CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPED CONVERSATIONS)

by

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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 by Mike L. Leamy

July 10 - 20, 1980

Ghent, Belgium

Introduction

These conversations were commenced in 1977 when Dr. Smith was consultant in taxonomy to the New Zealand Soil Bureau. The conversations commenced with a discussion on the diagnostic horizons and the reasons for many of the parameters that had been used, and by the time Dr. Smith left New Zealand, they had not been concluded. The continuation of these conversations took place in Ghent, Belgium in July 1980, and concentrates initially on a large number of questions which have been collected world wide about many aspects of Soil Taxonomy. These questions have been derived from people who are using Soil Taxonomy to make or interpret soil surveys or to teach Soil Taxonomy to students.

Question 1

The first question which is of concern to many people who are using Soil Taxonomy to make soil surveys, is about the large number of taxa that is possible to identify within a mapping unit particularly at large scale soil surveys. Traditionally pedologists have regarded mapping units as a homogeneous concept for mapping purposes and while they have known that there is variation within that mapping unit, it has not been able to be defined. Now with Soil Taxonomy it is possible to define the variations, and in some very detailed surveys in New Zealand some mapping units on flood plains may contain three or even four different Orders. This is a source of some concern particularly for younger pedologists and I would like to ask you to comment on the advantages or disadvantages and on the philosophy behind the mapping unit-taxonomic unit as expressed through *Soil Taxonomy*.

Guy Smith:

It seems likely that at least some of those who are bothered by this problem have not read chapter 19, page 407 of *Soil Taxonomy*. We stress there, that there is a distinction between the taxonomic unit, which is conceptual, and the mapping unit which portrays or attempts at least to portray the real bodies of soil that we find in the field. The limits of the polypedon, which is a taxonomic unit, are controlled by natural factors of soil formation. The limits of the mapping unit which attempts to portray polypedons or associations or complexes of polypedons, are controlled by another set of factors, namely the distribution and the size of the polypedons. Natural bodies that match the definition of polypedons are controlled by the same factors as the concept of the polypedons but the limits of the mapping unit are controlled by another set of factors which include both the scale of the map that we are making and the purposes for which we are making the map. If the map is being made for very intensive land use, such as irrigated agriculture, we normally must use a larger scale and we must show the variations in the soil that are going to affect the use of a particular spot for irrigation. The same area being mapped for extensive use, as range land, is going to be made at a very much smaller scale and we would ignore differences that we are required to show on the map made for irrigation. The problem arises when we attempt to use the same name for a taxonomic unit and a cartographic unit. The concepts of the polypedon require the maker of the map to study the mapping unit and the kinds of soil that it includes rather carefully so that he knows something about the actual variability of soil properties within the area that he includes within a single map delineation.

Having done this, he must decide in putting a name on the mapping delineation, on the kinds of variabilities he has and how these affect the use of the soil for the probable uses that he can foresee. Soil differences that change the classification of the mapping unit from one Order to another, perhaps from Inceptisol to Mollisol because of a difference of a few centimeters in the thickness of the epipedon which changes it from ochric to mollic, may not be relevant to the use of the soil. If, both the soils with and without the mollic epipedon have exactly the same family modifiers in the family name, it is unlikely that this difference is going to be relevant to any particular use. Therefore, in selecting the name, the maker of the map may select whichever of these taxa are more extensive in the field. The user of the map is not particularly concerned with the taxonomy, he is concerned with the interpretations that he is furnished by the maker of the map. The important thing for the map maker is that he does not mislead the user of the map. If there are differences within the kinds of soil that are included within the map delineation that are significant to the prospective uses of the soil, the maker of the map in selecting his name must then consider the alternatives for names to reflect the presence of soils that behave in a significantly different manner. If the percentages are very small, he may either choose to neglect these in naming the unit or to indicate the locations of the contrasting soils with spot symbols. Or if the variability is such that it affects management of the entire mapping unit he will probably choose to name the mapping unit as a complex or as an association so that the map user is warned that there are going to be specific problems in the use of that particular mapping delineation. In the U.S. there are certain conventions that are agreed upon for reconciling the differences between the conceptual soil taxa in the cartographic mapping units. These standards have changed in the past and will probably change again in the future. Soil surveys in other countries will find it necessary if there are many workers to agree upon some standards so that there may be then be uniformity in the naming of the mapping units in the various surveys that are conducted concurrently.

Question 2

There is another question which is related in some way to the one you have just answered and it is question number 11 (of Rust). It appears to us that since Soil Taxonomy was adopted by the soil survey, mappers and correlators have emphasized soil taxonomic units at the expense of natural soil landscape units, even more than they had before. Often the soil in the field needing to be classified at the series level fits the concept of a widely used series A according to its stratigraphy and geomorphology but fails to meet some profile property requirement of that series or a requirement accumulated at higher categories of *Soil Taxonomy*. Hiding behind taxadjuncts and variance does not satisfy the correlators. Then using the descriptions and profile data the soil is classified at the family level, the list of series having that classification is consulted and series B is selected even though its stratigraphy, geomorphology and setting does not match the soil in the field. This results in a soil map that is technically accurate according to *Soil Taxonomy* but does not fit the landscape. Do you recognize this problem and if so how do you propose to solve it? Stated another way, is an underlying assumption of *Soil Taxonomy* that soil profile properties are more important than soil landscape properties?

<u>Guy Smith:</u>

The opening part of this question appears to be a criticism more of the correlators application of *Soil Taxonomy* rather than of *Soil Taxonomy* itself. This is not a place for a comment on that part of the statement rather than the question. The closing sentence in the question is a question that deserves an answer and it does relate to the application of *Soil Taxonomy* to the soil survey. Soil landscape properties are probably not well defined and since the man who submitted the question is not here to explain it, I am required to make my own interpretation of the question. I am assuming that an example of the problem referred to in this question might be a landscape in which we have a soil on an interfluv in the uplands and on a terrace bordering the stream which has the same properties that have been used as diagnostic in Soil Taxonomy. The one on the upland is presumably a different soil landscape than the one on the terrace nearby. In this situation I believe *Soil Taxonomy* assumes that the soil properties are more important then the difference in the geomorphic history of that particular landscape. There are many areas that are covered by loess in the midwestern states where the same loess, blankets the terraces of different levels and the uplands. In this situation Soil Taxonomy surely assumes that the soil properties are more important than the geomorphological history. If there is a probable difference in some particular behavior of the soil on the terrace and the soil on the upland, in that perhaps on the terrace a well is more apt to find water than in the upland, then *Soil Taxonomy* is very clear that this is an appropriate use of a soil phase.

Question 3

The next question concerns the classification of dry polar soils and arises from considerable activity by New Zealand soils scientists in the Antarctic over the last 10 to 15 years. The question that they have, now that they are generating considerable data about these soils, is how should they be classified.

Guy Smith:

At the time Soil Taxonomy was written, there was virtually no modern description of a dry polar soil in the literature. The definition of the aridic soil moisture regime is such that there is no provision made for a polar soil that has an aridic soil moisture regime and accumulated significant amounts of salt. A soil that never gets as warm as five degrees at 50 centimeters depth cannot be an Aridisol because it does not have an aridic soil moisture regime and it never reaches the 5 degree limit during the year so that it cannot be dried more than half of zero time. The gap left between the definition of aridic, ustic and xeric soil moisture regimes was deliberate. We have no information about these soils that enable us to develop that part of the taxonomy and had we attempted to close that gap so that there would be a place for every soil, we feared that the pedologist might attempt to classify the soil by simply applying the definitions in Soil Taxonomy. *It must be remembered that classification involves not only the application of the rules to see where the soil fits in Soil Taxonomy but equally importantly, it requires that the classifier study that classification to see whether that is appropriate.* Many of the limits in *Soil Taxonomy* were selected to group the soils of the U.S. into classes that had some real meaning. The purpose of classification is to put together the objects that belong together. How does the classifier decide what things, do or do not belong together? The classification problem is not too difficult; he has the rule that the things that belong together have common properties and common behavior characteristics. A soil that has accumulated an appreciable conductivity under irrigation, may be capable of supporting at least one or even two crops a year under rain-fed agriculture and yet the rules of taxonomy say that it is an Aridisol. This is obviously absurd if one considers whether such a soil that accumulated its salts under irrigation and can loose them readily, if they are leached to reclaim the soil from its saltiness. We would then have a soil that changes back and forth from an Aridisol to an Inceptisol according to the year that the leaching is carried out. The absurdity of this sort of classification should be apparent to anyone who is more concerned with putting the things that belong together into a taxon, than following the rules that are set by the limits of Soil Taxonomy. The classification of the polar soils is going to be determined by this general principle that the things that belong together have similar morphologies and similar behavior. We left this question hanging so that those who have studied the soils can propose a reasonable classification.

