CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPED CONVERSATIONS)

by

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Compiled by an editorial committee at

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Soil Management Support Service

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Addendum to:

THE GUY SMITH INTERVIEWS: RATIONALE FOR CONCEPTS IN SOIL TAXONOMY

by Guy D. Smith

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Preface

Many papers have been published explaining the rationale for properties and class limits used in *Soil Taxonomy, a system of soil classification for making and interpreting soil surveys* (U.S. Department of Agriculture, 1975) before and since its publication. Since *Soil Taxonomy* does not provide these rationale, many scientists felt that it would be useful to document the reasons for many of the decisions explaining the selection of properties and class limits.

The one person who was fully conversant with the system and who co-ordinated its design was the late Dr. Guy D. Smith. In 1976, Dr. M. Leamy and staff of the Soil Bureau of New Zealand conducted a series of interviews with Dr. Smith. These interviews were published in the *Newsletter* of the New Zealand Soil Science Society and later reprinted in *Soil Survey Horizons*. 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 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.

Dr. H. Eswaran put an extraordinary amount of work in transcribing a large set of original tapes. These were at a later stage compiled, edited and indexed by Dr. T. Forbes, who also coordinated the final publishing.

As indicated previously, the interviews are not necessarily complete. There are still many more questions that could be asked. However, this monograph serves to provide some aspects of the thinking that was behind the formulation of the document. From this point of view, we hope this will be a useful document to all users of *Soil Taxonomy*.

Interview at the Agronomy Department of Cornell University

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Question 1

A. *Perspective of Concepts that had Major Impacts on Soil Taxonomy and of the Procedures Used by the Soil Survey Staff in its Development*

Publications about *Soil Taxonomy* have dealt with a large number of elements of this topic. Yet it appears that misconceptions exist about some that have been discussed as well as some that have not. Some of the questions that follow have been selected to help rectify certain of the more common misconceptions. Others relate to the way in which the system was developed, for most of those who were not involved in the development of *Soil Taxonomy* have little idea about how the Soil Survey Staff operated, the number of people involved, or the magnitude of their collective efforts.

I. Impact of historical concepts on Soil Taxonomy

The nomenclature and ordering of taxa are so unlike those of earlier classifications that concepts prominent in the first half of this century are so well camouflaged in *Soil Taxonomy* that many appear to perceive little transference of ideas from the old to the new. Will you comment specifically about the impact of the following on the ideas that underlie *Soil Taxonomy* or its parts? We are asking primarily for comments that concern transference of ideas as they may relate to form and substance of the system.

a. The fundamental theses of Dokuchaiev

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 ground water the topography, and the age of the landform.

Question 2

It seems to me that people like Dokuchaiev were working at a pretty high level, rather than that at series and type level like the Americans were using in soil genesis, morphology, and taxonomy. Do you want to comment on that?

Guy Smith:

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).

(So the answer to the first question is that, really, the fundamental thesis of Dokuchaiev has been used quite specifically in *Soil Taxonomy* and has had a tremendous impact on *Taxonomy.* Is that correct?)

That's correct. We acknowledge that in Chapter 1.

Question 3

The question relates to the impact of Marbut's concept of the normal soil. It is used in *Soil Taxonomy* and it has influenced the structure of taxonomy, especially at high levels. Could you comment on this?

Guy Smith:

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 4

What would happen to Histosols in his concept of normal soils?

Guy Smith:

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 5

Guy, don't you think that, except for the Entisols and the Inceptisols, the other orders more or less correspond to normal soils of Marbut, or not?

G.D. Smith:

No, you would 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 6

Marbut translated Glinka's book from German, which was derived or incorporated Dokuchaiev's ideas. How much change was there as Marbut took the translations from German, in terms of the original ideas, of Dokuchaiev?

Guy Smith:

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 loessial 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 7

We may shift to the discussion on the concepts of zonality which, I think, are derived from the concepts of normal soils. We may also include the intrazonal and azonal soils. Had these three concepts an influence on the structure of *Soil Taxonomy*?

Guy Smith:

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 1 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 1 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 8

Maybe I could be a little bit more specific. I still think the intrazonal concept in terms of poorly drained soils. There was a possibility to make an order of poorly drained soils--putting all the aquic suborders together you would have more or less an order. That was not done, although it was for people who had to map and had to classify an easy way to get rid of a group of soils that was rather disturbing sometimes. You put aquic properties at the suborder level. What were the major reasons not to have an aquic order?

Guy Smith:

First 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 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 suborders 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 9

One thing that has caused us quite a bit of a problem is that the concept of Aquic moisture regimes is reflected in different categories in *Soil Taxonomy*, just like you could have Udic moisture regime in Entisols reflected, say, at the great group, where in Alfisols we have it reflected at the suborder. Would it be helpful to be more systematic?

Guy Smith:

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 10

The series concept as used in the 38 classification system does not seem to have changed a whole lot as it currently stands in *Soil Taxonomy*. Would you agree that there was very little change between classification systems on the series, and that most of the changes were in fact at higher categorical levels?

Guy Smith:

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. 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 11

Guy, would you say that the resistance to shift in series has been parallel with the advancement in soil survey programs in the States. I could almost see in some States it made no difference, like Nevada, but in States like Wisconsin I can remember that they had Miami-Wisconsin and that was to distinguish the Miami soil that was in Michigan and Illinois. It does not seem as if the resistance to change was uniform across the United States, but rather that where you had strong programs you got more resistance. Would you say this is true?

<u>Guy Smith:</u>

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 mapping 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 12

The next question deals with the relation of the central concepts of the zonal great groups of the 49 system in general to ideas and concepts in the development of *Soil Taxonomy*. Marlin had indicated that he could give some good concrete examples of the 49 system with respect to categories in *Soil Taxonomy*.

Guy Smith:

I might use the red-yellow podzolic and the grey-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 grey-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 grey-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 13

Do you want to say something of the concepts of the intrazonal great soil groups in relation to development and concepts of *Soil Taxonomy*?

Guy Smith:

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 14

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 Paleudalfs 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?

Guy Smith:

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. In the study of the inter tropical 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 not available to us at that time. We have similar situations in Malaysia that we have learned about within the last 2 or 3 years, and so this is one reason that we have an International Committee on Oxisols, which is desperately needed. We had only a limited number of samples from Puerto Rico and Hawaii that we could study, and these were virtually all formed in basic igneous rocks. These were by no means a good selection of the Oxisols as compared to the soils of South America, Africa, or Southeast Asia.

Question 15

Another potential theoretical genetic sequence would be Entisol-Inceptisol-Spodosol. In this instance, however, exclusion of loamy sand and coarser textures from cambic horizons also exclude soils having subsoil horizons, that grades from the Inceptisol order, disrupting the theoretical sequence in coarse textured but not in finer textured soils. One of Professor Rust's contributors noted that this is confusing to students. Will you comment on this apparent discrepancy?

Guy Smith:

This question relates to the exclusion from cambic horizons of sands and loamy sands. We tried at one time to develop the concept of a color B horizon; the cambic horizon at that time was called a color B, because it was redder in hue and higher in chroma than the horizons above and below. The color B, however, in the sands and loamy sands do not necessarily reflect any particular amount of change due to genetic processes. It takes very little free iron and humus to coat the sand grains, but it takes a great deal more to change the colors of silts and loamy soils. The soils with sand and loamy sand particle size, as I mentioned elsewhere, have a number of very important common properties: namely low water holding capacity, blowing, poor trafficability when dry, and 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. I ran across a number of late Pleistocene or Holocene sands in western Europe that have a rather distinct difference in color between the B horizon position and the horizons above and below. Yet we cannot identify any significant differences in the degree of alteration of the sandy parent materials in these horizons. We rather suspect that the change in color is a result of manuring rather than of soil development.

Question 16

The work on geomorphology in relation to soils was well advanced while *Soil Taxonomy* was being developed. What impact, if any, did the concepts derived from this work have on *Soil Taxonomy*?

Guy Smith:

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 it 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 17

What place does the transport/deposition of parent materials have as they influence soil genesis. Do you see that as being an important feature like soils that have discontinuities due to different episodes of deposition and erosion and those kinds of things. How do these fit into *Soil Taxonomy*?

Guy Smith:

We tried to keep them out of the higher categories of *Soil Taxonomy*, to restrict them largely to the family category, where the transport was 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 18

In regards to that last statement, one soil order that is giving us quite a bit of trouble in New York is Spodosol, especially if you cannot qualify a spodic horizon based on field criteria, and you are forced to bring the samples into the lab and go through analysis of iron and aluminum. We find the chemical criteria quite restrictive, as it currently stands, and only your best spodics will meet the criteria. Those are usually fairly obvious in the field, and some of the Canadians and some other people have been developing criteria that are less restrictive so that will leave these soils clearly in Spodosols instead of putting them back in Inceptisols. I was wondering if you could comment on that chemical criteria for spodics, and whether or not it should remain as restrictive as it is.

Guy Smith:

There is a gradual transition from soils with cambic horizons to soils with spodic horizons and this is in New York State particularly troublesome. 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. Now our Canadian chemists complain we put too much emphasis on field identification. Our Canadian field men complain we put too much emphasis on laboratory identification. That's about the best compromise we were able to reach with the state of knowledge at that time.

Question 19

I think the problem with Inceptisols and Spodosols is quite like the color B and cambic horizon. You do get a morphology of a podzol within 50 years, whereas you do not get the other associated chemical changes more indicative of good podzols.

Guy Smith:

I do not know. Fifty years may be a little short, but we have some studies in Alaska that indicate that it can be identified in the laboratory after about 75 years. I might add that 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. It is exactly the same mechanism as we get in the soils from ash that causes the pH to rise.

Question 20

Soil Taxonomy (page 71) states that after defining soil orders to reflect concepts of dominant sets of soil-forming processes, soils thought to have significantly different genesis within a given order are segregated at the suborder level. Many of the former intrazonal soils segregated at the highest level in the 1938 and 1949 systems are included in the same order as their zonal and azonal counterparts in *Soil Taxonomy*. What considerations led to subordination of concepts of genesis at the order level?

Guy Smith:

This question is partially answered elsewhere. 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 to 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. The reasons for including the Aquolls with the Udolls and Ustolls are discussed under the question (to be added later). 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 21

Some individuals apparently think that the architects of *Soil Taxonomy* identified important properties of soils, weighted them in some manner, and only then applied them in the system to arrive at a hierarchy of taxa of the higher categories. That idea may arise from the fact that it is consistent with the way *Soil Taxonomy* is applied to classify soils. Will you describe in general terms (a) how the taxa of the higher categories evolved, (b) the approximate numbers of soil scientists involved, and (c) the magnitude of the effort that was expended in the assembly of field and laboratory data, examination of potential schemes and repeated testing.

Guy Smith:

The concepts of the taxa of the higher categories evolved only slowly; we tested many alternatives by placing the soil series into the system according to the definitions of the various approximations starting with the third. John Stuart Mill points out that the best classification is the one which permits the largest number of the most important statements to be made about the object, and by importance he means that these statements are of high relevance to the objective for which the classification is to be used. Therefore, you may have several equally valid classifications of the same objects according to the purpose that the classification is to serve. *Soil Taxonomy* was intended to serve the purposes of the soil survey which includes mapping and interpretations. So, starting with the third approximation, the correlation staff of the Soil Conservation Service was requested to classify all of their series as best they could according to their current knowledge to see what kinds of groupings evolved. At that stage we were not yet prepared to go into much detail at the family or subgroups level, though concepts were developing of what kinds of criteria should be used for these two categories. When the correlation staff, which included something like 100 people at the Washington, regional, and state offices, examined the kinds of groupings that resulted from the criteria proposed in the various approximations they found various defects; there were soils that were genetically dissimilar included in the same taxa; there were soils with rather different sets of horizons included in the same taxa; there were soils that had no place; and these deficiencies then 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, and the development for example of the sampling of the soil in such a way that we could get a measure of the total amount of organic matter for even 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 for 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 whether 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 be to 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 that against 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 22

If you had to go through this whole process again, or if the whole thing had been started over, would you make any changes that would streamline it, or would you look at some aspects differently?

