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THE GUY SMITH INTERVIEWS: RATIONALE FOR CONCEPTS IN SOIL TAXONOMY

by Guy D. Smith

Edited by

T.R. Forbes

Reviewed by

N. Ahmad
J. Comerma
H. Eswaran
K. Flach
T.R. Forbes
B. Hajek
W. Johnson
J. McClelland
F.T. Miller
J. Nichols
J. Rourke
R. Rust
A. Van Wambeke
J. Witty

Soil Management Support Services
Soil Conservation Service
U. S. Department of Agriculture

New York State College of Agriculture and Life Sciences
Cornell University
Department of Agronomy

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Foreword

Soil classification is the basis for agrotechnology transfer and for national or regional planning. Soils information is communicated through a common international system such as *Soil Taxonomy*. Using *Soil Taxonomy* will provide a strong basis for transferring information from soils in other parts of the world where important research results are available.

This monograph summarizes and discusses the various rationale behind many of the concepts implicit in *Soil Taxonomy*. These concepts and rationale were presented in a series of interviews by the late Guy D. Smith in 1980-81.

It is hoped that many of the concepts which users of *Soil Taxonomy* have encountered over the years will be clarified. And also that knowing the rationale behind these concepts will help with future revisions and changes to the classification system itself.

This monograph was produced by the Agronomy Department of Cornell University with funding from SMSS. It is one of a series of monographs by SMSS on *Soil Taxonomy* and soil resource inventories.

We wish to thank all the institutions who welcomed Dr. Guy Smith for giving the interviews, and supplied the facilities for recording and transcribing.

Hari Eswaran
Program Leader SMSS
August, 1986

Preface

This monograph has been compiled from a series of interviews given by the late Guy D. Smith in 1980 and 1981. The interviews covered by this monograph were held at Cornell University in 1980, by Dr. H. Eswaran in Ghent, Belgium in 1980, by Dr. M. Leamy in Ghent, Belgium in 1980, at the University of Minnesota in 1981, at Texas A&M University in 1980, in Maracay, Venezuela in 1981, and by Dr. J. Witty and Dr. R. Guthrie in Belgium in 1980.

The Guy Smith interviews were first started by Dr. M. Leamy of New Zealand. In 1976, Dr. M. Leamy and staff of the Soil Bureau of New Zealand conducted a series of interviews with Dr. Smith. The articles from the early interviews originally appeared in various volumes and issues of the *New Zealand Soil News*. Later, these and other interviews and articles were reprinted in *Soil Survey Horizons*. *Guy D. Smith Discusses Soil Taxonomy* a compilation of the *Soil Survey Horizons* articles summarized these early interviews by Guy Smith.

The considerable interest shown in these interviews was the impetus necessary for the Soil Management Support Services (SMSS), established in October 1979, to continue this effort. In 1980 and 1981, SMSS arranged a series of interviews at the University of Ghent, Belgium, Cornell University, University of Minnesota, Texas A&M University, and with the Soil Conservation Service (SCS). Dr. Smith also travelled to Venezuela and Trinidad and was interviewed by colleagues at institutions in these countries.

The format of the interviews were similar at each place. All interested persons were invited and were free to ask questions on all aspects of *Soil Taxonomy*. However, the coordinator of the interviews at each place also developed a list of major subject matter areas for discussion. Both the questions and answers were taped and reproduced.

Although the intent was to cover as much of *Soil Taxonomy* as possible, Dr. Smith's failing health forced the termination of the interviews in late 1981. Dr. Smith, did not have an opportunity to review the transcripts and consequently the transcripts on microfiche are reproduced with only some editorial changes. Readers are advised to bear this in mind when they use these transcripts.

The success of the interviews is also due to the large number of persons who came to discuss with Dr. Guy D. Smith. It is not possible to list all the names but we would like to recognize the main co-ordinators, who are:

Dr. M. Leamy (New Zealand); Dr. R. Tavernier (Belgium); Dr. R. Rust (Minnesota); Dr. B. Allen (Texas); Dr. A. Van Wambeke and Dr. M. G. Cline (Cornell); Dr. L. Wilding (Texas); Dr. J. Comerma (Venezuela), and Dr. N. Ahmad (Trinidad). Staff of the Soil Conservation Service, particularly Dr. R. Arnold, R. Guthrie (formerly SCS) and J. Witty (Washington, D.C.); J. Nichols (Texas); S. Riegen (Alaska) and F. Gilbert (New York) also contributed to the interviews.

The interviews for this monograph were transcribed from the recorded tapes by Dr. H. Eswaran, Dr. J. Nichols and Dr. T.R. Forbes. A complete transcript of all the interviews can be found on microfiche in the pocket attached to the back of this monograph.

After transcription of the original interviews, the editor assigned title and subject index words to each of the questions and answers of all the interviews. The listing of these index words appears on the final pages of this monograph. The interviews were then completely taken apart and reorganized using word processor and database manager programs. Segments of interviews covering the same subject were then grouped together into chapters similar to *Soil Taxonomy* and printed out. These interview segments carried supplementary index words and also references to the original interview and question number. The raw chapters compiled from the segments were then distributed to various technical reviewers, soil scientists, for review and reorganization. The reviewers were asked to put together the various segments into a text without changing the meaning that Guy Smith originally intended nor to add any statements that weren't specifically given or implied in the interviews.

The recompiled and edited versions were then returned to the editor at Cornell. They were retranscribed (if necessary) and put into a standard format that would be used throughout the monograph. All final chapters were reindexed and cross-checked with the original interviews.

The final document contains a table of contents, the main body which follows the topics of *Soil Taxonomy*, and an index which includes a listing of questions and interviews, and key words. This cross-referencing should help those readers who want to study the exact words and context of the interviews themselves (as found on the microfiche in a pocket on the back cover of the book).

T.R. Forbes, Editor
Cornell University
August 1986

In Memoriam
Guy D. Smith

Dr. Guy D. Smith, former director of the Soil Survey Investigations Division of the Soil Conservation Service (1973), and distinguished international soil scientist, died on 22 Aug. 1981, at his residence in Ghent, Belgium. Dr. Smith was a native of Iowa and a graduate of the University of Illinois. After receiving a Master's degree in 1934 from the University of Missouri, he worked in Illinois for 2 years for the Resettlement Administration. From 1935 until 1942, he was a professor of agricultural science at the University of Illinois. In 1940, he received his Ph.D. Degree from the University of Illinois. Dr. Smith served with the Army Air Forces in the China-Burma-India theater during World War II. After that, he joined the Soil Conservation Service in 1946 and worked in Iowa as a soil correlator. In 1950 he moved to Washington, D.C., and became the Director of Soil Survey Investigations in 1952.

Dr. Smith is the author of numerous publications on soil science. Four of Dr. Smith's many publications are noted as outstanding contributions that characterize the development of his career. His study of claypans and the translocation of clay in soils, published as Missouri Agricultural Experiment Station Research Bulletin 210 in 1934, brought him recognition as a pedologist. His stature as a soil scientist was firmly established by his landmark study of pedologic interpretations of the properties and distribution of Illinois loess, published as Illinois Agricultural Experiment Station Bulletin 490 in 1942. This became a model for studies of soil genesis in the Midwest. His role as soil correlator for the Soil Survey is characterized by the article "Prairie Soils of the Upper Mississippi Valley" in *Advances In Agronomy*, 1950, of which he was senior author. Finally, *Soil Taxonomy* published in 1975 as Agriculture Handbook 436 was the culmination of 23 years of his leadership of a project to develop a new comprehensive system of taxonomic soil classification. That work brought him international recognition, perhaps more wide-spread than that of any pedologist to this time.

Dr. Smith was the recipient of many honors and awards. Some of the more notable ones are the Department of Agriculture's Distinguished Service Award in 1962, and the Soil Research Award from the American Society of Agronomy in 1964. He was president and an honorary member of the Soil Science Society of America and a fellow of the American Society of Agronomy. He was awarded the degree Doctor of Science by the University of Ghent (Belgium) in 1968.

Dr. Smith held the Francqui Chair at the University of Ghent, Belgium in 1964-65 for collaboration with European soil scientists in the development of *Soil Taxonomy*. After retirement, he consulted widely on the application of the system, including service as Correlator in Trinidad for the Organization of American States (1973-74), Correlator for the Centro Nacional de Investigaciones Agropecuarias in Venezuela (1975-76), and as Soil Scientist for the Soil Bureau of New Zealand (1976-77).

In addition to his outstanding contributions to soil science and the Soil Survey, Dr. Smith was a well-liked and respected individual by all of those who knew him. At times Dr. Smith stood alone in receiving harsh and undeserved criticism regarding the development of *Soil Taxonomy*. From the beginning, he realized that the task was too large for one person to handle and that he needed the cooperation of soil scientists throughout the United States and the world. His quiet, patient fostering of scientific attitudes and cooperation gave him the strength to complete such a monumental task.

List of Reviewers

N. Ahmad, Head of Soil Science Dept., University of the West Indies, St. Augustine, Trinidad.

J. Comerma, Fondo Nacional Investigaciones, Agropecuarias, Maracay, Venezuela.

H. Eswaran, Project Leader, SMSS, SCS/USDA, Washington, D.C., 20523.

K. Flach, Special Assistant for Science and Technology, SCS/USDA, Washington, D.C. 20013.

T.R. Forbes, Senior Research Associate, Department of Agronomy, New York State College of Agriculture and Life Sciences, Cornell University, Ithaca, N.Y. 14853.

B. Hajek, Professor of Soil Classification, Department of Agronomy and Soils; Auburn University, Auburn, Alabama, 36849-4201.

W. Johnson, Retired, formerly Deputy Administrator of SCS; Gleneden Beach, Oregon, 97388

J. McClelland, Retired, formerly - Director of Soil Classification and Correlation, SCS/USDA, now in Morgan Hill, California 95037

F. T. Miller, Head, Soil Survey Staff, Northeast National Technical Center, SCS/USDA, Chester, Pennsylvania 19013.

J. Nichols, Head, Soils Staff, South National Technical Center, SCS/USDA, Fort Worth, Texas 76115.

J. Rourke, Retired, formerly - Principal Soil Correlator, Head, Soils Staff, Northeast Technical Service Center, SCS/USDA, West Chester, Pennsylvania 19380.

R. Rust, Professor of Soil Genesis and Characterization, Dept. of Soil Science, University of Minnesota, St. Paul, Minnesota 55108.

A. Van Wambeke, Professor of Soils, Department of Agronomy, Cornell University, Ithaca, New York 14853.

J. Witty, National Leader for *Soil Taxonomy*, SCS/USDA, Washington, D.C., 20013.

Chapter 1

SOIL CLASSIFICATION

reviewed by W. Johnson[1]

Foreword

I don't have any particularly startling comments to make. For the most part I rather carefully concealed the reasons for doing this when I wrote *Soil Taxonomy* for the simple reason that if I had explained why we did this or that, the reader would be more apt to pay attention to the reason than to the actual definition. We wanted a test of the definitions, not of the reasons. The reasons, of course, are going to be very highly disputed by people who have other backgrounds than those of us who developed *Soil Taxonomy*. It seemed best if we wanted to test the definition that the reasons had better be kept out of the book for the most part.

There are a few places in *Soil Taxonomy* where I did spell out a few of the reasons for one requirement or another, but for the most part they are very carefully hidden. The reason was that this was a staff effort. A lot of these definitions were prepared by committees of the Regional Work Planning Conferences and the special meetings at the regional Technical Service Centers. I couldn't sit in on all those discussions of the committees; so very frequently I didn't know the reasons that they proposed for a specific definition. These definitions then were presented generally to the entire conference and were accepted or rejected according to what the conference felt, at that date. Frequently the conferences would reverse themselves from one year to another. I think most of you know that Division 5 asked Professor Rust of the University of Minnesota to prepare a list of questions. I agreed that I would do my best to answer the questions if I knew what they were. But I didn't know precisely what things were bothering whom.

Professor Rust prepared a list of questions and Dr. Cline of Cornell University prepared another list of questions. I will go from here to the University of Minnesota to a meeting that will be somewhat similar to this one at Lubbock, Texas, and from there I am proposing to go to Venezuela for interviews on the soils of intertropical regions. I had hoped that by coming to Lubbock I could introduce into these questions those things that were bothering people familiar with the semi-arid and arid regions of the U.S. After Venezuela, I will go to Trinidad to see what questions exist now in the West Indies. I finished the interviews with Dr. Leamy from New Zealand. This will then give me, I hope, a good cross section of questions. There will be many duplications. I will have to sort the questions according to subject matter and consolidate some of the answers. **Question 1, Texas**

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1. Retired, formerly Deputy Administrator of SCS; Gleneden Beach, Oregon, 97388

1.1 Definition of Soil

There are many soils where the rooting is in the O horizon and yet we classify the soil on the basis of the mineral part where the soil has virtually no roots. Histosols with shallow rooting are an unresolved question so far as the O horizon is concerned and would require considerable thought on the part of the people who knew something about these soils. In most of the U.S., the Soil Conservation Service staff, the Experiment Station staff are not concerned with such soils. They don't have them other than in the forest. So the lower boundary of soil in that situation, as in Histosols, has got to be somewhat arbitrary. We pointed this out in *Soil Taxonomy*, that the lower boundary was a very difficult one and that in many instances, in many kinds of soils, the lower boundary could only be an arbitrary limit. In *Soil Taxonomy* we have treated two meters as its arbitrary limit, this limit being taken on the basis that it is impractical in most soil surveys to examine the soil frequently enough below two meters to have any reliability in our observations. With respect to unvegetated soils, I'm going to have to draw a line somewhere between the field of pedology and the field of geology. Normally we left the barren areas to the geologists although they concern themselves generally more with the bedrock than with what's above it. There is a question where the regolith is thick and the soil scientist stops at two meters and the geologist starts at 50 meters - who's field is this one in between? In some instances, as where we are irrigating a new project, we need to know what is going to happen to the leaching water and it is necessary for our interpretations to make rather deep observations in the regolith to figure where that water is going to surface. This requires power drilling equipment and is only practical for very intensive uses, such as irrigation. The salt flats in some cases do carry vegetation, in which case they become a soil and then there is a problem - is the salt flat a salty parent material or is it a saline horizon? In general, however the purpose of *Soil Taxonomy* is to facilitate soil surveys and their interpretations. It is inconceivable to me that we are going to spend very much money studying these unvegetated areas; they are going to be left to the geologist rather than brought into the classification. There are some soils in Antarctica but there are very few. There is no particular reason to make very many soil surveys in Antarctica except to get at the history of the area. And that's not a good reason for most soil surveys. I think that most that are going to be made (in Antarctica), probably have been made already by the people in New Zealand. **Question 4, Minnesota**

If I go to the definition of soil, the lower limit of biologic activity is the common rooting of native perennial plants, a matter of 1 or 2 meters. In general the series control section stops at 2 meters. There are only one or two exceptions that I can think of in *Taxonomy* where we consider the soil to go below 2 meters. No argument that the writing can be improved; I am to blame for it; I am just not smart enough. **Question 106b, Cornell**

I don't think that people doing pot experiments in greenhouses should use the series name for the materials in their pots. They should perhaps say where they got it from, what soil. But it is not a soil in the sense that we are classifying soils. A soil has many meanings and ... People have written me that if they told their wives there wasn't any soil in the pot where she was growing her plants that they'd be thrown out of the house. I don't know of any common word that one would substitute. Soil has a number of meanings in the English language. If you look it up in the *Oxford Dictionary* of the English language it takes about two to three pages. **Question 157, Minnesota**

We say in *Soil Taxonomy* that when the water gets deep enough that we have only floating plants, that ceases to be soil. That probably does vary some according to the environmental conditions of wetlands. If we are going to use these soils, then certainly we must find ways and means of making soil maps that will be helpful in predictions of the consequences of use. If the wrong wetland is drained, you may wind up with a Sulfaquept. Some of them, at least for three or four hundred years, will grow nothing. Those are the extreme ones. We must be able to warn people not to drain such soils. You can't find out about their existence in the brackish waters without finding some way to get over the ground and collect samples to study. Shrinkage of these soils on drainage, the ones that do not become acid, can also make very serious problems in the engineering use of the soils. You may have seen the New Orleans subdivision where the soil has shrunk away from the house and the

garage, so we must be able to predict this shrinkage, and the architects must understand the problems they are going to get into if they build on those soils. I would say that if there is a perspective use of the soil system, if you're proposing to use it for something, then we do need a soil survey, although it may be difficult. I've waded in water up to my hips to look at these things--fortunately, in a warm country. **Question 50, Texas**

From what little I've read of the work, mostly by Professor Tedrow, in the high, dry Arctic Islands, you do have plants. If the vegetation is absent most of the year, but may be there for a short period during the beginning of the warm season, then it comes within our present definition of soil. However, we specifically mentioned in the introduction that we don't know enough about these soils and they are not brought into the taxonomy at present. This is a job for the future. **Question 5, Minnesota**

1.2 The Pedon and the Polypedon

The pedon is a somewhat arbitrary volume of soil. It has virtually no natural boundaries. It is so small that it cannot have shape without considering the elevations of other pedons. I observed particularly in my travels in Europe that there were many soils in which horizons such as argillic horizons, spodic horizons were either forming or undergoing destruction. The argillic, the spodic horizons were not continuous, but were intermittent on varying scales from a matter of 5 or 10 centimeters to perhaps 5 or 7 meters. Because these were repetitive discontinuities in the horizons, it seemed that in the U.S. we would prefer to classify these soils as a single series with the intermittent horizons rather than as a complex of very tiny bodies of contrasting kinds of soil. We must identify the soils, at times collect samples, and we need a minimum volume for the study of the arrangement of the horizons and for sampling them. If there is no lower limit to the size of the sampling unit we run into problems. I have seen when sampling where the pedologist took his pen knife and took a tiny sample of soil on the point of his knife and carried that off to the laboratory for analysis. Well this is getting to the extreme and it seems to me to be too small. We must tolerate discontinuous horizons where a root penetrated a horizon and surficial material has fallen in and filled the hole or a worm has made its hole which has a coating around the edges, the sides, but which has not been filled. These things we have to tolerate and accept them, but they are examples of holes in horizons rather than any discontinuous horizon, because the horizons surround the hole, whereas in the discontinuous horizon there may not be any kind of horizons that surround the particular break in the horizon itself. Because there normally are no natural boundaries in pedons, you can have an infinite number of pedons and polypedons according to where you start your measurement.

The basis for setting the limit [for a pedon] at somewhere between a square meter and 10 has been criticized on the grounds that the properties that are determined by cycling of bases may be quite different where one tree has grown from those where another species of tree has grown. In the parts of Africa, Zaire for example, we have species that collect calcium and the trunk (the wood) of the tree contains large chunks of calcium carbonate, and next to it may be a sulfur collector, and the base saturation under these two trees are very different, and it has been proposed that the pedon be enlarged to something like the canopy spread of a mature tree. By and large this is a little too large for sampling, and so we have put most of our emphasis on subsurface horizons where the effect of the growth of one tree has its effect in the surface horizons but not in the subsoil, and we anticipate that many of the differences that we find under forest soils, in base saturation, organic carbon, nitrogen, and so on, are in the surface horizons, and subsoils are much the same because the subsoil properties are not influenced so much by the growth of one generation of a specific species of tree. So while we have discussed the possibility of enlarging the pedon to the canopy area of a mature tree, this did not seem to facilitate sampling and analysis if we could base most of the properties on the subsoil horizon rather than the surface horizon. **Question 26, Cornell**

I'd say very briefly that the pedon has no natural boundaries. Its boundaries are almost completely arbitrary depending on where you start your examination. You can have an infinite

number of pedons in most soils in a few acres and so I don't see how it can be considered anything but an arbitrary sampling device. **Question 35, Minnesota**

Both genetic and interpretive implications were considered in defining the pedon. The actual limits were set by the normal range in the size of the variability in the Vertisols, for example. It's the same in soils with permafrost, the same size. We took the maximum size to give us the fewest complexes as possible. In the design of a structure, a house for example, on a Vertisol, you have to consider the swelling nature of the whole soil, and not just the center or the edge of the polypedon. You control your shrink-swell by keeping the whole soil moist or dry, so that the moisture doesn't change over the year. These are things that you don't manage as spots; you manage as fields or as good size polypedons. **Question 15, Texas**

One thing that the cyclic variability in the pedon of variable size accomplishes is the simplification of numbers of soil series that are required in mapping the landscape. Where you get this regular repeating pattern, (more or less regular repeating, never exact), would seem to be as good a characteristic of a soil series as the nature of the clay and the amount of clay, and so on. It is variable. It gets a little complicated in some situations where the diagnostic horizons either are just beginning to form or are being destroyed. Let's start first with the destruction of a spodic horizon by liming and fertilization. The destruction starts in spots, and doesn't proceed uniformly over the whole pedon. These pedons normally can be about a meter in size, because the spots where the spodic horizon is biologically destroyed are normally a matter of a few centimeters rather than a matter of a meter or so. Where the horizon is starting to form, as in the situation with a Xeralf with rather shallow limestone, the argillic horizon is not a continuous thing. As the rock becomes shallow the clay, that has been mobile, is moving from the shallowest spots to the deepest spots in the landscape. If you had complexes, it would require a considerable number of series, rather than one series in one ruptic subject. The intent was to simplify the manner of record-keeping of series as well as to show the genetic differences where the horizons are being formed, or being destroyed. Where the variability is oscillatory, you have some of the same problems as where it's cyclic. You will have for each area, a range in thickness permitted in the various horizons. If the oscillatory one exceeds that range, then you would most likely have a complex unless the oscillations were very closely spaced. There are many places where you have to have complexes in your mapping. I keep calling to mind a situation in southern Illinois, where I first started to map soils. We had, what I think you probably call slickspots in Texas. Many of them no larger than this room, and on any reasonable scale, there was no option but to set up complexes. You had several complexes according to the percentage that you estimated was occupied by the slickspot soils with natric horizons.

But, by and large, if we can keep the numbers of complexes to a minimum, it is easier to explain the soils to our users and it is much easier to maintain records on the series. It does cost to keep records on every one of these series that we have. Now I'm told the number of series in the U.S. is approaching 14,000 to 15,000. **Question 8, Texas**

The pedon is intended as a sampling unit to let us classify the polypedon. The polypedon is the one we must classify if we are making a large-scale map. That is what we try to delineate if our map scale is suitable. With small-scale maps the question is the opposite way. We can not concern ourselves with delineating the polypedons on small-scale maps. The polypedon has properties that its individual pedons do not have. It has natural boundaries which a pedon does not have, where one polypedon grades to another kind of soil. You have a wider range of properties within the polypedon than you do within any single pedon. The polypedon has a shape that the pedon may or may not have but particularly where one is growing row crops in a soil that is naturally somewhat wet, the individual pedon has a man-made slope that the polypedon does not have. So, you have slope phases of the polypedons and these would be very different for an individual pedon. Where the row has been raised you may have quite a steep slope, actually, in the pedon, where the polypedon is flat. So you have in the polypedon a wider range of properties; you have natural boundaries to other kinds of soil and you have shape, none of which you have in the pedon per se. The polypedon, the individual polypedon again, is restricted in its range and properties relative to the series. **Questions 29 and 171, Cornell**

You examine the soil mostly at what amounts to points. When you are sampling the pedon, you have a volume that covers an area of at least a square meter. If you find no variability within that square meter, you have fixed pretty much the size of your pedon. But if you find there is variability within that meter, you must probe around your initial pit to determine whether that variability is a boundary between this soil or whether that variability is a cyclic thing, and if it is cyclic, how large the cycle is. The polypedon is supposed to consist of adjacent pedons that do not cross the boundaries of a limit between taxa at some category above that at which you are making your map legend. Your pedon is a sample of your polypedon. You have worked out in advance the limits of your taxon where you have the borders that adjoin kinds of soil that differ. **Question 136, Cornell**

Let's consider at least two or three attributes that the polypedon has that is not possessed by an individual pedon. First, the boundaries of a pedon are, to a large extent, purely arbitrary and depend on where you start to dig your pit. The boundaries are not the same as the bulk of the pedons that you might study for identification of the polypedon. They are real, natural boundaries. You may not be able to see the polypedon in its entirety on any single day, but you can, and in detailed soil surveys we try to represent the boundaries of the polypedon on our soil maps. They are obviously imperfect because of cartographic problems of scale and because of sampling errors. We make an effort to indicate the boundaries of the polypedons which are the natural boundaries. The second attribute would be the matter of the slope. The polypedon has a slope which can be measured with a simple abney level if you like. Many of the cultivated soils have been either put into beddings and the pedon slope would be quite different from the polypedon slope. The cultivation of many crops requires that the soil be ridged with the crop planted in the ridge and so the slope of the pedon may be very steep as against the nearly level slope of the polypedon. It has been proposed that this is no problem because you just compare the elevation of this pedon with that one but then you're using another soil to classify this one. We have said you must not do that. You must classify the soil on its own properties so these are two reasons for choosing the polypedon as the unit to classify.

At one stage we used the analogy to the individual basic cell of the mineral. The polypedon is defined as a group of contiguous pedons that do not differ significantly in any diagnostic property. Though that sort of analogy would appeal to most any mineralogist, it would not really appeal to the man who's making soil surveys. **Question 47, Texas**

Replacement of the old term "soil individual" with the term "polypedon" was just something for consistency in terminology. I think that having defined a pedon to get at the so called "soil individual" would have been confusing. We went to the term "polypedon" to relate it to the pedon. It's not clear to me, certainly, whether the soil individual that we used to talk about was a pedon, or a polypedon, or a profile. I think it very commonly was a profile. **Question 121, Minnesota**

The purpose of the polypedon was to permit classification within a series of somewhat contrasting kinds of soil, such as are illustrated in *Soil Taxonomy* --that is a natural landscape unit with great local variability. It seemed unnecessary to mess up our map unit name with an association or a complex of 3 or 4 different series. This sort of local variability seemed to belong at a very low categoric level if anywhere, because the variability is a property of that soil. **Question 137, Cornell**

You make a soil survey for a particular reason, or you should. Knowing that reason, then, you will design your map legend so, that your survey will meet those anticipated needs. This may or may not require that you delineate polypedons. In Alaska and Nevada, they are not particularly concerned with polypedons there; they add virtually nothing to the interpretations that you can make. The only thing we must do, then, is to name what we have enclosed by our boundaries in such a way that it is intelligible. **Question 142, Cornell**

On the Russian steppes, where you have a loess mantle and a subhumid climate, you can have some very large polypedons if that loess has not yet been dissected. If the loess has been dissected by geologic erosion your polypedons may be quite small. Particularly in arenic areas you might not be able to find enough that you can map; virtually everything is going to be a complex or association. **Question 141, Cornell**

We have slope built into the classification of at least two great groups and we need it in some others. The two we have are Aquolls and Aquults. These are often wet soils; they must be drained for cultivation, and the common practice is to shape these nonsloping soils to provide surface drainage. The sloping members do not require shaping for drainage, and they require some sort of interception tile to cut off the seepage water. The same thing would be true for a good many of the Histosols. If these are cultivated and the polypedon is flat, then normally you have the soil ridged very steeply to provide for a better aerated medium for plant growth. We have other Histosols that are naturally sloping, in Malaysia, up to 50 percent or more. To get at the series one has to consider the polypedon shape rather than the slope of the individual pedon. **Question 33, Cornell**

I will comment on the soil geography of the Cerrado of Brazil, briefly. Around Brasilia you have largely Oxisols except on the steep valley sides and the floodplains below; those are not Oxisols, at least in the flood plains. You have a wide range of particle-size distribution. Some are intergrades to Quartzpsipsamments; some are very clayey; and these occur mixed up in the landscape according to the parent rock or the sedimentation that took place when weathered materials were laid down on the Planalto. So there are large areas, perhaps if you exclude the steep side-slopes where you have a gallery forest, there are large areas that would come out as Acrorthox, but they would have widely varying textures and water-holding capacities. So there would not be one polypedon, there would be many because there would be a number of subgroups and families in these large areas. We have not developed a concept comparable to the polypedon for use in small-scale maps. **Question 32, Cornell**

I do not know of any natural body like the polypedon that could substitute for the polypedon on small-scale maps. We have associations, contrasting kinds of soil that we can show on small-scale maps.

I have looked at one survey in Kansas where every soil is classified as a Mollisol. Maybe you could consider Mollisol the taxonomic unit there, but it is rare that you don't have some contrasting soil such as Aquolls and Udolls and Calciustolls mixed in with each other. So the smaller the scale the greater the necessity to go to a higher categoric level to define what is in your map delineations. **Question 31, Cornell**

1.3 Application of Soil Taxonomy to Soil Surveys

Making a soil survey is a rather complicated sequence of operations. You should not make a soil survey without knowing why you are doing it. What do you want to make that survey for? You must design your legend so that when the map is completed, you are able to make the important interpretations that are needed for the use for which that soil is apt to be put. The naming of your map units involves the correlation problem. If this breaks down, then you are defeating one of the purposes of your soil survey; you are not describing accurately what is in a particular map delineation.

The interpretations involve another step of reasoning. We have tried to build into the basic classification system the properties that are most apt to be important for interpretations. The importance is not everywhere in the world the same, just as the purpose of making the surveys are not everywhere the same. But one must go through another step in reasoning to make the interpretation. They are not there themselves but one can, then, still make interpretations if the important properties have been built into the definitions of the kinds of soil you have mapped. **Question 143, Cornell**

In our first approximation, we had one order for organic soils and another order for mineral soils. This created an extra category that we could see no use for because we could just as well subdivide the orders as to organic soils and mineral soils that have this and have that and have the other and we didn't need that extra order. We examined what we had in our earlier classifications and what other people had in classifications in other countries to see what we could devise in the way of orders. Marbut insisted on only two classes in his highest

category -- *Pedalfers and Pedocals*, and he ruined his system on this, because we have soils that have both accumulation of carbonate and accumulation of iron and aluminum, and we could readily say we'll give priority to one or the other. We also had soils with neither, and they had no place to go. So Marbut just dropped them out of this system and said we'll classify these on the basis of the soils that are around them because eventually a wet soil is going to be drained by geologic erosion and then it will begin to take on properties of either a Pedocal or a Pedalfer. How he was going to drain the coastal marshes, I don't know, except by dropping the ocean, it can't be done.

We examined what had been done in previous U.S. systems and in the other systems in countries where they were making soil surveys. We did not look into classifications in countries without a soil survey program. We didn't feel we would be apt to learn much from that. We wanted, then, to have enough classes in the order category that we could accommodate the major differences in genetic processes, but not more than we could readily remember. We figured one could understand 10 classes or a dozen without much trouble but not 50. We also needed in our taxonomy, a sort of a key that could be used for identification in the correlation process. When you start to correlate a soil, an unknown one, you first figure out what order it's in and then the suborder and then the great group and so on down. And in each step we don't want to have more classes than can be readily understood in the context of that particular taxon. *Taxonomy* has 5 subdivisions on the average. We didn't want to have 50 subdivisions although we wound up with a few families with 50 series. Still, the series are not defined in *Taxonomy*. We left it up to the correlation staff to devise their own keys for these large families. There was nothing sacred about the number 10. Now I've proposed an eleventh and that's an awkward number. I think I'll look around for a twelfth somewhere. Twelve is a much more satisfying number than 11. **Question 52, Texas**

It is quite likely that nature in building a landscape unit didn't pay much attention to our definition. I remember Professor R.S. Smith at Illinois University. He always said, "If I had the world to make over I could do it a lot better". Because some of the soils were "stubborn" and didn't fit anywhere into any series that we had in Illinois or that we could map. Sometimes there were real complexes that surely crossed family and subgroup definitions in the glacial till. In the loess it was much simpler to make a map that would contain relatively homogeneous properties througnout the delineation. The present family is somewhat at the level of generalization, that the soil series was in much of the U.S., say in the late 20's and early 30's. The number of soil series is now approaching 14,000 or 15,000. At that time the number was much lower, somewhere above the number of series of Marbut's *Atlas*. It may have been slightly fewer than we have families today but the same order of magnitude. There is nothing we can do to unravel some of nature's complexities except to map associations and complexes which we use depending on the scale we are mapping.

The taxadjuncts, I think, have been over-used because of people's failure to recognize the limits of significance of their observation and of the laboratory. There is an appreciable sampling error involved in the collection of samples. The soil survey laboratory has always insisted on matching samples, that is, sample 2 pedons in 2 different polypedons and you map them as closely as possible. So the differences between these two matching pedons can be used now with a little statistical analysis to determine the order of magnitude of the sampling error. So far as I know this really has not been done yet. It is an appreciable error. The laboratory technician who runs duplicate samples from time to time to check on himself has some notion of the magnitude of laboratory error. Particle-size analysis is a measurement that is made in the laboratory. But in the classification of the soil itself, you have these two sources of error, and I think that when you call a soil a taxadjunct because it has 5 percent too much silt, that you are ignoring the reliability of the laboratory measurements, which in turn are subject to the sampling error. I think it has been over-used, if the soil fits a given family except for 5 percent too much silt, then I don't think we should bother our users with calling it a taxadjunct. I think we should use a series name, but I am not responsible for correlation and for the nomenclature that's used in the soil surveys. The user of a soil survey is not concerned with whether that is a taxadjunct of Clarion, he is concerned with what we have to say about that soil in terms of its responses in use and management and he is not mislead if we use the wrong name provided we make the proper interpretations for it. He couldn't care less if it is Clarion or Clarion

taxadjunct as long as our interpretations are the same. That has nothing to do with *Taxonomy*. This is how we use it, it's the application to soil surveys. **Question 70, Minnesota**

As I have said before, the extent of a given kind of soil is not important in respect tc position in the taxonomy nor in respect to genetic interpretations. There probably somewhat different guiding principle involved in selecting the pedon that typifies or repres best the map unit. I shouldn't say "typifies" probably because I think that "representing perhaps a better term. If it is to be representative of all of the map delineations carrying particular symbol in the particular survey of the county, I think the area is a matter of sc consequence. You map a phase of a series in one county and you go two counties away and map that same phase of that same series but it is not necessarily quite the same as in the f county. And for some uses of these soil surveys, for example planning a secondary highway, engineers at least are interested in what they are going to run into most frequently on t particular map unit. In splitting up the continuum of soils in *Taxonomy* as I said, we triec avoid that but we had that one little inescapable bias that if the soil was so rare that we ne saw it, it wouldn't get into the system.

When we first started our cooperative work with the highway engineers, the Bureau Public Roads, we took three samples per county. One that represented about the center or middle of the range in properties in that particular series in that particular county. One t was marginal to some adjacent series but still within that same series. And another that marginal to a third series but still within the range of the first. For some years we sampled soils for the cooperative program with the Bureau of Public Roads.

The Bureau of Public Roads at that time was conducting a research program with the i of studying the relation between the map units and the engineering classification and tl wanted some idea of the range within the mapping unit that they might expect in a giv county and then, over time, the range within that same mapping unit but in other counti Once they had established to their satisfaction that they could use the soil survey data tl discontinued their research support for it and the cooperation then began with the St Highway Departments. At that time I lost track of it. **Question 134, Minnesota**

We have said in *Soil Taxonomy* that we have tried to put major emphasis on subsurf horizons rather than on surface horizons which are most apt to be lost by erosion. And so lc as we can identify remnants of that diagnostic horizon, in this case presumably it might ha been an argillic horizon at one time, we treat it as a soil that has an argillic horizon. In sucl soil we need only to be able to identify the clayskins. We do not require any increase in c with depth because we have so many soils that are truncated with plow layers in the argil horizon. We don't like to split the series into new series because of erosion as long as we c identify the diagnostic horizon. In the case of the Udalfs it's a part of the argillic horizon tl remains. If the diagnostic horizon has been completely lost then we must change t classification to classify the soil on its present properties and not on the properties that we thi it should have had at some time in the past. What you think it should have had and wha think it should have had may be very different.

The study of materials between the lower limit of the soil and the underlying bedro involves several problems. The first one of course is the difficulty of making enou observations to yield valid conclusions about the significance - where it is critical to t interpretations as in an irrigation project in which one needs to know what is going to happ to the leaching waters. Although it is difficult, it still would be essential that the pedologist f his interpretations have a drill rig and bore it out and find out just what underlies the soil a overlies the rock. It is a no-man's field and unless one feels that it is important interpretations, certainly the pedologist should not waste his time on it. If it is critical, then, is essential that the pedologist work out the distribution of the underlying unconsolidat materials. I only know of a few instances where this has been done and always for irrigatic **Question 125, Texas**

Orders of soil surveys - the orders 1, 2, 3, 4 came along since I retired. I am not familiar with them, but probably they mean detailed, semi-detailed, reconnaissance, and something else. We used to have names instead of numbers.

I know that good examples of small-scale soil surveys are scarce in the U.S. unless it is for the extensive range soils where you're making virtually only one interpretation -- the production of edible forage. And yet I consistently advise people in the developing countries to avoid using soil series at any cost. **Question 124, Minnesota**

I could point out that the soil survey of Belgium has never used soil series. The map symbol is the name that they use for the kinds of soil. It's not ordinarily what we would consider the series level. It's more apt to be at the family or subgroup level and phases. But we don't have anything other than the symbols on the map. They've had no troubles with this procedure. **Question 125, Minnesota**

I would, as I mentioned earlier, be inclined to use phases rather than series if I had to make different interpretations for a particular kind of soil on floodplains at Lincoln or Champaign-Urbana or St. Paul or Kansas City. For the production of maize it's almost certain that the estimates of yields are going to vary. I would use temperature phases instead of series as I went from north to south. From Lincoln to Champaign-Urbana I don't know whether on floodplains there's enough moisture difference that I would want to try to develop phases for soil moisture. If you went to the upland, I might take a very different point of view but on the floodplains I would not expect that to be a problem. The moisture differences between Lincoln and Champaign-Urbana are considerable on soils that do not receive extra moisture from flooding or runoff. The general rule in northwestern Iowa amongst the farmers is that, while they grow alfalfa, if they have the alfalfa down three or four years, they're going to have three or four poor crops of maize. It will take about as many years to remoisten the soil as the alfalfa stood there. There is no such rule at Champaign-Urbana. There the soil will remoisten the first year after you plow up the alfalfa. This would indicate a considerable difference in soil moisture relations that will not be corrected readily by plant breeding and I would incline to have this at the series or the subgroup level depending on the magnitude of the difference. **Question 128, Minnesota**

I might supplement this with the statement that I made earlier, that it was once the general policy not to carry the same series across major types of farming boundaries. **Question 129, Minnesota**

There has not been much U.S. experience with soil correlation at categoric levels above the soil series. In Alaska and in Nevada where the potential uses of the soil are limited to very extensive grazing either by cattle or reindeer or wildlife, the soil maps have been made without establishing series, but using phases of families or subgroups for the map unit. The major problem here has been that the potential users of the soil maps do not understand the technical names of the families so that interpretations then must be made in terms of the symbols that appear on the maps as the capital "A", little "a", is one kind of soil to the user. This appears in the legend with the technical family or subgroup name and the phase name, but the user does not have to go through the technical name. He goes directly from the symbol on the map to the interpretations that are of interest. **Question 21, Venezuela**

Mapping at a categoric level higher than the series is currently being done in the United States and in most of the developing countries. In Alaska for example, the Exploratory Soil Survey, the legend is largely based on categories higher than the series because they have no particular use for the series concept in areas of soil where about the only potential use we can see is grazing by wildlife. In Nevada, in the small-scale mapping they are not using series because again the utility of the series is small when the only foreseeable use is very extensive grazing by livestock. In the developing countries their first surveys generally are made at scales of 1:50,000 or smaller. They have so little experience with the use of the soils, that if they were to establish series they would go through the same process that we went through in the U.S. As they acquired knowledge about the soil behavior they would be constantly splitting their series and setting up new ones. My thinking is that the initial surveys, certainly mapping at the family category, is about the lowest categoric level that should be used: even in small-

scale maps, of course, you cannot map at the family level. You can only map as associations of subgroups or great groups. The soil map of the U.S. in the *Atlas* uses associations or phases of great groups as the basic taxonomic unit. **Question 67, Texas**

Surely we should encourage the making of more maps at a higher categorical level. In the lesser developed countries, where there is relatively little soils information, the use of series as the basis for map units of large-scale maps is going to result in the same kinds of problems we have had in the U.S. Since the survey was started in the U.S., series have been split time and time again and the names changed -- at least a large proportion of the series have been split. In such a situation as the lesser developed countries, I would encourage the use of a higher category until such time as we develop information that will permit us to make different interpretations for different parts of a soil family.

The same thing, I believe, is being done in Nevada where the only foreseeable use is very extensive grazing. It may take six hundred and forty acres to support one animal unit there. They are mapping these extensive areas of -- I hate to call it rangeland, because it is so barren -- but there they are not using soil series. There's one item that Sam didn't mention that I would like to -- if he had been able to spend the time to prove that he had two thousand acres of something or the other, would it be worth the cost of keeping books on all those series that undoubtedly exist in Alaska? I think the answer is no. When you establish a series, you have to keep records on it from then on until you discontinue it. So this is an additional cost and it's hardly justified when there's only one very extensive use that can be foreseen for the soil.

Soil series records have to be kept in the SCS State Office, in the Technical Center, and in Washington. That's three sets of records you must keep on one series when you can only make one interpretation for it. And that interpretation is the same as the one you make for a great many adjacent series.

Of course, you can make interpretations for phases of families or phases of subgroups. There has been, I have sensed, a great fear of using the subgroup or family names in legends. Now, I don't see any problems in this. You can have a short name for a family which consists of the symbols that appear on the map. The map symbols identify interpretations in your interpretative tables. There probably will be a separate table for the use of pedologists in which the symbols are related to particular subgroups or families. This is not going to bother the people who try to use these maps. They won't look at that technical soil classification; they will look at the interpretations that have been made.

Scientific soil names won't bother people indefinitely. There's a fear on the part of the pedologist that the user is going to be confused by these Latin and Greek names. The horticulturist doesn't hesitate to talk about a Rhododendron. That doesn't bother them a bit, but Rhodoxeralf, for some reason, seems to be a bad name. Certainly it is unfamiliar at the moment. **Question 123, Minnesota**

Some soil surveyors say it is their practice to ignore some soil variations outside series limits when mapping. I would hate to do that myself. If you begin to allow the limits to vary according to the feeling that day of the correlator who is naming the map units, then you are bringing a large element of subjectivity into the use of the taxonomy in correlation. I would rather the man in the field realizes - and he generally does - that there are quite a few inclusions of slightly contrasting soils within a given set, a given group of delineations that carry the same map symbol. He must decide whether or not these soils that lie outside the range of the series that the map unit will be named for are or are not inclusions. If they differ significantly in their behavior then it is up to the fieldman to consider a change in the name of the map unit. We have standards in the *Manual*, we have other standards in the present *Soil Handbook*, and probably they will be changed more before we get through. These are fairly fixed rules that can be used in any of the regional technical service centers. There's been a lot of complaints by a few people about using the name or the given series for the concept that we have of that series and using that same name as the name of a mapping unit which is not the same as that of the series. The concept of the map unit applies to real bodies of soil that are given a particular symbol with a line around it. It is two different usages of the same word but this does not really bother me because in context the user knows which meaning is intended.

When we use "Miami silt loam" in a published soil survey with a map symbol "MS", it is obvious that this is not the conceptual Miami of *Soil Taxonomy*. It is an application of that concept to a real body of soil out there somewhere in the county. The point is we must not mislead the users of that soil map, that's why we are in business. We make these soil surveys and people find them useful.

I am willing to allow class limits to stretch during mapping, yes, but I wouldn't want to allow a variable limit to the conceptual unit that is Miami silt loam in *Taxonomy*. **Question 71, Minnesota**

What has happened to taxonomies used for purposes other than soil survey? As soon as the man who developed them has retired, they have been replaced. Where the classification was intended for making soil surveys, as the Dutch classification, the system persists even though the original authors would disappear from the scene.

A classification system should be dynamic, in the sense that it should be continuously used and in the process continuously tested. You must remember that a classification is a creation of man and is a reflection of the state of knowledge at that time and the uses that were intended at that time. Both of these may and will change and the system should be able to accommodate these changes. If not it becomes decadent. **Question 153, Cornell**

1.4 Mapping Units and Taxonomic Units

There is a distinction between the taxonomic unit, which is conceptual, and the mapping unit which portrays or attempts at least to portray the real bodies of soil that we find in the field. The limits of the polypedon, which is a taxonomic unit, are controlled by natural factors of soil formation. The limits of the mapping unit which attempts to portray polypedons or associations or complexes of polypedons, are controlled by another set of factors, namely the distribution and the size of the polypedons. Natural bodies that match the definition of polypedons are controlled by the same factors as the concept of the polypedons, but the limits of the mapping unit are controlled by another set of factors which include both the scale of the map that we are making and the purposes for which we are making the map. If the map is being made for very intensive land use, such as irrigated agriculture, we normally must use a larger scale and we must show the variations in the soil that are going to affect the use of a particular spot for irrigation. The same area being mapped for extensive use, as rangeland, is going to be made at a very much smaller scale and we would ignore differences that we are required to show on the map made for irrigation. The problem arises when we attempt to use the same name for a taxonomic unit and a cartographic unit. The concepts of the polypedon require the maker of the map to study the mapping unit and the kinds of soil that it includes rather carefully so that he knows something about the actual variability of soil properties within the area that he includes within a single map delineation. Having done this, he must decide in putting a name on the mapping delineation, on the kinds of variabilities he has and how these affect the use of the soil for the probable uses that he can foresee. Soil differences that change the classification of the mapping unit from one order to another, perhaps from Inceptisol to Mollisol because of a difference of a few centimeters in the thickness of the epipedon which changes it from ochric to mollic, may not be relevant to the use of the soil. If, both the soils with and without the mollic epipedon have exactly the same family modifiers in the family name, it is unlikely that this difference is going to be relevant to any particular use. Therefore, in selecting the name, the maker of the map may select whichever of these taxa are more extensive in the field. The user of the map is not particularly concerned with the taxonomy, he is concerned with the interpretations that he is furnished by the maker of the map. The important thing for the map maker is that he does not mislead the user of the map. If there are differences within the kinds of soil that are included within the map delineation that are significant to the prospective uses of the soil, the maker of the map in selecting his name must then consider the alternatives for names to reflect the presence of soils that behave in a significantly different manner. If the percentages are very small, he may either choose to neglect these in naming the unit or to indicate the locations of the contrasting soils with spot

symbols. Or if the variability is such that it affects management of the entire mapping unit he will probably choose to name the mapping unit as a complex or as an association so that the map user is warned that there are going to be specific problems in the use of that particular mapping delineation. In the U.S. there are certain conventions that are agreed upon for reconciling the differences between the conceptual soil taxa in the cartographic mapping units. These standards have changed in the past and will probably change again in the future. Soil surveys in other countries will find it necessary if there are many workers to agree upon some standards so that there may then be uniformity in the naming of the mapping units in the various surveys that are conducted concurrently. **Question 1, Leamy**

The use of the same name for the series as a mapping unit or an actual physical body of soil and the use of that name for the conceptual taxonomic unit bothers some people. We say Miami silt loam as a taxonomic unit is a conceptual thing, you can't put your hand on it, you can't feel it, you can't sample it; it is a pure conceptual taxonomic unit. When we make a map, we dig a hole or we clean off a road cut and we examine the soil that is there and if that fits the concept of Miami silt loam we are apt to say that this is Miami silt loam. This is quite another meaning. We really are saying this soil has all the properties of the concept of Miami silt loam, a taxonomic unit. When we have finished our soil survey, the correlator comes along and you have a map unit that is designated as #128 or something, and the correlator says this mapping unit is Miami silt loam. These are three different meanings of the same word and the same phrase. This does not bother me because in context we always know what is meant and it is not unusual in the English language and in several others; one word has more than one meaning. I don't think it would be wise to do away with the series category. It is too well entrenched in usage by the general public. They are not particularly confused by the identification of a given delineation as Miami silt loam. They don't even know the conceptual definition of Miami silt loam, their concern primarily is with the interpretations that we make of that mapping unit. I am completely at ease with the use of the same word or phrase with the three meanings that I have mentioned. It does not bother the users of the soil survey and normally it doesn't bother the people who are making the soil survey. **Question 68, Texas**

To some extent, at least, the soil series are considered a category in the taxonomy, and yet they are not defined in *Soil Taxonomy*; there are too many. The definitions of the series themselves take quite a few filing cases, instead of the one microfiche. You can, of course, microfiche the series definitions and descriptions, but the series has always been a pragmatic category. We establish series with narrow ranges of properties and with relatively broad ranges in properties, according to whether or not that definition lets us make the best interpretations that we can make to meet the needs of a particular soil survey. The only limits that are imposed on the series are those that have accumulated in the family and the higher categories, and the pedologist is free to subdivide that range into as many series as proven useful. This is related to one of the earlier questions very closely.

We did drop the type as a category and moved it into a phase position. Presumably the type was supposed to reflect the texture of the plow layer, or its equivalent in an undisturbed soil, but nationwide, the usage of the type name was quite variable. In Iowa, Sharpsburg silty clay loam has an argillic horizon with a silty clay texture. When eroded, the plow layer is normally a complex of silty clay loam and silty clay. To be strictly accurate, the map units should have been named Sharpsburg silty clay loam and silty clay, where the soils were eroded; but they did not do that in Iowa or Missouri. Under the influence of some previous correlator these soils were named according to what they thought the surface texture had been originally. In other parts of the country, an Ultisol with a sandy loam plow layer overlying a clayey argillic horizon would be named as a clay texture if erosion had removed the sandy loam surface. The argument there, was that you had to do this because you could not be sure what the original texture had been before erosion. So we get Cecil sandy loam and Cecil clay in the southern States.

If we were going to retain the type as a category, then we had to make a change in the map-naming processes where they thought they could identify what the texture had been before erosion and require them to complicate their map names by listing all the textures that occurred within the mapping unit. This did not seem to be a useful sort of exercise, so we simply moved the surface texture to a phase level where it could be shown when it was important or

disregarded if it was not important. If one wants to drop the series as a category, I suspect you will have to go the same route with the family and use a large number of complicated phase names for the families. Again, this does not seem to be a useful sort of exercise. The names are complicated enough by phases as it is, and the family names are not usually well received by farmers. They are useful to pedologists, but the farmer prefers a simpler name, and he is the one we are trying to help in the rural areas. In the urban-planning process, we are dealing with people who are trained in one or more technical disciplines and they can master the meaning of the family name without much trouble. But they would be bothered by all of the phase features that we would have to specify for the family in order to arrive at something comparable to the series. **Question 129, Cornell**

There are many areas that are covered by loess in the midwestern states where the same loess blankets the terraces of different levels and the uplands. In this situation *Soil Taxonomy* surely assumes that the soil properties are more important than the geomorphological history. If there is a probable difference in some particular behavior of the soil on the terrace and the soil on the upland, in that perhaps on the terrace a well is more apt to find water than in the upland, then *Soil Taxonomy* is very clear that this is an appropriate use of a soil phase. **Question 21, Leamy**

The subgroups are a little better defined than the phases to get uniformit among all the Arab countries. The soil map of ..e United States is an example of the legend design. There was a great deal of opposition at the time that it was developed. There was a feeling on the parts of some that, for a small-scale map, all of the map units should be identified at the same categoric level. It was possible to delineate on the Great Plains the Ustolls, but there would always be a mixture in the landscape of Haplustolls and Argiustolls because the map scale is small and the argillic horizon is restricted to stable landscape forms. Instead of just calling this Ustolls, we thought we could convey a good deal more information about these soils if we used associations of subgroups rather than associations of great groups. So when you examine that legend, you will find that we speak of aridic subgroups, typic subgroups, and udic subgroups, and they arrange themselves neatly into a pattern that can be shown on a scale of something like 1:5,000,000. This helps you visualize and understand the cropping patterns that you see on these relatively large areas. In the aridic subgroups the fields are kept in fallow one year out of two. In the typic subgroups the fields are cultivated and planted every year. In the udic subgroups there is a change in the kinds of crops that are grown. Your legend should be designed in terms so that the map that results will convey the maximum possible information. In some instances this may involve using associations of subgroups rather than great groups. **Question 131, Cornell**

In some parts of the world, the number of taxa that must be identified in the name of the delineation will decrease considerably as one goes from a low categoric level to a high one. I looked at one county in Kansas and every soil in the legend was classified as a Mollisol. So that using the order, one could have a relatively pure map unit defined as Mollisols, and in this county I think one could also have a similar purity if one referred to Ustolls or Udolls. I think the normal situation is that you have associations of different orders and that going to the order level does not eliminate the need to mention that you have Entisols, Inceptisols and Alfisols in the county.

With respect to the apparent complexity of the patterns of soils on a small-scale map, one could describe or enumerate the phases of all the families that occurred, but it would not be reflected in the map itself. The complexity would be in the identification in the field and the interpretations of potential uses for that area that is drawn on the small-scale map.

One can always make more statements about the soils identified at the lower categoric level. As one goes from a lower to a higher categoric level, there is more heterogeneity and there are fewer statements that can be made for a great group than for a subgroup or for a suborder than for a great group. The business here has something to do with the purpose you have for making the map. If one makes a map just to hang on the wall or fill up a drawer somewhere, it does not matter what statements you make. These maps are expensive, even at a small-scale, and one should know clearly what he is doing and then design his nomenclature to bring out what is needed for the purpose of making that particular map. **Question 132, Cornell**

One would normally assume that the limits that are significant in the U.S. will have relevance in other parts of the world where conditions are similar. Obviously, what is relevant in New York state is not necessarily the same in Nevada. But the relevance of the differentiae used for series in New York state should not differ greatly from situations in Belgium or northern France. **Question 122, Cornell**

At one time, when I was making soil surveys in Illinois in an area covered by relatively thick loess, I thought that the limits of the series should be set pretty much by the nature of variability within a delineation. When I went from the thick loess which overlays Illinoian till to thick loess over a marine area of Wisconsin age, I had the same soil units in the landscape but what you could draw a boundary around was completely different. The series, defined to fit the landscape in loess-covered Illinoian drift area, were not mappable in the loess-covered Wisconsin drift area. If one is going to try to define the limits by the variability that you can draw boundaries around, one is in serious trouble. The series that fit in one place will not fit into another. A series has to have some defined range which may be readily mappable in one landscape and may only be able to be shown as a complex in another landscape.

If you try to define the series on the basis of what can be mapped, then you are going to have large overlaps between the series. This has been a theory that series should define mutually exclusive soils. They should be readily distinguishable in the field and the range should be something in the neighborhood of what the fieldman can identify while mapping. There are natural soil boundaries in the field; the boundaries between two series are very commonly boundaries between two families as well. To the best of my knowledge, there has never been a requirement, that a soil series be mappable under all conditions. They may be mappable in one place and occur only as complexes in another. The series were the concepts of taxa that predated *Soil Taxonomy* and the term polypedon.

I cannot see a good alternative to the concept of polypedon in large-scale soil surveys. In small-scale surveys, the concept is hardly relevant to the design of the map units. We are able to map polypedons without too much difficulty. In glacial till areas, variability within a delineation is generally a little greater than in loess because of the variability of the parent material--in texture, compaction etc. **Questions 123 and 124, Cornell**

In southern Illinois, where the loess is relatively thin, on the Illinoian till which is strongly weathered and relatively impermeable, we have many small spots with natric horizons. The scale of the polypedon on these is about $3m^2$ or a little more. To show these on maps requires a scale of about 1:1000 or larger. So, on what we would consider as large-scale maps, these have to be shown as complexes and one designs for his legend a series of complexes according to the percentage of the natric horizon in the delineations--it may be 10% or 75% of sodium-affected soils. Where the loess is thicker, the sodium-affected soils are not found in the landscape.

I believe that the statistical studies of composition of mapping units are valid. This illustrates a problem of natural variability within areas that can be delineated at the scale in which the maps are made. We have had some working rules covering the naming of the map units. This is what we are dealing with here. We have situations where the variability is small but does cross a family boundary, and so we have soils that have very similar behavior occurring in two families in varying proportions within one delineation or another in a soil survey.

The problem here is one of putting a name on the map unit, not so much as one of trying to purify the map units. One can get such a complex map, that even a trained pedologist cannot use it. With experience, we learned that instead of gaining anything, we lose in attempts to be extremely pure. The way in which the map units are named has varied over time and probably will continue to do so in the future, but at the time I retired we had a general understanding that one could name a map unit for the most abundant, most extensive taxon or series within that unit. It might not represent even half of the area that was delineated, but if it had a larger area than any other single kind of soil, we would go ahead and use the series name for that map unit.

In designing *Soil Taxonomy*, virtually no consideration was given to the mappability of the taxa. We were dealing with groupings of series whose mappability has been tested, and we felt no real concern for the mappability of families or higher categories. With the emphasis given to soil climate, some of the higher categories will obviously be mappable because the climate changes slowly over distance. **Question 121, Cornell**

1.5 Logic of Classification

As we pointed out in the *7th Approximation* (Soil Survey Staff, 1960), taxonomies are devices of men made for specific purposes, not truths that we have discovered. This is one general rule of logic and of classification. It has been recognized in the biological taxonomies starting with Linnaeus. In the Linnaean taxonomy of plants and animals, the principal kind of plant or animal, was the species. Linnaeus said that a botanist must know and remember every genus. John Stewart Mill (1891) pointed out, that objects had to be classified for a purpose and that if there were different purposes there could be several classifications or taxonomies; he called them scientific classifications, of the same objects depending on the purpose. Mill said that the best classification was the one that permitted the largest number of the most important statements about a given class of objects.

There is a distinction between the taxonomies of plants and animals on the one hand and soils on the other. The taxonomies of living organisms have in the past generally been built on the phylogenetic principles, namely that of descent. When we try to classify soils, then there are no principles of descent. There are no common ancestors of soils. They are not living organisms; they are as we all know, on the borderline area between the biological and earth sciences. But the logical principles of John Stewart Mill, that the best taxonomy or scientific classification is the one that permits the greatest number of the most important statements about the objects that have been classified still applies. In soil science these important statements are our interpretations, not our theory of genesis. Therefore in order to be able to make any statements of any sort about the classification or taxonomy of soils it is necessary to use the same criteria at different categories. The biological taxonomist makes very little, if any, use of the orders in the classes themselves; he is concerned with the species.

The pedologists who must make soil surveys at varying scales, makes principal use of the higher categories with small-scale maps and principal use of the lower categories with large-scale maps. In order to be able to make any statements whatever about the different orders, that can be used with extremely small-scale maps, the most important characteristics to the use of the soils must be used at the order level. With somewhat larger small-scale maps, the map units may be defined in terms of suborders or great groups (always in terms of phases of course). We may want to make somewhat smaller distinctions in the characteristics that we have used between the orders. When we get to the level of great groups or subgroups used in somewhat larger-scale maps, the same characteristics that we have used at the order level may or may not be important but we use them when they are important, important in the sense that they permit us to make statements about the classes that we are showing on our maps. In the detailed soil maps we must have all of the accumulated differences to the maximum extent possible, of features or properties such as base saturation, soil moisture, soil temperature. Many, many properties are used throughout the taxonomy. The general rule is that we make the least subdivisions of properties at the highest categories and the maximum subdivisions of properties in the lowest categories because the lowest categories are only used in the large-scale maps. The biologists who do not make maps at different scales are not concerned with this particular problem.

The problem is that cited by John Stewart Mill, that **the best taxonomy is that one that permits one to make the most statements about the most important properties of the units that have been classified. It is one of the strengths of *Soil Taxonomy* not the weaknesses that we use the same properties in different categories.** The fewest subdivisions of a given property are used in the higher categories. The largest number of subdivisions of the property are used in the lowest categories. This is completely in agreement with the logic of John Stewart Mill or

any other taxonomist I know. If the pedologist were to read the modern literature of the taxonomy of plants or animals, just a few books, he would find statements such as the one I would cite from Cain, that if the botanist or zoologist were faced with the problems of classifying plants or animals according to variability over both time and space, he would find the present system intolerable. The variability over space of a species of animals is something that can be observed today. The variability over time is something that depends on the fossil record and is not only imperfect but often completely absent. In soils we must deal with this variability over time and over space. This is, therefore, a major difference between taxonomy of soils and taxonomy of living organisms. We, therefore, must deal with the taxonomy of soils in a somewhat different manner than do the botanists or zoologists dealing with their taxonomies. Actually, Cain has suggested that it would be better to drop the system completely and devise a new system but he says because of the priority of nomenclature, we would get into so many troubles that it is probably not worthwhile. When we started to develop *Soil Taxonomy* we decided that if we were going to make a break with the past we had best make it completely at this moment that we published *Soil Taxonomy*.

Perhaps the root of the confusion about whether or not a taxon is a concept or a real thing lies in the custom we have of using the same name for a taxon such as Miami loam and using that name also for a unit on a map legend where we portray or try to portray the aerial extent of the soils that conform to our concepts of Miami loam. We are using the same name with two different meanings. The third one is that if we examine a pit and find a pedon that conforms to our concept of Miami loam, we say this is Miami loam. It is not Miami loam. Miami loam is a concept not a real thing but we call it Miami loam because it has all the properties that correspond with our concept of Miami loam. If Miami loam as a taxon was a real thing it would be impossible to change our concept of it because it would be fixed by nature. Therefore, the original concept of Miami loam, was that of a soil developed in glacial materials, glacial till, glacial outwash. We could not have subdivided Miami loam into something like the hundred or so soil series that were once called Miami loam.

There is no particular difference between the taxonomies of soils and the living organisms. One can take a particular plant and dry it and put it in an herbarium as a type sample of a particular species. However, the name of a species is binomial, it requires the name of the genus and of the particular individuals that constitute a class within the genus. However, one may not put a genus in an herbarium. That is impossible. The genus is obviously a concept or the botanist would not occasionally revise their classification and establish new genera. Many pedologists have had one introductory course in botany in which they had four or five lectures concerning taxonomy of plants. Everything, therefore, in plant taxonomy seems simple to them but the problems in plant taxonomy are probably more difficult than those in soil taxonomy if you start to delve into the literature. **Question 43, Leamy**

It might be possible to develop a table to portray the logic of *Soil Taxonomy* although this has not been attempted. We have discussed the logic of *Soil Taxonomy* in several places. Marlin Cline has discussed it in several papers (Cline, 1949, 1961, 1975). I have at least two papers in which the logic is discussed, in my lectures on soil classification (Smith, 1965) and in another paper (reference to be added later). Cline (1949) has pointed out that if the full reasons for this selection of a particular differentia are given, then the users are inclined to pay attention to those reasons which involve assumptions about soil genesis, and so more to the genetic assumptions than to the definitions. If he does so, it blinds him to the possibility that there are errors in these assumptions and it tends to freeze the taxonomy in a form that is not as good as it should be. Cain has pointed out that, in the botanical and in the zoological taxonomies there are the phylogenetic taxonomies in which there is an assumption that a particular character that distinguishes a family arises only once in the course of evolution and the fewer the similarities they find between plants or animals, the older this characteristic arose. This is just an assumption he goes on to say and may not be correct. It may have arisen independently at different times but the phylogenetic classification makes this other assumption and therefore it blinds the taxonomists in biology to this assumption which may or may not be correct and tends to freeze the taxonomy in an imperfect form. Therefore, in the development of *Soil Taxonomy* we carefully hid most of our assumptions about the genesis of the various diagnostic properties that we have used in classifying the soil. This was hidden to prevent the freezing of the taxonomy into a sterile system based on some genetic assumptions that might or

might not be correct. Whether or not the table could be developed that reflected all of these assumptions cannot be seen until someone tries to do it. However, the full intent is that this shall not be done so that the future taxonomists will not pay more attention to the faulty assumptions that we make today than to the definitions. The definitions if they do not work can be corrected. The assumptions if they do not work are more difficult to correct. **Question 9, Leamy**

I have mentioned elsewhere the fact that classification involves more than the sole application of the rules of taxonomy to the classification of the particular soil. It also involves the consideration of whether or not the soils that come together under the definitions of *Soil Taxonomy* are soils that belong together and that the soils that belong together are the soils that have similar properties and similar behavior. The similar behavior may apply to one use but not to another and if there are significant differences in any particular use, then at some categoric level, soils do not belong together. Now at the subgroup level we have in most definitions that the typic subgroup does or does not have properties *a, b, c, d*, etc. If a soil is like the typic except for *a*, it belongs in an aquic subgroup and generally if it is like the typic except for *b*, it belongs in another subgroup. We have provided a subgroup for the soils like *a* with or without *b* or the soils like the typic except for *b* with or without *a* except, of course, these definitions are not mutually exclusive. But such a soil might be found in another country and no subgroup is provided. It is like the typic except for *a and b* and *Soil Taxonomy* only provides for those that are like the typic except for one or the other, not for both. At this point then, the classifier is faced with a problem that he must create or propose a new subgroup or he must propose a modification of the definition of the subgroups already existing in *Soil Taxonomy*. In making his decision on what to do about this subgroup that is not provided for, he must go back to the general principle that one classifies the soils where they belong and this classification is based on the soil behavior rather than on the properties. There may be to some, an apparent contradiction in what I have just stated. Some soils in different orders do not behave differently in any significant manner. The soils form a continuum and the soils on one side of a limit and the soils on the other side of the limit, just barely do not have significantly different behavior. So that we have, for soils in one great group and in another great group, no important difference in interpretation. If both are close to the limits that are given in *Soil Taxonomy*, here is a problem where the classifier must use some judgment and should propose some general sort of rule for application in other countries. *Soil Taxonomy* will not be useful internationally if there are no internationally agreed-upon definitions of the taxa. **Question 6, Leamy**

You ask about the influence of private conversations and of other individuals on the development of *Soil Taxonomy*. First, closed-doors conversations were too lengthy to put into this record. I think I was really more influenced by my reading and my field experience than I was by an individual, although admittedly many individuals in our discussions have had appreciable influence on my thinking but I couldn't pick out one name and say he's the one.

Both John Stuart Mill and Bridgeman had a very large effect on me. The other books on logic that I read, I returned to the library. But Bridgeman and John Stuart Mill I got for my own library. They had an enormous impact on me in the development of *Soil Taxonomy*. **Question 175, Minnesota**

1.6 Basic Principles of Soil Taxonomy

The philosophy of *Soil Taxonomy* is that a soil should be classified on its own properties, and not on those that are presumed to have existed at some time in the past, and not on the properties of adjacent soils. The use of the mollic epipedon to group the grassland soils of the great plains was unavoidable with the knowledge that we had of those soils at the time we developed *Soil Taxonomy*. We did state that we preferred to use subsurface horizons for the definitions of the higher categories because these would be the last horizons to be removed by erosion. There was, however, no criterion that we could find to retain the grouping that existed in the previous classification for these dark-colored soils of the subhumid and humid grasslands.

The possible alternative would be to find some characteristic that was common to Mollisols and was not found in other orders besides the mollic epipedons. I do not know what this might be. An alternative approach might be to recall that we are not classifying pedons, but we are classifying polypedons. The pedon is merely a sampling unit of the polypedon. The vast bulk of the eroded areas of Mollisols will have a mollic epipedon as well as pedons that do not have a mollic epipedon. In classifying these soils as Mollisols, when the mollic epipedon has been removed in places, perhaps most places even, it might be possible to write definitions such that when applied to a polypedon, the presence of these less eroded areas would be considered justification for putting the soil into the Mollisol order. This will require some study in the field, and there was no time to do this while *Soil Taxonomy* was being written. This question has been bothering the soil scientists of the midwestern states for many years, and we attempted at one time to get a study in Iowa of these soils with statistical controls, and somehow or other we never were able to find funds and personnel to do it. **Question 11, Witty & Guthrie**

Why are there only 10 orders? We wanted to hold the orders to a minimum. I might elaborate just a little bit. In comparison to the Russian system, where the soils types are not organized into any higher category one has to then compare all the kinds of soil. The last count I had was 117 soils types of the plains, and there must be at least an equal number in the mountains. This does not facilitate identification, because 117 times 2 is too many soil types to keep in mind. The FAO-UNESCO legend for their soil map of the world recognizes about 23 major kinds of soil, most of which are subdivided on the legend. This is still quite a few to keep clearly in mind for rapid identification. It requires more or less constant checking of the definitions. Fifteen I think one can manage without too much trouble, but when it gets above 20 the normal mind is in trouble in carrying everything in mind without checking against the legends.

We need a multicategoric system of taxonomy, because we make soil maps at varying scales, and because we need a taxonomy that converts into a key for purposes of identifying the classification of a particular series. We have some 12,000 series we must keep track of. For purposes of the soil survey these series must be correlated and grouped in such a way that we can make the largest number of the most important statements about the series grouped in a particular taxon. To do this we must then start at the highest category to trace down the individual series and the related series with which we are concerned. The categories themselves really serve no other purposes, and there was no intent to have specific purposes for specific taxa above the family level. The family level was intended to be useful for making our major interpretations concerning use for growing plants or for engineering purposes.

The series category is intended to permit the most precise quantitative interpretations that our current knowledge permits. But above the series there are no particular specific purposes. The subgroup level is intended to show relations between soils in a given great group. The typic subgroup on the one hand which is our central concept but not necessarily the most extensive soil, the intergrades which share properties with other great groups and the extragrades which have properties which are not characteristics or typical of any other kind of soil. At the great group level, the subgroup, the order, the suborder, the purposes are to permit generalizations in small-scale maps and to assist us in the identification of a particular kind of soil. **Question 24, Cornell**

It appeals to a great many people to use one property in one category throughout the system. However, this leads to an enormous multiplication in the number of categories that we must form. You cannot, for example, distinguish the Histosols on the basis of clay mineralogy. Unless they have clay minerals you may not use mineralogy in soils that are organic in nature. This would be one example. It requires then a whole series of categories for the Histosols. We make soils maps at different scales for many purposes. Some maps are made at very small scales, some are made at large scales. For the small-scale maps it is desirable to use some parameters with very broad definitions, as for example, the soil moisture regimes--udic, ustic, xeric, aridic. For the large-scale map this is inadequate because we must make subdivisions of these broad classes of moisture regimes in order to make reasonable interpretations at the family level. So we cannot make all of our classes apply to the very broad map units of small-scale maps, and so we must use broader groupings. For the large-scale map where we are concerned with a specific field on a specific farm, to make the most precise interpretations possible, we

have to recognize small differences in the moisture regime. Therefore, it is necessary to use the same characteristics at more than one level in the taxonomy, or we must abandon the notion of making maps at different scales. **Question 9, Cornell**

The idea of using limits with operational definitions was implicit in *Soil Taxonomy* from the start. We had enough experience in trying to improve the 1938 classification with the old type of definition that it was decided, or I decided anyhow, that if I worked on the development of this classification we would not write our definitions in that manner. To an extent, we have retained the modal-individual in that we have typic subgroups of great groups which represent our central concept of that great group, but not defined in terms of a named series, rather defined in terms of a group of properties, presence or absence of various horizons and diagnostic features. The soil series really was not my business in developing *Taxonomy*. They were useful in deciding on how the definitions should be written in higher categories because we have the series and the assembled interpretations for the series to check on how our definitions worked. You'll have to ask the correlation staff about why they want a typifying pedon for a series because that's their business and never was mine.

The central concept of the typic subgroup is in no way a statistical mode. The typic subgroup, we must remember, is selected for convenience in naming intergrades to other kinds of soil, frequently it is not the most extensive kind of soil in a great group, but may represent only a small part of the great group. In the Cryochrepts, the typic subgroup does not have permafrost but the most common Cryochrepts in North America and in Russia seem to have permafrost. But in naming the subgroups, if we set our typic concept on a Cryochrept with permafrost we could not find a convenient name for the ones that lack the permafrost. It was much easier then to select the soil without permafrost as a typic concept and then have pergelic subgroups. This permits us to have a flexibility in our classification in that we may speak of *pergelic soils* and include all the soils that have permafrost. We can think of all of them at one moment if we speak of *pergelic soils*. There is no one hierarchy that serves all our purposes equally well and we have to have flexibility in the hierarchy and be able to set up *ad hoc* orders, such as *fragic soils*, called so for their fragipans, *duric soils*, called so for their duripans, and so on. And if we want the traditional European order of soils we can speak of aquic suborders and that makes a new order of all of the aquic soils other than the Histosols. **Question 104, Minnesota**

When we started on the development of *Soil Taxonomy*, a good many of the correlation staff thought that we should, as the botanists do, define our great groups or other taxa by type. Rustin and Norfolk were typed Red-Yellow Podzolic soils; Miami was typed Gray-Brown Podzolic soils, and Marshall, I suppose, the type Prairie Soils and so on. Then we would analyze these type theories and there would be no limits between the taxa, they would be the ones most closely related to that particular type that would be grouped in that taxonomy. This is an appealing way to define things but it leads to enormous difficulties of application unless you are going to run all of your decisions through one person. We found this was impossible. Our correlation process failed to keep up with our mapping when everything had to go through the office of the principal director of soil correlation. We had many arguments, between Marlin Cline and myself, about what he called "building fences". He says, in his *Agronomy Monograph* article, or in some of his papers, that a class is formed by ties from within not by limits from without. Now I used several times the illustration of Gray-Brown and Red-Yellow Podzolic soils prior to *Soil Taxonomy*. We had a field correlation trip between the northeastern and the southern states in Virginia and Maryland and we came on the Chester series in Maryland. The Chester series resembles the type Red-Yellow Podzolic soils in mineralogy, in base saturation. It resembled the Gray-Brown Podzolic soils in the thickness of the solum, and the colors. The correlators from the south said "that's a Gray-Brown Podzolic soil", the correlators from the north said "that's a Red-Yellow Podzolic soil" and we never could resolve the issue. It resembled one in one respect and the other in another respect. Which one to give priority? So it seemed to me that I preferred the logic of Bridgeman, and that's the logic of modern physics, how to write definitions. He was the first one to propose what became the "operational definition". You write your definition in terms of the operations you go through to reach your decision. This could be, then, something that could be applied uniformly by a great many people instead of going through a single mind. This was the rationale behind using limits to taxa instead of the central concept. That's the answer I would have given.

This taxa specimen in botany gets me in just as much trouble as taxa specimen in pedology. **Question 34, Minnesota**

When we started to develop *Soil Taxonomy* and the first approximations came out, the correlators and the state people complained bitterly that this approximation was splitting series wholesale. They wanted to keep the series as nearly as possible as they had been conceived. They were willing to split a series if, when they examined the split, they saw that it would improve their interpretations, but otherwise they wanted to retain the series uniquely. The series had been used for sixty years or more and people had become familiar with them. Highway engineers were using them; tax assessors were using them. When you saw an advertisement for a farm for sale in the Des Moines paper, it generally said, a hundred and sixty acres Carrington loam identifying the soils incorrectly, in most cases, and sometimes rightly according to the public soil survey at least. The problem was then to split the temperature continuum without splitting series and for a time it was acceptable in correlation to the Director of Classification and Correlation to change series when you crossed a major land use boundary. From one major land use area to another you could have very similar series but you were making different interpretations.

Soil Taxonomy was intended to be comprehensive or to be modified so that it would be comprehensive with a minimum of disturbance. Obviously, we cannot or should not classify a soil about which we know nothing. We should not prejudice the classification by providing for every possible contingency.

There are gaps in *Soil Taxonomy*. You cannot, for example, classify an arid soil in a polar region. It does not meet the definition of Aridisols because the temperature never gets up to 5° C, so it is not dry more than half the time that the temperature is above 5° C at 50 cm depth. That does not occur, so it cannot be an Aridisol, and yet it will not fit into any other order, and we specifically said that we simply did not know enough about these soils to propose a classification. At the subgroup level, where we have typic subgroups, the definition specifies a number of properties that are required of the typic subgroup. The intergrades and extragrades then, are soils that have some one or more aberrant properties relative to the typic, but the only subgroups we defined in *Soil Taxonomy* were those that were known to occur in the U.S. We had series that fitted into a particular subgroup. We defined that subgroup and discussed it briefly in the text. A few subgroups that are not known to occur in the U.S. were included if we had a specific request from some other part of the world to provide such a subgroup. There are many implied subgroups. For example, you have a soil that is like the typic except for *a*. And you have another one that is like the typic except for *b*. If you find a soil that is like the typic except for *a and b* this is an implied subgroup, but does not mean that you must have it. You must examine the nature of the soils that are like the typic *except for a and b*, and compare them with the other two subgroups. It is quite possible that the one that is like the typic *except for b* should be defined as like the typic *for b with or without a*, and unless there are some significant interpretive differences between these, we should modify the definition of the soil that is like the typic *except for b*. So that we do not have three subgroups when two will serve our needs. **Question 52, Cornell**

When should a new subgroup be recognized? The answer to that in my judgment would be as follows: that if at the family level phases of family interpretation, there are no significant differences between the proposed implied subgroup and the established subgroup, then the definition of the present subgroup should be expanded to include both. If, however, there are significant differences of interpretation of phases of family of the proposed subgroup and of the established subgroup, then I think that we should recognize a new subgroup rather than expand the definition of the old one. The whole thing hinges on the interpretation of the family. If you need two families because the interpretations differ, then you must have two subgroups in order to be sure that you have the two families. Now, as for the extent of minimum acreage, you are going to have some difficult decisions to make from abroad at least, where the man who proposes the subgroup has been working in an area without a soil survey, without a detailed soil survey, then he will not know what the acreage might be. And if the acreage is very minor, you can handle this distinction by phases. But if the area is considerable and the differences are important, you may want to establish the new subgroup, even for a smaller acreage, because the phases can get too complicated for the user of the survey to

understand. You may not have a dozen phases, different kinds of modifiers to the series name as a phase or the family name as a phase, and understand what has been done, because one of the reasons that we introduced moisture and temperature into the taxonomy was to simplify the matter of naming of phases. Too many phases are very bad in your legend. It can get too long and too complicated for the understanding of the nature of the map unit. **Question 1, Witty & Guthrie**

We do not specify a central concept consistently for any taxa except the typic subgroup which is considered a central concept of the great group. We have no basis for specifying a central concept of a suborder or an order. The mappers in this country in describing and defining a series, normally try to specify a central concept of the series and the permitted range in properties as they deviate from the series.

It is rather difficult for me to imagine the central concept of a family or of an order. The properties are too few. But, we do have the typic subgroup which represents, pretty much, the central concept of a great group though it is not necessarily the most extensive. There is confusion amongst people on this point. The world soil map of FAO and UNESCO is enormously biased by the aerial extent of kind of soil. With their map scale, a soil has to be very extensive before it can show up in the legend. Minor kinds of soil that would be extremely important on a given farm have no place to go in the legend because they are only dealing with the very extensive soils. It would be a little bit like deciding that the ants should be recognized as a separate kingdom because there are so many of them in the world. **Question 169, Cornell**

It has been suggested that properties of surface soil horizons be used as soil family criteria to enhance interpretive values. But no, I see no way that can be done economically. The physical, chemical properties of the plow layer, admittedly are critical to the growth of plants, and yet they can vary enormously from one system of management to another on what is essentially the same kind of soil You will see field boundaries in which the growth of the vegetation on one side of the fence is enormously different from that on the other side of the fence, and yet the kinds of soil along that fence line may be very similar. If the man with the poor crops changes his management to the same as that of the man with the good crops, in the course of time, generally a few years, there will be no difference along that fence line. The poor physical or chemical properties that stunted the crops of the man with poor management will have disappeared and you will have good chemical and physical properties on both sides of the fence. To build this in to the taxonomy is difficult. It is readily changed by the death of an owner or the sale of a farm, to bring in a new manager with higher managerial skills. That means you have to go back and remap every few years, and it is much better to have a stable taxonomy and to make your interpretations according to the level of management and the properties which will exist under different levels of management. The Russians do this in their mapping of the state and collective farms at the phase level. But in that situation they have firm control over the management system, whereas in this country this is a matter for private enterprise, and a man can ruin his farm or build it up if he sees fit. **Question 147, Cornell**

We use diagnostic horizons in *Soil Taxonomy* rather than the traditional letter designations because the letter designations of A, B, and C to which we add suffixes like t, or e, or what have you, make it impossible to avoid the use of A, B, and C considerations in the taxonomy. We found it necessary to get away from designating a given horizon as B or C by substituting the definition of, say, the oxic horizon. There is a second problem, here, in that a Bt horizon may not be an argillic horizon. The Bt horizon nomenclature is a designation that is placed on the horizon when the man describing the soil makes his interpretation. Certainly, in the sand with very thin lamellae, I would use in my description Bt for the lamellae, but it may not necessarily constitute an argillic horizon. There may be too few lamellae and they may be too thin. The designation Bt, then would make no distinction between a Psammentic Hapludalf and an Alfic Udipsamment because they both have lamellae, and they both have Bt's. In the Alfisol the lamellae are thicker and more frequent, and in the Psamment the lamellae are present but they are very thin and very few. **Question 147, Texas**

The problem of when to establish a new series or to use a phase of an existing series has been with us for many decades. The Office of Soil Correlation in Washington has really not

been very helpful in establishing guidelines. It was impossible to deal at any length with the series category in *Soil Taxonomy* because there were too many thousands of them, and ones that only include a few examples of families with the descriptions and data on the series in that family, and to analyze then the differences that had been used to justify series separations, that was about as far as it was possible to go in *Soil Taxonomy*. If one had the time and the information on the serie. in an appreciable number of families instead of two or three, I think one might be able to generalize to some extent on what should be used as phase criteria and what should be used as series criteria. It is obvious that in the thermic zone as well as in the mesic zone, differences in soil temperature than those recognized in the family levels would be useful to make interpretations about crop yields. It is possible then, to either phase this or to set up series. The series limits, if they were established, say within the thermic range or within the mesic range, might be valid at the moment that one established the series, but given a few years the plant breeders are going to produce varieties that will make those limits inappropriate. It would be my judgment that it would be better on temperature to phase the subdivisions within the thermic or mesic zone than to make series distinctions, assuming then that within the soils themselves one doesn't find any other difference than temperature. I cannot possibly generalize on this today except to warn against building into the taxonomy differentiae that will become invalid when another crop variety is produced. **Question 114, Texas**

You'll never find in defining your temperature regimes, that you will be able to set any one limit that will fit all crops. It would be necessary for interpretations then in terms of *Taxonomy* to have phases of temperature in addition to the isohyperthermic regime, put in a phase of mean annual temperature of 28° F, or 28° F to 35° F, or what have you. It is legitimate for your interpretations, if necessary, to have finer subdivisions for specific crops than we have in *Soil Taxonomy*. All that we have here is a general grouping of temperatures that we could not test in the U.S. We have the International Committee working on this problem at the moment in Venezuela. They are unhappy about the present temperature limits of the isohyperthermic. They want to subdivide that to show the extremely warm ones from the moderately warm ones. I would say there is nothing we can do but wait for the international committee to discuss and debate the problems and make recommendations for changes.

There is a great deal of unhappiness in Iowa about the classification of soils that they think used to have a mollic epipedon but have now lost it. These Mollisols have been changed to Inceptisols for the moment. I think the correlation staff has dealt with this by classifying the Inceptisols and eroded Mollisols and retaining the old series names. I believe that is what they have actually done, although, it is some violation of the principle of *Soil Taxonomy* that they are classifying soils not on the basis of their properties, but on the basis of what they think this property used to be. This was certainly one of the most bitterly debated points about the early approximations of *Soil Taxonomy*. Correlators did not want to classify the soils on their own properties. They preferred to be able to classify them on the basis of properties that they thought they used to have. Now for an international or general system this leads to enormous complications because in the U.S. you have the date when white settlers first came. This was the practice before this to classify the soils on the basis of what they thought was the virgin profile. In some parts of the world you have no cut off date. In Western Europe, for example, the soils have been cultivated for... we don't know how many thousands but several thousands of years most of which time there were no fertilizers, and the cultivators used to bring the litter in from the forest to put in the stable and the soils were depleted and became acid and the heather vegetation took over.

Soils that had been what they called Brown Forest soils that are now Dystrochrepts became Spodosols. So what date then are you going to use if you are going to reconstruct the virgin profile, because a spodic horizon formed under man's influence and there are other soils that have been receiving sediments under irrigation or under cultivation from flooding that never had a native vegetation. The only vegetation they ever had was the crops that the farmers grew, and so what sort of cut-off date do you use on those soils, to say this is what it used to be? You have to fix a date. Once you fix it, it is impossible to use it because you are not that precise. These soils that have changed under cultivation, I think, have to be classified on the basis of their present properties, and not on the basis of what somebody thinks they used to be like at some earlier time when there was no one around for recording properties. **Question 73, Texas**

If we were not trying to devise a classification that helps us with our soil survey purposes, I would see no conflict in bringing the andic subgroups into the same highest category with the Andepts. If you want to select some other purpose than the soil survey then it would be perfectly logical to keep them all together, though your mantle was 5 cm in one and 5 m in another. You could do that, if your purpose was to show the presence or absence of pyroclastic weathering products. I do not think that even Dr. Segalen has gone quite that far, however, in his proposed classification - according to the material composition of the materials, using that at the highest category, using presence or absence of horizons at lower categories. I think he would not take into account that 5 cm. We do not dare do it, because once it is plowed, you can no longer identify it in the field.

The problem is, first it is identification, but what is more relevant is that the plants, particularly the annuals, are more sensitive to 5 or 10 cm of ash sitting on the soil surface in comparison to an aquic subgroup property, which is influencing at a much deeper level; that bothers the roots. They die when it becomes anaerobic. But the surface 5 or 10 cm or even 15 cm, these properties are reserved to the phase level deliberately because the management of the soil has so much influence on the nature of the physical and the chemical properties. It was the intent that we would not change the classification of a soil as a result of plowing a few times to a normal plow depth. But for the use of the soil survey, I think this was a correct decision. **Question 93, Cornell**

Soils with pergelic temperature regimes are subject to many of the same turbations that we have in Vertisols and the possibility of having an order of you might say, "Turbosols", was discussed seriously, but this involves two very unlike things. The one where the turbation is due to frost and other where the turbation is due to the amount and nature of the clay, so we were grouping two rather unlike things on the basis of a single process, that of mixing of the soil by freezing and thawing in one case, and by shrinking and swelling in another. So, while we considered that seriously, we rejected it on those grounds--that just the turbation wasn't quite enough--that we had very unlike things put together.

You must notice that one's attitude toward which classes we should recognize, which ones should be combined or kept separate -- we are enormously influenced by our personal experience and by the geographic extent of the kind of soils involved. This is one of the basic difficulties with the FAO-UNESCO legend -- that only extensive soils can be handled in the classification. The inextensive ones, that may be extremely important on a particular farm, have no place to go except if they put it in someplace, it's the wrong place, because it doesn't behave like the other soils in that legend. **Question 53, Texas**

The arrangement of the taxa in the keys is primarily for the convenience of the user who wishes to identify a particular kind of soil. For example, if in the particular taxon we have soils with fragipans and all the soils with fragipans are placed in a particular great group, this great group would then be listed first in the key because if the soil has a fragipan, it automatically goes into that particular great group, irrespective of any other properties it might have. So the first position in the key is normally one that includes all soils in that taxon having a particular diagnostic horizon or property. There is no particular significance to the arrangement within the key other than that it is designed to simplify the identification of a particular kind of soil. As an example, I might site the amendment to *Soil Taxonomy* establishing a new great group of Fragixeralfs. The key had to be rearranged because all of the soils having a fragipan amongst the Xeralfs were grouped into the great group of Fragixeralfs. It was assumed in the key that the soil would not have both a fragipan and a duripan, but the order is intended to simplify the use of the key. **Question 23, Venezuela**

1.7 Methodology Used to Develop Soil Taxonomy

Starting in 1900, approximately, we began to build up a group of soil series which were defined with varying rigor at varying periods of time. But these soil series and types were the basis for the published soil surveys, and they had a good deal of actual testing in the field.

People became familiar with them, and they used them. At the same general period of time, beginning about 1920 in this case, Marbut introduced the concept of the Russian soil type, or as it became known here, the great soil group. Marbut's final publication, the *Atlas of American Agriculture*, gave his great soil groups that he recognized at that time and gave an example of 1 or 2 series for each of his great soil groups, but he was never able to arrange his series into the great soil groups. The great soil groups were continued in the *1938 Yearbook of Agriculture, Soils and Men*. Dr. Kellogg has often explained to me the problems they faced, that they had only one year to devise a new system, because they recognized the imperfections of Marbut's system and they could not be made to work. In that one year, they devised a number of descriptions of great soil groups, including summary arrangement of Marbut's great soil groups into suborders and orders. So we had, beginning about 1920 and running up until World War II, two systems of classification of soils. One was into soil series and types and the other was into great soil groups. These two systems have never been meshed. They were completely separate, one from the other.

At the end of WWII, Kellogg put his senior staff to work to develop better definitions of the great soil groups. He had a committee on Red-Yellow Podzolic soils and one on Gray-Brown Podzolic soils and one on Planosols. Members of these committees were almost invariably people who had worked with these particular kinds of soils. They tended to write some rather narrow definitions, so that for the most part, there were large gaps between the definitions of one great soil group and another that was bordering it.

The 1938 classification also had the defect that at the highest category soils were grouped into three classes, zonal, azonal, and intrazonal. We could not discover any common characteristic of the zonal soils that were not also shared by any of the intrazonal soils. We decided that it would be best to abandon the concept of zonality as a differentia in the taxonomy of our soils and find something to take its place.

We had quite a few thousands of series at this moment -- the exact number escapes me -- but it was something like 5 or 6 thousand. It was too many series for anyone to comprehend. While there were long arguments about the importance of grouping the series into successively higher categories, no one knew the series well enough to do this. It was necessary, then, to find some sort of a differentia or some groups of differentiae, for the higher categories, and to test those by seeing how the series fell in the definitions that had been proposed. We had the link between the great soil group and the series that was missing. We had no criteria in mind when we started to arrange the categories between the series and the great soil group. But we had been having discussions for many years about the intergrades between one great soil group and another -- soils that shared some characteristics of another or several other great soil groups. This seemed to be a logical basis for defining the subgroups. We still needed a link between the subgroups and the series. It was Dr. Allaway who made the suggestion that, at the subgroup level, we pretty well have taken care of all the genetic factors that concern us, so why don't we, at the family level, take into account the practical physical factors that affect the growth of plants and the engineering use of soils. We tested several concepts, beginning with the *Third Approximation*.

It was the beginning of the *Third Approximation* that we proposed the use of the physical properties that affected plant growth and engineering uses of soils. We tested a number of definitions by examining the groupings of series that resulted from the use of those definitions. These were modified rather substantially in the *Fourth* and *Fifth Approximations* as a result of this testing against the grouping of series. Beginning with the *Sixth Approximation*, we examined the interpretations that were made for the various phases of all the series that fell into a single family. The assumption was that if we had to make substantially different interpretations for comparable phases of the series in the family, there was something wrong with either the interpretations that we were making or with the definitions that produced those groupings. Basically, the family grouping is intended to permit us to group soils about which we make the same major interpretations for use and management. If we get soils in a family whose comparable phases require substantially different interpretations, we know there is something wrong. A number of such defects have come to life since *Soil Taxonomy* was published. There is a major problem at the moment about how *Soil Taxonomy* is going to be revised and kept up to date. That problem is unresolved as yet. There have been to my

knowledge no really approved changes in *Soil Taxonomy* since it was printed, although suggestions have been flowing into Washington from outside the U.S. as well as within. **Questic 1, Texas**

The effort to create a new soil taxonomy didn't happen naturally. I could see tl necessity for abandoning the 1938 classification as did Dr. Cline. The concept of zona intrazonal soils was untenable. If we were going to have a taxonomy it had to be completel revised because these were at the order level. I did not make the decision that we shoul develop this, that was done by Dr. Kellogg and behind closed doors we discussed this problen I pointed out to him that we had no alternative but to start all over and devise a ne classification. I hoped that someone else would have to do it. I thought that job belonged t the Director of Classification and Correlation. There was a closed door discussion about that. wound up with the task. The necessity for developing *Taxonomy* was the result of th difficulty of making soil correlations for our public soil surveys. The soil survey in the Burea of Plant Industry, Soils, and Agricultural Engineering had only a few soils going at any on time. By 1950 the Soil Conservation Service was mapping soils in nearly every county in th country. And it was apparent that we were going to be faced with the correlation problems c the country at one time. They tried to resolve this problem by setting up a committee of SC and Bureau people to do the correlation. This got into such serious trouble that the land grar university people went to the Secretary of Agriculture and insisted that the Soil Conservatio Service discontinue publication of their surveys; to consider them as expendable, having onc been used for planning the farm, their utility was supposed to be finished. Yet it seemed t some of us, that this was a terrible waste of federal funds because there should be som mechanism by which we could make use of the enormous activity of the Soil Conservatio Service in mapping, compared to the Plant Industry. This could not be done without taxonomy. We could not improve the old one, therefore, it was in the public interest to devis a new one. **Question 176, Minnesota**

The effort on development of *Soil Taxonomy* was principally by the principal correlator and the state correlators. Dr. Kellogg had very little time for this, Dr. Simonson, none. We hac quite a bit of response from university scientists at the work planning conferences, a great deal Every state was represented except New Jersey and Virginia. They were represented but ther

the same taxa; there were soils that had no place. These deficiencies in the definitions of the *Third, Fourth, Sixth,* and *Seventh Approximations* were brought to my attention, generally with suggestions for solution to the difficulties that were observed, and sometimes merely expressing an unhappiness with the groupings that resulted. Surely there must have been at least 100 man-years of work of the correlation staff involved in the development of the final *Taxonomy*. I suspect this is a gross underestimate of the actual time, but no specific records were ever kept.

In the laboratory we had to develop methods, for one thing. For example, to develop sampling of the soil in such a way that we could get a measure of the total amount of organic matter for a given volume. It is simple enough to sample and get the percentage of organic carbon, but it is a very different business to get comparable data for the total amount of carbon per unit volume of the soil because the bulk density depends in some soils enormously on the moisture content at time of sampling, particularly among the Vertisols and the vertic subgroups of other orders. So methods had to be developed for comparing soils at a standard moisture content so that if we were sampling in the dry season in one place and the rainy season in another our data would be comparable. This involved a number of man-years of work before we came up with the current method of coating the clods with Saran. Then we had to characterize a number of soils. We had a program from the start, of the Soil Survey Laboratories and the start also of *Soil Taxonomy*, to characterize the soils of specific mapping areas. We had at that time three laboratories. The data generated by this effort became the data that we had to consider when we were developing taxa about which we could make statements. The laboratory staff must have expended something like 300 or more man-years of work in collecting the data that were needed. Similarly we had a half dozen man-years of soil geomorphology teams working primarily to understand the relations between geomorphology and soil genesis and properties. The testing went on for over a rather long period of time, beginning about 1952 with the earlier approximations and continuing up to the present time so that the numbers I have given involved the testing through the *Seventh Approximation*. The *Seventh Approximation* was known to be deficient in many respects and had virtually nothing to say about organic soils--Histosols. The work on the classification of Histosols required another project for study of means of classifying Histosols according to their properties and not according to the presumed vegetation from which they came. Once the *Seventh Approximation* was published, we made a more concerted effort to test the taxa that evolved from the definition that we used against the interpretations that we were making at the level of phases of families. We felt that the given phase of a family should be subject to the same major interpretations, and so we requested our interpretation experts to get together with our classification experts and compare the interpretations that were being made for phases of all the series that were grouped in a family. This kind of testing brought a number of deficiencies to light that we had not been told about from a more general testing or examination of the groupings that resulted from application of the definitions. Consequently, a number of additional modifications had to be made in the definitions that came to light only because of the testing on the interpretations that we were making. This led to the publication of the supplements that came out in '64 before we began to use the taxonomy and in subsequent years once we had come to use it. Actually we had to use it. Starting in 1965 the staffs in the state and regional offices were being forced to examine the definitions more carefully than they had ever done before. **Question 21, Cornell**

The family category was included to bring together the series classification and great group classification. That was necessary for the correlation process. We have used the same characteristics at different categoric levels. Temperature limits for the family are smaller than those of the suborder. Again, those are there because of their value for interpretive uses, and we would be, I think, violating the logic of classification if we stayed blindly to the use of one characteristic at the same categoric level with all soils, because the logic of classification says that we should have classes about which you can make the greatest number and most important statements. For the most part, our concerns are the soil survey interpretations. We also have to bring together the soil classification and the capability classification. One was an interpretative classification and the other was taxonomic. You had to go by one additional step of reasoning to get from the taxonomy to the capability. It was about the only test we had of the validity of the way we had grouped our soils; namely, what could we say about their use and behavior. Those are our important statements in the soil survey.

Someone who is concerned with soil chemistry might not consider these important statements at all, from his point of view. If he doesn't like them, I think that he has every right to develop his own classification, but it's a major undertaking. One of the chief pedologists in ORSTOM, the French scientific overseas ministry, is developing his own classification at the moment, pretty much following the principles of Fields in New Zealand, i.e. according to composition. While we have considered composition in some orders, as in Oxisols, and in some suborders, as in Andepts, we have not given composition a particular place, a particular category in the system. We have used it to subdivide the soils in such a way that we do get homogenous groupings in the low categories. **Question 16, Texas**

Spatial consideration did not play a significant role in the design of *Soil Taxonomy*. We recognize that some kinds of soil have very different shapes from others, and the polypedons have very different sizes. Some occur on the ridges and some occur in the flood plains and so on, but by and large we were not going to concern ourselves with spatial considerations. We took the view that it was the nature of the soil rather than it's area and shape that was critical. I don't like analogies. They are often misleading but if I were going to use one I would go back to entomology where I first started college and we would have to have a separate kingdom for the ants because there are so many more of them than any other kind of animal. We make maps on all scales. In the soil survey we make small-scale maps and large-scale maps and, depending on our purposes, we may make very large-scale maps from small areas. Then the classification has to reflect the properties of the soils that are at the scale that we map, and that scale has to be determined by the purpose of the soil survey, which is to reflect the behavior of a soil under the foreseeable uses, of not all possible uses, but the foreseeable ones. **Question 54, Texas**

I was violently opposed to considerations of geographic extent. I am just as strongly opposed to the rule that you must have mapped two thousand acres to establish a series. We have lost some information as a result of that rule for very contrasting kinds of soil for which we could not get a series name because the total area involved was less than two thousand acres. But because of the extreme differences in the nature of the soil and the information we could get about soil genesis, if we could preserve the location of those small areas, I would have preferred to have had established series. The general principle was that area was not to be considered, except for this two thousand acre minimum for establishing a series. **Question 126, Minnesota**

The temperature limits were fixed by the necessity of avoiding the splitting of established series. It must be remembered that there was enormous pressure not to divide series unless there were some advantages in the way of improved interpretations from creating a new series from a part of an already established one. It so happens that in the U.S. the type of farming is closely related to the climate, and the soil temperature is also closely related to the climate. The length of growing season is quite important in determining what kinds of crops may be grown. In the cotton belt in the southern part of the United States, the growing season must be long and the interpretations for the soils in that part of the U.S. are quite different from those that we make in the corn belt where the growing season is shorter. The limit between the cotton belt and the corn belt then was a limit where the soil series all changed and this temperature, mean annual soil temperature, on this boundary was approximately 15° C. We could then establish the difference between the thermic and mesic at 15° C without affecting the classification of the series. Similarly, the limit between the mesic and the frigid involved another change in the type of farming and another change in the series that were warmer than 8° C or cooler than 8° C. One might then say that the major factor was the utilization of the soil because this determined the points at which the soil series were changed.