Question 4

The next series of questions concerns buried soils and they are not really questions but are comments which indicate that people have difficulty in handling the buried soil concept in Soil Taxonomy. Some of the comments ask for more explanation, others like one from Australia which is question number 43 and those put together by professor Rust state, we feel that *Soil Taxonomy* falls short of accounting for buried soil profiles, in particular those of the Murray-Murrumbidgee-Darling river valleys which form such a large portion of the arable areas in this vast but arid continent (referring to Australia). It is clear that people require some further explanation as to how to handle buried soils.

Guy Smith:

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

Similarly, the soils formed on volcanic ash and pumice normally have buried soils; very frequently one or more within the control section that we use for the Andepts or Andisols. Here if the buried soil is another soil formed in ash, the family level permits us to show the contrasting particle size distribution and the series level would permit showing a buried A but as we find this to be almost normal in soils from volcanic ash, we have generally kept such differences at the series level if they were relevant or we have disregarded them completely.

Question 5

The next group of questions relate to the Mollisol Order and I have had inquiries from New Zealand from Central and South America and there is a question in those collected by professor Rust (number 24) which expresses most of the concern that is expressed about this. I would like to know why so much emphasis is placed on the presence of the mollic epipedon at the highest level of classification. This seems to group very dissimilar soils at the Order level, when the only common features of these soils is a relatively thick dark surface with a high base status.

Guy Smith:

First, I should like to point out that the mollic epipedon is required for all Mollisols but is also permitted in a number of other Orders, including Inceptisols, Alfisols, Ultisols, and Vertisols. The mollic epipedon is not the only common feature of the Mollisols. The Mollisols must have, not only a mollic epipedon but they must have a base saturation of more than 50 percent in all sub horizons below the epipedon and within the control section. The mollic epipedon is only required for Mollisols, permissible in four other Orders, but prohibited in Entisols and Aridisols. The concept of the mollic epipedon is not only that of the dark colored surface horizon of the Chernozem. Rather it is the concept of a dark colored epipedon in which there has been decomposition of plant residues underground in the presence of considerable amounts of calcium. In developing the concepts of the Orders of *Soil Taxonomy*, we looked for some common feature that would group the soils of the former great soil groups of Prairie Soils or Brunizems, Chernozems, Chestnut soils, and Reddish Chestnut soils. These were soils that had formed under the influence of a dominantly grass vegetation. The only common features that we could find amongst these soils were, the presence of a dark colored surface horizon of variable thickness and high base saturation. In the U.S., very commonly there was a horizon of accumulation of calcium carbonates but this was not a universal feature because it was missing amongst the prairie soils or Brunizems. We had in the previous classification, that had been in use in the U.S., a suborder titled, "Dark Colored Soils of the Semiarid, Subhumid and Humid Grasslands." This suborder was a modification of the classification of Marbut in which he divided all soils into the Pedalfers and Pedocals. In 1938 it was desired to group the prairie soils with the Chernozems on the basis of the dark colored surface horizons and the grass vegetation. With this rather long traditional emphasis on the grouping of the grassland soils, it is not surprising that when we developed *Soil Taxonomy* we continued to give it an important place in the classification.

Nevertheless, we recognize that there were other dark colored soils that have low base saturation and that it was always possible in fact probable that many of these had received an application of lime adequate to change their former umbric epipedon into a mollic epipedon. This is why the mollic epipedon is permitted in soils that normally have rather acid subsoil horizons. Having reached the decision to use the presence of the mollic epipedon and high base saturation at the definition of the Order, we still had some other soils that did not have a grass vegetation but did have a mollic epipedon. Amongst these were the Rendzinas, and some brown forest soils. These had been considered amongst the others, as intrazonal soils and there was no readily available order to put them in on the basis of their genesis alone. So we simply included them with the Mollisols as a separate suborder.

There are of course still remaining serious problems about the definition of the mollic epipedon. We have many soils that have formed under a swamp vegetation that have a mollic epipedon and that are currently grouped with the poorly drained soils that had formerly a grass and sedge vegetation. Therefore, the suborder of Aquolls has a much wider geographic distribution then do the Ustolls or the Udolls.

The second problem relating to the mollic epipedon is the limits for thickness. In a number of soils the normal thickness of the mollic epipedon is just at the limit of 25 centimeters. This makes considerable trouble for a pedologist who is a purist and wants to classify everything on the basis of whether or not it fits the definition of a mollic epipedon without regard to whether or not the difference of one or two centimeters in thickness is relevant to the purposes of his soil surveys. We also have the problem of the soils with mollic epipedons in the inter tropical regions. The definitions of *Soil Taxonomy* are written primarily for the soils of the U.S. and other temperate regions. We point out specifically that we have no good opportunity to test the classification of the soils in the inter tropical regions in the U.S. and this testing must be done in other countries. We think, over time, that some of these problems can be worked out through the help of the international committees on taxonomic problems.

Question 6

The next question I would like to ask, concerns the use of subgroups and in particular many practicing soil surveyors in correlators in countries outside the U.S. become a little uncertain as to whether it is legitimate to apply subgroups in a Great group where that subgroup is not defined for the Great group. Now I know there are some things called implied subgroups and we have had some direction on that, but I would like you to discuss the use of subgroup generally and whether you can apply a subgroup which has been defined for a particular great group in one Order to another great group in another Order if it is applicable.

Guy Smith:

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

Question 7

In many active national soil surveys outside the United States, it is imperative that decisions about taxa be made with due dispatch so that the interpretations of the soil survey can be proceeded with and the benefit to the users be realized. It seems to me that in the case of definition of subgroups which have not been specified for in *Soil Taxonomy,* could well be delegated to correlation systems in nations that had active soil surveys and that international acceptance would be achieved by notification at regular intervals. Such notification being not for approval but for information. I do not see that there would be any great danger in this procedure as long as national correlators followed the rules which you have outlined in answering this question. I wonder if you would like to comment on this statement.

Guy Smith:

My first comment would be that this problem is not restricted to subgroups but also occurs in great groups and conceivably could occur at higher categories than the great groups. For example, in New Zealand we found in a Dystrochrept with a placic horizon, a combination of horizons that is not provided for in *Soil Taxonomy.* It was necessary, then, for me to consider whether or not placic horizon had an effect on the interpretations and required recognition of some sort of a Dystrochrept that had a placic horizon. Well, anyone who has seen a placic horizon realizes that it interferes seriously with movement of water and penetration of roots and has an important effect on our interpretations so that this combination of horizons required recognition of a new taxon at some categoric level. The normal rule for *Soil Taxonomy* was that the presence of a pan, like a placic horizon or fragipan, was recognized at the great group level. The combination of the ochric epipedon, the cambic horizon, and the placic horizon, not being provided for in *Soil Taxonomy* and requiring a new group if we were consistent with the recognition of pans in other taxa throughout the taxonomy, implied that a new great group was required. However, the ochric epipedon was always marginal to an umbric epipedon and we also had the combination of an umbric epipedon, a cambic horizon and a placic horizon, and the differences in the thickness of the horizon, dark-colored enough for umbric, was always close to the necessary limit of 25 cm. It might be 20 cm or it might be 30, but this was the normal range of thickness. Therefore, I made a proposal in New Zealand that we not worry about the presence of an umbric or an ochric epipedon in the definition of this combination of Inceptisols with placic horizons. This then required a change in the definitions of Ochrepts and Umbrepts so that one great group or another had either an ochric or an umbric epipedon and a placic horizon. This kept together this group of soils that belonged together. Similar occurrences of unanticipated combinations of horizons and properties are surely going to be found in all categories. The problem of the undefined subgroups is no different from that of the undefined great groups except that having more subgroups than great groups, it may be more common.

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

Question 8

The next question is number 44 and those put together by Dr. Rust and is as follows: In the suborder or great group level keys, the relevance of one criterion in the exclusion process is not uniform. The aquic suborder goes first in many orders but not in Mollisols, the pale great group is first in Xerults, second in Ustults, and fourth in Aquults. Why is this?