Guy Smith:

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 here. I would go about it the same way. I have found many errors in the classification. If we judge the classification by what statements we can make about the soils that are grouped, and for those errors I would correct them, but before they had to have an international committee, but some things requires a great deal of correspondence and discussion between knowledgeable people. There is no one man who can know 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 23

Do you think the mechanisms for updating *Soil Taxonomy* as it now stands are adequate?

Guy Smith:

It 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 24

Could you elaborate on the kinds of purposes each of the five categories in *Soil Taxonomy* are expected to serve? This will assist us when we make suggestions to refine the system.

Guy Smith:

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 25

When was the decision made to have only ten orders? Any particular reason to have only ten?

Guy Smith:

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 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.

Question 26

Will you enlarge on the concept of the pedon as a sampling unit and the basis on which its definition was based? One questioner criticizes the definition as vague, arbitrary, and without a firm basis in observed or measured characteristics.

Guy Smith:

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 that 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 carry 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 he starts his measurement.

The basis for setting the limit 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, 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 27

Has there been any discussion of having a concept of a minimum-size horizon? I think the pedon is the smallest sampling unit but it is complete as a soil. Has there been any discussion on having a minimum size for particular horizons during the development of *Soil Taxonomy*?

Guy Smith:

No, not that I can recall. There have been questions about the minimum thickness. This should be thick enough that it is observable to more than one person.

Question 28

The oxic horizon has to be 30 centimeters; that is the minimum size. Some horizons have been assigned a minimum size, but nobody ever talked about developing a concept of the minimum horizon I would say. When do you take into account the thickness?

Guy Smith:

We have minimum limits on cambic horizons, on spodic horizons, on oxic horizons, 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 2-1/2 centimeters minimum thickness for the argillic horizon.

Question 29

Was the sampling of Vertisols a basis for devising polypedons rather than pedons?

Guy Smith:

Well I do not quite agree to that. There are other reasons for the polypedon. This is the unit that we are trying to delineate on large scale maps. On small scale maps the polypedon has little relevance. The polypedon has properties that are in addition to the properties of any one pedon. There is a wider range in properties in a polypedon than in one pedon. The polypedon has natural boundaries where it grades into other kinds of soil, which the pedon does not have. The polypedon has shape; it has slope; if we try to classify according to slope percentages, for a pedon, it makes a big difference on whether that has been ridged for the growth of potatoes or something else. The slopes are very steep actually, although the polypedon may be very level or vice versa. 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.

Question 30

I think in our sampling and characterization programs within each State, we are forced to deal with the question of sampling and sample analysis, and so often we tend to select pedons that are going to fit some known series. So we tend to be involved in biasing our sample, usually towards the center of the range of the series. We select a pedon rather than a polypedon sample and analyze it. What I am wondering is if we are really getting information that is relevant to the polypedon ranges; in other words we pretend to be focusing on a pedon, usually near the median or center of the range. Do you think there should be more sampling of a series of pedons within a polypedon or do you like the concept of taking just one pedon in the middle of the range?

Guy Smith:

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 31

You say a polypedon is a natural unit delineated on large scale maps. Is there a natural body delineated on small scale maps like soil association maps?

Guy Smith:

I do not know of any. 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 32

When you are in a Spodosol area it is relatively easy to visualize a pedon or polypedon, series or higher categories. But when you are in some Oxisol areas like some of the Cerrados of Brazil, these have very large units and it is not easy to put a limit and say here we have a pedon and here a polypedon and probably that may be one of the reasons why people in the tropical areas do not appreciate this concept and this might apply to Histosol areas probably and some Aridisols.

Guy Smith:

I will go back with you to 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 33

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?

Guy Smith:

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 (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 34

The sampling for the characterization of series is rather different from the type of sampling that the soil surveyor does. The soil surveyor only samples parts of pedons with an auger so that only a small sample is brought up. How does that relate to the requirement of *Soil Taxonomy* that we classify using full pedons?

Guy Smith:

In sampling the pedon you are correct that in the deep horizons, 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 35

Related to the weighting of genetic concepts and interpretive value in the choice of criteria for the higher categories, several individuals have asked in various ways the basis on which decisions were made about the choice of criteria in the higher categories. When two, not always compatible objectives were to be served namely (a) to reflect natural genetic relationships for a fundamental understanding of soils in nature, and (b) to provide interpretive value for applied purposes. Will you describe how the two were reconciled in categories above subgroups, and which was given priority, if reconciliation did not appear feasible?

Guy Smith:

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. One might be interpretive; an example would be where we have grouped all the sands into Psamments, unless they had a spodic horizon or an argillic horizon, putting them into Entisols rather than splitting them between Entisols and Inceptisols on the basis of a small difference in color in the subsurface or subsoil horizon. So we have also in some of the Psamments a strong genetic relationship, namely where the soil is virtually 100 percent quartz sand. It is very difficult in such soils to form any horizons, because, of course, it is so difficult to weather and form horizons. However, some of the Psamments are very ancient soils, some are just recent dunes, and we have no way to distinguish them at the moment. This is a matter of interpretation of the individual; it is not anything that can be documented, unless you actually see the sand blowing, which is not too often the situation. So, 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 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 in their place.

Question 36

If my understanding is correct, above the subgroup the genetic considerations have priority above the interpretation.

Guy Smith:

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, then if we could not separate them at the subgroup level, we had contrasting kinds of soil in the family, with differing kinds of interpretation, and 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.

Question 37

Is this true, that genesis was given priority also at the suborder level, where you have the main subdivision on the basis of climate? Was that also a genetical separation, or was it land use consideration?

Guy Smith:

Climate is both, actually. It controls genesis, it does not result from genesis, but it controls it. It is exceedingly important to soil behavior. So many of these factors are both genetic and interpretive.

We take the present climate. If you tried to take the climate that was responsible for the development of the characteristic, you are speculating, and this depends on what your professor taught you as an undergraduate student, and on what you have seen, how much you have unlearned of what he taught you.

(I am teaching my students that at the suborder level you take the climate because the climate is so important for land use, and it is not a genetic consideration that put it up at that level. Am I wrong?)

Guy Smith:

You are wrong, I think. I think the climate is also important in the genesis of the soil. Do you have water going through the soil, and on down to the ground water so that you have a leaching environment? Or do you have water going in and then being withdrawn, so that the soluble things tend to accumulate? The calcic horizons are the result of soil genesis. They require a dry climate to form. So the climate in this situation, interacting with the carbonates of parent material, produce the calcic horizons in the Ustalfs. In the Aqualfs, calcic horizons are due to another factor. They are due to capillary rise of ground water and evaporation of the surface. This is another genesis of the calcic horizon.

Question 38

I am concerned that if you apply this principle that the present climate is going to reflect genetic factors, I think that in the tropics, where you have these strongly weathered materials, that past climates had an influence on the present characteristics, and that you selected for classifying the Oxisols, based on ustic and udic mainly for interpretation, and less for genesis.

Guy Smith:

Maybe so, but I will not agree with that. 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 39

When we talk about interpretations, would it be fair to say that we give more weight to agricultural interpretations in higher taxonomic classes than we do to urban or other kinds of uses?

Guy Smith:

Perhaps we do, 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 that 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 40

On the new modified concepts that *Soil Taxonomy* introduced--soil temperature and moisture. The question is, you have adequately addressed the question about whether or not soil temperature and moisture be considered soil properties. There remains, however, historical perspective of the decision to use them as criteria at the suborder level and great group level. Will you discuss this?

Guy Smith:

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 41

Was there any time that soil temperature and moisture had been considered to be used at the highest level, like some Russian systems have the soils of the tundra, soils of the tropics, at the highest level. Has this been ever considered when *Soil Taxonomy* was developed?

Guy Smith:

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 pedologist mapping 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 42

If you consider moisture regime and temperature being properties that are important to soil genesis, what was the reason only to place that at the second level instead of the first one? Has there been any discussion of that?

Guy Smith:

One could start 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 43

At the beginning stages of *Soil Taxonomy*, the several approximations, was this problem discussed quite strongly? Strong opposition came from some people, or was everybody in agreement at least in the United States?

Guy Smith:

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 44

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.

Guy Smith:

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 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 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. 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 45

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, may be down to a meter and a half of these older surfaces seem to indicate that material was not in place for a real 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 weathered material itself is more the pale feature.

<u>Guy Smith</u>:

There is no question but that this 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 completed 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 46

Please describe the concept that the term "Rhod" is intended to imply, and the background of the decision to recognize it at the great group level.

<u>Guy Smith</u>:

This was answered in question 28 from Dr. Leamy. I do not believe that I can elaborate on that, but for your information I can make a brief resume.

It is primarily from the Rhodudalfs, 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 deemphasized 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 47

If my understanding is correct, then the Rhodic groups were made for interpretation more than for genetic reasons?

Guy Smith:

There must be a genetic factor to have the dark red colors. Normally, this is because these soils were formed on basic or ultra basic 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 covering the same range of climate, namely the reddish brown lateritic soils, and the red-yellow podzolic soil. 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 48

Why was not Rhodic or similar color connotations used in the Oxisols?

Guy Smith:

It was 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 49

Please comment on the background for the decision to recognize the sombric horizon as a diagnostic horizon, and the genetic implications of its attributes.

Guy Smith:

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 in 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 inter tropical 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 inter tropical 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 and 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 50

What are the differences and similarities of sombric horizon, and the spodic horizon of a Humod?

Guy Smith:

We do not know much. 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 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 51

We have orthic suborders, we have Haplic great groups, and Typic subgroups. Will you characterize the concept each of these terms imply and differences among them? Do any imply a central concept or standards of comparison within the taxa of which they are members in the next higher category?

Guy Smith:

None of these 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" simply 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.

Question 52

The next question is related to it, on the intergrades and extragrades.

Guy Smith:

It was intended to be comprehensive or to be modified so that it will 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 degrees, so it is not dry more than half the time that the temperature is above 5 degrees 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 a 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 subgroup we define 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 53

In the extragrades and intergrades, one example is given of moderately deep soils. This comes up time and time again that we should have built into the system the 20 to 40 inch moderately deep soils as reflected much like we do the shallow soils. Clearly it is quite important for agricultural interpretations, why did we not recognize a moderately deep soils in the higher taxonomic levels.

Guy Smith:

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 correlations 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 ones, the lithic contact is of such overriding importance to interpretations that it seemed worthwhile to put it in a the subgroup level. It does represent not 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 that is the wrong place to put a cemetery.

Question 54

What about the second example, Guy? May be you could make some comments the way that you understand the problem?

Guy Smith:

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 Ultisols, 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 55

Permafrost is treated as an extragrade property in *Soil Taxonomy*. Some of these soils, particularly among those which are wet in the summer, exhibit the effects of cryoturbation to an extent that the mixing is analogous to that which controls the character of Vertisols. Some workers in the arctic contend that these soils merit recognition at the order level, that cryoturbation is the dominant process. Will you comment on that suggestion?

Guy Smith:

In the first place, these men are working in the arctic region, but they 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--the 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 56

Inclusion of Andepts in the Inceptisol order required enumeration of a number of their definitive properties in the definition of the Inceptisol order and enlarged the range of that order substantially. It is not surprising, therefore, that a new order of Andisols has been proposed. It could be helpful if you would (a) describe the basis for the decision to include these soils in the Inceptisol order, and (b) discuss the rationale for including soils of the Vitrandept great group in the Andept suborder, then finally discuss the rationale for recognizing an Andisol order.