I did not visualize that the whole world would accept *Soil Taxonomy* and use it as such, but I did visualize that the best system for the U.S. was one that would accommodate all soils of the world, so that we could transfer knowledge to (or from) anywhere in the world if the soils have been studied enough to place them in our system. We spent a good deal of time when we first began to develop this system in studying the soil classification systems and the soils of various developed countries, particularly in western Europe, that had on-going soil surveys. I could see no reason to visit a country where the soil classification was a theoretical sort of thing. I tried it and I found that it was useless. They had nothing to tell me. I could use only

what I could see about their soils myself. The justification for spending so much time in Europe with countries with soil surveys was that we could potentially benefit the American people if we could uncover some soils information in these countries that could be transferred to the U.S. This was all we could do according to law. Now AID has the opposite restriction. It is supposed to spend its money for the benefit of these other countries, increasing food production, what have you, rather than for the benefit of the U.S. directly. The cooperation now of AID with SCS permits us to work on a world-wide basis in countries that will admit us. **Question 31, Minnesota**

If I had it to do over, I would retire again. Or if they would not let me I would go through the process the same way--through approximations. You cannot bring a group of people together in a big committee and get useful proposals from them unless you give them something to react to. This is why we started from the beginning with approximations, because we could call our correlation staff together. But they would not do anything but argue, so that if we gave them something to react to, they could react positively or negatively, and we could get something from their time. I would go about it the same way again. I have found many errors in the classification. To correct the errors requires a great deal of correspondence and discussion between knowledgeable people. No one man knows enough about soils in general to devise a useful classification by himself. It requires the effort of a great many people knowledgeable in soils of their own areas in all parts of the world to develop a system that can be useful generally. **Question 22, Cornell**

1.8 Impacts of Historical Concepts on Soil Taxonomy

1.8.1 The Fundamental Theses of Dokuchaiev, Glinka and Marbut

We must recall that *Soil Taxonomy* was developed to be of assistance to the preparation of soil surveys which includes both the mapping and the interpretation of the significance of the map units. The pedologist who is making a soil map while working in the field expects to find a change in the nature of the soil wherever there is a change in one of the soil-forming factors first enunciated by Dokuchaiev and his school. If the slope changes radically, the pedologist looks for that border between polypedons at some point on the slope. When he locates it in one place, he tries then to extend that border on the basis of the landscape configuration. This is an enormous advantage in the preparation of the map because, knowing something about the factors that influence the nature of the soil, a pedologist does not have to bore at random and make a grid of his observations and then draw boundaries between the points on the grid. It not only greatly shortens the time necessary for mapping but it greatly increases the accuracy of the mapping. Particularly when the mapper draws his boundaries in advance of examination on the traverse that he proposes to follow. If he draws his boundaries in advance on his traverse, he then can check when he crosses the point on that traverse where the soil changes. If that is the point, he can have confidence in where he drew his boundary on routes that he did not traverse. If he finds that the boundary is not where he predicted, then he must reexamine what he is doing because his limits are going to be just as bad to the right or to the left as they were straight ahead on his traverse. Now this is the fundamental impact of Dokuchaiev's idea of soil taxonomy.

Dokuchaiev's major contribution was to recognize that soils were natural bodies; that they owed their properties to the five factors of soil formation, namely the parent material, the vegetation and animals, the biologic factors, the drainage, the groundwater, the topography, and the age of the landform. **Question 1, Cornell**

Dokuchaiev was making soil surveys on rather small-scale maps, not large-scale maps, for the purpose of locating regions in Russia that were suitable for development for agriculture and in some places, I have been told, that it was also used as a basis for assessment of taxes. There is a very large difference between what we show on large-scale and small-scale maps and

Dokuchaiev noticed first that the Chernozem was related to the climate and the vegetation. Those were the two factors that were important in the region where he was working, which was largely of uniform parent material - loess of Wisconsin age - and so the limits of his Chernozems corresponded with the drier parts of the Soviet Union, not the driest, but where he had grass vegetation, and the Chernozem was absent in the forest zone. So he first developed the notion of the Chernozem as a soil that forms under grass in a subhumid climate. This was our concept of, virtually, the order of Mollisols today (not entirely but rather, maybe I should say, of Ustolls). **Question 2, Cornell**

The concept of zonality was introduced by Dokuchaiev's students as a basis for arranging their soil groups into a higher category. This was done about 1900. In 1938 the U.S. Department of Agriculture introduced a new series of publications, the *Yearbooks of Agriculture*, which previously had consisted of statistics. It was decided that yearbooks would be produced by subject matter to make available the status of the current knowledge to people who were able to read something that was only moderately technical. The Secretary of Agriculture decided that the first such book should be about soils and appointed Charles E. Kellogg as chairman of a committee to arrange the contents of that book and to find the appropriate authors. The lead time was about one year between the appointment of the committee and the date that the manuscripts were due. Dr. Kellogg has told me many times that he told the Secretary that they could not prepare such a book because we had no system of classification of soils and we needed time to develop such a system. He was told by the Secretary (Henry Wallace) that this was precisely why he wanted these books: to document the current state of knowledge and that they were to go ahead with the preparation of the yearbook--*Soils and Men*. This gave them then one year in which to develop a classification of soils of the United States. There was no time really to develop a new system. They had to borrow one that had been proposed at some time in the past. They had no time to test any of the concepts that were presented in that book. There were no real definitions of any of the great groups; there were only more or less general descriptions. We were unable to find any single soil property that included all the soils that were called zonal and excluded the soils that were called intrazonal. The azonal soils were recognizable as the present group of Entisols, but the intrazonal and zonal soils were not clearly distinguished by any soil property. The literature says in some places that the zonal soils were all more or less freely drained, but this is untrue, because the tundra soils were included as a zonal great group, and the tundra soils were described as being grey, mottled, and wet. So before the work really started on the development of *Soil Taxonomy*, we had realized that if we classified soils as zonal and intrazonal we could not do it on the basis of their own properties, and it was a fundamental thesis even of Marbut that a soil should be classified on the basis of its own properties, even though Marbut failed to do that. **Question 7, Cornell**

There is little correspondence between the orders of *Soil Taxonomy* and Marbut's normal soils. You have to exclude the Vertisols, the Histosols, and Entisols; all of the aquic suborders would have to be excluded, as well as the soils with pans; all the fragic great groups and the duric great groups and the natric great groups would have been excluded. **Question 5, Cornell**

There was an enormous change as a result of Marbut's translation of Glinka's book. Glinka introduced to Marbut the idea of the classification at the level of what we now call the great group. Prior to that, the soil survey had two categories, the series and the type, and there was no arrangement of the series into any higher categories of any sort. Rather they were grouped on the geology of the parent material of the soils, so that we had the broad provinces--glacial and loessal for one, the piedmont and coastal plain for another. The theory at that time was that soils in the regions were developed more or less from the same kinds of parent materials over about the same length of geologic time, and a series could not be placed in two different provinces. The series had to change at the province boundary, but this was not actually a category in the classification prior to Marbut's translation of Glinka's book. **Question 6, Cornell**

Marbut's concept of normal soil had little influence in the development of *Taxonomy*. He more often called it a mature soil than a normal soil, but the two terms were more or less synonyms to him. These were soils that had A, B, and C horizons. The development of the B horizon was essential to the normal soil. The normal soil also had to be a relatively free-

draining soil, and his concept was that we could only classify these soils. The others could not be classified. Those without B horizons or those with overly developed B horizons could not appear in his classification at any level of any category. He used the analogy that in classifying an insect we do not try to classify the larvae but we wait and classify the adult insect. This was not a good analogy because soils are not going to change overnight; as a rule the changes are very slow, and they take a matter of human lifetimes to become very discernible. Marbut got around this difficulty with soils that did not have a mature profile by classifying those soils on the basis of the surrounding or neighboring normal soils. Thus a poorly drained soil which was not normal, he said, would eventually be drained in geologic time, and once the natural drainage was established the normal soil could begin to develop. How he was proposing to drain the lower coastal plain I do not know. And yet these soils had to be classified as though they were going to become well drained at some time in the future when somebody lowered the ocean level.

This concept of Marbut was untenable and was abandoned in the classification of Baldwin, Thorpe, and Kellogg in the 1938 *Yearbook of Agriculture - Soils and Men*. It had been abandoned as a tenable basis for classifying soils many years before we began work on *Soil Taxonomy*. **Question 3, Cornell**

I might use the Red-Yellow Podzolic and the Gray-Brown Podzolic great groups as examples of what happened to the central concepts of the zonal great soil groups. These two great soil groups were not defined, but were described very generally in terms of the central concepts. Some of the correlators wanted to use definition by type as the botanists do. The Norfolk series, the Ruston series were the central concept of the Red-Yellow Podzolic soils, and Miami Series was the central concept of the Gray-Brown Podzolic soils. There would be no confusion between these central concepts. However, about 1951 we had a joint meeting involving the correlators of the southern states where the soils were mostly Red-Yellow Podzolic soils and northern states where they were mostly considered Gray-Brown Podzolic soils. We worked on the border between Virginia and Maryland, because of the limits of the correlation areas. We examined quite a number of soils that we could all agree were Red-Yellow Podzolic soils, but when we got into Maryland we looked at a number of profiles of the Chester series. This had the clay mineralogy of the Red-Yellow Podzolic soils, but it was shallow compared to the Norfolk and Ruston; its depth comparable to that of the Miami. The base status resembled that of the Red-Yellow Podzolic soils rather than the Gray-Brown Podzolic soils. If we were able to find a virgin area, the color of the profile was more like the Gray-Brown Podzolic than the Red-Yellow soil in that the A horizon was not particularly bleached. The people from the southern states said this is a Gray-Brown Podzolic soil and the people from the northern states said this is a Red-Yellow Podzolic soil, and no agreement was ever reached about how the Chester series should be classified at the great soil group level. The central concepts of many of the great soils groups form the current concepts of several of the orders and a number of the suborders, and we will probably get into this in more detail as we go along. **Question 12, Cornell**

The intrazonal great soil groups were really the wastebasket of the classifications in use in 1950. They included many things--the soils that have natric horizons were all grouped into one intrazonal great soil group which covered a very wide range of kinds of soil. We found them in association with Boralfs, with Borolls, with Udalfs, with Xeralfs, with Xerolls, with Aquolls, with Aqualfs. These are the kinds of soils with which we get these tiny areas, the so called slick spots with natric horizons. They were all put into one great soil group, I think, in the 49 classification. **Question 13, Cornell**

1.8.2 Other Concepts

Within the Soil Conservation Service, work on *Soil Taxonomy* and soil geomorphology started at the same moment, and it is difficult for me to say, was well advanced. *Soil Taxonomy* was developed with rather primitive concepts of geomorphology. The impacts were important in some of the orders that were developed last, namely the Oxisols and the Aridisols. Soil geomorphology work did tell us a good deal about the genesis of the petrocalcic horizon,

which is most prominent in Aridisols but does occur in some Mollisols. It led to the concept of the "pale" great groups along with the work on the coastal plain in North Carolina, where we developed the concepts of Paleudults as distinct from the Hapludults. Soil geomorphology studies surely affected the classification of the soils at the great group level. **Question 16, Cornell**

We tried to keep discontinuities of materials out of the higher categories of *Soil Taxonomy*, to restrict them largely to the family category, where the transport was so long ago that we have some genetic horizons to base our classification on. So that the definition of the argillic horizon takes into account the potential increase in the percentage of clay due to a stratification of the parent materials. Current deposition is taken into account at a higher categoric level in the Entisols, where we distinguish Fluvents and Orthents at a suborder level. That is the current process, whereas the others are somewhat remote in geologic time. It's not always easy to recognize in the field a small difference in the sedimentation; unless the sand grains are large enough to be detected with the fingers or the teeth, one cannot always detect it in the field. A laboratory is required, and we prefer, in so far as possible, to base our classification on properties that can either be seen or felt in the field or that can be inferred from the combined knowledge of pedology and some other science such as botany, geomorphology, and climatology. **Question 17, Cornell**

Concepts of genetic processes do not dominate the differentiae at the order level. The dominant processes for the genesis of the Mollisols, for example, are considered to be the formation of the mollic epipedon as a result of underground decomposition of plant residues in the presence of appreciable calcium. This same process operates in some of the former intrazonal soils, but not the azonal ones. The intrazonal soils, the former Humic Gleys, have the same dominant process as the Ustolls and the Udolls. The grouping of the Mollisols differs from Marbut's in that he separated the Udolls from the Ustolls in his highest category--Pedalfers and Pedocals. In the 1938 classification it was decided that the Udolls with their dark-colored thick surface horizon belonged better with the Ustolls than they did with any other soils; so they were changed from intrazonal to zonal soils and were included with the suborder of dark-colored soils of subhumid and humid climates. This was a precedent in the '38 classification that carried over into *Soil Taxonomy* in developing the concept of the order of Mollisols. Marbut for some reason wanted to have only two orders. He wanted to classify all soils on the basis of some property, so that he would have only two orders. We could see no reason to limit the number of orders to two, and it seemed best to try to segregate these dominant sets of processes. **Question 20, Cornell**

Soil temperature and moisture as soil properties represent a considerable innovation. In historical perspective, we must remember that we were starting to build from the 1938 classification in which we had in the highest category zonal, intrazonal, and azonal soils. These were untenable as they were expressed in that classification, and we had to find substitutes of some sort. In a given area on the Great Plains, in the Appalachian mountains, the coastal plain, everywhere in the U.S. except perhaps the Rocky Mountains and the Sierra Nevada mountains, the temperature and the rainfall and their distribution were important factors in controlling the vegetation, as well as the possibility of leaching, the probability of permafrost, and so on. The temperature, the moisture, changing gradually over large distances, led to the grasslands of the Great Plains, the forests of the more humid regions, and they were the closest substitutes for the concept of zonality that we had. A good deal of the utility of the concepts of the zonal soils could be maintained if soil moisture and temperature were introduced at a high categoric level. Consequently, before I agreed to undertake the job of developing a new system, I reached an agreement with Dr. Kellogg that soil moisture and temperature would be introduced as soil properties at a high categoric level. This was decided before any work was undertaken with the purpose of maintaining as much continuity with previous classifications as possible. **Question 40, Cornell**

While insofar as parent material is related to mineralogy, particle-size distribution, and so on, it is, I think, fairly well taken care of in broad classes at the family level and you can still make as many subdivisions as are needed at the series level. We are not, I think, so much concerned with the parent material as a rule as we are of the nature of the soil itself. In the southeast on the Paleudults the parent material isn't too important any more. The thing that is

important is the mineralogy and particle size of the present soil. To determine the parent material is going to be difficult because you may have to go down 50 feet or so to find weatherable minerals in some of those. Was the surface mantle the same as that deep layer? We don't know that. More and more, we are finding that parent materials of soils is not what we thought it was. Soils that were supposed to have been developed in one parent material or another, we find, have significant admixtures of eolian and fluvial materials at the surface. **Question 87, Texas**

1.9 Differentiae

In considering the importance of a critical limit between orders, we must always keep in mind that soils form a continuum, that there are intergrades between most kinds of soils that may go through other orders. In order to have a clear cut definition that defines the limits of a taxon, whether it is an order or a subgroup, we have to put the limit at a point which will divide the soils on either side of that point into different taxa. Thus the two soils which are very similar, one on each side of that limit, are separated. They are more like each other than they are like the other soils in the taxon. The gradational change from one soil to another is reflected in the names. The Picacho is an Oxic Dystropept, and the Matanzas is a Tropeptic Haplorthox, indicating that these are gradational between the two orders. If one were to change the limit of the percentage of feldspars, it would only shift the subgroup nomenclature to another series, and would not eliminate any problem whatever. I do not at the moment foresee the need for special kinds of cambic horizons in intertropical soils. **Question 5, Eswaran**

In regard to the hard/massive criterion in the definition of the mollic epipedon, I don't know what problems have been encountered in the field. I know the soils that caused the formulation of this requirement are some of the Xerochrepts and some of the Xeralfs in southern California. When one samples these soils in the summer, you start with an air-hammer to get through the epipedon. It is just that hard, it is like digging in concrete. When moist, these soils would seem to have discernible structure in the epipedon, and they're easy to plow, they are soft and easy to dig. This is what the Australians called the "hard-setting A horizon". Once you have encountered it you have no trouble recognizing that extreme development. What problems you have on the intergrades, I don't know. In Illinois and Iowa, the Mollisols don't give this sort of trouble, for example, even though they are dry. Some of the soils in California may have the dark color, high base saturation, and the organic matter of a mollic epipedon but do not have the behavior of the Mollisols.

The farmers know a great deal more about the soils on their own farms than we do, and make much finer distinctions. **Question 79, Texas**

Some soils of Colombia are said to meet the requirements of Mollisols but only because mistreatment by heavy machines has destroyed the structure of their epipedon. When the Colombian soils are moist, they are very friable and have a favorable chemical condition. In reply, the first thing is that the southern California soils are exactly the same. When moist one would never suspect that they were going to become so hard when dried. But yet they do. I think there is a micromorphologic distinction that permits one to recognize these hard-setting A horizons when they are moist, but it has never been made quantitative.

Many of the complications of the definitions in *Soil Taxonomy* are due to the strong bias by the soil survey staff against changes in the definitions of soil series. And in an effort to avoid splitting the series, we have introduced what looked like inconsistencies in many places but really are consistently in favor of one reason, namely that we want to keep the soils together in the taxonomy if they really belong together because of their genesis and their behavior. We could not, of course, know everything about all the soils of the world when we developed *Soil Taxonomy* and so we disregarded those that we knew nothing about and paid attention to the soil surveys that we had already established and made known to the general public through the published soil surveys. If in the Colombian soils the moisture regime is udic and the epipedon is rarely ever dry, then the importance of the cementing properties is at a

minimum and if the definition creates problems, then it is important that it be brought to the attention of the correlation staff especially the staff leader for soil classification in the Soil Conservation Service so that the appropriate steps can be taken to correct the errors which I indicated in the original definition.

Of course, we can put in exceptions. Instead of saying that structure is strong enough that the epipedon is not both hard and massive when dry, we can say the epipedon is moist at all times or the soil has a humid moisture regime or the epipedon has a structure strong enough that it doesn't need to have any further definition. Mollic epipedons are in a number of kinds of soils, in several orders. There's a bias that is inescapable, insofar as there is a probability that we will fail to study a kind of soil of very small geographic extent. That's inescapable. It has to be extensive enough that we're going to find it. **Questions 6, Venezuela and 127, Minnesota**

At this moment it would be very difficult for me to suggest a precise number for the clay or the silt and sand to recognize a lithological discontinuity. The difference in clay and silt can be due to soil genesis or to a lithological continuity. For the most part, the recognition of the lithological discontinuity must be based on the distribution of the sand fraction in the clay-free medium. In other words, consider the ratios of the fine sand and medium and coarse sands on a clay-free basis. Otherwise, these ratios become more or less meaningless. The differences should be enough to be significant from the laboratory point of view considering the errors in sieving the sands and should also be enough to be significant from the viewpoint of collecting samples. We have in our Soil Conservation Service laboratories required the nearest thing that the fieldmen could find to duplicate pedons and the comparison of what we find by analysis of one pedon versus another gives us a notion of the sampling error. The laboratory people from time to time should run duplicate samples to determine or have a good notion of the validity of their laboratory numbers. So the differences must be enough to be significant numbers and beyond that I can not say whether it is 2% or 4% or 10%. The point is that we must have confidence that there is a difference. **Question 52, Venezuela**

The presence of soluble salts figures prominently in several other soil classifications but not in *Soil Taxonomy*. This is probably for two reasons and I don't know which is the more important, but they're related. For one thing, the series that were set up in the detailed soil surveys for irrigation areas used salinity as a phase. I think it was justified to use as a phase rather than bring it into the series definition because the salinity in soils where it is not extreme is subject to seasonal, annual, and periodic fluctuations according to the quality of the water, the amount of water, where you are in your leaching system. The salinity can go up and down during the growing season in one year; it can be reduced by leaching in the fall to get ready for another crop and if salinity is brought into the taxonomy above the phase level, then a series name has to change regularly and frequently. By setting a limit for salinity at a depth such that the variation will not be great according to the time of year or the leaching cycle, it might be possible to have a stable series. But this could easily involve bringing in to your taxonomy a part of the material that is really not part of the soil, and we have tried to classify the soil on its own properties rather than on the properties of something that lies below it. If the material lies below the soil, below the zone of rooting and if it's important, then it is entered as a phase differentia. **Question 33, Texas**

Rock structure is discernible or it is not. Within these limits, I would say it has rock structure or it does not have rock structure, rather than saying rock structure is strong or moderate. The rock structure can be very weak in sandy sediments. It can be discernible only by very careful examination of the soil using compressed air to blow out the finer sand from the coarser sands. Now this is not strong, but it is discernible with careful examination.

1.10 Non-uniform Use of Criteria

We have tried to keep together in the *Taxonomy* soils that are similar enough that we can make some important statements about them. Consider the difference between the Albolls,

where we use the albic horizon at the suborder level, and Albaquults, I think where we use it at the great group level. The Albolls are Mollisols that have an albic horizon. The drainage is always impeded to some extent, but they are a group of Mollisols with an albic horizon, and they cover the range from somewhat poorly to poorly drained. They did not want to separate them in the classification, according to the judgment of the field men about how wet they were. The horizons were easy to recognize; one could always, I think, have no problem in getting agreement about the presence or absence of an albic horizon, but great problems about getting agreement about the drainage class; so by separating the Albolls at the suborde. level, and giving priority to the albic horizon over the aquic moisture regime, we kept this natural group of soils together in the taxonomy.

In the Ultisols, we have used the aquic moisture regime to define the suborder because they are all wet, and some have an albic horizon, others have an umbric epipedon, others have an ochric epipedon. Those with the albic horizon generally have an ochric epipedon above it. The distinction between the Aquults with the ochric epipedon and the albic horizon versus those with the umbric epipedon carry over into the taxonomy the old distinction between the Humic Gley and Low Humic Gley soil of the souteastern states. They seem to think there that these were distinctions important enough to recognize at the great group level. We had used the moisture regime at the suborder level, so the first level at which we could bring in the differences in horizons was the great group level. Suppose we insisted that we use the albic horizon at the great group level, and all soils where it occurred. First, because it does not occur in all soils, we require an extra category to bring it in. Second, if we use it at the same categoric level in all soils where it does occur, then we split what seems to be a natural group of Albolls according to their natural drainage, which again does not always exist today, but is always restricted. These are soils that are naturally wet at some season, and the variability between the best and the worst drained members of the Albolls is not particularly significant so far as one can see.

The other terms, "andic," I suppose, refers to the use of andic properties as a suborder of Inceptisols and as a subgroup. Here we are dealing with differences in degree. The andic suborder has the andic properties throughout the upper 36 cm or more, in which case they are dominant in the root zone of most plants. The andic subgroup reflects a considerably lesser influence, a lesser thickness of the mantle which is derived from a pyroclastic material. If we consider an Andeptic Haploxeralf, where we have a thin mantle of ash, again, somewhat weathered, or we have no andic properties, but thick enough to have some influence in the root development, versus an Andept with a xeric moisture regime, but with a very thick mantle of ash, we are dealing with differences in degree of the influence of the ash mantle on the growth of plants and the engineering uses of that soil.

Because we make maps at varying scales, which I have mentioned before, we must not put ourselves into a box simply because we say we must deal with the same property at one and only one categoric level. Differences in degree should be reflected in different categoric levels, just as in the aquic suborder or great group, the aquic moisture regime is used at a fairly high categoric level and a difference of degree is used at a subgroup level. If I had a choice to make a new start, I probably would not have split the Inceptisols into Ochrepts and Umbrepts. This is leading to serious trouble outside of the U.S., whereas in the U.S. the Umbrepts are so rare that they make no problem here. **Question 89, Cornell**

1.11 Forming and Defining Taxa

It was no accident, as I wrote in more than one place that, *"Determination of the similarity of one kind of soil to others is not always a simple matter. There may be similarity in particle size to the members of one taxon, and to the base status to the members of another. One must decide which property is more important, and this decision must rest on the nature of the statements that one can make about the classes, that the kind of soil is grouped one way or the other. The best grouping should determine the definition, not the definition the grouping. If the grouping has imperfections, so does the definition, for our purposes, the statements about the*

nature of the soils and the interpretations that we might make to the various phases of the taxon. The grouping that helps us make the most precise and most important interpretations is the best. The taxonomy for the use of the soil survey must be tested by the nature of the interpretations that can be made." So, if just the interpretations give you trouble; there is something wrong with the definition. If there is something wrong with the definitions, it is not going to go away unless you suggest a change. There is no use in worrying about it this year, it is going to be with you for the rest of your life if you do not suggest and argue for a change. So, this problem should be brought to the attention of the Staff Leader in Soil Classification. **Question 108, Cornell**

Concerning the number of soil orders, I proposed a new order of the suborder of Andepts. I can visualize that one could easily take another order out of those soils. **Question 91, Cornell**

It is true that the definitions in *Soil Taxonomy* are complex. We have been over this once, but it would not do any harm to go over it again. The definitions are very complicated in many places in *Taxonomy* because there exists somewhere a few soil series that straddle the boundary between taxa at some higher categoric level and we want to keep them together in the classification. I can use the Glossudalfs as an example. There are 2 or 3 series in Washington and Oregon and there are 2 or 3 series in the southern Mississippi loess region. So far as I know, they are all formed in loess or at least in very silty sediments. The same thing holds in Western Europe. They are rare soils but they do occur. Their base saturation is a narrow range from about 30 to 40 percent. This just straddles the limit between Alfisols and Ultisols; but they are a natural unit. They should not be split arbitrarily into Alfisols if it is just above 35 percent and Ultisols if it is just below. So, in order to get the Glossudalfs all in one order we have to have a paragraph or two in the definition of Alfisols and in the definition of Ultisols to keep them out of one order and clearly put them in the other. This involves very small areas and very limited numbers of soil series, but it contributes a great deal to the bulk of these definitions in *Soil Taxonomy*. If these were omitted from the definitions, they could be greatly simplified and the occurrence of exceptions to a simplified definition could be inserted as a footnote. There are many such examples in *Soil Taxonomy* of complicated definitions intended simply to keep a few series that form a natural group, together. **Question 170, Cornell**

Well, it is possible to simplify these definitions enormously if we're willing to forget about, say, one percent of our soils. Maybe less than one percent. The greater part of the complicated part of the definitions are due to the presence somewhere of a group of soils that belong together. They're very similar in all their properties but they overlap one of the limits at a higher category. One should say to the students that these definitions are written for people who are actually classifying soils for the Soil Survey. For the people who use the map, the use of *Taxonomy* for other purposes, then these complicated definitions are unnecessary. And I think it can be done, too. The definitions can be greatly simplified by footnoting to a definition the presence of some exceptions. At one time I had thought to do this myself. **Question 106, Minnesota**

Most of the class definitions are at least a bit difficult. It is certainly true that the definitions of the classes are greatly simplified by referring to the diagnostic horizons. If one had to repeat all of the characteristics of a particular diagnostic horizon any time you used it the definitions would be unmanageable completely. Where it seemed critical to comprehension we did try to use, not necessarily horizons but features. We did try to define these, to simplify the definitions, well, I suppose, we didn't do any more of it because we didn't find it necessary. It's a very vague limit as to how far one should go in that direction. **Question 107, Minnesota**

To simplify the subgroup definitions in the aquic subgroups would surely improve them. But, they are not all the same, they vary from one order to another and from one temperature regime to another. In the Ultisols we do not require low chromas as we do in most other orders for the aquic subgroups or suborders. The warmer the soil gets, it seems, the more the evidence of wetness shifts to 2.5Y or 5Y hues accompanied by prominent mottles. In the temperate soils, we like low chromas, but in the intertropical soils, we are going to be forced to use the hue rather than the chroma -- but the hue would be used only if accompanied by segregations of iron and manganese in the form of mottles. Grouping and naming complex features could

greatly simplify almost all of the typic subgroup definitions, particularly all of those that have an aquic subgroup.

We should point out that the significance of the evidence of wetness also varies greatly according to the kind of soil. In the soils that have ustic moisture regimes, we probably will find, generally, that the aquic subgroups are to be preferred to the typics because they have more moisture than do the typics. In the boric orders, the presence of shallow groundwater is a serious handicap to use because the growing season which is already short, is further shortened in the aquic subgroups. So we must keep this in mind in writing any simplified subgroup definitions.

The differences in evidences of wetness could be explained at some length in the discussions of, I hate to say "aquic characteristics", but the kind of aquic characteristics that we use for the aquic subgroups and this might suggest another formative element.

I think one might ponder quite a bit about what the formative element would be because these are actually, for the most part, intergrades between the aquic suborders or great groups and the non-aquic suborders and great groups. And the use of the formative element "aquic" at the subgroup level emphasizes this relationship, that it is an intergrade. **Question 108, Minnesota**

Marbut at one time recognized the three subdivisions of the Chernozems according to latitude. He said it was improper to call them southern and northern and central but he did not come up with other names to the best of my recollection. And then he finally dropped it completely in his *Atlas of American Agriculture*. When we went back to look at the nature of the soil in the different latitudes, there were some fairly consistent differences between the Chernozems of North Dakota and the Chernozems of Nebraska and Kansas. As I have mentioned earlier, because we could draw a boundary at 8° C without splitting any series, we used the 8° limit - mean annual soil temperature - to cut out what had been Marbut's original northern Chernozem. But the distinct difference was the chroma of the soil. In North Dakota the Chernozems mostly have an epipedon with a chroma of 1, in Nebraska and Kansas it's mostly with a chroma of 2. But when we got into the drier parts of the cold Chernozems the chroma switched from 1 to 2 and there was no consistent difference other than that of the temperature. We used the chroma in North Dakota and South Dakota to distinguish the ustic subgroups. **Question 137, Minnesota**

Separation of aridic soils with pedogenic horizons from those without pedogenic horizons, I suppose, was a distinction that came from our experience with the 1938 classification where soils without horizons were grouped as azonal soils in one order. That was the only order that was based on a soil property - the azonal order. It probably came from the early experience with the European classifications where a coarse subdivision of soils was made on the basis of the horizon designations: soils with only a C horizon, those with AC horizons, those with ABC horizons. The first group of soils without genetic horizons was generally separated in the European classifications as well as the American. This is probably an inheritance from the previous classifications; most of them made this distinction of soils with and without genetic horizons. I can not recall any serious criticism of the idea of allowing the Entisols to have an aridic moisture regime in the arid landscapes. You have soils with and without horizons, just as you do in other landscapes. These were separated in other landscapes and we probably simply carried it on over into the arid regions. So we had the Aridisols which were considered to be soils of arid regions with genetic horizons. And the Entisols were considered to be truly azonal. They could have any moisture regime as long as they had no horizons. It's more difficult to explain why we had the Torrerts -- Vertisols with an aridic moisture regime -- instead of putting them into a vertic great group of Aridisols. Actually, their horizonation is extremely weak. The Torrox would be another suborder of the Oxisols with aridic moisture regimes -- and this has come up several times in these conversations -- why do we have these torric suborders instead of putting them all into Aridisols? The Torrox do have an oxic horizon. I can not say that Torrerts have very much horizonation but they do have the potential shrink-swell and cracks and so on of the other Vertisols. You would surely have to say that one may question the logic of all this, but the taxonomy evolved slowly and some of the ideas from some of the earlier approximations carried over, presumably because no one criticized them.

People who criticize *Taxonomy* because it doesn't work like a simple key are people who probably don't understand that *Taxonomy* has a purpose that's spelled out. They want a theoretical classification. To serve the functions of the soil survey, the taxonomy has to be usable as a key for correlation. You must be able to trace a soil down, but if you carry this idea that you must use a given characteristic in the same category for all soils, you are going to come up with, not an infinite number of categories, but a very large number of categories. Then you must completely abandon the nomenclature that we have. I don't think you'll find a better nomenclature in any taxonomy than the one we have. It's a useful one for communication. But this adherence to a strict theoretical insistence on using a given characteristic only once in the taxonomy and in the same category in all soils is going to enormously multiply the number of categories and destroy the nomenclature completely. You must also remember that we make soil surveys at different scales. For the small-scale maps we tend to use the higher categories, generally the great groups or even suborders. For the large-scale maps we use phases of series and families and even subgroups. If we are going to use a given property, such as the moisture regime, in only one category for all soils, then you don't have the choice of making a broad subdivision of soil climate for small-scale maps and a fine subdivision for large-scale maps. You are restricted in what you can do. The people who criticize *Taxonomy* forget completely that we do make soil maps at small scales as well as at large scales. The requirements of the surveys vary with the scale. The taxonomy is intended to permit broad subdivisions for the small-scale maps and fine subdivisions for the large-scale maps. **Question 170, Minnesota**

The correlation between soil color and organic matter content is pretty good in some places but not so good in others. Let's look at what we did with the Inceptisols. We have the Ochrepts and the Umbrepts in the temperate regions. We didn't want to tie ourselves to that color in the intertropical regions so we have the subgroup of Tropepts where we pay no attention to the color. There's certainly a very poor relationship between color and carbon in the intertropical soils. Review the first soil survey of Puerto Rico. You will find it says there that the Nipe is very low in organic matter when actually it has more carbon than the Mollisols of Iowa. It just doesn't show. Thirty-eight kilos of carbon per cubic meter. Lots of Mollisols don't have that much. On a percentage basis that is 6% carbon to 28 centimeters depth and 6% carbon is well above a lot of the Mollisols to a depth of 25 centimeters. So it's primarily in the warmer soils that there is no relation that I can detect between carbon and color. I examined a lot of data and descriptions on the soils of the West Indies. I could find no relation between value or chroma and carbon. **Question 203, Minnesota**

Early in the development of *Soil Taxonomy* there was a lengthy argument about allowing some of the range of series characteristics to be outside the boundaries of higher categories. I would refer you to Professor Cline's publication on *Soil Classification in the United States*, where he discusses the logic of classification. At the time that we were trying to develop our definitions, there were two more or less contrasting points of view about the range of a series. For the purposes of correlation in one regional technical service center versus another, the ideal definition is one that gives the limits of the class, because you can observe those. It is not something that you apply subjectively. If, on the other hand, you take the point of view that a taxon is something that should be bound from within, rather than circumscribed from without, then the judgement of the correlator in one state or one service center may be quite different from the judgement of another. I must remind you that one of the basic problems that we had to resolve with *Soil Taxonomy* was the correlation backlog. We could never get more than about 30 counties correlated in any one year. We had built up a backlog of unpublished soil surveys of 10 years or more. We had to decentralize the correlation process, but we had to keep it under reasonable control, in that what we do at Fort Worth and what they do at Lincoln will not be diametrically opposed. There seemed to be no reasonable solution to this backlog of unpublished surveys, unpublished because they couldn't be correlated, except to decentralize the correlation process to the states and the technical service centers. The only way the correlation process could be controlled was by means of the definition in terms of limits. If a soil exceeds a proposed series, exceeds the limits of some higher category, then you have 3 possibilities: 1) one is to have a new series, 2) to recognize a taxadjunct, or 3) to modify the definition so that in one combination of circumstances you have one limit, and in another combination of circumstances the limits may vary. This is one of the reasons that we have so many complaints about the complicated definitions. That we have kinds of soil that straddle one of the limits in

some higher category. And they may not deviate much from that limit, but they are on both sides. I would favor conventions that would help us to bring classification and correlation into closer agreement. As a general rule, these complicated definitions are that way because of a very few soils. They do concern someone who is classifying the soils; they don't concern anyone who is using the classification. I think these definitions could be greatly simplified for people who are using the classification. I see no good way to simplify it for the people who are doing the classifying. **Question 13, Texas**

In discussing the impact of genesis and soil interpretations on *Soil Taxonomy*, I must go back to John Stuart Mill's statement that the best classification is the one which permits the largest number and most important statements about the objects that are grouped. The end product that we want for large-scale maps is the interpretations about soil behavior or growth of plants and engineering purposes. So, we had to examine the interpretations that resulted from the choice of one criterion versus another. In general, we have tried to use genetic factors in the higher categories, and interpretive factors in the lower categories, but it is not always possible to do this. So if we cannot distinguish two kinds of soils that we believe to differ in genesis, but cannot prove, then we go to the interpretations. The final test was, what kinds of families we came out with. If we had contrasting kinds of soil grouped at the great group level, and if we could not separate them at the subgroup level, we had contrasting kinds of soil in the family, with differing kinds of interpretation. When we got that, we knew something was wrong with our definitions in the higher categories, and we reexamined those definitions to see where we could divide those contrasting soils above the family level so that we came out with relatively homogeneous families. **Questions 35 and 36, Cornell**

It is the present soil temperature and moisture regime that is used as a differentia in *Soil Taxonomy*, not the previous conditions. The past climate controls the presence or absence of some horizons, but it does not control the present biologic phenomenon. The present biologic phenomenon is controlled by the present climate. The present climate reflects what is going on in the soil today. **Question 38, Cornell**

As you know a given differentia is not always used at the same categorical level in the system. Consider the Entisols as an example. Entisols have no diagnostic horizons other than an anthropic epipedon. One could have used moisture and temperature to define suborders of Entisols. Certainly this is possible but the question is one of developing classes about which one can make the greatest number of statements about the things included in a given class. Amongst the Entisols there are several reasons why the soils do not have diagnostic horizons. One is that they are continually receiving new sediments. Another is that erosion is removing materials more rapidly than allows horizons to develop. The third one is that man has disturbed the soil to great depths and mixed horizons that have previously existed. If one considers then, these reasons why Entisols have no horizons, it seems that one might be able to make more statements in common about the soils which are receiving the alluvium than about the soils which are alternately moist and dry. Having decided to divide the Entisols according to the reasons why they lack horizons, although these are not specified in the definition, the next most important features of the soils seems to be moisture and temperature. At the first category possible then, moisture and temperature were recognized as differentiae, but in Entisols the suborder took up the causes for the lack of horizons and, therefore, the introduction of moisture and temperature could only be made at the great group level. Had we insisted on using one criterion at the same categoric level under all combinations of other properties we would have had an almost infinite number of categories and we would have been unable to make many statements about most of the units that resulted. Some pedologists consider depth to clay maximum and solum thickness as indications of differences in soil genesis. The depth to clay maximum, of course, is influenced by more than genesis, it's influenced by erosion that has taken place. These are fairly complex measurements. **Questions 41, Leamy and 135, Minnesota**

I have mentioned elsewhere that spatial variability of soils was not used directly in the design of *Soil Taxonomy*. Soil climate was used in the higher categories as a partial substitute for the old concept of zonality in soils. The spatial variability in soil climate is apt to be appreciably less than the spatial variability of the glacial till in this area. We have broad areas where the soil climate may be uniform or it may, as we have here (Ithaca, New York), be a mixture of aquic and udic regimes. **Question 133, Cornell**

Classification is not just an arbitrary system of subdividing when you know nothing about what you are doing. You have a purpose for classifying and as an example that has been used in other discussions, I would like to take the definition of the typic subgroup in which Item A is something, Item B is something else. We provide for a subgroup for soils like the typic except for A and other subgroups, soils like the typic except for B. Suppose we find then a soil that is like the typic except for A and B. This is called an implied subgroup but to decide whether we want that subgroup we have to have an example that we can study. It may be that we will prefer to avoid that implied subgroup by saying that the soils are "like the typic except for A with or without B". We wouldn't say "B with or without A" because those are parallel definitions. We would however, not want to establish that subgroup in the absence of any knowledge about its behavior. I can not quite agree with your questioner that to provide for every contingency would produce too many changes. **Question 109, Minnesota**

Obviously, in soil survey, if we cannot make interpretations for phases of taxa of a high categoric level, we cannot make any statements about the soils of the given map unit. No interpretations would be possible unless we devise a system that lets us make some statement about the greater part of our taxa. We cannot make any statement about Entisols as an order, except that they do not have horizons. This is not a very important statement, except genetically perhaps, but for other purposes of interpretation, it has no value whatever. One can make a great many statements about the order of Vertisols, likewise with Spodosols. There are not too many statements other than suitability for permanent agriculture, with and without soil amendments that one can make for Mollisols, Alfisols, Ultisols. The argillic horizon is used not because it is in itself too important, but because of its accessory properties. It is a mark of a certain stability of the land surface, some minimum age. In itself, it is not particularly important; it only has importance to the extent that the peds in the argillic horizon have clay coatings which are much richer in nutrients that are cycled by plants than the interiors of the peds. Otherwise, it has little importance. If you have a cambic horizon with blocky structure, no one has yet studied that to see whether or not the surfaces of the blocky peds or prisms have a different nutrient status than the interior. One may assume that there is a difference, but I do not know of any study on that. On the argillic horizon, Buol has several papers showing that in the argillic horizon there is a considerable difference in the nutrients that are cycled. We wanted a grouping of soils at the order level. We wanted to subdivide those groupings at the suborder level, and at the great group level, and so on down, so that we could have a means to identify the taxonomic position of a particular soil series. This is a very nice arrangement with about 10 orders, and each order, each taxon subdivided roughly 5 times in each lower category. So, for the most part, one can readily understand the nature of the soil included in the taxon. You get 50 or 100 subdivisions of a taxon, it is virtually hopeless to understand what is in that taxon, without some sort of a completely artificial key.

So, we have to assess the relative importance of some of these things. The argillic horizon is not important; the base status is, but these are soils of stable surfaces that we put into Alfisols and Ultisols. When we get to Mollisols, we have to weigh the importance of the argillic horizon versus the soil climate, and versus the presence or absence of a mollic epipedon. The more important grouping is that which let's us make the greater number and more important statements. So, the Mollisols were put together as a group because they have a mollic epipedon, and they had high base saturation throughout the whole soil. Having grouped them, then, what was the most important feature: the soil climate or the argillic horizon? Well, as I said, the argillic horizon by itself has little importance. The climate and temperature of the soil, the moisture regime of the soil, are extremely important to the nature of the statements we can make about the use of the soil at the order level. For the soils that do not have a mollic epipedon, we tried in several approximations to group the soils with and without argillic horizons by other properties, and in every instance, we met with serious resistance to the nature of the groupings that resulted. So, finally, we settled upon using the argillic horizon and the base saturation at the order level in Alfisols and Ultisols, not because the argillic horizon is important, but because it gave us what seemed to be groupings of soils homogeneous enough that we could make some statements about them, and they should be important statements, not that they have or do not have an argillic horizon, but because there is something else that we can say that is important for the purposes of soil survey. I should say, that in general, we gave priority to the properties of the soil that were most limiting to its use; so that if the soil limitation principally was its coldness, we gave that priority over the moisture regimes. If the

property that was limiting was principally moisture as in Venezuela, where the temperature does not limit except in the high Andes, we gave priority to the moisture regime over temperature. This was the general principle we followed in the development of the system. People who complain that we use the same characteristic at different categoric levels generally want a classification for an unknown or undisclosed purpose. I know of no other taxonomy which states the purpose for which it was made. These are classifications designed to satisfy somebody's intellectual fancies, not made for practical purposes, and yet it has been over a 100 years since John Stuart Mill pointed out that classification should be made for practical purposes. They are devices made by man and not truths to be discovered.

Most pedologists have never bothered to read a book about logic on taxonomies. Pedologists are remarkably uncurious about problems on taxonomy. **Question 109, Cornell**

The term, "andic", I suppose, refers to the use of andic properties as a suborder of Inceptisols and as a subgroup. Here we are dealing with differences in degree. The andic suborder has the andic properties throughout the upper 36 cm or more, in which case they are dominant in the root zone of most plants. The andic subgroup reflects a considerably lesser influence, a lesser thickness of the mantle which is derived from a pyroclastic material. If we consider an Andeptic Haploxeralf, where we have a thin mantle of ash, again, somewhat weathered, or we have no andic properties, but thick enough to have some influence in the root development, versus an Andept with a xeric moisture regime, but with a very thick mantle of ash, we are dealing with differences in degree of the influence of the ash mantle on the growth of plants and the engineering uses of that soil. Because we make maps at varying scales, which I have mentioned before. We must not put ourselves into a box simply because we say we must deal with the same property at one and only one categoric level. Differences in degree should be reflected in different categoric levels, just as in the aquic suborder or great group, the aquic moisture regime is used at a fairly high categoric level and a difference of degree is used at a subgroup level.

Concerning Oxisols - Ultisols intergrades, we were guided by the interpretation that could be made. As the question is worded, it seems that it would be impossible to have an ultic subgroup of an oxic great group if the soil did not meet the requirements of the order of Oxisols. If I reverse that, one could have an oxic subgroup of an Ultisol, which is something that we did have. This was based on clay activity. The *"International Committee on the Classification of Alfisols and Ultisols with Low-Activity Clays"* has been discussing the possibility of ultic subgroups of Oxisols which meet the requirements of Oxisols. The most important guidelines which should govern the proposals for new subgroups would be the interpretation that we are making at the family level. If they are all the same, then it is better not to establish an implied subgroup but rather to modify the definition of the subgroup which is so similar. **Question 54, Cornell**

1.12 Laboratory Methods and Analyses

When we began the development of *Soil Taxonomy* in 1950, there was no body of laboratory data about the soils of the United States that was available generally to any interested pedologist. The filing drawers in the agricultural experiment stations were full of unpublished data that nobody could find. We just did not know much about the base saturation, for example, of the soils of the United States. There were different methods for determining base saturation that could not readily be compared. A sum of bases, using triethanol-amine, was almost never the same as the base saturation by ammonium acetate at pH 7. We did not know why they differed at that moment. The concept of pH-dependent charge did not really become generally accepted until some years after we started our work.

Base saturation by the sum of bases seemed to give reproducible figures for noncalcareous soils, but in many parts of the Great Plains, the soils were calcareous and the exchange capacity by that method was obviously unsatisfactory. We used then, in the soil survey laboratories, the sum of bases for the noncalcareous soils which were generally in the more humid parts of the

country. We used the ammonium acetate method for the Great Plains which had mar calcareous soils. We had troubles in making comparisons between the two methods. Tl numbers of data were quite limited in published form. Our data in the laboratories suggest that in Mollisols the base saturation by ammonium acetate never dropped below 50 percent. the humid regions, the base saturation by either method was frequently well below 50 percer but if the soil had received applications of limestone, the base saturation in the epipedon w readily changed. We proposed 50 percent by ammonium acetate as a limit for the moll epipedon with the idea that the people in the agricultural experiment stations would go throug their unpublished data and criticize that limit. No criticism was ever received from any (them. This is true for most of the limits that you will find in *Soil Taxonomy*. The proposa that were not criticized were carried over from one approximation to another, and final became more or less entrenched in *Soil Taxonomy*. What the reasons were for no criticisms, do not know. It may be that the initial proposals, based on very fragmentary data, wei reasonable. It may be that there was simply a lack of interest at the agricultural experimei stations in going through their filing cabinets and digging out their unpublished data.

I can recall that I once, in preparing a paper for *Advances in Agronomy*, mentioned th: we had the percentages of carbon, but we did not have any bulk densities and we could nc calculate the amount of organic carbon in the soil per unit volume. The percentage values ai really inadequate in accessing the organic cycles in soil, because if you have a lot of coars fragments, you tend to increase the percentage of organic matter in the fine earth, but not in given volume of soil. After the article was published, I was told by one of the workers in th experiment station where we got the data that they had the bulk densities, but they had nc published them. The people who were at that moment at the experiment station did not kno that these data existed. Two of the joint authors on that paper were located at that experimer station. These data get lost in files very readily and this led to SCS policy that all the dat would be published if they covered a more or less complete characterization of a pedon. worked for many years at the experiment station in Illinois and there we had pages and pages o data and analyses, all unpublished. I spent the better part of two winters assembling those dat for publication before World War II. The assemblage was completed, but I have never seen an published data yet from the Illinois Experiment Station. They are completely lost in the file: **Question 61, Cornell**

The criteria in *Soil Taxonomy* that require laboratory measurement can generally, w hope, be inferred from our combined knowledge of soil genesis, climatology, botany, geology geomorphology, etc. Some few benchmark determinations must be made so that we know wha part of the universe the soils that concern us represent. If you have a pH above 7, you cai infer you have a high base saturation. If you have a pH of 4.5 you cannot draw the opposit inference. So, we have to have occasional laboratory determinations. We can have fiel portable laboratory measurements, as in the case of Dr. Fields's test for allophane. We hav developed and I presume there is still available for sale, very portable laboratory kits whicl permit the measurement of most of the parameters that we use in *Taxonomy*. We canno estimate the percentage of silt, sand, or clay. We cannot measure that readily in the field bu the field men, by having some laboratory determinations made and practicing at identificatior can do not too unreasonable a job of estimating percentages of clay, silt, and sand. So, if one i: working in a new area where we have no data and no experience, certainly one has to hav access to a laboratory or he has to carry his portable laboratory with him. I have had to do tha in some of the West Indian Islands. I needed to know what kind of clay I was dealing with anc why there were no determinations on that. So, I estimated the percentage of clay and we measured the CEC of the soil sample and the CEC was well under 18 meq. per hundred gram: clay and I said to myself, "That's kaolinite," and I classified the soil that way. But withou knowing the CEC of the soil and without estimating the percentage of clay that wa: contributing to that CEC, I would have had no notion about the mineralogy of the soils of one of the larger islands in the Caribbean. **Question 172, Cornell**

When you go to a higher categoric level, one above the family at least, relatively much less information is required about the soil, in terms of quantitative laboratory information. One map at the subgroup level can be made with relatively little quantitative information. That which is required above the subgroup level can generally be inferred in some very simple measurements that can be made in the field, or, if one requires something more sophisticated, I

hope we still have this portable laboratory, about the size of my briefcase. It can be taken to the field and will make most of the measurements that are required for a classification at the subgroup level at least. **Question 32, Minnesota**

Lack of laboratory information may be a handicap at present. It is one that can be resolved, I think, without too much trouble. If one insists on classifying soils without knowing anything about them, that is his business, but his classification is no better than his mouth. And it will be thrown out just as soon as they find someone who's willing to acquire that information. Most classifications, early ones, have placed great emphasis on color because that was something that could be seen. Not consistently, because what is brown to one person is yellowish brown to another and so on until we got the Munsell color standards. Now one can arrive at a defined nomenclature for color. The human eye is variable. If there is a serious dispute about the Munsell value it can always be measured in a laboratory but these laboratories don't exist in developing countries. I think we have greatly de-emphasized color although there was a non-pedologist at Lubbock who thought we over-emphasized color, but he didn't know the emphasis placed in Russia and France and Germany on color. **Question 33, Minnesota**

There has to be a limit to the quantity of laboratory data needed to classify soils. I use the example of the point of zero charge as an expensive laboratory measurement, and I would not actually want to bring that into *Taxonomy*. They are working in Hawaii on a relatively simple laboratory method that approximates the point of zero charge. **Question 98, Minnesota**

You must always remember that there are two sources of error and you must consider the magnitude of that error in making a decision. The one is in the laboratory and the laboratory people know pretty well what this amounts to because they can and have run duplicate samples a number of times. They know the variability that they get. What they don't realize is that there's also a sampling error. And you may not pick the best sample for them to study. They assume you did. When I was at SCS we always tried to have someone from the laboratory present if there were a major study involved but we permitted the fieldman to send in samples for dual analysis. In this case, you might ask comparison between two samples, A and the B. We do know that, in the studies we've made, where we have a laboratory man present that the sampling error is appreciable. Two samples from the same pit may differ by 3 or 4% carbon.

In sampling Aridisols where the ratio of carbon is varying with respect to the sand/clay ratio, we've collected a number of satellite samples to find out something about the variability of organic carbon within short distances. It is very large: a difference of .1 in the pit against .3 or .4 on a composite sample collected at a radius of about 5 meters from the pit. If you relied exclusively on the sample that came from the pit you'd be neglecting the probability of a sampling error. It's quite common in the Aridisols, where much of the surface is exposed, that you will get very different conductivity under the plants, very different sodium adsorption ratios, than you get in the bare ground between the plants. It is a tendency of people to avoid sampling under the plants. It's more work to dig there and to sample there than it is on that beautiful bare ground between. **Question 183, Minnesota**

1.12.1 Laboratory Methodology

Most of the CEC data in the world as a whole has been made by ammonium acetate. Data on the effective CEC are not yet very common. The sum of bases plus aluminum are about the best approximation of that and again, many laboratories have not bothered to measure aluminum. Europeans, particularly, have been concerned with iron but never have looked at aluminum. I suppose that's because it has no color. They're getting interested now in this concept of using base saturation by sum of bases plus aluminum. We may get additional data in the not too distant future on that subject but the numbers of data are still quite small in the western European countries. I think it would be much simpler if we have a standard method for all kinds of soil if that method is applicable. I'm not sure about ammonium acetate or about sum of bases plus aluminum in calcareous soils; how reliable that might be. I believe at one time in the laboratory you had a method for measuring exchangeable cations in calcareous soils in the *Lab Manual Number 1*. It is there but it is a little more difficult than we had thought so

we aren't using it. In any case the procedure was developed out at Riverside and used for a while and then we stopped using it partly because the time requirement and partly because we weren't getting what we thought it would do. Right now we don't have a standard procedure in our laboratory to get at the sum of exchangeable cations in the presence of carbonates.

It seems to be rather difficult to use a standard method for all soils. There are some complaints about our exchangeable sodium, for example, in saline soils. A correction we make is for the sodium in the saturation extract, but there are people who question the reliability of that. Maybe this would not constitute a serious problem because the calcareous soils wouldn't be present in a great many orders. You won't find them in Spodosols or Oxisols very often. Theoretically, they could occur in an Oxisol as a result of recalcification though I haven't seen it. If we have too many methods it confuses the students. It increases the cost of equipment necessary to make the determinations. If there was some way to substitute sum of bases plus aluminum for another method in calcareous soils, I'd say our problem's solved. But I don't know at this moment how one would manage to come down to a single method. It's perhaps a little bit like organic carbon. Commonly this is measured with acid dichromate. However, when you get into soils with appreciable sulfides this breaks down completely, because all the sulfides come out as carbon. You would have to use then, perhaps, a gravimetric method for oxidizing carbon with dichromate or by combustion. They should be very similar. But only the gravimetric method then could be used and people object to that because they say it is so time-consuming. **Question 151, Minnesota**

Some have suggested that effective cation exchange capacity be used as a basis for divisions of some classes. There is no question that some changes are coming in this direction. *The International Committee on the Classification of Soils with Low-Activity Clays*, on the Oxisols, on the Andepts, are all considering these problems. At the time that we were working on *Soil Taxonomy*, many of these properties were not well understood, and many of the things the chemists talk about still cannot be measured conveniently - point of zero charge, for example. There is no reasonable procedure for determining this, that is practical. It is just too expensive to do on a great number of samples. If you have no data on your soils, you can't propose a definition and consider what changes it's going to make, because you don't have the data to see how they fall under any proposed definition. While there has been considerable discussion about using point of zero charge, it's just not possible at this moment. Somebody may someday devise a reasonable method for estimating it, but to actually measure it, so far as I know, is always going to be very difficult and time-consuming. We don't have the laboratory money for that sort of thing, particularly in the countries where it is important, the developing countries.

We have the further problem in developing *Taxonomy* that we were not allowed to split series. I wanted, at one time, to use CEC, admittedly, buffered at pH 7, in some of the definitions of the soils of the Southeast. But if we did that we split the Ruston series in two or more, because in the Mississippi Valley the CEC is influenced by a bit of montmorillonite dust blowing around, and your CEC per 100g clay there is in the neighborhood of 30 meq or more per 100g clay. The same series on the Atlantic Coastal Plain runs about 6 meq. Now, the correlators would not agree to split those series, and it couldn't be done without their approval. We have Prof. Buol who has been bringing this up at the Southern Regional Work Planning Conferences year after year, and he may get it through in a couple more years, that the Ruston and Norfolk series should be split, because their management requirements are conditioned by the activity in the clay. The use of the sum of bases plus KCl-extractable aluminum is a potential substitute for CEC by ammonium acetate or by sum of cations. That has been used to some small extent in *Soil Taxonomy*, particularly with Oxisols. The three international committees that are examining these problems include a number of chemists, as well as field men. They are corresponding with each other and precisely what they will finally come up with is unpredictable to me. **Question 11, Texas**

It sometimes happens that there is excessive dependence on the laboratory. I think it's perhaps a normal tendency, but one that should be resisted. It surely is characteristic that the laboratory men have full confidence in the field men. And the field men have full confidence in the laboratory results and believe each other but one field man doesn't necessarily believe another, he knows the potential for error. **Question Minnesota, 184**

1.12.2 Selective vs. Random Sampling

If I had relatively unlimited funds for sampling in laboratory work, theoretically I would prefer random sampling, but we do not live in that sort of an environment. We like to have more than one sample of a particular series. They used to require that we have a minimum of two pedons from different polypedons and these be matched as closely as possible. We waived that requirement if we were sampling a transect where we ran across one kind of soil to another. So the transect sampling is perhaps closer to random sampling and not nearly so expensive. The requirement for attempting to match two pedons also gave us some element of quality control for the field work, because if the samples matched very badly we had every reason to be suspicious of the quality of the work that had been done in that particular survey area. **Question 130, Cornell**

In sampling the deep horizons in the pedon where we have no reason to think there is any significant variation, rather than dig a deep hole we may sample with an auger. If, however, we examine the pedon while we are excavating, we see that there are or there are not significant variations within the pedon. If there are, then proper sampling requires that we subsample each different kind of profile within the pedon. This often has not been done, but in a number of cases it has been. In a mottled horizon, they sample the gray parts separately from the rusty brown spots to measure free iron and so on. These are subsamples to reflect the different kinds of features that we find within the pedon. As a general rule there is not much variability within the pedon. That is the exceptional situation where you must sample separately. It is more common in Spodosols perhaps than in any other kind of soil. **Question 34, Cornell**

1.13 Buried Soils and Depth of Soil

It was assumed in the discussion of buried soils in *Soil Taxonomy*, that the buried soil was covered by a mantle of largely unaltered materials because we specify that it normally shows fine stratification; it would, therefore, be quite a recent deposit. We would find it on flood plains, say, where a dike has burst, or near volcanoes where there is a mantle of very recent ash or pumice, or in areas where dunes are moving across the landscape. These were the things we had in mind. It is certain that the discussion can be improved considerably to draw the line on what is largely unaltered. The presence of an argillic or a spodic horizon would seem to be clearly eliminated. The presence of a very weakly developed cambic horizon of course, could be tolerated as a part of a recent mantle, because we surely can develop the cambic horizon in places, given the proper environment, in something like a matter of a hundred years or so. The definition of a buried soil in the *Soil Survey Manual* is really a statement that the man who is describing the soil makes the assumption that the material at the surface is of another age than the underlying material, and that the horizon, then, in the underlying materials are indicated by the subscript little "b" in the horizon designation. It is stated clearly that this is the interpretation of the man describing the soil, and that the confirmation of his interpretation may later require laboratory analyses to validate his opinion at the moment that he describes the soil. The intent was to include only those mantles that had no diagnostic horizons other than an ochric epipedon, and many would hardly have that if they were finely stratified. It would have no epipedon, in fact. We had in mind materials that were that recent. The definitions of Inceptisols and Entisols states there is no argillic horizon unless it is a buried horizon. The thought was that the new material would be new enough, recent enough, that there would be no diagnostic horizon and that the buried soils would occur only amongst Entisols. **Question 7, Witty & Guthrie**

1.13.1 Thickness/Depth Criteria in Buried Soils

The recent mantles are normally from alluvial or aeolian deposition on a preexisting sc We have a lot of new ones today somewhere in the neighborhood of Mt. St. Helens. When ı we classify the soil on a basis of a buried soil or on the basis of the surface mantle and treat as an overwash or overblown phase? You have to have some rules. We did consider that ı could normally disregard in *Taxonomy* a surface mantle of 10 or 25 or 30 centimeters and tre it as a phase. But what would be the maximum thickness at which we would be unable to tre the soil as an overblown or overwashed phase and have to treat it on the basis of the properti of the new mantle. We needed some sort of sliding scale according to the strength ı development of the buried soil in flood plains, in rivers, and in soils from volcanic deposit You normally have a succession of buried soils, all weakly developed, but still apparent in tl field. So the sliding scale that we proposed was the one that is questioned. There were ı criticisms of that and again the original proposal which was arrived at by discussion principal of the Washington staff has come down in print in *Soil Taxonomy*. **Question 80, Cornell**

One of the changes I have proposed is to handle an overburden of pyroclastic materi separately. I ran into a situation for the first time where I had a thin mantle of pyroclast materials over a buried soil. Under the conventions of *Soil Taxonomy*, if that mantle were le than 30 cm, we would invariably disregard it except at the phase level. It also so happens th with that mantle over the buried soil, we have an organic carbon value that decreases irregular with depth, which we use at the suborder level to classify a soil as a Fluvent. So I found situation where, on the ridge, this mantle persisted and we had Fluvents on the ridge. On tl side slopes the mantle had been removed, and we had an Orthent or something else. So we ha the Fluvent at the high point, the Orthent below it, and then down below on the lower grour we went back to Fluvents again. This wasn't the intent of the definition of the Fluvents. Yc must have the same thing around Mt. St. Helens today. So I proposed a solution to this in or of my letters to the correlation staff. It's irrational; it was not foreseen. **Question 17, Texas**

On rereading what *Soil Taxonomy* has to say about the use of *thapto* subgroups, it seem clear that more could be said about our intent for its use. We have only a few thapto subgrou that we accepted in the U.S. and these all involve buried Histosols that came within the contr section of alluvial derived soil. The use of the term 'thapto' is discussed on page 88 (*So Taxonomy*) under the heading "Names of Multiple Subgroups Intergrading between the Tw Given Great Groups". The concept of a thapto subgroup was that of a particular kind of a intergrade although the name is listed in the table of extragrading terms, the footnote says th the thapto subgroups are not strictly extragrades. With this concept of thapto subgroups as special kind of intergrade between different kinds of soil, the thapto subgroup cannot be used i the buried soil has the same classification as the soil at the surface. It would be absurd to hav a Haplargid that intergraded to a Haplargid, so that a Thapto Haplargidic Haplargid would b an odd name. We, therefore, have generally kept the use of the properties of a buried so either at the family or at the series level provided that the presence of the buried soil wa relevant to some purpose of the soil survey. If the buried soil has a strongly contrasting particl size then it would normally show up at the family level. This would be in line with the rule for showing particle size in the family level. If the particle-size distribution of the buried so is so similar to that of the surface soil, that is the modern soil above the buried soil, then i would be possible to show the presence of the buried soil at the series level. This should b done if the buried soil has some relevant effect on the intended purposes of the soil survey However, our purpose for making a soil survey is rarely to show the geomorphic history of th soil. It is not uncommon in arid regions that we have an Aridisol buried by another Aridisol and one would have to be making a special sort of survey with special purposes to find thi relevant to show at a very high categoric level.

Similarly, the soils formed on volcanic ash and pumice normally have buried soils; ver frequently one or more within the control section that we use for the Andepts or Andisols Here if the buried soil is another soil formed in ash, the family level permits us to show th contrasting particle-size distribution and the series level would permit showing a buried A. Bu as we find this to be almost normal in soils from volcanic ash, we have generally kept sucl

differences at the series level if they were relevant or, we have disregarded them completely. **Question 4, Leamy**

I have been asked how I would classify a soil of the central Venezuelan Llanos that has an aeolian mantle of coarse sand from 50 to 100 cm. thick, lying on a buried soil that could be classified as a Tropaqualf. The Tropaqualf would come within the definition of *Soil Taxonomy* of a buried soil, so the classification would rest on this surficial mantle of sand. There are no horizons in this surficial mantle so it would go into the order of Entisols, but the sand is less than one meter in thickness so it would have to be placed in the suborder of Orthents. The distinction here would primarily be at the family level where the particle-size class would be sandy over something else. Pending on the nature of the particle size of the buried soil. It could not be considered a Psamment because the deposit is less than a meter thick and the sandy texture, therefore, does not extend to a depth of one meter. The problem of using a thapto subgroup would depend on the importance of the nature of the buried soil. If one had a variety of soils that were buried, as for example a Tropaqualf on one place and a Tropaquept in another, and it was felt that the presence of that buried argillic horizon was critical to the use of the soil, then a thapto subgroup might be considered. In this case, it might be a Thapto Aqualfic Ustorthent. It's a Thapto Aqualf. This subgroup then not having been recognized in *Soil Taxonomy* would need to be proposed and a definition written that would include it and would exclude it from the Typic Ustorthents so that modification would be necessary in the definitions of the Orthents.

The comment is that if one goes through the key, this soil would not be an Orthent, but would be a Fluvent because the organic carbon decreases irregularly with depth. The reply to this comment would be that it would be wrong to consider this soil a Fluvent, because it happens to be a buried soil. If the text of *Soil Taxonomy* is vague on this point, then it does need to be clarified in the text that the buried soil in this situation would not make the other soil a Fluvent. We have had similar problems in New Zealand and in the U.S. now where we have a pyroclastic mantle resting on a buried soil. The mantle perhaps being one year old or 50 years old, has no horizons but the buried soil below it is high in carbon and creates a situation where using the carbon of the buried soil, puts Fluvents on the tops of the hills in New Zealand and Orthents on the slopes. Some changes are definitely needed in the text of *Soil Taxonomy* to clarify this situation. **Question 10, Venezuela**

1.14 Nomenclature – Naming of Mapping Units

In the earlier classifications, there has been confusion because the same names were used for quite different kinds of soils in different countries. For example, the podzols of Russia were not the podzols of western Europe or North America. The difficulties arose from the lack of definitions that could be interpreted in the same manner in different countries.

It would be my personal preference that if one does not like the particular taxa that are defined in *Soil Taxonomy* such as a Typic Hapludalf and one wants to define another sort of taxon that would include some Typic Hapludalfs and perhaps some Mollisols and so on, one should take a new name for such a taxon rather than retaining the presently defined name and presenting a new definition for the same name. From my own experience, I would much prefer to present a new name for a new taxon. Any decision to give priority to nomenclature in pedology would require some sort of international agreement by the International Soil Science Society. I am only one member of that Society and I may not dictate what it decides to do but to this date, there have never been to my knowledge proposals to give priority to particular names. The FAO/UNESCO legend for the soil map of the world uses a few of *Soil Taxonomy*'s names but for the most part uses its own names. While the legend for a soil map such as the FAO/UNESCO soil map of the world is not itself a taxonomy, still the units are named as though it were a taxonomy. If one wants to give priority to nomenclature, then the FAO/UNESCO legend would have been impelled to use the soil names in *Soil Taxonomy*, but for the legend this might not have necessarily been convenient or useful. It would be my own opinion that if one establishes a system of priority, it would be a serious mistake at this moment

because it would tend to freeze the existing names and would prevent changes in definitions. **Question 44, Leamy**

The terms *orthic* suborder, *haplic* great group and *typic* subgroup do not imply a standard of comparison within the taxa at the next higher category. In general, the orthic suborders represent something about the relative extent of the soils. The Orthods, for example, are the most common ones in our present experience. The Orthids, likewise, represent the most common soils in Aridisols. The "hapla" formative element means simple. The haplic great groups have the fewest horizons required to place the soil in that particular order and suborder. They are not necessarily the most extensive; they can be, but they merely represent the minimum of horizons. For example, the Hapludalfs have an ochric epipedon and an argillic horizon and nothing else in the way of diagnostic horizons. If you find a fragipan below that, that is an additional horizon, and is placed in the Fragiudalfs instead of Hapludalfs because it takes the three horizons to get into that. So "hapla" derived from "simple" in Greek, and it means that it has the fewest diagnostic horizons. The typic subgroups are defined in terms that permit us to show relations to other great groups in that particular great group or in some other great group. The typic subgroups may not be the most extensive; in several instances they are relatively inextensive, but they permit the definition of intergrade and extragrade subgroups with the simpler nomenclature. For example, the Typic Cryaquepts do not have permafrost, do not have a pergelic temperature because it is so much easier to intergrade or to make an extragrade of the soils that have a pergelic temperature and permafrost than it is to find an intergrade for the ones that do not. The nomenclature becomes very complicated and we were striving for the simplest possible nomenclature, with the fewest possible combinations of formative elements.

The terms "aridic" and "torric" indicate the same moisture regime but in different categories. This is because it was the structure of the terminology. We did not want to repeat the same formative element in different categories, because then we found when we got to the subgroup, we had intergrades in which we had to repeat that formative element twice. This was unsatisfactory. **Question 63, Texas**

Some pedologists have proposed that variants of limits, consistent with the probable error of their estimates, be permitted when properties are considered in combinations for placement of taxa. This again is a problem of the naming of the mapping units in my opinion. Certainly if the mapping inclusions, based on criteria of *Soil Taxonomy*, are within the range of the probable error of the estimates, I would not consider that this would be a serious problem. If the field man is unable to determine, say, the percentage of silt or clay within the range of 2, you must be prepared to tolerate that sort of inclusion. He is doing the best that can be done, and this does not detract from the utility of his maps. To some extent in the higher categories, we have done precisely this, permitting more variability in a given property when it is associated with another property. Many of the complications in the definitions of taxa are there simply because we have natural groups of soils that we want to maintain in a single taxon. An example might be the Glossudalfs; those in the southern Mississippi Valley and in Belgium have a range in base saturation that covers the boundary between Alfisols and Ultisols. So the definition of those two orders is complicated by the necessity to waive the base saturation, provided you have other characteristics which are specified. If you do not specify these, then you do not have a taxonomy. The range that is desired in one place may be entirely different than that in another, and this is going to complicate your correlation problem; it throws *Taxonomy* completely out of control. It was a correlation problem that caused us to feel that we had to have some system of classification that could be applied uniformly. So the office of the correlator has to put down some general rules that are supposed to be followed, and these rules are going to vary over time with different correlators. **Question 128, Cornell**

Currently in the U.S. soil survey, taxadjuncts are the main device to handle the problem of homogeneity in mapping units. I was involved in the development of the present practice before I retired but have had little or no opportunity to keep track of what has been done since then. **Question 134, Cornell**

Depending on the uses of the soil, those that can be or are foreseen to be made, we do need to know what variability we have within the area around which we draw a boundary in

the field. The estimation of that variability by sampling on a transect is not exactly new, but on the other hand, it was not done 50 years ago. It has spread gradually in the last 20 or 30 years to find out what variability we have, either by transect or by random sampling. It is fairly important in many surveys that we know something about this before we assign a given name to the map unit. There was a time when I first started making maps that we did not worry about this. We drew a boundary and then never went back at another date to see what was in that boundary. Our boundary was drawn on the basis of a couple of samples of auger holes, and instead of really boring it out, a random pattern or a transect pattern, we just assumed it was uniform. Then when people began to study this variability, we discovered that we were not as good as we thought we were. Many areas named for a series should have been named for an association of series. There were significant inclusions of soils that behaved differently. We have the rule that we can tolerate some small areas that have very different interpretations from the series or family or whatever we name the map unit for, though we like to tell people that if possible these inclusions should be designated by a spot symbol of some sort, just to warn the user that it is not homogeneous.

Our taxonomy is still a rather coarse grid compared to what the farmer sees on his farm. He always sees much finer differences than the pedologist can put on his map. **Question 135, Cornell**

1.15 Geomorphology and Soils: Landscape Relationships of Soils - Slope, or Shape of Soil; Cryoturbation.

The impact of geomorphology on *Soil Taxonomy* has been much less than it has been on soil mapping. The classic concept of down-wearing of surfaces versus lateral retreat of slope has considerable influence on what the mapper does in the field, but it hasn't had a great deal of influence on *Soil Taxonomy* itself. The geomorphic studies have had their principal impact in the "pale" great groups, in which we tried to distinguish the soils of the very old from the younger geomorphic surfaces. We have grouped somewhat unlike things in "pale" great groups as in the Aridisols where we use the petrocalcic horizon and abrupt textural change to define the Paleargids. This gives us two unlike kind of things at the great group level which must be separated at the subgroup level. Then we have the Petrocalcic Paleargids versus the Typic Paleargids where we settled on the abrupt textural change and the fine texture for the Typic. I can't think of any evidence of where soil geomorphology studies have greatly influenced *Taxonomy* other than that it helped us understand what we already knew about these soils. How does the petrocalcic horizon form? What is its genesis?

Our soils-geomorphology studies may have had some impact on pedologists, but I have no specific comments on that. I have not been attending the various excursions and meetings of the geomorphologists for a number of years and I don't know what the impact has been. I do know that in the soils-geomorphology studies that were conducted in Soil Survey Investigations we were trying to relate the nature of the soil to the geomorphic history. It seemed essential there that we not use circular reasoning, but that we establish the nature of the geomorphic surface first and then relate the nature of the soil to that. With a reasonable number of such studies, the nature of the geomorphic surface can be then identified by the circular method of extrapolating from areas where the studies have been made to unstudied areas, using the nature of the soil to indicate the geomorphic history. And I would think that would be a fairly promising thing, provided we have the basic studies first.

Shaw's classification of soils according to stages of development (*First International Soil Science Society Conference* 4:291-317, 1928) probably had a distinct impact in California. But not as much as it probably should have had because when I first visited California to study the Non-Calcic Brown soils I was shown the same series with and without a duripan. It was treated as a phase rather than as a series differentia. In the Middle West the studies of development of the argillic horizon in soils formed in loess led to very much the same sort of concepts that Shaw had, namely, that you have a continuum between the Hapludolls, the Argiudolls and the Albolls, and that this was split into segments in the Middle West. It was this study, actually,

that lead to the use of the various properties that intergrade between one great group and another at the subgroup level. It could not be shown at the great group level but it seemed important to show this at some categoric level and the subgroup took over for this function. But I don't recall if Shaw had anything of this nature in his classification, he merely used the presence or absence of these overly developed horizons on the higher terraces and their absence on the lower terraces to show that these differences were due to the time factor, but I don't believe you can find the idea of an intergradation, in Shaw's papers. **Question 99, Minnesota**

I know that there are places in the world where the intent is to map the geomorphic surfaces, and then to sample those to find out what kinds of soil are present from one surface versus another. But those are not really soil maps, they are geomorphological maps, and they are being interpreted in terms of the kinds of soil that is found. But their boundaries have nothing to do with soils, and perhaps they are useful in some parts of the world where the variability in use is limited by some factor, such as an arid climate or a very cold climate. **Question 140, Cornell**

It has been suggested that *Soil Taxonomy* could help to predict the movement of water and dissolved salts within and between landscape segments. I think, by and large, that this interdependence that is mentioned is something that requires some very detailed geomorphological study. The things that are apt to move in the landscape from point A to a lower-lying point B are either the water or something dissolved in the water. I don't think that *Soil Taxonomy* is going to be of any great help in working out these relations. I think they are going to have to be drilled out and sampled, measured. It is going to be a rather expensive study and will not contribute a great deal to anything other than the understanding of the genesis of the soil. This may prove in time to be more helpful than we might think today. But until we have a few more of these studies I would have to keep an open mind on it, on whether they are worth it but I don't think *Soil Taxonomy* will be particularly helpful. **Question 119, Minnesota**

The question has arisen, "Is the emphasis on morphology at the expense of landscape a problem in the application of *Taxonomy*?" I think the basic answer is one that is related to another question that has come up repeatedly, what are we classifying, pedons or polypedons? The polypedon is a landscape in the sense that it has shape that the individual pedons do not have. It has transitional borders to other polypedons and it has natural borders. The pedon does not have a natural border, it's shape may be very different from that of the polypedon in which it belongs. **Question 120, Minnesota**

I used to believe that, let's say a given series that occurred in one area on the level divide and, in another area, on the sloping interfluve -- this difference in position in the landscape indicated some serious difference in the behavior of the soil or the genesis of the soil, and I always felt that this required two series. I think that the Director of Correlation and Classification had pretty much the same attitude so that discussion of the landscape of a given series, I would consider to be quite important. I'm thinking of the old Clinton series in Iowa and Wisconsin and Illinois, where, in some parts of that loess-covered area, the Clinton, which was supposed to be mottled at fairly shallow depth and have some drainage impedance, had those mottles because of the slow permeability of the argillic horizon. In other places on the flat landscapes it could have had those mottles because of a fluctuating water table where there was no possibility of surface drainage.

The identification of a single series in two or three different landscape positions suggests that neither the genetic nor use relationships of the soil have been sufficiently studied.

I was shocked when I first discovered that on the Great Plains there was a series that ranged from the western to the eastern side of the Great Plains. On the western side it was in depressions, on the eastern side it was on moderate slopes, in the middle it was on the high flats. It didn't seem to me that that was a single series, although there were morphologic resemblances. Landscape positions were completely different. One soil received runoff; one soil lost water by runoff; and the third one had to dispose of what fell on it. **Question 122, Minnesota**

1.15.1 Slope or Shape of Soil

I would be a little slow in accepting a proposal to eliminate the sloping families of the aquic great groups. The differences in normal sloping phases are not so much in the nature of the soil as in the hazards of erosion. The differences in these sloping families are not concerned with erosion, but are concerned with the difficulty of removing the surplus water, almost the impossibility of removing it, and the genetic differences in the ground water levels. The normal users of the soil surveys have associated sloping phases with the problems of soil management related to erosion. They could easily be confused by the use of the sloping phase where the problem is almost completely another problem, one of drainage. The differences in the genesis, of course, are related to the fact that the water in the sloping phases is coming from seepage, rather than from the rain that falls directly on the soil. *Soil Taxonomy* states that they should not be used in Aquods where in many soils the wetness is due to a placic horizon, or in the Albaqualfs, where the intent was to keep the old clay pan Planosols together. I should also comment that I think it would be desirable in the case of the Histosols to use sloping families, as well as in the Aquolls and the Aquults. Whether or not sloping phases of Aquults exist, I do not know at this moment, I have not myself seen such. **Question 3, Witty & Guthrie**

1.15.2 Cryoturbation

The men working in the arctic region, are not making very many large-scale soil surveys in the arctic region. It should serve no purpose to make large-scale surveys in areas of the sort described here. We discussed the possibility of broadening the definition of Vertisols to include those where the churning was due to frost as well as due to shrinking and swelling. Nobody on the staff seemed willing to accept this as a valid classification. They felt that the ruptic subgroups would permit ample recognition of the affects of cryoturbation. The principle proponents of this sort of thing generally are geologists rather than pedologists.

You have to have stones in order to see the effects of cryoturbation--stone stripes and stone polygons and so on. On the other hand, you can have cryoturbation in uniform textured materials, in that you have two possibilities. You may have a histic epipedon, or even a peat which may be either at the borders of the polygons or in raised centers. It can go both ways in the absence of stones. **Question 55, Cornell**

In some serious situations you may have, say, an Aquoll on a side slope where there is seepage water coming out, or you'll have a Histosol on that side slope where it has still more water. The drainage of these soils engineering-wise is entirely different from the drainage of the others, and it is a difference of such magnitude that we thought it should come out at the family level instead of the series level. **Question 47, Texas**

1.16 Soil and Engineering Interpretations - Impacts of Agricultural Interpretations on the Structure of Soil Taxonomy

Interpretations were the major control in design of *Soil Taxonomy*, the major control at the family level and the series. We would like to have as many interpretations as possible for each taxon. While we can make some statements about Vertisols and Alfisols, there is no statement we can make about Entisols. That is a taxon about which you can say nothing of any importance, just that they don't have horizons, and what does that mean? Nothing. **Question 14, Texas**

The interpretative value of the higher categories, the great group, suborder, and order, is not great. The use of soil moisture and temperature in the definitions in these categories does

give us some control over potential uses. We can make statements about the benefits that we can expect from following the Ustolls or Xerolls with mesic temperatures. These can be more quantitative than the interpretations that we used to be able to make about Chernozems and Prairie Soils, where the Prairie Soils included the xeric soil and the udic soils. In general, I am not sure that I can give many other examples of how *Taxonomy* has improved interpretative values for higher categories, but you can say about Xerolls that without irrigation you cannot grow summer crops. You could not say that about Prairie Soils, because they were combined with xeric and udic moisture regimes.

We have subdivided the old great soil group of Planosols, according to the nature of the pan, and according to the soil moisture and temperature regimes. This does permit better interpretations for Durixeralfs for example, with a mesic temperature. The interpretations would be quite different from those of an Albaqualf in a humid climate. But in general, the interpretations that we make are mostly for large-scale maps, certainly 1:1,000,000 or larger. At the 1:1,000,000 scale, the numbers of interpretations are rather limited because of the heterogeneity of map units and the specific interpretations at the great group levels are difficult to quantify. One can generally, though, make some interpretations at the great group or higher level. If we consider the presence or absence of a fragipan, which is reflected in the taxonomy, you can say two things about that: (1) it is going to make troubles for highway construction, and (2) it is going to make troubles for urbanization of areas with the use of septic tanks. You can forget septic tanks in these soils. But it is not easy to specify whether those are going to grow 30 or 100 bushels of corn with proper use of fertilizer without the introduction of a rather complicated phase terminology. **Question 148, Cornell**

Interpretations at any categoric level are normally made for phases of taxa in that category. We cannot say that Mollisols are suited to cultivation without specifying something about the slope, and we cannot say that Aridisols need irrigation without phasing again, because if the soil properties are not suited for cultivation, then the irrigation is impractical. They do not need it unless they are going to produce a reasonable crop after irrigation. We can say that an Aridisol cannot be successfully cultivated unless irrigated; we can say that a Mollisol can produce some sort of vegetative crop, but not which one, unless we specify the slope. Then we still, for any quantitative or qualitative interpretation as to what kind of crop, have to then come down below the order level to bring into our interpretive information the nature of the soil climate. The Mollisols of the valley must be irrigated for summer crops, but they are commonly in use to produce grass seed, without irrigation. The Mollisols of Iowa may produce grass seed if the slopes are steep, but if the slopes are suited for cultivation, they primarily are in grain crops, and the yield will depend on the properties at the subgroup and family level, more so than on the great group. For the precise quantitative interpretation, one must get the phase of the series. **Question 149, Cornell**

Perhaps we give more weight to agricultural interpretations in higher taxonomic classes than we do to urban or other kinds of uses, but I am not convinced we should. We give attention to the foreseeable uses of the area that we are mapping at a large scale. It is true that there are larger areas used for agriculture than for housing. But a foreseeable use requires the intensity of interpretation, whether it is urban development, highway, airport design, what have you. So we weight these interpretations according to the uses that we anticipate will be made of the soils in that particular area.

Aristotle said "It is as hard to unlearn as it is to learn." (*Politics of Aristotle*, translated by B. Jowett, Oxford University Press, 1885.) I should like to comment that for me it is more difficult to unlearn than to learn. One starts with preconceived ideas, and he must bump his head repeatedly against the hard facts in nature to realize that what he was taught is not right; that the truth lies somewhere in another direction. **Question 39, Cornell**

To get really good interpretative value from a map that includes rather heterogeneous kinds of soil, the basic problem is whether or not the soil variability is identified within those map units, to get some notion of the relative extent of the different kinds of soil within that map unit. From thereon, the interpretative value is partly a function of what is known about the behavior of the soil under another system of management than the one it is presently under. An area of Oxisols being farmed under shifting cultivation does not require large numbers of

interpretations, and they can be rather general. If, on the other hand, you are going to use that area for the production of a plantation crop, with a fairly high level of management, the interpretations will have to be a function of how much you know about the behavior of that soil. One purpose of the taxonomy is to let us extend the experience of a plantation to an area of similar soil that has been farmed under shifting cultivation. What will be, then, the affect of bringing this second area into plantation use as the first one, where we get our experience. Depending on the variability, then, and how carefully we record the nature and the aerial extent of the variations and on our knowledge about the soil behavior, we can make limited interpretations for areas of very considerable variability. *Soil Taxonomy* will not enhance the interpretations that you can make unless you are rather careful in your control of knowing what that variability is in the soils and their effect on the interpretations. **Question 145, Cornell**

Someone asked why we should not make some of the major categories purely morphogenetic. To a large extent, the three higher categories are reflections of the kinds of horizons and properties that the soils have. You could call that morphogenetic. When we get to the family level, it is much more for practical purposes, but it is not the only thing that affects the interpretations. The presence of a pan alerts us that this may affect our interpretation seriously. You consider a soil under forest with a fragipan and with an occasional hurricane going by, you realize that the forest may blow over, and depending on the frequency of hurricanes you may decide that this soil is or is not going to produce a certain volume of wood, because the marketable wood may not be produced due to trees blowing over too frequently. **Question 144, Cornell**

1.16.1 Profile vs. Landscape Aspects of Soil Surveys

In general, the man who is making the map is very concerned with landscape positions because he is going to draw boundaries on his map at these points. Where some genetic factor has obviously changed he can expect changes in the nature of the soil. And so if the ridgetops are long and narrow, he is limited in what he can show on a large-scale map by the breadth of those ridges and his boundaries are pretty well fixed by the land point. Having put that boundary on his map he proceeds to try to identify what he has drawn his line around; to find out the nature of the soil that has been bounded by that natural boundary. When one is writing about the soil survey for the general public, this is subordinated, the discussion of this disappears for all practical purposes except that we have slope phases. The user of the map is not able to identify immediately whether one delineation is on a ridgetop or on a footslope, below a hillside, or on the hillside. If he is using the map in the field this relation would become obvious to him very quickly. But for the most part he is not particularly concerned with the genesis of the soil. The user of the map is concerned with what we say about the use of the soil. These are our interpretations and he could care less, for the most part, about the taxonomic name of that soil, in fact he can't pronounce it. And he looks over the series and associations or complexes of series which are common names that he can remember. The interpretation, of course, requires, as Cline has pointed out, an additional step of reasoning from the nature of the horizons in the soil to the importance of this nature to the various uses - each different use that we can foresee. And the users of the soil surveys are concerned with these interpretations. If we don't make the interpretations, then we are going to stop making soil surveys very quickly because money is always in short supply in government and the ministers who decide what they are going to do with the money will stop putting it into soil surveys if people are not able to use the surveys. The use they want is the interpretation. So they are an essential part of making a soil survey. It's not finished until we have made the interpretation. And this is what our users are interested in and its why the soil survey in the U.S. is so well funded at the moment. We are making interpretations that really concern people who make use of the land. **Question 6, Minnesota**

1.16.2 Additional Parameters for Non-Farm Uses

In general, if we added additional parameters I think they would probably need to be for engineering interpretations. We would be competent, I think, to make our major agricultural interpretations for growing plants from the techniques we already have. To relate our classification to the engineering classification may be difficult and the trouble may be with either one of the classifications. I rather doubt that the engineers would be very interested in changing their classification. They are more inclined, as a rule, to consider that they have to sample their soils at fixed intervals in order to design a highway, for example, and I think they are probably fairly well content with their present classification. If they wanted to relate their classification to the kinds of soil as we see them, some changes might be necessary that would become very difficult. A few of the engineering experiment stations have compared the engineering classification with our detailed soil maps. Illinois is one state and, in general, they have concluded that they can use the soil surveys to enormously reduce the amount of sampling and testing that they have to do. It may be that there are other states in the Union in which this situation would not apply. I don't know. The Illinois engineering station studied the soil surveys in DeWitt County and Livingston County, one in loess and one in till, and their conclusions were that they could use these soil surveys to reduce the cost of planning. Michigan started much of this work many years ago making what they called agricultural soil surveys for engineering interpretations. It all started there.

There's a very large education job needed among the engineers, but it needs to be done by engineers. **Question 116, Minnesota**

1.17 International Acceptance of Soil Taxonomy

The Russians are now trying to stimulate the FAO and the International Society to extend their legend from the Soil Map of the World by adding two more categories. They had one meeting in Sophia last summer. How they are going to get along with that I don't know. The Russians have adopted, in principal, diagnostic horizons and indicate that they are willing to substitute soil moisture and temperature or climate and I think they will develop eventually a compatible system because their legend uses all *Soil Taxonomy* definitions for its diagnostic horizons.

They want to add two categories - the present orders as orders, but add two more categories. Because the way it now stands, if they map a cooperative farm in Russia, they can't use the FAO legend. It's only designed for a five millimeter scale map. And the five millimeter scale map on a cooperative farm is useless.

The French have a system that was taught in the French schools, but they have a soil survey of France now and it doesn't bear a lot of relation anymore to a system. It's a compositional classification similar to that of Fielde in New Zealand. First, he classified the material from which the soil is formed. He has about ten orders based on that. But, ORSTOM isn't going to buy his system. They had a meeting last summer amongst the ORSTOM people and they would not accept this. The French soil survey of France proper is in a ferment. The Germans have abandoned Muckenhausen's classification and are looking around for something to use.

Brazil doesn't use *Soil Taxonomy* officially, but they are well acquainted with it and use it in their work and, in conversation, look at the principles of it. In Brazil there are several organizations making soil surveys and some of them use *Soil Taxonomy*, but mostly they use the old Brazilian systems.

Australians rejected the classification of Stevens and they had Northcote develop a new classification which he did when he made his map of Australia. Then, while I was in New

Zealand, they advertised for a man to come to Australia to develop a new system of soil classification. They hired a soil chemist from Aberdeen whose experience in classification has been lacking. What they will come out with, I don't know, but I'm dubious about what they'll accomplish. When we had our meeting in Malaysia, he had an opportunity to come to learn something about *Soil Taxonomy*. But he didn't show up at all. We had Australians there, but not him. I know they are in trouble in Australia. New Zealand is trying *Soil Taxonomy*. The Soils Bureau in New Zealand decided they would use *Soil Taxonomy*. Some of the old timers are opposed to it, it's natural. Presently, younger people at the Soil Bureau are just going to have to work at it. There is no way around it.

Tedrow is never going to accept it in New Jersey. At least he no longer has any responsibility for soil surveys, so the state college is using *Soil Taxonomy*. When Sam Obenchain retired, his successor immediately adopted *Soil Taxonomy* for teaching. Sam never would mention it.

Reading about the British statistical approach to soil survey, I have a hard time seeing how it might fit into practical use. I cannot imagine how it's going to work. You must remember that the Soil Survey of England and Wales was located at Rothamsted. The emphasis was on pure science, pure research. And at least one director of that survey retired because they would not allow him to make interpretations of the soil survey. That wasn't pure research, that was applied research, if he made interpretations. So if you are not trying to make interpretations, you can make soil surveys. **Question 11, Minnesota**

For some years we tried very hard to get the Canadians to cooperate with us in the U.S., to develop one system for the two countries. And I thought for a while we were going to do it, but I wasn't at the meeting of one of your work planning conferences, in the prairie provinces, where one of the Canadian fire-eaters got up and said, "When are we going to quit copying the U.S.?" and that carried to date. We have had no cooperation since then. **Question 12, Minnesota**

1.18 The International Committees

Under the stimulation of AID, which is attempting in one of its major functions, to increase food production in the developing countries, the Soil Conservation Service (SCS) has established a number of international committees to examine the function of *Soil Taxonomy*, particularly in intertropical regions. It was impossible to spend much time in the study of these soils when *Soil Taxonomy* was being developed, because the appropriations to the department of agriculture are exclusively for the benefit of the American people. And studies of soils in the developing countries were intended to be for their benefit, not that of the U.S. We could not say that we were going to learn a great deal that could be applied to the soils of the U.S. by working with the people in Kenya or in Zaire or Uganda. We did examine the European soils rather carefully, and the European systems of classification, on the basis that these were advanced countries, that soil science has started there, that we could probably learn considerably from their experience with the European soils, and that we could transfer their experience to the U.S. if we had a system that was based on the soils of both continents.

The first of the international committees established under the chairmanship of Dr. Frank Moormann concerned the classification of soils with low-activity clays. They've been working now about 6 or 7 years on the classification of these soils which are extensive in Africa and South America, and much less so in the U.S., although they exist in the southeastern states. Most of the work with soil management in the U.S. concerned the soils of the glaciated regions of the U.S. There was relatively little work done with the soils of the southeastern humid, warm regions. The bias in *Soil Taxonomy* is strictly in favor of the soils that occur in western Europe where the last glaciation, Wurm II, disturbed virtually all the soils and left us with completely new surfaces to weather, and with similar soils in the northern half of the United States.

The committees have quite good international representation. The committee on the classification of soils from volcanic ash had about 75 people who indicated an interest in this subject. They came from virtually all parts of the world because the volcanoes don't much care where they erupt. The work is slow.

There is much dissension among the committees; there are always, on each committee, several people who want to scrap *Soil Taxonomy* completely and develop their own system. This is not in the mandate that has been given to the committees. They are supposed to suggest improvements with the minimum of disturbance to the structure of the system, though in no case is there ever going to be any unanimous agreement on anything. The report chairmen of the committees are going to be faced with the problem that I had in the development of *Soil Taxonomy*, that there was sometimes a consensus of agreement, but there were always vigorous objectors. How far *Soil Taxonomy* can be improved to make it an international system, I will not yet predict. I think that the functioning of these committees is going to go a long way towards gaining acceptance for *Soil Taxonomy* in the developing countries. It is not going to pacify the Russian pedologists who are attempting now to develop an international system, one that, they say is truly international, under the auspices of FAO. How far they will go, I do not know, but the Russians, at the first meeting to develop this international system, went along way toward accepting some of the basic principles of *Soil Taxonomy* that they resisted violently at the time of the International Congress in Bucharest. They have accepted now the use of diagnostic horizons and features as a basis for the new system, and it is very likely that anything that is developed will be compatible with *Soil Taxonomy*, so that it will be possible to compare *Soil Taxonomy* with whatever sort of system they eventually develop. They have had a distinct impact on the classification of soils in the more developed countries, where they have their own system of classification, as in France, Germany, Canada, Holland, New Zealand, Brazil, and so on. The classification is being reexamined in most of these countries, but not yet all of them. The previous classifications in France and Germany have been pretty much abandoned, and they are working now on the development of new systems which will probably be compatible, or more nearly compatible with *Soil Taxonomy* than the older systems. **Question 2, Texas**

We have at the moment six international committees at work, and AID funds them at least to the extent of one meeting a year in an area where there are extensive soils of the sort they are working on. Most of their work is by correspondence. But once a year, they are able to get together. The problem is getting the money from AID. And so they generally have about three weeks, one week of discussion of something like this, and two weeks out in the field where they can look at the actual soil and discuss things so that they can realize whether or not they are using the same language. **Question 14, Minnesota**

One of the committees is working on the classification of soils with low-activity clays - Ultisols and Alfisols and their clay minerals; one on Oxisols; one on Vertisols; one on Aridisols; and one on Andepts. There are two or three more proposed, but aren't yet organized. The committee on the reclassification of the Andepts into an order is chaired by Dr. Leamy in New Zealand. He has about seventy-five people from all over the world with whom he is corresponding. And they are trying to come up with an international meeting that is still at least two years away. The next one will be peripheral to Andisols, a meeting in a country, where there are volcanoes and Andisols.

The meeting for '82 was planned in Sudan. And AID funds the SCS soil survey laboratory to go to these countries a couple of years in advance and sample and analyze the soils where we have the meeting. Then we have all the laboratory data that is relevant to *Soil Taxonomy* on each profile. **Question 15, Minnesota**

Frank Moormann chairs the Committee on low-activity clays. They are due to submit their final report now at the meeting in June. And then the SCS will distribute that report and ask for comments within one year. And at the end of that year, depending on the comments they receive, they will adopt it, or adopt it with some modifications, going back to Moormann and his committee. It surely will go back once more with the comments that are received. Within about two years that report should be finalized. **Question 17, Minnesota**

SCS will probably rely heavily on the chairmen of the International Committees. There isn't anybody in Washington competent to consider whether or not to adopt, except as he relies on the Committee itself. But these are truly international committees with representatives from all over the world where there are such soils. The Canadians didn't get in on this low-activity clay business because they don't have any. International committees will probably get *Taxonomy* into international acceptance as much as anything we can do.

The scientists in the Benchmark Soils Project have laid out experimental fields on the basis of the soil family to see whether or not results within the one family are consistent enough that research experience can be transferred at the family level. For all the fine details, we have series, but still the general management of a family is supposed to be very similar. The Benchmark Soil Project is based at the Universities of Hawaii and Puerto Rico. The Soil Science Department in Hawaii has a newsletter that reports the news on this about four times a year, I think. They have sites in the Far East and Africa. They tried desperately to establish at least one in Venezuela, but Comerma was away and the people that were there refused to do a thing about it. They have some very nice places to set up stations in Venezuela, but they just didn't respond to Beinroth's influence, and so nothing is in Venezuela that I know of. But the University of Puerto Rico has some fields in Africa and in Brazil.

1.19 Revisions of Soil Taxonomy

An international agreement is needed to handle proposals and approval of changes in *Soil Taxonomy*. About the need for new subgroups or great groups or suborders, I have stated elsewhere that the decision is based on considerations of what soils belong together and that decision is based on both the similarities in soil properties and similarities in soil behavior. To amplify on the business of soil behavior I should like to comment that the interpretations are reflections of soil behavior. A significant difference in an interpretation for behavior under one management system or another or one use or another is the basis for a decision that the behavior is the same. If there is a difference in any significant interpretation under any management system or any use, then we must conclude that the soils do not belong together at some categoric level. The distinction may belong at the series level or the family level or a much higher categoric level, but the soils that belong together at a great group level surely cannot all belong together at a series level. These distinctions go by steps and the decision does require some judgment and it does require some sort of international agreement if we are going to have an internationally useful *Soil Taxonomy*. **Question 7, Leamy**

1.19.1 Concerning Documentation to Support Proposals for Changes in Soil Taxonomy

In general, I think that we should require a description of at least one pedon, a description of the extent of the soils that require separation, laboratory data on at least one pedon or on the critical parts of the diagnostic horizons that are used to propose new taxa. I think that there should be some discussion of the significance of the separation to the interpretations that might be made, and why a new taxon is required rather than a phase. The proposal, then, should include the data, the description, the differences in interpretations from other soils. If the soils under discussion are not known to occur in the United States, I believe the approval could be given rather readily, perhaps following the discussion with the principal correlators to confirm the absence in the U.S. If they are willing to say they do not know of such soils, then I think the decision to approve or disapprove should be made by the Washington Office people working in soil classification. If the soils do occur in the United States, we originally proposed that the suggestion should be sent to the principal correlator and discussed at the regional work planning

conference. The approval should wait on the discussion at the regional work-planning conference[2]. **Question 6, Witty & Guthrie**

The problem with updating *Soil Taxonomy* is not just the mechanism. It is a problem of people. When I retired we had one man responsible for soil survey operations, for correlation, and for classification, and he was a totally overworked man. We now have three vacancies to deal with these problems in SCS. So having the vacancy perhaps may be an improvement, but actually it is not until they get at least one of them filled. **Question 23, Cornell**

The present techniques then indicate that we should, when we find a defect in the taxonomy, bring it to the attention of the Washington office, through or around channels, it doesn't matter which. And there should be someone there to deal with it. At present we have no one to deal with it. That's about all we have had since *Soil Taxonomy* was printed. The suggestions or changes have piled up without anyone having time to pay attention to them. Dr. Arnold is aware of this problem. The solutions depend on the nature of the government administration, the desire to hold down positions, and the expenditure of money and what have you. What will be worked out, I'm sure he doesn't know at this point.