Guy Smith:

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

I should point out that in Chapter 7, on page 91, the use of keys throughout the text is discussed. We point out that the reader or the user should use the key first to the Order and to then select the most probable order that he can find for the classification of a particular kind of soil. He then goes to the page indicated in the key and at that point he will find a complete definition of the Order in terms of the properties of the order and the distinctions between that Order and other Orders. If the soil that he is concerned with meets the requirements of that particular order definition, then he continues on to the key to the suborders. Again he selects the most probable suborder, turns to the indicated page, and then checks the particular soil against the definition of that suborder and so on down the line through the keys to the complete definitions of the various taxa.

Question 9

Question #50 compiled by Professor Rust: Is an organized order-by-order, suborder-by-suborder, etc., table of differentiae within each category of the taxonomy possible that would portray the logic of the system as it was developed? And the comment is made: Similar tables are given using the nomenclature but none seems to have been devised to portray the logic.

Guy Smith:

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

Question 10

Question #47 and those compiled by Professor Rust: the concept of Inceptisols seems to be one of the more difficult to accept. Can you give some background on the Order?

Guy Smith:

During the development of *Soil Taxonomy* certain groups of soils appeared with many characteristics or a few characteristics that were common that seemed to be closely related enough to justify recognition as an order. For example the Vertisols, with their expanding clays and their occasional or frequent dry seasons, with their wide cracks, constituted a group of soils that it seems should be recognized as distinct from other kinds of soils. Similarly, the Mollisols with their mollic epipedons and high base status, seem to warrant the recognition of their own order, the Entisols lacking any diagnostic horizons seem worthy of recognition as an order and so on. Eventually we had nine apparently satisfactory groups of soils that could be used to recognize orders. However, every taxonomy has a waste basket. When we finished with the nine Orders we still had many soils left over that appeared better not grouped with any of the other Orders. We tried, for example, to group soils with cambic horizons and with argillic horizons in various approximations and none of the groupings seemed to be satisfactory. From a genetic point of view, one could group some of the Inceptisols with the Alfisols on the basis that they are going to develop into Alfisols over time but we had to reject this kind of an assumption on the basis that the Inceptisols, being weakly developed, might develop into Alfisols or Ultisols over time but on the other hand if erosion exceeded the rate of soil development they might develop into Entisols. It was not possible to group the soils on the basis of a genetic assumption that over time they would develop into another kind of soil. So the Inceptisols represented the kind of soils that did not seem to fit into any other order. Over time now we have concluded that the suborder of Andepts has enough properties in common that they should be recognized as an eleventh Order. In time, there may be other Orders cut out of the suborder of Inceptisols but this is a matter of future knowledge rather than the present knowledge that we had at the time we developed *Soil Taxonomy*.

Question 11

The next question concerns soil temperature regimes and it is #45 compiled by Professor Rust how the criteria for soil temperature regimes, that is, 0-8, 8-15, 15-22, and greater than 22 degrees C soil temperature are based on temperature-crop needs. Is the concept of zonality based on commercial crops valid in a natural system?

Guy Smith:

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

To classify the soils on an ascending order, meant grouping soil series into natural taxa on the basis of their affinities. However, there were too many thousands of series to be comprehended by any one individual so that the system as it developed was a sort of compromise that soils were subdivided from the top down on the basis of certain properties and the classes that resulted were tested by examining the nature of the series that were grouped by the criteria from the top down. So the development of the taxonomy was a compromise classifying by subdivision or classifying by combinations.

To understand the problems that were involved in classifying the soils by groupings of series one must understand that there were thousands of series (too many to be understood, by any single individual) and that the series had been in use for many years and had been tested by use and found to be useful. The newspaper advertisements in Iowa, for example, would advertise a farm for sale as 160 acres of Carrington loam. This was a use of the series name. The buyer and the seller got information from the use of the name Carrington. The highway engineers used the series as a basis for planning their secondary road construction. When the highway engineers heard that we were developing a new system of taxonomy, they required me to appear before the Highway Research Board to explain what we were going to do with the soil series because they wanted them retained to the maximum extent possible and they were placated when I explained that it was our goal to split as few series as possible and only, when the split seemed to warrant an improvement in our interpretations. When the criteria proposed in the earlier approximations were examined by seeing how the series were grouped, I received repeated complaints that this is not good because this splits our series; the goal was to retain the series as nearly as possible with their previous use. However, the series were not defined on the basis of temperature or moisture. These were inferred characteristics and related to the series but not appearing in the series definitions. Where the type of farming changed we made different interpretations. For example, the interpretations for soils cropped to cotton were not the same interpretations that we made for soils cropped to maize or to spring wheat. Therefore, the series normally changed with the type of farming. How it happens that the limit between the Cotton Belt and the Corn Belt, between the Cotton Belt and the Winter Wheat Belt, between the Red Desert soils and the Gray Desert soils was the same, always at 15 degrees C mean annual temperature. Therefore, this was a natural limit that did not split series. The Red Desert-Gray Desert separation was based on the natural vegetation--creosote bush--being present in the Red Desert and absent in the Gray Desert. If one studies the general soil map of the United States that was published in the 1938 Year Book of Agriculture - Soils and Men - it is immediately obvious that the boundary between Red-Yellow Podzolic soils and Gray-Brown Podzolic soils follows the 15 degree C soil temperature isotherms. This was not based on the type of farming because we currently have in the Alfisols, the recent soils on loess along the Mississippi Valley, although these were previously called Red-Yellow Podzolic soils and now they are thermic Hapludalfs and so on. While the correlation is imperfect, the differences in type of natural vegetation were rather apparent but with an imperfect correlation between the distinction between thermic and mesic soil temperature regimes.

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

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

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

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

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

Questions 12

The next question is #46 on those compiled by Professor Rust. The classification of most soils, possibly excepting the Oxisols, is based on morphology to a depth of about 1 meter. On the other hand, many applications in engineering and agriculture require information on materials below this depth. How do we reconcile this problem?

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

Question 13

The next series of questions concern particular queries that we have received from people working with *Soil Taxonomy* throughout the world, about different aspects of the diagnostic horizons. The first one concerns the oxic horizon and we have the same query from two sources question number 19 and those compiled by Professor Rust and also a question from a soil survey people in Thailand. The Thai people ask why the oxic horizon must have more than 15 percent clay? They commented that some soils in Thailand have red color, diffuse boundary, very high porosity but do not have weatherable minerals, or clay coatings and the sub horizons fits all the requirements for oxic horizon except the clay parameter and they consider that it is not comfortable to identify this horizon as a cambic horizon because the soil is old and highly weathered. The question compiled by Professor Rust reads as follows: we have found sandy soils with about 5 percent clay content that have all the properties of Oxisols except the clay content and really these properties give them very special nature and behavior. I do not really know the reasons for restricting oxic horizon to horizons with greater than 15 percent clay; there are extensive areas with sandy soils that have, besides the property of sandy soils, marked
characteristics that identify them as Oxisols. Should we not permit very sandy soils in the order of Oxisols?

<u>Guy Smith</u>:

It would be possible to permit the oxic horizon to have a texture of sand and to lack all weatherable minerals. In this case, the horizon would consist of quartz, free oxides, perhaps traces of 1:1 lattice clays. However, the Oxisols grade into the Quartzipsamments and if we include in Oxisols, soils with 5 percent clay then we must find some limit between 5 and 5/10 percent clay to stop the oxic horizon and go into Quartzipsamments because Quartzipsamments are frequently highly weathered and the clay fraction consists of kaolinite and free oxides. There must be some sort of limit between the oxic horizon and the Quartzipsamments because on the landscape they grade one into another particularly in the Zaire and other parts of Southern Africa.