Guy Smith:

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. 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. However, the proposal I made for establishment of a new order of soils was based on my experience with them in the West Indies and in New Zealand. The soil series of the various islands of the West Indies could be classified by the definitions in *Soil Taxonomy*, but having classified them by the definitions, there was nothing you could say about them except that they had these diagnostic properties. They had very little else in common. The use of color in defining the various great groups was greatly overemphasized. We called for dark colors on some of these soils, and we found them in the island of St. Vincent, black soils, but the black color was from the cinders, not from 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. The classification did not distinguish according to the soil climate, so that one could have for small scale maps a variety of climates from polar to equitorial. Only at the family level could one distinguish the differences. When I finished classifying the soils of the West Indies, I realized that it was impossible to make any interpretations at the family level. I could find there no relation whatever between the value of the surface horizon, the chroma of the surface horizon, and the content of the organic matter. It just did not exist. There is, it has been pointed out, apparently some colorless organic matter in inter tropical soils, and while they are realizing the imperfections of the classification of this suborder. I thought that New Zealand would be a good place to work on this problem. 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. I went to New Zealand with one purpose: to try to devise a more rational classification of these soils from volcanic ash and cinders and pumice. 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, and it was this 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 the soil climate, much as we did Alfisols and Ultisols and so on. The moisture holding capacity varied enormously between the skeletal classes, sandy skeletal, loamy skeletal, in which the rock fragments were pumice, and the skeletal families in which the rock fragments are limestone or granite or something else. Very sandy skeletal pumiceous soils in New Zealand will hold, in an available form, more than 1 year's rainfall for the growth of the *Radiata* pine. Any other skeletal soil will have 1/10 or 1/20 of the moisture-holding capacity of the pumiceous soils. So we had to have a new grouping of particle-size classes for the soils from pyroclastic materials. There is a difference between pumice and cinders in terms of their moisture-holding capacities. The cindery soils are very much lower. We needed a new set of particle-size classes for the soils from pyroclastic materials. We needed to be able to bring in the soil climate in such a way that we could make some interpretations when we got down to the family level.

Question 57

Many soils disturbed or made 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, if not outside *Soil Taxonomy*, at least a separate taxa at a high categorical level. One questioner, for example, asks whether a great group listing as Udorthent is needed for mine spoils in the humid regions. Another possibly referring to Arrent suggests that *Soil Taxonomy* provides little guidance for classification of soils greatly disturbed by man. Will you discuss the guiding principles that should govern the placement of such soils in *Soil Taxonomy*? It may also be useful to remind readers of the devices available to segregate those manmade or disturbed soils whose attributes give them unique potential for use from other soils with which they are grouped at a high categorical level.

Guy Smith:

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 that you have not had a chance to see transcribed as yet. My experience with the Arrents 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 the soils that had been badly gullied with, in the loess 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 smooth 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 Arrents 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 burrow pit than to identify it as an Arrent. 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 58

How would you consider the terrace soils? Large areas of the Middle East have terrace soils or paddy soils in Southeast Asia. How do these fit into this?

Guy Smith:

Those 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 introduction, I believe, or the preface. 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 59

The areas with strip mines are becoming very large in the U.S., clearly in Pennsylvania, Ohio, West Virginia, and to a lesser extent the Midwest and West. We do have a lot of documentation, descriptions, and data on those. I think it is relevant to address the question whether we need a new suborder or great group such as "Spolents", whether or not they should be defined by series criteria. A number of different States are submitting proposals both on a series basis and as a great group or family classification. Do you have any feelings on how this should be handled?

Guy Smith:

I again, am quite ignorant on a good deal of this. 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 60

Often in strip mine spoils we do find there is an order because coal is usually a particular seam if stripped. There are usually certain beds, whether it be limestone or shales or some particular beds above it, and the strip mining process often deals with the same procedure to extract it, and very often we have large areas with calcareous shales on the surface, or other areas where there was a sandstone band which comes out in large stones. There are certain pH's, certain slopes. There is, surprisingly, a high degree of repetitive surface materials on strip mine spoils in certain areas, depending on the mining techniques. Actually, they do repeat themselves, and you can define certain physical properties in each map unit.

Question 61

The basis of limits defines soil criteria and taxa. The first point is the historical perspective of the evolution of criteria and the limits. The question is, will you discuss in detail to illustrate how the limits of taxa and the definitions of criteria were established, including the successive approximations, the testing, and the field and laboratory investigations involved. Perhaps, an example such as how the limits, defined for the mollic epipedon, evolved would be helpful.

Guy Smith:

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

Question 62

What was the basis for using textural class limits of the fine-earth fraction in the family particle-size classes different from those of the conventional textural requirements? You might wish to mention the common misuse of the term "family texture."

Guy Smith:

Texture refers to the particle-size distribution of the textural triangle published in the 1952 *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 non plastic 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 non plastic. 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 non plastic 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 non plastic 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, so the very stony soils with coarse stones boulder-size were described in terms of the particle-size distribution of the fine earth fraction, and of the gravel and the stones that were not large but the boulders were disregarded. 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 63

Why, in lithic soils, did the family criteria go from 11 groups down to 9 groups? What was the reasoning behind that for particle-size class?

Guy Smith:

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 particles 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 interpretive value of the groupings 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 64

You have not mentioned anything about the difference in the mineral composition of the clays, for example, going from the silicate clay minerals to the amorphous material and its effect on plasticity, and how it came out in terms of the percentage of clay.

Guy Smith:

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. (I will try to add the reference here.) 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. 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 non plastic in the Atterburgh 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, the plasticity are changed irreversibly on drying such soils.

Question 65

Why must the base of a horizon extend below a depth of 25 cm to qualify as cambic? Specifically, why a shallower cambic horizon should not be recognized in arid regions, where horizons are commonly thin and shallow, especially since shallow natric horizons are recognized.

Guy Smith:

The natric horizon is much easier to identify in the field than the cambic horizon. The latter is not easily defined, and some limit must be specified, or we will have cambic horizons in every soil that we find anywhere in the world, provided by definition, of course. It may not be transitional to an argillic horizon or a natric horizon. In the arid regions, the cambic horizon would be identified as such if we had no thickness limit. There would be no Aridisols of any significance in any arid climate. Everybody would be able to find a horizon of 1 cm or more thickness somewhere between the surface of the soil and the underlying material. The 25 cm limit was proposed, therefore, to insure that the cambic horizon would be thick enough that different people would agree on it. If the carbonates accumulated at a depth of less than 25 cm, then there would be no cambic horizon in the soil. We did the same, not just for the soils of arid regions, but also for soils of humid regions, provided that the temperature was not extremely cold, that the likelihood of any potential cultivation was virtually nil. Admittedly, there are many soils in arid regions that will not be irrigated and cultivated. There just is not enough water to go around the world's arid regions. Nevertheless, we did not want to change the classification of these soils if they were irrigated. We wanted to keep, as we did throughout the system, the cultivated and the undisturbed soils together in the classification. The limit of 25 cm was proposed because that is the normal depth to which the soil is disturbed under cultivation. 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 and the undisturbed soils.

Question 66

Why were cation exchange procedures for limits within the Andept suborder not modified taking into account the pH-dependent charge of these soils? The questioner notes that the methods used may bias the results of base saturation determined by the ammonium acetate method.

Guy Smith:

It 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, still we have some that compares the retention of pH 7. We realize that the base saturation should not be used as a differentiae in these soils with x-ray amorphous clays. So we have an international committee reexamining the classification of such soils.

Question 67

Why does the oxidic mineralogy class require less than 90 percent quartz? See the table on page 387 of *Soil Taxonomy*. The questioner was referring specifically to Oxisols, Ultisols, and Alfisols derived from basic igneous rock.

Guy Smith:

I cannot understand this question. The soil derived from basic igneous rock is not going to have 90 percent quartz. Such a soil cannot exist, and there is no way that I can answer such a question. I think it will drop out of the publication. Ninety percent quartz from a basic igneous rock!

Question 68

Why have the carbonates been excluded from weatherable minerals?

The question referred specifically to soil of arid regions.

Guy Smith:

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 a well developed argillic horizon, that were noncalcareous at one time. If we examined the soil carefully, the carbonates are on the ped surfaces and not in the ped interior. These are soils that have been recalcified, presumably from blowing of calcareous dust or from calcium that is brought in by the rain. In thinking about the weatherability of the carbonates as a factor indicating prior weathering, because 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, we excluded them specifically from weatherable minerals so that we could take into account what had happened in some previous weathering cycle.

Question 69

What was 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.

Guy Smith:

The basis here 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 70

What were the bases for the 18, 35, and 60 percent clay; 15 percent fine and coarse sands; and 35 percent by volume rock fragments, limits of the family particle-size classes?

Guy Smith:

We have already discussed the 18 percent limit for clay in the family particle size class under Question 62. The 35 percent limit on clay again was set by the comparison of the soil texture, and the Atterburgh limit. There seems to be a significant break at about that limit. Even though one stratified the samples by orders, the important change in the Atterburgh limits was in the neighborhood of 35 percent clay. The same study indicated that there was another important break in Atterburgh limits somewhere in the neighborhood of 60 percent clay. Again, without regard to the nature of the clay, whether 1 to 1 or 2:1 clay; theamorphous 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 71

These limits in the family textural triangle have not been defined, but there are places in *Soil Taxonomy* where other numbers are used. For example, in the oxic horizon, at least for the moment, we use 15 percent clay as a minimum amount of clay. Would you consider that when we change parts of *Soil Taxonomy* to change those numbers so that they fit the family criteria we have now?

Guy Smith:

I have proposed the complete removal in the clay limit in the definition of the oxic horizon.

(My question is, so that we can standardize these numbers throughout the *Taxonomy*.)

Without the limit of 15 percent clay in the oxic horizon, the families would be standardized at 18 percent.

There are not many other places. That was a serious mistake, based on the assumption that there would be no silt in such a soil. [illegible] unhappily the evidence that we have accumulated now is that there may be an appreciable amount of silt, quartz silt. It may be an artifact of the mechanical analysis. It may be that the dispersion process produces the silt, but it is measured in the laboratory.

Question 72

What were the bases for (1) the 35 percent base saturation between Alfisols and Ultisols, and (2) the 50 percent base saturation requirement to qualify as Mollisols having argillic or cambic horizons?

Guy Smith:

The first question, on the 35 percent 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 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. 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 percent, but ammonium acetate showed base saturation in excess of 50 percent. 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. We have (in the second part of the question) 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 73

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.

Guy Smith:

This was discussed. 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 74

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?

Guy Smith:

We had no information on such soils, and when we wrote *Soil Taxonomy* we did not know of their existence. 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.

(You did not anticipate all the soils to get that far north, did you?)

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 75

It looks to me that the purpose of base saturation, the use of base saturation data is more nearly directed towards recognizing different soil groups, and the significance of these limits do not seem to be evident too clearly in crop growth (may be) and certainly not in plant composition. At least I cannot see any. I think it is a useful tool for classifying soils, but not for understanding plant growth and plant uptake. Base saturation only seems to be useful only as an estimate of the reserve that the soil has of that particular element.

Guy Smith:

It 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 76

I think the other concern which I see in this particular question, and which has been voiced by many other people is the reason why 35 percent base saturation by sum of cations was used for Alfisols/Ultisols, while a 50 percent base saturation by ammonium acetate CEC was used for Mollisols and Inceptisols.