When I retired we had worked out a provisional soils memorandum outlining procedures for making changes. We know changes are going to be essential for at least one or two reasons. We find soils whose existence we never suspected, or we learn more about soils and we find that for our interpretations we must use parameters that did not occur to us at the time that we were developing *Soil Taxonomy*. I am personally of the opinion that these changes should be considered very broadly by a group of people or groups of people who have some familiarity with the soils that are under discussion or the changes that are under discussion before they are accepted. This is why we have these international committees working on necessary changes in kinds of soil that we don't have in the United States. Where the kinds of soils are well represented in the U.S. and in other countries and do not significantly differ from ours, I think that international committees are unwarranted.

Taxonomy was developed by, let's say, starting at the top. We in Washington would discuss these problems and we would put ideas together. I had the time, weekends, and no one else did, to write these approximations. Then we had them examined by the principal (correlators), the work planning conferences, regional and national. We had some special conferences for this. We involved people from the Forest Service, BLM, from the experiment stations, and from SCS on these committees. These people were familiar with kinds of soils to get the definitions written and knew about all kinds of soil. It was a group effort. **Question 28, Minnesota**

Concerning criteria for revising *Soil Taxonomy*, my first point would be that it should not be much simpler to establish a new subgroup, than it is to establish a new series. The SCS policy has been to require a limited number of descriptions of different pedons to support the proposal for the creation of a new series. I believe this number is something like ten. Interpretations are also required for new series, to show that the proposed series differs in some interpretation from some closely-related established series. It has also been the policy of SCS, when a proposal is made for laboratory analyses for a given kind of soil, that at least two pedons must be sampled and they must be from differing polypedons, and at least 1.6 kilometers apart. When one compares the data from the two pedons, one then gets some notion of the reliability of the judgement of the individuals who were trying to select pedons that are as nearly similar as possible. If one examines the results, one finds that the individuals who made the request are not always competent to evaluate the soils properties prior to the receipt of the analyses, and many of the pedon data that are supposed to be similar are rather widely divergent. It would seem to me, therefore, that one requirement should be that descriptions and laboratory analyses of at least two pedons, at least something like two kilometers apart in the landscape, should be submitted as part of the supporting data. If supporting laboratory data are presented from a laboratory in an LDC, I think it would be desirable to require the submission,

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2. Editor's Note: See also "Proposed Procedures for Updating Soil Taxonomy" by Klaus W. Flach, in Soil Taxonomy News, No. 7, pp. 16-17, January 1984.

along with the data, of at least one subsample of one horizon of each pedon that has been analyzed. Proposals for the creation of new taxa above the series level should be accompanied by estimates of the areal extent of the kind of soil, and by interpretations for the proposed kind of soil showing some significant difference in behavior from the most closely related taxa in *Soil Taxonomy*. The proposal should also include long-term climatic data if possible, if the soil is one in which there is the possibility of a udic, xeric, ustic or aridic moisture regime. Proposals for changes in definitions of diagnostic horizons or features, or of existing taxa should also be documented with descriptions of the soils that cause the proposal to be made, the interpretations of the soils with the present definitions, and with the proposed definitions.

To the best of my knowledge, I have seen no approved changes for *Soil Taxonomy*, although I did see a document that said certain changes had been approved. The present feeling in the Soil Conservation Service is that approval was premature. The SCS is reexamining everything that was listed as approved. After some years of debate among a considerable number of people, the international committees, perhaps, offer one major route to make changes. I think that they will come out with well-reasoned proposals for changes. It is not easy to suggest how other changes should be made. There are small problems that probably don't warrant an international committee. The Soil Conservation Service at one time had Dr. McClelland as the director of Soil Survey Operations, Classification and Correlation. That was a serious overload for any one man; he simply could not give proper attention to any part of that work. They now have three positions to cover that: operations, one position; classification, one position; and correlation, one position.

1.19.2 Other Changes

I could give one example of a change that is needed that probably doesn't require an international committee: the definition of Inceptisols excludes soils with a conductivity of 2mmho or more within certain defined depth limits. We see over and over again, on one continent or another, that if soils that have a relatively low precipitation are irrigated, the conductivity increases, and then an Inceptisol becomes an Aridisol. You have this in Texas, in the lower Rio Grande. These Inceptisols that can be used for dry farming are suddenly grouped with Aridisols when they're irrigated. The major thing we want to say about the Aridisols is that they're too dry to cultivate without irrigation. Suddenly, we find we can't even say that about Aridisols without changing the definition. This change is so obviously needed, I don't understand why it hasn't been made, except that they're tied up in vacancies in Washington. I don't think an international committee is needed for problems like that. I've been working since I retired, first in the West Indies, in Venezuela, then in New Zealand, and I have page after page of minor changes that are obviously needed. The problem is how to get these approved and to get them into circulation so that the pedologists around the world can know what changes are approved. It was proposed at one time to publish these approved changes in the *Soil Science Society of America Journal*. That is also under reexamination. They're planning now to publish them in the *Soils Handbook*[3]. **Question 3, Texas**

But for the kinds of changes we've been discussing on cryo-soils, I think it would be advisable if we could have an international committee. Because of the changes in the Russian attitude within the last couple of years, it is not inconceivable that they would be willing to cooperate on this, given one of two or three things. First, that they could travel to countries outside of the U.S.S.R. or that they could arrange for travel within the U.S.S.R. for these committees. It's quite likely that they have a great deal of experience that would be useful to us in northern Canada and northern U.S. They do cultivate rather extensive areas with permafrost in the Soviet Union, but this is not common in North America. And from the publications I have been able to find, I don't see how they can do it when we can't. It may be they have techniques we don't know about, it may be that things are very different, that they have much hotter summers than we do. It's very difficult to read the translations of their literature and figure this out. I have tried.

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3. Editor's Note: They are actually currently published in the latest Keys to Soil Taxonomy.

I've been asked if it would be useful to compare soil colors of epipedons under forest with those under savannah, as a possible basis for proposing changes in differentia. One never knows what studies are going to be useful until they are completed or at least well along. There certainly is no harm done to examine the available data from this point of view and whether or not the conclusions will prove useful will depend on what the data show.

1.20 Aids to Use of Soil Taxonomy, Keys, Teaching Soil Taxonomy

A number of people are interested in computer access to *Soil Taxonomy* and in programs to facilitate soil classification using *Soil Taxonomy*. Lester Blakemore of New Zealand has devised a set of pullcards for all of the diagnostic horizons that could easily be computerized. In New Zealand they're working now on the pullcards for the orders and suborders and so on. I don't remember whether I've seen one for an order yet or not. I have, however, seen one with all the diagnostics on it (Leslie Blakemore). That's not for the diagnostic horizon but it shows you what they had been doing. There is an International Committee report circular letter on this matter. **Question 204, Minnesota**

Practice exercises in soil classification according to *Soil Taxonomy* are useful for students of pedology. I would prefer to have a group of students work on the classification of such a soil as a group, rather than as individuals because it is complicated enough that a beginner is apt to make some serious blunder. If you have a half-dozen people working on the classification of the same descriptions and data, what one man overlooks someone else will pick up and such a group generally can come out with the same answer. Whereas an individual will make a mistake that he will not notice - that is one suggestion. The pedologist who works in the field has a much narrower universe as a rule than a beginning student. He quickly learns where he has made mistakes in his classification in the area where he is classifying soils and can avoid them in the future, but for students I think the group judgment is the best approach. **Question 59, Texas**

There is an important distinction between a taxonomy and a key. Both are classifications of a sort, but a key is almost purely an artificial classification rather than a natural or taxonomic classification. The order in which the taxa appear in a key, in *Soil Taxonomy* at least, is based entirely on ease of comprehension of the definitions. Consider first the aquic suborders. In most orders, the soils with aquic soil water regimes and with the necessary qualifications for the definition of the aquic suborders, namely low chromas and so on, is a common requirement of the aquic suborders. In the Mollisols, the suborder of Albolls comes ahead of Aquolls, and some of the Albolls have all of the requirements necessary for an Aquoll, but some are not quite so wet. But the presence of the albic horizon plus the indications of impeded drainage were considered more important in the Albolls than just the presence of the characteristics of poorly drained soils. To keep together in one taxon the soils that belong together from their behavioral characteristics, we wanted to permit the soil drainage to range from, perhaps, imperfectly or somewhat poorly drained to poorly drained. This was accompanied by the requirement for the presence of an albic horizon and of an abrupt textural change between the albic horizon and the argillic horizon, and it was much simpler to put these soils first in the key, because the key became much shorter than it would have been had we put the Aquolls ahead of the Albolls. This is purely artificial and was done to permit the shortest possible statements in the key. The same principle applied to the "pale" great groups in the Ultisols. If plinthite or a fragipan was present, we wanted to emphasize this in the taxonomy, and in the construction of the key, it was much simpler to put these ahead of the "pale" great groups which did not have plinthite or fragipans.

I should point out that in Chapter 7, c . page 91, *Soil Taxonomy*, the use of keys throughout the text is discussed. We poin out that the reader or the user should use the key first to the order and to then select the most probable order that he can find for the classification of a particular kind of soil. He then goes to the page indicated in the key and at that point he will find a complete definition of the order in terms of the properties of the order and the distinctions between that order and other orders. If the soil that he is concerned with

meets the requirements of that particular order definition, then he continues on to the key to the suborders. Again he selects the most probable suborder, turns to the indicated page, and then checks the particular soil against the definition of that suborder and so on down the line through the keys to the complete definitions of the various taxa. **Question 8, Leamy**

1.21 Soil Horizons

I do not recall any discussion on having a minimum size for specific horizons. There have been questions about the minimum thickness. They should be thick enough that they are observable to more than one person. **Question 27, Cornell**

We have minimum limits on cambic horizons, spodic horizons, oxic horizons, and in a few instances even an argillic horizon; it is supposed to be 1/10 the thickness of the overlying horizon, but if they have been removed by erosion this becomes infinitely small, so we like to have something observable like 2.5 centimeters minimum thickness for the argillic horizon. **Question 28, Cornell**

You ask why the albic horizon is not given greater prominence and why there could not be a suborder of Albods. On page 8 of *Soil Taxonomy*, the sixth attribute that we desired for the taxonomy was that the differentiae should keep an undisturbed soil and its cultivated or otherwise man-modified equivalence in the same taxon insofar as possible. If the albic horizon is thin, the mere clearing of the forest, seeding of grass, and pasturing can destroy a rather respectable albic horizon. This, I demonstrated in one of the type locations of one soil in New Zealand where, in the road bank there was a good albic horizon but if one crossed the fence into the pasture it was gone. This would mean then that if we are going to emphasize the presence or absence of an albic horizon more than the presence of a spodic horizon, one would have to draw a boundary along the fence, because that is where the albic horizon stopped. It would be possible, of course, to emphasize the albic horizon at the expense of the nature of the spodic horizon and if we did that, we would have, perhaps, an Albod and a Chromod and then these would be subdivided at the great group level into humic and other types of spodic horizons. We felt, when we developed *Taxonomy*, perhaps erroneously, that the nature of the materials that accumulated should be given greater weight than the presence or absence of an albic horizon. Certainly, if one were to emphasize the importance of the albic horizon, the definition would have to require that it extends to depths greater than 25 cm; otherwise plowing would change the nature of the classification of the soil. **Question 24, Leamy**

1.22 Categories of Soil Taxonomy

Why did we not recognize moderately deep soils at the higher categoric levels? We had discussions about this. It is specified as a series property. It must be separated at the series level. The feeling was on the part of the correlation staff that this could be handled at the series level. We did not have to have another family. If we did not need another family, we did not need another subgroup.

In so many of the shallow soils, the lithic contact is of such overriding importance to interpretations that it seemed worthwhile to put it in at the subgroup level. It does not represent an intergrade to another kind of soil, but an intergrade to what we would call "not soil". That is the concept of the lithic subgroup. The soil is truncated; it comes from the old concept in the '38 classification of Lithosols. It was downgraded considerably in *Soil Taxonomy*, but it is important not only to plants but to engineering uses of the soils. If you ever tried to dig a grave in a cemetery in a lithic subgroup you would find out quickly that is the wrong place to put a cemetery. **Question 53, Cornell**

1.22.1 Order

1.22.1.1 Similar series in two different orders

(Concern is given because two very similar series separated at the order level, Dystropept versus Haplorthox, because one has some feldspars and the other lacks weatherable minerals.) In considering the importance of a critical limit between orders, we must always keep in mind that, soils form a continuum, that there are intergrades between most kinds of soils that may go through other orders. In order to have a clear cut definition that defines the limits of a taxon, whether it is an order or a subgroup, we have to put the limit at a point which will divide the soils on either side of that point into different taxa. Thus, the two soils which are very similar, one on each side of that limit, are separated. They are more like each other than they are like the other soils in the taxon. The gradational change from one soil to another is reflected in the names. The Picacho is an Oxic Dystropept, and the Matanzas is a Tropeptic Haplorthox, indicating that these are gradational between the two orders. If one were to change the limit of the percentage of feldspars, it would only shift the subgroup nomenclature to another series, and would not eliminate any problem whatever. I do not, at the moment, foresee the need for special kinds of cambic horizons in intertropical soils. **Question 5, Eswaran**

1.22.1.2 Permafrost soil order

(Concerning the idea that properties of pergelic soils occurring in different orders are much more closely related to each other than to other non-permafrost soils within the same order) there is nothing sacred about the number of orders in *Soil Taxonomy*. It merely reflects what knowledge we had at the time we developed the system, and we may have made a serious mistake. This is not a matter for the judgement of one person, (rather) a group judgement as to the importance of permafrost, cryoturbation as compared to the distinction between organic Histosols and the various mineral soils and so on. It would, I think, be a very good topic for discussion by, in this case, a small international committee because not many nations have such soils. The Russians would not be expected to cooperate, although they have plenty of them, the Canadians, the North Americans, and the New Zealanders would be the principal ones who could work on such a committee. I should very much like to see this proposed to the international soils group in Washington as a good subject for an international committee.

In defining such an order, as I say, one normally would use, not a single property, but a combination, and one might want to distinguish the permafrost mineral soils from the others at the order level, but not include the Histosols in that group. That would be a possibility. And it is a matter that should be discussed, I think, by people who have some experience with these soils and know something about them. Personally, I have never been in Alaska. The only soils with permafrost I have seen are at very high altitudes in Norway and they were mineral soils. So, I would say this is not something on which my opinion would be important, but it is something that should be discussed by an international committee. I would like to see a twelfth order, I love twelve as a number, much more than I do eleven. **Question 26, Minnesota**

1.22.2 Subgroup

The subgroup is the lowest category in which we consider genesis in forming our definitions. When we go below the subgroup into the family and the series, we find that the distinctions are largely pragmatic, that we want one series or two series because of some differences important to our interpretations, and this has been the basis for justifying, establishing series. And the family definitely is designed to reflect important differences in soils that affect the response to management of soils for growing plants or for engineering

manipulations. There is much talk, I think, rather loose talk, about building your classification upward or downward in the case of soils at least. When you are dealing with ten thousand or twelve thousand soil series, there is no possibility of understanding the series well enough to organize them into classes and build them up into families, subgroups, great groups, suborders. It can not be done with the human mind. Perhaps some day a computer can do something about it, but the data in the computer were grossly inadequate when we were working on *Soil Taxonomy*. There is a substitute; we were forced into a compromise in which we devised definitions in the higher categories and then examined what kinds of series were grouped by those definitions. We tested it really from both directions, up and down. **Question 100, Minnesota**

The rationale of the subgroup category comes from a long-standing approach toward classification in the Middle West, in particular, where we have sequences of soils of varying stages of development ranging from the Hapludolls to the Argiudolls and finally to the Albolls. In classifying these soils, the concept developed that we had sequences, chronosequences if you please, or some other kind of sequences going from grasslands or forest soils, depending on how long the forest had covered the soil; the mollic epipedon would become thinner and disappear. We had then series to classify, we would say one is a Mollisol, one is an Alfisol and the intermediate one is an intergrade between a Mollisol and a Alfisol. So the concept of the subgroup included the intergrades, soils that had a dominant requirement of one taxon but that had subsidiary requirements of another. The drainage sequences would be another example where you have a well drained soil, and a very poorly drained soil, and in between you have the soils that show mottling and low chroma with depth. These go into the aquic subgroups of the well drained soils, or the aeric subgroups of the wetter soils. But not all soils with aberrant features show characteristics of any defined taxon. One example would be the cumulic subgroup of the Hapludalfs in which there has been slow accumulation of material at the surface that produces a greatly overthickened mollic epipedon, maybe a meter or more thick. We have no particular kind of soil that is defined as having such an overthickened mollic epipedon. These we considered to be extragrades, soils that had aberrant features that were not typical of the great group, but that did not seem to intergrade to any other defined kind of soil. The lithic subgroup would be another example where the soil has been truncated, you might say, or shortened by the shallowness of the bedrock. It is, perhaps, some sort of an intergrade to "not-soil", but still is a soil. We have this kind of subgroup that has the aberrant features that are not characteristic of any currently defined kind of soil and these we call extragrades. I, in my own thinking, have not had much trouble with getting this concept to students, but these were all graduate students and had been exposed to this before.

The naming of a subgroup that is not defined and that differs from the typic in two features should be governed basically by the interpretations that can be made. Normally, you would examine your interpretations of that new kind of soil to see where it would best go. If it is markedly different from either of the defined subgroups, then you set up the third subgroup for it, but if it behaves like one or the other of these two defined subgroups, then you modify your definition rather than setting up a third subgroup. **Question 78, Texas**

1.22.3 Families

Families were designated in the past by the name of the dominant series in them. That is still done as far as I know. You will have then slope phases, erosion phases. If you want to drop the series category, you are going to have to phase out about 40 other characteristics. In some families that have a wide geographic spread, they have used a series from Iowa as a family name there and another series from Oregon as a family name there. For the most part this represents a defect in the *Taxonomy* because these should not be in the same family. The one with virtually no rainfall in summer can only be used with irrigation to grow maize; the one in Iowa produces very good yields without irrigation, and they do not belong with the same family. The proposal has been made to correct this defect, particularly true in Aqualfs, for example, or other aquic great groups where you have a wet/dry climate versus where you have a humid climate. **Question 130, Cornell**

For the most part, the control section for the classification of soils at the family category, stops at a meter. The control section for series is permitted to run below a meter. If there are significant differences below the depth of 1 meter and above the depth of 2 meters, for the most part the classification would be reflected at the level of the soil series. Significant differences at this depth must be shown by some means for interpretations. If the differences occur below 2 meters, the man making the soil survey will have relatively few observations compared to his observation in the surface meter, which he can examine readily with a soil auger. Differences below the depth of 2 meters also need to be reflected in the map units if they are significant to the anticipated uses of the soil. However, these differences would necessarily be used as phases rather than as series or family differentiae. It is important that any difference at any depth be shown at some categoric level or as a phase, if they affect the anticipated uses. However, the difference at a depth of 6 or 8 meters requires a power drill to determine and one has relatively few observations and the phase is about the only possible way to show these differences. **Question 12, Leamy**

1.22.4 Series

The series has been a classification of its own since the Soil Survey started. The first series came about 2 years after the initiation of the Soil Survey. While general details of the concepts of the series have been modified greatly since 1900, the general concept of the series had undergone very little change. So in 1920 when Marbut began to work on the taxonomy of soils in general, we already had some several thousand series divided into several thousand more types. When Marbut introduced the concept of the great soil group, that carried on through the 1938 classification. There was an inadequate knowledge and inadequate time to relate the series to the great groups. Consequently we had two classifications of soils: one into series and one into great soil groups and other higher categories. The link between the series and the great soil groups had not been developed until well along in the various approximations of *Soil Taxonomy*. There was enormous resistance to doing anything in *Soil Taxonomy* that would have a wholesale effect on the definitions of the soil series.

When the potential uses of soil are extremely limited, as in Nevada where one can use them for nothing but grazing and very extensive grazing at that, the series can be defined much more broadly than in a State like Illinois or Wisconsin where the soils are very highly productive. If the yield potential ranges from 30 bushels of corn to 150 bushels, that range (in order to make predictions) must be subdivided into quite a few map units, mostly at the series level. Where the potential production of edible forage ranges from 200 to 400 pounds per acre, one doesn't need too many series in order to make reasonable interpretations of the significance of the map units. So in the regions where we have our highest productions, we find that we have far and away the largest number of series. The Typic Hapludalfs would include a very large number of series compared to the Typic Haplargids. This is necessitated by the differences in the productivity of the soils. **Question 11, Cornell**

For the most part the definitions that have been published of soil series, the categories, have stated that the soil series is developed on a particular kind of parent material. You will find this in the *Soil Survey Manual*, the first edition and the 1951 revision. The implication of this definition of series is that parent material is important, but there is no clue as to what a given kind of parent material is. There are almost an infinite number of kinds of variations in the glacial tills of the northern states. How much difference does one require, say in the clay content of the glacial tills, before one decides it is another kind of parent material? The clay percentages of the Wisconsin tills in Illinois will range from less than 10 percent to over 80. In mapping, that continuum was broken into four steps. It would be the coarse-loamy, fine-loamy, fine, and the very fine. When one has everything between four possible subdivisions or eight possible subdivisions, if one goes by steps of 10 percent for example, what is meant by parent material is undefined actually. It is a matter of judgment of the man who is making the survey and the purpose for which the surveys are being made. If one set up rigid limits of any one property of materials that would distinguish one parent material from another, I fear it would cause great troubles when surveys were made for different purposes. The soil survey of Alaska,

for example, would not find the same subdivisions useful as one would find in North Dakota where the soils are virtually all cultivated. We would like to, I think, keep some flexibility.

It isn't quite true that *Soil Taxonomy* is focused on the solum, I tried to avoid using that word in *Soil Taxonomy*, except perhaps in an explanatory method. It does not appear in the definitions of diagnostic horizons because people won't agree on what the soil is. The Americans and the Canadians differ violently on the accumulation of carbonate. The *Soil Survey Manual* says the horizon of the accumulation of carbonate is part of the C horizon, now the solum is supposed to be the A and B, not the C. The Canadians call the accumulation of carbonates a B horizon, a Bca. If one uses that concept of A and B and C and solum in the definitions of the diagnostic horizons or the taxa, then one gets into endless arguments about what is A and what is B, and what is C, or what is parent material or solum. There is no general agreement whatever amongst the world's pedologists about the meanings of these terms.
Question 115, Texas

In Illinois and Iowa, when a farm was advertised for sale in the local newspaper, they would very commonly say 60 acres of Carrington loam--the series. The highway engineers who were designing the rural roads used the soil series and the soil maps as a basis for their design of these secondary roads. The tax assessors used the soil surveys as partial basis for taxing the farms and they all knew the names of these series and what they meant; they didn't know all of the thousands of series in the U.S. but they knew those in their county or the area where they were working, and it was desired to avoid changes in concepts of series unless those changes permitted better and more precise interpretations. The highway research board has been renamed now, but when the highway engineers found that we were developing a new classification system they became alarmed, because they wanted to retain the series they knew, and they demanded that I appear before their annual meeting to explain what we were doing about the soil series. When I explained what we were doing, that we were trying to arrange the series into higher categories without disturbing them more than was necessary, they were greatly relieved at this. They continue to use the soil series as a basis for their highway design. So the concept of the series has been refined as we learn more about soils and what properties are important to soil use, but it's been a refinement that has not been due particularly to *Soil Taxonomy* per se but to our increasing knowledge about soil behavior. What we have done has been to develop one classification of soils rather than the two that we had prior to 1950.
Question 10, Cornell

1.22.5 Disturbed or Man-Made Soils

Once we had succeeded in defining soil, it became obvious that these disturbed things were soil, and that if we were going to have a system that could be applied potentially to the soils of the world, some place had to be made for them. This was covered in some detail in the discussions in Washington. My experience with the Arents at the time we wrote *Soil Taxonomy* was restricted to some of the disturbed soils of Europe in which the disturbance was the result of deep spading, so that we had fragments of spodic horizon (if you please) that could be identified, fragments the size that would fit on the shovel with which the soil was turned. It seemed logical that in this country we provide for the soils that had been badly gullied in the loess belt in the Southern States, for example, where on the narrow ridges we had Udalfs, Hapludalfs, and in between we had Orthents. When these were reclaimed, leveled with bulldozers, and so on, we would be able to find these same fragments of argillic horizons in the smoothly shaped land that was left by the bulldozers. But we had really no observations of what was present in these areas. I can hardly lay down any rules in the absence of some studies as to the kind and variabilities that are found in these. On what scale does the variability occur? Do you get these fragments within each pedon or not as the sampling is described? It would be necessary to have at least one identifiable fragment in each pedon or you would then have a complex of Arents and Orthents, or something of that sort that would require two series. It would be possible to continue to identify these as miscellaneous land types. It is really more informative to users of the soil survey to identify an area as a borrow pit than to identify it as an Arent. So that in the naming of the map units, there is no harm in naming these according to whether it is a burrow pit or a fill, or what it may be. In the classification, which is

technical, which we do not actually use much with the users of the soil surveys, we can simply identify these to them as unit *BP*, for pit, and in our legend, taxonomic classification, *BP* appears instead of a series and is identified taxonomically. Admittedly, the technical nomenclature is not intended for use by users of soil surveys. They should go from the legend of the map, the symbol that they find in the area that concerns them, to the important interpretations that they are concerned with. They can completely bypass the technical nomenclature, but this nomenclature is intended for use by the people who make the soil surveys, rather than by the people who are interested in finding out what their land can be used for. Until we have some more studies of this problem in the U.S., I certainly have no valid suggestions. These problems occur, for example, in the areas which are subject to fill by dredges, in which the dredge pumps the sand and the silt out and spreads it over an area that they want to raise above the water table. These are stratified just like the Fluvents, but they are not subject to flooding like the Fluvents. I do not think they would belong with them. But as the present definition is written, that is where they come out. **Question 57, Cornell**

Again, I am quite ignorant on strip-mine spoil. I have seen a few strip mine areas in southern Illinois. We do know that on the natural Orthents, that there is some sort of order to the occurrence of the stones of various sizes, and so on. They are not present at random with a chunk of limestone next to a chunk of sandstone, and a chunk of shale. There is an order to the natural soil that is missing in these strip mines. This I can only say is a suggestion to someone who wants to propose something different--that he probably will have to base it on the absence of any order between the coarse fragments. **Question 59, Cornell**

I still don't know how large areas of strip-mined land and spoil are going to fit into *Taxonomy*. I think you have to examine what does accumulate there in the way of an area to see what can be done with it. There are a number of things possible, one of them is to put a series name on it perhaps, although I suspect these mixed things are going to be too complicated for a single series. I don't object strenuously to having the old miscellaneous land types which were, in effect, areas of unclassified soil.

I think both philosophically and bureaucratically you have the opportunity to raise this problem with your regional and national work-planning conferences. It can be thoroughly debated before you make any final decisions. I think that is the way you should do it. That is what these conferences are for. I would trust the group judgment much better than I would my own, because I have had very little experience and most of those involved will have limited experience, but when you get the group together you may have quite a wide spectrum of experience. Out of the debate then, I think you will come up with something you can live with.

Many areas of man-made soils are at least straight-sided and not necessarily rectangular, maybe triangular or something of that sort. The plaggen epipedon does stop mainly along the fence line.

There are many straight-line boundaries between soils in Europe. Many more than we have here because of differences in the use of the land on one side of the fence or the other. If the land belonged to a nobleman and was kept in forest the soil is now significantly different from the soils that were cultivated in the surrounding areas. Many of the Glossudalfs of Europe are in these forests that have been kept in forest because they belong to the nobility, and were used for hunting, mostly. **Question 86, Texas**

1.22.6 Wet Soils

In *Soil Taxonomy* we divided up the wet soils and we put them at the suborder level, not at the order level. Most other taxonomies have an all wet soils group. Background on that started when I first mapped soils in Illinois in the northeastern part of the state where all the soils are Udolls and Aquolls. My first year's experience was restricted to those two kinds of soil and they were entirely different to me. I then took the soil maps of the various experiment stations and located the plots that were all Udolls and the plots that were all Aquolls and compared the yields on the two sets of plots. They were identical, which shook me badly. I

puzzled over that until finally I realized that on these plots the Aquolls had been drained so that when you drained the Aquolls, the Mollisol properties became important as well as the udic properties that we get from the summers in Illinois. Then if I compared say, the Red-Yellow Podzolic soils with the Low-Humic Gleys soils in the southeast, I compared what would happen. If you drained the Low-Humic Gley soils, you would have a soil with the same properties as the Red-Yellow Podzolic soils. There was a zonality to the soils with aquic moisture regimes and this would be best reflected if the aquic soils with aquic moisture regimes were separated below the order level. I argued in some of the conferences that the separation should be made at the Great Group level so that we would have an aquic correlative of the Xeroll and an aquic correlative of the Ustoll and one of the Udoll. It was too big a leap for the people of that time to do that. I could get no support whatever for that treatment of soils with aquic moisture regimes.

We are coming around now to somewhat the same thing in that the committee on intertropical soil moisture and temperature regimes is considering making subgroups for the soils with aquic moisture regimes, such as the Aquoll in Venezuela where you have six months of heavy rain and six months with no rain. These soils do not behave as do the Aquolls in Illinois because they require drainage at one season and irrigation at another. The same thing holds for the Aquolls in the Willamette Valley in Oregon. They can not grow corn or soybeans on Aquolls without irrigation because it is a pronounced wet-dry climate. (The international committee put in an ustic subgroup of an Aquoll.) They must drain in winter and then irrigate in the summer, or else about the only crop they can grow is grass for seed. That's what you see lots of there. These soils have wonderful chemical and physical properties. They lack only the evenness of the moisture distribution that we get in Illinois and Iowa where they are the most productive soils. We are coming around to it, but instead of making the subdivision at the great group level, I think probably we will wind up and make it at the subgroup level. The committee proposes ustic, xeric, and udic subgroups of all of the aquic great groups except that one of those will be set as typic. Probably the udic will become the typic. Then they will have ustic, xeric subgroups. **Question 67, Minnesota**

In respect to classification of wet soils, the Europeans are mostly stuck with their former prejudices about it. They want one order for all the wet soils. An aridic subgroup has been mentioned yet there is no aquic suborder of Aridisols.

One of the big problems is the manner of definition of the aquic suborders. Some people think of intrazonal soils as being poorly drained. But, not all the intrazonal soils are poorly drained. Those with what Marbut called "excessively developed profile" were also included as intrazonal. Soils with natric horizons were included as intrazonal, though not all are poorly drained. I might go back in my own personal experience when I first started to map soils. I worked in a county in Central Illinois where all of the soils virtually were Mollisols. The big differences that I saw as a beginning mapper were the differences between the well-drained and the poorly drained soils. Later I undertook to study the crop yields that were obtained on the experimental stations, and I classified the soils (all Mollisols) on the basis of their natural drainage. I determined the yield that had been obtained on the naturally poorly drained soils after drainage with the yield on the naturally well drained soils. There was no significant difference. Once the poorly drained soils were drained, they behaved like the naturally well drained soils. If one goes into the Southeast, in the region of Ultisols, one would have the same experience, that after drainage the naturally poorly drained soils will behave like the naturally well drained soils of that area. So the Aquolls have many of the same properties as do the Udolls; after drainage, they have a mollic epipedon, they are rich in bases, and they produce the same kinds of crops with the same yields. The Aquults are low in fertility, they do not have a high base status, and they require about the same management as do the Udults. So it seems that if we established an order of the aquic great groups, we would have some very strange bedfellows. We would be better off to keep the Aquolls with the other Mollisols and the Aquults with the other Ultisols. This notion certainly met with enormous objections in the early approximations. It was my notion that it would have been better to have had aquic great groups than aquic suborders, but the staff generally was so strongly opposed to having aquic great groups that I had to abandon the notion of bringing in soil drainage at the great group level rather than the suborder. There would have been advantages to doing this. For example, your committee on moisture and temperature regimes is having to deal now with the differences

among the aquic suborders according to whether, after drainage and flood protection, they will have a natural udic moisture regime or a natural ustic moisture regime. At present the aquic great groups in the wet/dry climates are very wet in the rainy season and extremely dry in the dry season, whereas the aquic great groups in regions of uniform rainfall distribution are never dry in the sense that they lack available water for plants. This is not reflected in the present taxonomy, but needs to be. **Questions 8, Cornell and 68, Minnesota**

The paddy soils of Asia are greatly disturbed by man. I have not found many descriptions of the terraced paddy soils. I can visualize what must be there, but while writing *Taxonomy* I could only lay my hands on one description of such a soil. I have seen them myself in China, but I have not had a chance to look at the soil, just the landscape. I stated specifically that no provision was made for these soils, and it is in the preface, I believe. It is pointed out that no provision is made for the naturally well drained paddy soils.

If the soil is naturally wet, I do not think there has been much disturbance, but if it is on a slope, in order to build the terrace, the soil has to be moved from the upslope to the downslope position, so that at the terrace edge, you are going to have a much deeper soil than you are at the base of the terrace next to the next higher terrace. And until we have some studies on descriptions of these, I do not see any good way to make definitions. I envisage, since these are flooded, that we will have gleying at the surface that will disappear with depth. That I would predict, and the only profile descriptions that I had was of such a situation, but I do not know how to write definitions on the basis of one description. **Question 58, Cornell**

1.22.7 Numerical Taxonomies

In recent decades interest has grown, especially in biology, in numerical taxonomy. For microorganisms, single-celled organisms, it may be a good approach. In my experience, it seems to be less useful for soils. The first argument for a numerical system is that you do not weight the properties--all properties have the same weight. This in itself is a weighting. You cannot avoid this. Secondly, any of the examples I have seen on the application of numerical taxonomy to soils involve a rather careful correlation analysis on how properties are interrelated. If there is a high correlation between two properties, they throw one out. This ignores the possibility that it is a correlation but is not a one to one relationship. There are serious discrepancies between clay content and CEC according to the nature of the clay and the method used to determine the amount of clay and CEC. **Question 110, Cornell**

Numerical taxonomy has been suggested as a mechanism to select criteria to sort out different soils within a class. It depends on what properties you select. There was a paper on numerical taxonomy in the *Proceedings of the Soil Science Society of America.* They developed clusters of soils which we can look at. They clustered a very salty Aridisol with an Aquoll from Iowa. These were closely related according to the procedure they followed. As the procedure grouped the most productive with the most unproductive soil, we have to question the methodology. The reason is that they used the wrong properties for the clustering. The numerical taxonomists insist that they are unbiased as they do not weight the properties. My opinion is that, as they are weighting them equally, they are as wrong as if they gave different weights.

For mono-celled organisms where the identification of the organism is based on its behavior, there are insufficient characteristics to classify them, and numerical taxonomy is very useful. But these are limits to any system of taxonomy. When you weight color as being equal to base saturation, it is not serving the purposes of soil survey. **Question 111, Cornell**

It has been asked if numerical taxonomy can be used to alter, improve or create soil classifications. I would reply that it has potentials, but we are not yet ready to explore numerical taxonomy. The studies that have been published have been very discouraging for the use of numerical taxonomy for a number of reasons. It is quite common, for example, that one starts with multiple correlation between particle size and organic carbon and so on. If you find a high correlation between two properties you use only one of those properties for your

classification, you eliminate the other. The advantage that the proponents claim for numerical classification is that it is not biased by judgment because each property is given an equal weight, whereas in *Soil Taxonomy* we weight some properties more highly than others. This advantage is a fictitious one, because in assigning an equal weight to each property, you are still weighing it, the only difference is that you are weighting them the same. And it seems absurd to me to say that the color hue of a soil has the same importance as any other property of the soil. I do recall one such numerical taxonomy classification in which the Haplaquolls of Iowa were most closely related to the Calciorthids of the arid region. Now this seemed incomprehensible, and, of course, it comes from the selection of the wrong properties. That's the central problem of numerical taxonomy and it seems to me that we will not get anywhere with numerical taxonomy, until we quit eliminating properties because they are correlated with other properties. This correlation is imperfect, and in one part of the world it may hold, and in another part of the world it may break down completely. **Question 101, Minnesota**

Chapter 2

DIAGNOSTICS FOR MINERAL SOILS

reviewed by J. Witty[4]

2.1 Diagnostic Horizons

(About the questions - how did you arrive at the general concept of diagnostics horizons?) in the early approximation that led up to the development of *Soil Taxonomy* we tested the groupings of soils according to the nature of the horizons; soils with A horizons only, soils with A and B horizons were grouped into separate taxa in the approximations. We shortly realized that the nature of the B horizon was important and we began to talk about the textural B horizons, the podzol B horizons, etc. This was the first step toward the use of the diagnostic horizons. It was not too many approximations along, before we realized that the pedologist would not agree on a B horizon. And yet this has been used in the highest categories of the approximations. I sent out the list of approximations in which I spoke of a latosolic B horizon and gave a partial definition of what it should be like. The comments I received were mostly concerned with whether or not this was a B horizon, and I got no comments on whether the definition will produce useful groupings or not. So at this point, I stopped referring to A, B, and C horizons and started the use of the diagnostic horizons.

There were further problems that some of the diagnostic horizons could, only with great difficulty, be considered A, B, or C; there were other kinds of horizons. For example, what is a duripan? It is not an A, because its a subsurface horizon, it is a horizon of accumulation of silica and occasionally of iron, but is it a B horizon? Pedologists generally would not consider it a B horizon if there were an overlying argillic horizon which is a very common situation. The Canadian pedologists consider the horizon of accumulation of carbonates as a B horizon; American pedologists consider it as a C horizon, Cca versus the Canadian Bca and there seemed no prospect of any international agreement on whether a horizon of carbonate accumulation is a B horizon or a C horizon. Therefore, the use of A, B, and C was impossible in a general system because of lack of agreement amongst pedologists and, the only alternative was the substitution of diagnostic horizons about which the original concept of A, B, and C would not interfere with general agreement. **Question 35, Leamy**

The date for the adoption of the diagnostic horizons is hard to fix because we were speaking of different kinds of A horizons and different kinds of B horizons. The use of named diagnostic horizons dates from the *Sixth Approximation*. The *Sixth Approximation* was issued in 1957. **Question 116, Texas**

I don't think that the argillic horizon is "weighted" higher than other diagnostic subhorizons. We look at *Taxonomy* and we find that the mollic epipedon is given priority to the argillic horizon and that the presence or absence of an argillic horizon is recognized only at the great group level in Borolls, in Ustolls and Udolls. The oxic horizon generally is given precedence over the argillic horizon. We made the statement here that the argillic horizon by

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4. National Leader for Soil Taxonomy, SCS/USDA, Washington, D.C., 20013.

itself has virtually no significance to soil classification except to indicate some sort of landscape stability. When taken in combination with other properties, it can become important. The statement may have been extreme, maybe it's more important than I think, particularly in respect to plant growth. The argillic horizon normally has fairly well developed clay skins and these differ in composition from the rest of the argillic horizon. Only a few studies of this, mostly by Dr. Buol, in a doctorate thesis in Wisconsin and some other papers on the Ultisols of North Carolina, show that the clay skins are much richer in nutrients that are cycled in the soil than the ped interiors. This could be a very critical problem in the Ultisol in particular, where we commonly have calcium deficiencies in the subsoil that are severe enough that the plant roots are unable to enter. The presence of the clay skins with their higher nutrient content may explain why we find roots in some Ultisols, where the growth analysis of the whole soil, the whole subsoil, shows little calcium so that there's no way to understand how the roots got there, the ones that are described. But if you read the description closely, you will see that these roots remain between the peds and do not enter the peds.

I don't understand why, with a microprobe, the soil scientists haven't made more studies of this sort. But even Buol in South Carolina forgot to analyze for calcium in clay skins and that was perhaps the most critical element that he should have been looking at. **Question 156, Minnesota**

(Concerning the importance of micromorphology studies) we did try to describe the micromorphology of the cambic horizon, the argillic horizon, and the spodic horizon. There had been enough, I think, studies of those that we could have some confidence in the micromorphology there. I have been concerning myself with the possibility that the micromorphology of the oxic horizon might be more diagnostic than that of the cambic horizon. But I am unable to find very many thin sections with descriptions of the oxic horizons. It does seem to me to offer considerable potential in the definition of the oxic horizon. There, even in the field, the morphology seems rather distinctive in that, in the fresh pit, it appears to have no structure, and yet when you examine it, you find that you have a very strong granular structure, but the granules are so small that they are not visible to the naked eye.

We've not used micromorphology of the epipedons because this is so subject to change by soil management. We have some descriptions of the micromorphology of fragipans and of duripans, particularly the studies made at Riverside on the duripans. This has not been generally available, I suppose.

The micromorphology is an expensive thing to study, and I don't imagine that we will ever have many studies of micromorphology compared to the descriptions that we get of soils that are written in a pit somewhere in the field. Data, I suspect, are always going to be limited because of cost. **Question 97, Minnesota**

Our concept (concerning whether or not opalized horizons in duric subgroups are analogous to the calcic horizon) was that the duric subgroups were soils in which either the duripan was developing in spots rather than as a continuous horizon, or as being soils in which there was not enough soluble silica being precipitated to form a complete duripan, but rather limited amounts of silica available as a cement. This was an either/or basis that included both. Not entirely analogous to the calcic/petrocalcic sequence where the carbonates occur first as pendants on stones, and then the horizon becomes plugged with secondary carbonates, and finally the laminar horizon develops at the surface. The water reaches the plugged horizon and is free to move laterally and deposits carbonate that smooths the surface of the petrocalcic horizon. It's somewhat analogous, perhaps, in that the initial accumulation in the calcic horizon does occur as spots of carbonates. They may be hard if they are present as pendants on stones. In the absence of stones you get the nest of more or less soft carbonates. In that respect, it's somewhat similar, in that it accumulates more in spots than in the whole horizon in some soils at least. In other soils with a calcic horizon the lime is well disseminated throughout the whole horizon without any hardening whatsoever. The duric subgroups have the durinodes which are weakly cemented with silica, so the cementation is generally more obvious in the developing duripan than in the developing petrocalcic horizon.

(Concerning whether there really is or is not a direct analogy between the two sequences of cementation) it's not a good one, no. **Question 173, Minnesota**

I do not recall any (reticence to recognize the duripan as a pedogenic horizon as compared to that for the petrocalcic horizon). There is still reticence to accept the petrocalcic horizon. Particularly in North Africa amongst the ORSTOM people. **Question 174, Minnesota**

We have two precedents in *Soil Taxonomy* for handling this particular situation, where the transition horizon overlies the argillic horizon, and has all the characteristics of an oxic horizon. The first precedent is that of the cambic horizon, which by definition, may not overlie an argillic horizon, unless it is separated from it by an albic horizon. The other precedent is where we have a spodic horizon that overlies an argillic horizon. In this case, the horizon is not transitional, and the order is determined by the overlying surficial horizon, on the assumption that represents best the present processes going on in the soil. In dealing with the material horizon that has the properties of an oxic horizon, but rests on an argillic horizon, it is possible to use either of these precedents. The limit of 30 cm thickness was set without thought that this would be a transitional horizon. In the discussions of ICOMLAC, I proposed that this limit be increased to 50 cm on the grounds that if it is that thick, the soil would behave more like an Oxisol than like an Ultisol. In this situation then, one could establish an ultic subgroup of Oxisols to separate soils with this horizon sequence at the subgroup level rather than at the order level. **Question 8, Eswaran**

This statement ("The dryness seems to be essential to the genesis" of Albaqualfs - bottom of page 109, top of 110 in *Soil Taxonomy*) is primarily a statement coming from geographic correlation between the occurrence of Albaqualfs and the dryness in the warm summer months. There is one from northern Missouri where the Albaqualfs are very extensive in the loess. Across Illinois and into Indiana, the Albaqualfs virtually disappear and are replaced by Glossaqualfs. The Missouri Albaqualfs are the famous Putnam series. In southern Illinois the Cisne and Cowdon are considered representative Albaqualfs. They run on over into Kansas and Oklahoma, but I have never seen them in those states.

The dryness is probably not essential to the development of the argillic horizon, because the Glossaqualfs have argillic horizons also. They don't have that abrupt boundary that occurs in the Albolls and the Albaqualfs. There was no good genetic theory to explain this at the time that we were working on *Soil Taxonomy*. In recent years, the process of ferrolysis has been worked out to a considerable extent. Most of these soils have groundwater perched on the argillic horizon at some season of the year. That is one condition that seems essential for ferrolysis, which is basically destruction of the clay under anaerobic conditions.

In the FAO UNESCO legend, the statement appears, "in these soils the clay has been destroyed in the A horizon". That is a serious overstatement because there may have been some destruction of clay, but there also has been translocation of clay into the argillic horizon. It may be a combination of the two. This is a field in which there is still a great deal to be learned. Along about 1934 in the old *Soil Survey Association Proceedings* Roger Bray presented a series of papers on the genesis of the B horizon, it was then called, now the argillic horizon, in these soils. He worked out a series of calculations about clay formation in place and translocation, and explained the difference between the A and B horizon of the Albaqualfs basically on translocation rather than destruction. Clay difference could be due in part to both processes. We can't, in any way at the moment, quantify how much is due to one and how much to the other. **Question 126, Texas**

Well, let's put it this way. We have some series that are neither Albolls nor Albaqualfs that have this abrupt (knife-edge) boundary between the epipedon and the argillic horizon. There is no albic horizon in between. These are still drier than the Albaqualfs and the Albolls. Probably the albic horizon is not there because they are not saturated for long enough periods to destroy any clays. Yet there they are, a fact and the abrupt boundaries are genetically a bit of a problem. We have them not only in these soils but also in most Spodosols. I think most people agreed that a spodic horizon is due to translocation and precipitation of humus and aluminum or humus, aluminum and iron. There is no good theory yet to explain any of the abrupt boundaries.

Dr. Flach has worried quite a bit about the abrupt boundary in Spodosols. He has presented a hypotheses, though not in writing to my knowledge, that the humus to be precipitated does not get enough aluminum while it is in the albic horizon, but when it gets to the spodic horizon it picks up some of the aluminum that is already there and that has been put into an available form for further precipitation by biologic destruction of the ligands that bond the aluminum to the humus. This won't explain anything in terms of an argillic horizon. **Question 126, Texas**

(There are transitional horizons between the mollic epipedon and the argillic horizon which meet all the color and carbon requirements of the mollic epipedon but also present some clay skins. Which of the diagnostic horizons represent this transitional horizon?) In the discussion of the mollic epipedon it was pointed out that it is not mutually exclusive of the argillic horizon, yet the same subhorizon may be both a part of the mollic epipedon and of the argillic horizon. So that one does not have a choice of either one or the other, but one may have both in the same subhorizon. A mollic epipedon may extend into or completely through and below a very well defined argillic horizon and the same horizon, in this situation, would be part of the mollic epipedon and would constitute the argillic horizon. **Question 57, Venezuela**

It seems probable that whoever asks this question (why do we have to use the weighted average of the control section for soils containing coarse fragments when a thin and dense band of gravel such as ironstone can make root penetration impossible even though the weighted average of the iron stone is less than 35%) is concerned with a diagnostic horizon that has not yet been defined or named. While I have seen many soils in the tropical region that contain considerably more than 35% ironstone, I have never seen a horizon that was important to the penetration of roots or water. I have had proposals for a diagnostic horizon containing ironstone but the proposals were not acceptable because they will transfer any stonelines in tropical soils into a diagnostic horizon. In other parts of the world, stonelines are not recognized as diagnostic properties. *Soil Taxonomy* gives considerable weight to horizons that interfere with the penetration of roots. If such horizons exist in the tropical regions there should be some proposals for a new diagnostic horizon. In my travels, no one has ever concerned themselves with showing me such a horizon, so I assume that it is not an important property. This is an assumption and it may not be a valid one for interpretations of soil on a particular plantation or a particular tract of land that someone wants to cultivate.

Soil Taxonomy does not concern itself with areal extent. We are concerned with the interpretations that must be made for a particular tract of land, whether or not this is a common or extensive situation or whether it is rare. The legend for the soil map of the world by FAO and UNESCO, is one that, of necessity, depends on areal extent. Soils that are very extensive in the world can be shown. Occasional soils of small extent cannot be shown in their presence while important. A particular tract of land cannot be indicated in the legend because in the world as a whole they are exceptional even though the properties are extremely important on that particular piece of land. Therefore, if there are such horizons with ironstone that are impenetrable by roots, someone should suggest a new diagnostic horizon just as I have, since retirement, suggested the densipan and the lithoplinthic horizons. There are certainly other horizons that should appear in *Taxonomy* that have not been suggested. **Question 39, Leamy**

2.2 Abrupt Textural Change

(The general rationale utilized in defining the abrupt boundary categories was that) we were trying to get at a definition that would keep together the bulk of the soils that had been called Claypan Planosols at one time. Where we commonly have a silt loam albic horizon over a fine or very fine-textured argillic horizon where the nature of the argillic horizon causes the water to perch above the argillic horizon in the albic horizon. In the laminar soils the water does not perch above the argillic band but perches within it, and does not introduce problems of aeration, and anaerobic groundwaters which kill the roots of any air-loving plant that happens to be growing there. There was an effort made to utilize some of the concepts of the 1938

classification in the development of *Soil Taxonomy*. The ones that were judged to have a maximum utility we tried to retain in *Soil Taxonomy*. **Question 48, Texas**

The emphasis (on the abrupt textural change) was placed in the Udolls, the Aquolls, the Udalfs, (and) the Aqualfs, because in these soils this abrupt textural change results in a perched water table when the soil remoistens in the fall and winter. This produces a serious problem for the plant and for the highway designer. Soils of drier climates may have this abrupt textural change but don't have the perched water table above the argillic horizon. Above the natric horizon you normally expect some perched water. You have evidence of the perched water in the presence of an albic horizon above the argillic or the natric horizon.

As the soil gets drier, you have a similar interpretation that this abrupt textural change indicates considerably greater age than a soil without such a feature. And yet it does not have the same significance for the use of the soil. An abrupt textural boundary was also used to define some of the "pale" great groups in the drier soils, but since these do not have the albic horizon above the argillic horizon we did not use the abrupt textural change at quite the same categoric level as we did, say in the Albolls.

We have some instances where it (a perched watertable) is normal in soils with natric horizons. There will be some perched water because they normally have an A2 horizon above the natric horizon. It normally might be quite thin but it would meet the requirements of an albic horizon. At Lubbock, I found that nobody was familiar with the European work on the genesis of this albic horizon where it results from a perched water table. The process they called ferrolysis results in the destruction of the clay rather than its removal by eluviation. It explains some things that we never did understand about our Great Plains Planosols, say, in the Middle West. It is a German work, I don't have the reference with me, but it's gaining quite wide acceptance in Europe. I think you will find a reference to it in the legend of the U.N. FAO Soil Map of the World, Vol. 1., Brinkmann. **Question 65, Minnesota**

(Why do abrupt boundaries with less than 20 percent clay in the eluvial horizon require only a doubling of the clay content within a vertical distance of only 7.5 cm?) Laminar argillic horizons in the sands have abrupt boundaries, without question, but they did not interfere so much with the soil permeability as did the abrupt boundaries with the illuvial horizons that had more clay than you have in the sand. Laminar abrupt boundaries are actually beneficial to the farmer in that they will at least double the available water-holding capacity of such a soil because the water hangs in the base of the clay laminae. **Question 48, Texas**

1.2.1 Lithologic Discontinuities

(With lithologic discontinuities) we are concerned primarily, I think, with two distinctly different situations. One, there is a serious change in the pore-size distribution which causes water either to hang above or below the lithologic discontinuity. If you have silty material over sand, the water will perch in the silt, and with difficulty enter the lower material. If you have sand over silt, the water will perch where the pores become smaller. In either case, if there is a marked contrast in pore sizes, this concerns us at the family level.

The other situation is in the identification of an argillic horizon where you have a finer-textured original material at some depth in the soil so that there is some inherited clay, and the change in the percentage of clay may be entirely due to the stratification of the parent material, or it may be in part due to the stratification and in part due to accumulation of translocated clay. Different people recognize lithologic discontinuities at different intervals, and some people can see them in nearly every soil, and some people can almost never see them. This is not a problem with an easy solution. The definition of the argillic horizon is intended to allow one to bypass the percentage of increase required for an argillic horizon by substituting the percentage of clay skins in the finer-textured material. On the field excursions of the committee on low-activity clays where the identification of an argillic horizon was commonly a problem in the field; some of the participants would see several lithologic discontinuities in the data of the same profile where others would see none. How significant these changes should be

before one abandons the use of the percentage increase in clay, I frankly do not know. Most of my experience in northeast, midwest states rarely gave us many problems. We did have some terrace soils that appeared to have a clay pan and yet there was no evidence in the field of any translocation of clay, because the late Holocene terraces seem to be purely a stratification. **Question 89, Texas**

I would, a little, prefer not to use, as the *Soil Survey Manual* provided, a Roman prefix for something that is similar enough that I cannot distinguish it in the field. A small difference in the ratios of fine and medium sand can become very large ratios, there is a continuum there and it is difficult to say precisely when one should recognize the lithologic discontinuity. Dr. Arnold can see one in every soil, even in loess I believe. I think one should be able to identify it in the field. **Question 90, Texas**

2.3 Aquic Moisture Regime

I think I know what you are talking about, (i.e. chroma that is, most of the time, above 3 in wet soils). I ran into this in the West Indies and Venezuela. In the Ultisols we do not require a chroma of 2 or less for the soil to be classified as an Aquult. We accept low chromas as evidence of wetness, but we also accept a hue of 2.5Y or 5Y as evidence of wetness. In the intertropical regions I ran into this over and over again, very wet soil that had a 2.5Y hue and had prominent mottles; in every order in which I found these wet soils.

I did propose then that we modify our evidence of wetness in the intertropical regions by adding to Alfisols, Mollisols, Oxisols, and Inceptisols, the same status that we have now for Ultisols. So that a mottled horizon with a 2.5Y hue and a chroma of 4 or 6 would be considered to have evidence of wetness. It must be mottled, of course, before you can accept the hue as indicative of anything, because there are plenty of sediments that start out with a 5Y hue, and as they weather they may get a redder hue. There's plenty of Mollisols around here with a 2.5Y hue, too, but without the mottles. **Question 187, Minnesota**

Once you form a mottle there is no way to get rid of it (after free drainage is established), except by mixing from animals or plant roots. Once the iron has gotten there in a segregated form there's no way to diffuse it. **Question 191, Minnesota**

I don't know (how long mottles are retained in a soil), but I'm reasonably confident it's a matter of some millions of years, unless you have some biologic mixing of the soil. **Question 192, Minnesota**

2.4 Densipan

I first ran into the densipan in New Zealand and Australia in 1959, but didn't understand what they were. I went back to my notes on that trip, and I found that I tried drying them and seeing whether they would slake, and they did. Then I forgot about that slaking, and I proposed a great group of Duraquods, because this was more like a duripan than any other pan that we had at that moment. Although, as the duripan is now defined these are not cemented at all and they are merely extremely compact. When I got to the West Indies, I found these again, but overlying an argillic horizon. My original proposal for the densipan was that we would require a great group of Densiaquults. Well, I got some feedback from that little note that there were similar pans in the wet Spodosols in Malaysia. When I got to New Zealand I started to try to find a duripan in an Aquod. According to our theory about the formation of a duripan we must have a period of dryness in the soil, by dryness I mean below wilting point to precipitate the silica. It shouldn't have occurred in an Aquod.

I spent a great deal of time trying to find the duripan in the Aquods in New Zealand and they were all densipans. We have them, then, in Ultisols and Spodosols. They make a soil

uniquely worthless because the densipan is an albic horizon, that for some reason has becor extremely compact, the reason being unknown. The roots then are restricted to about t surface 10 to 15 cm. Below this depth the roots cannot penetrate. Therefore, a very shc rainless period is going to seriously effect the use of the soil. The ability to store water virtually nil in soils with densipans. They are very wet, of course, after a moderate ra because the water is perched above the pan and the soil becomes saturated above the pan to t surface. The sugar plantations in Guyana have tried cultivating these soils because they ha large fields and they want to farm the fields rather than the soil. Their experience has be that it is useless to try to harvest those. They don't produce anything. **Question 109, Texas**

In the West Indies, in South America, the only densipans I know are above an argill horizon in an Aqualf. **Question 41, Minnesota**

I don't know (whether or not the densipans reform after ripping or deep plowing). V did try the effects of one or two dryings on a remolded densipan. The initial bulk density the dry remolded densipan material was 1.7, which is about the limit which roots can penetrat normally if it is 1.7 to 1.8 or higher you cannot get penetration of roots. I suggested that th might reform after ripping. The experience of the sugar plantation in Guyana would be su that would discourage the attempt to farm such a soil if it occupied a large part of the fiel **Question 110, Texas**

In Venezuela we sampled one and took it to Maracaibo and dried it. We wanted to ru tests on it to see if we could get some measurements there. So we got the professor from tl university to bring his penetrometer into my office and he studied the problem. We had chunk that would've been 60 centimeters in diameter, something like that, and 15-2 centimeters thick. He looked at it. He had his little penetrometers. We went back to his offi and he brought out a large penetrometer. About 3 feet long that you could almost stand on. S he applied pressure to that and he increased the pressure. Presently he broke the fragment ar his penetrometer was bent. It did not give. We abandoned the penetrometer test because w had no more machines. We should have confined it. **Question 148, Minnesota**

Yes, (this is the type of thing that is referred to in the last paragraph under the duripa horizon in *Soil Taxonomy*). That paragraph, where I referred to a third kind of duripan th forms in an albic horizon, is in error. It is not a duripan, it is not cemented, it is n indurated, it is merely compact. The compaction is so pronounced that it is impractical to bo a hole or to dig with a spade. To get through it, one must use a bar or a pick. Having dor that, one can break out large chunks that come away with an abrupt lower boundary to th argillic horizon or the spodic horizon. **Question 111, Texas**

The albic horizon in a soil as an Alboll or an Albaqualf in Illinois and Iowa, like Putna and Cisne, when dry, has some of the characteristics of the densipan in that you spend five ten minutes in getting an auger through it, you grind at it. But it differs distinctly when th soil is wet; there is no resistance to an auger, in those albic horizons. I don't know of measur of the bulk density from these albic horizons. I saw one profile in northern Michigan in whic I was a little puzzled about the nature of the albic horizon. It was quite difficult to get throug with the auger. When I got through, the surface water ran down the auger hole an disappeared. **Question 96, Minnesota**

A densipan is an albic horizon (and not compact glacial till with a density approaching 2 to begin with. It has the 'powers' of an albic horizon and it has the position of an albic horizo It is shallower and I would say a density of 2 would be rather rare when you restrict yourself t the fine earth fraction, even in drumlins. If you include the gravels, it's not too difficult to g up to 2, but not if you take the fine earth fraction. **Question 93, Minnesota**

2.5 Fragipan

The origin of the term (fragipan) was Latin *fragilis*, for brittle, because, in some parts of the country it was called a brittle pan. I coined the term about 1948, I think. It was when we were trying to improve the '38 classification and I was chairman of the committee on Planosols and I realized there were at least three kinds of Planosols in the U.S. - those with clay pans, those with fragipans and those with duripans. **Question 37-38, Minnesota**

There are enough problems in identification of a fragipan, that we eventually decided in the U.S. that it was an either/or situation, that the soil had a fragipan or it did not have a fragipan. But we did provide for intergrades where the brittleness was observable in an appreciable part of the fabric of the horizon, but the roots were present at intervals of less than 10 cm. For example, page 129 *Soil Taxonomy*, we provide for a Fragic Glossudalf. They are like the typic except that they do not have a brittle matrix. It is much simpler to define these subgroups on the percentage of the matrix that is brittle when moist, than it is to establish a new diagnostic horizon that is somewhere between a fragipan and no fragipan. **Question 38, Leamy**

I think there is little question that the definition is completely inadequate. There is no operational definition possible at this moment. There is probably no diagnostic horizon that has been the subject of so many doctorate theses, and we still don't know very much about it. It is quite possible, and that is implied in *Taxonomy*, that there is a 'cement' of some sort in the fragipan. But it's not necessarily the same in all fragipans. The studies using agents to remove silica or aluminum or iron show that one pan is aggregated by one treatment, and another by another, but a single reagent does not 'cement' all fragipans. So it seems likely to me that there is more than one kind of cement in different fragipans, and yet, we have no general theory whatever to account for this. **Question 37, Minnesota**

The only thing I would know (to emphasize in a definition of a fragipan) would be the brittleness when the soil is moist or wet. The brittleness is weakened, compared to the dry pan, but still the brittleness remains when the soil is moist or wet, in a weaker form, but detectable by the fingers on a sample that hasn't been disturbed by an auger. (Brittleness is the sole common characteristic between the things that are being called fragipans in terms of an operational definition) that's all that I know of. **Question 39, Minnesota**

The distinction between fragipans and compact basal till such as one gets on a drumlin, was discussed at some length at Cornell and the same problem would exist here in Wisconsin and Minnesota. The very compact tills are as much a barrier to water and root development as is the pan, and yet one can not blame all pans on compaction by glaciers when one sees them in loess in Mississippi and Louisiana. Those have never been glaciated and, so far as we know, have never been frozen at any time. Fragipans in Belgium and Scotland are commonly attributed to permafrost but this is speculative at this moment, so far as I am concerned. If permafrost forms fragipans, Dr. Reiger should have been finding some in Alaska. There are many who do in Scotland and Belgium. **Question 37, Minnesota**

(Studies made in Minnesota show that the fragic characteristics go on down into the C horizon to considerable depths; they lack polygonal structure and the proposal is being made to drop the fragipan from their classification.) I saw one soil or two on a noncalcareous till. I think the series was Nokay. I thought it had a fragipan. I have to go back to my notes but I remember telling Nygard that I thought it was a fragipan. Now, I don't know whether that's one of those that you're involved in here. I've also seen, on drumlins in this part of the world (Minnesota), an extremely compact till. They have the same sort of thing in New York State, particularly on drumlins, the till is extremely compact. They have been discussing in New York State and New England how these soils should be classified, as shallow families with a paralithic contact or as soil with a fragipan. The influence of the compact till is the same as that of the fragipan in stopping movement of water and preventing entrance of roots. **Question 194, Minnesota**

I would say that, to the best of my recollection, there is no strict requirement of polyhedrons in the fragipan, because in my experience, as the climate becomes more humid, the polyhedrons tend to become larger and larger until you get only discontinuous leached cracks that do not completely surround the polyhedron. Yet, they have all the characteristics of fragipans except for this failure to form complete polyhedrons; they're incomplete. I think I pointed that out in the peudic regimes. They don't always have complete polyhedrons. **Question 194, Minnesota**

If the properties are not pedogenic, if they are properties of the basal till, I would not want to include it as a fragipan. There are so many that have formed in loess, they have an affinity for parent materials. In these you can not blame the pan onto compaction by ice. It can only be pedogenic and I might comment, I guess I have already, about freezing. It always puzzled me why there were no fragipans in the loess in southern Wisconsin and northern Illinois until I realized that these soils freeze deeply most years. You have a January thaw that takes away the snow. Then a cold front comes down and you get frost down to 5 or 6 feet. I don't think you ever will find a fragipan in such a soil. We never have yet; or anything that suggested one. I think that deep freezing has loosened the loess and prevented the formation of the pan that occurs beginning at St. Louis all the way down to the Gulf of Mexico where it doesn't get cold enough for the soil to freeze deeply. And we find them in the more northern areas, sometimes even with cryic temperature regimes, but in snow belts where the snow insulates the soil. In the middle of the coldest month you can go through the snow and find the soil is unfrozen below. **Question 196, Minnesota**

We have in Belgium, in the loess, similar problems (as in Ohio with fragipans in one field and not in another field). Forest on one side of the fence and cultivated field on the other. They have the color pattern of the fragipan in the cultivated field but no pan. We have distinct pans in the area under forest. I doubt there that it would be due to freezing. It could be but I would suspect not because it doesn't get as cold there as it does in southern Minnesota and Wisconsin and northern Illinois. **Question 197, Minnesota**

I tried once to get an experiment in Michigan on the effects of freezing on fragipans because, in my experience, the fragipan in nature never freezes and I wondered what would happen in Michigan when the forest was cleared and we have a bare field lying there through the winter and frost would reach to depths of greater than the fragipan. I wondered what would happen to the fragipan. I never could get that study off the ground. **Question 44, Minnesota**

The genesis is not clear as to why we get this compaction other than it is a zone of low biologic activity. The soil is not frozen even though it has a cryic temperature regime. If you have a fragipan you will find the soil doesn't freeze to that depth because of the snow mantle. There is no frost action to loosen the soil. It is virtually free of small animals except, perhaps, in the cracks between the polyhedrons. It is also virtually free of roots except in the same place. The roots are frequently very flattened in these cracks indicating that they are unable to compress the soil any further, it is as compact as pressure of the growing root can make it. I would comment that I have learned a little bit about fragipans since I wrote *Soil Taxonomy*.

In New Zealand, I found fragipans are normally in soils that have an ustic moisture regime, not an udic moisture regime. It is so typical in New Zealand that if they do find a fragipan in a soil with an udic moisture regime, they think they must be misjudging the moisture regime. These are in noncalcareous sediments, mostly loess. Fragipans like loess and glacial till in particular, noncalcareous, primarily from [I'll add the name later of the rock]. The rocks are abraded by the glaciers and by stream action on the mountains and the sediments are blown into the upland and fragipans are normal with an ustic moisture regime in New Zealand. I think we can consider that in the U.S., the fragipans are normal in the humid areas, but they are absent in the soils that have a high carbonate content close to the rivers. I think, perhaps, the carbonate has something to do with preventing the formation of the fragipan, but why it forms I don't know. **Question 104, Texas**

(When dry material from a fragipan is dropped into water) it fractures into gravel-size fragments for the most part. Mostly they will be less than 7 1/2 centimeters. I don't know

whether an individual sand grain will fall off or not; they probably will. You understand (the size) depends a little on the operations you use when you are slaking. If you put in a large chunk, the sides slake off but they compress the interior of your large chunks. The fractures may form there, but it doesn't fall apart because it is held by the fragments around it. It does not slake like a densipan which simply becomes a fluid mixture of water and silt and sand-size particles and slakes, forming an angle of repose of less than 15 degrees as a mud. The fragipan does not slake in that manner, but it does fracture. The duripan is cemented to the point where the dry fragment will not fracture when put in water. That is an operational distinction between the fragipan and duripan where one leads into the other. **Question 39-40, Minnesota**

(The fragments) should be air-dried or oven-dried. Air-dried is the normal procedure because we can do that in the field and we don't have an oven in the field. I do not know (the results of repeated wetting and drying), I haven't read of such trials. Many of the theses on fragipans are unpublished and only some are in the literature. I don't know the answer there. On the densipan we did try this approach. We slaked both disturbed and undisturbed material and the bulk density of the dried slurry, in either case, was the same and it was 1.7 g/cc. **Question 40, Minnesota**

(For the identification of the distribution of roots in a fragipan) you must have a pit for your observation. The roots of perennial plants are normally able to enter the leached non-brittle material between the browner brittle interiors of the polyhedrons. These roots, if woody, are often greatly flattened by the pressure. In the absence of a plant that has woody roots the fine fibrous roots generally penetrate deeply enough that you will find either the living roots from this year or perhaps dead roots from last year in the great cracks. In some instances, and again under grass, in New Zealand there is a layer that is very hard when it is dry. One might think it was a fragipan from the difficulty you have in digging with a spade, but if you break the polyhedrons into fragments, you'll find the fine roots are everywhere within the interior of the polyhedrons. This is the limit between the fragipan and not fragipan in New Zealand, but when digging one wants to call these fragipans. The plants don't seem to realize that they are there. Alfalfa is not a common plant to grow on a soil with a fragipan. After it has been there for a year or two, the farmer will plant another crop. **Question 43, Minnesota**

The size of the units (are) pretty well standardized throughout *Taxonomy* at ten centimeters or more that are free of roots. **Question 39, Minnesota**

(It is) very difficult (to determine the lower limit of the fragipan, it is) sometimes extremely diffuse. In unglaciated areas in central Tennessee, the base of the fragipan is something that is even worse than the base of an argillic horizon so far as two pedologists agreeing within 50 centimeters or a meter. In Tennessee, where we had loess over sand, in the loess you could trace the gray streaks down to the base of the loess. They went down even to the sand although that was not a fragipan. It was a loamy sand or sand. But gray streaks went right on down, well down into the sand. I never could understand that, frankly. **Question 195, Minnesota**

I got a lot of questions about (trying to determine where the bottom of the fragipan is when it is underlain by dense basal till) at Cornell. The different states handled it differently. Some consider it just a compact till and not a fragipan. Other states considered this to be a fragipan in the basal till. The practice is not uniform. There is no reason for them to worry about where the base of the fragipan is for *Soil Taxonomy*. There is no question that there are compact tills that behave like a paralithic contact. These are just basal tills and particularly on the drumlins in the midwest in Wisconsin, for example. In New York the till in these drumlins is extremely compact, more so than most fragipans. **Question 105, Texas**

The reason for the statement that there is an illuvial horizon above a fragipan, unless the soil has been truncated, is simply that, in the experience that I have had, such a horizon always exists. Because we do not understand very well the genesis of the fragipan, there is no genetic reason to this. It is only a matter of general observation. It is very likely, now, that I have had an opportunity to look at fragipans at some length in New Zealand that the illuvial horizon that I described is one in which ferrolysis has been an important factor, ferrolysis being the destruction of clay under alternating wet and dry conditions. **Question 38, Leamy**

(Concerning whether there is a possibility for a threshold of silt content necessary for fragipan development) I think not, unless you substitute the very fine sand and add that to the silt. We do have fragipans in Belgium in loamy very fine sands that have virtually no silt. The very fine sand content is quite high. I think, just as the textural particle-size classes we had to combine the very fine sand with the silt. I think there is a point somewhere around that which probably is a critical limit, but not at 50 microns. **Question 107, Texas**

(Some states have used Bx horizons and some states have used Cx horizons, but) we haven't said that a fragipan is a B or a C horizon. It may be either as far as I am concerned. Some soils are obviously a B horizon. I don't know that it is so obviously a C horizon in many soils, but it could be in a Spodosol, for example, because the C horizon is not well defined. In a Spodosol you stop the B horizon when the color changes and you start C horizons. Actually there has been a lot of alteration in the fragipan. I think I said all that I knew in *Soil Taxonomy*. **Question 104, Texas**

I don't know about the proposal to drop the fragipan as a diagnostic horizon. That may be under discussion in the Northeastern states. The argument there has been about whether or not the basal till is or is not a fragipan. They are still arguing about that. My experience with these soils is quite limited and I can only say that their judgment would be much better than mine. The effect may be the same. I don't know, some of these basal tills are very compact, particularly on the drumlins. They are in effect a paralithic contact. If they were shallow, one would have to recognize a shallow soil just as one does when he has a paralithic contact with bedrock. There are fragipans in New York state, I am sure, and the few soils that I saw in New Hampshire and Maine. Spodosols seemed to me to have a fragipan. This is what they are arguing about today, whether it is a pan or whether it is just a compact till. **Question 112, Texas**

2.6 Mollic Epipedon

2.6.1 The Mollic Epipedon, an Illustration of the Evolution of the Limits of Taxa and Definition of Criteria in the Development of Soil Taxonomy

Base saturation by the sum of bases seemed to give reproducible figures for noncalcareous soils, but in many parts of the Great Plains, the soils were calcareous and the exchange capacity by that method was obviously unsatisfactory. We used then, in the soil survey laboratories, the sum of bases for the noncalcareous soils which were generally in the more humid parts of the country. We used the ammonium acetate method for the Great Plains which had many calcareous soils. We had troubles in making comparisons between the two methods. The numbers of data were quite limited in published form. Our data in the laboratories suggested that in Mollisols the base saturation by ammonium acetate never dropped below 50 percent. In the humid regions, the base saturation by either method was frequently well below 50 percent, but if the soil had received applications of limestone, the base saturation in the epipedon was readily changed. We proposed 50 percent by ammonium acetate as a limit for the mollic epipedon with the idea that the people in the agricultural experiment stations would go through their unpublished data and criticize that limit. No criticism was ever received from any of them. This is true for most of the limits that you will find in *Soil Taxonomy*. The proposals that were not criticized were carried over from one approximation to another, and finally became more or less entrenched in *Soil Taxonomy*. What the reasons were that there were no criticisms, I do not know. It may be that the initial proposals, based on very fragmentary data, were reasonable. It may be that there was simply a lack of interest in the agricultural experiment stations in going through their filing cabinets and digging out their unpublished data.

The 50 percent limit for base saturation for the mollic epipedon represented a preliminary judgment as to how low that base saturation might go in the soils that we wanted to classify as

Mollisols. No criticism of that limit was received to the best of my recollection. It was an initial approximation based on limited data, and it has come right down to us in *Soil Taxonomy*. As a general rule, most of the limits about which questions have been asked had the same history. **Question 61, Cornell**

2.6.2 Criteria

(The chernozemic A) was the only horizon that I could find that was common to the soils of the 1938 classification and suborder of dark-colored soils of the subhumid and humid climates, that is, the old Chestnut, Chernozem and Prairie Soils. I could find no other common feature they had. When combining that with a high base status, we were able to arrive at the concept of some diagnostic horizon that would tie those soils together in the taxonomy. This was where they traditionally had been, tied together but without a definition. When you examine the data, the descriptions of the soils that had this range of moisture from the Chestnut to the Prairie, it was immediately obvious that the drier the soil became the thinner was this dark-colored A horizon, which we began to call chernozemic A to distinguish it from the more acid ones of the humid forested region or not necessarily forested, particularly under heather in Europe.

If we put a limit of 25 cm of thickness as the minimum that we would recognize, then we exclude the drier range of the soils in the Great Plains. If we develop a sliding scale, based on the depth to secondary lime, with the maximum thickness of 25 cm, then we could tie them all together. This was what we tried to do in defining the mollic epipedon. In so doing, we included some of the former intrazonal soils like the Rendzina. There seemed to be no good way to exclude them. At the time that we were developing the taxonomy, these soils on limestone were commonly called Rendzinas even though they would have been called Chernozems in the absence of the limestone. This was the reason that we restricted the Rendzinas to soils that have udic moisture regimes. The Rendolls are restricted to udic moisture regimes because there they are only truly what was considered intrazonal. **Question 48, Minnesota**

(The 0.6% carbon required for the mollic epipedons) is a very low limit. It comes from a few sandy soils on the Great Plains in the southern United States where the wind action has winnowed the carbon and the clay from the sand without appreciably changing the color. If the limit were put perhaps at 1% instead of 0.6%, these particularseries would have been split. And you must remember throughout the whole *Taxonomy*, the purpose was to avoid splitting series unless there was some distinct advantage to doing so. We would have preferred to have a sliding percentage of carbon according to the, perhaps, percentage of clay. But we had inadequate data to develop such a scale at the time we were working on *Soil Taxonomy*. **Question 28, Venezuela**

(The difference in color between the soils that formed under grass and the lighter-colored soils that formed under a forest vegetation) was the basic emphasis used to define the mollic epipedon as having a color value of 3.5 when moist, less that 6 when dry. It made a fairly clean separation between the grassland and the forest soils in central and northern United States. It also seemed to make a fairly clean distinction between the Ultisols and Inceptisols that had a dark-colored organic epipedon. Most of the latter had a grass vegetation instead of a forest. There were, of course, exceptions. There are a number of soils having ochric epipedons that had a grass vegetation when the settlers arrived in the United States but the evidence has accumulated since then that the grass was of very recent origin and that the soils had previously had a forest vegetation so that they really developed under forest. All the grass took over during the late middle Holocene times. **Question 29, Venezuela**

We didn't try to cover (the grey area between the so-called incipient A2 horizon or even a well developed A2 horizon with platy structure that's dark colored in the albic horizon). If the colors, dry and moist, are dark enough for a mollic epipedon, the distinction of the platy structure was not brought into *Taxonomy*. I had long discussions in Iowa about whether or not, say in the loess in Northeastern Iowa, we could identify three or four series - the one without

any forest influence, the one without any grass influence showing in the profile, then a prairie soil intergrading to a forest soil and the forest soil intergrading to the prairie soil. And the general feeling in Iowa was that we could only recognize one intergrade, not two. And having had those long discussions when we got into the business of writing *Soil Taxonomy* we did not provide for both intergrades, only for one, the forest soil that still shows a prairie influence. **Question 161, Minnesota**

2.7 The "Pale" Concept

The concept involved in the term "pale" at the great group level was proposed fairly late in the development of *Soil Taxonomy*. It came about as a result of geomorphology (studies) of the coastal plain soils in the southeastern United States and the Aridisols and the Mollisols of the arid and the semiarid land of the southwestern United States. The concept that was held when I started working in soil science was of the lowering of the land surface on the interfluves and the replacement of this concept by the notion of linear retreat of the slopes was much later. It was pretty much assumed by pedologists of Europe and the northeastern United States that all soils were about the same age, and that the differences were due to other kinds of soil-forming factors. When we started the geomorphology studies, we found that the soils in any of these landscapes which were not covered by the glaciers was quite variable. Some of the soils were very early Pleistocene or Pliocene in age, and others were Holocene. We began to look at the differences in these soils with such greatly varying age. Obviously, if one goes back to Pliocene or even early Pleistocene there have been a number of differing climates under which these soils developed.

In the southeastern states, the Ultisols, the older surfaces which have been dated by Dr. Daniels and associates at well over a million years, we found that we had something very similar in chemical properties to many Oxisols. They were mixtures of quartz, kaolin, and free oxides. When we went on to the late Pleistocene or even early Holocene surfaces in the coastal plain, we found soils with completely other suites of mineralogy. There were many feldspars, we had montmorillonite and illite in place of kaolinite, although mostly they were mixtures. The activity of the clays were much higher than in the soils of very old landscapes. So we tried to define the Paleudults in terms of measurable properties, not in terms of age. So we put the limit of weatherable minerals on the silt and sand fraction, on the Paleudults, and the thickness of the B horizon, to distinguish them from the Hapludults.

Amongst the Aridisols and the Ustolls, we found that in the Holocene soil we never had appreciable areas with petrocalcic horizons; we never had thick argillic horizons, we had thin argillic horizons. On the older surfaces in the western states, we normally had a petrocalcic horizon that had formed, which was a barrier to movement of water and roots. So the "pale" concept of the Aridisols, as an example, included two kinds of soil, one with a very thick argillic horizon and clay texture in the argillic horizon, and an abrupt boundary between the argillic horizon and the overlying horizon. We also had the old soils that had a petrocalcic horizon at a shallow depth. If the carbonates which were present in the parent material, or came in the dust and rain, were adequate we get petrocalcic horizons developed in sediments that had virtually no calcium to begin with. So we developed the concept of the Paleargids and the Paleorthids; in the Argids to the presence or absence of the petrocalcic horizon, and according to the abrupt upper boundary and clay texture of the argillic horizon. This was our first opportunity to develop the "pale" concept in the Argids and the Orthids, so at the subgroup level, we distinguished these as typic and petrocalcic subgroups. In the glaciated parts of the U.S., these "pale" great groups do not exist. This is where soil science began--in the Soviet Union, in Western Europe, and in the northeastern United States. **Question 44, Cornell**

There is no question but that this (that these "pale" features, like in the Paleudults, may be more a condition of the origin of the parent material being highly weathered, and not the fact that the soil is formed and has been there a million years) is a possibility, and it was recognized at the time that we developed *Soil Taxonomy*. In our southern coastal plains, the sediments coming from the Piedmont were unweathered when they were laid down, but sediments coming

from Oxisols might arrive completely weathered, and one might get "pale" great groups in relatively late Holocene sediments, just enough time to develop an argillic horizon. We hope that the limit on weatherable minerals would separate these, but it is not necessary that they do. A soil coming from a very small watershed may consist of completely weathered sediments. The soil coming from a relatively large watershed will normally have some areas of unweathered sediments that are transported to mix in some unweathered minerals, but the small watersheds could get us into trouble. This was not only the case in Nigeria where you experienced it but also we have run into it in doctorate theses from Malaysia where we cannot identify weatherable minerals in relatively late Holocene sediments. The solution to this has been discussed at some length at Ghent, a proposal has been made to resolve it, but whether or not that will be acceptable to other people I do not know. **Question 45, Cornell**

The concept of the "pale" great groups was intended to group the soils of very considerable age into separate taxa from those of late Pleistocene or Holocene age. We have no good geomorphic studies of the Paleboralfs, but we do have in these soils evidences of downward movement of the argillic horizon as they have tongues of albic material going into the argillic horizon, with tiny remnants of argillic horizon remaining in the albic horizon.

We observed that these albic horizons vary enormously in thickness. On the more stable surfaces, we can find these albic horizons to be more than two meters thick. There is always an underlying argillic horizon, and at the contact between the albic and argillic there is evidence of destruction and downward movement of the argillic horizon. We, therefore, made an assumption that, when the albic horizon becomes very thick, this is an evidence of considerable age in the soil. Those of late Pleistocene normally have an albic horizon of less than 50 cm. But there are also Boralfs with more than 2 m of albic horizon. This was a characteristic we could use for the Boralfs. The destruction of the argillic horizon is not so obvious in the Ultisols. So in the Paleudults, Paleustults, in order to distinguish the soils with the long-term genesis of the soil, we have to emphasize the properties of the argillic horizon instead of the albic horizon. **Question 117b, Cornell**

(In the midwest there are no "pale" great groups for soils like the Yarmouth Paleosol. Was the bias towards the red color?) The red hues that enter into the definition of some of "pale" groups are there in the definition simply because all the soils we knew that we wanted in that group did have the red hue or they had the mottles which are not indicative of wetness. They had one or the other in the definition. The mottles have very high chromas compared to the mottles in the wet soils of the Midwest. We did not consider anything about the buried soils in the Middle West that have a red hue, because of that red color with aging. I have seen no explanation. They blame it on temperature but that is a little hard for me to accept because there are so many soils in the tropics that are not red but the temperatures are high. Somebody is going to have to study the form of iron perhaps by methods not yet available to find out why the older soils are redder. **Question 69, Minnesota**

(For Paleustalfs) the absolute clay increase must be met at the top of the argillic in either a 20% increase within 7.5 cm or a 15% increase in 2.5 cm. It starts at the top of the argillic horizon from the material above the argillic and the material in the argillic.

(Why is there no provision for the deep clay distribution in the Paleargids as there are in the Paleustalfs?) I presume that's because we never found that deep distribution of clay in Aridisols. The water just doesn't go deep enough or hasn't gone deep enough to move the clay and to produce the clay by weathering in such deep horizons as a 1.5 m. Theoretically, you should be able to find some polygenetic Argids that have such a deep clay distribution. They haven't been reported to me, I didn't run across them in the development of *Soil Taxonomy*. **Question 135, Texas**

(If the argillic horizon is lamellar, do we distinguish between pale- and haplic great groups using the same criteria as with a continuous argillic horizon?) In general the requirements for the "pale" great groups are that the clay content does not decrease from its maximum by as much as 20% within the depth of 1.5 meters. If the argillic horizon consists of a series of lamellae the clay content in the inter-lamellar areas will almost always be 20% less than that of the lamellae themselves and we would interpret this generally to exclude the soil

from a "pale" great group and throw it into a haplic great group. While some psammentic subgroups are provided in "pale" great groups, the exclusion from the typic subgroup of the "pale" great group requires that the particle size be finer than (loamy fine sand). The typic subgroup is required to have an argillic horizon that is continuous horizontally, that is continuous vertically for at least the upper 20 centimeters and that has a texture finer than loamy fine sand. The soils getting into the "pale" great groups then can be put into the psammentic subgroups on the basis of the loamy fine sand texture of the argillic horizon, rather than on the presence or absence of lamellae. **Question 35, Venezuela**

(Should soils be classified as "pale" great groups that have a thin argillic horizon over shale and the clay percentage does not decrease with depth?) There are "pale" great groups in several orders, Alfisols, Aridisols, Mollisols, Ultisols are examples, and in each order or suborder the definitions of the "pale" great groups vary. In the Ultisols, the "pale" great groups must, in addition to the clay distribution, lack very many weatherable minerals. In the other groups the definition varies, suborder by suborder, but have no relation to the clay distribution alone. In every definition there are characteristics other than the clay distribution and in addition to the clay distribution. In some "pale" great groups, an abrupt textural change between A and B is used as a part of the definition of the "pale" great groups. In others a reddish hue or mottles of high chroma are a part of the definition. It would not be intended to group a soil in a "pale" great group solely on the clay distribution. **Question 29, Leamy**

2.8 Placic Horizon

The placic horizon is a rather distinctive sort of horizon when you see it in the field. It is very thin; it is involute; it is hard; it makes a barrier to the water enroute just like any other pan, although it wasn't called a pan. We don't understand the genesis of the placic horizon as well as we understand the genesis of the spodic horizon, though there are many similarities in the composition. There are significant differences in the composition, in at least some placic horizons. The placic horizon consists of an accumulation of iron, aluminum, organic carbon, and manganese. Manganese has never been found, to my knowledge, in a spodic horizon. This accumulation of manganese may be on the upper or the lower boundary of the placic horizon.

We have no reasonable genetic theory that explains the development of placic horizons. We know from geographic correlation that they are always in soils with perudic moisture regimes. They are continuously moist throughout the year. Beyond this, we really don't know much about them. They can occur in very skeletal materials; they can occur in clays - very fine textured materials, normally, within a depth of 50 cm or 1 m. They are important to the movement of water in the soil; they are important to the growth of roots in a very different manner than the spodic horizons. Now, they have, like the spodic horizons, virtually completely pH-dependent charges and almost never a permanent charge. The major difference between them is the thickness, the barrier to water-use and the presence of manganese. Manganese is not always present in the placic horizon, but it may be. The studies I have seen suggested that, when the manganese is at the base, you have water moving laterally below the placic horizon from higher ground somewhere nearby. But manganese is a mark of some alternate oxidation-reduction process.

As I said, the placic horizon is a barrier to water and roots. It occurs in very unlike kinds of soil. We have them in the Hydrandepts which are the soils that are mostly oxides, clay-size, that dehydrate irreversibly in drying. We have them in Spodosols. They may be above or in the spodic horizon. We have them in Dystrochrepts. If we treat that as a spodic horizon, then, we put together some of the Hydrandepts with some of the sandy Spodosols and we get a class that contains soils about which we can make no statement. **Question 49, Texas**

2.9 The Rhodic Concept

(What is the concept that the term "rhod" is intended to imply?) It is primarily from the Rhodudalfs, the Rhodoxeralfs, the Rhodudults where we observed the same phenomena. We know from pragmatic experience that these dark red soils are intensively cultivated, that the structural problems are very easy to manage compared to the non-rhodic soils. In most Alfisols and Ultisols that retain an A horizon, or that even have been eroded into the B, the structure of the plow layer is critical to germination and growth of seedlings. The rhodic great groups, in the absence of any quantitative measures of the amount and form of the free iron, had to be defined on color. We know now that the free iron and its form are important factors in determining the pH-dependent charge on the clay. We have to accumulate more data on the amounts of free iron to see whether the definition can be improved, using color simplified identification in the field, and relates well to land use. In general, in *Soil Taxonomy*, we have de-emphasized color relative to all other classification. But this was one point in which we thought the dark red color was an important mark of an important property. **Question 46, Cornell**

There must be a genetic factor to have the dark red colors of rhodic great groups. Normally, this is because these soils were formed on basic or ultrabasic rocks. It is a different kind of parent material. To that extent, it illustrates the problem of zonality and intrazonality, where we have two different zonal groups, namely the Reddish-Brown Lateritic soils, and the Red-Yellow Podzolic soil, covering the same range of climate. They both were considered zonal soils, but the difference was due to difference in parent material. This perhaps was an error in the '38 classification, but it is also a fact that it should be impossible to have two contrasting zonal soils that have exactly the same geographic range. **Question 47, Cornell**

This is a general principal. When you find these apparently anomalous differences the reason was that somewhere in the U.S. there was a soil series that would get split badly if we wrote the definition in another way. To split the series would have added nothing to the interpretations we could make about the phases of the series. If we obtained no improvement, we preferred not to split the series. We have tried to keep together in the *Taxonomy*, soils that are similar enough that we can make some important statements about them.

In the U.S., the Rhodudalfs always have a red hue, as far as we now know. However, in other parts of the world, it is possible to find Ultisols, and I will cite the example from Tasmania where we have one lava flow a few hundred meters above sea level. We went from a mesic to thermic temperature regime on soils of the same lava flow -- same age. When one starts at sea level, we have the dark red colors of Rhodudults of the US. As the elevation increases, the hue becomes browner and the value remains the same. The Tasmanians did not think that these should be separated on the basis of the hue. So, we defined the Rhodudults on the color value and not on the hue. The Rhodudalfs, if we find some that are very dark brown in colors, might require a change in definition. **Question 117, Cornell**

2.10 Albic Horizon

(Not requiring a minimum thickness in the definition of the albic horizon) may have been pure oversight. Many of the Boralfs have a relatively thin albic horizon where the argillic horizon has a fine or very fine texture and, if plowed, this is mixed and cannot be observed anymore, but you can still observe the argillic horizon. The only place in *Soil Taxonomy* where I find the albic horizon used as a diagnostic horizon is in the suborder of Albolls. The minimum thickness of albic horizons in other kinds of soil would not be critical because the presence or absence of an albic horizon is not diagnostic to the classification. It was our desire, generally, to keep in the same series, in the same family, the cultivated and the undisturbed soil so that the series would not be changed by a few plowings. There are soils, such as the Boralfs, which may have a very thin albic horizon if the argillic horizon is fine or very fine in texture,

and these are kept together in the classification by not making the albic horizon diagnostic, rather we have used temperature, primarily, to define the suborder of Boralfs.

The albic horizon is normal in these soils and has been recognized by the Canadians as a diagnostic feature. They, however, do not mind the thinness of the albic horizon because they classify the soil on the basis of the presumed virgin profile, rather than what is there today. The other group where the albic horizon is common is in the Spodosols. In the Russian classification, the Australian classification, and the New Zealand classification, classified as Podzols, soils that had an albic horizon, irrespective of the nature of the B horizon--argillic or spodic. There has been in those countries considerable resistance to *Soil Taxonomy* because it does not use the presence or absence or the thickness of the albic horizon as a diagnostic in the classification. **Question 46, Texas**

(Would you care to comment on the lack of use of A2 or albic horizon as differentiating properties in *Soil Taxonomy*?) Well, I could sympathize with the desire to use the albic horizon as a diagnostic horizon. I would be opposed to using an A2 which is extremely difficult to define. In general we have tried to avoid the use of A, B, C horizon nomenclature in the *Taxonomy* because people don't agree around the world on what is an A, and what is a B, and so on. Mr. Giles had enormous problems in using the ABC terminology in the *Desert Project*. What was an A1, and what was an A2, and so on. The use of the mollic epipedon as a diagnostic horizon was undesirable, but I could find no escape from it to find some horizon or some property that would group the soils of the Great Plains that had consistently been kept together in every taxonomy. There are exceptions to most any one that you can find except of the presence of the mollic epipedon.

We commented on how A2 horizons have been used in various countries to classify the soil as Podzols if they have an albic horizon. This would have been possible but, I think, it is undesirable to group all soils that have an albic horizon into some categoric level because the albic horizon is produced by the removal of something. The processes that remove the clay that colors the ochric epipedon of most Alfisols, would not necessarily, obviously not, be the same process that produces the albic horizon above the spodic horizon. There is something very different going on in those soils. The end product may be the same, in that you stripped everything except the quartz which imports the light color of the albic horizon. **Question 75, Texas**

We observed that albic horizons vary enormously in thickness. On the more stable surfaces, we can find albic horizons to more than two meters thick. There is always an underlying argillic horizon, and at the contact between the albic and argillic there is evidence of destruction and downward movement of the argillic horizon. We, therefore, made an assumption that, when the albic horizon becomes very thick, this is an evidence of considerable age in the soil. Those of late Pleistocene age normally have an albic horizon of less than 50 cm. But there are also Boralfs with more than 2 m of albic horizon. This was a characteristic we could use for the Boralfs. The destruction of the argillic horizon is not so obvious in the Ultisols. So in the Paleudults and Paleustults, in order to distinguish the soils with the long term genesis of the soil, we have to emphasize the properties of the argillic horizon instead of the albic horizon.

2.11 Argillic Horizon

(In answer to the question that both the presence of clay skins and a clay bulge are required for an argillic horizon in most cases) surely, no clay increase can be required in soils that have been truncated or in soils in which there is a lithologic discontinuity giving rise to an argillic horizon even with less clay then in the surface mantle. In these cases where there is no increase in clay between the argillic horizon and any overlying horizon, such as a plowlayer, we do require 1 percent cutans. If the clays have 2:1 lattice minerals, the argillic horizon does not need to have clay skins, if there are skeletans in an overlying horizon. It is pointed out in the discussion, rather than the summary, that in some places particularly those with wet/dry

seasons, that special field studies are needed more than laboratory studies to identify the presence of the horizon with illuvial clay.

If the polypedon has a range in elevation and if the boundary between the surface and the heavier-textured underlying horizon is clear or abrupt, it may be necessary to trace the finer-textured horizon laterally to be sure that it is not a depositional feature. If the increase in clay is marked enough to be observable, and if the boundary is clear or abrupt, it is extremely difficult to assign the origin of the finer-textured horizon to differential weathering. Moisture conditions in the soil do not change abruptly but rather gradually. But in the discussion, rather than in the summary, it is pointed out that the field studies alone can realistically indicate the illuvial nature of the finer-textured horizon.