I have proposed that the limit on clay be dropped and that a limit on texture be substituted. Namely the oxic horizon should have a sandy loam texture and the Quartzipsamments should have a sandy texture. The taxonomy provides for oxic subgroups of the Quartzipsamments and one within the landscape go from an Oxisol to an Oxic Quartzipsamment and finally to a Typic Quartzipsamment in which the sand grains are largely un coated. The clay limit was inserted originally in order to make a break between the sandy loams and the sands or loamy sands on the assumption that the highly weathered soils have virtually no silt. However, we have found a number of soils that have no weatherable minerals that have less than 15 percent clay actually 10 to 12, and have enough silt that they have a sandy loam texture rather than a sand or loamy sand texture. To classify these soils as Quartzipsamments are impossible because of the sandy loam texture. To classify them as Oxisols is impossible according to the limits in Soil Taxonomy. They become orphans, and knowing something about the soils it seems obvious they belong better with the Oxisols then they do with the Entisols which is where they would have to be put if they had no diagnostic horizon. We have found such soils in Venezuela, and it seems very likely that they will also occur in other parts of the world. For the most part the Quartzipsamments have more than 99 percent unweatherable minerals in the silt and sand fraction although the limit in taxonomy is set at 95 percent. They represent soils that may be very recent in origin, occurring on coastal dunes where the sands on the beach are almost pure quartz. They occur on very old landscapes where the sands have been in place for a long time and have had all of the weatherable minerals removed. They also occur as greatly over-thickened albic horizons with an underlying spodic horizon that is more than two meters deep. Most of such soils with the thick albic horizon or those on the recent dunes, are almost totally lacking in clay and dominated by quartz. The intergrades between the Oxisols that have a sandy loam oxic horizon and the Quartzipsamments, that are almost completely lacking in clay, must find some place in the taxonomy. The Psamments were distinguished from other soils on the grounds that they have some very specific physical properties. They are, when dry, subject to blowing and drifting. When dry they are also very difficult to traverse with wheeled vehicles. The Oxidols on the other hand do not have these specific properties. So we need a limit somewhere between the Oxisols and the Psamments including Quartzipsamments that is based on the point at which we begin to develop these particular properties of psamments. It seemed reasonable to us when we developed the taxonomy, since there is a continual gradation between Oxisols and Quartzipsamments, to have some limit that recognize the point at which we begin to develop trafficability and blowing problems. This was the basic reason for the 15 percent clay limit, which we know now was wrong because the presence of the appreciable silt plus clay does not produce the peculiar properties of the Psamments.

We prepared, about 10 years ago, a manual of field soil survey investigations of showing the things that the field men could do in their offices without requiring the existence of services from a laboratory. One of the tests that we described is one for the estimation of the percent age of quartz in the sand fraction. This is based on covering the sand fraction with a liquid that has the same refractive index as quartz. When one does this the quartz becomes invisible and the other kinds of sands remain in plain sight in the liquid.

Question 14

Question number 16 compiled by Professor Rust reads as follows: Argillic horizons are accepted in profiles which belong to the order of Aridisols, Mollisols, and Vertisols. Other properties (moisture regime, mollic epipedon, spodic horizon, etc) were considered more important and given more diagnostic weight to create Orders than the argillic horizons. This was not the case when the oxic horizon was considered, why? The 7th approximation (Soil Survey Staff, 1960) gave the prominence in the key for soil Orders, to either the argillic or the oxic horizon. Finally, in Soil Taxonomy (Soil Survey Staff, 1975) the argillic was given priority over the oxic. Why? Question number 12 compiled by Professor Rust also relates to the same question as to why more weight in the soil key is given to the argillic than the oxic and asks if there is a reason for that.

Guy Smith:

I should point out first that the emphasis to the argillic horizon over the oxic horizon applies only to the soils in which there is an argillic horizon overlying the oxic horizon. An argillic horizon underlying an oxic horizon is not grounds for keeping a soil out of Oxisols. We generally use the principle in developing *Soil Taxonomy* that, if we have two subsurface diagnostic horizons in the soil, the preference is given at the higher category to the horizon nearest the surface. Thus the soil with both a spodic horizon and an argillic horizon is normally classified as a Spodosol because the spodic horizon overlies the argillic horizon and the assumption is, that the more recent processes that dominate in the genesis in the soil produce the diagnostic horizons closer to the surface than the older process, which produce the diagnostic horizon at a greater depth. This assumption is consistently used in the various Orders where we have the two or more diagnostic subsurface horizons. There is no distinction between the use of the argillic horizon in spodosols and in Oxisols. I suspect these questions about the argillic horizon's significance to soil classification arise from a failure to read carefully the full text of the discussion of the argillic horizon. On page 20 under the heading, 'Significance to Soil Classification', there appears this statement "It is stressed that the argillic horizon is no more important to soil classification and to soil genesis than many other horizons. It has been used at a higher categoric level in some parts of the system only because that use has produced groupings of soils that have the largest number of common properties that are important to use of the soils."

Question 15

The next question comes from Thailand and concerns a particular situation in which you may find an apparent argillic horizon. Soils developed on foot hill slopes consists of rock fragments not rock structure and clay coatings can be observed especially around the rock fragments. Should this be identified as an argillic horizon even though the soil occurs on the relatively young landscape?

Guy Smith:

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

Question 16

The next question concerns the difficulty of recognizing an argillic horizon in which soils with a high clay content, say 80 percent clay throughout the solum; it is very difficult to identify clay skins in such material and the parameter recommended in *Soil Taxonomy* is the ratio of fine clay to coarse clay, which is not easily recognizable in the field. The question is, are there any field recognition tests which might help the fieldman to identify an argillic horizon in such soils?

Guy Smith:

I know of no field clues that can be used generally to recognize argillic horizons in soils with very high clay contents. If the eluvial horizon has 80 percent clay and the illuvial horizon, 90 percent clay, the distinction can be recognized by a very experienced pedologist. However, the beginners will not recognize the distinction between horizons that have 10 percent differences of clay when the clay content is so high. The things that one may see in the field would be the colors of the ped faces in such soils. Not much else can be observed. To make interpretations of the presence or absence of argillic horizon some laboratory studies would be required. The ratio of fine/coarse clay is one of the most important. The difference in clay content in such soils is commonly due to a difference in sedimentation rather than pedogenesis. However in a particular survey area, with some laboratory analysis of the coarse to fine clay ratio one can judge that there should be an argillic horizon or should not. Some benchmark studies in such soils in the laboratory would be essential in order to have confidence of the presence or absence of an argillic horizon. These benchmark studies must be on a survey area basis. No general statement is possible.

Question 17

The next question is number 34 and is compiled by Professor Rust and raises the whole question of the use of soil temperature regimes at categoric levels higher than the family. The question concerns the use of tropic great groups instead of tropic subgroups. For example, if an Aquult with an isohypothermic temperature regimes has plinthite, a fragipan, an abrupt textural change, or a 'pale' curve, it falls into a particular great group regardless of the temperature regime. While in the same general landscape a soil without any of the above features would be classified as a Typic Tropaquult.

In other words, when does or should an isomesic or warmer isotemperature regime become of equal importance at the great group level to soil properties? I would much rather see the use of tropic as a subgroup modifier.

Another alternative would be to drop the use of the tropic modifier since the soil is already identified by the isomesic, isothermic, or isohypothermic temperature designation.

Guy Smith:

This is not one question but a least three and I must answer one at a time. He asks, for example, if an Aquult with an isohypothermic temperature regime has plinthite, fragipan, or abrupt textural changes or pale clay curves it falls into a particular great group regardless of the temperature regime. I should like first to comment on this one. The Aquults with plinthite are not uncommon in tropical regions with isotemperature regimes; they become rare in the mid-latitudes where the temperature regimes are hyperthermic or thermic. They are unknown where the temperature is mesic. If the temperature is frigid, they are excluded by the definition of Ultisols. Though we have the range of the Aquults with plinthite from the mid-latitudes where the temperatures are thermic or warmer and in the mid-tropics. Actually, we do not have any Plinthaquults in the United States, to my knowledge. So that in devising the definitions it was not necessary to exclude soils with hyperthermic or thermic regimes from the definitions.

Fragipans are virtually unknown in inter tropical regions. They are restricted to soils with thermic in Ultisols or mesic temperature regimes and it seemed unnecessary to write restrictions in the definitions that eliminated soils that were not known to occur.

In the mid-latitudes, the soils with hyperthermic, thermic, and mesic temperature regimes are divided at the great group level according to the nature of the epipedon. We have the Umbraquults and the Ochraquults in the mid-latitudes. If we did not write a requirement of temperature in the Tropaquults, we would have to divide the tropical soils according to Umbraquults and Ochraquults. In the tropics, the amount of organic matter is very poorly related to the darkness of the epipedon. Therefore, having for the United States a separation of Aquults into Umbraquults and Ochraquults, we would have to carry this distinction into the inter tropical regions, where the color has virtually no relation to the organic matter content. Therefore, we have brought in the temperature limits into the Tropaquults, so that we would not be bound by the distinctions of Umbraquults and Ochraquults in soils where color has no relation to organic matter.

The second question implies that soil temperature is not a soil property because it states that an isomesic or warmer isotemperature regime should be of equal importance to soil properties. *It is a basic assumption in Soil Taxonomy that soil temperature and soil moisture regimes are soil properties*, although it is disputed by many. The questioner is one of those who seems to dispute this assumption. If so, he should, I think, devise his own classification system instead of complaining about *Soil Taxonomy*. There is no question but that soil temperature can be estimated approximately from altitude and latitude but there are still considerable differences between soils with the same elevation and latitude depending on aspect and also depending on climatic factors. We have for example inter tropical soils that do not have isotemperature regimes particularly in southeastern Asia. These may occur in other parts of the world but the soil temperatures so far have rarely been measured.

