CONVERSATIONS IN SOIL TAXONOMY
(ORIGINAL TRANSCRIPTIONS OF TAPE CONVERSATIONS)

by

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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 Service (SMSS), established in October 1979, to continue this effort.

In 1980 and 1981, SMSS arranged a series of interviews at the University of Ghent, Belgium, Cornell University, University of Minnesota, Texas A&M University, and with the Soil Conservation Service (SCS). Dr. Smith also travelled to Venezuela and Trinidad and was interviewed by colleagues at institutions in these countries.

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

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

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

Dr. M. Leamy (New Zealand); Dr. R. Tavernier (Belgium); Dr. R. Rust (Minnesota); Dr. B. Allen (Texas); Dr. A. Van Wambeke and Dr. M. G. Cline (Cornell); Dr. L. Wilding (Texas); Dr. J. Comerma (Venezuela), and Dr. N. Ahmad (Trinidad). Staff of the Soil Conservation Service, particularly Dr. R. Arnold, R. Guthrie (formerly SCS) and J. Witty (Washington, D.C.); J. Nichols (Texas); S. Riegen (Alaska) and F. Gilbert (New York) also contributed to the interviews.
Dr. H. Eswaran put an extraordinary amount of work in transcribing a large set of original tapes. These were at a later stage compiled, edited and indexed by Dr. T. Forbes, who also coordinated the final publishing.

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

December 1980

Lubbock, Texas
Allen:

Do you have any opening statements or comments?

Guy Smith:

I don't have any particularly startling comments to make. This is the 3rd meeting since the AID people and Div. 5 of SSSA decided to prepare a book about the rationale of Soil Taxonomy. For the most part I rather carefully concealed the reasons for doing this and that, when I wrote Soil Taxonomy for the simple reason that if I had explained why we did, this or that, the reader would be more apt to pay attention to the reason than to the actual definition. We wanted a test of the definitions not of the reasons. The reasons, of course, are going to be very highly disputed by people who have other backgrounds than those of us who developed Soil Taxonomy. It seemed best if we wanted to test the definitions the reasons had better be kept out of the book, for the most part.

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

Professor Rust prepared a list of questions and Dr. Cline prepared another list of questions. The questions from Dr. Cline have been answered but we have as yet no transcript of those conversations. They only ended the 22nd of December. I will go from here to the University of Minnesota to a meeting that will be somewhat similar to this, Lubbock, Texas, and from there I am proposing to go to Venezuela for interviews on the soils of intertropical regions. I had hoped that at this group by coming to Lubbock I could introduce into these questions those things that were bothering people familiar with the semi-arid and arid regions of the U.S. People at Cornell and the people at Washington, D.C. probably would never dream of asking the questions that I hope will come up here. After Venezuela, I will go to Trinidad to see what questions exist now in the West Indies. I finished the interviews with Dr. Leamy from New Zealand. This will then give me, I hope, a good cross section of questions. There will be many duplications. I will have to sort the questions according to subject matter and consolidate some of the answers.

Allen:

I think if there are times that if questions are asked here and you know that they have already been asked at Cornell, for example, if you prefer not to dwell on that you can just say so.
Question 1

Wilding:

In the construction of *Soil Taxonomy*, the system was built from the base up and in sort of an ascending manner, as well as from the top down in a descending manner. It is my understanding that the family category was perhaps the last constructed as the link between subgroups and series. What was the rationale in constructing the family category and criteria used for the family classes?

Guy Smith:

This answer requires going back a long way in time, and I propose to put it into an introduction to the whole book. Starting in 1900, approximately, we began to build up a group of soil series which were defined with varying rigor at varying periods of time. But these soil series and types were the basis for the published soil surveys, and they had a good deal of actual testing in the field. People became familiar with them, and they used them. In Iowa, farms advertised for sale in the newspapers generally said 160 acres of Carrington loam. The tax assessors used the soil series and types, the highway engineers used the soil series and types, and they became familiar with them, and they well established their utility. At the same general period of time, beginning about 1920 in this case, Marbut introduced the concept of the Russian soil type, or as it became known here, the great soil group. Marbut's final publication, the Atlas of American Agriculture, gave his great soil groups that he recognized at that time and gave an example of 1 or 2 series for each of his great soil groups, but he was never able to arrange his series into the great soil groups. The great soil groups were continued in the 1938 yearbook of agriculture, Soils and Men. Dr. Kellogg has often explained to me the problems they faced, that they had only one year to devise a new system, because they recognized the imperfections of Marbut's system and they could not be made to work. In that one year, they devised a number of descriptions of great soil groups, including summary arrangement of Marbut's great soil groups into suborders and orders. So we had, beginning about 1920 and running up until World War II, two systems of classification of soils. One was into soil series and types and the other was into great soil groups. These two systems have never been meshed. They were completely separate, one from the other. At the end of WWII, Kellogg put his senior staff to work to develop better definitions of the great soil groups. He had a committee on Red Yellow Podzolic soils and one on Gray Brown Podzolic soils and one on Planosols. Members of these committees were almost invariably people who had worked with these particular kinds of soils. They tended to write some rather narrow definitions, so that for the most part, there were large gaps between the definitions of one great soil group and another that was bordering it. The 1938 classification also had the defect that at the highest category soils were grouped into three classes, zonal, azonal, and intrazonal. We could not discover any common characteristic of the zonal soils that were not also shared by any of the intrazonal soils. We decided that it would be best to abandon the concept of zonality as a differentiation in the taxonomy of our soils and find something to take its place. We had quite a few thousands of series at this moment -- the exact number escapes me -- but it was would be something like 5 or 6 thousand. It was too many series for anyone to comprehend. While there were long arguments about the importance of grouping the series into successively higher categories, no one knew the series well enough to do this. It was necessary, then, to find some sort of a differentiae or some groups of differentiae, for the higher categories, and to test those by seeing how the series fell in the definitions that had been proposed. We had the link between the great soil group and the series that was missing. We had no criteria in mind when we started to arrange the categories between the series and the great soil group. But we had been having discussions for many years about the intergrades between one great soil group and another -- soils that shared some characteristics of another or several other great soil groups. This seemed to be a logical basis for defining the subgroups. We still needed a link between the subgroups and the series. It was Dr. Allaway who made the suggestion that, at the subgroup level, we pretty well have taken care of all the genetic factors that concern us, so why don't we, at the family level, take into account the practical physical factors that affect the growth of plants and the engineering use of soils. We tested several concepts, beginning with the *Third*
Approximation. It was the beginning of the Third Approximation that we proposed the use of the physical properties that affected plant growth and engineering uses of soils. We tested a number of definitions by examining the groupings of series that resulted from the use of those definitions. These were modified rather substantially in the fourth and fifth approximations as a result of this testing against the grouping of series. Beginning with the Sixth Approximation, we examined the interpretations that were made for the various phases of all the series that fell into a single family. The assumption was that if we had to make substantially different interpretations for comparable phases of the series in the family, there was something wrong with either the interpretations that we were making or with the definitions that produced those groupings. Basically, the family grouping is intended to permit us to group soils about which we make the same major interpretations for use and management. If we get soils in a family whose comparable phases require substantially different interpretations, we know there is something wrong. A number of such defects have come to life since Soil Taxonomy was published. There is a major problem at the moment about how Soil Taxonomy is going to be revised and kept up to date. That problem is unresolved as yet. There have been to my knowledge no really approved changes in Soil Taxonomy since it was printed, although suggestions have been flowing into Washington from outside the U.S. as well as within.

Question 2

Wilding:

Do you believe that there is now an adequate international representation through various committees looking at revisions of Soil Taxonomy, that the system will truly evolve as the acceptable international system of soil classification, and how does the international emphasis affect the category definitions that were based primarily on the knowledge of soils in the U.S.?

Guy Smith:

Under the stimulation of AID, which is attempting in one of its major functions, to increase food production in the developing countries, the Soil Conservation Service (SCS) has established a number of international committees to examine the function of Soil Taxonomy, particularly in intertropical regions. It was impossible to spend much time in the study of these soils when Soil Taxonomy was being developed because the appropriations to the department of agriculture are exclusively for the benefit of the American people. And studies of soils in the developing countries were intended to be for their benefit, not that of the U.S. We could not say that we were going to learn a great deal that could be applied to the soils of the U.S. by working with the people in Kenya or in Zaire or Uganda. We did examine the European soils rather carefully, and the European systems of classification on the basis that these were advanced countries that soil science has started there, that we could probably learn considerably from their experience with the European soils, and that we could transfer their experience to the U.S. if we had a system that was based on the soils of both continents. The first of the international committees established under the chairmanship of Dr. Frank Moormann concerned the classification of soils with low-activity clays. They've been working now about 6 or 7 years on the classification of these soils which are extensive in Africa and South America, and much less so in the U.S., although they exist in the south eastern states. Most of the work with soil management in the U.S. concerned the soils of the glaciated regions of the U.S. There was relatively little work done with the soils of the southeastern humid, warm regions. The bias in Soil Taxonomy is strictly in favor of the soils that occur in western Europe where the last glaciation, Wurm II, disturbed virtually all the soils and left us with completely new surfaces to weather, and with similar soils in the northern half of the United States. The committees have quite good international representation. The committee on the classification of soils from volcanic ash had about 75 people who indicated an interest in this subject. They came from
virtually all parts of the world because the volcanoes don’t much care where they erupt. The work is slow. There is much dissension among the committees; there are always on each committee several people who want to scrap Soil Taxonomy completely and develop their own system. This is not in the mandate that has been given to the committees. They are supposed to suggest improvements with the minimum of disturbance to the structure of the system, though in no case is there ever going to be any unanimous agreement on anything. The report chairman of the committees are going to be faced with the problem that I had in the development of Soil Taxonomy, that there was sometimes a consensus of agreement, but there were always vigorous objectors. How far Soil Taxonomy can be improved to make it an international system I will not yet predict. I think that the functioning of these committees is going to go a long way towards gaining acceptance for Soil Taxonomy in the developing countries. It is not going to pacify the Russian pedologists who are attempting now to develop an international system, one that, they say is truly international, under the auspices of FAO. How far they will go, I do not know, but the Russians at the first meeting to develop this international system, went along way toward accepting some of the basic principles of Soil Taxonomy that they resisted violently at the time of the International Congress in Bucharest. They have accepted now the use of diagnostic horizons and features as a basis for the new system, and it is very likely that anything that is developed will be compatible with Soil Taxonomy, so that it will be possible to compare Soil Taxonomy with whatever sort of system they eventually develop. They have had a distinct impact on the classification of soils in the more developed countries, where they have their own system of classification, as in France, Germany, Canada, Holland, New Zealand, Brazil, and so on. The classification is being reexamined in most of these countries, but not yet all of them. The previous classifications in France and Germany have been pretty much abandoned, and they are working now on the development of new systems which will probably be compatible, or more nearly compatible with Soil Taxonomy than the older systems.

Question 3

Hallmark:

You responded in a brief manner to the following question, but I would like to give you an opportunity to respond in a little more detail. Soil Taxonomy recognizes the need for changes in classifications as new knowledge is discovered. However, in the five years since Soil Taxonomy was released in the hardback, we have seen very few changes. Do you think that Soil Taxonomy is too hard to change under the present vehicle, and do you have any ideas on speeding this process along?

Guy Smith:

To the best of my knowledge, I have seen no approved changes for Soil Taxonomy, although I did see a document that said certain changes had been approved. The present feeling in the Soil Conservation Service is that approval was premature. The SCS is reexamining everything that was listed as approved. After some years of debate among a considerable number of people, the international committees, perhaps, offer one major route to make changes. I think that they will come out with well-reasoned proposals for changes. It is not easy to suggest how other changes should be made. There are small problems that probably don’t warrant an international committee. The Soil Conservation Service at one time had Dr. McClelland as the director of Soil Survey Operations, Classification and Correlation. That was a serious overload for any one man; he simply could not give proper attention to any part of that work. They now have three positions to cover that: operations, one position; classification, one position; and correlation, one position. And to the best of my knowledge these are all vacant. The recent changes in civil service regulations make it very difficult to recruit people to move
to Washington. How they are going to resolve this problem, I have no idea, but it is a very serious problem. Dr. Arnold is very well aware of its importance, but his hands are tied a little bit by Civil Service regulations, and by federal law. I could give one example of a change that is needed that probably doesn't require an international committee: the definition of Inceptisols excludes soils with a conductivity of 2mm or more within certain defined depth limits. We see over and over again on one continent or another, that if soils that have a relatively low precipitation are irrigated, the conductivity increases, and then an Inceptisol becomes an Aridisol. You have this in Texas in the lower Rio Grande. These Inceptisols that can be used for dry farming are suddenly grouped with Aridisols when they're irrigated. The major thing we want to say about the Aridisols is that they're too dry to cultivate without irrigation. Suddenly we find we can't even say that about Aridisols without changing the definition. This change is so obviously needed, I don't understand why it hasn't been made, except that they're tied up in vacancies in Washington. I don't think an international committee is needed for problems like that. I've been working since I retired, first in the West Indies, in Venezuela, then in New Zealand, and I have page after page of minor changes that are obviously needed. The problem is how to get these approved and to get them into circulation so that the pedologists over the world can know what changes are approved. It was proposed at one time to publish these approved changes in the Soil Science Society of America Journal. That is also under reexamination. They're planning now to publish them in the Soils Handbook. The Soils Handbook is not generally available around the world. This will not solve the problem. Although it will be useful to the soil surveyors in the United States, it will be of limited use in any other country.

Question 4

Allen:

You mentioned the Russian system as being somewhat altered to make it international in scope. How is that going to fit into the existing FAO system. What is going to be the relationship there?

Guy Smith:

It seemed obvious at the meeting in Sofia, Bulgaria that the Russians are prepared to abandon their old classification. They were represented there by three people: Professor Gerasimov, Professor Kavda, and Dr. Friedland. They would like to use the present legend for the FAO and UNESCO Soil Map of the World as a basis for devising a new system of classification with at least 4 categories. They accepted the use of diagnostic horizons that they attacked so bitterly at the time of the International Congress in Bucharest. They accepted the use of soil moisture and soil temperature rather than climate as a basis for definitions. It would appear that what they have in mind is to extend the map units for the Soil Map of the World into what amounts to a classification system. As it now stands, the legend specifically says this is a legend, not a classification system, although they turn around and behave as though it were a classification with 2 categories. Now they want to extend this to 4 categories, but the basis for the definitions in the legend of the Soil Map of the World are copied very nearly exactly from Soil Taxonomy, with one exception. They objected to the use of soil moisture and soil temperature in the taxonomy. They (the Russians) violated this principle in only one situation, where they had soils of arid regions that had the same profile characteristics as soils in Mediterranean climates. The uses of the two soils were entirely different. In this one situation, they brought in the soil moisture regime. Extending that legend [FAO] and adding some categories will probably still result in a system that is compatible with Soil Taxonomy, especially now that everyone at the meeting in Sofia, with the exception of one man from FAO, everyone accepted the use of moisture and temperature as soil properties.
Question 5

Sortmann:

One of the things that intrigued me is the different temperature regimes. How will the temperature regimes at the family level carry over if you try to go to a truly international system? Also in line with that, how did you come about those particular criteria or temperature ranges for that?

The basic fear that people had of the new system was that it was going to split a lot of series that had been in use, were well known, and had been tested. Every definition was examined to see whether or not it split series in a wholesale manner. If so, what splits were good, in that they permitted better interpretations and splits that were bad, in that they merely made more series to keep track of. It so happens that at the time we began to develop Soil Taxonomy there was more or less a rule of thumb in soil correlation that a series should not be carried very far across a major land use boundary. In other words, if we went out of the cotton belt into the corn belt, the series virtually all changed. It was quite possible to select limits for temperature that did not split very many series. The limit between the cotton and the corn belt works out at 15 degrees C mean annual temperature. The northern limit of the corn belt, the limit between winter wheat and spring wheat worked out at 8 degrees C. The series all changed at these temperature limits. They may not work as well in other countries, but this is something that needs to be tested. In Venezuela they are reexamining the limit between the isohyperthermic and isothermic temperature regimes. They are tentatively proposing a split in the present isohyperthermic temperatures. We have an international committee that is working on this particular problem of temperature and moisture in these areas. We won't know how these temperature limits will work out until we've had a chance to actually test them in these other countries mostly through the international committees.

Question 6

Sortmann:

How much actual data is there on these temperature regimes? For example, what is the percentage of soil series that have data on soil temperature regimes?

Guy Smith:

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

Dr. Smith, this is Leroy Daugherty. One of the biggest problems we have in soil survey in the west is with the use of and recognition of soil moisture regimes. Do orographic influences affect soil moisture? We can vary from an aridic moisture regime to an udic moisture regime within 25 miles. Would you discuss the rationale of the moisture regimes in the transitional subgroups, especially as they apply to the west?

Guy Smith:

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

Question 8

Wilding:

The pedon concept is under reexamination, and especially the cyclic pedon that provides for variable space considerations of, say, 1 to 10 square meters. For example, in Vertisols, what can be accomplished with a cyclic pedon that could not be accomplished with a complex of soil series, if the pedon were decreased to a constant size of approximately 1 square meter? How does oscillatory, but not cyclic changes in soils fit with the pedon concept? For example, changes in solum thickness in soils derived from limestone residuum or pimple mounds on young Pleistocene surfaces of the coastal plains which are not cyclic but oscillatory.

Guy Smith:

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

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

Question 9

Wilding:

In terms of record keeping on the series with the cyclic pedon, we very often as the case with Vertisols, need to sample different parts of the pedon to define the pedon. A concept of

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sampling is one of the considerations in revising a pedon to possibly a constant area. We still have the problem of record-keeping in definition, even though we would include cyclic concepts in the pedon.

Guy Smith:

I might comment that in other parts of the world, they would like the pedon to be larger than 10 square meters. They would prefer to have it more nearly the area under the canopy of the larger natural species that grew there. In Zaire you can have one tree that collects sulphur growing next to a tree that collects calcium, and the base saturation characteristics under these two trees are very unlike.

Question 10

Wilding:

In the establishment of the rationale of the pedon, oscillatory changes as well as cyclic changes were a part of that original concept?

Guy Smith:

This has been answered before, whether we’re classifying pedons or polypedons, and why. I’ll simply say that the pedon does not have all the properties of the polypedon, which is what Soil Taxonomy says was the actual body being classified. The polypedon has shape, it has transitions to other polypedons which are natural boundaries, whereas the boundaries of the pedon is pretty much arbitrary, depending on where you start your measurements.

Question 11

Hallmark:

Soil Taxonomy has been criticized often by soil chemists for our use of cation exchange criteria determined in buffered solutions, particularly when we make calculations of base saturation. Should we begin shifting to effective cation exchange data as a base for division of classes? For example, the mollic versus the umbric epipedon and the Ultisols versus the Alfisols division.

Guy Smith:

There is no question but that some changes are coming in this direction. The International Committees on the classification on soils with low-activity clays, on the Oxisols, on the Andepts, are all considering these problems. At the time that we were working on Soil Taxonomy, many of these properties were not well understood, and many of the things the chemists talk about still cannot be measured conveniently. Point of zero charge, for example. There is no reasonable procedure for determining this, that is practical. It is just too expensive to do it on a great number of samples. If you have no data on your soils, you can’t propose a
definition and consider what changes it's going to make, because you don't have the data on the
soils to see how they fall under any proposed definition. While there has been considerable
discussion about using point of zero charge, it's just not possible at this moment. Somebody
may someday devise a reasonable method for estimating it, but to actually measure, so far as I
know, is always going to be very difficult and time-consuming. We don't have the laboratory
money for that sort of thing, particularly in the countries where it is important, the developing
countries. We have the further problem in developing Taxonomy that we were not allowed to
split series. I wanted, at one time, to use CEC, admittedly, buffered at pH7, in some of the
definitions of the soils of the Southeast. But if we did that we split the Ruston series in two or
more, because in the Mississippi valley the CEC is influenced by a bit of montmorillonite dust
blowing around, and your CEC per 100g clay there is in the neighborhood of 30 meq or more
per 100g clay. The same series on the Atlantic Coastal Plain runs about 6 meq. Now, the
correlators would not agree to split those series, and it couldn't be done without their approval.
We have Prof. Buol who has been bringing this up at the Southern Regional Work Planning
Conferences year after year, and he may get it through in a couple more years, that the Ruston
and Norfolk series should be split, because their management requirements are conditioned by
the activity in the clay. The use of the sum of bases plus KCl extractable aluminum is a
potential substitute for CEC by ammonium acetate or by sum of cations. That has been used to
some small extent in Soil Taxonomy, particularly with Oxisols. The three International
Committees that are examining these problems include a number of chemists, as well as field
men. They are corresponding with each other and precisely what they will finally come up
with is unpredictable to me.

Question 12

Nichols:

Could we go back to the definition of a series for a moment, that you alluded to before.
The decision was made to include the entire series range within the limits imposed by the
higher categories. Was this done early in the Taxonomy, and was there an argument for,
perhaps, allowing some of the range of characteristics to go outside the boundaries of the
categories?

Guy Smith:

There was a lengthy argument about this. I would refer you to Professor Cline's
publication on Soil Classification in the United States, where he discusses the logic of
classification. At the time that we were trying to develop our definitions, there were two more
or less contrasting points of view about the range of a series. For the purposes of correlation in
one regional technical service center versus another, the ideal definition is one that gives the
limits of the class, because you can observe those. It is not something that you apply
subjectively. If, on the other hand, you take the point of view that a taxon is something that
should be bound from within, rather than circumscribed from without, then the judgement of
the correlator in one state or one service center may be quite different from the judgement of
another. I must remind you that one of the basic problems that we had to resolve with Soil
Taxonomy was the correlation backlog. We could never get more than about 30 countries
correlated in any one year. We had built up a backlog of unpublished soil surveys of 10 years
or more. We had to decentralize the correlation process, but we had to keep it under reasonable
control, in that we do at Fort Worth and what they do at Lincoln will not be diametrically
opposed. There seemed to be no reasonable solution to this backlog of unpublished surveys,
unpublished because they couldn't be correlated, except to decentralize the correlation process to
the states and the technical service centers. The only way the correlation process could be
controlled was by means of the definition in terms of limits. If a soil exceeds a proposed series,
exceeds the limits of some higher category, then you have 3 possibilities: 1) one is to have a
new series, 2) to recognize a taxadjunct, or 3) to modify the definition so that in one
combination of circumstances you have one limit, and in another combination of circumstances
the limits may vary. This is one of the reasons that we have so many complaints about the
complicated definitions. That we have kinds of soil that straddle one of the limits in some
higher category. And they may not deviate much from that limit, but they are on both sides.
We have tried in some places to keep these natural groups together, as in the Glossudalfs, where
the base saturation at the critical depth runs between about 30 and 40 percent. It's never much
above the limit between Alfisols and Ultisols, and never much below, so we've got some
paragraphs in the definitions that make them difficult to understand, but that keep these natural
groups together by allowing the variations in one property if it is accompanied by a variation in
another.

Question 13

Nichols:

There are not too many of those situations, as the one you mention, where, if you have
mixed mineralogy, you can allow the base saturation to vary. Do you think that using more of
that convention would perhaps get rid of some of the arguments that we have in getting
correlation and classification into closer alignment?

Guy Smith:

Well, I would favor more of that if it can be managed. As a general rule, these
complicated definitions are that way because of a very few soils. They do concern someone
who is classifying the soils; they don’t concern anyone who is using the classification. I think
these definitions could be greatly simplified for people who are using the classification. I see
no good way to simplify it for the people who are doing the classifying.

Question 14

Nichols:

Did interpretations, then play quite an important part in the designing of Taxonomy, so
that the taxonomy would fit the use made of the soils?

Guy Smith:

They were the major control. The major control at the family level and the series. We
would like to have as many interpretations as possible for each taxon. While we can make some
statements about Vertisols and Alfisols, there is no statement we can make about Entisols. That
is a taxon about which you can say nothing of any importance, just that they don’t have
horizons, and what does that mean? Nothing.
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Question 15

Nichols:

I have wondered about the cyclic soil concept that Dr. Wilding mentioned a while ago, I never knew how much interpretations entered into that, but I suspected that the cyclic soil limits were set so that if a soil was split into two soils, you would still have to interpret them as one, such as a house would occupy both of the soils in the cycle and a large tree would occupy both of the cycles. Was that, in fact, a consideration on the size of the cyclic pedon?

Guy Smith:

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

Question 16

Allen:

You alluded to interpretation at the family category, and I've been aware of this all along, that it seemed to be the major consideration for development of families. I was wondering why that particular category was selected. What is the background?

Guy Smith:

We had to bring together the series classification and great group classification. That was one thing that was necessary for the correlation process. We have used the same characteristics at different categoric levels. Temperature limits for the family are smaller than those of the suborder. Again, those are there because of their value for interpretive uses, and we would be, I think, violating the logic of classification if we stayed blindly to the use of one characteristic at the same categoric level with all soils, because the logic of classification says that we should have classes about which you can make the greatest number and most important statements. For the most part, the things that concern us with the soil survey are interpretations. We also have to bring together the soil classification and the capability classification. One was an interpretative classification and the other was taxonomic. You had to go by one additional step of reasoning to get from the taxonomy to the capability. It was about the only test we had of the validity of the way we had grouped our soils. Namely, what could we say about their use and behavior. Those are our important statements in the soil survey. Someone who is concerned with soil chemistry might consider that those are not important statements at all, from his point of view. If he doesn't like them, I think that he has every right to develop his own classification, but it's a major undertaking. One of the chief pedologists in ORSTOM, the French overseas ministry, is developing his own classification at the moment, pretty much following the principles of Fields in New Zealand, according to composition. While we have considered composition in some orders, as in Oxisols, and in some suborders, as in Andepts, we have not given composition a particular place, a particular category in the system. We have
used it to subdivide the soils in such a way that we do get homogeneous groupings in the low
categories.

### Question 17

**Wilding:**

Does the rationale of excluding recent overburdened up to thickness of less than 30 cm (or
sometimes as much as 50 cm) in *Soil Taxonomy* hold for all soil orders (i.e. Vertisols) and for
all overburden materials (i.e. pyroclastics)? For example, consider a 20 cm mantle of pumicitic
ash over a soil which meets all other Vertisol criteria except for the requirement of a 30% clay
content in the surface 18 cm after the soil is mixed. It appears that pyroclastic material are
handled separately as an overburden material because of their unique properties. Would you
please comment on this matter?

**Guy Smith:**

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

### Question 18

**Wilding:**

In the concept of Vertisols, what was the rationale of maintaining the clay content at 30%
all the way to the surface? Was it to avoid a break in hydraulic conductivities so that the soils
would desiccate and crack to the specified dimensions, or was there some other underlying
reason?