It also pointed out that the significance of the argillic horizon to soil genesis is not particularly more important than that of any other kind of diagnostic horizon. Too much attention generally has been given to the presence or absence of clay skins in soils. The important thing about the clay skins is that normally they have marked influence on the amount of nutrient elements that are cycled by plants. They have more nitrogen, phosphorus, potassium, than do the ped interiors. If the finer-textured subsurface horizon is not actually illuvial it is not so important to the plants as is the nature of the nutrient content of the ped coatings of the surfaces of the peds. This is the important thing in relation to plant nutrition. In many soils with extremely low fertility, soils in which the nutrients are maintained in the soil by plant cycling, the roots are able to reach the subsurface horizon and extract water because calcium is cycled and is present in the coatings on the peds; voids with coatings along which water carrying the recycled nutrients move. Without the calcium the roots cannot enter the subsurface horizons and, therefore, cannot utilize the available water and the soils become extremely droughty. **Question 33, Leamy**

(Concerning the source of the criterion of 1 percent or more of oriented clay in the argillic horizon) the assumption was, in this limit, that if the clay-skins could be identified in the field there would be a least 1 percent in thin sections. This assumption may not have been justified, and it was proposed for testing, but (because) no one criticized it, (it was left unchanged) when it came time to print *Soil Taxonomy*. One must first remember that the limit is 1 percent of the oriented clays not 1.0. A 10 percent error, therefore, is not only permissible but probably expected. If the point count shows 1/2 percent or more, the rounding of numbers will bring it to the necessary 1 percent. **Question 32, Leamy**

The basis (for the 3 percent, 1.2 ratio, and 8 percent, increase in clay content required between an overlying eluvial and an underlying argillic horizon at less than 15, 15 to 40 percent, and more than 40 percent clay in the illuvial horizon, respectively) was the ability of the field man to estimate the percentage clay. We wanted to set the limits at a point at which we could get reasonable agreement among the field men as to the change in the clay content. If the soil is very sandy, one could have 100 percent increase in clay, going from 1 to 2 percent clay, but you cannot estimate it in the field with that precision. There had to be some minimum limit for the soils with very sandy textures, and we thought perhaps the change with 3 percent clay might be enough that most field men could agree upon it. Similarly, at the upper limit, when you have 60 percent clay, what is the minimum change that is discernible in the field? We thought that probably most field men could tell the difference between 60 to 68 percent clay. In between, we use the 1.2 ratio because it should be discernible. If you have 20 percent clay, a change of 4 percent clay might generally be discernible to the fingers. Thirty percent clay is a 6 percent increase; these limits were set by what we thought field men could estimate. **Question 69, Cornell**

We, of course, can not use the ratio or the difference in clay percentages in soils that have been eroded and in which the plow layer's base is in the argillic horizon, there is no possibility of using any ratio there. Where there is a distinct difference in the parent materials as on some of the late Pleistocene and early Holocene terraces in the middle west, one can find a lacustrine clay that is capped by a silty alluvium or colluvium and one can get very similar clay distribution in those soils as in soils with argillic horizons. Along the Mississippi floodplain when the levee bursts, you get sand on a clay. There is an enormous change in the clay percentage but it doesn't bother anybody. Nobody I have ever met has wanted to say that was

an argillic horizon, although it does have some of the properties, in that it does perch water in the sand on top of clay. The usefulness then, would be restricted to soils in which there has been an appreciable clay movement or an observable clay movement as indicated by clay skins in the subsurface horizon. How far does that have to go in an untruncated soil before we want to say this is an argillic horizon? It is generally, I think, more useful on soils in loess than it is on soils in glacial till. The French use the ratio of 1.4, but they're concerned with soil such as you have in Ohio, the Hapludalfs, and the Paleudalfs and so on. The 1.4 would work well there but it doesn't work in the Mollisols. **Question 84, Minnesota**

We don't have enough hard data (to know if the value of the ratio of fine to total clay, in the illuvial compared to the eluvial horizon, as suggested in the definition of the argillic horizon, is more of a central value rather than a borderline value). The bulk of the measurements of fine clay have come from Ohio State laboratory, but we had fragmental data from North Dakota and a few other places and where an occasional soil had been studied but not on a routine basis. Only Ohio State, that I know of, at that time at least, had measured the fine clay. The definition changed gradually as a result of the introduction of that ratio in some of the early supplements to the *Seventh Approximation*. Some additional studies were stimulated and we ran into soils that we were confident had an argillic horizon but in which the ratio did not change appreciably. So (the ratio) was removed as a requirement and left as some sort of a supplemental observation that one might make in case of doubt, but it is not required at all any more. There are two qualifications there and I think the words are 'usually' and 'about'. We have very few data on Ultisols, the ratio of fine and coarse clay. It is very hard to find in the literature, and the Lincoln lab, so far as I know, does not yet make these except very occasionally for particular studies. **Question 152, Minnesota**

In my experience the ratios have been illuminating, but the data are largely restricted to soils of late Wisconsin age. If one were to get more ratios on older soils, one might find that the ratio has little meaning. But in the absence of data, its impossible to make a definitive statement. **Question 36, Venezuela**

(What is the reasoning for using the eluvial horizon as a reference for the required increase in clay for the argillic horizon instead of the underlying horizon?) There are several difficulties in using the C horizon as a reference point for identification of an argillic horizon. One is that, we would necessarily have to use the statement that "the clay decrease in an underlying horizon" because we have consistently avoided the use of A, B, and C in *Soil Taxonomy*. The pedologists can argue endlessly about what is B, and what is C without ever reaching an agreement and the base of the argillic horizon, as assigned by different pedologists, will then vary greatly in depths. Secondly, if there is a lithologic discontinuity near the base of the argillic horizon, then using the underlying horizon in such situations means that the definition cannot be applied universally or can only apply to some kinds of soil where the parent material is uniform. What then does one do when the parent materials are not uniform? In this situation one must have some other kind of reference, and the only universally applicable differential that I can find was the presence of clay skins in the horizon that we would like to call an argillic horizon. **Question 34, Leamy**

If I wanted to define the lower boundary (of the argillic horizon) in a soil that had no secondary carbonates or no lithic contact or paralithic contact, I would certainly specify that the decrease of 1.2 would be from, the maximum clay content in the argillic horizon. If you have an argillic horizon with a maximum clay content of 35%, then to get to the base of the argillic horizon you would use that 35% as your starting point or your reference point (35./1.2=29.2). **Question 86, Minnesota**

(By implication, the lower boundary would be where the clay content drops off to 80% of what it was at the maximum.) If I had agreed to anything like this, I would have insisted on something like that. But I might even insist on something greater. I certainly wouldn't word it this way. It would come at a lithic or paralithic contact. It could come at the top of the horizon of accumulation of calcium carbonate. I know the original intent as well as anyone and this wasn't it. (That "the lower boundary is determined using the same curve - as used for the upper boundary - and is the depth at which the clay content is less than that of the minimum requirement for an argillic horizon.") **Question 88, Minnesota**

It is very common in soils that have a skeletal or fragmental particle-size distribution to find coatings of clay on the rocks. *Soil Taxonomy* specifies the importance of clay coatings, clay skins on peds and in pores and in the skeletal and the fragmental particle-size classes. One does not ordinarily find peds because the structure is controlled by the rock fragments and in fragmental particle-size classes because there are no peds, and there are no pores other than the large ones that are not filled with fine earth. The coatings of clay on the rocks in skeletal and fragmental particle-size classes indicate that the clay has been moved, as a rule, because if one studies the coarse fragments, you do not find evidence of weathering sufficient to produce the clay by weathering in place. I have seen in Norway, and in Maine, skeletal or fragmental particle classes, with clay coatings that extended to a depth of more than 5 meters. This does indicate that the clays are in transit but it does not indicate an accumulation of translocated clay. In the majority of the soils in my experience, in which one claims coatings of clay on the rocks, there is not a sufficient accumulation of clay to satisfy the requirements of an argillic horizon. The clays seem to be in transit in leaving the soil completely but not really accumulating. Therefore, *Soil Taxonomy* refers to, accumulation of clay in clays skins, in pores and on peds but it does not refer to accumulations on rocks. It was the intent that the accumulation of clays on rocks would not be considered adequate for recognition of an argillic horizon. **Question 15, Leamy**

(The lamellae in the argillic horizon) can readily be a combination of both (pedogenic and geologic processes). These lamellae, however, where they are pedogenic, are stuffed with oriented clay. The finer-textured strata in the sands are not. The lamellae conceivably start to form at a point where there is a change in the particle size of the sands. They will follow stratifications if they can, but they often cut from one stratum to another in such a manner that it is difficult to imagine the sedimentary process that would be responsible. The probability is that these lamellae formed because at some stage in early development the down-moving water hangs, is withdrawn by evapotransporation and deposits whatever it is carrying at that point. This accentuates the difference that originally caused the water to stop there. Water stops when there is a change in pore size.

The lamellae that we have in the soils of Pleistocene age are not found in calcareous sediments. When you reach carbonates in the Pleistocene sands in Iowa and Illinois there are no lamellae below. It is difficult to understand this if it is geologic, because you may find them to a depth of 50 cm in one soil and 2 meters in another. In all cases they are in the noncalcareous sand. The argument for their being geologic would conceivably come from the tendency of these lamellae to follow stratifications in the sands. We get the same forms of the lamellae in the sandy Spodosols. These lamellae are restricted to relatively coarse-textured soils with low clay contents. I used to say that we had no lamellae in loess, but unhappily, the Belgians have found some. **Question 132, Texas**

(What is the background on the thinking that went into "bridging in sand"?) I would be inclined, in light of what I have seen in sand, to conclude that if I have the laminae with bridging that this probably represents translocated clay. The few thin sections that I have seen are always highly oriented and would constitute a Bt, but might not constitute an argillic horizon. If you didn't have enough lamellae that were thick enough, I would label it in my notes Bt, but I would not consider it an argillic horizon.

We wanted to put the soils of about the same age, landscape age, together, and the limits on the numbers and thicknesses of the laminae in sand was an attempt to relate the argillic horizon in sands to the argillic horizon in other kinds of finer-textured parent material. Now, this may have been a serious error because we have only a few personal observations on this. There will always be corrections. But the laminae in the sands are very important to the storage of moisture in the sand. You don't have to have enough for an argillic horizon to have an appreciable effect on the moisture. **Question 91, Minnesota**

I know of no field clues that can be used generally to recognize argillic horizons in soils with very high clay contents. If the eluvial horizon has 80 percent clay and the illuvial horizon 90 percent clay, the distinction can be recognized by a very experienced pedologist. However, the beginners will not recognize the distinction between horizons that have 10 percent differences of clay when the clay content is so high. The things that one may see in the field

would be the colors of the ped faces in such soils. Not much else can be observed. To make interpretations of the presence or absence of argillic horizon, some laboratory studies would be required. The ratio of fine/coarse clay is one of the most important. The difference in clay content in such soils is commonly due to a difference in sedimentation rather than pedogenesis. However, in a particular survey area, with some laboratory analysis of the coarse to fine clay ratio, one can judge that there should be an argillic horizon or should not. Some benchmark studies in such soils in the laboratory would be essential in order to have confidence of the presence or absence of an argillic horizon. These benchmark studies must be on a survey area basis. No general statement is possible. **Question 16, Leamy**

(For classification purposes is it necessary to distinguish between the argillic horizon and the proposed fine-textured subsurface horizon?) The original proposal to recognize the fine-textured subsurface horizon as a basis for placing a soil in a Paleudalf or a Paleudult was the difficulty of getting agreement amongst different pedologists as to whether or not there was an argillic horizon. The proposal was to put into the definition, then, of Alfisols and Ultisols this distinction in texture with depth, as being the equivalent of an argillic horizon, so that no decision would be needed as to whether or not there was an argillic horizon in a particular soil. This reason is that it should not be recognized as a diagnostic horizon, but as a diagnostic feature, perhaps, but certainly not a diagnostic horizon. So, a soil might have an argillic horizon and have this fine-textured subsurface horizon, and no decision would be necessary then, as to whether or not that horizon was or was not an argillic horizon. This was only proposed for use in the low-activity clay soils and nowhere else in *Soil Taxonomy*. **Question 4, Witty & Guthrie**

There is (a logical reason why the definition of the argillic horizon should not be expanded to include the concept of the fine-textured subsurface horizon). There are many soils with lithologic discontinuities where you have a coarser surface deposit on the finer subsurface layer. Mostly these occur in alluvium, but occasionally you find them in soils developed from rocks of very contrasting mineralogy and particle-size distribution. If you extend this definition generally, then, to all soils, by including this concept in the definition of the argillic horizon, you will then put into argillic horizons all kinds of stratified parent materials, such as the alluvium along the Mississippi, (Fluvents if you please) where you have a layer of sand over backwater clays. And you don't want to do that; you don't want to make an argillic horizon out of a stratification of parent material. This proposal was (not) restricted just to soils that could have an argillic horizon, but where there was a question about whether or not this subsurface horizon was an argillic horizon or was not. **Question 5, Witty & Guthrie**

(When you say draw a smooth curve through the data points, there are two different ways that people draw a smooth curve. One is to literally draw it through the points. The other is to consider that we sample by depths and mix; then we should plot bar graphs for each sample layer. Then if you draw a smooth curve through the bars, it should include and exclude equal areas of each bar. You can get a rather different looking curve for some soils, depending on which sort of "smooth curve" is drawn.) The latter is what I thought was a smooth curve. So your maximum is determined by several of the subhorizons of your argillic horizon, more than the measured maximum of any one of the individual subhorizons. **Question 89, Minnesota**

In general, the control section would stop at two meters so that the argillic horizon would need to start within the two meter depth to be recognized as a diagnostic horizon. When it is this deep, as a general rule, the soils are very sandy and fall into the grossarenic subgroup. **Question 34, Venezuela**

2.12 Gypsic Horizon

With depth, the increase and decrease in the percent gypsum is erratic (in a gypsic horizon). I would say, by and large, (the following) application of the definition probably would be as good as any: If it did decrease in some subhorizon by 5 percent this would qualify it for a gypsic horizon.

I don't know why it is erratic and I haven't seen many data on soils with gypsic horizons. As a general rule in the world where we have them, I don't think they make any problems but they were never recognized in the West before *Soil Taxonomy*, and they were included there because of soils in the Near East rather than the soils in the West. The gypsic horizon has great importance if you are going to irrigate the soil. You have to continuously level again and again because of uneven settlement, or you must sprinkle, one or the other. It was in the Near East countries, where the pedologists were working to design irrigation systems, that the importance of the gypsic horizon was brought out and was introduced to give them a handle to keep the soils separate from others, that once leveled were saline.

I have never seen one (a petrogypsic horizon) myself, so I cannot help. **Question 60, Texas**

2.13 n Value

The *n* value was borrowed from the Dutch soil scientists, who have perhaps the most experience with reclaiming wetlands in the world. I don't know what substitute measurements we might have made. It is something that you can determine from the sample in the laboratory.

Bearing value -- I'm not sure about the engineering tests. It would be virtually nil in the normal Hydraquent. It would vary somewhat with the sand content, but not greatly because there is a limit on the minimum amount of clay they would have. It was the only suggestion we found in the literature that addressed this problem. The engineers have not concerned themselves with it much, so far as I know. Typically, they take their samples and dry them out before they start their test, and that's too late.

As we develop new methods to measure things, we will doubtless change our own definitions. But I don't know a current method (for measuring bearing-capacity in the field as a substitute for *n* value). **Question 51, Texas**

2.14 Paralithic Contact

(About the origin of the concept of the paralithic contact) we first had the lithic contact which was a contact through some sort of bedrock that was of significance to the use of the soil and which reflected a shortening of the soil itself from the bottom. In other words, the soil just hadn't developed into this sort of material. It was a clear base of the horizons that were genetic horizons where we had it. The lithic contact created a problem for a time before the concept was proposed as a property, because we said we would not classify soils on the basis of anything other than their own properties. When you get below the lithic contact, you're out of soil and into the problems of geologists in the rock.

But having devised the concept of the lithic contact, then comes a question of the salt rocks. They are just as effective in stopping roots and engineering. They're a different sort of material because they're more easily moved with power machinery, whereas we wanted to restrict the lithic contact to materials that required blasting for engineering construction work. That was the goal. Whether we achieved it or not, I don't know. The proposals were made (for a paralithic contact), they were criticized a little bit by the laboratory and modified in accordance with their suggestions. I don't recall suggestions for modification from any other source. But (the material below a paralithic contact is) a horizon in the sense that you can see it in the field, you can sample it separately and so on, but it's hard to call it genetic, the result of soil genesis. **Question 94, Minnesota**

("Decomposed granite" which excludes roots is presumed to be paralithic-contact material.) That was the intent (and so-called "compact glacial till") would also fall in there. **Question 95, Minnesota**

(This concerns the problem in the Northeast about the presence or absence of a fragipan. Is it a genetic horizon or relatively unaltered material?) There is no question that some glacial tills are extremely compact, and if unweathered, they amount to a paralithic contact, particularly on drumlins. There is no reason why the glacial till cannot have been compacted other than by the pressure of the ice against the drumlin; though the basal till can be compacting now. Normally, in these soils the compact nature of the till does not greatly affect the movement of water. It does not affect the water nearly as much as it does roots. So in Minnesota, in Illinois, the basal till, which may have 20 percent lime, is not penetrated by roots. Even in a severe dry season, the basal till maintains the same moisture content throughout the year. It does not dry, and this indicates the failure of roots to be able to attract water.

These basal tills however, in the Middle West, the calcareous ones do not have any characteristics of the fragipan. They are in no way brittle. You have no trouble putting an auger into one at the end of the summer when presumably the moisture is low, but the studies of moisture extraction show that the moisture content is virtually uniform the year round. The fragipan in this moment is virtually impossible to define by operational methods, but we would expect the fragipan to perch water, at the end of the growing season. We would expect that, with a shallow observation hole, you would find water perching on top of the fragipan. I do not know of any studies of this sort. They are not difficult for the field men to make, but I do not know of any one who had the curiosity to make the observations and then write them up. This is something that could be done. The basal till normally does not appear to perch water; you never find mottles above them, whereas you normally find mottles in or above the fragipan. I can make no other suggestions than that you take a close look. You have a manual of field procedure, which describes how to put in these observations. The best thing to do, instead of arguing, is to collect some information. **Question 99, Cornell**

Well if (a C horizon) becomes that restrictive (to roots - more restrictive than some diagnostic subsurface horizons such as a fragipan) it probably would constitute a paralithic contact. In general, we try to use properties that were the result of genesis or that control genesis, in our definitions. Now this seems to be something other than that. It has been used according to its depth, as a depth class at the family level, but not at any higher categoric level because it is virtually unrelated to soil genesis. (This could be, like we mentioned, compact glacial till that would be considered a genetic horizon.) That's right. I think that's what they are talking about. This came up at Cornell too, and went through considerable discussion. **Question 92, Minnesota**

2.15 Plaggen Epipedon

(The plaggen epipedon) is primarily a European epipedon. When there were no fertilizers available under pre-historic or Roman culture, the farmers would bring in litter from the forest, or when the heather replaced the forest, they would go out and cut sod from the heather and bring it to the barn as bedding for the livestock. The sod in particular then, contained a lot of sand and the following spring the farmers would put this litter from the barn on small fields close to the house where they grew their food crops and then send their cattle out to graze in the heather. Over time then with all this addition of the sand from the sod plus the manure from the animals, the surfaces of these fields, small fields close to the houses, were raised. It commonly is more than a meter higher than anything around from this application of the mixture of sod and manure. They are very obviously different soils from those around them. We found no reasonable root in Greek or Latin for this, so we had to take the German word for "sod".

(How do you keep them separated from the field that has been mechanically leveled, or where part of the field has been moved to another part of the field, or from a large cattle feedlot where soil has been built-up through the years to say a meter or more?) I haven't looked at the cattle feed lots. These soils have dark colors because of the nature of the sods. They are full of artifacts, chunks of brick, tile, and what have you, throughout the whole plaggen layer. Not just on the surface, but throughout the soil. If you examine them in a pit

you will have no trouble in seeing the spade marks and the fine stratifications that form in a spaded field after a heavy rain at considerable depths in the soil. It is obviously a greatly over-thickened plow layer. Its identification in the field in Europe is commonly based on the change in elevation which follows the line in the old fence line of the infield, the Scotts call it, that is, a small field near the house where the food crops were grown. In identifying it in Europe, it is the simplest thing, as you approach it, you know what you are going to get before you get there. **Question 84, Texas**

2.16 Ruptic

The term 'ruptic' indicates that the horizons within the soil are not continuous over the area of a pedon. The discontinuous nature of the horizons may be due to one of at least three things. (First) you may have a horizon that is just forming, and it forms in spots rather than uniformly over the whole area, this is not uncommon, but perhaps it is more common when one starts with a uniform parent material (and) horizon development proceeds uniformly over a large lateral area. But (second), it may also indicate the destruction of horizons, where the horizons when destroyed, are destroyed in spots, tongues, what have you, rather than uniformly over large areas. This is the normal destructive process.

The third is the soil movement which we get in at least two kinds of soil; one is in the Vertisols, where the soil shrinks and swells. There is considerable movement in Vertisols; the underlying material is often pushed up in the centers of the polygons, polyhedrons perhaps, and emerges at the surface in Vertisols. Exactly the same thing can happen in the presence of a pergelic temperature regime where you get frostboils sometimes in the centers of your polygons, but the horizons are not continuous anymore. If you have a frostboil in the center of your pedon and (if) you have thick organic material on the edge of your polyhedron, the ruptic merely means that the horizons are discontinuous on a very small scale which is repetitive. This disagreement on the International Society tour reminds me that when we made the soil map of North America for FAO and UNESCO, this difference of opinion existed already. And we had a lot of trouble in drawing a boundary that roughly parallels that border. Apparently, there were differences of opinion, and nobody has done a great deal of work on either side of that border. **Question 23, Minnesota**

(As was pointed out) cryoturbation and ruptic are not synonymous. We have different kinds of cyroturbation. In the French classification, they deal with these cold soils according to the shape of the organization of the stonestripe types or in polygons. I don't know what they propose to do with a pergelic soil that doesn't have stones, because you can't get a stone stripe or a polygon in the absence of stones. It can't be used generally. I suspect (in) those that have a continuous histic epipedon (turbic soils that are not ruptic), you will find differences in thickness from one part of the pedon to another, maybe thicker in the center in the polygon or at the edges, you can have either one, but it doesn't become ruptic. We have sort of thought, I have, in the absence of much experience with these soils, that pergelic would indicate the probability of cryoturbation. **Question 24, Minnesota**

2.17 Sombric Horizon

The sombric horizon was identified first by the Belgian pedologists working in the Belgian Congo, now what is Zaire. It was a horizon that they found in a number of kinds of soils. They found it, the sombric horizon, in Ultisols, in Inceptisols, in Oxisols, and they concluded that the horizon would tend to help identify the soils in the cooler mountains in intertropical regions. We actually had very little information about sombric horizons when *Soil Taxonomy* was published. There was one study of an Ultisol with a sombric horizon, which did suggest strongly that this was not a buried A1, but was the result of translocation and accumulation of a dark-colored humus of some sort. In thin sections, in the argillic horizon, the dark colors were restricted to the exterior coatings on the peds. If it had been a buried A1 horizon, the dark

colors should have gone through the peds rather than forming on the ped surfaces. So, this seemed to be evidence, admittedly very weak evidence, because only one profile was examined, but it was a proof.

The Belgians were anxious that it be recognized. It was an additional horizon of unknown genesis, its importance was that it was restricted to the relatively cool and humid intertropical regions. For small-scale maps, it would then be useful to recognize it at a fairly high categoric level, because the great group-suborder associations are about all one can show on a map at 1:1,000,000. Yet one, at that scale, might be interested somewhat in the agricultural potentials, and the genetic importance was, and I think still is, virtually unknown. There are differences of opinion yet that are quite pronounced about the sombric horizon. **Question 49, Cornell**

We do not know much (about the differences and similarities between a sombric horizon and the spodic horizon of a Humod). We know very little. The translocated organic matter in the spodic horizon, we think, is precipitated primarily by aluminum, and to some extent by iron; I think aluminum may be essential, because we always find it. We just do not know much about the organic carbon, the organic matter that is in the sombric horizon. The spodic horizon organic matter reacts with fluoride to produce a highly alkaline solution.

I do not know of anyone who has tried the fluoride (test) on a sombric horizon. We do not have them in the U.S. We cannot study them and so we just simply must say this is something we do not know. We have studied the organic matter that has moved in the soils with natric horizons, and this is not associated with aluminum. **Question 50, Cornell**

2.18 Amorphous Material

The impression (that ash influence was more strongly recognized in the more humid soils than in the more arid soils) is correct. The presence of allophane, the glass in the humid areas, is something that generally we can identify, and it creates some problems of management. In the arid regions we made the assumption that as the glass weathered, it went to smectite rather than to allophane. This may not be true, but this was an assumption that we made, and on the basis of the limited data that we had, I think we probably were justified in making that assumption. With the high bases that you get in arid regions from ash, the clays do not seem to be amorphous in general. But we do, in these regions, get very strongly developed duripans, and while we do not specify the ash in the taxonomy there, we do specify the duripans. So that the ash-derived soils in an arid region, given a little time for development, get into a duric subgroup or a duric great group. It is not specified, it is the horizon that results. **Question 178, Cornell**

2.19 Calcic Horizon

The percentage of carbonates to make us consider a horizon calcareous is not specified in *Soil Taxonomy*, but rather we do specify that there must be effervescence when hydrochloric acid, cold hydrochloric acid is added to the soil. The data that one gets sometimes from the laboratory seem to contain a systematic error in that a few tenths to 5% carbonates are reported in the soils that have a pH well below 7 in KCl and that do not effervesce in hydrochloric acid. These are noncalcareous. The laboratory data sometimes must be questioned by the fieldmen. The fieldmen are inclined to accept laboratory numbers without question, but they may not do this.

We thought that there was a distinction between the calcic horizon of the Calciaquolls from the calcic horizons, say, in your normal Borolls. The calcic horizon in the Calciaquolls, we thought, was due to capillary rise and evaporation from the surface. Whereas, in the Borolls, we thought the calcic horizon was due to downward-moving water and withdrawal of that water precipitating the carbonates. It's quite possible that you can have something that's halfway

between. In theory that could happen, you could get precipitation from capillary rise of ground water and you could also have downward movement at another season of the year of the carbonates stopping at about the same point. You could, theoretically, have a calcic horizon formed as a result of both processes instead of one or the other. But your problems would involve first a proposal of a subgroup if you think it is necessary that you should have that.

(There) could have been (a subgroup left out between the Aeric Calciaquolls and Calciborolls), I'm not familiar enough with the precise situation to say what you should or should not do, other than that if you feel it's needed, you should propose a subgroup.

It is very common to find a distinct pattern to the calcic horizon - at the surface in North Dakota and perhaps in northern Minnesota; in southern Minnesota, Iowa, and Illinois they often have the shape of a donut, for example. Or depending on what I interpret to be the water depth, there may be a slight rise in an Aquoll, and you find the Calciaquoll on the rise instead of in the low part of the landscape. You can get it both ways. I've seen also rings in the landscape in the Dakotas where the calcic horizon has the shape of a donut around the margins of the depression. Those rings are relatively higher than the bottoms of the depressions. How wet they are, I don't have any personal knowledge because I have only seen them in the summers. **Question 201, Minnesota**

(Is there some rationale for the use of the calcic horizon that is formed by an upward vector water movement versus that of a downward water vector movement?) In some of the Aquolls, the calcic horizon is at the surface. This is clearly upward movement and evaporation. These were at one time called Calcium Carbonate Solonchaks. Where the calcic horizon is at depth, say 50 cm or more, the determination of how it got there is quite subjective and depends on your experience and training and was not considered.

I mentioned earlier that it was a serious mistake to have used calcium carbonate as a distinction between the udic and the ustic moisture regime because it does not work where the parent materials are noncalcareous. We need something that can be applied more universally. The emphasis on it, of course, goes back to Marbut's distinction between Pedalfers and Pedocals on the basis of presence or absence of free carbonates in the sola. **Question 29, Texas**

My memory is not too clear on (the background for the change of the percentage of calcium carbonate that's required for coarse-textured soils as compared to finer-textured soils), but I think I can understand it. If you have a sandy gravel, you can have a very distinct accumulation of calcium carbonate before you reach the 15% by weight, but in the case of these particle sizes we require at least 5% by volume of the secondary carbonates. This would be consistent with the 5% limit of secondary carbonates in the calcic horizon by weight, which is roughly the same limit. Perhaps there could have been (a sliding scale used, but no one came up with that proposal).

(It seems to be quite a drastic jump especially when we deal with soils right around the 18% clay break to be required to have 15% calcium carbonate for the calcic. If you have a soil with 20% clay you need 15% $CaCO_3$, if you have a soil with 17% clay you only need to have 5% $CaCO_3$.) You must also look at the uses made of the calcic horizon. In North Dakota the glacial tills normally have more than 15% carbonate when they are laid down, and so it is very easy to meet the requirements for a calcic horizon if you have 5% more in the ca horizon than you do in the underlying till. Many of the tills there, are marginal in this respect, and we pay no attention in some series definitions as to whether or not there is a calcic horizon. It is not even considered at the series level where we have a calcareous parent material. The presence or absence of the ca horizon is considered important, but not the absolute amount of the calcium carbonate in the till. **Question 58, Texas**

I am afraid I cannot answer that particular question (of what kinds of situations would secondary soft powdery lime be expected in skeletal horizons with less than 15 percent calcium carbonate equivalent). The requirements for 15 percent calcium carbonate was waived for the sandier soils because we commonly have very distinct accumulations in these soils with considerably more carbonate than the underlying horizon. Being more or less siliceous by nature of the sandy parent material, it never reached the 15 percent limit. We were really more

concerned with the 5 percent limit than with the 15 percent limit. We enumerated there the particle-size classes which were involved in this waiver of the 15 percent limit. Whether or not we listed the proper classes, I could not say. I just have no good experience with this. You have in your desert project probably seen many such soils. I do not know under what condition one would get soft powdery lime. I suspect you would be more apt to get pendants under the stones, in arid climates. **Question 124, Texas**

I think that we took care of that (the problem of gypsic horizons associated with calcic horizons) in *Taxonomy*. It can happen and you then decide which one takes priority. **Question 201, Minnesota**

2.20 Laboratory Methods or Analyses

(Do you see any problems with waiting for lab data to determine if in fact there is 1.2 times more clay to separate the Alfisol from the Inceptisol?) We took that 1.2 ratio because we thought that was representing a large enough difference that the fieldman should be able to identify it consistently. That's where we got the ratio. When there is very little clay we took the 3% increase because we felt that could be identified in the field, and the intent was that would be a large enough difference that you wouldn't have to wait for the laboratory data. Admittedly the laboratory might come back with a 1.16 ratio. Round that, and you get 1.2. But these ratios seem to be taken as sacred. You must always remember that there are two sources of error and you must consider the magnitude of that error in making a decision. The one is in the laboratory and the laboratory people know pretty well what this amounts to because they can and have run duplicate samples a number of times. They know the variability that they get. What they don't realize is that there's also a sampling error. And you may not pick the best sample for them to study. They assume you did. **Question 183, Minnesota**

2.21 Natric Horizon

The study of these sodium-containing soils is not finished. Just as we are not really finished with *Soil Taxonomy* until we stop learning about soils. There is still a great deal to learn about the influence of sodium in the genesis and in the properties of the soil. At the time we switched to the SAR, the revised *Salinity Manual* had been edited and was about ready to go for printing. The Director of the Salinity Lab retired and a new one came in, and it's never been printed. He wasn't satisfied with the SAR or with something that was in there, and stopped the publication. **Question 36, Texas**

Admittedly, in some arid soils or semiarid soils, the reclamation process of removing the sodium involved deep plowing to bring gypsum to the surface. That is the cheapest way to eliminate the sodium. The shallow natric horizon in these soils is obliterated by this reclamation process, but it seemed that when the soil was so seriously disturbed by reclamation that we could justify changing the classification of the disturbed from the undisturbed soils. **Question 65, Cornell**

2.22 Particle-Size Classes

(What was the basis for using different size limits of the fine-earth fraction in the family particle-size classes from those of the conventional textural requirements? You might wish to mention the common misuse of the term "family texture"). Texture refers to the particle-size distribution of the textural triangle published in the 1951 *Soil Survey Manual*. Because we felt we needed somewhat different classes of particle-size distribution, for interpretations, we have had to invent a substitute term for texture, and so we simply use, I think, a correct technical

term, "particle-size distribution", dropping out the word "distribution" for simplification. The various soil surveys of the world have used various groupings of particle-size distribution. The Dutch have one, the Belgians have another, the French have one, and they are not the same as that of the USDA.

The principle difficulty with the textural triangle was for engineering interpretations. The range in clay content of a silt loam was from 0 to 27 percent clay. For engineering interpretations, this grouped quite unlike soil textures. The limit of 18 percent clay between coarse- and fine-silty and coarse- and fine-loamy was made to relate our soils to the engineering classifications of soils. Somewhere in the neighborhood of 18 percent clay there is a change from nonplastic to plastic, and this is considered by the engineers to be a very important distinction.

We took all of the soils for which we had data on the Atterburgh limits, and mechanical analyses, and we ran a correlation between the clay content and the limit between plastic and nonplastic. It seemed that the limit was somewhere in the neighborhood of 18 percent clay. It is not exact, for some soils with as much as 20 percent clay would come out as nonplastic, and some with as little as 16 percent clay would come out as plastic, but the 18 percent limit seemed to be somewhere in the right neighborhood. We compared the mechanical analyses with the descriptions of the field men, and we observed consistently that if they had 20 percent or more clay, if the soil deformed in a plastic manner, they described it as a silty clay loam, although by the laboratory methods it was a silt loam. We were trying to preserve the series without serious disruption, and when we noticed the discrepancy between the texture described in the field and that measured in the laboratory, it was obvious that most of our field men were describing texture by the plasticity, not by the estimate of the clay content, so that putting the limit somewhere around 18 percent merely brought the series concept into line with the laboratory measurements. Soils that had a silt loam texture, but exhibited plasticity, were normally described as silty clay loams or clay loams, although the laboratory could not find the clay, the Atterburgh limits did indicate the plasticity of the soil.

The other textural triangles in the world, generally, had a limit somewhere in the neighborhood of 18 percent. Some were 20, but they were mostly close to that, and for the engineering interpretations, then we needed to introduce a limit between the plastic and nonplastic soils and, therefore, we had to modify our textural triangle.

The textural triangle of the *Soil Survey Manual*, I should say, for some inexplicable reason to me, considered that a boulder was not part of the soil. This seemed unreasonable from the point of view of the plant, which has to deal with these boulders in its rooting system. So we had to begin to recognize the distinction between a soil that was 75 percent coarse fragments versus one that had none, and this again required a modification of the concept of soil texture because the plants are concerned with these coarse fragments which do not retain water.

We had no way to deal with the soils that were entirely or almost entirely coarse fragments. The skeletal class included those with fine earth, but we had in the perhumid climate of Hawaii, for example, a-a lava, in which there was no fine earth fraction. But, because it rained nearly every day, we had beautiful forests growing on this fragmental material, and so modifications of the textural triangle were essential to deal with the diversity that we actually found in nature. **Question 62, Cornell**

Where the clays were primarily kaolin and oxides, it seemed to the correlation staff that there was nothing to be gained by making distinctions between very fine and fine particle size. Where the clays were 2:1 lattice structure, it seemed rather important to make a distinction between a soil that had 75 percent clay versus one that had 40 percent clay. With 2:1 clays, the permeability is considerably influenced by the percentage of clay. Where the clays are mostly oxides, there seemed to be no such relation, and the correlation staff, in the Southern States in particular, felt that they did not want to distinguish between 70 percent clay and 40 percent clay, that it added nothing to the interpretative value of the groupings at the family level to make this distinction. Now, there are differences in viewpoints. Those who have worked in the intertropical regions have suggested to me, since publication of *Soil Taxonomy*, that such a

distinction might be useful in Oxisols. This is a problem for the International Committee for Classification of Oxisols to review. **Question 63, Cornell**

Curiously, many of the finer-textured soils with x-ray amorphous clays have the engineering properties that the liquid limit is reached before the plastic limit is reached, and they come out as nonplastic in the Atterberg system. Traditionally, all soils have been air dried and screened before laboratory analyses are made, and when, because of irreversible changes on air drying, most laboratory analyses of soils with x-ray amorphous clays have relatively little validity. The moisture retention, the particle-size distribution, the cation exchange capacity, the plasticity are changed irreversibly on drying such soils. **Question 64, Cornell**

The term percentage of clay, clay that is not clearly defined, in general, is measured on the whole-soil basis. However, if the carbonates are secondary origins they may be of clay size. These carbonates we specify are to be treated as silt rather than clay. If the secondary carbonates are of silt size, of course they are treated as silt. So I would assume that in general the meaning was the percentage of silicate clay in the whole-soil matrix. The clay-size secondary carbonates are treated as silts because they do not seem to have the physical properties of the silicate clay. They do not retain moisture in the same manner and one can seriously misjudge the amount of silicate clay that may include the carbonate clay with it. **Question 121, Texas**

We had no method that seemed valid for the measurement of the particle-size distribution in soils with x-ray amorphous clays. There has been a method proposed to disperse these; I think it is with lithium. We have no data by such a method. We cannot use the moisture at 15-bar tension as an estimate of the clay with amorphous clays, because the 15-bar water content may exceed 200 percent on soils with these clays, and you cannot have more than 100 percent clay. So with these, we had no valid laboratory methods. We had these soils segregated into the suborder of Andepts and the order of Spodosols. **Question 64, Cornell**

2.23 Plinthite

While we are on the subject of plinthite, I should like to explain a little of the background for its recognition in *Soil Taxonomy* and to explain a little of the discussion that is going on about its importance in soils relative to other features.

We have relatively little plinthite in the United States, and *Soil Taxonomy* is strongly biased by the soils of the U.S. because the appropriation for the Department of Agriculture has more or less precluded our doing any work in intertropical regions with USDA money. But within the U.S. where we have soils with relatively small amounts of plinthite in the subsoil, the horizon containing the plinthite acts like a pan in that it stops water. The water perches on top of that horizon, the roots do not enter it. It behaves just as a fragipan in the soil.

The soils are not as well-drained as those without the plinthite, and the trees growing on the soil tend to be quite shallow-rooted so that a strong wind will overturn the tree. Whereas in the soils without the plinthite, a hurricane that overturns the trees on the soils with plinthite will break the trees on soils without the plinthite, but does not blow them over. Because of this behavior of the soils that had small amounts of plinthite in the subsoil, we made the genetic assumption that larger amounts would be more important and the committees now, the international committee under Dr. Moormann, which is considering the classification of soils with low-activity clays, has had to consider the relative importance of the plinthic great groups in the Alfisols and Ultisols. They have had considerable debate on this subject without reaching any real unanimity of opinion, but in the last circular letter, which is addressed in their report, they have retained the plinthic great groups. The plinthic subgroups in the intertropical regions that I have seen do not seem to have the same behavior as plinthic subgroups in the U.S., in that I do not find any evidence that the roots of plants are unable to enter the horizons with either small or large amounts of plinthite. This is a feature that so far, in my experience is restricted to the United States. **Question 45, Venezuela**

The first definition of plinthite included the domains in the soil that would harden on repeated wetting and drying and exposure, and the hardened relicts of that material. Subsequently, the term was restricted to the material that had not yet hardened irreversibly. At present, the plinthite name has been used as a formative element in two additional kinds of material: one, the nodular, hard ironstone, which has been called petroplinthite. This usually is a transported material, and occurs in the soil as stone lines. The other proposal for using plinthite as a formative element is for litho-plinthite, which is a material which has hardened irreversibly, in place, with a tubular structure which permits it to transmit water, and permits roots to penetrate through it.

Plinthite has been used as a formative element for several great groups, in which the plinthite forms an interconnected matrix, or forms more than half of the matrix of some subsurface horizon. It is also used as a formative element in a number of subgroups, in which it is present in smaller amounts than in the plinthic great groups. The desirability of retaining the plinthic great groups has been receiving considerable discussion in the international committee on Ultisols and Alfisols that have clays of low activity. At this time, it is impossible to predict what recommendations the committee will make on the use of plinthite in the classification.

The plinthic great groups were established because we had little information about them in the United States, and the importance of laterite had been stressed so much in the literature. The plinthic subgroups were recognized in the United States because they are brittle when moist, and are slowly permeable to water, and nearly impermeable to roots. They behave much as does a fragipan. Plinthic great groups in intertropical regions apparently do not have this particular property, and there is no question in my mind but that some changes in *Soil Taxonomy* will be required to reflect these differences. **Question 3, Eswaran**

The soils that have plinthite at a shallow depth were included with Oxisols in an attempt to keep them all in one part of the *Taxonomy*, irrespective of what underlay the superficial plinthite. These soils were thought to be of extremely small extent. They have been described to me from Africa, but I have never seen them myself. They lie, for the most part, on a colluvial slope below an escarpment that is protected from retreat by petroplinthite or some other form of hardened ironstone. They contain large amounts of ironstone, but they receive seepwaters from the soil above, and are thus kept wet. If cleared, the plinthite hardens at the surface and the soil is destroyed for the growth of plants for an almost unlimited time. Our feeling was, then, this characteristic overshadowed all others, and they should be kept together in the *Taxonomy* in one order or another, and since they commonly are associated with Oxisols, we put them in the order of Oxisols. **Question 10, Eswaran**

I don't think (the definition of plinthite requires it to be considered as) an event predicted in the future, because there is no assurance that these will ever be exposed and harden irreversibly in the next million years or so. In general, I think it is quite possible for the pedologist who sees these dark red mottles to decide whether or not they will harden irreversibly. There are 2 ways of doing it; one is to throw some of it on the ground surface and come back a year later and see what happens. Ray Daniels and some others have pointed out that not all dark red mottles will harden, and I've done away with more plinthite in Venezuela, by far, than I've created, because you can have a twenty-year-old embankment with red mottles that does not harden.

On the other hand, you've got another exposure that's a year or two old, and there they are -- hardened. And if you examine the nature of these red mottles away from the exposure where they have not hardened, there are certain properties that they have if they are going to harden. They are brittle in character. There is enough iron relative to the surface of the silt and sand that, if exposed, they will harden. If they are going to harden, they will be brittle in the fresh pit. As a general rule, then, one can check the presence of plinthite by locating a site that has been exposed, preferably one that faces the sun at some time of the year. **Question 38, Texas**

Yes, (there was a particular reason that plinthite was used for distinguishing this limiting layer rather than the red reticulately mottled zone in which it occurs). We didn't care about the

presence or absence of plinthite. That didn't matter a bit. It was a marker of a horizon that did restrict water and root development and has the behavior of a fragipan. It may be that we should have included these in our definition of a fragipan, but this is being examined very carefully by the committee on classification of Alfisols and Ultisols with low-activity clays. There has been much discussion about this. **Question 39. Texas**

The plinthite is considered to form a continuous phase when the domains in the soil, which harden on exposure of wetting and drying, are interconnected, or that occupy more than half of the volume of the soil horizon. We do not know how many cycles of wetting and drying are essential to the identification of the red mottles as plinthite. We do know that in a number of instances, the wetting and drying has hardened the plinthite into ironstone within a year's time. We do not know how many wetting and drying cycles occurred during this year, but this has been observed in Trinidad and as far north as the state of Oregon in the United States. A pit, dug one year and refilled, but leaving some of the plinthite at the surface, on reexamination a year later showed that the plinthite had hardened.

In general, the plinthite may be identified in areas where roads have been constructed because the grading for the road will leave some road banks in which the plinthite is exposed at the surface, and an examination of an old road cut that shows no petroplinthite or any hardening of the red mottles, would indicate that plinthite was absent. There have been, since these questions were asked, some papers on field identification of plinthite in the *Soil Science Society of America Journal*, but I do not have these references in my head. **Question 42, Venezuela**

There's no necessary difference between plinthite and laterite. The later term is one that has been used by geologists for well over a hundred years and has acquired over that time many meanings according to the particular author of the paper that you are reviewing. The situation was so confused that we decided to abandon the term laterite and substitute plinthite by using Greek roots instead of Latin roots. Plinthite, then, is identical to some of the geologists' laterite. **Question 43, Venezuela**

Laterites included what we now call plinthite, sesquioxide sheet, an acric horizon, and the literature about laterites is an extremely confusing one to read. As a consequence, we decided not to use the term in the later approximations, and we introduced the term plinthite and sesquioxide sheets in the *Fifth Approximation*, as substitutes. **Question 3, Eswaran**

(Concerning the question as to whether exposure to the sun is necessary for the hardening of plinthite or are there cases where the plinthite has hardened within the soil rather than at the surface). There's a great deal more that I do not know about the hardening of plinthite than I do know. I can cite a few examples where the plinthite has hardened in a road cut that was facing the sun. In Brazil, this happens to be on a north-facing roadcut and the plinthite had hardened there, but the south-facing roadcut it had not. So that is one suggestion, but it's not really very good evidence. The other instances in which the plinthite has hardened have all been rather ambiguous.

The hardening reported by Alexander and Cady in their bulletin on the hardening of laterites included a building in Africa in which the same building material, the same ironlike, plinthic material had been used for the wall of the house and for a sun-dial in the garden. Under the porch of the house, the plinthite had not hardened, but the walls that were exposed had hardened, and the sun-dial had hardened. This, they said then, meant that the plinthite required alternate wetting and drying, but it is also true that the plinthite under the porch, which had not been wet and dried was also shaded from the sun, so that one could ascribe the hardening to either the sun, or the wetting, or drying, or a combination of the two. **Question 44, Venezuela**

Litho-plinthite is a more or less continuous seam of iron-cemented material containing numerous tubes which are filled with clay material similar to those that underlie the litho-plinthic horizon. The water in the roots can penetrate through the tubes of the litho-plinthite. The petroplinthite is normally a gravelly material that has been transported. It consists of gravels that are cemented with iron and rounded by transport. It may occur at any depth in the

soil; the litho-plinthite may have, in places, been buried to various depths. In the literature throughout the intertropical regions, there are reports of stonelines that consist largely of rounded petroplinthite. Now the petroplinthite is then a gravel. The litho-plinthite is more like a rock. **Question 45, Venezuela**

2.24 Salic Horizon

The salic horizon is defined more or less on salt content rather than on genesis. The one great group of soils that we provided for which the salic horizon was diagnostic was a group of soils in which there is relatively shallow salty groundwater, and the salts accumulate at the surface of the soil from capillary rise and evaporation.

The Salorthids are supposed to have groundwater at some season of the year before the salic horizon becomes diagnostic. The photograph of the Salorthids in *Soil Taxonomy*, plate 5D, page 101, is of a soil that had groundwater at one time, but stream entrenchment has lowered the water table so that it no longer is shallow enough to strictly meet the requirements in *Soil Taxonomy*. Nevertheless, it seems best to consider them as Salorthids because the genesis was the same, that of capillary rise and evaporation.

There are other kinds of salic horizons in the most arid regions of the world. Peru would be an example, where the salt content is adequate for a salic horizon, but it is not at the surface. It is a subsurface horizon, and has been formed by the leaching from the occasional rain that they get on the Peruvian Coastal Plains. The salts there may accumulate to the extent that the salic horizon becomes indurated and you get what could be considered a petrosalic horizon. These have not been considered diagnostic of anything, in the past. The International Committee on Aridisols that has just begun its work may have another feeling. It was the feeling of our correlation staff, since these didn't exist in the United States, that they wouldn't worry about them. When *Taxonomy* use is extended to other countries, however, this will become a problem that will need debate by the International Committee on Aridisols. **Question 140, Texas**

I don't believe this issue (that any and all horizons in a profile should be considered as salic so long as they meet the requirement of having a product of 60 or more, cm times the percent salt) was ever settled. There was discussion about what to do with some of the salt flats in Utah, where the salt crust that has formed is thicker than the soil. How these were to be classified was discussed but no agreement was reached. At the time that we were developing *Soil Taxonomy* there were no series for the salt flats. They were mapped as miscellaneous land types, and identified as salt flats. This would be an extreme situation. There are plants growing on these salt flats so they come within our definition of soil. It is another problem I presume that should be brought up before the International Committee on Aridisols. These are not formed by capillary rise from a ground water. They are not formed by the occasional leaching, but rather they are evaporites from former lakes and could be considered a parent material rather than a soil. **Question 141, Texas**

2.25 Tonguing of Albic Materials

(Why was the 15 percent minimum figure used as a criterion for tonguing?) We have to have some sort of minimum figure or one tongue to a meter or so would be considered tonguing, a tongue that is only 5 mm thick. This 15 percent limit, to the best of my recollection, comes from the work planning conference of the North Central States where they probably considered what series they wanted to put into great groups that had tonguing and what soils they did not want to put into that. I am sure I did not propose this myself. I had rather relied on the recommendations of the work-planning conference committees that discussed this particular definition. Tonguing is most common in the northcentral states of Wisconsin and Minnesota. **Question 127b, Texas**

I've seen many tunnels made by crayfish, but it never had occurred to me that they would be interpreted as tonguing of albic materials because of their shape. They do not penetrate between peds, but they disrupt peds. I think this tonguing has some limits about thickness on joining ped faces which don't appear in the animal burrows. There is a possibility, in better drained soils that have an albic horizon, of albic materials falling down the channel left by a tree root. And again, this normally has disrupted the peds, and a little careful dissection will show that this is just material that has fallen down into a void left by the decomposing root rather than an actual disruption of the ped coatings by removal of the clay. It certainly would not be within the intent (that the genetic mechanism was stripping out of materials or degradation of the argillic horizon) of the definition of tonguing of albic materials to include either crayfish burrows or root channeling.

(Could it be possible that the crayfish activity hastened the tonguing during a long time period?) I would suggest you look for peds in the severely crayfish-affected areas. In my experience, you don't have an argillic horizon to begin with--the crayfish have prevented its formation by constant mixing. I've also noticed they will penetrate to depths of 3 or 4 meters where the groundwater fluctuates drastically. They like to stay very close to the water itself. They add so much material at the surface that you just frequently don't find horizons in these soils. You could justify a cambic horizon, perhaps, but not anything else. **Question 42, Texas**

2.26 Anthropic Epipedon

The original intent (of the anthropic epipedon) was to deal with the kitchen middens of the Indians in North and South America and of the early settlers in Western Europe. The nomadic people who settled for periods in one spot year after year would bring in shells and animals and the bones would be thrown on the soil, and in time, it developed a soil that had the appearance of a Mollisol, although the surrounding soils might all be Alfisols. These would be perhaps an acre or something like that, maybe five acres, not much larger.

When Roger Bray was working on his phosphorus tests, he sampled one of these and was astonished to find that this test didn't work on these soils. This puzzled him because he got no blue color whatever for phosphorus. Yet he could see bone fragments in the soil. He finally discovered that he had so much phosphorus that it precipitated all his reagents. This was the basis for thinking that we might separate these from Mollisols by the phosphorus found in it.

The soils are rare enough that very few people have studied them in the U.S., and so we went to Europe for their experience on comparable soils that had formed under the early prehistoric settlements. They proposed the phosphorus limits that we used there, and we accepted their proposal because we had no data on soils in the U.S.

When I went to Venezuela and checked over the soils that they had sampled and analyzed, I found quite a few soils that I thought should be Aqualfs but that had the phosphorus required for an anthropic epipedon in the Orinoco Valley. The headwaters have deposits of rock phosphate. The stream sediments coming down originated in such an area that you could have many times the phosphorus that is permitted in a mollic epipedon. They couldn't blame that on refuse from the kitchen midden, no Indian squaw would tolerate a seafood made at a camp in a swamp. This is just a sedimentation. That is not the intent of the anthropic epipedon. When I examined the phosphorus distribution on those soils that were very high, I found that sediments would come one year from one stream and another year from another stream, and just like the carbon decreases irregularly in the Fluvents, the phosphorus abundance was irregular with depth, it didn't necessarily decrease it or increase, but it was a very irregular thing. I proposed in Venezuela that we modify the definition so that such soils would be excluded from having an anthropic epipedon. The definition of the anthropic epipedon would require the phosphorus to decrease regularly with depth.

We had already excluded from the anthropic epipedon the soils that developed in rock phosphate, for example in Florida, Tennessee, and Kentucky on the basis of phosphorus there was not due to any influence of man. **Question 82, Texas**

I haven't seen (the mollic epipedon that might form in arid regions as a result of long-term irrigation from Indian habitation) but they have been reported in Egypt in regions that have virtually no natural rainfall. They have been irrigated for a long time, and they have accumulated the dark colors, the good structure, the high carbon, narrow carbon/nitrogen ratios, and so on that we expect in the mollic epipedon. They haven't been fertilized, particularly, except for the sediments that are in the irrigation water. And our feeling was they didn't belong with Mollisols. In such an arid environment they could not be used except for the irrigation. They would be much better if kept out of the Mollisols. Having very little knowledge about them, we just included them with soils having an anthropic epipedon. I think it is pointed out in *Soil Taxonomy* that it might be desirable to define some other sort of epipedon than anthropic. Knowing that they exist we didn't want them with Mollisols. They didn't fit the definition of Aridisols. We didn't have a class for irrigated soils like the Russians do, so we put them in soils with an anthropic epipedon. That lets them be classified as Aridisols in anthropic subgroups. **Question 83, Texas**

2.27 Cambic Horizon

The Inceptisol order is the wastebasket for certain. We have the concept from Europe of the B horizon. It was the only sort of B we had in the soils we now call Dystrochrepts. There was no accumulation of anything, it was purely a subsurface horizon that had been altered by weathering and by soil-forming processes, that is, mixing by roots and by animals to destroy the original rock structure. (It was found in) the very extensive soils in western Europe in the higher altitudes, such as the Black Forest, the Ardennes, the Central Massif in France.

At one time, as the concept of diagnostic horizons was forming, we were talking about podzol B's and textural B's, (and the color or structural B's?) which is our concept now of the cambic horizon. We tried in the various approximations to group these with the various other soils that had spodic horizons or argillic horizons, mostly. No one was ever happy with the groupings of series that resulted. They always objected to the inclusion of these soils in what's now the Alfisols and Ultisols.

Originally, the cambic was defined primarily on color. We got into trouble with that because in some of the western European sands we had a distinct color difference in the sand in the position where we would normally look for a B horizon. Yet, when we made a laboratory analysis of these color B's in the sands you couldn't find a thing. Presumably it was some sort of translocated humus from the cultivation that had been practiced on the sands. So we excluded the sands from the cambic horizon on the ground that so little alteration is necessary to produce a color change. Dr. Simonson said it doesn't take much paint to make a barn red. And in this case it doesn't take much to color a sand grain.

Having tried various combinations of the soils with cambic horizons and soils with other kinds of B horizons, the argillic in particular, and having had nothing but objections to these trials, we tried to group the soils with argillic horizons according to their base status and soils with spodic horizons and oxic horizons and then we had some soils left over. This was the original Brown Forest Soil concept, actually, but some with high base status and some with very low base status. These being left over, after we had all ourother orders defined, we threw together into the Inceptisols.

We put too much in the Inceptisols in that we should have recognized a separate order for the Andepts. Those are young soils. I can not find one where the ash is dated as much as 20,000 years ago. Mostly the ash is dated considerably less than 20,000 years. Now when we get an ash that's dated 20,000 or more we're more apt to find there a soil with an argillic

horizon. So they come out as Alfisols and Ultisols and Mollisols and Spodosols and what have you.

I have a lot of trouble with the cambic horizon in some of the wetter soils and in the supplement, I think of 1964, we had a Fluventic Haplaquept. This was criticized primarily by the Dutch on the grounds that if they had, say, a silty parent material they would find the fine stratification in the soil. That kept it as an Entisol but, in the slack water deposits that have a clayey texture, the deposits did not originally show the fine stratification. They were absent and it took very little time after deposition before the soil could be considered to have a cambic horizon by our definition because it had soil structure. So we eliminated that subgroup by requiring that there be enough evidence of alteration in the cambic horizon to reduce the inherited organic matter to a low level.

This then in turn was criticized by people in New Zealand and in Venezuela and other places on the grounds that if they had a well or moderately well drained soil, it would have a cambic horizon but the wet soil that was associated with the Fluventic Dystrochrepts, for example, would come out as an Entisol. Everyone objects to the mixing of orders in the same landscape and in parent materials of the same age. So I did propose that we remove the limitation on organic carbon in the cambic horizon of the Aquepts and substitute for it, the presence of a significant amount of iron-manganese concretions that were hard enough to withstand a normal dispersion process for mechanical analysis. This, we tested in New Zealand and it has been tested now in other places. It seems as though it might work. It hadn't been approved and I don't know whether any tests have been made in the U.S. on this proposal. Probably not because I doubt that anyone but Dr. McClelland saw this proposal. But it would reestablish the Fluventic Haplaquepts if it were adopted. It would then put a Fluventic Haplaquept and a Fluventic Dystrochrept as association in one landscape on deposits of one geologic age. **Question 181, Minnesota**

(The cambic horizon) is primarily eluvial in the sense that it has lost something in the dry regions. It's lost carbonates. In the humid regions it has lost original carbonates in all probability. I suspect it has been subject to the loss of some clay either by weathering and destruction or by eluviation without the formation of an underlying illuvial horizon. As I pointed out in *Taxonomy*, the argillic horizon seems to be absent in soils with perudic moisture regimes. I've never yet found one at least. This suggests that the clay that is lost from the cambic horizon with a perudic moisture regime just goes on down and disappears somewhere underground. Certainly, I have seen evidences of clay movement in marine shales in Maine and in Norway. The clay seems to coat the blocky fragments of the marine shale formed when it was first uplifted and drained. These go down to more than 30 feet. I was lucky enough to find an interstate highway under construction in Maine where I could examine what was there to a depth of 30 feet. There were coatings on those blocks of marine siltstone actually. **Question 182, Minnesota**

The limit of 25 cm to the lower boundary of the cambic horizon was set to avoid changing the classification of a soil by plowing a very thin cambic horizon so that it becomes mixed in the plow layer and ceases to be identifiable. We have tried to help *Taxonomy* to keep cultivated and uncultivated soils together as long as it remains possible. If, of course, under cultivation, new horizons form, then the classification needs to reflect this, but the transfer of experience from a cultivated soil to virgin areas of the same soil is complicated if we changed the classification as a result of a few plowings. **Question 7, Venezuela**

The base of the cambic horizon, of course, is not easy to determine unless you have either dominance of rock structure or a strong accumulation of calcium carbonate. In non-calcareous alluvium the base of the cambic horizon is about as difficult to determine as the base of an argillic horizon. It is not in itself an important horizon in that it hasn't much effect on the plants that grow or the structures that are put in.

The thickness limit (for the cambic horizon) was waived for the very cold soils which we thought would be unlikely to be cultivated. One could have argued that the Camborthids should have had a similar limit to the pergelic soils because they are not likely to be cultivated. But chances are much greater with the Camborthids that somebody is going to irrigate and plow.

The cambic horizon is such a weak sort of a diagnostic horizon that it doesn't seem to make a great change in properties of the soil if a thin cambic horizon is plowed up, if it is very similar to the virgin soil. It is not similar to what happens when a thin natric horizon is plowed where the whole horizon is gone and has a much greater impact on soil behavior. These are soils that if the slopes are suitable, Camborthids certainly can be plowed everywhere there is water. The serious problem is the water. **Question 91, Texas**

The upper limit of the cambic horizon did not seem important. Normally, in a cultivated soil it would be at least at the base of the plow layer if the epipedon is ochric, or it would be at the base of mollic or umbric epipedons and does not become critical to the classification of the soil, so that it did not seem important to specify where the cambic horizon begins. This is a difficult problem in soils that have an ochric epipedon. It is not particularly difficult if the epipedon is umbric or mollic. It is the presence of the cambic horizon that is relevant to the classification, not the thickness. The lower limit is relevant to the classification if plowing is going to obliterate it.

Remember that cambic horizons can not be a part of the mollic or umbric epipedon. It must lie below it, if it is present. It is not like the argillic horizon which can be a part of a mollic epipedon for example. The cambic horizon may not because it's not easily identifiable in a mollic epipedon. We already assume that that has been altered appreciably. In some soils, particularly in the Andes, the mollic epipedon may be as much as two meters thick in which the cambic horizon if present lies below the control section and becomes irrelevant to the classification. **Question 7, Venezuela**

No, there is not (a certain grade of structure and type of structure which must develop to qualify for a cambic horizon). We mentioned that it shows soil structure or the absence of rock structure. In particular, it would be the absence of rock structure. Any kind of granular, blocky or prismatic structure of any grade would qualify as soil structure any way as long as it was discernible. **Question 30, Texas**

If you read the various definitions in *Soil Taxonomy*, you will notice that we have specified the absence of structure in some soils, or the presence, but we have never specified a degree of structural development, weak, moderate and strong. We have not done this because in traveling with other pedologists, we find that there are serious differences in opinion about the degree of structural development. It depends, first of all, on the moisture content at which you examined the soil, and it also depends to a considerable extent on the background and experience of the men describing these soils. The Belgian pedologists who have worked mostly in the Congo, where structure is extremely weak in virtually all the soils, will put a moderate structure on any soil in which they can see any structure whatever. And one has to interpret their descriptions with great care because.....(end of tape). **Question 39, Venezuela**

The cambic horizon is supposed to show at least weak expression of the rearrangement of particles in the soil by fauna or the roots of plants and some other evidence normally of weathering either the stronger chroma or redder hues that extend down to more than 25 cm in depth. Where you have the superficial materials lying on an ash, they're very apt to qualify for the color difference just because of the lithologic discontinuity. **Question 31, Texas**

(Soils with brown sandy B horizons cannot have cambic horizons because they are sandy. This same concept is extended to the wet sandy horizon. Was this something that just tagged along behind that other decision or was it deliberate?) It just tags along. But it does not need much alteration to produce mottles in the soil. I have gone out on the Missouri floodplains and when the water had just run out, here was this year's alluvium and it was already mottled. That was a matter of a few days. Of course, it could have gotten its mottles when it was still under water but when the water withdrew and we went out on it, the mottles were already there. (You've seen them then in fresh floodwash?) Put that into Inceptisols! **Question 185, Minnesota**

The soils with sand and loamy sand particle size, have a number of very important common properties: namely low water-holding capacity, blowing, and poor trafficability when dry, so we finally decided it would be best to keep all of these soils in one place in the

taxonomy. Therefore, we had to modify our concept of the color B horizon to exclude the soils that have these very coarse textures. **Question 15, Cornell**

There are problems that are unresolved yet. The limits on texture of the cambic horizon could be modified to throw out the skeletal soils. The sandy-skeletal soils are excluded but the loamy and clayey-skeletal soils are not excluded. The clayey-skeletal soils in the drier countries are not very common in my experience, but loamy-skeletal is not at all uncommon. Where the pebbles are touching each other, and you simply have some finer earth in the interstices between the pebbles, soil structure is not easily determined. One could perhaps say we had there the absence of rock structure. **Question 92, Texas**

(A soil of moderate and medium subangular blocky structure that contains carbonates only in the upper 50 to 60 centimeters could possibly be produced by a process of recalcification. Should this soil be considered to have a cambic horizon or should it be considered rejuvenated and lacking a cambic horizon?) The recalcification of a soil which has been leached of its carbonates would normally be due to addition of carbonates at the surface either by wind or water action. If the recalcification is the result of flooding, the calcium carbonate that is present normally would be accompanied by fresh alluvial sediments. If the carbonates are brought in by wind, there is no necessary addition of other mineral sediments than the calcium carbonate. In the first case where the recalcification might be due to flooding, I would be inclined to consider that the leached horizons were part of a buried soil and classify the soil accordingly. If the carbonates had been brought in through aeolian action, it is generally common to find the secondary carbonates on the surfaces of the peds and absent in the interiors of the peds. In this case, I would be inclined to consider the soil to have a cambic horizon and that it has been rejuvenated by the addition of carbonates from the atmosphere.

Actually, this is not uncommon in the arid areas of the United States where the soil may have at one time had an argillic horizon even, instead of a cambic horizon, but the recalcification processes is generally rather clear because the secondary carbonates coat the peds and do not penetrate the interiors. **Question 25, Venezuela**

In my experience, so far, the accumulation of secondary carbonates from capillary rise has been restricted to soils in which there are carbonates at depth. The accumulation of carbonates as a result of seepage of calcareous carbonate-bearing waters is a possible explanation, though in this situation it would be, I think, rather obvious to the pedologist how the accumulation took place because he would see the landscape position of the soil with the calcareous surface. If it is a result of evaporation of carbonate-bearing waters through seepage, I would not anticipate that there would be any fresh alluvium on the soil and I would be inclined to consider the soil to have a cambic horizon just as though the accumulation had come from the aeolian sources. **Question 26, Venezuela**

2.28 Duripan

I think that Professor Shaw's experience (with the duripan which he used the term "iron pan") was largely restricted to the soils of California and his classification was intended for them, not for a more general system of soils of the U.S. or any larger area than California. In California the duripans do contain appreciable amounts of iron, if one judges by the color, as well as opal. In some of them, at least, there are pretty well preserved clay skins with oriented clays that have been impregnated with silica. In the arid regions the accumulation of silica generally goes along with the accumulation of lime rather than of iron.

Shaw, at his family level, distinguishes soils according to the kinds of root-inhibiting layers: clay pans and iron pans. The latter, I think, are included in the present duripan. Shaw's lime-iron pans may refer to the duripans, say, of Nevada. I do not know any lime-iron pans. What Shaw would have done with some of the duripans, such as those in the Durorthids and the Durargids, I do not know. But his principle of separating soils according to the kind of pan is consistent with what we have done in *Soil Taxonomy*. We, in our committee on Planisols, in

attempting to reorganize and improve the 1938 classification, recognized the different kinds of pans also as different kinds of Planisols, one of which was the Noncalcic Brown Soils which had the hardpan. It was distinctly different from the soils with fragipans of the Mid-west and the eastern states or the soils with clay pans from the midwestern states.

We first called the duripan a silica pan or hardpan. But it's not necessarily the only kind of hardpan. We finally changed it to duripan using the Australian terminology for the same kind of horizon. In examining the arid soils, with very prominent hardpans particularly in Nevada, we found some of the hardpans are partly cemented with carbonates and grade to the petrocalcic horizon, and some have relatively small amounts of carbonates compared with the silica. We broadened our definition, or concept, of the duripan and in the discussion in *Taxonomy*, we point out that duripans have different appearances in different environments.

The duripan under the Alfisols tends to consist of very large polyhedrons with silica coatings on the sides and, in some, across the tops of the polyhedrons and in others not. I guess in the U.S. we have no duripans in Ustalfs. They do occur in other countries but I think in the U.S. probably not. They are not known to occur in the U.S. according to *Soil Taxonomy*. They do occur in the West Indies; they do occur in New Zealand. In the West Indies the Durustalf pan looks like the Durixeralf pan of California. In New Zealand it is more clayey, consisting of huge polyhedrons. It apparently can have either appearance in ustic regimes. The concept then varied with our knowledge of the moment and if anyone is studying it now, I'm really unaware of it. **Question 172, Minnesota**

2.29 Lithic Contact

Those (in which cracks in the bedrock are at intervals less than 10 cm) would be handled as skeletal families. We've looked at those together. It's possible to dig though, because the fractures are both vertical and horizontal. We have no way to deal with it at the moment except as a skeletal family of some sort, provided you get roots in the cracks. **Question 28, Texas**

2.30 Oxic Horizon

(Please discuss the following numbers used in the definition of the oxic horizon: 30 cm thickness; ECEC of less than 10; CEC of less than 16; More than 15% clay - why not 18%; 5% rock structure).

2.30.1 30 Cm Thickness

The minimum thickness of an oxic horizon was set with the notion that the oxic horizon was resting on some sort of saprolitic material. We have prohibited in *Soil Taxonomy*, a cambic horizon that overlies an argillic horizon because, it is really a transition between the epipedon and the argillic horizon. We had the same thought that a material that is transitional between the epipedon and the argillic horizon would not be called an oxic horizon; even though it has the properties of an oxic horizon, it is a transitional horizon, and so we put the 30 cm limit of thickness on the oxic horizon with the notion that it would not be a transitional horizon between an epipedon and an argillic horizon. We also thought that if the thin oxic horizon rested on saprolite, which either retains weatherable minerals or has rock structure, some minimum thickness was required. Otherwise, people would begin to find an oxic horizon that was one cm thick or a half cm thick, and the 30 cm comes from the notion that the oxic horizon should be thick enough to have some significant effect on plant roots. **Question 7, Eswaran**

2.30.2 ECEC of less than 10

The ECEC, which is the sum of bases extractable by ammonium acetate and the sum of aluminum extractable by KCl, was used in the definition of the oxic horizon because we felt it was easier to determine with precision than the CEC by ammonium acetate buffered at pH 7. Different laboratories frequently get what appear to be significantly different CEC's of the same horizon by ammonium acetate. The sum of bases plus the KCl-extractable aluminum can be measured, we think, with more precision than the ammonium acetate CEC.

The limit of 10 was selected because in the limited data that we had for soils of the United States, this was about the maximum that we could find in the soil that we thought belonged with the Oxisols. With more data from other parts of the world, it may be desirable to modify this number. While it was proposed for criticism, no criticism was ever received, nor were there ever any suggestions for changing the numbers. Therefore, what was proposed for criticism became a number that appeared in *Soil Taxonomy*. **Question 7, Eswaran**

2.30.3 CEC of less than 16

The CEC by ammonium acetate of less than 16 was proposed again for criticism and was never criticized. The reason for the 16 was precisely the same reason as the 10 for the ECEC. **Question 7, Eswaran**

2.30.4 More than 15% Clay (Why Not 18%)

The limit of 15% clay as a minimum for an oxic horizon, was proposed because we were concerned with a limit between Oxisols and Quartzipsamments, which may also be completely weathered. The intergrades then, or the limit was to separate an Oxic Quartzipsamment from a Psammentic Oxisol. We chose 15% clay on the assumption that material so completely weathered would have virtually no silt. In Venezuela, we have soils that have less than 15% clay, but have too much silt and clay to become Quartzipsamments. They, therefore, come out from the key as Entisols, although they are completely weathered, and may be very stable in the landscape. I do not like the idea of having an Entisol that represents really an intergrade between an Oxisol and a Quartzipsamment. This seems to me to be unreasonable, so I have proposed that, that limit be dropped completely, and that the limit between the Oxisol and the Quartzipsamment be set at the limit between loamy sand and sandy loam particle-size classes.

A limit of 18% would be a change in the wrong direction, because it would increase the area of soils of Entisols that lie between the Oxisols and the Quartzipsamments. **Question 7, Eswaran**

2.30.5 5% Rock Structure

The limit of 5% by volume of rock structure in the oxic horizon was set to exclude from the oxic horizon, materials that were completely weathered chemically, but were not yet physically weathered. We want to restrict the Oxisol to the oxic horizon to a material that was completely weathered, or nearly so, both physically and chemically.

This limit was proposed for criticism, and never received any. Therefore, it has come on over into *Soil Taxonomy*. One can find a weathered basic igneous rock that has been completely altered, mineralogically. The primary minerals have all been altered, and yet it may be so hard that one must use a hammer to break it. We did not think that this material should

constitute a part of the oxic horizon. It is not in any sense a part of the soil; it is, rather, the bedrock. **Question 7, Eswaran**

We should, note in answer (as to whether the oxic horizon is horizon or material), that *Soil Taxonomy* has used the concept of the nature of the material the soil at the order level, in some soils, as in Vertisols, and in some suborders, as in Aquepts. However, the general philosophy of *Soil Taxonomy* has used horizons to arrange and define the orders and has used other features, such as moisture regimes, nature of the material, and so on, at the suborder level. One could, as Segalen has proposed, use the nature of the material which forms the horizons, rather than the nature of the horizons themselves. This, however, has not been done. We have used the spodic horizon to identify the Spodosols. We have used the mollic epipedon as one identifying horizon of Mollisols. It is of no material consequence whether one considers the oxic horizon as a horizon or as material, because the horizon is defined in terms of the materials that compose it.

It would be very difficult for me to assert that most Oxisols are developed on preweathered and transported materials. It is true that the material must be physically weathered before it can be transported, but whether or not the oxic horizon has formed in materials which were weathered physically, or both physically and chemically, is currently only a matter of speculation, and cannot in any way be used as a part of a definition. **Question 4, Eswaran**

The restriction against water-dispersable clay was at one time in the definition of the oxic horizon. However, in Amazonia, the Brazilians published analyses of a number of soils which had water-dispersable clay in all horizons but had no weatherable minerals, had no clay increase with depth and these would have had to be classified as Entisols, though they are amongst the oldest soils in the landscape. They cannot have a cambic horizon because there are no weatherable minerals. They cannot have an argillic horizon because there is no clay increase. And they could not have an oxic horizon because there was water-dispersable clay. Rather than put these with the Entisols we took out the restriction on water-dispersable clay in oxic horizons. This was protested by some people working in Brazil but when they were asked what should be done with these soils they would give no answer. **Question 38, Venezuela**

2.31 Petrocalcic Horizon

(What is the rationale for having a minimum thickness for the petrocalcic horizon over bedrock, but none over an unconsolidated deposit?) Well, the petrocalcic horizon has much less significance to use and management when it lies on bedrock, because the bedrock has the same practical effects as the petrocalcic horizon. Instead of just having a thin film of lime before we recognize it as important, we put a minimum thickness on it. The normal petrocalcic horizon that I've seen is much thicker than this minimum thickness on bedrock. That's the normal situation. I suppose one could find one that was 5mm thick, but I don't believe I've ever seen one. It was just the relative importance of a very thin cap over an impermeable material versus a thicker one. **Question 55, Texas**

(In the definition of the petrocalcic horizon it says the laminar capping commonly is present but is not required. Could you comment on the identification of the petrocalcic horizon where the laminar capping is not present?) Well, in the work that Giles has done in the Las Cruces study area, he has pointed out the various stages of formation of the petrocalcic horizon in which first you get pendants, then you get above the horizon almost completely filled with carbonates, and then finally when it becomes impermeable, the water film moving over the surface of the plugged horizon, you develop this laminar horizon which smooths the surface of the petrocalcic horizon into what Ruhe used to call "troweled surface", I believe. Looks like a plaster job that I might do.

The distinction of the presence or absence of the laminar horizon is probably genetically related to whether or not you get an occasional rainfall that is hard enough to bring the soil

above field capacity above the petrocalcic horizon. So that the water bearing the carbonates moves laterally and evaporates and deposits it in the fine pores of the cross-sections of the petrocalcic horizon. This results in the sands, gravel, and so on (being) separated by the carbonates and pushed apart as though the horizon was building up from the base to the top of the present laminar horizon. I can't think of too much trouble that people would have in deciding whether this horizon is cemented or not; certainly it should be free of fine roots at close intervals. There are still one or two questions about it that we cannot explain -- the very curious radio-carbon dates of the carbon in the petrocalcic horizon versus the radio-carbon dates of the calcium carbonates. **Question 56, Texas**

I was under the impression that we had the same general rule for petrocalcic horizons and duripans about the spacing of cracks that we had for a lithic contact. I do not immediately put my eyes on the sentence in *Soil Taxonomy* that says so. If it doesn't have a statement to that effect, that's an error in the writing of the definition of the petrocalcic horizon because, in practice, we have followed that rule of at least 10 cm between prime roots in fragipans, duripans, and petrocalcic horizons and so on. We looked at soils on the High Plains with petrocalcic horizons, and discussion centered on what would be the average spacing of the cracks. We were certainly considering distance between cracks for the petrocalcic horizon at that time. **Question 27, Texas**

I don't think I know the answer to (why the definition of the petrocalcic horizon requires a thickness times percent carbonates if the laminar horizon rests on bedrock but similar criteria is not applied to those petrocalcic horizons underlain by loamy material). These definitions all went through a number of statements that were modified from time to time, as we learned more about how the soils were grouped by the definitions we had written. If it is unimportant, then we should consider changing the definition. **Question 65, Texas**

2.32 Weatherable Minerals

We exclude carbonates from weatherable minerals because we need to take into account what had happened in some previous climate. It is not at all uncommon that we have a soil that had undergone repeated humid and dry cycles in arid regions, going back to Pliocene or early Pleistocene time. We have soils in which we have well developed argillic horizons, that were noncalcareous at one time (but are now calcareous). If we examined the soils carefully, the carbonates are on the ped surfaces and not in the ped interiors. These are soils that have been recalcified, presumably from blowing of calcareous dust or from calcium that is brought in by the rain...the carbonates could be a very recent addition. Therefore, we excluded the carbonates from the weatherable minerals of the arid soils in particular. We do not find them in humid regions. **Question 68, Cornell**

(Concerning the 10 percent weatherable mineral break for the siliceous mineralogy) not having the recent information when we were proposing this limit, we attempted to set a limit that would make the distinction, say between the soils of the lower coastal plains and the next higher one. It would not make a complete clean separation because the sandier deposits are going to have fewer feldspars and micas than the loamy ones. We do have in North Carolina, Quartzipsamments of a very recent age, as a matter of, perhaps, less than a hundred years, because the sands are nearly pure quartz when you get out into the ocean beds. In examining the limited amount of available data that we had at that time, the glacial Pleistocene sands had generally appreciably more than 10 percent weatherable minerals, but the Pleistocene surfaces on sandstones might yield Quartzipsamments in the glaciated country. So the parent material has some effect there, as well as the degree of Quaternary and Holocene weathering.

I don't believe (that there is some kind of crop nutrient-supplying capacity for crops at about the 10 percent weatherable mineral level) because there is so much difference in the release of nutrients according to the nature of the weatherable minerals present. The calcium feldspars weather very rapidly, and it doesn't take many thousands of years for them to disappear from the soil in the humid climate. But muscovite is very resistant to weathering and

being resistant to weathering we would expect that the nutrient release would be very slow. The committee on classifications of soils with low-activity clays has been discussing this limit, and may come up with some recommendations. Should Cecil and Appling series be included with the low activity clay soils or excluded? If we use weatherable minerals they are excluded. If we use, strictly, the nature of the activity of the clay then they are included. **Question 129, Texas**

Chapter 3

SOIL CLIMATIC REGIMES

reviewed by A. Van Wambeke[5]

3.1 Rationale for the Use of Soil Climate

3.1.1 Zonality

The original intent was to introduce moisture and temperature as a partial substitute for the old concept of zonality. **Question 162, Cornell**

Before work on the development of *Soil Taxonomy* was started, it was recognized that the concepts of zonality and intrazonality were not tenable in a natural classification because they were not based on soil properties; that is, not based on the properties of the soils that were being classified. It was necessary to classify the soil as zonal or intrazonal on the basis of properties of other soils than those being classified. Having recognized that soils could not be classified as zonal or intrazonal on the basis of their *own properties*, one had to find substitutes for the highest category. The use of soil moisture and soil temperature was a natural substitute for the concept of zonal and intrazonal soils. In general the soils of a given region with the same rainfall have roughly similar soil moisture and soil temperature regimes so that, with the exception of the soils with aquic moisture regimes, one had a sort of substitute for zonality that was based on the properties of the soils being classified. The soil temperature and soil moisture regimes were useful for classifying soils from the top down in a descending order **Question 11, Leamy**

In historical perspective, we must remember that we were starting to build from the 1938 classification in which we had in the highest category zonal, intrazonal, and azonal soils. These were untenable as they were expressed in that classification, and we had to find substitutes of some sort. In a given area on the Great Plains, in the Appalachian Mountains, the Coastal Plain, everywhere in the U.S. except perhaps the Rocky Mountains and the Sierra Nevada Mountains, the temperature and the rainfall and their distribution were important factors in controlling the vegetation, as well as the possibility of leaching, the probability of permafrost, and so on. The temperature, the moisture, changing gradually over large distances, led to the grasslands of the Great Plains, the forests of the more humid regions, and they were the closest substitutes for the concept of zonality that we had. A good deal of the utility of the concepts of the zonal soils could be maintained if soil moisture and temperature were introduced at a high categoric level. Consequently, before I agreed to undertake the job of developing a new system, I reached agreement with Dr. Kellogg that soil moisture and temperature would be introduced as soil properties at a high categoric level. This was decided before any work was undertaken with the purpose of maintaining as much continuity with previous classifications as possible. **Question 40, Cornell**

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5. Professor of Soils, Department of Agronomy, Cornell University, Ithaca, New York 14853.

There was very strong opposition in the United States, and everywhere else in the world to using soil moisture and temperature at any categoric level, and there are still complaints that we used them in different levels. **Question 43, Cornell**

The Russian system did not consider soil moisture or temperature, they considered climate. Now, the two are related, but imperfectly. The temperature of the soil on a south-facing slope in the northern hemisphere or the southern hemisphere differs from that on the slope in the opposite direction. In many instances in the literature we have examples where the south-facing slope has Inceptisols, the north-facing the Spodosols, because, I think, of the difference in moisture and temperature. It is a combination: the colder the soil, with the given rainfall, the more humid it is. I should mention that those who prefer to use climate to classify the soils may readily get in trouble, because the climate is not as uniform as very small-scale maps of climate would suggest. We have rain shadows of mountains which are not reflected in the climatic maps. If the mapping pedologist is not required to investigate the soil moisture, the soil temperature, he is apt to forget about it completely, so that when he finishes his map, it is impossible to make any interpretation whatever. This has happened many times, and while the FAO/UNESCO legend of their soil map of the world uses soil moisture in only one place, the substitution of climatic maps is inadequate, because the climatic maps are not detailed enough to permit interpretations of specific areas, even fairly large ones. **Question 41, Cornell**

3.1.2 Classification Principles

3.1.2.1 Comprehensiveness

Not all soil climatic regimes have been defined. The *gap left between the definition* of aridic, ustic and xeric soil moisture regimes was deliberate. We have no information about these soils that enable us to develop that part of the taxonomy and had we attempted to close that gap so that there would be a place for every soil, we feared that the pedologist might attempt to classify the soil by simply applying the definitions in *Soil Taxonomy. It must be remembered that classification involves not only the application of the rules to see where the soil fits in Soil Taxonomy but equally importantly, it requires that the classifier study that classification to see whether that is appropriate.* Many of the limits in *Soil Taxonomy* were selected to group the soils of the U.S. into classes that had some real meaning. The purpose of classification is to put together the objects that belong together. How does the classifier decide what things do or do not belong together? The classification problem is not too difficult; he has the rule that the things that belong together have common properties and common behavior characteristics. A soil that has accumulated an appreciable conductivity under irrigation, may be capable of supporting at least one or even two crops a year under rain-fed agriculture, and yet the rules of *Taxonomy* say that it is an Aridisol. This is obviously absurd if one considers whether such a soil that accumulated its salts under irrigation and can lose them readily, if they are leached to reclaim the soil from its saltiness. We would then have a soil that changes back and forth from an Aridisol to an Inceptisol according to the year that the leaching is carried out. The absurdity of this sort of classification should be apparent to anyone who is more concerned with putting the things that belong together into a taxon, than following the rules that are set by the limits of *Soil Taxonomy.* **Question 3, Leamy**

3.1.2.2 Categorical Level

The climatic criteria are not always used at the same categorical level. It appeals to a great many people to use one property in one category throughout the system. However this leads to an enormous multiplication in the number of categories that we must form. You cannot, for example, distinguish the Histosols on the basis of the clay mineralogy. Unless they have clay minerals you may not use mineralogy in soils that are organic in nature. This would be one example. It requires then a whole series of categories for the Histosols. We make soils maps at

different scales for many purposes. Some maps are made at very small scales, some are made at large scales. For the small-scale maps, it is desirable to use some parameters with very broad definitions as of the soil moisture regime--udic, ustic, xeric, aridic. For the large-scale map, this is inadequate because we must make subdivisions of these broad classes of moisture regimes in order to make reasonable interpretations at the family level. So we cannot make all of our classes apply to the very broad map units of small-scale maps, and so we must use broader groupings. For the large-scale map, where we are concerned with a specific field on a specific farm, to make the most precise interpretations possible, we have to recognize small differences in the moisture regime. Therefore, it is necessary to use the same characteristics at more than one level in the taxonomy, or we must abandon the notion of making maps at different scales. **Question 9, Cornell**

Consider the Entisols as an example. Entisols have no diagnostic horizons other than an anthropic epipedon. One could have used moisture and temperature to define suborders of Entisols. Certainly this is possible, but the question is one of developing classes about which one can make the greatest number of statements about the things included in a given class. Amongst the Entisols there are several reasons why the soils do not have diagnostic horizons. One is that they are continually receiving new sediments. Another is that erosion is removing materials more rapidly than allows horizons to develop. The third one is that man has disturbed the soil to great depths and mixed horizons that have previously existed. If one considers then, these reasons why Entisols have no horizons, it seems that one might be able to make more statements in common about the soils which are receiving the alluvium than about the soils which are alternately moist and dry. Having decided to divide the Entisols according to the reasons why they lack horizons, although these are not specified in the definition, the next most important features of the soils seems to be moisture and temperature. At the first category possible then, moisture and temperature were recognized as differentiae but in Entisols the suborder took up the causes for the lack of horizons and therefore the introduction of moisture and temperature could only be made at the great group level. Had we insisted on using one criterion at the same categoric level under all combinations of other properties we would have had an almost infinite number of categories and we would have been unable to make many statements about most of the units that resulted. **Question 41, Leamy**

3.1.3 Choice of Criteria

3.1.3.1 Soil Climate a Soil Property?

It would appear from the question that if one inserted a thermometer into a soil one would not get a reading; the soil has no temperature according to the question. This is a rather general problem with people who have not had an experience with soils over a wide geographic range. The temperature at one moment or at one day is not necessarily the same as the temperature at another moment or another day. Yet there is a temperature. When the late Dr. Kellogg went to Canada some years ago to examine the reasons why alfalfa (lucern) was suffering from deficiencies of sulfur, he carried with him a thermometer. The soils have a layer of gypsum at about 50 cm depth and yet the alfalfa was suffering severely from sulfur deficiencies in the presence of gypsum. He demonstrated to his Canadian host that the horizon that contained gypsum had a temperature that was too low to permit the alfalfa roots to enter, and he demonstrated also that there were no roots in that horizon. Is this then not a soil property? In my judgement it is the low soil temperature that prevented the alfalfa roots from entering the horizon with gypsum and obtaining the necessary sulfur. What causes the low temperature of the soil may be the climate perhaps, and probably is, but still it is also a soil property. The soil temperature can be increased in the summer by removing an insulating layer such as an O horizon so that with a given climate the soil temperature is not necessarily the same in soils that are undergoing the different uses. This does not mean, however, that there is no temperature. The soils of northern Canada have very different temperatures from the soils from the West Indies. The soil temperature is not only important to the growth of plants. If it becomes low enough to impede the growth of the roots, then it is also an important cause of soil

differences. The temperature is exceedingly important in the rate of chemical processes and, therefore, in the rate of weathering of the primary minerals of the soil parent material. *It is a basic assumption in Soil Taxonomy that the properties that are the result of genesis or that are factors in the genesis and therefore causes of other properties, are the factors that should be used in the definitions.* John Stewart Mill pointed out that properties that are causes of other properties are preferable in developing a classification. **Question 40, Leamy**

3.1.3.2 Selection of Critical Limits

Soil moisture and soil temperature are amongst the most important soil properties in controlling the uses of the soil. We wanted to devise a grouping of series that would permit us to make the largest number of most important statements about the soil behavior. Moisture and temperature could not be disregarded if we were to do that. We were greatly influenced in our definitions of udic and ustic moisture regimes and of xeric moisture regimes by the dryland stations of the Great Plains, some of which were located in Texas and from Texas to North Dakota. That was the only body of data we could find on soil moisture. They did measure the soil moisture. And we could recalibrate their measurements which were in percentages to moisture tensions by resampling and determining the moisture tension characteristics of these dryland stations. We have records running up to thirty years. Our definitions of soil moisture were based, in part, on these dryland station records of soil moisture. The actual classification of the soils was predetermined. We decided in advance that we wanted certain areas to be udic. We wanted certain areas to be ustic. In the ustic groups we wanted intergrades to the Aridisols and to soils that had udic moisture regimes. If you go across Nebraska or Kansas, you will find that in the extreme eastern parts of the states you have a system of farming that is based now on corn and soybeans. As you approach the central part of the Great Plains, you have a system of farming that's based largely on wheat and sorghum. As you approach the Aridisols, you have a system of farming that's based on alternate fallow and cultivation because they get more total production by fallowing one year and cultivating the next than they do by cropping every year. We've decided where they must fallow to get maximum production, we would want to put those into an aridic subgroup of an ustic great group. Where they get the maximum production by cultivation every year, we wanted to put those into the typic subgroups of the ustic great groups. We plotted on maps where these boundaries should come. Having located the boundaries, we then developed the model for calculating the presence or absence of available moisture and we adjusted our definitions to the boundaries that had been predetermined in the field. Now, this is not the situation you asked about, but this is how we got at the definitions. When you are working in mountainous regions and you do not have this very gradual change in climate as you have on the Great Plains, then the location of the boundaries is going to be largely a matter of inference. You should know which plants are characteristic of which moisture regimes. And in making your detailed maps in the field, you will be guided by the nature of the plants. We have said that the properties we use should be measurable in the field or they should at least be able to be inferred from combined knowledge of soil science and one or more other scientific disciplines. In this situation, for getting at the moisture, your plant science is the best you can get to use. You know a great deal about range in these western states and which plants belong where. A man coming from New York State would be lost for a time until he had gone into the problems of distribution of the range plants and of certain forest plants. Temperature, you can measure very readily, I think. That's been studied in a number of countries and they always come out with the same conclusion, that if you know the elevation and the latitude, you can estimate the mean annual temperature very precisely. **Question 7, Texas**

When the criteria proposed in the earlier approximations were examined by seeing how the series were grouped, I received repeated complaints that this is not good because this splits our series; the goal was to retain the series as nearly as possible with their previous use. However, the series were not defined on the basis of temperature or moisture. These were inferred characteristics and related to the series, but not appearing in the series definitions. Where the type of farming changed, we made different interpretations. For example, the interpretations for soils cropped to cotton were not the same interpretations that we made for soils cropped to maize or to spring wheat. Therefore, the series normally changed with the type of farming.

How it happens, is that the limit between the Cotton Belt and the Corn Belt, between the Cotton Belt and the Winter Wheat Belt, between the Red Desert Soils and the Gray Desert Soils was the same, always at 15°C mean annual temperature. Therefore, this was a natural limit that did not split series. The Red Desert-Gray Desert separation was based on the natural vegetation, creosote bush, being present in the Red Desert and absent in the Gray Desert. If one studies the general soil map of the United States that was published in the *1938 Year Book of Agriculture - Soils and Men*, it is immediately obvious that the boundary between Red-Yellow Podzolic Soils and Gray-Brown Podzolic Soils follows the 15°C soil temperature isotherms. This was not based on the type of farming because we currently have it in the Alfisols, the recent soils on loess along the Mississippi Valley, although these were previously called Red-Yellow Podzolics soils and now they are thermic Hapludalfs and so on. While the correlation is imperfect, the differences in type of natural vegetation were rather apparent, but with an imperfect correlation between the distinction between thermic and mesic soil temperature regimes.

It is impossible to use the natural vegetation as a basis for classifying soils because many soils have as their natural vegetation, commercially cultivated crops. Examples might be the soils of the irrigated valleys of the Nile, Tigres, or the Euphrates where the sediments have accumulated and the original soil is buried deeply below the present control section. The only vegetation that has grown on these soils has been commercial crops. Rice, cotton, for example, in Southeastern Asia; in the U.S. we have similar situations on floodplains where the sediments have accumulated under cultivation and the original soil is now deeply buried, perhaps to depths of 2 or 3 or 4 meters and the only vegetation these soils have had may be corn or cotton. These are their natural vegetation.

There are similar changes in type of farming and in vegetation that cross the country and the 8°C isotherm and at the 22°C isotherm. The limit between the Corn Belt and the small grains or the corn grown for silage comes at 8°C. The limit between winter wheat and spring wheat comes at 8°C. The limit in the northeastern states, in New England, where we change from "sol brun acides" or Dystrochrepts to Spodosols, comes at 8°C. So the series changed again at 8°C across the country until one reached the Aridisols. However, there are few series of Aridisols in the frigid zone; so that the splitting of series there was not of serious consequence.

The limit of 22°C in the eastern part of the United States separates the citrus belt and the winter vegetable belt from the other soils and again we had other series. So the use of the particular limits of 22°, 15°, and 8°C, produced the least possible disturbance of the soil series. It coincided with the general but not universal changes in the natural vegetation, where the natural vegetation could be determined.

In the tropics where we have isotemperature regimes, the natural vegetation frequently is not possible to determine. The ecologists are still arguing about the origin of the savannahs in the tropics. The isotemperature limits were selected for convenience to have the same limits as the others, mainly 22°, 15°, 8°C, for convenience of the user of *Taxonomy*. We felt he could remember one set of limits much more easily than he could two. The limit of 8°C for isofrigid from isomesic was wrong and suggestions have been made to change it. The limit of cultivation in the intertropical regions has a mean annual temperature of the soil of about 10°C rather than 8.

It seems important, in a soil survey that is made to facilitate interpretations as well as mapping, that there be some relation between potentials for cultivated crops and the soil properties. We attempted in drawing the limits between the Aridisols and other soils to draw the limit between what could be cultivated without irrigation and what could not. In the case of the isofrigid temperatures we would again want to draw the limit between what can be cultivated and what cannot because of nightly frosts. **Question 11, Leamy**

I would like to make one more comment on this that we pointed out in *Soil Taxonomy*, that we had predetermined the classification of the soils on the Great Plains. We then fit the definition to this predetermined boundary, using climatological data to do it. If we subsequently found that our definitions were in error, then we were much more apt to change the definition than the classification, which was predetermined. We said we want these soils to

be in aridic subgroups of ustic great groups, or in udic subgroups of ustic great groups, or typic subgroups of ustic great groups. This was based on a lot more experience with land use than it was on the climatological data. The moisture control section was a device that permitted us to infer from the definitions. **Question 113, Texas**

3.1.3.3 Selection of Categorical Level

One could start it out with moisture and temperature at the order level, but we thought that their effects were integrated into the formation of horizons of varying sorts, and that we could integrate them much better by using the horizons and other diagnostic properties, at the order level, and then bringing in temperature and moisture at the suborder level, where that was possible, or at the great group level where something else seemed more important than moisture and temperature. **Question 42, Cornell**

The soil climate is brought in to the *Taxonomy* at about the first possible category below the order. In some of the orders, it is brought in at the order level as in Aridisols. In most of the orders it is brought in at the suborder level, but when we came to the Entisols it seemed that it was more important to distinguish the reasons for lack of horizons than it was to bring in the temperature and moisture at the suborder level and then subdivide them according to the reasons at the great group level. That could have been done. But we weighted the importance of whether you had a soil on a hillside that was eroding or a soil on a flood plain that was agrading for interpretive values. It seemed that it was much more important to distinguish the Fluvents and the Orthents, and the Psamments at the suborder levels than to have the suborder of Ustents and Udents and then put in a "Fluvoustent" and an "Orthustent" and so on. You could get the same combinations either way. It seemed that if you weighted the importance of the reasons for lack of horizons versus the soil-forming factors of the soil climate in a soil that had no development, it was better to bring soil climate in at the first category below the suborder which was the great group. Many people are bothered by the use of a given soil property in different categories in different orders. What we are trying to do is to develop a grouping of soils about which we can make the greatest number and most important statements. If we do that, I don't see that any logic is violated, because our logic is simply that, to be able to make statements that are important, that is our purpose. We can achieve our purpose by using a given property in one category in one set of circumstances as a given order, and in another category in another order. That just makes the most statements, that is really the logical thing to do. **Question 93, Texas**

A question was asked in Washington: "Why do we have Torrox instead of Oxids?" Which is more important, the oxic horizon or the aridic soil moisture regime. We may have made the wrong decision, but we decided that if a soil with an oxic horizon (and an aridic soil moisture regime) was irrigated, the oxic properties still remain limiting to use. Similarly with Torrerts, it was more important to recognize the shrink-swell potential than the soil moisture regime which, though a limitation, could be corrected. So in these two examples, we decided to bring the moisture regime at a lower level. In the Entisols, we thought it was important to recognize at the suborder level the reason why the soil had no horizons. It was either losing material too rapidly through truncation or receiving additions too rapidly for horizons to form. Having used that particular set of characteristics to define the suborder, we brought the moisture regime in at a lower level. If we try to bring in these properties all into a single category, we have too many categories and we do not have the opportunity to reflect the major differences in the high categories for small-scale maps and the smaller differences in these properties for the large-scale maps. **Question 112, Cornell**

The exclusion of the Oxisols that have an aridic moisture regime was primarily because they will, under irrigation, behave like other Oxisols. We would have all of the difficulties that you would expect from management of other Oxisols from that group. We might as well keep them together as Oxisols. In that situation we could deal with the arid climate at the suborder level instead of the great group level because they seem to be the most important subdivision of the Oxisols according to their soil moisture regime. The exclusion of the Vertisols that have an aridic moisture regime or at least have an arid climate, I think is parallel to the exclusion of the

Oxisols. Under use they are going to behave like other Vertisols. In Sudan in the Gezira Scheme the irrigated soils are Vertisols and they crack, and the cracks close and so on every year and have slickensides, parallelepipeds, and what have you. Just at the boundary of that Gezira Scheme I am told that the soils are not Vertisols. Because they never get moist enough to swell, they are dry enough to be cracked and the cracks that are there are filled with granules, but because there is so little movement in the absence of irrigation, you cannot find slickensides. This will illustrate the reason why the Vertisols probably should be kept together as a group instead of being split according to their moisture regime. **Question 64, Texas**

When we used a term in one categoric level, if we use the same concept in another category we substituted another term. Therefore, we have the Torriorthents and not the Aridiorthents. **Question 63, Texas**

3.1.4 Alternate Choices

3.1.4.1 Soil Phases

The problem of when to establish a new series or to use a phase of an existing series has been with us for many decades. The Office of Soil Correlation in Washington has really not been very helpful in establishing guidelines. It was impossible to deal at any length with the series category in *Soil Taxonomy* because there were too many thousands of them, and ones that only include a few examples of families with the descriptions and data on the series in that family, and to analyze then the differences that had been considerably greater in Des Moines than it is here because the growing season is longer. Now it is conceivable that one could use this, say, at the series level, because the soil is colder here than at Des Moines, or it can be used as a phase. The minute you build it into your taxonomy as a series, the plant breeders are going to come along and change all this, and you will find your taxonomy is tied to an agriculture that no longer exists. For this sort of thing I would prefer a phase. I can give an example in Canada where you made an interpretive map for wheat production in the prairie provinces and before you could get it printed, the plant breeders came along and pushed the wheat line many many miles to the north. The map was made doubtful because it had been made as an interpretation rather than based on soil properties. So for this sort of thing, I much prefer phases to putting it in small, say one or two degree, increments of temperature as series limits. **Question 27, Minnesota**

3.1.4.2 Vegetation

In the absence of data there is not much you can do except use the vegetation, but when it is potential vegetation rather than what is there, it is a matter of judgement and what one man says is the potential vegetation another man will argue about. It isn't anything that can be demonstrated. It is the same sort of thing that caused us to try to keep genesis out of our definitions. By and large in areas where there is a lot of natural vegetation, as in Venezuela, the relation between vegetation and moisture is excellent. **Question 165, Cornell**

But when it is potential vegetation rather than what is there, it is a matter of judgement and what one man says is the potential vegetation, another man will argue about. It isn't anything that can be demonstrated.

3.2 Soil Moisture Regimes

3.2.1 Measurement of Soil Moisture Regimes

3.2.1.1 Actual Measurements or Calculations?

The answer is: the bulk of the classification is made by calculating the soil moisture regime from meteorological data. There have been only a few studies of the actual moisture conditions and these have not run for more than a few years at a time, so that their validity is subject to some question. An effort was made to teach the mappers to recognize a soil when the moisture was held at a tension of 15 bars or more by asking the fieldman to estimate whether or not the soil was dry or was moist. The fieldmen then made their estimates, submitted samples to the laboratory where the moisture was measured. And we did learn that it is quite feasible for the fieldmen with some help from the laboratory to identify a horizon in which the soil is dry. **Question 14, Venezuela**

Dr. Grossman, before I retired, was working with a number of soil scientists, including some in Texas. They were cooperating in that they would sample the soil and estimate whether or not it was above or below wilting point and send a sample to the laboratory which would confirm that it was or wasn't. Some of the pedologists with a year of experience and some calibration became very good at estimating whether or not moisture was held at 15 bars or more. It was our hope that if we could develop this skill among the fieldmen that we would begin to accumulate data on the actual moisture regime. Having been away for eight years now, I don't know how that has progressed. **Question 113, Texas**

3.2.1.2 Estimates by Study of Vegetation

The criticism of moisture regimes made most commonly is that you cannot measure it. I have to admit that it has rarely been measured. But one can, with the knowledge of the ecology of the plants which are growing there and the climate, make a good estimate of the moisture regime. The correlation between the vegetation and climate is generally pretty good.