Guy Smith:

I have not yet come to the second part of this question. 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 cannot assume what the exchange capacity really is. 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 77

I did wonder about the reason behind the 60 percent base saturation for separating Dystrochrepts and Eutrochrepts.

Guy Smith:

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. The next question refers to another example, where we tried to keep soils together that were sitting just on the limit of the break between Ultisols and Alfisols.

Question 78

I do not quarrel with the 60 percent limit between Dystrochrepts and Eutrochrepts, but there is some fine print in *Soil Taxonomy* that says "or carbonates within the soil." That brings us back to the basic definition of what is soil, but there is some fine print in the first couple of pages of *Soil Taxonomy* that said rooting depth of perennial plants which could be 1 or 2 meters. So, in the state now we have 3 or 4 soil series that we have very low base saturation data in the 25 to 75 centimeter control section, or depth that you normally look, but we also have carbonates at a depth of 70 or 80 inches very close to the 2 meter depth. As a consequence, we are forced to call them or we think we are forced to call them Eutrochrepts when we know they have a very low base status in the rooting zone. But I was wondering why we did not set a lower depth limit of carbonates say at 1-1/2 meters or something more reasonable than to look clear down even at 2 meters in depth. Do you have any comment on that, or how you would decide a case like that?

Guy Smith:

Simply that we 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 in the Soil Conservation Service to criticize that proposal, and no criticism was received. So the proposal being un criticized has come down into *Soil Taxonomy*. Now it is time, when you have examples, to reexamine the definitions.

Question 79

Would you comment on the colors of the argillic horizon?

Guy Smith:

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 possibly get, we changed the depth limits according to color to keep these soils from basic rocks together.

Question 80

What was the basis for establishing (1) a 50 cm depth limit for surface mantles of buried soils having thick (more than 100 cm) diagnostic horizons, and (2) a minimum of 30 cm in surface mantle and at least half of a thickness of thinner diagnostic horizon of buried soils?

Guy Smith:

The recent mantles normally are from alluvial or aeolian deposition on a preexisting soil; we have got a lot of new ones today somewhere in the neighborhood of Mt. St. Helens. When do we classify the soil on a basis of a buried soil or on the basis of the surface mantle and treated as an overwash or overblown phase; you have to have some rules. We did consider that we could normally disregard in the *Taxonomy* a surface mantle of 10 or 25 or 30 centimeters and treat it as a phase. But what would be the maximum thickness at which we would be unable to treat the soil as an overblown or overwashed phase and have to treat it on the basis of the properties of the new mantle. We needed some sort of sliding scale according to the strength of development of the buried soil in flood plains, in rivers, and in soils from volcanic deposits. You normally have a succession of buried soils, all weakly developed, but still apparent in the field. So the sliding scale that we proposed was the one that is questioned. There were no criticisms of that and again the original proposal which was arrived at by discussion principally of the Washington staff has come down in print in *Soil Taxonomy*.

Question 81

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?

Guy Smith:

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 82

Why were different methods of determining base saturation criteria specified for Inceptisols and Alfisols?

Guy Smith:

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 83

What was the basis for the 24 milliequivalents per hundred gram clay limit for low activity clays?

Guy Smith:

I suppose this 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.

(Is not the 24 meq. limit going to make many oxic subgroups in the southeastern part of the United States?)

No, because it is not used by itself. It is used in combination with weatherable minerals.

(Just that for clarification did the 24 limit also come from the Congo classification on the Ferrisol/Ferralsol, or did they take it from *Soil Taxonomy?*)

I am not sure. I would have to do some reading before I could answer that. But never having visited the Congo, I did not get it there. The Congo may have had the same limits as Brazil. I do not know about that.

Question 84

My question is on the differences in depth of mottling for aquic subgroups of Dystrochrepts versus aquic subgroups of Fragiochrepts. This may not be appropriate at this time.

Guy Smith:

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 85

This gives us a problem in New York. Commonly with the Fragiochrepts it would seem more appropriate in the aquic subgroups if they were mottled at 12 inches. Usually they are wet enough and have an aquic regime, and you slide clear past that possibility into the Fragiaquept, and consequently most of our moderately well drained soils still wind up being classified in typic subgroups. It is a problem that can be handled by the series rather than in *Taxonomy*.

Guy Smith:

In general, the correlation staff thought that the well drained or moderately well drained soils could be kept together in the *Taxonomy*, and the distinction handled as a series difference. When your 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 the four subdivisions that were possible. We had the freely drained soils; we had the aeric subgroups of the poorly drained soils, and the typic subgroups. Four possible classes: two typic and two intergrades. But we have five drainage classes and so we were in a bind. We could only get four separations into the *Taxonomy* where we had 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 the drainage, the depth to mottling (which was defined) instead of the five classes that were provided in the manual but with rather vague definitions.

Question 86

In orders like Alfisols and Ultisols, especially when they get interwoven in the landscape, then you start dealing at the subgroup level, where your aquic properties come in. I think one criterion 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. I was wondering why we did not consider a more standard depth for considering mottling?

Guy Smith:

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 87

A number of times in discussing the criteria for the different limits here, you made mention that a number of these things should probably be reexamined since the writing of *Soil Taxonomy* and more and more data is coming in; are you satisfied with the rate of reexamination, the rate of attitude towards looking at *Soil Taxonomy* as a changing model, which is the way that it was presented specifically in the United States. Are people looking at it at a rate that should be discussed?

Guy Smith:

No, I am not satisfied. I think that there is too much tendency to accept what is written there, without critical examination. Somewhere or other Marlin Cline said that that is not the gospel according to St. Guy, and Armand knows that when I am in the field that I wear holy shirts (coming from my cigar).

Question 88

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 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.

Guy Smith:

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 89

The application of the same concepts and criteria at different categorical levels, the use of different criteria for the same concept, and other seeming inconsistencies.

You have answered a number of questions that relates to this topic in your conversations with Dr. Leamy, and in some of the answers you have mentioned the principles involved. Nevertheless, so many users of *Soil Taxonomy* appear to have uncertainties about them that we consider it may be useful to explore the topics further, even at the expense of some repetition. We will deal (1) with the use of concepts and criteria at different categorical levels, (2) with the use of different criteria for the same general concept, and (3) the omission of potential taxa that could be identified on the basis of concepts applied elsewhere in the system.

1. Concepts and criteria used at different categorical levels.

You have touched on the reasons; this is done in your answer to Mike Leamy's question No. 41. Will you elaborate further here on why it was impractical to apply a given criteria uniformly at the same categorical level to all soils to which it is relevant? In the topics which follow under this heading, we would like to have you illustrate specific applications on the principles you discuss: (a) criteria associated with the terms albic, andic, fluvic, humic, ochric, and umbric. These differentiate among soils having moisture regimes, drier than aquic at the suborder level, if they are relevant, but they differentiate at the great group level, among the counterparts of these soils having aquic moisture regimes.

<u>Guy Smith</u>:

The general answer would be that 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 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*. 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 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. 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 us no problem here.

Question 90

The next question, which is similar in concept, is about the criteria associated with Udic, Ustic, and Xeric moisture regimes. These differentiate among mineral soils for which they are relevant at the suborder level if the soil has well expressed genetic properties. They also differentiate among Vertisols at that level. They are used at the great group level, however, to differentiate within the Andisols and Inceptisol orders which have weakly expressed genetic properties.

Guy Smith:

First, I should comment about the Vertisols, that we have not used a moisture regime to differentiate among them, but rather the periods of cracking. The concept of saturation of water that we have used for somewhat more permeable soils simply cannot be used with Vertisols. When we come to the Entisols it seemed to us 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. So we wanted, 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, the Fluvents, at the suborder level, we wanted to bring in the moisture regime at the next lower category, the highest that was possible if we kept the first subdivision of Entisols according to the reason why the soils had no horizons, and these were extremely important separations from an agricultural viewpoint, and we wanted to get them on maps of small scale; large scale maps do not concern us 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.

Among the Inceptisols, we made a first break according to the nature of the epipedon--umbric or ochric. As I mentioned a moment ago, this probably was an error, but it was related, so far as the United States goes, to the moisture regime. The Umbrepts that I know of 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 we do have Ochrepts with an ustic moisture regime, and a xeric moisture regime. There we used it at the great group level, the reason being, I suppose, that we must have thought the umbric and the ochric epipedons were of more importance than the soil moisture regime. Frankly, Inceptisols were a wastebasket that included everything that did not belong in some other order. That classification should have been criticized much more severely than it ever was. I am 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 91

Is it not that you had a big wastebasket because you only have 10 orders; if you only have 10 orders and you want to keep them extremely pure then, of course, you get this soil that does not belong to one of the 10 orders, but do you think that an increase in the number of orders would be an improvement.

Guy Smith:

I proposed a new order of the suborder of Andepts, and I can visualize that one could easily take another order out of that.

Question 92

I have a more specific question on the use of moisture regimes. You said yesterday that you used as criteria properties that are a result of genesis or properties that influence genesis. In the moisture regimes, could you comment on the use of the names of the following subgroup: the Udic Paleustoll; you have it udic, so you use udic, and then you have Paleustoll. The Ustoll is given here because of calcium carbonate in the soil, and then this is because it is udic moisture regime. So you use the result of genesis at a higher level than the property which influences the formation of Udic Paleustoll.

Guy Smith:

The Udic, as you say, in the Udic Paleustoll gets into the Ustoll because of the secondary lime. Udic Paleustoll is defined as having the 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 begin with. In the U.S. in the steps 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--intertropical regions, 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 93

I would like to come back to the andic subgroups, and for the question I will use the aquic subgroup as a comparison. From the point of view of logic for the system, it is correct to have andic, or acric subgroups if they are subordinate properties of the soil, but from a point of view of management, particularly if you have a thin layer of ash on the surface, it can be argued that the andic property should be brought out at a higher categoric level, although for the soil system as a whole it is a subordinate property. Is this a conflict in *Soil Taxonomy*?

Guy Smith:

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 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 94

We have also found that generally in the red soils of the area but also in most of our soils in northern New York and Northern New England, where the temperature regime is frigid, we just do not seem to get the color manifestation indication that it is wet, i.e., the morphological manifestation.

Guy Smith:

We said that soils like that were too few in the U.S. to permit much testing of the limits that we proposed. You seem to have an opportunity to test these limits; if you have not done it, I suggest you should.

Question 95

I did talk to John Witty; at one point I wondered about whether or not the red soils did or did not reduce artificially, and I guess they checked the same thing in Pennsylvania. They will reduce, but this does not happen in nature, apparently. It not only happens in the Fragiaquepts or Haplaquepts, but also we do not get the colors indicative in the aquic subgroups, either, that you would expect for the drainage class that you see in the field.

Guy Smith:

It is too bad your predecessor did not check these definitions more carefully before they got printed.

Question 96

The wetness problem did bother us in Long Island, because all we had was sand down there, and it just refused to get any color other than what the sand grains had. Along on the thinking on that, there are very few Haplaquods currently recognized, and I do not know whether this is a matter of we have not been doing that much work in these areas, or if requirements for mottling in the upper part of the spodic horizon keeps pushing the soils out of this possibility. When they say upper part, I assume that this does not necessarily mean the topmost subdivision of the spodic, but if you took the various subdivisions or sub horizons of the spodic collectively, I presume the upper part would be something in the upper half of all the sub horizons of the spodic.

Guy Smith:

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. I have to check that. If it does require mottles it is a serious error, because we know that normally the wet Spodosols have, what we used to call B1. There is a discernible transition between the albic horizon and the major part of the spodic horizon. There is one profile, one analysis here of an Aeric Haplaquod, which is the Leon series from Florida.