**Guy Smith:**

One reason was that we wanted to maintain the cultivated soils and their virgin
counterparts in the same taxon. We didn’t want to change the classification except in unusual
circumstances, as a result of a single plowing or a couple of plowings. There are a few
situations where that can happen, as in some of the soils with very thin natric horizon in arid
regions. If they’re reclaimed by deep plowing to bring gypsum up and get the sodium out, the
natric horizon is destroyed, completely mixed. It's a drastic amount of change in the soil and enough to warrant a change in the classification. We have among the Vertisols, particularly in Australia, and in some parts of the southeastern states, a very thin eluvial horizon, a matter of just a few centimeters. These are cyclical, too. If the soil were plowed, they would disappear completely. There was a lot of discussion about how much percent clay we should have and what CEC we should have. The people who knew the most about the vertisols of the Blacklands wanted to have more than 30% clay and a lower limit on the CEC of the soil. However, this experience was all in the Blacklands of Texas. When we got into other kinds of Vertisols in other parts of the world, the 30% limit seemed to be a reasonable compromise. Having proposed it, it never got criticized. Many features in Soil Taxonomy are there because I made a proposal and nobody ever bothered to criticize it. They'll get around to it someday.

**Question 19**

Gile:

I have another question about young mantles and buried soils. It seems that some of the grossarenic subgroups, for example the Grossarenic Paleustalfs as currently identified by some, consist in part by buried soils. The basic decision in classification is whether thick sandy sediments constitute an epipedon that developed with the underlying argillic horizon, or are younger materials that have buried the argillic horizon. For extensive areas with thick sandy surficial sediments in the high plains, the evidence is clear: the young sediments have buried the underlying argillic horizon. In our 1972 move from the desert project in New Mexico to the high plains of Texas, I noticed that there seemed to be a difference in the way the buried soils were handled, where they are buried by young materials without diagnostic horizons. In the desert project, and I believe in the western states generally, such buried soils are handled by using the rules for classification of Entisols. But about the Texas line and region boundary, this seems to change and for surficial sandy sediments the arenic and grossarenic subgroups are employed, instead of recognizing buried soils. Would it not be better to treat these subgroups as in fact what they really are: relatively thin Ustipsammnts that overly buried soils. Then there would be an expected and natural gradation from thick Ustipsammnts to thin ones as the youthful deposits become thinner in various places.

Guy Smith:

Just that one of the reasons that you were moved to Lubbock, here, was just that problem. This problem exists also in the southeast, where we have arenic and grossarenic Ultisols. In some of them, the break is very obvious in the particle-size distribution of the sand fraction between the epipedon, and the argillic horizon and is a lithologic discontinuity that is very obvious to the project. It would be my feeling that the subsoil should not be in the arenic or grossarenic group. But it also happens in other places that there is no discontinuity, as in the coastal plains geomorphology studies. A doctoral thesis of Erling Gamble examined the sand-size distribution in the Arenic and the Grossarenic Paleudults. While he found there was a very great variability in different parts of his thesis area of Johnston County, North Carolina some were much coarser sand, or much finer sands than others. Still, the sand distribution in the A and the B horizons in every instance was the same. It seemed impossible to figure out how, then, one could get a mantle of sand deposited over this county area in which the recent sand always had the same size distribution as the underlying material. I think these are good evidences that they are legitimate Arenic and Grossarenic Paleudults. I realize that even though we have tried to lay down rules for correlation, that there are or have been differences of opinion between the regional staffs on this particular problem, especially in Florida. Where I have looked at the soils and I find a fine sand that is 1.5 m thick that overlies a sandy clay loam in which the sand is rather coarse, to me, this is buried soil underlying the recent sand.
But just what the correlators have done with these I couldn't say. I know it has been discussed in Washington D.C. what we could do about it, but we don't have the answer in Washington, D.C. for every problem that comes to us.

Question 20

Wilding:

Following that question just a little further, what was the rationale of using an abrupt illuvial contact as evidence of age or pedogenic development intensity in the ustic moisture regime? Further, in the Ustalfs we utilize this abrupt contact as a criterion of the "pale" great groups, but in the Ustults we do not. This seems to be incompatible.

Guy Smith:

The rationale, to start with, was the observation that as the soil climate became drier, with more intense and greater frequency of moisture changes in the soil, we got stronger and stronger development of the argillie horizons. Probably our experience with the old great group of Planosols had something to do with this, because the Planosols with clayey argillic horizons, or claypans have that abrupt boundary, where the climate is udic, marginal to ustic. Where the climate is udic, then the abrupt boundary becomes very tongued and ceases to exist as an abrupt boundary. Now, we made an assumption that this abrupt boundary was an indication of age. It took time to develop it. This assumption may not have been too valid. Recent studies of clay destruction in the presence of an intermittent groundwater table would suggest that we had the wrong basic assumption about the development of the abrupt boundaries on some of these soils. In the Ultisols, we had another group of correlators than we had with Alfisols. I think in general they fix their concepts of Ultisols on soils that did not have an abrupt textural change between the A and the B. In the soils that they showed me in my travels, the Ustults of east central Texas, I did not see this abrupt boundary. Although I did in some of the Ustalfs in east central Texas. It may be that they exist without anyone realizing that I should see it.

That was the intent. To use "pale" for soils with considerable age, and with overly developed or over-thickened horizons of one sort or another. It was not the intent to get a soil of a "pale" great group in Holocene deposits, although we have run into situations where that's what happened. We had a student at the University of Ghent on a doctoral thesis last year. He was working with Holocene deposits where there was an argillic horizon, and where the underlying sediments were fine-textured so that there was no decrease in the percentage of clay with depth. We originally introduced the limit of weatherable minerals with the idea that you would find weatherable minerals in Holocene deposits. This was in the Ultisols rather than the Alfisols. Holocene sediments in Malaysia were all pre-weathered when deposited and had no weatherable minerals. We have made a proposal for [not clear on tape!] over 18 years a new definition at least for the Ultisols. The Alfisols haven't come to my attention in this connection.
Question 21

Wilding:

In vertisols, what was the rationale for establishing the chromic versus pellic great groups? Were these intended to provide an inference for the oxidative state and organic carbon preservation or to separate materials of different parent lithology? It appears in Texas to differentiate both conditions. And how are the definitions on periodicity of cracking patterns in Vertisols established?

Guy Smith:

First the pellic and chromic great groups were intended to distinguish between the Vertisols that could be given surface drainage and those that could not. We had at one time a suborder of Aquerts, and the present international committee on Vertisols is discussing the reintroduction of that group. That was dropped because we had no reasonable basis that I could see to define the wettest of the Vertisols. You can, in a soil of medium or sandy texture, put in a bore hole and measure the groundwater table. But you cannot do that in Vertisols; you don't know where the groundwater is. That suborder was dropped and in its place we substituted the pellic and chromic groups. These are not working well, and this is one of the reasons there is an international committee on Vertisols. I found myself unable to suggest a solution to the misclassification of a number of Vertisols in the West Indies, except by the introduction of slope. Normally slope is reserved to the phase level in most soils in a few aquic great groups, and in some Histosols slope is needed to distinguish between the soils that are wet due to seepage and those that are wet due to low permeability or high rainfall. An entirely different drainage system must be devised where it's due to seepage.

For the second part of your question, we cannot use the soil moisture regime as such in Vertisols because our model doesn't work in a soil that wets from the bottom of the crack as well as the surface of the soil. All that we could do was to predetermine the classification of some of these soils. We did that by keeping the Xeric, Ustic, and Udic great groups together with other Xeric, Ustic, and Udic great groups. With inquiries among people who were familiar with the soil Vertisols, we proposed a definition of cracking periods and cycles. There was never any criticism of the proposed definition.

Question 22

Daugherty:

We have been discussing Vertisols. In the interest of consistency in Soil Taxonomy why wasn't there an attempt to make great groups for Torrerts?

Guy Smith:

We have that in connection with Oxisols, not in connection with Vertisols. In the Vertisols, the variability of the Torrerts that we knew in the U.S. was very small. None of them had low chromas. In part, I think that this is due to the fact that most of our Torrerts are in closed depressions and periodically flood. The water stays for months or even a year or so. In some areas there is very little vegetation on the Torrerts except for some annual weeds. That means there is no energy source for the microorganisms that reduce the iron to give you the gray colors. Because we didn't find any particular variability, within the soils of the Vertisols
of the arid regions, we saw no need for a great group. The suborder was the same as the great group. We could, of course, have put a name on a great group but we would have had only one. There are other places in the system, as in the Rendolls, where we have figured there was no need for another name for another category. We would treat the suborder as a great group.

**Question 23**

**Allen:**

We were already discussing moisture regimes to some extent, but I would like to come back to one question that I have. It has bothered me, and I'm sure it has bothered a number of field men who have worked in west Texas and eastern New Mexico on the boundary between the ustic and aridic moisture regime. This is a very imprecise boundary. I know basically what the concept was, but would you give us a little more background if possible on this boundary.

**Guy Smith:**

We tried, but not always with much success, to introduce properties that are readily recognized in the field, in the soils that are marginal and transitional between one moisture regime and another. That is why we got the limit of 2 mmhos conductivity into the definition of Inceptisols and Aridisols. We assumed, perhaps wrongly, that if we had enough growth of grass to produce a mollic epipedon, that we would then view that as a marker on this boundary, rather than the estimate of soil moisture. The soil that had enough moisture to produce enough grass to give a mollic epipedon, we assumed that we could put it into an ustic regime, or an ustic great group, so that in the definition, I think you will find an ustoll can have an aridic moisture regime. I find it in the limits between the ustic and in the udic we used the presence of secondary lime or soft powdery lime. This did not work at all. When we got into regions of Mollisols where the parent materials were not calcareous we did not find the secondary lime even though the soil was marginal to an Aridisol intergrading to a udic subgroup. This was one of the big problems with classification of soils of the Orinoco valley in Venezuela. They're all noncalcareous, practically. So the udic subgroup of the Ustolls border right on the Aridisols as well as on the Udalfs and the Udults. It made no separation whatever. It was useful. I was told by Dr. McClelland that you have some similar problems in Texas. You have typic, udic, and aridic subgroups all in the same area where there is no known difference except in the depth to lime. I propose that be dropped, and that some other differentiae be used.

**Question 24**

**Wilding:**

Following that question, in some of the ustolic intergrades to Aridisols, a sliding sand-clay ratio has been employed with organic carbon requirements. We know that strongly calcareous sediments appear to be effective in preserving organic carbon against oxidative decomposition many times, even in aridic environments. Hence, in utilizing organic carbon as an index of moisture regime, it would seem to be confounded by the carbonate status of the parent materials. What is the validity, then, of organic carbon as such an index?
Guy Smith:

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

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

Question 25

White:

I've got a problem on a chronosequence in southern California. I think the same sort of problem is going to show up on the Brazos Valley terraces in Texas. The chronosequence specifically starting from the top terrace down is a Xerochrept, Palexeralf, Palexeroll, and a Fluvent. It's my contention that the top terrace is truncated and the argillic horizon is now a surface horizon. I can find nothing in Soil Taxonomy nor talking to people like Dr. Wilding or Dennis Nettleton at the NSSC in Lincoln, who has been on the sites with me. What we do when an Inceptisol is on the top terrace like the example you gave this morning with a Fluvent, Ochrept then back into a Fluvent. You would not expect the Inceptisol to be on the highest terrace over a Palexeralf and a Palexeroll. What do we do in those situations?

Guy Smith:

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

White:

You can still see cutans, clayskins, argillans—whatever you want to call them—in the surface horizon with the naked eye. You don't need any magnification. They are there. It has been enriched or clay illuviation is evident.
Guy Smith:

Then it would be classified, by me, as though it had an argillic horizon.

White:

So, it would be another form of a Xeralf then.

Guy Smith:

Yes.

White:

Instead of a Xerochrept.

Guy Smith:

Correct. You'll find in Soil Taxonomy that no clay increase with depth is required in the soil that has been truncated.

White:

That's the thing that we've looked and looked for and could never find and how to handle that. That's Lenny Lund and Nettleton and a little bit with Wilding but mostly with Lund and Nettleton in California working on that. I think we're going to have the same situation on those Brazos Valley terraces. The top terrace has just remnants left. The soils have formed in the alluvium. They're not formed from bedrock material. It's transported parent material and they're going to be truncated. The little gravel pits on top, of the terraces, Larry. There's not much left of what we would call an A horizon.

Question 26

Wilding:

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

Guy Smith:

I suppose primarily that these differences exist because the definitions were written in different parts of the country. The correlators in the cooler sections of the country are concerned with the low chroma mottles indicating wetness because they shorten the growing season for the plants and delay the period when the soil can be prepared and seeded. In the southern part of the country, where the temperatures are appreciably warmer, the growing season may be shortened but the difference is not critical to the use of the soil and this may be why the correlators in the north central and the northeastern states took the different view about the thickness of the unmottled zone from the southern correlators. I do not know
precisely what was in their minds but they were the ones who proposed these depth limits after considerable discussions among themselves and the state representatives.

Wilding:

So these depths are primarily arbitrary based upon regionality considerations.

Guy Smith:

I think so.

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**Question 27**

Hallmark:

Breaks in lateral continuity are allowed in bedrocks when recognizing lithic or paralithic contacts, for instance, the minimum average horizontal spacing between cracks could be 10 cm or more. The criteria for the petrocalcic horizon requires a continuous cemented or indurated calcic horizon. Both bedrock and the petrocalcic horizon cause rooting and water impediment and various engineering problems. Why is the criterion for petrocalcic horizons with regard to lateral continuity more strict than that for lithic contacts?

Guy Smith:

I was under the impression that we had the same general rule for petrocalcic horizons and duripans about the spacing of cracks that we had for a lithic contact. I do not immediately put my eyes on the sentence in *Soil Taxonomy* that says so. If it doesn't have a statement to that effect, that's an error in the writing of the definition of the petrocalcic horizon because, in practice, we have followed that rule of at least 10 cm between prime roots in fragipans, duripans and petrocalcic horizons and so on.

Guy Smith:

We looked at soils on the High Plains with petrocalcic horizons and discussion centered on what would the average spacing be of the cracks. We were certainly considering distance between cracks for the petrocalcic horizon at that time.

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**Question 28**

Gile:

I have a question also about the lithic contact in cracks and it might be well to bring it up here. My question is about desert soil with bedrock at shallow depths. Some soils on bedrock are not covered by definitions of the lithic and paralithic contact. The bedrock concerned would be an R horizon, in the sense of the 1962 supplement, in being consolidated. Because of
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common cracks based at intervals substantially less than 10 cm, the top of the bedrock does not qualify as a lithic or paralithic contact. How should these soils be handled?

Guy Smith:

Those would be handled as skeletal families. We've looked at those together. It's possible to dig though, because the fractures are both vertical and horizontal.

Gile:

The soils concerned have cracks and the materials are very closely spaced and they dig out very difficultly even with a back hoe. The reason I ask is that I know we have classified these as loamy-skeletal and it's gotten a pretty negative reaction because rock of that hardness that's so difficult to displace should be recognized in the system as something other than sedimentary deposits.

Guy Smith:

We have no way to deal with it at the moment except as a skeletal family of some sort, provided you get roots in the cracks.

Question 29

Wilding:

It seems that the use of calcic horizons as a differentiae between udic and ustic moisture regimes assumes the downward vector of water movement. In udic regimes, calcic horizons under some conditions form by upward capillary water movement of calcium carbonate-charged waters. The carbonates are then precipitated at some depth, either at the surface or close to the surface. For example, in east Texas along the Coast Prairies, Mollisols that have a calcic horizon within 75 cm of the surface are placed as Calciustolls even though they're surrounded by Arguidolls, Haplaquolls and Glossaquolls. Is there some rationale for the use of the calcic horizon that is formed by an upward vector water movement versus that of a downward water vector movement?

Guy Smith:

In some of the Aquolls, the calcic horizon is at the surface. This is clearly upward movement and evaporation. These were at one time called Calcium Carbonate Solonchaks. Where the calcic horizon is at depth, say 50 cm or more, the determination of how that got there is quite subjective and depends on your experience and training and was not considered. I mentioned earlier, this morning, that it was a serious mistake to have used calcium carbonate as a distinction between the udic and the ustic moisture regime because it does not work where the parent materials are noncalcareous. We need something that can be applied more universally. The emphasis on it, of course, goes back to Marbut's distinction between Pedalfers and Pedocals on the basis of presence or absence of free carbonates in the sola.
Question 30

Daugherty:

I'd like to ask you to comment on the concepts of the cambic horizon, in particular, the development of structure in the cambic horizon. Is there a certain grade of structure and type of structure which must develop to qualify for a cambic horizon?

Guy Smith:

No, there is not. We mentioned that it shows soil structure or the absence of rock structure. In particular, it would be the absence of rock structure. Any kind of granular, blocky or prismatic structure of any grade would qualify as soil structure any way as long as it was discernible.

Question 31

Allen:

Along the same line since the cambic horizon has been mentioned, -- in the past week I was in the field and there is a rainfall area of about 10 inches down in the Trans Pecus. I had a very difficult time deciding whether to call the second horizon down an A12 or a B2 (A2 or BW in new horizon nomenclature). In other words, if I went the B2 route I was saying now is this a cambic horizon, or not? I really didn't know which way to go on this. This was over ash and it's about 30 cm and so I think it was very questionable which way to go on this. It could make the difference between an Entisol or an Inceptisol. Would you have any guidelines in this kind of a situation?

Guy Smith:

I'm afraid not. The cambic horizon is supposed to show at least weak expression of the rearrangement of particles in the soil by fauna in the roots of plants and some other evidence normally of weathering either the stronger chroma or redder hues that extend down to more than 25 cm in depth. Where you have the surficial materials lying on an ash, they're very apt to qualify for the color difference just because of the lithologic discontinuity. Without studying the soil in question I surely have no comment to go with that one.

Allen:

I think this question arises quite commonly.

Guy Smith:

No secondary carbonates?

Allen:

Yes, no secondary carbonates, it is calcareous throughout.
Question 32

Calhoun:

Many of the Mollic Vitrardepts in El Salvador were Inceptisols based solely on the presence of a mollic epipedon. On steeper slopes under intensive row-crop cultivation, the mollic epipedon was often severely eroded and these soils were then classified as ashy Ustorthents. Should the Andisol order, if adopted, include ashy (and other combined texture-mineralogy families) soils with only an ochric epipedon?

Guy Smith:

The suborder of Andepts currently presented is based on composition primarily. It has many defects—the definition of the suborder and the classification at great group and subgroup lower levels. From my personal point of view I think I should prefer to keep these soils with an ochric epipedon and nothing else as Entisols with an ashy mineralogy but I think that more than one mind has to be consulted on this, so we have a committee of about 75 that will be arguing about it. I should mention that this proposal arose from my experience in the West Indies on the volcanic islands where I found, even though I had the family classification, I could make no interpretations for bases. When I got to New Zealand, it was primarily to have a look at the soils from ash or pyroclastics in a country where they had studied these soils intensively and there was no language problem. I had the same problems there on interpretations with the proposed classification as Andepts that I had in the West Indies. Dr. Leamy came one day with a problem that they were supposed to meet with the horticulturalists and suggest to them where horticulture could be expanded in New Zealand and with a knowledge of family classification, I could not tell him. I had to inquire and inquire and inquire for additional information before I could suggest that this particular soil might be useful for horticulture.

The skeletal soils where the soil is actually a mixture of pumice with little ash to store rainfall in available form. Now, you can't do this with glacial gravels from granites and we cannot use entirely the geologist's classification of pyroclastics. The andesitic and rhyolitic vesicular ejecta behave the same but only one can be called pumice, the rhyolitic. Dr. Leamy is publishing my proposal in a book that they're issuing in New Zealand for the meetings of next month because he says they're not generally available.

Question 33

Hallmark:

Salinity strongly affects use and management of soils. Why was the presence of soluble salts not given more prominence in Soil Taxonomy?

Guy Smith:

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

Question 34

Thompson:

We have the Natrustalfs which have more than 15 percent exchangeable sodium and have
an argillic horizon. We've also discovered that we have soils with more than 15 percent
exchangeable sodium that lack argillic horizons in other words; they could be some type of
Inceptisols. Is there any reason why there was not a criteria set up for recognizing sodic soils in
Inceptisols?

Guy Smith:

I wouldn't expect too many in Inceptisols. We do have them in Aridisols. We have them
in Vertisols. We have one place in the Taxonomy where we have such a subgroup, not for soils
in the U.S. but as a request from the pedologists in India. These were fairly heavy clays.
Sodium saturation was 65, 75, 80 percent. They had very serious problems with them and they
didn't feel that the series would be adequate to deal with this problem. So, there is this one and
I could look it up and insert it later. In the Vertisols, we had at one time recognized the
sodium saturation when it became high as a subgroup characteristic but, in Puerto Rico, we
have some experiment stations on Vertisols and the behavior of the sodic Vertisols and the
others were exactly the same. This used to puzzle me for awhile until I realized that once the
Vertisols swelled up they were just as impermeable without the sodium as they were with it. So
it didn't matter except as a potential pH difference.

Question 35

Wilding:

Following that same question on the 15 percent exchangeable sodium percentage, I wonder
if some possible exclusion from natric horizons should not have been proposed for those soils
that contain carbonates. We have found instances where the exchangeable sodium percentage
can be between 15 and 20 percent in the presence of 20-30 percent calcium carbonate so the
soils do not act as dispersed natric horizons, rather they act as soils dominantly having calcium
on the exchange complex.
Guy Smith:

After *Soil Taxonomy* had been largely written, the Salinity Laboratory at Riverside switched from sodium saturation to sodium absorption ratio and we didn't think that we should deviate from what they were doing so we made the same switch. We used the sodium absorption ratio rather than sodium saturation for the most part.

Wilding:

These particular soils that Dr. Hallmark was commenting on would also have high SAR's.

Allen:

I'd like to ask Dr. Wilding a question. What do you mean by high SAR's?

Wilding:

The SAR's are between 20 and 25 with ESP's between 15 and 20 percent and they're in the presence of dominantly carbonate-charged systems up to 20-25 percent calcium carbonate so they really don't act as natric horizons though they meet the criteria of natric horizons.

Guy Smith:

Structure as well as SAR? I can't be of assistance on that.

**Question 36**

Allen:

I was wondering something along the same line. I have some concern as to whether a lot of soils with SAR's meet the requirement of being in the natric horizon as long as we have a lot of soluble salts that doesn't act significantly different from the natric horizons with this greater than 15 percent exchangeable sodium or the magnesium plus sodium, in fact I was just reviewing that in *Taxonomy*.

Guy Smith:

The study of these sodium containing soils is not finished. Just as we are not really finished with *Soil Taxonomy* until we stop learning about soils. There is still a great deal to learn about the influence of sodium in the genesis and in the properties of the soil. At the time we switched to the SAR the revised Salinity Manual had been edited and was about ready to go for printing and the Director of the Salinity Lab retired and a new one came in and it's never been printed. He wasn't satisfied with the SAR or with something that was in there and stopped the publication.
Question 37

Wilding:

The upper and lower boundaries of the moisture control section have been defined as that depth at which a dry soil will be moistened by 1" and 3" of water respectively. For example, in a loamy-textured soil, this would correspond roughly to 4 to 12" below the surface. Yet in morphological inferences of soil moisture regimes, we commonly use not only the morphological evidence in that zone but throughout the argillic horizon or other diagnostic horizons to approximately 1 meter. Why has the moisture control section been defined at such a shallow depth while morphological inferences are made at greater depths? Further, many of the important interpretive considerations of aquic moisture regimes are not restricted to shallow depths but include zones up to 1 meter or more.

Guy Smith:

I think that any soil that is saturated say with groundwater standing more or less long periods at a depth of about 4 to 12 inches is going to have periods when the water table comes to the surface as long as there is going to be rains. I attempted to write the definition so that such soils would meet the requirement for an aquic moisture regime. I may have failed. We only wanted the capillary fringe to come to the ground surface.

Wilding:

My concern is those soils that clearly have an aquic moisture regime in the lower sola, say in the lower Bt but in which the upper part of the argillic horizon has only occasional gray mottles but not dominantly 2 chroma. These soils would be placed as aeric intergrades to aquic moisture regimes when in fact they may not meet the aquic definition. They may be saturated, but not reduced for significant periods in the upper Bt.

Guy Smith:

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

Wilding:

On the other side of the coin, in view of your comments above, recent research work on the Coastal Plains of east Texas suggests that aquic moisture regimes occur in the presence of 3 chroma conditions. This would be in agreement with your comments about not requiring low chromas with warmer temperature regimes.
Guy Smith:

Yes, and no restriction on the chroma. Where you have high chroma mottles—if it’s 2.5Y or 5Y hues you can have any chroma. This was because in the southeastern states on the coastal plain such hues have long been observed that very wet soils.

Wilding:

That’s in the Aquults though. I’m talking about the Aquults with 10YR or redder hues.

Guy Smith:

No, I’ve proposed that these changes be extended. Where we have hyperthermic, isothermic, or isohyperthermic temperatures, so that the Mollisols, the Alfisols, and the Inceptisols would be treated parallel to the Ultisols.

Wilding:

I think you may want to consider thermic as part of that proposal.

Guy Smith:

In the Ultisols it is thermic. I didn’t have any examples of that in the intertropical regions; that’s why these proposals should go for review in other parts of the world where they have an impact.

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**Question 38**

Hallmark:

Dr. Guy Smith, *Soil Taxonomy* was formulated in such a way as to minimize genetic bias and to concentrate on the properties of the soil as these properties exist at the time the soil is classified. Part of the definition of plinthite requires an individual to project into the future and basically determine if an iron-rich, humus-poor material will irreversibly harden on repeated wetting and drying. Could you tell us the rationale for selecting this criterion, which must be essentially predicted as an event in the future?

Guy Smith:

I don't think it's an event predicted in the future, because there is no assurance that these will ever be exposed and harden irreversibly in the next million years or so. In general, I think it is quite possible for the pedologist who see these dark red mottles to decide whether or not they will harden irreversibly. There are 2 ways of doing it; one is to throw some of it on the ground surface and come back a year later and see what happens. Ray Daniels and some others, I'll have to add to the reference later, has pointed out that not all dark red mottles will harden and I've done away with more plinthite in Venezuela, by far, than I've created, because you can have a twenty-year-old embankment with red mottles that does not harden. On the other hand, you've got another exposure that's a year or two old and there they are hardened. And if you examine the nature of these red mottles away from the exposure where they have not harden, there are certain properties that they have if they are going to harden. They are brittle in character. There is enough iron relative to the surface of the silt and sand that, if exposed, they will harden. If they are going to harden, they will be brittle in the fresh
pit. As a general rule, then, one can check the presence of plinthite by locating a site that has been exposed, preferably one that faces the sun at some time of the year.

Question 39

Hallmark:

Following up on that question if I may. In my experience in the Southeast, it often seemed that the most important use in management application of the recognition of plinthite was not necessarily the plinthite itself but the reticulately mottled red zone in which it occurred and the fact that this zone limits water movement and root penetration. Was there a particular reason that plinthite was used for distinguishing this type of material rather than the red reticulately mottled zone?

