; M.G. Wagger, Dep. of Crop Science, Box 7620, North Carolina State Univ., Raleigh, NC 27695-7620. Contribution from the Dep. of Soil Science, North Carolina State Univ. Paper no. 12060 of the Journal Series of the North Carolina Agric. Res. Serv., Raleigh, NC 27695-7601. Received 6 Mar. 1989. *Corresponding author.

Published in Soil Sci. Soc. Am. J. 54:849–854 (1990)

9