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. If the definition needs modification, certainly that should be proposed through channels to the SCS.

Question 97

About the only time you get mottles in the subsoils is where you get a discontinuity in texture.

Guy Smith:

You cannot get mottles without iron, and the Haplaquods normally are free of iron.

Question 98

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 water table.

Guy Smith:

Generally you do not find these in the Aquods, but you do in the Humods, in particular, and 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 99

In the Northeast, we have been having quite a controversy over the concept of fragipan. There have been rival factions, I guess, between people in New England and people in New

York for the concept of a Cx horizon versus a Bx horizon. With the connotation that the people in New York have taken, that a fragipan is a genetic horizon, and the more we start to look at fragipans within New York, we are finding more and more of them are actually probably parent material (C material) that is really relatively unaltered material, and yet in the classification scheme we have, due to the interpretive nature of our classification tied in with our interpretation, that we classify these as fragi at the great group level. I was wondering if you might care to comment on this angle. I do not know if you are aware that this has been going on in the Northeast for a number of years.

Guy Smith:

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, when 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 100

Further on that particular subject, we spent quite a bit of time through the regional TSC's office and the correlation people in each state on fragipan studies, primarily in frigid regions of New York and New England. Most of the evidence that we have collected from field observations and some laboratory analysis does suggest that we'll probably do away with the great group classification as it cuts across a land resource area boundary; plus crop yields, particularly corn yields for grain, in the frigid areas, do not seem to be that much different than in most of our mesic areas.

Guy Smith:

Podzolic, now Dystrochrepts, to Spodosols again -- Brown Podzolic and Podzols. So, if we drew the temperature limit at somewhere in the neighborhood of 8 degrees, we did not split very many series. It was an absolute minimum. The 15 degree 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 Podzolic. 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 degrees. 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. This has been written in a book that Elsevier plans to publish, but there is no harm in repeating it here.

Question 101

I guess the reason I asked that question is that we are having, again, this is kind of a localized problem in New York. Through our temperature data, we find a split up in the northern part of the state, St. Lawrence county, but it is not necessarily allowing any physiographic boundaries, or any necessarily, I think it in New York state over the past 3 or 4 years have I guess, this is in reference to the mesic versus the frigid break, and I have either read or heard that that break was set because of the inability to grow winter wheat above in the frigid zone. I was wondering, is that correct? What is the reason for the break at 8 degrees?

Guy Smith:

The major criticism that I got in proposing the definitions for taxa was that I was splitting series; and nobody liked that: not the engineers, not the agronomists, not the correlators. Certainly everyone objected to any split of a series, unless that split made it possible to improve the interpretations. It so happens that there was at one time a general, but not too well known, principle on correlation, that a soil that occurred in two different major land use areas could be divided into two series, because in one major land use area, you might be making interpretations for cotton and sorghum, and in another one for soybeans and corn. So, not many series went from one major land use area to another. 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 here to New York state, you generally went from what were called Gray-Brown Podzolic to Podzols. In Belgium, the fragipans run a little bit into the cryic temperature regime. The frigid zone does not occupy a large extent there, but in the Ardennes, the higher part of the Ardennes are definitely cryic, and the lower parts of the Ardennes are frigid. The fragipan, the Fragiochrepts are pretty much the normal soil on the ridges, and the Dystrochrepts on the slopes, and I cannot see anything different about those fragipans from the thermic ones in Mississippi and Tennessee. They are the same, with all the bleached cracks and polyhedrons, the bleached zone surrounding the polyhedrons, absence of roots within the polyhedron, the presence in the bleached zone between them. This seems to be normal there. But the parent materials there are not calcareous to begin with. If you have material, with 20-30 percent lime, once that lime is dissolved and removed, you have lots of void space, and fragipans seem to be more difficult to form in such materials.

Question 102

You were talking about temperature regimes, and we have been doing some studies on temperature regimes here. I guess some things are absent from them; for example, usually you see very little development in any of this, and the fragipan usually in those eroded situations, there is no pan, in other words it does not follow the landscape unit as you would think of any type of development, and we have kind of assumed that it has to be genetic or it could not be fragipan. So you have to have some obvious mark which would show that there's some soil development going on, and we haven't been able to support that. We talked to Dr. Cline quite a bit about this, and I don't think he has any objections of calling these Cx and indicating what properties they have. They probably will not show up at the great group level.

Guy Smith:

Presumably, I do not know what soils you are discussing. I have looked in the Appalachian plateau at soils that had fragipans that were virtually identical to those in Tennessee and Mississippi. This is not basal till.

Question 103

What temperature regime would that have been, frigid or mesic?

Guy Smith:

What we were noticing, it seems to be less a chance of fragipan development for some reason in frigid areas. At least that is where we have most of the problem in the Northeast.

I think the other problem, too, with this is that we do not necessarily see any morphologic differences in the soil. In fact, the soils that we are now classifying as frigid would have the same description, would look the same as those soils in adjacent counties that are, in fact, called mesic.

No, in the Dystrochrepts, I would not expect you to find much difference in the morphology, but surely for your interpretations, you would recommend different varieties of maize for the frigid zones versus the mesic zones. You would be very apt to; that is an interpretation.

Question 104

Has there been any comment from any part of the United States concerning that 8 degree break?

Guy Smith:

No, not to my knowledge.

Question 104b

Just one more to follow up on that. I guess I want to know, even though we have the data to back up to justify changing to frigid soils, I was wondering what your thoughts might be, to tie it more to, say physiographic regions or to differences that are fairly readily observed in the soil, morphological differences.

Guy Smith:

Normally, the soil temperature can be inferred from the latitude and the elevation. Since the subdivision is only made at the family level, rather than the subgroup or great group, one would not anticipate any particular morphological difference. If there were a morphological difference, it would have been brought into the taxonomy at a higher level.

Question 105

There has been some talk of redefining Spodosols to relax the requirement to classify soils into the Spodosol order. I can not give you any specific suggestions; there has just been some talk that so many of the soils as we see them in the field look like Spodosols, but then you run them through the lab, and get the lab data on them, and they will fail on one or the other three major requirements.

Guy Smith:

There is no requirement for any laboratory data whatever on the presence of a spodic horizon. All you need is a good 60-power hand lens, and normally you can identify the spodic horizon without any laboratory measurements. We have a boundary between the Dystrochrepts and the Spodosols, and there was a problem about where that should be, and so Dr. Cline went to the field with our laboratory people, and he classified the soils where he thought they belonged: Spodosols or Dystrochrepts. Then the laboratory sampled, and fitted their definition to Dr. Cline's classification. This was then circulated for criticism, not only in the U.S., but in other countries. The chemists in Canada complained bitterly that too much emphasis was given to the field identification. The field people complained bitterly that too much emphasis was given to laboratory characteristics. We do know that many beautiful Spodosols will not meet the laboratory requirements, and pointed out here that, 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. Since everyone objected to the definition, both the lab and the field men, I thought maybe it was the best we could do in the state of our knowledge at that moment. I know of no proposals for modification at this moment, though McClelland may have received some.

The laboratory method is pretty completely relaxed. 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. If you can identify it in the field, that is enough. You can stop there, and do not bother the laboratory here with samples, because they all by any means meet them. The chemical requirements are for the intergrades with the Dystrochrepts, and only for that.

Question 106

Our general experience is, if they can not see a good spodic in the field, it does not pass the chemical test. Occasionally ones that look real good in the field have a hard time meeting the criteria. And as I mentioned yesterday, 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.

I should also mention one other thing, that in Europe, at least, the pedologists use the fluoride test. Just put a pinch of the soil on filter paper, saturated and dried with phenolphthalein, put a drop of sodium fluoride on it; if it turns red, they call it spodic.

We considered at one time the possibility of subgroups of Spodosols, defined on the basis of pH in fluoride, but we never could accumulate enough data to find out whether it would work or not.

Question 106b

Did you discuss yesterday the definition of Dystric Eutrochrepts? I guess the problem arises from determining what the lower limit of soil is.

Guy Smith:

Well, the definition of Eutrochrepts and Dystrochrepts does hang on the definition of the soil. So, if I go to the definition of soil, the lower limit of biologic activity are 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 107

That is one area we seem to have, not a major problem, but at least a problem in the correct classification of some of the soils; in fact, one soil that has been considered a Dystrochrept, I guess, from the time that it was placed. It was an outwash soil, and it was found to have free carbonates down at some depth less than 2 meters, but probably in the neighborhood of 65 or 70 inches, and based on this it was reclassified as a Dystric Eutrochrept. Then we have other soils; some we looked at this summer where the same question comes up, and base saturation certainly below the 60 percent within a depth of 75 cm. Right now we are treating them as Dystric Eutrochrepts based on the distinction given in *Taxonomy*.

Guy Smith:

You will find a few roots here, but if you are talking about native vegetation, then we are talking about tree roots getting into there, and I suspect they do. Now, the question is what is common rooting; maybe just a few fine roots down there does not fulfill the requirement for common rooting depth.

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 Soil 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 Ao was still present. I looked under that, and 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 108

In a case like this, the base saturation we had is 14 percent, which is real low. To me, the common sense thing is that it is more typical of a Dystrochrept than it is of a Eutrochrept. Somewhere in the system we start dealing with definitions, and you know you are just violating the interpretations if you put it the way that you do not feel comfortable with its placement. Somehow we have to deal with that; you either have two choices: you change the rules, which will take you about 10 years, or you can make some adjustments, I think, as to how you place your soils so that you do not ruin your interpretations. If you felt that you were in a situation where you were, you know you were putting it in the wrong box, how would you handle it?

Guy Smith:

I would propose a change; I admit that it may take 10 years to make the change, but I would give the example of why you need that change for your interpretation. 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 109

We have discussed the use of the moisture regimes at different levels, and now we arrived at point C, which is related to criteria associated with the aridic soil moisture regime. These criteria differentiate the Aridisols from most other mineral soils at the order level, in the Vertisol and Oxisol levels at the suborder level, and in the Entisol order at the great group level. So the question again is why do you use the same set of criteria at different levels?

Guy Smith:

We come back to the purpose of the classification. This is a general answer to this whole set of questions, using concepts at different categoric levels. I have to go back, again, to John Stuart Mill, a hundred years ago, who said that the best classification is the one that permits you to make the largest number and the most important statements about the obvious truths, for the purpose of making your classification. For the soil survey, we are interested in facilitating field work, mapping, and in developing the best interpretations possible for the soil maps that they make, which may be made at a scale of 1:5,000,000 or at a scale of 1:5,000. Obviously, 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 -- the order of Entisols. The order of Vertisols one can make a great many statements about; Spodosols you can make many statements about; Mollisols, Alfisols, Ultisols, there are not too many statements other than suitability for permanent agriculture, with and without soil amendments. The argillic horizon is used not because it is in itself too important, but because of its accessory properties. It is a mark of some 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 (reference to be added later) 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 this 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. I just read you the statement here from *Soil Taxonomy* that we decide this, which is the more important by which grouping let us make the greater number and of the most 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. 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 that we tried that, 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 the 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 110

What about numerical taxonomy? These are being increasingly used by biologists.

Guy Smith:

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 111

There are soils with common properties, and numerical taxonomy may be a mechanism to select criteria to separate different kinds of soils within a class.

Guy Smith:

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 112

The question refers to criteria associated with the aridic (torric) soil moisture regime. These differentiate the Aridisols from most (?) other mineral soils at the order level, within the Vertisols and Oxisols orders at the suborder level, and within the Entisol order at the great group level.

Guy Smith:

So far as the aridic soil moisture regimes 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 by Dr. Eswaran 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 SMR) 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 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 113

Would you develop the "trop" concept?