Guy Smith:

Yes. We didn't care about the presence or absence of plinthite. That didn't matter a bit. It was a marker of a horizon which that did restrict water and root development and has the behavior of a fragipan. It may be that we should have included these in our definition of a fragipan, but this is being examined very carefully by the committee on classification of Alfisols and Ultisols with low activity clays. There has been much discussion about this. When one finds plinthite in a soil in Venezuela we do not find this restriction on water and root development. The plinthite was not the best marker we could have used. So they will come up with some recommendations for changes on that. One alternative is to include these with the fragipans. There is a mention in *Taxonomy* about some soils that are classified as having fragipans even though they didn't meet the requirements but they did have this restrictive layer. It has been observed and reported to me at least, that, when a hurricane comes through, the trees blow over on soils with plinthite and they're broken off on soils without, so it is associated in the U.S. but not necessarily in other parts of the world.

Question 40

Wilding:

The mineralogy families have probably come under progressive attack for a concern in placing soils into families that have common interpretive values. You eluded to this earlier in a question regarding the mixed family, which in my judgement, is far too inclusive and the vermiculitic and chloritic families which appear to be far too restrictive. In fact, looking at the series in the U.S. in the last revision I think we have only one vermiculite series in the U.S. Apparently, a part of that problem deals with the fact that chlorite and vermiculite families are only recognized on the basis of less than 2 micron fraction in fine families. What was the rationale for not recognizing chlorite and vermiculite mineralogy in the sand and silt fractions in, for example, the piedmont region of the U.S. and other places where it would be a prominent occurrence?
Guy Smith:

I really cannot answer that question. The suggested definitions for the mineralogy classes came from the soil laboratory people, and what they had in mind when they made their recommendations, I do not know. I do know there has been criticism of mixed mineralogy. We have not always used the less than 2 millimeter fraction for mineralogy. This difference in behavior is a mineralogic difference and we did not say that the mineralogy classes are mixed any more than any other part of the *Taxonomy*. It is a problem that needs to be studied by the people who work in the laboratory as well as on experimental stations and they should make suggestions for changes in the mineralogy classes.

**Question 41**

Thompson:

I’d like to ask a question along the same line on mineralogy, Dr. Smith. On the soils with more than 95% quartz, for example more than 95% quartz is a Quartzipsamment and less than 95% quartz is a Udipsamment, was there a rationale behind the very narrow tolerance between these two or was this based on water retention values or what caused this very narrow criteria for the differentiae?

Guy Smith:

Presumably, it’s because a proposal was made that never got criticized. Actually, the Quartzipsaments that I have seen analyses on are much closer to 99.9 than they are to 95. The 90 percent limit was set to keep in siliceous families those that still had an appreciable amount of weatherable minerals. I have suggested to the correlation staff that, instead of combining them, they raise the limit on the Quartzipsamment to more than 99 and leave the 90 alone.

**Question 42**

Hallmark:

Does tonguing of albic materials exclude genetic pathways such as animal activity? I’m thinking particularly of crayfish activity?

Guy Smith:

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

**Hallmark:**

We had a number of discussions along this line in Florida in which soils did have crayfish
activity. In the definition of tonguing nothing was said about the genetic mechanism. In the
Glossaqualfs the genetic mechanism is mentioned in the write-up but not necessarily in the
definition. From that we assume that the genetic mechanism was to be that of stripping out of
materials or degradation of the argillic horizon.

**Guy Smith:**

That was the intent.

**Thompson:**

Could it be possible that the crayfish activity could have hastened the tonguing during a
long time period. Just on my experience in the field I know that crayfish have been active in
soil movement or disturbance and still see the channel and so forth but in that same soil there
are also areas that qualify for the glossic features. I can't really tell whether they were crayfish
or whether this really happened as a result of some genetic or morphologic feature.

**Guy Smith:**

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

**Wilding:**

Following up on the question of crayfish activity and how they might be handled in *Soil
Taxonomy*. We have a number of soils in the Texas coast prairie as well as in the coastal plains
of Texas where the crayfish activity is restricted to the upper part of the argillic horizon and to
the zone which has undergone significant degradation by tonguing. The soils have well
expressed argillic horizons below a depth of 1 m or 1.5 m. Is there any reason in looking at the
rationale of *Soil Taxonomy* that a suborder of Vermaqualfs would not be a satisfactory manner
to handle these soils? We feel that the activity of crayfish have had a very important pedogenic
process on these soils in terms of their hydrological properties and thus should be recognized
from other kinds of Aqualfs or Glossaqualfs.

**Guy Smith:**

I would see no objection to your making such a proposal. The vermic great groups were
recognized because their horizons were commonly next to a meter or more. Certainly a crayfish
can do as much or more than the earthworm. He makes bigger holes, brings more materials to
the surface -- much larger particle sizes. You'll find small gravel in the casts of the crayfish
but not in the earthworm's casts.
Texas Interview

Question 43

Allen:

Dr. Smith, I noticed in your latest conversations with Leamy in Ghent, that you proposed, or at least mentioned, the possibility of expanding the definition of the oxic horizon to include all sandy loams, if I'm not mistaken. Now, I know this is going to reflect my lack of experience as far as oxic horizons, but I really wonder about soils with such low clay contents and how they react versus those so many oxic horizons have such high clay contents.

Guy Smith:

They all have relatively low available water-holding capacity whether they're loamy or clayey. One of the principal defects of Oxisols is the low available water-holding capacity. We originally put a restriction on the lower limit of the clay content of an oxic horizon at a point where we thought it would distinguish sandy and loamy soils. We were mistaken. We went on the assumption that, in these extremely weathered soils, there would be very little silt present. But, when we looked at the data from laboratories, we find that there sometimes is an appreciable amount of silt that it measures, almost totally quartz. This may be actually present in the soil in nature, or it may be a laboratory artifact, I do not know which it is. But, we do know that silt and clay can be generated by dispersion processes and mechanical analysis. Just to simplify the business of how much silt is or isn't present I simply propose that we drop the clay content completely and substitute the difference between the sandy material and the loamy material. The sandy material cannot be an oxic horizon because we want to have an intergrade between the Quartzipsammnts and the Oxisols. We get very sandy Oxisols and we get very strongly weathered quartzipsammnts, and we wanted to have the Oxic Quartzipsammnts as well as the psammentic Oxisols. We thought there was plenty of room there, management wise, for the two central concepts and one intergrade on each side of the boundary. The simplest way to define that boundary and to avoid the silt problem is just to say loamy or sandy.

Allen:

And all the loamy ones would go as an oxic horizon?

Guy Smith:

Yes.

Question 44

Wilding:

I'm curious as to why calcite and dolomite minerals are not considered as weatherable minerals? In soils developed from calcareous sandstones in East Central Texas where the sandstones are composed primarily of carbonates and quartz and other more resistant components, and where the control section is fine-loamy or coarser, these soils in ustic moisture regimes are placed in siliceous families even though they may have calcic horizons in the lower solum. What is the rationale of not permitting carbonate minerals to be considered as weatherable minerals?
Guy Smith:

This comes about from the soils of the arid parts of the U.S. Where we may have a Paleargid, perhaps, that has now become recalcified. From the dust, from calcium in rainwater and so on, the interiors of the blocky peds will not effervesc while the exteriors are coated white with calcium carbonate. This is obviously a soil that has been decalcified at some point in the past, but, in the present dusty and dry environment the carbonates are accumulating again. It just seemed that in the definition on weatherable minerals, we had better leave them out. There had been plenty of questions about this before.

Question 45

Daugherty:

Dr. Smith, in teaching Soil Taxonomy to students, I find that one of the biggest problems that they have is understanding the differences in the family control sections for soil texture and mineralogy. Could you discuss the rationale on choosing the different depths and different thicknesses of family texture control sections?

Guy Smith:

We generally made the distinction between soils that had an argillic horizon and soils that did not. If there was no argillic horizon, we used a more or less arbitrary control section of 25cm to 1m. If the soils there had an argillic horizon we generally used the upper 50cm of the argillic horizon. Now I must confess that, when I taught, I required my students to place the soils that they were studying at the family level. On examinations I gave them exercises, where they wrote the descriptions, and required that they identified the family and they couldn’t do it. The trouble is the way the book is written, so I developed a little chart for the students and I guess copies have been made available by Dr. McClelland to SCS Staff and you can probably get copies. (Note: Smith was writing on a black board.) I simply listed the orders across here [horizontal at top] and family differentiae on this axis [vertical at side] and then I came out with a lot of blocks and I gave the control section and then the number of particle-size classes and mineralogy classes and so on, and, [coming down here,] moving to a box would list which families could be used in a particular order. Once I gave them this they had no trouble. So if you would contact the State SCS Office I think they could give you a copy.

Question 46

Allen:

Dr. Smith, I often have a problem, again, on the albic horizon. It is difficult to explain to students where there is no minimum thickness required in the definition. Is there some reasoning behind that?
Guy Smith:

It may have been pure oversight. Many of the Boralfs have a relatively thin albic horizon where the argillic horizon has a fine or very fine texture and, if plowed, this is mixed and cannot be observed anymore but you can still observe the argillic horizon. When you look at the use we made of the albic horizon, I can't think of anywhere offhand, where it's diagnostic.

Allen:

So then maybe it's like being a diagnostic horizon, perhaps is the reason it's not listed.

Guy Smith:

I'll have to check Soil Taxonomy. The only place in Soil Taxonomy where I find the albic horizon used as a diagnostic horizon is in the suborder of Albolls. The minimum thickness of albic horizons in other kinds of soil would not be critical because of presence or absence of an albic horizon is not diagnostic to the classification. It was our desire, generally, to keep in the same series in the same family the cultivated and the undisturbed soil so that the series would not be changed by a few plowings. There are soils, such as the Boralfs, which may have a very thin albic horizon if the argillic horizon is fine or very fine in texture, and these are kept together in the classification by not making the albic horizon diagnostic, rather we have used temperature, primarily, to define the suborder of Boralfs. The albic horizon is normal in these soils and has been recognized by the Canadians as a diagnostic feature. They, however, do not mind the thinness of the albic horizon because they classify the soil on the basis of the presumed virgin profile, rather than what is there today. The other group where the albic horizon is common is in the Spodosols. In the Russian classification, the Australian classification, and the New Zealand classification, classified as Podzols, soils that had an albic horizon, irrespective of the nature of the B horizon--argillic or spodic. There has been in those countries considerable resistance to Soil Taxonomy because it does not use the presence or absence or the thickness of the albic horizon as a diagnostic in the classification.

Question 47

Wilding:

I know that you have commented on this question recently, and the questions raised at Cornell, but I'd like to have you respond here also. The pedon in Soil Taxonomy is considered the unit of sampling. The polypedon is considered the unit of classification. Now, the polypedon is certainly conceptual because we cannot view a polypedon at any one instantaneous point in time, but we certainly can view, at least portions of, the pedon. Why is the unit of classification the polypedon rather than the pedon?

Guy Smith:

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

Wilding:

The pedon, however, is defined as a three-dimensional body of soil that has lateral dimensions large enough to include representative variations in the shape and relationship of horizons and in the composition of the soil. The polypedon is a conceptual unit that includes holes in that conceptual unit which is presumably an attempt to portray, taxonomically, the cartographic unit which is the real landscape body. So, it seems to me that if you go to classification systems such as mineralogical systems which use a unit cell as the basic individual building block on which the classifications are constructed, that the pedon could serve in a similar role for classification in Soil Taxonomy.

Guy Smith:

I cannot comment too much on that, as you say I've discussed this a number of times before. At one stage in the preparation of the manuscript we used that analogy to the individual basic cell of the mineral. The polypedon is defined as a group of contiguous pedons that do not differ significantly in any diagnostic property. Though that sort of analogy would appeal to most any mineralogist. It would not really appeal to the man who's making soil surveys.

Wilding:

But, is it true that the shape attributes of the soil comes out as a phase criteria outside of the taxonomic system except for Orthents and I guess its utility in some of the higher classes but, generally speaking, for our interpretive purposes slope, for example, is handled as a phase of a series.

Guy Smith:

Generally, but not in the more serious situations where you have say an Aquoll on a side slope where there is seepage water coming out, or you'll have a Histosol on that side slope where it has still more water. The drainage of these soils engineering-wise is entirely different from the drainage of the others, and it is a difference of such magnitude that we thought it should come out at the family level instead of the series level.

Question 48

Wilding:

Why do abrupt boundaries with less than 20 percent clay in the alluvial horizon require only a doubling of the clay content within a vertical distance of only 7.5 cm? For example, an eluvial horizon of sand with 3 percent clay content over a Bt laminae with 6 percent clay content could be considered an abrupt boundary. What was the general rationale utilized in defining the abrupt boundary categories?
Guy Smith:

Laminar argillic horizons in the sands have abrupt boundaries, without question, but they did not interfere so much with the soil permeability as did the abrupt boundaries with the illuvial horizons that had more clay than you have in the sand. Laminar abrupt boundaries are actually beneficial to the farmer in that they will at least double the available water-holding capacity of such a soil because the water hangs in the base of the clay laminae. We were trying there to get at a definition that would keep together the bulk of the soils that had been called Claypan Planosols at one time. Where we commonly have a silt-loam albic horizon over a fine or very fine textured argillic horizon where the nature of the argillic horizon causes the water to perch above the argillic horizon in the albic horizon. In the laminar soils the water does not perch above the argillic band but perches within it, and does not introduce problems of aeration, and anaerobic ground waters and death to the roots of any air-loving plant that happens to be growing there. There was an effort made to utilize some of the concepts of the 1938 classification in the development of Soil Taxonomy. The ones that were judged to have a maximum utility we tried to retain in Soil Taxonomy.

Question 49

Allen:

You have already answered this question in some of your other sessions, but since very few of us have even seen placic horizons, I am still unclear as to why placic horizons are separated as a distinct type of horizon from spodic horizons, even after reading your answers.

Guy Smith:

The placic horizon is a rather distinctive sort of horizon when you see it in the field. It is very thin, it is involute, it is hard, it makes a barrier to the water enroute just like any other pan, although it wasn't called a pan. We don't understand the genesis of the placic horizon as well as we understand the genesis of the spodic horizon, though there are many similarities in the composition. There are significant differences in the composition, in at least some placic horizons. The placic horizon consists of an accumulation of iron, aluminum, organic carbon, and manganese. Manganese has never been found, to my knowledge, in a spodic horizon. This accumulation of manganese may be on the upper or the lower boundary of the placic horizon. We have no reasonable genetic theory that explains the development of placic horizons. We know from geographic correlation that they are all always in soils with perudic moisture regimes. They are continuously moist throughout the year. Beyond this, we really don't know much about them. They can occur in very skeletal materials; they can occur in clays - very fine textured materials, normally, within a depth of 50 cm or 1 m. They are important to the movement of water in the soil; they are important to the growth of roots in a very different manner than the spodic horizons. Now, they have, like the spodic horizons, virtually completely pH-dependent charges and almost never a permanent charge. The major difference between them is the thickness, the barrier to water-use and the presence of manganese. Manganese is not always present in the placic horizon but it may be, and as I said it may be at the surface or at the base on the placic horizon. The studies I have seen suggested that, when the manganese is at the base, you have water moving laterally below the placic horizon from higher ground somewhere nearby. But manganese is a mark of some alternate oxidation-reduction process.
Thompson:

Then the principal reason, I gather, is why it is set out at the categoric levels where it is, primarily because of a difference in its resistance to water and root penetration as contrasted to spodic horizons?

Guy Smith:

No, that's not entirely true. As I said, it is a barrier to water and roots. It occurs in very unlike kinds of soil. We have them in the Hydrandepts which are the soils that are mostly oxides, clay-size, that dehydrate irreversibly in drying. We have them in Spodosols. They may be above or in the spodic horizon. We have them in Dystr6chrepts. If we treat that as a spodic horizon, then, we put together some of the Hydrandepts with some of the sandy Spodosols and we get a class that contains soils about which we can make no statement.

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Question 50

Hallmark:

As use and management pressures are becoming more acute in the wetlands, we find ourselves mapping in areas that are constantly inundated by brackish water. What are your feelings on extending mapping into such areas and, in particular, can you give us your ideas on whether soils should be terminated as the water depth is increased?

Guy Smith:

Well, we did say in Soil Taxonomy when the water gets deep enough that we have only floating plants, that ceases to be soil. That probably does vary some according to the environmental conditions of these wetlands. If we are going to use these soils, then certainly we must find ways and means of making soil maps that will be helpful in predictions of the consequences of use. If the wrong wetland is drained, you may wind up with a Sulfaquept which is finished, some of them at least for three or four hundred years, it will grow nothing. Those are the extreme ones. We must be able to warn people not to drain such soils, and you can't find out about their existence in the brackish waters without finding some way to get over the ground and collect samples to study. Shrinkage of these soils, on drainage, are the ones that do not become acid, the shrinkage can also make very serious problems in the engineering use of the soils. You may have seen the New Orleans subdivision where the soil has shrunk away from the house and the garage, so we must be able to predict this shrinkage, and the architects must understand the problems they are going to get into if they build on those soils. I would say if there is a perspective use of the soil system, if you're proposing to use it for something, then we do need a soil survey, although it may be difficult. I've waded in water up to my hips to look at these things--fortunately, in a warm country.
Question 51

Hallmark:

(Note: this question is stated and restated - could be shortened). A follow-up to this is the use of the n value to bring out soils which basically have been permanently saturated and are not able to support particular animals in a normal grazing fashion. Why was it that started to go with an n value, rather than to utilize something that would more directly get at what was really the simple concept in establishing an n value, that is to measure the bearing-capacity in some fashion or another directly, rather than indirectly with an n value.

Guy Smith:

The n value was borrowed from the Dutch soil scientist, who has perhaps the most experience with reclaiming wetlands in the world. I don't know what substitute measurements we might have made. It is something that you can determine from the sample in the laboratory. Bearing value -- I'm not sure about the engineering tests. It would be virtually nil in the normal Hydraqunt. It would vary somewhat with the sand content but not greatly because there is a limit on the minimum amount of clay they would have. It was the only suggestion we found in the literature that addressed this problem. The engineers have not concerned themselves with it much, so far as I know. Typically, they take their samples and dry them out before they start their test, and that's too late.

Hallmark:

A group of individuals has proposed, out of the Southern Regional Work-Planning Conference, to possibly test in the field some lightweight apparati which would get a handle directly on the bearing-capacity rather than n values because n values require quite a bit of laboratory manipulation. First of all the sample has to be maintained in a wet status and then on top of that you're going to run organic carbon and particle-size plus the percent water in that natural state and some of these things are very high in organic matter and the error factor then becomes increasingly higher as the organic matter increases.

Guy Smith:

As we develop new methods to measure things, we will doubtless change our own definitions. But I don't know a current method.

Question 52

Wilding:

We have a soil taxonomy system that is constructed with 10 orders. Following the logic of classification it seems that we could have ended up with maybe even two orders -- organic and inorganic soils -- and then as suborders the various differences among the inorganic soils. What was the logic in determining how many orders there would be and what soils that they would combine or group?
Guy Smith:

In our first approximation, we had one order for organic soils and another order for mineral soils. This created an extra category that we could see no use for because we could just as well subdivide the orders as to organic soils and mineral soils that have this and have that and have the other and we didn't need that extra order. We examined what we had in our earlier classifications and what other people had in classifications in other countries to see what we could devise in the way of orders. Marbut insisted on only two classes in his highest category -- Pedalfers and Pedocals, and he ruined his system on this, because, while we have soils that have both accumulation of carbonate and accumulation of iron and aluminum, and we could readily say well we'll give priority to one or the other. We also had soils with neither, and they had no place to go. So Marbut just dropped them out of this system and said we'll classify these on the basis of the soils that are around them because eventually a wet soil is going to be drained by geologic erosion and then it will begin to take on properties of either a Pedocal or a Pedalfer. How he was going to drain the coastal marshes, I don't know, except by dropping the ocean, it can't be done. We examined what had been done in previous U.S. systems and in the other systems in countries where they were making soil surveys. We did not look into classifications in countries without a soil survey program. We didn't feel we would be apt to learn much from that. We wanted, then, to have enough classes in the order category that we could accommodate the major differences in genetic processes, but not more than we could readily remember. We figured one could understand 10 classes or a dozen without much trouble but not 50. We also needed in our taxonomy, a sort of a key that could be used for identification in the correlation process. When you start to correlate a soil, an unknown one, you first figure out what order it's in and then the suborder and then the great group and so on down. And in each step we don't want to have more classes than can be readily understood in the context of that particular taxon. In Taxonomy it says 5 subdivisions on the average. We didn't want to have 50 subdivisions although we wound up with a few families with 50 series. Still, the series are not defined in Taxonomy. We left it up to the correlation staff to devise their own keys for these large families. There was nothing sacred about the number 10. Now I've proposed an 11th and that's an awkward number. I think I'll look around for a 12 somewhere. Twelve is a much more satisfying number than 11.

Question 53

Wilding:

With regard to that, I just returned from Canada, visiting with colleagues there. They felt very strongly about an order called Cryosols. Would you care to comment about the way we've handled soils with pergelic temperature regimes?

Guy Smith:

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

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

**Question 54**

_White:_

May I ask a historical question here? Rick and I are, I believe, the only geographers in the room and I think that we can both vouch that the Soil Taxonomy has taken a great deal of verbal abuse from geographers, especially the old time geographers that were trained under the '39 -'49 system. When you developed the system as we see it today, was there any consideration given to the spatial distribution of what the taxonomic classes would finally appear as on a map? I don't see it as much of a problem because I think the new maps that we can make using this system reflect more of the truth of the landscape. I will agree that cartographically they aren't as aesthetically pleasing as the old ones because the boundaries become more convoluted rather than nice smooth rounded ones under the old system. That doesn't bother me a great deal but it does bother a lot of the other geographers. Did you have any spatial recognition or spatial logic as you went through testing the various approximations?

_Guy Smith:_

I'd say, basically, no. We recognize that some kinds of soil have very different shapes from others, and the polyedons have very different sizes. Some occur on the ridges and some occur in the flood plains and so on, but by and large we were not going to concern ourselves with spatial considerations. We took the view that it was the nature of the soil rather than it's area and shape that was critical. I don't like analogies. They are often misleading but if I were going to use one I would go back to etymology where I first started college and we would have to have a separate kingdom for the ants because there are so many more of them than any other kind of animal. This is the trouble with the FAO-UNESCO legend as a classification. The soil must be extensive enough to be shown on a 1:5 million map or it doesn't appear anywhere. We make maps on all scales. In the soil survey we make small-scale maps and large-scale maps and, depending on our purposes, we may make very large-scale maps from small areas. Then the classification has to reflect the properties of the soils that are at the scale that we map, and that scale has to be determined by the purpose of the soil survey, which is to reflect the behavior of a soil under the foreseeable uses, of not all possible uses, but the foreseeable ones.

**Question 55**

_Wilding:_

Returning to the concept of petrocalcic horizon, we have a minimum thickness definition when the petrocalcic rests on bedrock, however, in situations where it does not rest on bedrock,
I do not see a minimum thickness. What is the rationale for having a minimum thickness over bedrock, but none over an unconsolidated deposit?

**Guy Smith:**

Well, the petrocalcic horizon has much less significance to use and management when it lies on bedrock because the bedrock has the same practical effects as the petrocalcic horizon. Instead of just having a thin film of lime before we recognize it as important we put a minimum thickness on it. The normal petrocalcic horizon that I’ve seen, and Lee Gile tell me if I’m wrong, is much thicker than this minimum thickness on bedrock. That’s the normal situation. I suppose one could find one that was 5mm thick, but I don’t believe I’ve ever seen one.

**Wilding:**

Guy, to rephrase the question, as you move from west to east in the ustic moisture regimes where petrocalcic horizons occur in Texas, it’s not uncommon to find an indurated laminar zone of calcium carbonate 5 to 10mm thick over a paralithic material, marls, for example. It is my understanding that the petrocalcic horizon was established because it was a restrictive horizon to root growth and water movement. Yet these thin laminar caps do not meet the current definition of petrocalcic horizons over bedrock (2.5 cm or more thick). What was the rationale used in determining how thick a petrocalcic horizon should be before it is recognized at the higher categorical levels in *Soil Taxonomy*?

**Guy Smith:**

(Tape turned over while he continued to talk) I am not sure I understood. A thin laminar horizon rests on what?

**Wilding:**

Well it commonly would rest on a paralithic material.

**Guy Smith:**

But bedrock can be lithic or paralithic; in Taxonomy we said bedrock and not lithic or paralithic material.

**Wilding:**

OK, let me rephrase the question again and ask it in another way - so long as a laminar cap has developed it implies to me that it turns water and it probably would likewise turn roots. It doesn’t really make any difference how thick it is.

**Guy Smith:**

Not if the underlying material also turns water and roots; and the paralithic materials generally do.

**Wilding:**

In this particular situation we believe that perhaps the marls may have a significant water-holding capacity that would be available for the plant root if the petrocalcic horizon were not present, or the thin laminar caps were not present. From the use and management point of view we have been concerned about the minimum thickness of the petrocalcic horizon. Obviously we could still differentiate the soils with indurated calcic horizons too thin to meet the petrocalcic at the series level but we are interested in the rationale utilized in determining what the minimum thickness of a petrocalcic should be for interpretive purposes.
Guy Smith:

It was just the relative importance of a very thin cap over an impermeable material versus a thicker one.

Question 56

Thompson:

In the definition of petrocalcic it says the laminar capping commonly is present but is not required. Could you comment, Dr. Guy Smith, a little on the identification of the petrocalcic horizon where the laminar capping is not present as might appear in soils developed, in say, limestone geology?

Guy Smith:

Well, in the work that Giles has done in the Las Cruces study area, he has pointed out the various stages of formation of the petrocalcic horizon in which first you get pendent, then you get above the horizon almost completely filled with carbonates and then finally when it becomes impermeable the water film moving over the surface of the plugged horizon you develop this laminar horizon which smooths the surface of the petrocalcic horizon into what Ruhe used to call "troweled surface", I believe. Looks like a plaster job that I might do. The distinction of the presence or absence of the laminar horizon is probably genetically related to whether or not you get an occasional rainfall that is hard enough to bring the soil above field capacity above the petrocalcic horizon, so that the water bearing the carbonates moves laterally and evaporates and deposits it in the fine pores of the cross-sections of the petrocalcic horizon normally so that the sands, gravel, and so on are separated by the carbonates and pushed apart as though the horizon was building up from the base to the top of the present laminar horizon. I can't think of too much trouble that people would have in deciding whether this horizon is cemented or not; certainly it should be free of fine roots at close intervals. There are still one or two questions about it that we cannot explain -- the very curious radio carbon dates of the carbon in the petrocalcic horizon versus the radio-carbon dates of the calcium carbonates.