For example, in wet/dry climates of Venezuela, you do not find a plantation of bananas unless it is irrigated. Around Maracay, they cannot grow commercial bananas without irrigation, but they do grow with irrigation. There are many crops which cannot stand moisture stress. The moisture control section has nothing to do with these limitations; we have to consider the whole soil. **Question 114, Cornell**

In the estimates of moisture regimes, we surely are concerned with the cultivated plants, where that's the expected use. Where the cultivated plants are absent, as they are in many of the federal lands in the western mountains, there's no experience among the local people on the soil moisture conditions. The farmers on the Great Plains have a great deal of experience with the average moisture condition. Do we have to have thirty years of records? I say we'd like as long a record as we can find, but a ten-year record will yield a good deal of information with perhaps somewhat less reliability than a thirty or fifty-year record. The native vegetation conceivably can be affected by accidents such as fires. Consider northern Minnesota where we originally had conifer forests and that has shifted over to Aspen because of failure to control burning. The conifers may be coming back now, I don't know, but what is the native vegetation? It is what you find there, an untended plant. What you have can be due to soil moisture and temperature or it can be due to accidents. So one must be a little careful about using vegetation to draw boundaries. **Question 130, Minnesota**

You can't always have a network of meteorological stations, or study the soil moisture over a ten-year period. We had quite a good discussion about this in Lubbock in which some of the men who were concerned with mapping of federal lands in so-called native vegetation said that a good man could just look at the assembled vegetation and give you an excellent idea of the soil moisture and temperature regime at that point. Their experience is extremely important, and we've said in *Soil Taxonomy* that we shouldn't use properties that can not be measured or at least estimated from the combined knowledge of pedology and one or more other disciplines. For example, we estimate mineralogy for some of these soils from our knowledge of pedology and geology. We get at the ages from our combined knowledge of pedology and geomorphology. We get at the moisture regime from the combined knowledge of pedology and the experience of the range people, the foresters, the botanists. On the plains we have also the common knowledge of the cultivators which is probably better than our knowledge from the meteorological stations. **Question 130, Minnesota**

The danger of mapping vegetation instead of soils is a possibility. In general, we can dispose of temperature easily because it's readily measured compared to the moisture. In soils of the Great Plains the moisture-supplying power of the soil changes rather gradually with distance. For the most part, one has no question that the moisture regime is ustic or when you get to Illinois and Indiana it is udic. There is a vast body of knowledge on the soil moisture in the hands of the cultivator. They know much more about it than the pedologist who is out there who just wants to make a soil survey. They can from their knowledge give him a great deal of help in deciding whether he is dealing with ustic moisture regime or not. They know what crops may safely be grown and how often there will be drought that will dry the soil out so that the crop does not mature. I think when you combine the common knowledge of the cultivator with the inferences that you may draw from the vegetation, you are not going to restrict yourself just to mapping vegetation. **Question 113, Texas**

3.2.1.3 Identification of Moisture Regime in Drained or Irrigated Land

The aquic suborders or great groups are supposed to have an aquic moisture regime or artificial drainage. This is a man-made change in the soil and because the ground water level has been altered by the artificial drainage there is no way that is practical or feasible for the soil surveyor to determine what the groundwater level was before the drainage. We don't want to close the tile drains to find out what it becomes if we stop the drainage. Further in the definition of the moisture regimes and in many of the taxa where we are referring to periods of dryness in the soil, we specify that these periods apply to soils in which there is no artificial management of the soil moisture as by fallowing, water collection, or irrigation. The Typic Ustochrepts have an item which reads "when neither irrigated nor fallowed to store moisture". Then we specify the length of dryness. So these are examples of proof that we did consider, the artificial management of soil moisture. **Question 146, Texas and 103, Minnesota**

You are required to classify an irrigated soil as its non-irrigated counterpart. You have to assume it is not irrigated. Similarly the moisture regime of an oasis soil surrounded by aridic soils cannot be inferred from atmospheric data. **Question 115 and 116, Cornell**

3.2.2 The Moisture Control Section

3.2.2.1 Need of a Control Section

If one is going to use the concept of soil climate, the periodicity of dryness and availability of moisture in the soil must be determined relative to some fixed part of the soil. And the moisture control section was devised to permit the estimation of the soil moisture condition from climatological data. The 25 millimeter limit was so that the period of dryness would not be interrupted by a brief, light shower during the dry season. The 75 millimeter

lower boundary of the moisture control section was set to give some arbitrary limit for reference when calculating the soil climate. The moisture control section itself, its content of available water was calculated from the measured moisture contents of the dryland stations where records have been kept for up to about 30 years. A model was devised for estimating recharge following rains and withdrawal between rains and the periods of time during which the moisture control section was dry in some parts or dry in all parts or moist in all parts was calculated for these dryland stations. This was not perfect because the correlation observed between calculated moisture conditions and measured moisture conditions had a coefficient of correlation of about 0.8 leaving nearly 1/3 of the differences unaccounted for. **Question 13, Venezuela**

The purpose of the moisture control section was to permit the calculation of moisture regimes from the climatic data because we are quite aware that it would rarely be measured. The model, I think we have discussed this, the model that we designed to measure the wetting and drying of the soil was devised with the help of the records from the old dryland stations. Without some sort of a defined moisture control section one would find it very difficult to say that the soil was dry or moist or partly dry or partly moist - where is it dry and where is it moist? The upper limit of the moisture control section was placed below the surface so that a very small shower would not interrupt the dry period in the soil. The soil can be dry throughout the moisture control section, but plants can still survive if their roots go below it When we say the soil is dry, that is a very different statement from saying that the moisture control section is dry. We need to be able to define the part of the soil that we were talking about, being dry or moist. **Question 66, Texas**

The moisture control section can be completely dry even though the crops are surviving and making moderate growth because of available moisture below the moisture control section. We cannot obviously define these various soil moisture regimes without some sort of a control section. The one that we select seems to permit an estimation by the model developed by Newhall. The assumption is always that there is no loss of water by runoff or accumulation by runon. This will modify the moisture conditions in the soil. **Question 114, Cornell**

3.2.2.2 Measurements of the Limits of the Moisture Control Section

In soils that are never dry, you are not really concerned about the moisture control section. It does not matter where it is. If you know that it is udic or perudic, you do not have to have a moisture control section for predictions.

If you are in the field and you do not know that you have a udic, or ustic, and you do not know the depth of the moisture control section, it is difficult to know when the moisture control section is going to be completely dry, or partly dry, or partly moist, or completely moist. You need a kind of diagnostic depth of the moisture control section in these marginal cases to be able to say, am I in a udic or a ustic moisture regime.

In soils that are dry at some time, the moisture control section was thought to be something that you could either estimate or, if you were quite uncertain you could actually measure by simply adding water to the soil at the moment that it is dry. We gave some rough approximations of the limits according to the particle-size distribution, but these are approximate only; they are influenced by structure and by organic matter, and other things than just particle-size. We did not think that there would be very many measurements to determine the upper and lower limits of the moisture control section. We did not think that there would be very many studies to find out whether the soil moisture control section was moist in all parts, or dry in all parts, or dry in some parts. We do think that there should be some studies on this to relate the truth to the calculations that we make with the help of the computer. **Question 154, Cornell**

In a humid region where the soil never dries out I don't know precisely how one would make the measurements. In a dry climate where the soil does become dry, one could readily apply the 2.5 cm of water and wait the 24 hours necessary and then excavate and see the depth

of penetration of the wetting front in that time. One could do the same with the 7.5 cm to see where the wetting front had reached. **Question 66, Texas**

3.2.3 Use of Morphological Properties

3.2.3.1 Calcium Carbonate Accumulations

In the marginal area between the ustic and udic moisture regimes we tried to use presence or absence of soft powdery lime in the profile to put the soil in the Udalfs or Ustalfs. This was all done to avoid the necessity of actually determining the moisture regime. Now, certainly the presence or absence of soft, powdery lime is not a good marker between Udalfs and Ustalfs in non-calcareous parent materials, especially in regions where there is very little calcareous dust in the air. I suspect that several or most of these attempts are going to prove impractical once we've focused attention on them by putting them into *Taxonomy* and we may have to modify them. It's going to make it more difficult to map. **Question 145, Minnesota**

The distinction between the Udolls and the Ustolls included the presence or absence of secondary lime. If it had secondary lime within certain depths, it was considered an Ustoll irrespective of the moisture regime. If there was no secondary lime, it could, I think, be a udic subgroup of Ustolls or a Udoll depending probably on the moisture. This doesn't work, say, in South America and in Venezuela. The sediments in the Orinoco Basin are dominantly non-calcareous and it's only on calcareous sediments that you find any secondary lime in the Orinoco Basin. In Argentina I have not studied the soils myself, but I am told there are some serious problems also between Udolls and Ustolls. They tell me there are petrocalcic Udolls in Argentina which certainly do not occur in the U.S. So we have an international committee at the moment working on these moisture regime definitions - particularly with reference to inter-tropical areas, but at the same time they can not separate them from the moisture regimes in more temperate climates. They must consider both but the committee was set up because of serious problems in intertropical regimes. Any recommendations they make there are going to have an impact in temperate regions, so that committee is going to debate the problems in the moisture regimes and will come up in a few years with some recommendations. What they will be, at this moment, I do not know. **Question 56, Minnesota**

When I was working in Venezuela, I made a proposal on the subdivision of the soils with ustic moisture regimes, with or without regard to the presence or absence of carbonates. Certainly the fact that the moisture regime is marginal to udic is much more important than the presence or absence of secondary carbonates. I proposed that we have subgroups of the ustic great groups in which we would have a central concept that would be used for typic subgroups, an udic subgroup and an aridic subgroup based on the length of the period in terms of consecutive days when the moisture control section was partly dry or wholly dry. Because this was a rather drastic change in the concept and really requires an additional soil moisture regime to distinguish the type of ustic regime that we have in Venezuela from the type of ustic regime we have in the United States. I made it as a proposal to be discussed.

If they are adopted, then the use of carbonates to distinguish udic, ustic and aridic subgroups and ustic great groups will disappear completely. It has certainly little validity even in the United States. We have udic, ustic, and aridic subgroups of Ustalfs all in the same neighborhood and have all the same potentials for production of plants. **Question 54, Venezuela**

It must be remembered that, while *Soil Taxonomy* was intended to group the soils of the United States, with which we had experience, it was also intended that it should be possible to extend the definitions so that they would be applicable to soils of other countries. In the United States the soils with ustic or xeric moisture regimes are almost always from parent materials that have carbonates or there are carbonates in the dust that falls on the soils. The original taxonomy used in the United States, that of Marbut, divided all the soils at the highest

category according to the presence or absence of a horizon of accumulation of calciun carbonate. The emphasis on this horizon has been greatly reduced in the classification of 193: and in *Soil Taxonomy*. However, the prejudice in favor of using this horizon continues to exis because of its long traditional use in classification. The definitions in which the presence at : given depth according to particle-size distribution of a horizon of calcium carbonat accumulation, assumed a relationship between the depth of water penetration into the soil whicl in turn was correlated with the moisture regime. The limits of depth were selected according t the traditional concepts that the depth to the carbonates varied with the rainfall. These wer always in regions in which the rainfall was limited, and genetically the depth to the horizon o accumulation of calcium carbonate was a function of the total rainfall and of the soi temperature.

In Venezuela, I found that the soils with ustic moisture regimes and with dry period ranging from 6 to 9 months had carbonate accumulation at depth provided that the paren materials were calcareous. Noncalcareous parent materials gave rise to soils without carbonate accumulation irrespective of the length of the dry season, that is the length of time on the average during which the moisture control section was partly or entirely dry. Therefore, we had soils from noncalcareous materials that were marginal to Aridisols but had to be placed in udic subgroups by the definitions in *Soil Taxonomy*. This is irrational; correlation between the depth to carbonates and the moisture regime is very imperfect. The relationship depends not only on the amount of rainfall but on the distribution of the rainfall and on the carbonate content of the parent materials. Therefore, in Venezuela, having reviewed the application of the definitions of *Soil Taxonomy* to soils in a wet/dry tropical climate, it was obvious that we could not use carbonates as a basis for defining udic and aridic subgroups of Mollisols or Alfisols. I, therefore, proposed that the definitions be changed and that the depth to secondary carbonates be eliminated completely from the definitions and that the definitions be rewritten on the basis of the length of time during the average year or during some percentage of years that the moisture control section was dry in some part or in all parts. **Question 30, Leamy**

3.2.3.2 Conductivity and Salinity

We looked at conductivity. The conductivity limit, unhappily, came into the distinction between Aridisols and Inceptisols. An irrigated Inceptisol can be converted into an Aridisol by the definition we have. That was a mistake. We could not make the conductivity work with the Mollisols. We could not make the accumulation of monovalent cations at depth work to distinguish Aridisols and Mollisols or to distinguish ustic from udic moisture regimes. Conductivity distinguished Udolls and Aridisols by and large, although there may still be exceptions. If someone can come up with something, perhaps a computer, someday when we get enough data stored, perhaps we can come up with relations that would suggest something that no one has thought of. We tried everything we could think of before we went directly to the moisture regime. **Question 57, Minnesota**

I proposed the solution that we drop that limitation on salinity in the Inceptisols. This will require a slight modification in the definitions of both Inceptisols and Aridisols. As they are now defined, the Aridisols are supposed to pick up any Inceptisols that have become saline by irrigation. If we drop the limitation on the salinity of Inceptisols, then the definition of the Inceptisols and the Aridisols would differ primarily by the moisture regime.

It is quite a common situation in the Near East where the moisture regime is aridic to irrigate and to salinize the soils. If the irrigation is stopped these soils will still produce crops. I ran into a situation in Venezuela where we had an ustic moisture regime and the government had irrigated one farm for a nursery for cocoa. When you sampled the soils on that one farm they became Aridisols because of the salinity and yet all around them the farmers were growing one good crop of maize every year. This was an island of Aridisols created by this definition. If irrigation were stopped the salinity would disappear within a year or two. It is a similar situation in the U.S. where they're irrigating citrus with Colorado River water in California and the soils are mostly Xeralfs or Xerochrepts. Where you have a seepage spot at the base of a hill, the wetter soil on the landscape becomes an Aridisol, if it doesn't have an argillic horizon.

This is irrational; we have the same problems on the lower Rio Grand Valley in Texas. **Question 69, Texas**

3.2.3.3 Organic Carbon as an Index of Moisture Regimes

Its validity is probably not very great. We recognize that in strongly calcareous materials there is preservation of organic carbon. However, we did want to make a distinction between the typic subgroups of Aridisols, which may have virtually no organic carbon, particularly in North Africa in the margins of the Sahara where the rains come once in a hundred years or so, if ever, and the Aridisols such as you have in eastern New Mexico and Southwest Texas, where there is more rain and more production of grass but not enough to produce a mollic epipedon. We thought these were not the typic Aridisols which go for years without rain. In Ustollic Aridisols you have a reasonable summer rain and a flush of ephemeral grasses if the soil is not too badly eroded. At least they developed with a grass vegetation, but that evidence may now be missing because of soil blowing.

At one of our meetings we asked the correlators on the Great Plains to work out a definition. This was done by Arvad Cline and some associates. They were not happy with it when they gave it to me but they said this is the best we can do with our present knowledge. They said it's not good but it's the only thing we can suggest. **Question 24, Texas**

3.2.3.4 Hard-Setting Surface Horizons

This criterion came from the experience of looking at the Noncalcic Brown Soils in California and comparable soils in South Australia, mostly cultivated soils. Nobody really ever showed me a virgin soil, I think, in this environment. In South Australia the soil with a hard, massive epipedon was called a "hard-setting stage" and is comparable to the cultivated Xeralfs in the U.S. They disappear over a distance of only three or four miles. We went into more arid climates and there we found soils with argillic horizons, they had a very soft epipedon. It seemed to work on the basis of the soils that they showed me in Australia and in southern California. Ustalfs can do the same thing; they do in Venezuela, at least. As you go from the Ustalf or the Ustult to the Aridisol, the epipedon is first hard, massive and then soft. Experience generally can be utilized as a field criteria where you are just on the margins between ustic or xeric on one hand and aridic on the other. The intent was that it would avoid the necessity of forming judgements about which side of that boundary you were on. Focusing attention on it then causes people to make more observations. If I'd left it out, it wouldn't have been the subject of any studies whatever. Even though it is aridic. We did the same thing between the Aridisols and the Mollisols. We said that if you had a mollic epipedon, a Mollisol could have an aridic moisture regime. **Question 145, Minnesota**

It was my observation in the United States, in Australia, in Venezuela that as we approach the boundary of the ustic and the aridic moisture regime, that the soils with argillic horizons had a hard and massive epipedon where the regime was ustic and had a granular and soft epipedon where the regime was aridic. In field work, in mapping, the boundary between Aridisols and Alfisols or Ultisols is much more easily determined by the structure and consistence of the epipedon than by the moisture regime. So we tried in a number of places to supplement the distinction between the moisture regimes with readily observable field properties, and it was for this reason that we thought that we could simplify the mapping problem if we restricted the Aridisols to soils that have a structured or soft epipedon.

I said that we use the nature of the epipedon in an attempt to eliminate the need for the mapper to decide about the moisture regime and I did not say that this was entirely successful. The Australians have reported to me verbally somewhat similar situations where their Paleargids do not have a soft-structured epipedon. There's probably considerable need for reexamination for this criterion and there is now an international committee reexamining the classification of Aridisols. I would prefer that you should take this up with that committee and you will get

some support from the Australians in trying to find another solution for the marginal cases, then. In this situation of yours and in the Australian situation, the moisture regime is not marginal to ustic at the moment. It's clearly aridic, and I personally, never having seen these soils, have no suggestion as to what modification in the definitions might be needed, but it seems clear from the verbal reports that I get that some modification is required in the definitions of the Alfisols, Ultisols, and Aridisols. **Question 46, Venezuela**

3.2.4 Definitions of Soil Moisture Regimes

3.2.4.1 Number of Years to Consider

The 6 out of 10 years in the definitions really had no significance except to get "most of the time." For dry, in 6 out of 10 years, that was one way of saying, in most years. If I used percentages then I get an extra decimal, that is not significant. I can't say 60 percent because then 59 percent is less than 60. If I say 6 out of 10, then 59 percent of the time, rounds off to 60.

We wouldn't want to use data for less than 10 years to calculate the moisture regime of a soil. Our practice in SCS has been to use the number of years for which data are available. The weather stations here are mostly 30 or more years. We did, throughout the Great Plains at one time, pay the experiment stations to put these long-time weather stations on tape. The Weather Bureau was recording on tape the current data, but they had no funds to go back and pick up the previous data. SCS paid to have the experiment stations record the long-time stations. We used the longest period we could find. **Question 103, Texas**

3.2.4.2 Use of 5° C and 8° C Limits

The 8° C at 50 cm depth was thought to be high enough that we surely had a growing season that was controlled by moisture and not by temperature. The 5° was used in the aridic moisture regime definition. It does happen that we have soils on the Great Plains that do dry out in the early summer or early fall, and winter comes and they remain dry all winter. They do not moisten up again until the spring rains arrive. We did not want to count that dry period as a part of any possible growing season; we wanted to allow those soils to be dry all winter without adding to the length of time that the soil was dry. We put the 5° limit in, on the grounds that during the winter when the soils were dry the temperature would be below 5°. These were rather early proposals and no one has criticized them as yet. It is quite likely that the definitions can be modified in a way to make them more useful.

There would not be any problem I think in using 8°C in both cases. **Question 155, Cornell**

3.2.4.3 Use of 22° C Temperature Limit in the Definition of Xeric

If you have a hyperthermic temperature, your growing season is controlled by the moisture, not by the temperature. It does not matter whether the rains come in the calendar summer or the calendar winter. You have a wet season and a dry season. The wet season can be in any month or months of the year and the temperature has no control over the growing season. The normal xeric moisture regime that we wanted was one in which we had a winter of some sort with some control of the growing season by both temperature and moisture. So we did not want to allow the xeric moisture regime to exceed the limits of the thermic temperature regime. You go to Venezuela and you have a pronounced rainy and a pronounced dry season. But in one part of the world or another this may come in the calendar winter or the calendar

summer, but winter and summer have no meaning there; it is the wet season and the dry season that are critical. Another reason was that, I did not want to have Oxisols with a xeric moisture regime because the name is patented. I thought I was excluding "Xerox" from any possibility of occurring.

There is a report I think of some higher elevations in Mexico, that we have hyperthermic temperatures that we essentially have the winter rainfall. They are getting some cold season, but the temperature comes out as hyperthermic.

You have in North Africa many places that have all the characteristics of xeric except you have hyperthermic temperatures. They become ustic. In the coastal plains of Lebanon, Syria, Israel it becomes ustic because the summer is too hot. **Question 156, Cornell**

3.2.4.4 The Aquic Moisture Regime

Why not an Aquic Order?

I might go back in my own personal experience when I first started to map soils. I worked in a county in Central Illinois where all of the soils virtually were Mollisols. The big differences that I saw as a beginning mapper were the differences between the well-drained and the poorly drained soils. Later I undertook to study the crop yields that were obtained on the experimental stations, and I classified the soils (all Mollisols) on the basis of their natural drainage. I determined the yield that had been obtained on the naturally poorly drained soils after drainage with the yield on the naturally well drained soils. There was no significant difference. Once the poorly drained soils were drained, they behaved like the naturally well drained soils. If one goes into the Southeast, in the region of Ultisols, one would have the same experience, that after drainage the naturally poorly drained soils will behave like the naturally well drained soils of that area. So the Aquolls have many of the same properties as do the Udolls; after drainage, they have a mollic epipedon, they are rich in bases, and they produce the same kinds of crops with the same yields. The Aquults are low in fertility, they do not have a high base status, and they require about the same management as do the Udults. So it seems that if we established an order of the aquic great groups, that we would have some very strange bedfellows. We would be better off to keep the Aquolls with the other Mollisols and the Aquults with the other Ultisols. This notion certainly met with enormous objections in the early approximations. It was my notion that it would have been better to have had aquic great groups than aquic suborders, but the staff generally was so strongly opposed to having aquic great groups that I had to abandon the notion of bringing the soil drainage at the great group level rather than the suborder. There would have been advantages to doing this. For example, your committee on moisture and temperature regimes is having to deal now with the differences among the aquic suborders according to whether, after drainage and flood protection, they will have the natural udic moisture regime or a natural ustic moisture regime. At present the aquic great groups in the wet/dry climates are very wet in the rainy season and extremely dry in the dry season, whereas the aquic great groups in regions of uniform rainfall distribution are never dry in the sense that they lack available water for plants. This is not reflected in the present taxonomy, but needs to be. **Question 8, Cornell**

Differences in Control Sections for Aquic Subgroups

There are different thickness criteria for recognition of aquic moisture regimes. For the aquic intergrades in Glossudalfs and Hapludalfs, gray mottles must occur in the top 25 cm of the argillic horizon; for the Paleustalfs and Paleudalfs, they must occur in the upper 75 cm of the soil; for Hapludults, they must occur in the top 60 cm of the argillic horizon; and for Paleudults in the upper 75 cm of the soil, or in some cases throughout the top 12.5 cm of the argillic.

I suppose primarily that these differences exist because the definitions were written in different parts of the country. The correlators in the cooler sections of the country are concerned with the low chroma mottles indicating wetness because they shorten the growing

season for the plants and delay the period when the soil can be prepared and seeded. In the southern part of the country, where the temperatures are appreciably warmer, the growing season may be shortened but the difference is not critical to the use of the soil and this may be why the correlators in the north central and the northeastern states took the different view about the thickness of the unmottled zone from the southern correlators. I do not know precisely what was in their minds but they were the ones who proposed these depth limits after considerable discussions among themselves and the state representatives. **Question 26, Texas**

These subgroup definitions were developed in Work-Planning Conferences that I could not always attend. If I did attend one, I could only sit in the discussions of one committee. I simply do not know the answer. If it seems irrational and irrelevant to interpretations, then changes should be proposed. I think that we must not tie our hands by trying to be completely consistent at this moment. Our only consistence is that we want to get the taxa about which we can make the most important statements and the greatest number of them.

I should point out that when you are dealing with Udalfs and/or Udults the shallow water table can be an impediment to use. When you are dealing with Ustalfs and Ustolls the shallow ground water may be a benefit. In northwestern Iowa where we have a relatively thin mantle of loess over a fine-textured till, the ground-water perches above the till. Crop yields are better because of it, because the soils then retain and can supply more water. These are considered Udolls at the moment but they are getting marginal to the Ustalfs, and I don't have much personal experience with the Ustalfs. **Question 137, Texas**

Use of Oxygen Availability and Mottling

The amount of oxygen hasn't been often measured. The main studies on that were done by Ray Daniels in North Carolina and the best meter he could get for measuring the oxygen didn't go low enough to reach the anaerobic levels of oxygen, but they approached it and probably it was anaerobic but he couldn't prove it. There's been studies made in Maryland and in Pennsylvania between the groundwater fluctuations and the depth to low chroma mottles and they generally show a good correspondence. The inferences that the fieldmen make about the depth to the anaerobic conditions are probably valid. The interpretations based on the depth of mottling are surely valid from the studies that have been made of depth to watertable in wells and soil descriptions indicating depth to the low chroma mottles. I should perhaps point out that in the Aquults we do not require low chroma mottles, only 2.5Y or 5Y hues accompanied by mottles. When I got into the intertropical regions, I found this should have been done generally for soils with isothermic or warmer temperatures. One of my proposals was to change the definitions of these aquic suborders to provide for other colors for the isothermic and isohyperthermic soils.

I've proposed that these changes be extended, where we have hyperthermic, isothermic, or isohyperthermic temperatures, so that the Mollisols, the Alfisols, and the Inceptisols would be treated parallel to the Ultisols. **Question 37, Texas**

Paddy Soils

There is a related problem concerning the soils that are artificially flooded for the production of rice. These soils, many of them, originally were freely drained soils but have now under centuries of production of rice under flooding conditions, developed evidences of superficial wetness. This may be more nearly the situation that one has regarding the soils on the floodplains that are flooded occasionally, during the rainy season.

The soils used for paddy rice are not treated in *Soil Taxonomy* for lack of enough description to be able to define such a group of soils. They have been studied rather extensively in Japan and there is a small literature concerning their classification. **Question 19, Venezuela**

Subdivision into Ustic Subgroups

We have a precedent in *Soil Taxonomy* of xeric subgroups of Albolls, Xeric Argialbolls, for example. The Albolls like the Aquolls are inclined to be wet at some season. In the case of the Albolls the potential uses of the xeric subgroup is very different from that of the typic subgroup that has either an aquic moisture regime or a udic moisture regime. I think it is essential that we distinguish these "wet-dry" soils at the subgroup level so that our families do not contain soils of vastly different potential uses. The Aquolls of the Willamette Valley in Oregon, for example, cannot be cultivated for summer crops without irrigation. Yet they come into the same family as the Aquolls of Iowa and Illinois which are potentially extremely highly productive for summer crops. I have proposed, myself, that we should establish ustic subgroups of all of the aquic great groups for soils like your Lufkin which are too wet in one season and too dry in another so they must be both drained and irrigated to be used for the production of crops. This is a very extensive situation in the wet-dry tropics. It includes the Aqualfs, the Aquepts, the Aquolls, the Aquults and so on. They all, I think, require some subgroups to distinguish them from those which are in the humid parts of the tropics or the U.S., and if drained, they really have the udic moisture regime rather than an ustic or aquic moisture regime. I think the International Committee on Moisture and Temperature regimes is going to examine my proposal and we will see how they come out. I proposed that the typic subgroup be restricted to soils that would not become dry for more cumulative days than we permit in an udic moisture regime, and that the others be distinguished as ustic subgroups. **Question 70, Texas**

Artificial Moisture Regimes

To some extent quite a bit of attention was given (in developing *Taxonomy* for artificial moisture regimes where the soil moisture is controlled through drainage and/or irrigation) in that the aquic suborders or great groups are supposed to have an aquic moisture regime or artificial drainage. This is not a man-made change in the soil and we discussed this at some length because the ground water level has been altered by the artificial drainage and there is no way that is practical or feasible for the soil surveyor to determine what the groundwater level was before the drainage. We don't want to close the tile drains to find out what it becomes if we stop the drainage. **Question 146b, Texas**

Concept of Epiaquic

The concept of the epiaquic regime originally was one of soils that had occasionally very heavy rainfalls and become saturated in the upper horizons but not in the lower horizons. Most of the soils are on good slopes and are never flooded, but they are very wet during the height of the rainy season and there is some considerable reduction of iron at this time as evidenced by the 10YR hues that are in the upper horizons but that disappear in the lower horizons where the soils become appreciably redder. The horizons with the 10YR hues also show some rather prominent mottles indicating the movement and segregation of iron in the upper horizons. This concept of the epiaquic regime is currently being reviewed in the United States by the work-planning conference committees, particularly in the southern states. There might be some disadvantage in broadening this concept to include problems with the soils that flood. The flooding can be prevented by engineering measures such as dikes, levees. But the high rainfall that produces the epiaquic regime as it was originally conceived can hardly be controlled by engineering practices. It is true, surface drainage can be improved on many of the soils, but it cannot be prevented by engineering practices. **Question 19, Venezuela**

3.2.4.5 Number of Rainy Seasons

There's been discussion of subdivisions of moisture regimes on the basis of one or two rainy seasons. In Aridisols these are not severe rainy seasons, you understand, but the soils that have two rainy seasons can occur under very low or very high rainfall and in the latter the two rainy seasons are important. Such soils are much to be preferred to soils with only one rainy season because you have a relatively dry season during which you can harvest one crop and

plant the second. In Venezuela, with only one rainy season, they are only able, at the moment to grow one crop per year, although the growing season is long enough for two crops. The maturing of the first crop comes at the height of the rainy season when they can't harvest it They cannot plant the second crop except with hand labor. This is one of the things the committee on moisture and temperature regimes will undoubtedly discuss. Whether they will get out into the Aridisols with this discussion, I don't know. **Question 82, Minnesota**

3.2.4.6 Nomenclature - Aridic versus Torric

We did not want to repeat in different categories the same formative element, because then we found when we got to the subgroup we had intergrades in which we had to repeat that formative element twice. This was unsatisfactory. When we used a term in one categoric level, if we use the same concept in another category, we substituted another term. Therefore, we have the Torriorthents and not the Aridiorthents. The names of the orders were such that we required a single formative element at the suborder level which we took from the name of the order. All the Aridisols in the suborders end in "id" and all the other lower categories. We didn't want an Aridio, or an Aridiorthent because then at the subgroup level we have aric meaning an order and a great group. We can't tell to what taxa that intergrade subgroup belongs. We got into serious trouble with that in our first attempt to revise the nomenclature. You don't see it until you see the names that you've made. Then you realize you can't tell where this intergrades. **Question 63, Texas**

3.2.4.7 Perudic Moisture Regime

I would have liked (for the perudic moisture regime to have received more consideration in *Soil Taxonomy*). The definition never got tested because it wasn't used. But I like to separate things that have about the same horizon sequences for different reasons. I'll give you an example from Maryland in which on the tops of the mountains we have a lot of Dystrochrepts on stable surfaces. It is perudic, never gets dry enough to form an argillic horizon. When we come down on the coastal plain in Maryland we have an udic moisture regime and it is dry enough that on a stable surface we have an argillic horizon. But on the sideslopes, where the land surface is very young, we have Dystrochrepts again. And here we have the same horizon sequence, the same properties other than the lack of a dry season, not particularly dry but enough reduction in the water content in the perudic regime to permit an argillic horizon to form. On the coastal plain the lack of the argillic horizon is a function of the time that the soil has had to form. I would like to distinguish those. They are currently distinguished at the series level because one is in the mountains and the other is in the coastal plains. There is no serious temperature difference that forces a family distinction. **Question 83, Minnesota**

3.3 Soil Temperature Regimes

3.3.1 Measurement of Soil Temperature

3.3.1.1 Data Base

There's an enormous amount of data, not on soil temperature, but on water temperature at varying depths below the surface. You'll come out with the same mean annual temperature and eventually you'll come at a depth to a zone where the temperature is constant the year round,

and this is the mean annual temperature of the soil above. Now the well water records give us an enormous volume of data on the temperature at this depth of constant temperature. That has been related to the mean annual air temperature, so that it is possible with relatively few actual measurements of soil temperature to relate the soil temperature to the air temperature. It's not everywhere the same, this relation. *Soil Taxonomy* says in much of the U.S. the soil is 2° F warmer than the air. That does not hold for the arid parts of the U.S. at all. It does not hold for Alaska where you have the snow insulation during the cold weather and no insulation during the warm weather. There the soil temperature can be very much warmer than the air. Where we lack data it is possible and in the course of a year or so, with only a few temperature measurements, to get at the mean annual temperature as well as the summer and growing season temperature. **Question 6, Texas**

3.3.1.2 Influence of Soil Cover and Irrigation

The soil temperature should be under whatever vegetation the soil is capable of supporting. The meteorologist will keep the soil bare, but this does not concern the soil survey because in nature, the soils do not remain bare. Nobody is going to go out and scrape all the vegetation off every week. Such areas are artifacts, artificial, and do not concern the soil survey. They are small, a matter of a few meters in dimensions, and you can not put them on maps. You are just going to forget the removal of the vegetation and under certain conditions the removal of the snow will affect the temperature but these are artificial. We assume that the soil is supporting whatever kind of vegetation it can support. There are bare spots in Aridisols. The ground cover, the grass, and the shrubs, probably do not shade 10 percent of the soil surface, but this is the natural condition. If you irrigate, the soil temperature changes rather drastically, so we specify that you should not use the temperature of an irrigated soil. **Question 160, Cornell**

We have, however, used different limits for soils with an O horizon than we used for soils with an Ap horizon. On the assumption that if there is an O horizon, there must be some trees somewhere around and in the forest, particularly in the cooler regions, the O horizon insulates the soil during the warm season and so the net affect is to lower the mean annual temperature and to lower the summer temperature. **Question 161, Cornell**

3.3.2 Definitions of Soil Temperature Regimes

3.3.2.1 Selection of Critical Temperatures

The temperature limits were fixed by the necessity of avoiding the splitting of established series. It must be remembered that there was enormous pressure not to divide series unless there were some advantages in the way of improved interpretations from creating a new series from a part of an already established one. It so happens that in the U.S. the type of farming is closely related to the climate and the soil temperature is also closely related to the climate. The length of growing season is quite important in determining what kinds of crops may be grown. In the cotton belt in the southern part of the United States, the growing season must be long and the interpretations for the soils in that part of the U.S. are quite different from those that we make in the corn belt where the growing season is shorter. The limit between the cotton belt and the corn belt then was a limit where the soil series all changed and this temperature, mean annual soil temperature, on this boundary was approximately 15° C. We could then establish the difference between the thermic and mesic at 15° C without affecting the classification of the series. Similarly the limit between the mesic and the frigid involved another change in the type of farming and another change in the series that were warmer than 8° C or cooler than 8° C. One might then say that the major factor was the utilization of the soil because this determined the points at which the soil series were changed. **Question 15, Venezuela**

It so happens that at the time we began to develop *Soil Taxonomy* there was more or less a rule of thumb in soil correlation that a series should not be carried very far across a major land use boundary. In other words, if we went out of the cotton belt into the corn belt, the series virtually all changed. **Question 5, Texas**

The major land use areas across the northern U.S., in the Great Plains, we had spring wheat and flax versus winter wheat and a diversity of other drought tolerant crops. In the Middle West more humid areas, say Wisconsin, Illinois, there was a break between corn grown for grain and for silage, at about that temperature. There was also a difference in the nature of the soil, that at about that temperature, you went from what was called a Gray-Brown Podzolic Soil to a Gray Wooded Soil. The A2 horizon became an albic horizon with the lower temperature, rather than just an ochric epipedon with brown colors. Crossing into Michigan, at about that temperature, you went from Alfisols to Spodosols, and when you came over to New York State, you generally went from what were called Gray-Brown Podzolics to Podzols. **Question 101, Cornell**

So, if we drew the temperature limit at somewhere in the neighborhood of 8° C, we did not split very many series. It was an absolute minimum. The 15° C temperature limit was set the same way. This was a point where the series changed in the arid regions, from Desert to Red Desert; in the semi-arid regions from Chestnut to Reddish Chestnut; in the humid regions from Gray-Brown to Red-Yellow Podzolics. You switched from an agriculture based on cotton to one based on corn in the humid regions, sorghum and wheat in the drier regions. No particular difference in the arid regions, except that you had creosote bush on the reddish desert, and you did not on the normal desert. These were boundaries that were related to some extent to natural vegetation. They would not have been recognized at an early date at different great soil groups.

In later years they were based on the difference in the type of agriculture, where we made interpretations for one group of crops at one temperature, another group of crops in another temperature; and that limit all across the U.S. was 15° C. This is how those limits got set; they did not split series. It was only a very few of the very old series, like those which went from New Jersey to the south end of Florida. In New Jersey they are used for summer vegetables, while those in south of Florida, for citrus and winter vegetables. **Question 100, Cornell**

The 5° C Limit as a Biological Zero

In one respect this concept is valid, I think, because we are considering normal cultivated or useful plants. Certainly there are plants that are adapted to much lower temperatures. The New Zealand microbiologist isolated bacteria that would sour milk in the refrigerator but not in the room. So it has a particularly remarkable ability to withstand cold but not warmth. The plants that are able to grow and multiply at temperatures below 5° C are plants that are found in the cold regions. They are plants with which, for the most part, the soil survey does not much concern itself. **Question 36, Minnesota**

3.3.2.2 Categorical Level of Soil Temperature Regime

We brought it in at three levels, actually: suborders, great groups, and families. The distinctions at the higher categoric levels are rather broad distinctions. When we came down to the family level, where we want to begin to make precise quantitative interpretations approaching the series level, not there yet, we need some relatively refined subdivisions of temperature, compared to those that we have made at the suborder and great group levels. So, we use the frigid, mesic, thermic, hyperthermic subdivisions with the idea that we can keep a single series from running from New Jersey in the north to the southern tip of Florida, which we used to have. You cannot make the same statements about the soils. **Question 180, Cornell**

At the great group and suborder level we use broader subdivisions of temperature than we do at the family. It often happens that people want to make interpretations of a sort from

small-scale maps. In the small-scale maps the temperature is used at the suborder and great group levels. These are the kinds of units that are used on the small-scale maps for cartography. If one does not use temperature in broad classes on small-scale maps, it becomes difficult to make interpretations. If you examine the soil map of the U.S. in the *National Atlas*, there is quite a large area of Alfisols that is shown in the mountains in Arizona, Colorado, and New Mexico. In the legend of the FAO Unesco map, these are grouped with the Alfisols of Ohio and Indiana because they have the same horizon sequence. There is no way from looking at the map to know what the elevation might be, you don't know the potential for farming from the small-scale map. Whereas if they are identified as Cryoboralfs or something like that you will know immediately that the area is not suited for cultivation. It may be used for forestry and perhaps for grazing, but not for farming. On the FAO Unesco map of the U. S. you cannot reach that conclusion. If you don't require the man who is making the map to determine what the soil temperatures are, he can very easily forget it. You come up then with a soil map at a very different scale from any climatic map that you might be able to lay your hands on, and the map might just as well have been made to put in a drawer or hang on the wall as any other purpose because you can't use it for anything without the temperature and the moisture. **Question 93, Texas**

3.3.2.3 The Iso Temperature Regimes

Significance to Soil Classification

One must keep in mind that one of the purposes of developing *Soil Taxonomy* was to facilitate interpretations about soil use. Consider the differences between soils that have a mean annual temperature perhaps of 10 to 12° C, one soil being in a temperate region and the other in an intertropical region. The growing season in the intertropical region is controlled by soil moisture not by soil temperature because the soil temperature does not fluctuate from one season to another by very many degrees. In the higher latitudes, the same mean annual temperature means that the soil is much warmer than the average in summer and much colder in the winter and the growing season may be controlled by both temperature and moisture. Therefore for interpretations at the higher categoric levels that one uses on small-scale maps it is necessary to make a distinction between the soils whose temperature vary widely between summer and winter and soils which have the same temperature in summer or in winter.

The limits of 5° C difference between summer and winter were proposed on the basis of an examination of the air temperatures at the two tropics. No criticisms were received before *Soil Taxonomy* was printed. However, it seems that probably the hyperthermic temperatures should have been included with the isohyperthermic temperatures for the basis of interpretations. This is a problem that needs examination perhaps more generally, and yet the tropic great groups which are defined by the difference between winter and summer temperatures probably should have included the soils that have hyperthermic temperatures. The distinctions between soil temperature classes are shown at the family level, but there are many small-scale maps that cannot use soil families in the legend and if the temperatures are not indicated generally by the name of the map unit in the taxonomy, then the temperature has to be introduced as a phase. In general, climatic phases are impractical because there is no universally acceptable classification of climate. In addition climatic maps are normally on a very small scale and cannot be useful for large-scale maps and the relation between the air climate and the soil climate is quite imperfect. There are not enough meteorological stations in the world to show the rain shadows that exist in the mountainous areas. **Question 42, Leamy**

From the point of view of soil genesis, the soils whose growing seasons are controlled by temperature, have, in the fall of the year a cessation of plant growth and one has a flush of new foliage on the surface of the soil. The leaves of trees, the dry grasses, and so on, the crop residues, all provide large amounts of fresh organic matter at the soil surface. In the humid parts of the intertropical regions where there is no dry season and no control on the growing season by moisture or temperature, there are no large flushes of fresh organic matter. One finds instead, that the leaves drop at any month of the year in small numbers, and there is a continuous accretion of organic litter at the surface, but no large flush of new organic matter at

the surface. In the intertropical regions where the growing season is controlled by moisture, the plants stop growing when the rains stop and the leaves fall, the grasses die, and there is little difference in the flush of fresh organic litter between the tropics and intertropical regions. Therefore, the tropic great groups are all defined as having a udic moisture regime rather than an ustic moisture regime. There seems to be a difference in the genetic effects of a large amount of organic matter coming over a short period and the same amount of organic matter coming over a full year. One sees differences between the soils of the humid tropics and the humid temperate regions that can hardly be explained other than on the basis of the key leading effects of large amounts of soluble organic materials coming within a short time and the same amounts coming very evenly spaced over the year. **Question 42, Leamy**

Redefinition of Tropo-Taxa

We had lengthy discussions with European pedologists who had worked in tropical areas about the classification of such soils. The distinction that we have made between the soils of temperate regions and tropical regions, that is in the tropic great groups, were restricted to udic and aquic great groups, that is correct. The European pedologists felt that in the humid tropics the leaf fall, the relations between vegetation and the soil were different from the temperate regions where the temperature controls the growing season as well as moisture. It is a genetic factor that in North Carolina and New York with the deciduous forest you get a flush of fresh organic matter in the fall when the leaves drop. In the tropics this is a continuous process. There is no flush at any season where the trees are evergreens. When we come to the drier regions, the Europeans felt that there was no such difference, that you got a flush of vegetation, say, when the grasses died because of the lack of water, and you got the same sort of thing in the intertropical regions where you had a distinct dry season. They advised strongly against making any distinction where the moisture regime was ustic or aridic. That is the way *Taxonomy* was organized. The hyperthermic temperatures were not included with the isotemperature regimes in *Soil Taxonomy*. It does seem that in the humid hyperthermic regions, as in Florida, there is little difference between the hyperthermic and isohyperthermic. There is no serious frost problem in either temperature. The crops are very similar. In Thailand, Professor Moormann, now at the University of Utrecht, but who worked in Thailand for about 12 years, could find no difference in farming patterns between the hyperthermic and the isohyperthermic areas as far as rice production is concerned, which is perhaps the most important crop in Thailand. Management practices are identical. He complained to me some years ago that there was no value in making that distinction in Thailand. He could have made the same statements about either one. I suggested to him that, perhaps, if the hyperthermic and isotemperatures were combined in the tropic great groups, that it might solve his problem. It would put similar things together instead of separating them. He thought for a moment about that and said yes that would solve the problem. When I think then about the hyperthermic areas in the U.S., which are also udic, that is largely in South Florida, and the udic areas of Venezuela, I can see no real reason for keeping them separate, putting one into a tropic great group and another into a different great group. They seem to behave the same and if they are separated we are separating things that are basically alike, that is the reason I have proposed this. Generally, it is where the regimes are udic that we should combine the hyperthermic temperatures with the tropic great groups. Now in the lower Rio Grande where you again have hyperthermic temperatures, you also have a control on the growing season by moisture rather than by temperature. I think that this is a problem for the International Committee on intertropical moisture and temperature that they are considering and will make recommendations on. These are people with much more experience in those areas than me. **Question 108, Texas**

In many ways the bulk of the hyperthermic temperature areas are more nearly tropical than temperate. We wanted to be able to use different criteria in intertropical regions from those we used in the temperate regions. One of the over-riding considerations is that so many of the intertropical soils have no relation that is discernible between soil color and organic matter. In New York State and in Illinois, in the temperate regions of North America and Europe, there is a relation between color and organic matter. This relationship disappears in intertropical regions. So we have biased our classification of the soils of the U.S. by using color value to define mollic epipedons, umbric epipedons, because the color is related to the carbon. But in intertropical regions if we use color, we are getting groupings that have no

meaning. Now the hyperthermic zone seemed more like the intertropical regions than the temperate regions. **Question 157, Cornell**

In several of the orders, as in Alfisols, Ultisols, Mollisols, etc., the suborders were defined primarily by the soil moisture regimes. In these orders then, the temperature regime was brought in to be used on small-scale maps as a subdivision of soils with a particular moisture regime. Therefore, in the Alfisols we had Udalfs, Ustalfs and Xeralfs at the suborder level and at the great group level we were able to recognize the tropic great groups to avoid the distinctions according to the darkness of the epipedon. In the Inceptisols the suborders were not defined on the basis of soil moisture. Instead, we had the suborders of Andepts, Ochrepts, Umbrepts, etc., and therefore, the suborder of Tropepts was set up to avoid the distinction at the suborder level of umbric and ochric epipedons. This is the same problem we had with the Ultisols where we needed to avoid this distinction between umbric and ochric epipedons in intertropical regions. It is possible that we made a serious mistake in subdividing the Inceptisols at the suborder level into Umbrepts and Ochrepts. This is a problem that must be considered by another generation that has more experience with intertropical soils than was available to us when we were developing *Soil Taxonomy*. **Question 18, Leamy**

It probably is not material whether one uses the "tropo" modifier at the great group or the subgroup level other than the problem that requires the extension of the umbric epipedon or the ochric epipedon importance into intertropical regions. The basic reason for using it at the great group level was to avoid the extension of these concepts that are applicable in temperate regions to intertropical regions, e.g. the weighting of the soil color value because it is related to the organic matter, in temperate regions. **Question 17, Leamy**

In the West Indies, I had hundreds of analyses of organic matter, each with the Munsell color value, and there is no relation whatever. These were not only isohyperthermic; they were also isothermic. **Question 168, Cornell**

3.3.2.4 Permafrost

Permafrost Criterion for Use at the Order Level

There is nothing sacred about the number of orders in *Soil Taxonomy*. It merely reflects what knowledge we had at the time we developed the system and we may have made a serious mistake. This is not a matter for the judgement of one person, rather a group judgement as to the importance of permafrost, cryoturbation as compared to the distinction between organic Histosols and the various mineral soils and so on. **Question 25, Minnesota**

In defining such an order, as I say, one normally would use not a single property but a combination, and one might want to distinguish the permafrost mineral soils from the others at the order level but not include the Histosols in that group. That would be a possibility. And it is a matter that should be discussed, I think, by people who have some experience with these soils and know something about them. So, I would say this is not something on which my opinion would be important but it is something that should be discussed by an international committee. **Question 26, Minnesota**

Problems with Classification

In the Cryoborolls, for example, in the western mountains, some are under forest, some are under grass. Their potentials seem to be very different and the reason for having forest vs. grass or forest vs. tundra probably are not presently understood. It may be entirely a non-soil factor, not necessarily the temperature. It may be a matter of wind, of snow accumulation, and so on. If it is the wind or the snow then, I think, the phase is the appropriate level for the distinction.

We often have seen a soil that is normally above timberline lying well below one that was below timberline because of frost pockets. Now this, again, is hardly a soil feature. It is a

matter of the length of growing season. The length of the growing season can be treated. If it can be related to soil temperature it can be treated at a series level. If it is unrelated to temperature, I wouldn't know how to do it. The minute you build it into your taxonomy as a series, the plant breeders are going to come along and change all this and you will find your taxonomy is tied to an agriculture that no longer exists. For this sort of thing I would prefer a phase. I can give an example in Canada where you made an interpretive map for wheat production in the prairie provinces, and before you could get it printed, the plant breeders came along and pushed the wheat line many many miles to the north. The map was made doubtful because it had been made as an interpretation rather than based on soil properties. So for this sort of thing, I much prefer phases to putting it (in) small, say one or two degree, increments of temperature as series limits.

It may be very difficult to separate these soils in *Taxonomy*. It may only be the growing season because you have willows in your tundra and they are one of your dominant vegetation (types). As suggested by some ecologists, the tundra/forest boundary may be reflected in temperature. It might be a very small difference. I don't know enough to really give a good answer only to explain what I see would be the principles involved. But you have lots of *Salix* on the tundra. They may not be greatly different from the birch. These are very small trees, you know. **Question 27, Minnesota**

3.3.2.5 Mesic vs Frigid

(Certain areas that receive large amounts of snow, like areas to the lee of the Great Lakes, have higher average annual soil temperatures than would be predicted from air temperatures and consequently qualify as mesic although in growing season, are more typical of frigid soils nearby. Is there justification for including summer soil temperatures as criteria to characterize the soils more nearly consistent with their biological environments?) There is no question that the mean annual soil temperature rises with the thickness of the snow mantle that insulates the soil during the cold season. The soil temperature is very appreciably warmer than the air temperature in Alaska, for example. In these snow belts it is doubtful that the soil ever freezes to depths of more than a few centimeters and once the snow has accumulated it is doubtful that there is any frost in the soil whatever. In defining cryic temperatures we took this into account and cryic temperatures have low summer temperatures but have no frost in the soil or they are frozen rather deeply and have limited maximum summer temperature. This was done to separate frigid and cryic temperature regimes.

Here you are dealing with something that is a distinction between frigid and mesic and I am not experienced in this. I really have no valid opinion except that if the people concerned with these soils feel there is a problem, then it is up to them to suggest a modification. I know that in New York State you have a snow belt where farming has stopped. The land is very cheap I am told. It is used now only for summer residences. It is not only the soil temperatures. The farmers were isolated by this thick snow. They just moved out. They would not live there. **Question 175, Cornell**

Chapter 4

THE SOIL FAMILY CATEGORY

reviewed by B. Hajek[6]

4.1 Introduction, Rationale and Diagnostic Criteria

An explanation of rationale in constructing the family category and of criteria for family classes requires going back a long way in time. Starting in 1900, approximately, we began to build up a group of soil series which were defined with varying rigor at varying periods of time. These soil series and types were the basis for the published soil surveys, and they had a good deal of actual testing in the field. People became familiar with them, and they used them. In Iowa, farms advertised for sale in the newspapers generally said 160 acres of Carrington loam. The tax assessors used the soil series and types, and they became familiar with them, they established well their utility.

At [about 1938] there were from five to six thousand soil series recognized in the U.S. This was too many for anyone to comprehend. While there were long arguments about the importance of grouping the series into successively higher categories, no one knew the series well enough to do this. It was necessary, then, to find differentiae or some groups of differentiae, for the higher categories, and to test them by seeing how the series fell into proposed definitions. We had no criteria in mind when we started to arrange the categories between the series and the great soil groups. But we were having discussions for many years about the intergrades between one great soil group and another -- soils that shared some characteristics of another or several other great soil groups. This seemed to be a logical basis for defining the subgroups.

The correlation process needed a link between the subgroups and the series. Dr. Allaway made the suggestion that, at the subgroup level, we had adequately taken care of all the genetic factors that concern us, so at the family level, we should take into account the practical physical factors that affect the growth of plants and the engineering use of soils. Several concepts were tested beginning with the *Third Approximation* that proposed the use of the physical properties affecting plant growth and engineering uses of soils. A number of definitions were tested by examining groups of series that resulted from the definitions. As a result of this testing, definitions were modified rather substantially in the *Fourth and Fifth Approximations*.

Beginning with the *Sixth Approximation*, the interpretations that were made for the various phases of all the series that fell into a single family were examined. The assumption was that if substantially different interpretations had to be made for comparable phases of the series in the family, there was something wrong with either the interpretations that were being made or with definitions that produced those groupings. Basically, the family grouping is intended to permit us to group soils about which we make the same major interpretations for use and management. If we get soils in a family whose comparable phases require substantially different interpretations, we know there is something wrong. A number of such defects have

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6. Professor of Soil Classification, Department of Agronomy & Soils; Auburn University, Auburn, Alabama, 36849-4201.

come to life since *Soil Taxonomy* was published. There is a major problem about how *Soil Taxonomy* is going to be revised and kept up to date. That problem is unresolved as yet. There have been to my knowledge no really approved changes in *Soil Taxonomy* since it was printed, although suggestions have been flowing into Washington from outside the U.S. as well as within. **Question 1, Texas**

A review of diagnostics used in *Soil Taxonomy* will show that we have used the same characteristics, such as temperature, at different categoric levels. However, temperature limits for the family are smaller than those of the suborder. This was done because of the value of some properties for interpretive uses, and we would be, I think, violating the logic of classification if we blindly used one characteristic at the same categoric level with all soils. The logic of classification dictates that we should have classes about which you can make the greatest number and most important statements. For the most part, the important things that concern us with the soil survey are interpretations. We also have to bring together soil classification and capability classification. One is an interpretive classification and the other is taxonomic. You have to go one additional step in reasoning to get from taxonomy to capability. It was about the only test we had of the validity of the way we had grouped our soils. Namely, what could we say about their use and behavior. Those are our important statements in the soil survey. **Question 16, Texas**

The problem of using engineering properties such as Atterberg limits for diagnostics at the family level is that we have so few determinations of Atterberg limits. If we used them we wouldn't know how to interpret them and we wouldn't know how they caused the groupings of our soils to be changed. It is simply a lack of data. The engineers have a large volume of data on Atterberg limits but not by kinds of soil. **Question 115, Minnesota**

Most families have only one series which could suggest that the family is too narrowly defined or the series is too broadly defined. The U.S. experience clearly indicates that the first series were too broad for quantitative interpretation. So the series over the years, have continuously been narrowed in their definition. Take the Carrington loam as an example, at one time it was included in the concept of the Miami series, though Carrington is a Mollisol and Miami is an Alfisol. The original concept of the Miami included the Carrington. With continued experience with the use of Carrington, it was subdivided. Today there are 20 to 30 series which were originally in the concept of the Carrington loam.

It is rather general that when one first starts making soil surveys, one knows only a little about the relevance of a property that he can see. If he starts with series, he is going to make them quite broad. If you examined the publication on the classification of soil series in the U.S., you will find that there is not a uniform distribution of series within families. We have a few families with a large number of series and we have a large number of families with only one or a very few series. Some of this reflects the range of production one can find within a family according to management. The family category is adequate for broad interpretation but is inadequate for quantitative interpretations. Some of this is reflected in the geomorphology of an area. The very strongly weathered materials lose a lot of variability as a result of weathering and there are large areas where, when sediments were first deposited, they probably were heterogeneous but are now homogeneous. We tend then, in the more strongly weathered materials to have fewer series per family than we do in the young glacial landscapes. I had the experience in Venezuela and it is interesting to note that they are just the same as in the U.S. when we started. **Question 125, Cornell**

4.2 Technology Transfer - Interpretation

One of the principles followed in the construction of *Soil Taxonomy* is that we should be able to make the largest number of the most important statements about the soils that are grouped in any taxon at any categoric level. For the most detailed interpretations, one does have to go to the family or even to the series level. However, if there are known factors and one is mapping at a small scale so that application at the family level is impossible, it is still

practical to use phases of subgroups or great groups to increase the number of interpretations that can be made. The phases may include family criteria that are pertinent to the foreseeable uses of the soil. For example, if the reaction is known but the clay mineralogy is not, one can use a phase at the subgroup level to indicate a non-acid reaction provided that this seems important to foreseeable uses of the soil. **Question 20, Venezuela**

I do not know at what level in *Taxonomy* interpretations are being made in the Benchmark Soils Project. However, since they were irrigating as one system of treatment, and because they were using mechanical cultivation, I suspect they have all plots on level land, just like all other experiment stations. If this is the case they obviously have used phase criteria when they selected sites. They have not taken the full range of soils within the family; they have selected the more level areas. In their current interpretations they are beginning to specify that they have selected this phase, which initially was not in their statement; they just specified selection at the family level. Now, for interpretations being made they recognize that they cannot interpret for the whole family. This recognition is based on the experiment and location they have. The whole family would include slope or stoniness or other sets of characteristics that we might consider either phases or properties that could be used as phases. I know that they do not use a series, but they could phase to attempt to get the more important properties of series. **Question 150 and 151, Cornell**

The family level was not intended for the most precise quantitative interpretations such as yield of rice per hectare. It was intended to indicate that for a given phase of a family, the yields would be adequate to make the production of annual crops practical or impractical. There are some general implications of the nature of the annual crops that are suited for that particular soil. These are the major interpretations of our soil maps for soil conservationists in the USDA-Soil Conservation Service. The conservationist uses capability as an interpretive classification, and it must mesh with *Taxonomy*, or there is something wrong with one or the other, or both.

Interpretations for annual crops should always include specification of the plan of management to be followed. Conservationists do not tell a farmer what to do; the farmer tells us what he is going to do, and then according to what he plans, we can tell him what kinds of problems he can expect. As a result he may change his plans because of the consequences of having the wrong management in mind. By using phases of families, major interpretations such as these should be possible. The intent of *Soil Taxonomy* was that they would be possible.

As an example using nearly level or gently sloping phases of Oxisols in Malaysia and with a system of management that involved the use of shifting cultivation with long fallow; you can predict rather safely that a farmer is not going to get very good yields. It may be the only way he can utilize the soil, but he will not get rich.

Map unit interpretations should be feasible even if the unit includes a number of taxa. It should be possible to make interpretations first for the individual taxa, specifying the relative area of a given taxon, the interpretations for the use of that taxon and then the use of that map unit. This is still interpreting by the taxon, rather than by the mapping unit. An example of map unit interpretation is a wet drainage way crossing an area in which it is going to be a limitation for passage with wheeled vehicles. Normally it shows up as a line on a soil map. Since generally, in Soil Conservation Service programs, interpretations are made by fields as well as by kinds of soils, this unfavorable condition can reduce the potential of a much more favorable condition in the field. If you must cultivate and plant late because of a wet area in the field, we would advise a farmer that he is going to continue to have trouble unless he installs adequate drainage. The conservationist does not tell him he should, but he can tell him he is going to be planting late and his yields are going to be reduced because of the wet condition.

Map unit interpretations for other than the growth of plants should also be feasible at levels above the family, for instance consider a map unit which is a mixture of Fragiaqualfs and Fragiudalfs. The whole map unit would be unsuited for the development of housing with septic tanks and special basements would be needed for the houses. **Question 152, Cornell**

Many agrononic and non-agricultural interpretations are based on depth of soil. As I interpret the definitions of the family and series control sections, we have a break at 20 inches for the lithic contact or the shallow soil in both the family and the subgroup. Another break at 40 inches, or a meter to be more precise, is required at the series level. Below 40 inches, or a meter, there is no strict requirement for a new series. It is possible to phase such soils in the family to improve your interpretations. If your interpretations are not good without the phase then I think we are remiss in not using phases of the family. After all, all of our interpretations are for phases of the families, not for families. **Question 128, Texas**

4.3 Family Control Section

In defining the control section for particle-size and mineralogical families, we generally make the distinction between soils that have an argillic horizon and soils that do not. If there is no argillic horizon, a more or less arbitrary control section of 25 cm to 1 m is used. If soils have an argillic horizon, the upper 50 cm of the argillic horizon is used. Now I must confess that, when I taught, I required my students to classify the soils that they were studying at the family level. On examinations I gave them exercises, where they were required to write the descriptions, and identify the family, and they couldn't do it. The trouble is the way *Soil Taxonomy* is written. To overcome this, I developed a little chart. I guess copies of this chart have been made available to Soil Conservation Service Staff and you can probably get copies. I simply listed the orders across (horizontal) the top and family differentiae on the y-axis (vertical at side) and then I came out with a lot of blocks in which I gave the control section, the number of particle-size classes, mineralogy classes and so on, and finally moving to a box that listed which families could be used in a particular order. Once I gave this to my students they had no trouble.[7] **Question 45, Texas**

The upper and in some cases the lower boundary of the family control section was related to the argillic horizon because of the prejudices of some correlators. For example, in the Ultisols, specifically the Paleudults, which were the type Red-Yellow Podzolic soils at one time, the upper part of the argillic horizon normally has less clay than the middle or lower parts. Some correlators working with Ultisols wanted to tie the family control section to the upper part of the argillic horizon rather than to the lower part, which has very little rooting. In the Midwest, the upper part of the Alfisol argillic horizon is the part that has the most clay. In the younger soils, such as Alfisols and Mollisols, if the maximum clay content is in the upper part of the argillic horizon, the lower part will show a considerable decrease in the percentage of clay. It is that maximum part, the maximum amount of clay, that controls permeability and other properties in younger soils. For both old soils and young soils, there were reasons why the correlators preferred to use the upper 50 centimeters of the argillic horizon. One reason is that no two pedologists could agree on where the argillic horizon stops. It had to be an arbitrary thickness in the upper part of the argillic horizon since it is operationally possible, as pointed out in *Soil Taxonomy*, for pedologists to agree on the upper limits of the argillic horizon. The method they can use requires drawing a smooth curve from the percentage of clay and the point at which the ratio reaches 1.2 times the clay content of the epipedon is the top of your argillic horizon. This is the method we proposed; it does require laboratory analyses but it is possible to do it. Pedologists may disagree on the upper argillic limit in the field since clay content is estimated.

Soil Taxonomy defines the control section consistently throughout. If there is an argillic horizon its upper 50 centimeters are used. If there is no argillic horizon, we used about the closest equivalent depths we could where there are no real morphological benchmarks that you can tie to within the soil. For example the distinction between an ochric epipedon and a cambic horizon is not a very clear thing. We use an arbitrary 25 cm depth for the upper boundary and one meter for the lower control section boundary. **Question 61, Minnesota**

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7. Editor's note: See Table 2, SMSS Monograph No. 6.

These control section limits were about the best that could be set within time constraints and level of effort. If somebody has the time to make an analysis of alternative systems, a better decision could probably be made. At the time we worked on this there was really not much opportunity to study many alternatives. In fact, it was very difficult to get the correlation staff and the state people to even check families versus the capability classification. They were supposed to have done that several times but in fact I think they took a bunch of data and looked at them and approved it without actually going into a detailed study. They were very pressed for time because they were busy making maps and correlating completed soil surveys. There is more time now to go back and reexamine what was done and whether it was done properly. **Question 62, Minnesota**

4.4 Family Particle-Size Classes

There are clear differences between soil textural classes and family particle-size classes. Texture refers to the particle-size distribution of the textural triangle published in the 1952 *Soil Survey Manual*. For interpretations we felt we needed somewhat different classes of particle-size distribution. We had to invent a substitute term for texture which was a correct technical term, "particle-size distribution", dropping out the word "distribution" for simplification. The various soil surveys of the world have used various groupings of particle-size distribution. The Dutch have one, the Belgians have another, the French have one, and they are not the same as that of the USDA. The principal difficulty is that the USDA textural triangle was for engineering interpretations. The range in clay content of a silt loam was from 0 to 27 percent clay. For engineering interpretations, this grouped quite unlike soil textures.

The limit of 18 percent clay between coarse and fine silty and coarse and fine loamy was made to better relate our soils to engineering classifications. Somewhere in the neighborhood of 18 percent clay there is a change from nonplastic to plastic and this is considered by the engineers to be a very important distinction. We took all of the soils for which we had data on the Atterberg limits, and particle-size analyses, and ran a correlation between clay content and the limit between plastic and nonplastic. It seemed that the limit was somewhere in the neighborhood of 18 percent clay. It is not exact, for some soils with as much as 20 percent clay would come out as nonplastic and some with as little as 16 percent clay come out as plastic but the 18 percent limit seemed to be somewhere in the right neighborhood. We compared the particle-size analyses with the descriptions of field men, and observed consistently that if they had 20 percent or more clay, and if the soil deformed in a plastic manner, they described it as a silty clay loam, although by the laboratory methods it was a silt loam. When we noticed the discrepancy between the texture described in the field and that measured in the laboratory, it was obvious that most of our field men were describing texture by the plasticity, not by an estimate of clay content, so that putting the limit somewhere around 18 percent merely brought the series concept into line with the laboratory measurements. Soils that had a silt loam texture, but exhibited plasticity, were normally described as silty clay loams or clay loams. The laboratory could not find enough clay, but Atterberg limits did indicate the plasticity of the soil. Other textural triangles in the world, generally, have a limit somewhere in the neighborhood of 18 percent. A few are 20, but most are close to 18 percent.

The 35 percent limit on clay was set by comparison of soil texture, and Atterberg limits. There seems to be a significant break at about that limit. Even though one stratified the samples by orders, the important change in the Atterberg limits was in the neighborhood of 35 percent clay. The same study indicated that there was another important break in Atterberg limits somewhere in the neighborhood of 60 percent clay. Again, without regard to the nature of the clay, whether 1:1 or 2:1 clay; the amorphous clays, of course, do not fit into this system readily because we still have no way to determine how much of the soil is of clay size. **Question 70, Cornell**

The textural triangle of the *Soil Survey Manual*, I should say, for some inexplicable reason to me, considered that a boulder was not part of the soil. This seemed unreasonable from the point of view of the plant, which has to deal with these boulders in its rooting system. So we

had to begin to recognize the distinction between a soil that was 75 percent coarse fragments versus one that had none and this again required a modification of the concept of soil texture because the plants are concerned with these coarse fragments which do not retain water. We had no way to deal with soils that were entirely or almost entirely coarse fragments. The skeletal class included those with fine earth, but we had in the perhumid climate of Hawaii, for example, lava, in which there was no fine earth fraction. But because it rained nearly every day we had beautiful forests growing on this fragmental material, and so modifications of the textural triangle were essential to deal with the diversity that we actually found in nature. **Question 62, Cornell**

The relative weight of the engineering influence versus the agronomic influence on the choosing of boundaries for family classes is about equal, though I would hate to be very specific on that. We had to subdivide the loams and the silt loams somewhere in the neighborhood of 18 percent clay. That is an important limit in the engineering classification, but it also has some considerable importance to the growth of plants. The silt loams, for example, in the old textural triangle ranged from zero to 27 percent clay and when you are in a coarse-silty family you have a number of problems with the growth of plants. Their structure is bad and permeability is very slow. Because of poor structure, the soil puddles rapidly and you don't get much penetration of your sprinkler water. It runs off unless you apply it very slowly. So there is an important agricultural difference between coarse-silty or coarse-loamy but particularly coarse-silty and fine-silty particle-size classes. We had a great deal of difficulty in deciding what to do about the very fine sand, and Dr. Whiteside and I had much correspondence about this. We tried to get the engineers and the geologists and pedologists to agree on a common classification and each society basically said, "We are willing to have a common one if you choose ours." So that effort broke down after quite a few years, without solving our problem of what to do with very fine sands which, in general, behave more like silt than they do like sand. This is especially true in terms of capillary rise and available moisture-holding capacity. So I could see nothing to do but sort of let this distraction float in the particle-size distribution grouping so that if the soil was otherwise a sand, examination of summation curves showed that the bulk of the very fine sand was more than seventy-four microns in diameter, but if it was otherwise a silt loam the bulk of the very fine sand was less than seventy-four microns. So we arrived at a grouping that is very similar to that of the engineers. The geologists used sixty-four, I believe, but this was not purely for engineering interpretations because these properties of capillary rise or moisture-holding capacity are also important to the growth of plants. In general, I think one can say that most of the properties that are important for the growth of plants are also important for engineering uses or vise versa. **Question 114, Minnesota**

The history of the family textural classification with respect to agriculture indicates that continued use of our old textural triangle required that we make some rather drastic changes. In the first place, a boulder of a meter in diameter was not part of the soil. How this idea originated, I don't know, but the larger stones were not considered as part of the soil although trees growing there and so on, noticed the stones. The fragmental family class had no place in the old textural triangle. A soil may be a hundred percent coarse fragments, but if these are large fragments, then there isn't any soil there despite tree growth. So we could not use the old textural triangle for a variety of reasons. It lacked the break between fine- and coarse-loamy and silty which approximates the engineering break between plastic and nonplastic. It ignored the skeletal classes completely. The bulk of those could not be part of the soil. You can't bring them into your textural triangle. The fact that it is sixty or seventy percent by volume of boulders and stones comes out exactly the same as a soil in which there are no boulders or stones. If the boulder gets on the surface, it is treated as a phase, but otherwise it is ignored in the old textural triangle. They are revising the manual. I don't know what they are going to do about that. They maintained that we must have the two terms "texture" which relates to the old triangle and "particle-size" class which relates to *Soil Taxonomy*. I think it gets rather confusing at times. **Question 117, Minnesota**

Fine and very fine distinctions were not made in Ultisols because where the clays were primarily kaolin and oxides, it seemed to the correlation staff that there was nothing to be gained. Where the clays were 2:1 lattice structure, it seemed rather important to make a distinction between a soil that had 75 percent clay versus one that had 40 percent clay. With 2:1 clays, the permeability is considerably influenced by the percentages of clay. Where the

clays are mostly oxides there seemed to be no such relation, and the correlation staff in the southern states in particular felt that they did not want to distinguish between 70 percent clay and 40 percent clay, that it added nothing to the interpretive value of the grouping at the family level to make this distinction. Now, there are differences in viewpoints. Those who have worked in the inter-tropical regions have suggested to me since publication of *Soil Taxonomy* that such a distinction might be useful in Oxisols. This is a problem for the International Committee for Classification of Oxisols to review. **Question 63, Cornell**

There are some clay limits in *Soil Taxonomy* that are not defined on any textural triangle or family class. I have proposed the complete removal of such a clay limit in the definition of the oxic horizon. Without the 15 percent clay limit, families would be standardized at 18 percent. There are not many other places in *Soil Taxonomy* that this occurs. I consider this a serious mistake in the definition of an oxic horizon. The limit was based on the assumption that there would be no silt in such a soil. But unhappily, the evidence that has accumulated is that there may be an appreciable amount of silt in an oxic horizon. It may be an artifact of the particle-size analysis or it may be that the dispersion process produces the silt, but nevertheless, it is measured in the laboratory. **Question 71, Cornell**

Florida was concerned with a family distinction between coated and uncoated classes of Psamments. The only data they had was on moisture equivalent. That is all that was available, nothing else. The definitions had to be written in terms of available data or we wouldn't have any notion as to what we were doing with the classification of our soils. I would surely agree that the very fine sand fraction, particularly that part less than about 74 microns is just as important to moisture properties as is the silt. In the taxonomy as written, you might talk about eyeballing. I looked at the cumulative curves of a number of sands. If the soil was a sand the bulk of the very fine sand was in the largest half of the very fine sand fraction. We had some data on very fine sand effects on capillary rise and moisture retention from Michigan. Consequently, using the definitions of families of the particle-size classes as they now stand, treat that very fine sand fraction in a floating manner so that if the bulk of the sand is medium and coarser sand, it is treated as sand. It was generally appreciably in the upper half of the very fine sand range. **Question 134, Texas**

Family particle-size classes could be determined by mixing of samples or weighting by horizon thickness. Normally we would prefer not to mix samples because we lose information if we do, but rather by weighting particle-size by thickness of the various subhorizons that were taken. As a general rule one gets along better by fitting a smooth curve of particle-size data, and as a function of depth, and then identifying the control section and from that taking the average of the control section. It often happens that the sampler doesn't sample the control section as such. By drawing this smooth curve, one can get at the particle-size distribution of the control section. **Question 139, Texas**

4.5 Family Mineralogy Classes

The suggested definitions for mineralogy classes came from the soil laboratory people, and what they had in mind when they made their recommendations, I do not know. I do know there has been criticism of mixed mineralogy. It is a problem that needs to be studied by both personnel of NSSL in the laboratory and state experiment stations. They should be the ones to make suggestions for changes in mineralogy classes. **Question 40, Texas**

Clay mineralogy is not specified in soils with less than 35 percent clay because obviously, the more clay you have, the more important clay mineralogy becomes. As a general rule, when you have only five percent clay, clay mineralogy is not as important as is the mineralogy of silt and sand. There are places in *Taxonomy* where we have used clay mineralogy at the subgroup level rather than the family for soils with only five percent clay. If experience shows that in the fine loamy and fine silty fraction clay mineralogy is as, or more, important than silt and sand mineralogy, we would probably be inclined to change the family definition for the U.S. as a whole. When we were doing this part of *Taxonomy*, we checked and found that the correlation

with a kind of clay, at least 1:1 and 2:1 lattice clays, was quite good. For example, with the identification of a subgroup of Mollisols, you had 2:1 lattice clays, while with a subgroup of Alfisols you had a subgroup of 1:1 lattice clays. The correlation was not perfect but it was pretty good. Now with the engineers concerned about the difference between illite and montmorillonite, they can make that distinction at the series level, instead of the family level. We don't make all our interpretations at the family level; we make only the major interpretations. **Question 60, Minnesota**

Field criteria can often be used to recognize family mineralogy of a fine-textured soil. For example, pedologists are able to distinguish soils in which the dominant clay mineral consists of kaolin with accessory oxides of iron and aluminum. Knowledge of soil genesis for example would tell us that the family mineralogy of an Oxisol was either kaolinitic, oxidic or ferritic, but would not tell us which of those three. Knowledge of bedrock geology and geomorphology gives us some clues as to the nature of the clay minerals in the soil, but is hardly adequate to let us say definitely which one it is. Now, when you have a measure of cation exchange capacity one can infer a good deal about the nature of the clay. If the CEC by ammonium acetate is 60 milliequivalents per 100 grams of clay, one can be confident that the clay fraction is dominated by either montmorillonite or vermiculite. With some background information from the laboratory, one can usually infer which of these it is.

The CEC can be estimated in the field with the help of a small portable laboratory about the size of a briefcase. You can estimate the clay with your fingers and from those two, you can get an estimate of the nature of the clay. If it's below 24 milliequivalents or below 16, it certainly is kaolinitic. Somewhere between 24 and 45, it's going to be mixed. This however, requires the use of the field laboratory kit to get at the CEC per hundred grams of soil. Without that, it's very difficult. In working in the West Indies, we did use the field kit and we arrived at kaolinitic mineralogy for some Paleudults. This is what they should have been, but we had to check it out and it came out about 16 milliequivalents per hundred grams of clay. **Question 11, Venezuela**

With respect to the definition of oxidic mineralogy, there are still two alternative courses of action. If you decide you don't want the oxidic mineralogy in Alfisols and Ultisols, that is as far as you should go in your proposal because they may still want these in Oxisols, for example. There are many oxidic families of soils in Hawaii. Before you drop it completely, you must examine its impact in other orders than Alfisols and Ultisols. **Question 138, Texas**

4.6 Family Temperature Classes

Physiographic regions or morphological differences that are fairly readily observed in the soil, were not considered in setting limits for temperature classes because normally, soil temperature can be inferred from latitude and elevation. Since the subdivision is only made at the family level, rather than at the subgroup or great group levels, one would not anticipate any particular morphological difference. If there was a morphological difference, it would have been brought into *Taxonomy* at a higher categorical level. **Question 104b, Cornell**

4.7 Taxonomic and Map Unit Names

There is a lot of difficulty among some people in dealing with the use of the series name for two different sorts of things. one, the concept of the series, which is pure so that we have a description giving the ranges of properties, for example, of Miami. But this is conceptual. When we get into the field, we may examine the pedon, and if the properties that we find lie within the ranges of that concept, we may say this is Miami. If they do not; it still may be an example of Miami. Having made our map, we use Miami loam as a name for one of the map units which has a significantly wider range in properties than does the concept of the Miami series. The three different concepts for the same term do not disturb me because it is very

common in the English language to have a single word with a number of meanings. In such cases one determines the meaning intended from the context in which the word is used. **Question 126, Cornell**

It has been suggested that the confusion resulting from the use of series names for both taxonomic and map units may justify reserving the long established convention of series names for map units, and in effect dropping the soil series category from *Soil Taxonomy*. To some extent, at least, the soil series are considered a category in the taxonomy, and yet they are not defined in *Soil Taxonomy*; there are too many. The definitions of the series themselves take quite a few filing cases, instead of the one microfiche. You can, of course, microfiche the series definitions and descriptions, but the series has always been a pragmatic category. We establish series with narrow ranges of properties and also with relatively broad ranges in properties, according to whether or not that definition lets us make the best interpretations that we can make to meet the needs of a particular soil survey. The only limits that are imposed on the series are those that have accumulated in the family and the higher categories, and the pedologist is free to subdivide that range into as many series as can be proven useful.

We did drop type as a category in the previous system and moved it to phase level. Presumably, type was supposed to reflect the texture of the plow layer, or its equivalent in an undisturbed soil, but nationwide, the usage of type names was quite variable. In Iowa, Sharpsburg silty clay loam has an argillic horizon with a silty clay texture. When eroded, the plow layer is normally a complex of silty clay loam and silty clay textures. To be strictly accurate, the map units should have been named Sharpsburg silty clay loam and silty clay, where the soils were eroded; but they did not do that in Iowa or Missouri. Under the influence of some previous correlator, these soils were named according to what they thought the surface texture had been originally. In other parts of the country, an Ultisol with a sandy loam plow layer overlying a clayey argillic horizon would be named as a clay texture if erosion had removed the sandy loam surface. The argument there was that you had to do this because you could not be sure what the original texture had been before erosion. So we have Cecil sandy loam and Cecil clay types and map units in the southern states.

If we were going to retain type as a category, then we had to make a change in the map-naming process where they thought they could identify what the texture had been before erosion and require them to complicate their map names by listing all the textures that occurred within the map unit. This did not seem to be a useful sort of exercise, so we simply moved the surface texture to a phase level where it could be shown when it was important or disregarded if it was not important. If one wants to drop their series as a category, I suspect you will have to go the same route with the family and use a large number of complicated phase names for the families. Again, this does not seem to be a useful sort of exercise. The names are complicated enough by phases as it is, and the family names are not usually well received by farmers. They are useful to pedologists, but the farmer prefers a simpler name, and he is the one we are trying to help in the rural areas. In the urban-planning process, we are dealing with people who are trained in one or more technical disciplines and they can master the meaning of the family name without much trouble. But they would not be bothered by all of the phase features that we would have to specify for the family in order to arrive at something comparable to the series. **Question 129, Cornell**

If the family name is shortened by using the name of the most dominant series in it, you will still have slope phases and erosion phases. If you want to drop the series category, you are going to have to phase about 40 other characteristics. In one family that has a wide geographic distribution, they have used a series from Iowa as a family name there and another series from Oregon as a family name there. For the most part this represents a defect in *Soil Taxonomy* because these should not be in the same family. The one with virtually no rainfall in summer can only be used with irrigation to grow maize; the one in Iowa produces very good yields without irrigation, and they do not belong with the same family. The proposal has been made to correct this defect, particularly true in Aqualfs, for example, or other aquic great groups where you have a wet/dry climate versus where you have a humid climate. **Question 130, Cornell**

4.8 Variability in Map Unit Delineations

I believe statistical studies showing as much as 50 percent of map delineation areas outside series ranges are valid. This illustrates a problem of natural variability within areas that can be delineated at the scale in which the maps are made. We have had some working rules covering the naming of map units. This is what we are dealing with here. We have situations where the variability is small but does cross a family boundary, and so we have soils that have very similar behavior occurring in two families in varying proportions within one delineation or another in a soil survey.

The problem here is one of putting a name on the map unit, not so much as one of trying to purify the map units. One can get such a complex map, that even a trained pedologist cannot use it. With experience, we learned that instead of gaining anything, we lose in attempts to be extremely pure. The way in which the map units are named has varied over time and probably will continue to do so in the future, but at the time I retired we had a general understanding that one could name a map unit for the most abundant, most extensive taxon or series within that unit. It might not represent even half of the area that was delineated, but if it had an area larger than any other single kind of soil, we would go ahead and use the series name for that map unit.

4.9 Drainage Classes

In general, the correlation staff thought that well drained or moderately well drained soils could be kept together in *Taxonomy*, and the distinction handled as a series difference. When drainage got worse than moderately well drained, it was considered to be important enough that they needed other families, and the families required a subgroup separation -- as an aquic subgroup. We had only four subdivisions that were possible. We had the freely drained soil, aeric subgroups of the poorly drained soils, aquic subgroups, and typic subgroups. But we have five drainage classes which were ill-defined in general. It seemed to me that they should be able to get by with four classes, according to drainage, and depth to mottling (which was defined) instead of the five classes provided in the *Soil Survey Manual*. **Question 85, Cornell**

4.10 Sloping Families of Aquolls and Other Great Groups

Slope is used extensively as a phase criterion because of its importance in soil management. I would be a little slow in accepting a proposal to eliminate the sloping families of the aquic great groups. The differences in normal sloping phases are not so much in the nature of the soil as in the hazards of erosion. The differences in these sloping families are not concerned with erosion, but are concerned with the difficulty of removing the surplus water, almost the impossibility of removing it, and the genetic differences in the ground water levels. The normal users of the soil surveys have associated sloping phases with the problems of soil management related to erosion. They could easily be confused by the use of the sloping phase where the problem is almost completely another problem, one of drainage. The differences in the genesis, of course, are related to the fact that the water in the sloping phases is coming from seepage, rather than from the rain that falls directly on the soil. *Soil Taxonomy* states that sloping families should not be used in Aquods where in many soils the wetness is due to a placic horizon, or in the Albaqualfs, where the intent was to keep the old claypan Planosols together. I think it would be desirable in the case of the Histosols to use sloping families. Whether or not sloping phases of Aquults exist, I do not know at this moment. I have not seen such soils. **Question 3, Witty & Guthrie**

Chapter 5

ALFISOLS

reviewed by T.R. Forbes[8]

5.1 Order Criteria

5.1.1 Base Saturation - Historical Perspective

The 35% base saturation limit, reflected a desire to retain some of the zonality that we found between the Red-Yellow Podzolic soils of the southern U.S., and the Grey-Brown Podzolic soils of the glaciated regions in the northern part of the U.S. The examination of the data indicated generally that the base saturation in the Red-Yellow Podzolic soils decreased with depth below the B horizon, or even within the B, whereas in the Alfisols, the base saturation increased. The Ultisols in general were conceived of as soils in which the reserve of bases was maintained by recycling by plants. In the Alfisols, the reserve of bases was maintained not only by recycling of the bases by plants, but by weathering of primary minerals. We felt that the Ultisols were soils that could not be brought into permanent cultivation without the use of soil amendments, whereas we have plenty of examples of permanent cultivation of Alfisols without amendments, in Western Europe and in the northern parts of the United States. We had to find some basis, then to distinguish between the soils that could be used only for shifting cultivation without amendments, and the soils that could support a permanent agriculture, and examination of the data suggested that the 35% limit by the sum of bases method might make such a separation. Soils that had been considered as Red-Yellow Podzolic soils with large amounts of free oxides had enough varied pH-dependent charge that the sum of bases method showed base saturation below 35%, but ammonium acetate showed base saturation in excess of 50%. To keep the soils together that had been considered Red-Yellow Podzolic soils, therefore, we chose sum of bases, not knowing that the free oxides contributed so much to the low base saturation when we used sum of bases. We simply examined the groupings that we got by using the two methods, and we had only a few data by ammonium acetate on the Red-Yellow Podzolic soils.
Question 72, Cornell

We had no basis to propose limits on the total extractable bases that seemed to make a distinction between Alfisols and Ultisols of the sort we wanted. We wanted to more or less keep the Gray-Brown Podzolic soils as we had conceived them in the 1938 classification. These can be very sandy, and have fewer bases than a clayey Red-Yellow Podzolic soil. There was a question, and there still is, as to which is the most important--the base saturation or the total bases. I do not know, myself, of any research that would establish that total bases are more important than base saturation. In general, I would question that at the moment, because with layer-lattice clays, if the base saturation becomes extremely low, the aluminum comes in and you have not only a low base saturation, but a high aluminum saturation. What little work I have seen would suggest that the aluminum toxicity may be more important than the total

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8. Senior Research Associate, Department of Agronomy, New York State College of Agriculture and Life Sciences, Cornell University, Ithaca, N.Y. 14853.

amount of bases that are present, at least to plants that are not aluminum collectors. **Question 73, Cornell**

Base saturation is intended as a sort of index of the reserve and how it got there. Cycling by plants versus weathering of primary minerals. If we had defined the difference between Alfisols and Ultisols as being, whether or not the soils could be cultivated permanently without amendment, we would have, then, an enormous element of subjectivity in the classification of a given soil. It would all depend on whether or not the man thought this could be cultivated indefinitely without amendments, and opinions are going to vary enormously on that point. You cannot write a definition of that sort. **Question 75, Cornell**

[The differentiation of Alfisols and Ultisols based on 35% base saturation] was a long time brewing. From the early data that we had when we began this work, it was obvious that in the Gray-Brown Podzolic soils the base saturation increased with depth, or was 100%, whereas in the Ultisols, the base saturation decreases with depth in the soil. At one stage we tried to make the distinction on the base saturation of the argillic horizon relative to the underlying horizon. The base saturation was low and it decreased with further depth. I think we had a limit at that time of 35% and, in the *Sixth Approximation*, the order that became Ultisols was defined as having a textural B with base saturation less than 35% or base saturation which decreases with depth from B to C. After this *Sixth Approximation* came out, I believe we kept much the same definitions in the *Seventh*. This stimulated some studies, particularly in Maryland, Pennsylvania, New Jersey where it had been a practice since the settlers first came to the U.S. to apply small amounts of burned lime to soil once a rotation. We had these soils that were on the coastal plain, very old soils in a humid climate that had been limed for upwards of about three hundred years. If we sampled in the forest areas that had not been cleared, we had extremely low base saturation, but if we sampled in the fields that had long been cultivated and limed, base saturation was commonly about 60% through the argillic horizon. We still had the problem of whether or not this was a large enough change to recognize new series for the woodlots as distinct from those of the fields on the farms in this area. Most of the people felt that it was not warranted to change the series because one was a woodlot and the other was cultivated but it would be useful to keep the same series so that the experience the people had from the cultivated field could be extended into the woodlots. To keep these soils as Ultisols instead of Alfisols, we had to modify the definition and we set the depth at which the base saturation should be under 35% at, I think, one meter or 1.8 meters. If we did this, then we could keep the soils together in a series. We have a complication in that definition, that comes from the soils from basalt in the southeast where the base saturation hangs just above or just below 35% at one meter eight. So there's a very complicated definition that is in there just to keep a few soils from basalt in the same series. And it is, admittedly, not an easy thing to map when the base saturation at that depth is unpredictable. You know it is going to be in the neighborhood of 35% but it may be 30, it may be 40. This is not a wide range but the soils that cause this complicated definition on depth were minor in extent in the U.S. but important in some countries. **Question 158, Minnesota**

Why is percent base saturation determined at 1.25 m below the top of the argillic horizon or at 1.8 m below the soil surface. In addition, what happens if at 1.8 m, there is a lithologic discontinuity with contrasting material? Also, what is meant by identification of base saturation at certain depths below the argillic horizon?

The first comment is, on what is meant by the word at a depth of one and a quarter meters or 1.8 meters. This means, according to the English language, "at". It can be measured in one of two ways. Either one takes a sample of a thin subhorizon *at the specified depth* or one samples all horizons above and below the critical depth and then makes a smooth curve of the data and the depth at which that curve crosses the 35% base saturation is either above or below the critical depth of 1.8 m or 1.25 m. If the smooth curve crosses the 35% base saturation limit at a depth shallower than the critical of 1.25 or 1.8 m then the base saturation is certainly less than 35% at the critical depth.

more statements and more precise statements about the soil use then we could otherwise make with another limit of base saturation or another limit of depth.

If there is a lithological discontinuity at or above the critical depths of 1.25 or 1.8 m the base saturation at these critical depths is still the 35% limit between the Alfisols and Ultisols. The base saturation of a specific horizon is not just a property of that specific horizon but it reflects the entire process of leaching and recycling of bases in the soil which affects the whole soil in all horizons not just the one horizon. The base saturation curves are quite interesting properties of the whole soil rather than of any specific horizon. **Question 26, Leamy**

5.1.2 Limits - Alfisols vs. Ultisols vs. Mollisols

What were the bases for (1) the 35% base saturation between Alfisols and Ultisols, and (2) the 50% base saturation requirement to qualify as Mollisols having argillic or cambic horizons?

We had no data on the Mollisols on base saturation by the sum of cations, because in calcareous soils it is impossible or was impossible to determine the base saturation. We could assume the calcareous soil was saturated, but we could not assume what the exchange capacity really was. This was the only method by which we had any data, and so we had to define the method by the availability of the data. In most soils with a low pH-dependent charge, the 50% base saturation is equivalent to 35% by sum of cations, but if there is a high pH-dependent charge, this relationship breaks down. **Question 76, Cornell**

We had regionalized our laboratories and in the eastern part of the U.S. where we had most of our Alfisols, the laboratory used the sum of cations to measure the base exchange capacity and base saturation. On the Great Plains where we had a lot of calcareous soils the laboratory at Lincoln used ammonium acetate extraction because the sum of cations doesn't work in the calcareous soils. Most of our data on the Mollisols were accumulated at the Lincoln lab where pH was measured and base saturation was measured by ammonium acetate at pH 7. Most of our data on Ultisols were from the Beltsville laboratory where these same measurements were made by the sum of cations. When we began to look at 35% or 50% or what have you, as a limit that would affect the classification of the series, we could not very well compare the two methods because we had only the sum of cations on the Ultisols and only ammonium acetate on the Mollisols and the Inceptisols. We had a few soils of which we had both. And one of those was the pedon I used in the *Seventh Approximation* as an example of an Ultisol. Now it just happened that that was quite rich in free oxides as well as kaolinite. It had a very considerable pH-dependent charge. So that it went as an Ultisol, if we used sum of cations, and it went as an Alfisol, if we used ammonium acetate. Some of the best Ultisols were Red-Yellow Podzolic soils in the southeast at that moment. So without realizing what caused that pH-dependent charge at that moment we went ahead and said, well, this soil, a representative Red-Yellow Podzolic soil, is an Ultisol if we use sum of cations and 50% by ammonium acetate, but where you have a large pH-dependent charge that breaks down and it just happens that that particular soil was one that had a large pH-dependent charge. That's how it happened. **Question 149, Minnesota**

We did specify sum of bases for Alfisols and ammonium acetate for Inceptisols. That is the only thing we had data on in the bulk of the Ultisol/Alfisol separation. In the Inceptisols we used ammonium acetate because in general, over the world, that is the method that has been used, and if you use a method on which you have no data, you do not know what sort of classification you are developing. You must use methods which yield enough data to let you determine what you are doing with your definitions. What kinds of groupings you are making. **Question 82, Cornell**

What was the basis of the depths limits of 50 cm below the top of a fragipan, for the 35% base saturation limit between Alfisols and Ultisols, considering the fact that the fragipan is a root barrier?

The first point is that these soils are sometimes severely eroded, and what was originally at a depth of 1 meter, we now find at a depth of 50 cm, and we did not want to have to change the series because of erosion as long as we retained an identifiable part of the diagnostic horizons of the series. Erosion was to be considered a phase property. The upper boundary of the fragipan is something that generally can be identified in the field. It may be closer to the surface in an eroded soil than an uneroded soil, but it is identifiable, and if we put a limit below that point, rather than a limit in terms below the surface, it is a more stable limit. The fragipan is a barrier, but not a complete barrier to roots. It normally has the bleached nonbrittle surfaces around the polyhedrons in the pan, and the roots penetrate that rather readily, although sometimes they are flattened by pressure. Still we do extract some water and some nutrients from the pan itself. **Question 81, Cornell**

5.1.3 Argillic Horizon

Why was the argillic horizon clay increase established as a 20% relative increase compared to the overlying eluvial horizon?

The French taxonomy uses an increase of 40% as a basis for recognizing different classes, particularly the *sol lessive*. Amongst the Mollisols, the existence of an argillic horizon is rather widespread and marks the break between the late Pleistocene Mollisols and the Holocene Mollisols. In these soils, the break between the eluvial and illuvial horizons is at about an increase of 20% in clay. This is actually the minimum limit in the Mollisols at which we thought the field man could identify the change in the particle-size distribution or texture. Therefore, in *Soil Taxonomy* we took the absolute relative increase of 20% from the Mollisols as our minimum for recognition of an argillic horizon. In Alfisols and Ultisols, the normal situation is that the increase is 40% or more. It must be remembered that this increase of 20% is applied only to soils having clay contents ranging between 20 and 40% in the eluvial horizon. The 20% increase in a soil which has 20% clay means the field man must distinguish between 20 and 25% clay, with his fingers. We desired to have definitions that could be applied in the field without referring samples to the laboratory. **Question 31, Leamy**

The Inceptisol-Alfisol distinction rests in part on the clay ratio in the argillic horizon. We took that 1.2 ratio because we thought that was representing a large enough difference that the fieldman should be able to identify it consistently. That's where we got the ratio. When there is very little clay we took the 3% increase because we felt that that could be identified in the field and the intent was that that would be a large enough difference that you wouldn't have to wait for the laboratory data. Admittedly the laboratory might come back with a 1.16 ratio. Round that, and you get 1.2. But these ratios seem to be taken as sacred, which was not our intention.

We don't have enough hard data [on the required increase of fine clay in the argillic vs the overlying horizons,] really. The bulk of the measurements of fine clay have come from Ohio State's laboratory but we had fragmental data from North Dakota and a few other places and where an occasional soil had been studied but not on a routine basis. Only Ohio State, that I know of, at that time at least, had measured the fine clay. The definition changed gradually as a result of the introduction of that ratio in some of the early supplements to the *Seventh Approximation*. Some additional studies were stimulated and we ran into soils that we were confident had an argillic horizon but in which the ratio did not change appreciably. So that was removed as a requirement and left as some sort of a supplemental observation that one might make in case of doubt, but *it is not required at all any more*. There are two qualifications there and I think the words are 'usually' and 'about'. We have very few data on Ultisols, for the ratio of fine and coarse clay. It's very hard to find in the literature, and the Lincoln lab, so far as I know, does not yet make these except very occasionally for particular studies. **Question 152, Minnesota**

5.2 Differentia, Albic Horizon

[No minimum thickness in the definition of the albic horizon] may have been pure oversight. Many of the Boralfs have a relatively thin albic horizon where the argillic horizon has a fine or very fine texture and, if plowed, this is mixed and cannot be observed anymore but you can still observe the argillic horizon. When you look at the use we made of the albic horizon, I can't think of any place offhand, where it's diagnostic.

The only place in *Soil Taxonomy* where I find the albic horizon used as a diagnostic horizon is in the suborder of Albolls. The minimum thickness of albic horizons in other kinds of soil would not be critical because of presence or absence of an albic horizon is not diagnostic to the classification. It was our desire, generally, to keep in the same series in the same family, the cultivated and the undisturbed soil, so that the series would not be changed by a few plowings. There are soils, such as the Boralfs, which may have a very thin albic horizon if the argillic horizon is fine or very fine in texture, and these are kept together in the classification by not making the albic horizon diagnostic. Rather, we have used temperature, primarily, to define the suborder of Boralfs. The albic horizon is normal in these soils and has been recognized by the Canadians as a diagnostic feature. They, however, do not mind the thinness of the albic horizon because they classify the soil on the basis of the presumed virgin profile, rather than what is there today. The other group where the albic horizon is common is in the Spodosols. In the Russian classification, the Australian classification, and the New Zealand classification, soils classified as Podzols are soils that had an albic horizon, irrespective of the nature of the B horizon--argillic or spodic. There has been, in those countries, considerable resistance to *Soil Taxonomy* because it does not use the presence or absence or the thickness of the albic horizon as a diagnostic in the classification. **Question 46, Texas**

5.3 Differentia, Hard-Setting A Horizons

I'm familiar with the prejudice of the Iowans and the Illinoisians about grass vegetation vs. forest vegetation. I think they should make a trip down to the southern states and see those grasslands that are classified as Alfisols. I think what you need to do is take them out there sometime when the soil is dry on the surface and ask them to dig a hole. You may make believers out of them because these are hard-setting A horizons. **Question 74, Texas**

[These Alfisols with hard-setting A horizons are] very extensive in Australia. Quite extensive in parts of Spain where parts of the epipedons are still present, that is, the original A horizon. I haven't studied personally the soils of the Middle East, although I would expect them to be there. I have seen very few of them in South America. This may be largely because the soils of xeric moisture regime are pretty much confined to the West Coast which is largely covered with ash. I don't recall seeing them in Venezuela or in the West Indies.

[Most that I have seen are xeric] or ustic, in west Texas we have them. Then, of course, the Alfisols and Ultisols that have not been truncated would have this hard-setting A horizon if they ever became dry. We don't notice this because they are so rarely dry. If you go to southern Illinois the Albaqualfs occasionally become dry, and it takes ten minutes, perhaps, to get an auger through the A horizon into the argillic horizon below, you grind and grind and grind and can't dig it. These do become dry occasionally in some years. This would be a characteristic of Alfisols and Ultisols if they should become dry, that you do have this problem with soil structure with these soils. **Question 80, Texas**

What about the West African Alfisols in ustic soil moisture regimes, such as Niger, Upper Volta, and Mali as far as hard-setting properties, there seems to be a severe problem there too.

I haven't traveled in that part of the world, but I would expect that it would be. **Question 81, Texas**

It was my observation in the United States, in Australia, in Venezuela, that as we approach the boundary of the ustic and the aridic moisture regime, that the soils with argillic horizons had a hard and massive epipedon where the regime was ustic and had a granular and soft epipedon where the regime was aridic. In field work, in mapping, the boundary between Aridisols and Alfisols or Ultisols, the man making the map is much more easily able to determine the structure and consistence of the epipedon than he can the moisture regime. So we tried, in a number of places, to supplement the distinction between the moisture regimes with readily observable field properties, and it was for this reason that we thought that we could simplify the mapping problem if we restricted the Aridisols to soils that have a structured or soft epipedon.

I said that we use the nature of the epipedon in an attempt to eliminate the need for the mapper to decide about the moisture regime, and I did not say that this was entirely successful. The Australians have reported to me verbally somewhat similar situations where their Paleargids do not have a soft-structured epipedon. There's probably considerable need for reexamination of this criterion and there is now an international committee reexamining the classification of Aridisols. I would prefer that you should take this up with that committee and you will get some support from the Australians in trying to find another solution for the marginal cases, then. In this situation of yours and in the Australian situation the moisture regime is not marginal to ustic at the moment. It's clearly aridic, and I personally, never having seen these soils have no suggestion as to what modification in the definitions might be needed, but it seems clear from the verbal reports that I get that some modification is required in the definitions of the Alfisols, Ultisols, and Aridisols. **Question 46, Venezuela**

5.3.1 Field Test for Hard-Setting Horizons

I don't know (whether you should be encouraged to look for some field test to come up with a quantifiable number when making the determination "hard when dry"). I think it would be interesting to see some studies of the micromorphology of these in that, I think, when I look at a soil that is moist, I can identify the ones that will become hard and massive when dry, using just a ten power hand lens. Professor Tavernier also agrees. He thinks that's possible. He calls it a "ruined structure". But I haven't seen any thin-sections on any of these; somebody someday may undertake some. We've looked at them in many places in Italy and Spain in the xeric soils.