The third part of the question proposes the use of the tropic modifier at the subgroup level or to drop it entirely as the soil is already identified by the soil temperature regime at the family level. This identification at the family level is adequate where one has large scale maps and the map units are defined in terms of family or series characteristics. In small scale maps where the mapping units are mostly in terms of categoric units higher than the family, the soil temperature regime is not indicated by the name. In general, one may assume that the soils between the tropics have isotemperature regimes but one cannot be safe in assuming that the soil temperature is isomesic or warmer between the tropics. It is possible that it is isofrigid. If this is not indicated by the name of the great group, then the temperature must be inferred from a map that shows elevations of the soils, and the maps of the elevation are difficult for the pedologist to obtain but essential for any interpretations whatever. The soil temperature regime should be indicated roughly by the name if we are going to make interpretations of soil map units that are made on small scale maps. If it is not implied by the name of the mapping unit, then the implication must be added as a phase and this complicates legends of small scale maps. There are already so many phases that must be shown, slope, stoniness, depth of soil, textures, etc. It simplifies the matter of phases if the temperature and the moisture regimes can be indicated by the name of the taxon that is used to identify the mapping unit.

A common feature of the three questions is the use of tropic at the great group or the subgroup level. It probably is not material whether one uses the tropo modifier at the great group or the subgroup level other than the problem that requires the extension of the umbric epipedon or the ochric epipedon importance into inter tropical regions. The basic reason for using it at the great group level was to avoid the extension of these concepts that are applicable in temperate regions to inter tropical regions.

Question 18

Further to that question, I would like to ask you why the "trop" formative element is used in some orders at suborder level as in Tropepts rather than at great group level.

Guy Smith:

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

Question 19

Leading on from the last question, it is interesting to note that in the Andisol proposal we have separated Tropands at suborder level. Could you give the reasons for this?

Guy Smith:

The recognition of isotemperature regimes in the Andisols at the suborder level is parallel to the recognition of these temperature regimes in the Inceptisols. The reasons are the same. In the Andisols as well as in the Inceptisols, Ultisols, etc., the color value is very poorly related to the content of organic carbon. If the soils are not separated above the great group level where we recognize amongst Andisols, the Melanudands which are very dark colored, this color value must be extended to the inter tropical soils where the color value is not related to the organic matter content. On the island of St. Vincent in the West Indies, for example, the northern half of the island is covered by a black cinder deposit which dominates the color of the soil. The blackness is not related to the organic carbon, and the more weathered the cinders become the lighter they become in color. We wanted to avoid using color value as an indication of organic matter contents in the inter tropical regions.

Question 20

The next question is from Thailand and asks in essence, why the criteria used to distinguish Tropaquults are not used to distinguish Tropaquepts or Tropaqualfs.

Guy Smith:

The question is not quite properly phrased. In the Paleaquults and Tropaquults, the requirement for color is only that the hue by 2.5Y or 5Y accompanied by mottles due to segregation of iron, or, if the hue is 10YR or redder, then the low chromas are required. Working in Venezuela, I examined the evidences of wetness for aquic great groups and suborders, and made the proposal that the definition used for Ultisols be extended in all orders to the inter tropical regions. Namely, the Inceptisols, the Mollisols, the Oxisols, the Entisols, etc., in Venezuela if they were wet, commonly marked the wetness by the yellow hues accompanied by mottles. The criteria used for the Ultisols might have been applied more generally in *Soil Taxonomy* had we had few examples of other kinds of inter tropical soils.

Question 21

Questions regarding the spodic horizon and Spodosol, generally. The first one is number 8 in those listed by Professor Rust and reads as follows: the spodic horizon as presently defined, relies heavily on laboratory data and in fact, can hardly be established without it. Does not this requirement put a considerable burden on the field soils scientists in their attempt to classify soils?

Guy Smith:

Soil Taxonomy specifically provides criteria for identification of spodic horizons in the field without laboratory data. In my experience it is rather rare that laboratory data are required except in transitional soils where the spodic horizon is marginal to a cambic horizon. I have been criticized by the people working in laboratories that identification of the spodic horizon is too often by field criteria and that the chemical properties are inadequately emphasized.

For field identification the only equipment needed, is a rather powerful hand lens or a pocket microscope that is capable of giving the 60 power magnification. If with this lens the individual is able to identify the crack coatings on sand grains or the pellets, the identification can be made in the field without any laboratory analysis. It is also true that the spodic horizon reacts to fluoride and the Field's test for allophane is used in a number of countries where there is no volcanic ash, to identify the spodic horizon and distinguish it from the cambic horizon.

Question 22

We have had some trouble in New Zealand in classifying soils with spodic characteristics which have also high clay contents; these are some of the classic Kauri Podzols of the previous classification and we find that the horizon with spodic characteristics, may have clay contents as high as 80 percent and if we calculate the pyrophosphate iron and aluminum to clay ratio on the basis of the clay content of the whole horizon, we cannot have a spodic horizon because it is far less than is required by the definition. Can you advise or recommend what we should do about the classification of such soils which obviously have spodic characteristics to some degree?

Guy Smith:

I have seen such soils in New Zealand where the Kauri has produced, what appears to be a spodic horizon and I have also seen such soils in Europe where there has been a vegetation of *Caluna*. In both cases the high clay content appears to be due to the presence of an argillic horizon and the clay skins or coatings are commonly very obvious in the fine textured horizons. The spodic characteristics occur as a very thin layer above the argillic horizon and as rather thick coatings within the argillic horizon where there is obviously some sort of tonguing due to the illuviation of the clay. It is more or less reminiscent of a glossic horizon, but the zones in which the clay has been removed are now in filled with a very highly carbonaceous, very black material, that has all the characteristics of the spodic horizon. If the soil horizon is sampled as a bulk sample the clay content does prevent the identification in the laboratory of the horizon as a spodic horizon. However, if the soil is sampled not as a bulk horizon but as parts, then the spodic parts will not be found to have the high clay content; the high clay content is not the black material that you find between the peds.

Question 23

We have a small point of inquiry about the criterion for the spodic horizon concerning the ratio of pyrophosphate iron and aluminum to percent clay, which is equal to or greater than 0.2. Ragg, Mckeague, and Simonson, have stated that if the ratio is equal to or greater than 0.15 it qualifies for a spodic (that is using only one significant figure). Is this what was intended?

Guy Smith:

The intent of the definition of the spodic horizon as well as in all definitions throughout *Soil Taxonomy* that the numbers follow the normal mathematical rules for rounding. If only one decimal is used in the definitions, the numbers that are intermediate are rounded according to normal mathematical rules, e.g., 0.16 is rounded to 0.2. This was done throughout *Soil Taxonomy* except in the definition of the Andepts where one decimal too many was inserted and we used the numbers .85 for bulk density which is more precise than can be measured in the laboratory and we should have used 0.9 or 0.8 or some number without two significant decimals.

Question 24

One of the greatest changes in the classification of Spodosols in New Zealand when we use taxonomy is that, previously these soils were mostly called podzols and were identified most readily in the field by the presence of pale colored horizons analogous to the albic horizon. Now that we have used taxonomy, this horizon has no impact on the classification until below family level. Some pedologists are concerned at losing the utility of this horizon in their field identification and mapping and this question refers, in general terms, why an albic horizon in Spodosols is not given greater prominence at a higher level in the classification and in particular why there could not be a suborder of Albods.

Guy Smith:

On page 8 of *Soil Taxonomy* the sixth attribute that we desired for the taxonomy was that the differentiae should keep an undisturbed soil and its cultivated or otherwise man-modified equivalence in the same taxon insofar as possible. If the albic horizon is thin, the mere clearing of the forest, seeding of grass, and pasturing can destroy a rather respectable albic horizon. This I demonstrated in one of the type locations of one soil in New Zealand where, in the road bank there was a good albic horizon but if one crossed the fence into the pasture it was gone. This would mean then that if we are going to emphasize the presence or absence of an albic horizon more than the presence of a spodic horizon, one would have to draw a boundary along the fence because that is where the albic horizon stopped. It would be possible of course to emphasize the albic horizon at the expense of the nature of the spodic horizon and if we did that, we would have perhaps an Albod and a Chromod and then these would be subdivided at the great group level into humic and other types of spodic horizons. We felt when we develop taxonomy, perhaps erroneously, that the nature of the materials that accumulated should be given greater weight than the presence or absence of an albic horizon. Certainly if one were to emphasize the importance of the albic horizon, the definition would have to require that it extends to depths greater than 25 cm; otherwise plowing would change the nature of the classification of the soil.