Guy Smith:

This has been criticized enormously by some people. We can take the Inceptisols as an example where we got into a trap, according to whether or not we have an umbric or ochric epipedon. In the US, this difference is closely related to the content of organic matter in the soil and also to the base status.

The Umbrepts in the US have a relatively high content of organic matter, compared to the Ochrepts. In the tropics, this relation breaks down almost completely. You cannot find any good relation between soil color and its content or organic matter. So, to get away from this problem of trying to subdivide the soils of the inter tropical areas according to color-which has no accessory properties--we have to bring in the "trop" concept into the suborder. This is one of the principle reasons for the trop concept. It is a way to not to use standards of temperate regions in the inter tropical regions.

Question 114

It was stated earlier that moisture regimes are used as properties that have an influence on genesis. What is the purpose of the moisture control section? *Soil Taxonomy* says that the intent of the soil moisture control section is to facilitate estimation of a soil moisture regime from climatic data. Most people consider that the control section is a mechanism to know how much water is available to plants. This is apparently not correct and if so what is the intent?

Guy Smith:

The moisture control section can be completely dry even though the crops are surviving and making moderate growth because of available moisture below the MCS. We cannot obviously define these various SMR 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 run-on. This will modify the moisture conditions in the soil.

A case that was brought to my attention was a Torrifluvent. The soil is flooded several times a year in summer during the growing season. His record shows that he can obtain 1,000 lbs. of edible forage per acre from the Torrifluvent while the other soils with aridic SMR in the neighborhood are producing about 100 lbs. He does not believe that the soil is properly classified.

If the SMR is computed strictly from precipitation, it is an aridic SMR. The criticism of MR 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 129

The next question deals with the conflict of series names for both taxa and mapping units. It has been suggested that the confusion resulting from the use of series names for both taxonomic and mapping units may justify reserving the long established convention of series names for mapping units, and in effect dropping the category of soil series from *Soil Taxonomy*. Will you comment on the suggestion?

Guy Smith:

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, a 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 130

Wasn't there a suggestion at one time to shorten the family name by giving it the name of the most dominant series?

Guy Smith:

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 131

There are many countries which are now starting small-scale maps on scale 1:1,000,000 like the soil map of the Arab world. The tendency to include a large amount of detail makes them want to use subgroups in the legend, although they could achieve the same purpose by using phases of great groups. Probably one of the reasons for this phasing out at higher categoric levels in Taxonomy is not spelled out in *Soil Taxonomy* or many other documents. Do you have any suggestions for this?

Guy Smith:

The subgroups are a little better defined than the phases to get uniformity among all the Arab countries. The soil map of the 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 1:51,000,000. 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 132

The next question is on *Soil Taxonomy* and small-scale maps. A surprising number of people appear to believe that using taxa of higher categories automatically insures that the areas they occupy will be large. Will you comment on (a) the degrees in numbers of taxa identifiable in the large land areas represented on small-scale maps as one uses taxa of successively higher categories to identify them, (b) the difference in apparent complexity of the patterns of soils identified as taxa at low and at high categoric levels in such areas, and (c) the differences in the number and specifity of statements that can be made about the soils of such areas when they are identified in terms of taxa at low and at high categoric levels.

Guy Smith:

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. The point 'b' is a little difficult to understand for me. 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 that 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 small-scale, and one should know clearly why he is doing that and then design his nomenclature to bring out what is needed for the purpose of making that particular map.

Question 133

The last question on this section deals with the special variability of diagnostic properties in relation to the categorical level at which they are used. To what extent did spatial variability of diagnostic properties enter into the choice of categorical levels at which they are used. This may require an answer in terms of breadth of perspective, that is the perspective of local landscapes versus that of broad regions.

Guy Smith:

I have already commented on the use of the soil climate 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, be a mixture of aquic and udic regimes.

Question 134

Do you have any good suggestions on how to name mapping units other than the current practice that is being used to get around this problem of homogeneity and inclusions, taxadjuncts?

Guy Smith:

No, 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.

Comment: There has been some suggestion that we need somewhere a correlation book to describe principles, concepts, and guidelines for correlation.

The late Dr. Kellogg tried to have such a manual written but never could get anyone to write it.

Question 135

There was a principle of using mono-taxa unit names for map units, as much as possible. As we gather more transect data to determine what is in the map units and statistically look at them, we are finding that at least in the Northeast, our units should be named as multi-taxa units. As we learn more it may play havoc with a system that's been deeply entrenched in the use of mono-taxa for the utility of interpretations. Would you care to comment on that?

Guy Smith:

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 136

This question is related to mapping units and taxonomic units. We have been saying that *Soil Taxonomy* classifies polypedons, and some people have been making the interpretation that if in a landscape they find an argillic B here, or they find mollic epipedon that they put these things together and say this is what I am classifying. If they tell me they are classifying polypedons, they have the wrong interpretation of polypedon. I think it would be good to clarify that this is not the way to do it, if you agree with me.

Guy Smith:

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 137

What do you do with a unit that is cyclic; by taking it in smaller units one could have several different taxa. How have you traditionally thought of handling that in Taxonomy?

Guy Smith:

The purpose of the polypedon was to permit classification within a series of somewhat contrasting kinds of soil, such as I have illustrated here--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.

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But this same situation very likely occurs not too far away, where there are larger contiguous areas that have both the albic and the bleached clay that would classify as one. There may be another area in which it is primarily the calcareous clay close to the surface. Both of those would be classified as separate series, probably different taxa other than series, and yet here they happen to be combined because of the uniqueness in the landscape, and then we say, then, how do I classify this one, where the two separate components are classified, separately if they are a little but larger bodies [sic]?

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Well, they get into different families in that situation, because one is a ruptic family and the other is not.

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But then how do you classify the pedon?

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On the basis of the variability and the nature of the horizons within very short lateral distances.

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Soil Taxonomy states that the mapper delineates polypedons on large-scale maps, but then it carries on to say that there are various problems by doing this. It lists three major problems. It is particularly difficult to recognize a polypedon in nature, particularly where there are no lateral changes that can be recognized. A common answer has been to map geographic or geomorphic elements rather than trying to identify polypedons. Would you say that this particular problem has led to a breakdown in the polypedon concept?

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If you check any detailed soil map in the U.S., carry it out to the field and start looking at what the pedologist did, you will see that he tried to draw his boundaries around the kinds of soil. His knowledge of geomorphology might have suggested to him that when he changes from one landform to another he is pretty apt to be changing from one soil to another, but he will normally try to delineate the polypedon if he can. If he cannot do it, then he reflects that in the name that he puts on that map unit as a complex, or an association, or what have you.

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Can you say generally at what scale the polypedon becomes applicable?

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No, because it is not the same everywhere in the United States or the world. 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 been dissected yet. 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.

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Section F Soil Interpretations

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This is a particularly important question because it deals directly with use management of soil for many purposes particularly for annual crops. The performance of these crops are determined to a large extent on the properties of the surface horizons. Should the properties of surface horizons have been incorporated into the criteria of soil families to enhance their interpretive value?

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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 129

The next question deals with the conflict of series names for both taxa and mapping units. It has been suggested that the confusion resulting from the use of series names for both taxonomic and mapping units may justify reserving the long established convention of series names for mapping units, and in effect dropping the category of soil series from *Soil Taxonomy*. Will you comment on the suggestion?

Guy Smith:

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, a 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 130

Wasn't there a suggestion at one time to shorten the family name by giving it the name of the most dominant series?

Guy Smith:

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 131

There are many countries which are now starting small-scale maps on scale 1:1,000,000 like the soil map of the Arab world. The tendency to include a large amount of detail makes them want to use subgroups in the legend, although they could achieve the same purpose by using phases of great groups. Probably one of the reasons for this phasing out at higher categoric levels in Taxonomy is not spelled out in *Soil Taxonomy* or many other documents. Do you have any suggestions for this?

Guy Smith:

The subgroups are a little better defined than the phases to get uniformity among all the Arab countries. The soil map of the 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 1:51,000,000. 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 132

The next question is on *Soil Taxonomy* and small-scale maps. A surprising number of people appear to believe that using taxa of higher categories automatically insures that the areas they occupy will be large. Will you comment on (a) the degrees in numbers of taxa identifiable in the large land areas represented on small-scale maps as one uses taxa of successively higher categories to identify them, (b) the difference in apparent complexity of the patterns of soils identified as taxa at low and at high categoric levels in such areas, and (c) the differences in the number and specifity of statements that can be made about the soils of such areas when they are identified in terms of taxa at low and at high categoric levels.

Guy Smith:

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. The point 'b' is a little difficult to understand for me. 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 that 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 small-scale, and one should know clearly why he is doing that and then design his nomenclature to bring out what is needed for the purpose of making that particular map.

Question 133

The last question on this section deals with the special variability of diagnostic properties in relation to the categorical level at which they are used. To what extent did spatial variability of diagnostic properties enter into the choice of categorical levels at which they are used. This may require an answer in terms of breadth of perspective, that is the perspective of local landscapes versus that of broad regions.

Guy Smith:

I have already commented on the use of the soil climate 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, be a mixture of aquic and udic regimes.

Question 134

Do you have any good suggestions on how to name mapping units other than the current practice that is being used to get around this problem of homogeneity and inclusions, taxadjuncts?

Guy Smith:

No, 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.

Comment: There has been some suggestion that we need somewhere a correlation book to describe principles, concepts, and guidelines for correlation.

The late Dr. Kellogg tried to have such a manual written but never could get anyone to write it.

Question 135

There was a principle of using mono-taxa unit names for map units, as much as possible. As we gather more transect data to determine what is in the map units and statistically look at them, we are finding that at least in the Northeast, our units should be named as multi-taxa units. As we learn more it may play havoc with a system that's been deeply entrenched in the use of mono-taxa for the utility of interpretations. Would you care to comment on that?

Guy Smith:

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.

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This question is related to mapping units and taxonomic units. We have been saying that *Soil Taxonomy* classifies polypedons, and some people have been making the interpretation that if in a landscape they find an argillic B here, or they find mollic epipedon that they put these things together and say this is what I am classifying. If they tell me they are classifying polypedons, they have the wrong interpretation of polypedon. I think it would be good to clarify that this is not the way to do it, if you agree with me.

Guy Smith:

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.

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What do you do with a unit that is cyclic; by taking it in smaller units one could have several different taxa. How have you traditionally thought of handling that in Taxonomy?

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Question 148

It has been suggested by Dr. Kellogg and others that one reason for replacing the 1949 system was to improve the interpretive value of taxa in the higher categories. As a number of successful attempts to use the taxa of higher categories for interpretation has been small, has *Soil Taxonomy* accomplished that objective? If the system has enhanced interpretive value at higher categoric levels, will you specify ways in which that has been accomplished?

Guy Smith:

The interpretive 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 interpretive 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, numbers of interpretations are rather limited because of the heterogeneity of map units and the specific interpretations at the great groups level 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 say 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 149

Will you describe the kinds of interpretations that are feasible to taxa of higher categories? By kinds we refer to both purposes and levels of generalization.

Guy Smith:

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 want, for any quantitative or qualitative interpretation as to what kind of crop, we have to then come down below the order level to bring into our interpretive information the nature of the soil climate. The Mollisols of 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 150

The basic premise of the Benchmark Soils Pedon Project is that they can transfer technology using soil families. Would you prefer that they had used phases of families?

Guy Smith:

I hope that they had used phases of families when they located their plots. I do not know what they have done, but because they were irrigating as one system of treatment, and because they were using mechanical cultivation, I suspect they got everything on all level land like all other experiment stations are.

Question 151

The other criticism or comment has been that, even at the family category, the properties that one could derive of a control section, which is 25 to 75 cm. depth, and not the surface soil, which is more relevant for the performance of the annual crops. So, for technology transfer, should they go to the series level as the basis, or just phases of families?