Question 57

Allen:

(to Wilding) Do you find a kind of petrocalcic horizon that you referred to? Have you observed them over materials or in materials that won't qualify as a paralithic contact? The reason that I am asking that, I can't think of a single paralithic contact that I have seen in West Texas that is as thin as we are talking about and yet which is not too well fractured. I mean punctured enough that there is no problem on digging through it. It looks like remains of petrocalcic horizons. I was wondering if you had any statement on that?
Wilding:

I can't recall that I have seen these over materials that were not either lithic or para-lithic. Coryell County would be probably the best prospect in terms of the soil surveys that we are currently making to look for this kind of phenomenon. The other situation in which it, although I have not observed it, which this situation has been described to me has been in foot slope positions where seepage waters of both from upslope to toeslope positions. These apparently are of variable thickness but I have not observed those directly.

Question 58

Daugherty:

I would like to solicit your comments on the limits in the definition of calcic horizons. In New Mexico we have very few Camborthids Aridisols due to the definition of calcic horizon and to the nature of the key. I would like to solicit your comments on the change of the percentage of calcium carbonate that's required for coarse-textured soils as compared to finer-textured soils and some of the background behind the setting up of those limits.

Guy Smith:

My memory is not too clear on this, but I think I can understand it. If you have a sandy gravel you can have a very distinct accumulation of calcium carbonate before you reach the 15% by weight, but in the case of these particle sizes we require at least 5% by volume of the secondary carbonates. This would be consistent with the 5% limit of secondary carbonates in the calcic horizon by weight greater than the underlying material, which is roughly the same limit.

Daugherty:

On some of the soils when you approach 18% clay it is very difficult to visually see 5% calcium carbonates, and I guess maybe the background behind my question would be - Why wouldn't there be a sliding scale such as the ratio used for argillic horizons for calcium carbonate?

Guy Smith:

Perhaps there could have been, but no one came up with that proposal.

Daugherty:

It seems to be quite a drastic jump especially when we deal with soils right around the 18% clay break to be required to have 15% calcium carbonate for the calcic. If you have a soil with 20% clay you need 15% CaCO₃, if you have a soil with 17% clay you only need to have 5% CaCO₃.

Guy Smith:

You must also look at the uses made of the calcic horizon. In North Dakota the glacial till normally have more than 15% carbonate when they are laid down and so it is very easy to meet the requirements for a calcic horizon if you have 5% more in the Ca horizon than you do in the underlying till. Many of the tills there, are marginal in this respect and we pay no attention in some series definitions as to whether or not there is a calcic horizon. It is not even considered...
Question 59

Wilding:

In attempting to teach *Soil Taxonomy* at the undergraduate or graduate levels in the university system, it has not been for many of us an easy task. Do you have any suggestions in terms of the educational process how you would most effectively transmit the kind of information that we have developed in *Soil Taxonomy* to the point that a student would be able to pick up a description or a series of students would be able to pick up the same description and consistently place the soil at least down through the family level?

Guy Smith:

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

Question 60

Allen:

I found it quite difficult in the field to determine whether gypsic horizons are present often or not, and here are some of the problems. The definition on page 46 reads in part --- "secondary sulfates that are 15 cm or more thick, has at least 5 percent more gypsum than the C horizon or the underlying stratum ...". It has been my experience in these things with gypsic horizons that it often goes like this, in other words there are bulges and then there will be a little bit of a decrease and then another bulge and so forth so what is your standard of reference below there?

Guy Smith:

What you are saying is not going to be recorded here when you wave your hand, so I will see if I can put it on tape. With depth, the increase and decrease in the percent gypsum is erratic. I would say, by and large, this application of the definition probably would be as good...
as any. If it did decrease in some subhorizon by 5 percent this would qualify it for a gypsic horizon.

Allen:

  In some subhorizon?

Guy Smith:

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

Allen:

  I don't understand your statement about sprinkler irrigation.

Guy Smith:

  I guess now we would use a different system (?) that didn't exist in use in any practical form at the time we were developing these horizons.

Allen:

  I have also found that it is very difficult to recognize the petrogypsic horizon when it is overlying gyosite. There are some suggestions of laminations in many cases there (?) is and I'm often confused by what I'm actually seeing.

Guy Smith:

  I have never seen one myself so I cannot help.

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**Question 61**

Wilding:

  You have responded to this question at least in part at the Cornell interviews, I am wondering what the impact of geomorphology and the application of geomorphic principles has been on Soil Taxonomy.

Guy Smith:

  It has been much less than it has been on soil mapping. The classic concept of downwearing of surfaces versus lateral retreat of slope has considerable influence on what the mapper does in the field, but it hasn't had a great deal of influence on Soil Taxonomy itself. The geomorphic studies have had their principal impact in the pale great groups, in which we tried to distinguish the soils of the very old from the younger geomorphic surfaces. We have
grouped somewhat unlike things in pale great groups as in the Aridisols where we use the petrocalcic horizon and abrupt textural change to define the Palaearids. This gives us two unlike kind of things at the great group level which must be separated at subgroup level. Then we have the Petrocalcic Palaearids versus the Typic Palaearids where we settled on the abrupt textural change and the fine texture for the Typic. I don't think of any evidence of where soil geomorphology studies have greatly influenced the Taxonomy other than that it helped us understand what we already knew about these soils. How does the petrocalcic horizon form? What is its genesis. What is the difference between the Paleudults and the Hapludults of the Southeast? Going back to the 1938 classification the southern correlators argued that the definitions should be by type rather than by limits. The Ruston was the type Red-Yellow Podzolic soils, and the Houston black was the type Grumusols, and so on. These definitions are not really workable when you have so many thousands of series because there are so many that are alike, one type of Red-Yellow Podzolic soil in one respect and one type a Gray-Brown Podzolic soil in another property and which one are you going to weight? I use the example of the field trip we had with the northern-southern correlators on the piedmont soils in Maryland and Virginia where the soils have a solum that is comparable in thickness to that of the Miami which was supposed to be the type Gray-Brown Podzolic soil.

But they have the clay mineralogy and so on of the type Red-Yellow Podzolic soil of the Ruston and the Norfolk. We never did resolve how to classify the Chester series because neither group would have anything to do with it. Those who worked with Gray-Brown Podzolic soils says it is a Red-Yellow Podzolic soil and those who worked with Red-Yellow Podzolic soils says it is a Gray-Brown Podzolic soil and we couldn't resolve it with that type of definition. The soils geomorphology studies helped straighten us out. The study helped us to understand a little better. We had a lot more typical Red-Yellow Podzolic soils in the south than Ruston and Norfolk. They were clear off at one end of the spectrum so they became the Paleudults. The Hapludults are soils like the Cecil soils. They are much more extensive, much more representative of modern processes of development and these very old ones according to Daniels may be up to 2 million years old without serious additions or losses.

Wilding:

Yes, this was my thought if perhaps geomorphology provided some of the pedogenic thread that helped us understand the morphogenetic link with diagnostic horizons on stable land surfaces in a particular area we could expect certain kinds of diagnostic horizons.

Guy Smith:

That is of principal benefit to the man who is making the map rather than the man who is trying to design the taxonomy.

Wilding:

But doesn't it help us understand the taxonomic-cartographic bridge?

Daugherty:

Larry, it is very difficult to go from a soils map to a geomorphic map. You can't very well make that leap, that direction.

Wilding:

There will be some people who might think that is possible. Parsons mapped a lot of series on given geomorphic surfaces in Oregon, for example, and different series on the next geomorphic surfaces. Taxonomically there was a relationship, I think, between the chronologic age of those surfaces and the intensity of pedogenesis of the soils developed on different surfaces.
Daugherty:

I think most of the thrust is going from the geomorphic surface to the soil rather than from the soil to the geomorphic surface. Rick, you worked up on the Colorado (?) which way do you find it easier to go?

Rick:

Generally from the surface to the soil. That is one thing to keep in mind, you can have quite a variety of soils on a single geomorphic surface. Keep this in mind it will help. Use the surface as a kind of chronological frame work in which to arrange the soils, very useful. Yes, that direction is more useful.

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**Question 62**

Wilding:

The Vertisols had been restricted to the mesic temperature regime or warmer. What was the rationale for excluding soils that had in essence all of the properties of Vertisols in frigid temperatures?

Guy Smith:

There was none. I tried to get rid of that unsuccessfully.

Nichols:

That change has been made.

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**Question 63**

Hallmark:

The terms, aridic and torric are used for the same moisture regime but in different categories in the taxonomy. What was the logic in having those meaning basically the same thing?

Guy Smith:

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

Question 64

Calhoun:

Not all soils with aridic soils moisture regimes are classified as Aridisols even though the introductory statement in Taxonomy for that particular chapter states that these are..."soils that do not have water available to mesophytic plants for long periods." Why was it decided to exclude arid climate soils with oxic horizons, vertic properties or no diagnostic horizons from the Aridisols order?

Guy Smith:

The first point on the soils without diagnostic horizons was that they came originally from the concept of the Azonal soils. They were soils without diagnostic horizons and we wanted to keep them together as an order because without any subsurface diagnostic horizons there are really no statements you can make about the Entisols except that they lack subsurface diagnostic horizons. The statement is not very important to the soil survey.

Their arid climate was shown only at the great group level because in the Entisols we wanted first the suborder level to sort them out according to the reasons why they had no subsurface diagnostic horizon. For example, there is a big difference between the Orthents and the Fluvents, and agricultural importance. Perhaps more people in the world get their food from Fluvents than any other single kind of soil. The exclusion of the Oxisols that have an aridic moisture regime was primarily because they will under irrigation behave like other Oxisols. We would have all of the difficulties that you would expect from management of other Oxisols from that group. We might as well keep them together as Oxisols. In that situation we could deal with the arid climate at the suborder level instead of the great group level because they seem to be the most important subdivision of the Oxisols according to their soil moisture regime. The exclusion of the Vertisols that have an aridic moisture regime or at least have an arid climate, I think is parallel to the exclusion of the Oxisols. Under use they are going to behave like other Vertisols. In Sudan in the Gezira Scheme the irrigated soils are Vertisols and they crack and the cracks close and so on every year and have slickensides, parallelepipeds, and what have you. Just at the boundary of that Gezira Scheme I am told that the soils are not Vertisols. Because they never get moist enough to swell, they are dry enough to be cracked and the cracks that are there are filled with granules but because there is so little movement in the absence of irrigation you cannot find slickensides. This will illustrate the reason why the Vertisols probably should be kept together as a group instead of being split according to their moisture regime.
Question 65

Thompson:

Going back to the petrocalcic horizon, in the petrocalcic horizon definition the criteria states that a thickness times percent carbonate is required if the laminar horizon rests on bedrock. Why was a similar criteria not applied to those petrocalcic horizons that are underlain by loamy highly calcareous materials?

Guy Smith:

I don’t think I know the answer to that. Perhaps Lee Giles can help us.

Giles:

No, I really can’t. This might have been left over from the definition of the petrocalcic horizon as it was originally formulated in which the laminar horizon was required for all petrocalcic horizons.

Guy Smith:

These definitions all went through a number of statements that were modified from time to time, as we learned more about how the soils were grouped by the definitions we had written.

Thompson:

This question is asked frequently by field soil scientists.

Guy Smith:

If it is unimportant then we should consider changing the definition.

Question 66

Daugherty:

Would you please discuss the rationale behind the limits of the moisture control section? There is no discussion in Soil Taxonomy as to physically how to measure the soil moisture control section. It mentions to add 1 inch and 3 inches of water but there is no methodology, I have never seen anybody actually do it in the field.

Guy Smith:

I never have either and yet it would be feasible to do it if you had a soil that from time to time actually ran out of available moisture. In a humid region where the soil never dries out I don’t know precisely how one would make the measurements. In a dry climate where the soil does become dry one could readily apply the 2.5 cm of water and wait the 24 hours necessary and then excavate and see the depth of penetration of the wetting front in that time. One could do the same with the 7.5 cm to see where the wetting front had reached. The purpose of the moisture control section was to permit the calculation of moisture regimes from the climatic
data because we are quite aware that it would rarely be measured. The model, I think we have
discussed this, the model that we designed to measure the wetting and drying of the soil was
devised with the help of the records from the old dryland stations. Without some sort of a
defined moisture control section one would find it very difficult to say that the soil was dry or
moist or partly dry or partly moist - where is it dry and where is it moist? The upper limit of
the moisture control section was placed below the surface so that a very small shower would not
interrupt the dry period in the soil. The soil can be dry throughout the moisture control section
but plants can still survive if their roots go below it. When we say the soil is dry that is a very
different statement from saying that the moisture control section is dry. We need to be able to
define the part of the soil that we were talking about, being dry or moist.

Daugherty:

As a follow-up to that question do you think it would be feasible to have different
moisture control sections in arid soils or marginally arid soils or a different moisture control
section to deal with the subgroup transitional areas?

Guy Smith:

It would be possible but I don't know of anyone who is thinking about that problem at the
moment.

Question 67

Giles:

This question concerns application of the taxonomic system and mapping areas designated
as broadly defined or low intensity. In some cases examinations of these areas have shown a
number of soils not mentioned in the mapping unit description. It probably means the soil
series is still used in areas designated as broadly defined. I wonder if this should be done if
there is too little time to classify soils at the series level? Would a different approach be
helpful in this case, such as, mapping the soils in categories higher than series? Mapping great
groups, for example, generally show the presence or absence of horizons that are particularly
significant to land use. Mapping Paleothids for example, would show the presence of
petrocalcic horizon without having to spend the time necessary to make distinctions of more
categories. Will you comment on this?

Guy Smith:

This is partly a correlation problem, partly a matter of legend design. It is not
particularly a problem of Soil Taxonomy, but it is a part of the application of Soil Taxonomy to
soil surveys. It is currently being done, that is, mapping at a categoric level higher than the
series. It is currently being done in the United States and in most of the developing countries.
In Alaska for example, the Exploratory Soil Survey, the legend is largely based on categories
higher than the series because they have no particular use for the series concept in areas of soil
where about the only potential use we can see is grazing by wildlife. In Nevada, in the small-
scale mapping they are not using series because again the utility of the series is small when the
only foreseeable use is very extensive grazing by livestock. In the developing countries their
first surveys generally are made at scales of 1:50,000 or smaller. They have so little
experience with the use of the soils, that if they were to establish series they would go through
the same process that we went through in the U.S. As they acquired knowledge about the soil
behavior they would be constantly splitting their series and setting up new ones. My thinking is
that the initial surveys, certainly mapping [at the] family category, is about the lowest categoric
level that should be used: even in small scale maps, of course, you cannot map at family level. You can only map as associations of subgroups or great groups. The soil map of the U.S. in the Atlas uses associations or phases of great groups and as the basic taxonomic unit.

Question 68

Nichols:

In reading over the questions that were asked at Cornell I noticed a question that I did not quite understand about whether or not series should be kept as a part of the classification system. Do you remember what the meaning of that question was and what you gave for an answer?

Guy Smith:

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

Question 69

Allen:

In some of the other sessions in which you have participated you discussed the problem of changing the classification of soils from Inceptisols to Aridisols with salinization under irrigation, of course, this would be in ustic or xeric moisture regimes. I am still unclear as to your thinking about the solution of this problem.
Guy Smith:

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

Allen:

So these would remain as Inceptisols?

Guy Smith:

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

Question 70

Thompson:

This proposal has been made previously but I would like to hear your comments concerning the rationale. What is your comment on the proposal to recognize ustic subgroups of Albqualfs and Ochraqualfs? These subgroups would identify those soils that are dry for long periods during the summer months, but are saturated at some season. The Lufkin series is one of several in Texas that meets this concept. Perhaps there might be a better suggestion that you would have to recognize what I would term as "wet-dry" soils from the "wet-wet" soils.

Guy Smith:

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

Question 71

White:

In this same vein the Lufkin is still a Vertic Albaqualfs, aren't we still making a decision on use and management that the wet-dry relationship is more important than the vertic at the subgroup level? Can we really do that?

Guy Smith:

You can manage this by having an Ustertic subgroup of the Albaqualfs.

White:

Using a double syllable there, the subgroup.

Guy Smith:

Yes there is. It indicates as to what kind of Vertisols you're intergrading.

Question 72

Nichols:

We have soils in Florida that seem to fit in the Albolls and yet the nearest Albolls are in south central Oklahoma or north central Texas.

Guy Smith:

I wouldn't really have a comment without knowing a great deal more about the soils in Florida.

Nichols:

The soils chemically and morphologically seem to be little different from those in south central Oklahoma, except that they are in considerably more humid climate. Most of the Albolls are in ustic moisture regimes or border-line ustic regimes. We hesitated to classify those soils as Albolls.
Guy Smith:

We have Albolls in central Illinois which have a udic moisture regime or an aquic moisture regime. The Albolls are keyed out ahead of the Aquolls because they straddle the limits of the udic, aquic or ustic/aquic moisture regime. We thought that it was undesirable to split them but if they have the properties of Albolls then I see no reason not to classify them as Albolls. Keeping in mind that the Albolls do have xeric subgroups and probably should have ustic subgroups or udic subgroups one or the other. I would prefer the Ustic subgroup and fix my concept of Albolls on soils that straddle the limit between udic and aquic moisture regimes.

Nichols:

We did take Albaqualfs into Florida on both aquic/ustic moisture regimes but the chemistry fit so well that you couldn't tell by looking at the chemistry of the profile whether they come from East Texas or Florida. That didn't worry me, but the Albolls did, because of the cropping habit.

Question 73

Calhoun:

Diagnostic horizons, soil temperature and moisture regimes and other diagnostic features are, I consider, the basic building blocks for Soil Taxonomy. I guess we can view them that way. Many of these differentiating characteristics, as we are all aware, are not used uniformly at a given categorical level in Soil Taxonomy. However, such decisions as to which categorical level in a specific order a particular characteristic is used may in fact have a temperate/climate bias. If Soil Taxonomy is to be an effective tool for "Making and interpreting soil surveys" as indicated in the subtitle of the book. In the tropics, or more specifically in a specific country in the tropics, would there be some merit in maintaining the integrity of the principles of Soil Taxonomy, using the same building blocks so to speak but possibly shuffling these differentiating characteristics according to the needs of the country? Albeit some countries are very small and others are much larger but still everything is within the context of the political boundaries of that country and the environment, of course, that go along with it are probably narrower in scope than say the U.S. including Hawaii. Adding on to this too and redefining some of these characteristics possibly, for example, soil temperature and soil moisture regime and into ranges that are more realistic for that country. In other words use the principles but restructure it to environments of the country. Now just let me give you an example of what we ran into in El Salvador. The pedologists that I was working with there were concerned, first of all, about the emphases on mollic epipedon. Well first of all, when I arrived in El Salvador I had to convince them that 1) they had mollic epipedons and I also had to convince them that 2) they did not have oxic horizons. After doing that they seemed to be a little concerned about over emphasis of mollic epipedons and having to deal with soils classified as Argiustolls but yet a very intensive agriculture in that country because of limited land resources. Once we got on the slopes those mollic epipedons were easily eroded and in classifying specific pedons too often because of erosion they would classify them back as Haplustalfs, or if it did not have an argillic horizon then it would revert back to an Inceptisol. They were concerned about man's influence making these soils shuttle back and forth between three different orders. That is one example, another example would be in soil temperature regimes. Again looking at some of the natural boundaries in terms of say bean production, of Phaseolus production. Phaseolus cannot even be grown below 400 meters or so in elevation which was sort of right in the middle of the isohyperthermic temperature regime. That was a natural cutoff for them in terms of interpretations for agriculture production. Coffee, for example, also wasn't produced below about 500 meters in elevation, and there were several other natural boundaries of that sort.
There were gradation from East to West in the country in terms of the type of ustic that they had, and in terms of rainfall probabilities and dependabilities. Would you have any comments on those types of observations and the possible merit of that approach?

Guy Smith:

This would amount in effect to having a separate classification in each country. By using the same general criteria so that one could readily convert from the national classification to Soil Taxonomy. You'll never find in defining your temperature regimes, that you will not be able to set any one limit that will fit all crops. It would be necessary for interpretations then in terms of taxonomy to have phases of temperature in addition to the isohyperthermic regime, put in a phase of mean annual temperature of 23 degrees F, or 28 degrees F to 35 degrees F, or what have you. It is legitimate for your interpretations if necessary to have finer subdivisions for specific crops than we have in Soil Taxonomy, all that we have here is a general grouping of temperatures that we could not test in the U.S. We have the International Committee working on this problem at the moment in Venezuela. They are unhappy about the present temperature limits of the isohyperthermic. They want to subdivide that to show the extremely warm one from the moderately warm ones. I would say there is nothing we can do but wait for the International committee to discuss and debate the problems and make recommendations for changes.

Calhoun:

Just as a follow-up in terms of my advice to the Salvadoran pedologist was of course that, 1) the most important thing was to make good soil descriptions in the field and 2) we also set up a laboratory to backstop, and in terms of publications whatever modifications were made in taxonomy to suit the needs of that country that the soils be classified both ways and to the modified local system and also according to taxonomy and in any other system they might be interested in publishing.

Guy Smith:

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

Soils that had been what they called Brown Forest soils that are now Dystrochrepts became Spodosols. So what date then are you going to use if you are going to reconstruct the virgin profile, because spodic horizon formed under man's influence and there are other soils that have been receiving sediments under irrigation or under cultivation from flooding that never had a native vegetation. The only vegetation they ever had was the crops that the farmers grew, and so what sort of cut-off date do you use on those soils, to say this is what it used to be? You have to fix a date. Once you fix it, it is impossible to use it because you are not that precise. These soils that have changed under cultivation, I think, have to be classified on the basis of their present properties, and not on the basis of what somebody thinks they used to be like at some earlier time when there was no one around for recording properties.
Question 74

Nichols:

I would not add any objection in Oklahoma and Texas to correlating eroded soils as other kinds of soil. There are only three series that are Alfisols that are the eroded counterparts of Mollisols. Mainly we looked at the areas and many of the eroded areas still have slightly more than half of the soils that are still Mollisols, and the others are similar so we can use the correlation convention of saying that they are dominantly Mollisols and the others are similar soils. They are encouraging us now to go to more complexes, and to set up more soils. That's in, Charlie, a cover letter to a new Chapter 5 to the Manual. We haven't gone into that much yet, but I don't look for much objection from Oklahoma and Texas on that. There is a related question to the eroded Mollisols. There is a very sizable acreage of soils that are classified as Alfisols that have grassland vegetation. A number of people, especially from the midwest have been worried to find that we have this many soils classified as Alfisols that were formerly Reddish Chestnuts and Reddish Prairie. Does this concern you?

Guy Smith:

Well no, it does not. I'm familiar with the prejudice of the Iowans and the Illinoisi ans about grass vegetation vs. forest vegetation. I know they make a trip down to the southern states and see these grasslands that are classified as Alfisols. I think what you need to do is take them out there sometime when the soil is dry on the surface and ask them to dig a hole. You may make believers out of them because these are hard-setting A-horizons.

Question 75

Nichols:

Some of them are, but there are still sizable acreages that are not hard-setting. Mainly they are fine sandy loams that just don't seem to have quite enough organic matter for Mollisols. They don't fit mollic subgroups as they are currently written. We have had some suggestions that we should rewrite mollic subgroups that cover some of these soils. A little work indicates that you wouldn't cover all of them anyway without including soils that you really wouldn't want to include. There is another related question that I jotted down, i.e., a number of earlier soil scientists gave great importance to A2(E) horizons. Would you care to comment on the lack of use of A2(E) or albic horizon as differentiating properties in Taxonomy? One thing I was thinking about in particular was if you got a lot of proposals to include A2's albic horizons as diagnostic horizons in the various parts of taxonomy?

Guy Smith:

Well, I could sympathize with the desire to use the albic horizon as a diagnostic horizon, I would be opposed to using an A2(E) which is extremely difficult to define. In general we have tried to avoid the use of A, B, C horizon nomenclature in the taxonomy because people don't agree around the world on what is an A, and what is a B, and so on. Mr. Giles had enormous problems in using the ABC terminology in the Desert Project. What was an A1, and what was an A2(E), and so on. The use of the mollic epipedon as a diagnostic horizon was undesirable, but I could find no escape from it to find some horizon or some property that would group the soils of the Great Plains that had consistently been kept together in every taxonomy. That have a great many common properties, but there are exceptions to most any one that you can find.
except of the presence of the mollic epipedon. There was, we commented on this yesterday, the A2 horizons of the Spodosols and how they have been used in various countries to call the soil classified as Podzols if they have an albic horizon. This would have been possible but, I think it is undesirable to group all soils that have an albic horizon into some categoric level because the albic horizon is produced by the removal of something. The processes that remove the clay, that colors the ochric epipedon of most Alfisols would not necessarily, obviously not, the same process that produces the albic horizon above the spodic horizon. There is something very different going on in those soils. The end product may be the same, in that you stripped everything except the quartz which imports the light color of the albic horizon.

Question 76

Nichols:

We have another somewhat related question to Frank’s question about Mollisols in tropical countries. There is nothing in taxonomy that keeps soils out of Mollisols that have quite low cation exchange capacities per 100 grams of clay. Do you believe soils with 12-20 milliequivalents CEC/100 grams of clay by the NH4OAc method should be classified as Mollisols? There were a number of these soils in the arid lands project in Kenya. There was objection on the part of the Dutch soil scientist and the Kenya soil scientist to classifying these soils as Mollisols.

Guy Smith:

We have recognized in a few places the Oxic subgroups of Mollisols. The many young Oxisols, reexamining the question; do the Mollisols of the U.S. and of Europe have mostly 2 to 1 lattice clays? They are on late Pleistocene or even Holocene surfaces for the most part. Minerals are not weathered to the extent that they are in some parts of the tropics. I am quite happy to have the International Committee on Soils of Low Activity Clays and on Oxisols to read them these questions and let them make suggestions.

Nichols:

I think again there when you look at the Mollisols in southern Oklahoma and Texas there are Mollisols with relatively low CEC/100 gm which would be gradational between Mollisols of the cornbelt and Mollisols of the tropics. There isn’t an area of a large gradient jump, it is a variable change.

Question 77

Calhoun:

In the process of keying-out dark surfaced soils of El Salvador, I seemed to always have difficulty in understanding and applying section G.4.c of the keys on page 93 which relate to restrictions on Mollisols of intertropical regions. Could you paraphrase this requirement and also explain why it is constructed in this manner?
Texas Interview

Guy Smith:

This was the problem in Puerto Rico in particular where we had at the base of a slope a Vertisol, which is permitted to have a mollic epipedon, not required but permitted. As we moved up the slope the soils became thinner (the rocks were basic rocks). The soils were clayey with montmorillonitic mineralogy, but they were not Vertisols because the bedrock was too shallow. Going further up the slope we came into rather shallow lithic subgroups of Inceptisols. As we went from the very shallow Inceptisols at the top of the slope to the Vertisols at the base, we had a lot of vertic subgroups that had a mollic epipedon. We wanted to permit these vertic subgroups to be with or without a mollic epipedon. They were all marginal, one way or the other, but we didn't want to force a split in the series as we intergraded from Inceptisols on the upper slopes to the Vertisols on the lower slopes. We wanted to keep that range of soils together in one series. This was the basis for this particular requirement. What we have done there is to define the vertic subgroups. These things could be greatly simplified if they didn't have here and there some soils that straddle one of the limits of a diagnostic horizon, and desiring to keep them as a natural unit we had to permit the presence or absence of the mollic epipedon in the Inceptisols. So you will find something parallel to that in the full definition of the Inceptisols. You won't find it in the key because we have already taken care of it under the Mollisols. The Inceptisols are just, "other soils that".