Question 25

In many young landscapes (this is particularly so in the south island of New Zealand), the landscape comprises Inceptisols grading to Spodosols and detailed soil surveys in this environment reveal intergrades between Inceptisols and Spodosols. There is no provision in *Soil Taxonomy* for spodic subgroups of Inceptisols. Could you comment on what should be done in a situation like this?

Guy Smith:

The *Soil Taxonomy* was designed so that the least possible disturbance would be made if new knowledge and experience indicated that we should change some part of the system. In this situation where an intergrade may be desired between an Inceptisol (a Dystrochrept) and a Spodosol, the people who have some experience with these soils must propose that this intergrade be introduced into the system. In making such a proposal it would be essential that the man who makes the proposal, proposes also a definition for the spodic subgroup of the Inceptisol. This is perhaps the most difficult part for making a proposal for a change. *We need to have not only the proposed definition but we need also to have some reason why the change should be made.* Does it improve accuracy of interpretations, if so this should be spelled out in the proposal.

Question 26

The next question comes from both New Zealand and from Thailand and concerns the identification of base saturation to distinguish between Alfisols and Ultisols. The Thai question reads, why is percent base saturation determined at 1.25 m below the top of the argillic horizon or at 1.8 meter below the soil surface. In addition, the Thai pedologist would like to know what to do if at 1.8 meters, there is a lithologic discontinuity with contrasting material. The New Zealand question asks, what is meant by identification of base saturation at certain depths below the argillic horizon. Seconoly, it asks if the sample should be from a thin layer or is an auger sample from the approximate depth satisfactory or should the whole horizon in which the depth occurs be sampled. Thirdly, what is the intent of the requirement of base saturation at depth particularly when the dystro-eutro distinction is made on the basis of base saturation of the major part of the profile?

Guy Smith:

The first comment is, on what is meant by the word at a depth of one and a quarter meters or 1.8 meters. This means according to the English language, "at". It can be measured in one of two ways. Either one takes a sample of a thin subhorizon *at the specified depth* or one samples all horizons above and below the critical depth and then makes a smooth curve of the data and the depth at which that curve crosses the 35 percent base saturation is either above or below the critical depth of 1.8 m or 1.25 m. If the smooth curve crosses the 35 percent base saturation limit at a depth shallower than the critical of 1.25 or 1.8 m then the base saturation is certainly less than 35 percent at the critical depth.

The reason for the choice of 35 percent at the critical depth specified, is the simple one that is common to all the definitions in the taxonomy. It is that we got groupings that permitted us to make more statements and more precise statements about the soil use then we could otherwise make with another limit of base saturation or another limit of depth.

If there is a lithological discontinuity at or above the critical depths of 1.25 or 1.8 m the base saturation at these critical depths is still the 35 percent limit between the Alfisols and Ultisols. The base saturation of a specific horizon is not just a property of that specific horizon but it reflects the entire process of leaching and recycling of bases in the soil which affects the whole soil in all horizons not just the one horizon. The base saturation curves are quite interesting properties of the whole soil rather than of any specific horizon.

Question 27

Question number 3 compiled by Professor Rust reads why was base saturation chosen as the (only?) differentiating criteria between Alfisols and Ultisols? By using it at different levels, does not this "violate" some rule of logic and or taxonomy?

Guy Smith:

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

There is a distinction between the taxonomies of plants and animals on the one hand and soils on the other. The taxonomies of living organisms have in the past generally been built on the phylogenetic principles, namely that of descent. When we try to classify soils, then there are no principles of descent. There are no common ancestors of soils. They are not living organisms; they are as we all know, in the border line area between the biological and earth sciences. But the logical principles of John Stewart Mill, that the best taxonomy or scientific classification is the one that permits the greatest number of the most important statements about the objects that have been classified still applies. In soil science these important statements are our interpretations, not our theory of genesis. Therefore in order to be able to make any statements of any sort about the classification or taxonomy of soils it is necessary to use the same criteria at different categories. The biological taxonomist makes very little, if any, use of the orders in the classes themselves; he is concerned with the species. The pedologists who must make soil surveys at varying scales, makes principal use of the higher categories with small scale maps and principal use of the lower categories with large scale maps. In order to be able to make any statements whatever about the different orders, that can be used with extremely small scale maps, the most important characteristics to the use of the soils must be used at the order level. With somewhat larger small scale maps, the map units may be defined in terms of suborders or great groups (always in terms of phases of course); we may want to make somewhat smaller distinctions in the characteristics that we have used between the orders. When we get to the level of great groups or subgroups used in somewhat larger scale maps, the same characteristics that we have used at the order level may or may not be important but we use them when they are important, important in the sense that they permit us to make statements about the classes that we are showing on our maps. In the detailed soil maps we must have all of the accumulated differences to the maximum extent possible, of features or properties such as base saturation, soil moisture, soil temperature, many, many properties are used throughout the taxonomy. The general rule is that we make the least subdivisions of properties at the highest categories and the maximum subdivisions of properties in the lowest categories because the lowest categories are only used in the large scale maps. The biologists who do not make maps at different scales are not concerned with this particular problem. The problem is that cited by John Stewart Mill, that *the best taxonomy is that one that permits one to make the most statements about the most important properties of the units that have been classified. It is one of the strengths of Soil Taxonomy not the weaknesses that we use the same properties in different categories.* The fewest subdivisions of a given property are used in the higher categories. The largest number of subdivisions of the property are used in the lowest categories. This is completely in agreement with the logic of John Stewart Mill or any other taxonomist I know. If the pedologist were to read the modern literature of the taxonomy of plants or animals, just a few books, he would find statements such like the one I would cite from Cain, that if the botanist or zoologist were faced with the problems of classifying plants or animals according to variability over both time and space, he would find the present system intolerable. The variability over space of a species of animals is something that can be observed today. The variability over time is something that depends on the fossil record and is not only imperfect but often completely absent. In soils we must deal with this variability over time and over space. This is therefore a major difference between taxonomy of soils and taxonomy of living organisms. We therefore must deal with the taxonomy of soils in a somewhat different manner that do the botanists or zoologists deal with their taxonomies. Actually, Cain has suggested that it would be better to drop the system completely and devise a new system but he says because of the priority of nomenclature, we would get into so many troubles that it is probably not worthwhile. When we started to develop *Soil Taxonomy* we decided that if we were going to make a break with the past we had best make it completely at this moment that we publish *Soil Taxonomy*.

The reason that we chose base saturation to separate Alfisols and Ultisols was again that it let us make the most precise statements about the soils included in Alfisols versus Ultisols. The particular statements that we wanted to make about these two classes of soils depended on whether they could be used with or without amendments. The Ultisols cannot be used for a permanent agriculture without amendments except through a system of fallow. The Alfisols have been used without fertilizers or amendments for some few thousands of years and a permanent agriculture can be based on the Alfisols without amendment. This is not generally so with Ultisols, which after a time, generally the soils become so depleted of bases that they cannot be used for agriculture at all even with fallow. This seemed an important statement that we would like to make about the order, at the order level, and it was therefore the base saturation that we selected to reflect the difference between the soils requiring fallow periods and soils that could be used permanently.

Question 28

I have always been interested in the rhodic great groups and in some of the rhodic subgroups because of the distinctive color and I have often wondered what this color signifies in terms of accompanying characteristics and what was the reason for setting up the rhodic great groups.

Guy Smith:

If one examines the soils that are included in Alfisols and Ultisols at any given suborder level, one finds generally that there are soils that have severe problems of soil structure. The physicists, the agricultural engineers have not been able to quantify methods for measuring soil structure and yet this is one of the most important properties to agriculture that a soil possesses. Examining the soils that are classified as Xeralfs or Udults, one runs into soils that have no structural problems occasionally but generally there are serious problems of soil structure under cultivation. The soils that do not have serious problems of structure, we first had found in the U. S., always had dark red colors. The original definitions of the rhodic great groups required values moist of 3 or less and hues redder than 5YR. In Tazmania, I was shown soils on a single lava flow from high elevation to sea level, and the hue became less red as the elevation increased but the structural stability of the soils was the same irrespective of hue. The value did not change with elevation. Therefore, in the Ultisols in defining the rhodic great groups, the requirement of the red hue was dropped but the requirement of the low value was retained.