Guy Smith:

The Benchmark project is exercising some control over the properties of the upper 25 centimeters through use of fertilizers, through the selection of crops and their rotation. I think they are trying to apply uniform treatment on each of the Benchmark sites, but you may know more about this than I, I have only looked at the layout on paper of one of the Benchmark sites to see what kinds of treatment they were applying.

First they obviously have used phase criteria when they selected sites. They have not taken the full range of soils within that family; they have selected the more level areas. Now, in their current interpretations they are beginning to specify that they have selected this phase, they have done these things, which initially was not in their statement; they said just at the family level. Now, in the interpretations they are beginning to make, they recognize that they cannot interpret for the whole family, based on the experiment they have, because of slope or stoniness or other sets of characteristics that we might consider either phases or properties that we could use as phases. They do not use a series, but they could phase to attempt to get the more important properties.

Question 152

The other comment that I have with my experience in Malaysia indicates that, for perennial crops like rubber and oil palm, which are deep-rooting, we can with a certain amount of confidence predict the yields which we are going to get based on family classification, but we cannot do the same thing with annuals, particularly rice, for other reasons. What kind of criteria do we need, particularly at the lower categoric level, at series, so that we can have some measure for prediction of performance of the crop?

Guy Smith:

The family level was not intended for the most precise quantitative interpretations. 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. With some general implication of the nature of the Annual crops that were suited for that particular soil. These are our major interpretations of our maps for the work of the soil conservationist in the SCS. Capability classification is an interpretive classification, and it must mesh with the taxonomy, or there is something wrong with one or the other, or both.

The interpretations for the annual crops that we make, always involve the specification of the plan of management that he proposes to follow. We do not tell him what to do; he tells us what he is going to do, and then according to what he plans to do, we can tell him what kinds of problems he is going to run into, and he may change his plans because of the consequences of having the wrong management in mind. These major interpretations we think, given the phases of the family, should be possible. It was the intent that they would be possible.

You could say on the Oxisols of Malaysia, that for nearly level or gently sloping phase and with a system of management that involved the use of shifting cultivation with long fallow; you can predict rather safely that he 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.

The interpretations in published surveys of the National Cooperative Soil Survey are primarily, but not solely, interpretations of taxa, not of mapping units. The two are not necessarily the same. Do you see any feasible way to present interpretations for mapping units in such publications.

It should be feasible where the mapping unit includes a number of taxa. It should be possible to make interpretations first for the individual taxa, specifying then the relative area of a given taxon, and then the interpretations for the use of that taxon. This is still interpreting by the taxon, rather than by the mapping unit; but then there are interpretations that would apply to mapping units rather than to taxa. For example, a wet drainage way crossing an area is going to be a limitation for passage with wheeled vehicles. Normally it shows up as a line on your map, and one does, generally in the SCS program, make interpretations by fields as well as by kinds of soils, because the presence of an unfavorable condition can reduce the potential of a much more favorable condition in the field. It 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 drains that. We do not tell him he should, but we can tell him he is going to be planting late and his yields are going to be reduced because of that.

Interpretations for other than the growth of plants would be feasible, as I mentioned earlier with the fragipans, the certain uses might be a mixture of Fragiaqualfs and Fragiudalfs. The whole map unit would be unsuited for the development of housing with septic tanks and special basement would be needed for the houses.

Question 153

Other taxonomies that have been developed that have not been primarily used for soil survey, what has generally been their fate, what has happened to those taxonomies?

Guy Smith:

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 154

Some people say it is extremely difficult to decide in the field the depths of the moisture control section, especially in soils that are never dry. Would you care to comment?

Guy Smith:

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. Actually, classification of the soils in the U.S. was predetermined. We decided in advance that we wanted soils in certain counties to have an ustic moisture regime. It was considered typic. We wanted in other counties to have a moisture regime that was ustic but grading toward aridic, and we drew these boundaries and fitted them to the calculated moisture conditions. We are much more apt to change the moisture regime definitions than we are the classification.

Question 155

In some same definitions a 5 degree temperature limit is used. Taking into account the number of days that the moisture control section is moist or partly moist. In other cases you use the 8 degree Celsius. Once *Soil Taxonomy* uses 5 Celsius, and once it uses 8 degree Celsius. What were the reasons to have two different limits?

Guy Smith:

The 8 degree Celsius 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 degree 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 degree limit in, on the grounds that during the winter when the soils were dry the temperature would be below 5 degrees. 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 degree in both cases.

It would not make much change, no.

Question 156

This question is on the definition of xeric moisture regime. What were the reasons not to accept soils with a regime in which the mean annual soil temperature is more than 22 degrees Celsius, or where the difference between summer and winter is less than 5 degrees? Why don't you accept in the xeric moisture regime, mean annual temperature of more than 22 degrees?

Guy Smith:

There are two reasons. In the first place, 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 plain of Lebanon, Syria, Israel it becomes ustic because the summer is too hot.

Question 157

A proposal that you made to include all soils with hyperthermic soil temperature regimes into the "tropo" subgroups. What were the reasons to make that proposal?

Guy Smith:

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 inter tropical regions from those we used in the temperate regions. One of the overriding considerations is that so many of the inter tropical 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 inter tropical 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 inter tropical regions if we use color, we are getting groupings that have no meaning. Now the hyperthermic zone seemed more like the inter tropical regions than the temperate regions.

Question 158

We have the general principle in Taxonomy that we keep the cultivated and noncultivated together. Sometimes this creates some problems. An example which I recently encountered was in the Gezira scheme in Sudan, where there was a field which was under fallow, according to them, for more than 25 years and the soil there is a Vertic Camborthid. Deep cracks have been in filled. But across the field where they have been continuously irrigating, you have development of very nice slickensides. You have the parallel epipedons and all the features to classify the soil as a Torrert. So the question was, here we are seeing the effect of irrigation on changes in soil properties. How does this effect the principles in *Soil Taxonomy*?

Guy Smith:

It is easy to understand why under irrigation you find all the properties of the Vertisol. 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 Torrerts 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 159

There is also the reverse situation where you have the potential acid sulfate soil -- the Sulfaquents-- on draining they transform rather rapidly to Sulfaquepts and so over a short time you have the problem of the accuracy of your map.

Guy Smith:

That is a drastic reclamation. To drain the Sulfaquent requires drastic drainage treatment and the change in the soil is an enormous one, once it is drained. This was discussed when we first started to work on taxonomy, that we did not want to change the classification of a soil just because of artificial drainage, but when we have the development of a Sulfaquept the changes were so drastic that we really had little alternative but to change the classification. There is nothing much you can do with a Sulfaquept; but the Sulfaquent, as long as you do not drain it, still has a number of potential uses.

Question 160

I still have another question on the temperature regimes. To define the temperature regime of a soil, under what conditions should the temperature be measured? I have had many criticisms, especially from Australia, on the methodology of measuring soil temperature. Should it be under a shelter, under the crop, on the bare soil, or on the natural vegetation? *Soil Taxonomy* does not seem to give any instruction on that.

Guy Smith:

It 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 161

The main concern was between a soil that is cultivated and a soil that is still under forest and that the temperature changes quite drastically in the cultivated soil. In mapping and classifying, by just cutting the forest and opening fields you may change classification of the soil itself. Would not it be useful to have the temperature measurements made under standard conditions? Has this been considered?

Guy Smith:

That could be done. We have also, 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 162

Was it the purpose of the temperature classes to regionalize a temperature property or was it to classify pedon by pedon, because we find in our well or moderately well drained soils we exhibit the mesic temperature class. Some of our poorly drained soils within the same catena exhibit frigid temperature class. Soil Survey as it stands in the United States right now does not deal with that within a survey area. Was it the intent to regionalize the temperature concept or was it the intent to classify polypedon by polypedon?

Guy Smith:

The original intent was to introduce moisture and temperature as a partial substitute for the old concept of zonality. We did recognize that in a very small area the temperature of one soil might differ significantly from that of another soil, particularly according to the aspect of the slope. The situation you mentioned is a little difficult for me to visualize because all the records we found showed the mean annual temperature of the soil was independent of drainage. Summer and winter temperatures were affected but they had the same mean annual temperature if they were the same elevation, latitude and aspect. Getting a frigid in a mesic temperature according to drainage was not in the books according to the literature. We stressed that in that technical publication (reference to be added) on moisture and temperature. I would wonder if your statement is really correct. How much data do you have to back it up that those temperatures *are* different?

(We have been looking at it for about three and a half years now. Our temperature break is falling right at the 8 degree centigrade. The somewhat poorly drained seem to be falling below the 8 centigrade and the well drained are in the range between 8.2 and 8.4. Technically according to the classification it comes out. Management-wise it might not be all that different. There is a large zone across northern New York and New England when you come to these gray zones, you have a transition zone between more frigid to the north mesic to the south. It has posed quite a problem as far as reading a series based on this.)

I do not think you should concern yourself about a difference of 2 tenths of a degree mean annual temperature. Even three years with a rather limited number of measurements you can make, it's nothing of any significance. You could of course get a very good check on that if you made a measurement at a depth of which there is no annual change in temperature. That is the most accurate method. You get a 30 or 40 foot well that is being pumped regularly and just measure the water temperature. I think you are defeating the whole purpose of taxonomy when you start a quibble over a couple of tenths of a degree in the mean annual temperature of a soil.

Question 163

Along that line, they have also found locally that in some places, for example, in some valleys in Vermont, they have cleared the forests and that will now go cryic and where it remains in forest it remains frigid, which is the reverse of what we thought the intent was. So that has created problems locally where it seems to go against the initial rationale of the O horizon. Some people said we might adjust that by adjusting the temperature limits that we associate with the O horizon. It creates a narrow band the way it is written now.

Guy Smith:

We had very few data on soil temperature in terms of the temperature during the growing season. When we tried to define the cryic temperature we wanted to get into the taxonomy, the limit at which a soil is so cold that it is hardly worthwhile to use it for crops. Admittedly, plant breeders can shift the zone in which a particular crop will grow. They can develop new varieties that will grow in shorter and shorter growing seasons. In general, we thought we wanted to exclude those soils from the same taxa as the warmer ones that were normally cultivated. With inadequate data we could propose some limits and leave them for someone to study. And, having studied it and found it does not work the way we thought at times we have considered proposing that we change the limits.

Question 164

At the time you did this, did you have very much information, say, in the west, on vertical zonality, temperature and moisture?

Guy Smith:

Practically none. The studies of soil temperature were made in the mountains of the west after we began to use *Soil Taxonomy*. In several States they measured the soil temperatures and related them to elevation. Everywhere that this has been done, one comes up with a very good relation between elevation, latitude, soil temperature and aspect. All the places where they keep records or soil temperatures, they are flat.

Question 165

Along with that, we noticed in some of the mapping in the west where some groups, primarily the Forest Service, also uses either potential natural vegetation or the habitat type as one of their criteria for mapping units. They are, in effect, using that as a substitute for some of the temperature moisture relationships, primarily the moisture relationships. Where we have seen those two kinds of surveys come together, where one relies more on natural vegetation than the other; we have ended up with different classifications of the soil, neither one of us having adequate data. One using their concept of the potential natural vegetation as a substitute and others trying to use their best guess on the weather records and also their concept of potential vegetation. And we have had that now several times where they come together with the map. This one has that because they are coming from high areas, they are more moist. They are bringing the moist zone down farther and here they are coming from the dry one and they overlap about one class in taxa. I just wondered if you had also had this experience where people were trying to use the vegetation as a substitute for moisture when we had very little data.