I can not quantify the term "ruined structure" very well in the absence of good terminology. I only know that when I look at the soil in a hand lens I think I see a distinct difference. To quantify that would require work with thin sections first. **Question 146, Minnesota**

Yes, (when you get to the Aridisol side of the boundary, from a Xeralf or an Ustalf, then you see a non-ruined structure which) seems quite different under a hand lens. **Question 147, Minnesota**

5.4 Rhodic Features

If one examines the soils that are included in Alfisols and Ultisols at any given suborder level, one finds generally that there are soils that have severe problems of soil structure. The physicists, the agricultural engineers have not been able to quantify methods for measuring soil structure, and yet this is one of the most important properties to agriculture that a soil possesses. Examining the soils that are classified as Xeralfs or Udults, one runs into soils that have no structural problems occasionally, but generally there are serious problems of soil structure under cultivation. The soils that do not have serious problems of structure, we first had found in the U. S., always had dark red colors. The original definitions of the rhodic great groups required values moist of 3 or less and hues redder than 5YR. **Question 28, Leamy**

In the U.S., the Rhodudalfs always have a red hue, as far as we now know. However, in other parts of the world, it is possible to find Ultisols, and I will cite the example from Tasmania, again, where we have one lava flow a few hundred meters above sealevel. We went from a mesic to thermic temperature regime on soils of the same lava flow -- same age. When one starts at sealevel, we have the dark red colors of Rhodudults of the U.S. As the elevation increases, the hue becomes browner and the value remains the same. The Tasmanians did not think that these should be separated on the basis of the hue. So, we defined the Rhodudults on the color value and not on the hue. If we find some Rhodudalfs that are very dark brown in colors, it might require a change in definition. **Question 117, Cornell**

Because, so far as we now know these soils are always developed from basic parent materials such as basalts, limestones, etc. The contents of phosphorus are generally higher in the rhodic great groups than in the others. The use of the color value and the chroma was predicated on the assumption that these features were correlated with the structural problems, with the phosphorus contents, and so on. There were many covarying properties that were extremely important to soil use in the rhodic great groups. No matter where one finds them, they are about the most intensively farmed soils of the particular suborder. Rhodic great groups were not set up in Mollisols because there were no particular differences in soil structure with soils that have a mollic epipedon. The formative element 'rhodic' implies red, whereas the actual characteristic used is the color value. This may disturb some people but one must recall that there are rhododendrons that are purple in color. **Question 28, Leamy**

It is primarily from the Rhodoxeralfs, the Rhodudults where we observed the same phenomena. In most Alfisols and Ultisols that retain an A horizon, or that even have been eroded into the B, the structure of the plow layer is critical to germination and growth of seedlings. The rhodic great groups, in the absence of any quantitative measures of the amount and form of the free iron, had to be defined on color. We know now that the free iron and its form are important factors in determining the pH-dependent charge on the clay. We also know from pragmatic experience that these dark red soils are intensively cultivated, that the structural problems are very easy to manage compared to the non-rhodic soils. We have to accumulate more data on the amounts of free iron to see whether the definition can be improved. Using the color simplified identification in the field, and relates well to land use. In general, in *Soil Taxonomy*, we have de-emphasized color relative to all other classification. But this was one point in which we thought the dark red color was an important mark of an important property. **Question 46, Cornell**

5.5 "Pale" Features

The concept of the "pale" great groups was intended to group the soils of very considerable age into separate taxa from these of late Pleistocene or Holocene age. We have no good geomorphic studies of the Paleboralfs, but we do have, in these soils, evidences of downward movement of the argillic horizon as they have tongues of albic material going into the argillic horizon, with tiny remnants of argillic horizon remaining in the albic horizon.

We observed that these albic horizons vary enormously in thickness. On the more stable surfaces, we can find these albic horizons to more than two meters thick. There is always an underlying argillic horizon, and at the contact between the albic and argillic there is evidence of destruction and downward movement of the argillic horizon. We, therefore, made an assumption that, when the albic horizon becomes very thick, this is an evidence of considerable age in the soil. Those of late Pleistocene normally have an albic horizon of less than 50 cm. But there are also Boralfs with more than 2 m of albic horizon. This was a characteristic we could use for the Boralfs.

When we get into the semi-arid and arid regions, we have to use the presence or absence of the petrocalcic horizon among other properties that we use - the thick argillic horizon, clayey textures and abrupt boundary between the material above the argillic and the argillic. **Question 117b, Cornell**

One rationale, to start with, was the observation that as the soil climate became drier, with more intense and greater frequency of moisture changes in the soil, we got stronger and stronger development of the argillic horizons. Probably our experience with the old great group of Planosols had something to do with this, because the Planosols with clayey argillic horizons, or claypans have that abrupt boundary, where the climate is udic, marginal to ustic. Where the climate is udic, then the abrupt boundary becomes very tongued and ceases to exist as an abrupt boundary. Now, we made an assumption that this abrupt boundary was an indication of age. It took time to develop. This assumption may not have been too valid. Recent studies of clay destruction in the presence of an intermittent groundwater table would suggest that we had the wrong basic assumption about the development of the abrupt boundaries on some of these soils. In the Ultisols, we had another group of correlators than we had with Alfisols.

That was the intent, to use "pale" for soils with considerable age, and with overly developed or over-thickened horizons of one sort or another. It was not the intent to get a soil of a "pale" great group in Holocene deposits, although we have run into situations where that's what happened. We had a student at the University of Ghent on a doctoral thesis last year. He was working with Holocene deposits where there was an argillic horizon, and where the underlying sediments were fine-textured so that there was no decrease in the percentage of clay with depth. We originally introduced the limit of weatherable minerals with the idea that you would find weatherable minerals in Holocene deposits. **Question 20, Texas**

5.6 Differentia, Low-Activity Clay

The original proposal to recognize the fine-textured subsurface horizon as a basis for placing a soil in a Paleudalf or a Paleudult was the difficulty of getting agreement amongst different pedologists as to whether or not there was an argillic horizon. The proposal was to put into the definition, then, of Alfisols and Ultisols this distinction in texture with depth, as being the equivalent of an argillic horizon, so that no decision would be needed as to whether or not there was an argillic horizon in a particular soil. This reason is one that was suggested it should not be recognized as a diagnostic horizon, but as a diagnostic feature, perhaps, but certainly not a diagnostic horizon. So that a soil might have an argillic horizon and have this fine-textured subsurface horizon and no decision would be necessary then, as to whether or not that horizon was or was not an argillic horizon. This was only proposed for use in the low-activity clay soils and nowhere else in *Soil Taxonomy*. **Question 4, Witty & Guthrie**

Some people still probably very strongly feel that the separation of the Alfisols and Ultisols should have been based on charge characteristics, and they can justify this with good reasons. I think for purposes of the record, it would be helpful if you can state if this alternative was discussed during the development of *Soil Taxonomy*, and what were the arguments for using base saturation to make this split.

Surely there was not very much discussion of the use of charge characteristics, rather than base saturation. There was not a great deal known about charge characteristics. For example, extractable aluminum was almost never reported in the literature. At the time the *Seventh Approximation* was written you could not find any data. You could not consider then, how the use of other things than base saturation was going to affect your classification. You knew what soils you wanted to keep together but you did not know what the use of charge characteristics would do to your groupings. It was not really considered until we had the International Committee on the Classification of Soils with Low-Activity Clays. It has been discussed at length in that committee and I think they are retaining base saturation rather than the low-activity clays for the distinction between Alfisols and Ultisols. They are raising charge characteristics to a higher categoric level in their recommendations but not to the order level. **Question 88, Cornell**

On soils of low-activity clay, what is your opinion? Some I have seen seem to be a lot more like other Oxisols than they are to the concept of Alfisols as I envision the concept of Alfisols from the midwestern U.S.

[Low-activity clay soils] are not like those (Oxisols), in the U.S. they are more like the central concept of the Paleudalfs. But still composition-wise they would be Paleudalfs, probably have a lot of kaolinite, but still aren't oxidized as much. They don't have as many of the oxidic minerals as some of these in question.

We don't have too many Paleudalfs in the U.S. to judge by. Soils in the valleys and south from Pennsylvania range from Alfisols in Pennsylvania to Ultisols in Alabama. There are a lot of them, and certainly they are very red. Now many of them are very dark red and have acidic mineralogy rather than kaolinitic. They have no ideal place for sure. They are very thick with very fine texture. Those in Africa are derived from more acidic rocks and much more quartz sand in the limestone valleys. You get soils from limestone there and they will be very similar soils. There is not much limestone in Africa. **Question 19, Minnesota**

The older soils of the intertropical regions in Africa are dominantly Alfisols, if you have a very distinct dry season. In the absence of a dry season, they are dominantly Ultisols. Now the morphology of these, as such, is very similar between the Paleudults and the Paleudalfs. But they have this other property, that of the moisture regime, which seems to correlate very well with the base saturation in the studies that I have been told about in Africa. And they may still be Ultisols if the moisture regime is udic. There is still quite a bit to learn about South American soils. The committee on the classification of soils with low-activity clays have been wrestling with this problem. They have a proposal that we should establish an order of soils with low-activity clays. But the committee generally has been in favor of retaining these soils as Alfisols and Ultisols, although they may remove them from Mollisols before they finish. **Question 188, Minnesota**

This particular [low-activity clay] problem is a much more extensive problem perhaps than we realize. It is not only very common in South America amongst the Ultisols but the identical problem exists in Africa amongst the Alfisols. You asked my opinion and I can say only this, that we have recognized this problem for a number of years. We have now two international committees working on a solution to the problems. The Agency for International Development has become interested in the use of *Soil Taxonomy* as a tool for transfer of experience between developing countries to increase food production, one of the main problems that they face in these countries. They have contracted now with the Soil Conservation Service of the U. S. Department of Agriculture to furnish financial assistance to pedologists, from any country, who are concerned with the problems of improving the definitions and the classification that is proposed by *Soil Taxonomy*. There have been six of these committees established so far and AID provides funds through SCS and through the University of Puerto Rico for the members of these committees to meet once a year in a country where the particular problems that they are concerned with exist. This particular problem (low-activity clays) was the one faced by the first of these international committees, under the chairmanship of Professor Frank Moormann of the University of Utrecht in Holland. Field study is important because, as yet, there is still considerable differences of opinion amongst pedologists about the meanings of various technical words, and the committee members cannot be sure they understand each other unless they can examine a number of the same profiles in the field together and discuss between themselves, in person, about the impressions that they get from these particular soils. My opinion is of very little importance in this, and it is a difficult problem and needs the international consideration and debate that it has been getting. **Question 1, Venezuela**

5.7 Aqualfs

5.7.1 Tropaqualfs

Why are the criteria used to distinguish Tropaquults not used to distinguish Tropaquepts or Tropaqualfs?

The question is not quite properly phrased. In the Paleaquults and Tropaquults, the requirement for color is only that the hue be 2.5Y or 5Y accompanied by mottles due to segregation of iron, or, if the hue is 10YR or redder, then the low chromas are required. Working in Venezuela, I examined the evidences of wetness for aquic great groups and suborders, and made the proposal that the definition used for Ultisols be extended in all orders to the intertropical regions - namely, the Inceptisols, the Mollisols, the Oxisols, the Entisols, etc. In Venezuela if they were wet, the wetness was commonly marked by the yellow hues accompanied by mottles. The criteria used for the Ultisols might have been applied more generally in *Soil Taxonomy* had we had a few examples of other kinds of intertropical soils. **Question 20, Leamy**

5.7.2 Albaqualfs

In the Albaqualfs the intent was to keep the old Clay-Pan Planosols together.

[There is primarily a] geographic correlation between the occurrence of Albaqualfs and the dryness in the warm summer months. There is one from northern Missouri where the Albaqualfs are very extensive in the loess. Across Illinois and into Indiana, the Albaqualfs virtually disappear and are replaced by Glossaqualfs. The Missouri Albaqualfs are the famous Putnam series. In southern Illinois, the Cisne and Cowdon are considered representative Albaqualfs. They run on over into Kansas and Oklahoma, but I have never seen them in those states. The dryness is probably not essential to the development of the argillic horizon because the Glossaqualfs have argillic horizons also. They don't have that abrupt boundary that occurs in the Albolls and the Albaqualfs. There was no good genetic theory to explain this at the time that we were working on *Soil Taxonomy*. In recent years the process of ferrolysis has been worked out to a considerable extent. Most of these soils have groundwater perched on the argillic horizon at some season of the year. That is one condition that seems essential for ferrolysis, which is basically destruction of the clay under anaerobic conditions. In the FAO UNESCO legend, the statement appears, "in these soils the clay has been destroyed in the A horizon". That is a serious overstatement because there may have been some destruction of clay, but there also has been translocation of clay into the argillic horizon. It may be a combination of the two. This is a field in which there is still a great deal to be learned. Along about 1934 in the old soil survey association proceedings, Roger Bray presented a series of papers on the genesis of the B horizon, it was then called, now the argillic horizon, in these soils. He worked out a series of calculations about clay formation in place and translocation, and explained the difference between the A and B horizon of the Albaqualfs basically on translocation rather than destruction. Clay difference could be due in part to both processes. We can't in any way at the moment quantify how much is due to one and how much to the other.

We have some series that are neither Albolls nor Albaqualfs that have this abrupt boundary between the epipedon and the argillic horizon. There is no albic horizon in between. These are still drier than the Albaqualfs and the Albolls. Probably the albic horizon is not there because they are not saturated for long enough periods to destroy any clays. Yet there they are, a fact. And the abrupt boundaries are genetically a bit of a problem. **Question 126, Texas**

Many argillic horizons have more carbon than the horizons above, and so the disclaimer on the irregular decrease of organic matter is going to throw out all your Albaqualfs, because the argillic horizon normally has more organic carbon than the overlying albic horizon. Such a change would result in some very complicated definitions that are extremely difficult to understand. **Question 5b, Witty & Guthrie**

5.7.3 Vermaqualfs

I would see no objection to your making such a proposal (of recognizing a great group of Vermaqualfs for Aqualfs with crayfish activity in the upper part of the argillic horizon). The vermic great groups were recognized because their horizons were commonly next to a meter or

more. Certainly a crayfish can do as much or more than the earthworm. He makes bigger holes, brings more materials to the surface -- much larger particle sizes. You'll find small gravel in the casts of the crayfish but not in the earthworm's casts. **Question 42b, Texas**

5.8 Aquic Subgroups

Why did the depths to 2 chroma mottles for aquic subgroups vary from within one meter for Argiustolls to within 75 cm for Haplustalfs, to within 75 cm and the upper 12.5 cm of the argillic for Haplustults?

This is another question that I cannot answer because these subgroup definitions were developed in work-planning conferences that I could not always attend. If I did attend one I could only sit in the discussions of one committee. I simply do not know the answer. If it seems irrational and irrelevant to interpretations then changes should be proposed. I think that we must not tie our hands by trying to be completely consistent at this moment. Our only consistence is that we want to get the taxa about which we can make the most important statements, and the greatest number of them.

I should point out that when you are dealing with Udalfs and/or Udults, the shallow water table can be an impediment to use. When you are dealing with Ustalfs and Ustolls, the shallow groundwater may be a benefit. In northwestern Iowa where we have a relatively thin mantle of loess over a fine-textured till, the groundwater perches above the till. Crop yields are better because of it, because the soils then retain and can supply more water. These are considered Udolls at the moment but they are getting marginal to the Ustalfs, and I don't have much personal experience with the Ustalfs. **Question 137, Texas**

At the subgroup level, where your aquic properties come in, one criterion for Alfisols is the upper 25 cm of the argillic, whether it is mottle-free or not. The other criteria in Ultisols is about 50 cm mottle-free or not. In the same landscape it starts to get fairly confusing that we use different depths. First of all, you do not know where your argillic is going to start. Then it starts at different depths and then once you have it started you go to actually different depths within the argillic. This seems to create some confusion; why not consider a more standard depth for considering mottling?

This reflects the thinking in different groups of states. The southern states had one opinion and we used their opinion for Ultisols, and the northern states had another opinion and we used their opinion for Alfisols. If you get into trouble about it, I can only suggest that you ask that this be reexamined. **Question 86, Cornell**

5.9 Differentiae, Temperature and Moisture Regimes

5.9.1 Xeralfs vs. Boralfs

At the time *Soil Taxonomy* was being developed, we had very little information about the soils that have a xeric moisture regime and a frigid temperature regime. We gave priority to temperature over moisture where the soils were cold enough that the temperature was a limiting factor. We thought that it was simpler to change the soil moisture through irrigation in dry soils than it was to change the soil temperature. We know of no way that the temperature can be altered appreciably. Therefore, the soils that were frigid or cryic were grouped into Boralfs. The Xeralfs that were frigid were left as Xeralfs because we actually have no knowledge of their use and management. This may have been a mistake, and it may well be that the definition of Boralfs should include soils that have winter precipitation, but that are cold enough that temperature is more significant than lack of rain in the summer. The soils in

question now appear to have winter precipitation, but do not become dry throughout in the summer for a long enough period to be xeric. **Question 12, Witty & Guthrie**

5.9.2 Xeralfs and Ustalfs

[One of the criteria used to distinguish the Xeralfs from the Aridisols, when the Xeralf has an aridic soil moisture regime bordering on xeric, is that the epipedon is both massive and hard or very hard when dry.]

This criterion came from the experience of looking at the Noncalcic Brown soils in California and comparable soils in South Australia, mostly cultivated soils. Nobody really ever showed me a virgin soil, I think, in this environment. In South Australia the soil with a hard, massive epipedon was called a hard-setting stage and is comparable to the cultivated Xeralfs in the U.S. They disappear over a distance of only three or four miles. We went into more arid climates and there we found soils with argillic horizons, they had a very soft epipedon. It seemed to work on the basis of the soils that they showed me in Australia and in southern California.

Ustalfs can do the same thing; they do in Venezuela, at least. As you go from the Ustalf or the Ustult to the Aridisol, the epipedon is first hard, massive and then soft. Experience generally can be utilized as a field criteria where you are just on the margins between ustic or xeric on one hand and aridic on the other. The intent was that it would avoid the necessity of forming judgements about which side of that boundary you were on. Focusing attention on it then causes people to make more observations. If I'd left it out, it wouldn't have been the subject of any studies whatever. Even though it is aridic.

We did the same thing between the Aridisols and the Mollisols. We said that if you had a mollic epipedon, a Mollisol could have an aridic moisture regime. And in the marginal area between the ustic and udic moisture regimes we tried to use presence or absence of soft, powdery lime in the profile to put the soil in the Udalfs or Ustalfs. This was all done to avoid the necessity of actually determining the moisture regime. Now, certainly the presence or absence of soft, powdery lime is not a good marker between Udalfs and Ustalfs in non-calcareous parent materials, especially in regions where there is very little calcareous dust in the air. I suspect that several or most of these attempts are going to prove impractical once we've focused attention on them by putting them into *Taxonomy* and we may have to modify them. It's going to make it more difficult to map. **Question 145, Minnesota**

5.9.3 Secondary Carbonates

There is no question but that the present definitions which stress the presence or absence of secondary carbonates to define moisture regimes are not going to be applicable to the soils of Venezuela. When I was working in Venezuela, I made a proposal on the subdivision of the soils with ustic moisture regimes, with or without regard to the presence or absence of carbonates. Certainly, the fact that the moisture regime is marginal to udic is much more important than the presence or absence of secondary carbonates. I proposed that we have subgroups of the ustic great groups in which we would have a central concept that would be used for typic subgroups, an udic subgroup and an aridic subgroup based on the length of the period in terms of consecutive days when the moisture control section was partly dry or wholly dry. This was a rather drastic change in the concept and really requires an additional soil moisture regime to distinguish the type of ustic regime that we have in Venezuela from the type of ustic regime we have in the United States. I made it as a proposal to be discussed, not one that was ready for adoption. The committee, ICCOMORT, under Professor Van Wambeke is considering this suggestion, I might say, rather than "recommendation" and they will eventually submit a report and recommendations on this. The other committee on the classification of Alfisols and Ultisols with low-activity clays is probably going to propose some new great groups amongst the Alfisols and Ultisols, the Kandiustalfs and Kandiustults. Now if they have such a great group, then the

nomenclature of Udic Kandiustalfs, Typic Kandiustalfs, Aridic Kandiustalfs will be greatly simplified. This seems to be about what they are proposing for the Ustalfs. The Kandiustalfs would have the clay activity less than 24 milliequivalents. The Kandiustults would have less than 16 milliequivalents. This committee, having worked for about 7 years, is about to submit its final report in June of this year, and I anticipate that their recommendations will be adopted. If they are adopted, then the use of carbonates to distinguish udic, ustic and aridic subgroups and ustic great groups will disappear completely. It has certainly little validity even in the United States. We have udic, ustic, and aridic subgroups of Ustalfs all in the same neighborhood and all have the same potentials for production of plants. **Question 54, Venezuela**

5.10 Eutric Great Groups

Why do the criteria for Eutroboralfs require greater than 60% base saturation in *all* subhorizons of the argillic horizon, whereas those for Eutrochrepts require 60% base saturation in only *some* subhorizons between depths of 25 and 75 cm?

Eutrochrepts: the limited data we had for soils in Pennsylvania, New York, and many with fluventic subgroups, indicated that the base saturation hovered somewhere around 50% in some subhorizon. We did not want to make a lot of separation in this great group, as it would have no practical value. So we set the limit at a point which will keep them all together. We started at 50%, but later we had to raise it as this was the medial value for most of these soils.

The Eutroboralfs were intended to group the Boralfs of the drier range. It is not only the base saturation limit but also moisture. So a Eutroboralf cannot occur in an udic area. In the more humid range of Boralfs, in Michigan, one might, find base saturation exceeding 60% locally as under a tree. We wanted to keep these humid ones together and so the definition was written to do that.

This is a general principal. When you find these apparently anomalous differences, the reason was that somewhere in the U.S. there was a soil series that would get split badly if we wrote the definition in another way. To split the series would have added nothing to the interpretations we could make about the phases of the series. If we obtained no improvement, we preferred not to split the series. **Question 117c, Cornell**

5.11 Fragic Subgroups

In the classification system, we have the fragic subgroup in the Ultisols but not in the Alfisols. Is there a reason why you went this direction when you developed the system?

It's only that, when we provided the subgroups in *Soil Taxonomy*, we listed only the ones for which we had series in the U.S. Now it's my judgement that such soils exist in the Alfisols but the correlation staff, the state representatives, did not suggest anything along this line for Alfisols. I'm sure they exist in Belgium but it was not our principle to include subgroups for other countries unless they requested them.

I tried once to get an experiment in Michigan on the effects of freezing on fragipans because, in my experience, the fragipan in nature never freezes, and I wondered what would happen in Michigan when the forest was cleared and we have a bare field lying there through the winter and frost would reach to depths of greater than the fragipan. I wondered what would happen to the fragipan. I never could get the study off the ground. I tried to get them to study it and our administrative people were not interested.

It could be documented to see if one had a fenceline with a fragipan under the forest and then see what we have in the cultivated fields. We have in Belgium in the loess in the cultivated fields the color pattern of the fragipan (with the polyhedrons with the mottled brown colors and

the gray fillings between the polyhedrons) but the roots go all the way through everything and this stops at the fenceline; under the forest is a fragipan. I think it would be good evidence the fragipan has been destroyed by something.[9] **Question 44, Minnesota**

5.12 Arenic and Grossarenic Subgroups

5.12.1 Buried Soil or Not

It seems that some of the grossarenic subgroups, for example the Grossarenic Paleustalfs as currently identified by some, consist in part by buried soils. The basic decision in classification is whether thick sandy sediments constitute an epipedon that developed with the underlying argillic horizon, or are younger materials that have buried the argillic horizon.

This problem [of identifying the presence of buried soils as in Grossarenic Paleustalfs] exists also in the southeast, where we have arenic and grossarenic Ultisols. In some of them, the break is very obvious in the particle-size distribution of the sand fraction between the epipedon, and the argillic horizon and is a very obvious lithologic discontinuity. It would be my feeling that the subsoil should not be in the arenic or grossarenic group. But it also happens in other places that there is no discontinuity, as in the coastal plains geomorphology studies. A doctoral thesis of Erling Gamble examined the sand-size distribution in the Arenic and the Grossarenic Paleudults. While he found there was a very great variability in different parts of his thesis area of Johnston County, North Carolina, some were much coarser sand, or much finer sands than others. Still, the sand distribution in the A and the B horizons, in every instance, was the same. It seemed impossible to figure out how, then, one could get a mantle of sand deposited over this county area in which the recent sand always had the same size distribution as the underlying material. I think these are good evidences that they are legitimate Arenic and Grossarenic Paleudults. I realize that, even though we have tried to lay down rules for correlation, that there are or have been differences of opinion between the regional staffs on this particular problem, especially in Florida. Where I have looked at the soils, and I find a fine sand that is 1.5 m thick that overlies a sandy clay loam in which the sand is rather coarse, to me, this is buried soil underlying the recent sand. But just what the correlators have done with these I couldn't say. I know it has been discussed in Washington D.C., what we could do about it, but we don't have the answer in Washington, D.C. for every problem that comes to us. **Question 19, Texas**

5.12.2 Aquic Arenic Subgroups

Why were the aquic arenic subgroups excluded from Paleudalfs, and why was it felt that no aquic grossarenic subgroups were needed in Udalfs, Ustalfs, and Udults?

This is a question that I cannot answer. In theory the bulk of these Arenic and Grossarenic Paleudalfs and Paleudults are in the region of the southern states. We are not dealing with two different groups of people we are dealing with the same group. It is one thing with the Paleudults in Florida and another thing with the Paleudalfs in Texas. Their recommendations were accepted. I was not in on their discussions at the work-planning conferences.

Certainly, yes, [one could propose an addition to] Paleudalfs. In the Alfisols this is an implied subgroup in that these definitions for the Aquic Paleudalfs exclude the arenic subgroups and the definition for the arenic subgroups does not mention the aquic properties. It is an

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9. Summarized in Soil Sci. Soc. of Am. Proc. v. 24, No. 5, pp 396-407, 1960.

implied subgroup. If an examination of your interpretations suggest that you need that subgroup then it should be proposed. If your examination of your interpretations suggests that you make the same interpretations for the Arenic Paleudalfs, let us say, that also meet the restrictions on the aquic subgroup, then you should propose a modification of the definition of the arenic subgroup. Bear in mind that the only subgroups listed here are those that appeared in the printout of the classification of the soils of the United States. Many other implied subgroups exist throughout the taxonomy but are not spelled out simply because we had no series that had been so classified.

The limits were proposed by the regional groups based on their experience with the significance of the depth to the gray mottles. In general, the sandier the soil the less importance one is inclined to put on the gray mottles. Particularly in thermic soils, the importance of the depth to the gray mottles decreases because you have a long growing season. If the soil is inclined to be a little wet in the winter, it is not so important as it is in the frigid and mesic soils where your growing seasons are shorter and the delay in planting due to wetness may be very critical. **Question 136, Texas**

5.13 Glossudalfs - Alfisols vs. Ultisols

Here is a question that pertains to the Ultisols, if you will just look at page 349, Chapter 16, it is item 1 under the definition of Ultisols. It says they are mineral soils that "1. Do not have tongues of albic materials in the argillic horizon that have vertical dimensions of as much as 50 cm if there is greater than 10% weatherable minerals in the 20- to 200- micron fraction." The question on that is: 1) why we have to discuss tonguing, vis-a-vis, the Alfisols vs. the Ultisols, and 2) why that is tied into percent weatherable minerals?

This is intended to keep out of Ultisols the Glossudalfs that have base saturation slightly under the limit between Alfisols and Ultisols. We wanted to keep all the Glossudalfs together. So far as we know they were all formed in Holocene materials mostly in loess. I have seen a few in solifluction materials. They just straddle the limits between Ultisols and Alfisols in terms of base saturation. The weatherable minerals were in there because, as I say, they mostly are in loess but they are in very late Pleistocene materials. We have Ultisols that have tonguing of albic materials that are very strongly weathered in soils where the B horizon apparently has formed and then undergone serious destruction and reformed another argillic horizon at a greater depth. These are mostly classified I think as Paleudults in the U.S. This was the definition that was suggested by those from Belgium to keep their Glossudalfs out of Ultisols to avoid splitting them between Alfisols and Ultisols.

This is an actual problem in the lower Mississippi Valley, I think, that we have these Glossudalfs there, they have only been reported to me, I don't remember seeing them. I have seen them in Oregon where they are again in loess.

There is one way to try to simplify the definition and that is to delete the first statement in the definition because there are so few of these in the world. **Question 99, Texas**

Well, it is possible to simplify the definitions [of *Soil Taxonomy*] enormously if we're willing to forget about, say, 1% of our soils, maybe less than 1%. The greater part of the complicated part of the definitions are due to the presence somewhere of a group of soils that belong together. They're very similar in all their properties, but they overlap one of the limits at a higher category. I've used a number of times the Glossudalfs as an example. These soils have a rather narrow range of base saturation at the limit between Alfisols and Ultisols. They straddle that limit, but they never get far from it. And they have so many similar properties that they needed, we thought, to be kept together. When writing the definition then to permit Alfisols to have a base saturation of less than 35%, we introduced a serious complication into the definition of Alfisols and of Ultisols, both orders. One should say to the students that these definitions are written for people who are actually classifying soils for the Soil Survey. For the people who use the map, the use of *Taxonomy* for other purposes than these complicated

definitions is unnecessary. And I think it can be done, too. The definitions can be greatly simplified by footnoting to a definition the presence of some exceptions. At one time I had thought to do this myself. I still may do it but this current book we are talking about seems to have a higher priority. And I received considerable discouragement when I discussed this possibility with the Washington correlation staff. **Question 106, Minnesota**

5.14 Differentia, Natric Horizon

There are Haplustalfs with a high sodium content in the argillic horizon but not enough to meet the requirements for a natric horizon. Would it be interesting to show this characteristic at the subgroup level by creating a subgroup of Natric Haplustalfs?

This was done at one time in one of the various approximations or rather one of the supplements to the *Seventh Approximation*. The subgroup was eliminated on the grounds that it was very difficult to estimate the sodium saturation or the SAR in the field. There were not always adequate visible clues to the presence or absence of sodium. And when the interpretations were checked against the data that we had from the laboratory on the sodium saturation we could find no evidence in the U.S. at least, that a sodium saturation say of 10 or 12% was significant to the behavior of the soil. So we had two factors working against this natric subgroup: 1) the difficulty of its recognition in the field and 2) the similarity of interpretations for soils with and without the significant but smaller amounts of sodium. If there is evidence that suggest that the behavior of the soils in Venezuela with say 10 or 12% saturation with sodium is significantly different from the others, then a proposal should be made for a modification of the definition of Typic Haplustalfs.

We have a precedent for natric subgroups in the Alfisols in that there is a subgroup of Natric Haploxeralfs and the subgroup of Natric Palexeralfs. In these, the sodium is high, but high at considerable depth. In the definitions of these subgroups, the sodium exceeds 15% within one meter of the surface. Similar provisions could be inserted for the Ustalfs if it is felt to be important. If these soils are to be irrigated then, as the Xeralfs are commonly in California and in Spain and in North Africa and so on, the sodium becomes potentially important, because if too much water is applied, you will create a groundwater that will come up by capillary rise bringing the sodium up into the active rooting zone where it becomes an important factor in soil management. **Question 8, Venezuela**

5.15 Differentia, Plinthic Properties

Why is there no subgroup of Plinthic Tropaqualfs? There exists such soils as Tropaqualfs with plinthite and without plinthite in Venezuela that have geographic extent.

It must be remembered that the subgroups that are listed in *Soil Taxonomy* are those that were known to exist in the United States or that were specifically requested in other countries. So that the failures to list such a subgroup only means that no one asked for it and it was not known in the U.S. Had we had such a soil in the U.S., we surely would have created a plinthic subgroup of Tropaqualfs because it would have been consistent with the recognition of plinthite in other great groups and in other orders. **Question 12, Venezuela**

Chapter 6

ARIDISOLS

reviewed by K. Flach[10]

6.1 The Place of Aridisols in Soil Taxonomy

Well, I would like to quote Dr. Kellogg on why Aridisols is the only order defined by its soil moisture regime. One of the most important boundaries on soil maps is the limit between the sown and the unsown. The land that can be cultivated and the land that can only be grazed, and so it seemed to us that it would be useful to have an order that included the bulk of the soils that were too dry to be cultivated and that did have some horizons. **Question 94, Texas**

"Some people have criticized *Soil Taxonomy* for not using moisture regime consistently at the same categorical level. Why are soil moisture regimes not used consistently?" These are people who probably don't understand that *Taxonomy* has a purpose that's spelled out. They want a theoretical classification. To serve the functions of the soil survey, the taxonomy has to be usable as a key for correlation. Insistence on using a given characteristic only once in the taxonomy and in the same category in all soils would enormously multiply the number of categories and destroy the nomenclature completely. Such insistence would also destroy the usefulness of *Soil Taxonomy* for naming map units of small-scale maps where we tend to use the higher categories, generally the great groups or even suborders. If we were to use a given property, such as the moisture regime, in only one category for all soils, then we would reduce our choice of making a broad subdivision of soil climate for small-scale maps and a fine subdivision for large-scale maps. **Question 170, Minnesota**

The aridic soil moisture regime is used to define the Aridisols, Torrox and Torrerts, as well as torric subgroups of Entisols. The question was asked, "Why do we have Torrox instead of Oxids?" Which is more important, the oxic horizon or the aridic soil moisture regime? We may have made the wrong decision, but we decided that if a soil with an oxic horizon (and an aridic soil moisture regime) was irrigated, the oxic properties still remain limiting to use. Similarly with Torrerts, it was more important to recognize the shrink-swell potential than the soil moisture regime which, though a limitation, could be corrected. In the Entisols, we thought it was important to recognize at the suborder level the reason why the soil had no horizons. It was either losing material too rapidly through truncation or receiving additions too rapidly for horizons to form. Having used that particular set of characteristics to define the suborder, we brought the moisture regime to a lower level. If we tried to use soil moisture regimes all in a single category, we would have too many categories. **Question 112, Cornell**

The decision to keep Entisols out of Aridisols can in part be traced back to our experience with the 1938 classification where soils without horizons were grouped as Azonal soils in one order. That was the only order that was based on a soil property. This is probably an inheritance from the previous classifications; most of them made a distinction between soils with

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10. Special Assistant for Science and Technology, SCS/USDA, Washington, D.C. 20013.

and without genetic horizons. I can not recall any serious criticism of the idea of allowing the Entisols to have an aridic moisture regime in the arid landscapes. You have soils with and without horizons, just as you do in other landscapes. These were separated in other landscapes and we probably simply carried it on over into the arid regions. So we had the Aridisols which were considered to be soils of arid regions with genetic horizons and the Entisols which were truly Azonal. They could have any moisture regime as long as they had no horizons. It's more difficult to explain why we had the Torrerts instead of putting them into a vertic great group of Aridisols. Actually, their horizonation is extremely weak, but they do have the potential shrink-swell and cracks of the other Vertisols. One may question the logic of all this, but *Soil Taxonomy* evolved slowly and some of the ideas from some of the earlier approximations carried over, presumably because no one criticized them. **Question 170, Minnesota**

6.1.1 Other Differentiae for Aridisols: Structure of the Epipedon and Electrical Conductivity

We observed in the United States, in Australia, and in Venezuela that the soils with argillic horizons had a hard and massive epipedon where the moisture regime was ustic and a granular and soft epipedon where the regime was aridic. In field work, the man making the map is much more easily able to determine the structure and consistence of the epipedon than the moisture regime. So we tried in a number of places to supplement the distinction between the moisture regimes with readily observable field properties and restricted the Aridisols to soils that have a structured or soft epipedon. This was not entirely successful. The Australians have reported to me verbally situations where their Paleargids do not have a soft-structured epipedon. There's probably considerable need for reexamining this criterion. At this time, I have no suggestion as to what modification in the definitions might be needed, but it seems clear from the verbal reports that I get that some modification is required in the definitions of the Alfisols, Ultisols, and Aridisols. **Question 46, Venezuela**

The limit of 2 mmhos conductivity was introduced in an effort to provide a field criterion for distinguishing the Orthids from the Inceptisols. It has not worked, whether we use two millimhos or four millimhos. The use of conductivity to make this distinction breaks down whenever the soil is irrigated. There are large areas in the Middle East, in the Rio Grande Valley, in Texas and in southern California where Inceptisols are irrigated. The conductivity may become quite high and I proposed when I was in Venezuela that we drop all reference to conductivity to distinguish between Inceptisols and Aridisols. *Taxonomy* has avoided the use of conductivity everywhere except this one place. Elsewhere, the salinity is used as a phase rather than as a taxonomic differentia. We have kept the use of salinity to the phase level and outside of the taxonomy deliberately for two reasons. One was the precedent in the mapping in the United States in which salinity was used as a phase for soil series, and if salinity were introduced as a taxonomic differentia, these series would have been split. Splitting series was generally considered a very serious thing. The other is that the salinity under irrigation is quite variable for several reasons. One is the quality of the irrigation water, one the length of time that has passed since the soil has gone through a leaching cycle, and one is the overuse of water so that soils become water-logged and the salts come up by capillary rise. Hence, the electrical conductivity of the irrigated soils is an extremely dynamic feature. If then, we introduce absolute limits on conductivity into the taxonomic classification, we have soils that will shift with each leaching cycle from one taxon to another. Or, where you have a seepage spot at the base of a hill, the wetter soil on the landscape becomes an Aridisol, if it doesn't have an argillic horizon. This seems to us to be irrational and this is why we have kept salinity out of the taxonomy itself except for this one place. But salinity is extremely important and must be used as a phase criterion. Our interpretations are always made for phases of taxa, not for the taxa. **Questions 47, Venezuela and 69, Texas**

6.2 Taxa of Aridisols

6.2.1 "Pale" Great Groups in Aridisols and Other Orders

It has been stated that "pale" great groups and subgroups have been used in several orders and at several categorical levels; their use in places seems to be inconsistent. The concept involved in the term "pale" at the great group level was proposed fairly late in the development of *Soil Taxonomy*. It came about as a result of studies of the geomorphology of the coastal plain soils in the southeastern United States and the Aridisols and the Mollisols of the arid and the semiarid land of the southwestern United States. The concept of landscape evolution that was held when I started working in soil science was one of lowering of the land surface on the interfluves. The replacement of this concept by the notion of linear retreat of the slopes came much later. It was pretty much assumed by pedologists of Europe and the northeastern United States that all soils were about the same age, and that the differences were due to other kinds of soil-forming factors. When we started the geomorphology studies, we found that the soils in any of these landscapes which were not covered by the glaciers was quite variable. Some of the soils were very early Pleistocene or Pliocene in age, and others were Holocene. We began to look at the differences in these soils with such greatly varying age. Obviously, if one goes back to Pliocene or even early Pleistocene, there have been a number of differing climates under which these soils developed. In the southeastern states, the Ultisols on the older surfaces, dated by Dr. Daniels and associates (Daniels et al.) at well over a million years, are very similar in chemical properties to many Oxisols. They have very thick argillic horizons and their mineralogy is a mixture of quartz, kaolin, and free oxides. On late Pleistocene or even early Holocene surfaces in the coastal plain, we found soils with completely other suites of mineralogy. There were many feldspars in the silt and sand fractions and montmorillonite and illite, in place of kaolinite in the clay fractions. The activity of the clays were much higher than in the soils of very old surfaces.

We tried to define the Paleudults in terms of measurable properties, not in terms of age. In order to distinguish them from the Hapludults, we required the Paleudults to have a very thick argillic horizon and very few weatherable minerals in the sand and silt fractions. **Question 44, Cornell**

Among the Aridisols and Ustolls, we found that the Holocene soils did not have appreciable areas with petrocalcic horizons; and they never had thick argillic horizons. On the older surfaces in the western states, we normally had a petrocalcic horizon which was a barrier to movement of water and roots. So the "pale" concept of the Argids included two kinds of soil, one with a very thick and clayey argillic horizon and an abrupt boundary between the argillic horizon and the overlying horizon and one with a petrocalcic horizon at shallow depth. We showed that there were enough carbonates in the dust and rain to form petrocalcic horizons even in sediments that had virtually no calcium to begin with. In the glaciated parts of the U.S., these "pale" great groups do not exist. This is where soil science began--in the Soviet Union, in western Europe, and the northern United States.

6.2.2 The Petrocalcic Horizon: Used as Differentia and for Naming Taxa in Several Categories

In a sense, the petrocalcic horizon is a part of the definition of the great group even in Paleargids where we distinguish Petrocalcic Paleargids at the subgroup level. The petrocalcic horizon is one criterion for classifying the soil in the "pale" great group which consists of two kinds of soils: one with more than 35 percent clay somewhere in the argillic horizon and an abrupt textural change between the A and the B, and another one with a petrocalcic horizon. Therefore, since we were grouping these soils with and without petrocalcic horizons in a single great group, we had to separate them at the subgroup level rather than the great group level. We avoided at least one additional great group in the Argids. The Paleorthids are defined in

terms of having a petrocalcic horizon, although the name petrocalcic does not appear as a formative element in the name of the great group.

It would, of course, be perfectly possible to define the Argids having a petrocalcic horizon as a separate great group from those that do not have one. At the time that we were writing *Soil Taxonomy*, this possibility either did not occur to us, or we were trying to be economical in the numbers of great groups that we established. **Question 8, Witty & Guthrie**

6.2.3 Ustollic and Xerollic Subgroups: Their Purpose and the Usefulness of Organic Carbon as Differentia

We wanted to get those soils whose lack of moisture was extreme, into the typic Aridisols to make the distinction between Aridisols that may have virtually no organic carbon, particularly in North Africa in the margins of the Sahara, where the rains come once in a hundred years or so, if ever, and the Aridisols, such as in eastern New Mexico and southwest Texas, where there is more rain and more production of grass but not enough to produce a mollic epipedon. We thought these were not typic Aridisols. In Ustollic Aridisols there is reasonable summer rain and a flush of ephemeral grasses if the soil is not too badly eroded. At least they developed with a grass vegetation, but that evidence may now be missing because of soil blowing. **Questions 82, Minnesota and 24, Texas**

The validity of the organic matter criterion is probably not very great. We recognize, for example, that in strongly calcareous materials there is preservation of organic carbon. Such soils tend to contain more organic matter than one would expect from their climatic environment. At one of our meetings we asked the correlators on the Great Plains to work out a definition. This was done by Arvad Cline and some associates. They were not happy with their definition when they gave it to me, but they said this is the best we can do with our present knowledge. **Question 24, Texas**

6.2.4 Salorthids and the Salic Horizon

The salic horizon is defined more or less on the salt content rather than on the genesis. The one great group, the Salorthids, for which the salic horizon is diagnostic is a group of soils in which there is relatively shallow salty groundwater, and the salts accumulate at the surface of the soil from capillary rise and evaporation. Presence of groundwater at some time of the year is part of the definition of Salorthids. The photograph of the Salorthids in *Soil Taxonomy*, plate 5D page 101, is of a soil that had groundwater at one time, but stream entrenchment has lowered the water table so that it no longer is shallow enough to strictly meet the requirements in *Soil Taxonomy*. Nevertheless, it seems best to consider this soil as a Salorthid because the genesis was the same, that of capillary rise and evaporation. There are other kinds of salic horizons in the most arid regions of the world, Peru would be an example, where the salt content is adequate for a salic horizon, but it is not at the surface. It is a subsurface horizon, and has been formed by the leaching from the occasional rain that they get on the Peruvian Coastal Plains. The salts there may accumulate to the extent that the salic horizon becomes indurated and may be considered a petrosalic horizon. These horizons have not been considered diagnostic of anything, in the past. It was the feeling of our correlation staff, since these didn't exist in the United States, that they wouldn't worry about them. When *Taxonomy* use is extended to other countries, however, this will become a problem that will need to be considered. **Question 140, Texas**

There was discussion about what to do with some of the salt flats in Utah where the salt crust that has formed is thicker than the soil. How these were to be classified was discussed but no agreement was reached. At the time that we were developing *Soil Taxonomy* there were no series for the salt flats, they were mapped as miscellaneous land types, and identified as salt flats. There are plants growing on these salt flats, so they come within our definition of soil. These salt crusts were not formed by capillary rise from ground water or by occasional

leaching, but rather they are evaporites from former lakes and could be considered a parent material rather than a soil[11]. **Question 141, Texas**

6.2.5 Nomenclature of Aquollic Salorthids

"Inasmuch as Salorthids are defined as having a shallow water table, the designation "aquollic" seems to be redundant". I don't know precisely why they were called aquollic rather than mollic. The proposal for the subgroup came from the soils staff in the State of Texas for soils virtually at sea level and very close to the coast where the salt conceivably was coming from the Caribbean sea rather than from a salty aquifer. When the proposal was made, the southwestern people thought that because these soils did occur in a much more humid environment than the normal Salorthids, they needed to be distinguished. In the *Seventh Approximation* and the first supplements, no such subgroup was provided, but it was thought that for interpretations a distinction needed to be made. **Question 143, Texas**

6.3 Soils Without a Proper Home

6.3.1 Salorthids with Poor Drainage Not Qualifying as Aquollic Salorthids

"Aquollic Salorthids are defined in terms of their organic matter content. Salorthids, in some parts of Texas for example, show differences in chroma that seem to be related to differences in soil moisture regimes. Some are wet only short periods of time and have chromas of 3 or 4 whereas others are wet most of the year and have chromas of 2 or less. These soils, however, do not have the necessary organic carbon content to qualify as Aquollic Salorthids." There is certainly a potential place for such soils, but Salorthids are already defined as having ground water at some season. The low chromas of the wetter soils may or may not indicate differences in the wetness of the soil. My experience in the West Indies may be relevant. I was concerned with working out a better definition of Pellusterts and Chromusterts. On the Island of Jamaica, the highest chromas, I think I found, were in the wettest soil. It was not only wet but very salty and extremely low in organic carbon. I think the high chromas were simply the effect of the inability of reducing microorganisms to function under these conditions. In these salty soils, I would say we would need considerable discussion about the use of chroma as an indication of wetness. I don't know precisely what the effect of a very high conductivity would be on the iron-reducing microorganisms. Perhaps the soil microbiologists should be consulted on that before any decisions are made. But the definition might better be based on the depth to the water table instead of on the chroma. **Question 142, Texas**

6.3.2 Vermic taxa

The way the definition of the vermic groups is written, the disturbance is due to animals but not necessarily to worms. If we begin to find significant numbers of soils that have been disturbed by other kinds of animals, then we might consider changing the formative element in the name from one suggesting worm to something else. What it would be I would not know. We have a few soils in the U.S. where the disturbance has been due mostly to the prairie dog. I forget where I have seen these, I think Montana. But it was in the northwest somewhere where we have a loess over basalt and everything has been mixed by burrowing mammals down to the basalt. **Question 164, Minnesota**

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11. Reviewer: If they support plants they fall within the definition of soil.

In Venezuela, an Aridisol, for some reason, was sampled in an ant mound. This particular ant carries organic matter underground. The description mentions the presence of holes that are filled with organic materials to a considerable depth. The pH of that soil was about 3.6 and the conductivity of the saturation extract was somewhere around 12 to 15 mmhos. The anions and cations that we normally determine wouldn't balance, so I asked the laboratory to run nitrates. They found large amounts. Now that would be a significant difference, but there may have been only one or two pedons of that. When I went back and sampled a transect that I thought should cross the point where they had taken this sample, I couldn't find anything remotely resembling the nitrate contents. **Question 164, Minnesota**

6.3.3 Soils with More than 15 Percent Sodium Saturation but without a Natric Horizon

We do have them in Aridisols and Vertisols. I wouldn't expect too many in Inceptisols. They are recognized in one place in the taxonomy as Natric Camborthids, not for soils in the U.S. but at the request from pedologists in India. These were fairly heavy clays. Sodium saturation was 65, 75, 80 percent. They had very serious problems with them and they didn't feel that the series would be adequate to deal with this problem.[12] **Question 34, Texas**

6.3.4 Dry Polar Soils

At the time *Soil Taxonomy* was written, there was virtually no modern description of a dry polar soil in the literature. The definition of the aridic soil moisture regime is such that there is no provision made for a polar soil that has an aridic soil moisture regime and accumulated significant amounts of salt. A polar soil cannot have an aridic soil moisture regime. It never reaches a temperature of 5° C at 50 cm so that it cannot be dry more than half of zero time. The gap left between the definition of aridic, ustic and xeric soil moisture regimes was deliberate. We had no information about these soils that would have enabled us to develop that part of the taxonomy. Had we attempted to close this and other gaps so that there would be a place for every soil, we feared that the pedologist might attempt to classify the soil by simply applying the definitions in *Soil Taxonomy*. *It must be remembered that classification involves not only the application of the rules to see where the soil fits in Soil Taxonomy but equally importantly, it requires that the classifier determines whether this placement is appropriate.* Many of the limits in *Soil Taxonomy* were selected to group the soils of the U.S. into classes that had some real meaning, to put together the objects that belong together. How does the classifier decide what things, do or do not belong together? The classification problem is not too difficult; he has the rule that the things that belong together have common properties and common behavior characteristics. The classification of the polar soils is going to be determined by this general principle. We left this question hanging so that those who have studied the soils can propose a reasonable classification. **Question 3, Leamy**

6.4 The Genesis of Some Aridisols

6.4.1 Aridisols Having Argillic Horizons at Greater Depth than can be Explained by the Present Climate

My experience with Aridisols is quite limited. I cannot be sure of any explanation at the moment. It is possible that one can have a genetic sequence of horizons of a thick sandy epipedon overlying an argillic horizon, and an aridic moisture regime, if the soils formed under

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12. Reviewer: ESP is also used to define Halaquepts.

a higher rainfall than they have today. They would not necessarily qualify as Paleargids because there might be no petrocalcic horizon or an argillic horizon containing more than 35 percent clay. Or they may be polygenetic soils. **Question 8, Witty & Guthrie**

6.4.2 Very Acid Aridisols

We have such soils in Venezuela and in a few places in Wyoming. They are not at all uncommon where the Ultisols grade up against the Aridisols. The rainfall is just marginal between ustic and aridic and the base saturation is very low. The explanation, of course, is hypothetical but we do notice that the rainfall pattern, the soil-moistening pattern, is very different from that in temperate regions. In these intertropical regions we have no summer and no winter, but we have a rainy season and a dry season. In the rainy season, 400 mm of rain may fall within three months, which is enough to moisten the soil and send a little on down below the limit of where we set the base saturation determination. We even have Venezuela Aridisols with pH's of about 3.6 in water. How those developed is purely speculative at the moment. The acid Aridisols in Wyoming developed in materials that contain a fair amount of pyrite. In the soils I looked at in Venezuela, I was unable to find any pyrite in the rocks below the soil, but the soil contained 6 milliequivalents of aluminum in the saturation extract which had an electrical conductivity of about 15 or 16 mmhos. These soils, obviously, are not leached. Where the aluminum came from, I am not sure. It may be the rainfall pattern that basically produces leaching when you have a rainy season and a dry season. But then there shouldn't be the high electrical conductivity. In most of Africa there are Alfisols rather than Ultisols in this situation. In most of South America we find Ultisols in these wet/dry climates if the soils have any age. **Question 102, Texas**

We had one soil sampled in Venezuela, an Aridisol, where, for some reason, they sampled in an ant mound. This particular ant carries organic matter underground. The description mentions the presence of holes that are filled with organic materials to a considerable depth. The pH of that soil was about 3.6. I went to look at the soil to see what was going on, but I couldn't recover the exact site although I could get close to it. I couldn't find the ant mound that they had sampled. Conductivity of the saturation extract was somewhere around 12 to 15. When we looked at the anions and cations that we normally determine, they wouldn't balance, so I asked the laboratory to dig the sample out and run nitrates. It had large amounts of nitrates in the saturation extract of the soil in that mound. Now that would be a significant difference, I suspect, but you'd only have one or two pedons of that. But only by accident, did I find that this situation existed, because when I sampled a transect that I thought should cross the point where they had taken this sample, I couldn't find anything remotely resembling the nitrate contents. But 3.6 is not an uncommon pH for Aridisols in Venezuela. It's not necessarily due to the nitrates. It's due to the aluminum in the saturation extract. **Question 164, Minnesota**

6.5 Diagnostic Horizons and Features

6.5.1 Definition of the Calcic Horizon in Coarse-Textured Soils

"The requirement that the calcic horizon contain 15% or more $CaCO_3$ is waved for soils in sandy, sandy-skeletal, coarse loamy and loamy-skeletal families. This causes abrupt changes in classification in response to rather minor changes in texture." My memory is not too clear on this, but I think I can understand it. In sandy gravel there may be a very distinct accumulation of calcium carbonate before the 15% (by weight) limit is reached, but in the case of these particle sizes we require at least 5% by volume of the secondary carbonates. This would be consistent with the 5% limit of secondary carbonates in the calcic horizon by weight greater than the underlying material, which is roughly the same limit.

One must also look at the uses made of the calcic horizon. In North Dakota, the glacial till normally has more than 15% carbonate when it was laid down and so it is very easy to meet the requirements for a calcic horizon. Many of the tills there, are marginal in this respect and we pay no attention in some series definitions as to whether or not there is a calcic horizon[13]. **Question 58, Texas**

6.5.2 Calcite and Dolomite: Why are They not Weatherable Minerals?

This comes about from the soils of the arid parts of the U.S. where we may have a Paleargid, perhaps, that has now become recalcified. From the dust, from calcium in rainwater and so on, the interiors of the blocky peds will not effervesce while the exteriors are coated white with calcium carbonate. This is obviously a soil that has been decalcified at some point in the past, but in the present dusty and dry environment the carbonates are accumulating again. It just seemed that in the definition on weatherable minerals, we had better leave them out. **Question 44, Texas**

6.5.3 Ustic and Aridic Moisture Regimes in the Tropics

In the intertropical regions the season in which it rains is immaterial. Seasons cannot be defined in terms of temperature. In Venezuela, for example, there is no summer and winter, but areas with aridic moisture regimes have two short rainy seasons. There's been discussion of subdivisions of moisture regimes on the basis of one or two rainy seasons. In Aridisols, these are not severe rainy seasons, but the soils that have two rainy seasons can occur under very low or very high rainfall and in the latter the two rainy seasons are important. Such soils are much to be preferred to soils with only one rainy season because you have a relatively dry season during which you can harvest one crop and plant the second. In those parts of Venezuela, where they have only one rainy season, they are only able, at the moment, to grow one crop per year although the growing season is long enough for two crops. The maturing of the first crop comes at the height of the rainy season when they can't harvest it. They cannot plant the second crop except with hand labor. **Question 82, Minnesota**

We would eventually recognize the basic difference between an ustic moisture regime in intertropical and temperate regions. The current definition of the ustic moisture regime as applied in the U.S. puts a very different set of moisture conditions in with the wet/dry tropics. Here the growing season is controlled by temperature and moisture and maximum rainfall occurs during the growing season in summer and spring. In the intertropics or tropics there is no such control by temperature. **Question 102, Texas**

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13. Reviewer: A calcic horizon within or immediately underlying the mollic epipedon is diagnostic for Calcic and Calcic Pachic Cryoborolls.

Chapter 7

ENTISOLS

reviewed by N. Ahmad[14]

7.1 Historical Concepts

Concepts for the soils without diagnostic horizons came originally from the concept of the azonal soils. I suppose that this was a distinction that came from our experience with the 1938 classification where soils without horizons were grouped as azonal soils in one order. That was the only order that was based on a soil property - the Azonal Order. It probably came from the early experience with the European classifications where a coarse subdivision of soils was made on the basis of the horizon designations: soils with only a C horizon, those with AC horizons, those with ABC horizons. The first group of soils without genetic horizons was generally separated in the European classifications as well as the American. This is probably an inheritance from the previous classifications; most of them made this distinction of soils with and without genetic horizons.

I can not recall any serious criticism of the idea of allowing the Entisols to have an aridic moisture regime in the arid landscapes. You have soils with and without horizons, just as you do in other landscapes. These were separated in other landscapes and we probably simply carried it on over into the arid regions. So we had the Aridisols which were considered to be soils of arid regions with genetic horizons. And the Entisols were considered to be truly azonal. They could have any moisture regime as long as they had no horizons. They were soils without diagnostic horizons and we wanted to keep them together as an order because without any subsurface diagnostic horizons there are really no statements you can make about the Entisols except that they lack subsurface diagnostic horizons. The statement is not very important to the soil survey. **Question 170, Minnesota**

The arid climate was shown only at the great group level because in the Entisols we wanted first the suborder level to sort them out according to the reasons why they had no subsurface diagnostic horizon. For example, there is a big difference between the Orthents and the Fluvents, and agricultural importance. Perhaps more people in the world get their food from Fluvents than any other single kind of soil. **Question 64, Texas**

Another reason why Entisols have no horizons is because they are either losing material too rapidly through truncation or receiving additions too rapidly for horizons to form. Having used that particular set of characteristics to define the suborder, we brought the moisture regime in at a lower level. If we try to bring in these properties all into a single category, we have too many categories and we do not have the opportunity to reflect the major differences in the high categories for small-scale maps and the smaller differences in these properties for the large-scale maps.

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14. Head of Soil Science Dept., University of the West Indies, St. Augustine, Trinidad.

7.2 Abrupt Textural Change

7.2.1 Transport/Depositional Processes

The relative importance of abrupt textural change on soil genesis and classification due to transport/depositional processes of parent materials is controlled by the age of the processes. For older phenomena, the aim is to keep them out of the higher categories of *Soil Taxonomy* and to restrict them largely to the family level where the transport was so long ago that there are some genetic horizons on which to base the classification. In this case the definition of the argillic horizon takes into account, the potential increase in the percentage of clay due to a stratification of the parent materials.

Current deposition is taken into account at a higher categoric level in the Entisols, where Fluvents and Orthents are differentiated at a suborder level. It is not always easy to recognize in the field a small difference in sedimentation unless the sand grains are large enough to be detected with the fingers or the teeth; one cannot always detect them in the field. Confirmatory laboratory analyses are required in these instances. However, in so far as possible, the classification should be based on properties that can either be seen or felt in the field or that can be inferred from the combined knowledge of pedology and some other science such as botany, geomorphology, and climatology. **Question 17, Cornell**

7.2.2 Buried Soils

In rationalizing the general guidelines, given in the *Soil Survey Manual*, of a buried soil with the more specific definition of such a soil in *Soil Taxonomy*, the assumption is made in *Soil Taxonomy* that the buried soils were buried by a mantle of largely unaltered materials, since it is specified that it normally shows fine stratification; it would therefore be quite a recent deposit. Such occurrences can be found on flood plains, say, where a dike has burst, or near volcanoes where there is a mantle of very recent ash or pumice, or in areas where dunes are moving across the landscape. The presence of an argillic or a spodic horizon would seem to be clearly eliminated. The presence of a very weakly developed cambic horizon, of course, could be tolerated as a part of a recent mantle, because such a horizon can develop in places, given the proper environment, in something like a matter of a hundred years or so.

The definition of a buried soil in the *Soil Survey Manual* is really a statement that the man who is describing the soil makes, i.e. that the material at the surface is of another age than the underlying material, in which case the horizon in the underlying materials is indicated by the subscript little "b" in the horizon designation. The confirmation of this interpretation may later require laboratory analyses to validate. The intent was to include only those mantles so recent in origin that they have no diagnostic horizons other than an ochric epipedon, and many would hardly have that if they were finely stratified, so that normally there would be no epipedon. The definitions of Inceptisols and Entisols state that they have no argillic horizon unless it is a buried horizon. The thought was that the new material would be new enough and recent enough, that there would be no diagnostic horizon and that the buried soils would occur only amongst Entisols. **Question 7, Witty & Guthrie**

In extreme cases such as in the Central Llanos of Venezuela, where the soils have an aeolian mantle between 50 cm and one meter thick, lying on a buried soil which may be, for example, a Tropaqualf, this part of the profile would come within the definition of a buried soil; therefore, the classification would rest on this superficial mantle of sand. Since there are no horizons in this superficial mantle, these soils would go into the order of Entisols, and since the sand is less than one meter in thickness, they would be placed in the suborder of Orthents. The distinction here would primarily be at the family level where the particle-size class would be sandy over something else, the exact nature depending on the particle size of the buried soil.

It could not be considered a Psamment because the deposit is less than a meter thick and the sandy texture, therefore, does not extend to a depth of one meter.

The problem of using a "thapto" subgroup would depend on the importance of the nature of the buried soil. If one had a variety of soils that were buried, as for example, a Tropaqualf in one place and a Tropaquept in another and it was felt that the presence of that buried argillic horizon was critical to the use of the soil, then a thapto subgroup might be considered. In this case, it might be a Thapto Aqualfic Troporthent. The thapto would proceed the aqualfic, because that is the buried soil. This subgroup then, not having been recognized in *Soil Taxonomy*, would need to be proposed and a definition written that would include it and would exclude it from the Typic Ustorthents. This would also require modifications in the definitions of the Orthents.

One point of view is that soils such as those mentioned above could be keyed out as Fluvents because the organic carbon decreases irregularly with depth. It would, however, be incorrect to consider these soils Fluvents because they happen to be buried soils. If the text of *Soil Taxonomy* is vague on this point, then it does need to be clarified in the text that the buried soil in this situation would not make the other soil a Fluvent. There are similar problems in New Zealand and in the U.S. where there is a pyroclastic mantle resting on a buried soil. The mantle may be very recent and has no horizons but the buried soil below is high in carbon; this creates a situation where using the carbon of the buried soil puts Fluvents on the tops of the hills in New Zealand and Orthents on the slopes. Some changes are definitely needed in the text of *Soil Taxonomy* to clarify this situation. **Question 10, Venezuela**

7.3 Control Section

7.3.1 Psamments

There are two aspects to the depth criterion of the control section for sandy soils (Psamments). The definition of the Entisols prohibits a horizon such as an argillic horizon, unless it is a buried horizon and provided that its upper boundary is considered to be a soil without a diagnostic horizon and therefore falls into the Entisols. These soils with such thick epipedons are almost always sandy soils.

The control section for the Psamments that distinguishes them from other Entisols such as Orthents and Fluvents extends to one meter, but the control section for defining the order to which a soil belongs extends to two meters in the sandy soils. The limit of two meters was taken because the difficulty of making observations at depths greater than two meters in sand is enormous, and some limit must be set that will permit the mapper to determine in the field, without specialized drilling equipment, whether or not there is a diagnostic horizon. If the diagnostic horizon is present but deeper than two meters, it was believed that its influence on the use of the soil would be minimal. **Question 16, Venezuela**

7.3.1.1 Quartzipsamments

It was desired to keep together the loamy sands and sands without distinctive horizons such as spodic horizons or argillic horizons in one suborder because of the very common problems in the sands of low moisture-holding capacity, blowing, poor trafficability when dry and a number of other common properties. These are important properties to the uses of the soil, and we thought that keeping them in similar taxa or closely related taxa would permit us to make the most important statements. We made provision, we thought, at two categoric levels: the family and the subgroup, to distinguish these soils from others that were included with Psamments. This proposition or question includes the assumption that the coarser Psamments

are recently deposited, but this is not the situation of the sands from the Kalahari desert in Africa have drifted far to the east and north in relatively ancient times, back in Pliocene times. These are not recent sands, and so we provided for an oxic subgroup of the Psamments as well as a psammentic subgroup of the Oxisols. That gives us the central concept of the Oxisols and of the Psamments and one intergrade in each direction which is the maximum we can get without establishing a new great group. If we consider the coarser Psamments that are in uncoated families at the moment, some of these are very ancient soils and some are very recent soils. The age seems immaterial in the coarser Psamments, in so far as the possibility of weathering of the coarse, is virtually nil, and they may be of recent origin as on the coastal dunes in Florida where the wind and the waves are bringing up coarse sands and depositing them as dunes along the coast. These would be our Typic Quartzipsamments with an uncoated family. Going inland a bit, into Florida, we have the older sands, some of which are uncoated and are considered Typic Quartzipsamments. The probabilities are that, certainly, many of these have a spodic horizon at depths greater than two meters. Where there is no spodic horizon, it is quite common but not universal to find coated families, and in these families, we for the most part, have the subgroup of Oxic Quartzipsamments. This surely is the case in much of southern Zaire where sands are very extensive and where there is nothing weatherable except, well there is only quartz and free oxides, but they're coated and belong in the oxic subgroup. It seemed to us that this was a high enough categoric level to deal with the coated sands. If there are difficulties of interpretation, certainly then, we would want to consider the possibility of another taxon somewhere in the system.

I should like to add that we have had laboratory problems in applying the definition of the oxic subgroup of Quartzipsamments. We have, in the soils in Zaire, analyses of the clay fraction and we find there nothing but iron oxides, kaolin and quartz, and yet, the measured CEC's relative to the measured percentage of clay is 20, 25, 30 milliequivalents. This is a laboratory artifact of some sort, it's not the nature of the soil. Similarly with some of the more sandy Oxisols where we had a provision that required 16% or more clay in the oxic horizon, we have another laboratory artifact. We assumed there was no silt in such soils of any consequence, but in the laboratory, as a result of this version, a good bit of the coarse sand is broken down to silt and so we come out with measured sandy loams that have less than 16% clay. The proposal was made to the Soil Conservation Service, as a result of the Zaire data, that we drop the reference to the cation exchange capacity of the clay fraction and substitute the mineralogy of the clay fraction in its place. And as a result of the Venezuelan data, we proposed that we permit Oxisols in their oxic horizons to have less than 16% clay if they have a sandy loam texture. These proposals have accumulated in the Soil Conservation Service, but I believe now that they have one man who is responsible for soil classification that we will begin to see approvals of these proposals. **Question 49, Venezuela**

7.3.2 Fluvents and Fluventic Subgroups

A feature of Fluvents and fluventic subgroups is the irregular distribution of organic matter with depth due to depositional effects and the incorporation of organic matter at depth due to the influence of soil cracks. The irregular nature of the distribution is also an important aspect in diagnosing a cambic horizon which is an important point in the classification.

As to the extent of the variation of organic matter content which constitutes an irregular distribution, there are no fixed values other than that the difference should be significant. If the difference is less than the reliability of the laboratory determination, it must be disregarded. On the other hand, if the difference is greater than the reliability of the laboratory measurement and greater than the probable error of sampling, it is considered to be a significant reflection of an irregular distribution.

Although chemists appreciate the variation of results in the measurement of organic carbon between laboratory duplicates, they do not understand the probable sampling error for this determination. For example, if one takes two samples from a pedon, one from each side of a pit, the difference may be vastly greater than the laboratory error. It may amount, in some soils, to a difference of 3% carbon, perhaps. Especially in Aridisols, the sample taken from the

pit may have a value perhaps of 3/10% carbon, but if one then takes a composite sample at a distance of 5 meters from the sample collected in the pit, the value may be something like 8/10% based on the composite sample. This is because within the Aridisols, the organic carbon varies enormously according to the position of the vegetation. Soil pits are normally dug in barren areas between the vegetation and so they have a bias toward a low carbon value. On the other hand, if one makes a circle around that pit and samples every few meters and composites the samples, one is likely to get a number of samples close or under the sparse vegetation and these are generally higher. Because of soil variations such as this, one must consider not only the laboratory error, but the possibility of the sampling error. This error is, of course, much greater in the surface layers than it is at depth. In the case of a Fluvent or a fluventic subgroup, the sampling error would normally be very small if there were no disturbance or animal activity that was visible in the soil.

The irregularity in carbon is normally associated more closely with the particle-size class or the percentage of clay than with any other soil property. It was assumed in the definition that the Fluvents and fluventic subgroups would be stratified in many instances, and if so the stratification would be reflected in the content of organic material in any event. **Question 24, Venezuela**

7.4 Disturbed Soils

Many soils disturbed by man were once treated as miscellaneous land types or were unclassified as soil. The idea that their heterogeneity merits recognition as a unique group or groups has persisted and the question arises of the recognition of this at least as a separate taxa at a high categorical level. The suborder Arent, as presently defined, provides little guidance for classification of disturbed soils. It might also be considered that a great group Udorthent is needed for mined soils in the humid regions. There is a feeling, too, that a new suborder or great group, probably called Spolents, be introduced since there is now a great deal of documentation, description and data on soils on disturbed land and, therefore, their classification should be more clearly defined. **Question 85, Texas**

Once we had succeeded in defining a soil, it became obvious that these disturbed materials were soil, and that if there was going to be a system that could be applied potentially to the soils of the world, some place had to be made for them. The experience with the Arents, at the time *Soil Taxonomy* was written, came from some of the disturbed soils of Europe. In these soils the disturbance was the result of deep spading, so that there were fragments of spodic horizons of a size that would fit on the shovel with which the soil was turned. In the U.S., where soils had been badly gullied, such as in the loess in the southern states, for example, where on the narrow ridges there are Udalfs and Hapludalfs and in between there are Orthents, when these were reclaimed, leveled with bulldozers and thoroughly mixed, there would be the same fragments of argillic horizons in the smooth shaped land that was left by the bulldozers. **Question 57, Cornell**

These problems also occur, for example, in the areas which are subject to fill by dredging, in which the dredge pumps the sand and the silt out and spreads them over an area to be raised above the water table. These are stratified just like the Fluvents, but they are not subject to flooding like the Fluvents. Clearly, such soils should be classified differently but as the present definition is written, that is where they come out. However, few field observations were made and, therefore, one cannot be specific in the absence of some studies as to variabilities that are found in these. In such land, it would be necessary to have at least one identifiable fragment in each pedon, otherwise the soil would have to be identified as a complex of Arents and Orthents and the area mapped as miscellaneous land types.

It is really more informative to users of the soil survey to identify an area as a borrow pit than to identify it as an Arent. Therefore, in the naming of the map units there is no harm in naming these according to whether it is a borrow pit or a fill, or whatever it is. In the classification, which is technical, and which the users of the soil survey are not much concerned

with, we can simply identify these as unit "BP" for "borrow pit". In the legend which outlines a taxonomic classification, BP appears instead of a series and is identified taxonomically. Users of the soil map should identify the symbol on the soil map in the areas which concern them and then refer to the legend for the appropriate taxonomic groupings. Such users can then get the important interpretations that they are concerned with from the text. They can completely bypass the technical nomenclature; which is intended for use by the people who make the soil surveys, rather than by the people who are interested in finding out what their land can be used for. **Question 57, Cornell**

An important aspect in the classification of these soils is the lack of order or arrangement in the various layers which is present in the natural Orthents for example. There is probably the need now for a more detailed classification of these soils, and there is more information now available to support this. However, the basic concept for their classification would probably have to be the absence of any other material between the coarse fragments. A new suborder, Spolents, has been suggested by some in which disturbed land could be classified. While there is no objection to this idea, it must be emphasized that the definition for this should be made with great care. **Question 85, Texas**

7.5 Families

7.5.1 Psamments

In distinguishing families within the Psamments, properties affecting moisture retention are of great importance since this characteristic largely determines their agricultural use. In this property, it was found that particle-size distribution and the extent of oxide coatings on the sand grains affect moisture retention. The indication is that the content of very fine sand is a more important criteria than the extent of coatings for moisture retention.

When *Soil Taxonomy* was written, there was little information on these properties of sands particularly from Florida where they are so important. Only moisture equivalent values were available, and these by themselves were not very helpful. The definitions were, therefore, written on the basis of available data. It is thought that the very fine sand fraction, particularly that part less than about 74 microns is just as important to moisture properties as is the silt. In the development of the classification of these sands, particle-size summation curves of representative examples were studied. In general, it was found that very fine sand dominated the fine sand fraction. There were also some data available on very fine sand effects on capillary rise and moisture retention from Michigan. Consequently, the definitions of the families of the particle-size classes, as they now stand, treat that very fine sand fraction in a floating manner so that if the bulk of the sand is medium and coarser sand, the sand is treated as sand, but if fine and very fine sand dominate, the texture is considered as silt. At the time when *Soil Taxonomy* was being written, there was a relative absence of data, and yet, if no proposals had been made, nobody would ever examine these things in all probability. Scientists can now object to the groupings as they currently exist and initiate research to produce relevant data so that corrections and improvements can be made. **Question 134, Texas**

7.5.2 Fluvents

According to *Soil Taxonomy*, in pergelic temperature regimes it is not possible to distinguish alluvial soils (Fluvents) from Pergelic Cryorthents. In this temperature regime, both upland and alluvial soils show irregular distribution of organic matter with depth, but due to different reasons. However, in these areas, the alluvial soils are the main agricultural soils, and for this reason, it would be desirable to differentiate these two soil conditions at fairly high categories (i.e. the subgroup level) of soil classification.

It was suggested that one way to overcome this is to create an alluvial moisture regime, i.e. relatively short periods of total saturation followed by long periods of non-saturation. The criterion of flooding should be used to differentiate these soils. This can be considered as a modification of *Soil Taxonomy* - it was simply not proposed before.

One feature of permafrost soils is the accumulation of organic matter just above the permafrost layer. In many alluvial soils, this layer could be between 2-3 meters deep so this organic layer does not occur in such cases, and this can be used as another point for differentiation. **Question 198, Minnesota**

In aridic moisture regimes, the situation is somewhat different. The organic matter content throughout the profile can be extremely low, because the source of the alluvium is from soft eroding rock in which there is no original organic matter. Where this situation occurs, the soils are classified not as Fluvents, but as Torriorthents. **Question 199, Minnesota**

7.6 Intergrades

7.6.1 Oxisols

The definition of the oxic horizon has been modified to include soils of coarser textures. The reason is that they all have relatively low available water-holding capacity whether they are loamy or clayey. One of the principal limitations of Oxisols, regardless of texture, is the low water-holding capacity and in this respect they are similar to the Psamments. The limit of clay content for an oxic horizon was set at a point where it was felt that the water-holding capacity would distinguish between sandy and loamy soils, but in fact, it did not. In order to classify very sandy Oxisols on the one hand and very strongly weathered Quartzipsamments as well as Psammentic Oxisols, it was felt that there was a need for this modification at least from a point of view of management, for the two central concepts and one intergrade on each side of the boundary. The simplest way to define that boundary and to avoid the complication of the silt content, which can be appreciable for some Oxisols, is to describe the soils as loamy and having an oxic horizon, or sandy (psammentic). **Question 43, Texas**

7.6.2 Vertisols, Mollisols, Inceptisols

In distinguishing between a Vertic Tropaquept and a Vertic Fluvaquent, the difference would be primarily one of those listed in the definitions of the orders of Inceptisols and Entisols. The most common distinction would be that the Vertic Tropaquepts would have a histic epipedon, a mollic epipedon or an umbric epipedon and the Fluvaquent would not have any one of these. The presence or absence of the cambic horizon as a distinction would be very exceptional. One would rather expect that the soils would be very similar below the thickness or the depth to which the umbric, the normal epipedon, would extend.

The histic, mollic or umbric epipedon is not required if presence of a cambic horizon can be demonstrated. This can be the situation if the sediments are old enough that the organic carbon has disappeared. By the present definition, the carbon must decrease regularly and reach low levels, less than 0.2% at a depth of 1.25 meters.

The restriction against a hard or very hard consistence and the massive structure in a mollic epipedon was introduced to keep out of Mollisols certain soils that have a xeric moisture regime such as occur in southern California. These soils have what the Australians call a hard-setting A horizon which would need the use of a pneumatic drill for sampling when dry. Below this hard surface epipedon, digging by shovel is possible. These soils have a color and a carbon content that is just marginally adequate for a mollic epipedon, but it was considered desirable to

keep them out of the Mollisols and to classify them as a group, whether or not there was just a little more carbon, or a little less, or whether the color value was closer to three than four, or lay between. The Mollisols in the U.S. do not present these same problems with sampling or plowing since they are structured enough that they may be plowed when dry. The British groundnut scheme in East Africa failed because of the hard-setting nature of the epipedon as described above. When dry, the soils could only be worked with big tractors and heavy plows, but the plows were destroyed like trying to plow up a concrete pavement. It was considered necessary to keep soils like these out of the Mollisols. There are problems. If the moist consistence is used when in this state, these hard-setting soils are more like Mollisols. **Question 5, Venezuela**

7.6.3 Inceptisols

For soils with aquic moisture regimes, the identification and characterization of a cambic horizon is most important in their proper classification. The present definition requires that the carbon decrease regularly and reach levels of less than 0.2% at 1.25 meter depth before a cambic horizon can be identified. In fine-textured soils in aquic moisture regimes, organic matter may be present at considerable depths due to cracking and incorporation of top-soil down such cracks. In this case, however, the organic matter is irregularly distributed in the profile. This problem has been identified in different temperature regimes such as in Holland, New Zealand and Venezuela. In these instances, the use of the organic matter distribution with depth in characterizing the cambic horizon is unsatisfactory. **Question 4, Venezuela**

As an alternative to the use of organic matter distribution with soil depth as criteria for the identification of a cambic horizon, it has been suggested that the presence of 0.25% or more of iron-manganese concretions cemented strongly enough that they would withstand normal laboratory dispersion or disintegration, be used. **Question 53, Venezuela**

7.7 Limnic Soil Material

The Fifth Congress of the Venezuelan Soil Science Society considered an unsolved taxonomic problem of Venezuelan soils involving limnic materials. Frequently, the limnic materials are found as layers of organic soil and, as presently provided in *Soil Taxonomy*, such soils are classified as Histosols. There still seems to be a problem, though, in the classification of soils developed on limnic materials with little organic matter. In the Lake of Valencia, soils are developed, in part, from lacustrine material with little organic matter, consisting principally of marl, and secondarily, diatoms. The soils of recent emersion are classified as aquic and those with a greater time of exposure, in the Ustolls. Between these two extremes of the chronosequence, there are Fluvents, an important group of lacustrine soils. Because of the origin and the particular characteristics, low bulk density, high water content with appreciable shrinking, very rapid infiltration and so on, this grouping is unsatisfactory. **Question 32, Venezuela**

To correct the deficiencies of the system that is found in the classification of limnic materials, it is proposed to create a suborder of Limnents and a great group of Limnaquents.

It is also suggested to create a limnic subgroup and families of marly and diatomaceous mineral soils. In terms of the soil system, soils more strongly calcareous and with mollic epipedons are more adequately classified in the Rendolls and not in the Ustolls. This would mean the creation of a new class of soils, that of the Limnic Ustirendolls.

The situation of the soils formed in the limnic sediments at Lake Valencia is not unique in the world, though to the best of my knowledge, the soils are not particularly extensive. Similar soils occur in The Netherlands where the genesis may have been due to the cutting of the

overlying peat for fuel and as they presently occur, the soils are composed of limnic sediments with too little organic matter to classify them with the Histosols.

The Venezuelan Soil Science Society should submit a resolution proposing the necessary changes to the Soil Conservation Service together with some documents about the nature of these soils. Specific data on bulk density should be given and mention should also be made of the cracks in the soil, even though they have been out of the bottom of the lake for an appreciable time. The original cracks which appeared at the family level are still present in at least some of the soils. The low bulk density is very apparent in field, but it is not apparent to someone reading the documents in the society unless some numbers are included to document how low this bulk density is.

A second proposal should also be aimed at modifying the definition of Rendolls, as given in *Soil Taxonomy*. This will be a more disputable proposal than the one about the limnic groups, because the soil survey staff in the U.S. has gone through this particular argument before, involving soils in ustic moisture regimes with very prominent segregations of secondary carbonates, soils that are now classified as Calciustolls. Many of these soils do have a calcic horizon, and those have distinct accumulations of secondary carbonates. There is no harm in making this proposal to the Soil Conservation Service, but it is likely to be disputed more than the proposal for the classification of the soils that have the low bulk density, the high infiltration, the cracks etc, described above since these soils do not fit comfortably into any family that now exists in *Soil Taxonomy*. **Question 32, Venezuela**

7.8 Weatherable Minerals

Consideration was also given to the grouping of Psamments according to their age and degree of weathering, i.e. the classification of the so-called lateritic sands compared to the recently deposited sands with low weathering regimes. At the present, both these soils of vastly different ages are included in the Psamments. It was thought desirable to keep together the loamy sands and sands without distinctive horizons such as spodic or argillic in one suborder, because of the very common features such as low water-holding capacity, wind erosion and poor trafficability when dry and a number of other common properties. These are important properties to the uses of the soil, and it was thought that keeping them in similar taxa or closely related taxa would permit the making of the most important statements about them.

Provision was made at two categoric levels: the family and the subgroup, to distinguish these soils from others that were included with Psamments. This proposition or question includes the assumption that the coarser Psamments are recently deposited; however, this is not the situation for the sands from the Kalahari Desert in Africa, for example, which have drifted far to the east and north in relatively ancient times and back in Pliocene times. These are, therefore, not recent sands and provision was accordingly made for an oxic subgroup of the Psamments as well as psammentic subgroups of the Oxisols. That gives us the central concept of the Oxisols, and of the Psamments, and one intergrade in each direction which is the maximum that can be obtained without establishing a new great group. Some of the coarser Psamments that are in uncoated families at the moment are very ancient soils, and on the other hand, some are very recent soils. The age seems immaterial in the coarser Psamments, in so far as the possibility of weathering is virtually nil; in addition, they may be of recent origin as on the coastal dunes in Florida where the wind and the waves are bringing up coarse sands and depositing them as dunes along the coast. These would be our Typic Quartzipsamments with an uncoated family. Going inland a bit, into Florida, we have the older sands, some of which are uncoated and are considered Typic Quartzipsamments. The probabilities are that certainly many of these have a spodic horizon at depths greater than two meters. Where there is no spodic horizon, it is quite common, but not universal, to find coated families, and in these families the soils should belong to the subgroup of Oxic Quartzipsamments. This surely is the case in much of southern Zaire where sands are very extensive and where there is nothing weatherable, the material consisting essentially of quartz grains coated with oxides. This is, more likely, a high

enough categoric level to deal with the coated sands, and if there are difficulties of interpretation, the possibility of another taxon somewhere in the system can be considered.

It must be emphasized also, that there are analytical problems in applying the definition of the oxic subgroup of Quartzipsamments. For example, in the soils in Zaire, analyses of the clay fraction show that they consist essentially of iron oxides, kaolin and quartz and yet the measured CEC is between 20-30 milliequivalents per 100 g soil. This is a laboratory artifact of some sort since these values cannot represent the nature of the soil. A similar situation exists with some of the more sandy Oxisols where there was a provision that required 16% or more clay in the oxic horizon. It was assumed there was no silt of any consequence in such soils, but in the laboratory, as a result of dispersion, appreciable coarse sand is broken down to silt resulting in measured sandy loams that have less than 16% clay. The proposal was made to the Soil Conservation Service as a result of the Zaire data that we drop the cation exchange capacity requirement and that the reference to the cation and the mineralogy of the clay fraction be substituted in its place. And as a result of the Venezuelan data it was proposed that Oxisols and oxic horizons could have less than 16% clay if they have a sandy loam texture. **Question 49, Venezuela**

In *Soil Taxonomy*, the limit of sand content for Quartzipsamments is greater than 95 percent and for the Udipsamments, less than 95 percent sand. The rationale for this division is that most Quartzipsamments are much closer to 99.9 than they are to 95 percent sand. The 95 percent limit was set to keep in siliceous families, those that still had an appreciable amount of weatherable minerals. It was suggested to raise the limit to more than 99 percent sand for Quartzipsamments and leave the 95 percent limit for the Udipsamments. **Question 41, Texas**

7.9 Soil Temperature and Moisture Regimes

The moisture regimes (udic, ustic and xeric) are used at different categoric levels in *Soil Taxonomy*. For mineral soils that have well-expressed horizons, the moisture regimes are used to differentiate them at the suborder level. They also differentiate among Vertisols at that level. In the Andisols, Inceptisols and Entisols which have weakly expressed genetic properties, the moisture regimes are used at the great group level to differentiate the soils. **Question 90, Cornell**

With the Entisols, it seemed that it was important to maintain the old concept of alluvial soils, because they are so important agriculturally in the world, and they are so different from the other Entisols which are generally of little use. It was, therefore, considered desirable, at the highest possible level, to distinguish between the Fluvents and the Orthents. That seemed more important than the moisture regime. Having made that distinction between the Psamments, the Orthents and the Fluvents at the suborder level, the moisture regime was introduced at the next lower category. This was the highest category that was possible in order to base the first subdivision of Entisols on the reason why the soils had no horizons. There were also extremely important separations from an agricultural viewpoint which needed to be indicated on small-scale maps since large-scale maps are not involved at these high categoric levels except as a matter of identification of the taxonomic class of a particular series. Higher categories are needed there to function as a key for identification. **Questions 35 and 90, Cornell**

In an arid climate, the moisture regime (aridic) is used to differentiate those soils showing profile development into the Aridisols. But the Entisols, those soils occurring in an aridic moisture regime without genetic horizons, are found at the great group level, e.g. Torriorthents. **Question 170, Minnesota**

In acid sulfate soils, a change in moisture regime, i.e. by drainage, could cause very important changes in which potentially acid soils (Sulfaquents) can be transformed rather rapidly to acid sulfate soils (Sulfaquepts). And so over a short period of time, the accuracy of a soil map may be in doubt. Whereas, in more normal soils it is not necessary to change the classification by artificial drainage. In acid sulfate soils the drainage of a Sulfaquent is a drastic

treatment resulting in enormous changes in soil behavior leading to the development of Sulfaquepts. In this event, there is little that can be done but to change the classification. The land use implications are also great on drainage, since there is little that can be done with Sulfaquepts, but Sulfaquents still have a number of potential uses. **Question 159, Cornell**

Chapter 8

HISTOSOLS

reviewed by R. Rust[15]

8.1 Definition and Classification of Histosols

The organic carbon level used to separate mineral and organic soils was basically taken from the European experience. In the 1938 classification we had a rather vague definition and the organic soils, I think, were supposed to have only 30 cm of organic material. The people who have done most on this in Europe are the Dutch. The American classification of organic soils was extremely weak in the 1938 classification. In Marbut's classification they didn't exist. We used then, in the U.S. for classification of organic soils, mostly the history of the bog, as revealed by different layers at different depths, and the nature of the so called plants that grew in the bog: woody peats vs. fibrous peats vs. other kinds. The limit then was one that had been worked out by the Dutch who had sampled and studied their Histosols much more carefully than anyone had ever done in the U.S. The limit comes directly from their classification. **Question 97, Texas**

For Marbut the Histosols were treated the same way as the other poorly drained soils. Eventually the bog would be drained, the organic matter would be oxidized, and you would begin to develop one of his normal soils. Though they did not appear in Marbut's classification above the second category from the bottom, there was no place for them in the higher categories. They appeared somehow spontaneously in the lower categories, and how he managed that in his mind I cannot imagine. He recognized their existence, but they were not considered a part of his Pedalfers or his Pedocals or his great soil groups. **Question 4, Cornell**

We need considerable further discussion on classification of some of the Alaskan soils where you have quite a thick O horizon over a minimal soil which may be a Spodosol or Andisol or what have you. Virtually all the rooting is in the O horizon and these are considered mineral soils. Should they be? This needs discussion on the part of the people who know something about these soils. It's not outside of my experience. I've seen such soils in the Alps in Europe but to just see one pit does not suggest how we should classify them. So, I think that when and if we have a committee to discuss the organization, re-organization of the Histosols classification, that they should consider this particular problem also - the definition of the Histosol. **Question 167, Minnesota**

At present, I know of no committees that are studying the problems of the classification of Histosols. One of the main troubles was that we had our series defined in completely different terms than we used in *Soil Taxonomy*. The series in Histosols required revisions before we could test anything that was being proposed. How far along the correlation staff has gotten in redefining their Histosols series, I just do not know. There is no International committee working on them. It seemed likely when we published *Soil Taxonomy* that we had provided for a lot of subgroups on a theoretical basis that we thought might exist, so we

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15. Professor of Soil Genesis and Characterization, Dept. of Soil Science, University of Minnesota, St. Paul, Minnesota 55108.

couldn't test the numbers of subgroups that we had. In the long run I think we will have fewer and fewer subgroups of Histosols instead of more and more. **Question 98, Texas**

We will need, eventually, some sort of an international committee to re-examine the whole problem of the classification of Histosols, and I think the formation of that committee should wait until we have actually accumulated more experience and more descriptions and analyses of the soils. At the moment, I suspect we are still rather short in the U.S., at least in descriptions and analyses of Histosols. They have a very low priority for study, partly because their extent is so limited. **Question 110, Minnesota**

8.1.1 Theoretical Classification vs the Use of Pedons

I think the Histosols would be one good example in which we did not insist on an actual pedon on which to base our classification, but we worked out a theoretical classification that provided for foreseeable contingencies. We had no alternatives with the Histosols because we had no well defined series of Histosols in the U.S. against which we could test our proposals. We have probably more subgroups amongst Histosols that are proposed than we will ever have soil series in the U.S. We will have to completely re-examine what has been done in Histosols. This does not suggest that providing for soils, that we do not know, would simplify anything. In fact, it will require more changes. The general rule that we followed, of not providing for a taxon until we had some knowledge of it's existence, was because we did not want to prejudice the classification of a soil that is currently unknown. We wanted to wait until we had a chance to study that soil and it's behavior in order to decide how it should be classified. Classification is not just an arbitrary system of subdividing when you know nothing about what you are doing. You have a purpose for classifying. **Question 109, Minnesota**

8.1.2 Distinctions between O Horizons and Histosols

You avoid classifying a soil with an O horizon as a Histosol by changing the definitions in *Soil Taxonomy*. Nobody is perfect, and there seems to be some confusion in the definition of organic and inorganic soils. I know that under the Kauri trees in New Zealand the O horizon can be more than a meter thick near the tree, getting a few meters away from the trunk of the tree, the litter becomes much shallower. Nevertheless, one does not want to have a complex of Histosols and mineral soils with the limit being a small circle around the trunk of the existing tree. The definition in *Soil Taxonomy*, however, requires that the soil be classified as a Histosol if the O horizon is more than 60 cm thick. Some clarification in the next addition of *Soil Taxonomy* seems to be necessary, and as a general rule, when one runs into a situation of this sort where the soil is obviously misclassified, some comments should be made to the Soil Conservation Service so that they will be aware of the deficiencies in the current edition. **Question 37, Leamy**

8.1.3 Use of Histosols

Under cultivation organic soil materials do oxidize and disappear. Not just in Florida, many of the soils mapped around 1912 and 1915 in Illinois were described as peat, whereas they are now mineral soils. It hasn't worried me that when the diagnostic horizon disappears, the classification can change. Eventually, even the very thick peats in Florida are going to disappear. They may last for some hundreds of years but not forever. It's only in the Histosols. Again the 8° C temperature works out pretty well. The European studies show that you can maintain a Histosol by careful management if the temperature is less than 8° C, but that when the temperature goes much above that the Histosol is going to disappear no matter how carefully it is managed. **Question 59, Minnesota**

The Department of Energy is interested in mining peat deposits in America. They want to know the quality and the quantity of these deposits. Certainly peat deposits have been an important source of energy in the past and they still are, particularly in Ireland, where many of the electric generating plants burn peat. This is the major country where I have seen important harvesting of *Sphagnum* peat. You are getting rid of something that is agriculturally worthless. When you get the *Sphagnum* off, you'll have productive farmland remaining. There may be, in some of the communist countries, mining of *Sphagnum* for energy. I do see, traveling by rail, very commonly *Sphagnum* is being harvested for heating homes and cooking, but this is on a small scale. **Question 64, Minnesota**

8.2 Criteria and Discussion of Suborders

8.2.1 Control Sections

I have no distinct reaction for or against the control section limits for Canada, as described by Dr. Tarnocai, where they "found in Canada that two control sections, 130 and 160 cm, were not very useful and also complicated the classification. In the mid-70's (they) changed that and are now using only one control section, 160 cm; 0 to 40 cm, surface tier; 40 to 120 cm middle tier, etc." The two control sections were provided on a theoretical ground. The whole classification that was proposed for Histosols was a theoretical one that we could not test in the U.S. because of a lack of defined series. The theoretical basis, as I recall, was that if we had a very low bulk density material before drainage, it would have about the same control section that the higher bulk density organic materials would have after drainage. Now, if it isn't being drained, certainly it is not useful. But this was only a theoretical consideration and if it doesn't work in practice it surely should be abandoned. **Question 167, Minnesota**

We could say generally that our control section is adequate for agricultural uses. Where we need interpretations that involve examination of the soil materials to a greater depth, that is, unconsolidated materials, I think we're fully justified. I do not think the nature of the materials below our present control section should be brought into the taxonomy. I think it should be a matter of phases. It might require phases that include not only the criteria that we have used in the proposed classification of Histosols, but phases according to the calorie content of the materials, sulfur content of the materials, the things that are critical to the use of the material for production of energy. This can be phased. **Question 111, Minnesota**

8.2.2 pH as Criteria

The criteria pH was not considered at the time of the development of *Soil Taxonomy*, rather than base saturation that I know of. pH was considered but it didn't get written into any definition, except as it appears in the definitions of the Sulfaquepts. At the family level, we have some pH limits for Histosols and so on. But otherwise, we have kept pH out. The pH is quite a variable thing with respect to base saturation, and it varies quite a bit from one place to another. It depends on when you take your sample, what the pH is going to be. It can have half a unit, or occasionally even a unit, variability with the season. Some of the most careful studies have been on Histosols in Finland where they found the pH varying practically one unit seasonally. I think Michigan has some studies of this sort. **Question 159, Minnesota**

8.2.3 Fibrists

I should like to comment on the procedure we followed in response to Dr. Farnham's comments that " Florida has a Tropofibrist, Minnesota has a Borofibrist. (He doesn't) believe in

the middle west there is a Medifibrist, like Ohio. There may be some *Hypnum* mosses that do exist in these in-between temperature regimes. They exist here in Minnesota. They are few and far between but there is *Hypnum* moss over rock. It's in this in-between climatic regime." **Question 110, Minnesota**

If we had not made these proposals and focused people's attention on the possible combinations of characteristics, we would not have people studying the Histosols and writing descriptions that were more intelligible than the old ones in which we had woody peats. These were largely classified on the basis of what was growing on the bog rather than what was in it. **Question 110, Minnesota**

8.2.4 Folists

The division for contrasting materials in the Histosol classification does not take care of the situation where, as Dr. Tarnocai (Canada) described "what really is happening is that we have a wetland and then, for some reason, forest invades this. So we have peat and the forest comes over. Of course, the wetland situation stops. Then you have an upland forest, mainly hemlock and red cedar, a heavy growth about 110 feet tall and several feet in diameter. We are talking about heavy timber. This situation produces litter which is a Folist. What Ugolini described in southern Alaska is just the opposite. You have a Folist developing first and then some kind of a natural drainage change. We have both situations. That's how the two materials arise." **Question 169, Minnesota**

We have in Hawaii these two kinds, two suborders of Histosols. They probably are fibrous on the island of Hawaii, rain-fed Histosols. Very few examinations have ever been made of them because they have up to six hundred inches of rain a year. The other kind is extensive on the island of Hawaii where you have lava. It's a Folist. There is nothing between the chunks of lava except organic materials. The concept of the Folists really comes from those soils in Hawaii. If you don't have a place for them you'll have to call them 'not soil', but they support a fairly good forest. Under the forest, I suppose, there is a thin O horizon which decomposes in that warm climate fairly rapidly, but between the chunks of lava there are just organic materials. These are obviously very different from what you have in midwestern states. I had a lot of trouble with the committee. They weren't interested in these soils. They were interested in the thick organic materials that are so typical of Minnesota, but I finally got them into the classification because I've seen enough of them in the world. The O horizon rests on hard rock and yet supports quite a good forest. **Question 66, Minnesota**

The term "freely drained" is often used in relation to Folists and refers to the absence of groundwater for certain periods either the year-round or so many months a year. I can not suggest what sort of limits you should use. The concept comes from the soils that we have on the island of Hawaii where we have a forest growing on lava and a litter which falls down the cracks between the blocks of the lava. On these soils there is never any groundwater, but if it doesn't rain today it's a drought. **Question 167, Minnesota**

Yes, basically, this is the situation that we are looking at when these soils occur in a high rainfall area, let's say rainfall precipitation is a hundred inches or sometimes more in the Pacific coast and I think, Alaska too. After a rain these soils are saturated, but if you have a rain-free period for a few days, they are freely drained. They would fit our concept of well drained soils, but there is a period of time, I think, when they are saturated. This is a little bit confusing, when it is compared to the definition of well drained. This is where we are having problems. If you have a wet period of a few days, they are saturated for a week or so, or if you have a longer one, they are saturated for a longer period. It depends on how long the rainy period lasts. The water just pours into a bore hole, almost like a heavy groundwater discharge. These moving groundwaters, in general, seem to carry oxygen. Where I've seen the soil with moving groundwater, there was no evidence of mottling or reduction of iron, segregation. Sometimes there has been evidence of removal of iron from the soil, but not reduction and segregation. This was built into the definition of saturation with water. **Question 167, Minnesota**

Page 217 of *Soil Taxonomy* doesn't speak of free drainage. It's in the discussion. These are the more or less freely drained Histosols. But then the definition says they're never saturated with water for more than a few days following heavy rains etc. That's page 217, where it starts. That's not the definition. That's the general concept. The definition is on the next page, 218 at the top, where we don't use the term. Saturated with water is discussed under the aquic moisture regime, page 54. Perhaps it could have been written better by saying we do not have an aquic moisture regime, instead of not saturated with water. I should also comment that I think it would be desirable in the case of the Histosols to use sloping families. **Question 167, Minnesota and Question 3, Witty & Guthrie**

Chapter 9

INCEPTISOLS

reviewed by J. Rourke[16]

9.1 Background of the Order

During the development of *Soil Taxonomy* certain groups of soils appeared with many characteristics or a few common characteristics that seemed to be closely related enough to justify recognition as an order. For example, the Vertisols, with their expanding clays and their occasional or frequent dry seasons, with their wide cracks, constituted a group of soils that it seems should be recognized as distinct from other kinds of soils. Similarly, the Mollisols with their mollic epipedons and high base status, seem to warrant the recognition of their own order. The Entisols, lacking any diagnostic horizons, seem worthy of recognition as an order and so on. Eventually we had nine apparently satisfactory groups of soils that could be used to recognize orders. However, every taxonomy has a waste basket. When we finished with the nine orders, we still had many soils left over that appeared better not grouped with any of the other orders. We tried, for example, to group soils with cambic horizons and with argillic horizons in various approximations, and none of the groupings seemed to be satisfactory. From a genetic point of view, one could group some of the Inceptisols with the Alfisols on the basis that they are going to develop into Alfisols over time, but we had to reject this kind of an assumption on the basis that the Inceptisols, being weakly developed, might develop into Alfisols or Ultisols over time. But on the other hand, if erosion exceeded the rate of soil development, they might develop into Entisols. It was not possible to group the soils on the basis of a genetic assumption that over time they would develop into another kind of soil. So, the Inceptisols represented the kind of soils that did not seem to fit into any other order. Over time now we have concluded that the suborder of Andepts has enough properties in common that they should be recognized as an eleventh order. In time, there may be other orders cut out of the suborder of Inceptisols, but this is a matter of future knowledge rather than the present knowledge that we had at the time we developed *Soil Taxonomy*. **Question 10, Leamy**

9.2 Definition

9.2.1 Difference between a Vertic Tropaquept and a Vertic Fluvaquent

The difference would be primarily one of those listed in the definitions of the orders of Inceptisols and Entisols. The most common distinction that I would visualize would be that the Vertic Tropaquept would have a histic epipedon, a mollic epipedon or an umbric epipedon and the Fluvaquent would not have any one of these. The presence or absence of the cambic

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16. Retired, formerly Principal Soil Correlator, Head, Soils Staff, Northeast Technical Service Center, SCS/USDA, West Chester, Pennsylvania 19380.

horizon as a distinction would be very exceptional, in my opinion. One would rather expect that the soils would be very similar below the thickness or the depth to which the umbric, the normal epipedon would extend. **Question 5, Venezuela**

The histic, mollic or umbric epipedon is not required if presence of a cambic horizon can be demonstrated. And this can be the situation, if the sediments are old enough that the organic carbon has disappeared. By the present definition, the carbon must decrease regularly and reach low levels less than 0.2 percent at a depth of 1 and 1/4 meters. **Question 5, Venezuela**

9.2.2 Proposed Solution to the Classification of Irrigated Xeric and Ustic Inceptisols

I think they should remain as Inceptisols. It is quite a common situation in the Near East where the moisture regime is aridic to irrigate and to salinize the soils. If the irrigation is stopped, these soils will still produce crops. I ran into a situation in Venezuela where we had an ustic moisture regime, and the government had irrigated one farm for a nursery for cocoa. When you sampled the soils on that one farm they became Aridisols because of the salinity and yet all around them the farmers were growing one good crop of maize every year. This was an island of Aridisols created by this definition. If irrigation were stopped the salinity would disappear within a year or two. It is a similar situation in the U.S. where they're irrigating citrus with Colorado River water in California and the soils are mostly Xeralfs or Xerochrepts. Where you have a seepage spot at the base of a hill the wetter soil on the landscape becomes an Aridisol, if it doesn't have an argillic horizon. This is irrational; we have the same problems on the lower Rio Grand Valley in Texas.