Because, so far as we now know these soils are always developed from basic parent materials such as basalts, limestones, etc. The contents of phosphorus are generally higher in the rhodic great groups than in the others. The use of the color value and the chroma was predicated on the assumption that these features were correlated with the structural problems, with the phosphorus contents, and so on. There were many covarying properties that were extremely important to soil use in the rhodic great groups. No matter where one finds them they are about the most intensively farmed soils of the particular suborder. Rhodic great groups were not set up in Mollisols because there were no particular differences in soil structure with soils that have a mollic epipedon. The formative element 'rhodic' implies red, whereas the actual characteristic used in Ultisols is the color value. This may disturb some people but one must recall that there are rhododendrons that are purple in color.

Question 29

Some soils in the tropics and in Thailand, in particular, have a thin argillic horizon over shale. Because the shale is high in clay, the clay percentage does not decrease with depth. The question is, should these soils be classified as 'pale' great groups.

Guy Smith:

There are pale great groups in several orders, Alfisols, Aridisols, Mollisols, Ultisols are examples, and in each order or suborder the definitions of the pale great groups vary. In the Ultisols, the pale great groups must in addition to the clay distribution lack very many weatherable minerals. In the other groups the definition varies, suborder by suborder but have no relation to the clay distribution alone. In every definition there are characteristics other than the clay distribution and in addition to the clay distribution. In some pale great groups, an abrupt textural change between A and B is used as a part of the definition of the pale great groups. In others a reddish hue or mottles of high chroma are a part of the definition. It would not be intended to group a soil in a pale great group solely on the clay distribution.

Question 30

In *Soil Taxonomy*, a common strategy is to chose a parameter which has morphological expression and is covariant with other properties which are of interest for the classification. A case in point is the use of the presence of secondary carbonates to distinguish ustic and udic subgroups or moisture regimes. This parameter was chosen because it was assumed to have covariant properties wherever it occurred. However, testing has indicated that it is not always covariant and in many landscapes, particularly in non continental landscapes, where loess is noncalcareous, moisture regime changes are not recognized or not accompanied by secondary carbonate depositions. Would you like to comment on that in particular, and also on the flexibility that *Soil Taxonomy* has, to accommodate to that situation?

Guy Smith:

It must be remembered that, while *Soil Taxonomy* was intended to group the soils of the United States, with which we had experience, it was also intended that it should be possible to extend the definitions so that they would be applicable to soils of other countries. In the United States the soils with ustic or xeric moisture regimes are almost always from parent materials that have carbonates or there are carbonates in the dust that falls on the soils. The original taxonomy used in the United States, that of Marbut, divided all the soils at the highest category according to the presence or absence of a horizon of accumulation of calcium carbonate. The emphasis on this horizon has been greatly reduced in the classification of 1938 and in *Soil Taxonomy*. However, the prejudice in favor of using this horizon continues to exist because of its long traditional use in classification. The definitions in which the presence at a given depth according to particle size distribution of a horizon of calcium carbonate accumulation, assumed a relationship between the depth of water penetration into the soil which in turn was correlated with the moisture regime. The limits of depth were selected according to the traditional concepts that the depth to the carbonates varied with the rainfall. These were always in regions in which the rainfall was limited, and genetically the depth to the horizon of accumulation of calcium carbonate was a function of the total rainfall and of the soil temperature.

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

Question 31

The next question is #30 in those compiled by Professor Rust and asks why was the argillic horizon clay increase established as a 20 percent relative increase compared to the overlying eluvial horizon?

<u>Guy Smith:</u>

The French Taxonomy uses an increase of 40 percent as a basis for recognizing different classes, particularly the sol lessivé. Amongst the Mollisols, the existence of an argillic horizon is rather widespread and marks the break between the late Pleistocene Mollisols and the Holocene Mollisols. In these soils the break between the eluvial and illuvial horizons is at about an increase of 20 percent in clay. This is actually the minimum limit in the Mollisols at which we thought the field man could identify the change in the particle size distribution or texture. Therefore, in *Soil Taxonomy* we took the absolute relative increase of 20 percent from the Mollisols as our minimum for recognition of an argillic horizon. In Alfisols and Ultisols, the normal situation is that the increase is 40 percent or more. It must be remembered that this increase of 20 percent is applied only to soils having clay contents ranging between 20 and 40 percent in the eluvial horizon. The 20 percent increase in a soil which has 20 percent clay means the field man must distinguish between 20 and 25 percent clay, with his fingers. We desired to have definitions that could be applied in the field without referring samples to the laboratory.

Question 32

The next question is #31 in those compiled by Professor Rust. Where does the criterion of 1 percent or more of oriented clay in the argillic come from? We think it is very difficult to follow the criteria with reasonable precision because the statistics of point counting in the microscope, show that we need about 3,500 counts for each thin section to be 95 percent sure in a 10 percent error.

Guy Smith:

One must first remember that the limit is 1 percent of the oriented clays not 1.0. A 10 percent error therefore is not only permissible but probably expected. If the point count shows 1/2 percent or more, the rounding of numbers will bring it to the necessary 1 percent.

The assumption was, in this limit that if the clay-skins could be identified in the field there would be a least 1 percent in thin sections. This assumption may not have been justified and it was proposed for testing but that no one had ever criticized it when it came time to print *Soil Taxonomy.*

Question 33

The next question, I would like to ask is: I believe it is a widely held assumption of practicing pedologists anyway, that both the presence of clay skins and a clay bulge are required for an argillic horizon in most cases. I understand now from talking with you that the emphasis on this is not quite correct.

Guy Smith:

Surely, no clay increase can be required in soils that have been truncated or in soils in which there is a lithologic discontinuity giving rise to an argillic horizon even with less clay then in the surface mantle. In these cases where there is no increase in clay between the argillic horizon and any overlying horizon, such as a plowlayer, we do require 1 percent cutans. This limit of 1 percent was set so low that if the pedologist in the field would identify some he surely would have the necessary number of clay skins in the soil.

If the clays have 2:1 lattice minerals, the argillic horizon does not need to have clay skins, if there are skeletons in an overlying horizon. It is pointed out in the discussion, rather than the summary, that in some places particularly those with wet/dry seasons, the special field studies are needed more than laboratory studies to identify the presence of the horizon with illuvial clay. If the polypedon has a range in elevation and if the boundary between the surface and the heavier textured underlying horizon is clear or abrupt, it may be necessary to trace the finer textured horizon laterally to be sure that it is not a depositional feature. If the increase in clay is marked enough to be observable, and if the boundary is clear or abrupt it is extremely difficult to assign the origin of the finer textured horizon to differential weathering. Moisture conditions in the soil do not change abruptly but rather gradually. But in the discussion rather than in the summary it is pointed out that the field studies alone can realistically indicate the illuvial nature of the finer textured horizon.

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

Question 34

The next question is number 32, compiled by Professor Rust, and it asks, what is the reasoning for using the illuvial horizon (or surface) as a reference for the required increase in clay for the argillic horizon, instead of the underlying horizon? In other types of pedogenic horizons, such as calcic, gypsic etc. the C horizon is used as, at least partially, as a reference point. It seems to me that in soils that are truncated, the C horizon would be an acceptable reference point. Likewise, the C horizon could be a reasonable reference point in a soil developed in a relatively uniform parent material in a stable landscape.

Guy Smith:

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

Question 35

The next question is number 28 from those compiled by Professor Rust and asks how did you arrive at the general concept of diagnostics horizons.

Guy Smith:

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

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

Question 36

The next question is number 53 from those compiled by Professor Rust and asks why is 12 kilograms of organic carbon used to separate humic great groups.

Guy Smith:

There is only one great group, Humitropept, where 12 kilograms is used as a limit to separate the great group from others in the same suborders. There are some suborders, as in the case of Humult, where 12 kilograms of carbon is used. There are other humic suborders such as Humox in which the limit is not 12 but 16 kilos of carbon. The reason is again the same as in other questions that the groupings that were achieved by using these limits seem to permit more statements about the taxa that were formed and more accurate interpretations. We tested limits of 20 kilos of carbon, 16 kilos of carbon, 12 kilos of carbon, and no one limit seemed to be useful in all of the different orders. The 12 kilos of carbon per cubic meter seem to give the best groupings for Ultisols and Inceptisols but a higher limit of 16 kg seem best for Oxisols. In every case, the limit was not strictly on carbon but also involved the temperature regime of the soil.