Nichols:

The original idea was to keep from introducing Mollisols in an area where there were no other Mollisols?

Guy Smith:

This definition was written to take care of the soils where the epipedon is marginal to a mollic epipedon. They just made it or it just didn't. This was a natural unit that we thought should be kept together in the Taxonomy. It takes all of this verbiage here for just a few hectares of soil, practically.

Question 78

Daugherty:

One problem we had in teaching Soil Taxonomy is with the subgroup categoric level. The entire population of soils cannot be handled in the categoric level, there seems to be no hierarchical approach in this category like there is in some of the higher categories. Would you discuss the rationale of this category?

Guy Smith:

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

Daugherty:

The problem in teaching, I think seems to be, as you go from the order to the suborder to the great group, the key that is in Soil Taxonomy seems to be fairly straightforward. When you get to the subgroup level, the keys are not the same, the change is that there is not a spot for every soil that occurs, especially in the tropics.

Guy Smith:

Well, that is correct. The keys are not inclusive in the sense that it provided for every conceivable kind of soil. We preferred to deal with the soils that we know about. When we find the kind of soils that are not provided for then we think we should examine them to see how they should be defined and how they should be classified so that the definitions for the tropical soils commonly don't even list subgroups because we don't have them in the U.S. We don't know enough about them, and we don't want to prejudge their classification. We would rather that they be classified after we have examples of them in front of us, and know something about their behavior. The Typic subgroup is defined with varying precisions according to what we know about the soils within the United States. The subgroups that are listed here are listed because we have series that belong to them in the U.S. If we had no series that was unlike the typic in two or more respects, we did not deal with that particular problem. If, for example, we have soil that is like the typic except for item a and another one is like typic except for item b, the subgroups are listed as we have series. If we find one that is like the typic except for a and b then we have to examine that it may be that we don't want another subgroup. It may be we prefer to define that and include it with the soils that are like the typic except for a, then we say we like the typic except for a or b. These are what you call implied subgroups, in your correlation process.

Daugherty:

How does one handle the problem when there is a subgroup like typic except for a and subgroup like typic except for b, and you have a soil that lacks both of those features and there is no defined subgroup or the defined subgroup only has one or both. There is no hierarchical order in which one you name first.

Guy Smith:

Normally you would examine your interpretations of that new kind of soil to see where it would best go. If it is markedly different from either of the defined subgroups then you set up the third subgroup for it, but if it behaves like one or the other of these two defined subgroups then you modify your definition rather than setting up a third subgroup.
Texas Interview

Question 79

Allan:

If I have interpreted the requirements for a mollic epipedon correctly, the soil cannot be both hard or very hard and massive. First of all is that correct?

Guy Smith:

That is correct, when dry.

Allan:

I find some field soil scientists that say it has to be either of those and, of course, I never thought that was what you meant by Soil Taxonomy. Have there been problems in interpreting these criteria in the field? It always seems to me that the distinction between massive and weak structure is rather difficult, sometimes, to determine, and I think that there is also quite a problem interpreting consistence in the field. I was wondering if you had any comments along these lines.

Guy Smith:

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

Nichols:

There are some of those soils in Texas. One other characteristic is there is a counterpart of Alboll in Oklahoma. That Alboll in Oklahoma is prized for wheat, scarcely hardening when cleared and planted to wheat, yet when you encounter the soil in Texas most of it lays out in weeds or grasses so that the farmers know the difference. It does fit the requirements of the mollic epipedon except that it doesn't have structure and it is very hard when dry. Apparently it is also droughty.

Guy Smith:

The farmers know a great deal more about the soils on their own farms than we do, and make much finer distinctions.
Question 80

Allen:

Continuing along the same line I knew that you had mentioned some of these California soils in some of your other discussions. I assume that those were in mind when the taxonomy was developed. Since your discussion here, you have mentioned something along this line in Australia. I was wondering if you have encountered similar soils in other parts of the world, what I am getting at is just how extensive are such soils?

Guy Smith:

Very extensive in Australia. Quite extensive in parts of Spain where parts of the epipeds are still present, that is, the original A horizon. I haven't studied personally the soils of the middle east although I would expect them to be there. I have seen very few of them in South America. This may be largely because of the soils of xeric moisture regime are pretty much confined to the West Coast which is largely covered with ash. I don't recall seeing them in Venezuela or in the West Indies.

Allen:

In your experience though most of them that you have seen have been of the xeric moisture regime.

Guy Smith:

Or ustic, in west Texas we have them. Then, of course, the Alfisols and Ultisols that have not been truncated would have this hard-setting A horizon if they ever became dry. We don't notice this because they are so rarely dry. If you go to southern Illinois the Albaqualfs occasionally become dry and it takes ten minutes, perhaps, to get an auger through the A horizon into the argillic horizon below, you grind and grind and grind and can't dig it. These do become dry occasionally in some years. This would be a characteristic of Alfisols and Ultisols if they should become dry and if you do have this problem with soil structure with these soils, one of their characteristics is that you have structural problems.

Question 81

Calhoun:

In reference to this, what about the West African Alfisols in ustic soil moisture regimes, such as Niger, Upper Volta, and Mali as far as hard-setting properties, there seems to be a severe problem there too.

Guy Smith:

I haven't traveled in that part of the world, but I would expect that it would be.
Allen:

I saw one soil in Niger that probably would meet this requirement, most of them are not and I also thought that this would not have a matric horizon, but we didn't do anything about this.

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Question 82

Daugherty:

Would you discuss the intent of the anthropic epipedon and the limits that were used?

Guy Smith:

The original intent was to deal with the kitchen middens of the Indians in North and South America and of the early settlers in Western Europe. The nomadic people who settled for periods in one spot year after year would bring in shells and animals and the bones would be thrown on the soil and in time it developed a soil that had the appearance of a Mollisol. Although the surrounding soils might all be Alfisols, these would be perhaps an acre or something like that, maybe five acres, not much larger. When Roger Bray was working on his phosphorus tests he sampled one of these and was astonished to find that this test didn't work on these soils. This puzzled him because he got no blue color whatever for phosphorus. Yet he could see bone fragments in the soil. He finally discovered that he had so much phosphorus that it precipitated all his reagents. This was the basis for thinking that we might separate these from Mollisols by the phosphorus found in it. The soils are rare enough that very few people have studied them in the U.S., and so we went to Europe for their experience on comparable soils that had formed under the early pre-historic settlements. They proposed the phosphorus limits that we used there, and we accepted their proposal because we had no data on soils in the U.S. When I went to Venezuela and checked over the soils that they had sampled and analyzed, I found quite a few soils that I thought should be Aqualfs but that had the phosphorus required for an anthropic epipedon in the Orinoco Valley. The headwaters have deposits of rock phosphate. The stream sediments coming down originated in such an area that you could have many times the phosphorus that is permitted in a mollic epipedon. They couldn't blame that on refuse from the kitchen midden, no Indian squaw would tolerate a seafood made at a camp in a swamp. This is just a sedimentation. That is not the intent of the anthropic epipedon. When I examined the phosphorus distribution on those soils that were very high. I found that sediments would come one year from one stream and another year from another stream, and just like the carbon decreases irregularly in the Fluvents, the phosphorus abundance was irregular with depth, it didn't necessarily decrease it or increase, but it was a very irregular thing. I proposed in Venezuela that we modify the definition so that such soils would be excluded from having an anthropic epipedon. The definition of the anthropic epipedon would require the phosphorus be decreased regularly with depth.

Allen:

I am very interested in your comments along this line and I have a note down here to ask you about it because I ran into a similar situation along the Rio Perco in New Mexico, in that most of the soils we sampled and some of the old Spanish fields established by the Spaniard nearly all of them had greater than 250 parts per million, P<sub>2</sub>O<sub>5</sub>. On this particular project we didn't sample at depth. I don't know whether we had this erratic distribution or not. I don't think that it was human habitation in each field that caused the high P<sub>2</sub>O<sub>5</sub> content, and I have no way of telling. What you are proposing is that these be sampled at depth to see whether it was an erratic distribution.
Guy Smith:

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

**Question 83**

Daugherty:

Could you address the mollic epipedon that might form in arid regions as a result of long-term irrigation from Indian habitation?

Guy Smith:

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

**Question 84**

Daugherty:

On these same lines will you discuss the plaggen epipedon?

Guy Smith:

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

**Daugherty:**

Specifically, my intent in asking the question is how you keep them separated from the field that has been mechanically leveled, or where part of the field has been moved to another part of the field, or from a large cattle feedlot where soil has been built-up through the years to say a meter or more than it has been in the past?

**Guy Smith:**

I haven't looked at the cattle feed lots. These soils have dark colors because of the nature of the sods which came mostly from [?Humods?]. They are full of artifacts, chunks of brick, tile, and what have you, throughout the whole plaggen layer. Not just on the surface but throughout the soil. If you examine them in a pit you will have no trouble in seeing the spade marks and the fine stratifications that form in a spaded field after a heavy rain at considerable depths in the soil. It is obviously a greatly over-thickened plow layer. Its identification in the field in Europe is commonly based on the change in elevation which follows the line in the old fence line of the infield, the Scotts call it, that is, small field near the house where the food crops were grown. In identifying it in Europe, it is the simplest thing, as you approach it, you know what you are going to get before you get there.

**Question 85**

**Daugherty:**

While we are on the subject of asking questions about man-influenced soils, I would like to ask another question. There has been a drive by some to set up the new suborder of Entisols called Spolents. This would be a suborder which would deal with mine spoil. Would you react to this means of handling mined land and indicate how you would classify disturbed land?

**Guy Smith:**

I don't think I have had enough experience with these soils to have a valid opinion. I prefer to leave this to the people who have to make maps of them now and have an opportunity to look at them. The strip mines of Illinois used to be wasteland, actually. They didn't level it because that was not required and I would say they certainly had no diagnostic horizons. They were not bedrock anymore, at least they may have been at one time, but having piled up the overburden in these huge ridges, there was enough fine-earth material to support trees and other vegetation if it wasn't too acid. I would be inclined to put them in the Entisols but what suborder if they can define Spolents it is all right with me. The problem would be to watch your definition.

**Daugherty:**

Under the current system in some cases they would fall out as Fluvents because of the erratic distribution of organic carbon with depth.
Texas Interview

Question 86

Nichols:

I have the same question. There has been considerable discussion and the problem seems to be between those people that think the Arrents and Orthents are adequate for the classification of these disturbed soils, and the group on the other end that want more specific classification even to the very fine breakdown such as Garbents to define the kind of material that is there. It appears that your question was very well thought out and very well answered because there are not many people that would be willing to step in and start to moderate between these two extreme groups at this time. It is a real problem.

White:

In that same vein, Texas is now undergoing large chunks of land that are going to go in lignite mines by strip mining in which as the mine moves, the actual mining face, the material taken out has be "reclaimed". Let's move twenty years down the road which is the expected life of a given plant, like that just outside College Station, which is going to be big. Now we have all this material to put back and in that plant they are not going to save the topsoil and put it back. It is going to be a total mixed situation. What do we do with a soil like this now? Now there are no diagnostic horizons. I don't know whether the organic carbon is going to be enough that will give us the erratic distribution of depth. I doubt it. Looking at some of the stuff that comes out of the deep borings that we have. What are we to do now? We are going to have very large parcels, thousands of acres? Where do we fit them into the system, or how do we attempt to fit them into the system?

Guy Smith:

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

Daugherty:

I suppose I could rephrase the question. What do we do philosophically? Then within the bureaucracy and hierarchy of the Federal Government. How do we approach this from that standpoint? If somebody comes up that has been working in this area, how do they try and propose a solution to this, and kick it up the ladder, so that it can be used relatively universally? Especially in the United States now, with the new EPA ratings we are not going to have much plain old mine spoil as we have in the past. I see a strong cultural change there. What do we do philosophically or bureaucratically?

Guy Smith:

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

Daugherty:

I am thinking of the debates in California on the gold mining tailings from the dredges that have now been mined four times and now I have heard that the Yuba River Mine is open.
again and they are dredging it for the fifth time. There is no consensus of opinion as to what
to do with that other than miscellaneous land areas.

Allen:
Back to the plaggen epipedons in Europe that we were discussing. I guess it goes without
saying that you end up with rectangular areas on the soil maps.

Guy Smith:
Yes, at least straight-sided and not necessarily rectangular, maybe triangular or something
of that sort. The plaggen epidon does stop mainly along the fence line.

Thompson:
Leroy has probably seen the same thing in areas where there has been drastic surface
modification through land-leveling. We have some straight land fill in the lower valley in
Hidalgo County. We have some straight soil lines because of that cutting and filling operation
that they do in land-leveling.

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

Question 87

Daugherty:
Somewhat in relation to the question that I asked about Spolents, I guess it's kind of a
philosophical question also. Do you feel that the parent material differences will be dealt with
in future changes in Soil Taxonomy?

Guy Smith:
While insofar as parent material is related to mineralogy, particle-size distribution, and so
on, it is I think fairly well taken care of in broad classes at the family level and you can still
make as many subdivisions as are needed at the series level. We are not, I think, so much
concerned with the parent material as a rule as we are of the nature of the soil itself. In the
southeast on the Paleudults the parent material isn't too important any more. The thing that is
important is the mineralogy and particle-size of the present soil. To find out what the parent
material was is going to be difficult because you may have to go down 50 feet or so to find
weatherable minerals in some of those. Was the surface mantle the same as that deep layer?
We don't know that. More and more, we are finding that parent materials of soils is not what
we thought it was. Soils that were supposed to have been developed in one parent material or
another we find have significant admixtures of aeolian and fluvial materials at the surface.
Texas Interview

Question 88

Allen:

Dr. Smith, I would like to ask you to review what has developed and some of the rationale behind the proposal for a new order of soils called Andisols? Some of us have seen this document and probably some have not in which this proposal was made and I would like to get some of the background.

Guy Smith:

The trouble started for me when I spent a year in the West Indies. There are no particular problems on the islands from sedimentary rocks, but on the volcanic islands there were serious troubles with the classification, in that, at the family level I was unable to make any interpretations whatever. There were a number of difficulties with the classification of the Andepts as presented in Soil Taxonomy. I will not try to list them all here since they are listed in my memorandum on the new order. One was the use of base saturation by ammonium acetate at pH7, because the exchange capacity is so strongly pH-dependent it became very difficult to get a base saturation of as much as 60 percent unless the pH of the soil in the field was pH7 and above, then the base saturation would exceed 50 percent or well over a 100 percent in some. Another serious defect was the over emphasis on color, particularly color value. The original concept of the Andepts came from the concept developed in Japan of the Korobuco(?), soils which have very dark colors, fairly strongly weathered, and very high in their percentage of organic matter. We had on the island of St. Vincent in the Lesser Antilles a series of eruptions in 1902 and 1903. The north half of the island was blanketed by a rather thick mantle of black cinders. The color of the parent material was black before it was weathered. In the 75 years that had elapsed after that eruption a number of them had accumulated more than one percent organic carbon, and so although they were very coarse in texture they were principally black cinders that came out with the Andisols, the Korobuco(?) soils, because the colors were the same, but the organic matter was not the same. The further problem had to do with the particle-size class which alluded in the classification of the Andepts. We used a combination of mineralogy and particle-size, rather than strict particle-sizes, but we had too few classes that we could not distinguish between cinders and pumice. There is an enormous difference in the available water-holding capacity between these two materials. So we needed another set of particle-size classes. We needed to de-emphasize the base saturation, and we needed to give more emphasis to the milliequivalents of exchangeable bases for 100 grams of soil. We had in New Zealand many Andisols whose total exchangeable bases plus aluminum in materials that have a feeling of a silt loam, less than 0.2 of a milliequivalent total bases plus aluminum or a ECEC. Where we had a mixture of pyroclastic materials plus other materials as we have in much of the alluvium and some of the loess in New Zealand and in the U.S. we have a fairly high ECED, but the bulk of the ECEC is the form of KCl-extractable aluminum. These are extremely high in aluminum compared to the bases. These were properties that certainly were not brought out in the Soil Taxonomy classification. Further, the soils from pyroclastic materials are found in any environment, from the arctic to the tropics, from the desert to the per humid tropics. Soil moisture was not used, soil temperature was used only at the family level. We were short one category because the Andisols were only a suborder. We were short the category at which we normally brought in moisture and temperature regimes. That meant that we had to combine these things at some categoric level or raise the suborder to an order. It seemed much easier to propose a new order than to find some way, perhaps at the subgroup level to bring in the moisture regime, or perhaps, even at the great group level it might have been done. We also had to get rid of base saturation and substitute total bases for it. These were the principal problems that come immediately to mind, but in my proposal I had to define my particle-size class terms, my mineralogy terms, because I could not use them exactly as they are used in the AGI Glossary. The geologists make a break at 4 mm and the pedologists at 2 mm. It didn't seem rational to adopt the AGI particle-size classes for soil science. We retained our present size limits for gravel. We had to either redefine pumice or substitute another term for something with the same properties but from a more basic magma. The AGI terms restrict pumice to materials that
are nearly white. If you have the same vesicular materials from a more basic magma, the colors are not white they become quite dark, in fact. The brittle vesicular nature, the very low apparent bulk density of pumice and pumiceous-like materials are quite important in their engineering uses, and even more important to the growth of plants because they will store so much more moisture. A pumice blanket that has say a mean particle-size particle diameter of 10 cm or more can still store the whole year's rainfall in New Zealand in an available form. The foresters have studied the growth of the Ponderosa Pine and measured the moisture extraction and it will store well over a meter of water within the rooting zone. Whereas, a skeletal material that is composed of rounded chunks of granite will store virtually nothing and yet particle-size is virtually the same. The mineralogy can be the same in the two. The basic materials are quite full of glass. The soft gravel, for example, will not store water, but it is just about as pyroclastic as the pumiceous-like materials that are blown into the air. The whole proposal is being considered by an International committee as I have mentioned with about 75 current active members, and it is being published in the book that the New Zealand Soil Science Society is publishing on soils with variable charge. There will be one chapter that will include that proposal.

**Question 89**

Daugherty:

Will you discuss lithologic discontinuities and to what extent their identification specifically were to apply and be used in *Soil Taxonomy*?

Guy Smith:

We are concerned primarily I think with two distinctly different situations. One, there is a serious change in the pore-size distribution which causes water either to hang above or below the lithologic discontinuity. If you have silty material over sand the water will perch in the silt and with only difficulty enter the lower material. If you have sand over silt the water will perch where the pores become smaller. In either case if there is a marked contrast in pore sizes this concerns us at the family level. The other situation is in the identification of an argillic horizon where you have a finer textured original material at some depth in the soil so that there is some inherited clay, and the change in the percentage of clay may be entirely due to the stratification of the parent material, or it may be in part due to the stratification and in part due to accumulation of translocated clay. Different people recognize lithologic discontinuities at different intervals and some people can see them in nearly every soil, and some people can almost never see them. This is not a problem with an easy solution. The definition of the argillic horizon is intended to allow one to bypass the percentage required for an argillic horizon by substituting the percentage of clay skins in the finer-textured material. On the field excursions of the committee on low-activity clays where the identification of an argillic horizon was commonly a problem in the field; some of the participants would see several discontinuities in the data of the same profile where others would see none. How significant these changes should be before one abandons the use of the percentage increase in clay, I frankly do not know. Most of my experience in northeast, midwest states rarely gave us many problems. We did have some terrace soils that appeared to have a clay pan and yet there was no evidence in the field of any translocation of clay, because the late Holocene terraces seem to be purely a stratification. In general we didn't argue much about presence or absence of argillic horizons in the Alfisols or Mollisols that had 2:1 lattice clays. When one gets into a group of soils with 1:1 lattice clays as in the Paleudults of the south east or in a number of the intertropical soils the evidences of clay translocation are not so clear. The best evidence I know is the more or less abrupt irregular boundary between the ochric epipedon and the finer-textured argillic horizon below. The clay moved so long ago I suppose that the clay skins have
all been disrupted now by animals and roots until you get into the horizons a couple of meters below the land surface where the biologic activity has been minimal. There you can begin to pick them up. These are among the kinds of soils that the committee on Alfisols and Ultisols with low-activity clay have been evading now for a number of years. The final report is due about June of this year. The proposal is that that will be distributed for testing in parts of the world where these soils are common and after a year of opportunity for testing the comments are due in Washington and the final decision will be made on what changes to make. They are proposing a number of new great groups with the formative element "kandi" to imply the low activity clays. Not all of them really are kaolinitic some of them are mostly free oxides. "Kandi" will be used as indicative of low-activity clay.

Question 90

Allen:

Another question on lithologic discontinuities. I have no problem when I can see the change in the field and is confirmed by the laboratory data. I am concerned when I can't see this in the field and yet I get changes in sand/silt fraction ratios, and so forth, in the laboratory. I have never known whether those warrant the lithologic discontinuity designation or not. I wonder if you would comment on this. In other words, if it is not enough that you can see in the field, should we put that distinction on?

Guy Smith:

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

Allen:

Well, just to put a point along that line, most of our soils here on the high plain shows certainly some differences in sand/silt-size fraction ratios. When you think how they were farmed on this aeolian mantle, then that is to be expected. But very often I can't see any indication of a lithologic discontinuity in the filed.

Question 91

Thompson:

I want to ask a question about the cambic horizon and you perhaps have already addressed this previously, but let me state it and then if you don't choose to answer now that will be fine. From the definition of cambic horizon, the thickness criteria is at the base of the cambic horizon at least 25 cm below the soil surface, unless the soil temperature regime is cryic or
pergelic. Why was this criteria on thickness used instead of perhaps a similar criteria used in plotting the thickness of argillic horizons?

**Guy Smith:**

The thickness limit was waived for the very cold soils which we thought would be unlikely to be cultivated. We likewise had some variability in the spodic horizon in such soils. In general throughout the *Taxonomy* as a whole tried to keep the virgin and cultivated soils together in the same taxon. The 25 cm limit to the base of the cambic horizon didn't seem too unreasonable for many of the soils in the midwest and the north-eastern states. One could have argued that the Camborthids should have had a similar limit to the pergelic soils because they are not likely to be cultivated but chances are much greater with the Camborthids if somebody is going to irrigate and plow and the cambic horizon is such a weak sort of a diagnostic horizon that it doesn't seem to make a great change in properties of the soil if a thin cambic horizon is plowed up if it is very similar to the virgin soil. It is not similar to what happens when a thin natric horizon is plowed where the whole horizon is gone and has a much greater impact on soil behavior. These are soils that if the slopes are suitable Camborthids certainly can be plowed everywhere there is water. The serious problem is the water. Would Gile like to comment on that? On his Desert Project he was going to have some very thin cambic horizons.

**Gile:**

Yes, the original depth of the cambic was 20 cm (8 inches). When that dropped to 25 cm (10 inches) we lost a lot of cambic horizons. I remember asking Guy at the time the rationale of this and his answer was as he has given it here. As compared to the argillic horizon it would not be as strongly developed and for that reason they put the depth down to 25 cm.

**Guy Smith:**

You are more apt to have a cambic horizon of some sort in a zone of alteration if you put the depth down to 25 cm. The base of the cambic horizon, of course, is not easy to determine unless you have either dominance of rock structure or a strong accumulation of calcium carbonate. In non-calcareous alluvium the base of the cambic horizon is about as difficult to determine as the base of an argillic horizon. It is not in itself an important horizon in that it has much effect on the plants that grow or the structures that are put in.

**Question 92**

**Allen:**

A further point that I would like some clarification on if possible about the cambic horizon. I have great difficulty in the skeletal soils in determining whether there is structure of pedogenetic origin, or whether peds that I am seeing are merely because of the coarse fragments. In fact we used to use the term occasionally roc-controlled structure.

**Guy Smith:**

I still do.

**Allen:**

Do you have any guidelines along this line?
Guy Smith:

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

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**Question 93**

Daugherty:

In most soils the temperature is used at the family level. Could you discuss the rationale for using the temperature at the great group and suborder level in such things as Cryoboralfs and Tropepts?

Guy Smith:

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

Daugherty:

Would you discuss the rationale for not using such features as soil climate at the same categorical level throughout. In other words, soil climate is not used at that level in the Entisols.

Guy Smith:

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

Question 94

Allen:

Somewhat of a follow up to Leroy's question. I have great difficulty in explaining to students the bringing in of moisture at the order level in the case of Aridisols. I think I know why it was done, but I am wondering if you can clarify this so it would help me when I am trying to tell students why this is so. Up to that point we use diagnostic horizons, by and large, but when we get to Aridisols we have got to have that aridic moisture regime.

Guy Smith:

Well, I would like to quote Dr. Kellogg on this. One of the most important boundaries on soil maps is the limit between the sown and the unsown. The land can be cultivated and the land that can only be grazed, and so it seems to us that it would be useful to have an order that included the bulk of the soils that were too dry to be cultivated and that did have some horizons. Now in the Entisols you have that same border, but it comes at a lower categoric level.

Question 95

Thompson:

I would like to ask a question on Vertisols. In Vertisols the definition includes a statement that at some depth between 25 cm and one meter slickensides are close enough to intersect. Have you ever seen soils that had slickensides that did not intersect?
Texas Interview

Guy Smith:

Yes. In the Fargo Lake plains, the Red River Lake plains in North Dakota and Minnesota we have very fine-textured montmorillonitic soils with a udic moisture regime, really, they rarely crack seriously, very little movement in the soil itself because of lack of serious moisture changes. In these soils in a given pit you may find one large slickenside that runs for a meter or so at least, at a much less angle to the horizontal than we get in most Vertisols but very well developed slickensides, but there may be only one in a pedon. Whether these that we find are due to frost or to occasional wetting and drying, I don't know. It is very difficult to be confident in these soils that freeze so deeply as to whether the freezing has produced the movement or the shrinking and swelling due to moisture changes.

Thompson:

I have one other comment over soils or questions for you and this is going to be strictly an opinion, I suppose. Do you have reservations about classifying soils as Vertisols that occur on recent aged material such as the Trinity River bottom in north central Texas?

Guy Smith:

I suppose I would not, provided the soils met the definition. You say they are recent and I don't know how recent.

Thompson:

Very recent.

Guy Smith:

I see no reason why one couldn't if he had the proper parent material developed an Entic Vertisol in a relatively short time.