I proposed the solution that we drop that limitation on salinity in the Inceptisols. This will require a slight modification in the definitions of both Inceptisols and Aridisols. As they are now defined, the Aridisols are supposed to pick up any Inceptisols that have become saline by irrigation. If we drop the limitation on the salinity of Inceptisols, then the definition of the Inceptisols and the Aridisols would differ primarily by the moisture regime. **Question 69, Texas**

9.2.3 Conductivity to Differentiate between Aridisols and Inceptisols

This limit on conductivity was introduced in an effort to provide a field criterion that could be used for distinguishing the Orthids from the Inceptisols. It has not worked. Whether we use 2 millimhos or 4 millimhos, the use of conductivity to make this distinction breaks down whenever the soil is irrigated. There are large areas in the middle east, in the Rio Grande Valley, in Texas and in southern California where Inceptisols are irrigated, the conductivity may become quite high, and I proposed when I was here in Venezuela that we drop all reference to conductivity to distinguish between Inceptisols and Aridisols.

It is pointed out that there are some soils in which the conductivity is appreciably higher than 2 or 4 millimhos, up to 12 millimhos. In this situation the salinity is a limitation for many crops, and these still must be included in Inceptisols if the requirement for conductivity is dropped completely. This brings us to the use of conductivity in the taxonomy; we avoided the use of conductivity everywhere except this one place. Elsewhere, the salinity is used as a phase rather than as a taxonomic differentia. We have kept the use of salinity to the phase level outside of the taxonomy deliberately for two reasons. One was the precedent in the mapping in the United States in which salinity was used as a phase for soil series, and if salinity were introduced as a taxonomic differentia, these series would have been split and it was, in general, considered a very serious thing to split a series. The other is that the salinity under irrigation is quite variable according to several features. One is the quality of the irrigation water. One is the length of time that has passed since one has gone through a leaching cycle, and one is the overuse of water so that soils become water-logged and the salts come up by capillary rise. The conductivity of the irrigated soils is an extremely dynamic feature of the soil and is dependent on the water and the irrigation practices. If then we introduce absolute limits on conductivity

into the taxonomic classification, we have soils that will shift with each leaching cycle from one taxon to another and so the theories will have changed every time the soil is leached and this seems to us to be irrational, and this is why we have kept salinity out of the taxonomy itself, but it is extremely important and must be used as a phase. Our interpretations are always made for phases of taxa, not for the taxa. **Question 47, Venezuela**

9.2.4 Ochrepts and Umbrepts

Among the Inceptisols, a first break was made according to the nature of the epipedon, i.e. umbric or ochric. This was probably an error, but it was related, so far as the United States goes, to the moisture regime. The Umbrepts, commonly occurring in the U.S., are in mountains relatively cool and very humid and have extremely low base saturation. The Ochrepts, on the other hand, have somewhat drier moisture regimes than perudic in the U.S., though there are some in the Appalachians and southern New York, perhaps that certainly are marginal to perudic moisture regimes.

The subdivisions of the Ochrepts and the Umbrepts were not made on the basis of the moisture regime, in general, although there are Ochrepts with ustic and xeric moisture regimes. There it is used at the great group level, the reason being, that the umbric and the ochric epipedons were of more importance than the soil moisture regime. Inceptisols were regarded as a wastebasket that included everything that did not belong in some other order, and its classification should have been criticized much more severely than it ever was. It is hopeful not only that the Andepts will be reexamined closely by an international committee, but that the rest of that wastebasket will also be reexamined. **Question 90, Cornell**

If I had a choice to make a new start, I probably would not have split the Inceptisols into Ochrepts and Umbrepts. This is leading to serious trouble outside of the U.S., whereas in the U.S. the Umbrepts are so rare that they make us no problem here. **Question 89, Cornell**

9.3 Great Groups

9.3.1 The Omission of Potential Eutric and Dystric Great Groups

Eutric and dystric great groups were recognized in the suborders of Andepts, Ochrepts and Tropepts, but not in Aquepts. I cannot recall any serious discussion of this particular problem. The omission might have been an oversight; certainly, no one could have seriously argued in favor of it, or it would probably would have been included. We may have found a base saturated Aquept, if we had searched for it. **Question 118, Cornell**

Once it is drained, there is not much difference between an Aquept with a high base status and a parallel family of an Ochrept with a high base status. The aquic moisture region will certainly affect the rooting of most plants, except rice. I am afraid I cannot elaborate more on that. **Question 118 and 119, Cornell**

9.3.2 Selection of 60 Percent Base Saturation to Separate Dystrochrepts and Eutrochrepts

The limited data for Eutrochrepts, which we had for soils in Pennsylvania and New York, many with Fluventic subgroups, indicated that the base saturation hovered somewhere around 50% in the same subhorizon. We did not want to make a lot of separation in this great group, as it would have no practical value. So we set the limit at a point which would keep them all

together. We started at 50%, but later we had to raise it, as this was the medial value for most of these soils. **Question 56, Cornell**

The studies we had for the Inceptisols in the northeastern states, in Pennsylvania, in New York, and Maryland shows that the most common range of base saturation in these soils was between about 45 and 65 percent. By moving the limit up to 60 percent we kept all of the related soils very much together. If we put the limit at 50 percent we would have cut down the middle of these series. They are so similar that the fieldmen can't tell what the base saturation is. They had to go to the laboratory. The sensible thing to do was to use another number because none of them get very far above or very far below the 50 percent limit. **Question 127, Texas**

If we were going to make a distinction, we had to get a limit somewhere. The Dystrochrepts may have only 5 percent base saturation, the Eutrochrepts may have up to 100 percent. Somewhere along the line, there has to be a distinction, a limit. We have been using the 50 percent for the distinction between high base status and low base status in other parts of the taxonomy, so it seemed logical to extend it there. The definitions were firmed only by testing what soils were grouped and how these soils that were grouped behaved in the field. In the northeastern states we had a lot of soils where the base saturation was just 45 percent or 55 percent. The 50 percent was the most common figure that we got, and we did not want to split these soils all over the landscape, so we figured that if we raised the limit to 60 percent from 50 then we had the limit from which there were not too many soils that we found in nature. And those with 55 percent and with 45 percent, which occurred more or less mixed up in the landscape, particularly on the river terraces in the northeastern states, would remain as a single group. Many of the apparent discrepancies in *Soil Taxonomy*, the exception here and there, are made just to keep a small group of soils together. They sit with a property that is just on the limit between two classes in a higher category, and to avoid splitting a natural group, we made exceptions here and there. So we use 60 percent on Dystrochrepts and Eutrochrepts, and we use 50 percent on Mollisols.**Question 77, Cornell**

9.3.3 Ochrepts with Low Base Saturation in the Control Section but with "Carbonates Within the Soil"

We simply did not know about the existence of such soils. We knew nothing about their behavior. There was an opportunity for the people in the experiment stations and in the Soil Conservation Service to criticize that proposal, and no criticism was received. So the proposal being uncriticized has come down into *Soil Taxonomy*. Now it is time, when you have examples, to reexamine the definitions. **Question 78, Cornell**

I used to have a standard answer for problems of this sort for people who did not like the definition. They were dissatisfied with it. I recognized that, but I asked them in order to stop the arguments, to suggest another definition. That generally ended all the discussion.

This concept of the Eutrochrept came from the old great soil group of Brown Forest Soils, and when I looked at the soils that were classified as Brown Forest Soils and was trying to get some notion as to what they were, I found a good many with rather shallow carbonates, 50 cm, 40 cm, or less. I also found Andisols where there wouldn't have been the presence of volcanic ash as parent materials. The first Brown Forest Soil, I guess, I saw was a very good Andisol, out in Montana. I saw Brown Forest Soils in Minnesota that made me think of Spodosols, but there was no A2 horizon. They cut the forest and had repeated fires, poplars replaced the conifers, and the earthworms came in. The earthworms started turning the soil and left a layer of worm casts, except when I located a small area or two where the fire had not burned the litter from the original forest. The A_0 was still present. I looked under that, and saw a beautiful albic horizon about 5 or 6 cm thick, and that disappeared laterally within a distance of about two feet, a mixture of spodic worm casts and albic worm casts, and then you got two feet away and it was all worm casts. This is going on at the moment with a number of their Boralfs. There were no worms there in nature; the fishermen brought them in for bait, and they got away. So they are chewing up the albic horizon of some very good Boralfs in the neighborhood

of the lakes and they are spreading rapidly. The glaciation destroyed the worm. They do not spread distances of very many miles very rapidly. So the boundary for a soil, then, included Entisols and Spodosols that were wormy, and then what we have retained as a concept, these calcareous parent materials. Perhaps you would do better to propose that you require carbonates within a particular depth limit, rather than within the soil, which is admittedly vague. **Question 107, Cornell**

9.3.4 Soils Associated with the Kauri Pine in New Zealand

You get a variety of soils, very commonly Dystrochrepts. The Kauri pine can not make an albic horizon in a Vertisol. But it can in a material with a considerably coarser texture. They are mostly coarse to fine loamy Dystrochrepts. **Question 143, Minnesota**

9.3.5 Dystrochrepts with Placic Horizons

A viable procedure for national soil surveys outside the United States to gain recognition of subgroups not defined in *Soil Taxonomy* is not restricted to subgroups but also occurs in great groups and conceivably could occur at higher categories than the great groups. For example, in New Zealand we found in a Dystrochrept with a placic horizon, a combination of horizons that is not provided for in *Soil Taxonomy*. It was necessary, then, for me to consider whether or not placic horizons had an effect on the interpretations and required recognition of some sort of a Dystrochrept that had a placic horizon. Well, anyone who has seen a placic horizon realizes that it interferes seriously with movement of water and penetration of roots and has an important effect on our interpretations so that this combination of horizons required recognition of a new taxon at some categoric level. The normal rule for *Soil Taxonomy* was that the presence of a pan, like a placic horizon or fragipan, was recognized at the great group level. The combination of the ochric epipedon, the cambic horizon, and the placic horizon, not being provided for in *Soil Taxonomy* and requiring a new group if we were consistent with the recognition of pans in other taxa throughout the taxonomy, implied that a new great group was required. However, the ochric epipedon was always marginal to an umbric epipedon, and we also had the combination of an umbric epipedon, a cambic horizon and a placic horizon, and the differences in the thickness of the horizon, dark-colored enough for umbric, was always close to the necessary limit of 25 cm. It might be 20 cm or it might be 30, but this was the normal range of thickness. Therefore, I made a proposal in New Zealand that we not worry about the presence of an umbric or an ochric epipedon in the definition of this combination of Inceptisols with placic horizons. This then required a change in the definitions of Ochrepts and Umbrepts so that one great group or another had either an ochric or an umbric epipedon and a placic horizon. This kept together this group of soils that belonged together. Similar occurrences of unanticipated combinations of horizons and properties are surely going to be found in all categories. The problem of the undefined subgroups is no different from that of the undefined great groups except that having more subgroups than great groups, it may be more common. **Question 7, Leamy**

9.3.6 Ustochrepts with a Petrocalcic Horizon

(Why was no provision made for Ustochrepts with a petrocalcic horizon in *Soil Taxonomy*?) The subgroups that were provided in *Soil Taxonomy* were primarily those for soil series that were either established or tentative in the United States. A few subgroups were provided that were not known to occur in the United States, but this was only done when we had a specific request. If we have a series in the U.S. of Ustochrepts, or Xerochrepts with a petrocalcic horizon then it is very likely that we would have provided such a subgroup. It is an implied subgroup in *Soil Taxonomy* in that the Typic Ustochrept has a calcic horizon or soft powdery lime but no petrocalcic horizon is provided for. There is no question if a soil had a petrocalcic horizon instead of a calcic horizon; we would have recognized two series. One for

the petrocalcic horizon, and one for the calcic horizon. Had we had such a tentative series or established series, I think, without question, that a petrocalcic subgroup would have been provided. **Question 146, Texas**

9.4 Subgroups

9.4.1 Depth of Mottling for Aquic Subgroups of Dystrochrepts versus Fragiochrepts

I cannot give you a good answer to that. These proposals originated in the correlation staff of the different regional offices and states. These were their proposals, and I accepted what they proposed. There must have been, I am sure there was, a good deal of discussion at a number of regional work-planning conferences. We had committees on these various groupings, according to kinds of soil, and their thinking evidently was that mottling limits should vary with the kind of soil. **Question 84, Cornell**

9.4.2 No Provision for Mollic Subgroups

We knew that the Inceptisols that had a high base status normally had a somewhat darker epipedon than those of low base status. That is a generalization, probably there are exceptions. There seemed to be no desire on the part of the people who had these darker-colored Inceptisols to put them into a mollic subgroup, so it was not done. It could be done. **Question 47, Minnesota**

9.4.3 No Provision for Spodic Subgroups

Soil Taxonomy was designed so that the least possible disturbance would be made if new knowledge and experience indicated that we should change some part of the system. In this situation where an intergrade may be desired between an Inceptisol (a Dystrochrept) and a Spodosol, the people who have some experience with these soils must propose that this intergrade be introduced into the system. In making such a proposal it would be essential that the man who makes the proposal, proposes also a definition for the spodic subgroup of the Inceptisol. This is perhaps the most difficult part for making a proposal for a change. *We need to have not only the proposed definition but we need also to have some reason why the change should be made.* Does it improve accuracy of interpretations, if so this should be spelled out in the proposal. **Question 25, Leamy**

9.4.4 No Provision for Aquoxic, Plinthoxic and Plinthaquoxic Subgroups of Dystropepts

Soils that are classified in the great groups of Dystropepts may have cation exchange capacity less than 24 milliequivalents per 100 grams of clay, and also have characteristics as follows: a phreatic watertable between 75 centimeters and one meter depth, a horizon with more than 5% plinthite within one and a half meters depth, the combination of these two characteristics. It is possible to classify them within the subgroups Aquoxic, Plinthoxic and Plinthaquoxic Dystropepts respectfully. No provision has been made in *Taxonomy* for these soils. It is, of course, possible to have these three subgroups if the behavior of the soils is such that it seems desirable to have all three rather than two. No provision is made in *Taxonomy*, as we have mentioned earlier, for soils that do not appear in the United States and soils for which no foreign request has been made for special subgroups. **Question 31, Venezuela**

9.4.5 Soils with Mollic Epipedons and Vertic Properties in the Vertic Subgroups of Tropepts

It so happens that the Vertisols are permitted, but not required, to have a mollic epipedon. In Puerto Rico we have, in many places, a transition from an Inceptisol on a side slope of a hill to a Vertisol at the base of the hill. The epipedon in these soils are sometimes mollic and sometimes not, but they are always marginal to the limit between a mollic and an ochric epipedon. It seemed desirable to keep these soils together in the classification even at the series level so that if we were to do so, we had to permit a mollic epipedon in the vertic subgroup of the Tropepts. **Question 31, Venezuela**

9.5 Andepts

9.5.1 Soils with Large Amounts of X-ray Amorphous Materials

The decision to include the soils with large amounts of x-ray amorphous materials in the Inceptisols, if there were no particular diagnostic horizons other than a cambic horizon, was the subject of discussion. It was discussed as a possible eleventh order at the time that the orders were being attempted. The concept of the Inceptisols at that time was pretty much the concept of rather weakly weathered soils. We did not fully realize that we could get rather strongly weathered soils in that order, if we had the proper moisture regime and temperature. It was more or less a wastebasket order for the soils that did not fit any of the other nine orders, and when we examined what was in that order, the Andepts stood out as a rather unique group and the staff thought generally that it would be adequate to have a suborder for these soils. **Question 56, Cornell**

9.5.2 Classification of Ashy Soils with Only an Ochric Epipedon

The suborder of Andepts, currently presented, is based on composition primarily. It has many defects - the definition of the suborder and the classification at great group and subgroup levels. From my personal point of view, I think I should prefer to keep these soils with an ochric epipedon and nothing else as Entisols with an ashy mineralogy, but I think that more than one mind has to be consulted on this, so we have a committee of about 75 that will be arguing about it. I should mention that this proposal arose from my experience in the West Indies on the volcanic islands where I found, even though I had the family classification, I could make no interpretations for phases. When I got to New Zealand, it was primarily to have a look at the soils from ash or pyroclastics in a country where they had studied these soils intensively and there was no language problem. I had the same problems there on interpretations with the proposed classification as Andepts that I had in the West Indies. Dr. Leamy came one day with a problem that they were supposed to meet with the horticulturists and suggest to them where horticulture could be expanded in New Zealand, and with a knowledge of family classification, I could not tell him. I had to inquire and inquire and inquire for additional information before I could suggest that this particular soil might be useful for horticulture.

The soils were skeletal; the soil is actually a mixture of pumice with little ash to store rainfall in available form. We cannot use, entirely, the geologist's classification of pyroclastics. The andesitic and rhylotic vesicular ejecta behave the same but only one can be called pumice, the rhylotic. Dr. Leamy is publishing my proposal in a book that they're issuing in New Zealand for some meetings, because he says they're not generally available. **Question 32, Texas**

9.5.3 Bias for Cation Exchange Procedures for Limits within the Andept Suborder

pH-dependent charge surely does bias the results. It is very difficult to get a high base saturation in such soils unless the pH of the soil is naturally somewhere in the neighborhood of 7. However, you must keep in mind the following facts: at the time that we developed *Soil Taxonomy*, there were virtually no data of any sort on the cation exchange capacity of the Andepts in the United States. We speak of the pH-dependent charge, which one can estimate perhaps by the difference between the retention of bases at the pH of soils in the field versus the retention of bases at pH 7. Such measurements were simply not available at the time that we began the development, or even reached well toward the development of *Soil Taxonomy*. Now that we have some data, not as much as we would like, we still have some that compares the retention at pH 7. We realize that the base saturation should not be used as a differentia in these soils with x-ray amorphous clays. So we have an international committee reexamining the classification of such soils. **Question 66, Cornell**

9.5.4 The Vitrandept Great Group

The Vitrandepts were included with the other Andepts partly because of the geographic association. For small-scale maps, one is apt to get rather coarse-textured pyroclastic materials close to the volcano, getting finer and finer with distance. They are largely of glass. What fine earth there is, in the way of weathered products, is going to be similar to that formed in the volcanic ash rather than the coarser pumice. They have, therefore, a number of properties in common with the other Andepts: relatively high phosphate fixation; relatively good moisture holding capacities; if climate is perhumid, irreversible changes on drying of samples. I would still favor including the Vitrandepts with the other Andepts, if I had it to do over again. **Question 56, Cornell**

9.5.5 Uncultivated Andepts in Ecuador

They didn't get satisfactory yields. It wasn't entirely a matter of soil temperature. The Andepts you can find at any elevation. But the yields are so low that they're rarely cultivated. Remember, they don't have access to fertilizers. **Question 155, Minnesota**

9.6 The Proposed Order of Andisols

9.6.1 Recognition of the Order

The original concept of the Andepts came from the concept developed in Japan of the Korobuco soils which have very dark colors, are fairly strongly weathered and are very high in their percentage of organic matter. **Question 88, Texas**

The trouble started for me when I spent a year in the West Indies. There were no particular problems on the islands from sedimentary rocks. However, on the volcanic islands there were serious problems with the classification; in that, at the family level, I was unable to make any interpretations whatever.

There were a number of difficulties with the classification of the Andepts as presented in *Soil Taxonomy*. One was the use of base saturation by ammonium acetate at pH 7 because the exchange capacity is so strongly pH-dependent, it became very difficult to get a base saturation

of as much as 60 percent unless the pH of the soil in the field was pH 7 and above; then the base saturation would exceed 50 percent or well over 100 percent in some. **Question 88, Texas**

Another serious defect was the over emphasis on color, particularly color value, in defining the great groups. On the island of St. Vincent in the Lesser Antilles there were a number of eruptions in 1902 and 1903. The north half of the island was blanketed by a rather thick mantle of black cinders. The color of the parent material was black before it was weathered. In the 75 years that had elapsed after that eruption, a number of them had accumulated more than one percent organic carbon, and so, although they were very coarse in texture they were principally black cinders that came out with the Andepts; the color was the same, but the organic matter was not the same. The black color was from the cinders, not from the organic matter, and they just barely qualified for Inceptisols. Some of them did not; some of them had to be classified as Entisols--Psamments. But others had just enough B horizon, just enough organic matter to qualify as Umbrandepts, although the black color was entirely due to the color of the cinders. **Question 88, Texas**

Soil Taxonomy did not distinguish the Andepts according to the soil climate, so that one could have, for small-scale maps, a variety of climates from polar to equatorial. Only at the family level could one distinguish the differences. Because the Andepts were only a suborder, we were short the category at which we normally brought in moisture and temperature regimes.

Another problem had to do with the particle-size class which is alluded to in the classification of the Andepts. We used a combination of mineralogy and particle-size rather than strict particle-sizes, but we had too few classes. We could not distinguish between cinders and ash. The moisture-holding capacity varied enormously between the skeletal classes, sandy skeletal and loamy skeletal, in which the rock fragments were pumice and the skeletal families in which the rock fragments are limestone, granite or something else. Very sandy skeletal pumiceous soils in New Zealand will hold, in an available form, more than one year's rainfall for the growth of the *Radiata* pine. Any other skeletal soil will have 1/10 to 1/20 of the moisture-holding capacity of the pumiceous soils. **Question 88, Texas and Question 56, Cornell**

The classification of the Andepts in *Soil Taxonomy* did not provide for soils that are extremely high in aluminum compared to the bases. We had in New Zealand many Andepts whose total exchangeable bases plus aluminum, in materials that had a feeling of a silt loam, less than 0.2 milliequivalents total bases plus aluminum or a ECEC. Where we had a mixture of pyroclastic materials plus other materials, as we have in much of the alluvium and some of the loess in New Zealand and in the U.S., we have a fairly high ECEC, but the bulk of the ECEC is the form of KCl-extractable aluminum. These ar extremely high in aluminum compared to the bases. **Question 88, Texas**

I went to New Zealand with one purpose: to try to devise a more rational classification of the soils from volcanic ash and cinders and pumice. I had no language problem there, and they had a great deal of data on the properties of their soils and a great deal of experience with their use. While there, the horticulturalists on the north island wanted to explore the regions where horticulture could be extended in New Zealand, and this is where most of the Andepts, all of the Andepts in New Zealand, are found. They brought me the series with analytical data and asked which ones of these would be good for horticulture. It was impossible to answer that question without a great deal of information that was not in the family name. We had the families, but we could not interpret them. That was the purpose of that category, to be able to make interpretations. **Question 56, Cornell**

It was the complete inability to make interpretations for the families of the Andepts that led to the proposal for an order subdivision (an order category), an eleventh order in which we could bring in soil climate, much as we did Alfisols and Ultisols and so on, in such a way that we could make some interpretations when we got down to the family level. It seemed much easier to propose a new order than to find some way, perhaps at the subgroup level, to bring in the moisture regime, or perhaps even at the great group level it might have been done. We also had to get rid of base saturation and substitute total bases for it. Since I could find no relation whatever between the value of the surface horizon, the chroma of the surface horizon and the

content of the organic matter, we needed to de-emphasize the color. **Question 56, Cornell and Question 88, Texas**

We needed a new set of particle-size classes for the soils from pyroclastic materials. Because of the difference between pumice and cinders in terms of their moisture-holding capacities, we needed to be able to differentiate between the two. In my proposal I had to define my particle-size class terms and my mineralogy terms, because I could not use them exactly as they are used in the AGI Glossary. The geologists make a break at 4 mm and the pedologists at 2 mm. It didn't seem rational to adopt the AGI particle-size classes for soil science. We retained our present size limits for gravel. We had to either redefine pumice or substitute another term for something with the same properties but from a more basic magma. The AGI terms restrict pumice to materials that are nearly white. If you have the same vesicular materials from a more basic magma, the colors are not white; they become quite dark, in fact. The brittle vesicular nature, the very low apparent bulk density of pumice and pumiceous-like materials are quite important in their engineering uses, and even more important to the growth of plants because they will store so much more moisture. A pumice blanket that has say a mean particle size diameter of 10 cm or more can still store the whole year's rainfall in New Zealand in an available form. The foresters have studied the growth of the Ponderosa Pine and measured the moisture extraction and it will store well over a meter of water within the rooting zone. Whereas, a skeletal material that is composed of rounded chunks of granite will store virtually nothing and yet particle size is virtually the same. The mineralogy can be the same in the two. The basic materials are quite full of glass. The soft gravel, for example, will not store water, but it is just about as pyroclastic as the pumiceous-like materials that are blown into the air.

The whole proposal is being considered by an international committee as I have mentioned with about 75 current active members, and it is being published in the book that the New Zealand Soil Science Society is publishing on soils with variable charge. There will be one chapter that will include that proposal. **Question 88, Texas**

9.6.2 The Suborder of Tropands in the Proposed Order of Andisols

The recognition of isotemperature regimes in the Andisols at the suborder level is parallel to the recognition of these temperature regimes in the Inceptisols. The reasons are the same. In the Andisols as well as in the Inceptisols, Ultisols, etc., the color value is very poorly related to the content of organic carbon. If the soils are not separated above the great group level where we recognize amongst Andisols, the Melanudands which are very dark colored, this color value must be extended to the intertropical soils where the color value is not related to the organic matter content. On the island of St. Vincent in the West Indies, for example, the northern half of the island is covered by a black cinder deposit which dominates the color of the soil. The blackness is not related to the organic carbon, and the more weathered the cinders the lighter they become in color. We wanted to avoid using color value as an indication of organic matter contents in the intertropical regions. **Question 19, Leamy**

Chapter 10

MOLLISOLS

reviewed by J. McClelland[17]

10.1 Mollisols and the Mollic Epipedon

In defining Mollisols emphasis is placed on the presence of a mollic epipedon with a high base status. While the Mollisol order includes many dissimilar soils, extensive areas in the United States and other countries are Mollisols.

Although the mollic epipedon is required for all Mollisols it is also permitted in a number of other orders, including Inceptisols, Alfisols, Ultisols, and Vertisols. The mollic epipedon is not the only common feature of the Mollisols. The Mollisols must have, not only a mollic epipedon, but they must have a base saturation of more than 50 percent by NH_4OAc in all subhorizons below the epipedon and within the control section. The mollic epipedon is required for Mollisols, permissible in four other orders, but prohibited in Entisols and Aridisols. The concept of the mollic epipedon is not only that of the dark-colored surface horizon of the Chernozem. Rather, it is the concept of a dark-colored epipedon in which there has been decomposition of plant residues underground in the presence of considerable amounts of calcium. In developing the concepts of the orders of *Soil Taxonomy*, we looked for some common feature that would group the soils of the former great soil groups of Prairie Soils or Brunizems, Chernozems, Chestnut soils, and Reddish Chestnut soils. These were soils that had formed under the influence of a predominantly grass vegetation. The only common features that we could find amongst these soils were, the presence of a dark-colored surface horizon of variable thickness and high base saturation. In the U.S., very commonly there was a horizon of accumulation of calcium carbonates, but this was not a universal feature because it was missing amongst the prairie soils or Brunizems.

We had in the previous classification, that had been in use in the U.S., a suborder titled, "Dark Colored Soils of the Semiarid, Subhumid and Humid Grasslands." This suborder was a modification of the classification of Marbut in which he divided all soils into the Pedalfers and Pedocals. In 1938 it was desired to group the prairie soils with the Chernozems on the basis of the dark-colored surface horizons and the grass vegetation. With this rather long traditional emphasis on the grouping of the grassland soils, it is not surprising that when we developed *Soil Taxonomy* we continued to give it an important place in the classification.

Nevertheless, we recognize that there were other dark-colored soils that have low base saturation and that it was always possible, in fact probable, that many of these had received an application of lime adequate to change their former umbric epipedon into a mollic epipedon. This is why the mollic epipedon is permitted in soils that normally have rather acid subsoil horizons. Having reached the decision to use the presence of the mollic epipedon and high base saturation at the definition of the order, we still had some other soils that did not have a grass vegetation but did have a mollic epipedon. Amongst these were the Rendzinas, and some

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17. Retired, formerly Director of Soil Classification and Correlation, SCS/USDA, now in Morgan Hill, California 95037

Brown Forest Soils. These had been considered amongst the others, as intrazonal soils, and there was no readily available order to put them in on the basis of their genesis alone. So we simply included them with the Mollisols as a separate suborder.

There are, of course, serious problems still remaining about the definition of the mollic epipedon. We have many soils that have formed under a swamp vegetation that have a mollic epipedon and that are currently grouped with the poorly drained soils that had formerly a grass and sedge vegetation. Therefore, the suborder of Aquolls has a much wider geographic distribution then do the Ustolls or the Udolls.

Setting the limits for thickness of the mollic epipedon created some difficulties. In a number of soils, the normal thickness of the mollic epipedon is just at the limit of 25 centimeters. This makes considerable trouble for a pedologist who is a purist and wants to classify everything on the basis of whether or not it fits the definition of a mollic epipedon without regard to whether or not the difference of one or two centimeters in thickness is relevant to the purposes of his soil surveys. We also have the problem of the soils with mollic epipedons in the intertropical regions. The definitions of *Soil Taxonomy* are written primarily for the soils of the U.S. and other temperate regions. We point out specifically that we have no good opportunity to test the classification of the soils in the intertropical regions in the U.S. and this testing must be done in other countries. We think, over time, that some of these problems can be worked out through the help of the international committees on taxonomic problems.
Question 5, Leamy

10.2 Concepts and Criteria Used at Different Categorical Levels

In developing concepts and criteria used at different categorical levels, we have tried to keep together in *Taxonomy* soils that are similar enough that we can make some important statements about them. Consider the difference between the Albolls, where we use the albic horizon at the suborder level, and Albaquults, I think where we use it at the great group level. The Albolls are Mollisols that have an albic horizon. The drainage is always impeded to some extent, but they are a group of Mollisols with an albic horizon, and they cover the range from somewhat poorly to poorly drained. They did not want to separate them in the classification, according to the judgment of the field men about how wet they were. The horizons were easy to recognize; one could always, I think, have no problem in getting agreement about the presence or absence of an albic horizon, but great problems about getting agreement about the drainage class. So by separating the Albolls at the suborder level, and giving priority to the albic horizon over the aquic moisture regime, we kept this natural group of soils together in the taxonomy.

The distinction between the Aquults with the ochric epipedon and the albic horizon versus those with the umbric epipedon carry over into *Taxonomy* the old distinction between the Humic Gley and Low Humic Gley Soil of the southeastern states. They seem to think there that these were distinctions important enough to recognize at the great group level. We had used the moisture regime at the suborder level, so the first level at which we could bring in the differences in horizons was the great group level. Suppose we insisted that we use the albic horizon at the great group level, and all soils where it occurred. First, because it does not occur in all soils, we require an extra category to bring it in. Second, if we use it at the same categoric level in all soils where it does occur, then we split what seems to be a natural group of Albolls according to their natural drainage, which again does not always exist today, but is always restricted. These are soils that are naturally wet at some season, and the variability between the best and the worst drained members of the Albolls is not particularly significant so far as one can see.

10.3 Relationships in Soil Taxonomy to Zonal, Azonal, and Intrazonal Soils with Examples from Mollisols

The dominant process for the genesis of the Mollisols is considered to be the formation c the mollic epipedon as a result of underground decomposition of plant residues in the presenc of appreciable calcium. This same process operates in some of the former intrazonal soils, bı not the azonal ones. The intrazonal soils, the former Humic Gleys, have the same dominar process as do the Ustolls and the Udolls. The grouping of the Mollisols differs from Marbut' in that he separated the Udolls from the Ustolls in his highest category--Pedalfers and Pedocal: In the 1938 classification it was decided that the Udolls with their dark-colored thick surfac horizon belonged better with the Ustolls than they did with any other soils; so they wer changed from intrazonal to zonal soils and were included with the suborder of dark-colore soils of subhumid and humid climates. This was a precedent in the '38 classification tha carried over into *Soil Taxonomy* in developing the concept of the order of Mollisols. Marbut for some reason, wanted to classify all soils on the basis of some property, so that he woul have only two orders. We could see no reason to limit the number of orders to two, and i seemed best to try to segregate these dominant sets of processes. **Question 20, Cornell**

10.4 Classification of Eroded Mollisols

The soils that have lost their mollic epipedon through erosion create some questions abou their classification. The philosophy of *Soil Taxonomy* is that a soil should be classified on it: own properties, and not on those that are presumed to have existed at some time in the past nor on the properties of adjacent soils. The use of the mollic epipedon to group the grasslanc soils of the great plains was unavoidable with the knowledge that we had of those soils at the time we developed *Soil Taxonomy*. We did state that we preferred to use subsurface horizons for the definitions of the higher categories because these would be the last horizons to be removed by erosion. There was, however, no criterion that we could find to retain the grouping that existed in the previous classification which called these soils dark-colored soils of the subhumid and humid grasslands. The possible alternative would be to find some characteristic that was common to Mollisols and was not found in other orders besides the mollic epipedons. I do not know what this might be. An alternative approach might be to recall that we are not classifying pedons, but we are classifying polypedons. The pedon is merely a sampling unit of the polypedon. The vast bulk of the eroded areas of Mollisols will have a mollic epipedon as well as pedons that do not have a mollic epipedon. In classifying these soils as Mollisols, when the mollic epipedon has been removed in places, perhaps most places even, it might be possible to write definitions such that when applied to a polypedon, the presence of these less eroded areas would be considered justification for putting the soil into the Mollisol order. This will require some study in the field, and there was no time to do this while *Soil Taxonomy* was being written. This question has been bothering the soil scientists of the midwestern states for many, many years, and we attempted at one time to get a study in Iowa of these soils with statistical controls, and somehow or other we never were able to find funds and personnel to do it. **Question 11, Witty & Guthrie**

In the development of criteria for the mollic epipedon there was no discussion about dropping the color requirements, providing the organic carbon content was at least 0.6 percent for the required thickness. I am quite aware of your problem of Mollisols that have lost most of their mollic epipedon. It is not unique to the U.S., this problem. It occurs in other parts of the world also. Here again, I tried to get some hard-core information about these eroded areas, what was actually present. I could never find out what the problem was, so I made no attempt to solve it without knowing what was there. I thought that, since we are classifying the polypedon and, in the eroded areas that I knew in Iowa, there would surely be a higher percentage of any particular polypedon that retained its mollic epipedon. I thought that potentially it would be possible to derive a definition that would keep the whole polypedon as a

Mollisol even though it has eroded spots. But I could not get the hard information I needed and finally the time came I had to write the book. **Question 46, Minnesota**

In respect to eroded and uneroded Mollisols I suspect you will find very little difference in the percentage of organic carbon between them. One percent is an extraordinarily low limit for a Mollisol and we simply lack the data to develop a sliding scale for a relation between carbon and clay and silt in the mollic epipedons. The one percent limit was established for some soils from the western part of the Great Plains that were fine sands. In cultivation they get winnowed and a good bit of the clay and carbon are blown away but the color remains that of the Mollisol, the uneroded member of the series. The correlators on the Great Plains wanted to keep the series together and one percent was about the lowest level that we could get for the winnowed sand. **Question 73, Minnesota**

The relative number of pedons that are mollic within a polypedon of an eroded Mollisol needs further study. I would like to see some data on those to make up my mind about that point. I doubt that one point, one pedon would satisfy me, but I have a feeling you will find a great many if you take a look. This came up at Lubbock relative to some soils in Central America. They did have some numbers and it was something like 60 percent where the mollic epipedon remains and 40 percent where it was gone. In the case of your eroded Mollisols in Iowa, certainly if you have something like (60 percent) you should classify it as a Mollisol and allow these eroded areas to remain because their behavior is not greatly different from that of the uneroded Mollisols. It's very, very similar and it's a matter of a difference of a few centimeters, maybe 8, between the soil that is properly a Mollisol in *Taxonomy* and one that is not. It may be only 5 cm. However, eroded places might be and often are separated as segments of the polypedon according to the degree of erosion. This is acceptable philosophically, although I would reword some paragraphs in *Soil Taxonomy*. **Question 72, Minnesota**

10.5 Soil Moisture Regimes in Mollisols

Soil moisture regimes are related to the natural vegetation as well as to cropping practices. For example, in central Iowa in the Great Plains there are Mollisols with tall grass vegetation, mixed grasses in eastern Nebraska, and short grasses farther west. The precipitation gradually decreases from east to west. For establishing limits for moisture regimes, Newhall's model for predicting climatic data was used. I don't know of any other method that is being considered. The distinction between the Udolls and the Ustolls included the presence or absence of secondary lime. If the soil had secondary lime within certain depths, it was considered an Ustoll irrespective of the moisture regime. If (there was no secondary lime) it could, I think, be an udic subgroup of Ustolls or an Udoll depending probably on the moisture regime. This doesn't work, say, in South America and in Venezuela. The sediments in the Orinoco basin are dominantly non-calcareous, and it's only on calcareous sediments that you find any secondary lime in the Orinoco basin. In Argentina I have not studied the soils myself, but I am told there are some serious problems also between Udolls and Ustolls. They tell me there are petrocalcic Udolls in Argentina which certainly do not occur in the U.S. So, we have an international committee at the moment working on these moisture regime definitions, particularly with reference to inter-tropical areas. At the same time they can not separate them from the moisture regimes in more temperate climates. They must consider both, but the committee was set up because of serious problems in intertropical regimes. Any recommendations they make there are going to have an impact in temperate regions, so that a committee is going to debate the problems in the moisture regimes and will come up in a few years with some recommendations. What they will be, at this moment, I do not know. **Questions 54 and 56, Minnesota**

In the Udolls around Champaign/Urbana, and some other areas with Udolls, there is some secondary lime. But it's not soft powdery lime, it's hard lime concretions. They're excluded from the definitions.

In the inter-mountain areas the vegetation-moisture regime relationship is obscure. For example, for the Cryoborolls of the inter-mountain region, that if I collected all the series descriptions of the soils in a given family, some were under forest, some were under grass. Forest types might be one thing or another, Ponderosa pine or what-have-you. The vegetation and land use as described for the series varied appreciably from one series to another. I was not happy with what had been done, but I got no proposals for anything from anybody. I thought the best we could do was to start a study of morphology of some of these cryic soils in the west, but I found I had nobody to do it before I retired. Those of you who work with these soils should come up with some suggestion. **Question 58, Minnesota**

In addition to the Desert Project, I started a study on the High Plains for the reason that when I collected all of the descriptions and the data on the Paleustolls, not a one of them fitted the definition. I thought something must be wrong there. We should have had at least one sample of a pedon that fitted the definition of a Paleustoll, we had lots of series classified that way. It seemed logical to move from the desert to the margin of the desert on the High Plains because much of the information we got from the Desert Project was pertinent to the High Plains. **Question 58, Minnesota**

10.6 Proposal for the Classification of Soils Developed in Limnic Sediments with Low Organic Matter Content

The Venezuelan Soil Science Society has pointed out that there are deficiencies in *Soil Taxonomy* regarding the classification of soils developed in limnic sediments. Where limnic and organic soil materials are interlayered the soils are included in Histosols. But where the organic soil materials are lacking, a suborder of Limnents and a great group of Limnaquents are proposed. A limnic subgroup and families of marly and diatomaceous mineralogy are also proposed. Some of the soils involved are strongly calcareous, have a mollic epipedon, and are more appropriately classified as Rendolls rather than Ustolls. For these, Limnic Ustirendolls are proposed.

The situation of the soils formed in the limnic sediments with low organic matter content near Lake Valencia is not unique in the world, though, to the best of my knowledge the soils are not particularly extensive. I have seen somewhat similar soils in The Netherlands where the genesis may have been due to the cutting of the peat for fuel, but at any rate, the soil is composed of limnic sediments with too little organic matter to classify them with the Histosols. To remedy the situation the society should, therefore, submit their resolution to the Soil Conservation Service together with some documents about the nature of these soils. It is specified that the bulk density is low, but what is low? How low? There must be some measurements of the bulk density of the soil. I should also point out that you might, advisedly mention the presence of the cracks in the soil, even though the soils have been out of the bottom of the lake for an appreciable time. The original cracks which appeared at the former level are still present in at least some of the soils that I have been shown. The low bulk density is very apparent in the field, but it is not apparent to someone reading the documents of the Society, unless some numbers are included to document how low this bulk density is. **Question 32, Venezuela**

10.7 Mollisols with Relatively Low CEC

Oxic subgroups of Argiudolls and Haplustolls are based entirely or in part on a CEC of less than 24 meg per 100 g clay by NH_4OAc. The Mollisols of the U.S. and Europe mainly are on late Pleistocene or even Holocene surfaces. Minerals are not weathered to the extent they are in some parts of the tropics. Do the Mollisols have mostly 2:1 lattice clays? It is suggested that the International Committee on Soils with Low-Activity Clays and on Oxisols should consider the classification of these relatively low CEC Mollisols.

The only good examples of oxic subgroups of Mollisols, that I know of, comes from the assembled data on the soils of the former Belgian Congo or Zaire, where we have soils that have properties of Mollisols as they are defined in *Taxonomy*, but that have kaolinitic clays and free oxides for the argillic horizon. These are intertropical soils, I think not necessarily from weathered sediments, but possibly from preweathered sediments. Under the high temperatures and the high rainfalls there, the surprising thing is that one finds Mollisols. Their presence may be due to the vegetation which would mostly be calcium-collecting evergreen forest trees.

I don't know the species or their classification. I have never visited these areas. I think that the collected data from INIAP or INEAC on the soils of Zaire probably will list the botanic names of the native vegetation that was growing when they sampled the soil. The botanic classification is useless to tell me whether it is a tree, grass, shrub, legume, or a non-legume or what have you. I only know it is a plant because the book says so, it is the vegetation. **Question 123, Texas**

10.8 Mollisols in Intertropical Regions

In the key to orders of *Soil Taxonomy* item G.4.C. was designed to exclude some soils with a mollic epipedon and an isomesic or warmer isotemperature regime from Mollisols. The problem was recognized in Puerto Rico, in particular, where we had a Vertisol at the base of a slope which may or may not have a mollic epipedon. The soils were developed from basic rocks and became thinner as we moved up the slope. The soils were clayey with montmorillonitic mineralogy, but they were not Vertisols because the bedrock was too shallow. Going further up the slope we came into rather shallow lithic subgroups of Inceptisols. As we went from the very shallow Inceptisols at the top of the slope to the Vertisols at the base, we had a lot of vertic subgroups that had a mollic epipedon. We wanted to permit these vertic subgroups to be with or without a mollic epipedon. They were all marginal, one way or the other, but we didn't want to force a split in the series as we intergraded from Inceptisols on the upper slopes to the Vertisols on the lower slopes. We wanted to keep that range of soils together in one series. This was the basis for this particular requirement. What we have done there is to define the vertic subgroups. These things could be greatly simplified if they didn't have, here and there, some soils that straddle one of the limits of a diagnostic horizon, and desiring to keep them as a natural unit we had to permit the presence or absence of the mollic epipedon in the Inceptisols. So you will find something parallel to that in the full definition of the Inceptisols. You won't find it in the key because we have already taken care of it under the Mollisols. The Inceptisols are just, "other soils that". It takes all of this verbiage here for just a few hectares of soil, practically. **Question 77, Texas**

10.9 Skeletans in Argillic Horizons and Incipient A2 (E) Horizons

In the stable uplands of Iowa the loess-derived soils may have developed, at times at least, under woodland vegetation succeeded by grasses. In Illinois we discussed the difference between the Tama in one part of Illinois versus another. We have the same differences in Iowa. In some of the Tama, the argillic horizons show very distinct skeletans and, in other kinds of Tama in other areas, do not. We began to discuss this at least in 1930 in Illinois. The work of Ruhe and Walker on the vegetative sequence in Iowa would suggest that at least some of the Tama at one time had a forest vegetation, and these skeletans in the argillic horizons, may date from that time. This was a boreal forest and the skeletans are much more distinct in the Boralfs now than in the Udalfs. So far as I can see, there is this genetic difference within the Tama series in both Illinois and Iowa. We never could make any different interpretations for one kind of Tama than we made for the other and while we have discussed both in Illinois and Iowa about the wisdom of making the separation; nobody has ever seriously proposed that they separate them in mapping. **Question 166, Minnesota**

In an earlier publication, *Prairie Soils of the Upper Mississippi Valley*, the thinking at that time did not consider as Prairie Soils, soils having a lighter-colored eluvial horizon above the argillic horizon, even though the plow layer of the soil was six or seven inches thick and was dark in color. In that paper we were considering the various soils that had been called Prairie Soils, but we knew nothing about those in the western states or on the southern plains. So we specifically titled the paper to eliminate those Prairie Soils from the discussion. Our thinking, at that time, was that those were Gray-Brown Podzolic soils and could be distinguished from the Prairie Soils by the presence of what we then called an A2 horizon. And those, I think, have remained as Alfisols, not as Mollisols. **Question 160, Minnesota**

While there are some soils with a so-called incipient or more recognizable A2 horizon with platy structure; these are disregarded in *Soil Taxonomy*. If the colors, dry and moist, are dark enough for a mollic epipedon, the distinction of the platy structure was not brought into *Taxonomy*. I had long discussions in Iowa about whether or not, say in the loess in northeastern Iowa, we could identify three or four series: the one without any forest influence, the one without any grass influence showing in the profile, then a prairie soil intergrading to a forest soil and the forest soil intergrading to the prairie soil. And the general feeling in Iowa was that we could only recognize one intergrade, not two. And having had those long discussions when we got into the business of writing *Soil Taxonomy*, we did not provide for both intergrades, only for one, the forest soil that still shows a prairie influence. **Question 161, Minnesota**

10.10 Sloping Families of Aquolls, Other Great Groups and Histosols

There are sloping families provided for in Aquolls and Aquults. These are often wet soils; they must be drained for cultivation, and the common practice is to shape these nonsloping soils to provide surface drainage. The sloping members do not require shaping for drainage, although they require some sort of interception tile to cut off the seepage water. The same thing would be true for a good many of the Histosols. If these are cultivated and the polypedon is flat, then normally you have the soil ridged very steeply to provide for a better aerated medium for plant growth. We have other Histosols that are naturally sloping with slopes (in Malaysia) up to 50 percent or more. To get at the series one has to consider the polypedon shape rather than the slope of the individual pedon. **Question 33, Cornell**

10.11 Methods of Determining Base Saturation

There were two reasons why the base saturation for some soils is based on the sum of cations and for others on ammonium acetate extractable cations. One reason we didn't fully understand at the time, but we knew that the difference existed. One was that we had regionalized our laboratories, and in the eastern part of the U.S., where we had most of our Alfisols, the laboratory used the sum of cations to measure the base exchange capacity and base saturation. On the Great Plains where we had a lot of calcareous soils so the laboratory at Lincoln used ammonium acetate extraction, because the sum of cations doesn't work in the calcareous soils. Most of our data on the Mollisols were accumulated at the Lincoln lab where pH was measured and base saturation was measured by ammonium acetate at pH 7. Most of our data on Ultisols were from the Beltsville laboratory where these same measurements were made by the sum of cations. When we began to look at 35 percent or 50 percent or what have you, as a limit that would affect the classification of the series, we could not very well compare the two methods because we had only the sum of cations on the Ultisols and only ammonium acetate on the Mollisols and the Inceptisols. We had a few soils of which we had both. And one of those was the pedon I used in the *Seventh Approximation* as an example of an Ultisol. Now it just happened that that was quite rich in free oxides as well as kaolinite. It had a very considerable pH-dependent charge. So that it went as an Ultisol, if we used sum of cations, and it went as an Alfisol if we used ammonium acetate. Some of the best Ultisols were Red-Yellow Podzolic soils in the southeast at that moment. So without realizing what caused that pH-dependent charge at that moment, we went ahead and said, well, this soil, a representative

Red-Yellow Podzolic soil, is an Ultisol if we use sum of cations and 50 percent by ammonium acetate but where you have a large pH-dependent charge that breaks down and it just happens that that particular soil was one that had a large pH-dependent charge. That's how it happened. **Question 149, Minnesota**

The second reason was we had no data for the Mollisols on base saturation by the sum of cations because in calcareous soils it is impossible or was impossible to determine the base saturation. We could assume the calcareous soil was saturated, but we could not assume what the exchange capacity really was. This was the only method by which we had any data, and so we had to define the method by the availability of the data. In most soils with a low pH-dependent charge, the 50 percent base saturation is equivalent to 35 percent by sum of cations, but if there is a high pH-dependent charge, this relationship breaks down. **Question 76, Cornell**

10.12 Aquic Subgroups

In the definitions of aquic subgroups, depths to 2 chroma mottles are within one meter for Aquic Argiustolls, 75 cm for Aquic Haplustalfs, and 75 cm and the upper 12.5 cm for Aquic Haplustults. I cannot tell you why these different depths were selected because these subgroup definitions were developed in work-planning conferences that I could not always attend. If I did attend one I could only sit in the discussions of one committee. I simply do not know the answer. If it seems irrational and irrelevant to interpretations then changes should be proposed. I think that we must not tie our hands by trying to be completely consistent at this moment. Our only consistence is that we want to get the taxa about which we can make the most important statements and the greatest number of them.

I should point out that when you are dealing with Udalfs and/or Udults the shallow water table can be an impediment to use. When you are dealing with Ustalfs and Ustolls the shallow ground water may be a benefit. In northwestern Iowa where we have a relatively thin mantle of loess over a fine-textured till, the ground-water perches above the till. Crop yields are better because of it, because the soils then retain and can supply more water. These are considered Udolls at the moment but they are getting marginal to the Ustalfs, and I don't have much personal experience with the Ustalfs. **Question 137, Texas**

10.13 Cumulic, Fluventic, and Pachic Subgroups

There are implied differences in locations in the landscape in which soils in cumulic, fluventic, and pachic subgroups occur. Cumulic soils receive fresh sediments at a rate sufficiently slow for organic matter to build up and the mollic or umbric epipedons are thicker than those of the typic subgroup. The carbon content of the soil varies irregularly with depth, or the content is higher than is present in typic soils at stipulated depths (usually 1.25 m), or both. Many of these soils are at the bases of concave slopes where sediments accumulate slowly. But the landscape is not stable long enough for argillic horizons to form. Cumulic subgroups are provided in Mollisols and Umbrepts.

Fluventic subgroups are in less stable areas in which sedimentation is sufficiently rapid that the thickness of the epipedon does not exceed the limits of the typic soils. The organic carbon content of the soil decreases irregularly with depth or it is higher at a stipulated depth than allowed in the typic subgroup, or both. Fluventic subgroups are provided for some Inceptisols and Mollisols. Fluventic and cumulic subgroups were not recognized in the Andepts because it was assumed that these soils would have repeated additions of volcanic ash or pumice. Buried A1 horizons were considered normal in Andepts rather than abnormal.

Pachic soils are in more stable positions in the landscape than cumulic or fluventic soils, but for some, the reason for the pachic soils is not always apparent. Pachic is used with

Mollisols both with and without argillic horizons and with Umbrepts. The thought was at one time this over-thickened epipedon (thicker than normal for the soil environment) may reflect some local variation in moisture availability.

We have, in the Ustolls, some soils that have a much thicker mollic epipedon than their neighbors. As the Ustolls get drier we normally expect the mollic epipedon to thin, but in the regions where normally the mollic epipedon is thin, there are Ustolls with a rather thick mollic epipedon. The reasons for this, at the time we were working on *Taxonomy*, were unknown. As far as I know, they are still unknown. The correlation staff felt that these should be separated from the soils with the thinner mollic epipedons. Soils with thickened mollic epipedons were recognized at the series level, and the correlation staff wanted to carry this to a higher categorical level, so the pachic subgroup was introduced. I'm told, at Lubbock, that these pachic soils are more productive than the others, although they receive, so far as anyone knows, the same precipitation. Precipitation is one of the controlling factors on productivity in the Ustolls.

In the Udolls we don't have this variability in thickness of the mollic epipedon within the U.S., except where it is presumably the result of erosion, post-cultural erosion. In some Udolls of the world, we now have to think a little bit about Borolls instead of Udolls. There are Udolls in Ecuador with a two meter mollic epipedon that runs from sideslope across the ridge and down the other side so it is not due to accumulation of materials as a result of erosion, natural or cultural. Soils recognized in pachic subgroups are principally in ustic or xeric moisture regimes except some Umbrepts and Borolls which have a udic moisture regime. **Question 50, Minnesota**

10.14 Composition of Organic Matter

The Russians use the ratio between humic and fulvic acid in organic matter as diagnostic criteria. We don't have a lot of data in the U.S. on this subject. You have to go to other countries. For example, I have to go to Canada for a moment, where they took a soil, I think it was in Saskatchewan, and with fertilization over a period of a couple of decades, the ratio reversed itself. I believe it's a very unstable thing in the soil. That was the reason, after having looked at what data I could find, I found this reversal of the ratios as a result of cultivation using reasonable fertilization in contrast to the soil under the natural vegetation. It may be that it has a good deal of genetic significance in uncultivated soils. But if we're going to keep the cultivated and the uncultivated equivalents together, it's a difficult thing to use. ORSTOM, the French overseas soils people, commonly make that analysis. They find, between the Mollisols developed in ash and the Andepts, there's a very large difference. Some of the Mollisols in ash have almost a hundred percent humic acid and there's virtually none in the Andepts of Ecuador. This certainly reflects something that has been going on in those soils. The Mollisols are cultivated in Ecuador and have been for some hundreds of years, and the Andepts mostly are left alone and grazed. But there's an enormous difference in this ratio there. You find this in publications of *Pedologie* and in ORSTOM's *Cahier de Pedologie*. **Question 153, Minnesota**

In cultivated soils humic acid, rather than fulvic acid, predominates.

I've never seen such Mollisols as they have in the ash in Ecuador where the clay is pure halloysite. Those soils have been cultivated by the Incas for an unknown length of time, but without fertilization. I talked with one cultivator who was about to harvest his corn, and I estimated that his yield would be about 40 bu/ac. I asked him what fertilizer he used, and he said he had never used any. It strengthened my desire to keep the Mollisols together.

Yes, (at one time in the development of the mollic epipedon concept we had the notion of using the carbon-nitrogen ratio as a part of the definition, but it was abandoned). As a general rule, the C/N ratio in the Mollisols will be 12, 11, 10, something in that range, but we kept finding the exceptions for reasons that are unknown to me, where the C/N ratio went up to 15 or 16, particularly in the Aquolls. And so we thought if we had to go that high it wouldn't

make any particular distinction from other kinds of soil, and we dropped that ratio. As I recall, the very wide ones were always in an Aquoll. **Question 155, Minnesota**

10.15 Hard and Massive Surface Soils

The restriction against a hard or very hard consistence and the massive structure in a mollic epipedon was introduced to keep out of Mollisols certain soils that have a xeric moisture regime in southern California. These soils have what the Australians call a "hard-setting" A horizon such that, if one wants to sample a soil in the summer, he starts with an air-drill such as they use to break concrete pavement. Once you're through the epipedon, digging by shovel is possible. These soils have a color and a carbon content that is just marginally adequate for a mollic epipedon and we wanted to keep them fairly out of the Mollisols and keep them together whether or not there was just a little more carbon or a little less or whether the color value was closer to three than four but lay between. The Mollisols that we know in the U.S. do not present these same problems with sampling or plowing. They are structured enough that they may be plowed when dry, whereas, the ones we wanted to keep out are very difficult. The British groundnut scheme failed because of the nature of the epipedon. They tried to work the soils with big tractors and heavy plows but the plows were destroyed as they would be trying to plow up a concrete pavement. It is the hard and very hard dry consistence of the massive hard-setting epipedons that we want to keep out of the Mollisols.

In South Australia the soil with a hard, massive epipedon is comparable to the cultivated Xeralfs in the U.S. They may disappear over a distance of only three or four miles. We went into more arid climates and there we found soils with argillic horizons, they had a very soft epipedon. The restriction seemed to work on the basis of the soils that they showed me in Australia and in southern California. Ustalfs can do the same thing; they do in Venezuela, at least. As you go from the Ustalf or the Ustult to the Aridisol, the epipedon is first hard, massive and then soft. Experience generally can be utilized as a field criteria where you are just on the margins between ustic or xeric on one hand and aridic on the other. The intent was that it would avoid the necessity of forming judgements about which side of that boundary you were on. Focusing attention on it then causes people to make more observations. If I'd left it out, it wouldn't have been the subject of any studies whatever, even though it is aridic. We did the same thing between the Aridisols and the Mollisols. We said that if you had a mollic epipedon, a Mollisol could have an aridic moisture regime. And in the marginal area between the ustic and udic moisture regimes, we tried to use presence or absence of soft, powdery lime in the profile to put the soil in the Udalfs or Ustalfs. This was all done to avoid the necessity of actually determining the moisture regime. Now, certainly the presence or absence of soft, powdery lime is not a good marker between Udalfs and Ustalfs in non-calcareous parent materials, especially in regions where there is very little calcareous dust in the air. I suspect that several or most of these attempts are going to prove impractical once we've focused attention on them by putting them into *Taxonomy*, and we may have to modify them. It's going to make it more difficult to map. **Question 145, Minnesota**

10.16 Mollic Epipedon in Intertropical Regions

We have recognized while developing *Soil Taxonomy*, that in intertropical regions, the color value of the epipedon is not as well related to the carbon content as it is in temperate regions. We set up the suborder of Tropepts in order to avoid being tied by the distinction between umbric and ochric epipedons in the temperate soils. We have permitted a mollic epipedon in a number of the Tropepts if they have the characteristics of a vertic subgroup. It would be legitimate, in my judgement, to attempt to modify the definition of the mollic epipedon or of Mollisols where soils with mollic epipedons are associated with soils with similar epipedons, except for color, but have the same use potential. Precisely how to do it, I do not know. Some suggestions from those who are familiar with the soils in question would be essential in my judgement. **Question 27, Venezuela**

10.17 Thickness of Mollic or Umbric Epipedons in Typic Subgroups

The maximum thickness of the mollic or umbric epipedon for typic subgroups is not the same in all classes. For most frigid soils and Calciaquolls it is 40 cm, for ustic and xeric moisture regimes it is usually 50 cm, and for Haplaquolls and Hapludolls it is 60 cm. These depths were set by the correlation staff and others. For example, in Calciaquolls the base of the mollic epipedon rests on a calcic horizon with a dry color value of 6 or more. The upper boundary of the calcic horizon is usually above 40 cm. This same depth was considered to be appropriate to limit the thickness of the mollic epipedon in typic Borolls. Because these thicker than typical mollic epipedons were not caused by sedimentation, the soils with these thickened epipedons were classified in pachic subgroups.

We had the general principle that we would not use cumulic in soils with argillic horizons. If the landscape was stable enough, you had an argillic horizon. That, we thought, indicated too much stability for a cumulic subgroup. Well, you may have cumulic or pachic in the haplic great groups and only pachic in the argic great groups. Argiustolls can be pachic, Haplustolls can be cumulic or pachic. **Question 51, Minnesota**

We have provided a cumulic subgroup in the Haplaquolls, but not in the Argiaquolls. Now I presume this goes back to our general decision that we would not recognize cumulic subgroups, even though the mollic epipedon was thick, if the soil had an argillic horizon. This was on the theoretical grounds that the presence of an argillic horizon indicated more stability than the presence of a cambic horizon or the absence even of a cambic horizon. I think most of the Haplaquolls in Iowa would qualify as having a cambic horizon. But not all. The cumulic ones, probably not. The Typic Haplaquoll, I think, would have a cambic horizon. That would be something like a Webster. **Question 52, Minnesota**

10.18 Albolls

In Albolls, we use the albic horizon at the suborder level. The Albolls are Mollisols that have an albic horizon. The drainage is always impeded to some extent, but they are a group of Mollisols with an albic horizon, and they cover the range from somewhat poorly to poorly drained. They did not want to separate them in the classification, according to the judgment of the field men about how wet they were. The horizons were easy to recognize; one could always, I think, have no problem in getting agreement about the presence or absence of an albic horizon, but great problems about getting agreement about the drainage class; so by separating the Albolls at the suborder level, and giving priority to the albic horizon over the aquic moisture regime, we kept this natural group of soils together in the taxonomy.

Albolls are soils that are naturally wet at some season, and the variability between the best and the worst drained members of the Albolls is not particularly significant so far as one can see. **Question 89, Cornell**

Some Albolls in central Illinois, which have either an udic moisture regime or an aquic moisture regime, are keyed out ahead of the Aquolls because they straddle the limits of the udic, aquic, or ustic, aquic moisture regimes. We thought that it was undesirable to split them, but if they have the properties of Albolls; then I see no reason not to classify them as Albolls. Keeping in mind that the Albolls do have xeric subgroups and probably should have ustic subgroups or udic subgroups one or the other, I would prefer the ustic subgroup and fix my concept of Albolls on soils that straddle the limit between udic and aquic moisture regimes. **Question 72, Texas**

10.19 Argialbolls

The upper and lower part of a mollic epipedon may be separated by an albic horizon. This exception to the vertical continuity of the mollic epipedon was introduced to keep similar soils similarly classified. Some of the Argialbolls have an albic horizon within plow depth and some do not. Some of the cultivated ones, then, are going to lose their albic horizon the first time they are plowed. We don't want to change the classification because of plowing, as I have expressed a number of times. We do like to keep the similar soils together when they are marginal on the limit between taxa. The Argialbolls typically have a mollic epipedon that is thick enough to qualify without considering the nature of the argillic horizon below the albic horizon, but a few soils do have a very shallow albic horizon and/or a very thin one. The photograph on page 107, plate 11D, shows that the base of the albic horizon is about 25 cm. From the looks of it, it is about 15 cm to the top of the albic horizon. So if this got plowed just a little deeper than 25 cm, it would lose that albic horizon. Let's go to the Argialbolls to see how we handled that. We didn't specifically address that problem in the key to Albolls, page 273. If we mixed that whole albic horizon up it would drop out of Albolls, wouldn't it, by the present definition. It doesn't have a thick enough mollic epipedon, as an A1 horizon, to qualify as a Mollisol. If we add the A1 plus the upper part of the argillic horizon it would qualify as a Mollisol. **Question 101, Texas**

10.20 Calcic Horizon in Calciaquolls and Calciborolls

The calcic horizon of the Calciaquolls and the calcic horizons in your normal Borolls seemed to have different origins. The calcic horizon in the Calciaquolls, we thought, was due to capillary rise and evaporation from the surface. Whereas, in the Borolls, we thought the calcic horizon was due to downward-moving water and withdrawal of that water precipitating the carbonates. It's quite possible that you can have something that's halfway between. In theory, that could happen, you could get precipitation from capillary rise of a ground water, and you could also have downward movement at another season of the year of the carbonates stopping at about the same point. You could theoretically have a calcic horizon formed as a result of both processes instead of one or the other. But your problems would involve first, a proposal of a subgroup if you think it is necessary that you should have that. An aquic subgroup of Calciborolls is provided in *Soil Taxonomy* but has not been defined because no soils in this subgroup have been identified.

It is very common to find a distinct pattern to the calcic horizon - at the surface in North Dakota and perhaps in northern Minnesota. And in southern Minnesota, Iowa, Illinois they often have the shape of a donut, for example. Or depending on what I interpret to be the water depth, there may be a slight rise in an Aquoll, and you find the Calciaquoll on the rise instead of in the low part of the landscape. You can get it both ways. I've seen also rings in the landscape in the Dakotas where the calcic horizon has the shape of donut around the margins of the depression. Those rings are relatively higher than the bottoms of the depressions. How wet they are I don't have any personal knowledge because I have only seen them in the summers. Where gypsic and calcic horizons are associated, a decision must be made as to which is more important. I think that we took care of that in *Taxonomy*. It can happen and you then decide which one takes priority. **Question 201, Minnesota**

10.21 Borolls

The frigid Ustolls were changed to Borolls at the request of the Midwest Regional Technical Service Center, and I don't recall being in on much of the discussion about it. The potential reason is that it is simpler to control the soil moisture than it is the soil temperature and by putting all the Borolls together you have then a group of soils where one very limiting

element is soil temperature. On that, is superimposed the moisture problem which we take care of at the subgroup level rather than the suborder level. In the *Seventh Approximation* most of the Borolls were Albolls.

The far-western mountain ranges have xeric moisture regimes in their soils at lower elevations. It was assumed that at higher elevations with a cryic soil temperature regime it is so cold that the soils would probably be udic even though the bulk of the precipitation comes in the form of snow in winter. The growing season is short enough when evapotranspiration is important, that the soil shouldn't be dry long enough to get into xeric or ustic moisture regimes. That was an assumption. If the moisture regime is xeric, a proposal for their classification will be needed. We did specify that the cold dry soils were not accommodated in *Soil Taxonomy* for lack of knowledge. **Questions 136 and 139, Minnesota**

10.22 Leptic Natriborolls

The Leptic Natriborolls were provided for because of the feelings of the correlators and the state soil scientists and the experiment station people, primarily in North Dakota and Montana, that they needed a distinction between soils with a very shallow solum and soils with a moderately thick solum. The northcentral regional correlation staff and the work-planning conferences went along with this desire for the Leptic subgroups. When we get to other kinds of soils where we are dealing with different groups of people, the feeling might not have been so strong or might have been absent about the importance of the thickness of what we used to call the solum. We are dealing with, not only different kinds of soil, but different groups or committees of people. **Question 122, Texas**

10.23 Vermiborolls

There's been considerable discussion about the point at which earthworm or faunal activity should be considered in *Soil Taxonomy*. I can give you an example from Europe, not from the U.S. There it is possible to maintain a soil under grass for some hundreds of years particularly in some of the Dutch orchards. And if you have a pit, you find the odd remnant of a blocky ped of an argillic horizon that has not been chewed up by worms as yet. I thought at one time, and still think, we probably need a 'Vermiorthent'. Professor Tavernier in the Near East has pointed out to me in conversation that many of the long-term irrigation soils are extremely wormy and that they need to be distinguished from the soils that have been irrigated for short periods and do not have the faunal activity. The irrigation in those soils is commonly with somewhat muddy water. You get fine stratifications that would make the soil an Entisol where there is no worm activity, but the worms destroy that within a matter of some hundreds of years at least. Now, I have not seen these soils, nor have I seen a description of them, but they came up for discussion at the International Correlation Conference that was held in Syria and Lebanon last summer. The proceedings of that conference will probably have something to say about these soils. In New Zealand I strongly considered the definition of a Vermic epipedon. There the agriculture is almost entirely pastoral on most of the two islands and the worms can multiply. They were introduced and they have multiplied under the permanent grass with high fertilization. They make a problem for us in that the epipedon is dark enough for a mollic epipedon, base saturation is high enough, and the dark colors extend to the depth at which the worms spend the winter. This is just in the neighborhood of the 25 cm that's required for a mollic epipedon. So we get these soils with an epipedon that is mollic to 26 or 27 cm. On the other side of the pit it's 24 cm thick. It's just on the 25 cm limit, and it's causing a problem in the application of *Soil Taxonomy* in New Zealand. It's entirely due to worm activity but an activity that terminates at about 25 cm, whereas the Vermiborolls of the steppes of Russia show intense worm mixing to depths of at least 2 meters. That's the thickness of the mollic epipedon in these soils of the Russian steppes. Those were the ones that caused us to establish the vermic great groups of Borolls, Udolls and Ustolls. We have all three in Europe. **Question 162, Minnesota**

(The reason they have the intense earthworm activity in the Russian steppes and we don't see it here in our grasslands to that degree is because) they have another species of worm. This is the so-called rain-worm of Europe, which we do not have in the U.S. They have been introduced here now, but they were native there and as long as those soils remain under grass, there are enormous populations of earthworms in those soils. When they are cultivated, the population drops, but the evidence of their activity persists. That's *Lumbricus terrestris*. **Question 163, Minnesota**

The way the definition of the vermic groups is written, the disturbance is due to animals but not necessarily to worms. If we begin to find significant numbers of soils that have been disturbed by other kinds of animals, then we might consider changing the formative element in the name from one suggesting worm to something else. What it would be, I would not know. We have a few soils in the U.S. where the disturbance has been due mostly to the prairie dog. I forget where I have seen these, I think Montana. But it was in the northwest somewhere where we have a loess over basalt, and everything has been mixed by burrowing mammals down to the basalt. **Question 164, Minnesota**

10.24 Rendolls

The Rendzinas of Europe form pretty much our central concept of Rendolls. Originally, Rendzinas were considered to be intrazonal soils whose characteristics are due to the parent material rather than to the climate and the vegetation. Rendolls are dark-colored soils resting mostly on marl in a humid climate. The Chernozems are not considered to be as Rendzinas. Some pedologists who visited Texas identified some of the dark-colored soils on limestone with an ustic regime as Rendzinas, although there was a marked difference in these soils from the Rendzinas of Europe, in that the Texas soils had a pronounced horizon of carbonate accumulation. These soils reflected then, the climate, not the bedrock. I found that the Texans had Rendzinas all over Texas wherever the soil was shallow on limestone. These would have been dark-colored soils irrespective of the nature of the rock, just as in Iowa, what was called a Prairie Soil, shallow over limestone, would have to be called a Rendzina because it had no horizon of carbonate accumulation. These would be quite unlike kinds of soil. However, variability at the suborder level could have been handled at the great group level. It was mostly to keep the grassland soils together and separated from the forest soils of the humid regions that we restricted the Rendolls to soils that have a udic moisture regime. The soils on limestone with ca horizons and an ustic moisture regime were then clearly separable from the Rendzinas of Western Europe.

In the absence of carbonates in the parent materials we don't find much secondary lime in the intertropical regions. In Venezuela there are soils developed on calcareous parent materials that retain some lime. The rainfall at Maracay is something like a 1,000 mm in a six-months rainy season. It is enough to saturate the whole soil, but it does not seem to be enough to get the lime out of a moderately calcareous parent material. This is my only experience with secondary lime in the intertropical regions. In the West Indies, I do not at this moment recall any Calciustolls. **Question 119, Texas**

Rendolls are extensive in Western Europe. They are relatively inextensive in the United States, if one judges by the numbers of series that have been classified as Rendolls. They are quite extensive in parts of France and Belgium. The Paris Basin is very largely composed of Rendolls. According to Calhoun in Guatemala, there are extensive areas of Rendolls, although I thought they might be dark-colored Vertisols. **Question 120, Texas**

The Venezuelan Soil Science Society would like to modify the definition of Rendolls as given in *Soil Taxonomy*. This will be a disputable proposal because the soil survey staff in the U.S. has gone through this particular argument before, where there are soils in ustic moisture regimes with very prominent segregations of secondary carbonates, soils that are now classified as Calciustolls. At one time, someone from Europe went through Texas and told the Texans that these were Rendzinas and so this was accepted by the Texans and they started the argument

about whether a Rendzina could have a calcic horizon. Many of theirs do, and those that do not have a calcic horizon have distinct accumulations of secondary carbonates. There is no harm in making this proposal to the Soil Conservation Service but it will be disputed more than the proposal for the classification of the soils that have the low bulk density, the high infiltration, the cracks and so on, soils that do not fit comfortably into any family that now exists in *Soil Taxonomy*. **Question 32, Venezuela**

10.25 Carbonates in Udolls

In Udolls we only prohibit soft powdery lime, we don't prohibit accumulation of carbonates. I don't know in what form you find the carbonates in Minnesota and northern Iowa. Plenty of Aquolls in Illinois have horizons of lime concretions or large amounts of lime concretions, but they're always too hard for our definition of soft, powdery lime. We made the genetic assumption that in an udic environment an accumulation of lime would be in the form of concretions. That assumption may not have been warranted. Removal of this distinction is being discussed. **Question 202, Minnesota**

10.26 Paleustolls

The Udic Paleustoll is defined as having secondary lime at greater depth than the Typic Paleustoll. This was a serious mistake in *Soil Taxonomy*. It does not work in the rest of the world if the parent materials are not calcareous to begir with. In the U.S., in the steppes of the Soviet Union, all parent materials practically are calcareous, and the depth of the accumulation of secondary lime is related to the penetration of the rainfall. If one goes into a wet/dry climate in intertropical or subtropical regions, the relationship breaks down completely. I surely have proposed that this definition be modified, or that the definition of the ustic moisture regime be modified. One or the other is essential. We have now in the U.S., I am told by Dr. McClelland, aridic, typic, and udic Paleustolls associated in the same landscape, depending on the carbonate content. There are no differences in interpretations for those three subgroups, whereas there should be serious differences of interpretations. The udic subgroup should imply that the rainfall is higher than that of the typic, and the aridic should imply that the rainfall is less than that of the typic. The aridic subgroup is defined in terms of the soil moisture rather than depth to carbonates, which I think is proper. But the udic subgroup is mystifying. **Question 92, Cornell**

Chapter 11

OXISOLS

reviewed by H. Eswaran[18]

11.1 Historical Concept of Oxisols

The concepts of Oxisols emerged rather gradually in the earlier approximations. At one time, we separated the soils in the highest category according to whether or not they had horizons. They had an A horizon that was very distinct or they had an A horizon and a B horizon, or currently a cambic horizon, or they had a B horizon of accumulation of clay or of amorphous materials. In the *Sixth Approximation*, we adopted the concepts of the diagnostic horizons rather than the A, B, C horizons. The very strongly weathered soil horizons that we have in Oxisols were recognized as a special kind of B horizon, using the concept of the Latosols of Dr. Kellogg. This concept was very similar to that of the present concept of the oxic horizon. There was no big change in concepts, there was only a change in the application of the concepts. The soils with high variable charges developed from pyroclastic materials in Hawaii, were called Latosols. The only generally common feature of soils that were called Latosols that I could discover, seemed to be that they occurred in intertropical regions. Nearly everything was called a Latosol in the soil survey of Hawaii. These included the present Andepts or proposed Andisols as well as the Oxisols and Ultisols. At the time that we were working on the development of the *Seventh Approximation*, we had virtually no data on the chemical properties of the soils of Hawaii other than their total analyses. The first concepts we had of the Oxisols, then, were a mixture of what we now call Oxisols and Andepts.

It took some years to straighten out these differences. We recognized early that we needed an order for a kind of soil such as the Nipe of Puerto Rico, soils that consisted of completely weathered materials. The Nipe would be a good example, I think, of Dr. Kellogg's concept of a Latosol, although his concept was broader than that. He included soils such as Nipe along with soils that have distinct argillic horizons. The original definition was in descriptive terms, not in quantitative terms. Dr. Kellogg spoke of the low activity of the clay, but did not specify what that was, and what is low to one person may be high to another; it depends on their experience and training. In developing *Soil Taxonomy*, it was recognized early that definitions could not be uniformly interpreted if they were written in qualitative terms rather than in quantitative terms. Qualitatively what is high and what is low in any property depends entirely on the experience of the individual who is trying to classify a particular soil, and it was our goal that the definitions would be such that competent pedologists, using the same information, would arrive at the same classification of the soil. **Question 1, Eswaran**

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18. Project Leader, SMSS, SCS/USDA, Washington, D.C., 20013.

11.2 Oxic Horizon

As has been pointed out in *Soil Taxonomy*, some consider the oxic horizon as B, and others as a C horizon. In the recent French approach of Segalen, they prefer to consider it as a diagnostic material, which perhaps is also correct, as we are in the process of making changes, we would like to know the reasons for considering it as a diagnostic horizon, and would there be any advantages for considering it as a diagnostic material? **Question 4, Eswaran**

We should, note in answer, that *Soil Taxonomy* has used the concept of the nature of the material of the soil at the order level in some soils, as in Vertisols, and in some suborders, as in Andepts. However, the general philosophy of *Soil Taxonomy* has used horizons to arrange and define the orders and has used other features, such as moisture regimes, nature of the material, and so on, at the suborder level. One could, as Segalen has proposed, use the nature of the material which forms the horizons, rather than the nature of the horizons themselves. This, however, has not been done. We have used the spodic horizon to identify the Spodosols. We have used the mollic epipedon as one identifying horizon of Mollisols. It is of no material consequence whether one considers the oxic horizon as a horizon or as material, because the horizon is defined in terms of the materials that compose it.

It would be very difficult for me to assert that most Oxisols are developed on preweathered and transported materials. It is true that the material must be physically weathered before it can be transported, but whether or not the oxic horizon has formed in materials which were weathered physically, or both physically and chemically, is currently only a matter of speculation, and cannot in any way be used as a part of a definition. **Question 4, Eswaran**

There are some numbers given in the definition of the oxic horizon. It will be useful to have some remarks on the origin of these numbers.

(a) 30 cm thickness,

(b) ECEC of less than 10,

(c) CEC 7 of less than 16,

(d) More than 15% clay (why not 18),

(e) 5% rock structure. **Question 7, Eswaran**

11.2.1 30 cm Thickness

The minimum thickness of an oxic horizon was set with the notion that the oxic horizon was resting on some sort of saprolitic material. We have prohibited in *Soil Taxonomy*, a cambic horizon that overlies an argillic horizon because, it is really a transition between the epipedon and the argillic horizon. We had the same thought that a material that is transitional between the epipedon and the argillic horizon would not be called an oxic horizon; even though it has the properties of an oxic horizon, it is a transitional horizon, and so we put the 30 cm limit of thickness on the oxic horizon with the notion that it would not be a transitional horizon between an epipedon and an argillic horizon. We also thought that if the thin oxic horizon rested on saprolite, which either retains weatherable minerals or has rock structure, some minimum thickness was required. Otherwise, people would begin to find an oxic horizon that was one cm thick or a half cm thick, and the 30 cm comes from the notion that the oxic horizon should be thick enough to have some significant effect on plant roots. **Question 7, Eswaran**

11.2.2 ECEC of less than 10

The ECEC, which is the sum of bases extractable by ammonium acetate and the sum of aluminum extractable by KCl, was used in the definition of the oxic horizon because we felt it was easier to determine with precision than the CEC by ammonium acetate buffered at pH 7. Different laboratories frequently get what appear to be significantly different CEC's of the same horizon by ammonium acetate. The sum of bases plus the KCl-extractable aluminum can be measured, we think, with more precision than the ammonium acetate CEC.

The limit of 10 was selected because in the limited data that we had for soils of the United States, this was about the maximum that we could find in the soil that we thought belonged with the Oxisols. With more data from other parts of the world, it may be desirable to modify this number. While it was proposed for criticism, no criticism was ever received, nor were there ever any suggestions for changing the numbers. Therefore, what was proposed for criticism became a number that appeared in *Soil Taxonomy*. **Question 7, Eswaran**

11.2.3 CEC 7 of less than 16

The CEC by ammonium acetate of less than 16 was proposed again for criticism and was never criticized. The reason for the 16 was precisely the same reason as the 10 for the ECEC. **Question 7, Eswaran**

11.2.4 Low-Activity Clay Concept

I suppose the basis for the 24 milliequivalent per hundred gram clay limit for low-activity clays refers to the oxic subgroups of various taxa in the classification. This has come up before on the 16 meq limit for Oxisols. We did not have enough data in the United States to have any basis for making a proposal.

We knew that some was needed, some sort of limit, and we got this 24 meq limit from the Brazilian pedologists who have to deal with these soils in huge areas. The basis that they used for this 16/24 meq limit was just the way that they grouped their soils. They thought that it made natural groupings of the Brazilian soils and having no other basis for proposing a limit we took the limits that they were using at that moment. They were concerned with the distinction between what they called Latosols and Red-Yellow Podzolic soils. They wanted intergrades between the two great soil groups. The limits that they were using were limits that seemed to reflect, to them, natural groupings of the variability that existed within Brazil. **Question 83, Cornell**

11.2.5 More than 15% Clay (Why Not 18)

The limit of 15% clay as a minimum for an oxic horizon, was proposed because we were concerned with a limit between Oxisols and Quartzipsamments, which may also be completely weathered. The intergrades then, or the limit was to separate an Oxic Quartzipsamment from a Psammentic Oxisol. We chose 15% clay on the assumption that material so completely weathered would have virtually no silt. In Venezuela, we have soils that have less than 15% clay, but have too much silt and clay to become Quartzipsamments. They, therefore, come out from the key as Entisols, although they are completely weathered, and may be very stable in the landscape. I do not like the idea of having an Entisol that represents really an intergrade between an Oxisol and a Quartzipsamment. This seems to me to be unreasonable, so I have proposed that, that limit be dropped completely, and that the limit between the Oxisol and the Quartzipsamment be set at the limit between loamy sand and sandy loam particle-size classes.

A limit of 18% would be a change in the wrong direction, because it would increase the area of Entisols that lie between the Oxisols and the Quartzipsamments. **Question 7, Eswaran**

It has been stated that some soils in Thailand have red color, diffuse boundary, very high porosity but do not have weatherable minerals, or clay coatings and the subhorizons fit all the requirements for an oxic horizon except the clay parameter and they consider that it is not comfortable to identify this horizon as a cambic horizon because the soil is old and highly weathered. Further, sandy soils with about 5 percent clay content have all the properties of Oxisols, except the clay content, and really these properties give them a very special nature and behavior. There are extensive areas with sandy soils that have, besides the property of sandy soils, marked characteristics that identify them as Oxisols. Should we not permit very sandy soils in the order of Oxisols? **Question 13, Leamy**

It would be possible to permit the oxic horizon to have a texture of sand and to lack all weatherable minerals. In this case, the horizon would consist of quartz, free oxides, perhaps traces of 1:1 lattice clays. However, the Oxisols grade into the Quartzipsamments and if we include in Oxisols, soils with 5 percent clay, then we must find some limit between 5 and 5 to 10 percent clay to stop the oxic horizon and go into Quartzipsamments because Quartzipsamments are frequently highly weathered and the clay fraction consists of kaolinite and free oxides. There must be some sort of limit between the oxic horizon and the Quartzipsamments, because on the landscape they grade one into another, particularly in Zaire and other parts of southern Africa. **Question 13, Leamy**

I have proposed that the limit on clay be dropped and that a limit on texture be substituted. Namely, the oxic horizon should have a sandy loam texture and the Quartzipsamments should have a sandy texture. The taxonomy provides for oxic subgroups of the Quartzipsamments and ones within the landscape go from an Oxisol to an Oxic Quartzipsamment and finally to a Typic Quartzipsamment in which the sand grains are largely uncoated. The clay limit was inserted originally in order to make a break between the sandy loams and the sands, or loamy sands on the assumption that the highly weathered soils have virtually no silt. However, we have found a number of soils that have no weatherable minerals that have less than 15 percent clay actually 10 to 12, and have enough silt that they have a sandy loam texture rather than a sand or loamy sand texture. To classify these soils as Quartzipsamments is impossible because of the sandy loam texture. To classify them as Oxisols is impossible according to the limits in *Soil Taxonomy*. They become orphans, and knowing something about the soils, it seems obvious they belong better with the Oxisols then they do with the Entisols, which is where they would have to be put if they had no diagnostic horizon. We have found such soils in Venezuela, and it seems very likely that they will also occur in other parts of the world. For the most part, the Quartzipsamments have more than 99 percent unweatherable minerals in the silt and sand fraction, although the limit in *Taxonomy* is set at 95 percent. They represent soils that may be very recent in origin, occurring on coastal dunes where the sands on the beach are almost pure quartz. They occur on very old landscapes where the sands have been in place for a long time and have had all of the weatherable minerals removed. They also occur as greatly over-thickened albic horizons with an underlying spodic horizon that is more than two meters deep. Most of such soils with the thick albic horizon or those on the recent dunes, are almost totally lacking in clay and dominated by quartz. The intergrades between the Oxisols that have a sandy loam oxic horizon and the Quartzipsamments, that are almost completely lacking in clay, must find some place in the taxonomy. The Psamments were distinguished from other soils on the grounds that they have some very specific physical properties. They are, when dry, subject to blowing and drifting. When dry they are also very difficult to traverse with wheeled vehicles. The Oxisols on the other hand do not have these specific properties. So we need a limit somewhere between the Oxisols and the Psamments including Quartzipsamments that is based on the point at which we begin to develop these particular properties of Psamments. It seemed reasonable to us when we developed the taxonomy, since there is a continual gradation between Oxisols and Quartzipsamments, to have some limit that recognizes the point at which we begin to develop trafficability and blowing problems. This was the basic reason for the 15 percent clay limit, which we know now was wrong because the presence of the appreciable silt plus clay does not produce the peculiar properties of the Psamments. **Question 13, Leamy**

All oxic horizons have relatively low available water-holding capacity whether they're loamy or clayey. This is one of the principal defects of Oxisols. We originally put a restriction on the lower limit of the clay content of an oxic horizon at a point where we thought it would distinguish sandy and loamy soils. We were mistaken. We went on the assumption that, in these extremely weathered soils, there would be very little silt present. But, when we looked at the data from laboratories, we find that there sometimes is an appreciable amount of silt measured, almost totally quartz. This may be actually present in the soil in nature, or it may be a laboratory artifact, I do not know which it is. But, we do know that silt and clay can be generated by dispersion processes and mechanical analysis. Just to simplify the business of how much silt is or isn't present, I simply propose that we drop the clay content completely and substitute the difference between the sandy material and the loamy material. The sandy material cannot be an oxic horizon because we want to have an intergrade between the Quartzipsamments and the Oxisols. We get very sandy Oxisols, and we get very strongly weathered Quartzipsamments, and we wanted to have the Oxic Quartzipsamments as well as the psammentic Oxisols. We thought there was plenty of room there, management wise, for the two central concepts and one intergrade on each side of the boundary. The simplest way to define that boundary and to avoid the silt problem is just to say loamy or sandy. **Question 43, Texas**

I should like to add that we have had laboratory problems in applying the definition of the oxic subgroup of Quartzipsamments. We have, in the soils in Zaire, analyses of the clay fraction and we find there nothing but iron oxides, kaolin and quartz, and yet the measured CEC's relative to the measured percentage of clay is 20, 25, 30 milliequivalents. This is a laboratory artifact of some sort, it's not the nature of the soil. Similarly with some of the more sandy Oxisols where we had a provision that required 16% or more clay in the oxic horizon, we have another laboratory artifact. We assumed there was no silt in such soils of any consequence, but in the laboratory as a result of this version a good bit of the coarse sand is broken down to silt, and so we come out with measured sandy loams that have less than 16% clay. The proposal was made to the Soil Conservation Service as a result of the Zaire data that we drop the reference to the cation exchange capacity of the clay fraction and substitute the mineralogy of the clay fraction in its place. And as a result of the Venezuelan data we proposed that we permit Oxisols to oxic horizons to have less than 16% clay if they have a sandy loam texture. These proposals have accumulated in the Soil Conservation Service, but I believe now that they have one man who is responsible for soil classification that we will begin to see approvals of these proposals. **Question 49, Venezuela**

We prepared, about 10 years ago, a manual of field soil survey investigations showing the things that the fieldmen could do in their offices without requiring the existence of services from a laboratory. One of the tests that we described is one for the estimation of the percentage of quartz in the sand fraction. This is based on covering the sand fraction with a liquid that has the same refractive index as quartz. When one does this, the quartz becomes invisible and the other kinds of sands remain in plain sight in the liquid. **Question 13, Leamy**

11.2.6 5% Rock Structure

The limit of 5% by volume of rock structure in the oxic horizon was set to exclude from the oxic horizon, materials that were completely weathered chemically, but were not yet physically weathered. We want to restrict the Oxisol to the oxic horizon, to a material that was completely weathered, or nearly so, both physically and chemically.

This limit was proposed for criticism, and never received any. Therefore, it has come on over into *Soil Taxonomy*. One can find a weathered basic igneous rock that has been completely altered, mineralogically. The primary minerals have all been altered, and yet it may be so hard that one must use a hammer to break it. We did not think that this material should constitute a part of the oxic horizon. It is not in any sense a part of the soil; it is, rather, the bedrock. **Question 7, Eswaran**

11.3 Key to Oxisols

11.3.1 Soils with Argillic Horizons

Argillic horizons are accepted in profiles which belong to the order of Aridisols, Mollisols, and Vertisols. Other properties (moisture regime, mollic epipedon, spodic horizon, etc.) were considered more important and given more diagnostic weight to create orders than the argillic horizons. This was not the case when the oxic horizon was considered, why? The *Seventh Approximation* (Soil Survey Staff, 1960) gave the prominence in the key to soil orders, to either the argillic or the oxic horizon. Finally, in *Soil Taxonomy* (Soil Survey Staff, 1975) the argillic was given priority over the oxic. Why? **Question 14, Leamy**

I should point out first that the emphasis to the argillic horizon over the oxic horizon applies only to the soils in which there is an argillic horizon overlying the oxic horizon. An argillic horizon underlying an oxic horizon is not grounds for keeping a soil out of Oxisols. **We generally use the principle in developing *Soil Taxonomy* that, if we have two subsurface diagnostic horizons in the soil, the preference is given at the higher category to the horizon nearest the surface. Thus the soil with both a spodic horizon and an argillic horizon is normally classified as a Spodosol because the spodic horizon overlies the argillic horizon and the assumption is, that the more recent processes that dominate in the genesis in the soil produce the diagnostic horizons closer to the surface than the older process, which produce the diagnostic horizon at a greater depth. This assumption is consistently used in the various orders where we have the two or more diagnostic subsurface horizons.** There is no distinction between the use of the argillic horizon in Spodosols and in Oxisols. I suspect these questions about the argillic horizon's significance to soil classification arise from a failure to read carefully the full text of the discussion of the argillic horizon. On page 20, under the heading, 'Significance to Soil Classification', there appears this statement, "It is stressed that the argillic horizon is no more important to soil classification and to soil genesis than many other horizons. It has been used at a higher categoric level in some parts of the system only because that use has produced groupings of soils that have the largest number of common properties that are important to use of the soils." **Question 14, Leamy**

An Ultic Haplorthox is frequently misclassified as an Oxic Tropudult. People ignore the sentence (page 329 *Soil Taxonomy*), "An appreciable increase in the percentage clay with depth is a property shared with Ultisols, and defines the ultic subgroups (in Haplorthox etc.)." The subgroup and the explanatory sentence emphasizes the fact that the clay increase by itself is insufficient to identify an argillic horizon. If the subsurface horizon has oxic properties, it is an oxic horizon, and so will be keyed out as an Oxisol. Indirectly, it implies that the oxic horizon has priority over the argillic horizon. Is this the intent?

The soils that have finer-textured subsurface horizons appear to be giving considerable trouble in the field. The pedologists seem to be unable to agree generally, as to whether or not this subsurface horizon is an argillic horizon. The problem has received much discussion from ICOMLAC, and it is quite likely that some changes in the definition of the Oxisol will be needed and will be proposed by ICOMOX. **Question 13, Eswaran**

The definition of Oxisols has created problems, especially with people who are not aware of the intent. The first problem is the classical question "Where does an argillic horizon end and an oxic horizon begin, or vice versa?" Take the classical situation in Malaysia. The pedon has an A1 of about 10 cm, a B1 which meets all the requirements of an oxic horizon and is 40 cm thick, and this is underlain by B21t, B22t, etc. We happily called this pedon a Tropeptic Haplorthox until, during a recent workshop, some classified it as a Typic Paleudult.

We have two precedents in *Soil Taxonomy* for handling this particular question, where the transition horizon overlies the argillic horizon, and has all the characteristics of an oxic horizon. The first precedent is that of the cambic horizon, which by definition may not overlie an

argillic horizon, unless it is separated from it by an albic horizon. The other precedent is where we have a spodic horizon that overlies an argillic horizon. In this case, the horizon is not transitional, and the order is determined by the overlying surficial horizon, on the assumption that that represents best the present processes going on in the soil. In dealing with the material horizon that has the properties of an oxic horizon, but rests on an argillic horizon, it is possible to use either of these precedents. The limit of 30 cm thickness, mentioned under question 7, was set without thought that this would be a transitional horizon. In the discussions of ICOMLAC, I proposed that this limit be increased to 50 cm on the grounds that if it is that thick, the soil would behave more like an Oxisol than like an Ultisol. In this situation then, one could establish an ultic subgroup of Oxisols to separate soils with this horizon sequence at the subgroup level rather than at the order level. **Question 8, Eswaran**

11.3.2 Soils with a Spodic Horizon

According to the definition, a spodic horizon is not permitted to lie over the oxic horizon. Can such a situation occur in nature? If not, why does the statement appear in the definition?

I have seen in the Amazon, soils that have a spodic horizon overlying what appears to be an oxic horizon, though I have no data on the soils in question other than my visual and manual observations. The soil in question, probably at one time, was an Ultisol, with a rather thick epipedon of a loamy sand or sand texture. With great age, the argillic horizon seems to have been degraded into an oxic horizon, but the thick sandy epipedon was favorable for the formation of a distinct spodic horizon above the oxic horizon. In accordance with the other taxa in which we have a spodic horizon overlying another horizon, we would assume that the current processes probably are those that lead to the strength of that spodic horizon, and therefore we would put it into the order of Spodosols, and establish a subgroup of oxic Spodosols, to distinguish these soils from the alfic and ultic Spodosols. **Question 9, Eswaran**

11.3.3 Lack of Color Qualifications

Why was not rhodic or similar color connotations used in the Oxisols?

They were not used in Oxisols simply for lack of information about them. We just did not know what was important in Oxisols. Most of our Oxisols were quite red having come from basic rocks, and we had no other experience to go on. No one suggested any changes in the concept of the classification of the Oxisols in the *Seventh Approximation*. I just got no comments. **Question 48, Cornell**

Wet oxic horizons are frequently mistaken for cambic horizons, primarily because of a color difference with the "C", and an apparent better structure. This may be one explanation for the lack of Aquox descriptions. Do you see this as a real problem, and how can we rectify it?

I think, perhaps, the principle reason for the lack of Aquox descriptions is the small areas that they occupy in the world. The Aquox that I have seen have normally been small, polypedons, a matter of a few hectares at the most, and they are generally far apart in the landscape. They do exist, and a lack of description probably reflects the facts that their area is extremely small compared to the areas of the other kinds of Oxisols. I think that one would not have much trouble in identification of the Aquox, if one finds a wet soil surrounded by other kinds of Oxisols. Its position in the landscape should be enough to guide the pedologist in his classification, even in the absence of any laboratory data. **Question 6, Eswaran**

11.3.4 Plinthite

Why was plinthite at shallow depth made a defining criteria for some kind of Aquox? The way the order is defined makes it possible for a soil to be an Aquox by having plinthite, and without an oxic horizon.

The soils that have plinthite at a shallow depth were included with Oxisols in an attempt to keep them all in one part of the taxonomy, irrespective of what underlay the surficial plinthite. These soils were thought to be of extremely small extent. They have been described to me from Africa, but I have never seen them myself. They lie, for the most part, on a colluvial slope below an escarpment that is protected from retreat by petroplinthite or some other form of hardened ironstone. They contain large amounts of ironstone, but they receive seepwaters from the soil above, and are thus kept wet. If cleared, the plinthite hardens at the surface and the soil is destroyed for the growth of plants for an almost unlimited time. Our feeling was, then, this characteristic overshadowed all others, and they should be kept together in the taxonomy in one order or another, and since they commonly are associated with Oxisols, we put them in the order of Oxisols. **Question 10, Eswaran**

Why is plinthite near the surface with an aquic moisture regime, included with Oxisols without regard to the presence or absence of an oxic horizon?

We know very little about the soils that were intended to be included in the superic subgroup of Plinthaquox. These are the soils that are reported to have the plinthite at the surface. They occur normally at the base of a slope where there is an outcrop of petro-litho-plinthite above. They receive seepage rich in iron and the plinthite reforms and recements the petroplinthite that has been transported down slope. If cleared, these soils form an iron crust at the surface and are permanently useless. The intent was to keep all these soils together because the hazard of removing the forest from these soils is enormous and we do not know the kinds of horizons that we find in them. There are no studies reported of these soils in the literature, only reports from pedologists who have seen them in passing. It is simply a matter of keeping together the soils that have this over-riding problem that precludes the clearing of the forest without permanently destroying the productivity of the soil. **Question 18, Venezuela**

Plinthite is formed by the reduction, movement and segregation of iron oxides in a soil in the presence of a fluctuating water table. The iron can be mobilized and segregated much more quickly than many of the soil minerals can be destroyed by weathering, or altered by weathering to kaolin and free oxides. We have plinthites in a number of parts of the world in which the mineral portion, in addition to the free iron consists of weatherable minerals, even of calcium carbonate. In this situation, we find the plinthite in the Inceptisols and the Alfisols. A new cycle of weathering can begin to remove the iron oxides from the plinthite, leaving the matrix rather rich in weatherable minerals. It is a mistake to relate plinthite to the oxic horizon, although by error in *Soil Taxonomy*, we said that it is highly weathered. This was an error.