Guy Smith:

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. That 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 166

I think we have seen more of the difficulties as we go to the semiarid, for example, New Mexico and Arizona where prior management has had a drastic effect on vegetation than exists there today. Overgrazing in one case will obliterate certain species there and you are not sure they can regenerate.

Guy Smith:

In New Mexico and Arizona, once you have overgrazed the land and destroyed the native grasses the soil management people have never found a way to bring them back. It is a permanent change in the vegetation.

Question 167

I think the other problem, is that you have salt effects in grazings that are reflected through the plants, like tolerance of greasewood versus sagebrush. I would wonder about the use of plants. We have some groups that very rigidly attach taxonomic things to their potential natural vegetation or their concepts of the vegetation and they have subdivided, say, precipitation belts and temperature belts and it looks very good and very systematic but it is not based on very much data. But that is what they use because they can apply it rather systematically in their mapping program. In Venezuela, when you have an evergreen forest, you do not have to wait around to see whether you have a pronounced dry season or not. You know the species. You know they do not drop their leaves. You are safe in saying that is a Udic moisture regime. If you have a deciduous forest, you are safe in saying there is a pronounced dry season here and the only survival mechanism the trees have is to shed their leaves in the dry season. When you do not have a forest but just have cacti; you are pretty safe in saying this is aridic. It is in this aridic zone that you begin to run into these salt problems. You do not have to worry about your moisture regime. Your plants will tell you that this is *arid*? If it is salty or not salty this is a function of the position in the landscape.

Guy Smith:

Soil moisture, since it is used so high in the system. The vegetation indicates the moisture that affects the placement of the soil in Taxonomy. When you are playing the margin between the Ustic/Aridic then the plant indicator may tell you depending on whether you are coming from the aridic back toward the Ustic or from the ustic toward the aridic, and since we allow one intergrade on one side, well it has been put on one side it is not on the other, so it depends on which way you are mapping as to which subgroup you are going to use. That is where we have had this conflict. It is always the margin. Once you move away from the margin there is no question. It is as the two comes together.

Question 168

You proposed including the hyperthermics in the "tropo". Would there be an advantage to confining the "tropos" to the hyperthermics and the isohyperthermics and leaving the other "iso" out? When I look around, from the point of view of use of the soil, this seems to be an important limit rather than bringing in the isothermics and the isomesics into the concept of the "tropos".

Guy Smith:

I am willing to leave this to the committee on soil moisture and temperature in inter tropical regions. Surely the isomesic soils that I have seen have very, very different uses from the mesic soils. They grow the same crops, yes, but they grow them the year round--three crops a year instead of one. I consider that an important difference in use.

(I agree, that is, one kind of importance. The other kind is when you compare the isomesics with the isohyperthermics. There is a big difference in the use of the soil in those kinds of environments.)

Yes, but I have already mentioned that the basic reason for the "trop" was to get out of the bind we find ourselves in from the temperate regions, of weighting the soil color value heavily because it is related to the organic matter.

In the west Indies I had hundreds of analysis 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 169

This is related to the mapping questions that we covered yesterday. In *Soil Taxonomy*, taxa are defined by ranges rather than central concepts and it is easy to understand why -- because of reproducability -- but when we try to classify a soil that is very close to a taxonomic limit, would it not help if we could compare the soil that we are classifying with the central concepts of taxa?

Guy Smith:

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 170

Why are the definitions in *Soil Taxonomy* so complex? Perhaps an explanation of that should be on record.

Guy Smith:

We have been over this once. It would not do any harm to go over it again because I can put them together when I get the transcriptions. 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 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. It is comparable to this little narrow temperature range that we discussed this morning.

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 171

Many soil scientists that I have spoken with, regard *Soil Taxonomy* as a system for naming pedons. This seems to be because soil scientists can see the pedon whereas the polypedon can only be sampled and they have to make correlations between the sample of the polypedon and the landscape elements. How would you reply to this?

Guy Smith:

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 polypedons 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.

Question 172

Will you comment on the potential for quantifying field criteria for estimating or replacing criteria that are laboratory dependent?

Guy Smith:

The criteria in *Soil Taxonomy* that require laboratory measurement can generally, we 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 what part of the universe the soils that concern us represent. If you have a pH above 7, you can infer you have a high base saturation. If you have a pH of 4.5 you cannot draw the opposite inference. So, we have to have occasional laboratory determinations. We can have field portable laboratory measurements, and in the case of Dr. Fields's test for allophane. We have developed and I presume there is still available for sale, very portable laboratory kits which permit the measurement of most of the parameters that we use in taxonomy. We cannot estimate the percentage of silt, sand, or clay. We cannot measure that readily in the field but the field men, by having some laboratory determinations made and practicing at identification can do not too unreasonable a job of estimating percentage clay, silt and sand. So, if one is working in a new area where we have no data and no experience certainly one has to have access to a laboratory or he has to carry his portable laboratory with him. I have had to do that in some of the West Indian Islands. I needed to know what kind of clay I was dealing with and there were no determinations on that. So, I estimated the percentage clay and we measure the CEC of the soil sample and the CEC was well under 18 m.eq. per hundred grams clay and I said to myself, "That's kaolinite," and I classified the soil that way. But without knowing the CEC of the soil and without estimating the percentage of clay that was 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 173

Will you comment on the historical perspective of the way soils that have developed across the context of contrasting parent materials are handled in *Soil Taxonomy*? It is our impression that such soils are much more extensive than was appreciated while *Soil Taxonomy* was being developed. Are there potentially better ways of handling them in the future?

Guy Smith:

The problem of identifying argillic horizons in materials in which there was an initial difference in the percentage clay was recognized when we developed *Soil Taxonomy*. The problems of very marked changes in pore-size distribution that affect movement of water through the soil were recognized when we developed *Soil Taxonomy*. If these differences are not marked, I do not think they are going to constitute any serious problems in classifying the soils or in soil management. When we go into the field with some people, one man in the group may be able to identify a contrasting material that has so little contrast that the bulk of the group will not see it. That has been demonstrated on our excursions of the international workshops. By and large I have not felt that this was a serious problem except in the identification of argillic horizons in some few soils, particularly those that do not have readily discernible clayskins. I do not know of any way to handle this. There will continue to be differences of opinion, I fear.

Question 174

Some soils that are subject to frequent flooding fail to meet either a) irregular distribution of organic carbon with depth or b) more than 0.2 percent organic carbon at depth of 1.25m. Did the original concept of Fluventic soils include such polypedons? If so, should the criteria be reevaluated?

Guy Smith:

I would be quite happy to see the criteria reexamined. Before we began work on *Soil Taxonomy* we were dealing with concepts of Regosols, Alluvial Soils, and Lithosols, and there was a question in my mind of how much difference it actually made if, say, a calcareous loess were eroded from a hillside and the deposit spread out on a floodplain below. Was the soil drastically modified by that transport and redeposition? Some properties generally are modified by that process and some organic matter is deposited with the sediment that was not in the original material that was being eroded. The concept of alluvial soils, of course, led to the concept of the Fluvents. We wanted to distinguish them at a rather high categoric level because of their enormous agricultural potential compared to other kinds of Entisols. The only features I could find that would define them were these two points mentioned. I am aware that we have soils in alluvium in arid regions eroded from higher lying areas which have virtually no organic matter in them and these do not meet either of these requirements. They get classified as Orthids. However, they do not flood frequently. It is rare that they actually flood, but they do. They are subject to flooding. One thing that we should like to be able to say about Fluvents is that, unless protected, they are subject to flooding.

Question 175

Certain areas that receive large amounts of snow, like areas to the lee of the Great Lakes, have higher winter soil temperatures than would be predicted from air temperatures. Consequently, average annual soil temperatures are higher and qualify as mesic although in both growing seasons, air and soil temperature 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?

Guy Smith:

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 176

In some soils the decision about whether or not a cambic horizon is present rests on identification of developed soil structure. That identification is very difficult in some soils. Can you describe the criteria that you have used to establish the limit for minimal soil structure necessary for a cambic horizon?

Guy Smith:

I do not think that soil structure is really required for a cambic horizon. I would have to look at the book. I am quite confident we said that rock structure must be absent and tried to specify what rock structure was. We say soil structure or absence of rock structure in at least half the volume of the horizon you want to call cambic. Where you have an alluvium and it has been in place one to three hundred years and you are concerned with whether this is a Fluventic Inceptisol of some sort. The rock structure is just the fine stratifications and normally is difficult to see unless you use special techniques. A pump for inflating a tire is a very useful thing to find that rock structure. You make a cut and then you blow a jet of air on that and if you do have these fine stratifications you normally will find them unless, of course, the soil is saturated with water. Then you are in trouble. You have to come back another day. I have only run into this problem a few times in my travels when I was uncertain as to whether or not rock structure was present in half the volume. I suppose this will have to be left to the people who are revising the Soil Survey Manual.

(It has been difficult because in some cases there are alluvial sediments that have enough biological activity so that you get what appears to be, more or less, a constant change of structural things. You do not see stratification but you see evidence of biological activity that has modified it. Sometimes you wonder if the rock structure is adequate as a soil structure to recognize it as a cambic.)

The biological activity either results in the formation of warm casts or in the mixing of the soil by the growth of the roots of plants. Generally, unless the parent material is just marginal to a loamy sand, you should find some blocks developed or prisms. Even in perhumid climates there is a weak development of blocky structure. I do not think I would want to try to do what they ask here, to describe how I would recognize it.

Question 177

Could you tell us something of the rationale of how you treated the soils formed from the serpentine soils? In some ways you get a very pronounced chemical effect, in some ways you have a pronounced morphologic effect. What were your ideas on how some of these soils were separated? What was the rationale when you were thinking about separation of the soils?

Guy Smith:

I think one can get more than one kind of soil from serpentine. Take the example of the Nipe of Puerto Rico--an Oxisol. Almost without exception, the soils from serpentine seems to have serious soil management problems; the natural vegetation is quite different between a soil on serpentine and a soil on limestone or basalt. We actually have very few clues as to why; people speculate, but we do not know why. Until we have some fairly good data covering a variety of soils, and identifying the nature of the chemical problem, we can not propose a way to handle it. But on Tobago and Trinidad, some soils that should be productive soils but nothing will grow. We have soils from serpentine, and we have soils that behave like them, but we can not identify the serpentine. It is probably some chemical property that is still unknown to me.

Question 178

In soils found from volcanic ash, I get the impression that ash influence was more strongly recognized in your more humid soils than in your more arid soils. Would you comment on that?

Guy Smith:

That impression 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 179

Do you have any suggestions on how we could bring some of these observations into *Soil Taxonomy*?

Guy Smith:

No, I think not, the problem is quite complicated. In New Zealand, the ash is all on the north island, and we have no arid zones on the north island. We do have some relatively dry areas there, and we have duripans in the soil, very nice ones. As we go up in elevation, the rainfall increases and the duripan grades into a fragipan. Going still higher, we get more and more ash and then we go into an Andept. These duripans and fragipans are certainly a function of the glass in the parent material, not pure ash, but if you have a duripan or fragipan there, you can always identify some pyroclastic components above the pan.

We have fragipans in New Zealand that we cannot assign to any ash component on the south island. A fragipan is a bit of a problem here. We assume fragipans did not occur in dry climates, but they do in New Zealand. They take it as evidence of a dry climate if they find a fragipan, because that is where most of them occur, as you know. I cannot make a suggestion on this; I can only react to one.

Question 180

Getting back to soil temperature. Bringing it into family level, the other components of the family level seem to be very specific for within a survey area. Why was it brought in at the family level and not at a higher level, or it has been suggested by some people, bring it in at a lower level? Would you comment on why it came in at the family level.

Guy Smith:

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.