Question 37

In many cool temperate forested regions, litter accumulates to considerable depth beneath the forest trees. The question asks, how to distinguish between an O horizon and a Histosol when you have litter to depth of greater than 60 cm. It doesn't seem as if it is appropriate to classify such a soil as a Histosol but it is not clear how you avoid it.

Guy Smith:

You avoid it by changing the definitions in *Soil Taxonomy*. Nobody is perfect, and there seems to be some confusion in the definition of organic and inorganic soils. I know that under the Kauri trees in New Zealand the O horizon can be more than a meter thick near the tree, getting a few meters away from the trunk of the tree, the litter becomes much shallower. Nevertheless, one does not want to have a complex of Histosols and mineral soils with the limit being a small circle around the trunk of the existing tree. The definition in *Soil Taxonomy*, however, requires that the soil be classified as a Histosol if the 0 horizon is more than 60 cm thick. Some clarification in the next addition of *Soil Taxonomy* seems to be necessary and as a general rule, when one runs into a situation of this sort where the soil is obviously misclassified, some comments should be made to the Soil Conservation Service so that they will be aware of the deficiencies in the current edition.

Question 38

The next question concerns the fragipans and asks specifically why or what is the genetic reason for the condition in the description, that fragipan has an illuvial horizon. Comment is made that not many New Zealand fragipans have an illuvial horizon. The question further asks to comment on whether or not there should be a fragic diagnostic horizon which would be one that does quite meet the fragipan definition - nothing particularly with regard to root spacing.

Guy Smith:

The reason for the statement that there is a illuvial horizon above a fragipan, unless the soil has been truncated, is simply that, in the experience that I have had, such a horizon always exists. Because we do not understand very well the genesis of fragipan there is no genetic reason to this. It is only a matter of general observation. It is very likely, now, that I have had an opportunity to look at fragipans at some length in New Zealand that the illuvial horizon that I described is one in which ferrolysis has been an important factor, ferrolysis being the destruction of clay under alternating wet and dry conditions. There are enough problems in identification of a fragipan that we eventually decided in the U.S. that it was an either/or situation, that the soil had a fragipan or it did not have a fragipan. But we did provide for intergrades where the brittleness was observable in an appreciable part of the fabric of the horizon but the roots were present at intervals of less than 10 cm. For example page 129 *Soil Taxonomy*, we provide for a Fragic Glossudalf. They are like the typic except that they do not have a brittle matrix. It is much simpler to define these subgroups on the percentage of the matrix that is brittle when moist, then it is to establish a new diagnostic horizon that is somewhere between a fragipan and no fragipan.

Question 39

The next question comes from Thailand and concerns the particle size class at the family level and the question is why do we have to use the weighted average of the control section for soils containing coarse fragments (2 mm in diameter or larger). A thin and dense band of gravel such as ironstone can make root penetration impossible even though the weighted average of the iron stone is less than 35%

Guy Smith:

It seems probable that whoever asks this question is concerned with a diagnostic horizon that has not yet been defined or named. While I have seen many soils in the tropical region that contain considerably more than 35% ironstone, I have never seen a horizon that was important to the penetration of roots or water. I have had proposals for a diagnostic horizon containing ironstone but the proposals were not acceptable because they will transfer any stonelines in tropical soils into a diagnostic horizon. In other parts of the world, stonelines are not recognized as diagnostic properties. *Soil Taxonomy* gives considerable weight to horizons that interfere with the penetration of roots. If such horizons exist in the tropical regions there should be some proposals for a new diagnostic horizon. In my travels, no one has ever concerned themselves with showing me such a horizon, so I assume that it is not an important property. This is an assumption and it may not be a valid one for interpretations of soil on a particular plantation or a particular tract of land that someone wants to cultivate. *Soil Taxonomy* does not concern itself with areal extent. We are concerned with the interpretations that must be made for a particular tract of land whether or not this a common or extensive situation or whether it is rare. The legend for the soil map of the world by FAO and UNESCO, is one that of necessity depends on areal extent. Soils that are very extensive in the world can be shown. Occasional soils of small extent cannot be shown in their presence while important. A particular tract of land cannot be indicated in the legend because in the world as a whole they are exceptional even though the properties are extremely important on that particular piece of land. Therefore if there are such horizons with ironstone that are impenetrable by roots someone would suggest a new diagnostic horizon just as I have, since retirement, suggested the densipan and the lithoplinthic horizons. There are certainly other horizons that should appear in taxonomy that have not been suggested.

Question 40

I would like now to discuss with you generally the use of climatic parameters in *Soil Taxonomy* and perhaps we can start by using one of the questions compiled by Professor Rust (number 2 in his list) which states why were temperature and moisture chosen as differentiating criteria when in fact they would seem to be external factors to the soil.

Guy Smith:

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

Question 41

Soil temperature and moisture parameters are not always used at the same categoric level in all orders and notably in Inceptisols and Entisols, moisture regimes are not used any higher than the great group level; in many of the other orders, moisture regimes are used at suborder level. Could you comment on the reasons behind this?

Guy Smith:

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

Question 42

The next question is, how did you come to develop a concept of isotemperature and what do you think the significance of isotemperature is to the *Soil Taxonomy*.

Guy Smith:

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

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

From the point of view of soil genesis the soils whose growing seasons are controlled by temperature, have, in the fall of the year a cessation of plant growth and one has a flush of new foliage on the surface of the soil. The leaves of trees, the dry grasses, and so on. the crop residues, all provide large amounts of fresh organic matter at the soil surface. In the humid parts of the inter tropical regions where there is no dry season and no control on the growing season by moisture or temperature, there are no large flushes of fresh organic matter. One finds instead that the leaves drop at any month of the year in small numbers and there is a continuous accretion of organic litter at the surface but no large flush of new organic matter at the surface. In the inter tropical regions where the growing season is controlled by moisture, the plants stop growing when the rains stop and the leaves fall, the grasses die, and there is little difference in the flush of fresh organic litter between the tropics and inter tropical regions. Therefore the tropic great groups are all defined as having a udic moisture regime rather than an ustic moisture regime. There seems to be a difference in the genetic affects of a large amount of organic matter coming over a short period and the same amount of organic matter coming over a full year. One sees differences between the soils of the humid tropics and the humid temperate regions that can hardly be explained other than on the basis of the key leading effects of large amounts of soluble organic materials coming within a short time and the same amounts coming very evenly spaced over the year.

Question 43

You have often said to me and I have often heard you state in meetings, that taxa are not real things but are concepts. I think a lot of people have continuing difficulty with that statement because it requires a lot of thought and it requires an understanding of how taxonomy was developed. For the benefit for the rest of us here that don't have quite such a clear grasp of that statement, would you like to elaborate for us?

Guy Smith:

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

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

Question 44

In plant and animal taxonomies there has always been a rule of priority of name. In soil classifications in the past, this rule of priority has not been practical because the definitions of taxa have not been sufficiently precise. Now that we do have a *Soil Taxonomy* with precise definitions, should there be a rule of priority in terms of the names of taxa?

Guy Smith:

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

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

Question 45

The next question is a common one among pedologists working on floodplains with Entisols and Inceptisols and concerns the difficulty of distinguishing a cambic horizon where the soil has an aquic moisture regime. The question is directed both from Thailand and from New Zealand and asks if there is any better method of distinguishing the degree of alteration for soils having a aquic moisture regime.

Guy Smith:

While in New Zealand, you made a proposal based on New Zealand experience for an additional criterion for identifying such a cambic horizon. The proposed change reads: *In Soil Taxonomy, page 34, left column, starting on the 9th line from the bottom where it reads, "One evidence of alteration" delete the balance of the paragraph and substitute the following column. "One evidence of pedogenic alteration is the formation of iron or iron manganese concretions of sand or coarser size. Although mottles of low chroma may develop before the flood waters that deposit the alluvium have receded, concretions of iron or iron manganese forms slowly. But, mottles can grade into concretions, and some limit must be set between them. A concretion, to be diagnostic of alteration, must be cemented to the point that it does not break down under normal particle size dispersion methods, and can be separated by screening. One concretion in 10 grams of soil would not seem significant in a poorly drained soil, but if something like 1/4 percent of the soil, by weight, consists of concretions, the evidence of alteration adequate for a cambic horizon is satisfied. Concretions may be distinguished from dark colored mineral grains by their fragility under a gentle pressure, the presence of* MnO_2*, or magnetic properties that change after ignition."*

Change item 4.a.1., page 36, left column, to read: "Have at least 1/4 percent by weight of iron or iron manganese concretions of sand size or larger within the surface 50 cm that do not disperse on overnight shaking in hexametaphosphate.

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