Professor Armand Van Wambeke has reported verbally to me that in his studies in the Amazon basin in Colombia, he found many Inceptisols with plinthite and even with petroplinthite. The plinthite and petroplinthite there were rich in weatherable minerals such as feldspars and micas. Professor Frank Moormann, working in Southeastern Asia, has reported to me verbally that he has found many areas with petroplinthite which contain free carbonates in the interiors of the ironstone nodules. **Question 41, Venezuela**

11.3.5 Aridic Moisture Regimes

Not all soils with aridic moisture regimes are classified as Aridisols, even though the introductory statement in *Taxonomy* for that particular chapter states that these are..."soils that do not have water available to mesophythic plants for long periods." Why was it decided to

exclude arid climate soils with oxic horizons, vertic properties or no diagnostic horizons from the Aridisols order?

The first point, on the soils without diagnostic horizons, was that they came originally from the concept of the azonal soils. They were soils without diagnostic horizons and we wanted to keep them together as an order, because without any subsurface diagnostic horizons there are really no statements you can make about the Entisols, except that they lack subsurface diagnostic horizons. The statement is not very important to the soil survey. **Question 64, Texas**

The arid climate was shown only at the great group level because, in the Entisols, we wanted first, the suborder level to sort them out according to the reasons why they had no subsurface diagnostic horizon. For example, there is a big difference between the Orthents and the Fluvents, and their agricultural importance. Perhaps more people in the world get their food from Fluvents than any other single kind of soil. The exclusion of the Oxisols that have an aridic moisture regime was primarily because they will, under irrigation, behave like other Oxisols. We would have all of the difficulties that you would expect from management of other Oxisols from that group. We might as well keep them together as Oxisols. In that situation we could deal with the arid climate at the suborder level instead of the great group level because they seem to be the most important subdivision of the Oxisols according to their soil moisture regime. **Question 64, Texas**

The Torrox presents a conceptual problem which needs your remarks. Conceptually, Aridisols are soils with aridic soil moisture regimes, and with a diagnostic subsurface horizon. If they are recent soils with no diagnostic subsurface horizons, they go into Entisols - Torriorthents and so on. But if they have an oxic horizon and an aridic moisture regime, they go into Oxisols. Why not Oxids instead of Torrox? If they have andic soil materials and an aridic soil moisture regime, they cannot go into the new Andisols, but instead go into Aridisols. Do we have a conceptual hiatus?

It would be possible to put the soils that have an oxic horizon and aridic soil moisture regime into either Oxisols or Aridisols. They were put into Oxisols rather than Aridisols on the assumption that if irrigated, they would behave more like Oxisols than like any other Aridisol. They do differ enormously in their properties from the vast bulk of the Aridisols. **Question 11, Eswaran**

11.3.6 Gibbsic Properties

Some Gibbsiorthox have also acric properties, and from a management point of view, the latter is a more limiting factor. It appears desirable to key the Acrorthox earlier, and provide gibbsic subgroups. Any particular reason why the present key was preferred?

Only two series of Gibbsiorthox have been recognized to date in Hawaii. None have been recognized in Puerto Rico. They are known, however, to occur in other islands in the South Pacific. Both of the series of Gibbsiorthox in Hawaii have a higher pH in KCl than in water, and are considered to have a net positive charge. This is obviously important from a management point of view because of the relative inability of such materials to retain bases against leaching. However, also from a management point of view, the Gibbsiorthox have multiple sheets of gibbsite with root mats above the gibbsite sheets. These sheets behave as do the thin iron pans called placic horizons, and other kinds of pans, although we have not defined them as a pan. This is, perhaps, the principle reason why the Gibbsiorthox were not included with the Acrorthox, which do not have these pans. **Question 14, Eswaran**

Chapter 12

SPODOSOLS

reviewed by F. T. Miller[19]

12.1 Spodic Horizon - Identification and Characterization

The spodic horizon is one in which active amorphous, organic-sesquioxide material accumulated. It usually lies below an eluvial mineral horizon. The identification of a spodic horizon can be chemical and it can be morphologic, something you can identify in the field. **Question 179, Minnesota**

There is a gradual transition from soils with cambic horizons to soils with spodic horizons. We had therefore, a lot of trouble in drawing a boundary between the Spodosols and Dystrochrepts of New York State. It is the reason why, when we tried to write our definition, we came to New York State. We had Dr. Cline classify the soils as he thought they should be classified and then we took the samples to the laboratory to see what criteria would make this same classification. This is how it was developed, to draw a line between Spodosols and Inceptisols. When we got additional data on Spodosols that were much older than those that we were studying in New York State, we found that many of them did not meet the chemical requirements that were needed to separate the Inceptisols from Spodosols in New York State. So we introduced the concept of field identification of spodic horizons by the crack coatings and the pellets, in which case it was not necessary to take the soil to the laboratory. We sent our proposed definition to the Canadians to be criticized and the people who worked there in the laboratory objected to the definition on the grounds that we gave too much emphasis to field identification. The field people objected on the grounds that we gave too much emphasis to the chemical properties. That was, I thought, about the best we could get at the stage of our knowledge at that moment. Many of the most strongly developed Spodosols will not meet the chemical requirements. They don't worry me because when they're that strongly developed you don't need the laboratory analysis to identify them. I thought we might well get along without creating a big demand for laboratory work. There's no argument about some of the Spodosols in the Carolinas and Florida. These are mostly Aquods, when they get that far south. You do not find any laboratory data on them. But I have seen Spodosols in Minnesota. I have a photograph of one. **Questions 18, Cornell and 17[illegible], Minnesota**

12.2 Lab Vs Field Identification

Soil Taxonomy specifically provides criteria for identification of spodic horizons in the field without laboratory data. In my experience it is rather rare that laboratory data are required except in transitional soils where the spodic horizon is marginal to a cambic horizon. I have been criticized by the people working in laboratories that identification of the spodic

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19. Head, Soil Survey Staff, Northeast National Technical Center, SCS/USDA, Chester, Pennsylvania 19013.

horizon is too often by field criteria and that the chemical properties are inadequately emphasized.

For field identification the only equipment needed, is a rather powerful hand lens or a pocket microscope that is capable of giving the 60 power magnification. If with this lens the individual is able to identify the crack coatings on sand grains or the pellets, the identification can be made in the field without any laboratory analysis. **Question 21, Leamy**

We do know that many beautiful Spodosols will not meet the laboratory requirements, and presumably, over time the organic ligands are broken, that makes the spodic material soluble. So that some of our best Spodosols will not meet the chemical test, but the chemical test is not required, only the field observation is required. In the event that it is cemented, or in the event that you can identify pellets in the spodic horizon, or the cracked coating on the sand grains; there is no requirement there for any chemical test. The chemical requirements are for the intergrades with the Dystrochrepts, and only for that. They represent the properties of the spodic horizon as it is just beginning to form. I am not aware of proposed changes in the chemical requirements. We did the best we could at the time. There were difficulties that we were quite aware of, mainly that many of the best Spodosols have a spodic horizon that does not meet the chemical requirements, and so we put in the field identification of the spodic horizon to permit their identification. The best developed of the Spodosols, generally miss the chemical requirements. *The chemical requirements were actually based on a study of the intergradation between Spodosols and Dystrochrepts.* **Question 105, Cornell**

It is also true that the spodic horizon reacts to fluoride, and the Field's test for allophane is used in a number of countries where there is no volcanic ash, to identify the spodic horizon and distinguish it from the cambic horizon. I do not know of any studies on this in particular, but I do know that the Spodosols normally react to the addition of fluorides and the pH goes up above 9 in the spodic horizon. The cambic horizon normally does not show this reaction unless, as in New Zealand, there is an appreciable amount of glass floating around in the area. There the use of the fluoride reaction test did not prove entirely satisfactory, but in Europe the soil surveys use fluoride as an indication of the presence of the spodic horizon. The pedologists there just put a pinch of the soil on filter paper, saturate and dry with phenolphthalein; put a drop of sodium fluoride on it; and if it turns red, they call it spodic.

We considered at one time the possibility of subgroups of Spodosols, defined on the basis of the pH in fluoride, but we never could accumulate enough data to find out whether it would work or not. **Questions 21, Leamy and 19, Cornell and 106, Cornell**

I am aware of the work that is going on in the development of color tests for identification of spodic horizons. Pedologists in the Soil Survey Laboratory in Lincoln, Nebraska have been actively pursuing the development of a field kit that is based on a color test of the extract which is related to the organic accumulation in the spodic horizon. Mr. Blakemore, in the Soils Bureau of New Zealand, has been doing similar work in the laboratory. **Question 144, Minnesota**

I am also aware of the problems created by the various ratios used in the chemical criteria. The Canadians use a little bit different ratio of pyro-phosphate extractable, iron and aluminum. I think at some time, there will be another study as to whether or not the chemical test should be changed slightly or the ratios changed to get a better match. **Question 106, Cornell**

There are also questions as to specific application of the criterion. Some for example, have interpreted statements regarding ratios equal to or greater than 0.2 as meaning they must be 0.2 or more. Others consider anything equal to or greater than 0.15 as qualifying. The intent of the definition of the spodic horizon, as well as in all definitions throughout *Soil Taxonomy*, that the numbers follow the normal mathematical rules for rounding. If only one decimal is used in the definitions, the numbers that are intermediate are rounded according to normal mathematical rules, e.g., 0.16 is rounded to 0.2. This was done throughout *Soil Taxonomy*, except in the definition of the Andepts where one decimal too many was inserted and we used the numbers .85 for bulk density which is more precise than can be measured in

the laboratory, and we should have used 0.9 or 0.8 or some number without two significant decimals. **Question 23, Leamy**

12.3 Spodic and Argillic Horizons

There have also been problems reported in identifying spodic horizons in soils which exhibit spodic characteristics but also have high clay contents. I have seen such soils in New Zealand where the Kauri has produced what appears to be a spodic horizon, and I have also seen such soils in Europe where there has been *Caluna* vegetation. In both cases the high clay content appears to be due to the presence of an argillic horizon and the clay skins or coatings are commonly very obvious in the fine-textured horizons. The spodic characteristics occur as a very thin layer above the argillic horizon and as rather thick coatings within the argillic horizon where there is obviously some sort of tonguing due to the illuviation of the clay. It is more or less reminiscent of a glossic horizon, but the zones in which the clay has been removed are now infilled with a very highly carbonaceous, very black material, that has all the characteristics of the spodic horizon. If the soil horizon is sampled as a bulk sample the clay content does prevent the identification in the laboratory of the horizon as a spodic horizon. However, if the soil is sampled not as a bulk horizon but as parts, then the spodic parts will not be found to have the high clay content; the high clay content is not the black material that you find between the peds. **Question 22, Leamy**

Where the two materials are distinctly separated so that you can scrape that centimeter of black material off the top of the gray clay materials and dig it out of the tongues between the prisms, we proposed another kind of intergrade between Ultisols and Spodosols in New Zealand. The present intergrade is defined as having a horizon with all the properties of a spodic horizon except the accumulation index. This is quite different from the soils of New Zealand where the spodic materials are, perhaps, adequate. Even in some parts of the pedon, if you hit a tongue and sampled vertically, you will get an adequate index of accumulation. If you miss the tongue, you won't. We can only view these things when we can study them. But so far, I have yet to find what could be identified as a spodic horizon with much over 22-24% clay. There's an antagonism there of some sort. It's currently unknown. **Question 142, Minnesota**

To improve our understanding of the use of spodic horizons and other diagnostic subsurface horizons in soil classification, we need to keep in mind some basic principles used in developing *Soil Taxonomy*. An important one to recall is that, if we have two subsurface diagnostic horizons in the soil, the preference is given at the higher category to the horizon nearest the surface. Thus, the soil with both a spodic horizon and an argillic horizon is normally classified as a Spodosol because the spodic horizon overlies the argillic horizon and the assumption is, that the more recent processes that dominate in the genesis in the soil produce the diagnostic horizons closer to the surface than the older process, which produced the diagnostic horizon at a greater depth. This assumption is consistently used in the various orders where we have the two or more diagnostic subsurface horizons. There is no distinction between the use of the argillic horizon in Spodosols and in Oxisols. **Question 14, Leamy**

12.4 Spodic and Oxic Horizons

The same principle applies to soils that have a spodic horizon overlying what appears to be an oxic horizon. I have seen in the Amazon such soils, though I have no data on the soils in question other than my visual and manual observations. The soil in question, probably at one time was an Ultisol, with a rather thick epipedon of a loamy sand or sand texture. With great age, the argillic horizon seems to have been degraded into an oxic horizon, but the thick sandy epipedon was favorable for the formation of a distinct spodic horizon above the oxic horizon. In accordance with the other taxa in which we have a spodic horizon overlying another horizon, we would assume that the current processes probably are those that lead to the strength of that spodic horizon, and therefore, we would put it into the order of Spodosols, and establish a

subgroup of oxic Spodosols, to distinguish these soils from the alfic and ultic Spodosols. **Question 9, Eswaran**

It is well known that in some subtropical areas, there are spodic and argillic horizons with 10 to 15 feet of quartz sand above them. The question then arises as to whether we should consider this to be soil material or geologic material. We pointed out specifically here, that when the spodic horizon is more than two meters deep, that it's presence or absence is not too important to the use of the soil above, except perhaps, as a source of sand. We draw the limit at two meters on that and we classify such soils as Quartzipsamments. The reason being that the difficulty of observation in two meters of sand is enormous. One commonly has to have drilling equipment and case the hole with his drill in order to get down to the spodic horizon. It didn't seem that this would be a good investment of money for the soil survey. The presence or absence may be of some importance; the occasional boring to find out whether or not the spodic horizon is there would be of some interest from a soil genesis point of view. I've been enormously puzzled on these as to where the aluminum in the spodic horizon can come from. I have no answer to that question yet, except that because there's nothing but quartz overlying the spodic horizon, the aluminum must come from some outside source, perhaps a moving groundwater, in which you have the humus coming down from the surface and the aluminum coming in laterally and then the two can meet and precipitate. It's the only hypothesis I can think of, how it checks at the moment, I don't know. You have a somewhat similar situation in North Carolina with Dr. Daniel's geomorphology study, when under some of the Paleudalfs, at some depth below the argillic horizon, one comes into sands that have every appearance of a spodic horizon. An argillic horizon above a spodic horizon you can see, but it is so deep that we have only few observations of it.

Soils such as the Leon series, has, in deep pits, multiple spodic horizons. And there was a long argument at one time about whether these represented different positions of the groundwater or whether they were buried. Radiocarbon dates on the organic carbon in the spodic horizon of the Leon, were around twelve hundred years and the first next lower spodic horizon was a bit over twenty thousand years. So, I concluded that was enough investigation, that we would consider these as buried soils. **Question 118, Minnesota**

12.5 Depth Requirements

Another rule of application important to remember is that in meeting depth requirements for a spodic horizon, measurements should be made from the mineral surface. The general intent was that the O horizon would not be included in the depth measurements. The O horizon is transient and may be destroyed by fire, which would then change the classification of the soil overnight, even though the O horizon will reform within a few years. It is normally not feasible to include an O horizon in the definition for depth unless the O horizon is thick enough that the primary rooting zone in the soil is in the O horizon. In such soils the climate is normally so cold and humid that there is no particular hazard of fire destroying the O horizon. And one of the unresolved questions is what to do about an O horizon that is perhaps 50 cm or more thick overlying a mineral soil with normally rather well developed spodic horizons, when the rooting of the plants is almost entirely in the O horizon. **Question 16, Witty & Guthrie**

12.6 Albic Horizons

Considerable discussion has occurred over the fact that although albic horizons are common in Spodosols, they are not diagnostic. The only place in *Soil Taxonomy* where I find the albic horizon used as a diagnostic horizon is in the suborder of Albolls. The minimum thickness of albic horizons in other kinds of soil would not be critical because the presence or absence of an albic horizon is not diagnostic to the classification. It was our desire, generally, to keep in the same series in the same family the cultivated and the undisturbed soil so that the series would not be changed by a few plowings. There are soils, such as the Boralfs, which may

have a very thin albic horizon if the argillic horizon is fine or very fine in texture, and these are kept together in the classification by not making the albic horizon diagnostic, rather we have used temperature, primarily, to define the suborder of Boralfs. The albic horizon is normal in these soils and has been recognized by the Canadians as a diagnostic feature. They, however, do not mind the thinness of the albic horizon because they classify the soil on the basis of the presumed virgin profile, rather than what is there today. In the Russian classification, the Australian classification and the New Zealand classification, soils that have albic horizons are classified as Podzols irrespective of the nature of the B horizon. They could have any kind of B horizon. They could be argillic or they could be spodic. In fact, in some instances they don't have to have any horizon of accumulation of anything. For example, in the sands in Australia where the upper 50 centimeters of the sand was bleached and white and there was no accumulation of anything, these soils were considered strong Spodosols. There has been in those countries considerable resistance to *Soil Taxonomy* because it does not use the presence or absence or the thickness of the albic horizon as a diagnostic in the classification. **Questions 46, Texas and 142, Minnesota**

On page 8 of *Soil Taxonomy* the sixth attribute that we desired for the taxonomy was that the differentiae should keep an undisturbed soil and its cultivated or otherwise man-modified equivalence in the same taxon insofar as possible. If the albic horizon is thin, the mere clearing of the forest, seeding of grass, and pasturing can destroy a rather respectable albic horizon. This I demonstrated in one of the type locations of one soil in New Zealand, where in the road bank there was a good albic horizon, but if one crossed the fence into the pasture, it was gone. This is why I always insisted on crossing the fences into the pasture. Under grass with fertilization you can not find that albic horizon anywhere. It's still a Spodosol in *Soil Taxonomy* because we don't emphasize the presence of the albic horizon as do the Zealanders or the Australians. If we did emphasize the presence or absence of an albic horizon more than the presence of a spodic horizon, one would have to draw a boundary along the fence because that is where the albic horizon stopped. It would be possible, of course, to emphasize the albic horizon at the expense of the nature of the spodic horizon and if we did that, we would have perhaps an Albod and a Chromod, and then these would be subdivided at the great group level into humic and other types of spodic horizons. We felt when we developed *Taxonomy*, perhaps erroneously, that the nature of the materials that accumulated should be given greater weight than the presence or absence of an albic horizon. Certainly if one were to emphasize the importance of the albic horizon, the definition would have to require that it extends to depths greater than 25 cm; otherwise plowing would change the nature of the classification of the soil. **Questions 24, Leamy and 131, Minnesota**

In order to keep undisturbed and cultivated soils in the same taxon, it was necessary in some instances to add specific criteria. For example, one of the requirements of Typic Haplorthods is that there is a minimum content of organic carbon in the upper part of the spodic horizon. The intent of this item is to provide for the cultivated Spodosols that have had at least a part of the spodic horizon mixed into the plow layer. **Question 15, Witty & Guthrie**

12.7 Sombric Horizon

The sombric horizon is another kind of subsurface horizon that contains illuvial humus. Unfortunately, we know very little about them. We do not have them in the U.S. We cannot study them, so we just simply must say this is something we do not know. The translocated organic matter in the spodic horizon, we think, is precipitated primarily by aluminum, and to some extent by iron; I think aluminum may be essential, because we always find it. We have studied the organic matter that has moved in the soils with natric horizons. It is not associated with aluminum. We just do not know much about the organic matter that is in the sombric horizon. The spodic horizon organic matter reacts with fluoride to produce a highly alkaline solution. I do not know of anyone who has tried the fluoride on a sombric horizon. **Question 50, Cornell**

12.8 Placic Horizon

The placic horizon is a thin, black to dark reddish pan cemented by iron, by iron and manganese, or by an iron-organic matter complex. There's been a lot of confusion between what has been called podzolization and the placic horizon. Many people feel that this represents translocated iron and aluminum and, therefore, represents podzolization. The placic horizon differs from the spodic horizon so far as I know, in only two respects, the thickness and the common presence of accumulations of manganese as well as iron and aluminum. We can not find in the normal spodic horizon an accumulation of manganese. This is an indication of ground water effect of some sort that we do not find in the Humods or the Orthods. And so far as I know, we don't find it in the Aquods other than the soil with the Placaquod where you may have manganese in the placic horizon but not in the spodic horizon that underlies it. **Question 141, Minnesota**

The position of the placic horizon within the profile determines the classification of the soil. For example, if a placic horizon in the Aquods is either above a spodic horizon or above a fragipan, it is classified as a Cryic Placaquod; however, if the placic horizon is within the spodic horizon or below it, it's either a Placic Haplaquod or a Placic Humod. This comes from the study of the British Podzols with thin iron pans. They have (in Britain) this very involuted horizon. If the placic horizon is separated by some depth from the fragipan that underlies it, there is a spodic horizon under the placic horizon. But in the deeper involutions of the placic horizon there is no spodic horizon because the placic horizon rests directly on the fragipan. This was a desire to keep this kind of soil from becoming a complex of a great many series. The definition was written in this way so we could have this ruptic spodic horizon in the thin iron pan soils of Great Britain. **Question 140, Minnesota**

More recent studies, particularly in Alaska, have raised questions concerning the logic for separation of soils based on position of the placic horizon. Very similar soils are being separated based on very minor differences.

The criteria we are using appeared in fairly early approximations. We really didn't know very much about these soils other than that they existed. The folks in Europe were satisfied with the criteria and without any way to test them, they were introduced into our approximations. They really never got criticized in the U.S. or even in Canada, to the best of my recollection. This happens throughout *Soil Taxonomy*. Proposals are made that came through by default, lack of criticism. **Question 141, Minnesota**

12.9 Aquods

The Aquods are Spodosols that have an aquic moisture regime or are artificially drained and that have morphological characteristics associated with wetness. There has been some misunderstanding concerning morphological properties indicating wetness, namely, mottles. Some have interpreted the statements in *Soil Taxonomy* as requiring mottles to be present. You cannot get mottles without iron, so to say that Haplaquods must have mottles is in error. **Question 97, Cornell**

The normal Haplaquod does not have an appreciable amount of free iron in it; not enough to produce mottles. So, you will find some in which there are some mottles in the lower part of the spodic horizon, but there may be no mottles within the first two meters, because there is no iron, manganese, or cobalt. So the definition is written so that mottles are not required for Haplaquods.

Actually, the Haplaquods went unrecognized for a long time, because the organic aluminum complex that makes the spodic horizon has a red color itself, and this can be checked in the field easily by just ignition, to see whether or not the sandy materials become red on ignition. **Question 96, Cornell**

12.10 Humods

The Humods are the more or less freely drained Spodosols that have a large accumulation of organic carbon. Sometimes in these soils you get, what are more like wetting fronts occurring where the iron is precipitated. It goes to a certain depth, and just about dries in place, and gives you a mottled look, primarily because the iron is precipitated out at that point. Usually it is clearly a redder color, but it gives you a mottling pattern just from the fact that it went down a certain way and dried before it hit the watertable. I have a photograph in here to illustrate this wetting pattern that looks exactly like the leakage of water from a sand into the substratum. I think that the sand, that has a medium dimension with the sand grains of less than a millimeter, will hold when dry about 2 1/2 cm of water in the surface before it begins to move downward. There are innumerable photographs of this leakage of water in a dry soil into the substrata. The Spodosols rarely become air dry, and the leakage comes from the accumulation of the amorphous materials that makes the spodic horizon; the water hangs in that horizon until it becomes saturated, before it leaks into the sand below, and once this starts, it is a self-accelerating process. The more spodic material that accumulates in the spodic horizon, the more common this is, the water hangs in the spodic horizon, and will not enter the underlying sand. These are quite common soils in western Europe under the heath vegetation. **Question 98, Cornell**

12.11 Relation to Inceptisols

To date we have no provision in *Soil Taxonomy* for spodic subgroups of Inceptisols. Soil Scientists in New Zealand and the U.S. have indicated a need for recognizing intergrades between Inceptisols and Spodosols. It is possible to make these kinds of additions. *Soil Taxonomy* was designed so that the least possible disturbance would be made if new knowledge and experience indicated that we should change some part of the system. In this situation where an intergrade may be desired between an Inceptisol (a Dystrochrept) and a Spodosol, the people who have some experience with these soils must propose that this intergrade be introduced into the system. In making such a proposal it would be essential that the man who makes the proposal, proposes also a definition for the spodic subgroup of the Inceptisol. This is perhaps the most difficult part for making a proposal for a change. *We need to have not only the proposed definition but we need also to have some reason why the change should be made.* Does it improve accuracy of interpretations, if so this should be spelled out in the proposal. **Question 25, Leamy**

Chapter 13

ULTISOLS

reviewed by J. Nichols[20]

13.1 Ultisols vs. Alfisols

(The decision to use 35 percent base saturation to distinguish between Ultisols and Alfisols) was a long time brewing. (It) reflected a desire to retain some of the zonality that we found between the Red-Yellow Podzolic soils of the Southern U.S., and the Grey-Brown Podzolic soils of the glaciated regions in the northern part of the United States. The examination of the data indicated generally that the base saturation in the Red-Yellow Podzolic soils decreased with depth below the B horizon, or even within the B, whereas in the Alfisols, the base saturation increased. The Ultisols, in general, were conceived of as soils in which the reserve of bases was maintained by recycling by plants. In the Alfisols, the reserve of bases was maintained not only by recycling of the bases of plants, but by weathering of primary minerals. We felt that the Ultisols were soils that could not be brought into permanent cultivation without the use of soil amendments, whereas we have plenty of examples of permanent cultivation of Alfisols, without amendments in Western Europe and in the northern parts of the United States. We had to find some basis, then to distinguish between the soils that could be used only for shifting cultivation without amendments, and the soils that could support a permanent agriculture, and examination of the data suggested that the 35 percent limit by the sum of bases method might make such a separation. **Questions 158, Minnesota and 72, Cornell**

At one stage we tried to make the distinction on the base saturation of the argillic horizon relative to the underlying horizon. The base saturation was low and it decreased with further depth. I think we had a limit at that time of 35% and, in the *Sixth Approximation*, the order that became Ultisols was defined as having a textural B with base saturation less than 35% or base saturation which decreases with depth from B to C. After this *Sixth Approximation* came out, I believe we kept much the same definitions in the *Seventh*. This stimulated some studies particularly in Maryland, Pennsylvania, New Jersey where it had been a practice since the settlers first came to the U.S. to apply small amounts of burned lime to soil once a rotation. We had these soils that were on the coastal plain, very old soils in a humid climate that had been limed for upwards of about three hundred years. If we sampled in the forest areas that had not been cleared, we had extremely low base saturation, but if we sampled in the fields that had long been cultivated and limed, base saturation was commonly about 60% through the argillic horizon. We still had the problem of whether or not this was a large enough change to recognize new series for the woodlots as distinct from those of the fields on the farms in this area. Most of the people felt that it was not warranted to change the series because one was a woodlot and the other was cultivated, but it would be useful to keep the same series so that the experience the people had from the cultivated field could be extended into the woodlots. To keep these soils as Ultisols instead of Alfisols we had to modify the definition and we set the depth at which the base saturation should be under 35% at, I think, one meter or 1.8 meters. **Question 158, Minnesota**

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20. Head, Soils Staff, South National Technical Center, SCS/USDA, Fort Worth, Texas 76115.

The question about the colors of the argillic horizon in defining the depth limit for base saturation came about because we have a group of soils in the southeastern states from basic igneous rocks which were red in color and at the depth of 1.8 meter. The most common base saturation was 35 percent. It varied a little bit above, a little bit below, but not very much above or below. And to keep from splitting all those series according to measurements that you could not possibility get, we changed the depth limits according to color to keep these soils from basic rocks together. **Question 79, Cornell**

We have, in the part of the question (concerning the use of the 50 percent base saturation requirement to qualify as Mollisols), soils that originally had low base saturation in an umbric epipedon and in an underlying cambic or argillic horizon. If such soils are limed, of course, the epipedon can readily become a mollic epipedon, but the base saturation of the underlying horizons is not so readily changed. It would require probably some hundreds of years to bring up the base saturation to 50 percent. We have such soils in the southern part of the Great Plains area; mostly soils that have undergone one or more interglacial pluvial periods. The base saturation of the argillic horizon is low, but there has been enough dust and enough liming that the epipedon has become mollic. The problem, then, was whether the people who knew these soils felt that they should be classified as Alfisols or Ultisols. Their preference was to have them as Ultisols. That is the way it was done. **Question 72, Cornell**

13.2 Argillic Horizon

It is proposed that Ultic Haplorthoxes are frequently misclassified as Oxic Tropudults. People ignore the sentence (page 329 *Soil Taxonomy*) "An appreciable increase in the percentage clay with depth is a property shared with Ultisols, and defines the ultic subgroups (in Haplorthox etc.)." The subgroup and the explanatory sentence emphasizes the fact that the clay increase by itself is insufficient to identify an argillic horizon. If the subsurface horizon has oxic properties, it is an oxic horizon, and so will be keyed out as an Oxisol. Indirectly it implies that the oxic horizon has priority over the argillic horizon. Is this the intent?

It was the intent that the oxic horizon has priority over the argillic horizon. In fact, on page 20 of *Soil Taxonomy*, we stated, "The argillic horizon by itself has little importance to soil classification. It is the accessory properties that are important." The soils that have a finer-textured subsurface horizon appear to be giving considerable trouble in the field. The pedologists seem to be unable to agree generally, as to whether or not this subsurface horizon is an argillic horizon. The problem has received much discussion from ICOMLAC, and it is quite likely that some changes in the definition of the Oxisol will be needed and will be proposed by ICOMOX. **Question 13, Texas**

The definition of Oxisols has created problems, especially with people from LDCs who go by the letter, as they frequently are not aware of the intent. The first problem is the classical question, "Where does an argillic horizon end and an oxic horizon begin, or vice versa?" I like to take the classical situation in Malaysia. The pedon has an A1 of about 10 cm; a B1, which meets all the requirements of an oxic horizon and 40 cm thick, and this is underlain by B21t, B22t, etc. We happily called this pedon a Tropeptic Haplorthox until, during a recent workshop, some experienced pedologist classified it as a Typic Paleudult. As the soil also shows the clay increase for the argillic horizon with clay skins in the major part of the B. The "Kandi" concept of ICOMLAC will not solve this problem.

We have two precedents in *Soil Taxonomy* for handling the situation, where the transition horizon overlies the argillic horizon, and has all the characteristics of an oxic horizon. The first precedent is that of the cambic horizon, which by definition may not overlie an argillic horizon, unless it is separated from it by an albic horizon. The other precedent is where we have a spodic horizon that overlies an argillic horizon. In this case, the horizon is not transitional, and the order is determined by the overlying surficial horizon, on the assumption that that represents best the present processes going on in the soil. In dealing with the material horizon that has the properties of an oxic horizon, but rests on an argillic horizon, it is possible to use

either of these precedents. The limit of 30 cm thickness was set without thought that this would be a transitional horizon. In the discussions of ICOMLAC, I proposed that this limit be increased to 50 cm on the grounds that if it is that thick, the soil would behave more like an Oxisol than like an Ultisol. In this situation then, one could establish an ultic subgroup of Oxisols to separate soils with this horizon sequence at the subgroup level rather than at the order level. **Question 8, Texas**

The International Committee on Low-Activity Clays (ICOMLAC) is proposing a new diagnostic horizon, the fine-textured subsurface horizon. It seems that if acceptance is given for the new diagnostic horizon, that for some (and maybe many) soils there could be strong disagreement among soil scientists as to whether a soil has a fine-textured subsurface horizon or an argillic horizon. For classification purposes a distinction, apparently, is not needed. Is this so?

The original proposal to recognize the fine-textured subsurface horizon as a basis for placing a soil in a Paleudalf or a Paleudult was the difficulty of getting agreement amongst different pedologists as to whether or not there was an argillic horizon. The proposal was to put into the definition, then, of Alfisols and Ultisols this distinction in texture with depth, as being the equivalent of an argillic horizon, so that no decision would be needed as to whether or not there was an argillic horizon in a particular soil. This is one reason that it was suggested that it should not be recognized as a diagnostic horizon, but as a diagnostic feature, perhaps, but certainly not a diagnostic horizon. So that a soil might have an argillic horizon and have this fine-textured subsurface horizon and no decision would be necessary then, as to whether or not that horizon was or was not an argillic horizon. This was only proposed for use in the low-activity clay soils and nowhere else in *Soil Taxonomy*. **Question 4, Witty & Guthrie**

The next question asks why was the argillic horizon clay increase established as a 20 percent relative increase compared to the overlying eluvial horizon?

The French taxonomy uses an increase of 40 percent increase in clay as a basis for recognizing different classes, particularly the *sol lessive*. Amongst the Mollisols, the existence of an argillic horizon is rather widespread and marks the break between the late Pleistocene Mollisols and the Holocene Mollisols. In these soils the break between the eluvial and illuvial horizons is at about an increase of 20 percent in clay. This is actually the minimum limit in the Mollisols at which we thought the field man could identify the change in the particle-size distribution or texture. Therefore, in *Soil Taxonomy* we took the absolute relative increase of 20 percent from the Mollisols as our minimum for recognition of an argillic horizon. In Alfisols and Ultisols, the normal situation is that the increase is 40 percent or more. It must be remembered that this increase of 20 percent is applied only to soils having clay contents ranging between 20 and 40 percent in the eluvial horizon. The 20 percent increase in a soil which has 20 percent clay means the field man must distinguish between 20 and 25 percent clay, with his fingers. We desired to have definitions that could be applied in the field without referring samples to the laboratory. **Question 31, Leamy**

13.3 Fragipans

(What was the basis of the depth limits of 50 cm below the top of a fragipan, for the 35 percent base saturation limit between Alfisols and Ultisols, considering the fact that the fragipan is a root barrier?) The first point is that these soils are sometimes severely eroded, and what was originally at a depth of 1 meter we now find at a depth of 50 cm, and we did not want to have to change the series because of erosion as long as we retained an identifiable part of the diagnostic horizons of the series. Erosion was to be considered a phase property. The upper boundary of the fragipan is something that generally can be identified in the field. It may be closer to the surface in an eroded soil than an uneroded soil, but it is identifiable, and if we put a limit below that point, rather than a limit in terms below the surface, it is a more stable limit. The fragipan is a barrier, but not a complete barrier to roots. It normally has the bleached nonbrittle surfaces around the polyhedrons in the pan, and the roots penetrate that

rather readily, although sometimes they are flattened by pressure. Still we do extract some water and some nutrients from the pan itself. **Question 81, Cornell**

In the classification system, we have the fragic subgroup in the Ultisols but not in the Alfisols. Is there a reason why you went this direction when you developed the system?

It's only that, when we provided the subgroups in *Soil Taxonomy*, we listed only the ones for which we had series in the U.S. Now it's my judgement that fragic subgroups exist in the Alfisols but the correlation staff, the state representatives, did not suggest anything along this line for Alfisols. I'm sure they exist in Belgium but it was not our principle to include subgroups for other countries unless they requested them.

I tried once to get an experiment in Michigan on the effects of freezing on fragipans because, in my experience, the fragipan in nature never freezes, and I wondered what would happen in Michigan when the forest was cleared and we have a bare field lying there through the winter and frost would reach to depths of greater than the fragipan. I wondered what would happen to the fragipan. I never could get that study off the ground as our administrative people were not interested. **Question 44, Minnesota**

Conditions of fragipans being destroyed by freezing could be documented if one had a fenceline with a fragipan under the forest and then see what we have in the cultivated fields.

We have in Belgium, in the loess, in the cultivated fields, the color pattern of the fragipan (with the polyhedrons with the mottled brown colors and the gray fillings between the polyhedrons) but the roots go all the way through everything and this stops at the fenceline; under the forest is a fragipan. I think it would be good evidence the fragipan has been destroyed by something. **Question 44, Minnesota**

13.4 Differentia, Abrupt Textural Change

What was the rationale of using an abrupt illuvial contact as evidence of age or pedogenic development intensity in the ustic moisture regime? Further, in the Ustalfs we utilize this abrupt contact as a criterion of the "pale" great groups, but in the Ustults we do not. This seems to be incompatible.

The rationale of the abrupt textural change, to start with, was the observation that as the soil climate became drier, with more intense and greater frequency of moisture changes in the soil, we got stronger and stronger development of the argillic horizons. Probably our experience with the old great group of Planosols had something to do with this, because the Planosols with clayey argillic horizons, or claypans have that abrupt boundary, where the climate is udic, marginal to ustic. Where the climate is udic, then the abrupt boundary becomes very tongued and ceases to exist as an abrupt boundary. Now, we made an assumption that this abrupt boundary was an indication of age. It took time to develop it. This assumption may not have been too valid. Recent studies of clay destruction in the presence of an intermittent groundwater table would suggest that we had the wrong basic assumption about the development of the abrupt boundaries on some of these soils. In the Ultisols, we had another group of correlators than we had with Alfisols. I think in general they fixed their concepts of Ultisols on soils that *did not* have an abrupt textural change between the A and the B. In the soils that they showed me in my travels, the Ustults of east central Texas, I did not see this abrupt boundary; although I did in some of the Ustalfs in east central Texas. It may be that they exist without anyone realizing it.

13.5 Differentiae, Moisture Regimes, Ustic

We have soils with low base saturation in ustic moisture regimes in Venezuela. They are not at all uncommon where the Ultisols grade up against the Aridisols. The rainfall is just marginal between ustic and aridic and the base saturation is very low. The explanation, of course, is hypothetical, but we do notice that the rainfall pattern, the soil-moistening pattern, is very different in these intertropical regions where we have no summer and no winter, but we have a rainy season and a dry season. The rainy season may give you your 400 mm of rain within three months, which is enough to moisten the soil and send a little more on down below the limit of where we set the base saturation determination. We even have Venezuela Aridisols with pH's of about 3.6 in water. How those developed is purely speculative at the moment, but there are also some Aridisols with those low pH's in Wyoming. These are developed in the U.S. in materials that have a fair amount of pyrite. In Venezuela in the soils I looked at, I was unable to find any pyrite in the rocks below the soil, but we had 6 milliequivalents of aluminum in the saturation extract with conductivity of about 15 or 16. Where the aluminum came from I am not sure because I had expected, when we went to look, to be able to identify the pyrite in the rocks but I couldn't. I think it is the rainfall pattern that basically produces leaching when you have a rainy season and a dry season. In most of Africa you don't find Ultisols, you find Alfisols in this situation. In most of South America you find Ultisols in these wet/dry climates if they have any age. **Question 102, Texas**

13.6 Differentiae, Temperature Regimes

When does or should an isomesic or warmer isotemperature regime become of equal importance at the great group level to soil properties?

An Aquult with an isohypothermic temperature regime that has plinthite, a fragipan, or an abrupt textural change or "pale" clay curve falls into a particular great group regardless of the temperature regime. The Aquults with plinthite are not uncommon in tropical regions with isotemperature regimes; they become rare in the mid-latitudes where the temperature regimes are hyperthermic or thermic; they are unknown where the temperature is mesic. If the temperature is frigid, they are excluded by the definition of Ultisols. Though we have the range of the Aquults with plinthite from the mid-latitudes where the temperatures are thermic or warmer and in the mid-tropics, actually, we do not have any Plinthaquults in the United States, to my knowledge. So that in devising the definitions it was not necessary to exclude soils with hyperthermic or thermic regimes from the definitions.

Fragipans are virtually unknown in intertropical regions. They are restricted to soils with thermic temperature regimes in Ultisols or mesic temperature regimes, and it seemed unnecessary to write restrictions in the definitions that eliminated soils that were not known to occur.

In the mid-latitudes, the soils with hyperthermic, thermic, and mesic temperature regimes are divided at the great group level according to the nature of the epipedon. We have the Umbraquults and the Ochraquults in the mid-latitudes. If we did not write a requirement of temperature in the Tropaquults, we would have to divide the tropical soils according to Umbraquults and Ochraquults. In the tropics, the amount of organic matter is very poorly related to the darkness of the epipedon. Therefore, since we have for the United States, a separation of Aquults into Umbraquults and Ochraquults, we would have to carry this distinction into the intertropical regions, where the color has virtually no relation to the organic matter content. Therefore, we have brought in the temperature limits into the Tropaquults, so that we would not be bound by the distinctions of Umbraquults and Ochraquults in soils where color has no relation to organic matter.

Soil temperature is a soil property. *It is a basic assumption in Soil Taxonomy that soil temperature and soil moisture regimes are soil properties*, although it is disputed by many. The

questioner is one of those who seems to dispute this assumption. I think those who disagree should devise their own classification system instead of complaining about *Soil Taxonomy*. There is no question but that soil temperature can be estimated approximately from altitude and latitude, but there are still considerable differences between soils with the same elevation and latitude depending on aspect and also depending on climatic factors. We have, for example, intertropical soils that do not have isotemperature regimes particularly in southeastern Asia. These may occur in other parts of the world, but the soil temperatures so far have rarely been measured.

There is a question proposing the use of the "tropic" modifier at the subgroup level or to drop it entirely, as the soil is already identified by the soil temperature regime at the family level. This identification at the family level is adequate where one has large-scale maps and the map units are defined in terms of family or series characteristics. In small-scale maps where the map units are mostly in terms of categoric units higher than the family, the soil temperature regime is not indicated by the name. In general, one may assume that the soils in the tropics have isotemperature regimes, but one cannot be safe in assuming that the soil temperature is isomesic or warmer between the tropics. It is possible that it is isofrigid. If this is not indicated by the name of the great group, then the temperature must be inferred from a map that shows elevations of the soils, and the maps of the elevation are difficult for the pedologist to obtain but essential for any interpretations whatever. The soil temperature regime should be indicated roughly by the name if we are going to make interpretations of soil map units that are made on small-scale maps. If it is not implied by the name of the map unit, then the implication must be added as a phase, and this complicates legends of small-scale maps. There are already so many phases that must be shown, slope, stoniness, depth of soil, textures, etc. It simplifies the matter of phases if the temperature and the moisture regimes can be indicated by the name of the taxon that is used to identify the map unit.

It probably is not material whether one uses the "tropo" modifier at the great group or the subgroup level other than the problem that requires the extension of the umbric epipedon or the ochric epipedon importance into intertropical regions. The basic reason for using it at the great group level was to avoid the extension of these concepts that are applicable in temperate regions to intertropical regions. **Question 17, Leamy**

13.7 Differentia, Tonguing

"Do not have tongues of albic materials in the argillic horizon that have vertical dimensions of as much as 50 cm if there is greater than 10 percent weatherable minerals in the 20- to 200- micron fraction." The statement is intended to keep out of Ultisols the Glossudalfs that have base saturation slightly under the limit between Alfisols and Ultisols. We wanted to keep all the Glossudalfs together. So far as we know they were all formed in Holocene materials mostly in loess. I have seen a few in solifluction materials. They just straddle the limits between Ultisols and Alfisols in terms of base saturation. The weatherable minerals were in there because, as I say, they mostly are in loess but they are in very late Pleistocene materials. We have Ultisols that have tonguing of albic materials that are very strongly weathered in soils where the B horizon apparently has formed and then undergone serious destruction and reformed another argillic horizon at a greater depth. These are mostly classified I think as Paleudults in the U.S. This was the definition that was suggested by those from Belgium to keep their Glossudalfs out of Ultisols to avoid splitting them between Alfisols and Ultisols.

This situation is also a problem in the lower Mississippi Valley, where I think, that we have these Glossudalfs. They have only been reported to me, I don't remember seeing them. I have seen them in Oregon where they are again in loess.

There is one way to try to simplify the definition and that is to delete the first statement in the definition because there are so few of these in the world. **Question 99, Texas**

13.8 Differentia, Base Saturation

What was the basis of the depths limits of 50 cm below the top of a fragipan, for the 35 percent base saturation limit between Alfisols and Ultisols, considering the fact that the fragipan is a root barrier?

Why, with the Alfisols, the base saturation is on sum of the bases. For a mollic epipedon the base saturation is determined by the ammonium acetate method. How was that decision made?

There were two reasons why two methods of determining base saturation is used in *Soil Taxonomy*. One of which we didn't fully understand at the time, but we knew that the difference existed. One was that we had regionalized our laboratories and in the eastern part of the U.S. where we had most of our Alfisols, the laboratory used the sum of cations to measure the base exchange capacity and base saturation. On the Great Plains where we had a lot of calcareous soils, the laboratory at Lincoln used ammonium acetate extraction because the sum of cations doesn't work in the calcareous soils. Most of our data on the Mollisols were accumulated at the Lincoln lab where pH was measured and base saturation was measured by ammonium acetate at pH 7. Most of our data on Ultisols were from the Beltsville laboratory where these same measurements were made by the sum of cations. When we began to look at 35 percent or 50 percent or what have you, as a limit that would affect the classification of the series, we could not very well compare the two methods because we had only the sum of cations on the Ultisols and only ammonium acetate on the Mollisols and the Inceptisols. We had a few soils of which we had both. And one of those was the pedon I used in the *Seventh Approximation* as an example of an Ultisol. Now it just happened that that was quite rich in free oxides as well as kaolinite. It had a very considerable pH-dependent charge. So that it went as an Ultisol if we used sum of cations, and it went as an Alfisol if we used ammonium acetate. Some of the best Ultisols were Red-Yellow Podzolic soils in the southeast at that moment. So without realizing what caused that pH-dependent charge at that moment we went ahead and said, well, this soil, a representative Red-Yellow Podzolic soil, is an Ultisol if we use sum of cations and 50 percent by ammonium acetate, but where you have a large pH-dependent charge that breaks down, and it just happens that that particular soil was one that had a large pH-dependent charge. That's how it happened. **Question 149, Minnesota**

What about the rule of the differentiation based on the 35% base saturation distinguishing between Ultisols and Alfisols. Was this criteria a long time brewing, as we say, or how did it develop?

From the early data that we had when we began this work, it was obvious that in the Gray-Brown Podzolic soils the base saturation increased with depth, or was 100%, whereas in the Ultisols, the base saturation decreases with depth in the soil. We have a complication in that definition, that comes from the soils from basalt in the southeast where the base saturation hangs just above or just below 35% at one meter eight. So there's a very complicated definition that is in there just to keep a few soils from basalt in the same series. And it is admittedly not an easy thing to map when the base saturation at that depth is unpredictable. You know it is going to be in the neighborhood of 35%, but it may be 30, it may be 40. This is not a wide range but the soils that cause this complicated definition on depth were minor in extent in the U.S. but important in some countries. **Question 158, Minnesota**

The next question comes from both New Zealand and from Thailand and concerns the identification of base saturation to distinguish between Alfisols and Ultisols. The Thai question reads, why is percent base saturation determined at 1.25 m below the top of the argillic horizon or at 1.8 meter below the soil surface. In addition, the Thai pedologist would like to know what to do if at 1.8 meters, there is a lithologic discontinuity with contrasting material. The New Zealand question asks, what is meant by identification of base saturation at certain depths below the argillic horizon. Secondly, it asks if the sample should be from a thin layer or is an auger sample from the approximate depth satisfactory, or should the whole horizon in which the depth occurs be sampled. Thirdly, what is the intent of the requirement of base saturation at depth

particularly when the dystro-eutro distinction is made on the basis of base saturation of the major part of the profile?

The first comment is, on what is meant by the word at a depth of one and a quarter meters below the top of the argillic horizon or 1.8 meters below the soil surface. This means, according to the English language, "at". It can be measured in one of two ways. Either one takes a sample of a thin subhorizon *at the specified depth*, or one samples all horizons above and below the critical depth and then makes a smooth curve of the data and the depth at which that curve crosses the 35 percent base saturation is either above or below the critical depth of 1.8 m or 1.25 m. If the smooth curve crosses the 35 percent base saturation limit at a depth shallower than the critical of 1.25 or 1.8 m, then the base saturation is certainly less than 35 percent at the critical depth.

The reason for the choice of 35 percent at the critical depth specified, is the simple one that is common to all the definitions in the taxonomy. It is that we got groupings that permitted us to make more statements and more precise statements about the soil use then we could otherwise make with another limit of base saturation or another limit of depth.

If there is a lithological discontinuity at or above the critical depths of 1.25 or 1.8 m the base saturation at these critical depths is still the 35 percent limit between the Alfisols and Ultisols. The base saturation of a specific horizon is not just a property of that specific horizon but it reflects the entire process of leaching and recycling of bases in the soil which affects the whole soil in all horizons, not just the one horizon. The base saturation curves are quite interesting properties of the whole soil rather than of any specific horizon. **Question 26, Leamy**

Why did you choose a percentage on the Ultisols/Alfisol break, instead of dealing with the magnitude of the bases? For example, if you have a soil that has a very low CEC by some method, and you have just a few bases left, but in magnitude very small, often it is enough to throw you over the 35 percent break. And yet from the point of view of root growth and recycling the bases, it is such a small amount of bases anyway that maybe it would better be classified as an Ultisol.

We had no basis to propose limits on the total extractable bases that seemed to make a distinction of the sort we wanted. We wanted to more or less keep the Gray-Brown Podzolic soils as we had conceived them in the 1938 classification. These can be very sandy, and have fewer bases than a clayey Red-Yellow Podzolic soil. There was a question and there still is as to which is the most important--the base saturation or the total bases. I do not know myself of any research that would establish that total bases are more important than base saturation. In general, I would question that at the moment, because with layer-lattice clays, if the base saturation becomes extremely low, the aluminum comes in and you have not only a low base saturation but a high aluminum saturation. What little work I have seen would suggest that the aluminum toxicity may be more important than the total amount of bases that are present, at least to plants that are not aluminum collectors. **Question 73, Cornell**

Base saturation is intended as a sort of index of the reserve in soils and how it got there. Cycling by plants versus weathering of primary minerals. If we had defined the difference between Alfisols and Ultisols as being whether or not the soils could be cultivated permanently without amendment, we would have then an enormous element of subjectivity in the classification of a given soil. It would all depend on whether or not the man thought this could be cultivated indefinitely without amendments, and opinions are going to vary enormously on that point. You cannot write a definition of that sort. **Question 75, Cornell**

Profile 12 in the *Seventh Approximation*, Pedon 15 *Soil Taxonomy* has an argillic horizon with a 5YR 5/6-5/8 color and it has the reticulate mottling below. These are things we don't really expect in the Ultisols as a general rule. However, the CEC per hundred grams of clay is a bit over 50 milliequivalents which would not be representative of most, or a great many at least, of our Ultisols. The clay mineralogy was not known at this moment, but with that you certainly have a lot of 2:1 lattice clay as well as a lot of free iron. **Question 150, Minnesota**

13.9 Differentia, Hard-Setting A Horizons

These soils with hard-setting A horizons are very extensive in Australia. Hard-setting soils are quite extensive in parts of Spain where parts of the epipedons are still present, that is the original A horizon. I haven't studied personally the soils of the Middle East, although I would expect them to be there. I have seen very few of them in South America. This may be largely because of the soils of xeric moisture regime are pretty much confined to the West Coast which is largely covered with ash. I don't recall seeing them in Venezuela or in the West Indies.

We have them in West Texas. Then, of course, the Alfisols and Ultisols that have not been truncated would have this hard-setting A horizon if they ever became dry. We don't notice this because they are so rarely dry. If you go to southern Illinois the Albaqualfs occasionally become dry and it takes ten minutes, perhaps, to get an auger through the A horizon into the argillic horizon below, you grind and grind and grind and can't dig it. These do become dry occasionally in some years. This would be a characteristic of Alfisols and Ultisols if they should become dry and if you do have this problem with these soils, one of their characteristics is that you have soil structure problems. **Question 80, Texas**

13.10 Differentia, Low-Activity Clay

Surely there was not very much discussion of the use of charge characteristics, rather than base saturation to make the separation of Alfisols from Ultisols. There was not a great deal known about charge characteristics. For example, extractable aluminum was almost never reported in the literature. At the time the *Seventh Approximation* was written you could not find any data. You could not consider then, how the use of other things than base saturation was going to affect your classification. You knew what soils you wanted to keep together but you did not know what the use of charge characteristics would do to your groupings. It was not really considered until we had the International Committee on the Classification of Soils with Low-Activity Clays. It has been discussed at length in that committee, and I think they are retaining base saturation rather than the low-activity clays for the distinction between Alfisols and Ultisols. They are raising charge characteristics to a higher categoric level in their recommendations but not to the order level. **Question 88, Cornell**

The question has a rather clear statement of the problem involved and proposes an addition to the words in *Soil Taxonomy* to clarify or to help solve the particular problem of soils with low-activity clay, which is a much more extensive problem perhaps than we realize. It is not only very common in South America amongst the Ultisols but the identical problem exists in Africa amongst the Alfisols. You asked my opinion and I can say only this, that we have recognized this problem for a number of years. We have now two international committees working on a solution to the problems. The Agency for International Development has become interested in the use of *Soil Taxonomy* as a tool for transfer of experience between developing countries to increase food production, one of the main problems that they face in these countries. They have contracted now with the Soil Conservation Service of the U. S. Department of Agriculture to furnish financial assistance to pedologists, from any country, who are concerned with the problems of improving the definitions and the classification that is proposed by *Soil Taxonomy*. There have been six of these committees established so far and AID provides funds through SCS and through the University of Puerto Rico for the members of these committees to meet once a year in a country where the particular problems that they are concerned with exists. This particular problem was the one faced by the first of these international committees, under the chairmanship of Professor Frank Moormann of the University of Utrecht in Holland. They had a meeting in the field in Brazil two years ago. They had a meeting in the field in Thailand one year ago at which time there was a discussion in a conference room followed by about two weeks of studying in the field the soils with which the committee was concerned. The field study is important, because as yet there is still considerable differences of opinion amongst pedologists about the meanings of various technical words, and the committee members cannot be sure they understand each other unless they can

examine a number of the same profiles in the field together and discuss between themselves in person about the impressions that they get from these particular soils. This committee has been working for about seven years now so the problem is not a simple one. And there has been at one time or another something like 40 different members from virtually every continent in the world where such soils exist. They're due to report, to make their final report, this coming June in London where the committee on the classification of Oxisols is meeting with them. The committee on Oxisols and the committee on the classifications of these soils with low-activity clay in Ultisols and Oxisols have a common problem, the boundaries between the argillic horizon and the oxic horizon, and they must have a joint meeting of the two committees. The first joint meeting of the two committees took place in mid-Asia just preceding the meeting in Thailand last year. A final meeting will be in Rwanda in June of this year after which the committee will submit a joint proposal to the Soil Conservation Service for circulation to anyone anywhere in the world who expresses an interest in it. My opinion is of very little importance in this and it is a difficult problem and needs the international consideration and debate that it has been getting. **Question 1, Venezuela**

13.11 Ultisol Order - Distribution

In West Virginia, Pennsylvania, and Ohio (the southeastern part) we have a lot of soils that have lithic contacts, clearly within 40 inches, and sometimes within 20, that result in very acid root zones, just above the lithic contact, and the base saturation is quite low in these cases. So as a result, we have Ultisols running up through those three States almost into New York. Was that the intent? Obviously, that is what happened, but does that concern you, that we ran Ultisols this far north up the Allegheny plateau?

We had no information on low base saturation soils occurring north almost to New York when we wrote *Soil Taxonomy*. If you have data now, it is new data, and I should point out, we said specifically in *Soil Taxonomy* that the groupings that result from these limits must be continually tested against the functioning of the soils. How do they behave: like Alfisols or like Ultisols? If they behave like Alfisols, then you have to make some changes in the definitions. As we accumulate new knowledge, we must continually examine these definitions.

I anticipated the Ultisols running into New Jersey on the coastal plain, but I did not really expect them in the valleys. I was afraid that some might exist in New England. We had no data on base saturation. None. Not one analysis that was published that we could find. So I put a temperature limit on the Ultisols so you would not have to worry about it. **Question 74, Cornell**

13.12 Ultisol - Oxisol Topographic Relationships

I recently finished the study of the genesis of some soils on the Coastal Plain Formation of West Africa in a very humid environment around southeastern Nigeria. This area is typically Paleudults on the upper surfaces, and in the closed depressions which form in this region you have small valleys which seem to be filling in with sediments. Now the original materials on this whole formation are already preweathered before they were deposited in a shallow marine environment at the end of Pleistocene or Pliocene. So in these shallow depressions, along the younger slopes, we find what are classified as Oxisols, and at the bottom in the upper surfaces we find Ultisols, because this is where you see evidence of clay translocation. Do you think this changes the Ultisol-Oxisol sequence, and does this affect at all the classification?

In the development of *Soil Taxonomy* we had no reasonable opportunity to study the Oxisols of Africa and South America. The U.S. Department of Agriculture appropriations are limited to uses that will benefit the American people. The study of the intertropical soils, which is intended primarily to develop a classification to help them exchange experience, would not be of benefit to the U.S. So this travel was impossible with USDA appropriations.

Actually, as you say, your studies are very recent, and this was knowledge that was n available to us at that time. We have similar situations in Malaysia that we have learned abo within the last 2 or 3 years, and so this is one reason that we have an International Committ on Oxisols, which is desperately needed. We had only a limited number of samples from Puer Rico and Hawaii that we could study, and these were virtually all formed in basic igneo rocks. These were by no means a good selection of the Oxisols as compared to the soils South America, Africa, or Southeast Asia. **Question 14, Cornell**

13.13 Aquults Suborder

The next question is from Thailand and asks in essence, why the criteria used distinguish Tropaquults are not used to distinguish Tropaquepts or Tropaqualfs.

In the Paleaquults and Tropaquults, the requirement for color is only that the hue of 2.5 or 5Y accompanied by mottles due to segregation of iron, or, if the hue is 10YR or redder, the the low chromas are required. Working in Venezuela, I examined the evidences of wetness fo aquic great groups and suborders and made the proposal that the definition used for Ultisols b extended in all orders to the intertropical regions, namely, the Inceptisols, the Mollisols, th Oxisols, the Entisols, etc. In Venezuela, if the soils were wet, commonly the wetness wa marked by the yellow hues accompanied by mottles. The criteria used for the Ultisols migh have been applied more generally in *Soil Taxonomy* had we had more examples of other kind of intertropical soils. **Question 20, Leamy**

13.14 Aquic Subgroups

The various depths and color criteria for aquic subgroups reflects the thinking in differer groups of States. The Southern States had one opinion and we used their opinion for Ultisols and the Northern States had another opinion and we used their opinion for Alfisols. If you ge into trouble about it, I can only suggest that you ask that this be reexamined. **Question 86 Cornell**

I cannot answer the question of "Why were the aquic arenic subgroups excluded fror Paleudalfs, and why was it felt that no aquic grossarenic subgroups were needed in Udalfs Ustalfs, and Udults." In theory the bulk of these Arenic and Grossarenic Paleudalfs an Paleudults are in the region of the southern states. We are not dealing with two differen groups of people we are dealing with the same group. It is one thing with the Paleudults i Florida and another thing with the Paleudalfs in Texas. Their recommendations were accepted I was not in on their discussions at the work-planning conferences. In the Alfisols this is a implied subgroup in that the definitions for the Aquic Paleudalfs excludes the arenic subgroup and the definition for the arenic subgroups does not mention the aquic properties. It is a implied subgroup. If an examination of your interpretations suggest that you need that subgrou then it should be proposed. If your examination of your interpretations suggests that you mak the same interpretations for the Arenic Paleudalfs, let us say, that also meet the restrictions o the aquic subgroup, then you should propose a modification of the definition of the areni subgroup. Bear in mind that the only subgroups listed here are those that appeared in th printout of the classification of the soils of the United States. Many other implied subgroup exist throughout the taxonomy but are not spelled out simply because we had no series that hac been so classified.

The limits were proposed by the regional groups based on their experience with the significance of the depth to the gray mottles. In general, the sandier the soil the less importance one is inclined to put on the gray mottles. Particularly in thermic soils, the importance of the depth to the gray mottles decreases because you have a long growing season. If the soil is inclined to be a little wet in the winter it is not so important as it is in the frigid and mesic

soils where your growing seasons are shorter and the delay in planting due to wetness may be very critical. **Question 136, Texas**

In the Ultisols, we have used the aquic moisture regime to define the suborder because they are all wet, and some have an albic horizon, others have an umbric epipedon, others have an ochric epipedon. Those with the albic horizon generally have an ochric epipedon above it. The distinction between the Aquults with the ochric epipedon and the albic horizon versus those with the umbric epipedon carry over into *Taxonomy* the old distinction between the humic gley and low humic gley soil of the southeastern states. They seem to think there that these were distinctions important enough to recognize at the great group level. We had used the moisture regime at the suborder level, so the first level at which we could bring in the differences in horizons were the great group level. Suppose we insisted that we use the albic horizon at the great group level, and all soils where it occurred. First, because it does not occur in all soils, we require an extra category to bring it in. Second, if we use it at the same categoric level in all soils where it does occur, then we split what seems to be a natural group of Albolls according to their natural drainage, which again does not always exist today, but is always restricted.

13.15 Humult Great Groups

In the definition of Humults, there is required either 0.9% organic carbon in the upper 15 centimeters of the argillic horizon or 12 kilograms of carbon per square meter to a depth of one meter. The range here appears to be very great.

It was always our desire to keep together in the classification the soils that were virgin, the cultivated soils, and also, the eroded soils, so that the experience of the use of one could be extrapolated to the other. In reviewing the data for the Humults that we have in the United States, it seemed that the soils that had 12 kilograms of carbon in a cubic meter also had 0.9% carbon in the upper part of the argillic horizon. We have a number of these soils in the U.S., some of which have been eroded to the point where the present content of carbon is less than 12 kilos per cubic meter but where the carbon is at or above 0.9% in the part of the argillic horizon that remains. So that these soils can remain as Humults, even though rather severely eroded, is only when a major part of the argillic horizon has been lost that they get changed from Humults to some other suborder. The range does look great, and yet when we examine the data there was a relation in the virgin soils between the two numbers.

If some Humults are so classified because of the carbon in the argillic horizon and others because of the total carbon in the upper meter, and there are differences between the two kinds of Humults that are not due to erosion, then it seems likely that some sort of separation at the subgroup level would be surely warranted. *Soil Taxonomy* provides for ustic subgroups of the Tropohumults because these were known to exist in Zaire by a Belgian pedologist who has worked there. Is it possible that the differences between the Humults in Venezuela can be associated with differences in the moisture regime? Is there a difference in these soils between the udic and the ustic regimes? Can someone answer that?

If the differences are not associated with the moisture regime, either ustic or epiaquic and it is felt important that the kinds of soils be separated because of their differences in behavior, then it is necessary that those who know the soils make some more or less specific proposals for modification of the definitions. The definitions do provide for the typic, the epiaquic and the ustic subgroups, but perhaps these are inadequate for conditions that were unknown to us in Puerto Rico and Hawaii. These are the only places where we have experience with the Tropohumult. **Question 9, Venezuela**

There is only one great group, Humitropept, where 12 kilograms is used as a limit to separate the great group from others in the same suborders. There are some suborders, as in the case of Humult, where 12 kilograms of carbon is used. There are other humic suborders such as Humox in which the limit is not 12 but 16 kilos of carbon. The reason is again the same as

in other questions that the groupings that were achieved by using these limits seem to permit more statements about the taxa that were formed and more accurate interpretations. We tested limits of 20 kilos of carbon, 16 kilos of carbon, 12 kilos of carbon, and no one limit seemed to be useful in all of the different orders. The 12 kilos of carbon per cubic meter seem to give the best groupings for Ultisols and Inceptisols but a higher limit of 16 kg seems best for Oxisols. In every case, the limit was not strictly on carbon but also involved the temperature regime of the soil. **Question 36, Leamy**

13.16 "Pale" Features in Ultisols

In general we didn't argue much about presence or absence of argillic horizons in the Alfisols or Mollisols that had 2:1 lattice clays. When one gets into a group of soils with 1:1 lattice clays as in the Paleudults of the Southeast or in a number of the intertropical soils the evidences, of clay translocation are not so clear. The best evidence I know is the more or less abrupt irregular boundary between the ochric epipedon and the finer-textured argillic horizon below. The clay moved so long ago I suppose that the clay skins have all been disrupted now by animals and roots until you get into the horizons a couple of meters below the land surface where the biologic activity has been minimal. There you can begin to pick them up. These are among the kinds of soils that the committee on Alfisols and Ultisols with low-activity clay have been evading now for a number of years. The final report is due about June of this year. The proposal is that that will be distributed for testing in parts of the world where these soils are common and after a year of opportunity for testing the comments are due in Washington, and the final decision will be made on what changes to make. They are proposing a number of new great groups with the formative element "kandi" to imply the low activity clays. Not all of them really are kaolinitic some of them are mostly free oxides. "Kandi" will be used as indicative of low-activity clay. **Question 89, Texas**

That was the intent: to use "pale" for soils with considerable age, and with overly developed or over-thickened horizons of one sort or another. It was not the intent to get a soil of a "pale" great group in Holocene deposits, although we have run into situations where that's what happened. We had a student at the University of Ghent on a doctoral thesis last year. He was working with Holocene deposits where there was an argillic horizon, and where the underlying sediments were fine-textured so that there was no decrease in the percentage of clay with depth. We originally introduced the limit of weatherable minerals with the idea that you would find weatherable minerals in Holocene deposits. This was in the Ultisols rather than the Alfisols. Holocene sediments in Malaysia were all pre-weathered when deposited and had no weatherable minerals. We have made a proposal for over 18 years for a new definition at least for the Ultisols. The Alfisols haven't come to my attention in this connection. **Question 20, Texas**

13.16.1 Paleudults, Particle-Size Criteria

While we are mentioning Udults, the statement for "pale" great groups does not require solum exceeding 1.5 meters in thickness if the soil does not have lithic or paralithic contact within 1.5 meters, nor a decrease in clay by more than 20 percent. This has worried a number of soil scientists who thought that all" pale" great groups should require thick sola. Would you care to comment on that?

We thought that the Paleudults, as they reflected a soil of great age, should not have rock within 1.5 m of the soil surface. That was the first thought. One can stop the solum at the rock. There is no problem on that, but otherwise, if there is no rock, it is very difficult to decide where the solum stops. At one time we had a statement in item b in the definition on page 349 of *Soil Taxonomy* that required, people thought, that we identify the base of the argillic horizon which presumably would be the base of your solum. We had to take that out because that is a limit that pedologists of equal competence can disagree violently upon very

widely. As it now reads, the definition does not require that one determine the thickness of the solum beyond the depth to the rock or the base of the argillic horizon. In the Ustalfs we don't have precisely the same definition for the Paleustalfs or the Paleargids, though there are some similarities. **Question 100, Texas**

13.16.2 Paleudult Great Groups

The concept of the "pale" great groups was intended to group the soils of very considerable age into separate taxa from these of late Pleistocene or Holocene age. We have no good geomorphic studies of the Paleboralfs, but we do have, in these soils, evidences of downward movement of the argillic horizon as they have tongues of albic material going into the argillic horizon, with tiny remnants of argillic horizon remaining in the albic horizon.

We observed that these albic horizons vary enormously in thickness. On the more stable surfaces, we can find these albic horizons more than two meters thick. There is always an underlying argillic horizon, and at the contact between the albic and argillic there is evidence of destruction and downward movement of the argillic horizon. We, therefore, made an assumption that, when the albic horizon becomes very thick, this is an evidence of considerable age in the soil. Those of late Pleistocene normally have an albic horizon of less than 50 cm. But there are also Boralfs with more than 2 m of albic horizon.

This was a characteristic we could use for the Boralfs. The destruction of the argillic horizon is not so obvious in the Ultisols. So in the Paleudults, Paleustults, in order to distinguish the soils with the long term genesis of the soil, we have to emphasize the properties of the argillic horizon instead of the albic horizon.

When we get into the semi-arid and arid regions, we have to use the presence or absence of the petrocalcic horizon among other properties that we use - the thick argillic horizon, clayey textures and abrupt boundary between the material above the argillic and the argillic. **Question 117b, Cornell**

Will you characterize the concept involved in the term "pale" beyond the simple statement of excessive development, given on page 89 of *Soil Taxonomy*? Historical prospective of the evolution of a concept as the system developed may be helpful.

The concept involved in the term "pale" at the great group level was proposed fairly late in the development of *Soil Taxonomy*. It came about, as I mentioned earlier (about geomorphology studies), as a result of geomorphology of the coastal plain soils in the southeastern United States and the Aridisols and the Mollisols of the arid and the semiarid land of the southwestern United States. The concept that was held when I started working in soil science was of the lowering of the landsurface on the interfluves and the replacement of this concept by the notion of linear retreat of the slopes was much later. It was pretty much assumed by pedologists of Europe and northeastern United States that all soils were about the same age, and that the differences were due to other kinds of soil-forming factors. When we started the geomorphology studies, we found that the soils in any of these landscapes which were not covered by the glaciers was quite variable. Some of the soils were very early Pleistocene or Pliocene in age, and others were Holocene. We began to look at the differences in these soils with such greatly varying age. Obviously, if one goes back to Pliocene or even early Pleistocene there have been a number of differing climates under which these soils developed.

In the southeastern States, the Ultisols, the older surfaces which have been dated by Dr. Daniels and associates at well over a million years, we found that we had something very similar in chemical properties to many Oxisols. They were mixtures of quartz, kaolin, and free oxides, and they had something very similar in chemical properties to many Oxisols. When we went on to the late Pleistocene or even early Holocene surfaces in the coastal plain, we found soils with completely other suites of mineralogy. There were many feldspars, we had montmorillonite and illite in place of kaolinite, although mostly they were mixtures. The activity of the clays were much higher than in the soils of very old landscapes. So we tried to define the Paleudults in

terms of measurable properties, not in terms of age. So we put the limit of weatherable minerals on the silt and sand fraction, on the Paleudults, and the thickness of the B horizon, to distinguish them from the Hapludults. **Question 44, Cornell**

Do you see any problems, like in the Paleudults, that these "pale" features may be more a condition of the origin of the parent material being highly weathered, and not the fact that the soil is formed and been there a million years? I have found, with some of my chronological studies, that in Nigeria at least, maybe down to a meter and a half of these older surfaces, seem to indicate that material was not in place for a very long period of time. It was in a constant state of deposition and transport, so that maybe the feature of the argillic horizon is not so much a "pale" feature, but it could be just the pre-weathered parent material itself is the real "pale" feature.

There is no question of the possibility that some soils may have "pale" features because they are from pre-weathered sediments and have not been in place a long time. This was recognized at the time that we developed *Soil Taxonomy*. In our southern coastal plains, the sediments coming from the Piedmont were unweathered when they were laid down, but sediments coming from Oxisols might arrive completely weathered, and one might get "pale" great groups in relatively late Holocene sediments, just enough time to develop an argillic horizon. We hope that the limit on weatherable minerals would separate these, but it is not necessary that they do. A soil coming from a very small watershed may consist of completely weathered sediments. The soil coming from a relatively large watershed will normally have some areas of unweathered sediments that are transported to mix in some unweathered minerals, but the small watersheds could get us into trouble. This was not only the case in Nigeria where you experienced it but also we have run into it in doctorate theses from Malaysia where we cannot identify weatherable minerals in relatively late Holocene sediments. The solution to this has been discussed at some length at Ghent. A proposal has been made to resolve it, but whether or not that will be acceptable to other people, I do not know. **Question 45, Cornell**

The reason for omission of the Orthoxic Paleudult subgroup is that the Paleudults, for which we had any information, were all of kaolinitic nature and the CEC's were all very low. It was considered normal that the Paleudult has orthoxic properties. The Norfolk and Ruston series were the basis for the definition of Paleudults and have a wide geographic range. We find them extending from the Atlantic coastal plain in the southern states across into west of the Mississippi River where we begin to get an admixture of montmorillonite. There was discussion of this matter and we had some data from the Mississippi Valley. If we had introduced an orthoxic subgroup, we would have split the series. The correlators in the southern states were opposed to this, but Dr. Buol has been arguing in the Southern Soil Survey Work Planning Conference of the National Cooperative Soil Survey that those series should be split. Where the mineralogy of the clay is kaolinitic on the coastal plain and mixed in the Mississippi Valley, the management practices do differ significantly. Dr. Buol has been in the position that I have been several times: when something is proposed for the first time, it is greeted with the utmost horror, and if I repeat it enough years, they tolerate it and finally they embrace it. He has about reached the stage where they will embrace it, but the problem is going to be resolved by ICOMLAC. **Question 120, Cornell**

13.16.3 Hapludults vs Paleudults

What is the difference between the Paleudults and the Hapludults of the Southeast? Going back to the 1938 classification the southern correlators argued that the definitions should be by type rather than by limits. The Ruston was the type Red-Yellow Podzolic soil, and the Houston black was the type Grumusols, and so on. These definitions are not really workable when you have so many thousands of series because there are so many that are alike, one type of Red-Yellow Podzolic soil in one respect and one type a Gray-Brown Podzolic soil in another property, and which one are you going to weight? I use the example of the field trip we had with the northern-southern correlators on the piedmont soils in Maryland and Virginia where the soils have a solum that is comparable in thickness to that of the Miami which was supposed to be the type Gray-Brown Podzolic soil.

But they have the clay mineralogy and so on of the type Red-Yellow Podzolic soil of the Ruston and the Norfolk. We never did resolve how to classify the Chester series because neither group would have anything to do with it. Those who worked with Gray-Brown Podzolic soils say it is a Red-Yellow Podzolic soil, and those who worked with Red-Yellow Podzolic soils say it is a Gray-Brown Podzolic soil, and we couldn't resolve it with that type of definition. The soils/geomorphology studies helped straighten us out. The study helped us to understand a little better. We had a lot more typical Red-Yellow Podzolic soils in the south than Ruston and Norfolk. They were clear off at one end of the spectrum so they became the Paleudults. The Hapludults are soils like the Cecil soils. They are much more extensive, much more representative of modern processes of development, and these very old ones, according to Daniels, may be up to 2 million years old without serious additions or losses. **Question 61, Texas**

13.16.4 Paleudults, Grossarenic and Arenic Subgroups versus Buried Horizons

There is a problem of whether some soils classified as arenic and grossarenic subgroups are in fact buried soils. This problem exists also in the southeast, where we have arenic and grossarenic Ultisols as well as Texas and New Mexico. In some of them, the break is very obvious in the particle-size distribution of the sand fraction between the epipedon and the argillic horizon and is a lithologic discontinuity that is very obvious. It would be my feeling that the subsoil should not be in the arenic or grossarenic group. But it also happens in other places that there is no discontinuity, as in the coastal plains geomorphology studies. A doctoral thesis of Erling Gamble examined the sand-size distribution in the Arenic and the Grossarenic Paleudults. While he found there was a very great variability in different parts of his thesis area of Johnston County, North Carolina, some were much coarser sand, or much finer sands than others. Still, the sand distribution in the A and the B horizons, in every instance, was the same. It seemed impossible to figure out how, then, one could get a mantle of sand deposited over this county area in which the recent sand always had the same size distribution as the underlying material. I think these are good evidences that they are legitimate Arenic and Grossarenic Paleudults. I realize that even though we have tried to lay down rules for correlation, that there are or have been differences of opinion between the regional staffs on this particular problem, especially in Florida. Where I have looked at the soils, and I find a fine sand that is 1.5 m thick that overlies a sandy clay loam in which the sand is rather coarse, to me, this is buried soil underlying the recent sand. But just what the correlators have done with these, I couldn't say. I know it has been discussed in Washington D.C., but we don't have the answer in Washington, D.C. for every problem that comes to us. **Question 19, Texas**

13.17 Rhodic Features in Ultisols

If one examines the soils that are included in Alfisols and Ultisols at any given suborder level, one finds generally that there are soils that have severe problems of soil structure. The physicists, the agricultural engineers have not been able to quantify methods for measuring soil structure, and yet this is one of the most important properties to agriculture that a soil possesses. Examining the soils that are classified as Xeralfs or Udults, one runs into soils that have no structural problems occasionally, but generally there are serious problems of soil structure under cultivation. The soils that do not have serious problems of structure, we first had found in the U. S., always had dark red colors. The original definitions of the rhodic great groups required values moist of 3 or less and hues redder than 5YR. In Tazmania, I was shown soils on a single lava flow from high elevation to sea level, and the hue became less red as the elevation increased, but the structural stability of the soils was the same irrespective of hue. The value did not change with elevation. Therefore, in the Ultisols, in defining the rhodic great groups, the requirement of the red hue was dropped, but the requirement of the low value was retained.

Because, so far as we now know these soils are always developed from basic parent materials such as basalts, limestones, etc. The contents of phosphorus are generally higher in the rhodic great groups than in the others. The use of the color value and the chroma was predicated on the assumption that these features were correlated with the structural problems, with the phosphorus contents, and so on. There were many covarying properties that were extremely important to soil use in the rhodic great groups. No matter where one finds them, they are about the most intensively farmed soils of the particular suborder. Rhodic great groups were not set up in Mollisols because there were no particular differences in soil structure with soils that have a mollic epipedon. The formative element "rhodic" implies red, whereas the actual characteristic used in Ultisols is the color value. This may disturb some people but one must recall that there are rhododendrons that are purple in color. **Question 28, Leamy**

13.18 Family Criteria

13.18.1 Oxidic Mineralogy

Dr. Bartelli and others thought that the oxidic mineralogy was going to help them in classification that would isolate certain kinds of soils. They thought that soil such as Tifton would have oxidic mineralogy, i.e. the soils that had the relatively low CEC per 100 grams of clay. Along in about 1968 or 1969 when they started getting some data, they found out, in fact, that Tifton did not have oxidic mineralogy nor did the Norfolk or Cecil or some of the other soils they thought would. They did find some soils that did have oxidic mineralogy, and they were the soils in the mountain areas in Tennessee. I believe one of them was the Alcoa series and another one, the Brevard series in the mountains of North Carolina. After they found out it wasn't making the split that they wanted, they declared a moratorium on it until they could get more data. Now after I came to Fort Worth in 1971, we talked several times about what to do about this and always put it off a little longer. When the low cation activity clay committee began, we were in hopes that they would solve the problem because the Kandi Udults, or whatever the final terminology would be when they were defined, might make the splits that they wanted. That committee has run a little longer than most people had thought it would at the beginning. There are still hopes that after that committee produces their work, that maybe oxidic mineralogy won't be needed. Before long we have to make a decision as to whether we will retain this or whether we won't. Apparently, the mineralogy is essentially inherited from the rocks in the area for the few soils that we have. I have the latest circular here for the low cation activity clay committee. If they stick with the same CEC break that is used for Oxisols of 16, there are going to be very few of those in the southeast. If they had used a 24 milliequivalent break at pH 7, there would have been a tremendous areas in the south east U.S. and southeast Texas. After much discussion it looks like they are going with the 16 milliequivalents, and we may again be left with the problem. That decision needs to be made fairly soon about what to do. If it isn't solved by the low cation activity clay committee we may want something like a task force similar to the one that we had on organic soils or the task force on the orders of soil surveys to try to solve that problem. Dr. Smith do you have a comment on this?

You may want to propose the elimination of oxidic mineralogy if the desired interpretation difference is not being met. There are still two alternative courses of action. If you decide you don't want the oxidic mineralogy in Alfisols and Ultisols, that is as far as you would go in your proposal, because they may still want these in Oxisols, for example. There are many oxidic families of soils in Hawaii. Before you drop it completely you must examine its impact in other orders than Alfisols and Ultisols. **Question 138, Texas**

The original intent of the oxidic mineralogy was to separate the soils that have enough free oxides to form a more or less complete coating of the oxides on the clay. These soils have an appreciable variable charge or pH-dependent charge, and it was thought that there were the two reasons for the separation: (1) that the variable charge would be more or less distinguished

from the soils with a permanent charge, and (2) that in general, there are many fewer problems of soil structure in soils that have oxidic mineralogy. The normal Alfisol or Ultisol will form a crust after cultivation, as a result of the first heavy rain. The soils then with oxidic mineralogy have a much more stable structure in the plow layer, and we wanted to make this distinction. The definitions of taxa of higher categories for the rhodic great groups, and subgroups were made because of the distinct difference in the tendency to crust when cultivated, and the oxidic mineralogy then makes some break within these rhodic great groups and subgroups; most of them are oxidic; a few turn out not to be. Rather than drop the oxidic mineralogy, I would think it better to put some sort of a limit on the minimum clay content at which the oxidic mineralogy is used. For example, require that the particle-size class be loamy or clayey rather than permitting sandy soils to be included in the oxidic mineralogy, or you may have only 3 or 4 percent clay, then the significance of the iron is greatly reduced. I should also say, I do not think I know enough at this point to have a very firm opinion on the utilities of the oxidic mineralogy in loamy and clay soils. There should be some examination of your data in the U.S. to see where, if you restrict oxidic mineralogy to finer textures, that restriction should be placed. **Question 2, Witty & Guthrie**

13.18.2 Sloping Family

Do you know of any examples where you need polypedon characteristics to be able to classify a pedon in *Soil Taxonomy*? Where you need shape, where you need slope to classify the series?

There are examples where polypedon characteristics are required to classify a pedon in *Soil Taxonomy*. We have slope built into the classification of at least two great groups and we need it in some others. The two we have are Aquolls and Aquults. These are often wet soils; they must be drained for cultivation, and the common practice is to shape these nonsloping soils to provide surface drainage. The sloping members do not require shaping for drainage, and they require some sort of interception tile to cut off the seepage water. The same thing would be true for a good many of the Histosols. If these are cultivated and the polypedon is flat, then normally you have the soil ridged very steeply to provide for a better aerated medium for plant growth. We have other Histosols that are naturally sloping with slopes up to 50 percent or more (in Malaysia). To get at the series one has to consider the polypedon shape rather than the slope of the individual pedon. **Question 33, Cornell**

Chapter 14

VERTISOLS

reviewed by J. Comerma[21]

14.1 The Order

14.1.1 The Limits of the Pedon in Vertisols

The actual limits were set by the normal range in the size of the variability in the Vertisols, for example. It's the same in soils with permafrost, the same size. We took the maximum size to give us the fewest complexes as possible. In the design of a structure, a house for example, on a Vertisol, you have to consider the swelling nature of the whole soil, and not just the center or the edge of the polypedon. You control your shrink-swell by keeping the whole soil moist or dry, so that the moisture doesn't change over the year. These are things that you don't manage as spots; you manage as fields or as good size polypedons. **Question 15, Texas**

14.1.2 The 30% clay Requirement for the Surface Soil

We wanted to maintain the cultivated soils and their virgin counterparts in the same taxon. We didn't want to change the classification except in unusual circumstances, not as a result of a single plowing or a couple of plowings. There are a few situations where that can happen, as in some of the soils with very thin natric horizons in arid regions. If they're reclaimed by deep plowing to bring gypsum up and get the sodium out, the natric horizon is destroyed, completely mixed. It's a drastic amount of change in the soil and enough to warrant a change in the classification. We have among the Vertisols, particularly in Australia, and in some parts of the southeastern states, a very thin eluvial horizon, a matter of just a few centimeters. These are cyclical, too. If the soil were plowed, they would disappear completely. There was a lot of discussion about how much percent clay we should have and what CEC we should have. The people who knew the most about the Vertisols of the Blacklands wanted to have more than 30% clay and a lower limit on the CEC of the soil. However, this experience was all in the Blacklands of Texas. When we got in to other kinds of Vertisols in other parts of the world, the 30% limit seemed to be a reasonable compromise. Having proposed it, it never got criticized. Many features in *Soil Taxonomy* are there because I made a proposal and nobody ever bothered to criticize it. They'll get around to it someday. **Question 18, Texas**

It's not uncommon among Vertisols, where the cracking pattern is large, that, in the centers of the big polyhedrons, you'll find an argillic horizon. That's quite common in Australia. And to keep those all together we require the surface 18 centimeters to be mixed to ensure we had 30% clay, because these albic horizons that get perched above the argillic horizon

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21. Fondo Nacional Investigaciones, Agropecuarias, Maracay, Venezuela.

are normally quite thin. Once you plowed you would be hard put to be sure that they had ever been there. **Question 177, Minnesota**

14.1.3 Rationale to Restrict Vertisols to a Mesic or Warmer Temperature Regime

There was none. I tried to get rid of that unsuccessfully. **Question 62, Texas**

14.1.4 Slickensides that do not Intersect - an Expression of Pedogenic Youth?

In the Fargo Lake plains, the Red River Lake plains in North Dakota and Minnesota we have very fine-textured montmorillonitic soils with a udic moisture regime, really, they rarely crack seriously, very little movement in the soil itself because of lack of serious moisture changes. In these soils, in a given pit, you may find one large slickenside that runs for a meter or so at least, at a much smaller angle to the horizontal than we get in most Vertisols but very well developed slickensides, but there may be only one in a pedon. Whether these, that we find, are due to frost or to occasional wetting and drying, I don't know. It is very difficult to be confident in these soils that freeze so deeply as to whether the freezing has produced the movement or the shrinking and swelling due to moisture changes.

I see no reason why one couldn't find an Entic Vertisol, if he had the proper parent material, developed in a relatively short time. **Question 95, Texas**

14.1.5 Cambic Horizons in the Taxonomy of Vertisols

The cambic horizon was not used as a diagnostic in the Vertisols because the arrangements of horizons are commonly so complex that it was considered undesirable to try to distinguish a Vertisol with a cambic horizon from one without. It is very common that a Vertisol will be developed in a calcareous parent material, but the churning processes that go on from the shrinking and swelling may push this calcareous parent material to the surface in parts of each pedon, while in other parts of each pedon the carbonates are leached rather deeply. We try then to distinguish between that part of the pedon that has a cambic horizon and that part of the pedon which does not have a cambic horizon. We are, in effect, complicating our classification of the soil. It is the intent of the pedon to permit soils to have intermittent horizons that do not occur everywhere in the pedon and the Vertisols are the most common group of soils that have these intermittent horizons. Some of them actually have natric horizons and have albic horizons and have argillic horizons and yet we don't recognize any of those in the Vertisols, yet they are telling us that a single plowing will obliterate them. In the case of the cambic horizons, it would be possible to make a distinction between the Vertisols with and without the horizons but then we complicate our nomenclature at the subgroup level and our series in the U.S. which have these intermittent horizons. They are split so we require complexes of series rather than single series with intermittent horizons.

You may have noticed that the question is whether it is incorrect to give the elimination of the B horizon to an area within a Vertisol, a pedon that is a Vertisol that has been leached in carbonates.

It would be perfectly correct in writing a description where you are using the ABC horizon terminology to label such a horizon as a B. You will notice, however, that we have not in *Soil Taxonomy* used the ABC horizon terminology. We have deliberately tried to substitute diagnostic horizons for that terminology. **Question 3, Venezuela**

14.1.6 Irrigation Creating Vertisols in an Arid Zone?

It is easy to understand why, under irrigation, you find all the properties of the Vertisol. This is because the soil is moistened and then dries, and you have the movement going on. Without irrigation, the soil simply remains dry the year round. Our Torrents generally in the U.S., are in closed depressions where the odd heavy rain shower will flood the playa and moisten the soil, then you may go 10 years before you get moisture again, but it is the same process as flooding or irrigation. I do not like the idea of changing the classification according to whether or not a field is irrigated, but admittedly that irrigation does affect the processes going on in the soil. This will be a problem for ICOMMERT to discuss and make recommendations about. **Question 158, Cornell**

14.2 Suborders

14.2.1 The Concept of the Aquic Moisture Regime and the Elimination of Aquerts

In the *Seventh Approximation* the Aquerts were not defined on their moisture regime but rather on the colors and depths to mottling. Aquic suborders in other groups were defined only as being saturated with water at some season or artificially drained and then in addition having certain specified colors. The concepts of the moisture regimes were not fully developed at the time of the *Seventh Approximation* because the ustic moisture regime had not yet been introduced and the concept of the aquic moisture regime had not been yet defined. In the 1967 supplement, the definition of "saturated with water" was put on an operational basis so that a borehole was required to determine the height at which the watertable stood. The distinction between the xeric and the ustic moisture regimes was also introduced in the supplements to the *Seventh Approximation*. The same problem persisted with the Aquerts that existed in the *Seventh Approximation* because the definition of saturation with water on an operational basis could not be applied to Vertisols, since the borehole measurements were unreliable in such slowly permeable soils.

The intent of the definitions in *Soil Taxonomy* was to provide operational definitions which could be applied uniformly by pedologists with very diverse backgrounds and experience and that would permit the classification of the same soil in the same place by these pedologists working independently and with varying backgrounds. To define the aquic moisture regime, we found it finally necessary to provide an operational definition which involves a borehole or an observational well in which one could observe the position of the water table in the soil by the depth at which the water stood in the borehole. We could not find any other definition which would be simple enough for field men to use. It would be possible to have written a definition in terms of zero tension but this would require that samples be collected and transported to the laboratory and would have been much more costly and time-consuming than the drilling of a borehole. However, in the Vertisols the hydraulic conductivity is so low that you can put a borehole in a Vertisol where the moisture content is virtually zero, that it is saturated, but no water will come from the soil into the borehole. As a consequence, the operational definitions which work in most kinds of soil cannot be applied to the Vertisols, and eventually we had to drop the originally proposed suborder of Aquerts until which time as we could find some operational definition to define that situation. At the time of the printing of *Soil Taxonomy* we had not found such a definition, and in the hopes that one could be found we created a third international committee on the classification of Vertisols.

The intent of the pellic great groups and chromic great groups in the Usterts and Uderts and Xererts was to make the separation that might have been made by the aquic moisture regime, but was then defined in terms of chroma rather than in terms of the soil moisture regime. The attempt did not work when we began to apply the definitions in the West Indies

and in Venezuela. We realized that we would have to find a substitute eventually for the definitions of the these great groups in terms of chromas.

This distinction does not seem to work as was intended and soils that should have low chromas do not always do so. For example, in Jamaica, the wettest Vertisol on the island has a chroma of 3 to 4 throughout in 10YR hues. This soil is frequently flooded for considerable periods of time, and in addition to being very wet, is quite saline. As a consequence, there has been very little vegetation on these soils during their development and apparently there has not been enough energy for the iron-reducing microorganisms to produce even faint models in this very wet soil.

This problem exists in a number of countries where there are Vertisols and not just Venezuela and the West Indies. It also exists in North Africa and India and to a considerable extent in Australia. Although the Australians don't use *Soil Taxonomy*, the problem is there. The French classification as used by ORSTOM has adopted the concept of Vertisols but have simply said that the two classes - the pellic and the chromic great groups are... well, the pellic great groups are those that cannot be drained with surface drainage, and the chromic great groups are those for which surface drainage can be provided, making it an engineering application but trying to solve the same problem of classification. It is the most important one amongst the Vertisols. **Questions 2 and 7, Venezuela**

14.2.2 Different Categorical Levels for Soil Moisture Regimes

To serve the functions of the soil survey, the taxonomy has to be usable as a key for correlation. You must be able to trace a soil down, but if you carry this idea that you must use a given characteristic in the same category for all soils, you are going to come up with, not an infinite number of categories, but a very large number of categories. Then you must completely abandon the nomenclature that we have. I don't think you'll find a better nomenclature in any taxonomy than the one we have. It's a useful one for communication. But this adherence to a strict theoretical insistence on using a given characteristic only once in the taxonomy and in the same category in all soils is going to enormously multiply the number of categories and destroy the nomenclature completely. You must also remember that we make soil surveys at different scales. For the small-scale maps we tend to use the higher categories, generally the great groups or even suborders. For the large-scale maps we use phases of series and families and even subgroups. If we are going to use a given property, such as the moisture regime, in only one category for all soils, then you don't have the choice of making a broad subdivision of soil climate for small-scale maps and a fine subdivision for large-scale maps. You are restricted in what you can do and the people who criticize *Taxonomy* forget completely that we do make soil maps at small scales as well as at large scales. The requirements of the surveys vary with the scale. The taxonomy is intended to permit broad subdivisions for the small-scale maps and fine subdivisions for the large-scale maps. **Question 170, Minnesota**

In rationalizing the use of moisture regimes in soil classification it must be pointed out that for the Vertisols this criterion *per se* is not used for differentiation but rather the periods of cracking. This is so because the concept of saturation of water that has been used for more permeable soils just would not apply to the Vertisols.

14.2.3 Soils with Vertic Horizons Excluded from Aridisols

So far as the aridic soil moisture regime which is used to group the Aridisols, Oxisols or Vertisols, but have some horizons so that they do not get into the order of Entisols, there are several situations. A question was asked in Washington: "Why do we have Torrox instead of Oxids?" Which is more important, the oxic horizon or the aridic soil moisture regime? We may have made the wrong decision, but we decided that if a soil with an oxic horizon and an aridic soil moisture regime was irrigated, the oxic properties still remain limiting to use. In that situation we could deal with the arid climate at the suborder level instead of the great group

level because they seem to be the most important subdivision of the Oxisols according to their soil moisture regime.

The exclusion of the Vertisols that have an aridic moisture regime or at least have an arid climate, I think, is parallel to the exclusion of the Oxisols. It was more important to recognize the shrink-swell potential than the soil moisture regime which, though a limitation, could be corrected. Under use they are going to behave like other Vertisols. In Sudan in the Gezira Scheme, the irrigated soils are Vertisols and they crack, and the cracks close and so on every year and have slickensides, parallelepipeds, and what have you. Just at the boundary of that Gezira Scheme I am told that the soils are not Vertisols, because they never get moist enough to swell. They are dry enough to be cracked, and the cracks that are there are filled with granules, but because there is so little movement in the absence of irrigation, you cannot find slickensides. This will illustrate the reason why the Vertisols probably should be kept together as a group instead of being split according to their moisture regime. **Questions 112, Cornell; 64, Texas; 170, Minnesota**

14.2.4 Measurement of Open Cracks

I should mention that in Venezuela, in trying to classify the Vertisols at the suborder level, there were no records or measurements of the length of time that the cracks were open to 50 cm depth. I solved the problem by discussing the presence of cracks with the cultivators, and they could give me the average date that these cracks appeared, and the average date at which they closed. There is much common knowledge among cultivators, that is better than we're ever going to get in terms of actual measurements. Soil moisture regimes were not used in the classification of Vertisols because the moisture control section is relatively meaningless in a soil that cracks. We used, as a substitute, the period that the cracks were open and the number of times that the cracks opened and closed during the year. It was the intent to define the periods of cracking in such a way that we would have Usterts associated with Ustalfs and Ustults. Whether or not we succeeded with the periods will depend on measurements of at least a few soils to guide us in the classification at the suborder level. **Question 13, Witty & Guthrie**

14.3 Great Groups

14.3.1 Chromic versus Pellic Great Groups

First the pellic and chromic great groups were intended to distinguish between the Vertisols that could be given surface drainage and those that could not. We had at one time a suborder of Aquerts, and the present international committee on Vertisols is discussing the reintroduction of that group. That was dropped because we had no reasonable basis that I could see to define the wettest of the Vertisols. You can, in a soil of medium or sandy texture, put in a bore hole and measure the ground water table. But you cannot do that in Vertisols; you don't know where the ground water is. That suborder was dropped and in its place we substituted the pellic and chromic groups. These are not working well, and this is one of the reasons there is an international committee on Vertisols. I found myself unable to suggest a solution to the misclassification of a number of Vertisols in the West Indies, except by the introduction of slope. Normally, slope is reserved for the phase level in most soils. In a few aquic great groups, and in some Histosols, slope is needed to distinguish between the soils that are wet due to seepage and those that are wet due to low permeability or high rainfall. An entirely different drainage system must be devised where it's due to seepage.

We cannot use the soil moisture regime as such in Vertisols, because our model doesn't work in a soil that wets from the bottom of the crack as well as the surface of the soil. All that we could do was to predetermine the classification of some of these soils. We did that by

keeping the xeric, ustic, and udic great groups together with other xeric, ustic, and udic great groups. With inquiries among people who were familiar with the soils, Vertisols, we proposed a definition of cracking periods and cycles. There was never any criticism of the proposed definition. **Question 21, Texas**

14.3.2 Lack of Great Group Subdivisions for Torrerts

In the Vertisols, the variability of the Torrerts that we knew in the U.S. was very small. None of them had low chromas. In part, I think that this is due to the fact that most of our Torrerts are in closed depressions and periodically flood. The water stays for months or even a year or so. In some areas there is very little vegetation on the Torrerts except for some annual weeds. That means there is no energy source for the microorganisms that reduce the iron to give you the gray colors. Because we didn't find any particular variability, within the soils of the Vertisols of the arid regions, we saw no need for a great group. The suborder was the same as the great group. We could, of course, have put a name on a great group, but we would have had only one. There are other places in the system, as in the Rendolls, where we have figured there was no need for another name for another category. We would treat the suborder as a great group. **Question 22, Texas**

14.4 Subgroups

14.4.1 Sodic Vertisols Dropped from Soil Taxonomy

In the Vertisols, we had at one time recognized the sodium saturation when it became high as a subgroup characteristic but, in Puerto Rico, we have some experiment stations on Vertisols, and the behavior of the sodic Vertisols and the others were exactly the same. This used to puzzle me for awhile until I realized that once the Vertisols swelled up they were just as impermeable without the sodium as they were with it. So it didn't matter except as a potential pH difference. **Question 34, Texas**

14.4.2 Vertisols Evolved from Soils with Argillic Horizons?

That's the theory that the correlators were told. They set up a subgroup of Vertisols because they thought those soils started out as Paleustolls or became Paleustolls first before enough clay had been formed by weathering to cause the churning process to start. In the lower part of the soil you will find a clay skin and so on that suggest it was a very fine-textured argillic horizon at one time. **Question 178, Minnesota**

Appendix A

Use of the Index and Microfiche

The index which follows has two components: a listing of key words and terms referenced by page numbers in the text, and a listing of individual interview questions and their location in the text by page number, e.g. Cornell (#105) 70 means part or all of Cornell interview question number 105 can be found on page 70.

In indexing terms and key words, it was noted that the same term may have been repeated sucessively over many pages of text which followed. In the particularly obvious cases, e.g. the term "Alfisol" in the chapter on Alfisols, the term was indexed only at the beginning of the corresponding section with just one entry as such in the index.

Individual interview questions were cited in the main body of the text and in the index itself. This was done mainly to facilitate searches of the original interviews on microfiche (479 pages total) which can be found in the envelope attached to the back cover of this book.

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