Thompson:

This is a comment we won't carry any further, but the reason I asked the question about the slickensides close enough to intersect is that at one time the thought prevailed that these materials had not been in place long enough to have formed the features diagnostic of Vertisols even though if you dig a pit it had all the features including gilgai microrelief if you really want to look for it. The way we wrote them out of Vertisols was that we said that they had slickensides that did not intersect. This is simply not true. They did intersect, all over the place, and we have changed our philosophy and thinking and we do recognize those soils now as Vertisols. I was just curious as to your philosophy on the Vertisol order as to whether or not it could be applied in very recent materials provided the other criteria were present.

Guy Smith:

I would not see any reason why they would differ markedly in behavior from other Vertisols in the same family. They would on the Trinity River perhaps, be flooded occasionally, whereas on some upland positions that would not be the case.

Nichols:

Likely they do not have the cyclic situation where the C horizon comes very higher in part of the pedon.

Thompson:

Well Joe, they very likely will not because they are formed in the sediments from the Black Land prairie and they were black to begin with. You do not have the situation that you
have on the typical Houston black clay formed in marl where you get the chimney. I think they are there, I just don’t think we can see them.

**Question 96**

**Daugherty:**

I would like to draw your attention to p. 156 in *Soil Taxonomy*, the lower right hand side. Would you discuss the background of the use of the organic carbon sand/clay ratio nomograph used for the Ustollic subgroups of Aridisols?

**Guy Smith:**

I thought we went over that yesterday. I recall answering that same question.

**Question 97**

**Daugherty:**

I would like to ask a question in terms of how did you establish the organic carbon level you used to separate mineral and organic soils?

**Guy Smith:**

That was basically taken from the European experience. In the '38 Classification we had a rather vague definition and the organic soils, I think, were supposed to have only 30 cm of organic material. The people who have done most on this in Europe are the Dutch. The American classification of organic soils was extremely weak in the 1938 Classification. In Marbut's Classification they didn't exist. We use then in the U.S. for classification of organic soils mostly the history of the bog, as revealed by different layers at different depths, and the nature of the so called plants that grew in the bog. Woody peats vs fibrous peats vs other kinds. The limit then was one that had been worked out by the Dutch who had sampled and studied their Histosols much more carefully than anyone had ever done in the U.S. The limit comes directly from their classification.
Texas Interview

Question 98

Allen:

Statements are made in *Taxonomy*, that the classification of the Histosols is not well developed. What do you perceive as progress or other committees studying the classification of the Histosols similar to what you have mentioned for some of the other soils?

Guy Smith:

I know of no committees that are studying the problems of the classification of Histosols. One of the main troubles was that we had our series defined in completely different terms than we used in *Soil Taxonomy*. The series in Histosols required revisions before we could test anything that was being proposed. How far along the correlation staff has gotten in redefining their Histosols series, I just do not know. There is no International committee working on them. It seemed likely when we published *Soil Taxonomy* that we had provided for a lot of subgroups on a theoretical basis that we thought they might exist so we couldn't test the numbers of subgroups that we had. In the long run I think we will have fewer and fewer subgroups of Histosols instead of more and more.

Nichols:

We don't have any big problems in using them.

What is in *Taxonomy* is working pretty well for the South region.

There are two things involved. With mineral soils you have a predictive value from the landscape in most cases except with fairly recent alluvial soils and the soil scientists have even learned to make the predictions there. There is a problem of prediction and investigation. It is quite difficult to make investigations on these soils and the tests you have to run are somewhat time-consuming. There is just an amount of detail that you can gather in your mapping procedure that is practical. I don't know that we have any problems, we seem to have about enough subgroups now, and no doubt we will have a few more series but probably not a great deal more as long as they are in natural conditions. With the laws on wet lands and the coastal zone management act it isn't likely that they are going to be draining any more of those.

Question 99

Calhoun:

I have a question that pertains to the Ultisols, if you will just look at page 349, chapter 16, it is item 1 under the definition of Ultisols. It says they are mineral soils that "I. Do not have tongues of albic materials in the argillie horizon that have vertical dimensions of as much as 50 cm if there is greater than 10 percent weatherable minerals in the 20- to 200- micron fraction." My question on that is, I never really had to struggle with that in the Ultisols that I have keyed out in the past but I am curious as to : 1) Why we have to discuss tonguing, vis-a-vis, the Alfisols vs. the Ultisols, and 2) why that is tied into percent weatherable minerals?
Guy Smith:

This is intended to keep out of Ultisols the Glossudalfs that have base saturation slightly under the limit between Alfisols and Ultisols. We wanted to keep all the Glossudalfs together. So far as we know they were all formed in Holocene materials mostly in loess. I have seen a few in solifluction materials. They just straddle the limits between Ultisols and Alfisols in terms of base saturation. The weatherable minerals were in there because, as I say, they mostly are in loess but they are in very late Pleistocene materials. We have Ultisols that have tonguing of albic materials that are very strongly weathered in soils where the B horizon apparently has formed and then undergone serious destruction and reformed another argillic horizon at a greater depth. These are mostly classified I think as Paleudults in the U.S. This was the definition that was suggested by those from Belgium to keep their Glossudalfs out of Ultisols to avoid splitting them between Alfisols and Ultisols.

Calhoun:

So that was more of a European problem?

Guy Smith:

It is an actual problem in the lower Mississippi Valley, I think, that we have these Glossudalfs there, they have only been reported to me, I don't remember seeing them. I have seen them in Oregon where they are again in loess.

Calhoun:

In South Carolina there is one series that would probably have to be split if Glossudults were set up unless weatherable minerals are taken out.

Guy Smith:

There is one way to try to simplify the definition and that is to delete the first statement in the definition because there are so few of these in the world.

Nichols:

If you simplify the definition you make the landscape more complicated for people trying to work with it.

Question 106

Nichols:

While we are mentioning Udults, the statement for pale great groups does not require solum exceeding 1.5 meters in thickness if the soil does not have lithic or paralithic contact within 1.5 meters nor a decrease in clay by more than 20 percent. This has worried a number of soil scientists who thought that all Pale great groups should require thick sola. Would you care to comment on that?

Guy Smith:

We thought that the Paleudults as they reflected a soil of great age should not have rock within 1.5m of the soil surface. That was the first thought. One can stop the solum at the rock
there is no problem on that, but otherwise, if there is no rock it is very difficult to decide where the solum stops. At one time we had a statement in item b [in definition on page 349] that required, people thought, that we identify the base of the argillic horizon which presumably would be the base of your solum. We had to take that out because that is a limit that pedologists of equal competence can disagree violently upon very widely. As it now reads the definition does not require that one determine the thickness of the solum beyond the depth to the rock or the base of the argillic horizon. In the Ustalfs we don't have precisely the same definition for the Paleustalfs or the Paleargids though there are some similarities.

Question 101

Daugherty:

I would like to ask a question concerning the background on the classification of such soils as Argialbolls, the old Planosols. Why is the mollic epipedon allowed to be split by an albic horizon?

Guy Smith:

Again, that is in order to keep similar soils together in the classifications. Some of the Argialbolls have an albic horizon within plow depth and some do not. Some of the cultivated ones, then, are going to lose their albic horizon the first time they are plowed. We don't want to change the classification because of plowing, as I have expressed a number of times. We do like to keep the similar soils together when they are marginal on the limit between taxa. The Argialbolls typically have a mollic epipedon that is thick enough to qualify without considering the nature of the argillic horizon below, below the albic horizon, but a few soils do have a very shallow albic horizon and/or a very thin one. The photograph on page 107, plate 11D, shows that the base of the albic horizon is about 25 cm. From the looks of it, it is about 15 cm to the top of the albic horizon. So if this got plowed just a little deeper than 25 cm it would lose that albic horizon. Let's go to the Argialbolls to see how we handled that. We didn't specifically address that problem in the key to Albolls pg. 273 if we mixed that whole albic horizon up it would drop out of Albolls, wouldn't it, by the present definition. It doesn't have a thick enough mollic epipedon, as an A1 horizon, to qualify as a Mollisol. If we add the A1 plus the upper part of the argillic horizon it would qualify as a Mollisol.

Question 102

Allen:

Dr. Smith, this may be more of a soil genesis question than a taxonomy problem, but because of your experience in so many different parts of the world, I would like to mention an observation to you and see what you think. I have already discussed this with Frank over the telephone. In a short visit to Niger, I sampled a number of pedons in a rainfall belt of approximately 400 mm. I was surprised in two ways; first of all, no free carbonates to a depth of 2 meters and secondly, a base saturation percentage of less than 35 percent in most cases. The soils had a weakly developed argillic horizon, textures for the most part, however, were
loamy sands and sandy loams throughout the profile. I am just wondering if you have encountered such soils, and if so, what kind of explanation you might have? Of course, again I am biased because of my experience in west Texas, and certainly not expecting this type of thing.

Guy Smith:

We have such soils in Venezuela. They are not at all uncommon where the Ultisols grade up against the Aridisols. The rainfall is just marginal between ustic and aridic and the base saturation is very low. The explanation, of course, is hypothetical, but we do notice that the rainfall pattern, the soil-moistening pattern, is very different in these intertropical regions where we have no summer and no winter, but we have a rainy season and a dry season. The rainy season may give you your 400 mm of rain within three months, which is enough to moisten the soil and send a little more on down below the limit of where we set the base saturation determination. We even have Venezuela Aridisols with pH's of about 3.6 in water. How those developed is purely speculative at the moment, but there are also some in Wyoming. Aridisols with those low pH's. These are developed in the U.S. in materials that have a fair amount of pyrite. In Venezuela in the soils I looked at, I was unable to find any pyrite in the rocks below the soil, but we had 6 milliequivalents of aluminum in the saturation extract with conductivity of about 15 or 16 something like that. Where the aluminum came from I am not sure because I had expected when we went to look, to be able to identify the pyrite in the rocks but I couldn't. I think it is the rainfall pattern that basically produces leaching when you have a rainy season and a dry season. In most of Africa you don't find Ultisols, you find Alfisols in this situation. Most of South America you find Ultisols in these wet/dry climates if they have any age.

Calhoun:

I gave the explanation of a concentrated rainfall in a short period. I mention this here as the only explanation for this. It had a lot more effective leaching power because of that concentration than it would have in most places. Just one further comment - I think these are like the Venezuelan soils. It isn't the geologic setting for these soils to have pyrite in them. So I don't think that is the answer. I think they are truly leached.

Guy Smith:

The Aridisols obviously are not leached with a conductivity of 15 mmhos.

Calhoun:

Based on our comments right now about ustic soil moisture regimes in intertropical regions, those areas of the world that respond to the intertropical convergence zone vs ustic regimes that are more typical of temperate regions where we normally make ustic on the basis of cumulative days rather than consecutive days and we traditionally make them on the basis of both in tropical regions. Would you think there is merit, and I am sure this is being considered now by the International Committee, in recognizing that difference? It is important in crop production and certainly very important as far as soil genesis is concerned also.

Guy Smith:

We would eventually. The current definition of the ustic moisture regime as applied in the U.S. puts a very different set of moisture conditions in with the wet/dry tropics. Here the growing season is controlled by temperature and moisture. You get your maximum rainfall here during the summer and spring when you have the maximum growth. In the intertropics or tropics then is no such control of temperature. The International Committee under Dr. Van Wambke is surely considering this. In their circular letters they were talking about a tentative name for what they should use in the tropics.
Texas Interview

Question 103

Allen:

This next question was relayed to me by Dr. Ronald Paetzold who is with Agristars Soil Moisture Program now in Beltsville. What was the rationale in choosing 10 years for the soil moisture regimes, such as 6 out of 10 years, rather than a longer cycle because there are data that suggest that there are weather cycles from 20 to 25 years, especially dry cycles. That has been repeatedly mentioned by people in this part of the world. Some of the data pretty well suggests this.

Guy Smith:

For dry, in 6 out of 10 years, that was one way of saying, in most years. If I used percentages then I get an extra decimal, that is not significant. I can’t say 60 percent because then 59 percent is less than 60. If I say 6 out of 10, then 59 percent of the time, rounds off to 60.

Allen:

So the 6 out of 10 years really had no significance except to get "most of the time."

Guy Smith:

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

Question 104

Daugherty:

The fragipan has been rather confusing to a lot of people and it has had a lot of controversy. Would you comment on what you perceive as to what its genesis is, and the intent for usage as a diagnostic horizon in Soil Taxonomy? Some states have used Bx horizons and some states have used Cx horizons.

Guy Smith:

We haven’t said that a fragipan is a B or a C horizon. It may be either as far as I am concerned. Some soils are obviously a B horizon. I don’t know that it is so obviously a C horizon in many soils, but it could be in a Spodosol for example, because the C horizon is not well defined. In a Spodosol you stop the B horizon when the color changes and you start C horizons. Actually there has been a lot of alteration in the fragipan. I think I said all that I knew in Soil Taxonomy. The genesis is not clear as to why we get this compaction other than that it is a zone of low biologic activity. The soil is not frozen even though it has a cryic temperature regime. If you have a fragipan you will find the soil doesn’t freeze to that depth.
because of the snow mantle. There is no frost action to loosen the soil. It is virtually free of small animals except, perhaps, in the cracks between the polyhedrons. It is also virtually free of roots except in the same place. The roots are frequently very flattened in these cracks indicating that they are unable to compress the soil any further, it is as compact as pressure of the growing root can make it. I would comment that I have learned a little bit about fragipans since I wrote Soil Taxonomy. In New Zealand I found fragipans are normally in soils that have an ustic moisture regime not a udic moisture regime. It is so typical in New Zealand that if they do find a fragipan in a soil with udic moisture regime they think they must be misjudging the moisture regime. These are in noncalcareous sediments, mostly loess. Fragipans like loess in glacial till in particular, noncalcareous, primarily from [I'll add the name later of the rock]. The rocks are abraded by the glaciers and by stream action on the mountains and the sediments are blown into the upland and fragipans are normal with an ustic moisture regime in New Zealand. I think we can consider that in the U.S. the fragipans are normal in the humid areas but they are absent in the soils that have a high carbonate content close to the rivers. I think perhaps the carbonate has something to do with preventing the formation of the fragipan, but why it forms I don't know.

Question 105

Daugherty:

There is often times under the fragipan dense basal till which has about the same bulk density as the fragipan itself, which lends a problem in trying to determine where the bottom of the fragipan is. Do you have a comment on that?

Guy Smith:

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

Question 106

Allen:

I know you mentioned in New Zealand, that it's primarily in the ustic moisture regime. Now if you made a statement as to why or what the theories are why this is so, I missed it and am asking why it is in the drier climate in New Zealand. Do you have thoughts along that line? Did you say noncalcareous materials?
Guy Smith:

Yes, they are noncalcareous materials. They can occur in udic moisture regimes. The only really good buried fragipans I have seen have been on the coast where you have three different loess [deposits], each with a fragipan that protrudes even more in these exposures than the one I photographed for Soil Taxonomy. There must be some sort of cement in a fragipan in my judgment. I don’t think compaction alone accounts for the slaking properties of the fragipan. I compare fragipan with densipan which has bulk density of about 2 as a general rule. It is much more dense than a normal fragipan which is around 1.5 to 1.6. When dry fragments of a densipan are placed in water it will slake completely to the individual silt and sand fractions. The fragipan will not do that. It will fracture into gravel-size fragments. It won’t slake the way the densipan does. I would conclude the densipan is not cemented with anything, merely compact, but something prevents the fragipan from slaking in the same way that the densipan slakes. Now it is not necessary that all fragipans have the same cement. There are reasons to believe from the studies that have been made that it is unlikely that they all have the same cement, but they seem to have some. We do observe in New Zealand going from an ustic to a udic moisture regime on the North Island where we have a beautiful duripan in soils that we can identify a few mm of pyroclastic materials at least. As we go up the mountain side into a more humid environment that seems to be udic and still find these thin layers of pyroclastic materials, we find that the fragipan has replaced the duripan. I have seen similar situations in the U.S. where duripan grades imperceptibly into fragipans. In these I would suspect that the silica is a cement in the duripan, I would suspect that it is one possible cement in a fragipan. There are plenty of studies of trying to slake these fragipans with different complexing agents to take out the silica, the iron, or the aluminum. What works on one pan doesn’t work on another, which does suggest that there are different kinds of cements in different kinds of fragipans. There is just not enough known, as yet, although it has been well studied in doctorate theses. We haven’t yet found the solutions.

Question 107

Allen:

Are these intergrades from fragipans to duripans mostly in a xeric moisture regime?

Guy Smith:

In New Zealand it is ustic.

Allen:

You were talking about New Zealand then.

Guy Smith:

In California it is xeric. The duripan is in the ustic moisture regime and the fragipan is in the udic, in New Zealand.

Calhoun:

I have some general observations following a plinthite field study, with Dr. Roy Simonson. We discussed the fragic and plinthic subgroups as they occur in the southeast and the fact that we could not find morphological features typical of fragipans in fragic subgroups of soils. One thing I mentioned was the fact that seemingly better expressed fragipans were
developed in the southeastern tier of the United States especially in the lower coastal plains as you moved west towards Mississippi. We have very little silt in soils especially in the lower coastal plains and in northwestern Florida, it is basically sand and clay. As you move west towards the Mississippi you start picking up more and more silt. Is there a possibility if there is in fact a threshold of silt content necessary for fragipan developments?

Guy Smith:

I think not. Unless you substitute the very fine sand and add that to the silt. We do have fragipans in Belgium in loamy very fine sands that have virtually no silt. The very fine sand content is quite high. I think, just as the textural particle-size classes we had to combine the very fine sand with the silt. I think there is a point somewhere around that which probably is a critical limit, but not at 50 microns.

Allen:

I agree.

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**Question 108**

Calhoun:

In your interview with Mike Leamy - one of the questions was question 42 on page 59 in the notes - You state in remarks to a question concerning soil climate - "It seems that probably the hyperthermic temperature should have been included with the isohyperthermic temperatures for the basis of interpretations." Your statement implies, the way I read it, that this would be done only for udic and possibly aquic soil moisture regimes. Could you expand on your possible reasons for combining these two temperature regimes? That is one question, and the second question in discussing this, would you also include ustic soil moisture regimes in this argument?

Guy Smith:

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

Question 109

Allen:

Dr. Smith, you mentioned the densipan that had been proposed. Would you please continue your comments on this?

Guy Smith:

I first ran into the densipan in New Zealand and Australia in 1959, but didn’t understand what they were. I went back to my notes on that trip and I found that I tried drying them and seeing whether they would slake and they did. Then I forgot about that slaking and I proposed a great group of Duraquods because this was more like a duripan than any other pan that we had at that moment. Although, as the duripan is now defined these are not cemented at all and they are merely extremely compact. When I got to the West Indies I found these again but overlying an argillic horizon. My original proposal for the densipan was that we would require a great group of Densiaquults. Well I got some feedback from that little note that there were similar pans in the wet Spodosols in Malaysia. When I got to New Zealand I started to try to find a duripan in an Aquod. According to our theory about the formation of a duripan we must have a period of dryness in the soil, by dryness I mean below wilting point to precipitate the silica. It shouldn’t have occurred in an Aquod. I spent a great deal of time trying to find the duripan in the Aquods in New Zealand and they were all densipans. We have them, then, in Ultisols and Spodosols. They make a soil uniquely worthless because the densipan is an albic horizon, that for some reason has become extremely compact, the reason being unknown. The roots then are restricted to about the surface 10 to 15 cm. Below this depth the roots cannot penetrate. Therefore a very short rainless period is going to seriously affect the use of the soil. The ability to store water is virtually nil in the soils with densipans. They are very wet, of course, after a moderate rain because the water is perched above the pan and the soil becomes saturated above the pan to the surface. The sugar plantations in Guyana have tried cultivating these soils because they have large fields and they want to farm the fields rather than the soil. Their experience has been that it is useless to try to harvest those. They don’t produce anything.
Question 110

Nichols:

Do the densipans reform after ripping or deep plowing?

Guy Smith:

I don’t know. We did try the effects of one or two dryings on a remolded densipan. The initial bulk density of the dry remolded densipan material was 1.7, which is about the limit which roots can penetrate, normally if it is 1.7 to 1.8 or higher you cannot get penetration of roots. I suggested that they might reform after ripping. The experience of the sugar plantation in Guyana would be such that would discourage the attempt to farm such a soil if it occupied a large part of the field.

Question 111

Allen:

Then this is the type of thing that is referred to in the last paragraph under the duripan horizon in Soil Taxonomy?

Guy Smith:

Yes. That paragraph where I referred to a third kind of duripan that forms in an albic horizon is in error. It is not a duripan, it is not cemented, it is not indurated, it is merely compact. The compaction is so pronounced that it is impractical to bore a hole or to dig with a spade. To get through it, one must use a bar or a pick. Having done that one can break out large chunks that come away with an abrupt lower boundary to the argillic horizon or the spodic horizon.

Question 112

Gile:

We’re talking about pans and I understand from our discussions last night that there is a proposal to drop some of the fragipans as a diagnostic horizons. Will you comment on this and in particular which ones, if any, should be dropped?

Guy Smith:

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

A comment:

The ones that I've worked with in New England have been on drumlins and they are a demonstrable pedogenic feature. There may be others that there are questions about.

Question 113

Daugherty:

I would like to ask you a philosophical question or make kind of a two part comment. I get somewhat concerned when we classify soil using non-soil parameters, we discussed this the other day a little bit. The case in point is the classification of soil moisture regimes and temperature regimes. One must use vegetation to extrapolate these parameters, like you pointed out the other day. If the soil scientist is not careful he will end up mapping vegetation and never test or never measure the soil property moisture. A side comment, how can we then keep from falling into this trap? Another comment, even when we test soil moisture most soil scientists test only to define the soil moisture control section. They don't test to see whether the soil moisture control section is useful to the soil survey in their area or their region. Do you care to comment?

Guy Smith:

First I will disagree with your assertion that moisture is not a soil property.

Daugherty:

No, I didn’t say it wasn’t.

Guy Smith:

I understand that you did. Perhaps you should restate that.

Daugherty:

If a soil scientist is not careful he will end up mapping vegetation and never test or measure the soil property, moisture.

Guy Smith:

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

Daugherty:

My main area of concern lies in the Western states where we are starting to do soil surveys on the millions of acres of wild land, the BLM land. The main concern would be when a soil scientist assigns a certain moisture regime to Ponderosa pine another moisture regime to Pinyon-Juniper type vegetation, another moisture regime to Grama grass and then to map these vegetations as if they fit these moisture regimes exactly. Then never test the moisture regimes and we get million of acres of soil surveys with never testing the moisture regime.

Guy Smith:

While in these areas there is no body of common knowledge that the mapper can draw on. These are National Forest lands or other public lands, I presume. I can readily see that it would be possible to fall into a serious trap because the presence of the Grama grass vs the Ponderosa pine may be due to something other than the moisture. Both will tolerate considerable drought.

Daugherty:

Especially the way the moisture is just held within the moisture control section. The moisture control section as we use it in Soil Taxonomy might not apply at all to those kinds of vegetation.

Nichols:

The vegetation might be due to temperature instead of moisture, such as the break from Bluegrama on the rolling plains. That is another danger.

Guy Smith:

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

Nichols:

We really haven’t done too much on soil moisture work lately. We started trying to work with the figures from the Weather Bureau two years ago. We haven’t proceeded far enough to have a meeting on those yet. We really haven’t done as much as we should have lately.

Guy Smith:

I would like to make one more comment on this that we pointed out in Soil Taxonomy that we had predetermined the classification of the soils on the Great Plains. We then fit the definition to this predetermined boundary, using climatological data to do it. If we subsequently found that our definitions were in error that we were much more apt to change the definition than the classification, which was predetermined. We said we want these soils to be in aridic subgroups of ustic great groups, or in udic subgroups of ustic great groups, or typic subgroups of ustic great groups. This was based on a lot more experience with land use than it
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was on the climatological data. The moisture control section was a device that permitted us to infer from the definitions. One of the tests of the classification is the interpretations that we make and in developing the system we repeatedly tested the taxonomy against the capability classifications. As Joe Nichols has pointed out there was a problem between Colorado and Kansas that we were quite aware of in Washington. There was a farmer living on one side of the state line could get credit where the one on the other side of the state line could not because of the discontinuity in the capability classification. While we have to test out Taxonomy against the capability classification the reverse is also true, that there may be troubles with it rather than with the taxonomy. We have to match the two instead of adjusting one to the other. We have to consider the possibility that there may be an error in either one or even in both.

Question 114

Nichols:

The taxonomy is well defined down to the family level, the soil series is a part of Taxonomy, but the limits of soil series are not as well defined. When to phase the soil series and when to establish a new phase is especially troublesome. An example is that sugar cane is grown in the southern part of the thermic temperature regime in Louisiana and Texas, but not in the middle or northern part. Since soil series are a part of Taxonomy should better guidelines be written for guidance on this question?

Guy Smith:

The problem of when to establish a new series or to use a phase of an existing series has been with us for many decades. The office of Soil Correlation in Washington has really not been very helpful in establishing guidelines. It was impossible to deal at any length with the series category in Soil Taxonomy because there were too many thousands of them, and ones that only include a few examples of families with the descriptions and data on the series in that family, and to analyze then the differences that had been used to justify series separations, that was about as far as it was possible to go in Soil Taxonomy. If one had the time and the information on the series in an appreciable number of families instead of two or three, I think one might be able to generalize to some extent on what should be used as phase criteria and what should be used as series criteria. It is obvious that in the thermic zone as well as in the mesic zone, differences in soil temperature than those recognized in the family levels would be useful to make interpretations about crop yields. It is possible then to either phase this or to set up series. The series limits, if they were established, say within the thermic range or within the mesic range, might be valid at the moment that one established the series, but given a few years the plant breeders are going to produce varieties that will make those limits inappropriate. It would be my judgment that it would be better on temperature to phase the subdivisions within the thermic or mesic zone than to make series distinctions, assuming then that within the soils themselves one doesn't find any other difference than temperature. I cannot possibly generalize on this today except to warn against building into the taxonomy differentiae that will become invalid when another crop variety is produced.
Question 115

Allen:

This question is related to one that Leroy Daugherty asked yesterday, and it was left by Dr. Wilding, however, I believe that there is one part of the question here that has not been answered. Soil Taxonomy has focused on the sola of soils intended to exclude the significance of parent material from its use and behavior standpoint. Has the pendulum swung too far away from parent material implications as a series criteria. I believe it was that last one there that you alluded to yesterday, but I would like to hear your comments on it.

Guy Smith:

For most part the definitions that have been published of soil series, the categories, have stated that the soil series is developed on a particular kind of parent material. You will find this in the Soil Survey Manual, the first addition and the 1951 revision. The implication of this definition of series is that parent material is important, but there is no clue as to what a given kind of parent material is. There are almost an infinite number of kinds of variations in the glacial tills of the northern states. How much difference does one require, say in the clay content of the glacial tills, before one decides it is another kind of parent material. The clay percentages of the Wisconsin tills in Illinois will range from less than 10 percent of over 80. In the mapping that continuum was broken into four steps. It would be the coarse-loamy, fine-loamy, fine, and the very fine. When one has everything between four possible subdivisions or eight possible subdivisions, if one goes by steps of 10 percent for example, what is meant by parent material is undefined actually. It is a matter of judgment of the man who is making the survey and the purpose for which the surveys are being made. If one set up rigid limits of any one property of materials that would distinguish one parent material from another, I fear it would cause great troubles when surveys were made for different purposes. The soil survey of Alaska, for example, would not find the same subdivisions useful as one would find in North Dakota where the soils are virtually all cultivated. We would like to, I think, keep some flexibility. It isn't quite true that Soil Taxonomy is focused on the sola, I tried to avoid using that word in Soil Taxonomy, except perhaps in an explanatory method. It does not appear in the definitions of diagnostic horizons because people won't agree on what the soil is. The Americans and the Canadians differ violently on the accumulation of carbonate. The Soil Survey Manual says the horizon of the accumulation of carbonate is part of the C horizon, now the sola is suppose to be the A and B, not the C. The Canadians call the accumulation of carbonates a B horizon, a Bca. If one uses that concept of A and B and C and sola in the definitions of the diagnostic horizons or the taxa, then one gets into endless arguments about what is A and what is B, and what is C, or what is parent material or is sola. There is no general agreement whatever amongst the world's pedologists about the meanings of these terms.

Question 116

Giles:

Your comment comes right at the start of a question that I had and answers part of it, but I would just like to frame this for any additional comments that you have. The concept of diagnostic horizons is a very important contribution of Soil Taxonomy. Can you give us some of the details of how this concept developed as a tool for the new system? You have answered a good bit of this, but are there other details concerning the history of the development of the diagnostic horizons as a tool that you could give us?
Guy Smith:

Yes. We started very early in the approximations to use the general European concept that some soils had AC profiles, some soils had C profiles, some ABC profiles, or (A), B profiles and these were classified differently according to the horizon sequences using ABC terminology. We very early in our approximations recognized that there were different kinds of B horizons, as eventually ended up as spodic horizon and the argilllic horizon. At first we called these Podzol B horizons, and textural B horizons, and Chernozemic A horizons, and so on. In the 5th Approximation we were beginning to focus on the classification of soils that had been called Latosols. We tried to develop Kellogg's concept of Latosols into concept of a B horizon such as we might expect in a Latosol and proposed a definition of a Latosolic B horizon. So many of the workers in the tropics were insulted at the notion that this was a B horizon because it was not a horizon of accumulation of anything. It was a horizon of weathering and of loss of materials but not of accumulation by translocation. All comments that I got on that definition were comments that said this is not a B horizon. None of them examined the definition to see whether it would produce any useful groupings of soils. At this point I realized that is was necessary to eliminate the use of the concepts of A, B, and C and go to named diagnostic horizons. This then avoided the argument about what is a B horizon, which has raged I suppose ever since Dokuchaiev recognized soils as natural bodies. There his B horizon in his Chernozem was a transition between the mollie epipedon and the underlying stratum. It was not anything else not even a cambic horizon necessarily. The concept of A, B, and C was introduced into Western Europe where the soil had horizons of an accumulation of translocated materials, as in the Podzol and in the Sols lessives, sol bruns lessives of the French. The Western Europeans adopted the B horizon as a horizon of accumulation of translocated materials which was quite different from Dokuchaiev's original use of B. This became entrenched in Western Europe, but did not become entrenched necessarily in all parts of the world. We have then, the current dilemma between the Canadians and the U. S. about a horizon of accumulation of carbonates. To the Canadians who adopted the Western European conventions it is a B horizon. To the Americans who did not insist on the B horizon being one of accumulations of translocated materials, the horizon of carbonate accumulation was commonly at considerable depth, and was considered as a part of the C horizon. The date for the adoption of the diagnostic horizon is hard to fix because we were speaking of different kinds of A horizons and different kinds of B horizons. The use of named diagnostic horizons dates from the 6th Approximation. The 6th Approximation was issued in 1957.

Question 117

Gile:

We discussed earlier how lowering the depth requirement for the cambic horizon eliminated substantial areas of potential Camborthids. Another change in a definition eliminated large areas of potential Mollisols. This is a change involving chroma. Would you go into the reason for the change of chroma requirements for the mollic epipedon?

Guy Smith:

I suppose this came from my experience with the former Reddish-Brown Lateritic soils of the northwest Pacific States. These had substantially more organic matter than was required for a mollic epipedon. They were all from basic rocks and they all had relatively low color values, mostly 3 when moist. They were soils that formed under forest in a humid environment and they seemed more closely related to the Reddish-Brown Lateritic soils of the Southeast than they did to Mollisols of the Great Plains. The only distinction that I could arrive at was that in these soils we normally had a chroma of 4 in the epipedon.
Question 118

Allen:

While we are discussing mollic epipedons I would like to pose a question concerning Mollisols. As defined in *Soil Taxonomy* the central concept was based upon the dark relatively deep soils developed under tall grass mostly in the midwest prairies. It has always been difficult for me to explain to students why our extensive shallow soils in Texas, for example, the Ector series over bedrock, should be Mollisols. What was the thinking in the development of *Taxonomy* to break between Mollisols and Entisols along this line. The use of these kinds of soils is very different from the cultivated soils of the midwest.

Guy Smith:

In general the first point is that these soils have shallow-lithic contacts or paralithic contact. We have similar soils in Iowa with shallow limestone bedrock. We have to put some sort of a limit on the thickness of the mollic epipedon where it lies on the bedrock. In order to group the soils of the Great Plains that had formerly been grouped in the 1938 classification as dark-colored soils of the semiarid, subhumid, and humid grasslands. We had to have some limit of thickness to distinguish them from some of the soils that had formerly been called Gray-Brown Podzolic soils which have an A1 horizon that is for all practical purposes identical to the mollic epipedon except for thickness. Once plowed, of course, this disappears in the former Gray-Brown Podzolic soils because it is mixed with the underlying horizons. The mollic epipedon was thickest in the humid parts of the grasslands of the Great Plains and thins as the soil climate becomes dryer. We developed a sliding scale of thickness based on the depth to the accumulation of carbonates for the grasslands in order that they would all have a mollic epipedon with one third of the thickness of the depth to secondary lime or a minimum or maximum, up to a maximum of 25 cm. When we came to the bedrock then they were often less than 25 cm. There was not necessarily any secondary lime in these soils. That sliding scale had to be modified for soils where the mollic epipedon rested directly on the lithic or paralithic contact. I proposed a minimum of 10 cm for these soils with no necessary maximum. This proposal never got criticized.

Question 119

Calhoun:

Another Mollisol-related question. Why were the Rendolls restricted to a udic soil moisture regime?

Guy Smith:

This comes from the original concept of Rendzinas as intrazonal soils whose characteristics are due to the parent material rather than to the climate and the vegetation. The Rendzinas of Europe form pretty much our central concept of Rendolls. They are dark-colored soils resting mostly on marl and they are in a humid climate. They do not consider the Chernozems as Rendzinas. To some pedologist, some who visited Texas, identified some of your dark-colored soils on limestone with an ustic regime as Rendzinas. Although there was a marked difference in these soils from the Rendzinas of Europe in that they had a pronounced horizon of carbonate accumulations. They reflected then, the climate, not the bedrock. I found that the Texans had Rendzinas all over Texas wherever the soil was shallow on limestone. These would have been
dark-colored soils irrespective of the nature of the rock, just as in Iowa, what was called a
Prairie soil, shallow over limestone, would have to be called a Rendzina because it had no
horizon of carbonate accumulation. These would be quite unlike kinds of soil. Although,
variability at the suborder level could have been handled at the great group level. It was mostly
to keep the grassland soils together and separated from the forest soils of the humid regions that
we restricted the Rendolls to soils that have a udic moisture regime. The soils on limestone
with ca horizons and an ustic moisture regime were then clearly separable from the Rendzinas
of Western Europe.

Calhoun:

As a follow-up to add, do you find the same observation to be true in ustic soil moisture
regime to the tropics, in finding the zone of secondary calcium carbonate accumulation? I ask
this question because in the northwestern portion of El Salvador we had a small area of
limestone. Although, we don't have data nor detailed morphological descriptions or observations
we basically had a mollie epipedon that was resting over limestone with no secondary
accumulation of either clay or calcium carbonate or anything else but was very distinctly an
ustic soil moisture regime. However, it is a wet ustic with 1600 to 1700 mm of rainfall
occurring in a very distinct six-month dry season.

Guy Smith:

(Note: This is not the beginning of Smith's answer, the tape was started after he had
begun his comment. TDC) In Venezuela developed on calcareous parent materials. In the
absence of carbonates in the parent materials we don't find much secondary lime in the
intertropical regions. The rainfall at Maracay is something like a 1,000 mm in a six-months
rainy season. It is enough to saturate the whole soil, but it does not seem to be enough to get
the lime out of a moderately calcareous parent material. This is my only experience with
secondary lime in the intertropical regions. In the West Indies I do not at this moment recall
any Calciustolls.

Question 120

Allen:

Are Rendolls extensive in any parts of the world with which you are familiar except
Eastern Europe?

Guy Smith:

Western Europe. They are relatively inextensive in the United States if one judges by the
numbers of series that have been classified as Rendolls. They are quite extensive in parts of
France and Belgium. The Paris Basin is very largely composed of Rendolls.

Calhoun:

Just as a sideline to that. The area around Ticao (?) in Guatemala, a continuation of the
lower basin of the Yucatan peninsula, I don't know how far up into the Yucatan, depends on
whether we go from udic to ustic, somewhere north on that peninsula would be predominantly
classified as Rendolls. There are large extensive areas that supported a very intensive Mayan
agriculture.
Guy Smith:

I thought maybe they were Vertisols myself. They are dark colored.

Question 121

Gile:

Does the term clay, as used in the definition of the various "Pale" great groups refer to silicate clay on a whole-soil basis, not carbonate-free?

Guy Smith:

The term percentage of clay, clay that is not clearly defined. In general this is measured on the whole-soil basis. However, if the carbonates are secondary origins they may be of clay size. These carbonates we specify are to be treated as silt rather than clay. If the secondary carbonates are of silt size, of course they are treated as silt. So I would assume that in general the meaning was the percentage of silicate clay in the whole-soil matrix.

Gile:

That is what I thought, but I just wanted to hear your answer.

Guy Smith:

The clay size secondary carbonates are treated as silts because they do not seem to have the physical properties of the silicate clay. They do not retain moisture in the same manner and one can seriously misjudge the amount of silicate clay that may include the carbonate clay with it.

Question 122

Daugherty:

Would you explain the extra-grade subgroup leptic as in Leptic Natrialbolls and the lack of the use of this term in Natrargids etc.?

Guy Smith:

The Leptic Natrialbolls were provided for because of the feelings of the correlators and the state soil scientists and the experiment station people, primarily in North Dakota and Montana that they needed a distinction between soils with a very shallow solum and soils with a moderately thick solum. The North central regional correlation staff and the work-planning conferences went along with this desire for the Leptic subgroups. When we get to other kinds of soils where we are dealing with different groups of people, the feeling might not have been so strong or might have been absent about the importance of the thickness of what we used to
call the solum. We are dealing with, not only different kinds of soil, but different groups or committees of people.

**Question 123**

Calhoun:

Under what environmental conditions would an Oxic Aruidoll occur? It was mentioned that Oxic subgroups are provided for some Mollisols. An Oxic Aruidoll I find rather fascinating and possibly it relates back to some of these things that Joe Nichols was mentioning in terms of Kenya.

Guy Smith:

The only good examples of such soils that I know of come from the assembled data on the soils of the former Belgian Congo or Zaire, where we have soils that have properties of Mollisols as they are defined in Taxonomy, but that have kaolinitic clays and free oxides for the argillic horizon. These are intertropical soils, I think not necessarily from weathered sediments, but possibly from preweathered sediments. Under the high temperatures and the high rainfalls that we have, the surprising thing is that one finds Mollisols. Their presence may be due to the vegetation which would mostly be calcium-collecting evergreen forest trees.

Calhoun:

Is it possible that many of these are leguminous in nature and are nitrogen-fixing evergreens.

Guy Smith:

I don't know the species or their classification. I have never visited these areas. I think that the collected data from INIAP or INEAC on the soils of Zaire probably will list the botanic names of the native vegetation that was growing when they sampled the soil. The botanic classification is useless to tell me whether it is a tree, grass, shrub, legume, or a non-legume or what have you. I only know it is a plant because the book says so, it is the vegetation.

**Question 124**

Gile:

We talked briefly about the change in the definition of the calcic horizon in which the 15 percent requirement of calcium carbonate equivalent was waived if the horizon had at least 5 percent by volume more soft powdery lime than an underlying horizon. In the skeletal classes, cited in the definition, consistence of carbonate that I have seen has been harder than soft. In what kinds of situations would secondary soft powdery lime be expected in skeletal horizons with less than 15 percent calcium carbonate equivalent. The definition is on page 45, Guy, if you would like to look at it.
Guy Smith:

I am afraid I cannot answer that particular question. The requirements for 15 percent calcium carbonate was waived for the sandier soils because we commonly have very distinct accumulations in these soils with considerably more carbonate than the underlying horizon. Being more or less siliceous by nature of the sandy parent material it never reached the 15 percent limit. We were really more concerned with the 5 percent limit than with the 15 percent limit. We enumerated there the particle-size classes which were involved in this waiver of the 15 percent limit. Whether or not we listed the proper classes, I could not say. I just have no good experience with this. You have in your desert project probably seen many such soils. I do not know under what condition one would get soft powdery lime. I suspect you would be more apt to get pendants under the stones, in arid climates.

Gile:

Yes, and as accumulation between the pebbles sometimes localized, but usually they are harder than soft.

Question 125

Allen:

This question was left by Tommie Hallmark, before leaving on Monday. It has been alluded to, but I would like some additional comments if you are still inclined. The soil scientist is concerned with the upper two meters, at best. Most geologists are concerned with the bedrock. In many areas such as the Gulf Coastal Plain the material between the lower limit of the soil and bedrock is somewhat of a no-mans land, which controls many interpretations. But it is rarely studied. Should we as pedologists take the leadership in studying these materials?

Guy Smith:

There are several problems involved here. The first one of course is the difficulty of making enough observations to yield valid conclusions about the significance. Where it is critical to the interpretations as in an irrigation project in which one needs to know what is going to happen to the leaching waters. Although it is difficult, it still would be essential that the pedologist for his interpretations have a drill rig and bore it out and find out just what underlies the soil and overlies the rock. It is a no-mans field and unless one feels that it is important to interpretations, certainly the pedologists should not waste his time on it. If it is critical, then, it is essential that the pedologist work out the distribution of the underlying unconsolidated materials. I only know of a few instances where this has been done and always for irrigation.
Question 126

White:

Under Albaqualfs Soil Taxonomy talks about the albic horizon resting abruptly on the argillic horizon. It goes on about must have a mesic or thermic temperature regime and be irrigated, because they are dry for short periods. The dryness seems to be essential to the genesis. Could you give me some background on the sentence "The dryness seems to be essential to the genesis." Is that to the genesis of the albic, to the argillic, or to the boundary between the albic and the argillic? This is at the very bottom of page 109 top of 110 in Soil Taxonomy.

Guy Smith:

This statement is primarily a statement coming from geographic correlation between the occurrence of Albaqualfs and the dryness in the warm summer months. There is one from northern Missouri where the Albaqualfs are very extensive in the loess. Across Illinois and into Indiana the Albaqualfs virtually disappear and are replaced by Glossaqualfs. The Missouri Albaqualfs are the famous Putnam series. In southern Illinois the Cisne and Cowdon are considered representative Albaqualfs. They run on over into Kansas and Oklahoma, but I have never seen them in those states. The dryness is probably not essential to the development of the argillic horizon because the Glossaqualfs have argillic horizons also. They don't have that abrupt boundary that occurs in the Albolls and the Albaqualfs. There was no good genetic theory to explain this at the time that we were working on Soil Taxonomy. In recent years the process of ferrolysis has been worked out to a considerable extent. Most of these soils have groundwater perched on the argillic horizon at some season of the year. That is one condition that seems essential for ferrolysis which is basically destruction of the clay under anaerobic condition. In the FAO UNESCO legend the statement appears, "in these soils the clay has been destroyed in the A horizon". That is a serious overstatement because there may have been some destruction of clay, but there also has been translocation of clay into the argillic horizon. It may be a combination of the two. This is a field in which there is still a great deal to be learned. Along about 1934 in the old soil survey association proceedings Roger Bray presented a series of papers on the genesis of the B horizon, it was then called, now the argillic horizon, in these soils. He worked out a series of calculations about clay formation in place and translocation, and explained the difference between the A and B horizon of the Albaqualfs basically on translocation rather than destruction. Clay difference could be due in part to both processes. We can't in any way at the moment quantify how much is due to one and how much to the other.

Calhoun:

Just before we started up again we were talking at this end of the room about these sharp contacts. The Lufkin soils in Brazos County which are Vertic Albaqualfs has an extremely sharp, knife-edge boundary between the surface and subsurface. I am not even going to call them horizons because that starts another problem. Joe mentioned that there are also several of these in Oklahoma. The question arose what pedogenic process can produce a knife edge boundary? If it is translocation, which is the story I have been given in Texas for the strong boundary, I can't believe the translocation gives you a knife-edge. If it's clay destruction, I hadn't heard that, and will dig that out of the library. Do you have any thoughts along that line with these extremely sharp boundaries?

Guy Smith:

Well, let's put it this way. We have some series that are neither Albolls nor Albaqualfs that have this abrupt boundary between the epipedon and the argillic horizon. There is no albic horizon in between. These are still drier than the Albaqualf and the Albolls. Probably the albic horizon is not there because they are not saturated for long enough periods to destroy any clays. Yet there they are, a fact and the abrupt boundaries are genetically a bit of a problem. We
have them not only in these soils but also in most Spodosols. I think most people agreed that spodic horizon is due to translocation and precipitation of humus and aluminum or humus, aluminum and iron. There is no good theory yet to explain any of the abrupt boundaries. Dr. Flach has worried quite a bit about the abrupt boundary in Spodosols. He has presented a hypotheses, though not in writing to my knowledge, that the humus to be precipitated does not get enough aluminum while it is in the albic horizon, but when it gets to the spodic horizon it picks up some of the aluminum that is already there and that has been put into an available form for further precipitation by biologic destruction of the ligands that bond the aluminum to the humus. This won’t explain anything in terms of an argillic horizon.

**Question 127**

**Daugherty:**

Looking at some of the past questions that you have been asked. You have been asked questions on reasons for base saturation and separation between Ultisols and Alfisols and between mollic epipedon and umbric. I have a question that relates to base saturation, also. How was the 60 percent base saturation decided upon for the division between Eutrochrepts and Dystrochrepts? Fifty percent is used for Mollisols and seems logical that this might be the logical separation for Eutrochrepts and Dystrochrepts instead of 60 percent?

**Guy Smith:**

The reason for that is that the studies we had for the Inceptisols in the northeastern states, in Pennsylvania, in New York, and Maryland shows that the most common range of base saturation in these soils was between about 45 and 65 percent. By moving the limit up to 60 percent we kept all of the related soils very much together. If we put the limit at 50 percent we would have cut down the middle of these series. They are so similar that the field men can’t tell what the base saturation is. They had to go to the laboratory. The sensible thing to do was to use another number because none of them get very far above or very far below the 50 percent limit.

**Daugherty:**

When this gets into areas of soils with different kinds of mineral suites, it might cause some problems, wouldn’t it?

**Guy Smith:**

It might only have transferred the problem to some other part of the world but we have no data on those other parts yet.

**Thompson:**

This has to do again with the tonguing. Penetrations of albic material must occupy greater than 15 percent of the matrix of some part of the argillic or natric horizon to be considered as tongue. I was curious as to why the 15 percent minimum figure was used as a criterion.

**Guy Smith:**

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

Question 128

Nichols:

Family groupings are useful for many interpretations. They would be useful for many more interpretations with depth classes of about 20 to 40 inches, 40 to 60 and greater than 60 inches, were a part of the system. Do you think depth has a place in the family property of the systems?

Guy Smith:

That is depth to a lithic or paralithic or some other barrier to roots?

Nichols:

If we go in now to the family classification we'll have soils that range from 20 to 80 inches or more to lithic or paralithic rock. It makes it very difficult to try to make common statements about them especially non-agricultural interpretations.

Guy Smith:

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

Question 129

Nichols:

For several years we thought that the 10 percent weatherable mineral break at the family level coincided with important landscapes breaks. With additional data this is not proven to be true. We have siliceous Ustic Torripsamments, such as Penwell, on the Texas and New Mexico line. Both siliceous and mixed mineralogy on the Pamlico surface in South Carolina. Would you care to comment on the rationale for the 10 percent mineralogy break? The 10 percent weatherable mineral break for the siliceous mineralogy.
Guy Smith:

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

Nichols:

I was wondering if there was some kind of crop nutrient supplying capacity for crops at about the 10 percent weatherable mineral.

Guy Smith:

I don't believe so because there is so much difference in the release of nutrients according to the nature of the weatherable minerals present. The calcium feldspars weather very rapidly and it doesn't take many thousands of years for them to disappear from the soil in the humid climate, but muscovite is very resistant to weathering and being resistant to weathering we would expect that the nutrient release would be very slow. The committee on classifications of soils with low-activity clays has been discussing this limit, and may come up with some recommendations for them.

Nichols:

I remember the last couple of circular letters from them did not have anything on mineralogy.

Guy Smith:

Well, they have been discussing it in particular, I think, and probably you have been involved in this, that is, what to do with Cecil and Appling series. Should they be included with the low activity clay soils or excluded. If we use weatherable minerals they are [the Cecil soils] excluded. If we use, strictly, the nature of the activity of the clay then they are included.

Nichols:

It appears if they exclude those they may exclude a lot of soils they wanted in, other parts of the world.

Question 130

Daugherty:

Would you address the fine clay/total clay ratio as part of the argillic horizon definition and the quantitative requirements for the increase in this fine clay/total clay ratio? It says in Soil Taxonomy it needs about one third or more increase.
Guy Smith:

We had relatively few data on the ratios of fine to total clay when *Soil Taxonomy* was written. The studies that we had principally came from the northern states where we have Mollisols and Alfisols with a more or less mixed clay mineralogy. The implication was very clear that in these soils on which we had data, the bulk of the difference in clay between the argillic and the overlying horizons was due to translocation of the finer part of the clay. The montmorillonite is normally much higher than the illite or the kaolinite and seems to move preferentially, and have a big effect on the ratio of fine to total clay. We have more data now than we had then. The introduction of some such statement as this does often stimulate studies. The laboratory we used to have at Riverside made some studies of some of the soils for the arid parts of the U.S. and reported back that they couldn't find any difference. So the ratio of fine to total clay was not made an absolute requirement. If you read what it says here—"The ratio of fine to total clay in the argillic horizon is normally greater than in the illuvial horizon by about one third or more." We have "normally" and "about" in that sentence. I thought this would be used as a clue where it was useful, but should not be an absolute requirement in the definition of the argillic horizon. The sliding scale of 3%, 1.2, and 8% is related to the discussion on how sensitive field men could feel these increases. What could they consistently determine in the field? In the sandier soils we finally decided that a competent field man should be able to recognize the difference of 3 percent. In the loamier soils an increase of 1/5 [20%] could be recognized. In the clayey soils it would require at least 8 percent, but that was less than 1/5 if you got up into 60 or 70 percent clay in the very fine textures.

**Question 131**

Allen:

I have often wondered why that was not continued from 15 percent up to 40 percent on a percentage basis rather than the one to two ratio. I never quite understood the reason switching back to 1.2 ratio.

Guy Smith:

This was, we thought, something that was observable in the field with the finger. If you have 25% clay then 5% difference should be recognizable. If you have 30% clay it would take a 6% difference to be recognizable. The more clay you get the less precise are your estimates. The French use a ratio of 1.4. Of course 1.4 is readily observable. As we examined the data, particularly in the Mollisols, the soils that we thought had rather distinct argillic horizons did not reach the 1.4 ratio because the A horizons of the mollic epipedon contained too much clay. When we came to Alfisols the 1.4 ratio would have worked.

Allen:

The sliding percentage scale between 15 and 40 could have been constructed, I guess.

Guy Smith:

It was by using the ratio. That was a sliding scale.
Daugherty:

I would like to ask another question in relation to the argillic horizon. Would you discuss the lamella in the argillic horizon. There seems to still be a question among states or among regions as to whether all of these lamellae are pedogenic or geologic?

Guy Smith:

They can readily be a combination of both. These lamellae, however, where they are pedogenic are stuffed with oriented clay. The finer-textured strata in the sands are not. The lamellae conceivably start to form at a point where there is a change in the particle size of the sands. They will follow stratifications if they can, but they often cut from one stratum to another in such a manner that it is difficult to imagine the sedimentary process that would be responsible. The probability is that these lamellae formed because at some stage in early development the down-moving water hangs, is withdrawn by evapotranspiration and deposits whatever it is carrying at that point. This accentuates the difference that originally caused the water to stop there. Water stops when there is a change in pore size. The lamellae that we have in the soils of Pleistocene age are not found in calcareous sediments. When you reach carbonates in the Pleistocene sands in Iowa and Illinois there are no lamellae below. It is difficult to understand this, if it is geologic because you may find them to a depth of 50 cm in one soil and 2 meters in another. In all cases they are in the noncalcareous sand. The argument for their being geologic would conceivably come from the tendency of these lamellae to follow stratifications in the sands. We get the same forms of the lamellae in the sandy Spodosols. These lamellae are restricted to relatively coarse-textured soils with low clay contents. I used to say that we had no lamellae in loess but unhappily the Belgians have found some.

Daugherty:

We have found some that appear to be lamellae in sandy and lightly gravelly material in the desert regions in New Mexico. The question is whether these are pedogenic or geologic. Maybe Leland Gile can speak about them better.

Gile:

Which ones are you talking about? Which lamellae in New Mexico?

Daugherty:

The ones we saw around Socorro, NM. John Hawley mentioned that you had some on the desert project that you thought maybe were agrading due to clay carried in arroyos.

Gile:

Yes, these are in very gravelly deposits though. They could be below a real nice argillic horizon, that could represent a great depth of clay accumulation or maybe some lateral accumulation. I haven't seen them in the gravelly deposits in Holocene soils. I think from the standpoint of trying to explain them in the sandier soils than with the rationale for explaining them according to contact of different size materials, such as Guy mentioned.

But interestingly though, we haven't found the lamellae in the Las Cruces area where it is very dry in Holocene soils. These would be in sandy sediments. But we do have them out here in the Bailey County area where we have got precipitation of about 16 inches. This suggests that somewhere between the two extremes of precipitation of about 16 inches. This suggests that somewhere between the two extremes of precipitation range of about 16, there is a place where lamellae formations kind of tail off and you just don't get them in the desert soils. From what I have seen, at any rate.
Guy Smith:

I have not seen them in the sandy Aridisols in Australia. They are very common in the sandy soils of Australia where the moisture regime is ustic, xeric, or udic.

Gile:

This again seems to suggest that we need a little more moisture than 8 or 10 inches to get the lamella forming. The only additional comment that I would have about the lamellae or bands is that it would help to use the more or less standard tests for horizons in determining whether these are pedogenic or not. In our Bailey-Talmage study area sola are relatively thin and in many places are underlain by C horizon material and is quite readily demonstrated that most or all of the lamellae that are in that area are of pedogenic origin. The problem is more difficult in areas where there is more precipitation, the wetting-front goes deeper and you run into zones that have bands of uncertain origin, at least uncertain at this time. These areas I think need a lot more study.

Question 133

Daugherty:

This is a philosophical question and is not really asked as a question maybe just to formulate a comment on your part. How do we overcome some of the bias toward creative thinking which a document such as Soil Taxonomy would create. This would be especially true in areas where we would set up arbitrary boundaries or class limits rather than ones that have a special meaning. You have to guard against stifling of progress in soil genesis. Do you have any comments?

Guy Smith:

Just really one. The reason I am here is that I very carefully tried to hide all of this stuff in Soil Taxonomy to force the people to examine the definitions to see how they grouped the soils. If I had given all the background on all these questions then people, I feared, would pay more attention to the reasons why we did something than to what we said. Then they would be less inclined to examine the groupings of soils that result from the definitions in Soil Taxonomy. I don't see how as it is written Soil Taxonomy can stifle creative thinking because it only forces you to examine the groupings. If you don't like the groupings that result you then have a perfect right to suggest changes in limits and natures of definitions that will produce better groupings.

Question 134

Calhoun:

How significant are coating-classes in Psammments? Moisture release curves for Florida Psammments indicate that very fine sand content is a more important criteria especially when a
tension somewhere between 60 and 100 cm is used as field capacity for sands rather than 345 cm.

Guy Smith:

Well it was primarily the Florida people who were concerned with this family distinction between coated and uncoated. The only data they had was on moisture equivalent. That is all that was available, nothing else. It had to be written, the definition, in terms of available data or we wouldn't have any notion as to what we were doing with the classification of our soils. I would surely agree that the very fine sand fraction, particularly that part less than about 74 microns is just as important to moisture properties as is the silt. In the taxonomy as written you might talk about eyeballing. I looked at the cumulative curves of a number of sands. If the soil was a sand the bulk of the very fine sand was in the largest half of the very fine sand fraction. We had some data on very fine sand effects on capillary rise and moisture retention from Michigan. Consequently using the definitions of the families of the particle-size classes as they now stand treat that very fine sand fraction in a floating manner so that if the bulk of the sand is medium and coarser sand, it is treated as sand. It was generally appreciably in the upper half of the very fine sand range. (NOTE: tape starts in the middle of a sentence.) and it should be treated as silt. We were dealing with relative absence of data and yet if we made no proposals nobody would ever examine these things in all probability. When they object to the groupings they get here they are stimulated to do some work and to try to make corrections and improvements.

Question 135

Allen:

I will give somewhat of a lengthy preamble here before I get to the question, Dr. Smith. A soil can qualify as a Paleustalf by either having a petrocalcic horizon or one of the following: 1) A clay distribution that does not decrease by as much as 20% of the maximum within 1.5 meters of the surface in addition to some color requirements, 2) or have an argillic horizon in which the upper part is clayey and there is an increase of at least 20% clay within a vertical distance of 7.5 cm or an increase of at least 15% clay within a vertical distance of 1.5 cm of the upper boundary. This is on page 138 in Soil Taxonomy. Now question No. 1 - Does a 20% clay increase apply to any part of the argillic horizon where it says 7.5 cm? But my question is, when the clay increase is 20% within 7.5 cm does the 20% increase have to occur at the top of the argillic, somewhere within 7.5 cm of the top of the argillic, or anywhere in the argillic because for a 15% increase it states at the top of the argillic.

Guy Smith:

A 20% clay increase. This clay increase of 20% within a vertical distance of 7.5 cm or 15% within a vertical distance of 2.5 cm is an absolute increase. In other words, in going from 30% clay to 50% clay or from the 15% from 30 to 45 percent clay.

Allen:

But my question is, when the clay increase is 20% within 7.5 cm does the 20% increase have to occur at the top of the argillic, somewhere within 7.5 cm of the top of the argillic, or anywhere in the argillic because for a 15% increase it states at the top of the argillic.
Guy Smith:

It says at the upper boundary. Then absolute clay increase must be met at the top of the argilllic in either a 20% increase within 7.5 cm or a 15% increase in 2.5 cm. It states at the top of the argilllic horizon from the material above the argilllic and the material in the argilllic.

Allen:

In contrast of the Paleustalfs a soil can qualify as a Paleargids only in two ways, p. 165 1) A petrocalcic horizon or 2) either a stated percentage increase at the top of an argilllic horizon, the amount of which depends upon whether or not Ap is present. The question is: Why is there no provision for the deep clay distribution in the Paleargids as there are in the Paleustalfs. There are only two ways it can qualify?

Guy Smith:

Yes.

Allen:

Now in the Southwest part of the High Plains we have Aridic Paleustalfs that border onto the Ustalfic Haplargids I wonder why we don’t have that provision in case of the Paleargids.

Guy Smith:

I presume that’s because we never found that deep distribution of clay in Aridisols. The water just doesn’t go deep enough or hasn’t gone deep enough to move the clay and to produce the clay by weathering in such deep horizons as a 1.5 m. Theoretically you should be able to find some polygenetic Argids that have such a deep clay distribution. They haven’t been reported to me, I didn’t run across them in the development of Soil Taxonomy.

Question 136

Thompson:

I might have one last question Dr. Smith, then I am going to about wind my questions up. Why were the aquic arenic subgroups excluded from Paleudalfs, and why was it felt that no aquic grossarenic subgroups were needed in Udalfs, Ustalfs, and Udults. I readily recognize that the 75 cm depth to gray mottles splits the class limits of arenic subgroups. This is on page 135 and 365 if you care to look at the Alfisols and Paleudults.

Guy Smith:

This is a question that I cannot answer. In theory the bulk of these Arenic and Grossarenic Paleudalfs and Paleudults are in the region of the southern states. We are not dealing with two different groups of people we are dealing with the same group. It is one thing with the Paleudults in Florida and another thing with the Paleudalfs in Texas. Their recommendations were accepted. I was not in on their discussions at the Work-Planning conferences.
Thompson:

I presume then if we felt it would serve a useful purpose we could propose an addition of Aquic Arenic Paleudalfs?

Guy Smith:

Certainly, yes. That is Paleudalfs. In the Alfisols this is an implied subgroup in that these are the definitions for the Aquic Paleudalfs excludes the Arenic subgroups and the definition for the Arenic subgroups does not mention the Aquic properties. It is an implied subgroup. If an examination of your interpretations suggest that you need that subgroup then it should be proposed. If your examination of your interpretations suggests that you make the same interpretations for the Arenic Paleudalfs, let us say, that also meet the restrictions on the Aquic subgroup, then you should propose a modification of the definition of the arenic subgroup. Bear in mind that the only subgroups listed here are those that appeared in the print out of the classification of the soils of the United States. Many other implied subgroups exist throughout the Taxonomy but are not spelled out simply because we had no series that had been so classified.

Guy Smith:

The limits were proposed by the regional groups based on their experience with the significance of the depth to the gray mottles. In general the sandier the soil the less importance one is inclined to put on the gray mottles. Particularly in thermic soils the importance of the depth to the gray mottles decreases because you have a long growing season. If the soil is inclined to be a little wet in the winter it is not so important as it is in the frigid and mesic soils where your growing seasons are shorter and the delay in planting due to wetness may be very critical.

Question 137

Nichols:

Why did the depths to 2 chroma mottles for aquic subgroups vary from within one meter for Argiustolls to within 75 cm for Haplustalfs to within 75 cm and the upper 12.5 cm of the argillic for Haplustults?

Guy Smith:

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

Nichols:

The midwest probably would not notice this, because they don't have the Alfisols associated with Mollisols like we do have Ustalsfs and Ustolls together. With 30 inches or 75 cm on one and a 100 cm on the other one when they occur side by side seems a little odd to our people and they wouldn't notice that further north. We don't have that many of these kinds of
soils and they are soils with high water tables. We haven't had many complaints on that, it just seems odd. They make up an extremely small part of the landscape anyway.

Allen:

I don't think you have complaints so much as you have questions as to why. Nobody questions the validity of the requirement simply why was it set that way.

Guy Smith:

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

Nichols:

The aquic and the pachic subgroups have higher yields than the typic for the most part in the ustic areas. The aquic subgroups are also the highest yielders in the Cherokee Prairies. The Dennis series in an aquic subgroup. The soil has a 2w land capability class, but it is higher producing than a typic subgroup because the extra moisture is more of a benefit than it is a handicap.

Guy Smith:

Then it doesn't belong in 2w because w implies that wetness is a limitation and not a handicap.

Nichols:

The soil doesn't grow alfalfa. You are limited somewhat on the number of crops. It may delay the corn planting a little but the yields on the crops other than alfalfa are higher. It seems ideally suited to winter wheat which can take just enough of the moisture during that season to give higher yields.

Question 138

Nichols:

Dr. Bartelli and others involved thought that the oxidic mineralogy was going to help them in classification that would isolate certain kinds of soils. They thought that soil such as Tifton would have oxidic mineralogy. The soils that had the relatively low CEC per 100 grams of clay. Along in about 1968 or 1969 when they started getting some data they found out in fact that Tifton did not have oxidic mineralogy nor did the Norfolk or Cecil or some of the other soils they thought would. They did find some soils that did have oxidic mineralogy and they were the soils in the mountain areas in Tennessee. I believe one of them was the Alcoa series and another one, the Brevard series in the mountains of North Carolina. After they found out it wasn't making the split that they wanted they declared a moratorium on it until they could get more data. Now after I came to Fort Worth in 1971, we talked several times about what to do about this and always put it off a little longer until when the low cation activity clay committee
began we were in hopes that they would solve the problem because the Kandi Udults, or whatever the final terminology would be when they were defined, might make the splits that they wanted. That committee has run a little longer than most people had thought it would at the beginning. There are still hopes that after that committee produces their work that maybe oxidic mineralogy won't be needed. Before long we have to make a decision as to whether we will retain this or whether we won't. Apparently the mineralogy is essentially inherited from the rocks in the area for the few soils that we have. I have the latest circular here for the low cation activity clay committee. If they stick with the same CEC break that is used for Oxisols of 16, there are going to be very few of those in the southeast. If they had used 24 milliequivalent break at pH 7, there would have been a tremendous areas in the south east U.S. and southeast Texas. After much discussion it looks like they are going with the 16 milliequivalents and we may again be left with the problem. That decision needs to be made fairly soon about what to do. If it isn't solved by the low cation activity clay committee we may want something like a task force similar to the one that we had on organic soils or the task force on the orders of soil surveys to try to solve that problem. Dr. Smith do you have a comment on this?

Guy Smith:

There are still two alternative courses of action. If you decide you don't want the oxidic mineralogy in Alfisols and Ultisols that is as far as you should go in your proposal because they may still want these in Oxisols, for example. There are many oxidic families of soils in Hawaii. Before you drop it completely you must examine its impact in other orders than Alfisols and Ultisols.

Nichols:

We talked to Beinroth and Ikawa in fact on the trip to Brazil. They thought it was doing something for them at the time. Another possibility might be that there could be some change in the formula which you use. The moratorium still exists and Stan Buol and Dr. Ben Hajek are two of the people who have been working on this and have been concerned with it.

Question 139

Daugherty:

How was it intended that the family particle-size classes be determined from the data on horizon basis and weighting by thickness of horizon or by mixing of the samples?

Guy Smith:

Normally we would prefer not to mix the samples because we lose information if we do, but rather by weighting particle-size by thickness of the various subhorizons that were taken. As a general rule one gets along better by fitting a smooth curve, to the data and as a function of depth. Then identifying the control section and from that taking the average of the control section. It often happens that the sampler doesn't sample the control section as such. By drawing this smooth curve one can get at the particle-size distribution of the control section.
Question 140

Allen:

I wonder if you would comment on the concept of the salic horizon and problems that you have encountered, if any, in applying use of the salic horizon in Soil Taxonomy.

Guy Smith:

The salic horizon is defined more or less on the salt content rather than on the genesis. The one great group of soils that we provided for which the salic horizon was diagnostic was a group of soils in which there is relatively shallow salty groundwater, and the salts accumulate at the surface of the soil from capillary rise and evaporation. The Salorthids are suppose to have groundwater at some season of the year before the salic horizon becomes diagnostic. The photograph of the Salorthids in Soil Taxonomy, plate 5D page 101, is of a soil that had groundwater at one time but stream entrenchment has lowered the water table so that it no longer is shallow enough to strictly meet the requirements in Soil Taxonomy. Nevertheless, it seems best to consider that as the Salorthids because the genesis was the same, that of capillary rise and evaporation. There are other kinds of salic horizons in the most arid regions of the world, Peru would be an example, where the salt content is adequate for salic horizon, but it is not at the surface. It is a subsurface horizon, and has been formed by the leaching from the occasional rain that they get on the Peruvian Coastal Plains. The salts there may accumulate to the extent that the salic horizon becomes indurated and you get what could be considered a petrosoalic horizon. These have not been considered diagnostic of anything, in the past. The International Committee on Aridisols that has just begun its work may have another feeling. It was the feeling of our correlation staff, since these didn’t exist in the United States, that they wouldn’t worry about them. When Taxonomy use is extended to other countries, however, this will become a problem that will need debate of the International Committee on Aridisols.

Question 141

Allen:

To continue our discussion on salic horizons and Salorthids. It would be my understanding, or this is the way I have applied it, that any and all horizons in a profile should be considered as salic so long they meet the requirement of having a product of 60 or more cm times the percent salt. Is that correct? We have sampled some soils in which every horizon would have met this requirement.

Guy Smith:

I don’t believe this issue was ever settled. There was discussion about what to do with some of the salt flats in Utah where the salt crust that has formed is thicker than the soil. How these were to be classified was discussed but no agreement was reached. At the time that we were developing Soil Taxonomy there were no series for the salt flats, they were mapped as miscellaneous land types, and identified as salt flats. This would be a similar situation that you asked about but perhaps even more extreme. There are plants growing on these salt flats so they come within our definition of soil. It is another problem I presume that should be brought up before the International Committee on Aridisols. These are not formed by capillary rise from a groundwater. They are not formed by the occasional leaching, but rather they are evaporites from former lakes and could be considered a parent material rather than a soil.
Question 142

Allen:

In a study done here at Texas Tech for an MS thesis on two Salorthids, one soil classified as Typic Salorthid and usually had chromas of 3 or 4, and is wet only a short time in most years. The other soil is wet most of the year and has chromas of 2 or less. However, this latter soil does not have the necessary organic carbon content to qualify as an Aquollic Salorthid. The term Aquollic to me implies both wetness and a relatively high organic carbon content. Yet nothing is said about wetness in the taxonomy as indicated by the low chromas. This is the real question. Is there a place for an Aquic subgroup, which this would have been in my opinion, since it did not meet the organic carbon requirement?

Guy Smith:

There is certainly a potential place for such a soil, but as the Salorthids are defined they are suppose to have ground water at some season. The low chromas of the wetter soils may or may not indicate differences in the wetness of the soil. My experience in the West Indies, I was concerned with working out a better definition of distinction between Pellusterts and Chromusterts. On the Island of Jamaica the highest chromas I think I found were in the wettest soil. It was not only wet but very salty and extremely low in organic carbon. I think the high chromas were simply the effect of lack of energy for the reducing microorganisms. In these salty soils I would say we would need considerable discussion about the use of chroma as an indication of wetness. I don’t know precisely what the effect of a very high conductivity would be on the iron-reducing microorganisms. Perhaps the soil microbiologists should be consulted on that, and would be I think, before any decision was made.

Allen:

I believe, however, that these two soils had a distinct difference in drainage. There were other indications besides chromas according to the field soil scientists the one that had the low chromas, was a poorly drained soil.

Guy Smith:

But the definition might better be based on the depth to the water table instead of on the chroma, I don’t know.

Question 143

Thompson:

I am curious on the nomenclature of the subgroup Aquollic Salorthids as to why the AQU was placed on the name of Aquollic Salorthids rather than just Mollic Salorthids, since Salorthids are defined as being wet?

Guy Smith:

I don’t know precisely why it was called Aquollic rather than Mollic. The proposal for the subgroup came from the soils in the State of Texas at sea level virtually and very close to the coast where the salt conceivably was coming from the Caribbean sea rather than from a

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salty aquifer somewhere. When the proposal was made the southwestern people thought that because these soils did occur in a much more humid environment than the normal Salorthids that they needed to be distinguished. In the 7th Approximation and the first supplements no such subgroup was provided, but for interpretations the correlation staff and perhaps the Texas soil scientists thought that a distinction needed to be made between these Salorthids in the more humid environment from those in the arid environment where most Salorthids had been recognized.

**Question 144**

**White:**

Many alpine slope locations, specifically in Colorado, receive considerable amounts of snow and due to the insulating properties of the snow have higher winter temperatures than are obtained by using ambient air temperature data. Thus the soil temperatures are actually higher and should be classified as mesic, however, the growing season and the soil temperature are typical of a frigid soil. That is the end of his question so I am assuming that he is trying to say - Can we use a different measure for these soils or not? I am sure these are Rick's soils that he is getting on top and the edge of these rock glaciers up in the Rockies. He is getting a different regime from air temperature and a different regime from what the response of the soil is.

**Guy Smith:**

I don't know of any requirement on the winter temperatures for frigid or cryic soils. It is rather typical of some of the cryic soils of southeastern Alaska that they never freeze. They do not have a great deal of snow but they are cold in the summer. I would be astonished if these snow-covered soils came up with a mean annual temperature as high as mesic. The snow in the winter is an insulation and it does raise the mean annual temperature, that is correct. In Alaska the mean annual temperature may be a number of degrees higher than the air temperature, 8 to 10 perhaps. The air temperature is cold enough that these remain as cryic soils or even pergelic. I would want to see some actual measurements on the temperatures of the soil.

**White:**

He was telling us last night that he has tried to get temperatures. The problem some of them discussed yesterday about how do you insert the thermocouples and the problem of disturbing the soil and what have you. Last summer during the field season they were out there and they tried measuring with thermocouples and digging a hole and pressing them into the side and filling the hole back up and they were getting temperatures in the ice of a rock glacier of 48 degrees. There is obviously something wrong in their technique.

**Guy Smith:**

Yes there is. One thing they could do would be to bore a deep hole and put a thermometer down and let it equilibrate with the soil around it and then read the thermometer. We did this in North Carolina to see how it would work. Dr. Daniels was the one who did it and there were no problems in North Carolina, in getting the mean annual temperature from the zone of constant temperature in the soil.
White:

I think I can go a little further than Rich had here. Continue that for the moment. We have had some of those same problems in California. My experience out there getting what we thought were correct soil temperatures. Under these conditions that Rick is working with of high elevations or cold climates. Would it be more beneficial for the taxonomic classification if we were to use say a summer temperature or growing days as a better piece of evidence for pedogenic process than how long it is frozen?

Guy Smith:

Well the definition of, the distinction between cryic and frigid is based on the summer temperature.

White:

He thinks he is over into mesic by looking at the soils. I have not seen these sites but have just talked with him. He thinks the soil reflects more of a low mesic regime because a lot of them are on south slopes so they get a great deal more insulation during the summer, the long days, the higher elevation, less is blocked. He feels up there that summer temperatures might be more beneficial. Maybe something on the old term of degree days like the Soviets use.

Guy Smith:

Summer temperatures are used in the distinctions between frigid and cryic, but not in the distinction between frigid and mesic. The summer temperatures should be relatively easy to measure. Three measurements one a month during the summer at 50 cm depth is all that is required and that is not a problem to find soils that you can bore a hole through 50 cm and insert your thermometer.

White:

This summer he is going to start boring the holes then filling them with styrofoam balls, the little tiny ones like they are using on these solar energy walls to see if he can stop the flow, the flux of energy up and down the hole and giving these aberrant readings.

Guy Smith:

I think it is much simpler to bore a hole to 50 cm and not to go to all the trouble he is going too.

White:

I know in California we had real problems getting temperatures without doing some kind of insulation in the hole, we were using thermocouples on a stalk. We had to backfill with various things. We ended up using that close cell urethane foam that comes out of an aerosol can and inserting the stalk and filling the hole with that to stop the flux. If I may continue with a philosophical question that Rick and I have and a lot of our colleagues. Many of the of the geographers and geologists, earth scientists shall we say, leaving out soil scientists, still refuse to accept Soil Taxonomy. Rick and I each have a little button behind our ear we push and if the tape comes off we feel the benefits of this of Soil Taxonomy opposed to others. I published on it and Rick is attempting too. In your experience with these sorts of individuals outside of soil science, or the so called young turks of geography, what have you found to be a worthwhile technique to convince these hard-headed geologists that the shortcomings of the 1938 system are for the most part overcome with Soil Taxonomy. I am convinced but I have difficulty convincing some of the others. That can be on our tape Charley, I am just so mad at those people.
Guy Smith:

There are geographers who are relatively enthusiastic about Soil Taxonomy, Prof. Bunting would be an example who has published a fair bit on this. Amongst the geologists, like the pedologists I should say, there is strong resistance to Soil Taxonomy from those who learned another system when they were in school. I don't think that anything can be done with some of them. I would mention Dr. Hunt as an example of a geologists who is violently opposed to Soil Taxonomy, and who doubtless will not change his mind. It is necessary for us then to outlive some of these people, and trust that the next generation who learn Soil Taxonomy in the University will take a more moderate attitude.

White:

You don't have any little gimmick that would kick some over the edge.

Guy Smith:

I haven't had enough contact with these people to ever persuade anyone about anything.

Question 145

Calhoun:

Could you comment on the operational usage of Fluventic, Pachic, and Cumulic extragrades? In your comment I would like to know: are there implied landscapes relationships in terms in these three extragrades as they are predominantly used in the Inceptisols and Mollisols, and finally in terms of these three extragrades why weren't these really provided for in the Andepts? That relates back with landscape aspects of these.

Guy Smith:

There were implied differences in landscape positions between the cumulic, pachic, and the fluventic subgroups. The cumulic and the fluventic were supposed to be soils which were receiving fresh sediments at a rate at which the accumulation of organic matter would maintain a thickened mollic epipedon. If the sediments come very rapidly, of course then, the development of the mollic epipedon will not be able to keep up with the rate of accumulation. This was intended to be the fluventic subgroup. Where the accumulation is slow as at the base of a slope, in a concave position, the original intent was that this should be cumulic. Then the pachic subgroup was a rather curious sort of thing for some unknown reasons perhaps. We had soils with ustic moisture regime, but with a much thicker mollic epipedon than normal. The Pachic, I don't believe was used in soils with udic moisture regimes. Pachic is used with Mollisols that have argillic horizons. Whereas cumulic and fluventic are not used for Mollisols that have argillic horizons. The pachic subgroup is in the more stable landscape position than the cumulic or the fluventic soils. The thought was at one time that this over-thickened mollic epipedon, and by over-thickened I mean, thickness that was greater than normal for the environment of the soil, might reflect some local variation in moisture availability. No comparable subgroups were recognized in the Andepts because it was assumed that where you had a soil forming in volcanic ash or pumice that there would be repeated falls of ash or pumice, and that buried soil horizons, buried A1 horizons were considered normal in the Andepts rather than abnormal.
Question 146

Allen:

Both Petrocalcic and Calcic Ustolls and also Paleorthids are relatively extensive in the southwest. It seems reasonable to me to expect the existence of Ustochrepts with a petrocalcic horizon in the area. Such soils were recorded from Iran in a paper published about two years ago. I believe a Petrocalcic Ustochrepts subgroup was recommended. I am wondering why no provision was made for such soils in *Taxonomy*?

Guy Smith:

The subgroups that were provided in *Soil Taxonomy* were primarily those for soil series that were either established or tentative in the United States. A few subgroups were provided that were not known to occur in the United States, but this was only done when we had a specific request. If we have a series in the U.S. of Ustochrepts of Xerochrepts with a petrocalcic horizon then it is very likely that we would have provided such a subgroup. It is an implied subgroup in *Soil Taxonomy* in that the Typic Ustochrept has a calcic horizon or soft powdery lime but no petrocalcic horizon is provided for. There is no question if a soil had a petrocalcic horizon instead of a calcic horizon we would have recognized two series. One for the petrocalcic horizon and one for the calcic horizon. Had we had such a tentative series or established series, I think without question that a petrocalcic subgroup would have been provided.

Allen:

To make a correction I believe the proposed subgroup in the paper from Iran was a Petrocalcic Xerochrept rather than a Petrocalcic Ustochrept. To switch a little, Dr. Smith, this question is from Dr. Ron Paefzold. Has there ever been any consideration given in developing *Taxonomy* for artificial moisture regimes where the soil moisture is controlled through drainage and/or irrigation?

Guy Smith:

To some extent quite a bit of attention was given in that the aquic suborders of great groups are supposed to have an aquic moisture regime or artificial drainage. This is not a man-made change in the soil and we discussed this at some length because the ground water level has been altered by the artificial drainage and there is no way that is practical or feasible for the soil surveyor to determine what the groundwater level was before the drainage. We don't want to close the tile drains to find out what it becomes if we stop the drainage. Further in the definition of the moisture regimes and in many of the taxa where we are referring to periods of dryness in the soil, we specify that these periods apply to soils in which there is no artificial management of the soil moisture as by fallowing, water collection, or irrigation. The Typic Ustochrepts have an item which reads "When neither irrigated nor fallowed to store moisture". Then we specify the length of dryness. So these are examples of proof that we did consider, the artificial management of soil moisture. We also specified at the beginning of the book that we had not attempted yet to classify soils that were artificially flooded for rice production. The development of this was not practical in the U.S. One of these days someone is going to ask for an International Committee to consider the classification of these soils. A committee has been established the International Committee on Aquic soils (ICOMAQ).
Question 147

Allen:

Dr. Smith, I have one additional question here. After teaching horizon designations and the meaning of various suffixes, for example, p, ca, etc. in soil classification courses, I find it difficult when I switch to the meaning of argillic, calcic and so forth in the teaching of Soil Taxonomy per se. Switching to the named diagnostic horizons. Now you have already given the reasons behind this and I am merely asking in your teaching do you have any particular short cut on this to really get the point across as to why this diagnostic horizons is needed rather than the symbols?

Guy Smith:

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

Allen:

This concludes our discussions in Soil Taxonomy. Dr. Smith, I think all of us who are here agree that these discussions have been extremely valuable to us and we trust that the material that will come from this will be valuable to many other people and we are confident that they will; so again we want to thank you very much.